Gender Norms and Specialization in Household Production: Evidence from a Danish Parental Leave Reform*

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Abstract

Parental leave is viewed as crucial to alleviating the gender inequalities that arise upon parenthood, but policies are often ineffective. This paper examines the impact of expanding parental leave in Denmark. The results show that mothers increase their leave by 5 weeks, while the average leave duration of fathers remains unchanged. In turn, the earnings gap within couples increases. Leave duration is unaffected by relative earnings, and is instead highly consistent with the role of gender norms. I document both inter-generational spillovers from maternal labor supply and peer effects among sisters who take a longer leave if exposed to the reform-induced change in leave duration.

JEL classification: D13, J13, J16, J18, J22

Keywords: Intra-household specialization, gender norms, parental leave, peer effects

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1 Introduction

Parenthood and persistent gender inequalities are intricately linked. After their first child, women reduce their labor supply and instead spend more time on child care and household responsibilities. Meanwhile, men's labor market trajectories are largely unaffected. This pattern is the main explanation for the remaining gender wage gap (e.g. Kleven, Landais, & Søgaard (2019); Lundborg et al. (2017); Ejrnaes & Kunze (2013)). To alleviate the cost associated with motherhood, most high- and middle-income countries have implemented some paid maternity leave. While policy design varies across counties, their effectiveness in closing gender gaps in the labor market has been put into question (e.g. Ginja et al. (2020); Dahl et al. (2016); Canaan (2022)). An expansion of parental leave in Denmark allows me to investigate the drivers of gender inequality around parenthood.

To understand the role of generous parental leave policies in reducing or reinforcing gender inequality, I exploit an unexpected and rapid institutional change in Denmark. In 2002, a parental leave reform improved new parents' economic compensation while on leave, effectively increasing the leave duration. At the same time, policymakers removed two weeks of earmarked leave specifically allocated to fathers. With the reform, the household could decide how to distribute the extended parental leave within the couple. I ask which factors influence take-up. My analysis is two-fold. First, I evaluate the reform effect on leave duration for the universe of eligible parents and investigate the role of relative earnings within couples in the 18-month window surrounding the implementation date. I follow these parents for 5 years and show the dynamic effects on their earnings. Second, I evaluate how leave behavior is affected by family behavior. I investigate inter-generational spillover in time allocation. I do this by linking historical information on maternal labor supply for women in the reform window. Further, I identify sisters who have a child after the reform and investigate peer effects on leave duration.

I investigate which factors influence households' behavior when they are given the opportunity to take an extended leave, and the long-term impact on the earnings of mothers and fathers. I find that mothers increase their leave by 5 weeks while the average leave duration of fathers is unchanged. This increases gender inequality in earnings. The effect is largest in the initial year but persists. Surprisingly, leave duration is largely unaffected by relative earnings in the household. Instead, the households behave in a way that is consistent with gender norms. I show that women who had a mother with a high labor supply take a shorter leave compared to those who had a stay-at-home mother. Moreover, I document peer effects among sisters. Women with a sister in the reform treatment group take on average take 1.1 weeks longer leave compared to those with a sister in the reform control group. This corresponds to peer effects of 17 %. The reform induces a change in behavior among mothers, and I argue this translates into new prescriptions regarding mothers' time allocation. This is transmitted via sisters and shows up here as peer effects.

While the reform effects, heterogeneous results, and peer effects are highly consistent with the notion of gender norms, it is possible to imagine other factors that could drive parts of these results. Such factors include comparative advantages, information transmission, and consumption externalities and I discuss each of these factors in detail below. Comparative advantages - including biological factors, such as breastfeeding and unobservable differences across men and women - are a key potential explanation for different time allocations across men and women, and could explain the overall reform effect. I infer the comparative advantages that would be implied from the observed time allocation to correspond. Moreover, it is difficult to see how this explains the heterogeneity in maternal labor supply and peer effects. Information transmission and consumption externalities can explain the peer effects but fail to provide a useful explanation for the reform effects. Importantly, gender norms is the only explanation useful for understanding both the reform and peer effects.

Children contribute to the majority of the gender gaps in labor market outcomes in high- and middle-income countries (Harkness & Waldfogel (2003); Daniel et al. (2013); Ejrnaes & Kunze (2013); Angelov et al. (2016); Lundborg et al. (2017); Kleven et al. (2019); Berniell et al. (2021)). While parental leave in principle is available for mothers and fathers, mothers are the primary users of these policies.[1] The literature investigating these policies sometimes refers to an 'inverse U-shape' where the introduction of maternity leave is improving women's outcomes with decreasing and even negative returns to extending the leave duration (see Olivetti & Petrongolo (2017) and Rossin-Slater (2018) for reviews). This paper links the literature evaluating leave reforms to the literature showing the importance of gender identity for female labor force participation (Fernandez et al. (2004); Fernandez & Fogli (2009); Farré & Vella (2013); Finseraas & Kotsadam (2017)). Empirical evidence on the relationship between gender norms and parents' time allocation is still scarce. The primary contribution of this paper is to show that extended parental leave increases inequality within the household and shed light on the mechanism. A key insight is that take-up is not driven by financial incentives. Instead, the patterns are highly consistent with gender identity and related norms. This insight can inform the design of parental leave policies. If the aim is to increase fathers' share of leave and reduce gender inequality, the findings here show that general extensions will not achieve this. To achieve greater gender equality, an explicit targeting of fathers is likely needed.

This paper also contributes to the literature on social networks and individuals' interactions with public programs. Evaluation of parental leave policy is one example in a larger literature also studying e.g. pension reforms (Brown & Laschever, 2012), educational admission (Angrist & Lang (2004); Altmejd et al. (2021); Dahl et al. (2021)), eligibility to tax

[1] Some countries have implemented policies targeting fathers. While the use of these policies has been gradual (Dahl et al. (2014); Ma, Andersson, Duvander, & Evertsson (2019); Ekberg et al. (2013)), studies report positive effects on women's wages (Druedahl, Ejrnæs, & Jørgensen (2019); Farré & González (2019)). In some contexts, eligible fathers spend more time on housework (Patnaik (2019); Kotsadam & Finseraas (2011)) and childcare (Kluve & Tamm, 2013). Studies also find effects on marital stability (Olafsson & Steingrimsdottir (2020); Margolis, Hou, Haan, & Holm (2019); Avdic & Karimi (2018)).

credit and avoidance (Alstadsæter et al. (2019); Wilson (2022)). A key insight from this literature is that the full effect of a policy is not fully captured by focusing on the individuals who just became eligible compared to those just rendered ineligible. The paper most similar to mine is Dahl, Løken, & Mogstad (2014). They use the implementation of the earmarked paternity leave in Norway, and document peer effects on both the extensive and intensive margin among brothers and male co-workers. Similarly, Welteke & Wrohlich (2019) document spillovers between female co-workers in Germany after a reform extended maternity leave in Germany. I document spill-overs in sister-pairs following an extension of parental leave, rather than paternity or maternity leave. I show that a general extension of parental leave reinforces an existing gender gap in time allocation as a result of both the reform effect and subsequent peer effects. My results on spill-overs in families support the interpretation that this is due to the interaction between the existing norms and the policy. This is important when considering the design and implementation of various programs.

My results provide insights into the mechanisms behind the persistent gender gaps around parenthood. I show that households behave in a way highly consistent with gender norms, largely disregarding financial incentives. Parental leave is often proposed as a crucial tool to alleviate the child penalty, but my results show that such a policy reinforced gender gaps. The remainder of the paper proceeds as follows. Section 2 connects the paper to the related literature. Section 3 describes the setting, the data, and the empirical strategy. Graphical and regression-based results are reported in Section 4 together with a deeper examination of other potential explanations. Section 5 concludes.

2 Household Behavior Following Parenthood

Gender is a social category with great importance for individual choices. Calling this to the attention of economists, Bertrand (2020) described an insight from social psychology; gender stereotypes are not merely descriptive but serve a prescriptive role. They motivate men and women to adjust their self-view and choices to what is deemed appropriate for their gender and this results in gender identity. Gender differences are persistent and reinforced through everyday interactions where individuals adapt their behavior to align with what is expected of them based on their gender to avoid sanctions.[2] Supporting this notion, a large literature documents penalties for both men and women when behaving in counter-stereotypical fashion (e.g. Rudman & Phelan (2008); Moss-Racusin et al. (2010); Bertrand et al. (2015); Kuwabara & Thébaud (2017); Folke & Rickne (2020); Exley et al. (2020); Rickne & Folke (2022)).

[2] To understand how gender identity is constructed and enforced, the work by sociologists West & Zimmerman (1987) is useful. They view gender as "an emergent feature of social situations: both as an outcome and as a rationale for various social arrangements and as a means of legitimating one of the most fundamental divisions of society" (Ibid., p. 126).

By tradition, women have been given the vast responsibility for child-rearing and home production, and thus men and women likely face very different norms around parenthood. Mothers are expected to engage in care work and unpaid labor, while fathers are met with other expectations. To comply, women allocate extensive time to home production and notions of the male breadwinner induce men to allocate less time to home production. Akerlof & Kranton (2000) formalized this by extending utility functions with negative pay-offs from transgressing norms. Cortés & Pan (2020) models the argument presented by Bertrand (2020) and propose a parameter capturing women's higher disutility from working in the market, or alternatively, by women valuing household public goods more than men. Such work mirrors Folbre (1994) description of norms as "structures of collective constraint". However, such approaches tend to overlook the societal diversity in gender norms.

Gender norms are not monolithic nor are they static: they may change with exposure to individuals deviating from the norm (Boelmann et al. (2020); Olivetti et al. (2020); Dahl et al. (2014)). Careful studies of peer effects show that changes in behavior in one individual causality change the behavior of relevant peers such as family members, friends, and co-workers (Brown & Laschever (2012); Fadlon & Nielsen (2019); Altmejd et al. (2021); Alstadsæter et al. (2019); Wilson (2022)). While much attention has been paid to identification (Manski (1993); Angrist (2014)), less attention has been paid to the mechanisms. However, this literature does provide suggestive evidence of the role of gender norms on women's labor market outcomes. For example, Neumark & Postlewaite (1998) show that labor market choices of women spur similar choices by close peers regardless of income effects. Nicoletti et al. (2018) also document peer effects on female labor force participation.

The constraints gender norms impose on women's choices arise from many sources. As the family is a particularly important site for the formation of gender identity and related values a rich empirical literature focuses on the family environment (see review by Bau & Fernández (2021)). Maternal labor supply is a well-established proxy for having been exposed to more progressive gender norms (e.g. Fernandez et al. (2004); Morrill & Morrill (2013); Finseraas & Kotsadam (2017); Olivetti et al. (2020). Fernandez & Fogli (2009) pioneered the literature showing that women with parents originating from countries with higher female labor force participation are more likely to work, also when controlling for individual characteristics. Another approach focuses on the role of societal gender norms and uses shocks to gender norms such as the HIV/AIDS epidemic (Fortin, 2015), WWII (Fernandez et al. (2004); Goldin & Olivetti (2013)), settlers in Australian (Grosjean & Khattar, 2019), and the German reunification (Lippmann, Georgieff, & Senik (2020); Boelmann, Raute, & Schönberg (2020); Beblo & Görges (2018)) to understand the role of societal wide norms. Finally, few papers use changes in economic incentives to estimate the effects of gender norms on female labor supply (Ichino, Olsson, Petrongolo, & Thoursie (2019); Rubolino (2022)). This literature has largely focused on labor market participation and labor supply, while less attention has been paid to time allocation around parenthood.

As the majority of the gender wage gap can be attributed to the arrival of children, the scarcity of empirical research on gender norms and parenthood is somewhat surprising. Empirical evidence so far shows that educational level and relative earnings have very little predictive power over the size of the child penalty (Kleven et al., 2019) and time allocation to child-rearing (Daly & Groes, 2017). This provides suggestive evidence of families complying with gender norms rather than standard economic incentives.

Meanwhile, gender essentialism - that men and women are fundamentally different - characterizes much of the discussion around gender inequality (Lundberg, 2023). Observing large gender differences in time allocation following parenthood, explanations inspired by Becker (1981)'s influential model offers two potential reasons. If women are facing a lower wage than their partner and households are optimizing their production as long as women are at least as productive as their partner in the home. An alternative explanation for the observed time allocation is that women are more productive than their partners in both market and home production, a comparative advantage that may arise from biological differences.[3] Siminski & Yetsenga (2022) tests these predictions by using time-use data in combination with relative wages, and inferring the absolute advantages of women required for the households to be maximizing production. They find that predicted parity in household work occurs when a woman's wage is 109 times that of her male partners. This exercise is motivated by decades of decreasing female comparative advantages in the home driven by closed gaps in educational attainment, and large structural changes. While these development has led to dramatic increases in women's labor force participation, men's take-up of domestic work has not materialized.

Testing if absolute advantage in the market is driving the division of labor upon parenthood is straightforward. In households where the mother out-earns the father, he should be taking a long leave than in households with a male breadwinner. The reform - outlined below - provides couples with the opportunity of taking a longer leave, but at a replacement well below their market wage. Thus, primary earners should respond less to the reform. On the other hand, the reform also provides the opportunity to understand if gender norms are driving the time allocation. In line with the literature on inter-generational transmission of gender identity, women who had a mother with a high labor supply should respond less to the reform. If the reform induced new norms of extended maternity leave, those with a sister in the reform treatment group are exposed to this norm via their sister, while those in the control group do not. This should show up as peer effects in the empirical investigation.

[3] While bargaining models - which do not assume that families are efficient - have gained prominence in the family economics literature (see e.g. Lundberg & Pollak (1996)), they do not offer an alternative explanation to the sex specialization around parenthood.

3 Identification and Empirical Strategy

This section provides an overview of the institutional setting before and after the reform, the data used, and the empirical strategy with an emphasis on the assumptions required for the identification of reform and peer effects.

3.1 Institutional Context

Denmark has, like the other Nordic countries, a long tradition of family-friendly policies enabling the vast majority of mothers to participate in the labor market (Datta Gupta, Smith, & Verner, 2008). These policies include heavily subsidized day care for children, paid parental leave, and job protection while on leave. Moreover, couples in Denmark face individual taxation - rather than joint taxation as in Germany or the US - which creates a strong incentive for women, who often are secondary earners, to participate in the labor market (Selin, 2014). In the 1990s, 84 % of Danish mothers with children below the age of 10 worked outside the home and 2/3 worked full time (Leira, 2002). Since the '80s, the duration of parental leave with economic compensation has been gradually expanded (see Andersen (2018) for an overview) and childcare reached almost universal coverage in 2000 (Leira, 2002). While paternal leave formally is available to both parents, it is viewed as something relevant for mothers (Datta Gupta et al., 2008) who by tradition have been given the responsibility of childcare.

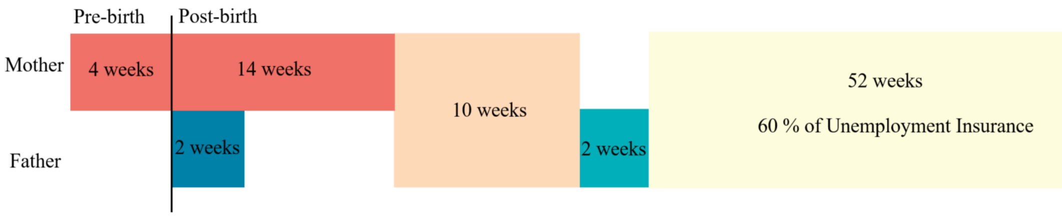
Figure 1 provides an overview of the 2002-reform which reorganized the parental leave system. In short, the duration of well-paid leave was extended by 22 weeks. Childcare leave, which was poorly compensated, was abandoned and shared parental leave was increased by 22 weeks. At the same time, the reform reduced the number of weeks allocated to the father by two weeks, walking back 2-week 'daddy quota' that was introduced in 1998. Policymakers argued that parents – not the government – should decide the distribution of leave (Deding & Holt, 2012).

This change implied better economic compensation from weeks 26 to 46 where new parents would receive compensation corresponding to full unemployment insurance. Parents could receive compensation of 90 % of former earnings up to a flat rate with an average compensation rate of 66 % (Datta Gupta et al., 2008). In addition to the public transfer, some employers pay additional compensation which is often determined through collective bargaining. While there are large sectorial differences in both level and duration, the vast majority of new parents would face a period with compensation substantially lower than their labor market earnings. For example, women working in the public sector, which has a long history of generous leave schemes than the private sector, received a full salary for 14 weeks after giving birth and then up to 10 weeks which could also be transferred to the father if he also worked in the public sector. This provides publicly employed women with up to 24 weeks of fully paid leave. Men who worked in the public sector were offered up to 10 weeks of leave with full wages. The financial sector has also historically offered more generous compensation with women likely receiving their full wage during pregnancy leave and for the 14 weeks of earmarked maternity leave (marked in red in Figure 1) and any parent would receive a full salary for 3 months of the shared leave.

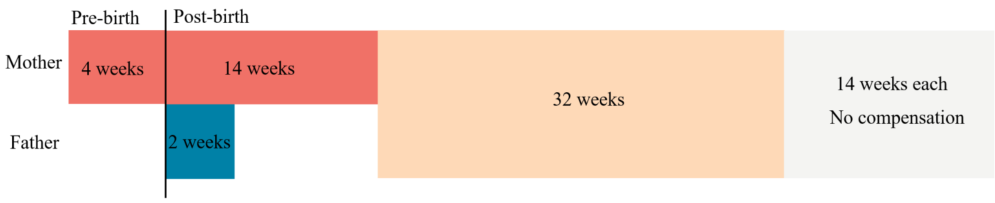
Substantially flexibility was provided in terms of how and when to use the shared leave, incl. simultaneous leave of both parents, part-time leave, and postponement of leave until the child turned 8. Ensuring leave beyond 48 weeks, each parent can extend their leave for up to 14 additional weeks with employment protection but without benefits. Prior to the reform, parents were entitled to a total period of 28 weeks with compensation after childbirth of which 4 were allocated to the father, 14 to the mother, and 10 could be shared. This period was followed by a period of 52 weeks at a reduced rate corresponding to 60 % of the previous benefit. With the reduction in leave specifically allocated to fathers, I argue that the policy was conceived as primarily relevant for mothers. The empirical investigation supports this.

Figure 1: Institutional Setting

(a) Duration of economic compensation (UI) before the reform of January 2002



(b) Duration of economic compensation (UI) after the reform of January 2002



The reform was presented in Parliament on January 7 2002 and adopted on March 27 2002. For all parents of children born on or after this date, the new rules apply. Parents with a child born between the 1st of January and the 27th of March were given the option to choose between the two schemes. Results show a jump in the average leave duration of mothers on January 1 2002 and no change in the average leave duration of fathers. On March 27, the change in average leave is barely visible, implying that the vast majority of couples preferred the new scheme. With similar results, Beuchert, Humlum, & Vejlin (2016) argue that almost all parents choose the post-reform rules if given the option.[4]

[4]Other studies have used this reform for identification. They have been concerned with maternal and child

In further support of the unexpectedness of the reform, a parliament election took place in November 2001 leading to a change in government. The incumbent government campaigned on earmarked paternity leave, while the opposition's promises were less precise. There was no reason to suspect such a major change immediately after the new government took office. The rapid implementation of the reform implies that no self-selection can occur. The discontinuity that arises from the reform provides a close-to-ideal set-up for evaluating both the reform effect and peer effects.

3.2 Data

To evaluate the effects of the reform and subsequent peer effects on leave duration, I combine information from several administrative registers obtained via Statistics Denmark. This data contains individual records and covers the full Danish population with a high degree of precision and allows for the identification of all children and their parents. I use the information on all parents who had a child between March 2001 and December 2005.[5] Family identifiers further allow for identification of mothers and sisters of the women in the reform window. The final data set includes rich covariates incl. education, labor market information, and historical labor supply of the maternal grandmother of the child. Details are reported in Appendix A. Labor market information for the parents is from the year prior to childbirth. This avoids any confounders due to job changes or any mechanical effects from income reduction while on leave (Nielsen, Simonsen, & Verner, 2004). For maternal labor supply of the previous generation (i.e. the maternal grandmother of the child), I follow Kleven et al. (2019) and obtain a historical measure of cumulated maternal labor supply that the new mother was exposed to as a child.

To measure the length of leave, I use information on weekly benefits from the DREAM register. I construct a variable containing a count of weeks during which a parent receives compensation due to parental leave a year following the birth of their child. This measure includes the full compensation corresponding to unemployment insurance and the reduced rate that was in place before the reform. The measure does not include leave taken prior to childbirth (pregnancy leave). Potential top-ups from employers are not observed. Restrictions on the sample exclude twin births, same-sex parents, and households where at least one parent does not live with their child. To ensure that both parents are entitled to full compensation during leave, households where either parent is enrolled in education, self-employed, or loosely affiliated to the labor market are also excluded. Similar to Beuchert

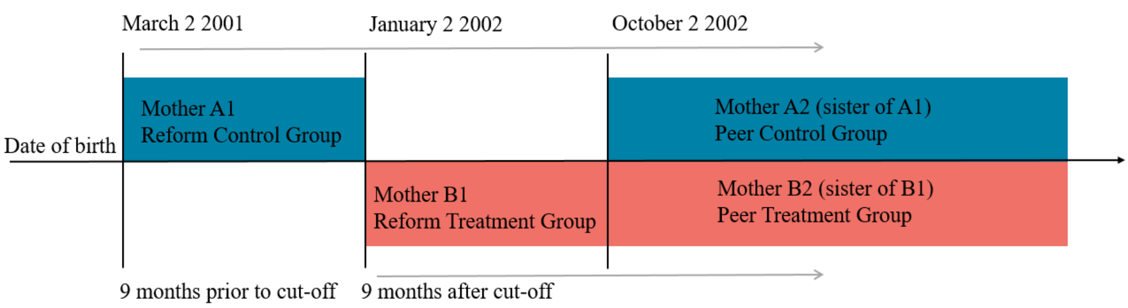
health (Beuchert et al., 2016), firm performance (Gallen, 2019), and women's income (Andersen (2018); Tô (2018)), and disregarded fathers' leave behavior. All studies document substantial change in leave behavior among mothers January 1, 2002.

[5] In December 2005, a new law that required all private sector employers to pay contributions to a Parental Leave Fund was announced. In turn, employers would be reimbursed for salaries paid during parental leave. As this law changed the economic incentives for leave-taking for parts of the population, 2005 will be the end year of this analysis.

et al. (2016), I impose a restriction so only mothers with at least 2 weeks of paid leave are included. Mothers are required to take two weeks of leave after childbirth, so mothers without any leave registered are likely not entitled to paid leave (i.e. they are not participating in the labor market). It is not possible to impose the same restriction for fathers, as they are not required to take any leave. The consequences of the restrictions for the sample size are reported in Appendix B. I only include the first child of a parent who had multiple children between 2001 and 2005.

I divide the population of parents into four groups: reform control, reform treatment, peer effect control, and peer effect treatment. The reform control group consists of parents who had a child prior to the reform, the reform treatment group consists of parents who had a child after the reform and could not know about the reform at the time of conception. These groups are used to evaluate the reform effects on both mothers and fathers. Both the peer effect control group and peer effect treatment groups contain mothers who had a child after the reform was implemented and knew about the new rules at the time of conception. The difference between these two groups is the date when their sister had a child. The four groups are depicted in Figure 2. Mother A1 refers to a mother who had a child nine months prior to the reform, and Mother B1 refers to a mother who had a child in the nine months following the reform implementation. Both Mother A2 and Mother B2 had a child after October 1 2002. Mother A1 and Mother A2 are sisters and Mother A1 was in the reform control group. Mother B2's sister is Mother B1, who was in the reform treatment group.

Figure 2: Reform group, peer group and peer effects



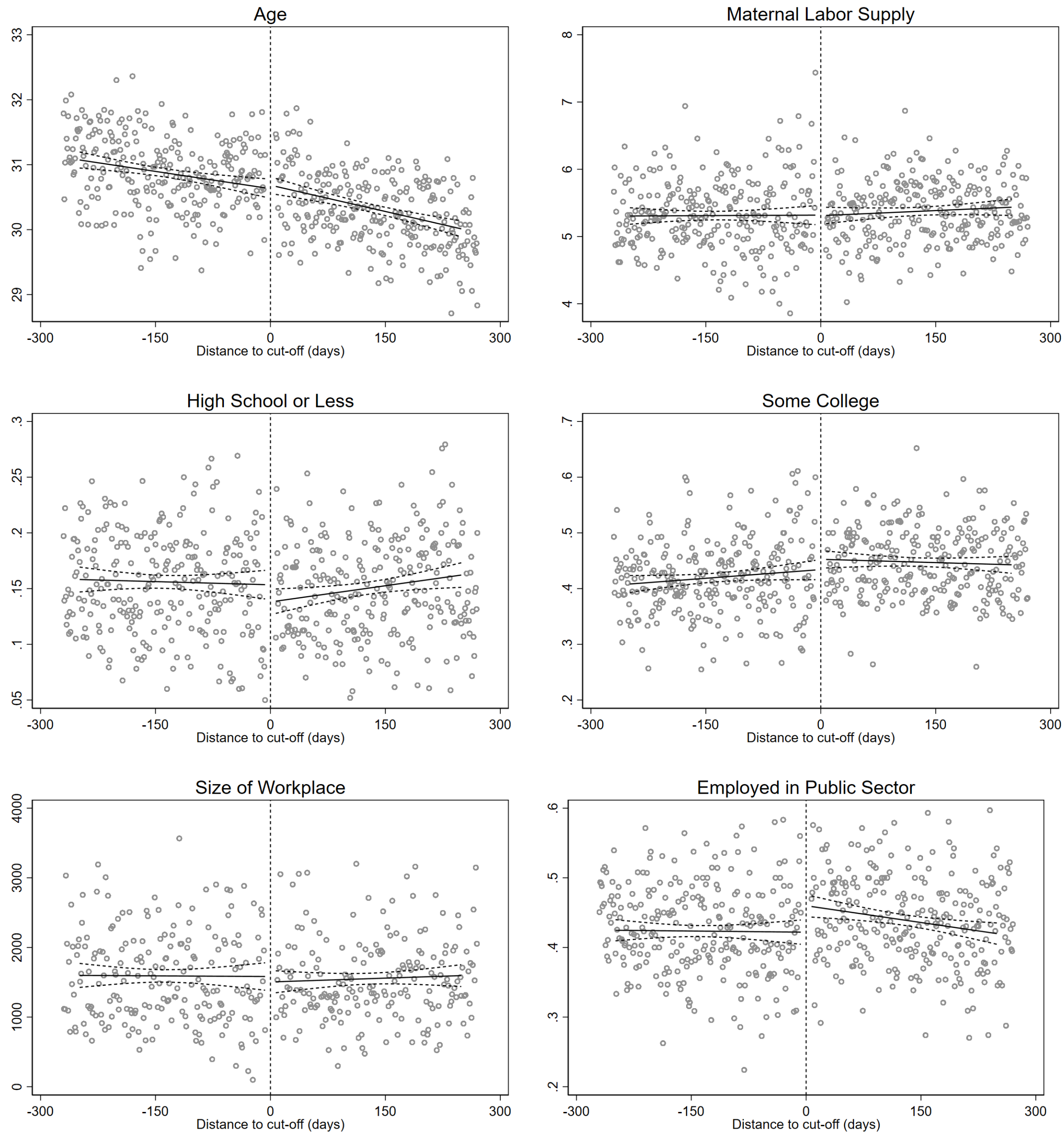
When the aim is to identify peer effects in naturally occurring peer groups, it raises a 'many-to-one' issue as multiple peers can affect the same individual. This problem arises if more than one peer became a parent around the reform date, particularly if there is a peer before the implementation of the reform and another peer after. Dahl et al. (2014) solve this by only including networks where only a single peer has a child in the reform window. Similarly, I drop mothers who have a child after 1st of October 2002 and have two or more sisters who give birth in the reform window. This also addresses the issue of using leave-out-means as measures of peer behavior raised by Angrist (2014) and Sacerdote (2014).

Formal checks show that the number of observations drops before the cut-off. This is normally a sign of manipulation into treatment. However, an inspection of the data shows that this occurs every year. Both formal checks and a graphical inspection of the drop in births around New Year are reported in the Appendix C. Why this happens is not obvious, but it could be due to planned fertility, planned C-sections, and labor induction during the holidays. For this reason, observations 7 days before and after the cut-off are dropped.[6] The final sample used to investigate reform effects contains 21,475 mothers in the control group and 22,481 mothers in the treatment group. The sample for investigating peer effects contains 1,915 mothers in the control group and 1,928 mothers in the treatment group.

To further rule out manipulation around the implementation date, I show the continuity in key covariates of the mothers over the implementation date. If certain groups manipulated the date of child birth, we would see a jump in these covariates at the cut-off date. This is depicted in Figure 3. I show age, maternal labor supply (of the grandmother of the child), a dummy indicating little formal education (high school or less), a dummy indicating some college (2-year degree or more), and characteristics of her workplace. There is a small jump in the likelihood of the mothers working in the public sector, but overall the two groups are very similar. For the remaining five variables there is no difference in the value at the reform implementation date, indicating a very valid research design.

[6] In '4.4 Robustness', I show that whether or not I include observations close to cut-off or I have larger 'donut', point estimates are extremely stable, providing reassurance that manipulation didn't take place.

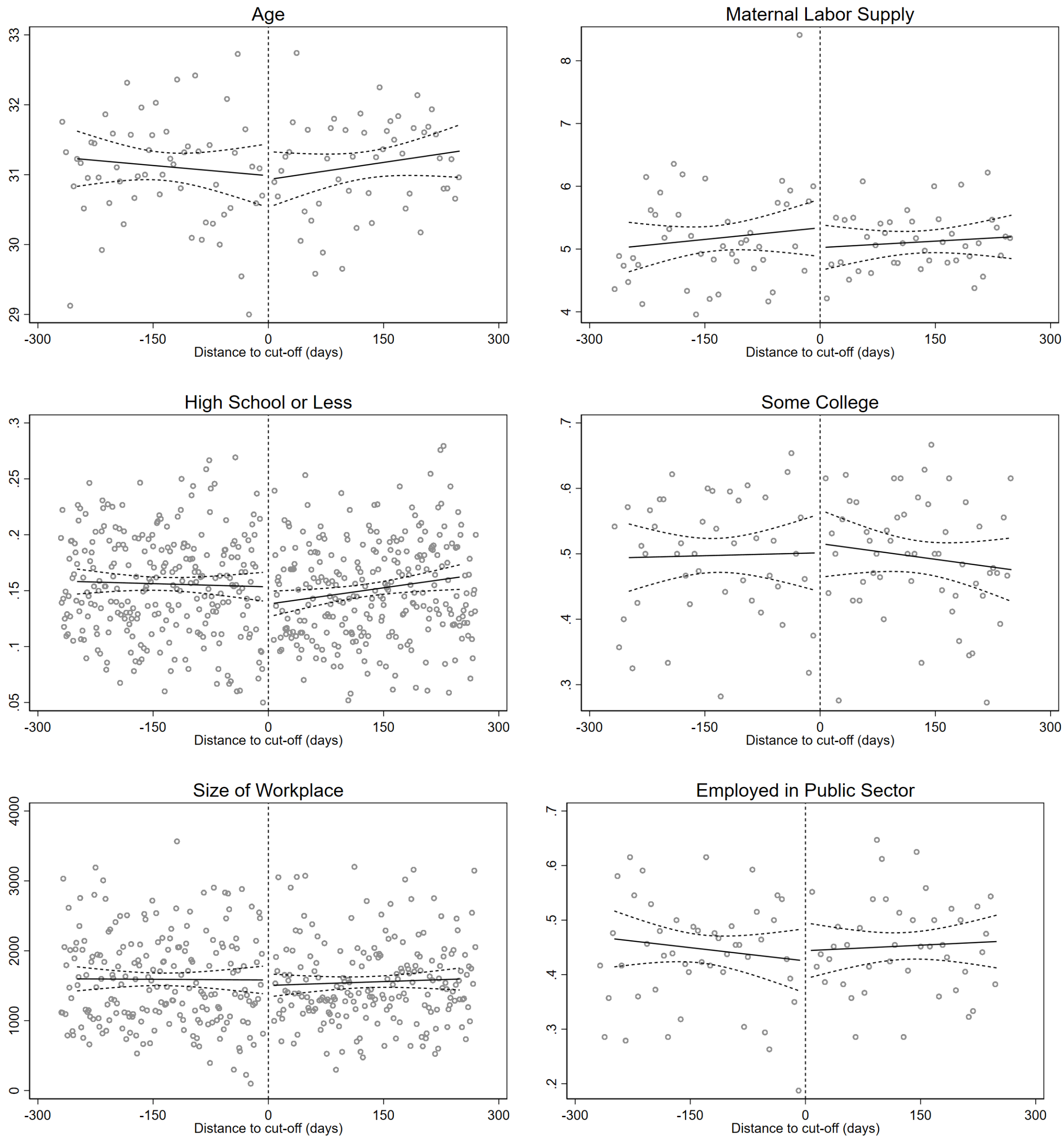
Figure 3: Pre-determined covariates, Mothers in the reform window



Notes: The figures show value of key covariates of mothers' in the reform window: age, her mothers' labor supply, a dummy indicating high school or less education, a dummy indicating some higher education, size of her workplace and a dummy indicating public sector employment. The running variable is date of child-birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples. Each bin includes 50 observations and kernels are uniform.

In addition, I document that there are no signs of manipulation into treatment among the mothers who have a sister in the reform window. Manipulation would require them to control the birthday of their niece or nephew. Figure 4 contains the same variables as Figure 3. Reassuringly, there is no difference across the treatment and control groups.

Figure 4: Pre-determined covariates, Mothers with sisters in the reform window

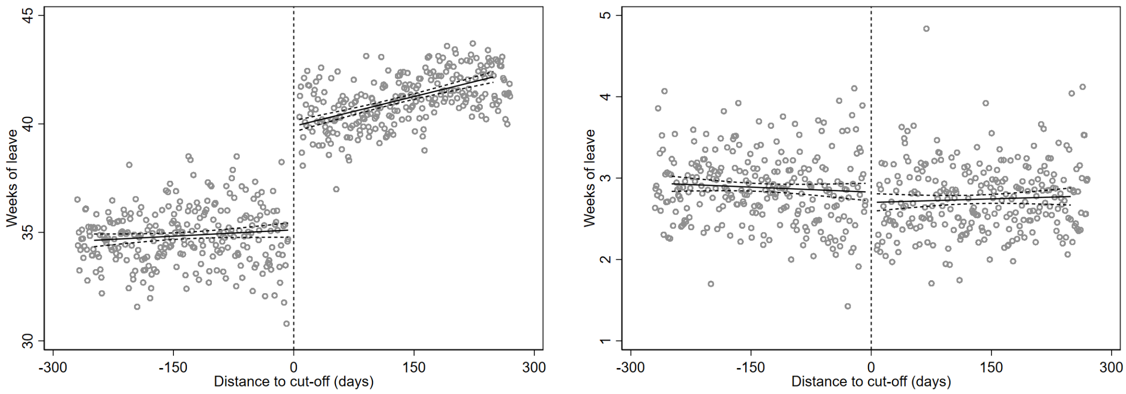


Notes: The figures show value of key covariates of mothers' with a sister in the reform window: age, her mothers' labor supply, a dummy indicating high school or less education, a dummy indicating some higher education, size of her workplace and a dummy indicating public sector employment. The running variable is date of child-birth of the sister's child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 2842 sister-pairs.

Figure 5 show the discontinuity in average leave duration at reform implementation for mothers and fathers, respectively. Figure 5a shows that mothers increase their leave with about 5 weeks at reform implementation, while Figure 5b shows no change in the average leave duration of fathers. The effects reported here are in line with results by other studies using this reform. Beuchert et al. (2016) focus on mothers' leave and report a 32-days increase in the leave duration of mothers. They consider a window of 60 days, where I use 9 months. Nielsen (2009) considers couples where both parties are employed in the public sector, and report larger estimates (approx. 50 days). In general, public sector employment is associated with longer leave of both parents. Gallen (2019) include sickness leave and find that women increase their time away from work by almost 7 weeks.

Figure 5: Leave Duration around the Reform Window

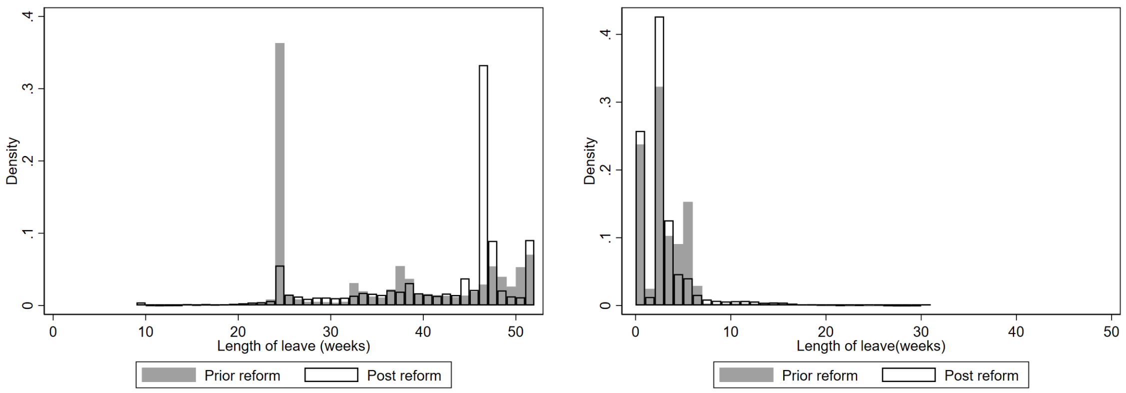
(a) Mothers' Leave (weeks) (b) Fathers' Leave (weeks)



Notes: The figures show average leave duration measured in weeks of mothers and fathers with children born in the reform window. This measure does not include leave taken prior to child-birth. The running variable is date of child-birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples. Each bin includes 50 observations and kernels are uniform. Using a quadratic fit barely makes a difference.

Figure 6: Histogram of Reform Effect

(a) Mothers' Leave (b) Fathers' Leave

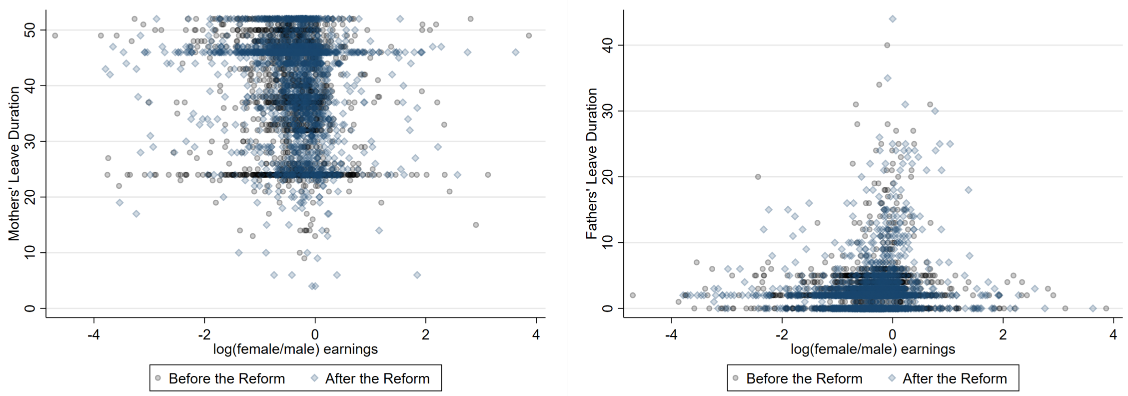


Notes: The figures show the distribution of weeks of parental leave prior to and after the reform for mothers and fathers. This does not include leave taken prior to child-birth. The Post reform group contains parents that have a child up to 9 months after 1st of Jan 2002 and the Prior Reform group contains parents that have a child up to 9 months before 1st of Jan 2002. The sample size is 44,316 couples.

Figure 6 shows histograms of the leave duration for mothers and fathers, respectively. For mothers, there is a substantial shift to a longer leave. Before the reform, the mode leave duration was 24 weeks with 37 % of all mothers ending their leave at this point. At this point, the benefits equivalent to UI are exhausted. After the reform, only 5 % of all mothers end their leave at 24 weeks. The new mode is 46 weeks with 34 % of all mothers, which is the new maximum duration of compensated leave.[7] However, for fathers, the mode leave duration both before and after the reform is 2 weeks, with 33 % of all fathers taking two weeks before the reform. With the reform, this share increases by 10 %-point. At reform implementation, the share of fathers who take 4 weeks of leave is reduced by 12 %-points. Moreover, 25 % of all fathers have no leave registered both before and after the reform. At first sight, this might seem like a registration issue, but upon closer inspection, this is also the case in the public sector where registration issues are believed to be of smaller concern. This is reported in Appendix C. Meanwhile, a longer and more dense tail shows that some but few fathers increase their leave. In other words, the reform implied that most fathers reduced their leave, but a small share substantially increased their leave. The picture in Figure 5b showing no reform effect on fathers' leave duration hides substantial heterogeneity. This will have consequences for the empirical strategy.

Figure 7: Leave Duration and Relative Earnings

(a) Mothers' Leave (b) Fathers' Leave



Notes: The figures show the distribution of weeks of parental leave prior to and after the reform for mothers and fathers. This does not include leave taken prior to child-birth. The Post reform group contains parents that have a child up to 9 months after 1st of Jan 2002 and the Prior Reform group contains parents that have a child up to 9 months before 1st of Jan 2002. The sample size is 44,316 couples.

Figure 7 shows the actual non-parametric relationship between relative earnings and leave duration of mothers (left panel) and fathers (right panel). This is plotted separately for couples with children born before and after the reform. Visually, there is no relationship between neither fathers' nor mothers' leave duration and the relative wage. For both mothers and fathers, two solid lines appear along the income distribution. These lines, at 24 and 46 weeks for mothers, and 0 and 2 weeks for fathers, correspond to the mode leave in Figure 6. On average, across the reform treatment and control group, mothers are taking

[7]Longer leave than 46 weeks is taken at a low rate using left over leave from any child born when the old scheme were in place or without any compensation for up to 14 weeks where employment protection is in place.

38.0 weeks of leave, and fathers are taking 2.9 weeks of leave. At wage parity, the father's duration of the shared leave is 1.5 weeks and women's use of shareable parental leave is 21 weeks. At the 98th percentile of the relative wage distribution, where women's wages are 3.4 times higher than their partners, fathers' average use of parental leave is 0.8 weeks, and mothers' leave is 23 weeks.[8]

3.3 Empirical Strategy I: Regression Discontinuity Design

The reform improved compensation for maternity leave with a duration beyond 24 weeks and paternity leave beyond 4 weeks. This creates a discontinuity in leave duration on the 1st of January 2002. I use this to implement a sharp Regression Discontinuity Design (RD-design) to estimate the reform effect. Following the work by Dahl et al. (2014), I implement a two-stage-least-squared (2SLS) estimator to estimate the peer effects on mothers' leave behavior. As the reform implies that the probability of being exposed to a peer who takes a long leave increases drastically at cut-off, I can implement a fuzzy RD to estimate the peer effects. I also estimate the reduced-form.

The main identifying assumptions are that parents in the reform window are not able to control the day of birth of their own child. The fast announcement and implementation of the reform imply that this is close to impossible. The reform was implemented with retrospective effects: it was announced in the first week of Jan 2002, but policy makers allowed all couples with a child born on Jan 1st or later to use the new scheme. The Parliament Election in November 2001 further supports the unexpectedness of the reform. For sisters exposed to the peer effects of extended leave, they should not be able able to control the day of birth of their peer's child. This seems even more unlikely to occur, especially taking the unexpectedness and rapid reform implementation into consideration.

When estimating peer effects, it is often an issue that peers affect each other and the researchers cannot observe the direction of this. This is what Manski (1993) refers to as 'the reflection problem'. I solve this with a time dimension that only allows the peer effect to operate in one direction. Manski (1993) also highlights the issues of endogenous group membership and correlation of unobservables due to contextual effects. By exploiting the fact that the reform is orthogonal to covariates and by defining group membership prior to treatment, the concerns voiced by Manski (1993) on the identification of peer effects should no longer be a concern. Treatment is then as good as randomly assigned.

The outcome of interest is a discrete variable counting the number of weeks that parents are receiving benefits due to parental leave. The assignment variable is the date of birth of

[8] These numbers are the average of the control and treatment groups for the reform. For those with children born prior to the reform, fathers are taking 3.7 weeks and mothers 35.1, while at the 98th percentile, fathers take 2.7 weeks and mothers take 32.6 weeks. In the reform treatment group, fathers take 3.2 weeks and mothers 39.2 weeks at wage parity, and 3.0 and 39.1 weeks at the 98th percentile.

the child, d_i. T_i is the treatment indicator for whether individual i (parent in the reform window) had a child prior to or after cut-off, d_0, 1st of January 2002:

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

where d_i is the distance (in days) from 1st of January 2002 to the birthday of the child of individual i. If the child is born on or after 1st of January, $T_i = 1$, and if the child is born before, $T_i = 0$. There is no jump of the treatment indicator, so any jump of the outcome at cut-off can be interpreted as the causal average effect of treatment (Imbens & Lemieux, 2008).

The reform effect for the full population with the outcome variable, L_i, indicating the length of leave of individual i is given by:

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + X_i + \varepsilon_i \tag{2}$$

where β_2 can be interpreted as the reform effect. β_1 and β_3 can be interpreted as the slopes on either side of the cut-off. X_i is a vector that contains individual characteristics. Variables that potentially vary over time (e.g. earnings and sectorial occupation) are measured the year prior to childbirth.

To test the predictions regarding the reform effect outlined in Section 2, I interact the treatment indicator T_i with a dummy D_i indicating relative earnings and inter-generational maternal labor supply, respectively.

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + \beta_4 T_i \times D_i + \beta_5 * D_i + X_i + \varepsilon_i \tag{3}$$

First, I interact the treatment indicator with a dummy taking the value 1 when the woman in the couple earns more than the man. I perform this exercise when evaluating the reform effect for both mothers and fathers. Then β_2 can be interpreted as the reform effect on those couples where the man is the primary earner. β_4 captures the additional reform effect on couples where the woman earns more than the man, and β_5 captures the initial difference in level across these two types of couples. This allows me to test the predictions from a Becker (1981)-model. When the outcome of interest is mothers' leave duration, we should expect $\beta_4 < 0$, and when investigating the effect of fathers' leave $\beta_4 > 0$. Second, I interact the treatment indicator with a dummy taking the value 1 in the case of low maternal labor supply. In this case, β_2 can be interpreted as the reform effect on the women that experienced a high maternal labor supply in childhood. Equivalently, β_4 captures the additional reform effect on women who grew up with a mother with a low labor supply, and $\beta_4 > 0$. β_5 captures any initial difference across these types of women. This allows me to test for the role of gender identity for mothers' take-up of leave. To investigate other types of heterogeneity, I implement models where I interact the treatment indicator with public sector employment and child parity.

Turning to my estimation of the peer effects, I adopt a 2SLS-estimator following the work by Dahl et al. (2014). The first-stage is equivalent to Equation 2:

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + X_{ip} + \varepsilon_i \tag{4}$$

X_{ip} is a vector that contains individual and peer characteristics. For both the mother in the reform window and the sister, education is included, the relative education of both households, absolute and relative income in both households, sectorial dummies for occupation and whether or not they are first-time mothers. Again, variables that change over time are measured the year prior to childbirth. The fitted values from the first-stage, $\hat{L}_i$, are used to estimate the peer effects on individual p, δ_2, in the second-stage:

$$L_p = \delta_0 + \delta_1[d_i|d_i < d_0] + \delta_2 \hat{L}_i + \delta_3[d_i|d_i \geq d_0] + X_{ip} + d_p + \varepsilon_p \tag{5}$$

δ_1 and δ_3 are the slopes of either side of the cut-off. A control for date of birth of the mother p's own child is added to capture any general time trend.

An alternative empirical strategy is the reduced form:

$$L_p = \lambda_0 + \lambda_1[d_i|d_i < d_0] + \lambda_2 T_i + \lambda_3[d_i|d_i \geq d_0] + X_{ip} + d_p + \varepsilon_p \tag{6}$$

In this case, the parameter λ_2 can be given an Intension-To-Treat (ITT)-interpretation. This estimate is the difference in leave decision among mothers who had peers with children born prior to and after the cut-off. The advantage of the reduced form is that it requires fewer assumptions to estimate the peer effect. I also extend this model with interactions as in Equation 3 to investigate heterogeneity.

Three assumptions are needed to interpret the obtained estimates obtained as the Local Average Treatment Effects (LATE). These assumptions are the exclusion restriction, the independence assumption, and the monotonicity assumption.

For the reform effects, the exclusion restriction holds if the behavior is only affected through the institutional set-up. This implies that there would have been no change in leave behavior in the absence of the reform. The independence assumption implies that treatment is as good as randomly assigned. As mentioned above, the implementation of the reform was unexpected and rapid, implying no selection into treatment is possible. The graphical inspection reported in Figure 3 supports this. As the reform allowed for a longer leave with a better compensation rate but removed the duration with lower compensation, defiers among mothers could be a concern. However, as argued both here and by Beuchert et al. (2016), data inspection shows that most couples choose the new scheme when given the option. As depicted in Figure 6a, 37 % of mothers previously took leave at the maximum duration with high benefits. After the reform, this share drops to 5 %, and the majority of mothers now take 46 weeks of leave, which is the new maximum. This suggests that the duration of leave with high benefits is an important factor. The monotonicity assumption for mothers in the reform window is then a small concern. However, from Figure 6b monotonicity concerns arise regarding fathers' leave: the reform implied that a large share reduced their leave from 4 to 2 weeks, while a smaller share started to take a long leave. I implement an alternative specification with the outcome variable being a dummy that takes the value 1, when the father takes a long leave (defined as 8 weeks or longer). This allows little room for defiers.[9] For the peer effects, the exclusion restriction implies that the only way that the birthday of the peer's child affects behavior is through the observed behavior of the sister in the reform window. This requires that there is no difference in leave decisions of mothers across the peer effect treatment group and control group in the absence of the reform. All the mothers experience the same institutional set-up and other changes (e.g. business cycles or changes in daycare availability) should on average affect the two groups in the same way. The assumption of independence requires that mothers are as good as randomly assigned to the peer treatment group. Selection into treatment is highly unlikely possible and correlation on unobservables among sisters should be dealt with as a result of the rapid implementation of the reform. The balanced observables across the two groups reported in Figure 4 suggest that this is indeed the case.[10] The monotonicity assumption requires that no mother reduces her leave after being exposed to a peer effect from the reform treatment group. Using the concept of prescriptions, I assume a preference for similar behavior to that of peers. That is, the reform-induced change in behavior implies that the women with a sister in the control group observe different prescriptions than women with a sister in the treatment group. These women are expected to behave accordingly when they have a child later in time. The monotonicity assumption is not possible to test. However, if this assumption is not met, the reduced form stated in Equation 6 will still consistently estimate the effect of having a sister in the new versus the old institutional set-up.

Overall, it seems reasonable that all three required assumptions are met for mothers when evaluating both reform and peer effects. For fathers, I also implement an alternative specification with the outcome being a dummy indicating a long leave (8 weeks or more). Any differences in behavior among parents in the reform window can be attributed solely to the reform. Any differences in behavior among mothers exposed to peers with a child born on either side of the cut-off can be attributed solely to the influence of peer effects.

[9]Changing this to 6 weeks provides virtually unchanged estimates. A lower threshold does not deal with the monotonicity concern.

[10]A related concern is that some sisters coordinate fertility. Women in the reform treatment group and their sisters have children with closer spacing than those in the reform control group. I directly test for this in Section '4.3 Alternative Explanations' and rule out that my estimates of peer effects are driven by sisters with the closest spacing of births.

3.4 Empirical Strategy II: Difference-in-Difference

To understand the long-term effect of this policy, I investigate the dynamic effects on labor market outcomes. While childbirth is associated with a permanent reduction in women's earnings, longer time spent on leave allows for less time spent on market work. Moreover, a longer leave may permanently shift more responsibility of childcare to women. To investigate the effect on labor market income and intra-household income share, I implement a Difference-in-Difference strategy. To this end, I extend the data and create a longitudinal data set that contains the total labor market income for all parents in the sample in the period 3 years prior to birth and 6 years following. Restrictions on student status and labor market participation are only imposed around birth.

The strategy for understanding the labor market effect is a difference-in-difference strategy. I compare the change in the labor market outcomes of couples with a child born in 2001 with couples with a child born in 2002. Under the assumption that women's labor outcomes would have evolved similarly around childbirth in the absence of the reform, I identify the effect of the reform on women's labor market earnings, men's earnings, and women's share of household income. Again, the identifying assumption is that it is random which couples have children just before or after the 2002-reform.

I estimate the following regression

$$y_{i,t} = \alpha_{i,t} + \theta T_i + \sum_{\tau=-3,t\neq-1}^{5} \lambda_\tau(\tau_{i,t} \cdot T_i) + \sum_{\tau=-3,t\neq-1}^{5} \psi_\tau \tau_{i,t} + \beta X_{i,t} + \upsilon_i + \varepsilon_{i,t} \tag{7}$$

Where $y_{i,t}$ denotes individual i's labor market outcome, at time t. The treatment indicator T_i equals one for those with a child born in 2002. λ_τ are the coefficients of interest, which captures the difference in outcomes between couples with children born in 2002 versus when couples with children born in 2001, in each year before and after birth, relative to the calendar year of birth.[11] X_i educational attainment, maternal age at birth, and a dummy for a first-born child. I include individual fixed effects and month-of-birth dummies. This account for seasonality in how childbirth affects labor market income. If a couple becomes parents in December will have almost unaffected earnings in the calendar year of the birth, while a couple becoming parents in January will experience the effect of parenthood on all 12 months of the year.

[11] I normalize $\lambda_{-1} = 0$.

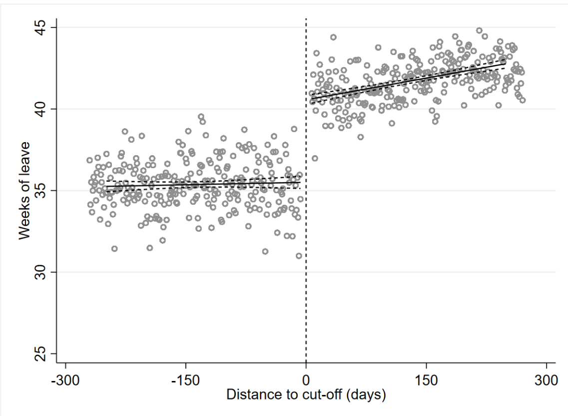
4 Results

4.1 Graphical Results

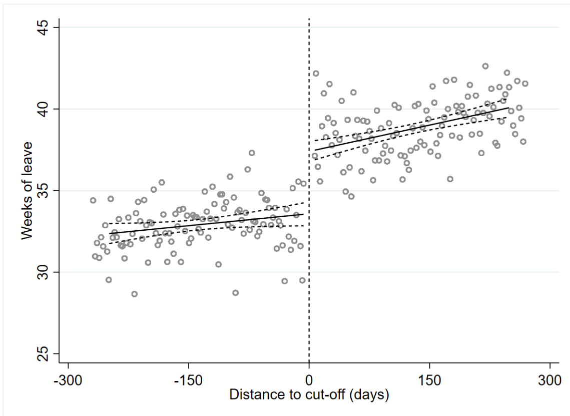
An RD-design provides a transparent and illustrative way of visualizing the identification of the treatment effects (Thistlethwaite & Campbell (1960); Imbens & Lemieux (2008)). The graphical results reported in this section are without individuals level controls. Figure 8 shows the average leave duration of the full population in the reform-window among mothers and fathers, split by relative earnings in the household. While there is a difference in the initial duration of mothers' leave, the reform affects the two groups similarly with a jump of 5 weeks. Among fathers who are primary earners, the reform leads to a small reduction in average leave duration. In households where the mother is the primary earner, there is no change in the average leave duration of fathers.

Figure 8: Graphical Illustration of the Reform Effects Split by Relative Earnings

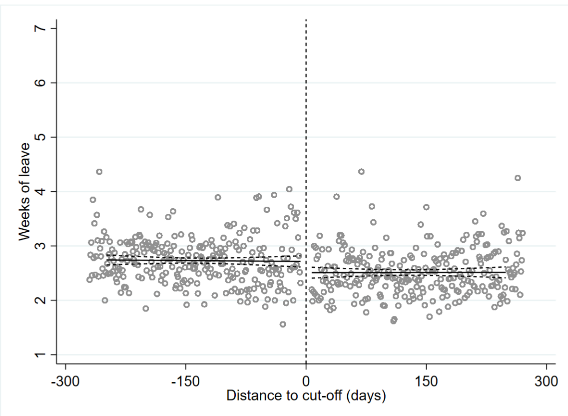
(a) Mothers' Leave, Father earns most



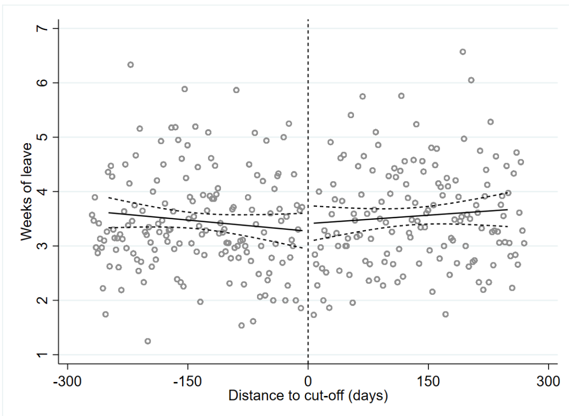
(b) Mothers' Leave, Mother earns most



(c) Fathers' Leave, Father earns most



(d) Fathers' Leave, Mother earns most

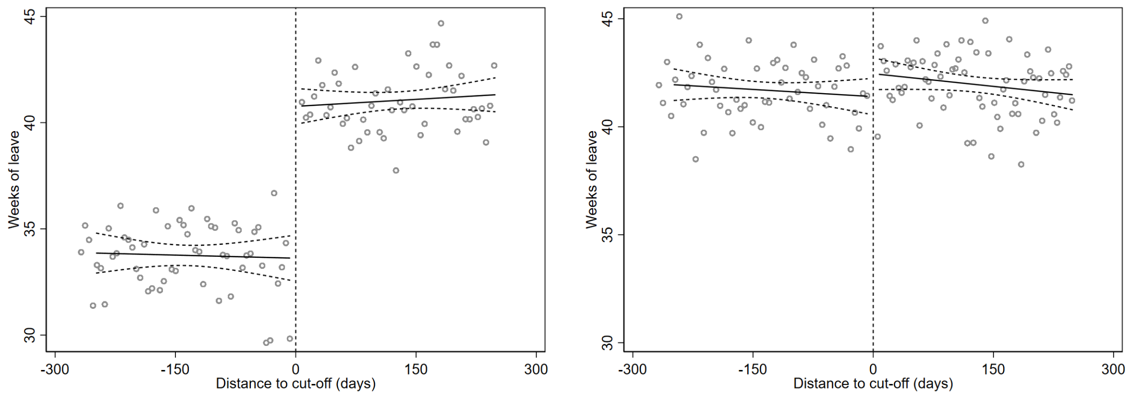


Notes: The figure shows the average leave duration measured in weeks of mothers (top panel) and fathers (bottom panel) stratified by relative earnings in the household in the year prior to childbirth. The measure of leave does not include leave taken prior to childbirth. The running variable is the date of birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples. Each bin includes 50 observations and kernels are uniforms. Using a quadratic fit barely makes a difference.

Figure 9 shows the average leave duration around reform introduction and the subsequent peer effects for sister-pairs. The reform window in Figure 9a illustrates the first-stage for mothers in the reform window who have a sister who gives birth between October 2002 and December 2005. There is a sharp jump in the average leave duration from 34 weeks to 41 weeks. The graphical depiction of the peer effects in Figure 9b corresponds to the reduced form, showing that mothers with a sister in the reform treatment group do indeed take a longer leave than those with a sister in the reform control group. The difference is around 1 week and appears to be borderline significant even without individual and peer level controls.

Figure 9: Reform and Peer Effect, Sister-pairs

(a) Reform Effect (b) Peer Effect



Notes: The figure shows average leave duration measured in weeks of sets of sisters. On the right side, the leave duration of the sister in the reform window is reported. On the left side, the average leave duration of the sisters who themselves give birth between 1st of October 2002 and the end of 2005 is reported. The measure of leave does not include leave taken prior to childbirth. The running variable is the date of childbirth of the sister in the reform window. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 3,808 mothers with sisters in the reform window. Each bin includes 35 observations and kernels are uniform.

4.2 Regression-based Results

This section first presents the results on leave duration following reform implementation and investigate if gender gaps in leave duration can be explained by gender gaps in earnings within the household. After having shown that relative earnings hardly influence leave duration, I document meaningful heterogeneity by maternal labor supply. In particular, women exposed to a mother who worked full-time respond less to the reform. Before moving on to the peer effects, I show the reform effect on earnings for both men and women with children born in the reform window.

4.2.1 Leave Duration

Table 1 presents the reform effects on mothers and fathers. The estimates for the baseline model for mothers are reported in column (1) and for fathers in column (3). Mirroring the results reported in Figure 5, the estimated reform effect on the mothers' leave is 4.9 weeks, and there is no effect on the average duration of fathers' leave. In column (2) and (4), I add an interaction term between relative earnings in the household and the treatment indicator, as outlined in Equation 3. Both before and after the reform, mothers in couples where the father is the primary earner take a longer leave compared to couples where she is the primary earner. Before the reform, the difference is 1.5 weeks. The interaction effect is far from statistically significant. This corresponds to a longer initial duration among mothers who are not primary earners, but no additional reform effect for mothers who are not primary earners.[12] This is in contrast to what the theory of specialization predicts. Before the reform, fathers are outearned by their partner take a 0.6 weeks longer leave compared to fathers who are primary earners. In the baseline specification, the reform effect is insignificant. However, when the interaction term is added the reform effect, which can be interpreted as the reform effect among fathers who are primary earners, becomes significant and negative, implying that fathers who are primary earners reduce their leave by 0.2 week upon the reform. Adding the reform effect and the interaction term together shows that fathers who were not primary earners did not change their leave duration.[13]

Columns (5) and (6) present an alternative specification of the reform effects on fathers to shed light on those fathers who take long leaves upon reform implementation. Defining the outcome as a dummy that takes the value 1 if the fathers take a leave of 8 weeks or longer, the reform implies an increase in 1.6 %-point probability of fathers taking a long leave. When adding an interaction term, we see that fathers who are not primary earners are more likely to take a long leave compared to those who are primary earners. With the reform, the size of this effect increases with 2.8 %-point, from 3.7 %.

Overall, relative earnings have a very small impact on the reform effect. Regardless of relative earnings, mothers respond similarly to the reform while fathers' leave duration is largely unchanged. Among the fathers with partners who out-earn them, the reform increases the likelihood of a long leave by 2.8 %-point. While this is in the direction of what the theory of specialization predicts, the magnitude is tiny compared to the 4.7 weeks increase in leave duration of the mothers in these households. Thus, the predictions provided by the theory of specialization are not matched by the data.

[12] The controls enter with the expected sign when they are significant (see Appendix D), but an interpretation of the controls should keep in mind that they are likely to correlate with unobservables. Notably, the estimates do not change whether the controls are included or not (see Section '4.4 Robustness' below).

[13] A alternative measure of productivity would be education. Using relative educational attainment provides very similar results, i.e. no meaningful heterogeneity. This is reported in Appendix D2.

TABLE 1: Reform effects on leave duration, effect from relative earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Mothers' leave duration (weeks)		Fathers' leave duration (weeks)		Fathers' taking long leave (dummy=1 if leave $\geq$ 8 weeks)[a]	
VARIABLES	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
Reform effect	4.921***	4.715***	-0.136	-0.196**	0.0163***	0.0104***
	(0.219)	(0.288)	(0.0830)	(0.0830)	(0.00453)	(0.00711)
Interaction						
Reform X		-0.262		0.278***		0.0279***
Mother primary earner		(0.226)		(0.0925)		(0.00512)
Mother primary earner		-1.517***		0.593***		0.0374***
		(0.200)		(0.109)		(0.00635)
Observations	44,091	44,091	44,091	44,091	44,091	44,091
R-squared	0.128	0.130	0.028	0.032	0.025	0.041
Controls						
Household covariates	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES

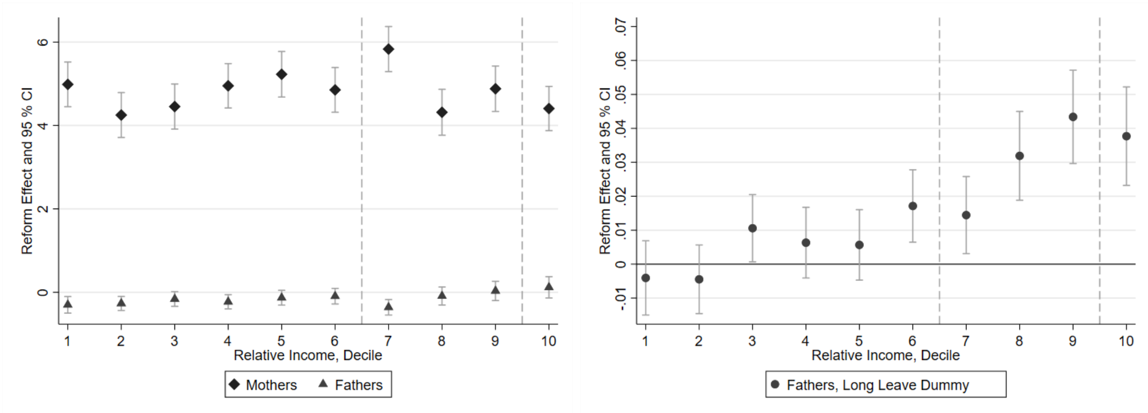
Notes: Full regression reported in the Appendix.
All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off.
Standard errors in parentheses are clustered on date of birth of child where *** p<0.01, ** p<0.05, and * p<0.1
[a]Changing this to 6 weeks provides virtually unchanged estimates. A lower threshold does not deal with the monotonicity issue as many fathers took 4 and 5 weeks of leave before the reform, as reported in Figure 6b.

Figure 10 presents the reform effect along the relative income distribution. Fathers' responses do not change along the distribution. Mothers who are earning more than half of the household income increase their leave duration by one week less than the baseline. Equivalent to the interaction term reported in Columns (2), (4), and (6) of Table 1, I interact the treatment indicator with deciles of relative income. The left figure reports the leave duration of mothers and fathers. The first 6 decile corresponds to the households where fathers are breadwinners. Decile 7 to 9 includes households close to parity (each member of the household earning no more than 20 % more than their partner) and decile 10 includes households where women outearn their partner by more than 20 %. If households maximize income, fathers in decile 7-10 should increase their leave more compared to other men, while women should increase their leave less compared to other women. Indeed, the reform effect on women in decile 9 and 10 is 4 weeks. However, fathers in these households are not increasing their leave equivalently. Instead, the reform effect on fathers' leave and the likelihood of them spending 8 weeks or more on leave is remarkably stable along the income distribution.[14]

[14]In Appendix D, I report results for heterogeneity across absolute earnings' of mothers in the year prior to childbirth. Again, only the absolute top-earners are reducing their leave, and fathers' are barely responding to their partner's income.

Figure 10: The Role of Relative Earnings

(a) Leave Duration (b) Long Leave (8 weeks)



Notes: The figures show the average leave duration measured in weeks of mothers and fathers (left panel) and a dummy for the father taking a long leave (8 weeks) (right panel) and interacting the treatment indicator with deciles of relative earnings in the household in the year prior to childbirth. I plot the reform effect along with 95 % CI. Dashed lines mark households with male breadwinners, equal households, and households with female breadwinners. The measure of leave does not include leave taken prior to childbirth. The sample size is 44,316 couples.

While the homogeneous response across relative earnings is indicative of the importance of gender norms for mothers' leave behavior, Table 2 presents the estimates obtained from Equation 3 with interaction terms to shed light on the role of inter-generational female labor supply, sectorial occupation, and child parity. Column (2) contains an interaction term between the labor supply of the maternal grandmother (of the child born in the reform window) and the treatment indicator; column (3) contains an interaction term between public sector employment of the mother and the treatment indicator and column (4) contains an interaction term between first-time mothers and the treatment indicator. Lastly, column (5) contains a model with all interaction terms.

Having a mother with a high labor supply reduces the reform effect. This is in line with the notion that gender identity is strongly influenced by family ties and the literature showing inter-generational transmission of gender identity and labor market choices. With the reform, those who had a mother with a low labor supply increased their leave by approx. half a week more than those with a mother with a high labor supply. Importantly, this specification includes important household covariates which may be influenced by maternal labor supply such as the new mothers' education, relative education within the couple, relative earnings, and age gaps.

TABLE 2: Reform effect on mothers' leave duration, alternative specifications

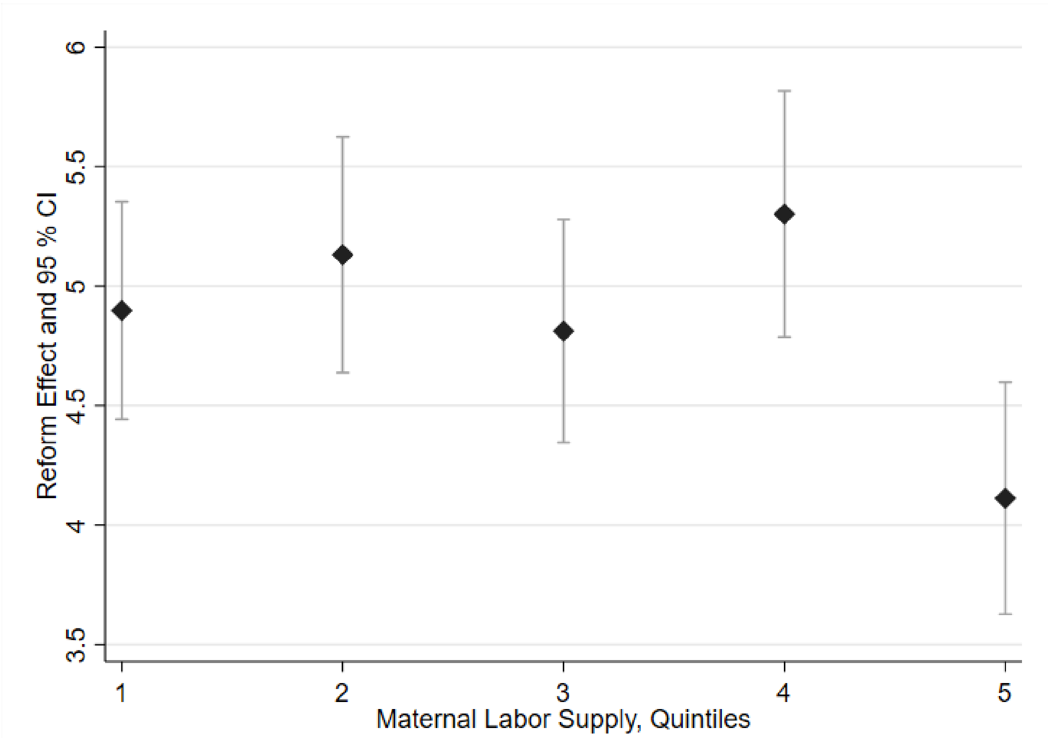
VARIABLES	(1) Baseline	(2) Maternal labor supply	(3) Sector	(4) Child parity	(5) Full model
Reform effect	4.912***	4.708***	5.566***	5.063***	5.485***
	(0.220)	(0.240)	(0.234)	(0.228)	(0.349)
Interactions					
Reform X		0.384**			0.324*
Low maternal labor supply		(0.180)			(0.180)
Low maternal labor supply		0.0199			0.0389
		(0.140)			(0.140)
Reform X			-1.540***		-1.565***
Publicly employed			(0.191)		(0.192)
Publicly employed			2.414***		2.286***
			(0.157)		(0.157)
Reform X				-0.350*	-0.400**
First-time mother				(0.181)	(0.182)
First-time mother				0.555***	0.620***
				(0.146)	(0.147)
Reform X					-0.242
Mother earning most					(0.229)
Mother earning most					-1.527***
					(0.200)
Observations	44,091	40,249	44,091	44,091	40,249
R-squared	0.129	0.127	0.129	0.127	0.130
Controls					
Household covariates	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES

Notes: All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off.
Maternal labor supply is defined as above or below the median in the sample.
Standard errors are clustered on date of birth of own child where *** p<0.01, ** p<0.05, * p<0.1

In Figure 11, I document that this effect is particularly strong for women with mothers who work full time. I show this by interacting the treatment indicators with quintiles of maternal labor supply. In the previous generation, approximately 20 % of mothers' worked full-time, corresponding to being in the highest quintile of labor supply. The reform effect is 4 weeks amongst these women, while the reform effect on women with mothers who worked less than full time is 5 weeks and stable.

Across sectorial occupation, there were substantial differences in average leave duration across the public and private sectors before the reform. Mothers working in the public sector took a 2.4 week longer leave than those working in the private sector. After the reform, a gap remains but the size is reduced. This is driven by a smaller reform response among mothers working in the public sector, who increase their leave duration by -1.5 weeks less than women in the private sector. That mothers employed in the private sector respond strongly and increase their leave by 5.6 weeks suggests that preferences for family-friendly work arrangements are not driving the results. The public sector in Denmark is known for offering more family-oriented amenities, while the private sector penalizes absenteeism more (Nielsen et al., 2004). Thus, it is not surprising that publicly employed mothers take a longer leave than those employed in the private sector, but the reduction in this gap is remarkable. I find a similar pattern for child parity although the magnitude is smaller. Before the reform, first-time mothers took half a week longer leave compared to mothers of higher parity. However, with the reform, this gap is reduced. The heterogeneity across sectors and child parity is greatly reduced with the reform implying that new mothers start to behave more similarly. The more homogeneous leave behavior in the population is driven by larger reform effects among mothers with characteristics that would have suggested a short leave in the absence of the reform.

Figure 11: The Role of Maternal Labor Supply



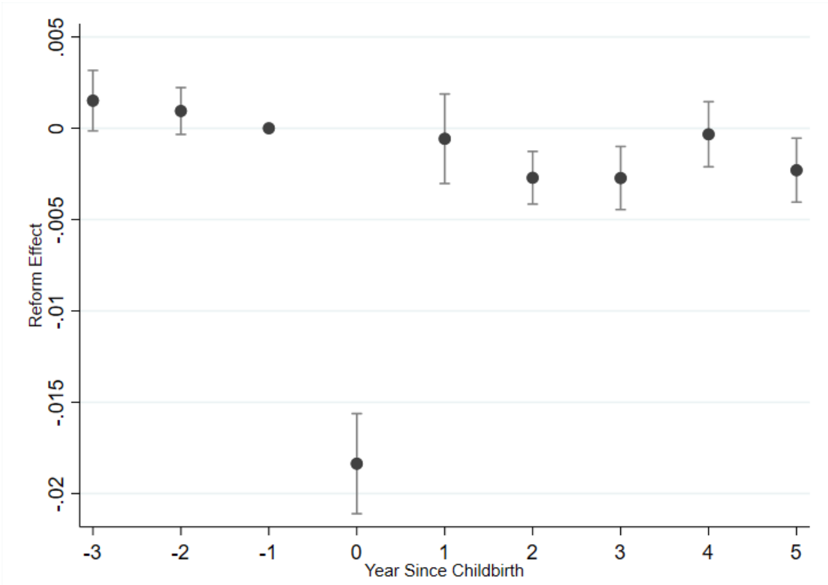
Notes: The figure shows the average leave duration measured in weeks of mothers after interacting the treatment indicator with deciles of maternal labor supply that the new mothers themselves were exposed to in childhood. This measure is obtained from mandatory pension scheme contributions that depend on hours worked. In the sample, approximately 20 % of mothers worked full time and less than 10 % didn't work at all. I plot the reform effect along with 95 % CI. The measure of leave does not include leave taken prior to childbirth. The sample size is 40,249 women as information on maternal labor supply is missing for 3842 women.

4.2.2 Earnings

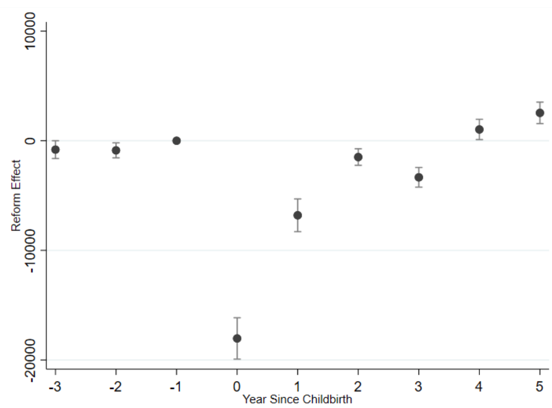
To understand the effect on earnings from extended parental leave, I implement a Difference-in-Difference design as specified in Equation 7, and report the results in Figure 12. The results show that the reform on average increased intra-household earnings inequality. The effect is primarily driven by women facing a larger earnings reduction in the first year following childbirth, but the reduction in women's labor market earnings remains statistically significant. In the first year, women's labor market income share is reduced by 18.000 DKK, and fathers' earnings are reduced by 8.200, corresponding to an increase in the intra-household earnings gap of 1.8 %. Figure 12 also shows the effect on earnings for up to year 5 and show that men's earnings are increasing more than women's in year 4 and 5.

Figure 12: Labor Market Outcomes

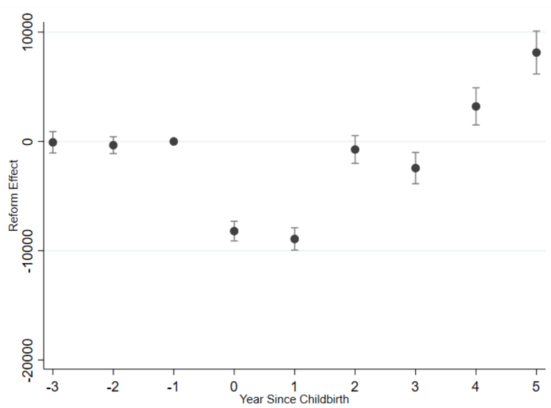
(a) Mothers' Share of Labor Market Income



(b) Mothers' Labor Market Earnings



(c) Fathers' Labor Market Earnings



Notes: The figure shows the dynamic reform effect from the reform on the share of household income earned by the mothers (top), on women's labor market earnings (bottom left), and men's labor market earnings (bottom right) in the years around parenthood. I plot the reform effect along with 95 % CI. The sample size is 44,316 couples.

4.2.3 Peer Effects

Table 3 presents the estimates of the peer effects for the sisters. The first-stage is reported in column (1). Column (2) reports the reduced form corresponding to Figure 9b, but now with added individual level and peer controls which improve precision. The point estimate corresponds to 1.1 weeks of additional leave among mothers with sisters who had a child after reform implementation. The 2nd stage estimate is reported in column (3) and shows a 17 % increase in leave duration compared to the reform effect. The reform-induced change in behavior of mothers in the reform treatment group implies that the peer sisters observe different prescriptions, depending on when their niece/nephew was born. They change their behavior accordingly so that those exposed to the behavioral norm of long leave take a longer leave themselves, and this shows up here as peer effects.

Additional interaction terms are added to the reduced form. Columns (4)-(10) contain the reduced form model with interaction terms for both own and peer category. The point estimate increases in size when adding the interaction effect of the labor supply of the maternal grandmother, sectorial employment of the mother exposed to the peer effects from the reform, and child parity of the sister in the reform window.

As a result of the reform, women with a sister in the reform treatment group observe their sister taking a longer leave, while women with sisters in the reform control group observe a shorter leave. These prescriptions are transmitted and show up here as a peer effect. Those who observe their sister taking a long leave to increase their own leave duration with 1.1 week compared to women who observe their sister taking a shorter leave. Interestingly, the peer effect is larger among those who had a mother with a high labor supply. Thus, observing a sister taking a longer leave reduces the intergenerational effect. Combined, reform effects and peer effects show that the reform reinforced existing gender gaps in intra-household specialization, and that difference in faced by mothers and fathers is the relevant mechanism behind this inequality in time allocation.

TABLE 3: Peer effects on mothers leave duration

VARIABLES	(1) 1st stage	(2) Reduced form	(3) 2SLS	(4) Maternal labor supply	(5) Own sector	(6) Peer sector	(7) Own earnings	(8) Peer earnings	(9) Own 1st child	(10) Peer 1st child
Peer effect	6.815***	1.145**	0.168**	1.423**	1.349**	0.986*	1.171***	1.255**	0.948	1.326**
	(0.709)	(0.554)	(0.0809)	(0.655)	(0.601)	(0.598)	(0.562)	(0.571)	(0.644)	(0.599)
Peer effect X				-0.524						
Low maternal labor supply				(0.536)						
Low maternal labor supply				0.766*						
				(0.392)						
Peer effect X					0.982***					
Publicly employed					(0.413)					
Publicly employed					1.446**					
					(0.429)					
Peer effect X						0.360				
Sister publicly employed						(0.572)				
Sister publicly employed						0.253				
						(0.421)				
Peer effect X							-0.119			
Mother primary earner							(0.627)			
Mother primary earner							-0.910			
							(0.627)			
Peer effect X								-0.498		
Sister primary earner								(0.702)		
Sister primary earner								0.152		
								(0.600)		
Peer effect X									0.386	
First-time mother									(0.551)	
First-time mother									0.146	
									(0.314)	
Reform X										-0.450
Sister first-time mother										(0.544)
Sister first-time mother										0.396
										(0.373)
Observations	3,154	3,154	3,154	3,154	3,154	3,154	3,154	2,996	3,154	3,154
R-squared	0.172	0.064	0.076	0.064	0.064	0.064	0.064	0.064	0.067	0.065
Controls										
Peer covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Own coviartes	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: All specifications include the running variable (d_i, date of birth) and the running variable interacted with the treatment indicator.
Standard errors in parentheses are clustered on date of birth of peer child where *** p<0.01, ** p<0.05, and * p<0.1.

4.3 Alternative Explanations

Women increase their leave behavior upon reform implementation while fathers' leave behavior is largely unchanged, regardless of relative earnings. Women who had a working mother themselves increase their leave by less than those who had a stay-at-home mother. Subsequently, women with sisters in the reform treatment group take a longer leave compared to those who have a sister in the reform control group. These results are highly consistent with the notion of pay-off from gender identity. However, a number of alternative explanations are investigated. While these channels cannot be ruled out definitively, none of these channels can explain both the reform effects and the peer effect. While arguments related to biology, in particular a wish for extended breastfeeding, could explain the gender gap in take-up after the reform. However, this can neither explain the heterogeneous effects by maternal labor supply nor the peer effects. Two channels are proposed to explain peer effects but fail to provide a meaningful explanation for the reform effect: information and consumption externalities. I argue that the reform was public knowledge when the sisters went on leave. Moreover, studies that evaluate leave of this length overwhelmingly show that private benefits are zero to small. By investigating heterogeneity by geographical proximity and close spacing of births, I directly show that consumption externalities are not driving the peer effect. In sum, no other explanation than that of gender identity and related norms provide a compelling explanation of both the reform and peer effect.

4.3.1 Biology

Biological differences between men and women, in particular breastfeeding, are often proposed as a potential explanation for diverging labor market outcomes after parenthood. In this setting, the average leave duration prior to the reform well extended the recommended period of full breastfeeding (Sundhedsstyrelsen, 2008). In 2002, 13.2 % of Danish children were exclusively being breastfed when they turned 6 months, and this number has been fairly stable (Johansen et al., 2016). At 4 months, this number was 63.3 % in 2002. Thus, the extended leave is being held when the child is reaching an age where other types of food are becoming an increasingly important component of the diet. Moreover, earmarked leave for mothers ensures 'sick days' for 3 months after childbirth and this component of the leave system was unchanged by the reform.[15] Recent research has provided compelling evidence against the physiological aspects of motherhood as the main factor contributing to the unequal division of care work. Biological and adoptive mothers face almost identical post-childbirth labor market trajectories (Rosenbaum (2021); Kleven et al. (2021)). Moberg & van der Vleuten (2022) show striking similarities in the division, length, and

[15] See Persson & Rossin-Slater (2019) for a framework specifically on the different types of leave around childbirth

timing of parental leave for biological and adoptive children. Among same-sex couples, the drop in earnings is smaller for the birth parent compared to heterosexual couples (Moberg (2016); Andresen & Nix (2022); Moberg, Evertsson, & van der Vleuten (2021)). In contrast to different-sex couples, Moberg et al. (2021) find that social parents in same-sex couples also experience a drop in earnings. Rosenbaum (2019) and Andresen & Nix (2022) find no meaningful within household differences in earnings trajectories following parenthood. This is also the case when accounting for intra-household earnings gaps prior to childbirth. Importantly, neither the heterogeneous effects by maternal labor supply nor the peer effects can be explained by any factors related to biology.

A related argument is that of comparative advantages. Using the observed duration of parental leave and the relative earnings in the household, one can infer the relative differences in productivity that would be required for the observed time allocation to be maximizing household production. I follow Siminski & Yetsenga (2022) who infer comparative advantages required time allocation to be in line with a standard Becker (1981)-model using an Australian time use survey. They conclude that Australian women need to be more than 109 times as productive as men for the observed time allocation to be efficient. In this setting, in couples at wage parity, women would need to be 14 times as productive in the home compared to men. At the 98th percentile, this number is 96.2.

4.3.2 Information

One mechanism that could explain the peer effects is information transmission. In particular two types of information come to mind; information about the reform and information about the private benefits of extended leave. Regarding the former, it seems very unlikely that the mothers having a child later than in the fall of 2002 did not know about this reform. First, this reform was widely reported in Danish media. Second, pregnancy arguably provides couples with sufficient time to seek out relevant information from government agencies, unions, and their employer. In order to directly test this, I drop the sisters bunching at the old cut-off of the high benefits which also translated into the mode leave duration before the reform, as shown in Figure 6a. If a lack of knowledge about the reform is the relevant mechanism, dropping the observations at the old threshold for paid leave should dramatically reduce the peer effect. However, this yield largely unchanged estimates. This is reported in Appendix E. Regarding benefits of staying at home with the child, it is extremely difficult to rule this channel out. However, research that evaluates this reform finds small effects on maternal health compressed among low-resource families and no effect on child health (Beuchert et al., 2016). A German expansion of leave coverage with a strong impact on mothers' leave behavior did not result in improved child outcomes (Dustmann & Schönberg, 2012). In Norway, Dahl et al. (2016) find that extended maternity leave does not affect child education, marital status or subsequent fertility. While these outcomes obviously don't capture all aspects of increased maternity leave, we might be willing to think of them as correlated with other types of private benefits. I document that women's earnings are on average reduced, and evaluating the same reform as me, Tô (2018) shows that women who increase their leave the most face a larger reduction in earnings. Dahl et al. (2016) find no effect on Norwegian women's labor market earnings. Overall, the evidence of non-monetary benefits of extended maternity leave of this length are zero to small and might even hurt women's labor market trajectories.

4.3.3 Consumption Externalities

Some women might enjoy being on maternity leave at the same time as their sister, and thus consumption externalities might arise. However, since births are spaced in time there is limited scope for the sisters to be on leave at the same time. To more directly investigate this, I add an interaction term between a dummy for living in the same municipal as one's sister and the treatment indicator. The results show that mothers in the control group who lived in the same municipal as their sister took a longer leave compared to those who did not live in the same municipal. This effect disappears with the reform. This could potentially be driven by those in the reform control group who used the leave at a reduced rate and potentially experienced positive externalities of being on leave at the same time as their sister. This opportunity for consumption externalities is reduced with the reform. Moreover, we might be worried that some sisters coordinate fertility and those are the ones that particularly enjoy being on leave at the same time. To rule out that the peer effects are driven by such sisters, I exclude sister-pairs where the second child was born between October 2002 and January 2003. The sample is reduced, but the point estimate increases slightly. This is the opposite of what should be expected if the peer effects were driven by coordinated fertility. Arguably, the peer effects estimated here are not driven by consumption externalities. These estimates are reported in Appendix E.

4.4 Robustness

The robustness checks show a research design with very stable results. Running the model without controls, allowing for a quadratic, cubic, or quartic shape of the running variable, varying the bandwidth and the excluded number of days around cut-off around implementation provide virtually unchanged point estimates. For all specifications, the point estimate of the reform effect is between 4 and 6 weeks of leave. Due to a small sample size, the precision decreases when the bandwidth is set to 30. This is reported in Appendix F. The average reform effect that comes out of this exercise is 5.3 weeks. Out of the 249 regressions, all estimates but two are significant on a 0.99 pct. level (average t-statistics is 20.396.). Performing the equivalent exercise for the reform effect on the fathers yields an average reform effect of -0.13 weeks which is far from being significant at conventional levels. For the reduced form estimate of the peer effects, the point estimate is stable at approximately 1 week. As the sample size is much smaller for peer effects than for the reform effect, the precision decreases, especially when individual-level controls are dropped, and having a bandwidth below 120 is not feasible.

5 Concluding Remarks

Using detailed administrative data from Denmark on parental leave duration, earnings gap within couples, and family behavior combined with a quasi-experimental RD-design, I provide robust estimates of 5 weeks increase in parental leave duration among mothers upon a reform that expanded parental leave. The reform also reduced mothers' share of intra-household earnings. Meanwhile, the average leave duration of fathers is unchanged. These estimates barely change across relative earnings. Instead, the results are highly consistent with a large role played by gender norms when households are deciding how to allocate time. In line with the literature showing inter-generational transmission of labor supply, those who are exposed to high maternal labor supply in childhood take a shorter leave upon reform implementation compared to those exposed to more traditional gender roles in childhood. Second, the reform-induced change in leave duration implies that women with a sister in the reform treatment group face a new norm of extensive leave. This shows up here as peer effects and further reaffirms gender-specific intra-household specialization.

As mothers are the primary users of extended leave, this widens the gender gap in earnings. The literature investigating parental leave policies often refers to an 'inversed U-shape', where the introduction of maternity leave is improving women's outcomes with decreasing returns to extending the leave and very long leaves tend to hurt women (Rossin-Slater, 2018). While the negative effect on women's share of household earnings reported here is small, it is persistent. The effect is partly driven by a reduction in women's earnings in the year following childbirth and later by a larger growth in men's earnings. In short, the reform increased intra-household inequality. In general, many family policies may have this effect. Indeed, the family-friendly policies in the Nordic countries have been characterized as a 'system-based glass-ceiling' (Datta Gupta et al., 2008) because they mainly affect the labor market outcomes of women. The results reported here support this reasoning and highlight the role of gender norms as an important factor for intra-household specialization. If family policies do not explicitly encourage fathers to use them, they will mainly be considered relevant for mothers and strengthen existing gender gaps.

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Appendix

Appendix A: Data description

The measure of leave duration is calculated based on data from the Danish Ministry of Employment's DREAM-database.

This database contains a weekly measure of individual benefits from the government. This include unemployment benefit, sickness benefit, old age benefits, education benefit, among others. If multiple benefits is received the same week, the highest amount is recorded. The measure of parental leave is constructed as a count of number of weeks a parent receives parental leave benefits ('Barselsdagpenge') or receives childcare benefits ('Børnepasningsorlov') is included.

Background variables and labor market data

Using BEF (population), UDDA (education), FIRM (firm), and IDAN (employment), I have background variables of all parents. The variables used include

Age	BEF
Gender	BEF
Family identifiers	BEF
Number of children in the family	BEF
Education	UDDA
Income and earnings	IDAN
Retirement contributions	IDAN
Sectorial occupation	FIRM
Occupation unit/firm	FIRM

Appendix B: Sample restrictions

Table B.1: Restriction on data

3*Year	Initial number of observations	Same-sex parents	Fathers co-habiting with child	Twin births	At least one parent enrolled in education	No ATP for for at least one parent	At east one parent is self-employed	Remaining number of observations
2001	58134	25	327	1135	6760	2730	3189	43968
2002	58385	25	302	1235	6953	3177	2655	44038
2003	59140	36	319	1255	7399	2852	3211	44068
2004	59093	39	298	1303	7594	2772	3211	43854
2004	58700	45	282	1296	7798	2697	3214	43368
%	100	0.06	0.52	2.12	12.44	4.85	5.28	74.74

Source: Own calculations based on data from Statistics Denmark

Table B.2: Additional restrictions on the data

3*Year	No information on earnings available for at least one parent	Remaining number of observations	No leave records on mothers	Remaining number of observations
2001	10745	33223	1614	31609
2002	9766	34272	2049	32223
2003	8937	35131	1467	33664
2004	7854	36000	1735	34265
2005	6811	36557	2010	34547
%	15.03	59.70	3.02	56.67

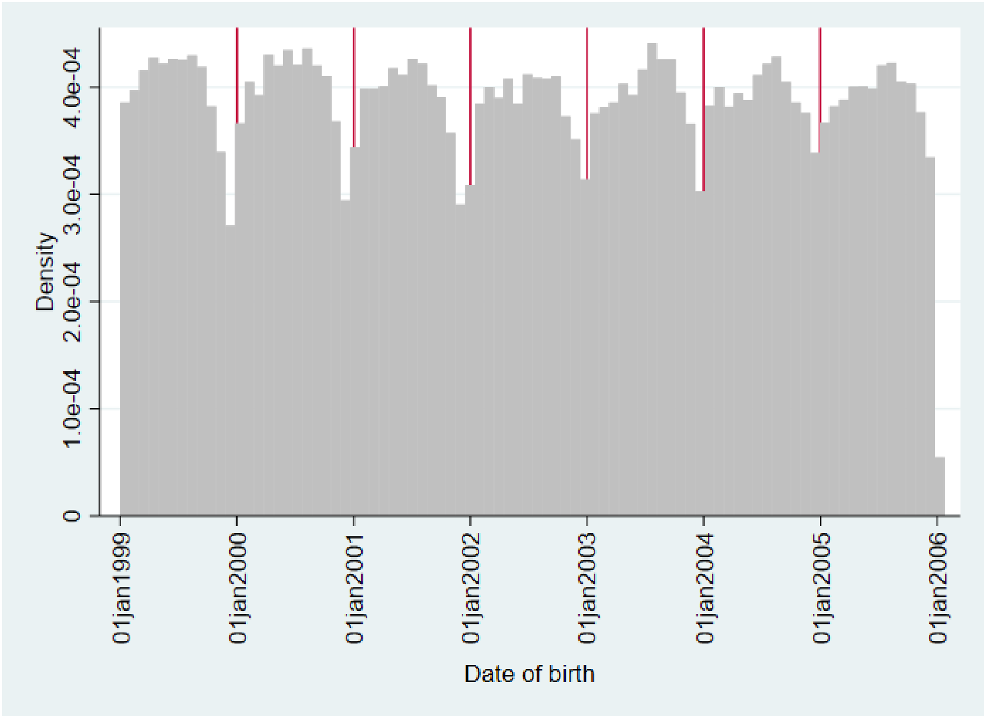
Source: Own calculations on data from Statistics Denmark

Appendix C: Leave duration

TABLE C1: Formal check of bulking at cut-off, polynomial density estimation

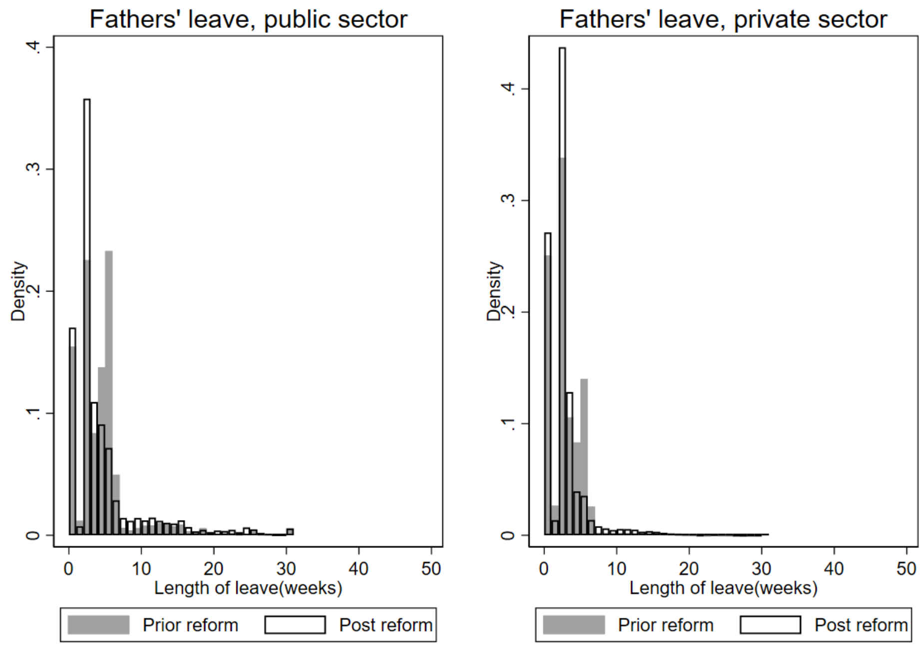
Reform window			Peers		
No donut	Left of c	Right of c	No donut	Left of c	Right of c
Cut-off			Cut-off		
Number of obs	21763	23409	Number of obs	1615	1640
Efficient # of obs	2628	4184	Efficient # of obs	250	493
Order est (p)	2	2	Order est (p)	2	2
Order bias (q)	3	3	Order bias (q)	3	3
BW est	48.684	49.910	BW est	59.894	76.730
Running variable: assign			Running variable: assign		
Method	T	P>$\|\|T\|\|$	Method	T	P>$\|\|T\|\|$
Conventional	9.178	0.0000	Conventional	3.361	0.0008
Robust	7.396	0.0000	Robust	1.507	0.1319
7 days	Left of c	Right of c	7 days	Left of c	Right of c
Cut-off			Cut-off		
Number of obs	21475	22841	Number of obs	1593	1600
Efficient # of obs	3183	4629	Efficient # of obs	234	446
Order est (p)	2	2	Order est (p)	2	2
Order bias (q)	3	3	Order bias (q)	3	3
BW est	50.650	55.840	BW est	60.042	75.227
Running variable: assign			Running variable: assign		
Method	T	P>$\|\|T\|\|$	Method	T	P>$\|\|T\|\|$
Conventional	5.773	0.0000	Conventional	2.973	0.0030
Robust	3.972	0.0000	Robust	0.988	0.3234
14 days	Left of c	Right of c	14 days	Left of c	Right of c
Cut-off			Cut-off		
Number of obs	21159	22267	Number of obs	1572	1562
Efficient # of obs	2287	4408	Efficient # of obs	213	408
Order est (p)	2	2	Order est (p)	2	2
Order bias (q)	3	3	Order bias (q)	3	3
BW est	52.69	64.98	BW est	60.331	75.625
Running variable: assign			Running variable: assign		
Method	T	P>$\|\|T\|\|$	Method	T	P>$\|\|T\|\|$
Conventional	4.171	0.0000	Conventional	2.610	0.0091
Robust	-0.172	0.864	Robust	0.535	0.5924

Drop in births at New Year



Notes: The figure shows the histogram of birth in the raw data before imposing any restrictions. Red lines mark New Year's. Every year there is a drop around the holidays.

Fathers' leave, sectorial occupation



Appendix D: Regression output

TABLE D1: Reform effects on leave duration, by relative earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Mothers' leave duration (weeks)		Fathers' leave duration (weeks)		Fathers' taking long leave (dummy) if leave $\geq$ 8 weeks)[a]	
	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
Reform effect	4.921***	4.715***	-0.136	-0.196**	0.0163***	0.0104***
	(0.219)	(0.288)	(0.0830)	(0.0830)	(0.00453)	(0.00711)
Interaction						
Reform X		-0.262		0.278***		0.0279***
Mother primary earner		(0.226)		(0.0925)		(0.00512)
Mother primary earner		-1.517***		0.593***		0.0374***
		(0.200)		(0.109)		(0.00635)
Running, before reform	0.00173*	0.00179*	-0.000454	-0.000480	2.75e-06	8.92e-07
	(0.000974)	(0.000974)	(0.000360)	(0.000361)	(1.86e-05)	(1.85e-05)
Running, after reform	0.00687***	0.00688***	0.000819	0.000810	7.39e-05***	7.30e-05***
	(0.00127)	(0.00127)	(0.000498)	(0.000498)	(2.82e-05)	(2.79e-05)
Co-variates (mother)						
Age	0.103***	0.113***	0.0307***	0.0264***	0.00179***	0.00148***
	(0.0134)	(0.0134)	(0.00533)	(0.00531)	(0.000282)	(0.000281)
High school education	-0.476**	-0.443*	0.332***	0.317***	0.0130***	0.0119***
	(0.238)	(0.238)	(0.0755)	(0.0756)	(0.00446)	(0.00445)
Vocational training	0.137	0.140	0.170***	0.168***	0.00331	0.00316
	(0.195)	(0.195)	(0.0545)	(0.0549)	(0.00317)	(0.00320)
Some college	-0.720***	-0.665**	0.626***	0.600***	0.0268***	0.0249***
	(0.277)	(0.276)	(0.0939)	(0.0935)	(0.00545)	(0.00542)
BA or equivalent	1.016***	1.120***	0.726***	0.679***	0.0393***	0.0359***
	(0.224)	(0.225)	(0.0673)	(0.0676)	(0.00411)	(0.00412)
MA or Phd	-2.146***	-1.939***	1.979***	1.885***	0.125***	0.118***
	(0.271)	(0.272)	(0.112)	(0.112)	(0.00713)	(0.00714)
Same edu level as partner	-1.027***	-1.044***	0.0966**	0.104**	0.00451	0.00498*
	(0.133)	(0.133)	(0.0485)	(0.0485)	(0.00274)	(0.00273)
More edu than partner	-1.517***	-1.493***	-0.0515	-0.0631	-0.00510	-0.00597*
	(0.143)	(0.144)	(0.0536)	(0.0535)	(0.00316)	(0.00315)
ln(household income)	-5.783**	-4.835*	12.17***	11.72***	0.346***	0.313***
	(2.705)	(2.706)	(1.031)	(1.033)	(0.0560)	(0.0557)
ln(household income)^2	0.163	0.115	-0.487***	-0.466***	-0.0140***	-0.0124***
	(0.106)	(0.106)	(0.0406)	(0.0407)	(0.00221)	(0.00220)
Share of hh income earned	-6.979***	-4.616***	0.867***	-0.188	0.0641***	-0.0101
	(0.262)	(0.346)	(0.114)	(0.142)	(0.00633)	(0.00804)
Working in the public sector	1.622***	1.480***	-0.0753*	-0.0117	-0.00932***	-0.00483**
	(0.109)	(0.110)	(0.0401)	(0.0401)	(0.00240)	(0.00237)
First child, dummy	0.374***	0.412***	0.374***	0.357***	0.0193***	0.0182***
	(0.100)	(0.100)	(0.0393)	(0.0390)	(0.00239)	(0.00236)
Constant	82.56***	75.77***	-74.83***	-71.72***	-2.210***	-1.986***
	(17.25)	(17.27)	(6.588)	(6.608)	(0.357)	(0.354)
Observations	44,091	44,091	44,091	44,091	44,091	44,091
R-squared	0.127	0.130	0.028	0.032	0.035	0.041

Standard errors in parentheses are clustered on date of birth of child
*** p<0.01, ** p<0.05, * p<0.1

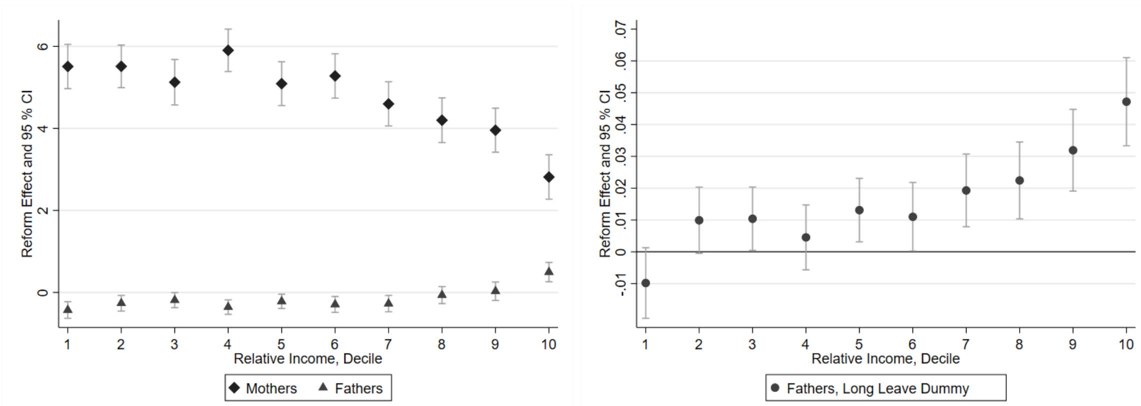
TABLE D2: Reform effects on leave duration, relative education

	(1) Baseline	(2) Mother's leave	(3) Father's leave	(4) Dummy
Reform Effect	4.921***	4.588***	-0.190*	0.00897
	(0.219)	(0.266)	(0.0990)	(0.00551)
Interaction				
Reform X		0.505**	0.0671	0.00828*
Same education		(0.233)	(0.0842)	(0.00501)
Same education	-1.027***	-1.285***	0.0624	0.000287
	(0.133)	(0.193)	(0.0594)	(0.00322)
Reform X		0.360	0.0748	0.0116**
Mother more educated		(0.243)	(0.0887)	(0.00522)
Mother more educated	-1.517***	-1.700***	-0.0898	-0.0111***
	(0.143)	(0.197)	(0.0657)	(0.00361)
Observations	44,091	44,091	44,091	44,091
R-squared	0.127	0.127	0.028	0.035
Controls				
Peer covariates	YES	YES	YES	YES
Own covariates	YES	YES	YES	YES
Time trend	YES	YES	YES	YES

The baseline category is 'mother less educated'.
Standard errors in parentheses are clustered on date of birth of child
*** p<0.01, ** p<0.05, * p<0.1

The Role of Absolute Earnings

(a) Leave Duration (b) Long Leave (8 weeks)



Notes: The figure shows the average leave duration measured in weeks of mothers and fathers (left panel) and a dummy for the father taking a long leave (8 weeks) (right panel) and interacting the treatment indicator with deciles of absolute earnings of the mother in the year prior to childbirth. I plot the reform effect along with 95 % CI. The measure of leave does not include leave taken prior to childbirth. The running variable is date of birth of own child. Cut-off is 1st of Jan 2002 and the window on each side of cut-off is 9 months. The sample size is 44,316 couples.

Appendix E: Alternative Explanations

TABLE E1: Information on eligibility

	Excl. those at old cut-off			Excl. those around old cut-off		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	First stage	ITT	2SLS	First stage	ITT	2SLS
Reform/peer effect	6.738***	1.091**	0.162**	6.773***	0.992**	0.146**
	(0.727)	(0.494)	(0.0736)	(0.732)	(0.487)	(0.0719)
Observations	3,059	3,059	3,059	3,002	3,002	3,002
R-squared	0.169	0.065	0.074	0.169	0.062	0.071
Controls						
Peer covariates	YES	YES	YES	YES	YES	YES
Own covariates	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES

Old cut-off is 24 weeks, the duration of benefits equivalent to UI prior to the reform and the mode leave duration observed in Figure 5. As an extension, those at 23 and 25 weeks is also excluded.
All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off.
Standard errors in parentheses are clustered on date of birth of peer child
*** p<0.01, ** p<0.05, * p<0.1

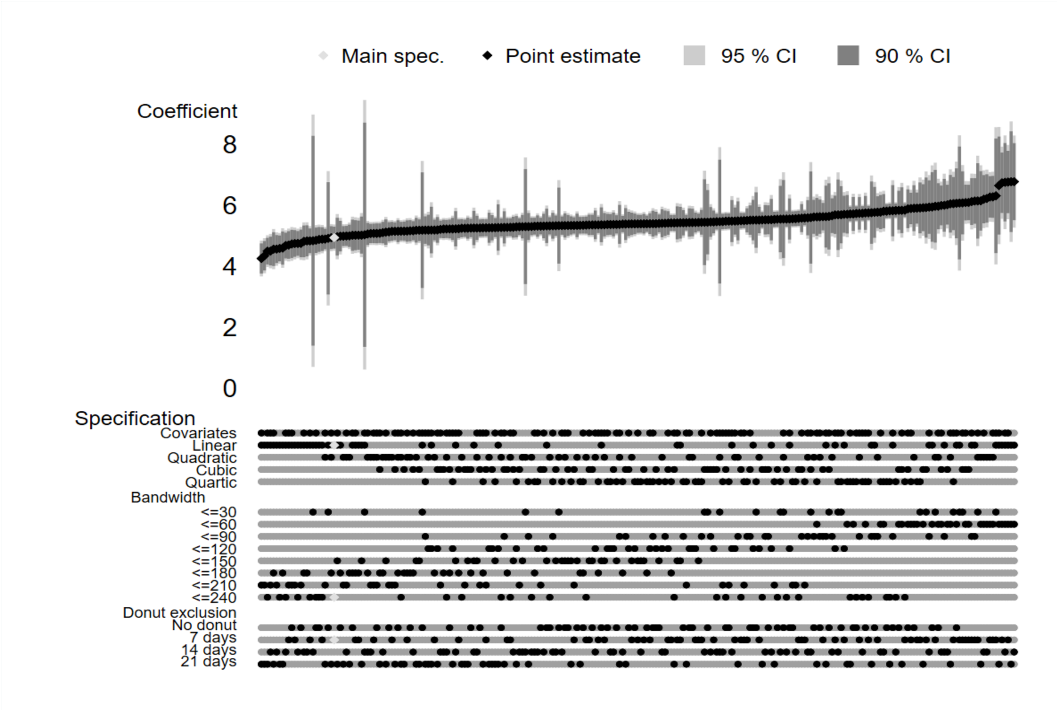
TABLE E2: Consumption Externalities

	Excl. children born in 2002			Interaction w. same municipal		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	1st stage	ITT	2SLS	1st stage	ITT	2SLS
Reform/peer effect	6.421***	1.215**	0.189**	6.643***	1.375**	0.207**
	(0.757)	(0.596)	(0.0923)	(0.743)	(0.582)	(0.0876)
Reform X				0.785	-0.102	-0.264
Living in the same municipal				(0.552)	(0.419)	(0.435)
Living in the same municipal				0.199	0.770*	0.729*
				(0.594)	(0.425)	(0.399)
Observations	2,848	2,848	2,848	3,154	3,154	3,154
R-squared	0.168	0.065	0.073	0.172	0.065	0.069
Controls						
Peer covariates	YES	YES	YES	YES	YES	YES
Own covariates	YES	YES	YES	YES	YES	YES
Time trend	YES	YES	YES	YES	YES	YES

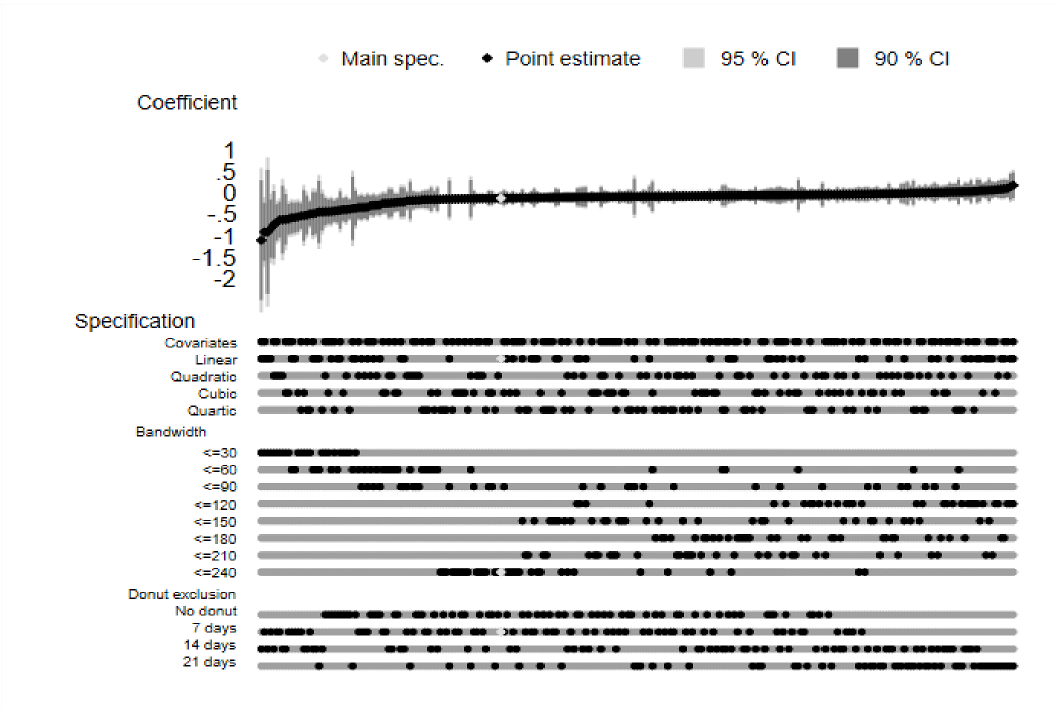
All specifications include the running variable (d_i, date of birth) and the running variable interacted with an indicator for whether childbirth occurred before or after cut-off.
Standard errors in parentheses are clustered on date of birth of peer child
*** p<0.01, ** p<0.05, * p<0.1

Appendix F: Robustness

Estimates of reform effect on mothers' leave behavior

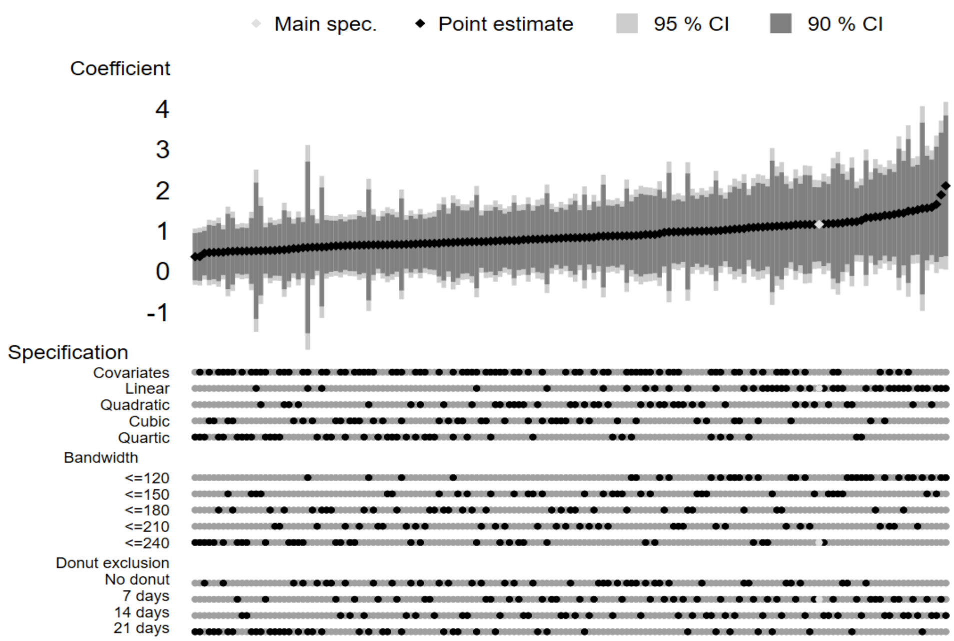


Estimates of reform effect on fathers' leave behavior



Notes: The figure shows estimates of the reform effect when varying (i) whether or not to include covariates, (ii) the shape of the running variable, (iii) varying bandwidth, and (iv) and excluded days around cut-off. The shaded 95 and 90 percent confidence intervals are based on standard errors clustered on date of birth. All specifications include the running variable (d_i) and the running variable interacted with an indicator for whether childbirth was before or after cut-off. For each plotted coefficient, the dark dots below indicate the corresponding specification.

Estimates of peer effects



Notes: The figure shows estimates of the reform effect when varying (i) whether or not to include covariates, (ii) the shape of the running variable, (iii) varying bandwidth, and (iv) and excluded days around cut-off. The shaded 95 and 90 percent confidence intervals are based on standard errors clustered on date of birth. All specifications include the running variable (d_i) and the running variable interacted with an indicator for whether childbirth was before or after cut-off. For each plotted coefficient, the dark dots below indicate the corresponding specification.