# Back to Work or Back to the Maternity Ward? The Effects of Extending the School Day on Fertility*

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**Abstract**

We study whether extending the school day in public schools—an implicit subsidy on childcare—affects women's fertility decisions in Mexico. Exploiting the staggered implementation of a nationwide policy directed to children 3 to 12 years of age and information on the universe of births, we find that full-time schools availability reduces fertility by 7.8% over the course of 8 years and that this effect is fully explained by reductions in fertility among second and higher-order births. Fertility decreases more for less educated women in municipalities with stronger labor markets and living in poorer municipalities.

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# 1 Introduction

Over the past decades, fertility has evolved differently in developed and less-developed countries. In developed countries, the economics of fertility appears to be entering a 'new era' (Doepke et al., 2022), where the growing prominence of alternative forms of childcare, changes in desired fertility, and the disappearance of child labor have blunted the role of the forces typically emphasized by first generation economic models—namely, the quantity-quality trade-off and models based on women's opportunity cost of time, both pointing to a negative relationship between income and fertility and between labor force participation (FLFP) and fertility. The new literature for developed countries highlights the importance of the compatibility of family and career as well as family-friendly policies among the main drivers of fertility. It also underlines the importance of understanding fertility along both the extensive and intensive margin, a feature that is by and largely absent from first- generation models (Aaronson et al., 2014). However, only a few studies explore these particular questions in the context of low- and middle-income countries (LMIC).

Additionally, while the mismatch between the length of the school day and the workday is known to be an important childcare gap for parents who are (or would want to be) in the workforce, the effect of policies extending the length of the school day and thus closing this gap—an implicit childcare subsidy—on fertility is unknown and, generally understudied. Theoretically, extending formal childcare may increase fertility rates due to the alleviation of childbearing costs. On the other hand, fertility rates may drop due to increased chances for mothers to participate in the labor market. This paper aims to fill this gap. We study whether extending the school day in public schools affects women's fertility decisions in Mexico.

Studying the effects of increasing the availability of childcare on fertility within the Mexican context is likely to be informative to less developed countries transitioning to a low-fertility setting but with still relatively high fertility rates. While Mexico has the second highest fertility rate (2.1 children born per woman in 2020) for OECD countries, it also experienced the most significant fall in fertility between 1980 and 2020 by far (a fall of 3.2 from 5.3 children per woman) (OECD, 2020).[1] This fall was mainly explained by reductions in third and higher-

[1]The second largest fall in fertility for this period of 2.6 was observed in Turkey. The OECD average fall was

ordered births in Mexico, as opposed to increases in the mean age of women giving birth, decreases in second-order births, and increased childlessness as was, in general, the case in more developed countries.

We study the causal effects of a large-scale full-time schools (FTS) program in Mexico on fertility. The FTS program extended the length of the school day from an 8:00a.m.–12:30p.m. school day to an 8:00a.m.–4:00p.m. one, an increase of three-and-a-half hours, in public pre- and elementary schools. The program started in the academic year 2007- 2008 and covered 500 schools in 15 states. Over time, it was gradually extended to other schools and states. By 2018, more than 25,000 schools distributed across all states in Mexico had implemented it (see Cabrera-Hernández and Padilla-Romo, 2020). We exploit the variation in the staggered roll-out of Mexico's FTS program across municipalities and use the universe of birth registries in Mexico from 2008–2018. This information allows us to identify women living in municipalities where pre- (ages 3 to 5) and elementary (grades 1 to 6) schools were exposed to the FTS program and the degree of exposure in years. Our final sample includes information on 22 million births across 27,000 year-municipalities in Mexico. Our data allows us to explore heterogeneous effects by birth parity, poverty level, and mothers' education. To the best of our knowledge, this is the first study providing causal evidence on the effects of extending the school day, and thus closing the mismatch between the school- and workday on fertility decisions, along both the extensive (selecting into motherhood; first-time mothers) and the intensive margin (higher-order births).

We find that extending the school day reduces fertility. The average size of the effect is 3.4% in an 8-year span, or 0.5 births per 1,000 inhabitants (there were 5.9 births per 1,000 inhabitants at program baseline). While average effects are not discernible from zero at conventional statistical levels, when separated by first and higher-order births, the intensive margin becomes stronger and statistically significant: FTS led to a 7.8% reduction in the number of births for women who had already selected into motherhood and did not delay the transition into motherhood nor increased the probability of childlessness. We disentangle the effects of this policy along the extensive and intensive margins by comparing first to higher-order births

0.6 children per woman.

to better understand the mechanisms of childcare availability on overall fertility. This type of analysis is often absent from many fertility analyses (Aaronson et al., 2014), particularly for less-developed countries (where childlessness is less frequent). We find a sizeable negative effect of FTS at the intensive margin—concentrated in women with 2–3 prior births—and no effects at the extensive margin. As the observed reduction concentrates on higher-parity births among (non-first-time) mothers and in municipalities with more robust labor markets (this is, those with higher FLFP and female employment in the pre-adoption period), we interpret our results as driven by mothers being more likely to (re-)enter the labor market and reducing future pregnancies. This endogenous fertility adjustment to childcare availability may amplify the responsiveness of FLFP to FTS in exposed municipalities (Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020) in the long run, as it may lead to a "fertility multiplier" effect, as in Jakobsen et al. (2022), which should be considered when evaluating the policy's long term effects.

Our estimates are robust to dynamic effects and heterogeneous treatment results across municipalities adopting the FTS program at different periods. In particular, our results are robust to the rapid staggered adoption and potential heterogeneous treatment effects across schools and over time (Sun and Abraham, 2020; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). Moreover, given that when decomposing the effect found in a standard TWFE regression we find that most of the effect comes from always treated and timing observations, we implement the methodology proposed by de Chaisemartin and d'Haultfoeuille (2020) in order for our estimates to be appropriately specified.

Our paper contributes to the recent literature showing how the relationship between FLFP and fertility has reversed in more developed countries (Doepke et al., 2022) by extending it to a LIMC. Our results complement previous findings in more affluent settings, showing that the availability of alternative forms of childcare is critical in mediating the relationship between FLFP and fertility. Our findings suggest that, while childcare availability may reduce the direct costs of raising children and thus increase fertility rates in wealthier settings, in less developed contexts, it may also increase the opportunity costs of staying home, leading to increases in FLFP and thus reducing or delaying women's decision to have (additional) children.

These results directly relate to previous empirical evidence offering mixed results on the effects of childcare on childbearing (Kleven et al., 2019; Aguilar-Gomez et al., 2019; Attanasio et al., 2022) and to the literature offering mixed-effects of childcare availability on fertility (Del Boca, 2002; Hank and Kreyenfeld, 2003; Rindfuss et al., 2007; Mörk et al., 2013; Bick, 2016).[2] Hence, whether fertility rates increase or not due to childcare availability may differ substantially across varying socioeconomic contexts, baseline FLFP and fertility levels—especially so in developing nations where opportunity costs of specializing in childbearing and baseline FLFP may vary across women facing different income constraints and labor opportunities, or where more informal substitutes of formal childcare may exist. Furthermore, our findings contribute to the discussion on how childcare availability affects fertility decisions differently at the extensive and intensive margins—this is, it may affect the decision to enter into parenthood or the decision to have additional children in different ways, which will, in turn, have different implications for overall fertility. To he best of our knowledge, our paper is the first to analyze how extending the length of the school day and thus closing the mismatch between school- and workdays affects fertility.

Our paper also contributes to the studies on the effects of FTS beyond the more frequent analyses of the quality of education and equity in the short term.[3] For example, there is evidence linking the extension of school days to a reduction in teenage pregnancy and youth crime (Berthelon and Kruger, 2011), as well as delaying childbearing along with increases in the educational attainment of treated women Dominguez and Ruffini (2021). Finally, while most empirical research has been based on survey questions about women's perception of constraints to childrearing related to the lack of formal childcare as well as on information about the local availability and costs in developed countries, our paper differentiates by using rich administrative data, on the universe of births and exploiting a nation-wide policy.

The rest of this paper is organized as follows. Section 2 provides background information

[2]These differences in the literature are partially explained by variation in country-specific characteristics, such as baseline FLFP, overall socioeconomic status, and may also be affected by other endogeneity issues that are in often not addressed by the methodologies used (Wood and Neels, 2019). For example, in the US, formal childcare is typically private, with most research and debate addressing prices and quality determined by endogenous supply and demand. On the contrary, formal childcare is mainly public in Europe, and the debate around it has specifically addressed rationing or universal provision.

[3]See for example, Berthelon and Kruger (2011); Padilla-Romo et al. (2022); Cabrera-Hernández and Padilla-Romo (2020); Agüero et al. (2021).

on the FTS program. Section 3 describes the data used for our analysis. Section 4 presents the methods we use. Section 5 presents the main results. Section 6 presents the conclusion.

# 2 Background

## 2.1 The FTS Program

Mexico's elementary education system consists of three levels: preschool for ages 3 to 5, primary school for grades 1 to 6, middle school for grades 7 to 9, and high school for grades 10 to 12. Net enrollment rates increased sharply over the last two decades. However, the quality of education remains a challenge in many developing countries.

Starting in the 2007/08 academic year, the government created a FTS program aimed at improving education and promoting equity, which increased the length of the school day in public elementary schools that adopted the program.[4] An explicit secondary objective of the program was to support working mothers by acting as an implicit childcare subsidy. Participating schools extended their school day from four-and-a-half to eight hours, starting at 8:00 am. It was introduced in 500 elementary schools, and by 2017/18 it was present in more than 25,000 schools—approximately 25% of all elementary schools, reaching more than three million students all over Mexico or approximately 12% of total students.

Across years, the FTS program has generally targeted both urban and rural schools operating exclusively in the morning shift before the policy change, with low academic achievement observed in standardized tests, and in high-poverty areas. While targeting has been a challenge and schools from different socio-economic contexts have participated, the general rules for the selection into the program have been followed (see Padilla-Romo, 2022).

The staggered implementation of the program across space and time and its intensity is depicted in Figure 1. By 2018, the FTS gradually reached nearly 80% of Mexican municipalities, generating rich variation in the contexts in which the full-time schools were present.

[4] In Mexico, 92% of children are enrolled in public schools

## 2.2 Female labour force participation

Mexican women have steadily increased their labor market participation in recent decades,[5] although FLFP remains low compared to OECD countries(OECD, 2017). The lack of family-friendly policies may well explain the lower level of participation (Blau and Kahn, 2013; Cascio et al., 2015; Thévenon, 2016; Winkler, 2016). However, Mexico's spending on childcare and preschool has remained at around 0.6% of the GDP in the last decade, averaging 900 USD per child 0 to 6 years old, compared to the OECD average of 4,600 USD.[6] Only 4% of children ages 0 to 3 years are enrolled in a public or private formal childcare institution, while 75% of children aged 0 to 6 are cared for by their mothers, and 13% by direct family, regardless of mother's employment status.[7]

Finally, despite a relatively high net enrollment in preschool (80% in ages 3 to 5) and the practically universal attendance for children in primary education, before FTS implementation, short school days implied a relevant mismatch between the school- and workday with a direct negative effect on mothers' labor force participation (Cabrera-Hernández and Padilla-Romo, 2020; Padilla-Romo and Cabrera-Hernández, 2019). Therefore, the FTS program offers an ideal setup for analyzing changes in fertility decisions in the context of low public investment in childcare and low FLFP, below the country's potential, as is the case in many developing countries.

# 3 Data

We use two main data sources. First, we use administrative data from the Ministry of Education to create a variable that measures the introduction and exposure of municipalities to the FTS program. Second, we use data on the universe of registered births to create fertility measures at the municipal level.

Administrative data on FTS comes from Mexico's Ministry of Education from school year

[5]The Mexican population census indicates that women ages 18 to 65 participating in the labor force grew from 24.2% in 1990 to 45% in 2019

[6]These numbers are expressed in constant 2015 dollars and are according to the latest figures in the OECD Family database downloaded in April 2020.

[7]Data on childcare coverage, grandmothers' childcare provision, and payments come from the Survey on Employment and Social Security in Mexico (ENESS, 2017).

2008/9 to 2017/8,[8] and identifies preschools and primary schools that participated in the FTS program during the school year. We link this data at the municipality-level to municipal level measures of fertility. We construct fertility variables using administrative records on the universe of birth registries in Mexico for the calendar years 2008–2018 collected by Mexico's Ministry of Public Health as part of the Subsistema de Informacion sobre Nacimientos (SINAC). This is a novel dataset and rare for developing countries coming from a unique birth registry issued by all health institutions at the time of birth: a document that is required later on to legally register a birth and obtain a birth certificate (*acta de nacimiento*). Birth registries from SINAC have been found to closely follow Vital Statistics and Census trends (Mie y Terán Rocha and García Guerrero, 2019).

Our data include information for all births registered in a given school-year containing demographic details about the mother and births such as, mother's date and place of birth, municipality of residence, civil status and education. When aggregated at the municipal level, these data are a useful source of municipal level fertility, age-specific fertility rates, and allow us also to identify first and higher-order births. As the data is available at the individual level, it allows us to calculate total fertility for mothers with different characteristics (such as age or education).

To construct fertility rates, we combine this information with population figures at the municipality level to obtain the number of births per 1,000 of the population using data from census and census projections provided by the National Council or Population (CONAPO). This Council also provides the marginality index we use to build our poverty measure at the municipality level. Finally, we merge data from the National Survey on Occupation and Employment (ENOE) 2005–2007 that allow us to identify municipalities with more dynamic labour markets in the pretreatment period in terms of FLFP and female employment.

The descriptive statistics by treatment status at the municipality level are provided in Table 1. FTS municipalities have more births per 1,000 of the population, are less poor and are larger in terms of population. Similarly, mothers in FTS municipalities receive more prenatal consultations on average, they have a higher access to public health (a lower proportion gives birth

[8] In Mexico, the school-year starts in September and ends in June of the next calendar year.

in private hospitals) and they are more educated. While this table shows there exist some differences in the level of these variables, our identification strategy controls for these differences previous to FTS implementation and test for no different pre-trends in our main results, as we discuss in the next section.

Figure 2 shows the number of births per group of age in the school year 2008/9. This information is helpful to contextualize our results when separated by age and parity (that is, first-time and higher-order births). The data shows that most higher-order births concentrate on women aged 25–29, whereas first-order births are most common among younger women (aged 15–19 and 20–24). In terms of birth intervals, the average spacing between births is 3.6 years (median of 3) and decreases with family size (e.g., for women with two births, the average interval is 4.2 years, and it is 3.8 years for women with three children) (ENADID, 2018). Placing these numbers into context, a woman considering having a third child would likely have one child in preschool and the other in primary school.

# 4 Identification Strategy

To identify the effects of full-time schools on fertility decisions, we exploit variation in preschool and primary school adoption of the FTS program in different municipalities across Mexico. That is, we compare changes in municipal fertility (overall and by parity and mothers' age and education) between municipalities exposed to the full-time schools versus municipalities not exposed to the program, according to the timing of adoption of FTS at the municipal level. In our preferred specifications, we show dynamic effects using event studies.

We first estimate a two-way fixed effects (TWFE) regression to obtain average effects on fertility as defined by the equation:

$$Y_{mt} = \nu_m + \theta_t + \delta FTS_m + X_{mt}\beta + u_{mt} \tag{1}$$

where $Y_{mt}$ is the natural logarithm of one plus total births in municipality $i$ in academic year $t$; $\nu_m$ are municipality fixed effects; $\theta_t$ are academic year fixed effects; $X_{mt}$ represents average mothers' schooling in the municipality and municipalities' population; $u_{mt}$ is an error term that

we allow to be correlated within municipalities. Our variable of interest $FTS_m$ indicates the exposure to full-time schools for mothers in municipality $i$. The coefficient $\delta$ identifies the average effect of the FTS program on log-births. We cluster standard errors at the municipality level.

We further allow our model to capture dynamic effects and visually test for pre-trends with the following event study regression to obtain the impacts on fertility measures:

$$Y_{mt} = \nu_m + \theta_t + \sum_{\substack{k=-6 \\ k\neq -1}}^{8} \delta_k FTS_{m,t-k} + X_{mt} + \lambda_m t + u_{mt} \tag{2}$$

where $Y_{mt}$ is the natural logarithm of one plus total births in municipality $m$ and for total first and higher order births in academic year $t$;[9] $\nu_m$, $\theta_t$, $X_{mt}$ and $u_{mt}$, are defined as above. In our preferred specification we include municipality-specific linear time trends ($\lambda_m t$) to consider differential trends in births across municipalities. Similarly, as the program roll-out increasingly focused on poorer municipalities across time, we include poor-by-year fixed effects that help compare municipalities within the same year in poorer and non-poor municipalities. Our variable of interest, $FTS_{m,t-k}$, indicates the degree of exposure to full time schooling in $k+1$ years for mothers in municipality $i$ in academic year $t$. For $k \geq 0$, this is an indicator equal to one $k$ years after the municipality opened its first FTS and zero otherwise. We test for pre-trend differences between municipalities for $k < -1$, as $FTS_{m,t-k}$ is equal to one for treated municipalities and zero otherwise in the pre-treatment period. We also assume that the effects are constant for $k \leq -6$ and for $k \geq 8$. The coefficient $\delta_k$ identify the average effect of $k+1$ years of implementation of the FTS program on births.

Given the program's staggered implementation and the possibility of heterogeneous treatment effects across groups and over time, we estimate our main Equations 1 and 2 using the estimation method proposed by de Chaisemartin and d'Haultfoeuille (2020). These estimates are robust to both dynamic and heterogeneous treatments effects across groups exposed to the FTS program at different time periods under standard common-trends assumptions. We prefer

[9]We use the $log(1+y)$ to proxy for the log transformation in order to interpret coefficients as percentage changes whilst dealing with zero-valued observations. Results are robust to using alternative transformation such as the quartic root, the inverse hyperbolic sine (IHS) (of the form $sinh_x^{-1} = \ln(x+\sqrt{1+x^2})$ and birth rates per 1,000 population, as presented in the Appendix Table A3

these estimates to other alternatives as the Goodman-Bacon (2021) decomposition of the 2-by-2 canonical comparisons driving the weighted difference-in-differences estimator come mostly from "forbidden comparisons", which account for a total weight of 57.9% (always treated versus timing (11.7%), early vs late (15.9%), and late vs early (30.3%)). These represent "forbidden comparisons" that bias the DD coefficient (see Table2 and Figure A1). In this set-up, de Chaisemartin and d'Haultfoeuille (2020) is adequate as it estimates the effects of being in a municipality that has adopted the FTS program in academic year $t-k-1$ as compared to municipalities that have not yet adopted the program in academic year $t$.[10] As a robustness check, in section 5.2, we also present the results for our main fertility outcomes using the method of DD imputation proposed by Borusyak and Jaravel (2017) and the Interaction-Weighted (IW) estimator proposed by Sun and Abraham (2021).

# 5 Results

Table 2 show the average effects on the program from estimating equation 1. Column 1 presents the canonical TWFE model without controls. Column 2 adds the following controls: municipalities' population, average mothers' schooling, poor-by-time fixed-effects and a municipality specific time trend. Column 3 uses the FTS predicted share of seats as an average estimate of the intensity of treatment.[11] Column 4 presents the average effect using the de Chaisemartin and d'Haultfoeuille (2020) estimator with no controls. Column 4 shows our preferred estimation adding controls and fixed-effects and the municipality specific time-trend. As discussed, TWFE estimates can be biased if the effects are heterogeneous in time or across municipalities exposed to the FTS program for the first time in different years. In this regard, Column 1

[10] We estimate the long-difference placebos as proposed by (de Chaisemartin and d'Haultfoeuille, 2020) instead of first difference placebo estimates testing common trends over pairs of consecutive periods. Long-difference placebos test the presence of common trends along several periods, and thus different trends in births are more likely to be detected.

[11] We avoid endogenous changes in schools capacity and the supply of FTS seats across time by estimating the predicted share of available seats following Padilla-Romo and Cabrera-Hernández (2019), where FTS predicted seats in a given school year are computed as follows:

$$FTS_{my} = \frac{\sum_{s \in m} \bar{e}_s FT_{sy}}{\sum_{s \in m} \bar{e}_s} \tag{3}$$

where $\bar{e}_s$, our proxy for school capacity, is the pre-program (2001-2006) average enrollment of school $s$; and $FT_{sy}$ is a dummy variable indicating whether school $s$ has adopted the FTS program in year $y$ or earlier.

also includes the Goodman-Bacon (2021) decomposition of the 2-by-2 comparisons showing that timing groups drive most of the average "naive" TWFE estimations (weights in squared brackets).[12] This decomposition provides a diagnostic test to examine the extent to which the staggered implementation of the FTS program is likely to bias the TWFE estimates. For example the always treated vs. timing comparison biases our estimation upwards (4.5 percentage points with a weight of 11.7%). Ideally, we want to compare students exposed to FTS to non-exposed students (never-treated or not yet treated). The "forbidden comparisons", represented by timing groups, are effectively avoided in columns 4 and 5. Our preferred estimation shows an average reduction on fertility of 3.3% that is not significantly different from zero at conventional levels.

Panel (a) in Figure 3 shows the change in the predicted capacity of FTS in a municipality after FTS adoption (the coefficients for the years previous to program adoption are zero by construction). On average, during the first year in which a municipality implements the program, the predicted FTS availability increases almost 18 percentage points. The effect reaches its maximum of 34 percentage points in the sixth year after the first FTS school opened, with an average of 27 percentage points. In this context, the effect in Column 2 of Table 2 would suggest an impact of the predicted share of seats of $(0.079 \times 0.27)$ for the average increase in FTS seats, a reduction in fertility of 2.1 percentage points. Yet, this estimation may be subject to biases caused by heterogeneous and dynamic effects.

Panel (b) in Figure 3 presents our preferred estimation coming from Equation 2 for all births independent of parity, including average mothers' schooling and municipalities' total population as controls; municipality-specific trends; and poor-by-year fixed effects. The results show an overall negative impact on births that is not statistically discernible from zero. The point estimators show that, on average, over the 8-year span after treatment, the estimates point to a reduction of 3.3% or nearly 0.5 less births per 1,000 of the population (from 15.9 at program start) or 65,500 less births from a base of 1,921,000. While, qualitatively, a clear decrease in fertility following program introduction is observed in the figure, point estimates are not statistically significant when looking at overall birthrates.

[12] These groups are Always vs. timing (weight of 0.116), Early vs. Late (0.0158) and Late vs. Early comparisons (0.302), that in sum explain 57.8% of the TWFE estimator.

We explore whether there are statistically significant effects on fertility driven by the extensive margin (i.e. women postponing or selecting out of motherhood) or the intensive margin (i.e., having further children given that a woman already has a child) to shed light on the possible mechanisms behind the FTS impact.

We first compare the effects of FTS using the aggregate number of first-order births (these are 36% of the base year total) at the municipality level to the effects on those of higher order. Figure 4 shows our estimations for the first births in Panel (a) and higher order births in Panel (b). This "intensive vs. extensive margin" estimations suggest that the effects concentrate in higher order births (intensive margin) rather than in women entering motherhood (extensive margin).[13] Hence, childcare availability in the context of FTS could be causing a reduction or, at least, a delay in mothers' decision of having more children.[14] The average reduction of 7.8% on higher order births, roughly translates into a fall of 96,000 births. Note that, in the pre-treatment period, according to Figure 3, the effects remain stable and around zero, and there is a change only one year after implementation. This offers support to our identification strategy.

To explore whether mothers are delaying having children or reducing their total pregnancies, we proxy mothers' number of children by using the reported order of birth in our mother-level dataset. Panel (a) in Figure 5 shows a significant reduction in birth order, suggesting that mothers exposed to the program have fewer children. Similarly, Panel (b) presents the results on the log-births at the municipality level for children with different birth orders. These results imply that the effect concentrates on reducing third (representing 19% of total births in the base year), fourth (these are 9% of the total) and higher order births.

These effects, concentrating on higher-order children, may also suggest that the change in mothers' preferences happens after the first child has been treated a number of years (or is old enough to enter pre- or elementary school). Considering that children can potentially enroll in an FTS school at the age of three and that, according to our data the average mothers' age for first and second-order births is of 22 and 25 years, respectively, the first child, on average, would not have been treated by the time a second child is born. Hence, it is natural that the

[13] In Appendix Figure A2 we present the dynamic effects on both, first and higher order births, excluding the always treated group. The results are fairly similar.

[14] Similarly, in Appendix Table A3 the results with alternative measures: Inverse Hyperbolic Sine (IHS) and Births per 1000 of the Population, show similar results and the same dynamics for first and higher order births

effect appears only when a third and higher-order birth has happened. This is at an average mother's age of 28 years and older when the first child has potentially been enrolled in an FTS for at least three years. A back-of-the-envelope computation with the results by order of birth suggests that in the absence of the program, births of third and fourth order would have been, on average, 6.5% and 12.5% higher, or 24,320 and 22,187 more births, respectively.

Exploring the effects across different mothers' age further, Figure 6—which graphically presents the post-period coefficients from regressions using de Chaisemartin and d'Haultfoeuille (2020) for births to women of different age-groups—shows that the impacts on total births are stronger for women 15–19 and 30–34 years old.[15] Furthermore, heterogeneous effects by first- and higher-order children also reflect that childcare effects on reducing the transition into motherhood are not significant for any group of age.

In Figure 7 we focus our attention on women of age 19 to 24 and between 30 and 34—where the largest effects shown in Figure 6 were concentrated—and show the dynamic effects of the exposition to the program. For these subgroups of women, pre-trends are flat and close to zero in magnitude and the effects appear to be increasing in time.

For women aged 15 to 19, while there does not appear to be an effect for first births, the average reduction in higher order births represents a fall of 13.1% from the base year (around 13,000 births from a base of 98,534). For women 30 to 34 years old, there is also no apparent effect for first births, while the average fall in higher order births corresponds to a fall of 9.9% with respect to the year before program implementation (or approximately 26,650 births from a base of 266,480).[16] The results for women 30–34 suggests that the overall decline in fertility we observe is consistent with a stopping of childbearing, plausibly due to increases in FLFP which may in term change women's preferences over the desired number of children, access to contraception, and empowerment. We explore this hypothesis in section 5.1.

[15] It is not uncommon for teenagers in Mexico to have more than one child before turning 20: second and higher-order births represented more than 25% of all births to adolescents in the early 2000s (Sánchez-Pájaro et al., 2019).

[16] We do not compute these changes in terms of births per 1,000 of the population as there are no available statistics on population sizes by groups of age and school-year.

## 5.1 Heterogeneous effects

To better understand the potential mechanisms driving the observed fall in fertility for higher parity births, we explore whether effects are different according to the type of municipality (e.g., poorer municipalities or municipalities with higher FLFP) and mother characteristics (e.g. schooling). For instance, we hypothesize that if the fall in fertility is driven by women returning to the labour force due to FTS, the effects should be higher in municipalities with stronger labour markets (that is, higher FLFP and female employment).

Figure 8 refers to the post-period heterogeneous effects of the FTS introduction in relatively richer and poorer municipalities and for municipalities where the average mothers' education is above and below the national median. We hypothesize that effects should be larger in poorer municipalities as a larger share of the population uses the public school system, where FTS operates (and similar for less educated women that are more likely to send their children to public as opposed to private school). While the direction of the average effects in higher order births is the same for all subgroups, we observe that the effects are significant in both, richer and poorer municipalities and for less educated women. Figure 9 shows these effects across time and suggest a similar statistically significant cumulative effect close to 30% for poorer and less educated women, with an average of 9.1% and 8.3%, respectively and for the 8-year period. These are reductions close to 0.85 higher order births from bases in the adoption year of 9.3 and 10.6 per 1000 of the population for poorer and less educated women, respectively.[17]

These effects directly relate to the previous evidence regarding the positive impact of the FTS program on FLFP among less educated and poorer women (Padilla-Romo and Cabrera-Hernández, 2019)[18]. They also relate to the evidence regarding the FTS positive effects on grandmothers' LFP as, in the Mexican case, grandmothers are the main caregivers when the mother works (Cabrera-Hernández and Padilla-Romo, 2020). In our case, the program could be having a stronger effect on fertility in municipalities with a more dynamic labour market, where women have higher chances to find a job and update their fertility decisions accordingly. If this is true, it could partially explain why they reduce higher order births. We explore this in

[17] Dynamic effects for total births and for first born children can be consulted in Figures A7 and A8 in the Appendix.

[18] The authors found an increase of 5.5 pp in labour force participation from a base of 44%

Figure 10 using the data from the National Occupation and Employment Survey (ENOE).

Figure 10 shows the effects on log-births including the effects on the transition into motherhood and for higher order births, separated for the sub-sample of women in municipalities with low- and high-FLFP and female employment for the three school-years before policy adoption (that is, between the third quarter of 2005 and the third quarter of 2007). As we hypothesized, results suggest that the effects at the intensive margin fully concentrate in municipalities with higher FLFP and female employment in the pre adoption period.

Figure 11 presents the results on higher order births for the dynamic changes after policy adoption. We observe a cumulative effect close to 30% in municipalities with stronger labour markets for females, with average effects between 6.4% and 7.4% in municipalities with high FLFP and high female employment, respectively, across the 8-year span of analysis. These changes translate into reductions close to 0.9 births per 1,000 of the population from a base of 11 or a total reduction of approximately 47,000 and 56,000 births, respectively.

## 5.2 Robustness Checks

Figure 12 shows the results for the first-born and higher order births using DD imputation (Borusyak and Jaravel, 2017) and the IW estimator (Sun and Abraham, 2021). The results are fairly similar on average and have the same dynamics, with a cumulative effect across time of the extended school days on higher order births, and no discernible effects on mothers of all ages transitioning into motherhood with cumulative effects close to a 10-15% reduction after 8 years of implementation.

Furthermore, given the positive effects of the program on FLFP documented in the literature, there could be an endogenous migration of working mothers towards FTS municipalities. These women may have an ex-ante lower preference for children. In such case, the effect we document, moreover for higher order births, may be explained by selective migration. To address this concern, we use mother's municipality of birth to create a variable that denotes whether at the time of birth a mother resides in the same municipality in which they were born (this is approximately 52% of our sample) or if they have moved to a different municipality.

While we do not know the time in which the mother has moved, we can use this defini-

tion to broadly classify them as migrant and non-migrant mothers and conduct the regression specification in Equation 2 for each subgroup. The intuition is that if endogenous migration happened due to the FTS program and migrating mothers have different fertility preferences, the effects would concentrate among them. The results presented in Panels A, B and C in Table 3 correspond to the log-transformed overall, first, and higher-order births, respectively—depicted graphically in Figure 13, suggest that even when the effects of the policy on migrant mothers are larger, these are not significantly different to that of non-migrant mothers, and that the pattern of a reduction in higher order births with no effect on first pregnancies, is the same for both groups of mothers. In this set-up, we could argue that effects on non-migrant mothers can be considered as a lower bound of our intent-to-treat estimates. Similarly, Figure 14 shows the dynamic effects of the program, being also significantly similar between migrant and non-migrant mothers across time.

# 6 Conclusions

By exploiting the staggered roll-out of full-time schools in Mexico, we find that increased access to the FTS program is associated with strong and significant negative effects on fertility and that effects are concentrated along the intensive margin. These effects are mostly driven by young and middle-age mothers who already have at least one child and by low-educated women. Effects are also concentrated in municipalities where we identify a more vibrant labor market for women. We interpret these results as being driven by greater FLFP of mothers in municipalities with access to the FTS program, that leads them to stop fertility earlier.

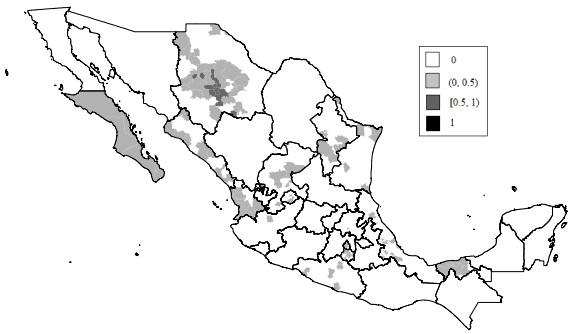
Our paper fills a gap in the literature analyzing the causal effects of increased access to childcare on fertility, both in terms of analyzing the effects along both the intensive and extensive margin and in being, to the best of our knowledge, the first study providing causal evidence on the effects of closing the mismatch between the school- and workdays on fertility decisions. Our work is also relevant as it shows evidence of an endogenous fertility adjustment to childcare availability, that may in turn may amplify the responsiveness of FLFP to FTS in exposed municipalities (Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo,

2020) in the long run by decreasing fertility, acting thus as a "fertility multiplier" (Jakobsen et al., 2022). Additionally, we analyze this question in the context of a middle-income country, Mexico, where there may be more uncertainty on how the forces highlighted by first-generation models vis-a-vis those emphasized in the new era of fertility economics will affect fertility decisions.

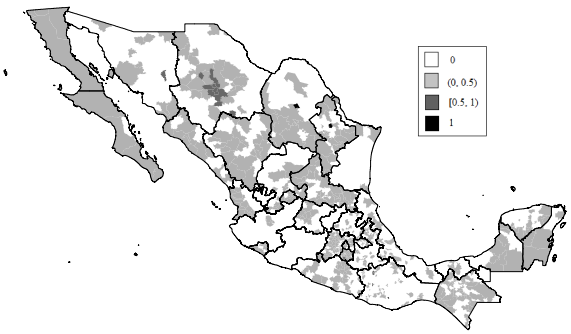
We believe that our results may inform the increasingly relevant and popular policy debates regarding how to increase fertility and how a policy—such as a subsidy on childcare for pre- and primary schools—may affect women that have already entered motherhood and those that may consider becoming parents differently. Additionally, we think that the case of Mexico will be informative for other LIMC transitioning to lower fertility, but where fertility levels are still relatively high.

Figure 1: FTS Staggered implementation across Mexican Municipalities 2007-2018

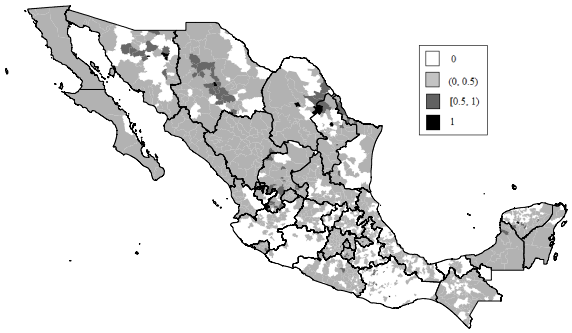
(a) 2007



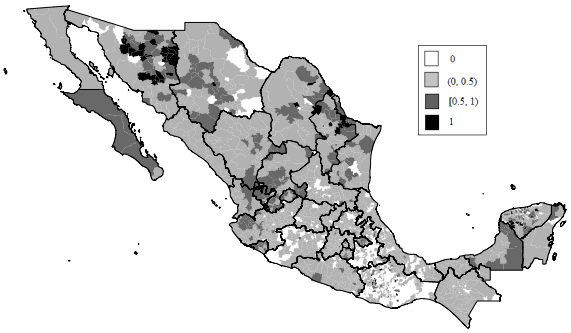

(b) 2009




(c) 2012




(d) 2018




Notes: The maps show the proportion of FTS pre- and primary schools out of the total schools per municipality in each year. The program started in 500 schools in 2007 reaching half of the 32 states in Mexico. By 2018 80% of all municipalities had at least one FTS present across all states in the country.

Figure 2: FTS Thousands of Births per Group of Age in the School Year 2008/9

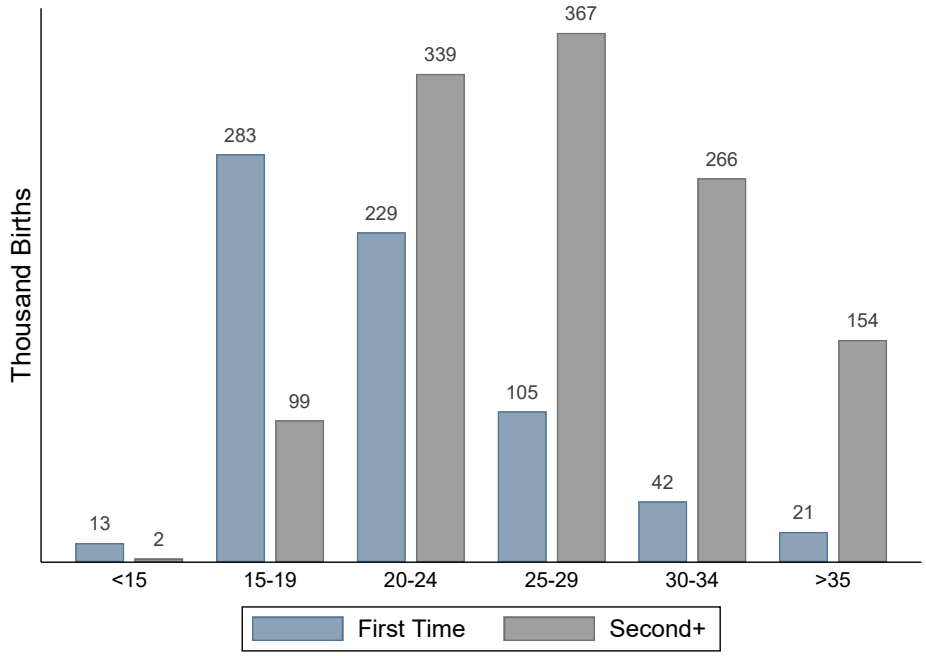
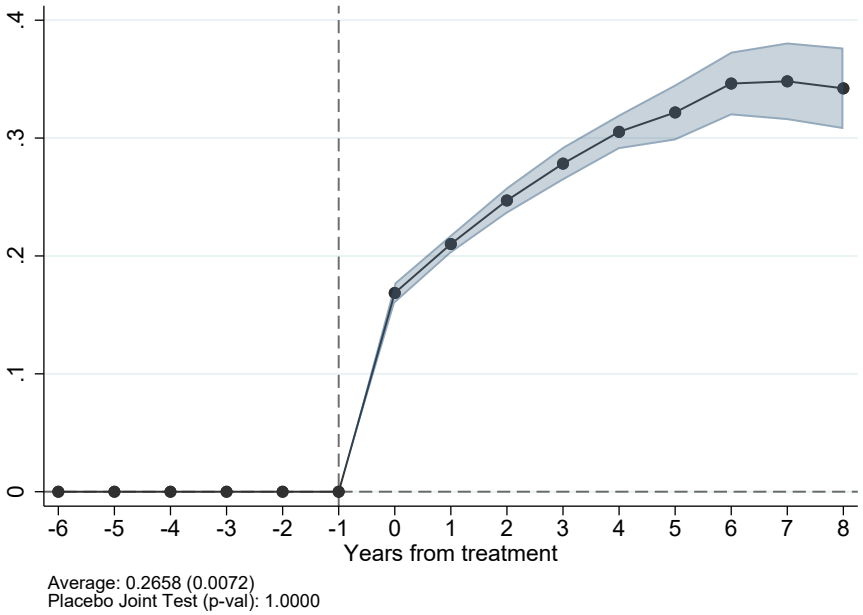
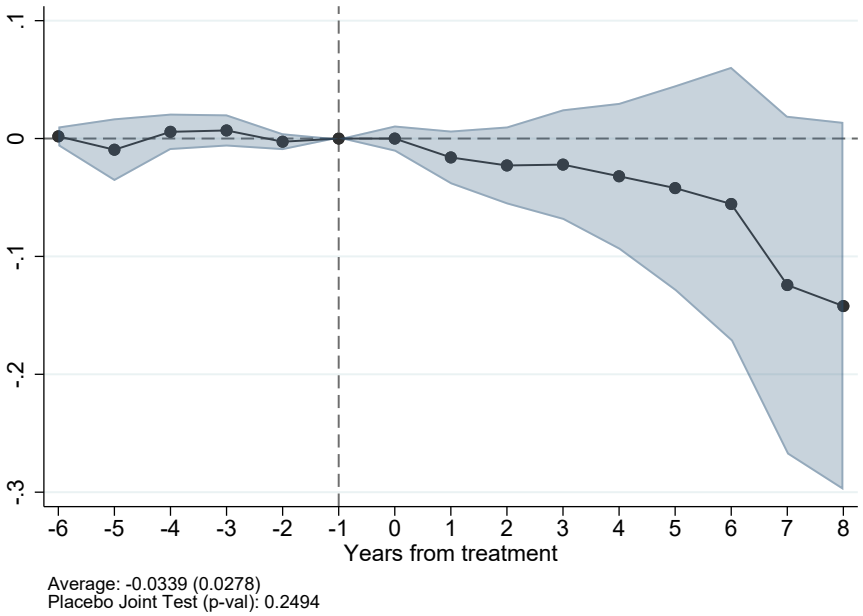


Figure 3: Estimated Effects on Municipalities' Predicted Full-time Schools Availability and Fertility Rate

(a) Average Effects on Municipalities' Predicted Full-time Schools Availability



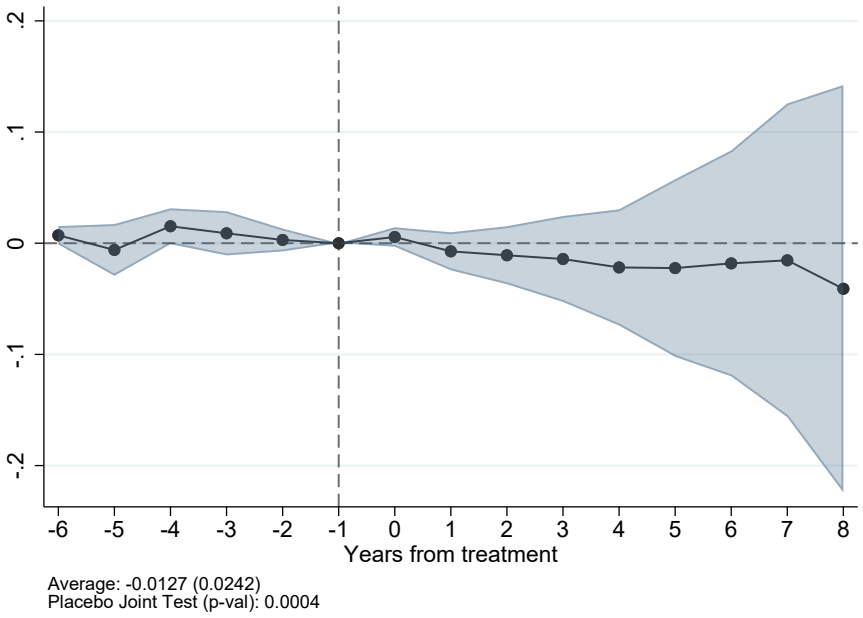

(b) Average Effects on Births



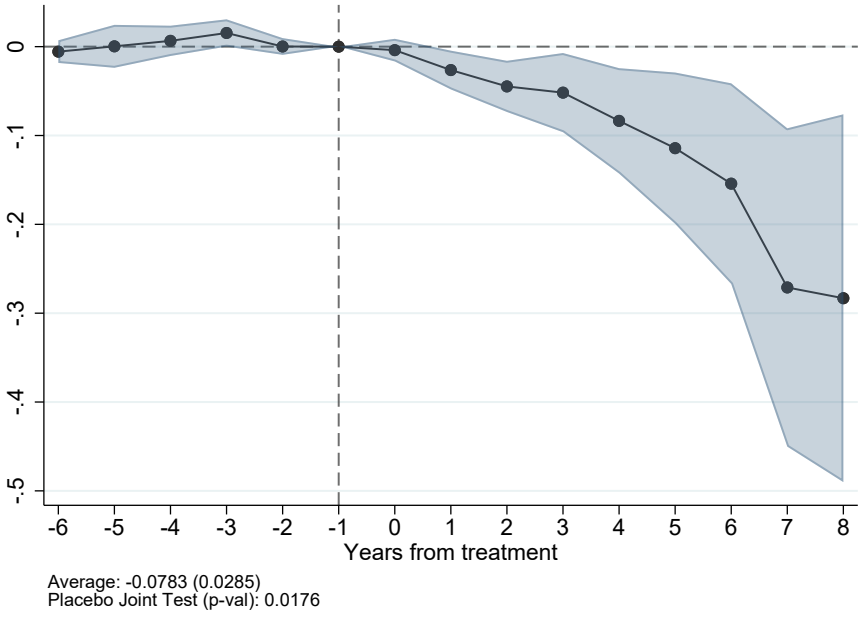

Notes: Panel(a) shows the evolution of the predicted share of Full-time Schools seats in a municipality after its first expansion. Panel(b) shows the estimated effects of Full-time Schools' availability on the logarithm of births plus one and their 95% confidence intervals for indicators of the years prior and after municipality adoption of the FTS program. All estimates come from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, and municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 4: Intensive vs. Extensive Margin

(a) First-Born



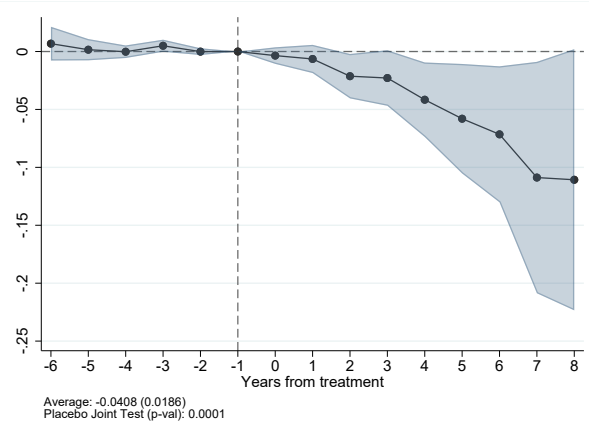

(b) Second+ Child



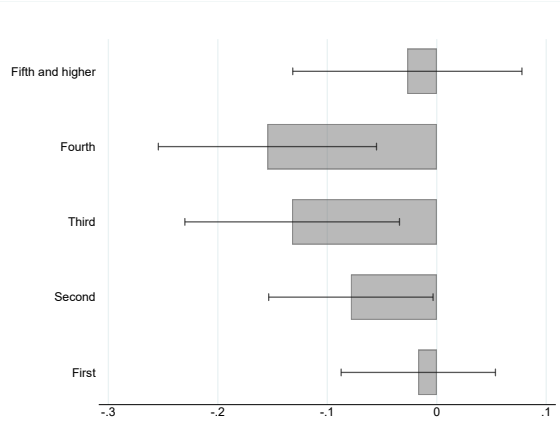

Notes: Panel(a) shows the effect of Full-time Schools on first order births. Panel(b) shows the estimated effect on higher order births. Estimated coefficients include their 95% confidence intervals for indicators of the years prior and after municipality adoption of the FTS program. All estimates come from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, and municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 5: Estimated Effects on Order of Birth, on Log-Births by Order of Birth and on Mothers' Age.

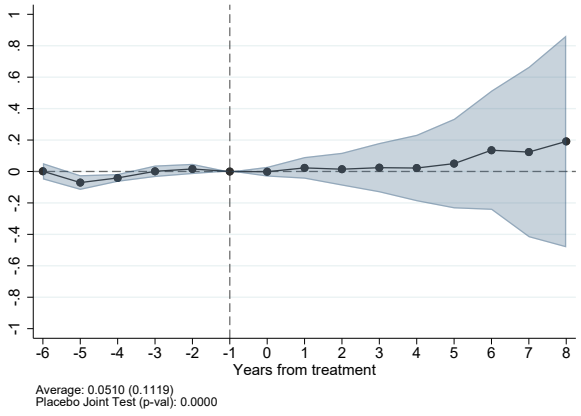
(a) Order of Birth




(b) Log-Births by Order

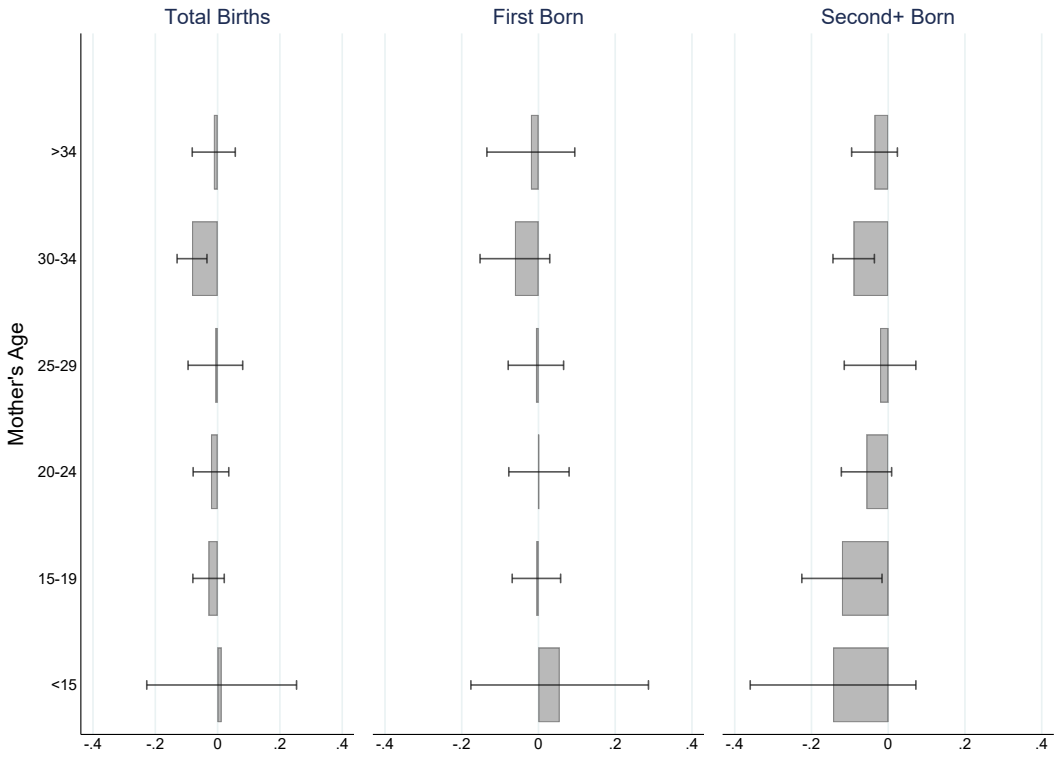



(c) Mothers' age




Notes: Panel (a) shows the effect of Full-time Schools on mother-level reported child's order of birth. Panel (b) shows the estimated effect on the log-births at the municipality-level by order of birth. Panel (c) shows the estimated effect on mothers' age at birth. Estimated coefficients include their 95% confidence intervals for the average effect after municipality adoption of the FTS program and for indicators of the years prior and after municipality FTS adoption. Each estimate comes from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for mothers' education, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

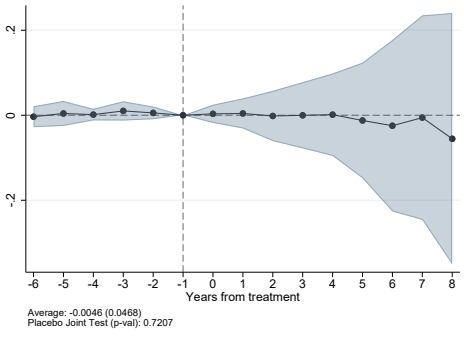
Figure 6: FTS Average Effects on Births by Mother's Age



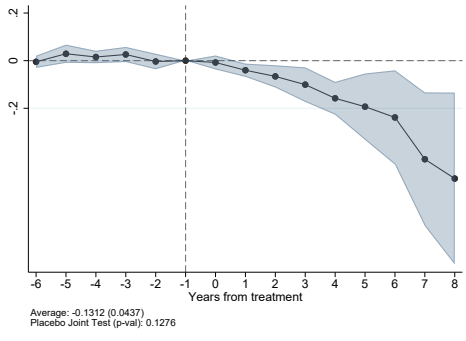

Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 7: Dynamic effects by selected groups of age

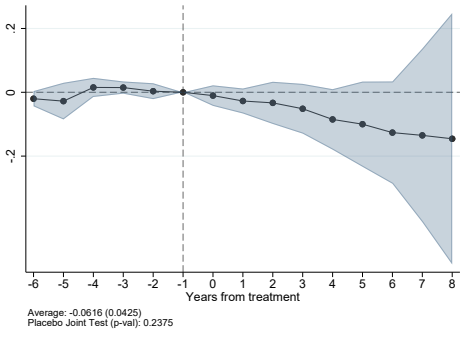
(a) First-Born (aged 15 to 19)



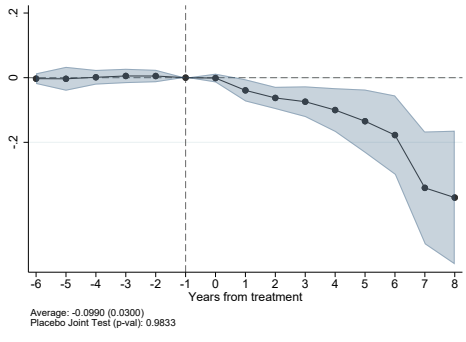

(b) Second Child+ (aged 15 to 19)




(c) First-Born (aged 30 to 34)

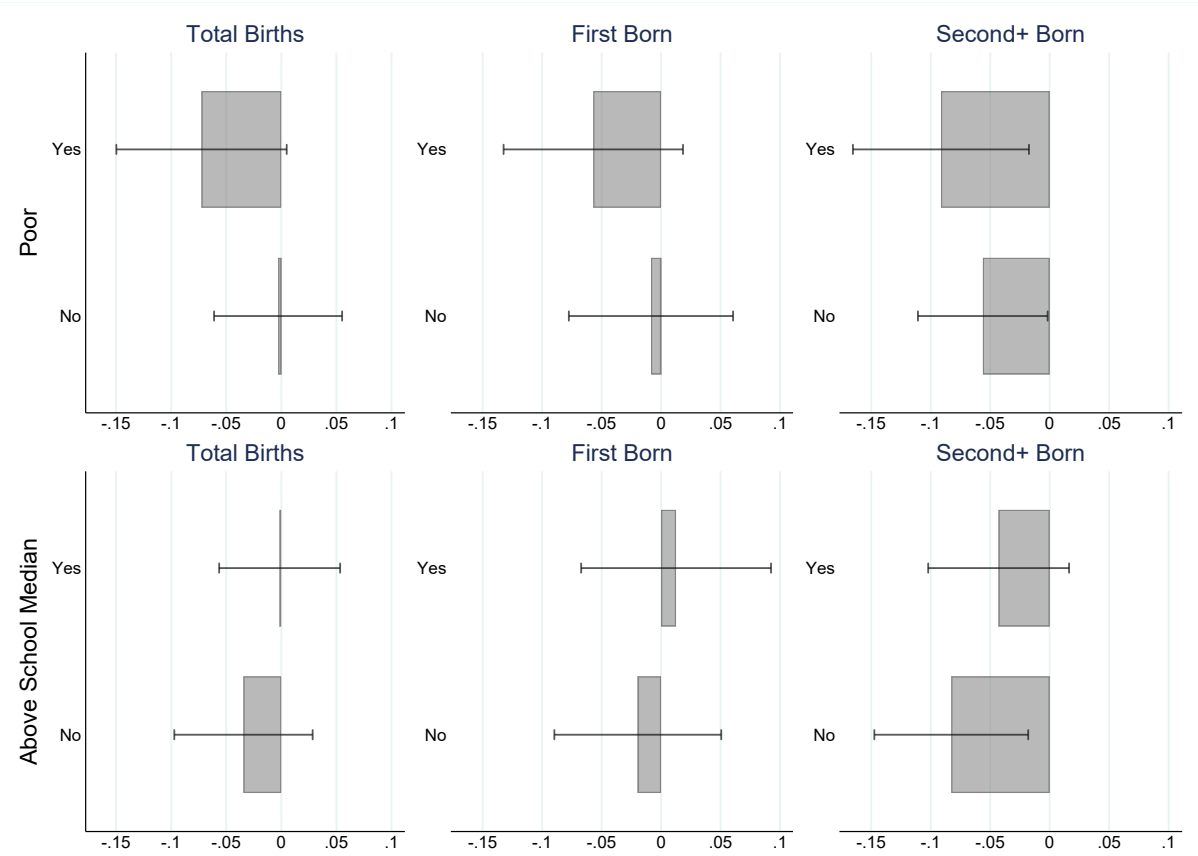



(d) Second Child+ (aged 30 to 34)



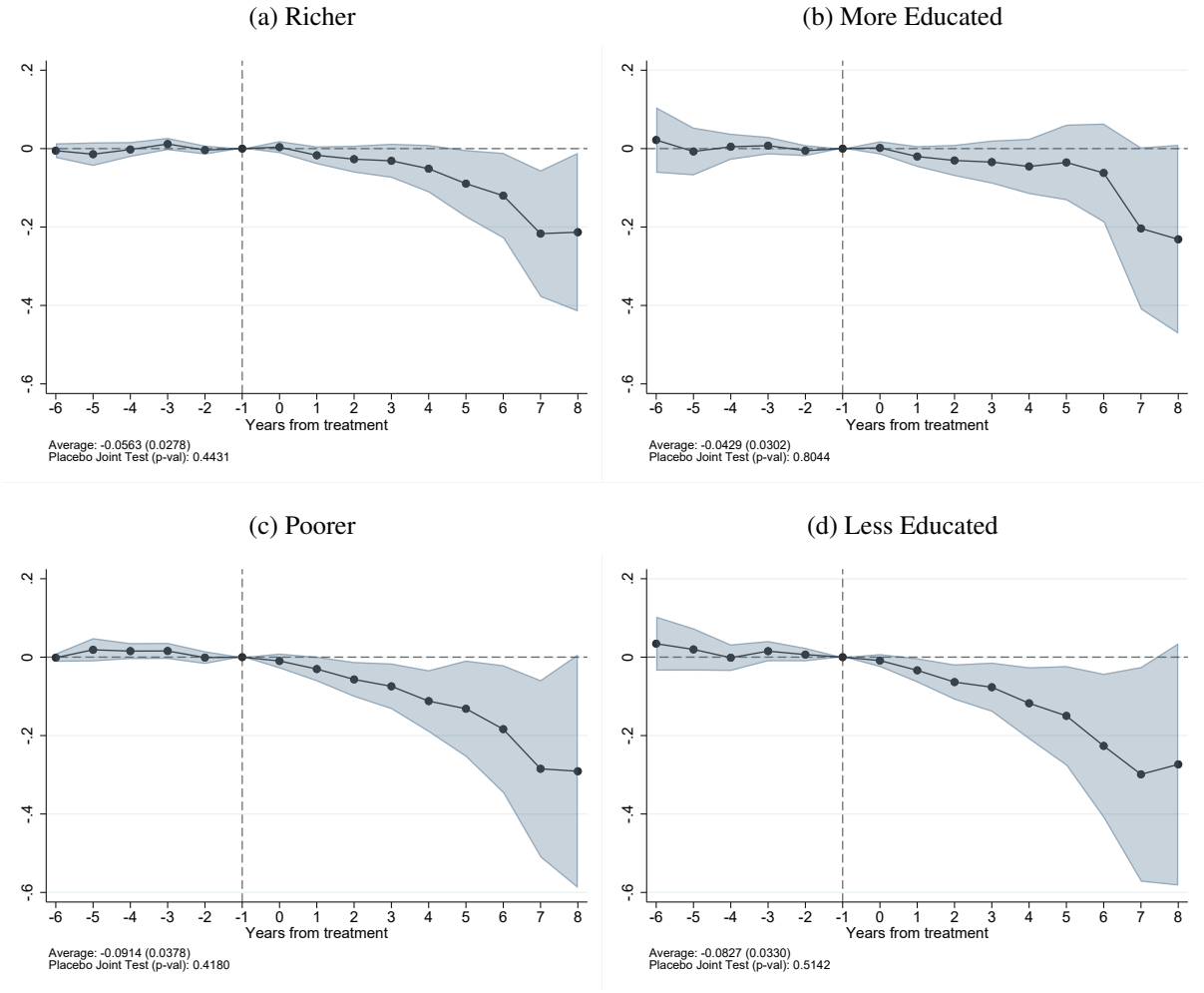

Notes: Each panel shows the estimated dynamic effects of FTS on first and higher order births for each subgroup of mothers' age. Estimated coefficients and their 95% confidence intervals are included for indicators of the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' education, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 8: Log-Births by mother's education and municipalities' poverty



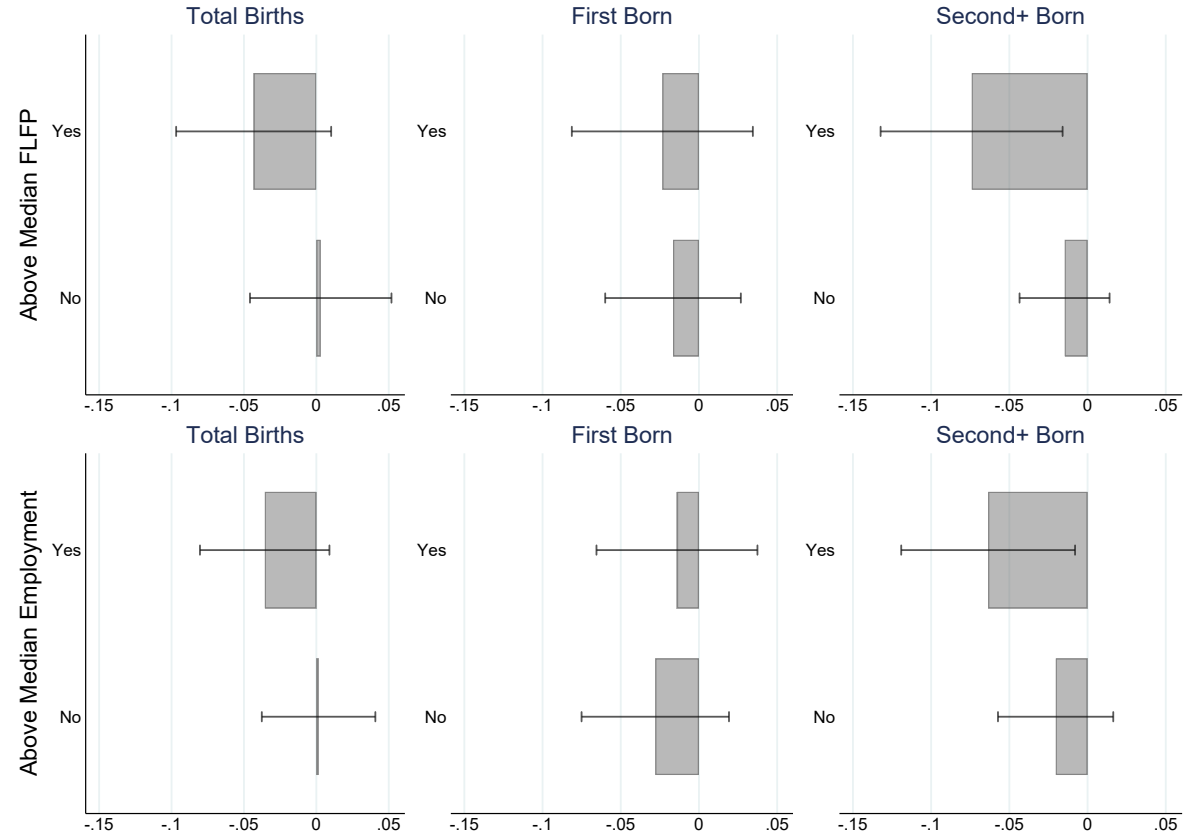

Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible, for municipalities' average mothers' education, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 9: Dynamic effects: Second+ Child by poverty and education levels




Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' education, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

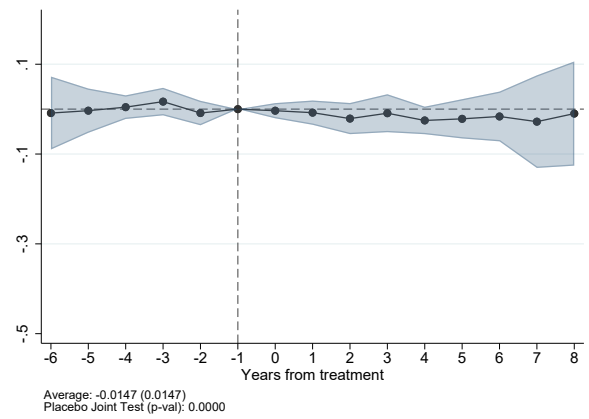
Figure 10: Log-Births by municipalities' employment and FLFP



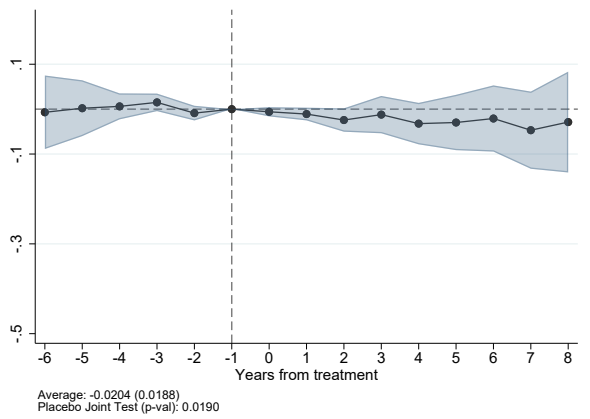

Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 11: Dynamic effects: Second+ Child by FLFP and Employment

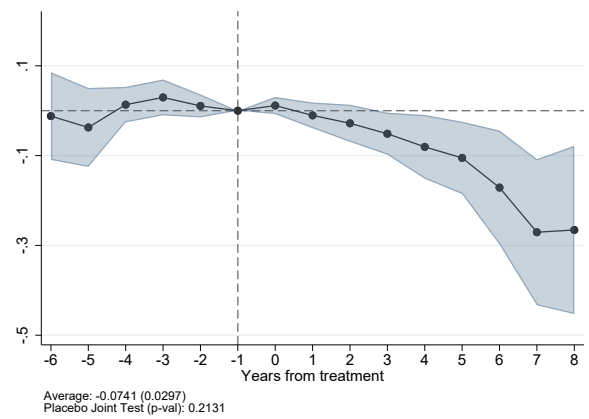
(a) Below Median FLFP



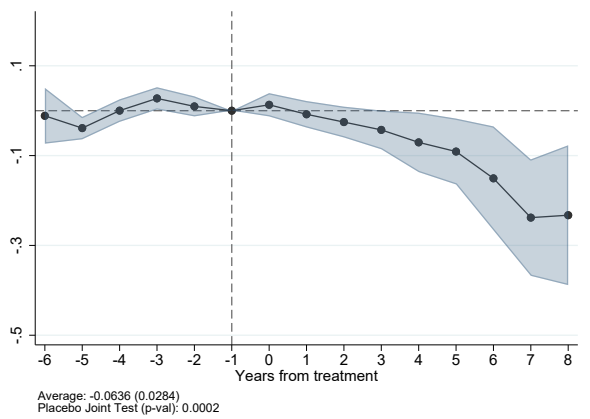

(b) Below Median Employment




(c) Above Median FLFP



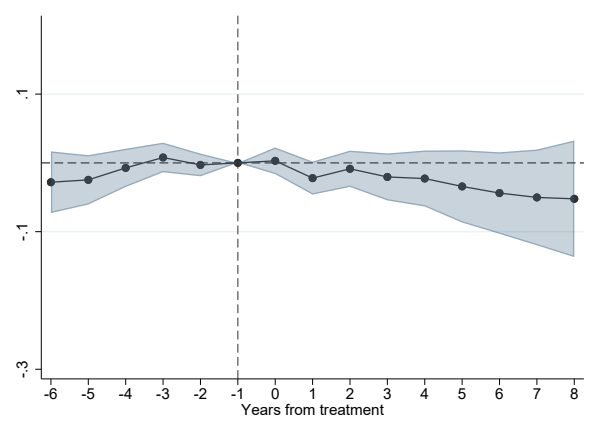

(d) Above Median Employment



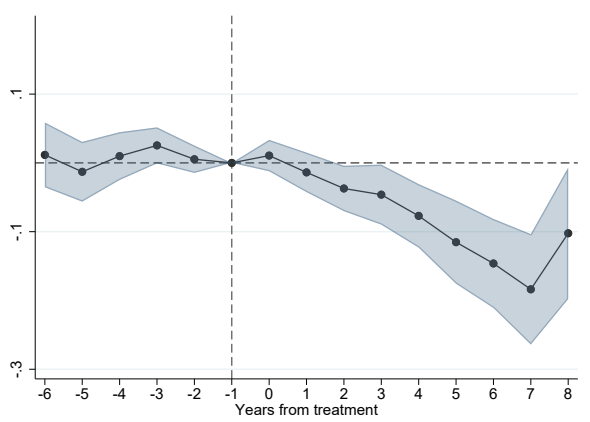

Notes: Estimated coefficients and their 95% confidence intervals for indicators of the years prior to and after the municipality adopted the FTS program. All estimates comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 12: FTS Effects on Fertility using alternative methods

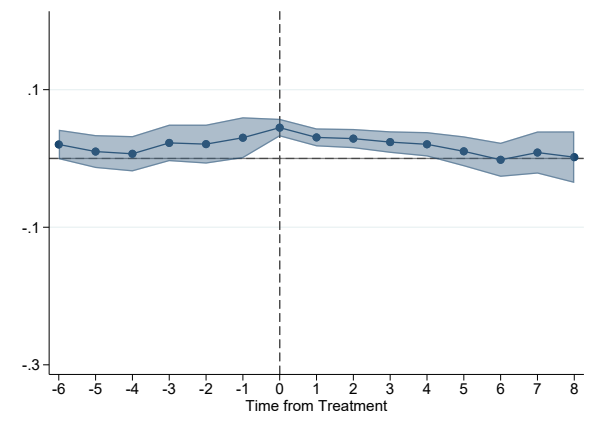
(a) IW estimator First Born



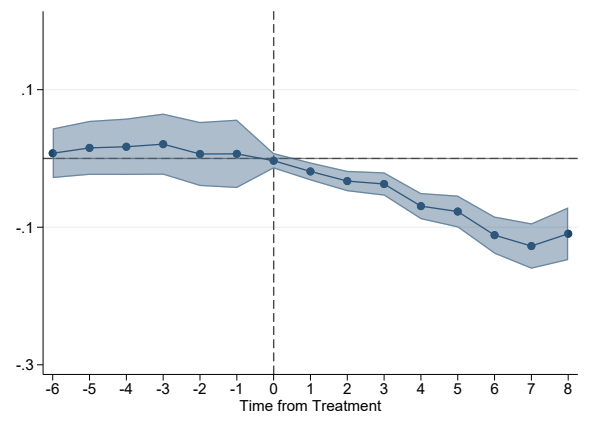

(b) IW estimator Second+ Child




(c) DD Imputation First Born

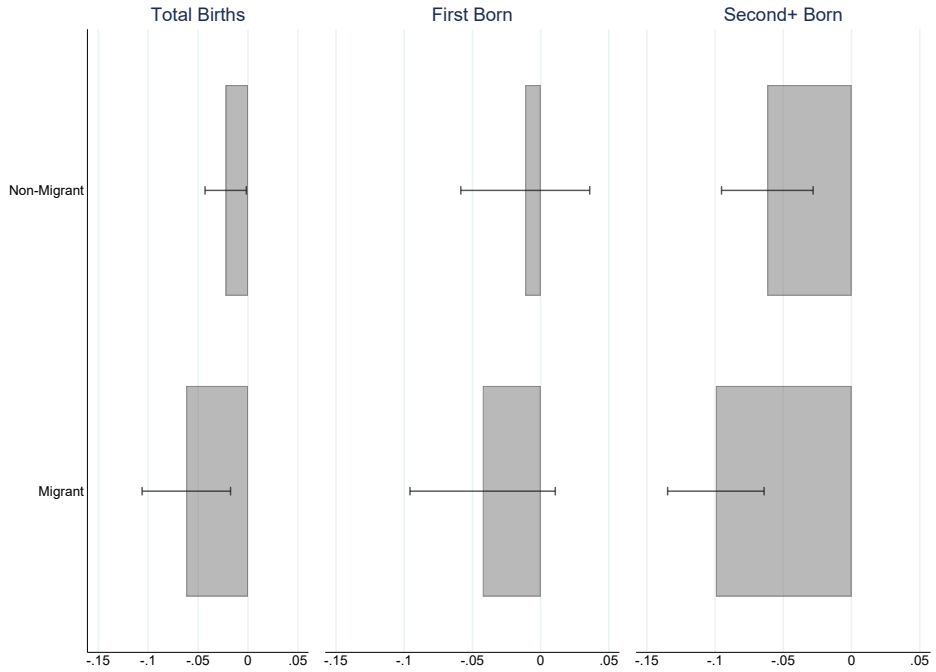



(d) DD Imputation Second+ Child




Notes: Estimated coefficients and their 95% confidence intervals for indicators of the years prior to and after the municipality adopted the FTS program. All estimates comes from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

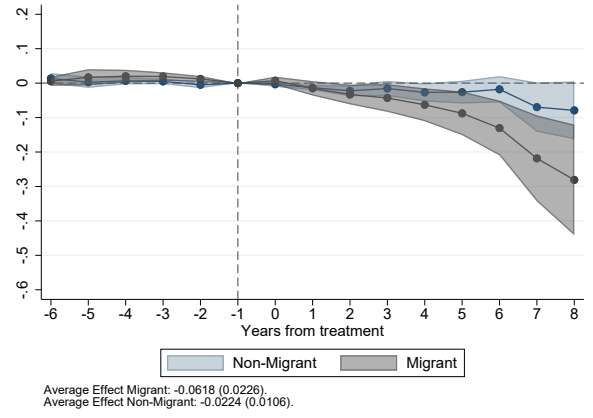
Figure 13: FTS Effects on births: migrant vs. non-migrant mothers



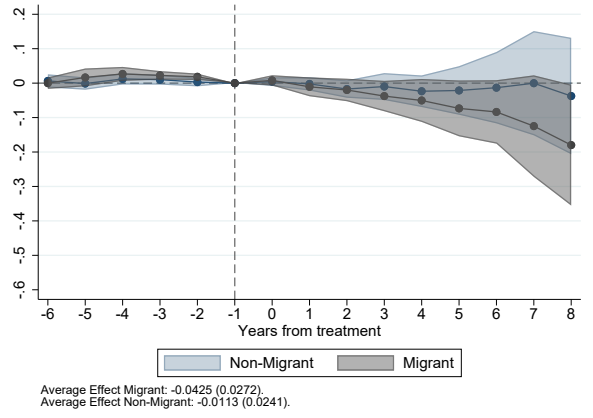

Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for mothers' average education, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure 14: FTS Effects on Fertility: Migrant vs. Non-Migrant mothers

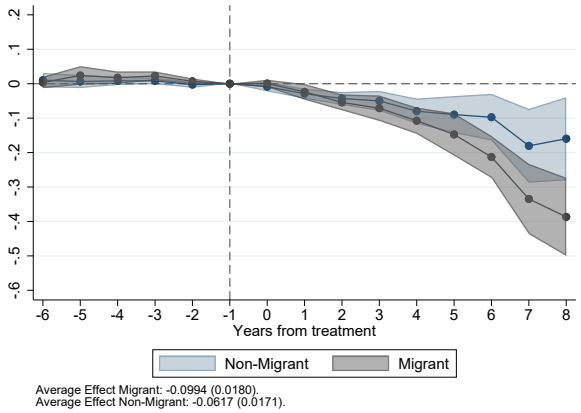
(a) Total Births




(b) First Born




(c) Second+ Child




Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression controlling for mothers' average education, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Table 1: Main Descriptive Statistics

| | Non-FTS | | Ever Treated | | | |
|---|---|---|---|---|---|---|
| | (1) Mean | (2) Std. Dev. | (3) Mean | (4) Std. Dev. | (5) Diff. | (6) t-test |
| *A. Municipality-Level* | | | | | | |
| Births per 1000 | 15.72 | 6.07 | 17.21 | 4.77 | -1.49 | (-22.14) |
| First Child per 1000 | 5.67 | 2.45 | 6.17 | 1.90 | -0.50 | (-18.47) |
| Second Child per 1000 | 10.05 | 4.12 | 11.04 | 3.22 | -0.99 | (-21.73) |
| FTS Predicted Share | 0.00 | 0.00 | 0.26 | 0.27 | -0.26 | (-113.37) |
| Poor Municipality | 0.60 | 0.49 | 0.39 | 0.49 | 0.21 | (35.49) |
| Total Population | 21004.54 | 60631.09 | 71766.51 | 170392.81 | -50761.97 | (-32.77) |
| Population aged 3-12 | 4647.85 | 11486.95 | 14009.89 | 29813.77 | -9362.04 | (-34.19) |
| Observations | 13,742 | | 12,834 | | 26,576 | |
| *B. Individual-Level* | | | | | | |
| Received prenatal attention | 0.97 | 0.16 | 0.98 | 0.15 | -0.00 | (-42.86) |
| Number prenatal appointments | 7.03 | 3.12 | 7.39 | 3.37 | -0.37 | (-211.90) |
| Prenatal care in first trimester | 0.71 | 0.45 | 0.75 | 0.44 | -0.03 | (-154.61) |
| Low birth weight child | 0.05 | 0.23 | 0.06 | 0.24 | -0.01 | (-48.74) |
| Birth Weight (grams) | 3158.31 | 468.21 | 3148.18 | 475.02 | 10.12 | (40.40) |
| Private Hospital | 0.30 | 0.46 | 0.19 | 0.39 | 0.11 | (505.38) |
| Single Mother | 0.09 | 0.28 | 0.10 | 0.31 | -0.02 | (-101.40) |
| Mother's Schooling | 10.41 | 3.00 | 11.26 | 2.92 | -0.85 | (-564.34) |
| Mother's Age | 25.15 | 6.30 | 25.37 | 6.31 | -0.22 | (-68.37) |
| Observations | 4,818,118 | | 17,366,386 | | 22,184,504 | |

Notes: Columns (1) and (2) show municipalities and mothers not exposed to the FTS program. Columns (3) and (4) show the same for exposed municipalities and mothers. Column (5) shows the difference between columns (1) and (3)

Table 2: Average Effects on Fertility and Bacon Decomposition

| | TWFE (1) | TWFE (2) | TWFE (3) | Ch&DH (4) | Ch&DH (5) |
|---|---|---|---|---|---|
| FTS × After | -0.015 | -0.002 | | -0.061 | -0.033 |
| | (0.006) | (0.007) | | (0.035) | (0.029) |
| FTS Predicted Share | | | -0.057 | | |
| | | | (0.014) | | |
| Obs. / Switchers | 26,884 | 26,829 | 26,829 | 59,132 | 59,132 |
| | *Decomposition* | | | | |
| Always vs. timing | 0.045 [0.117] | | | | |
| Early vs. late | -0.026 [0.159] | | | | |
| Late vs. early | 0.016 [0.303] | | | | |
| Never vs. timing | -0.049 [0.422] | | | | |
| Other Controls | no | yes | yes | no | yes |

Notes: Each column represents a different regression. All estimates use a balanced panel of municipalities. In columns 2, 3 and 5 we control for municipalities' average mothers' schooling, municipalities' population and poor-by-year fixed-effects. Estimates in columns 4 and 5 are computed using the *did_multiplegt* package developed by de Chaisemartin and d'Haultfoeuille (2020). Standard errors are clustered at the municipality level and in columns 4 and 5 are bootstrapped with 1000 repetitions. The Goodman-Bacon (2021) decomposition is presented in column 1 and the weights of the main four comparison groups are in squared brackets.

Table 3: Average FTS effects on fertility: Migrant mothers vs. non-Migrants

| | Migrant | Non-Migrant |
|---|---|---|
| *A.Total Births* | | |
| FTS X After | -0.062 | -0.022 |
| | (0.023) | (0.011) |
| *B. First Child* | | |
| FTS X After | -0.043 | -0.011 |
| | (0.027) | (0.024) |
| *C. Second+ Born* | | |
| FTS X After | -0.099 | -0.062 |
| | (0.018) | (0.017) |
| Observations | 57,660 | 56,534 |

Notes: Each column represents a different regression. We control for mothers' education and municipalities' average population. Standard errors are clustered at the municipality level and bootstrapped with a 1000 repetitions. Estimates are computed using the *did_multiplegt* package developed by de Chaisemartin and d'Haultfoeuille (2020).

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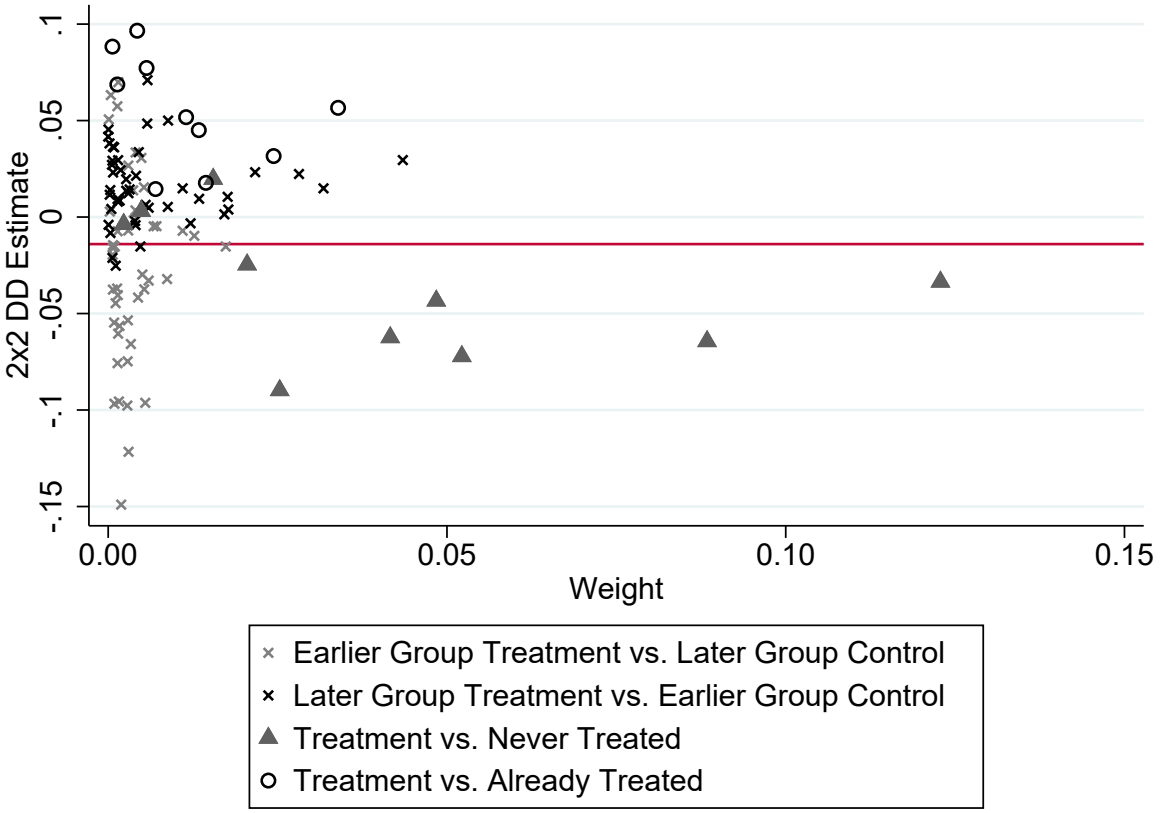
# Appendices

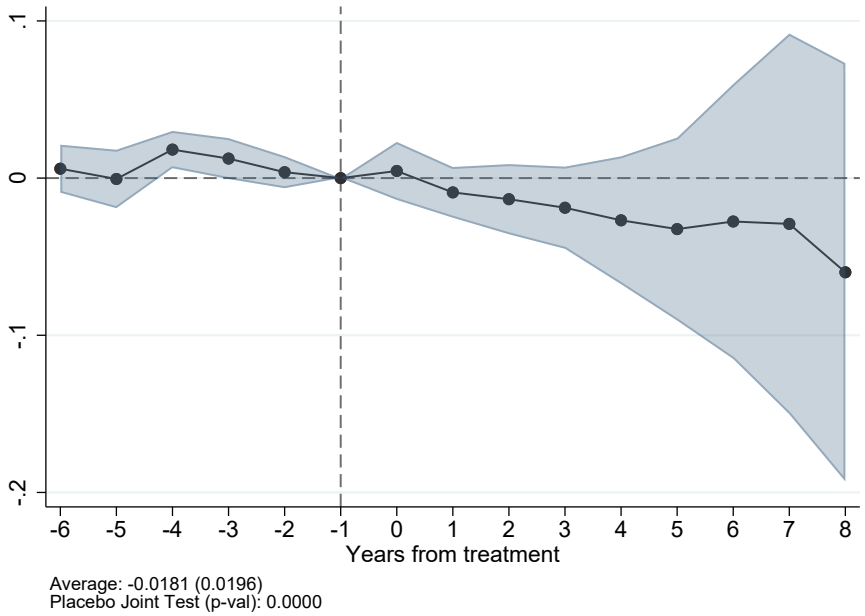
Figure A1: Bacon Decomposition FTS on Fertility



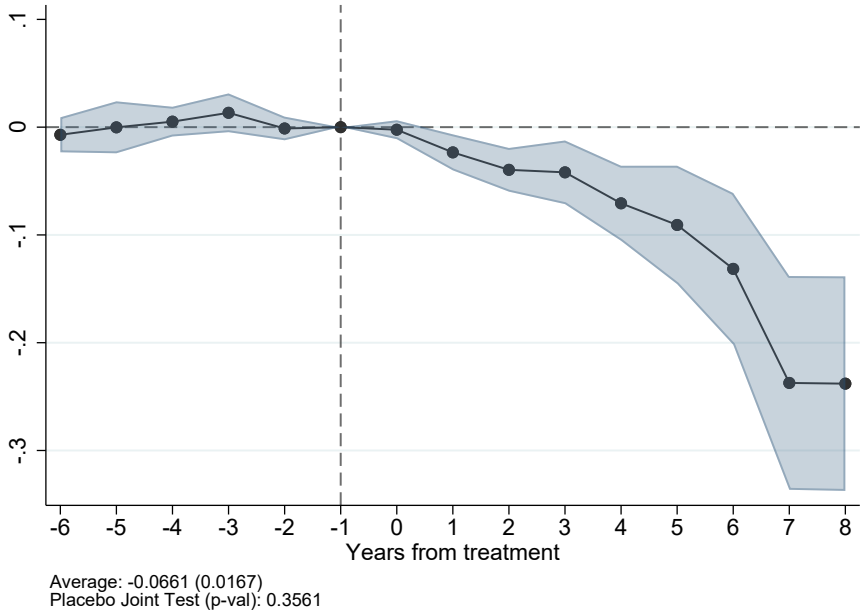

Notes: Estimates on a balanced panel across 12 timing groups with no controls using the *bacondecomp* package developed by (Goodman-Bacon, 2021).

Figure A2: Intensive vs. Extensive margin excluding always treated municipalities.

(a) First-Born



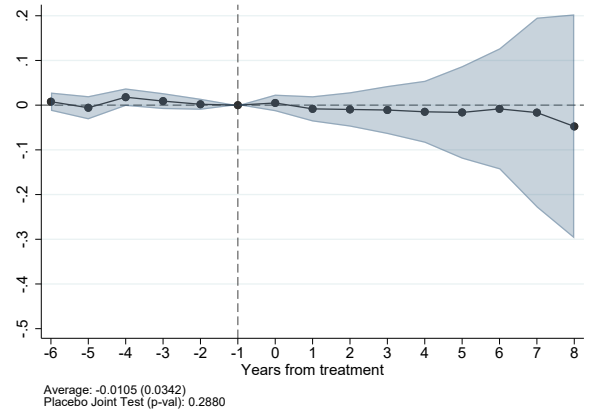

(b) Second+ Child



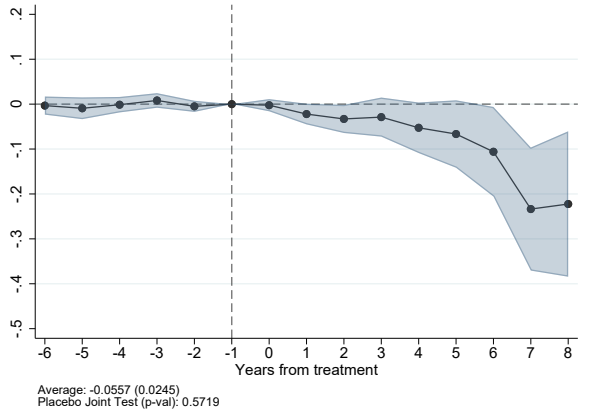

Notes: Panel(a) shows the effect of Full-time Schools on first order births. Panel(b)shows the estimated effect higher order births. Both excluding the 2007 cohort or the always treated group of schools. Estimated coefficients and their 95% confidence intervals for indicators of the years prior and after municipality adoption of the FTS program. All estimates come from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A3: FTS Effects on Fertility using alternative measures

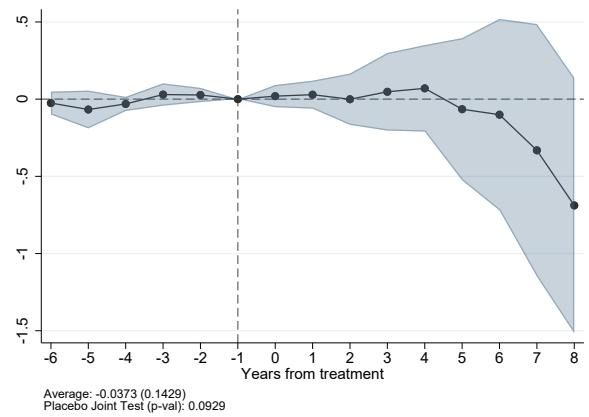
(a) IHS First Born



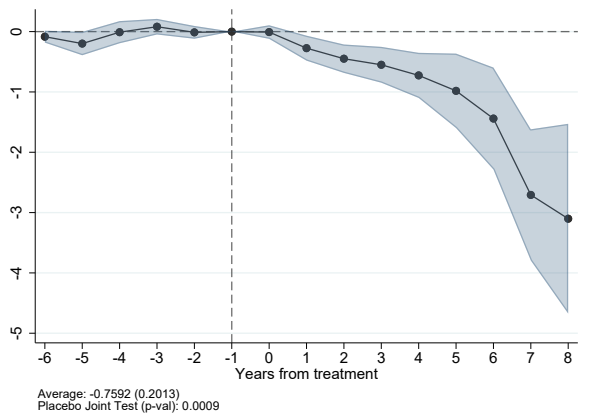

(b) IHS Second+ Child




(c) First Born /1000 population

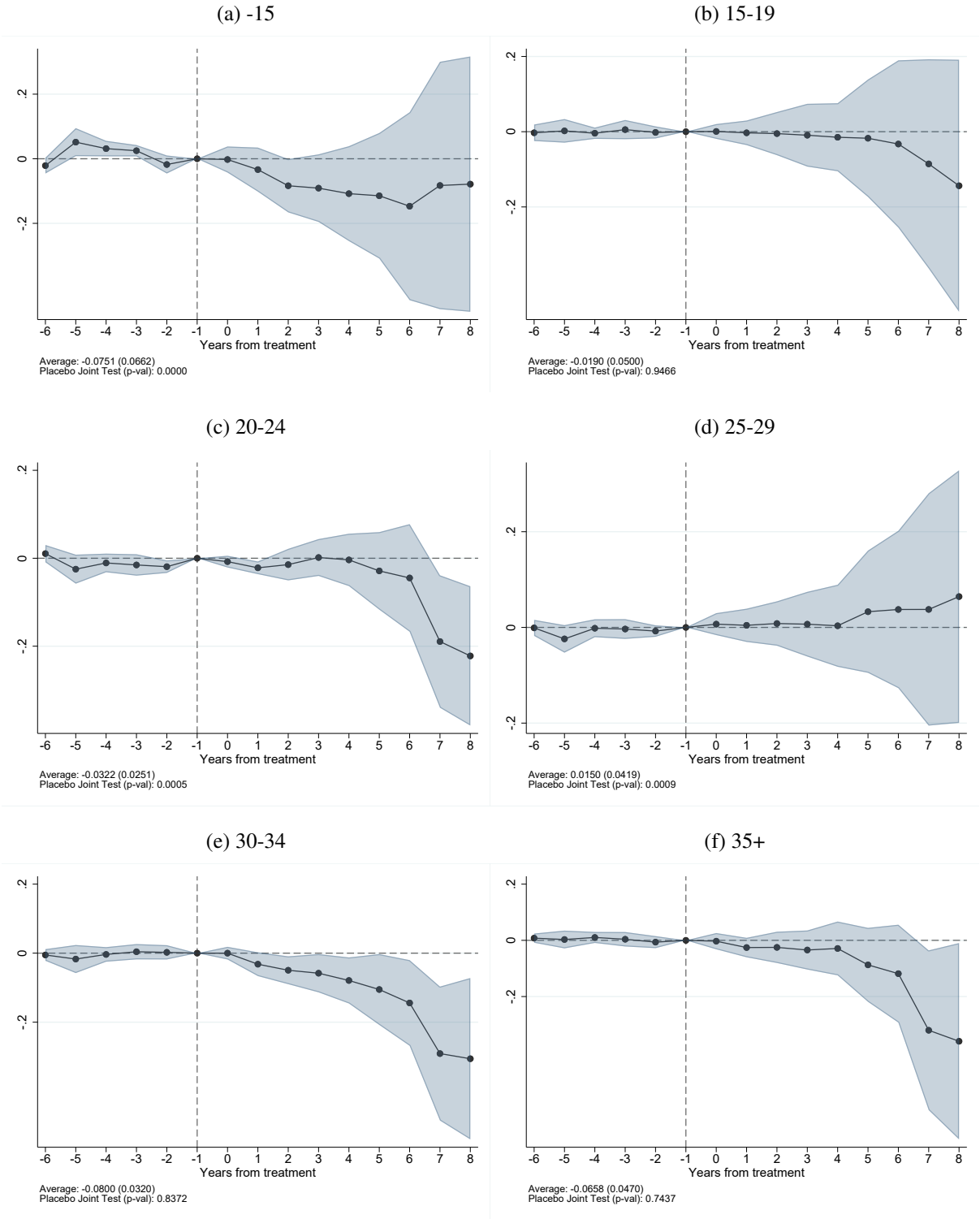



(d) Second+ Child /1000 population



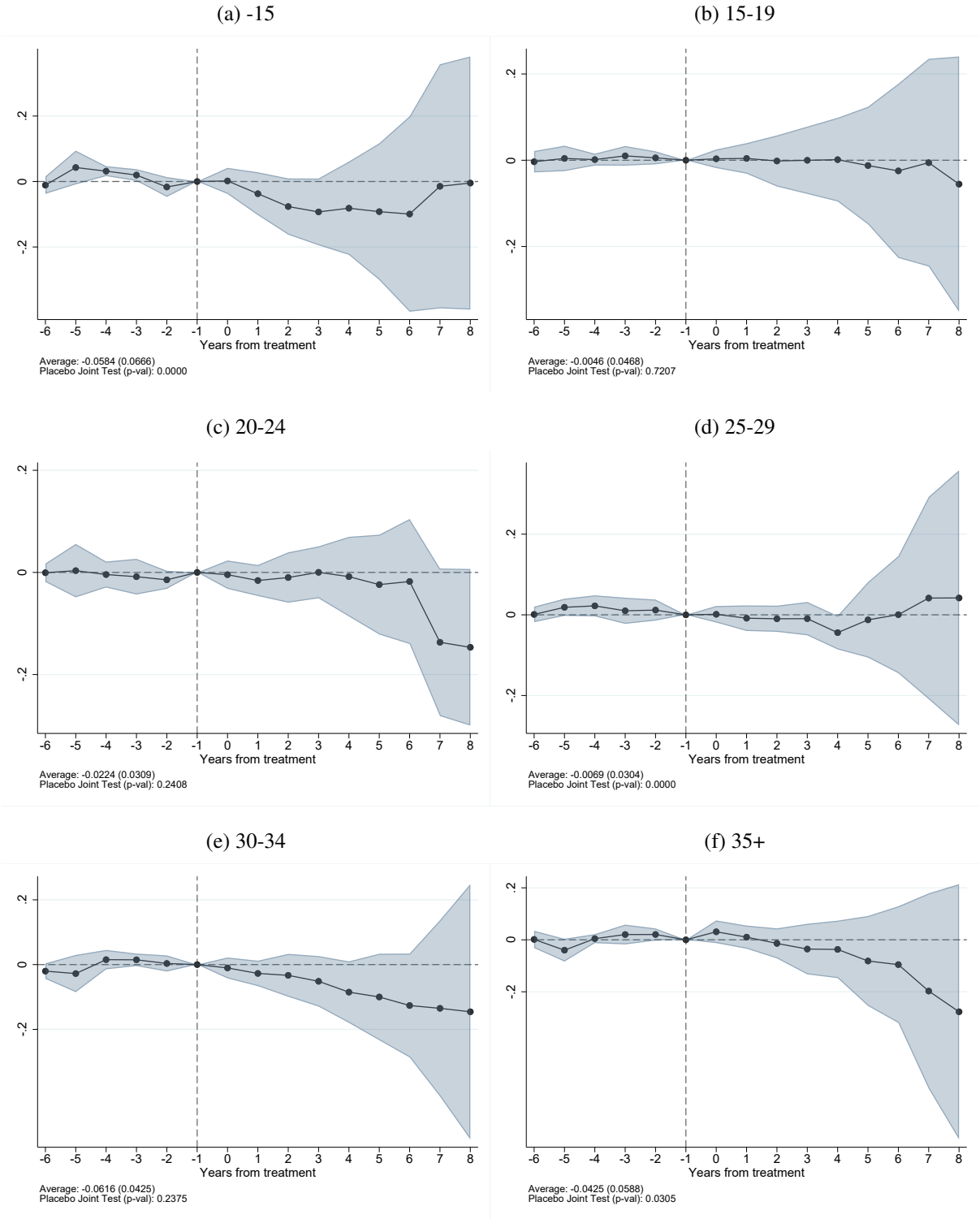

Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A4: Effects on Log-Births by Mother's Age




Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

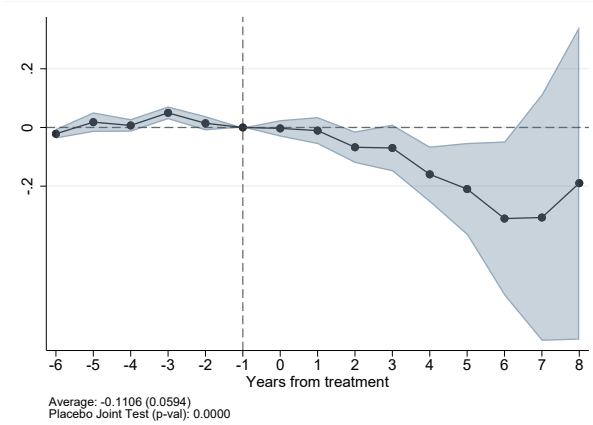
Figure A5: Effects on Log-Births by Mother's Age: First Born



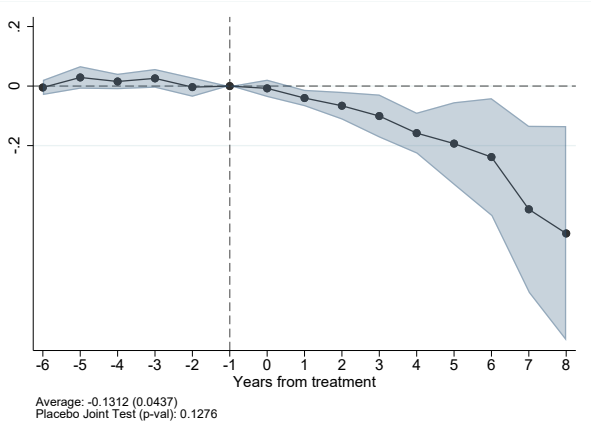

Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A6: Effects on Log-Births by Mother's Age: Second+ Born

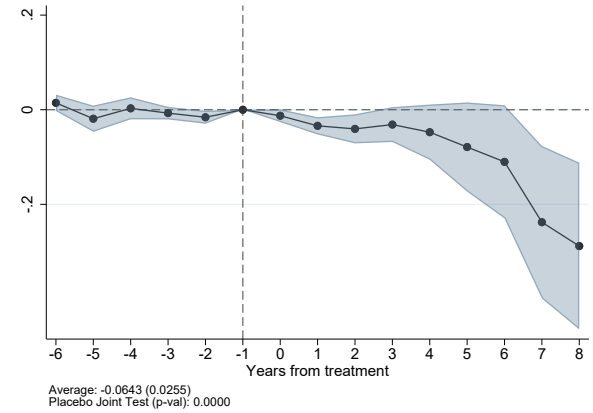
(a) -15



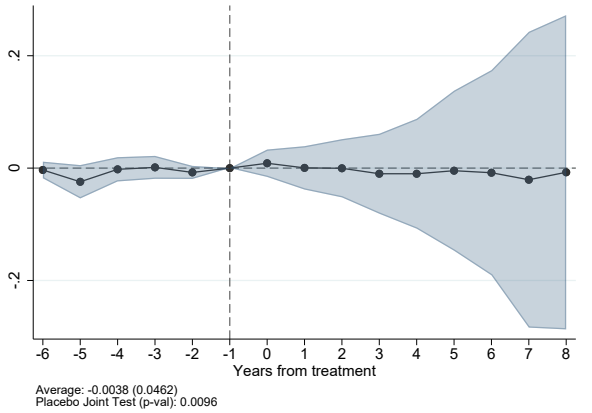

(b) 15-19



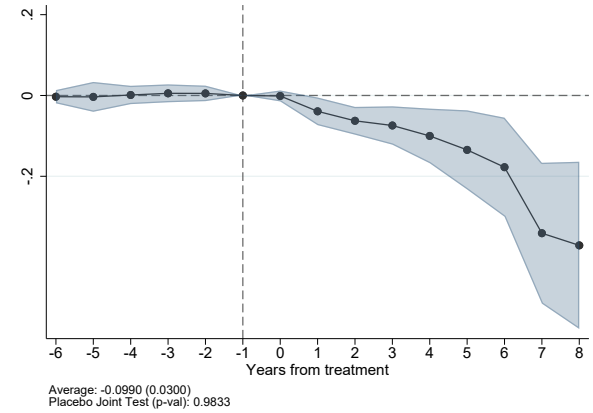

(c) 20-24



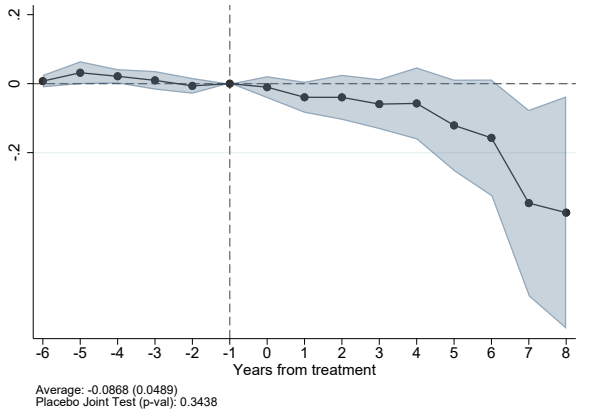

(d) 25-29




(e) 30-34

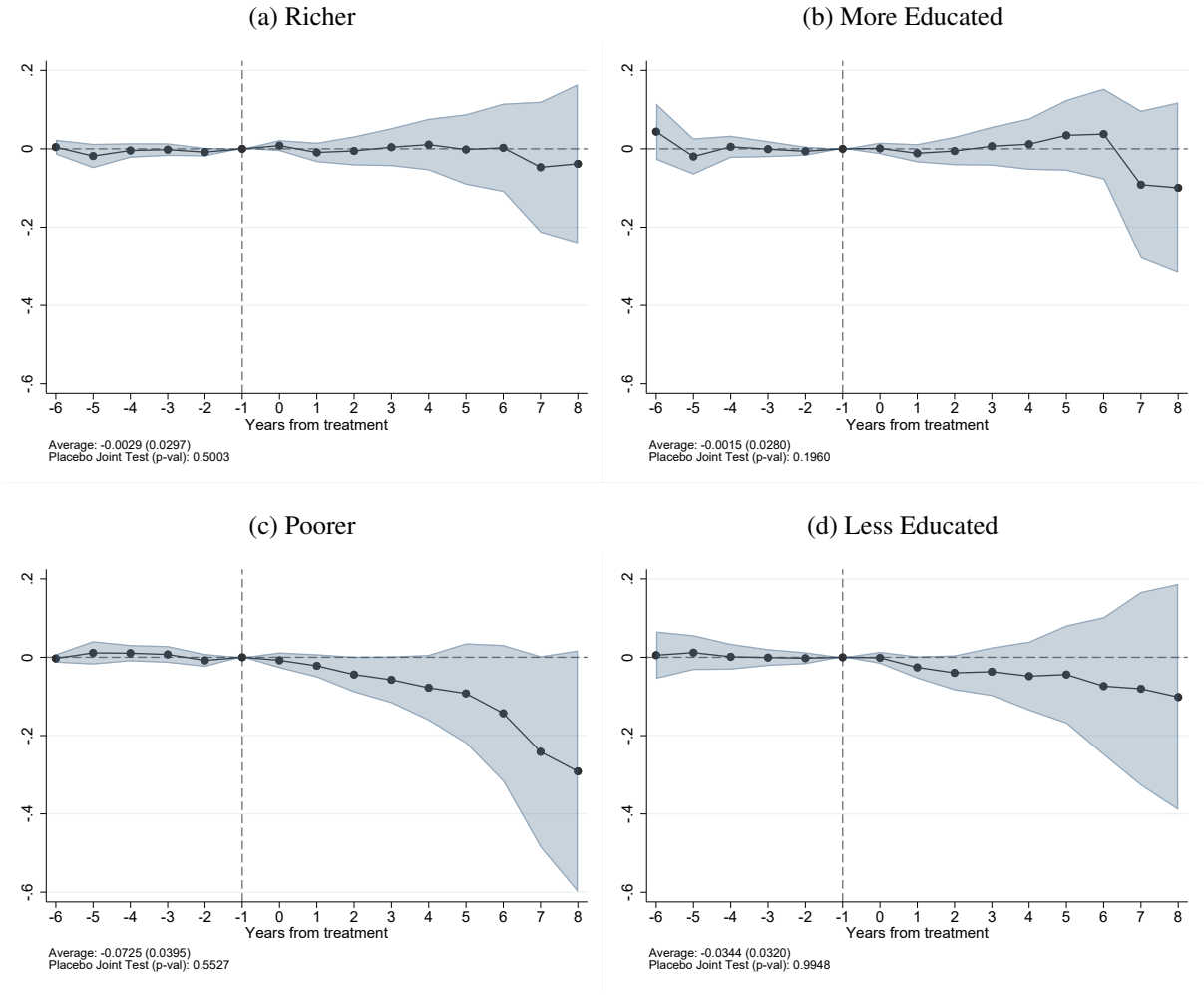



(f) 35+




Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

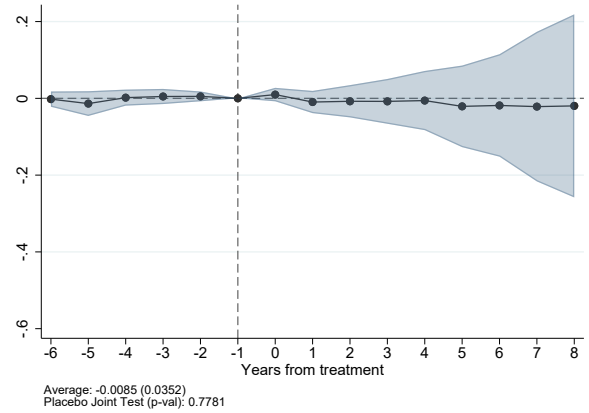
Figure A7: Dynamic Effects: Log-Births by poverty and education levels



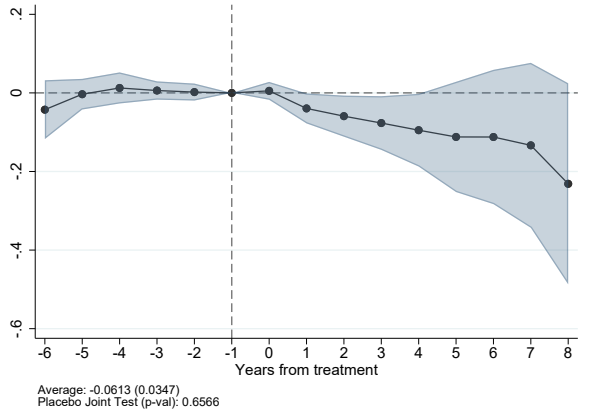

Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A8: Dynamic Effects: First Born by poverty and education levels

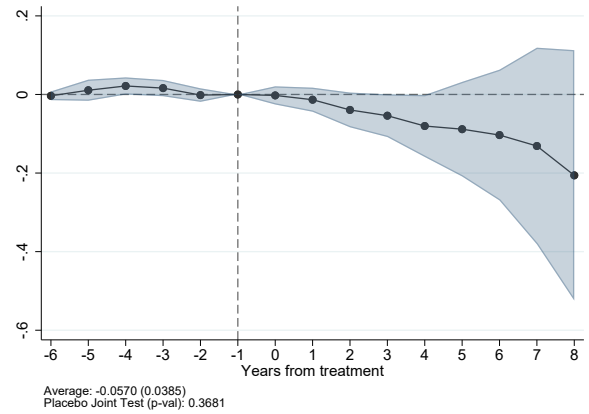
(a) Richer



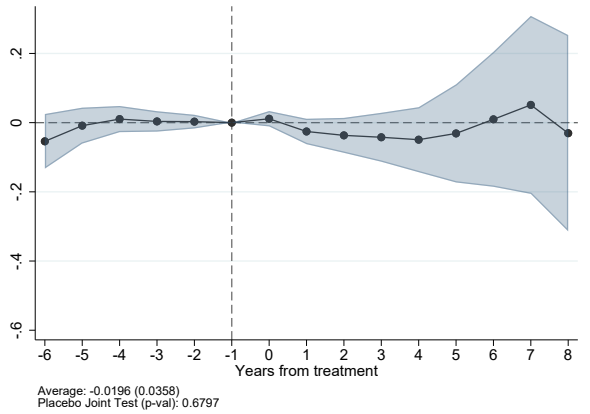

(b) More Educated




(c) Poorer




(d) Less Educated




Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for municipalities' average mothers' schooling, municipalities' population, including poor-by-time fixed effects and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.