The effect of parental leave extensions on firms and coworkers *

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Abstract

This paper studies the effects of parental leave on firms by examining a 2002 Danish reform which increased the length of fully-compensated parental leave by 22 weeks. Because the policy change imposed no direct costs on firms, was retroactively applied and unanticipated, it offers a unique setting in which to study the effects of major expansions of paid parental leave. The expansion has a negative effect on firm survival and the retention of mothers. Perhaps surprisingly, there is no effect of the reform on coworker earnings in the short or long run, but there is evidence of stress on coworkers in other dimensions: coworkers who change jobs have lower earnings in the short run as a result of the policy change and some coworkers delay children.

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1 Introduction

While a large literature is devoted to understanding the impact of parental leave on children's outcomes and the careers of women, less is known about the consequences of parental leave at the workplace. Does prolonged, job-protected worker absence affect firms? I study the impact of parental leave extensions by taking advantage of a 2002 reform in Denmark which substantially increased the length of parental leave taken by women.

The reform hurt small firms. Firms employing women who were eligible for more leave, based on a birthdate cutoff which was retroactively applied (and so could not be manipulated), are 3 percentage points more likely to be shut down within five years of the reform. This effect declines (to zero) with firm size. Despite measurable impacts on survival, firms exposed to the reform do not change their subsequent hiring practices compared to other firms. Conditional on survival, there are not long run differences in the fraction of the firm which is female, the wage bill, or in the size of the firm.

To better understand how parental leave extensions affect firms and to separate the effects on incumbents relative to new hires, I study the outcomes of coworkers of women taking leave, following them even if they change firms. On average, there is no short or long run impact of the reform on coworker earnings, job-changes, or employment. The point estimates are fairly precise—I can reject yearly earnings changes greater than $300 (slightly more than 1% of baseline earnings) in the year following the leave event.[1]

The reform does shift the timing of coworker leave-taking. Parental leave taking in the year following the reform is substantially reduced among those coworkers exposed to more leave, but is higher 2-5 years after the reform. This is caused by some delayed fertility among coworkers exposed to more leave. In contrast, sick leave is slightly elevated in the years following the reform, and lower in subsequent years. Finally, the average earnings of job-changers are lower as a result of the reform. Though there are no wage effects on average, these patters are consistent with some strain on employees facing a long spell of coworker absence.

These effects are identified by studying a 2002 policy change in Denmark which increased total parental leave by 22 weeks. Since the reform was retroactively applied—it was enacted in March, 2002 and first discussed (without detail or dates) in November, 2001—women could not

[1] Notably, the pattern of effects of parental leave taking on coworker wages differ from the effect of death on coworker wages as documented in Jäger and Heining [2019].

time their births to gain access to the new parental leave scheme. My identification strategy compares the outcomes of firms employing women eligible for the reform to those ineligible for the reform, using past and future cohorts of women giving birth to identify and remove month-of-birth fixed effects. As a robustness check, I use a regression discontinuity difference in difference to identify the effect of the policy.

The parental leave reform that I study was combined with the elimination of another type of leave—childcare leave—which paid 60% of what parental leave paid but could (before 2002) be added on to parental leave for up to one year. The combination of these reforms has, in theory, an ambiguous effect on the amount of time women spent away from work after childbirth. Empirically, however, this reform of the leave system had the effect of immediately increasing new mother's time away from work after childbirth[2] by an average of almost seven weeks. That's not to say that all firms experienced an increase in leave taking of seven weeks—many women did not change their leave-taking behavior in response to the reform, but those who did respond responded quite substantially (by the full length of the extension, 22 weeks).

Conditional on being exposed to an increase in leave, firms were exposed to very large increases in leave. This reform is very unusual in that increases in parental leave of this size generally occur only when leave was introduced in Nordic countries, or first expanded. Norway, for example, had a major reform in 1977 which introduced paid leave of 18 weeks Carneiro et al. [2015]. Extensions after this were frequent but short (on average, 2 weeks long) and ended ten years before the reform studied here Dahl et al. [2016]. Sweden extended parental leave by three months in 1987 (Pylkknen and Smith [2004]). The Danish reform is relatively recent and, again, comparatively large. Sweden also extended leave in 2002, but by one month (Avdic and Karimi [2018]). Norway and Sweden have introduced four weeks (at a time) of paternity leave which is leave reserved for fathers.[3] These expansions of the leave system are interesting, particularly as employers may be more surprised by paternity leave taking relative to maternity leave taking, but again are shorter than the Danish expansion and potentially initially involve a great deal of learning about employer beliefs and norms (and so a lot of selection in who takes leave), as discussed in Dahl et al. [2014].

[2]time away from work is measured as the *sum* of childcare leave, parental leave, maternity leave, and sickness leave. If a new mother took 18 weeks of parental leave, one week of childcare leave, and one week of sickness leave (without gaps during which she returned to work), I count this as 20 weeks leave time away from work.

[3]For a discussion of the effects of these "daddy months" see Avdic and Karimi [2018] and Dahl et al. [2014], for example.

The Danish parental leave reform studied here is certainly larger than current policy proposals in the US for paid maternity leave. This paper sheds light on the effect of a large and surprising increase in leave time, but one which was added to an already generous leave program. Two pieces of this program deserve more emphasis. First, the program was retroactively applied, and truly a surprise to firms and women already on leave. This may then provide an upper bound on the costs to firms, since costs may be lower when firms are more able to anticipate the total leave time of employees. I will provide evidence (by looking at variation in the exact timing of leave-taking relative to the announcement of the program) that the reform is not less costly for firms with more notice. Second, this program is an extension of already generous parental leave. This makes it potentially less informative for the costs of an introduction of parental leave (as in the US). Of course, the US is the only OECD country without any, let alone generous, paid leave. Policies considered in other developed countries are typically expansions of already long leave. A final consideration is that this policy did not impose direct costs on employers and all costs reflect the costs of work interruptions, replacement frictions, uncertainty about the mother's eventual return to work, and changes in the return-to-work behavior of mothers.

This paper is not the first to study parental leave extensions in general. Past work, however, has focused on the effects of parental leave extensions on mothers' health, mothers' employment outcomes, and their children's health and education outcomes. I adopt a design extremely similar to Dustmann and Schönberg [2012] who study the effect of German maternity leave extensions on children's outcomes. Dustmann and Schönberg [2012], Rasmussen [2010], Baker and Milligan [2010], and Beuchert et al. [2016][4] find no evidence of health or education benefits from parental leave extensions in Germany, Denmark, and Canada. In a cross-country setting, Ruhm [2000] finds a positive association between leave generosity and infant health, and Rossin [2011] finds positive effects of job protection in the US on children's health. Consistent with this, Carneiro et al. [2015] find positive impacts of Norway's introduction of paid leave on leave on long-run child outcomes.

The literature on the impact of parental leave on mother's labor market outcomes is also somewhat divided. Rossin-Slater et al. [2013] find find positive impacts of California's paid leave on mother's hours worked. Lalive et al. [2014] study the effect of an Austrian reform

[4]Beuchert et al. [2016] study the 2002 Danish reform discussed here, and are the first to do so. Tô [2018] also studies the 2002 Danish reform in a more recent paper to investigate the possibility that women signal type by (not) taking up leave.

which extended maternity leave and job protection from one to two years. By 5 years after the reform there are no differences in mother's probability of working for the same employer or their employment in general. Turning to German extensions, Schönberg and Ludsteck [2014] also does not find substantial long run effects of maternity leave extensions. Comparing countries with different maternity leave schemes, Olivetti and Petrongolo [2017] find small or no effects of more generous leave on women's labor market outcomes. Rossin-Slater [2017] summarizes the literature as finding no effects of parental leave *extensions* on mother's labor market outcomes, but positive effects of the *introduction* of paid leave on labor market (and child health) outcomes. Bailey et al. [2019] studies the introduction of paid leave in California using administrative data and comes to a different conclusion concerning the introduction of leave, finding that for new mothers, paid leave reduced employment and lowered wages substantially 6-10 years after giving birth.

What has not been investigated in this large literature is the focus of this paper: the cost of parental leave extensions on firms' survival and coworkers' wages, though a couple of contemporaneous studies offer a complimentary investigations of these effects. Jäger and Heining [2019] studies the impact of sudden worker death on coworker wages to understand how substitutable coworkers are, using a matching design. While both death and parental leave result in long absences of workers from their firm, they differ in important ways. First, when a worker dies, their capital is lost, while women on leave may still be available to provide advice to their replacements. Second, when a woman goes on parental leave, the firm faces substantial uncertainty about her eventual return to work, and must offer her the same job upon her return to work. This process creates an additional friction for firm's responding to worker absence, relative to the case of death.

Friedrich and Hackmann [2017] study a reform which increased leave taking for Danish nurses, finding that while hospitals were largely able to substitute for the nurse shortage which resulted from the leave increase (though readmission rates rose), nursing homes were not able to do so and the reform ultimately led to a 13% long run rise in nursing home deaths. Another recent paper studies the effect of parental leave on firms and coworkers in Denmark: Brenøe et al. [2018] adopt the design of Jäger and Heining [2019] to study the impact of leave-taking on firm and coworker outcomes. Matching firms in which women take leave to those in which a woman does not take leave, they find no effects of leave on firm survival or coworker outcomes. The designs of this paper differs substantially from Brenøe et al. [2018] and allows for identification of different aspects of parental leave costs. Since leave-taking is generally planned and not a surprise, it may be difficult to measure the full cost of leave by comparing firms in which women did or did not take leave, but of course, this design has the advantage of capturing the cost of a full leave event, relative to a leave extension, as in this paper. All of these papers contribute to our understanding of an important policy parameter: workplace effects of policies which encourage long employee absence.

The remainder of this paper is organized as follows. Section 2 provides an overview of the policy change and response to the policy change. Section 3 discusses the data and Section 4 presents the results in more detail. Section 5 concludes.

2 Details of the reform

The details of this policy reform make it particularly interesting and ideal for policy evaluation. While on parental leave in Denmark, workers receive full compensation from the government up to a cap of about 3,000 kroner per week (or $2000/month). Except in the case of mass layoffs, workers taking parental leave are entitled to get their job (or an equivalent job) back after they return from leave. Before 2002, mothers were entitled to 18 weeks of full compensation, fathers to 4 weeks of full compensation, and couples could split an additional 10 weeks of parental leave at full compensation. This parental leave could be taken by either the mother or the father but was almost always taken by the mother. After January 1st, 2002, maternity leave remained the same, paternity leave was reduced by two weeks, but parental leave was extended from 10 to 32 weeks at full pay. Previously, parents could supplement parental leave time with something called childcare leave for 52 weeks, but this was only paid at 60% compensation.[5] Childcare leave could be taken anytime before the child was 8 years old.

Before the reform, Denmark had a decades-long history of parental leave support. The last reform to parental leave was in 1984 when 10 weeks of shared parental leave were added to the existing 18 weeks of maternity leave. Announced in 1993 and enacted in 1994, Denmark added one year of childcare leave which could be taken anytime before the child was 8 years old. Compensation for this leave was 90, then fell year by year to 80, then 70, and by 2001 to 60

[5]For a full description of the law, see Statistics Denmark Library (2016).

percent of total wages (Pylkknen and Smith [2004], Friedrich and Hackmann [2017], Tô [2018]).

The 2002 reform extended parental leave and took away childcare leave. This reform was enacted by a center right government—in late November of 2001, the Social Democrats did not win the most seats in parliament for the first time (Wikipedia contributors [2019]). In March, the government announced that it was eliminating childcare leave and replacing it with longer parental leave. The policy change was unanticipated and in fact was retroactively applied to children born after January 1st, 2002 despite being enacted on March 27th, 2002. Parents giving birth from January to March could choose whether they wanted the old scheme (including childcare leave) or the new scheme (Beuchert et al. [2016]). In principle, it's not clear that total leave time would go up under the new policy. Empirically, however, it increased substantially for many women.

I discuss the effect of the reform in three steps below. First, I show that the policy change was salient and take-up was immediate by plotting weeks of leave by child birthdate. Second, I show the distribution of leave taking for those giving birth before the eligibility threshold vs. those after. The patterns suggest that many women did not change leave taking (very high earning women were unaffected by the policy), but those women who did respond responded substantially, by more than the average suggested in the first stage. Finally, I discuss bunching in January births and the source of this bunching, which will inform the empirical design.

Figure 1 plots leave taken by date of birth around the January 1st, 2002 eligibility cutoff. There was no meaningful learning time about the policy. Women giving birth one day after the eligibility cutoff took approximately as much leave as women giving birth 200 days after the cutoff, and this was 7 weeks more than women giving birth before the cutoff. Appendix Figures 16 and 17 include placebo tests of this first stage: there is no discontinuity in leave taking for women giving birth in 2000 vs. 2001 or in 2002 v. 2003 (when no reforms took place).

The first-stage understates the potential impact of the policy on firms. While seven weeks away from the firm is a non-trivial extension, affected firms were likely exposed to substantially more time away than this. As Figure 2 shows, the reform extended the average length of leave, from a modal 28 weeks in 2001 to around 50 weeks in 2002.[6] Between 28 weeks and 50 weeks, the slope is approximately the same in the two years surrounding the reform, which suggests that there are a number of mothers who do not change their leave behavior in response to the

[6] Appendix Figure 18 shows the cumulative distribution of leave length

policy (perhaps because they kept the agreement they originally made with their firm despite the option to take more leave), but large fraction of mothers increase their leave substantially. While some firms in the sample were not affected by the reform and by parental leave taking in general, those firms which were affected by the reform were exposed to a substantial increase in leave-taking.[7]

The modal amount of leave taking changed quite substantially and the data are consistent with many women whose leave decision is not changed by the policy, but a substantial fraction of compliers who always take the maximum leave at full compensation and so change their leave by the full 22 week extension at full compensation. In other words, a substantial share of women simply take the maximum length of leave time available which is paid at full compensation, and do not take leave at partial compensation[8]

Any changes in the outcomes studied in this paper, such as firm shut-down and coworker earnings changes are likely not driven by uniform increases in absence from work, but rather by the largest changes in leave-taking (as large a 22 weeks). Though we cannot observe how much each individual woman increased her leave-taking by, it is clear from the histogram in Figure 2 that the average masks heterogeneity in leave-taking and that many women who did not take leave at partial compensation before the reform do take the full amount of leave after the reform.

3 Data

The data used in this paper come from linking administrative registers on the Danish population. Included in this are records of the birthdate of all children, as well as a unique identifier of the child's mother and father. Figure 3 below gives the number of births in each calendar month from 1998 through 2005. While there is a slight rise in the number of births in January relative to December, the rise is no different than in previous years and is largely explained by elective c-sections which are not scheduled during the holiday season—350 of the of the approximately 500 excess births can be explained by this phenomenon. The possibility of differences

[7]In the early 2000s, Danish fathers took very little of the shared leave. Appendix figure 23 displays the number of paternity leave spells by the length of the spell. Despite the fact that parental leave can be taken by either the mother or the father, almost all men took 0-2 weeks of leave both in 2001 and 2002. Fathers not taking leave are omitted from the graph, which is in part why the number of parental leave spells are about one third of the total births in Denmark.

[8] About 20% percent of women who are observed giving birth both before and after the reform used exactly the maximum leave time available at full compensation (did not take any childcare leave for the earlier birth).

in unobservables between those giving birth in late December and early January (in particular) leads me to an empirical specification described below which controls for seasonal differences using month-of-birth fixed effects.

It is possible to link the mother's ID with detailed data on her demographics and labor market information, such as firm id, yearly earnings, hourly wages[9], and occupation.[10] The firm id can be liked to the same information for all workers at the firm. In general, the Danish administrative data has two types of firm identifiers: establishment id which corresponds to a physical work address (plant) and firm id, which is the tax ID of the firm. Purely because establishment id is more commonly available, I use that variable in this study whenever possible. Unless otherwise noted, any mention of effects on the firm refer to an establishment, and coworker groups are constructed based on working in the same physical location.

To form the sample, I consider the firms employing women in November 2000 who gave birth between October 1st, 2001 and March 31st, 2002. A coworker is someone working with these women in November 2000. There is a focus on November 2000 simply because data on workers is collected in November of each tax year and not all women who give birth in late 2001 are working for their previous and subsequent employer in November 2001.

There are 7,925 firms in the dataset which employed one woman in November, 2000 who gave birth between October 1st, 2001 and March 31st 2002. There are 20,724 women who gave birth in the sample period employed in November, 2000, but I exclude those who work in extremely small firms (fewer than 5 employees) or who work in firms with multiple births because the design in this paper is not well suited to estimate effects in these multiple-birth firms. There are not significant differences between firms employing women who gave birth before vs. after the policy cutoff. Table 1 reports average age and fraction male in firms as well as firm size, by whether women giving birth in these firms were eligible for extended paid leave, controlling for month fixed effects. To remove month fixed effects, I extend the sample to analogous firms and mothers in six additional birth cohorts: October 1st, 1998-March 31st, 1999, October 1st, 1999-March 31st, 2000, October 1st, 2000-March 31st, 2001, October 1st, 2002-March 31st, 2003, October 1st, 2003-March 31st, 2004, and October 1st, 2004-March 31st, 2005. All mothers in the last three birth cohorts would have been eligible for extended parental

[9] Hours in Denmark are reported in bins in this period, making wage figures approximate and unreliable for workers employed less than half-time.

[10] In regressions, I use 3-digit ISCO categories to bin occupations.

leave at full compensation, while all mothers in the first three birth cohorts were eligible for 28 weeks of leave at full compensation.

3.1 Main regression specification

In 2000, 4% of firms employed women who gave birth in the last months of 2001 or early months of 2002. I study the outcomes for employees at these firms, and the outcomes of the firms themselves, for firms employing exactly one woman giving birth in these months, and employing at least five total individuals in the base period (one year before any births take place). The regressions reported below take the form

$$y_{itc} = \sum_{t \in \{oct, nov, \ldots, mar\}} \alpha_t \mathbf{1}[month = t] + \sum_{c=1999}^{2005} \alpha_c \mathbf{1}[cohort = c] + \beta T \times POST_{itc} + \gamma X_i + \varepsilon_{itc} \quad (1)$$

where y_{itc} is the outcome of interest for firm (or worker) i at date t, exposed to women giving birth in cohort c. T indicates that the birth cohort was the 2001/2002 cohort in which the reform was applied to the last half of the cohort but not the first half. α_t are month fixed effects, α_c are birth cohort fixed effects, X_i are characteristics of person or firm i, and $POST_{itc}$ indicates whether the birth took place between January and March. In this specification, β is the coefficient which identifies the impact of the policy reform.

3.2 Regression discontinuity specification

The estimates are generally robust to a regression discontinuity difference in difference specification of the form

$$y_{itc} = \sum_{c=1998}^{2005} \alpha_{t,pre} YEAR_{itc} + \alpha_{t,post} POST_{itc} + \beta T \times POST_{itc} + \sum_{t=1998}^{2005} \gamma_{t,post} b_{itc} + \sum_{t=1998}^{2005} \gamma_{t,pre} b_{itc} + \epsilon_{itc} \quad (2)$$

In this case, the running variable b is measured in days since January 1st of the cohort year c. $\gamma_{t,*}$ are coefficients on the effect of birthdate, which may vary by cohort in the pre- January and post-January periods, and vary by year, and β remains the coefficient of interest, highlighting the effect of the reform above and beyond any discontinuities associated with births after January 1st in general. The interpretation of β is that it gives the effect of moving from a firm in which no women giving birth were eligible for the extra 22 weeks of leave at full pay induced by the

2002 reform to a firm in which all women giving birth in the sample window were eligible for the benefits.

The baseline rd specification has a bandwidth of one year. The Calonico et al. [2014] robust data-driven optimal bandwidth for firm shutdown is 154.64 days in the reform year, and varies by year. In the Appendix, I show bandwidth robustness of the main result. The year-long bandwidth is the most conservative in terms of effect size.

An alternative specification would take date of birth as an instrument for maternity leave, and the first stage in such a regression is strong. However, there are some concerns about monotonicity—that some women may reduce their maternity leave length when longer leave is offered—these are discussed in Beuchert et al. [2016]. In this paper, I present only effects of the policy, not the effect of extra leave time away from work.

3.3 Balance

Figure 4 presents bin scatter plots of various characteristics of the firms employing women in the year before their birth by date of childbirth, for women in the 2001/2002 cohort. With the exception of coworker age, there is no evidence of a discontinuity around January first. This effect disappears when controlling for month fixed effects as in equation (1). Table 1 presents the results of regressions of pre-birth even characteristics of firms and their employees on month and year fixed effects, as well as the interaction which summarizes the differences in firms affected by the policy vs. those unaffected. Accounting for month fixed effects, there are not any differences between firms employing women who are eligible vs. ineligible for the policy reform.

Though coworkers may be similar before and after the reform, the mothers taking leave may be different. In Appendix Figure 19, I additionally plot the distribution of occupations at the 1-digit ISCO level of mothers who gave birth in late 2001 compared to those giving birth in early 2002 (who were eligible for the new policy). There are no differences in the type of work that mothers do by the birthdate of their child. Finally, there are not significant differences in other characteristics of mothers, controlling for month-of-birth effects as in panel A of Table 1.

4 Results

The policy reform studied here had a large impact on the time away from work of new mothers. While some new mothers didn't substantially change their leave time, others nearly doubled their leave time. Firms did not have a great deal of time to react to the policy change as it was retroactively applied. Firms also could not be sure whether mothers would return to work after the reform, and 40% of new mothers *do not*.[11]

What was the effect of this reform on the firms? I find that overall, workplaces employing women who were eligible for the extra parental leave time offered by the 2002 reform are three percentage points more likely to be shut down by five years after the leave-event. Figure 5 plots the probability of shutdown by 2007 for firms employing women in November, 2000 who gave birth between 2001 and 2002, by days since January 1st, 2002, in the last column of the first row. The other figures create analogous graphs for 6 additional birth cohorts, centered around January 1st of the indicated year. The only year in which there is a significant discontinuity in early 2002 compared to late 2001 is the treated cohort. In other years, the point estimates are generally *negative*, though none are significant. Appendix Table 10 presents point estimates in each year using the Calonico et al. [2014] robust optimal bandwidth.

In a six month birth widow around the reform eligibility cutoff, the average difference in the 5-year shutdown probability for firms employing eligible vs. ineligible mothers was three percentage points, as in column 1 of Table 2. The effect on firm shutdown, using specification (1) is also three percentage points (column 2). The baseline probability of shutdown is a little over 22 percent, so this shutdown effect is large. This shutdown does not occur immediately, but gradually. Appendix Table 11 displays the probability of a firm in the sample shutting down in a given year. In all years except the first, the effect of the reform is positive. One year after the leave spell, there is not a significant difference in the shutdown probability of firms exposed to the reform.[12][13] Two years after the leave spell, there is a 1 percentage point, statistically significant difference in shutdown probability. These data are consistent with a model in which an extended leave spell stresses the firm, but does not immediately cause shutdown. Further

[11] I reserve the discussion of mother's leave taking behavior for the end of this section, but there are (substantial, negative, and immediate) impacts of the reform on mothers' return to her employer.

[12] In all the analysis of shutdown, I include only those firms which still exist in the year that leave-taking occurs

[13] The effect cumulates, but I cannot look beyond 5 years after the reform because the data for the control years (2005 birth cohort) is not yet available. I use the five year effect because it is the largest, but I cannot evaluate whether the effect stabilizes by year five using my empirical dsign.

idiosyncratic shocks are more likely to push these marginal firms to shutdown, but the maternity leave itself does not directly and immediately do so.

The effect is driven by small firms. Since such a large effect of one woman taking leave on shutdown would be surprising among large workplaces, this is reassuring. As Figure 6, the effects are small and insignificant for large firms, and they decline with firm size. Somewhat surprisingly, the effects are not (significantly) related to mother's earnings rank in the firm or average earnings per worker in the firm. There is an insignificant but suggestive relationship between the effect size and the average tenure of workers in the firm. Point estimates suggest that firms with workers with high tenure on average are *more* likely to shutdown as a result of the policy change.

Industry-by industry comparisons reveal further heterogenity in the shutdown effect. Table 3 presents estimates of equation (1) by industry for the four largest industry groups available.[14] Business services and the public sector are the most affected. Manufacturing firms are not affected by the parental leave extension. The last column includes all industries except the public sector, where many women work but establishment entry and exit may be less related to profitability, and the shutdown effect is slightly smaller but nontrivial—a two percentage point difference in the cumulative survival probability of firms.

Overall, there is a robust three percentage point effect of the policy on the probability that a workplace no longer exists. The turnover rate is large as well, reflecting the fact that there are many marginal firms in the economy and events like extended leave can trigger a long run response substantially larger than the immediate impact. Table 4 presents the results of the regression discontinuity difference in difference specification (2).[15] The effect size is similar to the results presented above, about 2.8 percentage points and significant at the ten percent level. Finally, as perviously discussed, there was not full take-up of the policy. Scaling by the inverse of the first stage implies an even larger (9%) effect of 22 extra weeks of leave on shutdown. The point estimates should be interpreted cautiously, since standard errors are large and the confidence intervals include much smaller (though non-zero) estimates.

I next turn to studying firm behavior in response to extended leave by employees, finding

[14] All excluded industries have fewer than five hundred treated firms and effects are extremely imprecisely estimated.

[15] Appendix Table 12 displays the results using smaller bandwidths in three month intervals. These actually give larger (significant) estimates, and none reject the 3 percentage point effect estimated in the simple difference.

no evidence that firms exposed to the policy differentially changed their hiring overall, or their hiring of women in particular after the policy. If firms exposed to the policy were more wary of parental leave spell exposure in the future, they may have less blunt instruments than hiring or not hiring a woman, so I also directly study cumulative leave-taking per worker for firms exposed vs. not to the policy. I find that firms exposed to the policy change are do not experience less future leave-taking per worker. It is important to highlight here that strategic firm response to parental leave reforms may be completely missed by the limitations of my design. After all, all firms in Denmark are equally exposed to future parental leave increases by their employees after the policy reform independent of past exposure.[16] Nonetheless, to the extent that firms experience substantial negative effects of extended leave, they may be more avoidant of it in the future.

Table 5 presents the effect of the reform on firm outcomes five years after the reform (for surviving firms), finding no effect on size, fraction female, fraction of new hires who are female, sickness leave taking, or parental leave taking. Standard errors are in general large, but I can reject differences in the fraction of new hires that are female of less than 4 percentage points, or a 6.5% change in the gender of new hires. Firms exposed to the policy are also not exposed to significantly more sick-leave per worker or parental leave per worker five years after the reform. The point estimates are very small, less than one day per worker over the course of five years, and confidence intervals rule out negative effects on sickness and parental leave of more than approximately two and a half days per worker over the course of five years.

The estimates presented above come from a policy change which had the (unrealistic) feature of being surprising. The extent to which this leave was a "surprise" to firms may substantially affect costs. However, by comparing women just eligible relative to those giving birth slightly later, I can test whether the estimates are larger for the more surprised firms. Table 6 presents the estimate of regression (1), where rather than combining all Jan-March births into the indicator $Post$, I separate the estimate by employee month-of-birth. Women giving birth in January found out about their eligibility for additional leave when they had used about half of their baseline leave time. Women giving birth in March had just started their parental leave and firms were not expecting their return in the near-term. The estimates of the shutdown effect do not differ across Jan births, Feb. births, or March births. This study is not irrelevant for typical leave

[16] except perhaps through avenues such as peer effects and knowledge diffusion or learning about firm preferences. I discuss these possibilities below.

taking in which firms can plan ahead, since there are not substantial differences in situations when firms had a couple of months to adjust relative to six months to adjust to the extra leave offered by the reform.

The behavior of coworkers the mothers themselves exposed to the reform can help explain the negative firms effects described above. I will discuss the effects of the reform on coworker earnings, job changes, employment, and leave-taking to understand whether the firm is able to fully and freely able to shift tasks to other employees when one goes on leave. At the end of this section, I discuss how the reform affected the behavior of women taking leave. Table 7 displays the coefficient on the interaction between the January-March births and the treated cohort for earnings five years after the birth event for three subgroups: the full set of coworkers of women in the sample, the coworkers of women working in firms with fewer than 30 employees (small firms), and coworkers of women in the same occupation as women giving birth. These latter groups are coworkers more likely to be affected by the reform. In smaller firms, the absence of one women is more likely to affect coworkers. In any firm, coworkers in the same occupation as a woman taking leave are (presumably) most likely to be asked to complete her tasks. As in Table 7, despite the negative long run impacts of parental leave extensions on firms, I find no impacts on coworkers overall, or on coworkers when restricting to small firms. I can rule out earnings losses larger than \$ 340 in the long run among workers in small firms.

Previous research on a related topic, the effect of worker death on coworker earning, has also not found any long run effects of worker death on coworker earnings (Jäger and Heining [2019]). However, Jäger and Heining [2019] finds positive effects of a little under \$200 overall on coworker earnings in the short run following worker death, and larger effects for coworkers in the same occupation. These effects persist for four years after the event. I do not find a similar pattern. Figure 8 plots the time path of coworker earnings (the coefficient β in (1)) 1-5 years after a birth event among all coworkers in small firms. Coworker earnings effects are very slightly negative in the years following the leave-event. I cannot reject that all estimates 1-5 years after the leave-event are 0, with a p-value of 0.68. In contrast, while I cannot reject the estimates of Jäger and Heining [2019] in a given year, I can reject the pattern of elevated coworker earnings for a period of four years described there. The p-value for an F-test of equality of the 5-year time-series in Figure 8 and the point estimates in Jäger and Heining [2019] is 0.0014.

Coworkers in the same occupation as new mothers have slightly elevated earnings in the year after the leave event, but point estimates are insignificant everywhere and economically insignificant in every year except the first year post-reform, as in Figure 9. I find no significant effects overall on the probability of working in a new job over time[17], though as the last column of Table 7 suggests, by five years after the reform, same occupation coworkers are slightly more likely to have changed jobs if their coworker was eligible for extended parental leave. The baseline probability of a job change five years after a leave event is approximately 20 percent, as shown in Figure 10 which plots a variety of coworker outcomes for the 2001/2002 cohort by mother's birthdate. Appendix Figures 21 and 22 document that there are no effects on coworker employment in the long or short-run as a result of the reform.

Even though I do not find major changes in coworker earnings, there are additional tests of worker stress. If coworkers are unhappy with their workload after a colleague has a child and takes extended leave, they may move to lower-pay jobs. In addition, the extra demands of work may cause them to take more sick-days. Finally, coworkers may change the timing of their own births either due to the extra demands of work or due to the effect of their coworker's leave on the firm and their expectations of the future overall. Indeed, I find evidence of the effects of the policy on these margins.

First, as Figure 11 reveals, those coworkers who stay at their firm do not experience any changes in their earnings, consistent with the overall pattern described above. However, when focusing on coworkers who do change jobs between the leave event and one year after the leave event and implementing the regression in (1) in each year 1-5 after the leave spell, I find a negative impact of the reform on mover earnings in the year of their job change. Figure 12 plots the coefficient β from a regression of earnings in each year 1-5 after the reform. While the *fraction* of workers who changed jobs did not change significantly as a result of the reform, the composition seems to have changed substantially. Workers who change jobs and were exposed to the reform take jobs paying $1500 less compared to job changers who were not exposed to the reform. However, this earnings loss does not persist. By the second year after their job change, earnings have recovered.

Coworkers of women eligible for extra leave also change the timing of their own births. Figure 13 plots the coefficient β from a regression of total days of parental leave in the years surrounding the reform. First, the sample is balanced in the pre-period. In the year of the leave event, when

[17]see Appendix Figure 20

it would largely not have been possible to change births even if extended leave put stress on coworkers (since the time from conception to birth is nine months), there is no effect of the reform on births. In the year following the leave, event, however, there is a significant reduction of half a leave-day per worker as a result of the policy reform. This effect is not consistent with a model of peer effects in which longer leave normalizes or in some other way engenders longer leave taking by coworkers.[18] Instead, coworkers exposed to long leave put off children slightly. As in Figure 14, the effect is not an intensive margin effect but is explained by the expensive margin decision to have a child in a given year. There is no cumulative effect in parental leave taking by coworkers by five years after the reform.

While coworkers move to lower pay jobs as a result of the reform and slightly delay children as a result of the reform, I do not find significant effects on sickness leave taking before three years after exposure to parental leave, as in Figure 15. These effects on sickness leave taking are difficult to separate from the countervailing effects on parental leave, since one cannot take sickness leave while on parental leave. Overall, the pattern of coworker effects is consistent with some stress on coworkers of women eligible for extended leave, and is less consistent with traditional peer effects in leave taking emphasized in the literature or with a model in which employees are well compensated for any additional duties they are asked to perform in the temporary (but long) absence of a coworker.

Having discussed the effects of the reform on firms and coworkers, I now turn to the effects on the women actually taking leave—the mothers themselves. One of the channels through which these negative and long-lasting effects of a leave spell impact the firm may be changes in the behavior of mothers after the leave period passes. Women who give birth seem to use the leave-time in part to find new jobs. Women have a 60% probability of returning to their pre-birth workplace by five years after the leave-event. This is substantially lower than can be explained by firm turnover. Does extended parental leave allow women to search for better jobs and decrease their return probability? As Table 8 implies, extended leave does increase the probability that a woman is working in a different job. By one year after the leave spell,[19] women eligible for extra leave after the 2002 reform were 3.5 percentage points more likely to be

[18]see Dahl et al. [2014] for a discussion

[19]For the cohort of 01/02, the exact definition of the outcome variable in column 2 ("New Job") is whether the main job of the woman in 2003 differed from her main job in November, 2000. For workers giving birth in March, 2002, this is a few months less than one year post-leave. For workers giving birth in Oct, 2001, this is slightly longer than one year post-leave. The variable is defined similarly in other years.

working in a different firm than their pre-birth employer. By five years after the leave-taking, these women were seven percentage points more likely to be working for a different firm. Notice that this effect is substantially larger than even the largest coworker effects reported, and about twice as large as the shutdown probabilities of the firms themselves. The leave extensions did not affect women's earnings or probability of working by five years after their leave concluded.

Overall this result suggests that one of the major effects of parental leave extensions is the change in return to work by women giving birth. This change highlights the importance of leave for aiding the post-birth job search of women but also highlights a potential channel for the costs of leave to firms: job protection during a leave period after which an employee may not return. In the short term, the firm must substitute for a worker's labor (but not permanently) and face uncertainty in the return-to work of this employee. In the longer term, they face a higher probability of needing to replace the worker permanently. The behavior of mothers on leave generates thus expands the frictions experienced by the firm in substituting a worker's labor.

5 Conclusion

This paper studied the workplace effects of a 2002 Danish reform which increased the length of paid parental leave available to mothers by 22 weeks. The timing of the reform—first discussed in the November, 2001 elections, implemented in March, 2002 and retroactively applied to women who gave birth January 1st, 2002 or later—creates exogenous variation in the length of parental leave for which similar women were eligible. This paper is the first to document the effects of such a parental leave extension on coworkers of the women taking leave and their firms. When workers are difficult to temporarily replace these workplace effects could potentially be large.

I find that this leave is costly to small employers. Firms are overall three percentage points more likely to be shut down five years after the Danish parental leave extension if they employed a women who was eligible for the leave relative to those employing women who were not eligible. This result is robust across a variety of specifications, including simple differences, difference-in-difference estimates, regression discontinuity estimates, and regression discontinuity difference-in-difference estimates. This effect declines with firm size and because of this, the employee-weighted effect on shutdown is negligible. I find no evidence that firms respond to extended leave by employees by changing hiring or in other ways reducing their future parental leave exposure.

The behavior of mothers eligible for the new policy differed from those who were not eligible, and this may contribute to the negative effect on firms. Women were substantially less likely to return to, and more likely to later separate from, to their pre-birth employer when they were eligible for more leave.

Perhaps surprisingly, there is no evidence of an overall impact of the reform on coworker earnings and I can reject a pattern of elevated earnings of about 1% in the five years following the leave event. Same-occupation coworkers in small firms have elevated earnings only in the year in which a woman takes leave. Under a simple model in which coworkers are substitutes for the labor of a worker on leave, coworker wages would rise with exposure to extended parental leave, especially since many women do not return to their employer after taking leave. The data do reveal patterns of stress on coworkers: coworkers exposed to the reform who change jobs in the year of leave are more likely to have moved to lower-pay jobs. Coworkers also take less parental leave in the short run (more in the long run), driven by delayed childbearing.

The economic impacts of the Danish parental leave reform on the workplace *overall* is small. On average, there are no earnings effects on coworkers and firm effects are concentrated in small firms. The results do suggest some stress—moving to lower pay jobs and delayed fertility—on coworkers in the same occupation as a woman taking leave, but these effects are not dramatic and cumulatively there is no effect on either long-run earnings or fertility.

The policy studied here was a dramatic expansion of paid parental leave for a relatively recent cohort of mothers. The way in which the reform was enacted—retroactively—gives researchers interested in the effects of paid leave expansions an excellent case to consider. The drawbacks of this reform were that women were already on leave when it was announced, potentially muddling the effects of reform with the effects of surprises to which firms cannot adjust. Checking the effects of the reform on firm outcomes month-by-month, I find no evidence that the costs are a result of surprise—firms with more warning had the same outcomes as firms with less warning. While this reform cannot directly speak to the costs for firms when paid leave is first introduced (important for the US), it is quite relevant for all other developed countries when considering long parental leave extensions. Since there are no direct costs to firms of a woman taking state-paid leave in Denmark, but this leave nonetheless affects some firms, the policy studied here gives a cautionary and important lower bound on the costs to firms of parental leave reform.

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Figures

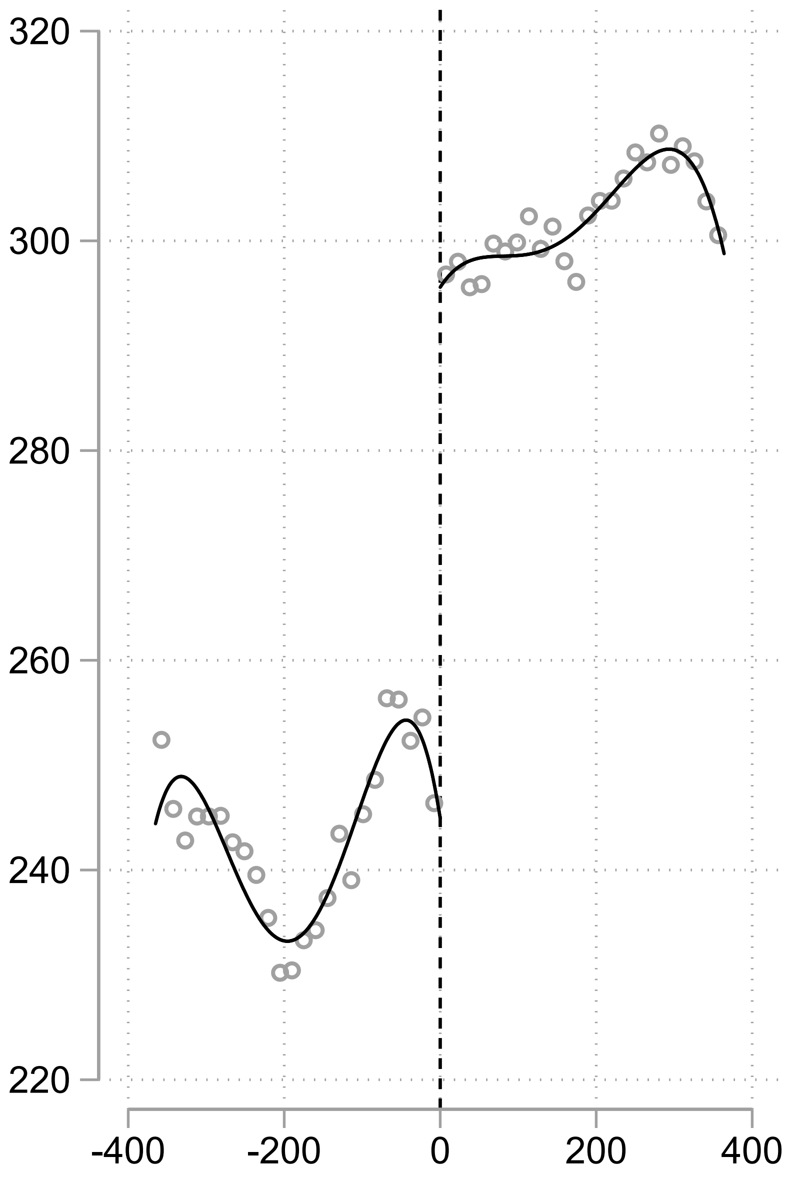

Figure 1: This figure displays the change in leave taking immediately at the eligibility cutoff. Leave includes any time away from the firm, summing childcare leave, maternity leave, parental leave, and sickness leave taken consecutively after the child is born beginning up to four weeks before a child is born. Leave length is per child, and divided by the number of children in the case of multiples.

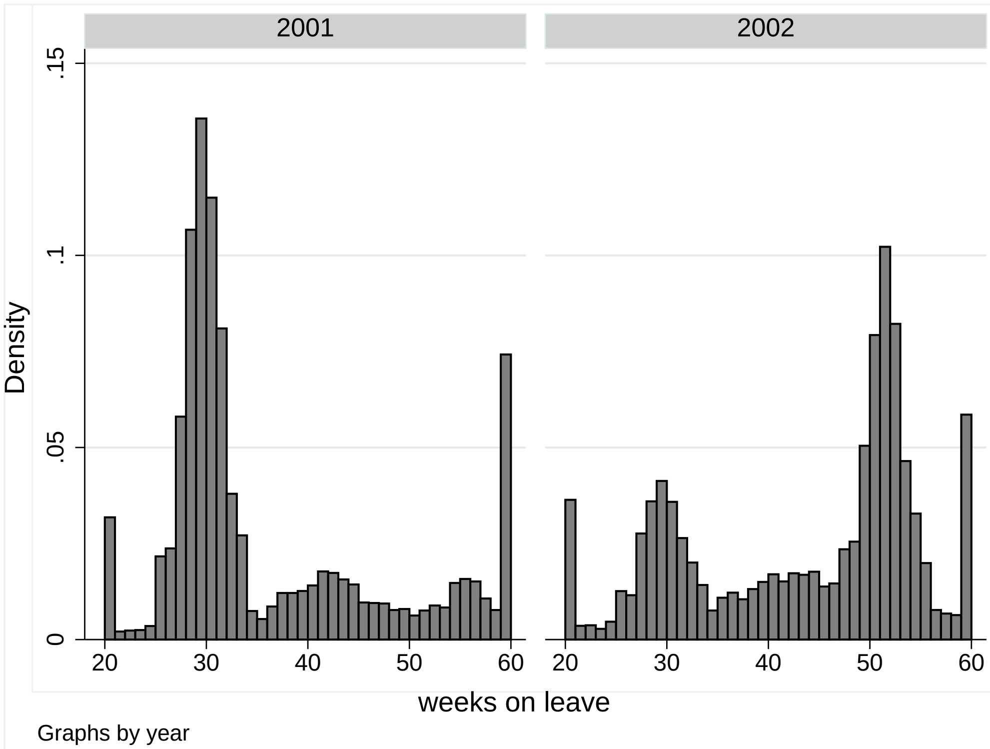

Figure 2: This figure displays the number of parental leave spells taken by mothers by the length of the spell. This figure includes all leave from the firm (the sum of childcare leave, maternity leave, parental leave, and sickness leave taken consecutively after the child is born beginning up to four weeks before a child is born). Leave length is per child, and divided by the number of children in the case of multiples.

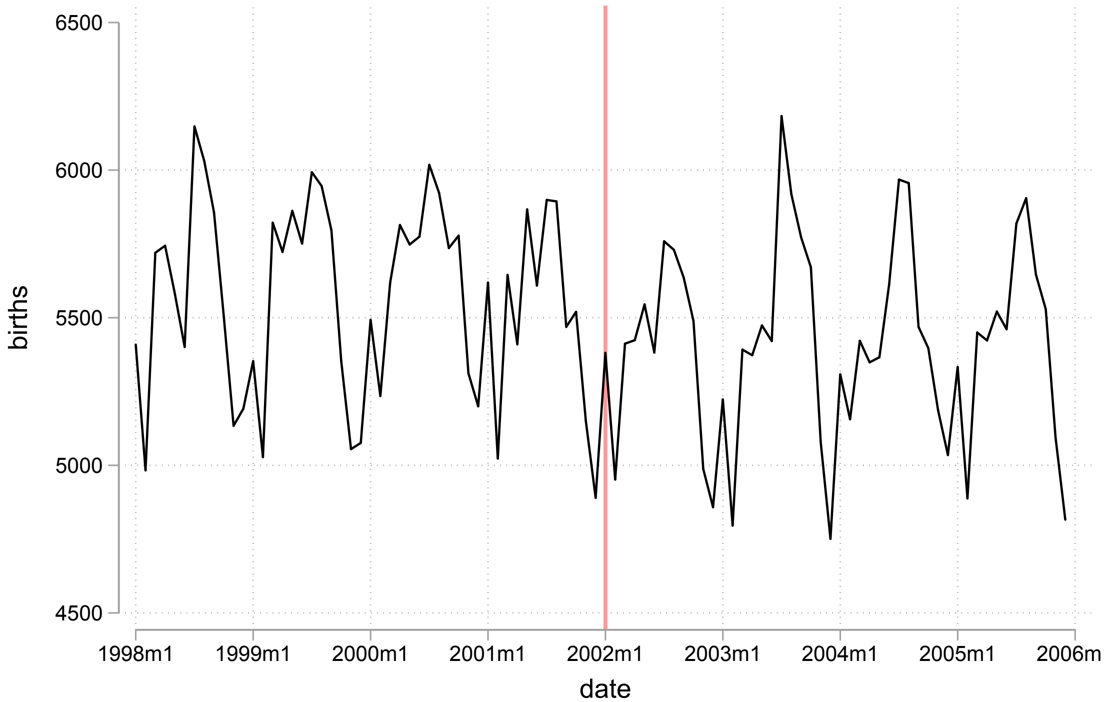

Figure 3: This figure plots the number of births by date. The date of eligibility for the new parental leave policy is highlighted.

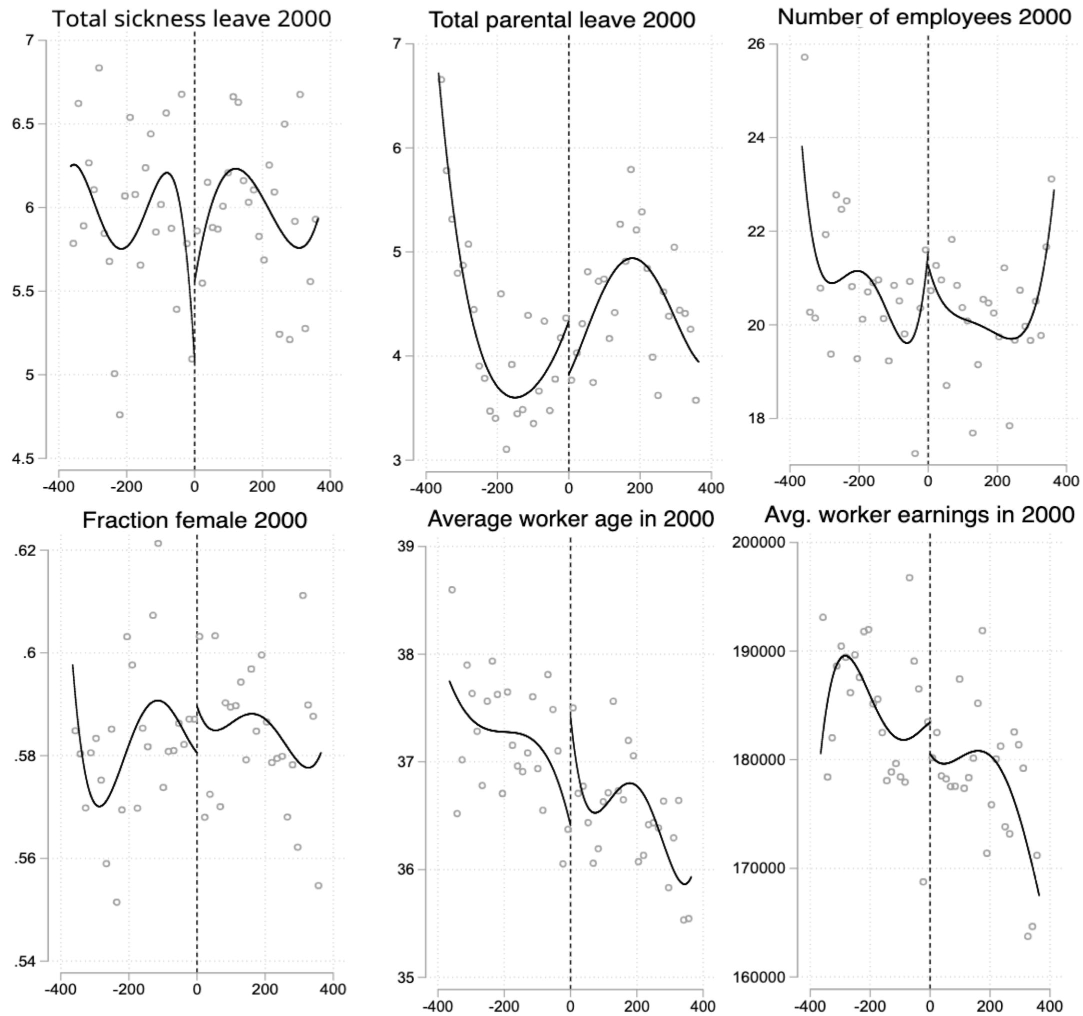

Figure 4: This figure is a bin-scatter of various firm or worker level characteristics as measured in Nov. 2000. The only significant discontinuity is coworker age around the cutoff. Though co-workers are slightly older in early January compared to late December, this is a seasonal difference and disappears when including seasonal effects as in Table **??**

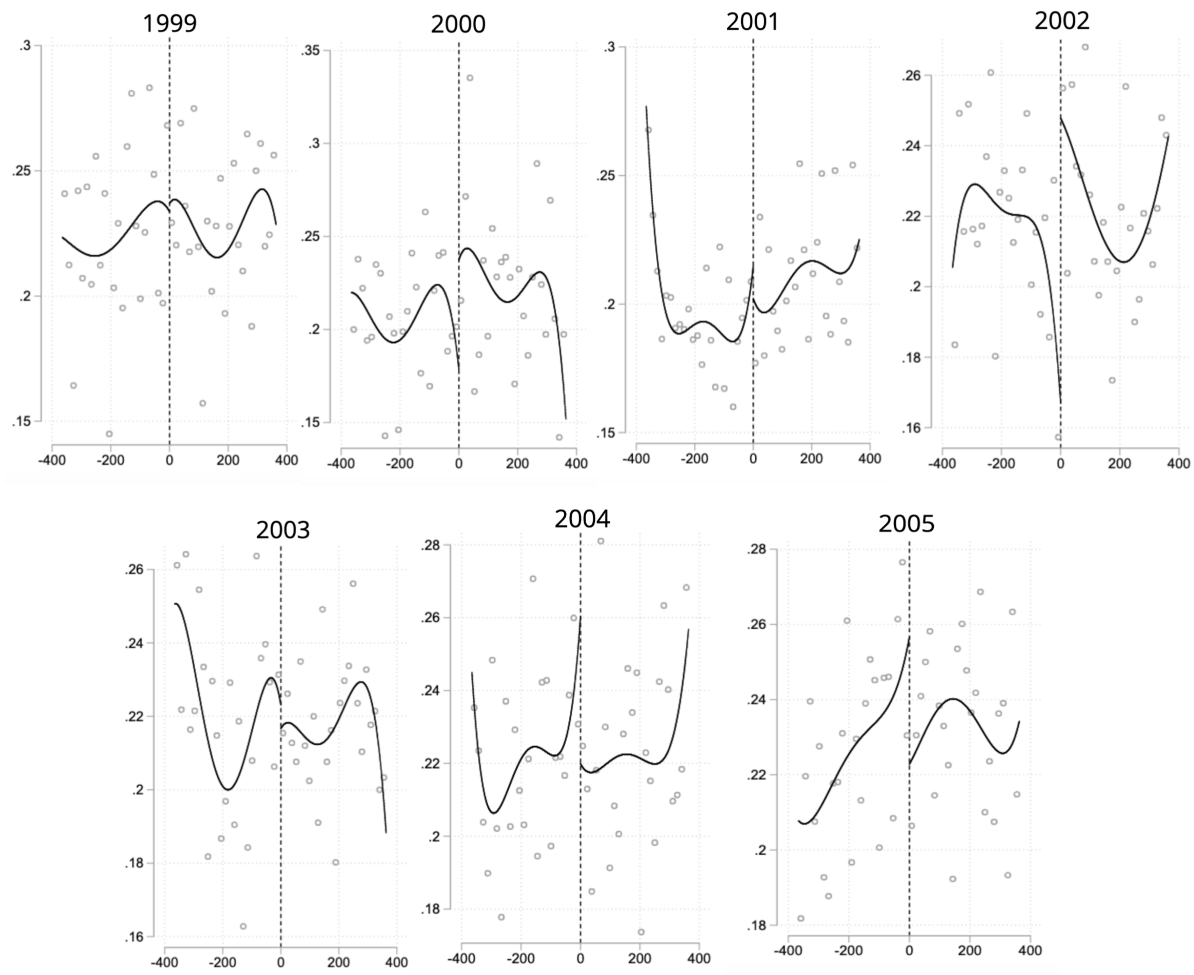

Figure 5: This figure is a bin-scatter of the probability of firm shutdown, measured as appearing in the data for the last time five years or fewer from the time of the leave event, by mother's birthdate, over the 7 years in the sample. The parental leave extension occurred (only) in 2002.

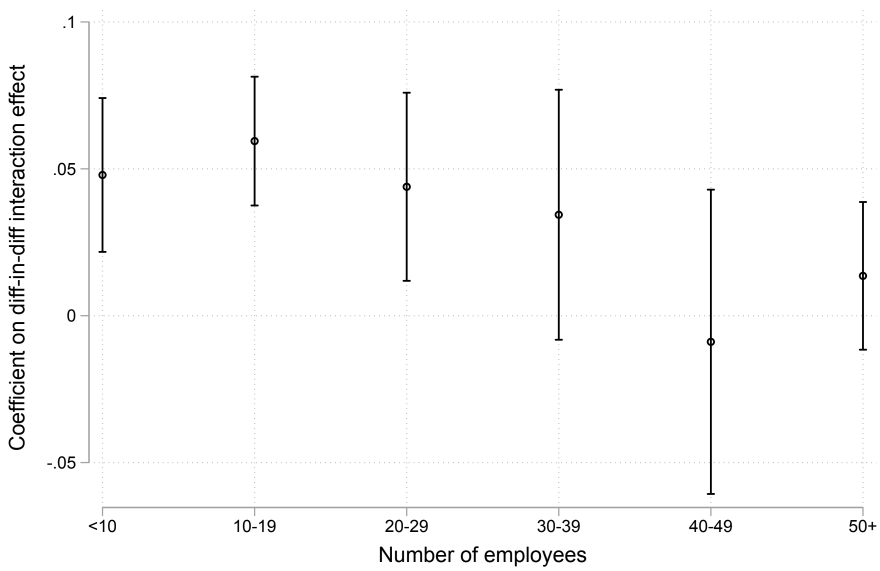

Figure 6: This figure gives the probability of shutdown by firm size. The plotted estimate is the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 when regression (1) is restricted to firms in the indicated size bins as measured in November a year before the birth occurs.

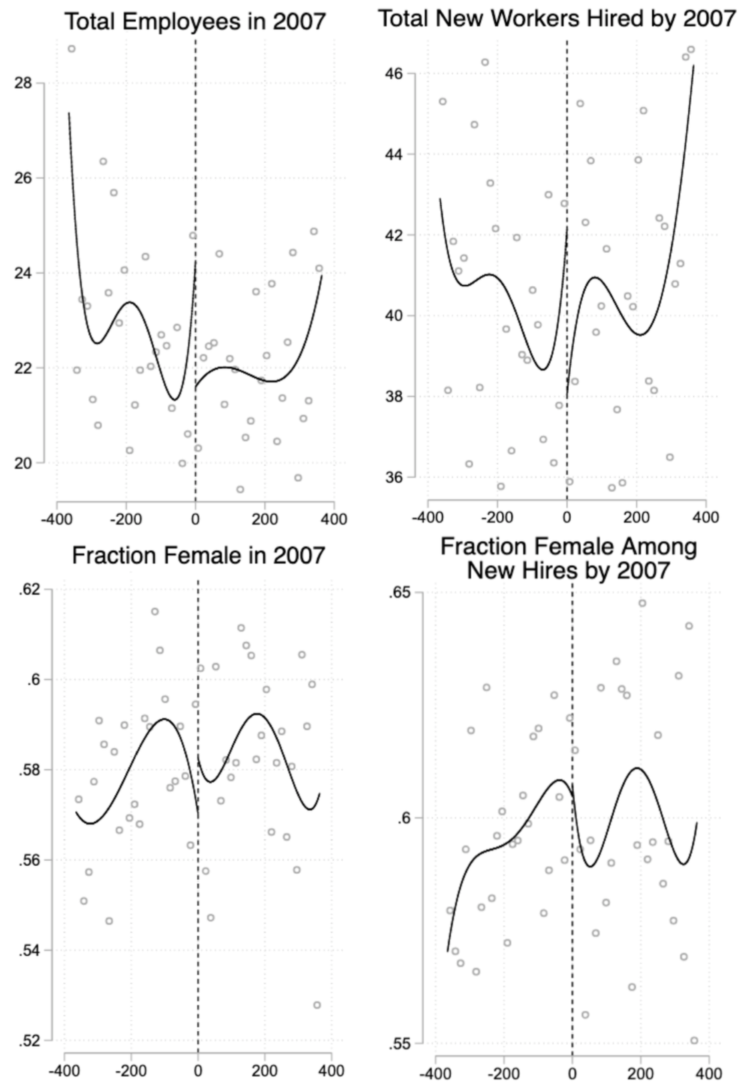

Figure 7: This figure gives bin-scatters of the indicated outcome variables in 2007 for firms exposed to a woman who gave birth between January 1, 2001 and December 31st, 2002. Women giving birth after the 0-line (January 1st, 2002) were eligible for extended parental leave. Earnings are in Danish Kroner.

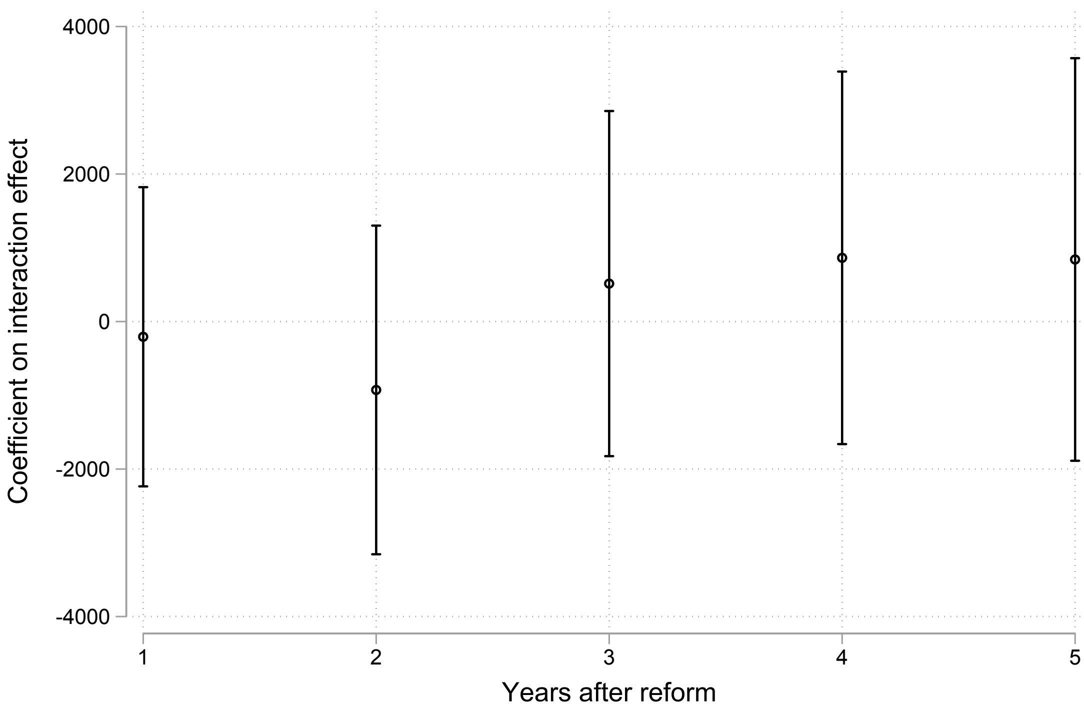

Figure 8: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is earnings 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to small firms (fewer than 30 employees in the baseline period) only.

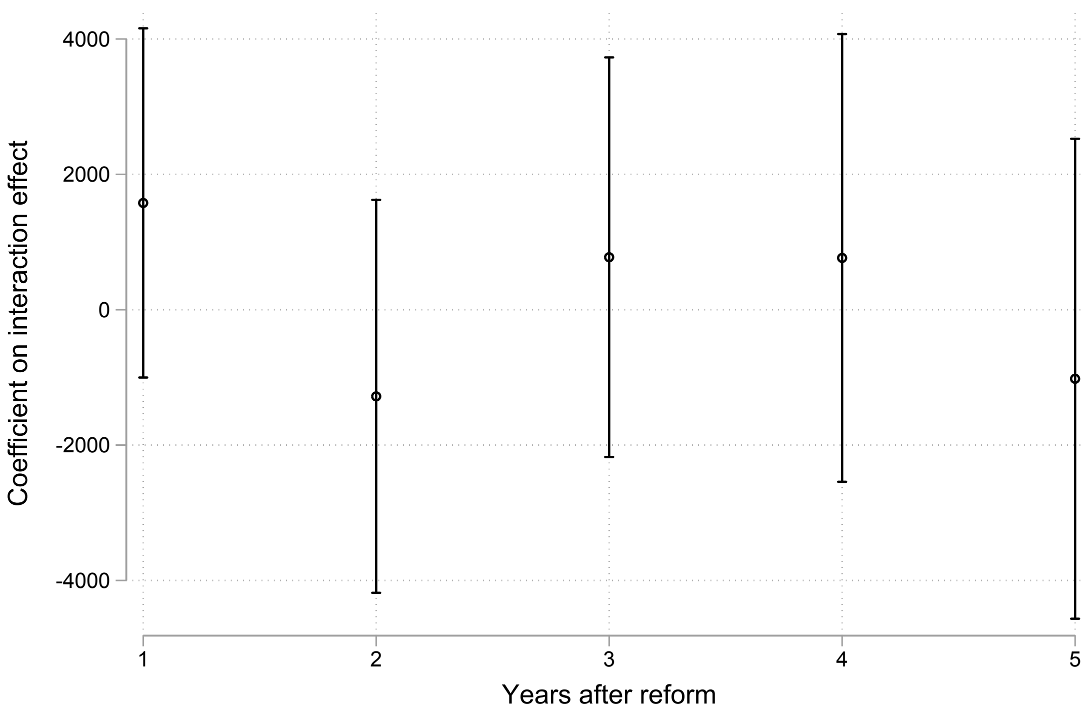

Figure 9: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is earnings 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to coworkers in the same occupation as the employees giving birth and is restricted to small firms (fewer than 30 employees in the baseline period) only.

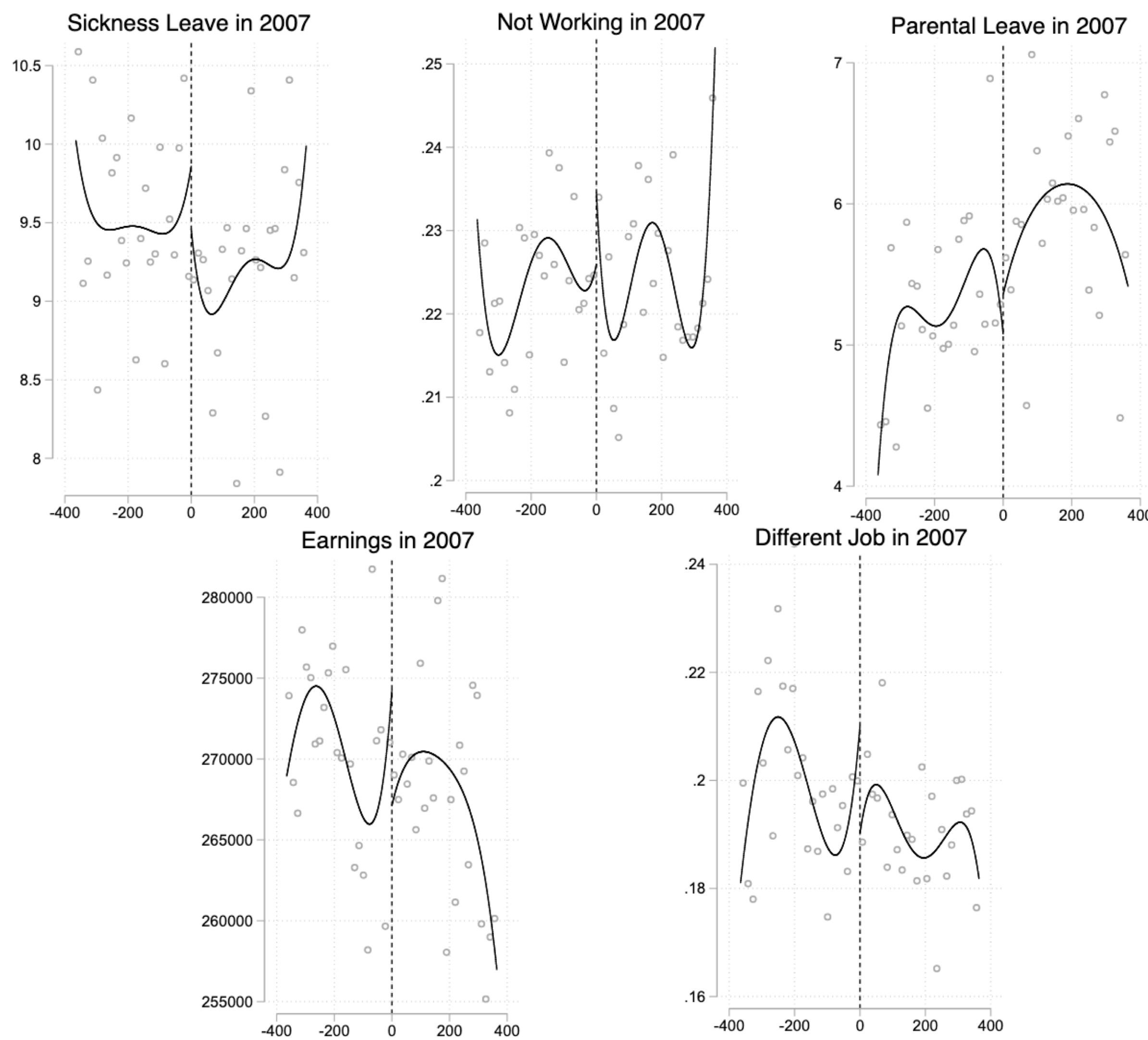

Figure 10: This figure gives bin-scatters of the indicated outcome variables in 2007 for workers exposed to a woman who gave birth between January 1, 2001 and December 31st, 2002. Women giving birth after the 0-line (January 1st, 2002) were eligible for extended parental leave. Earnings are in Danish Kroner.

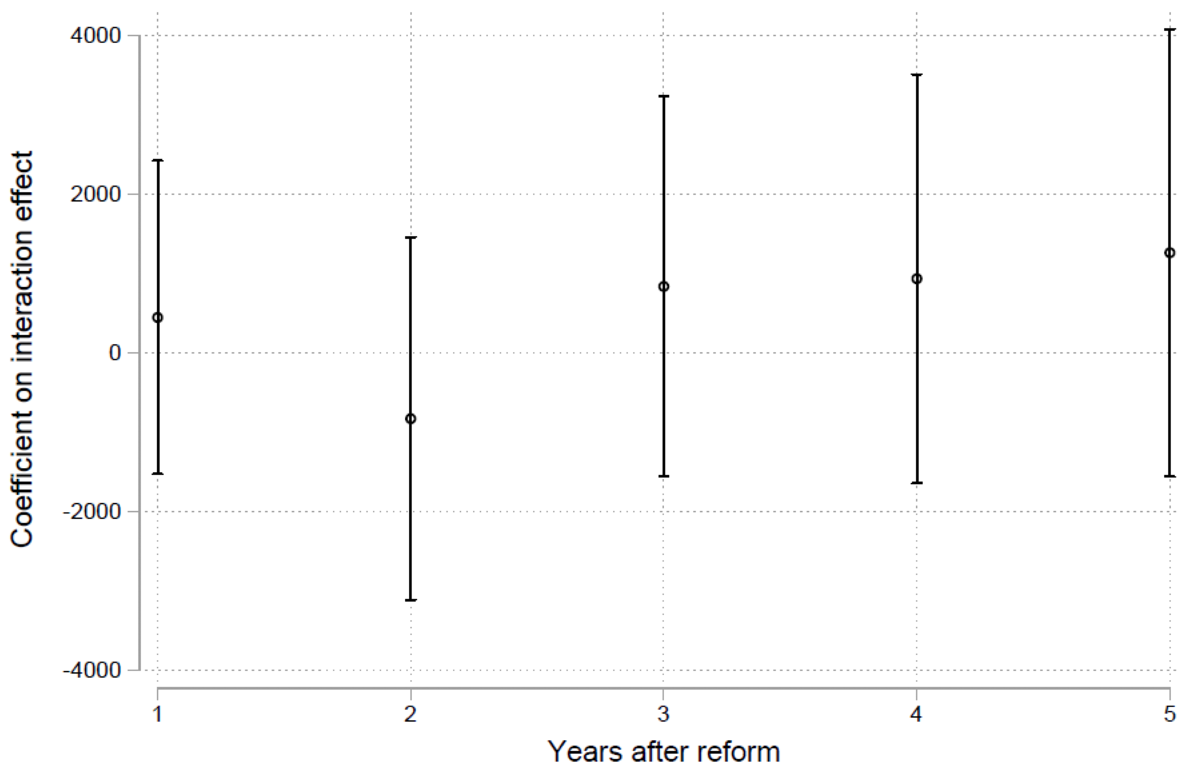

Figure 11: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is earnings 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to coworkers who remain at their firm in the year of the leave event and is restricted to small firms (fewer than 30 employees in the baseline period) only.

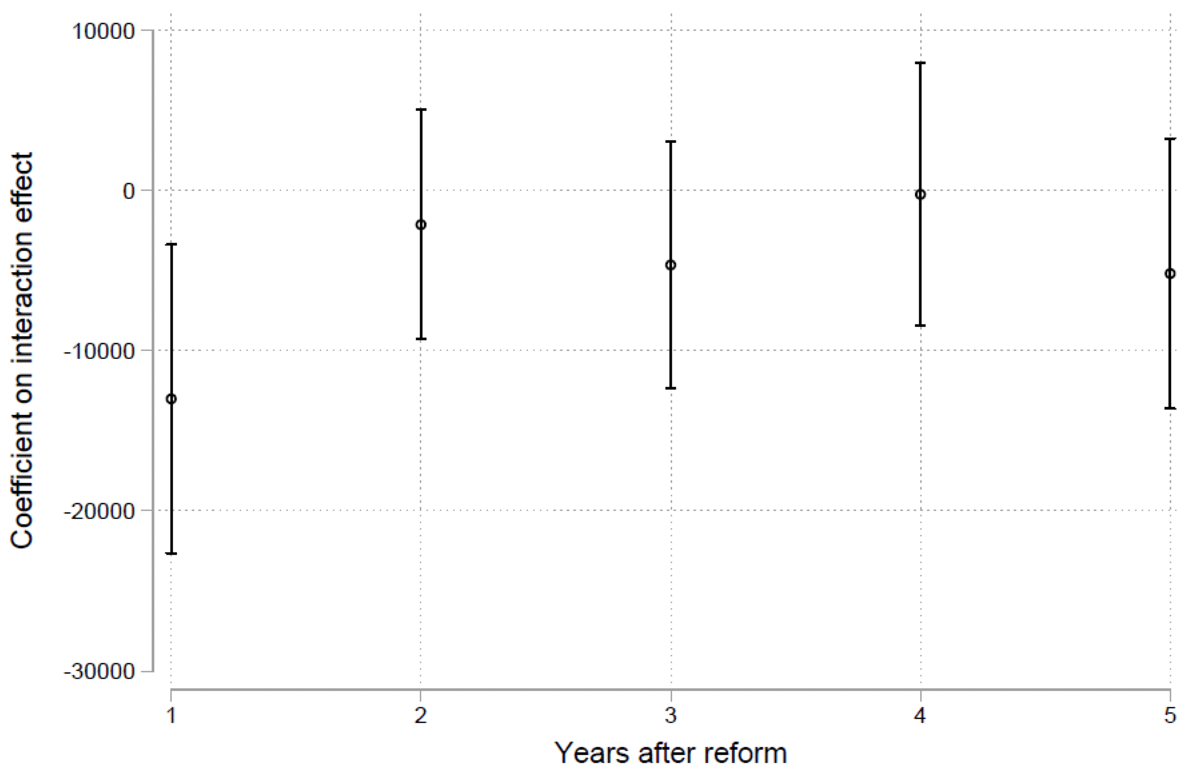

Figure 12: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is earnings 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to coworkers who change jobs in the year of the leave event and is restricted to small firms (fewer than 30 employees in the baseline period) only.

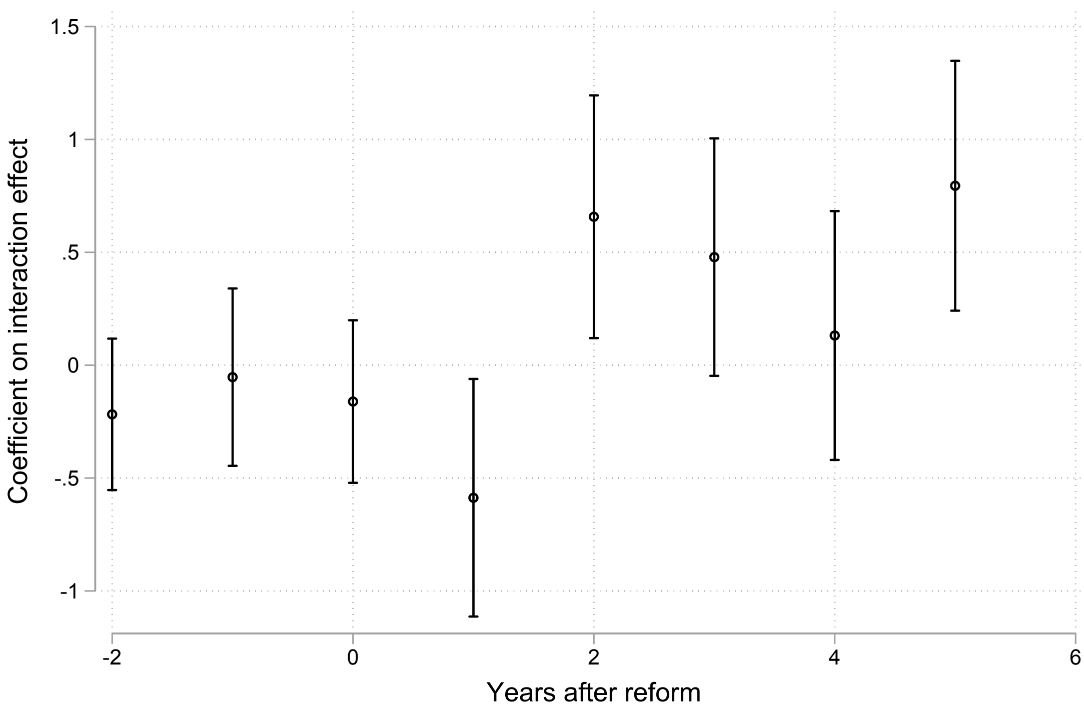

Figure 13: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is days on parental leave in the years before and after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to small firms (fewer than 30 employees in the baseline period) only.

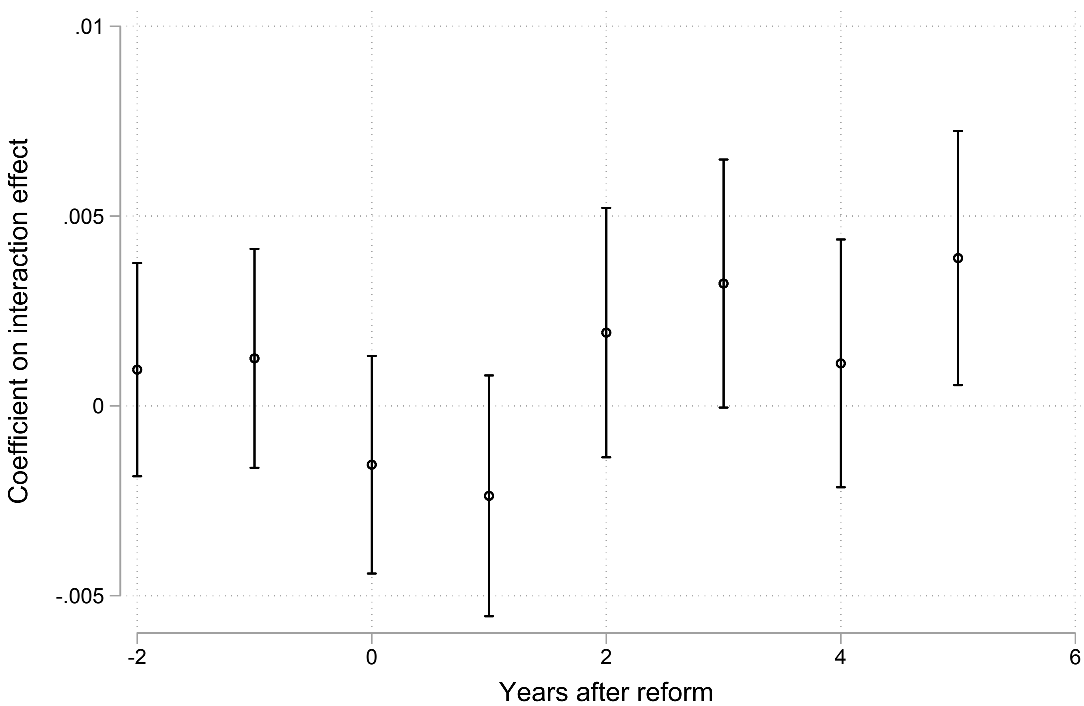

Figure 14: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is an indicator of whether a coworker took any parental leave in the years before and after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to small firms (fewer than 30 employees in the baseline period) only.

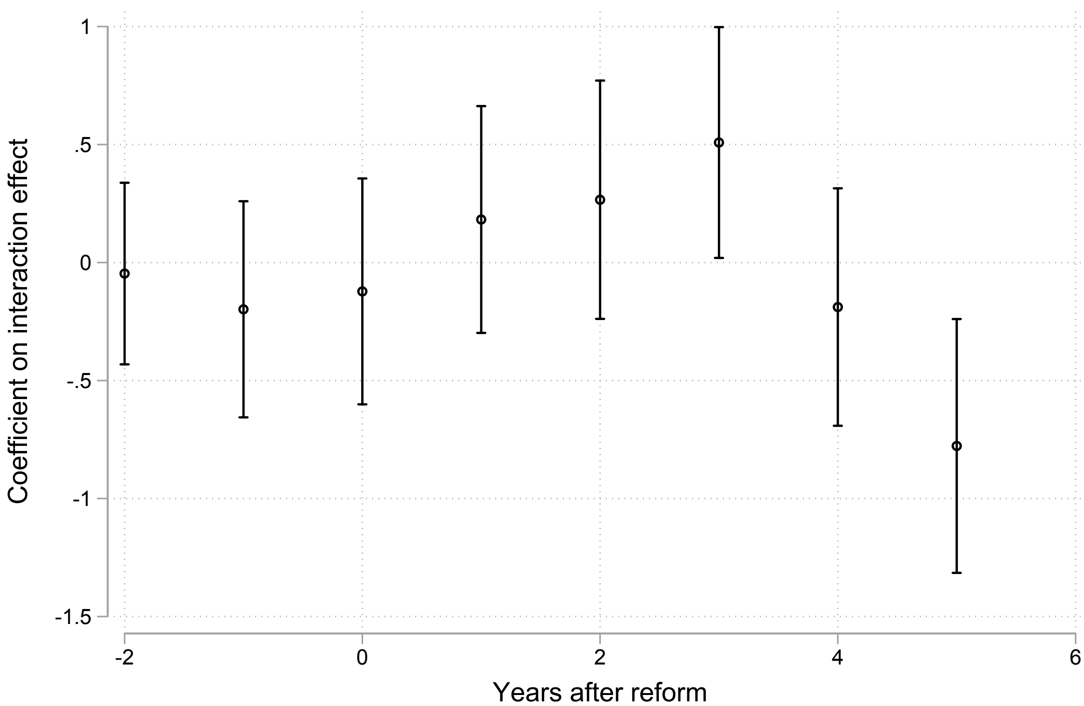

Figure 15: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is days on sickness leave taking in the years before and after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to small firms (fewer than 30 employees in the baseline period) only.

Tables

Table 1: Balance at 2 years pre-leave spell

Panel A	Mother's characteristics				
	Age	Earnings	Parental	Sickness	
Post×01/02 Cohort	0.01887	-1082.334	-0.5014	-0.1146	
	(0.10583)	(2269.363)	(1.0825)	(0.5348)	
N	59881	59881	59881	59881	
Panel B	Co-worker characteristics				
	Male	Age	Earnings	Parental	Sickness
Post×01/02 Cohort	0.00421	0.00368	-1029.6	-0.337	-0.0945
	(0.00848)	(0.224)	(2693.6)	(0.216)	(0.255)
N	494612	494612	494612	494612	494612
Panel C	Firm characteristics				
	N	F/N	Parental/N	Sickness /N	
Post×01/02 Cohort	1.867	-0.00308	-0.164	-0.0573	
	(1.760)	(0.00711)	(0.241)	(0.234)	
N	37675	37675	37675	37675	

Sample is restricted to firms employing exactly one women giving birth in the window around January first. There are no controls (except month of birth and cohort year fixed effects to identify the effect of the policy reform). The middle panel includes only co-workers at firms with fewer than 30 employees (the focus of co-worker level analysis), while the bottom panel includes all firms. Results are unchanged when restricting to only small firms in the bottom panel, except that the difference in the number of employees shrinks to nothing (results available upon request). Earnings are measured in 2000 DKK, so that \$1000 Kroener is approximately \$150 USD.

Table 2: Firms shutdown regressions

	(1)	(2)	(3)	(4)	(5)
Post	0.035***				
	(0.00916)				
Post×01/02 Cohort		0.033***	-0.0120	0.0456***	0.0574*
		(0.0100)	(0.0341)	(0.0137)	(0.0297)
Post×01/02× Avg Tenure pw			0.0148		
			(0.00912)		
Post×01/02× Mom's rank				-0.000386	
				(0.000433)	
Post×01/02× Avg Earnings pw					-0.0098
					(0.00143)
Sample	2001/2002	Full	Full	Full	Full
N	7925	34665	34657	34055	34055

The first row of this table reports the average difference in shutdown probability by five years after a leave event (2007) for firms employing women who gave birth in Oct. 1, 2001- Dec. 31, 2001 relative to firms employing women who gave birth in Jan 1, 2002 to Mar 31, 2002, where the latter subset were eligible for an additional 22 weeks of parental leave at full compensation. The next rows extend the sample to analogous months in three years before and three years after the reform, and give the coefficient on the interaction between the cohort exposed to the policy change and those giving birth in the first three months of the year. In these rows, the coefficient identifying the impact of the reform is Post×01/02 Cohort (at least for the 0 level of the interacted variables in regressions (3)-(5)). The sample is restricted to firms employing exactly one women giving birth in the window around January first. There are no controls (except month of birth and cohort year fixed effects to identify the effect of the policy reform). Avg. Earnings/worker are in tens of thousands of DKK.

Table 3: Firms shutdown regressions by industry

Industry	W/R Trade	Public	Business Services	Manufacturing	All ex. Public
Post×01/02 Cohort	0.0496**	0.0505**	0.0752**	-0.0230	0.0231*
	(0.0203)	(0.0173)	(0.0456)	(0.0327)	(0.0126)
Industry mean	0.2041	0.2325	0.2200	0.2072	0.2174
N	6459	8747	1888	2290	25918

This table gives the estimates of regression (2) in Table 2 by industry. The coefficient identifying the impact of the parental leave reform is Post×01/02 Cohort. The sample is restricted to firms employing exactly one women giving birth in the window around January first. There are no controls (except month of birth and cohort year fixed effects to identify the effect of the policy reform). Public is category 9 under the Danish Industry Code and standard groupings consisting of Public administration, defense, police and judiciary, teaching, and health and social care. Business services in category 8, Wholesale and retail trade is category 4 (and includes also transportation), and Manufacturing is category 2. All other 1 digit categories are substantially smaller and I do not report their (extremely imprecise) estimates–none are significantly negative and most are insignificantly positive. Sample is restricted to firms employing exactly one women giving birth in the window around January first in the industry indicated. Industry category documentation is available here `https://www.dst.dk/Site/Dst/Udgivelser/GetPubFile.aspx?id=16252&sid=21dbs`.

Table 4: Firms shutdown regressions: RD Difference in Difference

	(1)	(2)	(3)	(4)
Post×01/02 Cohort	0.0284*	0.00385	0.0705*	0.0648*
	(0.0150)	(0.0405)	(0.0397)	(0.0351)
Post×01/02 Cohort× Avg Tenure pw		0.00649		
		(0.0110)		
Post×01/02 Cohort× Mom's rank			-0.000558	
			(0.00277)	
Post×01/02 Cohort× Avg Earnings pw				-0.00205
				(0.00174)
Birthdate	0.0000220	0.0000721	0.0000315	0.0000839
	(0.0000210)	(0.0000561)	(0.000213)	(0.0000490)
Post	-0.000417	0.00622	-0.0143	0.000186
	(0.00608)	(0.0161)	(0.0164)	(0.0140)
Post×Birthdate	-0.00000944	-0.0000834	0.000274	-0.0000941
	(0.0000295)	(0.0000779)	(0.000306)	(0.0000676)
N	98490	98490	98490	98490

This Table gives the estimates of β in equation (2) where the outcome is firm shutdown by five years after the leave event. The running variable, birthdate, is included linearly and firms in the sample have exactly one birth across the entire two year birth cohort. Avg Earnings/worker are measured in tens of thousands of DKK.

Table 5: Firm responses by 5 years post-leave spell

	N	F/N	F^{new}/N^{new}	N^{new}	Sickness $/N$	Parental$/N$
Post×01/02 Cohort	1.908	-0.0082	-0.0055	3.856	-0.890	-0.0458
	(2.118)	(0.009)	(0.011)	(3.531)	(0.874)	(1.122)
N	27343	27343	27224	27224	24085	24085

This table reports the effect of the reform on the number of employees (N), the fraction of employees who are female (F/N), the fraction of employees who are female among employees hired after the leave event (F^{new}/N^{new}), cumulative number of hires after the leave event (N^{new}), cumulative days of sickness leave per worker, and cumulative days of parental leave per worker by five years after a leave event (by year $y+5$). The effect compares outcomes of firms employing women who gave birth in Oct. 1- Dec. 31 relative to firms employing women who gave birth in Jan 1 to Mar 31 of year y, where the latter subset were eligible for an additional 22 weeks of parental leave at full compensation only in year $y = 2002$, controlling for month fixed effects. The coefficient identifying the impact of the reform is Post×01/02 Cohort. The sample is restricted to firms employing exactly one women giving birth in the window around January first which are not shutdown. There are no controls (except month of birth and cohort year fixed effects to identify the effect of the policy reform).

Table 6: Ability to Plan and Firm Costs

Jan×01/02 Cohort	0.035*
	(0.013)
Feb×01/02 Cohort	0.032*
	(0.014)
March×01/02 Cohort	0.033*
	(0.014)

This Table presents the estimate of regression (1) (as in column (2) of Table 2), where rather than combining all Jan-March births into the indicator *Post*, I separate the estimate by employee month-of-birth. Women giving birth in January found out about their eligibility for additional leave when they had used about half of their baseline leave time. Women giving birth in March had just started their parental leave and firms were not expecting their return in the near-term. The sample is restricted to firms employing exactly one women giving birth in the window around January first.

Table 7: Coworker effects five years later (coefficient on Post$\times$01/02 Cohort)

Outcome	All Firms	Small Firms	Same Occ.
Earnings	-841.8	841.4	-2948.1
	(1516.8)	(1654.1)	(2096.0)
Changed jobs	0.0118	0.00241	0.0210*
	(0.00635)	(0.00519)	(0.0106)
Not working	0.000213	-0.00281	-0.00189
	(0.00222)	(0.00359)	(0.00349)
N	2466002	396339	743279

This table reports the effect of the reform on coworkers earnings five years after a leave event (by year $y + 5$), whether the coworkers were working in a different firm in year $y+5$ relative to year $y-2$, and whether the coworkers had no labor income in year $y+5$. The effect compares outcomes of firms employing women who gave birth in Oct. 1-Dec. 31 relative to firms employing women who gave birth in Jan 1 to Mar 31 of year y, where the latter subset were eligible for an additional 22 weeks of parental leave at full compensation only in year $y = 2002$, controlling for month fixed effects. Sample is restricted to firms employing exactly one women giving birth in the window around January first. Controls include earnings two years before the leave-spell, gender, age, and occupation (at the three digit ISCO level) indicators. Earnings are in yearly DKK, 1000 DKK$\approx$ \$150. Standard errors are clustered at the firm-level. Column (2) restricts the sample to firms employing fewer than 30 employees, and column (3) restricts the sample to only coworkers in the same occupation (3-digit ISCO code) as the woman taking leave.

Table 8: Mother's outcomes in the Short and Long Run

	Earnings	New Job	Unemployment
One Year After Leave-Taking			
Post×01/02 Cohort	-5764.4**	0.0345***	0.00273
	(2101.5)	(0.00619)	(0.00906)
Five Years After Leave-Taking			
	Earnings	New Job	Unemployment
Post×01/02 Cohort	-1236.9	0.0737***	0.00206
	(2633.9)	(0.00968)	(0.00870)
N	59881	59881	59881

This Table reports the effect of the reform on the women taking leave, measuring mother's earnings, probability of working in a different firm than her pre-birth (year $y-2$ employer) and probability of having no labor earnings as measured in November of one year after the leave event $(y+1)$, and five years after the leave event $(y+5)$. The reported coefficients compares outcomes for women who gave birth in Oct. 1- Dec. 31 relative to firms employing women who gave birth in Jan 1 to Mar 31 of year y, where the latter subset were eligible for an additional 22 weeks of parental leave at full compensation only in year $y = 2002$, controlling for month fixed effects. Sample is restricted to women giving birth between October and March in 1999-2005, working in the previous year in a firm in which exactly one woman gave birth in the sample period. Earnings are in yearly DKK, 1000 DKK≈ \$150

6 Appendix Figures

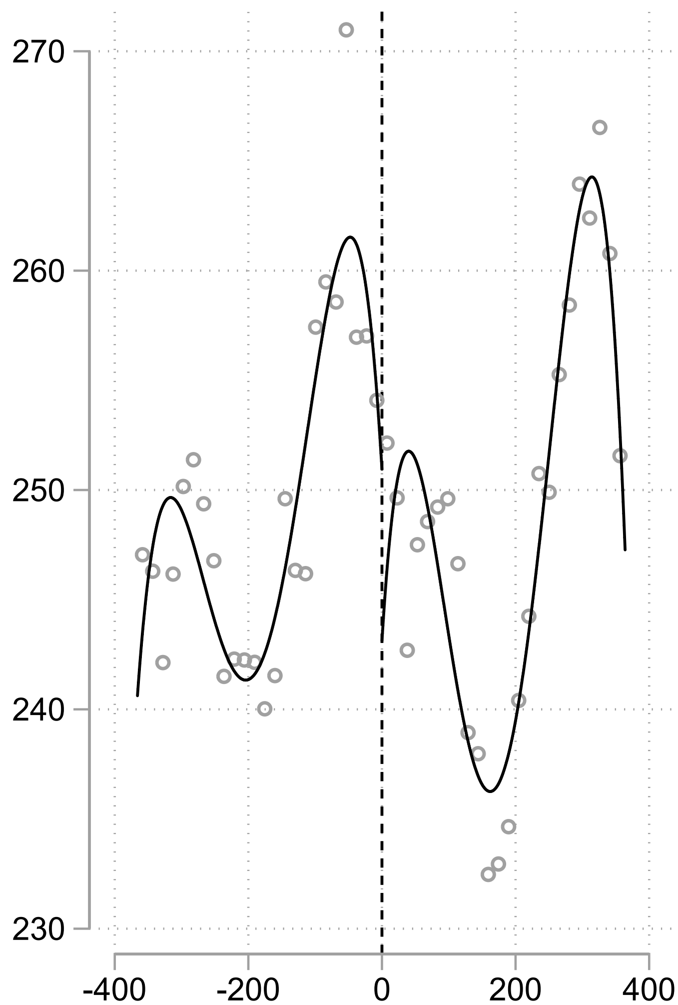

Figure 16: This figure gives a placebo the first stage: women giving birth after vs. before January 1st, 2001 were eligible for the same amount of leave and took the same amount of leave.

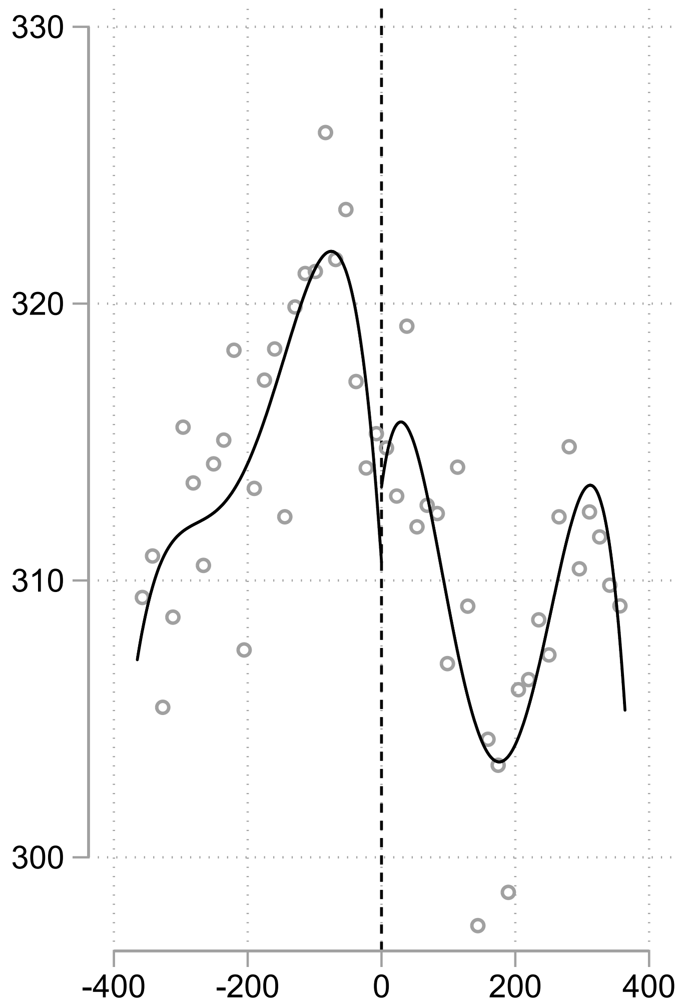

Figure 17: This figure gives a placebo the first stage: women giving birth after vs. before January 1st, 2003 were eligible for the same amount of leave and took the same amount of leave.

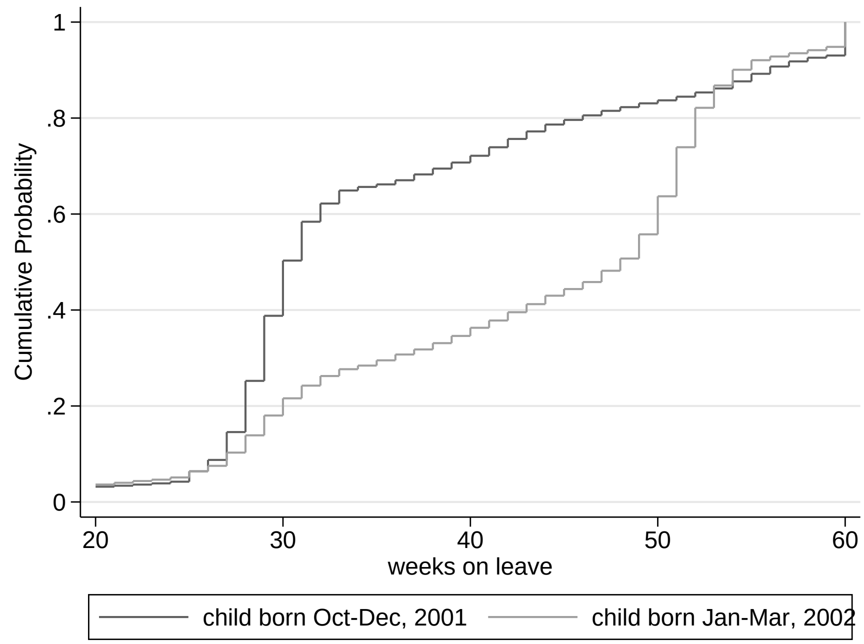

Figure 18: This figure displays the cumulative distribution of total continuous leave spells after having children (the sum of parental leave, childcare leave, and sickness leave).

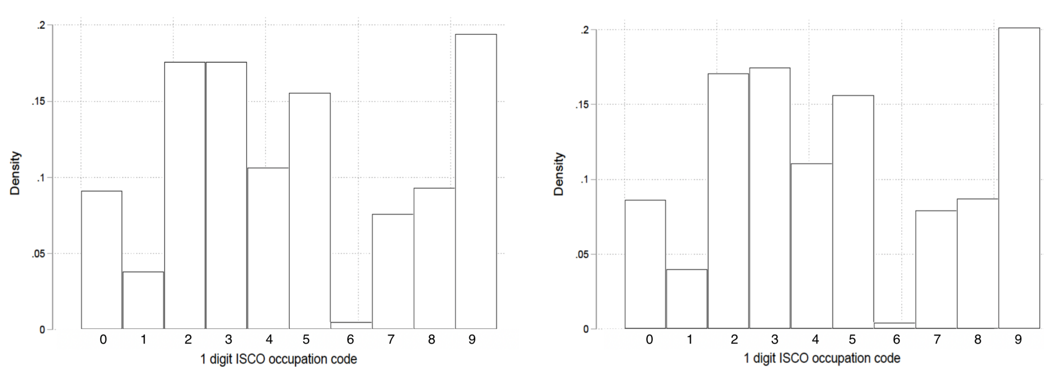

Figure 19: This figure is a historgram of the occupation distribution of mothers giving birth in 2001 compared to mothers giving birth in 2002.

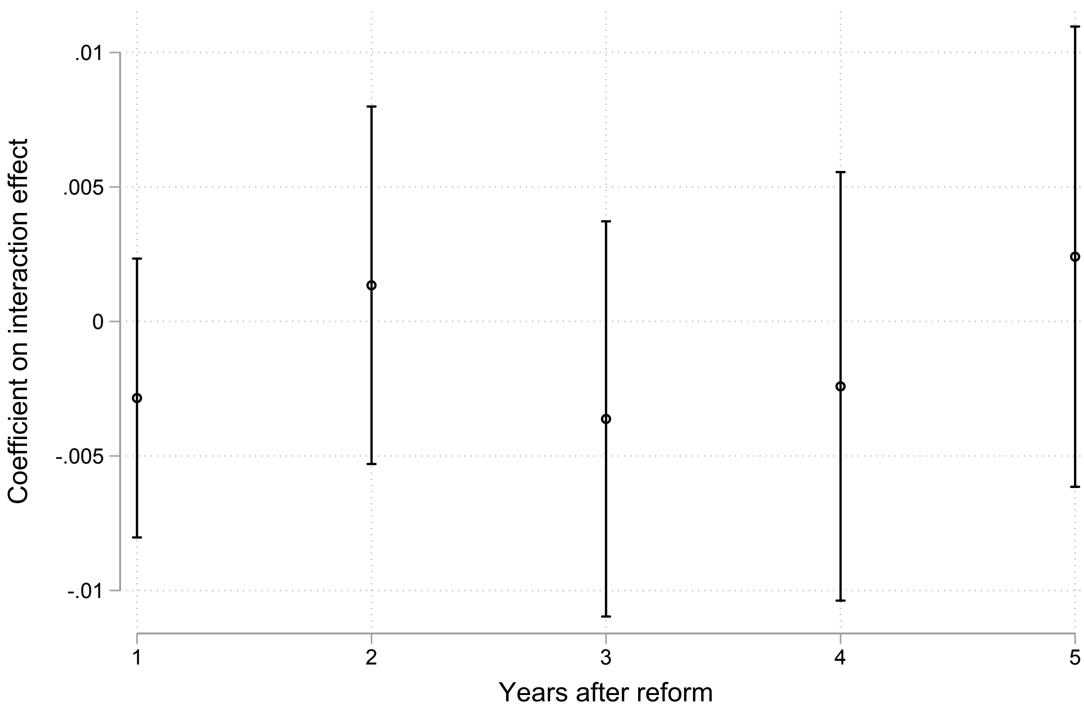

Figure 20: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is working in a new firm 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to small firms (fewer than 30 employees in the baseline period) only.

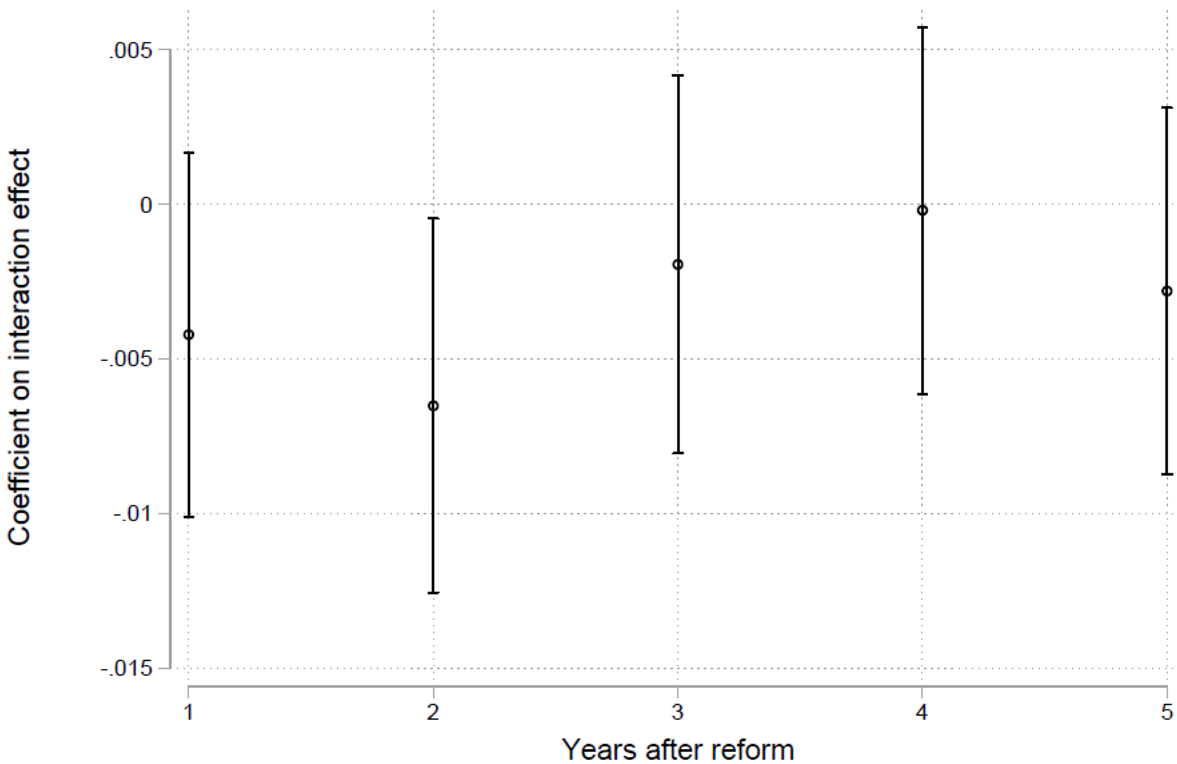

Figure 21: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is an indicator of not having labor earnings 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to small firms (fewer than 30 employees in the baseline period) only.

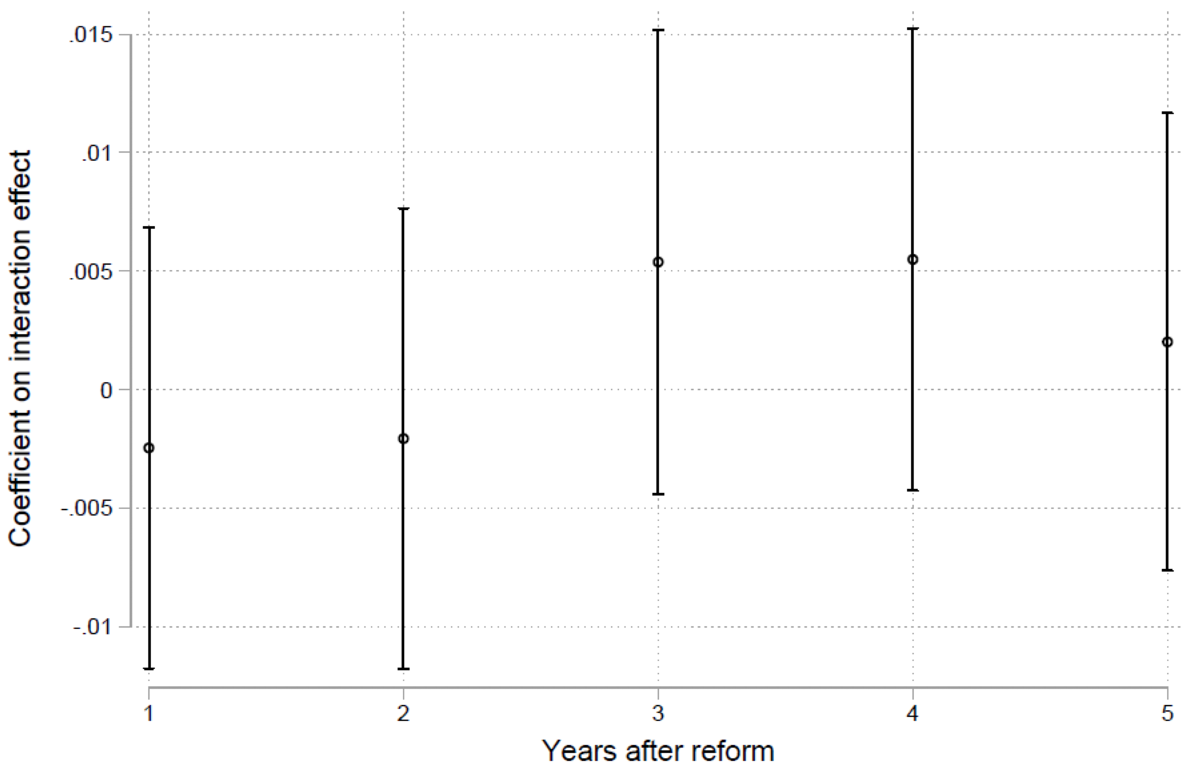

Figure 22: This figure plots the interaction between the post period (Jan-Mar) and the treatment cohort giving birth between Oct, 2001 and Mar, 2002 of regression (1) where the outcome variable is an indicator of not having labor earnings 1-5 years after the co-worker birth event (a separate regression for each year). In these regressions, the sample is restricted to coworkers in the same occupation as the employees giving birth and is restricted to small firms (fewer than 30 employees in the baseline period) only.

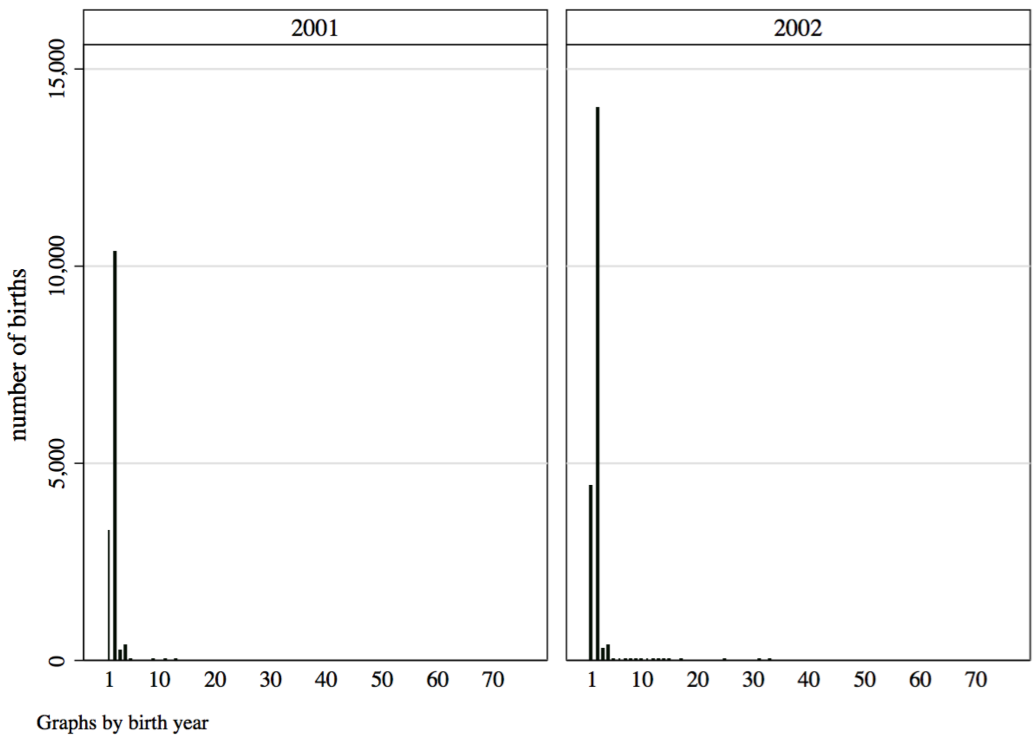

Figure 23: This figure graphs father's parental leave taking after a child is born. Leave length is per child, and divided by the number of children in the case of multiples.

7 Appendix Tables

Table 9: Firm balance (RDDD) by 2 years pre-leave spell

	N	F/N	Sickness $/N$	Parental$/N$
Birthdate	-0.00345**	0.0000191	-0.00440***	0.000468
	(0.00123)	(0.0000130)	(0.000451)	(0.000449)
Post	0.419	0.00179	0.953***	-0.177
	(0.355)	(0.00377)	(0.130)	(0.130)
Post×Birthdate	0.00195	-0.00000862	0.00597***	-0.000618
	(0.00172)	(0.0000183)	(0.000633)	(0.000630)
Post×01/02 Cohort	-0.146	0.00120	0.230	0.400
	(0.875)	(0.00930)	(0.322)	(0.320)
N	98490	98490	98490	98490

Sample is restricted to firms employing exactly one women giving birth in the year before and after January 1st. Interactions with 01/02 Cohort (except the estimate of interest) are omitted for compactness but available upon request.

Table 10: Firms shutdown regressions: RD by year

	1999	2000	2001	2002	2003	2004	2005
RD Estimate	0.000689	0.0590	-0.00284	0.0643**	-0.00552	-0.0408	-0.0306
	(0.0365)	(0.0369)	(0.0250)	(0.0254)	(0.0307)	(0.0284)	(0.0243)
N	8064	8384	14640	14743	14694	14723	14578
Optimal Bandwidth	127.7	112.2	124.9	125.1	94.70	109.9	145.1

Table 11: Firms shutdown year by year

Years after birth event	1	2	3	4	5
Post×01/02 Cohort	-0.000233	0.00940**	0.000904	0.01396***	0.00986**
	(0.00467)	(0.00475)	(0.0456)	(0.00444)	(0.00444)
N	52046	52046	52046	52046	52046

Table 12: Firms shutdown regressions: RD Difference in Difference Bandwidth Robustness

	All firms			
Bandwidth	3 months	6 months	9 months	12 months
Post×01/02 Cohort	0.0510**	0.0687***	0.0500**	0.0284*
	(0.0203)	(0.0169)	(0.0156)	(0.0150)
N	48021	71464	84052	89826

Sample is restricted to firms employing exactly one women giving birth in the window around January first.

Table 13: Coworker effects five years later: RDDD

	Earnings	Changed Jobs	Unemployed	Parental	Sickness
Post×01/02 Cohort	442.8	0.00169	-0.00617	0.846*	-0.580
	(1839.0)	(0.00426)	(0.00429)	(0.400)	(0.393)
N	859802	1081452	1081452	1081452	1081452

Sample is restricted to firms employing exactly one women giving birth in the window around January first. Controls include earnings two years before the leave-spell, gender, age, and occupation (at the three digit ISCO level) indicators. Firms with fewer than 30 employees only are included.