Who Benefits from a Maternity Leave Extension? Evidence from Chile*

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February 24, 2026

Abstract

This paper examines the short- and medium-term effects of extending maternity leave on women's labor market outcomes, exploiting a reform implemented in Chile in 2011 that increased maternity leave from 84 to 168 days. I combine administrative data on leave claims with employer-employee data to estimate the effect of longer leave on women's employment and wages seven years after childbirth. The results show that, compared to ineligible workers, eligible women extend their maternity leave by 79 days and reduce their use of other sick leave claims. They are more likely to be formally employed for up to three years after giving birth, with no negative effects on earnings. The positive employment effects are driven by women with low labor market attachment prior to giving birth, who experience a reduction in separation rates and an increased likelihood of working under a permanent contract in the medium term. These results suggest that a longer leave incentivizes employment by helping mothers remain in the formal labor market.

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1 Introduction

Parental leave policies are designed to insure families against income loss and enable mothers to take time off from work to recover from childbirth and care for the newborn (Rossin-Slater, 2018). Several countries in the world have adopted or extended government-funded maternity leave that grants mothers at least 54 weeks after giving birth (Dahl et al., 2016); however, this is not the case in most developing and middle-income nations, where benefits for mothers are low and leave for fathers is almost nonexistent (Banerjee et al., 2024). Female labor force participation in these settings is low, partly due to a lack of family-friendly benefits that incentivize mothers' employment (Heath et al., 2024). In these contexts, the effect of maternity leave policies on women's employment remains an understudied topic.

This paper studies the consequences of extending maternity leave on women's employment and wages, as well as on the substitution between maternity and sick leave. I exploit a 2011 reform in Chile that allowed eligible mothers to extend maternity leave from 12 to 24 weeks (84 to 168 days), creating a change in eligibility based on the child's date of birth. Specifically, the reform created three groups of women: those who gave birth before May 2 were not entitled to an extension (ineligible); those who gave birth between May 2 and July 25 were entitled to a partial extension (partially eligible); and those who gave birth after July 25 were entitled to a complete 12-week extension (eligible).

This project extends the work of Albagli and Rau (2019), who investigated the same reform using survey data, and focused their analysis on children's outcomes and the employment of mothers in the first year after giving birth. In this paper, I complement their work by estimating the effects of the reform up to seven years after childbirth using administrative data. To do so, I first implement a regression discontinuity design that compares the maternity leave duration of eligible and ineligible women around the July 25 cutoff using a sample of maternity leave claims. The validity of the research design relies on the assumption that the reform affects eligible and ineligible mothers differently only through their capacity to extend maternity leave. To support this claim, I provide evidence that both groups of workers have, on average, similar demographics at the time of birth, with no discontinuities at the time the reform was passed.

The RDD results show that women make use of their right to extend maternity leave. Eligible mothers extend maternity leave by 79 days on average and reduce both their use and length of other sick leave claims within the first year of giving birth by 2.4 days. In particular, they reduce the likelihood of claiming paid sick leave by 45.3% and the use of paid mental health leave by 78%.

Then, I combine my sample of maternity leave claims with employer-employee records between 2008 and 2018, which include data on private sector employment. Using the same RDD strategy, I find that eligible mothers are 6.8 percentage points more likely to remain employed in the formal sector between years 1 and 3 after childbirth, corresponding to a 19% increase compared to ineligible mothers. The average effect on employment fades out after year 4, and it is not statistically different from zero.

To better understand the dynamics of the employment effects, I estimate a difference-in-difference model that compares eligible and ineligible women before and after childbirth. The difference-in-difference results support the findings from the RDD estimates. Eligible mothers are more likely to be formally employed for three more years after childbirth, compared to ineligible mothers, but the effects fade out in the medium term. This is because ineligible mothers are more likely to return to work after three years of giving birth at the same rate as eligible mothers. While the employment effects are not permanent, the cumulative effect corresponds to one additional year of work experience for eligible women. The positive employment effects for three years are more than it has been documented for developed economies (Rossin-Slater, 2018), and suggest that maternity leave with a duration below one year may contribute to reducing the child penalty on employment for women in a middle-income country, like Chile (Kleven et al., 2024).

The effect on formal wages follows a similar trajectory to that of the employment effects. Eligible women have higher earnings in the first three years after giving birth, but the difference becomes small and not statistically different from zero in the medium term. Conditional on employment, there is no effect of the reform on wages, which suggests that the increase in total formal earnings is driven by higher employment of mothers who would not be employed in the absence of a longer leave. In addition, conditional on formal employment, eligible women reduce their separation rates from their pre-birth employer and are more likely to work under a permanent contract after seven years. These results indicate that eligible women increase their tenure within the firm.

To investigate who benefits the most from an extension, I study heterogeneous effects by demographics. I find no differential effects by women's marital status, age at birth, education, or pre-birth wages. However, the main differences are between women with low pre-birth tenure versus those with high tenure or high pre-birth labor market attachment. I define women with low tenure as those with less than 10 months of formal wages (which proxy for social security contributions) in the year before they start maternity leave. The results indicate that women with low tenure benefit the most from an extension, as they increase formal employment the most. For this group, wages increase the most in the short run, with small negative effects in the medium run (after year 4) that are not statistically different from zero.[1] For high-attachment women, the reform increases their employment but at lower rates compared to low-attachment workers, but interestingly, they reduce their use of other sick leave claims the most. The total reduction in days of leave due to their own sickness or their child's sickness is four days.

These findings align with the motivations cited by Chilean policymakers for passing the reform: the low labor market attachment of working mothers and the lack of childcare alternatives for newborns. I next explore whether this lack of access to childcare in the municipality of residence reinforces the previous results. I find suggestive evidence that the employment effects of the reform are larger among women living in municipalities with a low supply of childcare services and who have low pre-birth attachment to the formal labor market.

This paper contributes to several strands of the literature. First, it contributes to the literature examining parental leave policies on women's labor market outcomes (Rossin-Slater, 2018; Gruber, 1994; Ruhm, 1998; Schönberg and Ludsteck, 2014; Thomas,

[1] In previous research on more advanced economies, programs that incentivize women's employment have found the opposite result: women with high tenure benefit the most (Kuka and Shenhav, 2024).

2021; Bergemann and Riphahn, 2023).[2] A set of papers that use similar identification strategies includes Bailey et al. (2025), for women in California; Dahl et al. (2016), for Norway; and Stearns (2019), for Great Britain. In contrast to these papers, my results show that an extension to maternity leave that is shorter than one year positively impacts women's attachment to the labor market, especially among those with low pre-birth tenure.

This paper also contributes to the literature studying the determinants of female labor force participation in developing countries (Heath et al., 2024), with a small set of papers studying parental leave, finding mixed results (Vu and Glewwe (2022); Liu et al. (2024); Machado et al. (2024); Ghosh et al. (2025)). This paper complements the work of Albagli and Rau (2019) and Duarte et al. (2024), who studied the same maternity leave reform, with a focus on children's outcomes. My results show that the positive effects that Albagli and Rau (2019) find in the first year extend up to three years after childbirth.

Finally, this paper relates to the literature on social protection in low- and middle-income countries (Banerjee et al., 2024). While most studies have focused on childcare policies, only a few examine parental leave. This is partly due to limited access to maternity leave, a lack of administrative data that follow workers over time, and the absence of parental leave for fathers. Studying these policies is especially important in contexts with large informal labor markets, where gender gaps and child penalties tend to be greater (Berniell et al., 2021; Galván et al., 2023).

The rest of the paper is organized as follows. Section 2 describes the 2011 reform and a conceptual framework. Section 3 describes the data. Section 4 presents results on take-up, and Section 5 presents results on labor market outcomes. Section 6 studies mechanisms, Section 7 discusses the policy implications of the reform, and finally Section 8 concludes.

[2] Two recent strands of the literature in developed countries study the consequences of parental leave policies granted to fathers and their effects on mothers' employment (Farré and González, 2019; Bartel et al., 2018) and the effects on firms and coworkers (Ginja et al., 2023; Gallen, 2019).

2 Maternity Leave Reform in Chile

2.1 Reform of 2011

In Chile, maternity leave was enacted in 1924, and the legislation did not face significant changes since 1980 (Romanik Foncea, 2014). Discussions to pass a reform extending maternity leave began in Congress in February 2011, after several failed proposals in previous years. The main motivations cited at the time were related to women's low labor market attachment, particularly low-income women, the lack of quality childcare alternatives, and the fact that women on maternity leave were artificially extending their leave by filing other sick leave claims, particularly sick-child leave for caregivers of children under one year old and mental health sick leave.

Before 2011, working mothers were entitled to 6 weeks of paid maternity leave before childbirth and 12 weeks (84 days) after giving birth, with full income replacement up to the 90th percentile of women's earnings distribution. This subsidy was paid through social security contributions. Additionally, job protection continued for one year after the completion of maternity leave. All working mothers with at least 3 months of social security contributions prior to the 6 weeks before the due date were entitled to maternity leave, regardless of occupation, industry, or contract type (temporary or permanent). Fathers did not have the benefit of parental leave.

On October 17, 2011, a reform extended maternity leave after giving birth from 12 to 24 weeks. It allowed mothers to choose between the following options: 1) extend their leave from 12 to 24 weeks or 2) not extend their leave but 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 either of these options, leave before childbirth remained as 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 giving birth.

Although the discussions were widely covered in the news, the reform passed relatively quickly and was approved within eight months. Therefore, it is unlikely that working mothers anticipated the exact date of the reform and strategically delayed childbearing. Given the reform's short implementation period, the timing effectively randomized workers who were already on maternity leave or were soon to be. Thus, the extension's timing around July 25, 2011 provides a clean identification of the reform's effect on women's labor outcomes.

As a result of the legislation change, working mothers could extend their leave based on their newborn's date of birth. Albagli and Rau (2019) show that the reform created three groups of women who faced different eligibility options depending on their child's date of birth. Mothers of children born on or after July 25, 2011 were *eligible* for a full 84-day extension and thus to a total of 168 days of maternity leave. Mothers of children born before May 2, 2011 were *ineligible* for an extension and could only take 84 days of maternity leave in total. Women who gave birth between May 2 and July 24 of 2011 were *partially eligible* for an extension, as they were already on leave when the reform was passed and were entitled to extend their maternity leave retroactively. They could increase their leave by the number of days between the standard 84-day leave and the total number of days they had already taken. Figure 1 replicates Albagli and Rau (2019)'s eligibility function for the number of days of leave as a function of a child's date of birth.

2.2 Expected effects of maternity leave extension on women's outcomes

The effects of maternity leave extension on women's labor outcomes may be ambiguous, as they depend on whether the policy encourages women to return to work or instead increases the time they spend away from it (Olivetti and Petrongolo, 2017). As Rossin-Slater (2018) notes, for developed contexts, usually leaves that are longer than one year may be harmful on women's labor force participation, but extensions to maternity leave have not been found to impact women's employment negatively, and if positive, the effects fade out after one year.

In the context of this paper, the Chilean reform extended maternity leave from 12 to 24 weeks. Given the previous discussion, one may hypothesize that the effects of the extension on women's participation may be positive but small. However, this does not consider other aspects of social protection in middle-income countries that differ from contexts like Norway, the UK, or Denmark, for which most of the evidence is available.

The first aspect to consider is access to childcare. In many developing and middle-income countries, childcare is not universal, quality varies considerably, and it is not guaranteed after women return from parental leave. Mothers have to rely on informal networks or family members. An extension to maternity leave may be socially desirable because it allows women to replace the lack of childcare for three more months.

The second aspect is worker retention. As women spend more time at home to take care for the new born, a longer leave helps women preparing better to return to work, and to be more attached to their pre-birth employer. A question here is why employers do not provide extended leave to retain their female workers, regardless of the law mandate. One possibility could be that higher retention may also imply that workers may realize the benefits of higher attachment with a different employer, and thus, firms have lower incentives to provide this amenity, making the universal policy socially desirable. Another possibility is that, in some firms, a longer leave may increase their hiring costs as a consequence of replacing a worker for longer months, which may lower the incentives to hire female workers (Ginja et al., 2023).[3] Hence, the total effect may be ambiguous.

Finally, a third aspect is labor informality. In Chile, between 2017 and 2024, 27.4% of total employment is informal, and women are overrepresented in this sector with a share of 28.8% of female employment being informal.[4] The government may have a special interest in extending maternity leave if this incentivizes formal employment, because of higher worker retention, and tax and pension contributions that have positive impacts in the longer run. On the one hand, this is relevant because formal contributions are needed to be eligible for the maternity leave subsidy. On the other hand, the effects may be ambiguous if an extended leave incentivizes a more flexible schedule after birth with women moving out of formal employment. As Berniell et al. (2021) show,

[3]Anecdotal evidence supports the idea of worker retention in large firms. Some firms pay their high-earning workers on maternity leave the difference between the subsidy and their pre-birth wage, as the subsidy is capped by law. While I do not have evidence of this being systematic, it provides suggestive evidence of worker retention practices. I learned about this from conversations with managers and supervisors in a medium-sized firm in Chile, but unfortunately, there is no systematic data on these practices at the firm level for the whole country that I could incorporate into my analysis.

[4]Own calculations from Chile's National Employment Survey (ENE).

after the birth of a first child, women as opposed to men, are more likely to transition into informal work, hence the total effect will depend on whether the worker retention channel dominates.

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, known as the AFC (Administradora de Fondos de Cesantía). The AFC is a monthly employer-employee dataset that tracks all workers and their employers between 2008 and 2018. It provides data on workers' employment histories in the formal sector. For employed individuals, the AFC reports monthly wages, years of education, gender, industry, municipality of employment, and firm-to-firm transitions. It also includes firm-level characteristics, such as firm size (number of employees), and municipality.

The second dataset is SIMSIL, an information system used by the government to manage workers' leave payments starting in 2011. This dataset belongs to Chile's Superintendence of Social Security, which administers and supervises leave payments to workers. In this dataset, I observe parental leave claims filed by working parents. For each parental claim, I observe the child's birth date, the start and end dates, the amount of the subsidy, the worker's age at the start of leave, the type of health insurance (private versus public), and municipality of residence. I also observe if a worker has ever filed other sick leave claims or sick-child leave claims. I match all workers in the AFC dataset to their medical and parental leave claims from January 2011 to December 2018.

Combining these two datasets, I construct a sample of working women who filed a maternity leave claim in 2011 and their employment histories in the formal sector through the end of 2018. The advantage of using administrative data is that I can observe women who filed a maternity leave claim along with their taxable wages in the private sector. The public sector in Chile employs about 10% of formal workers.

Importantly, women in the public sector have the same maternity leave benefits as those in the private sector; however, workers in the private and public sectors may differ in other respects. Although I do not observe data for public workers, in Appendix Table A.1, I compare the women in my sample (private sector only) with a representative sample of women who filed maternity leave in Chile (private, public, and independent workers). In general, the average woman in my sample looks very similar to the average woman on maternity leave, but their wages are 4% higher, and they are significantly more likely to have private insurance. This speaks to the selection of women who work in the private sector in Chile.

Despite the advantages of the employer-employee data, the AFC records only include formally employed workers. One disadvantage is that when a worker disappears from the sample, it is unclear whether they became unemployed, moved to the public sector, transitioned to the informal sector, or left the labor force. While this omission may not threaten the validity of my estimates around the time of the reform, it changes their interpretation if the reform changed the composition of employment for eligible mothers. For example, if eligible women increase their formal employment, it might be at the expense of reducing unemployment or informal employment; hence, a positive effect on formal employment is an upper bound on the total employment effect. While I cannot distinguish between unemployment and informality in the AFC data, in Section 6, I provide evidence that the observed wage effects are more likely driven by improvements in the quality of jobs that eligible women hold in the medium term.

3.2 Sample selection and summary statistics

I restrict the estimation sample to mothers aged 18–49 who filed a maternity leave claim between January 1, 2011 and November 23, 2011, and who have wage data in the AFC during the three months preceding the start of their leave. This ensures that I can match each maternity leave claim to the worker's employer at the time the leave begins.[5] I also drop mothers with parental leave claims beyond 365 days, as these

[5] I do not observe claims filed by men. Before the reform, only widowed fathers were entitled to parental leave, and with the 2011 reform, only five fathers in the whole country chose to take five days of parental leave in the reform's first year. Additionally, I drop adoptive mothers and other caregivers entitled to parental leave, such as grandparents, from the sample.

are likely administrative mistakes.[6] Finally, I select women who gave birth between January 1 and November 23, as this guarantees the largest symmetric window around the partially eligible group of women.[7] This leaves me with a sample of 10,500 working mothers.

Table 1 describes summary statistics for women in the sample. Column (1) shows that the mean age is 29.8 years, 89% of them have private insurance, and their average wage is CLP$484,000, which is equivalent to US$615/month in 2018. Importantly, as previously discussed, the women in the sample are not representative of the average working woman in Chile, 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 shows that women who can access maternity leave in Chile are positively selected on their wages. This positive selection is also reflected in their education: 41% are college educated, and 48% are high school graduates. Only 27.4% have ever married, and on average, they are observed for 7.5 months in the AFC before the start of their maternity leave. Most women in the sample hold a permanent contract at baseline, as opposed to a temporary one (13.4%). They also work in large firms with more than 3,000 workers and a high average share of female workers (59.5%).

Columns (2)–(4) show summary statistics by eligibility group, and the coefficients in column (5) report the results from regressing each baseline characteristic on the eligibility function in Figure 1, normalized between 0 and 1 ((eligible days – 84)/84). The unconditional coefficients in column (5) show small differences between eligible and ineligible women. Eligible women are older by 0.275 years and have slightly higher wages at the start of their leave; however, these differences are small. In fact, the age difference has been found in previous settings. Carneiro et al. (2015) and Bailey et al. (2025) show that in Norway and California, respectively, there is cyclicality in the age at which women give birth throughout a calendar year, as women who give birth in late trimesters are older than those who give birth in earlier trimesters. In Figure A.2,

[6] For reference, the 99th percentile in the distribution of maternity leave claims is 210 days; hence, I am dropping less than 1% of claims in my sample.

[7] I observe data on leave claims for November and December of 2010, but I do not include women who filed a claim before 2011. The data for 2010 are not reliable because the Social Security Administration was implementing a new system to report parental leave claims in preparation for a future maternity leave reform.

I plot the age at birth for women from 2010 to 2014, and the same pattern appears in the Chilean data.

Finally, column (6) repeats the exercise of column (5) but excludes partially eligible women, and the results are the same: women who are eligible for a full extension are nearly identical to those who are ineligible in their demographics and labor market outcomes before the start of maternity leave.

4 Effects of the reform on the number of days of leave

4.1 Number of days on maternity leave

This section estimates the reform's effect on the length of maternity leave and the use of other sick leave. To do this, I first follow Albagli and Rau (2019)'s strategy, which is estimating the difference in outcomes between eligible mothers and ineligible mothers, using the following specification:

$$y_i = \alpha + \beta \cdot eligible_i + u_i, \tag{1}$$

where y_i is the number of days on leave for mother i who claimed maternity leave, and $eligible_i$ is a transformation of the function in Figure 1 and equal to $(\frac{eligible\ days - 84}{84})$. This variable varies between 0 and 1 and indicates the proportion of days allowed for an extension as a function of a child's date of birth. Hence, under the assumption that eligibility is quasi-randomly assigned to mothers, β estimates the local effect of the reform as the difference in days on leave between women who can fully extend maternity leave and women who cannot. Errors are clustered at the level of week of birth.

I start the analysis by showing that eligible women who file a maternity leave claim extend the duration of their maternity leave. Panel (a) of Figure 2 plots the average number of days of leave by child's week of birth. On average, women take the number of leave days to which they are entitled, both before and after the reform, with an increase observed among women eligible for the extension. The pattern of this increase closely follows the eligibility function shown in Figure 1, though partially eligible women lie above the 45-degree line on average. This may reflect the transitional process experienced by partially eligible women who received the extension retroactively.

Panel (b) of Figure 2 presents the distribution of days of leave for eligible and ineligible women. Most ineligible women are on leave for 84 days. The distribution of the number of days of leave for eligible women (including partially eligible women) is shifted to the right, with the majority of women taking 168 days of maternity leave and a small share for 84 days. Note that around 1% of women are on maternity leave for 210 days, which is allowed when a child is born premature (before 33 weeks) and/or with very low birth weight (under 1,500 grams).

Table 2 presents the regression results for equation 1. Column (1) is equivalent to panel (a) of Figure 2 and shows that, on average, eligible women extend maternity leave by 79.3 days. Because there were some small imbalances in covariates between eligible and ineligible mothers, in column (2) I include controls for age, type of insurance, and wage before leave. Importantly, the inclusion of these covariates does not change the result, as eligible women extend their leave in 79.25 days.

Figure 2 showed that women who were partially eligible to extend were on leave for longer than they were supposed to. Hence, in columns (3) and (4), I repeat the previous exercise but exclude from the sample women who were partially eligible. The effect is stronger and equal to an increase of 81.7 days of leave for eligible women relative to ineligible women.

4.2 Donut Regression Discontinuity

The previous evidence showed eligible women use their extension of maternity leave. Because take-up follows the eligibility function but not exactly, a more flexible approach to estimate the effect of the reform would allow for different slopes before and after the cutoff in the form of a regression discontinuity. Given that in this setting there is a group of partially eligible women, one could omit the partially eligible observations in the form of a "donut" between May 2 and July 25, and estimate a donut RDD coefficient at the cutoff of July 25 (Noack and Christoph, 2026). To do so, I estimate the following specification:

$$y_{ia} = \alpha + \lambda \widetilde{eligible}_i + \gamma(date_a - \text{July 25}) + \delta \widetilde{eligible}_i(date_a - \text{July 25}) + \varepsilon_{ia}, \qquad (2)$$

where y is the number of days of maternity leave for worker i who gave birth on date a, and $\widetilde{eligible}$ is a dummy that indicates a worker is eligible to extend maternity leave if she gave birth after July 25. The coefficient of interest is λ, and in the context of a donut RDD, it is estimated as the difference between the predicted mean to the right of the cutoff (blue line in Figure 2 (a)), and the predicted mean to the left of the cutoff, that is an extrapolation of the data before May 2 (dotted green line in Figure 2 (a)).

For a donut RDD design to be valid, the same tests as with a regular RDD are required. First, there are no jumps at the cutoff on covariates. I provide evidence of balanced covariates in column (7) of Table 3, where I report coefficients λ from regression 2 on covariates. The results show that this strategy removes the imbalances in age and wage before parental leave, and none of the coefficients is statistically different from zero.

Second, there is no manipulation of the running variable (McCrary, 2008). To do this, I show two exercises. First, I do not find evidence of a discontinuity in the number of births, nor the number of maternity leave claims around July 25, 2011. The results are in Appendix Figure A.2. Second, I run a McCrary test in the sample of observations without the partially eligible women. Appendix Figure A.3 shows no evidence of a mass of claims around the date of the reform, and Appendix Table A.2 reports p-values of the McCrary test for different polynomial levels before and after the cutoff, providing evidence of no manipulation around July 25th, 2011.

Given the previous results, I estimate the effect of the reform on days on leave using equation 2. The estimate for λ can be found in column (5) of Table 2. The donut RDD estimate is 78.9 additional days of leave, which is very similar to the estimates in the previous columns. This result confirms that eligible women are on average on maternity leave for 79 more days compared to ineligible women.

Importantly, the previous results are RDD estimates for the largest symmetric sample I can observe, given my data constraints. In Appendix Table A.3, I estimate the

RDD coefficient using the optimal bandwidth algorithm suggested by Calonico et al. (2014), and my results are almost unchanged. Hence, I proceed with the largest sample in the following analyses.

4.3 Use of other sick leave claims

As described in Section 2, one of the main motivations for legislators to pass the reform was that working mothers on maternity leave were artificially extending their leave by filing other sick leave claims, especially mental health leave claims and sick-child leave for children below the age of one. Using the SIMSIL data, I link workers on maternity leave to all other sick leave claims between 2011 and 2018. I estimate the change in the number of days of leave for four different types of claims filed between the end of maternity leave and the child's first birthday. Importantly, while on maternity leave, workers are not allowed to file other leave claims. I study the effects of the reform on four types of sick leave claims: accident or illness, sick-child leave, pregnancy-related sickness, and mental health. Figure 3 presents the results.

The results, estimated using the same donut RDD specification as column (5) of Table 2, indicate that eligible mothers reduce their use of other claims, with the number of days of leave falling by 1.115 days for illness (panel (a)), 0.373 days for sick-child leave (panel (b)), and 0.902 days for mental health leave (panel (d)). The total effect on the number of days of other sick leave claims is the sum of the coefficients, which equals –2.4 days. While the effects may appear small, they reflect reductions along both the intensive and extensive margins. Appendix Table A.4 shows that these results are driven by a reduction in the proportion of women who use these types of leave after finishing maternity leave, as eligible women are 45.3% less likely to claim a general sick day, and 78% less likely to claim a mental health leave.

Finally, Appendix Table A.4, column (5) shows that eligible women are not more likely to file a new maternity leave claim in the seven years following the current birth. Because women must be formally employed to file a leave claim, this null effect could reflect either null fertility effects or null formal employment. However, as I show in the next section, eligible women are more likely to be formally employed in the medium term. Therefore, the zero effect on the likelihood of filing a new maternity leave claim is more likely to be associated with no differences in future fertility between eligible and ineligible women. This result is in line with the small effect on fertility found by Farré and González (2019).

The reduction in sick leave use is consistent with the findings of Albagli and Rau (2019) and Duarte et al. (2024), who show that eligible mothers are more likely to report lower levels of stress when they return to work after one year, and their babies are healthier between months 6 and 12. This could result from spending more time with their baby, increased breastfeeding, and greater time to prepare to return to work. Additionally, because sick leave claims must be approved by the Social Security Administration, the observed effects on the use of mental health leave claims can be interpreted as a lower bound of the reform's true effect on women's mental health.

5 Effects of the reform on labor market outcomes

5.1 Employment effects

Motivated by the previous evidence and the discontinuous change in mothers' ability to extend maternity leave based on their child's birth date, I estimate the effects of the reform on women's formal employment.

I define formal employment as equivalent to workers being observed in the AFC. RD designs are very demanding in sample size; therefore, due to my sample being small and the low participation of women in the AFC, I proceed by pooling the employment outcomes. I define two outcomes: short-term employment as the average employment rate in the AFC between years 1 and 3 after childbirth, and medium-term employment as the average employment rate between years 4 and 7 after childbirth.

The results of estimating equation 2 for formal employment are in Figure 4. Panel (a) shows that eligible women are 6.8 percentage points more likely to be employed in the formal sector in the first three years after childbirth, which is equivalent to an increase of 19% compared with non-eligible women at the cutoff. However, the effect of the reform fades out in the medium-term, as it is shown in panel (b), where no employment effect is detected after four years. Because these results are estimated using average outcomes, I cannot explore the dynamics of employment between treatment and control groups; hence, I turn to a difference-in-difference estimation in the next section.

5.2 Difference-in-difference estimator

The previous analysis showed that eligible women have higher levels of formal employment compared to non-eligible women. These effects were present in the short run, up to three years after giving birth. A feature of a regression discontinuity design is that it requires large sample sizes to estimate local effects. In the context of this paper, using an RD design with a small sample of workers matched to the AFC, makes investigating dynamic effects and heterogeneity less feasible and noisier. However, I can take advantage of the panel structure of the AFC and estimate a "donut" difference-in-difference regression on women's labor market outcomes, where I compare eligible and ineligible mothers three years before and seven years after giving birth using the following equation:

$$y_{it} = \alpha + \delta \widetilde{eligible}_i + \sum_{\tau \geq -12, \tau \neq -4}^{28} \beta_\tau \widetilde{eligible}_i * 1[t = \tau] + \sum_{\tau \geq -12, \tau \neq -4}^{28} \gamma_\tau 1[t = \tau] + X_i' \theta + \psi_t + \varepsilon_{it}, \tag{3}$$

where y_{it} is a labor market outcome for worker i in calendar quarter-year t, and τ indexes the quarter relative to childbirth ($\tau \in [-4, 28]$). The variable $\widetilde{eligible}_i$ is a dummy that equals 1 if a woman gave birth after July 25 and 0 if she gave birth before May 2 (no partially treated observations are included). X_i is a set of controls one quarter before the start of maternity leave that includes age fixed effects, a private insurance dummy, and wage. ψ_t are quarter-by-year fixed effects that control for temporal differences in outcomes common to all workers, and ε is the error term. Standard errors are clustered by child's week of birth.

The coefficients of interest are β_τ, which estimate the difference in outcomes for women who can fully extend maternity leave versus those who cannot. The omitted category is the fourth quarter before childbirth, $\tau = -4$, and category 0 refers to the quarter when women give birth and start maternity leave. Because the data are on a monthly basis, the coefficients β_τ should be interpreted as the average monthly effect in quarter-year τ.

As in the previous subsection, I begin the analysis by estimating equation 3 on the likelihood of working in the formal sector. Panel (a) of Figure 5 plots the regression-adjusted employment series (residuals after controlling for covariates and time fixed effects) for eligible and ineligible women around childbirth, showing an increase in the probability of employment as women approach childbirth. This is consistent with the eligibility criteria for accessing maternity leave in Chile, as women must have at least three months of social security contributions in the six months prior to childbirth. The trends in employment probabilities for the treated and control groups are very similar before childbirth, which supports the hypothesis of no pre-trends between groups. After giving birth, the probability of formal employment drops for both groups—consistent with findings from the child penalty literature (Kleven et al., 2024)—but eligible women experience a smaller reduction over the next three years.

Panel (b) plots the corresponding β_τ coefficients. Importantly, the coefficients before childbirth are close to zero. The coefficient β_1 represents the increase in the likelihood of employment during the maternity leave extension period (months 4 to 6 after childbirth). This effect is mechanical: if a worker is on maternity leave, she will appear in the data as employed, with her wages paid through a social security subsidy received by the firm from the Social Security Administration. While mechanical, this result indicates that eligible women take longer leave than ineligible women. Specifically, the coefficient indicates that eligible women are 12 percentage points more likely to be employed per month—though on leave—during months 4 to 6 after giving birth. After six months, when both groups have returned from maternity leave, the difference-in-difference coefficients indicate that eligible women are more likely to be formally employed for the next three years. This effect fades out after year 3, when ineligible women catch up and return to formal employment at the same rate as eligible women. This is consistent with the donut RDD estimates from Figure 4.

Column (1) of Table 3 presents pooled difference-in-difference estimates on formal employment in the period after childbirth. The coefficient labeled as "Eligible$_{\text{2nd trimester}}$" should be interpreted as the estimate of coefficient β in the second trimester after birth, and "Eligible$_{\text{Years 2-3}}$" as the pooled estimate between years 2 and 3, $\beta_{\text{Years 2-3}}$, after childbirth. The results show that eligible women's post-birth employment increases by 8.2 percentage points in the very short run, corresponding to the first six months after the end of their maternity leave, representing a 15.2% increase relative to the sample mean in the same period. Over the next two years after giving birth, eligible mothers have a 5.8 percentage point higher employment rate per month, equivalent to a 16.1% increase. This employment difference fades out to zero over the next four years.

5.3 Work experience and earnings

Even though the employment effects are not permanent, they are large enough to generate positive implications for years of work experience. To estimate these effects, I follow Kuka and Shenhav (2024)'s methodology, which involves calculating the cumulative sum of the annual effects on employment after the first six months following childbirth and dividing by the number of years in each period. The results in column (2) of Table 3 show that the gains in experience total 0.296 years by the third year after childbirth and 0.945 additional years by year 7 for eligible mothers.

These gains in experience are reflected in higher formal wages. Figure 6 plots the difference-in-difference estimates for the average monthly wages in $CLP 1,000. Panel (a) shows the reform's effects on total formal earnings, which follows a pattern similar to that observed for employment. These estimates include zeros for non-employed workers, reflecting zero formal employment if a worker is not in the AFC. Importantly, the decrease in earnings in the quarter of birth likely reflects that women are receiving a lower subsidized wage while on leave, because the subsidy is calculated using past wages before maternity leave. However, the wages of treated women increase after they finish their extended leave. Column (3) of Table 3 shows that on average, eligible women earn $CLP 48,847 more in the second half of the first year after giving birth, and $CLP 35,879 more per month over the following two years, compared to ineligible mothers. As with employment, these effects fade out after the fourth year. However, because these patterns reflect both employment and wage changes, I isolate the earnings effect by restricting the sample to women who remain employed. Panel (b) of Figure 6 and column (4) of Table 3 present these results. The estimates indicate that eligible women experience small but not statistically significant increases in monthly formal earnings conditional on employment, relative to ineligible workers. This means the increase in formal earnings in panel (a) in the medium-term is mainly due to the increased employment of eligible mothers, and no penalty on earnings in the medium term due to a longer leave. Column (5) of Table 3, winsorizes extreme values of earnings at the 1% and 99% tails to make sure the results are not driven by outliers, and the conclusion on earnings remains. Finally, due to the large number of zeros in my sample, I follow Chen and Roth (2023), and estimate the effects of the reform on employment and earnings using a Poisson Pseudo-MLE model, and the results remain unchanged (see Appendix Table A.5).

5.4 *Robustness checks*

In this section, I provide evidence that the previous results are robust to changes in the main sample, the identification strategy, and the inclusion of other controls.

Smaller bandwidth. Figure A.5, panel (a) plots difference-in-difference estimates of the likelihood of formal employment for women in the full sample using a bandwidth of 121 days around partially eligible women and two smaller symmetric windows of 84 and 60 days. The results are nearly identical to those for the baseline sample, with higher take-up rates in the second trimester after giving birth as the bandwidth decreases.

Alternative sample restrictions and firm fixed effects. I run two exercises that vary the main sample of working mothers. First, I include the partially eligible group in the estimation. In this specification, the variable *eligible* in equation 3 takes a continuous value between 0 and 1 and corresponds to the increase in the number of days of maternity leave shown in Figure 1. The results, presented in panel (b) of Figure A.5, show that including the partially eligible group increases the employment effects. Therefore, not including them in the estimation provides a lower bound of the reform's effect on formal employment.

In the second exercise, I exclude eligible and ineligible women whose babies were born a week before May 2 and a week after July 25, effectively excluding women who are very close to the kinks in the eligibility function. The results, presented in panel (c) of Figure A.5, are based on a smaller sample, which leads to larger standard errors. Nonetheless, the estimated employment effects remain very similar to those in the main sample and are statistically significant for the first three years after childbirth.

Finally, Table A.6 reports the baseline results with firm fixed effects included as controls, measured at the time a worker starts her maternity leave.[8] Including these in the regression does not change the main findings on employment and earnings.

6 Mechanisms

The previous results show that allowing women to extend maternity leave from 12 to 24 weeks incentivizes formal employment for the next three years after giving birth and increases the return to experience. Additionally, among women who remained formally employed, their earnings do not decrease after seven years. In this section, I study the effects of the reform on employment quality and heterogeneous effects by demographics.

6.1 Separation and contract type

To understand what is driving the employment effects, I estimate difference-in-difference coefficients for the likelihood that a worker separates from her pre-birth employer and for the type of contract she holds while employed, either a temporary or an indefinite (more permanent) contract. Unfortunately, the AFC does not report hours worked, so I use contract type as a proxy for full-time contracts and employment quality.

Table 4 shows the results. The estimates in column (1) imply that most of the positive employment effects are driven by women who remain employed with their pre-

[8]Note that including firm fixed effects in the form of an AKM model can only be done in the sample of workers who remained employed in the AFC, which reduces the sample considerably. In the next section, instead of controlling for firm fixed effects, I study the effects on separation rates and contract type.

birth employer, which has positive implications for tenure within the firm. Column (2) shows a small increase in the proportion of eligible women working under a temporary contract, but this effect fades out after the first year. Columns (3) and (4) report results conditional on employment in the formal sector. Because this sample of women is much smaller, none of the results is statistically significant. However, the medium-term effects after seven years are not negligible and suggest that eligible women are 5.1 percentage points more likely to remain employed with their pre-birth employer (–16.4%) and 1.7 percentage points less likely to work under a temporary contract in the medium term (–22.4%). Overall, these results suggest that eligible women become more attached to the labor market compared to ineligible mothers, with positive implications for their formal earnings.

6.2 Heterogeneity by demographics

I investigate which groups of women benefit the most from an extension to maternity leave. First, I stratify coefficients β_τ and γ_τ in equation 3 by demographic groups at baseline. Motivated by previous research, I then examine differences by marital status, education, pre-birth tenure, wage, and age at birth.[9] The results for the likelihood of formal employment, presented in Figure 7, show no statistically significant differences by marital status, education, or age at birth. Most of the differences are driven by labor market attachment and wage level at baseline. In particular, panel (c) shows that women with low labor market attachment—defined as having fewer than 10 months of wage data (a proxy for social security contributions) in the year preceding the start of maternity leave—are four times more likely to take up the extension to maternity leave ($\beta_1 = 0.22$), compared to women with high pre-birth attachment, who also increase take-up but at a much lower rate ($\beta_1 = 0.05$).[10] This is because high-attachment women in the control group already have higher employment levels than low-attachment women

[9] Previous research has found that most of the effect is typically driven by the birth of the first child. Unfortunately, I do not know the birth order of the children in my sample. However, it is very likely that most births in the sample are first or second children, as in Chile, the average age of first birth for a woman employed in 2011 is 27.7 years.

[10] The median number of months with wage data the year before maternity leave is 10 months, and the average is 7.6 months. A very small proportion of workers have more than 12 months of wage data because they work for more than one employer in the same month.

in the second trimester after giving birth.

The employment effect in the first year is higher for low-attachment women, and the coefficients between groups are statistically different (p-value = 0.079). Panel (d) shows results by wage at the time of maternity leave. While high-wage women have higher take-up rates, the subsequent employment effects are similar between high- and low-wage workers.

Motivated by the large differences in take-up rates and employment effects in the first year after giving birth, in columns (2)–(7) of Table 5, I investigate whether these translate into differential effects on earnings, separation rates, and the likelihood of working under a temporary contract for all women in the sample and for those who remain employed after giving birth. The estimates show that eligible women with low pre-birth attachment benefit the most from an extended maternity leave: they not only increase formal employment and earnings at a higher rate than high-attachment women, but, when they return to work, they are also more likely to be employed with their pre-birth employer over the next seven years after giving birth, compared to ineligible low-attachment women. Conditional on employment, however, they have lower earnings, but they also reduce their likelihood of working under a temporary contract, which proxies for a more stable and higher-quality job (column (7)); though, the estimates are not statistically different from zero. This may reflect that the reform induces women with lower pre-birth tenure and lower wages to remain employed for longer with their pre-birth employer.

6.3 Access to childcare and labor market attachment

When the 2011 maternity leave reform was approved, legislators and policymakers cited women's low labor market attachment and the lack of childcare alternatives as two reasons for extending maternity leave. If one of the goals was to address the shortage of childcare options for working mothers, then, based on the results from the previous sections, one would expect women with limited access to childcare to be more likely to extend maternity leave (higher take-up rates), especially those with low pre-birth tenure.

I use data from Chile's Ministry of Education, an entity that reports the number of all educational establishments operating in the country.[11] I measure the supply of childcare establishments per capita by counting the number of childcare facilities per municipality in 2011, divided by the number of infants below the age of two, using census data. I match these records to workers' municipality of residence at the time they start their maternity leave, which I observe in the claims data. Then, I classify municipalities as having high childcare provision if they are above the median of per capita childcare facilities and low otherwise (median is 0.3). Finally, I stratify the treatment by high and low supply of childcare, and then I stratify the treatment into four groups: high and low attachment, and high and low supply of childcare. Figure 8 presents the results of estimating the reform's stratified treatment effects on formal employment and formal wages.

The results in panel (a) show that women with low access to childcare in their municipality of residence increase formal employment the most in the first year after giving birth, and these differences are statistically significant between groups (p-values are below 0.01 for coefficients below year 1). Panel (b) shows that the employment effects are the largest for women with low labor market attachment, regardless of childcare supply, but they are also positive and statistically significant for women with high attachment but low access to childcare (purple dots). In other words, mothers who respond less to the reform are those with high attachment and high access to childcare.

When examining wage effects in the bottom panels of Figure 8, the largest wage effects are for women who increase their employment the most, that is, for women with low access to childcare, and women who were less attached to the labor market before childbirth.

These results suggest larger returns to tenure for women with low pre-birth labor market attachment in municipalities where mothers have fewer formal alternatives for childcare.[12] In this sense, a longer leave incentivizes employment by allowing working mothers to spend more time at home for 12 additional weeks and serves as a substitute

[11] The data can be accessed here.

[12] Other forms of care could include extended family or informal childcare. Unfortunately, I cannot observe a household's composition in my data.

for early childcare (before the age of six months).

6.4 Differential effects on other sick leave claims by labor market attachment

Following the previous analyses, I examine which group of workers is more likely to reduce their use of other sick leave claims. In Appendix Table A.7, I estimate the reform's effects on the use and length of other sick leave claims by high- and low-attachment working mothers. The results show that high-attachment women are more likely to reduce their use of sick leave, sick-child leave, and mental health leave claims compared to low-attachment women. The total effect of the reform on the number of days of use of other sick leave claims is a reduction of 3.88 for high-attachment women and 1.12 days for low-attachment women. Column (5) shows that the reduction in fertility, measured as the likelihood of filing a new maternity leave claim in the following six years, is driven by high-attachment women. The effect corresponds to a 10% reduction compared to ineligible women.

Contrary to the results on employment, these results suggest that the substitution between maternity leave and other sick leave claims is driven by women who are more likely to return to the labor market after giving birth. This could be explained by eligibility rules for sick leave, which requires workers to be formally employed in order to file claims. Another interpretation is that the margin of adjustment is more relevant for women who had a higher short-term need for sick leave. This stands in contrast to the results on childcare access, where the largest effects were found among women in municipalities with the lowest supply of childcare facilities.

One interpretation of these results is that access to longer maternity leave incentivizes formal employment on the extensive margin for women with low labor market attachment and limited childcare alternatives. For high-attachment women, by contrast, a longer leave does not significantly incentivize formal employment, as they likely would have returned to work regardless of the policy. However, it reduces their use of sick-child leave and mental health leave, possibly because they are better prepared to return to work after giving birth.

7 Discussion and policy implications

In this section, I discuss how the previous estimates compare to maternity leave extensions in other settings, and the long-term implications of increasing formality among working mothers.

7.1 Comparison of estimates to other countries

As previously discussed in Section 2.2, the effects of maternity leave on women's outcomes may be ambiguous. The duration of maternity leave in developed countries is much longer than what is studied in this paper, usually over a year. My results suggest larger effects than those found in more developed settings for a 100% increase in the maternity leave duration, though in this case, the extension still allowed women to stay at home for less than a year. Hence, the effect on leave taking is large, but not so large as to cause women to drop out of formal employment.

Table A.8 summarizes findings on employment and earnings for the four most recent cases of maternity leave extensions studied in developing countries. In the context of this paper, the two most comparable are Albagli and Rau (2019) for Chile and Machado et al. (2024) for Brazil. First, Albagli and Rau (2019) investigate the same reform as this paper. They use survey data to measure mothers' employment one year after giving birth and find a 5.8 percentage point increase, with no effects on wages. My results on employment are consistent with theirs in the first year, but are larger, likely because I focus on formal employment only. My results may be interpreted as an upper bound on total employment. For example, if the reform incentivizes women to reduce informality but increase formal employment, the total effect on employment is lower than my estimate.

Second, Machado et al. (2024) study a reform in Brazil that extended maternity leave from 120 days to 180. They find positive employment effects after 10 months, but the effects fade out over time. A key difference in their setting is that firms must opt into the maternity leave program to receive tax reductions. The authors find a small take-up rate of 35.7%, with the greatest benefits accruing to women with higher pre-birth tenure. In contrast, take-up in my setting does not depend on employer participation, which increases the likelihood that eligible women use the extended leave. Interestingly, I find the opposite with respect to pre-birth attachment: women with lower tenure and limited access to childcare alternatives benefit the most.

7.2 Long-term implications of increasing formal employment

While this paper cannot speak to changes in total employment—only to formal employment—the results on separation and contract type suggest that eligible women are more likely to be formally employed in better jobs and to increase tenure with their pre-birth employer. Because these jobs are more stable and permanent, this indicates that eligible women are transitioning away from informality or unemployment.

These findings have implications for future consumption. The positive effects on formal employment translate into nearly one additional year of formal experience and higher earnings in the medium term. The estimates can be used to calculate the effect of the reform on increased social security contributions, which will, in turn, lead to higher pensions. The cumulative effect on earnings from the third month after giving birth through the end of year seven is US$1,895 in 2018, equivalent to 2.7 times the average monthly wage in the sample. The present value of this increase by age 60, when women retire, amounts to $4,671.[13]

According to Cabezón (2023), in Chile, each extra dollar of formal savings translates into a $0.72 increase in consumption after retirement.[14] Thus, if workers are mandated to contribute 10% of their formal wages to social security, the total effect on consumption after retirement for the average woman in my sample is 0.1*0.72*$4,671=$336.3, which is very close to one month of the minimum wage in 2018. Importantly, this estimate should not be generalized to the average working mother in Chile but rather applies to the average woman who has access to maternity leave. This is because women who claim maternity leave are highly selected in terms of their formal employment and earnings.

[13] The mandatory age of retirement for women in Chile is 60 years old, but in practice, the average woman retires at 62. These are estimates from Chile's Superintendence of Pensions.

[14] The author does not differentiate this estimate by gender.

8 Conclusions

This paper estimates the short- and medium-term consequences of extending maternity leave on mothers' labor market outcomes. I exploit a 2011 reform implemented in Chile that increased maternity leave from 84 days to 168 and created a change in mothers' ability to extend maternity leave based on their child's birth date. As a result, women who gave birth after May 2, 2011 were as good as randomly assigned to receive the extended leave.

I compare mothers who are eligible for the extension to those ineligible to estimate the effects of longer leave on labor market outcomes up to seven years after giving birth. First, I show that eligible women extend their maternity leave by an average of 79 days and reduce the use and length of other paid leave claims by 2.45 days in the first year after giving birth. Second, I find that the reform increases women's formal employment for three years after childbirth and that, conditional on employment, wages do not decrease in the medium term.

To investigate mechanisms, I study heterogeneity by worker demographics. The positive employment effects are driven by women with low pre-birth tenure, for whom I observe less than 10 months of social security contributions before maternity leave. These results contrast with previous research that finds that women with higher wages and high pre-birth tenure are more likely to benefit from longer maternity leave.

Overall, my results suggest that allowing women time to recover from childbirth can positively impact their careers, especially for those more likely to exit the labor market and for whom childcare alternatives are limited. However, it is important to interpret these findings with caution, as they are based on a selected group of women who were already employed before childbirth and were eligible for maternity leave. As such, these results open avenues for future research on maternal protection in developing countries and the design of policies that incentivize women's employment both before and after giving birth.

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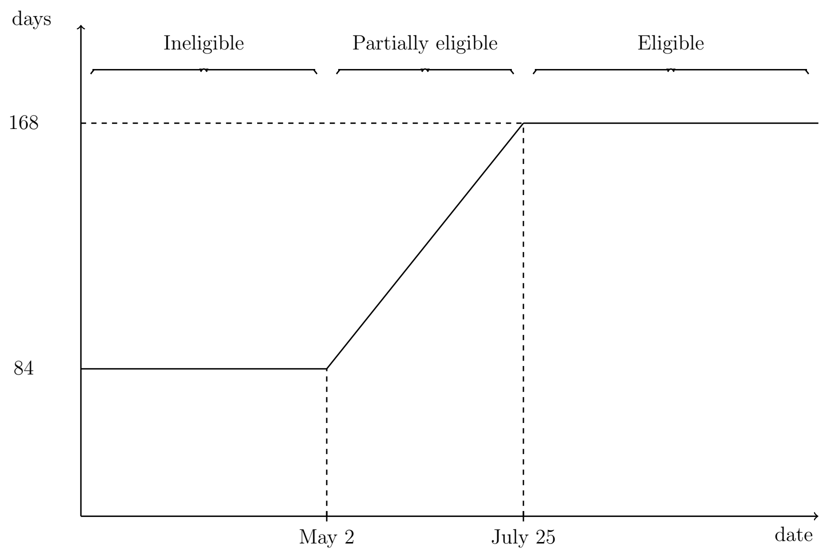
Figures and Tables

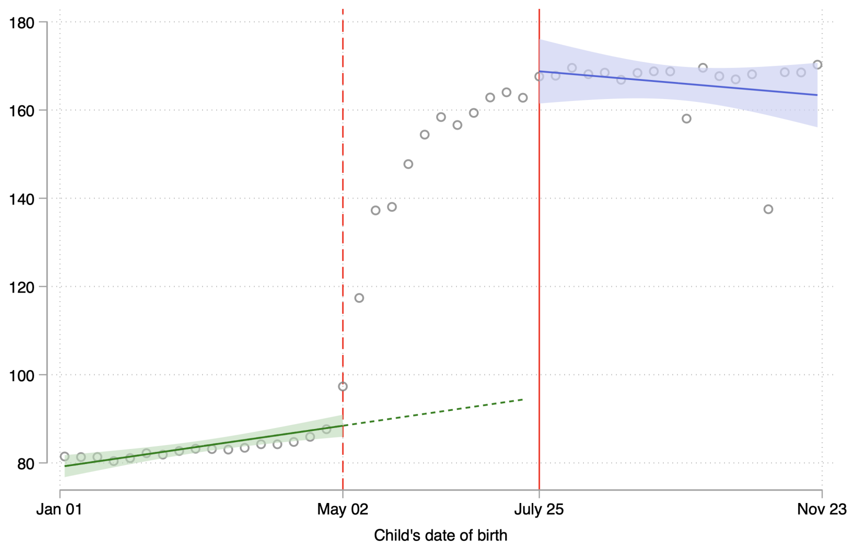
Figure 1: Eligibility function by child's date of birth



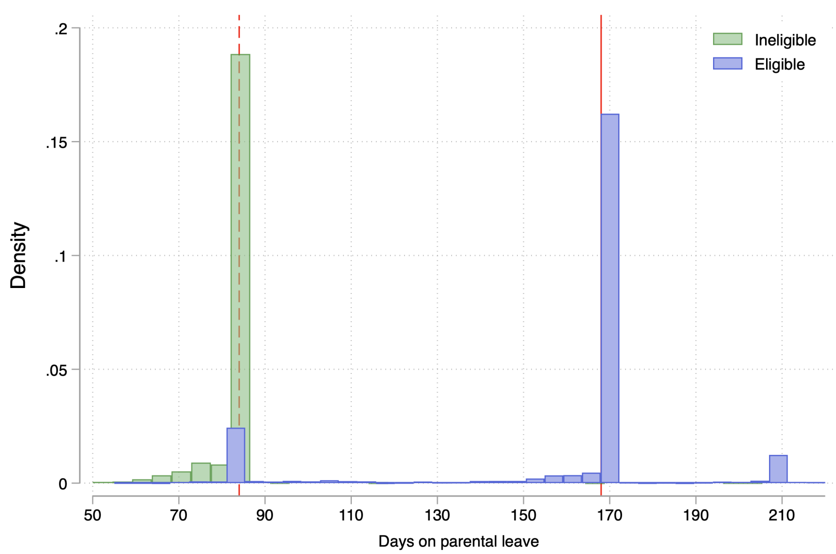
Notes: This figure replicates the exposure (eligibility) function of Albagli and Rau (2019) for the 2011 maternity leave reform in Chile.

Figure 2: Average number of days of maternity leave by child's date of birth

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



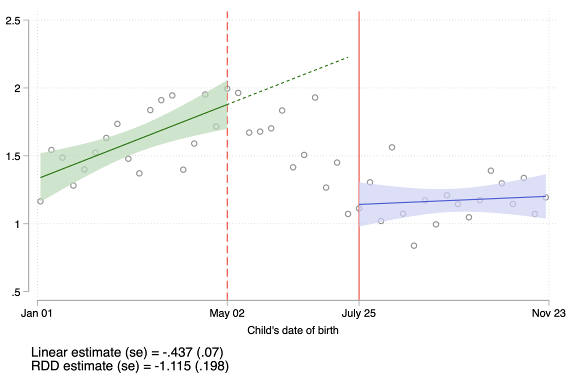
(b) Distribution of days of leave by eligibility



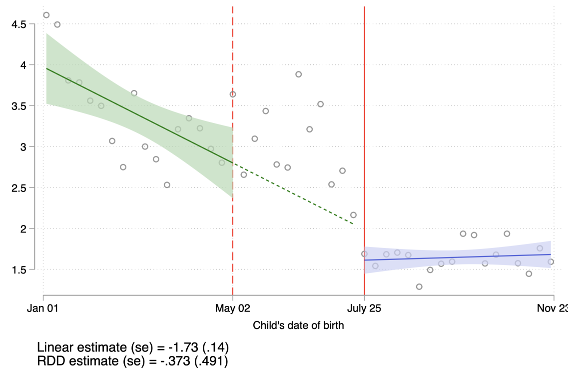
Notes: Panel (a) plots the average number of days of maternity leave by child's week of birth for women in the estimation sample, along with 95% confidence intervals for the linear estimates, shown separately for ineligible women in green and eligible women in blue. Panel (b) plots the distribution of days of leave for women in the sample by eligibility status. Blue bars in panel (b) include eligible and partially eligible women.

Figure 3: Effect of the reform on the number of days of other sick leave claims within the first year after childbirth

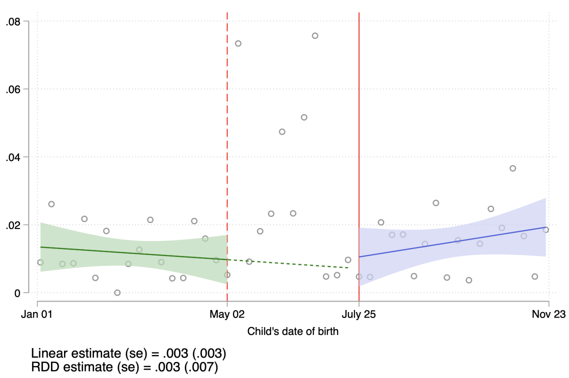
(a) Leave due to accident or illness



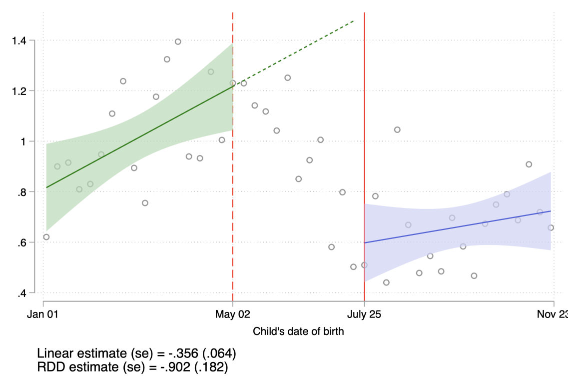
(b) Leave due to sick child below 1 y/o



(c) Leave due to pregnancy-related illness



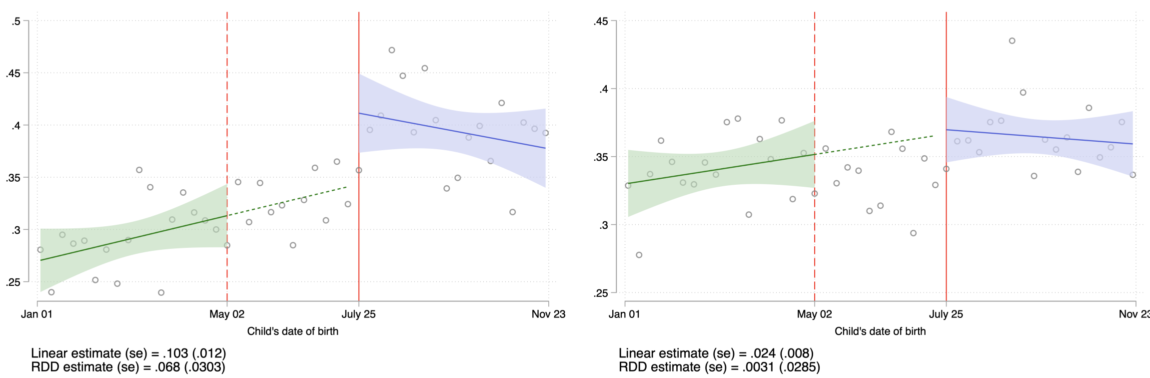
(d) Leave due to mental health



Notes: These figures report the reform's effect on the average number of days of leave by child's week of birth for women in the estimation sample, on different types of sick leave between the end of leave and a child's first birthday. They include 95% confidence intervals for the linear estimates, shown separately for ineligible women in green and eligible women in blue. The notes in each panel report reform estimates and their standard errors in parentheses, labeled as "Linear estimate" and "RDD estimate" equivalent to columns (4) and column (5) of Table 2, respectively. See the main text for details.

Figure 4: Donut RDD estimates on formal employment

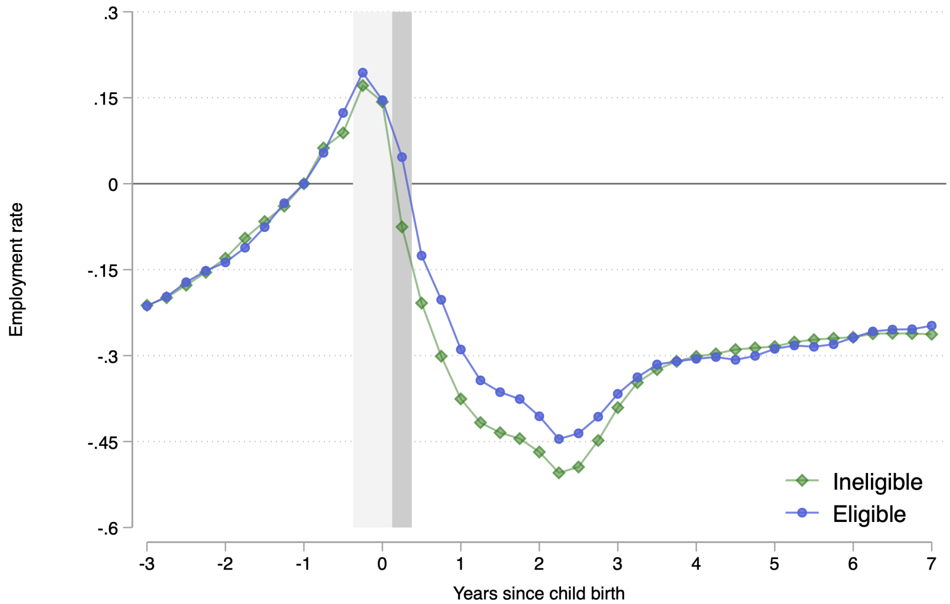
(a) Employment 1-3 years after birth **(b)** Employment 4+ years after birth



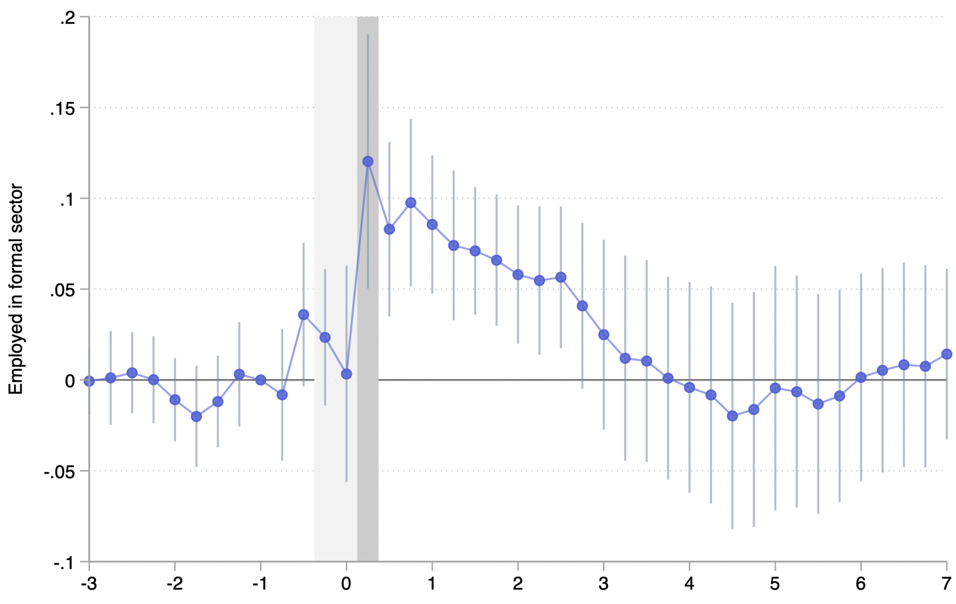
Notes: These figures plot donut RDD estimates of the reform's effect on formal employment of women in the baseline sample from 1 to 3 years after giving birth in panel (a), and from 4 to 7 years after giving birth in panel (b). Ninety-five percent confidence intervals are estimated separately before May 2 and after July 25. The notes in each panel report reform estimates and their standard errors in parentheses, labeled as "Linear estimate" and "RDD estimate" equivalent to columns (4) and column (5) of Table 2, respectively. See the main text for details.

Figure 5: Medium-term effects of the reform on women's formal employment

(a) Employment series



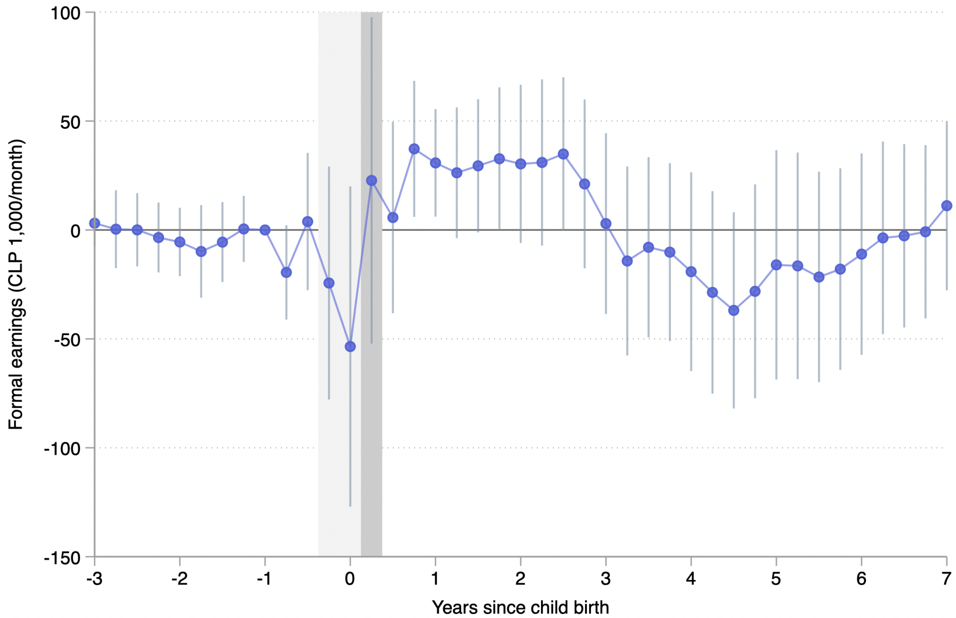
(b) Difference-in-difference



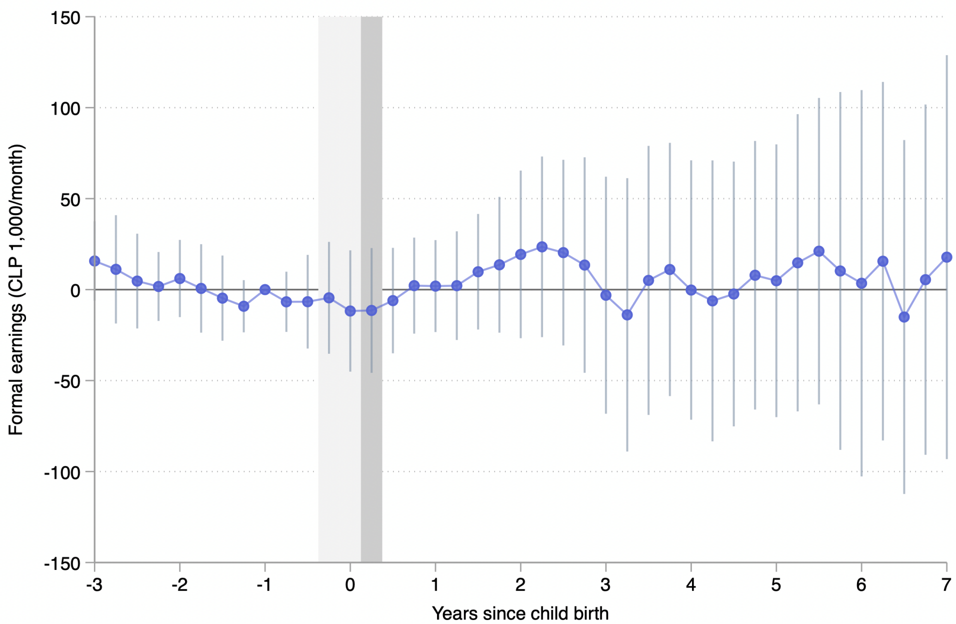
Notes: Panel (a) plots the employment trajectories of women in the formal sector for the eligible group in blue and the ineligible group in green, normalized to quarter –4, after adjusting for baseline controls and quarter-by-year fixed effects. Panel (b) plots the difference-in-difference estimates and their 95% confidence intervals from equation 3 in the donut sample of workers in a symmetric window of 4.2 months (121 days). The light gray area corresponds to the time of maternity leave from -6 to 12 weeks relative to childbirth. The dark gray area corresponds to the weeks an eligible woman can extend her maternity leave from 12 to 24. Controls include age fixed effects, a private insurance dummy, and wage at the time of maternity leave. Standard errors are clustered by child's week of birth.

Figure 6: Medium-term effects of the reform on women's earnings

(a) Formal earnings

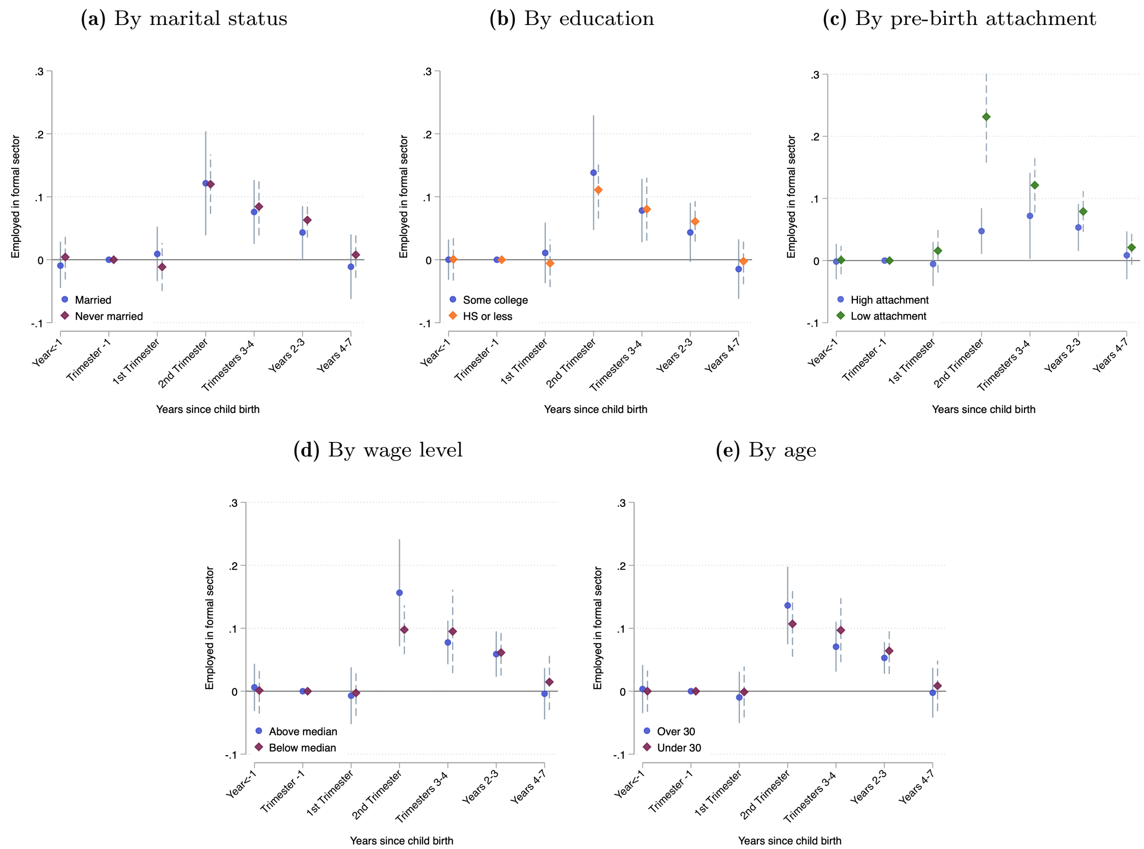


(b) Formal earnings if employed



Notes: This figure shows difference-in-difference estimates and their 95% confidence intervals from the sample of workers in a symmetric window of 4.2 months without partially eligible observations. The light gray area corresponds to the time of maternity leave from -6 to 12 weeks relative to childbirth. The dark gray area corresponds to the weeks an eligible woman can extend her maternity leave from 12 to 24. Panel (a) shows the reform's effects on wages that include zeros when a worker is not in the AFC. Panel (b) conditions on being employed in the AFC, hence, positive earnings.

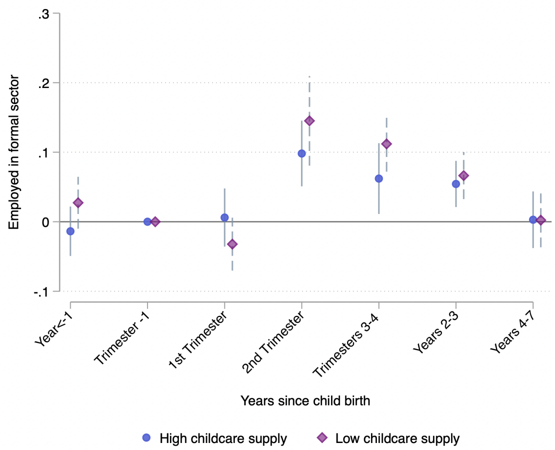
Figure 7: Heterogeneous effects of the reform on formal employment by baseline demographics



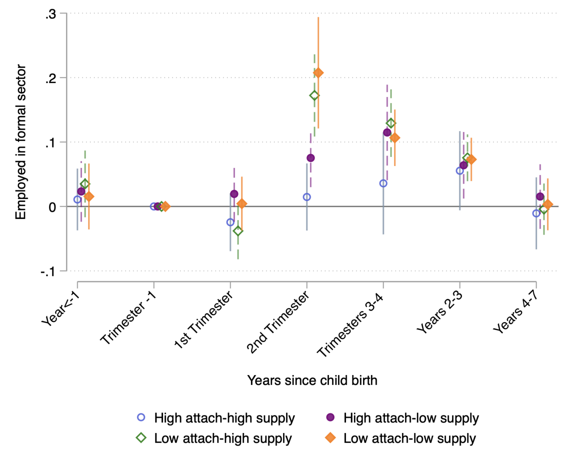
Notes: These figures show pooled difference-in-difference estimates, as in Table 3, stratified by baseline characteristics.

Figure 8: Heterogeneous effects of the reform on women's employment by access to childcare

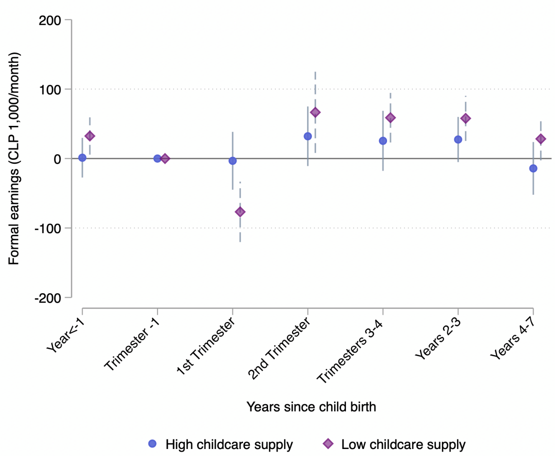
(a) Formal employment



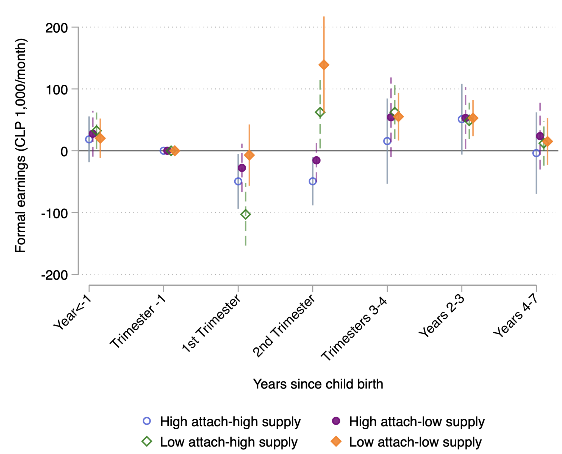
(b) Formal employment



(c) Earnings



(d) Earnings



Notes: These figures show pooled difference-in-difference estimates, as in Table 3. Panels (a) and (c) present results stratified by access to childcare in a mother's municipality of residence at birth. Panels (b) and (d) present results stratified into four groups defined by the combination of high and low pre-birth attachment and low and high supply of per-capita childcare facilities in a mother's municipality of residence at birth.

Table 1: Characteristics of workers before maternity leave

	Panel A. Sample Means				*Panel B. Balancing Tests*		
	All	Ineligible	Partially Eligible	Eligible	Unconditional	Unconditional (w/o partially)	Donut RDD (w/o partially)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	29.826	29.63	29.968	29.932	0.275**	0.302**	0.06
	[5.421]	[5.394]	[5.565]	[5.355]	(0.113)	(0.119)	(0.296)
Private insurance	0.89	0.891	0.892	0.888	-0.005	-0.003	0.003
	[0.313]	[0.312]	[0.311]	[0.316]	(0.008)	(0.008)	(0.028)
Wage before leave	483.767	481.304	465.62	497.176	17.707	15.872	5.2
	[358.953]	[351.23]	[358.726]	[366.067]	(14.218)	(15.101)	(38.999)
Average wage $t = -1$	470.131	455.408	457.506	492.14	34.305*	36.731*	-4.042
	[414.484]	[395.815]	[413.07]	[432.108]	(19.885)	(21.341)	(37.137)
College	0.408	0.407	0.403	0.413	0.01	0.007	-0.002
	[0.492]	[0.491]	[0.491]	[0.493]	(0.015)	(0.016)	(0.043)
High school	0.48	0.489	0.471	0.477	-0.014	-0.012	-0.005
	[0.5]	[0.5]	[0.499]	[0.5]	(0.013)	(0.014)	(0.04)
Less than high school	0.111	0.104	0.126	0.109	0.004	0.005	0.007
	[0.315]	[0.305]	[0.332]	[0.312]	(0.009)	(0.009)	(0.034)
Married	0.274	0.273	0.281	0.27	-0.004	-0.003	0.001
	[0.446]	[0.446]	[0.45]	[0.444]	(0.011)	(0.012)	(0.031)
Never married	0.726	0.727	0.719	0.73	0.004	0.003	-0.001
	[0.446]	[0.446]	[0.45]	[0.444]	(0.011)	(0.012)	(0.031)
# months in AFC $t = -1$	7.48	7.532	7.345	7.511	-0.056	-0.021	0.604
	[4.731]	[4.722]	[4.837]	[4.674]	(0.225)	(0.247)	(0.586)
Temporary contract $t = -1$	0.134	0.13	.133	.138	0.005	0.007	0.017
	[0.277]	[0.275]	[0.275]	[0.281]	(0.009)	(0.009)	(0.021)
Firm size $t = -1$	3255.639	3258.888	3091.458	3349.756	41.577	90.867	-211.162
	[3863.659]	[3889.03]	[3725.657]	[3916.356]	(289.61)	(309.76)	(391.806)
% female workers $t = -1$	0.595	0.597	0.595	0.592	-0.005	-0.005	-0.012
	[0.204]	[0.201]	[0.208]	[0.205]	(0.005)	(0.006)	(0.016)
Observations	10,500	3,983	2,441	4,014	10,500	8,041	8,041

Notes: This table reports summary statistics for women in the estimation sample. Subscript $t = -1$ stands for the monthly average during the year before a worker starts maternity leave. The unconditional differences in column (5) report estimates from a regression of each worker characteristic on the eligibility function shown in Figure 1, normalized between 0 and 1. Column (6) repeats the previous exercise but excludes partially eligible women. Column (7) estimates a donut RDD coefficient that excludes partially eligible women and allows different slopes for the dependent variable before May 2 and after July 25. Clustered standard errors by child's week of birth are reported in parentheses, and standard deviations are in brackets. 10%*, 5%**, 1%***.

Table 2: Effect of the reform on the number of days of maternity leave

	Days on maternity leave				
	All	All	W/o partially eligible	W/o partially eligible	Donut RDD
	(1)	(2)	(3)	(4)	(5)
Eligible	79.300***	79.251***	81.666***	81.696***	78.943***
	(3.456)	(3.460)	(2.637)	(2.590)	(2.591)
Adj. R^2	0.649	0.650	0.862	0.862	0.865
Ineligible mean	83.146	83.146	83.146	83.146	83.146
Observations	10,500	10,500	8,041	8,041	8,041
Controls		✓		✓	

Notes: This table shows estimates of the effect of the maternity leave reform on the number of days of leave. "All" stands for all workers in a symmetric window of 4.2 months (121 days) around partially eligible mothers, which corresponds to women with maternity leave claims filed between January 1 and November 23, 2011. "W/o partially eligible" is the sample that excludes partially eligible workers who gave birth between May 3 and July 24. The donut RDD estimate allows for different slopes of the outcome variable before May 2 and after July 25, estimated at the July 25 cutoff. Baseline controls include mother's age fixed effects, wage before the start of maternity leave, and a dummy for private insurance. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table 3: Difference-in-difference estimates on labor market outcomes

				Conditional on Employment	
	Employed	Years of experience	Earnings	Earnings	Earnings winsorized
	(1)	(2)	(3)	(4)	(5)
$\text{Eligible}_{\text{2nd trimester}}$	0.120***	-	48.847*	-7.344	-5.579
	(0.026)	-	(24.543)	(10.564)	(11.680)
$\text{Eligible}_{\text{3-4 trimester}}$	0.082***	0.041***	35.879**	1.227	1.847
	(0.021)	(0.010)	(17.305)	(9.559)	(11.490)
$\text{Eligible}_{\text{Years 2-3}}$	0.058***	0.296***	37.663***	11.285	11.586
	(0.013)	(0.060)	(13.590)	(15.201)	(17.431)
$\text{Eligible}_{\text{Years 4-7}}$	0.002	0.945***	2.530	4.755	0.699
	(0.019)	(0.306)	(14.544)	(30.685)	(24.119)
Adj. R^2	0.153	0.153	0.123	0.652	0.649
Outcome mean 2nd trim	0.728	-	375.377	515.708	524.381
Outcome mean 3-4 trim	0.540	-	281.027	520.042	525.685
Outcome mean 2-3 year	0.360	-	237.585	659.426	664.147
Outcome mean 4-7 year	0.212	-	216.141	1017.532	926.893
Controls	✓	✓	✓	✓	✓
Observations	1,061,412	1,061,412	1,061,412	373,843	366,421
Unique workers	8,041	8,041	8,041	8,041	7,978

Notes: This table shows pooled difference-in-difference estimates from the sample of workers in a symmetric window of 4.2 months without partially eligible observations. Pre-birth categories are omitted from the table. Column (3) includes zeros when a worker is not in the AFC, and column (4) conditions on being employed in the AFC. Means of the outcome variables are reported after childbirth for each time period. Controls include age fixed effects, a private insurance dummy, and wage before the start of maternity leave. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table 4: Difference-in-difference estimates on types of employment

	Separation from pre-birth employer	Temporary contract	Conditional on Employment	
			Separation	Temp. contract
	(1)	(2)	(3)	(4)
$\text{Eligible}_{\text{2nd trimester}}$	-0.119***	0.041***	-0.006	0.028*
	(0.027)	(0.009)	(0.013)	(0.014)
$\text{Eligible}_{\text{3-4 trimester}}$	-0.080***	0.014*	-0.003	-0.001
	(0.028)	(0.008)	(0.020)	(0.017)
$\text{Eligible}_{\text{Years 2-3}}$	-0.054***	-0.002	-0.008	-0.016
	(0.019)	(0.008)	(0.027)	(0.018)
$\text{Eligible}_{\text{Years 4-7}}$	-0.013	-0.010	-0.051	-0.017
	(0.019)	(0.009)	(0.037)	(0.024)
Adj. R^2	0.179	0.045	0.105	0.077
Outcome mean 2nd trim	0.268	0.091	0.019	0.124
Outcome mean 3-4 trim	0.457	0.055	0.032	0.102
Outcome mean 2-3 year	0.651	0.030	0.091	0.084
Outcome mean 4-7 year	0.815	0.019	0.237	0.088
Controls	✓	✓	✓	✓
Observations	1,061,412	1,061,412	373,843	373,843
Unique workers	8,041	8,041	8,041	8,041

Notes: This table shows pooled difference-in-difference estimates from the sample of workers in a symmetric window of 4.2 months without partially eligible observations. Pre-birth categories are omitted from the table. Columns (3) and (4) condition the outcome for workers employed in the AFC. Means of the outcome variables are reported after childbirth for each time period. Controls include age fixed effects, a private insurance dummy, and wage before the start of maternity leave. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table 5: Difference-in-difference estimates on labor market outcomes by pre-birth tenure

					Conditional on employment		
	Employed	Earnings	Separation	Temp. contract	Earnings	Separation	Temp. contract
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. High attachment							
$\text{Eligible}_{\text{2nd trimester}}$	0.047**	-17.466	-0.044**	0.050***	-21.018*	-0.007	0.039**
	(0.018)	(16.207)	(0.019)	(0.011)	(11.714)	(0.013)	(0.016)
$\text{Eligible}_{\text{3-4 trimester}}$	0.072**	48.868	-0.072*	0.015	5.186	-0.012	-0.002
	(0.034)	(29.113)	(0.038)	(0.010)	(11.227)	(0.019)	(0.018)
$\text{Eligible}_{\text{Years 2-3}}$	0.053***	51.395***	-0.049**	-0.000	19.567	-0.011	-0.018
	(0.019)	(18.184)	(0.024)	(0.009)	(16.824)	(0.026)	(0.019)
$\text{Eligible}_{\text{Years 4-7}}$	0.008	15.777	-0.018	-0.009	12.759	-0.056	-0.028
	(0.019)	(20.894)	(0.021)	(0.010)	(31.117)	(0.038)	(0.027)
Panel B. Low attachment							
$\text{Eligible}_{\text{2nd trimester}}$	0.231***	148.012***	-0.231***	0.033***	10.470	-0.022	-0.012
	(0.036)	(33.475)	(0.039)	(0.009)	(9.632)	(0.016)	(0.014)
$\text{Eligible}_{\text{3-4 trimester}}$	0.121***	52.095***	-0.116***	0.014*	-7.541	-0.011	-0.027
	(0.022)	(17.099)	(0.031)	(0.008)	(10.660)	(0.027)	(0.019)
$\text{Eligible}_{\text{Years 2-3}}$	0.079***	33.365**	-0.075***	-0.003	-18.841	-0.053	-0.040
	(0.016)	(13.271)	(0.022)	(0.008)	(16.862)	(0.040)	(0.024)
$\text{Eligible}_{\text{Years 4-7}}$	0.021	1.140	-0.031**	-0.008	-26.387	-0.105**	-0.020
	(0.014)	(16.795)	(0.014)	(0.008)	(41.558)	(0.049)	(0.029)
Adj. R^2	0.320	0.193	0.320	0.061	0.653	0.167	0.091
Observations	1,061,412	1,061,412	1,061,412	1,061,412	373,843	373,843	373,843
Controls	✓	✓	✓	✓	✓	✓	✓
p-val diff 2nd trim	0.000	0.000	0.000	0.096	0.004	0.276	0.000
p-val diff 3-4 trim	0.058	0.876	0.085	0.902	0.247	0.963	0.128
p-val diff 2-3 years	0.251	0.359	0.247	0.750	0.020	0.131	0.316
p-val diff 4-7 years	0.461	0.513	0.496	0.977	0.267	0.270	0.745

Notes: This table shows difference-in-difference estimates from the sample of workers in a symmetric window of 4.2 months without partially exposed observations. Pre-birth categories are omitted from the table. Regressions are stratified by attachment to the labor market before childbirth, that is, β_τ's are estimated for each group in the same regression. Low tenure is defined as having less than 10 months of employment records in the AFC in the year before the start of maternity leave, and high tenure is defined as 10 or more months. The rows labeled "p-val diff" correspond to the p-value of the null hypothesis that the difference between coefficients for low and high attachment in the corresponding period is equal to 0. All regressions control for age fixed effects, a private insurance dummy, and wage at the time of maternity leave. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

A Additional figures and tables

Table A.1: Characteristics of workers before maternity leave

	Sample of women on maternity leave (1)	Sample of women in AFC on maternity leave (2)	Difference [(2)-(1)] (3)
Days on leave	127.894	129.696	1.803***
	[44.895]	[45.107]	(0.676)
Age	29.936	29.826	-0.110
	[5.651]	[5.421]	(0.083)
Private insurance	0.757	0.890	0.133***
	[0.429]	[0.313]	(0.006)
# of births	1.000	1.001	0.0004
	[0.016]	[0.029]	(0.004)
Wage before leave	466.388	484.532	17.378***
	[370.004]	[360.177]	(5.457)
Observations	7,697	10,500	18,197

Notes: This table reports summary statistics for women on maternity leave between January 1, 2011, and November 23, 2011. Column (1) reports summary statistics for a 10% sample of all maternity leave claims in Chile in the corresponding period. Column (2) reports summary statistics for the estimation sample, which corresponds to a 10% sample of firms and their workers in the AFC. Column (3) reports simple differences in covariates between samples. Standard deviations are in brackets, and standard errors are in parentheses. 10%*, 5%**, 1%***.

Table A.2: McCrary test on manipulation of cutoff

Polynomial	All claims	Claims linked to AFC
	(1)	(2)
2	0.412	0.122
3	0.686	0.134
4	0.901	0.244
Observations	16,453	5,361

Notes: This table reports p-values for McCrary density tests for cutoff manipulation, for different polynomial levels (McCrary, 2008). The cutoff is defined as 0 for births occurring on July 25, 2011. The samples do not include partially eligible mothers; hence, the running variable is adjusted by 83 days for births that occurred before May 2, 2011 (84 days before July 25). Column (1) includes all claims in the sample, column (2) includes claims linked to employment records.

Table A.3: Effect of the reform on the number of days of maternity leave using optimal bandwidth

	Days on maternity leave					
	All	All	W/o partially eligible	W/o partially eligible	Donut RDD	Robust RDD
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible	75.415***	75.332***	82.074***	81.827***	62.912***	76.402***
	(7.129)	(7.091)	(0.847)	(0.868)	(8.472)	(2.762)
Ineligible mean	86.170	86.170	86.170	86.170	86.170	86.170
Adj. R^2	0.404	0.414	0.845	0.848	0.845	-
Observations	4,176	4,176	1,717	1,717	1,717	1,717
Controls		✓		✓		

Notes: This table shows estimates of the effect of the maternity leave reform on the number of days of leave, columns replicate samples in Table 2 in columns (1)–(5) with the restriction of optimal bandwidth that includes claims with births between April 5, 2011 and August 21, 2011. Column (6) shows robust RD estimate that omits partially eligible observations using the methodology by Calonico et al. (2014). Baseline controls in columns (2) and (4) include mother's age fixed effects, average wage before maternity leave, and a dummy for private insurance. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table A.4: Effect of the reform on the probability of filing a leave claim

	Type of sick leave				Maternity leave
	Illness	Sick child	Pregnancy	Mental health	
	(1)	(2)	(3)	(4)	(5)
Eligible	-0.189***	-0.003	0.002	-0.209***	0.000
	(0.034)	(0.044)	(0.006)	(0.033)	(0.000)
Adj. R^2	0.015	0.043	0.001	0.017	0.075
Ineligible mean	0.417	0.581	0.009	0.268	0.300
Observations	8,041	8,041	8,041	8,041	8,041

Notes: This table shows estimates of the reform's effect on the number of days of leave for other sick leave claims filed after the end of maternity leave within a year using equation 2. "Maternity leave" is any new maternity claim filed over the next seven years after 2011. Observations include women in the donut sample, which corresponds to those with maternity leave claims filed between January 1 and November 23, 2011, excluding those who gave birth between May 3 and July 24. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table A.5: Poisson Pseudo-MLE difference-in-difference estimates on labor market outcomes

	Employed	Earnings	Separation from pre-birth employer	Temporary contract
	(1)	(2)	(3)	(4)
$\text{Eligible}_{\text{2nd trimester}}$	0.173***	0.160**	-0.414***	0.456***
	(0.046)	(0.064)	(0.112)	(0.092)
$\text{Eligible}_{\text{3-4 trimester}}$	0.150***	0.156**	-0.189***	0.241*
	(0.037)	(0.063)	(0.066)	(0.137)
$\text{Eligible}_{\text{Years 2-3}}$	0.157***	0.176***	-0.085***	0.141
	(0.037)	(0.058)	(0.031)	(0.174)
$\text{Eligible}_{\text{Years 4-7}}$	0.016	0.022	-0.015	-0.027
	(0.069)	(0.069)	(0.028)	(0.218)
Controls	✓	✓	✓	✓
Observations	1,061,412	1,061,412	373,843	373,843
Unique workers	8,041	8,041	8,041	8,041

Notes: This table shows pooled difference-in-difference estimates estimated from a Poisson Pseudo-MLE model. The sample corresponds to workers in a symmetric window of 4.2 months without partially eligible observations. Pre-birth categories are omitted from the table. Controls include age fixed effects, a private insurance dummy, and wage before the start of maternity leave. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table A.6: Difference-in-difference estimates on labor market outcomes

	Employed	Earnings	Earnings>0	Earnings>0 winsorized
	(1)	(2)	(3)	(4)
$\text{Eligible}_{\text{2nd trimester}}$	0.131***	57.267**	-18.262**	-14.409
	(0.022)	(22.617)	(8.359)	(9.353)
$\text{Eligible}_{\text{3-4 trimester}}$	0.088***	40.330**	-11.299	-8.931
	(0.023)	(19.511)	(8.479)	(9.594)
$\text{Eligible}_{\text{Years 2-3}}$	0.067***	44.770***	-7.514	-5.341
	(0.013)	(13.978)	(14.464)	(15.833)
$\text{Eligible}_{\text{Years 4-7}}$	0.014	11.596	-3.831	-9.814
	(0.018)	(15.060)	(30.162)	(24.118)
Adj. R^2	0.153	0.123	0.652	0.649
Outcome mean 2nd trim	0.728	375.377	515.708	524.381
Outcome mean 3-4 trim	0.540	281.027	520.042	525.685
Outcome mean 2-3 year	0.360	237.585	659.426	664.147
Outcome mean 4-7 year	0.212	216.141	1017.532	926.893
Controls	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Observations	1,061,412	1,061,412	373,843	366,421
Unique workers	8,041	8,041	8,041	7,978

Notes: This table shows difference-in-difference estimates from the sample of workers in a symmetric window of 4.2 months without partially eligible observations. Pre-birth categories are omitted from the table. Column (3) includes zeros when a worker is not in the AFC, and column (4) conditions on being employed in the AFC. Controls include age fixed effects, a private insurance dummy, wage before the start of maternity leave, and firm fixed effects, measured before the start of maternity leave. Standard errors are clustered at the week-of-birth level. 10%*, 5%**, 1%***.

Table A.7: Effect of the reform on the number of days of leave for other sick leave claims filed, by labor market attachment

	Type of sick leave				Future
	Illness	Sick child	Pregnancy	Mental Health	maternity leave
	(1)	(2)	(3)	(4)	(5)
Panel A. Probability of filing a leave claim					
Eligible* High attach.	-0.102***	-0.215***	-0.001	-0.130***	-0.028**
	(0.020)	(0.017)	(0.002)	(0.018)	(0.012)
Eligible* Low attach.	0.008	-0.109***	0.001	-0.032***	-0.012
	(0.013)	(0.021)	(0.003)	(0.011)	(0.015)
Adj. R^2	0.030	0.048	0.001	0.031	0.076
Control mean high	0.507	0.641	0.008	0.344	0.282
Control mean low	0.333	0.525	0.010	0.198	0.317
P-val diff	0.000	0.000	0.394	0.000	0.350
Panel B. Days of leave					
Eligible* High attach.	-0.773***	-2.499***	-0.000	-0.612***	
	(0.111)	(0.169)	(0.004)	(0.101)	
Eligible* Low attach.	-0.063	-0.976***	0.007	-0.092	
	(0.066)	(0.145)	(0.004)	(0.055)	
Adj. R^2	0.036	0.077	-0.001	0.024	
Ineligible mean high	2.118	4.160	0.011	1.372	
Ineligible mean low	1.095	2.641	0.012	0.664	
P-val diff	0.000	0.000	0.276	0.000	
Observations	8,041	8,041	8,041	8,041	8,041

Notes: This table shows estimates of the reform's effect on the number of days of leave for other sick leave claims filed after the end of maternity leave within a year. "Maternity leave" is any new maternity claim filed over the next seven years after 2011. Observations include women from the donut sample, which corresponds to those with maternity leave claims filed between January 1 and November 22, 2011, excluding those who gave birth between May 3 and July 24. Baseline controls include mother's age fixed effects, wage at the time of maternity leave, and a dummy for private insurance. Standard errors are clustered by child's week of birth. 10%*, 5%**, 1%***.

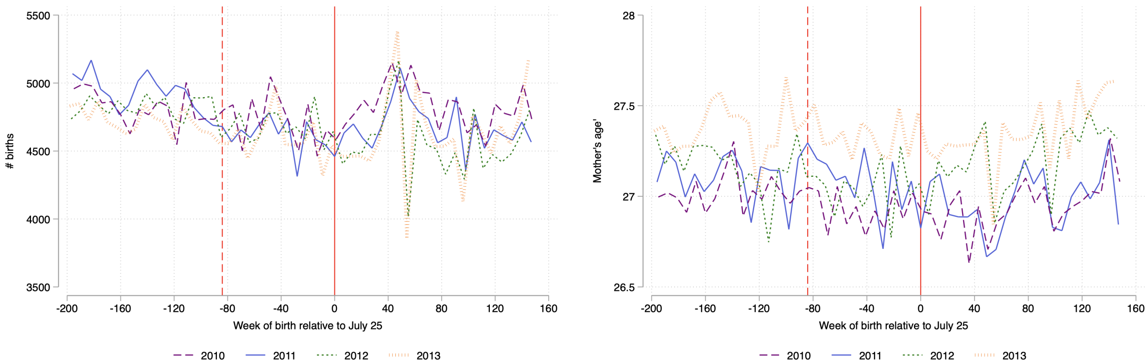
Table A.8: Effects of maternity leave reforms across countries

	Policy environment		Data	Identification	Treatment effects (se)[%]	
	Country	Policy change			Employment (pp)	Log(Earnings)
Albagli and Rau (2019)	Chile	Δ^+ ML from 3 to 6 months	Survey	Discontinuous eligibility by childbirth	5.8 pp (3.5) 1st year	-0.064 (0.056)
Vu and Glewwe (2022)	Vietnam	Δ^+ ML from 4 to 5 months	Survey	DiD for Young (25-44) vs Older (45-54) women	3.9 pp (1.3) 6 years	0.108 (0.064)
Machado et al. (2024)	Brazil	Δ^+ ML from 4 to 6 months	Admin.	Staggered DiD by firms	3.7 pp in 7 months [4%] Null after 10 months	Null effects
Liu et al. (2024)	China	14 weeks + Δ^+[10-90] days	Survey	Staggered triple diff in time, province, and gender	–1.1 pp (0.2) 1-2 years [–1.5%]	–0.002 (0.006)
This paper	Chile	Δ^+ ML from 3 to 6 months	Admin.	DiD discontinuity in eligibility by childbirth	8.3 pp (2.1) 1st year 5.9 pp (1.4) 2-3 years 0.2 pp (1.2) 4-7 years	0.018 (0.025) 0.073 (0.020) 0.069 (0.035)

Notes: This table summarizes the main findings in employment and earnings for the most recent research on maternity leave (ML) extensions in developing countries.

Figure A.1: Number of births and mother's age per week per year in Chile

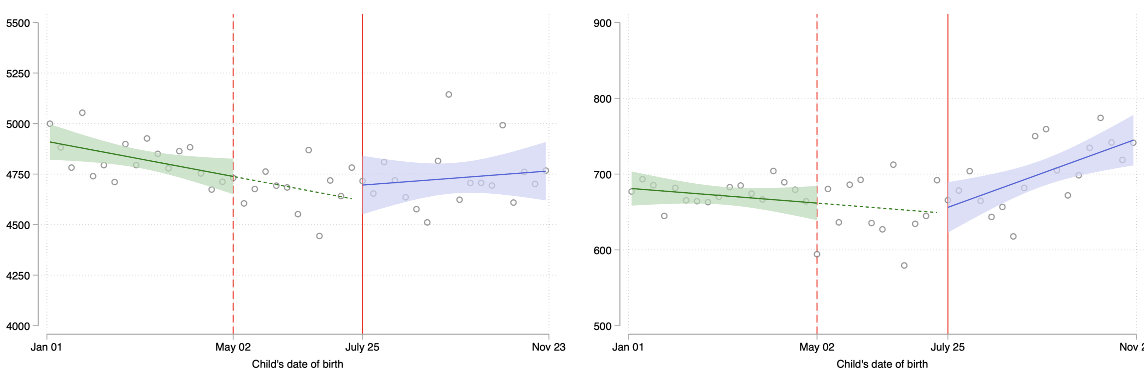
(a) Number of births | **(b)** Mother's age at birth



Notes: Panel (a) plots the total number of births, and panel (b) shows mother's average age at birth, for the full population of births in Chile between 2010 and 2013.

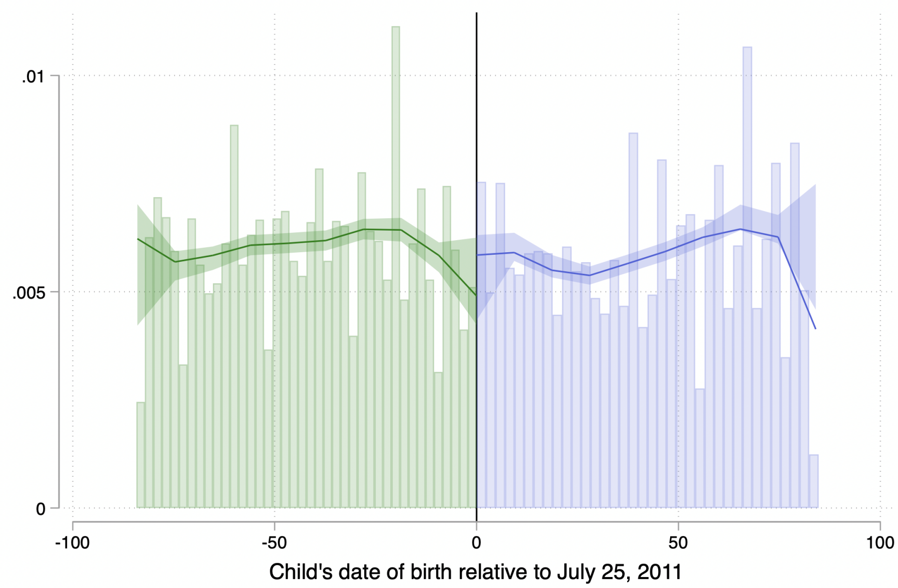
Figure A.2: Number of births and maternity leave claims around reform date

(a) Number of births | **(b)** Number of claims



Notes: Panel (a) plots the total count of births, and panel (b) shows the total number of maternity leave claims, by week of birth in Chile in 2011.

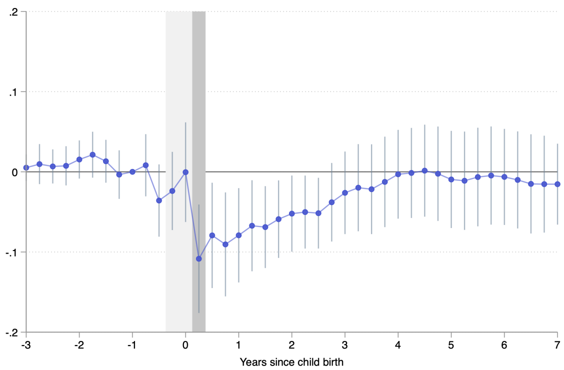
Figure A.3: Manipulation plot for McCrary test



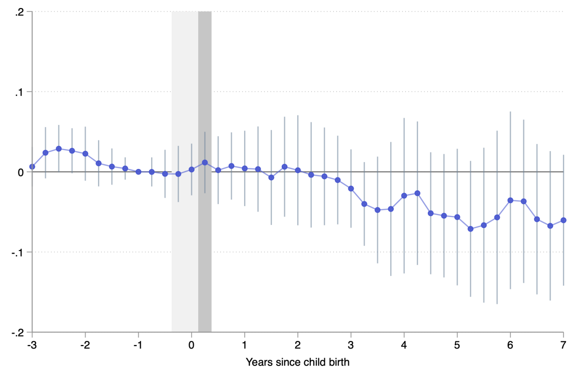
Notes: This figure plots the distribution of the running variable corresponding to the number of claims by week of birth relative to July 25, 2011. The sample to the left of the cutoff does not include observations that were partially treated. See Table A.2 for different versions of the McCrary test under different polynomial schemes before and after the cutoff (McCrary, 2008).

Figure A.4: Medium-term effects of the reform on women's labor market outcomes

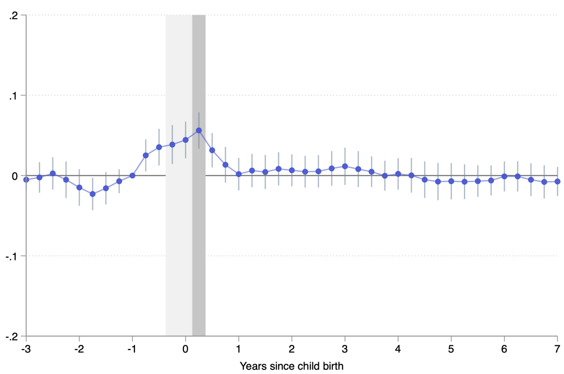
(a) Separation from employer pre-birth



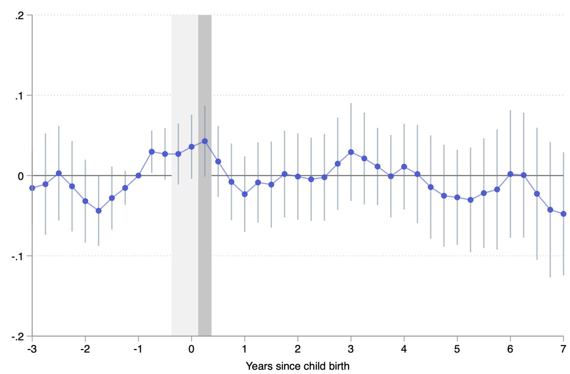
(b) Separation from employer pre-birth if employed



(c) Temporary contract



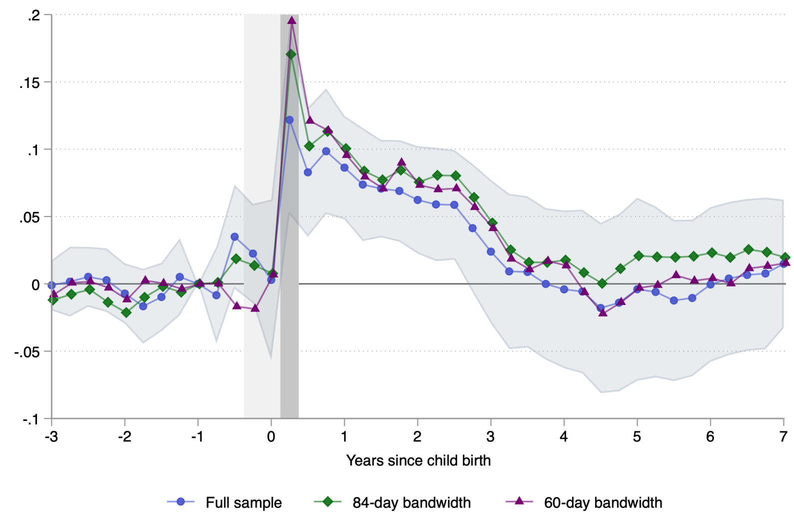
(d) Temporary contract if employed



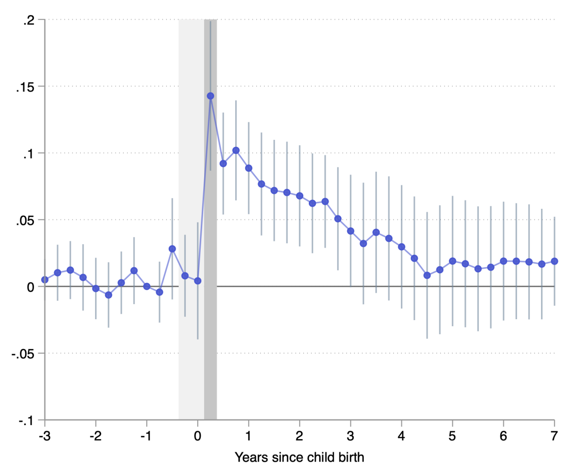
Notes: These figures show difference-in-difference estimates from equation 3 using the sample of workers in a symmetric window of 4.2 months (121 days) without the partially eligible observations. The light gray area corresponds to the time of maternity leave from -6 to 12 weeks relative to childbirth. The dark gray area corresponds to the weeks an eligible woman can extend her maternity leave from 12 to 24. Panel (a) shows the reform's effect on the likelihood that a worker is employed by a different employer than the one she had at the start of maternity leave. Panel (b) conditions on formal employment in the AFC. Panel (c) shows the reform's effect on the likelihood that a worker holds a temporary contract (versus a permanent one), and panel (d) restricts the sample to workers employed in the AFC.

Figure A.5: Robustness of employment effects to changes in bandwidth and sample

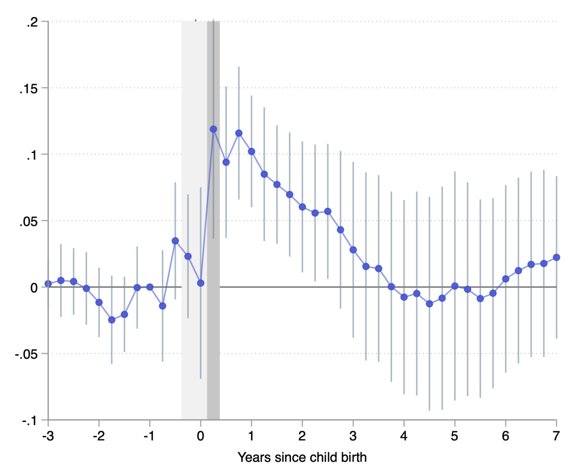
(a) Different bandwidths



(b) Sample includes partially eligible



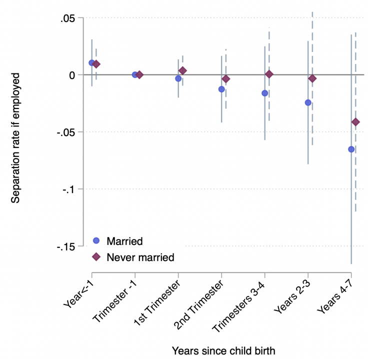
(c) Sample does not include kinks



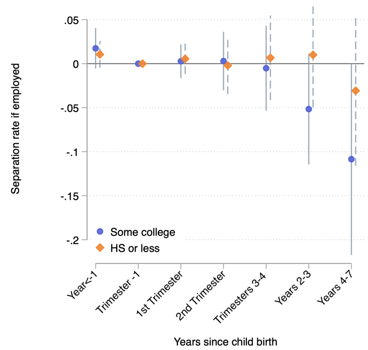
Notes: These figures show difference-in-difference estimates from equation 3 using the baseline sample of workers and their 95% confidence intervals. The light gray area corresponds to the time of maternity leave from -6 to 12 weeks relative to childbirth. The dark gray area corresponds to the weeks an eligible woman can extend her maternity leave from 12 to 24. Panel (a) varies the bandwidth, where "full sample" refers to the baseline symmetric window of 4.2 months (121 days), and 95% confidence intervals are plotted for the full sample only. Panel (b) includes partially eligible women in the sample (donut area). Panel (c) makes increases to the donut area by excluding women who filed a maternity leave claim two weeks before May 2 and two weeks after July 25.

Figure A.6: Heterogeneous effects of the reform on women's separation rate and contract type

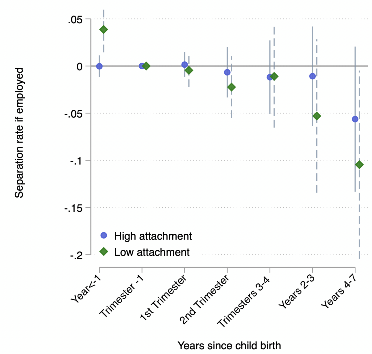
(a) Separation by marital status



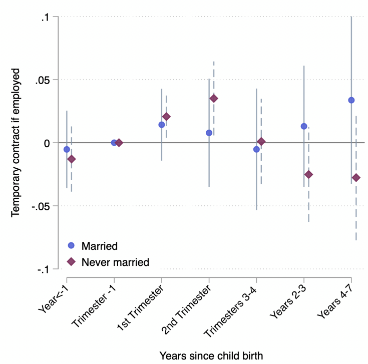
(b) Separation by education



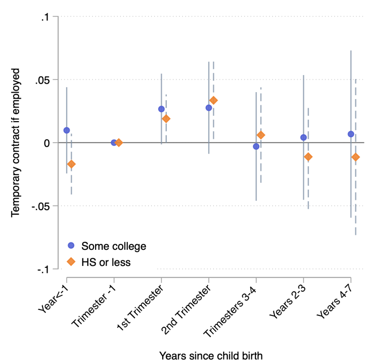
(c) Separation by attachment



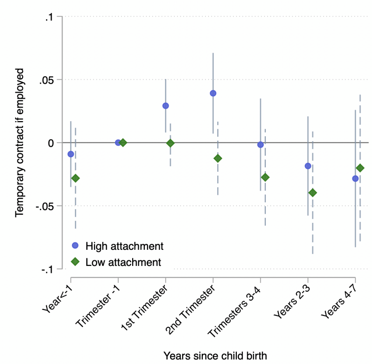
(d) Temp. contract by marital status



(e) Temp. contract by education



(f) Temp. contract by attachment



Notes: These figures plot difference-in-difference estimates of the reform's effect on separation rates and temporary contract use, stratified by workers' baseline demographics: marital status, education, and pre-birth labor market attachment. These estimates are restricted to women employed in the AFC.