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Speedy responses:

Effects of higher benefits on take-up and division of parental leave

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Speedy responses: Effects of higher benefits on take-up and division of parental leave[a]

by

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Abstract

Using population wide register data, I exploit the “speed premium” rule in the Swedish parental leave system to estimate the causal effect of a change in the level of benefits per day on the utilization of parental leave. The results show that a 1% (5 SEK $\approx$ \$0.54) increase in the mother’s benefit level per day increases her length of leave by 2.6 days ($\approx$ 1%). Fathers respond by reducing their own time on leave by 1.9 days, i.e. about 75% of the mother’s increase. This suggests that changes in the benefit level effects not only the recipient’s time on leave, but also the division of leave between parents. This is the first paper to causally estimate the elasticity of take-up duration (length of spell) with respect to the parental leave benefit level; which is found to be equal to 1 for mothers.

Keywords: parental leave; division of labor; labor supply; take-up elasticity;
JEL-codes: D13; J13; J16; J22;

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1 Introduction

Most countries in the world have some period of paid parental leave. There is, however, large variation in the precise design of these policies. Nearly 50% of the members of the International Labor Organization (ILO) have at least 14 weeks of paid maternity leave (ILO, 2010). In addition, most countries have some amount of paternity leave, ranging from 1 day (Saudi Arabia) to three months (Iceland, Norway, Sweden and Slovenia). Sweden, having one of the most generous systems, has 16 months of paid leave out of which 13 months are given at 80% wage replacement rate.

Despite the wide implementation of parental leave policies, relatively little is known about their overall effects. There is some evidence that introducing parental leave polices, if they are combined with job-protection, do increase female employment rates and have a long term positive impact on women's wages and earnings (Ruhm, 1998, Waldfogel, 1998). Other results suggest that adding a work-requirement to get full parental leave benefits increases mothers' labor force participation before and in-between births. However, a too high requirement (two years) seems not to have this effect (Stearns, 2016). These results suggest that parental leave systems may have positive effects on women's position in the labor market. However, it has also been suggested that there is a flip-side. Long periods of absence from the workforce could have a negative impact on women's careers through human capital depreciation or signaling effects. Albrecht et al. (2003, 2014) remark that women's prolonged time on leave due to generous parental leave policies might have created a "system-based" glass ceiling, in particular in Scandinavian countries, by generating negative expectations among employers about women's career commitment after having children. Government-sponsored leave taken mostly by mothers could also cement a traditional division of labor in the home and thereby affect women's labor market outcomes. Studies from Sweden and Denmark have shown that much of the remaining gender gap in income and wages can be linked to the uneven division of family responsibilities between men and women (Angelov et al., 2016, Kleven et al., 2016).

In this paper, I investigate the effect of the level of benefits on the utilization of parental leave, using the Swedish system as my case study. The limited previous literature on the impact of the benefit level indicates a positive relationship between the level of benefits and mothers' take-up, and a negative relationship between the level and mothers' employment. Kluve and Tamm (2013) and Bergemann and Riphahn (2010) both study the effect of a reform in Germany in 2007 where the system changed from a means-tested flat rate, to a system with wage replacement. The reform increased the benefit level for most women and is found to have increased mothers' time on parental leave. Lapuerta et al. (2011) exploit variations in generosity in replacement levels across regions in Spain. She finds that larger provisions during parental leave is associated with the mother spending a longer time on leave and a later return to work. Better understanding the role of the benefit level has wide policy implications for the design of parental leave systems. Just like in other government-funded transfer systems, the reimbursement level changes the opportunity cost of working and thus is expected to affect participation and take-up rates of the program. By determining the relative gain of working and the general appeal of the program, the benefit level should affect the likelihood and length of take-up of parental leave provisions. Knowing how responsive parents are to the generosity of the parental leave programs also improves our general theoretical knowledge of how individuals respond to incentives in the tax and transfer system.

The Swedish parental leave system consists of 13 months of paid leave at 80% of previous earnings and 3 additional months at a flat rate. Out of the 13 months at the earnings-related level, 2 months are earmarked to each parent and 9 months can be divided between the parents as they wish. Parents enjoy job protection during the first 18 months after childbirth and can thus spend more time on (unpaid) leave than what can be covered full-time by the parental leave benefit entitlements. In this study the focus is on paid parental leave, leaving unpaid leave outside the analysis.

The aim of this study is to investigate the impact of the level of parental leave benefits on the take-up and division of paid parental leave among parents. Since the benefit level depends on the parents' earnings before going on leave it is thus endogenous to parental characteristics. To identify the causal effect of the benefit level on take-up of parental leave I exploit a rule in the Swedish parental leave system known as the "speed premium". This rule says that if your second child is born within 30 months (2.5 years) of the first, you can use the earnings level that you had before the first child was born to calculate the benefit level when on leave with the second child. In practice this means that parents can keep the same level of benefits for the second child as they had with the first child, even if they have lower earnings in-between births. Since many women reduce their hours of work after having children, the speed premium can be of great economic significance when on leave with the second child. Due to the speed premium, parents with less than 30 months of spacing between births have a higher expected benefit level than those with more than 30 months of spacing. Assuming that couples cannot exactly plan the time of conception or birth of their second child, the speed premium creates exogenous variation in parents' benefit levels around the 30-month threshold. The empirical analysis is performed on Swedish register data covering the years 1990-2012. The data links parents and children and contains detailed information on take-up of parental leave benefits as well as socio-economic variables. I compare the behavior of parents who had their second child just before and just after the 30-month cutoff to study the effect of the difference in benefit levels. Using the speed premium as an instrument for the parents' benefit level, a fuzzy regression discontinuity strategy is used to estimate the causal effect of a shift in the benefit level on parents' take-up of parental leave benefits. Since mothers are affected to a much larger degree than fathers, this strategy allows me to study the cross-spousal effect of a change in the mother's benefits on the father's length of leave, and thus on the division of leave.

The results indicate that decisions on how to utilize parental leave entitlements are very sensitive to financial incentives such as the level of benefits. The speed premium is found to affect the benefit level of mothers, but not of fathers. A 5 SEK ($\approx$ \$0.54) increase in the mother's benefit level per day increases her length of leave by about 2.6 days. Since the mothers' average benefit level is 506 SEK per day ($\approx$ \$55), and they take on average 268 days of parental leave, this response is equivalent to an elasticity of take-up duration of parental leave benefits of 1 for mothers. This parameter has, to the best of my knowledge, not been estimated before. The mother's increase in take-up of days induces the father to reduce his time on leave by 1.9 days; about 75% of the mother's increase. The fathers' response, a 4% decrease in take up, is larger relatively speaking since fathers on average use 49.5 parental leave days, but is only slightly smaller in absolute numbers. This suggests that the couple's decision on the total time spent at home with the child is not that sensitive to financial incentives, but that the decision on the division of leave days between the parents is highly sensitive.

In a system where paid leave at home can be divided between parents at will, such as the Swedish system, it is reasonable to think that the *relative* benefit levels of parents could affect the parents' decision of their division of parental leave as my results indicate. A change in parents' division of time on leave, induced by a shift in relative benefit levels, might also affect parents' long term division of labor if it establishes other patterns of division of household chores and child care. If so, a change in division of parental responsibilities during the first years after a child's birth could have long term consequences for gender equality both in the home and in the labor market as mothers can spend more/less time on their careers. However, most previous research has been unable to find long-term effects on earnings due to changes in length and division of parental leave. For example, Karimi et al. (2012) investigate the effects of three parental leave reforms in Sweden and find that a 3-month extension increased fathers' and mothers' take-up of parental leave days by the same magnitude. The modest effects on the parents' labor earnings correspond directly to loss of income during the extra days on leave, which indicates that there were no long-term effects on wages or career development.[1] Liu and Skans (2010) investigate the same reform and find no significant effect on mothers' earnings seven years after child birth. Two much studied reforms are the Swedish "daddy-month" reforms that earmarked 30 days of leave for each parent for children born in 1995 or later, and another 30 days for children born in 2002 or later.Ekberg et al. (2013) finds that the first reform induced a 15 day increase in fathers' take-up of parental leave days and a 25 day decrease in mothers' take-up, but finds no short- or long-term effects on earnings or employment for mothers or fathers. [2] Karimi et al. (2012) find similar results for the parents' take-up and earnings. By contrast Johansson (2010), who use a fixed-effects model, finds that time on parental leave affects the individual's earnings for both mothers and fathers four years after the child was born. In addition, she finds cross-spousal effects in terms of the fathers' time on parental leave having a positive effect on the annual earnings of the mother. This result has not been confirmed in other studies. Most of this empirical evidence seems to suggest that small changes within an already generous parental leave system, such as the one in Sweden, only have minor labor supply effects in the short run and no long-run effects on earnings. However, longer extensions of leave have been found to have only small effects on long-run labor market outcomes. Lalive and Zweimüller (2009), for example, who investigate the effects of a reform in Austria 1990 that extended the parental leave from one to two years, found substantial delays in mothers' return to work, but only small negative effects on their employment and earnings three years after the child's birth, and no significant effects after ten years. Ekberg et al. (2013) concludes that the 15-day increase in fathers' take-up of parental leave resulting from the first daddy-month, did not have any significant effect on the fathers' share of days spent at home taking care of a sick child.[3]

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[1] The authors investigate the effect of a three months extension of the leave in 1989, and the introduction of the two daddy-months in 1995 and 2002.

[2] Eriksson (2005) find a similar sized increase in fathers' take-up due to the second "daddy-month" reform. It has also been suggested that "quota-months" could have even larger long term effects through changed social norms. Dahl et al. (2014) show that the introduction of a quota month for fathers in Norway had both a direct effect on eligible fathers and an indirect peer-effect on the father's brothers and co-workers.

[3] Measured as the father's take up of *temporary parental leave days*.

Currently, there is little evidence that inducing a more equal division of parental leave results in improved gender equality in the couple later on. The available evidence suggests instead that the exact division of parental leave in terms of number of days or weeks have little or no long-run effects on the within-couple earnings gap or division of child care.

This paper contributes to the literature in several ways. It is one of few papers estimating the effect of the parental leave benefit level in a causal setting that exploits an exogenous source of variation in benefit level. While previous papers rely on a one-time reform, my strategy allows me to aggregate over many years, controlling for the impact of time period in which the child was born. Since the speed premium rule applies to everyone in the same way, I am able to control for variations between regional labor markets and individual characteristics of the parents. In addition, instead of using a sample of parents, my data allows me to study the impact of a change in the benefit level on all Swedish parents. This study also contributes to the theoretical literature on how individuals and couples respond to changes in the tax and transfer system, as it provides empirical evidence on a part of this system where it has been lacking. Specifically, this paper presents an estimate of the elasticity of take-up duration of parental leave benefits. It also contributes to the discussion of what determines the division of parental leave and how to create a more even division. My paper also adds to the literature on the effects of the speed premium rule specifically. The rule was first introduced in 1980 with the intention of improving the financial situation for families with small children, and prolonged in 1986 moving the threshold from 24 to 30 months (the Swedish Government, 1984). Earlier studies have shown that the speed premium reduced spacing between the first and second child (Hoem, 1993). Ginja et al. (2018) find that the mother's eligibility to the speed premium increases the older sibling's educational outcomes as an effect of the positive income shock and more time spent with the mother. The effect is particularly strong for children of high income mothers. Aalto (2018) shows that the introduction of the speed premium in Finland led to a reduction in labor force participation between births among women. Lalive and Zweimüller (2009) studies the effect of an extension of the time limit to qualify for the speed premium in Austria from 12 to 24 months and finds that this led to an increase in childbirths.

The remaining part of the paper is organized as follows. Section 2 gives a description of the parental leave system in Sweden and the speed premium rule. Section 3 provides the identification strategy and econometric method. In section 4 the data is described and some descriptive statistics are presented. Section 5 goes through some potential threats to identification. Section 6 presents a graphical analysis and results from the regression analysis. Sections 7 and 8 present robustness checks and placebo tests. Finally, section 10 provides a concluding discussion.

2 The Swedish parental leave system

The Swedish parental leave system was first introduced in 1974, replacing an earlier system of maternity leave. Even though, since the start, it has been possible for parents to divide the parental leave evenly, mothers have always taken the vast majority of parental leave days. The fathers' share has increased slowly from 0.5 % in 1974 to around 25 % in 2013. The system has been extended and changed through several reforms. In 1989, the parental leave was extended from 12 to 15 months. In 1995, 30 days were earmarked for each parent, the first so-called "daddy-month" reform. In 2002, another 30 quota days were added, extending the total leave to 16 months. Since then, the paid parental leave consists of 480 days of parental leave (16 months) for each child. The parental leave days can be divided between the parents any way they want, except for the quota days that are reserved for each parent. During 390 of the days (13 months) the reimbursement depends on the individual's earnings prior to leave taking. The reimbursement during these days is equal to nearly 80 % of the individual's *Qualifying Income (QI)*[4]. There is a maximum level of compensation, currently 944 SEK ($\approx$ \$106) per day, which is the level if one has a qualifying income that is at or exceeds an inflation-adjusted cap level.[5] However, many employers top up the level to 80, 90 or 100 % of the wage, also if the employees' earnings exceed the cap level, during the whole or parts of the time on leave.[6] The qualifying income is usually equal to the individual's labor earnings during the past 12 months.[7] To have a valid qualifying income the individual must have worked for at least 240 days (8 months) before the child is born. If the individual does not have a valid qualifying income, there is a minimum level of compensation per day, currently 225 SEK ($\approx$ \$25).[8] During the remaining 90 parental leave days (3 months), the individual gets a low fixed amount per day on leave, currently 180 SEK ($\approx$ \$20) per day. The parental leave days with wage replacement, during which compensation depends on the individual's qualifying income, are hereafter referred to as *"QI-days"*.The remaining 90 days are referred to as *flat rate days*.

The parental leave system is very flexible from the parents' point of view. For example, the parent does not have to take a full parental leave day per calendar day but can use 0.75, 0.5, 0.25 or 0.12 parental leave days per calendar day. Thus the parental leave days can be smoothed out over a longer time period. Since parents enjoy 18 months of job protection after the birth of a child, this gives them the opportunity to be on parental leave significantly longer than the 16 months that constitute the maximum time if one parental leave day is used per calendar day. The parental leave days can be used at any time until the child turns eight.[9]

During the child's first 12 months, the qualifying income is protected, i.e. does not decrease even if the parent does not work and stays at home without any other type of benefits. After the child turns one, the parents need to take up as much parental leave as they reduce their hours of work in order to keep their qualifying income. For example, if the parent usually works five days a week, the qualifying income level is protected if he/she takes five full parental leave days a week. Since other benefits, such as sick leave benefits, also depend on the qualifying income, parents have great incentives to protect it.

Besides the 480 parental leave days described above, the father can take 10 days of leave that

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[4] In Swedish: *Sjukpenninggrundande Inkomst (SGI)* (Sickness Benefit Qualifying Income). The compensation level is calculated by taking 80 % of the qualifying income times 0.97 divided by 365

[5] In US \$ this is equivalent to \$106 in benefits per day. The cap level is at 10 *price base amounts*, which was equal to 444,000 SEK per year in 2014 ($\approx$ \$50,000). Before the 1st of July 2006, the threshold was 7.5 price base amounts.

[6] The level of generosity from employers depends on the collective agreement, and has varied over time and sector. In general, the public sector, especially the state, has more generous agreements. The private sector commonly only covers parts of the time on leave, for example the first 30 days. There is currently no compiled information on the regulations in different sectors over time. For this reason, this component of the reimbursements when on parental leave is disregarded in the analysis.

[7] There are some exceptions from this time frame, for example if the individual has been on parental leave during parts of the last year the time frame is prolonged with the same number of days as the leave taken.

[8] The minimum level was 60 SEK/day until 2001, 120 in 2002, 150 in 2003, and 180 in 2004-2012.

[9] For children born in 2014 or later, most of the QI-days can only be used before the child turns four.

can be used during the first 60 days after the child's birth. These 10 days are often referred to as the "daddy-days", and for convenience I also refer to them in the same way in this paper.[10] Despite the lengthy parental leave utilized by most parents in Sweden, most mothers do go back to work after their parental leave and few stay at home full time to raise the children. One reason for this is that Swedish municipalities offer high quality, heavily subsidized childcare from an early age.[11]

2.1 The speed premium

The speed premium rule stipulates that if a couple has another child within 30 months (2.5 years) of the previous child, the parents are entitled to use the same qualifying income that they had when the first child was born when calculating the level of benefits for the second child. Parents are also eligible in cases where the actual birth date occurs later than 30 months but the planned date of birth was within the 30-months time limit. Since many parents (mostly mothers) reduce their earnings after having children, they also reduce their qualifying income. The speed premium rule can thus be of economic significance for these parents who would otherwise be entitled to lower parental leave benefits when on leave with their second child.

There are at least two groups of parents who are in practice not affected by this rule. The first are parents whose qualifying income stays the same or increases in-between births. These are parents who go back to work at least a year before having another child and continue to have an income that is at least as high as before the first child was born. These parents will be entitled to a benefit level with the second child that is at least as high as when they were on leave with the first child. Thus, even if they fulfill the speed premium requirement that their children are born less than 30 months apart, this rule is of no financial significance. Another group that is in practice not affected by the rule are parents who did not have a valid qualifying income, or who had a very low one, before having their first child, and who were therefore only entitled to the minimum level of benefits for the first child. Since these parents could not get a lower benefit level even if they reduced their earnings further, eligibility for the speed premium does not lead to a higher benefit level for them.

The speed premium rule was implemented in the 1970s with a threshold of 12, and later 15, months. Between 1980 and 1985 the threshold was 24 months. Since January 1st, 1986 the 30-month threshold has been in place. The prolonged window from 24 to 30 months in 1986 was part of a larger reform with the intention of improving the financial situation for families with small children (the Swedish Government, 1984). Previous studies indicate that the speed premium reduced spacing between the first and second child among Swedish parents (Hoem, 1993). Figure 1, constructed using the Swedish register data used in this paper (described in section 4), shows a clear difference in the density graph for spacing between the first and second child during the early to middle 1980s (dotted line) and the late 1980s (solid line) after the new speed premium rule had been implemented. In the pre-reform period, 1983-1985, only 36 % of couples had their second child within 30 months, and the average number of months between the first and second birth was 39 months. In the years following the reform, 1987-1989, the share of couples who

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[10] The "daddy-days" can also be utilized by a friend or relative of the woman who gave birth.

[11] Parents can enroll their child in childcare from the day the child turns one and the municipalities are required by law to offer a place at a daycare center "without delay", which is generally interpreted as within three months.

had their second child within 30 months increased to 48 %, and the average number of months between births was 35 months.[12] These numbers and the graphical evidence suggest that when the 24-month rule was in place, many parents did not manage to utilize the speed premium. Under the new regime, parents were increasingly able to utilize the premium.

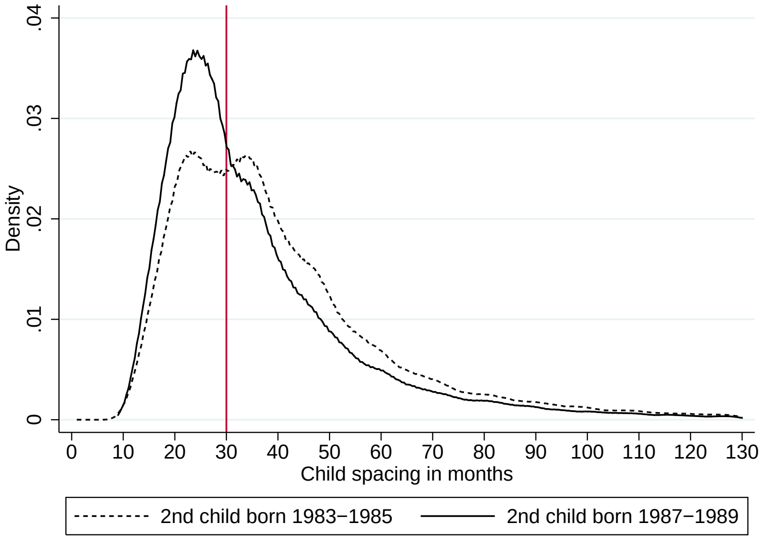

Figure 1: Spacing (in months) between first and second child's birth among families where the second child was born in 1983-1985 and 1987-1989 respectively. The graphs reveal a change in behavior after the speed premium rule was implemented in 1986. Kernel density functions calculated using Swedish register data.

3 Identification strategy

The aim of this study is to identify the causal effect of a shift in the parental leave benefit level on parents' utilization of parental leave. To this end I exploit the variation in benefit level that arises around the 30-month threshold due to the "speed premium" rule. More specifically, I use this rule as an instrument in a fuzzy regression discontinuity setting where spacing between children is used as assignment variable. Identification thus rests on the assumption that couples cannot perfectly manipulate the running variable, i.e. how long after the first child's birth their second child is born.[13] In other words, for the speed premium to be a valid instrument, there must be a random component in the determination of the precise spacing between children in the close neighborhood of the 30-month threshold.

Couples are eligible for the speed premium if the actual or expected birth date is within the 30-month threshold. The expected due date is determined by the date of conception. There are therefore two sources of variation in the second child's birthdate that I argue create random assignment of couples to either side of the threshold: variation in the date of conception and, given the

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[12] These numbers refer to couples who had their first and second child together, where the second child was born during the years indicated, who lived together at the time of both births, who did not have twins at the first or second birth and who had both children within 130 months.

[13] For an extensive discussion on this and other identifying assumptions in RD design see Lee and Lemieux, 2010.

date of conception, variation in the actual birth date. Both types of variation are relevant and are discussed in turn below.

3.1 Variation in the date of conception

Biologically, a woman in fertile age can get pregnant about one day every month.[14] Information about when this day occurs can be predicted based on the menstrual cycle, but cannot be controlled. Consider a couple that has decided to have a second child and who would like to try to become eligible for the speed premium. As long as more than nine months remain before the 30-month cutoff, they have one chance every month. Any given month, the chance to conceive is between 5 and 40 %, depending on the couple's characteristics.[15] Considering that it can take several months (or years) to successfully get pregnant, most couples who would like to be eligible for the speed premium probably start trying to conceive well in advance of the 30-month threshold, rather than close to nine months before. Since couples with at least one child have an idea from previous experience about how long it is likely to take them to conceive, they can use this as a prediction when planning when to start trying to conceive of the second child. Thus, around nine months before the threshold, many couples who would like to be eligible for the speed premium will already have conceived the second child.

However, some couples who would have liked to use the speed premium will fail to get pregnant in time in order to be eligible. If we make the assumption that such couples continue to try to conceive, whether a couple's expected due date will be in the month just before the threshold or in the month just after, is determined by chance. Thus, under the assumption that a couple's success in conceiving in month x or in month x + 1 is determined by chance, couples that conceive less than 10 months, but no more than 8 months, before the threshold can be considered as randomly assigned to treatment.

Arguably, when considering wider bandwidths, couples with different characteristics could sort into a specific period of spacing according to their preferences. For my identification to be valid, however, I do not need to assume that the time of conception is determined at random along the whole range of potential outcomes. Instead I only have to assume that treatment is randomly assigned among couples within one month of the threshold due to randomness in their date of conception. To test this (weaker) assumption, one can investigate if there is a downward shift in the frequency of births just after the threshold, or if there are any discontinuous jumps in parental characteristics at the threshold. If so, that would indicate that some couples change their mind about wanting to conceive when they realize that they have missed their chance to use the speed premium. In section 5 below I perform these tests and show that there is no evidence of sorting at the threshold.

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[14] I.e. when she's ovulating: On average, once every 28th day depending on the length of her menstrual cycle

[15] For example, the woman's age, weight, and smoking habits. The probability to conceive falls slowly between age 20 and 30, and then decreases more rapidly. At age 40 the chance of conceiving in a particular month is about 5%. There is some evidence that the man's age can also influence the chance of conceiving. Sources: Laufer et al. (2004), Dunson et al. (2002), Menken et al. (1986), e-mail correspondence with the National Board of Health and Welfare.

Table 1: Distribution of actual births around the expected due date (percent).

Week of pregnancy	Percent born	Cumulative percent
37	4,70	4,70
38	11,70	16,40
39 + 0	2,60	19,00
39 + 1	2,90	21,90
39 + 2	3,20	25,10
39 + 3	3,60	28,70
39 + 4	3,80	32,50
39 + 5	4,00	36,50
39 + 6	4,30	40,80
40 + 0	4,60	45,40
40 + 1	4,70	50,10
40 + 2	4,70	54,80
40 + 3	4,70	59,50
40 + 4	4,50	64,00
40 + 5	4,40	68,40
40 + 6	4,10	72,50
41	20,20	92,70
>= 42	7,30	100,00

Note: Percent of children born in Sweden, 2010–2012, at different gestational ages (pregnancy week and day). The expected due date is at 40 week and zero days (indicated as 40 + 0 in the table). Multiple births, planned c-sections and births before pregnancy week 37 are not included. Calculations made by The National Board of Health and Welfare using information from the Medical Birth Register. Information in table was first published by TT News Agency and SVT (SVT, 2014).

3.2 Variation in the actual birth day of the second child

The child's expected due date is calculated based on the mother's last menstrual period and measurements of the fetus made during an ultrasound in pregnancy week 17 or 18. The expected length of a pregnancy is 40 weeks (counted from the woman's first day of the last period). However, only about 5 % of children are born at exactly 40 weeks + 0 days. Detailed statistics on the distribution of actual births around the expected birth date are presented in Table 1. 25 % of children are born in the week before their expected due date (week 39 of the pregnancy), 32 % are born during week 40, and 28% are born in week 41 or later. About 16% are born before week 39, more than a week before the expected date.[16] Multiple births, planned c-sections and births before pregnancy week 37 are not included in these statistics. Other then that they include all single births, not just second births for which the pattern could be different. Still, these numbers suggest that the distribution of actual births around the expected due date is wide, with more couples exceeding their expected date than preceding it. This means that a large (about 50 % at the threshold) but decreasing proportion of the couples with an observed spacing between children of more than 30 months, are in fact eligible for the speed premium since their expected due date was before the threshold.[17] Likewise, almost half of the couples with observed spacing of just less than 30 months are eligible due to an early birth (rather than due to their expected due date). Because of the wide dispersion

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[16] These calculations were made by The National Board of Health and Welfare using information from the Medical Birth Register on all births in Sweden during 2010-2012.

[17] For example; for about 55 % of those whose expected due date is the day before the 30-month threshold, the child's actual birth date is beyond the threshold. Among the children born on the first day just beyond the threshold, more than 50 % are eligible due to an earlier expected date, since more people exceed their expected date than those who are born before or on that date.

of actual births around an expected due date, the proportion of couples who are eligible for the speed premium due to their expected due date decreases continuously to the right of the threshold. About 27 % of couples whose children are born in the first week after the 30-month threshold, and about 20 % in the week after that, are eligible because of their expected due date. The proportion of eligible couples is thus 100% as long as less than 30 months have passed on the child's actual birthdate, and less than 100%, starting at above 50% and decreasing continuously, among couples whose child was born beyond the threshold.

Using the child's actual birthdate, couples are randomly assigned to treatment (eligibility) under the assumption that parents do not strategically manipulate the child's birthdate. Earlier studies have shown that a strong economic incentive can induce parents to have an early birth or to delay a birth. The empirical evidence presented in Neugart and Ohlsson (2013) suggests that it is possible to delay a birth by about one week. As for inducing an early birth, Borra et al. (2015) show evidence of a shift in birth timing by between one and two weeks in response to the withdrawal of a 2,500 € baby-bonus in Spain.

In Sweden, the proportion of children born with Caesarean section (C-section) increased from 10 to 16 percent between 1994 and 2009. About 50 % of the C-sections are emergency C-sections. A planned C-section can be granted due to medical concerns or psychological reasons such as a strong fear of going through labor. However, a mere wish for the child to be born on a specific date due to practical reasons is not sufficient grounds for the parents to have a planned C-section according to official recommendations for the Swedish health care system (SOC, 2011). The proportion of vaginal births that were medically induced increased from 8 to 12 percent during the same period. The most common reason is that more than 41 pregnancy weeks have passed at which point the birth will be medically induced (SOC, 2009). A likely reason for the increase is that medical and age characteristics of mothers, which affect the risk of a prolonged pregnancy, have changed. A birth is only induced due to medical reasons. In section 5, the risk of strategic timing of births around the threshold is investigated and no evidence of it is found.

3.3 Implications for identification

Assuming that the distribution of actual births around an expected due date is the same on both sides of the threshold, the proportion of couples with an expected birthdate before the threshold decreases *continuously* over the 30-month threshold, when observing actual births over spacing. Likewise, the proportion of couples with an expected birth date to the right of the threshold increases *continuously*. Thus, the variation in expected birth date of the second child does not give rise to a discontinuous jump in the proportion of eligible couples at the threshold. The discontinuous jump in the proportion of eligible couples arises solely from couples whose expected due date was beyond the threshold, but whose child was born before it due to an early birth. Thus, under the assumption that couples cannot manipulate the actual timing of the second child's birth, the variation in actual birth dates of children creates random assignment to treatment status among couples around the threshold.[18] As explained in detail in section 4 below, the data available in this study only contains information about children's year and month of birth, not the exact date. Using

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[18] This assumption is discussed more in second 5.2 below where evidence of its reliability is presented.

this information only a crude measurement of the number of months between children's births can be calculated. However, as discussed above, couples who are further away from the threshold can also be considered randomly assigned to a specific side of the threshold due to randomness in the date of conception. For this reason, a comparison between observations in larger bins on either side of the threshold is a valid method to causally estimate the effect of a randomly assigned eligibility status between otherwise "similar" couples.

When taking the average over time intervals on each side of the threshold, the proportion of individuals who are eligible for the speed premium due to their expected due date will no longer change continuously, but rather in discontinuous steps when moving between bins. If the bin size is large enough, there will be a particularly large shift in the proportion of observations with an expected due date before the threshold when moving from the bin to the left of the threshold to the bin on the right. This line of argument is relevant for identification in this study since information on the actual birth date is not in the data, but the child's birth month and year is. The baseline analysis uses a proxy variable for the child's birthdate using detailed information on the dates of parental leave take-up (described in detail in section 4.2.1). However, the smallest bin size possible is one day.[19] In a complementary analysis, the monthly data is used instead.

3.4 Fuzzy regression discontinuity design

As explained above, couples both before and after the 30-month threshold can be eligible to the speed premium depending on the child's expected due date. Also, eligibility status is only relevant for parents who have a lower qualifying income at the time of the second child's birth than they had when the first child was born. For parents who increase their earnings between births, the speed premium rule does not have any economic significance, since it does not affect their benefit level. Hence, spacing between children only changes the likelihood of eligibility, but does not sharply determine it. Thus, when exploiting the speed premium rule as a source of variation in benefit level, a fuzzy regression discontinuity design is the appropriate strategy.

In fuzzy RD, estimation is made in two steps that can be described as a two-equation system. The first stage (1) and second stage (2) equations can be written as:

$$y_{1i} = \alpha_1 + \lambda 1[s_j < c] + 1[s_j < c]g_1(s_j - c) + 1[s_j \geq c]g_2(s_j - c) + \gamma_1 X_i + \epsilon_{1i} \tag{1}$$

$$y_{2i} = \alpha_2 + \beta y_{1i} + 1[s_j < c]f_1(s_j - c) + 1[s_j \geq c]f_2(s_j - c) + \gamma_2 X_i + \epsilon_{2i} \tag{2}$$

where c is the cutoff point at 30 months of spacing and s_j is the spacing between the first and second child of couple j. Further, g_1, g_2, f_1 and f_2 are unknown functional forms that describe the trends in the outcome variables in each equation. X_i is a vector of pre-determined covariates and ϵ_{1i} and ϵ_{2i} are error terms. The outcome variables are, in the first stage (1), individual i's level of parental leave benefits, and in the second stage (2), individual i's take up of parental leave days. In the first stage, any discontinuous jump in the parental leave benefit level at the threshold

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[19] If very detailed information on time of birth was available, for example hour and minute, an even more thorough analysis could be conducted.

is estimated. In the second stage, the predicted values from the first stage are used to estimate the effect of a one unit change in the benefit level on the utilization of parental leave days.

The λ parameter in equation 1 can be interpreted as an estimate of the jump in level of benefits at the threshold. β is the parameter of most interest as it gives an estimate of the causal effect of a one unit (1 SEK) change in the parental leave benefit level on the utilization of parental leave days. X_i is included to control for any sorting on covariates across child spacing that may affect the trends estimated by the functional form of g_1, g_2, f_1 and f_2. For this reason, and to increase precision of the β estimate, X_i is included in the model. Under the assumption of randomization in treatment (eligibility to the speed premium) around the threshold, including the covariate vector X_i should not affect the magnitude of the β estimator directly. However, including the X_i vector could affect the estimated trends on each side of the threshold. Thus, it could also affect the gradients and the end points of the trends and thereby affect the estimate of β.

3.5 Choice of functional forms and bandwidth

In the RD literature there is a discussion on how to choose what functional forms (i.e., g_1, g_2, f_1 and f_2 in equations 1 and 2) and bandwidth to use when implementing the method. One approach is the global parametric method where all data and flexible functional forms such as higher order polynomials are used. Another commonly implemented approach is the local non-parametric method, in which a smaller bandwidth “near” the cutoff is chosen based on some criteria and a less flexible functional form, typically linear, is used. The motivation for choosing linear trends is that it is realistic to assume a linear functional form near the cutoff (Hahn et al., 2001). However, higher order polynomials can also be included, a method sometimes referred to as local polynomial regression (Porter, 2003).

A drawback of the global parametric approach is that it is hard to know which functional form correctly describes the data at all ranges of the running variable. As demonstrated by Gelman and Imbens (2014), RD-estimates tend to be sensitive to the precise form chosen, which introduces a risk of bias. In light of this critique, I use the non-parametric approach, which implies that a bandwidth has to be chosen. Several methods for choosing the bandwidth have been proposed. Two of the most prominent are Imbens and Kalyanaraman (2011) and Calonico et al. (2014), who suggest data-driven methods for the choice of bandwidth (hereafter referred to as the IK and CCT optimal bandwidths). However, both of these methods were developed with a sharp RD design in mind: they derive an optimal bandwidth of the running variable based on *one* outcome variable, not *two* as is the case in a fuzzy RD setting (the first and second stage/reduced form outcome variable). Rather than choosing one of these methods, when in fact there is no consensus in the literature on which one to prefer, I present estimates for a large range of bandwidths, including the CCT and IK suggestions.[20] To apply equal transparency when it comes to the choice of functional form, I use a linear functional form in the main specification, but include higher order polynomials as a robustness check.[21] To give more weight to observations near the cutoff, triangular weights are used.

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[20] The CCT and IK optimal bandwidths are calculated using the program RD-Robust which has been developed by Calonico et al. (2014) and is described in detail in Calonico et al. (2014).

[21] As suggested by for example Jacob et al. (2012)

4 Data

The data used in this paper consists of population-wide register data covering all residents of Sweden aged 16-65 during the period 1985 - 2010. The dataset contains information on marital status, cohabitation (provided the couple has children together), number of children in the household, and other socioeconomic variables such as labor income and social transfers. The data links parents and children born up until 2009, and there is information on the children's year and month of birth, but not the exact birthdate.

Data on the take-up of parental leave is available for the years 1994-2012. The information is very detailed in the sense that it is possible to observe all periods (exact calendar days) when the individual took out parental benefits, as well as the precise amount of benefits received. The parent can use parental leave benefits "part time", using less than one full parental leave day (i.e. one net day) for each calendar day. However, for each period on leave, the total take-up of net days of leave is also specified.

Using this information, measures of the length of leave of each parent for each child are constructed. The length of leave is measured in three ways: first, in terms of *number of net days with benefits at the "qualifying income" level (QI-days)*; second, as *the total take up of net days also including flat rate days*; and third, in terms of *the number of calendar days with any benefits*. All leave periods that started within the first two years of the child's birth are included in these measures of the length of leave.

Only accounting for paid parental leave is likely to underestimate the actual time spent at home since parents can mix paid leave with unpaid leave. They are especially likely to do so during the child's first year when the qualifying income level is protected. Since information on hours of work per calendar day is not available it is hard to know exactly how long the parents actually stay home from work. Thus, any measurement of take-up of parental leave days should be viewed as a proxy for time spent at home with the child.

The measure of benefits per net day on parental leave is constructed using the individual's first period on leave.

4.1 Sample restrictions

From the data, all couples who had their first and second child together during the years 1994 - 2009 are sampled. The sample is restricted to couples that were married or cohabiting at the time of both children's births and where none of the partners had children before. Couples who had multiple births are dropped. Out of these, all couples are kept where both partners had positive earnings the year before the first child was born and whose parental leave benefit levels per net day were above the minimum level when they were on parental leave with the first child. I do this to better capture the group of parents that are more likely to be affected in a significant way financially by the speed premium. Parents who get the minimum level of benefits with the first child cannot benefit from the speed premium rule since they would get the same level of benefit with their next child even if they reduced their earnings between children. Parents who were working the year before their first child was born and whose qualifying income at the time of the first child's birth was high enough for their benefit level to be above the minimum are more likely to be at least somewhat established in the labor market. This implies that they have an actual choice of going *back* to work after staying on leave. The first three rows of table 2 describe how the number of observations in the sample changes when imposing these restrictions.

4.2 Measuring spacing between children

The data used for this study contain information about children's birth year and birth month, but not the exact birthdate. Spacing between the first and second child is therefore calculated using only children's year and month of birth. Depending on the exact day of birth this measurement might indicate (almost) one month too much or one month too little between siblings. Suppose, for example, that the first child is born late in the month, say the 25th, and the second child is born early, say the 1st. Suppose further that the second child is born in the 30th calendar month after the first child. Then the actual spacing between the children is 29 months and 5 or 6 days. However, using only the children's birth year and birth month to calculate spacing gives a measurement that is equal to 30. In fact, in all cases where the second child is born earlier in the month than the first, this measurement of spacing is one unit too large. This will happen in about 50 % of all cases assuming that the birthdays of the first and second child are randomly distributed over the months when they were born. For identification, it is essential to identify all observations as either within the 30-month time frame or beyond it to be able to estimate a discontinuity at the threshold. Hence such a crude measure of spacing is problematic. Using this measurement of spacing, the jump in benefit level *at the threshold* cannot be estimated.

However, as discussed in section 3, the causal effect of a shift in the benefit level can also be estimated by comparing observations within wider bins of spacing, under the condition that random assignment to an exact value of spacing can be assumed over a wider range of spacing. Assuming that there is a random component in the assignment of observations to either spacing equal to 30 or 31 (using the crude spacing-measurement), a comparison between these two bins of observations can be used to estimate the causal effect of a shift in the benefit level. For couples where my measurement of spacing is equal to 30, about 50 % are eligible because of the second child's expected due date, and a smaller proportion are eligible because of an early birth of their second child. For observations where my measurement of spacing is equal to 31, some proportion of the observations are eligible because the expected due date of their second child was within the 30-month threshold, although the actual birth date of the child came later. Hence, a larger proportion of the observations with spacing equal to 30, than among those with spacing equal to 31, are eligible for the speed premium. The speed premium rule thus exogenously creates a discontinuous jump in the expected benefit level when moving between these two bins. This drop in the share of eligible couples can be exploited to estimate the effect of a change in the benefit level.

4.2.1 Proxy variable for spacing between children.

Although causal inference estimating the effect of a higher benefit level on the utilization of parental leave is possible using the crude measurement of spacing described above, more detailed information on the precise spacing between births would improve identification for several reasons. First, information on the exact birth date of children would improve estimation of the trends included on each side of the threshold so that they better reflect the true functional forms in the data. Second, knowing the precise distribution of observations around the threshold would make it possible to exploit the discontinuity in probability of being eligible *at the threshold*. The observations that give rise to this discontinuity are couples with an expected due date beyond the threshold whose second child had an early birth. Thus, the assumption of randomness in date of conception is no longer needed: Identification rests only on the assumption that couples cannot exactly control the actual birth date of their second child. Third, with a measurement of spacing based on the date of birth, estimation can be made comparing observations in a closer proximity to the threshold than what is otherwise possible. The advantage of this is that the assumption of random assignment to a specific birthdate of the second child only needs to hold in a close neighborhood of the threshold, instead of over a wider time span.

To be able to perform estimations *at the threshold*, a proxy variable for spacing between children is created. First, proxy variables for the first and second child's birth days are created, and then the time between these days is calculated. This gives a measure of spacing in days, rather than in months.

The proxy variable for spacing measured in days is created using information about the father's take-up of his first daddy-day, and the mother's take-up of her first parental leave days. The reason for using these variables is that fathers typically use the daddy-days in connection with the child's birth, while using the parental leave days later. Mothers, on the other hand, typically go on parental leave in close connection with the child's birth. Fathers can use the ten temporary parental leave days, popularly called "daddy-days", at any time from the child's birthday up until 60 days later. Since fathers cannot use the daddy-days before the child is born, the first daddy-day is the best proxy for the child's birth day.

In cases where the father used his first "daddy-day" in the same month as the child was born, this day is used as a proxy for the child's birthdate. In about 54% of the cases the father used his first daddy-day during the child's birth month for both children. For these observations, only information about the father's take-up of daddy-days is used to create an alternative measurement of spacing. However, this sample might not be representative for all couples since fathers in this sample chose to spend more time with the child early on.

For children where the father did not use his first daddy-day during the child's birth month, the mother's first day on parental leave is instead used as a proxy for the child's birthday, but only if this day occurred in the same month as the child was born. Mothers can use their parental leave days starting 60 days before the child's expected due date. However, by only using this information if the day was taken in the same month as the child was born, the mother's first day on leave is a reasonable guess as to when the child's birthday occurred. By also using information on mother's take-up of parental leave days, more observations can be included. This way, the proxy variable for spacing covers about 75 % of the population instead of only 54 %. This improves the external validity of estimations made using the sample. For this reason this proxy for spacing is used in the main specification. The last two rows of table 2 describe the number of observations left in the sample when using this proxy variable and when using the spacing measurement based only on the father's utilization of daddy-days.

Table 2: Table of sample restrictions.

Sample restriction	No. of obs.
Couples who had their first and second child during the years 1994 - 2009 and were living together at the time of both births. Multiple births excluded.	323,748
Both partners had positive earnings the year before the first child was born.	272,085
Both parents had a parental leave benefit above the minimum, level when on parental leave with the first child.	176,291
Using information about the mother's first PL-day and the father's first "daddy-day" with each child to calculate spacing between children.	133,075
Using only information about the father's first "daddy-day" with each child to calculate spacing between children.	94,554

Note: The table shows the number of observations when applying different sample restrictions. The two last rows show the number of observations included when using the alternative measures for spacing between children: Using only information about the father's utilization of "daddy-days" (last row) or, in addition, also using information about the mother's first parental leave day (second to last row).

4.2.2 Defining treatment when using different measurements for spacing

The two different approaches to estimation described above, using the crude measurement of spacing or the proxy variable, lead to different interpretations of the estimates.

First, using the two different variables has different implications for the external validity of the estimates: The monthly spacing variable covers the entire population of parents, whereas the proxy variable only covers a nonrandom selection of parents who used either the first daddy-day or the mother's first PL-day during the child's birth month.

Second, there is a difference in what "treatment" entails when using the two different measurements of spacing as a running variable. Using the proxy variable, estimation is made at the threshold exploiting the variation in actual due date around the threshold among couples whose expected due date was beyond it. Under the assumption that couples cannot manipulate the child's actual birthdate, the observations that might give rise to a jump in benefit level at the threshold (i.e. the compliers) are those who had an expected birthdate that was beyond the threshold but who became eligible because of an early birth of the second child.

The proportion of individuals who are eligible because their expected due date was before the threshold decreases continuously over the threshold. Thus, at least in the (very) close proximity of the threshold, the expected due date of the second child should be balanced across spacing. (However, as soon as any range larger than at most one day on either side of the threshold is considered, this will no longer be true.) This means that, when estimating the discontinuity at the threshold, the eligibility status of the compliers was unexpected. In this case, treatment is thus to become eligible for a higher benefit level than anticipated.

On the other hand, when using the crude monthly measurement of spacing, estimations are made comparing all observations in month 30 to those in month 31. The main difference between these bins of observations is that a significantly larger proportion of those in month 30 had an expected due date within the 30-month time frame: an expected due date that they knew would make them eligible. Thus, when using the monthly variable, most of those who are treated in month 30 have known during the pregnancy that they were going to be eligible for the speed premium. They have thus been able to take this into account when planning for example how to utilize the parental leave with the second child, and their labor supply during the pregnancy.

When using the proxy variable, the eligibility status for the compliers is a surprise to them. Thus it could not have affected their planning on how to use the parental leave with the second child or behavior during the pregnancy. When estimating the response in behavior in this group, it should thus be interpreted as a direct response to an unexpected shift in benefits.

When using the monthly spacing variable, on the other hand, any estimate of a behavioral change should be interpreted as a combination of a response to unexpected eligibility for some couples in month 30 and (predominantly) a response to knowing with certainty that you will be eligible.

4.3 Utilization of parental leave

Figures A01*a* - A01*d* in the appendix show the density of observations over parental leave benefit levels of mothers and fathers with the first and second child. For both mothers and fathers there is bunching of individuals at levels equal to the maximum level of benefits in different years. The maximum level that an individual could be entitled to increased significantly on the 1st of July 2006. This explains why there is a larger density at high levels of benefits for the second child. More fathers than mothers receive the maximum amount of benefits (since they have earnings that exceed the cap level). 12.5% of mothers and 27.5% of fathers receive the highest level of benefit with their first child, and 11.5% of mothers and 27% of fathers receive the highest level with the second child. The average levels of benefit is, for mothers, 462.6 SEK with the first child, and 503.3 SEK with the second, and for fathers, 541.7 SEK with the first child and 600.3 SEK with the second.

Figures A02*a* - A02*d* show the distribution of observations over the total number of calendar days with parental leave benefits (both at wage replacement and the flat rate days) for mothers and fathers with their first and second child.[22] The histograms reveal bunching at 330 and 420 days for mothers and at 0 and 30 days for fathers. From 1995 and onwards, 330 has been the maximum number of QI-days that a parent could use. (Before 1995 the maximum number of days was 360). Adding the 90 days at the flat rate gives 420 days. The first quota month was also introduced in 1995, which meant that 30 days are reserved for each parent. The other quota month (30 days) that was introduced in 2002 does not seem to have resulted in bunching at 60 days. The percentage of fathers who did not use any parental leave days was 13.8% with the first child and 14.8% with the second child. The average number of days with PL-benefits is, for mothers, 311.4 days with the first child and 303 days with the second, and for fathers, 58.5 days with the first child and 55.5 days with the second.

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[22] These measurements do not include the 10 daddy-days.

5 Potential threats to identification

5.1 Misclassification due to measurement errors

Because the proxy-variable is constructed using information on the parents' use of daddy-days and PL-days (parental leave days), patterns in how the parents utilize these days could induce measurement errors in the proxy-variable for spacing. For example, if parents tend to utilize their days with their first and second child in different ways, systematic measurement errors in spacing are more likely to occur. As a result, observations could be mistakenly classified as lying above (below) the threshold when they should in fact be classified as lying below (above) the threshold. Such misclassification would blur out any discontinuous jumps that are actually present at the threshold, thereby decreasing or erasing an estimate of such jumps.

Since the actual date of birth is not available I cannot investigate the parents' utilization patterns in terms of distance to the child's actual birthday. Figures A03*a* - A03*d* in the appendix show the distribution of when in a month the father used his first daddy-days with the first and second child. Figures A03*a* and A03*c* show the distribution for the whole sample. In figure A03*b* and A03*d* the sample restriction that the father's first daddy-days should be in the child's birth month is imposed. The histograms for the full sample reveal no obvious pattern other than a uniform distribution of when during a month fathers start to use their daddy-days. The frequency for the 31st is lower because not all months have a 31st day. When imposing the sample restriction, there is a drop in frequency early in the month. This is probably because for children born late in a month, many fathers do not take the first daddy-day until for example the first day in the next calendar month.[23] Most important for identification is that the pattern of when fathers utilize their first daddy-days seems similar for the first and second child. Using the distance between the first daddy-day with the first and second child will thus not systematically over- or underestimate spacing between children, but could still place observations on the wrong side of the threshold due to measurement errors.

Figures A04*a* - A04*d* show the distribution the mothers' first day on parental leave with the first and second child over the month. Figures A04*a* and A04*c* show the distribution for the whole sample and figure A04*b* and A04*d* show the distribution when imposing the restriction that the mother's first PL-day should have been taken during the child's birth month. These histograms indicate a clear pattern for when mothers typically go on parental leave, namely the first day in a calendar month. This pattern remains but becomes weaker when imposing the restriction on mother's take up timing: In the restricted sample, about 11 % of mothers go on leave on the first day in the months, compared to around 4 % on any other day. The only other day that stands out is the 21st.[24] As with the daddy-days, patterns in take-up of PL-days only lead to systematic under- or overestimation of spacing if mothers act differently for their first and second child. This does not seem to be the case. However, at least in some cases when the mother goes on leave on the first of the month, this day is a bad proxy for when the child is born. About 25 % of the women who

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[23] The histogram indicates that fathers in general tend to use their first daddy-day up to a week after the child is born. The frequency of fathers taking their first daddy-day on the first day of the month is only about one third of the number on days beyond the first week of the month.

[24] This is possibly because the payments of PL-benefits are made late each month for leave days taken between the 21st of the last month until the 20th of the current month.

went on leave on the first day of the month with the first child also did so with the second child.[25] For these (relatively few) couples, using this day to calculate spacing between children gives the same measurement as the monthly spacing variable.

69% of fathers used their first daddy-day during the child's birth month with the first child, 65% with the second, and 54% with both children. Among couples where the father used his daddy-days later or not at all, 56% of mothers used their first PL-day during the child's birth month with the first child, and 57% did so with the second child.

Among 71 % of the observations in the sample when using the proxy variable, only information about the father's daddy-days is used to construct the proxy. For about 10%, only information about the mother's PL-days is used, and for the remaining 19 %, a combination of the two is used to calculate spacing. Among these 19 %, the proportion of couples where the first daddy-day is used for the first child and the mother's first PL-day in used for the second child, and vice versa, are about the same. Thus constructing the proxy variable in this way should not systematically over- or underestimate spacing between children. There will however be measurement errors that could in some cases be as large as (almost) two months. This type of measurement-error should create noise, but not bias, in the estimations.

5.2 Strategic timing of births

In section 3 I discussed the identifying assumptions that must hold to be able to assume random assignment of treatment status for observations close to the threshold. In this section I discuss what requirements need to be fulfilled in order for the date of conception and the child's actual birth date to credibly be assumed to be randomly assigned to couples. When it comes to the date of conception (which determines the expected due date) I do not need to assume that this is randomly assigned along the whole range of spacing, but only among couples within one month of the threshold. This assumption requires that couples who have just failed to conceive in time for the expected due date to be within the 30-month time frame do not change their mind about wanting to get pregnant. If this type of couple (or a selected sample of them) decides not to try to conceive in the next month, one would expect a discontinuous shift downwards in the frequency of births at the threshold. An additional indication that a selected sample of couples opt out of having another child because they have missed their chance to be eligible for the speed premium, would be if there were any discontinuous jumps in parental characteristics at the threshold.

When it comes to the actual birthdate, direct manipulation of the birthdate constitutes a potential threat to identification. Some couples in my sample with an expected due date close to but beyond the threshold have a strong economic incentive to try to have an early birth because of the speed premium. If they can somehow schedule a C-section or induce a vaginal birth before the threshold, then the assumption that the child's actual birthdate is randomly assigned is violated. Whether this is the case or not can again be tested by studying the distribution of births just around the threshold. If couples are able to manipulate their child's actual birthdate, one would expect a heap just before the threshold when plotting the distribution of births around it, and a sharp drop in frequency just after. Such manipulation could also create discontinuities in parental characteristics at the threshold

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[25] Taking into account that some children are actually born the first day of the month, about 2 % of all mothers seem to always go on parental leave on that day regardless of which day the child is born.

if only a selected sample of parents choose to manipulate the birthdate. Thus, a covariate balance test of parental characteristics is also a way to investigate whether there is manipulation in the actual birthdate of the second child.

5.2.1 Graphical investigation

Figure A05*a* and A05*b* in the appendix display the frequency of births over spacing when using the crude monthly measurement based on children's birth year and month. Figure A06*a* and A06*b* show the frequency over the same bandwidths, the first 100 months and between 24 and 36 months of spacing, using the proxy variable for spacing in weekly bins.

The histograms reveal that many couples aim to have their second child in time to become eligible for the speed premium. It seems, though, as if couples who want to become eligible for the speed premium give themselves some margin to succeed: The peak in frequency of birth occurs already around 25 months, rather than just before the threshold. Reassuringly, the histograms show neither apparent heaping in births just before the 30-month threshold, nor a sharp drop in number of births just after.

Figure A06*c* displays the frequency of births in the two weeks right before and after the cutoff in daily bins of the proxy variable (the smallest bin size possible). The histogram reveals a rather surprising weekly heaping pattern in the proxy variable. It is unclear why this patter occurs. Perhaps it can be explained by parents using their days of leave on specific weekdays. Figures A07*a* - A07*c* and figures A08*a* - A08*c* replicate the histograms in figure A06*a* - A06*c* but when using, first, only information about the fathers use of daddy-days, and second, using only the mother's first PL-day to calculate spacing. The histograms with larger bandwidths and weekly bins show the same type of patters as the histograms described above. The weekly heaping pattern is more pronounced when only using the daddy-days. This pattern could thus have to do with how fathers use their daddy-days. However, this pattern does not constitute evidence that parents are able to manipulate the precise birthdate of the child.

Even though the frequency pattern over wider ranges of spacing suggests that parents plan the timing of their second child's birth in order to become eligible for the speed premium, there is no indication of strategic manipulation just around the threshold. In regression discontinuity analysis, the parameter of interest is evaluated at the threshold. Thus, the overall higher frequency of births on the left side of the threshold is not in itself a threat to identification.

5.2.2 McCrary test

A formal test for whether there is manipulation in the running variable is the McCrary test (McCrary, 2008). It analyzes the number of observations on each side of the threshold and estimates the difference in the log density between the two sides. If there is a heap of observations just before the threshold, or a sharp drop in observations just after, this test will estimate a jump in the density of observations. The test is designed for a continuous running variable, and therefore I use the proxy variable when performing it. Figure A09*a* in the appendix shows graphically the result when using a 30-day bandwidth and a bin size of one day (the smallest possible). The estimate of -0.082 (s.d. 0.048) is not significant. However, the estimate is relatively large, which could be caused by the weekly cyclical pattern shown in the histogram of the proxy variable. To avoid bias caused by this pattern, the McCrary test is again estimated using weekly bins and multiple bandwidths between 7 and 182 days (26 weeks). Figure A09*b* graphically shows the results from this exercise. The estimate is close to zero for the first few months and then becomes significantly negative at about 4 months (125 days), but never larger than -0.085 (at 6 months). It is worth noting that when including observations more than 4 months away from the threshold, one would not expect the assumption of randomization in birthdate to hold. With this bandwidth, also observations from the "heap" in the distribution of birth (with a peak at 25 months of spacing) are included. Thus, it is not surprising or worrisome that the McCrary estimate is significant when including the larger mass of observations in this region. The same exercise is repeated using the "daddy-days"-proxy presented in figure A09*c* (when only information on daddy-days is used) with similar results.

5.2.3 Covariate balance

As an additional test of sorting, I test whether parental characteristics are balanced around the threshold. To this end, the first stage regression is estimated replacing the benefit level with another covariate: Mother's and father's years of schooling, yearly labor incomes, ages and immigration status, couple's marital status, first and second child's gender, and year and month of birth. Table A01 in the appendix presents population means for the sample when using the proxy variable for spacing, the mean values for all observations near the threshold/cutoff (less than 30 days from it), and estimates of jumps in mean value at the threshold for the sample near the cutoff, for a number of individual and household covariates. All variables that vary over time indicate the value the year before the first child's birth.

None of the estimated jumps are significant, and most of them are close to zero. In addition, the mean values for the sample near the cutoff are close to those in the whole population. The table shows that, on average, mothers have about additional 6 months of schooling but have lower earnings than fathers the year before the first child was born. Mothers are about two years younger than fathers. About one third of all couples were married the year before they had their first child. About 8.5 % of all mothers and fathers are immigrants, and about 1 percentage point less among those near the cutoff. Couples near the cutoff are also a little more educated and have somewhat higher labor earnings.

5.3 Trends in spacing and parental leave variables over time

Changes in the pattern of spacing or other variables over time could be a problem for estimation. Figures A010 and A011 in the appendix show the distribution of births across spacing over different birth years of the first and second child. The histograms reveal that the general patterns in spacing differ between years.[26] Figure A012*a* shows the average birth year of the first and second child for different values of spacing. The average birth year of the first child is about the same regardless of spacing, but the average birth year of the second child increases steadily across the 30-month threshold. The changing pattern in spacing over time means that the second child's birth year is not balanced over the ranges in spacing where the trends in parental leave benefits and take-up of

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[26] The distribution of births when the second child is born in 1996 is skewed to the left. This is because only couples who had their first child in 1994 or later are included in the sample. Thus, the only couples who could have 36 months of spacing in-between children in 1996 are those who had their first child in January 1994.

days are estimated.

Figure A012*b* shows the average benefit level and take-up of PL-days of mothers with the second child over the second child's birth year. Starting in 1998, the average benefit level increased continuously. In part this can be explained by institutional changes in the PL-system.[27] Because the maximum level of benefits is a function of the price base amount, which increases every year, the average level of benefits monotonically increases over time even if income levels do not change. Changes in the labor market is another explanation. During part of the period, there was rapid growth in real wages. The unemployment rate was at its highest level in 1997, after which labor market conditions improved.

At the same time as the average level of benefits grew, mothers' average take-up of PL-days decreased slowly. This reflects a general trend towards a more even division of the parental leave, where the fathers' share of days slowly increased.

Given that the average benefit level increased by about 50 % over the period, the lack of balance in the second child's birth year over spacing is likely to affect the estimated trends in benefit level over spacing. Since the jump in benefit level at the threshold is estimated at the jump between end points of the trends, the estimate of the parameter of interest could be affected. In the following sections I explain how I take this into account when performing the estimations.

6 Results

In this section, graphical evidence of changes in key variables at the 30-month threshold, as well as estimates of those discontinuities, are presented. First, any jumps in the benefit levels of mothers and fathers at the threshold are investigated (i.e. the first stage). If the speed premium affects the benefit level of eligible parents, then the average benefit level among those to the left of the threshold should be higher. If there is a clear jump in the benefit level at the threshold, this jump can be used to investigate how such a change affects the parents' utilization of PL-days. Second, any discontinuities in the parents' take-up of PL-days at the threshold are investigated in a reduced form analysis. Last, second stage estimates of the causal effect of a change in the benefit level of the parents' utilization of parental leave is estimated.

6.1 First stage (mothers)

Figures A013 (a) – (c) in the appendix show mothers' average level of parental leave benefits over spacing; first in daily averages over the nearest 30 days on each side of the threshold (a), then over the closest 180 days (b), and last in weekly averages over the closest 180 days around the 30-months cutoff (c). The figures also contain fitted lines on each side of the threshold that are equivalent to including linear trends in the first stage regression (equation 1). There is a negative jump in the benefit level at the threshold—precisely what one would expect if the speed premium rule leads to a higher level of benefits among those eligible (to the left of the threshold). The jump is clear and large (about 10 SEK) when using the smaller bandwidth, but it is smaller and less convincing when

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[27] The replacement rate was partially decreased between 1996 and 1997, but then increased in 1998 from 75 % to 80 % of the individual's wage. Before the 1'st of July 2006 the cap level was reached at a qualifying income of 7.5 price base amounts. After the 1st of July 2006 the cap is at 10 price base amounts.

using the larger (about 5 SEK). The explanation for the sensitivity to bandwidth becomes apparent when studying the residual plots from the first stage regression which control for the second child's birth year and birth month; figures A014 (a) and (b). As was shown in section 5.3, the frequency of births over spacing changes over time and the average level of benefits increases during the period of study (1994-2009). The increase in average benefit level combined with increase in average birth year of the second child makes any jump in benefit level at the threshold more difficult to detect. However, studying the residuals from the first stage regression when controlling for the second child's birth year and birth month, the jump in benefit level at the threshold is clear both for the smaller and wider bandwidth (figures A014*a* and A014*b*). The residual plot when using the 30-day bandwidth looks about the same as the figure for the raw data, which demonstrates that the jump in benefit level cannot be explained by sorting in timing of the child's birth just around the threshold.

Estimates of the jump in the mother's benefit level at the threshold using different bandwidths are presented graphically in figures 2 (a) – (c). In order to measure the effect of being treated (rather than the effect of *not* being treated), the estimates measure the jump when moving from just *above* the threshold to just *below* it.[28] The observations are weighted using a triangular kernel so that observations near the cutoff are given more weight in the regressions. Figures 2 (a) – (c) below display the first stage estimate when using bandwidths between 7 and 365 days (adding 7 days for each estimation). In panel (a), separate linear trends on each side of the cutoff but no control variables are included. In panel (b), controls for the second child's birth year and birth month are added. In panel (c), a vector of additional control variables is also included namely, mother's and father's age, type and level of education, marital status, region of residence, immigration status, and the first child's gender. Standard errors are clustered at household level. The figures also indicate the optimal bandwidths for the first stage according to Calonico et al. (2014) (CCT, indicated by the dashed line) and Imbens and Kalyanaraman (2011) (IK, indicated and by the dotted line) at 114 and 149 days respectively. When controlling for the child's birth year and birth month, the estimate is fairly stable at around 6-8 SEK for a large range of bandwidths (with the exception of very narrow bandwidths). The confidence intervals become gradually smaller when including more observations. When including all controls, the confidence intervals shrink further, and the estimate decreases slightly and stabilizes at about 5 SEK. The estimate is about the same and statistically significant for the CCT and IK bandwidths. In section 6.4 below, estimates from the most preferred specification are presented in table form.

In conclusion, the first stage graphs and estimates show that there seems to be a discontinuous shift in the mothers' average level of parental leave benefits. This jump becomes apparent when controlling for overall trends in average benefit level and average spacing over time.

When only including observations just around the cutoff, the estimate is smaller, more unstable, and more sensitive to including control variables. This could be because of large variation in benefit levels between individuals, which would make any estimate based on relatively few observations less precise. Another explanation is that measurement errors in the assignment variable

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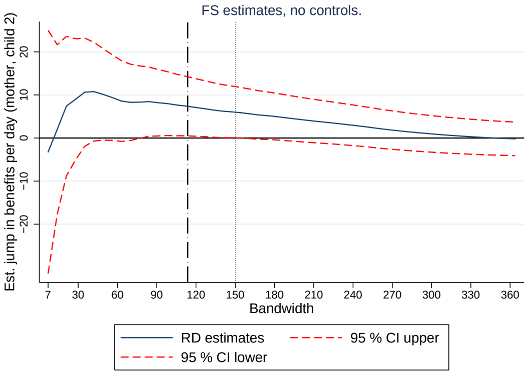
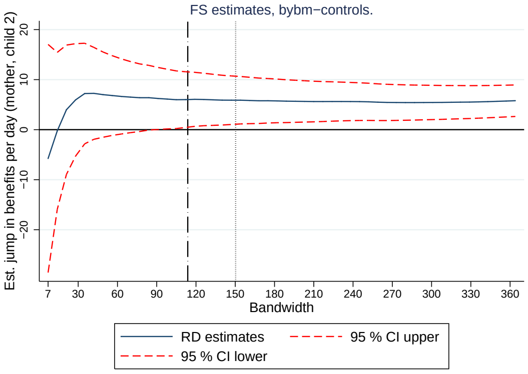
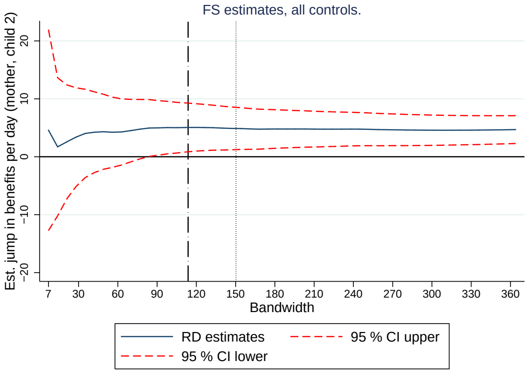
[28] This is opposed to the conventional way of performing RD-estimates; as the jump when moving across the threshold from left to right.



(a) No control variables. Bin size:1 days.



(b) Control variables for 2'nd child's birth year and birth month. Bin size:1 days.



(c) All control variables. Bin size:1 day.

Figure 2: Figures (a) - (c) show the first stage estimate for mothers when using bandwidths between 7 and 365 days. The dashed and dotted lines indicate the bandwidths suggested by the CCT and IK criteria respectively. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level.

(the constructed proxy variable for spacing) diffuse any real jump in the benefit level because observations are assigned to the wrong side of the threshold. When including observations a little bit further from the threshold, more observations with correct treatment assignment are included that will make the estimations of the linear trends more reflective of the true levels among individuals with each treatment status. This reduces the bias induced by the measurement errors.

When including more and more observations in the estimations, the trends are gradually based on more and more observations that are not very near the cutoff. The implications of this are that: 1) the trends are based on more observations where the measurement errors in assignment to treatment status are probably fewer, and, 2) the trends are estimated to a larger extent on observations where eligibility status was known before the child was born. Since I use linear trends (an inflexible functional form), the end points are influenced by observations further away, i.e. couples who knew their eligibility status during pregnancy.

6.2 First stage (fathers)

Next, let's turn to the variation in the fathers' benefit level around the threshold. Figure A015 (a) and (b) in the appendix show the daily averages of fathers' benefit level the nearest 30 days on each side of the threshold and in weekly averages over the nearest 6 months. Figure A016 (a) and (b) show the residual plots when controlling for the second child's birth year and birth month. Neither set of figures shows any clear discontinuity at the threshold. Figure A017 graphically displays the results when estimating the jump in the father's benefits using bandwidths between 7 and 365 days and controlling for the timing of the second child's birth. The estimate is close to zero and far from significant for all bandwidths.

In conclusion, there is no indication that fathers' parental leave benefit level is affected by the speed premium. There are at least two likely explanations for this. First, fathers are less likely to reduce their hours of work and/or earnings after having children, which implies that at the time of the second child's birth they are entitled to at least as high benefits as when the first child was born (and hence the speed premium will not be of any economic significance for them).[29] Second, since mothers usually take the first period of leave with the child, the father will have time adjust his labor supply after the child is born in order to qualify for a higher benefit level in the event that the couple is not eligible for the speed premium.

6.3 Reduced form (mothers and fathers)

As shown above, there are clear indications that the speed premium affects the parental leave benefit level of mothers, but not fathers. In this section, the potential effect of the jump in the mother's benefit level on the parents' take-up of parental leave days is investigated in a reduced form analysis. Figures A018 (a) – (f) in the appendix show the variation in take-up of parental leave days (net QI-days) among couples near the threshold, using bandwidths of 30 days and 6 months, in daily and weekly averages. Figures to the left show the take-up of mothers, figures to the right show that of fathers. If parents' take-up of PL-days is sensitive to the amount of benefits received per day, then we would expect to see a jump in the average number of days taken at the

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[29] Several studies (for example Angelov et al., 2016) as well as official Swedish statistics (SCB, 2016) have shown that men's earnings are less affected by becoming parents then women's earnings.

threshold. In all figures, there is a negative jump in the mother's take-up of PL-days, and a positive jump for fathers, when moving across the threshold from left to right. The jump in levels becomes more distinct when including more data (panels c – f) and when averaging over more days (panels e – f).[30]

Reduced form estimates for mothers and fathers estimated at different bandwidths are presented in figures 3 (a) – (f) below. As before the estimations are performed for all bandwidths between 7 and 365 days, adding 7 days on each side in every regression. The optimal bandwidths according to Calonico et al. (2014) (CCT, indicated by the dashed line) and Imbens and Kalyanaraman (2011) (IK, indicated by the dotted line) when using the mothers or fathers take-up of PL-days as the outcome variable are indicated at 149 (CCT) and 268 (IK) days for mothers and at 184 (CCT) and 298 (IK) for fathers.[31] The figures on the left-hand side show the estimates for mothers and the ones to the right for fathers. In figures (a) and (b), no control variables are included; in figures (c) and (d), controls for the second child's birth year and birth month are added; and in figures (e) and (f), the vector of controls for parental characteristics is included.

The estimate for mothers is large when only including observations in the very near proximity of the threshold, but shrinks and stabilizes for bandwidths of about 3 weeks and beyond. As for the first stage estimate, using a wider bandwidth does not change the magnitude of the estimate much but improves the precision. The jump in the mothers' take-up of PL-days at the threshold is estimated to be around 3 for bandwidths of around 140 days and beyond, including at the CCT and IK recommended bandwidths. When including control variables, the estimate decreases slightly, especially at large bandwidths. It is about 2.5 when including the full set of controls at the CCT bandwidth (149 days) and slightly lower, 1.9, at the IK bandwidth (268 days).

For fathers, the estimate of the jump in the take-up of days on parental leave is negative and statistically significant for (almost) all bandwidths displayed. As was concluded in the previous section (section 6.2), the fathers' benefit level is not affected by the speed premium. Thus, the interpretation of the negative reduced form estimate is that an increase in the mothers' take-up of PL-days (induced by a positive jump in her benefit level) results in a shift downward in the father's take-up of days. The estimate of the jump in the father's take-up of (net) days with parental leave benefits is large for narrow bandwidths. When including data between one and three and a half months, the estimate is relatively stable at 3.5 days. In the interval between the CCT and IK bandwidths, the estimate is at around 2 when including the control variables.

What is striking when comparing the estimates for mothers and fathers is that the shapes of the graphs are so similar, in the sense that one is the inverse of the other. Regardless of choice of bandwidth, the magnitudes of the estimates for the parents are relatively close to each other. The reduced form estimates thus indicate that a jump in one parent's benefit level, in this case the mother's, can induce redistribution of days where the reduction in time on leave is almost as large for the father as the increase is for the mother. Thus, in conclusion, changing the benefit level of

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[30] The average take-up of PL-days doesn't change as much over time as the average benefit level (see section 5.3). Hence, residual plots when controlling for the child's birth time do not differ radically from graphs of the raw data and are therefore not presented.

[31] The CCT and IK optimal bandwidths are derived based on only *one* outcome variable, which is why they are different for the first stage and reduced form for mothers and fathers.

one parent might not only directly affect that parent's time on parental leave, but can also affect the other parent's time on leave. Rather than affecting the total time the couple spends at home with the child, the change in benefit level seems to induce a change in the division of days between parents.

6.4 Second stage: Estimating the effect of the PL-benefit level on take-up of PL-days

Next, a fuzzy regression discontinuity is used to estimate the causal effect of a change in the parental leave benefits level on the take-up of PL-days, using the speed premium rule as an instrument for the mother's level of parental leave benefits. The second stage estimate is numerically equivalent to scaling the reduced form by the first stage estimate. To get accurate confidence intervals, however, the second stage is estimated using two stage least squares.[32] Standard errors are clustered at household level to adjust for any correlation in the error terms at individual and households level across time.Rather than presenting results for many different model specifications and bandwidths, results are first presented for one baseline specification, and results for alternative specifications are then presented as robustness checks (see section 7).

In my baseline specification, I choose a bandwidth of 180 days (6 months). As was apparent in previous sections, the CCT and IK methods each suggest different choices of optimal bandwidth for the first and second stage outcome variables, and for mothers and fathers. However, in fuzzy RD, the same bandwidth must be chosen in both regressions. In addition, to be able to compare estimates for mothers and fathers, the same bandwidth should be used for both. Thus the CCT and IK suggestions in this case give relatively little guidance.[33] The choice of bandwidth is ultimately a choice between increased risk of bias when estimating the trends based on observations further from the cutoff, and better precision when including more data. As was demonstrated in the previous sections, however, the estimates are relatively stable for bandwidths larger than about four weeks. At the same time, the precision improves drastically when using larger bandwidths, especially for the first stage. To achieve high precision, but at the same time avoid bias induced by including many data points far from the threshold, a bandwidth of 180 days is chosen. This is also the smallest bandwidth suggested for fathers (using the CCT criteria). The baseline specification is thus a local linear model with a bandwidth of 180 days (6 months). The vector of control variables for parental characteristics is also included.

6.4.1 Estimates for mothers

Tables 3 - 6 below present OLS, first stage, reduced form and second stage estimates of the direct effect on mothers and fathers, the indirect/cross spousal effect on fathers and the effect on couple level.

Turning first to the results for mothers (table 3), the OLS estimates describe the association between the mother's benefit level and her take-up of parental leave days. The take-up of parental leave days is measured in three ways: first, as the take-up of net QI-days, i.e. the same measurement

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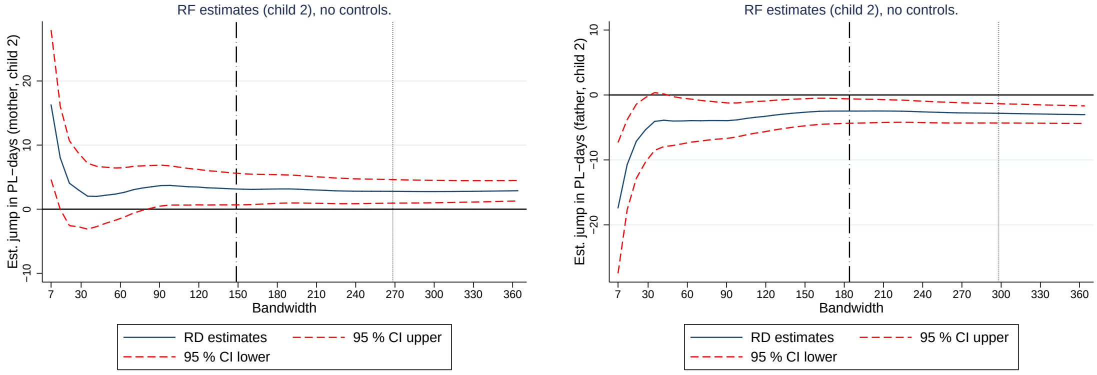
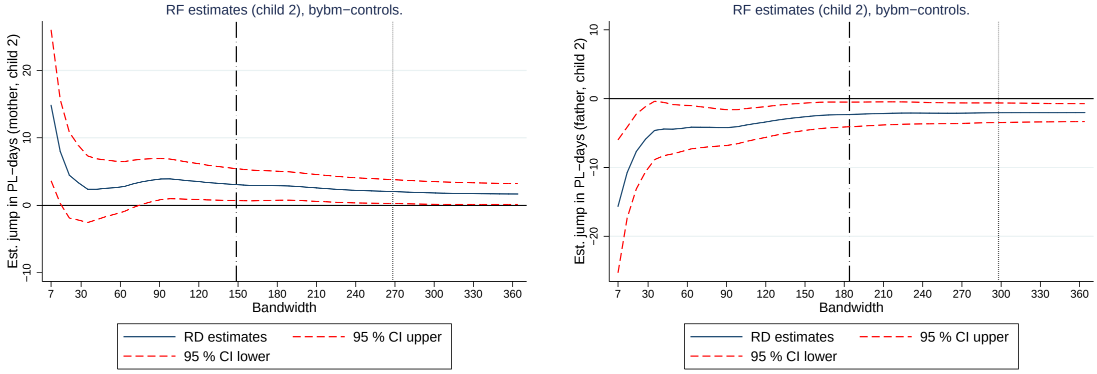
[32] As concluded by Angrist and Pischke (2008), the fuzzy RD design is conceptually equivalent to an instrumental variable strategy.

[33] Both Imbens and Kalyanaraman (2011) and Calonico et al. (2014) suggest that when applied to a fuzzy RD setting, one should calculate the optimal bandwidth based on the second stage outcome variable and use that also in the first stage regression. However, the methods were clearly designed for the case of sharp RD.



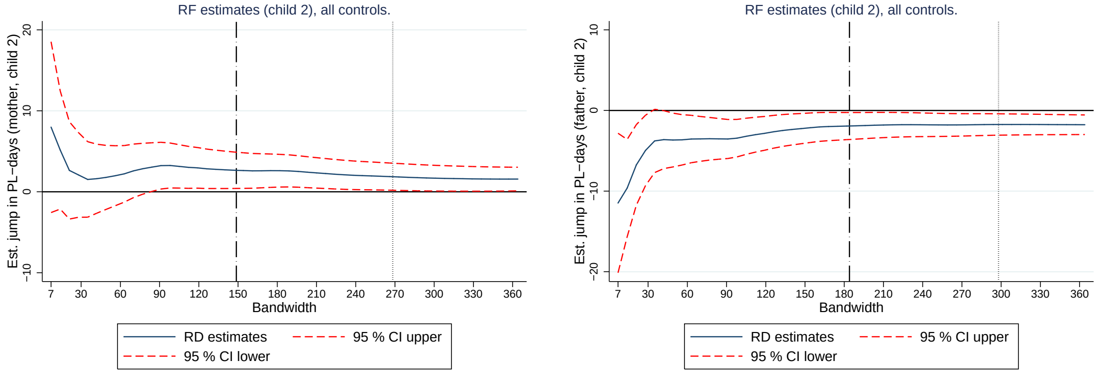
(a) No control variables. Bin size=1 days.

(b) No control variables. Bin size=1 days.



(c) Control variables for 2'nd child's birth year and birth month. Bin size=1 days.

(d) Control variables for 2'nd child's birth year and birth month. Bin size=1 days.



(e) All control variables. Bin size=1 day.

(f) All control variables. Bin size=1 day.

Figure 3: Figures (a) - (f) show the reduced form estimate for mothers and fathers over increasing bandwidths (7 to 365 days). The dashed and dotted lines indicate the bandwidths suggested by the CCT and IK criteria respectively. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level.

as was used in the graphs and reduced form estimates described in section 6.3; second, the total number of PL-days (both at the QI-level and the flat rate days); and third, as the total number of calendar days with any benefits. The table also contains the mean benefit level and the mean values of the three measurements of PL-days. The OLS estimates show that there is a negative correlation between the mother's level of benefit and her take-up of days. This is probably because mothers with higher earnings, who are entitled to higher benefits, are more career oriented and want to spend less time away from the work place. The second column in table 3, marked as FS, present the first stage estimates for mothers of 4.9 when using the baseline specification described above. The estimate is statistically significant at the 1 % level and has an F-statistic of 8.3.[34] The interpretation of this estimate is that the speed premium raises the benefit level of mothers by 4.9 SEK ($\approx$ \$0.54). Given the mean value of the mothers' benefit level (506 SEK $\approx$ \$55), the first stage estimate suggests that eligibility to the speed premium increases the level by about 1 %.

The reduced form estimates indicate that mothers who are eligible for the speed premium take up about 2.6 more net QI-days of parental leave, an increase of about 1 % given the mothers' average take-up of days. There is no additional increase in take-up when also including the flat-rate days. Mothers spread out the 2.6 additional net days over 3 calendar days. This implies that the actual increase in mothers' time spent at home is probably underestimated if one only considers the increase in net days.

Column four presents the second stage estimates, the causal effect of a 1 SEK increase in the benefit level per day on the take-up of parental leave. The interpretation of the estimate is that a 1 SEK ($\approx$ \$0.1) increase in the benefit level per day induces mothers to take about 0.5 more days of parental leave benefits. The second stage estimates are large but not very precisely estimated (significance levels are 10% for the net QI-days and number of calendar days).

An alternative interpretation is that of a cumulative effect; that an increase in the benefit level of 1 SEK per day during 268 days, adding up to a total of 268 SEK ($\approx$ \$30), induced the increase in take-up of 2.6 QI-days.

The estimates for mothers translate into an elasticity of take-up duration (length of spell) with respect to the benefit level of 1.[35] This elasticity measures the percentage increase in the number of days on parental leave due to a 1 % increase in the level of benefits per day. This means that, according to my estimates, the mother's take-up duration is very sensitive to changes in the benefit level. An elasticity of 1 is in magnitude a large estimate, but it is not that different from what has been found before for other types of benefits. For example, Meyer (1990) estimates the take-up duration elasticity of unemployment benefits to be 0.9.[36]

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[34] This is not too far off from the rule of thumb level of 10, suggested by Angrist and Pischke (2008).

[35] The elasticity of duration with respect to the benefit level, using the formula in Gruber (1997), is calculated as:

$$\varepsilon^B = \frac{b}{D}\frac{\partial D}{\partial b} = \frac{506.29}{268.39} \times \frac{2.621}{4.902} = 1.0086 \approx 1.0$$

where D is the duration measured as take-up of net days with benefits (net QI-days) and b is the benefit level per day.

[36] As far as I am aware, there are no previously studies that estimate the take-up duration elasticity for take-up of parental leave benefits.

Table 3: Mothers, direct effects (proxy variable).

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.028***	4.902***	2.621**	0.535*	268.39	55,780
	(0.003)	(1.702)	(1.036)	(0.287)		
Total net days	-0.060***	4.902***	2.595**	0.529	295.59	55,780
	(0.004)	(1.702)	(1.292)	(0.333)		
Calendar days w benefits	-0.059***	4.902***	3.064**	0.625*	303.88	55,780
	(0.004)	(1.702)	(1.396)	(0.370)		
Mean benefit level		506.29				
First stage F-stat		8.30				

Table 4: Fathers, direct effects (proxy variable).

	OLS	FS	RF	Mean	N
Net QI-days	-0.023***	-0.313	-1.957**	49.52	55,690
	(0.003)	(1.600)	(0.864)		
Total net days	-0.038***	-0.313	-1.753*	54.16	55,690
	(0.003)	(1.600)	(0.955)		
Calendar days w benefits	-0.043***	-0.313	-1.844*	57.07	55,690
	(0.003)	(1.600)	(1.020)		
Mean benefit level		602.06			
First stage F-stat		0.04			

Table 5: Fathers, cross spousal effects (proxy variable).

	OLS	FS	RF	SS	Mean	N
Net QI-days	0.022***	4.902***	-1.957**	-0.399*	49.52	55,780
	(0.003)	(1.702)	(0.864)	(0.229)		
Total net days	0.029***	4.902***	-1.753*	-0.358	54.16	55,780
	(0.003)	(1.702)	(0.955)	(0.236)		
Calendar days w benefits	0.029***	4.902***	-1.844*	-0.376	57.07	55,780
	(0.003)	(1.702)	(1.020)	(0.251)		
Mean benefit level		506.29				
First stage F-stat		8.30				

Table 6: Couple level effects (proxy variable).

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.006	4.902***	0.665	0.136	317.91	55,780
	(0.003)	(1.702)	(0.845)	(0.179)		
Total net days	-0.032	4.902***	0.842	0.172	349.75	55,780
	(0.003)	(1.702)	(1.085)	(0.232)		
Calendar days w benefits	-0.030	4.902***	1.221	0.249	360.95	55,780
	(0.004)	(1.702)	(1.241)	(0.271)		
Mean benefit level		506.29				
First stage F-stat		8.30				

Note: The tables contain OLS, first stage, reduced form and second stage estimates of the direct effect on mothers (3), the direct and cross-spousal effect on fathers (4 and 5), and couple level effects (6). Regressions are performed using the proxy variable for spacing, linear trends, a 180 day bandwidth, triangular weights and control variables for the second child's year and month of birth, mother's and father's age, type and level of education, marital status, region of residence, immigration status, and the first child's gender. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level. Stars indicate p-values (p): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.4.2 Estimates for fathers

Table 4 below contains the OLS, first stage, and reduced form estimates for fathers. As concluded in section 6.2, fathers' parental leave benefit levels are not on average affected by the speed premium. The first stage estimate and the F-statistic are close to zero. At the same time, the reduced form estimates indicate that fathers in eligible couples take almost 2 PL-days less than those in non-eligible couples. In magnitude this decrease is about 75% of the increase of mothers, and equivalent to a 4% reduction in the father's total take-up of net QI-days. The reduced form estimates for fathers indicate that a jump in the mother's take-up of PL-days, in this case induced by an increase in her benefit level, can lead to an almost as large jump of the fathers in the opposite direction.

Table 5 contains the cross-spousal estimates of the association between the benefit level of the mother on the father's utilization of parental leave. The OLS estimate indicates that the correlation between the mother's benefit level and the father's take-up of PL-days is positive. This is in line with descriptive statistics that show that couples where the woman's earnings are in the upper part of the distribution divide their parental leave more equally. The first stage estimates indicate the value for mothers (same as in table 3) while the reduced form indicates the estimates for fathers (same as in table 4). Column four contains the second stage estimates of the effect of a change in the *mother's* benefit level on the *father's* take-up of PL-days. The estimate of -0.399 for the take-up of net QI-days is significant at the 10% level and in magnitude about 75% of that of the mother. The significant second stage estimate for fathers confirms that changing the benefit level of one parent, in this case the mother, not only induces a change in that parent's take-up of PL-days, but can also lead to a change in the division of days between parents. For fathers, the reduction in calendar days with benefits seems to be on par with the reduction in net days. The magnitude of father's second stage estimate for the number of calendar days with benefits is about 60% of the corresponding estimate for mothers.

6.4.3 Estimates at couple level

Table 6 below contains the estimates of the association between the mother's parental leave benefit level and the couple's total take-up of PL-days. As indicated by the OLS estimates, there is no correlation between the mother's benefit level and the couple's total take-up of PL-days. Thus, the correlations shown between the mother's benefit levels and each parent's take-up of days (discussed above) seem to originate only from difference in the division of days between couples at different ranges of the income distribution of mothers. The second column replicates the first stage estimate for mothers. The reduced form and second stage estimates indicate that an increase in the mother's level of benefits leads to, at most, a small increase in the total take-up of net QI-days. The reduced form estimate is about 2/3 of a day which is equivalent to an increase of 0.2% in the couple's total take-up. The reduced form estimate for the couple's total number of calendar days with benefits is about twice as large at 1.2 which is equivalent to an increase of 0.3 %. These estimates are equivalent to take-up duration elasticities at the couple level of 0.22 for the total take-up of net QI-days with respect to the mother's benefit level. This is much lower than the elasticity of mothers which was calculated to be 1 (see section 6.4.1). The conclusion is that the mother's take-up of parental leave, and the couple's division of days, seem to be very responsive to changes in the benefit level, but that the couple's total take-up is relatively irresponsive to changes in one parents benefit level.

7 Robustness checks

In section 6.4 above, estimates when using the baseline specification with linear trends, a 180-day bandwidth, a vector of control variables and triangular weights were presented. In this section, the stability of these estimates is tested through re-estimation with alternative specifications. Robustness checks are performed for the direct effect on mothers (table 3) and the cross-spousal effect on fathers (table 5).[37]

Tables A02 and A03 in the appendix contain estimates on the direct effect for mothers and the cross-spousal effect for fathers for some alternative specifications and sensitivity checks. The top row in each table replicates the baseline estimates for the mother's first stage, and reduced form and second stage estimates for each parent for the three measurements of take-up of PL-days.

First, the sensitivity of the estimates to including different sets of control variables is tested. The second row in each table contains results when all control variables are dropped from the baseline model (the child's birth year and month and the X_i vector). Showing the estimates without any controls is important for transparency. However, because of the trends in the outcome variables over time, shown in section 5.3, not controlling for when the child was born makes the estimates less trustworthy. The third row contains estimates when only including controls for the second child's birth year and birth month.

Rows four and five re-estimate the baseline specification using alternative constructions of the proxy variable for spacing (and thus different samples). The first alternative proxy uses only information on the father's daddy-days to measure spacing. This reduces the sample size by about 1/4. The second uses the baseline proxy, but with the amendment that in the cases where the parents did not use their first PL- or daddy-day during the child's birth month, a randomly selected day in the child's birth month is used as a proxy for the child's birthday. The motivation for doing this is that using information from all data points can improve the estimations of the trends in the outcome variable. The downside is that using a random day as a proxy creates noise in the variable. Since many couples whose child was born close to the cutoff are going to be assigned to the wrong side of it, this method risks to cancel out some of the actual jump in the variable.

Next, the baseline results are re-estimated using the bandwidths recommended by Calonico et al. (2014), marked CCT, and Imbens and Kalyanaraman (2011), marked IK in the tables.[38]

Last, quadratic and cubic trends are included to allow for more flexible trends on each side of the threshold. As discussed in section 3, the proportion of couples with an expected due date before the threshold decreases continuously across the threshold. Thus, to the left of the threshold the proportion of eligible couples is 100%, out of whom a decreasing proportion are eligible because

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[37] Since there is no first stage effect for fathers there is no further investigation of the direct effect on them. Couple level effects are the sum of the effects on each parent. Thus, it is sufficient to test the stability of the effect on each parent.

[38] These criteria were created with sharp RD in mind and make separate recommendations for each outcome variable. However, to be able to compare the results for mothers and fathers and perform second stage estimations, all regressions are performed using the recommended bandwidth for mothers' reduced form outcome variable; take-up of net QI-days.

of their expected due date, and to the right of the threshold the proportion of eligible couples decreases continuously. If eligibility implies a higher benefit level, then it is possible that a linear approximation for the trend in benefit level over spacing is not the correct one just around the threshold. Even if linear trends are a reasonable assumption near the cutoff, it might be that a more flexible functional form can better describe the data further away from the cutoff.

Let's turn first to the stability of the first stage estimate (the jump in the mother's benefit level). The magnitude of the estimate is remarkably similar in most specifications (around 5 SEK $\approx$ \$0.54), although it loses precision in some. All the first stage estimates are within one standard error of the baseline. A few specifications change the estimates more than others: Adding the vector of controls for parental characteristics decreases the FS-estimate by 15 % compared to only controlling for timing of the child's birth. When using the "daddy-day" proxy, the first stage estimate is about 80% in magnitude of the baseline. When including all observations by adding a random day as a proxy for the child's birthday for observations that could otherwise not be included, the first stage estimate decreases to about 75% of the baseline. The reduction of the estimate could be explained by the fact that, with this method, some couples are assigned to the wrong side of the cutoff, which to some extent nullifies any jump in levels at the threshold.

The results when using the CCT and IK criteria confirmed what the graphical examination revealed (section 6.1), namely that the first stage estimate is stable over a wide range of bandwidths. Finally, introducing more flexible functional forms in the regression model hardly changes the estimate. This result is reassuring since it implies that using linear trends is not unreasonable.

When it comes to the reduced form estimates, most of the estimates are consistently lager or close to the baseline estimate. The exception is when using the alternative bandwidths and quadratic and cubic specifications for which some of estimates for mothers have a smaller magnitude. The reduced form estimates are less stable than the first stage. However, since they are less precisely estimated in the baseline model, perhaps this is not that surprising. Most of the sensitivity checks produce estimates that are within one standard deviation of the baseline estimate. The exceptions are some results for the models with more flexible functional forms: number of calendar days for mothers, and the estimates for all outcome variables for fathers. However, except for the estimates for fathers when adding a third order polynomial, these estimates are within the confidence intervals of the baseline reduced form estimates. It is well known that RD-estimates are sensitive to including higher order polynomials. A drawback of using higher order polynomials is that extreme values close to the intercepts could have a great influence on how the trend curves close to the end points and thus have a great influence on the estimates. In the baseline specification linear trends are used, but weights are imposed to give more importance to observations near the cutoff. Since the first stage estimate when using the flexible functional forms is very close to that in the baseline, this suggests that a linear model is reasonable.

The second stage estimates lose precision in many of the sensitivity tests. However, all but a few estimates are within one standard deviation of the baseline estimates.[39]

To conclude, the baseline estimates seem to be reasonably stable to the exclusion of control

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[39] The exceptions are the estimate for mothers when using the daddy-days proxy, and the models with flexible trends for fathers.

variables, using alternative measurements of the running variables, using other bandwidths, and excluding observations very near the cutoff. Crucially, the first stage estimate is particularly stable in magnitude. The reaction to the change in the mother's benefit level is more difficult to estimate precisely.

8 Placebo tests

8.1 Placebo thresholds

To further investigate if the estimates presented above are truly caused by the speed premium (or just by chance), the main specification for the mothers' net QI-days on leave is re-estimated using a number of placebo thresholds. Table A08 in the appendix contains first stage (FS) and reduced form (RF) estimates when, in addition to the (real) threshold at 30 months of spacing, estimating these parameters at all turns of the month between 25 and 35 months after the first child's birth. The estimates for the (true) 30-month cutoff is the same as in table 3 above.

Reassuringly, all of the placebo estimates are smaller in magnitude than those at the true threshold and many are close to zero. None of the first stage estimates at placebo cutoffs, and only one of the reduced form estimates, is statistically significant (at the 10 % level). The significant estimate is not too worrisome since, when using the 10 % significance level, one would expect two placebo estimates out of twenty to be significant just by chance. The same estimates, with 95 % confidence intervals, are presented again in figures A020 (a) (first stage estimates) and (b) (reduced form estimates).

8.2 Placebo first stage (mothers)

As an additional placebo test, the variation in the mother's benefit level when on parental leave with the first child is investigated. Since the PL-benefit level is based on the parent's previous earnings, more specifically the parent's qualifying income, the level when on leave with the first child should not be affected by spacing between the first and second child (see section 2). Thus, if there is any discontinuity in the benefit level with the first child at 30 months of spacing, that would indicate that parents, with specific levels of qualifying income before the first child was born, sort into being eligible for the speed premium. For example, parents with a high QI at the time of their first child's birth might aim to have their second child within 30 months so that they can base their benefit level with the second child on their old QI level. If so, the jump in level with the second child (the first stage estimate) could be caused by sorting, rather than by randomized eligibility status.

Figures A021 (a) and (b) show the mother's average PL-benefit level with the *first* child over spacing; a "placebo" first stage. Figures A022 (a) and (b) display the residuals when estimating the first stage regression, but now with the benefit level with the *first* child as the outcome variable, and controlling for the child's birth year and birth month. In the raw data, there is a negative jump when using a narrow bandwidth and a positive jump when using the larger. These discontinuous jumps diminish significantly when controlling for the child's time of birth, as demonstrated in the residual plots.

Figure A023 shows the estimates and confidence intervals when estimating the jump in the mother's benefit level with the first child, using different bandwidths and controlling for the child's birth year and birth month. The estimate is close to zero for most bandwidths and never significant. There is thus no indication that couples sort around the threshold based on the mother's benefit level with the first child (or qualifying income at the time of the first child's birth).

9 Results when using monthly data to measure spacing

In the previous sections all estimations were performed using the constructed proxy variable for spacing in which the father's first daddy-day or the mother's first PL-day is used as a proxy for the child's birthday. There are at least two drawbacks to this strategy. One is that not all observations can be included in the sample since in about 25% of the cases the parents did not use any of their daddy-days or PL-days during both children's birth months (see section 4.2.1). The other is that since this proxy is not always accurate, some observations are assigned to the wrong side of the threshold (as discussed in section 5.1).

As a complement to the analysis using the proxy variable, this section contains graphical evidence and estimation results when instead using only the child's birth month to calculate spacing between children. Since children's birth months are known for all children, behavioral differences when it comes to take-up between different groups of parents cannot systematically influence this measurement. By only using information which is defined in the same way for all observations, the calculation of spacing does not differ systematically between different groups of parents. For example, differences in how fathers with different characteristics use their daddy-days cannot systematically assign these fathers to a specific side of the threshold. Thus, this strategy eliminates at least one potential source of systematic measurement error. Also, by using the crude measurement of spacing, all observations can be included in the sample. This improves the external validity of the estimates.

When using this crude measurement of spacing, the identification strategy is different. Identification here rests on the assumption of randomization in the date of conception, which leads to randomization in the number of months between children. As discussed in section 4.2, the casual effect of a change in the benefit level on the parents' take-up of parental leave can be estimated by exploiting the jump in proportion of eligible couples between those with 30 and 31 months of spacing. Since spacing is measured as the number of months that passes between the child's birth month (month "zero") and the month when the second child is born, this measurement of spacing is sometimes one unit to small, but never larger than the actual number of months that has passed in-between the children's births. Among observations with calculated spacing equal to 30, about 50 % are eligible because the second child's expected due date was within 30 months, a smaller proportion are eligible because of an early birth, and the rest have an expected due date and an actual birth that was beyond the threshold. Thus, among couples with calculated spacing equal to 30, there will be those whose actual spacing was less than 30 months, and those whose actual spacing was more than 30 months. Among those with spacing equal to 31, on the other hand, all couples will have at least 30 months between their children, i.e. none of them will be eligible because of an early birth. However, some proportion of the couples with calculated spacing equal to 31 are eligible because the expected due date of their child was within the 30 month threshold (but the actual birthday came later). This implies that there is a discontinuous jump in the proportion of eligible couples when moving from month 30 to month 31.

Thus, with this strategy, the jump in outcome variables is not estimated *at the threshold*. Comparing observations with spacing equal to 30, to those with spacing equal to 31, the difference in "treatment" is mostly that more observations in month 30 knew during their second pregnancy that they were going to be eligible to the speed premium (because of the child's expected due date). Any estimate of a difference in take-up among couples in month 30 to those in month 31 comes mostly from a difference in behavior between couples who knew that they would be eligible and couples who knew that they would probably not be eligible. This is different from estimations performed in previous sections, where the estimates are evaluated at the threshold and thus the jump in proportion of eligible comes from couples who become eligible due to an early birth.[40]

Figures A019 (a) – (d) in the appendix display the mothers' average benefit level (a), the residuals when controlling for the second child's birth year and birth month (b), and the mothers' and fathers' average take-up of PL-days (c) and (d). The graphs display the values for observations with spacing between 25 and 36 months: six months before and after the month 30 and 31.

In the graph of the raw data, no jump in the mothers' benefit level is present. However, when controlling for the child's birth year and birth month, there is a clear shift in the mothers' level at the threshold.[41] Turning to the graphs for the parents' take-up of days with benefit, there is also a clear shift in the general level, both for mothers and fathers.

Tables A04 - A07 in the appendix show the results when estimating these discontinuities in a regression analysis similar to the one in section 6.4. As in section 6.4, a bandwidth of six months, linear trends, and triangular weights are used. Table A04 presents OLS, first stage, reduced form, and second stage estimates for mothers. Since all observations can be included, the sample size is larger than when using the proxy variable. Although the discontinuities in the outcome variables are now evaluated by comparing by comparing observations in month 30 and 31 (not at the threshold), the estimates are remarkably similar to those when using the proxy variable. The first stage estimates indicate that the jump in benefit level when moving from month 30 to 31 is about 4.8 SEK, almost the same as when evaluating the jump at the threshold using the proxy variable. The F-statistic is slightly larger at 9.73 (compared to 8.3 when using the proxy). The reduced form and second stage estimates are also very similar in magnitude to those in table 3. The estimates are more precisely estimated which could be a result of using a larger sample size.

Table A05 presents the results when estimating the direct effect on fathers.[42] There is a negative correlation between the father's level of benefits and his take-up of days, but no jump in benefit level when moving from month 30 to 31. The reduced from is somewhat smaller for the number of net QI-days than when using the proxy variable: about 70% in magnitude, -1.388 compared to -1.957. The other estimates are fairly similar. Since there is no first stage jump in the father's

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[40] At the threshold, the proportion of couples with an expected due date before the cutoff decreases continuously. The jump in proportion of eligible couples originates from the fact that some couples whose expected due date was beyond the threshold had an early birth, which made them eligible. Treatment, in that case, is to unexpectedly become eligible.

[41] To make these estimations as comparable as possible to those in section 6, the same specification is used here. Linear trends are included in all regressions and the observations are weighted using triangular weights.

[42] The sample size is slightly larger here because fathers who did not use any PL-days and could therefore not be included in the first stage regression.

benefit level, the second stage is not estimated.

Table A06 presents the cross-spousal effect on fathers by a change in the benefit level of the mother. As in table 5 in section 6.4, the OLS estimates indicate that there is a positive correlation between the mother's benefit level and the father's take-up of parental leave days. Columns two and three in the table reproduce the first stage estimate for mothers and the reduced form estimate for fathers. The second stage estimates the relationship between a change in the benefit level of the *mother* and the *father's* take-up of PL-days; the cross-spousal effect. Because of the smaller reduced form estimate, the second stage estimate for the take-up of net QI-days is somewhat smaller than when using the proxy variable.

Finally, table A07 presents estimates at couple level. The OLS estimate indicates the correlation between the mother's benefit level and the couple's total take-up of PL-days. The second column again reproduces the first stage estimate for mothers, and the reduced form indicates the jump in the couple's total take-up of PL-days at the cutoff between month 30 and 31. The second stage estimate indicates the causal effect of a change in the mother's benefit level and the couple's total take-up of days. Because the jump in the father's take-up of days is smaller, the reduced form and second stage estimate for the couple's total take-up of net QI-days is larger than when using the proxy-variable. The estimates for the total number of calendar days with benefits is very similar to the one in table 6, section 6.4.

In conclusion, the results when using the crude measurement of spacing based only on the child's birth month (not day) are very similar to the results when using the proxy-variable for spacing, where I use a proxy for the child's birth day. This is noteworthy since, as discussed in section 4.2.2, treatment is not defined in quite the same way when using the two different measurements of spacing. When using the proxy variable, any discontinuity in the take-up of days at the threshold originates from behavioral responses among couples who became eligible for the speed premium due to an early birth of their second child. Since these couples had an expected due date that was beyond the threshold, they did not expect to be eligible. Thus their response to the higher benefit level could not have been planned during the second pregnancy. When using the monthly spacing variable, on the other hand, the estimated jump in take-up of PL-days is an estimate partly of a response among those who are unexpectedly eligible, but mostly, a response among those whose expected due date was within the time frame for eligibility and who therefore knew during the second pregnancy that they would benefit from the premium. The response from these couples could potentially be different from the one when unexpectedly receiving a higher benefit level. For example, parents who know that they will be eligible might feel less compelled to continue to work during the second pregnancy. This could in turn affect the parent's opportunities to return to work after going on leave, which could prolong his or her length of leave. However, the estimates of the response, whether estimated at the threshold using the continuous proxy variable, or by comparing couples in month 30 and 31, are remarkably similar. This suggests these two types of "treatment" evoke very similar responses.

The fact that the same results are reached when using an alternative method strengthens the conclusions that were drawn from the baseline estimates, namely the following: The speed premium rule does affect the benefit level of the mother (but not the father) by shifting it upwards.

This positive shift induced the mother to spend about half a day more of benefits for every unit of increase (1 SEK). The change in the mother's benefit level not only makes the mother take more days, but induces a redistribution of days between the parents so that the father will end up spending fewer days on leave. On the whole, therefore, the couple's total take-up of days only changes marginally by 0.3 %.

10 Concluding discussion

This paper investigates the impact of the parental leave benefit level on the take-up and division of parental leave among parents in Sweden. The "speed premium" rule in the Swedish parental leave system is exploited in order to estimate the causal effect of a change in the benefit level on parents' utilization of parental leave. This rule permits parents who have their second child within 30 months of the first, to base their benefit level when on leave with their second child on their earnings before the *first* child was born, in practice significantly increasing the benefit level for many parents. Assuming that parents cannot precisely control when their second child is born, the rule creates exogenous variation in benefit level among parents whose child was born just before and just after the threshold. A fuzzy regression discontinuity design is applied to estimate the effect of the shift in benefit level at the threshold on parents' take-up and division of parental leave.

My results indicate that the speed premium does affect the level of benefits for mothers, but not for fathers, and that the relative benefit levels of parents can induce a strong response in their take-up behavior. Eligibility to the speed premium increases mothers' average benefit level by about 5 SEK ($\approx$ \$0.54) or about 1%. This shift in levels induces mothers to increase their take-up of parental leave by 2.6 days which is equivalent to an about 1% increase in the mother's total number of parental leave days. These estimates translate into a take-up duration elasticity equal to 1 for mothers. In other words, a 1% increase in the benefit level leads to a 1% increase in the take-up of net parental leave days. This is a large elasticity that has, to the best of my knowledge, not been estimated before in a causal setting.

Further, my results show that the change in the mother's benefit level does not just affect her behavior, but also induces a response among fathers as well. It turns out that the shift in the mother's take-up of leave days causes an almost as large decrease in the father's take-up of leave days. A 2.6 day increase in the mother's take-up is found to reduce the father's take-up of parental leave days by about 75% of that time: 1.9 days.

This result suggests that the parents' decisions on how many days to spend on parental leave are interdependent, and that if a shift in one parent's time on parental leave is induced, the duration of the other parent's leave will also be affected. The fact that the reduction in the father's number of days so closely follows the increase among mothers suggests that couples first decide of how many days of leave to take (for example, all days) and then on how to divide the days. The large effect on fathers' take-up behavior is worth highlighting, especially since the change in take-up is induced without any direct economic incentive for fathers. A 1.9 day decrease in take-up is equivalent to a 4 % reduction in fathers' total time on leave since the average take-up for fathers is 49 days, a large effect.

Another finding is that mothers and fathers seem to utilize the parental leave in different ways.

Mothers, to a higher degree than fathers, tend to spread out their benefits over more calendar days in order to prolong their time at home. A 2.6 day increase in mothers' take-up of parental leave days leads to a total of 3 calendar days more with benefits. For fathers the change in take-up of *net* days and the change in calendar days with benefit is essentially the same. The conclusion is thus that the speed premium rule induces a more uneven division of parental leave between parents than would otherwise have been the case.

My results have wide policy implications as family leave policies are present in many countries and others, like the US, are debating implementation. The results in this study suggest that policymakers should carefully consider the reimbursement level in parental leave systems as it seems like parents' behavior is very sensitive to the precise level of benefits. As concluded in this paper, it is not just the individual's own level that is important, but the parents' *relative* benefit levels also seem to have an impact on their decision of how to divide the time spent at home with the child.

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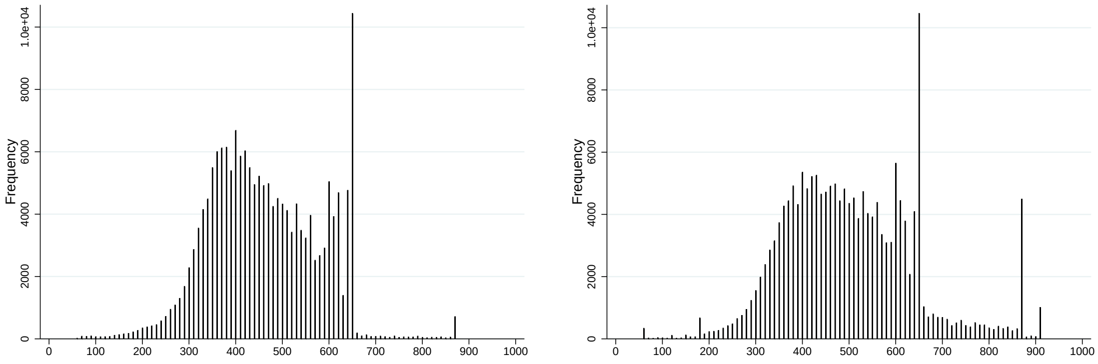
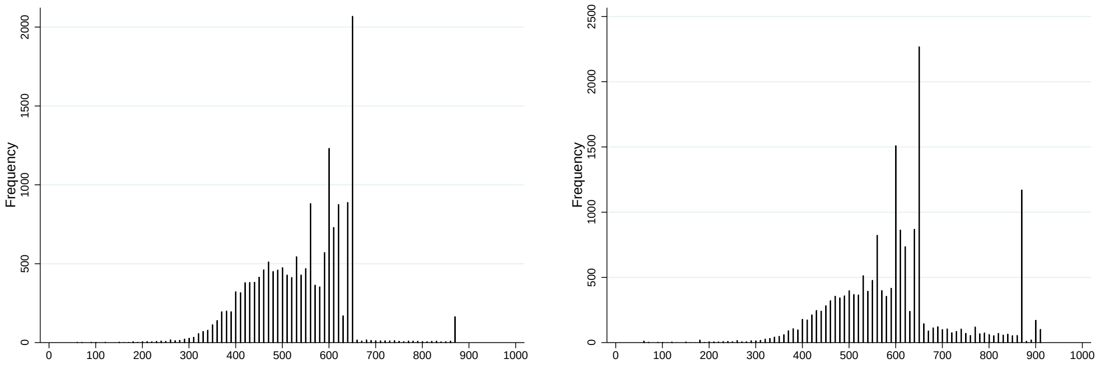
Appendix



(a) PL-benefit level, 1’st child, mothers.

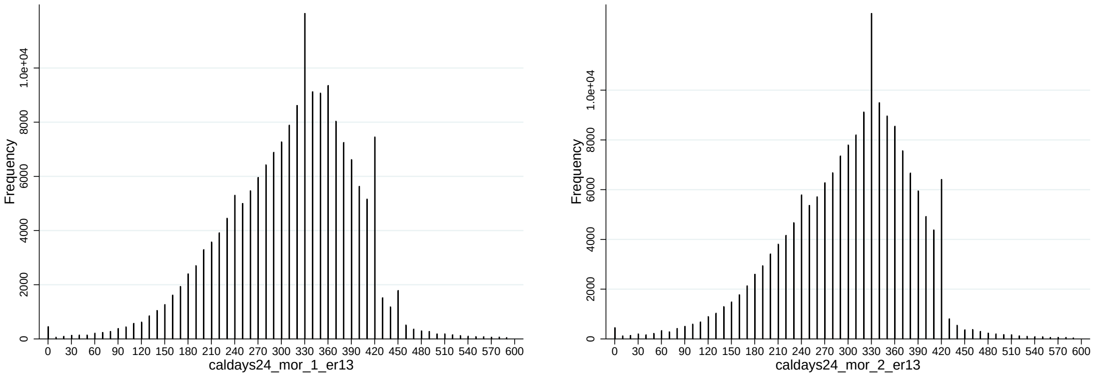
(b) PL-benefit level, 2’nd child, mothers.



(c) PL-benefit level, 1’st child, fathers..

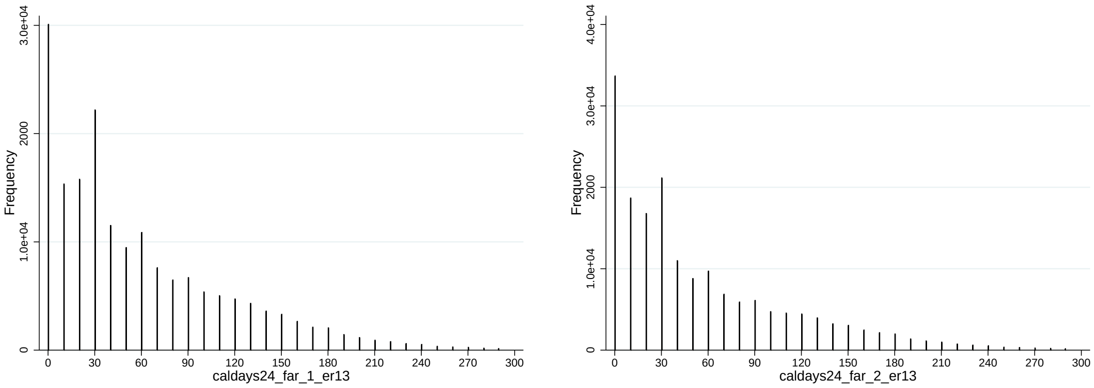
(d) PL-benefit level, 2’nd child, fathers.

Figure A01: Distribution of the PL-benefit level of mothers and fathers with the first and second child.



(a) PL-days, 1'st child, mothers.

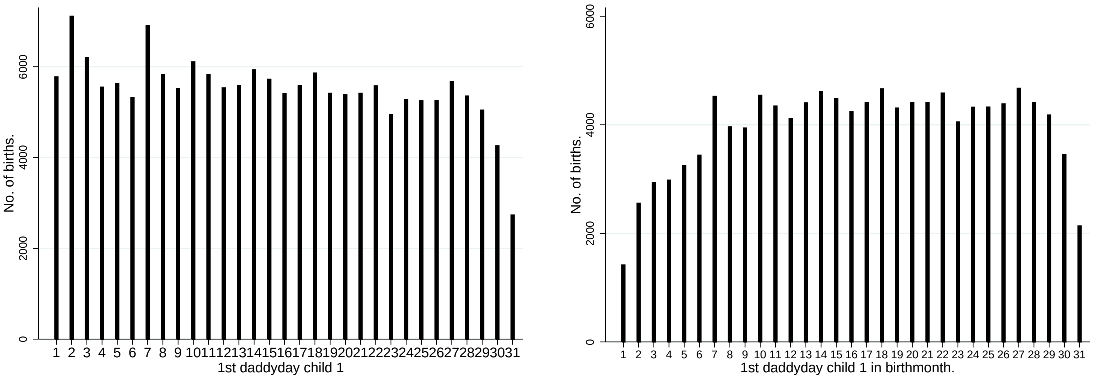
(b) PL-days, 2'nd child, mothers.



(c) PL-days, 1'st child, fathers.

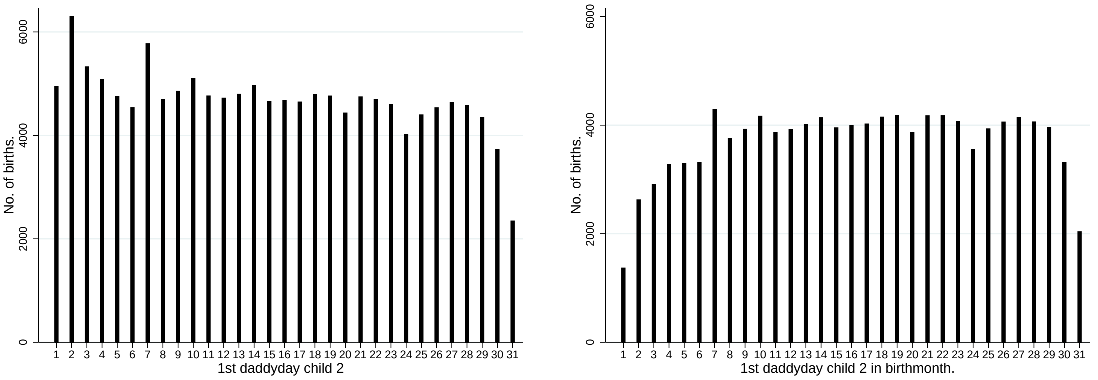
(d) PL-days, 2'nd child, fathers.

Figure A02: Distribution of the take-up of PL-days of mothers and fathers with the first and second child. Notice the difference in scales on the histograms for mother and fathers.



(a) Father's first PL-day with first child.

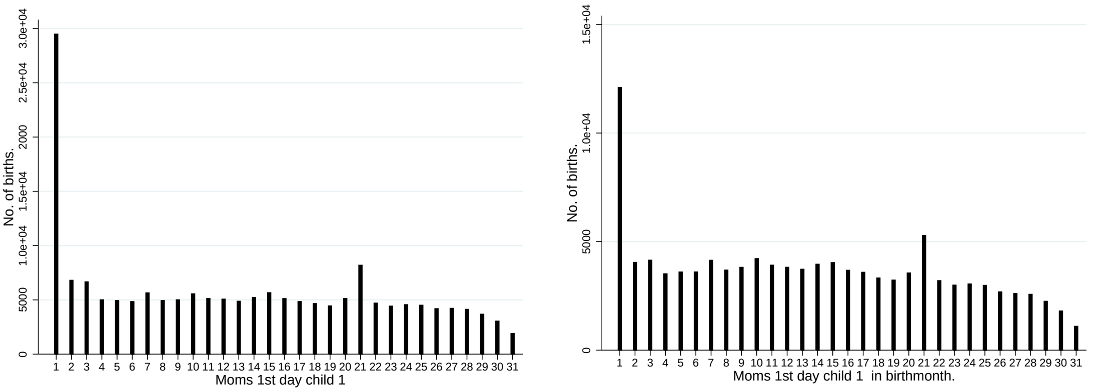
(b) Father's first PL-day with first child is in child's birth month.



(c) Father's first PL-day with second child.

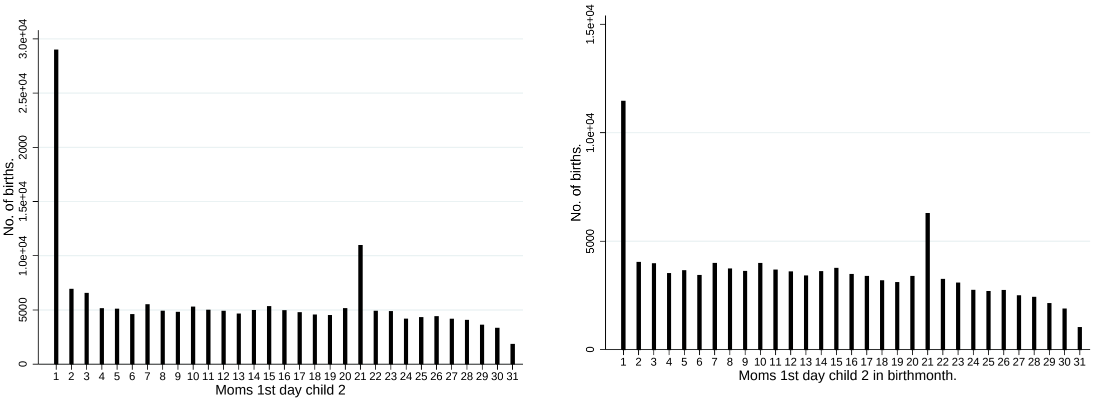
(d) Father's first PL-day with second child is in child's birth month.

Figure A03: Distribution of father's first "daddy day" with first and second child respectively over calendar days 1-31. All observations and only those where the first daddy day was in the child's birth month.



(a) Mother's first PL-day with first child.

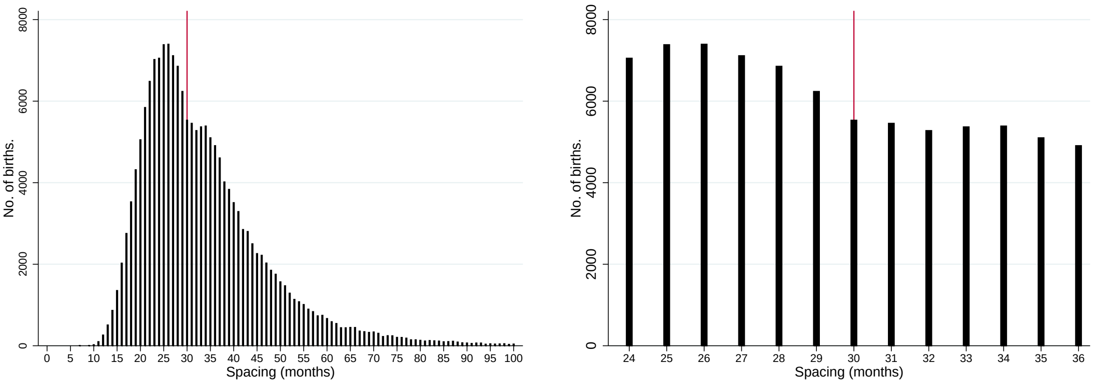
(b) Mother's first PL-day with first child is in child's birth month.



(c) Mother's first PL-day with second child.

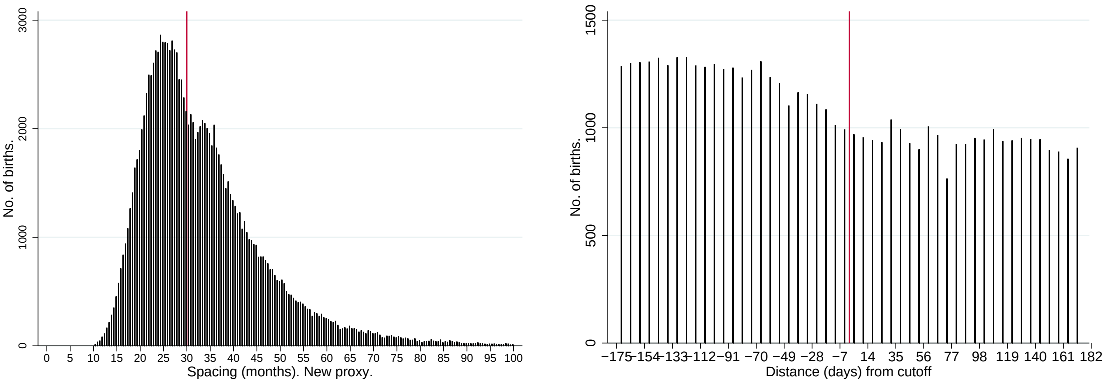
(d) Mother's first PL-day with second child is in child's birth month.

Figure A04: Distribution of mother's first day on PL with first and second child respectively over calendar days 1-31. All observations and only those where the mother's first day was in the child's birth month.



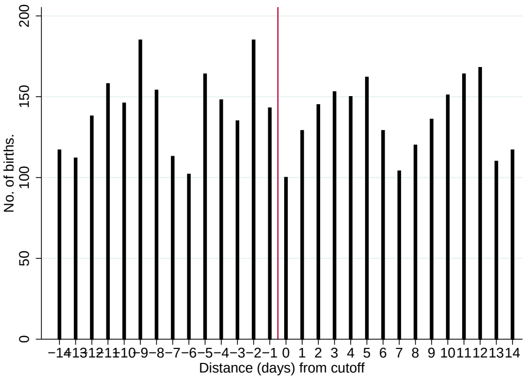
(a) Frequency of births: 0-100 months of spacing. (b) Frequency of births: 24-36 months of spacing.

Figure A05: Frequency of births over child spacing (in months) between first and second child's birth calculated using the children's birth year and month. Exact date of birth is not available in the data. There is no apparent discontinuity in the frequency of births around the 30-month threshold.



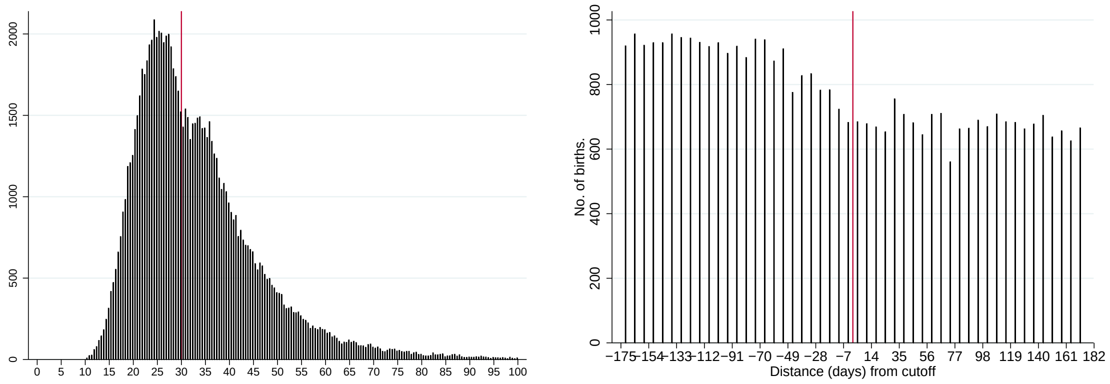
(a) Frequency of births: 0-100 months of spacing (proxy).

(b) +/- 175 days from 30-month threshold. Bin size=7 days.



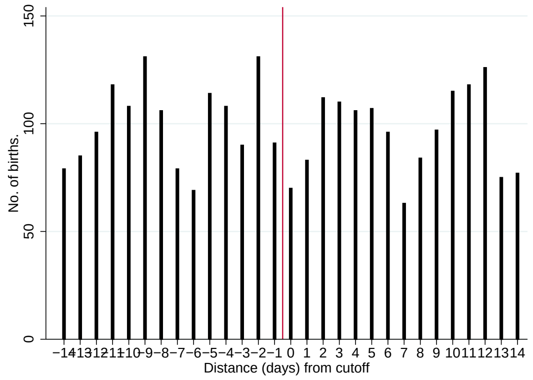
(c) +/- 14 days from 30-month threshold.

Figure A06: Figures (a) - (c) show the frequency of births over spacing were information on the fathers' first "daddy day" with the first and second child, and the mother's first PL-day with the first and second child, has been used to calculate spacing between children.



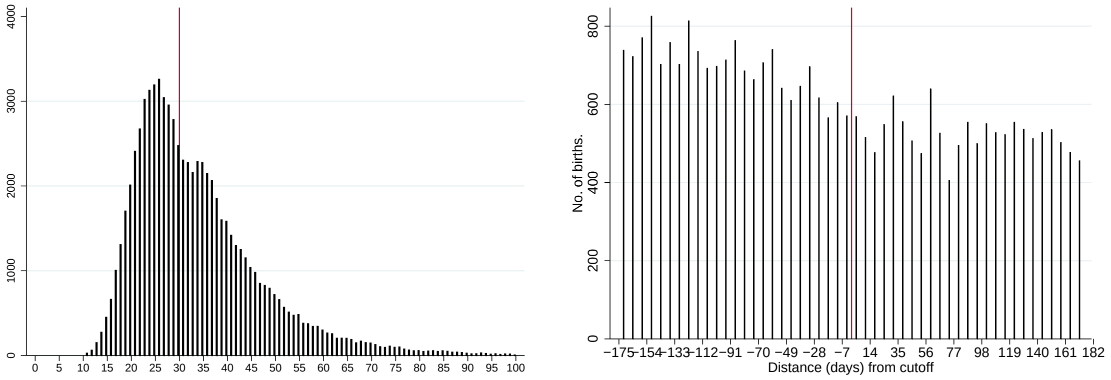
(a) Frequency of births: 0-100 months of spacing (daddy days proxy).

(b) +/- 175 days from 30-month threshold. Bin size=7 days.



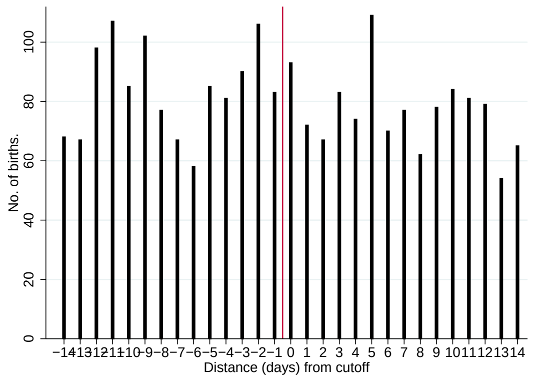
(c) +/- 14 days from 30-month threshold.

Figure A07: Figures (a) - (c) show the frequency of births over spacing measured as time between the fathers' first "daddy day" with the first and second child.



(a) Frequency of births: 0-100 months of spacing (mother's first PL-day proxy).

(b) +/- 175 days from 30-month threshold. Bin size=7 days.



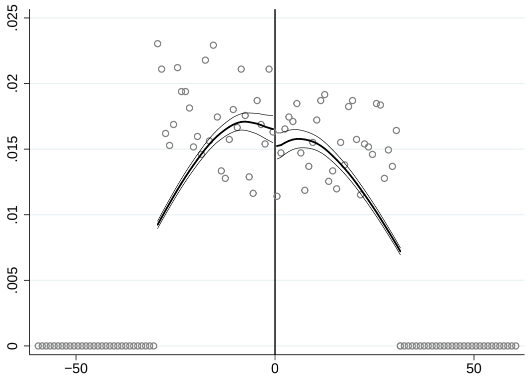
(c) +/- 14 days from 30-month threshold.

Figure A08: Figures (a) - (c) show the frequency of births over spacing measured as time between the mother's' first PL-day with the first and second child.

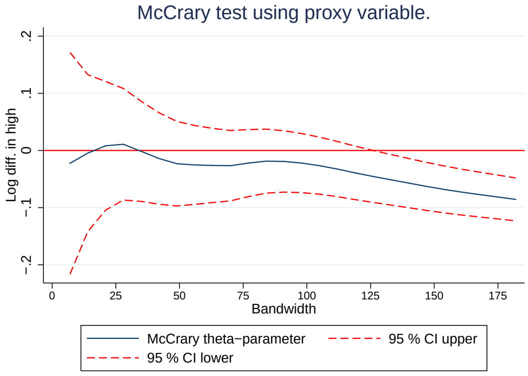
Table A01: Descriptive statistics and covariate balance test.

	Population mean	Mean near cutoff	Jump at cutoff
Mothers education	13.8	14.1	-0.1
	(2.5)	(2.5)	(0.1)
Fathers education	13.2	13.5	-0.0
	(2.5)	(2.6)	(0.1)
Mothers income	185,529.3	193,844.9	198.8
	(98,392.2)	(101,067.4)	(5,002.4)
Fathers income	239,288.2	243,629.6	-2,138.8
	(129,421.4)	(131,732.6)	(6,570.3)
Mothers age	27.4	27.5	0.3
	(3.8)	(3.8)	(0.2)
Fathers age	29.4	29.4	0.1
	(4.2)	(4.1)	(0.2)
Percent married	31.4	32.8	2.3
	(46.4)	(46.9)	(2.2)
Percent girls - 1st child	48.7	48.4	-2.3
	(50.0)	(50.0)	(2.4)
Percent girls - 2nd child	48.6	49.1	2.4
	(50.0)	(50.0)	(2.4)
Immigrant mother (percent)	8.6	7.5	-0.1
	(28.1)	(26.3)	(1.3)
Immigrant father (percent)	8.4	7.7	0.2
	(27.8)	(26.7)	(1.2)
1st child birth year	2,000.3	2,000.6	0.1
	(3.9)	(3.9)	(0.2)
1st child birth month	6.2	6.5	0.0
	(3.3)	(3.4)	(0.2)
2nd child birth year	2,003.1	2,003.1	0.1
	(3.9)	(3.9)	(0.2)
2nd child birth month	6.1	6.1	0.0
	(3.3)	(3.3)	(0.2)
N	133,075	8,768	8,768

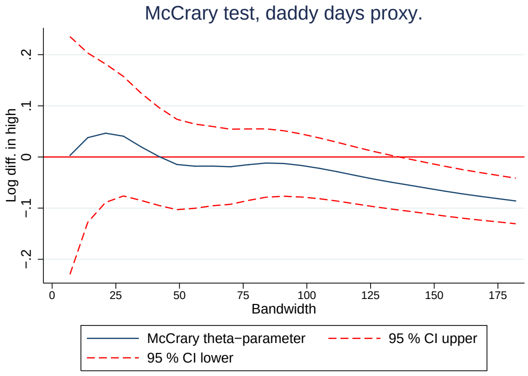
Note: Average levels of covariates of entire population of couples and for couples near the 30-months threshold (+/- 30 days). Estimates of jumps in covariates at the threshold among those near the threshold. All variables that vary over time indicate the value the year before the first child's birth. The parents' income indicates their yearly labor earnings.



(a) McCrary-test with bandwidth=30 days. Bin size=1 day.



(b) McCrary-estimates over different bandwidths. Proxy-variable. Bin size=1 week.



(c) McCrary-estimates over different bandwidths. Daddy-days proxy. Bin size=1 week.

Figure A09: Figures (a) - (c) show the results when running the McCrary-test with (a) 30-day bandwith, (b) bandwiths between 7 and 180 days using the proxy-varaible, and (c) bandwiths between 7 and 180 days using the Daddy-days-variable to measure child spacing.

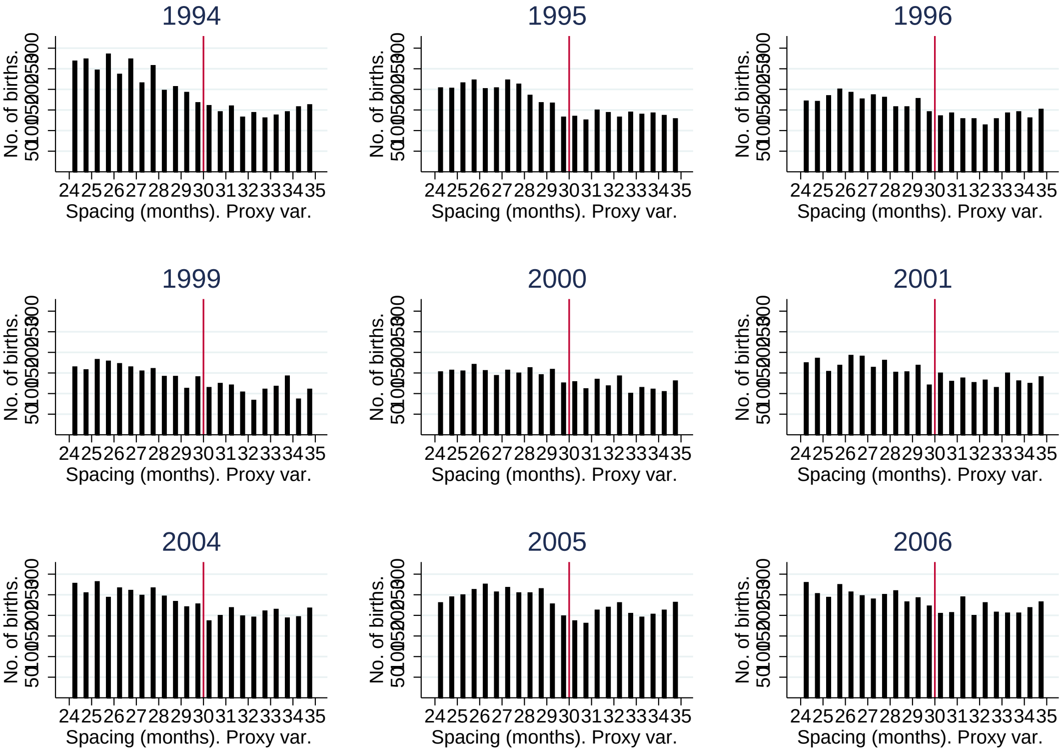


Figure A010: Frequencies of births over spacing over first child's birth year. Proxy variable for spacing.

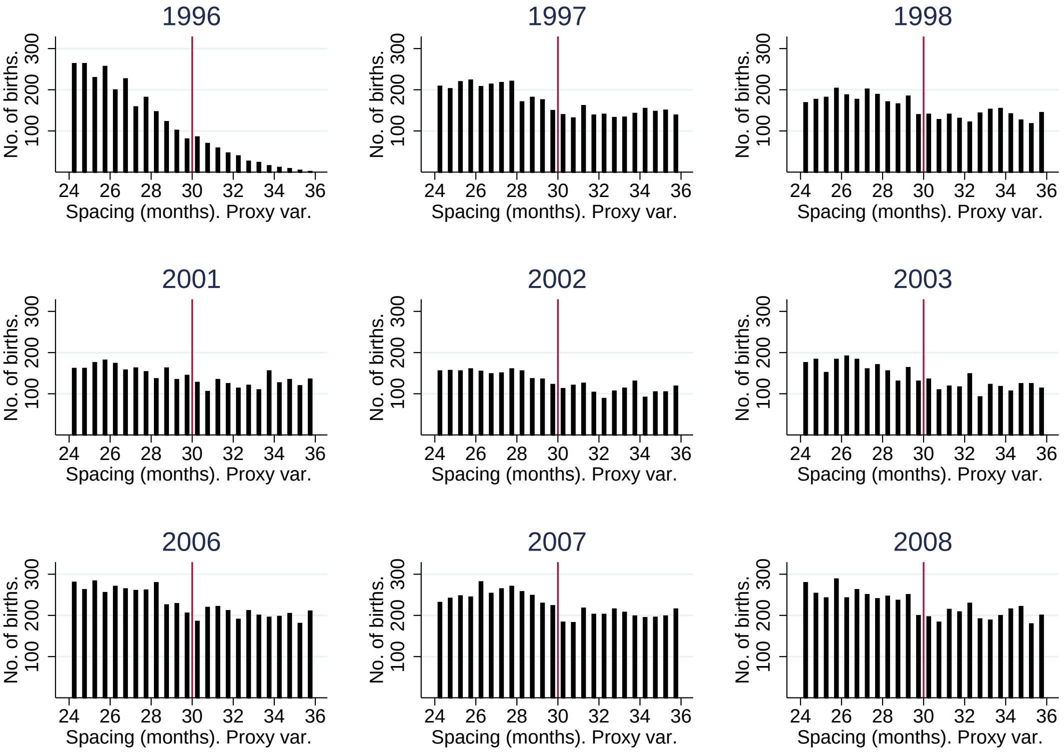
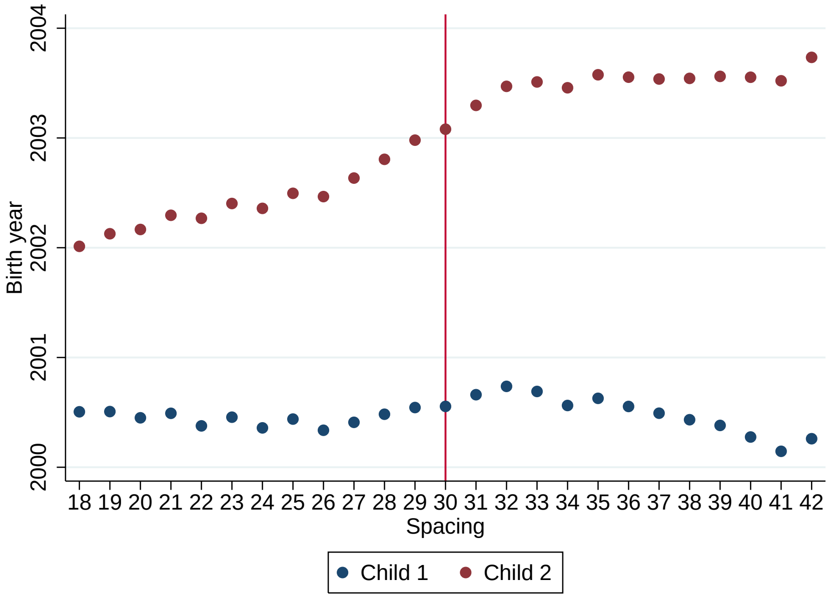
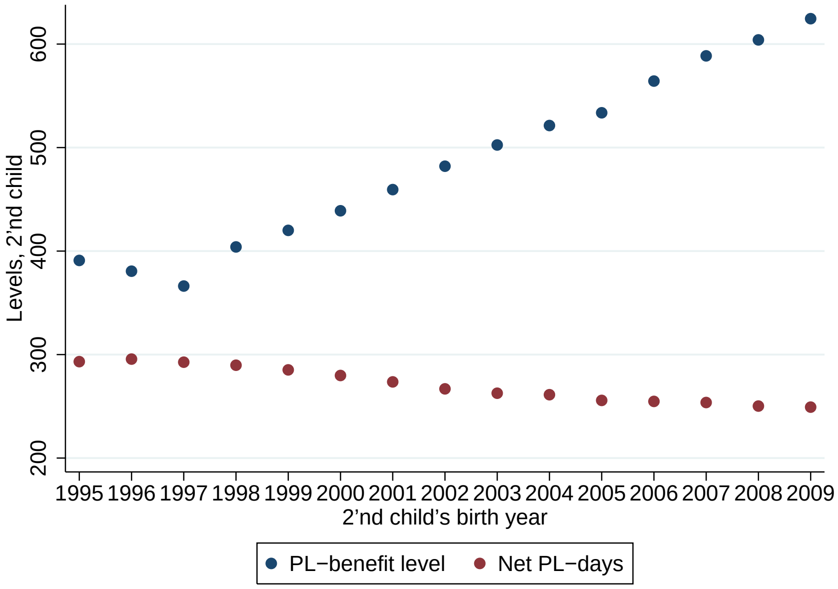


Figure A011: Frequencies of births over spacing over second child's birth year. Proxy variable for spacing.

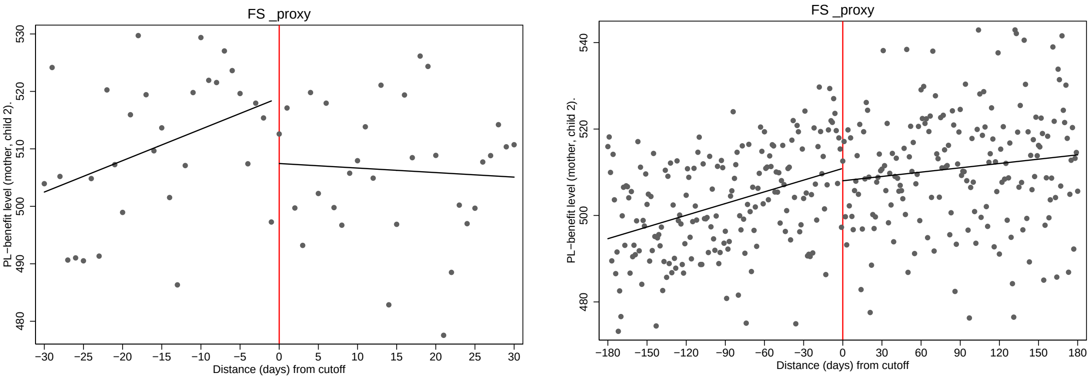


(a) Average birth year of first and second child over spacing. Figure reveals an upward trend in the second child's birth year around the 30-month threshold.



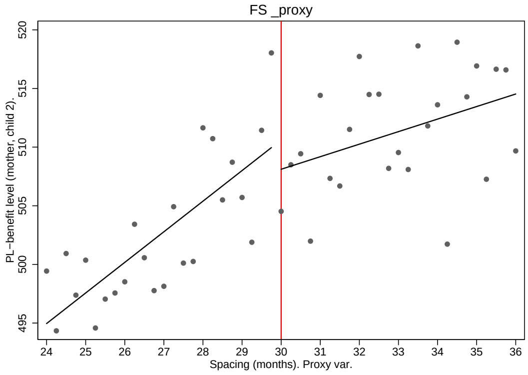
(b) Average levels of mothers' PL-benefits and take-up of net PL-days with the second child over second child's birth year.

Figure A012: Average birth year of first and second child over spacing (a) and mothers' average PL-benefit level and take-up of net PL-days with the second child over second child's birth year (b).



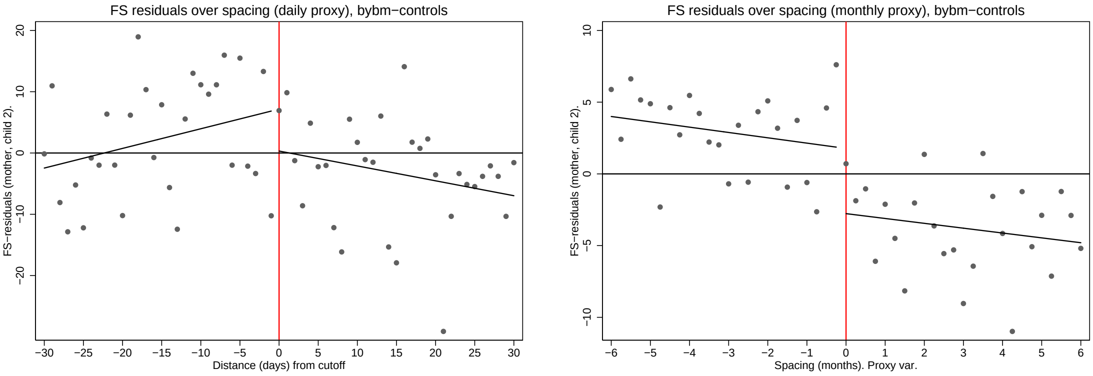
(a) +/- 30 days from 30-month threshold.
Bin size=1 days.

(b) +/- 180 days from 30-month threshold.
Bin size=1 days.



(c) +/- 6 months from 30-month threshold.
Bin size=1 week.

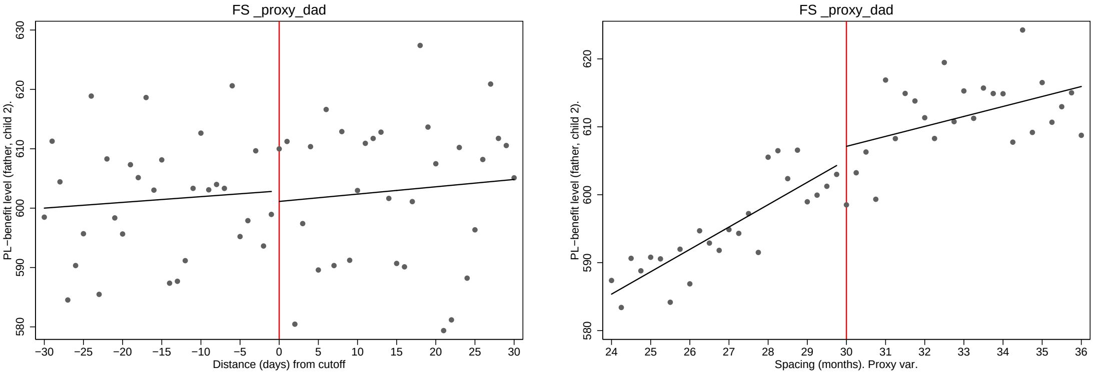
Figure A013: First stage graphs (mothers). Figures (a) - (c) show the average PL-benefit level of mothers with the second child over child spacing (proxy variable).



(a) +/- 30 days from 30-month threshold.
Bin size=1 days.

(b) +/- 6 months from 30-month threshold.
Bin size=1 week.

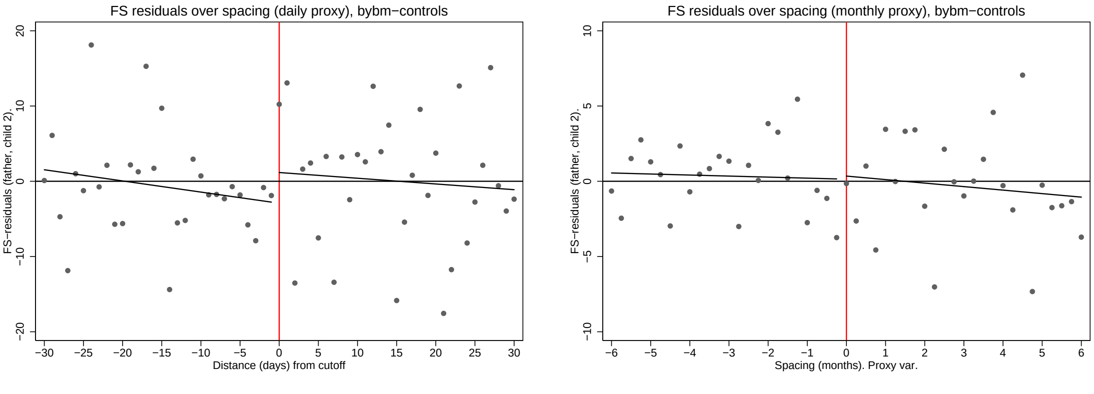
Figure A014: Residual plots from first stage regressions for mothers (mothers' average PL-benefit level with 2'nd child). Residuals when controlling for the child's birth year and birth month.



(a) +/- 30 days from 30-month threshold.
Bin size=1 days.

(b) +/- 6 months from 30-month threshold.
Bin size=1 week.

Figure A015: First stage graphs (fathers). Figures (a) - (b) show the average PL-benefit level of fathers with the second child over child spacing (proxy variable).



(a) +/- 30 days from 30-month threshold.
Bin size=1 days.

(b) +/- 6 months from 30-month threshold.
Bin size=1 week.

Figure A016: Residual plots from first stage regressions for fathers (fathers' average PL-benefit level with 2'nd child). Residuals when controlling for the child's birth year and birth month.

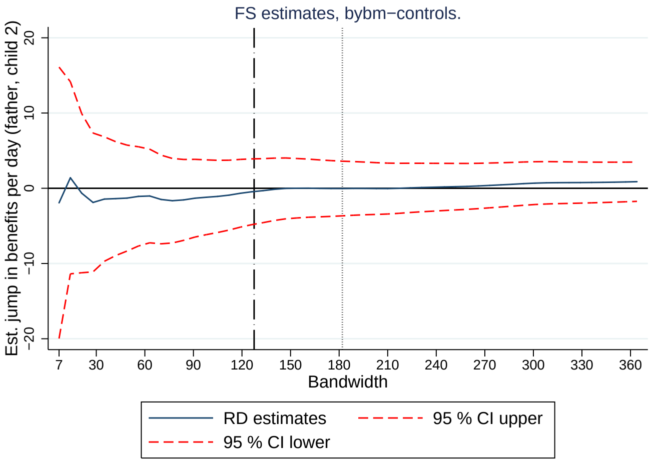


Figure A017: First stage estimate for fathers when using bandwidths between 7 and 365 days. Controls included for the second child's birth month and birth year. The dashed and dotted lines indicate the bandwidths suggested by the CCT and IK criteria respectively. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level.

Table A02: Robustness checks of the direct effects on mothers.

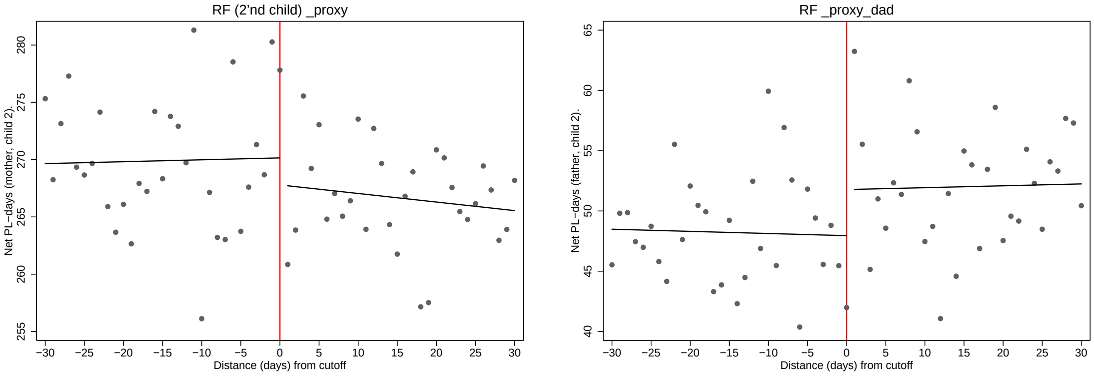
	FS, mothers	RF, net QI-days	SS, net QI-days	RF, total net days	SS, total net days	RF, cal days	SS, cal days	N
Baseline”	4.902***	2.621**	0.535*	2.595**	0.529*	3.064**	0.625*	55,780
	(1.702)	(1.036)	(0.287)	(1.292)	(0.333)	(1.396)	(0.370)	
	[8.298]							
No controls	5.026*	3.154**	0.628	3.314**	0.659	3.845**	0.765	55,780
	(2.773)	(1.141)	(0.470)	(1.456)	(0.543)	(1.528)	(0.596)	
	[3.287]							
BirthYnM	5.760**	2.906**	0.552	2.954**	0.569	3.437**	0.660	55,780
	(2.237)	(1.098)	(0.325)	(1.384)	(0.377)	(1.462)	(0.411)	
	[6.629]							
Daddydays	3.850*	2.980**	0.774	3.711**	0.964	3.733**	0.970	40,086
	(2.042)	(1.249)	(0.537)	(1.547)	(0.679)	(1.676)	(0.701)	
	[3.554]							
Random day	3.631**	2.146*	0.591	2.207*	0.608	2.380*	0.656	73,244
	(1.514)	(0.943)	(0.366)	(1.167)	(0.425)	(1.255)	(0.455)	
	[5.752]							
CCT (BW=149)	4.994***	2.670	0.535	2.207	0.442	2.511	0.503	46,279
	(1.871)	(1.139)	(0.310)	(1.422)	(0.340)	(1.538)	(0.372)	
	[7.128]							
IK (BW=268)	4.730***	1.872**	0.396*	1.979**	0.418*	2.645**	0.559*	80,476
	(1.404)	(0.853)	(0.220)	(1.064)	(0.265)	(1.146)	(0.303)	
	[11.350]							
Quadratic	5.042**	3.090	0.613	1.570	0.311	1.556	0.309	55,780
	(2.503)	(1.531)	(0.438)	(1.911)	(0.420)	(2.078)	(0.449)	
	[4.058]							
Cubic	4.974	3.410	0.686	1.379	0.277	1.297	0.261	55,780
	(3.310)	(2.025)	(0.622)	(2.528)	(0.553)	(2.771)	(0.594)	
	[2.259]							

Note: The table contains robustness checks for the first stage, reduced form and second stage estimates of the direct effect on mothers. The first row reproduces the baseline results from table 3. The table shows the results when dropping all control variables, just controlling for the second child’s year and month of birth, using the daddy-day proxy, including all observations by using a random day as proxy for the child’s birthday, using the bandwidth recommended by Calonico et al. (2014) (CCT) and Imbens and Kalyanaraman (2011) (IK), and when including quadratic and cubic functional forms. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level. Stars indicate p-values (p): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A03: Robustness checks of the cross-spousal effects on fathers.

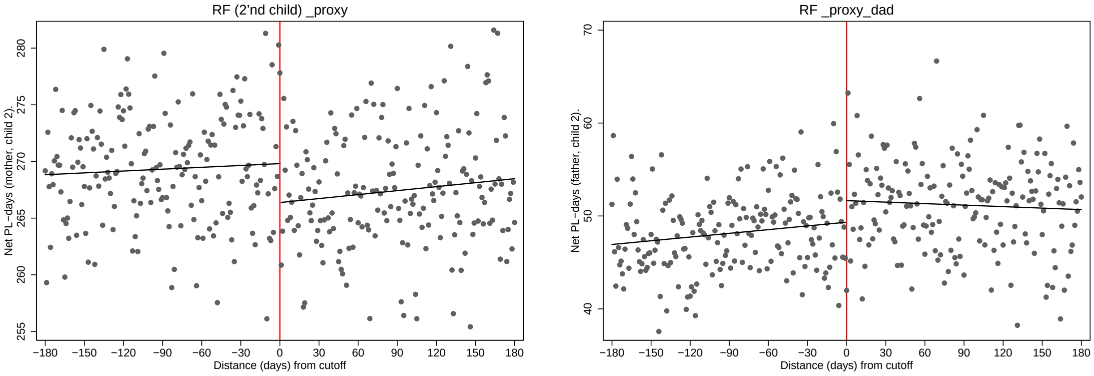
	FS, mothers	RF, net QI-days	SS, net QI-days	RF, total net days	SS, total net days	RF, cal days	SS, cal days	N
Baseline”	4.902***	-1.957*	-0.399	-1.753*	-0.358	-1.844*	-0.376	55,780
	(1.702)	(0.864)	(0.229)	(0.955)	(0.236)	(1.020)	(0.251)	
	[8.298]							
No controls	5.026*	-2.474**	-0.492	-2.330**	-0.464	-2.450**	-0.487	55,780
	(2.773)	(0.972)	(0.384)	(1.065)	(0.384)	(1.134)	(0.405)	
	[3.287]							
BirthYnM	5.760**	-2.318**	-0.438	-2.168**	-0.414	-2.277**	-0.439	55,780
	(2.237)	(0.924)	(0.267)	(1.018)	(0.273)	(1.087)	(0.290)	
	[6.629]							
Daddydays	3.850*	-1.423	-0.369	-1.322	-0.343	-1.695	-0.440	40,086
	(2.042)	(1.027)	(0.339)	(1.135)	(0.355)	(1.212)	(0.402)	
	[3.554]							
Random day	3.631**	-1.620*	-0.446	-1.533*	-0.422	-1.637*	-0.451	73,244
	(1.514)	(0.771)	(0.287)	(0.849)	(0.299)	(0.907)	(0.319)	
	[5.752]							
CCT (BW=149)	4.994***	-2.237**	-0.448	-1.962**	-0.393	-2.204**	-0.441	46,279
	(1.871)	(0.951)	(0.259)	(1.052)	(0.263)	(1.122)	(0.285)	
	[7.128]							
IK (BW=268)	4.730***	-1.804**	-0.381*	-1.827**	-0.386*	-1.790**	-0.378*	80,476
	(1.404)	(0.710)	(0.192)	(0.785)	(0.206)	(0.840)	(0.215)	
	[11.350]							
Quadratic	5.042**	-3.360**	-0.666	-2.925**	-0.580	-3.460**	-0.686	55,780
	(2.503)	(1.282)	(0.424)	(1.416)	(0.411)	(1.507)	(0.463)	
	[4.058]							
Cubic	4.974	-4.924**	-0.990	-4.529**	-0.910	-5.007**	-1.007	55,780
	(3.310)	(1.705)	(0.753)	(1.880)	(0.728)	(1.995)	(0.796)	
	[2.259]							

Note: The table contains robustness checks for the first stage, reduced form and second stage estimates of the cross-spousal effect on fathers. The first row reproduces the baseline results from table 4. The table shows the results when dropping all control variables, just controlling for the second child’s year and month of birth, using the daddy-day proxy, including all observations by using a random day as proxy for the child’s birthday, using the bandwidth recommended by Calonico et al. (2014) (CCT) and Imbens and Kalyanaraman (2011) (IK), and when including quadratic and cubic functional forms. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level. Stars indicate p-values (p): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



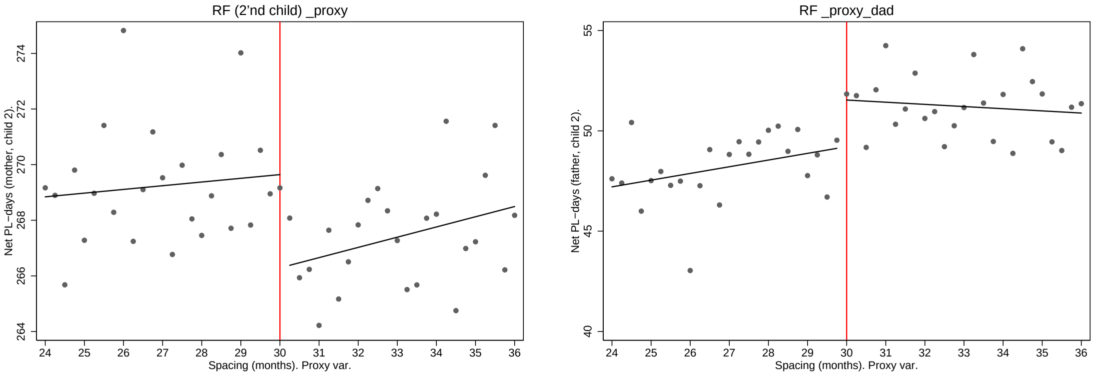
(a) RF graph - mothers, +/- 30 days from 30-month threshold. Bin size=1 days.

(b) RF graph - fathers, +/- 30 days from 30-month threshold. Bin size=1 days.



(c) RF graph - mothers, +/- 180 days from 30-month threshold. Bin size=1 days.

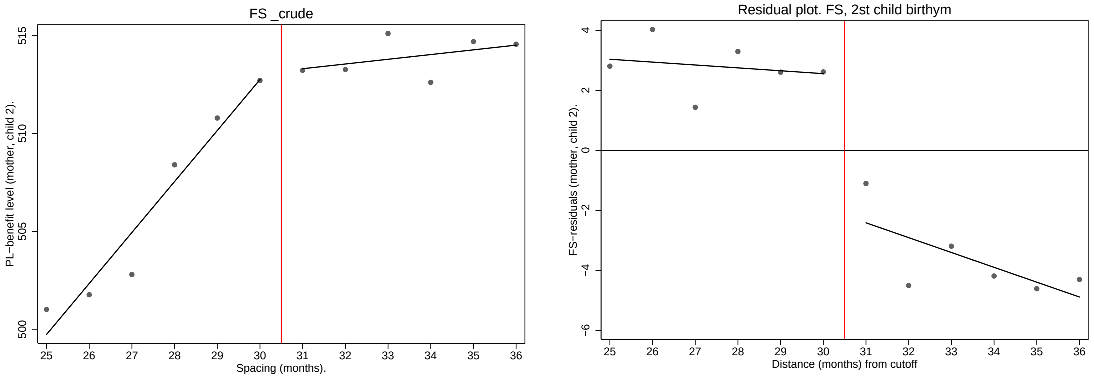
(d) RF graph - fathers, +/- 180 days from 30-month threshold. Bin size=1 days.



(e) RF graph - mothers, +/- 6 months from 30-month threshold. Bin size=1 week.

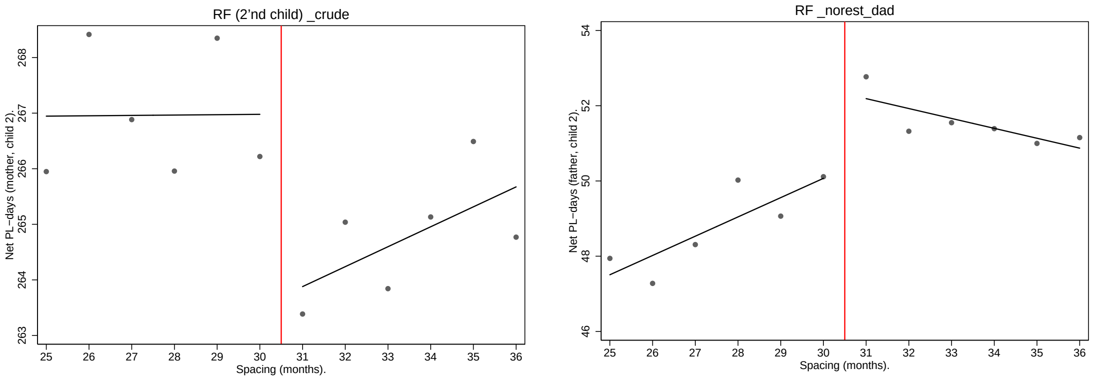
(f) RF graph - fathers, +/- 6 months from 30-month threshold. Bin size=1 week.

Figure A018: Reduced form graphs - mothers (graphs to the left) and fathers (graphs to the right). Figures show the parents' average take-up of PL-days with the second child over child spacing (proxy variable).



(a) FS graph - mothers, monthly spacing var.

(b) FS residual plot, mothers, monthly spacing var.



(c) RF graph - mothers, monthly spacing var.

(d) RF graph - fathers, monthly spacing var.

Figure A019: Graphical evidence when using the crude monthly spacing variable. All figures display the data for all observations +/- 6 months from 30-month threshold. Controls for the 2'nd child's birth year and birth month are included in the regressions for the residual plots.

Table A04: Mothers, direct effects (monthly spacing variable).

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.027***	4.827***	2.549***	0.528**	266.00	72,000
	(0.003)	(1.548)	(0.960)	(0.267)		
Total net days	-0.058***	4.827***	3.182***	0.659*	292.69	72,000
	(0.004)	(1.548)	(1.185)	(0.337)		
Calendar days w benefits	-0.055***	4.827***	3.109**	0.644*	301.27	72,000
	(0.004)	(1.548)	(1.269)	(0.346)		
Mean benefit level		509.35				
First stage F-stat		9.73				

Table A05: Fathers, direct effects (monthly spacing variable).

	OLS	FS	RF	Mean	N
Net QI-days	-0.024***	0.347	-1.388*	49.95	71,854
	(0.003)	(1.435)	(0.784)		
Total net days	-0.038***	0.347	-1.517*	54.49	71,854
	(0.003)	(1.435)	(0.862)		
Calendar days w benefits	-0.043***	0.347	-1.914**	57.50	71,854
	(0.003)	(1.435)	(0.921)		
Mean benefit level		603.08			
First stage F-stat		0.06			

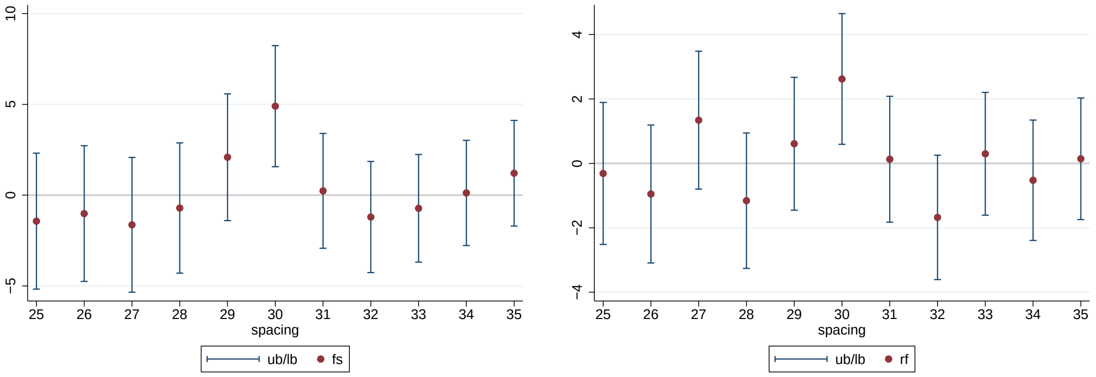
Table A06: Fathers, cross spousal effects (monthly spacing variable).

	OLS	FS	RF	SS	Mean	N
Net QI-days	0.018***	4.827***	-1.388*	-0.287	49.95	72,000
	(0.003)	(1.548)	(0.784)	(0.189)		
Total net days	0.025***	4.827***	-1.517*	-0.314	54.49	72,000
	(0.003)	(1.548)	(0.862)	(0.209)		
Calendar days w benefits	0.025***	4.827***	-1.914**	-0.397*	57.50	72,000
	(0.003)	(1.548)	(0.921)	(0.234)		
Mean benefit level		509.35				
First stage F-stat		9.73				

Table A07: Couple level effects (monthly spacing variable).

	OLS	FS	RF	SS	Mean	N
Net QI-days	-0.008	4.827***	1.162	0.241	315.95	72,000
	(0.002)	(1.548)	(0.787)	(0.182)		
Total net days	-0.033	4.827***	1.666*	0.345	347.18	72,000
	(0.003)	(1.548)	(0.998)	(0.240)		
Calendar days w benefits	-0.030	4.827***	1.195	0.248	358.77	72,000
	(0.003)	(1.548)	(1.125)	(0.250)		
Mean benefit level		509.35				
First stage F-stat		9.73				

Note: OLS, first stage, reduced form and second stage estimates of the direct effect on mothers (3), the direct and cross-spousal effect on fathers (4 and 5), and couple level effects (6), using the crude monthly measurement of spacing, linear trends, a 6 months bandwidth, triangular weights and control variables for the second child's year and month of birth, mother's and father's age, type and level of education, marital status, region of residence, immigration status, and the first child's gender. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level. Stars indicate p-values (p): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) First stage estimates estimated at placebo thresholds.

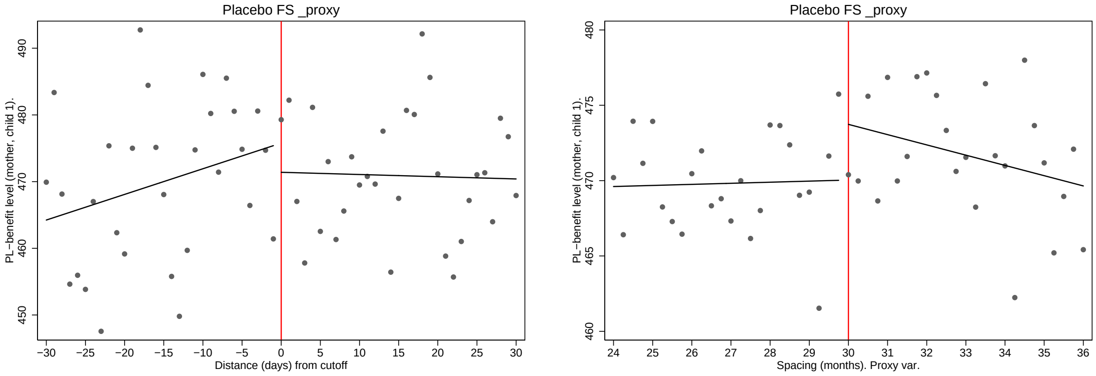
(b) Reduced form estimates estimated at placebo thresholds.

Figure A020: First stage (a) and reduced form (b) estimates, estimated at placebo thresholds, 25 to 35 months of spacing, and at the real threshold (30 months of spacing). Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level.

Table A08: Placebo tests.

	FS-estimate	RF-estimate
25	-1.433	-0.313
	(1.910)	(1.125)
	[0.562]	
26	-1.016	-0.951
	(1.906)	(1.093)
	[0.284]	
27	-1.635	1.342
	(1.894)	(1.092)
	[0.746]	
28	-0.711	-1.158
	(1.831)	(1.072)
	[0.151]	
29	2.088	0.609
	(1.781)	(1.052)
	[1.374]	
30	4.902***	2.621**
	(1.702)	(1.036)
	[8.298]	
31	0.236	0.129
	(1.614)	(0.997)
	[0.021]	
32	-1.206	-1.677*
	(1.562)	(0.985)
	[0.596]	
33	-0.726	0.297
	(1.513)	(0.973)
	[0.230]	
34	0.123	-0.524
	(1.478)	(0.953)
	[0.007]	
35	1.206	0.144
	(1.484)	(0.963)
	[0.661]	

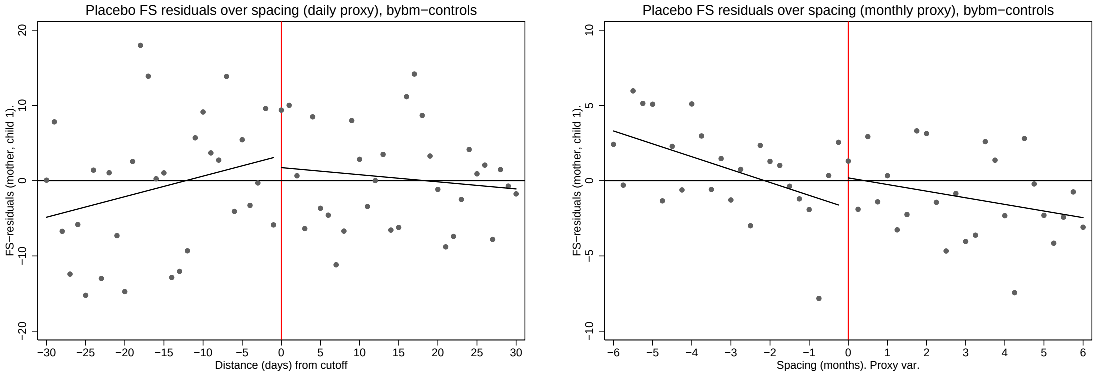
Note: First stage and reduced form estimated at placebo thresholds, 25 to 35 months of spacing, and at the real threshold (at spacing equal to 30). Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level. Stars indicate p-values (p): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(a) +/- 30 days from 30-month threshold.
Bin size=1 days.

(b) +/- 6 months from 30-month threshold.
Bin size=1 week.

Figure A021: Placebo first stage (mothers). Figures (a) - (b) show the average PL-benefit level of mothers with the first child over child spacing (proxy variable).



(a) +/- 30 days from 30-month threshold.
Bin size=1 days.

(b) +/- 6 months from 30-month threshold.
Bin size=1 week.

Figure A022: Residual plots – placebo first stage. Residuals of mothers' average PL-benefit level with 1'st child when controlling for the child's birth year and birth month.

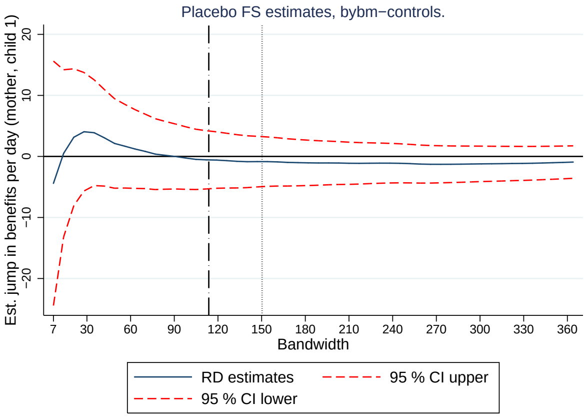


Figure A023: Placebo first stage estimate for mothers (PL-benefit level with 1'st child) when using bandwidths between 7 and 365 days. Control variables are included for the 2'nd child's birth year and birth month. Bin size=1 day. Regressions are run with separate linear trends and triangular weights. Standard errors are clustered at household level.