The effect of maternity leave extensions on firms and coworkers *

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Abstract

While a large literature is devoted to understanding the impact of maternity leave on children's outcomes and the careers of women, less is known about the consequences of maternity leave at the workplace. This paper studies the effects of maternity leave on firms and coworkers by examining a 2002 Danish reform which increased the length of parental leave by 22 weeks. The timing of the policy change gives random variation in the length of leave available to women who gave birth around the time of the reform. I find no detectable effect of the reform on the earnings or promotions of coworkers in any of the five years after the reform (point estimates are about $100) and can reject differences in yearly earnings larger than $425 overall and differences larger than $280 for female coworkers. While there are some costs for coworkers in the same occupation as women who give birth in the sample period, these costs are 1-1.5 percent of earnings. I also find evidence that the reform increases the probability of firm shut-down by about two percentage points five years after the reform, concentrated among relatively small firms. Conditional on survival, I find no impact of the reform on firm value added.

JEL Classification: J38, J32, J24, L23

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1 Introduction

Despite substantial discussion of the impact of maternity leave on the future career paths of those who take leave, we know nothing about the response of firms and coworkers to the absence of an employee on parental leave. This paper is the first to document the effects of an exogenous increase in the length of parental leave on coworkers' career paths as well as on firm growth. 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.[1]

The effect of maternity leave on coworkers and firms has played an important role in the policy discussion of the implementation of the The Family and Medical Leave Act (FMLA) in the United States. In a Senate hearing of the Committee on Health, Education, and Pensions, Wyoming Senator Mike Enzi notes that:

> We must also bear in mind that when Congress enacted the FMLA there were genuine concerns about weighing the needs of employees confronted by serious health issues, the burdens which their absence might place on their coworkers who must shoulder the additional work, and the legitimate need of employers for a steady and reliable work force. Those concerns are equally important today.

The hearing includes specific examples of these costs, for example, the testimony of a small business owner describing the problem of a woman taking 3 months of leave, during which the company paid for the employee's health insurance and supplemented the worker's income for 6 weeks [Senate, June 23, 2005]. The employee did not return to the company and the business owner argues that replacing her was very costly. In Denmark, 40% of women do not return to the same job after giving birth and it is generally personally optimal for workers to keep this information from their employers. In addition, when workers are not easily substitutable, or when a worker has a great deal of firm-specific knowledge which takes time to build, long absences can become costly to the firm.

The effect of maternity leave extensions on coworkers is not obvious. If these extensions are costly to firms, then we should expect this to show up in wages. If, however, employees' responsibilities increase in response to the parental leave taken by coworkers, then we may expect

[1] As I discuss in Section 3, before the 2002 change, workers could take a long parental leave, but only at partial (60%) compensation. The reform increased the amount of time they could stay away from work and receive full pay benefits.

earnings to increase and promotions to be more likely among remaining coworkers. If workers are not adequately compensated for taking on additional work, it may be that they eventually switch to lower paying (but less demanding) jobs. All of these outcomes are observable and testable using Danish register data linked to birth records around the time of the parental leave reform.

This paper estimates earnings paths of coworkers in firms employing women in 2000 who gave birth between October 2001 and March 2002, where the women who gave birth in the latter half of this period were eligible for 22 weeks more parental leave at full compensation than those who gave birth before the January 1st cutoff. On average, there is no impact of the reform on coworker wages or promotions. The point estimates are very precise—for female coworkers, I can reject yearly earnings losses greater than \$160 five years after the reform and gains greater than \$280 five years after the reform. However, there is some evidence of stress on workers in the same occupation as women taking leave. These workers have somewhat smaller earnings even 5 years after the leave-taking event and switch to lower-pay jobs in the period immediately following leave-taking. I also find some evidence that the reform is costly for firms. Firms exposed to more leave are 2 percentage points more likely to be shut down by five years after the reform. This effect is concentrated in firms with between 16 and 30 employees and affects a small fraction of the actual labor force. Conditional on survival I do not find any evidence of negative effects on firms. Placebo regressions for births which take place one year after the reform do not show any effect on coworker wages (even same-occupation coworkers) or firm survival.

The 2002 parental leave reform lengthened the amount of maternity leave taken by many women. The average effect on leave taking is an increase of a little over one month and the modal length of leave moves from around 28 weeks to 52 weeks. In this paper, I document the effects of the 2002 parental leave reform on the outcomes of coworkers of women who gave birth around the time of the reform. Since the reform was retroactively applied—it was enacted in March, 2002 and first discussed (without detail or dates) in November, 2001—it is unlikely that women who gave birth between January and March 2002 differed substantially from those who gave birth between October and December 2001, but these groups of women were eligible for different maternity leave lengths. I find some evidence of negative effects on coworkers in the subset of coworkers in the same occupation as women taking leave, however, the overall impact on coworkers of this parental leave reform is small.

The Danish parental leave reform studied here was much larger than proposed extensions in other European countries and certainly larger than current policy proposals in the US which feature paid maternity leave. The effect of the reform on actual leave-taking was approximately of the same magnitude as proposed reforms in the US, where actual time away from work will likely not change by as much as the reform allows. The effect of this massive reform on the workplace was quite small. It is still possible that more moderate policy changes will have dramatic effects on the workplace, but in light of the evidence presented here this seems less likely. Before discussing the results in more detail, section 2 of this paper discusses the literature estimating maternity leave effects. Section 3 gives a detailed description of the policy change and response to the policy change. Section 4 discusses the data and Section 5 presents the results in more detail along with robustness and placebo tests. Section 6 concludes.

2 Related literature

Dahl et al. [2016] study a series of reforms in Norway which increased paid maternity leave from 18 to 35 weeks. They found that in each reform the average increase in the number of weeks of leave taken was equal to the number of weeks granted (each expansion was between 2 and 4 weeks). Using a regression discontinuity design comparing women who gave birth immediately prior to the reforms to those who gave birth after the reforms, they find that increased maternity leave led to: no reduction in family income (the reforms generally replaced income at 100%), had little effect on children's school outcomes, had no effect on parental earnings and participation in the short or in the long run, had no effect on family structure outcomes such as completed fertility, marriage, or divorce. Finally the authors note that families with higher income are more likely to be eligible for paid leave (since they are working), so the expansions of paid leave are regressive. Given full take-up, the authors argue that these expansions in paid leave were very costly (about 0.5% of GDP in 1992), without any demonstrable effect on children's educational outcomes or family structure.

Relative to the Norwegian reforms, the reform I study here was larger (in the sense of increased leave time); the ultimate amount of leave time taken when using the full allowed time in Denmark is a substantial 50% longer. If time away from work has any impact on the workplace, it is likely larger when time away is longer. The Norwegian reforms studied by Dahl et al. [2016] ended ten years before the Danish reform studied here.

Baker and Milligan [2010] study a reform in Canada which increased paid maternity leave from about 6 months to one year (approximately the level of expansion studied here) in most provinces. Comparing across provinces with similar women but different levels of maternity leave due to the reform, the authors found that increased maternity leave did increase the amount of time mothers spent with their children and decrease their work hours, but it did not have any impact on the children's development. There was no impact on family dysfunction (measured through a survey-based scale) and no impact on the motor and social development of children. There was little to no impact on child temperament (how much crying or smiling the child does, for example, as reported by parents). Baker and Milligan [2008a] study the impact of leave length extensions on mother's re-employment and find that modest increases in mandated leave (introduction of mandated leave of 17-18 weeks) do not affect mother's time spent with their children, but did affect their probability of returning to the same employer after they give birth. Longer increases in maternity leave mandates (of up to 70 weeks) increased the time mother's spent away from work and also increased re-employment.

In another paper studying Canadian maternity leave mandates, Baker and Milligan [2008b] find that even though mothers do breastfeed more under maternity leave extensions, these increases in breastfeeding do not translate to better child or maternal health outcomes. Dustmann and Schonberg [2012] study reforms in Germany and find that substantial maternity leave expansions did not impact children's outcomes well after the reforms—the null results on children's wages at age 28 and days in school are quite precisely estimated. Beuchert et al. [2016] also study the impact of maternity leave expansions on the health outcomes of families, but their study uses exactly the Danish policy reform in this paper. They find little or no effect of the Danish reform on child or maternal health outcomes (measured by hospital admissions). However, comparing outcomes for infants across countries in Europe over time, Ruhm [2000] finds evidence of a positive health impact of more generous leave policies, noting that positive effects are concentrated in post-birth outcomes relative to pre-birth outcomes (which are unlikely to be affected by parental leave length).

The impacts of leave extensions on mother's labor market participation are also generally short-lived. Lalive et al. [2014] study the effect of an Austrian reform 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, Schnberg and Ludsteck [2014] also does not find substantial long run effects of maternity leave extensions. Appendix Table 9 displays the effects of the Danish reform studied in this paper on mother's labor market outcomes in the year of the reform, 3 years after the reform, and 5 years after the reform. There are no long run impacts of the reform, though it does affect time away from work in the short run.

In general, positive evidence of the health and labor market impacts of parental leave is concentrated in studies of the introduction of paid leave, rather than in studies of leave extensions (Rossin-Slater [2017]). Unlike the extensions described above, the introduction of paid leave in Norway also seemed to have positive effects on affected children's completion of high school and earnings (Carneiro et al. [2015]). Rossin [2011] finds positive effects of the introduction of the FMLA on child health, Rossin-Slater et al. [2013] find positive impacts of California's paid leave on mother's hours worked.

Of course, these direct effects of maternity leave expansions are an important part of understanding the optimal length of maternity leave from a policy perspective. However, these studies are missing the potential costs of long worker absences to firms and potential spillover costs to coworkers. Thomas [2015] documents the indirect effects of more maternity leave protection, specifically, the general equilibrium impacts of the FMLA in the US. She finds evidence that this job protection for mothers led to a lower rate of promotion for women and that part of the reason for the slow-down in promotions was that firms know that probability that any given women will reduce her hours in the future is larger when there are more future mothers in the workplace which happens when the workplace becomes more attractive to mothers. The firm response to this lower expected commitment is to reduce training and investment in all women, even those who would not have reduced their hours.

Dahl et al. [2014] study peer effects in take-up of paternity leave. They find that coworkers are 11 percentage points more likely to take up paternity leave if their peer was eligible for an extra month of paternity leave (which could not be shared with the mother). This could be because coworkers learn about the existence of this paternity leave or because they see how the firm reacts to fathers taking leave. In general this result will be important for the interpretation of the effects estimated in this paper, especially the impact of extra maternity leave on female coworkers. Results on female coworkers should be interpreted with caution, since it may be that these women learn about the generosity of parental leave through their coworkers and the results reported here on earnings and promotions reflect changes in their behavior based on this information, rather than costs of coworker absence. I split the sample by gender and find similar effects for men compared to women in the main results.

This paper directly estimates the total effects (positive or negative) of parental leave extensions on coworkers and firms. As suggested in Dahl et al. [2014], these reforms may result in behavioral changes but since maternity leave expansions have generally not been found to increase fertility [Dahl et al., 2016], any behavioral changes are likely small. This paper adds to a set of results which suggest that the direct effects of paid maternity leave expansions on the health and education of children are very small or zero. As I show in Section 5, the effect on long run female labor market participation is small, as is the case in other Nordic countries with already generous parental leave. This paper add the finding that there is some disruption to the workplace induced by extra parental leave, especially for workers in the same occupation as those taking leave, but that the overall workplace cost of extra parental leave is low.

3 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).[2] 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, maternal leave remained the same, paternal leave was reduced by two weeks, but parental leave was extended from 10 to 32 weeks at full pay. Previously, parents could extend their benefits to 52 weeks, but only at 60% compensation.[3] 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, for up to 52 weeks.

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. Since optimizing around the

[2]Firms may top-off this public benefit, and compensation schedules are usually decided at the sector-level in collective bargaining agreements.

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

policy would have required planning of at least nine months, it is likely that the only immediate impact of this extra maternity leave was the taking of extra maternity leave, not the birth of marginal children. As Figure 1 shows, the reform extended the average length of maternity leave, from a modal 28 weeks in 2001 to 52 weeks in 2002.[4] In 2001, some mothers do take longer maternity leave at a lower compensation rate but in 2002 the number of women taking 52 week leaves nearly quadruples. While some firms in the sample were not affected by the reform and by parental leave taking in general, those firms who were exposed to leave were also exposed to a substantial increase in leave-taking.

In the early 2000s, Danish fathers took very little of the shared leave. Figure 2 below 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.[5] In Figure 3, I replicate Figure 1 in Beuchert et al (2016), who impute from data on payments to mothers on leave a reliable measure of leave length. These absences include maternity leave, parental leave, and other child-related leaves. The jump in average time away from work—32 days—is not as large as the increase in parental leave, since some mothers were taking leave at partial compensation before the reform.[6] The jump in leave-taking by child birth-date occurs immediately and very discontinuously on January 1st, with no movement after the reform was announced in late March. This suggests that virtually all mothers took advantage of the reform, even though adoption of the new rules was optional for births between January 1st and March 26th.

Of course, 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 of 32 days in absence from work, but rather by the largest changes in leave-taking (as large a 22 weeks) which make up this average 32 day increase. Though we cannot observe how much each individual woman increased her leave-taking by, it is clear from the histogram in Figure 1 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 52 weeks of leave after the reform. In the next section, I

[4]Henceforth in this paper, I use the word "maternity leave" to indicate leave taken by the mother to care for a child and recover from childbirth, which is technically a combination of maternity leave, shared parental leave, and shared childcare leave.

[5]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.

[6]Using spell data directly, I get a larger estimate of the first stage, so this 32 day increase based on compensation data is the more conservative measure.

discuss the data used in this paper and the framework for analysis.

4 Data and Model

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 4 below gives the number of births in each calendar month between 2001 and 2003. 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. As a robustness check, I use data on hospitalizations and diagnoses to eliminate from the sample firms and coworkers of women receiving elective c-sections in the sample period. The average number of births between October and December is approximately equal to the number of births between January and March.

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[7], and occupation.[8] 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: *lbnr* which corresponds to a physical work address (plant) and *cvrnr* which is the tax ID of the firm. Purely because lbnr is more commonly available, I use lbnr in this study whenever possible. Studying firm value added per worker does require the cvrnr. Unless otherwise noted, any mention of effects on the firm refer to a physical plant/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.

It is also possible to know the length of maternity leave taken by women who gave birth in late 2001 and early 2002. In the Danish data, maternity payments received from either the

[7] I only use "high-quality" hours measures (tlonkval ≤ 50). This skews the hours sample to full-time workers.

[8] Broad occupation categories are manager, high-skilled, white-collar, and low-skilled. In regressions, I use 3-digit ISCO categories as control variables. When reporting differences across occupations, I use the broader categories.

government or the firm (who would receive a subsidy form the government) are recorded. For each employed mother, we know the date the first benefit was received and the total benefit received. Given benefit rates, Beuchert et al. [2016] are able to construct maternity leave spells. I use this maternity spell data to demonstrate the large take-up of benefits in the histograms in Figure 1 and 2. There are 9,734 firms in the dataset which employed one woman in November, 2000 who gave birth between October 1st, 2001 and March 31st 2002. An additional 3,069 firms employed more than one woman who gave birth in the sample period. There are 20,724 women who gave birth in the sample period employed in November, 2000. There are not significant differences between firms employing women who gave birth before they were eligible for 52 weeks of paid leave and those employing women who gave birth after the policy cutoff. Table 1 below reports average age and fraction male in firms as well as firm size, by whether women giving birth in these firms were eligible for 52 weeks of fully-paid leave.

The effect of an increase in maternity leave on coworker outcomes depends on the nature of production in the firm. Suppose, for example, that output follows the Kremer [1993] O-ring Model in which the slowest worker on a team determines that team's rate of production. In this case we would expect that large effects from a co-worker absence (assuming any replacement is less productive). Another possibility is that workers are perfect substitutes for one another. In this case, one worker going on maternity leave will be replaced by another worker, temporarily and at low cost in a flexible labor market.[9] Using German administrative data, Jäger [2016] finds that the wages of workers in similar occupations rise, while the wages of workers in other occupations fall when a coworker dies, effectively ruling out a frictionless model of the labor market in which workers are perfectly substitutable.

While worker death is more permanent than maternity leave and may involve psychic costs to coworkers, the fact that firms have to hold positions open for women taking maternity leave adds an additional friction for the firm to overcome. In the case of worker death, the firm's optimal policy is clear–they must find a replacement. In the case of job-protected absence, firms may not look for a replacement, shifting work onto coworkers. Firms cannot hire a permanent replacement for a worker taking leave without increasing the size of their labor force. The quality

[9]Denmark operates under a "flexicurity" model, its labor market is as flexible or more flexible than that of the US when looking at the number of new hires and separations within a year. Low costs of hiring and firing are supplemented with government-provided security in the form of generous welfare and job-finding help in the case of unemployment.

of a temporary substitute may be low enough to pull down all workers' wages. This may cause the firm to forgo hiring an additional worker and extract more hours and effort from its current workforce. Of 20,718 moms working in 2000 who gave birth in the sample window, only 9,088 were working in the same firm in November of 2003. 60% return to the same job at some point before November, 2007. Overall, the effect of job-protected leave on coworker pay is ambiguous, potentially large, and unknown.

Even if finding a substitute for a women going on maternity leave is not difficult, there may be promotion-based effects of increases in maternity leave on coworkers. In particular, if promotions are internal and depend in part on tenure at the firm, a new worker hired to replace a women taking maternity leave will be less likely to be promoted than she would have been. Holding the promotion rate fixed, her coworker's probability of promotion will increase.

In the next section, I present results the effect of the 2002 Danish reform on the path of worker's wages up to five years after they are exposed to a parental leave spell of a coworkers. I also look at promotion rates, wage changes associated with job-to-job transitions after the reform, and at heterogeneity in these effects by the occupation of workers. Finally, I present results on firm shutdown. Unfortunately, I don't have power to meaningfully test for changes in total number of hours in the firm or value added per worker, though point estimates suggest that firm size grows (conditional on survival) by about half of a full-time worker when firms are exposed to more parental leave and that there is no impact on value added per worker.

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. The regressions reported below take the form

$$y_{it} = \alpha + \beta \frac{\sum_{j \in \{c^{2000}\}} b_j 1[d_j > D]}{\sum_{j \in \{c^{2000}\}} b_j} + \gamma X_{it} + g(\bar{d}) + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome of interest for worker i at date t, $\{c^{2000}\}$ is the set of coworkers in 2000. b_j indicates whether coworker j gave birth in the sample window, d_j is the date of birth, and D is the date workers are eligible for the new policy. In this case, $D =$ January 1st, 2002. g is a flexible polynomial in the date of coworker birth, and can vary in the pre- vs. post- treatment window. In the main analysis, I use a linear term for g, which differs in the pre- and post- period, where the running variable is measured in days since January 1st, 2002. 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. Many specifications will require me to restrict attention to firms with only one birth between October 2001 and March 2002, in which case $\frac{\sum_{j\in\{c^{2000}\}} b_j 1[d_j > D]}{\sum_{j\in\{c^{2000}\}} b_j}$ is simply an indicator of whether the birth took place after the reform cutoff of January 1st.

In particular, adding date of coworker birth controls makes sense only when there is one coworker birth in the sample window. In this case the regression above is the traditional regression discontinuity design (see Lee and Lemieux [2010] for a discussion). All of the results reported below are robust to restricting the sample to one-birth firms with a flexible polynomial in date of birth. Of course most firms are one-birth firms since the sample window is small. The regressions below use the full sample and do not include the date of birth control unless otherwise noted. Appendix Figures 13 and 14 plot the average earnings and firm shutdown probability in 2007 by date that coworkers gave birth. There are clearly no trends related to the running variable and for this reason I provide not only regression discontinuity estimates, but simple regressions which omit the running variable and give average differences between treatment and control. This does not effect the direction or magnitude of results and gives me power to reject smaller effects.

I estimate the equation above in the five years before coworker birth and the five years after coworker birth: 1997-2007. There should be no effect of extra maternity leave on coworker outcomes before the birth takes place and policy change is announced, so the pre-reform years are included primarily to show that there are not pre-trends in coworker wages driving any results. These regressions also demonstrate balance since they effectively give the difference in outcomes for workers exogenously exposed to extra maternity leave.

An alternative specification would take date of birth as an instrument for maternity leave, and the first stage in such a regression is strong. The question asked here is the policy impact of maternity leave extensions, which include effects of partial take-up (or possibly reduced take-up for some workers[10]). I provide results of the instrumental variable strategy, which gives the

[10] As is discussed in section 4.3, one problem with using this instrument is that data is available on the length of state-funded parental leave, not on the actual absence of women from work. The straightforward interpretation of the IV specification would be that it gives the effect of increasing the length of leave taken as induced by the reform. But this is not necessarily the case since we do not know if workers took additional leave out of their vacation days, sick-days, or through special provisions in their contract with their firm. For this reason, I prefer

effect of an average month longer of leave among workers who responded to the policy, in the appendix.

5 Results

In this section, I present results on the impact of the 2002 maternity leave extension the coworkers of women affected by the reform and their firms. First, I note that the extension did not have major long run effects on the career paths of mothers themselves, consistent with Dahl et al. [2016] and Lalive et al. [2014]. The direct effect of this maternity leave extension on mother's long run earnings and employment is small and not statistically different from zero. As displayed in Appendix Table 9, by 2007 there is no difference in the probability that a mother who gave birth after January 1st, 2002 is more likely to work at a different firm or be unemployed relative to a mother who gave birth between October and December of 2001.

Column 1 of Appendix Table 9 gives the coefficient on a regression of labor market outcomes listed (2002, 2005, and 2007 earnings, workplace, and employment) on an indicator of whether a woman gave birth between January 1st and March 31st of 2002, where the sample includes mothers who gave birth between October 1st, 2001 and March 31, 2002. Column 2 of Table 9 gives the same coefficient but includes a variety of controls in order to reduce standard errors. Column 3 is a regression discontinuity, adding as a running variable date of the child's birth interacted with the post-reform indicator. Overall the regression discontinuity suggests an insignificant difference of at most 2444 danish kroner, or about $360 in earnings by 2007, and no difference in earnings under other specifications.

Next, I present results on coworker earnings and career paths. The sample includes only coworkers of women who gave birth between October, 2001 and March, 2002, where women who gave birth between January and March 2002 were eligible for almost twice as much paid leave at full compensation. In addition, I check for heterogenous effects by occupation (that of the woman taking leave and that of her coworkers) and firm size. Finally, I study the effects of eligibility for extended leave on firms, focusing on the probability that a branch shuts down.

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the cleaner analysis which relies only on proper measurement of birthdates. There are also 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]

5.1 Effects of maternity leave extensions on coworkers

Overall, the effects of maternity leave extensions on coworkers are small. Point estimates suggest that by 2007, coworkers of women eligible for extra leave due to the 2002 reform were making 659 DKK (or about $100) more per year than coworkers of women ineligible for the extra leave. Table 2 gives the results of regression (1) for 2002, 2005, and 2007 (the year of the reform, three years later, and five years later) under a variety of specifications. In no year and under no specifications is the effect of additional maternity leave eligibility on coworker earnings significantly different from zero. Moving from column 1, which includes no controls, to column 2, which includes controls for occupation, industry, age, and gender, substantially reduces standard errors, giving me power to reject even small differences in earnings. For female coworkers (column 4), I can reject differences in 2007 earnings larger than $280, or half of a percentage point of yearly earnings. Restricting to the subsample of firms with only one birth between October, 2001 and March, 2002 (column 5) also does not substantially change results. Adding a regression-discontinuity design in which the running variable is child's date of birth in column 6 raises standard errors to the they were at without any controls, but point estimates in 2007 are very small, as in the other specifications. Figure 5 plots estimates analogous to those in column 2 from 1997-2007. There is no economically significant difference in the time trends for workers exposed to more vs. less parental leave.

It is possible that there is some heterogeneity in effects by firm size. All else equal, workers at smaller firms should be on average more affected by one coworker absence than workers at larger firms. In addition, we may expect substantial heterogeneity by occupation—workers in the same occupation as a woman taking leave may be more affected compared than other coworkers. In Table 3 I give results restricting to firms with fewer than 30 employees (column 1), workers in the same occupation as a woman giving birth (column 2), and adding an interaction term for the number of employees in a firm (column 3) as well as the number of employees in the same occupation as a woman giving birth, for the set of small firms (column 4). For compatibility across the rows of this table, all of the regressions are restricted to one-birth firms. The results restricting to firms with fewer than 30 employees have point estimates as small as those in Table 2—there is no average effect of parental leave extensions on coworker earnings in any of the five years after the reform, with point estimates around $75 (500DKK) per year.

This is not the case when turning to column 2 of Table 3. In this case, earnings differences are about \$350-\$550 per year, and are significantly different from zero in 2005 and 2002. Figure 6 plots the coefficients b from the regression

$$y_i^t = \alpha^t + b^t 1[same\ occ] \times 1[d_j > D]_i + \beta_1^t 1[same\ occ]_t + \beta_2^t 1[d_j > D]_i + \gamma^t X_i + g(d_j) + \varepsilon_i^t \quad (2)$$

for $t = 1997-2007$, where $1[same\ occ]$ indicates that in 2000, worker i was in the same occupation as a woman j who took parental leave in the sample period, $1[d_j > D]$ indicates that this woman gave birth after the cutoff date of Jan 1, 2002 for additional leave eligibility, X includes age, gender, industry, and occupation dummies, and $g(d_j)$ is a linear running variable in date of birth which varies in the pre-and post eligibility period. Standard errors are clustered at the firm j level and only firms with one birth are included. This specification allows me to control for occupational heterogeneity in the effect of the reform, since simply restricting the sample to workers in the same occupation also selects the sample on occupations where women about to give birth tend to work. Though point estimates are different from zero at the 5% level only in two years—2003 and 2006—there is a clear dip in same-occupation coworker earnings beginning in 2001, when the future mothers were pregnant. There is not a difference on average in the probability that coworkers change jobs in response to more parental leave, however, there is a difference in the wage change associated with a job change in the period immediately following the maternity leave spell of a coworker. Figure 7 plots the difference in log wage changes for for workers changing jobs in that year explained by more exposure to parental leave. Conditional on changing jobs, workers exposed to more maternity leave take a 0.5% loss in wage growth in the year after their coworkers take parental leave. In other years, the wage changes are comparable between the groups[11]. Together these findings point to some stress on employees beginning around the time their coworkers are taking parental leave. If employers are asking these workers to take on additional tasks, workers may prefer to take lower-paying jobs (as in Figure 7). The initial negative effects in Figure 6 are only among workers who change jobs or leave the firm. When I restrict the "same occupation" group to workers who remain in their 2000 firm, the negative effects begin to appear only in 2003 (but are never significantly different from zero), as in Figure 8. Even in the long run, workers in the same occupation as women taking leave suffer some earnings losses when they are exposed to substantially more leave. In the next section, I

[11]this result is driven by workers in the same occupation as the women taking leave, though the difference is not significant

will discuss the effects on firms which may help explain this finding.

Returning to Table 3 we see that there are not significant differences in the main effect when interacting the post-reform indicator with the number of employees in a firm or with the number of employees in an occupation, though as expected more workers in a firm and more worker in an occupation mitigate the effects of expose to additional leave. Figure 9 splits the regression by the size of their firm. In large firms there is a persistent pre-trend of about \$150 difference between the earnings of coworkers who will eventually work with women who give birth after the reform eligibility compared to before, but this difference is driven by very large firms with many pregnancies (it disappears when restricting to firms with one birth in the sample period). In smaller firms the estimates fluctuate from a difference of -\$150 to \$150, but are not significantly different from zero in any period. The estimates for medium-sized firms are more stable and generally positive. They are not significantly different from zero in any period. From this figure and Table 3 I conclude that there are not substantial differences in the effect of additional parental leave on coworkers based on firm size.

Figure 10 plots the coefficient from a regression of log wages on the fraction of women in the firm giving birth eligible for additional leave due to the reform by the coworker's occupation. I use log wages rather than earnings to make effect sizes comparable in this decomposition, as managers make substantially more than low-skilled workers. Differences in year 2000 are indexed to zero and standard errors are omitted, but none of these estimates is significantly different from zero. The timeseries of these regression coefficients are not substantially different from one another based on coworker occupation. There are also not major differences by mother's occupation, though these are much more noisily estimated. Figure 11 plots estimates of the regression coefficients based on mother's occupation. There are too few managers giving birth in the sample period to include that breakdown, and there are fairly few high-skilled mothers so those estimates fluctuate enormously year-to-year. Most mothers in the sample are white-collar and low-skilled. The point estimates of the effect for these groups are low and follow similar paths.

Finally, I consider the promotion paths of workers. In particular, I take an indicator for whether a worker is in a management position as the outcome variable in regression (1). Table 4 gives the results of this regression under a variety of specifications, as in 2, restricting the sample to workers who were not managers in 2000. I can reject difference in the coworker's probability of being promoted to management as a result of the reform larger than a tenth of a percentage point. To give meaning to this figure, in 2007, 1.91% of workers who were not in management positions in 2000 were in management positions. So I can reject differences in promotion to management of about 5 percent of the level. Figure 12 plots a similar outcome over time—the coefficients on whether a worker is a manager (so that all years can be included). There is no baseline difference and no difference as a result of the taking of leave in the probability that coworkers are in management positions.

5.2 Effects of maternity leave extensions on firms

The final set of results in this paper turns from coworkers to firms. In this section, I restrict to firms with more than 5 employees to avoid family businesses whose employment records are less reliable and because accounting data is unavailable for firms with fewer than five employees. Figure 13 displays coefficients from a regression of whether it is the firm's last year in the dataset on the fraction of births at the firm which took place after the reform and were eligible for more parental leave. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002. Table 5 presents the probability that a firm is shutdown—there are no workers at that plant—in 2007. Overall, firms exposed to more maternity leave due to the reform are 6.94% (or about 2 percentage points) more likely to be shutdown by 2007. This result is remarkably stable across specifications, however it is just shy of significance at the 10% level using the regression discontinuity approach in column 4, despite increasing in magnitude to 3.2 percentage points.

Figure 13 presents the coefficients from regression (1) in which the outcome variable is an indicator of whether the firm exists in a given year. In the baseline there is a larger probability of shutdown among firms exposed to less maternity leave, and the difference is significant in 2002. However this is somewhat mechanical since women who give birth in 2002 will still be on employment rolls in that year, while women giving birth in 2001 may not be. In fact, restricting to firms with more than 15 employees, the 2002 coefficient is 0.0028 which is insignificantly different from zero. These firms are however more likely to be shut down in 2007. Table 6 describes the shutdown-probabilities by firm size. All firms are more likely to be shut down by 2007 if they were exposed to more parental leave, but the effect is largest for firms with 16-30 employees. About five percent of the workforce in my sample overall is in firms this size, though they represent about 25% of firms in the data.

In Denmark, accounting data is available for firms (broadly available after 1999). These data include measures of value added and total number of full time equivalent workers. There are three main drawbacks to using this data. First, this information is available at the firm rather than establishment level. This means that it excludes the public sector and in general has far fewer observations than the exercises above (because all plants will be linked together). A bigger problem with the data is that firms are surveyed based on size and while firms with 50 or more employees are well represented in the data (surveyed annually), smaller firms are not surveyed every year—the very presence of a firm in the accounting data indicates success. Imputed values are available for these firms, but measurement error coming from that imputation makes this exercise—particularly the regression discontinuity—impractical. Finally, the measure of full time equivalent workers (aarsv) explicitly excludes temporary workers, so I do not have a solid measure of the effect on firm size.

Nonetheless, I provide estimates of the effects of the maternity leave extension on value added per worker (revenue less the cost of material inputs and capital rental) and full time equivalent workers. Though I cannot reject large effects on firm value-added and labor force size, point estimates do not suggest that conditional on survival firms suffer from an increase in parental leave. In particular, Table 7 shows that though my confidence bands are about 5% differences in value added per worker, point estimates are very small, around 1 percent. Note that the workers in this sample are those currently in the firm, which differs from the coworker regressions above in which coworker groups were determined in 2000 and after that could change firms, become unemployed, etc. In addition, Table 8 implies that firms hire about 17 hours more per week of labor (half a full-time equivalent worker) and seem to keep this worker years after the reform when they are exposed to 22 weeks more of potential parental leave. Unfortunately, the standard errors on this estimate are wide and include zero.

Overall, the results suggest that parental leave extensions result in a 2 percentage point larger probability of firm shutdown 5 years after the reform. This shutdown is driven by firms with 16-30 employees, not by larger or smaller firms. There do not seem to be negative effects on value added per worker for surviving firms, though I can't rule out effects as large as 5%. In addition, according to my point estimates, these firms hire about 17 additional hours of labor in the year in which workers take maternity leave when they are exposed to 22 weeks more of potential leave, controlling for the amount of labor in the firm in 2000. Firms do not shed the extra labor even five years after the reform, though I note again that the results are only suggestive since the effects are not precisely measured. In the next section I consider the robustness of these results to the exclusion of women getting elective c-sections in the sample window, since these are not randomly distributed around the policy cutoff. In addition, I present IV versions of the main effects.

5.3 Robustness

The results above identify the causal effect of parental leave extensions on firms under the assumption that whether a woman gives birth in late 2001 or early 2002 is as good as random. The regression discontinuity specifications add a control for date of birth identifying off the break between late December, 2001 and early January, 2002. One problem with this assumption is that a small fraction of women receive elective c-sections which are scheduled in the window of a few weeks. In Figure 4, there is a jump in the number of births between December and January, which is surprising since these months have the same length. Most of this is explained by the fact that 350 elective c-sections occur per month in Denmark in this period, and these are not scheduled in the last weeks of December due to holidays. If possible, they are postponed to early January[12].

Since women who have elective c-sections may be a selected group, and so their coworkers and employers may be selected, I drop these women from the sample in the results reported in Appendix Tables 10 and 11. This has no effect on the baseline coworker earnings regressions (column 1 of Table 10), or on the heterogeneity results in the remaining columns. Omitting all women who had elective c-sections in 2001 and 2002 reduces the sample by enough to make the earnings results for coworkers in the same occupation insignificant, though the magnitude of the estimates is unchanged. The effects on firms are also unchanged. Table 11 gives the analogue of Table 5 but eliminates c-sections. The point estimates still suggest a little more than 2 percentage point higher probability of firm shutdown for firms exposed to an additional 22 weeks of potential leave. However, the regression discontinuity estimate falls by about one percentage point (and remains insignificant).

[12] I don't know the date of the procedure, only the year, so I cannot exactly count the number of cases which are postponements.

Next, I turn to the robustness of the effects to an IV specification. The data on leave-taking is specifically state-paid leave. It is possible that women take more leave by using sick-days, vacation days, and get more leave through their firm. For this reason, the effect of "additional leave" should be interpreted with caution—it is not necessarily true that a worker who took 30 weeks of state-paid leave was absent from work for 30 weeks. The number could be substantially higher. The interpretation of reform-eligibility used in this paper so far is less controversial and while it includes the effects for women who did not change their actual leave-taking in response to the reform, it is a cleanly measured policy response. Nonetheless, it is possible use data on state-paid leave to get a sense of the effects of leave-taking on coworkers and firms. Note that The first stage is in Table 12 and is a regression of total weeks of parental leave at the firm taken up to 1 year after childbirth by female employees in 2000 who gave birth between October 1st, 2001 and March 31st, 2002 on the fraction of births at the firm which took place after January 1st, 2002 and so were eligible for 52 weeks of leave. There is, on average, 5.6 weeks more parental leave taken at firms in which births took place after January 1st, 2002 compared to firms in which births took place before January 1st, 2002. Note that this should not be interpreted as "firms expected women to take 28 weeks, but instead they took 34 weeks." Turning back to the histogram in Figure 1, it is clear that conditional on taking a few months of leave, most women shifted to taking the full 52 weeks, or a few weeks less. The average increase is 5.6 weeks because many women took very few weeks of leave, and their behavior did not change when a non-binding constraint was loosened. While some firms in the sample were not affected by the reform and by parental leave taking in general, those firms who were exposed to leave were also exposed to a substantial increase in leave-taking.

Table 13 gives the effect of an additional week of actual leave taking induced by the reform on coworker wages and promotion to management. Neither of these is significantly different from zero and the magnitudes are very small. Multiplying the point estimates by the average increase in leave, the effects are larger than the non-IV effects, as expected since not all women take advantage of the reform. The firm effects in Table 14 are similarly stronger than the OLS-based results, and are significantly different from zero.

Finally, I consider placebo regressions in which rather than study workplaces and coworkers of women who gave birth right at the time of a change in parental leave policy, I study the workplaces and coworkers of women who gave birth one year later and who had no differential exposure to parental leave. In particular, I identify the year 2001 workplace of women who gave birth between October 1st, 2002 and March 31st, 2003 and run the regression specified in (1) where D now indicates whether the birth took place after January 1st, 2003. Since there was no difference in parental leave exposure between the two groups in this regression, there should be no significant post-reform effect. This is indeed the case. Appendix Table 15 displays the results of regressions mimicking Tables 2, columns 2, 5, and 6 and3, column 2 but moving the time-frame forward one year. There is no difference in earnings, even for workers in the same occupation as women giving birth in these regressions. Appendix Table 16 similarly finds that there is no difference in the probability of shutdown 1, 3, or 5 years after the baseline year among firms employing women who gave birth after January 1st, 2003 relative to those who gave birth between October and December of 2002.

6 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.

Comparing coworkers of women who gave birth between October 1st, 2001 and December 31st, 2001 to those of women who gave birth in the subsequent three months (and were eligible for 22 weeks more of leave), I find that the overall effect of parental leave extensions on coworkers wages are quite precisely zero. There is some evidence of stress on workers in the same occupation as women eligible for extra leave. Immediately following the leave, those workers who make job-to-job transitions have lower wages than those exposed to a shorter leave-spell. In addition, workers in the same occupation exposed to more coworker leave have yearly earnings about \$375 lower as a result of the reform in 2007. In other groups, there are no effects of the reform. I also precisely estimate no effects on promotion rates.

Turning to firm outcomes, I do find that firms are overall two percentage points, or about seven percent, more likely to be shut down five years after a Danish parental leave extension from

28 to 52 weeks if they employed a women who was eligible for the leave relative to those employing women who were not eligible. This effect is concentrated in firms with 15-30 employees—the employee-weighted effect on shutdown is negligible. Conditional on survival, I don't find any evidence that firm value added per worker is lower as a result of the increased parental leave. Point estimates suggest that firms do permanently grow by about half of a full time worker when they are exposed to 52 weeks of leave, but I'm underpowered to detect an effect different from zero for firm size.

The economic impacts of the Danish parental leave reform on the workplace overall is negligible. There are on average no earnings effects on coworkers and firm effects are concentrated in relatively small firms. The results do suggest some stress (both in terms of earnings and wage changes associated with job transitions) on coworkers in the same occupation as a woman taking leave, but these effects are not dramatic. Given the magnitude of the Danish reform studied here, the small effects on coworkers and firms are evidence that firms and coworkers in general are able to adjust to long absences induced by job-protected parental leave.

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Figures

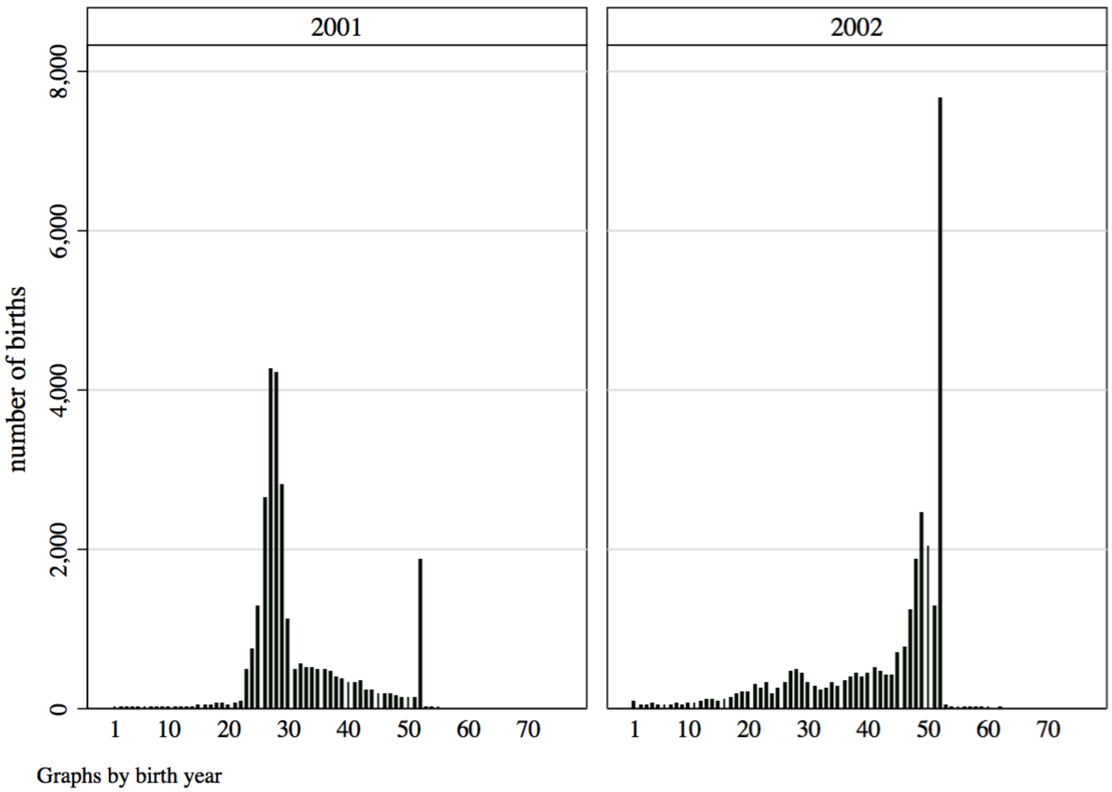

Figure 1: This figure displays the number of maternity leave spells by the length of the spell. In 2001, spells are concentrated around 28 weeks, though some mothers do take longer maternity leave at a lower compensation rate. In 2002, the modal length of maternity leave is 52 weeks and the number of women taking 52 week leaves nearly quadruples.

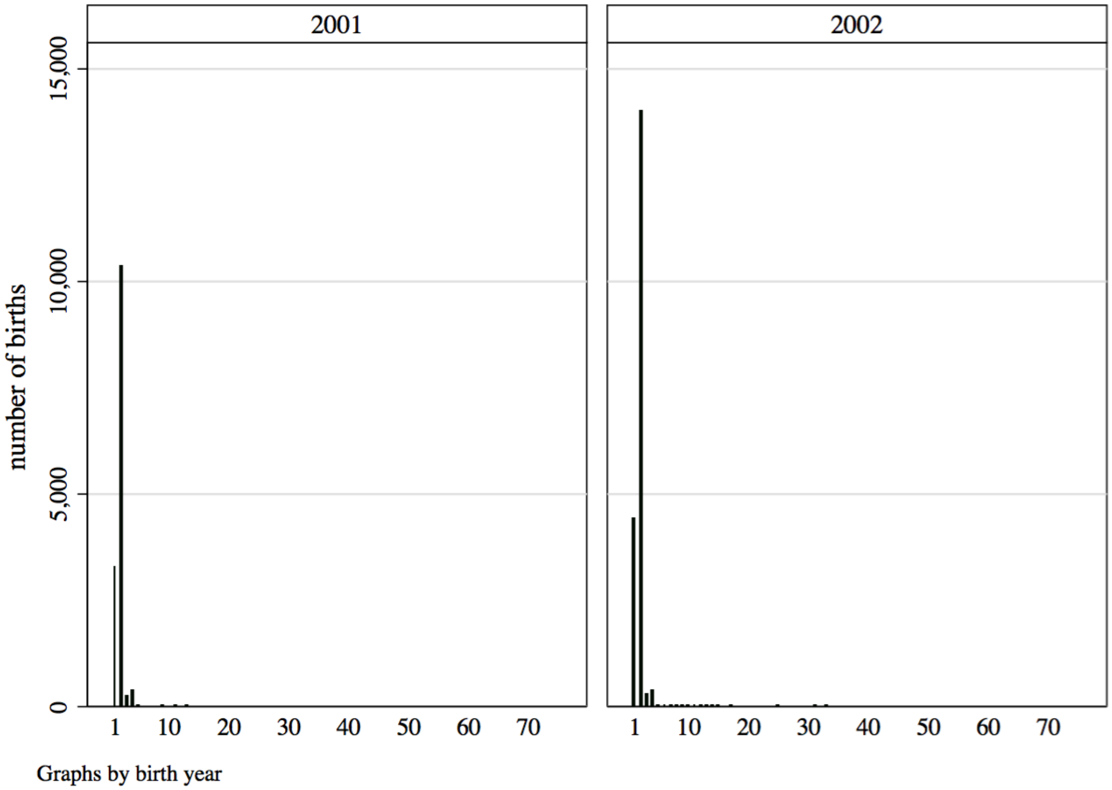

Figure 2: This figure displays the number of paternity leave spells by the length of the spell. Despite the fact that parental leave can be taken by either the mother or the father, almost all men took 0-2 weeks of leave both in 2001 and 2002 (fathers not taking leave are omitted from the graph).

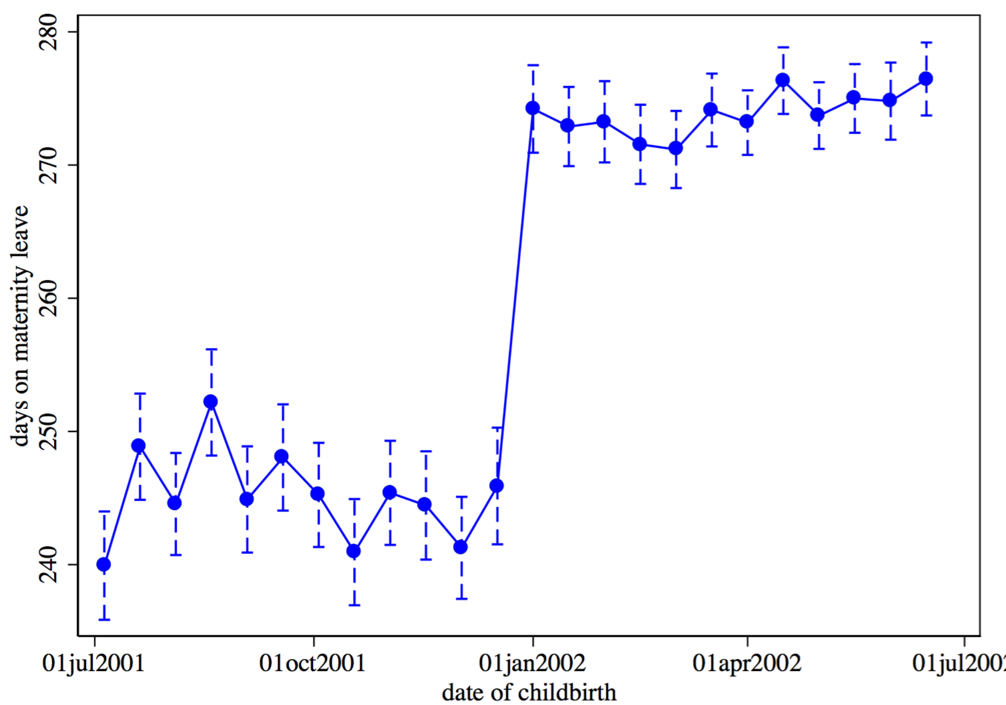

Figure 3: This figure displays the length of maternity leave by child birthdate and is replicated from Beuchert et al. [2016]

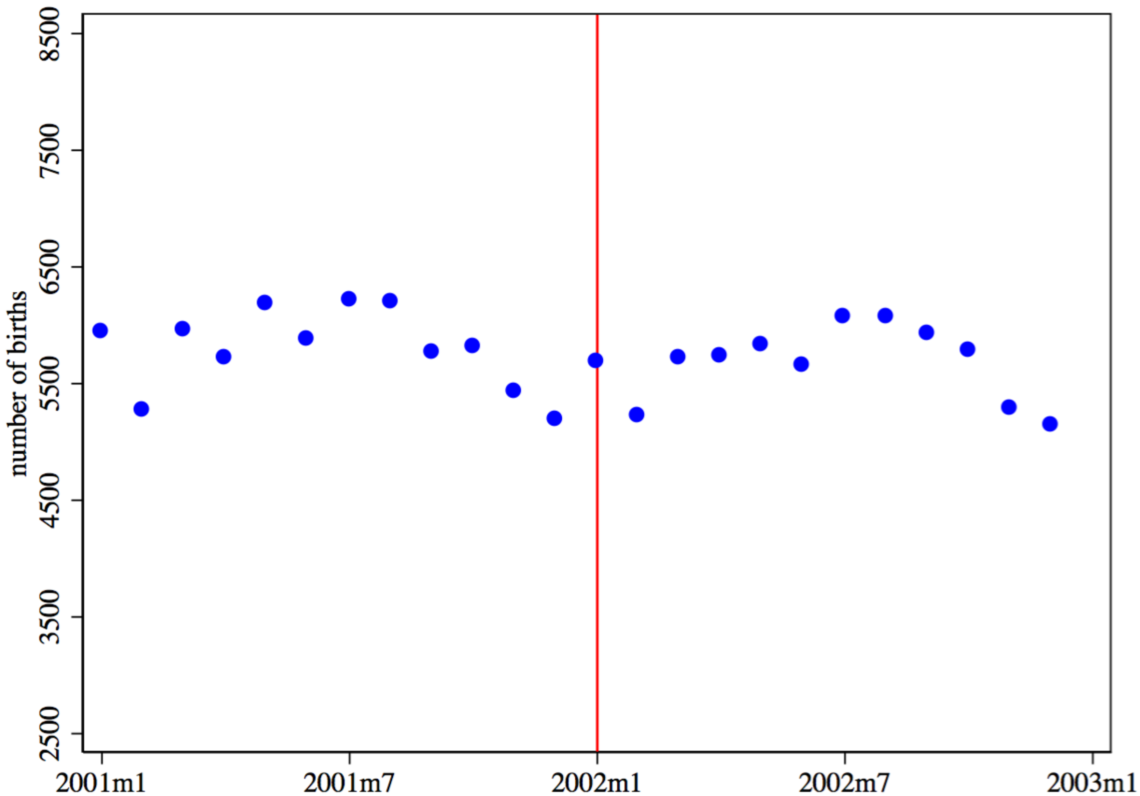

Figure 4: This figure displays the number of births on Denmark, by month, between January 1st, 2001 and December 31st, 2002.

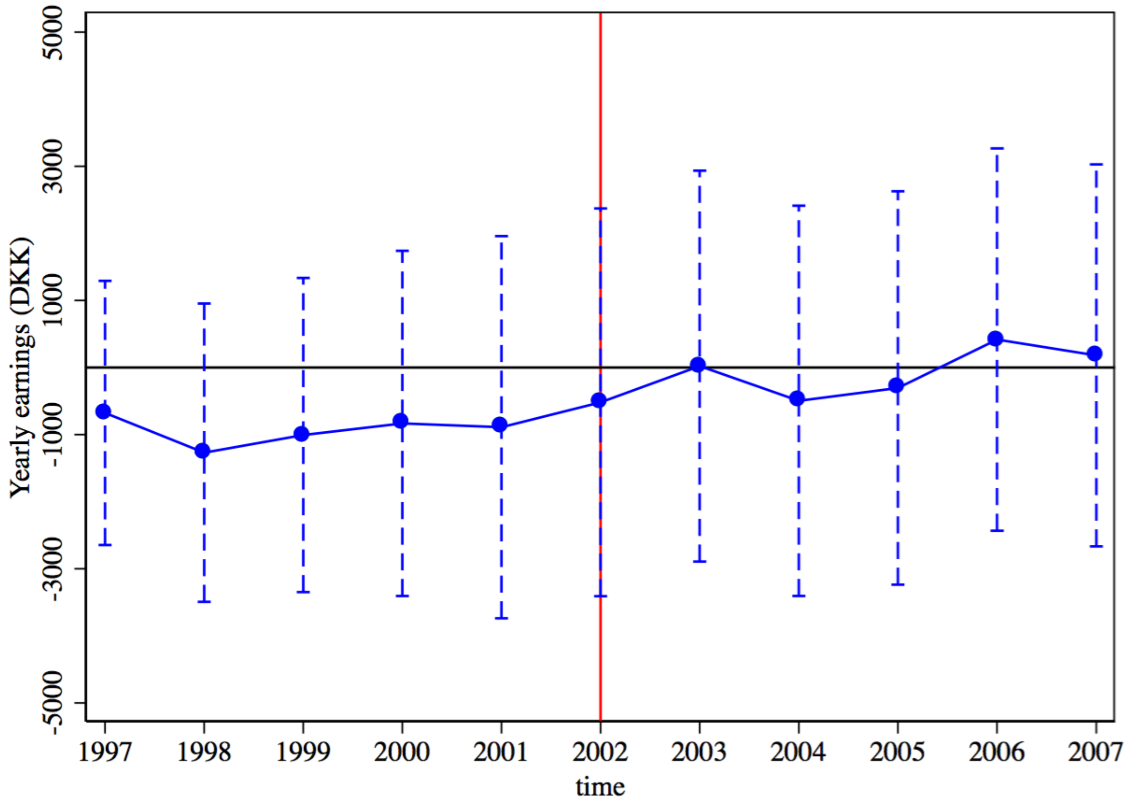

Figure 5: This figure displays coefficients from a regression of coworker yearly earnings (in Danish kroner) on the fraction of births at the firm which took place after the reform and were eligible for more parental leave. The sample consists of coworkers of women who gave birth between October 1st 2001 and March 31st 2002. Coworkers were determined in 2000. Standard errors are clustered at the firm level.

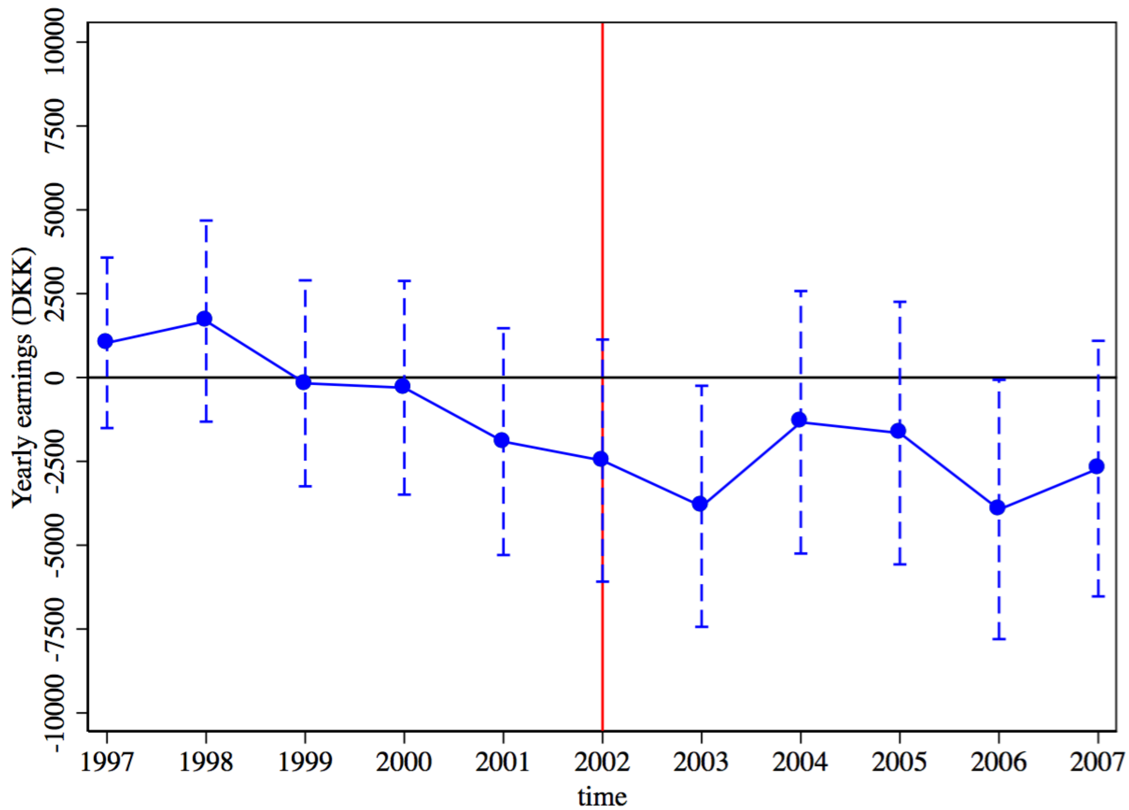

Figure 6: This figure displays coefficients from a regression of coworker yearly earnings (in Danish kroner) on an indicator of whether a coworker was eligible for 52 weeks of parental leave interacted with whether a worker was in the same occupation as a woman giving birth between October 1st 2001 and March 31st 2002 in firms which had only one birth during this 6-month period. Coworkers were determined in 2000. Standard errors are clustered at the firm level.

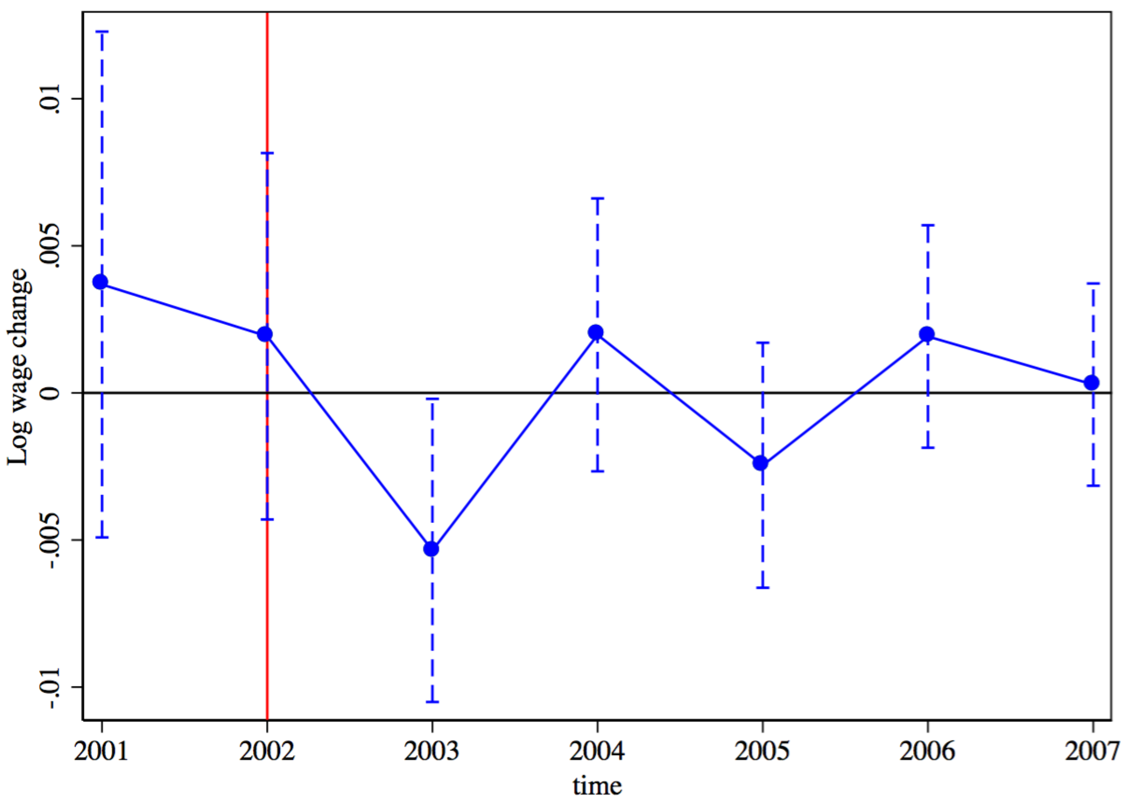

Figure 7: This figure displays coefficients from a regression of coworker log wage change is 2003 relative to 2002 for workers who changed jobs between 2002 and 2003 on an indicator of whether a coworker was eligible for 52 weeks of parental leave. The sample consists of coworkers of women who gave birth between October 1st 2001 and March 31st 2002 in firms which had only one birth during this 6-month period. Coworkers were determined in 2000. Standard errors are clustered at the firm level.

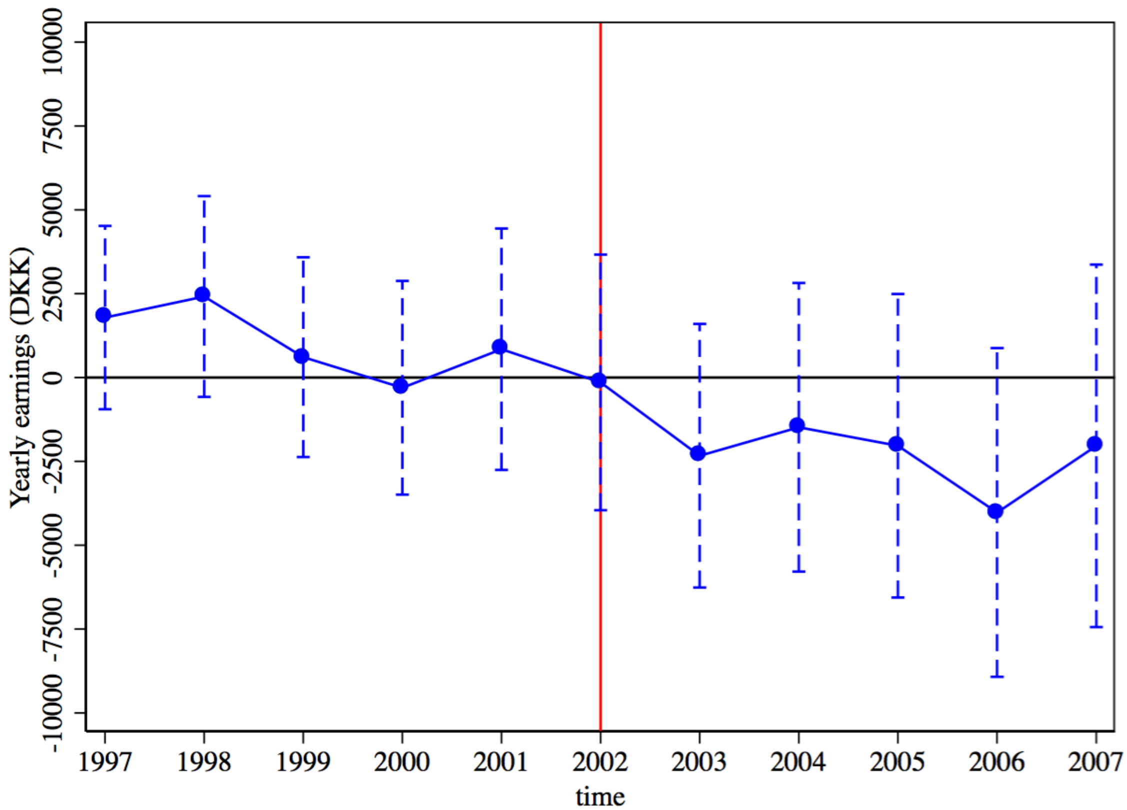

Figure 8: This figure displays coefficients from a regression of coworker yearly earnings (in Danish kroner) on an indicator of whether a coworker was eligible for 52 weeks of parental leave interacted with whether a worker was in the same occupation as a woman giving birth between October 1st 2001 and March 31st 2002 in firms which had only one birth during this 6-month period. Only workers who stay in the same job in a given year are included in the "same occupation" group. Coworkers were determined in 2000. Standard errors are clustered at the firm level.

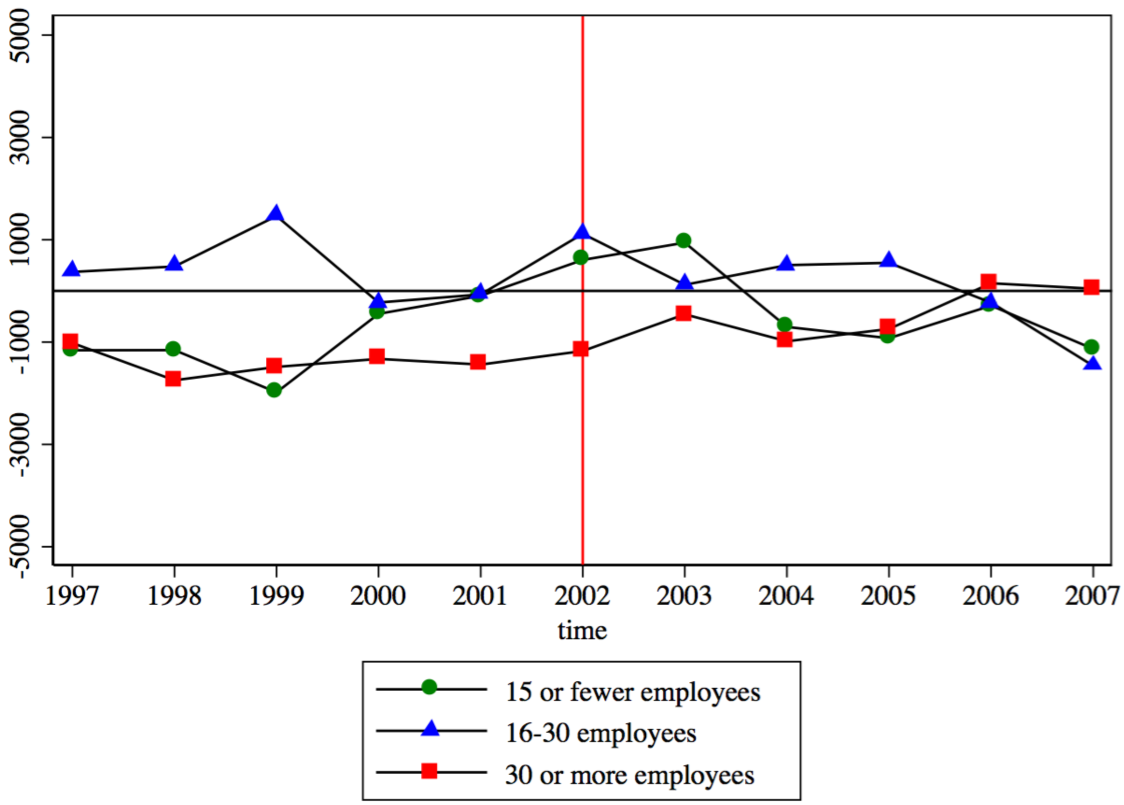

Figure 9: This figure displays coefficients from a regression of coworker yearly earnings (in Danish kroner) on the fraction of births at the firm which took place after the reform and were eligible for more parental leave, by firm size. The sample consists of coworkers of women who gave birth between October 1st 2001 and March 31st 2002. Coworkers were determined in 2000. Standard errors are clustered at the firm level.

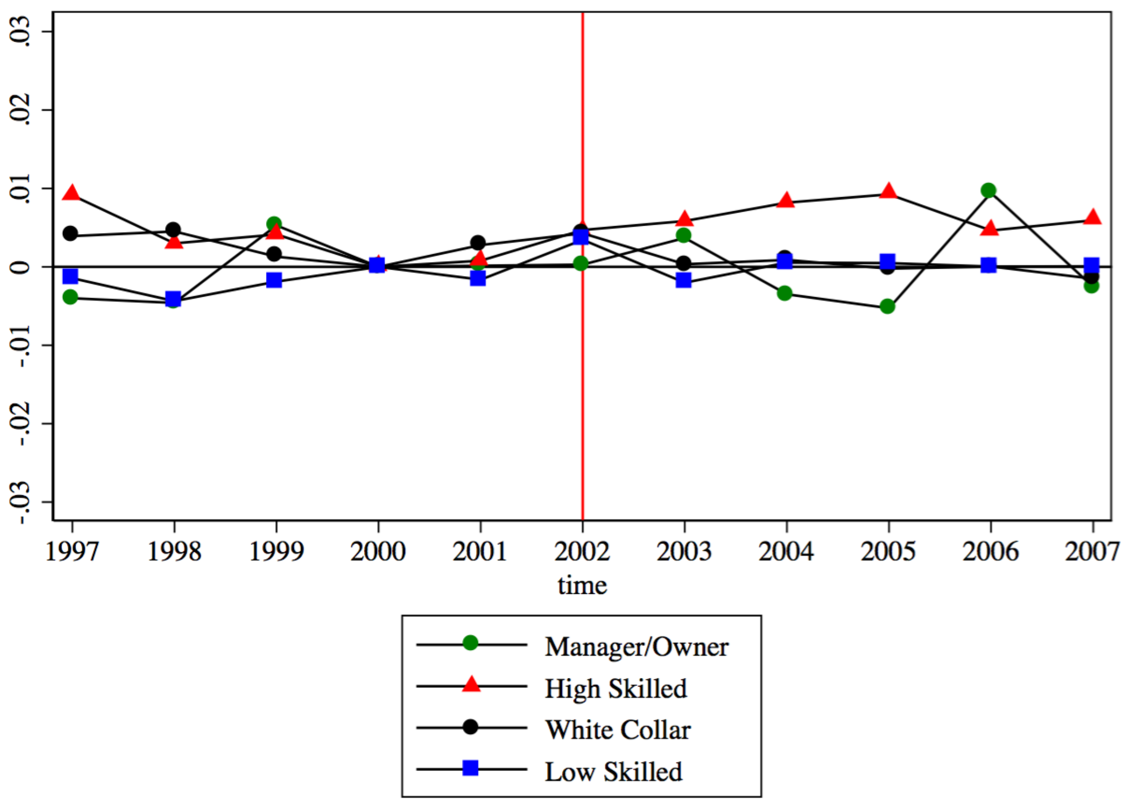

Figure 10: This figure displays coefficients from a regression of coworker log wages (in Danish kroner) on the fraction of births at the firm which took place after the reform and were eligible for more parental leave, by broad occupation groups. The sample consists of coworkers of women who gave birth between October 1st 2001 and March 31st 2002. Coworkers were determined in 2000 and the time-series are normalized to 2000 = 0. Standard errors are clustered at the firm level.

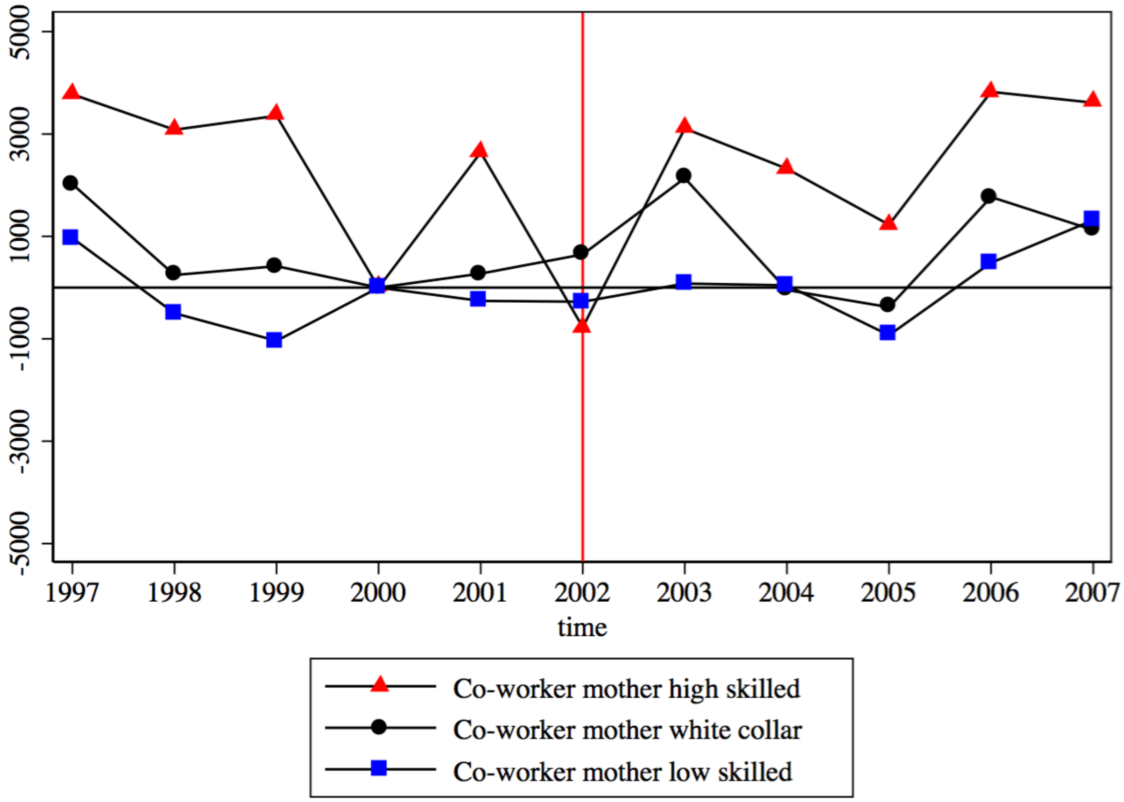

Figure 11: This figure displays coefficients from a regression of coworker yearly earnings (in Danish kroner) on an indicator of whether a coworker was eligible for 52 weeks of parental leave, by the mothers' broad occupation group. The sample consists of coworkers of women who gave birth between October 1st 2001 and March 31st 2002 in firms which had only one birth during this 6-month period. Coworkers were determined in 2000 and the time-series are normalized to 2000 = 0. Standard errors are clustered at the firm level.

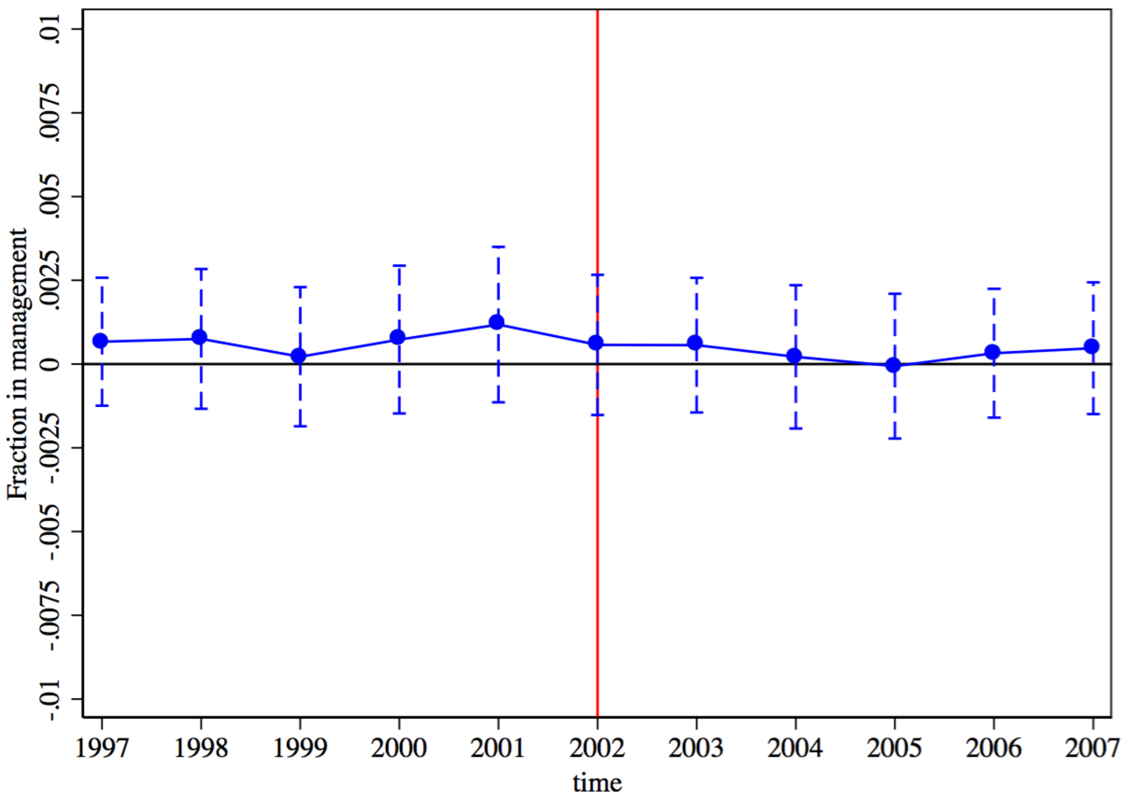

Figure 12: This figure displays coefficients from a regression of whether a worker is in a management position on the fraction of births at the firm which took place after the reform and were eligible for more parental leave. The sample consists of coworkers of women who gave birth between October 1st 2001 and March 31st 2002. Coworkers were determined in 2000.

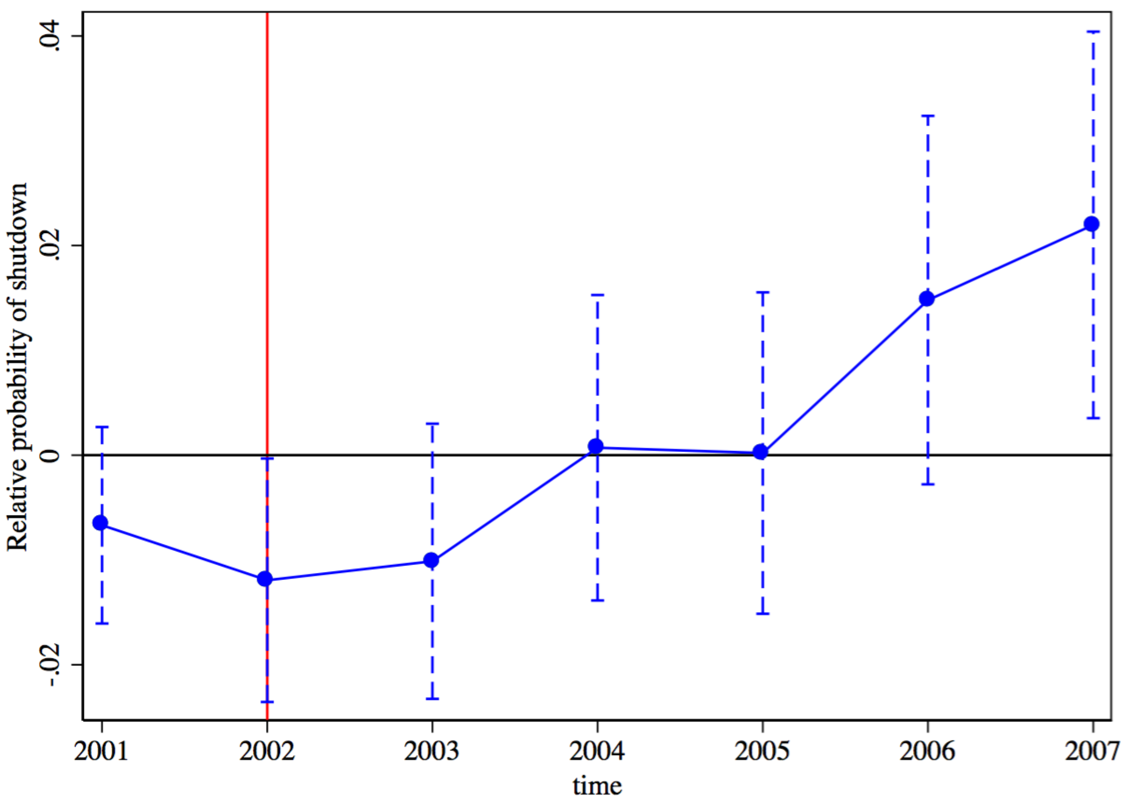

Figure 13: This figure displays coefficients from a regression of whether it is the firm's last year in the dataset on the fraction of births at the firm which took place after the reform and were eligible for more parental leave. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002. The sample is restricted to firms which existed (paid wages) in the years leading up to shutdown and did not re-emerge after shutdown.

Tables

Table 1: Summary statistics

	Birth between Oct. & Dec.	Birth between Jan. & Mar.	p-value
Fraction male	0.3717	0.3721	0.943
	(0.2538)	(0.2575)	
Average age	36.6411	36.6235	0.897
	(6.7061)	(6.7166)	
Average earnings	184423	183039.2	0.436
	(88256)	(86806.69)	
Number employees	36.0996	37.1830	0.363
	(60.5954)	(56.9248)	
N	4789	4945	

Sample is restricted for firms which had 1 birth between October 1st, 2001 and March 31st, 2002.

Table 2: Earnings regressions: coefficient on fraction of births post-reform

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
2002 earnings	-1341.9	66.6	-106.0	248.9	-570.9	1512.8
	(3175.47)	(952.48)	(1532.92)	(669.15)	(895.17)	(1917.26)
2005 earnings	172.0	200.1	81.9	392.5	-904.5	-2103.1
	(3006.5)	(1111.8)	(1788.3)	(807.5)	(1123.8)	(2316.3)
2007 earnings	1652.7	659.0	1011.3	379.7	886.9	-391.8
	(2905.20)	(1108.00)	(1907.72)	(759.17)	(1123.13)	(2584.60)
Sample	Full	Full	Males	Females	1 birth	1 birth
Controls	N	Y	Y	Y	Y	Y
Date of birth	N	N	N	N	N	Y
N	900742	900734	413295	487439	323002	323002

Dependent variable is a worker's earnings in a given year (in DKK). Controls include occupation (3-digit ISCO), industry (2 digit), age, and gender fixed effects, as well as year 2000 earnings. The last column is a regression discontinuity, with date of coworker's birth as the running variable. Other columns identify off average differences between the two groups. Standard errors, clustered at the establishment level, in parentheses. 1000 DKK $\approx$ \$ 150

Table 3: Earnings regressions with heterogeneity

	(1)	(2)	(3)	(4)
		2002 earnings (in DKK)		
Post	534.0637	-3139.471**	-1004.811	1826.344
	(987.5579)	(1594.548)	(1728.892)	(1582.271)
Post × n emp.			2.313004	
			(14.56994)	
n employees			-2.251122	
			(15.24144)	
Post × n in occ.				-177.5228
				(188.215)
n in occupation				533.8951***
				(134.5842)
		2005 earnings (in DKK)		
Post	368.8257	-3686.77**	-431.7932	1729.646
	(1141.52)	(1785.83)	(1984.053)	(1829.033)
Post × n emp.			-4.886115	
			(17.28158)	
n employees			-3.876897	
			(16.40887)	
Post × n in occ.				-188.5555
				(217.5679)
n in occupation				525.2902***
				(155.5732)
		2007 earnings (in DKK)		
Post	-570.9073	-2295.634	-905.1598	1482.247
	(1269.376)	(1726.421)	(1870.027)	(2034.029)
Post × n emp.			14.83248	
			(15.59159)	
n employees			-11.088	
			(14.15825)	
Post × n in occ.				-301.1767
				(241.9528)
n in occupation				411.4506**
				(173.0098)
Sample	1 birth, <= 30 employees	1 birth, Same occupation	1 birth	1 birth, Same occupation, <= 30 employees
N	67876	102666	323006	67876

Dependent variable is a worker's earnings in a given year (in DKK). Controls include occupation (3-digit ISCO), industry (2 digit), age, and gender fixed effects, as well as year 2000 earnings. The last column is a regression discontinuity, with date of coworker's birth as the running variable. Other columns identify off average differences between the two groups. Standard errors, clustered at the establishment level, in parentheses. 1000 DKK ≈ $ 150

Table 4: Probability in management

	(1)	(2)	(3)	(4)	(5)	(6)
2002 Pr(management)	0.000662	0.000566	0.001242	-0.000076	0.000526	0.000779
	(0.000534)	(0.000513)	(0.000854)	(0.000427)	(0.000582)	(0.001084)
2005 Pr(management)	-0.000620	-0.000415	-0.000946	-0.000022	0.000426	0.001451
	(0.000880)	(0.000770)	(0.001249)	(0.000586)	(0.000812)	(0.001495)
2007 Pr(management)	0.000461	0.000256	0.000252	0.000140	0.000133	0.000847
	(0.000778)	(0.000699)	(0.001151)	(0.000581)	(0.000808)	(0.001518)
Sample	Full	Full	Males	Females	1 birth	1 birth
Controls	N	Y	Y	Y	Y	Y
Date of birth	N	N	N	N	N	Y
N	876005	875997	394594	481403	311514	311514

Dependent variable is an indicator of whether a worker is in a management position in the reported year. Controls include occupation (3-digit ISCO), industry (2 digit), age, and gender fixed effects, as well as year 2000 earnings. The last column is a regression discontinuity, with date of coworker's birth as the running variable. Other columns identify off average differences between the two groups. Standard errors, clustered at the establishment level, in parentheses.

Table 5: Firms shutdown regressions: coefficient on fraction of births post-reform

Dependent variable:	(1)	(2)	(3)	(4)
Shutdown in 2007	.0215316**	.0219726**	.0297112***	.0325219
	(.0094423)	(.009408)	(.0101067)	(.0204759)
Sample	Full	Full	1 birth	1 birth
Controls	N	Y	Y	Y
Date of birth	N	N	N	Y
N	11066	11066	8063	8063

Dependent variable is an indicator of whether a firm has no records in the tax register in the reported year. Controls include industry (2 digit level), a quadratic in number of employees in 2000. The regression discontinuity control (1 birth sample) includes a linear term in date of birth which varies in the pre-and-post period. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002.

Table 6: Probability of shutdown by firm size

	≤15 employees	16-30 employees	> 30 employees
2002 Pr(shutdown)	-.0356***	.0141	-.0024
	(.0107)	(.0131)	(.0084)
2007 Pr(shutdown)	.0112	.0646***	.0104
	(.0156)	(.0202)	(.0144)
Controls	Y	Y	Y
Date of birth	N	N	N
N	3633	2107	5326

Dependent variable is an indicator of whether a firm has no records in the tax register in the reported year. Controls include industry (2 digit level), a quadratic in number of employees in 2000. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002.

Table 7: Log value added per worker

log VA per worker in year:	2002	2005	2007
Fraction post	-.0021	-.0133	.0079
	(.0210)	(.0247)	(.0263)
Sample	Full	Full	Full
Controls	Y	Y	Y
Date of birth	N	N	N
R-squared	0.2119	0.1278	0.0814
N	2365	2041	1882

This table displays coefficients from a regression of the log value added (revenue less the cost of material inputs and capital) per full-time equivalent worker in a firm on the fraction of births at the firm which took place after the reform and were eligible for more parental leave, controlling for the size of the firm in 2000, value added per worker in 2000 (both as quadratics) and industry in 2000. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002 who had non-imputed accounting data in 2000.

Table 8: Full-time equivalent workers

FTE in year:	2002	2005	2007
Fraction post	0.4590	0.4701	0.4396
	(0.4329)	(0.8103)	(1.0127)
Sample	Full	Full	Full
Controls	Y	Y	Y
Date of birth	N	N	N
R-squared	0.5039	0.2661	0.2083
N	2516	2318	2226

This table displays coefficients from a regression of the number of full-time equivalent workers in a firm on the fraction of births at the firm which took place after the reform and were eligible for more parental leave, controlling for the size of the firm in 2000, value added per worker in 2000 (both as quadratics) and industry in 2000. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002 who had non-imputed accounting data in 2000.

Appendix Figures

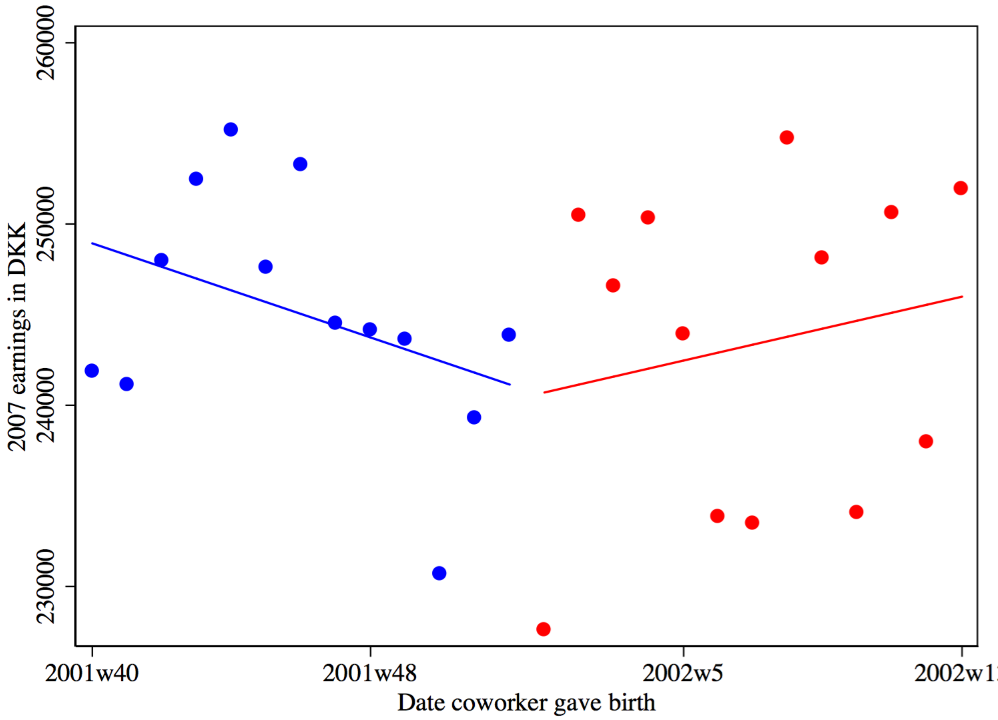

Figure 14: This figure displays average 2007 earnings of male workers by the day that their female coworkers gave birth. The sample is restricted to 1-birth firms. The difference between post-January 1st, 2002 and pre is -1772.034 DKK which is not significantly different from 0 (p-value is .544). The data reveal no evidence of trends of any kind. Because of this, I turn away from regression discontinuity towards analysis of differences in means between the two groups, which have more power, in the main text.

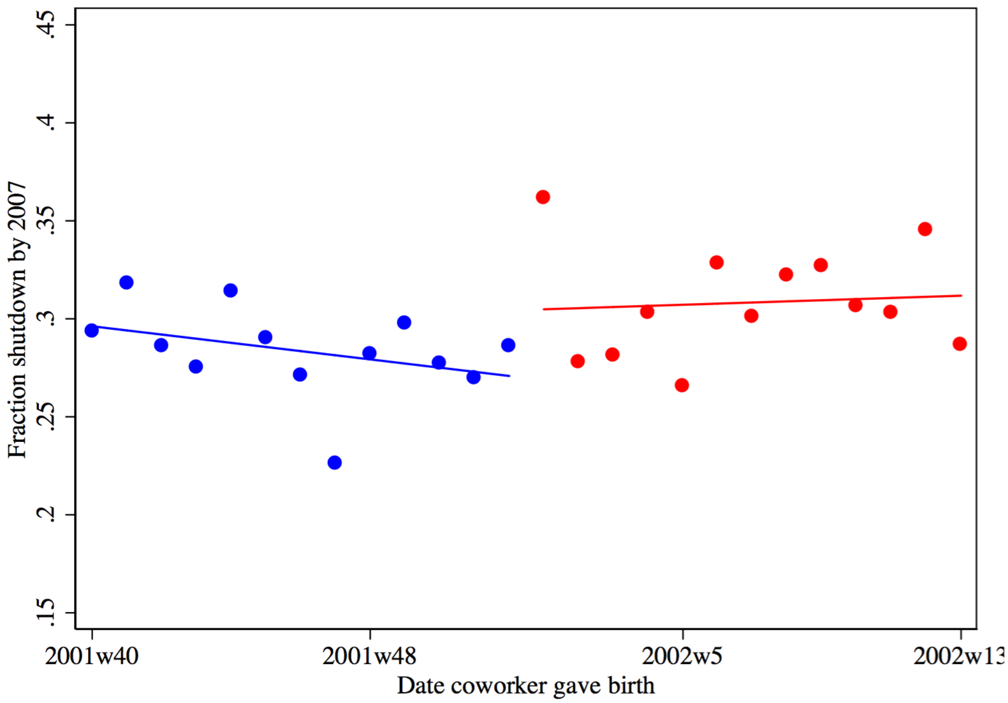

Figure 15: This figure displays average 2007 fraction of firms which have shutdown by the day that their female coworkers gave birth. The sample is restricted to 1-birth firms. The difference between post-January 1st, 2002 and pre is 0.025, different from 0 with a p-value of 0.033. The regressions reported in the main text, of course, effectively weight these observations by the number of women in each cell.

Appendix Tables

Table 9: Mother's labor market regressions: coefficient on whether birth was post-reform

Dependent variable:	(1)	(2)	(3)
2002 earnings	2305.4	2861.1**	2413.5
	(1591.0)	(1267.0)	(2571.8)
2005 earnings	548.5	1276.3	-2417.2
	(1889.8)	(1566.6)	(3187.9)
2007 earnings	-295.6	-477.2	-2443.5
	(2125.1)	(1837.8)	(3739.9)
2002 job change	-0.0200***	-0.0231***	0.0034
	(0.0057)	(0.0056)	(0.0115)
2005 job change	-0.0023	-0.0056	-0.0041
	(0.0069)	(0.0069)	(0.0140)
2007 job change	0.0076	0.0027	0.0068
	(0.0067)	(0.0067)	(0.0136)
2002 employment	-0.0577***	-0.0587***	-0.1149***
	(0.0063)	(0.0058)	(0.0119)
2005 employment	-0.0021	-0.0042	-0.0048
	(0.0050)	(0.0048)	(0.0098)
2007 employment	-0.0045	-0.0050	-0.0044
	(0.0046)	(0.0045)	(0.0091)
Controls	N	Y	Y
Date of birth	N	N	Y
N	20724	20724	20724

A worker's earnings in a given year are in DKK (1000 DKK $\approx$ \$ 150). Controls include occupation (3-digit ISCO), industry (2 digit), age, and year 2000 earnings. The last column is a regression discontinuity, with date of the mother's birth as the running variable. Other columns identify off average differences between the two groups. Standard errors are in parentheses.

Table 10: Earnings regressions with heterogeneity

	(1)	(2)	(3)	(4)	(5)
	2002 earnings (in DKK)				
Post	-1751.3	33.9	-2503.3	-1895.5	798.1
	(1499.4)	(1495.2)	(1744.9)	(1609.4)	(2535.5)
Post × n emp.				7.04	
				(14.9)	
n employees				-8.94	
				(15.3)	
Post × n in occ.					-99.61
					(288.3)
n in occupation					548.51***
					(202.73)
	2005 earnings (in DKK)				
Post	-2077.6	-19.5	-2660.8	-1727.8	1449.0
	(1584.6)	(1603.8)	(1894.5)	(1903.1)	(2720.7)
Post × n emp.				0.97	
				(18.1)	
n employees				-10.17	
				(17.5)	
Post × n in occ.					-208.91
					(315.1)
n in occupation					577.24**
					(224.2)
	2007 earnings (in DKK)				
Post	-446.7	-1241.3	-2104.3	-1651.5	627.6
	(1510.7)	(1699.2)	(1827.0)	(1775.3)	(2876.8)
Post × n emp.				17.15	
				(15.5)	
n employees				-13.98	
				(15.4)	
Post × n in occ.					-276.87
					(331.3)
n in occupation					434.70*
					(221.8)
Sample	Full	1 birth, <= 30 emp.	1 birth, Same occ.	1 birth	1 birth, Same occ., <= 30 emp.
N	253537	61183	79012	253540	61183

Dependent variable is a worker's earnings in a given year (in DKK). This sample excludes coworkers of women who received elective c-sections. Controls include occupation (3-digit ISCO), industry (2 digit), age, and gender fixed effects, as well as year 2000 earnings. Standard errors, clustered at the establishment level, in parentheses. 1000 DKK ≈ $ 150

Table 11: Firms shutdown regressions: coefficient on fraction of births post-reform

Dependent variable:	(1)	(2)	(3)	(4)
Shutdown in 2007	.0254285**	.0257709**	.0313589***	.0227443
	(.0103591)	(.0103152)	(.0108969)	(.0220931)
Sample	Full	Full	1 birth	1 birth
Controls	N	Y	Y	Y
Date of birth	N	N	N	Y
N	11066	11066	8063	8063

Dependent variable is an indicator of whether a firm has no records in the tax register in the reported year. This sample excludes coworkers of women who received elective c-sections. Controls include industry (2 digit level), a quadratic in number of employees in 2000. The regression discontinuity control (1 birth sample) includes a linear term in date of birth which varies in the pre-and-post period. The sample consists of firms employing women in 2000 who gave birth between October 1st 2001 and March 31st 2002.

Table 12: First stage regression results

Fraction post-reform	5.622769***
	(1.0554458)
Constant	22.84535***
	(0.7224492)
F-stat	28.38
N	12803

Outcome variable is total weeks parental leave at the establishment in 2000 from women giving birth between October, 2001 and March, 2002.

Table 13: IV regression results: coefficient on total parental leave

Dependent variable:	2002	2005	2007
Yearly earnings of year 2000 coworkers (in DKK):	79.6	183	275
	(313)	(305)	(314)
Fraction promoted to management:	-0.00009	0.00002	0.00007
	(0.0001)	(0.0002)	(0.0002)
N	12803	12803	12803

Coworker outcome IV regressions. Dependent variable is listed in the rows, independent variable is the total weeks parental leave at the firm in 2000 using the fraction of workers giving birth between October, 2001 and March, 2002 who were eligible for the reform as an instrument.

Table 14: IV regression results—firms

Year of Shutdown	2002	2005	2007
Total parental leave	-0.00178	0.00002	0.00215*
	(0.0012)	(0.0014)	(0.0016)
Constant	0.1441***	0.2181***	0.2943***
	(0.0139)	(0.0173)	(0.0204)

Firm outcome IV regressions. Outcome variable is probability that an establishment is gone in the indicated year, independent variable is the total weeks parental leave at that establishment in 2000 using the fraction of workers giving birth between October, 2001 and March, 2002 who were eligible for the reform as an instrument.

Table 15: Placebo co-worker regressions: coefficient on fraction of births post-2003

Dependent variable:	(1)	(2)	(3)	(4)
Earnings in 2003	279.1	-230.5	-615.2	-1548.5
	(1066.0)	(1056.3)	(1903.1)	(2181.6)
Earnings in 2006	-663.4	-704.3	-421.6	746.4
	(1090.8)	(1103.4)	(2105.0)	(2400.2)
Earnings in 2008	1502.5	1085.0	235.5	1999.9
	(1188.0)	(1237.6)	(2331.6)	(2326.4)
Sample	Full	1 birth	1 birth	1 birth
Controls	Y	Y	Y	Y
Date of birth	N	N	Y	Y
Same occupation	N	N	N	Y
N	941137	351810	351810	89831

These regressions mimic those in Tables 2, columns 2, 5, and 6 and3, column 2 but are run using data one year forward, looking at coworkers of women working at firms in 2001 and giving birth between between October 1st 2002 and March 31st 2003. In this time period, women were *not* exposed to differences in maternity leave options, and we would not expect any difference between coworkers in this regression. Controls include industry (2 digit level), a quadratic in number of employees in 2001. The regression discontinuity control (1 birth sample) includes a linear term in date of birth which varies in the pre-and-post period.

Table 16: Placebo firm shutdown regressions: coefficient on fraction of births post-2003

Dependent variable:	(1)	(2)	(3)	(4)
Shutdown in 2008	0.00001	-0.00085	-0.00509	-0.00250
	(0.00944)	(0.00934)	(0.01002)	(.02042)
Sample	Full	Full	1 birth	1 birth
Controls	N	Y	Y	Y
Date of birth	N	N	N	Y
N	11274	11274	8382	8382

These regressions mimic those in Table 5 but are run using data one year forward, looking at women working at firms in 2001 and giving birth between between October 1st 2002 and March 31st 2003. These firms were *not* exposed to differences in maternity leave options, and we would not expect any difference between firms in this regression. Dependent variable is an indicator of whether a firm has no records in the tax register in the reported year. Controls include industry (2 digit level), a quadratic in number of employees in 2001. The regression discontinuity control (1 birth sample) includes a linear term in date of birth which varies in the pre-and-post period.