Gender Norms and Specialization

in Household Production:

Evidence from a Danish Parental Leave Reform[*]

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Abstract

The arrival of children implies a sharp reduction in mothers' earnings and labor supply while fathers' labor market trajectories are unaffected. To understand this specialization, I exploit a Danish parental leave reform. Upon reform implementation, mothers increase their leave by 5 weeks while the average leave duration of fathers remains unchanged, irrespectively of relative earnings. Consistent with the role of gender identity, women who had a working mother take a shorter leave than those with a stay-at-home mother. Moreover, I document peer effects among sisters who take a longer leave if exposed to the reform-induced change in leave duration.

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); Ejrnaes & Kunze (2013)). While this divergence is well documented, it is poorly understood. As standard economic incentives appear to play a muted role in time-allocation upon parenthood (Kleven et al. (2019); Daly & Groes (2017)), another potential explanation is that of gender identity. This concept has been used to explain women's labor supply (e.g. Fernandez, Fogli, & Olivetti (2004); Fernandez & Fogli (2009); Olivetti, Patacchini, & Zenou (2020)) but less attention has been paid to the role gender identity plays upon parenthood. To improve the understanding of the striking gender gaps following parenthood, I investigate which factors that influence time allocation within the household, in particular the use of parental leave.

To understand this divergence, I exploit an unexpected and rapid institutional change in Denmark. In 2002, a parental leave reform improved the economic compensation new parents received while on leave, effectively increasing the leave duration. At the same time, policy makers 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. 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.

By implementing a Regression Discontinuity Design around the introduction of the reform, I investigate which factors influence households respond when they are given the opportunity to take an extended leave. Before turning to the empirical exercise, I briefly present a Becker (1981)-model on division on labor. Then, I turn to the role of identity and prescriptions as formulated by Akerlof & Kranton (2000). A set of testable predictions come out of this exercise and they are taken to the data. I find that mothers increase their leave by 5 weeks while the average leave duration of fathers is unchanged. Contrary to what a Becker (1981)-model predicts, estimates are largely unaffected by relative earnings in the household. Instead, the results are highly consistent with the notion of gender identity. 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 a 1.1 weeks longer

leave compared to those with a sister in the reform control group. This corresponds to peer effects of 17 %. Importantly, all these women give birth at least 9 months after the reform was introduced and face identical institutional settings. The reform induces a change in behavior among mothers, and I argue that the extended leave duration creates new prescriptions associated with motherhood. These new prescriptions are transmitted via sisters and show up here as peer effects.

While both the reform effects and the peer effects are highly consistent with the notion of gender identity, it is possible to imagine other factors that could drive parts of these results. Such factors include biology, information transmission, and consumption externalities and I discuss each of these factors in detail below. While biology - including breastfeeding or unobservable differences across men and women - could drive the overall reform effect, it is difficult to see how this explains the heterogeneity by maternal labor supply and the 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 identity is the only explanation useful for understanding both the reform and peer effects

This paper contributes to two strands of the literature. First and foremost, it contributes to the literature on gender inequality in the labor market.[1] In particular, this paper contributes to the literature on the well-established divergence in labor market outcomes upon parenthood (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)). To elevate some of the costs associated with motherhood, most developed countries have some maternity leave and a large literature investigates the causal impact of these policies (see Olivetti & Petrongolo (2017) and Rossin-Slater (2018) for reviews). Recently, the effectiveness of family policies for closing gender gaps in the labor market has been put into question (e.g. Canaan (2019); Kleven et al. (2020); Ginja et al. (2020)). I link 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); Morrill & Morrill (2013); Finseraas & Kotsadam (2017)).[2] The primary contribution of this paper is to show that when given the opportunity to take a longer parental leave, households behave in a way highly consistent with a model of gender identity.

Second, this paper contributes to the literature on peer effects. The literature on peer effects in labor market choices and gender goes back to Neumark & Postlewaite (1998). They show that labor market choices of women spur similar choices by close peers regardless of

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[1] An enormous literature has focused on gender differences in participation rates, education, and occupation choices (see reviews by for example Bertrand (2011), Goldin (2014), Blau & Kahn (2007); Blau & Kahn (2017)).

[2] While using family environment has been a prominent approach, another influential strategy to disentangle the role of gender norms from standard economic incentives has been to use 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 (Jessen (2021); Lippmann, Georgieff, & Senik (2020); Boelmann, Raute, & Schönberg (2020); Beblo & Görges (2018)) or changes of economic incentives (Ichino, Olsson, Petrongolo, & Thoursie, 2019) to estimate effects from gender norms to female labor supply.

income effects. More recently, Nicoletti et al. (2018), and Olivetti et al. (2020) show peer effects on female labor force participation.[3] To circumvent threats to identification, quasi-experiments that only influence peer behavior are commonly used (e.g. Angrist & Lang (2004); Kling et al. (2007); Brown & Laschever (2012); Fadlon & Nielsen (2019); Altmejd et al. (2021)). Parental leave reforms often provide an ideal setting for such an approach, but only two other studies investigate peer effects. They find, similar to me, that leave duration causally affects peers' leave duration. Dahl, Løken, & Mogstad (2014) use the implementation of the "daddy quota" in Norway, and document peer effects on brothers and male co-workers. Welteke & Wrohlich (2019) find peer effects among female co-workers in Germany after prolonged maternity leave. This paper document peer effects following an extension of parental leave, rather than paternity or maternity leave. This is important as mothers remain the primary users of family policies. 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. More general, my study also highlights how policies can interact with existing norms and prescriptions. This is important when considering the design and implementation of various programs.

Finally, this paper adds to a highly relevant policy area and can inform the design of parental leave policies which is receiving attention in the US (The White House, 2021) and the EU (Council of European Union, 2019). While many countries have supplemented maternity leave with 'gender-neutral' parental leave-schemes, mothers remain the primary users of leave (Olivetti & Petrongolo, 2017). In addition, some countries have implemented earmarked leave for fathers, also known as "daddy quotas".[4] To the best of my knowledge, Denmark is the only country that has *removed* a "daddy quota" and this paper is the first to evaluate how households respond to this.[5] If the aim is to increase fathers' share of leave, the findings here suggest that general extensions will not achieve this. To achieve greater gender equality, an explicitly targeting of fathers might be needed.

The structure of the paper is as follows. In Section 2, the relevant theories for intra-household specialization are presented along with predictions for the empirical investigation. Section 3 contains a presentation of the 2002-reform, the data set, and the empirical strategy. Graphical and regression-based results are reported in Section 4 together with robustness checks. Section 5 concludes.

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[3]More broadly, this paper also contributes to a large literature on spillover effects within families (e.g. Bingley, Lundborg, & Lyk-Jensen (2019); Daysal, Simonsen, Trandafir, & Breining (2020); Lindquist, Sol, & van Praag (2015); Brenøe (2021); Cools & Patacchini (2019); Black et al. (2020); Dahl, Rooth, & Stenberg (2020); Altonji, Cattan, & Ware (2017); Lundborg, Nilsson, & Rooth (2014)).

[4]While the use of these policies has been gradual (Dahl et al. (2014); Ma, Andersson, Duvander, & Evertsson (2019)), 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)).

[5]Other studies have utilized this reform for identification. They have been concerned with maternal and child health (Beuchert et al., 2016), firm performance (Gallen, 2019), and women's income (Andersen (2018); Tô (2018)), and disregarded fathers' leave behavior.

2 Household Behavior upon Parenthood

Before turning to the empirical investigation of households' responses to the reform, I outline two sets of theoretical framework that will guide the empirical investigation. I start with the standard Becker (1981)-model on division of labor. Then, I turn to the framework on identity developed by Akerlof & Kranton (2000). I arrive at two sets of testable predictions regarding the role of standard economic incentives and gender identity, respectively.

2.1 Financial Incentives and Comparative Advantages

In Becker's influential model of the household, intra-household specialization is determined by members' comparative advantages. Each member of the household can allocate time to each of the two sectors, the labor market and the home. Members are initially identical except for differences in human capital levels, broadly defined to include formal education, experience in both the labor market and with household specific tasks.[6] Members of the household corporate to maximize joint production and specialize according to their comparative advantages.

If women on average have invested more heavily in human capital relevant for home production and men have invested more heavily in human capital relevant for market production, women should on average specialize in home production and men in market production. On an aggregate level, this provides a compelling explanation for division of labor within families and why only women's earnings are affected by parenthood. However, in Denmark, as in most high and middle-income countries, there has been a rise in the educational level of women (Goldin, Katz, & Kuziemko (2006); Kleven & Landais (2017); Houlberg & Larsen (2011)), and today young women are on average better educated than young men. In couples where the woman has the highest earnings, productivity of the household could benefit from the man allocating more time to home production.

2.2 Gender Identity and Prescriptions

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). To understand this, I turn to the framework developed by Akerlof & Kranton (2000). In this framework, identity pay-off is derived from belonging to a social category, and for each category, a set of prescriptions is in place determining what is considered appropriate behavior. Akerlof & Kranton (2000) show that

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[6]I disregard any argument related to biological advantages, but expand on this in Section 4.4 Alternative Explanations

incorporation of identity and preferences for conforming to group behavior into a utility function yields equilibrium outcomes that are very different from what standard theory would otherwise predict. In this framework, prescriptions are defined locally as the average behavior among relevant peers such as school-mates (Akerlof & Kranton, 2002) and co-workers (Akerlof & Kranton, 2005). If relevant peers change their behavior, so does the optimal behavior of the individual.

Gender is a social category with great importance for individual choices. Recently, 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 result in gender identity. To improve on the understanding of 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 outcome and as rationale for various social arrangements and as a means of legitimating one of the most fundamental divisions of society" (Ibid., p. 126). 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. In this context, prescriptions are those sets of behavioral norms expecting mothers to engage in care work and unpaid labor, while fathers are met with other expectations.[7] When an individual does not comply, they incur a cost (West & Zimmerman (1987); Akerlof & Kranton (2000)).[8] By tradition, women have been given the vast responsibility for child-rearing and home production, and thus men and women face very different prescriptions upon parenthood. To comply women allocate extensive time to home production and notions of the male breadwinner induce men to allocate less time to home production. In the framework outlined by Akerlof & Kranton (2000), the pay-off from identity and prescriptions implies that conformity is utility maximizing.[9]

Adding to the complexity, there might be substantial differences in prescriptions within gender categories. Complementing societal wide gender roles and stereotypes, the family has been highlighted as an important site for the formation of gender identity to take place and prescriptions are defined (see review by Bau & Fernández (2021)). A growing amount of evidence shows that parents' attitudes and behavior are transmitted to their children. Importantly, having a working mother increases own labor supply (e.g. Fernandez et al. (2004); Fernandez & Fogli (2009); Morrill & Morrill (2013); Finseraas & Kotsadam (2017); Olivetti et al. (2020)) and is associated with a smaller child-penalty (Kleven et al., 2019).

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[7]This is in the line with the influential definition of norms as empirical and normative expectations by Bicchieri (2005): an individual prefers to comply with a norm under the conditions that a) they believe a sufficiently large part of the population behaves in a certain way *and* b) they believe that they are expected to behave in the same way.

[8]Bicchieri (2005) allows for sanctions but does not require it.

[9]A large and growing literature shows 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))

Women who had a working mother might hold a different set of prescriptions related to motherhood compared to those who had a stay-at-home mother. In turn, their optimal level of time allocated to child-rearing might be systematically different, regardless of standard economic incentives. Moreover, prescriptions are not static. If relevant peers (e.g. friends, co-workers, or siblings) change their behavior, the optimal behavior for the individual also changes.

2.3   Predictions

Based on two frameworks, two different sets of predictions can be outlined. In a setting with improved opportunities for parental leave, a standard Becker model predicts that the parent with a comparative advantage in the household takes the longer leave. Mothers who have an advantage in the market should respond less to a reform that allows for a longer leave compared to those who have an advantage in the home. As I cannot observe comparative advantages, I use relative earnings. Mothers who were primary earners prior to childbirth are expected to respond less to the reform than those who were not primary earners. Equivalently, fathers who are out-earned by their partner are expected to respond stronger to the reform than fathers who are primary earners.

However, if pay-off from gender identity and prescriptions determine time allocation, mothers would be the primary users of the extended leave, regardless of standard economic incentives. Instead, prescriptions for mothers and fathers influence the leave behavior. If prescriptions dictate that mothers should allocate more time to child-rearing than fathers, a large reform effect among mothers is expected. Fathers are not expected to use the opportunity to increase leave duration. In order to explore heterogeneity by gender identity, I use data on maternal labor supply of mothers (i.e. the grandmothers of the child) in the reform window. In line with the literature of inter-generational transmission of gender identity, those who had a mother with a high labor supply should respond less to the reform.

Subsequently, women who observe their sister taking a long leave - induced by the reform - observe a new set of prescriptions. Women with sisters in the control group observe their sister taking a relative short leave. These two groups of women are exposed to different prescriptions, and in turn, expected to behave differently even though they face the same institutional set-up. If the reform induced prescriptions of extended maternity leave, those with a sister in the reform treatment group experience these prescriptions of extended leave via their sister, while those in the control group do not. This should show up as peer effects in the empirical investigation.

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.
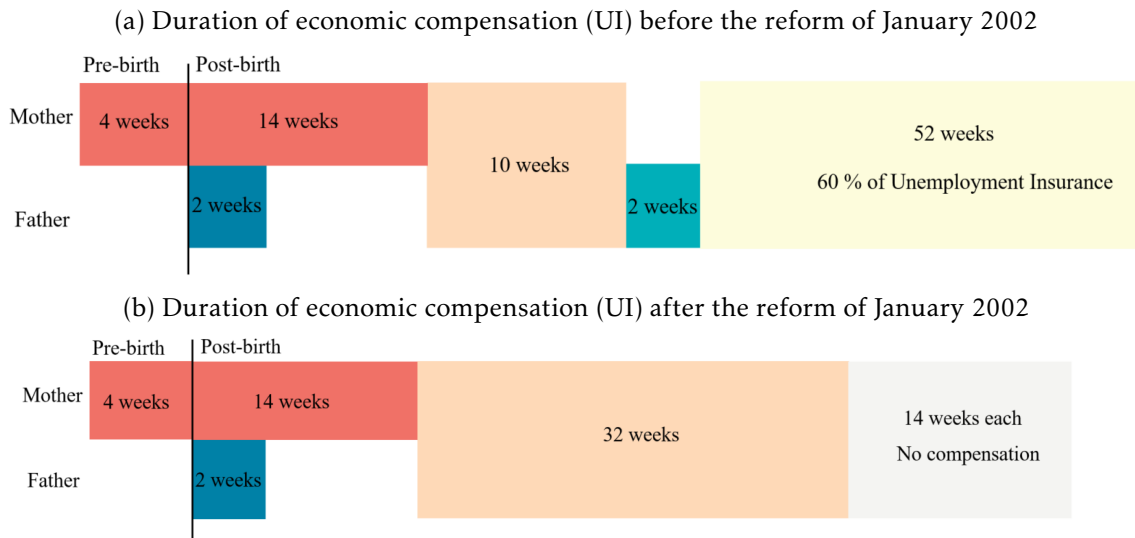
3.1   Institutional Context

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. In contrast to all other Nordic countries, where some version of a "daddy quota" is in place, Danish policy makers have refrained from implementing such policies and argued that parents – not the government – should decide the distribution of leave (Deding & Holt, 2012).

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. This change implied better economic compensation from week 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 after week 24 with compensation substantially lower than their labor market earnings.[10] Substantially flexibility

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[10]The public sector has a longer history of generous leave schemes than the private sector. At the time of the reform, women in the public sector received full salary for 14 weeks after giving birth and then up 10 weeks which could also be transferred to the father if he also worked in the public sector. This provides women with

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 the 7th of January 2002 and adopted on the 27th of March 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 average leave duration of mothers at 1st of January 2002 and no change in the average leave duration of fathers. On 27th of March, 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.[11] 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 gov-

up to 24 weeks of fully paid leave. After the period of paid leave from the employer, new parents would be on benefits equivalent to unemployment insurance.

[11] Beuchert et al. (2016) investigate health effects on mothers and children from the increase in leave duration of mothers. Nielsen (2009) and Tô (2018) also show substantial change in leave behavior among mothers at 1st of Jan 2002.

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ernment 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 information on all parents who had a child between March 2001 and December 2005.[12] 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 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 the Appendix B. I only include the first child of a parent who had multiple

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[12]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.

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 the 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 1st of October 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
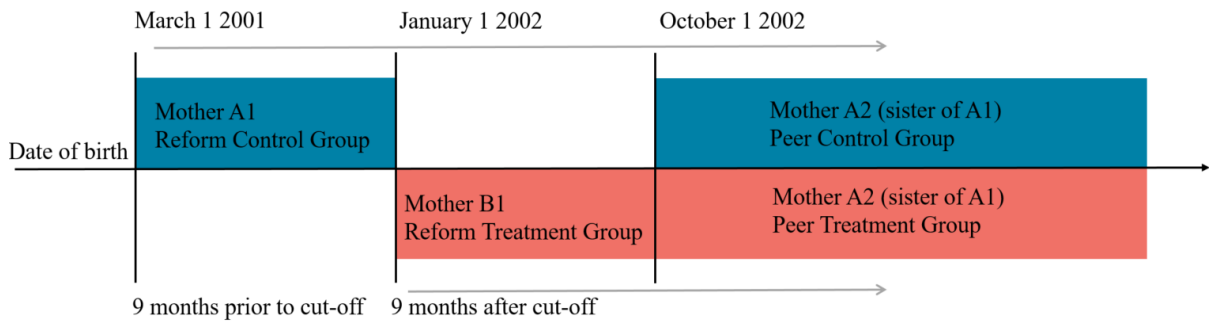
When the aim is to identify peer effects in naturally occurring peer groups, it raises a 'many-to-one' issue as multiple peers can affect the same individual. This problem arises if more than one peer became a parent around the reform date, particularly if there is a peer before implementation of the 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 cut-off. This is normally a sign of manipulation into treatment. However, 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 is 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.[13] The final sample

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[13]In '4.4 Robustness', I show that whether or not I include observations close to cut-off or I have larger 'donut',

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.

Figure 3: Pre-determined covariates, Mothers in the reform window
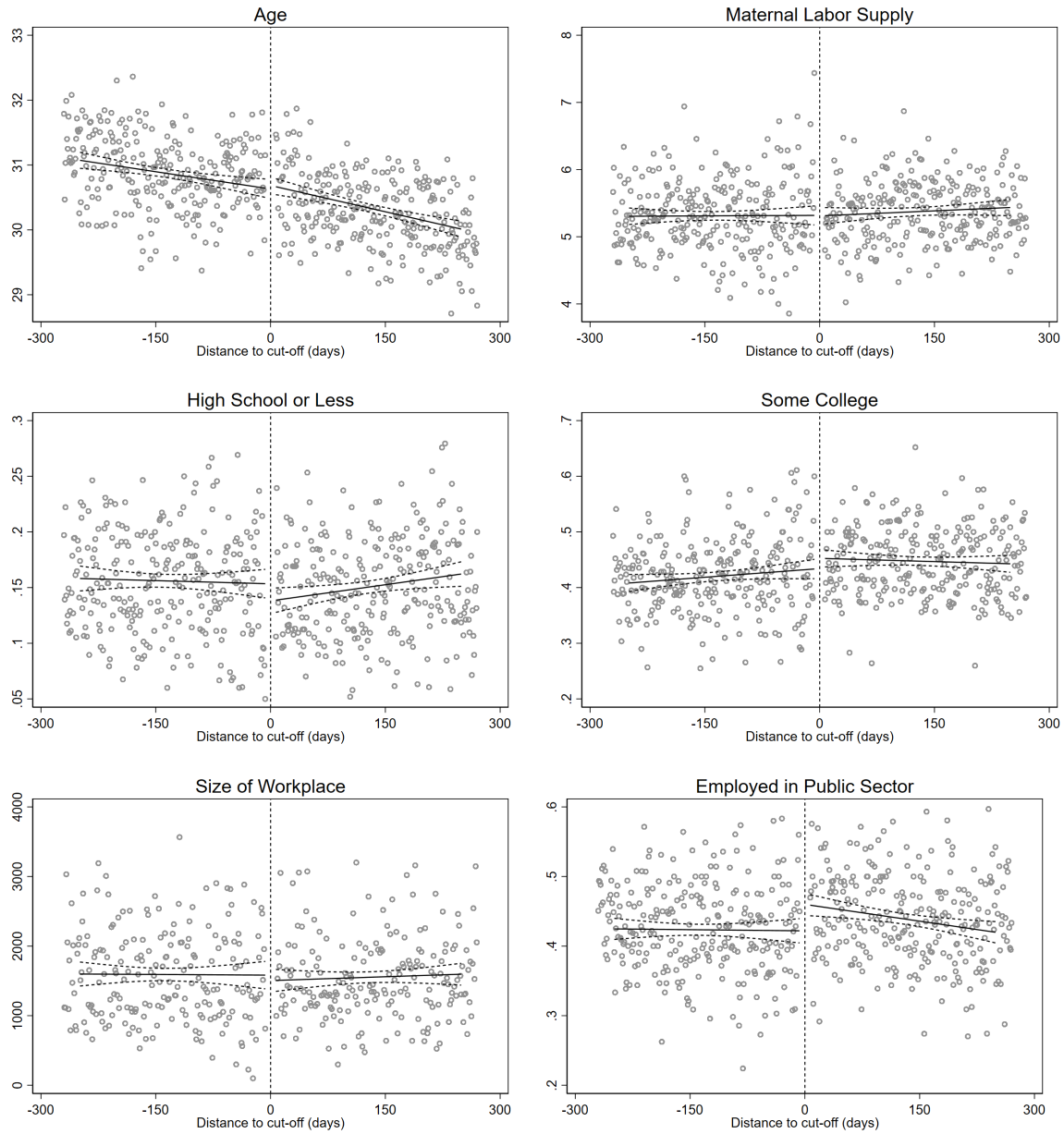
Notes: The figures show value of key covariates of mothers' in the reform window: age, her mothers' labor supply, a dummy indicating high school or less education, a dummy indicating some higher education, size of her workplace and a dummy indicating public sector employment. The running variable is 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.

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

point estimates are extremely stable, providing reassurance that manipulation didn't take place.

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.

Figure 4 show the discontinuity in average leave duration at reform implementation for mothers and fathers, respectively. Figure 4a shows that mothers increase their leave with about 5 weeks at reform implementation, while Figure 4b 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 4: Leave Duration around the Reform Window

(a) Mothers' Leave (weeks)　　　　　　　(b) Fathers' Leave (weeks)
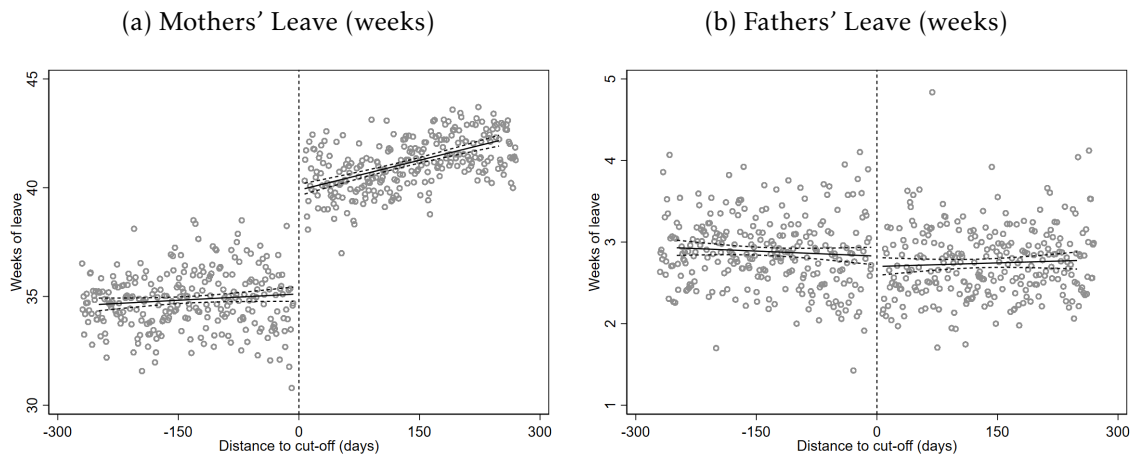
Notes: The figures show average leave duration measured in weeks of mothers and fathers with children born in the reform window. This measure does not include leave taken prior to child-birth. The running variable is date of child-birth of own child. Cut-off is 1st of Jan 2002 and the window on each 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 5 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

Figure 5: Histogram of Reform Effect

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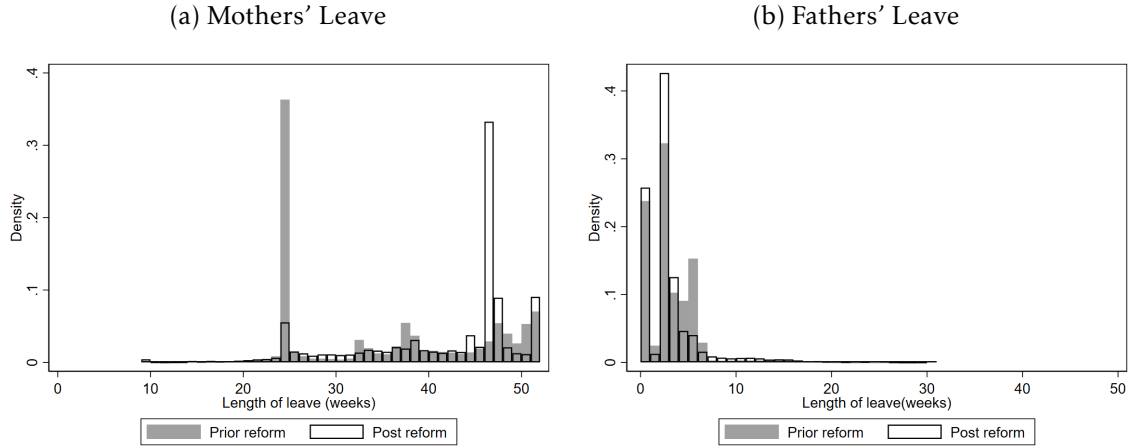
(b) Fathers' Leave

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.

the new maximum duration of compensated leave.[14] 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 4b showing no reform effect on fathers' leave duration hides substantial heterogeneity. This will have consequences for the empirical strategy.

Finally, I document that there is 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 6 contains the same variables as Figure 3. Unsurprisingly, there is no difference across treatment and control group.

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[14]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.

Figure 6: Pre-determined covariates, Mothers with sisters in the reform window
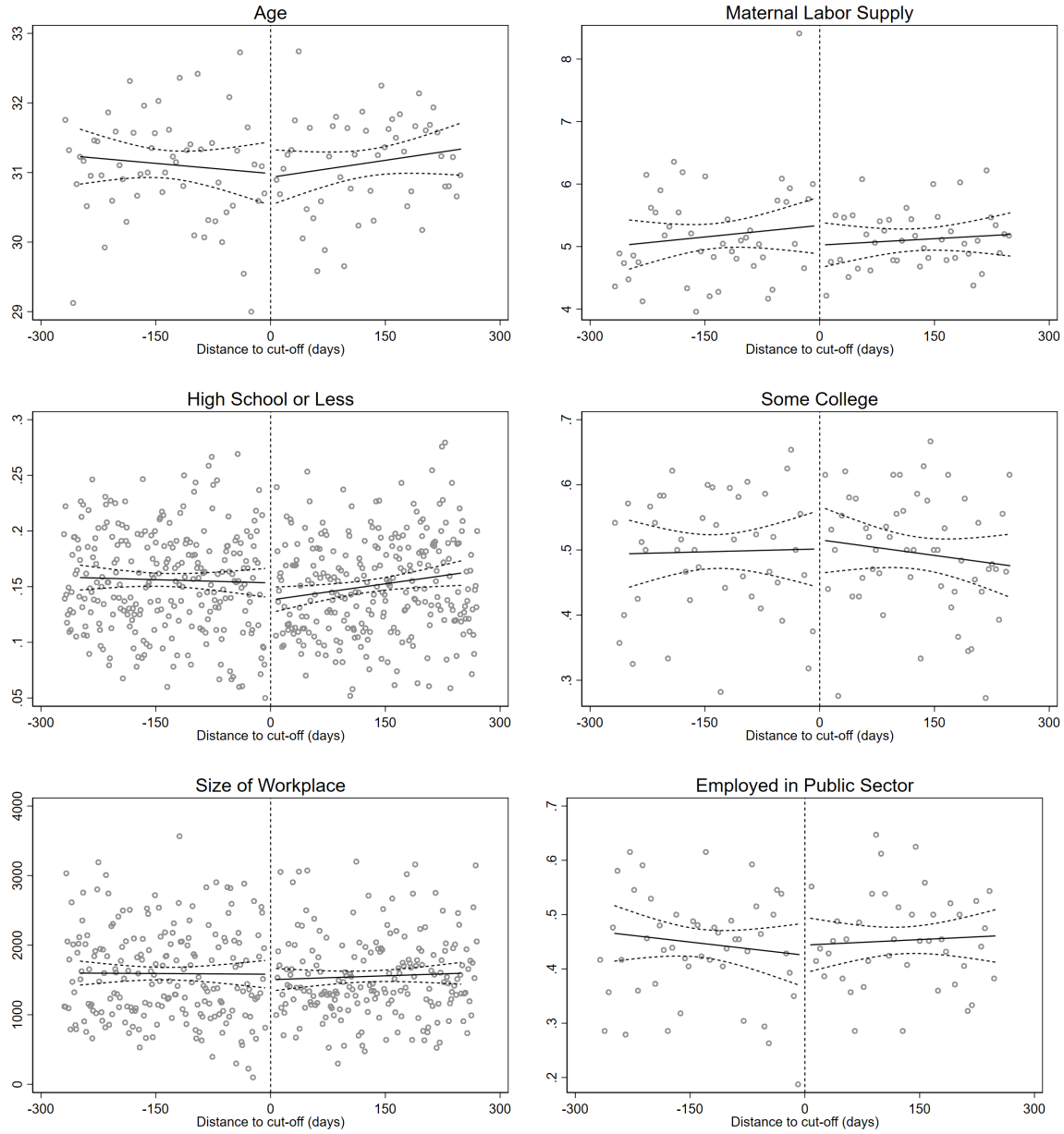
Notes: The figures show value of key covariates of mothers' with a sister in the reform window: age, her mothers' labor supply, a dummy indicating high school or less education, a dummy indicating some higher education, size of her workplace and a dummy indicating public sector employment. The running 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.

3.3 Empirical Strategy

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 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.

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

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 an 2SLS-estimator following the work by Dahl et al. (2014). The first-stage is equivalent to Equation 2:

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

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 5a, 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 5b 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. Therefore, 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.[15] In this specification, the monotonicity as-

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

sumption is met for fathers.

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 day care 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 6 suggest that this is indeed the case.[16] 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 peer mother exposed to 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 the monotonicity assumption is violated so 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.

4   Results

4.1   Graphical Results

An RD-design provides a transparent and illustrative way of visualizing identification of the treatment effects (Thistlethwaite & Campbell (1960); Imbens & Lemieux (2008)). The

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[16]A related concern is that some sisters coordinate fertility. Women in the reform treatment group and their sisters have children with closer spacing that 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.

graphical results reported in this section are without individuals level controls. Figure 7 shows the average leave duration of the full population in the reform-window among mothers and fathers, split by relative earnings in the household. Theory of specialization predicts that mothers who are primary earners should respond less to the reform than mothers who are not primary earners. On the contrary, the reform affects the two groups similarly with a jump of 5 weeks. There is a difference in the initial duration of mothers' leave. In households where the mother earns less than the father, she takes 2 weeks longer leave compared to households where the mother earns the most. 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. Again, a difference in leave duration across the two types of households remains largely unchanged. Both before and after the reform, fathers who are not primary earners take 1 week longer leave compared to fathers who are primary earners.

Figure 7: 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
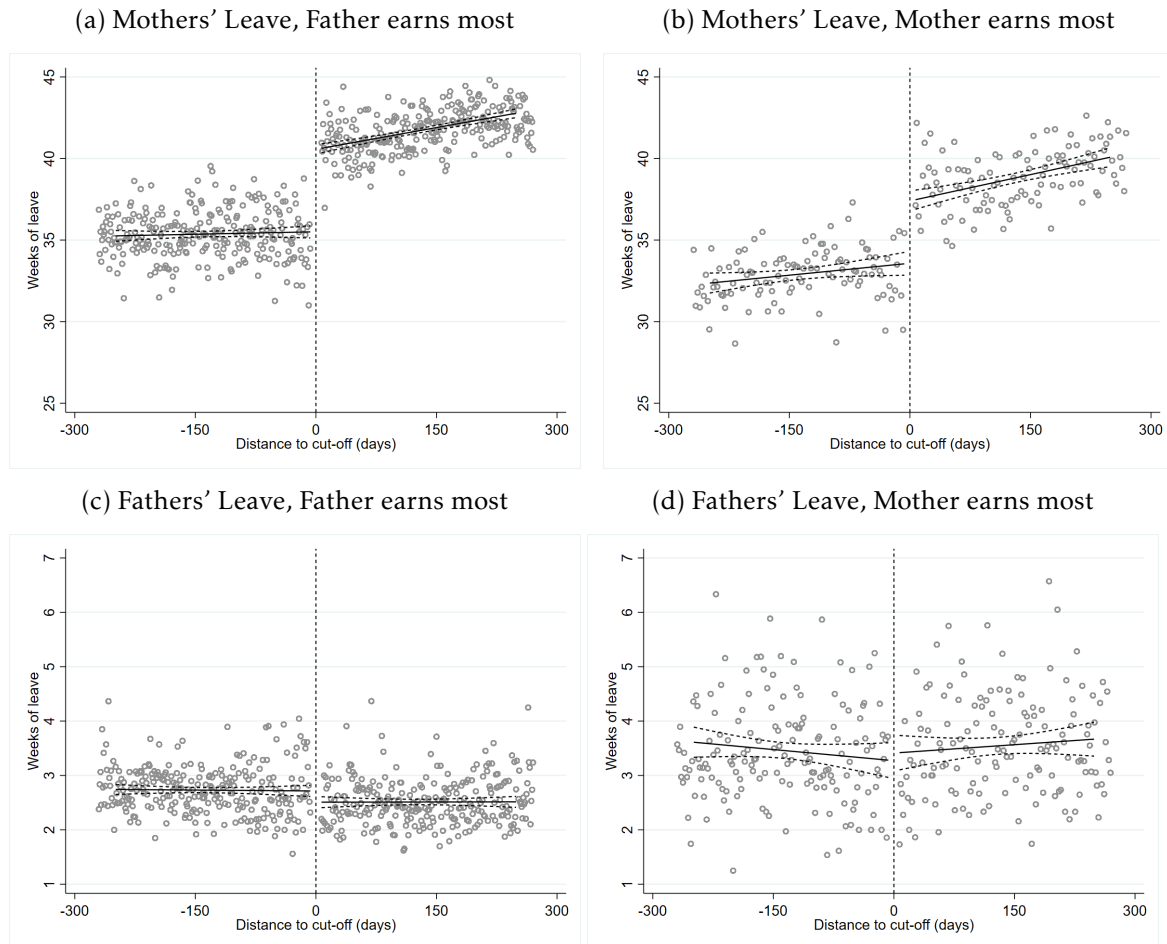
Notes: The figure shows average leave duration measured in weeks of mothers (top panel) and fathers (bottom panel) stratified by relative earnings in the household in the year prior to childbirth. The measure of leave does not include leave taken prior to child-birth. The running variable is date of 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. Each bin include 50 observations and kernels are uniforms. Using a quadratic fit barely makes a difference.

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Figure 8 shows the average leave duration around reform introduction and the subsequent peer effects for sister-pairs. The reform window in Figure 8a 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 8b 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 8: Reform and Peer Effect, Sister-pairs

(a) Reform Effect   (b) Peer Effect
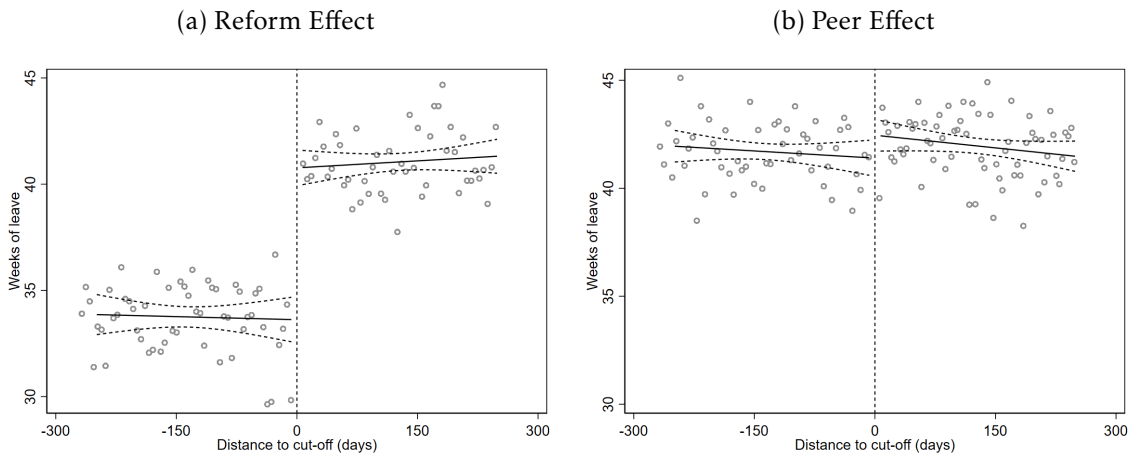
Notes: The figure shows average leave duration measured in weeks of sets of sisters. On the right side, leave duration of the sister in the reform window is reported. On the left side, average leave duration of the sisters who themselves give birth between 1st of October 2002 and end of 2005 is reported. The measure of leave does not include leave taken prior to child-birth. The running variable is date of child-birth 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

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 4, 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 being 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. 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 with 0.2 week upon the reform. Adding the reform effect and the interaction term together show that fathers who were not primary earners did not change their leave duration.[17]

Column (5) and (6) present an alternative specification of the reform effects on fathers to shed light upon 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 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 theory of specialization are not matched by the data.

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).

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[17]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

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	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 5b.

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. Before the reform, there was no difference in leave duration across those who had a mother with a high or low labor supply. However, with the reform, those who had a mother with a low labor supply increase their leave by approx. half a week more than those with a mother with a high labor supply.

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 sector and child parity is greatly reduced with the reform implying that new mothers start to behave more similar. While there is a meaningful difference in the reform response across mothers with high and low maternal labor supply, the magnitude is only 1/6 of the difference across the private and public sectors prior to the reform. The more homogenous 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.

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

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 8b, 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. Column (4)-(10) contain the reduced form model with interactions 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.

Women who where exposed to low maternal labor supply in childhood take a longer leave prior to the reform. However, the reform-induced change in prescriptions strongly reduces inter-generational effects from maternal labor supply and the peer effect is larger for those exposed to high maternal labor supply. Mothers working in the private sector took a shorter leave compared to those working in the public sector irrespective of when their sister had a child. However, this difference across sectorial occupation is reduced if the sister was in the reform treatment group. This is driven by mothers in the private sector responding stronger upon their sister taking a long leave. This mirrors the investigation of heterogeneous effects across women in the reform window; with the reform and peer effects, variance in leave duration is reduced, and this is driven by larger responses among mothers with characteristics indicating that they would have taken a short leave in the absence of treatment.

Combined, the results from both the investigation of the reform and peer effects are highly consistent with the notion of gender identity. Relative earnings do not offer a meaningful explanation of the reform effect, as the reform implied that most new mothers increased their leave duration with approximately 5 weeks. The differences in behavior across mothers and fathers can be explained by differences in prescriptions. Different prescriptions facing mothers and fathers determine the distribution of leave within the household and drives the estimate of the reform effect. The mothers who had a mother with a high labor supply respond less. This is consistent with the notion of gender identity being transmitted across generations.

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 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 Low maternal labor supply				-0.524						
				(0.536)						
Low maternal labor supply				0.766*						
				(0.392)						
Peer effect X Publicly employed					0.982***					
					(0.413)					
Publicly employed					1.446**					
					(0.429)					
Peer effect X Sister publicly employed						0.360				
						(0.572)				
Sister publicly employed						0.253				
						(0.421)				
Peer effect X Mother primary earner							-0.119			
							(0.627)			
Mother primary earner							-0.910			
							(0.627)			
Peer effect X Sister primary earner								-0.498		
								(0.702)		
Sister primary earner								0.152		
								(0.600)		
Peer effect X First-time mother									0.386	
									(0.551)	
First-time mother									0.146	
									(0.314)	
Reform X Sister first-time mother										-0.450
										(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 with 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 provide a compelling explanation of both the reform and peer effect.

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.[18] 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 (2021) show striking similarities in the division, length, and 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

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

(2016); Nix & Andresen (2019); 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 Nix & Andresen (2019) 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.

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 the 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 5a. If 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. Evaluating the same reform as me, Tô (2018) shows that a relative long leave negatively impacts the earnings of the mothers. This is in line with the literature showing that leave duration beyond 6-7 months hurts women's earnings (Ruhm (1998); Olivetti & Petrongolo (2017); Kyriacou, Rey, & Silva (2021)). 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.

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 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. Estimated reform and peer effects from the preferred specification are very robust to standard checks. 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, precision decreases when bandwidth is set to 30. This is illustrated in Figure 9. The average reform effect that comes out of this exercise is 5.3 weeks.[19] Out of the 249 regressions, all estimates but two are significant on a 0.99 pct. level. The average t-statistics is 20.396. A similar set of robustness checks are made for the reduced form estimate of the peer effects. This is reported in Figure 10. Again, the point estimate is fairly stable. However, as the sample size is much smaller for peer effects than for the reform effect, precision decreases, especially when individual level controls are dropped. Having a bandwidth below 120 is not feasible as the number of observations drops too much. After running these 161 regression, the average reduced form effect is 0.87 weeks with an average t statistics of 1.58.

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[19]Performing the equivalent exercise for the reform effect of the fathers yield an average reform effect of -0.13 weeks which is far from being significant at conventional levels.

Figure 9: Estimates of reform effect on mothers' leave behavior
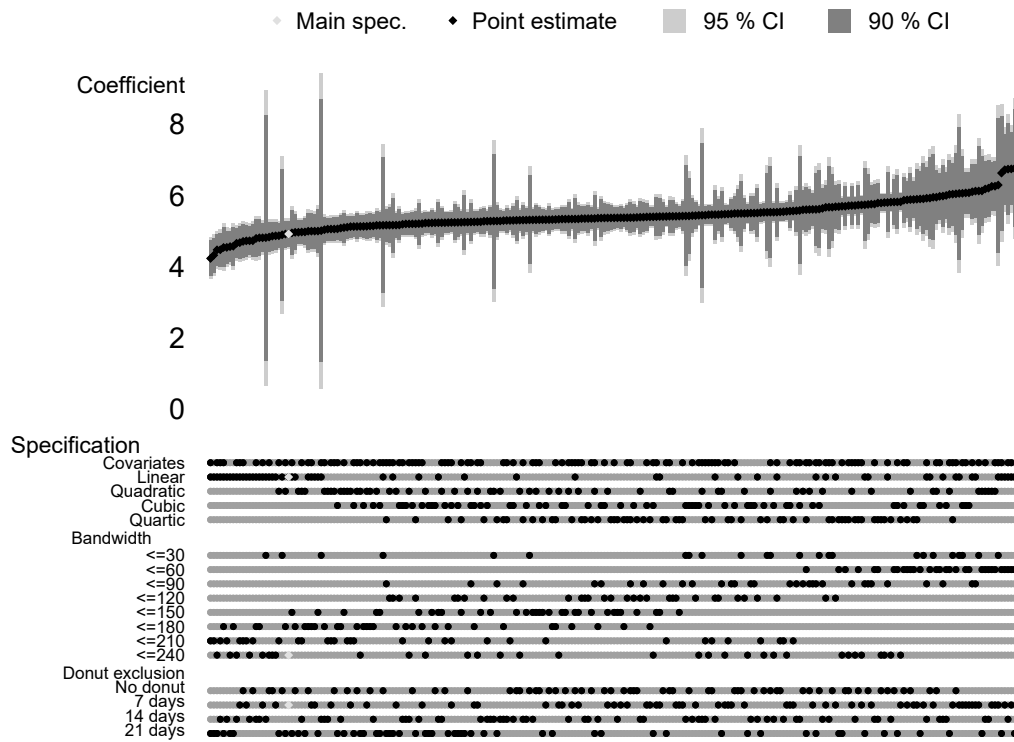
Notes: The figure shows estimates of the reform effect when varying (i) whether or not to include covariates, (ii) the shape of the running variable, (iii) varying bandwidth and (iv) and excluded days around cut-off. The shaded 95 and 90 percent confidence intervals are based on standard errors clustered 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.

Figure 10: Estimates of peer effects, reduced form
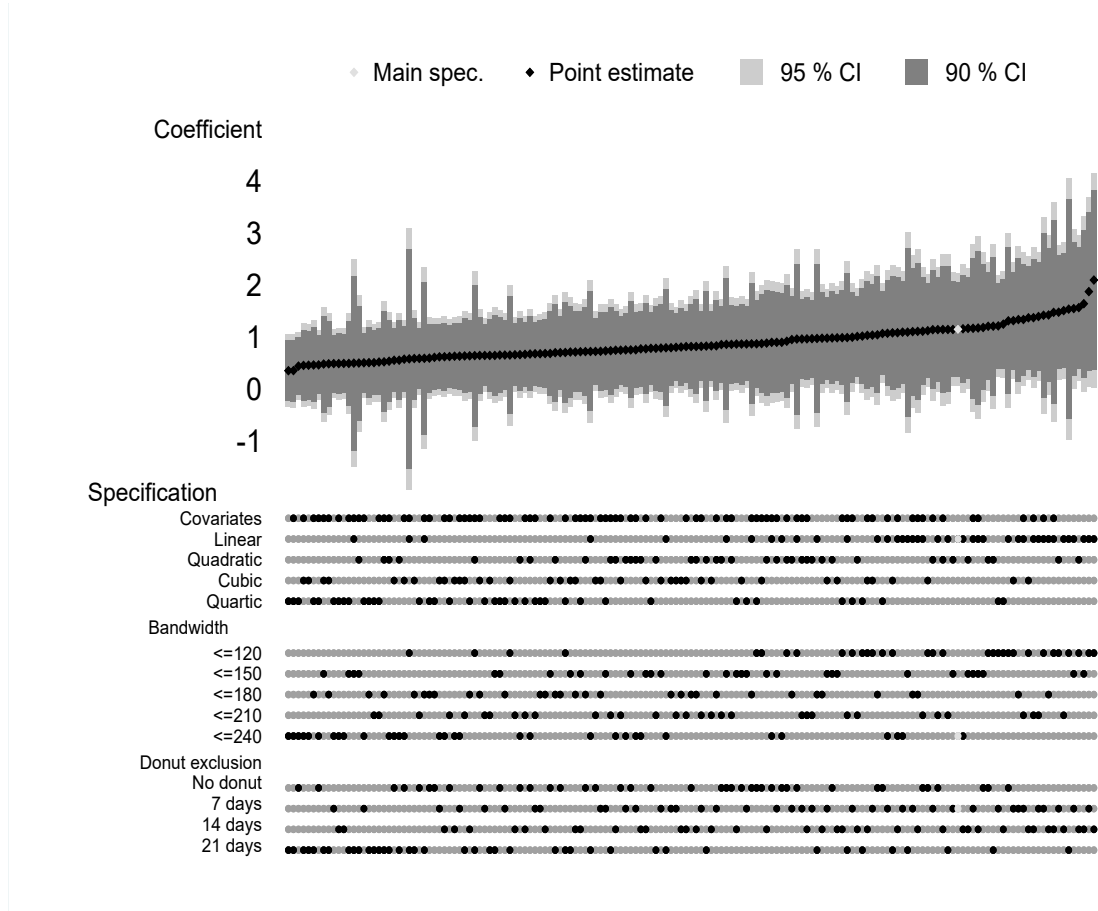
Notes: The figure shows estimates of the peer effect when varying (i) whether or not to include covariates, (ii) the shape of the running variable, (iii) varying bandwidth and (iv) and excluded days around cut-off. The shaded 95 and 90 percent confidence intervals are based on standard errors clustered 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.

5 Concluding Remarks

This paper highlights the role of gender identity and different prescriptions faced by mothers and fathers as an important factor for intra-household specialization. By using the discontinuity that arises from the parental leave reform in Denmark in 2002, an RD-design provides robust estimates of 5 weeks increase in parental leave duration among mothers. Meanwhile, the average leave duration of fathers is unchanged. These estimates barely change across relative earnings. Instead, gender identity and prescriptions affect the distribution of leave within the household. Mothers are the primary users of extended leave. In line with the role of inter-generational transmission of gender identity, those who 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 prescription of extensive leave. This shows up here as peer effects and reaffirms gender-specific intra-household specialization.

In general, many family policies may have this effect. Too long maternity leave policies have the potential to have negative effects on women's labor market outcomes (Ruhm (1998); Olivetti & Petrongolo (2017); Kyriacou et al. (2021)). Moreover, the effectiveness of family-friendly policies may be partly due to strong norms for maternal care (Kleven et al., 2020). 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. If family policies do not explicitly encourage fathers to use them, they will mainly be considered relevant for mothers and strengthen existing gender gaps in intra-household specialization.

The results in this paper highlights the role gender identity and different expectations of behavior of mothers and fathers have in determining the leave distribution. As mothers are expected to be the primary caregivers of children, Danish mothers take the vast majority of the leave that in principle could be shared with the father. The reform evaluated by Dahl et al. (2014) likely changed prescriptions regarding fathers' behavior by encouraging them to take leave and altered norms for paternal involvement (Lappegård & Kornstad, 2020). The reform in Denmark is arguably similar to the German reform evaluated by Welteke & Wrohlich (2019) in encouraging longer maternity leave and stressing the importance of staying home the year following childbirth. Women in West Germany respond stronger to the reform and peer effects are larger, consistent with West Germany having more traditional gender norms suppressing female labor supply (Boelmann et al., 2020) and increasing the size of the child penalty (Jessen, 2021). Instead of changing prescriptions regarding fathers' leave, Danish and German policies reinforced views regarding women's responsibility in childcare and home production. Both in Denmark and in Germany, families made choices aligned with existing prescriptions. This behavior is transmitted to close peers. This insight into the relationship between gender identity, prescriptions, and intra-household specialization is useful for understanding the persistence in various gender gaps.

This paper also provides new insight into empirical investigations of peer effects. As argued by Sacerdote (2014), studies of peer effects on social outcomes and labor market choices produce significant results more often than those on test scores. However, channels are rarely identified. Akerlof & Kranton (2000) highlights that prescriptions may be more influential than standard economic factors in many important choices. In empirical investigations, changes in prescriptions may show up as peer effects. The results reported here and interpretation of related studies can be explained as changes in prescriptions transmitted to close peers via social interactions.

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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

Figure 11: Drop in births at New Year
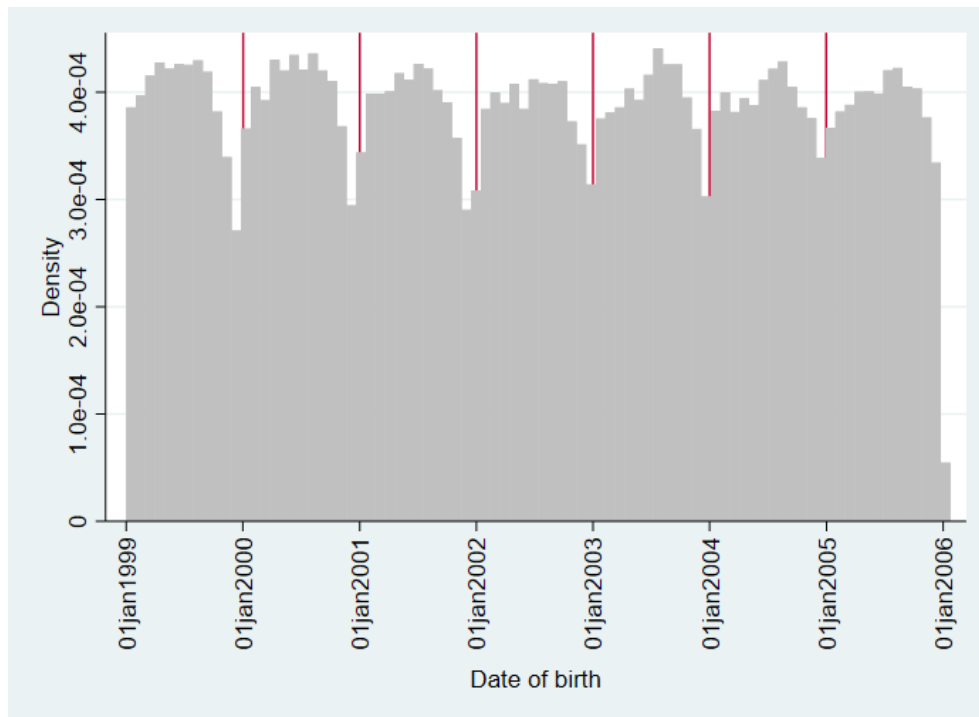
Notes: The figure shows the histogram of birth in the raw data before imposing any restrictions. Red lines mark New Year's. Every year there is a drop around the holidays.

Figure 12: Fathers' leave, sectorial occupation
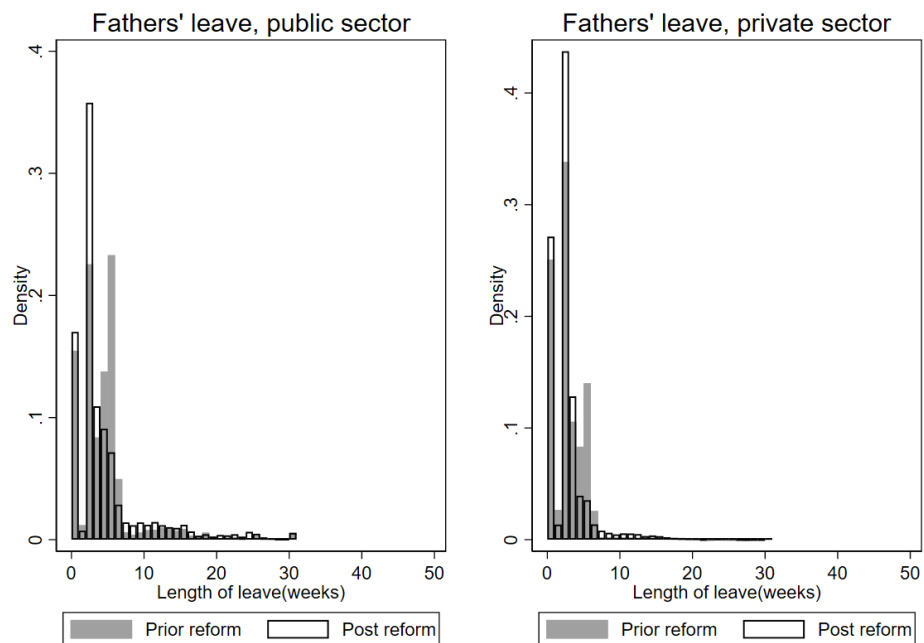
Appendix D: Regression output

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

Outcome	(1) Mothers' leave duration (weeks)	(2)	(3) Fathers' leave duration (weeks)	(4)	(5) Fathers' taking long leave (dummy) if leave ≥ 8 weeks[a]	(6)
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 Mother primary earner		-0.262		0.278***		0.0279***
		(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

VARIABLES	(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

Appendix E: Alternative Explanations

TABLE E1: Information on eligibility

| VARIABLES | Excl. those at old cut-off | | | Excl. those around old cut-off | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
	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

| VARIABLES | Excl. children born in 2002 | | | Interaction w. same municipal | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
	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
*** $p<0.01$, ** $p<0.05$, * $p<0.1$