

FREE CHILDCARE AND THE MOTHERHOOD PENALTY AND OTHER ESSAYS

By

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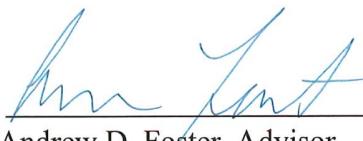
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This dissertation by João Marcos Bastos Vilar Garcia is accepted in its present form by the Department of Economics as satisfying the dissertation requirement for the degree of Doctor of Philosophy.

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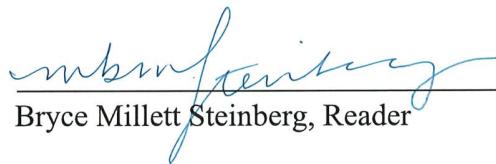


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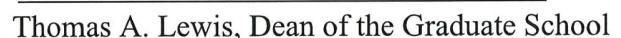
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I thank my wife Marcela, my parents, and my sister.

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CHAPTER 1

Free Childcare and the Motherhood Penalty: Evidence from São Paulo

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 “child penalty,” the dip in female labor-force participation after childbirth (Bertrand et al., 2010; Angelov et al., 2016; 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 and Mogstad, 2011; Olivetti and Petrongolo, 2017; Kleven et al., 2020). 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 within 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

¹Kassab was elected vice-mayor in 2004 and succeeded 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.

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. 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 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 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

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

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 the roll-out of public childcare effects on mothers' labor outcomes, finding mixed results (Baker et al., 2008; Berlinski and Galiani, 2007; Bauernschuster and Schlotter, 2015; Bettendorf et al., 2015; Havnes and Mogstad, 2011; Cascio, 2009; Andresen and Havnes, 2019; Rabaté and 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 (Carta and Rizzica, 2018; Kleven et al., 2020; Brewer et al., 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 within this second strand of literature, providing evidence from a middle-income country, a context more similar than that of a large share of women across the world.

This paper is closely related to Attanasio et al. (2022b), 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

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

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

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 Attanasio et al. (2013), Bernal and Fernández (2013); Bernal et al. (2019) and Attanasio et al. (2022a) 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 1.1, we present the background and describe the childcare program in São Paulo. Section 1.2 details the data used in this paper. We explain the methods in Section 1.3 and Section 1.4 presents the results. Finally, Section 1.5 concludes.

1.1 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

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

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.

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 and Verdú, 2016), result in high demand for public childcare. Despite increased participation, the motherhood penalty remains large. Figure 1.3 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 1.1. This rapid expansion was achieved almost exclusively through partnerships with nonprofit childcare providers. Figure 1.1 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 1.2 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.

1.2 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.

1.2.1 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.

1.2.2 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

⁶<http://dados.prefeitura.sp.gov.br/>

(Campello and Neri, 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).

1.2.3 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

⁷Some of the main programs that are conditional on registration include: Bolsa Família; 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).

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.

1.2.4 Descriptive Statistics

Table 1.1 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 1.2 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.

1.3 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.

1.3.1 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 and Sant’Anna, 2021; de Chaisemartin and 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

$$\theta(e) = \sum_{g \in \mathcal{G}} \mathbf{1}\{g + e \leq \mathcal{J}\} P(G = g | G + e \leq \mathcal{J}) ATT(g, g + e) \quad (1.1)$$

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 1.4 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.

1.3.2 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 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:

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

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.

1.4 Results

1.4.1 Main Effects

First, we find that, as expected, a childcare expansion strongly increases availability. Figure 1.5 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 1.6 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 1.7 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 1.8 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 1.3 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 robustness 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 the online Appendix, the results are almost identical to our baseline specification.

Second, we show that the results are robust to different estimation methods. Figure 1.9 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 apparent difference being Borusyak et al. (2021) exhibiting positive estimates for earnings before the treatment, but with large standard errors.

1.4.2 Within-District Results

As an alternative strategy, we estimate the effect of childcare availability directly in a fixed-effects regression. Figure 1.10 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 1.11 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.

1.4.3 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 1.4 Panel A shows estimated effects for migrants and natives. We define migrant mothers as women who were not born in São 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 1.4 Panel B 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 avail-

ability (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 homemakers⁸ Table 1.4 Panel C 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 1.4. 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.

1.5 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.

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

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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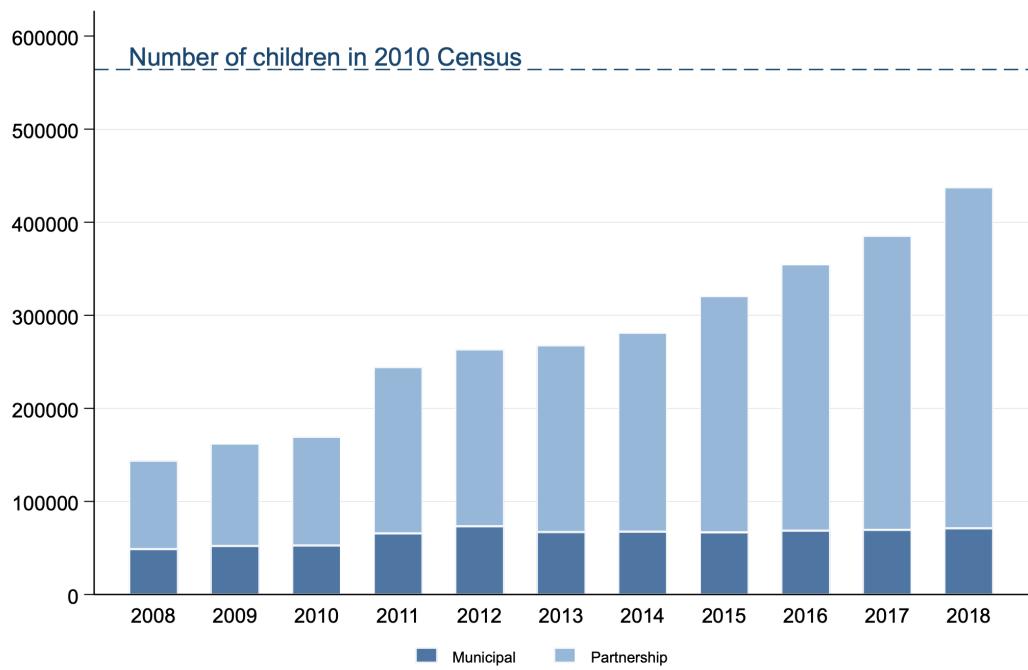
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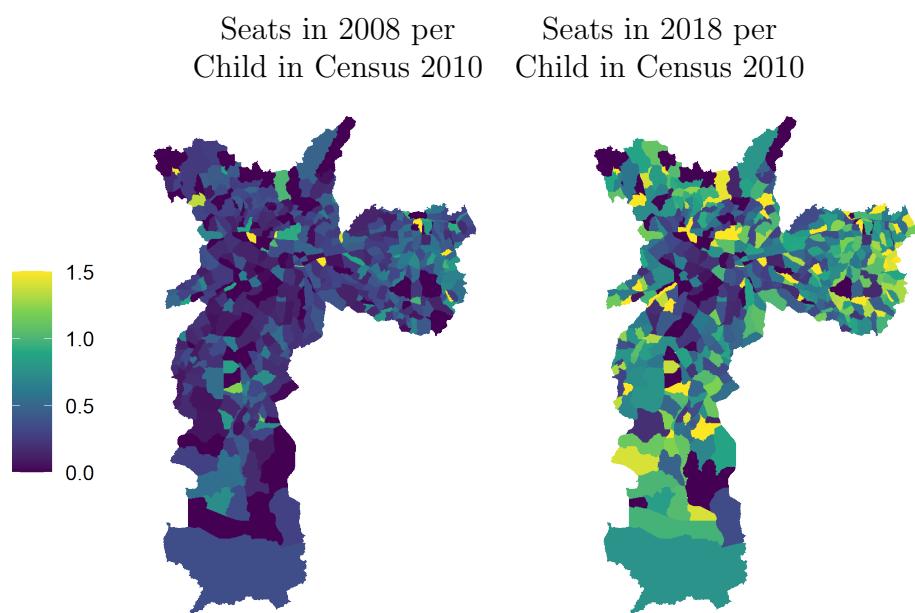
1.6 Figures and Tables

Figure 1.1: Children Attending Childcare by Type of Provider



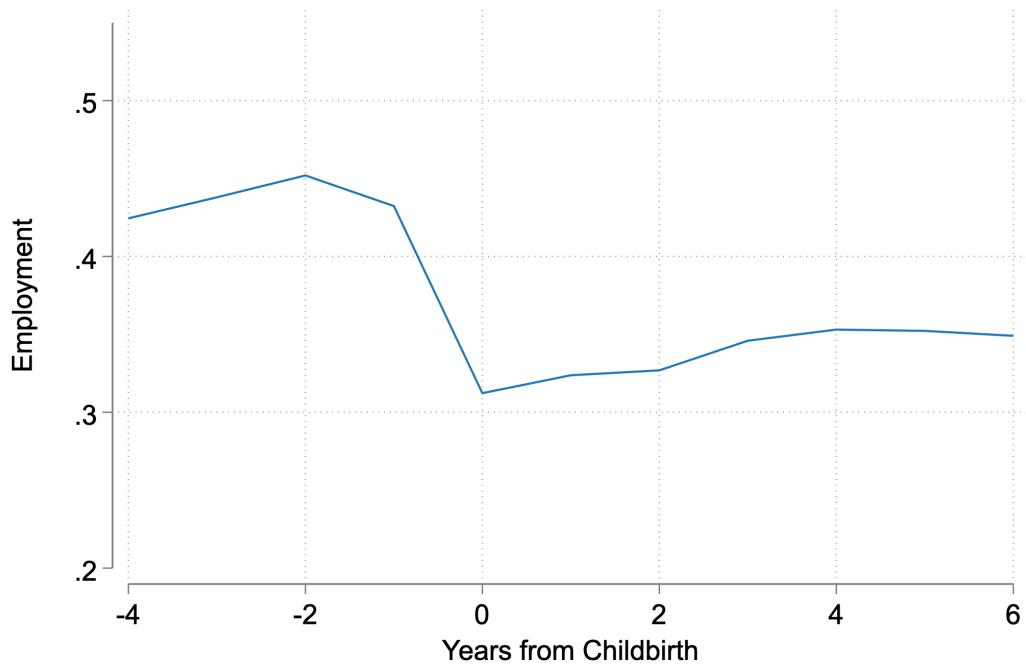
Notes: This figure shows total enrollment in the childcare system at a) facilities funded by the municipal government and operated by non-profit partners and b) facilities funded and operated by the municipal government.

Figure 1.2: Childcare Enrollment per Educational District



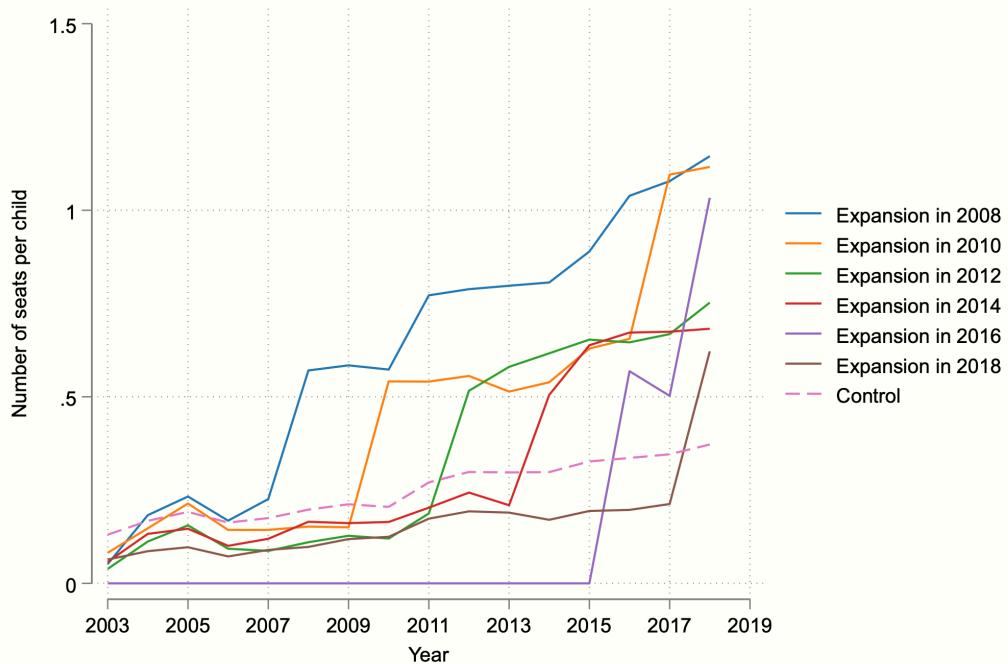
Notes: This figure shows childcare enrollment rates by educational districts in 2008 and 2018. The rate is defined as the ratio between the number of childcare seats in a given district divided by the population between 0 and 3 years of age residing in that district in the 2010 Brazilian Census.

Figure 1.3: Child Penalty



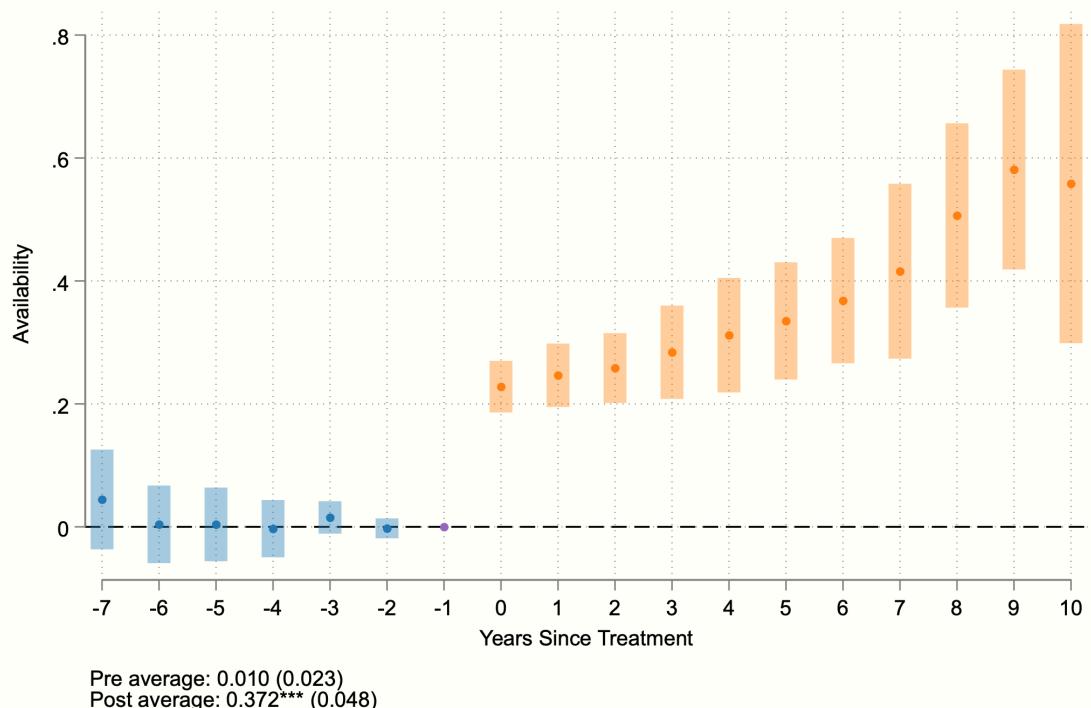
Notes: This figure shows the average employment rate in the formal sector for women in São Paulo around the year of birth of a first child, denoted as 0. Data includes years 2007 to 2018.

Figure 1.4:
Childcare Availability by Treatment Year



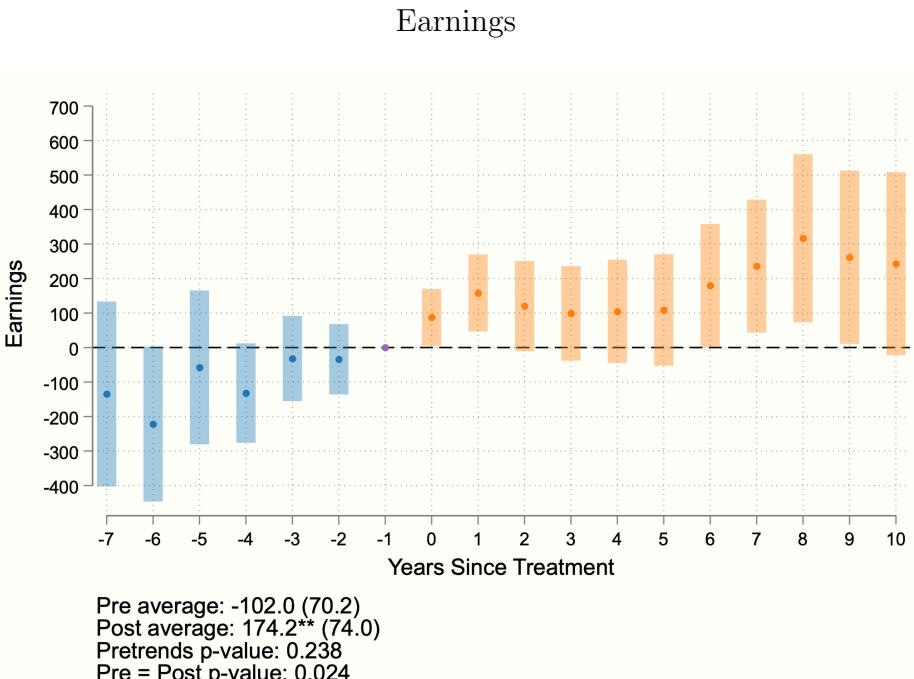
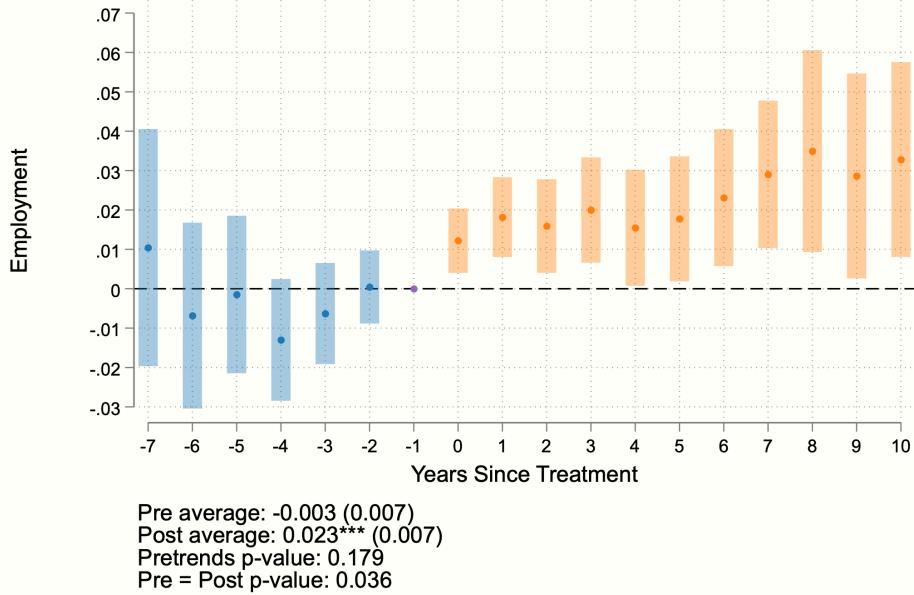
Notes: This figure shows the evolution of the number of total seats per child in the 2010 Brazilian Census. Data are grouped by the year of expansion, defined as the year of the largest increase or the year the first childcare facility opened in the district. The control group includes districts where 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 1.5: First Stage Effects on Childcare Seats per Child



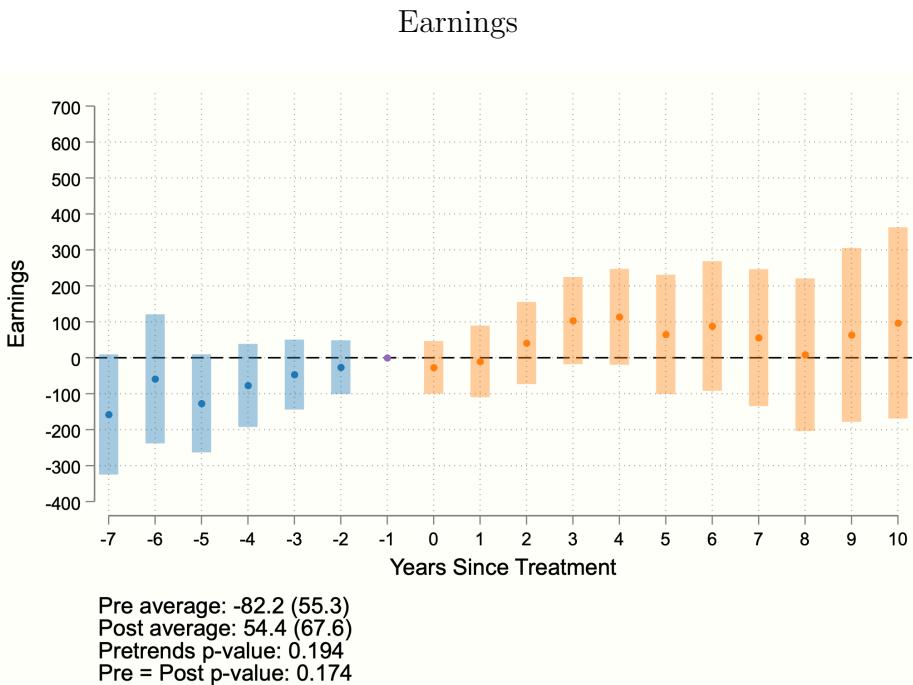
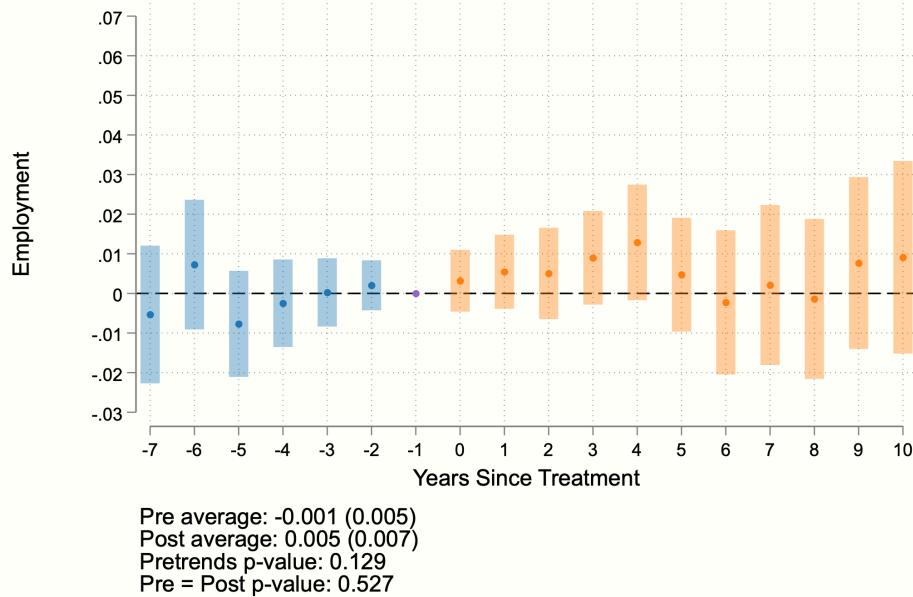
Notes: This figure shows the estimated effect of an expansion in childcare availability, defined as seats per child in the 2010 Brazilian Census. The statistics at the bottom show the average value of the Pre- and Post-treatment estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1.6: Effect of Expansion on Mothers
Employment



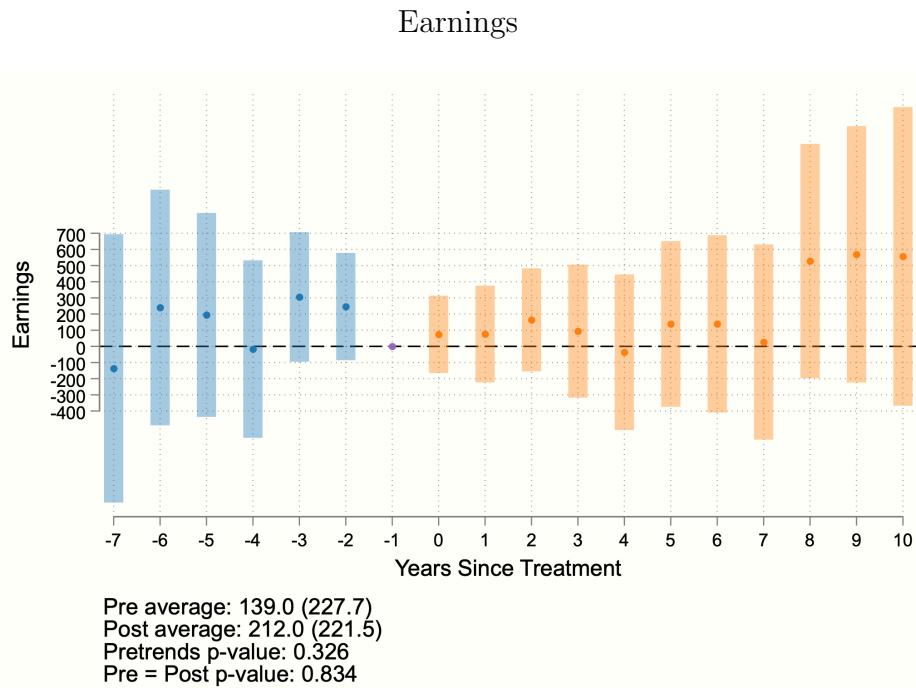
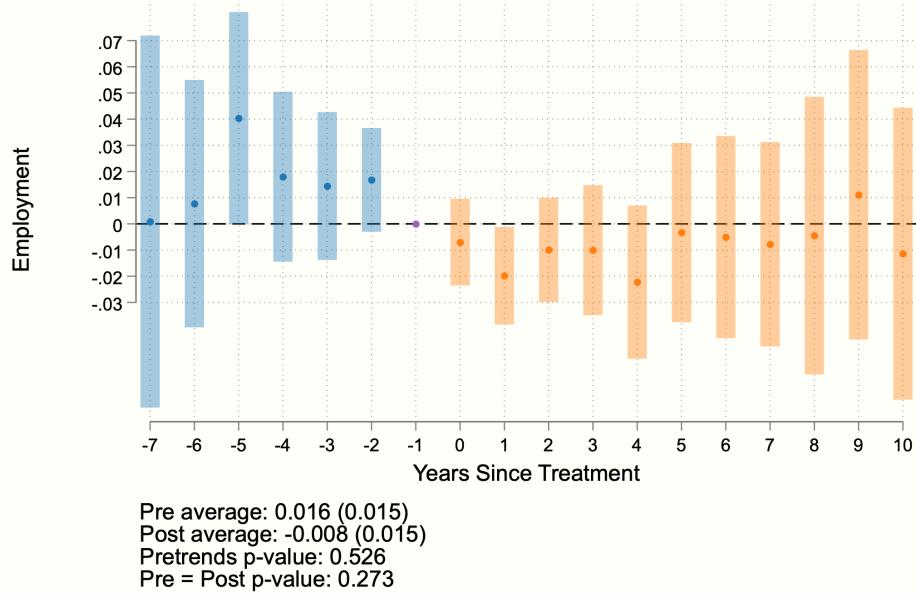
Notes: This figure shows the estimated effect of an expansion on mothers' employment and earnings. The sample includes mothers of children from 0 to 3 years of age. The bars represent uniform confidence intervals. The statistics at the bottom show the average value of the estimates pre-treatment (1) 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 1.7: Effect of Expansion on Mothers-to-be
Employment



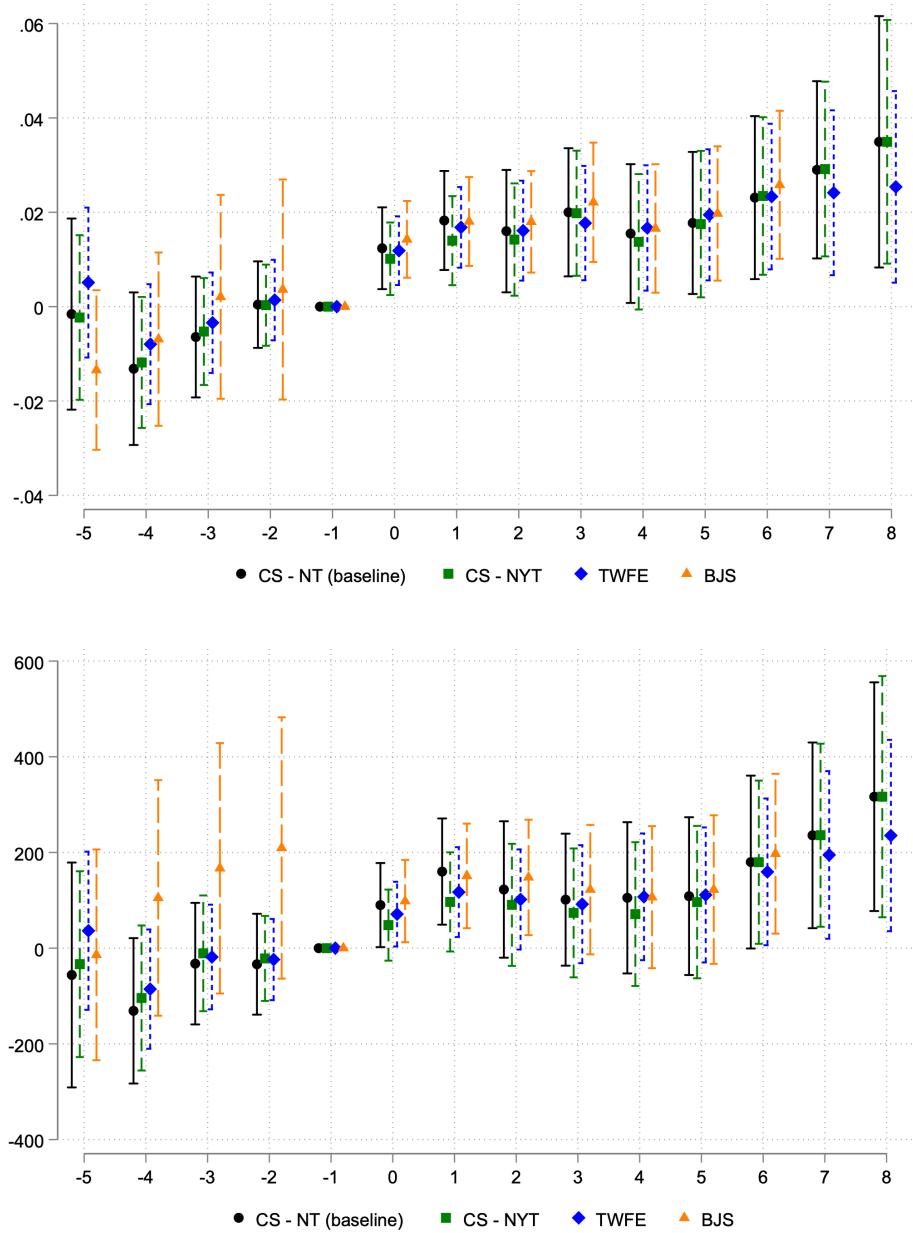
Notes: This figure shows the estimated effect of an expansion on the employment and earnings of mothers-to-be, including from 4 years before childbirth to 1 year before childbirth. The statistics at the bottom show the average value of the estimates pre-treatment (1) and post-treatment (2), the p-value 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 1.8: Effect of Expansion on Fathers
Employment



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-treatment (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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

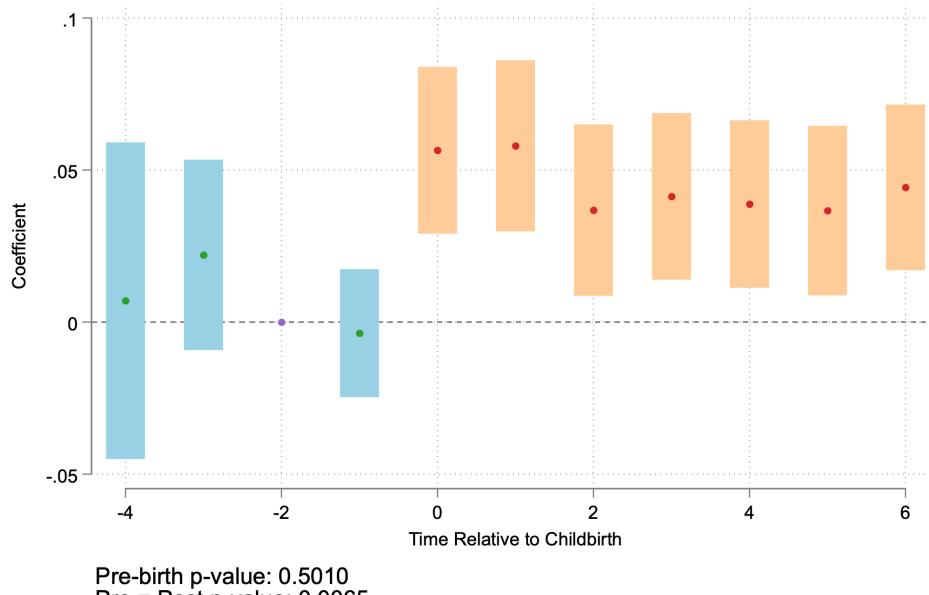
Figure 1.9: Robustness - Effect of Expansion on Mothers' Employment



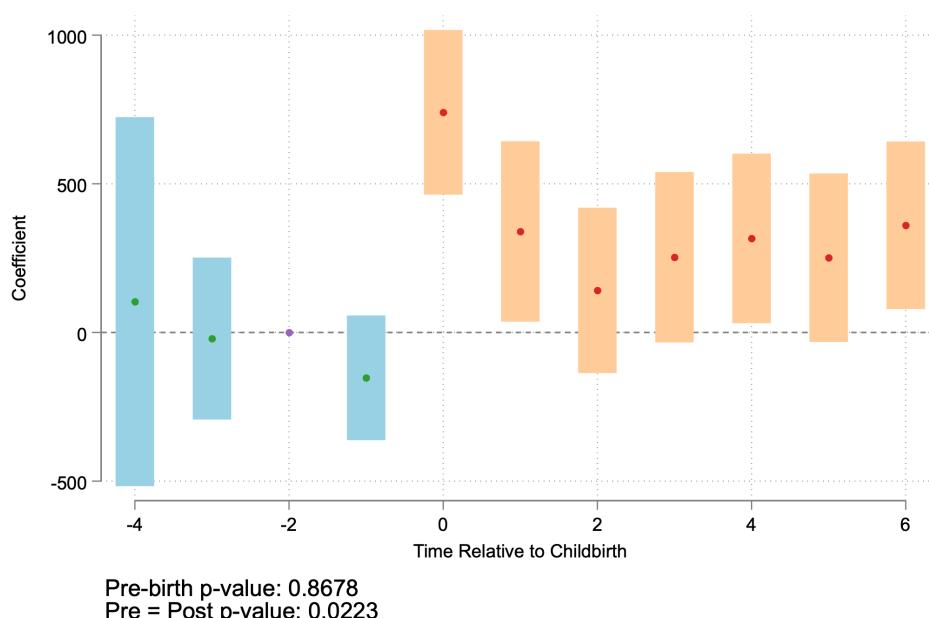
Notes: This figure shows robustness of the main effects to alternative estimators. The estimators presented are (1) Callaway and Sant'Anna (2021) with “never-treated” controls, (2) Callaway and Sant'Anna (2021) with “not-yet-treated” controls, (3) two-way fixed effects, and (4) Borusyak et al. (2021).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

Figure 1.10: Effects of Childcare Availability on Mothers by Time from Childbirth

Employment



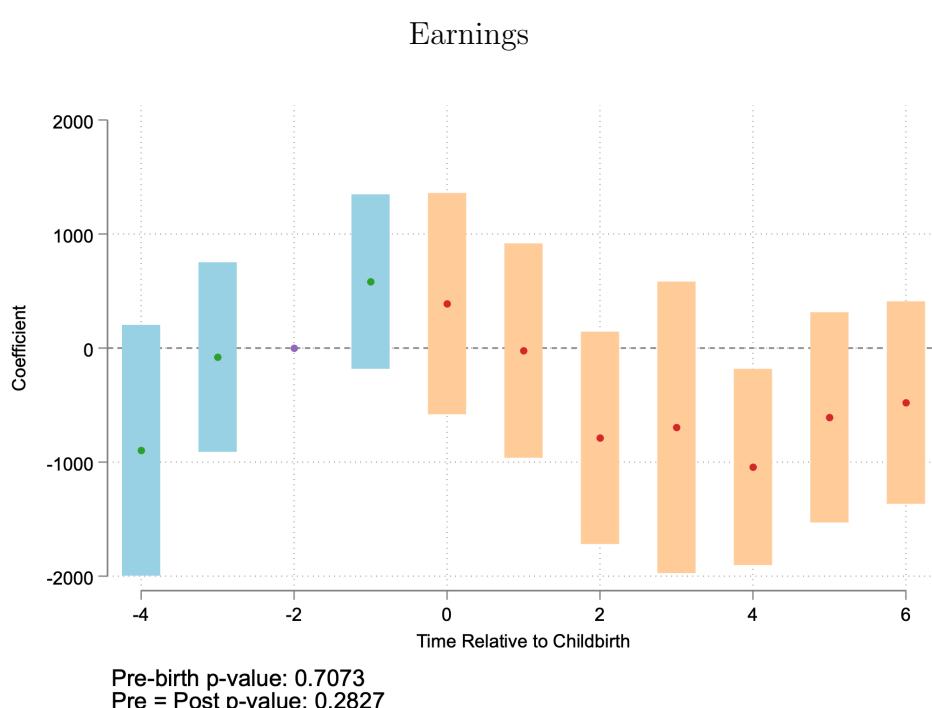
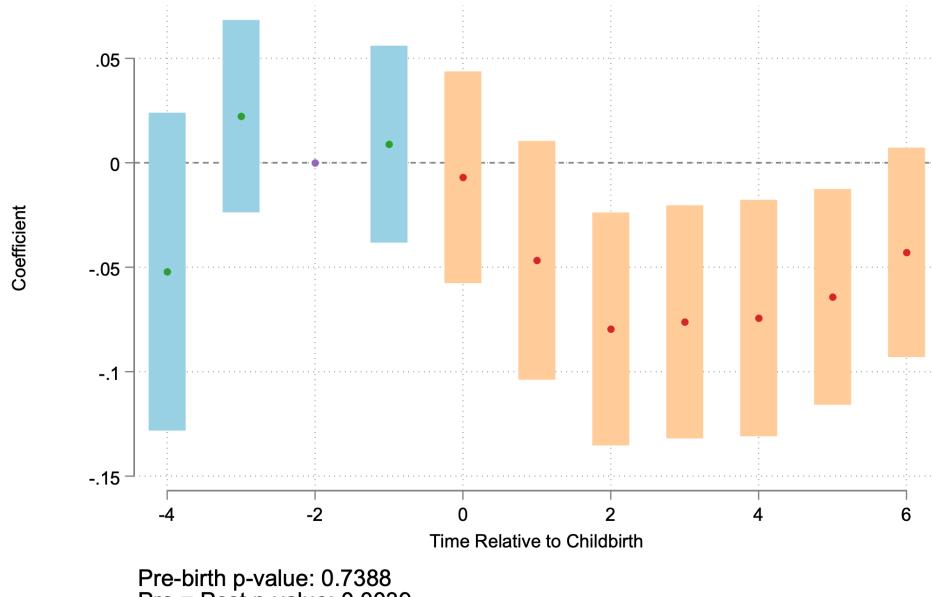
Earnings



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 1.11: Effects of Childcare Availability on Fathers by Time from Childbirth

Employment



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 1.1: Summary Statistics

	Mothers		Fathers	
	Before	After	Before	After
Share formally employed	0.44 (0.11)	0.33 (0.09)	0.55 (0.16)	0.54 (0.15)
Total earnings (Yearly)	3,465 (1,162)	2,217 (852)	5,622 (2,464)	5,883 (2,553)
Earnings if employed (Yearly)	7,749 (1,567)	6,607 (1,656)	10,235 (3,127)	10,912 (3,527)
Work hours if employed (Weekly)	29.04 (3.44)	24.36 (3.63)	31.62 (7.04)	32.25 (5.58)
Wage if employed (Hourly)	4.95 (0.78)	5.07 (1.44)	6.00 (1.49)	6.33 (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 1.2: 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 1.3: Effects of Childcare Expansion

	Availability	Employment	Earnings
	First Stage		
Post Expansion	0.328*** 0.042		
	Mothers		
Post Expansion		0.021*** (0.007)	154.7** (68.5)
Re-scaled Effect		0.064	471.6
	Mothers-to-be		
Post Expansion		0.007 (0.008)	87.7 (76.7)
Re-scaled Effect		0.021	267.4
	Fathers		
Post Expansion		-0.009 (0.013)	135.1 (196.3)
Re-scaled Effect		-0.027	411.9

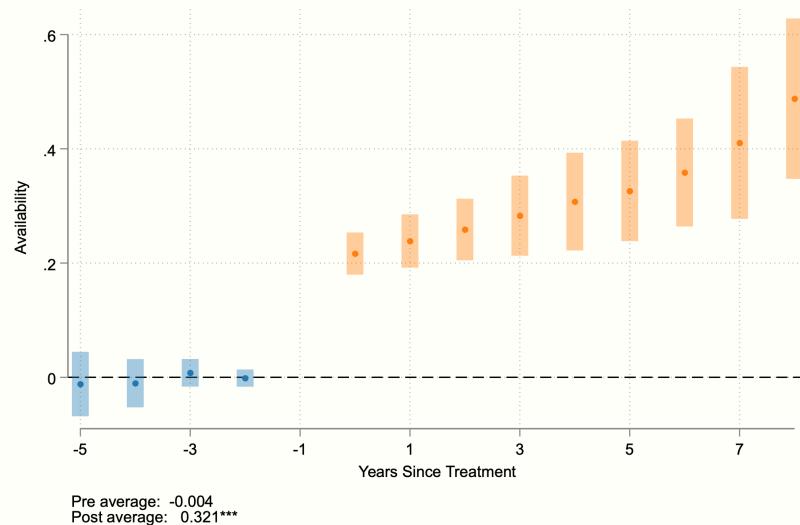
Notes: This table shows the average estimated effects over periods 0 through 8 after an expansion, for mothers, mothers-to-be and fathers. The mother and father samples include parents from 0 to 3 years after childbirth. The mothers-to-be sample includes 4 to 1 year before childbirth. The third line 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 1.4: 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 are in 2010 BRL. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1.12: 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-treatment estimates, 2) the average value of post-treatment estimates * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER 2

Congenital Disability Effects on Parents' Labor Supply and Family Composition: Evidence from the Zika Virus Outbreak

Mothers' labor market decisions are influenced by their children's characteristics, and severe, permanent disability may be one of the most profoundly impactful. Traditionally, women meet the additional demands, so the dip in labor market participation after childbirth may be larger for mothers of disabled children. This dip is especially problematic because disabled children also need more financial resources for medical treatment and adaptation in addition to time and attention. Therefore, estimating the effect of child disability on maternal employment is crucial for the design of policies that help these families.

The small existing literature on child disability and maternal employment faces challenges dealing with unobserved co-founders. For instance, mothers who follow preventive recommendations such as folate supplementation or abstaining from smoking are likely

different in other relevant dimensions than those who do not. This concern is identified and dealt with in various ways in the broader literature on child health and mother's work (e.g., Frijters et al. (2009) use instrumental variables, Breivik and Costa-Ramón (2022) use panel data to obtain a valid comparison group). Existing work on child disability, however, has not dealt explicitly with it (Salkever, 1982; Powers, 2001, 2003; Wasi et al., 2012; Gunnsteinsson and Steingrimsdottir, 2019; Chen et al., 2023; Cheung et al., 2023).

In this paper, we provide evidence on the causal effects of child disability on parental labor force participation, household composition, fertility, and income by exploiting a large shock to the incidence of child disability: the 2015 Zika virus outbreak in Brazil. The outbreak caused several thousands of children to be born with a severe disability, microcephaly. We argue that the sudden onset of this event and the characteristics of the infection rule out endogeneity of maternal health behaviors.

Using detailed data on the universe of births and formal employment links in the country, we show that, before childbirth, affected mothers had similar labor market trajectories to other mothers matched in a simple set of characteristics. However, starting the typical maternity leave (6 months), their labor force participation and earnings fall much faster. These mothers see a 60% larger motherhood penalty, corresponding to a fall from 15% to about 5%. For fathers, we do not find any effect in the formal labor market participation, nor see lower cohabitation rates. We also document that, for households where the first child was born during the Zika outbreak, families with a child with microcephaly are less likely to have other children.

The Zika virus outbreak in Brazil in 2015 provides a valuable case study because its particular characteristics rule out several threats to identification. Since it is transmitted by a common mosquito, anyone in affected areas could be exposed. The sudden introduction of the virus, along with its undiscovered link with natal defects, means that differences in preventive behavior are unlikely: no one could know to be concerned. Even after public health authorities identified the outbreak and raised awareness, prevention had only a

long-delayed effect because infection is more likely to cause microcephaly when it happens in the first trimester. Another potential threat to identification, selective abortion, is unlikely for two reasons: infection is asymptomatic in most cases, so women are unaware, and diagnosis of microcephaly is difficult before birth. Finally, Zika has no lasting effects on adults, ruling out direct effects on labor supply.

Children affected by the Zika outbreak developed microcephaly, a severe, life-long disability that puts significant strain on parental resources. The condition is characterized by underdevelopment of the brain, resulting in cognitive and developmental disabilities. Children often suffer from seizures and must have access to therapy to develop speech and movement. Brazil's public health care system offers free treatment, but families may have difficulty accessing it, particularly in remote areas. Furthermore, even with free medical treatment, families must spend significant time caring for affected children at home.

To study the impact of the outbreak on maternal labor outcomes, we use three administrative datasets. The first is SINASC/SUS, which logs all births in the country and details the municipality and date of the delivery, the mother's residence, the mother's date of birth, and whether the newborn has microcephaly. Microcephaly occurs very rarely due to causes unrelated to Zika, so we can confidently link cases during 2015-2016 to the outbreak. The second is the Annual Account of Social Information (Relação Anual de Informações Sociais, RAIS). This dataset allows us to follow an individual's employment history throughout the entire period and observe monthly earnings, hours, and maternity leave dates. We link these two datasets using the Single Registry, a federal registry of all recipients of social programs. Recipients undergo interviews with local government agents and answer a standardized questionnaire on the socioeconomic characteristics of all household members. Recipients must keep this information updated every couple of years to ensure eligibility for social programs.

To isolate the causal effect of child disability, we compare the labor market trajectory of mothers of children with microcephaly to a matched comparison group. This group

is composed of all mothers in the same municipalities who gave birth during the same months as mothers of children with microcephaly. We compare the average labor force participation between these two groups each month following maternity leave. We argue this method yields causal estimates for two main reasons. First, the unexpected nature of the epidemic and the characteristics of the infection make selection bias unlikely. Second, the groups are similar in observable characteristics, including previous trajectories in the labor force.

We find that mothers of children with microcephaly are about 50% (3.2 percentage points) less likely to have a job in the formal sector than matched mothers. This difference starts about six months after the start of maternity leave and persists for as long as we can estimate, i.e., 36 months. We find no effect on father's labor market outcomes using the same method.

The literature on the effects of child disability on parents' labor supply is still small. Salkever (1982); Powers (2001); Wasi et al. (2012). Chen et al. (2023) and Cheung et al. (2023) study the impact of congenital disability in Taiwan, and Gunnsteinsson and Steingrimsdottir (2019) study this question in Denmark. Our paper contributes to this literature by examining the case of an arguably exogenous increase in the chance of having a child with congenital disability. While this literature so far can only control for observable characteristics that are related to disability, the use of an exogenous shock provides a stronger argument for identification.

We also contribute to the literature on parental response to children's adverse health. Most previous research focuses on maternal labor supply only and relies mainly on survey data, which have limited capacity to examine parents' dynamic responses due to a lack of extended follow-up (Wolfe and Hill, 1995; Frijters et al., 2009; Burton et al., 2017; Lafférs and Schmidpeter, 2021). More recent work using longitudinal administrative data looked at parental labor supply response to various child health shocks (Eriksen et al., 2021; Breivik and Costa-Ramón, 2022; Adhvaryu et al., 2022; Chen et al., 2023; Cheung et al.,

2023; Vaalavuo et al., 2023). Our study is restricted to a particular type of congenital disease caused by an exogenous shock. Therefore it is unlikely to be correlated to parents' behaviors, genetics, or age, mitigating bias in the estimated effects.

Our paper is also related to the literature on the motherhood penalty and gender inequality (Budig and England, 2001; Kleven et al., 2019; Sieppi and Pehkonen, 2019; Cortés and Pan, 2023; Quinto et al., 2020; Musick et al., 2020; Berniell et al., 2021). We focus on the additional penalty associated with a disabled child, which can compound the adverse labor market effects on mothers and increase gender inequality, as childcare responsibilities tend to fall on mothers.

2.1 Background

The 2015 outbreak of Zika in Brazil provides an exogenous shock to the rate of child disability, with other characteristics that also help to isolate its effect on mother's employment. Selection driven by differences in preventive behavior is addressed by the sudden and widespread nature of the outbreak in the affected regions. Selective abortion is unlikely because diagnosis is difficult in the uterus, and adults have no symptoms. The lack of symptoms also rules out direct effects of the virus on labor outcomes.

The Zika virus was introduced to Brazil around 2014, where it had never been observed before. The virus spreads through a common mosquito, the Aedes Aegypti, which also transmits dengue, yellow fever, and Chikungunya. It affects around 2 million Brazilians per year. The outbreak was first identified in late 2015 following a spike in cases of microcephaly. The Northeast of Brazil was particularly affected, but infection was widespread within the region and anyone could be exposed.

Exposure to the Zika virus in pregnant mothers, especially in the first trimester, can cause microcephaly in the newborn, a severe, lifelong disability. Microcephaly is characterized by underdevelopment of the brain, resulting in smaller head circumference

than normal. Children with microcephaly need frequent medical and parental attention. They often suffer from seizures, vision and hearing problems, intellectual disabilities, and difficulty with motor and speech development. Brazil's public health care system offers free treatment, including continuing therapy, but families may have trouble accessing it, particularly in remote areas.

In contrast with the dramatic effects on newborns, Zika infection has no lasting effects in adults, so it should not directly impact labor supply. About 80% of adult cases show no symptoms (Haby et al., 2018). In the other cases, typical symptoms are fever and rashes lasting up to a week. One exception is that there have been reports of an increased chance of developing Guillan-Barré syndrome, a severe, potentially lethal condition. However, even this increased risk is extremely rare and would not have any relevant impact on our results.

The outbreak was focused on the Northeast, started suddenly, and ended fast. Figure 2.1 shows a map with the number of microcephaly cases per 1000 births in 2015 and 2016 in each of the five regions of Brazil. The Northeast region was hit the hardest by the epidemic, reaching an average rate of 1.55 microcephaly births per 1,000, or 1,305 total cases. The South was relatively untouched, and the other regions had intermediate levels of incidence. Other than this regional variation, there are no apparent spatial patterns that could indicate, for instance, strong clustering around cities receiving tourists at the time.

Figure 2.2 shows the timeline of the epidemic, with cumulative cases in the top graph and monthly cases in the bottom. During the second half of 2015, the number of cases increased abruptly, from close to zero to the peak incidence in just about three months. The subsequent fall in cases was almost as fast, with a much more modest second wave in the latter half of 2016.

Differential exposure to the virus based on differences in mothers' preventive behavior

is unlikely cause bias for two main reasons. First, Zika had never been observed in Brazil, and second, the link to microcephaly in newborns was unknown. The first signs of a new disease were observed in March 2015, and researchers first identified the increase in microcephaly in October. Researchers could only identify the causal link between these facts in 2016, so mothers would only know to take precautions afterward. Even then, preventive measures would probably only cause a reduction in cases of disabled children with a significant delay. Since the virus is more likely to cause microcephaly during the first trimester of pregnancy, its effects can be undetected for several months.

Another potential threat to identification, differential rates of abortions, is unlikely for several reasons. First, microcephaly is difficult to identify in the uterus, and mothers would have to decide to terminate pregnancy without confirmation that their baby is affected. Second, Zika infection is often asymptomatic, and otherwise can be similar to dengue, making it difficult for mothers to know if they have been infected. Third, even in infected mothers, the chance of the baby developing microcephaly is relatively low. Finally, abortion is illegal in Brazil except in cases of rape, or serious risk to the mothers' life.

Finally, one potential concern is that children with microcephaly have higher rates of mortality. In our main results, we do not adjust for this difference, meaning our results may be partially driven by the effects of child mortality as opposed to permanent disability (though the sign of the bias introduced is ambiguous). Infant mortality among children with Zika-induced microcephaly is 8 to 10 times higher than the average in Brazil at the time, about 12%-14% in the period 2015-2016. Although this could bias our estimates in theory, in practice, the absolute rate is small enough not to have a significant impact on our estimates.

2.2 Data

We use three administrative datasets that cover all births in the country and all formal employment links. The first is the SINASC (*Sistema de Informações de Nascidos Vivos*, or Information System on Live Births), a dataset collected by the Ministry of Health detailing every live birth within a health facility. Second, RAIS (*Relação Anual de Informação Social*, Annual Report of Social Information), is an administrative dataset used and made available by the Ministry of Labor, containing detailed information on employment links. Finally, we use the Single Registry (*Cadastro Único*), an administrative dataset used to manage and coordinate various social programs, covering essentially all of Brazil's poor population. We link these datasets using location, time of birth and, mother's age.

2.2.1 Data on Births

To identify the children affected by the Zika epidemic who were born with microcephaly, we rely on a publicly available administrative record of all births in Brazil, SINASC. We observe the municipality where the birth occurred, the municipality of the mother's residence, the date, the mother's age, and whether the newborn has microcephaly or any other birth anomaly.

This dataset contains detailed information on all live births in Brazil. It provides the location of the birth, the mother's municipality of residence, date of birth, and several variables, such as birth weight, APGAR score, and the ICD-10 codes for congenital malformations. We are able to identify whether a child is diagnosed with microcephaly at birth by the microcephaly ICD-10 code. These data are high quality and coverage is close to 100% (Oliveira et al., 2015).

2.2.2 Data on the Labor Market

To observe mothers' and fathers' labor market outcomes, we use administrative data covering all formal employment links in Brazil. We are able to follow an individual's employment history and observe monthly earnings, hours, and the dates of any maternity leave.

The 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 personal tax ID (CPF). It also includes information on many firm-level characteristics, notably the number of employees, municipality, firm tax id (CNPJ), and industry code.

2.2.3 Single Registry

To link the household members, we use the Single Registry (*Cadastro Único*) to observe families' characteristics and link different family members to formal employment data. 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 became the primary federal dataset on poverty. More than 20 social programs use it, covering virtually all of Brazil's poor (Campello and Neri, 2013). Single Registry aims to include all households with income per capita below one-half of the minimum wage (R\$255 in 2010), much higher than the official poverty threshold (R\$140 in 2010).

To be eligible for any government benefit that uses the 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 charac-

teristics, and personal tax ID (CPF). They have to inform authorities of relevant changes to family size or income.

2.2.4 Linking the Datasets

Because the public dataset on births does not include personal identifiers, we cannot directly link it to RAIS or Single Registry. We deal with this challenge using the mothers' date of birth, municipality of residence, and date of childbirth, available on Single Registry. Once we select the control and treated mothers in the Single Registry, we use their tax ID to find them in RAIS.

If we find a woman at least once in RAIS, we can re-construct her formal employment history. 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 monthly wages and hours worked from RAIS.

2.3 Empirical Strategy

For our main results, we compare the outcomes for families of children born with microcephaly to matched control families with children without this anomaly. We match families in relatively few variables: year and month of birth of the child, municipality of birth, age of the mother, and an indicator of the mother completing high school. Our key identification assumption is that, conditional on these variables, child microcephaly is as good as random. We test this hypothesis by comparing observable variables and find no pre-existing differences, and we argue that the characteristics of the epidemic made selection on non-observables unlikely.

Because we use exact matching with fairly coarse variables, it is possible for one treated unit to be matched to several possible controls, as well as for multiple treated units to have

identical characteristics. In this case, we call we cell of units with identical matching characteristics a match-group. For our main estimates, we give all treated units a weight of 1, and all control units a weight of $\frac{n_t(g)}{n_c(g)}$, where $n_t(g)$ denotes the number of treated units in the match-group, and $n_c(g)$ denotes the number of control units. Therefore, the total weight of the controls is identical to the total weight of the treated within each group.

While our main estimates are simple comparisons of (weighted) means, we also present differences-in-differences estimates, corresponding to the following model:

$$y_{ft} = \sum_{k \in (-18, \dots, 36), k \neq -9} \beta_k \cdot T_f \times \mathbb{1}(t - \tau(f) = k) + \alpha_{p(f)} + \delta_t + \varepsilon_{ft} \quad (2.1)$$

where y is the outcome of interest for family f at year-month t . T_f is a dummy indicating families with a child with microcephaly. $\tau(f)$ is the date of birth of the child of family f , such that k is the time relative to birth. Thus β_k , captures the difference between the outcomes of families with microcephaly and the other families. We control for pair fixed effects, $\alpha_p(f)$, to ensure we are comparing each treated family with the most similar control families. We also add for year-month fixed effects, δ_t , to capture to any time-trend common to all families. We normalize the coefficients relatively to nine months before the childbirth. ε_{ft} is the random error, clustered at the match-group level.

Our identification assumption is that, conditional on having a child around the same time, in the same municipality, and mothers's age and educational level, the incidence of microcephaly is uncorrelated with unobserved characteristics that affect the outcomes of interest. As discussed in details in Section 2.1, the characteristics of the outbreak rules out several threats to identification, making it plausible that unobserved characteristics, such as mothers' behaviors, are not correlated to the chance of having a child with microcephaly.

Selective fertility as a response to the outbreak could have important implications for our estimates. However, the delay with which the zika virus infection causes microcephaly means that, in practice, this channel is unlikely to affect our results. Because the infection is most dangerous in the first months of pregnancy, and has mild symptoms otherwise, it went practically undetected until after the first babies were diagnosed with microcephaly. Furthermore, any selective fertility response that followed the widespread recognition of the seriousness of the outbreak would only impact births with 9 months of delay, resulting in births in a period when cases were already far past the peak.¹

2.4 Results

In this session we present our estimates of the effects of child disability in the family. We find a decrease on mothers' labor supply and earnings corresponding to half the motherhood penalty, or about 15% relative to 9 months before childbirth and no effects for fathers. In terms of fertility response, parents of disabled children are less likely to have another child in the future. Parents of healthy children in areas with a higher prevalence of microcephaly cases also reduce their fertility compared to those in areas with lower prevalence.

2.4.1 Balance and Summary

Table 2.1 shows summary stats for affected mothers and for controls. Overall, our control group seem to be similar to the treatment group along observable characteristics. We do not reject the hypothesis of equality between the samples for all variables at the usual significance levels, and no difference is economically significant.

In our sample, the mean mother's age at first birth is 26.36 for mothers of children with microcephaly and 25.64 for control mothers. This is very similar to estimates of age

¹One exception is late-stage abortion, which could have a faster effect on births. Abortion is illegal in Brazil, except in case of risk to the mother's life, pregnancy resulting from rape, or fetal anencephaly.

at first birth for the country in general, suggesting no strong selection along this margin. In terms of its racial composition, our sample is considerably less white than average Brazilians (roughly 45%), reflecting the regions most affected. The large majority of the sample self-declare as *pardo*. Around 60% of the sample has at least some high school, with most of the others having at least middle school. Overall, the differences between control and treated in characteristics are minimal and not statistically significant.

2.4.2 Employment and Earnings

We find that after the birth of child with microcephaly, mothers' formal employment falls by an additional 3.2 percentage points (15%), in addition to the fall of 5.1 percentage points (27%) associated with childbirth in general. The impact on earnings follows a similar path. When we restrict the sample to mothers with previous work experience in the formal sector, we find similar patterns relative to the share of employed mothers at baseline. We do not find any impact on fathers' employment or earnings.

Figure 2.4 shows average labor force participation of mothers' around the time of childbirth for the treated and control groups. Even though this variable is not used for matching and there the estimates are not covariate-adjusted, we see virtually identical rates of employment month-by-month before childbirth, with, if anything, a very slight difference in favor the affected mothers. After month 6, corresponding to the end of typical maternity leave, we see that mothers of children with microcephaly see a fall in employment roughly 50% larger than that of the controls, and the difference is entirely persistent. After 36 months, we see a difference of about 6 p.p., with only about 2% of mothers of children with microcephaly formally employed. The effects on formal earnings mirror closely those of employment. Note that the peak in earnings at about 4 months after childbirth likely corresponds to extra payments relative to job termination (e.g. vacations due).

Figure 2.5 shows the results focusing on the sample of mothers who had previous

experience in the formal labor market. This difference in experience may make these mothers more attached to the labor force and may indicate higher human capital, which could help deal with the health shock. We find that formal employment at the time of childbirth is more than double the sample average. However, we see a very similar pattern in both employment and earnings. By the end of our sample window, employment for control mothers is 23%, compared to only 5% for mothers of children with microcephaly.

Table 2.2 shows the results with a DID specification. The estimates for the Treated coefficient confirm the result that the pre-existing differences are small in magnitude and not statistically significant. Further, we can directly compare the average effect of microcephaly after childbirth with the raw motherhood penalty in each specification. We find that the additional penalty corresponds to about 60% of the motherhood penalty, both in employment and earnings for the full sample, and about 40% for the sample with previous work experience.

We repeat the same analysis for fathers, finding no effects for formal employment or earnings. Figure 2.6 shows the results. Note that we maintain the same match-groups as the ones in the previous analysis, matched by mother characteristics. Therefore fathers do not necessarily have the same level of education and age by construction, as mothers do. Nevertheless, we find employment and earnings are remarkably similar, both in levels and in trends before childbirth. Table 2.3 shows results of the DID specification. The estimates indicate a null effect on employment and a positive but not statistically significant effect on earnings. Notably, employments and earnings tend to increase after childbirth, although this effect is also not significant after accounting for match-group fixed effects. This may suggest specialization in the household, with negative effects on labor market participation for women and positive for men. However, since we only observe the labor market outcomes of cohabiting fathers, so a strict causal interpretation of the parameter requires strong assumptions.

2.4.3 Fertility

One potential response to the demands of caring for a disabled child is that families may choose to avoid having more children, depressing subsequent fertility. Not only is this an important effect on its own right, it also informs the interpretation of the effects we found on the labor market. Since fertility tends to depress labor market participation, this causal channel will tend to make differences in participation smaller. We show that child microcephaly seems to have only a very small impact in future fertility compared to paired controls, and mostly not statistically significant.

Our measure of subsequent fertility comes from the Single Registry in 2019. Therefore, the affected child will be between 4 and 2 when the data is collected. We find the same family and the same mother and count the number of children born after the child with microcephaly or their matched control. In about half the cases, the reference child was the firstborn, and overall fertility over this interval is low over this time span, making detection of any possible effects challenging.

We estimate regressions of the form:

$$fertility_i = \beta \cdot T_i + \alpha_{p(f)} + u_i \quad (2.2)$$

where $fertility_i$ indicates the number of additional children by mother i . T_i is a dummy indicating whether mother i had a child microcephaly. We control for pair fixed-effect, $\alpha_{p(f)}$ to ensure we are comparing each treated family with the most comparable control families.

Table 2.4 shows that, accounting for the fixed effects, mothers with a child with microcephaly had 0.005 fewer children until 2019 compared to controls. If we restrict the sample to families with only one child at the initial period, the effect on fertility is of 0.022 (p value: 9.3%), as shown in Column (3). There is no effect on fertility for families

that already had more than one child, (column (4)). This is to be expected, since fertility above 2 children is relatively uncommon, so there is not the possibility of further reducing it much more.

2.4.4 Family structure

Child disability creates severe stress in the household, and one of the possible medium-term effects is divorce or separation of the parents. Following the zika epidemic, there were several news stories about divorce in households where in families with a child with microcephaly, providing anecdotal evidence that this may be an important dimension.

To test for this hypothesis, we try to identify the child's father in the Single Registry with the same family in 2017 and 2019. We estimate the Equation 2.2, with the outcome variable being an indicator of the presence of the father in the household. Overall rates of cohabitation in the population in the Single Registry are extremely low, on the order of 15% to 20%.

Table 2.5 shows the results. We find that, if anything, there is a slightly higher chance of the father being present in families with a child with disability, although the difference is small in magnitude and not significant once we adjust for match-group fixed effects and re-weight. The estimates are very similar for 2017 and 2019. In column (5), we attempt to see the effect in 2019 conditional on presence in 2017. We find that the father being present in a year is a strong predictor of being present afterwards, and adding this control renders the estimate of the effect of microcephaly equal to zero.

2.5 Conclusion

In this paper, we analyse how congenital microcephaly in a child affects the labor outcomes of the parents, subsequent fertility and family structure. We show that mothers' labor market participation falls by close to one half, an effect that does not seem to fade

over time. On the other hand, fathers' labor outcomes are not affected. We also find suggestive evidence that affected families have lower subsequent fertility and fathers are not more likely to divorce or leave the family.

We conduct our analysis in the context of the Zika virus epidemic. Unique features of the outbreak allow us to rule out or substantially reduce several concerns, such as endogeneity of maternal care and health behaviors and selective abortion or mortality. Our paper contributes to the literature studying the effects of this outbreak by highlighting the effects on families' labor market outcomes.

Overall, our results help quantify the enormous human costs associated with disease and disability, and highlight the disproportionate effect on women. A better understanding of the ways individuals and families deal with persistent health shocks and disabilities can be an important input in the design of public policy to address these issues.

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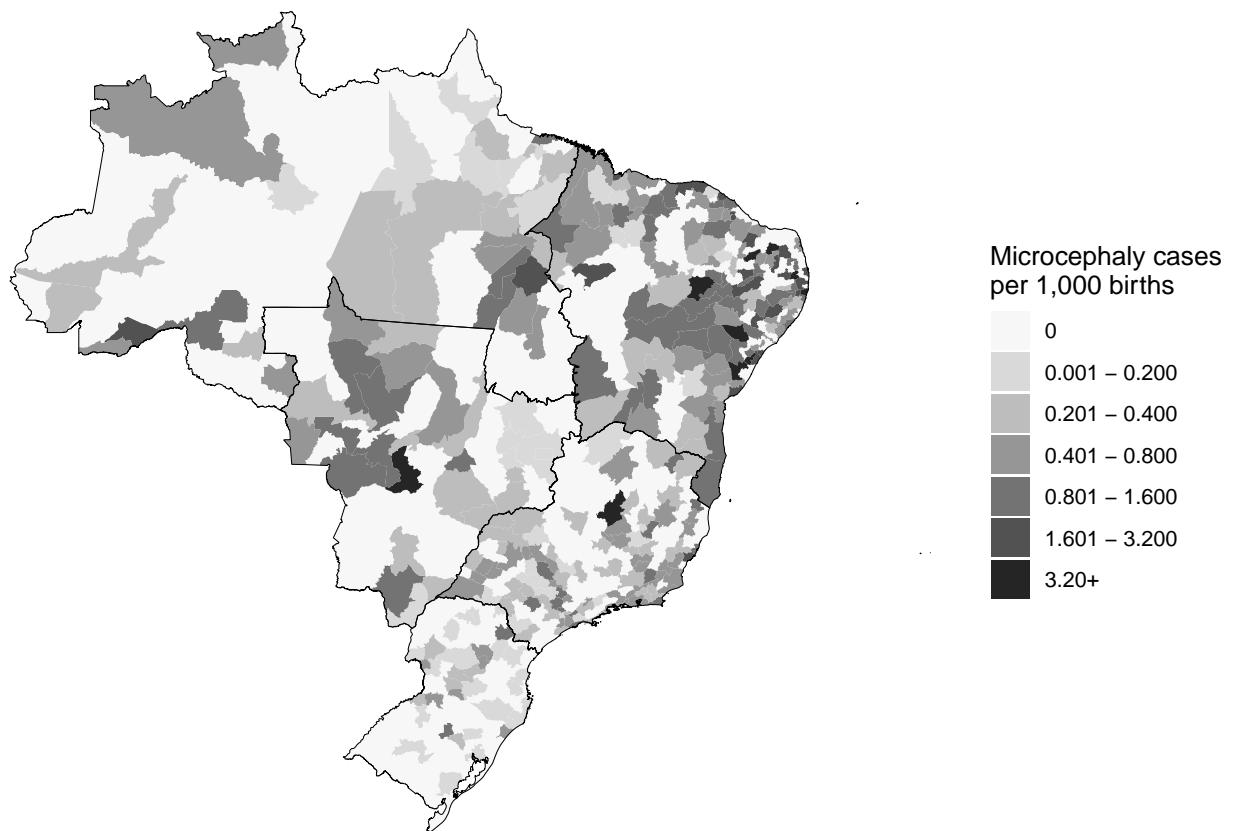
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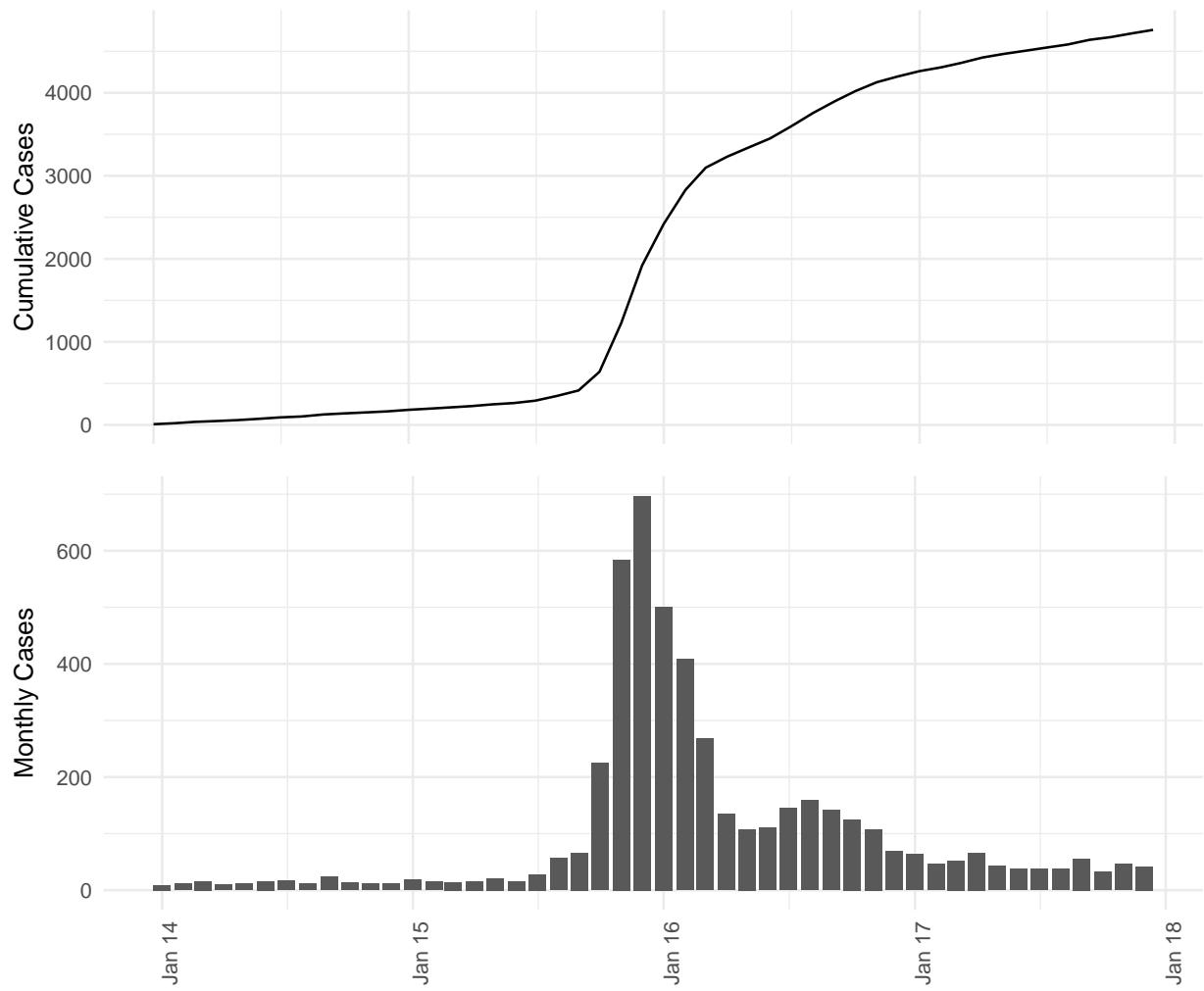
2.6 Figures

Figure 2.1: Geographic Variation on the Number of Microcephaly cases per 1000 Births



Notes: This figure illustrates the geographic variation on the number of microcephaly cases per thousand births in 2015 and 2016. Each polygon is a micro-region, comprising on average about 10 municipalities. Micro-regions with zero births in the period are assigned to the zero cases per 1,000 births category. The total number of births and cases of microcephaly is available from SINASC/SUS.

Figure 2.2: Microcephaly Cases by Month



Notes: These figures show the evolution in the total number of cases of microcephaly, over the Northeast and Southwest regions. The top graph shows cumulative cases, while the bottom shows monthly incidence. The data is from SINASC/SUS.

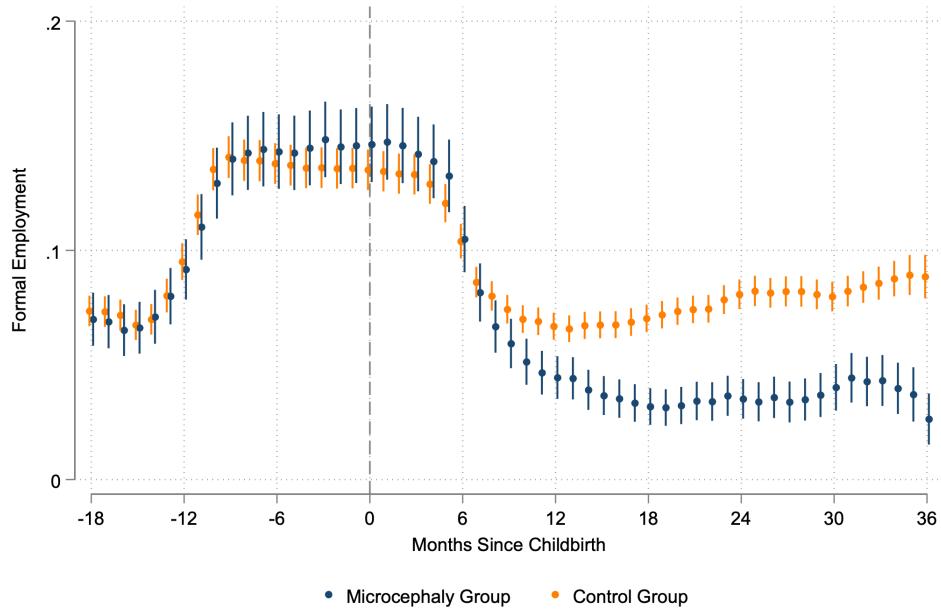
Figure 2.3: Mortality Rates of Children with Microcephaly



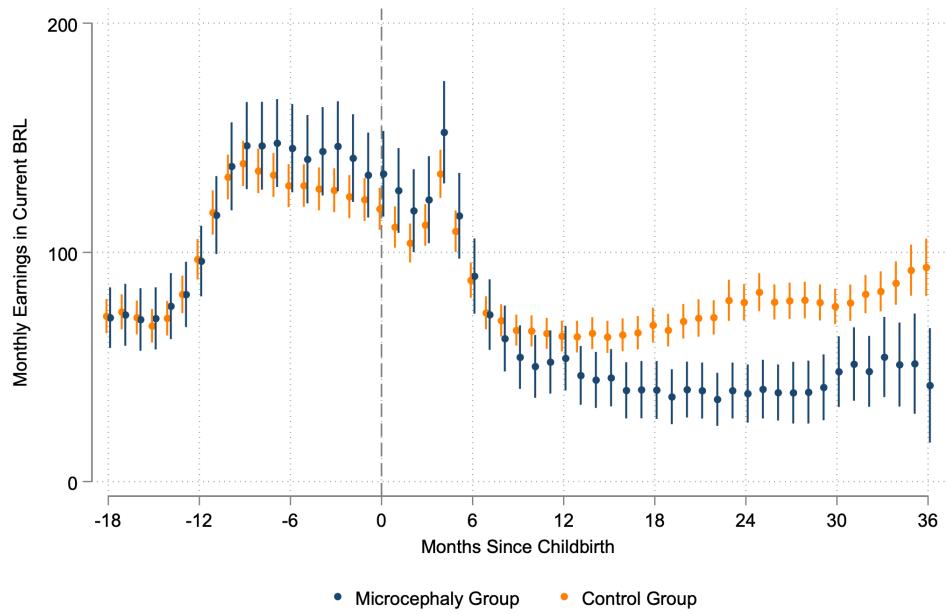
Notes: This figure shows mortality by age 5 per thousand births, separately for children born with microcephaly compared to others. The year indicates year of birth, not death. The total number of births and cases of microcephaly are made available by SINASC/SUS. Microcephaly is identified by the ICD-10 code Q02. Infant mortality is made available by SIM/SUS.

Figure 2.4: Mothers of Children Affected by Microcephaly and Matched Controls

Employment

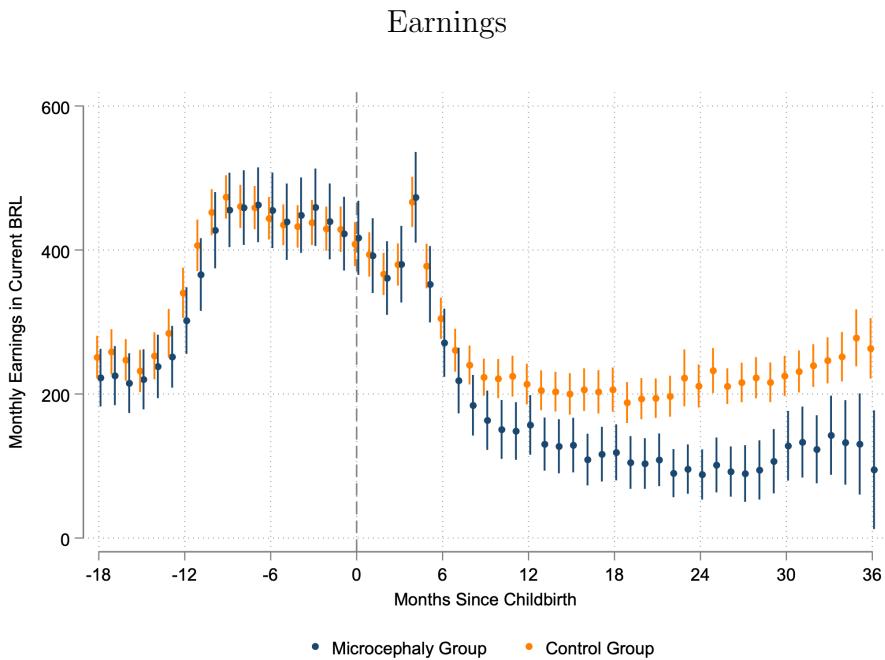
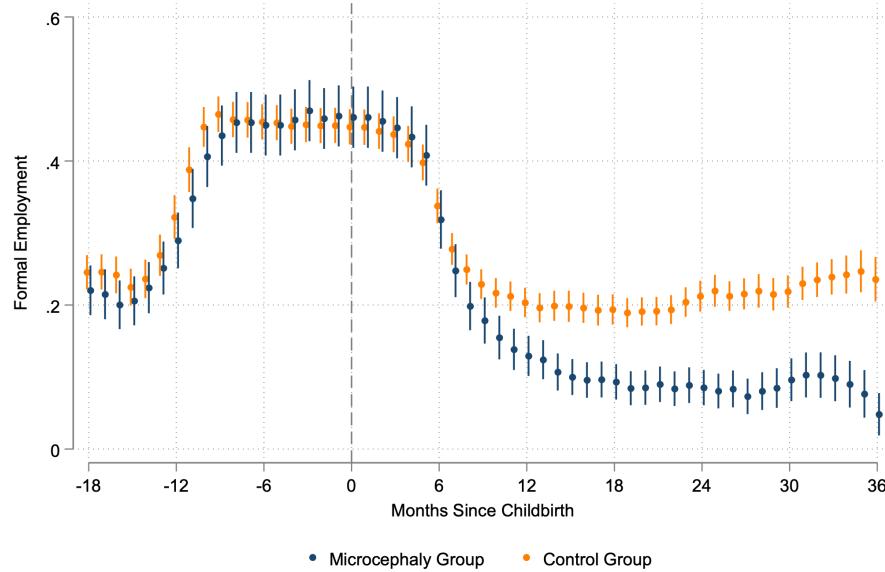


Earnings



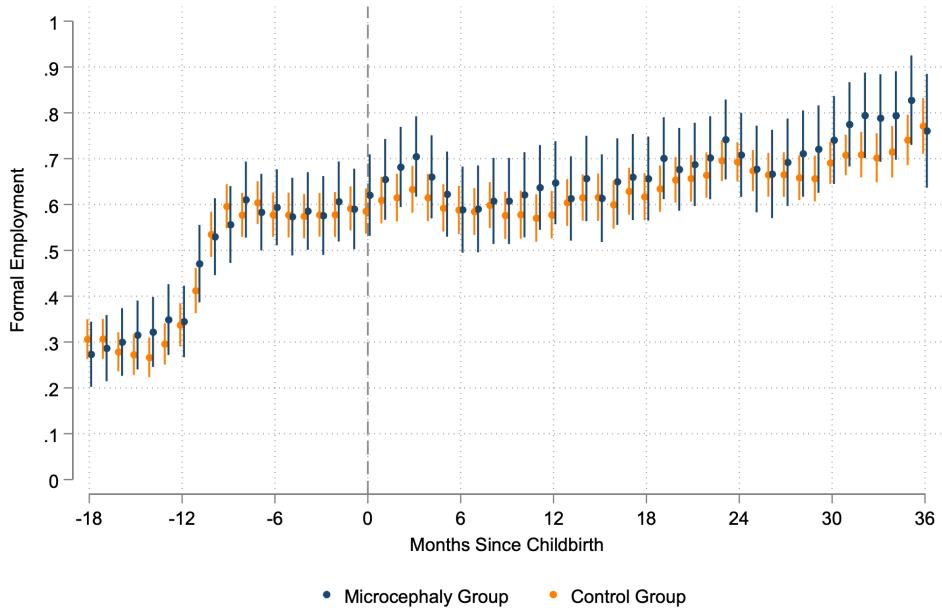
Notes: This figure shows the employment rate (above) and earnings (below) of mothers in the formal sector. The Microcephaly Group consists of mothers of children diagnosed with microcephaly, while the Control Group consists of mothers of children without this condition, matched in location, age and time of childbirth. Vertical dashed lines at 0 and 6 months indicate the month of childbirth and the typical end of maternity leave, respectively. Earnings are in BRL, and the error bars represent 95% confidence intervals.

Figure 2.5: Subsample with Previous Formal Employment
Employment

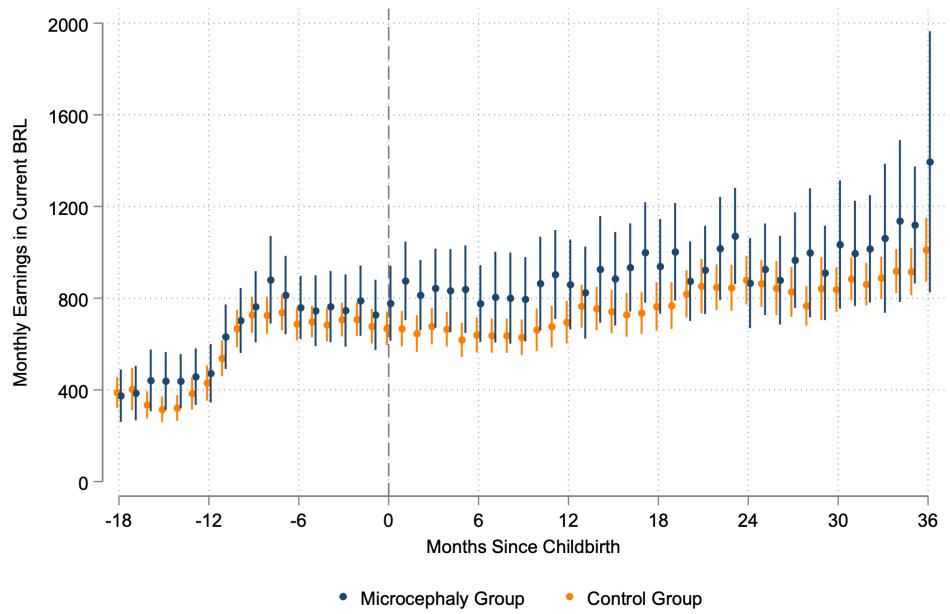


Notes: This figure shows the employment rate (above) and earnings (below) of mothers in the formal sector. This subsample is selected such that every mother had at least one month in the private sector in the two years before childbirth. The Microcephaly Group consists of mothers of children diagnosed with microcephaly, while the Control Group consists of mothers of children without this condition, matched in location, age and time of childbirth. Vertical dashed lines at 0 and 6 months indicate the month of childbirth and the typical end of maternity leave, respectively. Earnings are in BRL, and the error bars represent 95% confidence intervals.

Figure 2.6: Effects on Fathers
Employment



Earnings



Notes: This figure shows the employment rate (above) and earnings (below) of fathers in the formal sector. The Microcephaly Group consists of mothers of children diagnosed with microcephaly, while the Control Group consists of mothers of children without this condition, matched in location, age and time of childbirth. Vertical dashed lines at 0 and 6 months indicate the month of childbirth and the typical end of maternity leave, respectively. Earnings are in BRL, and the error bars represent 95% confidence intervals.

Table 2.1: Summary Statistics

	Treated	Control	p-value
Age	26.36	25.64	.767
Standard Deviation	(6.20)	(5.42)	
Race			
Indigenous	.005	.002	.933
White	.196	.222	.743
Black	.100	.105	.788
Asian	.013	.01	.657
Pardo	.693	.663	.831
Education			
Less than High School	.401	.296	.669
High School or more	.533	.644	.405
N	1,887	35,202	

Notes: This table shows means and standard deviations for the treated and control samples along demographic variables. The treated sample consists of mothers of children with microcephaly, and the control sample consists of matched mothers. The p-value is calculated based on a regression including match-group fixed effects.

Table 2.2: Effect of Microcephaly on Mothers' Labor Supply

Full Sample				
	Works		Earnings	
	(1)	(2)	(3)	(4)
Treated	.0065 (.008)	.0065 (.008)	14 (9.1)	14 (9.1)
Post	-.051*** (.0028)	-.049*** (.0028)	-49*** (3.1)	-46*** (3.1)
Treated \times Post	-.032*** (.0061)	-.032*** (.0061)	-32*** (7.1)	-32*** (7.1)
Number of Obs	1563559	1563559	1563559	1563559
Number of Clusters	1728	1728	1728	1728
Match FE	No	Yes	No	Yes
Mean Dep. Var. Baseline	0.14	0.14	142.69	142.69
Work Experience Sample				
	Works		Earnings	
	(1)	(2)	(3)	(4)
Treated	.00071 (.022)	.014 (.023)	4.5 (27)	29 (28)
Post	-.2*** (.0083)	-.2*** (.0085)	-190*** (9.9)	-186*** (11)
Treated \times Post	-.081*** (.018)	-.081*** (.018)	-80*** (22)	-80*** (22)
Number of Obs	356108	356108	356108	356108
Number of Clusters	507	507	507	507
Match FE	No	Yes	No	Yes
Mean Dep. Var. Baseline	0.45	0.45	462.32	462.32

Notes: This table shows the effect of having a child with microcephaly on mothers' employment. In the top panel, we show results for the full sample, while the bottom panel shows results for the sample of mothers that worked at least one month of the 36 months before birth. In Columns (1) and (3), there are no additional controls. In Columns (2) and (4), we add match-group fixed-effects. Control observations are weighted by the inverse of the number of controls in the match-group. Standard errors are clustered at the level of the match-group.

Table 2.3: Effect of Microcephaly on Fathers' Labor Supply

	Works		Earnings	
	(1)	(2)	(3)	(4)
Treated	.0023 (.038)	-.08 (.052)	71 (71)	-154 (103)
Post	.052*** (.019)	.027 (.018)	51 (34)	31 (34)
Treated \times Post	.037 (.04)	.0076 (.041)	87 (69)	52 (65)
Number of Obs	88621	88618	88621	88618
Number of Clusters	833	830	833	830
Match FE	No	Yes	No	Yes
Mean Dep. Var. Baseline	0.58	0.58	745.66	745.66

Notes: This table shows the effect of having a child with microcephaly on mothers' employment. In the top panel, we show results for the full sample, while the bottom panel shows results for the sample of mothers that worked at least one month of the 36 months before birth. In Columns (1) and (3), there are no additional controls. In Columns (2) and (4), we add match-group fixed-effects. Control observations are weighted by the inverse of the number of controls in the match-group. Standard errors are clustered at the level of the match-group.

Table 2.4: Effect on Subsequent Fertility

	Total Children After Treated/Control Child			
	(1)	(2)	(3)	(4)
Microcephaly	.000087 (.0081)	-.005 (.0087)	-.022* (.013)	.0076 (.014)
Constant	.13*** (.003)	.13*** (.0044)	.15*** (.0069)	.13*** (.0065)
Number of Obs	36856	36457	17093	18970
Number of Clusters	1729	1717	1289	1457
Match FE	No	Yes	Yes	Yes
Sample	Full	Full	Firstborn	Not firstborn

Notes: This table shows the total fertility up to three years after the birth of the child with microcephaly. Columns (1) and (2) include all families. We split the sample among families where the child with microcephaly or matched control was the first child (column (3)) and those where it was not(column (4)).

Table 2.5: Family Structure

	Father Present in 2017		Father Present in 2019		
	(1)	(2)	(3)	(4)	(5)
Microcephaly	.049*** (.0094)	.013 (.0098)	.043*** (.0088)	.0085 (.0093)	-.0028 (.0039)
Present in 2017					.86*** (.0095)
Constant	.15*** (.004)	.19*** (.0049)	.13*** (.0035)	.16*** (.0046)	.0023 (.0033)
Number of Obs	37089	37089	37089	37089	37089
Clusters	1728	1728	1728	1728	1728
Match FE	No	Yes	No	Yes	Yes
Weights	Uniform	Balancing	Uniform	Balancing	Balancing

Notes: This table shows the effect of having a child with microcephaly on the likelihood of cohabiting fathers. Columns 1 and 2 show effects in 2017 and columns 3, 4 and 5 show effects in 2019. Columns 1 and 3 are simple differences, while 2, 4 and 5 have fixed effects and reweighting.

CHAPTER 3

Optimizing Incentives for Rooftop Solar: Accounting for Regional Differences in Marginal Emissions

The Inflation Reduction Act of 2022 (IRA), a groundbreaking piece of legislation addressing climate change in the US, includes major incentives for the adoption of residential photovoltaic solar generators (PV). The IRA accomplishes this mainly through extending the Investment Tax Credit (ITC), a 30% tax credit for PV installation. While the ITC does not discriminate across locations, the literature has pointed out that the reduction in greenhouse gasses (GHG) associated with adoption varies substantially across space. Differences in the carbon intensity of the regional energy mix explain much of this variation. For instance, Sexton et al. (2018) estimate that rearranging the sites of solar generators would generate an additional \$1 billion per year in environmental benefits. Such differences suggest large efficiency gains may be had by changing subsidy rates state-by-state based on marginal emission reductions.

The impact of clean energy technologies depends crucially on local characteristics, especially the resource mix of local energy generation. Based on estimates from the

Environmental Protection Agency (EPA), the same nominal solar capacity can have as much as twice the impact if installed in Nebraska versus New York. This marginal impact on emissions is uncorrelated with residential PV installations or existing installation incentives, suggesting no existing mechanisms to target installations along this margin. While I show this lack of correlation at the state level, Sexton et al. (2018) document the same at the zip-code level.

In this paper, I estimate the gains from the optimal state-by-state subsidy schedule relative to a uniform subsidy for a given level of spending. To do so, I first produce new estimates of the supply and demand elasticities of PV adoption. I combine detailed data on installations and prices from two sources to identify elasticities based on variation in state-level incentives. Using zip-code level data on installations, I can focus on bordering counties to compare similar locations across states with different policies, minimizing unobserved heterogeneity. I then take my estimates to a simple supply and demand model and find that implementing the optimal incentive schedule decreases emissions by an additional 61%.

To assess the potential efficiency gains of fiscal incentives directing adoption to higher-impact states, we need to estimate a model of the PV adoption market. Besides the marginal impact per state, the crucial parameters are the elasticities and levels of demand and supply. The higher the price sensitivity of demand, the easier it is for incentives to redirect adoption leading to larger gains. Because subsidies are paid to submarginal adopters, it is more expensive to subsidize states with larger demand at a given price.

In the first part of this paper, I estimate the short-run elasticities of supply and demand in the residential PV market. I estimate key empirical parameters with minimal structural assumptions, using variation across US states for identification, following the approach detailed in Zoutman et al. (2018) and Dearing (2022). Identification is based on differences in incentive policies between bordering states, restricting the sample to counties along the border to minimize unobserved heterogeneity. This approach extends

previous work on estimating reduced-form parameters in the PV market (Hughes and Podolefsky, 2015; Pless and van Benthem, 2019; Dong et al., 2018). While these papers often focus on the largest markets, especially California, I can incorporate data from several states and estimate supply and demand parameters. This reduced-form approach complements previous work focused on structural dynamic models of adoption (Williams et al., 2020; van Blommestein et al., 2018; Islam, 2014). While my results do not speak to the “deep parameters” of the PV adoption problem, they provide empirical facts that may help calibrate structural models.

I find evidence for a highly elastic supply curve while the elasticity of demand is well below one. My regression analysis shows that higher incentives are associated with (1) significantly higher PV installations, (2) significantly lower price net of incentives, and (3) no difference in gross prices. Together, (1) and (3) imply a large elasticity of supply and full pass-through of incentives, consistent with a highly competitive environment. Dong et al. (2018) and Pless and van Benthem (2019) find similar results in analyses of PV incentives in California. My estimated elasticity of demand is 12%. While an extensive literature studies the relationship between incentives and adoption or prices, this is, to the best of my knowledge, the first paper to produce estimates of supply and demand elasticity for a large share of the American PV market.

In the second part of the paper, I take these elasticity estimates to a simple supply and demand model over states and find that targeting incentives improves outcomes by 61%. Assuming states have identical constant elasticities but different supply and demand levels, the model simulates the spending of \$1 billion in addition to existing incentives. I compare the scenario implementing a fixed incentive capacity unit against state-specific incentives. Supply and demand parameters, as well as existing incentives, are calibrated from 2021 data. Optimal state-specific incentives are highly concentrated in a few states, with Arizona responsible for a large share of the efficiency generated.

This paper contributes to the growing literature on encouraging environmental tech-

nologies with geographically varying benefits. Tibebu et al. (2021) derive optimal subsidies in the context of an explicit dynamic adoption model with technological progress. Holland et al. (2016) study this problem in the context of electric vehicle purchases.

3.1 Background

Residential PV generators are one technology that stands to grow even faster due to the incentives in the IRA, helping the US transition to clean energy. The Inflation Reduction Act is the most significant piece of legislation ever passed dealing with climate change, amounting to \$390 billion of spending in this area. Among many other stipulations, it includes \$128 billion for renewable energy, including \$9 billion for home energy improvement programs. It also extends for ten years the consumer tax credits under the ITC for direct ownership of residential PV generators.

However, there is substantial heterogeneity in the effect of solar installed in different states on emissions. Figure 3.2 shows the impact of adding 1MW of distributed nominal capacity in each state, estimated with the EPA's AVERT model (EPA, 2023), in tons of CO₂ per year. The effects range from as low as 800 tons parts of New England to as high as 1600 tons in the central plains region. These differences are not mainly due to the physical potential for solar generation but to the emission intensity of the marginal alternative energy source.

This pattern suggests potentially significant gains from directing PV installations toward high-impact areas, but actual residential installations have, if anything, gone in the opposite direction. Figure 3.1 shows the relationship between the marginal emission reduction in the horizontal axis and the log of cumulative installed capacity in 2021 in the vertical axis. In the largest solar market, California, 1 MW of solar capacity reduces emissions by less than 1000 tons, putting it in the bottom fifth in marginal impact. On the other hand, the adoption of residential solar has been very modest in the Midwest

and Central Plains areas.

To assess the potential efficiency gains of fiscal incentives directing adoption to higher-impact states, we need to estimate a model of the PV adoption market. Besides the marginal impact per state, the crucial parameters are the elasticities and levels of demand and supply. The higher the price sensitivity of demand, the easier it is for incentives to redirect adoption, leading to larger gains. Because subsidies also benefit submarginal adopters, it is more expensive to subsidize states with higher demand at a given price.

I rely on three primary data sources to identify supply and demand parameters and compute counterfactual emissions, complemented by several others. Data on emissions is from the EPA's AVERT model. Berkeley Lab's Tracking the Sun report is the main source for PV installations and prices, complemented by EnergySage's price data. The NC Clean Energy Technology Center's DSIRE database compiles information on federal and state-level incentives.

3.1.1 Emissions

To estimate the marginal reduction in emissions caused by PV installations, I use the EPA's AVERT model (EPA, 2023). EPA created the model explicitly to evaluate the emission impacts of energy policies such as PV installations. It takes EPA's data on energy load and emissions in every fossil fuel plant over a year as inputs and estimates solar energy output at a given site. From this, the model outputs the predicted reduction in CO₂ emissions resulting.

The first step in the estimation is modeling the relationship between fossil fuel energy load and emissions. The total hourly grid load on fossil fuels over the year is sorted in ordered bins. Then, for each fossil fuel power plant, AVERT computes a) the probability it is operational and b) a probability distribution of its power output as a function of grid load. It also estimates the distribution of emissions in each plant given its power output.

The next step is to predict the hourly generation from a given solar installation and subtract it from the grid load using the National Renewable Energy Laboratory’s PVWatts model (Dobos, 2014). This tool takes as input the nominal capacity of a rooftop solar generator and specific geographical coordinates for its location and estimates expected hourly generation across a year. It considers factors such as solar irradiance, weather variability, and efficiency losses. The generators are assumed to be placed in several cities, representing the largest load centers for each state.

Finally, using each plant’s generation and emissions distributions, AVERT simulates the expected emissions given the lower grid load. Figure 1 shows the estimated effects of installing 1 MW nominal capacity at each state. Differences between states are largely driven by the intensity of the use of coal versus gas within the fossil fuel category. The share of fossil fuels out of total power generation is comparatively unimportant.

Two limitations of this method are particularly relevant for this study. First, the analysis takes the generation profile of a given year as given. Changes to fossil fuel prices, plant openings or closings, or transmission changes could meaningfully affect results in ways the model does not consider. Any possible endogenous price responses, as well as the evolution of these characteristics over time, will impact these results in ways that are difficult to predict.

Second, AVERT models each state separately and assumes power imports and exports to other regions remain constant. These energy flows are substantial in practice; for instance, California imports around 25% of its electricity, making it the largest gross importer. Meanwhile, the Midwest and Central areas are net energy exporters and relatively more carbon-intensive than average. These patterns may be of particular concern, given we are studying the allocation of PV between states. If adjustments to the flows between balancing authority areas are a relevant margin of adjustment to additional solar energy, our estimates could be biased.

3.1.2 PV Installations

I use Berkeley Lab as the primary source for residential PV installations (Barbose et al., 2022). This dataset, compiled in collaboration with state governments and utilities, provides data on individual installations. It includes zip-code-level location, installation date, price, capacity installed, installer identity, and several system characteristics. The dataset covers 30 US states, with all large solar markets represented.

Berkeley Lab data is crucial because it includes geographic information at a level finer than the state. This information allows us to compare prices and quantities in bordering counties, minimizing unobserved heterogeneity. We complement this information with demographic data from the American Community Survey, including the number of housing units, mean house value, and median household income by zip code.

This dataset has two important limitations. The first is that coverage is not perfect, and different states may have different misreporting rates. We deal with this problem by comparing total installations to state-level data from the Solar Energy Industry Association (SEIA) and checking the sensitivity of results to different adjustments.

The second is that price data are unavailable for every included state. Since price information is crucial, I supplement Berkeley Lab’s data with proprietary data from Energy Sage. Energy Sage is a web-based platform that catalogs residential PV installers and recommends them to consumers based on their location, preferences, and other characteristics. I observe a random sample of searches and use prices of winning offers where price data is missing from Berkeley Lab.

For the simulation of results covering all the US states, I use data from 2021 by Wood—Mackenzie and SEIA (Mackenzie and SEIA, 2021). This dataset comprises information on residential PV installations at the state level across the entire country. It is based on proprietary industry information. Since it does not have the same coverage issues as the Berkeley Lab data, I consider it the “ground truth” in this paper.

3.1.3 Incentives

For my main instrument, I use the Database of State Incentives for Renewables and Efficiency (DSIRE) as a source for federal- and state-level incentives for PV adoption (Cummings, 2009). I observe tax credits, rebate programs, and other types of incentives from 2018 to 2021. Local and utility-level incentives are not included in the analysis. For incentives that only went into effect after September of a given year, I only include them in the analysis as affecting the next year.

Because some types of incentives are difficult to quantify, I focus on a) tax credits, b) direct rebates, and c) tax exemptions (mainly sales tax). These categories include the most important programs, particularly the federal ITC. I also include an estimate of the value of programs that give a rebate or tax credit depending on the assessed or actual production over a specific time horizon. Among the excluded incentives are property tax exemptions and carbon credit appropriations.

3.2 Model

I present a stylized model of the market for PV installations. The first part of this paper is concerned with estimating the price elasticities in the model from US data. The second part uses the estimates and the model to study the effects of counterfactual policy experiments changing the subsidy rate.

Let's consider a standard supply and demand system at each state, with constant elasticities. Denoting quantities demanded and supplied at location j , year t , respectively Q_{jt}^d and Q_{jt}^s ; prices p_{jt} , subsidies τ_{jt} , and the number of housing units N_{jt} that do not already have a PV system.

$$Q_{jt}^s = N_{jt} \exp(\gamma X_{jt}) (p_{jt})^\delta u_{jt} \quad (\text{Supply})$$

$$Q_{jt}^d = N_{jt} \exp(\alpha X_{jt})(p_{jt} - \tau_{jt})^\beta \epsilon_{jt} \quad (\text{Demand})$$

With u_{jt} and ϵ_{jt} as error terms. Taking logs and with $q_{jt} := Q_{jt}/N_{jt}$:

$$\ln q_{jt}^s = \delta \ln p_{jt} + \gamma X_{jt} + u_{jt} \quad (\text{Supply (log)})$$

$$\ln q_{jt}^d = \beta \ln(p_{jt} - \tau_{jt}) + \alpha X_{jt} + \epsilon_{jt} \quad (\text{Demand (log)})$$

The equilibrium condition:

$$Q_{jt}^d = Q_{jt}^s \quad (\text{E.C.})$$

I denote $Q^*(\tau)$ the quantity that solves the system as an implicit function of subsidies.

In the next sessions, I first deal with the question of identifying and estimating elasticities β and γ . Then, I use the model and the elasticity estimates to study the effects of variable subsidies τ .

3.3 Estimating Elasticities

I estimate supply and demand parameters relying on changes in state incentives for PV adoption, following the approach outlined in Zoutman et al. (2018) and Dearing (2022). In order to compare areas as closely comparable as possible, I focus on bordering counties between states with different incentive rates. By estimating the effect of incentives on prices to producers and to consumers, I am able to identify both supply and demand elasticities. Results suggest supply is very highly elastic, while demand elasticity is only around 12%. Results are imprecisely estimated, but are corroborated by alternative

methods.

3.3.1 Estimation

Zoutman et al. (2018) provides the theoretical framework for identifying both supply and demand elasticities using as instrument only the variation in a single tax rate. In our case, we can identify β and δ using the variation in state-level incentives as instruments. The key intuition for this result is that the supply and demand model provides theoretical restrictions that we can leverage to identify the relevant parameters.

The first assumption we need corresponds to the usual exclusion restriction. The require changes in subsidy rates to be uncorrelated with unobserved determinants of adoption. This assumption would fail, for example, if states where the population is becoming more worried about the environment are more likely to increase subsidies and also see increased demand for PV adoption.

The second assumption is what Zoutman et al. (2018) terms the Ramsey Exclusion Restriction, which stipulates that changes in subsidy rates can only affect demand and supply through their effects on prices. This assumption would fail if, for instance, higher subsidies increase the salience of solar energy and have an outsized effect in demand beyond the one through price. Alternatively, because incentive programs are often implemented as tax rebates, they may be themselves be less salient, as Chetty et al. (2009) find with sales taxes.

Under these assumptions, we can easily recover the supply and demand elasticities by instrumenting, respectively, the net price ($p_{jt} - \tau_{jt}$) or the full price p_{jt} by the subsidy rate. The identifying assumption is that τ_{jt} is uncorrelated to unobserved variation, condition on covariates: $E[\tau_{jt} u_{jt} | X_{jt}] = 0$, $E[\tau_{jt} \epsilon_{jt} | X_{jt}] = 0$.

To minimize the role of unobserved heterogeneity, I restrict the sample to bordering counties between two states. While market conditions may differ between contiguous states,

including physical conditions, i.e., solar irradiance, we can minimize these differences by focusing on the counties to either side of the border. This restriction focuses on areas with the identifying variation, assuming that the relevant unobserved heterogeneity changes continuously over space.

Therefore, my unit of observation is a county-by-border-by-year. I use only counties adjacent to a state border between two states if I have data for both during at least one year between 2018 and 2021. My measure of log quantity is the total capacity installed, divided by the number of housing units in the zip code, to make locations with different populations comparable. Similarly, prices are measured in dollars per kW capacity.

Two related issues exist when using fiscal incentives as instruments. First, most federal and state incentives have complicated, non-linear rules depending on prices and system sizes and usually include maximum values per household. Properly including these kinds of incentives in a regression framework is not straightforward. Second, demand for PV systems of different characteristics adjusts in response to the incentive design. For instance, smaller systems are relatively cheaper for consumers in states that include a lump-sum rebate for PV systems above a specific capacity.

I deal with these two issues using “simulated instruments” that apply the incentive rules of each state to a shared pool of installations. I start by taking each pair of states, say A and B, and pooling together all installations in a given year. Then, I compute the net price given system characteristics under the incentive scheme in state A for every installation in both A and B. The simulated incentive for state A is the average ratio between total price and net price (and correspondingly for B). Because the instruments for A and B are calculated using the same sample of installations, the differences are driven entirely by the incentive rules themselves, not any differences in composition. This type of approach has often been used to study the effects of different policy regimes in, e.g., taxation (Gruber and Saez, 2002), health (Cohodes et al., 2016), and labor (Cullen and Gruber, 2000).

To make the construction more explicit, let's call $I_{A,t,B}$ the collection of indices in the sample corresponding to installations in state A , year t . Each installation i has information on total price paid P_i and total capacity installed C_i . Let $f_{A,t}(P_i, C_i)$ be a function describing the total incentives due to an installation with total price P_i and capacity C_i according to the state laws in A during year t . Denote B the bordering state, remember the sample is restricted to counties along the border, and $n_{s,t}$ the total number of installations in the sample in state s , year t . Then, the instrument for incentives in A , bordering B , at year t is:

$$z_{A,t,B} = \frac{1}{n_{A,t} + n_{B,t}} \sum_{i \in I_{A,t} \cup I_{B,t}} f_{A,t}(P_i, C_i)$$

Therefore, our main estimating equations are the following.

$$\ln q_{j,t} = \eta_1 z_{s(j),t,s'(j)} + \eta_2 X_{j,t} + e_{j,t}^r \quad (\text{Reduced form})$$

$$\ln p_{j,t} = \theta_1 z_{s(j),t,s'(j)} + \theta_2 X_{j,t} + e_{j,t}^S \quad (\text{First stage: Supply})$$

$$\ln(p_{j,t} - \tau_{j,t}) = \phi_1 z_{s(j),t,s'(j)} + \phi_2 X_{j,t} + e_{j,t}^D \quad (\text{First stage: Demand})$$

$$\ln q_{j,t} = \delta p_{j,t} + \gamma X_{j,t} + u_{j,t} \quad (\text{IV: Supply})$$

$$\ln q_{j,t} = \beta(p_{j,t} - \tau_{j,t}) + \alpha X_{j,t} + \epsilon_{j,t} \quad (\text{IV: Demand})$$

With $X_{j,t}$ a vector of controls that includes an intercept, median household income,

average home values, and energy prices.

3.3.2 Results

Table 1 below summarizes the results. Column 1 shows that an extra thousand dollars in incentives is associated with an increase of 3.7% in capacity installed per capita. The same incentive increases prices by only 0.1%, with a wide confidence interval (Column 2). Net price, however, decreases strongly by 26% (Column 3). Although this effect has even larger errors, we reject the hypothesis that it equals zero.

Column 4 shows the IV estimates of the structural supply elasticity, that is, the effect of log price on log capacity installed per capita. Since the incentive instrument does not have an appreciable effect on price, that implies a highly elastic supply. A consequence of price insensitivity to the instrument is that the elasticity estimate is extremely noisy. The practical implication is that supply is close to the perfectly elastic case. Column 5 shows the estimates of the demand curve's elasticity. I find an elasticity of 12%, with the correct sign. Precision is low, with a 90% confidence interval covering from 23% to close to 0.

3.4 Optimal Incentives

To quantify the potential gains from target incentives, I apply the estimated elasticities to a simple supply and demand model calibrated to the 2022 PV market. I study the problem of maximizing the policy's emissions impact, given a budget constraint. My results indicate that, for a target spending of 1 billion dollars, the impact of state-specific incentives is about 60% larger than that of the uniform incentive.

3.4.1 Model

I study the problem of minimizing emissions, given an incentive budget constraint. The planner has a budget B and uses it to implement adoption incentives. In the first

case we study, this budget only finances extra incentives on top of existing ones, which I take as given. This framework represents the problem of enacting a new subsidy given existing policies. Write the total incentive in state j , τ_j as the sum of the already existing incentives $\bar{\tau}_j$, that are taken as given, and new incentives τ_j^* .

$$\tau_j = \bar{\tau}_j + \tau_j^*$$

Denote e_j the marginal emissions associated with one PV installation in state j . Then the problem is:

$$\begin{aligned} & \min_{\tau_j^*} \sum_J e_j Q_j^*(\tau_j) \\ & s.t. \sum_J \tau_j^* Q_j^*(\tau_j) \leq B, \\ & \quad \forall \tau_j^* : \tau_j^* \geq 0 \end{aligned}$$

Our key interest lies in comparing the objective function the planner can reach with flexible incentives compared to uniform incentives. The uniform incentives case is represented simply as the additional restriction $\tau_j^* = \tau^*$.

I take the model to US data from 2022, using installation data from SEIA and Wood and Mackenzie and price data from Energy Sage. I dropped two states from the analysis, Alabama and Tennessee because they have zero residential PV installations in the year. Thus, our model implies that no amount of incentives will induce demand. For another seven states, we do not have price data (KS, MS, MT, NB, ND, SD, WY). In these cases, we impute the average price across all other states. Because these are all small markets for PV, the sensitivity of results to this imputation is small.

3.4.2 Results

My main result is that, in the marginal expenditure exercise, targeting by state induces a 61.5% larger reduction in CO₂ emissions compared with a uniform incentive spending the same amount. The distribution of this incentive is very concentrated, with large discounts for installations in Oklahoma and three Southwestern states, with close to zero allocated to northern states. Each taken alone, marginal emissions, population size, or the scale of existing demand cannot fully explain the resulting distributions.

In the baseline exercise, I model the expenditure of \$1 billion in a flat incentive per installed unit of capacity. At this level of extra spending, the flat subsidy offered is \$0.244 per W, or about 8.8% of the average price before incentives. This level of incentives implies a reduction of 50.38 million tons of CO₂ emitted per year, on top of the business-as-usual estimated effect of 4.6 billion tons of CO₂.

Figure 3.3 shows the estimated optimal additional incentives. Four states stand out very clearly: Oklahoma (1.29), Arizona (1.08), Nevada (0.96), and Utah (0.86) have the largest incentives. Florida, New Jersey and South Carolina also have slight increases relative to the uniform incentive. Seven other states have lower levels that are still \$0.11 per W, while the others have rates close to zero. Figure 3.4 shows the optimal additional incentives added to existing federal and state incentives. While existing incentives are negatively correlated with marginal impact, the total incentives after adding this spending are not.

The emissions impact of the optimal subsidy schedule is 81.35 million tons of CO₂ per year, a 61% increase relative to the uniform subsidy case. Arizona is responsible for a large part of the efficiency gains, as the model predicts the increased subsidies will lead to an extra 31 million tons of CO₂.

Figure 3.5 shows how the estimated optimal incentive depends on four key variables: marginal emissions, existing incentives, the number of housing units in the state, and

the scale parameter of demand B_j . The four states with high optimal incentives have relatively high marginal emissions and demand and relatively low current incentives and number of units.

3.5 Conclusion

This paper highlights the potential for optimizing the state-by-state subsidy schedule for residential PV under the Inflation Reduction Act of 2022 (IRA). By leveraging new estimates of supply and demand elasticities of PV adoption, I demonstrate that implementing an optimal incentive schedule can lead to a 61% larger reduction in emissions than a uniform subsidy approach. This finding underscores the importance of tailoring incentives to the specific characteristics of each state's energy landscape, as indicated by the substantial variation in greenhouse gas reduction benefits across different locations.

This paper also reinforces the potential value of improving data reporting standards to help address practical challenges in environmental policy. The need for standardized, representative data on the PV market severely limits what we can learn about how to improve government policy in this area. Considering the amount of public investment, creating and distributing better data may be a public good with significant social returns.

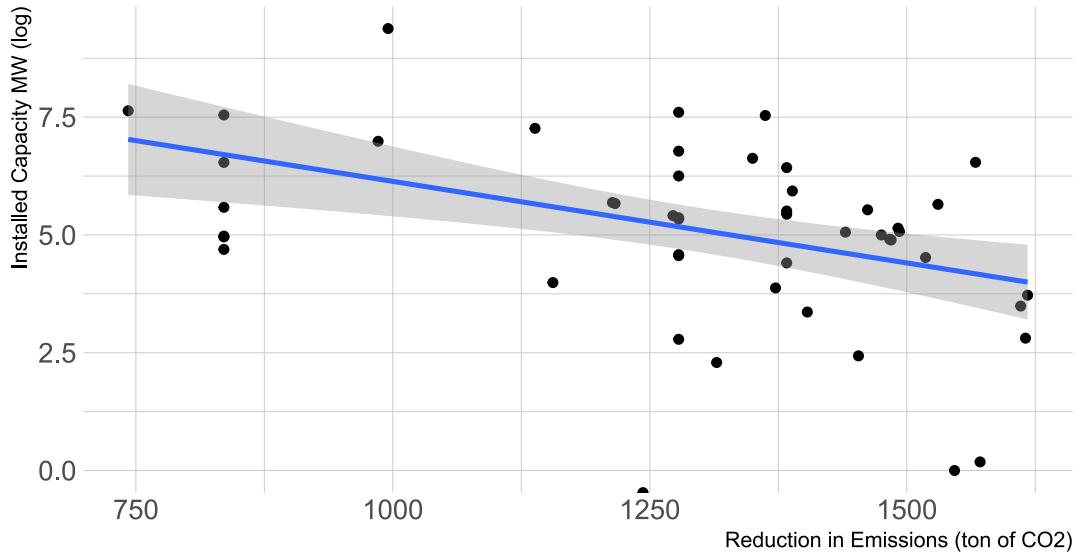
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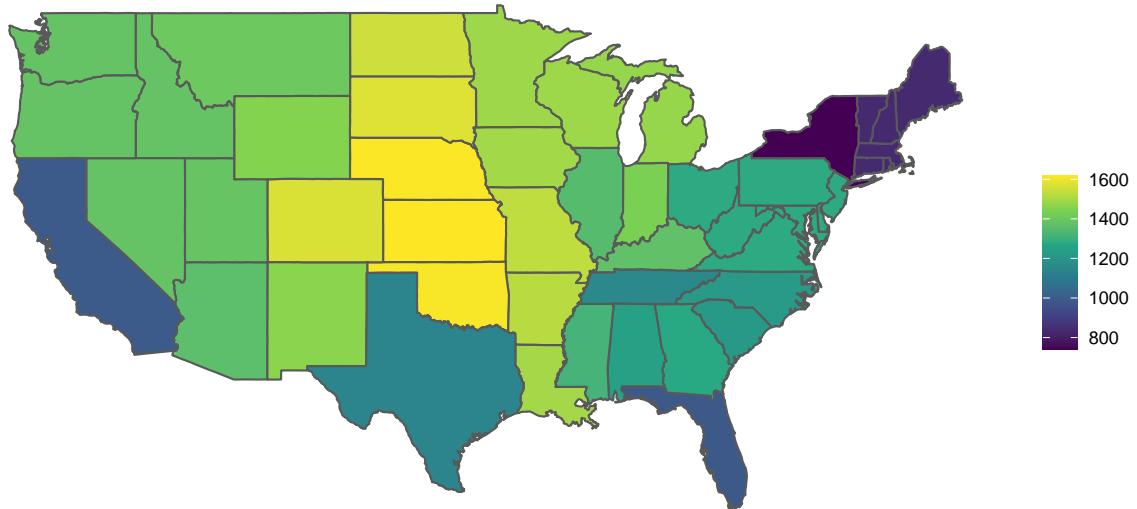
3.6 Figures and Tables

Figure 3.1: Effect of PV on Emissions vs Installed Capacity



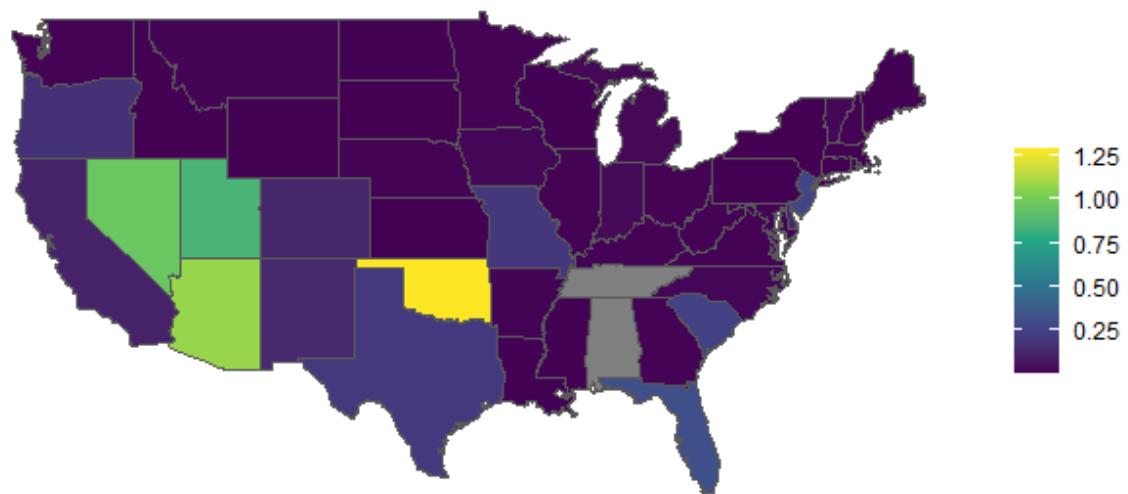
Notes: This figure shows, on the horizontal axis, the marginal reduction in CO₂ emissions caused by an additional 1 MW of installed solar capacity. On the vertical axis, the log total installed capacity as of 2021. Each point represents one US state among the lower 48. Alabama is excluded because, according to SEIA data, it has zero installations.

Figure 3.2: Reduction in yearly CO₂ emissions caused by 1MW of PV



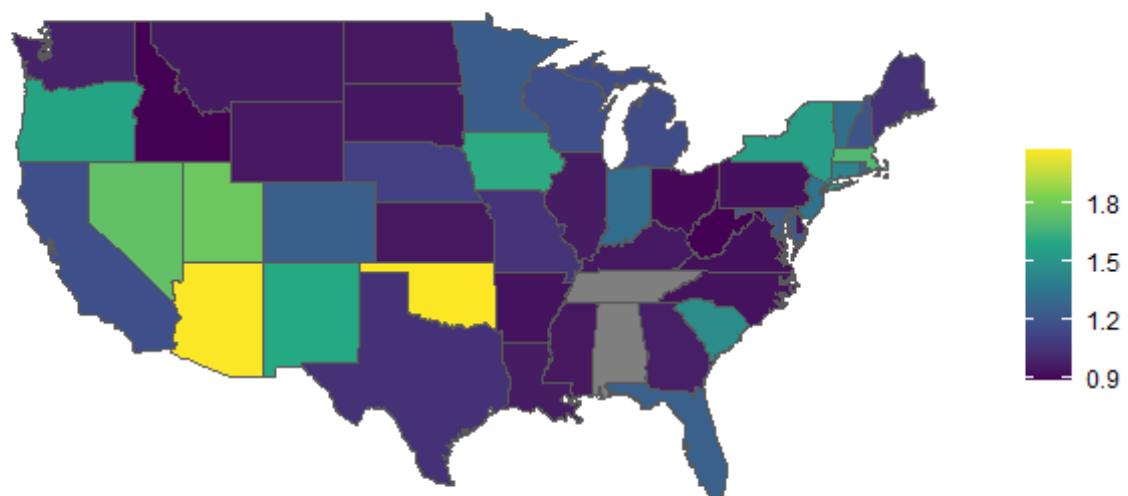
Notes: This figure shows the estimated reduction in annual CO₂ emissions caused by the installation of 1MW of nominal solar capacity in each of the states in the contiguous US. Emissions are measured in tons of CO₂ per year. The estimates come from the EPA's AVERT model.

Figure 3.3: Optimal Additional Incentives: τ_s^*



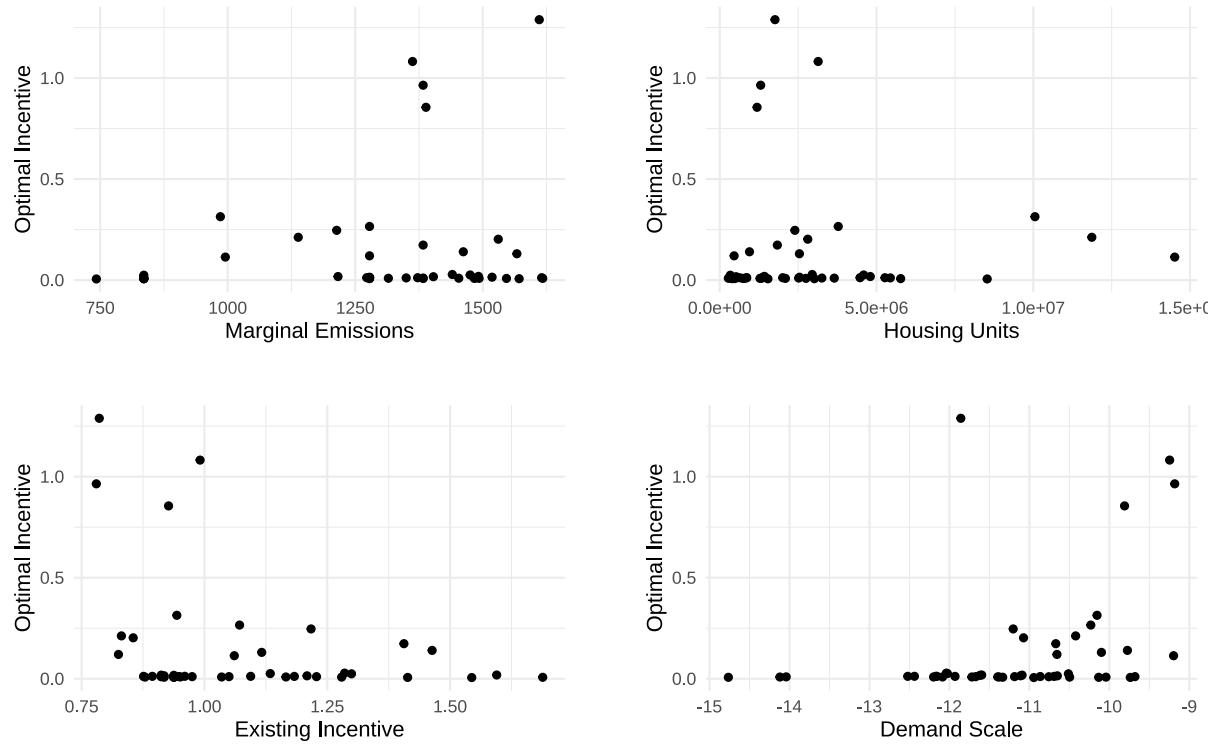
Notes: This figure shows the estimated τ_s^* , the additional adoption incentives that generate the optimal emission reduction for the given budget, in USD per kW of capacity, for each state in the contiguous US. Alabama and Tennessee are excluded from the model for having zero adoption.

Figure 3.4: Optimal Total Incentives: τ_s



Notes: This figure shows the estimated τ_s , the total adoption incentives that generate the optimal emission reduction for the given budget, in USD per kW of capacity, for each state in the contiguous US. This equals the additional incentives shown in Figure 3.4, plus existing incentives. Alabama and Tennessee are excluded from the model for having zero adoption.

Figure 3.5: Optimal Incentive vs State Characteristics



Notes: This figure shows the relationship of the estimated τ_s^* , the optimal additional adoption incentive, with state-level marginal emission intensity (top left), number of housing units in the ACS (top right), the existing level of incentives (bottom left), and the estimated demand scale parameter (bottom right). Each dot represents one of the contiguous US states. Alabama and Tennessee are excluded from the model for having zero adoption.

Table 3.1: Regression Results

	(1) ln Capacity pc	(2) ln Price	(3) ln Net Price	(4) ln Capacity pc	(5) ln Capacity pc
Incentive	0.0373 (0.0126)	0.00141 (0.0640)	-0.259 (0.108)		
ln Price				21.83 (986.4)	
ln Net Price					-0.119 (0.0690)
N	6622	5871	5871	5871	5871
Clusters	83	81	81	81	81
Year FE	Yes	Yes	Yes	Yes	Yes
Border FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	IV	IV

Notes: This table shows the main regression results. Observations are zip-codes by year, and clusters are counties by year. All columns include year fixed effects, border fixed effects and controls for median income and average housing value. Incentives are state level incentives, constructed based on equation 3.3.1. Price refers to full price before incentives, and Net Price to the price after incentives. In columns 4 and 5, the estimator is 2SLS with Incentive as the instrument.