# Breastfeeding and the return to work after childbirth of new mothers: evidence from a baby formula scare

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November 2024

# Breastfeeding and the return to work after childbirth of new mothers: evidence from a baby formula scare*

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November 3, 2024

**Abstract**

We use a baby formula "food scare" in Israel in 2003 as a plausible natural experiment to study the causal relationship between breastfeeding and mothers' return to work after childbirth. Analysis of administrative data covering the universe of births in the country shows that first-time mothers who gave birth shortly after the scare delayed their return to work. Their average months worked in the first six months after childbirth fell by about 11 percent relative to their counterparts in the previous year. Data from a major medical equipment lender in Israel indicates an increased likelihood of borrowing milk pumps, suggesting that the delay in returning to work was driven by an increase in breastfeeding. The results indicate that despite developments in technology and policy changes in recent decades, mothers still trade off work for the breastfeeding of their children.

**Keywords**: motherhood, labor supply, breastfeeding, food scare, maternity leave, return to work
**JEL Classifications**: I18 J13, J22, D1

*We are extremely grateful to Doron Sayag for his generous help and guidance with the CBS data and to the Yad-Sara organization. We thank seminar participants at SMU for their helpful comments. Izik Daniel provided excellent research assistance. Financial support from the Maurice Falk Institute for Economic Research in Israel and the National Insurance Institute of Israel is gratefully acknowledged.

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# 1 Introduction

Breastfeeding and returning to work after childbirth can present a challenge for many women. The conditions in the workplace, in both policy and infrastructure aspects, may not always support mothers who opt to breastfeed.[1] Although it is important to understand how breastfeeding decisions can impact mothers' return to work, empirical evidence on this topic remains scarce. In this study, we leverage a significant product safety hazard realization in Israel's baby formula market in 2003 (the "Remedia Event") to examine this link.

Our premise is that the Remedia Event triggered a 'food scare,' a sudden spike in concerns about the safety of a particular product, leading to an abrupt reduction in demand (Schlenker and Villas-Boas, 2009). In the aftermath of the Remedia Event, maternal concern regarding the safety of baby formula increased. This concern prompted some mothers to turn to breastfeeding as a perceived safer alternative, potentially resulting in an increase in breastfeeding rates.[2] This unanticipated shift towards breastfeeding provides us with a unique opportunity to examine the causal relationship between breastfeeding practices and the return-to-work decisions of new mothers. Specifically, if workplace policies and environments are insufficiently supportive of breastfeeding, we expect that the rising breastfeeding rates would lead to a delay in mothers returning to work.

To investigate the return-to-work response of new mothers following the Remedia Event, we draw on administrative data from the National Insurance Institute of Israel (NII). Our sample covers all births in Israel from 2001-2004 for households where both partners are employed. Our analysis zeroes in on first-time mothers—those who had their first child around the Remedia Event—as the *treatment* group. To establish causality, we use a dynamic difference-in-differences approach. We assess the return-to-work patterns of first-time mothers who gave birth in the year surrounding the Remedia Event, May 2003 - April 2004, and contrast them with those who gave birth during the same period in the preceding year, May 2002 - April 2003, serving as the *comparison* group. The month of delivery breakdown reveals that in the months preceding the event, the monthly difference-in-differences coefficients are minor and lack statistical significance. In contrast, first-time mothers who delivered right after the Remedia Event – in November 2003 – show a pronounced and statistically significant delay in their return to work. This effect is concentrated in the November 2003 cohort, with smaller and statistically insignificant estimates for mothers who gave birth in the subsequent months.

[1] Barriers to breastfeeding in the workplace include lack of nearby child care, rigid time schedules that do not allow for work from home or even nursing breaks, lack of a location providing privacy for breast-pumping, and no facilities for refrigeration of pumped breastmilk. For a detailed discussion on these challenges, see Spitzmueller et al. (2016), Johnston and Esposito (2007) and AAP (1982).

[2] Note that some mothers may have opted for different baby-food products instead.

To reinforce our findings, we conduct an auxiliary analysis using the same treatment group but compared to an alternative group: experienced mothers—those with at least one prior child—from the identical timeframe as the treatment group. We posit that these seasoned mothers were less impacted by the Remedia Event. Reassuringly, this analysis aligns closely with our primary results.

We further examine the pronounced effect, observed for the cohort of first-time mothers who delivered in November 2003, by analyzing the month-by-month return-to-work dynamics of these mothers using survival analysis. The analysis uncovers a consistent decrease in the return-to-work rate from the third to the ninth month post-childbirth. This result aligns with common breastfeeding patterns, suggesting that behind the moderate average decline in employment, there is a dynamic of significantly prolonging maternity leaves of some mothers who would otherwise return to work within three to nine months.[3]

We examine the heterogeneity in the effects of the Remedia Event across different subgroups of mothers. Our analysis reveals that the effect on mothers who gave birth immediately after the event – the November 2003 cohort – was more pronounced among younger mothers, aligning with the idea that less experienced, first-time mothers may respond more strongly. The effect was also larger when the baby was male, possibly reflecting a parental preference for boys. However, neither of these differences was statistically significant. Additionally, when looking at the average effect across all birth month cohorts from November 2003 to April 2004, we find it was more pronounced among high-income households. This stronger effect was driven by the birth month cohorts subsequent to November 2003. That is, in high-income households, not only the November 2003 birth cohort but also first-time mothers in later birth month cohorts were affected by the Remedia Event.

Labor market policy changes around the time of the Remedia Event may potentially pose a threat to our identification strategy. Notably, the two policy changes in that period, a reduction in welfare benefits and child allowance, targeted low-income households and families with more than two children, respectively. However, the response to the Remedia Event was concentrated among first-time mothers, a group that was least affected by these policy changes. Moreover, the goal of these policies was to strengthen the incentives to work, potentially discouraging mothers from extending their maternity leaves, hence, if anything, biasing our results downwards.

As highlighted earlier, we posit that the Remedia Event sparked a 'food scare,' leading new mothers to lean towards breastfeeding over baby formula. In the final section of our

[3] In this paper, the term "maternity leave" is used in a broad sense to denote the period of employment interruption that women may take following childbirth. This usage is distinct from "maternity leave policy," which refers to the legal framework outlining the rights and entitlements of women to take leave after giving birth.

analysis, we probe this assumption. In an ideal scenario, we would directly determine if the Remedia Event caused an uptick in breastfeeding. Yet, pertinent data on breastfeeding for that timeframe is unavailable to us.[4] To gauge this indirectly, we analyze trends in milk pump borrowing, using data from a large free medical equipment lender in Israel. The findings indicate a 13% increase in milk pump borrowing in November 2003, right after the Remedia event, reinforcing the notion that the employment response is attributed to the Remedia Event.

A line of recent research underscores the positive impact of extended parental leave benefits on breastfeeding. Huang and Yang (2015) observe that the introduction of paid maternity leave in California was associated with increased breastfeeding, and Kottwitz et al. (2016) link parental leave benefits in Germany to extended breastfeeding durations. Haider et al. (2003) use state-level changes associated with the 1996 US welfare-to-work reform to identify that work requirements embedded in the policy led to a decline of 5.5% in breastfeeding at infant age of six months.

While these studies highlight the influence of policy changes in leave duration and benefits on breastfeeding, our research explores a different relationship. We investigate whether the decision to breastfeed influences new mothers to delay their return to work post-childbirth in a given policy environment. Despite its potential significance for policy, this mechanism has received little attention in previous literature, possibly due to the challenge of finding variation in breastfeeding patterns that is exogenous to labor market outcomes.[5] The unique circumstances of the Remedia Event provide a rare opportunity to examine this relationship. Our findings indicate that the decision to breastfeed causes a delay in returning to work after childbirth. As our results capture the 'intent to treat' effect of the Remedia Event, this limits our ability to precisely quantify the magnitude of the effect. The observed average delay of one week for November 2003 deliveries represents a lower bound for the average treatment effect of the decision to breastfeed.[6] However, our analysis of return-to-work dynamics suggests that this delay does not persist beyond the first year postpartum.

Our study contributes to the recent literature showing that the "child penalty"—the substantial and persistent decrease in labor market income for mothers following child birth—accounts for most of the observed gender inequality in labor market outcomes (Kleven et al., 2019; Angelov et al., 2016; Cortés and Pan, 2023; Bertrand et al., 2010)

[4] A notable exception exists: two surveys by the Israeli Ministry of Health in 1999 and 2009. While they hint at an increase in breastfeeding from 1999 to 2009, data for the 2000-2008 interval is missing.

[5] A notable exception is the work of Mandal et al. (2014), who use a simultaneous equations framework and find evidence suggesting that breastfeeding decisions alter postpartum work.

[6] If we take the estimate of milk pump borrowing at face value as a first-stage estimate, the average treatment effect would be closer to two months.

While the existence of the child penalty is well-established, the underlying mechanisms driving this phenomenon remain less understood. Traditional explanations have focused on biological factors, since only women can give birth and have the option to breastfeed. However, recent research comparing child penalties across biological and adoptive families (Kleven et al., 2021), as well as among heterosexual non-adoptive, adoptive, and same-sex couples (Andresen and Nix, 2022), suggests that biology plays only a partial role, concentrated in the short term. These findings indicate that other factors, such as social norms, likely contribute significantly to the persistent aspects of the child penalty. Our research aligns with these recent findings. We demonstrate that breastfeeding by new mothers leads to a delay in returning to work, but this delay is particularly pronounced during the first nine months after childbirth. These findings may help explain the larger short term child penalty observed in biological families, compared to other family types.

Moreover, our study complements research utilizing randomized controlled trials to evaluate breastfeeding promotion policies, which have been shown to affect breastfeeding practices positively (Kramer et al., 2001; Morrow et al., 1999; Bhandari et al., 2003). While these studies offer insights into the efficacy of these policies, they do not account for the subsequent impact on women's postpartum employment choices mediated through breastfeeding. Our findings underscore the need to account for the labor market consequences of these policies in a comprehensive evaluation of their costs and benefits.

Finally, our study contributes to the broader literature on how gender roles in childcare, particularly infant feeding, influence new mothers' employment decisions. Relying on the decline in baby formula prices around the mid-twentieth century, Albanesi and Olivetti (2016) show that the diffusion of baby formula contributed to the remarkable increase in mothers' labor force participation between 1930 and 1960 by reducing the exclusivity of mothers in infant feeding. Albanesi and Olivetti (2016)'s study is part of a broader literature suggesting a positive causal link between declining childbearing and child-rearing costs and the significant rise in women's labor force participation during the twentieth century. Examples include the spread of birth control pills, infant formula, labor-saving household technologies, and advances in medical knowledge and obstetric practices, all of which helped ease the challenges of balancing work and motherhood (see Greenwood et al. (2005), Bailey (2006)).

The remainder of the paper is structured as follows. Section 2 provides some background about the circumstances surrounding the Remedia Event and the baby formula market and maternity leave policy in Israel. In section 3, we present the evidence on the impact of the Remedia Event on mothers' return to work after childbirth. In section 4, we analyze the effect of the Remedia Event on the borrowing of milk pumps. Section 5 concludes.

# 2 Background

## 2.1 The baby formula market in Israel and the Remedia Event

The baby formula market in Israel has been quite concentrated in the past few decades. Until 1999, Materna, a baby formula company that manufactures in Israel, had a market share of more than 50% of the baby formula market in the country. Consequently, Materna was declared a monopoly, and the government implemented price control in the baby formula market. In that period, there were two other main players in the baby formula market, Similac, a subsidiary of the international brand Promedico, and Remedia, an Israeli company partly owned by Heinz, that distributed imported baby formula. By 1999, the market shares of the two smaller companies increased at the expense of Materna's market share. As a result, the price control was gradually removed until it was dropped completely at the beginning of 2001. In 2003, before the Remedia Event, Materna held 37% of the market, Remedia held 37%, and Similac held 26%.[7]

In 2003, Remedia began offering a new soy-based formula manufactured by the German company Humana. In October 2003, several infants were hospitalized with symptoms of apathy and convulsions. The common cause of their illnesses, to be identified only a month later, in November 2003, was consuming the new formula. It was later discovered that the new formula contained an insufficient quantity of vitamin B1, causing the death of four infants and various long-term motor, neurological and cognitive damages to more than sixty others.[8] The Remedia Event received considerable media coverage and public attention.[9] Indeed, in December 2003, a month after the Remedia Event unraveled, Remedia's share in the baby formula market fell drastically from 37% to 5-7% and effectively declined to zero by the end of 2005.

Notably, the Remedia Event originated from the Remedia soy-based formula market. The usage and sales of soy-based infant formula vary worldwide, and its consumption in

[7]Information on annual market shares comes from the 2004 rating report summary of Maabarot, Materna's mother company, a public company.

[8]2,000-4,000 infants were exposed to the impaired formula to some extent (based on Remedia's market share of 37%, and about 4-7 months the impaired formula was in the market). As solid foods are typically introduced to infants around the age of six months, roughly 1,000-2,000 infants were exclusively fed by the impaired formula. About 3-6% of them were inflicted (more than 60 infants). Over the years, more victims with less severe symptoms, including ADHD and limb pain, were diagnosed, and one more victim died.

[9]It resulted in civil and criminal proceedings for the involved parties. Remedia's chief technology officer was convicted of wrongful death and was sentenced to 15 months in prison, and Israeli Health Ministry officials were sentenced to public service in the criminal process. In the civil process, the company and the victims' families reached a financial settlement. In Germany, Humana officials were fired, and the German authorities fined the company.

Israel is relatively high (Program et al., 2010). The soy-based formula represents roughly 15% of the baby formula market in ages 0-1, and it is typically used for babies with allergies or vegan nutrition. The negative demand shock caused by the Remedia Event was perhaps most substantial among mothers who were soy-based formula consumers. Unfortunately, we are unable to examine this issue directly.

## 2.2 Maternity leave policy in Israel

Several elements in Israel's labor laws and social safety net shaped the maternity leave policy in Israel in the relevant period. Until May 2007, according to the Israeli Woman's Labor Law, an employee was entitled to a maternity leave of 12 weeks.[10] An employer was not allowed to prevent an employee from taking maternity leave or fire her during the maternity leave or 45 days after that. Additionally, an employee was allowed to return to work no less than 12 weeks after childbirth. Therefore, at the time, a rule of a mandatory 12 weeks of maternity leave after childbirth applied.[11]

The mandatory maternity leave in Israel is accompanied by maternity allowance—a payment by NII that substitutes the employee's labor income during maternity leave. In the relevant period for this study, the maternity allowance amount was calculated based on the average daily wage in the three months before the maternity leave, for 12 weeks (84 days). Eligibility for the maximal allowance required employment in 10 out of the 14 or 15 out of the 22 months before delivery.[12] Therefore, women with a qualifying work history typically had a twelve-week paid mandatory maternity leave. Additionally, women with at least 24 months of work history with the same employer could delay the return to work for another month for every four months of employment history for up to a year. However, this delay was at their own expense, with no maternity allowance.

[10] In May 2007, this period was extended to 14 weeks, and in 2010, it was extended to 26 weeks as a default. However, a woman may shorten this period to no less than 14 weeks.

[11] Since 2007, a couple can share the mandated maternity leave period. Husbands can take the last six weeks of maternity leave if the wife provides written consent and returns to work while her husband is on leave.

[12] Partial maternity allowance for six weeks existed for women who accrued six months of work in the 14 months before delivery. Additionally, all women were eligible for a birth grant that depended on the child's birth parity.

# 3 The impact of the Remedia Event on maternity leaves

In this section, we address the main question of this paper: did mothers extend their maternity leaves as a result of the Remedia Event? We posit that the event introduced an exogenous shock to breastfeeding that could influence maternal behavior in the labor market. Specifically, We examine whether, by increasing breastfeeding, the Remedia Event caused a delay in mothers' return to work after childbirth.

## 3.1 Empirical strategy

We employ a difference-in-differences (DD) approach to examine the return-to-work decisions of first-time mothers following the Remedia Event. By comparing the decisions of mothers who gave birth shortly before and after the event to those of first-time mothers who delivered in the preceding year, we aim to isolate the potential impact of the Remedia Event on their labor market behaviors, accounting for other temporal factors and trends.

Existing research often distinguishes between novice and experienced mothers in the analysis of maternal behavior (see, e.g., Gameiro et al. (2009)). There are two main reasons to think that the labor supply response to the Remedia Event was more pronounced among first-time mothers compared to their experienced counterparts. First, first-time mothers typically face more stress and anxiety after giving birth (see Hung et al. (2011)), potentially leading to a larger increase in breastfeeding in response to the Remedia Event. Second, while experienced mothers may already have jobs accommodating child care, first-time mothers might hold positions that make balancing work and breastfeeding more challenging.[13] Therefore, our analysis primarily explores the Remedia Event's effects on first-time mothers.

To illustrate our strategy, consider a woman who delivered in October 2003, just before the Remedia Event, and opted to exclusively use formula for her baby. Upon the event's occurrence, reverting her decision would prove challenging, given the complexities of resuming breastfeeding after a pause. We, therefore, rely on the near-irreversibility of an exclusive formula-feeding decision, capturing the effect of the Remedia Event on mothers' return to work via breastfeeding *initiation*.

This approach's strength is that it draws on well-defined groups of mothers based

[13]This is underscored by Lalive et al. (2013), who observe that the labor market history of first-time mothers prior to childbirth is more indicative of their potential earnings compared to experienced mothers.

on the time of their childbirth relative to the event. However, it potentially misses the response of women who delivered prior to the event but were still actively breastfeeding during its onset. As we previously highlighted, reintroducing breastfeeding after committing to formula presents significant biological challenges. In contrast, mothers already breastfeeding have greater biological leeway regarding when to cease, making them more susceptible to influences from the Remedia Event. Take, for instance, a mother who gave birth in August 2003 and chose to breastfeed. Her decisions on breastfeeding and returning to work were initially unaffected by the Remedia Event up to October 2003. But with the event unfolding around her baby's fourth month, she might have opted to extend her breastfeeding period, consequently postponing her return to the workplace. Our approach does not capture the effect of the Remedia Event on this group of women and thus provides a lower bound of the overall effect.[14]

In our main specification, we implement a month-by-month dynamic DD strategy, comparing first-time mothers who gave birth in the year around the Remedia Event, May 2003 - April 2004 (the treatment group), to those who delivered in the preceding year, May 2002 and April 2003 (the comparison group). We run a regression of the form

$$y_{it} = \alpha + \beta \cdot TR + \sum_{\tau \neq -1, \tau = -6}^{5} (\gamma_\tau + \delta_\tau \cdot TR) \cdot M_\tau + \epsilon_{it} \tag{1}$$

where $y_{it}$ denotes our outcome variable: the number of months worked by mother $i$ who delivered in month $t$ of the sample period, May 2002 - April 2004, within the first six months following her delivery.[15] $M_\tau$ is a vector of monthly indicators for each month from May to April. These indicators are enumerated by $\tau$, relative to November, the time of the Remedia Event. Hence, $M_0$ is an indicator for November, $M_{-1}$ is the indicator for October, etc.

$TR$ is the indicator for belonging to the treatment group. In our main specification, $TR = 0$ applies to the comparison group of first-time mothers who delivered in the year preceding the event. October 2003, the month before the Remedia Event, is the omitted unit of this analysis corresponding to the coefficient $\delta_{-1}$. The coefficients (dynamic effects) $\delta_\tau$, measure the effect of the Remedia Event compared to the level one month before the event.

To further validate our analysis, we introduce another DD specification, which retains the same treatment group but contrasts it with experienced mothers. As previously discussed, we hypothesize that experienced mothers are less likely to be influenced by the

[14] To check this issue, we run a specification of Equation 1, moving the treatment time back to Aug 2003. See section 3.6.

[15] We also analyze a similar variable that sums the number of months worked within the first twelve months postpartum and report these results in the appendix.

Remedia Event.

To estimate the average effect across the six birth month cohorts, we run the following standard DD regression:

$$y_{it} = \alpha + \beta_1 \cdot TR + \beta_2 \cdot Post + \beta_3 \cdot Post \cdot TR + \epsilon_{it} \tag{2}$$

where $y_{it}$ continues as our outcome variable. $TR$ is again an indicator variable that takes the value one if the birth belongs to the treatment group, i.e., it occurred in the year around the Remedia Event, between May 2003 - April 2004, and zero otherwise. $Post$ is an indicator assigned a value of one if the birth occurred between November and April, which designates the post-Remedia Event period. The coefficient of interest, $\beta_3$, quantifies the average impact of the Remedia Event on the return-to-work decisions of mothers who delivered in the six birth month cohorts following the Remedia Event. Put simply, it measures the differential effect on the treatment group compared to the comparison group in the post-event period while factoring in variations in maternity leave durations.

## 3.2 Data

Our analysis draws on administrative data from the NII. The NII collects these data from various sources (including the Tax Authority and the Ministry of Interior Affairs) for internal use. The data contain information about the country's universe of employees, including months of employment in every tax year and annual income. These data also contain demographic information such as gender, date of birth, and marital status. Importantly for this study, the birth date of each child is also available, making it possible to link data about every childbirth in the relevant period with information about the mother's and father's employment history. Using these data, we observe the mother's months of employment around every childbirth in the relevant period. As we focus on the first months after childbirth, the period when the tradeoff between breastfeeding and employment potentially arises, we censor the employment data at twelve months after childbirth.[16]

**Sample creation.** We begin with the universe of all women who gave birth between May 2002 and April 2004 and were continuously employed in the twelve months preced-

[16]Exclusive breastfeeding is uncommon after age six months (Li et al. (2002)). According to the Israeli Ministry of Health, exclusive breastfeeding rates in Israel are around 15% at the age of six months (Keinan-Boker et al. (2014)).

ing childbirth, ensuring their eligibility for full maternity allowance. This initial sample includes approximately 98,300 births. In our analysis, we aim to focus on dual-earner households, which represent the large majority of households. However, due to the absence of data on husbands who are not employees, we are unable to distinguish between the self-employed and those who are not working. Consequently, we restrict our sample to households where the men were employed at some point during the year prior to the child's birth, though not necessarily for the entire twelve months. This approach ensures that our sample comprises dual-earners but excludes self-employed individuals.[17] Additionally, households headed by single mothers, although potentially interesting to study, constitute only a small fraction of our sample and are therefore excluded from the analysis.

We observe both spouses' employment histories for 76,194 households out of the initial sample. Of these households, we, unfortunately, do not observe the maternity leaves of 20,673 women. This data limitation largely arises because these women are teachers, and their maternity leaves are not accurately reported in the data. We rely on the report of months worked each year to determine the timing of a woman's return to work. However, for teachers, the data shows continuous employment after delivery, as they continue receiving income from their employer, typically the Ministry of Education.[18]

After we drop these women from the data, we are left with a final sample of 55,521 mothers from households with two employed spouses, of which there are 19,918 (35,603) births in the first-time (experienced) mothers group. In the sample of first-time mothers, we have 9,734 mothers who delivered in the year around the Remedia Event (the treatment group) and 10,184 mothers who delivered in the prior year (the comparison group). In the specification that uses experienced mothers as the comparison group, the entire final sample is used.[19]

**Main outcome variables.** We aim to understand the interplay between breastfeeding and the decision to return to work. Given that breastfeeding is relatively prevalent during the initial six months post-childbirth, our primary outcome variable measures the number of months a mother worked within the first six months after delivery. This outcome is designed to capture the employment patterns of mothers who opt for breastfeeding. To gauge more extended effects potentially stemming from the Remedia Event, we also

[17]Excluding the self-employed from the analysis is appropriate when studying paid family leave effects, particularly in the context of leave-taking by fathers and mothers, as eligibility typically requires employee status. This restriction also aligns with other research areas, such as child penalty studies (e.g., Andresen and Nix (2022)).

[18]This issue occurs because teachers receive payment for school holidays and the summer break. Mothers on maternity leave continue to receive these payments, making it appear in the tax data as if they are working despite being on leave. Below, we check the consequences of this data issue for our results (see Section 3.6).

[19] For the month-by-month dynamic DD strategy using experienced mothers as the comparison group, we "bin-up" all births from periods before May 2003 into $\tau = -6$ (see Schmidheiny and Siegloch (2019)). Dropping the births from periods before May 2003 generates similar results.

consider a secondary outcome variable: the number of months a mother worked in the year following childbirth.

**Descriptive statistics.** Table 1 provides some descriptive statistics of the first-time mothers sample. The table compares mothers that gave birth in the year around the Remedia Event: November 2003 - April 2004 (the post period) versus May 2003 - October 03 (the pre period) to the comparison group, mothers that gave birth in the respective periods in the prior year, namely, November 2002 - April 2003 versus May 2002 - October 2002. Columns (1)-(2) of the table summarize the characteristics of the pre and post groups in the year around the Remedia Event, and column (3) reports the difference between them. Columns (4)-(6) summarize the characteristics of the respective groups in the prior year, and the difference between them, and column (7) reports the difference-in-differences between the means of the four groups. Overall, the sample characteristics appear to be well aligned across the different groups with no statistically significant differences. Notably, the DD in the real monthly income (denominated to 2000 terms) of the husbands is about 500 NIS. This difference arises primarily because we calculate the groups' incomes using earnings from different tax years due to the coarseness of tax data.[20]

## 3.3 Main results

Figure 1 visualizes the $\delta_\tau$ coefficients from regression 1, illustrating the month-by-month impact of the Remedia Event on employment duration (months worked) within the first six months post-delivery. The figure shows the results for both comparison groups we have discussed, first-time mothers in the previous year and experienced mothers.[21] The blue crosses represent our primary analysis, first-time mothers compared to those from the preceding year, accompanied by their 95% confidence intervals. For the six months birth cohorts leading up to the Remedia Event, the coefficients are statistically insignificant, with a deviation of no more than 0.11 months from zero. However, for the November 2003 birth corort, there is a significant decrease of roughly 0.22 months (about a week) in employment duration. With an average of two months of employment within the first six months post-delivery, this signifies an 11% reduction in employment duration for first-time mothers who gave birth shortly after the Remedia Event. This effect is temporary. In the December 2003 birth cohort, the differences between the groups are statistically insignificant.

[20]We use the individual's annual income in the tax year before the year of childbirth because the income earned in the same tax year as the birth may include earnings generated after the birth and, therefore, would be less informative for earning capacity.

[21]Full regression results for both specifications are detailed in Appendix Table A.1.

To further substantiate our findings, we conduct an auxiliary regression using experienced mothers as an alternative comparison group. The grey diamonds in Figure 1 represent the coefficients from this regression, with dashed lines indicating their 95% confidence intervals. Reassuringly, these results echo our primary analysis. Before the Remedia Event, employment duration coefficients are statistically insignificant. Yet, in the November 2003 birth cohort, we observe a discernible and statistically significant reduction of about 0.18 months in employment duration. Birth cohorts in subsequent months, once more, show no significant differences. The analysis for months worked within twelve months of delivery shows qualitatively similar results (Appendix Figure A.1 and Table A.2 show the entire set of results).

Panel A of Table 2 presents the outcomes from Equation 2, quantifying the *average effect* in the six months cohorts following the Remedia Event ($\beta_3$ in Equation 2). Columns 1-2 present the results using the comparison group of first-time mothers in the preceding year. In column 1, we observe a small significant negative impact of 0.08 months on the months worked within six months post-delivery. This effect remains significant with the inclusion of household characteristics in column 2. However, when comparing to experienced mothers (columns 3-4), the effect diminishes and loses statistical significance. Given our previous findings of the immediate nature of the effect of the Remedia Event, we estimate a version of Equation 2 disaggregating the average effect on the post period (November 2003 - April 2004) birth cohorts into two components: the *immediate effect* (on November 2003 birth cohort) and the *subsequent effect* (on December 2003 - April 2004 birth cohorts), as shown in Equation 3:

$$\begin{aligned} y_{it} = &\alpha + \beta_1 \cdot TR + \beta_2 \cdot \text{immediate} + \beta_3 \cdot \text{subsequent} \\ &+ \beta_4 \cdot \text{immediate} \cdot TR + \beta_5 \cdot \text{subsequent} \cdot TR + \epsilon_{it} \end{aligned} \tag{3}$$

Panel B of Table 2 details these results. The immediate effect reveals a significant reduction in employment duration by approximately 0.21 months (column 1), adjusting to 0.175 with added controls (column 2). The subsequent effect, however, is minor and statistically insignificant, consistent with Figure 1. Results using experienced mothers as a comparison group (columns 3 & 4) corroborate these findings, with similar significant immediate effect magnitudes but insignificant subsequent effects.

To recap, our analysis indicates a notable decline in employment for first-time mothers who gave birth in November 2003, irrespective of the specification used. This cohort effect is immediate in the sense that it is significant only for births occurring in the month immediately following the Remedia Event, and we do not observe similar delays in return to work for mothers who gave birth in the months after November 2003. These findings align with existing research highlighting the short-term effects of food scares in other contexts (Ferrer et al., 2016).

## 3.4 Unpacking the immediate cohort effect, a survival analysis

Our main results indicate that the Remedia Event created an immediate cohort effect, significantly affecting only mothers who gave birth in the first month after it occurred. Next, our objective is to investigate the evolution of the impact of the Remedia Event on the November 2003 birth cohort over the course of the first year following childbirth. Using survival analysis, we break down the delay of approximately 11% in the return to work for mothers who gave birth in November 2003 into a month-to-month return to work decision, verifying its consistency with known patterns of breastfeeding and work.

Figure 2 displays Kaplan-Meier survival curves for the likelihood of being on maternity leave in the first twelve months after childbirth. These curves show the actual period mothers stayed at home before resuming work.[22] Panel (a) of the figure contrasts the survival curve of November 2003 birth cohort (represented by the red line) with the mean of May 2003 - October 2003 birth cohorts (depicted by the blue line). By construction, the probability of being on maternity leave is initially one for both groups, as all the women in our sample are on maternity leave in the first month after childbirth.[23]

By the third month after childbirth, around which many women finish the eligibility period for maternity allowance and the mandatory maternity leave period ends, roughly half of the women return to work.[24] At this point, a discernible gap between the groups opens, and the November birth cohort appears relatively less likely to return to work. The gap persists for another few months, yet it narrows in the course of the first year after childbirth. By the end of the first year after delivery, the gap is very small. The overall difference between the two survival curves is statistically significant, with a p-value of 0.0001 based on the log-rank test for equality of the two survival curves.

Panel (b) of Figure 2 shows the same survival curves for the prior year (the comparison group). November 2002 birth cohort is represented by the red line, and the mean of May 2002 - October 2002 birth cohorts are depicted by the blue line. As the figure shows, the two lines appear to coincide well. Indeed, one cannot reject the null hypothesis that the two survival curves are equal with a p-value of 0.0935.[25]

[22] We censor the functions at twelve months because breastfeeding is typically relevant in the first months after childbirth.

[23] As we noted above, this is, in fact, mandatory that these women do not work just after childbirth.

[24] We observe some return to work already in the second month after childbirth for two main reasons: (i) our data reports calendar months of work while the maternity leave period is counted in exact weeks, (ii) some women start maternity leave before the delivery and, therefore, their twelve-week count ends before the third month after childbirth.

[25] The month-to-month DD analysis appears in Appendix Table A.3

Notably, Figure 2 is compatible with the dynamics of substitutability between breastfeeding and work. Starting in the third month after childbirth, after the mandatory maternity leave period ends, mothers face the choice to delay their return to work at their own expense – at this point, the absence of maternity benefits augments their opportunity costs. By the seventh month after childbirth, different types of solid foods are gradually supplemented for infants (Eidelman, 2012). Therefore, the process of adaptation to solid foods is a challenging period for mothers to reconcile breastfeeding and work. By the tenth month, most infants typically adapt to solid foods, allowing more flexibility for the mother in breastfeeding. The correspondence between our estimates and the dynamics of substitution between breastfeeding and work supports the view that the delay in the return to work in the November 2003 birth cohort emanates from an augmented tendency to breastfeed.

The month-to-month analysis post-Remedia Event reveals a consistent decrease in the return-to-work rate from the third to ninth month post-childbirth by approximately three percentage points (pp) monthly. This shift reflects a marked positive effect on maternity leaves. Notably, in the third month, about 50% of mothers return to work. The observed effect of -3.7 pp. implies a 7% increase in mothers delaying their return to work. By the sixth month, where typically 85% of mothers have returned to work, the observed decrease (-2.6pp) suggests that an additional 17% of the remaining 15% of women choose to prolong their leave. In the ninth month, the decrease (-2.6pp) accounts for an extra 30% of the remaining 10% of mothers. Therefore, the proportion of women delaying their return to work remains relatively stable in percentage points, but it represents an increasing percentage of those still on maternity leave during this period. This pattern suggests that some mothers significantly extended their maternity leaves rather than a scenario where women initially planning longer leaves are predominantly driving the observed trend. Therefore, the one-week average reduction in employment of women who gave birth in November 2003 appears to mask more complex dynamics.

## 3.5 Heterogeneity

We explore the heterogeneity in the impact of the Remedia Event across different subgroups of mothers, focusing on three key dimensions of potential heterogeneity. First, we consider the mother's age, positing that younger, less experienced first-time mothers may exhibit a more pronounced response. Second, we explore the child's gender, informed by literature that underscores parental preferences for a child's sex (Biswas et al. (2023)) and documents gender disparities in parental time investments (Baker and Milligan, 2016). Lastly, we examine household income, since higher-income families generally experience fewer financial restrictions and have better access to information, which could influence

their labor market decisions post-event.

The results of this heterogeneity investigation are detailed in Table 3. We introduce a set of categorical variables, $Char$, coded as 1 for mothers above the median age (Panel A), or male children (Panel B), or households above the median income (Panel C). The analysis aims to discern whether these characteristics mediate the event's influence on the duration of maternal employment post-delivery. The first two rows of each panel of the table show the average effect analysis (Equation 2) and the subsequent four rows present the disaggregated analysis (Equation 3). Columns 1 & 2 report the results for $Char = 1$, and Columns 3 & 4 report the results for $Char = 0$, where the even columns include controls for household characteristics. Column 5 of the table presents the differential effect between the two categories of $Char$ derived from a fully interacted triple difference version of the relevant equation.

**Impact by mother's age.** Panel A of Table 3 segments the results by mother's age, with the first two rows presenting the average effect. For mothers above the median age, the results (Columns 1 & 2) show an insignificant effect. In contrast, younger mothers (Columns 3 & 4) exhibit a significant decrease in employment duration by about 0.13 months (0.14 with controls). However, the differential impact, shown in Column 5, is not statistically significant. The disaggregated analysis shown in rows 3-6 of Panel A reveals an insignificant employment decrease for above-median age mothers (Columns 1 & 2), whereas younger mothers (Columns 3 & 4) experience a significant immediate effect of 0.28 months, with a subsequent effect of 0.1 months (0.11 with controls). Despite the insignificance of group differences (Column 5), these results hint at a stronger impact on younger mothers.

**Impact by child's gender.** Panel B of Table 3 considers child gender. The average effect analysis (first two rows) indicates no substantial differences between groups, with negligible and insignificant differential estimates. The disaggregated analysis shows a significant immediate effect of 0.27 months decrease for boys (0.26 with controls) compared to an insignifficant immidiate effect for girls, though the differential impact is insignificant. Neither group shows significant subsequent effects. Overall, there is no evidence for a strong differential response by child gender, though the immediate effect seems stronger for boys.

**Impact by household income** Panel C of Table 3 examines the effect across different income levels. The average effect suggests a significant decrease of 0.16 in employment for high-income households, with a significant differential impact of 0.14 across the income groups. Disaggregating the effect reveals a significant immediate impact in both income

groups, with a reduction of 0.25 months (0.23 with controls) for high-income households and 0.18 months for low-income households, but insignificant differences across the income groups. The subsequent effect shows a notable significant difference of 0.16 across the income groups. This indicates that the average effect's disparity arises primarily from the differential subsequent response rather than the immediate effect.

## 3.6 Specification and placebo tests

In this section, we conduct various tests to validate the robustness of our results. Our findings show that the main results hold across these tests. Specifically, we observe no evidence that our findings are driven by the selection of women with coded maternity leave. Additionally, a placebo test centered around a fictitious event in November 2002 yields statistically insignificant outcomes. Similarly, shifting the analysis to August 2003 reveals no response before the November 2003 birth cohort, supporting the interpretation that breastfeeding initiation is the primary response channel to the Remedia Event.

**Omission of teachers from the sample.** As we explained above, we do not observe the maternity leaves of some of the women, mostly teachers, whose maternity leave is coded in the data as months of work as they continue to receive their income. One concern is that this selection out of our sample somehow coincides with the Remedia Event and influences our results. To alleviate this concern, we run a regression akin to Equation (1) on the first-time mothers sample with an indicator for not having a record of maternity leave or for being in the education sector. We find no indication that omission of these mothers drives our results (Appendix Figure A.4).

**Placebo event in 2002.** To further validate the results, we run a placebo test around a fictitious event in November 2002. Appendix Figure A.3 and Table A.5 report the results of this exercise, showing all estimates as insignificant.

**Women who delivered before the event.** As we noted above, our approach potentially misses the response of women who delivered before the event but were still actively breastfeeding during its onset. Specifically, mothers who delivered in August 2003 were three months after delivery when the Remedia Event occurred, just at the point when most mothers return to work. Therefore, these mothers could have theoretically been influenced by the Remedia Event. We explore this issue by running a specification of Equation 1, moving the treatment time back to August 2003. We report the results in Appendix Figure A.2 and Table A.4. This specification shows no indication of response

before the November 2003 birth cohort, suggesting that breastfeeding *initiation* is the main response channel to the Remedia Event.

## 3.7 Summary

In summary, the results indicate a notable extension in maternity leaves taken by first-time mothers, particularly for births occurring in November 2003 shortly after the event. Consistent with breastfeeding patterns, the delay in return to work was pronounced in the first nine months after giving birth. The immediate impact of the Remedia Event was slightly more pronounced among younger mothers and when the child was a boy. The subsequent impact was substantial among high-income households.

# 4 The impact of the Remedia Event on utilization of milk pumps

In this section, we want to establish our premise that following the Remedia Event "food scare," first-time mothers opted for breastfeeding over baby formula, increasing the overall tendency to breastfeed. While our aim is to directly quantify the event's influence on breastfeeding choices, we are constrained by the absence of specific breastfeeding data from Israel during that time. To indirectly infer the event's impact, we examine trends in milk pump borrowing, drawn from data provided by Israel's leading free medical equipment lender. Breast milk pumps are widely used by breastfeeding mothers for various reasons. It is common to use milk pumps to provide milk for the baby when mothers are away. Others use them in case of breastfeeding difficulties.[26] The use of milk pumps among breastfeeding mothers is expected to increase with a greater tendency towards breastfeeding.

## 4.1 Data

We draw on data from Yad-Sarah, a prominent Israeli non-profit organization known for freely lending medical and rehabilitative home-care equipment, including items like wheelchairs and walking canes. Notably, Yad Sarah is the country's largest lender of milk pumps.

[26] According to Fein et al. (2008) 84% of breastfeeding mothers continue breastfeeding after returning to work, most often by pumping milk at work.

We examine the monthly counts of devices lent from 26 Yad Sarah branches throughout Israel from May 2003 to April 2004, a twelve-month period encompassing the Remedia Event.[27] We count the number of milk pumps lent, which form our treatment group, relative to several other frequently borrowed items that serve as our comparison group. The descriptive statistics for this dataset are presented in Appendix Table A.7. Column (1) provides the mean items borrowed per month per branch and column (2) provides the standard deviation. On average, 26 milk pumps were borrowed per month in each of the 26 branches of Yad-Sara in the data, summed to about 680 milk pumps lent across Israel per month, with total milk pump lending numbers approaching a significant count of eight thousand during the year of our sample period. During this period, about twelve thousand wheelchairs, five thousand walking canes, and two thousand bassinets were borrowed from Yad-Sara.

## 4.2 Empirical analysis

We use a dynamic DD model akin to the one we specified in Equation 1 with some modifications

$$y_{jit} = \alpha + \sum_{\tau \neq -1, \tau = -6}^{5} (\gamma_\tau + \delta_\tau \cdot TR) \cdot M_\tau + X_j * K_i \cdot \beta + \epsilon_{jit} \tag{4}$$

Our outcome variable $y_{itj}$ is a count of a product category $i$ (e.g., milk pumps or wheelchairs) that was lent in branch $j$ in month $t$. $TR$ designates the treatment category – milk pumps and equals 0 for the other product categories. $X_j$ and $K_i$ are indicators for branch and product categories, respectively, i.e., we include a fixed effect for each category in every branch. We focus on $\delta_\tau$, the indicators of the differential monthly effect of milk pumps borrowed relative to other product categories.

We report the results in Figure 3 (and Appendix Table A.8). The indicators for the months before the Remdia Event are all insignificantly different from zero. The indicator for November 2003 is positive and significant, followed by insignificant coefficients in the subsequent months, suggesting that upon the Remedia Event, there was an immediate increase in milk pump borrowing. Specifically, given that the average number of milk pumps borrowed per month per branch is about 26, an increase of roughly 3.4 pumps per branch in November 2003 reflects a rise of 13% in milk pump borrowing relative to the other product categories. While the results are quite noisy, they provide some support to

[27] We have excluded data from four branches located within hospitals. This exclusion is based on the rationale that equipment lending in hospital settings may be influenced by hospital-specific conditions rather than reflecting independent individual choices. This exclusion does not impact the results.

the view that the extended maternity leaves in November 2003 we document, are driven by breastfeeding initiation.

# 5 Conclusion

In this study, we analyze the consequences of the Remedia Event, a realization of a product safety hazard in the baby formula market in Israel in 2003. We find that following the Remedia Event, first-time mothers who gave birth in November 2003, close after the Remedia Event, exhibited an apparent delay in their return to work. These mothers worked in the first six months after delivery 11% less than they would, had the Remedia Event not occurred. This effect arises despite the three months of mandatory maternity leave and arguably might have been larger in a setting with a less generous maternity leave policy. Moreover, consistent with the notion that an increase in breastfeeding drove the response to the Remedia Event, we find an increase in the lending of milk pumps in November 2003.

By generating a positive shock to breastfeeding via a "food scare," the Remedia Event provided an opportunity to assess the substitution between breastfeeding and the return to work after childbirth of new mothers. Indeed, the results show that despite technological improvements and policy changes throughout the $20^{th}$ century that helped reconcile breastfeeding and work, an unexpected increase in breastfeeding led to a rise in the length of maternity leaves of first-time mothers.

Given the substantial presence of women of childbearing age in the labor force, their decisions regarding labor supply and breastfeeding carry significant policy implications, particularly since employers are unlikely to internalize the full benefits of breastfeeding.[28] Implementing lactation accommodation policies in the workplace, like mandating lactation rooms, private spaces and time schedule adjusted for milk pumping, and refrigeration facilities to store pumped milk, may help to alleviate the potential breastfeeding-work tradeoff after childbirth.

Furthermore, employment promotion and gender-equality policies like paternal leave rights may prove more beneficial in promoting equity and return to work of new mothers if combined with measures that help reconcile breastfeeding and work. Current research shows that even when couples can freely share parental leave, the proportion of fathers taking leave remains relatively low (Kleven et al., 2024; Cools et al., 2015). We posit that

[28] On the benefits of breastfeeding for cognitive development of children see (Fitzsimons and Vera-Hernández, 2022).

breastfeeding may play a significant role in this disparity.[29] This suggests that policies aimed at supporting breastfeeding mothers in the workplace, alongside efforts to encourage paternal leave-taking, could be crucial in addressing the persistent gender imbalance in childcare responsibilities and career trajectories.

The recent infant formula shortage in the United States has sparked diverse viewpoints in the news and social media.[30] While some advocated for breastfeeding as an alternative to mitigate the shortage, others highlighted the challenges it presents, particularly for working mothers (Pearson, 2022; Sandoval et al., 2022). Our results inform this ongoing discussion, shedding light on the intricate interplay between breastfeeding practices and maternal employment.

The use of tax data in our study allows us to examine the extensive margin of employment (the decision to return to work). Nevertheless, one limitation of our study is that this data source does not capture intensive margin decisions, such as transitioning to part-time work or remote work (or other work styles that augment temporal flexibility in the workplace), which may be more closely linked to breastfeeding choices than the initial return to work. Furthermore, our analysis primarily focuses on dual-earner households, which is appropriate for studying paternal leave rights, though this may limit the direct applicability of our results to other household types, such as single-earner families or those headed by single parents. Lastly, our study relies on indirect measures of breastfeeding, specifically the borrowing of breast pumps, as direct breastfeeding data for this period is unavailable. Access to more comprehensive data on breastfeeding practices and working hours, coupled with an appropriate research setting, could significantly advance our understanding of this issue.

[29] While Bartel et al. (2018) demonstrate that introducing gender-neutral paid family leave (PFL) increases paternal leave-taking, and exclusive paternity leave leads to higher take-up among fathers' peers (Dahl et al., 2014; Cools et al., 2015), fathers' overall leave utilization remains substantially lower than that of mothers.

[30] In February 2022, a large-scale recall of several baby formula products was issued in the US due to contamination concerns that may have caused at least two infant deaths. The recall was followed by a shutdown of production at Sturgis, the largest plant of Abbott Labs, exacerbating pandemic supply-chain-related shortages. In May, nationwide out-of-stock rates were about 40%.

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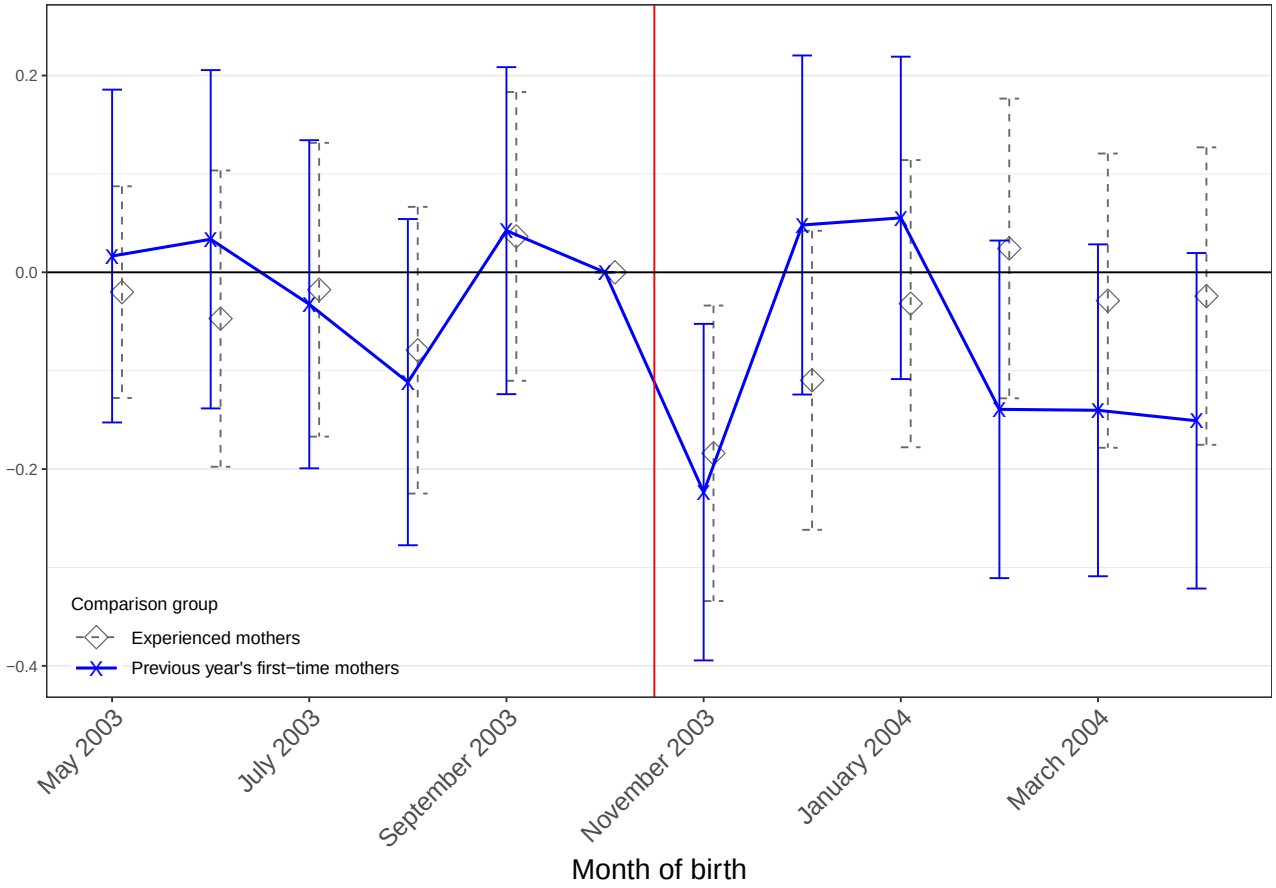
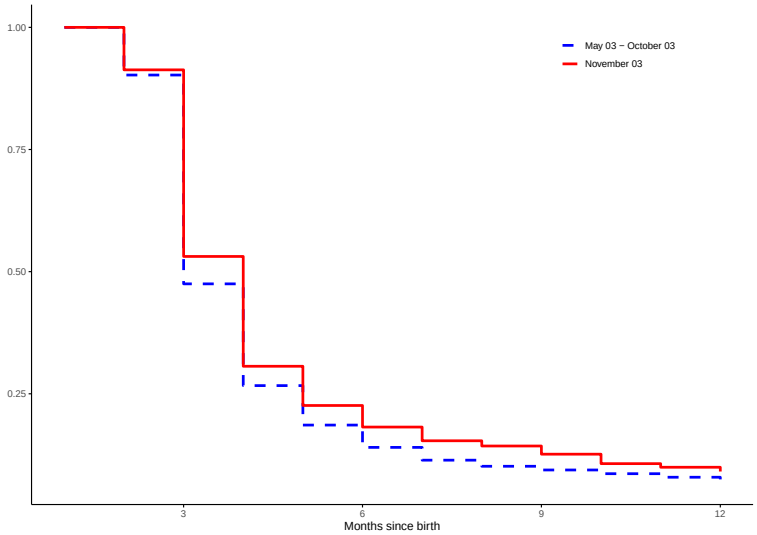
Figure 1: The impact of the Remedia Event on months worked within six months of childbirth



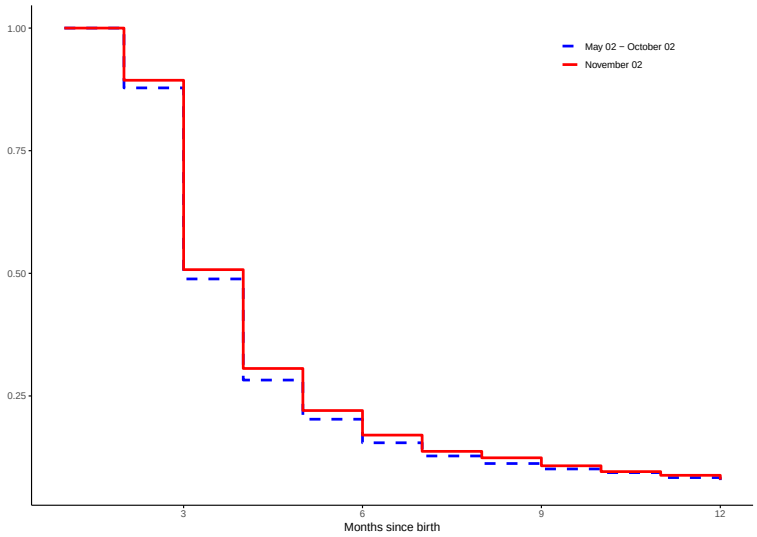

**Note:** This figure outlines the regression results corresponding to Equation 1, using the number of months worked within the first six months post-delivery as the outcome variable. The x-axis denotes the month of birth for the treatment group. The blue crosses represent first-time mothers relative to those from the previous year, with their 95% confidence intervals. The grey diamonds display coefficients from an additional regression, where experienced mothers serve as an alternative comparison group; their 95% confidence intervals are represented by dashed lines. The vertical red line positioned just before November 2003 indicates the timing of the Remedia Event. Detailed numeric values corresponding to this visual representation can be found in Table A.1. The applicable base rate for these estimates is derived from women in the control group during the pre-period, who, on average, worked two months within the six months following childbirth.

Figure 2: The likelihood of being on maternity leave after childbirth

(a) Around the Remedia Event

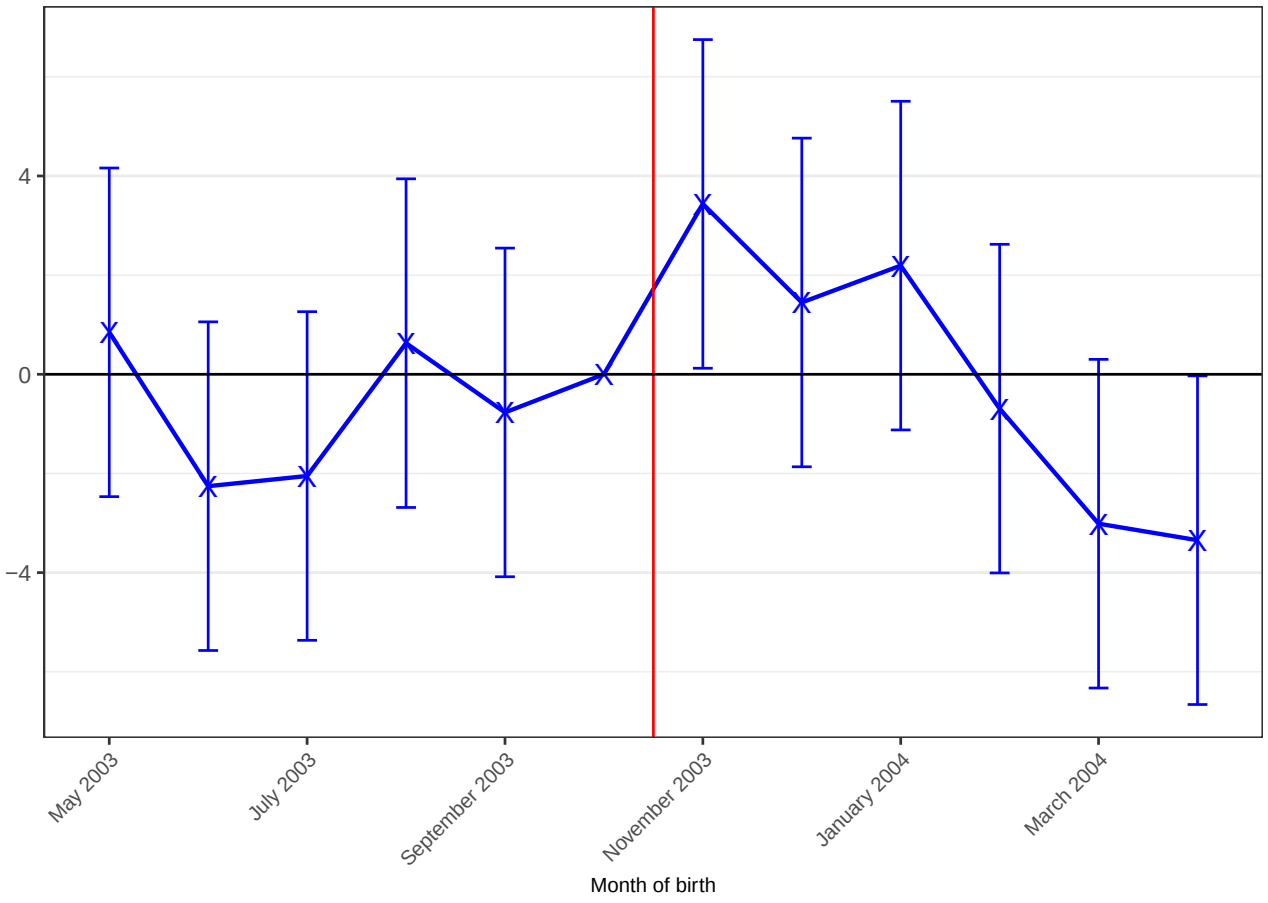



(b) Prior year




**Note:** The figure depicts the likelihood of being on maternity leave in the first twelve months after childbirth. Panel (a) denotes the period around the Remedia Event. The red solid line provides the survival curve for mothers who gave birth in November 2003. The blue dashed line describes the survival curve for mothers who delivered before the Remedia Event, in May-October 2003. Panel B provides that corresponding survival curves in the prior year, respectively.

Figure 3: The impact of the Remedia Event on the number of milk pumps lent




**Note:** This figure shows the regression results corresponding to Equation (4). The blue crosses represent the number of milk pumps lent relative to other commonly lent items (walking canes, wheelchairs, and bassinets), with their 95% confidence intervals. The vertical red line positioned just before November 2003 indicates the timing of the Remedia Event. Detailed numeric values corresponding to this visual representation can be found in Table A.8.

Table 1: Descriptive statistics, first-time mothers sample

| | Treatment: around Remedia | | | Comparison: prior year | | | DD |
|---|---|---|---|---|---|---|---|
| | Pre | Post | Diff | Pre | Post | Diff | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Share baby male | 0.50 | 0.50 | -0.00 | 0.47 | 0.49 | 0.02 | -0.02 |
| | ( 0.50) | ( 0.50) | ( 0.01) | ( 0.50) | ( 0.50) | ( 0.01) | ( 0.01) |
| Wife's age at childbirth | 27.88 | 28.14 | 0.26 | 27.82 | 27.90 | 0.08 | 0.18 |
| | ( 3.55) | ( 3.48) | ( 0.07) | ( 3.60) | ( 3.50) | ( 0.07) | ( 0.10) |
| Share wife Jewish | 0.94 | 0.95 | 0.01 | 0.92 | 0.92 | -0.00 | 0.01 |
| | ( 0.24) | ( 0.22) | ( 0.00) | ( 0.27) | ( 0.27) | ( 0.01) | ( 0.01) |
| Share wife native | 0.83 | 0.82 | -0.00 | 0.83 | 0.82 | -0.00 | -0.00 |
| | ( 0.38) | ( 0.38) | ( 0.01) | ( 0.38) | ( 0.38) | ( 0.01) | ( 0.01) |
| Wife's monthly income - nominal | 6,567.22 | 6,364.50 | -202.72 | 6,549.99 | 6,385.46 | -164.53 | -38.20 |
| | ( 4,074.23) | ( 3,967.99) | ( 81.56) | ( 4,392.01) | ( 4,232.47) | ( 85.47) | ( 118.12) |
| Wife's monthly income - real | 6,256.32 | 5,930.20 | -326.13 | 6,478.72 | 5,978.90 | -499.83 | 173.70 |
| | ( 3,878.72) | ( 3,696.78) | ( 76.86) | ( 4,344.22) | ( 3,962.99) | ( 82.38) | ( 112.65) |
| Husband's age at childbirth | 30.48 | 30.62 | 0.14 | 30.43 | 30.47 | 0.04 | 0.10 |
| | ( 4.15) | ( 4.01) | ( 0.08) | ( 4.26) | ( 4.11) | ( 0.08) | ( 0.12) |
| Share husband Jewish | 0.94 | 0.95 | 0.01 | 0.92 | 0.92 | -0.00 | 0.01 |
| | ( 0.24) | ( 0.22) | ( 0.00) | ( 0.27) | ( 0.27) | ( 0.01) | ( 0.01) |
| Share husband native | 0.83 | 0.83 | 0.00 | 0.83 | 0.84 | 0.00 | 0.00 |
| | ( 0.37) | ( 0.37) | ( 0.01) | ( 0.37) | ( 0.37) | ( 0.01) | ( 0.01) |
| Husbands's monthly income - nominal | 9,328.01 | 9,203.50 | -124.51 | 9,566.24 | 9,222.69 | -343.55 | 219.04 |
| | ( 6,897.48) | ( 6,679.34) | ( 137.70) | (11,848.95) | ( 6,893.72) | ( 191.81) | ( 236.55) |
| Husband's monthly income - real | 8,890.59 | 8,576.06 | -314.54 | 9,462.15 | 8,635.48 | -826.68 | 512.14 |
| | ( 6,577.16) | ( 6,223.31) | ( 129.88) | (11,720.03) | ( 6,454.79) | ( 187.19) | ( 228.28) |
| Observations | 4,961 | 4,773 | 9,734 | 5,060 | 5,124 | 10,184 | 19,918 |

**Note:** This table provides the descriptive statistics of the first-time mothers sample. Columns (1)-(2) summarize the characteristics of the pre- and post-groups in the year around the Remedia Event, and column (3) reports the difference between them. Columns (4)-(6) summarize the characteristics of the respective groups in the prior year, and the difference between them, and column (7) reports the differences-in-differences between the means of the four groups.

Table 2: The impact of the Remedia Event on months worked within six months of childbirth, two comparison groups

| | First-time prior year | | Experienced mothers | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **A. Average effect** | | | | |
| Post X TR | -0.0787* | -0.0785* | -0.0415 | -0.0373 |
| | (0.0357) | (0.0347) | (0.0260) | (0.0254) |
| **B. Immediate and subsequent effect** | | | | |
| Immediate X TR | -0.2147** | -0.1748** | -0.1654** | -0.1538** |
| | (0.0699) | (0.0676) | (0.0579) | (0.0564) |
| Subsequent X TR | -0.0526 | -0.0600 | -0.0166 | -0.0136 |
| | (0.0372) | (0.0363) | (0.0278) | (0.0272) |
| HH Characteristics | No | Yes | No | Yes |
| Observations | 19,918 | 19,918 | 55,521 | 55,521 |

**Note:** This table presents the DD estimates of the average effect (Equation (2)) and immediate and subsequent effects (Equation (3)) of the Remedia Event. The analysis is conducted using two different comparison groups: first-time mothers from the preceding year (shown in columns 1 and 2) and experienced mothers (presented in columns 3 and 4). Standard errors are computed using Huber-White heteroscedasticity correction. Statistical significance is denoted by asterisks: one asterisk (*) indicates significance at the 5% level and two asterisks (**) represent significance at the 1% level.

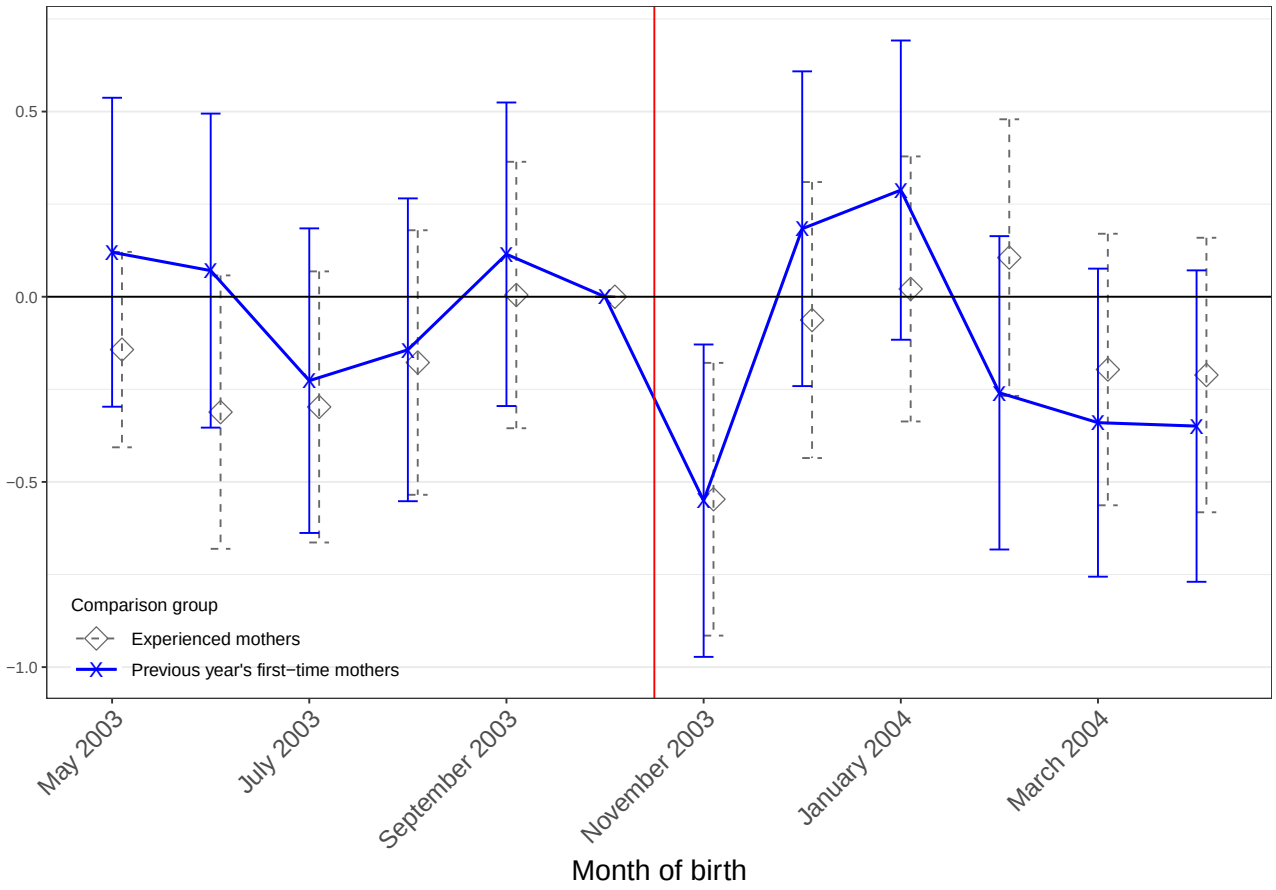
Table 3: The impact of the Remedia Event on months worked within six months of birth, heterogeneity across mother subgroups

| | Char = 1 | | Char = 0 | | Diff |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **A. Mother's age** | | | | | |
| Post X TR | -0.0264 | -0.0274 | -0.1295* | -0.1421** | 0.1031 |
| | (0.0505) | (0.0497) | (0.0505) | (0.0495) | (0.0714) |
| Immediate X TR | -0.1474 | -0.1401 | -0.2784** | -0.2840** | 0.1310 |
| | (0.0999) | (0.0980) | (0.0979) | (0.0958) | (0.1399) |
| Subsequent X TR | -0.0035 | -0.0060 | -0.1006 | -0.1146* | 0.0971 |
| | (0.0527) | (0.0519) | (0.0528) | (0.0518) | (0.0746) |
| Observations | 10,088 | 10,088 | 9,830 | 9,830 | 19,918 |
| **B. Baby's sex** | | | | | |
| Post X TR | -0.0809 | -0.0834 | -0.0742 | -0.0794 | -0.0067 |
| | (0.0509) | (0.0501) | (0.0501) | (0.0493) | (0.0714) |
| Immediate X TR | -0.2656** | -0.2582** | -0.1617 | -0.1694 | -0.1039 |
| | (0.0985) | (0.0962) | (0.0991) | (0.0981) | (0.1398) |
| Subsequent X TR | -0.0455 | -0.0499 | -0.0574 | -0.0620 | 0.0119 |
| | (0.0531) | (0.0524) | (0.0523) | (0.0515) | (0.0745) |
| Observations | 9,720 | 9,720 | 10,198 | 10,198 | 19,918 |
| **C. HH income** | | | | | |
| Post X TR | -0.1583** | -0.1568** | -0.0140 | -0.0344 | -0.1443* |
| | (0.0526) | (0.0515) | (0.0484) | (0.0475) | (0.0715) |
| Immediate X TR | -0.2470* | -0.2288* | -0.1850* | -0.1821* | -0.0620 |
| | (0.1052) | (0.1029) | (0.0932) | (0.0909) | (0.1406) |
| Subsequent X TR | -0.1416** | -0.1432** | 0.0203 | -0.0047 | -0.1619* |
| | (0.0548) | (0.0537) | (0.0507) | (0.0497) | (0.0746) |
| Observations | 9,085 | 9,085 | 10,833 | 10,833 | 19,918 |
| HH Characteristics | No | Yes | No | Yes | No |

**Note:** This table presents our heterogeneity analysis. The variable $Char = 1$ ($Char = 0$) represents mothers above (below) the median age (panel A), male (female) children (panel B), and households with income above (below) the median threshold (panel C). The first two rows in each panel display the average effect analysis (Equation 2), and the subsequent four rows detail the disaggregated analysis (Equation 3). Results for $Char = 1$ are reported in columns 1 & 2, and for $Char = 0$ in columns 3 & 4. The even-numbered columns include controls for household characteristics. Column 5 reports the differential effect between the categories of $Char$ derived from a fully interacted triple difference version of the relevant equation. Standard errors are calculated using the Huber-White method to correct for heteroscedasticity. Statistical significance is denoted by asterisks: one asterisk (*) signifies significance at the 5% level and two asterisks (**) at the 1% level.

# A Appendix

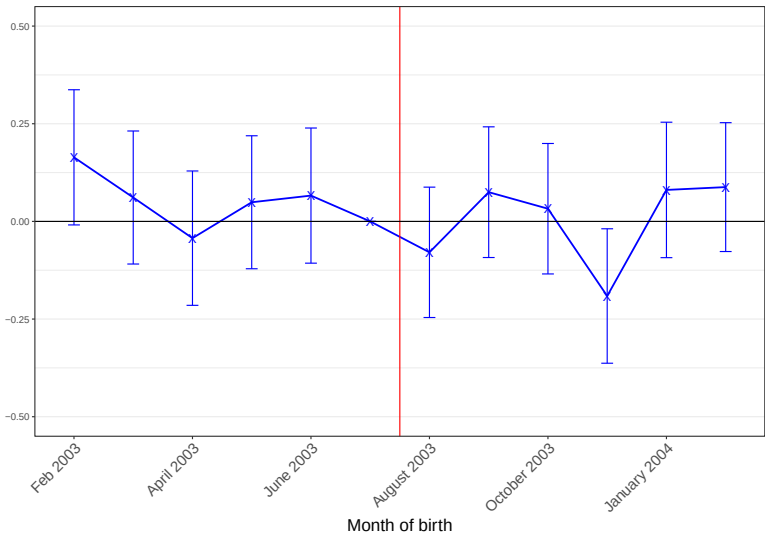
Figure A.1: The impact of the Remedia Event on months worked within twelve months of childbirth, two comparison groups



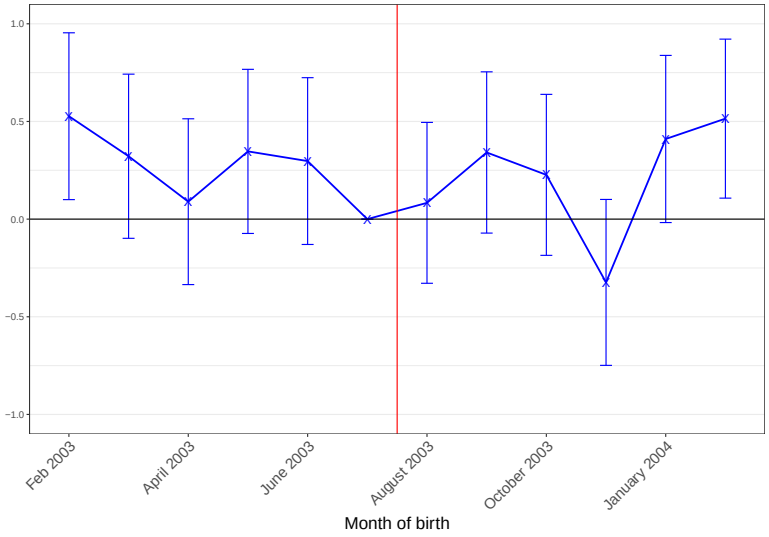

**Note:** This figure outlines the regression results corresponding to Equation (1), using the number of months worked within the first twelve months post-delivery as the outcome variable. The x-axis denotes the month of birth for the treatment group. The blue crosses represent first-time mothers relative to those from the previous year, with their 95% confidence intervals. The grey diamonds display coefficients from an additional regression, where experienced mothers serve as an alternative comparison group; their 95% confidence intervals are represented by dashed lines. The vertical red line positioned just before November 2003 indicates the timing of the Remedia Event. Detailed numeric values corresponding to this visual representation can be found in Table A.2. The applicable base rate for these estimates is derived from women in the control group during the pre-period, who, on average, worked 6.4 months within the twelve months following childbirth.

Figure A.2: The impact of the Remedia Event on months worked after childbirth, shifting the event to August 2003

(a) Months worked six months within childbirth

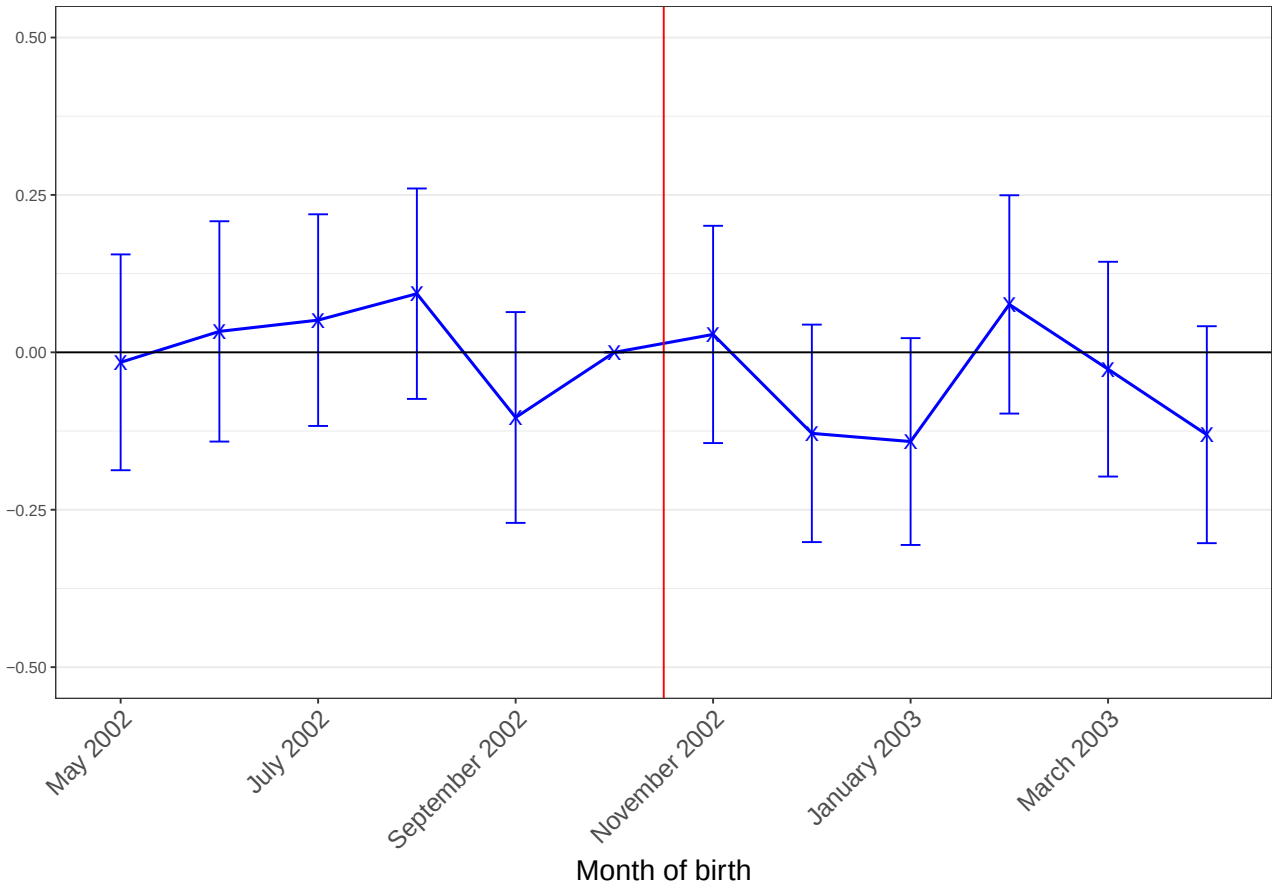



(b) Months worked twelve months within childbirth




**Note:** This figure displays the regression results as outlined in Equation (1). The vertical red line, positioned just before August 2003, signifies our redefinition of the event month from November 2003 to August 2003 for this specification test. Panels (a) and (b) show the average months worked within the first six months and twelve months post-delivery, respectively. The blue crosses in the figure represent first-time mothers compared to those from the preceding year, with their 95% confidence intervals. Detailed numeric values corresponding to this visual representation can be found in Table A.4.

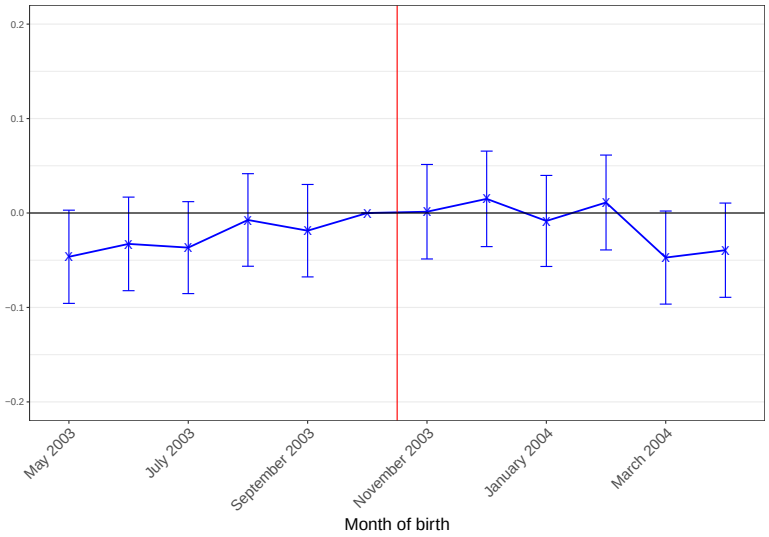
Figure A.3: Placebo test: the impact of a *fictitious* event around November 2002 on months worked after childbirth



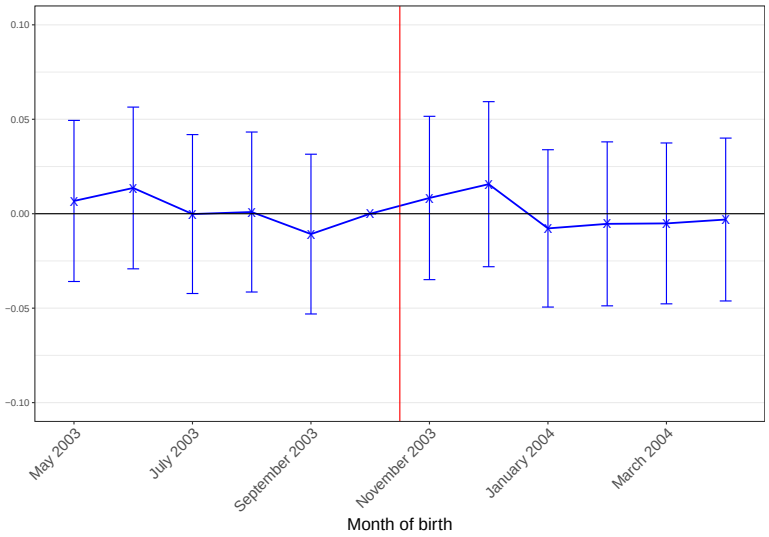

**Note:** This figure displays the regression results as outlined in Equation (1). The vertical red line is positioned just before November 2002, marking the fictitious reform for this placebo exercise. The outcome variable analyzed here is the number of months worked within the first six months post-delivery. The blue crosses represent first-time mothers compared to those from the preceding year with their 95% confidence intervals. Detailed numeric values corresponding to this visual representation can be found in Table A.5.

Figure A.4: Sample selection: mothers without coded maternity leave

(a) Outcome variable: not having a coded maternity leave




(b) Outcome variable: belonging to the education sector




**Note:** The figure reports the results of a selection into the sample exercise. Panels (a) and (b) present the coefficients from a regression akin to Equation (1). In panel (a) the outcome we examine is an indicator for not having a coded maternity leave and in panel (b) it is an indicator for belonging to the education sector. The blue vertical lines denote the 95% confidence intervals. The vertical red line, placed just before November 2003, marks the timing of the Remedia Event.

Table A.1: The month-to-month impact of the Remedia Event on months worked within six months of birth, two comparison groups

| | First-time prior year | Experienced mothers |
|---|---|---|
| | (1) | (2) |
| May 2003 | 0.0165 | -0.0202 |
| | (0.0863) | (0.0549) |
| June 2003 | 0.0336 | -0.0471 |
| | (0.0877) | (0.0768) |
| July 2003 | -0.0325 | -0.0177 |
| | (0.0851) | (0.0762) |
| August 2003 | -0.1116 | -0.0792 |
| | (0.0846) | (0.0743) |
| September 2003 | 0.0423 | 0.0365 |
| | (0.0848) | (0.0749) |
| October 2003 | 0.0000 | 0.0000 |
| | (0) | (0) |
| November 2003 | -0.2235* | -0.1840* |
| | (0.0873) | (0.0766) |
| December 2003 | 0.0481 | -0.1098 |
| | (0.0879) | (0.0775) |
| January 2004 | 0.0553 | -0.0319 |
| | (0.0836) | (0.0745) |
| February 2004 | -0.1393 | 0.0242 |
| | (0.0875) | (0.0777) |
| March 2004 | -0.1403 | -0.0288 |
| | (0.0861) | (0.0763) |
| April 2004 | -0.1509 | -0.0242 |
| | (0.087) | (0.0771) |
| Observations | 19,918 | 55,521 |

**Note:** This table presents the results of the dynamic DD regression analysis as defined in Equation (1). It uses the average months worked within six months post-delivery as the outcome variable. The analysis is conducted using two different comparison groups: first-time mothers from the preceding year (shown in column 1), and experienced mothers (presented in column 2). Significance is denoted by asterisks: one asterisk (*) indicates significance at the 5% level and two asterisks (**) represent significance at the 1% level.

Table A.2: The month-to-month impact of the Remedia Event on months worked within twelve months of birth, two comparison groups

| | First-time prior year | Experienced mothers |
|---|---|---|
| | (1) | (2) |
| May 2003 | 0.1201 | -0.1427 |
| | (0.2128) | (0.1346) |
| June 2003 | 0.0706 | -0.3115 |
| | (0.2163) | (0.1883) |
| July 2003 | -0.2266 | -0.2975 |
| | (0.2098) | (0.1867) |
| August 2003 | -0.1433 | -0.1776 |
| | (0.2086) | (0.1822) |
| September 2003 | 0.1147 | 0.0047 |
| | (0.2091) | (0.1835) |
| October 2003 | 0.0000 | 0.0000 |
| | (0) | (0) |
| November 2003 | -0.5506* | -0.5467** |
| | (0.2152) | (0.1879) |
| December 2003 | 0.1837 | -0.0629 |
| | (0.2168) | (0.1901) |
| January 2004 | 0.2879 | 0.0211 |
| | (0.2061) | (0.1826) |
| February 2004 | -0.2594 | 0.1057 |
| | (0.2158) | (0.1905) |
| March 2004 | -0.3400 | -0.1965 |
| | (0.2122) | (0.187) |
| April 2004 | -0.3492 | -0.2114 |
| | (0.2145) | (0.1891) |
| Observations | 19,918 | 55,521 |

**Note:** This table presents the results of the dynamic DD regression analysis as defined in Equation (1). It uses the average months worked within twelve months post-delivery as the outcome variable. The analysis is conducted using two different comparison groups: first-time mothers from the preceding year (shown in column 1), and experienced mothers (presented in column 2). Significance is denoted by asterisks: one asterisk (*) indicates significance at the 5% level and two asterisks (**) represent significance at the 1% level.

Table A.3: The likelihood of being on maternity leave after childbirth

| | (1) | (2) |
|---|---|---|
| Month 2 | 0.0049 | 0.0063 |
| | (0.0123) | (0.0122) |
| Month 3 | -0.0371 | -0.0309 |
| | (0.0205) | (0.0203) |
| Month 4 | -0.0160 | -0.0100 |
| | (0.0188) | (0.0184) |
| Month 5 | -0.0224 | -0.0175 |
| | (0.0169) | (0.0165) |
| Month 6 | -0.0260 | -0.0225 |
| | (0.0154) | (0.0151) |
| Month 7 | -0.0303* | -0.0277* |
| | (0.0142) | (0.0139) |
| Month 8 | -0.0298* | -0.0275* |
| | (0.0137) | (0.0134) |
| Month 9 | -0.0259* | -0.0238 |
| | (0.0130) | (0.0128) |
| Month 10 | -0.0185 | -0.0166 |
| | (0.0123) | (0.0121) |
| Month 11 | -0.0158 | -0.0141 |
| | (0.0118) | (0.0117) |
| Month 12 | -0.0151 | -0.0138 |
| | (0.0113) | (0.0111) |
| HH Characteristics | No | Yes |
| Observations | 13,277 | 13,277 |

**Note:** This table presents the results of a set of DD regressions corresponding to the survival analysis of the immediate effect of the Remedia Event. This breakdown allows for a detailed month-by-month analysis of the effect of the Remedia Event on the likelihood of mothers returning to work post-delivery. Each of the 11 rows corresponds to a specific month relative to childbirth, ranging from 2 to 12 months post-delivery. The outcome variable is a binary indicator, where one indicates being on maternity leave and zero represents working. Standard errors are calculated using the Huber-White method to correct for heteroscedasticity. Significance is denoted by asterisks: one asterisk (*) indicates significance at the 5% level, and two asterisks (**) represent significance at the 1% level.

Table A.4: The impact of the Remedia Event on months worked after childbirth, shifting the event to August 2003

| | Six months | Twelve months |
|---|---|---|
| | (1) | (2) |
| February 2003 | 0.1640 | 0.5269* |
| | (0.0883) | (0.2179) |
| March 2003 | 0.0612 | 0.3219 |
| | (0.0869) | (0.2144) |
| April 2003 | -0.0429 | 0.0892 |
| | (0.0877) | (0.2165) |
| May 2003 | 0.0490 | 0.3467 |
| | (0.0868) | (0.2143) |
| June 2003 | 0.0661 | 0.2972 |
| | (0.0883) | (0.2178) |
| July 2003 | 0.0000 | 0.0000 |
| | (0) | (0) |
| August 2003 | -0.0792 | 0.0833 |
| | (0.0851) | (0.2101) |
| September 2003 | 0.0748 | 0.3413 |
| | (0.0853) | (0.2106) |
| October 2003 | 0.0325 | 0.2266 |
| | (0.0852) | (0.2103) |
| November 2003 | -0.1910* | -0.3240 |
| | (0.0878) | (0.2167) |
| December 2003 | 0.0806 | 0.4102 |
| | (0.0885) | (0.2183) |
| January 2004 | 0.0878 | 0.5145* |
| | (0.0841) | (0.2077) |
| Observations | 19,871 | 19,871 |

**Note:** This table summarizes the DD estimates from Equation (1) for the specification test where the event month is shifted from November 2003 to August 2003 as illustrated in Figure A.2. The outcome varialbles in columns 1 and 2 are months worked within six and twelve months of childbirth, respectively. The comparison group for this analysis is first-time mothers from the previous year. Significance is denoted by asterisks: one asterisk (*) indicates significance at the 5% level and two asterisks (**) represent significance at the 1% level.

Table A.5: Placebo test, months worked within six months of childbirth around November 2002

| | Six months |
|---|---|
| | (1) |
| May 2002 | -0.0159 |
| | (0.0874) |
| June 2002 | 0.0332 |
| | (0.0893) |
| July 2002 | 0.0512 |
| | (0.0858) |
| August 2002 | 0.0931 |
| | (0.0853) |
| September 2002 | -0.1035 |
| | (0.0854) |
| October 2002 | 0 |
| | (0) |
| November 2002 | 0.0284 |
| | (0.088) |
| December 2002 | -0.1287 |
| | (0.0881) |
| January 2003 | -0.1417 |
| | (0.0838) |
| February 2003 | 0.0761 |
| | (0.0884) |
| March 2003 | -0.0267 |
| | (0.087) |
| April 2003 | -0.1308 |
| | (0.0879) |
| Observations | 19,867 |

**Note:** This table summarizes the DD estimates from Equation (1) for the placebo test where we analyze a fictitious event in November 2002 as illustrated in Figure A.3. The outcome varialble is months worked within six months of childbirth. The comparison group for this analysis is first-time mothers from the previous year. Significance is denoted by asterisks: one asterisk (*) indicates significance at the 5% level and two asterisks (**) represent significance at the 1% level.

Table A.6: The impact of the Remedia Event on selection into the sample

| | All | Teachers |
|---|---|---|
| | (1) | (2) |
| May 2003 | -0.0464 | 0.0068 |
| | (0.0252) | (0.0218) |
| June 2003 | -0.0327 | 0.0136 |
| | (0.0253) | (0.0218) |
| July 2003 | -0.0367 | -0.0002 |
| | (0.0248) | (0.0215) |
| August 2003 | -0.0074 | 0.0009 |
| | (0.025) | (0.0216) |
| September 2003 | -0.0187 | -0.0108 |
| | (0.025) | (0.0216) |
| October 2003 | 0 | 0 |
| | (0) | (0) |
| November 2003 | 0.0014 | 0.0083 |
| | (0.0255) | (0.0221) |
| December 2003 | 0.015 | 0.0156 |
| | (0.0258) | (0.0223) |
| January 2004 | -0.0084 | -0.0078 |
| | (0.0246) | (0.0212) |
| February 2004 | 0.0111 | -0.0054 |
| | (0.0256) | (0.0221) |
| March 2004 | -0.0472 | -0.0051 |
| | (0.0251) | (0.0217) |
| April 2004 | -0.0394 | -0.0031 |
| | (0.0255) | (0.022) |
| Observations | 25,504 | 25,504 |

**Note:** This table summarizes the DD estimates from the selection into the sample exercise presented in Figure A.4. The outcome variable in column 1 is an indicator for the absence of coded maternity leave, and in column 2, it indicates belonging to the education sector. First-time mothers from the previous year are used as the comparison group. One asterisk (*) denotes significance at the 5% level and two asterisks (**) indicate significance at the 1% level.

Table A.7: Descripitive statistics, item lender data

| | Mean per month per branch | SD |
|---|---|---|
| | (1) | (2) |
| Breast pump | 26 | 24 |
| Walking cane | 13 | 15 |
| wheelchair | 33 | 35 |
| Basinet | 6 | 6 |
| Observations | 312 | |
| Number of Branches | 26 | |

**Note:** This table provides descriptive statistics of the Yad Sarah data. Column 1 provides the mean items borrowed per month per branch. Column 2 is the standard deviation.

Table A.8: The impact of the Remedia Event on milk pump lending per branch

| | (1) |
|---|---|
| May 2003 | 0.85 |
| | (1.69) |
| June 2003 | -2.26 |
| | (1.69) |
| July 2003 | -2.05 |
| | (1.69) |
| August 2003 | 0.63 |
| | (1.69) |
| September 2003 | -0.77 |
| | (1.69) |
| October 2003 | 0.00 |
| | (0) |
| November 2003 | 3.44* |
| | (1.69) |
| December 2003 | 1.45 |
| | (1.69) |
| January 2004 | 2.19 |
| | (1.69) |
| February 2004 | -0.69 |
| | (1.69) |
| March 2004 | -3.01 |
| | (1.69) |
| April 2004 | -3.35* |
| | (1.69) |
| Observations | 1,248 |

**Note:** This table summarizes the DD estimates of the impact of the Remedia Event on utilization of milk pumps (Equation (4)). One asterisk (*) denotes significance at the 5% level and two asterisks (**) indicate significance at the 1% level.