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DOES CHILD CARE AFFECT PARENTS' SICKNESS ABSENCE? EVIDENCE FROM A NORWEGIAN PATERNITY LEAVE REFORM.

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Does child care affect parents' sickness absence? Evidence from a Norwegian paternity leave reform*

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Abstract

In several European countries, a paternity quota has been introduced as part of paid parental leave to provide incentives for fathers to increase their child care responsibilities and household involvement. In this paper, we explore the introduction of the first paternity quota in Norway in 1993. Through a regression discontinuity (RD) framework, we examine the sickness absence of parents who had children just before and after the reform—due to the parents' own illness and to care for close family members. Our findings suggest that the amount of sick leave taken by fathers has increased in the short and long term and that the amount of sick leave taken by mothers has decreased, although the estimates are not statistically significant. The results are supported by standard RD and robustness tests. We also address the relevance of a composition bias resulting from the unobservable latent sick leave of non-employed individuals. This sensitivity check shows that their latent absence may affect the estimated treatment effect.

Keywords: sickness absence, paternity leave, child care.

JEL classification: J13; J22; I38.

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1. Introduction

Household work and child care have traditionally been heavily skewed toward women while labor work has been directed toward men (Vaage, 2011; Haraldsen and Kitterød, 1992; Booth and van Ours, 2009; Boye, 2009). Over the last few decades, a demand for more equal distribution of household and labor work has been promoted in many European countries. The motivation behind this development is to increase female labor force participation, reduce the gender wage gap, and promote gender equality in general. To achieve these aims, politicians and policymakers have put forward various reforms and schemes. One of the relatively new interventions is paternity leave. Recently, a few European countries (Norway, Sweden, Island, Finland, Italy, and Austria) introduced paternity quotas of paid parental leave to provide incentives for men to increase their child care responsibilities and household involvement.

In this paper, we use Norwegian registry data to explore the impact of paternity leave. In 1993, fathers were for the first time granted four weeks of paid leave when their child was born. We are interested in whether the implementation of this quota, and the following increase in child care involvement, had a negative effect on fathers' sick leave, and, thus, led to fathers taking more sick leave.[1]

We expect to find this pattern for several reasons. First, consider a conventional labor-leisure choice framework where child care is part of leisure. When paternity leave reform is introduced, it causes a profound change in the price or value of leisure for men. In addition, when on leave, fathers learn more about their children and acquire more human capital needed for child care. This specialization consequently has a stronger future comparative advantage in household work (Ekberg *et al.*, 2005). These two effects, that is, the shock to the budget constraint and the following specialization in child care, would induce fathers to consume more leisure in the short and long run. In a two-parent household, this would imply a reallocation of the family resources to maximize their joint utility (see Becker, 1991 and Ermish, 2003 for theory on family production). One way to increase leisure without

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[1] We cannot exclude the opposite effect; that is, that child care involvement may have a positive effect on fathers, and, consequently, lower their sickness absence. This pattern could be explained by the fact that spending more time with one's children could increase life satisfaction and improve one's general physical health and, consequently, decrease the amount of sick leave.

recontracting work schedules, for example, working part-time or increasing flexibility, is to be absent from work (Allen, 1981; Barmby *et al*, 1994). Consequently, we could see an increase in sickness absence due to the reform. Additionally, being involved in child care could also change fathers' preferences. In brief, a shift in preferences from labor to leisure can deter the value of the labor work and thus change fathers' cost of absenteeism and lower their threshold for being absent. This would in turn lead to a higher sick leave rate. Second, increasing one's involvement with one's children without reducing the number of work hours will increase the overall workload for fathers. A theory launched as an explanation for women's higher sickness absence rate compared to men's is the "double burden" hypothesis. It states that a combination of labor force attachment and family duties may cause mental distress and affect physical health through the risk of role conflict and role overload, and result in more sickness absence (see Hochschild, 1989; Sieber 1974; Arber et al., 1985; Arber, 1991; Verbrugge, 1983, 1986). Now, with the equalization of family duties and child care between genders, this hypothesis could also apply to men. That is, the increased overall workload could cause adverse health effects and, thus, increase their absence due to sickness. Third, fathers could be more involved with their children in unexpected situations. More specifically, if a child gets sick, the father may stay home with the child instead of the mother.[2] Finally, being more around children could increase absenteeism through contamination. Children, especially toddlers, are sicker than the average adult, and, hence, being around them more can potentially increase the number of one's own illnesses and absences from work. This effect is obviously most relevant in the short run.

In addition to affecting fathers, the introduction of the paternity quota could affect mothers. When reallocating the family resources, mothers may be given incentives to be less absent from work for the same reasons fathers are given the opposite incentive. In addition, when fathers are more involved in child care and family duties, the mothers' workload in the household consequently decreases. This could have a positive health effect for mothers and, thus, lead to a reduction in sick leave. If a combination of labor and household work caused more stress and physical illnesses, then a reduction in the overall workload could bring about the opposite effect. In this paper, we therefore also address whether paternity leave leads to reduced sickness absence for mothers.

[2] Even if the father is not sick himself, he can report sick due to others' illness (normally close family members). We return to the sick leave measure in section 4.1.

Although the effects of maternity leave have been explored extensively (see Baker and Milligan, 2011, for a review), the impact of paternity leave on parents and children's outcomes has been investigated less. Regarding parents' absenteeism, Bratberg and Naz (2009) studied the effect on mothers' sickness absence by using data on Norwegian births from 1997 to 1999. The authors hypothesized that sharing child care equally may improve a mother's health by easing her burden at home, and consequently, the number of sick days she takes may decrease. The results show that in families in which fathers take more than four weeks of paternity leave (i.e., taking leave from the gender-neutral leave scheme, cf. section 2.2), the probability of mothers being absent from work is reduced by about 5-10 percent from an average of 20 percent. Using Swedish data on paternity quota reform in 1995, Ekberg *et al*. (2005) looked specifically at fathers' leave to care for sick children. The variable serves as a proxy for acquiring human capital for household work. The idea is that as fathers increase the human capital needed for child care, they will take more responsibility for their family, and consequently, the amount they care for children during illnesses should also increase. However, the findings suggest that the reform had no impact on absences to care for sick children. Finally, Ugreninov (2012) used Norwegian registry data for the paternity leave reform in 1993 to explore the same hypothesis as Bratberg and Naz (2009). She did not find any significant effects on mothers' or fathers' absenteeism. Looking at non-health-related outcomes, some studies have found that paternity leave increases fathers' long-term involvement with their children's care (see e.g. Rege and Solli, 2010; Brandth and Kvande, 2003; Haas and Hwang, 2008; Nepomnaschy and Waldfogel, 2007; Tanaka and Waldfogel, 2007). Regarding mothers and fathers' labor market outcomes, the results are mixed: Rege and Solli (2010) found that paternity leave decreases fathers' future earnings, Cools *et al*. (2011) found that mothers' work hours and earnings are negatively affected while Johansson (2010) found no causal effect on either parent's earnings.

When exploring paternity leave and sickness absence, we are faced with several empirical challenges. The most important concern is omitted variable bias. If a father's decision to take leave is correlated with unobserved individual characteristics that also affect his sick leave, the estimated results may be biased. For instance, fathers who tend to be involved in their children's care could systematically possess a lower threshold for reporting sick and thus be absent more frequently from work, or in contrast, they could, on average, be healthier and thus absent less frequently from work. To circumvent this problem and selection issues, we use the Norwegian paternity leave reform implemented on 1 April 1993. For the first time, fathers were given an exclusive paternity quota of the paid parental leave scheme. With a few exceptions, the quota is not transferable to the mother so if eligible fathers do not take their leave, the family loses it. Consequently, the reform caused a large increase in the uptake rate from about 4 percent to more than 40 percent when the reform was implemented (Cools *et al*., 2011; Rege and Solli 2010). With detailed registry data, we identify the exact birthdates of children born before and after the reform. The registries link the children to their parents, which allow us to observe parents' sick leave for several years after the reform was implemented. Consequently, we can explore the reform's short- and long-term consequences for sick leave. In addition, we can separate sick leave due to a parent's own illness from absence due to caring for sick children and other close family members. This allows us partly to separate the channels in which the reform may work.

The main approach we adopt to estimate the effects is a regression discontinuity (RD) design. The RD framework uses the large increase in parental leave and allows us to explore the difference between the outcomes of parents who had children when the reform was implemented. Furthermore, we address an often overlooked empirical challenge when exploring sickness absence: individuals who are not employed cannot be measured as absent from work. This creates a sample selection bias that may affect the interpretation of the results (see, for instance, Rieck and Telle, 2012). We address the relevance of the composition bias in section 5.

The analysis suggests that paternity leave leads to an increase in fathers' sickness absence and a decrease in mothers' absences. However, none of the estimates are significantly different from zero, but the signs of the estimates are in line with the hypotheses. Exploring the latent sick leave of non-employed individuals shows that the effects of the reform may be more substantial than the benchmark results. Evidently, it depends on the sick leave rate non-employed individuals would have had if they had been employed.

2. Background

2.1 Paternity Leave Reform

On 1 April 1993, a paternity quota for paid parental leave was introduced in Norway.[3] Of a total of 42 weeks, four weeks were now reserved exclusively for the father. Of the remaining weeks, nine were reserved for the mother (three weeks before birth and six weeks after), and the remaining period was gender neutral and fully sharable between the mother and father. Before the reform was implemented, fathers could take paid parental leave as a part of the gender-neutral quota; however, none was explicitly reserved for the father.

To be eligible for paid maternity leave, the mother have to work at least 50 percent during at least six of the last ten months before giving birth.[4] Paid paternity leave was contingent, however, on both parents fulfilling the same employment requirement. In other words, fathers had no independent right to paid leave, and during the five weeks of paternity leave, the mother had to work for the father to be eligible.[5]

The income compensation rate is 100 percent up to an established limit (about €60,900 in 2011) and calculated based on earnings during the last ten months before birth (an alternative scheme is 80 percent compensation for a total of 52 weeks). For mothers, compensation is based on pre-birth income alone, but for fathers, compensation is based on both parents' earnings. For example, if the father worked full-time for at least six of the last ten months before the child's birth, but the mother worked only part-time, the father's income compensation would be reduced in proportion to the mother's wage.

The introduction of the paternity quota led to a large increase in the uptake rates for fathers (see section 5.1. for figures in our data sample). Other studies exploring the reform report an uptake rate from about 4 percent in March 1993 to about 40 percent in April 1993 (Cools *et al*., 2011). About 75 percent of the fathers taking leave use all four weeks, and 95 percent exhaust their rights before the children are a year old (on average, the leave period starts when the child is nine months old).

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[3] At the same time mothers were given an extra pre-birth week of leave, and the number of gender-neutral weeks was increased by two weeks (reaching a total of 42 weeks).

[4] The definition of work is quite generous. Work days lost due to sickness, unemployment periods when receiving rights, and paid parental leave (from earlier children) also count as work.

[5] In July 1994, this requirement was relaxed, and fathers could be eligible even if mothers did not work during the fathers' leave (Brandth and Øverli, 1998).

An important aspect of the reform is that it was hard to anticipate. Expansion of the paid leave scheme was first introduced in the national budget for 1993 in the autumn of 1992 and passed in Parliament in December 1992. The national budget became publicly available on 7 October 1992, only 177 days before the reform was set in motion. At this time, the mothers who gave birth around the time the reform was implemented (1 April 1993) were already pregnant. In other words, it would be very difficult to strategically time conception to receive the extra weeks of paid leave. One possibility, though, is that the reform was a topic in the policy debate in Norway earlier in the autumn or summer of 1992. This could potentially cause a postponement in the decision to have children. However, it would be hard to anticipate the exact implementation date as earlier reforms in the parental leave scheme had taken place on different dates in previous years. Cools *et al*. (2011) investigated the possibility by searching newspaper archives but found nothing that suggests the implementation date was known before the national budget became public. In relation to our data sample, we investigate manipulations of dates of birth in section 6.1.

2.2 The Norwegian Sick Leave Scheme

Sick leave payment is provided by the Norwegian National Insurance (NNI) program. The program covers the entire population whose participation is mandatory. Paid sick leave is provided from the first day of illness to a maximum of one year. If an employee reports he or she is sick, he or she will be financed by the employer from day 1 to day 16 (with some exceptions), after which the NNI program takes over the expenses at day 17. As for paid parental leave, the compensation ratio is 100 percent up to an established limit, but employers often compensate for earnings above the limit. A sick spell can be based on a self-report the first three or eight days (depending on the type of firm). For longer sick periods, a medical certificate from a physician is required. If the maximum quota of one year is exhausted and the individual is still unable to return to work, the individual is directed to other public welfare services such as vocational training at a much lower compensation ratio (replaces up to about 66 percent of pre-sick leave earnings).

3. Empirical strategy

The aim of this study is to estimate the average impact of the paternity leave reform on parents’ sick leave. To do this, we compare parents of children born right before and right after the reform was implemented on 1 April 1993 who are eligible for parental leave.[6] In particular, we use a regression discontinuity (RD) design to explore the difference in outcomes at the threshold.

The RD framework exploits the fact that individuals' assignment to a treatment variable is solely determined by the value of a forcing variable (the birthdate) on either side of a fixed threshold. If all other factors are continuous regarding the dependent variable around the cut-off, the jump or discontinuity in the outcome at the threshold can be identified as the treatment effect (see Imbens and Lemiuex, 2008; Lee and Lemiuex, 2009; Hahn *et al*., 2001 for more). The key assumptions for a valid RD treatment effect are (i) that individuals are unable to *perfectly* manipulate the forcing variable and (ii) that all pre-determined characteristics have identical distributions on both sides of the cut-off.

Of the eligible fathers, not everyone has taken paternity leave from work since the reform was introduced. Previous studies exploring the same reform report an increase in the uptake rate from about 1 to about 40 percent (Rege and Solli, 2010; Cools *et al*., 2011). Since the probability of treatment then jumps by less than one, the average treatment effect cannot merely be interpreted as the jump in the relationship between the outcome and the birthdate. To identify the average treatment effect of the paternity quota reform, we must therefore account for the proportion of leave take-up at the threshold.[7] In this setting, we describe the empirical model with two equations

$$Y_{i,t} = \alpha + \tau Leave_i + f(DB_i - c) + \varepsilon_{i,t} \qquad (1)$$

$$Leave_i = \alpha + \delta Reform_i + g(DB_i - c) + v_i, \qquad (2)$$

where $Y_{i,t}$ denotes different outcome variables (typically different types of sickness absence, cf. section 4.2) for individual *i* in month *t*; $Reform_i$ is a binary variable equal to one if the child was born after 1 April 1993, and zero otherwise; DB_i is the exact birthdate for the child

[6] Non-eligible parents could indirectly be affected by the reform through social interaction or other types of peer interaction (see Manski, 1993 for details). However, we do not pursue this potential channel in this paper. In section 3, we compare eligible and non-eligible parents to get a sense of the differences between the groups.

[7] This setup is known as the "fuzzy" RD (that is, when the probability of treatment is less than one at the cut-off) compared to the "sharp" RD setting where the probability of treatment goes from zero to one at the threshold.

to individual i[8;] and $Leave_i$ is a binary variable equal to one if parent *i* took parental leave after the child was born. $f(\cdot)$ and $g(\cdot)$ are functional forms that specify the relationship between the outcome and the birthdate, respectively. $\varepsilon_{i,t}$ and v_i error terms where v_i is independent of DB_i.

We estimate the empirical model non-parametrically using local linear regression (LLR) as in Fan (1992), Hahn *et al*. (2001), and Porter (2003).[9] LLR is preferred over general kernel regression methods since it has been shown to reduce bias in estimations (Hahn *et al*., 2001). We use the triangle kernel since it is boundary optimal (Cheng *et al*., 1997). The standard errors are bootstrap and clustered on the individual level. Finally, as in any RD estimator we identify only a local effect for those born just around the threshold. Analogously to an instrument variable approach, the RD estimator in an empirical model is interpreted as a weighted local average treatment effect (LATE), where the weights reflect individual *í*'s likelihood of having a child near the threshold (Lee and Lemieux, 2009).

The outcome of interest is parental sick leave. During the first year after the birth of a child, parents are typically on parental leave. Cools *et al*. (2011) reported that more than 95 percent of eligible fathers took paid leave during the first 12 months after their children were born. Therefore, we start to measure fathers' sickness absence the second year after their children were born. Furthermore, paternity leave seems to increase fathers' long-term involvement in child care. Based on time use data, Rege and Solli (2010) found that fathers in general spent significantly more time with their children and less time working seven years after the reform compared to three years before the reform. Although this change cannot be directly attributed to the paternity leave reform, it is natural that any changes in child care cultures and norms could take time to develop and work in full length. Other studies have also found a positive association between paternity leave and long-term involvement in child care (Brandth and Kvande, 2003; Haas and Hwang, 2008; Nepomnaschy and Waldfogel, 2007; Tanaka and Waldfogel, 2007). Furthermore, there is evidence of more involvement over

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[8] The birthdate variable is transformed by subtracting the cut-off value *c*, i.e., from DB_i to $(DB_i - c)$, to make the computation easier.

[9] We also estimated the empirical model parametrically using polynomial regressions. More specifically, we used 2SLS (as suggested by Lee and Lemiuex, 2009) with the same order of polynomial for *g(·)* and *f(·)* ranging from a linear to a third-order polynomial, and different time windows (from 30 to 90 days at ten-day intervals). Overall, the results from the regressions do not deviate substantially from the non-parametric approach and are therefore not included in the results.

time as the proportion of fathers taking paid leave has increased steadily since the reform, reaching more than 80 percent of eligible fathers in 2005 (Cools *et al.*, 2011). Therefore, we also want to explore the potential long-term effects of the paternity leave reform by observing parental sickness absence up to ten years after a child's birth. To simplify the presentation of the results, we divided the years into three groups: the second through fourth years after the child's birth, the fifth through seventh years after the child's birth, and the eighth through tenth years after the child's birth.[10]

For mothers, we measure sickness absence in the same years as for fathers. The main motive is to make the results comparable across the genders. However, mothers did not necessarily reenter employment after parental leave as quickly as fathers. Among other issues, kindergarten availability was low in some regions, forcing some mothers to prolong their leave (paid or unpaid). We have to keep this in mind when interpreting the results.

When measuring sick leave, we must address an important point: individuals who are not employed cannot be measured as absent from work. If the paternity leave reform affects parents' employment status, the composition of fathers and mothers who can report sick will also be affected. This will in turn create a sample selection bias, in which the direction is unknown. If we had viewed the treatment and control groups separately, the composition bias would tend to reduce any estimate. Obviously, if the non-employed individuals had been employed they would have only a higher sick leave rate than zero (the level of absence observed when they are non-employed). However, when we take the difference between the two groups, as we do in the empirical model, the direction of the bias depends on the proportion of non-employed in the two groups and how much the estimates are being biased downward. Thus, we cannot depict the direction of the bias. In the main analyses, we include the individuals and time periods where employment is zero. Excluding them would cause even more composition bias since we would then select the sample based on an endogenous variable. However, we try to address the relevance of the sample selection in two ways. First, we compute treatment bounds that specifically account for this type of bias (Lee, 2009; Horowitch and Manski, 1995; 2000). Since the support for the outcome is bounded we follow Horowitch and Manski (1995) when computing the upper and lower bounds. Second,

[10] We also performed the analysis yearly and tried different groups (e.g., every second year). The results do not deviate in any substantial way from the results with the three groups presented here.

we assign different values to the latent sick leave variable and reestimate the empirical model to see what the result would be if the missing sickness absence observations had a different average than zero.

4. Data

4.1 Data Sources

The data used in the study is administrative data from Norwegian registries. The registries cover the complete resident population of Norway over the period 1992-2008. The main data source is the birth registry. Every birth is recorded with the exact birth date along with information on type (single, twin, etc.) and parity. A unique encrypted personal identifier for the child, the father, and the mother is provided, which enable us to link the child to his or her parents. Further, we use the parents' personal identifiers to match the registry to other administrative records such as the education, earnings, and employment registries, and the parental leave and sickness absence registries.

The sickness absence registry contains sick leave spells reported by the NNI for refunds. The general rule is that all sick spells lasting more than 16 days are refundable.[11] The leaves are recorded with entry and exit dates where the graduation scale (from 20 to 100 percent) can be adjusted during the duration. The type of sickness is also recorded: a sick leave due to an individual's own sickness is distinguished from an absence related to the sickness of other individuals. These individuals are normally the individual's children but could also be a spouse or other closely related family members. Using these records, we constructed monthly absences due to sickness variables for the two types of sick leave. The variables equal the proportion of sick leave days to the total number of days in each month. For example, if the variable equals zero, the individual has not been absent that month, and if the variable equals one, the individual has been 100 percent absent. The entry and exit dates are corrected for, as well as the changes in the graduation scaling.

Parental leave is recorded as payments from the NNI to parents eligible for paid parental leave with the exact entry and exit dates. We measure parental leave status with a variable

[11] There are some exceptions to the main rule, but they are not important in the context of this study.

that takes the value zero or one according to whether the parent is receiving payments or not in each month.

Employment is recorded for every employee with the exact entry and exit dates (self-employed individuals are excluded). The lowest level of employment is four hours per week. Individuals who work fewer than four hours per week are therefore not recorded as employed in the data. We record the employment status with a binary variable in each month as for parental leave.

Households are identified through a family registry that records individuals who live together (cohabitants, married couples, etc.). The household status is recorded in the calendar year after the child's birth.

Earnings and education are recorded yearly. Earnings are labor-related income from tax registries, and education is the total number of years with education. Both are added to the monthly panel according to the calendar year. When we examine the baseline covariates in section 6.2 and estimate regressions with covariates, the log of earnings, years of education, and age are measured in the year before the child's birth.

A shortcoming of this data set is that the sick leave records include only long-term sick spells. Spells lasting less than 16 days are more exposed to moral hazard problems and potentially affected by the paternity leave reform. However, an advantage is that we can to a large extent exclude contamination as a mechanism. Thus, the results mirror more profound involvement in child care. Another advantage is that long-term absences comprise the majority of the total absence spells.

4.2 Sample Definitions

Using the above data sources, our sample consists of fathers and mothers of children born three months before and three months after the reform was implemented, 1 April 1993, who are eligible for parental leave.

Since we do not directly observe eligibility status, we have to impose restrictions to ensure that the observed parents are eligible for parental leave (cf. section 2.1 for the government's criteria). First, in the calendar year of conception, the annual earnings of both parents must be at least one time the basic amount set by the NNI (the basic amount was about €10,100 in 2011).[12] In addition, they have to be employed in at least six of the last ten months before birth. This ensures that the parents are attached to the labor market. Second, only parents living together are included. Fathers who are not living with the mothers at time of the child's birth are not entitled to parental leave. Since we identify households in the calendar year after the child's birth, the restriction is potentially endogenous if the reform had an impact on marital status. For instance, the passing of the reform in parliament in December 1992 may have influenced parents' decision to live together before the child's birth to be eligible. Finally, we limit the sample to individuals born in Norway. Immigrants in general have a substantially weaker labor force attachment (Olsen 2008).

4.3 Summary Statistics

Summary statistics are presented in Table 1. We observe about 27,000 parents in 13,500 families, out of which 48 percent of the families are observed before the reform was implemented. In total, the number of observations exceeds 2.9 million. The overall sickness rate due to an individual's own sickness is 2.6 percent for fathers and, as expected, substantially higher for mothers (4.7 percent). The difference between the treatment and control groups is negligible. Likewise for employment, the mothers are employed in 82 percent of the months observed while fathers have a far higher employment proportion of about 93 percent. Female labor force participation is high in Norway; however, we have to consider the proportion of non-employed women (and men) when interpreting the results (cf. Section 5.3).

In total, about 50 percent of the families who had a child around the time the reform was implemented fulfilled the eligibility criteria. A comparison of the eligible families to the non-eligible families reveals they are different in terms of several characteristics.[13] Non-eligible fathers have a much higher sickness rate than eligible fathers (4.0 and 2.6 percent) while for mothers the figures are more similar (4.2 and 4.7 percent). The non-eligible parents also have a lower employment level (69 and 52 percent for men and women), in addition to on average being one year younger, having two fewer years of education, and lower

[12] We define the day of conception as the birth day minus 273 days (Myklebø, 2007).

[13] Non-eligible parents include students, unemployed individuals, and immigrants. The summary statistics table is not included in the paper.

earnings. In other words, the eligible and non-eligible parents are two different groups. This means that the results of the empirical analyses cannot be extrapolated to non-eligible parents without further investigation.

[Table 1 about here]

5. Results

An important choice in any RD analysis is the choice of bandwidth. As a default, we use 55 days on either side of the cut-off. Using the cross-validation procedure described in Imbens and Lemieux's (2008) and Lee and Lemieux's (2009) work, there is not a single optimal bandwidth when viewing all the outcomes under one. Appendix Figure 1 shows the cross-validation values for different outcomes, and we explore sensitivity of the results to other bandwidths in section 6. In the same section, we test the key assumptions of a valid RD design (continuous baseline covariates at the threshold and manipulation of the forcing variable) and test for jumps at non-discontinuity points. Finally, in the presentation of the results, absence due to an individual's own illness is emphasized because this type of absence comprises the majority of the sick leave.

5.1 Paternity Leave Uptake

Before proceeding to the estimation results, we want to establish that there is a discontinuity in treatment around the cut-off. Figure 1 illustrates the leave uptake by birthdate for fathers before and after the reform was implemented. The graph clearly shows a great jump in the leave uptake rate at 1 April 1993 from about 0.5 percent to more than 40 percent. Table 2 presents the regression analog of the graph using the same bandwidth as in the outcome regression (as suggested by Lee and Lemiuex, 2009). Fathers with children born after 1 April 1993 take on average 38 percentage points more leave than those with children born before the reform. The result coincides well with the figure. Overall, a discontinuity clearly exists in the treatment variable at the cut-off.

[Figure 1 around here]
[Table 2 around here]

5.2 Main Results for Men

Figure 2 presents the visual representation of the effect of the implementation of the paid parental leave reform on sick leave due to an individual's own sickness. There appears to be a small positive discontinuity in the short run (the second through fourth years after the child's birth) and in the long run (eight through tenth years after the child's birth) with an increase in the sick leave rate of about 3 and 6 percentage points, respectively. In the period in between (that is, the fifth through seventh years after the child's birth), there is no visual jump in the absence rate around the introduction of the reform.

Table 3 displays the estimated effect of the empirical model in equations (1) and (2) using non-parametric regression.[14] The point estimates are insignificant at conventional levels, but suggest quite substantial effects. For instance, in the long run there is an increase in the absence rate from 3.4 to 4.8 percent for fathers of children born before and after the reform, respectively. When we divide the sample into age cohorts, we find that the effect on sickness absence varies with age, although none of the estimates are statistically significant.[15] Table 4 displays the results. Almost all of the estimates are positive, but the sizes of the coefficients differ. For the youngest and oldest fathers, the positive effect on the sick leave rate is largest in the long run while for the fathers aged 30-34 the effect is largest in the middle period. Beyond this, there does not appear to be a mutual pattern across the age cohorts.

[Figure 2 around here]
[Table 3 around here]
[Table 4 around here]

Although the results are insignificant at conventional levels, the signs of the estimates are all positive. This is in line with the suggested hypothesis: that is, families reallocate family resources due to shock at the price of leisure, or the increased responsibility and work in the household have negative health effects on fathers. Most likely, we do not pick up illness through contamination since the sick leave encompasses only spells lasting more than 16

[14] Note that the estimated treatment effects in the table are not the regression analog of the graphs in Figure 2. The RD treatment estimator equals the discontinuity in the outcome at the threshold by the proportion of paternity leave used at the cut-off (cf. Section 3). The graphs, on the other hand, are the visual representations of the jump in the outcome at the cut-off.

[15] We also divided the sample by years of education and log of earnings. The results show the same pattern, i.e., no significant effects on the sick leave rate.

days. A possible explanation for the insignificant result is that the paternity quota is too short. It comprises only a few weeks of the total 42 weeks of paid leave. The length may not be enough to cause any substantial changes in fathers' absence behavior. In addition, the RD estimator identifies only a local effect for those born just around the cut-off (LATE). The immediate response to the reform might not be large enough to be picked up in the regression estimates. We know that the uptake rate of paternity leave kept rising steadily in the years following the reform and that fathers spent more time with their children (Rege and Solli, 2010; Cools *et al*., 2011). If we could include the births from the first year (or say two) after the reform was implemented, there could potentially be a larger jump in equation (1), which could alter the results.

Finally, the results for sick leave due to caring for family members are presented in Appendix Table 1 and Appendix Figure 2. The visual representation shows that there is a lot of variation in the sick leave rate and no sign of a significant jump around 1 April 1993. The estimation results are in general not statistically significant. The result supports Ekberg *et al*.'s (2005) findings. Evaluating the implementation of a parental quota during one month in Sweden in 1995, they found that fathers' share of caring for sick children was not significantly affected by the reform.

5.3 Main Results for Women

The results for women are also shown in Table 3, and the visual presentation is shown in Figure 2. The graphs in the figure show the opposite pattern as for men; for all observed years after the child's birth, sick leave due to an individual's own absence is lower for parents with children born after the reform. The jump is largest up to seven years after the reform was implemented while there appears to be no discontinuity in the long run.

The estimated treatment effects show a similar pattern; the signs of the coefficients are all negative. However, the point estimates are statistically insignificant. The effect is quite substantial, especially in the middle run, where the absence rate decreases from 4.5 to 2.9 percent for mothers of children born before and after the reform was implemented, respectively. The negative effects are in line with the hypothesis. Compared the results for fathers, the reform seems to lower the female absence rate and increase the male absence rate. One potential explanation for this pattern is that as fathers have become more involved in child care, the mothers' "burden" has been eased, thus causing a lower sick leave rate.

The results are in accordance with earlier findings on paternity leave. Bratberg and Naz (2009) found that the incidence of absence among mothers was reduced by about 5 to 10 percent from an average of 20 percent when fathers took more than four weeks of paternity leave (that is, take some of the gender-neutral quota). Ugreninov's (2012) results showed that treated mothers have on average 0.7 fewer days of sick leave (conditional upon being absent); however, the estimates are not statistically significant.

Looking across different age groups, none of the estimates are statistically significant. The results are presented in Table 4. As for men, no pattern appears across the age groups. There are some deviations between the groups where the youngest mothers show the most stable development across time. There are some positive coefficients, but the effects are quite small compared to the negative effects.

Regarding sick leave due to caring for family members, the results are shown in Appendix Table 1 and the visual representation of the jump in the outcome in Appendix Figure 2. As for men, we do not see any particular jump around the threshold in the graphs. The estimated results are all insignificant and positive. However, the average sick rate is below 1 percent, implying that the changes due to the reform are negligible compared to sick leave due to an individual's own illness.

When interpreting the results, we have to take some precautions. First, mothers' sick leave may be affected by the increase in parental leave beyond the paternity quota. Mothers were given an extra pre-birth week of leave, and the number of gender-neutral weeks was increased by two weeks (mainly taken by mothers, see Cools *et al*., 2011, Rege and Solli, 2010). However, as paternity leave is mainly used in the first years after a child's birth, we do not expect the leave increase to have a substantial long-term effect on the rate of absence due to sickness. Nevertheless, we cannot exclude that this could potentially affect mothers' absences due to sickness. Second, not all mothers reentered the labor market as quickly as the fathers after the paid leave period. Kindergarten was available in the 1990s, but not in all regions, forcing some mothers to prolong their leave. If the reform had any effect on the employment rate in the year we observe sick leave, this could cause a bias in the results. We return to this issue of composition bias in the next section.

5.4 Selection Issues

Non-employed individuals cannot be absent from work, and hence, we cannot measure their sickness absence. This creates a sample selection bias in the estimates (cf. section 3). In the time window we observe absenteeism (from the second through the tenth years after a child's birth), fathers and mothers are employed in 93 percent and 80 percent of the observed months, respectively. The rates are slightly higher than for the entire Norwegian population (82 percent and 77 percent for those aged 25-66) since we restricted the sample to only eligible parents. The employment rates indicate that the proportion of missing outcomes is quite small for men but somewhat larger for women. In this section, we investigate the composition bias more closely.

First, we compute the upper and lower bounds for the treatment effect (Horowitch and Manski, 1995; 2000).[16] The results for fathers and mothers are shown in Appendix Table 2. In the table, we see that the upper bound (xi) is positive, and the lower bound (xii) is negative for all estimates. This means that the treatment effect could be positive and negative for both parents, depending on the sick leave rate of those who are not employed. Although this can give us some idea of the possible treatment effect if the missing values were replaced with actual sick leave, it is not informative in the sense that the range of possible effects is very large. This applies especially for women, and is a known problem in the literature (Lee, 2009).

Therefore, and second, to get a better understanding of the composition bias we assigned different values to individuals and observations recorded as non-employed and reestimated the empirical model (as in Rieck and Telle, 2012).[17] The results are shown in Table 5. In specification (1), the average sick rate for the non-employed equals the average sick leave rate of employed individuals. If non-employed parents are not statistically different from employed parents (in characteristics that are determinate for the level of sickness absence), this could be a good measure of their latent sick leave rate. The results show that the estimated effects are very similar to those in the main analyses (shown in Table 3). This

[16] Note that the treatment bounds are boundaries for the jump in outcome, and not the estimated coefficients (that is, the Wald estimator that equals the jump in outcome divided by the jump in the treatment variable) that are shown in the regression results tables.

[17] Non-employed individuals can in some situations also report sick, for instance, when receiving unemployment benefits. In total, they comprise less than 2 and 4 percent of the non-employed observations for men and women. In these cases, we do not change their sickness absence but include their recorded value.

means that if employed and non-employed parents had the same sick leave rate (that is, 2.58 and 4.69 percent for men and women), we would be close to the “true” treatment effect. In specifications (2) through (5), the missing observations have been replaced with quartiles from 0.25 to 1 in increasing order. For men, this tends to amplify the treatment effect in the long run while turning the effect negative in the first years after the reform was implemented. The pattern is very stable across the different specifications. For women, we see the same stable pattern where the treatment effect is enlarged in the short and long run while becoming positive five to seven years after the child’s birth.

The analysis shows the latent sickness absence of non-employed individuals is important to the overall treatment effect. If their sick leave rate had been similar to the average level of employed parents, we would be close to the “real” treatment effect. If the non-employed individuals had been more prone to being absent, we would in general expect a larger effect. However, there is one precaution we have to take when interpreting the results. In this exercise, we replaced the missing values for the control and treatment groups with similar rates. The non-employed in the two groups might have different rates. If this difference is substantial, it could potentially affect the direction of the bias and the results.

[Table 5 around here]

6. Standard RD and robustness checks

There are two main conceptual concerns in applying RD designs: discontinuities in covariates at the threshold and manipulation of the forcing variable (day of birth). Imbalance and manipulation are threats to the validity of the results, and if present, they cast doubt on the assumption behind the RD design. In this section, we address both concerns. In addition, we explore the sensitivity of the results by testing for jumps at non-discontinuity points (i.e., placebo tests), and explore the results using different bandwidths.

6.1 Planning Births

If couples could systematically manipulate the day of birth, we would expect the number of births to be substantially higher right after the reform was implemented compared to right before. It is natural to assume that if you were given a choice between receiving and not receiving paid paternity leave, nearly everyone would accept. A simple method to check for manipulation of the forcing variable is to plot a histogram of the data at the level the data was collected and look for abnormal heaps (Lee and Lemiuex, 2009). Figure 3 shows the number of births 90 days before and 90 days after the reform was implemented. According to the graph, no systematic difference between the number of births on either side of the threshold appears. We also estimated the empirical model with the number of births per day as the outcome variable. The result is presented in Table 6 where the estimated coefficient is positive but insignificant.[18] As a result, we find no evidence of abnormal heaping to the right of the cut-off.

[Figure 3 about here]
[Table 6 about here]

6.2 The Role of Covariates

Table 7 shows the results from a number of demographic characteristics such as parents' age, total number of years of education, and log of earnings. All variables are recorded in the year before the child's birth so they cannot be influenced by the reform. The estimated regression is the same as for sickness absence where the dependent variable in equation (1) is replaced with the demographic variables. We also use the same bandwidth as the outcome variables as suggested in the literature (see e.g. Lee and Lemiuex, 2009; Imbens and Lemiuex, 2008). Figure 4 shows the graphical visualization of the jumps in covariates around the time the reform was implemented.

The graphs show some small discontinuity for age for both parents and education for men. The graphs suggest that parents of children born after the reform are somewhat younger, are less educated, and earn less. However, turning to the estimated regressions, the jumps are not significantly different from zero at conventional levels. Only for women the age variable is significant, but only at the 10 percent level. The parents of children born before and after the reform are therefore not in any significant way different from each other. We are not

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[18] Estimating the model with polynomial regressions with the instrumental variable approach (cf. footnote 9) shows no significant difference between the number of births before and after the reform.

concerned that any of the socioeconomic characteristics have influenced the realization of the birthdate (that is, if the child is born before or after the reform).

[Figure 4 about here]

[Table 7 about here]

6.3 Bandwidth Selection

Table 8 shows the estimates of the empirical model in equations (1) and (2) for bandwidths between 30 and 70 days. Overall, the results are qualitatively consistent across different bandwidths. The effect on sickness absence is mainly positive for men and negative for women, although not significant for any of the specifications. The size of the coefficients is quite similar for all but the smallest bandwidth. This is likely because for smaller bandwidths the number of observations available for regression decreases, which introduces more bias and less precision. To summarize, we find the results from the main analysis fairly robust.

[Table 8 about here]

6.4 Other Cut-offs

Finally, we examine whether there are any discontinuities where there should be no jumps. When implementing the test, we use the same RD setup as before but with a smaller bandwidth. Only observations on the left of the threshold are used when moving the cut-off point to the left of 1 April 1993 and likewise when moving to the right of the cut-off. This avoids estimating the regressions at the threshold point (as suggested by Imbens and Lemiuex, 2008).[19]

Table 9 displays the results for sick leave due to an individual's own sickness. For the sake of comparison, the estimates from the implementation of the reform, 1 April 1993, are included with the same bandwidth as for the other cut-offs. The results show that for men and women there are no significant discontinuities. Most of the estimates are unreasonably large with sizable standard errors. In other words, when we move away from the 'real' cut-

[19] Smaller bandwidth tends to increase the bias in the estimates and lower the precision because there are fewer observations in the regression sample. However, we estimated the model for a range of bandwidths, and the results are qualitatively not different from the ones presented here.

off, the estimates do not pick up a clear discontinuity, but rather noise. This gives support to the interpretation of the reform estimates as real treatment effects.

[Table 9 about here]

7. Concluding remarks

In this study, we investigated whether paternity leave has an effect on the sickness absence of mothers and fathers of children born before and after the first paternity quota was implemented in Norway on 1 April 1993. The reform was a “take-it-or-leave-it” option for eligible fathers in that it was non-transferable to mothers. The leave uptake rate rose abruptly after the reform was introduced, from about 0.5 percent in March 1993 to about 40 percent in April 1993. We hypothesized that the reform could have negative implications for fathers, causing a higher sickness absence rate, while affecting mothers in the opposite direction (less sick leave). There are several reasons why we would expect this, but mainly we argue that the reform could shift the price of leisure for fathers and thus induce them to be more absent from work, or that increased involvement in child care would increase the overall workload, which could result in poorer health and more sick leave. For mothers, these actions could spill over in a positive effect: relief of duties at home or increased value of labor work, which in turn could reduce their sickness absence.

The results for sickness absence due to an individual’s own illness show that fathers are not significantly affected by the reform. The signs of the coefficients are all positive, which is in line with the hypothesized mechanisms. Regarding mothers, the estimates suggest a decrease in the sick leave rate during the same period, but they are not statistically significant either. The findings are supported by standard RD and robustness tests.

From a policy perspective, our findings for fathers are positive. Paternity quotas of paid parental leave were introduced by policymakers to provide incentives for men to increase their child care responsibilities and household involvement, and to increase female labor participation. Although other studies confirm that short- and long-term child involvement has increased, our study shows that paternity leave has no deteriorating effect on fathers’ sick leave.

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Tables

Table 1. Summary statistics

	Men				Women			
	Control group		Treatment group		Control group		Treatment group	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Sick leave due to one's own absence	0.0261	0.1499	0.0256	0.1484	0.0463	0.1980	0.0474	0.2007
Sick leave due to others' illness	0.0004	0.0159	0.0004	0.0182	0.0008	0.0237	0.0008	0.0256
Parental leave	0.04	0.19	0.44	0.50	0.91	0.29	0.97	0.17
Employment	0.93	0.28	0.93	0.25	0.81	0.39	0.80	0.40
Age	31.22	5.20	30.96	5.13	28.67	4.48	28.37	4.40
Education	13.05	3.07	13.04	3.05	13.09	2.79	13.18	2.77
Log of earnings	12.30	0.38	12.29	0.38	11.88	0.41	11.86	0.42
Observations	696,924		765,936		696,924		765,936	
Individuals	6,453		7,092		6,453		7,092	

Note: The sample consists of fathers and mothers of children born around 1 April 1993 where both parents are eligible for parental leave. Sickness absence and employment are measured from the 2nd to the 10th years after the child's birth. Parental leave is a binary variable equal to one if the parent has been on parental leave after the child's birth. Age, education, and log of earnings are measured in the year before the child's birth. See section 4.2 for more details.

Table 2. Leave uptake by date of birth

	RD estimate
Reform	0.3838***
	(0.0169)
Mean of dependent variable	0.2430
N	13,545

Note: The sample consists of fathers of children born around 1 April 1993 where both parents are eligible for parental leave. The table displays the estimation result from local linear regression as in Hahn et al. (2003) with a triangle kernel. A bandwidth of 55 days is used, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 3. Results for sick leave due to one's own sickness

	Men	Women
2nd –4th year after birth	0.0061	-0.0089
	(0.0061)	(0.0096)
Mean of dependent variable	0.0181	0.0420
5th–7th year after birth	0.0008	-0.0158
	(0.0080)	(0.0122)
Mean of dependent variable	0.0251	0.0445
8th–10th year after birth	0.0141	-0.0024
	(0.0088)	(0.0133)
Mean of dependent variable	0.0340	0.0542
N	487,620	487,620

Note: The sample consists of fathers of children born around 1 April 1993 where both parents are eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel. A bandwidth of 55 days is applied, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4. Results for different age groups. Sick leave due to one's own sickness

	Age 17-29		Age 30-34		Age 35+	
	Men	Women	Men	Women	Men	Women
2^{nd}–4^{th} year after birth	0.0117	-0.0081	-0.0012	-0.0195	0.0097	0.0021
	(0.0127)	(0.0142)	(0.0113)	(0.0152)	(0.0141)	(0.0285)
Mean of dependent variable	0.0194	0.0479	0.0276	0.0471	0.0341	0.0569
5^{th}–7^{th} year after birth	0.0003	0.0001	0.0105	-0.0326	-0.0120	-0.0385
	(0.0156)	(0.0133)	(0.0145)	(0.0217)	(0.0179)	(0.0335)
Mean of dependent variable	0.0172	0.0357	0.0232	0.0409	0.0307	0.0503
8^{th}–10^{th} year after birth	0.0351^{*}	-0.0209	-0.0017	0.0180	0.0129	0.0090
	(0.0170)	(0.0160)	(0.0137)	(0.0177)	(0.0200)	(0.0337)
Mean of dependent variable	0.0276	0.0335	0.0246	0.0429	0.0382	0.0528
N	164,520	164,520	182,376	182,376	140,724	140,724

Note: The sample consists of fathers of children born around 1 April 1993 where both parents are eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel. A bandwidth of 55 days is applied, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 5. Results when replacing the missing sickness absence values (due to one's own sickness) with other values

	(1)		(2)		(3)		(4)		(4)	
	Men	Women	Men	Women	Men	Women	Men	Women	Men	Women
2^{nd}–4^{th} year after birth	0.0058	-0.0094	0.0010	-0.0119	-0.0042	-0.0148	-0.0094	-0.0177	-0.0146	-0.0206
	(0.0068)	(0.0092)	(0.0079)	(0.0122)	(0.0115)	(0.0218)	(0.0150)	(0.0313)	(0.0192)	(0.0392)
5^{th}–7^{th} year after birth	0.0014	-0.0129	0.0062	-0.0031	0.0116	0.0096	0.0170	0.0223	0.0224	0.0350
	(0.0098)	(0.0111)	(0.0101)	(0.0130)	(0.0157)	(0.0229)	(0.0200)	(0.0306)	(0.0241)	(0.0422)
8^{th}–10^{th} year after birth	0.0144	-0.0037	0.0159	-0.0069	0.0178	-0.0115	0.0196	-0.0160	0.0215	-0.0205
	(0.0097)	(0.0121)	(0.0115)	(0.0156)	(0.0172)	(0.0207)	(0.0211)	(0.0261)	(0.0280)	(0.0373)
N	487,620	487,620	487,620	487,620	487,620	487,620	487,620	487,620	487,620	487,620

Note: The sample consists of fathers and mothers of children born around 1 April 1993 where both parents are eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel. A bandwidth of 55 days is applied, and the standard errors are clustered on personal identification by bootstrapping. In specification (1), the missing sick leave is replaced with the average sickness absence of employed individuals per month. In specifications (2) to (5), the missing observations are replaced with 0.25, 0.5, 0.75, and 1, respectively. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 6. Estimation results for date of birth

	RD estimator
Date of birth	0.0004
	(0.0038)
N	180

Note: The sample consists of births per day around 1 April 1993 where both parents are eligible for parental leave. The table displays the estimation result from local linear regression as in Hahn et al. (2003) with a triangle kernel. A bandwidth of 55 days is used, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 7. Examining baseline covariates

	Men	Women
Earnings	-0.0323	-0.0138
	(0.0534)	(0.0499)
Mean of dependent variable	12.2882	11.8646
Years of education	-0.3220	-0.0168
	(0.3293)	(0.3242)
Mean of dependent variable	13.0441	13.1378
Age	-0.7243	-1.0398*
	(0.6157)	(0.5820)
Mean of dependent variable	31.0737	27.4700
N	13,545	13,545

Note: The sample consists of fathers of children born around 1 April 1993 where both parents eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel. A bandwidth of 55 days is applied, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 8. Sensitivity to bandwidth selection for sickness absence

Bandwidth	30	40	50	55	60	70
Men						
2^{nd}–4^{th} year after birth	0.0026	0.0057	0.0059	0.0061	0.0055	0.0039
	(0.0035)	(0.0030)	(0.0027)	(0.0026)	(0.0024)	(0.0023)
5^{th}–7^{th} year after birth	0.0005	0.0015	0.0010	0.0008	0.0005	0.0019
	(0.0043)	(0.0036)	(0.0032)	(0.0031)	(0.0029)	(0.0027)
8^{th}–10^{th} year after birth	0.0246	0.0196	0.0158	0.0141	0.0114	0.0075
	(0.0046)	(0.0040)	(0.0036)	(0.0034)	(0.0033)	(0.0031)
Women						
2^{nd}–4^{th} year after birth	-0.0004	-0.0073	-0.0093	-0.0089	-0.0085	-0.0083
	(0.0052)	(0.0045)	(0.0040)	(0.0038)	(0.0037)	(0.0034)
5^{th}–7^{th} year after birth	-0.0087	-0.0148	-0.0166	-0.0158	-0.0154	-0.0136
	(0.0056)	(0.0048)	(0.0043)	(0.0041)	(0.0039)	(0.0036)
8^{th}–10^{th} year after birth	-0.0130	-0.0059	-0.0031	-0.0024	-0.0010	0.0011
	(0.0061)	(0.0053)	(0.0047)	(0.0045)	(0.0043)	(0.0040)
N	487,620	487,620	487,620	487,620	487,620	487,620

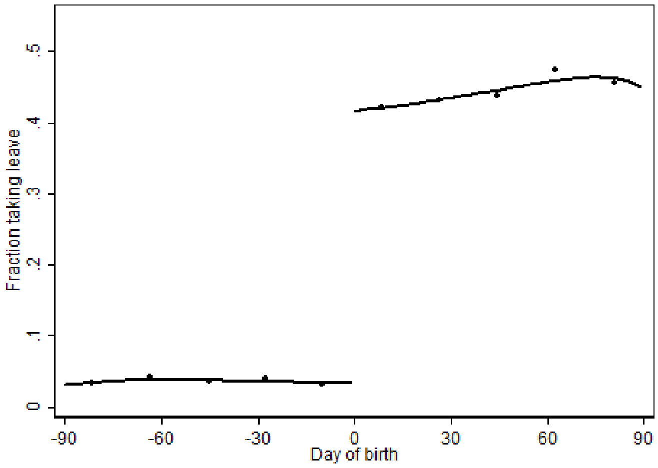
Note: The sample consists of fathers of children born around 1 April 1993 where both parents are eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel where different bandwidths have been applied. The standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 9. Examining cut-offs for sick leave due to an individual's own sickness

Cut off point	Bandwidth	Men			Women		
		2nd–4th year after birth	5th–7th year after birth	8th–10th year after birth	2nd–4th year after birth	5th–7th year after birth	8th–10th years after birth
1 February	30	-1.2158	-2.2085	-4.4980	-1.9784	-2.8754	2.6943
		(2.9635)	(2.3284)	(5.8750)	(2.4873)	(31.4180)	(4.9092)
15 February	30	0.1171	-0.4898	0.0373	-0.2473	-0.2450	0.4639
		(133.0209)	(43.3067)	(2.7578)	(0.6203)	(3.5429)	(10.9305)
1 March	30	0.2286	-0.2222	1.3600	0.2726	0.9771	0.5367
		(2.7176)	(1.6785)	(9.9123)	(2.9390)	(5.2122)	(36.2926)
1 April	30	0.0026	0.0005	0.0246	-0.0004	-0.0087	-0.0130
		(0.0084)	(0.0122)	(0.0137)	(0.0124)	(0.0156)	(0.0180)
1 May	30	-0.5509	-0.0597	0.2990	-0.0637	-0.0975	-0.1549
		(47.6377)	(0.9920)	(19.9094)	(1.3102)	(8.0340)	(1.0908)
15 May	30	0.2276	-0.1597	0.1422	0.1635	0.1844	0.0179
		(0.0319)	(0.0633)	(1.0615)	(6.8450)	(2.4514)	(1.0361)
1 June	30	0.2207	-0.1420	-0.1068	-0.0453	-0.4942	-0.6930
		(0.9841)	(3.4662)	(2.1168)	(1.9920)	(1.5410)	(5.7159)
N							

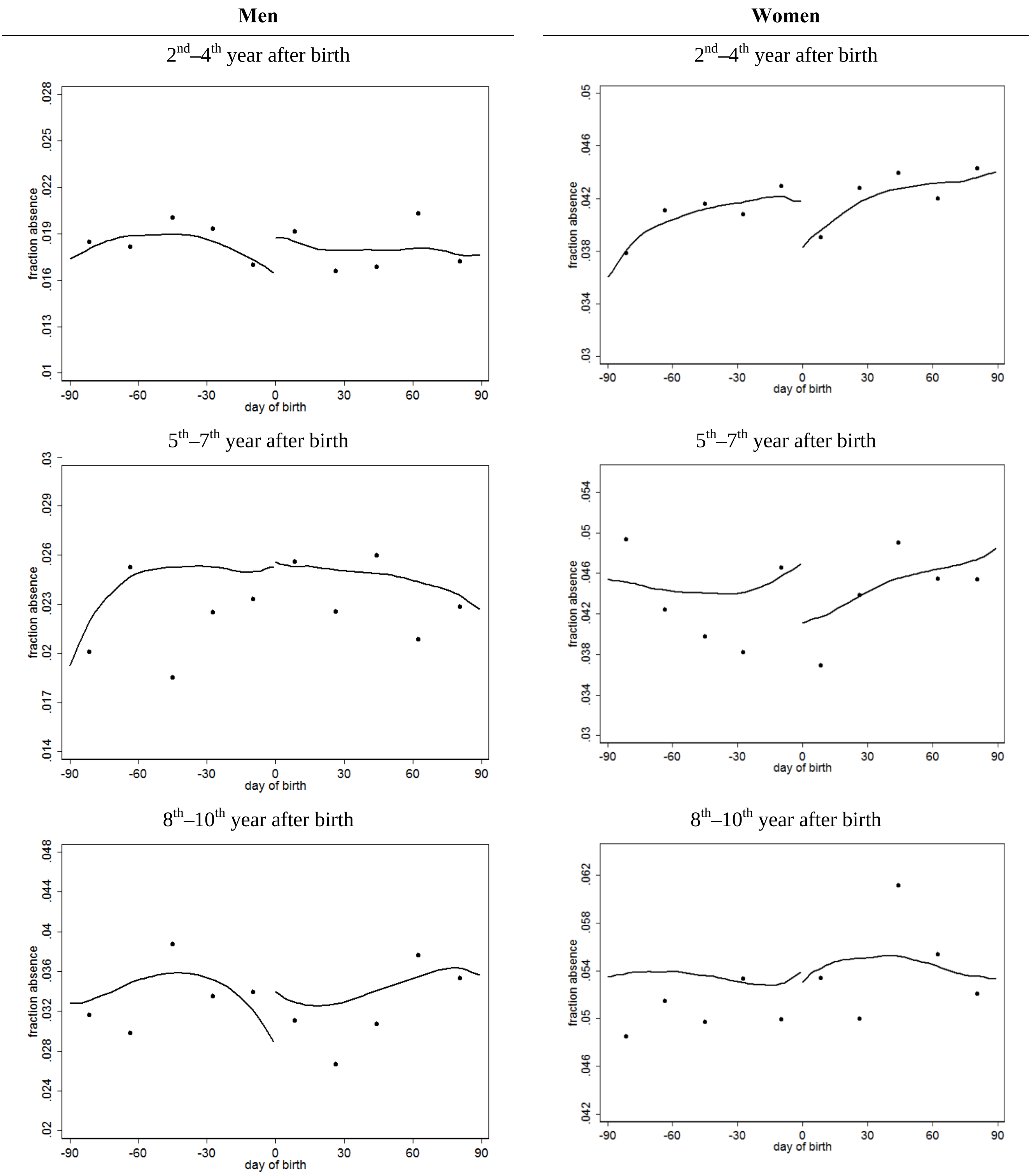
Note: The sample consists of fathers of children born around 1 April 1993 where both parents eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel where the threshold point has been moved to the left and the right of the "true" cut-off (1 April). A bandwidth of 30 has been applied, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Figure 1: Paternity leave uptake according to date of birth



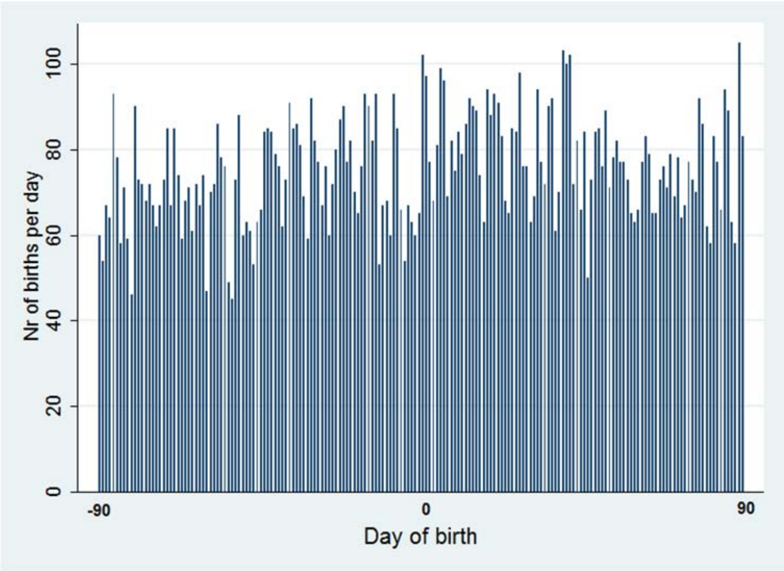
Note: The graph displays the proportion of fathers taking leave before and after the reform, 1 April 1993. The date of birth is transformed so that the cut-off at 0 equals the day of implementation. The solid line is fitted non-parametrically using a triangle kernel and a bandwidth of 55. The dots are the mean value for each 18th day.

Figure 2. Results for sick leave due to an individual's own sickness



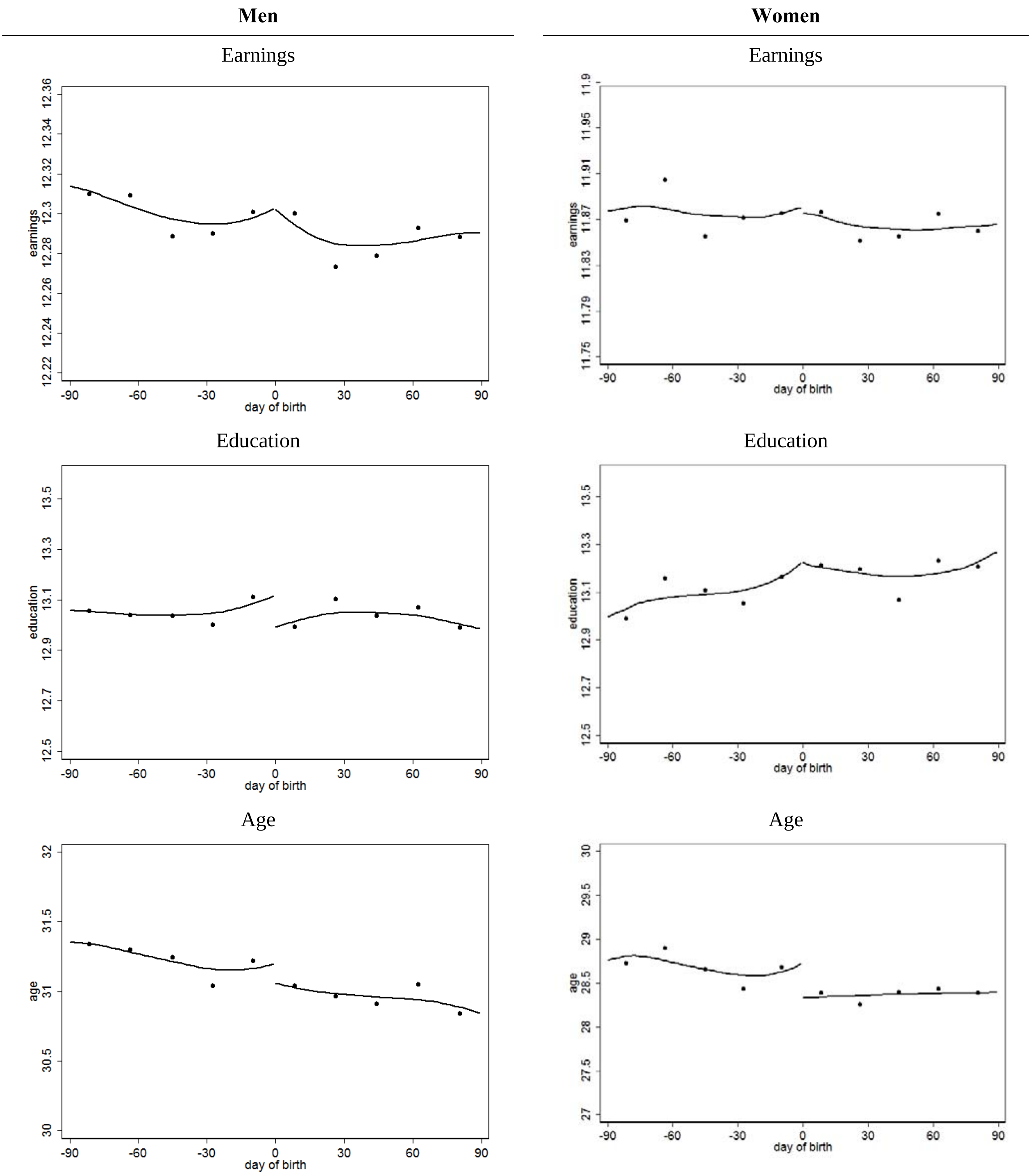
Note: The graphs display the sickness absence for fathers and mothers of children born before and after the reform, 1 April 1993. The date of birth is transformed so that the cut-off at 0 equals the day of implementation. The solid line is fitted non-parametrically using a triangle kernel and a bandwidth of 55. The dots are the mean value for each 18th day.

Figure 3: Histogram of the date of birth



Note: The graph displays the total number of births per day before and after the reform, 1 April 1993. The day of birth is transformed so that the cut-off at 0 equals the date of implementation.

Figure 4. Examining baseline covariates



Note: The graphs display age at birth, log of earnings, and years of education of parents of children born before and after the reform, 1 April 1993. The date of birth is transformed so that the cut-off at 0 equals the date of implementation. All of the baseline covariates are measured in the year before the birth of a child. The solid line is fitted non-parametrically using a triangle kernel and a bandwidth of 55. The dots are the mean value for each 18th day.

Appendix Table 1. Results for sick leave due to others' sickness

	Men	Women
2nd–4th year after birth	0.0002	0.0005
	(0.0003)	(0.0013)
Mean of dependent variable	0.0003	0.0007
5th–7th years after birth	0.0002	0.0005
	(0.0004)	(0.0005)
Mean of dependent variable	0.0004	0.0006
8th–10th years after birth	-0.0006	0.0002
	(0.0008)	(0.0013)
Mean of dependent variable	0.0006	0.0009
N	487,620	487,620

Note: The sample consists of fathers of children born around 1 April 1993 where both parents are eligible for parental leave. The table displays estimation results from local linear regression as in Hahn et al. (2001) using a triangle kernel. A bandwidth of 55 days is applied, and the standard errors are clustered on personal identification by bootstrapping. *** significant at 1%, ** significant at 5%, * significant at 10%.

Appendix Table 2. Bounds for sick leave effects due to an individual's own sickness accounting for different employment rates

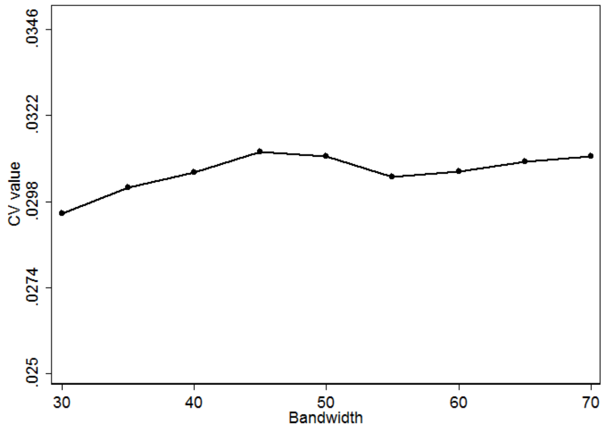
	Men			Women		
	2^{nd}–4^{th} year after birth	5^{th}–7^{th} year after birth	8^{th}–10^{th} year after birth	2^{nd}–4^{th} year after birth	5^{th}–7^{th} year after birth	8^{th}–10^{th} year after birth
Control group, fathers to children born < 1 April 1993						
(i) Observations	157,536	157,536	157,536	157,536	157,536	157,536
(ii) Employment rate	0.9516	0.9390	0.9118	0.7932	0.7903	0.8165
(iii) Mean sickness absence for employed observations	0.0175	0.0243	0.0348	0.0472	0.0476	0.0500
(iv) Upper bound	0.0651	0.0838	0.1199	0.2442	0.2473	0.2243
(v) Lower bound	0.0167	0.0228	0.0317	0.0037	0.0376	0.0408
Treatment group, fathers to children born ≥ 1 April 1993						
(vi) Observations	174,384	174,384	174,384	174,384	174,384	174,384
(vii) Employment rate	0.9493	0.9359	0.9124	0.8010	0.8174	0.8141
(viii) Mean sickness absence for employed observations	0.0171	0.0250	0.0331	0.0509	0.0600	0.0606
(ix) Upper bound	0.0669	0.0801	0.1178	0.2400	0.2316	0.2352
(x) Lower bound	0.0162	0.0160	0.0302	0.0408	0.0490	0.0493
Difference						
(xi) Upper bound: (ix)-(v)	0.0502	0.0573	0.0861	0.2363	0.1940	0.1944
(xii) Lower bound: (x)-(iv)	-0.0489	-0.0678	-0.0897	-0.2034	-0.1983	-0.1750
Estimate from regressions						
Wald estimator (increase in sick leave/increase in pat. leave)	0.0061	0.0008	0.0141	-0.0089	-0.0158	-0.0024
Increase in sick leave	0.0024	0.0003	0.0054	-0.0034	-0.0060	-0.0009

Note: The sample consists of fathers and mothers of children born around 1 April 1993 where both parents are eligible for parental leave. The upper and lower bounds for the support of sickness absence is 0 and 1, respectively. (iv) = (ii)x(iii) + [1-(ii)]x1 and (v) = (ii)x(iii) + [1-(ii)]x0. Rows (ix) and (x) are defined analogously.

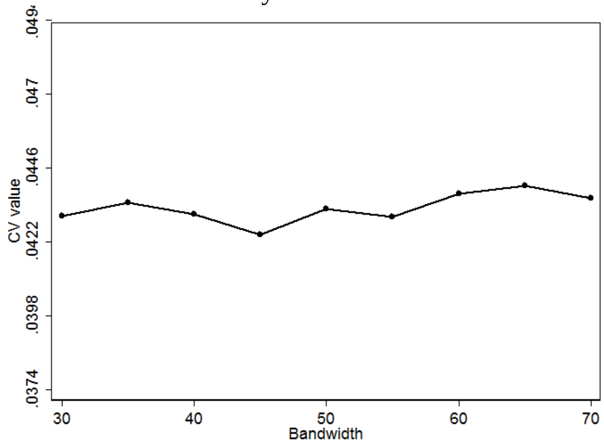
Appendix Figure 1. Cross-validation approach for choosing bandwidth

Men

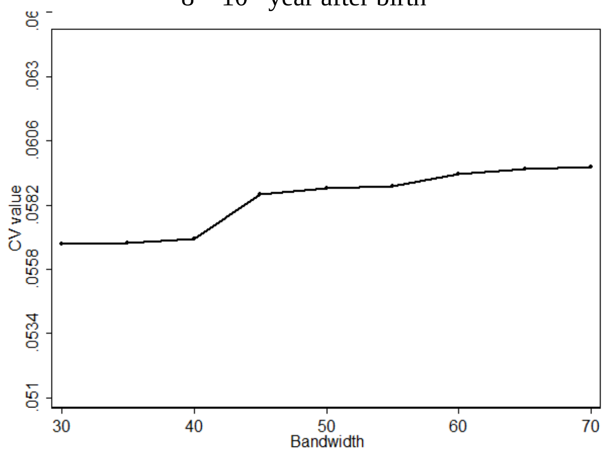
2^{nd}–4^{th} year after birth



5^{th}–7^{th} year after birth

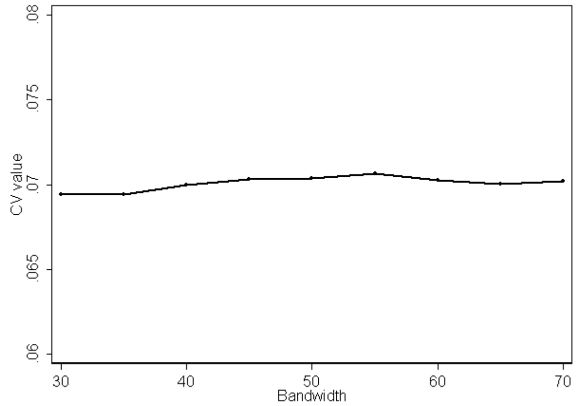


8^{th}–10^{th} year after birth

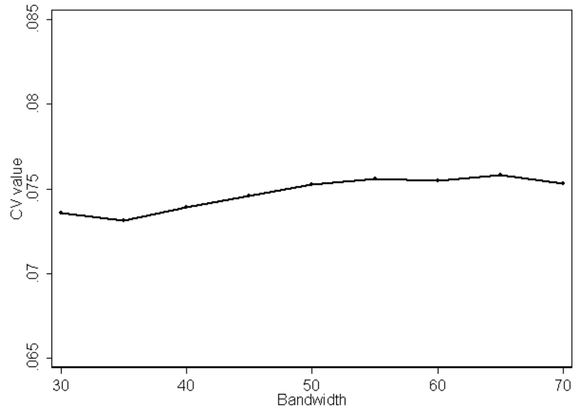


Women

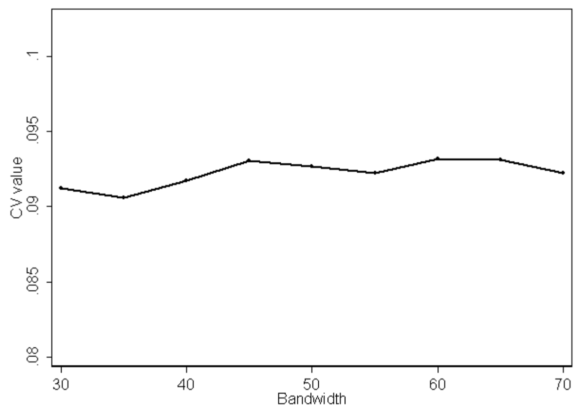
2^{nd}–4^{th} year after birth



5^{th}–7^{th} year after birth



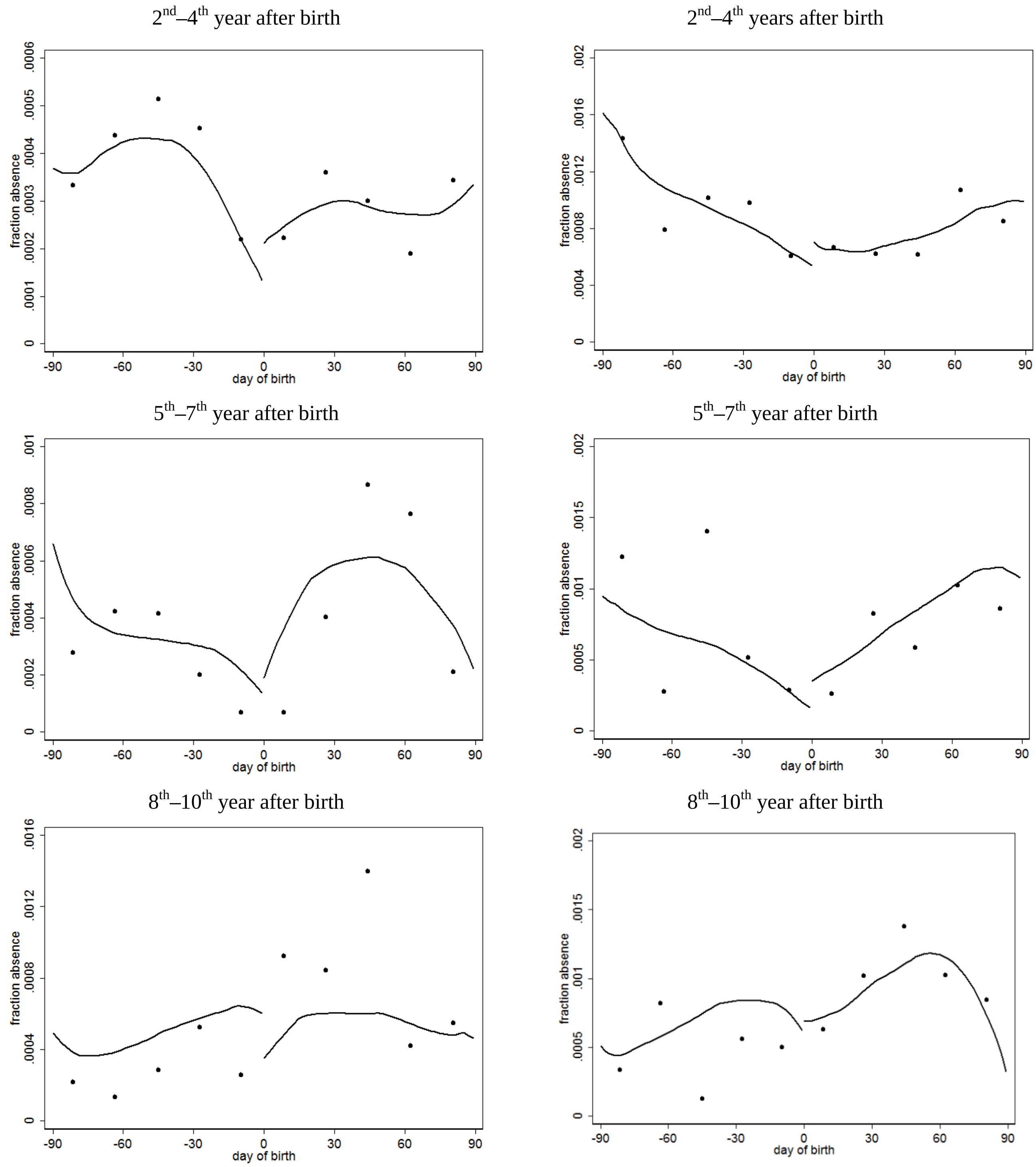
8^{th}–10^{th} year after birth



Note: The graph displays cross-validation results for both sides of the cut-off for different choice of bandwidth.

Appendix Figure 2. Results for sick leave due to others' sickness

Men	Women



Note: The graphs display the sickness absence for fathers and mothers of children born before and after the reform, 1 April 1993. The date of birth is transformed so that the cut-off at 0 equals the day of implementation. The solid line is fitted non-parametrically using a triangle kernel and a bandwidth of 55. The dots are the mean value for each 18th day.

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