

Who Benefits from a Maternity Leave Extension?

Evidence from Chile*

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Abstract

This paper studies the short- and medium-term effects of extending parental leave on women's labor market outcomes. I exploit a reform implemented in Chile in 2011 that allowed women to extend parental leave from 84 to 168 days. I combine administrative data on leave claims with employer-employee data to estimate the effect of a longer leave on women's wages and employment seven years after childbirth. My results show that eligible women extend parental leave in 78 days and reduce the use of other sick-leave claims, compared to women who cannot extend their leave. Eligible women are more likely to be formally employed for three years after childbirth, and conditional on employment, their wages are higher compared to those of non-eligible women. The positive employment effects are driven by single women and workers without a college education. These results suggest a longer leave protects single mothers from leaving the formal labor market after childbirth.

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1 INTRODUCTION

Over the past decades, most of the developed world has adopted or extended government-funded parental leave that grant mothers with at least 54 weeks after childbirth ([Dahl et al., 2016](#)). Many developing countries have mimicked this type of policy, and countries like Brazil, Argentina, Chile, and Uruguay have parental leave periods of at least 12 weeks that are fully paid, but all remain below the year. Female labor force participation is lower in developing contexts, in part because of the lack of family friendly policies that incentivize female employment ([Heath et al., 2024](#)), and thus, studying how parental leave works in these context remains an open question.

The literature is abundant with respect to the effects of these policies on labor market outcomes of working mothers, especially in Scandinavian countries with good access to administrative data. Previous research employs empirical strategies that use worker-level data and find small positive effects on employment in the first two years after childbirth. More recent evidence shows maternity leave can have long-lasting effects on women's labor market outcomes in terms of labor market attachment or advancements on their careers ([Bailey et al., 2019](#); [Thomas, 2021](#)), and negative responses from employers ([Gallen, 2019](#), [Ginja et al., 2023](#)). Longer leaves might increase the probability of women leaving the labor force, and increase separation rates because women move to more flexible jobs. This might be especially relevant when parental leave is long; however, shorter leaves and employment protection might be beneficial for working mothers, as they have enough time to recover ([Rossin-Slater, 2018](#)).

This paper studies the consequences of extending parental leave on women's employment and wages in the context of a developing country. I exploit a reform occurred in 2011 in Chile that allowed eligible mothers to extend parental leave from 12 weeks to 24 weeks (84 to 168 days). The reform was passed in July 2011, and it created a discontinuous change in mothers' ability to extend parental leave based on their child birth date. Women who gave birth after May 2 2011 were as good as randomly assigned to extend the number of days on leave. This creates two groups of mothers, the

non-exposed mothers who gave birth before May 2, and the exposed mothers, who gave birth on May 2 or after.

To estimate the effect of the reform, I estimate a difference-in-difference model where I compare the labor market outcomes of exposed women and non-exposed women three years before childbirth, and up to seven years after childbirth. To perform the estimation, I use data on mothers' parental leave claims between January 2011 and December 2011 that I match to a 10% sample of employer-employee records between 2008 and 2018. In this dataset, I observe private sector employment, monthly wages, and workers' firm-to-firm transitions. The validity of my research design relies on the fact that the reform affects exposed and non-exposed mothers differently only through their capacity to extend parental leave. To support this claim I show that the both groups of workers are identical on average based on their demographics at baseline, and I do not find evidence of a discontinuity of workers' demographics or fertility behavior at the cut-off date of May 2 2011.

My results show exposed mothers, relative to non-exposed mothers, extend their parental leave in 78 days on average, and reduce the use and length of other sick-leave claims within the first year of giving birth, especially they reduce the use of paid sick-child leave. In terms of their labor market outcomes, exposed mothers are more likely to remain employed in the formal sector. Exposed mothers are 4 to 5 percentage points more likely to be employed between years 1 and 4, compared to non-exposed mothers. The average effect decreases to 3 percentage points between years 5 and 7 and it is not longer statistically different from zero. Conditional on employment, the wages of exposed workers increase in 6.5 log-points in the medium term, and their separation rate, measured as the probability to be employed to a different employer before childbirth, is not different to those of the non-exposed mothers in the next seven years after giving birth. My results also suggest a small decrease in the probability of working under a temporary contract, though, the results are not different from zero. Finally, I do not find exposed mothers are more or less likely than non-exposed to file a new parental leave claim in the following years, which suggest no fertility effects for

the women in my sample.

Overall, these results show positive effects of the extension to parental leave on women's employment and wages. These results are larger than what the previous literature has found, and they last for longer (more than two years), which suggest that exposed women are more likely to remain employed due to the extension. To better understand the mechanisms behind these results I study how the effects of the reform vary by workers type. I focus my analysis on two margins, marital status and education. I stratify my sample between ever married women and single. The positive effects found on employment and earnings are almost entirely driven by never-married workers. Their employment is higher in 4 percentage points by the end of the 7th year, and their formal wages increase immediately after the second year. On the contrary, the reform does not impact the employment of married workers, but it does change its type: Married women in the exposed group are more likely to be employed under a temporary contract, instead of a permanent contract, which proxies for a more flexible schedule, and their wages are also lower after 7 years. When I study heterogeneity by education, I find that the positive employment effects are driven by women with a high-school diploma or less, and the effects are even larger if they never married. On average, college educated women are not differentially affected by the reform.

The fact that married and never married women respond differently to the reform suggests the extension protected women in different ways, depending on their flexibility schedules. Never married women are more likely to be the breadwinners, while married women choose a more flexible schedule because of another wage-earner in their homes. My results suggest that married women choose this path, as they are more likely to work under a temporary contract in the medium-term at the expense of lower wages. For singles, the reform prevented them from leaving formal employment, with positive effects on their wages. Based on the results by [Berniell et al. \(2021\)](#), who find a larger child penalty in the informal sector in Chile, the effects found in this paper suggest that the extension to maternity leave prevents the most vulnerable workers, singles and lower educated, from leaving formal employment, which has positive impacts in

their wages and careers. This was one of the main arguments accrued by policy makers when advocating for a longer maternity leave, as many working mothers were artificially increasing their parental leave by filing other sick-leave claims or simply leaving formal employment.

This paper relates to the literature concerning the short-run effects of maternity leave on employment, wages, and more recently, on children's outcomes. [Ruhm \(1998\)](#) studies the impact of enacting maternity leave policies in nine European countries between 1963 and 1993. His estimates show that rights to paid leave raise the percentage of women employed: leave legislation raises the female employment-to-population ratio by between 3 and 4 percent, with larger effects for women of childbearing age, but very little effects on earnings. [Gruber \(1994\)](#) studies how government (federal and state) mandates that stipulated that childbirth be covered in health insurance plans shifted the cost of these mandates to the wages of those women. The mechanism is the mandates changed the relative cost of insuring women of childbearing age.

More recently, children's outcomes and mothers' labor supply are studied ([Carneiro et al., 2015](#)). [Dahl et al. \(2016\)](#) study several reforms to maternity leave policies in Norway. [Rossin-Slater \(2018\)](#) concludes that depending on the extension of maternity leave, and if it is paid or not, that set of policies might have negative effects on women's labor experience in the short run, but potentially positive effects in the long-run ([Schönberg and Ludsteck, 2014](#)). In general the establishment of maternity leave policies increases children's health, but the extension of the leave does not. However, in the Chilean case, [Albagli and Rau \(2019\)](#) find that the extension from 12 to 24 weeks of parental leave has positive effects on children's cognitive outcomes and mothers' mental health outcomes. My average results are larger to those of [Schönberg and Ludsteck \(2014\)](#), and I take advantage of heterogeneity by workers type to show extending maternity leave has different effects on different workers.

Finally, this paper also relates to the literature that studies the determinants of female labor force participation in developing countries ([Heath et al., 2024](#)), and interestingly, not much has been said about the effects parental leave in this context,

probably due to a lack of data and policy variation. To the best of my knowledge, only a recent paper by [Machado et al. \(2023\)](#) study the effects of an extension to parental leave in Brazil, and find incomplete take-up and null long-term effects on women's labor market outcomes, as opposed to my results where I find complete take-up, probably because in the Chilean context it is mandatory conditional on eligibility.

The rest of the paper is organized as follows. Section 2 describes the reform of 2011. Section 3 describes the data used in this paper, and Section 4 presents the empirical framework. Section 5 presents baseline results and heterogenous effects, and Section 6 concludes.

2 REFORM OF 2011

In Chile, maternity leave was enacted in 1924, and legislation did not suffer from changes since 1980. Prior to 2011, working mothers were mandated 6 weeks of paid parental leave before the birth and 12 weeks (84 days) after the birth with full income replacement up to percentile 90 (in the earnings distribution), paid through social security contributions. In addition, job protection continued for one year after completion of parental leave. All working mothers with at least 3 months of social security contributions prior to childbirth were eligible for parental leave, regardless of occupation, industry, or type of contract (temporary or permanent). Fathers did not have the benefit of parental leave.

On October 17, 2011, a reform allowed the extension of maternity leave after the birth from 12 up to 24 weeks. The reform allowed mothers to choose between the following options: 1) to extend their leave from 12 to 24 weeks; 2) to not extend their leave but to return to work under a part-time arrangement after the first 12 mandatory weeks, with the option of transferring a fraction of their extended leave to the father. In any of these options, leave before childbirth was 6 weeks and job protection continued for one year after the completion of the leave. In addition, the reform allowed fathers to take 5 consecutive days of paid parental leave after childbirth.

Because of the reform, working mothers had the ability to extend their leave based on their child's date of birth. Mothers of children born in July 25 2011 and after were entitled to a full extension of 12 weeks, mothers of children born between May 2 and July 24 were entitled to a partial increase as they were already on leave, and mothers of children born before May 2 were not entitled to any extension. As [Albagli and Rau \(2019\)](#) show, these dates create three groups of women who face different levels of exposure to the reform as a function of birthdate. I replicate the authors exposure function in Figure 1.

The discussions to pass the law to extend maternity leave started in Congress in February of 2011, after a couple of failed proposals in previous years. The main motivations argued at the time were related to low labor market attachment of women, especially of low income women, low quality childcare alternatives, and the fact that women on maternity leave were artificially extending their parental leave by filling other leave claims, such as sick-child leave for caregivers of children below 1 year old, or mental health sick leaves.

Even though the discussions were highly public in the news, the reform passed relatively quickly as it was approved within 8 months. Thus, it is unlikely that working mothers anticipated the exact date of the reform and strategically delayed childbearing. Because of this, non-exposed workers, i.e. mothers of children born before May 2, serve as a comparison group for exposed mothers entitled to extend their parental leave.

3 DATA

3.1 *Administrative data*

The main dataset used in this study is a 10% random sample of private firms in the Chilean unemployment insurance system (AFC). The AFC is an employer-employee monthly dataset where I observe firms and all their workers between 2008 and 2018. In this data I observe workers' employment histories in the formal sector. Thus, if employed, I observe monthly wages, years of education, gender, industry, firm size,

municipality of employment, and firm-to-firm transitions.

I match all workers in the previous sample to all their medical and parental leave claims from January 2011 to December 2018. This dataset belongs to Chile's Superintendence of Social Security, the entity that administers and supervises the payments of leave claims to workers. In this dataset, I observe parental leave claims filed by working parents. For each parental claim I observe child's birthdate, starting date and ending date of leave, amount of subsidy, workers' age at start of leave, and type of health insurance (private versus public). I also observe if a worker ever files other types of sick-leave claim, or a sick-child leave claims.

By combining these two datasets I construct a sample of working women who filed a parental leave claim in the year 2011 and their employment histories in the formal sector until the end of 2018. The advantage of using administrative data is that I observe women who filed a parental leave claim and their taxable wages in the private sector. The public sector in Chile employs around 10% of formal workers, thus my data is representative of wage workers in Chile. In addition, women in the public sector have the same maternity leave benefits as women in the private sector.

One disadvantage of this dataset is that when I stop seeing a worker in the sample, I do not know whether they become unemployed, or if they are employed in the public sector, the informal sector, or they left the labor force. In the context of a developing country, the informal sector becomes relevant. In Chile it represents approximately 20% of the workers.

3.2 Sample selection and summary statistics

I restrict my main sample to women who filed a maternity leave claim between January 1 2011 and November 22, 2011, aged between 18 and 45 years old, with wage data in the AFC three months prior to childbirth.¹ I include women who give birth until November

¹I do not observe claims filed by men. Before the reform only widowed fathers were entitled to parental leave, and during 2011 only 5 fathers in the whole country chose to take 5 days of parental leave.

22, because it guarantees a symmetric window around the partially exposed group of women.

In Table 1 I present summary statistics for women by exposure to the reform, and in column (4) I report the estimate of running a regression of each covariate on the exposure variable. The first four columns report summary statistics for all women who filled a parental leave claim in 2011. The data shows that exposed and non-exposed mothers are very similar to each other in terms of their age, wage, education, and the characteristics of their employers in the year prior to starting maternity leave. There is only one variable in which the difference is different from 0 at the 5% significance level, that is age, however, the coefficient is very small. In the next four columns I repeat the exercise on the sample of women I find in the AFC in the quarter prior to giving birth, and the results are almost identical. These are the workers in my estimation sample.

On average, women in my sample are 30 years old, 90% have private health insurance. Their average wage was CLP\$490,000, which is equivalent to US\$615/month in 2018, and I observe them, on average, for 7.8 months in the AFC in the year prior to parental leave. It is important to mention that women in my sample are not representative of working women in the Chilean population, as the average wage of a female worker in 2018 was CLP\$400,000 and only 20% of the population has private health insurance. This is consistent with the fact that to be eligible, women should contribute to social security prior to maternity leave, and thus, women who are eligible are in the upper part of the earnings distribution, and more so the women in my estimation sample, because I restrict it to women who I find employed prior to parental leave.

4 EMPIRICAL STRATEGY

My identification strategy relies on the discontinuous change in mothers' ability to extend parental leave by child birth date. Women who gave birth after May 2 2011 were as good as randomly assigned to extend the number of days on leave. Let d_i be the extra number of days a mother is entitled to, and a_i her child's birth rate relative

to July 25. The relationship between d_i and a_i can be written as (Albagli and Rau, 2019),

$$d_i = \begin{cases} 0 & \text{if } a_i > 84 \\ 84 - a_i & \text{if } 0 < a_i < 84, \\ 84 & \text{if } a_i \leq 0 \end{cases}$$

hence, exposure to the reform can be expressed as

$$z_i = \text{exposure}_i = \frac{d_i}{84} \in [0, 1]. \quad (1)$$

To estimate the effects of the reform on women's labor market outcomes, I estimate a dynamic difference-in-difference model that compares exposed mothers ($z_i > 0$) with non-exposed mothers ($z_i = 0$) 3 years before and up to 7 years after childbirth using the following equation,

$$y_{it} = \alpha + \delta z_i + \sum_{\tau \geq -3, \tau \neq -1}^7 \gamma_\tau 1[t = \tau] + \sum_{\tau \geq -3, \tau \neq -1}^7 \beta_\tau z_i * 1[t = \tau] + X'_i \theta + \lambda_t + \varepsilon_{it}, \quad (2)$$

where y_{it} is a labor market outcome for worker i on calendar quarter-year t ; z_i is the exposure to the reform as described above; X_i is a set of controls at childbirth that include: worker's age in dummies, private insurance dummy, and average wage before parental leave; λ_t are quarter-year fixed effect that control for temporal differences in wages and employment, and ε is the error term. Finally, I cluster the standard errors by week of birth.

The coefficients of interest are β_τ that estimate the difference between a fully exposed woman relative to a non-exposed woman at year τ after childbirth, relative to the year prior to childbirth.

4.1 Balance of pre-claim characteristics of workers

The validity of my research design relies on the idea that the reform affects exposed and non-exposed mothers differently only thorough the extension of their parental leave. If this assumption holds, then I should not observe selection into childbirth, the date of child birth, or differences in the types of workers prior to parental leave.

In Figure 2 upper panels, I plot the average daily birth counts around the dates of the reform, controlling for seasonality. Panel (a) shows no significant differences of birth counts in Chile around May 2 2011 nor July 25. More importantly, in panel (b) I plot the average daily parental leave claim counts around the reform cut-offs, and as before, I do not observe differences between groups.

In the lower panels of Figure 2, I perform the same exercise on pre-claim demographics and labor market outcomes for the women in my estimation sample. In none of these variables I find statistical differences between exposed and non-exposed mothers who filed a claim around the dates of the reform, nor evidence of bunching close to the cut-offs. These results are consistent with the high balanced in demographics presented in Table 1.

5 RESULTS

5.1 Take-up

I start the analysis by showing that women who file a maternity leave claim and were entitled to extend increase the duration of their parental leave. In Figure 3 (a) I plot the average number of days on leave by child's date of birth. The figure shows that on average, women are on leave for the days they are entitled to before and after the reform. Figure 3 (b) shows the distribution of days on leave by exposed and non-exposed mothers, which shows that most of non-exposed mothers were 84 days on maternity leave, while the distribution of number of days on leave of exposed mothers is shifted to the right, with the majority of the exposed mothers being on parental day for 168

days or less. In Table 2, I regress the number of days on leave on the exposure variable (z_i), and the results indicate that exposed mothers increase their leave in 80.78 days on average (column (1)), and the coefficient is almost identical after controlling for age, type of insurance, and wages at baseline (column (2)). In the next two columns, I repeat the exercise in the sample of women who are observed in the AFC prior to the beginning of their parental leave, and for this group the average extension is only reduced in one day, thus women in my estimation sample extend their parental leave duration in 79 days (columns (3) and (4)).

5.2 Formal employment and wages

I continue the analysis by studying the formal labor market outcomes of women who filed a parental leave claim and work in the formal sector prior to childbirth. Figure 4 shows the event study coefficients from equation (2) on formal employment in panel (a) by quarters relative to childbirth, and Table 3 averages the effects by year. The results on formal employment show that for at least three years prior to giving birth, exposed and non-exposed women do not experience differential rates in formal employment. After giving birth, women entitled to extend their leave are 16.3 percentage points more likely to be employed between months 4 and 6 after childbirth. This result is mechanical because if a woman is extending her leave and was previously employed, as the first stage showed, she is coded as employed in the administrative data and she is receiving her monthly wage through a subsidy. Thus the relevant period to study the effects of the reform starts the third trimester after childbirth when women are expected to return to work. The results show that in the first year (months 7 to 12), exposed workers are 8 percentage points more likely to be employed in the formal sector, this effect remains positive and statistically significant for 3 more years and equal to 3.4 percentage points. After year 4 the effects are still positive but no longer significant, with an average of 1.7 percentage points (see Table 3 column (1)).

Panel (b) repeats the exercise for wages conditional on employment. Results are noisier but show an upward trend in the average wages of exposed mothers relative

to non-exposed. Table 3 column (2) shows that on average, after year 2 the wages of exposed mothers that remain employed are 5 to 7 log points higher than those of non-exposed to the reform.

In panels (c) and (d) I study the effects of the reform on types of employment. Conditional on being employed, exposed mothers are equally likely than non-exposed to work on a temporary contract, but after year 6, they are more likely to transition to a permanent contract. And, I do not observe large impacts on separation rates conditional on employment, that is, all women are equally likely to remain employed with their employer from before giving birth, and my results suggest this probability is larger for exposed women after year 4, though, the results are not statistically significant.

Finally, in panel (e) I estimate the RDD effect of filling a new parental leave claim. Notice this is not a Diff-in-Diff estimate because I do not observe parental leave claims before 2010. In the results, I do not find evidence that exposed women are more likely to file a new parental leave claim after 2011, which suggests no fertility effects of the reform.

Overall, these results suggest the average woman in my sample is positively impacted by the reform, and these effects are larger than what the previous literature has found for developed contexts. Exposed women are more likely to remain employed in the formal sector, these effects decrease with time, but last for 4 years after childbirth and remain small and stable after that. Women's wages increase, which suggests the reform incentivized the employment of women with higher wage growth, and these effects are notorious after year 2.

5.3 Other leave claims

One of the main reasons to pass the reform was that working mothers on parental leave were artificially extending their leave by filing other types of sick leave. In Figure 5 I study if exposure to the reform reduces the use and extension of other paid leave within the first year after childbirth. Panels (a), (c), and (e) show the exposure effect on the likelihood of filling a claim on sick leave, sick child leave, or mental health leave, and

panels (b), (d) and (f) show the average duration of these leaves in days.

The figure shows that exposed women are less likely to use other sick leaves between months 4 and 6, which is expected because they are on parental leave. The effect is especially large on the usage of paid leave due to a sick child below the age of one (panel (c)), which according to the Social Security Administration, was the most likely to be used to extend parental leave. After month 6, exposed women are more likely to use this leave, however, the decrease due to extended parental leave is larger than the increase after month 6. Panel (d) shows that on the intensive margin, women use this paid leave for shorter periods, the reduction is on average 1.6 days. Thus, the reform reduces both the use and the length of other sick leave claims, the average effects on days are in Table 4. If we take all these estimates together, I can estimate the effect on the total number of days on leave during the first year of childbirth: The total effect of the reform is 76.7 days (79.02-0.403-1.572-0.352), from a baseline of 89 days on leave for the non-exposed mothers.

5.4 *Heterogeneity*

The baseline results show exposed women are more likely to remain employed in the formal sector after a longer parental leave and their wages grow faster than those of non-exposed mothers; to understand the mechanisms, in this section I study heterogeneous effects by workers demographics, in particular marital status and education.

5.4.1 *Ever married and never married women*

In the claims data I do not observe marital status, but in the AFC I can observe workers characteristics by the end of my sample. With this information I stratify the sample between ever married women (married and divorced) and single women. I study how the effects of the reform are different by group, and the results are reported in the upper panels of Figure 6. For better exposition I drop the confidence intervals, but I report significance levels. The results show the positive average effects of the reform

on formal employment are almost entirely driven by single women (panel (a), who are 10 percentage points more likely to be employed in the first year after childbirth, the effect decreases with time, but remains positive and statistically significant at 10% over the entire sample period, after 7 years never married exposed women are 4 percentage points more likely to remain employed in the formal sector. On the contrary, ever married exposed women see an increase in their employment on year 1, but the effect becomes null after that. In panel (b) I look at their wages, and as with employment, the positive effects on earnings are driven by single women.

When I look at the types of employment in the second row of Figure 6, the results show single women are more likely to reduce the probability of working on a temporary contract and they do not separate more from their pre-birth employer, as opposed to ever married exposed women who by year six are more likely to be working on a temporary contract compared to non-exposed married women. These results imply that a longer leave induces married women to choose a more flexible schedule, at the expense of higher wages in the medium-term, while single women who are more likely to be sole earners remain employed and see advances in their career progressions measured as wage growth and permanent contracts.

5.4.2 Education

I repeat the previous exercise and stratify my sample by college educated workers and high school or less. I also include a category for missing education, which represents a 15% of the workers in my sample, without differences by exposure to the reform (see Table 4). The results are in Figure 6 lower panels. Women with low education are the most likely to remain employed, as opposed to college educated women, for whom the exposure effects of the reform are not different from zero after year 1. However, both groups of workers see an increase in their wages, with more stable changes for women with lower education, but these effects are not different from zero in levels. The effects on types of employment are not as clear as by marital status, and they are also noisier. By the end of the sample period both educational level groups see a reduction on the

probability of working on a temporary contract but these effects are not different from zero.

These results imply the reform impacted non-college versus college women in different ways, but its effects interact with marital status. In Appendix Figure A.1 I stratify my sample by marital status and education, and the results show the positive effects of the reform are driven by single women, even within educational levels. Probably the reform did not impact employment of college educated women because on average they were more attached to the labor market in the first place, compared to lower educated women; however, the reform did impact the wage progression of women in both groups, but especially of those who have never married, who are likely to be more constrained and cannot choose more flexible schedules after childbirth, thus, a longer leave is beneficial. [Albagli and Rau \(2019\)](#) find women who extended their parental leave in Chile using the same design had lower levels of anxiety and could breastfeed for longer, and thus could return to work having recovered better. In fact, they find that women who benefit the most, are lower educated women. Their results, combined with my results, suggest positive impacts on women's careers, especially among more vulnerable women (single and lower educated).

6 CONCLUSIONS

This paper estimates the short- and medium-term consequences of extending parental leave on mothers' labor market outcomes. I exploit a reform implemented in Chile in 2011 that increased maternity leave from 84 days to 168. The implementation of the reform creates a discontinuous change in mothers' ability to extend parental leave by child birth date, and thus women who gave birth after May 2, 2011 were as good as randomly assigned to extend the number of days on leave. This creates two groups of mothers, those exposed to the reform and those non-exposed.

I compare mothers exposed to those non-exposed to the reform to estimate the effects of an extension of maternity leave on labor market outcomes up to 7 years after

childbirth. First, I show eligible women extend their maternity leave in 79 days on average, and reduce the use and length of other paid leave claims during the first year they give birth. Second, my results show the reform increases women's employment for at least 4 years after childbirth, and wages grow especially after year 2.

To study the mechanisms I study heterogeneity by workers' demographics. The positive employment effects are driven by single women and lower educated. Especially among single workers, exposed mothers have higher employment even after year 4 relative to non-exposed women. Among ever married women, their employment is not different from the non-exposed after childbirth, but their wages grow at lower rates, and they are more likely to transition to more flexible jobs measured by being employed on a temporary contract.

My results suggest that allowing women to recover from childbirth has positive impacts on their careers especially among women who cannot afford more flexibility at home. Single women are more likely to be sole earners, as opposed to married working mothers. Hence, a longer leave increases single women's labor market attachment and wage progression in the medium term. These results go in line with [Albagli and Rau \(2019\)](#) who find exposed women relative to non-exposed have better mental health due to a longer leave, and the women who benefit the most are lower educated workers.

It is important to mention that because my results are local average treatment effects, and are estimated in a very selected group of women who were already working prior to childbirth and were eligible for maternity leave, that is, they were formally employed and contributing to social security, these should be interpreted with caution, and should not be extrapolated directly to the rest of the female working population in Chile, especially in the presence of a large informal labor market.

The previous literature that estimate RDD models suffer from the same concerns, but compared to those the results of this paper are larger and last for longer. This could be because Chile is a very different country compared to the Scandinavian countries, which have longer parental leaves, many of them above one year, and have universal childcare systems. This is not the case for most developing countries, thus, allowing

women to recover for longer and spend time with their children, knowing they can more easily return to their previous jobs, guarantees women are more attached to the labor market in the medium term, with positive consequences on formality and social security contributions in the longer run.

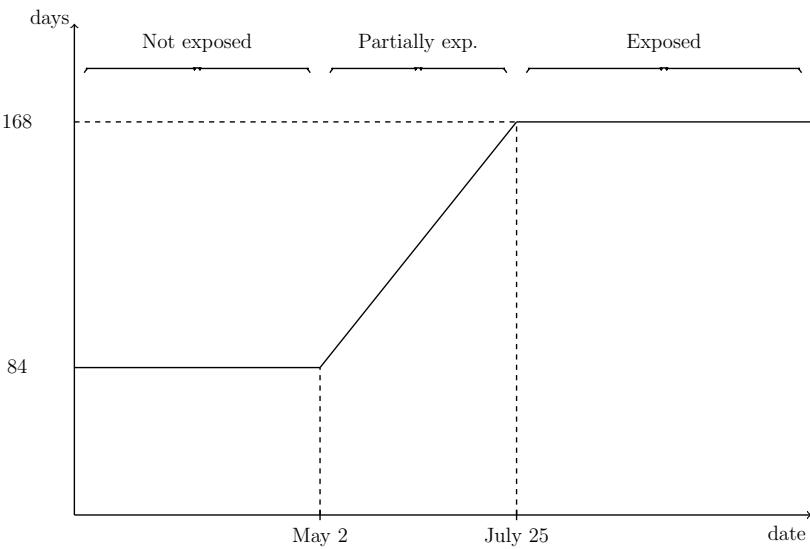
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FIGURES AND TABLES

Figure 1: Exposure function by child's date of birth



Notes: This figure replicates the exposure function of [Albagli and Rau \(2019\)](#)

Figure 2: Balance in number of births, claims and workers' pre-claim characteristics

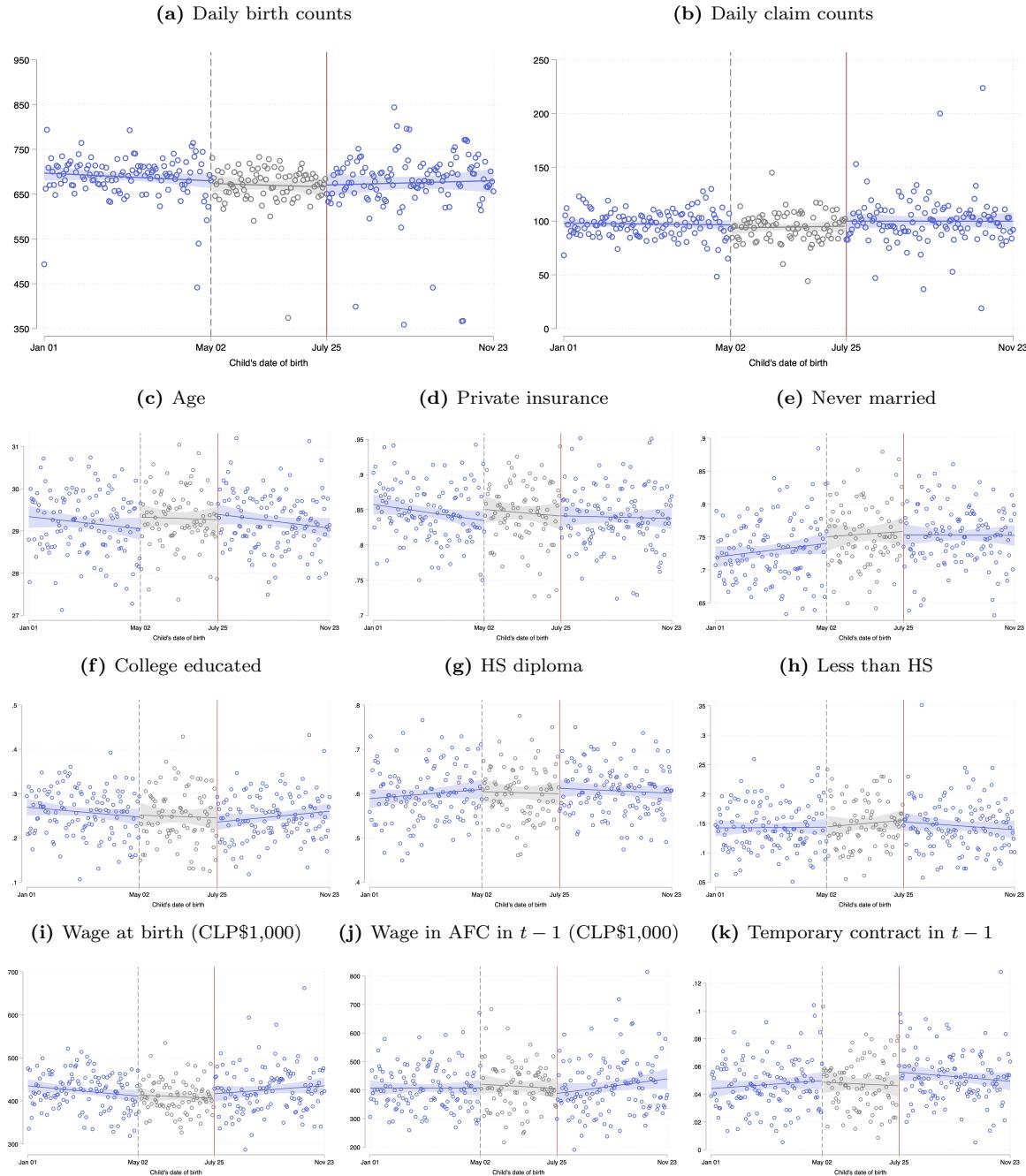
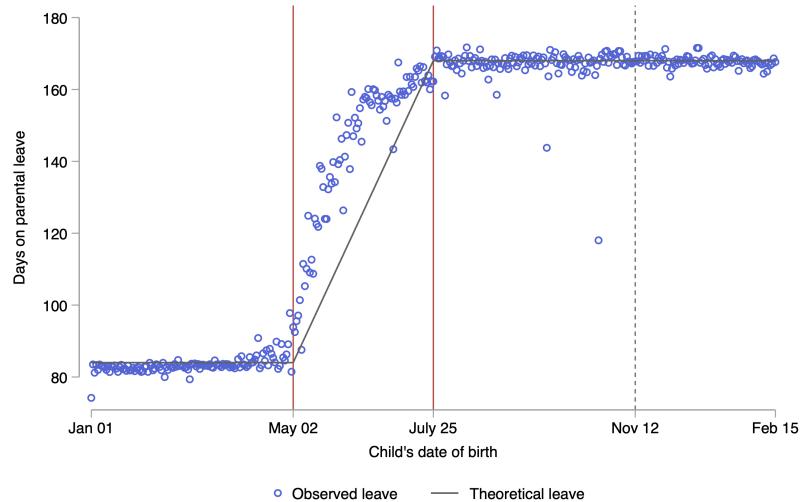


Figure 3: Number of days on parental leave

(a) Average leave by child's date of birth



(b) Distribution

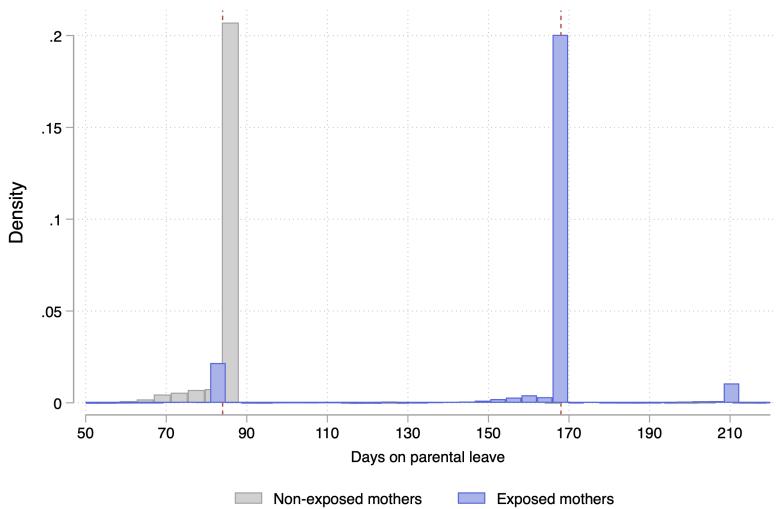
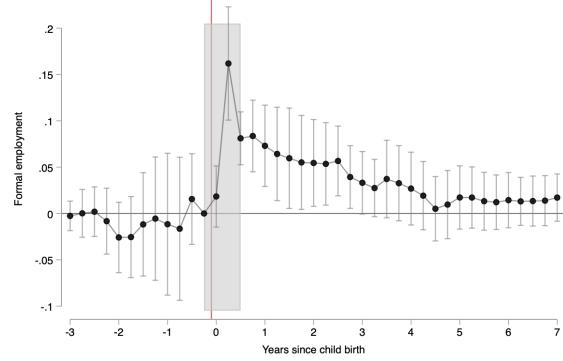
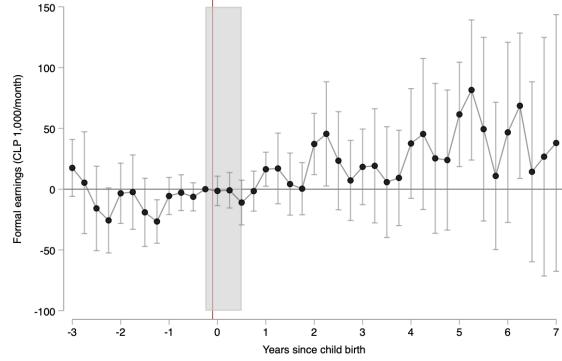


Figure 4: Labor market outcomes of mothers who file a parental leave claim

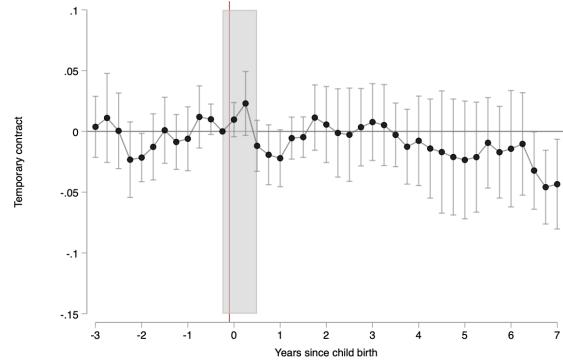
(a) Employed in formal sector



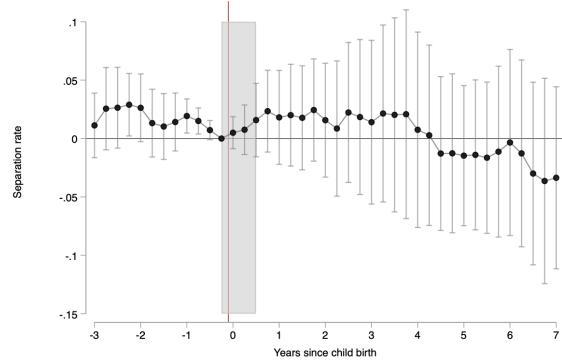
(b) Monthly earnings if employed (1,000CLP\$)



(c) Temporary contract if employed



(d) Separation rate if employed (different employer)



(e) New parental leave claim

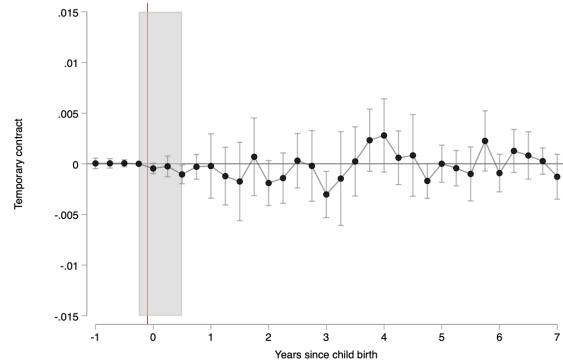


Figure 5: Exposure effect on the use of other leave claims in the first year after childbirth

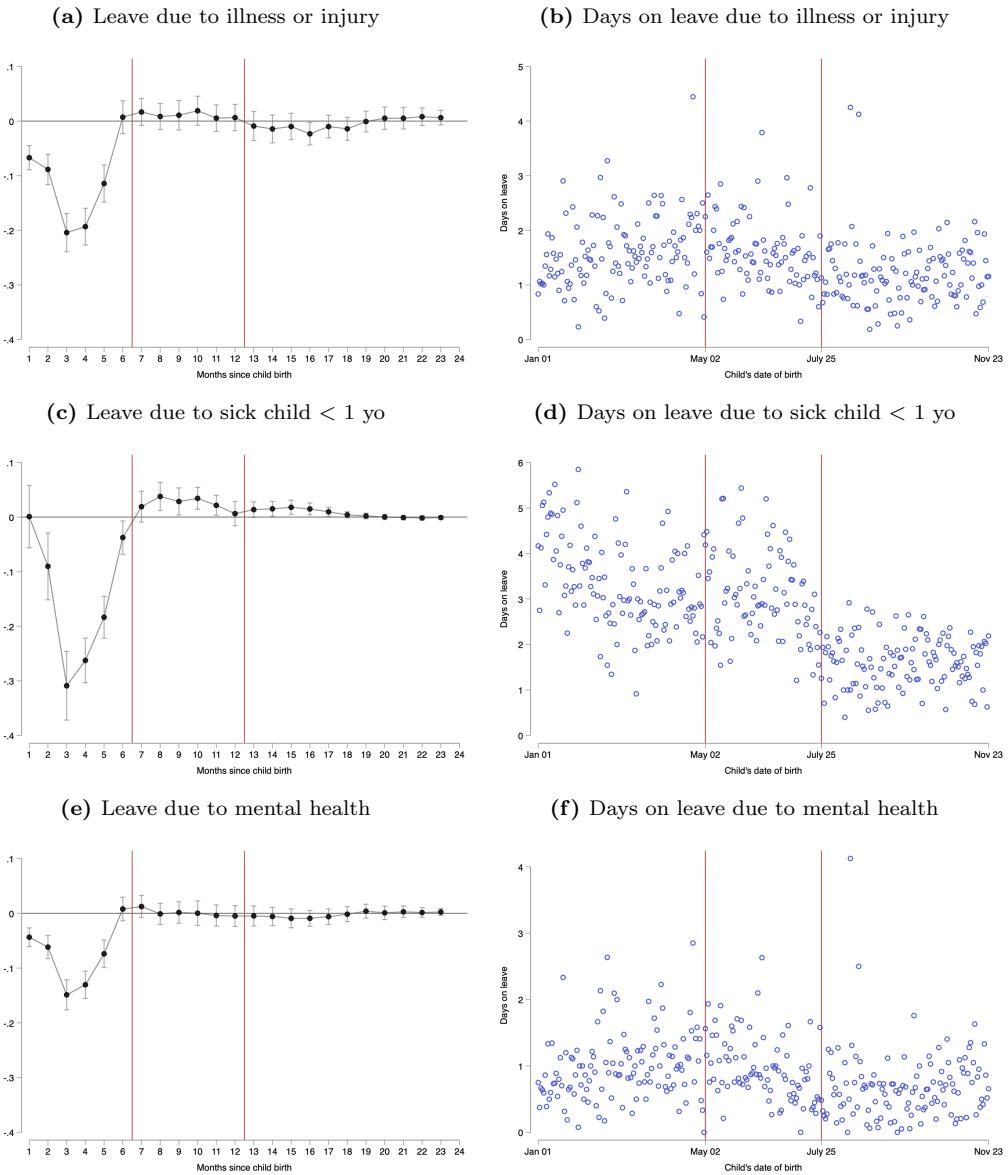
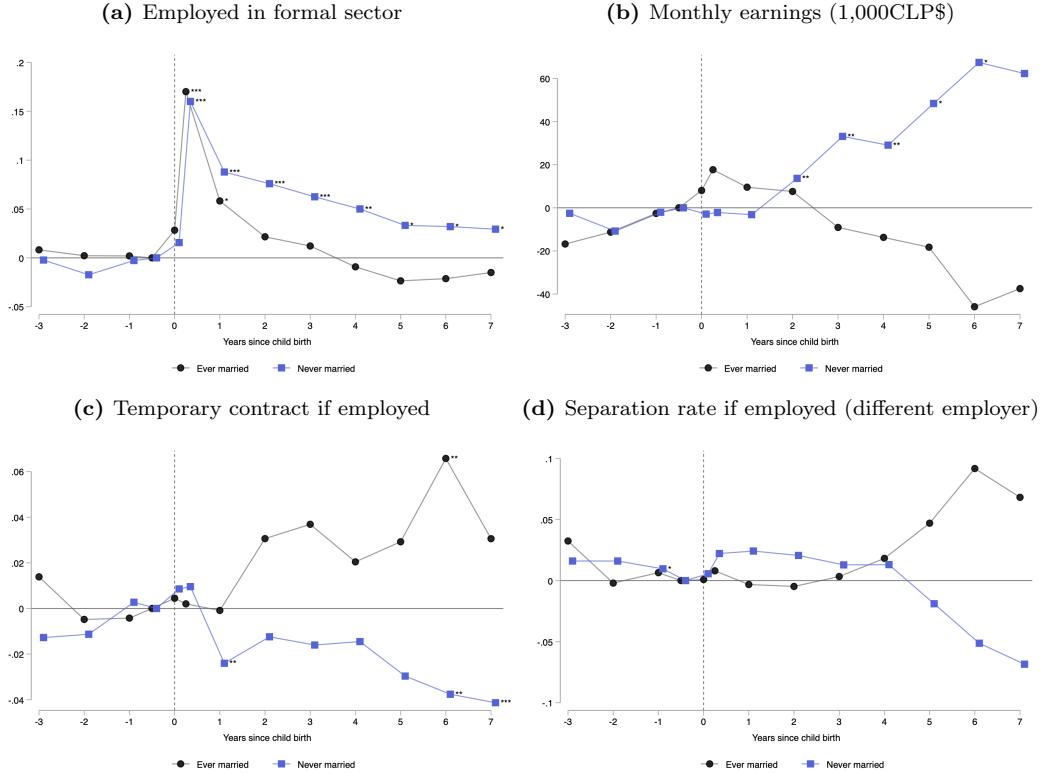


Figure 6: Labor market outcomes of mothers who file a parental leave claim by workers' characteristics

A. By marital status



B. By education level

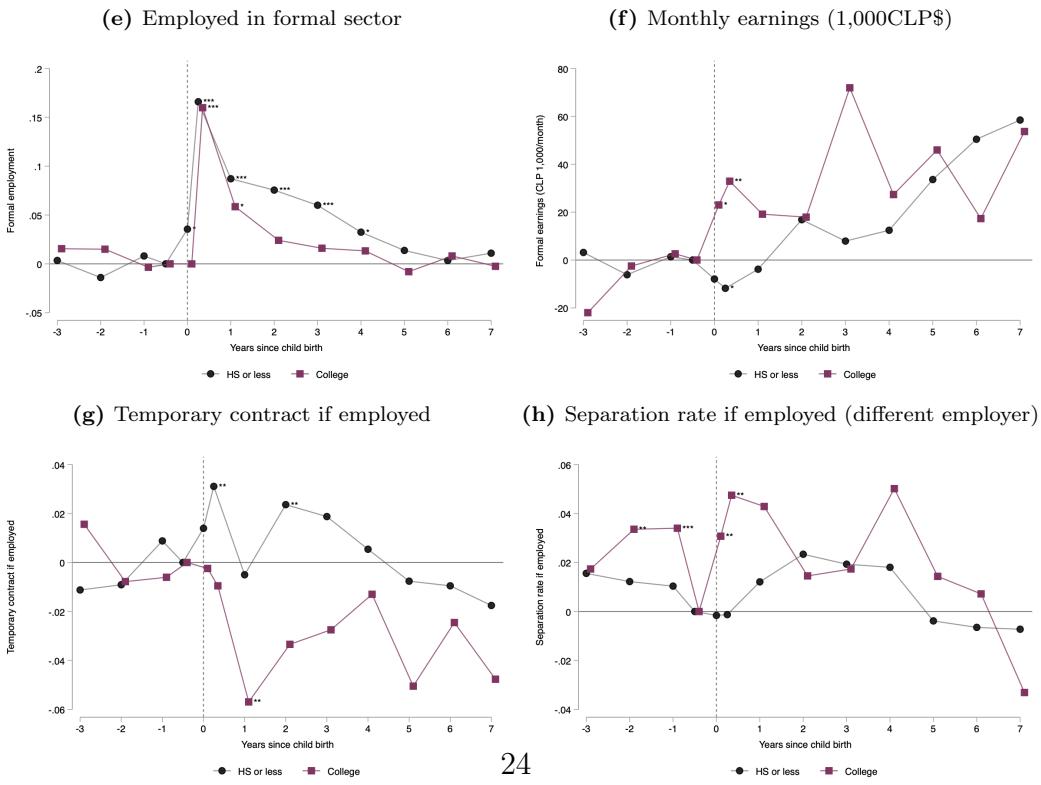


Table 1: Characteristics of workers matched to formal employment administrative data

Characteristics	All claims in 2011				Workers in AFC before leave			
	Non-exposed	Partially exposed	Fully exposed	β_{zexpo}	Non-exposed	Partially exposed	Fully exposed	β_{zexpo}
Age	29.16 (5.44)	29.31 (5.53)	29.39 (5.43)	0.21** (0.08)	29.73 (5.35)	30.06 (5.48)	29.99 (5.29)	0.22* (0.12)
Private insurance	0.85 (0.36)	0.85 (0.36)	0.84 (0.37)	-0.01 (0.01)	0.90 (0.30)	0.90 (0.30)	0.90 (0.30)	0.00 (0.01)
Wage $_{t=0}$	428.47 (337.46)	413.36 (332.04)	436.69 (343.33)	7.17 (6.95)	497.30 (358.16)	481.73 (364.73)	516.94 (373.63)	21.50 (15.46)
College	0.26 (0.44)	0.25 (0.44)	0.26 (0.44)	-0.01 (0.01)	0.29 (0.45)	0.29 (0.45)	0.29 (0.45)	0.00 (0.02)
High school diploma	0.59 (0.49)	0.60 (0.49)	0.60 (0.49)	0.01 (0.01)	0.59 (0.49)	0.57 (0.50)	0.59 (0.49)	-0.01 (0.01)
Less than HS	0.14 (0.35)	0.15 (0.36)	0.14 (0.35)	0.00 (0.00)	0.12 (0.33)	0.15 (0.35)	0.13 (0.33)	0.00 (0.01)
Education missing	0.16 (0.36)	0.16 (0.36)	0.16 (0.37)	0.00 (0.00)	0.18 (0.38)	0.18 (0.38)	0.19 (0.39)	0.01 (0.01)
Never married	0.73 (0.45)	0.75 (0.43)	0.75 (0.43)	0.02 (0.01)	0.72 (0.45)	0.71 (0.45)	0.73 (0.45)	0.00 (0.01)
# Months in AFC $_{t=-1}$	2.62 (4.5)	2.52 (4.44)	2.67 (4.51)	0.03 (0.06)	7.92 (4.77)	7.71 (4.91)	7.89 (4.76)	-0.07 (0.24)
Temp. Contract $_{t=-1}$	0.05 (0.17)	0.05 (0.17)	0.05 (0.18)	0.00 (0.00)	0.13 (0.28)	0.13 (0.27)	0.14 (0.28)	0.01 (0.01)
Firm Size $_{t=-1}$	3364.3 (3885.5)	3280.8 (3786.1)	3437.3 (3913.1)	36.99 (254.9)	3206 (3825)	3093 (3691)	3371 (3880)	113.62 (315.68)
% Female workers $_{t=-1}$	0.05 (0.17)	0.05 (0.18)	0.04 (0.16)	0.00 (0.00)	0.05 (0.16)	0.05 (0.17)	0.04 (0.16)	0.00 (0.00)
Same employer as $t = -1$	0.96 (0.19)	0.96 (0.19)	0.96 (0.20)	0.00 (0.00)	0.94 (0.22)	0.95 (0.21)	0.96 (0.17)	0.02* (0.01)
Observations	12,065	7,630	12,106	31,801	3,629	2,233	3,612	9,474

Notes: Difference correspond to a regression of each worker characteristics on z_i . Clustered standard errors by child's month of birth.
10%, 5**, 1***.

Table 2: Relationship between exposure to reform and days on parental leave

Exposed (z)	All claims in 2011		Workers in AFC before leave	
	Days on parental leave		Days on parental leave	
	(1)	(2)	(3)	(4)
Exposed (z)	80.777*** (2.164)	80.770*** (2.172)	79.095*** (3.632)	79.020*** (3.637)
Non-exposed mean	83.54	83.54	83.10	83.10
R ²	0.672	0.673	0.643	0.643
Controls t=0		yes		yes
Observations	31,801	31,801	9,474	9,474

Notes: Columns (1) and (2) include all parental leave claims filed between January 1 and November 22, 2011. Columns (3) and (4) restrict to workers in AFC in the months before the start of parental leave. Clustered standard errors by child's week of birth. 10%, 5**, 1***. Controls include: age in dummies, private insurance, and wage at time of parental leave.

Table 3: Effects of exposure to reform on women's labor market outcomes

	Employment (1)	Wage (2)	Log (wage) (3)	Separation (4)	If employed Temp. contract (5)
Exposure $t = -3$	0.001 (0.010)	-7.494 (9.201)	0.003 (0.018)	0.018** (0.008)	-0.002 (0.017)
Exposure $t = -2$	-0.012 (0.015)	-11.357 (7.910)	0.023 (0.016)	0.012 (0.008)	-0.005 (0.012)
Exposure $t = -1$	-0.001 (0.017)	-2.464 (5.091)	0.042*** (0.014)	0.011* (0.006)	0.006 (0.009)
Omitted $t = -0.5$					
Exposure $t = 0$	0.019 (0.015)	0.373 (6.312)	0.002 (0.013)	0.004 (0.006)	0.010 (0.008)
Exposure $t = 0.5$	0.163**** (0.027)	3.629 (8.327)	-0.010 (0.017)	0.006 (0.010)	0.023* (0.012)
Exposure $t = 1$	0.080*** (0.015)	0.818 (8.973)	0.011 (0.017)	0.016 (0.015)	-0.014 (0.012)
Exposure $t = 2$	0.061*** (0.016)	13.287 (13.393)	0.053** (0.022)	0.017 (0.020)	0.003 (0.014)
Exposure $t = 3$	0.049*** (0.014)	23.906 (15.968)	0.065*** (0.020)	0.011 (0.026)	0.002 (0.017)
Exposure $t = 4$	0.034** (0.015)	21.103 (22.456)	0.058* (0.030)	0.013 (0.032)	-0.003 (0.017)
Exposure $t = 5$	0.018 (0.015)	36.108 (23.062)	0.077** (0.031)	-0.009 (0.033)	-0.016 (0.019)
Exposure $t = 6$	0.017 (0.014)	45.915 (27.587)	0.068* (0.035)	-0.014 (0.034)	-0.017 (0.023)
Exposure $t = 7$	0.017 (0.013)	43.570 (33.125)	0.063 (0.038)	-0.030 (0.031)	-0.029 (0.025)
Adj. R^2	0.173	0.660	0.580	0.092	0.073
# workers	9,474	9,474	9,474	9,474	9,474
Observations	1,258,679	471,728	471,728	471,728	471,728

Notes: Regressions for workers matched to AFC with at least six wage records in the year prior to her child's date of birth. Clustered standard errors by child's month-year of birth. 10%, 5%, 1%. Controls include: average wage, type of contract, and number of months in AFC in the previous 12 months prior to child's birth. Monthly data, number of workers is 17,329.

Table 4: Effects of exposure to reform on # days on leave by type of claim within first year after childbirth

	Maternity leave (1)	Illness or injury (2)	Child < 1 yo (3)	Pregnancy related (4)	Mental health (5)
Exposure (z)	79.020*** (3.637)	-0.403*** (0.063)	-1.572*** (0.145)	0.002 (0.004)	-0.352*** (0.058)
Non-exposed mean	83.10	1.575	3.316	0.012	0.994
Adj. R^2	0.643	0.025	0.043	-0.001	0.017
Observations	9,474	9,474	9,474	9,474	9,474

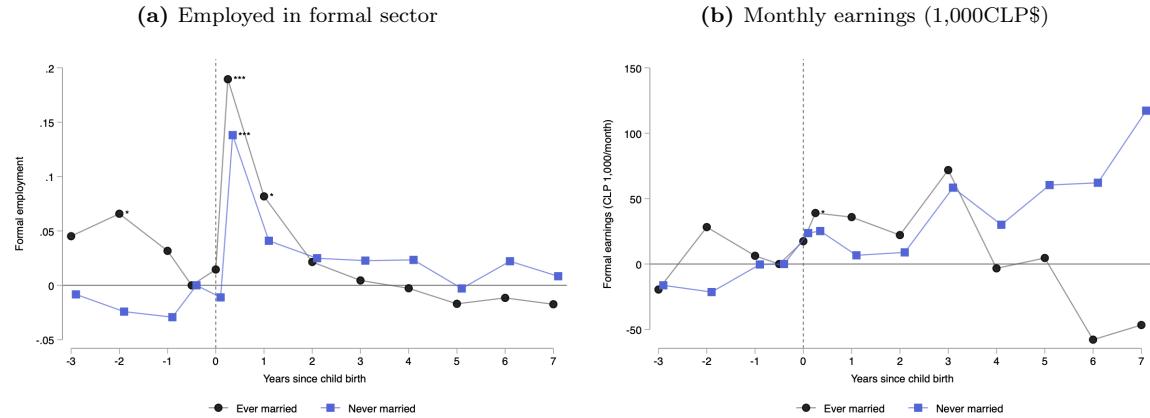
Notes: Regressions for workers matched to AFC with at least six wage records in the year prior to her child's date of birth. Clustered standard errors by child's month-year of birth. 10%*, 5%**, 1%***.

Controls include: average wage, type of contract, and number of months in AFC in the previous 12 months prior to child's birth.

ADDITIONAL FIGURES AND TABLES

Figure A.1: Labor market outcomes of mothers who file a parental leave claim by workers' characteristics (marst & educ)

A. College Educated



B. HS or less

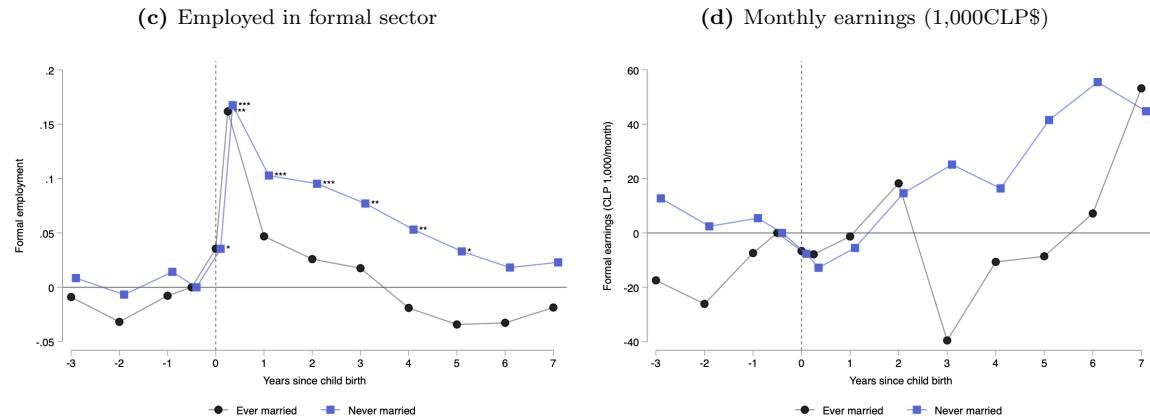


Table A.1: Effects of exposure to reform on # days on leave by type of claim within first year after childbirth

	Maternity leave (1)	Illness or injury (2)	Child < 1 yo (3)	Pregnancy related (4)	Mental health (5)
<i>Panel A. Marital Status</i>					
Exposure (z) * Never married	79.610*** (3.270)	-0.456*** (0.071)	-1.539*** (0.146)	0.003 (0.005)	-0.386*** (0.065)
Exposure (z) * Married	77.483*** (4.782)	-0.261** (0.103)	-1.655*** (0.195)	0.001 (0.007)	-0.259*** (0.089)
<i>Panel B. Education</i>					
Exposure (z) * HS less	79.724*** (2.796)	-0.515*** (0.081)	-1.812*** (0.164)	0.002 (0.006)	-0.450*** (0.069)
Exposure (z) * College	77.296*** (5.469)	-0.165 (0.115)	-1.242*** (0.216)	0.001 (0.007)	-0.136 (0.110)
Adj. R^2	0.643	0.034	0.047	-0.003	0.025
Observations	9,474	9,474	9,474	9,474	9,474

Notes: Regressions for workers matched to AFC with at least six wage records in the year prior to her child's date of birth. Clustered standard errors by child's month-year of birth. 10%, 5%, 1%***.

Controls include: average wage, type of contract, and number of months in AFC in the previous 12 months prior to child's birth.

Table A.2: Selection of workers who return to work after end of maternity leave

Covariate $t = 0$	Age (1)	Log(wage) (2)	Private Ins. (3)	Log(firm size) (4)	Never married (5)	HS or less (6)	College (7)
Returns after 6 months	-0.134 (0.167)	0.009 (0.029)	-0.022* (0.012)	0.369*** (0.136)	-0.009 (0.017)	-0.027 (0.021)	0.011 (0.017)
Exposed	0.283 (0.223)	0.070 (0.058)	0.008 (0.016)	0.259 (0.312)	-0.005 (0.022)	-0.027 (0.027)	0.008 (0.022)
Returns*exposed	-0.094 (0.254)	-0.033 (0.051)	-0.017 (0.015)	-0.279 (0.245)	0.012 (0.024)	0.019 (0.026)	-0.014 (0.022)
Adj. R^2	0.000	0.001	0.000	0.006	-0.000	0.001	0.000
Observations	9,474	9,474	9,474	9,474	9,474	9,474	9,474

Notes: