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Abstract

This paper studies how parental leave quotas may foster a more gender-equal division of parental responsibilities by increasing fathers' uptake of leave beyond the reserved amount. Specifically, the paper examines whether the introduction and expansion of 30-days parental leave quotas in Sweden generated spillover effects on male coworkers' leave-taking behavior. Using rich population register data and a regression discontinuity design, I find no evidence that the first quota introduced in 1995 affected male coworkers' uptake of parental leave. In contrast, the 2002 expansion of the quota led to a statistically significant increase of almost nine additional days of parental leave taken by male coworkers. The increase primarily occurred early in the child's life. As such, the increased uptake can be expected to contribute to a more equal division of parental responsibilities also in the long run. The absence of spillovers following the initial reform is consistent with the first quota being more distorting in nature and offering limited information about longer parental leave spells. The findings underscore the importance of societal context and policy design in shaping behavioral responses to parental leave reforms.

Keywords: Peer effects, Parental leave, Quotas, Co-workers

JEL Classification: J13, J16, J18, Z13

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1 Introduction

Fathers' uptake of parental leave continues to be low in most countries (OECD, 2016) and the uneven division of care-taking for children has been shown to explain much of the gender earnings gap (e.g., Bertrand et al., 2010; Angelov et al., 2016; Kleven et al., 2019). To encourage a more equal division of caring responsibilities between parents, the European Parliament mandated all member states in 2019 to reserve at least two months of paid parental leave for each parent (Directive 2019/1158). While the mandate led to the introduction of parental leave quotas in many European countries, the Nordic countries have a long history of parental leave quotas, starting in 1994 in Norway followed by Sweden the succeeding year. Since the introduction of the first Swedish quota in 1995, fathers' uptake of parental leave has increased steadily, but it remains unclear how much of this trend can be attributed to the quota policy itself.

Evidence from the introduction of the Norwegian quota shows that, beyond its direct effect, the reform also generated peer effects, leading to a further increase in the share of brothers and coworkers taking parental leave (Dahl et al. 2014). When few men take parental leave–as was the case in Norway before the reform–increased uptake of a peer can provide novel information and encourage marginal fathers in the network to take parental leave themselves. But what happens when most fathers already take some days of parental leave, can the direct effect of quotas still spill over and encourage fathers in the network to take longer parental leave?

In this paper, I investigate whether the introduction and expansion of parental leave quotas in Sweden each triggered spillovers at the workplace level, thereby increasing parental leave taken by male coworkers.[1] Already before the introduction in 1995, a majority of fathers took some parental leave and this rose to almost 90 percent by the extension in 2002 (own calculations), providing a setting substantially different from the Norwegian case studied in Dahl et al. (2014), and more similar to the western world today. Following the identification strategy first proposed by Dahl et al. (2014), I estimate spillovers on fathers' parental leave uptake using a Regression Discontinuity (RD) design. The *fathers* of interest are all covered by the quota, but differ in their random exposure to a *peer* at the workplace whose child was also covered by the quota. Focusing on the reduced-form estimates, the model captures the average indirect effect on fathers' uptake via peers, which adds to the direct reform effect when considering the overall impact on parental leave uptake. Parental leave is measured both in terms of the extensive margin, i.e., taking any parental leave, and the number of days of leave taken. The flexibility of the Swedish system also allows for an analysis of *when* the parental leave is taken.

The analysis shows no indication of spillovers from the first reform; the reduced form estimates are consistently insignificant. Meanwhile the second reform appears to have positive spillovers at the workplace. Although fathers working with a peer who was af-

[1] The study is restricted to opposite-sex parents defined from register data. Previous research finds significant differences in the division of labor after birth when comparing opposite-sex and same-sex couples (Evertsson et al., 2025

fected by the second quota were no more likely to take *any* parental leave, they took on average 8.8 more days of parental leave in the first two years of the child's life. The analysis suggests that the increased uptake in the first two years is reflecting a somewhat higher uptake overall, but also a reallocation of days to be taken when the child is younger. This is a potentially important effect since early parental leave is favorable for long-run effects on household responsibilities and subsequent labor market attachment, unlike parental leave taken at older ages to extend summer vacations, reduce working hours, and supplement household income (Duvander and Johansson, 2019). There are no significant differences in the peer effect when allowing for heterogeneity by peer, father or workplace characteristics.

Comparing the two reforms, the analysis suggests that the second reform was more successful at triggering a positive trend in fathers' involvement at a relatively young age of the child. A plausible explanation for the differential impact of the two reforms is that the extension provided better information about long spells of parental leave. Although the first stage in terms of days of parental leave taken by peers was similar across the reforms on average, the introduction of the first quota did not affect the intensive margin. In contrast, the second reform led to a substantial increase in leave-taking, extending even beyond the reserved days. Consequently, peers' parental leave behavior appears to have been more salient and informative after the second reform, which likely facilitated spillovers in the workplace. A contributing factor may be that men's demand for longer parental leave was likely higher in 2002 than in 1995, which would increase both the scope for peer effects to emerge and the likelihood that fathers respond to them. The presumed difference in demand may reflect both the design of the two reforms–where the first was more distorting (Avdic and Karimi, 2018)–and evolving gender norms. By 2002, it had become more socially accepted for fathers to take parental leave than in 1995, as suggested by higher average uptake among fathers and by attitudes recorded in the World Values Survey.

This paper contributes to the literature on spillovers from reserved parental leave policies. There is merely one previous paper estimating the spillovers on fathers, Dahl et al. (2014), who find substantial peer effects increasing the fraction of coworkers and brothers who take at least some parental leave. I add to the findings of Dahl et al. (2014) in primarily two ways. First, with a high share of fathers taking parental leave already before the Swedish reform, the findings are more informative about the effects of quotas introduced today–as it has become increasingly common for fathers to take at least some parental leave (Eurofound, 2019). For the same reason, the paper's second contribution is to examine the effects on the number of days of parental leave taken by fathers. The length of leave is the relevant margin of quota spillovers in many countries, yet indirect reform effects on fathers' leave days is previously unexplored. For mothers however, Lassen (2021) found a strong peer effect on the duration of leave for sisters in Denmark, and Welteke and Wrohlich (2019) found increased probabilities of staying home the first year among female coworkers in Germany. Another related paper, by Carlsson and Reshid (2022), use a "peer-of-peer" instrumental approach and find that norms are transmitted at the workplace, affecting parental leave uptake of both mothers and fathers in Sweden. Relative to the previous findings, the peer effect found in this paper following the second reform is large, and the differential impact of the two reform suggests that how and when quotas are implemented affects the scope for peer effects.

The first stage of both the introduction and the extension of Swedish parental leave quotas has been evaluated previously and the direct response is consistently strong (e.g., Eriksson, 2005; Duvander and Johansson, 2012; *Försäkringskassan*, 2019b). Further, the consequences of these reforms have been estimated for a wide range of outcomes.[2] While there is no evidence of the Swedish quotas improving gender equality in the longer run, reforms in other countries have typically found to have a positive impact on gender equality (e.g., Kotsdam and Finseraas, 2013; Druedahl et al., 2019; Patnaik, 2019).[3] The discrepancy makes Swedish quotas, and spillovers in particular, especially relevant for further study. Spillovers between peers following quotas have not been estimated in the context of Sweden before, yet the extent to which quotas trigger an increasing trend of fathers' involvement during infancy is presumably critical for the impact on gender equality in parenting. As many countries have or are in the process of implementing similar quotas, lessons from Sweden are relevant also internationally.

This paper proceeds as follows. Section 2 describes the institutional context. Section 3 presents the empirical strategy and Section 4 describes the data and descriptive statistics. The results are presented in Section 5, followed by Section 6 assessing the robustness of the findings. Section 7 studies heterogeneity and Section 8 concludes.

2 The institutional context

In 1974, the maternity leave insurance was replaced by the parental leave insurance, giving both parents equal rights to share the six months of paid leave. At that time, fathers took 0.5 percent of all parental leave days. Since the reform, the number of days with parental leave benefits available to both parents, as well as the fraction of days used by fathers, have increased gradually (*Försäkringskassan*, 2014). The total amount of parental leave benefits in 1995 (2002) was 450 (480) days. Out of these, 90 days are paid at a low flat rate, and the remaining days are income based. The income replacement is about 80 % based on capped income, which, in 1995, was binding for 12 percent of fathers and 4 percent of mothers (Ekberg et al., 2013). In addition to the benefits paid by the so-

[2]Employer responses (Ginja et al., 2023), human capital formation of children (Avdic et al., 2022), marital stability (Avdic and Karimi, 2018), fertility (Duvander et al., 2020), and mothers' sickness absence (*Försäkringskassan*, 2015). Regarding gender equality, Ekberg et al. (2013) estimated effects for household work (measured as care for sick children) and labor market outcomes, but found no robust effect from the first reform. Ekberg et al.'s (2013) findings are consistent with the insignificant effect on earnings found by Johansson (2010), Duvander and Johansson (2013), and Karimi et al. (2012), evaluating both reforms. Duvander and Johansson (2019) found a positive impact on fathers' share of care for sick children from the first reform (but not the second), although this is driven by a reduction of mothers' uptake rather than fathers' changed behavior. The different findings relative to Ekberg et al. (2013) are explained by differences in specification and time horizon for which outcomes are measured.

[3]See Canaan et al. (2022) for a review of the most recent literature.

cial insurance office, many employees are covered by collective insurances that increase the replacement during parental leave (Sjögren Lindquist and Wadensjö, 2005). Parents without a sufficiently high income receive a basic flat rate benefit (*Försäkringskassan*, 2022). In addition to the paid parental leave benefits, the parental leave scheme includes job protection for 18 months, and the sickness benefit qualifying income is maintained for 12 months irrespective of their use of parental leave benefits (*Försäkringskassan*, 2020). Consequently, parents who are willing to accept a lower replacement can disperse paid days to stay home longer, or save paid benefits to use when the child is older. It is possible to save days of parental leave benefits up to eight years, and on average, about 30 percent of the paid parental leave days is used after childcare enrollment to extend vacations or reduce working hours (Hall and Lindahl, 2018). Each parent is entitled to three periods of parental leave every year, if applied for at least two months in advance. At the time of birth, fathers are also entitled to ten days of temporary paternity leave, which is in addition to the 450 (480) days, and take-up rates of these have been at a constant high rate (own calculations, not included in the main analysis).

In 1994, the year before the first parental leave quota, 54.5 percent of all fathers took some parental leave, and the average number of days was 42.5 (by age eight, own calculations). In May 1994, the government bill targeting gender equality was passed and changes to the parental leave insurance were implemented on January 1, 1995. The reform aplied to all children born since this date. In addition to improve the labor market outcomes of mothers, a stated purpose of the reform was to foster close attachment between the child and both parents.[4] The new law stipulated that 30 days of the 360 days of parental leave with income replacement should be reserved for each parent. As this was more often binding to fathers, this reform has been referred to as a ”daddy-month” reform. The replacement rate was also lowered to 80 percent, except for the quota days, which remained at 90 percent.[5] Since 1998, the quota days were replaced at the same level as the remaining income based parental leave benefits at 80 percent until 2001, when the effective replacement rate was slightly reduced to 77.6 percent. The first quota month introduced in 1995 was followed by a second month in 2002.[6] Before the second reform, 88.8 percent of all fathers took some parental leave, and the average number of days was 68.9 (by age eight, own calculations). The second parental leave quota was accompanied by a corresponding increase of total days and the replacement rate was unchanged (*Försäkringskassan*, 2014).

3 Empirical strategy

The empirical strategy is based on Dahl et al. (2014), making use of pre-existing networks at the workplace to capture reform spillovers in a Regression Discontinuity (RD) design.

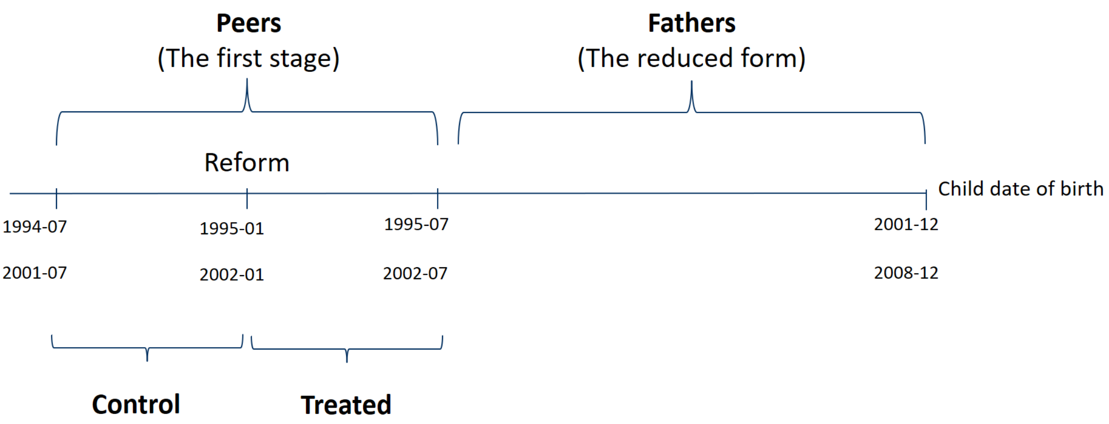
[4]Prop. 1993/94:147.

[5]Children born before the reform received the higher rate of parental leave benefits for days taken within two years (Ekberg et al. 2013).

[6]Prop. 2000/01:44, agreed upon on March 22.

Exogenous assignment of treatment to members in pre-existing peer groups efficiently deals with the problems related to estimation of peer effects raised by Manski (1993); the reflection problem, correlated unobservables and endogenous group membership. Random assignment of treatment deals with the first two problems and defining the group before the reforms effectively rules out endogenous group formation. Averages and functional form of pre-characteristics may differ on each side of the cutoff due to strategic planning of conception, but close to the threshold date of birth should be random such that the RD-estimates capture the causal effect of quotas. I apply a reform window of six months on each side of the date of implementation, January 1, 1995 and January 1, 2002, respectively (see Figure 1). The size of the reform window affects the composition of fathers at the included workplaces, thus methods to define the optimal bandwidth are not entirely applicable in this setting (but applied in the sensitivity analysis). Instead, I use the same six-month reform window in my main specification to match the previous study of reform spillovers for fathers by Dahl et al. (2014).

Figure 1: Timeline



Notes: Timeline of when children of peers and fathers are born for the first and second reform. The treatment status of both peers and fathers is determined by the birthdate of peers.

Males whose child is born within the reform window are referred to as *peers* and they are directly affected by the reform. The treatment status is based on the date when their child is born relative to the reform date, i.e., January 1, 1995 and 2002, respectively. Relative to control peers, treated peers are assigned 30 days of parental leave benefits that cannot be transferred to the mother and consequently are forfeited if not used by him. The spillovers of interest are measured for male coworkers whose child is born after the reform window (June the year of implementation) and within six years. These are referred to as *fathers* and are all covered by the quota. The Fathers differ only by the treatment status of their peers. Thus, the reduced form effects are driven by reform induced changes in the parental leave uptake of peers at the workplace.

I estimate the following regression discontinuity models:

$$PL_{j,g} = \alpha_j + 1(t_j \geq c)\Big(f_l(t_j - c) + \lambda\Big) + 1(t_j < c)\Big(f_r(c - t_j)\Big) + \beta X_g + e_{j,g} \tag{1}$$

$$PL_{i,g} = \gamma_i + 1(t_j \geq c)\Big(h_l(t_j - c) + \pi\Big) + 1(t_j < c)\Big(h_r(c - t_j)\Big) + \beta X_g + e_{i,g}, \tag{2}$$

where peers are denoted j and fathers are denoted i, employed at the same workplace g. The outcome of interest is parental leave uptake, denoted *PL* in the specification. Parental leave uptake is measured both as the extensive margin, and in terms of days with parental leave benefits. Moreover, different measures of parental leave are introduced to account for when parental leave is taken relative to the birth of the child. The cut-off—January 1, 1995 for the first reform and January 1, 2002 for the second—is indicated by c. t_j is the date of birth of the peer's child. The sign of the difference $t_j - c$ indicates treatment status, such that positive (negative) implies treated (control). f_l, f_r, h_l, and h_r are quadratic functions on each side of the cut-off. X_g captures fixed effects for the birth year of the father's child, as well as group specific covariates: share of males, number of employees, municipality fixed effects. For precision, I also include the predicted parental leave uptake of fathers and peers, accounting for possible imbalance in individual characteristics. Each workplace appears in the sample only once, thus there's no need to adjust for workplace fixed effects. Standard errors are robust to heteroskedasticity but in line with recent recommendations with a discrete running variable (Kolesar and Rothe, 2018), I deviate from the specification of Dahl et al. (2014) and do not cluster the standard errors in the main estimations. In the robustness section I show that the estimates are robust to clustered standard errors.

The coefficient λ is the first stage estimate, and captures the direct reform response in parental leave among peers. The reduced form estimate, π, captures the corresponding indirect effect on fathers with a treated peer, compared to those with a peer whose child was born before the reform. Consequently, the reduced form estimate captures the average effect of working with a peer for whom 30 (more) days of paid parental leave benefits are reserved to each parent. The identifying assumption is the independence assumption, which requires that the assignment variable (the date of birth of the peer's child) is non-manipulable and as good as random.

The reduced form estimate (equation 2) divided by the first stage (equation 1) gives the fuzzy RD (2SLS) estimation of the peer effect, as reported by Dahl et al. (2014). The estimate captures the *marginal* effect of increased peer parental leave–i.e., the father's response to a one-day increase in the uptake of the peer. However, this estimate relies on additional assumptions that are not credibly fulfilled in the Swedish setting. In particular, the exclusion restriction, which in this setting requires that fathers are affected only via the increased parental leave uptake of the peer. As Ekberg et al. (2013) have shown, the reforms not only affected the amount of parental leave but also when parental leave is taken. In addition, the first reform affected also the replacement rate, while the second reform increased the total days of parental leave available. Both features can affect the potential spillovers via, e.g., age at childcare enrollment.

4 Data and descriptives

The empirical analysis is based on data from several Swedish registries and individuals are linked by unique identifiers. The population of interest is children born between 1994 and 2008, and their fathers. Fathers (and mothers) are linked to children in the Multi-Generation Register where all biological and adoptive links are mapped. To this dataset, I add parental characteristics from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA) by Statistics Sweden, which covers everyone above the age of 15 registered in Sweden. From LISA I retrieve information about characteristics such as immigrant status, education, employment, and income both before and after birth of the child. The workplace identifiers enable the matching of peers and fathers. The workplace identifiers are also used to construct the workplace controls—i.e., the total number of employees and the share of men in the workplace the year before the reform.

The outcome of interest is the parental leave uptake of fathers. The measures of parental leave are based on data from the parental leave registry from the database MiDas provided by the Social Insurance Office (*Försäkringskassan*). The dataset contains date of birth and information about parental leave by child and beneficiary (most often the parents). The information includes days of paid leave, benefit amounts, replacement rates and exact dates of constructed spells. The spells consist of paid and unpaid days that are assessed to constitute a cohesive period of parental leave and is typically the time period covered by one application of benefits, allowing for at most 6 days of unpaid leave between paid days of parental leave (See Duvander (2013) for a discussion of the measure).

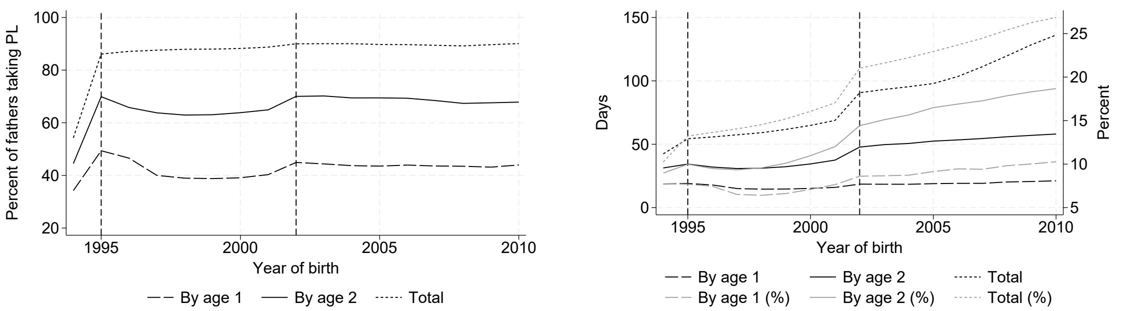
To capture individual characteristics of fathers and peers in one composite measure, parental leave uptake is predicted using parental leave uptake for children born 2 years before the reforms (i.e., 1993 and 2000 respectively). The predicted measure refers to fathers' total days of parental leave and the measure is predicted using their individual characteristics in terms of age, municipality, marital status, immigrant status, education (3 levels), earnings and disposable income, professional position, industry and sector.

4.1 Measures of parental leave

Several measures for parental leave are constructed using the parental leave registry, all of which are reported in terms of extensive margin as well as the number of days. Because Swedish parental leave benefits can be used quite flexibly, the uptake can be characterized in terms of when they are used. Uptake of parental leave benefits relative to the child's age is constructed using the child's exact date of birth. First, I measure parental leave in the first year of a child's life, and in the first two years. Because children are typically enrolled in childcare around 18 months of age (Duvander, 2006), parental leave before age two is likely to reflect a cohesive period of being the main caregiver. As such, these two measures are capturing parental leave uptake especially favorable for parent-child attachment, and where one could expect effects on long-run outcomes in terms of household responsibilities and subsequent labor market attachment (Duvander and Johansson, 2019). While parental leave in the first year is presumably more impactful in terms of long-run outcomes, it might not fully capture parental leave of fathers which typically occurs towards the end of period of parental care, after the mother (Boye and Evertsson, 2018). Therefore, the two measures of early parental leave both provide valuable information about the parental leave uptake, and it is also the relevant time window for implications to countries with less flexible parental leave schemes (e.g., Iceland, Finland, and Norway) (OECD, 2021). I also report the total number of paid days by age eight, and I construct two measures to identify parental leave likely used to reduce working hours, supplement income, and extend vacations; parental leave taken during weekends, and dispersion of parental leave. Weekends are defined as 1–3 days of benefits, with a majority of days allocated to the weekend. Dispersion is measured as the number cohesive episodes of parental leave.[7] In contrast to the measures of parental leave taken early in the child's life, these measures capture parental leave less in line with the stated intent of the reforms, i.e., to improve mothers' labor market outcomes and foster close attachment to both parents.

Figure 2: Parental leave of the full population of fathers

(a) Extensive margin (b) Days of parental leave



Notes: The uptake of paid parental leave of fathers in the first two years, the first one year, and in total (by age eight), extensive margin (panel a) and number of days (panel b). Percent in panel b is relative to the total household uptake. Vertical lines indicate the first and the second reform.

The measures of parental leave at different ages of the child are not all constant across time. Panel A of Figure 2 shows that for the full population of children born between 1994 and 2010, the extensive margin is relatively constant since the first reform in 1995, at least when considering the total uptake (by age 8). For shorter time spans, there is a decline in the share of fathers taking any leave after 1995. As the total uptake was constant, this is reflecting more fathers taking parental leave when the child was older, but this is reversed by the second reform in 2002.

Considering instead the amount of parental leave in Panel B of Figure 2, there is an overall positive trend in the uptake of fathers for all these measures, both in terms of the

[7]To construct the number of episodes, I collapse episodes (including unpaid gaps of at most 6 days) that are reported separately, but between which the unpaid gap is fewer than 5 days.

number of days (in black) and the percent of the household total (in gray). The question of interest is to what extent this trend can be attributed to the quotas. The amount of leave taken by fathers jumps as the quotas were introduced, but the reform effect is more pronounced for the total uptake–suggesting that some of the reform-induced increase is taken after the child's second birthday. For parental leave at a relatively young age, there is an increasing gap such that the share of leave taken by males is increasing more than the absolute number of days. In fact, the number of days taken by men in the first year is almost constant across the time period, meanwhile the share is increasing. The divergence between the two measures in Panel B of Figure 2 is driven by fewer days of paid parental leave taken by mothers, reducing the uptake of both parents combined. For total parental leave, the difference between relative and total increase is fairly constant since the second reform.

4.2 Sample restrictions

Reform spillovers are estimated for the first father at the workplace to have a child born after the reform window. Given the possible dynamic effects at the workplace in terms of competition as proposed by Johnsen et al. (2020), subsequent fathers are not included in the analysis. In the main analysis, sampled fathers have a single peer at the workplace whose child is born in the reform window which is one year centered around the reform. Because the reforms may affect workplace mobility, the main specification only requires the father and the peer to both work at the workplace the year before the reform.[8] There is no restriction on the parity of the child of either peer or father. However, to focus on spillovers on colleagues, pairs where the father is coded as a business owner are removed. Although the sampling restrictions imply that few large workplaces are included, I also impose a restriction of at most 100 employees in each workplace to target workplaces where peers and fathers are more likely to interact. The main results are also presented for workplaces with fewer than 50 employees.

Given the width chosen for the reform window, children of peers are born within 6 months of the reform. There is no implied restriction on the year of birth for children of the fathers. The main analysis includes births in the six years following the reform. That is, for the first reform children are born between 1995 and 2001 and for the second reform the corresponding range is between 2002 and 2008.[9] A sensitivity analysis shows that the results hold also for a shorter gap.

[8] Additional analysis estimates mobility directly and impose stronger restrictions on the peers and fathers working together, confirming the main findings.

[9] There is a strong correlation between the number of employees at the workplace and birth year of the first father; fewer employees imply a longer average gap between the births.

Table 1: Descriptive characteristics

	(1)	(2)	(3)	(4)
	First reform		Second reform	
	Out of sample	Sample	Out of sample	Sample
Panel A: Workplace characteristics				
Share of males	0.635	0.734	0.635	0.741
Number of employees	16.301	24.141	14.979	25.147
Observations	418,795	9,040	462,661	9,808
Panel B: Peer characteristics				
Income	1,489	1,880	2,211	2,475
Age	31.568	31.422	32.705	32.178
Married	0.466	0.450	0.425	0.407
University education	0.188	0.144	0.255	0.199
Immigrant	0.192	0.072	0.219	0.098
Share taking any parental leave (Control)	0.527	0.576	0.882	0.943
Share taking any parental leave (Treated)	0.856	0.928	0.895	0.950
Average days of parental leave (Control)	40.448	39.824	69.896	71.109
Average days of parental leave (Treated)	54.299	56.381	89.434	91.047
Observations	96,492	9,067	84,782	9,838
Panel C: Father characteristics				
Income	1,318	1,782	2,000	2,381
Age	28.628	29.624	29.521	30.614
Married	0.235	0.295	0.201	0.260
University education	0.167	0.140	0.262	0.200
Immigrant	0.202	0.070	0.236	0.094
Share taking any parental leave	0.873	0.936	0.893	0.948
Average days of parental leave	60.528	58.368	102.628	98.820
Observations	587,625	9,067	678,405	9,838

Notes: Mean values out of sample in columns 1 and 3, in the analysis sample in columns 2 and 4. Out of sample refers to the universe of workplaces, peers and fathers within the relevant time frames, that are not sampled. I.e., workplaces the year before each reform, peers whose child is born within the reform window, and fathers whose child is born after the reform window (at most 6 years after the reform). Income is reported in hundreds of SEK, adjusted to the consumer price index in year 2000.

The sample restrictions yield an analysis sample of workplaces that are non-representative of the full population, but constant over the two reforms. There is a variety of sectors included with a fair representation of male workplaces (construction, consultancy/business, and wholesale being in the top). Panel A of Table 1 shows that the percentage of males in sampled workplaces is about 10 percentage points higher than non-sampled firms, which is reasonable given that all female workplaces are removed. Similarly, sampling affects the average number of employees as few small workplaces are included, and single-employee workplaces are removed entirely. In both samples, the average number of employees is about 10 more than in non-sampled workplaces.

Similarly, sampled peers and fathers differ from those men that are not included in the sample, but have a child in the same time frame. Panel B and C of Table 1 show that sampled men have a higher income and are less likely to have an immigrant background, but are less likely to have a university education. The parental leave uptake is similar, and so is the peer response (comparing the *Treated* to the *Control*). Comparing the first and second reform, there are significant differences between the two samples for both peers and fathers. However, the differences are consistently small and do not indicate that either was better off overall.

4.3 The independence assumption

For the RD-design to consistently estimate the reduced form, the independence assumption must be satisfied. That is, the treatment status should be as good as randomly assigned, making the treated and controls comparable in all dimensions except for the treatment status.

First, there can be no manipulation of treatment status such that peers have planned the birth or conception relative to the reform. For the first reform there was an incentive to give birth before the reform. The reserved days were re-allocated from the shared leave and there was a coinciding decrease in replacement rate. However, parents were unable to time conception accordingly as the government bill was passed less than 9 months before implementation. Moreover, both C-sections and induced labor were rare at that time (Ekberg et al., 2013). For the second reform, the post period was advantageous since also the total number of days available increased by 30 days. The second reform was agreed upon almost exactly 9 months before implementation, leaving a small window of merely a few days where forward looking couples could respond and delay conception. However, this does not seem to be a concern. The densities in peer births reveals no significant imbalance, more children were born after the reform (see McCrary test in Appendix Figure A1), but this is similar across reforms despite opposite incentives. Thus, there is no reason to believe that the imbalance reflects manipulation of the treatment status. Instead, the corresponding densities of the placebo years suggest that the imbalance reflects a repeated seasonal pattern in timing of birth as the cutoff coincides with the turn of the year and the main specification is carried out using all observations.

Second, the independence assumption requires that there is no other discontinuity at the cutoff. The date of implementation does not coincide with any nation-wide reforms affecting the parental leave uptake of the studied fathers.[10] However, exposure to treated peers could be non-random if the timing of birth among peers correlates with workplace and father characteristics. The control group consists of peers whose child is born in the fall while treated peers have their child in the spring, and previous research has shown that timing of birth correlates with household characteristics; particularly, mothers of higher socioeconomic status tend to give birth in the spring (Buckles and Hungerman, 2013).

[10]In 2001-2003 there were several reforms changing the childcare system to increase accessibility and lower fees (Maxtaxa och allmän förskola m.m., proposition 1999/2000:129). However, this affected children of childcare age and did not have a systematically different impact on children born on either side of the cutoff.

This speaks in favor of the RD specification compared to the alternative Difference-in-differences specification, as the RD assigns a higher weight to births close to the cutoff.

To assess the concern that peers–and consequently workplaces and fathers–are different on each side of the cutoff, it is common to estimate the model for pre-determined characteristics. Table 2 presents the balance test, estimating the reform effect on features of the workplace (column 1-3), the peer (column 4) and the father (column 5). Insignificant estimates in Table 2 would indicate that the sample is comparable on each side of the cutoff, suggesting that any estimated effects can be attributed to the reform. As can be seen, this is true for the first reform where all estimates are small and statistically insignificant. Importantly, the predicted parental leave uptake of fathers is similar across treatment status for both reforms, meaning that they do not differ in terms of individual characteristics that are typically correlated with parental leave uptake. There is indication of workplaces treated by the second reform having a larger proportion of males. The estimate is relatively small, 3 percent, and significant only at the 10 percent significance level thus it does not pose a severe threat to the identification.

Table 2: Balance of workplace characteristics

	(1) Spacing	(2) Male share	(3) Size	(4) PL peer	(5) PL father
Reform 1995					
Treated	-0.076	-0.015	-1.255	-1.177	1.166
	(0.116)	(0.017)	(1.473)	(10.704)	(8.389)
Observations	9,410	9,410	9,410	9,410	9,410
Reform 2002					
Treated	0.004	0.030*	-0.981	-0.022	0.064
	(0.110)	(0.016)	(1.451)	(0.824)	(0.819)
Observations	10,051	10,051	10,051	10,051	10,051

Notes: Estimates from separate RD regressions on workplace characteristics. Spacing refers to the time between the birth of the peer's child and the father's child. Male share is the share of males at the workplace. Size is the number of employees at the workplace. PL peer and PL father refer to days of paid parental leave as predicted by their individual characteristics. All estimations include separate quadratic trends, triangular weights and a bandwidth of 6 months. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

To conclude, there are no serious concerns regarding the reduced form estimation. There is no theoretical reason to suspect manipulation of treatment status, nor does the data reveal any such indications. All measures in Table 2 are included as controls in all estimations to address the small imbalance.

5 Results

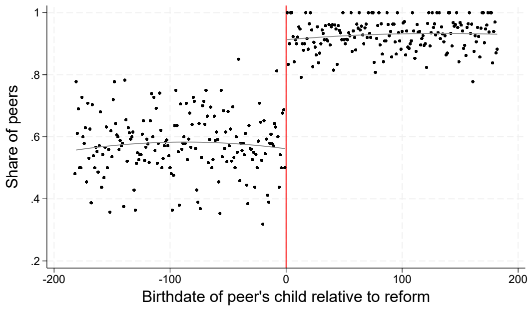
The primary interest of this paper is whether the parental leave quotas triggered a trend of increasing uptake of parental leave among subsequent fathers. Focusing on peer effects at the workplace, the first set of results verifies that the sampled fathers were in fact exposed to increased parental leave uptake of a peer at the workplace. Next, the reduced form estimations reflecting the peer effect, are presented.

5.1 The first stage

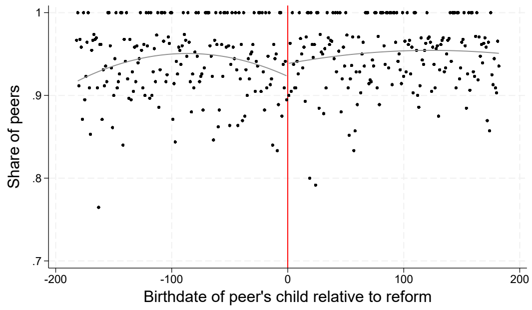
While the previous literature has shown that the overall response to the quotas was strong (e.g., Avdic and Karimi, 2018), this section characterizes the peer response in the non-representative sample of workplaces. The total parental leave uptake of peers is presented in Figure 3. The figure shows the raw average by the birth date of their child, together with the fitted line. As can be seen in the top panel, the first reform increased the share of peers taking parental leave to more than 90 percent. By the time of the second reform, the level was even higher before implementation, and the extended quota did not significantly affect the extensive margin. This is consistent with Aldén et al., (2025), showing that there is a small but relatively stable fraction of fathers since 1995 who takes no parental leave. The number of paid days displayed in the lower panel of Figure 3 instead shows significant increases of similar magnitude, about 20 days, from each reform.

Figure 3: Parental leave in total among peers

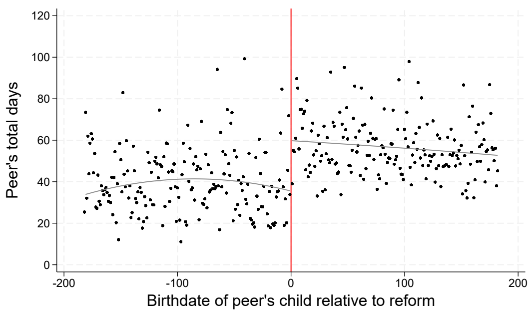
(a) First reform
Extensive margin



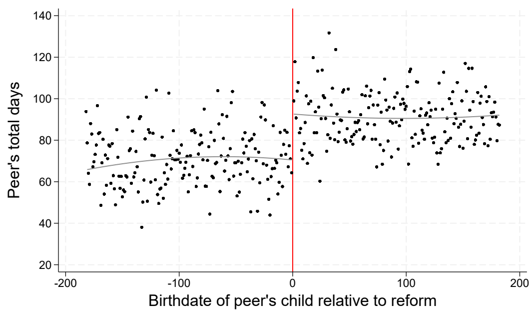
(b) Second reform
Extensive margin



(c) First reform
Days of parental leave



(d) Second reform
Days of parental leave



Notes: The top panel shows the share of peers taking parental leave and the bottom panel shows the average days of parental leave taken by peers, by date of birth of the peer child (normalized to the date of implementation). The vertical line indicates the date of the reform, January 1, 1995 for the left panel and January 1, 2002 for the right panel. All figures refers to parental leave taken in taken in total (until the child is 8 years old). The fitted line is a second order polynomial.

Table 3 reports the RD-estimates of the first stage, characterizing when and how the peers' uptake of parental leave was affected. The top panel presents the estimates for the extensive margin effects. As was visible in Figure 3, there was a sharp increase in the share of peers taking any parental leave following the first reform, while the second reform had no similar effect–largely explained by the high baseline. Already when the reform was first implemented in 1995, almost 60 percent of the peers in the sample took some parental leave and more than 90 percent took some parental leave by the time of the second reform. Focusing on parental leave taken early in the child's life in columns 2 and 3, both reforms significantly increased the extensive margin, albeit the first reform had a greater impact. The extensive margin effects reveal that the first reform induced peers to take any parental leave, and most of them did so before the child turned two years old. The second reform instead induced peers who would have taken parental leave either way, to do so earlier. There is also an increased uptake of parental leave during weekends, especially following the first reform where the incidence doubled.

For the number of days with parental leave benefits presented in the lower panel of Table 3, there is a substantial increase in the total uptake following both reforms. Peers whose child was born after the first (second) reform took on average 24 (23) more days compared to those whose child was born before the reform, meaning that almost all 30 days that was reserved were eventually spent. A slightly larger impact on the days of parental leave benefits following the first reform is consistent with the existing literature. However, previous estimates are smaller which is explained by the selected sample in the analysis at hand.[11]

Characterizing the increased parental leave uptake in terms of when and how the days are spent, there are observable differences between the two reforms, but it is not clear from Table 3 that one would be more favorable for peer effects. Event though both reforms significantly increased the uptake before the child's second birthday–about half of the days are taken within this time frame–the relative increase is larger for the first reform (42 percent compared to 27 percent). The first reform also had a significant effect on parental leave before age one, while the extension in 2002 did not affect this margin. On the other hand, the first reform significantly increased the days allocated to weekends and the estimate for spells is larger. Parental leave taken when the father would not otherwise work or dispersed across several spells is presumably less visible at the workplace, and consequently less likely to trigger peer effects among coworkers.

[11] Previous literature has shown that for all fathers, the first stage increase in parental leave is 4.9 and 3.4 days, respectively, in the first 17 months (Eriksson, 2005), 9.9 and 4.4 days, respectively, in the first two years (*Försäkringskassan*, 2019b) and about 15 days in the first eight years (Ekberg et al., 2014; Avdic and Karimi, 2018).

Table 3: First stage regression estimates

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
Panel A: Extensive margin					
First reform	0.354***	0.253***	0.342***	0.123***	-
	(0.029)	(0.035)	(0.032)	(0.026)	-
Baseline	0.576	0.373	0.485	0.124	-
Observations	8,987	8,987	8,987	8,987	-
Second reform	0.015	0.093***	0.078**	0.053*	-
	(0.018)	(0.035)	(0.031)	(0.032)	-
Baseline	0.943	0.470	0.738	0.293	-
Observations	9,754	9,754	9,754	9,754	-
Panel B: Days of PL					
First reform	24.109***	6.420**	14.731***	0.703**	2.677***
	(4.476)	(2.873)	(3.968)	(0.290)	(0.301)
Baseline	39.824	17.862	29.780	0.941	1.967
Observations	8,987	8,987	8,987	8,987	8,987
Second reform	23.272***	3.100	11.050**	0.293	1.728***
	(4.697)	(2.741)	(4.374)	(0.278)	(0.396)
Baseline	71.109	16.609	40.406	1.717	5.572
Observations	9,754	9,754	9,754	9,754	9,754

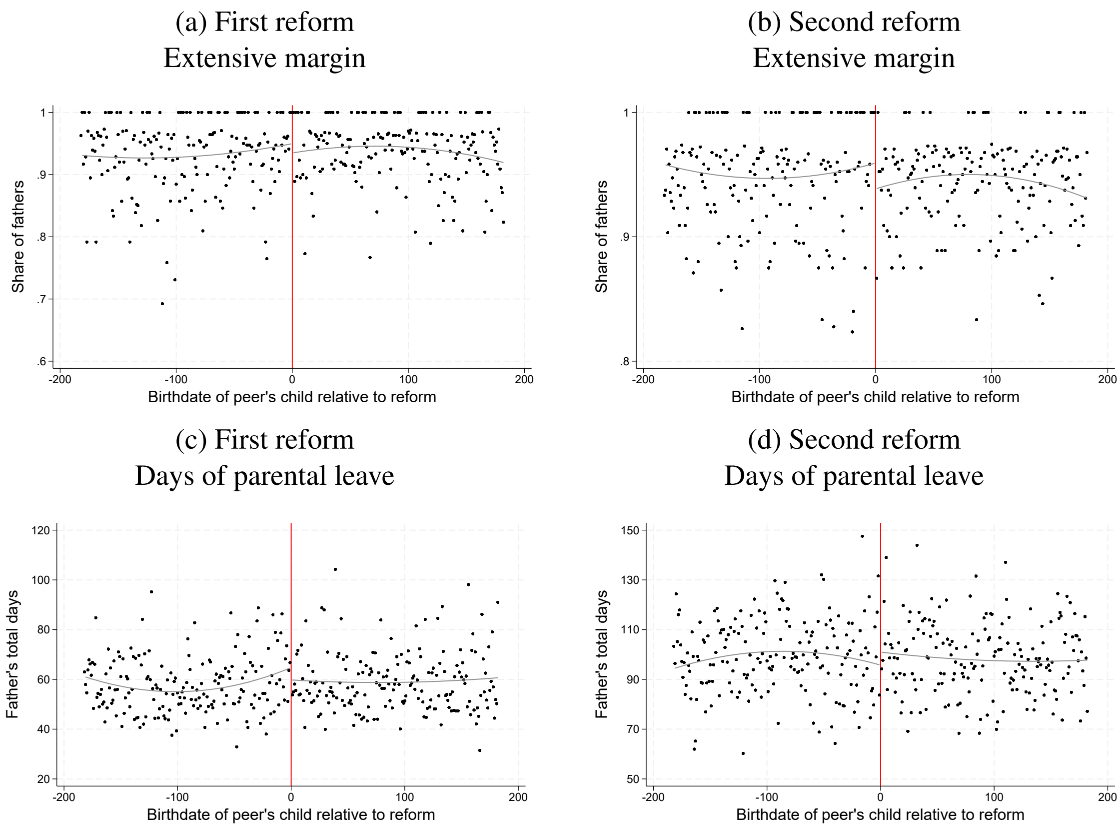
Notes: Estimates of the first stage from separate RD regressions for the first and second reform, on the share of fathers taking parental leave (top panel) and the days of parental leave taken by fathers (bottom panel). Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

To sum the analysis so far, both reforms substantially affected the uptake of parental leave among peers whose child was born after the reform, and the results in Table 3 does not identify one reform as more likely to trigger peer effects at the workplace than the other. A complementary measure of the first stage is the intensive margin response i.e., the average number of days among those who take at least some parental leave. Appendix Table A1 shows that the increased uptake among peers following the first reform did not translate into a significant increase of the average uptake among takers. Instead, most of the effect of the first reform is reflecting more peers using 21-40 days, which is low relative to the intensive margin baseline of 69 days (Appendix Table A3). Thus, the first reform provided novel information about taking the quota, but it was less informative about longer spells of parental leave. Meanwhile, the effect of the second reform was primarily increasing the intensive margin, and the higher uptake of parental leave among peers is visible also for days exceeding the quota of 60 days. For instance, peers whose child was born after the second reform were 4.6 percent more likely to take more than 90 days, which is more than a doubling relative to the baseline of 3.4 percent (Appendix Table A3). Taken together, a closer examination of the first stage indicates that the second reform provided more information about longer spells of parental leave, which is the margin that would primarily be affected by a peer in the Swedish context.

5.2 Workplace spillovers

In this section, focus is shifted to the spillovers of interest. Figure 4 shows the parental leave taken by fathers, by the date of birth of their *peer's* child. In line with the first stage variation, the top panel of Figure 4 shows that the extensive margin for fathers–who all have their child in the post period–is close to one. Since nearly all fathers on the left of the cutoff take some parental leave, there is little room for meaningful positive peer effects on the extensive margin. The bottom panel of Figure 4 shows instead the effect on the number of days taken by fathers. While the trend is slightly different on each side of the cutoff, with a tendency for a drop (jump) for the first (second) reform, the figure shows no stark discontinuity for either reform.

Figure 4: Parental leave among fathers



Notes: The top panel shows the share of fathers taking parental leave and the bottom panel shows the average days of parental leave taken by fathers, by date of birth of the peer child (normalized to the date of implementation). The vertical line indicates the date of the reform, January 1, 1995 for the left panel and January 1, 2002 for the right panel. All figures refers to parental leave taken in total (until the child is 8 years old). The fitted line is a second order polynomial.

Table 4 presents the corresponding reduced form RD-estimates in column 1, along with the measures characterizing the uptake of the father. Although peers are unlikely to increase the share of fathers taking any parental leave given the high baseline, revealed penalties at the workplace could affect the extensive margin by reducing participation. It is also possible that there is a peer effect in terms of when parental leave was first taken. However, the extensive margin estimates presented in the top panel of Table 4 are insignificant throughout, for both reforms. The one exception being column 2, suggesting a decline in the share of fathers taking parental leave before age one if their peer was entitled to the first quota. The estimate is however significant only at the 10 percent level. Overall, insignificant–and sometimes negative–estimates indicates that the reform-induced parental leave among peers at the workplace did not encourage more fathers to take parental leave, irrespective of the time frame considered.

Turning to spillovers in terms of the days of parental leave taken by fathers, the RD-estimates in the bottom panel of Table 4 are negative for the first reform, and positive for the second. However, as was shown in Figure 4, the estimates on the total uptake are imprecise. Although the magnitude is relatively large for each of the reforms; -5.5 and 6.7 respectively, the estimates are insignificant. Nor did either reform increase fathers' parental leave during weekends or the number of spells, which reflects a more evasive type of parental leave. Instead, there is indication of the second reform increasing the number of days taken early in the child's life, and in particular in the first two years–the type of parental leave from which we are more likely to expect effects on continued child rearing responsibilities. The estimate of almost 9 days is significant at the 5 percent level, and statistically different from the corresponding estimate of the first reform. The effect is also reflected in the ratio of parental leave taken by fathers in the first two years. As can be seen in Appendix Table A2, fathers' share of the total household uptake in the first two years increased with 3.6 percentage points, corresponding to 22.5 percent, suggesting that they substantially increase their participation early in the child's life. The share of the total uptake by age 8 has a smaller and marginally significant estimate of 2.1 percent, consistent with the imprecise estimate in column 1 of Table 4. However, evaluating the days of paid parental leave taken by fathers after age 2, there is no significant decline (Appendix Table A2). Thus, the evidence supports the interpretation that fathers whose peer was covered by the second quota increased their uptake in the first two years, reflecting both a reallocation of when parental leave is being used, and a somewhat higher uptake overall.

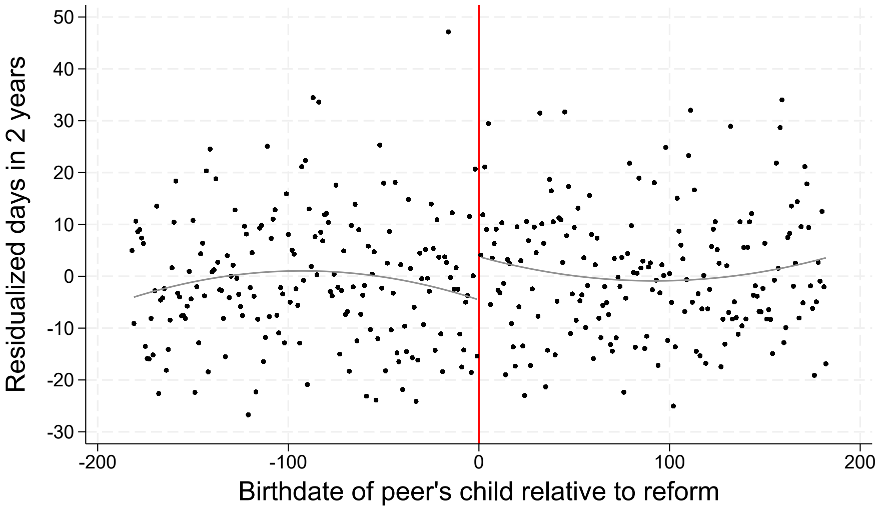
Table 4: Reduced form effects on fathers

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
Panel A: Extensive margin					
First reform	-0.015	-0.065*	-0.024	0.016	-
	(0.016)	(0.035)	(0.029)	(0.031)	-
Baseline	0.934	0.540	0.776	0.235	-
Observations	8,987	8,987	8,987	8,987	-
Second reform	-0.022	0.025	0.019	-0.013	-
	(0.015)	(0.035)	(0.028)	(0.034)	-
Baseline	0.949	0.532	0.806	0.375	-
Observations	9,754	9,754	9,754	9,754	-
H_0: Equality of reform effect	0.8	0.05	0.3	0.5	-
Panel B: Days of PL					
First reform	-5.511	-2.419	-4.389	-0.101	-0.014
	(4.292)	(2.576)	(3.621)	(0.267)	(0.362)
Baseline	57.682	17.895	34.955	1.355	4.948
Observations	8,987	8,987	8,987	8,987	8,987
Second reform	6.717	4.693*	8.802**	0.337	-0.139
	(4.878)	(2.760)	(4.228)	(0.480)	(0.515)
Baseline	99.428	19.915	53.367	2.969	8.057
Observations	9,754	9,754	9,754	9,754	9,754
H_0: Equality of reform effect	0.07	0.1	0.04	0.5	0.9

Notes: Reduced form estimates from separate RD regressions for the first and second reform, on the share of fathers taking parental leave (top panel) and the days of parental leave taken by fathers (bottom panel). Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Equality of reform effects is tested by running a pooled RD-model, fully interacted. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The significant jump in parental leave uptake in the first two years after the second reform is visualized in Figure 5. Each dot represents the average residualized uptake by the birthdate of the peer, net of controls. As can be seen, the jump at the cutoff is not sharp. There is substantial variation in the uptake among fathers, yet there is a visible increase in the average days among fathers whose peer is born after January 1st, 2002.

Figure 5: The effect of the second reform on the fathers' days of parental leave



Notes: The figure shows the residualized days of parental leave taken by fathers in the first two years, by date of birth of the peer child (normalized to the date of implementation). The residuals are obtained by running the specification without the treatment indicator. The vertical line indicates the date of the reform, January 1, 2002. The fitted line is a second order polynomial.

Further analysis indicates that the significant peer effect is not restricted to any specific part of the parental leave distribution. The increase is visible throughout, but somewhat larger towards the upper end i.e., 90 days or more, see Appendix Table A4. For the first reform however, there is a significant reduction in fathers taking more than the reserved amount of 30 days. Thus, if anything, it seems that fathers whose peer was entitled to the first quota, took less parental leave themselves and in particular, they were discouraged from taking more than the quota. The negative effect is consistent with the first reform revealing a penalty for fathers taking parental leave that otherwise would not have been exposed, but the estimates are too imprecise for such conclusions.

5.2.1 Discussion

The insignificant extensive margin effects differ from the findings of substantial spillovers following the Norwegian quota in Dahl et al. (2014). Further analysis in Appendix Tables A8 and A9 replicates the extensive margin estimations in Dahl et al. (2014) for total uptake and in the first two years. The Appendix tables also include spillovers between brothers, and the analysis reveals that the discrepancy to Dahl et al. (2014) is not driven by adjustments in the specification, nor is it specific to workplace spillovers. Instead, the lack of peer effects is more plausibly explained by Sweden's high baseline already before introduction of the first quota, implying a different context in terms of both attitudes and information.

As for the peer effect in terms of days with parental leave, an explanation for the differential impact of the two reforms could be that different types of peers responded to the first and second reform, possibly translating into different potential for spillovers. For instance, reports characterizing the reforms have shown that the impact of the introduction was larger among lower income- and education groups, while the middle class responded stronger to the extension in 2002 (e.g., *Försäkringskassan*, 2019b). If some individual characteristics are more or less favorable for peer effects, this could explain the different effects of the two reforms. However, I find no support for this. First, the sampling restrictions reduces the variation in characteristics for the analysis sample, meaning that the men included in the analysis are more similar than an average father. Second, heterogeneity by educational level of the peer does not indicate that one type of peer is driving the observed differences, not in terms of the peer response nor the peer effects (see Appendix Table A6 for the first stage and A7 for reduced form estimations). Instead, the more probable explanation–raised already in Section 5.1–aligns with the proposed explanation for the disparity between Sweden and Norway, namely differences in uptake before the reforms. Even though the first reform increased the exposure to peers taking any leave, it did not affect the intensive margin (Appendix Table A1). Meanwhile, peers responding to the second reform increased their days of parental leave also above and beyond the quota, meaning that fathers after the second reform likely received better information about longer spells of parental leave.

The reduced form estimate of the second reform is large relative to the direct response of peers, the magnitude for parental leave in the first two years is almost the same–8.8 relative to the first stage of 11.05. The relative size of the two suggests that fathers were affected also by peer parental leave taken after the age of two. Still, the response is seemingly large. To relate the findings to the existing literature, the IV-estimate corresponding to the significant reduced form estimate is presented in Appendix Table A5. The estimate of 0.378 means that fathers whose peer fully comply to the reform and increase their total uptake by 30 days, on average take an additional 11.3 days of parental leave in the first two years, corresponding to a 21 percent increase relative to baseline. As outlined in Section 3, the Swedish context does not satisfy the exclusionary restriction necessary for unbiased instrumental variable estimates and the estimate should therefore be interpreted with caution. Keeping in mind also that outcomes are not entirely comparable across studies, this IV-estimate is still reasonable given findings in the existing literature. Carlsson and Reshid (2022) find a somewhat smaller peer effect, 15-percent, on the length of parental leave for Swedish male coworkers. Using a peer-of-peer instrument they focus on the transmission of norms at the workplace. In contrast, my analysis focuses specifically on the reform-induced variation in parental leave uptake among peers–variation that is more salient to coworkers and therefore provides information about potential consequences while at the same time shifting norms. As such, the relative effect size aligns with expectations. Dahl et al. (2014) found extensive margin effects for male coworkers of 16 percent (11 percentage points), Welteke and Wrohlich (2019) found the probability of mothers staying home the first year to increase by 30 percentage points and Lassen (2021) found an intensive margin effect for mothers of 17 percent.

6 Robustness

Table 5 presents alternative specifications for the significant peer effect on days of parental leave benefits following the second reform in 2002. The estimates are generally smaller, but relatively stable and never statistically different from the main specification. The corresponding estimations for the first reform and the extensive margin consistently indicate that there are no meaningful effects along these margins (Appendix Tables A10-A12).

In panel A of Table 5, I further assess the independence assumption by examining how sensitive the estimates are to the inclusion of control variables. While the amount of leave in the first two years following the second reform remains comparably large, the estimates are somewhat smaller without control variables, suggesting that the imbalance detected in Table 2 adds noise to the estimation. There is no evidence of the imbalance driving the significant peer effect in Table 4. On the contrary, the imbalance biases the estimates downwards.

On a related note, RD-estimations are commonly presented without the individuals closest to the cut-off, using a so called donut, to ensure that manipulation of treatment status is not driving the effects. In this context however, peers are unlikely to manipulate the birth date of their child as a response to the quotas (see the discussion in Section 2). The more relevant concern here is that there is variation in parental characteristics across month of birth (Buckles and Hungermann, 2013), and therefore the main estimation includes the observations most similar to each other (i.e., closest to the cutoff). For presentational purposes, estimates with a one-week donut is presented in Panel B of Table 5. As expected, the effect for the second reform remains comparably large, but no longer significant. Similarly, the estimated effect decreases when estimated in a simple difference model comparing fathers on each side of the cutoff. The results presented in Panel B and C of Table 5 both point to the importance of emphasizing fathers close to the cutoff, which is comforting. Had the estimated peer effect been driven by covariate imbalance, it would increase as observations further away–which are arguably more different from one another–become more influential. As seen in Panel D, the estimates are similar when estimated in a Local Quadratic Regression, a method more robust to trends further away from the cutoff (Dahl et al., 2014). Using the optimal bandwidth in Panel E of Table 5, the estimate is essentially the same.[12]

[12] The restriction of a single peer within the reform window implies that the sample of workplaces changes with the bandwidth. Consequently, optimal bandwidth is not entirely applicable in this analysis. The suggested bandwidth of 3.5 months was found using the sample of 6 months, and this is the sample for which the estimates are calculated.

Table 5: Effects of 2002-reform on days of parental leave benefits, different specifications

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
Panel A: No controls					
Reform	5.251	4.399	6.425	0.503	-0.099
	(4.877)	(2.783)	(4.212)	(0.503)	(0.536)
Observations	9,754	9,754	9,754	9,754	9,754
Panel B: 1-week donut					
Reform	6.884	3.434	6.778	0.344	0.402
	(5.620)	(3.342)	(4.813)	(0.530)	(0.579)
Observations	9,460	9,460	9,460	9,460	9,460
Panel C: Simple difference					
Reform	1.500	-0.067	3.329**	0.161	0.030
	(1.897)	(1.030)	(1.625)	(0.202)	(0.208)
Observations	9,784	9,784	9,784	9,784	9,784
Panel D: Local quadratic regression					
Reform	5.899	4.276*	6.697*	0.518	0.146
	(4.633)	(2.39)	(3.755)	(0.456)	(0.508)
Observations	9,754	9,754	9,754	9,754	9,754
Panel E: Optimal Bandwidth					
Reform	5.923	3.932	8.594*	0.287	-0.452
	(5.932)	(3.324)	(5.166)	(0.585)	(0.628)
Observations	6,152	6,152	6,152	6,152	6,152

Notes:Reduced form estimates from separate RD regressions for the days of parental leave following the second reform. Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe–unless otherwise stated. The standard errors in the Local quadratic regression model is estimated with 2000 repetitions. The STATA-package *rdrobust* is used to identify the optimal bandwidth. Note however that this is done for the original sample (thus the definition of peers and fathers is not adjusted accordingly (see the discussion in section 3). The package *rdrobust* offers data-driven methods to select the optimal number of bins. I apply the default method *esmv*, which is a mimicking variance evenly-spaced method using spacings estimators. See Calonico et al., 2014 and Calanico et al., 2017 for details. For the second reform implemented January 1 2002, the corresponding window is 112. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

In the Appendix, more robustness exercises are presented. Table A13 shows that the results hold also when clustering the standard errors at the running variable.[13] The robustness of the estimated peer effect in the first two years is also verified using placebo estimations. First, using the same cut-off the year before the reform, i.e., 2001, the corresponding reduced form estimate is merely 0.21 days, and insignificant. Second, I perform

[13]This specification is advised against when using a discrete running variable (Kolesar and Rothe, 2018), but follows that of Dahl et al. (2014)

a number of placebo estimations where the true reform date is shifted one day. In line with the findings of Dahl et al., (2014), few placebo estimates exceeds the estimated peer effect of 8.8 days presented Table 4, suggesting that the analysis reflects a true effect of the reform (see Appendix Figure A2).[14] The estimated peer effect on the days of parental leave in the first two years following the second reform is also robust to inclusion of workplaces with multiple peers, and when focusing only on income based parental leave benefits–which is the type covered by the quota (see Appendix Table A15).

A potential concern specific to the Swedish setting is that the parental leave system allows continuous applications of parental leave. That is, the order of birth does not determine the timing of application nor actual uptake of parental leave. While the order of birth may still be a good predictor of who affects whom irrespective of when they eventually take their parental leave, it would be alarming if the peer effect was driven by peers who took parental leave *after* the father. Having confirmed that there is no reform effect on the time gap between when the peer and father have their child (see Appendix Table 2), Table 6 presents the effects of the second reform for a subsample excluding pairs where the order of causality is especially unclear. Reassuringly, the top panel of Table 6 shows that the first stage estimate is largely unaffected by removal of workplaces where the father takes parental leave before the peer, suggesting that reversed causality does not have a great impact on the direct reform response among peers. The reduced form estimate in column 3 is however slightly larger, which is consistent with peer effects being stronger if the father can observe the parental leave of the coworker and assess the costs and benefits of parental leave, before taking his own decision. Moreover, by restricting the analysis to peer-father pairs whose children are born at least two years apart, the peer parental leave in the first two years precedes the leave of the father by definition. As seen in columns 4-5 of Table 6, this drastically reduces the sample size and consequently worsens the precision of the estimates. The magnitude of the estimates are however large, especially the peer effect of parental leave in the first two years at 11.5 days.

[14] The significant placebo peer effects for the second reform are primarily found in the week before implementation (Appendix Figure A2b). As can be seen by the corresponding estimation of the first stage in (Appendix Figure A2d), we see a similar response among the peers whose child was born shortly before the reform. A positive placebo estimate is to be expected since the placebo-jump includes the true jump, but could also indicate that there are spillovers among the peers such that also control peers are treated. Given the flexibility of the Swedish system, this could be the case. If so, it would not render false positive effects but rather bias the estimates downwards, meaning that the estimated effects are a conservative measure of the true effects.

Table 6: Effects of 2002-reform on days of parental leave benefits, different sub-samples

	(1)	(2)	(3)	(4)	(5)	(6)
	Excluding reversed order			At least 2 y gap		
	Total	By age 1	By age 2	Total	By age 1	By age 2
First stage	22.235***	3.221	11.200**	21.078***	0.947	10.220
	(5.207)	(3.136)	(4.804)	(7.310)	(4.329)	(6.792)
Baseline	75.330	19.192	46.103	70.031	16.191	38.851
Observations	8,313	8,313	8,313	4,047	4,047	4,047
Reduced form	6.816	3.827	9.134**	8.008	9.587**	11.545*
	(5.144)	(2.753)	(4.423)	(8.301)	(4.230)	(7.001)
Baseline	98.888	19.088	52.575	102.232	19.559	55.291
Observations	8,313	8,313	8,313	4,047	4,047	4,047

Notes: Reduced form estimates from separate RD regressions for parental leave following the second reform. In columns 1-3, I removed peer-father pairs where the peer PL is taken after the father PL. In columns 4-6, I removed fathers whose child was born less than two years after the peer child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6.1 The strength of the tie

The peer effect on early parental leave due to the second reform stands out throughout the various specifications, but standard errors are often large. A possible reason for the imprecise peer effect is that the identification strategy fails to capture actual peer-father interaction. The main analysis is conducted on a sample including all father-peer pairs to maintain sample size, but some of these fathers are less likely to be affected by the uptake of their peers, which is explored in this section. Overall, focusing the analysis on peer-father pairs that presumably have a stronger tie, the analysis supports the notion that there is in fact a peer effect on fathers days of paid parental leave in the first two years.

Above all, the peer effect is presumably stronger if both the peer and the father are still working together when the peer takes his parental leave and when the father's child is born. As can be seen in Appendix Table A16, there is no significant reform effect on peer or father mobility related to either child birth or parental leave. Thus, a sensitivity analysis can remove the 20% of peer-father pairs no longer working together. Using the remaining 80% of the sample in Appendix Table A17, the estimated effect on the days of parental leave in the first two years increases to 11.5 days (compared to the main estimate of 8.8). Here, a similar increase is observed for total uptake, suggesting that the increased leave early in the child's life reflects an overall increase. Meanwhile, there is no evidence of a significant effect from the first reform.

Appendix Table A18 presents the reduced form estimates for other sub samples, excluding different pairs for whom peer effects are less likely. While register data provides limited possibilities to assess actual interactions between individuals, one could argue that the father is less likely to observe the behavior of the peer–and consequences thereof–if the workplace is large (more than 50 employees). Similarly, different levels of education (3 levels) could reflect belonging to different units or performing separate work tasks, limiting interaction between the two. The information is also less likely to be transmitted if the time elapsed is long (more than 4 years). As expected, the significant estimates are generally larger in these restricted sample (see appendix Table A18).[15]

To conclude the robustness section, there is no evidence of a positive peer effect following the first reform. If anything, the estimates are negative, but imprecise and not significant. For the second reform however, various tests and alternative specifications support the conclusion that the second reform had positive peer effects at the workplace. The increased uptake is almost exclusively found for days of paid parental leave taken in the first two years of the child's life.

7 Heterogeneous responses to peer behavior

To better understand the mechanisms underlying the significant peer effect of early parental leave following the second reform, this section explores potential heterogeneous responses with respect to the value of information. Drawing on the findings from the introduction of the quota in Norway, one would expect the peer effect to strengthen as the value of information increases (Dahl et al., 2014). However, finding a peer effect only after the second reform suggests that increased information about *any* parental leave may not be sufficient in the context of Sweden.

Following Dahl et al. (2014), peers who have a manager position (indicated by the highest or second highest income rank at the workplace) are considered to transmit more valuable information. Moreover, the value is expected to be higher if information is sparse beforehand. Low uptake of parental leave at the workplace is proxied by a workforce that is primarily male.[16] Moreover, first parity fathers are presumably more susceptible to peer influence as they have no prior experience.

Introducing interaction terms capturing the high information ties in Appendix Tables A20-A22, the differential reduced form estimates are consistently insignificant. The estimations show that the peer effects are similar across peer-, father- and workplace characteristics. There is a tendency for the direct effect among high information peers to be stronger–especially for peers who are managers–but the interaction term in the reduced form estimation is mostly negative. Thus–if anything–there is a tendency for the peer effect to be stronger where the uptake was comparably high already before the reform. This is consistent with the importance of information about long periods of leave, and slow changing norms.

[15]Appendix Table A19 validates that the results are not driven by the smallest workplaces (5 or fewer) where the peer-father dynamics are presumably different.

[16]Fathers have expressed more hinders to taking parental leave when working in male dominated workplaces (Duvander et al., 2005), thus the indicator identifies workplaces with lower average uptake. The indicator is preferred over actual uptake among coworkers since this would condition on the presence of fathers in that time period and consequently reduce sample size.

8 Conclusion

A parental leave quota lowers the financial cost of shifting parental leave from the mother to the father. It can also normalize parental leave of fathers such that norms are affected and the signaling value of parental leave uptake decreases. As such, parental leave quotas has the potential to increase parental leave of fathers over and above the reserved amount. This is potentially important, as increased father involvement during infancy helps develop their childcare skills and may foster a preference for caregiving, ultimately promoting greater gender equality in parenting (Duvander and Johansson, 2019).

In this paper, I study whether the introduction and expansion of parental leave quotas in Sweden triggered workplace spillovers, thereby increasing parental leave uptake among male coworkers. Each reform reserved 30 days for each parent. I find that the first reform, introduced in 1995, did not generate positive spillovers: the estimates are consistently insignificant and generally negative. However, there is evidence of spillovers in the number of parental leave days following the second reform implemented in 2002. Although standard errors are relatively large, the estimate for parental leave taken in the first two years is significant and substantial—almost nine days. The corresponding effect on total uptake is large but insignificant, suggesting that fathers working with a peer covered by the second quota may have increased their overall uptake to some extent, but also reallocated some leave to be taken earlier.

Welteke and Wrohlich (2019) discuss three channels where the duration of (mothers') parental leave can be affected by the uptake of their peers: leisure complementarities, conformity to social norms and transmission of information.[17] Given that the reduced form estimations of the second reform are similar when the analysis is restricted to peer-father pairs with children born at least two years apart (as seen in Table 6), leisure complementarities are unlikely to be driving the significant results. Instead, the fact that there appears to be spillovers from the second reform but not the first is consistent with conforming to gender norms. Although the first quota increased the uptake of parental leave, presumably affecting the norms of parenthood, attitudes were more favorable towards parental leave of fathers by the second reform,[18] possibly making fathers more responsive to the increased uptake of their peers. The findings are also consistent with information transmission. However, contrary to previous studies (Dahl et al., 2014; Welteke and Wrohlich, 2019; Lassen, 2021), the information is more likely to concern the benefits of parental leave rather than its costs. This interpretation is consistent with the second reform–implemented in a context of different gender norms and offering parents more flexibility–being more successful at triggering spillovers.[19] Comparing the first and second reforms, it appears

[17] Dahl et al. (2014) also discuss the channel of sharing practical knowledge about the childcare system but this is unlikely in the context of Sweden. For Swedish fathers, in particular by the second reform, the extensive margin was high and consequently most fathers were aware of the system.

[18] More reported a belief that working mothers can establish just as warm and secure a relationship with her children as a mother who does not work: 70.8 percent compared to 83.7 percent, as measured by the World Values Survey Wave 2 and 4.

[19] The first reform reduced the parental leave available to mothers. Also, by the second reform, the coverage of collective agreements was higher, implying a lower earnings loss of parental leave relative to

that when the extensive margin is already high prior to implementation, perceptions of the reform play a role in determining the potential for spillovers. Moreover, the analysis of peers' direct responses suggests that the second reform provided fathers with better information about taking longer periods of parental leave–something they previously had limited information about.

For Sweden, an important takeaway from this paper is that the timing of parental leave is an important dimension, in addition to the number of days taken. Consequently, by restricting an analysis to total parental leave or to a certain time span, some of the dynamics may be overlooked. In terms of policy evaluation, neglecting the timing of parental leave can therefore lead to misguided conclusions about reform effects. Furthermore, the timing of parental leave benefits is a potential policy lever: policymakers could restrict when benefits can be used, in addition to limiting the total number of benefit days.

The findings of this paper suggest that it is important to consider also spillovers when evaluating the effect of parental leave quotas; while the direct response was stronger to the first compared to the second reform (both in my sample and as found by others e.g., Eriksson (2005) and *Försäkringskassan* (2019b)), the second reform was more successful at triggering peer effects, implying a higher net impact on the parental leave uptake of fathers in the first two years.

working, compared to the first reform and the low flat rate was increased (tripled).

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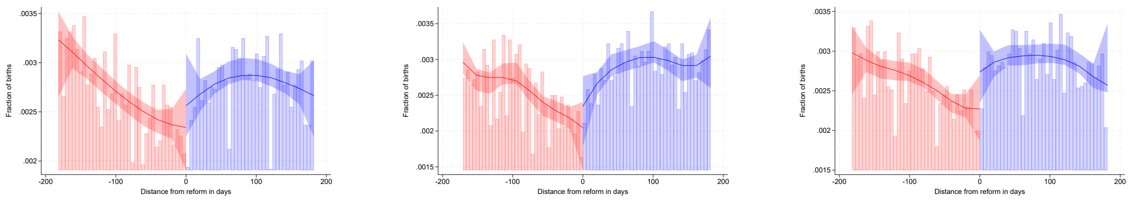
Appendix

Figure A1: McCrary test of density

(a) First reform | (b) Second reform | (c) Placebo



Notes: McCrary test of manipulation of density for the first reform (1995) in a), the second reform (2002) in b), and a placebo year (2001) in c).

Table A1: First stage intensive margin effect

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
First reform	2.071	-5.072	-2.224	0.402	1.878***
	(5.917)	(4.031)	(5.462)	(0.374)	(0.375)
Baseline	69.189	31.033	51.739	1.634	3.408
Observations	6,801	6,801	6,801	6,801	6,801
Second reform	23.298***	3.029	10.745**	0.312	1.777***
	(4.738)	(2.897)	(4.560)	(0.297)	(0.408)
Baseline	75.425	17.617	42.858	1.821	5.910
Observations	9,229	9,229	9,229	9,229	9,229

Notes: First stage estimates from separate RD regressions for the first (top panel) and second reform (bottom panel), on the intensive margin i.e., the average uptake among the peers taking any parental leave. Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Peer effects for alternative outcomes

	(1) Ratio age 2	(2) Ratio total	(3) Total days Excl. age 0-2
First reform	-0.015	-0.010	-1.122
	(0.012)	(0.011)	(2.359)
Baseline	0.103	0.141	22.727
Observations	8,987	8,987	8,987
Second reform	0.036***	0.021*	-2.084
	(0.013)	(0.011)	(2.978)
Baseline	0.160	0.227	46.060
Observations	9,754	9,754	9,754

Notes: Reduced form estimates from separate RD regressions for the first reform (top panel) and second reform (bottom panel). The father's share of the total parental leave for the child by age two (column 1) and total (column 2). All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3: First stage estimates on total days of parental leave grouped

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1-10	11-20	21-30	31-40	41-50	51-60	61-70	71-80	81-90	>90
Reform 1995	-0.070***	-0.021	0.220***	0.067***	0.019	0.028**	0.007	0.017	0.012	0.016
	(0.019)	(0.018)	(0.025)	(0.022)	(0.014)	(0.014)	(0.012)	(0.010)	(0.014)	(0.010)
Baseline	0.108	0.061	0.054	0.051	0.035	0.030	0.027	0.024	0.032	0.019
Reform 2002	-0.029***	-0.023**	-0.225***	-0.090***	0.004	0.145***	0.086***	0.031**	0.007	0.046***
	(0.011)	(0.011)	(0.022)	(0.019)	(0.015)	(0.022)	(0.022)	(0.015)	(0.013)	(0.013)
Baseline	0.023	0.037	0.222	0.130	0.073	0.059	0.051	0.043	0.037	0.034

Notes: First stage estimates from separate RD regressions for the first (top panel) second reform (bottom panel), on the total day of paid parental leave in bins. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Reduced form estimates on days of parental leave grouped

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First reform				Second reform			
	More than 10	More than 30	More than 60	More than 90	More than 10	More than 30	More than 60	More than 90
Panel A: Total days								
Reform	0.004	-0.027	-0.019	-0.018	-0.015	-0.006	0.010	0.005
	(0.021)	(0.035)	(0.033)	(0.029)	(0.017)	(0.020)	(0.033)	(0.034)
Baseline	0.907	0.570	0.291	0.183	0.939	0.901	0.655	0.419
Panel B: By age 2								
Reform	-0.018	-0.078**	-0.027	-0.026	0.036	0.040	0.031	0.055*
	(0.033)	(0.033)	(0.027)	(0.023)	(0.031)	(0.035)	(0.033)	(0.029)
Baseline	0.661	0.313	0.155	0.097	0.727	0.549	0.318	0.200
Observations	8,987	8,987	8,987	8,987	9,754	9,754	9,754	9,754

Notes: Reduced form estimates from separate RD regressions on parental leave days in bins. First reform in column 1-4, second reform in column 5-8. Total days (until age 8) in the top panel, and until age 2 in the bottom. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A5: IV form estimates on days of parental leave

	(1) Total	(2) By age 1	(3) By age 2
First reform	-0.229	-0.100	-0.182
	(0.181)	(0.108)	(0.153)
Observations	8,987	8,987	8,987
Second reform	0.289	0.202*	0.378**
	(0.209)	(0.122)	(0.187)
Observations	9,754	9,754	9,754

Notes: Instrumental variable estimation (2SLS), using the first stage estimation for total peer parental leave (until age 8). Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6: First stage estimation, heterogeneity by education of peer

	(1)	(2)	(3)	(4)	(5)	(6)
	First reform			Second reform		
	Total	By age 1	By age 2	Total	By age 1	By age 2
Reform	26.365***	7.693**	17.189***	29.540***	5.742*	16.518***
	(5.287)	(3.461)	(4.724)	(5.259)	(3.181)	(4.905)
Reform X Low educ.	-17.961	-14.601*	-20.502*	-12.080	-10.272	-10.729
	(13.287)	(8.711)	(11.954)	(16.599)	(9.808)	(16.274)
Reform X High educ.	4.352	11.166	8.353	-19.423	-6.741	-16.931
	(12.824)	(6.811)	(11.216)	(12.781)	(6.849)	(11.510)
Low education	20.391*	16.987**	19.354**	11.222	6.052	11.209
	(10.487)	(6.723)	(9.386)	(11.985)	(7.459)	(11.605)
High education	1.333	-6.816	-0.860	23.531**	-2.037	16.030*
	(9.830)	(4.768)	(8.612)	(10.025)	(5.249)	(8.659)
Observations	8,924	8,924	8,924	9,695	9,695	9,695

Notes: First stage estimations with interaction terms by the educational level of the peer. Medium level of education is omitted, Low refers to primary education (at most 10 years) and High refers to at least 2 years of tertiary education. First reform in columns 1-3, second reform in columns 4-6. Columns 1 and 4 reports the parental leave in total (by age eight), columns 2,3,5 and 6 reports parental leave by the age of the child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Reduced form estimation, heterogeneity by education of peer

	(1)	(2)	(3)	(4)	(5)	(6)
	First reform			Second reform		
	Total	By age 1	By age 2	Total	By age 1	By age 2
Reform	-3.434	-2.583	-3.911	3.284	3.282	4.173
	(5.000)	(3.043)	(4.333)	(5.751)	(3.180)	(5.024)
Reform X Low educ.	-2.817	4.115	7.621	11.428	-0.269	9.413
	(11.357)	(6.428)	(9.011)	(14.358)	(9.936)	(13.278)
Reform X High educ.	-13.984	-4.052	-15.553	6.680	7.294	17.285
	(13.959)	(7.781)	(11.686)	(13.635)	(7.543)	(11.477)
Low education	-2.876	-2.297	-7.723	-12.050	0.702	-7.295
	(8.256)	(3.997)	(5.729)	(9.854)	(6.766)	(9.411)
High education	15.302	-0.165	10.837	-2.618	-5.576	-6.771
	(11.414)	(6.035)	(9.613)	(8.726)	(3.823)	(7.057)
Observations	8,924	8,924	8,924	9,695	9,695	9,695

Notes: Reduced form estimations with interaction terms by the educational level of the peer. Medium level of education is omitted, Low refers to primary education (at most 10 years) and High refers to at least 2 years of tertiary education. First reform in columns 1-3, second reform in columns 4-6. Columns 1 and 4 reports the parental leave in total (by age eight), columns 2,3,5 and 6 reports parental leave by the age of the child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A8: Replication of results in Dahl et al. (2014)

	(1)	(2)	(3)	(4)	(5)
	Dahl et al.	Replication		Own specification	
	Main	All	Inc rest.	All	Inc. rest
		Coworkers			
First stage	0.317***	0.367***	0.366***	0.376***	0.360***
	(0.026)	(0.016)	(0.020)	(0.022)	(0.027)
Reduced form	0.035***	0.013	0.018	0.009	0.021
	(0.013)	(0.011)	(0.013)	(0.015)	(0.018)
2SLS	0.110***	0.035	0.049	0.023	0.059
	(0.043)	(0.029)	(0.035)	(0.039)	(0.049)
Observations	26,851	13,561	8,507	14,159	8,842
		Brothers			
First stage	0.304***	0.361***	0.358***	0.372***	0.358***
	(0.026)	(0.013)	(0.018)	(0.019)	(0.024)
Reduced form	0.047**	-0.009	-0.011	-0.010	-0.003
	(0.020)	(0.009)	(0.011)	(0.012)	(0.015)
2SLS	0.153**	-0.025	-0.031	-0.028	-0.007
	(0.065)	(0.025)	(0.031)	(0.033)	(0.042)
Observations	12,495	20,454	9,637	20,588	9,765

Notes: Estimates from separate regressions on the extensive margin taking any parental leave. Column 1 presents the results of Dahl et al. (2014) for Norway, this analysis is using all fathers in sampled workplaces whereas column 2-5 include only the first father. Column 2 and 3 replicate their main findings using Swedish data. Column 3 and 5 impose an income restriction of 41 000 SEK (measured in 1994) on both peer and peer mother in line with Norwegian eligibility criteria for paid parental leave. The workplace sample follows Dahl et al. (2014) and is restricted to workplaces of no more than 500 employees. The reform window is 6 months for the workplace network and 12 months for the family network. The replication use a one-week donut, standard errors are clustered at the running variable and trends are linear. The specification include controls for age, age squared, marital status, education and municipality of peer and peer mother, as well as the gender of the peer child (workplace network include controls for workplace size). Own specification is conducted on the same sample, but include quadratic trends, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, cohort- and muni-fe, and standard errors are robust. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Replication of results in Dahl et al. (2014), days in 2 years

	(1)	(2)	(3)	(4)
	Coworkers		Brothers	
	Replication	Own specification	Replication	Own specification
First stage	0.374***	0.371***	0.346***	0.363***
	(0.019)	(0.026)	(0.015)	(0.021)
Reduced form	-0.003	0.018	0.007	0.015
	(0.018)	(0.024)	(0.014)	(0.020)
2SLS	-0.009	0.048	0.021	0.041
	(0.049)	(0.064)	(0.040)	(0.054)
Observations	13,561	14,159	20,454	20,588

Notes: Estimates from separate regressions on the extensive margin on days in the first two years. Column 1 presents the results of Dahl et al. (2014) for Norway, this analysis is using all fathers in sampled workplaces whereas column 2-5 include only the first father. Column 2 and 3 replicate their main findings using Swedish data. Column 3 and 5 impose an income restriction of 41 000 SEK (measured in 1994) on both peer and peer mother in line with Norwegian eligibility criteria for paid parental leave. The workplace sample follows Dahl et al. (2014) and is restricted to workplaces of no more than 500 employees. The reform window is 6 months for the workplace netwwork and 12 months for the family network. The replication use a one-week donut, standard errors are clustered at the running variable and trends are linear. The specification include controls for age, age squared, marital status, education and municipality of peer and peer mother, as well as the gender of the peer child (workplace network include controls for workplace size). Own specification is conducted on the same sample, but include quadratic trends, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, cohort- and muni-fe, and standard errors are robust. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Effects of 1995-reform on the extensive margin, different specifications

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
No controls	-0.015	-0.057	-0.016	0.026	-0.014
	(0.017)	(0.035)	(0.029)	(0.031)	(0.017)
Observations	8,987	8,987	8,987	8,987	8,987
1-week donut	-0.007	-0.039	-0.025	0.005	-0.006
	(0.021)	(0.043)	(0.036)	(0.036)	(0.021)
Observations	8,680	8,680	8,680	8,680	8,680
First difference	0.013*	-0.021	-0.003	0.012	0.013*
	(0.007)	(0.014)	(0.012)	(0.012)	(0.007)
Observations	9,067	9,067	9,067	9,067	9,067
Optimal Bandwidth	-0.001	-0.078*	-0.013	0.006	-0.001
	(0.020)	(0.044)	(0.036)	(0.039)	(0.020)
Observations	5,265	5,265	5,265	5,265	5,265

Notes: Reduced form estimates from separate RD regressions for fathers taking any parental leave following the first reform. Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe–unless otherwise stated. The optimal bandwidth is identified using the STATA-package *rdrobust* on the original sample (thus the definition of peers and fathers is not adjusted accordingly (see the discussion in section 3). The package offers data-driven methods to select the optimal number of bins. I apply the default method *esmv*, which is a mimicking variance evenly-spaced method using spacings estimators. See Calonico et al., 2014 and Calanico et al., 2017 for details. For the first reform, the window is 104 days on each side of the implementation date January 1, 1995. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A11: Effects of 2002-reform on the extensive margin, different specifications

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
No controls	-0.021	0.022	0.010	-0.004	-0.021
	(0.015)	(0.035)	(0.028)	(0.034)	(0.015)
Observations	9,754	9,754	9,754	9,754	9,754
1-week donut	-0.012	0.042	0.027	0.013	-0.011
	(0.019)	(0.042)	(0.034)	(0.040)	(0.019)
Observations	9,460	9,460	9,460	9,460	9,460
First difference	-0.005	-0.002	0.005	0.003	-0.005
	(0.006)	(0.014)	(0.011)	(0.013)	(0.006)
Observations	9,838	9,838	9,838	9,838	9,838
Optimal Bandwidth	-0.021	0.015	0.007	-0.021	-0.020
	(0.018)	(0.043)	(0.034)	(0.041)	(0.018)
Observations	6,152	6,152	6,152	6,152	6,152

Notes: Reduced form estimates from separate RD regressions for fathers taking any parental leave following the second reform. Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe–unless otherwise stated. The optimal bandwidth is identified using the STATA-package *rdrobust* on the original sample (thus the definition of peers and fathers is not adjusted accordingly (see the discussion in section 3). The package offers data-driven methods to select the optimal number of bins. I apply the default method *esmv*, which is a mimicking variance evenly-spaced method using spacings estimators. See Calonico et al., 2014 and Calanico et al., 2017 for details. For the second reform implemented January 1 2002, the corresponding window is 112. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A12: Effects of 1995-reform on the days of parental leave, different specifications

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
No controls	-5.005	-1.900	-4.375	-0.047	0.050
	(4.278)	(2.529)	(3.566)	(0.278)	(0.370)
Observations	8,987	8,987	8,987	8,987	8,987
1-week donut	-6.608	-3.795	-4.202	-0.185	0.072
	(5.383)	(3.224)	(4.492)	(0.345)	(0.434)
Observations	8,680	8,680	8,680	8,680	8,680
First difference	1.155	-0.643	0.175	-0.092	0.274*
	(1.713)	(1.015)	(1.424)	(0.136)	(0.144)
Observations	9,067	9,067	9,067	9,067	9,067
Optimal Bandwidth	-3.525	-1.356	-4.090	-0.111	-0.033
	(5.291)	(3.204)	(4.470)	(0.329)	(0.454)
Observations	5,265	5,265	5,265	5,265	5,265

Notes: Reduced form estimates from separate RD regressions for the fathers' days of parental leave following the first reform. Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe–unless otherwise stated. The optimal bandwidth is identified using the STATA-package *rdrobust* on the original sample (thus the definition of peers and fathers is not adjusted accordingly (see the discussion in section 3). The package offers data-driven methods to select the optimal number of bins. I apply the default method *esmv*, which is a mimicking variance evenly-spaced method using spacings estimators. See Calonico et al., 2014 and Calanico et al., 2017 for details. For the first reform, the window is 104 days on each side of the implementation date January 1, 1995. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A13: Reform effect, clustered standard errors

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
Extensive margin					
First reform	-0.015	-0.065*	-0.024	0.016	-0.015
	(0.016)	(0.039)	(0.029)	(0.028)	(0.016)
Observations	8,987	8,987	8,987	8,987	8,987
Second reform	-0.022	0.025	0.019	-0.013	-0.022
	(0.015)	(0.033)	(0.024)	(0.038)	(0.015)
Observations	9,754	9,754	9,754	9,754	9,754
Days of parental leave					
First reform	-5.511	-2.419	-4.389	-0.101	-0.014
	(3.676)	(2.361)	(3.447)	(0.244)	(0.327)
Observations	8,987	8,987	8,987	8,987	8,987
Second reform	6.717	4.693*	8.802**	0.337	-0.139
	(5.004)	(2.456)	(3.822)	(0.434)	(0.520)
Observations	9,754	9,754	9,754	9,754	9,754

Notes: Reduced form estimates from separate RD regressions clustering standard errors at running variable. The first and second reform, on the share of fathers taking parental leave (top panel) and the days of parental leave taken by fathers (bottom panel). Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

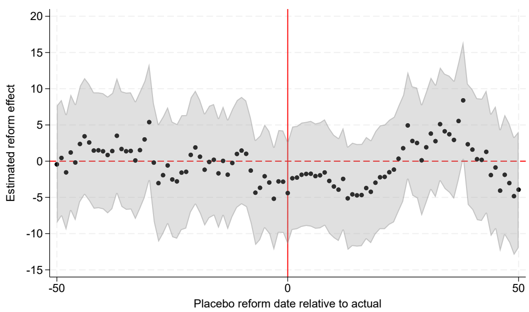
Table A14: Reduced form estimates for placebo year 2001

	(1)	(2)	(3)	(4)	(5)	(6)
	First stage			Reduced form		
	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin						
Reform	0.008	0.073**	0.068**	-0.006	0.028	0.005
	(0.017)	(0.034)	(0.032)	(0.014)	(0.035)	(0.027)
Baseline	0.935	0.452	0.717	0.945	0.523	0.786
Observations	9,353	9,353	9,353	9,353	9,353	9,353
Panel B: Days of parental leave						
Reform	4.701	-2.141	3.739	-3.807	-1.495	0.206
	(4.579)	(2.500)	(3.793)	(4.959)	(2.609)	(4.067)
Baseline	67.227	15.730	36.416	91.338	20.314	50.552
Observations	9,353	9,353	9,353	9,353	9,353	9,353

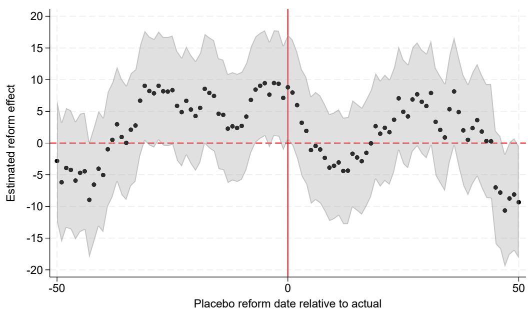
Notes: Reduced form estimates on parental leave. *** p<0.01, ** p<0.05, * p<0.1.

Figure A2: Placebo estimations in the first two years

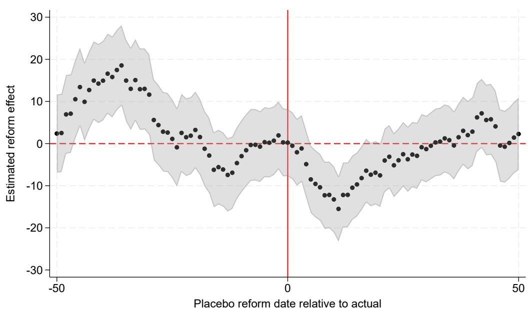
(a) First reform
Peer effect



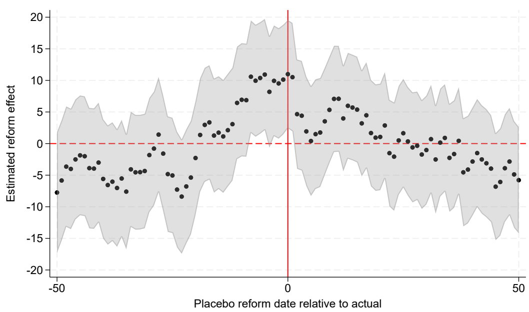
(b) Second reform
Peer effect



(c) Placebo year for the second reform (2001)
Peer effect



(d) Second reform
First stage



Notes: Separate placebo estimations shifting the true reform date by one day and repeat the estimation as specified in equations 1 and 2.

Table A15: Peer effect reform 2002, alternative specifications

	(1) Total	(2) By age 1	(3) By age 2	(4) Weekends	(5) Spells
Panel A: Allowing more peers in the reform window					
Second reform	4.112	4.241*	7.928**	0.184	-0.323
	(4.491)	(2.561)	(3.859)	(0.454)	(0.472)
Baseline	99.690	19.810	53.285	2.982	8.047
Observations	11,432	11,432	11,432	11,432	11,432
Panel B: Only income based benefits					
Second reform	6.780	4.603*	7.893**	-0.119	-0.347
	(4.209)	(2.744)	(3.906)	(0.243)	(0.488)
Baseline	85.497	19.464	49.167	1.128	7.683
Observations	9,754	9,754	9,754	9,754	9,754

Notes: Reduced form estimates from separate RD regressions on the days of parental leave taken by fathers. Panel A expands the sample to include also workplaces where there are multiple peers in the reform window with the same treatment status, using only the last peer (and first father). Panel B restricts the days of parental leave to the income based benefit, which is the one covered by the quota. Column 1 reports the parental leave in total (by age eight), Columns 2-3 reports parental leave by the age of the child, column 4 parental leave during weekends, and column 5 reports the number of parental leave spells. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A16: Reform effect on changing workplace

	(1) Either change before father child	(2) Peer change before peer PL	(3) Father change before peer PL	(4) Father change before father PL
First reform	-0.038	-0.026	-0.040	-0.012
	(0.028)	(0.028)	(0.031)	(0.032)
Baseline	0.330	0.0953	0.130	0.392
Observations	8,987	6,826	6,826	8,416
Second reform	0.007	0.005	-0.002	-0.013
	(0.027)	(0.026)	(0.030)	(0.033)
Baseline	0.305	0.109	0.159	0.358
Observations	9,754	9,231	9,231	9,248

Notes: Reduced form estimates from separate RD regressions for the first and second reform, on outcomes related to the peer or the father changing workplace. In column 1, the indicator takes 1 if either the father or the peer change workplace before the father's child is born. Column 2 takes the value 1 if the peer change workplace before the peer first takes parental leave, and column 3 takes 1 if either the peer or the father change workplace before the peer first takes parental leave–both excluding pairs where the peer takes no paid parental leave. Column 4 takes value 1 if the father move before the father first takes parental leave. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A17: Reform effect, stayers

	(1)	(2)	(3)	(4)	(5)	(6)
		First stage			Reduced form	
	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin						
First reform	0.326***	0.244***	0.333***	-0.004	-0.023	-0.024
	(0.035)	(0.044)	(0.039)	(0.019)	(0.043)	(0.033)
Baseline	0.588	0.380	0.497	0.939	0.574	0.800
Observations	5,985	5,985	5,985	5,985	5,985	5,985
Second reform	0.038*	0.095**	0.098***	-0.029*	0.023	0.034
	(0.020)	(0.043)	(0.037)	(0.017)	(0.043)	(0.034)
Baseline 0.946	0.471	0.743	0.954	0.540	0.815	
Observations	6,707	6,707	6,707	6,707	6,707	6,707
Panel B: Days of parental leave						
First reform	21.879***	5.382	13.133***	-4.964	0.814	-2.410
	(5.438)	(3.428)	(4.649)	(5.432)	(3.114)	(4.533)
Baseline	39.233	17.678	29.295	57.625	18.982	36.182
Observations	5,985	5,985	5,985	5,985	5,985	5,985
Second reform	27.826***	4.556	14.174***	11.173*	5.651*	11.513**
	(5.623)	(3.258)	(5.273)	(5.737)	(3.301)	(5.013)
Baseline	69.728	15.852	39.186	98.592	19.921	53.157
Observations	6,707	6,707	6,707	6,707	6,707	6,707

Notes: The sample is restricted to pee-father pairs still working together when the father has his child. First stage (column 1-3) and reduced form (column 4-6) estimates from separate RD regressions for the first and second reform, on the share of fathers taking parental leave (Panel A) and the days of parental leave taken by fathers (Panel B). Columns 1 and 4 reports the parental leave in total (by age eight), columns 2-3 and 5-6 reports parental leave by the age of the child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A18: Peer effect for strong ties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Small workplaces			Same education			Short birth gap		
	Total	By age 1	By age 2	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin									
First reform	-0.014	-0.040	0.001	-0.015	-0.039	-0.011	-0.011	-0.060	-0.038
	(0.018)	(0.038)	(0.032)	(0.020)	(0.045)	(0.037)	(0.018)	(0.038)	(0.031)
Baseline	0.932	0.532	0.769	0.935	0.549	0.784	0.935	0.556	0.787
Observations	7,838	7,838	7,838	5,735	5,735	5,735	7,533	7,533	7,533
Second reform	-0.015	0.027	0.028	-0.023	0.039	0.005	-0.017	0.035	0.036
	(0.017)	(0.038)	(0.031)	(0.017)	(0.043)	(0.033)	(0.016)	(0.039)	(0.031)
Baseline	0.948	0.527	0.802	0.953	0.544	0.822	0.950	0.524	0.803
Observations	8,419	8,419	8,419	6,459	6,459	6,459	8,286	8,286	8,286
Panel B: Days of parental leave									
First reform	-7.479	-2.283	-4.942	-4.528	-2.465	-3.496	-1.685	0.675	-1.664
	(4.553)	(2.746)	(3.863)	(5.145)	(3.185)	(4.553)	(4.686)	(2.717)	(3.839)
Baseline	57.244	17.949	34.703	58.359	18.189	35.260	56.457	18.199	34.721
Observations	8,419	8,419	8,419	6,459	6,459	6,459	8,286	8,286	8,286
Second reform treated	6.923	4.114	8.182*	8.156	6.159*	9.947*	8.020	5.749*	9.351**
	(5.335)	(2.987)	(4.648)	(6.014)	(3.363)	(5.212)	(5.135)	(2.937)	(4.550)
Baseline	98.520	19.986	52.393	101.279	20.251	55.107	96.255	19.421	52.257
Observations	8,419	8,419	8,419	6,459	6,459	6,459	8,286	8,286	8,286

Notes: Reduced form estimates from separate RD regressions for the first and second reform, on the share of fathers taking parental leave (Panel A) and the days of parental leave taken by fathers (Panel B). Small workplaces restricts the sample to those with less than 50 employees. Same education requires the peer and father to have the same level of education (measured as 3 levels; primary education; tertiary education; university education at least 2 years). Short gap limits the time between the child of the peer and the father to 4 years. Column 1, 4,7 reports the parental leave in total (by age eight), columns 2-3, 5-6, 8-9 reports parental leave by the age of the child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A19: Peer effect, excluding smallest workplaces

	(1)	(2)	(3)	(4)	(5)	(6)
	First reform			Second reform		
	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin						
Reform	-0.019	-0.073**	-0.042	-0.019	0.025	0.021
	(0.017)	(0.037)	(0.030)	(0.016)	(0.037)	(0.029)
Baseline	0.949	0.538	0.811	0.377	0.950	
Observations	0.936	0.547	0.785	0.949	0.541	0.814
Panel B: Days of parental leave						
Reform	-4.701	-1.303	-3.412	8.590*	5.727**	10.233**
	(4.553)	(2.705)	(3.833)	(4.914)	(2.751)	(4.272)
Baseline	57.563	17.715	34.885	100.171	20.062	54.249
Observations	8,281	8,281	8,281	9,060	9,060	9,060

Notes: The analysis is restricted to workplaces with at least 5 workers. Reduced form estimates from separate RD regressions for the first and second reform, on the share of fathers taking parental leave (top panel) and the days of parental leave taken by fathers (bottom panel). Columns 1 and 4 reports the parental leave in total (by age eight), columns 2-3 and 5-6 reports parental leave by the age of the child. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A20: Effect of 2002-reform interacted with peer manager status

	(1)	(2)	(3)	(4)	(5)	(6)
		First stage			Reduced form	
	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin						
Reform X Manager	0.036	0.044	0.060	0.019	0.004	0.018
	(0.046)	(0.088)	(0.084)	(0.040)	(0.091)	(0.075)
Reform	0.008	0.080**	0.061*	-0.027	0.021	0.014
	(0.019)	(0.039)	(0.034)	(0.017)	(0.039)	(0.031)
Manager	0.002	-0.100	-0.115*	-0.019	-0.063	-0.044
	(0.035)	(0.062)	(0.062)	(0.026)	(0.065)	(0.053)
Baseline	74.183	17.472	43.096	100.930	20.007	54.364
Panel B: Days of parental leave						
Reform X Manager	25.799**	12.287*	13.620	-0.802	6.686	-5.874
	(10.960)	(6.287)	(10.410)	(13.439)	(8.380)	(12.564)
Reform	17.713***	0.551	7.854	6.856	3.410	9.849**
	(5.402)	(3.184)	(5.006)	(5.263)	(2.921)	(4.464)
Manager	-23.933***	-9.510**	-17.577***	1.260	-3.664	1.558
	(7.291)	(3.795)	(6.762)	(9.032)	(5.102)	(8.634)
Baseline	74.183	17.472	43.096	100.930	20.007	54.364
Observations	9,754	9,754	9,754	9,754	9,754	9,754

Notes: First stage (columns 1-3) and reduced form (columns 4-6) estimates from separate RD regressions for the second reform, on the share of fathers taking parental leave (Panel A) and the days of parental leave taken by fathers (Panel B). Columns 1 and 4 reports the parental leave in total (by age eight), columns 2-3 and 5-6 reports parental leave by the age of the child. Manager is indicated by the peer having top 1 or 2 income at the workplace. Baseline refers to pre-reform average among peers who are not managers. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A21: Effect of 2002-reform interacted with workplace male dominated

	(1)	(2)	(3)	(4)	(5)	(6)
		First stage			Reduced form	
	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin						
Reform X Male dominated	-0.008	-0.002	-0.032	-0.003	0.092	-0.021
	(0.037)	(0.076)	(0.068)	(0.032)	(0.076)	(0.061)
Reform	0.017	0.095**	0.089**	-0.021	-0.001	0.026
	(0.022)	(0.042)	(0.037)	(0.018)	(0.042)	(0.034)
Male dominated	0.018	0.006	-0.025	0.001	-0.046	0.017
	(0.029)	(0.056)	(0.052)	(0.022)	(0.057)	(0.045)
Baseline	0.940	0.476	0.746	0.951	0.532	0.812
Panel B: Days of parental leave						
Reform X Male dominated	10.431	1.729	7.278	-1.268	-0.613	-3.044
	(10.230)	(6.324)	(9.647)	(10.301)	(6.029)	(9.076)
Reform	19.945***	2.549	8.586*	6.894	4.859	9.344*
	(5.607)	(3.060)	(5.169)	(5.940)	(3.318)	(5.163)
Male dominated	-4.277	4.127	-3.386	2.010	1.213	0.342
	(7.513)	(4.432)	(6.693)	(7.267)	(4.132)	(6.583)
Baseline	72.863	16.413	42.226	101.344	20.018	56.356
Observations	9,754	9,754	9,754	9,754	9,754	9,754

Notes: First stage (columns 1-3) and reduced form (columns 4-6) estimates from separate RD regressions for the second reform, on the share of fathers taking parental leave (Panel A) and the days of parental leave taken by fathers (Panel B). Columns 1 and 4 reports the parental leave in total (by age eight), columns 2-3 and 5-6 reports parental leave by the age of the child. Male domination is defined as at least 90% men at the workplace. Baseline refers to pre-reform average among peers who are not working in male dominated workplaces. All estimations include separate quadratic trends, triangular weights, workplace covariates (number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A22: Effect of 2002-reform interacted with First parity fathers

	(1)	(2)	(3)	(4)	(5)	(6)
		First stage			Reduced form	
	Total	By age 1	By age 2	Total	By age 1	By age 2
Panel A: Extensive margin						
Reform X First parity	-0.017	-0.046	-0.040	0.002	-0.003	0.049
	(0.036)	(0.072)	(0.063)	(0.032)	(0.072)	(0.058)
Reform	0.024	0.120**	0.101**	-0.024	0.026	-0.010
	(0.027)	(0.056)	(0.047)	(0.027)	(0.055)	(0.047)
First parity	0.952	0.474	0.742	0.941	0.481	0.768
	(0.028)	(0.052)	(0.048)	(0.022)	(0.053)	(0.043)
Baseline	0.973	0.530	0.804	0.960	0.535	0.850
Panel B: Days of parental leave						
Reform X First parity	8.003	0.996	4.416	-10.428	0.539	-4.866
	(9.818)	(5.894)	(9.173)	(9.669)	(5.550)	(8.453)
Reform	18.465**	2.481	8.390	12.776*	4.371	11.578*
	(7.893)	(5.033)	(7.457)	(7.142)	(3.956)	(6.171)
First parity	-15.106**	-7.482*	-13.237**	23.477***	4.625	14.639**
	(7.457)	(4.386)	(6.743)	(6.585)	(3.623)	(5.843)
Baseline	73.503	17.072	42.146	91.383	17.791	46.720
Observations	9,754	9,754	9,754	9,754	9,754	9,754

Notes: First stage (columns 1-3) and reduced form (columns 4-6) estimates from separate RD regressions for the second reform, on the share of fathers taking parental leave (Panel A) and the days of parental leave taken by fathers (Panel B). Columns 1 and 4 reports the parental leave in total (by age eight), columns 2-3 and 5-6 reports parental leave by the age of the child. First parity refers to fathers whose child is their first. Baseline refers to pre-reform average among peers whose child is higher parity. All estimations include separate quadratic trends, triangular weights, workplace covariates (share of males and number of employees), predicted parental leave uptake of fathers and peers, and cohort- and muni-fe. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1