When Careers Stall: The Effects of Temporary Employment on Fertility and Infant Health

Mario Martínez-Jiménez [1, 2] and Judit Vall Castelló [3]

[1] *Department of Health Policy, Stanford School of Medicine, Stanford University, United States of America*

[2] *Department of Economics & Public Policy, Imperial Business School, United Kingdom*

[3] *Department of Economics, Universitat de Barcelona, IEB CRES-UPF, Spain*

Abstract

We study the causal effect of temporary employment on fertility and neonatal health by exploiting exogenous variation in the availability of temporary contracts induced by the 1984 Spanish labor market reform as a quasi-natural experimental setting. This reform resulted in a significant liberalization of fixed-term contracts, leading to an increase in job insecurity and a reduction in the career prospects of low-skilled workers entering the labor market under this regime. Using administrative and survey data, we estimate a within-cohort difference-in-difference regression discontinuity design model. We find that low-skilled mothers affected by the reform have fewer children, which translates into lower completed fertility, while the timing of their first birth remains unaffected. We find modest but significant rises in preterm birth and low birth weight exclusively among mothers aged 30 and above. Two mechanisms appear to drive these results: affected mothers are less likely to hold permanent employment in the long run, and they fall short of their desired fertility. Our study provides the first causal evidence that temporary employment affects long-term fertility decisions, showing that job insecurity depresses completed fertility in developed countries with historically rigid labor markets.

Keywords: temporary employment, fertility, perinatal health
JEL Codes: I12, I14, I18, J13, J16, J24

Corresponding author: *Mario Martínez-Jiménez*. Email: mmarjim3@stanford.edu.

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1. INTRODUCTION

Low and declining fertility rates have drawn renewed attention in the United States and across Europe, even though in many countries this trend began decades ago (Kearney et al. 2022; Goldin 2025a).[1] Most high-income economies are now well below replacement level (OECD 2024). Previous research shows that rising childlessness at all ages and smaller completed family sizes signal a structural shift regarding motherhood rather than a temporary response to short-term shocks (Kearney & Levine 2025). Institutional changes, including labor market regulation, have influenced not only the opportunity cost but also social and cultural attitudes regarding maternity (Kearney & Levine 2025). Spain exemplifies these dynamics: total fertility dropped from 2.77 births per woman in 1976 to around 1.12 in 2023, a historic low (INE 2020). Over the same period, female labor force participation rose substantially, from 27.1% in 1976 to 52.6% in 2024 (Group 2024), and among young women (16–24) from 29.1% in 1976 to 84% in 2024 (OECD 2024). In this paper, we use the 1984 Spanish labor market reform as a quasi-natural experiment and provide what we believe is the first causal link between a nationwide expansion of temporary contracts and both fertility decisions and infant health. We estimate the long-term effects on completed fertility, birth timing, and rates of preterm birth and low birth weight.

Temporary employment is widespread across the OECD, with approximately 12% of workers on fixed-term contracts in 2024 (OECD 2024). Its rise has been especially consequential in Southern Europe. In this region, postwar labor markets featured strong employment protection for permanent workers: dismissals required strict justification, lengthy procedures, and carried substantial severance liabilities and reinstatement risks. These rules made separation costly and predictable for employees. When labor market reforms arrived in the second half of the 20th century, a liberalization in the margins (fixed-term hiring) rather than the core (permanent contracts) occurred, setting the stage for a dual labor market.[2] Spain exemplifies this evolution: following the 1984 labor market reform that permitted the broad use of fixed-term contracts with lower dismissal costs and minimal justification, temporary employment expanded sharply from around 2.2% in 1983 to 20% in 1987. Overall, 29.7% of Spanish workers have a temporary contract, with this percentage being higher among women (33.6%), those without college (INE 2020).[3] Prior research has linked this expansion to weaker long-term employment and earnings prospects, especially for low-skilled

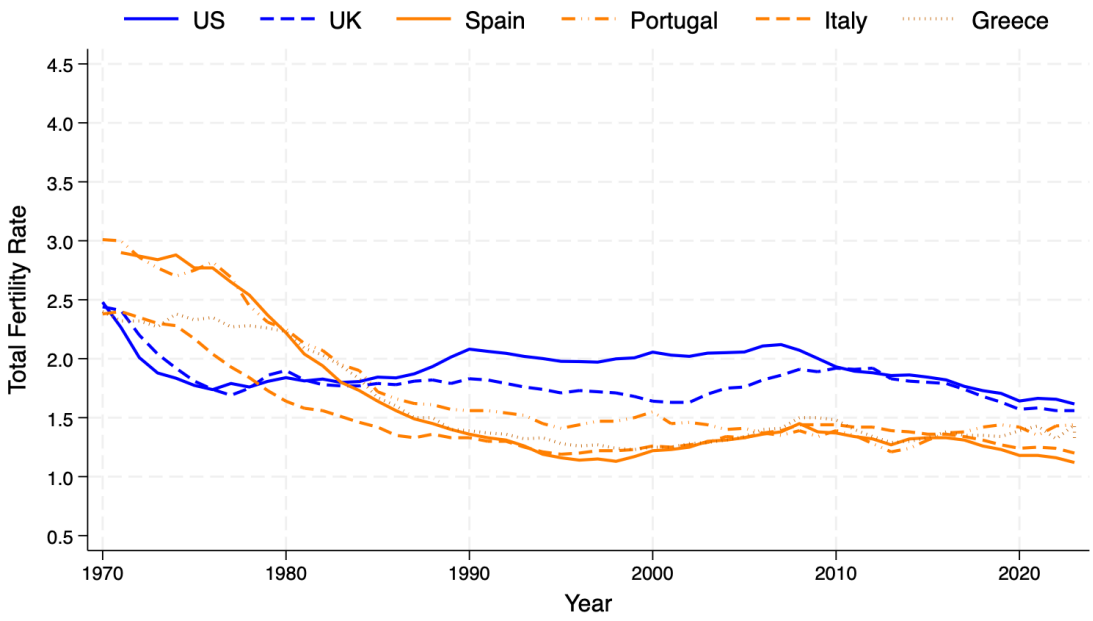
[1] The decline in total fertility rates in most OECD countries began in the mid-1980s for many high-income nations (Goldin 2025a), as shown in Figure 1.

[2] By a dual labour market, we mean a segmented system created when reforms liberalised fixed-term hiring at the margins but left permanent contracts highly protected, producing an insider–outsider divide—incumbents with strong protections versus new entrants confined to unstable temporary jobs.

[3] In Appendix B, Table B1 shows the types of contracts in place in 1984, including their severance and duration, while Table B2 presents the proportion of individuals holding temporary contracts, categorized by education, age, and gender.

entrants (Blanchard & Landier 2002; Oreopoulos et al. 2012a; García-Pérez et al. 2018)). This liberalization pushed low-skilled, early-career women into temporary jobs, raising their income and employment risks and reducing their eligibility for maternal benefits and job protection. However, its effects on family formation and early-life health are overlooked.

Figure 1: Total Fertility Rates for selected OECD Nations: 1970 to 2023



Notes: This figure shows the total fertility rates for two groups of economic development countries: blue countries experienced relatively continuous economic growth during the 20th century, while orange countries experienced rapid growth acceleration in the second half of the 20th century. The total fertility rate represents the number of children that would be born to a woman if she were to live to the end of her childbearing years and bear children in accordance with age-specific fertility rates of the specified year. *Source:* 1970 to 2023: World Bank Group: World Development Indicators. Accessed: 11/14/2025.

The 1984 Spanish labor reform liberalized temporary contracts while leaving stringent protections and high severance for open-ended jobs unchanged (Dolado et al. 2002). This one-time, nationwide shock produced a discrete increase in the share of labor-market entrants who began on temporary (rather than permanent) contracts, especially among low-educated women, without contemporaneous changes to other employment protections. We treat entry mother's cohort as an exogenous source of exposure and estimate a regression discontinuity design combined with difference-in-differences (following Oreopoulos et al. (2012a)), which compares low-educated mothers (exposed) with high-educated mothers (comparison) before and after the reform was implemented (following García-Pérez et al. (2018)). The specification allows to control for

group-specific trends and slope changes at the reform cut-off, and we validate identifying assumptions with event-study design.

We utilize administrative birth records for all deliveries in Spain (1984–2018)—reporting mothers' age at birth, perinatal outcomes, occupation, and date of birth—and microdata from the 1999 and 2018 Spanish Fertility Surveys, which provide rich sociodemographic characteristics and attitudes to study potential mechanisms. Our results show that low-skilled mothers who enter the labor market after the introduction of the reform have significantly fewer children, which translates into lower completed fertility, while the timing of their first birth remains unaffected. We find modest but significant rises in preterm birth and low birth weight exclusively among mothers aged 30+, while outcomes for younger mothers are unchanged. Two mechanisms appear to drive these results: affected mothers are less likely to hold permanent employment in the long run, and they fall short of achieving their desired fertility. There is no evidence that the reform altered social or cultural attitudes that would suggest that parenthood occupies a diminished role in life planning among low-skilled mothers.

This paper contributes to three strands of literature. First, we build on the foundational work of Goldin (2021, 2025a), Doepke & Kindermann (2019); Doepke et al. (2023), and Kearney et al. (2022); Kearney & Levine (2025), which provide potential explanations for the recent decline in fertility in high-income countries at the micro level. We focus on low-skilled mothers whose completed fertility we can observe, who entered increasingly dualized labor markets characterized by higher income and employment risks, and weaker eligibility for parental benefits and job protection. Compared to highly skilled women, these women often postponed having children to work, rather than to pursue further education (Goldin 2025a). Second, we expand on previous studies on the unintended consequences of labor market conditions on family formation (Adsera 2004; de la Rica & Iza 2005; Auer & Danzer 2016; Gatta et al. 2022) by providing the first causal analysis of how fixed-term jobs at entry affects both the completed fertility and infant health. Our analysis leverages a labor market reform that exogenously increased the use of temporary contracts to establish a causal link between (foreseeing) temporary employment and fertility decisions. Third, we contribute to a growing body of literature that broadens the perspective on the reasons for fertility declining (Goldin 2025b; Briselli & González 2025). We investigate changes in cultural attitudes, such as more egalitarian views regarding women's roles in household and religious beliefs, as well as other sociocultural factors, including their desired fertility once their fertility window ended. This enables a more comprehensive analysis of the channels through which institutions influence family formation beyond opportunity costs, prices, and income, which are the main focus of typical economic studies on fertility (van Wijk 2024).

2. BACKGROUND

2.1. *Conceptual framework*

Economic models explaining fertility behavior (Becker 1960; Black et al. 2013) propose that parents weigh the benefits and costs of having children, including disposable income, child-rearing expenses, and the time available for childcare. In this section, we present a conceptual framework to elucidate how (foreseen) continuous spells of temporary employment may influence fertility decisions and infant health.[4]

Temporary employment creates two types of effects: an income effect that decreases intra-household resources for childbearing (reducing demand for children) (Becker 1965; Borg 1989), and a price effect that lowers the value of working relative to time spent with children (potentially raising fertility) (Becker 1965; Schultz 1985; Hotz & Miller 1988). Job insecurity can also affect decisions to have children, potentially delaying or deterring family formation (Goldin 2025a). Temporary contracts are inherently insecure and, in many countries, lack certain employment protections. Workers in such jobs may also be excluded from financial services like loans or mortgages. Most Southern European labor markets exhibit duality, with both temporary and permanent contracts. Those starting with temporary contracts face a higher risk of unemployment and lower incomes, whereas permanent employees enjoy greater job security and more robust social protection (including maternity leaves).

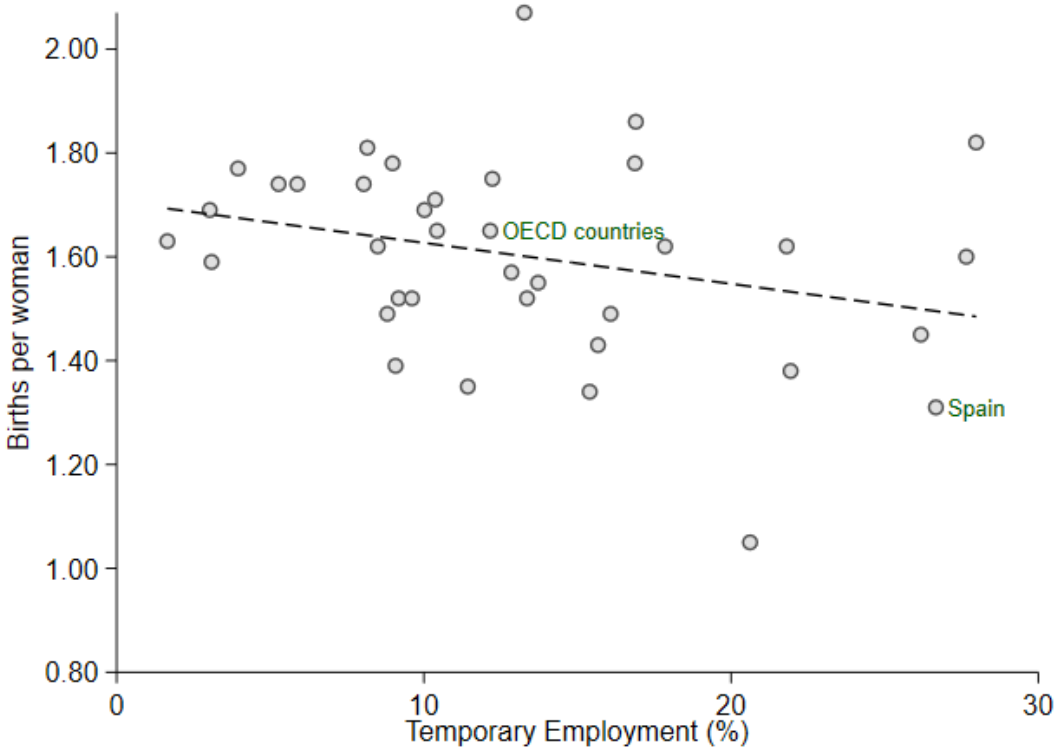
Women's temporary employment rates are higher than men's in many OECD countries, with significant gender differences observed in Japan, Finland, Greece, Korea, Spain, and Sweden (OECD 2022). It is reasonable to assume that young women view job insecurity as a crucial factor when making major life decisions such as having children (Becker 1960).[5] There are three main channels through which job insecurity may reduce fertility (Ranjan 1999; Sommer 2016). First, employment in positions with a high risk of dismissal introduces uncertainty regarding future earnings, which are essential for covering long-term childcare expenses. Second, women may postpone fertility while working on temporary contracts, seeking to gain tenure and secure promotions (Del Boca 2002). Although the long-term effects of employment uncertainty remain unclear (Bhaumik & Nugent 2005), individuals may still achieve their desired family size if uncertainty is temporary. However, persistent job insecurity may lead some women to have fewer children than they wish. Third, changes in labor market institutions can induce shifts in societal culture and norms. Recent studies

[4] We defined continuous spells of temporary employment as those workers who cycle from one fixed-term contract to the next with little or no break, implying persistent job instability and limited prospects for a stable, long-term income and work. This employment pattern is particularly pronounced in Southern European economies, where dual labour markets and high dismissal costs for permanent jobs are the norm (Oreopoulos et al. (2012b); García-Pérez et al. (2018)).

[5] Figure 2 shows the relationship between average annual fertility rates and the share of temporary employment among OECD countries in 2017, which a clear negative correlation.

highlight that fertility declines in high-income countries reflect long-term changes in adult priorities, rather than just temporary shocks. Evidence from Kearney & Levine (2025) shows rising childlessness and declining completed fertility, suggesting a diminishing role for parenthood amid changing social norms, opportunities, and constraints. Moreover, as more women gain autonomy and move away from traditional roles while many men retain them, mismatches in gender preferences create relationship difficulties that further lower birth rates (Goldin 2021, 2025a).[6]

Figure 2: Temporary employment and fertility in OECD countries



Notes: This figure shows the relationship between average annual fertility rates and the share of temporary employment in 2017. We can see that countries that rely heavily on temporary employment in the labor market have lower birth rates per woman. *Sources:* OECD statistics.

Finally, uncertainty in parental income and parents' time investments may also affect infant health (Heckman 2012). Prior studies link job insecurity with increased rates of suicide, mental illness, and poor physical health (Reeves et al. 2014; Rohde et al. 2016; Grigoriadis et al. 2018; Lepinteur 2021). A decline in a mother's well-being while pregnant may translate into worse perinatal health outcomes. We therefore inves-

[6] In Appendix A, we present a simple theoretical model of demand for children and examine how temporary employment at the time of female workers' entry may distort their fertility decisions.

tigate whether exposure to temporary employment at entry affects infant health, specifically preterm birth and low birth weight.

2.2. *Literature review*

Previous studies on the link between economic uncertainty and fertility have shown mixed results. Some research suggests that economic uncertainty delays motherhood (Del Bono et al. 2012; Prifti & Vuri 2013; Clark & Lepinteur 2022), while other studies find no effects, or even positive effects, of uncertainty on women's employment careers and fertility decisions (Kreyenfeld 2009; Santarelli 2011).

One consistent element found in the literature is the presence of different effects depending on the type of economic uncertainty. Bhaumik & Nugent (2011) conclude that employment-related uncertainty significantly lowers the likelihood of childbirth compared to financial or economic uncertainty. Kind & Kleibrink (2013) suggest that uncertainty at the individual level is more likely to influence fertility decisions than uncertainty at the macroeconomic level. This finding aligns with Scherer (2009) article about a decline in fertility due to personal subjective measures of insecurity, such as perceived job security and uncertainties about one's economic situation (Kreyenfeld 2009). The impact of job insecurity on fertility choices varies by gender and education level. Male career advancement increases fertility, while female job progression decreases fertility (Hotz & Miller 1988; Heckman & Walker 1990; Schaller 2016; Dorn & Hanson 2019). Hanappi et al. (2017) show that highly educated individuals were the only ones affected by employment uncertainty. However, highly educated women have more financial resources to manage the negative effects of job instability and tend to re-enter the labor force more quickly after having a child (Liefbroer & Corijn 1999; Adserà 2011).

Studies have shown that temporary employment negatively impacts fertility decisions (Adserà 2004; Gutiérrez-Domènech 2008; Modena et al. 2014; Vignoli et al. 2020). Studies conducted in Finland, Germany, and Spain reveal varying effects of temporary employment on fertility behavior. (Sutela 2012) finds that holding a temporary contract in Finland resulted in a decline in first-child fertility rates among 20- to 44-year-old employees, and (Auer & Danzer 2015) conclude that temporary contracts in Germany were linked to postponing first birth and reducing the number of children in the first decade after graduation. Ahn & Mira (2003) observe no significant associations between joblessness and part-time work on fertility behavior in Spain. However, Rica & Iza (2005) show that temporary female employment in Spain caused delays in motherhood compared to women with permanent jobs. However, these studies largely identify associations rather than causal effects: they lack plausibly exogenous variation in contract type (e.g., reforms or instruments that shift temporary versus permanent employment independently of fertility preferences), so selection into temporary work may confound the estimated relationships.

A growing literature is showing that institutional change shifts social norms, yielding persistent declines in completed fertility and rising childlessness that reflect long-run changes in preferences and constraints (Kearney & Levine 2025). A central channel is gender-preference mismatch: women's expanded market opportunities outpace men's adaptation in domestic specialization, creating partnering frictions that depress realized fertility (Goldin 2021, 2025a). These findings imply fertility shortfalls are not merely cyclical responses to shocks but stem from slow-moving cultural adjustments that empirical work should model alongside economic incentives. Building on this insight, we examine how an exogenous institutional reform propagated through social and cultural pathways—shifts in partnering, gender-role norms, and the gap between desired and realized fertility—to reshape attitudes toward motherhood over the long run.

This study examines the impact of employment uncertainty on fertility decisions in Spain. Here, we analyze the timing of first childbearing and overall fertility rates using administrative and survey data. Our novel approach measures an individual's (look-forwarding) job insecurity and uses a quasi-natural experiment to estimate a difference-in-differences regression discontinuity design analysis, which identifies an adverse causal effect of temporary employment on (completed) fertility and children's health at birth for low-skilled women. As there is limited evidence on causal effects, the potential impact on fertility and infant health of policies restricting temporary employment, such as the recent labor market reform introduced by the Spanish government in 2022, is still unclear.[7]

3. THE SPANISH LABOUR MARKET

Under the Franco dictatorship from 1939 to 1975, the Spanish labor market was highly regulated and based on two fundamental pillars. First, The Vertical Labor Union (1940-1970) was the only legal trade union, and membership was mandatory. Second, there were strict labor regulations that made it nearly impossible to fire a worker or go on strike. After Franco's death in 1975, the Spanish government introduced the Workers' Statute regulation in 1980, which extensively modernized the labor relations system.[8] Fundamentally, this law established permanent contracts as the standard framework, making temporary contracts highly restrictive and limited to seasonal jobs.

The government introduced a new labor market reform on August 2, 1984, which took effect in October of that year to increase labor market flexibility.[9] This reform aimed to make temporary labor contracts more

[7] Starting on March 30th, 2022, all job contracts will be considered permanent unless they are signed specifically for a temporary/seasonal activity that requires a structural change. This reform aims to reduce the use of temporary contracts, which are currently the most common, although it is still early to assess its impact accurately.

[8] More information related to Law 8/1980 "Estatuto de los Trabajadores" (ET), from 10th of March, of Workers' Statue could be found here: https://www.boe.es/buscar/doc.php?id=BOE-A-1980-5683.

[9] In August 1984, the socialist government began discussions with prominent labour and business organisations to bring about significant reforms. The goal was to achieve maximum social consensus for the proposed changes. Negotiations resulted in the official signing of the "Economic and Social Agreement (ESA)" for the upcoming two years of the legislative period, which included the Spanish labour market reform on October 10, 1984.

flexible by separating them from seasonal jobs and allowing them to be used for all regular job activities. This helped firms manage demand uncertainty by providing additional flexibility. As a result of the 1984 labor market reform, fourteen different types of contracts were introduced, with temporary contracts being the most widely used. Temporary contracts have a limited duration, cannot be renewed for the same job at the same company after three years and have a high likelihood of termination. Employers find them attractive due to their short duration and low severance payments. On the other hand, permanent contracts have no end date or time limit but are less likely to be terminated. Firing a permanent worker requires a payment of 20 days of wages per year worked, compared to the 12-day requirement for temporary contracts. As a result, companies have little incentive to switch from temporary to permanent contracts, which explains the Spanish duality between permanent and temporary employment (Aguirregabiria & Alonso-Borrego 2014).[10]

Figure 3: Total share of temporary employment and unemployment in Spain (1981-2018)

Notes: This figure shows the evolution of temporary employment and unemployment rates in Spain since records are available. We can see that temporary employment experienced a subtle rise immediately after the 1984 reform, from 2.25% in 1983 to around 14% in 1985. *Source:* Own elaboration using data to construct the temporary employment share and unemployment rate from the Spanish Labor Force Survey (LFS) and Spanish Chamber of Commerce for years between 1988-2018, and data from the Central Balance (Bank of Spain), Spanish Working Conditions Surveys (SWCS), the Economy and Labor ministries survey, World Bank, and newspapers data for years between 1981-1987. The vertical red line indicates the year of the labor market reform, 9th November 1984.

[10] See Table B1 for further details on the contract types implemented under the 1984 labour market reform.

Figure 3 shows the percentage of temporary employment between 1981 and 2018, i.e., before and after the 1984 reform was implemented (October 1984). The proportion of temporary employees as a percentage of total employment increased from 2.2% in 1983 to around 33% in the late 1990s. Although the temporary employment rate increased rapidly after the 1984 reform, the unemployment rate remained consistently high and followed its historical trends during the same period.[11] The 1984 Spanish labor market reform led to an increase in temporary contracts for new entrants, particularly youth, women, and unskilled workers. The percentage of young individuals aged 16-24 in temporary employment reached an impressive 73% in 1998. The average conversion rate from temporary to permanent contracts between 1987 and 1996 was only 14%.

4. DATA

4.1. *Birth certificates data*

Our study utilizes two distinct data sources. The first is a comprehensive administrative dataset from the Childbirth Statistics Bulletin, which covers all births in Spain. Provided annually by the Spanish National Statistics Office from 1984 to 2018,[12] this dataset includes information on the birth itself, infant health, and the parents' socio-demographic backgrounds (such as month and year of birth and occupational status). The data is collected through a mandatory document that parents or relatives complete to legally register the birth with the Civil Registry.[13]

Our final sample consists of native mothers born between 1962 and 1972, making them 43 to 53 years old in 2018.[14] This allows us to observe their largely completed fertility histories. When mothers register a newborn at the Civil Registry, they must report their current occupational status by selecting one of twelve official categories on the registration form.[15] We harmonize these categories into a binary skill measure based on the occupation's typical skill content. Specifically, we classify as high-skilled: professionals; executive leadership positions; senior management of public-administration bodies and private firms; and administrative and related occupations. We classify as low-skilled: small traders and shopkeepers; service-sector workers; farmers, plant breeders, fishers, and hunters; production workers and related groups, drivers, and laborers/pawns; professional members of the armed forces; and persons dedicated to housework. We exclude records listing students, retirees/pensioners, and unclassified/other from the analysis. As part of our robustness checks, we included students as a control group, confirming that their inclusion does not

[11] Table B3 details the evolution of temporary workers over the next year, highlighting the low rate of transition to permanent employment.

[12] Although birth certificate data has been collected since 1980, our analysis includes only those births from October 1984 onward, after the reform was implemented.

[13] Appendix C.1 provides more details on the birth certificate data, including a link where the microdata can be readily downloaded: INE database.

[14] Children of immigrant mothers differ significantly from those of Spanish women not only in origin, lifestyles, reproductive behavior, and birth outcomes, but also in having more interrupted and discontinuous labor market pathways.

[15] We are unable to use their educational status due to more than 30% of the data being missing.

materially affect our main results. Appendix D, Table D1, reports the complete crosswalk from the twelve registry categories to our high- and low-skill classification.

We focus on prospective job uncertainty at entry by assigning treatment based on whether a mother's labor-market entry occurred before or after the 1984 liberalization of fixed-term contracts. For low-educated women—who are most likely to enter at the earliest legal age—we proxy the quarter of labor-market entry at the fourth quarter of 1968 (1968Q4), since Spain's minimum working age is 16.[16] Our main analysis window covers low-educated cohorts born 1965Q1–1972Q4, who turn 16 1984Q3. Cohorts turning 16 before 1984Q3 are classified as pre-reform, while those turning 16 in/after 1984Q3 are post-reform. Therefore, the cohort born 1968Q3 (turning 16 in 1984Q3) is the first affected cohort to face the liberalized temporary-contract regime at entry.

For high-skilled women, we proxy labor market entry at age 18, the typical high school graduation age in Spain. We therefore include cohorts born between 1962 and 1969, who turn 18 between 1980Q1 and 1987Q4. Cohorts reaching 18 before 1984Q3 are classified as pre-reform, and those reaching 18 in/after 1984Q3 as post-reform. Because Spain's school year begins in September, women born in 1966Q1 graduate in summer 1984 at the earliest and constitute the first high-skill cohort exposed to the liberalized fixed-term regime at entry. Our design exploits skill-specific entry timing by cohort and quarter, yielding differential, exogenous exposure to the 1984 reform for identification of its effects on fertility. As a robustness check, we expanded the window to ± 12 quarters (adding four quarters on each side); the results remain stable using a larger bandwidth.

We construct our main fertility outcome, the number of births per mother's birth, using the universe of birth registers. To do so, we need to collapse the microdata from all birth records into cell-level counts, with each count representing the number of births to mothers in each mother's birth cohort (by quarter) (c), province of residence (p), and skill group (s), denoted as (B_{cps}). One baseline outcome is the natural logarithm of the resulting number of births ($ln(B_cps)$). To scale by cohort size, we construct a cohort exposure (denominator) using INE's monthly live-birth series (1962–1972) at the provincial level and multiply by the proportion of skilled and unskilled mothers in the studied sample, denoted N_{cps}. We aggregate these monthly data to quarters and map them to the mother's province and quarter of birth to obtain cohort size. Our second baseline outcome is then the cohort of birth rate, which is defined as the number of births divided by the total of women born in that cohort, province and skill group (B_cps/N_cps).[17]

[16] The minimum legal age in Spain for working individuals was 14 before 1980 and 16 since then. However, as we can see in García-Pérez et al. (2018), in 1977, there was already effective legislation that increased the age to 16 years old so that 16 was the binding minimum working age since 1977 in Spain.

[17] Appendix C1.1. includes a detailed, step-by-step explanation of how we created this denominator variable.

Our second main fertility outcome is the mother's age at the child's birth. The data do not clearly identify parity, so we cannot distinguish first from higher-order births at the micro level. Therefore, we just use the mother's age at birth as an outcome. For infant health outcomes, we use measured birth weight (grams) and gestational age (weeks), and define the following indicators: low birth weight (<2,500 g), high birth weight (≥4,000 g), and preterm (<37 completed weeks), following standard clinical thresholds defined by the World Health Organization.[18]

4.2. *Fertility survey data*

The second data source is the Spanish Fertility Survey (FS) waves 1999 and 2018, which provide nationally representative microdata on women's complete fertility histories alongside rich socio-demographic and cultural norms and values regarding fertility decisions.[19] We use these surveys to study fertility behavior and to study socioeconomic and cultural attitudes as mechanisms. To align with the birth records analysis, we restrict the sample to native mothers born 1962–1972 and reproduce the same treatment (post-1984 entry) and comparison definitions by birth cohort quarter and skill level. Because occupational status in the FS has substantial missingness, we define low-skill as high school dropouts and high-skill as women with at least a high school diploma; results are robust to using reported occupation when available (see Appendix C, Table C2). It is important to clarify that mothers were aged 27-37 in 1999 and 46-56 in 2018. Although including 1999 FS mothers indicates that their fertility was not yet complete, this inclusion increases our sample size.[20] From the FS we construct individual-level fertility outcomes: the number of children per woman and age at first birth. This analysis complements the cohort-level measures in the registers and allows us to distinguish effects on quantum (family size) from tempo (timing of entry into motherhood).

The 2018 wave also includes detailed economic, demographic, and cultural indicators that we use to investigate mechanisms underlying the relationship between temporary employment and fertility. We construct three groups of covariates to investigate the contemporaneous mechanisms linking temporary employment to fertility decisions. First, we include a set of socio-economic and demographic variables: marital status; homeownership; labor-market status (holding a permanent contract); self-reported financial strain (difficulty making ends meet); and a monthly income indicator for earnings below the poverty threshold.[21] Second, we utilize a set of variables that capture changes in social and cultural attitudes, as available in our data. These include a desired-fertility indicator (equal to one if the respondent wished for more children); age at economic

[18] Summarized definitions from the WHO overview: WHO guidelines.

[19] Appendix C.2 provides more details on the two Spanish fertility surveys, including a link where the microdata can be readily downloaded.

[20] Our estimates using just the 2018 Fertility Survey are quantitatively similar to those from the estimates from our main analysis.

[21] We defined the poverty threshold as having an income below the current minimum wage.

independence from parents; an indicator for informal childcare availability (no relative help for ages 0–3); declining religious observance (non-religious affiliation); and an egalitarian views index (the standardized average of seventeen items on gender roles, caregiving, partnership norms, and work–family trade-offs, where higher values indicate more egalitarian views). Third, we include covariates to determine whether our cohort of women was influenced by other confounding factors, such as: education (age at completion of studies), labor market composition (unemployment within the past six months, current employment status, age at first job) and shocks (prevalence of disability or chronic illness), and access to family-policy support (flexible childcare options at work).[22]

4.3. *Descriptive statistics*

Table 1 presents descriptive statistics on fertility and infant health outcomes, along with a set of socioeconomic characteristics, by mothers' skill level. On average, low-skilled mothers have their first child younger than high-skilled ones, but both groups end up having a similar number of babies, as shown in Panel B using the 1999 and 2018 Fertility Surveys. Panel C shows that low-skilled women have worse long-run career paths, with lower probabilities of obtaining a permanent contract and a higher likelihood of long-term unemployment and difficulties reaching the end of the month. In contrast, these women begin working and become economically independent of their parents at a younger age than high-skilled women.

We can get a graphical preview of our results in Figure 4, which plots the average age at first birth and the mean number of children by quarter cohort and by women's skill level, using the 1999 and 2018 Fertility Surveys. The vertical line marks the first quarter cohort of women affected by the reform (as defined in the previous section). We observe that, before the introduction of the labor reform, both high- and low-skilled mothers entering the job market showed similar trends in both outcomes. However, there is an apparent drop in the mean number of children, particularly affecting low-skilled mothers who enter the labor market during the quarter of the reform's implementation. There is no change in trend for any group of women in the age at first child before and after the introduction of the labor market.

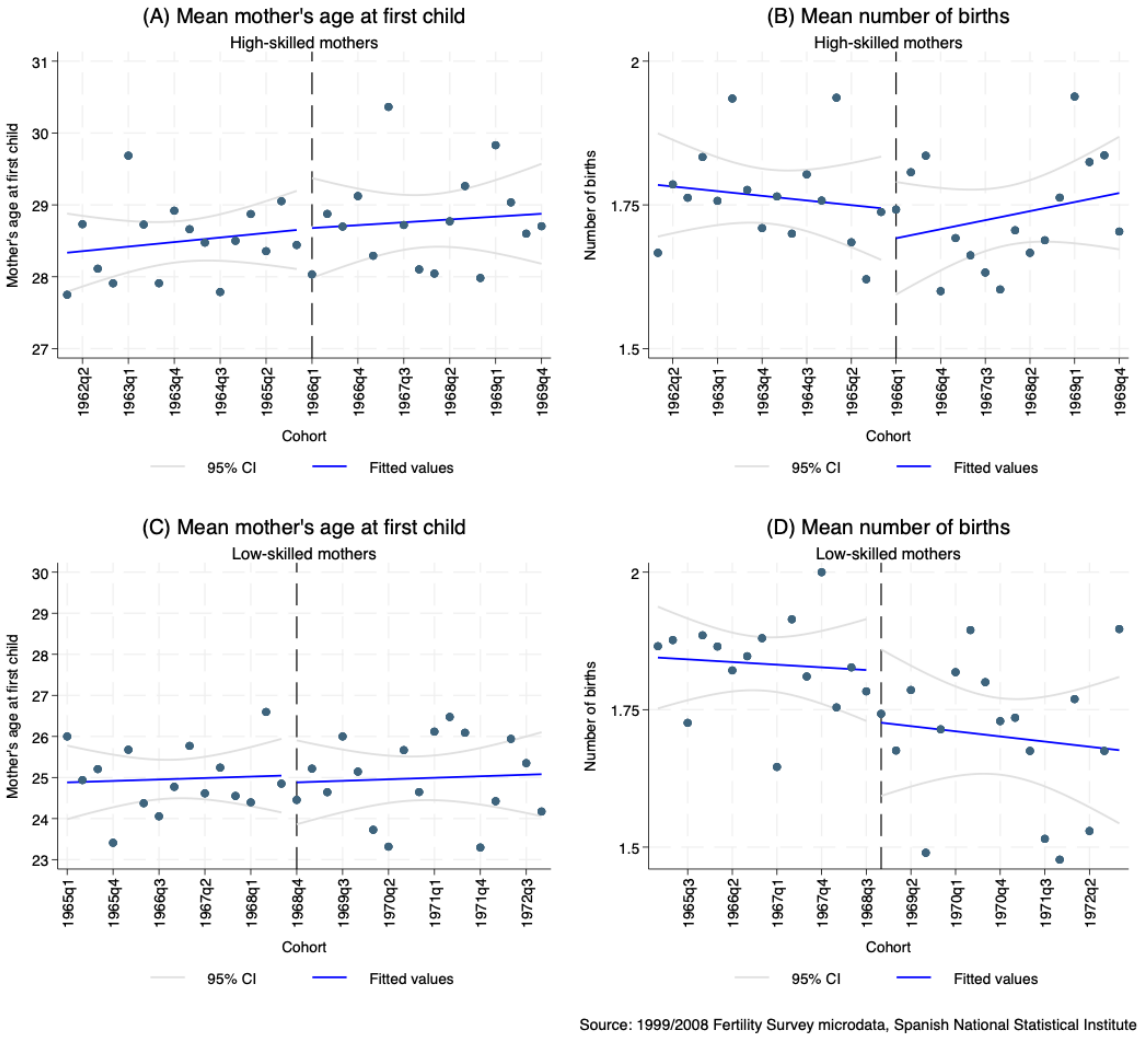
[22] Table D2 contains the variables used for this Egalitarian Views Index.

Table 1: Descriptive Statistics by mother's skill level

	Low-skilled mothers		High-skilled mothers	
Variables	Mean	SD	Mean	SD
*Panel A: Birth Records (1984–2018)**				
Mother's age at birth	27.96	1.216	31.50	0.687
Log number of children (per cohort)	6.615	0.965	6.020	1.061
Share of births (cohort-prov-skill) ‡	0.226	0.096	0.348	0.147
Birth weight (grams)	3220.9	54.0	3242.4	52.0
Low birth weight (<2,500g)	0.060	0.016	0.057	0.018
High birth weight (≥ 4,000g)	0.134	0.088	0.065	0.023
Gestational age (weeks)	39.00	0.208	39.07	0.197
Preterm birth (<37 weeks)	0.070	0.024	0.070	0.095
Observations	1,664		1,664	
Panel B: Fertility Survey (1999/2018)				
Mother's age at survey	40.821	9.645	46.565	8.561
Mother's age at any birth	26.503	5.616	30.318	5.226
Mother's age at first birth	24.939	5.049	28.657	5.025
Number of children	1.777	0.748	1.750	0.720
Observations	1,654		2,079	
Panel C: Fertility Survey (2018)				
Mother's age at survey	49.134	2.297	51.592	2.205
Temporary contract (curr.)	0.291	0.455	0.135	0.342
Currently working	0.491	0.500	0.743	0.437
Monthly income >1000	0.161	0.368	0.545	0.498
Married	0.776	0.417	0.787	0.409
Difficulty making ends meet	0.627	0.483	0.378	0.485
Any disability	0.077	0.267	0.047	0.213
Homeownership	0.846	0.360	0.904	0.293
Ever long-term unemployed	0.690	0.462	0.570	0.495
Age at first job	21.958	8.391	23.390	6.259
Age at econ. independence	21.918	4.053	23.363	3.669
Age finished studies	19.701	5.425	23.103	5.700
Egalitarian views (index)	0.704	0.123	0.756	0.111
Desired more children	0.225	0.417	0.308	0.462
No informal childcare	0.070	0.256	0.047	0.213
Observations	920		1,502	

Notes: Standard deviations in parentheses. Native mothers who are low-skilled (born 1965–1972) or high-skilled (born 1962–1969). *Birth Records sample collapsed by quarter-cohort, province, skill (covering 1,174,695 births to high-skilled; 1,544,375 to low-skilled). Fertility Survey outcomes are individual-level. ‡ Share of births by mother's cohort-quarter and province (denominator constructed per Appendix C).

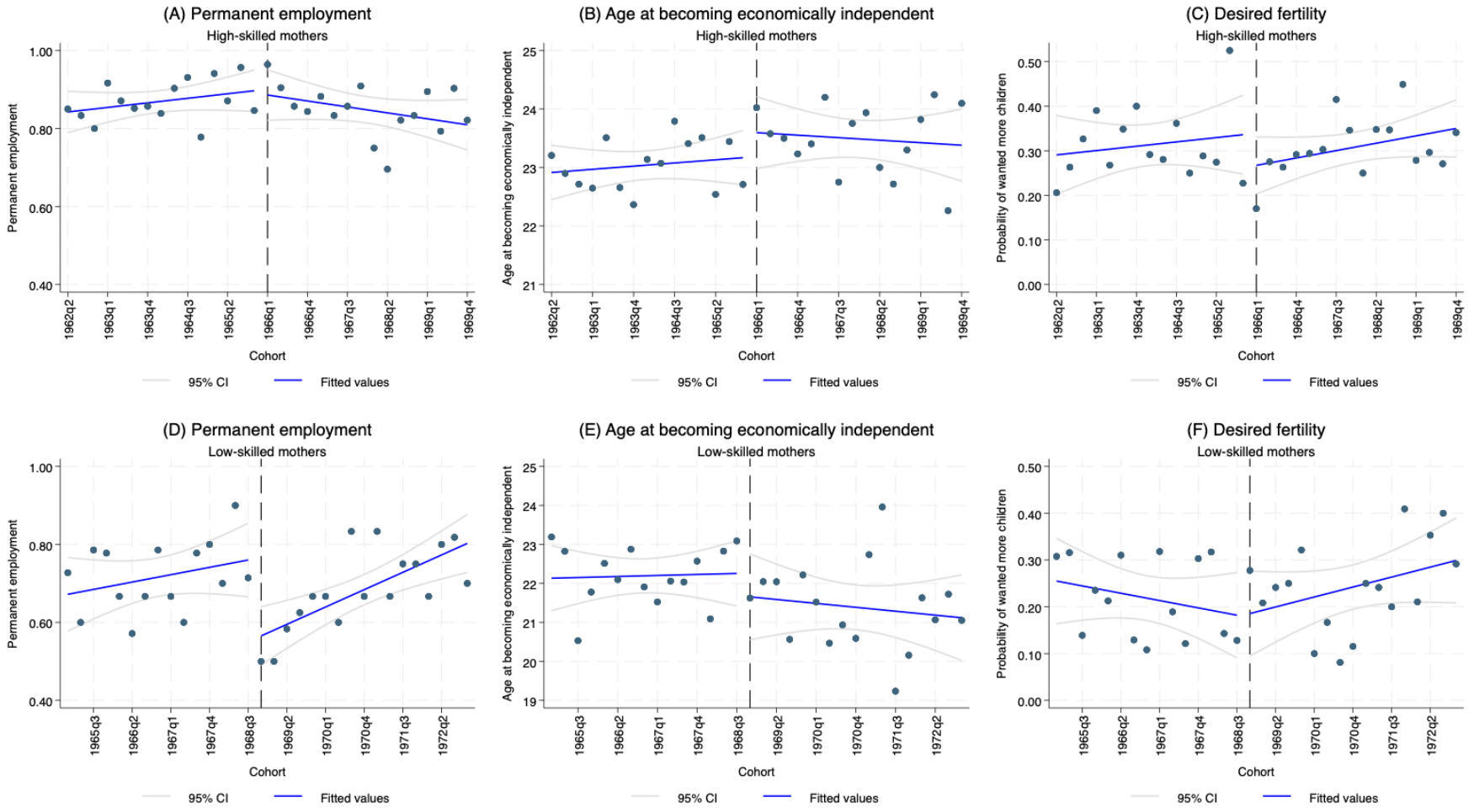
Figure 4: Mean of mother's age and number of children by mother's cohort and skill level



Notes: This figure shows the evolution and trends of the main fertility outcomes for low-skilled and high-skilled mothers. We use a sample of mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22). *Source:* 1999 and 2018 Spanish Fertility Survey.

Figure 5 shows that low-skilled women born in cohorts affected by the reform at labor market entry have an apparent decrease in their probability of having a permanent contract in the long run, while high-skilled mothers do not seem to be more exposed to temporary employment after the reform. We also observed upward trends in both groups regarding their desired fertility, meaning they wanted to have more children than they actually did. Additionally, they became economically independent from their parents at older ages. Surprisingly, we did not observe any changes in the probability of becoming financially independent from their parents for either group of mothers before or after the 1984 labor market reform. In fact, low-skilled mothers seem to show a downward trend in becoming economically independent at younger ages.

Figure 5: Mean of the main socioeconomic characteristics by mother's cohort and skill level



Notes: Panel (A) and (D) show the probability of having a permanent employment for high-skilled and low-skilled mothers, respectively. Panel (B) and (E) show the mean age at becoming economically independent from their parents for high-skilled and low-skilled mothers, respectively. Panel (C) and (F) show the probability of achieving the desired fertility for high-skilled and low-skilled mothers, respectively. *Source:* 2018 Spanish Fertility Survey.

5. EMPIRICAL STRATEGY

We estimate a regression discontinuity design difference-in-difference (RDD-DD) model to estimate the causal effects of temporary employment on fertility and infant health, exploiting the exogenous shock of the 1984 labor market reform on the availability of temporary contracts as a quasi-experimental setting. A key explanatory variable is the quarter of the mother's birth cohort, since this determines whether individuals enter the labor market before or after the liberalization of temporary employment.

For low-skilled women, we include the cohorts born in the first quarter of 1965 and the fourth quarter of 1972 and consider the cohort that turns 16 in the third quarter of 1984 (those born in the third quarter of 1968) as the first affected cohort, as they enter the labor market with the new legislation in place. For high-skilled women, we include cohorts of women born between 1962 and 1969, and the first cohort affected by the reform is the one born in the first quarter of 1966, as they will turn 18 years old (and will graduate from high school) in the summer of 1984 at the earliest. It has to be noted that the cohorts that we define as untreated are also affected by the reform, some quarters after entering the labor market. We argue that the impact of the reform on these cohorts is very small because the sensitivity of labor market outcomes to the availability of fixed-term contracts is by far the greatest at labor market entry. We have several reasons in support of this argument. First, the assumption that labor market outcomes are especially affected by conditions at entry is strongly supported by prior literature (Kahn 2010; Oreopoulos et al. 2012b). Second, for the Spanish case, we have seen that there is a very large change in the use of fixed-term contracts and apprenticeship contracts from the quarter before to the quarter after the introduction of the reform (as shown in García-Pérez et al. (2018)).[23]

We collapse the individual data at the level of mothers' quarters of birth cohort (c), province (p), and skilled level (s), as in Oreopoulos et al. (2012b) and García-Pérez et al. (2018).[24] This collapse not only enables us to quantify the number of births but also permits the inclusion of additional controls: fixed effects for different trends by skill level to account for macroeconomic factors, and province fixed effects to address disparities within the regional labor markets. The data are collapsed to obtain the number of births by the mother's cohort, as we cannot observe the total number of babies per woman in the administrative record.

Our estimated regression discontinuity design difference-in-difference (RDD-DD) model is as follows:

[23] Figure B1 shows the number of apprenticeship contracts relative to employees in non-agricultural private sectors. Following the November 1984 labor market reform, which liberalized fixed-term contracts, the share of apprenticeship (temporary) contracts rose sharply, with the increase concentrated among young workers—especially those aged 16–19.

[24] Our estimates using the Fertility Surveys are at the individual level and are quantitatively similar to those from the administrative records, as well as when including more cohorts closer to the cutoff.

$$y_{cps} = \alpha + \beta_1\big(BirthTrim_c - C_s\big) + \beta_2\big(BirthTrim_c - C_s\big)Post_{cs} + \beta_3 Post_{cs} + \beta_4 Treated_s + \beta_5\big(Treated_s \times Post_{cs}\big) + \eta_p + \delta_c + \varepsilon_{cps}\,. \tag{1}$$

where our outcome variables (y_{cps}) are measured at the mother's quarter-cohort of birth (c), province p, and skill group s. C_s is the skill-specific cutoff cohort quarter, i.e., low-skilled mothers' quarter of birth cut-off is the cohort born in the last quarter of 1968 (1968Q4), while high-skilled mothers have a cut-off defined for the cohort born in the first quarter of 1966 (1966Q1). The term $(BirthTrim_c - C_s)$ is a linear cohort trend calculated as the birth year cut-off (C_s) subtracted from the cohort's mother of birth quarter.[25] This trend is then interacted with the reform indicator $Post_{cs}$ to allow for a change in trend for the post-reform cohorts. $Post_{cs}$ is a post-reform indicator that is also specific to each skill level (when aged 16 for low-skilled and aged 18 for high-skilled occurs at/after the reform). $Treated_s$ equals 1 for low-skilled and 0 for high-skilled mothers. We include province fixed effects η_p.[26] The error term is ε_{cps}. We report robust standard errors clustered by mother's cohort.[27] Our parameter of interest is β_5, which captures the post-reform differential change among low-skilled mothers relative to high-skilled mothers at the discontinuity.

There are two elements worth mentioning concerning our identification strategy. First, the labor market reform could have affected not only the type of job (temporary/permanent contract) but also the probability of getting a job, which, in turn, could impact fertility decisions and infant health. We will test for that channel and show that, in our data, there are no differences in the probability of having a job due to the reform. Second, we can rule out any anticipated effects of the reform as it was processed on a high-speed track, being approved in August 1984 and being fully implemented in October 1984 (Bentolila et al. 2008). Yet, we have also included an event study design to test for pre-trends and examine the time-differential treatment effects of the reform. The common event study model (Miller 2023) that represents our setting is as follows:

$$y_{it} = \gamma_t + \delta_r + \sum_{m=-15}^{M} \beta_m\, d_{i,t-m} + \varepsilon_{i,t} \tag{2}$$

where γ_t are cohort-time fixed effects, δ_r are province fixed effects. [28]. $d_{i,t-m}$ indicates exposure m quarters relative to the reform. The coefficients $\{\beta_m\}_{m=-15}^{M}$ trace dynamic impacts within a common quarterly window, with $m < 0$ serving as placebo (pre-trend) periods and $m \geq 0$ as post-reform effects.

[25] This is the variable named "trend" in the tables of results.

[26] In the case of the Fertility Surveys, we only have information on the regions where the mother lives. There are 19 regions or Autonomous communities in Spain, while there are 52 provinces.

[27] Clustering at the cohort level is conservative with respect to serial correlation within cohort cells across provinces.

[28] When using survey waves (e.g., 1999, 2018 Spanish Fertility Surveys)

6. RESULTS

6.1. *Fertility*

Table 2 presents the main results on how temporary employment affects fertility, based on our two datasets: Panel (A) for birth records and Panel (B) for the 1999-2018 Fertility Surveys. We focus on two primary fertility outcomes: the number of births and the mother's age at birth. Regarding the birth records, the outcomes include: (i) the natural logarithm of births per quarter of the mother's birth cohort; (ii) the share of births by birth cohort quarter; (iii) the share of births to mothers aged 30 at the time of birth; and (iv) the mother's age. In contrast, the fertility surveys provide data on the average total number of children per woman (including those who have their first child at age 30), as well as the mother's age at each child's birth.

We obtain consistent evidence across both data sources and outcome definitions—whether fertility is measured at the cohort level (birth registers) or at the individual level (fertility surveys). The estimates indicate that the 1984 reform causally reduced fertility among low-skilled women who entered the labor market under the liberalized fixed-term regime, relative to their high-skilled counterparts who were less exposed. Using the Fertility Survey, which captures completed fertility at the individual level, we estimate that exposure to the reform lowered the average number of children per woman by approximately 5.3 percentage points. The effect is larger among low-educated women who postponed motherhood beyond age 30, for whom completed fertility falls by about 10.1 percentage points relative to comparable highly educated women.

We do not find any statistically significant effect of the reform on the age at first birth, as shown in Panel B, columns (i) and (v). This suggests that the timing of entry into motherhood remained largely unchanged for low-educated mothers exposed to the liberalized fixed-term regime. However, when using the universe of birth records—which provide information on mothers' age at any birth—we detect a mild upward trend among low-skilled women affected by the reform, indicating a gradual postponement of childbearing relative to their high-skilled counterparts. In terms of magnitude, the average maternal age at birth among exposed low-skilled mothers increased by approximately 1 percentage point, a small but directionally consistent effect with delayed fertility.

6.2. *Infant health*

We next turn to study the impact of temporary employment on infant health outcomes in Table 3. Using the universe of births from the birth records data, the estimated effects of exposure are small, statistically indistinguishable from zero for the perinatal health outcomes, and show no clear directional pattern

Table 2: Effects of temporary employment on fertility (RDD–DD)

PANEL A: Birth Records (1984–2018)				
	(i) **Log number of births per mother cohort**†	*(ii)* **Cohort of births rate by mother cohort**‡	*(iii)* **Cohort births rate to mothers aged 30+**	*(iv)* **Mother's age**
After × treat	−0.278***	−0.167***	−0.238***	0.286***
	(0.018)	(0.009)	(0.019)	(0.052)
Mean dep. var.	6.317	0.616	0.410	29.732
Mean dep. var. (treat= 1)	6.614	0.884	0.721	27.960
Treat	✓	✓	✓	✓
Trend	✓	✓	✓	✓
After	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
R^2	0.939	0.729	0.651	0.951
Observations	3,328	3,200	3,200	3,328

PANEL B: Fertility Survey (1999/2018)					
	(i) **Avg. # of children per woman**	*(ii)* **Avg. # children when first child ≥ 30:** *First child*§	*(iii)* **Avg. # children when any child ≥ 30:** *Any child*	*(iv.1)* **Mother's age at first child**	*(iv.2)* **Mother's age**
After × treat	−0.093**	−0.159***	−0.149**	−0.200	−0.493
	(0.042)	(0.044)	(0.056)	(0.337)	(0.306)
Mean dep. var.	1.762	1.574	0.159	27.009	28.601
Mean dep. var. (treat= 1)	1.777	1.531	0.096	24.939	26.503
Trend	✓	✓	✓	✓	✓
After	✓	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
Year Survey FE	✓	✓	✓	✓	✓
R^2	0.062	0.090	0.072	0.189	0.267
Observations	3,733	3,732	7,193	3,733	7,199

Panel (A) uses collapsed data at quarter-cohort of mother's birth, province, skill level. Robust standard errors clustered at the mother's cohort of birth level in parenthesis. *Outcome variable is the natural logarithm of the total number of children by a quarter cohort of mothers' birth when using the birth records dataset.‡Share of total births to the cohort of mothers' birth, province and skilled level divided by the total births occurring in all births that occurred in the cohort year of mother birth by province and skilled level. §Interaction term between treatment group, post, and having the (first) child at or after age of 30.No data for the number of births from 1962 to 1972 in Ceuta and Melilla provinces. Significance levels: ***p<0.01; **p<0.05; *p<0.1. *Source:* Childbirth Statistics Bulletin (1984–2018) provided by the Spanish National Statistics Office (INE) and 1999/2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22).

across margins. By contrast, restricting to mothers aged ≥30—who face a higher baseline risk of complications—yields precisely estimated but economically modest adverse effects: birthweight falls by about 3.6 g (≈0.1% of the mean), the probability of low birthweight rises by 0.2 percentage points (≈3–4% of the subgroup mean), and preterm birth increases by 0.4 percentage points (≈6% of the subgroup mean). The

decline in gestational age is 0.028 weeks (≈0.20 days), and the decline in high birthweight is 0.7 percentage points; taken together, these magnitudes are very small relative to baseline levels and should be interpreted as of limited practical significance, even when statistically significant.

Table 3: Effects of temporary employment on perinatal outcomes at birth (RDD–DD) using Birth Records

	Weight at birth (grams)	**Low birth weight <2.5kg)‡**	**High birth weight >4 kg**	**Gestational age (weeks)**	**Preterm <37 weeks ‡**
PANEL (A): All universe of births					
After × treat	1.843	−0.000	−0.010***	0.054***	−0.003***
	(1.382)	(0.000)	(0.000)	(0.005)	(0.000)
Mean dep. var.	3,231.65	0.058	0.114	39.031	0.067
Mean dep. var. (treat= 1)	3,220.93	0.059	0.134	38.995	0.070
R^2	0.757	0.529	0.860	0.593	0.691
Observations	3,328	3,328	3,328	3,328	3,328
PANEL (B): All births by mother age					
After × treat	−3.636**	0.002***	−0.007***	−0.028***	0.004***
	(1.525)	(0.000)	(0.002)	(0.010)	(0.001)
Mean dep. var.	3,232.60	0.059	0.091	39.058	0.065
Mean dep. var. (treat= 1)	3,183.10	0.071	0.073	39.012	0.067
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
After	✓	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓
R^2	0.759	0.535	0.864	0.598	0.707
Observations	3,328	3,328	3,328	3,328	3,328

Notes: Data is collapsed at the quarter-cohort level of the mother's birth, by province, and by skill level. Robust standard errors clustered at the mother's cohort of birth level in parentheses. ‡ Dummy indicator. Significance levels: ***p<0.01; **p<0.05; *p<0.1. Source: Childbirth Statistics Bulletin (1984–2018) provided by the Spanish National Statistics Office (INE). Sample of native mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22).

6.3. *Analyses of the mechanisms*

This section explores potential mechanisms underlying the effects of temporary employment on fertility and infant health outcomes. Table 4 presents the results concerning impacts on socioeconomic characteristics, whereas Table 5 covers outcomes that reflect social and cultural attitudes.

To assess whether changes in household circumstances help explain our main effects, we start with the socio-economic channels in Table 4. The DiD-RDD estimates show little evidence that exposure to the reform translated into large, systematic changes in core household circumstances. Coefficients on being married and homeownership are small and imprecisely estimated, as are financial strain (difficulty making

ends meet) and income below the poverty line. By contrast, conditional on being employed, exposure raises the probability of holding a temporary (non-permanent) job by about 23 percentage points. (0.231, s.e. 0.113, p<0.05), confirming that the reform meaningfully shifted treated women into less secure contracts. Relative to the treated-group mean of 0.299, this is around 77% increase. Given Spain's well-documented low transition rates from temporary to permanent contracts and the surge in temporary hiring triggered by the 1984 liberalization, the result points to a persistent deterioration in job stability for the affected cohorts (see, e.g., Güell & Petrongolo (2007); García-Pérez et al. (2019); de Graaf-Zijl et al. (2011)). In this context, weaker attachment to stable jobs is a plausible barrier to realizing planned births—particularly when combined with age-related biological constraints documented in our age-30-plus results.

Table 4: Mechanisms driving the relationship between temporary employment and social and cultural attitudes (RDD–DD) using the Fertility Survey 2018

	(a) **Married**	*(b)* **Home ownership**	*(c)* **Make ends meet**	*(d)* **Temporary employment***	*(e)* **Monthly income below poverty**
After × treat	−0.013	−0.061	−0.098	0.231**	0.113
	(0.080)	(0.076)	(0.119)	(0.113)	(0.083)
Mean dep. var.	0.783	0.882	0.473	0.180	0.399
Mean dep. var. (treat= 1)	0.776	0.846	0.627	0.299	0.161
Post	✓	✓	✓	✓	✓
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
Post × Trend	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
Birth Year FE	✓	✓	✓	✓	✓
R^2	0.030	0.025	0.103	0.057	0.163
Observations	2,422	2,422	2,422	2,232	2,422

Notes: Robust standard errors clustered at the cohort level in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.1. *Source:* 2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22). ΔTemporary employment is conditional on being employed.

We next consider whether preference- or norm-based mechanisms are at work, turning to Table 5 on social and cultural attitudes. Most outcomes show limited movement, suggesting the reform did not materially shift preferences or support structures captured in these measures. We find no detectable changes in informal childcare availability, egalitarian views, or religious observance, and only a non-statistically significant decline in age at economic independence. The one notable change is a higher probability of reporting desired additional children (+0.192, s.e. 0.108, p<0.10). The evidence indicates that preferences are not becoming

less family-oriented; if anything, affected women want more children but face tighter employment constraints in the long run - pointing to unmet fertility rather than altered preferences.

Table 5: Mechanisms driving the relationship between temporary employment and socio-demographic characteristics (RDD–DD) using Fertility Survey 2018

	(a) **Age at becoming economically independent‡**	*(b)* **No informal childcare (0–3 yrs)†**	*(c)* **Desired fertility§**	*(d)* **Egalitarian attitudes****	*(e)* **Declining religious observance¶**
After × treat	−1.402	0.000	0.192*	0.010	−0.011
	(1.051)	(0.059)	(0.108)	(0.027)	(0.070)
Mean dep. var.	22.83	0.056	0.277	0.736	0.241
Mean dep. var. (treat= 1)	21.91	0.070	0.225	0.704	0.198
Post	✓	✓	✓	✓	✓
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
Post × Trend	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
R^2	0.053	0.045	0.035	0.074	0.045
Observations	2,127	2,422	2,422	2,422	2,422

Notes: ‡Age at which the respondent becomes financially independent from their parents. †Indicator for "No informal childcare (0–3 years)". §Would or would like to have more children. **See Appendix D for more information on the composition of the Egalitarian Index. Robust standard errors clustered at the cohort level in parentheses. ¶ Dummy indicator of non-religious affiliation, Significance levels: $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$. *Source:* 2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972) or high-skilled (born between 1962 and 1969).

The pattern suggests a labor-market channel with more temporary employment, rather than sudden changes in household formation or cultural attitudes.

7. ROBUSTNESS CHECKS

We conduct several robustness checks on the exogeneity of our quasi-experimental setting and also examine whether the fertility effects of entering the labor market under more lenient regulation on temporary contracts are stronger for specific cohorts of mothers.

7.1. *Ruling out confounding shocks*

(i) Changes to decision-making in education—A potential concern is that the 1984 labor-market reform, by altering early-career prospects, might also have affected educational decisions, thereby confounding our estimates. In particular, the liberalization of fixed-term contracts could have discouraged further study

among low-educated women if short-term jobs became more accessible, or conversely, encouraged continued schooling if stable employment opportunities diminished. To test this, we examine whether the age at leaving education differed between the treated and comparison groups. The data indicate that, although the reform substantially increased temporary employment, it did not produce systematic shifts in schooling duration across cohorts. As shown in Table 6, column (d), the estimated effect of exposure to the reform on the age of completing studies for low-skilled women is small and statistically insignificant. This suggests that the reform did not induce meaningful changes in educational attainment and that our fertility estimates are unlikely to be driven by endogenous schooling responses.

Table 6: *Robustness Checks I.* Effects of 1984 labor market reform on mothers' labor, education and childcare policy outcomes (RDD–DD)

	(a) **Having a paid job**	*(b)* **Long-term unemployment***	*(c)* **Age at the first job**	*(d)* **Age at ending studies**	*(e)* **Suffering disabilities**	*(f)* **Flexible childcare at work†**
After × treat	0.056	0.059	−0.433	−2.156	0.048	0.029
	(0.100)	(0.108)	(1.878)	(1.617)	(0.031)	(0.078)
Mean dep. var.	0.647	0.616	22.88	22.487	0.079	0.078
Mean dep. var. (treat= 1)	0.491	0.690	21.95	19.701	0.077	0.468
Post	✓	✓	✓	✓	✓	✓
Treat	✓	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓	✓
Post × Trend	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
R^2	0.045	0.023	0.058	0.080	0.018	0.042
Observations	2,439	2,422	2,422	2,422	2,422	2,422

Notes: *Long-term unemployment is defined as being unemployed for at least 6 months. †Flexible childcare options at work are defined as the employer offering any flexible childcare options at work. "No form of childcare (0–3 yrs)" in other tables is defined as "No alternatives for the care of children from 0 to 3 years old". Robust standard errors clustered at the mother's cohort level in parentheses. Significance levels: $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$. *Source:* 2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972) or high-skilled (born between 1962 and 1969).

(ii) Changes in labor market conditions —A second concern is that our estimated effects might capture broader changes in the composition or conditions of employment, rather than the causal impact of temporary-contract exposure per se. To address this, we test whether the 1984 reform altered key dimensions of the labor market beyond contract type—such as overall employment participation, wage levels, part-time work, or financial strain. Specifically, we examine whether the reform affected (i) the probability of being unemployed, (ii) the likelihood of reporting financial hardship, and (iii) indicators of employment entry and

duration. As shown in Table 6, columns (a)–(c), we find no statistically significant effects of the reform on the extensive margin of employment: the probability of being unemployed or reporting difficulties making ends meet remains unchanged for low-skilled women entering the labor market under the liberalized regime relative to their high-skilled counterparts. This evidence suggests that the reform primarily affected the type of contract—temporary versus permanent—rather than overall employment opportunities or participation.

While our data do not allow us to track women's complete employment trajectories, external evidence from the Spanish Labor Force Survey corroborates the persistence of segmentation. Using simulated employment histories for 1987–1996, Toharia (1997) finds that only about 14% of temporary workers transition annually into permanent employment, implying that roughly three-quarters remain in precarious positions year to year. Similarly, Revenga (1994) documents that the reform entrenched a dual structure in which nearly one-fifth of the workforce (18.4%) spent over two decades on temporary contracts. These findings are consistent with our descriptive evidence (see Figure E1, in Appendix E), which shows that the reform did not alter the overall employment rate for low- versus high-skilled mothers but substantially widened the gap in contract stability.

Finally, prior studies demonstrate that the 1984 reform had minimal impact on other forms of work, such as part-time employment or student employment among those aged 15–29. However, it significantly affected wage formation and career earnings trajectories. In particular, Jimeno and Toharia (1992b) estimate that permanent workers earned 8–11% more than temporary workers after the reform, while Bentolila et al. (1994) and Jimeno et al. (1993) attribute this to increased bargaining power of permanent employees in a segmented market. More recent evidence by García-Pérez et al. (2019) confirms long-term scarring effects, with low-skilled workers experiencing 9.8% lower earnings in the first decade of employment and a persistent 7.3% annual wage penalty over 27 years. Together, these results reinforce our interpretation that the 1984 reform primarily increased employment duality, without affecting total employment, and that its fertility effects reflect exposure to long-term job instability rather than aggregate labour-market shocks.

(iii) Changes in family policies by public and private sectors—A final concern is that our estimates might reflect changes in family policy or political orientation, rather than the effects of the 1984 labor-market reform itself. To address this, we examine whether government family policies or employer-provided childcare benefits evolved differentially for low- and high-skilled mothers during the relevant period. Political control was stable throughout our study window: the Spanish Socialist Workers' Party (PSOE) held office continuously from 1982 to 1996, maintaining a consistent social-democratic orientation in family and labor policy. Major family-related reforms were implemented well after the 1984 labor-market liberalization. For instance, maternity leave of six weeks at full pay was introduced in 1989, paternal leave of two paid days

was added in 1999, and a 13-day compensated paternity leave followed in 2007 (see Appendix Table A1). None of these measures overlap temporally with our treatment cohorts, nor do they vary by skill group.

To empirically verify that contemporaneous family policy changes did not bias our estimates, we test whether employer-provided childcare or related family supports differed between the treatment and control groups. As reported in Table 6, column (f), we find no statistically significant differences in access to childcare offers by employer between low- and high-skilled mothers exposed to the reform. This evidence suggests that our estimated fertility effects are unlikely to be confounded by differential exposure to family policy changes or political ideology shifts during the period under analysis.

7.2. *Sample construction and treatment definition*

Our main specification distinguishes between low- and high-skilled mothers, but the underlying definition of skill necessarily differs across datasets. To assess the sensitivity of our results to these definitions, we conduct two complementary checks.

First, we verify that including mothers still in education in the control group of the Birth Records sample does not alter the results. As shown in Panel A of Table 7, the estimated coefficients remain virtually identical to the baseline, confirming that this classification choice is immaterial to our findings. Second, we test whether our results depend on whether skill is defined by occupation or education. In the Birth Records, occupation at the time of birth is the only available indicator of skill, as educational information is missing before 2007. In contrast, in the Fertility Survey, occupation status suffers from substantial non-response, so our baseline definition relies on education—classifying as low-skilled those without a high-school diploma.[29] To evaluate the robustness of our findings, we re-estimate the model using occupational status as the treatment definition in the Fertility Survey. This reduces the analytical sample by 35.26% owing to missing occupation data (see Appendix C, Table C2 for details).

The results, reported in Table 7, Panel B, show that the estimated coefficients remain highly consistent with the baseline. The estimated effect of exposure to the 1984 reform on the average number of children per woman is virtually unchanged, and the magnitude of the reduction for women postponing motherhood beyond age 30 is statistically indistinguishable from the main specification. These findings confirm that our results are not sensitive to the specific definition of treatment or to sample composition.

[29] For additional details on the treatment definition, please refer to Table A3 in Appendix, which contains all the relevant information.

Table 7: Robustness Checks II. Sample definition (RDD–DD)

PANEL (A): Birth Records: *including mothers in education to the control group*				
	Log number of births by mother's cohort†	**Cohort of births rate by mother's cohort**‡	**Cohort of births rate to mothers aged 30+**	**Mother's age**
After × treat	−0.276***	−0.139***	−0.005***	0.366***
	(0.018)	(0.007)	(0.000)	(0.049)
Mean dep. var.	7.442	0.678	0.506	29.237
Mean control group, pre	7.367	0.497	0.497	30.961
Men treatment group, pre	7.573	0.873	0.694	27.403
Treat	✓	✓	✓	✓
Trend	✓	✓	✓	✓
After	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
R^2	0.940	0.629	0.632	0.953
Observations	3,328	3,200	3,200	3,328

PANEL (B): Fertility Survey: *using current occupation information (only 2018 FS and contains 35.26% missing data)*					
	Avg. # of children per woman	**Avg. # children when first child ≥ 30:** *First child*§	**Avg. # children when any child ≥ 30:** *Any child*	**Mother's age at first child**	**Mother's age**
After × treat	−0.045	−0.265***	−0.105*	0.662	0.565
	(0.070)	(0.058)	(0.059)	(0.590)	(0.523)
Mean dep. var.	1.842	1.653	2.129	28.165	30.020
Mean control group, pre	1.808	1.686	2.069	30.113	31.951
Men treatment group, pre	1.832	1.529	2.094	26.850	28.759
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
After	✓	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
R^2	0.033	0.039	0.105	0.105	0.087
Observations	1,568	1,568	3,112	1,568	3,112

Notes: Panel (A) uses collapsed data at quarter-cohort of mother's birth, province, skill level. Robust standard errors clustered at the mother's cohort of birth level in parentheses. *Outcome variable is the natural logarithm of the total number of children by a quarter cohort of mothers' birth when using the birth records dataset. ‡ Share of total births to the cohort of mothers' birth, province and skilled level divided by the total births occurring in all births that occurred in the cohort year of mother birth by province and skilled level. § Interaction term between treatment group, post, and having the (first) child at or after age 30. No data for the number of births from 1962 to 1972 in Ceuta and Melilla provinces. Significance levels: ***p<0.01; **p<0.05; *p<0.1. *Source:* Childbirth Statistics Bulletin (1984–2018) provided by the Spanish National Statistics Office (INE) and 1999/2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22).

7.3. *Event Study Dynamics*

To complement the RDD–DD estimates, we plot an event study that interacts treatment status (low-skill × post) with entry-cohort indicators and normalizes to the last pre-reform cohort. This specification

allows us to trace the path of outcomes for treated and comparison groups around 1984 and visually assess whether parallel trends hold before the policy change. Across Figures E2–E7 in Appendix E, the pre-reform coefficients cluster around zero with wide, overlapping confidence bands, consistent with the absence of systematic differential trends prior to liberalization. The main nuance appears in Figure E2, the share of births by the mother's cohort using Birth Records, where the pre-reform coefficients show a gentle downward slope. This pattern is likely mechanical, reflecting composition effects in the denominator (cohort size), which is based on births by province and quarter and does not adjust for subsequent migration or mortality.

Turning to post-reform dynamics, the fertility coefficients become negative and remain persistently below zero for several cohorts (Figures E2–E4), indicating a sustained reduction in completed fertility among low-skilled women entering the labor market after the 1984 liberalization. The decline unfolds gradually, suggesting that the reform mainly affected progression to higher-order births rather than the timing of first births. For maternal-age outcomes (Figures E3, E5, and E6), we find a modest upward shift in the average age at childbirth, roughly one percentage point, which is small in magnitude but directionally consistent with mild postponement. Taken together, these patterns reinforce our identifying assumptions and provide a transparent picture of the timing and persistence of the effects.

7.4. *Continuous Exposure to the Reform*

As a further test of robustness, we exploit a continuous measure of exposure intensity to the 1984 labor-market reform. Rather than a binary post-reform indicator, we construct an exposure variable that increases with proximity to the reform date, capturing the gradual rise in treatment intensity among cohorts entering the labor market under progressively more liberalized conditions. Specifically, the variable takes its lowest value (Q1) for cohorts least affected—those entering furthest before the reform—and its highest value (Q16) for those entering entirely under the post-reform regime. Because entry age differs by skill, the exposure cutoff is set to the first quarter of 1966 for high-skilled women (entry at age 18) and the last quarter of 1968 for low-skilled women (entry at age 16).

The results, presented in Table 8,[30] confirm that the magnitude of the reform's impact increases systematically with exposure intensity. In the Birth Records (Panel A), coefficients become progressively larger in absolute value as exposure rises for cohorts only marginally affected (Q1–Q4), the effect on fertility is negative but modest, whereas for the most exposed cohorts (Q13–Q16) the decline in the number and share of births is substantially stronger and highly significant. The same monotonic pattern appears across outcomes, with a corresponding upward shift in maternal age, consistent with delayed childbearing among the

[30] Appendix F, Figure E3 and Figure E4, shows a visualization of the continuous effects of the reform.

Table 8: Robustness Checks III. Continuous exposure to the reform (After: Quarter 1–16) (RDD–DD)

	Log number of births by mother's cohort†	**Cohort of births rate by mother's cohort**‡	**Cohort of births rate to mothers aged 30+**	**Mother's age**
PANEL (A): Birth Records				
Treat × Q1–Q4	−0.368***	−0.196**	−0.111**	0.703***
	(0.025)	(0.055)	(0.055)	(0.044)
Treat × Q5–Q8	−0.307***	−0.163***	−0.315*	0.650**
	(0.022)	(0.008)	(0.067)	(0.036)
Treat × Q9–Q12	−0.262***	−0.119***	−0.102	0.389**
	(0.027)	(0.008)	(0.071)	(0.046)
Treat × Q13–Q16	−0.180***	−0.083***	−0.042	0.027
	(0.024)	(0.008)	(0.059)	(0.067)
Treat	✓	✓	✓	✓
Trend	✓	✓	✓	✓
After	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
R^2	0.941	0.633	0.320	0.957
Observations	3,328	3,200	3,200	3,328

	Avg. # of children per woman	**Avg. # children when first child ≥ 30:** *First child*§	**Avg. # children when any child ≥ 30:** *Any child*	**Mother's age at first child**	**Mother's age**
PANEL (B): Fertility Survey					
Treat × Q1–Q4	−0.173*	0.159*	−0.163	−0.334	−1.146*
	(0.073)	(0.075)	(0.101)	(0.498)	(0.463)
Treat × Q5–Q8	−0.144**	0.286**	0.167	0.455	0.307
	(0.052)	(0.091)	(0.167)	(0.455)	(0.307)
Treat × Q9–Q12	0.111**	0.365**	0.436***	−0.839	−0.711
	(0.041)	(0.115)	(0.090)	(0.691)	(0.524)
Treat × Q13–Q16	−0.127*	0.083	−0.013	−0.138	−0.761*
	(0.067)	(0.078)	(0.130)	(0.338)	(0.290)
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
After	✓	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
R^2	0.038	0.045	0.046	0.143	0.130
Observations	3,733	3,733	7,193	3,733	7,193

Notes: Q1 corresponds to cohorts least exposed (those entering the labor market well before the reform) and Q16 to those most affected (entering fully under the liberalized regime). Panel (A) uses collapsed data at quarter-cohort of mother's birth, province, skill level. Robust standard errors clustered at the mother's cohort of birth level in parentheses. *Outcome variable is the natural logarithm of the total number of children by a quarter cohort of mothers' birth when using the birth records dataset. ‡ Share of total births to the cohort of mothers' birth, province and skilled level divided by the total births occurring in all births that occurred in the cohort year of mother birth by province and skilled level. § Interaction term between treatment group, post, and having the (first) child at or after age 30. No data for births from 1962 to 1972 in Ceuta and Melilla provinces. Significance levels: ***p<0.01; **p<0.05; *p<0.1. *Source:* Childbirth Statistics Bulletin (1984–2018) provided by the Spanish National Statistics Office (INE) and 1999/2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22).

most affected women. Results from the Fertility Survey (Panel B) yield a similar picture. The negative association between exposure intensity and completed fertility is strongest for the highest-exposure cohorts, while changes in maternal age remain small but directionally positive. Taken together, these findings show that the reform's effects grew stronger as the temporary-contract system became fully established in the labor market.

Overall, the continuous-exposure specification supports the main conclusions: cohorts of low-skilled women who entered the labor market under greater exposure to temporary-contract liberalization experienced larger reductions in fertility. While the bin-by-bin estimates are not strictly monotonic across outcomes, the exposure gradient is directionally consistent with differential treatment by entry timing and, together with the clear post-reform discontinuity in our baseline RDD–DD, supports the validity of our specification. Taken together, these patterns are consistent with a causal, intensity-dependent response to the 1984 reform rather than compositional or period-specific shocks.

7.5. *Polynomial trends and bandwidth*

We assess the sensitivity of our estimates to functional-form assumptions and the choice of estimation window by adding higher-order cohort trends and varying the bandwidth around the 1984 cutoff. We first allow for more flexible cohort trends by augmenting the RDD–DD specification with second- and third-degree polynomials in the running variable, fully interacted with skill and post (Table 8). The baseline negative effect on fertility is unchanged: in Birth Records, the After×Treat coefficient remains large and precisely estimated for the log number of births and the cohort share of births, and we continue to find a small positive shift in mothers' age (Panel A). In the Fertility Survey, the estimated reduction in completed fertility and the rise in the share having a (first) child at $\geq$30 persist with similar magnitudes (Panel B). Overall, the results are robust to higher-order trend flexibilities, indicating that our findings are not an artefact of linear trend restrictions.

Table 9: Robustness Checks IV. Controlling for second and third polynomial trend (RDD–DD)

PANEL (A): Birth Records				
	Log number of births by mother's cohort†	**Cohort of births rate by mother's cohort**‡	**Cohort of births rate to mothers aged 30+**	**Mother's age**
After × treat	−0.274***	−0.138***	−0.238***	0.364***
	(0.019)	(0.008)	(0.019)	(0.054)
Treat	✓	✓	✓	✓
Trend	✓	✓	✓	✓
Trend^2	✓	✓	✓	✓
Trend^3	✓	✓	✓	✓
After	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓
After × Trend^2	✓	✓	✓	✓
After × Trend^3	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
R^2	0.940	0.629	0.652	0.954
Observations	3,328	3,200	3,200	3,328
PANEL (B): Fertility Survey				
	Avg. # of children per woman	**Avg. # children when first child ≥ 30:** *First child*§	**Avg. # children when any child ≥ 30:** *Any child*	**Mother's age at first child**	**Mother's age**
After × treat	−0.103**	−0.278***	0.108*	−0.261	−0.604*
	(0.044)	(0.046)	(0.048)	(0.344)	(0.314)
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
Trend^2	✓	✓	✓	✓	✓
Trend^3	✓	✓	✓	✓	✓
After	✓	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓	✓
After × Trend^2	✓	✓	✓	✓	✓
After × Trend^3	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
R^2	0.034	0.042	0.040	0.142	0.129
Observations	3,733	3,733	7,193	3,733	7,193

Notes: Panel (A) uses collapsed data at quarter-cohort of mother's birth, province, skill level. Robust standard errors clustered at the mother's cohort of birth level in parentheses. *Outcome variable is the natural logarithm of the total number of children by a quarter cohort of mothers' birth when using the birth records dataset. ‡ Share of total births to the cohort of mothers' birth, province and skilled level divided by the total births occurring in all births that occurred in the cohort year of mother birth by province and skilled level. § Interaction term between treatment group, post, and having the (first) child at or after age 30. No data for the number of births from 1962 to 1972 in Ceuta and Melilla provinces. Significance levels: ***p<0.01; **p<0.05; *p<0.1. *Source:* Childbirth Statistics Bulletin (1984–2018) provided by the Spanish National Statistics Office (INE) and 1999/2018 Spanish Fertility Survey.

We then probe bandwidth sensitivity by changing the cohort window analyzed. Beyond the baseline window (±4 cohort-years; 16 quarters), we re-estimate using an expanded window that adds eight cohorts (1961–1973) for each skill group (Table 10). Across these alternatives, the After×Treat effects on fertility

Table 10: Robustness Checks V. Adding eight cohorts to the analysis for each group (1961–1973) (RDD–DD)

PANEL (A): Birth Records				
	Log number of births by mother's cohort†	**Cohort of births rate by mother's cohort**‡	**Cohort of births rate to mothers aged 30+**	**Mother's age**
After × treat	−0.285***	−0.133***	−0.237***	0.336***
	(0.017)	(0.008)	(0.015)	(0.049)
Mean dependent variable	7.352	0.707	0.510	29.65
Control group mean, pre	7.254	0.457	0.457	31.31
Treatment group, pre	7.476	0.937	0.920	27.74
Treat	✓	✓	✓	✓
Trend	✓	✓	✓	✓
After	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
R^2	0.941		0.738	0.940
Observations	3,744	3,200	3,200	3,744
PANEL (B): Fertility Survey				
	Avg. # of children per woman	**Avg. # children when first child ≥ 30:** *First child*§	**Avg. # children when any child ≥ 30:** *Any child*	**Mother's age at first child**	**Mother's age**
After × treat	−0.086**	−0.303***	0.080*	−0.495	−0.820**
	(0.038)	(0.039)	(0.039)	(0.343)	(0.332)
Mean dependent variable	1.764	1.583	1.977	27.052	28.556
Control group mean, pre	1.769	1.519	1.863	28.467	29.778
Treatment group, pre	1.824	1.461	1.975	28.467	26.477
Treat	✓	✓	✓	✓	✓
Trend	✓	✓	✓	✓	✓
After	✓	✓	✓	✓	✓
After × Trend	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
R^2	0.030	0.040	0.035	0.140	0.127
Observations	4,466	4,466	8,697	4,466	8,697

Notes: Panel (A) uses collapsed data at quarter-cohort of mother's birth, province, skill level. Robust standard errors clustered at the mother's cohort of birth level in parentheses. *Outcome variable is the natural logarithm of the total number of children by a quarter cohort of mothers' birth when using the birth records dataset. ‡ Share of total births to the cohort of mothers' birth, province and skilled level divided by the total births occurring in all births that occurred in the cohort year of mother birth by province and skilled level. § Interaction term between treatment group, post, and having the (first) child at or after age 30. No data for the number of births from 1962 to 1972 in Ceuta and Melilla provinces. Significance levels: ***p<0.01; **p<0.05; *p<0.1. *Source:* Childbirth Statistics Bulletin (1984–2018) provided by the Spanish National Statistics Office (INE) and 1999/2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1965 and 1972 and start working before age 20) or high-skilled (born between 1962 and 1969 and start working before age 22).

outcomes remain statistically significant and quantitatively close to the baseline, while the modest increase in maternal age is stable. These checks demonstrate that the estimated reform effects are stable to added trend flexibility and reasonable changes in the estimation window.

8. CONCLUSIONS

This paper provides new evidence on the impact of temporary employment on fertility decisions and infant health. To identify the causal effect, we employ a natural experiment exploiting the 1984 labor market reform in Spain, which significantly liberalized temporary employment. We compare cohorts of women entering the labor market at the time of the reform, conditional on their occupational or educational skill level. We argue that the 1984 labor reform altered the expected employment prospects of low-skilled women at entry, leading to fewer opportunities to obtain permanent contracts, even at older ages when they are most productive. We also provide evidence that this shock was exogenous and did not affect other institutional settings, such as education opportunities or childcare support. This setting allows us to compare the completed fertility of a cohort of women exposed to a broad liberalization of fixed-term contracts, while also examining any socio-economic, cultural, and attitudinal changes.

In particular, we utilize micro-level, high-quality administrative data to capture the universe of all births from 1984 to 2018. We also utilize two fertility-survey datasets from 1999 and 2018, both of which contain rich information on mothers' birth dates and their occupational and educational statuses. This allows us to construct our counterfactual group of high-skilled mothers entering the labor market at age 18 or onwards. Our RDD–DD estimates show that exposure to temporary employment at labor market entry reduces completed fertility among low-skilled women: the average number of children per woman falls by 5.3 percentage points, with a larger 10.1 percentage-point decline for those who postpone motherhood beyond age 30, relative to comparable high-skilled women. The timing of first birth is unchanged, but birth-register evidence points to a small postponement overall (maternal age at any birth). For infant health, effects in the full sample are small and imprecise; among mothers aged ≥ 30, we detect economically modest but precisely estimated adverse shifts: birthweight -3.6 g (around 0.1% of the mean), low birthweight $+0.2$ percentage points (around 3–4% of the subgroup mean), preterm birth $+0.4$ percentage points (around 6%), gestational age -0.028 weeks (around 0.20 days), and high birthweight -0.7 percentage points. Conversely, there is no evidence that the labor market reform consistently affects marriage, homeownership, financial stress, poverty, informal childcare, egalitarian attitudes, or religiosity. The observed change in age at economic independence is minimal and not statistical

We examine possible mechanisms behind the main findings. Consistent with previous studies on temporary contracts and career scarring (Blanchard & Landier 2002; D'Addio & Rosholm 2005; Kahn 2010; Oreopoulos

et al. 2012a; García-Pérez et al. 2018), exposure correlates with a higher likelihood of holding a temporary (non-permanent) job—+23 percentage points, approximately 77% relative to the treated group—indicating greater job insecurity. Concerning norms and cultural shifts, we find no evidence that the reform created disparities in changes between our two groups. However, fertility preferences tell a different story: exposed women are more likely to report wanting more children by the end of their fertility years (+0.192, p¡0.10). Overall, the evidence suggests that employment instability, rather than changing preferences, is the more likely mechanism behind delayed and reduced fertility. Thus, the decline in fertility reflects increased restrictions on childbearing due to employment insecurity (Clark & Lepinteur 2022), rather than a change in personal preferences.

Our empirical analysis has some limitations. First, as with any quasi-natural experiment, external validity is restricted: the effects calculated for Spain's institutional setting and may not apply to other countries, periods, or policy environments. Second, fertility is a household choice, but we lack data on partners' or cohabitants' labor-market histories; the reform could have simultaneously influenced their job security, working hours, or earnings, making it impossible to track within-couple adjustments or bargaining responses. Third, our data are not longitudinal at the individual employment spell level: we estimate exposure at entry and how they are impacted when they respond to the 2018 Fertility Survey, but cannot observe the timing, duration, or intensity of temporary contracts during the fertile years, transitions to permanent contracts, or cumulative insecurity. Our estimates are static, simplified averages, and do not reflect dynamic household decision-making processes, such as spacing, stopping, or catch-up fertility. Fourth, in administrative birth records, the denominator is imperfect for cohort-specific fertility rates due to a lack of complete data on births from 1962 to 1972, as official records began in 1980; therefore, we rely on other sources and make assumptions regarding their skilled status. The survey complements this, but is subject to sampling and self-reported shortcuts. Fifth, although we analyze several mechanism proxies—marriage, homeownership, financial strain, poverty, informal childcare, egalitarian views, religiosity—they may not fully capture preferences, support networks, or firm-side responses. Future research linking partners' trajectories and employer behavior with matched longitudinal data would better quantify household dynamics and the time path of exposure.

Our contribution is to provide clear, causal evidence on how a significant, economy-wide shift toward temporary employment affects family formation and early-life health. We use Spain's 1984 liberalization of fixed-term contracts as a quasi-experimental setting and combine nationwide birth registers with nationally representative fertility surveys to track both completed fertility and birth outcomes. By focusing on low-skilled labor market entrants, we highlight a group for whom job insecurity is especially acute and whose transitions from temporary to permanent contracts are historically weak. Compared to highly skilled women,

these women often postponed having children to work rather than pursue further education (Goldin 2025a).[31] Spain experienced rapid economic growth in the second half of the 20th century, yet upheld traditional beliefs about gender roles and family, which heighten the conflict between women's work and childcare responsibilities and result in particularly low fertility (Kearney & Levine 2025; Goldin 2025b). In this context, our within-cohort regression discontinuity design with difference-in-differences shows a reduction in completed fertility without changes in the timing of first births. Our results suggest that employment instability, rather than changing preferences or family desires, is the main mechanism. Methodologically, we add value by (i) aligning cohort-level and individual-level fertility measures, (ii) distinguishing timing from completed fertility, (iii) testing alternative explanations such as attitudes and norms—including marriage, homeownership, financial strain, poverty, informal childcare, egalitarian views, and religiosity—and (iv) measuring the extent of exposure to job instability. Future research incorporating partners' or cohabitants' labor market experiences could better clarify household fertility decisions.

Our findings have clear policy implications. By accurately showing how increased reliance on temporary employment influences fertility, we demonstrate that labor-market regulations can spill over into family outcomes. This highlights the importance of stabilizing early-career employment (for example, limiting repeated renewals of fixed-term contracts, making hours and income more predictable, and enabling timely shifts to permanent positions). At the same time, family policy tools that lower the costs of having children for working parents—such as affordable childcare, tax-benefit systems that avoid penalties for secondary earners, and well-designed parental leave—are complementary. Together, these strategies can encourage family formation while maintaining economic flexibility, especially for young, low-skilled workers who face the highest job insecurity.

[31] Figure F1 plots the cohort fertility by education in U.S. Cohorts Born 1917–1982. This figure shows that the approximate annualized long change (semi-elasticity with respect to time) was -2.02 for those women with no bachelor's degree (BA) or not four years of college or no college and -1.23 for those with more than a BA.

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APPENDIX

A. A MODEL OF MOTHERHOOD

We present a simple model of demand for children among low-skilled women given the shock of entering the labor market with a forecasted period of temporary employment.[32] Also, we include the potential mechanisms influencing these decisions, which are examined in this paper. Low-skilled women face a choice between employment and children, given the probability that they can reap rewards from staying in the labor force while raising their children. The model will highlight the need for women to keep focusing on their careers until they secure a permanent job, but this will only be credible if they readjust their demand for children to be lower, but not their timing.

We assume that women live for two periods. In the first period, they can either work or get educated; yet, as we focus on low-skilled women, they will choose to work. In the second period, they can have children and continue to work in temporary jobs, or they can continue working to avoid any disruption in their ability to obtain a permanent job and postpone motherhood. A fixed-term contract pays w and a permanent contract pays $\bar{w}$, where $\bar{w} > w$. Only a fraction of women who would be able to get promoted without higher education $0 < \gamma \leq 1$, and just gain from continuous fixed-term employment.[33] One limitation of this model is that it does not take a partner into account. As a result, it assumes that women's preferences alone determine the demand for children, rather than viewing it as a household decision.

Yet, the dependability of a partner will be important only for women whose partner is a high type ($D = 1$), meaning that they provide care for their children, since low-type partners $D = 0$ will disrupt women's employment trajectories, as women would not be able to both care for the child and work in rigid low-skilled markets. Therefore, the child penalty is $(w - \bar{w})$. If women who are single mothers or have a child with a low-type partner, they will keep earning w once they return to work. If they have a child with a high-type partner or they keep working until getting a permanent job, they will earn w once they come back from maternity leave.

Low-skilled women will make two decisions. The first is entering the job market at the minimum legal age for remaining in the workforce until they get a permanent job (without any disruptions), or they decide to enter the job market and are able to exit and re-enter the labor force, always under a temporary contract. The second decision is whether they should have a child in the second period, given their employment foreseen

[32] Goldin (2025a) contains a related model.

[33] Here, we think that low-skilled women without higher education will only be able to obtain a permanent job after the implementation of the 1985 labor market reform if they do not experience long-term unemployment or any labor interruption (including maternity leave) and maintain continuous employment.

circumstances. We assume that all low skilled-women who have a child before securing a permanent job would not be ablet to get it after birth. All women will receive K a unit of value or utility from having a child. Therefore, a fraction of women $(1-\rho)$ who will decide to have a child without securing a permanent contract before will have a lifetime value (or utility) of $2w+K$. The fraction of women ρ) who will keep working and expect to obtain a permanent job will get $(\bar{w}+K\delta)$ in utility, where δ is being in a fertile window once a permanent contract is secure, meaning they are still able to have a biological child.

Women who choose to remain in the workforce, obtain a permanent job and then have a child, will receive $(\bar{w}+K)$, but if she remains in the workforce, obtain a permanent contract and has no children, she will receive $\bar{w}$. If she decided to exit the labor force under a temporary employment and has a child, she will receive $(2w+K)$. All low-skilled women will have a child as long as $K>0$. When $\delta=0$, the ρ-fraction of women who will remain in the workforce only if $\bar{w}>2w+K$, and none will have a child. At the other extreme when $\delta=1$, all low-skilled women will remain in the job market only if $\bar{w}>2w$ and will have a child. For values of $\delta>1-[K/(\bar{w}-w)]$, all women who invest in remaining in the labor force will have a child, and for values below, none will. If so, a low-skilled women who remains in the labor force will not have a child if $\bar{w}>\delta(\bar{w}+K)+(1-\delta)(w+K)$, because the expected value of having a child is less than the certain value of not having one. Therefore, the ρ-fraction of women who can invest in remaining in the labor force will do if:

$$\bar{w}>2w+K(1-\delta)$$

When, in addition, $\delta>1-[K/(\bar{w}-w)],\ )]$, all low-skilled women who remain in the labor force until getting a permanent contract will have a child. Instead, if $\delta<1-[K/(\bar{w}-w)]$, those who exit the labor force while in temporary employment will have a child, and the -fraction of low-skilled women will invest in continuing in the workforce until they get a permanent job if $\bar{w}>2w+K$.

The model underscores the importance of socioeconomic circumstances (such as marriage, homeownership, family income), informal and formal childcare support, and cultural and attitudinal preferences (including a mismatch of societal-individual and partner-mismatch values). We define modern low-skilled women as those who choose to pursue a successful career with the potential to progress to permanent jobs. For instance, when both partners are traditional, the demand for motherhood will be high. But when a modern woman marries a traditional man, it is when a within-couple mismatch occurs, and the demand for motherhood will decrease. Yet, we are not able to estimate this type of mismatch in this paper.

As more women entered the labor force in Spain in the second half of the 20th century, their chances of earning a good salary and their preference for pursuing a successful professional career increased, which in

turn decreased their demand for motherhood. The 1984 Spanish labor market reform assisted low-skilled youth in securing their first job, but it also made it more difficult for low-skilled women to obtain permanent contracts. The demand for motherhood will continue to decrease unless there is some institutional or partner support for mothers to combine work (and progression) with having children. Therefore, as more low-skilled women decide to work and pursue a successful progression in their careers, more benefits that substitute for the childcare tasks that these women traditionally have done are needed (e.g., subsidized childcare, paid maternity leave) or more opportunities are needed for those low-skilled women to help them progress sooner, when $\delta > 0$.

B. THE SPANISH LABOR MARKET IN FIGURES

Table B1: Contract types in 1984 and their terms

Type of Contract	Duration	Severance
Permanent contracts		
Permanent (standard)	Indefinite	20 days/year
Temporary (with cause)		
Work or service	Temporary	None
Production needs	Max. 6 months	None
Interim	Temporary	20 days/year
Launching new activity	6–36 months	None
Temporary (no cause)		
Fixed-term (standard)	12–36 months	12 days/year
Training	6–24 months	None
Apprenticeship	6–36 months	None
Other		
Part-time	Permanent or temp	Based on type
Retirement replacement	Temporary	Proportional

Notes: This Table shows the types of contracts that were in place in 1984 and their severance and duration. All temporary contracts, with and without an objective, were introduced by the 1984 labor market reform. *Source:* Milner et al. (1995), Table 1.

Table B2: Total share of temporary employment (%) by educational level, age and gender in Spain

Years	**1983**	**1987**	**1990**	**1998**	**2007**	**2018**
Average	2.2	19.8	29.7	32.8	31.7	26.8
By educational level						
Primary or less	2.6†	22.6†	33.8	35.9	37.4	31.9
Secondary	3.0†	26.1†	39.1	35.5	31.4	27.4
University	1.5‡	13.5‡	20.3	22.9	24.8	22.1
By age						
16–24	4.0†	35.6	65.4	73.0	62.5	73.2
25–39	1.6†	13.7	27.9	35.3	35.1	28.3
40–59	1.0†	8.5	14.9	16.8	20.5	14.3
By gender						
Males	2.1†	18.1	27.1	31.9	27.8	25.5
Females	2.7†	23.5	33.6	34.3	31.1	27.7

Notes: This table presents the proportion of individuals holding temporary contracts, categorized according to their education, age, and gender. Source: Spanish Labor Force Survey (LFS) and Eurostat started to collect data in 1987 by groups of population. 1983 information comes from the Central Balance (Bank of Spain). Information on temporary employment by population subgroup became available from 1987 onwards. †Eurostat/INE do not publish 1983 breakdowns by age/sex. We therefore scaled the 1987 composition to the 1983 overall level: for each cell, $rate_1983 rate_1987 / avg_1987 avg_1983$. Treat these as approximations. ‡ Consistent, public breakdowns by education are not available for those years in the standard series. We used the 1990 education composition as a proxy and scaled to 1987 and 1983 averages, i.e., $rate_y ear rate_1990 / avg_1990 avg_y ear$.

Table B3: Simulation: outcomes of temp workers after 1 year (1987–96)

Period	Stay Temp	Perm	Unemp	Out of LF	Other$^{\pm}$
1987.II–1988.II	48.6	22.0	18.0	5.5	5.9
1988.II–1989.II	56.6	19.1	14.6	3.8	5.9
1989.II–1990.II	57.6	17.8	14.9	5.0	4.6
1990.II–1991.II	63.7	12.8	14.1	4.3	5.1
1991.II–1992.II	57.5	14.0	17.0	6.5	5.1
1992.II–1993.II	56.9	9.4	23.7	5.5	4.5
1993.II–1994.II	63.0	9.2	19.6	4.6	3.5
1994.II–1995.II	64.4	10.8	16.4	4.9	3.6
1995.II–1996.II	62.7	11.0	17.3	5.3	3.7
Average	59.0	14.0	17.3	5.1	4.7

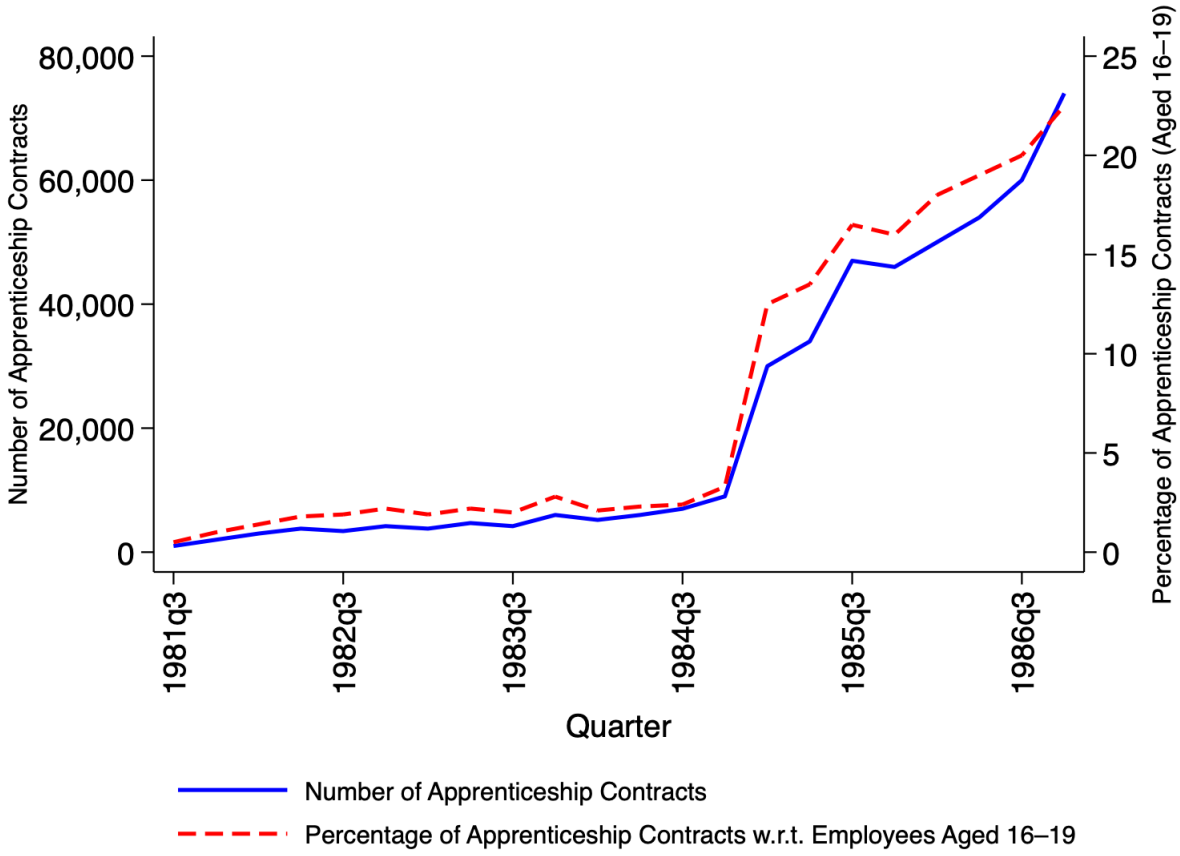
Notes: Annual transitions of temporary workers. $^{\pm}$Other includes unclassified, self-employed, conscripts, etc. Source: Toharia (1997), Table I.2; Adam & Canziani (1998), Table 11.

Table B4: Parental leave reforms in Spain.

	March 1980[a]	**March 1989**[b]	**November 1999**[c]	**March 2007**[d]
Fathers	2 days of paid job absence after the baby's birth	2 days of paid job absence after the baby's birth	2 days of paid job absence after the baby's birth	2 days of paid job absence after the baby's birth; 13 days of job-protected paid leave (non-transferable to the mother)
Mothers	14 weeks of job-protected paid leave (non-transferable to the father)	16 weeks of job-protected paid leave. The first 6 weeks after birth are compulsory and exclusively reserved to the mother. The last 4 weeks (of the 16) can be transferred to the father	The first 6 weeks after birth are compulsory and exclusively reserved to the mother. The other 10 weeks can be transferred to the father and taken simultaneously or subsequently to the mother's leave	No change

Notes: [a]Statute of Rights for Workers March 1984 Law 30/1984 for the reform of the Public Service. [b] Law 3/1989 to extend maternity leave to 16 weeks and to promote gender equality at the work place. [c] Law 39/1999 to promote work and family life. [d] Law 3/2007 on effective equality between men and women. *Sources:* Farré & González (2019), Appendix Table 1.

Figure B1: Total share of temporary employment and unemployment in Spain (1981-2018)



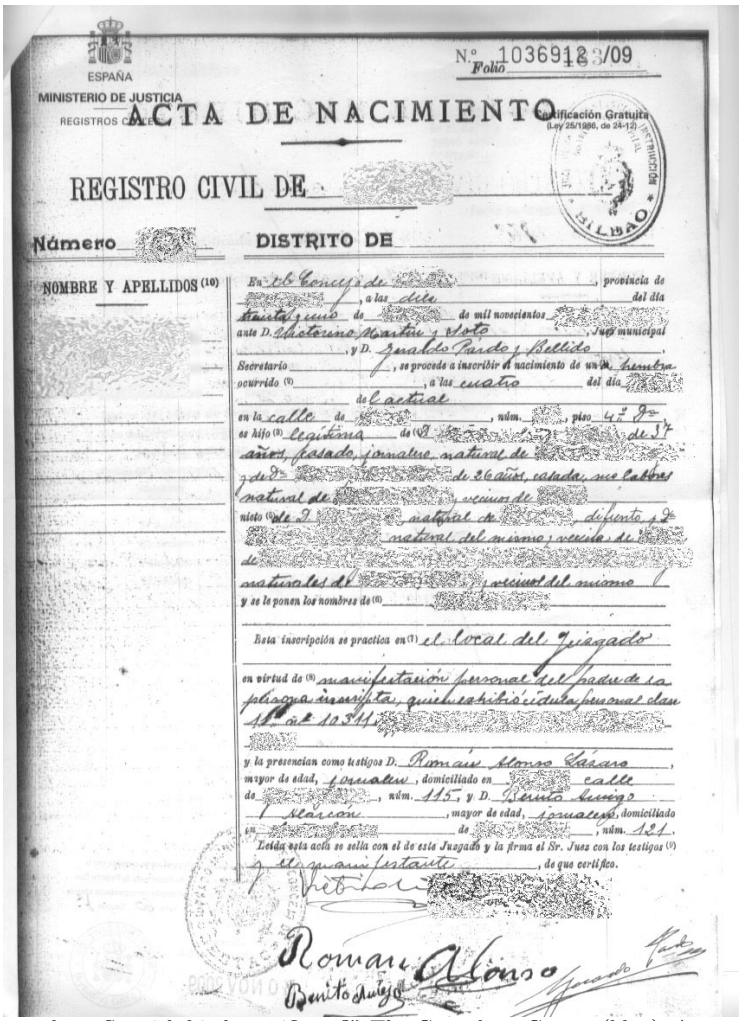
Source: Spanish National Institute of Statistics (EPA) and García-Pérez et al. (2018).

C. DATA DETAILS

C1. SPANISH BIRTH CERTIFICATE MICRODATA (19842018)

The data used in this study come from the Birth Statistics (Estadística de Nacimientos) compiled by the Instituto Nacional de Estadística (INE) as part of the Movimiento Natural de la Población (MNP). These records originate from the civil birth certificates (certificados de nacimiento) registered in local Civil Registries across Spain. The modern microdata series begins in 1980, following the reform of the national vital statistics system that standardized the collection and transmission of demographic events (births, deaths, marriages) to INE.

Figure C1.1: A traditional Spanish birth certificate before the introduction of telematic forms



Source: Source: Daniel, "How to order a Spanish birth certificate?" The Genealogy Corner (blog), August 23, 2016. Image captioned "A traditional Spanish birth certificate before the introduction of telematic forms." Available at: thegenealogycorner.com/2016/08/23/how-to-order-a-spanish-birth-certificate/ (accessed October 24, 2025)..

At the time, each birth was registered manually at the municipal Civil Registry, based on information provided by the parents and the medical birth certificate issued by the attending physician or midwife. These paper records were then forwarded to the provincial statistical offices and aggregated by INE. From 1980 until 2015, all information was recorded by hand on standardized paper forms, later digitized by INE for national statistics – see figure C1. In 2016, the system transitioned to an electronic civil registration system, allowing hospitals and registries to transmit birth information digitally in real time.

A unique feature of the Spanish system is that, beginning in the early 1980s, the medical birth certificate—attached to the civil record—started to include perinatal health information. This includes the newborn's birth weight (in grams), gestational age (in completed weeks), plurality of birth (single, twin, triplet, etc.), and the number of previous live births and stillbirths to the mother. These additions followed recommendations from the World Health Organization (WHO) and the United Nations Statistical Division to harmonize definitions of live births and perinatal indicators across countries. Spain adopted these standards earlier than many European countries, making its historical microdata series especially valuable for long-term research on perinatal outcomes.

Although data entry was manual, coverage and consistency improved markedly after the late 1970s. Minor issues persist with outliers in reported birth weight or gestational age in the earliest years of the microdata (1980–1985), reflecting transcription or reporting errors in handwritten records. INE's documentation notes that these anomalies are rare and easily identifiable for exclusion. Compared to other European countries, Spain stands out for having readily accessible, nationally representative microdata on births that include key perinatal variables for over four decades. This combination of civil registration detail and medical perinatal information, continuously recorded since 1980, makes the Spanish Birth Statistics a rare and valuable resource for studying historical trends in perinatal health, fertility, and demographic change at the population level. This microdata is publicly available through INE's online portal.

C1.1. CONSTRUCTION OF THE DENOMINATOR VARIABLE: COHORT BIRTH RATE BY MOTHERS QUARTER OF BIRTH

A limitation of our dataset is that individual birth records for mothers born between 1962 and 1975 are unavailable, as official microdata from the Instituto Nacional de Estadística (INE) begin in 1980. To overcome this limitation and obtain a suitable denominator for one of our main outcome variables—the cohort birth rate by mother's quarter of birth—we construct a dataset that captures the total number of births occurring during the mothers' own birth period. This variable reflects the size of the cohort into which each mother was born and allows fertility outcomes to be expressed relative to cohort size.

Data source and coverage—We use the historical vital statistics compiled by INE, which report monthly counts of live births for each of Spain's 50 provinces. These data are available for the period 1962–1972 and are disaggregated using the official two-digit INE province codes (e.g., "02 Albacete," "28 Madrid"). The series form part of INE's *Movimiento Natural de la Población* (MNP) and are widely used in demographic and historical research.

Aggregation to quarterly provincial cohorts—To align the data with the quarterly structure of our analysis, monthly birth counts were aggregated into quarterly totals by province. Quarters were defined according to the calendar year: Q1: January–March; Q2: April–June; Q3: July–September; Q4: October–December.

For each province p, year t, and quarter q, the quarterly number of births was obtained as the sum of all monthly births within that quarter:

$$B_{p,t,q} \;=\; \sum_{m \in q} \text{Births}_{p,t,m}.$$

Cohort-trimester identifier—Two identifiers were created to facilitate merging with other datasets. First, a numeric province code was extracted from the province label (e.g., "02 Albacete" recoded as 2, "28 Madrid" as 28). Second, a cohort-trimester identifier (`cohorttrim`) was defined by concatenating the year and the quarter number as:

$$\texttt{cohorttrim}_{t,q} \;=\; \texttt{concat}(t, q).$$

Thus, the first quarter of 1962 corresponds to `19621`, the second to `19622`, and so forth.

Use of the constructed denominator—The resulting dataset provides the number of births per mother's birth cohort and province, which serves as the denominator when calculating fertility rates later in the analysis. When combined with information on the number of children born to women in each cohort, this denominator allows the construction of the cohort birth rate by mother's quarter of birth, defined as:

$$\text{CohortBirthRate}_{p,c,q} \;=\; \frac{\text{Births to mothers from cohort } (c,q) \text{ in province } p}{\text{Total births recorded in province } p \text{ during } (c,q)}.$$

Births to mothers from cohort (c, q) in province p is the number of births attributed to mothers from cohort (c, q) born in province p; and the denominator is the total number of births recorded in that same province and quarter when those mothers were born.

The variable therefore represents the proportion of new births generated by each maternal cohort relative to its original cohort size, without applying any multiplicative scaling. This normalization ensures comparability across cohorts and regions while preserving the original data scale.

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C2. SPANISH FERTILITY SURVEY (1999 AND 2018)

This study also draws on the Spanish Fertility Survey (Encuesta de Fecundidad), conducted by the Instituto Nacional de Estadística (INE) in 1999 and 2018. Both surveys are nationally representative, cross-sectional datasets designed to capture the determinants, timing, and outcomes of fertility and reproductive behavior in Spain. While both share a common structure and comparable core indicators, the 2018 wave introduced important innovations in questionnaire design, population coverage, and attitudinal content, reflecting the profound social and demographic transformations that occurred in Spain over the preceding two decades.

The 1999 survey was carried out at the end of the fertility decline that began in the late 1970s, when Spain reached one of the lowest fertility rates in Europe. It collected retrospective reproductive histories for all women aged 15–49, along with a limited set of socioeconomic and attitudinal variables. Respondents provided detailed information on the timing and number of live births, contraceptive use, and union history, as well as basic background characteristics such as education, occupation, and region. Although the 1999 wave included a few questions on gender roles and family values, its main focus was on fertility timing and completed family size rather than on attitudes or household division of labor.

Nearly two decades later, the 2018 Encuesta de Fecundidad represented a major redesign of the survey, both conceptually and methodologically. It included men for the first time (aged 18–55), moved to a digital data collection system, and significantly broadened the scope of attitudinal and behavioral variables. The new modules captured not only detailed reproductive histories but also:

1. Fertility ideals and intentions, including desired number of children, perceived ideal age for motherhood and fatherhood, and reasons for delaying or foregoing childbearing.

2. Work–family reconciliation, covering perceptions of institutional support, maternity and paternity leave experiences, and difficulties balancing employment and family life.

3. Gender-role attitudes, such as agreement with statements on the appropriate division of household tasks, parental responsibilities, and the role of women in the labor market.

4. Partnership dynamics, including cohabitation history, partner's characteristics, and shared decision-making about fertility and domestic work.

These additions make the 2018 survey uniquely suited to examine how attitudes toward fertility, gender norms, and the sharing of household tasks interact with demographic behavior. The 1999 wave, by contrast, serves as a benchmark for fertility behavior during the earlier phase of Spain's demographic transition, but with much less information on underlying beliefs and family norms. Together, the two surveys allow for a

rare intertemporal comparison of reproductive intentions and gender attitudes over nearly twenty years of social change.

The Spanish Fertility Surveys are cross-sectional, not panel datasets; hence, there is no attrition across waves. However, both surveys collect retrospective fertility and union histories, which may be affected by recall bias, particularly for older cohorts. To mitigate this issue, INE incorporated detailed event-history modules that anchor births and partnerships to calendar years and key life events (education completion, first job, etc.), improving temporal accuracy.

Both surveys exhibit generally low levels of item non-response due to interviewer training and automated consistency checks (in 2018). However, some variables—especially attitudinal questions on fertility ideals, work–family reconciliation, or division of household tasks—show higher rates of missing values. Both surveys exhibit generally low levels of item non-response due to interviewer training and automated consistency checks (in 2018). However, some variables—especially attitudinal questions on fertility ideals, work–family reconciliation, or division of household tasks—show higher rates of missing values.

Despite differences in coverage and questionnaire structure, both surveys maintain comparable fertility-history modules, enabling consistent measures of parity, age at first birth, and timing of subsequent births. The inclusion of attitudinal and gender-norm variables in 2018 offers the opportunity to link actual fertility behaviour with stated beliefs about family and work roles—a dimension not systematically observed in the 1999 data. The microdata include identifiers for regions (NUTS-2 level), allowing researchers to explore spatial differences in fertility behaviour and gender attitudes across Spain. Both datasets are publicly available for research through the INE microdata portal.

D. VARIABLES AND TREATMENT DEFINITION ROBUSTNESS

Table D1: Treatment definition for the main analysis by datasets.

Birth Records Dataset	Fertility Survey Dataset
High-skilled workers	
Professionals, technicians, and related occupations	Second stage of secondary education and related education (with general or professional orientation)
Executive leadership positions in public administration and private corporations	Post-secondary non-higher education
Administrative professionals and related occupations	Vocational training, plastic arts and design, and higher-level sports and equivalent education; own university degrees that require a bachelor's degree, lasting 2 years or more.
	University degrees of up to 240 ECTS credits, university diplomas, university expert or specialist degrees, and similar
	University degrees of up to 240 ECTS credits, bachelor's degrees, master's degrees and *specialities* in Health Sciences through the residency system, and similar
	Doctoral training
Low-skilled workers	
Small traders and shopkeepers	Less than elementary education
Service industry workers	Elementary education
Farmers, plant breeders, fishermen and hunters	Lower secondary education and related education
Non-skilled production workers and related groups, driver teams, and support personnel	
Members of the armed forces	
People dedicated to housework	

Notes: Birth Records Dataset uses occupation status to define low vs skilled workers due to lack of information from the years before 1995. Fertility Survey uses educational information to define our treatment variables since occupation variables contains missing information. Observations from currently students, retired workers, pensioners and retired independent and non-classified persons are excluded.

Table D2: Composition of the Egalitarian Views Index

Variable	Variable information	Transformation for index (0–1)
v_juntos	It is acceptable for a couple to live together even if they do not intend to marry.	Direct ($x \rightarrow$ rescaled 0–1)
v_divorcio	It is acceptable for an unhappy married couple to divorce even if they have children.	Direct ($x \rightarrow$ rescaled 0–1)
v_hijosm	A woman must have children to feel fulfilled.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_hijosh	A man must have children to feel fulfilled.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_crecerfeliz	A child needs a home with both father and mother to grow up happily.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_criasola	A woman can raise a child on her own if she does not wish to maintain a relationship.	Direct ($x \rightarrow$ rescaled 0–1)
v_homosexual	It is acceptable for same-sex couples to have the same rights as different-sex couples.	Direct ($x \rightarrow$ rescaled 0–1)
v_prioridadm	For a woman, family should take priority over a professional career.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_principalcuidar	Either parent should be the main caregiver during ages 0 to 3.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_rel-cercanam	A working mother can have a relationship with her child as close as a non-working mother.	Direct ($x \rightarrow$ rescaled 0–1)
v_mganamas	If the woman earns more than her partner, that is not good for the relationship.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_casafam	Taking care of the home and family is as satisfying as paid work.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_tardan	Men should participate in household tasks to the same extent as women.	Direct ($x \rightarrow$ rescaled 0–1)
v_quedarconm	If *parents* divorce, it is better for the child to stay with the mother than with the father.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_trabescaso	When jobs are scarce, men should have greater right to a job than women.	Reversed ($4-x \rightarrow$ rescaled 0–1)
v_rel-cercanap	A working father can have a relationship with his child as close as a non-working father.	Direct ($x \rightarrow$ rescaled 0–1)
v_prioridadh	For a man, family should take priority over a professional career.	Reversed ($4-x \rightarrow$ rescaled 0–1)

Notes: Response scale per item: 1 = Disagree, 2 = Neither agree nor disagree, 3 = Agree. For the egalitarian index, each item is recoded (if applicable, reversed as 4–x), rescaled to 0–1 (0 = more traditional, 1 = more egalitarian), and then averaged.

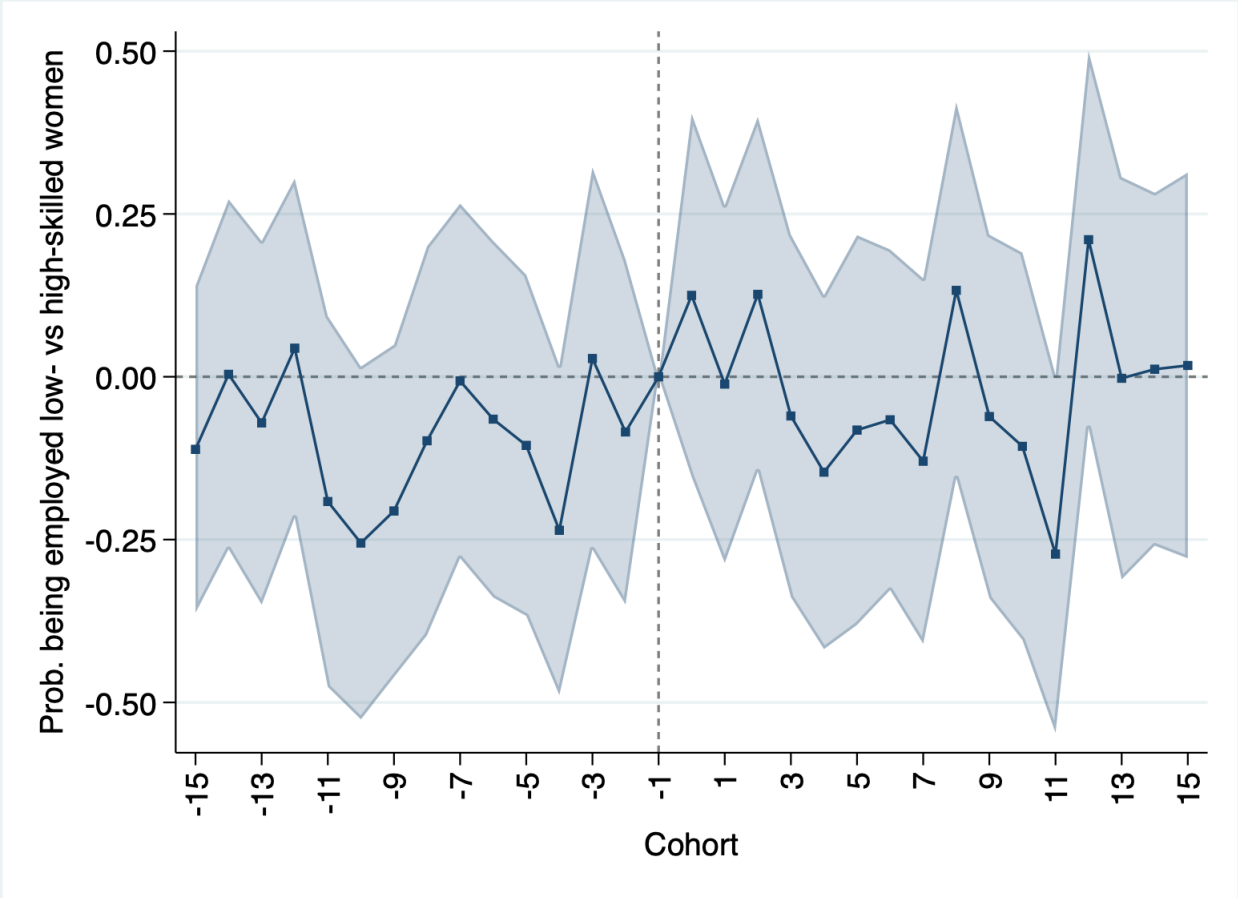
Table D3: Robustness Check. Treatment definition.

Fertility Survey Dataset	
Low-skilled workers	**High-skilled workers**
Employees of travel agencies, receptionists, and telephone operators; counter clerks and related workers (excluding ticket sellers).	Members of the executive branch and legislative bodies; leaders of public administration and social interest organizations; executive directors.
Administrative employees with public-facing tasks not classified under other headings.	Directors of administrative and commercial departments.
Waiters and restaurant owners.	Directors of production and operations.
Salaried workers in the restaurant services.	Directors and managers of accommodation, restaurant, and retail businesses.
Salesclerks in stores and warehouses.	Directors and managers of other service enterprises not classified under other headings.
Store owners.	Healthcare professionals.
Salespeople (except in stores and warehouses).	Professionals in early childhood, primary, secondary, and post-secondary education.
Cashiers and ticket sellers (excluding banks).	Other education professionals.
Workers in healthcare services for personal care.	Professionals in physical, chemical, mathematical, and engineering sciences.
Other workers in personal care.	Legal professionals.
Workers in personal services.	Specialists in public administration and business organization and marketing.
Workers in protection and security services.	Information technology professionals.
Skilled workers in agricultural activities.	Professionals in social sciences.
Skilled workers in livestock activities (including poultry, beekeeping, and similar).	Professionals in culture and entertainment.
Skilled workers in mixed agricultural and livestock activities.	Technicians in physical and engineering sciences.
Skilled workers in forestry, fishing, and hunting activities.	Supervisors in mining engineering, manufacturing industries, and construction.
Workers in structural construction and related occupations.	Health technicians and alternative therapy professionals.
Finishers in construction and installation (except electricians), painters, and related workers.	Financial and mathematical support professionals.
Welders, sheet metal workers, metal structure assemblers, blacksmiths, toolmakers, and related workers.	Representatives, commercial agents, and related professionals.
Machinery mechanics and adjusters.	Administrative support professionals; technicians in law enforcement.
Skilled workers in electricity and electrotechnology.	Support professionals in legal, social, cultural, sports, and related services.
Precision metalworkers, ceramists, glassworkers, craftsmen, and workers in the graphic arts.	Information and communication technology (ICT) technicians.
Workers in the food, beverage, and tobacco industry.	Employees in accounting, financial, production support, and transportation services.
Workers in wood, textiles, clothing, leather, footwear, and other craft-related occupations.	Library, postal, and related service employees.
Operators of fixed installations and machinery.	Other administrative employees without public-facing tasks.
Assemblers and installers in factories.	
Locomotive engineers, agricultural machinery operators, heavy mobile equipment operators, and sailors.	
Drivers of vehicles for urban or road transport.	
Domestic employees.	
Other cleaning staff.	
Food preparation assistants.	
Urban waste collectors, street vendors, and other elementary occupations in services.	
Agricultural, forestry, and fishing workers	
Construction and mining workers; Manufacturing industry workers	

Notes: Fertility Survey uses occupational information to define our treatment variables since occupation variables contains missing information.

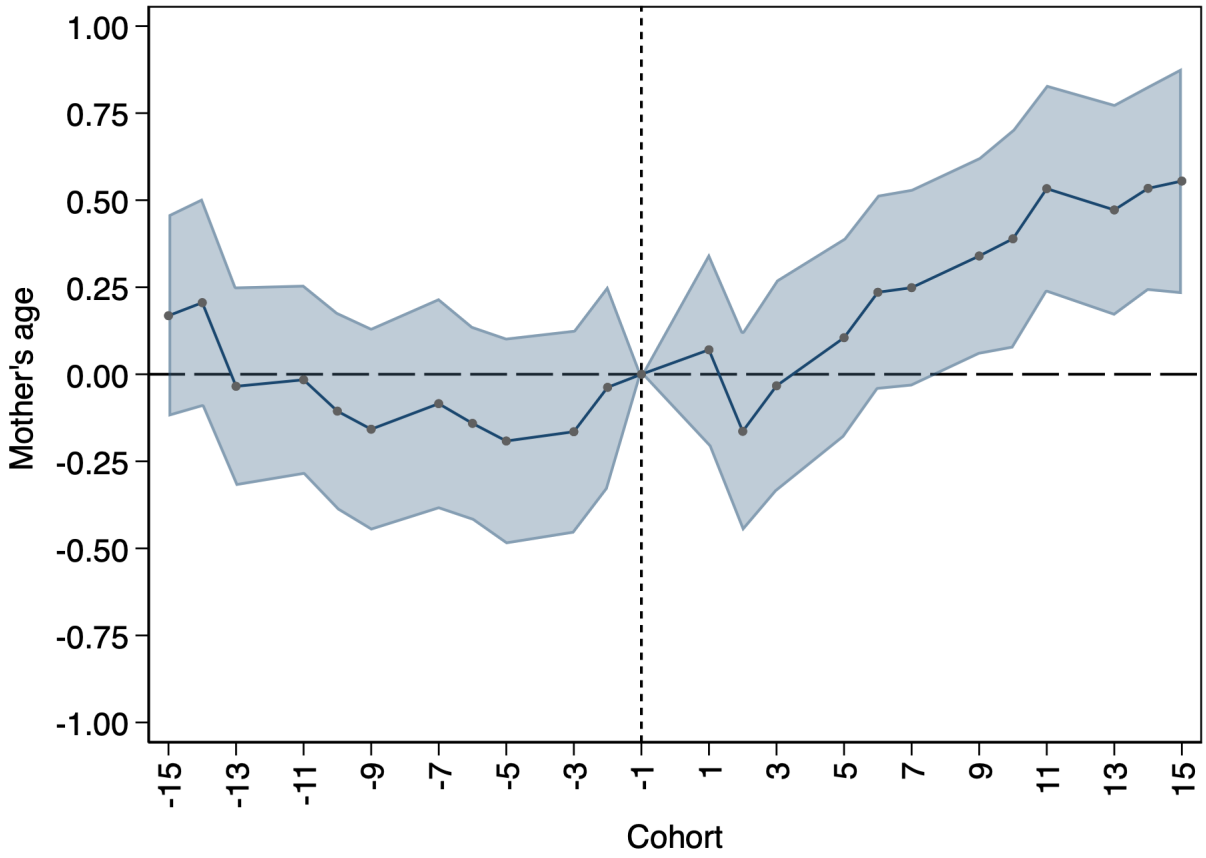
E. EVENT STUDY DYNAMICS

Figure E1: Event Study on the probability of being employed for High-Skilled mothers compared to low-skilled by cohort using Fertility Surveys 2018



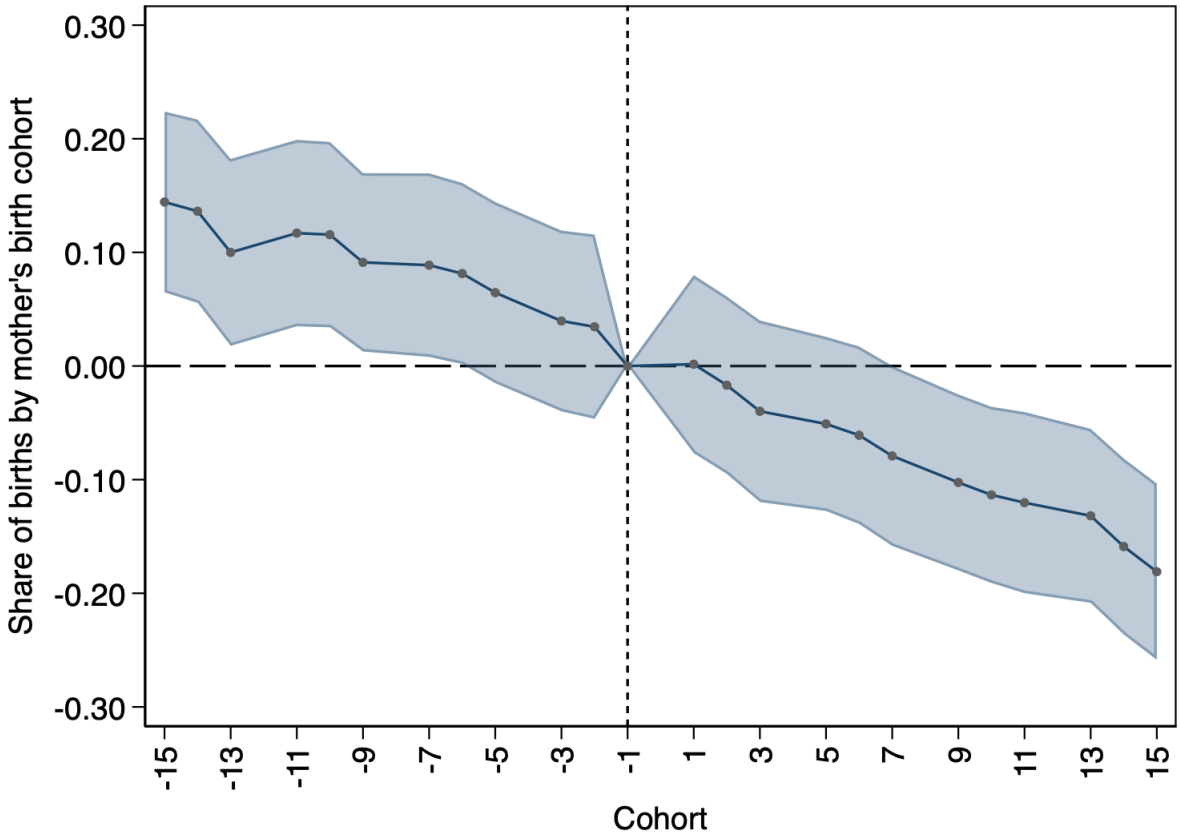
Notes: Sample of native mothers who are low-skilled (born between 1965 and 1972) or high-skilled (born between 1962 and 1969). Event study regression includes regions and a cohort of the mother's quarter of birth fixed effects. The outcome variable takes the value of one if the individual is currently employed and zero if he is unemployed. Includes Region and trends FE. Number of obs = 2,422; R-squared = 0.110. *Source:* 2018 Fertility Survey.

Figure E2: Event Study on mother's age using birth record dataset (1984-2018)



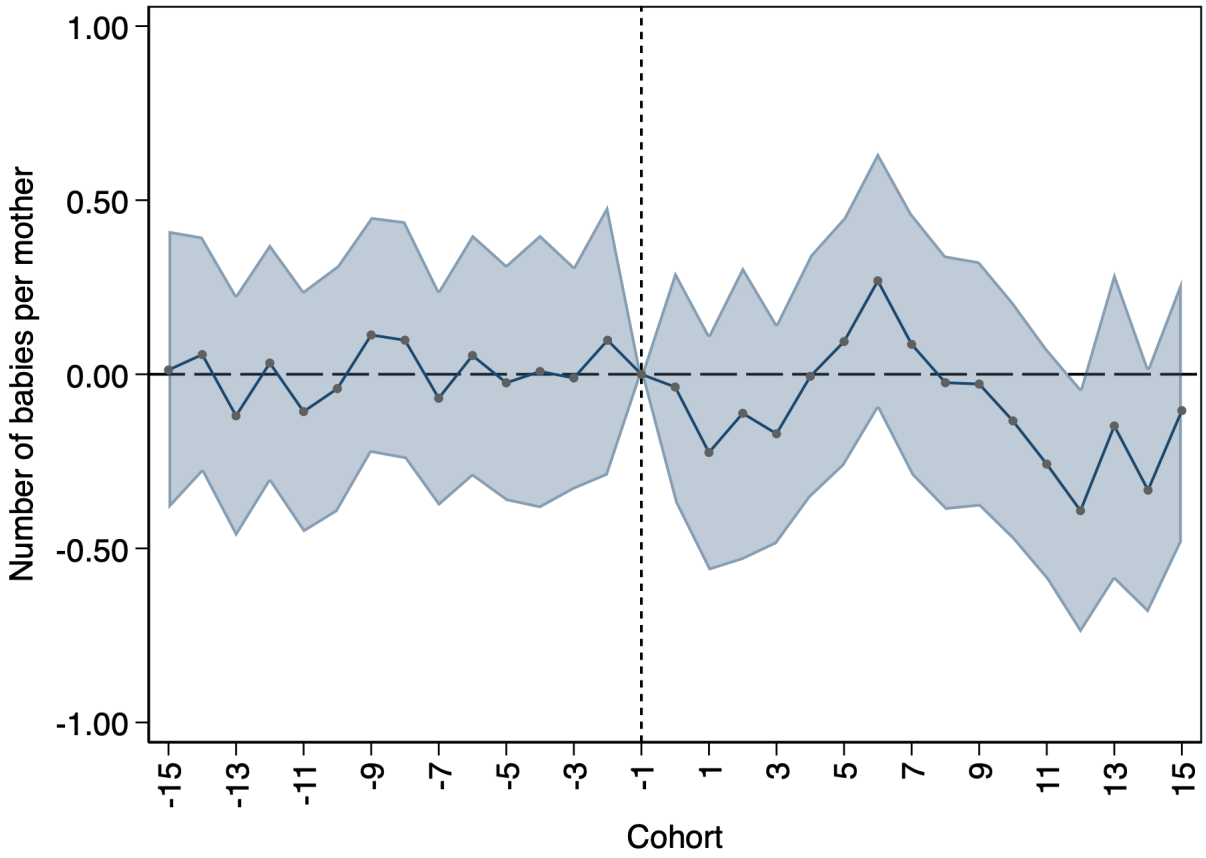
Notes: Cohort base category 1968q2 (prior intervention), event study regression includes province and quarter of birth fixed effects. *Source:* Childbirth Statistics Bulletin (1984-2018) provided by the Spanish National Statistics Office (INE)

Figure E3: Event Study: Cohort of birth rates by mother's quarter of birth cohort (1984-2018)



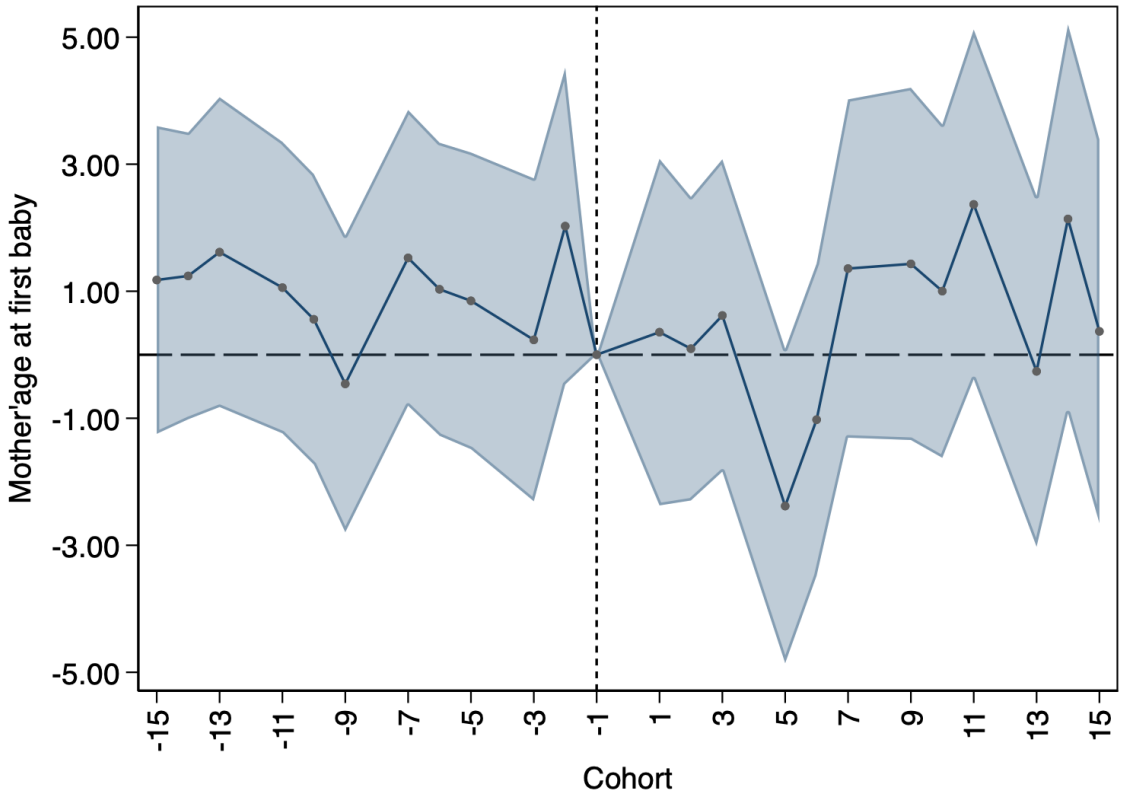
Notes: Treat takes one if the mother is an unskilled worker and 0 if she is a skilled worker. Event study regression includes province and quarter of mother's birth fixed effects. The cohort base category is 1968q2 since we have biannual cohorts. *Source:* Childbirth Statistics Bulletin (1980-2018) provided by the Spanish National Statistics Office (INE).

Figure E4: Event Study on the total number of babies by cohort using Fertility Surveys 1999-2018



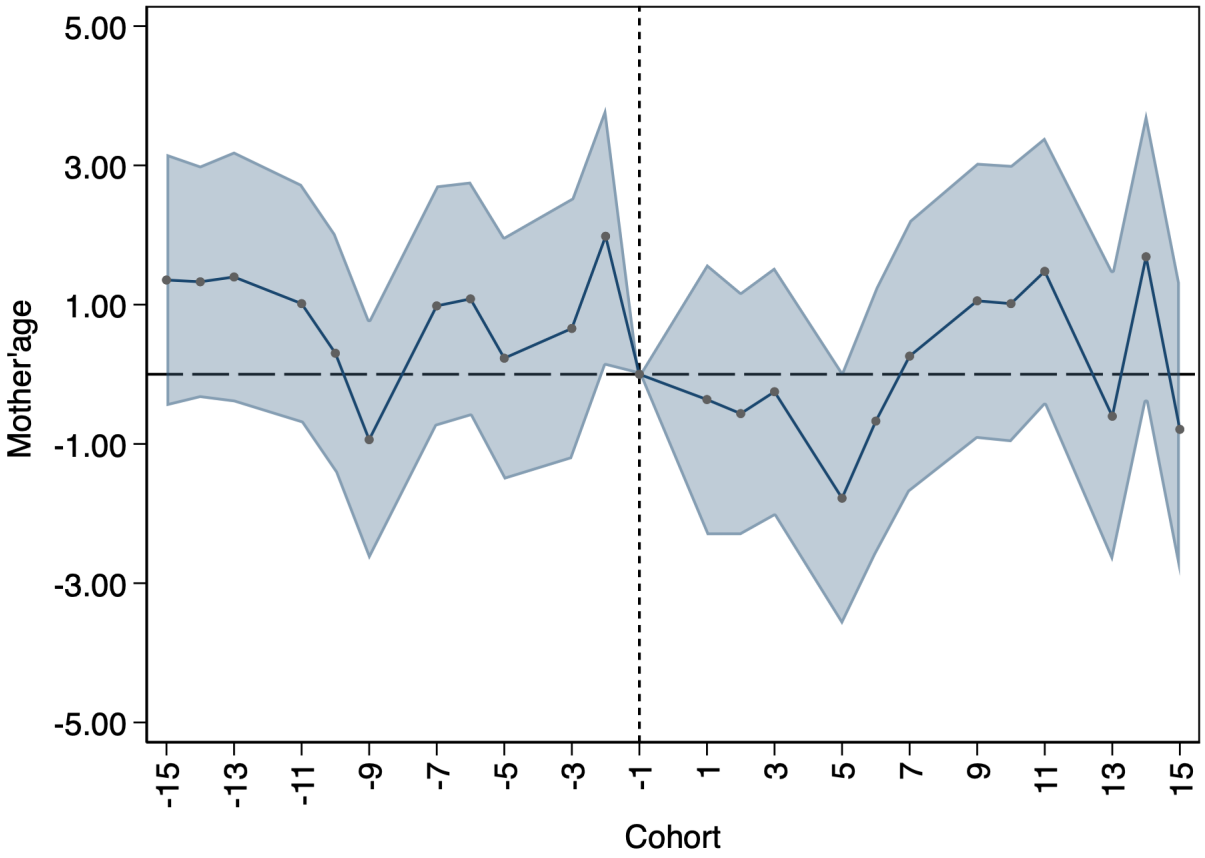
Notes: Sample of native mothers who are low-skilled (born between 1965 and 1972) or high-skilled (born between 1962 and 1969). Event study regression includes regions and a trend fixed effect. Trend is linear cohort trend calculated as the birth year cut-off (C) subtracted from the cohort's birth quarter. Includes Region FE, trends FE and year of survey FE. Number of obs = 3,733; R-squared = 0.074. Source: 1999 and 2018 Fertility Survey.

Figure E5: Event Study on mother's age at first baby by cohort using Fertility Surveys 1999-2018



Notes: Sample of native mothers who are low-skilled (born between 1965 and 1972) or high-skilled (born between 1962 and 1969). Event study regression includes regions and a trend fixed effect. Trend is linear cohort trend calculated as the birth year cut-off (C) subtracted from the cohort's birth quarter. Includes Region FE, trends FE and year of survey FE. Number of obs = 3,733; R-squared = 0.074. Source: 1999 and 2018 Fertility Survey.

Figure E6: Event Study on mother's age at first baby by cohort using Fertility Surveys 1999-2018



Notes: Sample of native mothers who are low-skilled (born between 1965 and 1972) or high-skilled (born between 1962 and 1969). Event study regression includes regions and a trend fixed effect. Trend is linear cohort trend calculated as the birth year cut-off (C) subtracted from the cohort's birth quarter. Region FE, trends FE and year of survey FE. Number of obs 7,193; R-squared = 0.270. Source: 1999 and 2018 Fertility Survey.

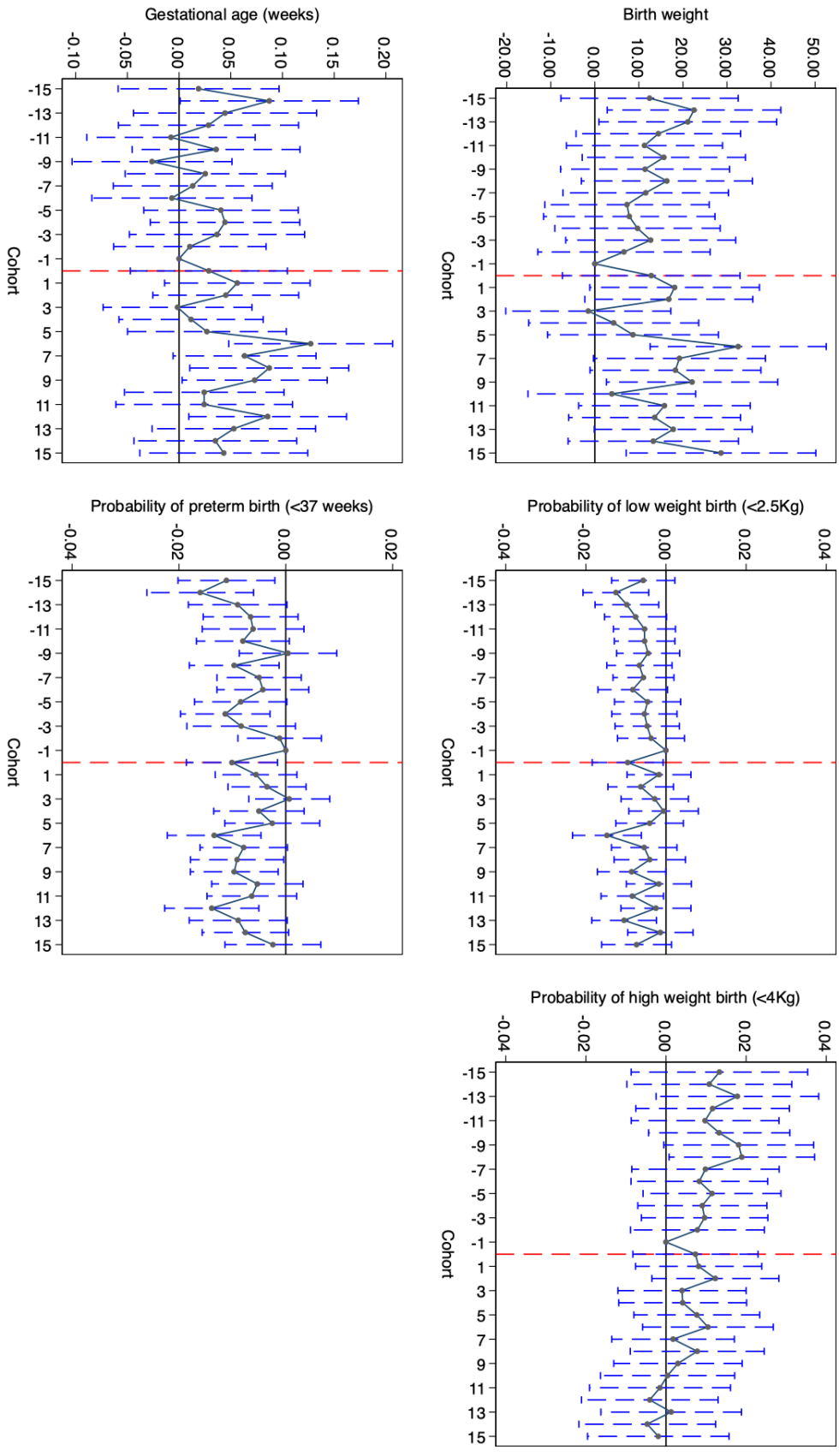
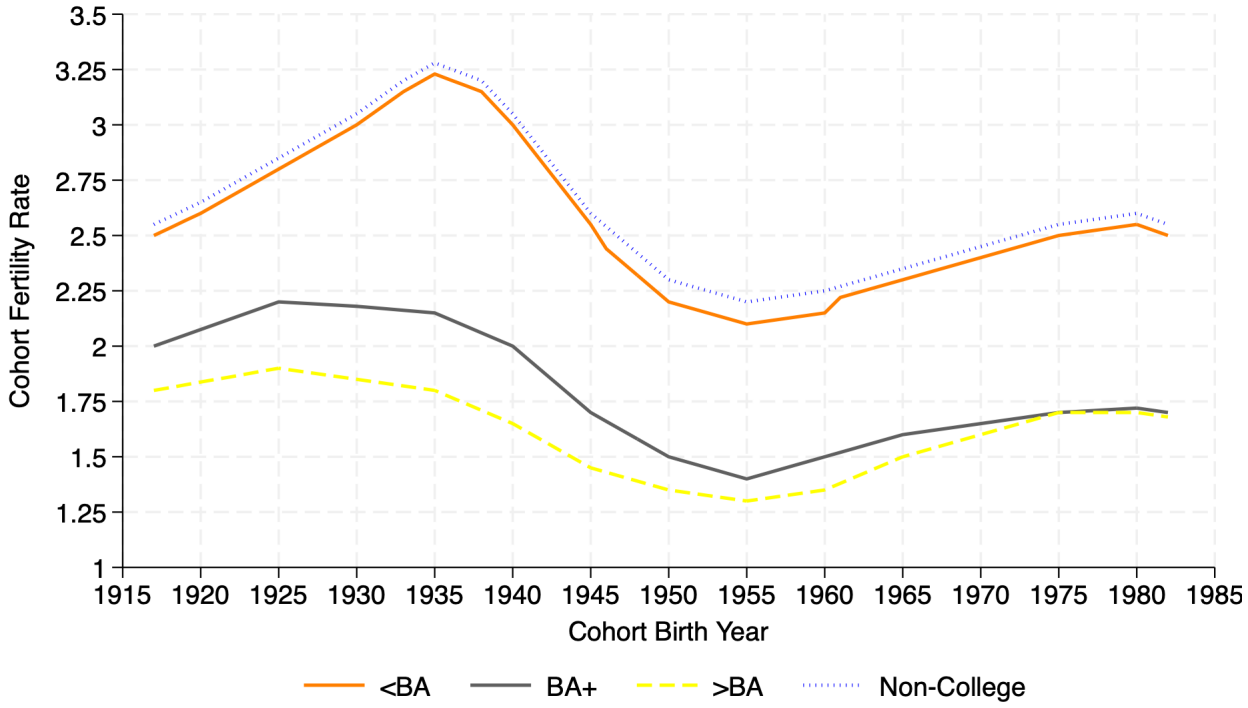


Figure E7: Event Study: Perinatal health outcomes

Source: Birth records (1980-2017), Spanish National Statistical Institute

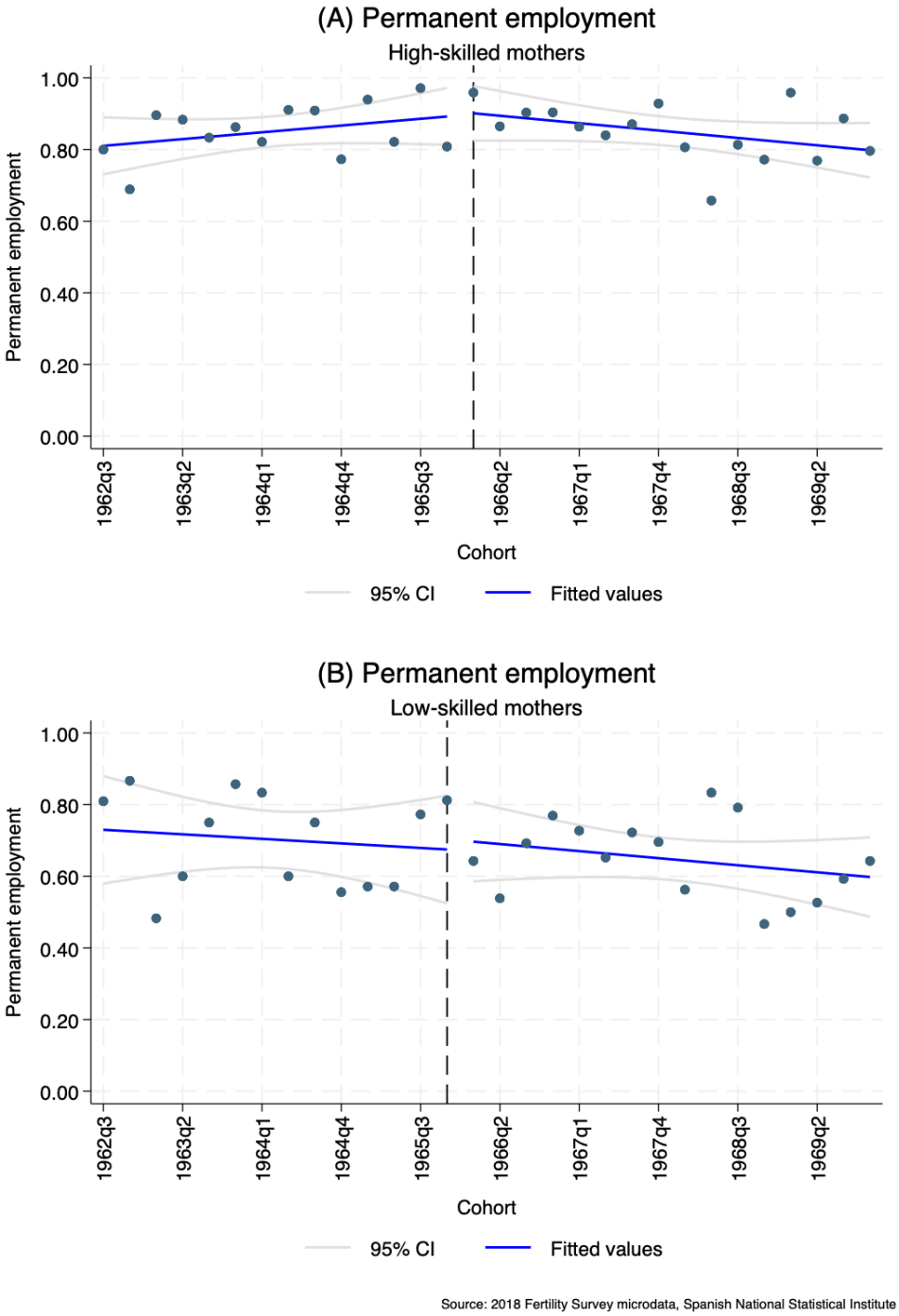
F. FURTHER TABLES AND FIGURES

Figure F1: Cohort Fertility by Education: U.S. Cohorts Born 1917–1982



Notes: BA means either a bachelor's degree or at least four years of college. BA+ means all women with a BA, as well as those with a postgraduate degree or more than 4 years of college. >BA are those with more than a BA. Non-college means no years of college at all. <BA means no BA or not four years of college or no college. <BA and BA+ would be exhaustive. Women 40+; CFR = weighted mean of CHBORN; cohorts 1917–1982. Education groups: Non-College, <BA, BA+, >BA; 3-year centered moving averages; Weights: WTFINL (or SUPPWT). This figure shows that the approximate annualized long change (semi-elasticity with respect to time) was -2.02 for <BA and -1.23 for >BA. *Sources:* U.S. Census Bureau, Current Population Survey (June Fertility Supplements), 1973–2022; authors' calculations replicating Golding (2015), Figure 6.

Figure F2: Placebo test on the probability of obtaining a permanent contract using other mothers' cohorts not supposedly affected by the reform at entry



Source: 2018 Spanish Fertility Survey. Sample of native mothers who are low-skilled (born between 1962 and 1969) or high-skilled (born between 1962 and 1969) adn therefore not affected by the reform at entry.

Figure F3: Robustness Checks III. Intensity of the reform using Fertility surveys 1999–2018

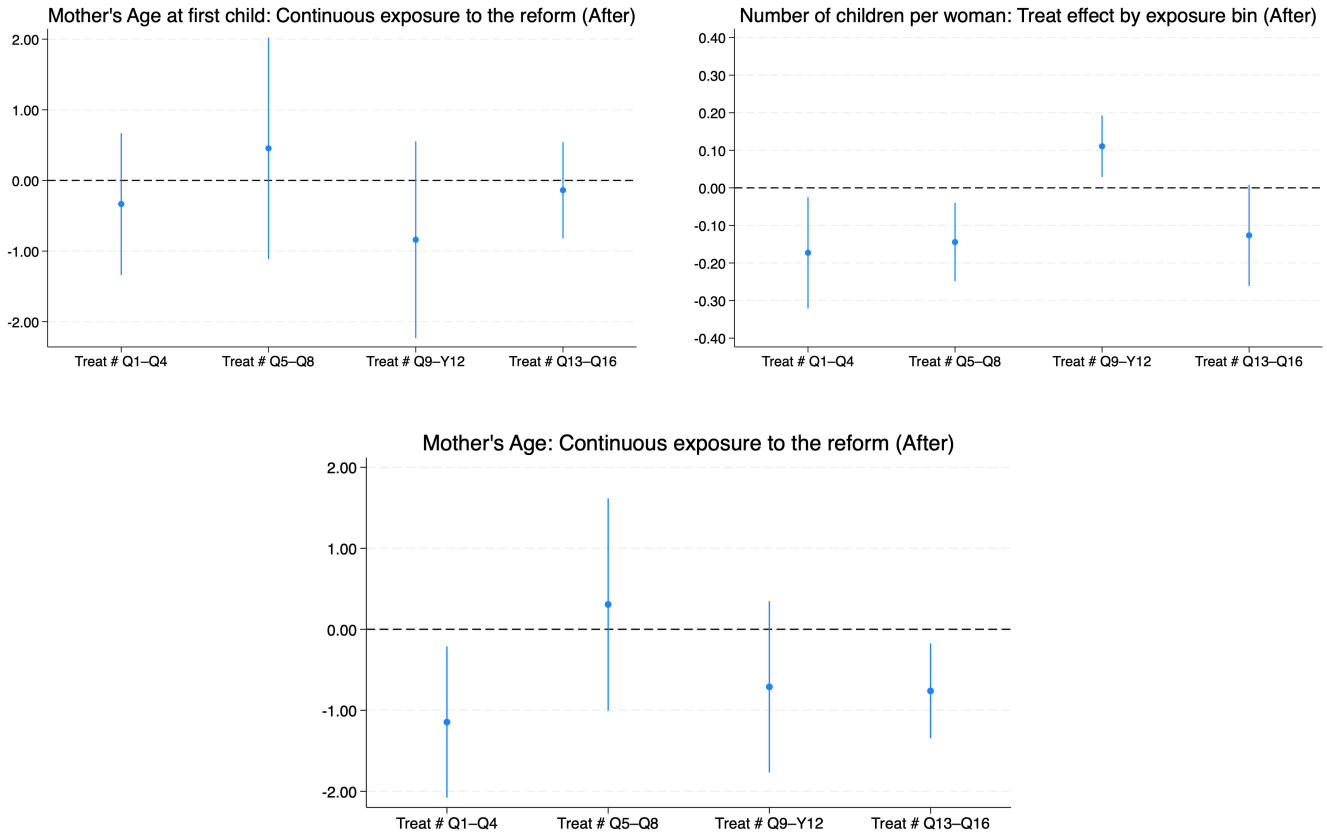


Figure F4: Robustness Checks III. Intensity of the reform (fertility outcomes) using birth certificate records data