Free Childcare and the Motherhood Penalty: Evidence from São Paulo

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Abstract

Latin America consistently has some of the world's largest child penalties (or motherhood penalties) for women, and while subsidized childcare is often advanced as a remedy, the literature on its effectiveness is scarce outside developed countries. This paper estimates the impact of a rapid expansion of public childcare on mothers' careers in the city of São Paulo. We leverage the precise location and timing of the expansion of childcare facilities, coupled with detailed data on the labor market and household characteristics to identify effects on mothers' labor market participation and earnings. Using a difference-in-differences approach, we compare the child penalty in districts that experienced a large and rapid expansion of childcare with districts with no significant expansion. Our results show that an additional seat per child leads to an increase of 6.4 p.p. (20%) in the mothers' formal employment after the first child's birth. We do not detect any effect of this expansion on two comparison groups: mothers-to-be and fathers.

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1 Introduction

Proposals for addressing gender inequality in the labor market often focus on providing subsidized childcare. A large part of the earnings gender gap is explained by the he "child penalty," the dip in female labor-force participation after childbirth (Angelov et al., 2016; Bertrand et al., 2010; Kleven et al., 2019). Affordable childcare provision could strongly impact maternal labor supply and thus help address the workplace gender imbalance. While some studies of childcare provision have shown positive effects, many others have found null results (Havnes & Mogstad, 2011; Kleven et al., 2020; Olivetti & Petrongolo, 2017). Latin America has some of the largest child penalties in the world, but to date very little evidence supports arguments for increasing childcare to alleviate the issue in the region (Kleven et al., 2023).

In this paper, we use the setting of São Paulo, Brazil, to examine how a large expansion of free public childcare impacted the child penalty. The expansion took place from 2008 to 2018, when the share of children enrolled in public childcare went from about 25% to close to 75%. We estimate the effects of this program on women's labor force participation and earnings using a difference-in-differences design that leverages the expansion rollout, accounting for heterogeneous effects. We use data from three matched administrative datasets that provide information on childcare coverage over time, formal employment and earnings, and household characteristics. We find that an additional seat per child increases nearby mothers' formal employment by 6.4 p.p. (20%) and total earnings by 20% following the birth of a first child. The timing of the effects coincides with the expansion and we find no evidence against the parallel trends assumption. We find null effects for mothers-to-be and for fathers, supporting the interpretation that results are driven by the supply of childcare itself, not by concomitant economic trends.

Prior to 2008, São Paulo had a free public childcare network, but the service

was heavily oversubscribed. High demand and constrained capacity lead to long wait times, such that, in many areas, parents were unable to get a spot for a child before the child was old enough to attend regular pre-school. In 2008, Mayor Gilberto Kassab was elected and declared as a key priority of his administration bringing wait times to zero.[1] The new administration started a fast expansion of public childcare: between 2008 and 2018, São Paulo created an average of 30,000 new free childcare seats every year. This rapid expansion was achieved mainly through partnerships with private-sector childcare providers. This model increased flexibility in regulations regarding location and hiring, allowing for a rapid expansion in new facilities, sometimes whithin a matter of a few months.

To measure mothers' labor outcomes and access to childcare, we combine three administrative datasets. First, we use data from the São Paulo Department of Education on each public childcare facility's opening date, the number of seats, and the location. Second, in order to study labor market outcomes, we use data from the Annual Account of Social Information (Relação Anual de Informações Sociais, or RAIS), an administrative panel containing information on all formal employer-employee links in the country. Third, we are able to match these two datasets through the Single Registry (Cadastro Único), an administrative dataset that centralizes information on families receiving any government benefit. The Single Registry allows us to observe families' characteristics, including addresses and the date of birth for each household member.

Our main empirical strategy exploits the timing of the roll-out of large expansions in childcare in São Paulo across districts, using a dynamic differences-in-differences framework. Although practically every district saw some increase in childcare availability over the period, we can identify periods of discrete increases in particular districts.

[1]Kassab was elected vice-mayor in 2004 and succeded from José Serra as mayor in 2006. He was directly elected in 2008 and served until 2013. The main changes his administration made to childcare policy were continued in the following administrations.

We first identify a "treatment year" for each district, which is either the year when the first childcare opened (if none existed before 2005) or the year with the single largest growth in childcare seats. The key comparison is between districts that had a large expansion (i.e., those in the top 40%) and those with only a small expansion (i.e., those the bottom 40%). We show that the results are robust to alternative definitions of the treatment. To address the concerns raised by the recent literature on staggered adoption with heterogeneous treatment[2] we estimate our parameters of interest following Callaway and Sant'Anna (2021). We also obtain similar results with a fixed-effects based strategy, close to that of (Kleven et al., 2020), that does not necessitate the identification of discrete expansion periods and that uses all variation in childcare availability. This alternative strategy focuses on the comparison between labor force participation of mothers and mothers-to-be within the same district, as childcare availability increases.

Our results show that free childcare leads to a significant and persistent reduction in the child penalty. We find that an expansion episode results in an extra 0.33 seat per child, and an average increase of 2 p.p. in mothers' labor force participation after the birth of a first child. These figures imply that each additional seat per child is associated with a 6.4 p.p (20%) increase in mothers' employment. Similarly, total annual earnings increase by 490 BRL (20%). We do not find any evidence that pre-existing differential trends for mothers in the labor market drive our results. As a placebo test, we show that the expansion of childcare did not affect mothers-to-be, a demographically similar population that should not be affected by childcare. We also find no effects for fathers, and find suggestive evidence that the effects are stronger for mothers with lower education levels and living in areas with a higher share of female household heads.

This paper contributes to the literature on the effects of childcare on mothers' labor market outcomes. A first wave of studies exploits quasi-experimental variation in

[2]See de Chaisemartin and D'Haultfoeuille (2019) for a summary.

the roll-out of public childcare effects on mothers' labor outcomes, finding mixed results (Andresen & Havnes, 2019; Baker et al., 2008; Bauernschuster & Schlotter, 2015; Berlinski & Galiani, 2007; Bettendorf et al., 2015; Cascio, 2009; Havnes & Mogstad, 2011; Rabaté & Rellstab, 2022).[3] A more recent wave of studies used richer sources of variation, typically combining the timing of childcare expansion with either timing of birth or eligibility criteria to generate alternative comparison groups (Brewer et al., 2022; Carta & Rizzica, 2018; Kleven et al., 2020). Identification is more credible in this case because the strategy can deal with time-varying unobservables correlated with childcare availability. Despite the methodological improvements, this second wave also finds mixed results.[4] This paper fits within this second strand of literature, providing evidence from a middle-income country, a context more similar that of a large share of women across the world.

This paper is closely related to Attanasio, de Barros, et al. (2022), which analyzes a similar context to ours, in the city of Rio de Janeiro. They provide evidence from a randomized controlled trial of the effect of being eligible for childcare through a randomized list. In contrast to our results, they find null effects on mothers' labor force participation, but a significant increase in grandparents' and siblings' employment. The different findings may be driven by contextual differences. In Rio de Janeiro, coverage of public childcare was between 7% and 15% during their study, much lower than even the starting level of the expansion we study. Families in their sample are also considerably poorer than the average in our case, and mothers are much more likely to be working in the baseline: 70% in their sample, 44% in ours. We interpret their results as complementary to ours and more informative of effects at a small coverage, while our setting may be more representative of contexts where a large share of children are covered.

Among other papers that studied the effects of childcare in Latin America are

[3]For a review of the literature, see Cascio et al. (2015) and Albanesi et al. (2023).

[4]Müller and Wrohlich (2020), Carta and Rizzica (2018) and Brewer et al. (2022) find significant positive effects, while Kleven et al. (2020) finds null results.

Attanasio et al. (2013), Bernal and Fernández (2013); Bernal et al. (2019) and Attanasio, Baker-Henningham, et al. (2022) in Colombia; Hojman and López Bóo (2019) in Nicaragua; Rosero (2012) in Ecuador; Araujo et al. (2019) in Peru. This literature has been focused on children's health and development outcomes. However, ignoring the impacts on the family more broadly may lead to understatement of the impacts, and thus, under-investment.[5] Among these, only Hojman and López Bóo (2019) and Rosero (2012) look at parental labor, finding positive effects on mothers.

This paper is organized as follows. In Section 2, we present the background and describe the childcare program in São Paulo. Section 3 details the data used in this paper. We explain the methods in Section 4 and Section 5 presents the results. Finally, Section 6 concludes.

2 Setting

To assess the impact of free childcare on the child penalty, we study the expansion of public childcare in São Paulo, a city of 12 million people. In this context, female labor force participation is relatively high compared to much of the developing world, including the rest of Brazil, leading to high demand for childcare services. As a response to this high demand, the municipal government prioritized increasing childcare availability, leading to the share of children enrolled in public childcare going from 25% to 75% between 2008 and 2018. The program preferentially matches families to childcare facilities within the same district, facilitating the identification of the relevant market for each childcare center. One key factor that made expansion without decreases in quality possible was the intense use of public-private partnerships with educational NGOs.

[5] See Evans et al. (2021) for a systematic review.

As in other developing countries, women's labor force participation in Brazil has increased substantially over the last few decades. From 1992 to 2012, the share of working women between the ages of 15 and 59 rose from 52% to 61% (Barbosa, 2014), reaching rates similar to those of developed countries. São Paulo, in particular, is a dynamic labor market that attracts economic migrants from other areas and has a high proportion of mothers working outside the home (73.3% in 2019). These factors, combined with low stigma around women working outside the home (Chioda & Verdú, 2016), result in high demand for public childcare. Despite increased participation, the motherhood penalty remains large. Figure 3 shows the motherhood penalty in the formal labor market in São Paulo from 2008 to 2018. After childbirth, employment falls by about 12 percentage points, and there is only a slight recovery after 6 years.

In response to the high demand for public childcare, and motivated by a change in the childcare administration, the city administration started to expand this service. In 2010, about 30% of children between 0 and 3 were enrolled in publicly funded childcare, and the wait time for a seat could exceed 400 days. Since then, the provision of free public childcare in São Paulo increased by a factor of more than 2.6 until 2018, as shown in Figure 1. This rapid expansion was achieved almost exclusively through partnerships with nonprofit childcare providers. Figure 1 shows that the number of seats provided under the partnership model more than tripled in the period. Meanwhile, direct municipal provision increased only very slightly. The number of exclusively private providers has also remained flat over the period.

Under the partnership model, the city government contracts with specialized nonprofits to provide childcare services. The government guarantees the physical space (usually rented) while the service provider has flexibility to make administrative decisions, including hiring and firing caretakers. The quality standards are the same as those of facilities under direct public provision, and in general they are relatively high. The city

stipulates an age-dependent maximum ratio of children for each provider. Public childcare facilities work five days a week, covering 10 hours between 7 a.m. and 7 p.m. but with a reduced schedule during school breaks. Besides daycare activities, like physical play and reading, the facilities also provide free regular meals and snacks, helping prevent malnutrition among the poorest children.

The rules for the allocation of seats in the childcare system imply that the relevant geographical unit of analysis is the educational district. To rationalize the enrollment process, in 2006 the city's administration implemented a centralized online system. In this system, parents request a spot for their child and an algorithm matches them to a facility with available seats in the educational district where they live. If there is no availability, they may be matched to a neighboring district with excess capacity or be placed on a wait list. Poorer families receive priority on the wait list.

This centralized system also allowed the municipality to identify places with an excess demand for public childcare in order to better direct the expansion efforts. Figure 2 shows the enrollment rates in different districts over time. While almost every district had some increase in enrollment, the largest gains happened in the relatively poorer periphery. Mothers in the city's outskirts are much more likely to be unable to afford childcare services, and to depend on wage income, and to live farther away from most jobs in the city. They therefore tend to place a high value on public childcare.

3 Data

To study the effect of childcare on mothers' formal employment, we start with the sample of families in the Single Registry. We use their addresses to match them to school districts and use childcare information from the São Paulo Department of Education to obtain the

supply of childcare services in the area. Finally, we use mothers and fathers personal IDs to match them to RAIS and reconstruct their formal labor market participation.

3.1 Childcare Centers

Our data on childcare provision come directly from the São Paulo city government, which makes it available through the city's open data portal.[6] The data include the contracts and opening dates of all childcare facilities as well as location and number of available seats. Childcare availability increased by about 300,000 seats between 2008 and 2018 throughout the city, particularly at the periphery, corresponding to an increase of about 50 p.p. in seats per child.

To obtain data on population per school district and other demographic data at this level, we do a spatial merge of census tracts and school districts. Since these areas are not designed to be exactly compatible, where necessary we assign population to different educational sectors proportionally to the area of overlap. There are 577 educational districts in the city, and an average educational district includes about 7,500 households and 24,000 people according to the 2010 Census.

3.2 Single Registry

We use data from the Single Registry (Cadastro Único) for two main purposes: linking school districts to labor outcomes in the RAIS and observing family characteristics. The Single Registry is a federal registry used for several social programs to verify eligibility and track recipients over time. It started exclusively as Bolsa Família's administrative database but evolved through the years to be the primary federal dataset on poverty.

[6] http://dados.prefeitura.sp.gov.br/

Currently, more than 20 social programs use it, covering virtually all of Brazil's poor (Campello, Neri, et al., 2013). The Single Registry aims to include all households with income per capita below one-half of the minimum wage (R$3060 in 2010), which is much higher than the poverty threshold (R$1680 in 2010).

To be eligible for any government benefit that uses the Single Registry,[7] families must have a valid (complete and up-to-date) registration, that they continue to update at least every two years. They must undergo interviews with local government agents where they answer a standardized questionnaire on their earnings, living conditions, demographic and occupational characteristics, and personal tax ID (CPF). They must also inform authorities of any relevant changes to family size or income.

We use a December 2017 extraction from the Single Registry to construct the primary analysis dataset in this paper. We start with the 3 million individuals with addresses in São Paulo and identify potential mothers. We classify as mothers all women between 16 and 65 years of age listed as household heads or spouses to the household head whose family contains at least one child aged 13 or below. Out of the initial 3 million individuals, 549,763 are classified as mothers. We geocoded the street addresses of all families with at least one mother in our dataset using Google Geocoding API. We then performed a spatial matching to the educational districts. We match this data to the RAIS using the personal tax ID (CPF).

[7]Some of the main programs that are conditional on registration include: Bolsa Familia; Benefício de Prestação Continuada (BPC, a payment for poor elderly or disable persons); Tarifa Social de Energia Elétrica (a discounted energy rate for low-income households); Minha Casa Minha Vida (a program that finances housing).

3.3 RAIS

RAIS, or the Annual Account of Social Information, is a longitudinal dataset of social security records for employees and employers. It is collected by the Ministry of Labor in a compulsory survey of all firms and their registered workers, covering around 230,000 formally registered firms and over 3.5 million workers annually. RAIS provides information on workers' demographics (age, gender, schooling, race), job characteristics (occupation, wage, hours worked), hiring and termination dates, and the personal tax ID (CPF). It also includes information on many firm-level characteristics, such as the number of employees, municipality, firm tax ID, and industry code.

We built a panel of formal workers from 2003 to 2018, amounting to 159 million worker-year observations in Brazil. We do not restrict our data on workers to São Paulo because some residents may have jobs in other municipalities. We match the sample of mothers in São Paulo obtained in the Single Registry to this panel of workers through their tax IDs. Out of the 156 million worker-year observations, 2.1 million are matched to our Single-Registry-based dataset of mothers in São Paulo. If we find a woman at least once in RAIS, we can re-construct her formal employment history, which allows us to document her pre- and post-childbirth work and earnings. If we do not find her in RAIS for any year, then we know she has not worked in the formal sector during this period. Our measure of employment is a dummy indicating whether the woman appears in the RAIS dataset in that year with at least one job reporting a non-zero amount of hours per week. We also obtain average yearly wages and hours worked from RAIS.

3.4 Descriptive Statistics

Table 1 shows descriptive statistics for mothers-to-be one year before childbirth (column 1), mothers one year after childbirth (column 2), fathers-to-be one year before childbirth (column 3), fathers one year after childbirth (column 4).

The data show a large child penalty for women, accompanied by a small reduction in hours. Comparing mothers one year after childbirth and one year before, we observe a dip in labor force participation of 11 p.p., with total earnings falling by a third. The fall in earnings is mostly explained by lower employment, together with a reduction in the number of hours worked for those employed. There is no reduction in hourly wages.

Men in the sample work and earn considerably more than women. Fathers-to-be have a higher participation in the formal sector (55%) compared to mothers-to-be (44%), and they see only a very small dip after childbirth that could be explained by overall labor market trends. All measures of men's labor market participation and wages are higher than those of women. As a result, women earn 61% as much as men before childbirth and 38% after childbirth. Our sample includes substantially fewer fathers than mothers due to a large fraction of single mothers. The smaller number of fathers leads to lower precision in our estimates.

Because our sample is selected from the Single Registry, it is heavily skewed towards poorer families. To gauge the differences between this sample and the overall population, we can compare it to the 2010 Census. Table 2 shows sample statistics for mothers in the Census and in our sample, restricted to 2010. Our sample covers about 51% of mothers in the Census. Compared to the overall population, they are less educated, more likely to be migrants, less likely to be white and slightly less likely to be formally employed. Their average and median incomes are much lower, and correspond closely to the minimum wage.

4 Empirical Strategy

We analyze the effect of childcare provision on mothers' labor market outcomes using two complementary strategies. First, we follow Callaway and Sant'Anna (2021) in estimating treatment effects in a dynamic difference-in-differences context, focusing on the comparison between districts with an expansion of childcare and districts without. To do so, we define a "time of treatment" for each district based on the timing of large expansion in childcare availability. While this approach deals robustly with concerns over heterogeneous treatment effects, choosing a specific time of treatment can be somewhat arbitrary in our context and does not make full use of the variation in the data. Therefore, we also present results in DID framework that focused on comparing mothers and mothers-to-be within each district, taking advantage of all the variation in the data.

4.1 DID – Between Districts

To study the effect of childcare on mothers' labor market outcomes, we employ a dynamic difference-in-differences strategy. The treatment is a large increase in the availability of childcare during one period, defined as the number of seats per child in the 2010 Census. We compare the evolution of outcomes in treated districts against districts where only a small or no expansion occurred between any two consecutive years. This approach lets us deal with some of the main challenges to identification. In particular, we know that new childcare centers were preferentially built in areas where the wait times were longer, meaning mothers were likely to be more eager to join the formal workforce. This strategy is robust to these level differences as long as the parallel trends assumption is valid.

We define the treatment as happening in the year of the largest expansion for each district. For each district, we compute the largest annual growth in the supply of childcare

seats in the sample period (2005 to 2018). If this level of growth was small relative to that of other districts (i.e. in the bottom 40%), we consider it never treated, while those with large increases (i.e. in the top 40%) are treated. For the districts where the first facility was opened in a given year, we mark that year as the year of the expansion and consider them treated. All other districts are dropped from the sample (i.e., those between the 40th and 60th percentiles).

Recent evidence suggests that "staggered access" estimations might be biased by heterogeneous effects over time (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeuille, 2019). To address this concern, we estimate the parameter of interest following Callaway and Sant'Anna (2021). Adopting their notation, denote by C the group of districts that did not have a large expansion between any two consecutive years and by G_g the group of districts that had a large expansion at some point in the study period. Let g indicate the period in which each district expanded childcare, and let e denote event-time. So, $e = t - g$ denotes the time that has elapsed since the treatment was adopted. Our parameter of interest is given by

$$\theta(e) = \sum_{g \in \mathcal{G}} \mathbf{1}\{g + e \leq \mathcal{J}\} P(G = g | G + e \leq \mathcal{J}) ATT(g, g + e) \tag{1}$$

where

$$ATT(g, t) = E[Y_t - Y_{g-1} | G_g = 1] - E[Y_t - Y_{g-1} | C = 1]$$

and $P(G = g | G + e \leq \mathcal{J})$ indicates the probability of being treated for the first time at time g.

Thus, $\theta(e)$ is the average effect of expanding childcare e time periods after the treatment has been adopted across all districts that are observed to have ever participated in the treatment for exactly e time periods. The key identification assumption is that

treated districts and comparison districts would have followed parallel trends in their outcomes in the absence of the expansion. We cluster the standard errors at the district level and weight the observations by the district's population.

This procedure seems to capture a real feature of the expansion process: somewhat lumpy growth concentrated in a few places each year. Figure 4 shows the evolution of childcare availability over time for the control group and for the groups treated each year. It is clear that each group shows a marked increase over the period we designate as the treatment period, in most cases substantially larger than in any other time span. Meanwhile, the control group shows only a very modest increase in available seats throughout the period. However, there is also a general upward trend in all groups, and several show considerable increases during other years, particularly in later years. To address this complication and to allow a natural interpretation of the results, we estimate the effects of an expansion on the number of seats per child over time. We interpret this parameter as a first stage and use it to rescale the labor market effects. We interpret the resulting estimates as effects of childcare availability on labor market outcomes.

One potential challenge to our strategy is endogenous migration. If families that place a higher value on access decide to move to areas with higher availability of childcare, that could be driving our results. To deal with this issue, we record families in their locations in the Single Registry for the year 2012 and keep it constant over time. This choice mitigates concerns over any effects through endogenous migration, but at the cost of potentially adding error to families' locations and consequently biasing the effects toward zero. However, because the expansion started before 2012, it is still possible that location is affected by earlier treatment. We cannot use earlier years because the Single Registry did not have good coverage prior to 2012.

4.2 DID - Within Districts

Our strategy comparing districts has some important limitations. One issue is that we do not make full use of the available data in two main ways. First, we drop districts that had a median increase in childcare availability and thus are included neither in the treatment nor in the control groups. Second, our primary identification strategy does not take into account smaller increases in childcare availability that are also informative. In order to create a binary treatment, we employ a somewhat arbitrary definition of an expansion. Another important limitation is that when we analyze outcomes for mothers, it is possible that the effects are, at least in part, driven by shocks to the local labor market that affect all workers and are correlated with childcare expansion.

In this section we provide an alternative strategy that deals with these concerns. In this alternative approach, the identification of the effect of childcare availability based on a comparison between mothers and mothers-to-be (that is, women who will give birth one or more years from that period but are not yet mothers) within the same district. Intuitively, we look at how the child penalty evolves as childcare availability increases in a given district. Under the hypothesis that labor market outcomes among mothers-to-be are not affected by the presence of childcare, we can identify effects even if childcare investments are correlated with arbitrary labor market trends, as long as these trends affect women irrespective of motherhood status. This alternative strategy does not rely on identifying particular periods as expansions and treats all changes in childcare availability equally.

To illustrate, let us consider a single district where childcare availability increased over time. Suppose we observe employment for mothers and mothers-to-be. In this context, we can identify the effects of childcare availability using a two-way-fixed-effects strategy with a continuous treatment. Mothers are the treated group, and mothers-to-be

are the comparison group, and we observe with them before and after an expansion in the supply of childcare. We denote mothers by $m = 1$ and mothers-to-be by $m = 0$, and we designate the period before the expansion as $t = 0$ and after the expansion as $t = 1$. Then:

$$Y_{m,t} = \alpha + \beta \cdot Availability_t \cdot \mathbb{1}_{m=1} + \gamma \cdot \mathbb{1}_{m=1} + \delta \cdot \mathbb{1}_{t=1} + u_{m,t}$$

In this regression, $Y_{m,t}$ is the outcome of interest (e.g., average employment of women in period t). $Availability_t$ is the ratio between childcare seats and children ages 0 to 3 at time t. The coefficient α is the constant for mothers-to-be in period 0, γ is the motherhood differential if $Availability_t = 0$, and δ is the period 1 differential.

In this case, β identifies the effect of childcare under the usual difference-in-differences assumptions. Importantly, the parallel trends assumption is different from the one required by the between-districts strategy. In this case, we require that the evolution of the potential outcomes of mothers and mothers-to-be has to follows the same trends over time. The Stable Unit Treatment Value assumption implies that mothers-to-be cannot be affected by childcare availability, either by anticipation or general equilibrium effects.

We build upon this simplified model in two ways. First, instead of the binary of mothers versus mothers-to-be, we use time since childbirth (τ), allowing childcare to have different effects depending on the age of the child / proximity to childbirth. Second, we stack all the different districts, with all fixed effects being fully flexible between districts. With i denoting an individual and t a year, we let:

$$Y_{d,t,\tau} = E[y_{i,t} | \text{district} = d, \text{time since childbirth} = \tau]$$

. The estimating equation is therefore:

$$Y_{d,t,\tau} = \alpha_{d,\tau} + \sum_{\substack{k=-4 \\ n\neq -2}}^{6} \beta_k Availability_{d,t} \cdot 1_{\tau=k} + \gamma_{d,t} + \varepsilon_{d,t,\tau}$$

In this regression, any stable, preexisting local patterns in the child penalty that are not related to childcare availability are captured in $\alpha_{d,\tau}$ (e.g., areas where mothers are particularly unlikely to work because of inadequate access to jobs). Any local labor market shocks or trends that are common to all women irrespective of motherhood status are captured in $\gamma_{d,t}$. Availability of childcare increases the proportion of women working at a rate that depends on the age of the child, β_τ: we expect a coefficient of 0 for $\tau < 0$ and a positive coefficient for $\tau \geq 0$.

Since childcare availability varies only with (d, t) but not τ, we need to choose a comparison group against which the effects of availability are defined, just as we did in the simplified example above. We use $\tau = -2$ as the reference and therefore, assume that childcare availability has no effect on women two years before having their first child. This allows for anticipation effects, as long as they are limited to one year before childbirth.

5 Results

5.1 Main Effects

First, we find that, as expected, a childcare expansion strongly increases availability. Figure 5 shows the effect of opening childcare facilities on the number of seats per child over time. We observe no significant difference in the years before the expansion. The expansion results in a large immediate increase in childcare, with continuing subsequent

growth. Right at the time of opening, there is an increase of about 0.23 seats per child, that increases gradually to about 0.5 after 8 years. Averaging across years 0 to 10 after treatment, the effect is 0.372 seats per child.

Figure 6 shows the effects of childcare expansion on formal employment and earnings for mothers of children aged 0–3 relative to the time of expansion of childcare. Before the treatment, the treated and control groups have no statistically significant difference; we do not reject the joint hypothesis that all pre-treatment effects are equal to zero. One year after the expansion, a statistically significant increase is detectable in the share of working mothers: of close to 1 p.p., increasing over time to about 3.5 p.p. in 8 years, with an average effect of 2.3 p.p. in the post period. Re-scaling this effect by the effect on seats implies that each additional seat available increases maternal employment by 6.2 p.p.

The effects on earnings show a similar pattern, with modest gains at first increasing over time. The magnitudes are fairly modest, consistent with effects being driven by the extensive margin. If we re-scale the average post-treatment effects of 174 BRL by the first stage coefficient, we obtain an effect of 467 BRL. Further re-scaling by the effect on employment results in 7,683 BRL, very close to the average earnings of mothers-to-be. This suggests that compliers are not strongly selected based on potential wages. Alternatively, as Felfe (2012) points out, mothers may choose other margins of adjustment following childbirth, such as trading off lower pay for flexibility and amenities. These results show no evidence of significant differences in wages, possibly because the minimum wage is binding or close to binding for many mothers in our sample.

We estimate the same model using the labor market outcomes among women who are not mothers, but will have a child in 1 to 5 years. If our results are driven by a correlation between general labor market trends and childcare expansions, we would expect to see a similar pattern for women who are not mothers. Since these women will

become mothers within a few years and are drawn from the same population, they are younger overall but otherwise demographically very similar. Figure 7 shows the results. In contrast with the results for mothers, we see no increase in employment or earnings for mothers-to-be. Due to large standard error, we cannot reject that the effects on mothers and fathers are the same in any particular year.

We also estimate the effect of childcare expansion on fathers. Theoretically, fathers' labor market choices could be affected by changes in the overall labor supply in the household, and some effects have been observed in different contexts (Krapf et al. (2020)). However, in this context, the employment rate for fathers tends to be very high, and it is unlikely that childcare will have an appreciable effect. Figure 8 shows that estimated effects are not statistically significant either in employment or earnings. However, the precision of these estimates is very low.

Next, we present these estimates aggregated in Pre and Post expansion effects. In our main results we focus on the interval from 4 years before the expansion to 8 years after it. This choice is motivated by a) progressively lower statistical power away from the expansion date, and b) the concern that the parallel trends becomes a stronger as we impose it on longer time horizons. Table 3 presents the estimates. On average, over the first 8 years, the effect of an expansion is an increase of 0.021 p.p. in the probability that mothers' will be employed, corresponding to an extra 154.7 BRL per year. Re-scaling by the 0.38 effect on seats per child means that one additional seat corresponds to 0.064 mothers employed and earning an extra 472 BRL per year.

As expected from the period-by-period figures, the average effects for mothers-to-be and for fathers are not statistically significant. The estimated effects on mothers-to-be are indeed much smaller and outside the confidence interval of the estimates for mothers. Effects for fathers, however, still have very wide confidence intervals, and effects

on earnings are of a similar magnitude as the ones for mothers.

We test the robust of the findings in two main ways. First, we explore different definitions of an expansion. Instead of considering the top 40% of the distribution of maximum annual childcare growth as treated, the bottom 40% as controls and dropping the middle, we consider the top half as treated and the bottom half as controls, not dropping any district. As shown in Appendix Figures 12 and13, the results are almost identical to our baseline specification.

Second, we show that the results are robust to different estimation methods. Figure 9 shows our main results under four different estimation strategies. First, in black, are the estimates with our baseline strategy, Callaway and Sant'Anna (2021) with "never-treated" districts as controls. In green, we show results also with Callaway and Sant'Anna (2021), but controls are "not yet treated" (i.e. districts that are eventually treated, in the period prior to treatment). In blue, we show estimates by two-way fixed effects. Finally, in orange, we present the results from Borusyak et al. (2021). Results are overall very robust, with the main aparent difference being Borusyak et al. (2021) exhibiting positive estimates for earnings before the treatment, but with large standard errors.

5.2 Within-District Results

As an alternative strategy, we estimate the effect of childcare availability directly in a fixed-effects regression. Figure 10 shows the results. Each bar shows the estimate of an increase of 1 seat per child on mothers (in orange) and mothers-to-be (in blue) by time relative to childbirth. The coefficient 2 years before childbirth is normalized to 0.

Consistent with previous findings, we find null effects for each of the years before childbirth, indicating no effects on mothers-to-be. We also find positive effects for all

years after childbirth. The effects do not seem to fade over time for either employment or earnings until up to six years. The magnitude of the effects on employment is somewhat larger than what we find using the between-districts strategy, but results are broadly consistent. The average effect after childbirth is 8.5 p.p.

Figure 11 shows results of the analogous estimation for men in our sample. In this case the results are less clear. Overall, results are consistent with null effects throughout the entire period. While there are almost no statistically significant coefficients individually, we do observe a large negative effect four years before childbirth, both for employment and earnings. This estimate is statistically significant at 5%, but the overall evidence does not support any effect on fathers either before or after childbirth.

5.3 Heterogeneity

In this section we investigate potential mechanisms by splitting our sample of mothers by migration status; educational attainment; conservative values, proxied by religious affiliation; and the share of female heads of household. We expect migrants to be more sensitive to increased public childcare availability due to being likely separated from extended family and neighbors. We also expect lower education mothers to be more strongly affected. We do find a larger effect on migrants, but education heterogeneity is inconclusive. All results in this subsection are based on the between-districts strategy.

Table 4 Panel A and Appendix Figure 14 show estimated effects for migrants and natives. We define migrant mothers as women who were not born in Sao Paulo, no matter how long they lived there. These people are less likely to have extended family networks they can use for informal childcare, and so may be more sensitive to public childcare availability. The results give only weak support to this hypothesis. While estimated effects are indeed higher for migrants, both in employment and earnings, the differences

are relatively small and not statistically significant.

Similarly, Table 4 Panel B and Appendix Figure 15 presents the estimates for mothers with high and low levels of education. High education is defined as having completed high school. Overall, the results are very similar in magnitude and do not support significant differences by education.

Gender norms are one important determinant of the likely impacts of childcare availability (Rabaté & Rellstab, 2022). We check for effect heterogeneity using the share of Neopentecostals in each district. Neopentecostalism is a growing religion in Brazil, supporting traditional gender norms and emphasizing the role of women as homemakers[8] Table 4 Panel C and Appendix Figure 16 show the results splitting the sample at the median share of Neopentecostals (11p.p.). Effects on employment are about 0.5p.p. higher in areas with more conservative gender roles, but the differences are not statistically significant.

Finally, we explore heterogeneity by the share of households with female heads, as declared on the Census. Results are in Table 4 and Appendix Figure 17. We find considerably stronger effects for districts with above median share of female heads-of-household i.e. above 44%. These areas have almost double the overall effect, while those below the median show effects very close to zero. However, in this case there is significant evidence of negative pre-trends, meaning this heterogeneity may be driven by a failure of the parallel trends hypothesis.

[8]See Mello and Buccione (2020) for a discussion.

6 Conclusion

This paper explores the impact of a significant expansion of free public childcare on the child penalty and maternal labor market outcomes in São Paulo, Brazil. We contribute to the growing literature on the effectiveness of free or subsidized childcare as a remedy for the motherhood penalty in the workplace. Employing a dynamic differences-in-differences framework and leveraging administrative datasets, we add to the literature by shedding light on the effects of childcare provision in a developing country context.

The findings reveal moderate and persistent reductions in the child penalty following the expansion of public childcare. An additional seat per child leads to an increase of 6.4 p.p. in mothers' labor force participation after the birth of their first child, corresponding to a 20% increase, and a proportional increase in earnings. These effects are substantial in comparison with the overall literature on childcare effects.

Overall, our findings highlight one context where free childcare had a substantial impact in reducing the child penalty, and did so at scale. Because childcare availability increased from roughly 25% to 75%, these findings are informative of effects over a substantial range, contributing to wider external validity. On the other hand, specific context and implementation details are key determinants of the policy effects in general. Therefore, this work contributes to a general understanding of this policy space by adding results from a new and understudied context.

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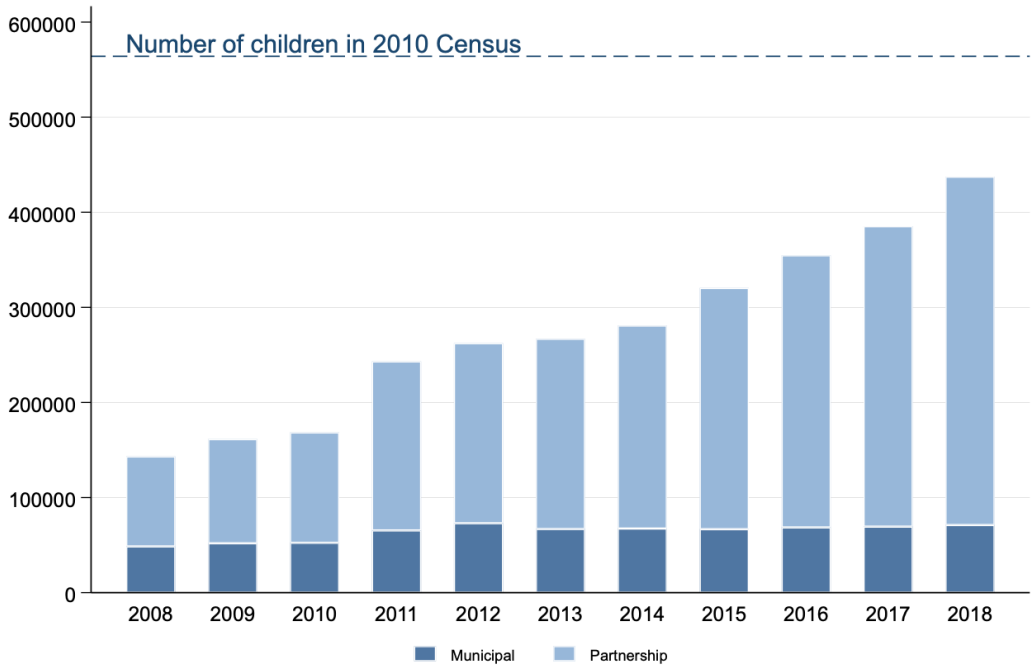
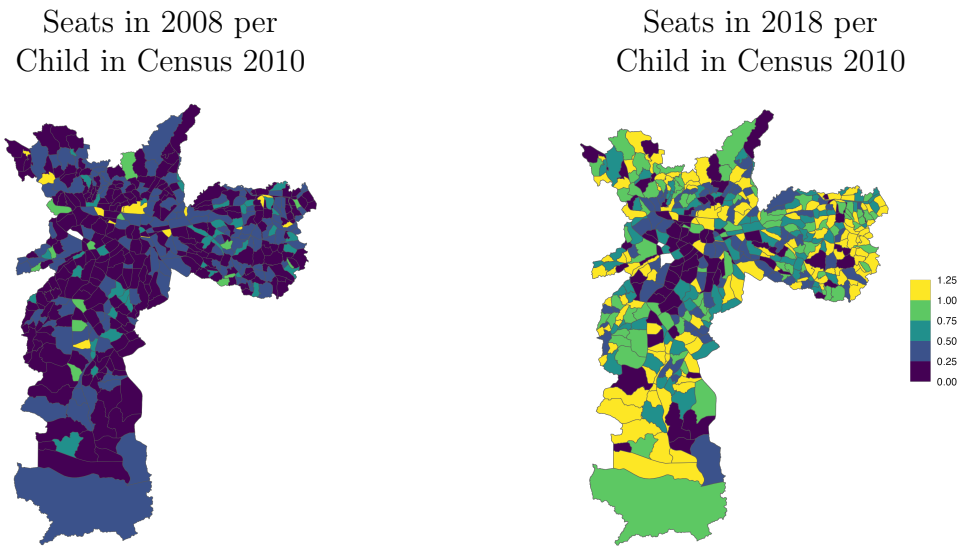
Figures and Tables

Figure 1: Children Attending Childcare by Type of Provider



Notes: This figure shows total enrollment in the childcare system at a) facilities funded by the municipal government and operated by non-profit partners and b) facilities funded and operated by the municipal government.

Figure 2: Childcare Enrollment per Educational District



Notes: This figure shows childcare enrollment rates by educational districts in 2008 and 2018. The rate is defined as the ratio between the number of childcare seats in a given district divided by the population between 0 and 3 years of age residing in that district in the 2010 Brazilian Census.

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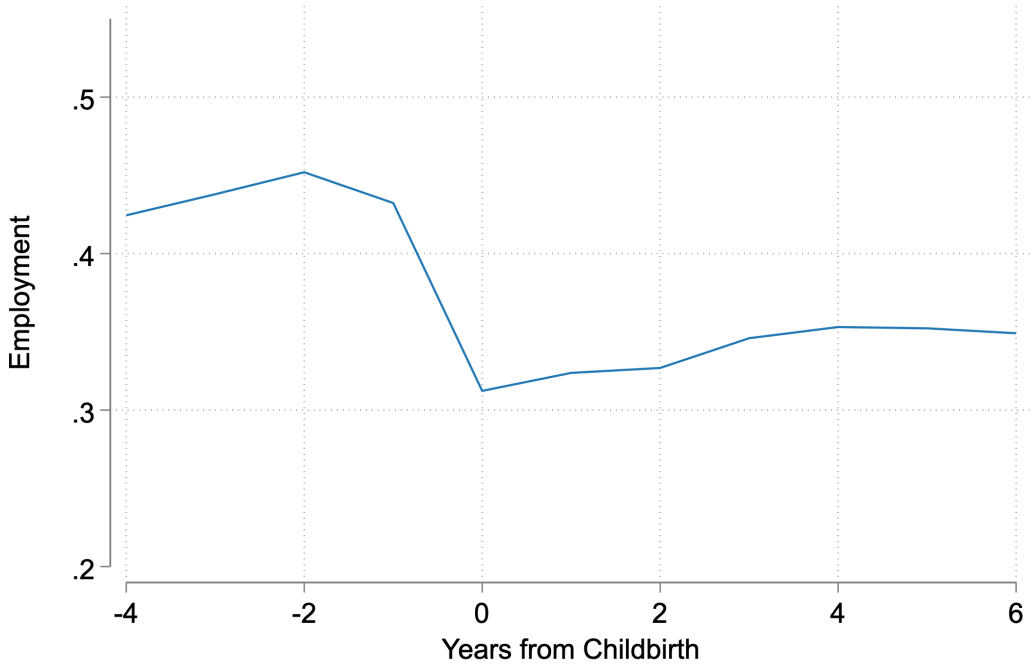
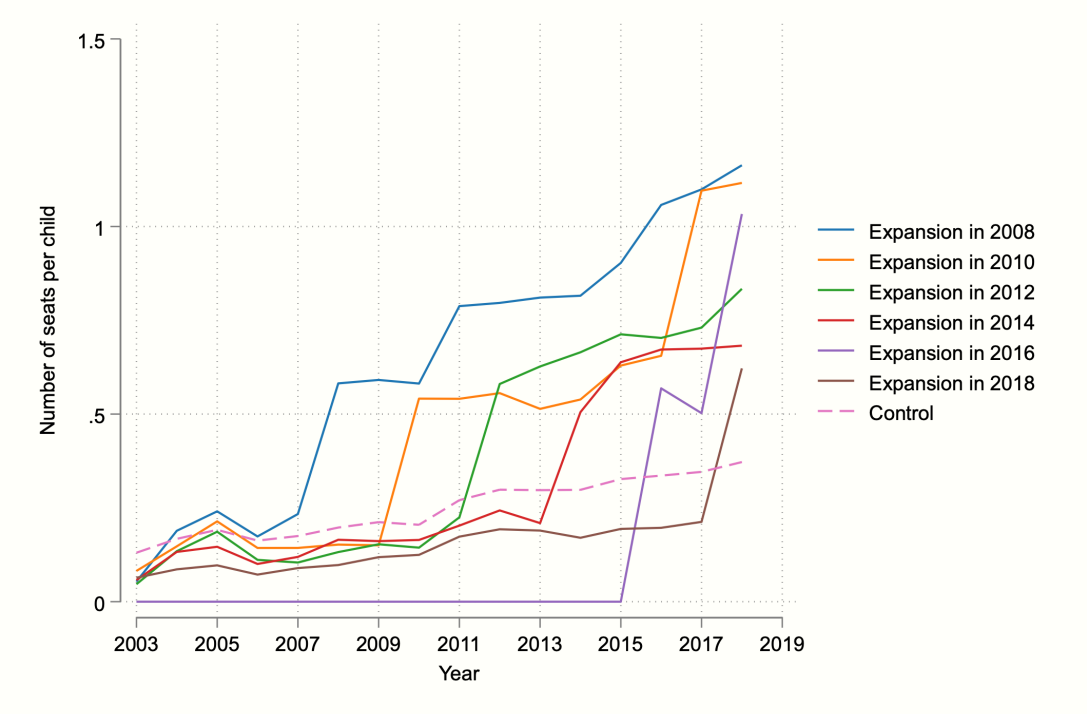
Figure 3: Child Penalty



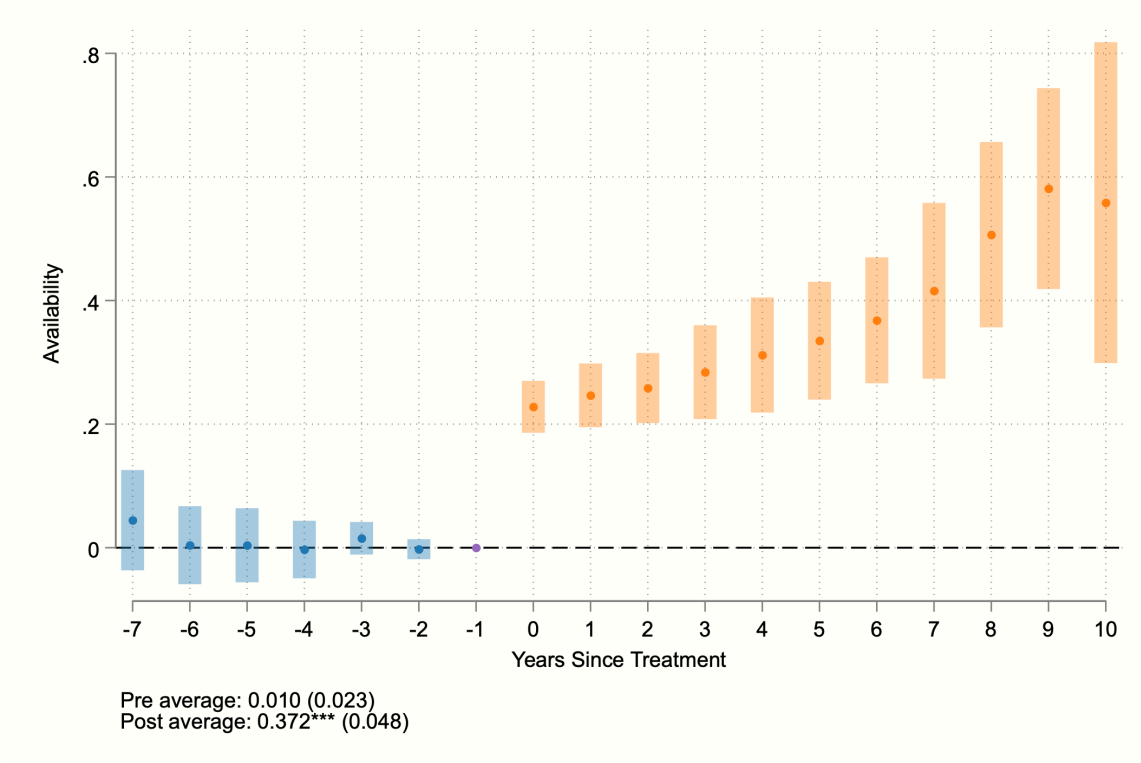
Notes: This figure shows the average employment rate in the formal sector for women in São Paulo around the year of of the birth of a first child, denoted as 0. Data includes years 2007 to 2018.

Figure 4:
Childcare Availability by Treatment Year



Notes: This figure shows the evolution of the number of total seats per child in the 2010 Brazilian Census. Data are grouped by the year of expansion, defined as the year of the largest increase or the year the first childcare facility opened in the district. The control group includes districts where the largest increase in availability was in the bottom 40% of the distribution. To improve visualization, only even years are shown in the plot.

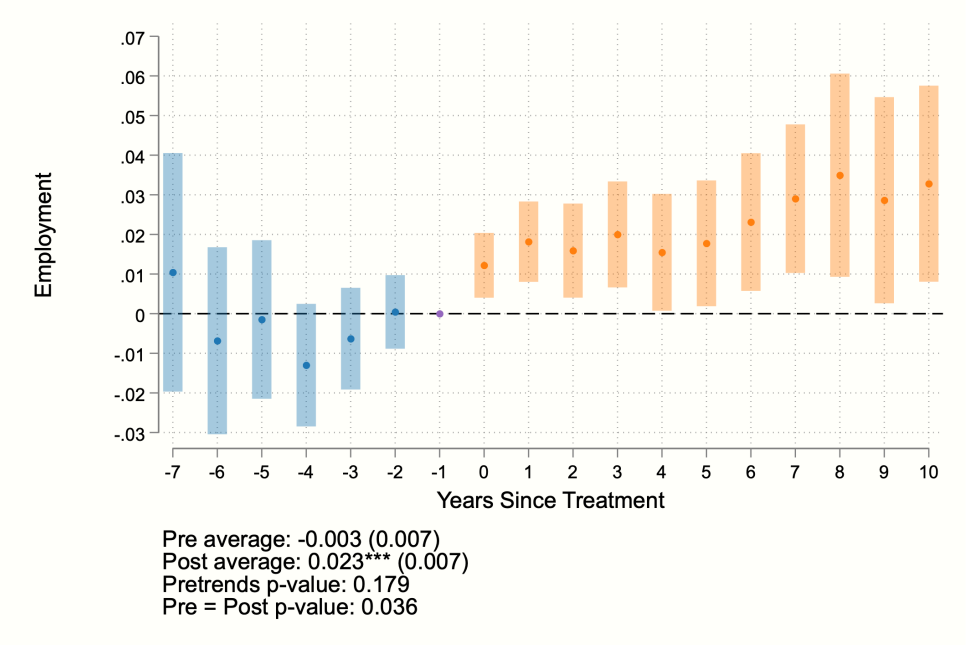
Figure 5: First Stage Effects on Childcare Seats per Child



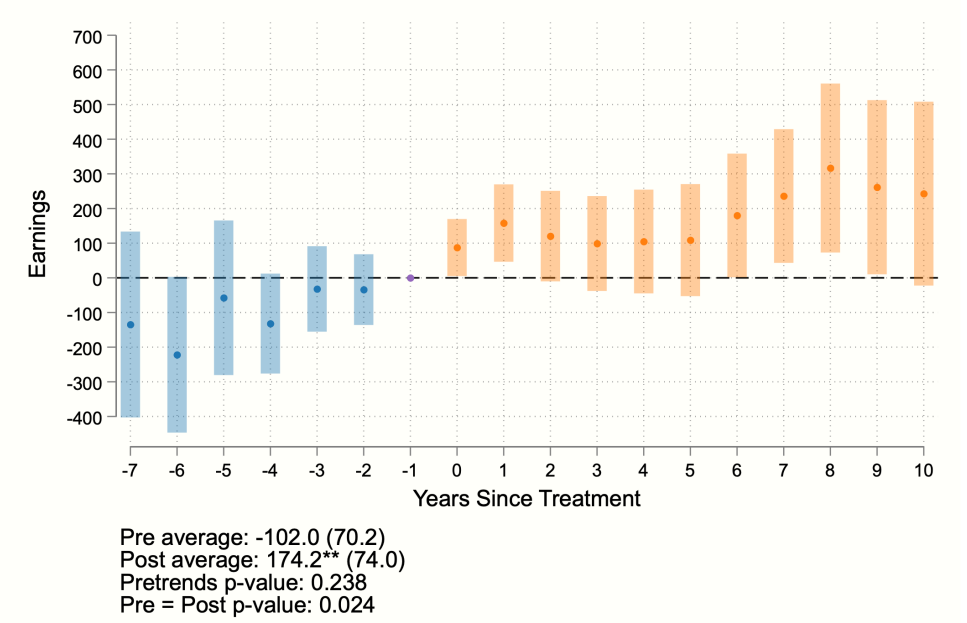
Notes: This figure shows the estimated effect of an expansion in childcare availability, defined as seats per child in the 2010 Brazilian Census. The statistics at the bottom show the average value of the Pre- and Post-treatment estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Effect of Expansion on Mothers

Employment



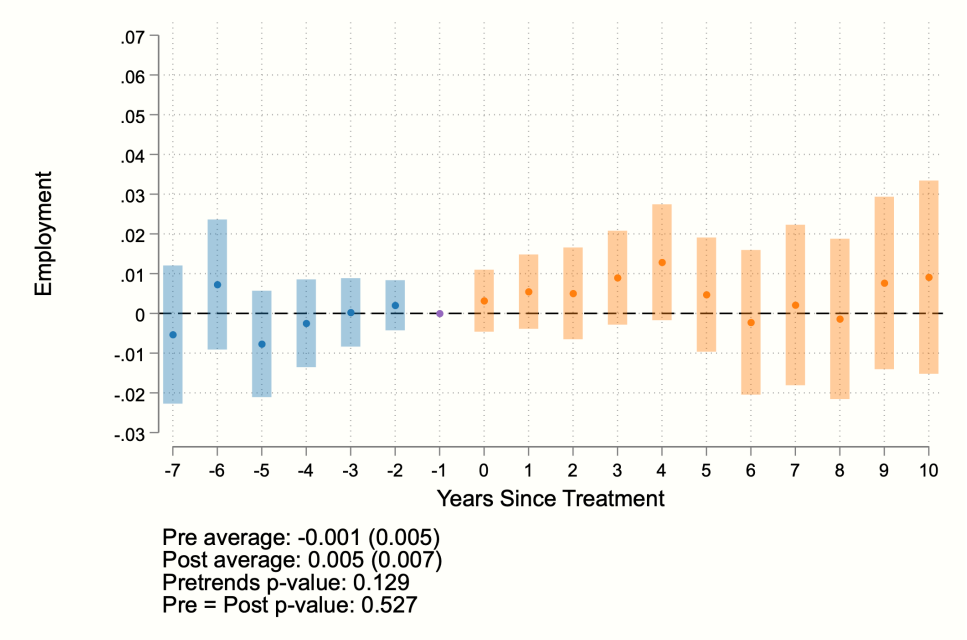
Earnings



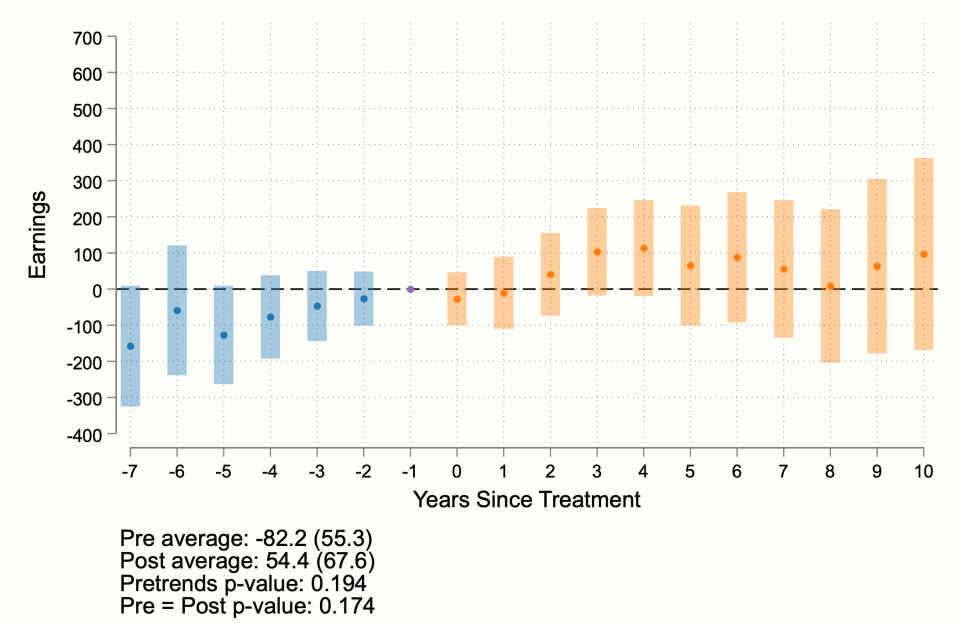
Notes: This figure shows the estimated effect of an expansion on mothers' employment and earnings. The sample includes mothers of children from 0 to 3 years of age. The bars represent uniform confidence intervals. The statistics at the bottom show the average value of the estimates pre-treatement (1) and post-treatment (2), the p-value for the test of the hypothesis that all pre-treatment estimates are equal to zero (3), and the p-value for the test of equality of averages pre- and post-treatment (4). Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 7: Effect of Expansion on Mothers-to-be

Employment



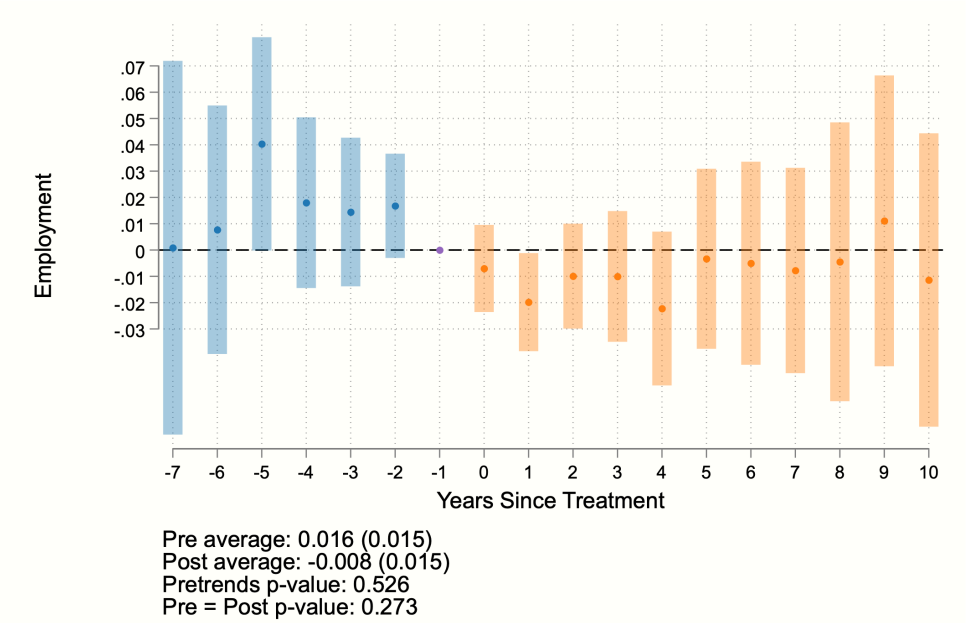
Earnings



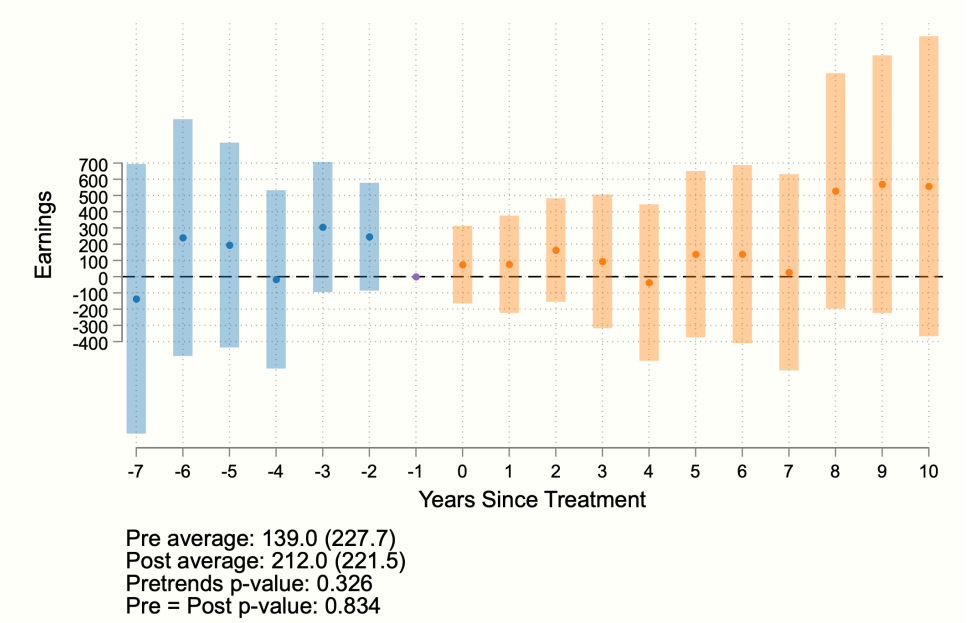
Notes: This figure shows the estimated effect of an expansion on the employment and earnings of mothers-to-be, including from 4 years before childbirth to 1 year before childbirth. The statistics at the bottom show the average value of the estimates pre-treatement (1) and post-treatment (2), the p-value for the test of the hypothesis that all pre-treatment estimates are equal to zero (3), and the p-value for the test of equality of averages pre- and post-treatment (4). Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 8: Effect of Expansion on Fathers

Employment

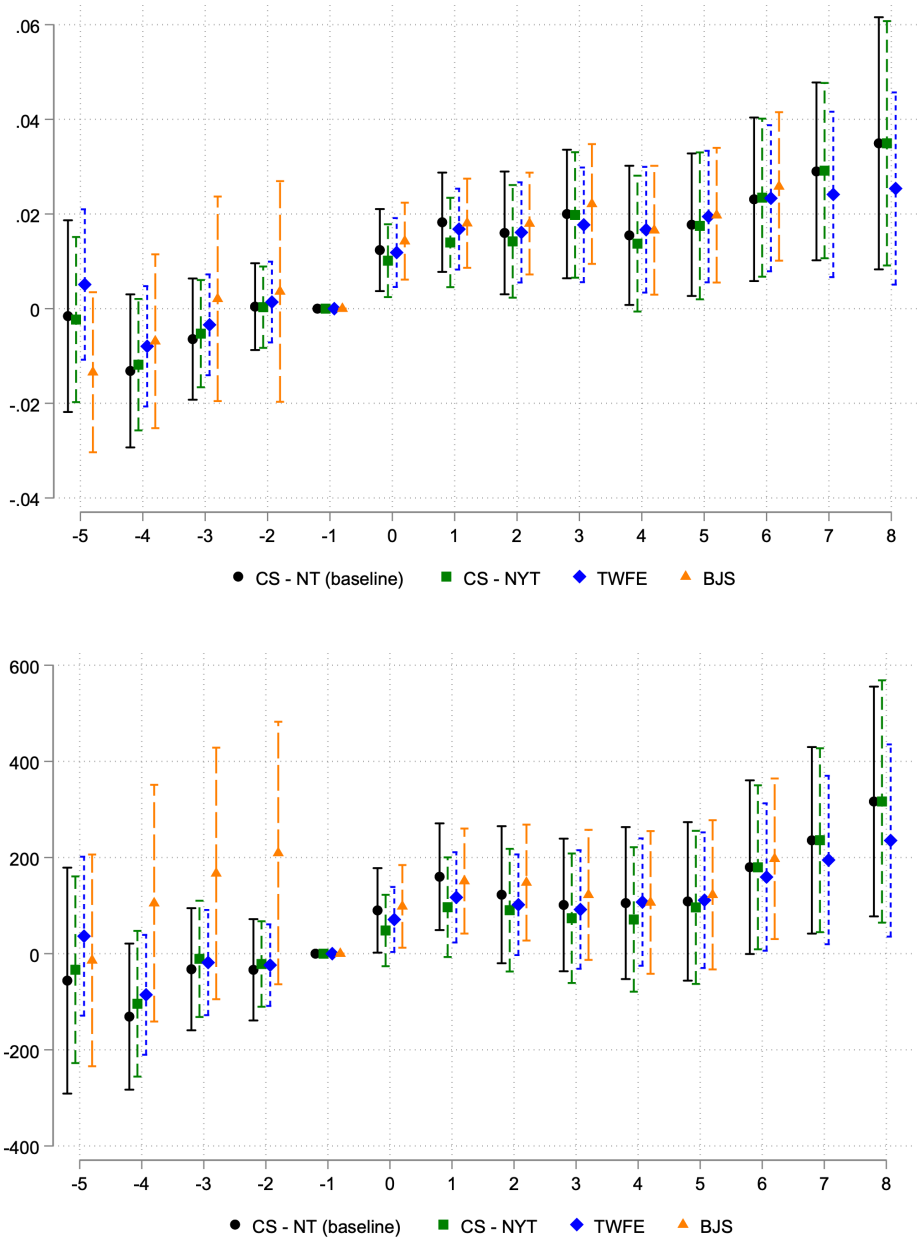


Earnings



Notes: This figure shows the estimated effect of an expansion on fathers' employment and earnings. The sample includes fathers of children from 0 to 3 years of age. The statistics at the bottom show the average value of the estimates pre-treatement (1) and post-treatment (2), the p-value for the test of the hypothesis that all pre-treatment estimates are equal to zero (3), and the p-value for the test of equality of averages pre- and post-treatment (4). Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

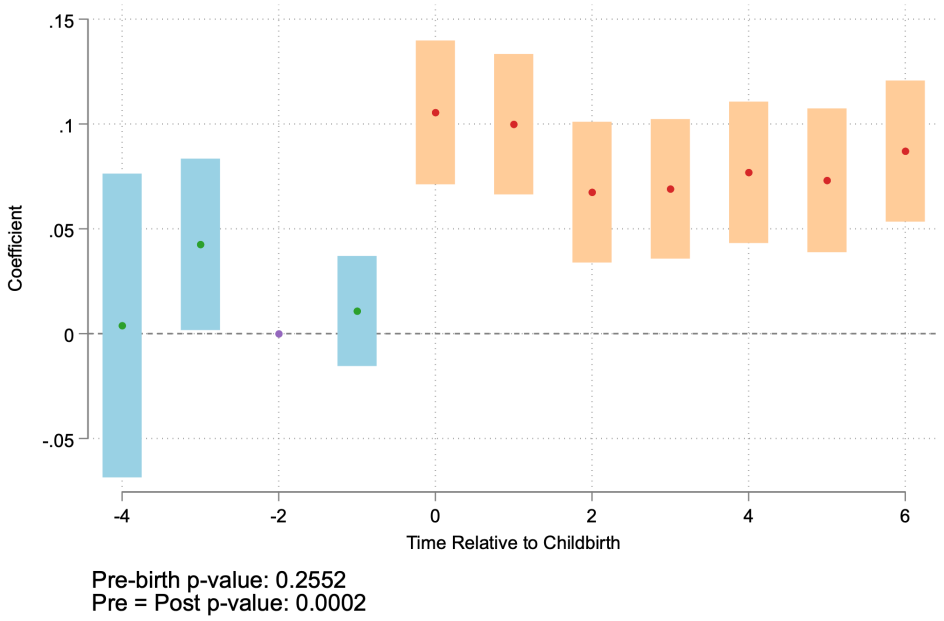
Figure 9: Robustness - Effect of Expansion on Mothers' Employment



Notes: This figure shows robustness of the main effects to alternative estimators. The estimators presented are (1) Callaway and Sant'Anna (2021) with "never-treated" controls, (2) Callaway and Sant'Anna (2021) with "not-yet-treated" controls, (3) two-way fixed effects, and (4) Borusyak et al. (2021). Earnings in 2010 BRL.

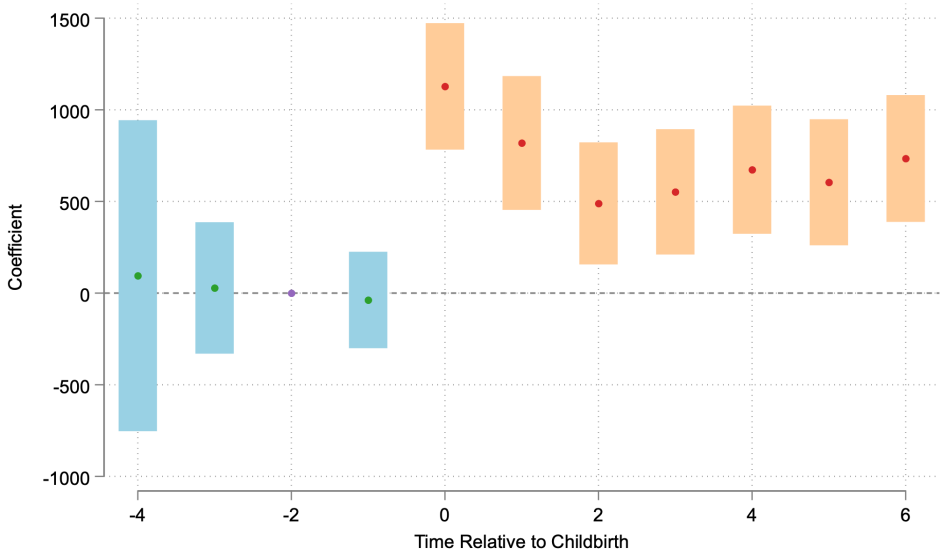
Figure 10: Effects of Childcare Availability on Mothers by Time from Childbirth

Employment



Pre-birth p-value: 0.2552
Pre = Post p-value: 0.0002

Earnings

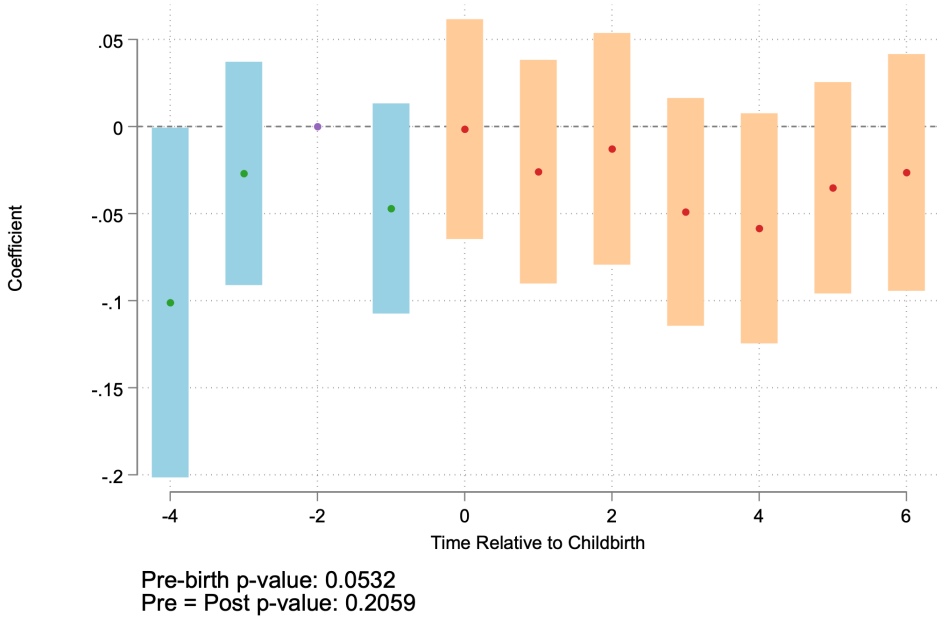


Pre-birth p-value: 0.8800
Pre = Post p-value: 0.0005

Notes: This figure shows the estimated effect of one additional seat per child on mothers' employment and earnings, by time relative to childbirth. Earnings in 2010 BRL.

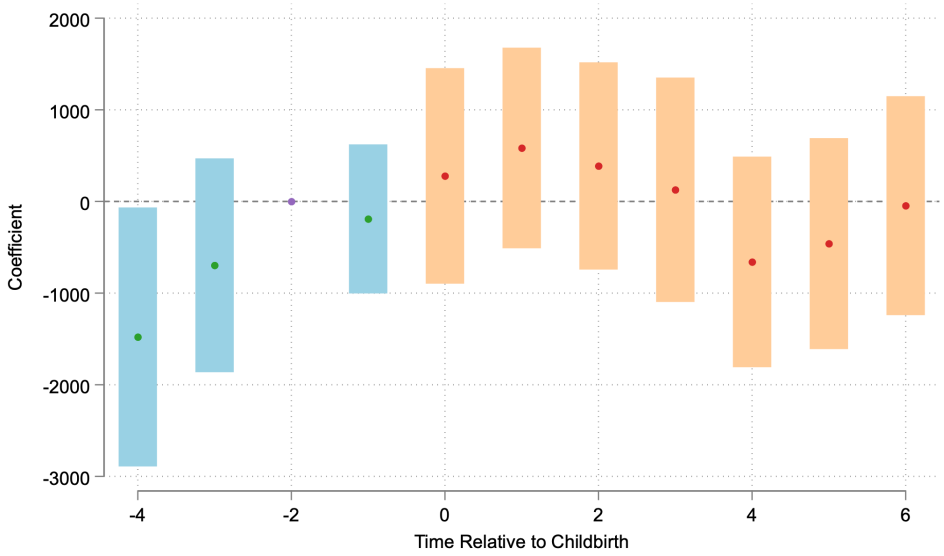
Figure 11: Effects of Childcare Availability on Fathers by Time from Childbirth

Employment



Pre-birth p-value: 0.0532
Pre = Post p-value: 0.2059

Earnings



Pre-birth p-value: 0.0706
Pre = Post p-value: 0.0618

Notes: This figure shows the estimated effect of one additional seat per child on fathers' employment and earnings, by time relative to childbirth. Earnings in 2010 BRL.

Table 1: Summary Statistics

	Mothers		Fathers	
	Before	After	Before	After
Share formally employed	0.44	0.33	0.55	0.54
	(0.11)	(0.09)	(0.16)	(0.15)
Total earnings (Yearly)	3,465	2,217	5,622	5,883
	(1,162)	(852)	(2,464)	(2,553)
Earnings if employed (Yearly)	7,749	6,607	10,235	10,912
	(1,567)	(1,656)	(3,127)	(3,527)
Work hours if employed (Weekly)	29.04	24.36	31.62	32.25
	(3.44)	(3.63)	(7.04)	(5.58)
Wage if employed (Hourly)	4.95	5.07	6.00	6.33
	(0.78)	(1.44)	(1.49)	(1.99)
N	306,841	401,033	82,399	101,327

Notes: This table shows summary statistics for the main sample. The included periods are 2013 to 2018, and the included districts are the ones that had either a large increase in childcare availability (above third quintile), or no year with an increase above the second quintile. Observations are year by districts, weighted by the total mothers/fathers in each district. Each column corresponds to, respectively, mothers-to-be 1 year before childbirth, mothers 1 year after childbirth, fathers-to-be 1 year before childbirth and fathers 1 year after childbirth. All monetary values are 2010 BRL.

Table 2: Comparison between Census and Single Registry

Variable	Census	Single Registry
Share Completed High School	0.85	0.61
Share Born in São Paulo	0.66	0.53
Share White	0.62	0.40
Share Employed - Formal Sector	0.39	0.35
Share Employed - Informal Sector	0.28	?
Average Yearly Income - Formal Sector	24,878	6,326
Median Yearly Income - Formal Sector	14,400	6,169
N	88,452	45,875

Notes: This table shows summary statistics for mothers in the 2010 Census (left) and our main sample when restricted to 2010 (right). The Single Registry does not include information on informal employment. Income figures are conditional on being employed in the formal sector. All monetary values are 2010 BRL.

Table 3: Effects of Childcare Expansion

	Availability	Employment	Earnings
	First Stage		
Post Expansion	0.328***		
	0.042		
	Mothers		
Post Expansion		0.021***	154.7**
		(0.007)	(66.0)
Re-scaled Effect		0.064	471.6
	Mothers-to-be		
Post Expansion		0.007	87.7
		(0.007)	(79.4)
Re-scaled Effect		0.021	267.4
	Fathers		
Post Expansion		−0.009	135.1
		(0.013)	(196.3)
Re-scaled Effect		−0.027	411.9

Notes: This table shows the average estimated effects over periods 0 through 8 after an expansion, for mothers, mothers-to-be and fathers. The mother and father samples include parents from 0 to 3 years after childbirth. The mothers-to-be sample includes 4 to 1 year before childbirth. The third line in each panel but the first shows the coefficients re-scaled by the first stage effect of 0.328. Earnings are in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

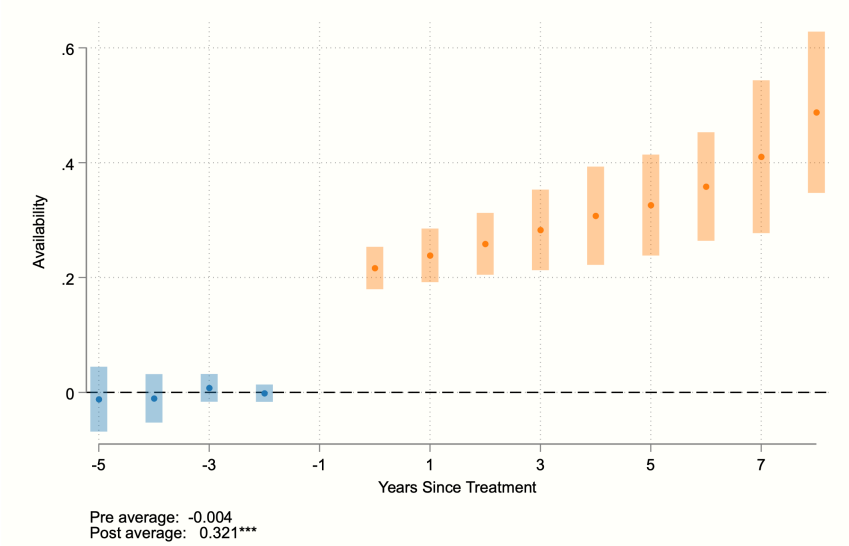
Table 4: Effects of Childcare Expansion - Heterogeneity

	Employment		Earnings	
	Pre	Post	Pre	Post
Migration				
Immigrants	−0.007	0.014**	−110.9	99.0
	(0.009)	(0.007)	(85.1)	(77.8)
Natives	0.001	0.019**	−44.3	147.1
	(0.009)	(0.009)	(126.8)	(114.0)
Education				
Low	−0.003	0.022***	0.4	210.7**
	(0.010)	(0.008)	(97.9)	(97.6)
High	−0.002	0.016*	−75.7	57.4
	(0.009)	(0.009)	(100.4)	(99.1)
Share of Pentecostals				
Low	0.004	0.017**	49.2	204.1*
	(0.010)	(0.008)	(90.1)	(108.5)
High	−0.011	0.024***	−132.3	142.7*
	(0.008)	(0.008)	(81.1)	(78.7)
Share of Female Household Heads				
Low	−0.016*	0.006	−139.6*	40.7
	(0.009)	(0.008)	(83.1)	(82.6)
High	0.009	0.038***	−4.5	266.9***
	(0.009)	(0.009)	(87.1)	(93.3)

Notes: This table shows the average estimated effects for the Pre- and Post-expansion periods, according to mothers migration status and educational attainment. Natives are defined as people who were born in São Paulo, while migrants are people who were born anywhere else. Low-education mothers are those that completed high school or less. The sample includes mothers of children from 0 to 3 years of age. Earnings are in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A Appendix

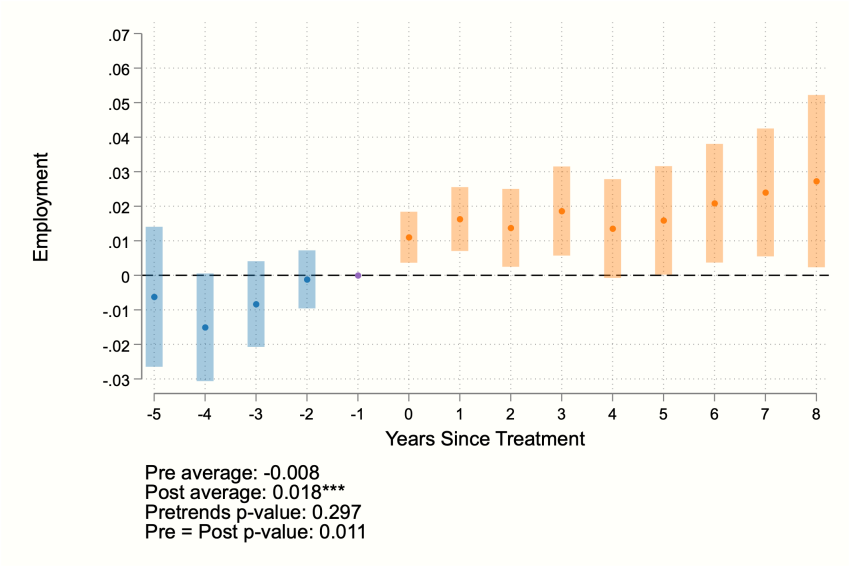
Figure 12: Robustness: Effect of Expansion on Childcare Availability



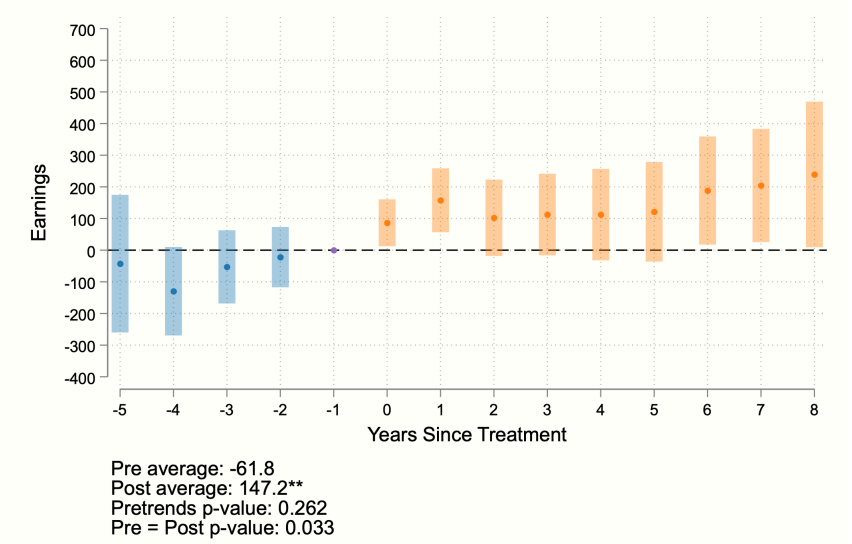
Notes: This figure shows the estimated effect of an expansion on mother's employment and earnings, using an alternative definition of the treatment for the "between-districts" strategy. A district is considered treated if its largest annual growth in childcare availability is in the top half among districts in the sample. Controls are the bottom half. The bars represent uniform confidence intervals. The statistics in the bottom show 1) the average value of the pre-treatement estimates, 2) the average value of post-treatment estimates * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 13: Robustness: Effect of Expansion on Mothers

Employment

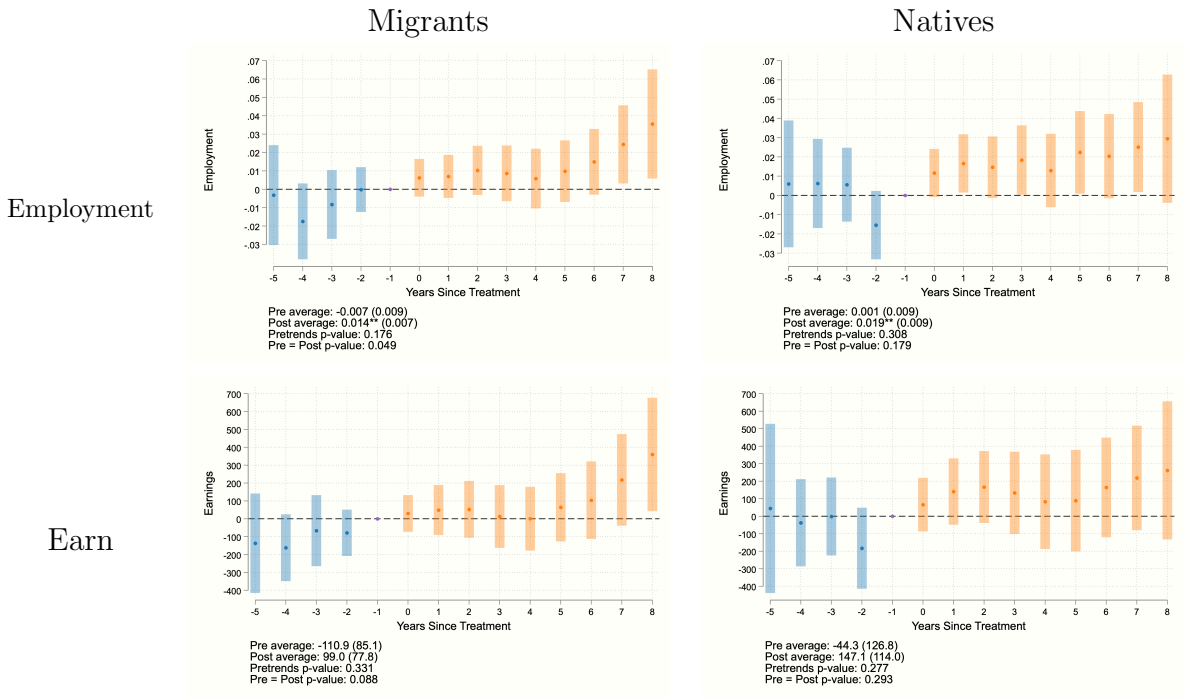


Earnings



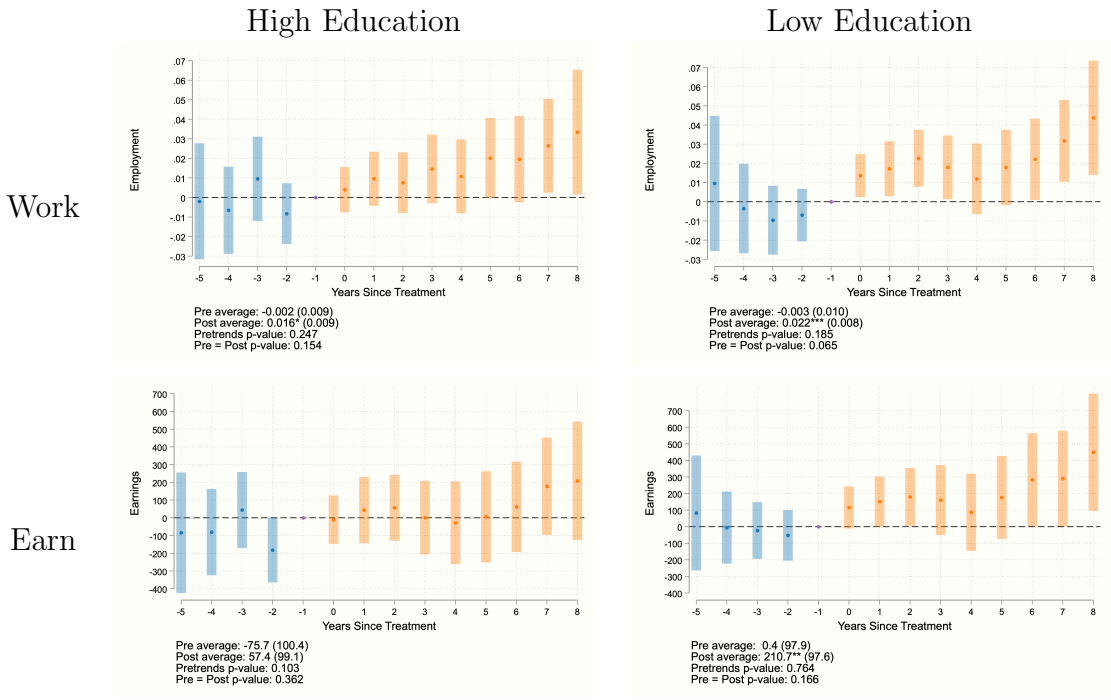
Notes: This figure shows the estimated effect of an expansion on mother's employment and earnings, using an alternative definition of the treatment for the "between-districts" strategy. A district is considered treated if its largest annual growth in childcare availability is in the top half among districts in the sample. Controls are the bottom half. The sample includes mothers of children from 0 to 3 years of age. The bars represent uniform confidence intervals. The statistics in the bottom show 1) the average value of the pre-treatement estimates, 2) the average value of post-treatment estimates, 3) the p-value for the test of the null hypothesis that all pre-treatment estimates are equal to zero, 4) the p-value for the test of equality of averages pre- and post-treatment. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 14: Heterogeneity: Migration



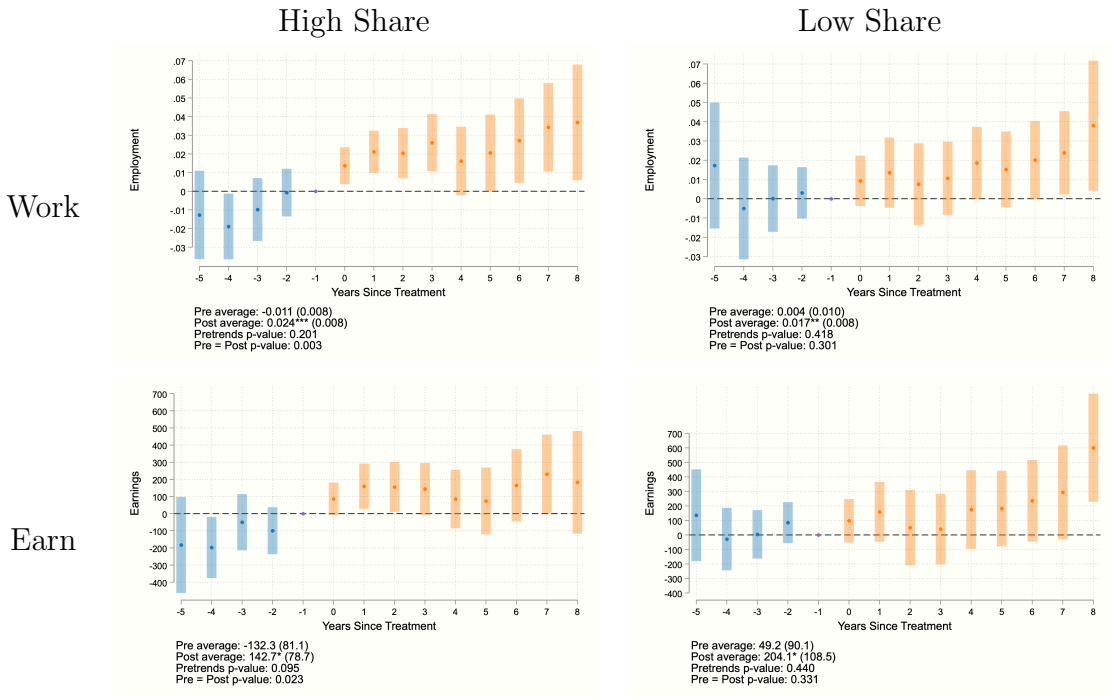
Notes: This figure shows the estimated effect of an expansion on mother's employment (top) and earnings (bottom), for mothers who are migrants (left) and natives (right). Natives are defined as people who were born in São Paulo, while migrants are people who were born anywhere else. The sample includes mothers of children from 0 to 3 years of age. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 15: Heterogeneity: Education



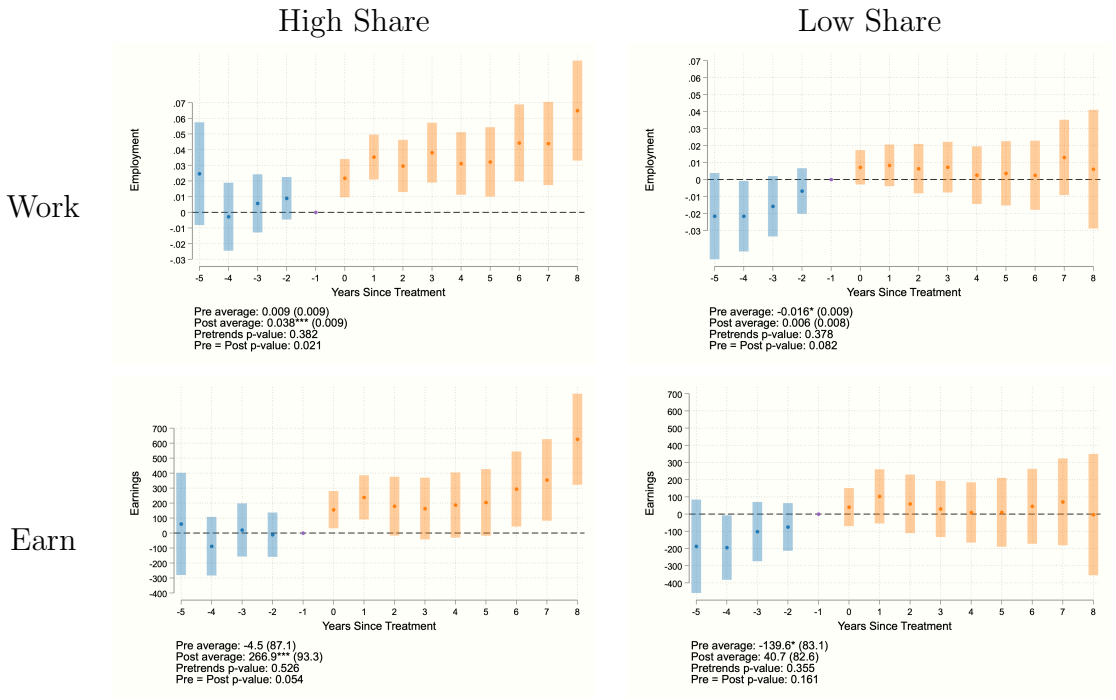
Notes: This figure shows the estimated effect of an expansion on mother's employment (top) and earnings (bottom), for mothers with low education (right) and high education (left). Low-education mothers are those that completed high school or less. The sample includes mothers of children from 0 to 3 years of age. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 16: Heterogeneity: Pentecostalism



Notes: This figure shows the estimated effect of an expansion on mother's employment (top) and earnings (bottom), for mothers living in districts with share of Neopentecostals above the median (left) and below the median (right). The sample includes mothers of children from 0 to 3 years of age. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 17: Heterogeneity: Female Heads-of-household



Notes: This figure shows the estimated effect of an expansion on mother's employment (top) and earnings (bottom), for mothers living in districts with a share of female heads-of-household above the median (left) vs. below the median (right). The sample includes mothers of children from 0 to 3 years of age. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$