IFAU
Institute for Evaluation of Labour Market and Education Policy

Modern family? Paternity leave and marital stability

Daniel Avdic
Arizo Karimi

WORKING PAPER 2016:23

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

More information about IFAU and the institute's publications can be found on the website www.ifau.se

ISSN 1651-1166

Modern Family? Paternity Leave and Marital Stability[a]

by

Daniel Avdic[b] and Arizo Karimi[c]

7th December, 2016

Abstract

We study the effects of unanticipated changes to the intra-household division of parental leave on family stability exploiting two parental leave reforms in Sweden. Using a fuzzy regression discontinuity design, we find that a decrease in the mother's share of parental leave increases the probability of separation among couples that were married or cohabiting at the time of the reforms. Our results also suggest a lower likelihood of cohabiting couples to upgrade to marriage. Examination of reform compliers reveal that the increased separation risk is mainly driven by more traditional couples, and among couples with previous children.

Keywords: marital stability; parental leave; intra-household division; regression discontinuity

JEL-codes: C26, D13, J13, J31

[a] We thank seminar participants at the Society of Labor Economists (SOLE) annual meeting in 2016, Institute for International Economics Studies (IIES), CINCH, Essen; Institute for Evaluation of Labour Market and Education Policy (IFAU), Uppsala; the Workshop in Family Economics 2015, University of Copenhagen; Crawford School of Public Policy, Australian National University; Department of Economics, University of Bergen, the "Fertility – New Economic Perspectives on an Old Topic" health conference in Essen, and the Economics Department, Linneaus University. We also thank Hans van Kippersluis, Erica Lindahl, Arash Nekoei, and J Peter Nilsson for valuable comments. Arizo Karimi gratefully acknowledges financial support from the Jan Wallander and Tom Hedelius foundation.

[b] University of Duisburg-Essen, IFAU and CINCH. Address: CINCH — Health Economics Research Center, Weststadttürme, Berliner Platz 6–8 DE-451 27 Essen, Germany. Phone: +49 (0)201 183 6326. E-mail: daniel.avdic@uni-due.de.

[c] Department of Economics, Uppsala University, Uppsala Center for Labour Studies (UCLS), and the Institute for Evaluation of Labour Market and Education Policy (IFAU). E-mail: arizo.karimi@nek.uu.se.

Table of contents

1 Introduction

The classical economic approach to family formation, beginning with the seminal paper by Becker (1973), views partnership formation as a rational choice when the gains from marriage exceed the gains from remaining single, with marital gains determined by production complementarities. However, with the increase in women's labor force participation and educational attainment, the comparative advantage of wives in the domestic sphere, relative to market work, has declined and therefore reduced the value of specialization within marriages. Thus, production complementarities have become decreasingly central to the modern family over time (see, e.g., Stevenson and Wolfers, 2007).[1]

Despite these comprehensive structural changes in the labor market, substantial gender gaps still remain in many areas of society. One particularly persistent gender difference, seemingly almost immune to women's labor market progress, is the time spent at home with children. As the gender gap in income *potential* has decreased over time, the gender earnings gap seems to increasingly appear as a *result* of childbearing in industrialized countries (see e.g. Kleven et al., 2015). For instance, in Sweden, a country with virtually no gender difference in labor force participation, where educational attainment favors women, and with long-standing equal parental leave rights for mothers and fathers, women still accounted for more than three quarters of the total parental leave uptake in 2012. Hence, from the viewpoint of classical family economics this appears a puzzle to the extent that women's comparative advantage in home production is derived from lower human capital investments and labor market experience. A recent literature has therefore instead explored these issues from the perspective of societal norms; in particular, behavioral prescriptions about what is considered appropriate behavior of men and women. Proponents of this perspective suggest that gender norms may indeed play a role in couple formation and dissolution,

[1]For instance, the development of labor-saving technology and service industries now allows much of what was once typically provided by women who specialized in non-market work to be purchased in the market.

as well as in the continuation of within-household specialization with respect to household tasks and child rearing (Bertrand et al., 2015; Kleven et al., 2015).

In this paper, we consider a potential explanation for the persistence in household specialization with respect to time spent at home with young children. In particular, we exploit exogenous shocks to the intra-household allocation of time devoted to child rearing among dual-earner couples, derived from two parental leave quota reforms in Sweden, and study how they affect marital stability of couples. Such shocks may theoretically alter any marital surplus from couple formation with respect to household specialization and therefore potentially forcing couples to re-optimize from their initial plans. Alternatively, policies inducing unexpected changes in the intra-household parental leave division might force couples to deviate from social norms regarding the appropriate allocation of time to paid labor and child rearing activities between husbands and wives. While our analysis does not explicitly separate between these two potential mechanisms, it allows us to gauge the potential value to couples of specialization when it comes to the division of child rearing activities – in the form of marital gains, or in conforming to social convention. We focus on couples with joint children, and quantify the impact of a change in the mother's share of parental leave on the likelihood of divorce among married couples, on separation among cohabiting couples, and on the probability of cohabiting couples to upgrade to marriage.

We exploit quasi-experimental variation in mothers' share of parental leave take-up generated by two reforms in the Swedish parental leave system implemented in 1995 and 2002, respectively. Before 1995, entitlement to parental leave was gender neutral, with mothers and fathers receiving the same number of paid leave days for a child. However, parents were free to transfer the paid days to each other, which in practice meant that fathers transferred most of their paid leave to the mother. To encourage fathers' involvement in child rearing the Swedish government earmarked one month of paid leave to each parent. Since mothers before the reform accounted for essentially all parental leave take-up, the reform implied that one month of paid leave was effectively reallocated from the mother to the father. All parents of children born on January 1st 1995 or later were subject to the new rules. To further increase fathers' share of parental leave, a second non-transferable month of paid leave was introduced, targeting parents of children born on January 1st 2002 or later. The 2002-reform was also accompanied by a general expansion of paid leave by one month, which was transferable. Thus, we study two different reforms representing two different ways of introducing paternity leave, namely the reallocation of already existing paid leave from mothers to fathers, or the expansion of entitlement to paid leave with the new paid leave entitlements attached to fathers.[2]

To implement the analysis, we use longitudinal individual-level data on fertility, parental leave take-up, and marital status from several Swedish administrative registers that allow us to identify couples by unique family identifiers. We use the introductions of the reforms in a fuzzy regression discontinuity design to study the causal effect of the intra-household division of parental leave on marital stability. Thus, our empirical strategy allows us to account for several important issues relating to empirical identification of our key parameter. First, individuals that expect a higher likelihood of divorce may, for instance, adjust their labor supply to insure against marital separation, rendering intra-household allocation of time endogenous to couple stability. For example, Johnson and Skinner (1986) find that women who anticipate a divorce are more likely to participate in the labor market, suggesting that causality may also run in the opposite direction.[3] Second, partner sorting in the marriage market may create a sample selection problem since couples may form on the basis of preferences for specialization so that match quality is higher for couples with high preferences for specialization, thereby reducing divorce risks. Finally, omitted variable bias poses a problem in that standard selection-on-observables methods may leave out important in-

[2] An example of the latter is found in Iceland, where parents can extend parental leave by one month provided that the father takes the additional leave.

[3] Furthermore, viewing time inputs for children as a marital-specific investment in child quality introduces an additional source of reverse causality: while child quality may raise the value of the marriage, the possibility of divorce may also discourage the accumulation of such marital-specific capital (Becker et al., 1977).

directly mediated correlates between specialization and divorce probability, such as, for example, spousal health status. While our approach is able to avoid all these problems, we instead rely on an assumption that parents cannot precisely control the birth date of their children which we carefully investigate to allay concerns of estimation bias.

Our analysis provides four main empirical findings: first, consistent with Ekberg et al. (2013) who found that the 1995-reform increased parental leave take-up of fathers, we show that the introduction of the gender quotas in parental leave significantly decreased mothers' intra-household average share of parental leave days. Second, our estimates reveal that the decreased specialization within the household *increased* the probability of couple dissolution, by roughly ten percent at baseline. Interestingly, despite differences in design, the two reforms yield relatively similar effects on couple dissolution risk, albeit effects are somewhat smaller in the 2002-reform. Third, among cohabiting couples, our results indicate that an unexpected decrease in household specialization also decreased the likelihood of upgrading to marriage. These two latter results hence suggest that considerations and agreements regarding specialization during couple formation and/or gender social norms may be critical for the longevity of relationships. Fourth, we find some evidence that cohabiting couples react more strongly than married couples to changes in the division of parental leave, suggesting, in line with the findings of, for example, Stevenson and Wolfers (2006), that higher costs associated with dissolving a marriage union inhibit separation. In a recent paper from Denmark, Svarer and Verner (2008) find that after controlling for the endogeneity of fertility to marital risk, having children increases the risk of marital dissolution. Our results show that the parental leave division may affect divorce risk over and above the effect of having children.[4]

Using supplementary data from a longitudinal household survey, we find no evidence that the effect is mediated by significant changes in the allocation to

[4]See also Lillard and Waite (1993) for a survey of the empirical literature studying the effects of children on marital dissolution.

labor market or home production; in fact, when studying spousal earnings responses, suggestive evidence shows that women compensate for the decreased paid parental leave with increases in unpaid leave. The negative effect of the reforms on mothers' earnings is consistent with Cools et al. (2015), who find similar effects from the Norwegian "daddy-month" reform. Nevertheless, the results run counter to studies on the effects of spouses' relative income on marital stability, which often find that unexpected increases in the wife's earnings capacity raises the divorce hazard, while the reverse is true for increases in the husband's earnings (Weiss and Willis, 1997; Heckert et al., 1998; Jalovaara, 2003; Liu and Vikat, 2004).[5] A possible explanation could be that the reforms induced couples to spend more time together at home, creating room for conflict over the allocation of work.

While our obtained results may at a first glance suggest that quota policies are harmful for family stability, there are some important caveats with this interpretation that deserve attention. Specifically, when analyzing reform compliers, we find that the effects are mainly driven by couples whose parental leave would have been very unequally divided in the absence of the reforms. Furthermore, exploring the distribution of complier characteristics, we find that complier couples are more likely to be "traditional" in the sense that the husband is the breadwinner of the household and the wife is more likely to have low education. Hence, one possible interpretation of the results is that, in such family constellations, breaking traditional gender norms may generate more tension than in other, more progressive, families. This interpretation is further reinforced in an analysis of the estimation of birth order effects, in which we find that the increased likelihood of

[5] In addition, Tjøtta and Vaage (2006) find that public transfers, child allowance and child support allowances have a significantly positive effect on divorce in Norway, and that the distribution of public transfers in favor of the wife increases this probability. A recent study by Bertrand et al. (2015) find that couples where the wife earns more than the husband and where the wife is predicted to earn more than her husband are more likely to separate and less likely to form, respectively. On the other hand, other studies suggest that income equality within the household is positively related to couple stability. Brines and Joyner (1999) find that partners whose earnings are similar face reduced risks of breaking up, but that the effects of inequality are asymmetric: inequality is more disruptive when the woman earns more than her partner.

separation due to the reforms are mainly driven by families who already had children before the reform, suggesting that first-time parents may be more open to challenging existing conventions in child-rearing. One possible policy implication could be that the short-run adverse effects of the reforms may be cushioned or even countered by longer-term impacts on gender norms. This evidence is in line with Dahl et al. (2014) who find positive and over time increasing spillover effects in parental leave among fathers, exploiting a similar reform in Norway. This discussion may also be broadly related to the issue of potential adverse short-run effects of gender quotas in general – in corporate boards or political representation – and their potential to alter norms in the long-run (see, e.g., Bertrand et al., 2014).[6]

2 Institutional setting and the reforms

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 (e.g., Lalive et al., 2014; Patnaik, 2016; Kotsadam and Finseraas, 2011; Rege and Solli, 2010; Dahl et al., 2013; Schönberg and Ludsteck, 2007), fertility (Lalive and Zweimüller, 2009), and child outcomes (e.g., Carneiro et al., 2015; Cools et al., 2015; Liu and Skans, 2010). The Scandinavian countries, however, were early adopters of governmentally paid leave, and the Swedish parental leave system was introduced already in 1974, replacing the preceding maternity leave to make eligibility to paid parental leave gender neutral. Both the mother and the father are given an equal number of paid leave for their children, but with the option of transferring paid leave days between each other. Parental leave benefits to care for young children are paid by the governmentally and divided into three components: First,

[6] In a recent paper, Folke and Rickne (2016) find that being promoted significantly increases the divorce risk for women, and the reverse for male promotions. They argue (lack of) spousal adjustment behavior as 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.

ten days of 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 60-180 SEK (during the time period studied). 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. For individuals that do not meet the work requirement, all parental leave days are compensated with a low, fixed amount per day. In total, parents thus receive 480 days of paid leave for each child.

The parental leave is job protected, and can be used very 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 with up to 25 percent until the child turns eight years old. Thus, the governmentally paid parental leave benefits do not have to be claimed in one sequence and, in addition, can be claimed on a part-time basis until the child's eighth birthday. This implies that parents are able to prolong their parental leave, by, e.g., claiming benefits for 75 percent, while staying at home full-time. Similarly, parental leave benefits can be "saved" and used to extend, e.g., vacations or claimed when the child is older. While employers cannot deny parental leave to workers, such requests must be made at least two months in advance.

2.1 Introduction of paternity leave quotas

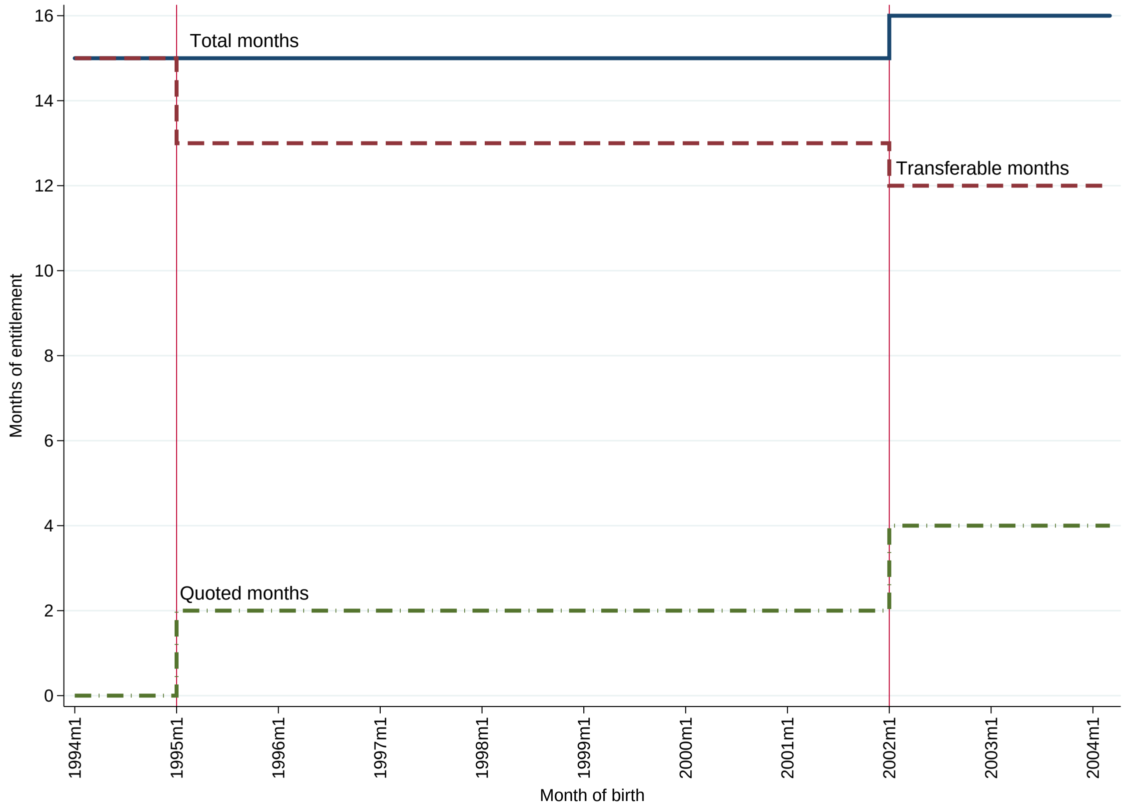
To analyze the effect of the intra-household division of parental leave, we exploit the implementations of two "daddy-month" reforms in 1995 and 2002. Before the introduction of the 1995-reform, parents were given an equal share of the total paid leave, but were free to transfer paid leave days to each other. In practice, this meant that fathers transferred most of their paid leave to mothers. The

1995-reform, however, implied that one month of the wage-replaced leave was earmarked to each parent, such that one month of paid leave could not be transferred to the other parent. For eligible parents, one month of parental leave benefits would thus be lost if the father 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 1st 1995 being subject to the new rules.

In order to further promote fathers' parental leave usage, the government introduced a second "daddy-month" in 2002. For parents to children born on or after January 1st 2002, one additional month of wage-replaced leave was earmarked to each parent. 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 effects of the 1995-reform were evaluated in a recent paper by Ekberg et al. (2013), who find strong short-term increases in fathers' parental leave take-up but no spillover effects on the long-term division of household work, measured as the relative share of leave taken to care for sick children.[7] Eriksson (2005) evaluated the effect of the second "daddy-month", introduced in 2002, and finds that this reform increased fathers' parental leave from around one month of leave to two months.

[7] Cools et al. (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.

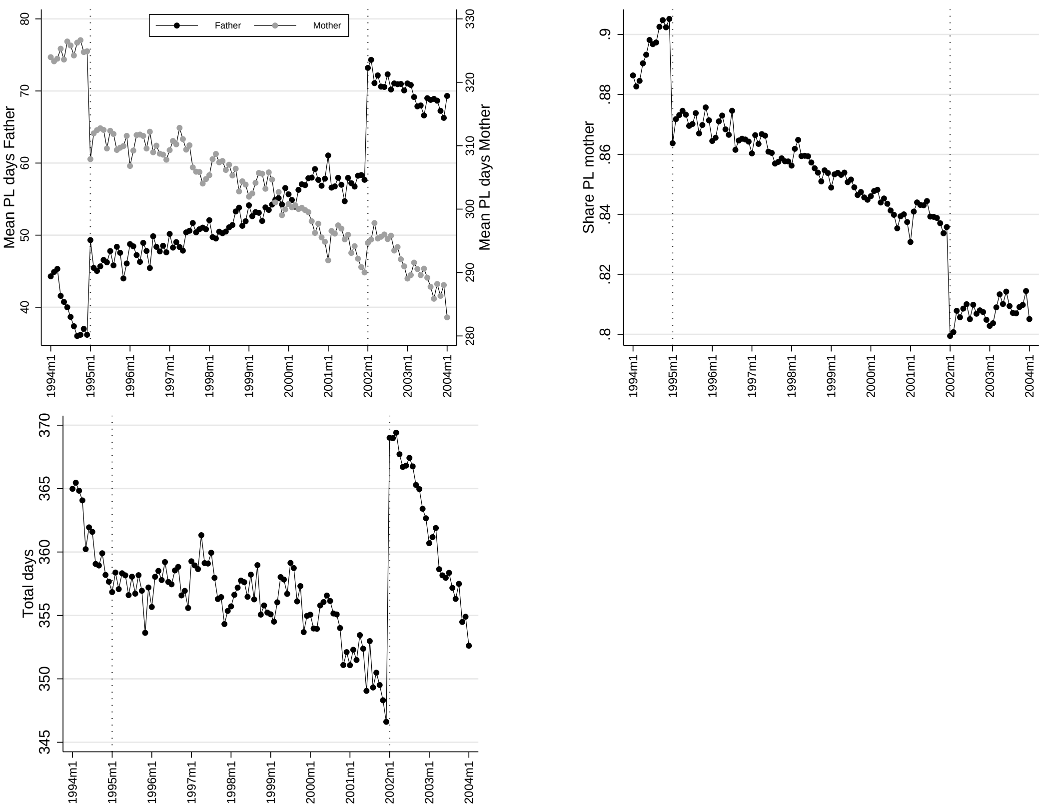
FIGURE 1.
Parental leave in Sweden: Entitlement rules over time



NOTE.— The figure illustrates the legislative impacts of the two "daddy-month" reforms in the Swedish parental leave system.

Consistent with previous work, we find that both the first and the second "daddy-month" reforms led to a sharp increase in fathers' take-up of parental leave and, in the case of the 1995-reform, a decrease in mothers' take-up. The upper left graph 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 immediately after December 1994 compared to parents of earlier-born children. The 2002-reform also increased fathers' take-up but also the parental leave taken by mothers due to the general increase in entitlement to paid leave of one month that accompanied the introduction of the second "daddy-month". As shown in the upper right graph of *Figure 2*, both reforms seem to have decreased the mother's intra-household *share* of parental leave take-up. Finally, the lower left graph shows that, in accordance with the new rules, the 2002-reform increased the total leave taken for children by around 25 days. Thus, parents seem to make full use of the entitled, wage-replaced parental leave benefits.

FIGURE 2.
Total parental leave uptake in Sweden by child birth month



NOTE.— The upper left graph shows the average number of parental leave days taken by mothers and fathers, respectively, by child birth month. The upper right graph shows mothers' share of parental leave, and the lower graph the total number of leave days taken, by child birth month.

2.2 Swedish family laws: custody and alimony

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 themselves, the other spouse is responsible for supporting them. Upon divorce, an economically disadvantaged divorcee 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 divorcee re-marries, their entitlement to alimony payments is maintained, although the need for this support may be re-evaluated.

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.

3 Data and empirical specification

The data set that we use to examine the relationship between the intra-household division of parental leave and family structure is based on a combination of several Swedish administrative registers. We use the multi-generational register, which links all children to their biological parents, to attain information on the birth year and birth month of individuals' children. The register includes unique identifiers for each child, mother, and father, allowing us to match couples with joint children via the child identifiers. We restrict attention to mothers whose first child was born during 1994-2005, and retain information on all their children and the father of each of the children. We then match this data to the annual, individual level administrative register LOUISE, containing information on age, educational attainment, and labor income (based on tax registers). The LOUISE register also includes annual information on marital status, indicating whether individuals are single, married, cohabiting, divorced, or separated; with unique family identifiers for each married couple, and for each cohabiting couple with joint children. These data span over the time period 1992 through 2007.

We then match the multi-generational and LOUISE register to a data set maintained by the Social Insurance Agency, with individual level information on the number of parental leave days taken for each child in our sample. Since parents are allowed to use their entitled parental leave benefits until a child turns eight years old, we calculate the total number of days taken (for each child) during the child's first eight years of life. For each child, we also calculate the share of parental leave taken by mothers, defined as the ratio between the mother's number of leave days and the total number of leave days taken for each child by the mother and father jointly.

Table 1 presents summary statistics for couples with children born 12 months before and after each reform cutoff date, respectively. Mothers' share of parental leave decreased between 1995 and 2002; from 88 percent to 82 percent, on average. The second reform sample is more likely to be married at birth compared to the first reform sample, while cohabitation is less common in the second reform sample. Over time, the share of couples in which both spouses have some college or a college education increased, reflecting the increased supply of highly educated individuals in general over the time period studied. Furthermore, women are on average two years younger than their spouses, more likely to be highly educated, but earn a lower labor income.[8]

[8] *Figure A.1* in the Appendix presents a graph of the number of births in Sweden during the relevant time period.

TABLE 1.
Summary statistics

	First reform	Second reform
Family characteristics		
Mother's share of parental leave	0.884 (0.155)	0.823 (0.162)
First born boy	0.513 (0.500)	0.515 (0.500)
Married at birth	0.365 (0.481)	0.434 (0.496)
Cohabiting at birth	0.533 (0.499)	0.469 (0.499)
Parents not together at birth	0.109 (0.312)	0.104 (0.306)
Both spouses highly educated	0.188 (0.391)	0.260 (0.439)
Father high, mother low educated	0.117 (0.321)	0.107 (0.309)
Father low, mother high educated	0.147 (0.354)	0.168 (0.374)
Both spouses low educated	0.548 (0.498)	0.465 (0.499)
Spousal characteristics		
Age mother	27.213 (4.775)	29.487 (4.811)
Age father	30.122 (5.585)	32.450 (5.621)
Mother foreign born	0.173 (0.379)	0.192 (0.394)
Father foreign born	0.158 (0.364)	0.180 (0.384)
Mother compulsory education	0.124 (0.330)	0.101 (0.302)
Father compulsory education	0.141 (0.348)	0.106 (0.308)
Mother highschool education	0.565 (0.496)	0.484 (0.500)
Father highschool education	0.560 (0.496)	0.533 (0.499)
Mother college education	0.311 (0.463)	0.414 (0.493)
Father college education	0.299 (0.458)	0.360 (0.480)
Mother pre-birth income, SEK	(64,151) (68,855)	92,554 (102,308)
Father pre-birth income, SEK	(193,455) (130,416)	285,015 (240,289)
Observations	89,856	165,344

NOTE.— Means and (standard deviations) of characteristics for the parents to children born 12 months before and after the first and second reform, respectively.

We use a fuzzy Regression Discontinuity (RD) design[9] to estimate the effects of the intra-household division of parental leave on family structure. The discontinuities that we use in the RD design arise from the introduction of earmarked parental leave days; parents of children born on or after January 1st 1995 were given one non-transferable month of paid leave each, and parents of children born on or after January 1st 2002 were given an additional non-transferable month of paid leave. Using data on parents to children born in 12-month windows around the respective reform cutoffs, our RD design is implemented by estimating the following regression equations:

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

$$S_i = \gamma + \mathbf{1}[t \geq c] g_r(t-c) + \mathbf{1}[t \geq c]\delta + \mathbf{1}[t < c] g_l(c-t) + \nu_i \quad (2)$$

[9] See Lee and Lemieux (2010) for a thorough exposition of the RDD econometric framework.

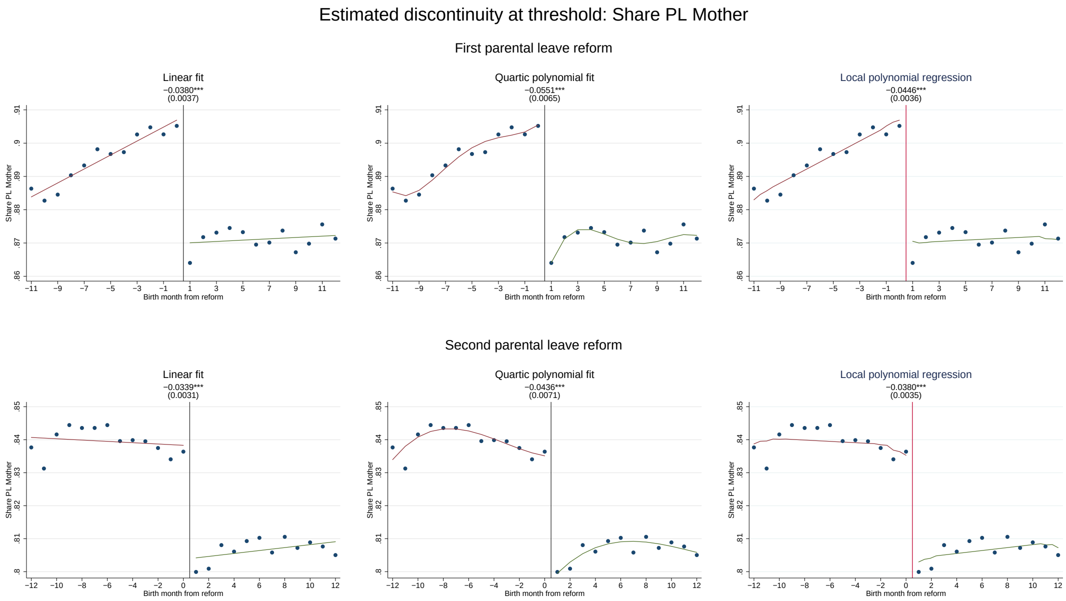
where y_i^τ is the outcome of interest for child i over some specified follow-up period τ (e.g., the probability of marriage dissolution of the parents), and S_i is the mother's share of total parental leave. Furthermore, t is the birth month defined in months from the cutoff date, c, $\mathbf{1}[\cdot]$ is the indicator function, and f_l, f_r, g_l, and g_r are unknown functions. We estimate Equations (1) and (2) applying both (local) linear and polynomial specifications of the functions. The Two Stage Least Squares (TSLS) estimate of β yields the effect of the mother's share of parental leave take-up on family structure, given the identifying assumption that parents cannot precisely control their children's date of birth, i.e., the assignment variable t, near the cutoff c. In addition, for TSLS to consistently estimate the effect of parental leave share on marital outcomes, we must also impose assumptions about monotonicity and functional form. We assess the validity of the identifying assumptions through a number of robustness checks in section 5 below.

4 Results

4.1 Graphical results

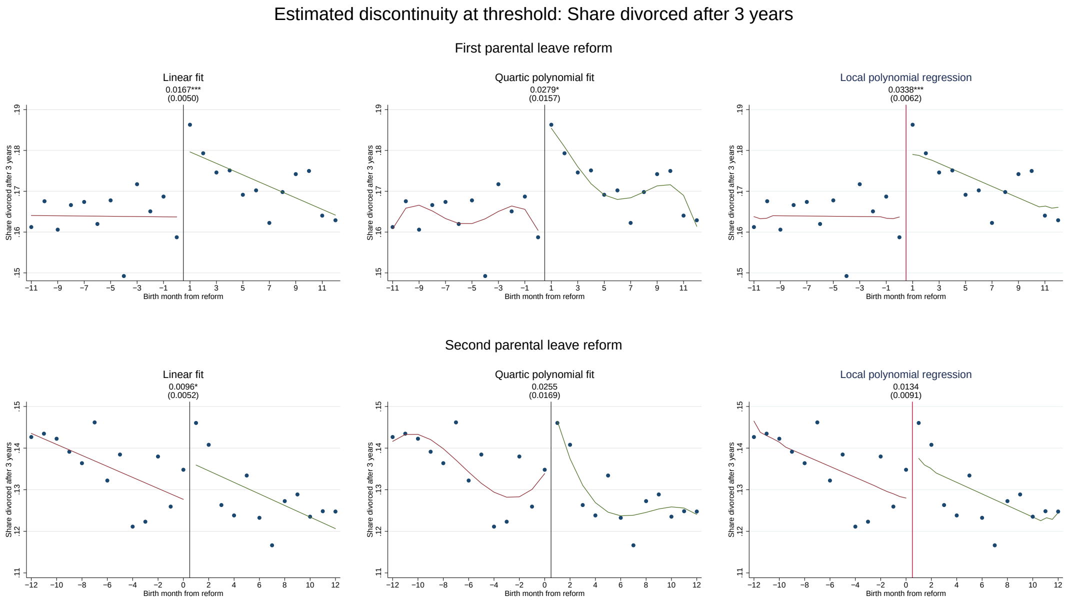
Figure 3 shows the mother's share of parental leave on each side of the cutoff dates for the 1995 (upper panel) and 2002 (lower panel) reform, respectively. Each of the sub-panels estimate the discontinuity at the threshold under different parametric assumptions; from left to right, under a linear, quartic polynomial, and locally smoothed restriction, respectively. For all specifications, and for both reforms, the estimated discontinuities in the mother's share of parental leave are highly significant at the cutoff, decreasing with around three to five percentage points. Hence, the graphical evidence strongly suggests that the introduction of the gender quotas in the Swedish parental leave system decreased the intra-household specialization in terms of parental leave division. *Figure 4* plots the corresponding reduced form, or intention-to-treat, effect of the parental leave reforms by replacing the parental leave share with the share of separated couples three years after child birth on the y-axis. Interestingly, there is a visibly strong increase in separation rates just after the 1995-reform was implemented. The magnitude of the estimates (1–3 percentage points depending on specification) is economically relevant, implying an increase in separation risk of about 10–20 percent. While a similar empirical pattern is visible around the time of the 2002-reform, it is somewhat less clear compared to the first reform.

FIGURE 3.
Regression Discontinuity estimates of parental leave reforms: First stage results



NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE 4.
Regression Discontinuity estimates of parental leave reforms: Intention-to-treat results



NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

4.2 Regression results

The specifications use monthly data on child birth, using data on one-year windows on either side of the cutoff. Our primary outcome variable, separation, captures couple dissolution irrespective of whether the couple was married or cohabiting when the child was born. Separations are measured at three years after birth, and defined as being equal to unity if the couple is no longer living together three years after the birth of their joint child. We choose to measure separation at three years after births since the majority of couples have used up most of their entitled parental leave days by that time (see *Table A.1* in the Appendix). However, we also estimate the specifications for different follow-up lengths. As both the affected sample and the contents of each reform are quite different, we estimate the effect separately for each reform.

Panel A of *Table 2* presents the results for the first reform, in 1995, and Panel B for the second reform, in 2002. In the first column, reporting the results from a simple bivariate OLS estimate, we see that the mother's share of parental leave is positively related to marital dissolution. The coefficient on mother's share of parental leave is positive for both reform samples, albeit smaller in magnitude for the 2002-reform sample. Thus, not taking potential endogeneity into account, both reform samples suggest that couples that are more specialized are more likely to end in separation.

Next, column (2) of the table reports the estimates for the intention-to-treat (ITT) effect of the reforms on couple dissolution. The reduced form estimates suggest that the 1995-reform increased the likelihood of parents having split up three years after birth by 1.4 percentage points. The corresponding number for the 2002-reform is around one percentage point. As expected, these results are in line with the graphical evidence from *Figure 4*.

The first stage and IV estimates are shown in columns (3) and (4), respectively. The reported coefficients from column (3) indicate that the reforms decreased the mother's share of total parental leave by, on average, 3.7 and 3.3 percentage points in the first and second reform, respectively. Furthermore, the first-stage F-statistic for instrument relevance is strongly significant in both specifications. Column (4) shows the TSLS estimates of the effect of mother's share of parental leave. The negative and significantly estimated coefficient indicates that decreased specialization within the household increased the likelihood of separation among couples who changed their division of parental leave days due to the reforms.

Applying the estimated coefficient from the IV specification in column (4) to predict the impact of decreasing mother's parental leave share with one standard deviation (about 15 percentage points from *Table 1*) implies a change in the dissolution probability of about five percentage points. The third row from the bottom in each panel reports the percentage effect of a one standard deviation change in parental leave, corresponding to an increase in the dissolution rate of about 39 (42) percent in the first (second) reform. While this effect may seem implausibly high at first glance, note that, since the reforms only changed the mother's share of parental leave days with around one-fourth (one-fifth) of a standard deviation in the first (second) reform, the reform effects would translate into a more moderate ten percent increase in divorce rates (corresponding to the ITT effect)[10]. Finally, note that the OLS estimate in column (1) indicates a positive correlation between couples' separation risk and the mother's share of parental leave, while the IV estimates suggest that a decrease in the mother's share of parental leave increases the separation rate. This suggests that the OLS estimates may be biased due to omitted variables or selective sorting.

Table A.2 in the Appendix shows the robustness of our main findings to the inclusion of selected covariates (see section 5.1). If our instrument is as good as randomly assigned, the inclusion of covariates should not affect our estimates. Indeed, *Table A.2* shows that our results are essentially unchanged after including covariates, but more precise: the ITT-effects are now significantly different from zero at the five percent level in both reforms.

Table 2 shows the results when the outcome, separation, is measured at year three after the birth of the child. To examine potential heterogeneous effects by time since birth, we perform the analysis for different follow-up lengths. *Figure 5* shows the IV estimates of the effect of mother's parental leave share on the dissolution rate for different years since birth, for the first and second reform samples, respectively. In the first reform the estimated coefficients exhibit a U-shaped pattern over time since birth, but remain negative and statistically significant up until five years after the reform. For the 2002-reform, however, the estimated relative change in the dissolution risk remains constant from year two after birth and onward. Thus, the estimated increase in separation risk resulting from an unexpected decrease in the mother's share of parental leave is robust with respect to time since birth.

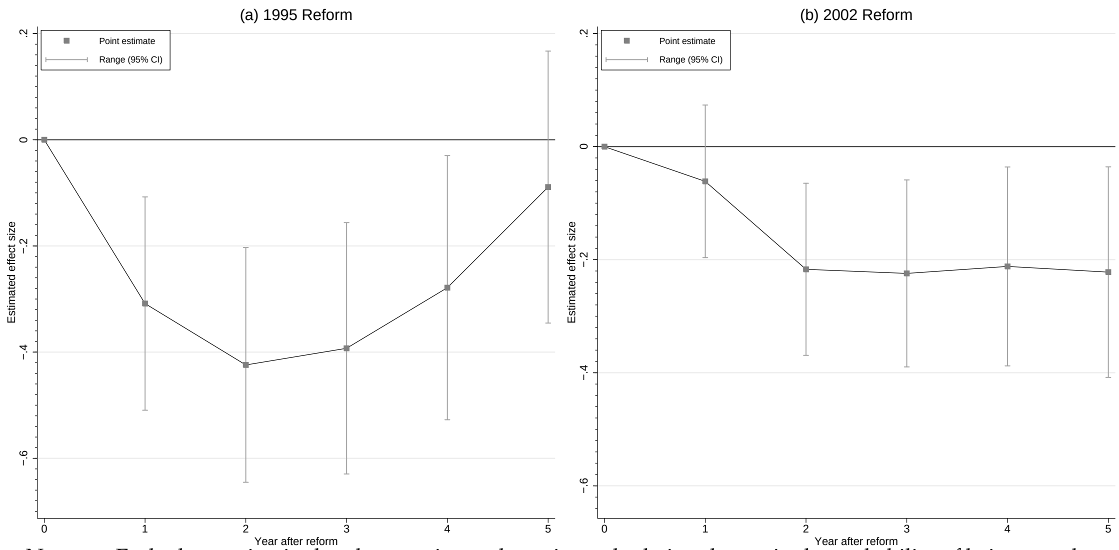
[10] Also see Section 6 where we study the compliers of the reform in detail.

TABLE 2.
Regression discontinuity estimates for mothers' share of parental leave: Couple dissolution

	(1) OLS	(2) ITT	(3) First stage	(4) IV
A. First parental leave reform (1995)				
Mother's share of PL	0.238***			-0.367*
	(0.011)			(0.203)
Born in 1995		0.014*	-0.037***	
		(0.007)	(0.003)	
Mean of outcome	0.144	0.144	0.887	0.144
% Effect Δ_{SD}	0.254	0.094	-0.041	-0.391
First stage F-stat			136.0	136.0
Observations	39,444	39,444	39,444	39,444
B. Second parental leave reform (2002)				
Mother's share of PL	0.174***			-0.270*
	(0.007)			(0.146)
Born in 2002		0.009*	-0.033***	
		(0.005)	(0.002)	
Mean of outcome	0.105	0.105	0.822	0.105
% Effect Δ_{SD}	0.269	0.085	-0.040	-0.416
First stage F-stat			178.1	178.1
Observations	72,911	72,911	72,911	72,911

NOTE.— The outcome variable is defined to equal unity if the couple is no longer together three years after the birth of their joint child. Before separation, the couple could be either cohabiting or married. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

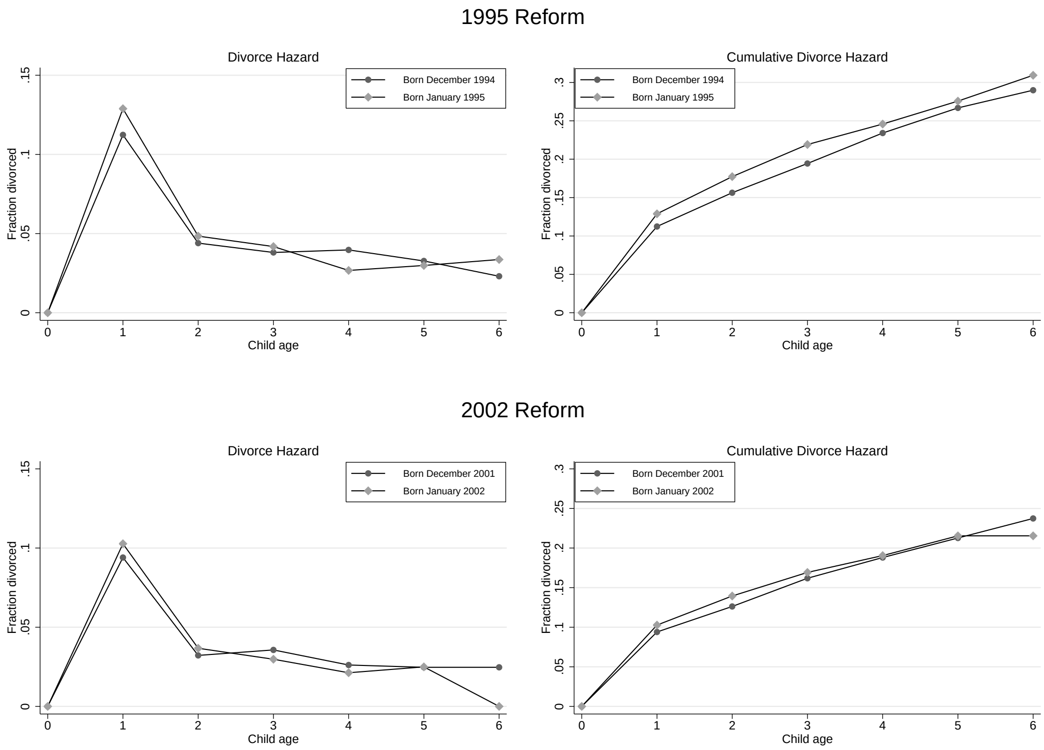
FIGURE 5.
IV effects by year from childbirth



NOTE.— Each observation in the plot pertains to the estimated relative change in the probability of being together t years after childbirth due to a change in the mother's parental leave, as inferred from estimation of the baseline IV model.

The main outcome variable is defined cumulatively. To get a more detailed picture of the dynamics of separation by time since birth, we next plot the separation hazard and the cumulative separation hazard with respect to child age, for parents of children born in December 1994 (2001) and January 1995 (2002), separately. The results are presented in *Figure 6* and show that, for both reform samples, the separation hazard is larger for the parents of January-born children in the first few years after birth. Thus, we find suggestive evidence that the increased separations are re-timed separations, rather than separations that would not have occurred in the absence of the reforms. In particular, the cumulative separation hazards in the 2002-reform converge for the two groups when the child is four years old. For the 1995-reform, however, the cumulative hazard is always higher for those with January-born children over the follow-up horizon, but the difference diminishes as the child becomes older. One potential interpretation of these findings is that the reforms implied an information shock to the spouses about their match quality, and thereby induced an earlier separation than would have been the case in the absence of the reforms.

FIGURE 6.
Separation hazard and cumulative hazard by treatment status



NOTE.— The figure shows the separation hazard and the cumulative separation hazard with respect to child age for the 1995- and 2002-reform, respectively.

5 Threats to identification

5.1 Treatment manipulation

The validity of our RD design requires that parents are unable to manipulate the assignment variable, that is, that individuals do not have precise control of the birth timing of their children. One concern is that couples with due dates close to the cutoff could have postponed induced births and planned cesarean sections or, alternatively, induced earlier births to avoid the new rules. However, cesarian sections are rare in Sweden and, as reported in Ekberg et al. (2013), planned birth surgery for other than health-related reasons are considered highly unethical by doctors. Nevertheless, in *Table 3* we show that, consistent with the evidence in Ekberg et al. (2013), the share of children born in January and December are similar across all years during our observation period. Thus, there are no indications that parents were able to manipulate the birth date of their children, as the aggregate distribution of children's birth date does not jump at the cutoff dates around the respective reforms.

TABLE 3.
The share of January- and December-born children among all children born during 1994-2004.

	(1) Total children born	(2) Children born in January	(3) Share January-born	(4) Children born in December	(5) Share December-born
1992	133,069	11,923	0.090	9,204	0.069
1993	125,705	10,957	0.087	8,771	0.070
1994	120,504	10,074	0.084	8,780	0.073
1995	111,443	9,554	0.086	7,576	0.070
1996	103,026	8,732	0.085	7,420	0.072
1997	98,123	8,502	0.087	6,914	0.071
1998	96,666	8,017	0.083	6,844	0.071
1999	95,377	7,852	0.082	6,985	0.073
2000	97,372	7,981	0.082	6,991	0.072
2001	97,418	8,263	0.085	6,835	0.072
2002	101,270	8,298	0.082	7,356	0.073
2003	103,894	8,468	0.082	7,643	0.074
2004	105,377	8,785	0.082	7,673	0.073

NOTE.— Frequency of births and share of births in January and December 1994-2004. The shaded areas indicate the reform years.

Furthermore, if couples are able to time the date of birth of their children to be able to benefit from, or to avoid, the new parental leave rules, we should expect to see a discontinuity also in predetermined characteristics around the reform cutoff dates. *Figure A.2–Figure A.11* in the Appendix show whether predetermined characteristics – such as the spouses' year of birth, immigrant status, and pre-birth education level – differ between individuals with children born on either side of the reform cutoff dates. There are no obvious visible trend shifts around the thresholds for the spouses' year of birth, father's immigrant status, father's educational level, or the likelihood that the couple was cohabiting at birth (compared to being married). For the mother's educational level, however, there is a small positive discontinuity at the threshold of the 1995-reform (significant only with a linear specification), and a negative discontinuity at the threshold of the 2002-reform. Furthermore, there appears to be a slightly smaller share of boys above the threshold in the 1995-reform, a lower share of mothers with non-native background and a higher share of first-time mothers and fathers in the 2002-reform.

However, in *Table A.2* in the Appendix, we augment our main specification with the inclusion of maternal education level dummies, a dummy for the child being a boy and whether it was the first child born to the mother or father, and an indicator for at least one of the spouses having a non-native background, and find that our main results are robust to this exercise. Nevertheless, to further alleviate concerns about potential unobserved factors driving our main findings, we employ a series of robustness checks in the next section, e.g., by running placebo tests using pre-reform data.

Moreover, to expand our set of variables for which the balancing tests are undertaken, we use data from the Medical Birth Register containing date of birth, matched to the Swedish Livings Conditions Survey (ULF/SILC) which contains a set of variables measuring the socioeconomic status of individuals, occupations, and parental occupation.[11] Thus, for a nationally representative sub-sample of mothers giving birth around our reform windows, we extract information on pre-birth log earnings, whether they live in an urban area, are blue-collar workers, and whether their own mother is a blue-collar worker. The results are presented in *Figure A.12–Figure A.15* in the Appendix; all these variables are smooth around the thresholds for both reforms.

5.2 Mechanical interactions between calendar year and birth month

Due to the nature of the data where the outcome variables are measured on an annual basis, one potential problem with our empirical specification is that the follow-up horizon is longer for families with children born in January compared to those with children born in December. In particular, this implies that parents to children born in January will have had longer time to separate compared to parents to children born in December. This mechanical interaction between birth month and the outcome variable implies that we might overestimate the effect of the reforms on separations, if separations increase as a result of the reform. To assess the magnitude of such mechanical interaction effects between calendar

[11] For data security reasons, we are unable to link these data to our main data set.

time and birth month we exploit pre-reform data and employ a placebo test by estimating the main specification on the turn of the year preceding the reform. If there is a mechanical effect of calendar time on the separation rate, this should be picked up by the estimate using the pre-reform year and be a valid test for our main estimates under the assumption that separation risk by time since birth is constant across (birth) years.

The results are shown in *Table 4*, depicting results from the placebo test based on data preceding the 2002-reform. Specifically, we roll back the reform one year and denote parents to children born in January 2001 as treated and parents to children born before January 2001 as non-treated, keeping the twelve-month before-and-after analysis window as used in the main design. The OLS results suggest that the mother's share of parental leave is positively correlated with separation risk, as in our main estimation sample, but there is no change in the separation probabilities nor a first stage effect on the mother's parental leave share at the (placebo) cutoff date.

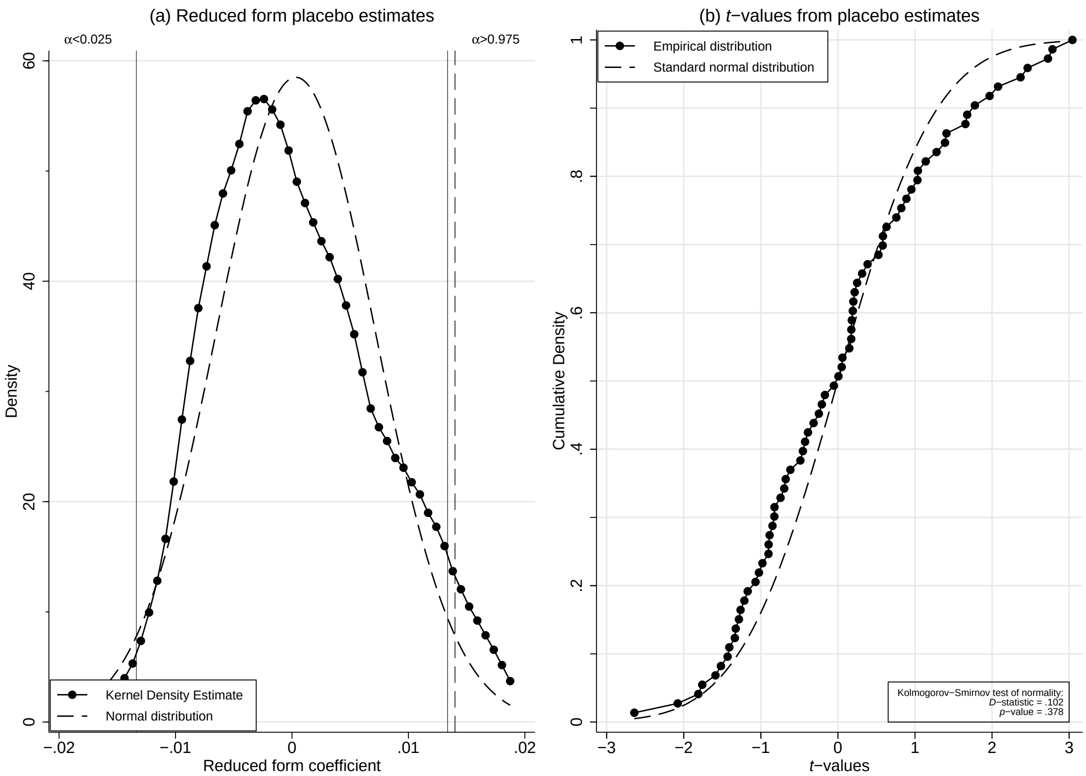
TABLE 4.
Regression discontinuity estimates of mothers' share of parental leave: Placebo test on pre-reform years

	(1) OLS	(2) ITT	(3) First stage	(4) IV
Mother's share of PL	0.158***			-14.161
	(0.007)			(290.186)
Born in reform year $-$ 1		0.002	-0.000	
		(0.005)	(0.003)	
Mean of outcome	0.113	0.113	0.840	0.523
% Effect Δ_{SD}	0.226	0.016	-0.000	-4.373
First stage F-stat			0.002	0.002
Observations	68,985	68,985	68,985	68,985

NOTE.— The table reports results from placebo tests based on data in the year preceding the 2002 parental leave reform. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

Because the parental leave data is truncated before 1994, we cannot implement a similar placebo analysis for the 1995-reform. However, using all post-1995 (but pre-2002) reform data we can perform a series of placebo analyses of the reduced form effect, shifting the policy intervention cutoff by one month at a time. Thus, we repeat the placebo intervention 72 times, with the intervention cutoff starting in every post-1995-reform month from June 1995 to June 2001. As in our main specification, we estimate the reduced form effect of birth month on couple dissolution in the third year after the child is born. In *Figure 7* we illustrate the distribution of point estimates from this placebo procedure, and the cumulative distribution of t-values from the series of regressions. The point estimates from the placebo interventions are almost always lower than our ITT-effect and are centered around zero. The right-hand side of the graph depicts the cumulative density of t-values from the 72 placebo interventions together with a plot of the standard normal distribution. A Kolmogorov-Smirnov test of normality of the empirical distribution of the placebo t-values cannot be rejected for any conventional significance level.

FIGURE 7.
Kernel density estimates of placebo reduced form estimates for couple dissolution



NOTE.— The left-hand side graph depicts the distribution of reduced form point estimates from 72 placebo interventions. The right-hand graph depicts the cumulative density of t-values from the placebo interventions together with a plot of the standard normal distribution. A Kolmogorov-Smirnov test of normality of the empirical distribution of the placebo t-values cannot be rejected for any conventional significance level (p-value: .378).

As an additional test, we estimate difference-in-differences models, focusing on parents of children born in 6 month windows on either side of the reform cutoff date of January 1st 1995 and 1996, respectively. Thus, we compare differences in outcomes of parents to children born in July–December 1994 (ineligible) to parents of children born in January–June 1995 (eligible) and account for seasonal effects with the difference in outcomes between parents of children born in the same calendar months in the subsequent year. We also test the sensitivity of the estimates restricting the sample to a 3-month and 1 month window on either side of the cutoff, respectively. We estimate the following regression equation separately for the different ranges of the data windows:

$$y_i = \gamma_0 + \gamma_1 Treated_i + \gamma_2 After_i + \gamma_3 (Treated_i \times After_i) + \mu_i \qquad (3)$$

where $Treated_i$ is a dummy variable that equals unity if the child is born in January–June, and zero otherwise; $After_i$ is a dummy variable that takes the value one if the child is born in July–December 1994 *or* January–June 1995, and zero if the child was born in July–December 1995 *or* January–June 1996. The coefficient of interest is thus γ_3, which captures the difference in outcomes between parents of children born in January–June 1995 with those born in July–December 1994, in comparison to the corresponding difference between parents of children born in July–December 1995 and January–June 1996.[12] We repeat this analysis for the 2002-reform, but using the turn of the year preceding the 2002 cutoff to capture seasonal effects.

The results show that for the 1995-reform, the specification with the widest range (12 months), reported in Column (1) of *Table 5*, yields a statistically significant increase in the separation rate, while we lose precision in the specifications using the shorter ranges (and thus less data). However, as we use data closer to the reform cutoff date, the ITT-effect estimated with the difference-in-difference specification becomes closer and closer to our estimated ITT-effect in the regres-

[12] The shorter time windows thus include parents of children born in January–March (January) compared to parents of children born in October–December (December) for the 3-month (1-month) window.

sion discontinuity design. In fact, the shortest time window produces a point estimate that is precisely the same as the ITT-estimate in the regression discontinuity design. Thus, the magnitude and direction of the estimated effect suggest an increase in the likelihood of couple separation among parents of children eligible for the quota reforms in line with our main regression discontinuity setup.

For the 2002-reform, we also see a point estimate approaching the reduced form in the regression discontinuity design as we reduce the window around the reform, but none of the estimated effects are significantly different from zero.[13] It is important to keep in mind that the two reforms are different in their design, and thus might have different consequences for marital stability.

TABLE 5.
Difference-in-differences estimates of the effects of the 1995 and 2002 paternity leave reforms on couple separation

	(1) 6 month window	(2) 3 month window	(3) 1 month window
A. First parental leave reform (1995)			
After × Treated	0.0199***	0.0128*	0.0140
	(0.00623)	(0.00689)	(0.0128)
Treated	-0.00741*	0.00168	0.0135
	(0.00420)	(0.00546)	(0.00882)
After	-0.00715	-0.00256	-0.00219
	(0.00474)	(0.00481)	(0.00872)
Constant	0.170***	0.167***	0.162***
	(0.00274)	(0.00426)	(0.00626)
Observations	83,660	43,778	13,988
Number of clusters	22	12	
Second parental leave reform (2002)			
After × Treated	0.00266	0.00616	0.00683
	(0.00627)	(0.00741)	(0.00928)
Treated	0.00127	-0.00195	0.00302
	(0.00364)	(0.00327)	(0.00665)
After	-0.0108**	-0.0103**	-0.00538
	(0.00468)	(0.00449)	(0.00671)
Constant	0.139***	0.144***	0.141***
	(0.00304)	(0.00265)	(0.00482)
Observations	139,352	71,916	22,643
Number of clusters	22	12	

NOTE.— The table presents OLS estimates from difference-in-differences analyses of the effect of the paternity leave reforms on the likelihood of separation three years after the child is born. Column (1) uses data on 6 month windows before and after each reform (August–December, and January–June, respectively), Column (2) uses data on 3 month windows before and after (October–December, and January-March, respectively), and Column (3) uses data on 1 month before and after the reform cutoff dates (December–January). Standard errors (in parentheses) are clustered at the birth month × birth year in Columns (1) and (2), and Column (3) reports robust standard errors. *** p <0.01, ** p<0.05, * p<0.1.

[13] Our difference-in-differences point estimates are also in line with those of Johansson (2010), who examined the impact of own and spousal parental leave on earnings, with some estimates on the effects of the two Swedish parental leave reforms on couple stability.

Finally, a potential issue with our research design is that we estimate the effect of mother's share of parental leave taken during the first eight years after birth on couple separations in the third year after birth[14]. Thus, the outcome may be endogenous with respect to treatment, if fathers increase their parental leave after divorcing. First, it is important to note that the majority of all parental leave days are used during the first three years of the child's life. *Figure A.16* shows the raw parental leave take-up by mothers and fathers, respectively, during the first three years after birth and shows that the level of PL take-up is close to the full-take up after eight years. Moreover, under the assumption that the potential increase in fathers' take-up due to divorce is the same for all couples, irrespective of reform-treatment status, such endogeneity should have been picked up in our placebo estimations of the reduced form effects presented in *Figure 7*, from which the point estimates are centered around zero. Lastly, the direction of the potential endogeneity is not clear, as fathers might also *decrease* their parental leave after the couple has dissolved. Nevertheless, we perform an additional robustness check, where we only use the parental leave days taken during the first three years after birth to estimate the effects on couple dissolution. The results are presented in *Table A.3* for couple dissolution in the third year after birth, and in *Figure A.17* for longer (and shorter) follow-up horizons for the outcome variable. The results are very similar to those from our preferred specification.

6 Compliers of the reform

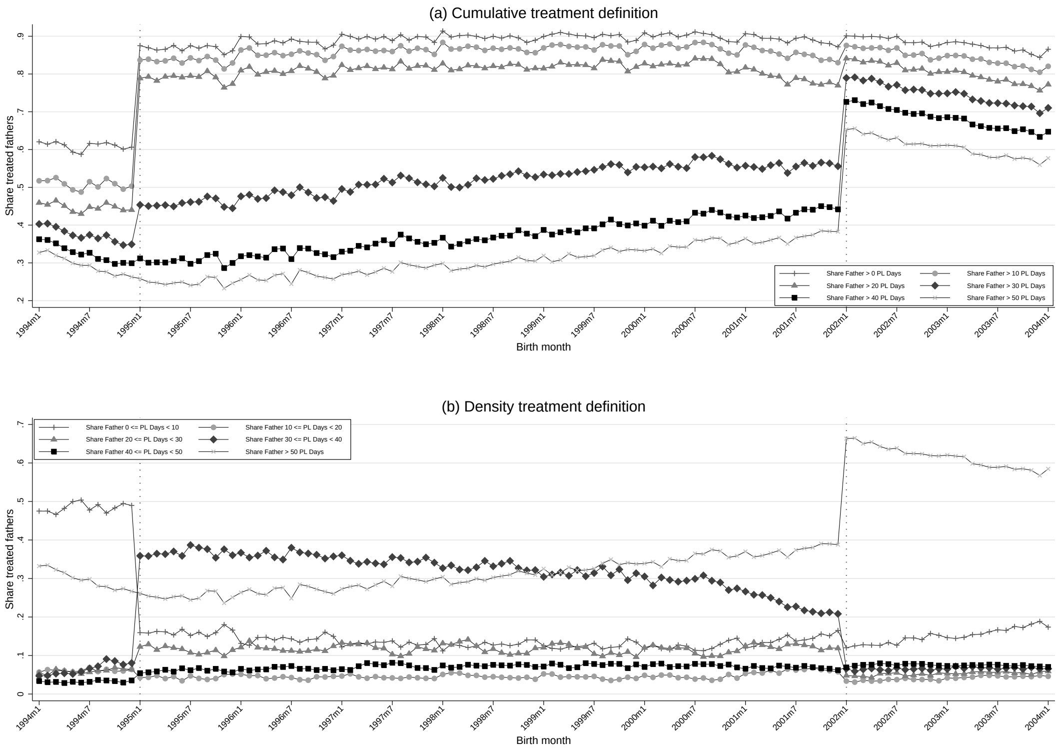
In the context of the causal model outlined in this paper, the IV estimate should be interpreted as an average effect of a one percentage point decrease in the mother's parental leave share for couples whose parental leave division was influenced by the reforms in the parental leave system. This group may not necessarily be a good representation of the entire population of couples with joint children, in which case a local average treatment effect (LATE) interpretation may be more

[14] We use this definition of parental leave share in order to have consistency across the different specifications with respect to follow-up time.

accurate (Imbens and Angrist, 1994). While we cannot identify individual reform compliers in our sample, we can study the type of households contributing most to our estimated LATE by analyzing the distribution of complier characteristics. In particular, full compliance would mean that those affected by the first reform were couples where the father would have taken very few parental leave days in the absence of the reform increased their uptake to 30 days after implementation. For the second reform, the same would imply that couples in which the fathers would have taken 30 days of parental leave now increased their uptake to 60 days.

To evaluate these conjectures we perform two different analyses. First, we study the share of fathers with different parental leave uptakes as a function of child birth month. Specifically, we define groups by the number of leave days they take: more than zero days; more than 10 days, and so on, up to an uptake in excess of 50 days. We plot these shares cumulatively as a function of child birth month in Panel A of *Figure 8*. The figure suggests that the first reform mainly decreased the share of fathers taking very few days of parental leave, while increasing the share of fathers taking more than 30 days. However, there are no jumps in the share of fathers taking more than 40 days. The second reform increased the share of fathers taking more than 30 days, while there are no changes in the share of fathers taking no or little leave. In other words, both reforms seem to have had effects in accordance with the magnitude of the quotas. This can clearly be seen from Panel B of *Figure 8* which shows that, for the first reform, the increase in the share of fathers taking 30–40 days is almost as large as the decrease in the share of fathers taking 0–10 days.

FIGURE 8.
Reform compliers for various treatment definitions

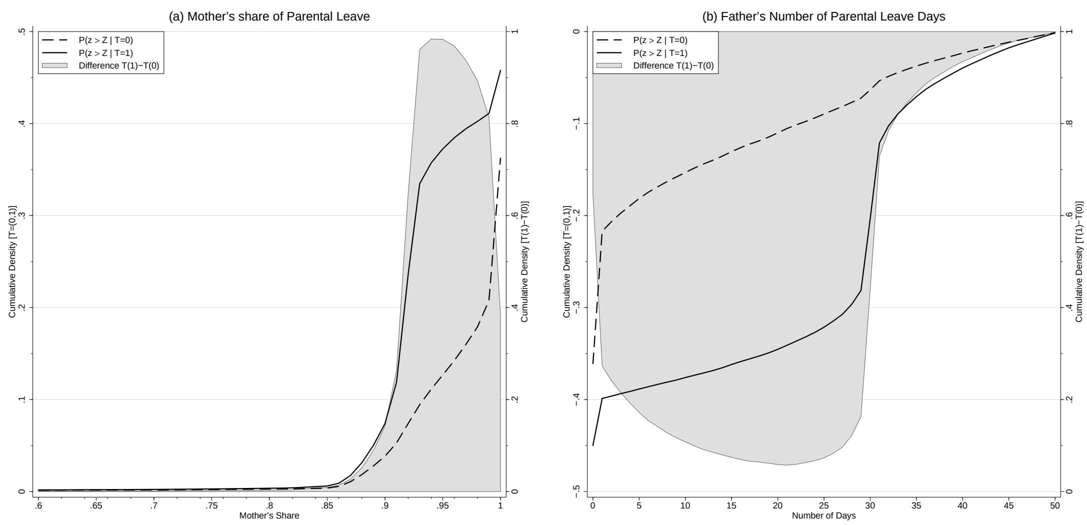


NOTE.— The lines pertain to different shares of paternity leave days extracted by the father over the sampling period. Panel (a) define shares cumulatively while panel (b) define shares within intervals. The dotted vertical lines indicate time when quota reforms were implemented.

To corroborate the evidence presented in *Figure 8*, we plot the empirical cumulative distribution function (CDF) of mother's share of parental leave take-up for treated and non-treated couples separately to gain knowledge about the type of couples contributing most to our estimated LATE. The weighting function underlying our main IV estimates is proportional to the difference between the CDF of mother's parental leave share between couples with the instrument switched on and off (see Angrist and Imbens, 1995), i.e., between couples whose child was born on or after January 1st 1995, or on or after January 1st 2002. For each level of mother's share, this proportion amounts to the share of the population whose parental leave is switched by the instrument from a share less than j to at least j. The results are shown for the 1995-reform in *Figure 9*, and in *Figure 10* for the 2002-reform. Starting with left-hand graphs of both *Figure 9* and *Figure 10*, the

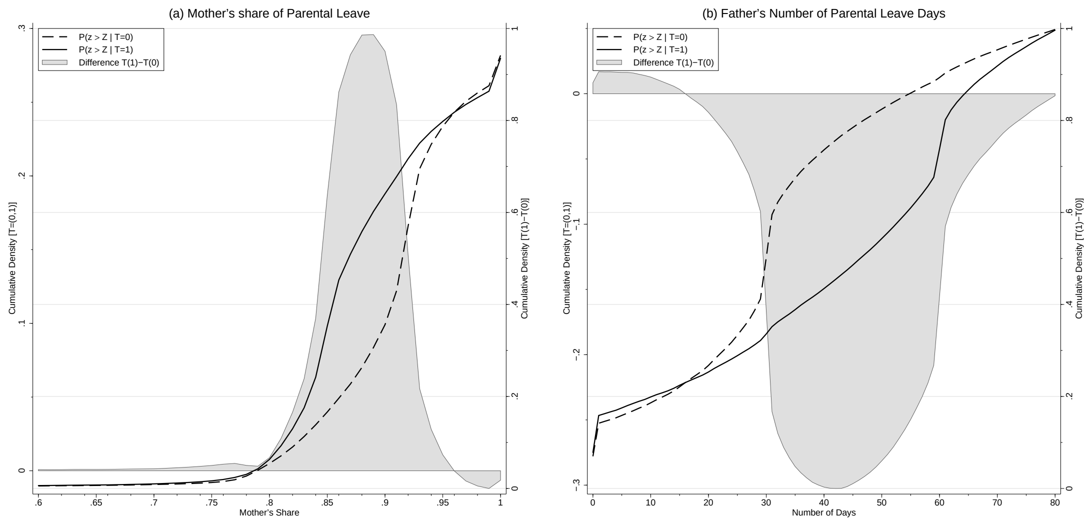
CDF of mother's share for the treated group is "shifted" inwards, which is consistent with our previous first-stage evidence. Secondly, the difference between the CDFs is greatest among couples with very unequally divided parental leave. The right-hand graph of *Figure 9* and *Figure 10* plots the corresponding difference between the CDFs of the father's number of days of parental leave. The differences in the cumulative densities decrease sharply at exactly 30 days in the 1995-reform, and at 60 days in the 2002-reform (as well as a sharp increase in the difference in the densities at 30 days in the second reform), corresponding to the implemented quotas. Thus, consistent with our prior, the reforms mainly affected couples in which the parental leave division was highly unequal between the spouses, and our expectation that the reforms induced fathers to increase their number of parental leave days from very few to 30 days (in the first reform) and from the already mandated 30 days (in the second reform) to around 60 days.

FIGURE 9.
CDF of Mother's Share of Parental Leave, and Father's Number of Parental Leave Days by Treatment Status: 1995-reform



NOTE.— The lines pertain to the CDFs of mother's share of parental leave (panel a) and father's number of parental leave days (panel b) by treatment status, here defined as child birth 12 months after reform implementation compared to 12 months before implementation. The shaded area illustrates the quantile-specific difference between the two CDFs.

FIGURE 10.
CDF of Mother's Share of Parental Leave, and Father's Number of Parental Leave Days by Treatment Status: 2002-reform



NOTE.— The lines pertain to the CDFs of mother's share of parental leave (panel a) and father's number of parental leave days (panel b) by treatment status, here defined as child birth 12 months after reform implementation compared to 12 months before implementation. The shaded area illustrates the quantile-specific difference between the two CDFs.

Thus, our compliant subpopulation consisted of couples with very unequally divided parental leave. But how do these couples differ in terms of personal characteristics from other couples? We use the evidence provided in *Figure 8–Figure 10* to define new binary treatment variables; since other types of couples hardly contribute to our estimated LATE, we define the new treatment variable for the 1995-reform to equal one if the father in the household takes at least 30 days of parental leave, and zero if he takes less than 30 days of leave. For the 2002-reform, we define the new binary treatment as taking at least 60 days. We then use these new treatment definitions to study how the compliers in the respective reforms differ from the overall sample. To this end, we follow Angrist and Pischke (2008) and learn about the distribution of complier characteristics by studying the variation in the first stage across covariate groups, for a number of characteristics that can be described by binary variables. Specifically, we estimate the distribution of complier characteristics with the ratio of the first stage for individuals with a certain characteristic $x_i = 1$, e.g., college graduates to the overall first stage:

$$\frac{P[x_{1i}=1|D_{1i}>D_{0i}]}{P[x_{1i}=1]} = \frac{P[D_{1i}>D_{0i}|x_{1i}=1]}{P[D_{1i}>D_{0i}]} = \frac{E[D_i|Z_i=1,x_{1i}=1]-E[D_i|Z_i=0,x_{1i}=1]}{E[D_i|Z_i=1]-E[D_i|Z_i=0]}$$

where D is the treatment status (paternity leave) and Z is the instrument (time of birth). The results for this exercise are given in *Table 6* which reports compliers' characteristics ratios for child gender; indicator variables for whether the child was the mother's and the father's first child, respectively; indicators for having at least some post-secondary schooling for each spouse; indicators for being born outside Sweden; indicators for the mother of each spouse to have some post-secondary schooling; and indicators for the mother of each spouse to have an average annual income over the (observed) lifetime that exceeds the median income of the grandmothers of the sample. Starting with the 1995-reform, children of compliers are just as likely to be a boy but slightly more likely to be the first child compared to the overall sample. Interestingly, the wives in the complier group are less likely to have some post-secondary schooling, while the husband is not significantly more likely to have some college education compared to the overall sample. Moreover, the complier father is less likely to have had a college educated mother, and less likely to have had a mother who earned an income exceeding the median income of the grandmothers in the sample. The female spouses, on the other hand, are more likely to have college educated mothers, although not significantly so, and more likely to have grown up with a high-income mother. The compliers in the 2002-reform are similar in most respects except for that the complier fathers are now more likely to have mothers who were highly educated and had high earnings compared to the full sample.

TABLE 6.
Complier characteristics for the birth-month instrument by parental leave reform

Control variable	All	Compliers	Ratio	Z-score
A. First parental leave reform (1995)				
Child Gender	0.510	0.512	1.004	0.208
Firstborn Father	0.805	0.894	1.110	6.034
Firstborn Mother	0.926	0.996	1.076	4.361
Female high ed	0.410	0.341	0.831	-6.106
Male high ed	0.478	0.494	1.033	1.546
Non-native mother	0.176	0.173	0.979	-0.254
Non-native father	0.170	0.119	0.699	-2.451
Mother's mother high ed	0.224	0.231	1.029	0.946
Father's mother high ed	0.217	0.178	0.820	-4.474
Mother's mother high inc	0.420	0.444	1.056	2.221
Father's mother high inc	0.418	0.392	0.939	-1.925
B. Second parental leave reform (2002)				
Child Gender	0.512	0.536	1.046	1.526
Firstborn Father	0.442	0.480	1.084	2.657
Firstborn Mother	0.486	0.522	1.075	2.468
Female high ed	0.375	0.348	0.928	-2.146
Male high ed	0.328	0.348	1.061	1.639
Non-native mother	0.183	0.189	1.030	0.256
Non-native father	0.169	0.207	1.227	1.617
Mother's mother high ed	0.314	0.333	1.062	1.324
Father's mother high ed	0.297	0.371	1.248	4.854
Mother's mother high inc	0.556	0.620	1.116	2.973
Father's mother high inc	0.562	0.708	1.259	6.271

NOTE.— The table reports mean values for the full sample and for reform compliers according to the approach used in section 6 of the papers in column (1) and (2) and for the 1995 (upper panel) and 2002 (lower panel) reforms, respectively. See the text for further information. The two last columns report the ratio of compliers to the overall population and the Z-score from a statistical test of the null that the ratio is equal to 1. Ratios below (above) 1 imply that compliers have mean values of the covariate below (above) the overall sample population.

Taken together, these results suggest that, on the one hand, complier couples are more "traditional" than couples in the overall, with lower educated women matched to higher educated men and parental leave share is higher than the husband's. On the other hand, complier women are more likely to come from families with a highly educated mother who earned higher incomes. The latter is consistent with the gender identity hypothesis, recently explored in detail by Bertrand et al. (2015), showing that women who have a higher income potential than their husbands "compensate" from this deviation from the norm (that women should earn less than their husbands) by taking on more of the household work.

7 Extensions

7.1 Cohabitation as learning

So far, we have analyzed separation among couples who were either married or cohabiting at the time of birth. While most marriages are preceded by cohabitation, however, not all cohabiting couples end in marriage, since cohabitation may be viewed as a substitute for marriage. Thus, while cohabitation can be used to reduce the uncertainty about the quality of a match, there may also be differential sorting into cohabitation and marriage, as couples who live together but are not married may be couples that gain less from marriage.

In this section, we consider couples that were cohabiting but not married at the time the parental leave reforms were implemented, i.e., at child birth. Viewing household formation as a dynamic process consisting of different stages, cohabitation is a stage during which the couples can learn about match quality. Cohabiting couples thus have three choices in each time period: remain cohabiting, dissolve the relationship, or marry.

Column (1) of *Table 7* reports RD estimates of the effect of having moved on to marriage three years after the birth of their first joint child, among cohabiting couples. Consistent with the IV results on dissolution, the IV estimate suggests that an unexpected decrease in the mother's share of parental leave decreases the probability of cohabiting couples to upgrade to marriage in the first reform sample (Panel A). The second reform sample, however, does not yield statistically significant effects, and the coefficient is close to zero. Two potential explanations for this apparent heterogeneity come to mind: first, couples affected by the second reform were already treated by the first reform, which could have attenuated the effect due to, for example, compositional changes in the fertile population. Moreover, as the second reform was less restrictive than the first (since it added extra parental leave rather than reshuffling existing days), compliers, from which the effect is estimated on, may be more accommodating to the institutional changes.

TABLE 7.
IV estimates of mothers' share of parental leave: Couple types

	(1) Marriage Upgrade	(2) Dissolution Cohabitation	(3) Dissolution Marriage
A. First parental leave reform (1995)			
Mother's share of PL	0.524*	-0.449**	-0.196
	(0.284)	(0.217)	(0.185)
Mean of outcome	0.196	0.192	0.152
% Effect Δ_{SD}	0.383	-0.440	-0.193
First stage F-stat	101.4	101.4	39.5
Observations	22,443	22,443	14,103
B. Second parental leave reform (2002)			
Mother's share of PL	0.068	-0.601***	-0.628***
	(0.282)	(0.199)	(0.148)
Mean of outcome	0.206	0.175	0.135
% Effect Δ_{SD}	0.048	-0.244	-0.465
First stage F-stat	94.7	94.7	69.4
Observations	36,571	36,571	31,783

NOTE.— The outcome variables are defined to equal unity if (1) the cohabiting couple is married; (2) the cohabiting couple is separated; (3) the married couple is divorced, three years after the birth of their joint child. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

7.2 Effect heterogeneity

Couple type

Dissolving a marriage compared to a cohabitation is likely to be associated with significantly higher costs. For example, the right to alimony payments for an economically disadvantaged divorcee does not extend to cohabiting couples upon separation. In addition, married couples may have been together for a longer duration and could therefore be more stable matches than cohabiting couples. Although we are studying cohabiting couples with joint children, it is not unlikely that cohabitation is a stage for learning the couple's match quality and marital gain and hence not simply an alternative to marriage.

In this section, we explore whether there are differential effects of the mother's share of parental leave on the probability of couple dissolution across cohabiting and married couples. Column (2) of *Table 7* shows that there are strong positive effects of decreased specialization on the probability that a cohabiting couple dissolves, in both reform samples. Furthermore, column (3) of *Table 7* reports the corresponding results on divorce for married couples. In the first reform sample, there is a non-significant effect on divorce. This is not due to a weak first stage but rather to the lack of a reduced form effect, implying that married couples stayed married even though they specialized less in the household. However, the effect on divorce in the second reform is very similar to the effect on separation among cohabiting couples. Hence, we find mixed evidence on the effects of dissolution costs on separation.

Birth order

Parents may vary in their parental leave behavior depending on the order of the child being born. In particular, it is likely that the parental leave reforms may influence first-time parents and parents who already have children differently. For instance, first-time parents may be more open to new options than parents who already have experience in dividing parental leave for a previous child where it might be harder to break old habits. This might be in particular for the first parental leave reform as the division of parental leave was much more one-sided before this reform compared to the second reform.

To investigate whether first-time parents were differently affected by the reforms compared to parents with previous children, we make use of information on birth orders for each parent in our data and run separate IV regression models for first time parents and parents who already had children. The results are reported in *Table 8* separately for mothers, fathers and for couples by reform. The estimated results mainly support the hypothesis stated above where the effect is mainly driven by parents who had children before the reforms. The estimated reform effects are relatively larger for these groups for all parental categories. Interestingly, the effects are also attenuated in the second reform, which would perhaps be expected if individuals became accustomed to a more evenly distributed parental leave in the period between the two reforms.

TABLE 8.
IV estimates of mothers' share of parental leave: Birth order effects

	Mothers		Fathers		Couples	
	(1) Firstborn	(2) Not firstborn	(3) Firstborn	(4) Not firstborn	(5) Firstborn	(6) Not firstborn
A. First parental leave reform (1995)						
Mother's share of PL	-0.140	-0.609***	-0.184	-3.896***	-0.179	-0.713***
	(0.136)	(0.184)	(0.135)	(1.187)	(0.175)	(0.268)
Mean of outcome	0.146	0.103	0.125	0.219	0.126	0.103
% Effect Δ_{SD}	-0.142	-1.282	-0.217	-3.232	-0.207	-1.528
First stage F-stat	318.7	69.7	282.8	16.4	175.3	33.7
Observations	67,403	5,747	59,477	13,673	32,055	2,952
B. Second parental leave reform (2002)						
Mother's share of PL	-0.174	-0.373***	-0.138	-0.310**	-0.340*	-0.464**
	(0.137)	(0.142)	(0.134)	(0.142)	(0.203)	(0.211)
Mean of outcome	0.121	0.089	0.108	0.103	0.102	0.084
% Effect Δ_{SD}	-0.223	-0.691	-0.201	-0.491	-0.518	-0.912
First stage F-stat	244.2	158.3	224.0	181.6	99.0	70.1
Observations	70,435	69,050	65,166	74,319	32,477	34,437

NOTE.— The outcome variable is defined to equal unity if the married couple is divorced three years after the birth of their joint child. Standard errors in parentheses. Firstborn is defined as the birth of the mother's or father's first child in the first four columns, respectively, and, in the last two columns, as the first child for both spouses. *** p <0.01, ** p<0.05, * p<0.1.

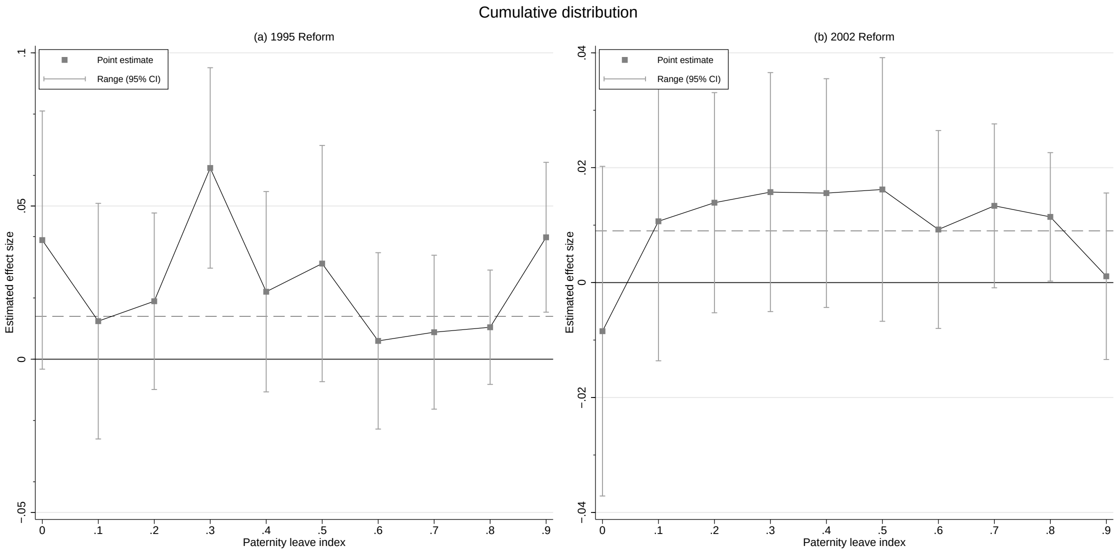
Peers

Peers may influence fathers' decisions to take paternity leave. In particular, prospective fathers living in neighborhoods where a greater share of men use parental leave may be less exposed to social stigma than other fathers living in areas where paternity leave is less common. If it is perceived as more socially acceptable with paternal leave this may affect fathers' own behavior and lead to less role conflicts in the household. To analyze this hypothesis we collect information on the average number of paternity leave days taken per child for the municipalities of Sweden and for each year of study and use this information to generate an aggregate index of paternity leave. The cross-municipal variation in paternity leave uptake is visualized in *Figure A.18* in the Appendix showing the mean number of paternity leave days taken per child across the 290 municipalities pooled over years 1992–2005. As can be seen, there is substantial spatial variation in paternity leave across the country.

To empirically analyze the impact of local paternity leave exposure on couple stability we use the CDF of the cross-municipal variation in average paternity leave days and run separate reduced form regression models for couples in each decile of the empirical distribution. The estimation results are plotted in *Figure 11* in which each point pertains to a separate regression estimate along with a corresponding 95% confidence interval. For comparison, the figure also indicates the zero line and the baseline effects from *Table 2*. The first thing to note from the figure is that the estimated effects are relatively homogenous across the paternity leave distributions for both reforms and in most cases close to the baseline results. Furthermore, the estimated reform effects are in general somewhat higher in the lower part of the distribution, indicating that couple separation was more likely in areas where paternity leave was relatively uncommon. In fact, comparing only couples above and below the median of the paternity leave distributions yields a statistically significant difference for the first reform.[15] The same pattern emerges for the second reform, but the point estimates are not statistically different at any conventional level of significance. As mentioned previously, this latter result may be explained by a potentially higher tolerance to paternal leave due to the sequential structure of the reforms. Hence, we conclude that this analysis gives some support for that the existence of peers may play a role for father's when coping with role conflicts in the family.

[15] Point estimates of 0.030 and 0.017 below and above the median paternal leave distribution, respectively. A two sample *t*-test for the equality of means yields a *p*-value of 0.04

FIGURE 11.
Reduced form effects by local pre-birth intensity of local paternity leave



NOTE.— Each point pertains to the estimated relative change in the probability of couple separation three years after childbirth due to a change in the mother's parental leave, conditional on the degree of paternal leave in the father's municipal of residence. The degree of paternal leave is measured by the deciles of the cross-municipal distribution of paternal leave in the year prior to the child's birth. The solid and dashed horizontal line indicates zero and baseline effects, respectively.

7.3 Household allocation of time

Both quota reforms in the Swedish parental leave system altered the within-household division of paid parental leave days; reducing the woman's share of paid leave. In this section, we investigate to what extent these effects translate to changes in women's allocation of time to paid and unpaid labor. The early literature on the relationship between earnings and marital stability often finds that (unexpected) increases in earnings for men increase marital stability, while the reverse is true for female earnings increases. To the extent that the reduced parental leave among women increases their intra-household share of earnings, this might offer a potential mechanism for the estimated increased separation risk. However, due to the significant flexibility of the parental leave system, where also 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 increased labor supply.

To investigate these issues, we begin by studying the effects on mothers' share of total household labor income during the first three years after birth. Due to the annual reporting of labor income, there is likely a mechanical interaction between the outcome variable and the treatment (child birth month), which complicates implementation of the RD design. We therefore estimate the effects on income using a difference-in-differences strategy, to account for (birth) seasonal variation in income. The results are presented in *Table 9*, and show that the first reform led to a decrease in the share of household labor income earned by the mother. This is contrary to what might be expected, if the reform was designed in order to increase mothers' labor supply or induce women to return to work sooner. The negative earnings effect for mothers is consistent with previous findings by Cools et al. (2015), who report no significant effects of the Norwegian quota reform on fathers' earnings and working hours, but evidence of negative effects on women's labor supply[16]. On the other hand, studying the same Norwegian reform, Kotsadam and Finseraas (2011) find that affected couples report lower level of conflicts over household division of labor, and are more likely to divide some household tasks equally.

In the context of our paper, one potential explanation for the negative earnings effects among women is that they compensated for the fewer parental leave days by taking unpaid leave (which is job-protected during the first 18 months after birth, after which working time can be reduced with up to 25 percent with job-protection). The 2002-reform seems to have had no impact on the earnings of women. However, the second quota reform was accompanied with a general increase of paid leave by one month, which was to a large extent used by women.

[16] However, Cools et al. (2015) find no significant effects on marital stability, although the point estimates suggest increased separation risk.

TABLE 9.
Difference-in-differences estimates on mothers' intra-household income share

	(1) Mother's income	(2) Father's income	(3) Mother's income share
A. First parental leave reform (1995)			
January × After	-12,585	-8,665	-0.0197**
	(7,788)	(17,027)	(0.00921)
Born in January	3,696	5,035	0.0178***
	(5,152)	(11,263)	(0.00609)
After	1,451	-34,943***	0.0131*
	(5,715)	(12,495)	(0.00676)
Mean of outcome	214,237.2	670,596.5	0.2616
Observations	10,721	10,721	10,721
B. Second parental leave reform (2002)			
January × After	-5,731	-6,939	-0.000414
	(7,418)	(16,407)	(0.00646)
Born in January	9,329*	25,386**	-0.00402
	(5,284)	(11,687)	(0.00460)
After	11,599**	219.5	0.00419
	(5,443)	(12,039)	(0.00474)
Mean of outcome	297,267.5	860,412.3	0.275
Observations	19,216	19,216	19,216

NOTE.— The table depicts the results from difference-in-differences estimation of the effect of the two reforms on the mother's intra-household share of labor income, earned during the first three years after child birth. *** p <0.01, ** p<0.05, * p<0.1.

To gain further insight into potential responses in the allocation of time within the household, we use data from the Swedish Living Conditions Surveys (ULF/ SILC). The survey is conducted annually and covers 11,000-13,000 nationally representative individuals (per year) aged 16 or older. Interviewees are asked about their health, financial resources, education, accommodation, market work, social relationships, and leisure activities. The ULF data cannot be matched to our register data sets, but it is matched to the national birth register, which includes information on the date of birth at a daily level. Thus, for mothers, we have information on date of birth, as well as on a number of variables concerning time use from ULF. Because we cannot match parental leave data to the ULF/SILC-sample, we are unable to perform IV estimation on the time-use outcomes. An additional caveat is that we lose many observations on the ULF-variables, since we restrict the sample to the interviews being conducted within three years of birth. Nevertheless, we estimate the reduced-form (ITT) effects of the reforms on mothers' time spent with co-workers (outside the workplace), time spent with close friends; time spent with relatives and acquaintances; time spent with persons outside the closest family; time spent with parents; and time spent with siblings. A higher value on these variables indicate more time spent in the respective activities. We also study the effect of the reforms on two dummy variables indicating whether the interviewee (female) was responsible for the household work, and whether the spouse of the interviewee (male) took care of the household chores. The results are presented in *Table 10* and show a statistically significant effect of the 1995-reform on time spent with relatives and acquaintances, and a significant effect of the 2002-reform on the time spent with friends or other persons outside the closest family. However, we find no evidence suggesting a shift in the distribution of household work. Taken together, the results on mothers' income share and on the time-use variables in ULF to some extent suggest that mothers compensate for the decreased paid parental leave days with unpaid leave, and increased their time spent in leisure activities outside the household. Thus, we find no evidence suggesting that the separation effects are mediated by an increase in the wife's earnings, nor by a change in the division of household chores[17]. An explanation for our findings could instead be that an altered division of parental leave take-up gives rise to general disagreement and marital conflict, by increasing the role of husbands in tasks that would otherwise have been assigned exclusively, or more extensively, to the wives. For instance, Perry-Jenkins and Folk (1994) find that, in particular among working-class families, marital conflict is avoided when the wife does "the woman's work", whereas for middle-class families marital conflict is more likely when the wife perceives that the husband is not taking on his "fair share" of the burden. Alternatively, increased marital conflict could arise if couples spend more time together at home due to the reforms, which is possible among couples with more than one child, if parental leave is taken out for one child each or as a means to extend joint holidays. This conjecture is to some extent supported by our findings that the separation effects are mainly driven by couples with more than one child.

[17]Caring for children is not included in this measure.

TABLE 10.
Reduced form effects on time-use variables

VARIABLES	(1) Time with Colleagues	(2) Time with Friends	(3) Time Outside HH	(4) Time with Others	(5) Time with Parents	(6) Time with Siblings	(7) Own house-work	(8) Partner house-work
A. First parental leave reform (1995)								
r	0.0628	0.0272	0.173*	0.114	0.160	0.124	-0.0123	-0.137
	(0.406)	(0.167)	(0.0923)	(0.256)	(0.479)	(0.341)	(0.0691)	(0.121)
x	0.00227	-0.00143	-0.0193	-0.0294	-0.0244	-0.0166	-0.00152	0.0123
	(0.0510)	(0.0215)	(0.0118)	(0.0328)	(0.0645)	(0.0443)	(0.00886)	(0.0155)
Observations	119	276	327	328	161	299	327	301
B. Second parental leave reform (2002)								
r	-0.388	0.217	-0.125	0.515*	0.719*	0.644	0.00779	-0.101
	(0.522)	(0.157)	(0.102)	(0.272)	(0.411)	(0.397)	(0.119)	(0.140)
x	0.0253	-0.0223	0.0113	-0.0494	-0.0643	-0.0848	-0.000280	0.00961
	(0.0702)	(0.0209)	(0.0137)	(0.0364)	(0.0549)	(0.0533)	(0.0159)	(0.0187)
Observations	62	207	226	226	220	210	227	210

NOTE.— The outcome variables are, from left to right, spending time with co-workers outside of work; spending time with close friends; spending time with persons other than close family members; spending time with relatives, friends or acquaintances (not co-workers or neighbors); spending time with parents; spending time with siblings; interviewee takes care of the household work; interviewee's spouse takes care of the household work. A higher value on the variables in columns (1)–(6) indicates more time spent in the respective activities. The variables in columns (7)–(8) are dummy variables. *** p <0.01, ** p<0.05, * p<0.1.

8 Summary and concluding remarks

This paper studies the effects of an unanticipated decrease in within-household division of parental leave on family structure. We exploit plausibly exogenous variation in the spousal share of parental leave generated by two reforms in the Swedish parental leave system, in a fuzzy regression discontinuity design. The reforms increased the incentives for fathers to take (more) parental leave, and we show that both reforms indeed led to a decrease in the mother's share of parental leave within households. Using the quasi-experimental variation in household specialization, we find that an unexpected decrease in the mother's share of parental leave increased the probability of separation among couples who were cohabiting or married at the time of the reforms. Thus, couples are likely to match on characteristics determining their intra-household allocation of work, e.g., earnings capacity, and react to unexpected changes to this division. In line with these findings our results also suggest a decreased probability for cohabiting couples to upgrade to marriage. We also find some evidence for differences in the effects of separation across married and cohabiting couples due to the potentially differential costs of dissolving a marriage as compared to cohabitation. While the instantaneous effects of the reforms implied higher separation rates, we also find some evidence that longer-term effects might have contributed towards a change of existing gender norms.

The two reforms studied in this paper represent two different ways of inducing fathers to take up leave, namely a pure reallocation of existing leave days from mothers to fathers, and earmarking leave while simultaneously extending the duration of paid leave. We show that the latter yields smaller increases in the separation risk, suggesting that they deferentially affect the decision processes within the household. In Norway, however, Cools et al. (2015) find no significant effects on marital stability from earmarking paid days to fathers, although their point estimates, albeit imprecisely estimated, also suggest increased separation risk. A third option, adopted in Iceland, is to extend the duration of paid leave, but where the additional leave can only be used by fathers. In a recent paper, Steingrimsdottir and Vardardottir (2015) estimate the Icelandic reform to decrease the divorce risk among treated couples. Thus, in terms of policy implications, it appears that different ways of introducing paternity leave may have quite different consequences for couples. This could be due to that the particular intervention is being perceived as more or less constraining among affected couples or on differences in the marginal groups who are effectively affected by the changes.

References

Angrist, J. D. and G. W. Imbens (1995). Two-stage least squares estimation of average causal effects in models with variable treatment intensity. *Journal of the American Statistical Association 90*(430).

Angrist, J. D. and J.-S. Pischke (2008). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.

Becker, G. S. (1973). A theory of marriage: Part i. *Journal of Political Economy 81*(4), pp. 813–846.

Becker, G. S., E. M. Landes, and R. T. Michael (1977). An economic analysis of marital instability. *Journal of Political Economy 85*(6), pp. 1141–1187.

Bertrand, M., S. E. Black, S. Jensen, and A. Lleras-Muney (2014). Breaking the glass ceiling? the effect of board quotas on female labor market outcomes in norway. Technical report, National Bureau of Economic Research.

Bertrand, M., E. Kamenica, and J. Pan (2015). Gender identity and relative income within households. *The Quarterly Journal of Economics 130*(2), 571–614.

Brines, J. and K. Joyner (1999). The ties that bind: Principles of cohesion in cohabitation and marriage. *American Sociological Review 64*(3), pp. 333–355.

Carneiro, P., K. V. Løken, and K. G. Salvanes (2015). A flying start? maternity leave benefits and long-run outcomes of children. *Journal of Political Economy 123*(2), 365–412.

Cools, S., J. H. Fiva, and L. J. Kirkebøen (2015). Causal effects of paternity leave on children and parents. *The Scandinavian Journal of Economics 117*(3), 801–828.

Dahl, G. B., K. V. Løken, and M. Mogstad (2014). Peer effects in program participation. *The American Economic Review 104*(7), 2049–2074.

Dahl, G. B., K. V. Løken, M. Mogstad, and K. V. Salvanes (2013). What is the case for paid maternity leave? Technical report, Forthcoming in *Review of Economics and Statistics*.

Ekberg, J., R. Eriksson, and G. Friebel (2013). Parental leave - a policy evaluation of the swedish "daddy-month" reform. *Journal of Public Economics 97*(0), 131–143.

Eriksson, R. (2005). Parental leave in sweden: The effects of the second daddy month. Technical report.

Folke, O. and J. Rickne (2016). The price of promotion: Gender differences in the impact of career success on divorce. Technical report. Unpublished manuscript.

Heckert, D. A., T. C. Nowak, and K. A. Snyder (1998). The impact of husbands' and wives' relative earnings on marital disruption. *Journal of Marriage and Family 60*(3), pp. 690–703.

Imbens, G. W. and J. D. Angrist (1994). Identification and estimation of local average treatment effects. *Econometrica 62*(2), 467–475.

Jalovaara, M. (2003). The joint effects of marriage partners' socioeconomic positions on the risk of divorce. *Demography 40*(1), 67–81.

Johansson, E.-A. (2010). The effect of own and spousal parental leave on earnings. Technical Report 2010:4, IFAU - Institute for Evaluation of Labour Market and Education Policy.

Johnson, W. R. and J. Skinner (1986). Labor supply and marital separation. *The American Economic Review 76*(3), pp. 455–469.

Kleven, H. J., C. Landais, and J. E. Søgaard (2015). Parenthood and the gender gap: Evidence from denmark. Technical report.

Kotsadam, A. and H. Finseraas (2011). The state intervenes in the battle of the sexes: Causal effects of paternity leave. *Social Science Research 40*(6), 1611 – 1622.

Lalive, R., A. Schlosser, A. Steinhauer, and J. Zweimüller (2014). Parental leave and mothers' careers: The relative importance of job protection and cash benefits. *Review of Economic Studies 81*(1), 219 – 265.

Lalive, R. and J. Zweimüller (2009). How does parental leave affect fertility and return to work? evidence from two natural experiments. *The Quarterly Journal of Economics 124*(3), 1363–1402.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economics. *Journal of Economic Literature 48*, 281–355.

Lillard, L. and L. Waite (1993). A joint model of marital childbearing and marital disruption. *Demography 30*(4), 653–681.

Liu, G. and A. Vikat (2004). Does divorce risk depend on spouses' relative income? a register-based study of first marriages in sweden in 1981–1998. MPIDR Working Papers WP-2004-010, Max Planck Institute for Demographic Research, Rostock, Germany.

Liu, Q. and O. N. Skans (2010). The duration of paid parental leave and children's scholastic performance. *The BE Journal of Economic Analysis & Policy 10*(1).

Patnaik, A. (2016). Reserving time for daddy: The short and long-run consequences of fathers' quotas.

Perry-Jenkins, M. and K. Folk (1994). Class, couples, and conflict: Effects of the division of labor on assessments of marriage in dual-earner families. *Journal of Marriage and Family 56*(1), 165–180.

Rege, M. and I. F. Solli (2010). The impact of paternity leave on long-term father involvement. (3130).

Schönberg, U. and J. Ludsteck (2007). Maternity leave legislation, female labor supply, and the family wage gap. (2699).

Steingrimsdottir, H. and A. Vardardottir (2015). How does daddy at home affect marital stability? Technical report. Unpublished manuscript.

Stevenson, B. and J. Wolfers (2006). Bargaining in the shadow of the law: Divorce laws and family distress. *The Quarterly Journal of Economics*, 267–288.

Stevenson, B. and J. Wolfers (2007). Marriage and divorce: Changes and their driving forces. *The Journal of Economic Perspectives 21*(2), pp. 27–52.

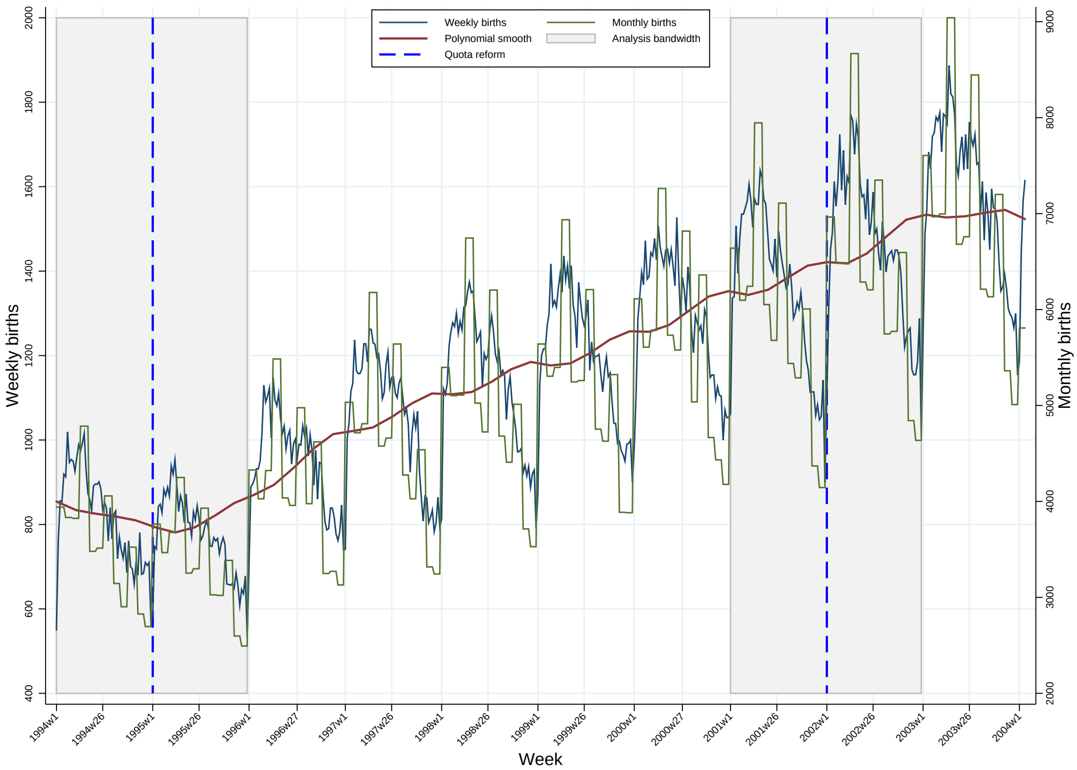
Svarer, M. and M. Verner (2008). Do children stabilize relationships in denmark? *Journal of Population Economics 21*(2), 395–417.

Tjøtta, S. and K. Vaage (2006). Public transfers and marital dissolution. *Journal of Population Economics 21*(2), 419–437.

Weiss, Y. and R. J. Willis (1997). Match quality, new information, and marital dissolution. *Journal of Labor Economics 15*(1), S293–S329.

Appendix

FIGURE A.1.
Child births in Sweden 1994–2004



NOTE.— Own calculations from Swedish inpatient and multi-generational registries. Pertains to mothers whose first child was born 1994–2004. Gray bars indicate PL quota reforms. Birth week obtained by linking birth month to inpatient spells.

TABLE A.1.
Parental leave take-up by child age

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

NOTE.— 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.

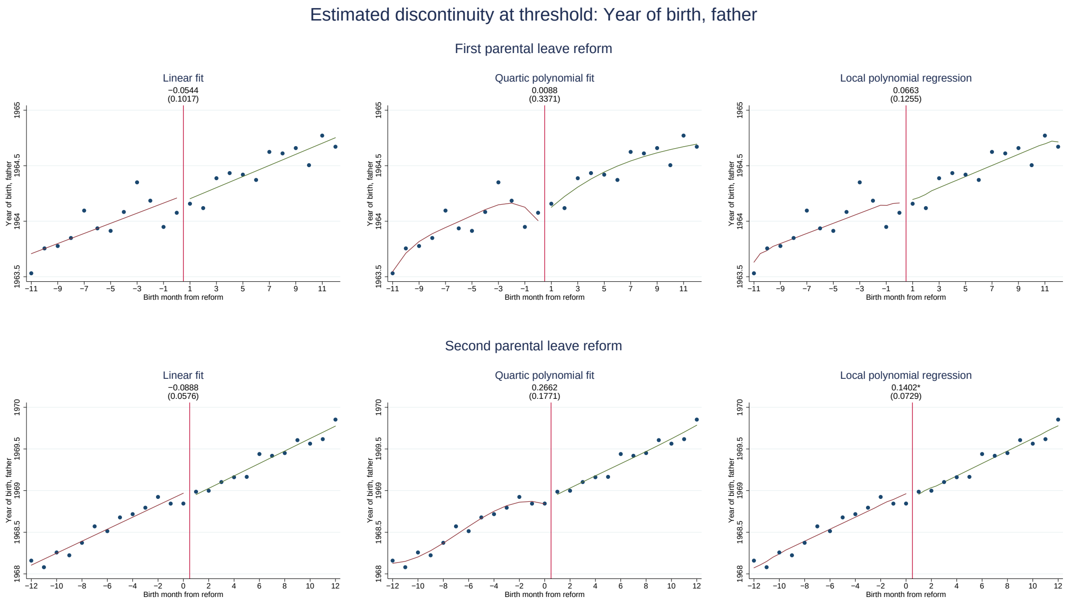
TABLE A.2.

Robustness check: Regression discontinuity estimates for mother's share of parental leave adjusted for observable characteristics: Couple dissolution

	(1) OLS	(2) ITT	(3) First stage	(4) IV
A. First parental leave reform (1995)				
Mother's share of PL	0.231***			-0.456**
	(0.012)			(0.210)
Born in 1995		0.016**	-0.036***	
		(0.007)	(0.003)	
Mean of outcome	0.144	0.144	0.892	0.144
% Effect Δ_{SD}	0.230	0.114	-0.0402	-0.455
First stage F-stat			144.7	144.7
Observations	38,306	38,306	38,306	38,306
B. Second parental leave reform (2002)				
Mother's share of PL	0.159***			-0.309**
	(0.008)			(0.150)
Born in 2002		0.010**	-0.033***	
		(0.005)	(0.002)	
Mean of outcome	0.105	0.105	0.827	0.105
% Effect Δ_{SD}	0.230	0.0959	-0.0395	-0.447
First stage F-stat			189.5	189.5
Observations	70,207	70,207	70,207	70,207
C. Placebo parental leave reform (2001)				
Mother's share of PL	0.151***			1.892
	(0.008)			(5.357)
Born in 2001		0.002	0.001	
		(0.005)	(0.002)	
Mean of outcome	0.113	0.113	0.847	0.113
% Effect Δ_{SD}	0.199	0.0205	0.00145	2.500
F-stat			0.259	0.259
Observations	66,484	66,484	66,484	66,484

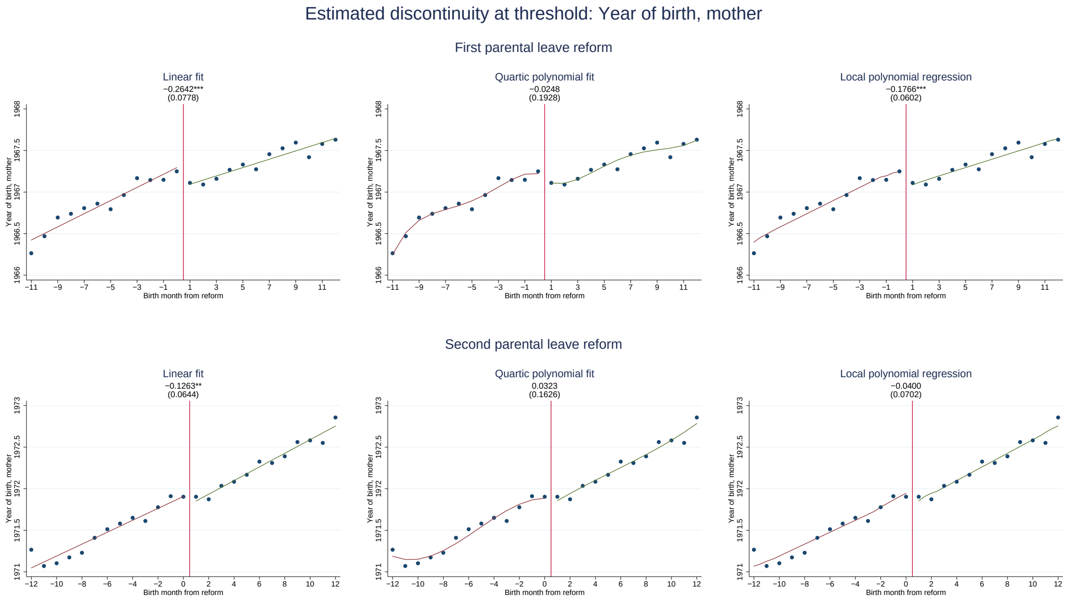
NOTE.— The table reports the number and share of parental leave days taken by the mother and father, respectively, and maternal share of total parental leave by child age. Covariates are included in the estimations reported in Columns (1)–(4). The included covariates are: dummy variables for the mothers' educational level (three categories), an indicator for whether at least one of the spouses is born outside Sweden, for the sex of the child and for whether the mother or father was a first-time parent. Standard errors are reported in parentheses.

FIGURE A.2.
Covariate balancing results: Father's year of birth



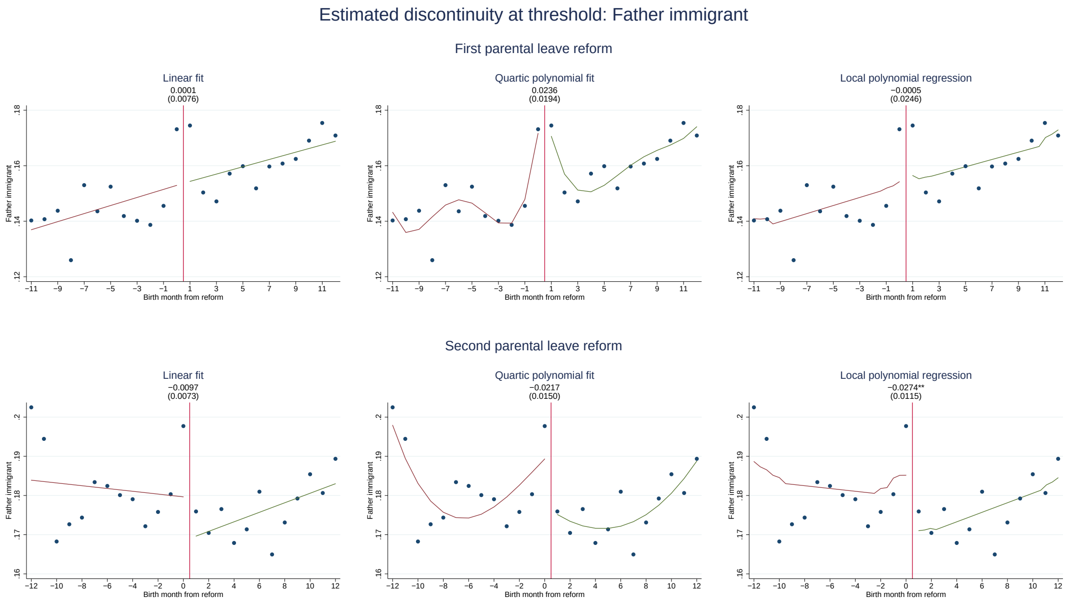
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.3.
Covariate balancing results: Mother's year of birth



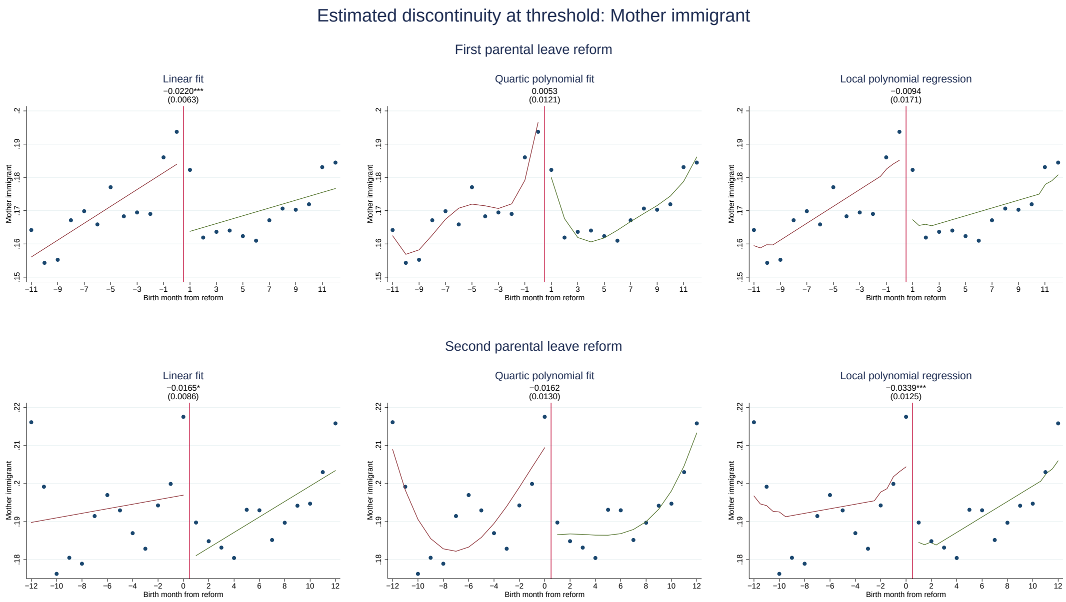
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.4.
Covariate balancing results: Father immigrant



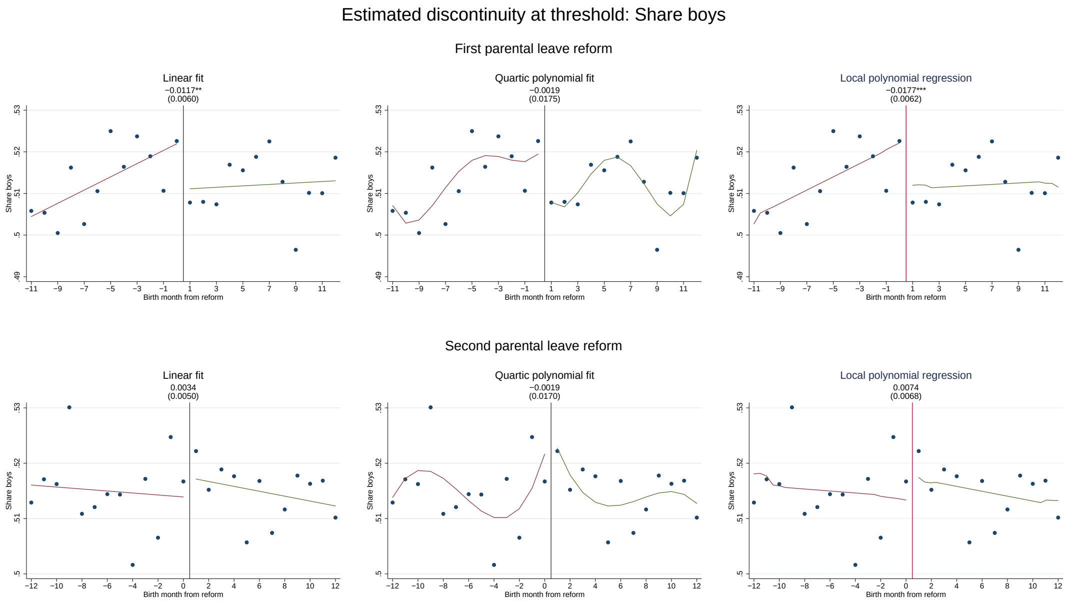
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.5.
Covariate balancing results: Mother immigrant



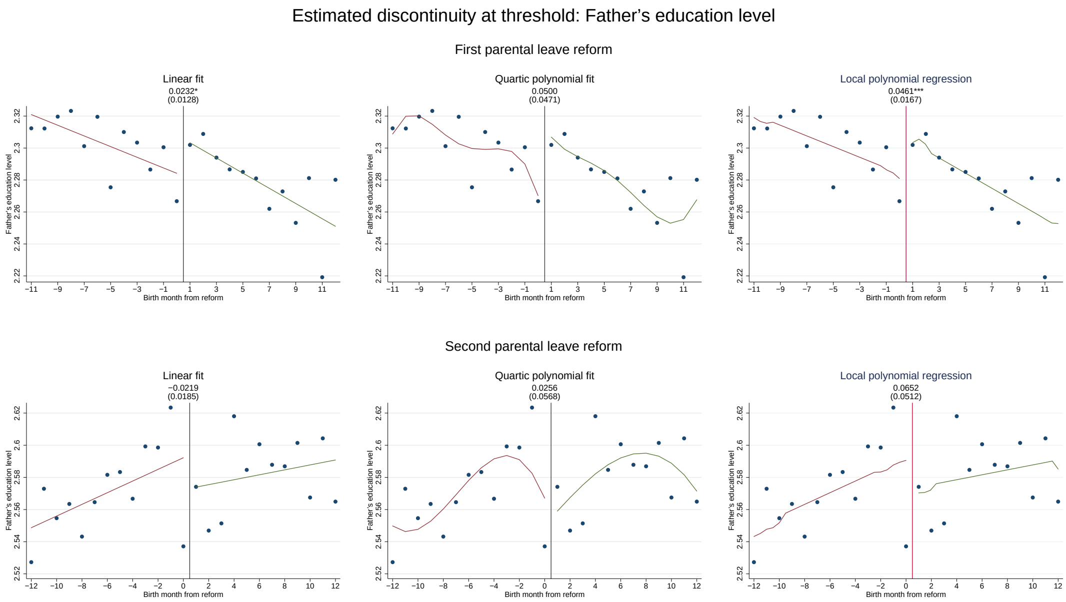
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.6.
Covariate balancing results: Child gender



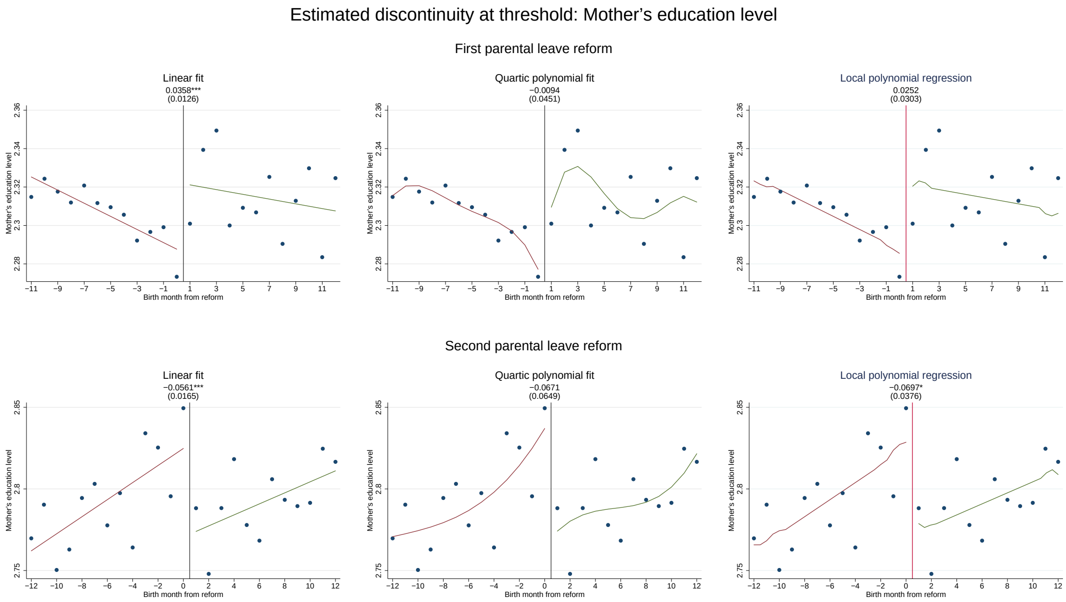
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.7.
Covariate balancing results: Father's education level



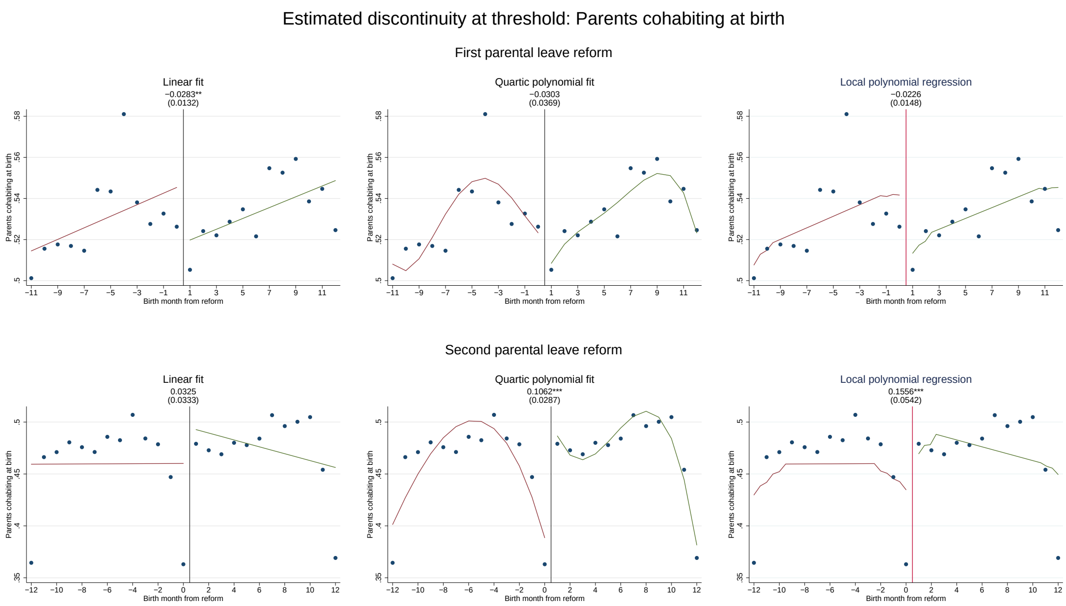
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.8.
Covariate balancing results: Mother's education level



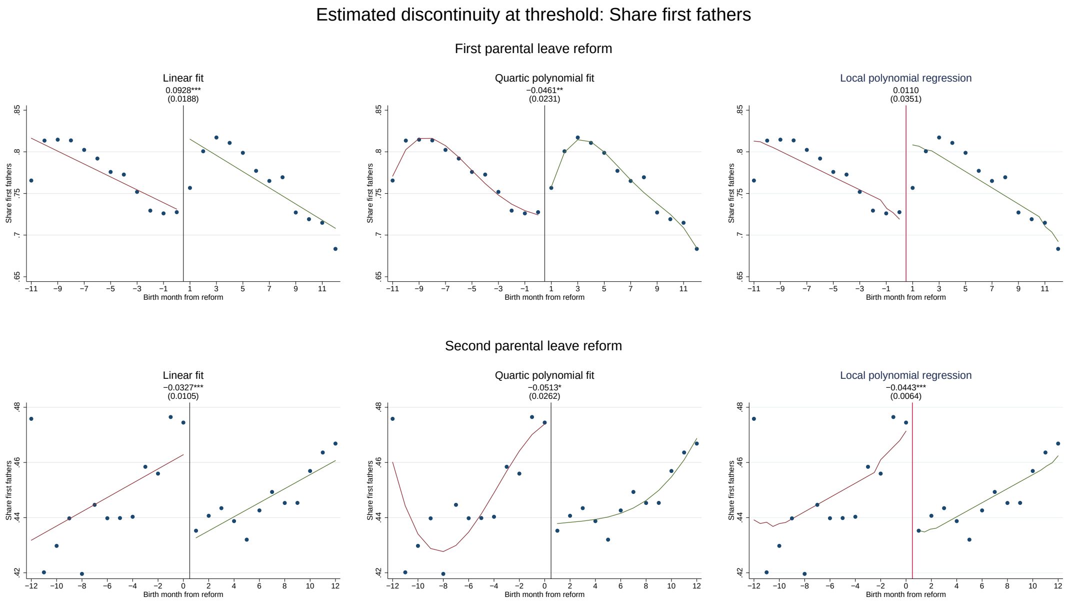
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.9.
Covariate balancing results: Cohabitation at birth



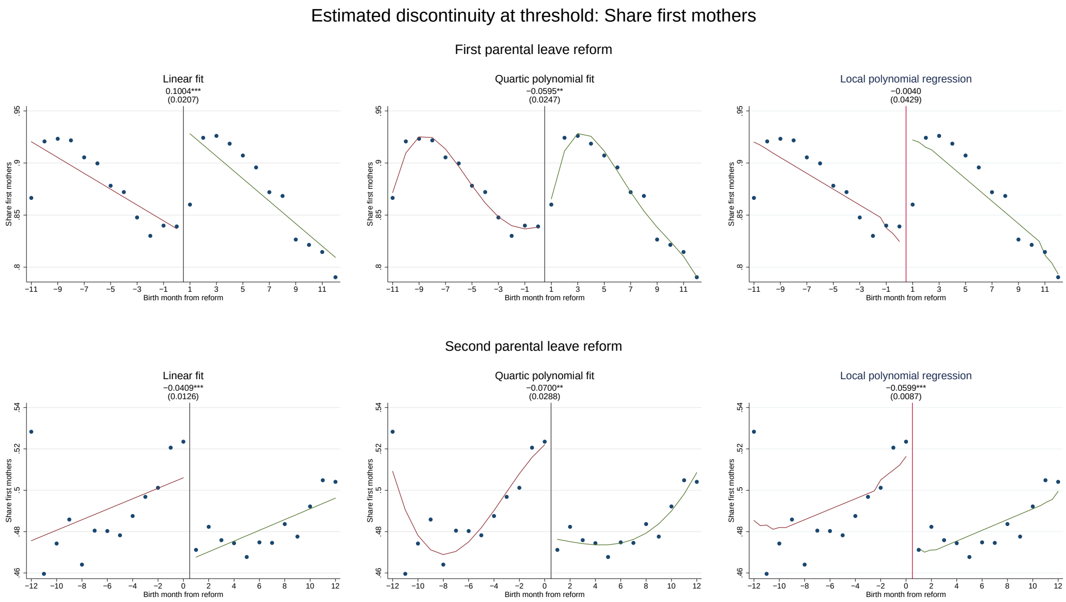
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.10.
Covariate balancing results: Share first fathers



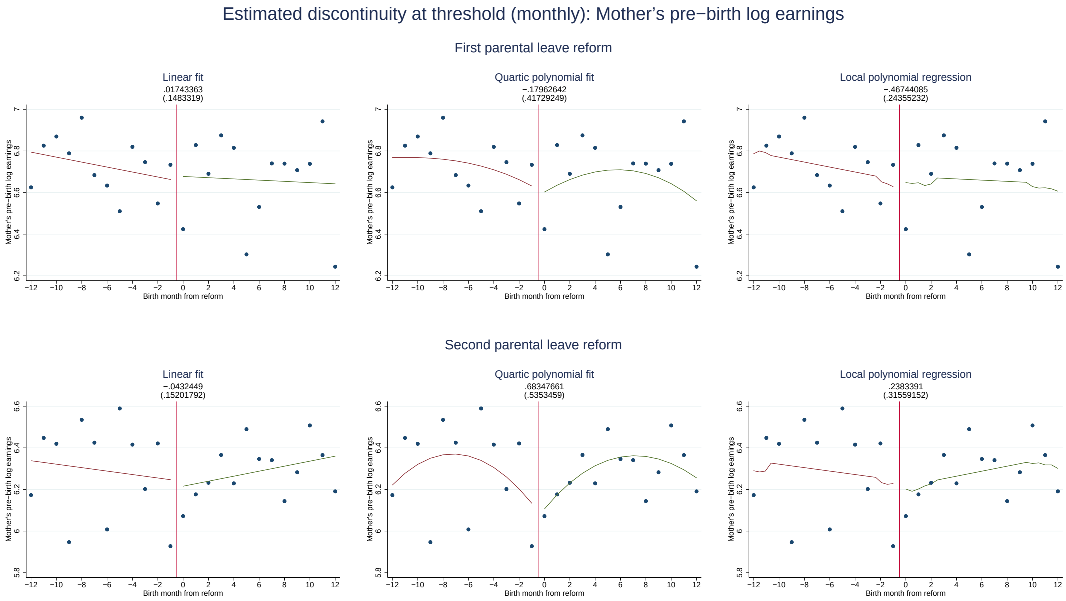
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.11.
Covariate balancing results: Share first mothers



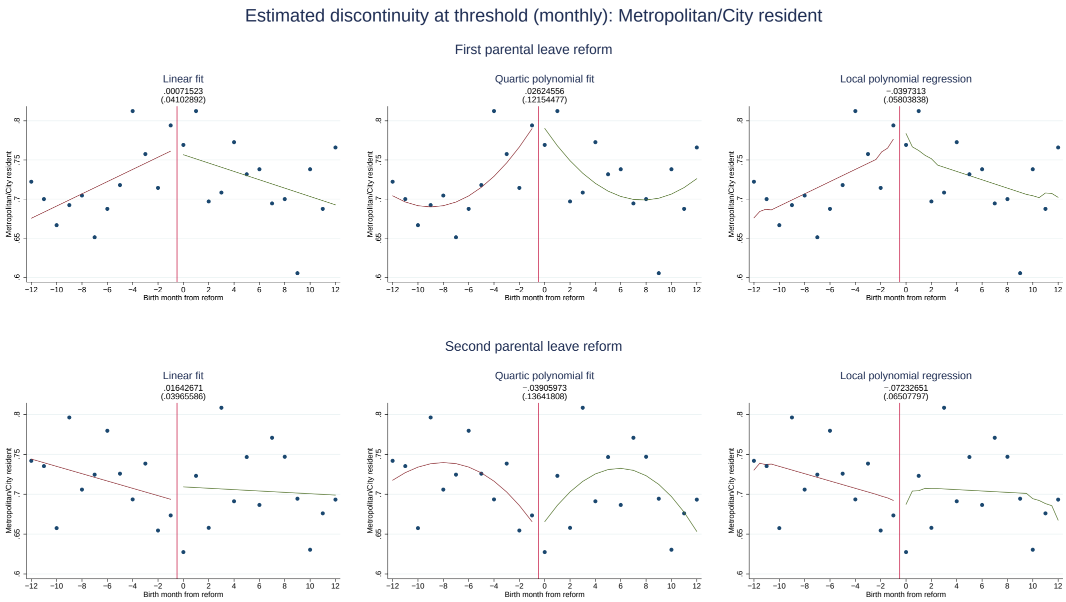
NOTE.— Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.12.
Covariate balancing results: Mother's pre-birth income



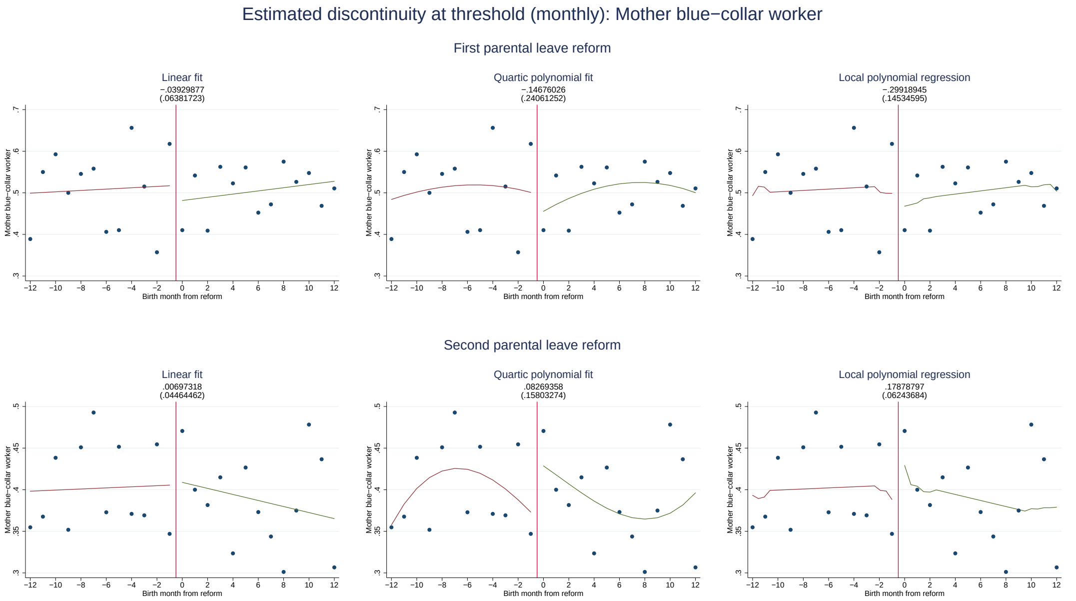
NOTE.— This graph was produced using a supplementary data set - Swedish Living Conditions Survey (ULF/SILC) - matched to some register data, but not matched to our main data set. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.13.
Covariate balancing results: Urban resident (pre-birth)



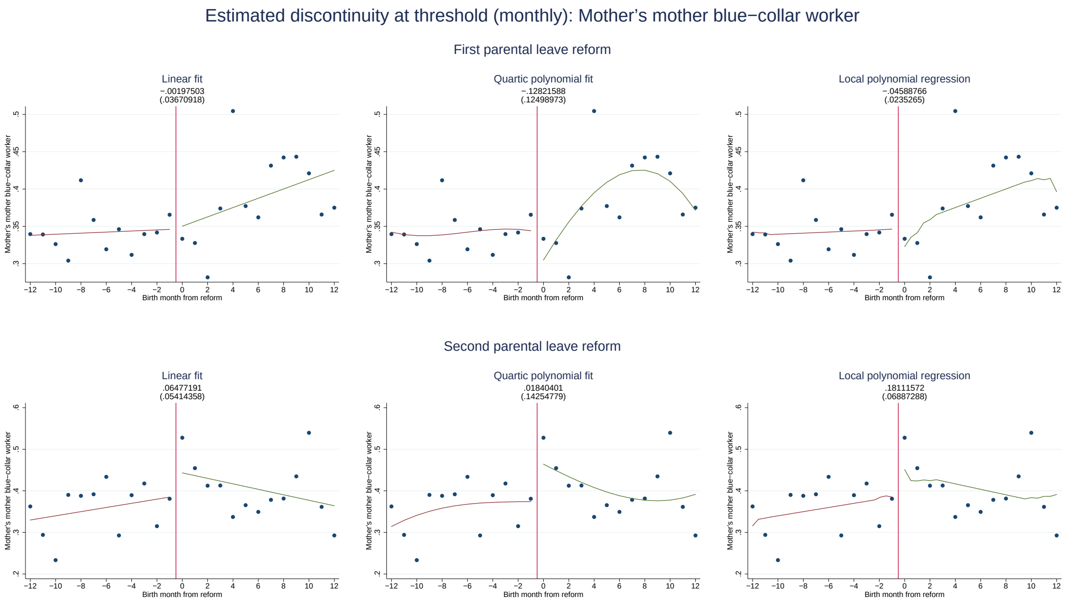
NOTE.— This graph was produced using a supplementary data set - Swedish Living Conditions Survey (ULF/SILC) - matched to some register data, but not matched to our main data set. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.14.
Covariate balancing results: Mother blue-collar worker (pre birth)



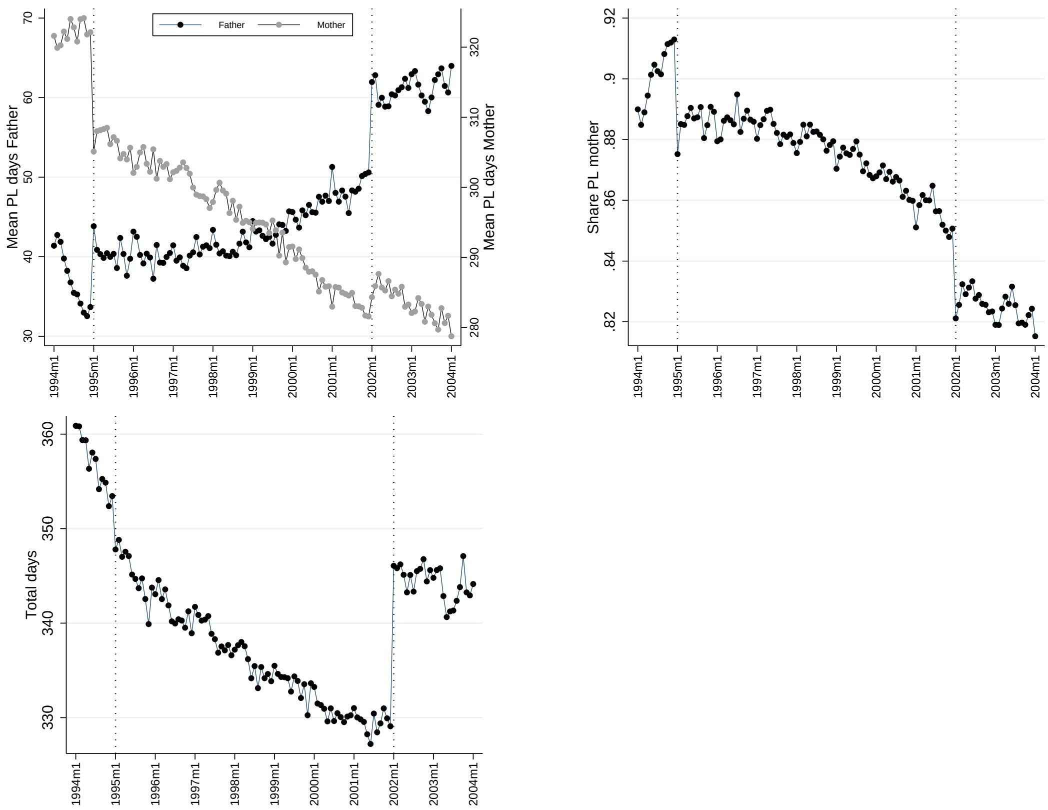
NOTE.— This graph was produced using a supplementary data set - Swedish Living Conditions Survey (ULF/SILC) - matched to some register data, but not matched to our main data set. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.15.
Covariate balancing results: Mother's mother blue-collar worker



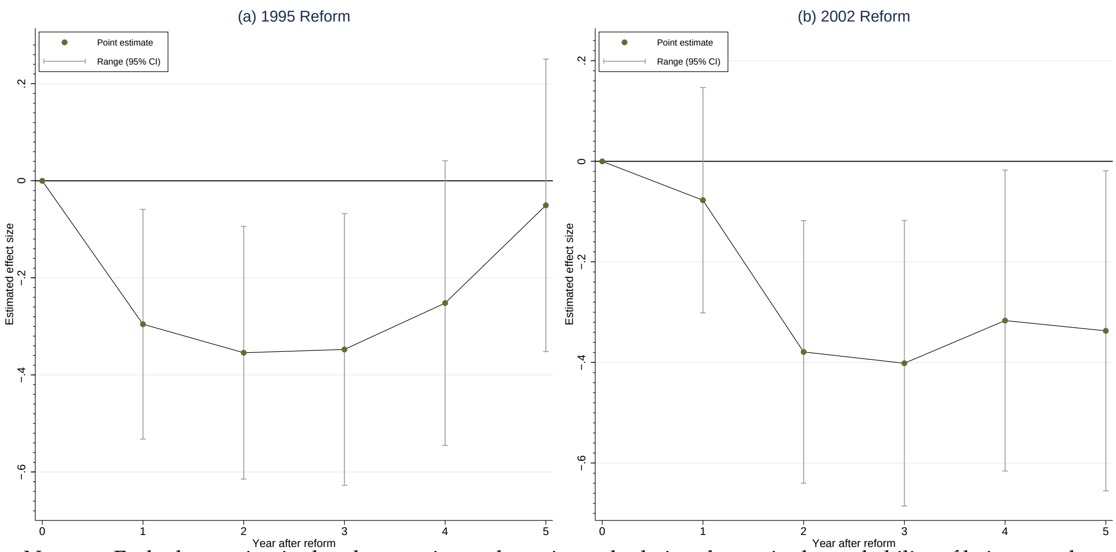
NOTE.— This graph was produced using a supplementary data set - Swedish Living Conditions Survey (ULF/SILC) - matched to some register data, but not matched to our main data set. Standard errors in parentheses. *** p <0.01, ** p<0.05, * p<0.1.

FIGURE A.16.
Total parental leave uptake during first three years after birth by child birth month



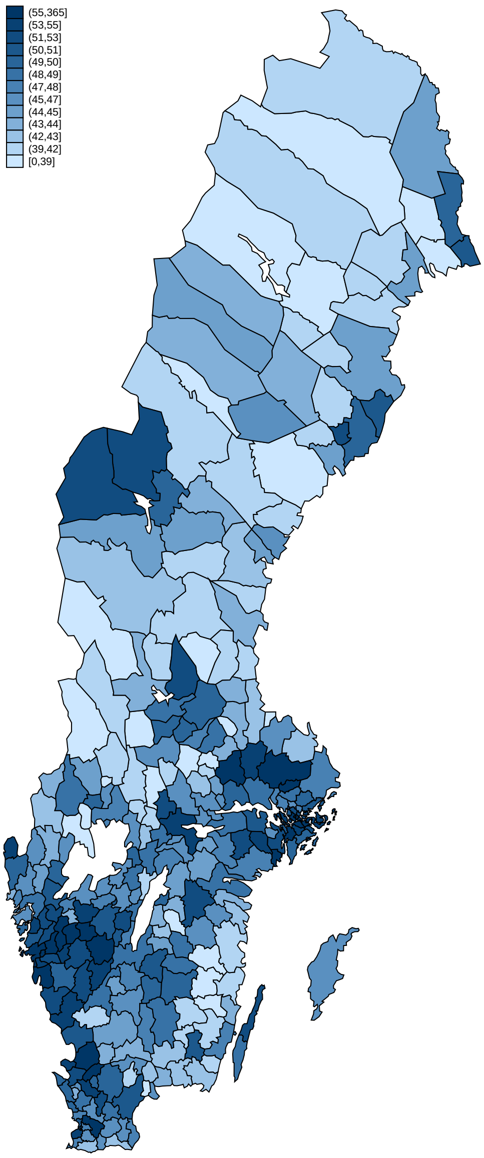
NOTE.— The upper left graph shows the average number of parental leave days taken by mothers and fathers, respectively, by child birth month. The upper right graph shows mothers' share of parental leave, and the lower graph the total number of leave days taken, by child birth month.

FIGURE A.17.
IV effects by year from childbirth - PL measured during the first three years after birth



NOTE.— Each observation in the plot pertains to the estimated relative change in the probability of being together t years after childbirth due to a change in the mother's parental leave, as inferred from estimation of the baseline IV model.

FIGURE A.18.
Paternity leave days distribution per child in Sweden, 1992–2005



NOTE.— The figure plots the average number of parental leave days per child for fathers who took at least one day of parental leave the year the child was born by municipality over the years 1992–2005. Categories are defined as deciles of the pooled paternity leave distribution.

TABLE A.3.
Robustness check: Regression discontinuity estimates for mothers' share of parental leave: Couple dissolution, using 3-year follow-up period of parental leave take-up

	(1) OLS	(2) ITT	(3) First stage	(4) IV
A. First parental leave reform (1995)				
Mother's PL share	0.260***			-0.329
	(0.013)			(0.220)
Born in 1995		0.012	-0.035***	
		(0.008)	(0.003)	
Mean of outcome	0.143	0.143	0.897	0.143
% Effect of Δ_{SD}	0.264	0.0810	-0.0394	-0.333
First stage *F*-stat			126.3	126.3
Observations	35,723	35,723	35,723	35,723
B. Second parental leave reform (2002)				
Mother's PL share	0.162***			-0.462*
	(0.008)			(0.245)
Born in 1995		0.010**	-0.022***	
		(0.005)	(0.003)	
Mean of outcome	0.105	0.105	0.841	0.105
% Effect of Δ_{SD}	0.247	0.0965	-0.0261	-0.703
F-stat			67.71	67.71
Observations	63,057	63,057	63,057	63,057

NOTE.— The outcome variable is defined to equal unity if the couple is no longer together three years after the birth of their joint child. The regressor of interest - Mother's share of PL - is now calculated on the parents' take-up during the first three years after the birth of the child. Before separation, the couple could be either cohabiting or married. Standard errors in parentheses. The regressor of interest - mother's parental leave share - is measured during the first three years after birth. *** p <0.01, ** p<0.05, * p<0.1.