

Modern Family? Paternity Leave and Marital Stability[†]

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We study how relationship stability of couples is affected by an increase in fathers' involvement in staying home from work with young children. We make use of a parental leave reform in Sweden that earmarked one month of paid leave to each parent in a regression discontinuity difference-in-differences (RD-DD) framework. Couples who were affected by the reform increased the take-up of fathers' leave but also increased their probability of separation compared to unaffected couples. We argue that the separation effect can be explained by the degree of restrictiveness of the policy in combination with role conflicts in traditional family constellations. (JEL D13, J12, J13, J16, J32)

One of the most significant changes in the labor markets of industrialized countries over the past decades has been the increase in female labor force participation and the accompanying narrowing of the gender gaps in both higher education and earnings. Despite these changes, some societal differences between men and women have remained largely rigid over time. One such particularly persistent gender gap relates to parents' time spent at home with young children. In Sweden, known as a progressive country with virtually no gender difference in labor force participation, where women's average educational attainment greatly exceeds that of men, and with longstanding equal parental leave rights for mothers and fathers, women still account for more than three quarters of the total parental leave uptake. Thus, while the gender gap in earnings potential has decreased in most high-income countries over time, the gender gap in earnings appears to increasingly arise as a consequence of childbearing and caring (see, e.g., Kleven, Landais, and Sjøgaard 2018; Angelov, Johansson, and Lindahl 2016).

To promote gender equality, the Swedish Government carried out a reform of the national parental leave system in 1995 with the explicit goal of increasing fathers'

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involvement in spending time with their children. This so-called “daddy-month” reform altered the existing system by earmarking one month of paid leave to each parent, hence, restricting the right to transfer leave days to the spouse as was previously common practice in many families. Ekberg, Eriksson, and Friebe (2013) have previously showed that the reform indeed increased the father’s uptake of paid leave, but did not alter the long-run division of household work, suggesting that it may not be a trivial task to increase equality in the domestic sphere by means of policy.

One potential explanation for the sluggish response in gender equality from the Swedish parental leave reform may be due to strong social norms regarding typical male and female behavior (see, e.g., Bertrand, Kamenica, and Pan 2015). According to this line of thought, the intervention in the parental leave system may have led to additional role conflicts in the family, which could have ultimately affected the stability of the relationship if couples were forced to deviate from their initial plans. Hence, studying the interaction between the parental leave reform and relationship stability may uncover information on how policymakers could act in order to improve long-term gender equality in the presence of gender norms.

In this paper, we exploit the 1995 intervention in the Swedish parental leave system to study its impact on the probability of separation of couples and underlying mechanisms. To this end, we use longitudinal individual-level data on fertility, parental leave take-up, and marital status from various Swedish administrative registers, allowing us to identify family members. We subsequently use the introduction of the reform in a fuzzy regression discontinuity difference-in-differences (RD-DD) framework, making use of the fact that assignment to treatment was based on a plausibly exogenous variable; time of birth of the child. To motivate this claim and to validate concerns about causal identification, we also provide results from a number of auxiliary sensitivity checks with reassuring results.

Our initial analysis provides two main findings. First, consistent with Ekberg, Eriksson, and Friebe (2013), we find that the introduction of a nontransferable month in the parental leave system significantly decreased mothers’ intra-household average share of parental leave. The consequential increase in fathers’ take-up correspond closely to the number of nontransferable days imposed, suggesting the reform had almost full effect. Second, we find that the probability of couple dissolution three years after the child was born increased by about 1 percentage point (8 percent) for parents whose children were born just after, compared to just before, the reform was implemented. These results are largely robust to different follow-up horizons and empirical specifications.¹

To study the mechanisms behind our main effects, we extend our analysis in several directions. First, analyzing the temporal pattern of separations we find that the parental leave reform induced re-timed (earlier) separations rather than separations

¹ In a recent paper, Svarer and Verner (2008) find that having children increases the risk of dissolution in Denmark. Our findings thus show that the parental leave division may affect separation probabilities over and above the effect of having children. See also Lillard and Waite (1993) for a survey of the empirical literature studying the effects of children on marital dissolution.

that would never have occurred in absence of the reform. One possible interpretation of this result is that fathers' increased involvement in child rearing implied an information shock to spouses about their match quality and thereby induced an earlier separation among poorer matches. Second, studying the shifts in the distribution of parental leave before and after the reform, we find that the reform exclusively impacted the extensive margin of fathers' parental leave take-up, possibly causing additional role conflicts within the household. Third, using data on earnings, we study the impact of the reform on the redistribution of income between spouses in the household. Several interesting results emerge from this analysis. First, we find no evidence that the separation effect was mediated by significant changes in the time allocation to market work between the spouses. In contrast, we find that the reform decreased earnings for both fathers and mothers, suggesting that women compensated for the decreased paid parental leave with additional unpaid leave, leading to a lower total income for the household. Furthermore, exploring heterogeneity by mothers' pre-birth earnings, we find that the increased separation probabilities were driven by couples where the mother had relatively low labor income; the same group in which mothers compensated the most and, thus, also experienced the largest family income losses. Finally, we study the responses to a second parental leave reform, implemented in 2002, which earmarked one additional month of paid leave but also added another transferable month of entitlement. By virtue of the first "daddy-month" already being in place, we find that the second earmarked month mainly affected the intensive margin of fathers' leave. Strikingly, we find no effects of the 2002-reform on the probability of parental separation; a result that suggest that the restrictiveness of the reform and the marginal group affected by the changes are crucial for the outcomes we study. Taken together, our results suggest that the increase in separations due to the 1995 reform were due to a combination of factors: the income effects that may have increased the scope for conflicts in the household about, for example, a smaller household budget and the restrictiveness of and specific parental group affected by the reform. Whereas more traditional couples were affected in the first, restrictive, reform, the more flexible reform in 2002 mainly affected couples in which fathers would otherwise have taken up at least one month of leave. In terms of policy, our results highlight that the way family parental leave policies are implemented matter and may have important spillover effects that should be taken into account. As a comparative example, Steingrimsdottir and Vardardottir (2015) find that an Icelandic parental leave reform, extending the duration of paid leave that could be used only by fathers, decreased the divorce risk among affected couples.

I. Institutional Context

Mandated parental leave policies have become a salient feature of most industrialized countries during the last decades and several papers have studied their impacts on parental labor supply or household allocation of time (see, e.g., Lalive et al. 2014; Patnaik 2015; Kotsadam and Finseraas 2011; Rege and Solli 2010; Dahl, Løken, and Mogstad 2014; Schönberg and Ludsteck 2007), fertility (Lalive and Zweimüller 2009), and child outcomes (e.g., Carneiro, Løken, and Salvanes 2015;

Cools, Fiva and Kirkebøen 2015; Liu and Skans 2010). The Scandinavian countries were early adopters of government paid leave; the Swedish parental leave system was introduced already in 1974, replacing the preceding maternity leave introduced in 1954, and making eligibility to paid parental leave gender neutral. Both the mother and the father were given an equal number of paid leave for their children, but with the option of freely transferring paid leave days between each other. Parental leave benefits to care for young children are mainly raised by employer social security contributions and paid out by the government's social insurance agency as part of the mandatory social insurance system.

The benefits are divided into three components. First, 10 days of wage-replaced leave are given exclusively to the father, which he can use during the first 60 days after the birth of the child. Second, since 1978, part of the parental leave is replaced at a fixed daily amount of 60 to 180 SEK during the time period covered in our analysis. To date, these "base-level" benefits are received for a maximum of 90 days for each child. Third, parents receive a total of 390 days of leave per child during which benefits replace wages at a rate of 75 to 90 percent during the time period covered in our analysis. The wage-replaced benefits are conditioned on at least 240 days of employment preceding child birth and capped at a relatively generous income ceiling. For individuals that do not meet the work requirement, all parental leave days are compensated with a fixed daily amount of 180 SEK. In total, parents thus receive 480 days of paid leave for each child.

The parental leave is job protected and can be used flexibly. During the first 18 months after birth both parents are legally entitled to full-time, job-protected leave, with or without collecting benefits. Thereafter, parents have the option of reducing their working hours up to 25 percent until the child turns 8 years old. This means that parents are able to prolong their parental leave by claiming part-time benefits while staying at home full time. Any saved parental leave days can also be used selectively when the child is older or to, for example, extend family holidays. While employers cannot deny parental leave to workers, such requests must be made at least two months in advance.

A. Introduction of Paternity Leave Quotas

To study the effects of the parental leave reform on couple stability we exploit the implementation of the "daddy-month" reform in 1995, which gave additional monetary incentives for fathers to take up parental leave. Prior to the implementation of the reform, parents were given equal shares of the total paid leave but were also free to transfer paid leave days between each other. In practice, this meant that most fathers transferred essentially all of their parental leave days to the mothers. In order to encourage more fathers to use parental leave, the 1995 reform earmarked 1 month (30 days) of the wage-replaced leave to each parent, implying that 1 month of paid leave would be lost if either parent refused or otherwise failed to take any leave. Eligibility for the 1995 reform varied with the child's birth month, with parents to children born on or after January 1, 1995 being subject to the new rules. In order to further promote fathers' parental leave usage, the government also implemented a second "daddy-month" in 2002 where an additional month of wage-replaced leave

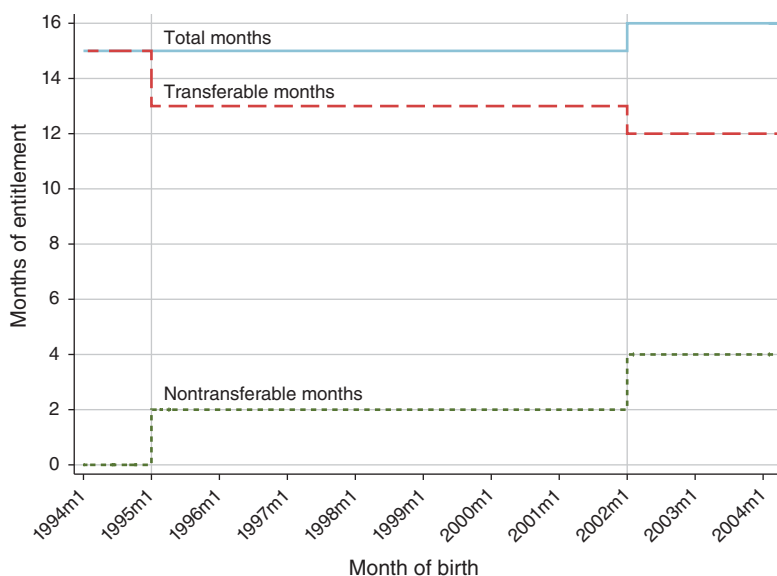


FIGURE 1. ENTITLEMENT RULES IN THE SWEDISH PARENTAL LEAVE SYSTEM

was earmarked for each parent with children born on or after January 1, 2002. At the same time, the total number of parental leave months were increased from 15 to 16 months. The changes in the entitlement rules are depicted graphically in Figure 1.

The effect of the 1995 reform on parental leave uptake has previously been studied by Ekberg, Eriksson, and Friebe (2013), who find strong short-term increases in fathers' parental leave take-up, but find no spillover effects on the long-term division of household work.^{2,3} Furthermore, Eriksson (2005) evaluated the effect of the second "daddy-month" in 2002 and found that this reform further increased fathers' average parental leave take-up from one to around two months.

Consistent with previous work, our data show that both reforms led to sharp increases in fathers' take-up of parental leave. Panel A of Figure 2 shows the average number of parental leave days taken during the child's first eight years of life by child birth month for mothers and fathers, respectively. We observe a substantial increase in fathers' take-up and a corresponding decrease in mothers' take-up among parents of children born in January 1995 compared to parents with children born in the previous year.

The 2002 reform implied a further increase in fathers' parental leave take-up, but also the parental leave taken by mothers due to the general increase in entitlement to paid leave that accompanied the reform. As shown in panel B of Figure 2, both reforms seem to have decreased the mother's intra-household share of parental leave take-up by about the same magnitude. In addition, panel C shows that the 2002 reform also increased the total leave taken for children in accordance with the

² Measured as the relative share of leave taken to care for sick children.

³ Cools, Fiva, and Kirkebøen (2015) study a similar reform in Norway, finding that fathers increased their parental leave as a result of the reform. However, they also find a *negative* effect on mothers' earnings, suggesting that the gender balance in home and market work did not change as a result of the reform.

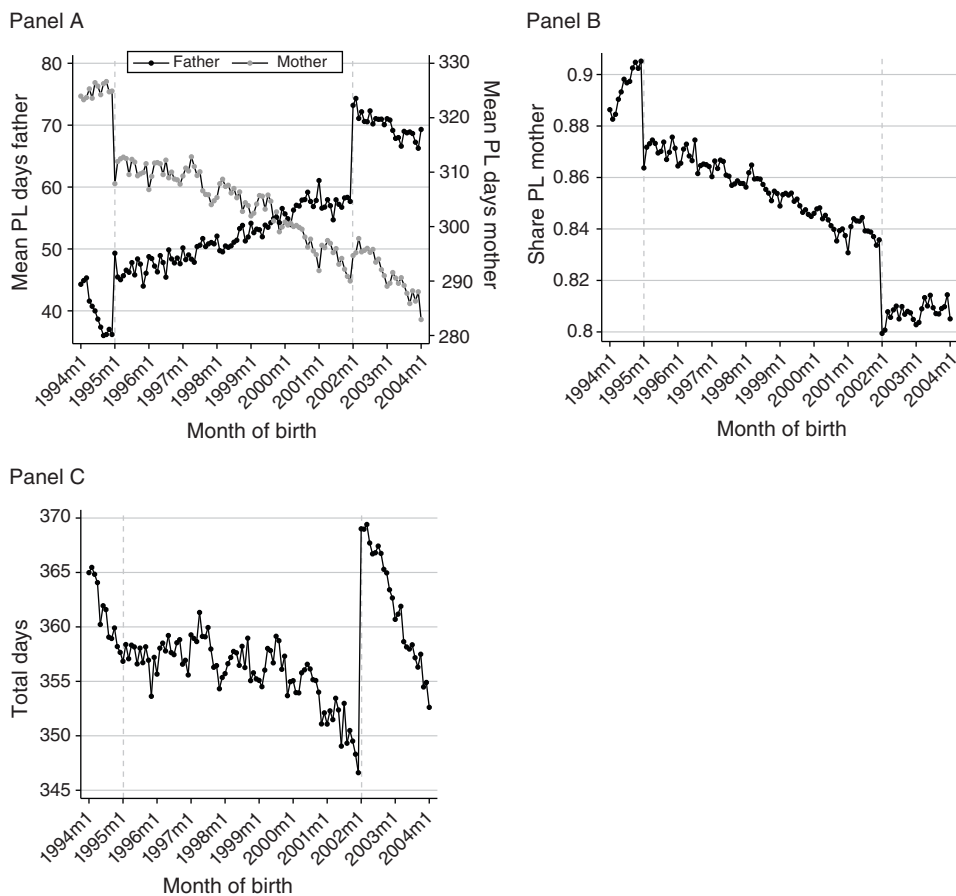


FIGURE 2. PARENTAL LEAVE UPTAKE IN SWEDEN BY CHILD BIRTH MONTH

Note: PL in the figure labels stands for parental leave.

new rules by around 25 days. Thus, parents seem to make close to full use of their entitled parental leave benefits.⁴

Due to the extension of the entitlement in the 2002 reform, its theoretical implications with respect to couple stability are less clear-cut for our purposes. Specifically, the additional transferable month from this reform was less restrictive in that it largely allowed parents to retain any distribution of parental leave irrespective of the parental leave regime and is thus less useful as an instrument for causal identification of the impact of decreased household specialization. For this reason we focus on the first parental leave reform in 1995 in our analyses and leave the results for the second reform as a comparison when discussing the mechanisms of our main effects.

⁴Figure 2 also exhibits a downward pre-reform trend in fathers' parental leave take-up prior to the 1995 reform. As discussed in Ekberg, Eriksson, and Friebe (2013), this trend is likely explained by the increase in unemployment that Sweden experienced in the early 1990s, which first hit male-dominated sectors (e.g., tradable industries). Since parental leave benefits can be postponed until the child is eight years old, an unemployed parent has incentives to collect unemployment benefits *before* making use of their parental leave rights.

B. Custody and Alimony in Sweden

Cohabitation is a common alternative to marriage in Sweden and, in terms of custody and alimony rights, there are some differences between the two forms of unions. During marriage, both spouses are responsible for their own as well as their partner's financial support; the Swedish marriage law stipulates that if one spouse is unable to support him- or herself, the other spouse is responsible for their support. Upon divorce, an economically disadvantaged divorcée is entitled to alimony payments during a transition period (which can be extended under some circumstances). However, the right to alimony payments does not extend to cohabiting couples upon separation. In the case the economically disadvantaged divorcée remarries, their entitlement to alimony payments is maintained, although the need for this support may be reevaluated.

For married couples, the law takes the husband as the legal father of his wife's children, and the custody of the children is thus joint by default. For cohabiting couples, however, the mother has the sole custody of a child by default. Thus, paternity must be established after birth, and parents must apply for joint custody. In practice, the identity of the father is established for nearly all children in Sweden. Parental leave is paid out to the legal parents of the children or to any other legal custodian. A parent with sole custody of a child is entitled to all 480 days of paid parental leave for a child.

II. Empirical Framework

A. Econometric Modeling

We apply a fuzzy regression discontinuity difference-in-differences (RD-DD) design to analyze the impact of the parental leave quota on family stability, exploiting the fact that parents whose children were born at the end of 1994 and beginning of 1995 were subject to different parental leave systems.⁵ Specifically, the discontinuities we use arise from the introduction of earmarked parental leave days in which parents of children born on or after January 1, 1995 were subject to one nontransferable month of paid leave each. We restrict our sample to parents whose children were born within a 12-month window around the reform.⁶

The basic regression-discontinuity (RD) setup motivates estimation of the following cross-sectional regression model by OLS:

$$(1) \quad y_i^\tau = \alpha + \mathbf{1}[t_i \geq c]\beta + \mathbf{1}[t_i \geq c] \times f_r(t - c, \gamma_r) + \mathbf{1}[t_i < c] \times f_l(c - t, \gamma_l) + \epsilon_i,$$

where y_i^τ is a binary indicator for whether the parents of child i separated within τ years, t is the birth month of the child, and c is the reform cutoff month (so that child birth month is centered around the cutoff), $\mathbf{1}[\cdot]$ is the indicator function and f_l

⁵ See Lee and Lemieux (2010) for a thorough exposition of the RD econometric framework.

⁶ We vary the sample bandwidth to six, three, and one month(s) in a robustness check. This yields qualitatively similar results.

and f_r are unknown functions with parameter vectors γ_l and γ_r , capturing seasonal trends in separation probability by birth month and year, respectively. Given that we correctly specify the trends, we can interpret $\hat{\beta}$ as the estimated discontinuity in separation probability of having children born just before and just after the turn of the year. Moreover, if we assume that parents do not have exact control of when their children are born in a neighborhood around the cutoff, we can interpret the estimated discontinuity as the causal effect of the parental leave reform on separation probability.⁷

One particular complication with the RD setup in our context is that the main outcome variable is measured annually while our empirical strategy requires within-year detail, since the follow-up period will otherwise mechanically vary between couples whose children are born late and early in the year, respectively.⁸ To handle this problem, we augment our RD design with a difference-in-differences model using non-reform years to wash out any such mechanical correlation between birth month and separation probability. This approach is valid under an additional common trends assumption that the underlying separation trends are comparable between reform and non-reform years. Specifically, we extend (1), using years 1994–2001, by additionally specifying a “treatment” indicator $T = \{0, 1\}$, equal to unity for the reform year cutoff in 1995 and zero otherwise, interacted with each included variable in the model:

$$(2) \quad y_i^\tau = \alpha + \sum_{s=0}^1 \mathbf{1}[T_i = s] \times \left\{ T_i \delta + \mathbf{1}[t_i \geq c] \beta_s + \mathbf{1}[t_i \geq c] \times f_r(t - c, \gamma_{rs}) + \mathbf{1}[t_i < c] \times f_l(c - t, \gamma_{ls}) \right\} + \lambda_{n_i} + \epsilon_i.$$

Equation (2) is essentially a fully interacted version of (1) with separate effects for reform and non-reform years, with the exception of fixed effects for each non-reform year, represented by λ_{n_i} . The parameter of interest here is β_1 , which can be interpreted as the causal effect of the introduction of the earmarked month on the probability of couple separation after τ years, conditional on any secular trends in the risk of separation. Since we a priori do not know the functional forms of f_l and f_r , we perform a number of specification checks to assess the robustness of the results to different trend definitions in order to obtain an unbiased estimate of the discontinuity at the cutoff. Following the steps in Lee and Lemieux (2010), we choose specification

⁷The assumption of imprecise control implies that child births are locally randomized around the cutoff and therefore that children with parents who have preferences for either parental leave system are not systematically born on one side of the cutoff. We perform a number of robustness checks to evaluate this concern below. In addition, we also need to assume that there are no other important changes of relevance for the stability of couples with children (such as other policy interventions) that become effective at the turn of the year. We are not aware of any such potential confounding factors.

⁸Specifically, we observe the marital status of an individual in November each year. The mechanical interaction between birth month and follow-up time implies that parents to children born in January will have had longer time to separate compared to parents to children born in December. Since the probability of separation deterministically increases with time in a relationship, the effect of the reforms on separations will be overestimated unless this mechanical trend is accounted for.

based on the diagnostic results with respect to the goodness-of-fit and balancing properties for a set of observable covariates.⁹

B. Data and Sampling

We combine linked data from several Swedish administrative registers in our empirical analysis. First, we use the multi-generational register to obtain information on the exact birth year and month of all children born in Sweden. The register includes unique identifiers for each child and his or her parents together with their biological link, allowing us to match couples with joint children. This information allows us to identify which parental leave system each child was born into. We restrict our attention primarily to mothers whose first child was born between 1994–2001 and retain information on all their children and the father(s) of their children. We subsequently match our family sample to the annual, individual-level, longitudinal register LOUISE, containing information on parent's age, educational attainment, and taxable labor income. In addition, the LOUISE register also includes annual information on marital status. Specifically, by means of unique family identifiers for couples with joint children, we can identify whether individuals are single, married, cohabiting, divorced, or separated.

To study to which extent the parental leave reform affected the distribution of parental leave uptake between spouses, we match our sample to data from the Swedish Social Insurance Agency containing parental leave spells on a daily level for each parent and child. We sum up the total number of days of parental leave taken for each child over the child's first eight years by parent and calculate the mother's share of this total.¹⁰

Column 1 of Table 1 of presents summary statistics for couples with children born within 12 months from the date of the implementation of the 1995 parental leave reform. Column 2 shows the corresponding information for the remaining non-reform years 1996–2001.

The upper panel of Table 2 reports estimates of β from estimation of equation (1) for different specifications of f_i and f_r using a set of covariates as outcomes: predicted divorces (estimated using a set of predetermined parental characteristics), child gender, parents' year of birth, parental immigrant status, and the education and income gaps between the spouses, respectively. For each specification (linear, quadratic, cubic, and quartic), the point estimate of β , its standard error, and the t -statistic for a test of whether β is different from zero is reported. Furthermore, the results from a test of joint significance of all included covariates using a seemingly unrelated regression model is reported at the bottom of each panel and specification. Reassuringly, even though a few individual covariates are statistically

⁹The covariate balancing test yields important information regarding the local randomization assumption needed for identification of β . Specifically, if covariates are unbalanced at the cutoff, this provide evidence that parents are systematically able to manipulate the timing of birth of their children.

¹⁰We have also used the annual share of parental leave days mothers take to match the timing definition of our main outcome variable (separation within x years), since the shares can be endogenously affected by the outcome. This exercise yields qualitatively similar results and is not reported here for brevity. The vast majority of parental leave is taken before the child turns three years old. See Table A1 in Appendix A.

TABLE 1—SAMPLE SUMMARY STATISTICS

	Reform year (1995)		Non-reform years (1996–2001)	
	Mean	SD	Mean	SD
<i>Family characteristics</i>				
Mother's share of parental leave	0.890	(0.141)	0.862	(0.135)
First born boy	0.511	(0.500)	0.514	(0.500)
Married at birth	0.342	(0.474)	0.399	(0.490)
Cohabiting at birth	0.569	(0.495)	0.527	(0.499)
Parents not together at birth	0.091	(0.288)	0.075	(0.264)
Both spouses highly educated	0.183	(0.387)	0.221	(0.415)
Father high-, mother low-educated	0.117	(0.321)	0.112	(0.316)
Father low-, mother high-educated	0.147	(0.354)	0.158	(0.365)
Both spouses low educated	0.553	(0.497)	0.509	(0.500)
<i>Spousal characteristics</i>				
Age mother	27.70	(4.49)	29.17	(4.44)
Age father	30.26	(5.37)	31.66	(5.29)
Mother foreign born	0.107	(0.310)	0.114	(0.318)
Father foreign born	0.107	(0.309)	0.119	(0.324)
Mother primary schooling	0.103	(0.304)	0.084	(0.278)
Father primary schooling	0.127	(0.333)	0.114	(0.317)
Mother secondary schooling	0.588	(0.492)	0.545	(0.498)
Father secondary schooling	0.572	(0.495)	0.553	(0.497)
Mother postsecondary schooling	0.308	(0.462)	0.370	(0.483)
Father postsecondary schooling	0.300	(0.458)	0.333	(0.471)
Mother pre-birth income, SEK	147,662	(88,745)	135,791	(105,243)
Father pre-birth income, SEK	190,055	(118,335)	221,097	(146,524)
Observations	72,819		611,001	

Notes: Means and (standard deviations) of characteristics for the parents to children within 12 months around the turn of each year, respectively. High educated is defined as having some postsecondary education. Pre-birth income is measured in the second year prior to the birth of the child.

different from zero in some specifications, the overall picture and joint tests suggest that there is little room for concern of systematic timing of birth in the data.¹¹

The lower panel of Table 2 presents diagnostic results from goodness-of-fit metrics by estimating the model with order one to four polynomial trends and a local linear estimator for different follow-up horizons of the outcome variable. Specifically, the first column provides the polynomial choice as recommended by minimizing the Akaike Information Criterion (AIC), which punishes additional model parameters with low information content. The second column reports the polynomial choice from a bins test by estimating models with a full set of birth month dummies together with the respective parametric trend where the recommended polynomial is the specification in which the set of bin dummies are jointly insignificant. Given the results from the balancing tests and the robustness of the results to the specification used, we work with the linear specification in our analyses.

¹¹ For completeness, Figure A1 in Appendix A provides graphical evidence of covariate balance.

TABLE 2—DIAGNOSTIC TESTS FOR THE REGRESSION-DISCONTINUITY SPECIFICATION

	Linear			Quadratic			Cubic			Quartic		
	Pt. Est.	SE	t-stat	Pt. Est.	SE	t-stat	Pt. Est.	SE	t-stat	Pt. Est.	SE	t-stat
<i>Panel A. Covariate balance tests</i>												
Predicted separation	0.001	0.000	1.453	-0.000	0.001	-0.223	0.000	0.001	0.353	0.001	0.001	0.520
Child gender	-0.007	0.008	-0.923	-0.010	0.012	-0.771	-0.026	0.018	-1.435	0.002	0.028	0.063
Mother's year of birth	0.185	0.069	2.665	-0.033	0.109	-0.305	-0.088	0.161	-0.548	0.039	0.239	0.162
Father's year of birth	0.239	0.096	2.498	0.082	0.147	0.554	-0.029	0.211	-0.139	-0.218	0.308	-0.709
Nonnative mother	-0.007	0.005	-1.527	-0.000	0.008	-0.045	0.008	0.011	0.757	0.006	0.017	0.342
Nonnative father	0.006	0.005	1.034	0.012	0.009	1.410	0.028	0.012	2.281	0.033	0.018	1.889
Spousal education gap	0.010	0.008	1.233	0.006	0.013	0.516	-0.002	0.018	-0.099	-0.019	0.026	-0.712
Spousal income gap	0.191	0.571	0.334	0.453	0.881	0.514	0.717	1.266	0.566	0.253	1.853	0.136
Joint χ^2			9.849			5.153			7.540			5.702
p-value			0.276			0.741			0.480			0.681
	AIC	Bins	Linear		Quadratic		Cubic		Quartic		LLR	
			Pt. Est.	SE	Pt. Est.	SE	Pt. Est.	SE	Pt. Est.	SE	Pt. Est.	SE
<i>Panel B. Specification tests</i>												
Mother's share of PL	4	4	-0.039	0.002	-0.038	0.003	-0.045	0.005	-0.051	0.007	-0.040	0.004
Separated < 1 year	2	2	0.012	0.005	0.008	0.007	0.001	0.011	0.015	0.016	0.009	0.007
Separated < 3 years	2	1	0.018	0.005	0.019	0.009	0.027	0.013	0.025	0.019	0.017	0.005
Separated < 5 years	2	1	0.006	0.006	0.009	0.009	0.025	0.014	0.026	0.021	0.005	0.008

Notes: Panel A reports the estimated β from equation (1) for a set of balancing variables using a linear, quadratic, cubic, and quartic RD specification, respectively. Predicted separation rate is obtained by estimating a logistic regression model of separation on a set of predetermined covariates: spousal income prior to child birth, mother's and father's age, parents' education level, immigrant status, whether the parents are married or cohabiting at child birth, and child parity. The χ^2 -test statistic of joint significance of all coefficients is obtained by estimating a seemingly unrelated regression equation system for the included variables, allowing for correlated error terms across equations. Panel B presents covariate balance tests for the linear, quadratic, cubic, and quartic RD specifications, respectively. Predicted separation rate is obtained by running an OLS regression of separation on predetermined covariates.

III. Main Results

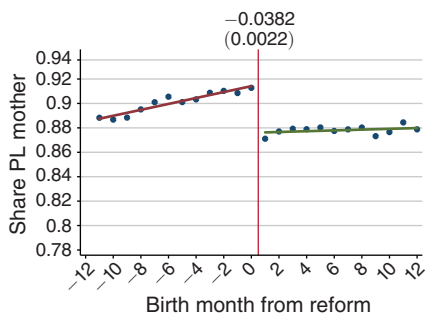
We first present some graphic evidence on the relationship between child birth month and outcomes using our 1995-reform sample. The upper panel of Figure 3 shows the average share of parental leave taken by mothers by birth month for our parental leave reform sample under a linear, quartic, and local linear trend specification, respectively. For all specifications the estimated discontinuities in the mother's share of parental leave are highly significant at the cutoff, decreasing by between 4 to 5 percentage points. Hence, consistent with Ekberg, Eriksson, and Friebe (2013), the graphical evidence indicate that the introduction of gender quotas in the Swedish parental leave system increased the father's share of parental leave.

The lower panel of Figure 3 analogously shows the separation effects of the parental leave reform by replacing the parental leave share with the share of separated couples three years after child birth. A clear increase of around 2 percentage points in the probability of separation is visible among couples with children born just after the reform was implemented. Only the estimate from the quartic specification cannot be statistically distinguishable from zero, but this is entirely due to low precision. The graphical evidence thus suggest that couples were more likely to be separated three years after the birth of their child as a consequence of the parental leave reforms.

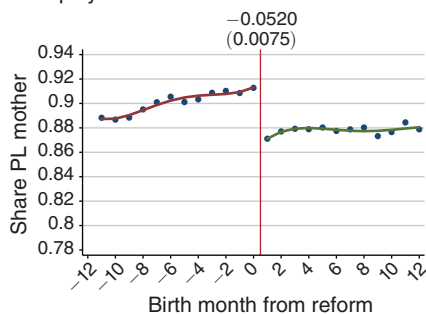
Table 3 reports point estimates of β and β_1 from estimating versions of (1) and (2), respectively. Column 1 shows the results from estimation of the basic RD setup

Panel A. Mother's parental leave share

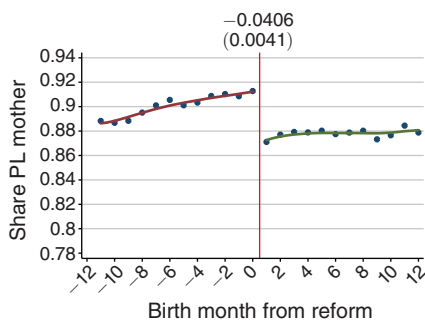
Linear fit



Quartic polynomial fit

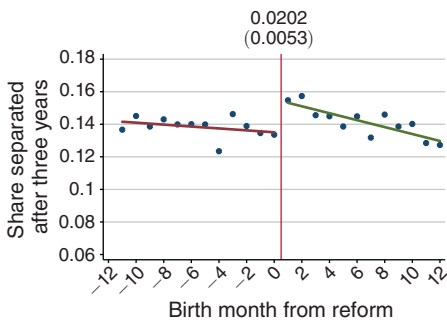


Local linear regression

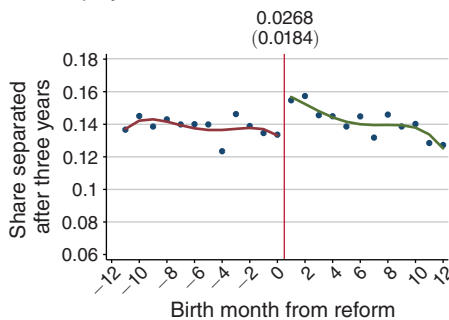


Panel B. Share separated after three years

Linear fit



Quartic polynomial fit



Local linear regression

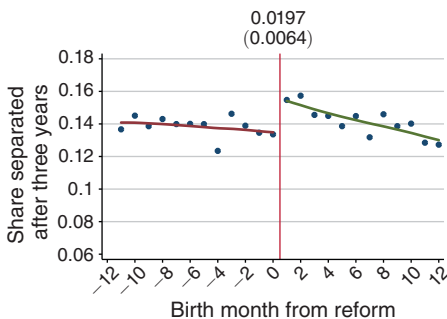


FIGURE 3. MOTHER'S PARENTAL LEAVE SHARE AND COUPLE SEPARATION

TABLE 3—THE EFFECT OF THE PARENTAL LEAVE REFORM ON SEPARATION

	RD reform year (1)	RD control years (2)	RD-DD (3)
Born after December 31	0.020 (0.005)	0.010 (0.002)	0.010 (0.005)
Control year fixed effects		✓	✓
Mean of outcome	0.140	0.115	0.118
Percent effect	0.144	0.087	0.086
Observations	72,819	611,001	683,820

Notes: Table estimates in column 1 and column 2 are based on the reform (1995) and non-reform years (1996–2001) reported in columns 1 and 2 of Table 1, respectively. Column 3 combines reform and non-reform year data. The table reports point estimates of β from equation (1) with linear trends in columns 1 and 2. The point estimate of β_1 in column 3 is obtained from estimation of equation (2). The percentage effects are calculated by dividing the point estimate with pre-reform mean of outcome reported in the table. Standard errors are in parentheses.

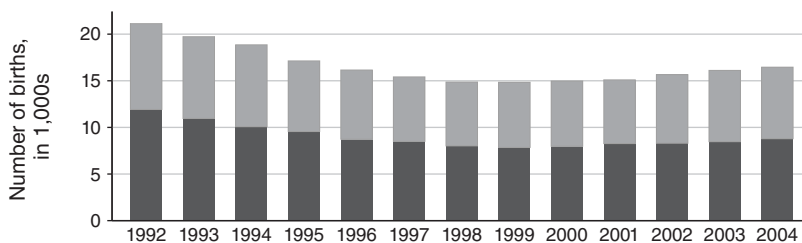
from (1) using only the 12-month interval around the reform cutoff in 1995, while column 2 provides corresponding results from pooling all the control years. Finally, column 3 reports point estimates of the RD-DD model from (2) including both reform and non-reform years.

The point estimate from column 1 is positive and statistically significant at the 1 percent level and suggests that the separation probability three years after child birth increased by 2 percentage points. Comparing this point estimate with the baseline couple separation and maternal parental leave shares in the pre-reform periods corresponds to an increase of around 14 percent. However, from column 2 we note that there also exists a separation “effect” in the years where no parental leave reform was implemented, suggesting that the mechanical relationship between birth month and follow-up time is indeed obscuring the estimate in column 1. Taking this into account in column 3 reduces the point estimate of the separation effect to around 1 percentage point and a more moderate 9 percent increase at baseline. This estimate is still highly significant.

IV. Robustness Checks

The RD design we apply relies on an assumption of local randomization of the running variable around the reform cutoff. In the current context this condition amounts to that parents do not have precise control of the timing of birth of their children. One potential concern is that couples with due dates close to the reform cutoff date might have postponed (or advanced) induced births and planned cesarean sections in order to be subject to (or avoid) the new rules. However, cesarean sections are relatively rare in Sweden and, as reported in Ekberg, Eriksson, and Friebe (2013), planned birth surgery for other than health-related reasons are considered highly unethical by health care providers. Furthermore, the results from the balancing tests for a set of potential confounding factors we previously provided did not indicate that manipulation is a serious issue in our application. Nonetheless, we also show in Figure 4 that the share of children born in January and December

Panel A. Number of January and December births



Panel B. Proportion January and December births

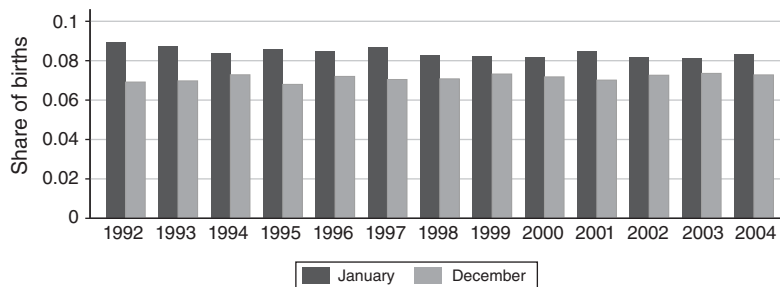


FIGURE 4. FREQUENCIES AND SHARES OF JANUARY AND DECEMBER BIRTHS

TABLE 4—SENSITIVITY TO BANDWIDTH CHOICE

	12-month window (1)	6-month window (2)	3-month window (3)	1-month window (4)	Donut RD (5)
Born after December 31	0.010 (0.005)	0.012 (0.007)	0.022 (0.011)	0.013 (0.016)	0.009 (0.005)
Control year fixed effects	✓	✓	✓	✓	✓
Mean of outcome	0.118	0.121	0.119	0.122	0.121
Percent effect	0.086	0.099	0.185	0.106	0.074
Observations	683,820	371,073	188,360	74,654	633,460

Notes: Table estimates are based on the reform (1995) and non-reform years (1996–2001) reported in columns 1 and 2 of Table 1, respectively. The table reports point estimates of β from equation (2) with linear trends for different bandwidths around the reform cutoff as indicated in the column title. The estimate in column 5 refers to estimating the equation excluding the two months closest to the cutoff (December and January). The percentage effects are calculated by dividing the point estimate with pre-reform mean of outcome reported in the table. Standard errors are in parentheses.

are similar across all years during our observation period. This provides additional evidence that manipulation of birth dates is unlikely to be a serious threat to our empirical strategy.¹²

Since the choice of a 12-month sampling window around the reforms might appear arbitrary, we provide additional estimates for 6-, 3-, and 1-month sampling

¹²In addition, a McCrary density test of manipulation around the reform cutoff could not be rejected at any conventional levels of statistical significance.

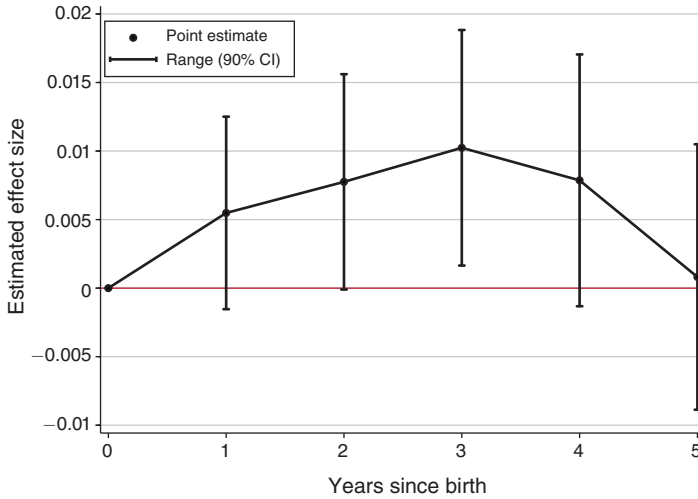


FIGURE 5. YEAR-BY-YEAR EFFECTS

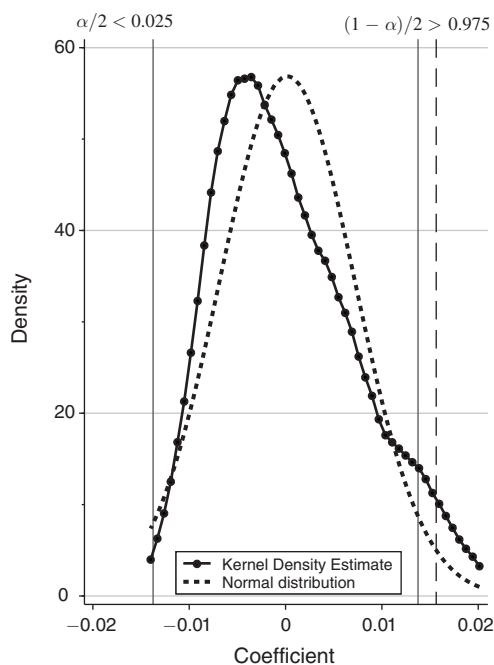
windows for the RD-DD estimate from Table 3 in columns 2, 3, and 4 of Table 4. We also report the results from a “donut” RD model in column 5, where we exclude the December and January months to assess whether potential manipulation of the birth date of children very close to the cutoffs drive the results. The table shows that the results are robust to the choice of bandwidth.

To examine potentially heterogeneous effects on separation by time since birth we estimate our RD-DD model for different follow-up periods up to five years after child birth. Figure 5 shows the estimates of the discontinuity at the threshold in our RD-DD setting by years since child birth. The estimated coefficients exhibit an inverse U-shaped pattern over time since birth, increasing until the third year, and thereafter decreases to a point estimate close to zero in the fifth year. Interestingly, since our outcome variable is defined cumulatively this pattern suggests that separations were re-timed (advanced) rather than separations that would not have occurred in absence of the reform.

Finally, we use both reform and non-reform years in a randomization inference design to perform a series of placebo analyses of the reform effect by shifting the policy intervention cutoff by one month at a time. Thus, we estimate a placebo intervention 72 times using our RD design defined in (1), with the intervention cutoff starting in every month from June 1995 to June 2001.¹³ As in our main specification we estimate the effect of birth month on couple separation in the third year after the child is born. Figure 6 illustrates the distribution of point estimates from this procedure (panel A) and the cumulative distribution of t -values from the series of regressions (panel B) compared to a zero-mean normal distribution. The point estimates from the placebo interventions are almost always lower than our estimated effect (indicated by the dashed vertical line) and, as expected, centered around zero

¹³ We use 6 rather than 12 months bandwidth in this analysis in order to obtain a more stable probability distribution of placebo coefficient estimates.

Panel A. Placebo estimates



Panel B. *t*-values from placebo estimates

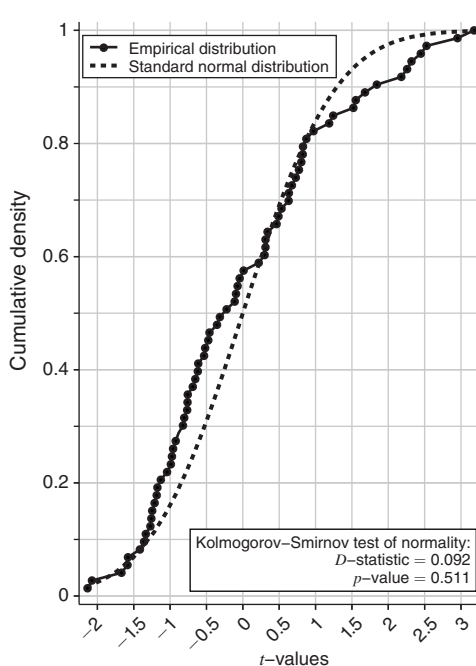


FIGURE 6. PLACEBO ESTIMATES FOR COUPLE SEPARATION

($\bar{\beta}_{placebo} = 0.0002$). Furthermore, a Kolmogorov-Smirnov test of normality of the empirical distribution of the placebo *t*-values cannot be rejected for any conventional significance level.

We conclude from this section that our estimates of increased separation from the implementation of the parental leave reform are largely robust to the choice of model specification, bandwidth, and outcome.

V. Mechanisms

Our main results from the previous section suggested that the 1995 parental leave reform, in addition to decreasing specialization within the household in terms of paid parental leave across couples, also increased the probability of separation. In this section, we attempt to probe the mechanisms underlying this response through a number of extensions.

A. Separation Dynamics

To further study the dynamics of the separation effects found in the previous section, we plot the separation hazard (panel A) and cumulative hazard (panel B) by child age for parents of children born in December 1994 and January 1995, respectively, in Figure 7. The separation hazard is greater for parents of January-born children up to three years after birth, after which the pattern reverses. The cumulative

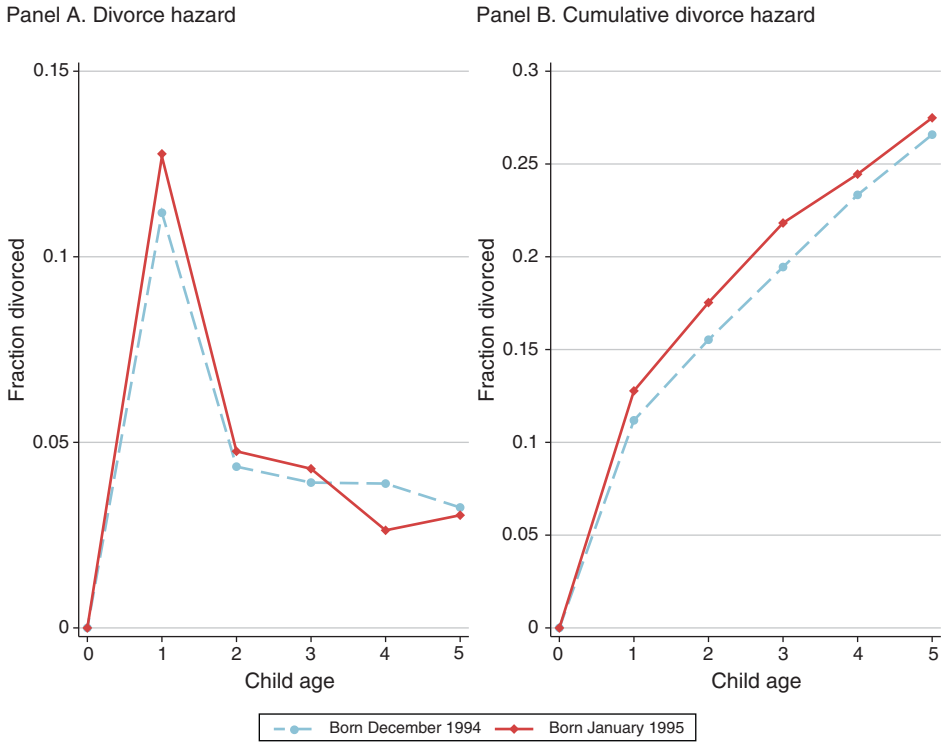


FIGURE 7. SEPARATION HAZARDS BY TREATMENT STATUS

hazard is always greater for those with January-born children over the follow-up horizon, but the difference diminishes with child age. This analysis hence suggests, in accordance with our previous findings, that the increased separations are re-timed separations rather than separations that would never have occurred in absence of the reform. One possible interpretation of this finding is that the fathers’ increased presence in the household implied an information shock to the spouses about their match quality. This conjecture is further supported by the timing of fathers’ parental leave uptake and the separation. Specifically, Table A1 in the Appendix shows that the majority of leave taken by fathers is used during the first two years after the child is born and the separation probability in Figure 7 appears to shift from year four to years one and two after child birth. This suggests that the separations occurred during the period when fathers used the majority of their parental leave.

B. Comparison with the 2002 Reform

The 2002 parental leave reform earmarked an additional month of paid leave to each parent, but was also accompanied with an extended duration of transferable paid leave of one month. This reform allowed parents to keep their previous distribution of parental leave take-up within the family and was thus less restrictive than the 1995 reform. Since the two parental leave reforms represents two different

TABLE 5—EFFECTS OF THE 2002 REFORM ON COUPLE SEPARATION

	RD reform year (1)	RD control years (2)	RD-DD (3)
Born after December 31	0.011 (0.004)	0.010 (0.002)	0.001 (0.004)
Control year fixed effects		✓	✓
Mean of outcome	0.103	0.115	0.112
Percent effect	0.106	0.087	−0.008
Observations	132,042	611,001	743,043

Notes: Table estimates in column 1 and column 2 are based on the reform (2002) and non-reform years (1996–2001). Column 3 combines reform and non-reform year data. The table reports point estimates of β from equation (1) with linear trends in columns 1 and 2. The point estimate of β_1 in column 3 is obtained from estimation of equation (2). The percentage effects are calculated by dividing the point estimate with pre-reform mean of outcome reported in the table. Standard errors are in parentheses.

ways of introducing paternity leave (reallocation of already existing paid leave from mothers to fathers and an expansion of entitlement to paid leave with the new paid leave entitlements attached to fathers), it is interesting to study whether they yield different results with respect to couple separation.

Table 5 reports point estimates of β and β_1 from estimating versions of (1) and (2) for the 2002 reform, using the 1996–2001 birth cohorts as control years. Column 1 shows results from estimation of the basic RD setup from (1) using only the 12 months interval around the reform cutoff in 2002, while column 2 provides corresponding results from pooling all the control years. Finally, column 3 reports point estimates of the RD-DD model from (2) including both reform and non-reform years.

The point estimate from column 1 is positive and statistically significant at the 1 percent level, suggesting that the separation probability three years after child birth increased by 1.1 percentage points; about half the magnitude of the 1995 reform. Moreover, from column 2, we see, as expected, an equally large separation “effect” in the years where no parental leave reform was implemented, suggesting that the estimate in column 1 is completely accounted for by the mechanical relationship between birth month and follow-up time. Explicitly taking this spurious relationship into account in the RD-DD specification reported in column 3 shows that the 2002 reform had no impact on the separation probability. Hence, the two reforms in 1995 and 2002 yield different results, perhaps due to that the latter reform was more flexible in terms of allowing couples to decide their within-household distribution of parental leave.

C. Reform Compliers

One possible channel that the 1995 parental leave reform may have affected couple separation might have been through a change in the intra-household distribution of parental leave of specific couple types. Specifically, if fathers who were unlikely to take any leave in absence of the reform were those primarily affected by the reform, then it is plausible that role conflicts might be an underlying cause for the increased

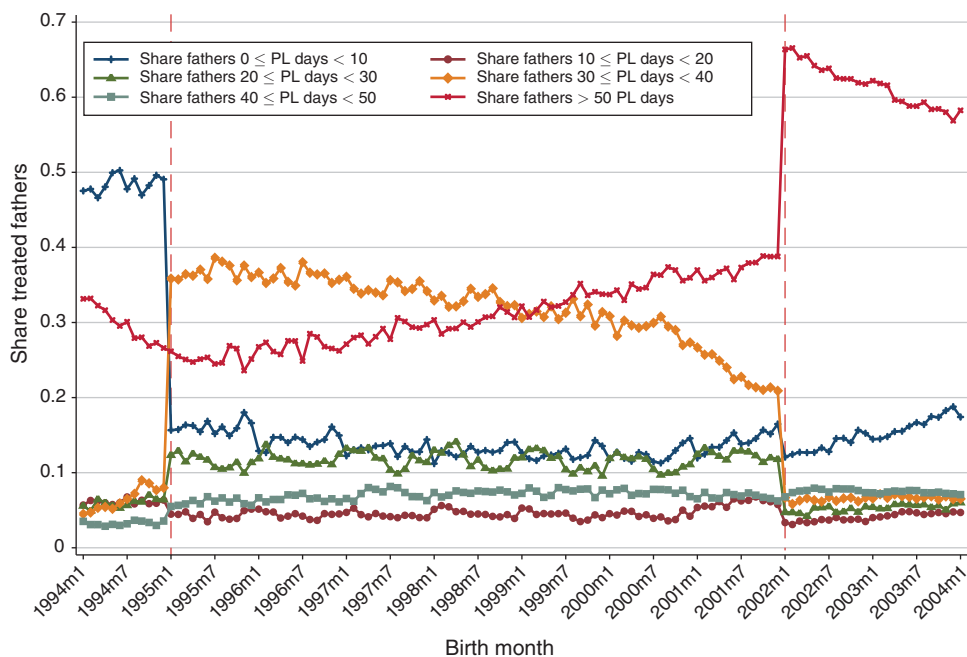


FIGURE 8. SHARE OF PATERNITY LEAVE GROUPS OVER TIME

separation probabilities. On the other hand, if affected fathers were mainly those that already planned to take some leave, such a story would be less convincing. To study this conjecture, we compare the distribution of fathers' parental leave before and after the reform was implemented. To this end, we generate groups defined by the number of leave days fathers were taking separately by child birth month: 0–9 days, 10–19 days, 20–29 days, 30–39 days, 40–49 days, and 50 and more days. The shares of each group are plotted by birth month for all children born between 1994 and 2004 in Figure 8.

The share of fathers who took very few days of parental leave (0–9 days) in 1994 was constant at around 50 percent in the period up until January 1995, when it dropped dramatically to around 15 percent. The corresponding increase in the group of fathers who took 30–39 days of parental leave (and the absence of significant changes in the other groups) suggests that the change in fathers' parental leave uptake was exclusively driven by fathers who were not planning to take any (or very few) days of leave, switching to taking leave equal to the quoted amount of days.

A similar pattern is visible around the introduction of the 2002 reform, but for a quite different subpopulation of fathers. The changes in the parental leave distribution is mainly driven by fathers who before January 2002 planned to take between 30 and 39 days of parental leave but, as a consequence of the reform, increased their uptake to more than 50 days. Interestingly, one can also observe a declining trend for fathers in the one “daddy-month” group (30–39 days) and a corresponding increase in the two “daddy-months” group (>50 days) in the time between the two reforms. This pattern might indicate a gradual change in increased acceptance of paternal leave over time, in addition to the direct impact of the reform. While the change

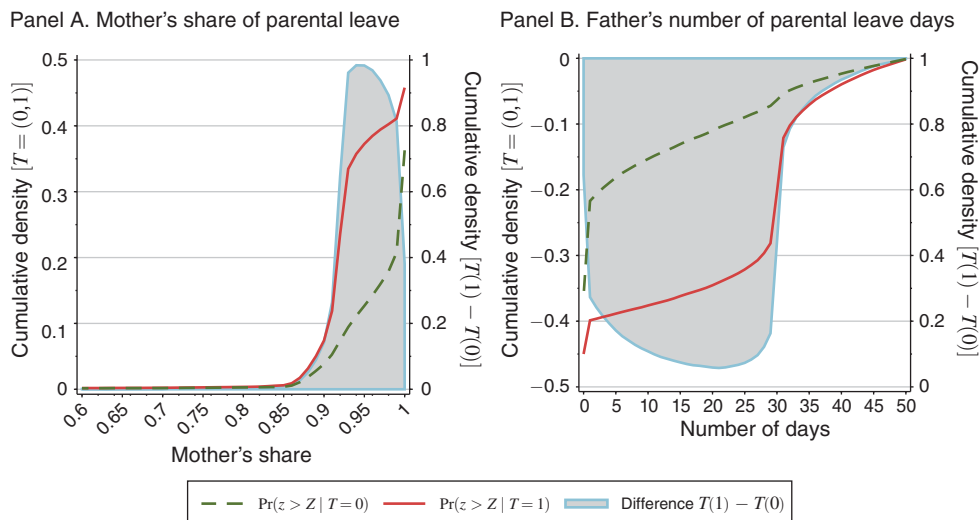


FIGURE 9. DISTRIBUTION OF SPOUSAL PARENTAL LEAVE TAKE-UP BY CATEGORY

in the take-up of fathers' parental leave was equal in terms of magnitude in the 2002-reform, the affected subpopulation was quite different from the 1995 reform and consisted mainly of fathers who were already intending to take substantial amounts of parental leave.

To evaluate how the distribution of parental leave changed due to the 1995 reform, we plot the empirical CDF of mother's share of parental leave for couples whose children were born before and after the reform cutoff using our reform sample with a 12-month bandwidth. Panel A of Figure 9 displays the CDF together with the quantile-specific difference. As expected, the shift in the mother's share of parental leave is entirely located in the upper part of the distribution with essentially all density located above the eighty-fifth percentile. To see more clearly how many days of paternal leave this shift corresponds to, panel B shows the empirical CDF for the fathers' number of days on parental leave. It is remarkably clear from the figure that the differences in the parental leave distribution are almost entirely due to a shift from 0 to 30 days. Hence, the compliers of the 1995 reform were almost exclusively traditional couples who would have allocated all parental leave days to the mother. This subpopulation of parents were arguable also the one where conflicts from a change in gender roles may have been the most acute, possibly explaining the separation effect in the 1995 reform (and the lack of one in 2002).

D. Income Effects

The parental leave reform altered the within-household division of paid parental leave days by reducing the mother's share of paid leave. To the extent that the reduced parental leave among women increased their intra-household share of earnings due to an increase in their labor supply, this might offer a potential mechanism for the estimated increase in separation probability. However, due to the significant

TABLE 6—REFORM EFFECTS ON SPOUSAL AND HOUSEHOLD LABOR INCOME

	Mother's income (1)	Father's income (2)	Household income (3)	Mother's income share (4)
Born after December 31	−24,091 (5,273)	−25,476 (13,615)	−60,804 (16,012)	−0.004 (0.004)
Control year fixed effects	✓	✓	✓	✓
Mean of outcome	422,686	1,103,090	1,536,050	0.303
Percent effect	−0.059	−0.023	−0.040	−0.013
Observations	683,699	617,809	617,688	615,910

Notes: Table estimates are based on the reform (1995) and non-reform years (1996–2001) reported in columns 1 and 2 of Table 1, respectively. The table reports point estimates of β from equation (2) with linear trends for different outcomes: the total labor income earned during the first three years after birth by the mother (column 1), the father (column 2), and by the two spouses jointly (column 3). Column 4 reports the results on the mother's share of total household income. The percentage effects are calculated by dividing the point estimate with pre-reform mean of outcome reported in the table. Standard errors are in parentheses.

flexibility of the parental leave system, where unpaid leave is job-protected and where parents have the right to reduce their working hours, it is not obvious that a decreased parental leave take-up among mothers translates to decreased labor supply.

To investigate these issues, we study the effects of the 1995 parental leave reform on the accumulated labor income by the mother and the father during the first three years after the birth of their child. Columns 1 and 2 of Table 6 report the effects on mothers' and fathers' total labor income during the first three years after birth, respectively. The results show that the parental leave reform decreased fathers' labor income by 2.3 percent, corresponding to a little less than one month of market work for this group. However, mothers' labor income also decreased, suggesting that they compensate for the decrease of parental leave days with unpaid leave rather than increasing their labor supply. This finding implies that the reform led to a decreased income for the family as a whole (column 3), but no change in the mothers' share of total household income (column 4). This might be a potential explanation for the effects we find on separation probabilities as the reform led to lower household income, which could potentially cause strain on the relationship. Moreover, an increase in mothers' unpaid leave could coincide with fathers' leave-taking, resulting in more room for conflicts.

To study this conjecture more closely, we estimate heterogeneous effects on both separation and mothers' labor earnings by mothers' pre-birth income quartile. The results are presented in Table 7 and indicate that the compensating behavior of mothers is concentrated in the lower tail of the income distribution; the exact same group for which the increased separation probability is the highest. In fact, we find no effects on separations for mothers belonging to the third and fourth quartiles of the pre-birth income distribution.¹⁴

¹⁴The conjecture that spousal adjustment behavior could affect separations is supported also in a recent paper by Folke and Rickne (2016), who find that being promoted significantly increases the divorce risk for women while the reverse seems to hold for male promotions. The authors argue (lack of) spousal adjustment behavior as

TABLE 7—HETEROGENEOUS EFFECTS BY MOTHER'S PRE-BIRTH INCOME

Income quartile	Q1		Q2		Q3		Q4	
	Separation	Income	Separation	Income	Separation	Income	Separation	Income
Born after December 31	0.023 (0.013)	-22,088 (7,247)	0.021 (0.011)	-13,480 (7,010)	0.001 (0.008)	-9,517 (5,762)	-0.003 (0.001)	-12,767 (16,048)
Control year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Mean of outcome	0.171	231,014	0.132	352,482	0.094	419,010	0.074	689,375
Percent effect	0.137	-0.096	0.156	-0.038	0.011	-0.023	-0.036	-0.019
Observations	170,457	170,449	170,473	170,466	170,446	170,441	170,452	170,447

Notes: Table estimates are based on the reform (1995) and non-reform years (1996–2001) reported in columns 1 and 2 of Table 1, respectively. The table reports point estimates of β from equation (2) with linear trends for different outcomes: couple separation and the total labor income earned by the mother during the first three years after birth. The model is estimated separately by mother's pre-birth income quartile. The percentage effects are calculated by dividing the point estimate with pre-reform mean of outcome reported in the table. Standard errors are in parentheses.

VI. Conclusion

Despite gender neutrality of the generous governmental financial support available to parents in Sweden, the vast majority of paid leave is taken by mothers. To increase incentives for fathers to increase their share, the Swedish government reformed the system in 1995 by earmarking one month of paid leave to each parent. Such changes may alter the marital surplus, positively or negatively, and leave room for unintended consequences of the reforms on family structure. We exploit this reform in a regression discontinuity difference-in-differences design to study its impact on the marital stability of couples with young children.

Consistent with previous research, we find that the reform altered the distribution of parental leave, leading to an increase in fathers' take-up with, on average, the full earmarked month. More novel, our estimates show that the reform increased the probability of separation of couples by about 9 percent three years after the birth of their child. This result is robust to a number of robustness checks we perform.

Results from a number of extensions suggest that the additional separations were re-timed dissolutions of poor matches rather than separations that would otherwise not have occurred. Furthermore, studying the distributional shifts of parental leave around the reform cutoffs, we find that the compliers to the reform mainly consisted of traditional couples in which fathers were otherwise unlikely to take any parental leave. We also find that labor income decreased for both fathers and mothers as a consequence of the reform, suggesting that mothers also took additional unpaid leave. Finally, comparing the results with a second reform in 2002, in which an additional transferable month was added in addition to another earmarked month, we find that this later reform did not affect marital stability. Taken together, we argue that the increased separations were probably due to a combination of the

a potential explanation; promoted women earning more than their husbands continue to do most of the housework while their husbands do not adjust their time spent in market or household work.

restrictiveness of the reform, the affected population, and the income effects. These factors may have increased household role conflicts and were also likely to be less severe in the 2002 reform. In terms of policy, our results highlight that the way in which family policies are implemented matter for important family outcomes. Research on such indirect effects deserve further attention in order to understand the mechanisms of gender norms and, in particular, to provide a knowledge basis for future parental leave policies.

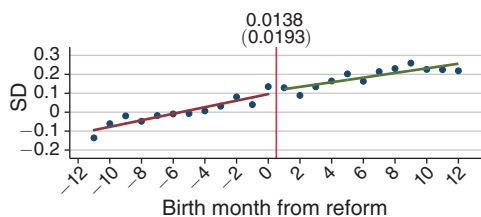
APPENDIX

TABLE A1—PARENTAL LEAVE TAKE-UP BY CHILD AGE

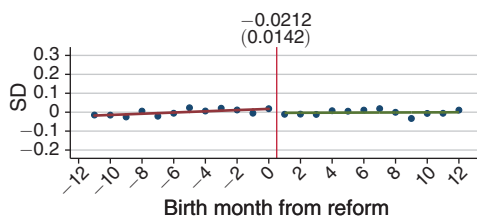
Child age	Mothers		Fathers		Mother's share
	Days (1)	Percent (2)	Days (3)	Percent (4)	Share (5)
First year	203.10 (87.29)	48.31	21.48 (41.92)	30.99	0.89 (0.17)
Second year	80.59 (57.76)	19.17	18.60 (30.30)	26.84	0.80 (0.25)
Third year	35.76 (41.06)	8.51	6.24 (16.26)	9.00	0.80 (0.28)
Fourth year	22.04 (30.66)	5.24	4.61 (13.52)	6.65	0.80 (0.29)
Fifth year	19.16 (26.26)	4.56	4.15 (12.71)	5.99	0.79 (0.30)
Sixth year	18.85 (26.81)	4.48	4.39 (12.99)	6.33	0.76 (0.30)
Seventh year	18.30 (25.63)	4.36	4.32 (12.76)	6.23	0.75 (0.31)
Eighth year	22.59 (28.03)	5.37	5.52 (14.23)	7.96	0.71 (0.31)
Total	420.39	100	69.31	100	—

Notes: The table reports the number and share of parental leave days taken by the mother and father, respectively, and maternal share of total parental leave by child age. Standard errors are reported in parentheses.

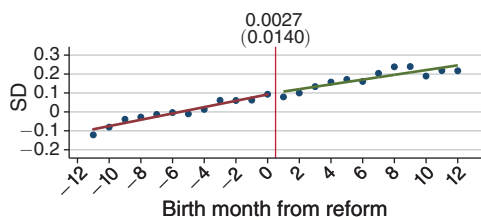
Panel A. Predicted separation



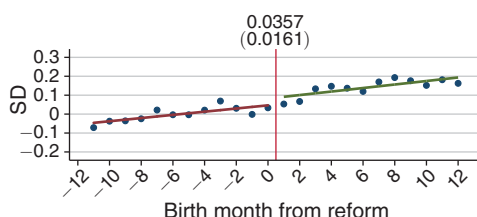
Panel B. Child gender



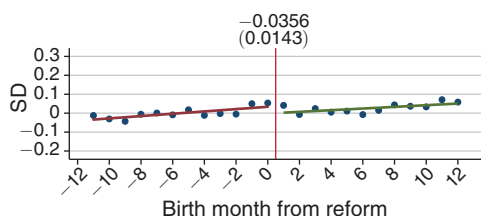
Panel C. Mother's year of birth



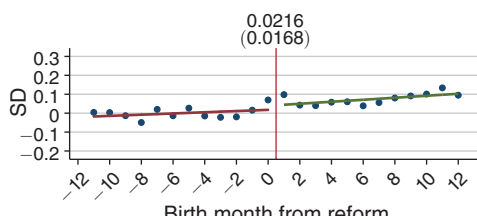
Panel D. Father's year of birth



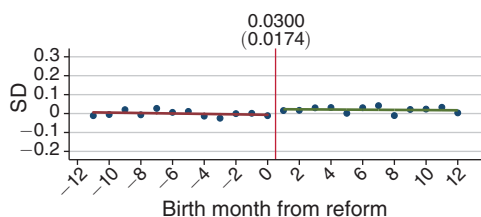
Panel E. Nonnative mother



Panel F. Nonnative father



Panel G. Spousal education gap



Panel H. Spousal income gap

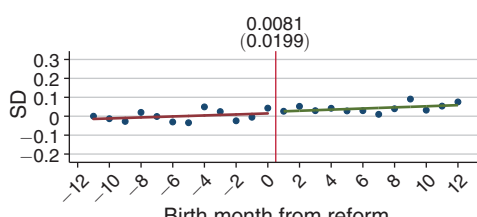


FIGURE A1. COVARIATE BALANCE TESTS

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