

Free Childcare and the Motherhood Penalty: Evidence from São Paulo

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Abstract

Latin America consistently has some of the largest child penalties for female work globally, and while subsidized childcare is often advanced as a remedy, the literature on its effectiveness is scarce in this context. This paper estimates the impact of a rapid expansion of public childcare on mothers' careers in the city of São Paulo. We leverage precise location and timing of the rollout and expansion of childcare facilities, coupled with detailed data on the labor market and household characteristics to identify effects on mothers' labor force 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 mothers-to-be or fathers.

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1 Introduction

Subsidized childcare is often proposed as a way to address gender inequality in the labor market . A large part of the earnings gender gap is explained by the dip in female labor-force participation after childbirth – the child penalty (Bertrand, Goldin, and Katz 2010; Angelov, Johansson, and Lindahl 2016; Kleven, Landais, and Søgaard 2019). This suggests cheap childcare provision may have a crucial effect on maternal labor supply and gender balance in the workplace. While some studies of childcare provision have shown positive effects, many others have found null results (Havnes and Mogstad 2011; Olivetti and Petrongolo 2017; Kleven, Landais, Posch, et al. 2020). Latin America has some of the largest child penalties in the world, but there is very little evidence of whether childcare, or any other policies, could help alleviate it (Kleven, Landais, and Leite-Mariante 2023).

In this paper, we study the impact of a large expansion of free public childcare in Sao Paulo, Brazil, on the child penalty. This expansion took place from 2008 to 2018 and increased the number of seats per child from roughly 25% to close to 75%. We estimate the effects of this program on women’s labor force participation and earnings using a difference-in-difference design that leverages the expansion roll-out, accounting for heterogeneous effects. To do so, 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 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 see no evidence against the parallel trends assumption. We find null effects for mothers-to-be, a demographically similar group that should not be affected by the policy. We also find no effects on fathers using the same strategy.

Prior to 2008, Sao 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, it was difficult for a mother to get a childcare seat before the child was old enough to attend regular pre-school. In 2008, Mayor Gilberto Kassab was elected, declaring that bringing wait times to zero would be a key priority in his administration.¹ The new administration started a fast expansion in public childcare; between 2008 and 2018, São Paulo created an average of 30,000 new childcare seats every year. This rapid expansion was achieved mainly through partnerships with private-sector childcare providers. The new model increased flexibility in location and hiring regulations, which allowed the rapid opening of new facilities, sometimes in a matter of a few months.

To measure mothers labor outcomes and access to childcare and their labor outcomes, we combine three administrative datasets. First, the São Paulo Department of Education provides data with the opening date of each childcare facility, the number of seats and the location. Second, in order to study labor market outcomes, we use data from 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 with the universe of families receiving any government benefit. The Single Registry allows us to observe families' characteristics, including their address and date of birth of each household member.

Our main empirical strategy exploits the timing of roll-out of large expansions in childcare in São Paulo across districts, in 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. 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

¹Kassab was elected vice-mayor in 2004 and took the mayoral position from José Serra in 2006. He was directly elected in 2008 and served until 2013. The main changes his administration made to childcare policy were carried on in the following administrations.

seats. The key comparison is between districts that had a large expansion (defined as among the top 40%) to those with only a small expansion (in the bottom 40%). We show results are robust to alternative definitions of treatment. To address the concerns raised by the recent literature on staggered adoption with heterogeneous treatment², 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, Landais, Posch, et al. 2020), that does not necessitate the identification of discrete expansion periods and uses all variation in childcare availability. This alternative strategy focuses on the comparison between labor force participation of mothers and mothers-to-be, in the same district, as childcare availability increases.

Our results show that free childcare leads to a significant and persistent reduction on 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 the mothers' labor force participation after the birth of the first child. These figures imply each additional seat per child is associated with a 6.4 p.p (20%) increase in mothers' employment. Similarly, total annual earnings are increased 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 do not find any effect for fathers. We find suggestive evidence that the effects are stronger for mothers with lower education and 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 the roll-out of public childcare effects on mothers' labor outcomes, finding mixed results (Baker, Gruber, and Milligan 2008; Berlinski and Galiani 2007; Bauernschuster and Schlotter

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

2015; Bettendorf, Jongen, and Muller 2015; Havnes and Mogstad 2011; Cascio 2009; Andresen and Havnes 2019; Rabaté and Rellstab 2022).³ A more recent wave of studies uses richer sources of variation, typically combining the timing of childcare expansion with either timing of birth or eligibility criteria to generate alternative comparison groups (Carta and Rizzica 2018; Kleven, Landais, Posch, et al. 2020; Brewer, Cattan, Crawford, and Rabe 2022). 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 in this second strand of literature, to which we add a focus on a large city in a developing country, a context we argue is particularly suitable for this policy.

This paper is closely related to Attanasio et al. (2022), which analyzes a similar context to ours, in the city of Rio de Janeiro, through a randomized trial. Our paper evaluates a non-randomized evaluation of a naturally occurring childcare expansion, while they provide RCT evidence in a context where availability was considerably lower and remained roughly constant over time. They find positive effects for grandparents and older siblings and no effects for parents, possibly highlighting contextual differences to our setting.

This paper is organized as follows. In Section 2, we present the background and describe the childcare program in Sao 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.

³For a review of the literature, see Cascio, Haider, and Nielsen (2015) and Albanesi, Olivetti, and Petrongolo (2023)

⁴Müller and Wrohlich (2020), Carta and Rizzica (2018) and Brewer, Cattan, Crawford, and Rabe (2022) find significant positive effects, while Kleven, Landais, Posch, et al. (2020) finds

2 Setting

To assess the impact of free childcare on the child penalty, we study the expansion of public childcare in Sao 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 the expansion possible, without large decreases in quality, was the intense use of public-private partnerships with educational NGOs.

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 15 – 59 year-old women working rose from 52% to 61% (Barbosa [2014](#)), reaching rates similar to that of developed countries. Although Brazilian women often rely on informal family ties for childcare, in large cities many households are separated from extended families due to the fast urbanization and migration. Sao Paulo, in particular, is a dynamic labor market, attracting economic migrants from other areas, and where a high proportion of mothers' work outside the home (73.3% in 2019). These factors, combined with low stigma against women's work outside the home (Chioda and Verdú [2016](#)), result in high demand for public childcare.

In response to the large 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 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 Sao 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 private sector childcare providers. Figure 1 shows that the number of seats provided under the partnership model more than tripled in the period. Meanwhile, provision increased only very slightly in the direct municipal provision. The number of exclusively private providers has also remained flat over the period.

Under the partnership model, the city government contracts with specialized non-profits 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 under direct public provision and are relatively high. The city stipulates a maximum ratio of children for each provider, depending on age. Public childcare facilities work five days a week, covering 10 hours between 7am and 7pm, except for a reduced schedule during school breaks. Besides daycare activities, like physical play and reading, the facilities also provide free regular meals, helping prevent malnutrition among the poorest children.

The rules for 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 same educational district where they live. If there is no availability, they may be matched to a neighboring district with excess capacity, or enter a wait-list. Poorer families receive priority in the wait-list.

This centralized system also allowed the municipality to identify places with an excess demand for public childcare, and 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 peripheries. Mothers in these areas are much more likely to be unable to afford childcare services and to depend on wage income, besides being farther from most jobs in the city, and therefore place a high value on public childcare.

3 Data

To study the effect of childcare on mothers' formal employment we match childcare information from the Sao Paulo Department of Education to employment records in RAIS. The match is possible at the district level, using detailed information on family addresses from Single Registry as an intermediate match. We also observe a family roster and several demographic and social characteristics in the Single Registry.

To study the effect of childcare on mothers' formal employment, we identify all mothers in the Single Registry and link them to RAIS using a personal identifier. Then we use their address coming from the Single Registry to match them to childcare information from the Sao Paulo Department of Education.

3.1 Childcare Centers

Our data on childcare provision comes directly from the Sao Paulo city government, available at the city's open data portal.⁵ There we observe the contracts and opening dates of all childcare facilities, as well as location and number of seats available. The availability of childcare increased by about 300,000 seats between 2008 and 2018 throughout the city, particularly in the peripheries, corresponding to an increase of about 50 p.p in seats per child.

⁵<http://dados.prefeitura.sp.gov.br/>

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 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. Currently, more than 20 social programs use it, covering virtually all of Brazil's poor (Campello, Neri, et al. 2013). 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 Single Registry, families must have a valid registration (complete and up-to-date), updated at least every two years. They must undergo interviews with local government agents, including a standardized questionnaire on their earnings, living conditions, demographic and occupational characteristics, and personal tax id (CPF). They have to inform authorities of relevant changes to family size or income.

We use a December 2017 extraction from 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 and whose family contains at least one child of 13 years of age or less. Out of the initial 3 million individuals, 549,763 are classified as mothers.

3.3 RAIS

The Annual Account of Social Information (Relação Anual de Informações Sociais, RAIS) 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, notably 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 Single Registry to this panel of workers through their tax id. 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 see her any year, then we know she has never worked in the formal sector. Our measure of employment is a dummy indicating if 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.

Figure 3 shows the share of women working in the formal sector relative to the birth of their first child. At the year of the child's birth, there is a drop of about 13 p.p. on the share of women employed in the labor market (from 55% to 42%), increasing to 45% three years after birth, and it remains constant the following years.

3.4 Descriptive statistics

Table 1 shows descriptive statistics for mothers-to-be (panel 1), mothers (panel 2), fathers-to-be (panel 3), fathers (panel 4) and average district characteristics (panel 5).

The data show a large child penalty for women, accompanied by a small reduction in hours. Comparing mothers 1 year after childbirth to 1 year before, the dip in labor force participation is 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 conditional on working. There is no reduction in hourly wages conditional on work.

Men in the sample work and earn considerably more than women. Fathers-to-be have a higher participation in the formal sector (57%) 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 labor market participation and wages are higher than those of women. As a result, women earn 54% as much as men before childbirth and 34% after childbirth. Our sample includes substantially fewer fathers than mothers, due to a large fraction of single mothers, leading to lower precision in those estimates.

4 Empirical Strategy

We analyze the effect of childcare provision on mothers' labor market outcome using two complementary strategies. First, we follow Callaway and Sant'Anna (2021) in estimating treatment effects in a dynamic differences-in-differences context. To do so, we define a "time of treatment" for each district based on large expansions 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 a fixed-effects framework that takes advantage of all the variation in the data.

4.1 Differences in Differences

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 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 with districts where there was only small or no expansion 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 work. 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 seats available the sample. If this largest growth was small relative to other districts (bottom 40%), we consider it never treated, while those with large increases (above top 40%) are treated.

For the districts where the first facility was opened in a given year, we attribute that year as the year of the expansion and consider them treated. All other districts are dropped from the sample, i.e. those between percentiles 40 and 60.

Recent evidence suggests that “staggered access” estimations might be biased by heterogeneous effects over time (Callaway and Sant’Anna 2021; Chaisemartin and D’Haultfoeuille 2019). To address this concern, we estimate the parameter of interest following Callaway and Sant’Anna (2021). Formally, 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 in which period each district expanded childcare. Let e denote event-time, i.e., $e = t - g$ denotes the time elapsed since treatment was adopted. Our parameter of interest is given by

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

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 was 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, a somewhat lumpy growth concentrated in a few places each year. Figure 4 shows the evolution on childcare availability over time for the control group, and for groups treated each year. It is clear that each group shows a marked increase in the period we designate as the treatment period, substantially larger than any other in most cases. 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 for the later years. To deal with 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 re-scale 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 in 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 location in the Single Registry in the year 2012 and keep it constant over time. This choice mitigates concerns that any effects through endogenous migration, at the cost of potentially adding error to families' location, and thus biasing the effects towards zero. However, because the expansion started before 2012, there is still the possibility 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 Within-District Strategy

Our main strategy has some important limitations. One issue is that we do not make full use of available data, in two main ways. First, we drop districts that had a median

increase in childcare availability, and thus are not 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 definition of an expansion is somewhat arbitrary. Finally, 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 is coming from 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) in the same district. Intuitively, we look at how the child penalty evolves as childcare availability increases in a given district. Under the hypothesis that mothers-to-be labor market outcomes 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. We do not rely on identifying particular periods as expansions and treat all changes in childcare availability equally.

To fix ideas, let us consider a single district, where childcare availability increased over time. Supposing we observe employment for mothers and mothers-to-be. In this context we can identify the effect of childcare availability using two-way-fixed-effects strategy with a continuous treatment, with periods before and after, mothers as the treated group, and mothers-to-be as the comparison group. Let us denote mothers by $m = 1$ and mothers-to-be as $m = 0$, and $t = 0$ as before the expansion and $t = 1$ as after. 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 mothers/mothers-to-be in period t , $Availability_t$ is the ratio between childcare seats and children zero- to three-years-old at time t . 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 dif-in-dif assumptions. Importantly, the parallel trends assumption is different from the one required by our main estimate. In this case, we require that the evolution of the potential outcomes of mothers and mothers-to-be has to follow the same trends over time. The SUTVA 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 mothers or 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, let:

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

Then, the estimating equation is:

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

In this regression, any stable, pre-existing 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 lack of 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 zero 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 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 childcare facilities opening on the number of seats per child over time. We observe a very small but statistically significant difference in the years before the expansion, indicating a small degree of pre-trends in childcare availability. However, 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 0.26 seats per child, that increases gradually to about 0.75 after 10 years. Averaging across years 0 to 10 after treatment, the effect is 0.45 seats per child, statistically significant at 1%.

Figure 6 shows the effects of childcare expansion on formal employment and earnings for mothers of children from 0 to 3 years relative to the time of expansion of childcare.

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. 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, there is a statistically significant increase in the share of working mothers of close to 1 p.p., that increases over time to about 3 p.p. in 10 years, with an average effect of 2.1% in the post period. Re-scaling this effect by the effect on seats implies that each additional seat available increases maternal employment by 6.4 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 rescale the average post-treatment effects by the first stage coefficient, we obtain an effect of 472 BRL. Further rescaling by the effect on employment results in 7,370 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.⁶

5.2 Mothers-to-be and Fathers

We estimate the same model using the labor market outcomes among women who are not mothers, but will have a child in 1-5 years. If our results are driven by a correlation

⁶Appendix Figure 10 shows the results extending to the maximum time horizon we can estimate. In a longer time frame estimated effects do not appear to fade, although errors grow larger.

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 demographically very similar, other than being younger. Figure 7 shows the results. In contrast with the results for mothers, we see no increase in employment or earnings for mothers-to-be. We cannot reject equality of effects year-by-year due to fairly large errors.

Finally, we estimate the effect on fathers. There are theoretical reasons for fathers' labor market choices to be affected by changes in the overall labor supply in the household, and some effects have been observed in different contexts e.g. Krapf, Roth, and Slotwinski (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, although precision is very low in this case.

In order to obtain more precisely estimated effects, we collect and average the effects over time into a “Pre” and a “Post” effects. Table 2 presents the estimates. On average, over the first 10 years, the effect of an expansion is an increase of 20 p.p. in the probability that mothers’ work, corresponding to an extra 200 BRL per year. Re-scaling by the 0.41 effect on seats per child means that one additional seat corresponds to 0.49 mothers employed, earning an extra 487 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.

5.3 Heterogeneity

In this session we investigate potential mechanisms by splitting our sample of mothers by migration status and educational attainment. 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.

Table 3 Panel A and Appendix Figure 11 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 3 Panel B and Appendix Figure 12 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 and 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 homemakers⁷. Table 3 Panel C and Appendix Figure 13 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

⁷See Buccione and Mello (2020) for a discussion.

significant.

Finally, we explore heterogeneity by the share of households with female heads, as declared on the Census. Results are in Table 3 and Appendix Figure 14. 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.

5.4 Within-District Estimation

As an alternative strategy, we estimate the effect of childcare availability directly in a fixed-effects regression. Figure 9 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 two years before is normalized to zero.

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. The magnitude of the effects on employment is larger than what we find using differences-in-differences. This may be explained by the fact that this strategy takes any changes in childcare availability into account, not only changes during expansion years. Since some places do see substantial increases in subsequent years, those are also important for identification.

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 Sao Paulo, Brazil. We contribute to the growing literature on the effectiveness of free or subsidized childcare as a remedy for the motherhood penalty at work. 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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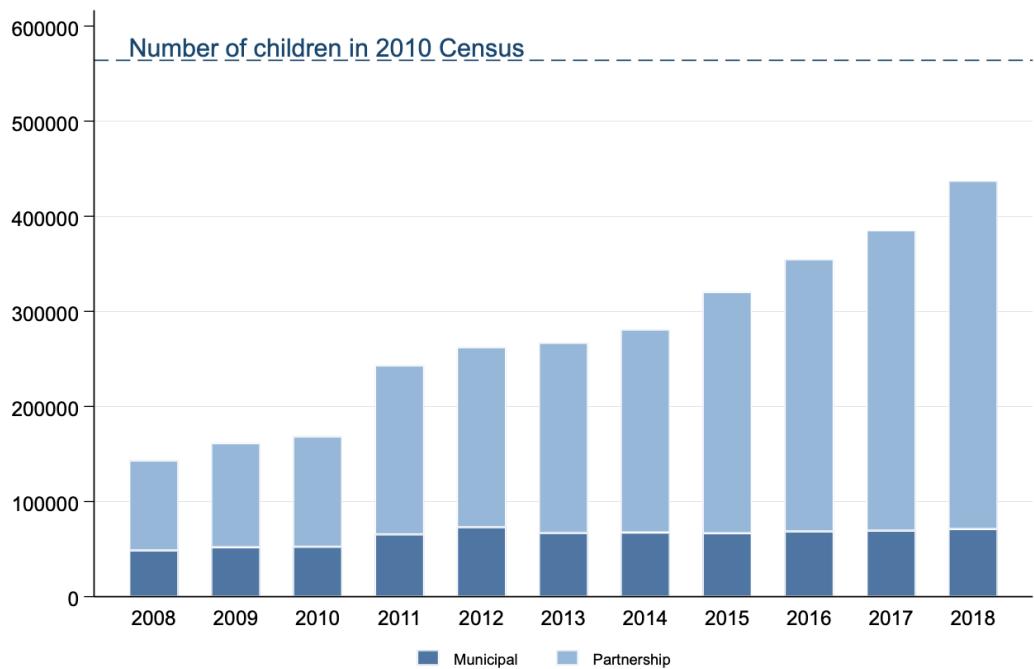
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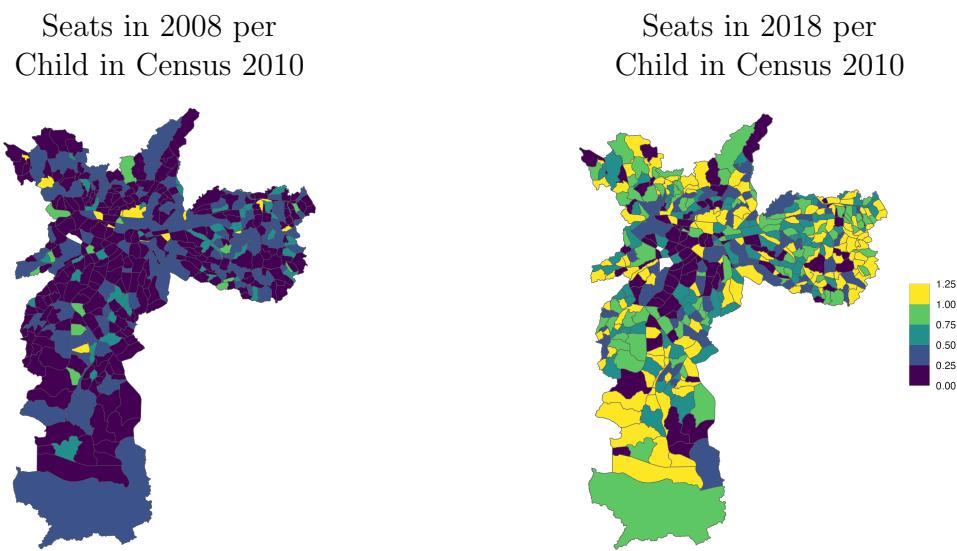
Figures and Tables

Figure 1: Children Attending Childcare by Type of Provider



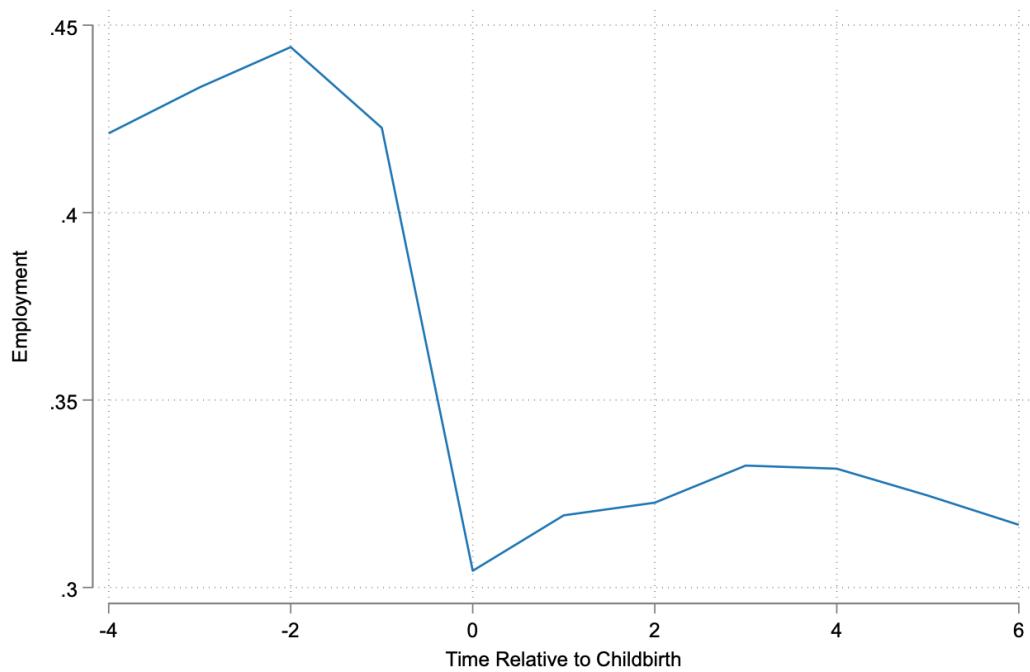
Notes: This figure shows total enrollment the childcare system in a) facilities funded by the municipal government and operated by non-profit partners, b) facilities funded and operated by the municipal government.

Figure 2: Childcare Enrollment per Educational District



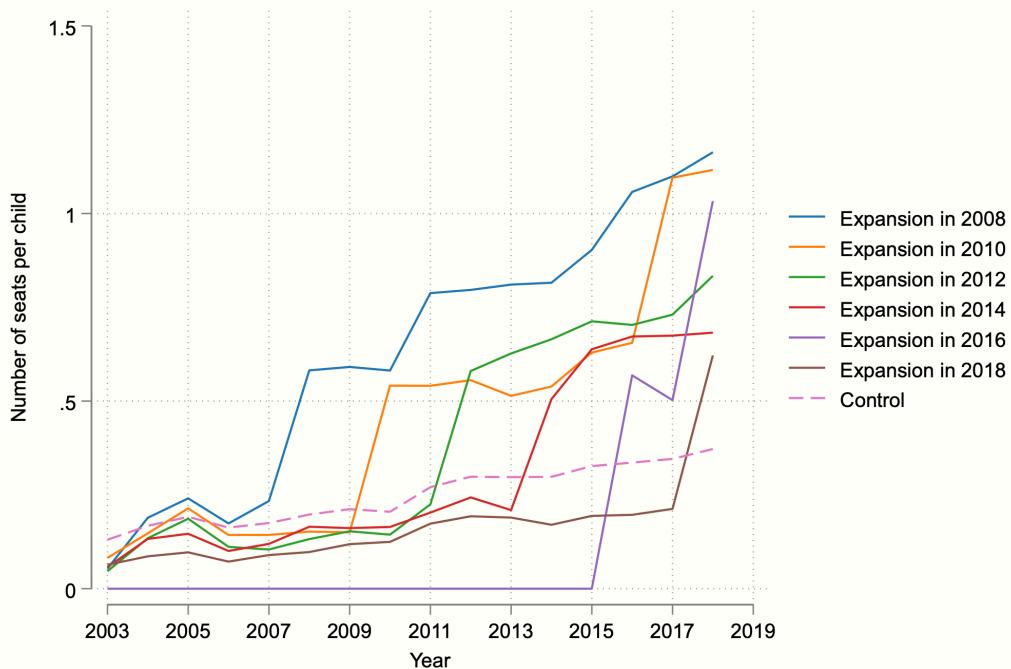
Notes: This figure shows childcare enrollment rates by educational districts in 2008 and 2018. The rate is defined as the ratio between the number of childcare seats in a given district divided by the population between 0 and 3 years of age residing in that district in the 2010 Census.

Figure 3: Child Penalty



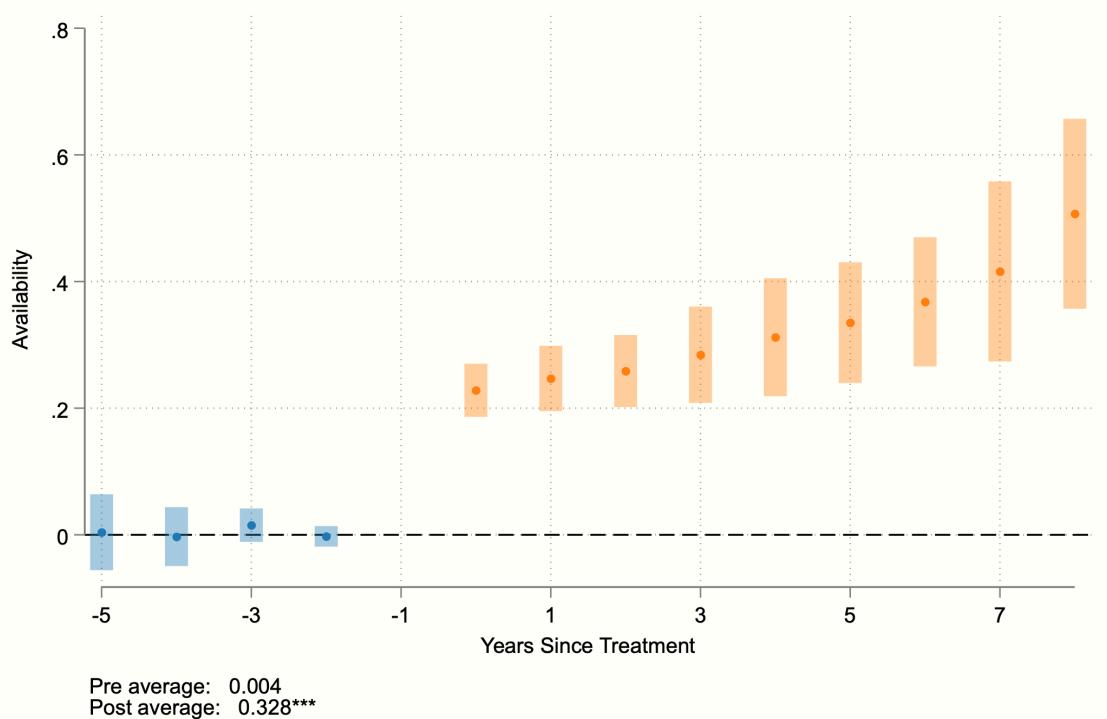
Notes: This figure shows the average employment rate in the formal sector for women in Sao Paulo around the year of first childbirth, denoted as 0. Data includes years 2007 to 2018.

Figure 4:
Childcare Availability by Treatment Year



Notes: This figure shows the evolution of the number of total seats per child in the 2010 Census. Data are grouped by the year of expansion, defined as the year of 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. Only even years are shown in the plot, to improve visualization.

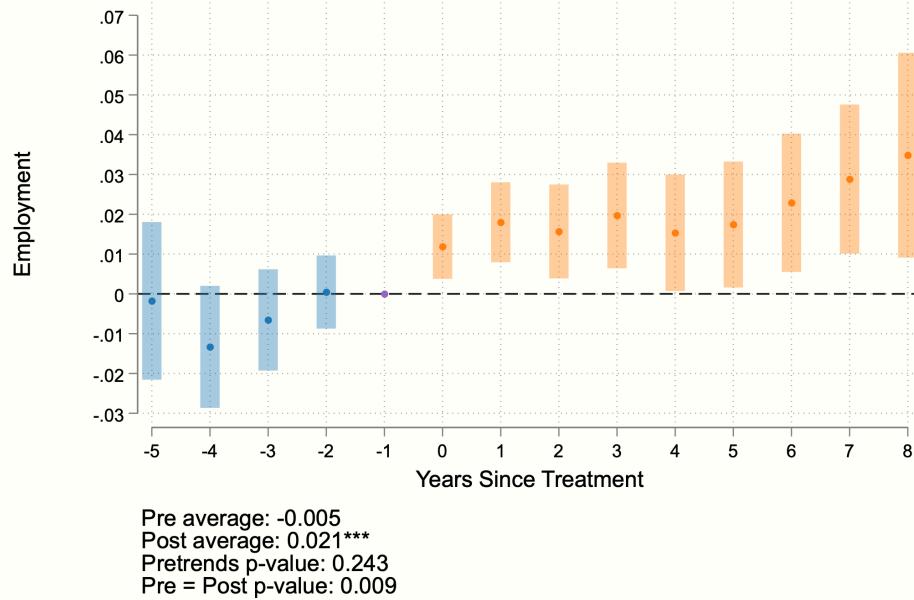
Figure 5: First stage: Childcare seats per child



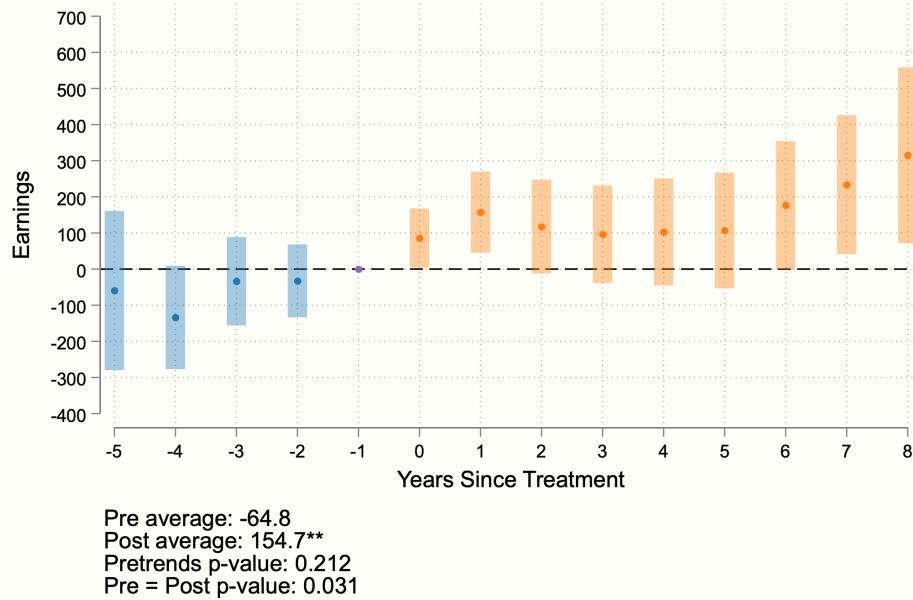
Notes: This figure shows the estimated effect of an expansion on childcare availability, defined as seats per child in the 2010 Census. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Effect of Expansion on Mothers

Employment



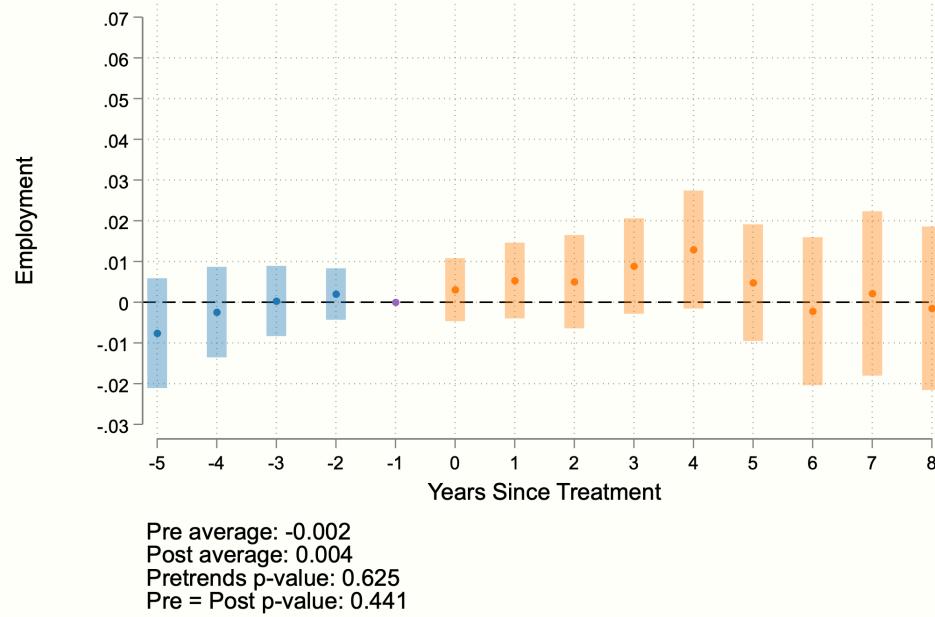
Earnings



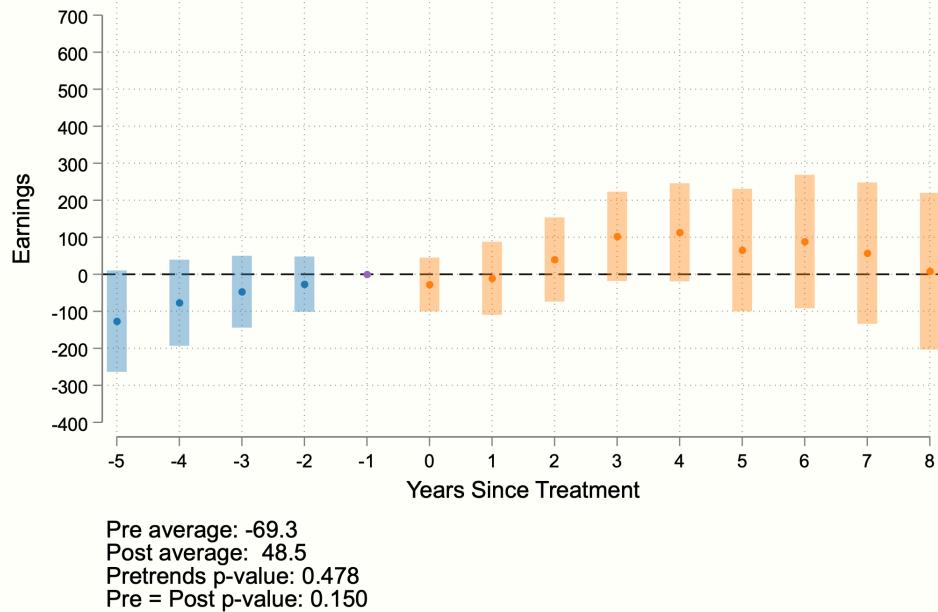
Notes: This figure shows the estimated effect of an expansion on mother's employment and earnings. The sample includes mothers of children from 0 to 3 years of age. The bars represent uniform confidence intervals. The statistics in the bottom show 1) the average value of the pre-treatment estimates, 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 7: Placebo - Effect of Expansion on Mothers-to-be

Employment



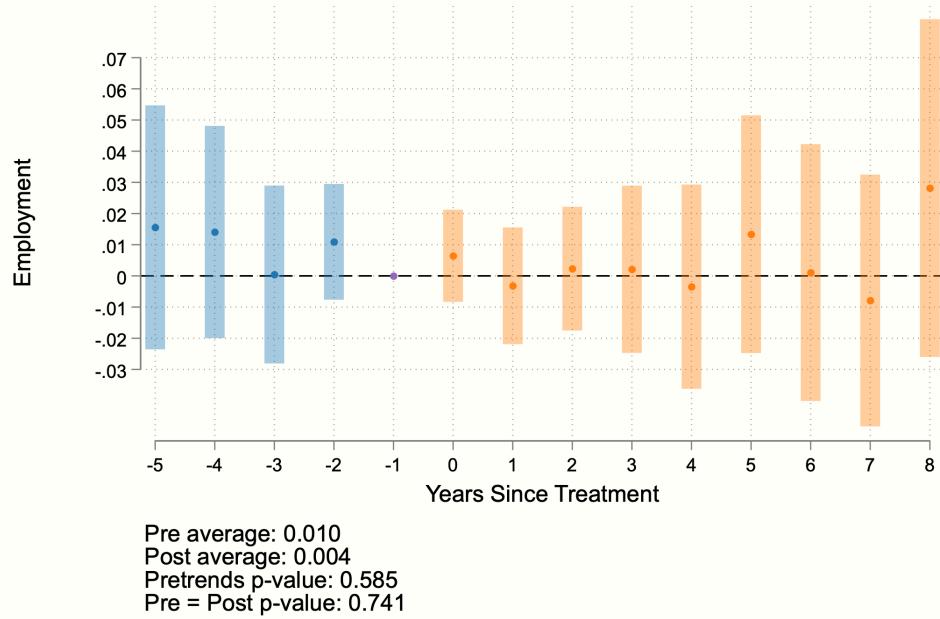
Earnings



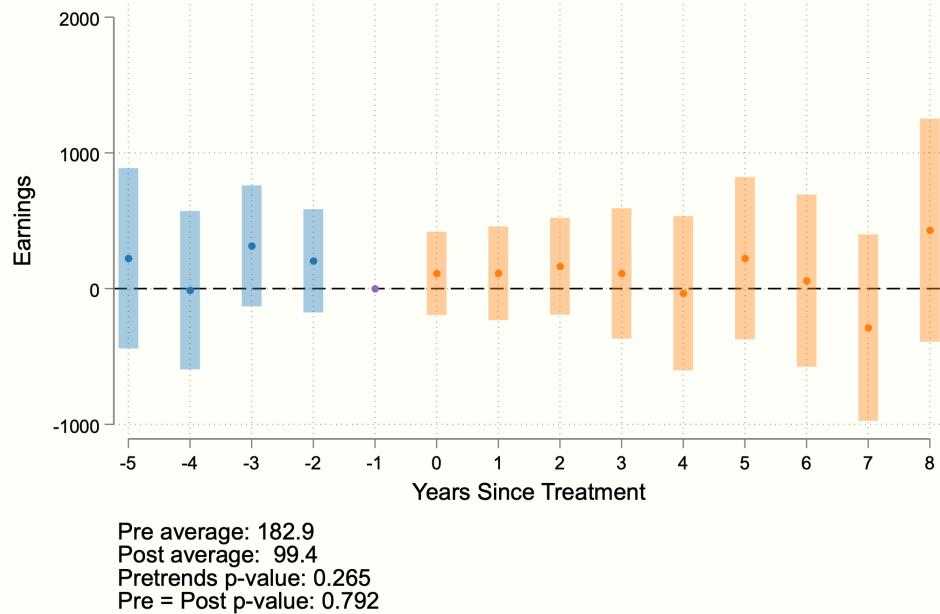
Notes: This figure shows the estimated effect of an expansion on mother's employment and earnings. The sample includes mothers-to-be 4 years before childbirth to 1 year before childbirth. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 8: Effect of Expansion on Fathers

Employment

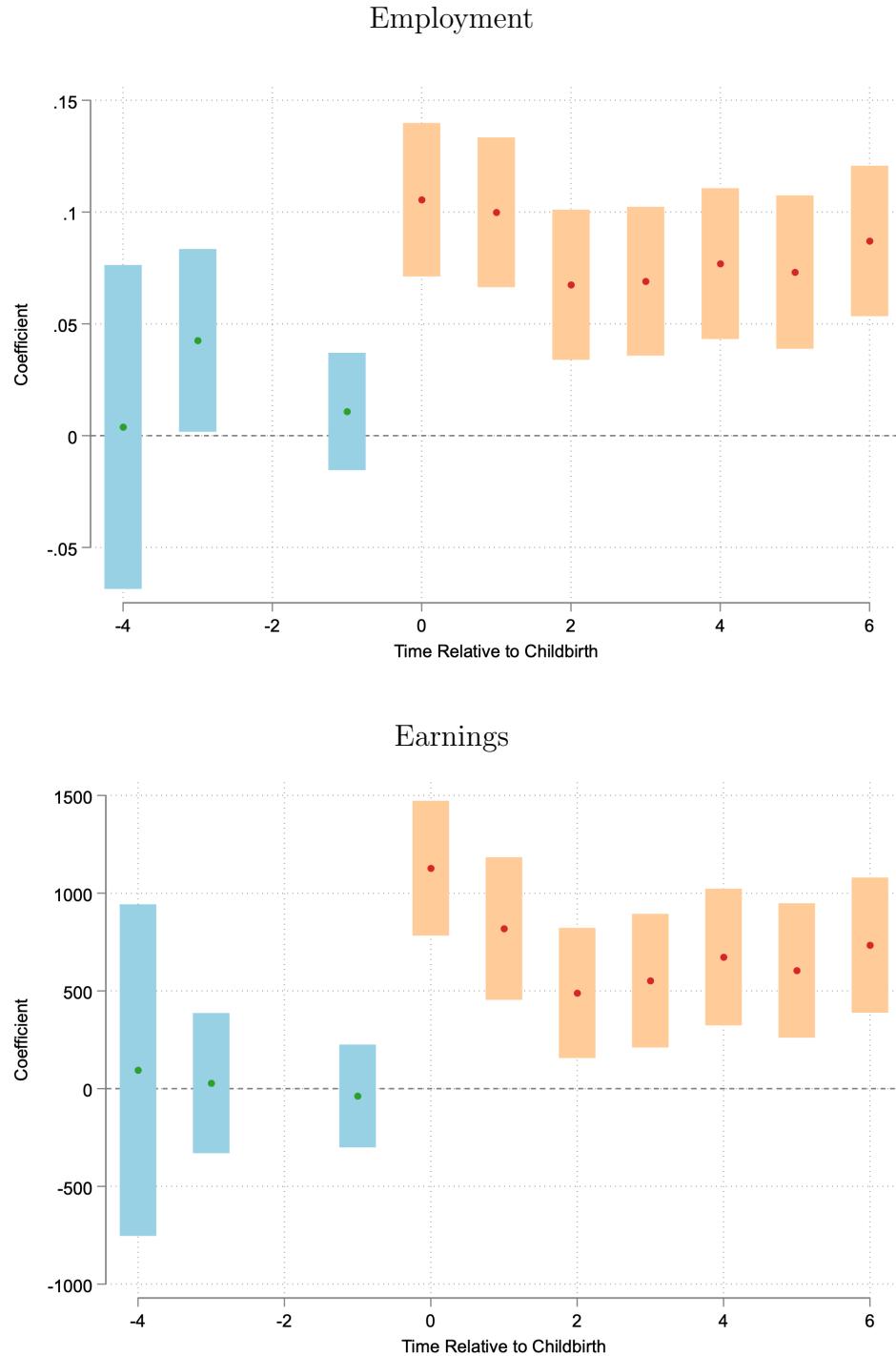


Earnings



Notes: This figure shows the estimated effect of an expansion on father's employment (left) and earnings (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 9: Effects of School Availability by Time from Childbirth



Notes: This figure shows the estimated effect of one additional seat per child on mothers' employment and earnings, by time relative to childbirth. The statistics in the bottom show the average value of the Pre- and Post-treatment estimates. Earnings in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1: Summary Statistics

Variable	Mean	SD	Obs	Weight
Sample: Mothers-to-be 1 year before childbirth				
Share formally employed	0.44	0.11	4,145	2,203,288
Total earnings (Yearly)	3465.11	1161.92	4,145	2,203,288
Earnings if employed (Yearly)	7749.01	1566.95	3,992	2,197,594
Work hours if employed (Weekly)	29.04	3.44	3,992	2,197,594
Wage if employed (Hourly)	4.95	0.78	3,992	2,197,594
Sample: Mothers 1 year after childbirth				
Share formally employed	0.33	0.09	5,076	2,581,852
Total earnings (Yearly)	2217.08	852.48	5,076	2,581,852
Earnings if employed (Yearly)	6606.77	1656.01	4,843	2,573,597
Work hours if employed (Weekly)	24.36	3.63	4,843	2,573,597
Wage if employed (Hourly)	5.07	1.44	4,843	2,573,597
Sample: Fathers-to-be 1 year before childbirth				
Share formally employed	0.57	0.17	3,875	904,030
Total earnings (Yearly)	6444.92	2897.08	3,875	904,030
Earnings if employed (Yearly)	11313.87	3713.83	3,575	885,704
Work hours if employed (Weekly)	33.16	7.15	3,575	885,704
Wage if employed (Hourly)	6.35	1.70	3,575	885,704
Sample: Fathers 1 year after childbirth				
Share formally employed	0.55	0.16	4,764	1,107,490
Total earnings (Yearly)	6448.60	2849.54	4,764	1,107,490
Earnings if employed (Yearly)	11740.74	3973.22	4,390	1,085,336
Work hours if employed (Weekly)	33.23	5.84	4,390	1,085,336
Wage if employed (Hourly)	6.65	2.37	4,390	1,085,336
Sample: Districts, 2010 Census				
Household income (Yearly)	38649	30734	4,710	4,710
Number of households	7575	5777	4,710	4,710
Population	23758	16892	4,710	4,710
Share of households with female head	0.44	0.05	4,710	4,710

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 panel corresponds to, respectively, mothers-to-be 1 year before childbirth, mothers 1 year after childbirth, fathers-to-be 1 year before childbirth, fathers 1 year after childbirth and the general population by districts in the 2010 Census. All monetary values are BRL.

Table 2: Effects of Childcare Expansion

	Employment		Earnings	
	Pre	Post	Pre	Post
Mothers	-0.005 (0.006)	0.021*** (0.007)	-64.8 (63.1)	154.7** (65.9)
Mothers-to-be	-0.002 (0.004)	0.004 (0.006)	-69.3 (47.3)	48.5 (59.5)
Fathers	0.010 (0.012)	0.004 (0.014)	182.9 (219.0)	99.4 (221.5)

Notes: This table shows the average estimated effects for the Pre- and Post-expansion periods, 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. Earnings in 2010 BRL.
 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effects of Childcare Expansion - Heterogeneity

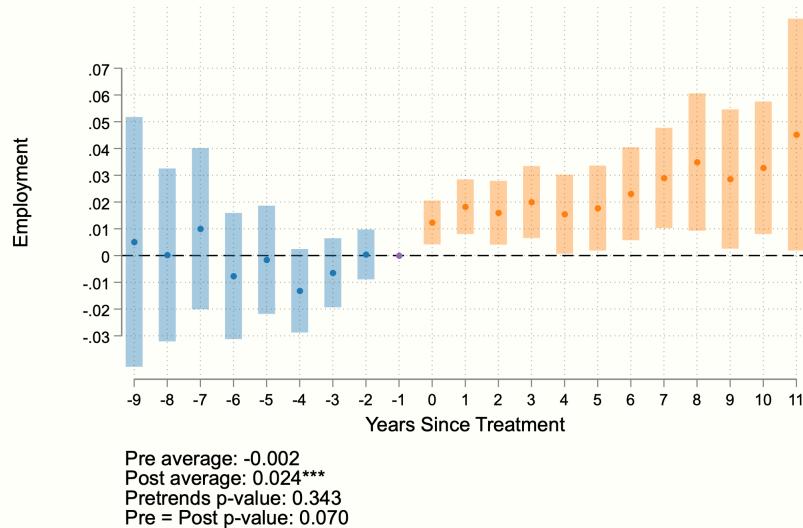
	Employment		Earnings	
	Pre	Post	Pre	Post
Migration				
Immigrants	-0.007 (0.009)	0.014** (0.007)	-110.9 (85.1)	99.0 (77.8)
Natives	0.001 (0.009)	0.019** (0.009)	-44.3 (126.8)	147.1 (114.0)
Education				
Low	-0.003 (0.010)	0.022*** (0.008)	0.4 (97.9)	210.7** (97.6)
High	-0.002 (0.009)	0.016* (0.009)	-75.7 (100.4)	57.4 (99.1)
Share of Pentecostals				
Low	0.004 (0.010)	0.017** (0.008)	49.2 (90.1)	204.1* (108.5)
High	-0.011 (0.008)	0.024*** (0.008)	-132.3 (81.1)	142.7* (78.7)
Share of Female Household Heads				
Low	-0.016* (0.009)	0.006 (0.008)	-139.6* (83.1)	40.7 (82.6)
High	0.009 (0.009)	0.038*** (0.009)	-4.5 (87.1)	266.9*** (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 in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

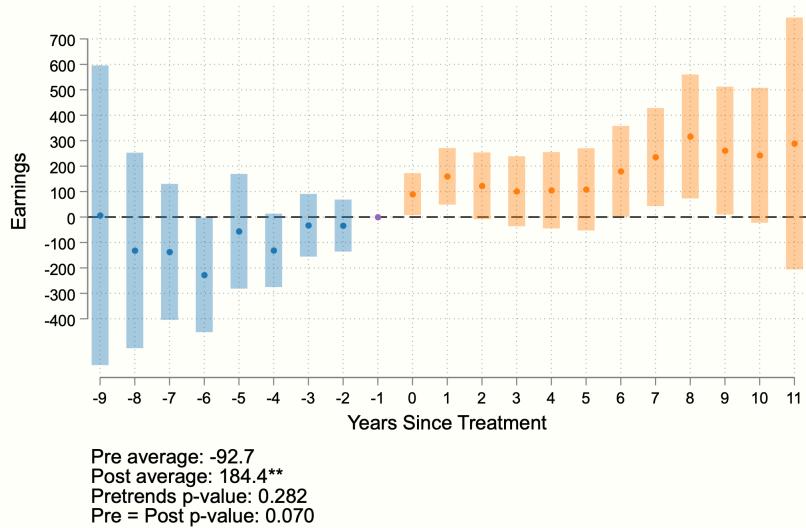
A Appendix

Figure 10: Effect of Expansion on Mothers - Extended

Employment

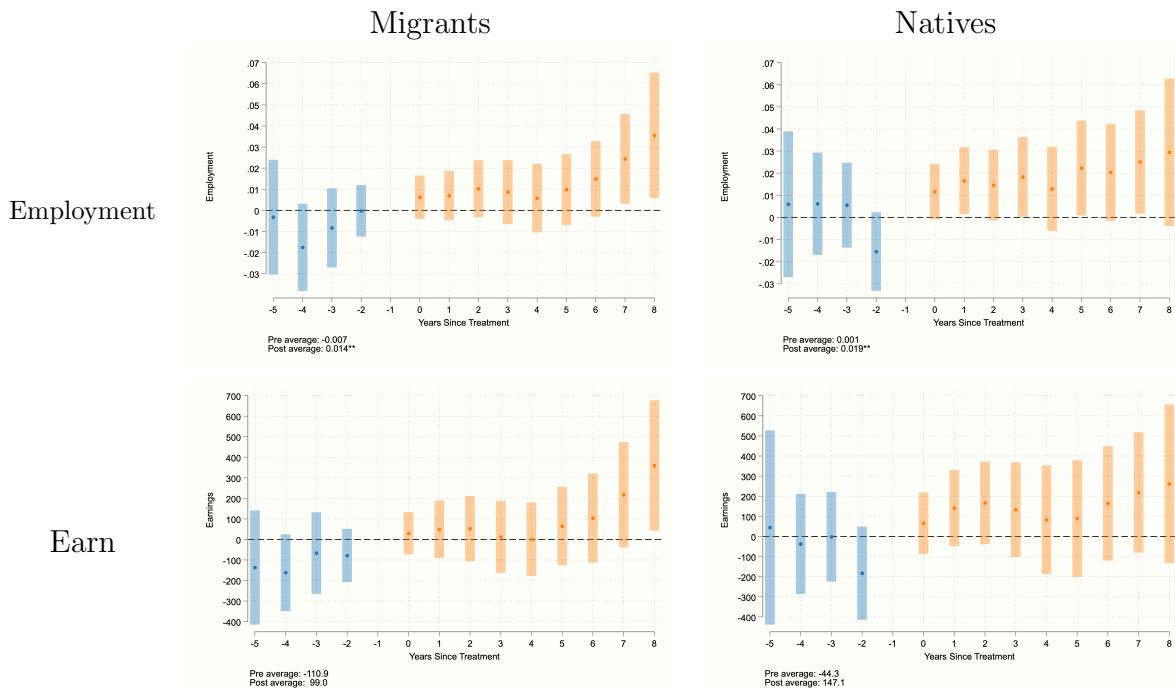


Earnings



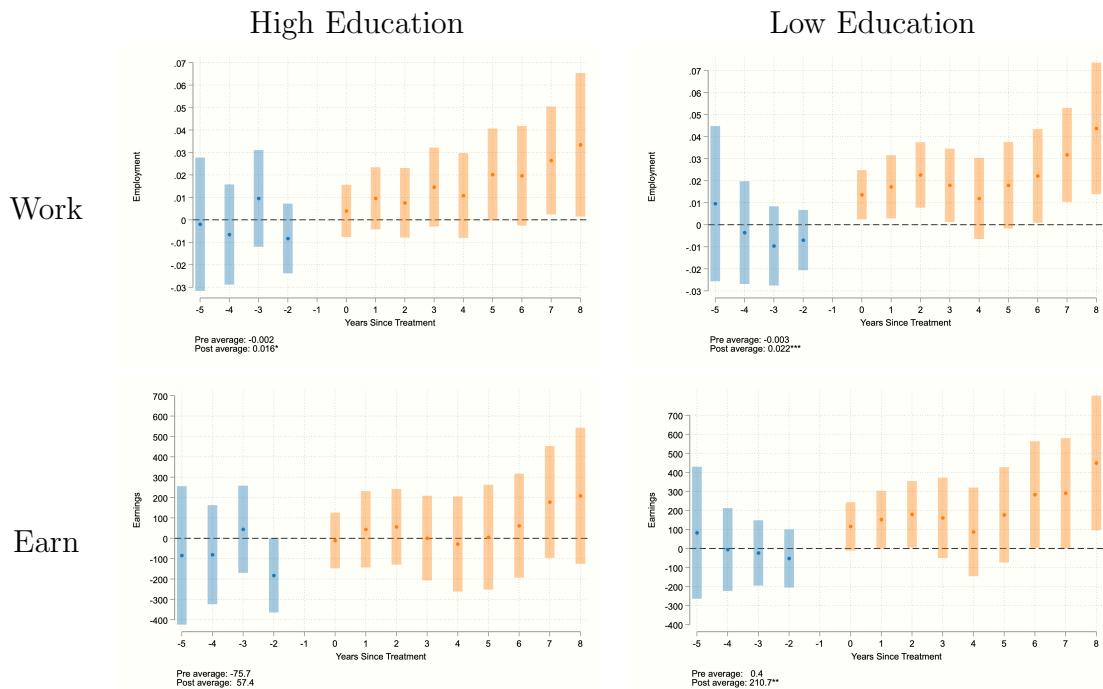
Notes: This figure shows the estimated effect of an expansion on mother's employment and earnings. The sample includes mothers of children from 0 to 3 years of age. The bars represent uniform confidence intervals. The statistics in the bottom show 1) the average value of the pre-treatment estimates, 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 11: Heterogeneity: Migration



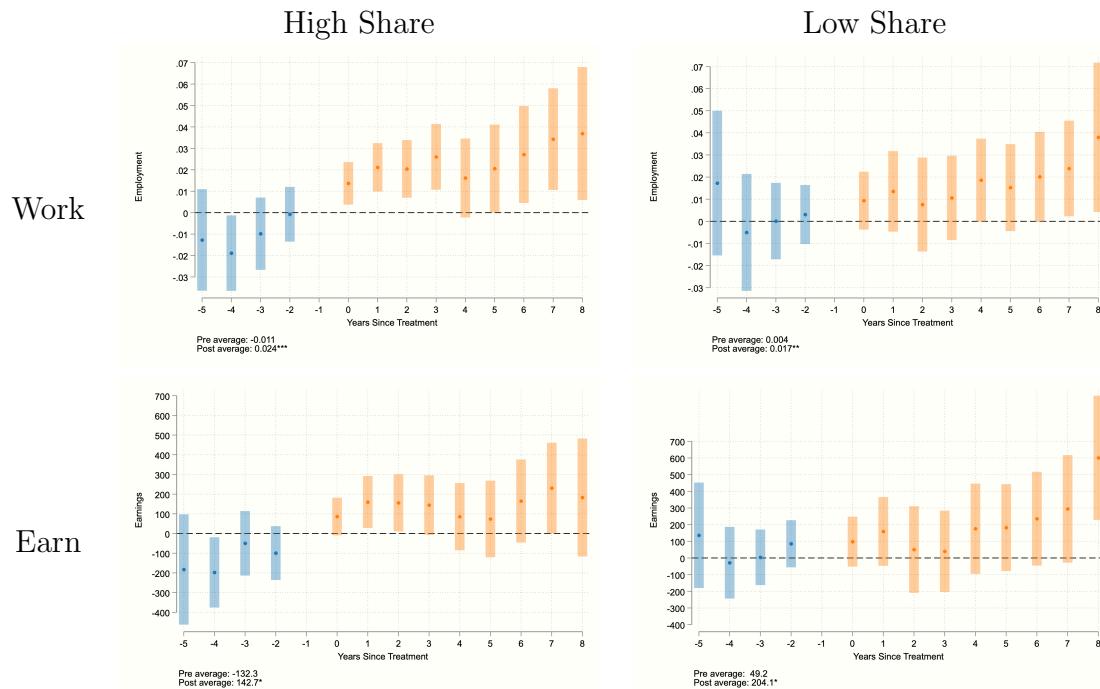
Notes: This figure shows the estimated effect of an expansion on mother's employment (top) and earnings (bottom), for mothers who are migrants (left) and natives (right). Natives are defined as people who were born in São Paulo, while migrants are people who were born anywhere else. The sample includes 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 12: Heterogeneity: Education



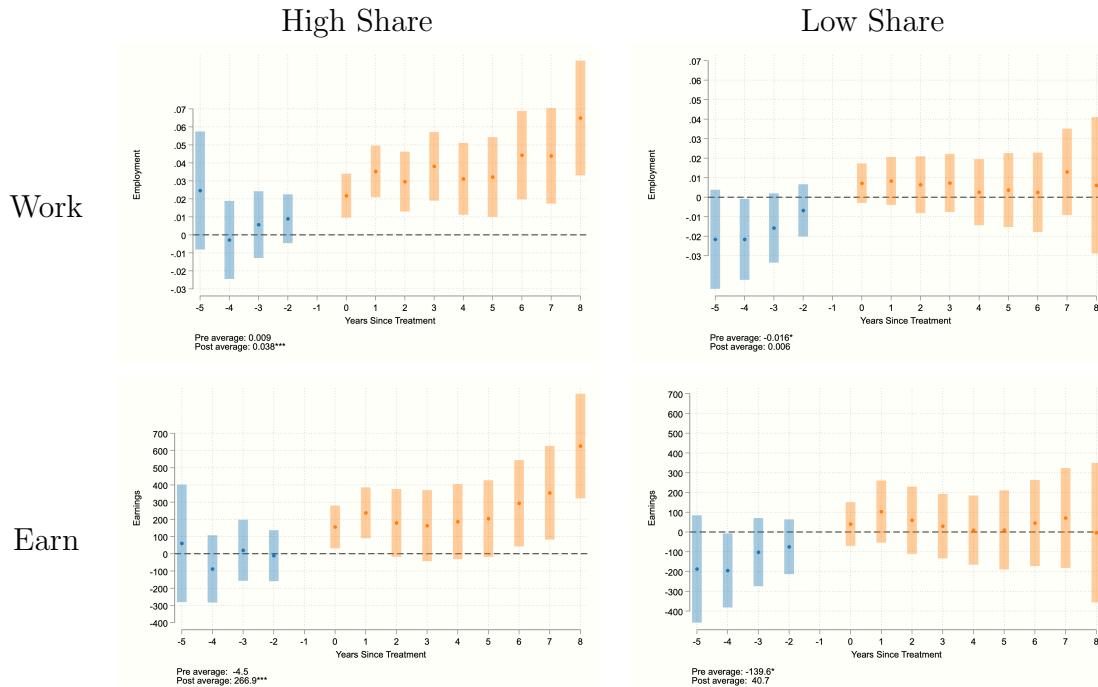
Notes: This figure shows the estimated effect of an expansion on mother's employment (top) and earnings (bottom), for mothers with low education (right) and high education (left). Low-education mothers are those that completed high school or less. The sample includes mothers of children from 0 to 3 years 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 13: 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 14: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$