# Parental Leave Policies and Fertility: the Emotional Cost of Maternal Care

Giorgia Conte and Herdis Steingrimsdottir

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**Abstract**

This paper studies how non-monetary maternal care costs influence women's fertility by exploiting a Danish reform that increased parental leave entitlement by an average of 5.3 weeks. Employing an instrumental variable strategy, we estimate heterogeneous effects on subsequent births over a three-year horizon according to the availability and employment status of women's mothers and mothers-in-law. We show that, among women whose own mother is likely available to provide childcare, extended leave significantly raises the probability of having another child in the short and medium term, particularly for first-time mothers. Conversely, for women whose own mothers and mothers-in-law are employed, deceased, or living abroad, and therefore unlikely to provide continuous support during the leave, extended leave duration is associated to a significant decline in the probability of giving birth in the short term, which does not persist in the medium term. We interpret these results as evidence that family support, particularly from the maternal grandmother, mediates the impact of longer leave on fertility by altering the non-monetary (emotional) costs of childcare. Extended leave duration can exacerbate emotional burdens if it prolongs periods of isolation for women recovering from childbirth as sole caregivers, leading postponement of future births. However, if complemented with family support, it can mitigate such non-monetary cost and encourages higher fertility.

# 1 Introduction

In a context of declining birth rates, a sizable strand of research has focused on understanding the monetary costs associated with maternal care affect fertility decisions. These include not only the direct financial burden of childcare (Bauernschuster et al., 2016; González, 2013; Haan and Wrohlich, 2011; Mörk et al., 2013)) but also the indirect costs, such as the impact of motherhood on women's careers and earnings, commonly referred to as the motherhood wage penalty (Adda et al., 2017; Goldin and Katz, 2010; Kleven et al., 2019).

However, far less is known about the role played by non-monetary costs associated with motherhood in shaping this outcome. A limited number of studies has examined how stress and psychological strains during motherhood can affect the timing of subsequent births (Margolis and Myrskylä, 2015; Myrskylä and Margolis, 2014; Zhao et al., 2024).

In this paper, we provide novel evidence on how the availability, or lack thereof of grandmaternal support in the early stages of motherhood can mediate the effect of prolonged maternal time on the probability of subsequent birth. We exploit the quasi-experimental nature of a 2002 Danish parental leave reform which increased parental leave entitlement by an average of 5.3 weeks[1]. Employing an instrumental variable strategy, we estimate heterogeneous effects on subsequent births over a three and five-year horizon according to the availability and employment status of women's mothers and mothers in law, as a proxy for family support.

The presence of grandmothers might be thought to mediate the impact of extended parental leave on fertility through two distinct channels. First, for mothers without access to informal care who would otherwise need to place their child in expensive formal childcare, longer leave substitutes for grandparental assistance by allowing them to delay external care, thereby reducing the "monetary cost of childcare"[2]. Second,

[1]Prior research has explored the reform's impact on various dimensions, including maternal health (Beuchert et al. (2016)), employment (Andersen, 2018; Tô, 2018), firms' performance and coworkers Gallen (2019)) gender norms (Lassen, 2021), and child well-being (Houmark et al., 2022).

[2]Rutigliano (2024); Thomas et al. (2022); Wang and Zhao (2022) find positive effects of presence

grandmaternal involvement can lower the non-monetary costs of maternal care—here defined as the "emotional cost of childcare"—associated with prolonged isolation as the sole caregiver. By supporting maternal well-being through childcare assistance, guidance and support during the postpartum recovery, grandmothers can enhance the extended-leave experience and encourage subsequent childbearing (Riem et al., 2023).

Evidence from Danish retirement reforms indicates that grandparents' presence and employment status positively influence women's fertility decisions (Laczek, 2024). However, in a setting with publicly available and highly subsidized formal childcare, the substitution channel may be weakened, leaving emotional support as the primary mechanism through which grandmotherly involvement shapes the fertility effects of extended leave.

A sizable strand of work has investigate the casual effects of extensions of parental leave policies on and fertility (Beuchert et al., 2016; Carneiro et al., 2015; Dahl et al., 2016; Lalive and Zweimüller, 2009; Liu and Skans, 2010). We complement it by introducing a new source of heterogeneity which sheds light on under which circumstance, extended leave duration can effectively impact fertility.

The findings of this paper support the complementarity hypothesis. Among women whose mothers and mothers-in-law are both absent or working, 100 extra days of leave reduce the probability of a subsequent birth within three years by 11 percentage points. This effect does not persist in the medium term, suggesting extended leave postpones rather than lowers overall fertility. Conversely, when at least one grandmother is likely available during the period of leave, the same leave variation increases the probability of subsequent birth in the following three years by 7 pp (the effect more than doubles in case of first-time mothers) and by 9 pp in the following five years. The positive effect appears to be driven by the presence of maternal grandmothers. In fact, for women with grandmaternal support, 100 extra days of leave rise the probability of having another child in the short term by 11 pp (23 pp for first time mothers). Generally, the estimates in the restricted sample of first-tme mothers are always less precise due to

of grandmothers as a substitute to maternal care on women fertility and labor supply the significantly smaller sample size. The positive increase in probability endures and grows to 14 pp five years after the reference birth.

The results suggest that extended leave duration can exacerbate the emotional cost of maternal care for women lacking family support during the leave by prolonging the time in isolation as sole caregiver. In response to this, women might choose to defer future birth and wait before repeating the experience. However, increased length of maternity leave can reduce the emotional cost of motherhood for women receiving assistance and guidance during the leave, particularly if from their own mother, thereby encouraging subsequent birth.

This paper proceeds as following. Section 2 presents the setting of the analysis. Section 3 details the data and the sample selection. Section 4 presents the econometric strategy. In Section 5, we report the findings of the empirical exercises with a focus on heterogeneous analysis and discusses the plausible mechanism driving the results. Section 6 concludes.

# 2 Institutional setting

## 2.1 Danish context

Traditionally, Scandinavian countries have been at the forefront of implementing generous family policies. In Denmark parental leave allowance had progressively expanded over the past decades facilitating individuals - particularly women - in the labor force in taking over families responsibilities while maintaining the attachment to the labor market. As a result, 74% of women at fertile age who is not enrolled in full time studies figures as part of the labor force[3]. Therefore, virtually the vast majority of women in Denmark are eligible to parental leave benefits because employed. Unemployed individuals are also eligible for the benefits as long as they qualify for unemployment insurance.

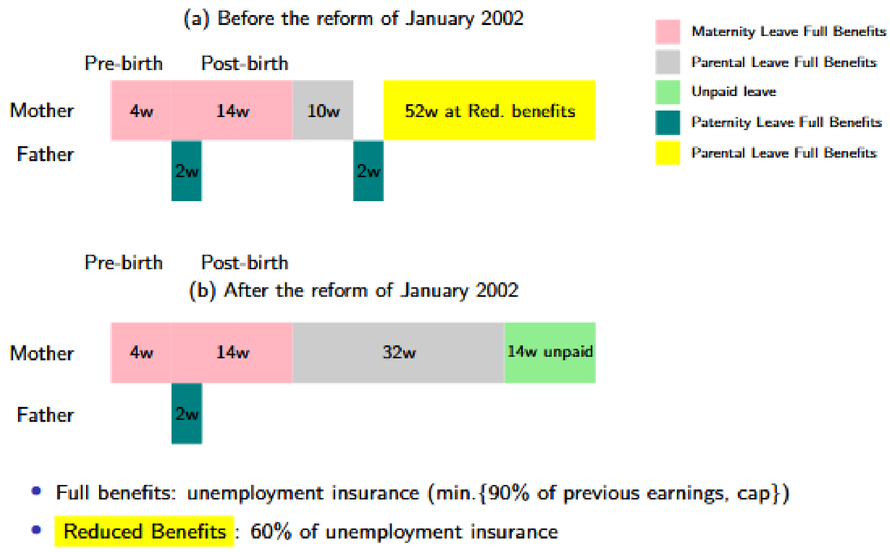
[3]We refer to fertile age as between 22 years old and 44. The share of employed women in this age group is computed using administrative data from Denmark.

## 2.2 Policy reform

The reform examined in this paper took effect in March 2002 after a swift legislative process that prevented anticipation or sorting into regimes. Under the pre-2002 system, mothers received 4 weeks of prenatal leave, followed by 14 weeks earmarked to them. Concurrently, 2 weeks were earnmarked for the other parent, followed by 10 weeks of flexible leave. All 24 weeks were paid at unemployment-insurance rates (up to 90% of earnings, averaging 66%) with some employers offering top-ups; thereafter, parents could take an additional 52 weeks at a reduced benefit (60% of the insurance rate). A snapshot of the old parental leave scheme is illustrated in **Figure 1 (a)**.

Starting from March 27, 2002, full-rate leave was extended by 22 weeks (raising the paid entitlement from 24 to 46 weeks) and the subsequent 52 weeks of reduced-rate leave were eliminated. Paternity leave was cut from 4 to 2 weeks. the new rules applied automatically to children born on or after March 27; parents of children born between January 1 and March 27 could opt for either regime. Finally, the reform also added flexibility in shared leave—allowing simultaneous or part-time leave and deferral until the child's eighth birthday—and permitted each parent to take up to 14 weeks of job-protected unpaid leave beyond the 48 weeks. Overall, the new leave scheme increased the entitlement of leave compensated at full benefits but shortened the total span of paid entitlement. **Figure 1 (b)** outlines these policy changes.

Figure 1: Parental leave schemes before and after the 2002 reform




# 3 Data and sample selection

## 3.1 Data

Our analysis is based on data from the Danish Registers of Population (BEF), income (IDAN), and education (UDDA), which provide detailed information on demographic characteristics, income, and education, respectively. We measure all control variables in 2001, to rule out potential interference of the reform with their values. By linking individuals' social security numbers, we access data from the DREAM-Register, which includes various government transfers to Danish citizens, including parental leave benefits. We compute individual parental leave duration by counting the number of weeks each individual receives either transfers from the government at full benefits compensation, *Barselsdagpenge*, or reduced benefits, *Orlov*, within a set number of months post-birth. A detailed description on how leave take-up is measured is provided in the Appendix.

## 3.2 Sample selection

Following Lassen (2021), we exclude twin births from the analysis. Moreover, consistently with Beuchert et al. (2016), we also exclude women who are unemployed and thus ineligible for parental leave because they lack unemployment-insurance coverage. Since the first two weeks of postnatal leave are mandatory for eligible mothers, they receive a government transfer during that period. We therefore classify as ineligible any woman who does not receive a transfer in the two weeks following her child's birth. No comparable mandatory transfer exists for fathers, so we assume all employed men in our sample are eligible.

Since parents giving birth between January 1st and March 26th could choose which parental leave scheme would have applied to them, we designate January 1st, 2002, as the pivotal cutoff for the new policy implementation. Notably, **Figure 2** illustrates the average leave duration for women in the six months before and after January 1st, 2002. The figure shows that most women in our sample opt for the post-reform entitlements as leave uptake presents a clear discontinuity around the date of January 1st while the pattern is stable around the end of March. In contrast, **Figure 3** shows that the reform had little overall effect on fathers' leave duration, though there is a slight decrease in leave days taken by fathers of children born after the reform. This is plausibly the result of the removal of two weeks of paternity leave. Consequently, we classify women who gave birth before the cutoff date as the control group and those who gave birth after the cutoff date as the treatment group (following Beuchert et al. (2016); Houmark et al. (2022); Lassen (2021); Tô (2018)).

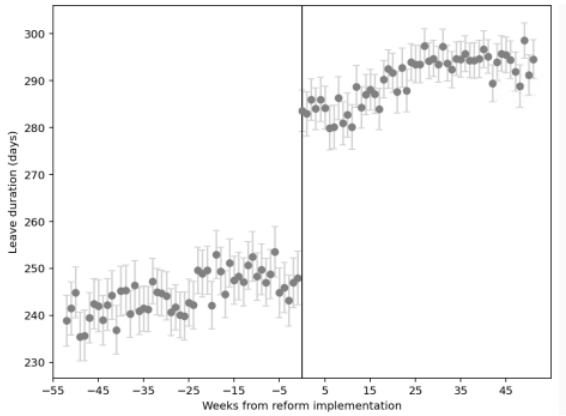



Figure 2: Discontinuity of maternity leave take-up observed over six months.

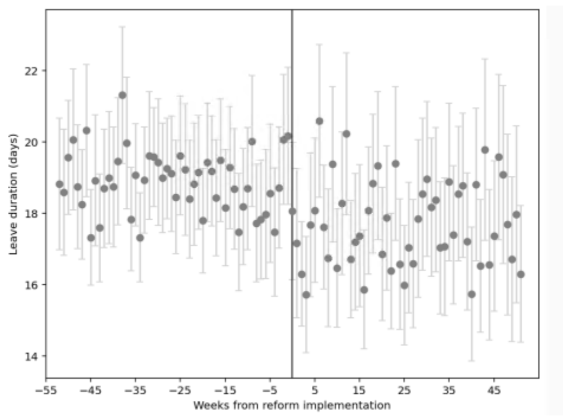



Figure 3: Discontinuity of paternity leave take-up observed over a six months window.

To limit bias due to seasonality, we restrict the sample to births occurring within a defined time window around January 1st, 2002. Specifically, for the analysis concerning the effect of the leave duration on fertility, the sample includes births from 8 weeks before to 8 weeks after the reform, excluding the week immediately preceding and the week immediately following the policy intervention[4]. This approach ensures that the analysis focuses on births occurring in similar seasonal conditions while limiting the possibility of manipulating the date of birth of the child to be eligible for the new PL scheme. After applying these selection criteria, the final sample comprises 13,452 births, with 6,406 in the control group and 6,893 in the treatment group[5].

## 3.3 Summary statistics

The summary statistics of the explanatory variables are presented in a **Table 1** which reports mean and standard deviation for the mothers and fathers within the reform window, alongside the results of a T-test for statistical significance in the difference between values in different subgroups. These statistics are initially provided for the entire sample, regardless of the birth order. Additionally, recognizing that first-time

[4]Therefore, we include in the sample birth happening in week 44 to 51 of 2001 and 2 to 9 of 2002

[5]The sample size is smaller that the one identified by Beuchert et al. (2016) who analyzes the same individuals using a different dataset to compute leave take-up (*dagpenge*)). The initial sample reduced when merging children born within the time range considered with data on their mother, then merging the resulting dataframe with DREAM, selecting women with positive leave take-up and income, as well as having partners reporting positive income, and finally selecting non-twin children who are still resident in Denmark in 2008 because of our decisions to exclude births of twins and our slightly smaller time window (56 days versus 60 days)

mothers may be particularly responsive to the reform, we report the summary statistics for women who gave birth to their first child within this reform window.

Table 1: Summary Statistics, entire sample

| Group | PL take-up mother | PL take-up father | PL take-up total | Age mother | N children | Years of education (mother) | Single | Danish Native | Maternal/ paternal grandmother not working | Maternal grandmother not working | Paternal grandmother not working | Age father | Years of education (father) | Earnings (mother) | Earnings (father) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Control** | | | | | | | | | | | | | | | |
| Mean | 247.52 | 18.55 | 266.79 | 30.44 | 1.76 | 13.45 | 0.06 | 0.98 | 0.52 | 0.31 | 0.32 | 32.82 | 13.14 | 204146 | 289633 |
| St. dev. | 76.56 | 23.98 | 77.01 | 0.47 | 0.84 | 2.60 | 0.00 | 0.00 | 0.01 | 0.06 | 0.01 | 0.07 | 0.04 | 2187 | 152370 |
| Sample size | 6406 | | | | | | | | | | | | | | |
| **Treated** | | | | | | | | | | | | | | | |
| Mean | 283.62 | 17.51 | 300.98 | 31.29 | 1.8 | 13.44 | 0.05 | 0.99 | 0.53 | 0.06 | 0.31 | 33.60 | 13.15 | 209165 | 290986 |
| St. dev. | 68.76 | 26.43 | 66.31 | 0.47 | 0.84 | 2.61 | 0.49 | 0.00 | 0.01 | 0.06 | 0.01 | 0.64 | 0.04 | 2063 | 149135 |
| Sample size | 6893 | | | | | | | | | | | | | | |
| Difference | -36.1 | 1.04 | -34.19 | -0.86 | -0.33 | 0.01 | 0.00 | -0.01 | -0.00 | -0.00 | -0.45 | -0.79 | -0.01 | -5019 | -1353 |
| Mean (all) | 266.23 | 18.02 | 284.47 | 30.88 | 1.78 | 13.44 | 0.05 | 0.99 | 0.52 | 0.31 | 0.32 | 33.23 | 13.15 | 206696 | 290334 |
| t-statistic | -28.64 | 2.23 | -25.35 | -11.15 | -2.24 | 0.23 | 3.36 | -33.6 | -1.07 | -0.26 | -0.48 | -8.43 | -0.26 | -2.77 | -0.45 |
| p-value | 0.00*** | 0.03** | 0.00*** | 0.52 | 0.02** | 0.82 | 0.00*** | 0.56 | 0.29 | 0.80 | 0.63 | 0.00*** | 0.79 | 0.01** | 0.45 |

Table 2: Summary Statistics, restricted sample to first-time mothers

| Group | PL take-up mother | PL take-up father | PL take-up total | Age mother | Years of education (mother) | Single | Danish Native | Maternal/ paternal grand. not working | Maternal grand. not working | Paternal grand. not working | Age father | Years of education (father) | Earnings (mother) | Earnings (father) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Control** | | | | | | | | | | | | | | |
| Mean | 243.70 | 19.25 | 263.37 | 28.71 | 13.55 | 0.07 | 0.99 | 0.43 | 0.27 | 0.25 | 31.04 | 13.14 | 206981 | 269382 |
| St. dev. | 74.84 | 24.58 | 76.02 | 4.18 | 2.58 | 0.50 | 0.26 | 0.11 | 0.50 | 0.44 | 0.43 | 2.64 | 102303 | 143921 |
| Sample size | 2875 | | | | | | | | | | | | | |
| **Treated** | | | | | | | | | | | | | | |
| Mean | 281.08 | 18.21 | 298.40 | 29.20 | 13.49 | 0.06 | 0.99 | 0.45 | 0.26 | 0.26 | 31.82 | 13.12 | 213150 | 277260 |
| St. dev. | 69.02 | 26.94 | 67.68 | 4.25 | 2.59 | 0.23 | 0.10 | 0.50 | 0.44 | 0.44 | 4.61 | 2.62 | 95343 | 140432 |
| Sample size | 2914 | | | | | | | | | | | | | |
| Difference | -36.1 | 1.04 | -34.19 | -0.88 | -0.06 | 0.01 | -0.00 | -0.02 | 0.01 | -0.01 | -0.79 | 0.03 | -6169 | -7878 |
| Mean (all) | 266.23 | 18.02 | 284.47 | 29.15 | 13.52 | 0.06 | 0.99 | 0.44 | 0.26 | 0.25 | 31.43 | 13.13 | 210004 | 273275 |
| T-statistic | -16.62 | 1.37 | -20.55 | -7.95 | 0.95 | 2.06 | -0.95 | -1.24 | 0.48 | -0.97 | -5.71 | 0.35 | -2.24 | -1.78 |
| P-value | 0.00*** | 0.17 | 0.00*** | 0.00*** | 0.34 | 0.04** | 0.34 | 0.22 | 0.62 | 0.33 | 0.00*** | 0.73 | 0.03** | 0.07* |

**Table 1** highlights a significant discontinuity in leave take-up between parents giving birth right before and right after the reform shown in **Figure 2** and **Figure3** but zooming within the narrow window of 8 weeks preceding and following the reform. Specifically, within this time frame, women eligible for the new PL scheme take on

average 36 more days (over 5 weeks) of leave than women who gave birth just before the reform (248 versus 284 days). On the other hand, plausibly in response to the abolition of two weeks of unmarked paternity leave dictated by the reform, men in the treatment group reduce their leave uptake by 1 day on average, shifting from an average of 18.5 to an average of 17.5 days of leave. These differences are statistically significant at the 1% level for women and at the 5% level for men. Narrowing down on first-time mothers, the reform seems to increase maternal average leave take-up by 37 days. These differences are significant at the 1% level, while the average decrease by one day in leave uptake by fathers in this sample is not statistically significant.

The analysis of the sample of women at all parities (i.e. the full sample) reported in **Table 1** shows that, on average, women in the control group have slightly fewer children, including the reference child born in the reform window, compared to women in the treatment group (1.76 vs. 1.80 children, respectively). Additionally, the proportion of single women is slightly higher in the control group (6%) compared to the treatment group (5%). Women in the control group are also more likely to be first-time mothers (45% vs. 42%), earn 5,019 DKK less per year (approximately 673 euros) and have partners earning 1,353 DKK less per year (about 181 euros).

Table 2 shows that first-time mothers in the control group tend to be slightly younger, with an average age of 28.7 years compared to 29.2 years in the treatment group. They are also more likely to be cohabiting with a partner (7% vs. 6%). Fathers in the control group are also slightly younger than those in the treatment group (the average age is 31 versus 31.8). Moreover, first-time mothers in the control group earn, on average, 6,169 DKK less per year (approximately 827 euros), and their partners earn 7,671 DKK less per year (about 1,056 euros) compared to the partners of the treatment group.

While the summary statistics reveal some differences in covariates between the treatment and control groups, the small magnitudes of these differences support the validity of the overall research design. The increase in maternal leave duration across different subgroups is reported in **Table 3**, with a distinction between subgroups in the whole sample and in the sample restricting to first-time mothers. Generally, first-time mothers present a larger increase in leave take-up following the reform, with the exception of public sector employees who are entitled to more generous paid leaves than average already before the reform.

Table 3: Increase in leave take-up by subgroup

| | **Full Sample** | **First-Time Mothers** |
|---|---|---|
| Average | 43.88 | 52.16 |
| Maternal and paternal grandmother absent | 47.26 | 52.8 |
| Maternal/ paternal grandmother present | 39.49 | 48.85 |
| Maternal grandmother absent | 42.11 | 53.22 |
| Maternal grandmother present | 36.80 | 45.14 |
| Paternal grandmother absent | 46.4 | 55.9 |
| Paternal grandmother present | 37.1 | 39.15 |

Following the reform, parental leave increased on average by 44 days, with a larger increase of 52 days observed in the restricted sample of first-time mothers. Examining various subgroups reveals that women without access to informal childcare options took fuller advantage of the reform by extending their leave more significantly. Specifically, women whose mother and mother-in-law were both absent (either because deceased, missing or employed) increased their leave by an average of 47 days, rising to 53 days for first-time mothers in this group. Women with an absent mother extended their leave by 42 days on average, or 53 days if they were first-time mothers. Similarly, women whose mother-in-law was unavailable increased their leave by 46 days, and by 56 days for first-time mothers in this category.

By contrast, women with a present mother increased their leave by 42 days, with first-time mothers in this group showing an identical increase of 53 days. If either mother of mother-in-law is more likely to be available, rise their leave take-up by 39.5 days, and 49 is first time mothers. Finally, women whose mother-in-law is more likely to be available (unconditionally on the availability of their own mother) prolong their leave by 37 days on average compared to the control group, and 39 if first-time mothers. This pattern highlights that women lacking informal childcare options tended to take greater advantage of the reform, significantly extending their leave duration compared to those with access to support from their mother or mother-in-law.

# 4 Empirical Strategy

The empirical strategy implemented here aims to estimate the Local Average Treatment Effect (LATE) of the reform. We rely on an instrumental variable (IV) approach that exploits the discontinuity in leave-taking behavior induced by the implementation of the 2002 reform. Women who gave birth before the policy cutoff date, and were therefore ineligible for the extended leave scheme, serve as the control group. Those who gave birth after the cutoff date compose the treatment group.

We estimate a two-stage least squares (2SLS) regression. In the first stage, we examine how the timing of birth—specifically its proximity to the reform cutoff—affects the likelihood of taking extended leave. In the second stage, we regress an indicator for whether a subsequent birth occurred within 36 or 60 months of the reference birth on the predicted leave duration. In doing so, we investigate whether increased maternity leave duration causally affects the probability of having another child within a given time horizon.

## 4.1 Identifying assumptions

The identification strategy enables causal inference, provided a set of assumptions is met. Specifically, these include the exclusion restriction, the monotonicity assumption, and the independence assumption.

First, the exclusion restriction requires that no other underlying event, apart from the reform, affects the outcome variables. While the exclusion restriction cannot be tested directly, we argue that our setting satisfies this condition. Since no other policy change coincided with the cutoff analyzed, there is no reason to believe that changes in fertility decisions or maternal labor market outcomes for parents of children born within a narrow window around the cutoff could be attributed to any factor other than the reform. This claim is further supported by a placebo test that estimates the effects of leave duration on the probability of future births within the same 16-week window in 2001, a period when leave eligibility was unaffected by the timing of birth (See **Appendix B**).

The monotonicity assumption implies that no woman would reduce her leave take-up because of the reform (i.e., ruling out the presence of defiers). This assumption is potentially concerning, as the reform extended the duration of generously paid leave but removed the option for leave at lower compensation. However, this risk is mitigated by the fact that women in the sample could choose the parental leave scheme that applied to them, and the 88% opted for the post-reform regime[6].

Finally, the independence assumption requires that the assignment of treatment be as good as random. The limited time-frame between the reform's announcement and implementation ensured that its unexpected nature prevented self-selection into the treatment group. As noted by Beuchert et al. (2016), the primary risk of manipulation could stem from planned C-sections, which could be postponed by up to one week for women expecting to give birth in late December. To address this, we exclude observations corresponding to the weeks immediately before and after the reform, as detailed in Section 3, thereby mitigating this potential source of bias.

[6]Concerns about potential violations of monotonicity also inform our choice not to include paternal leave in the construction of the endogenous variable. Descriptive evidence suggests that in some households, fathers' leave take-up declined due to the reform's reduction in earmarked paternity leave, which could confound the direction of the treatment effect.

## 4.2 Two-stage-least-square regression

In the first stage of the 2SLS we regress leave duration (denoted as *Leave*) against a binary variable $T_i$, which indicates if a child's birth date ($d_i$) falls before the January 1st, 2002 cutoff ($d_0$) (control group) or after (treatment group). Here, $T$ is assigned a value of 1 when $d_i - d_0 \geq 0$ and 0 otherwise.

$$T_i = 1[d_i \geq d_0] \tag{1}$$

In the first stage regression we include a linear measure of distance $\delta_t$ between the date of birth and the cutoff as well as the distance interacted with a dummy distinguishing between childbirth occurring before or after the cutoff.

*First stage*:

$$Leave_i = \alpha_0 + \alpha_1 T_i + \alpha_2 \delta_i + \alpha_3 \delta_{i,g} T_i + \eta \tag{2}$$

The regression model reported in equation 2 consists of a linear probability model which quantifies the effect of the reform on the probability of a specific event. Depending on whether the child was born before or after the cut-off date of January 1st 2002, their parents would be subject to different leave entitlement for the reference child. Consistently with a 2SLS, we regress $Leave_i$ on $T_i$ and include a flexible function of distance $\delta_i$ of birth from the cutoff. Having selected children born in the same season, we reduced the risk of bias caused by biological factor determining the success of future pregnancies, in line with previous work (Houmark et al., 2022).

*Second stage*:

$$y_i = \beta_0 + \beta_1 Leave_i + \beta_2 T_i + \beta_3 \delta_i + \beta_4 \delta_i T_i + \beta_5 X_i + \epsilon \tag{3}$$

In equation 3, $y_i$ identifies the outcome variable, probability of giving birth again.

The outcome variable consists of a binary variable identifying the birth of an additional child within 36 and 60 moths on the variable *Leave*, instrumented by the post-reform dummy $T$. To estimate the effect of longer leave on the probability of having another child, we employ a model controlling for different combinations of parental characteristics, $X$. The covariates include a second order polynomial for age, an indicator for the number of children ever born, an indicator for her years of completed education, a binary variable indicating whether she cohabits with a partner, and a variable indicating the logarithm of her earnings in the previous year.

Along with the set of variables indicating maternal characteristics, we include another set providing information on paternal characteristics: age dummies, an indicator for the father's years of completed education and the logarithm of paternal earnings over the previous year. The analysis starts by considering the entire sample of births within the chosen window regardless of the birth parity of the reference child (therefore parities 1+). A subsequent analysis focuses of first time mothers, i.e. the effect of the reform on women at parity 1.

#### 4.2.1 Main analysis: heterogeneity by availability of grandmother

We begin by estimating a baseline model that regresses the probability of having another child within a specified time frame on maternal leave take-up. Next, we conduct a heterogeneity analysis by interacting the instrumented maternity-leave duration variable with indicators of family support availability. To assess whether the effect of extended leave differs depending on the availability of family support, we include an interaction between the endogenous leave-duration variable and subgroup indicators. Each indicator distinguishes newborns whose grandmother was more likely to be available (alive and not-working) from those whose grandmother was unlikely to be available (deceased, living abroad, or working) during the leave. We further differentiate maternal grandmothers, paternal grandmothers, and the case of at least one versus neither grandmother available, using employment status recorded in November 2001. This binary subgroup variable is denoted *Group*.

Equation 4 exemplifies the first stage of the 2SLS accounting for heterogeneous effects. In the first stage, both the variable *Leave* indicating the duration on maternity leave and the interaction between *Leave* and the dummy identifying a subgroup-specific characteristic are endogenous and therefore regressed on the instrument $T$.

$$Leave_i \times Group = \gamma_0 + \gamma_1 T_i \times Group + \gamma_2 \delta_i \times Group + \gamma_3 \delta_i T_i \times Group + \eta \tag{4}$$

These interactions between *Leave* and the subgroup specific indicators *Group* are themselves instrumented by the interaction between *Group* and the dummy identifying the treatment and control group ($T$), as well as the same linear measure of distance between birth and cutoff $\delta_i$ and the interaction between the three variables.

# 5 Results

## 5.1 The effect of extending maternity leave of the probability of future births

The results of the instrumental variable estimation are presented in **Table 6** for the full sample of births and in **Table 7** for first-time mothers. Each column reports the coefficients of the interaction between maternity leave uptake and group-specific indicators, such as childcare provided by the maternal grandmother, paternal grandmother, or either grandmother. The tables separately display the coefficients for the baseline group and the interaction terms.

The effects are normalized such as to refer to 100 extra days of leave. For instance, for women whose mother is more likely to be available during the leave, the effect of increasing maternity leave uptake by one day on the probability of giving birth to another child within 36 months is calculated as the sum of two components: the baseline effect for women whose mother is unavailable (-5.5 pp) and the difference in effects between women with unavailable and available mothers (16.3 pp), resulting in a total effect of 11 pp.

As outlined in Section 3, the increase in maternity leave duration due to the reform ranges from 37 days to over 56 days, depending on the subgroup analyzed. To accurately compute the total increase in the probability of a future birth, one must multiply the coefficients of the baseline subgroups and interaction terms by the average subgroup-specific increase in leave uptake reported in **Table 3**.

### 5.1.1 Baseline effects

We begin by presenting the baseline results on how the policy affected fertility decisions for women impacted by the reform compared to those who were not. The empirical analysis reported by **Table 4** shows that, overall, the reform does not significantly affect the likelihood of giving birth to another child within 36 months since the reference birth. However, in the 60 months following the reform's implementation (presented in **Table 5**), there is a reduction in the probability of having another child by 5 percentage points, with a stronger effect observed among first-time mothers, where the likelihood decreases by an additional percentage point, although in this case the effects is not statistically significant. A potential explanation for the small increase in the probability of subsequent births within 36 months could be attributed to an income effect, as women giving birth after the reform benefit from more generous compensation for the extended 22 weeks of leave.

Table 4: Regression Outputs: probability of giving birth to another child, 3 years later

| | Full Sample | Restricted Sample - First-Time Mothers |
|---|---|---|
| **Leave Duration** | -0.009 | 0.010 |
| | (0.023) | (0.044) |
| **F-stat** | 181.10 | 81.33 |
| **R-squared** | 0.187 | 0.052 |
| **Observations** | 8,735 | 3,654 |

*Standard errors in parentheses*

** p < 0.1, * p < 0.05, ** p < 0.01*

Table 5: Regression Outputs: Probability of giving birth to another child, 5 years later

| | Full Sample | Restricted Sample - First-Time Mothers |
|---|---|---|
| **Leave Duration** | 0.051** | 0.060 |
| | (0.027) | (0.037) |
| **F-stat** | 420.17 | 81.25 |
| **R-squared** | 0.359 | 0.121 |
| **Observations** | 8,735 | 3,654 |

*Standard errors in parentheses*

** p < 0.1, ** p < 0.05, *** p < 0.01*

#### 5.1.2 Main results: heterogeneous effect by availability of grandmothers

**Effects of 100 extra days of leave on short-term probability of subsequent birth (within 36 months)**

The main analysis distinguishes the roles of maternal and paternal grandmothers in mediating the reform's effects on subsequent births; the results are reported in **Tables 6–9**.

Column (1) of **Table 6** presents heterogeneity by availability of family support. For women whose mothers were unlikely to be available during leave, 100 extra days reduce the probability of a second birth within 36 months by just over 5 percentage points (statistically significant at the 10% level) in the full sample. This decline is not statistically significant among first-time mothers (Column (1), **Table 7**). By contrast, when the maternal grandmother is likely to be available, each additional 100 days raises the short-term birth probability by 11 percentage points (statistically significant at the 5% level), and by 23 pp for first-time mothers (statistically significant at the 5% level; Column (1), **Table 7**). The difference between the effects of extended leave in presence of a maternal-grandmother and the effect in her absence is significant at the 1% level in the full sample and at the 5% level for first-time mothers.

Column (2) of **Table 6** shows that when both grandmothers are unavailable (deceased, abroad, or employed), 100 extra days cut the three-year birth probability by 11 percentage points (statistically significant at the 5% level). Restricting to first-time mothers yields an 11.5 point reduction (Column (2), **Table 7**) also statistically significant at the 5% level. Conversely, if at least one grandmother is living and not working, 100 extra days increase the probability of short-term subsequent birth by 7 percentage points (statistically significant at the 5% level), rising to 15 pp for first-time mothers (statistically significant at the 5% level; Column (2), **Table 7**). These differences between the effects are significant at the 1% level in both samples.

If the mother-in-law is unlikely to be available, 100 extra days reduce the birth probability by 4 percentage points overall and by 5 pp for first-time mothers (Columns (3) of **Tables 6** and **7**), but neither effect is significant. In contrast, if the mother-in-law is likely to be available, the probability rises by 5 pp, but the change is again not statistically significant. The difference between the effects in these subgroups is marginally significant (at the 10% level). Comparing columns (1) and column (3) of

**Table 6**), it appears that extended leave's positive impact on fertility presented in Column (2) of **Table 6** is driven chiefly by the maternal grandmother.

Table 6: Regressions Outputs, full sample, 3 years later

| | Maternal grand. | Either grand. | Paternal grand. |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Dependent Variable: of giving birth 3 years later** | | | |
| **Unavailable** | -0.0548* | -0.108*** | -0.0461 |
| | (0.028) | (-0.038) | (0.0307) |
| **Available** | 0.108** | 0.069** | 0.047 |
| | (0.042) | (0.030) | (0.037) |
| **Difference** | 0.163*** | 0.177*** | 0.0932* |
| | (0.051) | (0.049) | (0.048) |
| **F-stat** | 77.707 | 87.365 | 84.836 |
| **R-squared** | 0.172 | 0.167 | 0.180 |
| **Observations** | 8,735 | 8,735 | 8,735 |

*Standard errors in parentheses*

** p < 0.1, ** p < 0.05, *** p < 0.01*

Table 7: Regression Outputs, first time mothers, 3 years later

| | Maternal grand. | Either grand. | Paternal grand. |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Dependent Variable: Probability of giving birth 3 years later** | | | |
| **Unavailable** | -0.0384 | -0.115* | -0.0508 |
| | (0.048) | (0.060) | (0.052) |
| **Available** | 0.226** | 0.152** | 0.140* |
| | (0.105) | (0.066) | (0.084) |
| **Difference** | 0.264** | 0.267*** | 0.191* |
| | (0.116) | (0.089) | 0.099) |
| **F-stat** | 29.41 | 38.74 | 35.27 |
| **R-squared** | 0.027 | 0.017 | 0.032 |
| **Observations** | 3,654 | 3,654 | 3,654 |

*Standard errors in parentheses*

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Effects of 100 extra days of leave on medium-term probability of subsequent birth (within 60 months)**

Column (1) of **Table 8** presents the medium-term impacts of extended leave on subsequent births up to five years post-reform. For women whose mothers were unlikely to be available, the coefficient on leave duration is effectively zero and not statistically significant. By contrast, when the maternal grandmother is alive and not employed, an additional 100 days of leave increase the five-year birth probability by 14 percentage points (statistically significant at the 1% level), with the subgroup difference significant at the 5% level. Among first-time mothers in the latter group, the effect rises to 18 pp (statistically significant at the 10% level; see Column (1) **Table 9**), although the interaction between subgroups is not statistically significant in the subsample of first time mothers.

Women with at least one living, non-working grandmother—whether maternal or paternal—also experience a significant 9 percentage-point increase in the medium-term probability of subsequent birth (statistically significant at the 10% level), which grows to 11 pp for first-time mothers (statistically significant at the 10% level; Column (2) of **Table 9**). Conversely, for women with neither grandmother available, the previously observed negative short-term effect (reported in Column (2) of **Table 6**) vanishes five years after the reference birth: Column (2) of **Table 8** shows an insignificant coefficient, suggesting that extended leave duration leads to a postponement rather than an overall reduction of fertility. Therefore, although the short-term effects of extended leave on the probability of future births differ significantly between women more likely to receive support from either grandmother and those who are not, we cannot rule out that the two subgroups exhibit the same medium-term responses.

Finally, when analyzing the effects of leave extension on probability of subsequent birth depending on the availability of the paternal grandmother, independently on the presence of maternal grandmother, the results presented in Column (3) of **Table 8** show a positive but statistically insignificant effect for the full sample. Women whose mother-in-law is more likely to be available experience an 8 percentage point increase in the probability of giving birth within 5 years, compared to a 3 pp increase for women whose mother-in-law is unlikely available. Among first-time mothers whose mother-in-law is more likely available for support, longer leave are associated with an increase in the probability of giving birth within 5 years by 17 pp (see Column (3) of **Table 9**). In contrast, for first-time mothers whose mother-in-law is unlikely to be available, the reform results in a smaller, statistically insignificant 9 percentage point increase. Even for this group the difference in effects is not statistically significant. Even in this case, the difference between the interaction terms is not statistically significant.

Table 8: Regression Outputs for full sample, 5 yeas later

| | Maternal grand. | Either grand. | Paternal grand. |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Dependent Variable: Probability of giving birth 5 years later** | | | |
| **Unavailable** | 0.0159 | 0.00227 | 0.0317 |
| | (0.032) | (0.042) | (0.035) |
| **Available** | 0.137*** | 0.089*** | 0.083 |
| | (0.046) | (0.032) | (0.039) |
| **Difference** | 0.121** | 0.0863 | 0.0509 |
| | (0.060) | (0.055) | (0.056) |
| **F-stat** | 194.66 | 193.06 | 197.97 |
| **R-squared** | 0.352 | 0.356 | 0.358 |
| **Observations** | 8,735 | 8,735 | 8,735 |

*Standard errors in parentheses*

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

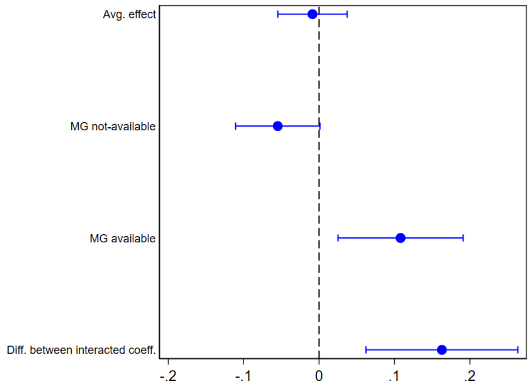
Table 9: Regression Outputs for Restricted Sample, 5 Years Later

| | Maternal grand. | Either grand. | Paternal grand. |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Dependent Variable: Probability of giving birth 5 years later** | | | |
| **Unavailable** | 0.0390 | 0.0231 | 0.0924 |
| | (0.039) | (0.048) | (0.057) |
| **Available** | 0.178* | 0.106* | 0.166** |
| | (0.096) | (0.057) | (0.083) |
| **Difference** | 0.139 | 0.0825 | 0.0734 |
| | (0.104) | 0.075) | (0.087) |
| **F-stat** | 29.07 | 38.74 | 22.24 |
| **R-squared** | 0.109 | 0.116 | 0.091 |
| **Observations** | 3,654 | 3,654 | 3,654 |

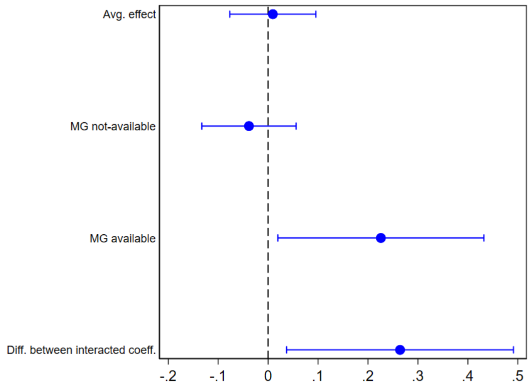
*Standard errors in parentheses*

** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$*

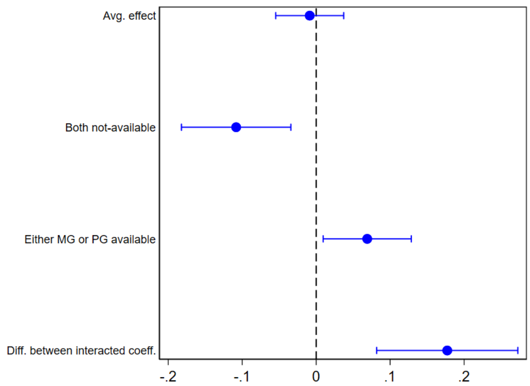
(a) Heterogeneity by availability MG -full sample, 3 yeas later



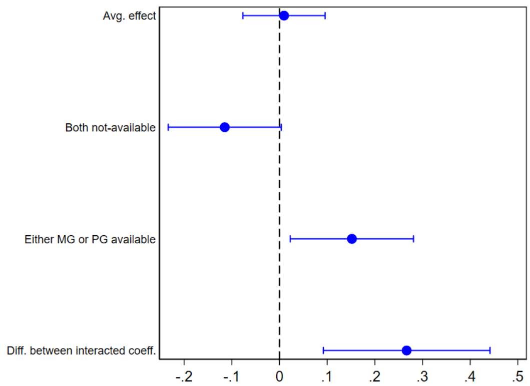

(b) Heterogeneity by availability of MG - - restricted sample (first time mothers)



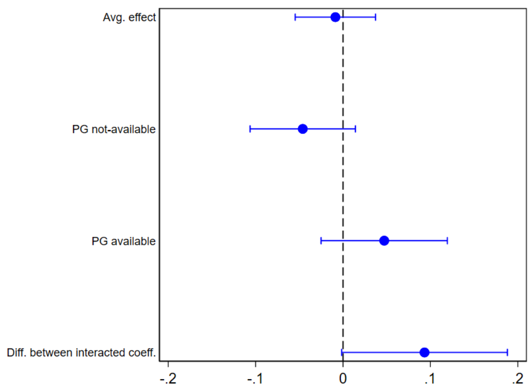

(c) Heterogeneity by availability of either G - full sample, 3 yeas later




(d) Heterogeneity by availability of G -restricted sample (first time mothers), 3 yeas later




(e) Heterogeneity by availability of PG -full sample, 3 yeas later




(f) Heterogeneity by availability of PG - restricted sample (first time mothers), 3 yeas later

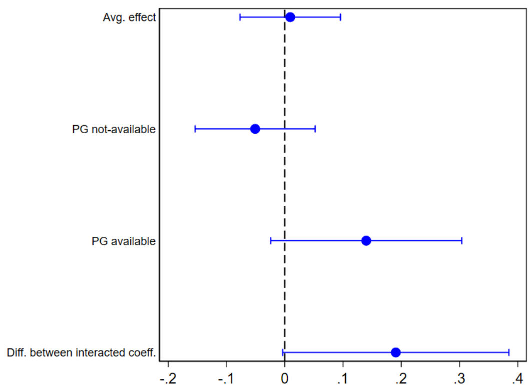
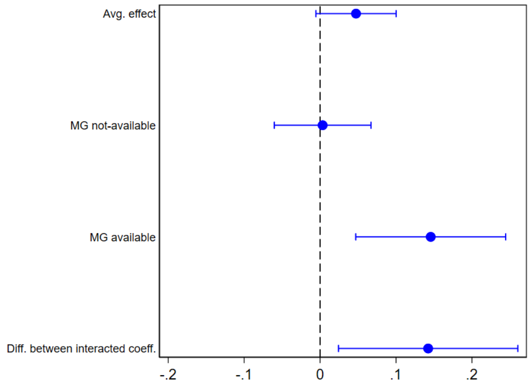
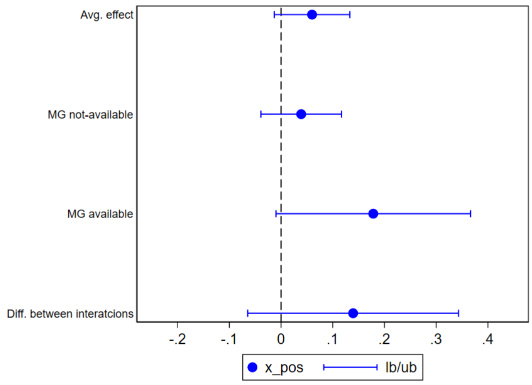



Figure 4: Heterogeneous effects by availability of grandmother childcare - full sample (Panels (A), (C), (E)) and restricted sample (Panels (B),(D),(F).

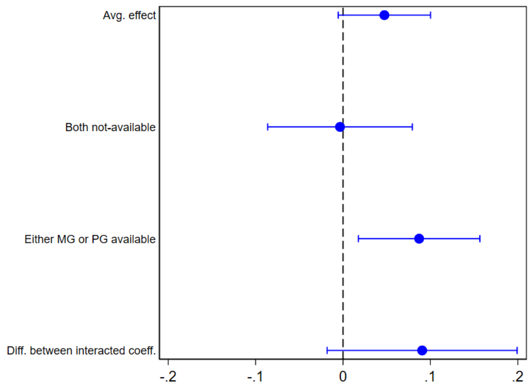
(a) Heterogeneity by availability MG - full sample, 5 yeas later



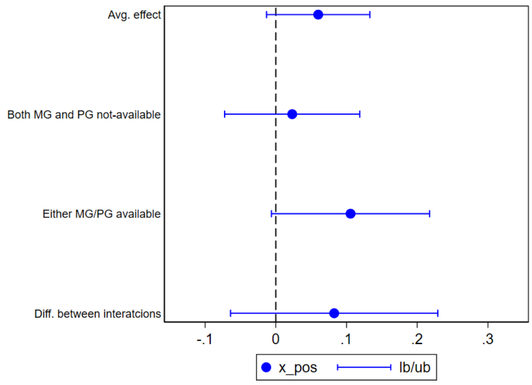

(b) Heterogeneity by availability of MG - - restricted sample (first time mothers), 5 yeas later



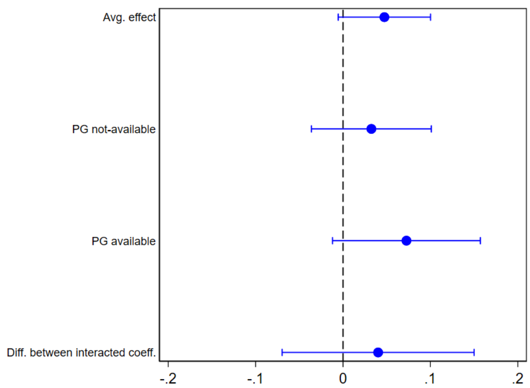

(c) Heterogeneity by availability of either G - full sample, 5 yeas later




(d) Heterogeneity by availability of G - restricted sample (first time mothers), 5 yeas later




(e) Heterogeneity by availability of PG - full sample, 5 yeas later




(f) Heterogeneity by availability of PG - restricted sample (first time mothers), 5 yeas later

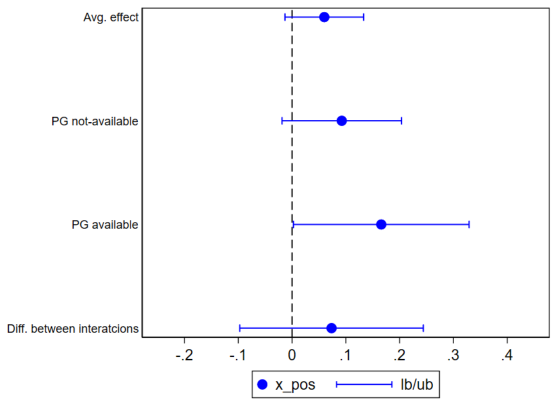



Figure 5: Heterogeneous effects by availability of grandmother childcare - full sample (Panels (A), (C), (E)) and restricted sample (Panels (B),(D),(F).

#### 5.1.3 Mechanism

Overall, these results suggest that women with limited access to informal childcare tend to respond to increased leave duration by delaying their fertility decisions, whereas those with greater access are more likely to give birth again in the short and medium term. When we focus on women more likely to have access to childcare provided by the maternal grandmother, the positive effects of extended leave on fertility are particularly pronounced for first-time mothers, who presumably benefit the most from assistance during an extended period off work. This pattern highlights the role of extended maternity leave as a complement to informal childcare.

A plausible underlying mechanism driving these results consists of the fact that women whose mothers or mothers-in-law are alive and not working may benefit more fully from extended leave. In fact, these women can use the additional time to recover while also receiving guidance and support with childcare duties from their mother or mother-in-law. The availability of such support may reduce the emotional cost associated with maternal care, encouraging women to have another child sooner, possibly knowing that they can rely on both the extended leave and ongoing help from their family.

In contrast, for women whose mothers or mothers-in-law are employed or deceased, extended maternity leave may substitute for the childcare support typically provided by the child's grandmother. In these cases, women without access to informal support must fully engage in childcare responsibilities during their extended leave. This can lead to a challenging and potentially isolating experience, which raises the emotional cost of maternal care. Consequently, longer leave can discourage women lacking family support from having another child in the near future.

The heterogeneous analysis on leave take-up presented in **Table 3** shows that women whose mothers (or mothers-in-law) are absent generally respond to the reform by taking up slightly more leave—about one extra week on average—than those with living, non-working grandmothers. This might indicate that women with limited access to family support are more reliant on the extended leave to bond with their child before switching to formal childcare. The negative effect on fertility for women in this group may simply reflect a postponement response: as prolonged time on leave can signal reduced workplace commitment (Tô (2018)), it can induce women to defer a subsequent pregnancy[7]. However, the difference in leave duration between the two groups is relatively small. Moreover, women with available childcare also lengthen their leave, yet showing positive fertility responses. Taking this into account, it is unlikely that the negative effects on probability of subsequent births among women with limited access to family support is driven by the signaling mechanism. Instead, the additional time spent alone may raise the emotional cost of childcare and thereby delay the next birth.

In line with the emotional cost of maternal care hypothesis, maternal grandmothers appear to be the primary providers of assistance, owing to their stronger emotional bond, greater trust, and more frequent engagement with their daughters. Moreover, first-time mothers are particularly impacted by this dynamic. As they transition into motherhood, first-time mothers may benefit significantly from receiving guidance on how to navigate this new experience, making the presence of a supportive maternal grandmother especially valuable.

# 6 Conclusions

This study investigates the role of the non-monetary costs related to maternal care in defining fertility decisions. We exploit an exogenous variation in maternity leave take-up and explore how extended leave duration affects women's higher order births depending on the availability of support form their mother and mother in law.

On the one hand, longer leave can compensate for absence of informal childcare, reducing the monetary cost of having children. On the other hand, in contexts with easily accessible and affordable formal childcare, the role of grandmothers can be pre-

[7]Moreover, as raise by Beuchert et al. (2016), longer leave may lengthen postpartum breastfeeding, whose natural contraceptive effect can make conception more difficult

dominantly complementary to maternal care during the period of parental leave. By providing crucial emotional support and guidance in the early stages of motherhood, grandmaternal care can enhance the leave experience, reducing the emotional costs of motherhood and potentially incentivizing future births and repeat the leave experience.

We shed light on this puzzle leveraging on the exogenous variation in maternity leave take-up caused by a Danish reform which increased the generosity of leave compensation. We implement an instrumental variable analysis exploiting the distance between the time of birth compared to the cutoff, corresponding to the date of policy implementation, as an instrument for leave duration. Our analysis reveals that extending maternity leave by 100 days has no significant effects on average on the probability of subsequent birth in the following 36 months. The same extension increases probability of giving birth in the following 60 months by 5 pp. However, these baseline effect hides substantial heterogeneity.

Specifically, women whose mother and mother-in-law are both employed, missing or deceased, show a 11 pp decrease in the likelihood of having another child within three years. The magnitude of the negative effect is marginally higher for first time mothers (11.5 pp). However, these effects to not persist in the medium term, suggesting a deferring of future birth in response to the leave extension rather than a decrease in fertility. In contrast, when at least one grandmother is more likely to be available, 100 extra days of leave are associated with an increase the probability of giving birth of 7 pp after within the following 36 months, and the effect persist 60 months since the reference birth.

The positive increase in birth probability for women who are more likely to have access to grandmaternal childcare appears driven by the presence of maternal grandmothers. In fact, women whose own mother is more likely to be available to support their daughter throughout the leave experience an 11 pp increase in the probability of having another child within 3 months. This rises to 23 percentage points for those who gave birth to their first child. The effect persists and rises to 13 pp in when considering 60 months since the birth. While paternal grandmothers appear to play a less prominent role, the effects mediated by the availability of mothers-in-law, although not statistically significant, tend to align with those observed for maternal grandmothers.

The findings point towards a prevalence of the mechanism of the emotional cost of maternal care as a mediation for the effect of parental leave extension on fertility outcomes. Extended time spent as a likely solo caregiver can intensify the burden of care and increase the emotional cost of motherhood. The challenges of managing newborn care without additional support for an extended period may lead to a less favorable maternity experience and a subsequent postponement of further pregnancies. Moreover, in context with easily accessible formal childcare, the monetary gains from sending the child to a facility later are marginal and therefore hardly compensate for the rise of the non-monetary cost.

The suggestive evidence that grandmother presence during an extended leave period improves the maternal experience and leads to higher birth rates carries important policy implications. First, simply increasing leave entitlement and generosity may not be sufficient to raise fertility, since the effects are highly heterogeneous. Second, policies that obstruct women's access to support from their own mother (or mother-in-law), such as raising the retirement age, should be designed carefully, as they may inadvertently depress fertility. Finally, our results point to the value of providing professional support figures, such as midwives, to assist and guide women during the early postpartum period. A notable example is Germany's *Familienhebammen* program.

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# 7 Appendix

## A Measurement of leave take-up

In the DREAM database, parents sometimes appear to take more leave than permitted. This discrepancy arises when they combine leave from multiple births, using residual leave weeks from previous births or combining leave entitlements for a subsequent birth occurring within a short time gap from the current birth[8]. To accurately assess the effect of leave duration on fertility, we devised a specific counting methodology.

We rely on the DREAM register to compute the uptake of parental leave by parents of children born within the narrow window spacing 8 weeks before and after the reform, excluding the week preceding and the week following the cutoff.

For parents who had a child before the reform, we count the weeks of full benefits within the first year after birth (0 to 52 weeks), separately for the mother and the father or co-mother, if present. This period includes 24 weeks of full benefits, followed by a phase of reduced benefits from week 24 to 78. This assumption is based on the premise that parents exhaust their full benefits before switching to reduced benefits. For post-reform births, we apply the same counting methodology: considering full benefits for the first 52 weeks. If parents opt for the pre-reform regime, reduced benefits are accounted for between weeks 24 and 78.

Our approach permits parents to utilize more than the usual maximum allotment of leave with full-benefit compensation for an individual child, provided that this leave is taken close to the time of the birth. This methodological decision is driven by the assumption that any leave beyond the standard maximum is used for the care of the child born within the period under study, as long as the leave is taken sufficiently close to its birth. Since the focus of this empirical exercise is to understand how the length

[8]Since parents are allowed to freely distribute their leaves within the first 8 years post-birth, attributing leave uptake to a single birth becomes challenging. Considering only leave taken within the first year could underestimate the total uptake, especially for families entitled to the pre-reform scheme when the maximum amount of weeks of leaves available was 78. Conversely, considering leave uptake within two years post-birth can lead to an overestimation whenever a sibling is born within that time frame.

of parental leaves influences the likelihood of having additional children, it is necessary to include all periods of leave used for childcare of the reference child. These include weeks initially allocated to previous births but not used until the birth of the reference child, as long as they are utilized for the care of a newborn.

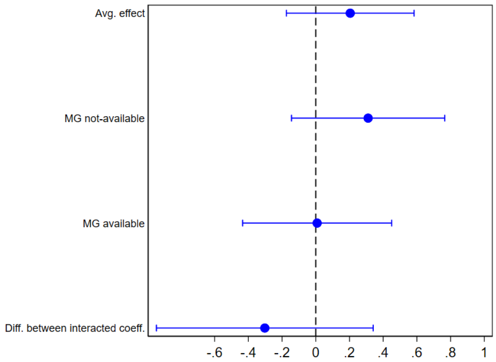
By implementing this counting method, we calculate the total number of weeks of parental leave associated with a single birth. This total is the sum of the weeks of full benefits and reduced benefits for the mother, combined with the weeks of full benefits and reduced benefits for the father. Each of these components is computed separately and then aggregated to provide a comprehensive view of the parental leave taken in correspondance to that particular birth.

# B Robustness check: Placebo analysis

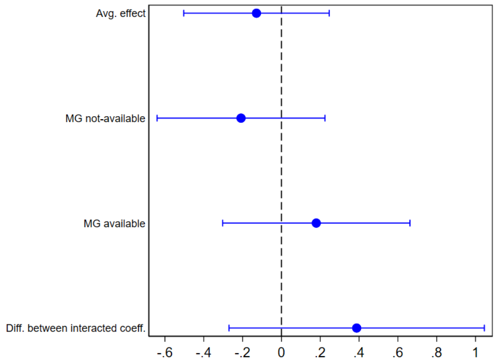
Figure 6 presents the results of the same analysis a sample of women giving birth within the same time window in the year preceding the reform (between 2000 and 2001). The confidence intervals are significantly larger in this sample, indicating a significantly greater variance compared to the main analysis and none of the effects appears significant.

Figure 6: Availability of grandmother childcare – Placebo analysis. Effects 3 years (Panels A, C, E) and 5 years (Panels B, D, F) after childbirth.

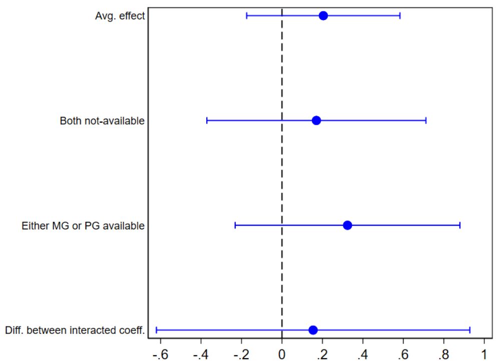
(a) Availability MG – full sample, 3 years



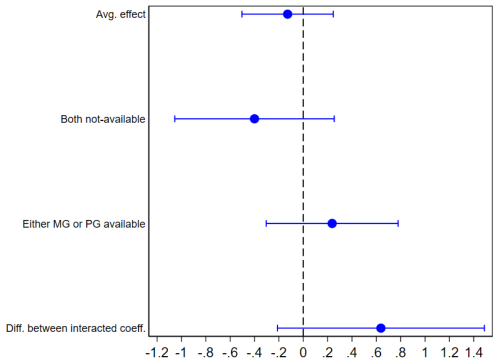

(b) Availability MG – full sample, 5 years



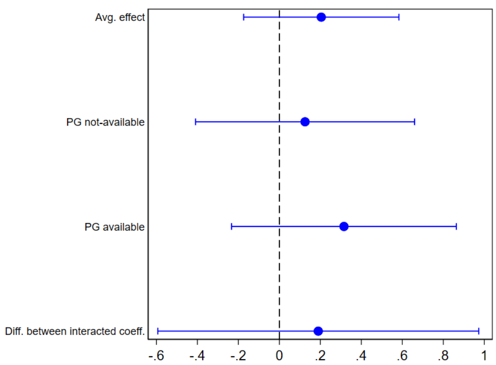

(c) Availability of either G – full sample, 3 years




(d) Availability of G – full sample, 5 years




(e) Availability of PG – full sample, 3 years




(f) Availability of PG – full sample, 5 years