What is Good for the Goose is Good for the Gander: The Effects of Child Care Provision in Mexico *

Gabriela Calderon†

Stanford University

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Abstract

In 2007, seeking to increase female labor force participation and more generally ease burdens on working women, the Mexican government introduced one of the most ambitious child care programs in an emerging economy: Estancias Infantiles para Apoyar a Madres Trabajadoras (EI). EI covers 90% of the cost of enrolling a child under the age of four at a formal child care center. The program is intended to benefit women who are looking for work, in school, or working -with the exception of those who already have access to child care because their job is covered by Mexico's social security system (IMSS). The roll-out of EI was so aggressive that by 2010 it enrolled 340,000 participants, more than double that of the 25-year-old IMSS child care program. However, EI was also rolled-out unevenly, owing to idiosyncratic variation in how quickly local offices processed applications from child care centers. I exploit the variation in the program's availability across time, across municipalities, and between eligible families and very similar ineligible ones. The essence of my approach is difference-in-difference-in-differences, but I extend this method in several ways –most importantly by adapting the Synthetic Control Method to my repeated cross-section data so that the ineligible comparison group is highly credible. Most of my results are intention-to-treat effects but I also show pseudo treatment-on-the-treated effects in which I assume that all the effects flow through families in which EI-eligible women significantly reduced how many hours they spent caring for their own children. I find that EI increased women's probability of working and reduced the time they devoted to child rearing. EI caused women to obtain more stable jobs, and it increased their labor incomes. EI also had effects -probably unintended- on men. Affected husbands spent less time on child rearing and housework, and they were more likely to switch to a better-paid job. However, husbands of EI-eligible women who were initially unemployed were less likely to re-enter the labor force.

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†Address: Encina Hall, 616 Serra St, Stanford, CA. 94305-6055. Email: gabcal@stanford.edu.

1 Introduction

Reductions in the cost of child care are thought to be an important reason why female labor force participation has risen so much in developed countries (Attanasio, Low and Sanchez-Marcos, 2008). Recently, the Mexican government began to subsidize child care centers greatly, expecting to achieve similar increases in females' probability of working. However, it is not obvious that reducing child care costs in a developing country will produce the same effects that it does in developed ones. Women's skills are different, labor demand is different, and the allocation of work within the family is often different. In this paper, I examine Mexico's recent, dramatic expansion of child care subsidies and investigate whether they are having the intended effects.

Policy makers, when implementing this type of policy, often think of women as though they live alone. In fact, women with children are usually part of a larger household, so they do not make unilateral decisions about supplying labor to the market, housework, and child care. Under most economic models of the household, the *partners* of women who are also beneficiaries of the subsidies may also change their behavior. I therefore investigate not only women's outcomes, but also those of their spouses. In particular, the main question that this paper will explore is: "What is the effect of a reduction in child care costs on a woman's and a man's labor supply decisions in a developing country?" The response of both the primary and secondary earner will determine the overall effect on the household's income.

Estancias Infantiles para Apoyar a Madres Trabajadores (EI) was initiated in 2007 in Mexico. It covers about 90% of the costs of child care for women whose children gain access to a subsidized place in an EI-qualified child care center. EI centers accept children who are at least one year old and under four years old. (At four, a child is eligible for public pre-school.) EI is intended especially for children whose mothers work in a job that is not covered by the Mexican social security system (IMSS). IMSS-covered jobs have a child care subsidy program of their own. By the second quarter of 2010, EI centers contained 358,000 spaces, enough to enroll 12% of children whose mothers were not eligible to use the child care services of the IMSS-covered sector.

Officially, EI targeted women who were working, actively looking for a job or studying. Furthermore, their household income was supposed to be lower than 6 times the minimum wage (Operating Rules, 2007). Because the government relied on self-reported activity and household income, however, nearly all women had access to the program.

EI child care places were allocated in a fairly idiosyncratic manner across Mexico's municipalities

during the initial period of its roll-out: 2007 to 2010. Essentially, local offices of the state governments were given the duty of approving child care centers' application to be EI-eligible. Some of these offices showed more alacrity than others and they approved more places, more quickly. I exploit the variation in EI space expansion using an empirical approach with a difference-in-difference-in-difference (DDD) identification strategy. That is, I simultaneously exploit variation across time, across locations, and across the families who were eligible and ineligible for EI. Given this strategy, the only threats to identification are other factors that affect the labor market outcomes of EI-eligible families in locations/times where more EI spaces are available relative to EI-ineligible families in the same location and time, relative to EI-eligible families at the same time in locations with lower EI availability, and relative to EI-eligible families at the same location at a time when there was different EI availability.

Factors that fit this "threat to identification" scenario are few and far between, but one of them could be idiosyncratic variation in labor demand. For instance, a possible scenario would be a manufacturer who moves into a municipality with timing that happens to coincide with the EI program and who happens to disproportionately demand the skills that women with children under the age of 4 happen to have. To ensure that such scenarios do not affect my results, I do not use a DDD strategy in which all ineligible people are treated as "controls" for the EI-eligible families. Instead, I use Synthetic Control Methods (Abadie and Gardeazabal, 2003; and Abadie, Diamond and Hainmueller, 2010) to ensure that my control group has the same mix of skills and preferences as my EI-eligible group.

The principal contributions of this paper are two-fold. First, I evaluate one of the most important child care programs in the developing world. Second, I develop a procedure for using Synthetic Control Methods in applications in which the data come in repeated cross-sections and in which people move in and out of eligibility for treatment over time. This procedure is likely to be helpful to many other researchers because we are often not in the conditions for which Synthetic Control Methods were originally designed–fully longitudinal data in which units do not switch from treatment to control or vice versa.

I mainly estimate intention-to-treat effects –that is, the effect of living in a municipality with available EI spaces and being eligible for those spaces. However, I also show pseudo treatment-on-the-treated effects where I classify a household as having been treated by EI if that household reports a significant reduction in the hours that an EI-eligible mother spends on child care–without

any accompanying change in her number of EI-eligible children. (If I could observe explicit EI use, then I would. However, since EI pays its subsidies directly to child care centers –not to mothers– a family's explicit use of the subsidy does not get recorded in any data that I was able to find.)

My results indicate that EI increased the probability of working and the earnings of EI-eligible women. (The last result occurs only for women with at most a high-school education.) EI-eligible women cut back on the time they spent caring for children, but did not reduce the time they devoted to housework. Unemployed men who are in EI-eligible families reduce their probability of re-entering the work force. This finding suggests that their wives' increased work is substituting for theirs. However, men in EI-eligible families who were already in the work force increased their probability of switching jobs and some switched to better paid jobs. EI-eligible men also reduced the time they spent on child care.

It is unclear whether the results that I find attained the Mexico government's goals for EI. These goals were somewhat vague: [the government has a] "need to develop services, with a focus on gender, that lead to conciliation between [women's] labor and family tasks in order to improve the quality of life for women" (Operating Rules, 2007). Moreover, they were expecting an increase in the female's labor income, which would contribute to increasing the household income (Operating Rules, 2007). Finally, one of the primary objectives of the National Plan of Development for the Public Administration was to promote gender equality, by "[eliminating] any type of discrimination on gender, and by guaranteeing equal opportunities for women and men," (Operating Rules, 2009). EI did promote market work by women –which fulfills the government's goal at least directionally– but it also seems to have induced some men to specialize more in market work by taking time away from caring for their own children. It seems likely that this was not a goal of the government. In addition, the effect on women is to increase earnings, but I did not find significant statistical increments in labor income at the household level in the short-run.

The literature related to the effects of child care on the female labor force participation has focused on highly developed countries like the US and Canada. See Cascio (2009), Baker, Gruber and Milligan (2008), and Gelbach (2002). While some papers have analyzed the effects of child care subsidies in developing countries, these papers have focused mainly on the how children are affected (Attanasio and Vera-Hernandez, 2004). Thus, this paper fills a substantial gap in the evidence by showing how labor supply responds to child care subsidies in a developing country. This paper is also one of the first to analyze the response of both the mother and father of the affected child.

Understanding the effects of child care subsidies on household decision-making is important because a number of other developing countries are considering similar programs: India, Colombia, and Chile are examples. A number of international organizations such as UNESCO, the OECD, and Inter-American Development Bank (IADB) have promoted child care subsidies in developing countries. For instance, the IADB has said that such programs are among its "highest priorities" (IADB, 2009). The World Bank gave US$300 million to childhood programs in Latin America and the Caribbean. Although it appears that these organizations have often been focused on early childhood outcomes when they promote child care programs, we nevertheless need to understand how the programs affect mothers' and fathers' behavior.

The organization of this paper is as follows: Section 2 describes the labor market in Mexico and the way the EI program started and evolved. Moreover, in this section, I describe other programs that have affected women with young children and that should be considered in the analysis. I review the related literature in Section 3. Section 4 presents a simple model that will help readers to predict the effects of the program. I describe the data and how I construct an "EI Exposure Index" in Section 5. I discuss the empirical strategy for estimating Intention to Treat Effects (ITT) in Section 6. Section 7 contains the results for various outcomes of interest. Section 8 describes my empirical strategy for estimating a psuedo treatment-on-the-treated effect. Section 9 examines several factors that explain where the idiosyncratic variation in the EI program originates. Section 10 presents a series of robustness checks, and conclusions comprise the final section.

2 Background

2.1 Covered and Uncovered Populations

In Mexico, workers can be classified into two types: covered and not covered by Social Security. Salaried workers, members of cooperative companies, and individuals specified by the executive power receive the benefits from the Social Security system.[1] These benefits include health insurance, day care for workers, disability insurance, and work-risk insurance, among others. In Mexico, there is a large population that is not covered by social security: self-employed and temporary workers, in addition to any workers who are hired by firms that do not pay Social Security taxes.

The government in Mexico does provide some social benefits to the uncovered population,

[1] Article 12 of the Social Security Law.

who a are not entitled to participate in the the Social Security system. Social benefits for this population usually depend on the household's level of income (as in Progresa/Oportunidades or the EI program), and some of them exclude covered workers (as in Seguro Popular or EI). On average, the Social Security system offers higher quality programs than those offered to the uncovered population. Given the high proportion of individuals that are uncovered, [2] the government has initiated important expansions of social benefits over the last ten years. The EI program is one such expansion that targets the uncovered population, which aims, specifically, at broadening the range of opportunities for women.

2.2 The EI program

One of the goals of President Felipe Calderon's Administration (2006- 2012) was to promote gender equity (First Government Report of Felipe Calderon, 2006); the EI program was designed and started during his administration to meet this goal. This program was designed to address the child care needs of the large portion of the population of women working that had had no access before the initiation of the program in 2007. The Federal Government made the EI program a cornerstone of its gender equality agenda and has promoted it extensively in the media since its inception.

The policy discussion was centered on providing support to women, or single fathers who were working, looking for a job or studying. Given the fact that 41% of the labor force was comprised of women, who are also largely responsible for child rearing, it was thought that the EI program would provide some relief to these women and improve their economic opportunities and quality of life. Facilitating the entry of women into the labor market was an explicitly-stated goal of the program (Second Government Report of Felipe Calderon, 2007). Policy makers expected that this program would increase household income, and even allow some households to escape poverty (Operation Rules, 2007). The government has expanded this program from its inception, to the second quarter of 2010, creating around 358,000 spaces. By the end of this period, it covered around 12% of the children eligible for the EI program.

Mothers with children between 1 year and 3 years, 11 months old are eligible for the EI program.[3] To be eligible, the mothers' income, combined with the income of other household members must

[2] According to the National Census of 2010, 36.62% of the total population are covered by Social Security.

[3] There are some exceptions for children younger than 1 or older than 4. However, less than 1% of the children using this program comprise this exception.

be less than six times the minimum wage,[4] and they should not have access to child care services provided by Social Security. Any person from the eligible population can apply to the EI program, by filling out an application and submitting it to the child care center they want to use or to the state's EI office. If they are accepted, they will receive a subsidy. The subsidy covers 90% of the cost of child care, and the family has to pay the rest. The cost to the household amounts to around 2% of the minimum wage.

The program targeted women who live below an established household income threshold, which has varied slightly over time. The government cannot verify the income of a person when they apply for this program, so mothers from any income group can actually apply, although high-income groups probably would not. For the purposes of this analysis, however, the definition of eligibility will not be restricted to any specific income group, since access *de facto* is not limited by income level.

Another restriction was that women should be working, looking for a job, studying, have no access to child care provided by the Social Security system. One exception to this case is that, women who should have access to the Social Security system may be eligible for the EI program if there is no space in the Social Security system. As a result, this restriction is actually very broad, and is both difficult and expensive for the government to enforce. Thus, this study will initially measure the effect on any woman with children between the ages of 1 and 3 years, 11 months old.

The main constraints on the program then are the number of spaces available and the budget available for the program. Spaces are available are on a first-come and first-served basis, until the budget for the program binds.

The government expanded the EI program by working with child care providers. It offered monetary transfers to any organization or civil association that would participate in the selection process for offering child care service. Those organizations that were selected received a one-time transfer (around 23 times a monthly minimum wage). The selection process required that the organizations should have physical space available for at least ten children (2 squared meters per child), own the space, have at least secondary education, submit three letters of recommendation, and pass an evaluation by the Ministry of Development, among others. The government also verified that the place complies with security conditions. Existing child care institutions could also be enrolled in the EI program. These institutions would also receive a one-time monetary transfer,

[4] The monthly minimum wage is approximately US$121.

which was lower than the transfer to new organizations (around 15 times a monthly minimum wage). The EI program is not the first child care service offered by the government. In 1973, the federal government began to provide child care services for the population that was covered by the Mexican Social Security program for salaried workers. These services are managed by the Mexican Institute of Social Security (IMSS, acronym in Spanish). However, the expansion of this program was slow. It took 25 years to create 100,000 spaces. In the mid-1990s, there were around 487 centers that attended to around 60,000 children, which is only 5% whose mothers were eligible (Staab and Gerhard, 2010). Between 1999 and 2006, they created 100,000 more spaces, which constituted a considerable expansion. Nevertheless, the pace of expansion slowed, and only 2,500 spaces were created between 2006 and 2010. Any mother who worked in a firm that pays Social Security fees should be eligible for child care through this program, although, in reality, there are far fewer spaces available than there are children eligible for the IMSS program. The variation in the IMSS program will be considered in the analysis of this paper.

The IMSS and EI programs offer mothers an opportunity for child care for the period until their children begin pre-school. All children are entitled to attend pre-school according to a law enacted in 2002.[5] The EI and IMSS programs thus provide a critical service to women whose children are not yet old enough for pre-school, which they can begin at age three. This condition is important since women who have a child close to the age of 4 cannot be a good comparison group since they have universal access to pre-school.

These two programs are the only options for child care that the government supports for children under the age of four. There are private child care services available, although these would be beyond the means of most low-income families. Table 1, in Appendix B, shows that the average child care expenditure in Mexico is expensive, even for families whose income is close to the median. The EI program might have a potential effect, not only on the poorest households. Moreover, the proportion of households that were using paid child care services was approximately 3.84%, and the average payment was \$975 pesos($\sim$ US\$75 or approximately 70% of a minimum wage) and \$758 pesos ($\sim US\$58$) for 2006 and 2008 respectively (Household Income and Expenditure Survey, ENIGH).

[5] Articles 3 and 31 of the Mexican Constitution

3 Literature Review

The literature related to the effects of subsidized child care or universal pre-school on female labor supply has focused mainly on the US and Canada. However, the literature analyzing these effects for the case of developing countries is limited. The general finding suggests that women labor force participation increases as women face a reduction in child care services. Due to the analysis of different policies and the usage of different methodologies, there are definitive differences among the estimated effects. For example, Fitzpatrick (2010) did not find any effects for some groups of women living in urban areas, whereas in the case of Quebec, Canada, Baker, Gruber and Milligan (2008) find an increase in employment of 7.7 percentage points (pp). The literature has not focused on the response of the male for this type of program. In this section, I will describe some of the literature analyzing the effects of a subsidized child care program in developed and developing countries. Moreover, I will emphasize where a gap exists in this type of literature, and the way this paper contributes to the existing literature.

Heckman (1974), Blau and Robins (1988), Gelbach (2002), Baker, Gruber and Milligan (2008), Cascio (2009), and Fitzpatrick (2010), among others, have examined the effects of child care on female labor force participation in developed countries and using a variety of methods found that the availability of child care services increases women's participation. In developing countries, however, where these programs have typically been established in response to interest in the children's development, the focus has been on determining how these programs affect children's health, etc. From that perspective, the results have been mixed– Attanasio and Vera-Hernandez (2004), Behrman, Cheng, and Todd (2003) find positive effects on children's outcomes (health, academic and psychological), while Ruel *et al.* (2002) found only small effects.

Yet, a gap remains in the literature with respect to the effect that these programs in developing countries have on women's participation in the labor force, which may help determine the broader efficacy and impact of these programs. The contribution of my paper to the child care literature will be to estimate the effects on the head of household and spouse in response to a reduction in the cost of of child care. Furthermore, I will analyze how the household that has a woman in the margin on entering the workforce responds with regards to time allocation and labor supply.

To my knowledge, the only published study, on the effects of child care on woman labor force participation in a developing country was conducted by Attanasio and Vera-Hernandez (2004). They analyzed a child care program, Hogares Comunitarios de Bienestar Familiar (HC) in rural

Colombia, for poor households. They estimated the effects on the HC program on children's health outcomes and female labor supply. They measured the participation in the HC program by using the location of individual households relative to the HC. Their main focus was controlling for the self-selection of poor households to participate in this program. They estimated an increase in the probability of employment between 0.12 to 0.37 for those women who participated in the program. They also found important effects on both the nutritional status of young children and on the academic performance of older children.

There are other studies related to child care provision in Latin American countries. However, they have focused primarily on the effects of child care programs on children. For example, Behrman, Cheng, and Todd (2003) studied a child care program in Bolivia on children's motor skills, psychosocial skills, and language acquisition. They used a large nonexperimental data set and a matching estimation strategy, assuming that selection into different exposures to the program is based on observables. Ruel et al. (2002) studied a child care program in one zone of Guatemala City. They did a case-control design of approximately 250 beneficiary children matched with control children of the same age and neighborhood, whose mothers also worked outside the home. The results were focused on health outcomes and found limited effects.

Some studies conducted in developed countries have focussed specifically on the female labor supply. One of the first papers to analyze effects was Heckman (1974). He studied the 1971-1972 tax reforms related to generous deductions to work-related child care expenses if a woman worked. He presented a method for directly estimating consumer indifference surfaces between money income and non-market time. Using this methodology, he estimated parameters of both hours-of-work and decision-to-work functions from a common set of parameters. His findings suggest that increasing a unit in the logarithm of the quality adjusted price of child care, divided by its standard error raises the marginal rate of substitution by 22% for whites and 29.7% for blacks.

Other studies in developed countries have detected important effects for some specific groups of women, but not for others. For example, Cascio (2009) analyzed the effect of an expansion on public school kindergartens in the US and found significant effects on single mothers whose youngest child was eligible for this program. The effects were not statistically significant for married mothers. Maria D. Fitzpatrick (2010) found significant effects for women living in rural areas when Universal Pre-K programs were implemented in Georgia, but no effects on employment in other areas. In this paper, I will also analyze heterogenous effects; however, in contrast to the previous results, I

do not find significant differences among them.

There is some research that uses the variation in time in child care policies comparing similar regions as is the case in Cascio(2009) or Baker, Gruber and Milligan (2008). The former uses state and time variation on the kindergarten funding, comparing similar mothers who were eligible for this program. The latter analyzed the response of low-income couples in Quebec over maternal labor supply and family well-being, to the availability and subsidy of child care. While maternal labor supply increases, the psychological and health status of children worsen. They used a difference-in-difference estimation strategy assuming that the program was exogenously allocated, and compared eligible families within Quebec and the rest of Canada. Their intention to treat estimates suggests that the probability that a child was in child care rose by 14.6 pp in Quebec. They analyzed different outcomes related to women's employment status and their usage of child care. On average, employment rose by 7.7 pp. Conditional on women using child care, employment increased by 12.5 pp, and conditional on women not using child care, it decreased by 4.8 pp. The proportion of mothers who do not work and who do not use child care, decreased by 10 pp.

In contrast, there are other papers that do not utilize regional variation and compare similar women who are eligible for a program while others are not. Gelbach (2002) estimates the effects of public kindergarten school enrollment on female labor supply outcomes. His empirical strategy consists of measuring school enrollment status with five-year-olds' quarter of birth. The sample was divided between women whose youngest child was five years old and those who had a child younger than five, since the latter group is constrained from participating in the labor force. The 2SLS estimates suggest that public school enrollment for the former group had an effect of increasing the total hours worked in the previous week by 2.7 hours (10% from the baseline); increased the probability of working (4 pp or 6% from the baseline); reduced by 4.4 pp welfare receipt (10% from the baseline); and increased wage and salary income by 24% in relation to the baseline.

4 Theory: Child Care Subsidy and Reallocation of Time

In this section, a simple theoretical model of a household which is deciding ways to allocate time for child care, work, and searching for a better paying job is developed. This model will be used to anticipate the potential effects of a child care subsidy.

A household, consisting of a man (m) and a woman (w), seeks to maximize a weighted sum

of its consumption utility and the quality of the child care (Q_{ch}). It is assumed that consumption utility is increasing and concave in consumption c, the man's leisure l_m and the woman's leisure l_w. It is also assumed that the quality of child care is increasing and concave in the total time the man and woman allocate to child care. The household maximization problem is then:

$$\max_{t_i^{child}, t_i^{work}, t_i^{search}} \theta U(c, l_m, l_w) + (1-\theta) Q_{ch}(t_w^{child} + t_m^{child}) \quad \text{for } i = m, w \tag{1}$$

Both the man and woman can allocate time to child rearing (t_i^{child}); to work to generate income (t_i^{work}); and to searching for a better paid job (t_i^{search}). For simplicity, I assume that sufficiently high search effort yields an new, better paying job with certainty. This avoids using the stochastic nature of job search but simplifies the analysis. More precisely, a worker i's wage is

$$w_i(t_i^{search}) = \begin{cases} \underline{w}_i & \text{if } t_i^{search} < \bar{t}^{search} \\ \underline{w}_i + w^P & \text{if } t_i^{search} \geq \bar{t}^{search} \end{cases}$$

where the wage premium is $w^P > 0$ and $\underline{w}_m$ and $\underline{w}_w$ are the baseline wages received by a man and a woman, respectively. The only (ex-ante) asymmetry between men and women will be the baseline wage they receive. I will assume $\underline{w}_m > \underline{w}_w$. I could analogously assume that $\underline{w}_m < \underline{w}_w$, and all results will be reversed.

Utility from consumption and leisure is assumed to satisfy the usual INADA conditions. Thus, $\frac{\partial U(c,l_m,l_w)}{\partial c} \to \infty$ as $c \to 0$, $\frac{\partial U(c,l_m,l_w)}{\partial l_m} \to \infty$ as $l_m \to 0$ and $\frac{\partial U(c,l_m,l_w)}{\partial l_w} \to \infty$ as $l_w \to 0$. Finally, for simplicity, I assume that if $l_m = l_w$ that $\frac{\partial U(c,l_m,l_w)}{\partial l_m} = \frac{\partial U(c,l_m,l_w)}{\partial l_w}$ – the man and woman receive the same marginal benefits from leisure when they consume the same amounts of leisure.

I assume that child care requires a fixed amount of time 1. The quality of the child rearing then depends on how much of this time is provided by the parents and how much is external. More specifically, the quality of the child is modeled as a production function that depends (only) on the total amount of time spent on child rearing by the man and woman ($t_w^{child} + t_m^{child}$). Implicit in this formulation is an assumption that the man's and woman's time are perfect substitutes in parental child rearing. However, paid child care need not be a prefect substitute for parental child care. I also assume that the marginal value of parental child care over paid child care is always (weakly) positive, but (weakly) decreasing in total parental child care. More formally, $Q_{ch}(t_w^{child} + t_m^{child})$ is weakly increasing and concave in $t_w^{child} + t_m^{child}$.

The household budget constraint is given by:

$$t_m^{work}(w_m(t_m^{search})) + t_w^{work}(w_w(t_w^{search})) \geq c + (1 - t_m^{child} - t_w^{child})p^{ch} \tag{2}$$

The household can use child care services such that $t_{ch}^{child} = 1 - t_w^{child} - t_m^{child}$, but child care services are costly and every unit of time used is priced at p^{ch}.

After the man and woman have allocated time to search for work, work and parental child care, they consume leisure with the remaining time they have left:

$$l_i = 1 - (t_i^{child} + t_i^{work} + t_i^{search}) \qquad \text{for } i = m, n \tag{3}$$

I consider the following exercise. I fix the parameters of the model other than the price of child care services and then I vary the price of child care services from a very high price to a very low price. The formal analysis of this exercise appears in Appendix A and here I present only a summary of the main results.

4.1 High Child Care Costs and Specialization

For very high child care costs, all child care must be parental. In equilibrium both the man and woman must then contribute time to child care. As the man has an advantage in working, but the man and woman are perfect substitutes for providing child care, the woman cannot spend any time working or searching for work. If the woman spends time working and searching for work, household consumption could be increased by reallocating the woman's time away from work and into child care and reallocating the man's time away from child care and into work. There is thus specialization in terms of work. Indeed, using this argument, the woman will never work or search for work in equilibrium while the man engages in child care.

As the price of child care continues to fall, eventually child care services will start to be purchased. When the household first starts to purchase child care, doing so yields more time to the household for leisure, working and searching for work. At least some of this additional time must be spent by the man working more. However, as the price of child care continues to fall, the effect

of the decrease in the man's time spent working can be ambiguous. The decrease in child care costs, yields an income effect that can result in the man working less. To summarize, for households who are just motivated to start consuming child care costs the man's labor supply must increase, but that is not necessarily true for households who consumed child care before a price decrease.

4.2 Increased Searching by the Man

If the man's response to the drop in the price of child care service (due to parameter values) is to increase his time working, then as p^{ch} drops, there is a point at which it is optimal for men to allocate time to searching for a new job. It is easy to see that the man will always choose to allocate no time to searching or $\bar{t}^{search}$ time to search. Which search effort will the man choose? The man will allocate $\bar{t}^{search}$ time to search whenever the benefits of searching for a new job exceed the benefits of working at a baseline wage. In other words, whenever the men decides to search for new employment, he automatically reduces the amount of time working. However, it might be beneficial because of the gains obtained as a result of the wage premium.

The man will choose to search once he allocates sufficient time to work. Men may therefore begin searching for work as the time allocated to parental child rearing decreases –a consequence of reducing the price of child care. However, it should be noted that this is not a strong prediction of the model. There are also parameter values for which the man will never choose to search and other parameter values for which the man will always choose to search.

4.3 Labor Supply from the Woman

As argued above, while the man spends a positive amount of time engaged in child care, the woman cannot work in equilibrium. However, if the cost of child care falls far enough, the woman may become the sole provider of parental child care. Once this occurs, further decreases in the price of child care might result in the woman spending a positive amount of time working. Whether the woman will start to work or not will depend on her baseline wage, the marginal value of additional consumption and her relative comparative advantage in increasing child quality over non-parental child care provision. Even for prices of 0, if the woman is sufficiently better at increasing the quality of child care than non-parental caregivers, she may not supply any labor in equilibrium. Whether the woman chooses to work will also depend on the income of the man. When the man's income is higher, consumption will be higher, and the marginal utility from additional consumption will be

lower. A woman's reservation wage from working will be:

$$\underline{w}_w^R \equiv p^{ch} + \frac{\frac{\partial Q_{ch}(t_w^{child})}{t_w^{child}}}{\theta \frac{\partial U}{\partial c}} \tag{4}$$

An implication of this analysis is that women with lower non-labor income, who contribute less to the child's quality, relative to the wage they could earn, should have a labor supply that is, ceteris paribus, more sensitive to a reduction in p^{ch}. Women of this type might be those with a lower level of education.

5 Data and EI Exposure Index

5.1 Data

The data used in this paper are derived from different sources. I used household data, census data, and administrative data. The two household data used are the National Survey of Occupation and Employment (ENOE, acronym in Spanish) from the first quarter of 2005 (2005.Q1) to the second quarter of 2010 (2010.Q2), and the National Survey of Employment (ENE, acronym in Spanish) from the second quarter of 2000 (2000.Q2) to the fourth quarter of 2004 (2004.Q4). The ENOE and the ENE data are national surveys that are representative at the national and state levels. Both were generated by the National Institution of Geography and Statistics (Instituto Nacional de Estadistica y Geografia, INEGI); they have a focus and structure similar to that of the Current Population Survey. They contain detailed information related to the labor conditions of the Mexican households; both are rotative panel data. There are always five different groups of households, based on those who enter the sample concurrently. They follow each group for five quarters. The discrepancy between them originates from the fact that in 2005 they revised the phrasing of some questions. Trying to match these data sets might be misleading with regard to variations that were actually not observed. Therefore, if I tried to join these two data, we would observe a "seam" problem.

The variables used in the household data (ENOE data) were linked to the administrative data at the municipality level, which contained information about the number of spaces available for

child care provided by the EI program and the IMSS program.[6] It was also linked to the Mexican Census Population data that contained information about the number of children who were eligible for the EI and IMSS programs.

The main population of interest will be women who have children older than 1 and younger than 4 years of age[7] The data will be restricted to those women who are heads of households or spouses of heads of households and are between 16 and 60 years of age. I stop observing an individual when she migrates to another city. From now on, this group will be defined as the *eligible women*. EI also had effects –probably unintended– on men. Therefore, the men who are coupled with the eligible women are our second population of interest. I define a "partner of an eligible woman" as those men who live with the eligible women and satisfy the same characteristics as the eligible women except for gender. The non-eligible women and men are those heads of households or spouses of the heads of households who do not have children under the age of four.

Table 2, in Appendix B, shows the summary statistics of the sample used one quarter before the EI program started (i.e. 2006.Q4).[8] The main variables analyzed are: *working*, a dummy variable indicating 1 if a person is considered to be working according to the definition of INEGI; *working conditional on not working in t-1*; a dummy indicating 1 if a person is working in period t but was not working in the previous quarter; *self-employed*, a variable that takes the value of 1 when a person is working but has no boss; *switching jobs*, a dummy variable that takes the value of 1 when a persons switches economic activity from one quarter to the other, or switches activities leaving one quarter in between not working; *monthly labor income*, a variable measured in 2008 real pesos, representing the monthly income earned from labor; *hours worked conditional on working in t-1* measures hours per week for the population who was working in the previous quarter; *time child rearing* measures hours per week allocated explicitly to parental child care; and *time housework* represents hours per week allocated explicitly to cleaning and fixing their home.

Table 2, in Appendix B, reveals that the eligible women and the non-eligible women have considerably different labor characteristics and amount of time allocated to child rearing compared to the non-eligible women. The proportion of women working is considerably lower for the eligible

[6]The Mexican Ministry of Development (SEDESOL) provided all the information used for the EI program, and the IMSS institution provided all of the information for the IMSS program.

[7]Women with children under the age of one will be considered separately, since they might also have been affected by the EI program by responding before using it. For example, they might leave the child with a relative and enter the work force, since they expect to use the EI program when their child becomes one year old. This group will be excluded from the primary analysis.

[8]Individuals who were living with an eligible woman or man were excluded from the non-eligible group.

group than for the non-eligible group (12.7 pp less). This group has a lower probability of becoming employed (approximately 5 pp less), conditional on those women who were not working in a particular quarter. As expected, they allocate on average around 15 hours per week more to child rearing, but there is no difference in the amount of housework among them.

The similarities in the variables of interest described above between eligible and ineligible men are high. Only 4.5% of eligible men were not working, compared to 7.2% of ineligible men. The principal difference between these groups is that among the eligible men who were not working in the 2006.Q3, 74.5% went back to work in the next quarter, whereas only 52.2% of ineligible men did. There is also an expected difference in the time allocated to child rearing, but the discrepancy is not large. Finally, ineligible men earn around US$40 more than eligible men, possibly reflecting having more years of experience.

Men allocate considerably less time (16.9 hours per week) on child rearing and housework than women do. Eligible women who are working, work on average 10 hours less than the eligible men. The labor income shown in the Table refers to labor income conditional on a person working. Approximately eligible men labor earnings are 37% higher than those of women. We can observe from 2, specialization within the household, especially among eligible couples.

5.2 EI Exposure

The degree to which the individual will be affected by the EI program depends on the number of EI spaces available within a municipality and by the number of eligible children in that region. Eligible children for the EI program will be defined as those who are between the age of 1 year old and 3 years, 11 months old, whose mothers are not eligible for the child care program run by the Social Security system for salaried workers (IMSS). In this way, the exposure of the program will be defined as the ratio of the number of places in that municipality over the number of eligible children for the EI program in the same municipality. Equation 5 represents the EI exposure measure for municipality m during quarter q and year t.

$$EIexposure_{mqt} = \frac{Spaces\ Available\ from\ Estancias\ Infantiles\ Childcare\ Centers\ in\ period\ q,t}{Number\ of\ Eligible\ Children\ for\ EI\ program\ in\ municipality\ m} \quad (5)$$

The Mexican Population Census from 2010 was used to determine the number of eligible children

in each municipality. The variation of the EI exposure at the state level [9] of the EI program for four different quarters can be seen in Figure 1 in Appendix B. The quarters selected were the first quarter of each year between 2007 and 2010. Note that there is considerable variation across states and time. The variation across time can be seen in the drastic increase in the EI exposure at the beginning of 2008. The variation across space is observed by comparing states like Nayarit (State 18) where 25% of the eligible children of the EI program were covered in 2010, versus Chiapas (State 7) where by 2010 the average eligible population of children covered by the EI was no more than 5%. To observe the variation at the municipality level, I present different graphs at different periods of time (2008.Q1, 2009.Q1, and 2010.Q1) of the EI exposure in Appendix B, Figure 2.

Women who applied to the EI program are able to apply to another child care institution if they do not find an available spot. This might cause some women to apply to neighboring municipalities, or even chose municipalities close to work and not necessarily where they live. In order to achieve the correct level of variation, those municipalities that are considered urban (more than 15,000 inhabitants) and are within the same metropolitan area will be considered to be one municipality. In the case of rural municipalities, this transformation will not be made since it is unlikely that women from one rural municipality will travel to leave their child in another rural municipality. There are 2,456 municipalities in Mexico. From these, 185 municipalities were grouped into 32 metropolitan areas.

Given the fact that I do not have information of which family received the program or not, I will use the variation of the EI exposure index, together with the eligibility condition (see below for further explanation) in order to identify the effects of the program. The relative availability of subsidized child care services, represented by the EI exposure measure, will reflect the proportion of individuals that are affected by a reduction in the child care services cost. Therefore, in those regions where there is a low EI exposure, the subsidized spaces will only be available for a few households. Similarly, those regions associated with a high EI exposure index, a large proportion of households will be facing a huge reduction on the price of child care services. In accordance to the model presented in Section 4, the reduction of price in child care comes from: i) it is available and ii) the fee they need to pay for child care services is low.

[9] State identifiers are shown in the graph. To see the name that corresponds to the number, see Appendix B, Table 3.

6 An Empirical Framework for Modeling Effects of Child Care

Fundamentally, there are three sources of variation in assessing the exposure to the EI program. First, there is variation among places at one point in time: from 2007 onwards, some places have more available child care spaces per eligible child than do others. Second, there is variation over time within each place: some places' stock of EI-qualified child care spaces grow faster than the stock of others. Third, there is variation among families, at any given location and time, in eligibility for the program. A family with a child aged 3.5 is eligible for EI child care space. A family with child aged 4.5 is not.

My empirical strategy, which is an adaptation of a difference-in-difference-in-differences (DDD) strategy, exploits all three of these sources of variation, simultaneously. The identifying assumption of the effects of the EI program is that there is no omitted factor that changes with the same timing and same geography as the EI exposure index and that disproportionately affects eligible families relative to ineligible, but otherwise similar, families. My key methodological contribution is defining "similar families" in a highly credible way– see explanation below.

Using a DDD-type strategy in my specification will control for many factors. It controls for i) any factor, observed or unobserved, that is the same within a place over time; ii) any factor, observed or unobserved, that is the same at one point in time across all places; iii) any factor, observed or unobserved, that is the same across all eligible families or that is the same across all similar, but ineligible, families. Two-way interactions among the above factors will control for factors that are the same across eligible and similar ineligible families, within one location at some time.

The measures of time, place and eligibility are the following. Time is measured as the quarter of the year. My measure of place is the municipality *except* that in urban areas I aggregate to a metropolitan area because families are likely to consider child care providers that are outside the boundaries of their municipality, but that are nevertheless proximate to their home or place of work. My measure of eligibility for women is whether the woman is the mother of a child who is aged 1 to 4 years old (eligible for EI-based child care). I also define a "partner of an eligible woman" measure because these partners may be indirectly treated by EI. Non-eligible individuals are adults who do not fall into either of the above categories.

As is well known, the key coefficient in a DDD-type estimating equation is the coefficient on the intention-to-treat variable that is specific to a time, place, and eligibility. In cases in which

the intention-to-treat variable is binary (yes/no), this variable is usually a triple-interaction among time, place, and eligibility indicators. In my case, the variable is the interaction between eligibility (e_i) which is a binary variable and EI exposure, which varies continuously but which is also specific to a time and place. Specifically, the triple interaction is:

$$e_i \cdot EIexposure_{mqt},$$

where e_i is an indicator for eligibility of the EI program (the family has at least one child who is at least 1 year old and less than 4 years old); i indexes individuals; m indexes municipalities; q indexes quarter of the year; and t indexes years. I measure $EIexposure_{mqt}$ by dividing the number of child care spaces in mqt by the number of EI-aged children in mqt who are not covered by the IMSS child care system.

Below, I described how I define a treatment variable, as opposed to the above intention-to-treat variable. In addition, see for my exact estimating equation below, which adapts the DDD framework to account for the fact that the intention-to-treat is not binary.

In any DDD-type strategy, the key challenge is establishing that any phenomena for which one does not explicitly control through covariates is a factor covered by one of the conditions enumerated *i* through *iv* above in this section. Some phenomena are obviously covered. For instance, prices that apply identically across all areas at one point in time are covered by condition *ii*. Other phenomena are more troublesome. Suppose, for instance, that some areas had a growing demand for the skills prevalent among women of child-bearing age and that, as a result, women had a greater desire to work in these areas *and* successfully petitioned their state governments to approve EI-qualified child care spaces more quickly. In such a case, it is essential that the demand for the skills of eligible women grows at the same pace as the demand for skills of similar but ineligible women. If demand for these two types grows at the same pace, then the behavior of the similar but ineligible individuals will provide an accurate picture of how the eligible individuals would have behaved *in the absence of the EI program*.

Thus, the key challenge in my empirical strategy is finding a group of ineligible individuals who are similar in the following sense: their behavior mimics what the eligible individuals would do under circumstances that exactly match those that prevailed (in their area and time) except that

the EI program not have been available.

Before proceeding to the construction of a synthetic control group, it is worth noting that a simpler empirical strategy, in the spirit of difference-in-differences (with no within-time-within place control group) or detrended differences-in-differences (differences from pre-existing trends), would not have worked well because the data reveal that labor force participation was on different trajectories in different states prior to the introduction of EI. Moreover, any simple extrapolation of each state's pre-EI trajectory does a poor job of predicting its post-EI trajectory largely because most labor market variables were off their prior trajectories from 2004 to 2007, when the Mexican economy was booming. However, even the 2000 to 2003 trajectories differ substantially among states. Therefore, it is valuable to have a within-time-within-place control group that can credibly predict what would have happened in a counterfactual world with no EI.

In Appendix B, Figure 3, I provide graphic evidence of the points made in the previous paragraph– that is, the differences between states' trajectories that made a difference-in-difference strategy, detrended or not, unlikely to produce credible results.

6.1 Establishing a Synthetic Control Group

The synthetic control method introduced by Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010) is based on the idea that a mixture of untreated individuals can potentially serve as a better control for a treated individual than can a single untreated individual. One of the most appealing aspects of the method is that the weight ultimately placed on each untreated individual is chosen to be one that maximizes the similarity of the treated and synthetic control groups during the pre-treatment period on the evolution of relevant outcomes. Thus, the method uses explicit optimization over the entire array of potential control data to form the control group, as opposed to selecting the control group in an *ad hoc* manner, as is commonly done in matching methods. The synthetic control method effectively relegates the choice of the weights *to the data itself*, leaving the researcher to choose only the outcomes(s) on which the maximization should be based. This is a simple choice in practice since the outcome(s) should be same one(s) for which the researcher wants to provide results. For instance, in this study, the outcome that is most obviously relevant is the probability of working.

If Mexico had long panels of longitudinal data and if the EI treatment were purely binary–for instance, "off" before 2007 and "on" 2007 thereafter– finding a synthetic control group among

the eligible population would probably not be difficult. Compared to the eligible population, the ineligible population is large and sufficiently diverse to contain numerous individuals whose skills, preferences, and constraints mimic those of the eligible population. That is, in terms of the populations, we have propitious conditions for identifying a synthetic control group who are sufficiently like the eligibles to predict their behavior in the absence of EI.

In practice, I faced a few challenges. First, the EI treatment is not binary –it grows over time within each place. Furthermore, individuals' eligibility status changes as their children age in and out of EI-qualified child care. Therefore, neither the control group nor the treatment group can be defined based on a single point in time. Second, Mexico's relevant datasets contain only short, five quarter-long panels of longitudinal data, equivalent to the panels of the United States Current Population Survey. As a result, I cannot specify that a person is "treated" or "control" and follow her forward throughout the entire period in which EI is relevant. Third, an important aspect of establishing a synthetic control group is examining pre-EI determinants of labor force outcomes. Therefore, it is unfortunate that ENE covers only the period up through the fourth quarter of 2004 and that ENOE replaced it in the first quarter of 2005. The sampling and questions changed somewhat in the replacement process.

Given these circumstances, which are not unusual for a policy evaluation, my synthetic control method adapts the method developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). Let s index an individual's state and let k index her sample wave –that is, the quarter of her entry into the ENE or ENOE. Within every state-by-wave group, I divide EI-eligible individuals into 8 cells based on (younger or older than 35 years old), educational level (below or above a secondary education), and urbanicity (urban/non-urban). Within every state-by-wave group, I divide non-eligible individuals into 32 cells based on age, education, urbanicity, gender, and whether they have children between 4 and 12 years of age. Men who live with an eligible woman are excluded from the cells of potential control individuals. For more on this, see below.

For each cell of eligible women in each state-wave combination, I use the synthetic control maximization to choose the optimal weights on the 32 potential control cells from the same state-wave combination. The resulting weighted average of the potential control cells is the "synthetic control" or "synthetic eligible" group.

Specifically, let J be the number of potential control cells (J=32 in practice). Let W_{sk} =

$(w_1, w_w, ..., w_J)$ be the $Jx1$ vector of non-negative scalar weights which sum to 1, for state s and wave k. Let the outcome we are trying to match be the probability of working. Allow the probability of working, for any given cell, to be a function of a vector of covariates X which can have different values *within* a js combination, and a vector of macroeconomic variables Z, which change over time at the state level for every cell j [10]. In particular, let $X_{1,sk}$ be the $Lx1$ vector of variables for the eligible group that can predict their probability of working, in the last period when wave k was observed in the sample. Let $X_{0,sk}$ be a LxJ matrix whose elements are these predictors, but for the control group.

If one thinks of the all the cells associated with a given cell js as being a "person" whom we follow longitudinally, one observes the role played by the variables in X and Z. Just as a person in a long panel dataset may change slightly over time and be affected by macroeconomic conditions beyond his control, the characteristics of cells js may change over time (although in practice such changes are very limited, given the tight cell definitions and consistent survey sampling) and the behavior of the js cell may be affected by macroeconomic conditions.

As Abadie and Gardeazabal (2003) specified, the optimal weighting matrix W_{sk} is the matrix that minimizes $(\tilde{X}_{1,sk} - \tilde{X}_{0,sk}W_{sk})'V_{sk}(\tilde{X}_{1,sk} - \tilde{X}_{0,sk}W_{sk})$ subject to $w_j \geq 0$ for $j = 1, 2, ..., J$; where $\tilde{X} = (X, Z)$. The matrix V_{sk} is chosen, such that the trends of the outcome variable of interest –probability of working, in my case– of the eligible group is best reproduced by the synthetic group.

In practice, the vector of covariates X includes: the average age in cell jsk, the average level of education in cell jsk, the average probability of living with other relatives in cell jsk, the average age of the youngest child in cell jsk, and the total number of children living in cell jsk. The macroeconomic variables,[11] all measured at the state-type level, are: the average real wage among men for each of the agriculture, manufacturing, retail, construction, and wholesale sectors; and the average real wage among women in the same sectors. After the weights are initially estimated, they are rescaled using the logistic function to ensure that they fall within the 0 to 1 interval.

My baseline set of synthetic control weights is selected in maximization, where the outcome is the probability of working and the pre-treatment period is 2000.Q1 and 2004.Q4. This period was selected as the pre-treatment period of interest because ENE and ENOE cannot be joined since sampling and questions changed –a pre-treatment period based solely on ENOE data would be too short for credible estimates– encompassing only two years of data: 2005 to 2006. A pre-

[10]The macroeconomic variables use are wages received by a certain type of control individuals.

[11]The macro-economic variables used for this exercise were taken from the household data ENE and the ENOE.

treatment period based on both ENE and ENOE data (2000 to 2006) is longer, but the additional statistical power gained by lengthening the period must be balanced against the *"seam"* problems caused by the ENE-to-ENOE switch, which affected sampling in particular. That is, primarily due to sampling, but also because of changes in the wording of questions, the covariates X in each js cell change more at the seam between the two datasets than between any other two years. As a result, when the ENE-to-ENOE seam is included in the pre-treatment maximization period, seam-caused variation (as opposed to true variation) dominates the estimation of the coefficients on the X variables, which consequently be biased. This is a form, although not a standard one, of measurement error bias. My baseline approach is the prudent one: to run the synthetic control maximization without the seam and use the resulting optimal weights on cells.

Recall that I excluded those men who live with an eligible woman from the potential control cells because these men will be affected by EI if the program changes intra-household allocations. Thus far, I have described the eligible individuals as if they were all women, but since men are a potentially treated group as well, I create synthetic controls for them. I examine their outcomes using a procedure exactly parallel to the procedure just described for eligible women.

A final issue is whether non-eligible individuals who are similar to the eligible women are affected by EI through general equilibrium effects. For instance, if EI were to increase the labor supply of eligible women considerably, they might substantially depress the equilibrium wages earned by non-eligible women and thereby affect the non-eligible women sufficiently to make them poor potential controls. After I compute my initial results, however, I will show calculations that clarify the fact that the plausible magnitude of such general equilibrium effects is too small to have a meaningful impact on my results.

6.2 Comparing Trends for Eligible Individuals and the Synthetic Controls

If the synthetic control method works as it should, trends in behavior should be very similar in the pre-treatment period for the eligible individuals and the synthetic controls. Figure 4, in Appendix B shows this exercise: the synthetic control group's time trend in the probability of working does a very good job of mimicking the eligible individuals' time trend for the same variable. Figure 7 illustrates a similar figure, in which the outcome is working conditional on not having worked in the prior quarter. Overall, the synthetic control method greatly improves the similarity between the controls and the eligible observations. This allows me to conduct a DDD-type estimation much

more credibly than I could if I simply had to choose all non-eligibles or any *ad hoc* subset of non-eligibles as the within-place-within-period control group.

Figures 4 and 7 in Appendix B, allow us to assess whether the synthetic controls do a good job of replicating the pre-treatment behavior of the eligible individuals. However, we can construct very similar figures, extending the period up through 2010, and get an initial sense of how EI affected behavior. Figure 5 and Figure 6 just do that.

The same procedure was done for the case of men. The comparison of synthetic controls and partners of eligible women are presented in Figures 8 and 9.

For instance, Figure 5 illustrates the proportion working for eligible women and the synthetic control group between 2000 and 2010. The vertical line in the figure is drawn at six months before the program started. Observe that the trends for eligible women and the synthetic controls begin to diverge after 2007, with the eligible women working noticeably more in 2009 and 2010.

6.3 The Estimating Equation and Final Details

In this section, I will describe my exact estimating equation. Before I proceed, I will emphasize a few issues: (i) the important role and usefulness of the synthetic control weights, which allow me to use a DDD-type empirical strategy in a non-standard situation that requires considerably flexibility; (ii) the unsuitability of a standard DDD framework for the variation I have found and more flexible replacement for it that I employ (without losing its essential logic); (iii) other important ways in which I relax typical DDD restrictions.

Recall that the typical DDD estimating equation has a binary intention-to-treat (on/off), a binary treatment period (before/after), and individuals who are either eligible or ineligible (if not permanently than at least constantly over the observation period). My application differs from the standard set-up. The EI intention-to-treat varies continuously over time from 2007 onwards. Every place starts with zero exposure, but each follows its own path toward more exposure. Therefore, the treatment period is not binary –the intention-to-treat is present during 14 quarters. Moreover, within a place, the intention-to-treat in one quarter is not independent of the intention-to-treat in the subsequent quarter. Child care spaces are serially correlated– while this quarter's growth in spaces may be random, an existing space rarely disappears. Finally, eligibility is not a permanent characteristic, even over the period I observe. Because households move in and out of EI eligibility, it is not obvious that there is an effect of being eligible that is truly constant over time within a

place.

Under these circumstances, the synthetic control procedure plays an important role and permits substituting the usual eligibility-by-space interaction (a two-way effect in the typical DDD equation). The usual eligibility-by-place interaction only allows the eligibles to differ from the non-eligibles by a constant that is fixed by their initial difference within a place. Because the synthetic control weights not only substitute the usual eligibility-by-place interaction, they minimize the difference between the treated and untreated groups. With synthetic controls, the weight assigned to each of the 32 ineligible groups is constant across time within a state, for any type of eligible person. However, potential control individuals who are weighted are specific to a metropolitan area, they are refreshed over time as individuals move in and out of eligibility, and –most important– the weights themselves were chosen to produce the best available counterfactual, given the data. This is a much more robust control procedure than a crude pre-treatment difference between treated and control observations.

In a typical DDD equation, there is a two-way eligibility-by-time interaction. For explanatory purposes, let us refer to the eligible group as the *group of interest*, for explanatory purposes. In the typical DDD equation, the two-way interaction is meant to absorb the behavior of the *group of interest* when they are facing no treatment. This is done by contrasting the *group of interest* that actually receives the intent-to-treat (=1) with those who are also part of the *group of interest*, who did not receive the intent-to-treat (=0). This two-way interaction is not necessary in my estimation because it is effectively contained in the $e_i \cdot EIexposure_{mqt}$ variable. That is, in my application, the effect of EI is always identified from the variation within a time in the exposure of eligible individuals. For example, a woman could live in a metropolitan area that receives almost no exposure ($EIexposure \approx 0$) and she acts as a control for a woman who lives in a metropolitan area that receives very generous exposure ($EIexposure \approx 1$). However, my estimation also exploits all the variation in behavior associated with places that have levels of exposure that are intermediate between 0 and 1.

The third two-way interaction in a typical DDD equation is the treated-place-by-after interaction. This is meant to absorb behavior typical of all individuals –not just eligibles– in the places that are treated (binary) in the (single) treated period. Such a term would obviously be too crude for my estimation. Yet, it would also not make sense to allow for a full array of independent interaction terms where each place indicator is interacted with each quarter indicator (or even with

each year indicator). This is because the child care spaces and individuals within each place are serially correlated over time. We know, therefore, that a full array of independent interactions would overcontrol– they would not only absorb all behavior intended to be absorbed by the place-by-after interactions, they would absorb most of the actual effect of EI treatment. Put another way, the place-by-time effects that we want to absorb (because they interfere with our discerning the effect of EI program) are changing, within a place, much more smoothly than EI itself because EI was being rapidly introduced. Recognizing this, I allow for place-by-time effects that are much more flexible than the typical DDD treated-place-by-after interaction, but less flexible than a full array of independent interactions between place and time indicators. Specifically, I allow each state's urban areas to have its own linear time and each state's non-urban areas to have another linear time trend– a total of 63 independent linear time trends. Later, as a specification check, I substitute a full array of state-by-year interactions for the linear time trends. This specification will reduce the precision of the estimates– as expected since valuable variation is absorbed by them. However, they do not materially affect the point estimates.

A final important way in which I relax the typical DDD restrictions is that I allow behavior to vary with finer differences in family composition than just comparing eligible versus ineligible. I relax this restriction because (i) fine variation in household composition really does affect the hours of child care that are needed and the capacity of household members to cover them and (ii)if metropolitan areas are sufficiently small, the distribution of fine household compositions is not the same across all areas within a time or across all times within an area. I allow behavior to vary with fine distinctions in household composition by including two types of variables. First, I define nine different household composition types and include a fixed effect for each type. (Note that these fixed effects subsume the typical eligible main effect.) The nine composition types are described in Table 4 in Appendix B. Second, I include a quadratic in the age of the household's youngest child. This quadratic function is designed to allow families to vary their behavior smoothly, but non-linearly, with the youngest child's needs. For instance, if a mother's labor supply behavior changes dramatically as her youngest child ages from infancy to school age, but thereafter changes more slowly, this quadratic would pick up the behavior. The combination of the quadratic and the fixed effect for each family composition type should almost completely control for the usual behavior of any sort of family.

The equation for estimating the effects of EI on women takes the form:

$$y_{ismqt} = \alpha + f_{iqt} + \delta_t + c_m + coh_i + c_s \cdot t + c_s \cdot u_i \cdot t + e_i \cdot u_i + \theta e_i \cdot EIexposure_{mqt} + \boldsymbol{X_{it}}^T \boldsymbol{\omega} + \epsilon_{imsqt} \tag{6}$$

In Equation 6, y_{ismqt} is the outcome of interest for individual i, in quarter q, in year t who lives in municipality m in state s. The key or triple interaction variable is $e_i \cdot EIexposure_{mqt}$, the only variable that varies by eligibility, place and time. The equation does not show weights but control observations are, in fact, weighted by the synthetic control weights so that they are the best counterfactual that the data can produce for the behavior of eligible individuals. As mentioned above, the estimating equation includes a fixed effect for each family composition type (f_i, this incorporates the main effect of eligible), a linear time trend specific to the rural individuals in each state (captured by $c_s \cdot t$), and a linear time trend specific to the urban individuals in each state (captured by $c_s \cdot u_i \cdot t$). The equation also includes a fixed effect for each municipality (c_m, this is the main effect of place and subsumes state fixed effects), a fixed effect for each cohort (coh_i), and the interaction $e_i \cdot u_i$, which allows eligible individuals in urban areas to behave differently than eligible individuals in non-urban areas, all else being equal.

$\boldsymbol{X}$, the vector of controls, includes the educational attainment of individual i and an indicator for other relatives living in the household (since they might provide child care). $\boldsymbol{X}$ also includes an indicator for the household's having a youngest child under the age of 18 and the interactions between this indicator and the age and the age squared of the youngest child. This is the quadratic function mentioned above.

To estimate the ITT effects of the EI program over the amount of time allocated on child rearing and on housework, I will use individual fixed effects. The reason for using individual fixed effects in these cases is that the response might be varying due to the individual interpretation of the question; however, this interpretation is constant over time for the same individual (Bound and Krueger, 1991). By using individual fixed effects, I reduced the standard errors of the estimates.

7 Results for Intention-to-Treat Effects

In this section, I will discuss the intent-to-treat effects of the EI program estimated in Equation 6 for the unconditional probability of working, for the probability of starting to work, for time

allocation variables, for income, and finally for labor conditions.

The EI exposure index will be normalized in order to measure the effect of 1 additional space for child care available for every 10 children who are eligible for the program (analogously, we could think about the effect of 100 spaces for every 1,000 EI-eligible children in a municipality). This means that the estimated effects will represent a response to having a 10% EI exposure in a municipality. All the effects will be analyzed based on this measure.

Moreover, the sample used consists of those households that have between 16 and 60 years of age. I will exclude from the eligible group those individuals who have a child between the age of 0 and 1 year, since this could be a potentially affected group. The objective is to measure the effects on those individuals who can potentially use the child care service.

7.1 Probability of Working

Table 5 shows the estimated intent-to-treat effect of 1 additional child care space for every 10 children (i.e. $\hat{\theta}$) over women's probability of working. The average effect for the eligible women is an increment of 1.5 pp, which reflects an increase of 4.310% in the proportion of women working. For the population of men who have a child between 1 and 3 years, 11 months old, we observe that there is a reduction of 1.2 pp in the probability of working. Considering the fact that most of the eligible men are working, this finding only reveals a decrease of 1.257% of the baseline observed during the fourth quarter of 2006, where 95.502% of eligible men were working.

At the national level, there were 1.2 spaces created for 10 EI-eligible children. This suggests that women's employment increased by 5.17% over the mean. In the case of men, this finding represents a reduction of approximately 1.50% over the mean.

Moreover, I use a duration model to estimate if more women or men start to work due to the EI program. In particular, I will use a proportional hazard model, using an exponential function and the same covariates used in Equation 6. I present estimates of the hazard ratio, which is the ratio of the hazard rate[12] of working for a person who is living in a municipality with EI exposure of 0.1, over the hazard rate of working without the presence of the program. This empirical strategy will help determine the change in the probability of a person entering the work force. The estimates are presented in Table 6 in Appendix B.

[12]The hazard rate represents the "instantaneous" probability that unemployment ends at t; i.e., the hazard rate is represented by $h(t|X) = h_0(t, \lambda)\phi(\mathbf{X}, \beta)$, where $h_0(t, \lambda)$ is the baseline hazard, and $\phi(\mathbf{X}, \beta) = e^{\mathbf{X}\beta}$.

The difference between a duration model and a simple linear regression of starting work, conditional on not working in the previous period is that the duration model consider decisions over time or the passage of time. That is, it treats the decision to work not as binary change (never working/ always working) but as one that involves decisions through time. Fundamentally, this is a different likelihood function.

I observe an increment of 4.5 pp, representing a 12.931% change over the mean of women entering the work force. Men reduce the probability of working by 5.8 pp, which represents a reduction of 6.073% over the mean.

7.2 Time Allocated to Child Rearing, Housework, and Work

The estimates of θ in Equation 6 using time allocation variables are presented in Table 7. EI-eligible women are reducing the time allocated per week to the child by 54 minutes. This represents a reduction from the average baseline of 4.131%. The effect of the EI program over the average time allocated to housework or work (conditional on working in the previous period) does not vary significantly. This finding suggests that, on average, women are not substituting child rearing for housework. For the case in which women are working, they do not seem to be using this extra time to work more. On the other hand, as expected, the unconditional hours allocated to work increases by approximately 34 minutes.

Table 7 shows that men are allocating less time to child rearing. They reduced by 3.739% the weekly time they used to allocate to child rearing. Furthermore, the results suggest that there is a reduction of approximately of 5% over the mean on time allocated to housework. In addition, there is a small increment in the average time allocated to work, which represents 0.503% with respect to the mean. This finding suggests that, on average, women are reducing the amount of time devoted to child rearing, whereas men are reducing the amount of time spent on both child rearing and housework. The amount of time allocated to work, for the individuals who were working, does not seem to vary substantially in response to the EI program.

7.3 Income and Other Labor Conditions

If women increase the probability of working, then an increase in income should also be expected. The sample is divided into those women who in the previous period were earning a positive amount of income and those who were earning zero income. Results are presented in Table 8 in Appendix

B. On average, there are no significant effects for women's income, conditional on having a positive income. There is a reduction of 0.9 pp in the probability of observing an eligible woman without income. This effect reflects a decrease of 1.266% in the proportion of women receiving no income. In the case of men who were receiving a positive labor income, there is no effect over labor income as a result of the EI program. There is a reduction of almost 5% in the proportion of those men who were receiving no labor income. In Mexico, there is a large proportion of men who work but do not receive a labor income.

Table 9 illustrates that there is no significant change in the probability of switching jobs for men or women. The estimates also suggest that there is a slight reduction (at a 10% level) for women in their probability of working in the uncovered sector.

7.4 EI Effects for the Population of Women Working

Thus far, we have analyzed the ITT effects for the average population. However, women who were already working before the implementation of the EI program have a different response to the availability of subsidized child care than the average population. Table 10 illustrates the ITT effects for women who are working in period t-1. So the estimate of θ in Equation 6 will represent the increase in the probability of *still* working in period t, given that a women was working in period t-1, in response to having an additional child care space. We also observe in Table 10 that there is no statistically significant reduction in the time spent on child rearing. This result was expected, since women who are working and have young children have already arranged to leave their children with someone while they are working.

7.5 Heterogenous Effects by Education Level and Urban/Rural Area

In this section, I will try to ascertain if there is a specific population that is more affected by EI exposure than the average population. First, I focus on a population that has at most a high-school education. One reason to expect that the average effects will be higher are that individuals with a college or more education can earn sufficiently high wages to pay for a private child care, or work in the covered sector where they can use IMSS child care services. In addition, for this population, I estimate the effects on the urban and rural population. The principal difference between these two populations is the average income that households earn. As we observed in the model, women who have a higher non-labor income have a higher reservation wage and are less likely to start

working. In this case, we should expect that the EI program has a higher effect on the proportion of women working in the rural population. On the other hand, if the rural population faces higher constraints to entering the work force, then the effect on the probability of working will be lower. In the seminal work of Boserup (1990), she finds a negative association between development and the proportion of women working. In this case, I will compare women with the same level of education, who experience a similar price reduction in child care between regions with different levels of development.

7.5.1 Women with at Most High-School Education

Table 11 presents the results for the population with at most a high-school education. Comparing these estimates with those presented for the general population, there is no statistical difference except for income. The effect of 1 child care space of the EI program over 10 EI-eligible children generates an increment in income of $74.981 real pesos for the eligible women with at most a high-school education. What could possibly be occurring is that the variance of the residual for this population is smaller; therefore, the estimates are more precise.

7.5.2 Women with at Most High-School Education: Urban vs Rural

Looking at the difference in the effects of the program between the urban and the rural populations shown in Table 12 in Appendix B, there is no statistically significant difference between these two regions in most variables, except for income. I compared both regions by running a regression including both populations and estimating the effect of the EI exposure interacting with an indicator of an urban area, and including the levels of EI exposure and urban area. The only variable on which these two populations diverge is the effect of EI on income. For the purpose of exposure, I present the estimates of regressions run separately.

Both regions reduce the amount of time allocated to child rearing; they increase the proportion of women working, and they present no variation in the amount of time allocated to housework. The primary difference between these two regions is the effect of the EI exposure on income for those women who were already receiving a positive income in the previous period. There is an increment of approximately $167 real 2008 pesos for eligible women living in urban areas, in response to an additional one child care space constructed for every 10 EI-eligible children. This statistic represents an increase of approximately 4.5% over the average income in the urban population of EI-eligible

women. Women do not seem to be switching jobs, and they do not seem to be allocating more time to work. This finding suggests that increments in productivity in urban areas are compensated whereas in rural areas they are not.

8 Pseudo-Treatment-on-the-Treated Effect

The next objective is to learn about the average response of those households that utilized the program. I do not have the information to identify whether households used the program or not, but I can identify those households that decreased the allocation of time devoted to child rearing due to the variation of the EI exposure. These households might not be exactly those who are using the EI program; however, it could be a good approximation by those who are using it. In fact, these households are presumably a subset of the compliers among those who received the EI child care subsidy.

I am going to define *pseudo-treated* households as those in which the woman is eligible and reduced the amount of time allocated to child rearing by at least 10 hours. This measure represents, on average, almost a 50% reduction in child rearing, since EI-eligible women allocate approximately 21 hours per week to child rearing. A 2SLS procedure will be used, in which the first stage will be the equation used to identify the intent-to-treat effect of the EI exposure, and the treatment will be defined as those households in which the women reduced the total amount of time allocated to child rearing by at least 10 hours from one period to the other.

The cut-off of 10 hours was chosen because it was the average reduction in time encountered by those women that diminished the amount of time spent on child rearing as a result of the EI program. The average reduction in time for those women who reduce their time spent on child rearing in response to the EI exposure is estimated using the following 2SLS procedure:

First Stage:

$$\begin{aligned}\mathbb{I}_{ismqt}^{reduction\,time\,child} = \quad & \alpha_i + f_{iqt} + \delta_t + c_s \cdot t + c_s \cdot u_i \cdot t + e_{iqt} \cdot u_i + \theta\, e_i \cdot EIexposure_{mqt} \\ & + \boldsymbol{X_{it}}^T \boldsymbol{\omega} + \epsilon_{1,imsqt}\end{aligned} \tag{7}$$

Second Stage:

$$TimeChildRearing_{ismqt} = \alpha_i + f_{iqt} + \delta_t + c_s \cdot t + c_s \cdot u_i \cdot t + e_{iqt} \cdot u_i + \beta\, \mathbb{I}^{reduction\, time\, child}{}_{ismqt} + \boldsymbol{X_{it}}^T \boldsymbol{\omega} + \epsilon_{2,imsqt} \tag{8}$$

where $\mathbb{I}^{reduction\, time\, child}_{ismqt}$ is an indicator function, which takes the value of 1 when a woman reduced the time allocated in time spent on child rearing, and $TimeChildRearing$ is a variable measuring the hours per week allocated to child rearing. This specification should capture the *Local Average Treatment Effect* (LATE), reflecting the average reduction in child rearing for the subset of women induced by the change in the instrument (EI exposure) to reduce the amount of time allocated to child rearing. The result for this estimation is displayed in Table 13, and shows an approximate reduction for the amount of time allocated to child rearing of 10 hours. The instrument used is not weak, since the F-test statistic for the first stage regression is 83.71.

In order to estimate the *pseudo-treatment* on the treated, I use Equations 9 and 10:

First Stage:

$$\mathbb{I}^{\geq 10hr\, Reduction\, on\, Child\, Rearing}_{ismt} = \alpha_i + f_{iqt} + \delta_t + c_s \cdot t + c_s \cdot u_i \cdot t + e_{iqt} \cdot u_i + \theta\, e_i \cdot EIexposure_{mqt} + \boldsymbol{X_{it}}^T \boldsymbol{\omega} + \epsilon_{3,imsqt} \tag{9}$$

Second Stage:

$$y_{ismqt} = \alpha_i + f_{iqt} + \delta_t + c_s \cdot t + c_s \cdot u_i \cdot t + e_{iqt} \cdot u_i + \gamma\, \mathbb{I}^{\geq 10hr\, Reduction\, on\, Child\, Rearing}_{ismt} + \boldsymbol{X_{it}}^T \boldsymbol{\omega} + \epsilon_{4,imsqt} \tag{10}$$

where y_{ismt} is the outcome variable of interest; and $\mathbb{I}^{\geq 10hr\, Reduction\, on\, Child\, Rearing}_{ismt}$ represents a dummy variable, which takes the value of 1 whenever the women reduces child rearing by 10 hours or more. As discussed earlier, individual fixed effects were employed to estimate the ITT over time allocated to child rearing since different individuals could respond differently to this type of question, but they will respond consistently compared to themselves in different periods of time. Therefore, in the second stage, the fixed effects will be added as well.

I use the fact that the EI exposure affects the reduction in the amount of time allocated to child rearing. I am assuming that $\mathbb{I}^{\geq 10hr\, Reduction\, on\, Child\, Rearing}_{ismt}$ is an endogenous variable in Equation 10. The actual usage of child care services may be endogenous for a number of reasons. For example,

there might be an omitted variable like taste for work, that will determine the option of using child care services. Moreover, I will assume that EI exposure does not affect the outcome variable directly; i.e., $e_i \cdot EIexposure_{mqt}$ is uncorrelated with $\epsilon_{4,imsqt}$.

Furthermore, I estimate the effect of a 1 hour reduction in the total number of hours on child rearing for women. However, the discussion of the results will be centered on the estimated effect for the *pseudo-treated* households. If most of the variation in the amount of time allocated to child rearing associated with higher EI exposure, results from reduction in the amount of time on child rearing by less than 10 hours, then we would not observe an effect on the *pseudo-treated* households.

8.1 Probability of Working

The estimates of γ of Equation 10 are presented in Table 14. These results suggest that eligible women who reduced the amount of time allocated to child rearing by more than 10 hours in response to the availability of 1 extra child care space, increase the probability of working by 8.4 pp. This represents an increment over the mean of 24.138%.

In the case of men, there does not seem to be any effect on the proportion of men working. The result of the intent-to-treat estimate is not reflected in the *pseudo-TOT*. The *pseudo-TOT* on men, only captures the effect of those men who live with women who have reduced some amount of time spent on child rearing. The major possibility of finding a discrepancy between the ITT and the *pseudo-TOT* in the probability of working for men originates from the fact that those men who are not working, live with women who are already working. However, those women who are already working do not present a significant reduction in the amount of time allocated to child rearing.

8.2 Time Allocated to Child Rearing, Housework, and Work

Table 15 depicts the results for the allocation of time. Considering the ITT and *pseudo-TOT* estimates, it seems that women do not modify their allocation of time to housework at all. We also observe in Table 15 that a reduction in the amount allocated to child rearing for more than 10 hours per week increases the number of hours worked per week by 9.984 hours. This finding corresponds to an increment over the mean of 85.295%. This population of EI-eligible women is not restricted to those who were working in the previous period.

On the other hand, men are not modifying the number of hours worked per week. However, they are decreasing the amount of time allocated to child rearing when their spouse is reducing the

total amount of time spent on child rearing. Men living in *pseud-treated* households reduce their time allocated to child care by approximately 1 hour 50 minutes.

On the other hand, I do not find strong evidence for the fact that the number of hours spent on housework are reduced for the men living in a *pseudo-treated* household. The estimate suggests that there is a reduction of approximately 1.9 hours per week on housework, but the estimate is not statistically significant at a 5% level. However, if I estimate the effect of a 1-hour reduction in the amount of time a woman spends on child rearing, I do observe a reduction in the amount of time allocated to housework at a 5% significant level. Why do we observe an increment in the variance for the number of hours allocated to housework in the case of men? When a woman substantially reduces the number of hours spent on child rearing from one period to the other, some men might respond by increasing the number of hours spent on housework.

8.3 Income and Other Labor Conditions

Men seem to be reducing the allocation of their time devoted to parental child care, whereas there is no effect on the amount of time allocated to work. I did not find strong evidence that there is variation in the amount of time allocated to housework. One possible explanation for this finding might be the fact that men are increasing their leisure time slightly. However, according to Table 16, it is observed that men are actually increasing the probability of switching to other jobs and to better-paid jobs. There is an increment of 42 pp in switching to better paid jobs for the *pseudo-treated* household. Therefore, in the short-run, men might be devoting their extra-time to look for other jobs.

Why is there an increment in the probability of switching to better-paid jobs and no significant effect for labor income in the case of men in *pseudo-treated* households? Two possibilities explain these results. One is that I am observing a short-term effect on labor income. It is important to recall the definition of switching jobs. Those men who switched jobs are changing economic activities from one period to the next, or leaving one quarter in between without working. As a result, some of them might be experiencing an increment in income one period after their partner reduced the time spent on child rearing by more than 10 hours per week. Another possible explanation for observing men switching to better-paid jobs and not observing an increment in income is that not all men will find a better-paid job, when taking the risk to look for a better paid job. According to my estimates, half of them are obtaining better-paid jobs. Therefore, some of them who are not

getting a better-paid job might be reducing their labor income.

In the case of *pseudo-treated* women according to Table 16, they earn \$830.990 real 2008 pesos more, representing a 20% increase over the mean. Women who reduce the amount of child rearing by more than 10 hours have a lower probability of switching jobs. These types of women also lowered the probability of being uninsured, while there is no effect on the probability of becoming self-employed. This result suggests that women who decide to enter the work force due to the EI program are not becoming self-employed, and some of them are getting jobs in the formal sector.

Table 17 presents the estimates for the *pseudo-treatment-on-the-treated* effect on the joint labor incomes of men and women. These estimates suggest that in the short-run the joint income of the household does not increase. Therefore, even if we observe an increment in the income of the women, we do not observe an increment in the joint household for those *pseudo-treated* households.

9 Sources of Variation in EI

In order to identify the effects of the EI program, I use variation across place, time and eligibility condition. Some of this variation might be correlated to the variation observed in female labor supply of the EI-eligible women. Other sources of variation in the EI exposure might not be correlated to the female labor supply at all. The purpose of this section is to provide some suggestive evidence that the variation in the EI exposure do not seem to be particularly problematic.

If the allocation of the EI child care spaces was mainly driven by those states in which the proportion of eligible women working was increasing, then I might be worried about a reverse causation problem. Consider a case in which there are some regions in which young women's labor is demanded. Some of these women might enter to the labor force, but some of them might not because they are constrained by the amount of time they have to allocate to child rearing. When the EI program starts, they might be lobbying more for child care spaces than other women do. In this way, we could potentially observe a higher EI exposure associated with higher labor participation weights for eligible women. As we will see in this section, we will not observe this type of association in the data. I will be testing whether the growth rates of the participation of women at a state level are correlated with the allocation of the program. Trends using normal weights will be used.

I could also imagine that those regions with higher levels of GDP or higher growth rates could

be associated with higher EI exposure. If richer regions have higher participation rates of women working, then I could think of a situation in which in these places EI-eligible women demanded more child care spaces. Under that scenario, eligible women increased their labor participation not because of the availability of the EI child care services *per se* but because those regions were experiencing growth rates, and their labor supply was just responding to such macro-economic factors. In this way, I will also test whether states that have higher levels or higher growth rates of GDP are getting higher EI exposure.

I will also test if urban populations received relatively more EI child care spaces than rural populations. Urban areas might be better suited to expanding child care services than rural areas. Group of individuals who want to create and join the EI network might have higher incentives to start this type of service in urban areas since there are more educated individuals with more resources to satisfy the requirements of providing EI services.

Variation in the EI exposure might be also associated with factors that are not necessarily correlated to the labor supply. For example, the federal government might be allocating resources across states where they do not differ drastically, for political reasons. In this way, smaller states in terms of population will receive higher EI exposure than larger states. It will be tested whether states that have smaller populations have higher EI exposure.

Elections could be another important source of allocation to the program. The program is managed at the federal level. Therefore, it could be that before elections at a municipality or state level more resources are allocated to the region where the mayors of a municipality are from the same party as the federal government. In order to obtain a higher proportion of votes in that region, they could start allocating more resources to that region in the form of social programs.

In order to test if the allocation of the EI program is correlated to the sources of variation mentioned above, the following specification will be run:

$$\begin{aligned} EIexposure2007.Q4_{su} = \alpha + \theta_1 GrowthRateWorkingEligibleWomen_{su} + \\ \theta_2 GrowthRateWorkingNonEligibleWomen_{su} + \\ \theta_3 lnGDP_s + \theta_4 GDPgrowth_s + \theta_5 PropPopulation_{su} + \\ \theta_6 Elections_{su} + \theta_7 Urban_{su} + \psi_{su} \end{aligned} \tag{11}$$

where s indicates the state, and u indicates if it is an urban or rural areas. First, every state was divided between urban and rural area. For every state at the urban-rural level, the averages of the proportion of eligible and separately, those for non-eligible women, were computed. The variable $EIexposure2007.Q4_{su}$ refers to the EI exposure index observed in the fourth quarter of 2007. This quarter is where we observe the most drastic allocation of the program.

The average change in percentage points between quarters of the proportion of eligible and non-eligible women working were computed, and are represented by $GrowthRateWorkingEligibleWomen_{su}$ and by $GrowthRateWorkingNonEligibleWomen_{su}$. These variables take a value between 0 and 1. I separated the growth rates observed using the sample between 2000.Q2 and 2004.Q4 (ENE data) and the sample between 2005.Q1 and 2010.Q2 (ENOE data) since the phrasing of some questions defining if a person is working changed. This means that I am trying to avoid the "seam" problem of joining the two data sets. The variable $lnGDP_s$ refers to the logarithm of the GDP. $GDPgrowth_s$ is a variable measuring the GDP growth measured as the first difference of the logarithm of GDP. These two variables that are functions of GPD only vary at the state level. In order to measure if smaller states in terms of population are receiving higher EI exposure, the variable $PropPopulation_{su}$ was created. This variable takes a value between 1 and 100, and it represents the percentage of individuals living in the region specified, with respect to the total Mexican population. The variable $elections_{su}$ measures the weighted average of municipalities that were in a period of elections where the former Mayor of that municipality was from the same party as that of the federal government. A period of elections in a municipality is considered to be three quarters before elections until the quarter in which that municipality held elections. Finally, the variable $Urban_{su}$ is a dummy variable that takes the value of 1 whenever the region considered is an urban region.

As can be seen in Table 18 there are statistically significant correlations between the EI exposure index and the urbanization area and the proportion of the population. Urban areas are getting considerably higher levels (approximately 11 pp more) of spaces for child care relative to the number of children eligible for this program. For every extra percentage point a region is larger than the average population, there is a reduction of 1.3 pp of intensity level.

It is important to observe that there is not a significant correlation between the average change in the proportion of eligible women working and the intensity of the EI program. The estimates also suggest that the positive correlation observed is small; for instance, for every 1 pp increase in

the growth of the proportion of women working; there is an increment of 0.03 pp in the intensity level. The growth rates for the non-eligible women are also not significantly correlated with the allocation of the EI program.

In addition, there is evidence to suggest that there is no correlation with the levels of GDP or the growth of GDP of the states. There is some suggestion is that the EI program was allocated more intensively to those regions that experienced lower GDP growth rates.

10 Robustness Checks

10.1 Placebo Test

In order to assess if the estimations capture responses to the allocation of the EI program and do not fail to capture some natural trends in the proportion of women working in those regions where there was a higher exposure of child care services, I will consider a hypothetical case, in which the EI program was implemented in the first quarter of 2001. This placebo test will consist of using Equation 6, which estimates the ITT effects, though the program had started in the first quarter of 2001, and assess if we observe a significant coefficient for the variable $e_i \cdot EIexposure$. The period used for this estimation encompassed 2000.Q1 to 2004.Q2. The rationale for choosing this period was that it allowed me to start in the first quarter of a year and use all the data of the EI exposure index; i.e. 14 periods or quarters. However, selecting different periods generates similar results.

I will use the ENE data and the same synthetic weights constructed in this paper. I will control for the allocation of the IMSS program, i.e. I will use Equation 6 and include the interaction of eligibility and IMSS exposure index. Analogous, to the EI exposure index, the IMSS exposure index represents the ratio of the number of child care spaces provided by IMSS in a certain municipality, over the number of children whose mothers are eligible to use the IMSS child care service. [13] I control for the IMSS exposure in this scenario because during this period there was an expansion of the IMSS child care services program.

Unfortunately, the ENE data do not contain information about the time allocated to child rearing or housework. Therefore, I will perform this placebo test over the probability of working and the number of hours worked per week. Table 19 illustrates the results for the ITT estimates in

[13] Mothers who are eligible to use the IMSS child care service are those who are working in the covered sector.

the case of women using the ENE data, as though the EI program had been allocated in the first quarter of 2001. We can observe that there is no economic or statistical effect on the proportion of women working. We also do not observe any statistically significant increment in the unconditional hours worked for women. This finding suggests that the ITT estimates are capturing effects of the EI program and are not capturing pre-trends of the proportion of women working that were correlated to those regions in which they had more exposure to the program.

10.2 Controlling for IMSS Exposure

A second test is to assess if the EI Exposure captures confounding effects with other important programs related to child care services: the IMSS program. During the period of 2005.Q1 and 2010.Q2, there were few new child care spaces created by the IMSS program (less than 2,500). However, there is a possibility that the ITT effects of the EI program could be capturing effects of the IMSS program. The estimates listed in Table 20 are the estimates using Equation 6 including the interaction of eligibility and IMSS exposure index. Table 20 illustrates two important facts: i) the ITT estimates controlling for the availability of IMSS child care services are very close to those presented in my main results in Table 5 and Table 7 where I did not control for the IMSS exposure index, and ii) the estimates suggest that between 2005.Q1 and 2010.Q2, the IMSS child care program did not affect the probability of working, switching jobs, hours worked, time allocated to child rearing or to housework.

11 Conclusions

In this paper, I showed that the provision of child care services in Mexico, generated an increase in the proportion of women working, and a decrease in the proportion of men working, between 16 and 60 years of age in those households eligible for the EI program. What was the effect of the EI program at the national level by the second quarter of 2010? At the national level, the government allocated on average 1.2 spaces for every 10 EI-eligible children. The intent-to-treat estimates suggest that women with an additional EI exposure of 0.12 increase their probability of working by 1.8 pp, which represents an increment of 5.17% over the mean. I found that both men and women reduce the amount of time allocated to child rearing. However, I did not find a reduction in the time devoted to child rearing for those women who were eligible for the EI program

and were already working.

If the government's objective was to increase gender equality in terms of income and labor force participation, it seems that this objective has been achieved. The effect of increasing the proportion of women working at the national level of the EI program is reasonable, but not profound. Approximately 8 out of 100 women who reduce child rearing by more than 10 hours are starting to work. There are still a considerable number of women who are reducing their time on child rearing and are not entering the work force. Women who were already working and have less than a high-school level of education (which presumably is the most affected population) are also benefiting from the program. They do not modify their initial time allocations to work, child rearing or housework, but they do increase their labor income. Some of the burden of child care was reduced for this type of women, reflected in their productivity.

One important downside of this program is that the burden is increasing for those women with a husband who was not working. As the theory predicted, when a man faces an increase in his non-labor income generated by his wife's labor income, his reservation wage increases. The ITT estimates suggested that every additional child care space for every 10 EI-eligible children reduces the probability of working by 1.2 pp. Since most men are working, this represents only a 1.2% reduction over the mean. For the case of households, where the man is working, some of the men are motivated to switch jobs and find better-paid jobs, although, I did not find a significant increase in their labor income in the short-run. In the long-run, we might expect higher earnings for the men as well.

Finally, women who reduced the amount of time in child rearing by one hour increased monthly labor income by \$72 real 2008 pesos. The *pseudo-treated* women (those who reduced the amount in child rearing by more than 10 hours) increased their labor income by \$831 real 2008 pesos. This sum represents almost 20% of the average income of EI-eligible women. Nonetheless, I did not find an increase in the total labor household income (the sum of men's and women's labor income). Some of the men who live with *pseudo-treated* women are switching jobs; some of them are finding better-paid jobs, but others are not. In the short-run, taking this type of risk might not pay-off, but in the long-run it probably will, and it will be reflected in the household income. Therefore, the EI program is not just benefiting the women but it is generating benefits for the men as well. Such benefits were probably unintended.

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Appendix A- Implications of First Order Conditions of the Model

The maximization problem is:

$$\max_{t_i^{child}, t_i^{work}, t_i^{search}} \quad \theta U(c, l_m, l_w) + (1-\theta) Q_{ch}(t_c^{child}) \tag{12}$$

subject to:

$$\begin{aligned}
t_m^{work}(\underline{w}_m + f(t_m^{search})) + t_w^{work}(\underline{w}_w + f(t_w^{search})) \\
-c - (t^{child} - t_m^{child} - t_w^{child})p^{ch} &\geq 0 && (13)\\
1 - t_w^{work} - t_w^{search} - t_w^{child} - l_w &\geq 0 && (14)\\
1 - t_m^{work} - t_m^{search} - t_m^{child} - l_m &\geq 0 && (15)\\
t_i^{child} &\geq 0 && (16)\\
t_i^{work} &\geq 0 && (17)\\
t_i^{search} &\geq 0 && (18)\\
t_m^{child} + t_w^{child} &\leq 1 && (19)
\end{aligned}$$

for $i = 1, 2$.

As the returns to search are discontinuous the maximization problem need not be well behaved. Solving the first order conditions may not then yield a optimum. However, we can deal with this problem in the following way. As argued in the main text, the man will only over choose $t_m^{search} \in \{0, \bar{t}^{search}\}$. We thus consider two separate maximization problems. One with a fixed wage for the man $\underline{w}_m$ and an exogenously specified $t_m^{search} = 0$, and a second one with a fixed wage $\underline{w}_m + w^P$ and an exogenously specified $t_m^{search} = \bar{t}^{search}$. A third possible case in which both the man and woman engage in search is not considered, but the analysis would readily extend to this case too.[14]

[14] It will never be optimal for $t_w^{search} = \bar{t}^{search}$ and $t_m^{search} = 0$.

11.1 Case 1

Considering the first case ($t_m^{search} = 0$). As utility is everywhere increasing in consumption and leisure, the budget constraint and time constraints must bind. Thus:

$$
\begin{aligned}
c &= t_m^{work}\underline{w}_m + t_w^{work}\underline{w}_w - (1 - t_m^{child} - t_w^{child})p^{ch} && (20)\\
l_w &= 1 - t_w^{work} - t_w^{child} && (21)\\
l_m &= 1 - t_m^{work} - t_m^{child} && (22)
\end{aligned}
$$

Substituting the above expressions into the objective function the problem simplifies to:

$$
\max_{t_i^{child}, t_i^{work}} \quad \theta U(c, l_m, l_w) + (1-\theta)Q_{ch}(t_c^{child}) \tag{23}
$$

subject to:

$$
\begin{aligned}
t_i^{child} &\geq 0 && (24)\\
t_i^{work} &\geq 0 && (25)\\
t_m^{child} + t_w^{child} &\leq 1 && (26)
\end{aligned}
$$

for $i = 1, 2$.

where c, l_m and l_w are as given above.

Setting up the Lagrangian, ($\mathcal{L}$), let λ_w^{child}, λ_m^{child}, λ_w^{work}, λ_m^{work} and λ_h^{child} be the Lagrange multipliers. The first order conditions are then:

$$\frac{\partial \mathcal{L}}{\partial t_w^{child}} = \theta\left(\frac{\partial U}{\partial c}p^{ch} - \frac{\partial U}{\partial l_w}\right) + (1-\theta)\frac{\partial Q_{ch}(t_m^{child}+t_w^{child})}{\partial t_m^{child}+t_w^{child}} + \lambda_w^{child} - \lambda_h^{child} = 0 \tag{27}$$

$$\frac{\partial \mathcal{L}}{\partial t_m^{child}} = \theta\left(\frac{\partial U}{\partial c}p^{ch} - \frac{\partial U}{\partial l_m}\right) + (1-\theta)\frac{\partial Q_{ch}(t_m^{child}+t_w^{child})}{\partial t_m^{child}+t_w^{child}} + \lambda_m^{child} - \lambda_h^{child} = 0 \tag{28}$$

$$\frac{\partial \mathcal{L}}{\partial t_w^{work}} = \theta\left(\frac{\partial U}{\partial c}(\underline{w}_w + f(t_w^{search})) - \frac{\partial U}{\partial l_w}\right) + \lambda_w^{work} = 0 \tag{29}$$

$$\frac{\partial \mathcal{L}}{\partial t_m^{work}} = \theta\left(\frac{\partial U}{\partial c}(\underline{w}_m + f(t_m^{search})) - \frac{\partial U}{\partial l_m}\right) + \lambda_m^{work} = 0 \tag{30}$$

11.2 Case 2

If instead $t_m^{search} = \bar{t}^{search}$ the problem would be analytically equivalent to Case 1. Specifically, the parameter values can be adjusted to yield a problem of the same form. Reduce the total time available to the man to $1 - \bar{t}^{search}$ and reset the man's baseline wage to $\underline{w}_m + w^P$. Thus as long as the man's search decisions remains constant the problems are identical. The search decisions only change the problem when the man optimally changes his search decision. We thus consider this change in the man's search decision separately from the other changes. For concreteness the following analysis is undertaken assuming that $t_m^{search} = 0$.

11.3 Regime 1: Specialization

For sufficiently high child care costs, all child care must be parental such that $t_w^{child} + t_m^{child} = 1$. Recall that: (i) each person has total time of 1; and (ii), by the INADA condition, as consumption of leisure goes to zero the marginal utility from leisure goes to infinity. It immediately follows that $t_i^{child} < 1$, $i = m, w$. Thus $t_w^{child} \in (0,1)$ and $t_m^{child} \in (0,1)$. Furthermore, as $\underline{w}_m > \underline{w}_w$, it will be shown that $t_w^{work} = 0$. The intuition for this result is simple. The man and woman are prefect substitutes in the production of child care but the man has an advantage working (as he receives a higher baseline wage). Thus the man always has a comparative advantage working rather than producing child care. As argued in the main text the man must do some child care, so the woman cannot work in equilibrium. I now make this intuition formal.

Suppose (in contradiction) that $t_w^{work} = \alpha > 0$. This implies that $t_w^{child} < 1 - \alpha$ by the woman' time constraint. Thus, as paid child care is prohibitively expensive, $t_m^{child} \geq \alpha$. Total household income will then be bounded from above by $Y_1 = \alpha(w_w) + t_m^{work}(w_m)$. As $w_w = \underline{w}_w < w_m = \underline{w}_m$,

setting $t_w^{work} = 0$, increasing t_w^{child} by α, decreasing t_m^{child} by α and increasing t_m^{work} by α increases total household income while respecting all time constraints.[15]

For very high child care costs, if the woman spent a positive amount of time working, the household would increase its income while maintaining the quality of child care. Thus the woman cannot work a positive amount of time in equilibrium (under the assumption that $\underline{w}_w < \underline{w}_m$) with very high child care costs.

To summarize, we have shown that when child care costs are sufficiently high, then in equilibrium, $t_w^{work} = 0$, $t_w^{search} = 0$, $t_w^{child} > 0$ and by the INADA conditions on leisure $l_w > 0$ and $l_m > 0$. As $t_w^{child} + t_m^{child} = 1$, $t_m^{child} > 0$ and by the INADA conditions on consumption $t_m^{work} > 0$. This characterizes the first regime which applies whenever child care costs are sufficiently high for no child care to be used.

As the price of child care falls, it will become profitable for the man and woman to start using child care. To see when this occurs, we can examine the first order conditions of the Lagrangian. By complementary slackness, we know that at the boundary point between regime 1 and regime 2 that: $\lambda_w^{work} > 0$, $t_w^{work} = 0$, $\lambda_w^{child} = 0$, $t_w^{child} > 0$, $\lambda_m^{work} = 0$, $t_m^{work} > 0$, $\lambda_m^{child} = 0$, $t_m^{child} > 0$, $\lambda_h^{child} = 0$ and $t_m^{child} + t_w^{child} = 1$. This last complementary slackness condition is not generally satisfied but is satisfied at the boundary point where consumption of child care first starts.

Consider the man's work choice. From equation 28 we have that:

$$\frac{\partial U}{\partial c} p^{ch} + \frac{(1-\theta)}{\theta} \frac{\partial Q_{ch}(1)}{\partial t_m^{child} + t_w^{child}} = \frac{\partial U}{\partial l_m} \tag{31}$$

From equation 30 we have that:

$$\frac{\partial U}{\partial c}(\underline{w}_m) = \frac{\partial U}{\partial l_m} \tag{32}$$

Combining equations 31 and 32 we get that:

[15] $w_w > w_m$ is only possible if the woman allocates time to searching and the man allocates no time to searching. In this case consider the deviation in which: $t_w^{work} = 0$, $t_w^{search} = 0$, t_w^{child} increases by $\alpha + \bar{t}^{search}$, t_m^{child} decreases by $\alpha + \bar{t}^{search}$. This change respects all time constraints and results in an increase in income.

$$p^{ch}_{crit} \equiv \underline{w}_m - \frac{(1-\theta)}{\theta}\frac{\frac{\partial Q_{ch}(1)}{\partial t^{child}_m + t^{child}_w}}{\frac{\partial U}{\partial c}} \tag{33}$$

This condition identifies the boundary child care price. Above this price the household consumes no child care, but below this price the household begins to consume child care. This boundary is higher such that child care consumption occurs earlier when the man's baseline wage is higher and when the costs of substituting parental care for hired child care are low.

We now consider how the household uses the additional time they receive once they start to use child care. Using the complementary slackness conditions and combining them with equations 27 and 28 we get that:

$$\begin{aligned}
\theta\left(\frac{\partial U}{\partial c}p^{ch} - \frac{\partial U}{\partial l_w}\right) & \\
+(1-\theta)\frac{\partial Q_{ch}(t^{child}_m + t^{child}_w)}{\partial t^{child}_m + t^{child}_w} + \lambda^{child}_w &= \theta\left(\frac{\partial U}{\partial c}p^{ch} - \frac{\partial U}{\partial l_m}\right) \\
&\quad +(1-\theta)\frac{\partial Q_{ch}(t^{child}_m + t^{child}_w)}{\partial t^{child}_m + t^{child}_w} + \lambda^{child}_m \\
\frac{\partial U}{\partial c}p^{ch} - \frac{\partial U}{\partial l_w} &= \frac{\partial U}{\partial c}p^{ch} - \frac{\partial U}{\partial l_m} \\
\frac{\partial U}{\partial l_w} &= \frac{\partial U}{\partial l_m} \qquad (34)
\end{aligned}$$

An implication of this condition is that, by the symmetry of leisure assumption, the man and woman consume the same amount of leisure: $l_w = l_m$. However, consider the change in the man's time spent working. From Equation 30:

$$\frac{\partial U}{\partial c}\underline{w}_m = \frac{\partial U}{\partial l_m} \tag{35}$$

Thus consumption and the man's leisure time must move in the same direction. At the boundary point where $t^{child}_m + t^{child}_w = 1$, a decrease in the price of child care results in households having more time to allocate to leisure and work. The above equation then ensures that the man's labor supply increases so that both consumption and leisure can increase. However, away from the boundary

point where child care is being consumed before the child care price decrease, it is possible for the man to supply less labor following an reduction in the price of child care. Total expenditure on child care can decrease following the price reduction yielding higher consumption without the man working more.

Ambiguous predictions in the theory mean that I need to look at the data in order to determine the response of the husbands.

11.4 Regime 2: Possible Labor Supply by the Women

As the cost of child care continues to fall, the man may no longer undertake any child care.[16] Once this occurs the woman may begin to supply labor. Specifically, the woman will supply labor when the price of child care falls below the following level:

$$p^{ch}_{crit2} \equiv \underline{w}_w - \frac{(1-\theta)}{\theta}\frac{\frac{\partial Q_{ch}(t^{child}_w)}{\partial t^{child}_w}}{\frac{\partial U}{\partial c}} \tag{36}$$

From Equation 36 we know that women who have a relatively high baseline wage or a low contribution to child quality will enter to the labor force without p^{ch}_{crit2} to be that low. However, if the baseline wage is extremely low then the price of child care should be considerably low.

11.5 Search

As I explained before, when p^{ch} drops, men can either increase or decrease their labor supply. If men increases labor supply, then there exists a point where the man has reduced sufficient time on child rearing and the man decides to search for a new job. It is easy to see that the man will always choose to allocate no time to search or $\bar{t}^{search}$ time to search.

Which search effort will the man choose? Suppose that man allocates total time $t^{job}_m \equiv t^{search}_m + t^{work}_m$ to working and searching. The man will then spend time $\bar{t}^{search}$ searching if and only if:

[16] The child quality function will determine whether this occurs or not. Observing Equation 33 and noting that p^{ch} cannot be negative, it might be optimal at some point to allocate $t^{child}_m = 0$.

$$
\begin{aligned}
(t_m^{job} - \bar{t}^{search})(\underline{w}_m + w^P) &\geq t_m^{job}\underline{w}_m \\
t_m^{job} &\geq \bar{t}^{search}\left(1 + \frac{\underline{w}_m}{w^P}\right)
\end{aligned}
\tag{37}
$$

This means, the men will allocate time to search whenever the time left for working times the sum of the baseline wage and the wage premium is higher or equal than the benefits of working with a baseline wage could give. Thus the man will choose to search once he would allocate sufficient time to working anyway. Men may therefore begin searching as the time they spend in parental child rearing decreases. This may therefore be a consequence of reducing the price of child care. However, it should be noted that this is not a strong prediction of the model. There are also parameter values for which the man will never choose to search and other parameter values for which the man will always choose to search.

Appendix B

Table 1: Average Child Care Expenditures Shown as the Proportion of Income, by Deciles

Household Income Decile	2006	2008
1st	12.4%	14.7%
2nd	7.1%	8.5%
3rd	5.3%	6.4%
4th	4.2%	5.2%
5th	3.5%	4.2%
6th	2.8%	3.5%
7th	2.2%	2.8%
8th	1.8%	2.3%
9th	1.3%	1.7%
10th	0.6%	0.7%

Source: ENIGH, 2006 and 2008.

Table 2: Summary Statistics

Variables:	Eligible Women	Ineligible Women	Eligible Men	Ineligible Men
working	0.348	0.475	0.955	0.928
	(0.007)	(0.003)	(0.003)	(0.002)
working conditional on not working in t-1	0.117	0.166	0.745	0.522
	(0.006)	(0.004)	(0.037)	(0.015)
self-employed	0.293	0.319	0.285	0.359
	(0.011)	(0.004)	(0.007)	(0.003)
uninsured	0.754	0.762	0.668	0.691
	(0.010)	(0.004)	(0.007)	(0.003)
switching jobs	0.049	0.073	0.207	0.197
	(0.003)	(0.001)	(0.006)	(0.002)
monthly labor income (pesos)	$ 4,195.272	$ 4,407.187	$ 5,776.070	$6,243.626
	(120.996)	(47.305)	(93.726)	(54.902)
(in US dollars)	(US315)	(US331))	(US434)	(US460)
hours worked conditional on working in t-1	35.526	37.306	46.691	46.277
	(0.576)	(0.198)	(0.317)	(0.147)
time child rearing	21.811	7.155	4.974	1.467
	(0.222)	(0.077)	(0.124)	(0.037)
time housework	30.163	30.466	3.527	4.006
	(0.178)	(0.091)	(0.081)	(0.041)

Source: ENOE data, 2006.Q4

Note 1: Eligible women are those who have at least one child under the age of four, are heads of household or spouses of heads of household, are between 16 and 60 years of age. Eligible men are those who live with an eligible women and satisfy the same characteristics as the eligible women except for gender. Non-eligible women and men are head of households or spouses of heads of household who do not fall into either of the above categories
Note 2: Variables related to time (hours worked, time allocated to child rearing and housework) are measured as hours per week. Labor income variables are deflated to 2008 pesos. The first five variables are dummy variables indicating if the person is working, if she or he is working in the quarter observed but not in the previous quarter, she or he is self-employed conditional on working, if she or he is uncovered by social security conditional on working, and if she or he is switching jobs. Note 3: The exchange rate for 1 US dollar is $13.31 pesos on October, 2011.

Figure 1: EI Exposure Index of the EI program by State

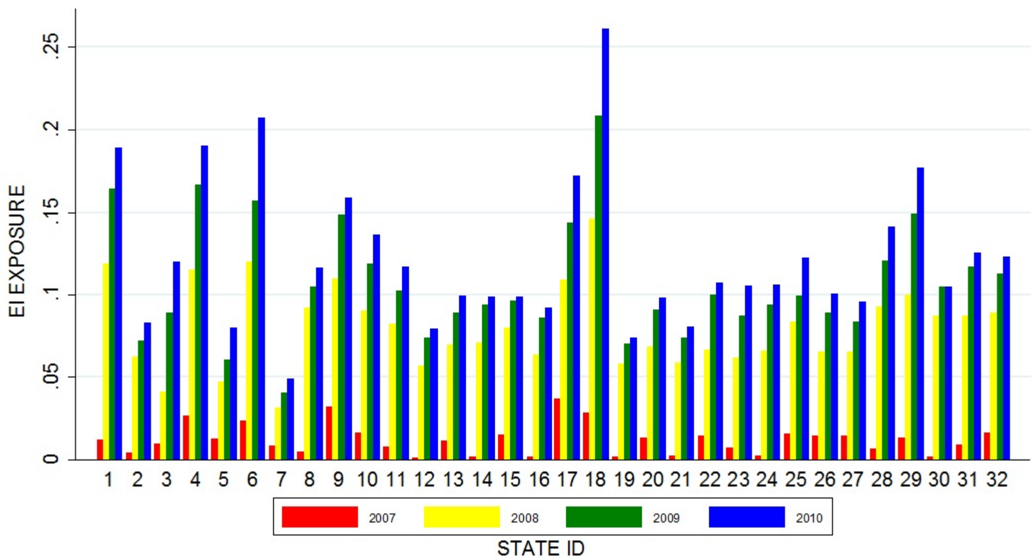


Table 3: Mexican State Identifiers

State ID	State
1	Aguascalientes
2	Baja California
3	Baja California Sur
4	Campeche
5	Coahuila
6	Colima
7	Chiapas
8	Chihuahua
9	Distrito Federal
10	Durango
11	Guenajuato
12	Guerrero
13	Hidalgo
14	Jalisco
15	Mexico
16	Michoacan
17	Morelos
18	Nayarit
19	Nuevo Leon
20	Oaxaca
21	Puebla
22	Queretaro
23	Quintana Roo
24	San Luis Potosi
25	Sinaloa
26	Sonora
27	Tabasco
28	Tamaulipas
29	Tlaxcala
30	Veracruz
31	Yucatan
32	Zacatecas

Table 4: Family Type Based on the Number and Age of their Children

Family type index	Number of children	Number of children between 1 and 3 years and 11 months old	Percentage of households in this category
1	0	0	43.67%
2	1	0	16.58%
3	1	1 or more	4.13%
4	2	0	9.02%
5	2	1 or more	5.84%
6	3	0	2.62%
7	3	1 or more	3.32%
8	4 or more	0	2.59%
9	4 or more	1 or more	12.23%

Note: The sample consists of those households that have at least one woman who is a i) head of household or a spouse of a head of household, ii) she is between 16 and 60 years old, iii) and she was observed between 2005.Q1 and 2010.Q2

Figure 2: EI Exposure in Three Different Points in Time

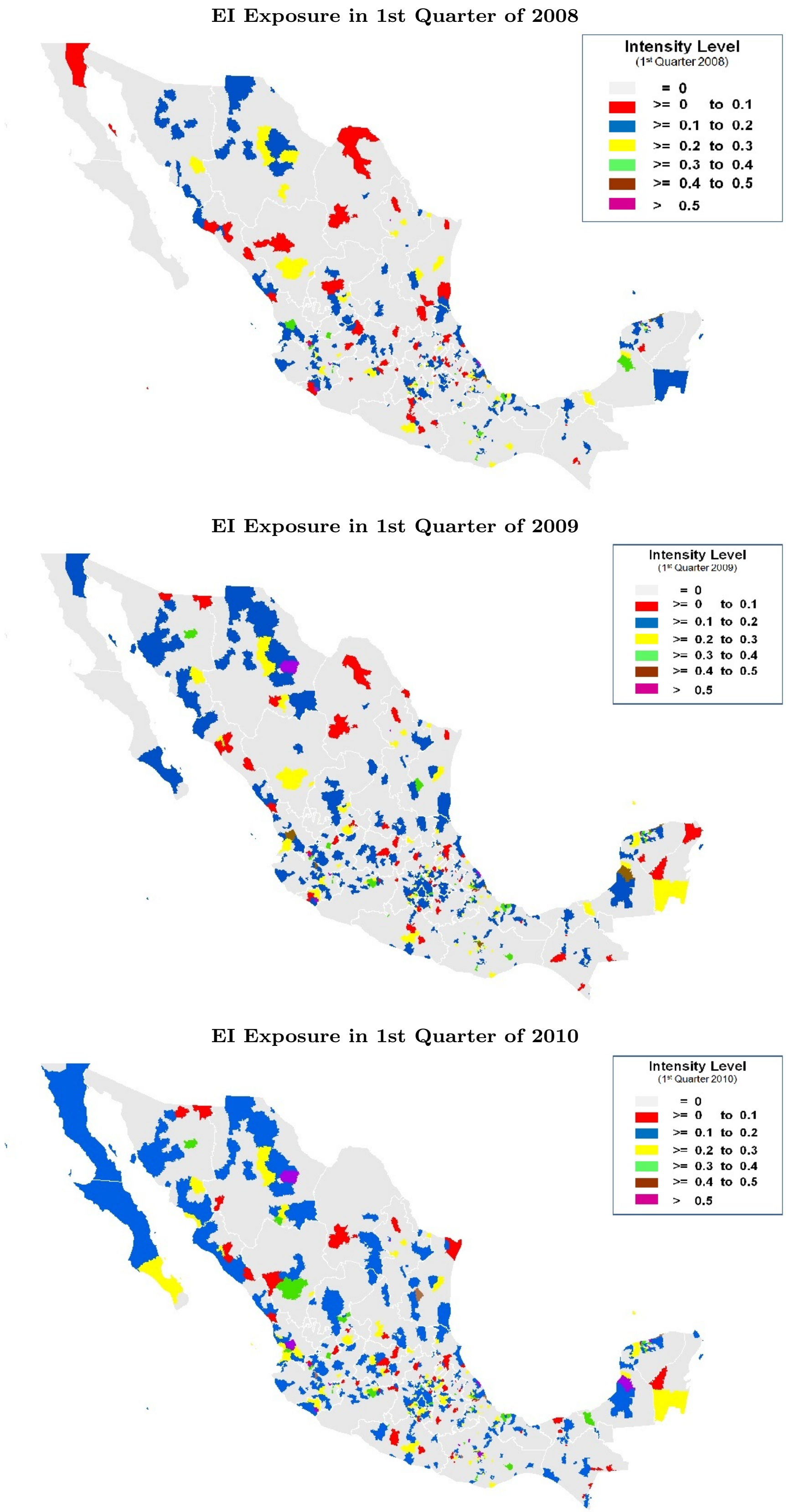
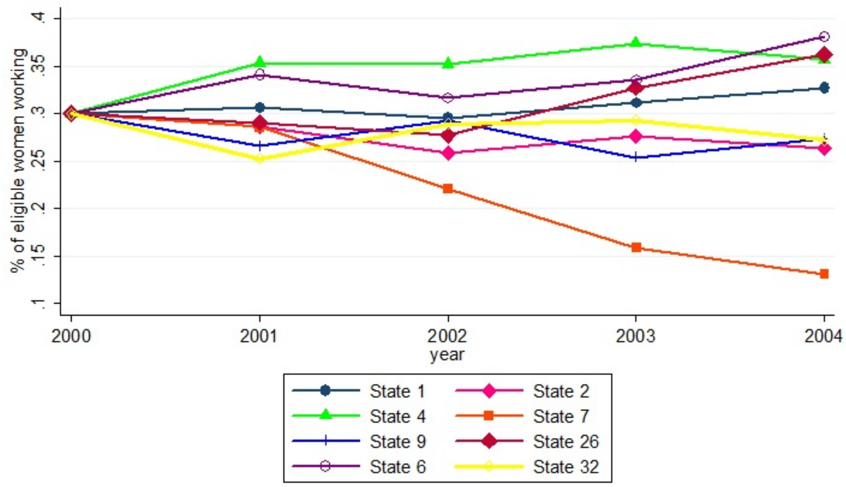
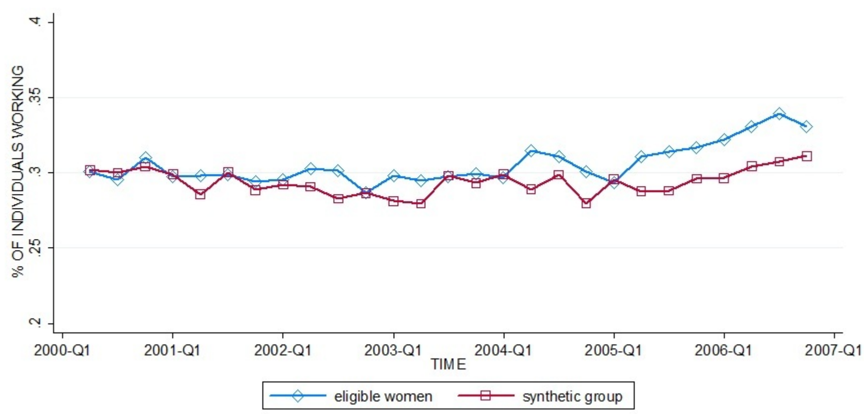


Figure 3: Pre-trends of Proportion of Women Employed Among Eight Different Trends



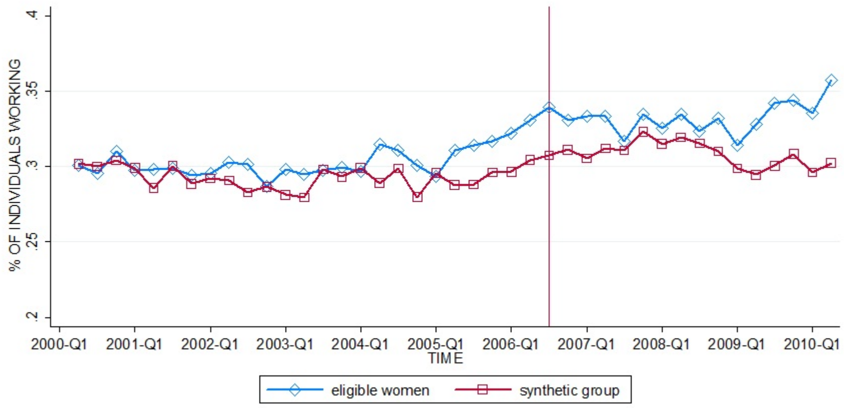
Note: The trends for the synthetic control group are re-scaled to start at the same level as the eligible women.

Figure 4: Pre-trends of the Proportion of Individuals Working: Eligible Women vs Synthetic Group, from 2000.Q2 to 2006.Q4



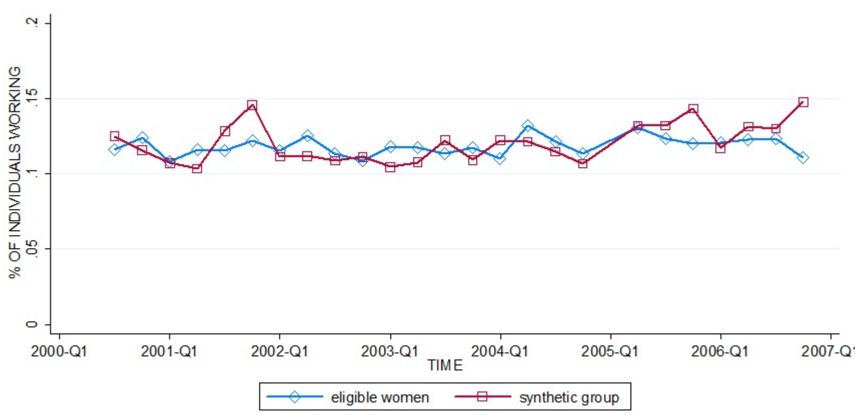
Note: The trends for the 8 states are re-scaled in order to start at the same level in 2000.Q2.

Figure 5: Trends of the Proportion of Individuals Working: Eligible Women vs Synthetic Group, from 2000.Q2 to 20010.Q2



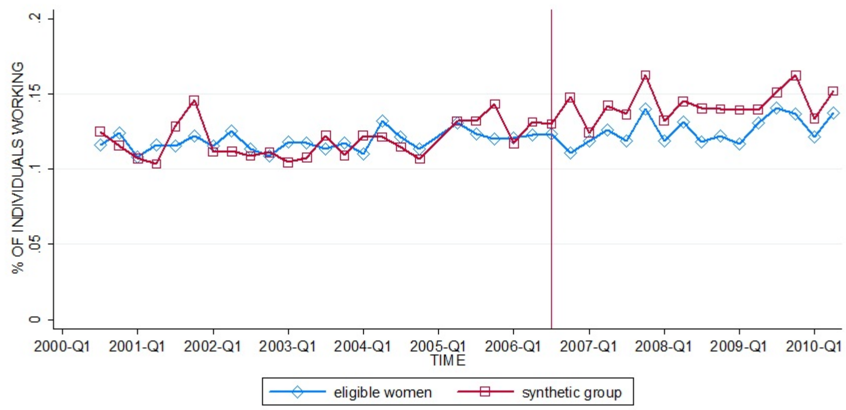
Note: The trends for the synthetic control group are re-scaled to start at the same level as the eligible women.

Figure 6: Pre-trends of the Proportion of Individuals Working Conditional on Not Working in t-1: Eligible Women vs Synthetic Group, from 2000.Q2 to 2006.Q4



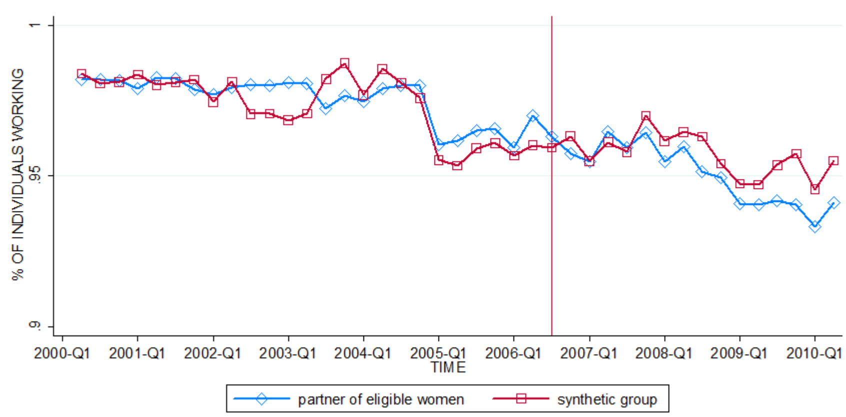
Note: The trends for the synthetic control group are re-scaled to start at the same level as the eligible women.

Figure 7: Trends of the Proportion of Individuals Working Conditional on Not Working in t-1: Eligible Women vs Synthetic Group, from 2000.Q2 to 2010.Q2



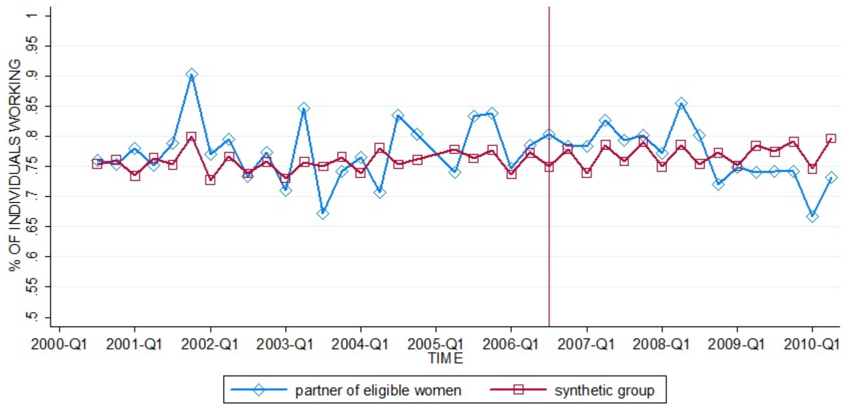
Note: The trends for the synthetic control group are re-scaled to start at the same level as the eligible women.

Figure 8: Trends of the Proportion of Individuals Working: Partners of Eligible Women vs Synthetic Group, from 2000.Q2 to 2010.Q2



Note: The trends for the synthetic control group are re-scaled to start at the same level as the eligible men.

Figure 9: Trends of the Proportion of Individuals Working Conditional on Not Working in t-1: Partners of Eligible Women vs Synthetic Group, from 2000.Q2 to 2010.Q2



Note: The trends for the synthetic control group are re-scaled to start at the same level as the eligible women.

Table 5: Effect of 1 Child Care Space for Every 10 EI-eligible Children over Women's and Men's Probability of Working

	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:				
Panel A: Eligible Women				
working	0.015*** (0.003)	0.348	4.310%	2,162,860
Panel B: Eligible Men				
working	-0.012*** (0.002)	0.955	-1.257%	2,105,691

Note: EI level of variation is at the municipality level. All specifications include family type fixed effects, municipality fixed effects, year fixed effects, urban fixed effects, a double interaction of urban and eligibility, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 6: Effect of 1 Child Care Space for Every 10 EI-Eligible Children Over Women's and Men's Probability to Start Working

	(Hazard Ratio) $e_i \cdot EI\_intensity$	Percentage Point Change	Mean	Percentage Change	Observations
Dependent Variable:					
Panel A: Eligible Women					
working	1.045***	4.5 pp	0.348	12.931%	1,560,315
	(0.011)				
Panel B: Eligible Men					
working	0.942*	5.8 pp	0.955	-6.073%	357,611
	(0.032)				

Note: EI level of variation is at the municipality level. This specification uses a duration model with an exponential distribution. The estimates in the first column represent a hazard ratio. The explanatory variables include: family type fixed effects, municipality fixed effects, year fixed effects, urban fixed effects, a double interaction of urban and eligibility, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 7: Effect of 1 Child Care Space for Every 10 EI-eligible Children over Women's and Men's Time Allocation (Hours per Week)

	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:				
Panel A: Eligible Women				
time child rearing	-0.901*** (0.318)	21.811	-4.131 %	2,162,860
time housework	-0.034 (0.212)	30.163	-1.127%	2,162,860
time working conditional working in t and t-1	-0.043 (0.223)	35.526	0.121%	906,844
time working (unconditional)	0.581*** (0.163)	11.705	4.964%	2,162,860
Panel B: Eligible Men				
time child rearing	-0.186* (0.108)	4.974	-3.739%	2,105,691
time housework	-0.162* (0.087)	3.527	-4.962 %	2,105,691
time working conditional working in t and t-1	0.235** (0.116)	46.691	0.503%	991,797
time working (unconditional)	-0.115 (0.154)	44.68012	-0.257%	2,105,691

Note: EI level of variation is at the municipality level. Specifications where the dependent variable is *time for child rearing* and *time for housework* include: individual fixed effects, family type fixed effects, year fixed effects, urban, fixed effects, double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Specifications where the dependent variable is *time working conditional on working in t and t-1* and *time working* include: family type fixed effects, municipality fixed effects, year fixed effects, urban fixed effects, a double interaction of urban and eligibility, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 8: Effect of 1 Child Care Space for Every 10 EI-eligible Children over Women's and Men's Income

	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:				
Panel A: Eligible Women				
income conditional on having positive income in t-1	40.810 (48.061)	$4195.272	0.973 %	759,145
having no income conditional on having zero income in t-1	-0.009*** (0.003)	0.652	-1.266%	1,320,509
Panel B: Eligible Men				
income conditional on having positive income in t-1	-11.793 (34.560)	$5776.07	-0.204%	823,884
having no income conditional on having zero income in t-1	-0.008*** (0.003)	0.161	-4.969%	1,193,199

Note: EI level of variation is at the municipality level. All specifications include family type fixed effects, municipality fixed effects, year fixed effects, urban fixed effects, a double interaction of urban and eligibility, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 9: Effect of 1 Child Care Space for Every 10 EI-eligible Children over Women's and Men's Labor Conditions

	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:				
Panel A: Eligible Women				
switching jobs	0.001 (0.001)	0.049	2.041 %	2,162,860
uninsured	-0.005* (0.003)	0.754	-0.663%	2,162,860
Panel B: Eligible Men				
switching jobs	-0.002 (0.003)	0.207	-0.966 %	2,105,691
uninsured	-0.004 (0.005)	0.668	-0.599%	2,105,691

Note: EI level of variation is at the municipality level. All specifications include family type fixed effects, municipality fixed effects, year fixed effects, urban fixed effects, a double interaction of urban and eligibility, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 10: Effect of 1 Child Care Space for Every 10 Children of the EI Program for Women who were Working in t-1

	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:				
<u>Labor Status</u>				
working	-0.024 (0.015)	0.766	-3.197%	1,013,723
switching jobs	-0.032** (0.015)	0.182	-17.565%	1,013,723
<u>Time Allocation</u>				
time child rearing	-0.422 (0.439)	19.262	-2.191%	1,013,723
time housework	-0.050 (0.363)	25.849	-0.197%	1,013,723
<u>Income</u>				
Income conditional on having positive income in t-1	62.244 (86.256)	$4546.636	-17.565%	759,145
zero income conditional on having no income in t-1	0.023 (0.056)	0.363	-17.565%	171,372

Note: EI level of variation is at the Municipality Level. Specifications where the dependent variable is *time for child rearing* and *time for housework* include: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. For the rest of the specifications include: family type fixed effects, year fixed effects, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard Errors are clustered at the State Level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 11: Effect of 1 Child Care Space for Every 10 Children of the EI Program for Women with Lower or Equal than High-school Education

	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:				
Labor Status				
working	0.014*** (0.004)	0.309	4.531 %	1,820,233
switching jobs	0.000 (0.001)	0.044	0 %	1,820,233
Time Allocation				
time child rearing	-1.003*** (0.389)	21.691	-4.624%	1,820,233
time housework	-0.018 (0.226)	31.064	-0.058%	1,820,233
Income				
Income conditional on having positive income in t-1	74.981** (35.840)	$3141.386	2.387%	618,732
zero income conditional on having no income in t-1	-0.010*** (0.003)	0.747	-1.339%	1,140,066

Note: EI level of variation is at the Municipality Level. Specifications where the dependent variable is *time for child rearing* and *time for housework* include: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. For the rest of the specifications include: family type fixed effects, year fixed effects, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard Errors are clustered at the State Level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 12: Effect of 1 Child Care Space for Every 10 Children of the EI Program for Women with Education Lower or Equal to High-school, by Urban and Rural Area

	Panel A: Urban Area				Panel B: Rural Area			
	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations	$e_i \cdot EIexposure$	Mean	Percentage Change	Observations
Dependent Variable:								
Labor Status								
working	0.012*** (0.003)	0.338	3.550%	1,083,300	0.016** (0.007)	0.291	5.507%	736,933
switching jobs	0.001 (0.002)	0.052	1.908 %	1,083,300	-0.000 (0.002)	0.039	0 %	736,933
Time Allocation								
time child rearing	-1.064*** (0.508)	25.088	-4.241%	1,083,300	-0.870 (0.544)	19.513	-4.459%	736,933
time housework	0.011 (0.313)	29.493	0.037%	1,083,300	-0.035 (0.321)	32.071	-0.109%	736,933
Income								
Income conditional on having positive income in t-1	166.952*** (45.148)	$ 3648.644	4.576%	380,798	-19.082 (58.957)	$ 2740.624	-0.696%	237,934
zero income conditional on having no income in t-1	-0.009*** (0.003)	0.714	-1.260%	662,235	-0.011** (0.005)	0.768	-1.432%	477,831

Note: EI level of variation is at the Municipality Level. Specifications where the dependent variable is *time for child rearing* and *time for housework* include: individual fixed effects, family type fixed effects, year fixed effects, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. For the rest of the specifications include: family type fixed effects, year fixed effects, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard Errors are clustered at the State Level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 13: Average Reduction on the Amount of Time for Child Rearing due to the EI program

	$e_i \cdot EIexposure$	$\mathbb{I}^{reduction\ time\ child}$	Observations
Dependent Variable:			
First Stage			
$\mathbb{I}^{reduction\ time\ child}_{ismqt}$	0.096***		2,076,029
	(0.006)		
Second Stage			
$TimeChildRearing$		-10.742***	
		(2.622)	

Note: EI level of variation is at the Municipality Level. In the first and second stage regression I included: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. The F-test for the first stage regression 83.71. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 14: *Pseudo-Treatment-on-the-Treated* Effect on the Probability of Working (by Gender)

	Reduction of 1hr of Child Rearing	% Change over the Mean	$\mathbb{I}^{\geq 10hr\ reduction\ time\ child}$ (dummy)	% Change over the Mean
Panel A: Eligible Women				
working	0.012**	3.448%	0.084***	24.138%
	(0.006)		(0.033)	
Panel B: Eligible Men				
working	0.000	-0.030%	0.005	0.524%
	(0.003)		(0.027)	

Note: Number of observations for women is 2,076,029, and for men is 1,869,928. EI level of variation is at the Municipality Level. In the first and second stage regression I included: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. The F-test for the first stage regression, where the dependent variable is *time on child rearing* in the case of women was 13.95, for the case of men was 8.63. The F-test for the first stage regression, where the dependent variable is $\mathbb{I}^{reduction\ time\ child}_{ismqt}$ in the case of women was 71.07, for the case of men was 48.35. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 15: *Pseudo-Treatment-on-the-Treated* Effect on Time Allocation (by Gender)

	Reduction of 1hr of Child Rearing	% Change over the Mean	$\mathbb{I}^{\geq 10hr\, reduction\, time\, child}$ (dummy)	% Change over the Mean
Panel A: Eligible Women				
time housework	-0.073 (0.181)	-0.242%	-0.633 (2.192)	-2.099%
hours worked	0.761*** (0.272)	6.501%	9.984*** (2.522)	85.295%
Panel B: Eligible Men				
time child	-0.163* (0.088)	-3.27%	-1.767* (0.961)	-35.525%
time housework	-0.175** (0.084)	-4.138%	-1.902 (1.769)	-53.912%
hours worked	-0.146 (0.227)	-0.392%	-0.115 (4.666)	-0.257%

Note: Number of observations for women is 2,076,029, and for men is 1,869,928. EI level of variation is at the municipality level. In the first and second stage regression I included: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. The F-test for the first stage regression, where the dependent variable is *time on child rearing* in the case of women was 13.95, for the case of men was 8.63. The F-test for the first stage regression, where the dependent variable is $\mathbb{I}_{ismqt}^{reduction\, time\, child}$ in the case of women was 71.07, for the case of men was 48.35. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 16: *Pseudo-Treatment-on-the-Treated* Effect on Labor Conditions (by Gender)

	Reduction 1hr of Child Rearing	% Change over the Mean	$\mathbb{I}^{\geq 10hr\, reduction\, time\, child}$ (dummy)	% Change over the Mean
Panel A: Eligible Women				
switching jobs	-0.009**	-18.557%	-0.116**	-239.175%
	(0.004)		(0.047)	
switching jobs getting higher earnings	-0.003	-37.500%	-0.036	-450.000%
	(0.002)		(0.024)	
income	71.609**	1.707%	830.990***	19.808%
	(29.557)		(302.088)	
uninsured	-0.009***	-1.195%	-0.114***	-15.139%
	(0.003)		(0.031)	
self-employed	0.005	1.706%	0.065	22.184%
	(0.004)		(0.046)	
Panel B: Eligible Men				
switching jobs	0.040***	19.324%	0.810***	391.304%
	(0.014)		(0.147)	
switching jobs getting higher earnings	0.020**	43.573%	0.427***	930.283%
	(0.008)		(0.944)	
income	27.089	0.469%	-450.86	-7.806%
	(56.557)		(1474.221)	
uninsured	0.003	0.449%	-0.026	-3.892%
	(0.004)		(0.085)	
self-employed	0.002	0.702%	-0.0628	-22.070%
	(0.005)		(0.095)	

Note: Number of observations for women is 2,076,029, except for income that is 1,823,747. Number of observations for men is 1,869,928, except for income that is 1,669,631. EI level of variation is at the municipality level. In the first and second stage regression I included: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. The F-test for the first stage regression, where the dependent variable is *time on child rearing* in the case of women was 13.95, for the case of men was 8.63. The F-test for the first stage regression, where the dependent variable is $\mathbb{I}_{ismqt}^{reduction\, time\, child}$ in the case of women was 71.07, for the case of men was 48.35. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 17: *Pseudo-Treatment-on-the-Treated* Effect on Total Household Income

	Reduction 1hr of Child Rearing	F-test for First Stage	$\mathbb{I}^{\geq 10hr\, reduction\, time\, child}$ (dummy)	F-test for First Stage
household income	-112.668 (88.443)	13.95	-1,383.311 (972.606)	50.40

Note: The number of observations is 1,637,123. EI level of variation is at the municipality level. In the first and second stage regression I included: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. The F-test for the first stage regression was 13.95, where the dependent variable is *time on child rearing*, for the case of men was 8.63. The F-test for the first stage regression was 50.40, where the dependent variable is $\mathbb{I}^{reduction\, time\, child}_{ismqt}$. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 18: Allocation of the EI program, According to EI Exposure Index of 2007.Q4.

	(1) *EIexposure* (2007.Q4)
growth rate of eligible women working (2000.Q2- 2005.Q4)	0.035 (0.026)
growth rate of eligible women working (2005.Q1- 2006.Q4)	0.034 (0.027)
growth rate of non-eligible women working (2000.Q2- 2005.Q4)	-0.048 (0.057)
growth rate of non-eligible women working (2005.Q1- 2006.Q4)	-0.061 (0.078)
ln(GDP)	-0.001 (0.011)
first difference of ln(GDP)	-0.304 (0.431)
proportion of population	-0.013** (0.006)
elections	0.082 (0.104)
urban	0.110*** (0.021)
constant	0.089 (0.214)
Observations	63
R-squared	0.504

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.10

Table 19: Effect of 1 Child Care Space for Every 10 EI-Eligible Children using ENE Data (2000.Q1-2004.Q2)

	$e_i \cdot EIexposure$	Observations
Dependent Variable:		
working	0.003 (0.003)	2,831,444
hours worked (unconditional)	0.126 (0.147)	2,831,444

Note: EI level of variation is at the municipality level. All specifications include family type fixed effects, municipality fixed effects, year fixed effects, urban fixed effects, a double interaction of urban and eligibility, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard errors are clustered at the state level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 20: Effect of 1 Child Care Space for Every 10 Children of the EI Program on Women, controlling for IMSS Exposure

	$e_i \cdot EIexposure$	Mean	Percentage Change	$e_i \cdot IMSSexposure$	Observations
Dependent Variable:					
<u>Labor Status</u>					
working	0.014*** (0.003)	0.348	3.920%	0.003 (0.002)	2,162,860
switching jobs	0.001 (0.001)	0.049	2.288%	0.001 (0.000)	2,162,860
<u>Time Allocation</u>					
time child rearing	-0.898*** (0.318)	21.811	-4.115%	-0.019 (0.089)	2,162,860
time housework	-0.017 (0.210)	30.163	-0.057 %	-0.090 (0.084)	2,162,860
<u>Income</u>					
Income conditional on having positive income in t-1	25.486 (64.708)	$4195.272	0.607%	41.616 (62.356)	759,145
zero income conditional on having no income in t-1	-0.009*** (0.003)	0.711	-1.246%	-0.001 (0.001)	1,320,509

Note: EI level of variation is at the Municipality Level. Specifications where the dependent variable is *time for child rearing* and *time for housework* include: individual fixed effects, family type fixed effects, year fixed effects, urban fixed effects, a double interaction between urban and eligibility, urban and rural linear time trends specific for each state, a quadratic function of the age of the youngest child, living with other relatives, and level of education. For the rest of the specifications include: family type fixed effects, year fixed effects, urban and rural linear time trends specific for each state, cohort fixed effects, a quadratic function of the age of the youngest child, living with other relatives, and level of education. Standard Errors are clustered at the State Level. Robust clustered standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.