Work half-time, receive full-time pay: Effect of novel family policy on female labor market outcomes *

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Abstract

Many countries provide paid family leave to increase female labor force participation, improve gender equality, and foster family wellbeing. Yet, a large percentage of women do not return to work after maternity leave. Can a policy that allows flexible re-introduction to work increase labor force participation after childbirth? To answer this question, I study a unique subsidy implemented in Uruguay in 2014 that allows mothers to work half-time while receiving full-time pay for four months after maternity leave ends. I use eleven years of employment survey data in a difference-in-differences and triple difference framework and find an increase in the likelihood of employment of 17% for eligible mothers up to 1 year after childbirth, and a decrease in hours worked of 5% without a significant reduction in income up to 3 years after childbirth. These findings demonstrate that policies easing the transition back from maternity leave can increase female labor force participation and reduce "child-penalties" in the short and medium-run.

Keywords: family policies, female labor-market outcomes, gender equality, part-time employment, development

JEL Codes: J22, J21, O17

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1 Introduction

While the participation of women in the labor force has increased steadily over the past decades, gender inequalities in the labor market remain substantial (Blau & Kahn, 2017; Goldin, 2006). Significant gender differences in wages, hours of work, and occupational choices continue to exist in the vast majority of countries. Globally, women are paid approximately 20 percent less than men, and this gap is largest in low-income countries (International Labor Organization, 2019). This differential cannot be explained solely by gender differences in schooling, number of hours worked, experience, and job characteristics (Blau & Kahn, 2017; Kleven et al., 2018).

These gender differences start or are expanded when a child is born; the so called "child penalties" refer to the reduction of women's income observed immediately after childbirth that persists for years (Bertrand et al., 2010; Goldin, 2014a; Kleven & Landais, 2017; Kleven et al., 2018). Often, women work part-time while simultaneously being the main providers of child care within the family (Paull, 2008; Ciccia & Verloo, 2012). Recognizing that childbirth is a key moment in the life of workers, countries around the world have invested significant resources in maternity leave and other types of family policies to increase female labor force attachment, improve gender equality, and foster family wellbeing. However, women still experience larger child penalties than men. Even with maternity leave policies in place, women often do not return to their job after maternity leave, and studies show mixed results about how effective extending maternity leave is at improving female empowerment in the long-run (Bailey et al., 2019).

In this paper I study the introduction of a novel form of family policy implemented in Uruguay in 2014: A subsidy that allows parents to work half-time while receiving full-time pay, from the time maternity leave ends, and until the child is 6 month old. The argument for this new form of family policy is that a more gradual return to work after maternity leave might increase labor force attachment and job retention in the short-run for mothers with a young child (Schott, 2012). The hypothesized mechanism to yield these results is increased flexibility, which allows mothers to retain work related human capital that they would otherwise lose on full maternity leave. However, if this subsidy fosters traditional gender divisions of household work it could encourage women's part-time work and lower the probability of upward occupational moves increasing gender wage gaps (Blau & Kahn, 2013; Evertsson & Duvander, 2011). Hence the importance to study empirically its effects in the short and medium-run.

I examine the effect of the parental care subsidy on female labor market outcomes using household survey data in a difference-in-differences (DD) and triple difference (DDD)

framework. My identification strategy relies on the facts that only workers employed in the formal-private sector are eligible, and that utilization consists almost entirely of women. Women employed in the public and informal sectors as well as men whose spouse does not work or is employed informally, are not eligible or affected by the policy, and can serve as control groups.

I find that mothers are more likely to be employed following the implementation of the parental care subsidy in the short and medium-run. Mothers are 6.1 percent more likely to be employed during the first year of their child from a pre-treatment mean of 54 percent. This effect is driven by an increase in the likelihood of being employed in the formal-private sector, which increased by 17 percent from a pre-treatment mean of 26 percent. Moreover, I find evidence that this effect is heterogeneous and largest for disadvantaged mothers (those who are less educated).

Next, I study the effect that parental care subsidy has on the intensive margin by looking at income, job experience, and number of jobs, conditional on employment. For this, I leverage the fact that public sector workers and informal workers do not have access to the new subsidy and can therefore serve as a control group within the sample of employed mothers of infants. I find a large increase in subsidies received from the government, consistent with program take-up, with no significant changes in total income for mothers of infants employed in the formal-private sector after the implementation of the policy.

In the medium-run, 1 to 3 years after the birth of a child, I find an increase in the likelihood of employment for mothers. Moreover, mothers of a young child born after the implementation of the policy and employed in the formal-private sector decreased their hours worked by 5% without a significant decrease in income.

Overall, this study provides novel evidence that extensions of flexibilities after birth in the form of fully paid half-time employment causes higher labor force attachment for female workers in the short and medium-run, especially for disadvantaged mothers (those with lower levels of education). The results are robust to changes in the specification and to an extended "event–study" analysis which allows for visualization of pre-treatment trends as well as the evolution of the average treatment effect in the post period.

My paper contributes to the literature on family policies in several ways. This is the first paper to evaluate a parental leave policy with the *work half-time, receive full-time pay* characteristics. Understanding the effects of this unique approach provides new insights and informs policy discussions and innovation. Second, I expand the literature by providing the first analysis of a family policy implemented in a non-OECD country. I leverage differences between formal and informal employment and show that the parental care subsidy is effective at increasing formal employment among mothers of infants. Third, I provide a causal

examination of the short and medium-run effects of the parental care subsidy on female labor market outcomes. This complements studies that focus on take-up determinants and short-run effects of parental leave (Bartel et al., 2018; Romero-Balsas, 2012; Tanaka & Waldfogel, 2007).

The paper proceeds as follows: Section 2 provides background on existing family policies in Uruguay, and the policy reform. Section 3 describes the data. Section 4 details the empirical methods used. Section 5 presents short and medium-term results in the extensive and intensive margin. Section 6 shows robustness checks, and section 7 concludes.

2 Background

In this section I describe the institutional background of Uruguay, the novel parental care subsidy and the main characteristics of maternity and paternity leave provisions that also exist in the country. I briefly outline the history and current state of leave policies across countries and review the literature on the effects of paid family leave, child penalties, and half-time work for mothers of young children.

2.1 Parental care subsidy

The two most common types of family policies designed to balance work and child care are the provision of child-care services and the provision of leave for parents around the time of childbirth to care for an infant child. In this paper I focus on a novel family policy that has characteristics of both common provisions but is not easily classified as either child-care or leave.

In November of 2013 Uruguay passed a law regulating a novel parental care subsidy for workers in the private sector. Under this new policy, mothers and fathers have access to full-time pay while working half-time from the time maternity leave ends until the child is 6 month old, which in most cases amounts to 4 months of benefits. This half-time period is job-protected and parents can decide whether the mother, father or both (alternating) will receive the subsidy.[1] Beneficiaries receive monthly transfers from the social security authority equivalent to their salary for the reduced hours.

The parental care subsidy was implemented in January 2014 and the introduction was gradual. At the onset, the subsidy covered from the end of maternity leave until the child was 4 months old. In January of 2015 the subsidy was extended to cover until the child

[1] In the literature "job-protected" refers to strong regulations that protect parent against discrimination or retaliation for taking up paid leave. In practice it means that employers cannot fire or change working conditions during and immediately after the parental care subsidy.

was 5 months old. Since 2016 the subsidy covers until the child is 6 months old. Figure 1 depicts the evolution of take up for maternity, paternity and parental care subsidy provisions for workers in the private sector. Take-up was expanded as the coverage of parental care subsidy increased from 2014 to 2016. The average number of beneficiaries in 2014 was 738 and increased to 1327 in 2015 and for the period 2016-2019 it reached 2200. Maternity and paternity leave had an average number of beneficiaries per month in 2019 of 1125 and 1113 respectively.

Even though both mothers and fathers can alternate access to the parental care subsidy, in practice, approximately 98 percent of beneficiaries at any given month are women. Figure 2 presents the evolution of the number of beneficiaries by gender. This extreme gender gap in utilization has also been documented in the literature regarding gender-neutral family leave policies in other countries. Additionally, only dual earner families are eligible. For the mother or father to apply for the subsidy, the mother needs to have taken maternity leave. If the father is employed in the private sector but his wife is not employed or employed informally, he is not eligible for the parental care subsidy.

The parental care subsidy is fundamentally different than maternity leave because the benefits are conditional on returning to work after maternity leave ends and staying employed until the end of the benefits. The parental care subsidy changes the budget constraint that the mother faces at the time of returning from maternity leave. Figure 3 illustrates the new labor-leisure choice. With the subsidy, the budget constraint now has a kink. Mothers working half-time receive full-time pay which puts them at a higher indifference curve. If we only consider the income-leisure choice and assume indifference curves do not change with the birth of a child, we should see large take up of the subsidy and increased labor force attachment during the first months of life of the infant. However, the prediction is less clear in the long-run.

2.2 Maternity and paternity leave in Uruguay

In addition of the parental care subsidy family policies in Uruguay include maternity leave and paternity leave. The details of each program as well as when they where implemented varies according to the sector of employment. Public and private workers are subject to different regulations.

Maternity leave for workers in the private sector was implemented in 1980 and consisted of 12 job-protected, fully-paid weeks. In January 2014 workers' rights in the private sector were expanded in magnitude and coverage.[2] Maternity leave was extended by 2 weeks and

[2]Law 19,161 of 2013 replaced the benefits regulated by Law 15,084 of 1980.

included non-dependent workers covered by the social security authority with up to one employee, single-tax workers and workers covered by unemployment insurance. Maternity leave starts 6 weeks before the scheduled day of the birth and ends 8 weeks after. Public sector female workers have similar benefits, since 1990 they can access maternity leave for 13 weeks that start one week before the estimated birth date of the child. After maternity leave ends workers that are breastfeeding have the right to work half-time while receiving full-time pay until the child is 1 year old maximum.[3] Male public workers have the right to paternity leave for 10 working days from the date of the birth of the child. These policies provide 100 percent of wage replacement paid by the employer (in this case the government). [4]

Paternity leave was introduced gradually from January 2014, and since January 2016 it consists of 13 working days. It includes workers on private and public activity, non-dependents covered by social security with up to one employee and personal contractors who are up to date with the contributions and are not delinquent on alimony payments. In both cases, the social security system provides 100 percent wage replacement during the leave.[5]

2.3 Literature on part-time work and the effects of maternity leave

Recent work highlights the importance of parenthood for the persistence of gender inequality in labor market outcomes. Kleven, et al. (2019, May) show that, in the U.S., U.K., Scandinavian countries, Austria, and Germany, the earnings of men and women evolve similarly before parenthood—after adjusting for life cycle and time trends—but diverge sharply after parenthood. Women experience a large, immediate and persistent drop in earnings after the birth of their first child, while men are essentially unaffected. In general, the so called child penalty in earnings can come from three margins: The extensive margin of labor supply (employment), the intensive margin of labor supply (hours worked), and the wage rate. Goldin (2014) argues that the gender earnings gap persists because mothers of infants are more likely to select into flexible jobs and these work-life balance preferences interact with sector-specific long-hours premium.

Family policies, especially maternity leave, are important because they are seen as a potential instruments to decrease the earning gender gap by increasing female labor force attachment after childbirth. However, maternity leave also has the potential to increase the

[3]The worker is required to provide a medical certification of breastfeeding to the employer during the duration of the subsidy.

[4]Law 19,121 of 2013 replaced the benefits regulated by Law 16,104 of 1990.

[5]Same-sex couples have access to same level of leave, the gestational parent receives maternity leave, while the non-gestational parent receives paternity leave. Public and private workers who adopt a child also have the right to paid leave for 6 weeks, followed by 6 months of half-time work while receiving full-time pay

likelihood of mothers selecting into more flexible jobs with lower wage rates.

The International Labor Organization (ILO) guidelines propose the extension of maternity leave to 18 weeks with a benefit equivalent to 100 percent of earnings. Figures 12 and 13 in the Appendix plot current maternity and paternity leave regulations across countries. Almost all OECD countries achieve ILO's recommendation: the average number of weeks of paid maternity leave is 18. However, only 13 percent of countries in Latin America and the Caribbean meet ILO's guidance on maternity leave. Given these differences in the length and coverage of leave provisions in low and middle-income countries it is important to study policies that extend flexibilities for workers but are less costly than fully paid maternity leave for governments to implement. Hence the importance of studying the labor market effects of the novel subsidy in the short and middle-term for women.

An extensive literature examines the labor market effects of parental leave policies in high-income countries. Rossin-Slater (2017) provides a review of the literature on the impacts on women and children of the wide variety of family leave policies across Europe and North America. She concludes that parental leave shorter than one year can improve women's job continuity, while longer leave may negatively impact career advancement.

More recently, Stearns (2018) finds that different components of parental leave laws in Great Britain had opposing effects. Whereas wage replacement tended to increase short-term employment, laws granting job protection and increasing leave duration tended to impact career advancement negatively in the longer-term. One possible explanation for these findings is that while paid family leave policies may increase labor-force participation in the short-term, they could also increase statistical discrimination and occupational segregation in the longer term. Women in OECD countries who have more generous paid leave are more likely to work part-time and less likely to hold management positions (Blau and Kahn 2013).

The evidence of the effects of parental leave on low and middle-income countries is limited. Albagli and Rau (2018) study an extension of maternity leave from 12 to 24 weeks in Chile (also an OECD country) utilizing a linear model. They find an increase in the probability of employment of 5.8 percentage points one year after birth, but do not find effects on wages.

I expand on this literature not only by studying a novel policy but also by considering employment by sectors. Increasing formal employment is a mayor policy goal in low and middle-income countries. Formal jobs are of better quality, provide higher wages and operate within the labor regulations of the country. Increasing access and continuity of formal employment for women is specially relevant to decrease gender wage gaps in resource-constraint households.

3 Data

The household and individual level data used in this study come from Uruguay's Continuous Household Survey (ECH), for the years 2009-2019. ECH is a nationally representative repeated cross-sectional survey, conducted monthly by the National Institute of Statistics (INE). Each individual in the household is questioned about their demographic characteristics, employment status, sector of employment, and non-labor income.

The data contain information that is crucial for the identification strategy. First, ECH allows me to identify the composition of the household and using the age of the children I can identify parents of infants (a child younger than one year old). Second, it allows me to identify formal versus informal workers by asking individuals whether they make contributions towards their pensions to the social security authority (BPS). I use this question, following the literature, to classify formal versus informal workers in the private sector.[6]

There are two primary limitations to using ECH data. First, ECH lacks of precise information on child birth dates, only reporting the age of the individuals in years. As a result, to identify short term effects as well as evidence of program utilization I focus on women with a child that is less than one year old. Second, the survey only asks the number of hours worked normally, and it does not refer to a specific reference week. Thus, even if a parent is working half-time during the length of the subsidy they might still report the otherwise full-time job hours. Because of this, I cannot distinguish the number of hours worked during the subsidy from the number of hours that the person would have worked otherwise.

I examine labor market outcomes for women with infants by comparing their experiences to those of men that are at a similar stage of life and career but that are not directly or indirectly treated by the new subsidy. Given the extremely low take-up of this program among men (on average lower than 2%) it is unlikely they will be directly affected by the program. However, if their spouses utilize the subsidy many intra-household dynamics might change which in turn will also affect men. To avoid these indirect effects in the control group, I select men whose spouses are not eligible for the parental care subsidy. This consists of men whose spouses are not employed or are employed informally and thus have not experienced any change in benefits.

The sample that I use to conduct the analysis consists of mothers 18 to 45 years old with an infant child at the time of the survey (treatment group); and men 18 to 45 years

[6]The formal sector is defined as the group of employed individuals who work for companies that are registered with the tax authority and pay taxes towards social security. The informal sector is formed by employed individuals working for unregistered employers or that do not pay taxes to the social security authority. Public sector workers are in large majority formal workers.

old, father of an infant at the time of the survey whose spouse or partner is not employed or is employed informally (control group). Table 1 shows the mean and standard deviation of employment, income and hours worked and individual characteristics for treatment and control groups, before and after the implementation of the parental care subsidy in January 2014.

There is a large employment gap between mothers and untreated fathers of infants. Only 54 percent of mothers of infants are employed before the implementation of the policy, whereas 92 percent of fathers of infants are employed during this period. Interestingly, conditional on employment, total labor income is less unequal between the two groups. Before January of 2014 mothers and fathers of infants earned on average UY$ 14200 and UY$ 15300 in total labor income respectively. However, during the post-treatment period the total labor income increased more for mothers than for fathers of infants in the sample.

Mothers and untreated fathers have similar number of children and ages. However, mothers of infants are less likely to be married or living with her partner, have higher levels of education, and are more likely to live in the capital city than untreated fathers of infants. I control for these differences in individual characteristics in my identification.

4 Empirical strategy

4.1 Employment effects in the short-run

I study the effect of an extension of family policies on female labor force attachment by leveraging the quasi-experiment provided by the implementation of a parental care subsidy in Uruguay in January of 2014. I employ a difference-in-difference (DD) design that compares mothers of infants, to untreated father of infants (as defined in section 3), before and after the implementation of the parental care subsidy.

To understand the effect of the policy on the employment decision of mothers of infants, I estimate the following generalized DD equation:

$$Y_{it} = \beta_1 Woman_i * After_t + \gamma' X_{it} + \delta_i + \theta_t + \epsilon_{it} \tag{1}$$

where i is an individual and t represents a month-year. Y_{it} is the outcome of interest. This binary variable takes the value of one if individual i is employed in the formal-private sector at time t. $Woman_i$ is a binary variable that takes the value of one if individual i is a mother, and zero for fathers. $After_t$ is a binary variable that take the value of one for periods after December of 2013, and zero otherwise. The coefficient of interest is β_1 and it captures the effect of the parental care subsidy on employment among mothers of infants.

The vector X_{it} includes a set of individual and household controls such as individual's age, education, marital status, ethnicity, number of children in the household, and region. Gender fixed effects (mother or father) are captured by δ_i and control for any characteristics that are common to each gender and do not change over time. Time fixed effects are captured by θ_t and control for shocks that might affect labor market outcomes and are common to all individuals, such as an economic recessions in the country. Standard errors are clustered by month-year to allow for correlation within time periods and provide standard errors that are robust to heteroskedasticity and autocorrelation.

To interpret the estimated β_1 coefficient as the causal effect of the subsidy on employment, it needs to be true that in the absence of the parental care subsidy the control group (untreated fathers of infants) would have had similar trends (but not necessarily level) to the treated group (mothers of infants), and that no other factors affecting labor market outcomes occurred at the same time as the implementation of this new policy. To explore the validity of the design, I extend the DD analysis to an "event-study" analysis. In practice, this means estimating equation (1) with year effects interacted with the treatment indicator. I then plot the treatment effect by year, which allows for visualization of pre-treatment trends and the evolution of the average treatment effect in the post period. Results are included in section 6.

4.2 Labor hours and income effects in the short-run

Next, I study the effect that the parental care subsidy has on the intensive margin by looking at hours worked, labor income, total income, job experience, and number of jobs, conditional on employment. For this analysis I restrict the sample to all employed fathers and mothers of infants and employ a triple difference (DDD) design. The first difference compares these outcomes across time (before and after the implementation of the policy), the second difference compares mothers to fathers of infants, and the third difference refers to the sector of employment. In this third difference, I take advantage of the fact that public sector workers as well as informal workers do not have access to this subsidy and thus can serve as a control group within the sample of employed mothers of an infant. This strategy controls for any possible differential trends across sectors of employment that affect all of its workers.

This DDD approach allows me to study the effect of the policy on the intensive margin of work by comparing mothers of infants to fathers of infants, employed in the private sector (treated) versus public and informal sector (control), before and after the parental care

subsidy was implemented. Specifically, I estimate the following DDD equation:

$$\begin{aligned} Y_{ist} =& \beta_1 After_t * Woman_i * Private_{is} + \beta_2 After_t * Private_{is} + \\ & \beta_3 Woman_i * After_t + \beta_4 Woman_i * Private_{is} + \\ & \gamma' X_{it} + \delta_i + \theta_t + \eta_s + \epsilon_{it} \end{aligned} \tag{2}$$

where $Private_{is}$ is a binary variable that takes the value of one if individual i works formally in the private sector and zero otherwise, and η_s is a sector (private, public, and informal) fixed effect. The rest of the variables and parameters are as defined in equation 1. The main parameter of interest is β_1 (the triple-difference estimate), and β_2, β_3, and β_4 are the estimates of the double interaction terms. Standard errors are clustered at the month-year level.

4.3 Medium-run effects on labor market outcomes

To understand whether the parental care subsidy's effects are persistent, I focus on the labor market outcomes of mothers whose youngest child is aged 1 to 3 years old. These women are not currently exposed to the subsidy, but depending on when their children were born, could have been exposed to the subsidy in the past. At this point one data restriction deserves mention. The ECH does not allow me to directly identify women affected by the program, because I do not observe individuals history of employment and thus I do not know whether the mothers in the sample were employed around the time of childbirth, therefore, I estimate ITT effects.

To study the medium term effects of the policy on employment, I use a DD design that compares mothers of young children (1 to 3 years old) to untreated fathers of young children, that were born after 2013 (exposed to the subsidy), versus before 2013.

$$Y_{it} = \beta_1 Woman_i * Post_t + \gamma' X_{it} + \delta_i + \theta_t + \epsilon_{it} \tag{3}$$

where $Post_t$ is a binary variable that takes the value of one if individual i has a child born after 2013, and zero otherwise. In addition to all the controls included in equation 1, X_{it} includes fixed effects for the year of birth and age of the youngest child of individual i. These additional fixed effects allow me to compare across individuals whose youngest child has the same age and was born in the same year. The rest of the variables and parameters are as specified in equation 1. The coefficient of interest is β_1, it captures the medium-run effect of the parental care subsidy on employment among mothers of infants.

In a similar fashion, I estimate the medium-run effect of the parental care subsidy on hours worked and income using a DDD approach. I compare mothers of young children to fathers of young children, employed in the private sector (treated) versus public and informal sector (control), whose youngest child was born before and after the parental care subsidy was implemented. Specifically, I estimate the following DDD equation:

$$\begin{aligned} Y_{ist} =& \beta_1 Post_t * Woman_i * Private_{is} + \beta_2 Private_{is} * Post_t + \\ & \beta_3 Woman_i * Post_t + \beta_4 Woman_i * Private_{is} + \\ & \gamma' X_{it} + \delta_i + \delta_t + \eta_s + \epsilon_{ist} \end{aligned} \tag{4}$$

All variables and parameters have the same meaning as above. The main parameter of interest is β_1 (the triple-difference estimate) and it indicates the medium-run causal effects of the parental care benefit on the intensive margin of work (hours worked and income).

Since both, the DD and DDD methods (equations 1 and 3; and equations 2 and 4) leverage variation in the exposure to the subsidy rather than actual take-up, I estimate *intent-to-treat* (ITT) which is preferable to estimates of *treatment on the treated* (TOT) for several reasons.[7] First, TOT estimates could be subject to the same bias from selection into treatment that cross-sectional studies have been criticized for. Second, from a policy-making perspective ITT effects may be more relevant as they allow for feedback effects, whereby the new subsidy could have changed expectations and norms beyond the effects of actually using the half-time option. The implementation of this new form of family policies sent a strong public message about the importance of parental involvement in child-rearing which may have incentivized mothers (and potentially also fathers) who were exposed to the policy but not treated to nevertheless be more involved in the home. However, it is safe to assume that feedback effects on parents who were exposed but not treated are smaller than the first-order-effects on parents who reduced hours of work, such that the ITT results presented here underestimate the true causal effect of parental care subsidy on those who take it.

[7]ITT refers to the average effect of the policy on everyone exposed to it regardless of whether they where treated, whereas TOT refers to the average effect of the policy for those that where actually treated by it. In this case TOT would represent the effect of parental care subsidy for those mothers that reduced hours worked to four or less per week by receiving the parental care subsidy.

5 Results

5.1 Short-run effects on maternal labor market outcomes

Table 2 presents regression results from equation (1) where the dependent variable is an indicator of employment by sector. Overall, I find a statistically significant increase in the likelihood of a mother of an infant being employed following the implementation of the parental care subsidy. The ITT effect is 3.3 percentage points. The magnitude of the estimated effect is considerable. Pre-program employment for the treatment group averages 54 percent. Relative to this baseline, the estimate suggests an increase in employment of 6.1 percent.

As described in section 2, I can distinguish 3 distinct sectors in the labor market of low and middle income countries: Formal-public, formal-private, and informal sectors. The increase in employment among mothers of infants is driven by an increase in employment in the formal sector as shown by column 2. I then further desegregate this effect by looking at private and public employment. The likelihood of being employed in the private sector increased by 17 percent from a pre-treatment mean of 26 percent. This is to be expected because the private-formal sector is the one that was treated. Column 4 also indicates a strong increase in the likelihood of being employed in the public sector of 24 percent from a pre-treatment mean of 9 percent. However, I show in the robustness section that employment in the public sector does not satisfy the underlying identifying assumption of parallel trends, making this result likely bias. Columns 5 and 6 show there are no significant changes in the likelihood of being employed informally or being a "stay-at-home" mom as a result of the policy.

Table 3 presents regression results from equation (2) where the sample is restricted to employed mothers and fathers of infants. *After*Woman*Private* captures the effect of parental care subsidy on the outcome variables among mothers of infants employed in the private sector. *Subsidies* are the self-reported amount in transfers received from the social security authority related to medical and maternity subsidies (including the parental care subsidy). If there is take up we should see an increase in subsidies and a decrease in income received from the employer. Indeed, women employed in the private sector experience a large and statistically significant increase in transfers. The amount of these specific transfers increased by UY$3,819 which represents a 166 percent increase from a pre-treatment mean of UY$2,220. The increase in subsidies is equivalent to 29.5 percent of the average pre-treatment salaries. The increase in subsidies is accompanied by a slightly larger decrease in salaries received from employer of UY$5,300 a 40 percent decrease from a pre-treatment mean of UY$12,927. The increase in subsidies balanced with a decrease in income received from the employer

produced an statistically insignificant decrease in total income of UY$1,890, a 12 percent increase from a pre-treatment mean of UY$15,503.[8]

Hours worked refers to the self-reported number of hours worked in a standard week, it does not refer to the number of hours in a reference week. Thus, even if women are working half-time during the length of the subsidy they might still report the otherwise full-time job hours. Because of this I cannot distinguish the number of hours worked during the subsidy from the number of hours that the person would have worked otherwise. Keeping this caveat in mind, the parental care subsidy had an small but statistically significant negative effect on hours worked of 2 hours and 21 minutes per week from a pre-treatment mean of 37 hours and 40 minutes. This could be product of some mothers reporting half-time work during the subsidy, or it could be a result of some mothers reducing their normal hours after the utilization of the parental care subsidy. Given aggregate data of program take up, this effect is likely bias downwards by mothers reporting their contractual work hours and not their actual hours of work during the utilization of the subsidy. Finally, the number of jobs mothers of infants hold does not change with the policy.

5.2 Heterogeneity

Prior research suggests that disadvantaged mothers may be more responsive to paid family leave than their more advantaged peers (Baker & Milligan 2008b; Rossin-Slater, Ruhm, & Waldfogel 2013; Carneiro, Loken, & Salvanes 2015; Stearns 2015; Lichtman-Sadot & Pillay Bell 2017). I study whether this is also the case for Uruguay's parental care subsidy by examining whether this policy produces heterogeneous treatment effects according to several markers of disadvantage, including: education, age, and whether the husband or partner lives in the same home. Additionally, there is evidence that suggests that these effects might differ according to birth order, thus I distinguish between mothers that have one child and those that have more children at the time of the survey.

Table 4 shows point estimates, standard errors and pre-treatment means for the *Woman*After* coefficient from equation (1) estimated using subsamples according to individual's characteristics mentioned. Column (1) presents the effects on overall employment, columns (2) to (4) present the effect by sectors and column (5) shows the effect on "stay-at-home" mom. The effects of the parental care subsidy on employment are the largest for women that have low-education. The likelihood of employment of a mother of an infant that has not finished middle school increased by 10 percent. This large effect is composed by an increase in employment in both formal-private and public sectors. In particular, parental care subsidy is

[8]UY$ denotes Uruguayan pesos, as of December 31, 2019 UY$1 = US$0.0268, or US$100 are equivalent to UY$3,660.13

associated with a 4.9 percentage point (30 percent), and 1.9 percentage point (190 percent) increase in employment in formal-private and public sector respectively. On the other hand, women with higher levels of education show insignificant increases in employment and an increase in the likelihood of being a "stay-at-home" mom of 1.7 percentage points (7.7 percent). Parental care subsidy increases the likelihood of employment for older, married women, with more than one child the most, as a result of large increases in formal employment and some decreases (not always significant) in informal employment and "stay-at-home" mom. Overall the heterogeneity analysis supports evidence from the literature that paid flexibilities, in this case reduction of hours worked without wage loss, benefits disadvantaged mothers the most.

5.3 Medium-run effects on maternal labor market outcomes

To understand whether the parental care subsidy's effects are persistent, I next examine the medium-run effects of the parental care subsidy on labor market outcomes of mothers whose youngest child are 1, 2, or 3 years old.

Slightly more than 60 percent of mothers with a child that is 3 years old or younger worked in the reference week. Table 5 summarizes the effect of the parental care subsidy on medium-term employment outcomes. There is a significant increase in employment on the extensive margin, between the pre- and post-treatment periods, compared to untreated men. Overall employment increases by 2.7 percent. This effect is driven by an increase in the likelihood of being employed in the public sector, which increased by almost 18 percent from a pre-treatment mean of 9 percent. It is worth mentioning that public sector job arrangements are usually more stable, flexible and provide more benefits for the worker and her family. Additionally, the results suggest a decrease in informal employment, although not significant, and a significant decrease in the likelihood of being a "stay-at-home" mom. These results point at the potential benefit of this policy to foster paid work as well as increase the quality of jobs by increasing formal employment among mothers of young children.

Next, I explore the effects of the parental care subsidy in the intensive margin by looking at income, hours worked, job experience, and number of jobs held by mothers of young children compared to untreated fathers of young children, born before and after the implementation of the policy, conditional on employment. Table 6 presents the DDD estimates from equation 4. Given that parents are eligible for the parental care subsidy only until the child is 6 months old, there should not be any significant differences between subsidies received by mothers and fathers, before and after the implementation of the policy. Column (1) shows that this is case, which also supports the validity of this variable as a proxy for policy take-up in the short-run. Moreover, there is no significant change in total income for

mothers of young children employed in the private sector.

The DDD estimates indicate a significant decrease in hours worked, of slightly more than 2 hours per week from for a pre-treatment mean of 38 hours, and a reduction of job experience of almost half a year from a pre-treatment mean of 4.3 years. A possible explanation for these findings is that the parental care subsidy increases preferences for flexible job arrangements which induced some mothers to switch from full-time work to part-time work. However, additional research is needed to identify the exact pathways for the observed effects. Lastly, there are no significant effects of the policy on the numbers of jobs held by mothers of young children in the medium-run.

6 Robustness

A key assumption in a DD analysis is that in the absence of the parental care subsidy the control group (untreated men) would have similar trends (but not necessarily level) to the treated group (women), and that no other factors affecting labor market outcomes occurred at the same time as the implementation of this new policy. To explore the validity of the design, I extend the DD analysis to an "event-study" analysis. In practice, this means estimating equation (1) with year effects interacted with the treatment indicator. I then plot the treatment effect by year, which allows for visualization of pre-treatment trends and the evolution of the average treatment effect in the post period. Figure 4 to 9 present these results.

Figure 3 shows parallel pre-treatment trends on employment as well as a break in the trend in 2015 with a positive and (slightly) significant DD coefficients after that. Analyzing each sector individually reveals different behaviors across sectors. Figure 5 indicates parallel pre-treatment trends and increasing coefficients in the post-treatment period, probably as a result of increased take-up as shown in Figure 1. Figure 6, revels a different scenario for employment in the public sector. There seems to be an positive pre-treatment trend where women with infants were already increasing their likelihood of employment, and this trend seems to stop after 2014. The result presented in table 1 is likely bias given the absence of parallel trends. Lastly, Figure 7 shows overall no changes in informal employment from 2009 to 2019.

Additionally, the identification assumption of the DD design would be violated if the parental care subsidy induced selection into the sample through impacts on fertility patterns. Ideally I would like to study whether women employed in the private sector are more or less likely to have an additional child as a result of the policy. Unfortunately the survey does not include information on the history of employment of mothers, thus I do not observe the

sector of employment at the time of previous births. However, I do observe the number of children at the time of the survey and their age profile which allows me to address this challenge.

To evaluate the plausibility of selection into or out of the sample of mothers of infants as a result of changes in fertility I estimate regression (2) using number of children as the outcome variable. If women employed in the formal-private sector have more children as a result of the new subsidy, we should see a positive and significant effect on the number of children. In other words, the new born would be of higher birth order compared to those of mothers employed in the public or informal sector. Figure 10 presents the estimated coefficient disaggregated by year. Mothers of infants employed in the formal-private sector do not have more children after the implementation of the PCS compared to mothers of infants employed in public or informal sectors. Overall the effect is small, negative and insignificant.

I then repeat the exercise using the medium-term sample of employed women whose youngest child is 1, 2 or 3 years old. Figure 11 presents the estimated coefficient disaggregated by year of birth of youngest child. Even though the aggregated result is insignificant the event study plot shows that the introduction of the policy is associated with a small increase in the number of children of employed women in the formal sector compared with employed women in the public or informal sector. In line with short term and medium-term results on employment, this suggests that the parental care subsidy made private sector jobs more attractive for mothers of young children increasing their labor force attachment.

7 Conclusions

I study the introduction of a novel form of family policy implemented in Uruguay in 2014: A subsidy that allows parents employed in the formal-private sector to work half-time while receiving full-time pay, from the time maternity leave ends, and until the child is 6 month old. Even though both mother and father can alternate access to the parental care subsidy, in practice, approximately 98 percent of beneficiaries at any given month are women, highlighting an extreme gender gap in utilization, which has been documented in the literature for other gender-neutral family leave policies.

In particular, I analyze the effect that this policy has on labor force participation, labor income and hours worked for mothers of infants utilizing household survey data in a difference-in-differences and triple-difference framework.

I find that mothers are more likely to be employed following the implementation of the parental care subsidy in the short and medium-run. Mothers are 6.1 percent more likely to be

employed during the first year of their child from a pre-treatment mean of 54 percent. This effect is driven by an increase in the likelihood of being employed in the formal-private sector, especially by disadvantaged mothers (those who are less educated). I study the effect that the parental care subsidy has on the intensive margin by looking at income, job experience, and number of jobs, conditional on employment. For this, I leverage the fact that public sector workers and informal workers do not have access to the new subsidy and thus can serve as a control group within the sample of employed mothers of infants. I find a large increase in subsidies received from the government, consistent with program take-up, with no significant changes in total income for mothers of infants employed in the formal-private sector after the implementation of the policy.

In the medium-run, 1 to 3 years after the birth of a child, I find an increase in the likelihood of employment for mothers. Moreover, mothers of a young child born after the implementation of the policy and employed in the formal-private sector decreased their hours worked by 5% without a significant decrease in income.

Overall, this study provides novel evidence that extensions of flexibilities after birth in the form of fully paid half-time employment causes higher labor force attachment for female workers in the short and medium-run, especially for disadvantaged mothers. The results are robust to changes in the specification and to an extended "event–study" analysis which allows for visualization of pre-treatment trends as well as the evolution of the average treatment effect in the post period.

This paper contribute to the literature on family policies in several ways. First, the analysis of parental leave policies is largely focused on high-income countries, and on the length of leave and who gets it. My paper pushes the literature in a new direction by describing and evaluating a totally different kind of policy. Expanding the scope of work on this topic increases our understanding of the effects of different policy characteristics and can lead to policy innovation. Second, I provide a causal examination of the short and medium-run effects of the parental care subsidy on female labor market outcomes. This complements studies that focus on take-up determinants and short-run effects of parental leave (Bartel et al., 2018; Romero-Balsas, 2012; Tanaka & Waldfogel, 2007). Third, my work has policy implications specially relevant for low and middle-income countries that currently have low levels of parental leave and are specially interested in increasing formal employment for women.

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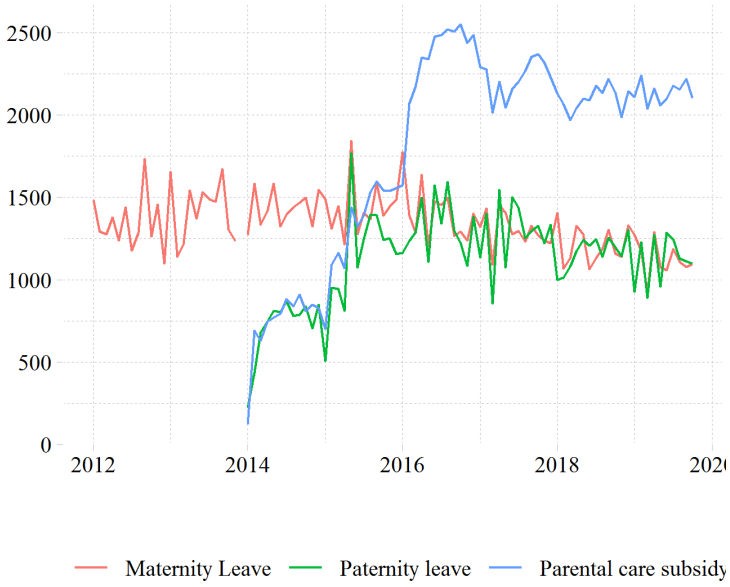
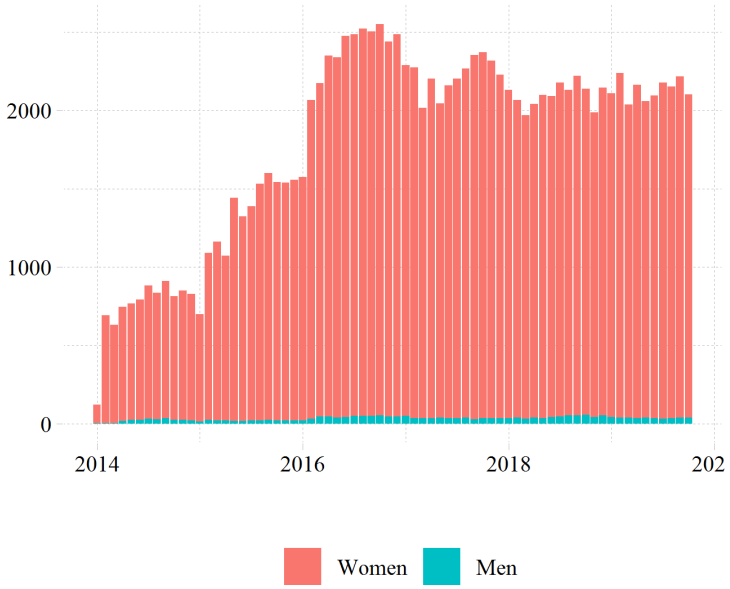
8 Figures and Tables

Figure 1: Number of beneficiaries



Notes: Aggregated data from program utilization published by BPS. Number of beneficiaries per month by type of subsidy.

Figure 2: Parental care subsidy by gender of beneficiary



Notes: Aggregated data from program utilization published by BPS. Number of beneficiaries of parental care subsidy per month by gender.

Figure 3: Labor-leisure choice with new budget constraint

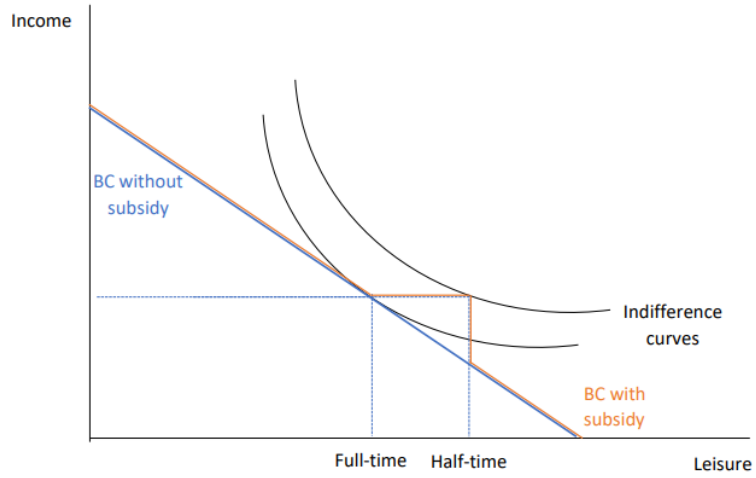


Table 1: Summary statistics

	Fathers				Mothers			
	Pre-January 2014		Post-January 2014		Pre-January 2014		Post-January 2014	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Employment								
Employed	0.92	0.27	0.89	0.32	0.54	0.50	0.55	0.50
Formal	0.62	0.48	0.59	0.49	0.40	0.49	0.45	0.50
Formal-Private	0.51	0.50	0.49	0.50	0.29	0.45	0.33	0.47
Public	0.09	0.29	0.08	0.27	0.09	0.29	0.11	0.31
Informal	0.30	0.46	0.30	0.46	0.14	0.34	0.10	0.31
Inactive-Housework	0.00	0.06	0.00	0.07	0.31	0.46	0.31	0.46
Panel B: Income and Hours Worked								
Income from subsidies	60	814	92	1230	1277	6955	3761	18564
Income from employer	15056	15343	25393	24075	12026	16177	21647	21024
Total labor income	15368	15327	25937	24194	14235	17015	27141	26848
Hours worked	45.46	14.32	42.61	13.65	33.40	15.01	32.41	13.92
Job experience (years)	4.20	5.46	4.18	5.18	4.28	4.56	4.71	4.34
Number of jobs	1.10	0.34	.07	0.29	1.12	0.39	1.13	0.42
Panel C: Individual Characteristics								
Number of children	2.50	1.53	2.32	1.35	2.32	1.45	2.13	1.24
Age	29.77	7.36	29.67	7.49	29.18	7.07	29.61	6.98
Married	0.84	0.37	0.85	0.36	0.75	0.43	0.78	0.41
Ed: Primary	0.47	0.50	0.44	0.50	0.34	0.47	0.30	0.46
Ed: Middle	0.28	0.45	0.28	0.45	0.29	0.45	0.27	0.44
Ed: Secondary	0.19	0.39	0.22	0.42	0.34	0.47	0.41	0.49
Capital city	0.35	0.48	0.31	0.46	0.40	0.49	0.38	0.48
Sample size	3876		3477		8008		7797	

Notes: This table shows summary statistics for mothers of infants (treatment group), and untreated fathers of infants (control group) before and after January 2014, when the parental care subsidy was implemented. Labor market outcomes are dummy variables. *Formal* is not equal to *Formal-Private* plus *Public* because neither category includes entrepreneurs with more than one worker who are registered with the social security authority but are not eligible to receive the parental care subsidy.

Table 2: Extensive margin: Short-term effects of parental care subsidy on employment

	Dependent variable:					
	Employed	Formal	Formal-Private	Public	Informal	"Stay-at-home"
	(1)	(2)	(3)	(4)	(5)	(6)
Woman*After	0.033***	0.062***	0.044***	0.022***	−0.004	0.006
	(0.010)	(0.013)	(0.013)	(0.008)	(0.008)	(0.008)
Mean	0.54	0.4	0.26	0.09	0.07	0.31
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	23,158	23,158	23,158	23,158	23,158	23,158
R^2	0.254	0.227	0.095	0.086	0.051	0.199
Adjusted R^2	0.250	0.222	0.089	0.080	0.044	0.193
Residual Std. Error	0.411	0.441	0.452	0.280	0.265	0.368

Notes: This table contains regression estimates where the treated group consists of all mothers of infants and the control group are untreated fathers of infants (whose wife is unemployed, or employed informally). Each column reports a different regression where the outcome variable is a dummy variable that takes the value of one if the individual is employed (1), employed in a specific sector (2-5), or a "stay-at-home" parent. Individual controls include: Age, number of children, education dummies, marital status dummies, region dummies, and ethnicity dummies. Standard errors are clustered at the year-month level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

Table 3: Intensive margin: Short-term effects of parental care subsidy, DDD estimates

	Dependent variable:					
	Subsidies	Labor income-employer	Total income	Hrs. worked	Job experience	Number of jobs
	(1)	(2)	(3)	(4)	(5)	(6)
After*Private	168.430**	5,195.807***	5,235.082***	2.030***	0.456**	0.027**
	(71.580)	(903.959)	(920.647)	(0.687)	(0.225)	(0.014)
After*Woman	39.858	720.900	1,545.727**	2.404***	0.040	0.008
	(159.679)	(785.143)	(784.051)	(0.701)	(0.243)	(0.015)
Woman*Private	1,967.170***	−2,082.215***	−510.868	3.998***	1.431***	−0.028*
	(213.325)	(726.986)	(736.521)	(0.642)	(0.194)	(0.014)
After*Woman*Private	3,819.157***	−5,300.101***	−1,715.908	−2.346***	0.088	0.015
	(701.963)	(1,126.570)	(1,269.810)	(0.847)	(0.267)	(0.020)
Mean	2220.84	12927.48	15950.34	37.7	4.14	1.1
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	15,254	15,254	15,254	15,254	15,254	15,254
R^2	0.062	0.299	0.268	0.241	0.266	0.052
Adjusted R^2	0.052	0.292	0.261	0.234	0.259	0.043
Residual Std. Error	10,386.520	16,775.590	19,111.710	13.443	4.186	0.359

Notes: This table contains regression estimates where the treated group consists of mothers of infants employed in the formal-private sector and the control group are mothers of infants employed informally or in the public sector and employed fathers of infants. Each column reports a different regression. *Hrs. worked* refers to number of hours a workers works normally, it does not refer to the number of hours in a reference week. Thus even if women are working part-time during the length of the subsidy they might still report the otherwise full-time job. *Subsidies*, *Labor income-employer* and *Total income* refer to the total amount received last month from the social security system, employer and in total, respectively. Individual controls include: Age, number of children, education dummies, marital status dummies, region dummies, and ethnicity dummies. Standard errors are clustered at the year-month level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Table 4: Heterogeneous effects

	Employed	Formal-Private	Public	Informal	"Stay-at-home"
Low Education	0.036**	0.049**	0.019**	0.003	0.009
	(0.017)	(0.019)	(0.008)	(0.013)	(0.016)
	[0.35]	[0.16]	[0.01]	[0.09]	[0.46]
High Education	0.015	0.026	0.023*	−0.015	0.017**
	(0.013)	(0.020)	(0.013)	(0.011)	(0.008)
	[0.65]	[0.33]	[0.14]	[0.05]	[0.22]
1 child	0.021	0.058**	0.021	−0.007	0.015
	(0.019)	(0.026)	(0.015)	(0.015)	(0.013)
	[0.6]	[0.32]	[0.12]	[0.05]	[0.25]
2 or more children	0.040***	0.036**	0.026***	−0.003	0.001
	(0.013)	(0.016)	(0.009)	(0.010)	(0.011)
	[0.5]	[0.23]	[0.08]	[0.07]	[0.34]
Age <30	0.027*	0.052***	0.010	0.0005	0.029**
	(0.015)	(0.018)	(0.010)	(0.014)	(0.012)
	[0.42]	[0.22]	[0.05]	[0.07]	[0.36]
Age>30	0.045***	0.040**	0.039***	−0.008	−0.024**
	(0.012)	(0.019)	(0.013)	(0.010)	(0.010)
	[0.66]	[0.32]	[0.13]	[0.06]	[0.26]
Married	0.034***	0.053***	0.024***	−0.006	−0.005
	(0.010)	(0.016)	(0.009)	(0.008)	(0.008)
	[0.54]	[0.27]	[0.11]	[0.05]	[0.34]
Single	0.073**	0.032	0.014	0.009	0.030
	(0.029)	(0.030)	(0.013)	(0.026)	(0.018)
	[0.52]	[0.25]	[0.05]	[0.12]	[0.22]

Notes: Standard errors in parentheses. Pre-treatment means in brackets. The sample in each row consists of individuals that share that characteristic. All coefficients come from different regressions, where the dependent variable is denoted in the column's name and the subsample is indicated in the panel's name. Regressions control for all other individual characteristics and fixed effects in equation (1). Standard errors are clustered at the year-month level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Table 5: Extensive margin: Medium-term effects of parental care subsidy on employment

	Dependent variable:					
	Employed	Formal	Formal-Private	Public	Informal	"Stay-at-home"
	(1)	(2)	(3)	(4)	(5)	(6)
Woman*Post	0.017**	0.026***	0.012	0.016***	−0.009	−0.011**
	(0.007)	(0.009)	(0.010)	(0.006)	(0.007)	(0.005)
Mean	0.62	0.44	0.33	0.09	0.18	0.24
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	62,316	62,316	62,316	62,316	62,316	62,316
R^2	0.185	0.200	0.095	0.078	0.065	0.154
Adjusted R^2	0.183	0.198	0.092	0.075	0.062	0.151
Residual Std. Error	0.409	0.448	0.464	0.283	0.392	0.342

Notes: This table contains regression estimates where the treated group consists of all mothers whose youngest child is 1, 2 or 3 years old, and the control group are untreated fathers of young children (whose wife is unemployed, or employed informally). None of these regressions are conditional on employment. Post is a dummy variable that takes the value of one if a child in the family was born during or after 2014. Regressions control for all individual characteristics and fixed effects in equation (1) and fixed effects for the age of youngest child in the family and their year of birth. Standard errors are clustered at the year-month level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels.

Table 6: Intensive margin: Medium-term effects of parental care subsidy, DDD estimates

	Dependent variable:					
	Subsidies	Labor income-employer	Total income	Hrs. worked	Job experience	Number of jobs
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Private	71.231*	4,107.106***	4,203.214***	1.800***	0.745***	−0.001
	(39.356)	(1,065.117)	(1,074.996)	(0.510)	(0.175)	(0.010)
Post*Woman	8.367	−214.544	876.898	2.954***	0.423***	−0.019*
	(15.007)	(754.589)	(762.124)	(0.548)	(0.142)	(0.011)
Woman*Private	18.759	−673.979	−993.611**	4.270***	1.462***	−0.022***
	(18.851)	(440.155)	(433.974)	(0.368)	(0.120)	(0.008)
Post*Woman*Private	−29.984	−526.412	−1,045.497	−2.139***	−0.474**	0.022
	(44.126)	(1,096.909)	(1,106.518)	(0.648)	(0.188)	(0.015)
Mean	109.92	16854.42	18067.87	38.1	4.3	1.12
Time FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Observations	44,468	44,468	44,468	44,468	44,468	44,468
R^2	0.008	0.289	0.284	0.217	0.273	0.043
Adjusted R^2	0.004	0.286	0.281	0.214	0.270	0.039
Residual Std. Error	981.730	18,545.560	18,570.240	13.933	4.430	0.356

Notes: This table contains regression estimates where the treated group consists of mothers of young children (1 to 3 years old) employed in the formal-private sector and the control group are mothers of young children employed informally or in the public sector and employed fathers of young children. Each column reports a different regression. *Hrs. worked* refers to number of hours a workers works normally, it does not refer to the number of hours in a reference week. Thus even if women are working part-time during the length of the subsidy they might still report the otherwise full-time job. *Subsidies*, *Labor income-employer* and *Total income* refer to the total amount received last month from the social security system, employer and in total, respectively. Post is a dummy variable that takes the value of one if a child in the family was born during or after 2014. Regressions control for all individual characteristics and fixed effects in equation (3) and fixed effects for the age of youngest child in the family and their year of birth. Standard errors are clustered at the year-month level. ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1, 5, and 10 percent levels.

Figure 4: Effect of Parental Care Subsidy on Employment–Event Study

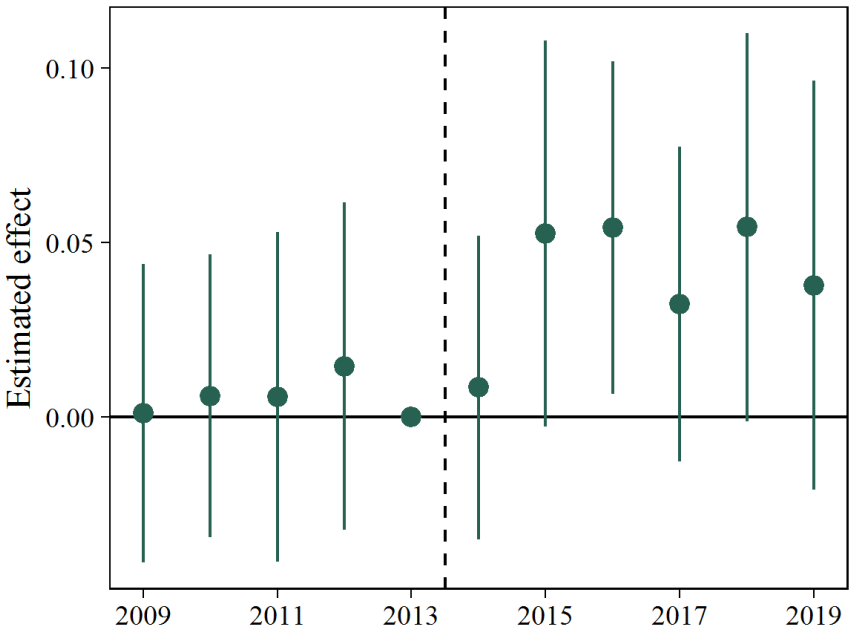


Figure 5: Effect of Parental Care Subsidy on Formal Employment–Event Study

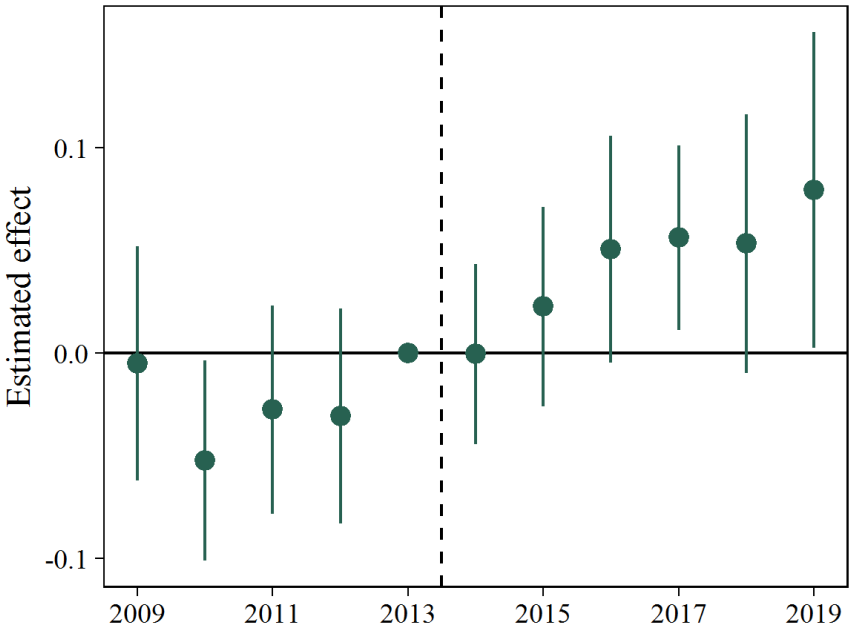


Figure 6: Effect of Parental Care Subsidy on Formal-Private Employment–Event Study

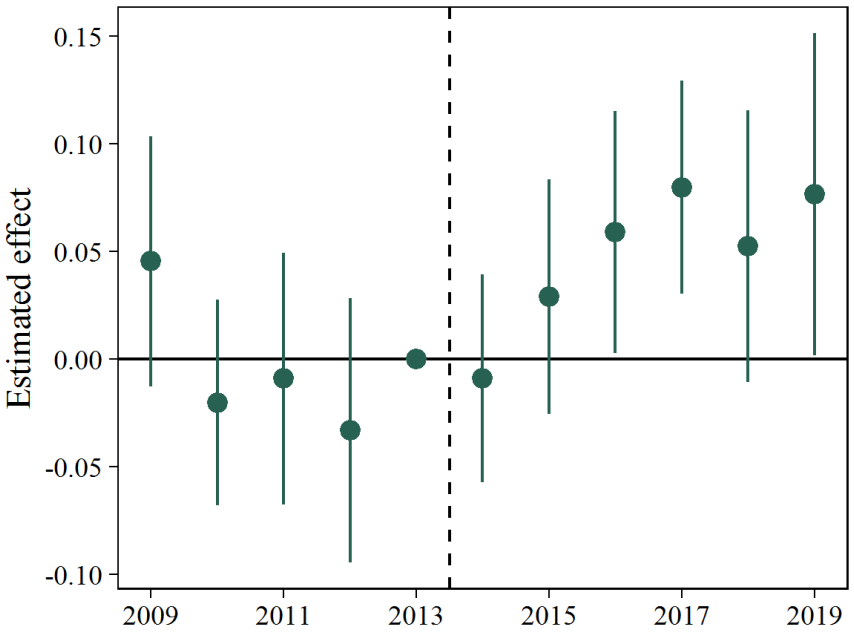


Figure 7: Effect of Parental Care Subsidy on Formal-Public Employment–Event Study

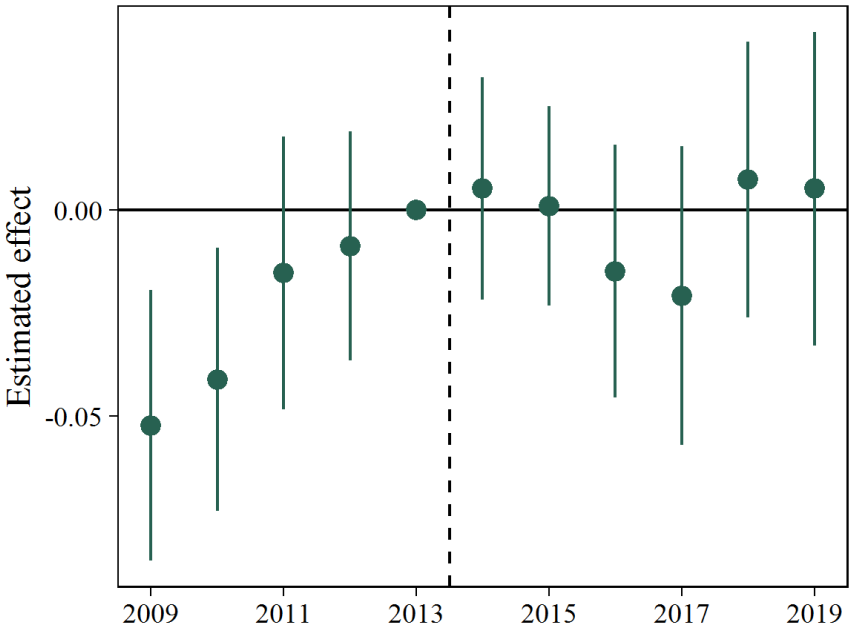


Figure 8: Effect of Parental Care Subsidy on Informal Employment–Event Study

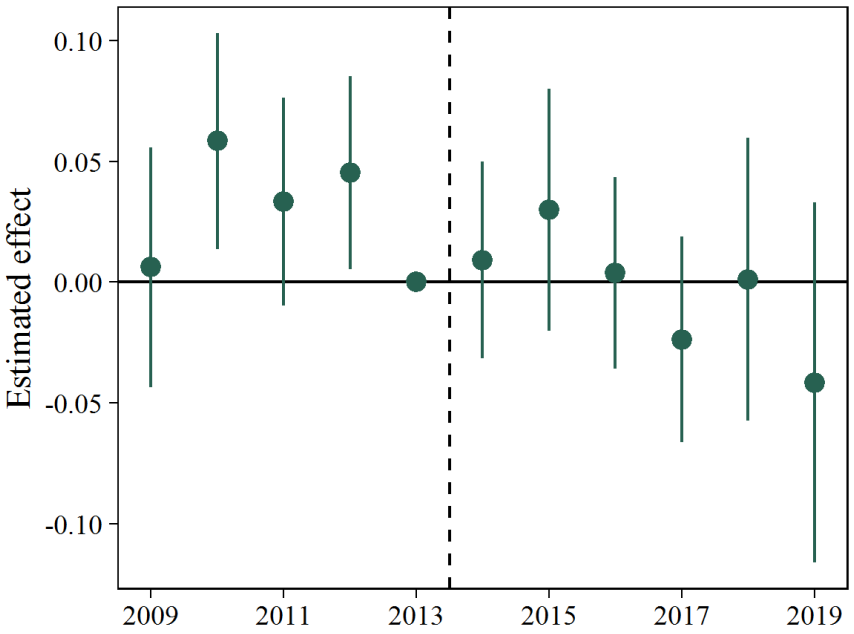


Figure 9: Effect of Parental Care Subsidy on "Stay-at-home"–Event Study

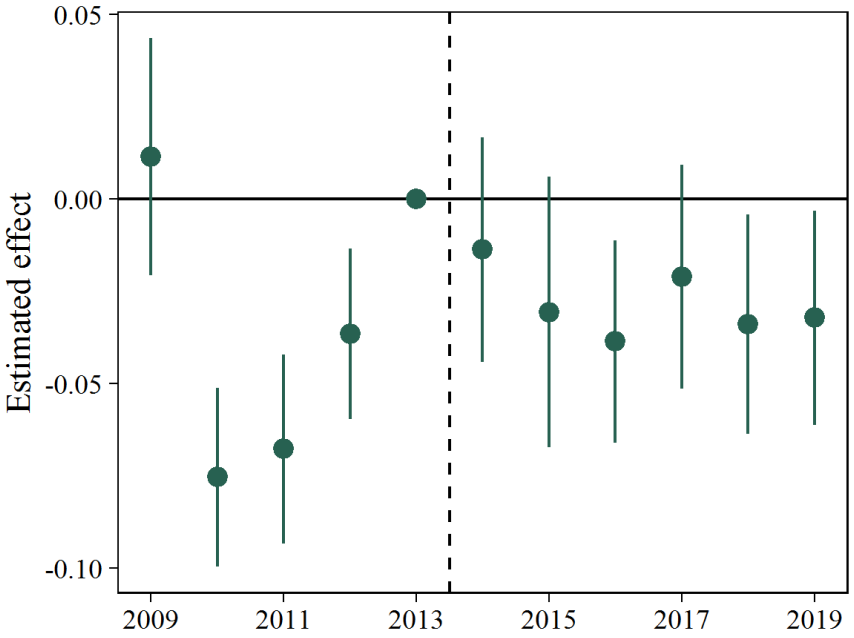


Figure 10: Association between PCS and birth order of infant for employed mothers

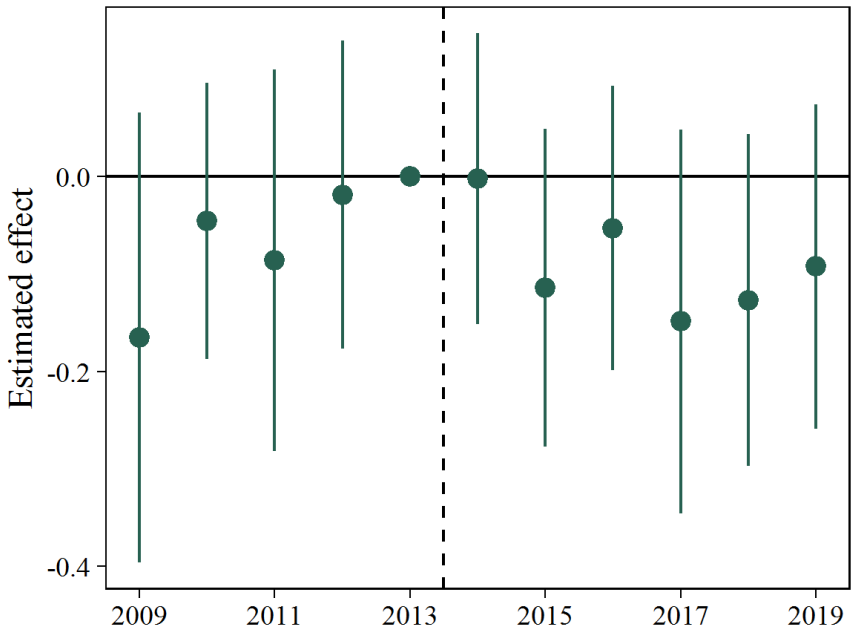
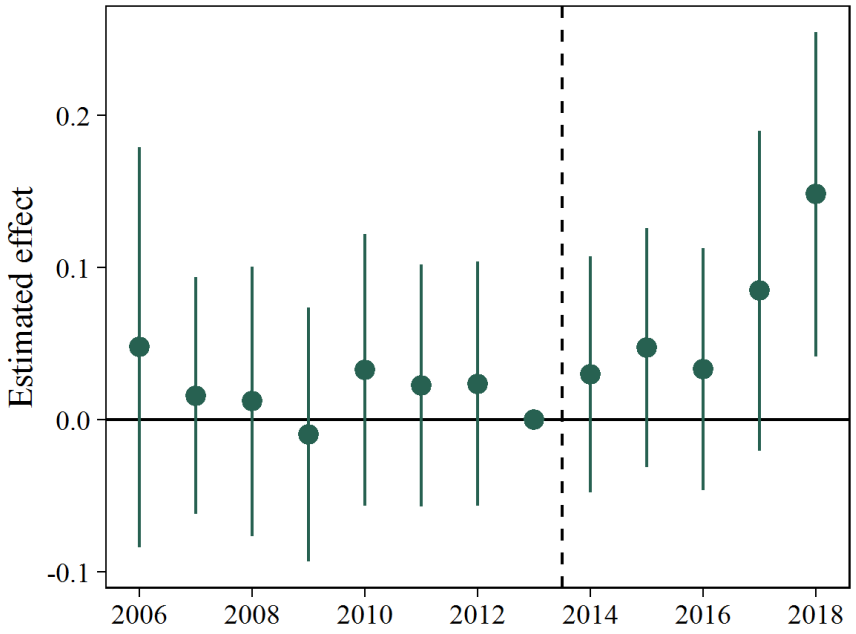
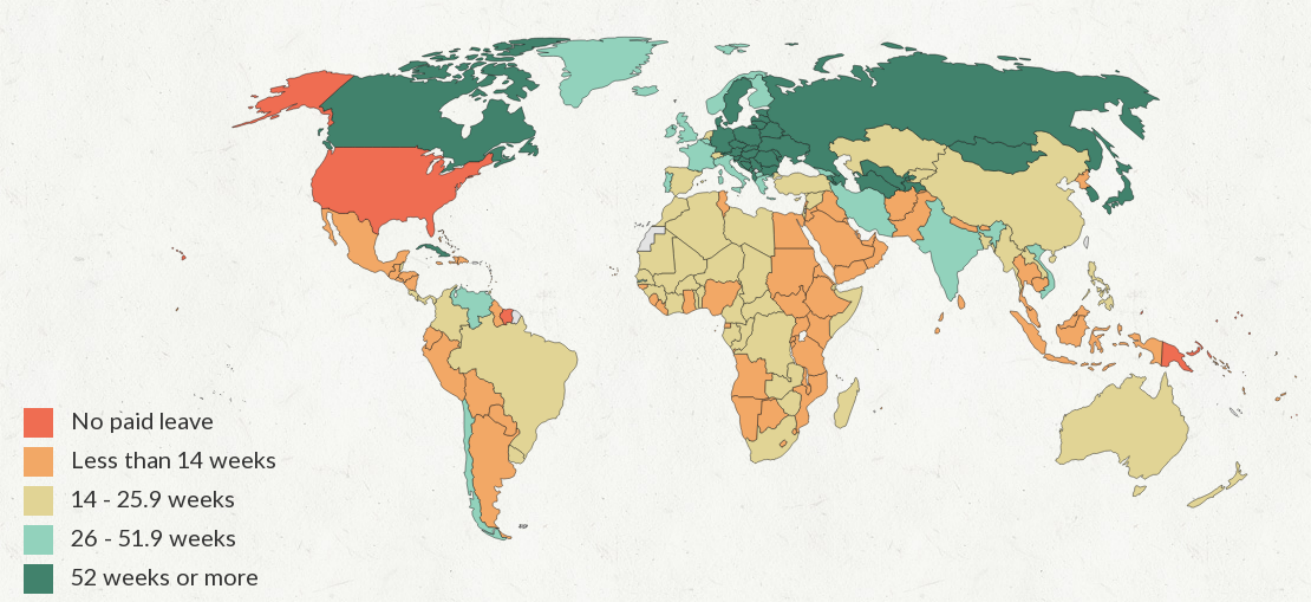


Figure 11: Association between PCS and number of children of employed mothers



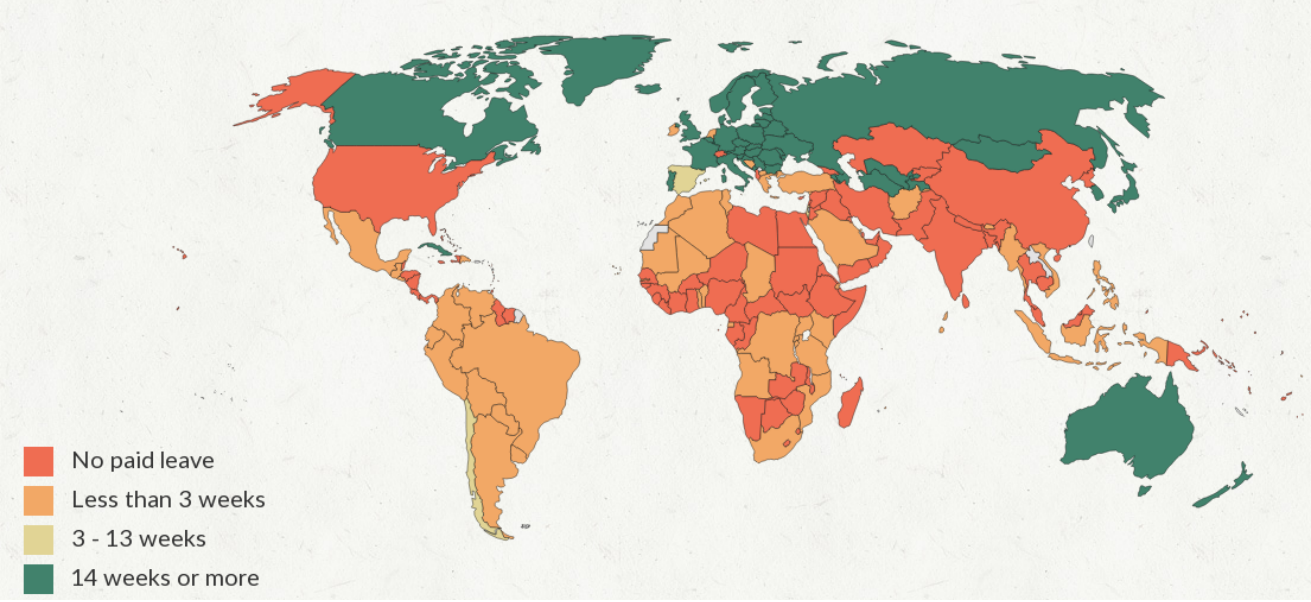
Appendix

Figure 12: Maternity leave regulations across countries



Source: 2020 WORLD Policy Analysis Center

Figure 13: Paternity leave regulations across countries



Source: 2020 WORLD Policy Analysis Center