Back to Work or Back to the Maternity Ward? The Effects of Full-Time Schools on Fertility

PRELIMINARY DRAFT
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Abstract

Formal childcare may have important effects on fertility as it may increase fertility rates due to the alleviation of childbearing costs, or reduce them due to increased chances for women to participate in the labour market. We study whether extending the school-day in public schools—an implicit subsidy on childcare—affects women's fertility decisions both overall and across different ages and socio-economic contexts. We contribute to the empirical evidence often based on surveys and focused on the effects of childcare costs in richer countries and for young children, by exploiting information on the universe of births and the staggered implementation of a nation-wide policy directed to children aged 3 to 12 in a less developed country. Our results, robust to the staggered adoption of the program, suggest that full-time schools availability reduces fertility by 4.4% on average over the course of 8 years. The effect is mainly driven by reductions at the intensive margin—this is, by mothers who already have one child—and for less educated women in municipalities with stronger labour markets. The programme also has a direct effect on reducing first pregnancies for young women aged 14 or less. Effects on mothers' healthcare utilization show that an increase in prenatal care, especially among poorer and less educated mothers.

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1 Introduction

Over the past few decades, most countries—developed and developing alike—have experienced both large increases in female labor force participation (FLFP) and decreases in fertility at the same time. There exist empirical and theoretical grounds on why these two variables may be determined simultaneously, as it is difficult to imagine households determining one of these variables while ignoring the other. Given this context, the availability of childcare is a key variable which may mediate and impact the relationship of FLFP and fertility. However, the theoretical and empirical evidence is inconclusive regarding the effects of childcare on childbearing: while childcare availability may reduce the direct costs of raising children and thus increase fertility rates, 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 children (Kleven et al., 2019; Aguilar-Gomez et al., 2019; Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020). Whether fertility rates increase or not due to childcare availability may differ substantially across varying socio-economic contexts, and it is ultimately an empirical question that remains open—especially so in developing nations where opportunity costs of specializing in childbearing and baseline FLFP may vary across women facing different income constraints and labour opportunities, or where more informal substitutes of formal childcare may exist (and where the empirical evidence is scarcer).

In this paper, we provide evidence on the effects of full-time schooling availability—an implicit childcare subsidy—on fertility rates. We do this by studying the causal effects of a large-scale full-time schools (FTS) program that increased the length of the school day by three-and-a-half hours in public elementary schools in Mexico on fertility decisions by using the universe of births for a panel of municipalities between 2008 and 2018.

We exploit the variation in the staggered roll-out of Mexico's FTS program across municipalities and we link this with birth records at the individual level. Our data comes from the FTS's administrative data and rich birth and birth-related health records on 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 schools (grades 1 to 6) were exposed to the FTS programme, along with 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 a series of 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 full-time schooling on fertility decisions.

The FTS was first introduced in the academic year 2007-2008 covering 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, 2020). Our estimations take this into consideration and are robust to the rapid staggered adoption and heterogeneous treatment effects across schools and over time (Sun and Abraham, 2020; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). Given that most of our comparisons come from always treated and timing observations, we implement the methodology proposed by de Chaisemartin and d'Haultfoeuille (2020). Our estimates are then robust to both dynamic effects and heterogeneous treatment results across municipalities adopting the FTS program at different time periods.

Full-time schools (FTS) have shown direct results on the quality of education and equity in the short term.[1] Moreover, FTS programs have shown complementary effects that are important to evaluate their overall social and economic contribution beyond education outcomes. 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). At the same time, as an implicit childcare subsidy, FTS have shown effects on FLFP (Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020) in exposed municipalities. Yet, there is still a lack of causal evidence of their effects regarding women's fertility decisions across different ages.

While several studies have considered the relationship between formal childcare and fertility, and despite the theoretically grounded and perhaps more intuitive hypothesis of a positive relationship between the two, the empirical evidence is mixed and inconclusive. Most empirical research is based on survey questions about women's perception of constraints to childrearing

[1] See, for example,(Berthelon et al., 2015; Padilla-Romo, 2022; Cabrera-Hernández, 2020; Agüero et al., 2021)

related to the lack of formal childcare as well as on information about the local availability and costs in developed countries. While some studies present evidence of positive effects of increased access to different forms of childcare on fertility, others find the opposite.(Del Boca, 2002; Hank and Kreyenfeld, 2003; Rindfuss et al., 2007; Mörk et al., 2013; Bick, 2016). These differences 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).[2] We contribute to the literature by analysing the effects of a national-level expansion in the public provision of implicit childcare in the context of a less developed country and for poorer households, where baseline FLFP and informal institutions of childcare might affect its potential effects on fertility. We also contribute to the growing yet scarce evidence on the externalities of FTS. Finally, our paper contributes to the literature on the potential direct effects that FTS may have on teenage pregnancy, if an increase in the time spent in school by women in this age group reduces the opportunities for risky behaviors leading to unwanted pregnancy. Evidence consistent with this type of effects has been observed in diverse settings such as Norway and the US (Black et al., 2008) and Brazil (Foureaux Koppensteiner and Matheson, 2021).

Our results suggest that full-time schools, as an implicit childcare subsidy, reduce fertility. The size of the effect is 4.4% in an 8-year span, or 0.7 births per 1,000 of the population compared to the year of program introduction (of 15.9 births per 1,000). This reduction is stronger for mothers who already have one child, those who are are younger (aged 15 to 19) and aged 30 to 34, and with less education in both poorer and richer municipalities. The programm also shows an impact on reducing first pregnancies among young women (aged 14 or less). Notably, the effects on higher order births concentrate in municipalities with stronger labour markets, showing reductions between 7.3% to 8.3%, suggesting that higher employment levels among women are one of the main mechanisms for reducing further pregnancies. Results on pregnancy and birth-related health show that the probability of prenatal consultations slightly increased an average of 0.6 percentage points (pp) and around 1.2 pp for poorer and less educated mothers.

[2]For example, in the US formal childcare is typically private with most research and debate addressing prices and quality that is determined by endogenous supply and demand. On the contrary, in Europe, formal childcare is mainly public and the debate around it has mainly addressed rationing or universal provision.

The rest of the paper is organized as follows. Section 2 provides background information 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: The FTS program, childcare and Female LFP in Mexico

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 enrolment rates have been increasing sharply over the last two decades. Preschool participation is relatively high (71.8%), elementary school enrolment has reached almost every child and middle school education's national net enrolment increased from 66.5% in 2000/01 to 87.9% in 2015/16. However mandatory education is not enforced by law, and students can effectively drop out of it at will. Overall, drop out rates in higher levels and the overall quality of education remains a challenge.

In this context, starting in the 2007/08 academic year, the government created an FTS program aiming to improve the quality of education and promote equity, which increased the length of the school day in public elementary schools that adopted the program. Notably, an explicit secondary objective of the program was also to support working mothers 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, this is 60% of the total eligible schools or 25% of all basic schools reaching more than three million students all over Mexico, or approximately 12% of the total students. The staggered implementation of the programme across space and time, and its intensity, can be depicted in Figure 1. By 2018 the FTS gradually reached nearly 80% of all Mexican municipalities generating a large variation in the contexts in which the full-time schools were present.

Across years, the FTS program has generally targeted urban and rural schools with low academic achievement observed in standardized tests, and in high-poverty areas. However targeting has been a challenge and, in practice, schools from different socio-economic contexts have participated. The program also signified a sharp increase of resources for all schools. FTS funds could be used to complement teachers' salaries, acquire teaching materials, equip schools for the extended schedule, and provide school lunches. Participating schools initially received approximately USD 10,000 dollars per school-year and 60% of them also provided hot breakfasts and meals, for which dining rooms and kitchens were built (Cabrera-Hernández, 2020; Padilla-Romo, 2022).

Regarding female labour force dynamics, while Mexican women have steadily increased their labor market participation in recent decades,[3] female LFP remains low compared to OECD countries(OECD, 2017). This lower level of participation may well be explained by the lack of family-friendly policies (Blau and Kahn, 2013; Cascio et al., 2015; Thévenon, 2016; Winkler, 2016). Yet, 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 4600 USD.[4] Consequently, only 4% of working mothers' children ages 0 to 6 years are enrolled in a public or private formal childcare institution. Naturally, 75% of children ages 0 to 6 are cared for by their mothers and 13% by direct family, regardless of mother's employment status.[5]

Similarly, despite the relatively high net enrollment in preschool and primary education, before FTS implementation, short school days had a direct negative effect on mothers' labour 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 female participation in the labor market below the country's potential.

[3]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

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

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

3 Data

We use administrative data from Mexico's Ministry of Education from school year 2007/8 to 2017/8,[6] identifying preschools and primary schools that participated in the FTS program in the current school year. We link this data at the municipality-level to individual-level information on birth and birth-related health outcomes. These data comes from administrative records on the universe of birth registries in Mexico from 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 anonymized 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, as well as health data regarding prenatal care utilization and birth outcomes (namely, place and date of birth, birth weight and size, Apgar scores, gestational age, order of birth, and type of delivery). When aggregated at the municipal level, these data are a useful source of municipal level fertility, and age-specific fertility rates. At the individual-level the data is useful to explore outcomes on birth characteristics. We also 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) 2005-2018 that allow us to identify municipalities with more dynamic labour markets 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 they have

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

nearly 3.5 times more population. Similarly, mothers in FTS municipalities receive more prenatal consultations on average, they may have more access to public health (a lower proportion gives birth on 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.

4 Identification Strategy

To identify the effects of full-time schools on fertility decisions, we exploit variation in preschool and primary schools enrolment in different municipalities across Mexico. That is, we compare changes in mothers' outcomes at the municipality and the individual-level between those exposed to the full-time schools versus mothers living in municipalities not exposed to the program, according to the timing of adoption of FTS at the municipal level.

We estimate the following fixed effects regression to obtain the impacts on fertility measures:

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

where Y_{it} is the natural logarithm of one plus total births in municipality i in total and for first and higher order births in academic year t;[7] ν_m are municipality fixed effects; θ_t are academic year fixed effects; and u_{mt} is an error term that we allow to be correlated within 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 δ_k identify the average effect of

[7]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 Annex Table A2

$k+1$ years of implementation of the FTS program on births.

For the variables related to individual mothers' information and birth characteristics, we estimate the following equation:

$$Y_{it} = \nu_i + \theta_t + \sum_{k=-6,k\neq-1}^{8} \delta_k FTS_{i,t-k} + u_{it} \tag{2}$$

where Y_{it} represents response variables for mother's i: a dummy indicating if the mother attended to at least one prenatal medical appointment, the total number of these during pregnancy, a dummy indicating if the first medical appointment happened during the first trimester, as well as a variable denoting whether a newborn had low birthweight, in each academic year t; ν_m are municipality fixed effects; θ_t are academic year fixed effects. The rest of the terms are defined as in Equation 1.

Given the program's staggered implementation and the possibility of heterogeneous treatment effects across groups and over time, we estimate our main Equations 2 and 1 using the estimation method proposed by de Chaisemartin and D'Haultfœuille (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 these estimates to other alternatives as the Goodman-Bacon (2021) decomposition of our treated and control groups shows that the 2-by-2 comparisons driving the weighted Difference-in-Differences estimator come from timing groups that represent "forbidden comparisons" (see Figure A1). In this set-up, de Chaisemartin and D'Haultfœuille (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.[8] As a robustness check, in section 5.3, 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).

[8]We estimate the long-difference placebos as proposed by (de Chaisemartin and D'Haultfœuille, 2020) instead of first difference placebo estimates testing commong 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.

5 Main Results

Figure 2 shows our main estimation results using de Chaisemartin and d'Haultfoeuille (2020), and including a set of controls and municipality-specific trends. The results show an overall negative impact on births. The effect reaches nearly 20% after 8 years of exposure. On average, over the 8-year span after treatment, the effect is of 4.4% or nearly 0.7 less births per 1,000 of the population (from 15.9 in the FTS introduction year)[9] or 84,500 less births from a base of 1,921,000. Note that in the pre-treatment period, according to Figure 2, the effects remain stable around cero and there is a small but clear change only one year after implementation. This offers support to our identification strategy.

While the results from Figure 2 are informative about the overall effect of FTS on births, it is difficult to infer whether this results is driven by an extensive margin effect (i.e. women postponing or selecting out of motherhood) or by an effect on the intensive margin (i.e., having more children given that a woman has a child already), or if it is driven by women from a specific age-group. We compare the effects of FTS using the aggregate number of first births at the municipality level to the effects on those of higher order. Figure 3 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 children (intensive margin) rather than in women experimenting the transition into motherhood (extensive margin). Hence, childcare availability in the context of FTS would be causing a reduction or, at least, a delay in mothers' decisions of having more children.[10]

Notably, as shown in Figure 4—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 the effects on total births are only identified for women 30-34 years old. Heterogeneous effects by first- and higher-order children show that childcare effects on reducing the transition into motherhood are not significant for women older than 15 years old.

[9]Note that we do not have data on births for the pre-treatment period, this is before school-year 2007, hence we use the introduction school year 2007/2008 as a base for our back-of-the envelope computations.

[10]Similarly, in the Annex Table A2 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

The average results by age also suggest that the effects on the reduction of higher order births concentrate among younger women, specifically those aged 15 to 19 and among women 30 to 34 years old. However the effects on teenage women, younger than 14 years, are negative and clearly significant for first born children despite the low number of cases (approximately 4.5% of total births), suggesting a direct impact of the programme on teenage pregnancy which is consistent with the effects of increased (compulsory) schooling on teenage pregnancy (**?**).[11]

In Figure 5 we focus our attention on women of ages less than 19 years old, and 30 to 34 and show the dynamic effects of the exposition to the program. For all subgroups of women pre-trends remain close to zero and the effects are cumulative across time. For younger women (less than 15 years old) the average effect represents a fall of 19.3% or a reduction of 2526 births from a base o of 13295 births.

For women aged 15 to 19, the average reduction in higher order births represents a fall of 13.2% from the base year (or 13,000 births from a base of 98,534), while for women 30 to 34 years old the average fall in higher order births represents 7.5% of the base year (or approximately 20,000 births from a base of 266,480).[12] 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.

5.1 Heterogeneous effects

Figure 6 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 median. 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 7 shows these effects across time and suggest a similar statistically significant cumulative effect close to 30% for poorer and less

[11] All the dynamic effects and pre-trends, separated by age and for first and higher order births can be found in Figures A3 to A5 in the Annex.

[12] 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 women's age and school-year.

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.[13]

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; Cabrera-Hernández and Padilla-Romo, 2020)[14]. In this 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 or delay higher order births. We explore this in Figure 8 using the data from the National Occupation and Employment Survey (ENOE).

Figure 8 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 9 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 7.2% and 8.2% 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 47,800 and 56,400 births, respectively.

5.2 Health and Mother's Characteristics

Theoretically and empirically, one can argue that if mothers dispose of more time due to childcare availability, this can have an effect on their chance to attend prenatal consultations, on the

[13]Dynamic effects for total births and for first born children can be consulted in Figures A6 and A7 in the Annex.

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

number of visits to the doctor during pregnancy and, in general, the quality of care during the gestation period. We can also hypothesize that women with higher levels of employment and earnings due to FTS introduction, may now have more available resources to afford medical guidance. This is more relevant in Mexico where, according to our data, 15% of women did not have access to public health in 2018. This means that most of their consultations are paid out of pocket.

Figure 10 shows the effects on individual-level prenatal healthcare utilization and on a commonly used measure of newborn health—low birthweight—, on average and for selected subgroups of mothers: mothers of first- and higher-order children, more and less educated mothers, and those living in a relatively poorer or richer municipality. Panel (a) shows the effects on the probability of attending at least one prenatal consultation. Panel (b) uses the number of total consultations as outcome. Panel (c) reveals the estimations on the probability of having attended to prenatal medical consultations during the first semester of pregnancy. Panel (d) explores the probability of having a low birth weight children (that is, below 2500 grams).

Overall, there are significant effects on the probability of attending prenatal consultation, with an overall increase of 0.75 percentage points. While this may seem like a negligible effect, the average prenatal consultation rate is of 97% for which marginal increases are harder to observe. Notably, the effects concentrate among poorer and less educated women, with an effect close to 1.2 pp. This evidence suggests that the program may be releasing time for mothers, increasing the chance that they attend to medical appointments. These hypothesis is corresponded by the effect on total consultations that is higher for poorer mothers as observed in panel (b) (a small but significant increase close to 0.1 consultations). No effects are observed for the probability of a newborn having low birthweight, overall or for any given subgroup.

5.3 Robustness Checks

Figure 11 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% reduction after 8 years of implementation.

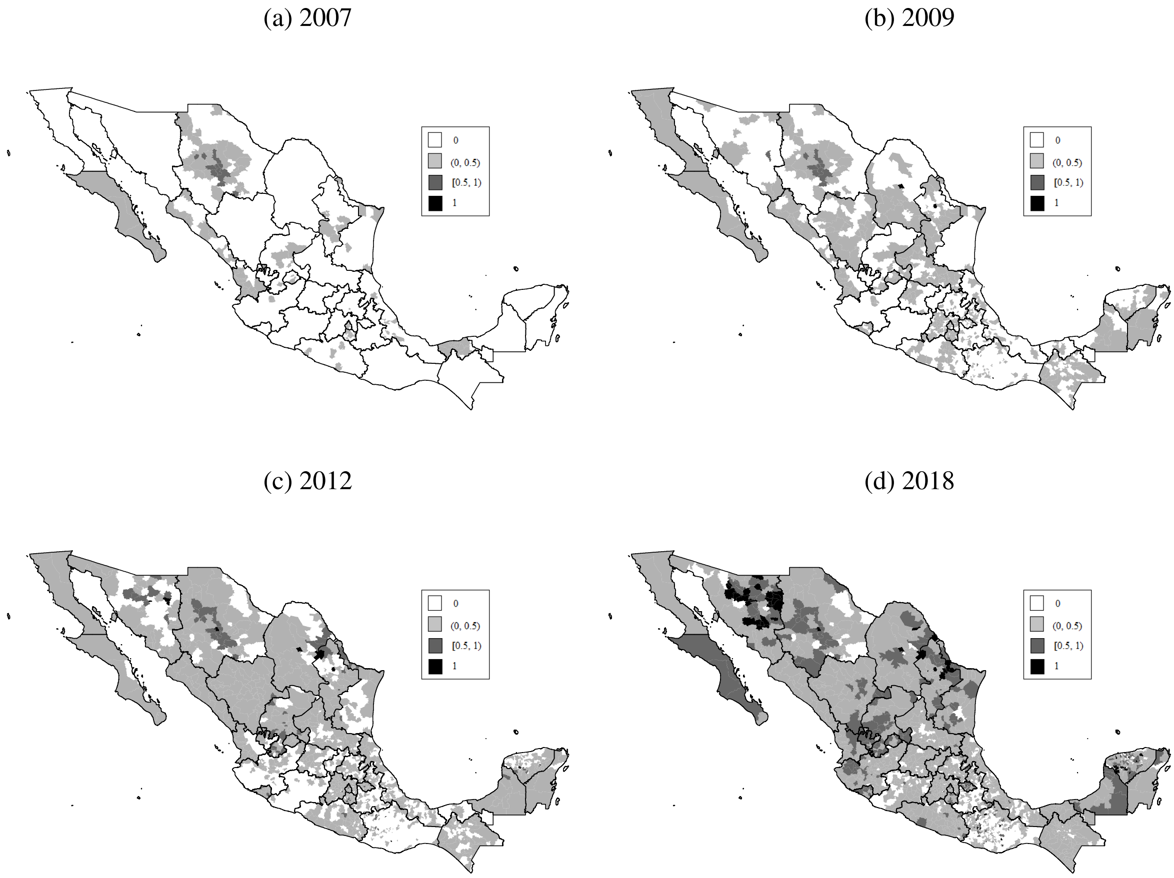
Given the effects of the programme on FLFP documented in the literature there can still be endogenous migration of working mothers towards FTS muncipalities, these women may ex-ante have a lower preference for children. In this case the effect we document, moreover for higher order births may be explained by selective migration. To address this concern, we use mother's municipality of birth to create a variable that denotes if the mothers exposed to the FTS had migrated from a different municipality, respect to mother not exposed to the programme. The results using our specification in Equation 2 are presented in Figure 12 suggest that the program is, on the contrary, reducing migration 1.8 pp on average.

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. These effects are mostly driven by young and middle-age mothers who already have at least one child, low-educated women in lower SES municipalities. We interpret these results as being driven by greater FLFP of mothers in municipalities with access to the FTS program. We also find effects on increased utilization of prenatal care, but no effect on newborns' health (i.e., low birth weight). Finally, we find a decrease in average schooling of mothers, a finding that is consistent with the decrease in fertility being concentrated among high-schooling women.

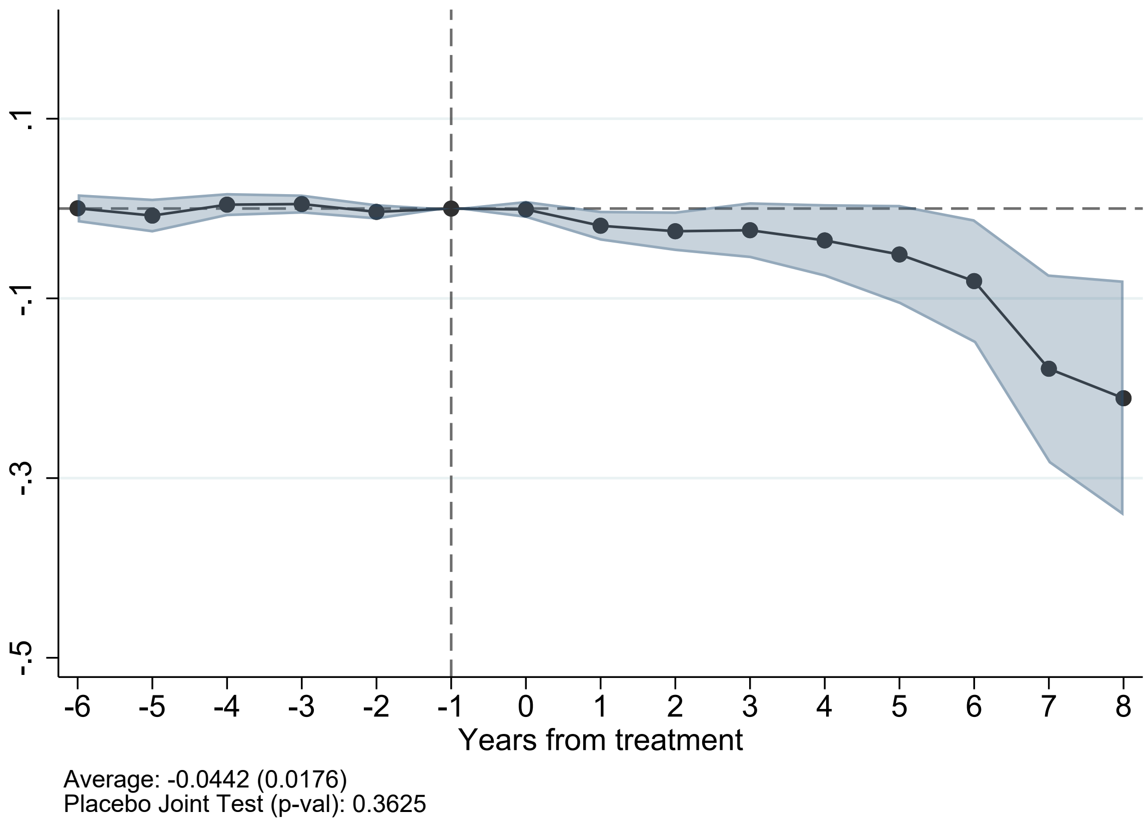
Our paper fills a gap in the literature analyzing the causal effects of increased access to childcare on fertility, both in terms of analyzing this question in the context of a middle-income country, Mexico, and in being, to the best of our knowledge, the first study providing causal evidence on the effects of *full-time schooling* on fertility decisions.

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



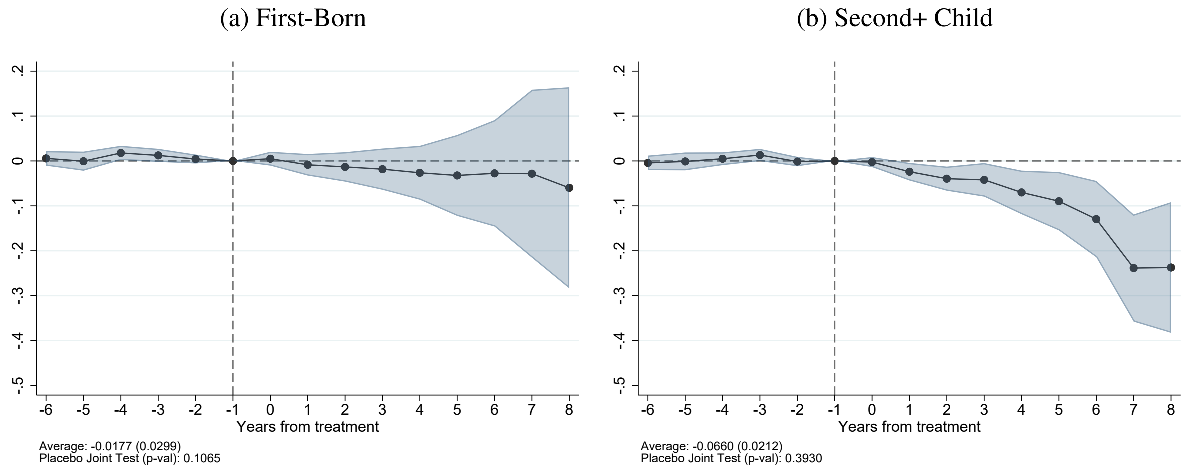
Notes: The maps show the proportion of FTS pre- and primary schools out of the total schools per municipality in each year. The programme started in 500 schools in 2007 reaching half of the 32 states in Mexico. By 2018 80% of all municipalities had at least one FTS present across all states in the country.

Figure 2: FTS Effects on Log-Fertility



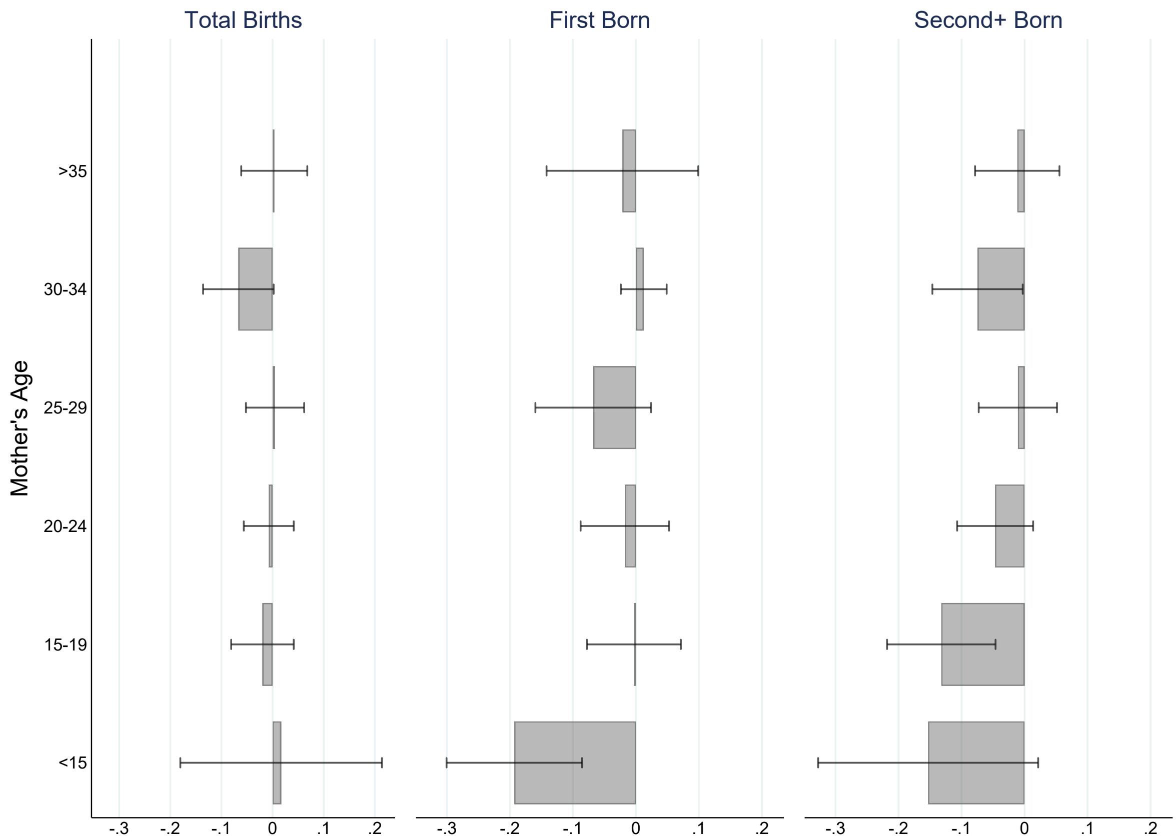
Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after municipality adoption of the FTS program. All estimates come from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for poverty level (above vs. below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

Figure 3: Intensive vs. Extensive Margin



Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. Each panel comes from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling for poverty level (above vs. below the median), average mothers' schooling and age, municipalities' population and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

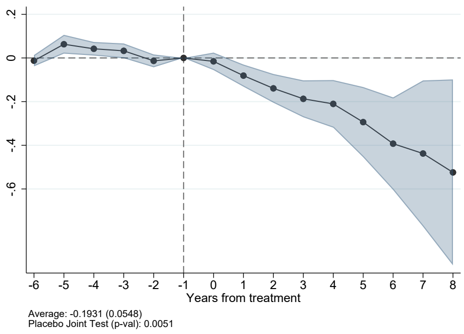
Figure 4: Log-Births by mother's age



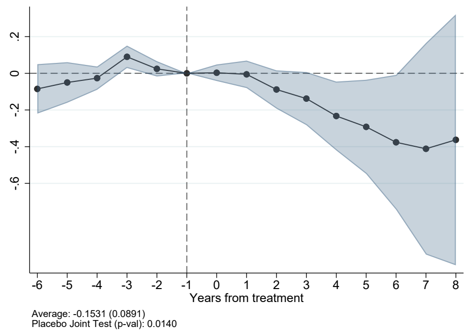
Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible,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

Figure 5: Dynamic effects by selected groups of age

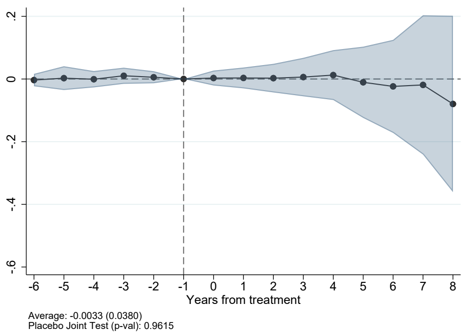
(a) First-Born (aged -15)



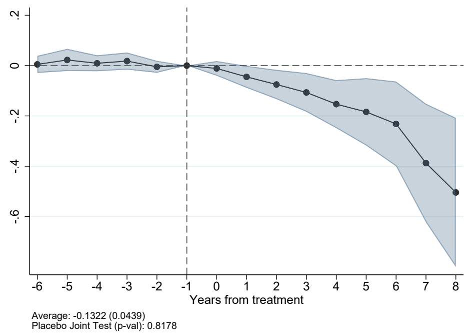
(b) Second Child+ (aged -15)



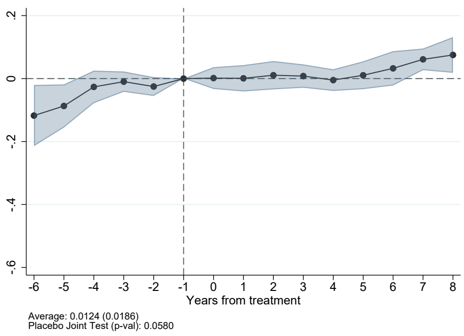
(c) First-Born (aged 15 to 19)



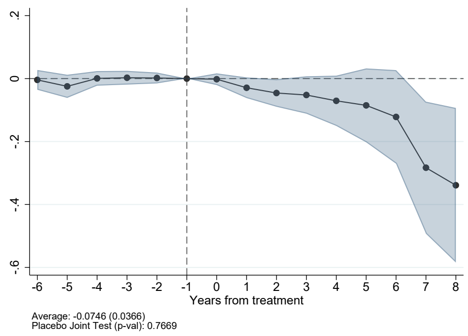
(d) Second Child+ (aged 15 to 19)



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

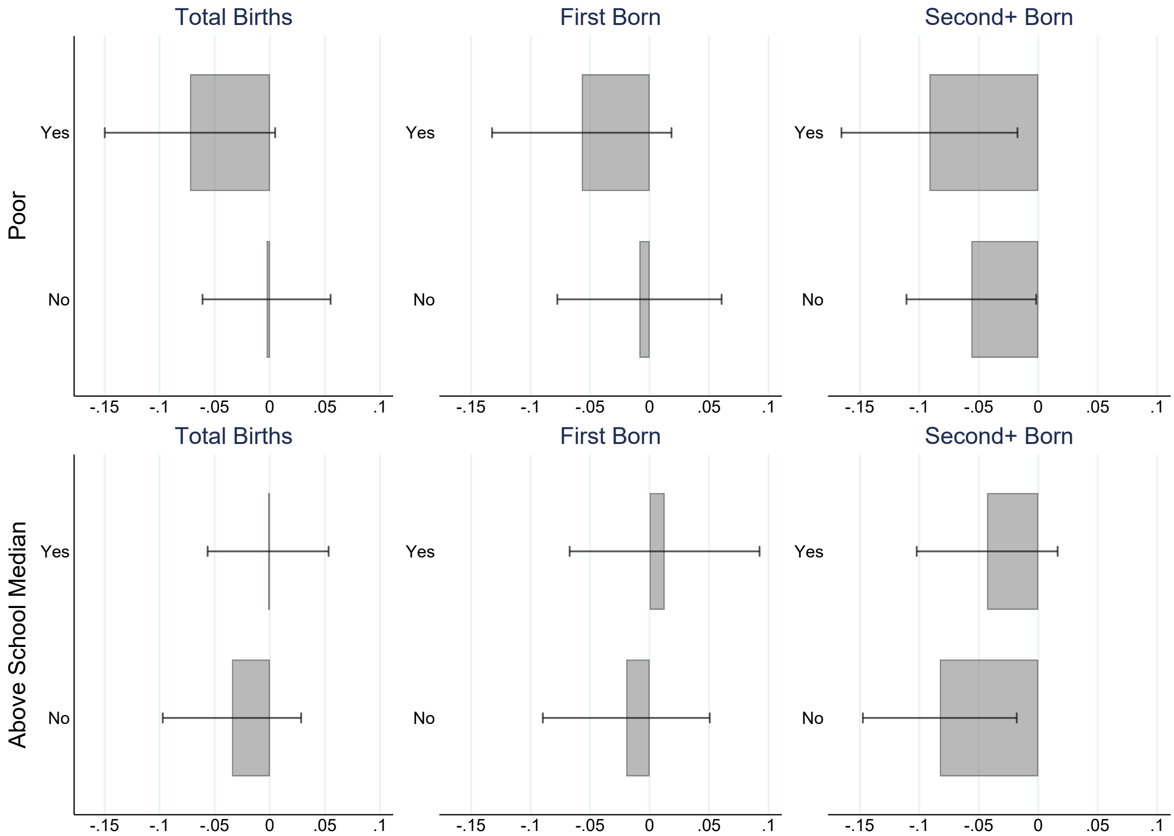


(f) Second Child+ (aged 30 to 34)



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 poverty level (above vs. below the median), average mothers' schooling, municipalities' population and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

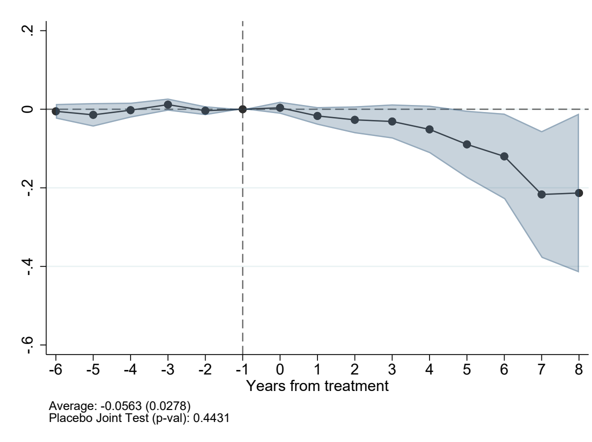
Figure 6: Log-Births by mother's education and municipalities' poverty



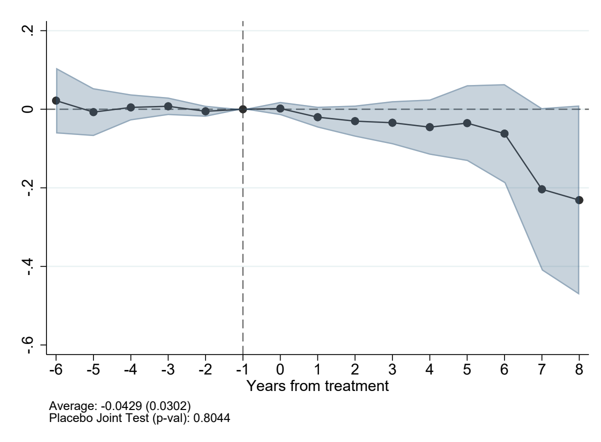
Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible,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

Figure 7: Dynamic effects: Second+ Child by poverty and education levels

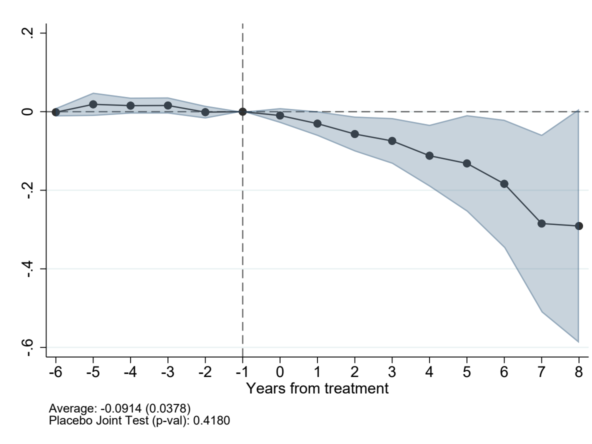
(a) Richer



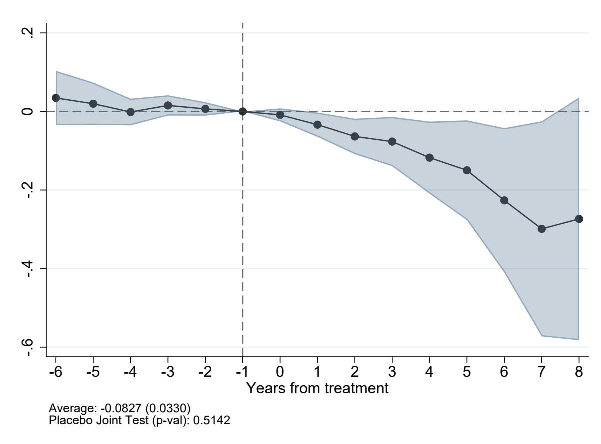
(b) More Educated



(c) Poorer

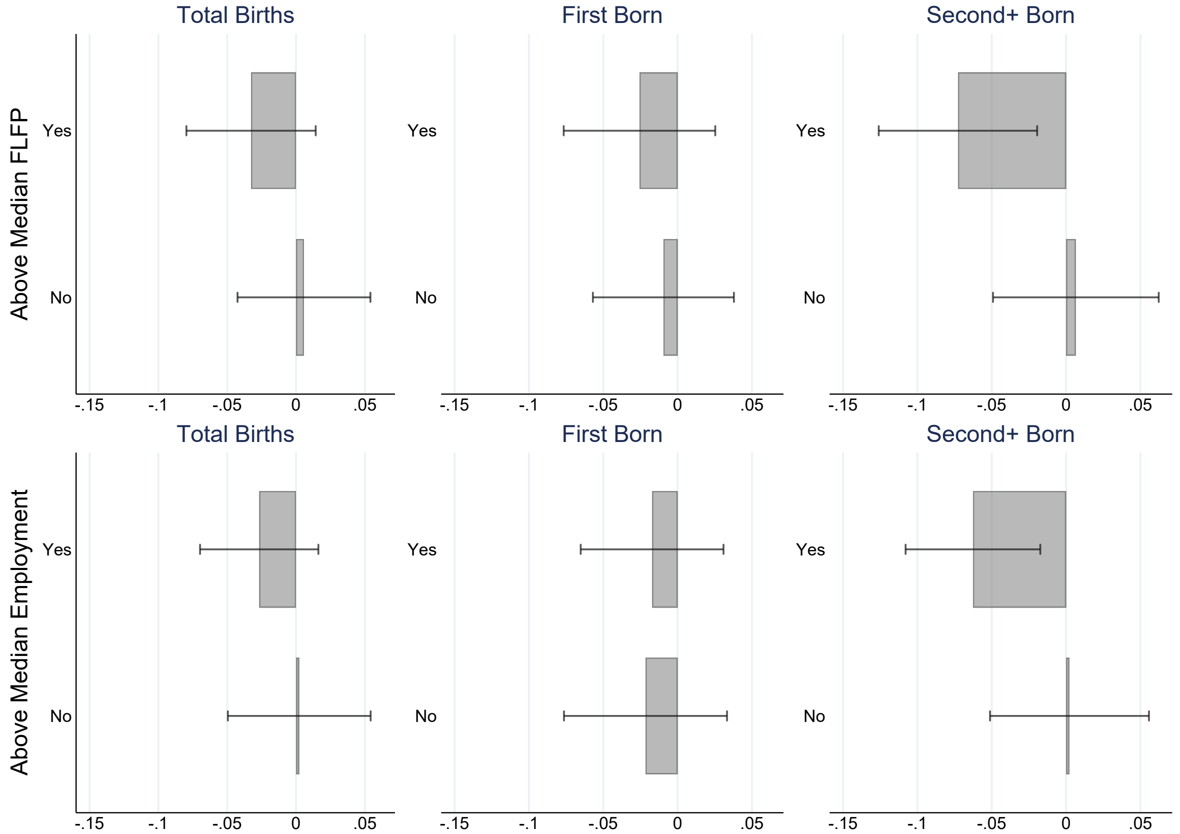


(d) Less Educated



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 using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible, for poverty level (above vs. below the median), average mothers' schooling and age, municipalities' population and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

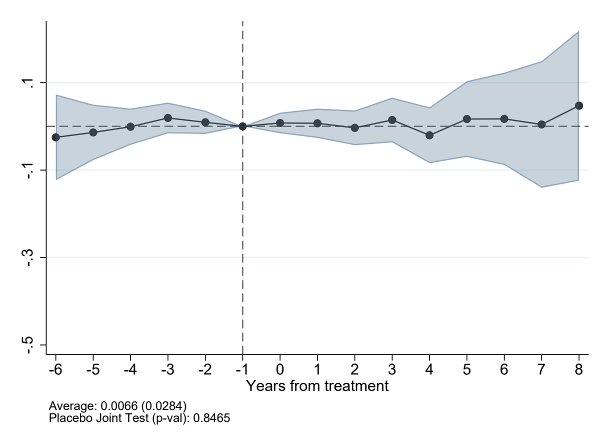
Figure 8: Log-Births by municipalities' employment and FLFP



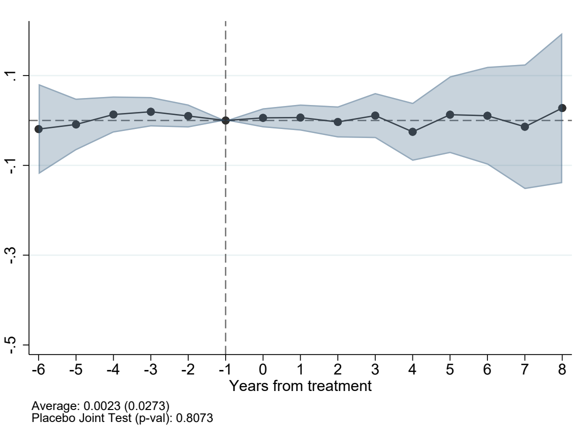
Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible, for poverty level (above vs. below the median), average mothers' schooling, municipalities' population 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 FLFP and Employment

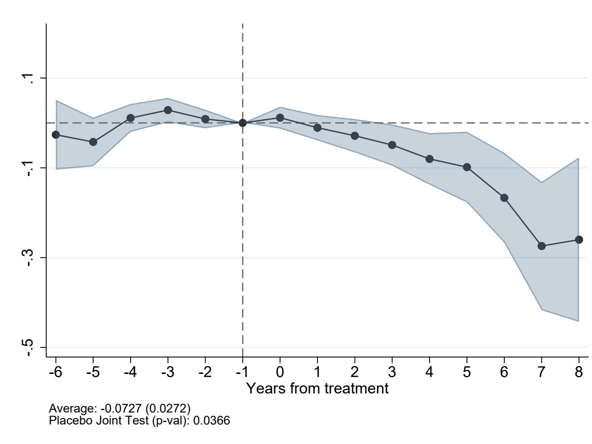
(a) Below Median FLFP



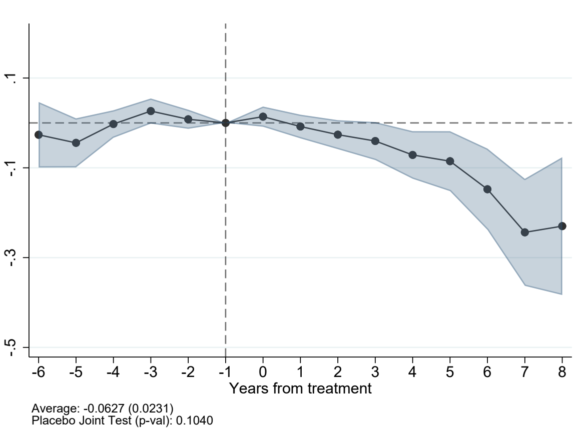
(b) Below Median Employment



(c) Above Median FLFP



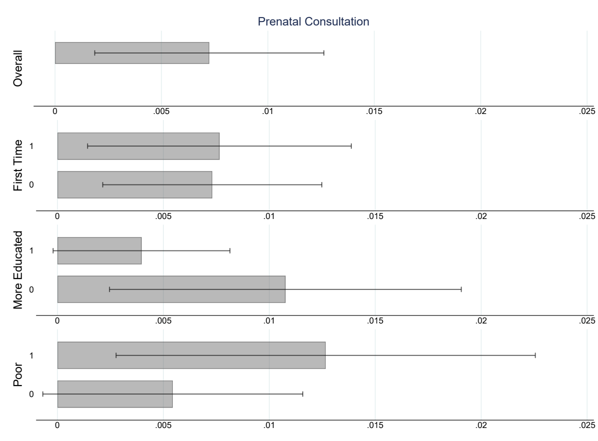
(d) Above Median Employment



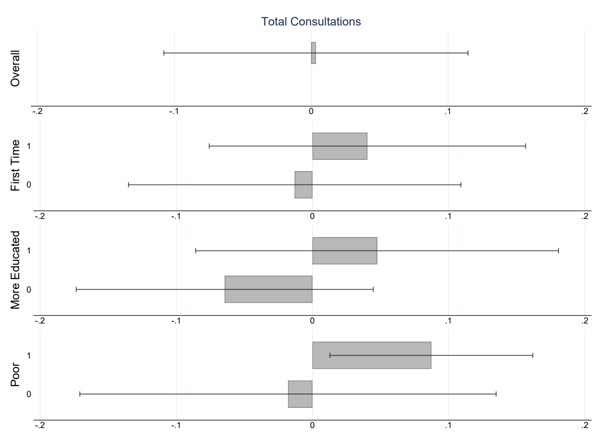
Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible, for poverty level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

Figure 10: Mothers' and Children's Health

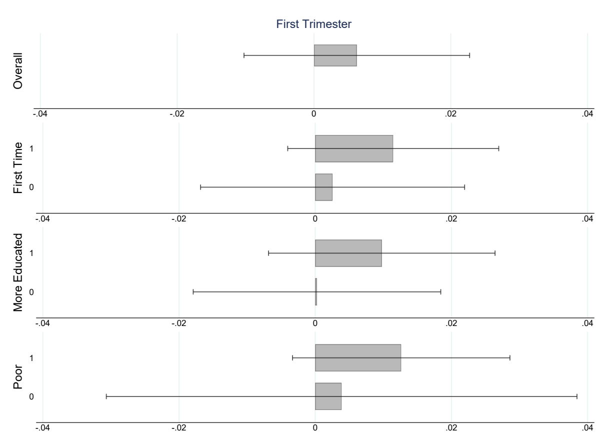
(a) Prenatal Consultation (=1)



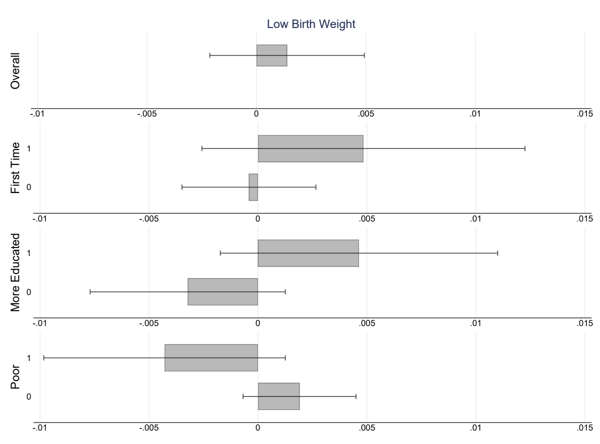
(b) Number of Consultations



(c) Consultation in First Trimester (=1)



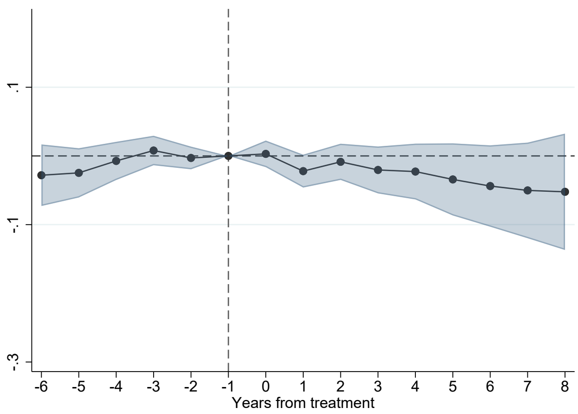
(d) Low Birthweight



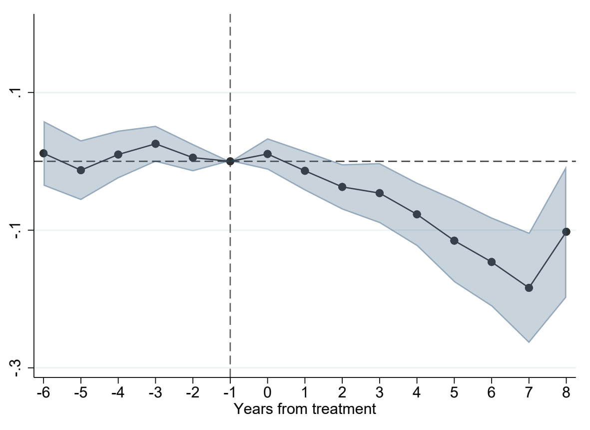
Notes: Estimated coefficients and their 95% confidence intervals for the average effects after the municipalities adopted the FTS program. Each estimate comes from a single regression for the respective sub-sample using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible,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

Figure 11: FTS Effects on Fertility using alternative methods

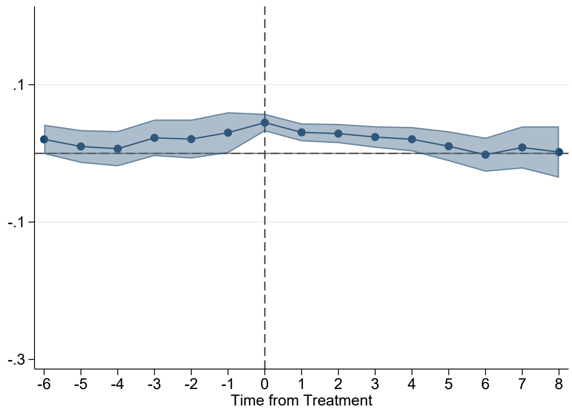
(a) IW estimator First Born



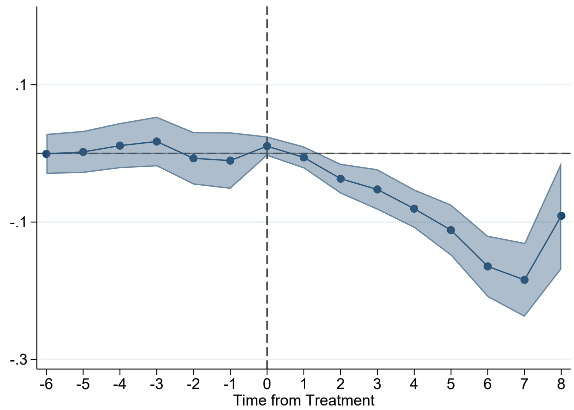
(b) IW estimator Second+ Child



(c) DD Imputation First Born

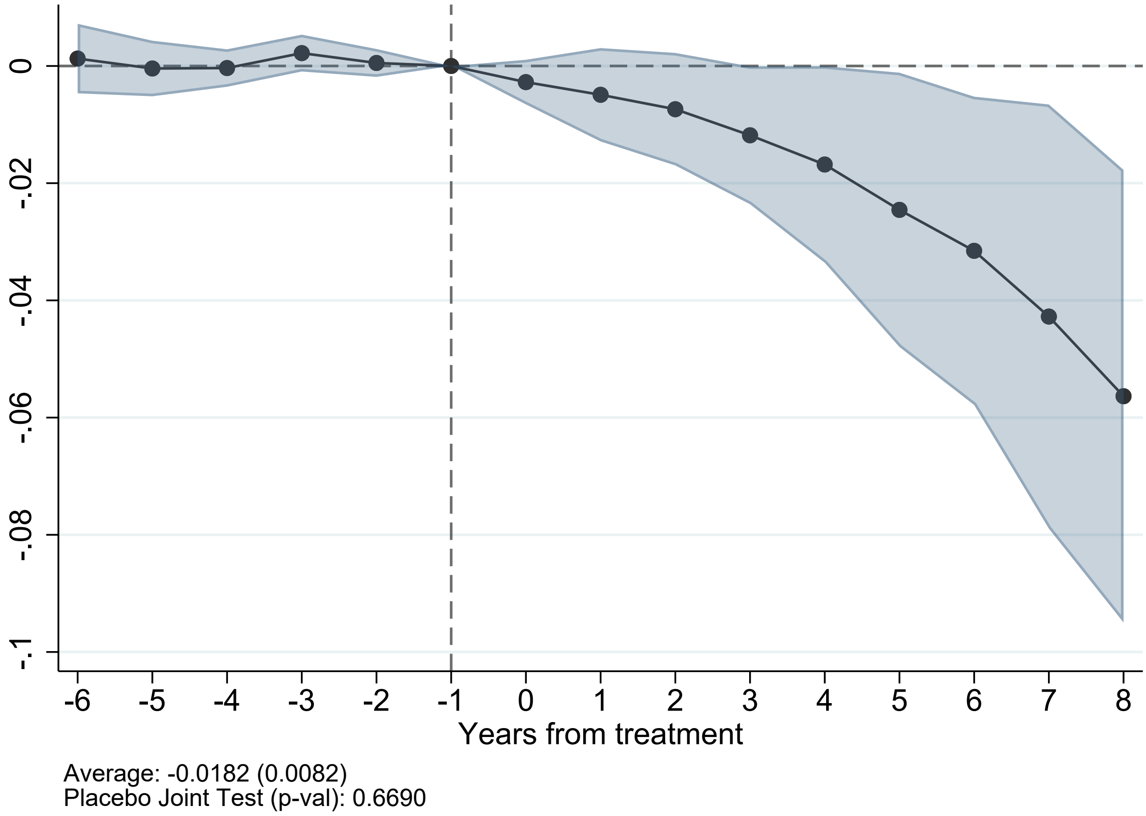


(d) DD Imputation Second+ Child



Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression controlling for poverty level (above vs. below the median) and mothers' group of age. Standard errors are clustered at the municipality level.

Figure 12: FTS Effects on migration from mother's birth municipality



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 poverty level (above vs. below the median), mothers' schooling and age, municipalities' population and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Table 1: Main Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Non-FTS Municipalities			FTS Municipalities			
	N	Mean	SD	N	Mean	SD	Diff.
A. Municipality-Level							
Births per 1000	82416	15.72	6.07	76986	17.21	4.77	-1.49***
First Child per 1000	82416	5.67	2.45	76986	6.17	1.90	-0.50***
Second Child per 1000	82416	10.05	4.12	76986	11.04	3.22	-0.99***
Poor Municipality	82452	0.60	0.49	77004	0.39	0.49	0.21***
Total Population	82416	21004.54	60629.25	76986	71766.51	170387.28	-50761.97***
Population aged 3-12	82452	4647.85	11486.60	77004	14009.89	29812.80	-9362.04***
B. Individual-Level							
Received prenatal attention	4779624	0.97	0.16	17248984	0.98	0.15	-0.00***
No. of prenatal appointments	4638007	7.03	3.12	16881460	7.39	3.37	-0.37***
Prenatal care in first trimester	4818118	0.71	0.45	17366386	0.75	0.44	-0.03***
Low birth weight child	4818118	0.05	0.23	17366386	0.06	0.24	-0.01***
Birth Weight (grams)	4559969	3158.31	468.21	16462363	3148.18	475.02	10.12***
Private Hospital	4818118	0.30	0.46	17366386	0.19	0.39	0.11***
Single Mother	4818118	0.09	0.28	17366386	0.10	0.31	-0.02***
Mother's Schooling (years)	4818118	10.41	3.00	17366386	11.26	2.92	-0.85***
Mother's age	4794293	25.15	6.30	17297933	25.37	6.31	-0.22***

Notes: Columns (1) to (3) show munipalities and mothers not exposed to the FTS program. Columns (4) to (6) show the same for exposed municipalities and mothers. Column (7) shows the difference between columns (2) and (5) and its significance at conventional levels
*, **, *** Significant at the 10%, 5%, and 1% levels, respectively.

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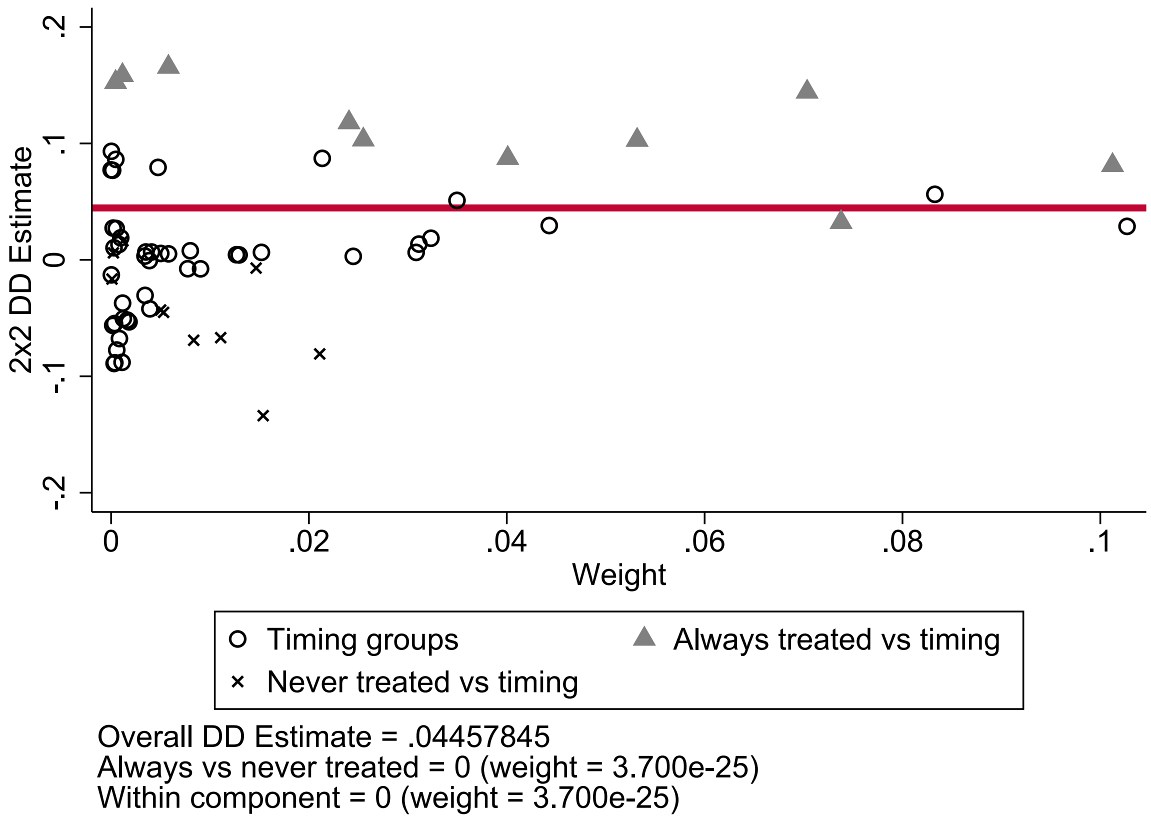
Appendices

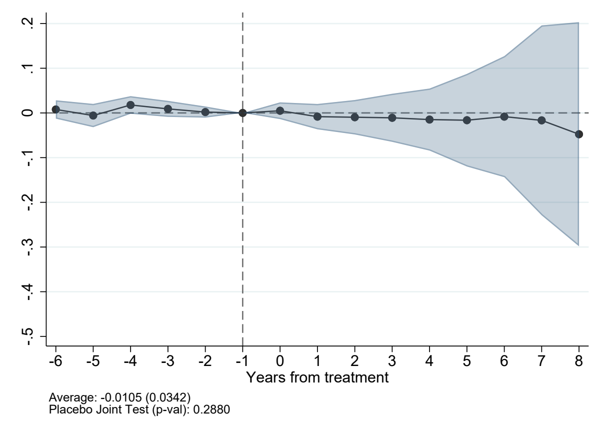
Figure A1: Bacon Decomposition FTS on Fertility



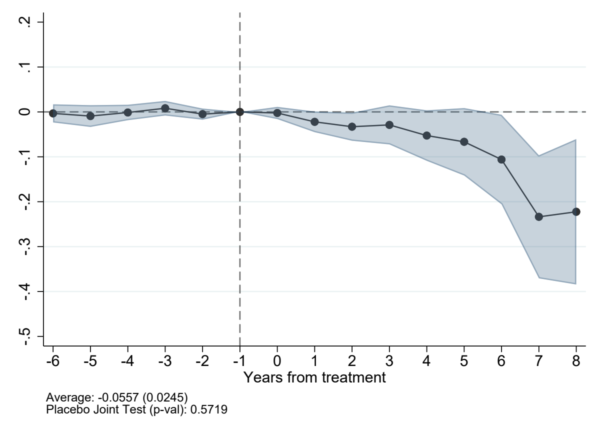
Notes: Estimates on a balanced panel and municipality population weights

Figure A2: FTS Effects on Fertility using alternative measures

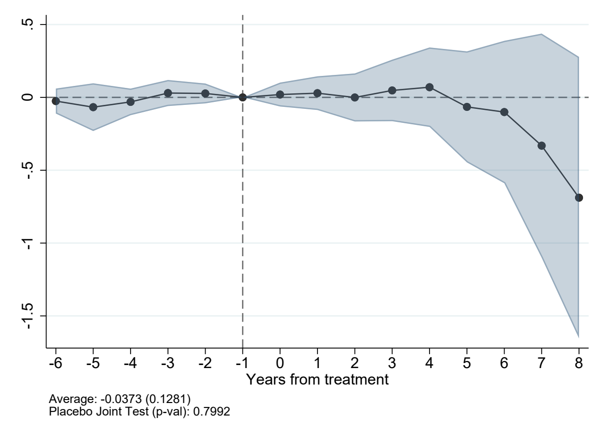
(a) IHS First Born



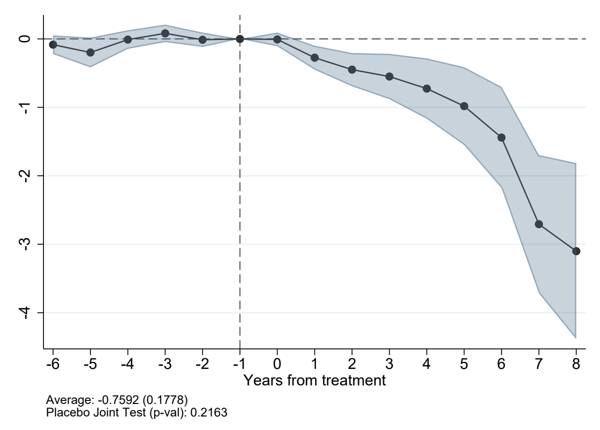
(b) IHS Second+ Child



(c) First Born /1000 population

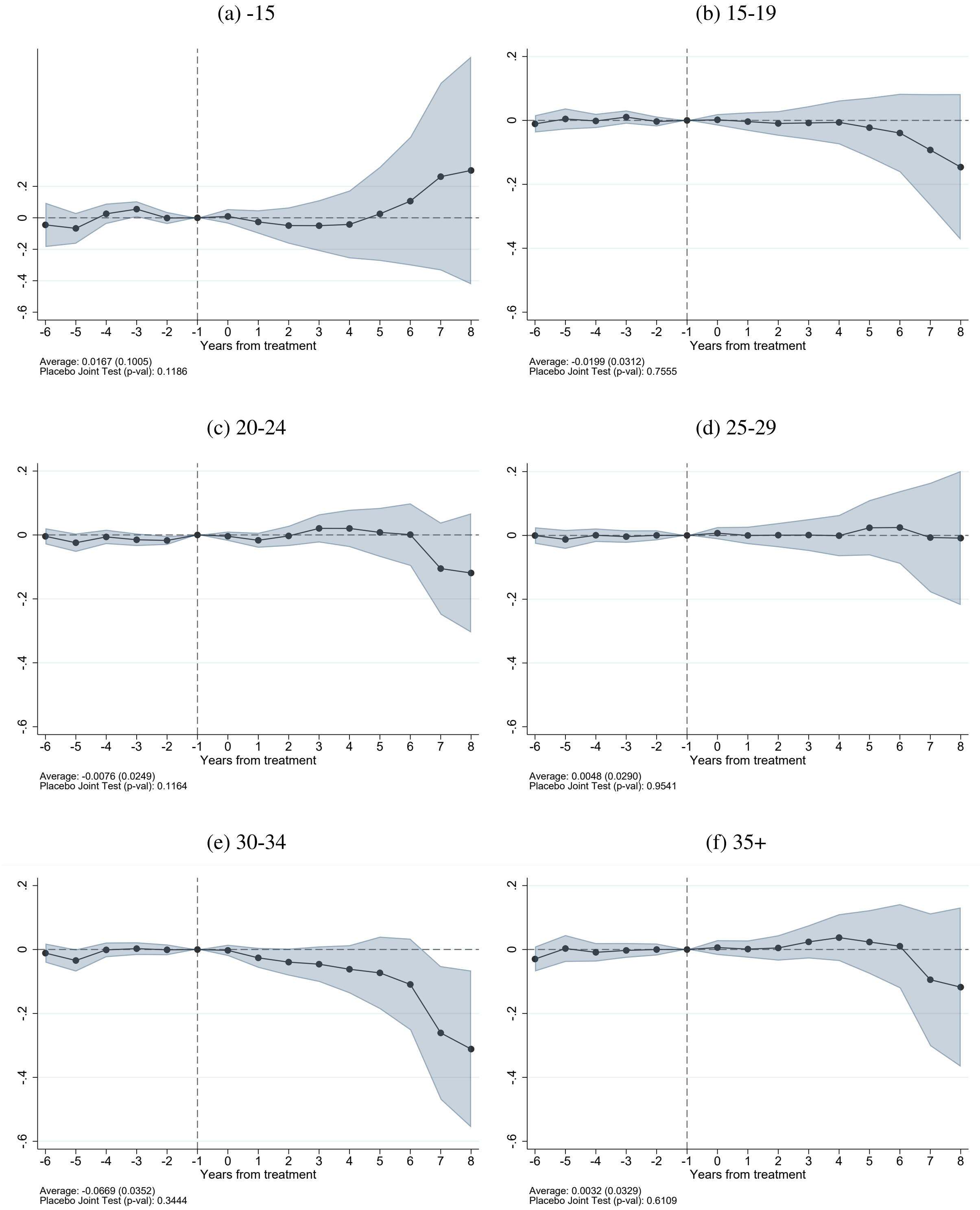


(d) Second+ Child /1000 population



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 controlling for poverty level (above vs. below the median) and average mothers' age and schooling. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

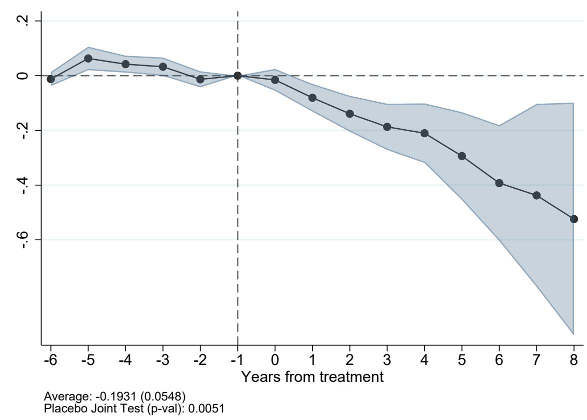
Figure A3: Effects on Log-Births by Mother's Age



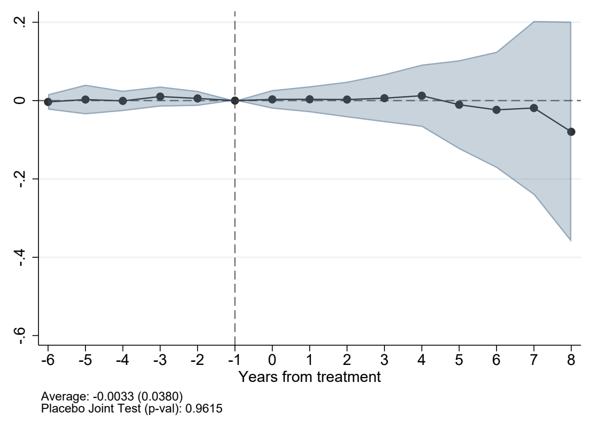
Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression controlling for municipality poverty level, average mothers' schooling, proportion of single mothers and the proportion of births in private hospitals. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

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

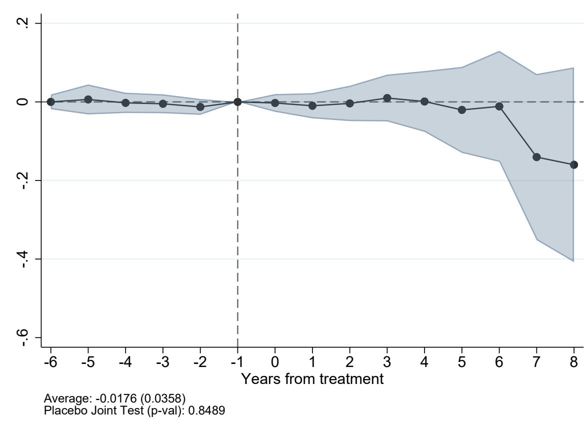
(a) -15



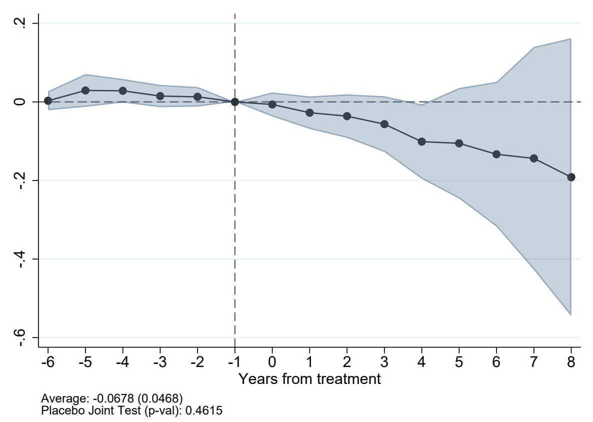
(b) 15-19



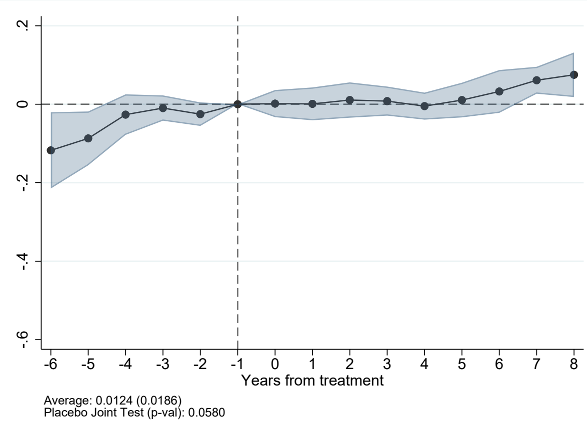
(c) 20-24



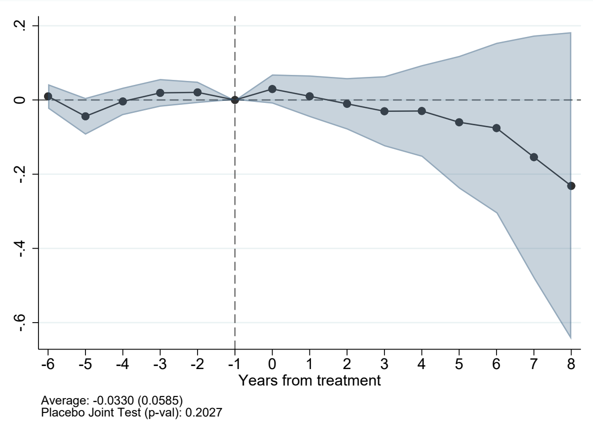
(d) 25-29



(e) 30-34



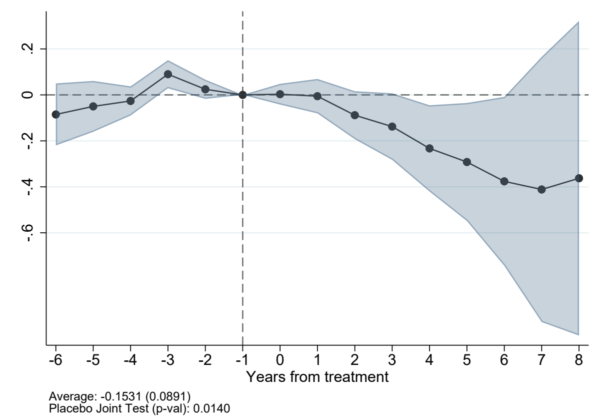
(f) 35+



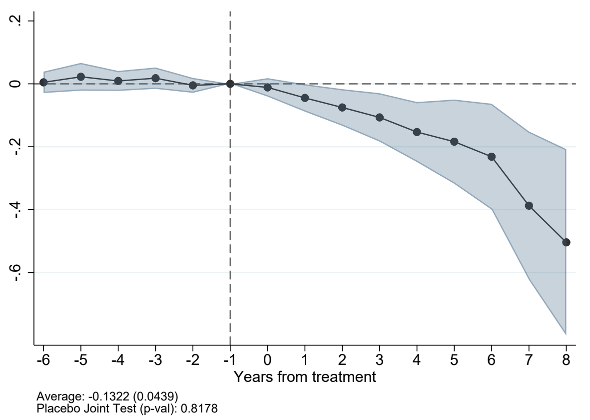
Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression controlling for municipality poverty level, average mothers' schooling, proportion of single mothers and the proportion of births in private hospitals. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A5: Effects on Log-Births by Mother’s Age: Second+ Born

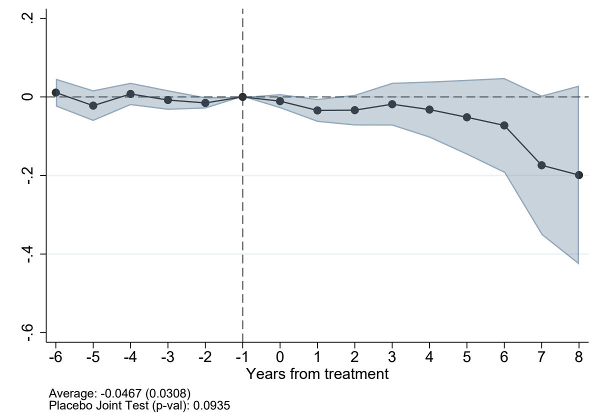
(a) -15



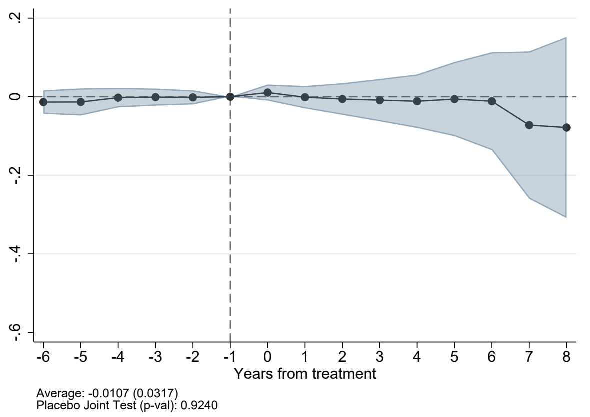
(b) 15-19



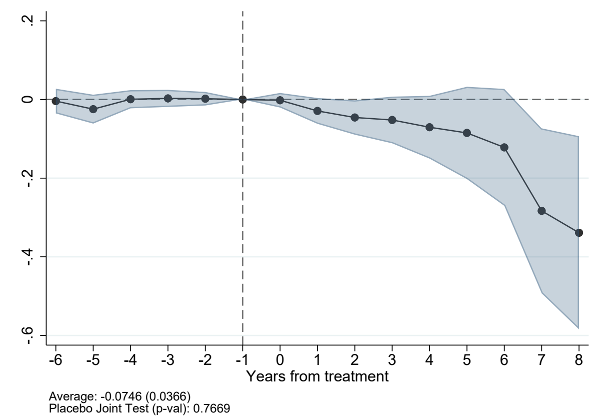
(c) 20-24



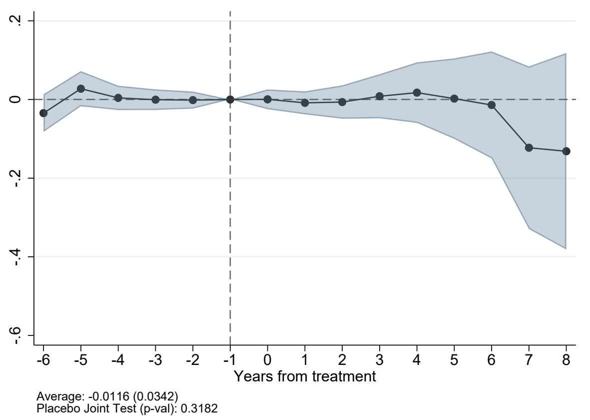
(d) 25-29



(e) 30-34



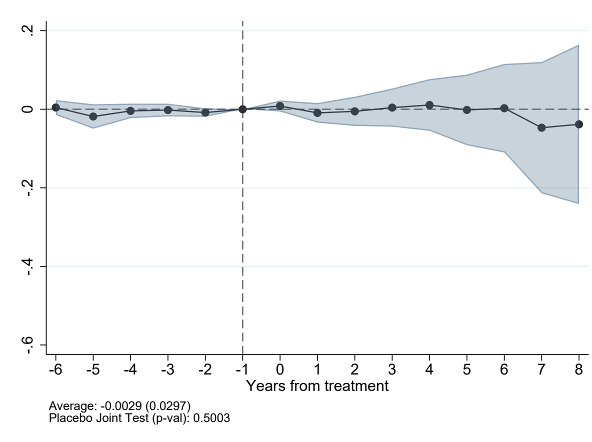
(f) 35+



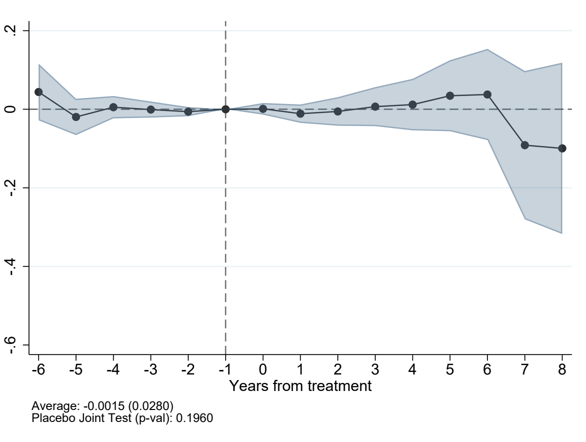
Notes: Estimated coefficients and their 95% confidence intervals for indicators for the years prior to and after the municipality adopted the FTS program. All estimates come from a single regression controlling for municipality poverty level, average mothers’ schooling, proportion of single mothers and the proportion of births in private hospitals. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A6: Dynamic Effects: Log-Births by poverty and education levels

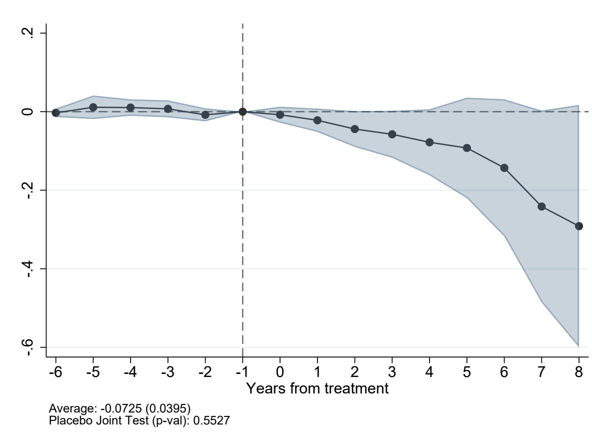
(a) Richer



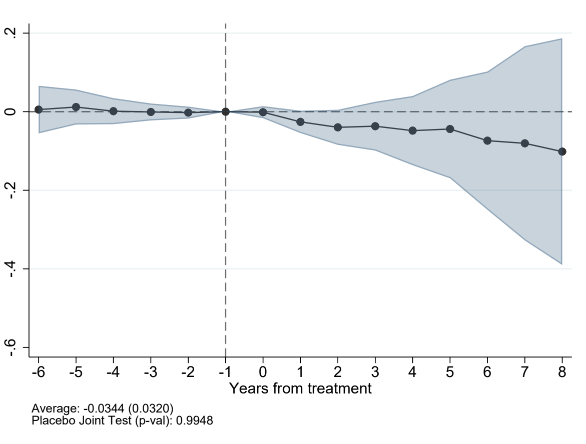
(b) More Educated



(c) Poorer

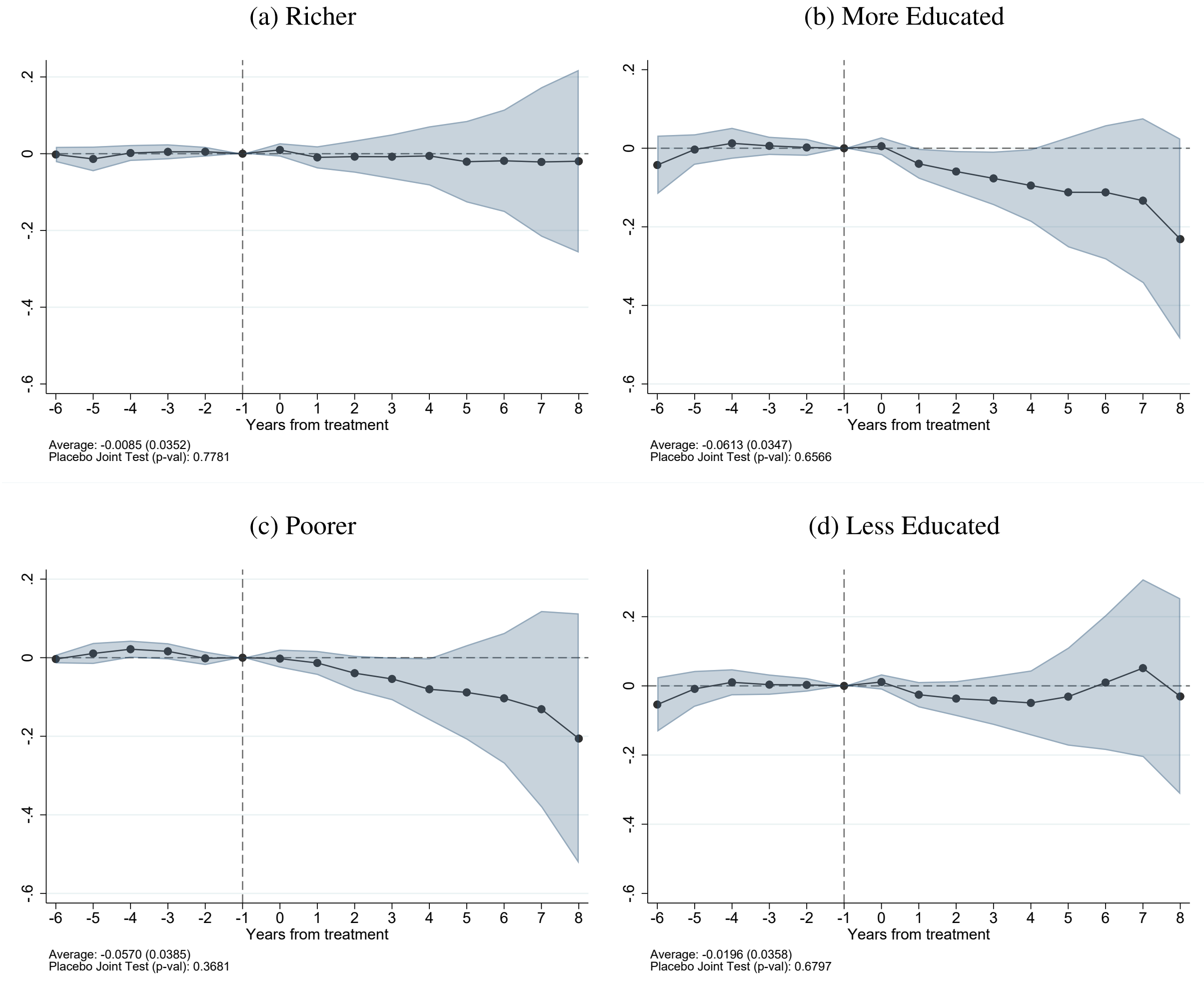


(d) Less Educated



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 using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible, for poverty level (above vs. below the median), average mothers' schooling and age, municipalities' population and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.

Figure A7: Dynamic Effects: First Born 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 using de Chaisemartin and d'Haultfoeuille (2020) method and controlling, when possible, for poverty level (above vs. below the median), average mothers' schooling and age, municipalities' population and municipality-specific time trends. Standard errors are clustered at the municipality level and bootstrapped with 1000 repetitions.