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How Does Daddy at Home Affect Marital Stability?

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Abstract

We investigate whether paying fathers to stay at home with their newborn child affects marital stability. Our empirical analysis is based on a reform in Iceland that offered one month of parental leave earmarked to fathers with a child born on or after January 2001. This reform created substantial economic incentives for fathers to be more involved in caring for their children during their first months of life, and the take-up rate in the first year was 82.4%. We apply a regression discontinuity framework to assess the effect of this reform on the probability of separation among couples and find that parents who are entitled to paternity leave are less likely to separate. The effect persists throughout the first fifteen years after the child is born. Interestingly, the paternity leave has the strongest impact among couples where mother has higher, or equal, educational attainment to that of the father.

JEL classifications: J12, J13, J16, J18

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Divorce can wreak havoc on the families in which it occurs. Marital dissolution has a strong negative effect on the mental and physical health of both spouses, and there is strong evidence of a close connection between growing up in a one-parent family and suffering long-term economic and social difficulties (McLanahan and Sandefur, 1994; Waite and Gallagher, 2000; Gottman, 1998; Burman and Margolin, 1992). However, growing up in a household in which the parents have marital problems can also adversely affect a child. Marital distress and conflicts are, for example, associated with anxiety, poor social competence, health problems, poor academic performance, and reduced cognitive performance among children (Dadds and Powell, 1991; Gottman, 1989; Ghazarian and Buehler, 2010; Hinnant et al., 2013). Therefore it would not necessarily be beneficial to lower the divorce rate, as it is doubtful whether much would be gained if the reduction simply resulted from more unhappy couples staying together. Although influencing marital satisfaction is usually thought to be outside the role of policy makers, the prevalence of marital conflicts and dissolutions makes their negative impact highly relevant to societal outcomes. A policy that could lower divorce rates by directly reducing household stress and conflicts could therefore be highly valuable.

A few countries have introduced paternity leave, or a "fathers' quota," into their parental leave systems to encourage fathers to take a greater part in childcare. One of the main motivations for these reforms is the idea that gender equality in the household is a necessary condition for gender equality in the labour market. Although these policies are not aimed at increasing marital stability as such, they do affect the division of labour in the household and may therefore affect marital discord. In this paper we examine the introduction of a fathers' quota in Iceland to investigate whether reserving part of the parental leave to fathers affects divorce risk. In Iceland three months of paternity leave were added to the existing six-month-long leave. The reform was implemented in stages, so that in 2001, one month of the parental leave was earmarked to fathers, which increased to two in 2002, and finally to three in 2003. We focus on the effect of the first month to be added, because the announcement of the reform occurred too late to affect the fertility choices of those parents who had children during the last months of 2000 and the first months of 2001. These parents make up our treatment and control groups.

The Icelandic policy reform is particularly interesting because it gave men the largest non-transferable share of parental leave (three months out of nine) in the world. Furthermore, Iceland and Sweden are the only countries that give equal non-transferable parental leave rights to mothers and fathers.[1] The take-up rate of the paternity leave in Iceland was also high, and the growth in men's share of the total parental leave taken has been much steeper there than in the other Nordic countries. Among fathers who had children in 2001, 82.4% took paternity leave (Eydal and Gislason, 2008). The average paternity leave was 39 days, that is, slightly more than the one month earmarked to them. In 2003, when the paternity leave had been increased to 3 months, 86.6% of fathers took leave, and the average length of the paternity leave was 97 days. In effect, the policy shifted fathers from taking 0% share to taking one third of the total leave.

We use a detailed, Icelandic register-based panel dataset, to identify the causal effects of the reform by comparing these two groups of parents, using both regression discontinuity (RD) and difference-in-differences (DD) method. Our identification strategy is based on the fact that parents who had their child after the reform date did receive a treatment, namely paternity leave, whereas parents who had their child before the reform did not. The analysis is based on the intention-to-treat. We do not observe who is treated in our sample, but as our data consists of the relevant population, we expect around 82.4% of the fathers in our treatment group to have taken paternity leave. The policy process was very fast —the new law was passed on May 9, 2000, and went into effect January 1, 2001—as a result, parents who gave birth around the time of the reform could not have known about it at the time of conception. This allows us to assess whether a shift towards greater equality, by facilitating more equal sharing of responsibility for childcare and housework between men and women, makes marriages more stable.

Our results show that the introduction of the paternity leave had a significant and

[1] In Sweden, men and women each get two non-transferable months out of 16 months total

sizeable effect on the probability that parents would stay together, in particular during the first years after their child was born. Regression discontinuity difference-in-differences (RD-DD) estimates for couples, who had children within a fifteen week window around the reform, show that the paternity leave reduced the probability of parents' separation by 11.6 percentage points five years after having their child, and by 8.9 percentage points fifteen years after having their child.[2] The main effect stems from parents who have the same level of education, or couples where the woman has higher level of education. On the other hand, the estimated long term effect on divorce probability is positive (although insignificant) when we look at couples where the father has a higher level of education than the mother.

To the best of our knowledge, this is the first paper to focus on the effect that paternity leave and greater equality in child-rearing have on divorce risk.[3] The paper draws on, and contributes to two strands of literature. The first is the growing literature on parental and paternity leave. A number of studies have looked at how parental leave (which in most cases is only used by mothers) affects parents and children (e.g., Lalive and Zweimüller, 2009; Carneiro et al., 2015; Dahl et al., 2016). On the other hand, only a handful of papers have investigated the causal impact of earmarking a portion of the parental leave for fathers. Among these is a paper by Johansson (2010) that investigates the effects on earnings of Swedish paternity leave reform in 1995 and 2002 using a DD approach. Johansson fails to find any evidence that paternity leave affects mothers' and fathers' earnings. Rege and Solli (2013) estimate a DD model that exploits an exogenous variation in paternity leave in Norway provided by the introduction of a four-week paternity quota in 1993. They find that this paternity leave had a negative effect on fathers' earnings. Cools et al. (2015) confirm this by combining an instrumental variable (IV) approach with the DD approach to obtain the causal effect of the same reform, and they find

[2] This is as far as we can follow the parents who had children around the reform because data after this time is not yet available.

[3] One notable exception is Cools et al. (2015) who investigate the effect of the introduction of a four-week paternity leave in Norway in 1993 on various children's and parent's outcomes, including divorce when the children turn 14, but they find no significant effect.

that the reform had a negative impact on the earnings and employment of mothers as well, both in the medium and long term. Ekberg et al. (2013) find that incentives have strong short-term effects on male parental leave uptake, but find no significant effect on parents' long-term wages and employment. Furthermore, they find no significant effect on how parents split the household work, measuring the shares of household work by the shares of the leave taken for care of sick children. By contrast, Kotsadam and Finseraas (2011) apply a RD approach to survey data to estimate the effect of the Norwegian parental leave reform and find that the "daddy quota" reduced conflicts over the division of household labour and led to more equal sharing of housework in the long run. In a recent study Patnaik (2019), estimates a DD model, using a policy reform in Canada, and finds that paternity leave had a large and persistent effect on domestic equality, as exposed fathers contribute more to home production, and exposed mothers spend more time at the workplace.

The second strand is the empirical literature on the causes and consequences of marital dissolutions. Recent studies have identified a number of factors that increase the probability of divorce. In addition to those discussed previously in this section—children, wives' relative wages' and wives' labour market participation—negative financial shocks have been found to increase the probability of a divorce. Rainer and Smith (2010), for example, find that negative home-price shocks increase the risk of separations, and Rege and Solli (2013) find that plant closures significantly increase the risk of marital dissolutions among workers in the affected plants. In addition, Tjøtta and Vaage (2008) find that governmental support for children and for divorced families increases the probability of divorces, and Dahl and Moretti (2008) find that parents in the U.S. are more likely to divorce if their firstborn child is a girl than if it is a boy. Furthermore, previous studies (e.g., Wolfers, 2006) suggest changing divorce laws can only have a minor role for changing divorce rates. Our study adds significantly to this literature by looking at a policy that affects divorce rates by incentivising couples to change their division of labour in the household.

Our findings are policy relevant for a number of reasons. The effects of changes to parental leave schemes on marital stability may either exacerbate or dampen the financial and welfare costs associated with having a baby. In addition, externalities of marital dissolution in our setting may be substantial because children are involved. Given the high level of current interest in policy to equalise parental leave and create a level playing field for men and women in the workplace, it is worth better understanding the broader consequences of such policy.

The remainder of the paper proceeds as follows. We discuss the theory of marital stability in Section 2. In Section 3 we describe the institutional setting and the reform we are examining while we describe our data and the outcome variables under consideration in Section 4. We present our empirical approach in Section 5 and the main results in Section 6. Section 7 concludes the paper.

1 Theory of Marital Stability

Becker (1973) was the first one to provide a theoretical framework for studying the institution of marriage, and Becker et al. (1977) were the first to provide a theoretical analysis of marital dissolution. Their economic approach to the family interprets such behaviours as childbearing, marriage, and divorce as active choices made by maximising individuals. According to this view, the marriage institution is a highly efficient setup for individuals in which one partner specialises in market work while the other specialises in domestic work.

As a consequence, if partners "invade" each other's territories, their specialisation is reduced and the gains to be made from the marriage decline. Furthermore, the decision to stay married depends on a comparison between the utility associated with being married and the utility associated with the outside option of a divorce, so this decline reduces the desirability of staying married. Because men generally have greater attachment to the labour force and higher wages, whereas it is unavoidable that women take care of carrying and breastfeeding their children, the most stable marriages are said to be those in which the husband exchanges economic support for his wife's household tasks, and vice versa. Some evidence has been offered in support of this view, showing that men and women have preferences for traditional gender roles and that a woman's financial dependence on her spouse is itself an important contributor to marital stability; in particular, divorce is more likely if a woman's income exceeds her husband's (Bertrand et al., 2015; Heckert et al., 1998; Jalovaara, 2003; Liu and Vikat, 2004).

A growing literature looks at the role of gender identity on family formation and marital stability. Bertrand et al. (2015) show that societal norms, such as the idea that wives should not earn more than their husbands, affect the formation of marriages. Moreover, couples in which the wives earn more than the husbands tend to be less satisfied with their marriages and are more likely to divorce. Finally, women who earn more than their husbands have also been found to carry out a greater share of the household chores than women whose partners earn more than them. This contradicts the Beckerian view, which holds that benefits of marriage stem from specialisation and predicts a negative relationship between one's share of the households' tasks and the share of the household income one provides.

However, a number of recent studies find that shared responsibility for bringing home the bacon makes relationships more robust. Cohabiting couples in the U.S. have been found to be more stable when the partners are more equal in terms of household chores and income (Brines and Joyner, 1999). Schoen et al. (2006) find that wives' full-time employment is associated with increased marital stability, and the findings of Sayer and Bianchi (2000) and Sayer et al. (2011) suggest that the economic independence of women is not the cause of marriage dissolutions but rather allows already unhappy wives to leave. Furthermore, Sigle-Rushton (2010) finds divorce rates to be lower in families in which husbands take a greater part in the housework, shopping and childcare.

Other empirical findings have also been used to cast doubt on the Becker (1973) view. Contrary to the his predictions, people do engage in positive assortative mating by wages, other things equal, which suggests that the gains from marriage are not brought about just by specialisation. Lam (1988) offers one explanation for this documented regularity. He develops a model in which the joint consumption of public goods is an important source of gains from marriage, and shows that this generates a tendency toward positive assortative mating by wages because spouses have similar demands for public goods.

The Beckerian model was heavily criticised by Oppenheimer (1994, 1997) on both theoretical and empirical grounds. She provides an alternative view, often referred to as the flexibility model that makes different predictions about the effects of female employment on marital stability. One of her main criticisms is directed at the unrealistic assumption of lifelong employment, and she argues that a high degree of specialisation puts relationships at risk because any temporary or permanent incapacity of a specialised agent would result in functions vital to the household not being carried out. In contrast with the Beckerian view, the flexibility model predicts that shared responsibility for both income and housework makes marriages more robust by reducing income risk and securing greater financial stability.

The inner workings of the household have changed considerably in the last decades, for multiple reasons. It has become easier to control pregnancy, there are more laboursaving devices in the home, and there is more work outside the home. This has led the share of married women in the U.S. who are employed to rise from 6% in 1900 to 30% in 1960 and 70% today. As a result, couples have more time and money, and it has become more important to individuals to have partners they enjoy sharing these with. It can therefore be argued that marriage today is fundamentally different from what it was 50 or 60 years ago. It has moved from a factory model in which husbands are breadwinners and wives are homemakers —that is, a model with production complementarities —to a hedonic model with consumption complementarities.

There have been other changes during this period. Stevenson and Wolfers (2007) have documented a declining trend in both marriages and divorces in the U.S. over the last 30 years, meaning that a greater proportion of today's marriages will remain intact 30 years into the future. This raises the question whether public-goods and risk-sharing channels are more important for marital stability than specialisation, and whether greater equality among men and women makes for greater marital stability. However, the fact that gradual increase in equality among couples has coincided with a trend toward more stable marriages does not mean that the former caused the latter. In this paper, we investigate whether such a causal link exists by taking advantage of an unexpected parental leave reform aimed at equalising the labour market prospects and the childcare responsibilities of men and women. The reform thereby provides us with a setting where we can test the predictions of Becker vs. Oppenheimer.

1.1 Children, Fathers' Quota, and Marital Stability

In traditional economic models, children stabilise marriages. Becker (1991) views the production and rearing of children as the main purpose of marriages and families. Children are a long-term marital-specific investment and make specialisation even more beneficial. The value of children is not fully realised if the marriage breaks up and children therefore make the value of marriage higher and thus make divorce more costly. Economic theory furthermore suggests that the more stable their marriage is, the more likely a couple is to invest in children and become parents (see, e.g., Becker, 1973; Becker et al., 1977; Weiss, 1997). Although a number of studies have found a positive correlation between children and marital stability, a recent analysis by Svarer and Verner (2008) shows that when correcting for couples' self-selection into parenthood, this relationship disappears. This suggests that the observed correlation is mainly due to happier couples having children, rather than children stabilising marriages.

In contrast to economic theory, there is an extensive literature within psychology and sociology on the ways in which children decrease marital satisfaction and increase divorce risk. Twenge et al. (2003) summarise the explanations given for this negative association, which can be grouped into four theoretical models: (1) the role conflict model, (2) the restriction of freedom model, (3) the sexual dissatisfaction model, and (4) the financial cost model. According to the role conflict model, parenthood leads to a reorganisation of social roles along more traditional lines. This can cause stress and conflicts when the parents do not prefer traditional roles, for example when the woman does not want to give up her career. The restriction of freedom model emphasises the fact that children require time and attention, and that childcare responsibilities must interfere with and compete with the parents' pursuit of their own pleasures.[4] In the sexual dissatisfaction model marital problems stem from the fact that the presence of children decreases the parents' opportunities for sexual intimacy, and in the financial cost model, children bring about marital conflicts through the stress they put on family finances. Empirically, Twenge et al. (2003) find that the restriction of freedom model and the role conflict model are the most important in explaining the destabilising effect of children. The negative effect of children on marriages is more severe among high socioeconomic groups, younger birth cohorts, and in more recent years.

Introducing father's quota to a parental leave system increases domestic equality. Arnalds et al. (2013) use time use surveys to compare parents who had children before the policy reform in Iceland, to parents who had children after the reform, and find that children born after the change received considerable more care from their fathers. Looking at a policy reform in Canada, Patnaik (2019) also finds that paternity leave had a large and persistent effect on domestic equality, as exposed fathers contribute more to home production, and exposed mothers spend more time at the workplace. In a Beckerian world, this would decrease the value of marriage, because specialisation would be reduced. According to the flexibility model, however, the value of a marriage increases when the parents share their responsibilities more equally. Moreover, in the psychology models, in which children increase divorce risk, a fathers' quota is predicted to increase marital satisfaction and stability, and a policy that encourages fathers to participate in childcare may be of particular importance in the role conflict model.

[4]The authors note that this is similar to the role conflict model in many ways and that empirically it can be difficult to separate the two mechanisms.

2 Institutional Setup

2.1 The Parental Leave Scheme

In the year 2000, the Icelandic Act on Parental Leave underwent significant changes. A paternity quota was introduced to the country's paid parental leave beginning January 1, 2001. One month of the seven total months of paid parental leave was reserved exclusively for the father. This month was not transferable, so if it was not taken by the father the couple would lose it. Importantly, the right to parental leave in Iceland does not depend on the marital/cohabitation status, and a non-custodial parent has a right to maternity/paternity leave if the custodial parent consents (see Act on Maternity/Paternity Leave and Parental Leave No. 95/2000, Article 8), whereas even a sole custodian may not use the leave earmarked for the other parent.[5]

The new law makes it clear that gender equality was given serious consideration (Act on Maternity/Paternity Leave and Parental Leave, No. 95/2000) in its formulation. The law's stated main goals are (1) to ensure that children get to spend time with both parents and (2) to enable men and women to balance work and family life. Furthermore, even though this was not explicitly said to be a main goal, the law also mentions that the division of childcare between the parents is a prerequisite for their equality in the labour market.

The new paternity leave accompanied an increase in the total amount of parental leave from six to nine months. Iceland thereby gave men the largest non-transferable share of parental leave (three months out of nine) in the world. Parents who were active in the labour market were paid 80% of their average salaries while on leave.

Prior to this reform, there was a six month long paid parental leave. The first month of the leave could only be used by the mothers, while in theory the remaining five months could be divided between the parents as they preferred. Importantly though, fathers did not have a separate or independent right to paternity leave, and in practice, only a

[5]The only case in which the earmarked leave can be used by the other parent is when one of the parents dies before the child reaches the age of 18 months

negligible percentage of parents used their right to share the parental leave under the old law.

The case of Iceland is quite unique even among the Nordic countries. First, although paid parental leave has a long history in the other Nordic countries, such laws were enacted much later in Iceland.[6] Furthermore, as can be seen from Figures 1 and 2, the trend in Iceland when it comes to parental leave has also deviated quite strongly from the other Nordic countries in recent years. For a long time, men took almost none of the parental leave (their share was 0.1% in 1995), but after the reform in 2001 the growth in men's share of the total leave time has been quite steep. In 2000 their share was still fairly low (3.3%), but in 2001, after men received the non-transferable right to a one-month-long paternity leave, the percentage of total leave days used by fathers reached 11.5%. In 2002, men had the right to a two-month-long paternity leave, and their leave accounted for 19.6% of all parental-leave days used. In 2005, three years after men received the non-transferable right to a three-month-long paternity leave, this number had reached 32.7%. Since 2002, Icelandic men have used the largest share of total parental leave among men in the Nordic countries.

2.2 Households

In our analysis we do not differentiate between married and cohabiting couples, and a divorce is defined as the separation of parents, that were either married or cohabiting. There is little difference between cohabiting and married couples in the Nordic countries socially, culturally, or legally, and cohabitation is very common in all of them, including Iceland. At the time of the reform, 41% of individuals aged 25-40 were married, while 21% were cohabiting.

According to the OECD (2012), Iceland has the highest share of children born out of wedlock among the OECD countries, around 64%.[7] This is not due to a high rate

[6] the first parental leave laws were enacted in 1901 in Denmark, in 1917 in Finland, in 1892 in Norway, and in 1900 in Sweden, but not until 1946 in Iceland.

[7] Out-of-wedlock births are defined as those in which the parents are neither married nor living in a

of teenage pregnancies, as the same report notes that Iceland falls in the middle of the ranking distribution of OECD countries for this. Numbers from Statistics Iceland show that cohabitation is common among people who have children. Between 2001 and 2006, 57% of firstborn children and 50% of second children were born to cohabiting parents, while the numbers born to married parents were 19% and 39%, respectively.

Fertility rates in Iceland are also high relative to other developed countries, as can be seen in Figure 3, and divorce and union dissolution are common.[8] In 2001, 32.7% of divorces and terminations of cohabitation occurred among couples without children. Furthermore, most children are under the age of seven at the time of their parents' divorce or termination of cohabitation.

3 Data

We use a rich register-based panel dataset comprising the population of Icelandic couples who had children in 2000 and 2001. For our sample we compiled data of income and demographic characteristics from Statistics Iceland into a panel covering the parent in our sample over a period of 27 years (1990-2016). The demographic data includes information on age, gender, marital status, education, dummies for whether the individual lives in the capital city or other urban areas, the number of children the individual has, and spouse identifiers. The education variable specifies the highest level of education the individual has completed, whether this is compulsory education, high school, or university. Income is reported by individual source and is divided into three categories: income from employment, capital income, and other income.

Our data are taken from the Icelandic Longitudinal Income Database (ICELID), maintained by Statistics Iceland, which has gathered it from different sources, mainly administrative registers. Icelandic residents are taxed on their income, and Statistics Iceland

comparable legal partnership during the year in which the birth occurs.

[8] According to Statistics Iceland, the incidence of divorces among married individuals in 2000, just before the reform, was 40%. In 2011, the incidence was 34%.

therefore has a parliamentary mandate to collect extensive information on the finances of every individual in the country. Because the data are collected by a single, central agency, and are used for tax purposes, we believe that our data set is of a very high quality. Furthermore, because the data are register-based and cover a large and representative sample of the population, results drawn from them will not be influenced by self-selection biases.

Table 1 provides summary statistics on the variables we use in this study. We show separate statistics for men and women in the treatment group, defined as couples who had children in the twelve weeks after the policy reform (columns (i) and (ii)), and the control group, defined as couples who had children in the last twelve weeks before the new parental leave system took effect (columns (iii) and (iv)).

4 Empirical Framework

To estimate the intention-to-treat effect of the parental leave reform on marital stability we apply a regression discontinuity (RD) design. Access to paternity leave depends on the child's date of birth. No couple who had a child in the period before the policy reform had access to the new parental leave system, and all parents who had a child after 1st of January 2001 had access. We therefore compare parents who had children just before, and just after, the policy reform. Our outcome variable is marital status for couple i at time t, and is denoted D_{it}. The outcome variable takes value one if the couple has separated, and zero if they are still married or cohabiting. We let $T = 1$ for those individuals who had a child after the policy reform became effective and $T = 0$ for those who had children before the policy reform. Our assignment variable is child's week of birth.

Having access to paternity leave is therefore a deterministic and discontinuous function of the week of birth, w_i, and we therefore use a sharp RD approach to estimate the impact of the daddy quota. The assignment variable is centred at zero for 1^{st} of January 2001, which yields the following:

$$T_i = \begin{cases} 0 & \text{if} \quad w_i < 0 \\ \\ 1 & \text{if} \quad 0 \leq w_i \end{cases}$$

The key assumption behind our analysis is that the relationship between w_i and marital stability is smooth around the threshold so that any discontinuity at the threshold can safely we interpreted as the causal effect of the parental leave reform. The idea behind the RD design is that by comparing observations that are sufficiently close to the threshold the discontinuity sample will be a close approximation to a randomised trial and therefore it should be unnecessary to include a covariates or trends in the estimation. However, because there are relatively few observations in a local neighbourhood of the assignment threshold in our data, we use control function approach as the preferred method in our RD analysis. We estimate a model of the form:

$$D_i = \alpha + \beta X_i + \tau T_i + [1 - T_i] f_l \left(w_i\right) + T_i f_r \left(w_i\right) + \epsilon_i, \tag{1}$$

where effect of week of birth is captured by the function $f\left(w_i\right)$, i.e. it is supposed to be an adequate description of $E[D_{0itc}|w_i]$. If the correct specification of the control function, $f(w_i)$, is used, i.e. the true conditional mean function $E[\omega_i|w_i]$, it will capture all dependence between T_i and ϵ_i so that the conditional mean independence assumption will hold, i.e.

$$E[\epsilon_i|T_i] = 0.$$

This procedure will therefore render the OLS estimates consistent and even unbiased in the case of local linear control functions (see Hahn et al., 2001) or correctly specified control functions (see Porter, 2003), i.e. the causal effect of the treatment variable, T, on the outcome variable, D, will be captured by β. However, this regression-based estimation approach requires a specification of the functional form $f(\cdot)$ and a misspecified control function is likely to produce inconsistent estimates. By including the interaction term between the control function and the treatment dummy, we allow the slope coefficients to differ on each side of the threshold. We furthermore check whether our estimates are robust to allowing different functional forms of the control function.

There is a trade-off between having groups that are as similar as possible (obtained by reducing the time window around the reform) and having a larger sample size (by widening the window). We therefore also report our findings for several time windows, and by using the mean square error (MSE) and coverage error rate (CER) optimal bandwidth estimators suggested by Calonico et al. (2017).

Finally, to confront the inference problem arising in the case of childbirth-period specific random effects we cluster standard errors by the child's calendar week of birth when looking at larger bandwidths.

4.1 Threats to Identification

One threat to the identification of causal effects is endogenous sorting: parents may have planned the time of birth in anticipation of the policy. Although parents may have had an incentive to affect the date of birth, children born close to the treatment determining threshold were already in utero when the law was passed on May 9^{th} 2000 so parents who had children close to the threshold, January 1^{st} 2001, did not know about the reform at the time of the conception. Furthermore, it is evident from news coverage that the new law did not reach widespread public awareness until the late fall of 2000, and there appears to have been a substantial level of uncertainty with regards to the implementation of the reform. This adds further support to our choice of treatment group because it implies that it is unlikely that the policy affected timing of births until late spring or summer of 2001.

A related concern is whether couples expecting a child around the threshold date would still be able to manipulate the date of birth. In general, postponing births is more difficult than advancing the time of birth. However, we cannot rule out the possibility that the parental leave reform impacted the date of birth for scheduled inductions and c-sections. To check whether our results are robust to parents possibly manipulating the day of birth around the threshold, we also show estimates where we exclude births in the two weeks before, and two weeks after 1^{st} of January 2001.

Another concern, is that even without the parental leave reform, parents who have their children at the end of the year are different from parents who have their children early in the year. In their study, Buckles and Hungerman (2013), find that maternal characteristics vary by the month of birth. Mothers who give birth during the winter months are younger, less likely to be married, and less educated, than mothers who have their children at other time or the year. We take several measures to address this.

First, in order to know whether the parents in our control group, and our treatment group resemble each other on potentially confounding variables, we provide graphical evidence of covariate balance in Figure A.1 in the Appendix where we look at earnings and education of mothers and fathers who had children around the reform, one year prior to birth, as well as the number of older siblings and the probability of the parents having been together 5 years before birth. The covariate balancing test yields important information regarding the local randomisation assumption needed for identification of β. Specifically, if covariates are unbalanced at the cutoff, this provide evidence that parents are systematically able to manipulate the timing of birth of their children. We do not see any evidence of covariates being unbalanced around the cutoff, allowing us to conclude that the sample selection appears successful in minimising confounding factors.

Second, since the groups may still differ on unobservable characteristics we use data from the previous year to look at a placebo treatment effect, i.e. we estimate equation (1), using births around 1^{st} of January 2000, i.e. when no parental leave reform took place.

Finally, we estimate a regression discontinuity difference-in-differences (RD-DD) model using the placebo group, in addition to our main sample. This approach yields a valid estimate if without the treatment the differences between the couples that have children late in the year, and the couples that have children early in the year would follow the same pattern for main sample and the placebo group. We estimate a model of the form:

$$\begin{aligned} D_i = \alpha + \beta X_i + \sum_{n=0}^{1} \mathbb{1}[R_i = n] \\ \times\{\delta R_i + \tau_n T_i + [1 - T_i] f_{l,n}(w_i) + T_i f_{r,n}(w_i)\} + \epsilon_i, \end{aligned} \quad (2)$$

where R is a reform indicator that takes value one if the birth is around the policy reform, i.e. 1^{st} of January 2001, and zero otherwise. T_i is defined as before, except that now it also treats 1^{st} of January 2000 as a reform threshold, i.e, T_i takes the value one if the child is born in the months after 1^{st} of January (either in 2000 or 2001) and zero otherwise.

A final potential concern is that other policies are also related to the same date cutoffs. To the best of our knowledge, however, there were no other policy reforms taking place around the same time that could be confounding in our analysis..

5 Results

The first change in the parental leave system took place on 1^{st} of January 2001, when the total leave was extended from six months to seven, with one of the seven months earmarked to the father. The following section details our results, using a sample of 600 families that had children in the three months before and three months after this cutoff date.

5.1 Graphical Illustrations

The top panel of Figure 5 shows the evolution of the cumulative divorce hazard in couples that had a child within a three-month time window around the implementation of the parental leave reform in 2001 and those that had a child within the same window the previous year. The couples that had a child after 1^{st} of January 2001, were entitled to paternity leave, while those who had a child before were not. There are some noteworthy patterns. The graph suggests that paternity leave is associated with a reduction in the number of divorces immediately after the child is born. Figure 5 also indicates that the drop in divorces is not just transitory, but rather appears to be a permanent one, as the difference in the proportion of couples divorced remains throughout the fifteen-year period that we follow them.

When we compare (a) those who were entitled to a paternity leave to those who had a child in the same period the year before, and (b) those who had a child just before the reform to those who had a child in the same period the year before, we find added support for our conclusion that the paternity leave reduced the number of divorces among people who were entitled to it. Looking at the share of couples that divorced within ten years after their child was born, we see that there are fewer divorces among those entitled to paternity leave than among those who had a child in the same period the year before. In the bottom panel of Figure 5 we compare parents who had a child in the same period the year before, and find no such difference.

In Figure 6 we look at the dynamics of divorce risk, by the age of their child. We can see that for both the control group and the treatment group the divorce risk is highest when the child is small. This is furthermore the period where we see the largest differences between the two groups. The evidence from Figures 5 and 6 therefore suggests that the parental leave reform decreased divorce risk, in particular in the first years after the child was born, and that the impact has a persistent effect on the average family structure in the two groups.

Figure 7 provides a nice visualisation of the RD-DD identification approach and provides further graphical evidence on the relationship between entitlement to paternity leave and marital stability. The left hand side column shows the binned averages of separations by children's month of birth for children who were born in the months around the reform, 5, 10, and 15 years after the reform. The right hand side column shows the same for the placebo reform one year earlier. These figures show that for the reform sample there is a discontinuous drop in the separation rate at the reform threshold and that this drop is persistent, i.e, there is still a discrete jump 15 years after the reform. However, when we look at the placebo sample there is no such jump.[9]

5.2 The Effect on Marital Stability

In Table 2, we present the estimated effect of the introduction of the paternity leave on marital stability. More specifically, the table reports the effect of being entitled to paternity leave on the separation probability of couples whose children were born just after and just before the law changed. We start by comparing observations close to the threshold (columns (i)-(iv)). While these estimates are imprecise they suggest that there is a persistent negative effect on divorce probabilities among parents who had children after the policy reform. In columns (v)-(x) we extend the window around the threshold and use local linear methods to control for a possible underlying relationship between the dependent variable and the week of birth. We see large significant effect on cumulative divorces. Our results imply that if the couples who had children just before the reform would also have had the opportunity for one month of paternity leave, the number of divorces in the next fifteen years would have been significantly lower. To be more specific, five years after the birth of the child, the effect is large and significant, showing that if the control group had also been entitled to the same paternity leave, the divorce rate in that group would have been 6.5-8.2 percentage points lower. The long term estimates are less precise, but suggest that the drop in divorce rates are persistent.

To address the concern that our findings are due to seasonality in the data, or due to unobserved difference between parents who have children at the beginning and the end of the year, we repeat the regressions we carried out in Table 3, as if the reform had taken place exactly one year earlier. The estimates in columns (i)-(iv) suggest that

[9] The figures here are based on a linear specification but are not much affected by using other specifications. Figures A.2-A.4 in the Appendix show results based on quadratic, cubic, and local linear trend specifications.

it is important to control for dependance between the divorce rates and week of birth, even when on looks at observations close to the threshold. The estimates in columns (v)-(x), where we control for local linear trends on each side of the threshold, are negligible and not significant under any specification. This implies that our estimated effect of the introduction of a father's quota in columns (v)-(x), Table 2, can plausibly be interpreted as causal relationships.

To further check the robustness of our findings we estimate the treatment effect, employing a local polynomial regression discontinuity approach where we use the MSE and CER-optimal bandwidth estimators suggested by Calonico et al. (2017) and employing a triangular kernel which has good properties when applying a RD approach due to being boundary optimal (Cheng et al., 1997). The results are reported in Table 4. As in Table 2, we see a large negative effect on divorce probabilities among the parents who had their child after the policy reform.[10]

Finally, we estimate a RD-DD model, described in Equation 2 (see Table 5) and find treatment effect very close to those estimated in Table 2.[11] More specifically, we find large and significant effect on divorces in the first five years after the child was born, and evidence that there is a persistent long-run effect as well. In Table 6, we estimate Equation 2, excluding births close to the threshold (two weeks before, and two weeks after), to alleviate the concern that there is a selection into treatment among those who had due dates close to 1^{st} of January. This does not impact our findings.

[10] Discrepancies between the results in columns (ix) and (x) in Table 2 and columns (i) and (ii) in Table 4 where the bandwidth and time horizon are the same can be explained by the fact that the we employ a uniform kernel in Table 2 while we employ a triangular kernel (giving more weight to observations close to the threshold) in Table 4. For comparability we therefore also report regression results for local polynomial regressions where we employ a uniform kernel in Appendix Table 1. These results show that the results are in line with the results based on the local linear RD approach.

[11] We use windows of 10, 15, and 20 weeks around the reform since the estimates in Table 4 suggest that the optimal bandwidth lies approximately within this range.

5.3 Heterogeneity in Treatment Effects

In columns (i)-(iv) in Table 7 we look at whether the treatment effect is driven by less experienced parents, i.e. parents who just had their first child, or those who have older children. We find that the paternity leave causes a reduction in divorce rates among both groups, and that in the short run the largest impact is on the more experienced parents. However, we find that the long term impact is indeed driven by parents who had their first child around the policy change.

Our hypothesis is that the paternity leave affected marital stability as it increased domestic equality, and therefore alleviated the shock on the division of labour within households that takes place when couples have children. One implication of our hypothesis is that the positive effect of the paternity leave on marital stability should be strongest among parents who aim for equal division of labour at home, rather than those who prefer to specialise. We have no information on parents' preferences but in columns (v)-(x) in Table 7 we separate parents by their relative education, i.e. we look separately at parents who have the same level of education (columns (v) and (vi)), parents where he mother has higher level of education (columns (vii) and (viii)), and parents where the father has higher level of education (columns (ix) and (x)). The idea is that education is a rough measure of labour market specialisation. For parents who have the same level of education, the effect is strong and significant, both in the short run, and in the long run. For this group, having access to paternity leave reduces divorce risk by ten percentage points. Among parents with the same level of education, 37% are separated fifteen years after the birth of their child. According to the point estimates, the daddy month could have reduced the ratio to 27%. The estimated treatment effect for couples where the mother has higher education are also large and significant, when we look at divorce rates five years after the birth of the child. In contrast, the results for the sample where fathers have higher level of education than the mothers suggest that there is no effect in the short run and even an increase in divorce probability in the long run.

6 Conclusions

Parental policy has been under debate in recent years. With the growing number of dual-earner families around the world, there has been an increased demand for a universal paid parental leave. There is little agreement, though, on the optimal system in terms of length, form, or payments. While longer maternity leaves have been found to increase women's labour market participation, they have also been found to have a negative effect on women's earnings (Ruhm, 1998), and still other papers suggest that this only holds in the short run (Lalive and Zweimüller, 2009; Lalive et al., 2014). In response to this evidence, and in an attempt to narrow the gender gap in the labour market, several countries have earmarked part of their parental leave for fathers. Families have to forgo this parental leave if it is not used by the father, which creates strong economic incentives for fathers to take part in caring for their children during their first months. However, few attempts have been made so far to evaluate how well these policies work. Furthermore, the main emphasis in evaluations of parental leave policies has so far been on labour market outcomes, e.g., wages and labour market participation. Our results show that the impact of such policies is not restricted to labour market outcomes.

It is well established that the presence of young children is a risk factor for marital dissolution, and a number of studies have found that having children significantly decreases marital happiness, and increases divorce risk (Svarer and Verner, 2008; Lawrence et al., 2007, 2008). We find that the addition of one month of paternity leave to an existing six-month parental leave in Iceland significantly decreased divorce rates among parents of young children. Parents who had their children right after the policy was implemented were considerably less likely to divorce than parents who had their child just before the paternity leave was introduced. The effect is sizeable and indicates that if the control group had been subject to the new parental leave policy, their rate of separations within the following five years would have been reduced by around thirty percent. Even fifteen years after the birth of their children, there is still a substantial difference between couples in the two groups, indicating that around twenty percent of the separations in the control group would have been avoided had their children been born after the reform took effect. These results suggest that engaging fathers in childcare has a substantial long-term effect on marital stability.

In societies where women are becoming more career oriented, a parental leave system, that encourages both parents to participate, may decrease the divorce risk by easing couples' transition into parenthood. Women have significantly increased their education levels and labour market participation in recent decades, but have at the same time remained the main caretakers in the household. Furthermore, when children are born, couples often reorganise their social roles towards more traditional family patterns according to which men are the breadwinners and women take care of the home and children. This shift in household responsibilities is likely to be more dramatic and cause more stress and conflicts among couples where specialisation is not expected. This hypothesis finds support in our results, where we find that the effect of the paternity leave is especially strong among parents where the mother's education is equal to, or greater education than, the father's. In contrast, the effect on couples, where the father has higher level of education than the mother, is negligible, or even positive.

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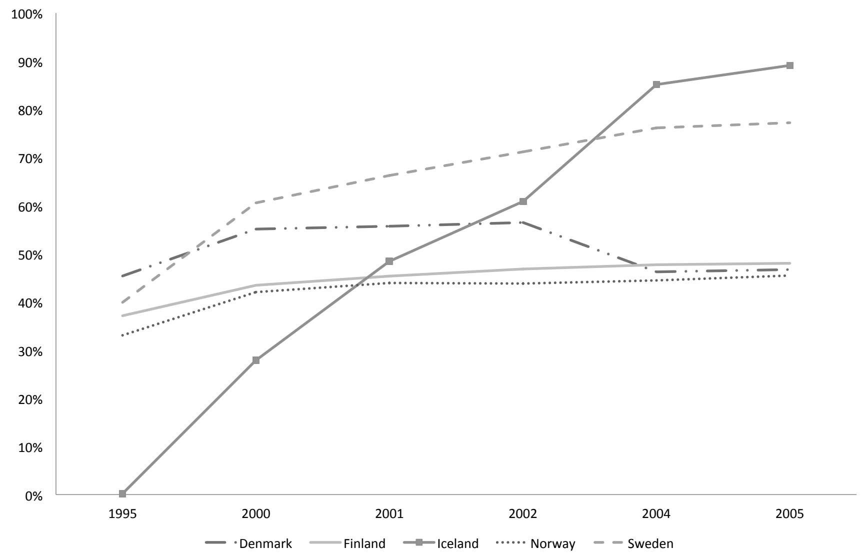


Figure 1: Share of men relative to women receiving parental leave in the Nordic countries

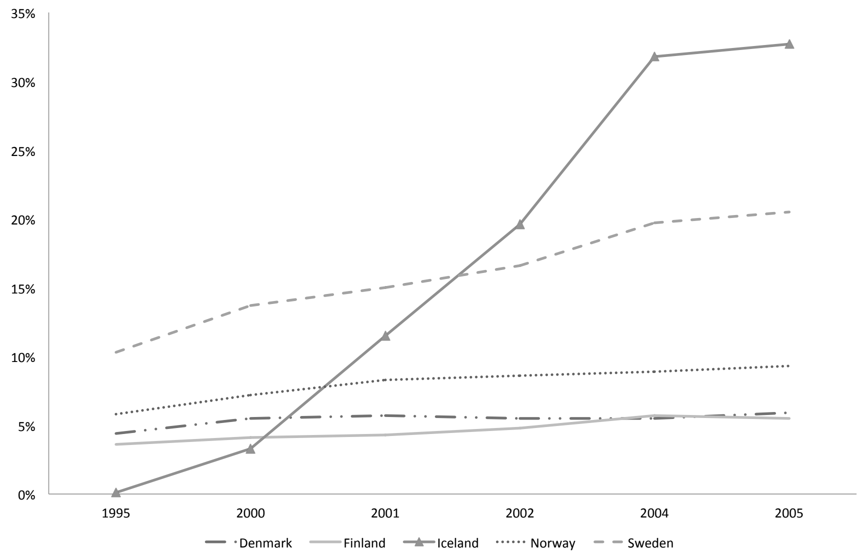


Figure 2: Share of parental leave days used by men in the Nordic countries

Figure 3: Fertility in Iceland 1990-2010

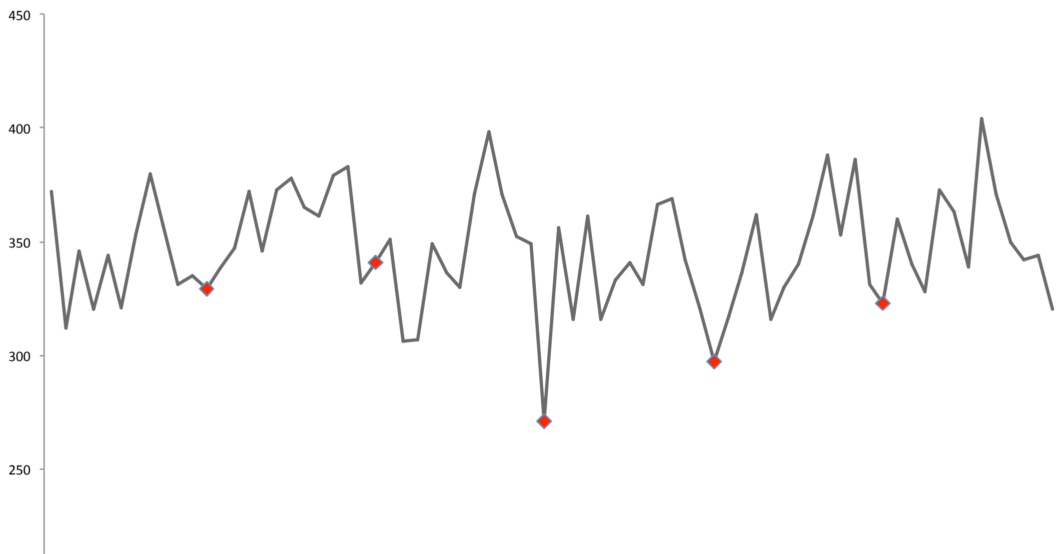


Figure 4: Monthly fertility in Iceland 1999-2004

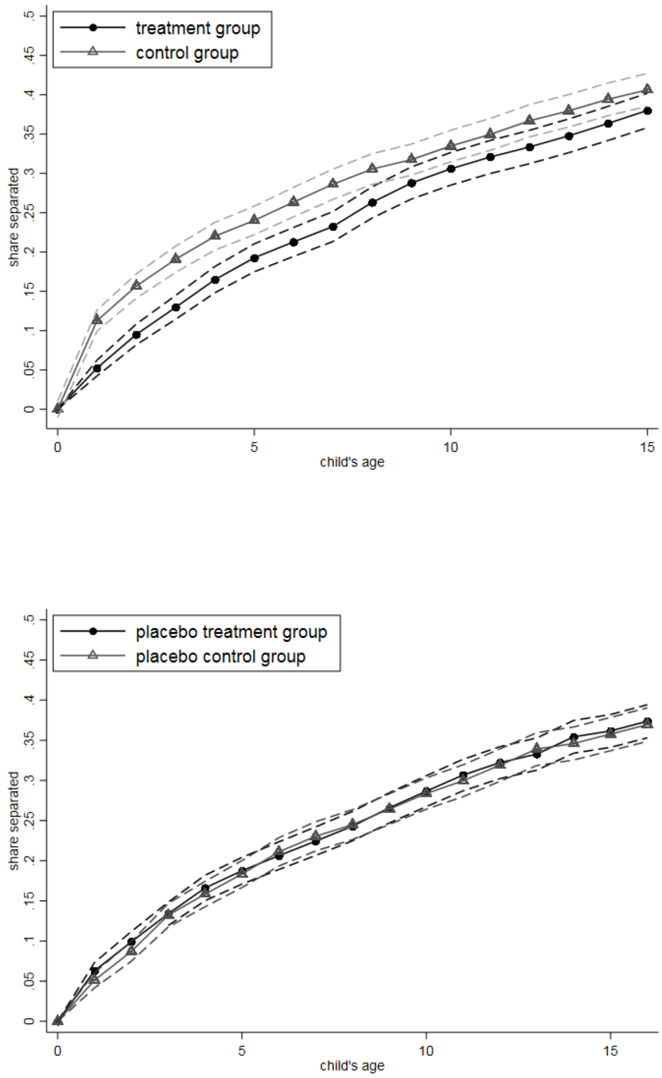


Figure 5: Comparision of cumulative divorce hazard (with 95% CI) for the treatment and control group (top panel), and for the placebo treatment and placebo control groups (bottom panel).

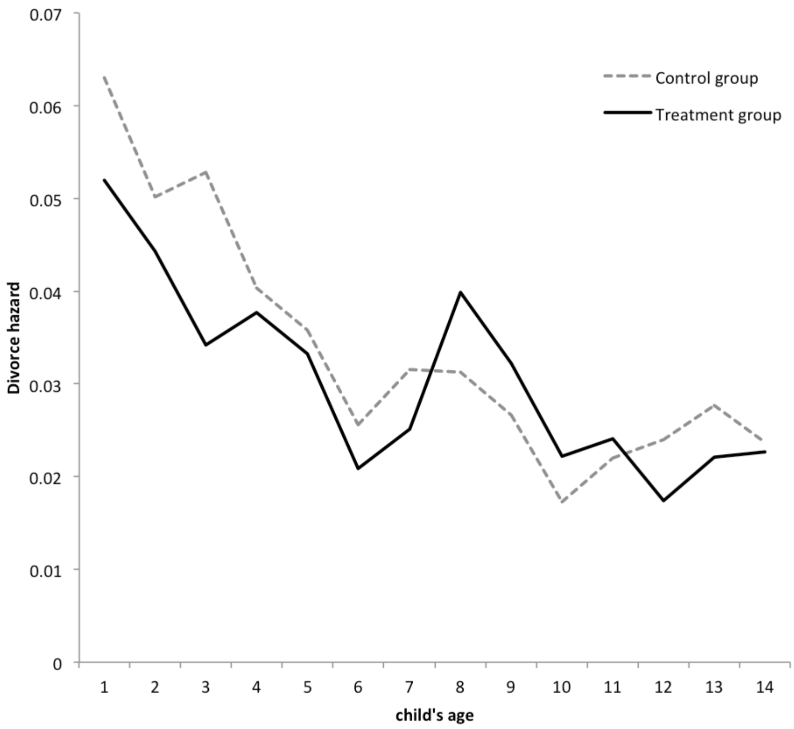
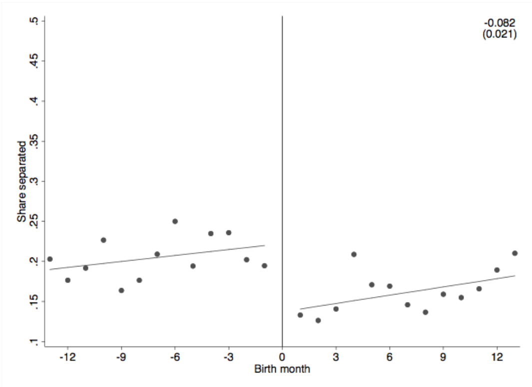
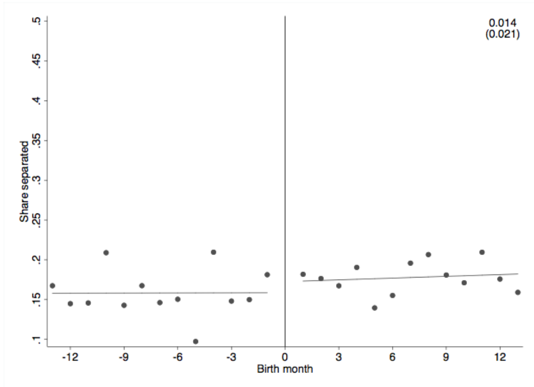


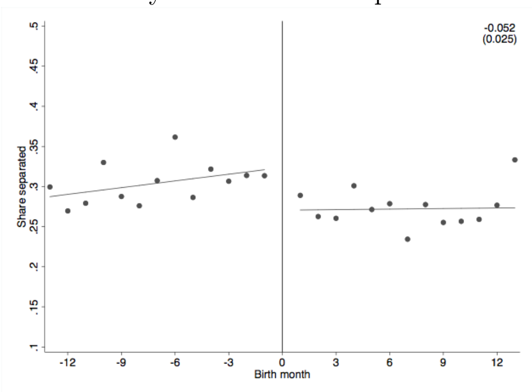
Figure 6: Comparing divorce risk for the treatment and control group, by the age of the child



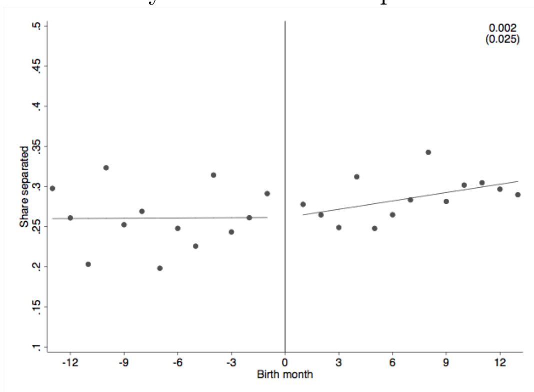
5 years - Reform sample



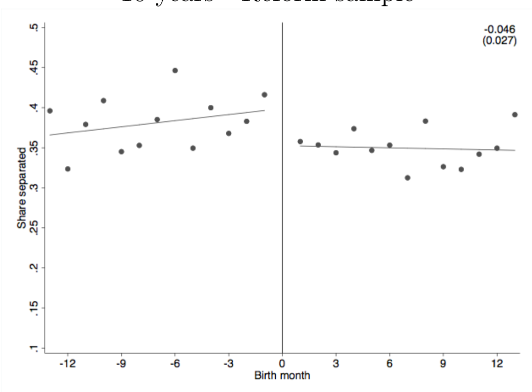
5 years - Placebo sample



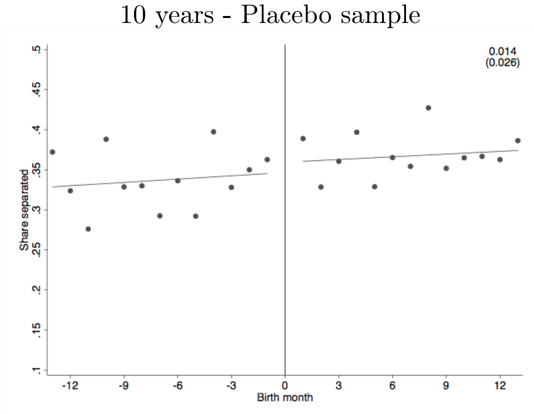
10 years - Reform sample



10 years - Placebo sample



15 years - Reform sample



15 years - Placebo sample

Figure 7: Divorce risk around the treatment threshold and around the placebo treatment threshold - Linear fit

The treatment group is to the left of the threshold in the first column and the control group on the right. The placebo treatment group is on the left of the placebo treatment threshold and the placebo control group on the right.

Table 1: Summary Statistics for Treatment and Control Group

	Treatment group		Control group		Difference	
	Men	Women	Men	Women	Men	Women
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Birth year	1968.8	1971.2	1968.4	1970.9	0.4	0.3
Elementary school	0.33	0.36	0.33	0.32	0.00	0.04*
High school	0.44	0.38	0.39	0.40	0.04*	-0.02
University	0.21	0.25	0.27	0.27	-0.05**	-0.02
First child dummy	0.37	0.39	0.35	0.39	0.02	0.00
#of older children	1.02	0.96	1.07	0.94	-0.05	0.00
Earnings 1999	2,631,189	1,169,693	2,645,733	1,207,217	-14,544	-37,523
Capital income 1999	94,191	74,490	123,416	86,352	-29,225	-11,862
Couple's earnings gap 1999	1,439,858	1,439,858	1,366,746	1,366,746	73,112	181,696
Married Couples	0.50		0.47		0.03	
Cohabiting Couples	0.50		0.53		-0.03	
Separated in 2002	0.05		0.11		-0.06***	
Separated in 2003	0.09		0.16		-0.08***	
Separated in 2004	0.11		0.18		-0.08***	
Separated in 2005	0.13		0.21		-0.08***	
Separated in 2006	0.17		0.23		-0.06***	
Separated in 2007	0.19		0.25		-0.06***	
Separated in 2008	0.22		0.28		-0.07***	
Separated in 2009	0.25		0.30		-0.06**	
Separated in 2010	0.27		0.31		-0.04*	
Separated in 2011	0.29		0.33		-0.04*	
Separated in 2012	0.31		0.34		-0.03	
Separated in 2013	0.32		0.36		-0.04*	
Separated in 2014	0.33		0.37		-0.03	
Separated in 2015	0.35		0.39		-0.04*	
Separated in 2016	0.37		0.40		-0.03	
#observations	585		608			

Note: * p<0.1 ** p<0.05 *** p<0.01. Treatment group refers to those couples that had children in the 12 weeks after the policy change, and the control group includes couples that had children in the 12 weeks before the policy change.

Table 2: The effect of paternity leave on parental separations: regression discontinuity estimates

	Discontinuity at threshold				Local linear method					
window	+/-2 weeks		+/- 1 week		+/- 52 weeks		+/-24 weeks		+/-12 weeks	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)
5 years after birth	-0.049	-0.026	-0.102	-0.076	-0.082***	-0.079***	-0.070**	-0.065**	-0.069*	-0.071
	(0.053)	(0.053)	(0.083)	(0.086)	(0.029)	(0.028)	(0.038)	(0.039)	(0.052)	(0.054)
10 years after birth	-0.053	-0.059	-0.088	-0.095	-0.052*	-0.052*	-0.023	-0.021	-0.061	-0.065
	(0.064)	(0.066)	(0.098)	(0.109)	(0.029)	(0.028)	(0.038)	(0.039)	(0.052)	(0.054)
15 years after birth	-0.083	-0.079	-0.093	-0.058	-0.046*	-0.041	-0.027	-0.015	-0.104**	-0.101**
	(0.067)	(0.069)	(0.101)	(0.110)	(0.027)	(0.027)	(0.039)	(0.039)	(0.041)	(0.043)
Number of observations	209	207	100	100	5,425	5,327	2,459	2,413	1,193	1,177
Controls	no	yes	no	yes	no	yes	no	yes	no	yes

Each entry is a separate regression and presents the estimated discontinuity in the probability that a couple is separated, five, ten, and fifteen years after the birth of their child. Controls include mothers' and fathers' year of birth, education, parents' income, number of older children in the household, and an urban dummy. Specifications (v)-(x) control for local linear trends and standard errors are clustered by week of birth. * p<0.1 ** p<0.05 *** p<0.01

Table 3: Placebo analysis: regression discontinuity estimates for the previous year

	Discontinuity at threshold				Local linear method					
window	+/-2 weeks		+/- 1 week		+/- 52 weeks		+/-24 weeks		+/-12 weeks	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)
5 years after birth	-0.027	-0.009	-0.034	-0.053	0.005	-0.002	0.009	-0.001	-0.015	-0.003
	(0.054)	(0.056)	(0.067)	(0.074)	(0.021)	(0.022)	(0.028)	(0.029)	(0.038)	(0.042)
10 years after birth	-0.060	-0.052	-0.098	-0.114	-0.004	-0.008	-0.009	-0.017	-0.036	-0.019
	(0.064)	(0.067)	(0.084)	(0.089)	(0.021)	(0.022)	(0.028)	(0.029)	(0.038)	(0.042)
15 years after birth	-0.059	-0.021	-0.126	-0.0100	0.005	0.002	-0.001	-0.010	-0.054	-0.038
	(0.069)	(0.073)	(0.093)	(0.104)	(0.028)	(0.029)	(0.039)	(0.039)	(0.054)	(0.056)
Number of observations	198	191	111	109	5,441	5,421	2,532	2,529	1,206	1,206
Controls	no	yes	no	yes	no	yes	no	yes	no	yes

Each entry is a separate regression and presents the estimated discontinuity in the probability that a couple is separated, five, ten, and fifteen years after the birth of their child. Controls include mothers' and fathers' year of birth, education, parents' income, number of older children in the household, and an urban dummy. Specifications (v)-(x) control for local linear trends and standard errors are clustered by week of birth.
* p<0.1 ** p<0.05 *** p<0.01

Table 4: The effect of paternity leave on parental separations - local polynomial regression discontinuity approach using optimal bandwidths

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
5 years after birth	-0.037	-0.036	-0.018	-0.049	-0.036	- 0.095**
	(0.040)	(0.043)	(0.053)	(0.050)	(0.057)	(0.046)
Bandwidth	12	9	15	11	17	13
Effective obs.	1,193	906	1,509	1,088	1,730	1,287
10 years after birth	-0.044	-0.028	-0.026	-0.010	-0.010	-0.011
	(0.049)	(0.055)	(0.061)	(0.067)	(0.071)	(0.074)
Bandwidth	15	12	19	15	23	18
Effective obs.	1,509	1,193	1,928	1,509	2,356	1,844
15 years after birth	-0.087**	-0.076*	-0.089**	-0.077*	-0.059	-0.048
	(0.037)	(0.039)	(0.041)	(0.044)	(0.046)	(0.056)
Bandwidth	14	11	21	16	20	15
Effective obs.	1,396	1,088	2,135	1,629	2,035	1,509
Polynomial order	First	First	Second	Second	Third	Third
Optimal bandwidth estimator	MSE	CER	MSE	CER	MSE	CER

Each entry is a separate regression and presents the estimated discontinuity in the probability that a couple is separated, five, ten, and fifteen years after the birth of their child employing a triangular kernel. The full sample includes births in the 52 weeks before and after the policy reform, that is, 5,425 observations. The estimates employ a triangular kernel, and use an MSE-optimal and a CER-optimal bandwidth estimators suggested by Calonico et al. (2017) . Standard errors are clustered by week of birth. * p<0.1 ** p<0.05 *** p<0.01

Table 5: The effect of paternity leave on parental separations - regression discontinuity difference-in-differences estimates

window	+/-20 weeks		+/-15 weeks		+/-10 weeks	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
5 years after birth	-0.090**	-0.089**	-0.106**	-0.116**	-0.021	-0.045
	(0.043)	(0.041)	(0.051)	(0.049)	(0.061)	(0.063)
10 years after birth	-0.028	-0.019	-0.047	-0.053	-0.004	-0.023
	(0.043)	(0.042)	(0.051)	(0.049)	(.069)	(0.052)
15 years after birth	-0.061	-0.043	-0.094*	-0.089*	-0.094	-0.117*
	(0.044)	(0.042)	(0.052)	(0.047)	(0.071)	(0.065)
Number of observations	4,213	4,144	3,120	3.073	2,029	2,001
Controls	no	yes	no	yes	no	yes

Each entry is a separate regression and presents the estimated discontinuity in the probability that a couple is separated, five, ten, and fifteen years after the birth of their child. Controls include mothers' and fathers' year of birth, education, parents' income, number of older children in the household, and an urban dummy. All specifications control for local linear trends and standard errors are clustered by week of birth. * p<0.1 ** p<0.05 *** p<0.01

Table 6: The effect of paternity leave on parental separations - regression discontinuity difference-in-differences estimates - excluding the two weeks before and after 1st of January

window	+/-20 weeks		+/-15 weeks		+/-10 weeks	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
5 years after birth	-0.113**	-0.116**	-0.139**	-0.159***	-0.063	-0.040
	(0.044)	(0.046)	(0.051)	(0.052)	(0.061)	(0.067)
10 years after birth	-0.024	-0.016	-0.049	-0.060	-0.045	-0.019
	(0.048)	(0.051)	(0.061)	(0.061)	(0.100)	(0.104)
15 years after birth	-0.046	-0.031	-0.085	-0.084	-0.076	-0.114
	(0.054)	(0.054)	(0.065)	(0.062)	(0.103)	(0.101)
Number of observations	4,004	3,937	2,911	2,866	1,820	1,794
Controls	no	yes	no	yes	no	yes

Each entry is a separate regression and presents the estimated discontinuity in the probability that a couple is separated, five, ten, and fifteen years after the birth of their child. Controls include mothers' and fathers' year of birth, education, parents' income, number of older children in the household, and an urban dummy. All specifications control for local linear trends and standard errors are clustered by week of birth.. * p<0.1 ** p<0.05 *** p<0.01

Table 7: The effect of paternity leave on parental separations - heterogeneity in RD-DD estimates

	First child dummy				Parents' education					
	= 1		= 0		Fathers = Mothers		Fathers < Mothers		Fathers > Mothers	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)
5 years after birth	-0.048	-0.063	-0.112***	-0.104**	-0.103	-0.094	-0.141*	-0.128*	-0.014	-0.026
	(.081)	(0.075)	(0.041)	(0.043)	(0.076)	(0.074)	(0.082)	(0.073)	(0.059)	(0.053)
10 years after birth	-0.020	-0.010	-0.032	-0.029	-0.054	-0.045	-0.116	-0.076	0.101	0.100
	(0.087)	(0.076)	(0.051)	(0.053)	(0.072)	(0.072)	(.114)	(0.109)	(0.078)	(0.073)
15 years after birth	-0.154**	-0.121*	0.002	0.007	-0.124	-0.113	-0.085	-0.033	0.081	0.087
	(0.072)	(0.065)	(0.059)	(0.063)	(0.085)	(0.082)	(0.102)	(0.097)	(0.096)	(0.096)
Number of observations	1,637	1,592	2,576	2,552	1,980	1,794	1,116	1,090	1,117	1,096
Controls	no	yes	no	yes	no	yes	no	yes	no	yes

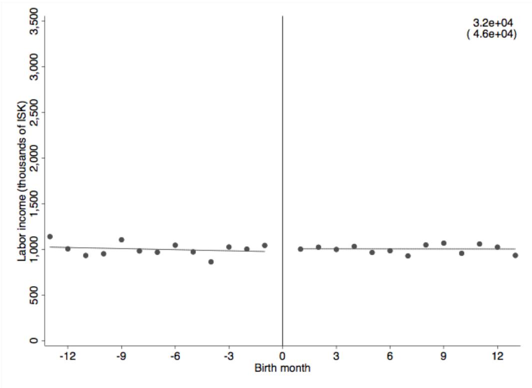
Each entry is a separate regression and presents the estimated treatment effect of paternity leave on the probability that a couple is separated, five, ten, and fifteen years after the birth of their child. All estimates are based on a sample that includes all births in the 20 weeks before, and 20 weeks after 1st of January 2000 and 1st of January 2001. Controls include mothers' and fathers' year of birth, education, parents' earnings, number of older children in the household, and an urban dummy. All specifications include calendar week of birth dummies. Standard errors are clustered by week of birth. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Appendix

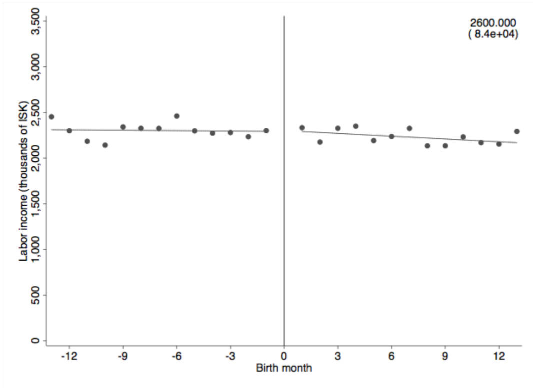
Table 1: The effect of paternity leave on parental separations - local polynomial regression discontinuity approach using optimal bandwidths (uniform kernel)

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
5 years after birth	-0.079**	-0.046	-0.063	-0.019	-0.018	- 0.105**
	(0.034)	(0.039)	(0.046)	(0.050)	(0.069)	(0.045)
Bandwidth	14	11	19	15	15	12
Effective obs.	1,396	1,088	1,928	1,509	1,509	1,193
10 years after birth	-0.063	-0.020	-0.052	-0.019	-0.010	-0.001
	(0.048)	(0.057)	(0.058)	(0.066)	(0.074)	(0.075)
Bandwidth	13	10	20	15	23	18
Effective obs.	1,287	996	2,034	1,509	2,356	1,844
15 years after birth	-0.100**	-0.081*	-0.105**	-0.051	-0.063	-0.075
	(0.040)	(0.044)	(0.049)	(0.056)	(0.051)	(0.051)
Bandwidth	12	10	16	12	23	18
Effective obs.	1,193	996	1,629	1,193	2,356	1,844
Polynomial order	First	First	Second	Second	Third	Third
Optimal bandwidth estimator	MSE	CER	MSE	CER	MSE	CER

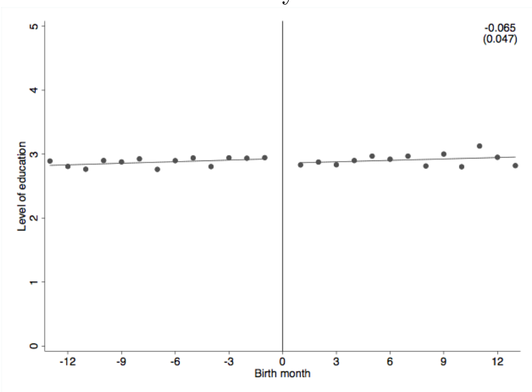
Each entry is a separate regression and presents the estimated discontinuity in the probability that a couple is separated, five, ten, and fifteen years after the birth of their child employing a uniform kernel. The full sample includes births in the 52 weeks before and after the policy reform, that is, 5,425 observations. The estimates employ a uniform kernel, and use an MSE-optimal and a CER-optimal bandwidth estimators suggested by Calonico et al. (2017) . Standard errors are clustered by week of birth. * p<0.1 ** p<0.05 *** p<0.01



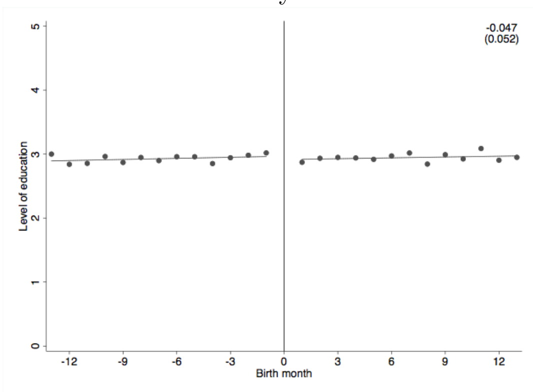
Mother's income 1 year before birth



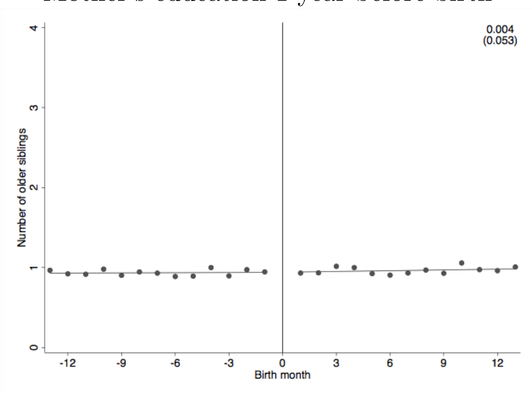
Father's income 1 year before birth



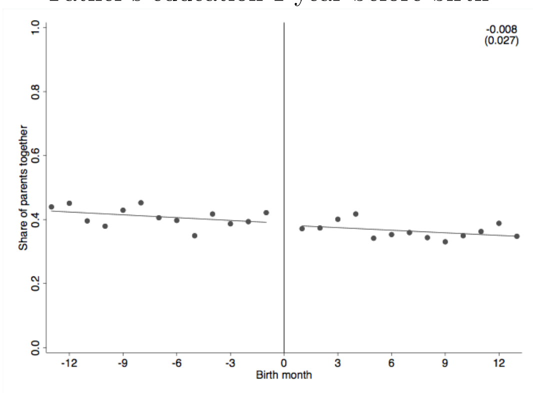
Mother's education 1 year before birth



Father's education 1 year before birth

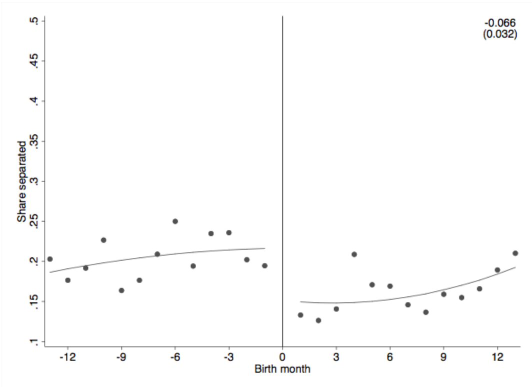


Number of older siblings

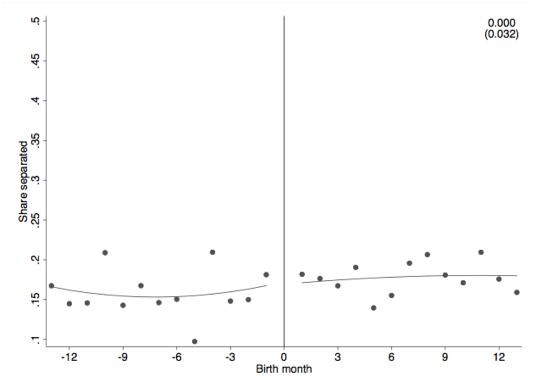


Share of parents who were together 5 years before birth

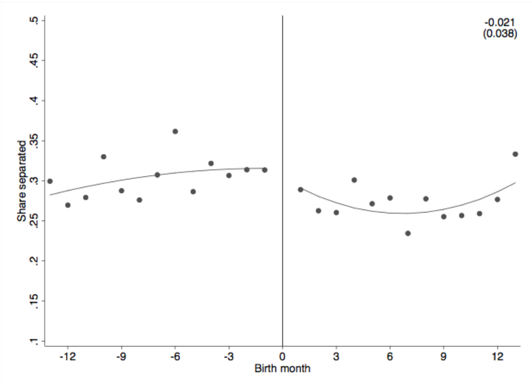
Figure A.1: Graphical evidence of covariate balance
The treatment group is to the left of the threshold in the first column and the control group on the right.



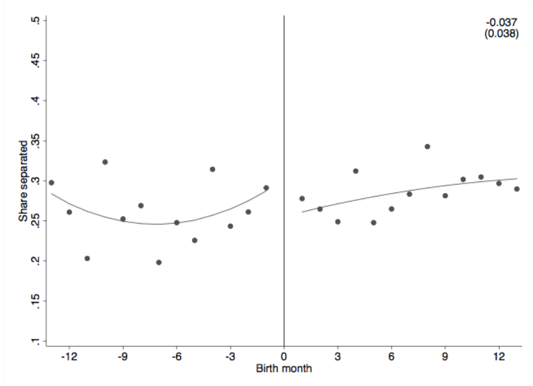
5 years - Reform sample



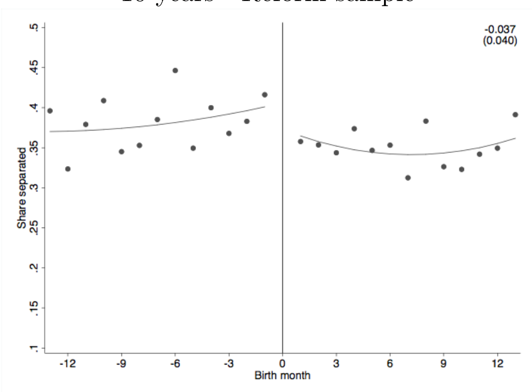
5 years - Placebo sample



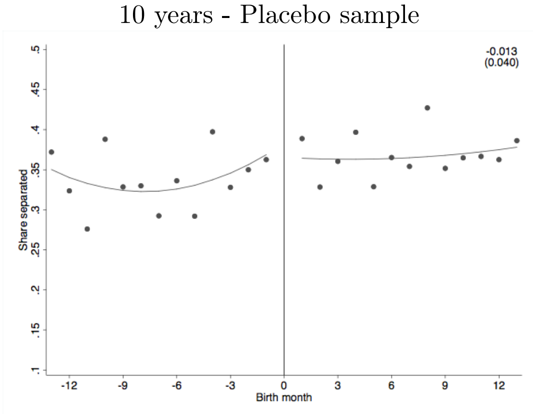
10 years - Reform sample



10 years - Placebo sample



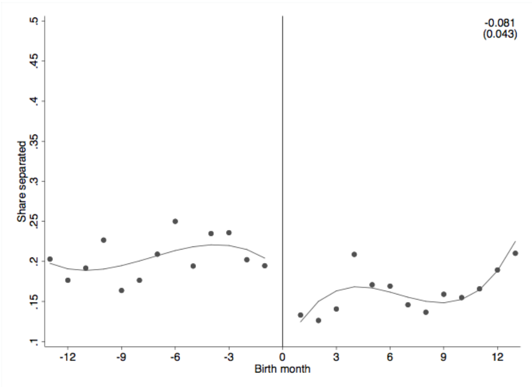
15 years - Reform sample



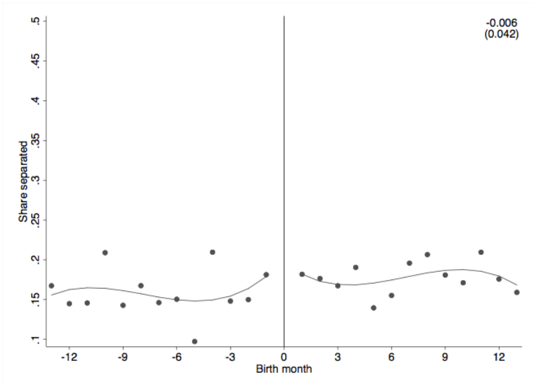
15 years - Placebo sample

Figure A.2: Divorce risk around the treatment threshold and around the placebo treatment threshold - Quadratic polynomial fit

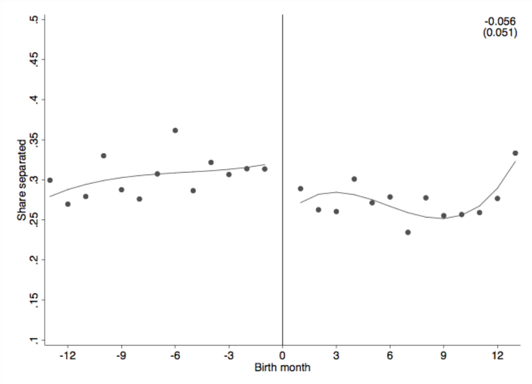
The treatment group is to the left of the threshold in the first column and the control group on the right. The placebo treatment group is on the left of the placebo treatment threshold and the placebo control group on the right.



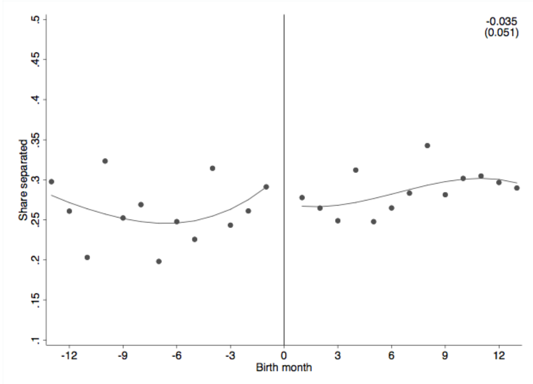
5 years - Reform sample



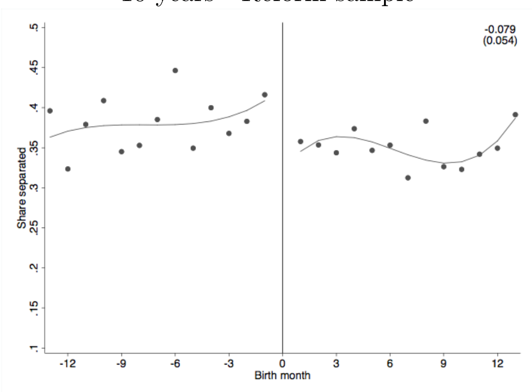
5 years - Placebo sample



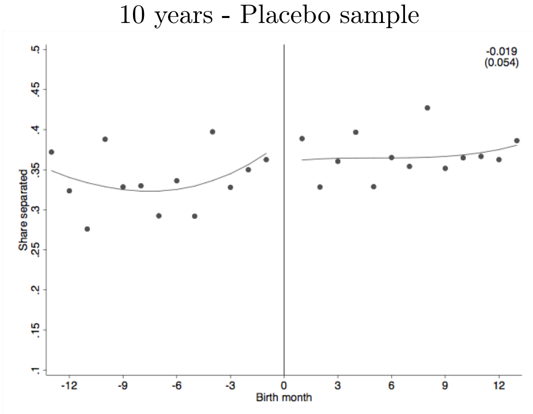
10 years - Reform sample



10 years - Placebo sample



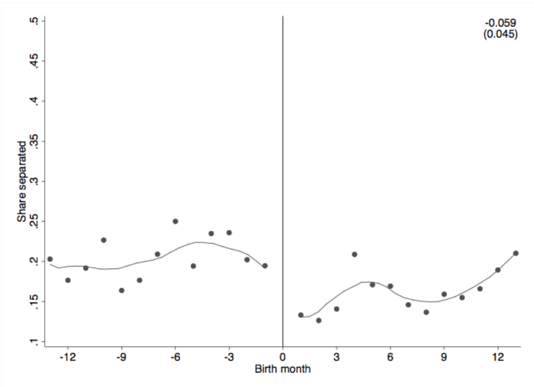
15 years - Reform sample



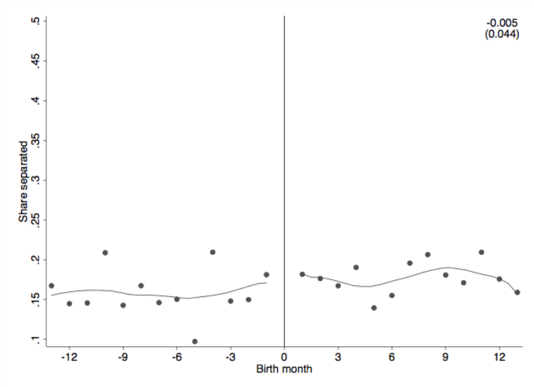
15 years - Placebo sample

Figure A.3: Divorce risk around the treatment threshold and around the placebo treatment threshold - Cubic polynomial fit

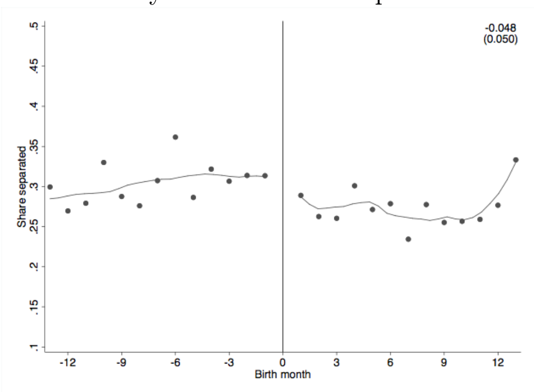
The treatment group is to the left of the threshold in the first column and the control group on the right. The placebo treatment group is on the left of the placebo treatment threshold and the placebo control group on the right.



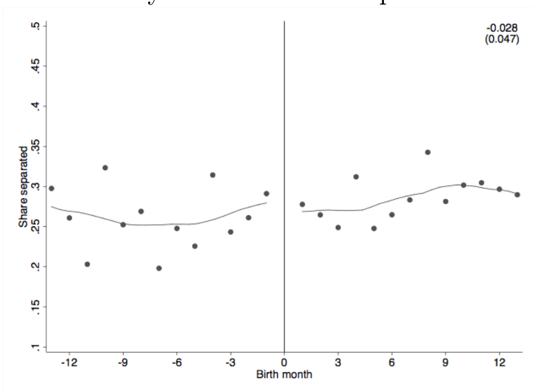
5 years - Reform sample



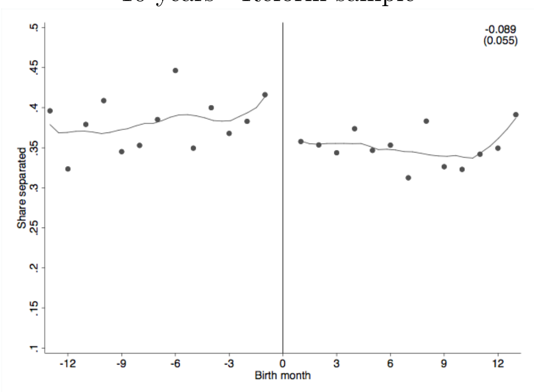
5 years - Placebo sample



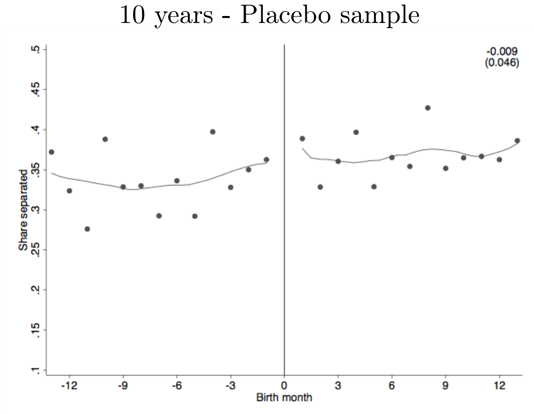
10 years - Reform sample



10 years - Placebo sample



15 years - Reform sample



15 years - Placebo sample

Figure A.4: Divorce risk around the treatment threshold and around the placebo treatment threshold - Local linear regression

The treatment group is to the left of the threshold in the first column and the control group on the right. The placebo treatment group is on the left of the placebo treatment threshold and the placebo control group on the right.