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

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Abstract:
This paper shows that decisions regarding intra-household specializations are determined by gender norms rather than standard economic incentives. To test theoretical predictions of both the standard model of intra-household time allocation and the role of gender identity, social category and prescriptions, I use variation from a Danish parental leave reform. I find large effects among mothers and virtually unchanged behavior among fathers, irrespective of relative earnings in the household. This is consistent with the notion of pay-off from gender identity. Subsequently, I find peer effects among sisters and interpret this as reform-induced prescriptions regarding extensive leave for mothers.

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

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

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I Introduction

While the gender gaps in labor force participation and earnings have decreased over the last century, this development has stagnated over the last three decades (Blau & Kahn, 2007; 2017). Researchers have highlighted the role of gender norms as a potential explanation for the persistence in the labor market gaps (see Bertrand (2010); Giuliano (2020) for review). Additionally, recent evidence suggests that gender norms also affect intra-household specialization and time allocation to child-rearing. For example, evidence show that the size of the child penalty, the reduction in women's earnings upon motherhood, is unaffected by educational levels (Kleven, Landais & Søgaard, 2019) and relative earnings in the couple barely affect time allocation to child-rearing (Daly & Groes, 2017). Although these findings support the notion of the importance of gender norms for intra-household specialization, this is not direct evidence. This paper provides this evidence by showing that gender norms is the dominant factor when households decide which member allocates time to child-rearing.

In order to disentangle the effects from gender norms from that of standard economic explanations on various gender gaps, an improved understanding of how gender norms are constructed and enforced is needed. To this end, the work by sociologists West & Zimmerman (1987) is useful. In their view, gender is "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 inequality is then persistent and reinforced through everyday interactions and practices where individuals adapt their behavior according to gender norms. I use the concept of prescriptions to refer to behavioral norms associated with a gender category. 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. When an individual does not comply with the prescriptions of their gender category (i.e. transgress gender norms) this associated with a cost (Ibid.). In line with the argument of West & Zimmermann (1987), this paper shows that prescriptions play a crucial role when households decide on time allocation, and further, that social interactions transmit prescriptions across households.

Specifically, I show that family policies that formally allow any parent to allocate extended time to household production and child-rearing are almost exclusively used by mothers. To show this, I take advantage of a large Danish parental leave reform that was implemented in 2002. The reform improved the economic compensation during parental leave for the vast majority of new parents. At the same time, the reform removed two weeks of earmarked leave specifically allocated to fathers. In other words, the reform left the decision of how to distribute the extended parental leave to the household. Among mothers, I find a strong response to the possibility of longer leave, while fathers barely respond. These findings do not change across relative earnings in the household. This is consistent with the interpretation of different prescriptions relevant for mothers and fathers as the dominant factor in the leave decision. In further support of this interpretation, I find significant peer effects among mothers who had a sister in the reform window and had a child themselves after the reform was implemented. Those with a sister in the treatment group take a significantly longer leave than those with a sister in the control group. Combined, these findings show that the reform reinforced existing gender gaps in intra-household specialization and different prescriptions relevant for mothers and fathers are the mechanism behind this inequality in time allocation. Arguably, many family policies operate in this manner and thus enforce existing gender gaps in child-rearing and home production more generally.

The literature on gender norms primarily focuses on labor market outcomes of women with a secondary focus on household formation and fertility. Very little attention is paid to intra-household specialization. As it is difficult to disentangle the effect from norms from that of standard economic incentives, this literature has a strong focus on making causal claims. One strand of the literature has focused on the labor supply of women from earlier generations (Farré & Vella, 2014) and shown inter-generational effects on current female labor market supply. To strengthen the causal claim, Fernandez & Fogli (2009) use fertility and female labor market participation in the ancestral country for second-generation American women and find that these measures have meaningful effects on both labor market choices and fertility. Finseraas & Kotsadam (2017) have replicated this approach on rich Norwegian administrative data and find robust effects on female employment. Another approach uses shocks to gender norms such as the HIV/AIDS-epidemic (Fortin, 2015) and WWII (Fernandez, Fogli & Olivetti, 2004) or changes of economic incentives (Ichino, Olsson, Petrongolo & Thoursie, 2019) to estimate effects from gender norms to female labor supply. These papers highlight the role of gender norms on women's labor market decisions, and thus it seems natural to suspect that gender norms also affect intra-household specialization. However, no paper explicitly shows this. This paper fills this gap by directly addressing how gender norms affect the intra-household decisions of time allocation to child-rearing.

It is well-established that the arrival of children implies a major cost for women in term of earnings, and this effect is long lasting (Kleven et al., 2019; Ejrnæs & Kunze, 2013; Harkness & Waldfogel, 2003). To elevate some of the costs associated with motherhood, most developed countries have introduced some sort of maternity leave system (Olivetti & Petrongolo, 2017). Although the specifics vary greatly across countries, leave schemes were first put in place due to concerns for maternal and child health (Ibid.). Motivations for extending leave schemes have been more mixed; some policies have the intention of allowing women to combine careers and motherhood, other policies have reaffirmed women's roles as mothers and caregivers (Ibid.). Many countries are supplementing maternity leave with 'gender-neutral' parental leave-schemes, but mothers remain the primary users of this (Ibid.). To increase fathers' share of parental leave, some countries have implemented earmarked leave for fathers, also known as 'daddy quotas'. Although the use of these policies has been gradual (Dahl, Løken & Mogstad, 2014; Andersson, Ma, Duvander & Evertsson, 2019), research have found positive effects on women's wages (Druedahl, Ejrnæs & Jørgensen, 2019), decreases in divorce rates (Steingrimsdottir & Olafsson, 2020), and more equal division of housework (Patnaik, 2019).

To understand how gender norms affect time allocation within the first year of the parenthood, Denmark provides a very useful setting. Historically, Denmark has, as the other Nordic countries, implemented family-friendly policies enabling a large share of women to participate in the labor market (Smith, Datta Gupta & Verner, 2008). These policies include heavily subsidized day care for children, paid parental leave, and job protection while on leave. However, in terms of both recent policy and social norms, Denmark diverges from the other Nordic countries. Among all Nordic countries, Danish fathers take the least leave (Nordisk

Statistik, 2017). Moreover, in all other Nordic countries, policy makers have implemented some version of a 'daddy quota' to increase fathers' use of leave. In constrast, Danish policy makers have refrained from implementing such policies and argued that parents – not the government – should decide the distribution of leave (Deding, 2012).

To guide my empirical investigation, I outline existing theories that provides predictions of household behavior at reform implementation. With this in hand, I use the 2002-leave reform and detailed Danish register data to implement a Regression Discontinuity Design. In my preferred specification, I compare the leave behavior of families with a child born in the 9 months prior to the reform with those with a child born 9 months after the reform. The empirical investigation shows that mothers increase their leave with 5 weeks upon reform implementation. Among fathers, the average leave duration is unchanged. However, closer inspection of the data shows that the leave of fathers changed in two directions; some fathers reduced their leave, while few extended their leave. Across the population, 1.6 pct. of fathers extended their leave. Theory of specialization would predict different responses across relative earnings of the household, but this barely influences the leave duration of neither mothers nor fathers. This is consistent with the notion of gender identity and difference in prescription faced by mothers and fathers. Subsequently, I define peers as sisters and identify sisters of mothers in the reform window. I then compare the leave behavior among these sisters who all had a child after the reform-window. They face the same institutional set-up and only differ in terms of when their sister had a child. Any differences in leave duration across those with a sister in the reform treatment group and those with a sister in the reform control group can be attributed to the leave scheme under which their niece/nephew was born under. I find that mothers with a sister in the reform treatment group take a 1.1 week longer leave than those with a sister in the control group, corresponding to peer effects of 17 pct.. I interpret this as reform-induced prescriptions of extensive leave duration among mothers, and that social interactions among close peers transmit the prescriptions. Objections to my interpretation of the findings might be that the gender gap is simply due to biological differences across men and women,[1] but this does not explain the peer effects. Further, one might think that

[1]For a longer discussion of the role of biology in gender gaps and the child penalty, see Kleven, Landais and Søgaard (Forthcoming), who show that biology is not an important channel.

the reform effect is driven by comparative advantages. For this to be the case, we would need to the women who out-earn their spouse to also be more productive in home production. Although this cannot be ruled out, this line of reasoning still fails to explain the peer effects.

This paper contributes foremost to the growing literature on gender norms and subsequent inequality in economic outcomes. As mentioned, little attention has been paid to the effects on intra-household specialization. The primary contribution of this paper is to show how gender identity and prescriptions affect the use of parental leave and how the formation of this interact with public policy. Naturally, my paper also contributes to the literature on parental leave. Unlike most studies, which solely estimate the immediate effect, I also study social multipliers. Few other papers do this; Dahl et al. (2014) investigate implementation of a 'daddy quota' in Norway. They find large take-up rates and subsequent peer effects on brothers and male co-workers. Welteke & Wrohlich (2019) find peer effects on female co-workers in Germany after a reform that encouraged mothers to stay at home the year following childbirth. In contrast, I investigate the effects of a parental leave scheme where leave can be used be either parent. I show that the difference in prescriptions faced by mothers and fathers play a crucial role of both the immediate take-up and peer effect.

Finally, this paper contributes to the literature on peer effects. The literature on peer effects in labor market choices and gender goes back to Neumark & Postlewait (1995). They show that labor market choices of women can spur similar choices by close peers regardless of earnings and income effects (see Nicoletti, Salvanes & Tominey (2018) for a recent investigation). As voiced by Manski (1993) the peer effects literature needs to address serious empirical challenges to avoid issues related to endogenous group membership, the reflection problem, and contextual effects. To circumvent these threats to identification, researchers use quasi-experiments such as event-studies (Nielsen & Fadlon, 2019) and implementation of policies (e.g. Angrist & Lang, 2004; Brown & Laschever, 2012; Kling, Liebman & Katz, 2007) to ensure well-identified effects. However, less energy is directed into disentangling the mechanisms behind the effects. In a critical survey, Sacerdote (2014) concludes that these insights are far from a point that is useful for policy recommendations. However, he points out that social outcomes (e.g. crime, drinking behavior) and labor market choices provides promising results. To emphasize this point, I highlight one potential mechanism for peer effects, which can guide future empirical investigations: changes in prescriptions. Specifically, peer effects should show up in empirical investigations when the relevant prescriptions change.

The structure for the remaining part of the paper is as follows. In Section II, the existing theories are presented, and hypotheses are formed. Section III contains a presentation of the 2002-reform, the data set, and the empirical strategy. Graphical and regression-based results are reported in Section IV together with robustness checks. Section V contains an interpretation of the results in relation to the presented theories. Section VI concludes.

II Household behavior upon parenthood

To understand how households respond when they are given the opportunity to take an extended leave, I briefly outline the standard Becker (1981) model on division of labor. In this model, members of the household corporate to maximize joint production and specialize according to their comparative advantages. This provides a plausible explanation for the child penalty and testable prediction of leave behavior upon reform implementation. However, findings (e.g. Kleven et al., 2019; Daly & Groes, 2017) suggests that standard economic factors fail to account for the observed behavior in households. To provide alternative hypotheses for expected leave behavior, I turn to Akerlof & Kranton (2000; 2002; 2004) and their theoretical framework drawing on insights from sociology and social psychology for how to think about identity, social categories, and prescriptions in economics.

II.I Financial incentives and comparative advantages

In Becker's influential model of the household, the key insight is that intra-household specialization determined by members comparative advantages. 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.[2] Each member of the household can allocate time to each of the two sectors, the labor market and the home.

[2] I disregard any argument related to biological advantages. The average maternity leave before the reform well extended the Danish authorities' recommended period of full breastfeeding. Moreover, earmarked leave for mother ensures 'sick days' after giving birth (see Persson & Rossin-Slater (2019) for a theoretical framework specifically on the different types of leave around childbirth).

If women on average have invested more heavily in human capital relevant for household production and men have invested more heavily in human capital relevant for market production, women should on average specialize in household 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; Larsen & Petersen, 2013), 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.

II.II Gender identity and prescriptions

Empirical evidence so far show that educational level and relative earnings have very little predictive power over the size of the child penalty (Kleven et al., 2019) and time allocation to child-rearing (Daly & Groes, 2017). This support the notion that economic factors cannot fully account for intra-household time allocation. To understand this, I turn to the framework developed by Akerlof & Kranton (2000; 2002; 2004). 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.

If standard economic factor incentivize behavior different from that of other members of one's social category, acting according to this is associated with a utility-cost. Pay-off from identity and prescriptions implies that conformity is a rational choice. This enables us to understand how compliance with prescriptions of one's social category can be considered utility maximizing behavior. Akerlof & Kranton (2000; 2002) show that 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.

Akerlof & Kranton (2000) have highlighted gender as a social category with great importance for individual choices and argue that different prescriptions relevant for men and women can explain differences in education, occupation, and labor supply. 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. By applying this line of thinking to households' decisions on time allocation between market and home production, it is then utility maximizing behavior for women to allocate extensive time to home production regardless of economic incentives. Similarly, norms of the male breadwinner might induce men to allocate less time to home production that what standard economic theories would predict. In this framework, prescriptions are defined locally as the average behavior among relevant peer such as school-mates (Akerlof & Kranton, 2002) and co-workers (Akerlof & Kranton, 2004). If relevant peers change their behavior, so does the optimal behavior of the individual.

II.III Hypotheses

Based on two frameworks, two different sets of hypotheses 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 use the opportunity of a 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 where not. Equivalently, fathers who are not primary earners 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, large reform effects among mothers is expected. Fathers are not expected to use the opportunity of increase leave duration. Subsequently, women who observe their sister taking a long leave - induced by the reform - then observe a new set of prescriptions. Women with sisters in the control group do not observe their sister taking a long leave. These women are then exposed to different prescriptions, and are thus expected to behave differently even though they face the same institutional set-up. If the reform implied 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.

When evaluating the reform effect, it is of course possible that the women who are out-earning their partners also are more productive in the home. As I have no measure for human capital relevant for home production, I cannot rule out this explanation. However, this explanation cannot account for peer effects. Similarly, any arguments related to endemic biological differences across men and women that could account for the reform effect fail to provide an explanation for the peer effects.

III Identification and empirical strategy

III.I Institutional context

Denmark has a long tradition for substantial family-friendly policies and high female labor force participation. In the 1990s, 84 pct. of Danish mothers with children below the age of 10 worked outside the home and 2/3 worked full time (Leira, 2010). Over the past three decades, the duration of parental leave with economic compensation has gradually been expanded. Childcare options have also been expanded with almost universal coverage in 2000 (Ibid.). While these policies in principle are relevant for both parents, they are viewed as something primarily relevant for mothers (Smith et al., 2008) who by tradition have been given responsibility of childcare.

The last major extension of parental leave opportunities took place in 2002. This reform substantially extended the total number of weeks with compensation corresponding to unemployment insurance ('Barselsdagpenge'), but reduced the number of weeks allocated to the father with two weeks (Extension of maternity leave and change of childcare leave, 2002). 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. The replacement rate was 90 pct. of former earnings up to a flat rate with an average compensation rate of 66 pct. (Smith et al., 2008). This period was followed by a period of 52 weeks at a reduced rate corresponding to 60 pct. of the previous benefit. With the reform, the total leave period after childbirth with compensation was extended to 48 weeks of which 2 were allocated to the father, 14 to the mother and 32 weeks are shared. The period with employment protection, but without benefits, covers 14 additional weeks for each parent. Figure 1 provides an overview of the institutional change.

Figure 1: Institutional change due to the 2002-reform

The reform provided a longer period with better economic compensation. With the simultaneous reduction in leave specifically allocated to fathers, I argue that the policy was conceived as primarily relevant for mothers. The empirical investigation supports this.

The reform was presented in Parliament 7th of January 2002 and adopted on 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 1st of January and 27th of March were given the option to choose between the two schemes. Results will show a jump in average leave duration of mothers at 1st of January 2002, and no change in the average leave duration of fathers. At 27th of March, the changes in average leave are barely visible, implying that the vast majority of couples preferred in the new scheme. With similar results, Beuchert, Humlum & Vejlin (2016) argue that almost all parents choose the post-reform rules if given the option.[3] As 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 campaign promises where less precise. There was no reason to

[3]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.

suspect such a major change immediately after the new government took office. The rapid implementation of the reform implies that no self-selection can occur. The discontinuity provides a close-to-ideal set-up for evaluating both the reform and peer effects.

In addition to the compensation from the government, some employers pay an additional compensation. There are large sectorial differences in both level and duration, but the vast majority of new parents face a substantial period with compensation that is lower than their earnings.[4]

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. This law changed the economic incentives to leave taking for parts of the population. 2005 will, therefore, be the end year for this analysis.

III.II Data and Descriptive Statistics

To evaluate effect of the reform and subsequent peer effect, I use Danish register data. This data contains all parents who had children between March 2001 and December 2005. I combine information from several administrative registers from Statistics Denmark. The data set contains individual records and cover the Danish population with a high degree of precision and allows for identification of all children and their parents. Family identifiers allow for identification of peers as sisters. My final data set includes rich covariates such as information on education, labor market information of parents 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. As many women in Denmark change job into family-friendly sectors upon having children (Nielsen, Simonsen & Verner, 2004), this avoids any confounders due to job changes or any mechanical effects from income reduction while on leave.

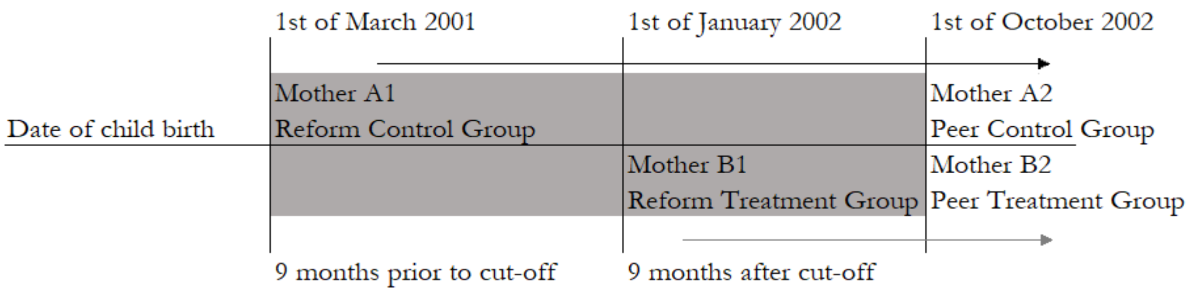
To measure the length of leave, I use information on weekly benefits from the DREAM-register. I construct a variable containing a count of weeks during which a parent receives compensation

[4]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.

due to parental leave a year following the birth of their child. This measure includes the full compensation corresponding to unemployment insurance ('Barselsdagpenge'), and the reduced rate that was in place before the reform ('Børneorlov'). It does not include leave taken prior to child birth (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 and 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 children between 2001 and 2005.

I divide the population of parents is 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 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

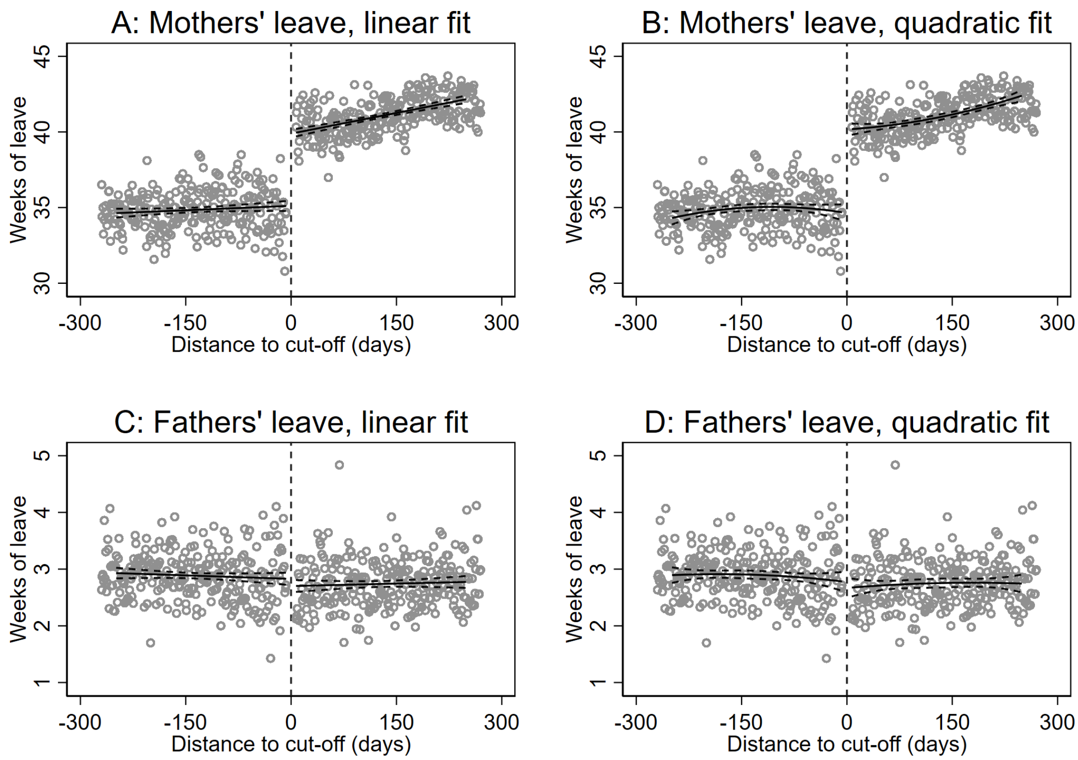


When the aim is to identify peer effects in naturally occurring peer groups, it raises a 'many-to-one' issue as many peers can affect the same individual. This problem arises if more than one peer became a parent around the reform date, particularly if there is a peer before implementation of the reform 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. When implementing a similar solution, 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 solution 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 and this could be 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 could be due to planned fertility, specifically planned C-sections and labor induction during the holidays. For this reason, observations 7 days before and after the cut-off are dropped. The final sample used to investigate reform effects contain 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 shows the reform effect on average duration of paid leave and the discontinuity in average leave duration at reform implementation. The top panel shows that mothers increase their leave with about 5 weeks at reform implementation. The bottom panel shows no change in average leave duration of fathers. Beuchert et al. (2016) focus on mothers' leave and report 4.6 weeks increase in leave duration of mothers. They only consider a window of 60 days, where I use 9 months. Nielsen (2009) only consider couples where both parties are employed in the public sector, and her estimated reform effect on mothers is larger (approx. 50 days). Results reported later will also show that public sector employment increases leave duration of mothers. Thus, the effects reported are in line with results by other studies using this reform.

Figure 3: Change in leave with compensation at the implementation of the reform



Notes: The figure shows average leave duration measured in weeks of mothers (top panel) and fathers (bottom panel) with either a linear or quadratic fit. 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.

Table 1 shows a similar picture with an increase in 5 weeks of average leave of mothers, and a very small reduction in average leave of fathers. The variance in leave duration of mothers decreases with the reform, implying that women behave more similar after the reform. For the fathers, the variance increases. Mean and standard deviations of covariates are reported in column (1) and (2) for the mothers in the reform window together with a t-test. Mothers in the reform treatment group are slightly older (30.7 years vs. 31.1 years), in households that earn 14.000 DKK/year (aprox. 1900 €) more and are also better educated than the control group. For the mothers with sisters in the reform window, covarites are reported in column (3) and (4) together with a t-test. Here, no covariates are significantly different across treatment and control group. This indicates a very valid research design. These mothers are more likely to be first-time mothers than those the reform window. This is by construction, as only one child for the period 2001-2005 is allowed.

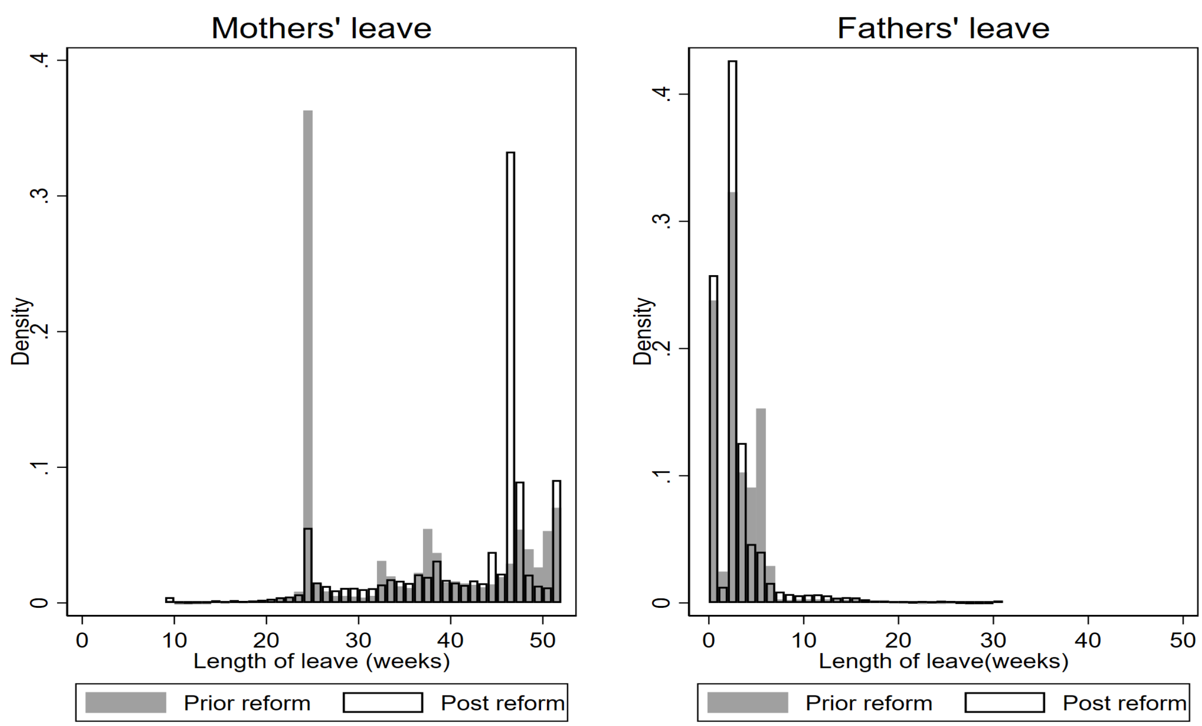
Figure 4 shows histograms of the leave duration for mothers and fathers, respectively. For mothers, there is a substantial shift to a longer leave. Before the reform, 37 pct. of all mothers take 24 weeks of leave. After the reform, only 5 pct. of all mothers take 24 weeks of leave. Instead, 34 pct. of all mothers now take 46 weeks of leave, which is the new maximum duration of leave with full compensation.[5] However, for fathers, the most common leave duration both before and after the reform is 2 weeks with 33 pct. of all fathers taking two weeks before the reform. With the reform, this share increases with approximately 10 pct.-point. At reform implementation, the share of fathers who take 4 weeks of leave is reduced with 12-pct. points. Moreover, 25 pct. of all fathers have no leave registered both before and after the reform. As 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 the Appendix C. Meanwhile, a longer and more dense tail shows that some but few fathers increase their leave. Then, the reform implied that most fathers reduced their leave, but a small share substantially increased their leave. This will have consequences for the empirical strategy.

[5]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.

TABLE 1: Covariates across control and treatment groups

	Couples in reform window			Mothers exposed to peer effects		
	(1)	(2)		(3)	(4)	
	Control	Treatment		Control	Treatment	
Number of observations	21,475	22,841		1,915	1,928	
	Mean	Mean	ttest	Mean	Mean	ttest
	(SD)	(SD)	p (dif!=0)	(SD)	(SD)	p (dif!=0)
Mothers' leave	34.8	41.1	-66.49	41.7	42.0	-1.01
	(10.8)	(9.1)	0.0000***	(7.9)	(7.9)	0.3143
Fathers' leave	2.9	2.8	4.0	3.2	3.3	0.77
	(3.5)	(3.9)	0.0001***	(4.2)	(4.1)	0.4371
Mothers' age	30.7	31.1	-10.42	31.1	31.1	-0.41
	(4.1)	(4.1)	0.0000***	(3.8)	(3.9)	0.6820
Household inc. (DKK)	510,962	524,977	-7.17	565,852	560,452	0.81
	(202,542)	(208,312)	0.0000***	(193,132)	(182,514)	0.4169
Income share earned by mother	41.2	41.6	-2.33	42.9	43.1	-0.33
	(18.1)	(18.1)	0.0199**	(13.6)	(13.2)	0.7447
Share, public sector employed	41.2	42.1	-1.98	44.6	45.1	-0.32
	(49.2)	(49.4)	0.0477**	(49.7)	(49.8)	0.7526
Share, first-time mothers	41.8	41.1	1.59	51.3	51.0	0.20
	(49.3)	(49.2)	0.1128	(50.0)	(50.0)	0.8434
Education level, mother						
Share w. primary edu.	9.7	9.2	1.99	7.3	6.4	1.02
	(29.6)	(28.8)	0.0460	(26.0)	(24.4)	0.3098
Share w. high school edu.	6.9	6.8	0.17	4.6	5.8	-1.48
	(25.3)	(25.3)	0.8687	(21.1)	(23.4)	0.1386
Share w. vocational edu.	40.8	39.3	3.17	38.5	38.4	0.06
	(49.1)	(48.8)	0.0015**	(48.7)	(48.6)	0.9510
Share w. some college (∼2 years)	5.6	5.9	-1.18	6.6	7.2	-0.73
	(23.1)	(23.6)	0.2377	(24.8)	(25.9)	0.4636
Share w. Bachelor's or equivalent	28.3	28.5	-0.52	32.4	31.7	0.34
	(45.0)	(45.1)	0.6040	(46.8)	(46.6)	0.6978
Share w. Master's or PhD	8.7	10.3	-5.67	10.6	10.4	0.16
	(28.2)	(30.4)	0.0000***	(30.8)	(30.6)	0.8746
Relative education						
Share, mother less educated	24.8	24.6	0.30	21.5	22.8	-0.96
	(43.2)	(43.1)	0.7617	(41.1)	(42.0)	0.3391
Share, same education level	41.9	41.4	0.98	40.11	42.00	-1.08
	(49.3)	(49.3)	0.3278	(49.0)	(4.94)	0.2786
Share, mother more educated	33.4	34.0	-1.30	38.4	35.1	1.93
	(47.2)	(47.4)	0.1945	(48.7)	(47.8)	0.0537*

Figure 4: Histogram of leave duration before and after the reform, mothers and fathers



Notes: The figure shows 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 sample is the same as in Figure 3.

III.III Empirical Strategy

The reform allows for long leaves with better compensation and creates a discontinuity in leave duration at 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 announcement and implementation of the reform implies 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 researchers cannot observed 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 identification of peer effects should no longer be a concern. Thus, treatment is 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:

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + \beta_4 X_i + \varepsilon_i \tag{2}$$

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 child birth.

When estimating the peer effects, I adopted an 2SLS-estimator following the work by Dahl et al. (2014). The first-stage has the outcome variable, L_i, indicate the length of leave of individual i with a child in the reform window is given by:

$$L_i = \beta_0 + \beta_1[d_i|d_i < d_0] + \beta_2 T_i + \beta_3[d_i|d_i \geq d_0] + \beta_4 X_{ip} + \varepsilon_i \tag{3}$$

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 child birth. 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:

$$L_p = \delta_0 + \delta_1[d_i|d_i < d_0] + \delta_2 \hat{L}_i + \delta_3[d_i|d_i \geq d_0] + \delta_4 X_{ip} + \delta_5 d_p + \varepsilon_p \tag{4}$$

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 by δ_5.

An alternative empirical strategy is the reduced form:

$$L_p = \lambda_0 + \lambda_1[d_i|d_i < d_0] + \lambda_2 T_i + \lambda_3[d_i|d_i \geq d_0] + \lambda_4 X_{ip} + \delta_5 d_p + \varepsilon_p \tag{5}$$

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.

Three assumptions are needed to interpret the estimates obtained from eq. (2), (3) and (4) as the Local Average Treatment Effect (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. As the reform allowed for a longer leave with better compensation rate, but removed the duration with lower compensation, defiers among mothers could be concern. However, as argued both here and by Beuchert et al. (2016), data inspection show a that most couples choose the new scheme when given the option. As depicted in Figure 4, a 37 pct. of mothers previously took leave at the maximum duration with high benefits. After the reform, this share drop to 5 pct., 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, for fathers the reform implied that a large share reduced their leave from 4 to 2 weeks, while a smaller shared 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. In this specification, the monotonicity assumption 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 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. The main argument for this to hold is that all the mothers experience the same institutional set-up. 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 must be as good as randomly assigned to the peer treatment group. As peer groups are defined as sisters, selecting into treatment from the peer effects is not possible. Any correlation on unobservables among sisters should be dealt with due to random assignment of the reform. The balanced observable across the two groups reported in Table 1 suggest that this is in indeed the case. 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 to 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 behavioral norms than women with a sister in the treatment group. These women are the 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 eq. (5) will still consistently estimate the effect of having a peer mother exposed to the new versus the old institutional set-up.

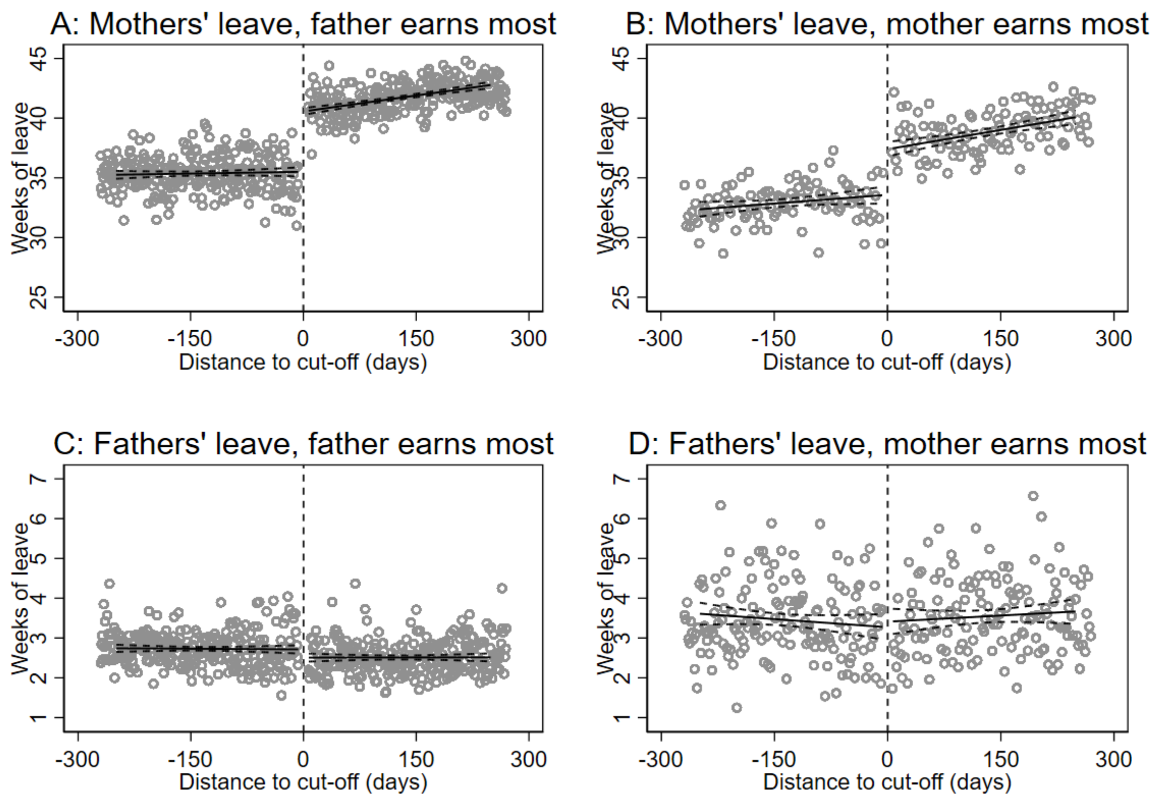
Overall, it seems reasonable that all three required assumptions are met for mothers when evaluating both reform and peer effects. Because the monotonicity assumption cannot be tested, the reduced form will also be implemented in order to evaluate the peer effects. For fathers the monotonicity asumption is violated so I also implement an alternative specification where the outcome is 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.

IV Results

IV.I Graphical Results

An RD-design provides a transparent and illustrative way of visualizing identification of the treatment effects. Figure 5 shows the average leave duration in the full population in the reform-window among mothers and fathers, split on 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. The reform effect does, however, affects both groups similarly with a jump of 5 weeks in both groups. There is however a difference in the initial level. Mothers in more traditional households take 35 weeks of leave prior to the reform compared to 33 weeks among mothers who are the primary earners. 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 appears to be no change in average leave behavior of fathers. There is a difference in the initial average duration of 1 week across the two groups.

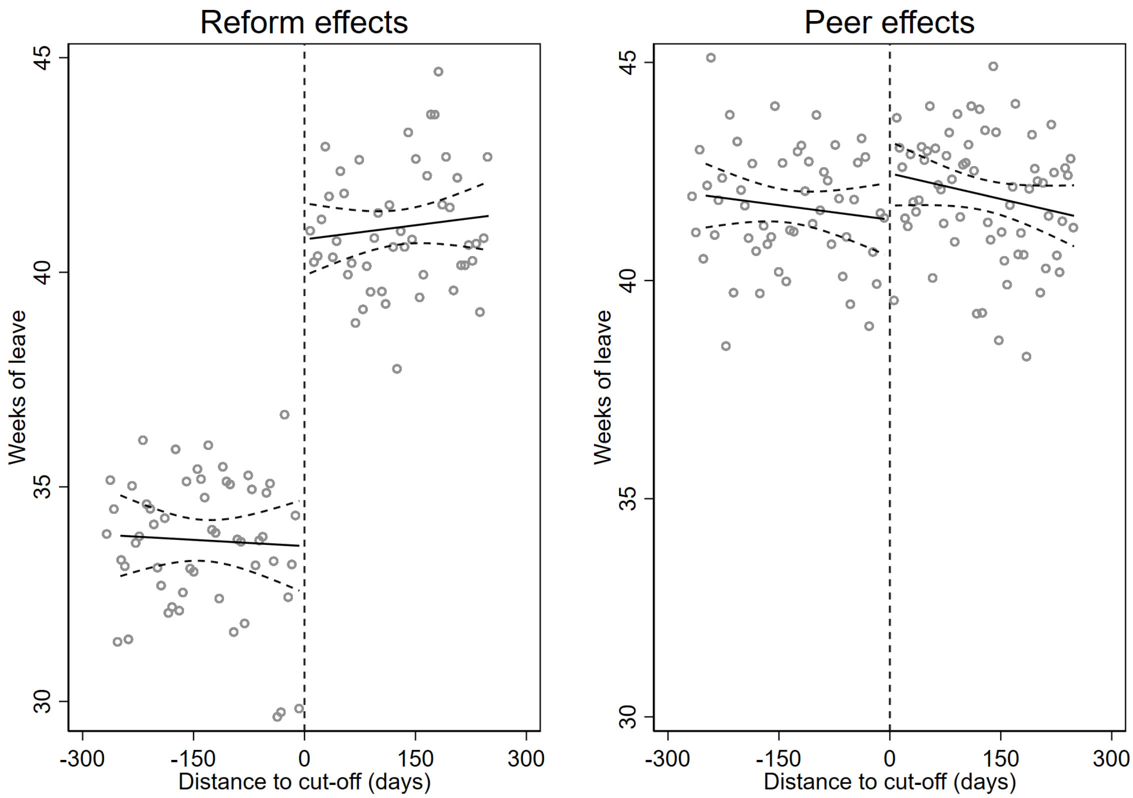
Figure 5: Graphical illustration of the reform effects divided on relative earnings



Notes: The figure shows average leave duration measured in weeks of mothers (top panel) and fathers (bottom panel) split on relative earnings in the household in the year prior to childbirth. This does not include leave taken prior to child-birth. The running variable is date of birth of own child. The sample is the same as in Figure 3.

Figure 6 shows the average leave duration around reform introduction and the subsequent peer effects among mothers. The reform window illustrates the first-stage. There is a sharp jump in the average leave duration from 34 weeks to around 41 weeks. The graphical depiction of the peer effects illustrates the reduced form, indicating 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 the effect appears to be significant.

Figure 6: Graphical illustration of the effects on mothers, reform effects and peer effects



Notes: The figure shows average leave duration of 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.

IV.II Regression-based Results

Table 2 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). Similar to the results reported in Figure 3, the estimated reform effect on the mothers' leave is 4.9 weeks, while there is no effect on fathers' leave. In column (2) and (4), I add interaction terms between relative earnings in the household and the reform. In column (2), we see that both before and after the reform, mothers in couples where the father is the primary earner take a longer leave compared to couples where the mother is the primary earner. Before the reform, the effect is 1.5 weeks. After the reform, the effect is 1.8 weeks. These two estimates are not statistically significantly different from each other. This corresponds to a longer initial duration among mothers who are not primary earners, but no additional reform effect for women who are not primary earners. In column (4), we see that fathers who are primary earners take a 0.6 weeks shorter leave compared to fathers who are not primary earners before the reform. This estimate increases to 0.9 weeks after the reform. Again, these estimates are not statistically significantly different from each other.

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 pct.-point probability of fathers taking a long leave. When adding interaction terms, we see that fathers who are primary earners are less likely to take a long leave compared to those who are not primary earners. With the reform, the size of this effect increases from -3.7 pct. to -6.5 pct.. With the reform, there is a 3.8 pct.-point increase in probability that fathers who are not primary earners take a long leave.

The controls enter with the expected sign when they are significant (see Appendix D), but interpretation of the controls should keep in mind that they are likely to correlate with unobservables. Notably, the estimates of the effects does not change whether the controls are included or not (see 'IV.III Robustness' below).

TABLE 2: Reform effects on leave duration, effect from relative earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Mothers' leave duration (weeks)		Fathers' leave duration (weeks)		Fathers' taking long leave (dummy=1 if leave $\geq$ 8 weeks)	
VARIABLES	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
Reform effect	4.921***	4.715***	-0.136	0.0825	0.0163***	0.0383***
	(0.219)	(0.288)	(0.0830)	(0.127)	(0.00453)	(0.00711)
Interactions						
Prior to reform X		1.517***		-0.593***		-0.0374***
Father primary earner		(0.200)		(0.0925)		(0.00512)
Post refrom X		1.779***		-0.871***		-0.0653***
Father primary earner		(0.188)		(0.0911)		(0.00591)
Observations	44,091	44,091	44,091	44,091	44,091	44,091
R-squared	0.028	0.130	0.028	0.032	0.028	0.032
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

Table 3 presents the model with additional interaction terms. Column (2) contains an interaction between public employment of the mother and the treatment indicator; column (3) contains an interaction between first-time mothers and the treatment indicator; and column (4) between labor supply of the maternal grandmother (of the child born in the reform window) and the treatment indicator. Lastly, column (5) contains a model with all interaction terms. As expected, mothers working in the public sector take longer leave than mothers in the private sector both before and after the reform, but this difference decreases from 2.4 weeks to 0.8 weeks with the reform. This is driven by a large reform response among mothers' working in the private sector, who increase their leave duration with 5.6 weeks. First-time mothers also take longer leave both before and after the reform, but the size of this effect decreases from 0.6 weeks to 0.2 weeks which is only significant at 10 pct. level. This implies that variance in mothers' leave duration decreases after the reform. The more homogenous leave behavior in the population is driven by larger reform effects among mothers with characteristics that prior to the reform would have suggested a shorter leave. The only interaction term for which this does not hold is labor market supply of maternal grandmother. Before the reform, there is no effect on own leave behavior from the labor supply of the maternal grandmother of the child. However, the reform effect is 0.4 weeks larger among those with a mother with low labor supply compared to those with mothers with high labor supply. This is in line with the literature showing inter-generational transmission of gender identity and labor market choices.

TABLE 3: Reform effect on mothers' leave duration, alternative specifications

VARIABLES	(1) Baseline	(2) Sector	(3) Child parity	(4) Maternal labor supply	(5) Full model
Reform effect	4.912***	5.566***	5.063***	4.736***	5.485***
	(0.220)	(0.234)	(0.228)	(0.256)	(0.349)
Interactions					
Prior to reform X		2.414***			2.286***
Publicly employed		(0.157)			(0.164)
Post reform X		0.871***			0.684***
Publicly employed		(0.128)			(0.131)
Prior to reform X			0.555***		0.588***
First-time mother			(0.147)		(0.158)
Post reform X			0.204*		0.163
First-time mother			(0.120)		(0.128)
Prior to reform X				0.0862	0.109
Low maternal labor supply				(0.148)	(0.147)
Post reform X				0.442***	0.374***
Low maternal labor supply				(0.121)	(0.121)
Prior to reform X					1.490***
Father earning most					(0.212)
Post reform X					1.675***
Father earning most					(0.199)
Observations	44,091	44,091	44,091	40,249	40,249
R-squared	0.129	0.129	0.127	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.
Standard errors are clustered on date of birth of own child where *** p<0.01, ** p<0.05, * p<0.1

Table 4 presents the estimates of the peer effects for the mothers. The first stage is reported in column (1). Column (2) reports the reduced form corresponding to Figure 6. The point estimate corresponds to 1.1 week of additional leave among mothers with sisters who had a child after reform implementation. The 2nd stage estimate is reported in column (3) and show an increase in leave of 17 pct. increase in leave compared to the reform effect. The reform-induced change in behavior in of mothers in the reform treatment group implies that the peer sisters observe different behavioral norms, 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 show up here as peer effects.

Additional interaction terms are then added to the reduced form. Column (3)-(10) contain the reduced form model with interactions terms corresponding to Table 3 for both own and peer category. The point estimate increases in size when adding the interaction effect for sector employment of the mother exposed to the peer effects from the reform (column (4)), for labor supply of the maternal grandmother (column (8)) and when the sister in the reform window is a not a first-time mother (column (10)).

Notably, those who had working mothers themselves took shorter leave if their sister was in the control group, but this effect disappears after the reform. The reform-induced change in prescriptions strongly reduces inter-generational effects from maternal labor supply. Similarly, women who have who work in the private sector took a shorter compared to mothers who work 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, as women in the private sector respond stronger upon observing their sister taking a long leave. This mirrors the investigation of heterogeneous effects across women in the reform window; the women who respond stronger are the women with characteristics who in the absence of treatment would have taken a short leave.

In sum, economic incentive is not be a driving force in the leave decision as the reform effect among mothers are highly homogeneous across relative earnings in the household. Average leave duration of fathers is unchanged. Even among those with the strongest economic incentive to leave taking, very few fathers respond to the reform by taking a long leave. Thus, the predictions provided by theory of specialization are not matched by the data. Instead, this is highly consistent with the notion of gender identity; mothers are expected to allocate time to the home production, while fathers are not. The differences in behavior across mothers and fathers can be explained by differences in prescriptions. This determines the distribution of leave within the household and drives the estimate of the reform effect. Moreover, variance of leave duration of mothers is reduced with the reform. This is driven by a stronger reform response among mothers with characteristics that suggest that they would have taken a shorter leave in the absence of the reform such as private sector employment and not being a first-time mother. These groups start to behave more similar to mothers in the public sector and first-time mothers.

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, where those who observe their sister taking a long leave take a longer leave themselves compared to women who observe their sister taking a shorter leave. Again, effects are larger among those with characteristics which would otherwise have suggested a shorter leave such as private sector employment and having a working mother. Combined, reform effects and peer effects show the reform reinforced existing gender gaps in intra-household specialization and that different prescriptions relevant for mothers and fathers is the relevant mechanism behind this inequality in time-allocation.

TABLE 4: Peer effects on mothers leave duration

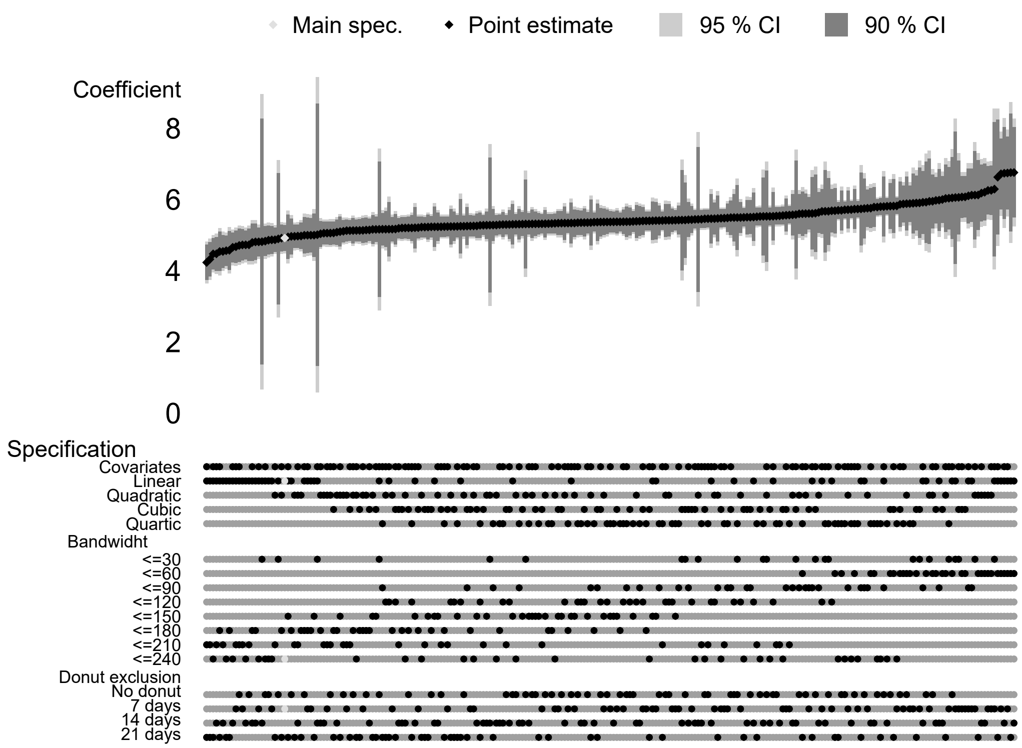
VARIABLES	(1) 1st stage	(2) Reduced form	(3) 2SLS	(4) Own sector	(5) Peer sector	(6) Own earnings	(7) Peer earnings	(8) Maternal labor supply	(9) Own 1st child	(10) Peer 1st child
Peer effect	6.815***	1.145**	0.168**	1.349**	0.986*	1.096	0.751	1.523**	0.948	1.326**
	(0.709)	(0.554)	(0.0809)	(0.601)	(0.598)	(0.813)	(0.792)	(0.683)	(0.644)	(0.599)
Prior to reform X				1.446***						
Publicly employed				(0.429)						
Post reform X				0.982**						
Publicly employed				(0.413)						
Prior to reform X					0.253					
Peer publicly employed					(0.421)					
Post reform X					0.612					
Peer publicly employed					(0.419)					
Prior to reform X						2.304***				
Father primary earner						(0.530)				
Post reform X						2.388***				
Father primary earner						(0.504)				
Prior to reform X							0.007			
Peer father primary earner							(0.514)			
Post reform X							0.498			
Peer father primary earner							(0.519)			
Prior to reform X								1.063**		
Low maternal labor supply								(0.423)		
Post reform X								0.351		
Low maternal labor supply								(0.387)		
Prior to reform X									-0.0489	
First-time mother									(0.406)	
Post reform X									0.337	
First-time mother									(0.428)	
Prior to reform X										0.396
Peer first-time mother										(0.373)
Post reform X										-0.0541
Peer first-time mother										(0.398)
Observations	3,808	3,808	3,808	3,808	3,808	3,808	3,808	3,618	3,808	3,808
R-squared	0.171	0.058	0.072	0.058	0.059	0.053	0.059	0.059	0.058	0.058
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.

IV.III Robustness

The robustness checks indicate a very valid research design with 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 (i.e. the 'donut') around implementation provide virtually unchanged estimates. For all specifications, the point estimate 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 7.

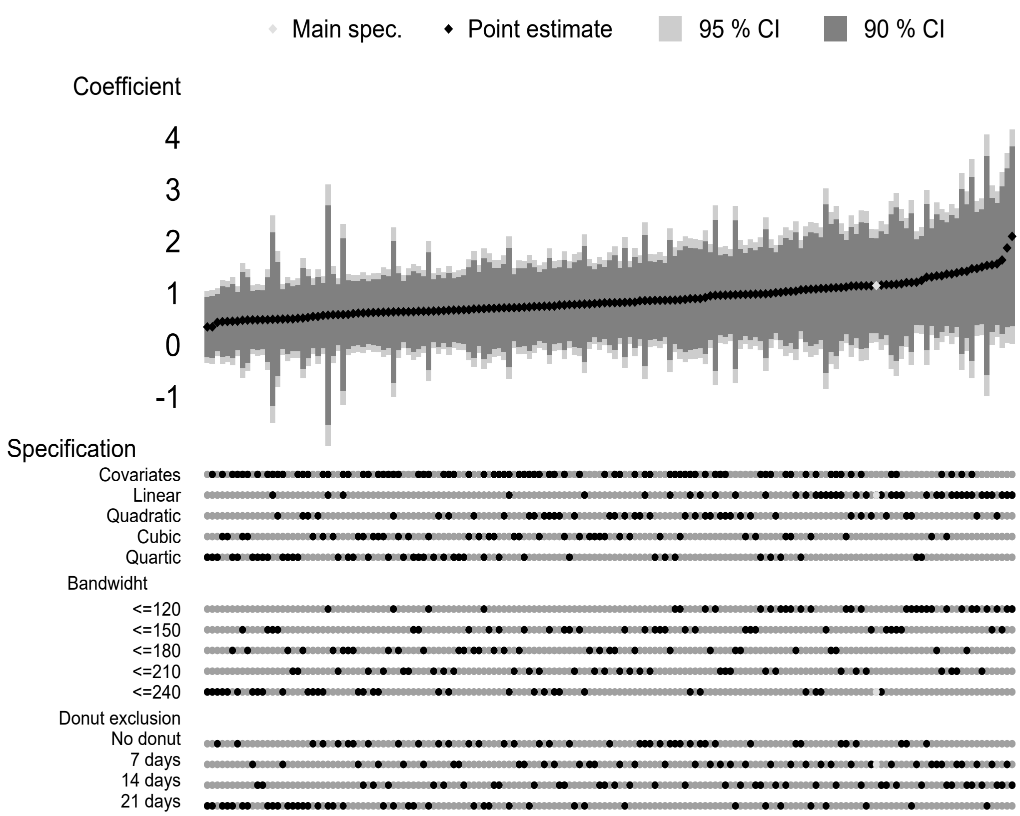
Figure 7: Estimates of reform effect on mothers' leave behavior



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.

A similar set of robustness checks are made for the reduced form estimate of the peer effects. This is reported in Figure 8. Again, the point estimate is very stable. However, as the sample size is much smaller for peer effects than for the reform effect, precision decreases. Having a bandwidth below 120 is not feasible as the number of observations drops too much.

Figure 8: Estimates of peer effects, reduced form



Notes: See Figure 7.

In order to rule out that the peer effects are driven by positive consumption externalities or coordination in fertility of sisters, I exclude mothers who had a child between October 2002 and Jan 2003. The sample is reduced, but the reform estimates increase slightly. This is the opposite of what should be expected if the peer effects were driven by coordinated fertility. In addition, I interact a dummy for living in the same municipal as one's sister with the treatment indicator. Mothers who lived in the same municipal as their sister took a longer leave than those who did not live in the municipal prior to the reform. This effect disappears with the reform. This could potentially be driven those in the reform control group who used the leave at reduced benefit, who could potentially experience positive externalities of being on leave at the same time as their sister. This opportunity is reduced with the reform. Thus, the peer effects estimated here are not driven by consumption externalities. These estimates are reported in the Appendix E.

V Discussion

The empirical investigation provides stable estimates of 5 weeks increase in parental leave among Danish mothers after the parental leave reform of 2002. Meanwhile, average leave behavior among fathers is unchanged. The estimates barely change across relative earnings. The empirical results are aligned with the hypotheses highlighting the importance of gender identity and different prescriptions faced by mothers and fathers. The results are interpreted as evidence of gender identity and prescriptions being more important than standard economic incentives for the decision of intra-household specialization. Peer effects of 17 pct. among mothers with sisters in the reform window further support this interpretation.

The results reported show that variance in leave duration of mothers decreases with the reform. This is driven by a slightly stronger reform response among mothers with characteristics suggesting that they would have taken a shorter leave in the absence of the reform, incl. private sector employment. Similarly, those that respond the strongest to observing their sister taking a long leave are those with working mothers and private sector employment. The public sector generally offer more family-friendly policies (Nielsen et al., 2004), so that mothers employed in the private sector respond strongly to both the reform and to their sister taking a long leave suggests that preferences for family-friendly work arrangements are not driving these results. Having a working mother is positively associated with own labor force participation (Farré & Vella, 2014; Fernandez et al., 2004; Finseraas & Kotsadam, 2017) and a smaller child-penatly (Kleven et al., 2019). If parents' attitudes are transmitted in childhood where working mothers arguably are important role models, the exposure to a sister who take a longer leave appears to counteract this effect. Thus, this reform first allowed gender identity and prescriptions to directly affect the leave distribution in the household. Second, the reform-induced change in leave behavior implies that those with a sister is the reform treatment group face prescription of extensive leave. This is show up here as peer effects and reaffirm gender-specific intra-household specialization.

In general, many family policies might have this effect. Researchers have argued that too long maternity leaves policies have a potential for negative effects on women's labor market outcomes (Ruhm, 1998; Rossin-Slater, 2018). Other have argued that the effectiveness of family-friendly policies are muted 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 class-ceiling' (Smith et al., 2008) because they mainly affect the labor market outcomes of women. Adding further weight to this argument, the results reported here suggest that family policies that do not challenge existing prescriptions relevant for mothers and fathers also have this effect. If family policies do not explicitly encourage fathers to use them, they will be considered mainly relevant for mothers and thus strengthen existing gender gaps in intra-household specialization.

My findings suggest that in the absence of explicit policies that target fathers' involvement, gender identity and different expectations of behavior of mothers and fathers determinate the leave decision. In contrast, the Norwegian policy evaluated by Dahl et al. (2014) arguably changed prescriptions regarding fathers' behavior by encouraging them to take leave (Lappegaard & Kornstad, 2020). The reform in Denmark is then similar to the German reform evaluated by Welteke & Wrohlich (2019) which encouraged longer maternity leave and stresses the importance of staying home more heavily. The Danish policy improved leave opportunities with compensation, while simultaneously removing the leave specifically allocated to fathers. As argued in this paper, this implies that the distribution of leave is decided in accordance with prescriptions. As mothers are expected to be the primary caregivers of children, mothers take the vast majority of the leave that in principle could be shared with the father. Instead of changing prescriptions regarding fathers' leave, Danish and German policies reinforced views regarding women's responsibility in childcare and home production.

In general, norms and attitudes evolve with an exposure to a phenomenon. When more women work, the views of the appropriateness of this change. When more fathers take long parental leave, views around this also change although this appear to happen slowly (Andersson et al., 2019). This paper shows that even in a country with decades of high female labor force participation, the underlying views on appropriate division of time upon having children are largely unchanged. A policy that offers families to make choices aligned with existing prescriptions is met with a strong response among mothers irrespectively of relative earnings and further 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 investigation of peer effects. As argued by Sacerdote (2014), studies of peer effects on social outcomes and labor market choices procedure significant results more often than those on test scores. However, channels are rarely identified. As argued by Akerlof & Kranton (2000; 2002; 2004), prescriptions might be more important than standard economic factors in various decisions. In empirical investigations, change in prescriptions can show up as peer effects. The results reported here and interpretation of related studies support the notion of prescriptions as a potential channel. Other studies might investigate this in other areas.

VI 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. In contrast to standard models of intra-household specialization, hypotheses that consider the role of gender identity, social category and prescriptions are consistent with the observe leave behavior. This paper shows that leave decisions are driven by prescriptions rather than standard economic incentives. By using the discontinuity that arises from the parental leave reform in Denmark in 2002, an RD-design provides robust estimates of 5 additional weeks of leave taken by Danish mothers while the average behavior of fathers is unchanged behavior. This is also the case in households where mothers have a strong economic incentive to allocate time to the market. That the results are driven by gender identity and prescriptions is further supported by the fact that women with a sister in the reform treatment group take a 1.1 week longer leave compared to those with sister in the reform control group. I argue that reform-induced prescriptions regarding extensive leave for mothers drives the peer effects.

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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 child care 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

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
Pct.	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

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
Pct.	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 9: Drop in births at New Year

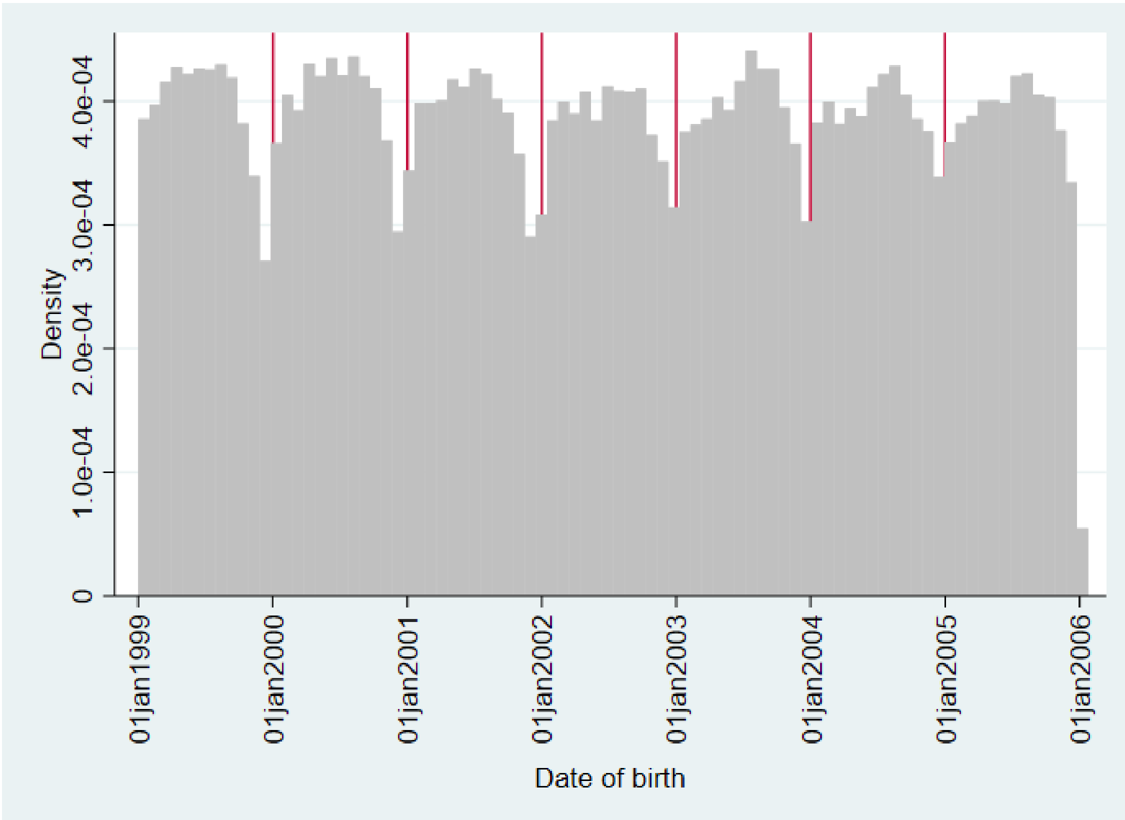
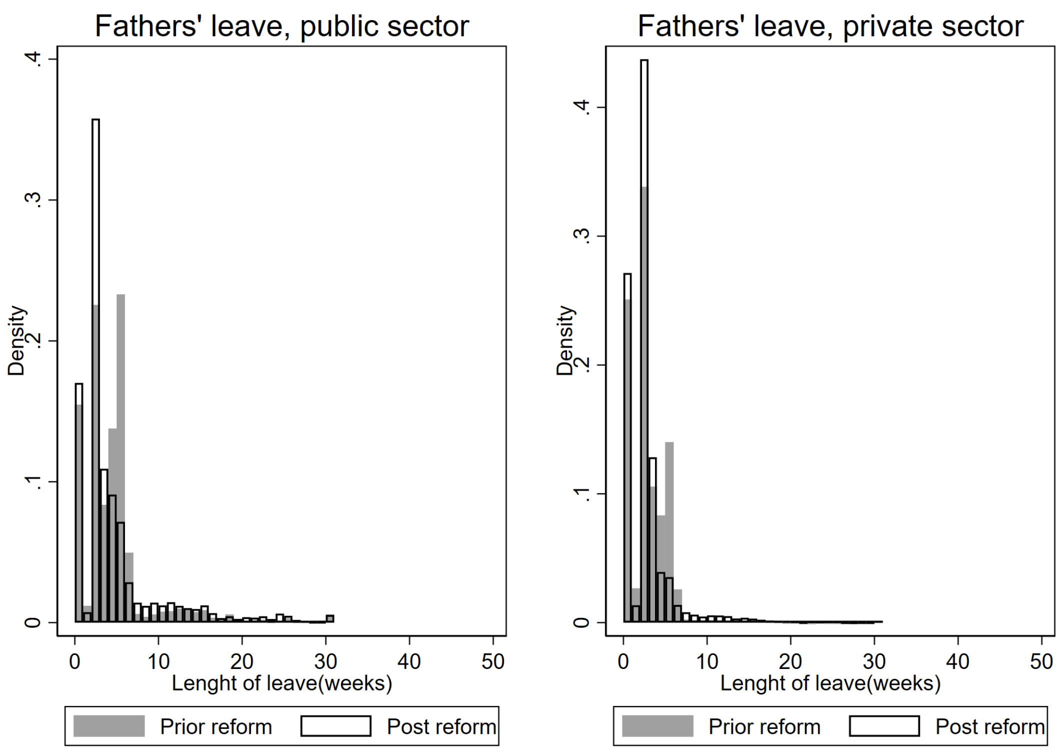


Figure 10: Sector split on fathers' leave



Appendix D: Regression output

TABLE D1: Reform effects on leave duration, effect from relative earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Mothers' leave duration (weeks)		Fathers' leave duration (weeks)		Fathers' taking long leave (dummy)	
VARIABLES	Baseline	Interaction	Baseline	Interaction	Baseline	Interaction
Reform effect	4.921***	4.715***	-0.136	0.0825	0.0163***	0.0383***
	(0.219)	(0.288)	(0.0830)	(0.127)	(0.00453)	(0.00711)
Interactions						
Prior to reform X		1.517***		-0.593***		-0.0374***
father earning most		(0.200)		(0.0925)		(0.00512)
Post refrom X		1.779***		-0.871***		-0.0653***
father earning most		(0.188)		(0.0911)		(0.00591)
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

Appendix E: Robustness

TABLE E1: Consumption externalities

	Excl. children born in 2002			Interaction w. same municipal		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	1st stage	ITT	2SLS	1st stage	ITT	2SLS
Reform/peer effect	6.421***	1.215**	0.189**	6.643***	1.375**	0.207**
	(0.757)	(0.596)	(0.0923)	(0.743)	(0.582)	(0.0876)
Prior to reform X				0.199	0.770*	0.729*
Living in the same municipal				(0.594)	(0.425)	(0.399)
Post reform X				0.785	-0.102	-0.264
Living in the same municipal				(0.552)	(0.419)	(0.435)
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