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When Dad Can Stay Home: Fathers' Workplace Flexibility and Maternal Health

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Abstract

While workplace flexibility is perceived to be a key determinant of maternal labor supply, less is known about fathers' demand for flexibility or about intra-household spillover effects of flexibility initiatives. This paper examines these issues in the context of a critical period in family life—the months immediately following childbirth—and identifies the impacts of paternal access to workplace flexibility on maternal postpartum health. We model household demand for paternal presence at home as a function of domestic stochastic shocks, and use variation from a Swedish reform that granted new fathers more flexibility to take intermittent parental leave during the postpartum period in a regression discontinuity difference-in-differences (RD-DD) design. We find that increasing the father's temporal flexibility reduces the risk of the mother experiencing physical postpartum health complications and improves her mental health. Our results suggest that mothers bear the burden from a lack of workplace flexibility—not only directly through greater career costs of family formation, as previously documented—but also indirectly, as fathers' inability to respond to domestic shocks exacerbates the maternal health costs of childbearing.

JEL classification: I12, I18, I31, J12, J13, J38

Keywords: workplace flexibility, intra-household spillovers, maternal postpartum health, paternity leave

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1 Introduction

Temporal flexibility in the workplace is increasingly important for modern households in which both parents work. Workplace flexibility allows parents to rearrange their work hours in case of an unforeseen family need—such as a child's sickness or a snow day—while minimizing work interruption. In other words, workplace flexibility often generates flexibility in *when to stay home from work*. As mothers are more likely to be "on call" for unanticipated domestic events (Weeden et al., 2016), a burgeoning literature identifies workplace flexibility as a key factor for improving maternal labor market outcomes and further reducing the gender pay gap (Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016).

Yet other important aspects of workplace flexibility remain less well understood. First, little is known about *fathers'* demand for workplace flexibility. Second, while a few studies show that work-related stress propagates throughout the family—e.g., individuals' work hours negatively affect their relationship quality and their partners' wellbeing (Shafer et al., 2018; Fan et al., 2019)—there is scarce evidence on the possible intra-household spillover effects of workplace flexibility policies. Such impacts would be consistent with a broad range of economic models of the household, which posit that an expansion of the choice set for one spouse (as a result of workplace flexibility initiatives, for example) would induce household re-optimization that may alter the wellbeing of the other spouse (see, e.g., Becker, 1973; Chiappori, 1992; Lundberg and Pollak, 1993; Persson, Forthcoming). Third, relative to our understanding of the consequences of workplace flexibility for the *career* cost of family formation, we know less about its impacts on the other costs associated with having children.

This paper begins to fill these gaps by analyzing fathers' demand for workplace flexibility and the spillover effects of fathers' access to workplace flexibility on maternal wellbeing. We focus on a critical period in family life, when spillovers may be especially important: the months immediately following childbirth. In this period, for a mother, the major cost of having a family is *not* the cost to her career—which grows in magnitude and importance over time since childbirth (see, e.g., Kleven et al., 2018)—but instead the health cost associated with postpartum recovery. A substantial share of all new mothers experience physical health problems, and many have complications that require medical care.[1] Postpartum mental health

[1]Studies from multiple countries document that between 23 and 83 percent of new mothers experience pain

issues are also common and inflict large private and social costs.[2] Thus, we ask whether workplace flexibility for new fathers generates spillover benefits through improvements in maternal postpartum health.

To answer this question, we take advantage of a Swedish social insurance reform that effectively increased workplace flexibility for new fathers by relaxing a central restriction in the parental leave system. At the time of the reform, Swedish households were granted 16 months of job-protected paid leave (per child), to be allocated across the two parents.[3] However, parents were generally not allowed to be on leave *at the same time*—in fact, simultaneous leave use was permitted for only 10 days around childbirth (hereafter referred to as "baseline leave"). Since nearly all mothers take full-time leave in the months following childbirth, this rule effectively limited fathers' ability to use paid leave alongside the mother.[4]

The "Double Days" reform, implemented on January 1, 2012, relaxed this restriction by allowing both parents to use full-time leave benefits at the same time for up to 30 additional days during the child's first year of life. Importantly, these "Double Days" could be taken intermittently; thus, fathers were effectively granted more flexibility to choose, on a day-to-day basis, whether to claim paid leave to stay home together with the mother and child.

A father's access to such workplace flexibility could impact maternal health in several ways. If a mother experiences post-childbirth complications, breastfeeding difficulties, or severe fatigue, the father's ability to stay home may allow her to rest or seek prompt medical attention. This option may be especially important for mothers who have a history of medical issues prior to childbirth, and therefore are more prone to postpartum complications. Fathers may also play a role in reducing maternal loneliness and stress, which have been found to

in various parts of their bodies (including the perineum, cesarean-section incisions, the back or the head) in the months following childbirth (see Cheng et al., 2006 for an overview). In the United States, more than one out of every 100 new mothers is readmitted into the hospital within 30 days after childbirth (Clapp et al., 2017). In Sweden, our data show that 5 percent of new mothers are hospitalized in the first 6 months after childbirth, while 8 and 16 percent require prescription painkiller and antibiotic drugs, respectively.

[2]Recent estimates suggest that about one in nine women in the U.S. report symptoms of postpartum depression (Ko et al., 2017). In Sweden, around 11 and 14 percent of new mothers are found to have depressive symptoms based on the Edinburgh Postnatal Depression Scale at two months and one year post-childbirth, respectively (Rubertsson et al., 2005). Our data also show that 4 percent of new mothers are prescribed anti-depressant or anti-anxiety medication in the first 6 months after giving birth.

[3]Parents faced some restrictions on how to split this leave. In particular, at the time of the reform, two months were earmarked for each parent. See Section 2 for details.

[4]Among parents of firstborn singleton children born in 2008-2011, the median mother was at home alone on full-time leave for about 14 months, after which she returned to work and the median father took two months of leave. See Section 2 for more details.

be related to the onset of postpartum depression and anxiety symptoms (Chan et al., 2002; Cairney et al., 2003; Honey et al., 2003; Corwin et al., 2005). Alternatively, it is possible that providing fathers with the option to spend more time at home could negatively affect maternal well-being, especially if the quality of the parental relationship is low.[5]

To understand household demand for father presence at home as well as the potential impacts of father presence on maternal wellbeing, we begin with a simple theoretical framework. We consider a household consisting of a mother, a father, and one child. The household decides, on a day-to-day basis, whether the father should work in the labor market or stay at home with the mother (and child). On any given day, this decision reflects a trade-off between the household's benefit from father presence and the associated costs (stemming from his lost income as well as the opportunity cost of a future parental leave day). Importantly, the benefit of father presence at home varies from day to day; intuitively, additional support for the mother may be particularly valuable on some days, such as when she is not feeling well, needs medical care, or suffers from stress or anxiety. Our analysis of optimal household behavior in this framework emphasizes that, in a setting where households have the flexibility to decide when to take simultaneous leave, the *timing* of the take-up of a joint day of parental leave is not random. Instead, households optimally respond to the need for maternal support by removing the father from the labor force on precisely the days when the household has private information that the benefit of doing so is the highest.[6]

Next, to provide a comprehensive empirical analysis of the effects of the "Double Days" reform on fathers' leave use and maternal health, we link multiple sources of Swedish administrative data, including birth records, parental leave claims, as well as inpatient, specialist outpatient, and prescription drug records. We use data on parents with first births of singleton children in 2008-2012, and implement a Regression Discontinuity Difference-in-Differences (RD-DD) research design. Our preferred specification compares the outcomes of parents of

[5]Fatigue and stress associated with having a newborn may exacerbate conflict between parents. Studies show that intimate partner violence (IPV) and psychological abuse can escalate during pregnancy and the postpartum period (Cheng and Horon, 2010; Brownridge et al., 2011), leading to maternal physical injuries and mental health problems (Romito et al., 2009).

[6]In this simple setting, providing fathers with the option to spend more time at home would always (weakly) positively affect household utility, as this represents an expansion of the household's choice set. Providing fathers with this option could instead negatively affect household utility if the household has incorrect beliefs about the benefits and costs of joint leave-taking, e.g., if simultaneous leave induces an unanticipated deterioration of the relationship.

children born in the 3 months before and after the reform, relative to the analogous difference between these birth months in the three preceding years. Our empirical strategy thus exploits the change in eligibility for simultaneous leave for parents of children born shortly after the reform, while differencing out other sources of variation in family outcomes between October-December and January-March births.[7]

We first document households' demand for paternal workplace flexibility. The "Double Days" reform raises the likelihoods that fathers use more than the 10 days of baseline leave (hereafter referred to as "post-baseline leave") in the first 60 and 180 days after childbirth by 3.9 and 5.9 percentage points, respectively, corresponding to 50 and 24 percent effects relative to the sample means. Interestingly, while the effects on *any* post-baseline leave use are substantial, we only observe a one to two day average increase in the total number of leave days taken by fathers in the first six months post-childbirth. Thus, it appears that the reform primarily affects fathers' leave use on the extensive, rather than intensive, margin.

Next, we show that workplace flexibility for fathers has positive spillover effects on maternal health. We find that the reform leads to a 1.5 percentage point (14 percent) reduction in the likelihood of a mother having an inpatient or specialist outpatient visit for childbirth-related complications, and a 1.9 percentage point (11 percent) reduction in the likelihood of her having an antibiotic prescription drug in the first six months postpartum. Further, we find an improvement in maternal postpartum mental health—we observe a marginally significant 0.3 percentage point (26 percent) reduction in the likelihood of any anti-anxiety prescription drug in the first six months post-childbirth. When examining the timing of these effects, we find that the reduction in anti-anxiety drugs is particularly strong (and statistically significant) in the first three months after childbirth. The effects on maternal physical and mental health are larger in both absolute and relative terms for mothers with pre-birth medical histories.[8]

The large maternal health effect magnitudes are consistent with the theoretical prediction that fathers take leave on days when the marginal benefit of doing so is especially high. To

[7]Such differences may stem from a variety of factors, including seasonality in births, differences in holiday time off work, and differential sorting because of school starting-age laws (see, e.g., Buckles and Hungerman, 2008; Currie and Schwandt, 2013; Black et al., 2011).

[8]We define mothers with a pre-birth medical history as those who have either any inpatient visit in months 1-24 before childbirth or any specialist outpatient visit for mental health reasons in months 1-60 before childbirth or any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth. See Section 4 for more details.

provide further support for this conjecture, we show that among families in which mothers have pre-birth medical histories, the "Double Days" reform increases the likelihood that the father takes at least one day of leave on the same day as when the mother has an encounter with the health care system. This result suggests that the option to take simultaneous leave allows fathers to stay home and care for their infants while mothers get medical care. The fact that we also find an overall reduction in maternal health care encounters with hospitals and specialist providers (as well as in prescription drug use) additionally suggests that fathers' flexibility to be able to stay home averts health complications that necessitate medical intervention in the first place.[9]

In sum, the central insight that emerges from our analysis is that mothers bear the majority of the cost of a lack of workplace flexibility—not only directly through greater career costs of family formation (as documented in prior literature)—but also *indirectly*, as fathers' inability to respond to domestic shocks exacerbates the maternal health costs of childbearing.[10]

Because the workplace flexibility initiative that we study is a reform of the Swedish parental leave system, our results relate to the large literature on parental leave (for some overviews, see: Olivetti and Petrongolo, 2017; Rossin-Slater, 2018; Rossin-Slater and Uniat, 2019). In the pre-reform period, Sweden constrained fathers' ability to take leave at the same time as the mothers. Similar inflexibility is built into parental leave systems in numerous other countries because policymakers view paternity leave as a way of promoting father-child bonding, changing gender norms, and improving maternal labor market outcomes. These goals are perceived to be more attainable if fathers are encouraged to stay at home *alone* with the child and for a *consolidated* time period.[11] While the evidence on the potential (bonding

[9]We do not have any data on primary care visits. It is possible that allowing fathers the option to take leave at the same time as mothers allows mothers to seek prompt primary care and thus avoid more serious health complications that require specialist or inpatient treatment.

[10]Work-family conflict is a major source of stress (Shockley et al., 2017) that is associated with adverse physical and mental health outcomes (Frone, 2000; Allen and Armstrong, 2006; Backé et al., 2012; Berkman et al., 2015; O'Donnell et al., 2019). While there is some evidence that public and organizational policies that promote workplace flexibility can mitigate this relationship (Dionne and Dostie, 2007; Kelly et al., 2011; Moen et al., 2013; Ziebarth and Karlsson, 2014; Bloom et al., 2014; Moen et al., 2016; Pichler and Ziebarth, 2017; Stearns and White, 2018), most studies use relatively small samples of workers in specific firms or industries, and focus on interventions that increase workers' autonomy in navigating their typical day-to-day workloads (e.g., shortened work hours, work-from-home options, and sick leave days). Further, little is known about the potentially distinct impacts of workplace flexibility during *critical* periods in workers' lives, such as shortly after the birth of a child.

[11]Indeed, nearly all existing studies of paternity leave focus on the consequences of so-called "Daddy Month"

or labor market) benefits of such inflexibility is mixed,[12] our study demonstrates that doing the opposite—letting fathers take leave *intermittently* and *jointly* with the mother—could be critical to maternal postpartum recovery.[13] Moreover, our results suggest that moral hazard concerns about workers taking leave to shirk from their jobs—which are prevalent in discussions of other workplace flexibility initiatives such as sick leave (e.g., see Pichler and Ziebarth, 2017)—are not supported by the data: Post-reform, fathers take just a few additional days of leave alongside the mother out of the full 30 days that they are allowed. Our finding of a substantial maternal health benefit stemming from this small increase in the average number of leave days taken by fathers further implies that workplace flexibility policies can be highly cost-effective—they leverage families' private information about when it is most desirable to stay home relative to the cost of missed time at work, and households' equilibrium choices ensure that they reap large benefits relative to the number of days used.

More broadly, our results relate to our understanding of how policy influences maternal postpartum health. While discussions about maternal health often center around the role of the medical system,[14] less attention has been paid to the mother's postpartum environment *at home*, where women spend the majority of their time in the months following childbirth.[15] This

reforms, which are *inflexible* by construction, in that they generate a lumpy leave-taking pattern, where fathers take leave *after mothers return to work*. See, e.g., Duvander and Johansson, 2012; Ekberg et al., 2013; Duvander and Johansson, 2014, 2015; Avdic and Karimi, 2018; Rege and Solli, 2013; Dahl et al., 2014; Cools et al., 2015; Dahl et al., 2016; Eydal and Gislason, 2008; Schober, 2014; Bünning, 2015; Patnaik, 2016; Luna and Farré, 2017.

[12] While correlational studies suggest that Swedish fathers who take longer leaves share household tasks and childcare more equally than those who take shorter leaves (Almqvist and Duvander, 2014), studies that exploit quasi-experimental variation from the reforms find less consistent results with regard to parental childcare duties or labor market trajectories (Ekberg et al., 2013; Duvander and Johansson, 2015).

[13] We are aware of one prior study from Great Britain, which uses multivariate regressions to show that self-reported health outcomes of postpartum women whose partners took two weeks of paternity leave are better than those of postpartum women whose partners took no leave, controlling for selected observable characteristics (Redshaw and Henderson, 2013). Related, a correlational study using Swedish data finds that infants of fathers who do not take any paternity leave are less likely to be breastfed than infants of fathers who do (Flacking et al., 2010). Månsdotter et al. (2007) also use Swedish data and show that among all fathers of first-born children born in 1978, those who took paternity leave were less likely to have died by 2001 than those who did not. We address endogeneity concerns associated with unobservable differences between these families with fathers who do and do not use paternity leave by exploiting the "Double Days" reform in an RD-DD design.

[14] For example, the "Lost Mothers" special series by the National Public Radio (NPR) largely focuses on the role of the medical system in contributing to rising maternal mortality in the United States. See: `https://www.npr.org/series/543928389/lost-mothers`.

[15] Consistent with the idea that the home environment could be important for maternal health, a growing literature shows that *maternity* leave benefits are associated with improvements in mothers' health outcomes (Hyde et al., 1995; Staehelin et al., 2007; Baker and Milligan, 2008; Chatterji and Markowitz, 2012; Aitken et

paper emphasizes the importance of a particular aspect of a new mother's home environment: the presence of the father.

2 Institutional Setting

Sweden implemented its gender-neutral paid parental leave policy in 1974, replacing the previous maternity leave system that only covered mothers.[16] The program is largely funded through employer social security contributions. Since the early 2000s, the program has featured a per-child benefit of 13 months of wage-replaced leave, as well as an additional 3 months of leave with a flat-rate benefit.[17] Parental leave benefits do not need to all be used in one spell; they can be claimed at any point until the child turns 8 or, more recently, 12 years old.[18] Moreover, the benefits can be claimed on a part-time basis.[19]

Parental leave is job protected in Sweden, with different rules applying during the first 18 months post-childbirth and beyond. During the first period, parents are entitled to full-time leave with job protection. Then, until the child turns 8 (or 12) years old, parents are legally able to reduce their working hours by as much as 25 percent while still working at the same job.[20]

Additionally, although leave in the original system was completely transferrable between parents, the vast majority of the leave days was taken by mothers.[21] In an effort to promote

al., 2015; Avendano et al., 2015; Beuchert et al., 2016; Butikofer et al., 2017; Hewitt et al., 2017; Heymann et al., 2017; Jou et al., 2018; Guertzgen and Hank, 2018; Bullinger, 2019).

[16] Sweden's parental leave program is not tied to marital status. Thus, it confers benefits to the (biological or adoptive) parents of a child regardless of whether they are married or not. In practice, a substantial share of parents are unmarried but cohabiting at childbirth (Persson, Forthcoming), and, as we discuss further below, we control for marital status in our empirical models.

[17] During the time period covered in our analysis, the replacement rate was approximately 78 percent of prior gross earnings, up to a ceiling. The flat-rate benefit has increased over time: from 180 SEK per day in the mid-2000s to 250 SEK (approximately $27) per day in 2016. To be eligible for the wage-replaced benefits, individuals must have had at least 240 days of employment paid at or above the flat-rate (e.g., 250 SEK per day in 2016) before the expected date of childbirth. Individuals who do not meet this employment requirement receive the lower flat-rate benefit only (Duvander et al., 2017).

[18] Specifically, for children born before January 1, 2014, parental leave benefits can be claimed until the child turns 8 or finishes the first year of school; for children born thereafter, benefits can be claimed until the child turns 12 years old.

[19] In particular, a parent can file for 100% leave (corresponding to 8 hours), 87.5% leave (corresponding to 7 hours), and so on, down to the smallest claim amount of 12.5% leave (1 hour).

[20] An employer is not allowed to deny this request as long as the parent notifies the employer of the intent to take parental leave at least two months in advance.

[21] Duvander and Johansson (2012) report that men used 0.5 percent of all parental leave days at the time

a more gender-equitable division of parental leave, the Swedish government has implemented three reforms (in 1995, 2002, and 2016) that each earmarked one month of wage-replaced leave to each parent. In other words, if a parent does not use his/her earmarked leave, the family loses that amount of leave. Since virtually all mothers take more than three months of leave throughout this time period, these reforms are in actuality only binding for fathers, and therefore colloquially referred to as the "Daddy Month" reforms.

Restrictions on simultaneous leave use. While both parents have access to paid leave in Sweden, there are important restrictions on the *simultaneous* use of leave. Specifically, until 2012, fathers were only entitled to ten "baseline days" of wage-replaced leave that could be used while mothers claim full-time leave, and they could only use them during the first 60 days after childbirth.[22] Beyond these ten days, parents could only be on leave simultaneously part-time while also working part-time, as long as the total amount of leave claimed by the two parents did not exceed the equivalent of a full-time job. In practice, however, since nearly all mothers were taking full-time leave in the months following childbirth, a father could only claim paid leave if the mother did not claim her benefit on that day (i.e., she took unpaid leave for the day).

Appendix Figure A1 presents a stylized representation of how the median Swedish family allocated leave between parents, using data on parents of firstborn singleton children born in 2008-2011. The figure shows that other than a maximum of ten baseline leave days that could be taken by fathers shortly after childbirth, the median mother was at home alone on full-time leave for about 14 months. After she returned to work, the median father took two months of leave. Children then typically entered public daycare, and the parents could use any remaining days of leave on a sporadic basis until the child's 8th birthday. As children's summer school breaks are usually longer than parental vacation time off, in practice these days are often used to cover the childcare gap during the summer.

This figure highlights that most policy efforts surrounding encouraging fathers to take leave are focused on *sequential* (rather than simultaneous) and *lumpy* (rather than intermittent)

of the program's inception in 1974, and this number rose only slightly over the next two decades.

[22]These ten days of baseline paternity leave do not count toward the total amount of wage-replaced parental leave that the parents divide between them.

leave. Indeed, as evidenced by the picture, the median Swedish father was taking the full two "Daddy Months" that were available during the 2008-2011 time period, but he was doing so in one stretch after the mother returned to work. Yet while policies that incentivize fathers to stay home on their own for a consolidated stretch of time may be important for father-child bonding and promoting paternal participation in household work (despite mixed evidence on these outcomes), they also preclude the father from having flexibility to be home during the vulnerable postpartum period.

"Double Days" reform. On January 1, 2012, Sweden implemented a "Double Days" reform, which changed the parental leave system such that parents were now allowed to take full-time wage-replaced leave *at the same time* for up to 30 additional days (beyond the baseline days) during the child's first year of life. Importantly, all other policy details—including total leave duration, the wage replacement rate, and the amount of earmarked leave—remained unchanged. Thus, the reform essentially provided families with more flexibility in choosing how to allocate the timing of their leave; fathers could now take full-time paid leave during the postpartum period while the mothers were also at home on paid leave.

Figure 1 provides the first indication that the "Double Days" reform affected the timing of paternity leave take-up. We plot the cumulative distribution of the initiation of post-baseline parental leave among fathers by month following childbirth, separately for fathers of firstborn singleton children born in 2011 and 2012. The figure demonstrates that the 2012 distribution is everywhere to the left of the 2011 distribution, meaning that fathers of children born in 2012 initiate their post-baseline leave earlier than fathers of children born in 2011. We provide more analysis of the effects of the "Double Days" reform on leave use and maternal health using an RD-DD design, as described in Section 5 below.

3 A Model of Household Parental Leave Use

To motivate our empirical analysis, we begin with a theoretical framework about household parental leave use. Specifically, consider a household consisting of a mother, father, and child. The household is eligible for a total of T days of wage-replaced parental leave pertaining to child c, with E days earmarked for each parent. A division of this parental leave can be described by the vector $D = \{d_{1p}, d_{2p}, ..., d_{Tp}\}$, where $p \in \{mom, dad\}$ indicates whether the day is claimed by the mother or father, respectively.

We denote by T_p the total number of days taken by parent p. Thus, any division of parental leave days taken by the mother and father (T_{mom}, T_{dad}) that satisfies the following conditions is permissible:

$$T_{mom} + T_{dad} \leq T$$

$$T_{mom} \leq T - E$$

$$T_{dad} \leq T - E.$$

We assume that household decisions are efficient. Thus, households choose the allocation of parental leave across the two parents that maximizes total household utility.

The general problem of choosing an allocation among the large set of permissible ones is complex and dynamic: The household's optimal leave division depends both on how the relative benefit of the mother versus father staying at home varies over time, as well as on the evolution of the parents' respective costs of being absent from the workforce. Further, the set of permissible parental leave allocations is very large; intuitively, even in the presence of a requirement that leave must be taken sequentially (as in the pre-reform period), parents are in principle permitted to alternate every second day between working at home and in the labor force.

In practice, however, as illustrated in Appendix Figure A1, households in the pre-reform period typically allocated a large number of days to the mother directly after birth (423 days, or approximately 14 months, in the median household), followed by a smaller number of days to the father (60 days in the median household). Children then typically entered public daycare, and the parents used the remaining days of parental leave on a sporadic basis, over several years, in short spells that essentially served to cover childcare gaps during school holidays. This pattern of leave use has three implications. First, it suggests that the net household benefit of the mother staying at home (the benefit less her foregone income) is generally larger than the corresponding net benefit of the father staying at home.[23] This difference could be

[23]This conjecture is also consistent with the fact that fathers took almost no parental leave before the "Daddy Month" reforms, as discussed in Section 2 above.

due to the benefit of the mother staying home being larger or her foregone income being lower; the latter is consistent with mothers earning less than fathers on average. Second, the fact that parents typically switch at some point—with the mother returning to the labor force and the father staying at home—suggests that the difference between the net benefit of maternal and paternal presence in the household *decreases* over time. This trend could arise for a number of reasons, including, for example, the benefit of breastfeeding declining over time, the maternal health cost of returning to work falling over time, and/or the benefit of the father bonding with the child increasing over time.[24] Third, the fact that both parents typically return to work and send the child to daycare before they have used all T days of parental leave (i.e., allocating some days for sporadic use in connection with holidays and vacations) suggests that, for both parents, the benefit of staying at home for child-rearing purposes eventually falls below the cost of leave-taking.[25]

Given this typical pattern of parental leave take-up, we distinguish between *core* and *miscellaneous* parental leave days in our model. Core days are the longer parental leave spells taken in the first two stretches, by the mother and father, respectively. Miscellaneous days are the ones left for residual use. As we shall see, the fact that households typically allocate a portion of their T parental leave benefit days for miscellaneous use is helpful, as it essentially converts the dynamic parental leave allocation problem into a static (and hence less complex) one.

We let the net benefit of a miscellaneous day be given by $m = l - (1-\alpha)w_{mom}$, where l represents the household's benefit from leisure on a miscellaneous day of leave, α represents the parental leave wage replacement rate, and w_{mom} is the mother's daily wage. By setting the cost of a miscellaneous day to be the foregone (non-replaced) daily wage of the mother, $(1-\alpha)w_{mom}$, we assume that a miscellaneous day would eventually be used by the mother. This assumption is consistent with what typically happens when parents need to cover childcare during school breaks, as mothers are the default childcare providers in most households.

Lemma 1: The net benefit of a miscellaneous day, m, is weakly lower than the net benefit

[24] In the presence of "Daddy Months," the fact that the father eventually takes leave suggests that households perceive the net benefit of the father's leave use to be positive (that is, the net household benefit more than outweighs the lost income).

[25] In Sweden, children become eligible for public daycare at age 12 months, so the relative value of parents staying at home falls for most families around that time.

of a core day.

Intuitively, this follows from the observation that households cease take-up of core days, and leave the remainder for miscellaneous use, once the net benefit of staying home for child-rearing purposes is sufficiently low.

Next, we introduce the right to take *double days* by allowing the father to claim parental leave during at most 30 of the mother's core days. Let the net household benefit of the father staying at home alongside the mother on day d be given by $b_d = q + h_d - (1-\alpha)w_{dad}$.[26] Here, q is the benefit to the household of having the father stay at home that is not directly related to maternal health (e.g., the father's utility from bonding with the baby, leisure, as well as any leisure complementarities between the two parents). h_d represents the improvement in maternal health resulting from the father staying at home on day d. This term is time-varying, which captures the fact that additional support for the mother may be more valuable to the household on some days (e.g., when she is not feeling well, is fatigued, or is having mental health issues) than others. The last term, $(1-\alpha)w_{dad}$, captures the foregone daily wage income of the father.

The household decides, on a day-to-day basis, whether the father should work in the labor market or stay at home alongside the mother.

By Lemma 1, if a double day is taken, then it will be taken out of the household's stock of miscellaneous days, rather than out of the core days, so long as a miscellaneous day is available.

Assumption 1: The number of double days used does not exceed the household "budget" of miscellaneous days.

Under Assumption 1, the opportunity cost of using a double day is always equal to m, the net benefit of a miscellaneous day. This thus represents a lower bound on the threshold that b_d must exceed in order for the household to choose to have the father stay at home alongside

[26] Before the "Double Days" reform, fathers could stay at home together with the mother by either taking an unpaid day off or by having the mother not claim her paid leave benefit on that day. Under the assumption that the mother's wage is lower than the father's, it would have been optimal for the household to have the father claim the paid leave benefit, meaning that the net benefit of a double day in the pre-reform period would be equal to $b_d = q + h_d - w_{mom} - (1-\alpha)w_{dad}$. Thus, the "Double Days" reform effectively lowers the price of having the father stay home from work alongside the mother. It is also possible that, by creating an explicit national policy about parental simultaneous leave use, the reform lowers any stigma or career concerns costs associated with fathers taking time off work.

the mother.

Lemma 2: Under Assumption 1, the household follows a simple threshold rule: The father claims a double day if and only if:

$$b_d > m$$

$$q + h_d - (1-\alpha)w_{dad} > l - (1-\alpha)w_{mom}$$

$$h_d > (l-q) + (1-\alpha)(w_{dad} - w_{mom})$$

That is, households choose to have the father stay at home on days when household utility from father presence is especially high; in particular, when the measurable maternal health benefit exceeds the sum of the leisure differential and ($(1-\alpha)$ times) the wage differential between the mother and father.[27]

Given that a household is allowed a maximum of 30 double days, if less than 30 days satisfy the above inequality, then the household takes all the double days that it desires to. If, in contrast, more than 30 days satisfy the above inequality, then the household chooses the 30 days with the highest h_d.

In what follows, we empirically examine the effects of the "Double Days" reform on both paternity leave use and maternal postpartum health. Our simple model highlights that maternal health benefits could materialize even if there is only a small quantitative impact of the reform on the total number of leave days taken by fathers. Indeed, the key point of the model is that, when parents have the flexibility to decide when to take joint leave on a day-to-day basis, the *timing* of the take-up of a joint day of parental leave is informative about the household's perceived net benefit of a double day: Because a day of paternity leave joint with the mother has several opportunity costs, the household optimally responds to the need for maternal support, by removing the father from the labor force on precisely the days when the household benefit of doing so is perceived to be the highest.[28]

[27] For simplicity, we abstract away from discounting.

[28] In this simple setting, providing fathers with the option to spend more time at home would always (weakly) positively affect household utility, as this represents an expansion of the household's choice set. Providing fathers with this option could instead negatively affect household utility if the household has incorrect beliefs about the benefits and costs of joint leave-taking, e.g., if simultaneous leave induces an unanticipated deterioration of the relationship.

4 Data

Our analysis uses multiple Swedish administrative data sets: birth records data from the National Board of Health and Welfare (NBHW; in Swedish *Socialstyrelsen*), population register data from Statistics Sweden containing demographic and labor market information on the parents, data on parental leave claims from the Swedish Social Insurance Agency (*Forsakringskassan*), as well as inpatient, outpatient, and prescription drug claims data from NBHW to measure maternal health outcomes.

Births data. We have data on all Swedish births from 2000 to 2016, with unique parental and child identifiers, and with detailed information on pregnancy and delivery characteristics and birth outcomes, including child gender, birth order, birth type (singleton versus multiple birth), gestational age in days, expected due date, birth weight in grams, the Apgar score, an indicator for small-for-gestational-age (SGA), and indicators for cesarean section (c-section) deliveries, inductions of labor, and various pregnancy risk factors and labor/delivery complications. We use these data to identify firstborn singleton live births during our analysis time frame, and to calculate the children's exact dates of birth using information on gestational age and expected due date.[29]

Demographic information and parental leave claims. We use administrative data from Statistics Sweden to obtain information about each mother's and father's age, educational attainment, marital status, and income in the year before the first child's birth. To measure take-up of parental leave, we add spell-level data from the Swedish Social Insurance Agency. For each child, we observe the universe of parental leave spells taken from 1993 until 2016. For each spell, the data contain the exact start and end dates, as well as information about the type of compensation (wage-replaced or flat-rate day), as described in Section 2 above. We merge the two data sets to the birth records data using parental identifiers.

Our main measures of parental leave are indicators for any post-baseline leave taken by fathers during various time periods in the year following childbirth. We also calculate the

[29]Specifically, we subtract 280 days (40 weeks) from the expected due date to obtain the conception date, and then add the gestational age in days to obtain the actual date of birth.

total number of leave days taken by fathers (including baseline leave) during these periods.[30]

Maternal health outcomes. We merge information from inpatient care, specialist outpatient care, and prescription drug records using maternal identifiers. We have access to inpatient records from 1995 to 2016, specialist outpatient records for 2001 to 2016, and prescription drug records for 2005 to 2017. The inpatient records contain information on the universe of a patient's visits to the hospital that result in hospital admission, including cases where the individual is admitted and discharged on the same day. The outpatient data records all visits *excluding* primary care. In Sweden, primary care (e.g., regular postpartum check-ups and annual physical exams) is provided at municipal "care centers" (*Vårdcentraler*), which are mostly staffed with nurses. "Care centers" can provide referrals to more specialized outpatient care, which is what we observe in the outpatient records. The drug records contain the universe of an individual's prescription drug purchases made in pharmacies, but do not include drugs administered in hospitals.

For each visit to an inpatient or specialized outpatient provider, the data contain information on the date of the visit, the associated International Classification of Diseases (ICD-10) diagnosis codes, and the length of stay (for inpatient data only). For each occasion when a prescription drug was bought, the prescription data contain information about the drug name, active substance, average daily dose, and the drug's exact Anatomical Therapeutic Chemical (ATC) code.[31] The ATC classification allows us to link the drugs to the conditions they are most commonly used to treat.

Our main analysis focuses on maternal health outcomes measured in the first 180 days (6 months) following childbirth, but we also explore other time periods, as discussed in Section 6 below. Using the inpatient and outpatient data, we define indicators for any inpatient or outpatient visit following the child's birth (excluding the birth itself), as well as indicators for any visits associated with the following three distinct diagnosis groups: (i) conditions related to pregnancy, childbirth, or the puerperium period, (ii) diagnoses for mental, behavioral, and

[30] For both measures, we count any day with any leave benefit claimed, regardless of whether it is wage-replaced or a flat rate, and regardless of whether it is full-time or part-time, as a day of leave.

[31] The ATC classification system is controlled by the World Health Organization Collaborating Centre for Drug Statistics Methodology (WHOCC), and was first published in 1976.

neurodevelopmental disorders, and (iii) external causes and medical counseling.[32]

In the prescription drug data, we create indicators for any drug claims in the following four categories: anti-anxiety, anti-depressant, antibiotic, and painkiller. Appendix B lists the exact ICD and ATC codes for all of our outcomes.[33]

Finally, to examine a particularly vulnerable sub-group of mothers, we use information from the inpatient, outpatient, and prescription drug records to measure pre-birth medical histories. We classify mothers as having a medical history if they satisfy any of the following conditions: (i) any inpatient visit in months 1-24 before childbirth, (ii) any specialist outpatient visit for mental health reasons in months 1-60 before childbirth, or (iii) any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth.[34]

Analysis sample and summary statistics. To analyze the effects of the 2012 "Double Days" reform, we first limit our data to the 233,981 firstborn singleton children born in 2008-2012. In order to implement an RD design that uses the running variable expressed in days, we further limit our analysis to the 222,638 observations for which we can calculate exact dates of birth.[35] Additionally, in most of our specifications, we use a three-month bandwidth, and therefore constrain our sample to only include children born in October through December of 2008, 2009, 2010, and 2011 and January through March of 2009, 2010, 2011, and 2012 (hereafter referred to as the RD-DD sample).

Table 1 reports sample means of selected parental background characteristics and maternal

[32] We refer to visits that are coded as "factors influencing health status and contact with health services" as medical counseling. These codes, which all start with the letter Z in the ICD-10 system, are used for occasions when there are circumstances other than a disease, injury, or other diagnosed external cause that lead to a health encounter. Most relevant to our study, these codes can be used to classify visits in which a new mother receives medical counseling or advice, but is not diagnosed with any particular condition (e.g., she may receive advice regarding postpartum "baby blues," but is not formally diagnosed with depression).

[33] We also explored effects on the total number of inpatient and outpatient visits, as well as the total number of prescription drug claims. We found that our estimated effects are driven entirely by extensive margin responses (results for total visits and drug claims are available upon request).

[34] We choose these time frames such that we capture women with a medical history in a time period sufficiently close to childbirth, and that we retain enough sample size to have sufficient statistical power. We choose to focus on outpatient visits and prescription drugs related to mental health since most women have at least some kind of (non-mental-health-related) specialist outpatient visit or prescription drug in the months before childbirth. Our results are not sensitive to small alterations to the time windows used to measure medical histories.

[35] We are unable to calculate exact dates of birth for the approximately 5 percent of observations that are missing data on the expected due date. However, all observations have information on the month and year of birth. We have estimated all of our models including the observations with missing data and expressing the running variable in months instead of days, obtaining very similar results, which are available upon request.

health outcomes measured in the first six months after childbirth. Column (1) includes all firstborn singleton children born in 2008-2012. Column (2) limits the sample to children with information on exact date of birth. Column (3) uses our primary RD-DD sample, while column (4) further limits the RD-DD sample to families with mothers who have a pre-birth medical history. About 45 percent of mothers and 57 percent of fathers have a low education level (defined as high school or less), respectively, and the average mother (father) is 29 (32) years old in the year before birth. Maternal and paternal average annual employment income in the year before birth is 208,000SEK ($29,060) and 276,000SEK ($38,498) in 2010, respectively. About 21 (22) percent of the mothers (fathers) in our data are born outside of Sweden. There are no large differences in these characteristics across the first three columns, while families in which mothers have a pre-birth medical history (column 4) have lower average education levels and incomes.

The table further shows that about 5 percent of new mothers have at least one inpatient visit in the first six months postpartum, while 33 percent have at least one specialist outpatient visit during the same time frame. Ten percent of mothers have an inpatient or outpatient visit for childbirth-related complications, 2 percent have a visit for mental health reasons, while 1 percent have a visit for external causes or medical counseling. About 4 percent of new mothers have an anti-anxiety or anti-depressant drug prescription, while 8 and 16 percent have painkiller and antibiotic prescriptions, respectively, during the first six months after giving birth. Not surprisingly, the means of the maternal health outcomes are higher among mothers with pre-birth medical histories in column (4).

5 Empirical Methods

Our goal is to examine the causal link between fathers' access to workplace flexibility and maternal postpartum health. We study this question by exploiting the natural experiment stemming from the 'Double Days" reform on January 1, 2012. Our analysis essentially compares individuals whose children are born very close to, but on opposite sides of, the reform date, and we difference out seasonality effects using parents of children born in the same months but in other non-reform years. Specifically, our primary specification compares the outcomes of mothers and fathers of firstborn singleton children born in January-March 2012 and October-December 2011 ("reform sample"), relative to the difference in outcomes in the same months in the previous three years (January-March 2011, 2010, and 2009 versus October-December 2010, 2009, and 2008; "non-reform sample"). Our regression model, which uses the child's day of birth, d, as the running variable, can be expressed as follows:

$$\begin{aligned} y_{idp} &= \alpha + \beta_1 \mathbf{1}[d \geq c] + \beta_2 R_i \times \mathbf{1}[d \geq c] \\ &+ f(d-c) + \mathbf{1}[d \geq c] \times f(d-c) + \mathbf{x'}_i \kappa + \theta_p + \varepsilon_{idp} \end{aligned} \tag{1}$$

for each family of first-born singleton child i born on day of the year d in time period p, where we refer to each October through March as a separate period (e.g., October 2008 - March 2009, October 2009 - March 2010, etc.) y_{idp} is an outcome of interest, such as an indicator for any post-baseline leave use in the two months after childbirth or an indicator for a maternal inpatient or outpatient visits in the six months following childbirth. c denotes January 1, the day of the reform. R_i is an indicator set to 1 for children who are in the reform sample (i.e., October 2011 - March 2012 births), and 0 otherwise. The dummy variable $\mathbf{1}[d \geq c]$ is set to 1 for children born in January-March in any year. $f(d-c)$ is a flexible function of the running variable, day of birth centered around January 1, for which we use a quadratic polynomial in our main specifications and allow for it to have a different shape on opposite sides of the threshold in all periods. We also include fixed effects for every time period, θ_p.[36]

The vector $\mathbf{x}_i$ includes a dummy for child gender, as well as the following family control variables, measured in the year before birth: maternal and paternal earnings (in 1000s of real SEK in year 2010 terms), indicators for each parent's age groups (<20, 20-24, 25-34, 35+), indicators for each parent's education levels (high school or less, some college, university degree or more), an indicator for the parents being married, and indicators for each parent being foreign-born. ε_{idp} is an unobserved error term. The key coefficient of interest is on the interaction between the reform sample dummy, R_i and the dummy for January-March births, $\mathbf{1}[d \geq c]$, and is denoted by β_2. It represents an estimate of the difference in parental outcomes between January-March and October-December births in the reform sample, relative to the

[36]Note that the main effect of R_i, the dummy for being in the reform sample, is absorbed with the inclusion of period fixed effects.

analogous difference in outcomes in the non-reform sample.

Identifying assumption. The standard RD design relies on the assumption that only the treatment variable—in our case, eligibility for the "Double Days" reform at the time of childbirth—is changing discontinuously at the reform date; all other variables possibly related to our outcomes of interest should be continuous functions of the assignment variable (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In our application, this assumption implies that parents should not be able to strategically manipulate the timing of childbirth and that there are no other discontinuous policy changes at the same time as the reform.

As documented in multiple prior studies, there are important differences in the number and composition of births across months of the year due to non-random fertility patterns and environmental or health factors such as the timing of the influenza season (Buckles and Hungerman, 2008; Currie and Schwandt, 2013). Additionally, January 1 is the school starting age cut-off date in Sweden, implying that parents who wish to have their children be the oldest or youngest in the class may strategically sort on different sides of the cut-off. Further, and relevant to our study of leave use, there are differences in the number of holidays when parents can stay home from work across these months. To net out all the differences between January and December births unrelated to the "Double Days" reform, we use births in the same months in three years before the reform, as described above. Thus, for our setting, we rely on an assumption that any discontinuities in other variables at the reform date are not distinguishable from those in the non-reform years.

To assess the plausibility of the identifying assumption, we first perform the RD-DD version of the McCrary (2008) test. Specifically, we collapse our data into week-of-birth bins, and estimate a version of model (1) using the collapsed data with the number of firstborn singleton births as the dependent variable and a 26-week (6 month) bandwidth. The running variable is the week of birth normalized relative to the first week of January in every period, and we report coefficients from RD-DD models that use 1st through 6th order polynomials in the running variable. Table 2 presents the results, and we also report the Akaike Information Criterion (AIC) in the bottom row of each table. The results are very stable across the different specifications, and, importantly, we detect no significant discontinuities in the number of births at the time of the reform. Figure 2 presents analogous graphical evidence: sub-figure (a) plots the total number of births by birth week in the reform sample, while sub-figure (b) plots the average of the total number of births by birth week across all years in the non-reform sample. The fitted lines are predicted from 4th order polynomial models; we follow Lee and Lemieux (2010) by selecting the model with the smallest AIC value.

We next check for any discontinuities in pre-determined characteristics at the reform date. Appendix Tables A1 and A2 report results from estimating versions of model (1), omitting the controls in vector $\mathbf{x}_i$ and instead using parental characteristics, children's birth outcomes, and maternal pre-birth medical history indicators as the dependent variables. Out of the 20 coefficients reported across the two tables, only one is statistically significant at the 5% level. Moreover, in both tables, a joint F-test from seemingly unrelated regression models yields insignificant results. These results are reassuring and suggest that differential selection into birth at the reform date is unlikely to bias our main estimates reported below.

6 Results

Effects of the "Double Days" reform on paternity leave use. We begin by providing evidence that the "Double Days" reform affects paternity leave use in the months following childbirth. Figure 3 plots means of three paternity leave outcomes by the child's birth week for births in 2011-2012, along with the predictions and 95% confidence intervals from estimating local linear polynomial models on each side of the reform threshold. We show graphs for the following leave outcomes for fathers: (a) any post-baseline leave in the first 60 days post-childbirth, (b) any post-baseline leave in the first 180 days post-childbirth, and (c) the total number of leave days in the first 180 days, including both baseline and post-baseline leave. The figure shows increases in leave use in the first two and six months after childbirth, and a more muted impact on the total number of days of leave. The fact that leave use appears to increase starting with births in the weeks preceding the reform (i.e., the last few weeks of 2011) is consistent with parents of children born shortly before the reform becoming eligible for "Double Days" on the reform date. Thus, for example, a father of a child born on December 1, 2011 can take post-baseline leave at the same time as the mother starting when his child turns one month old. To account for this treatment pattern, we assess the robustness of our results to dropping families of children born in the last few weeks of the year, and to estimating models that use as the treatment variable the share of days that a family is eligible for "Double Days" during different windows in the child's first year of life. We discuss these results further below.

Table 3 presents results from estimating equation (1) using the three paternity leave variables as outcomes, separately for the whole sample and for the sub-sample of families with mothers who have a pre-birth medical history. In the overall sample, columns (1) and (2) show 3.9 and 5.9 percentage point increases in the likelihoods of any post-baseline leave use among fathers in the first two and six months postpartum, respectively. The magnitudes correspond to 50 and 24 percent increases relative to the sample means. We observe bigger impacts in absolute terms among fathers in families with mothers who have a medical history, although in relative terms the magnitudes are comparable to those in the overall sample. Additionally, while the effects on *any* post-baseline leave use are fairly large, we only observe a one to two day average increase in the total number of days of leave in the first six months post-childbirth. These estimates suggest that the "Double Days" reform primarily impacts fathers' leave use shortly after childbirth on the extensive, rather than intensive, margin.

Importantly, our theoretical framework in Section 3 highlights that households may reap gains from a reform that grants flexibility in the use of joint parental leave, even if fathers, *ex post*, end up taking only a few extra days of leave. The availability of joint leave allows families to keep the father in the household on precisely the days when his presence is particularly valuable for the family. Next, we examine how fathers' ability to use post-baseline leave affects maternal postpartum health.

Effects of the "Double Days" reform on maternal health. Figures 4 and 5 present graphical evidence for our main maternal health outcomes in the inpatient/outpatient and prescription drug data, respectively. As with the paternity leave variables, we plot week-of-birth means overlayed with predictions and 95% confidence intervals from local linear polynomial models estimated separately on each side of the reform date. Sub-figures (a) and (b) of Figure 4 suggest that there is a decline in the likelihood of a maternal inpatient or specialist outpatient visit in the first six months after childbirth, driven by a reduction in visits for childbirth-related complications.[37] There appears to be no change in visits with mental health-related diagnoses or those associated with external causes and medical counseling (sub-figures (c) and (d)). When it comes to the outcomes measured with prescription drug data, the graphs suggest declines in the likelihoods of any anti-anxiety and antibiotic drug use (sub-figures (a) and (d) of Figure 5), and no apparent change in anti-depressant or painkiller medications (sub-figures (b) and (c)).

Tables 4 and 5, which present estimates from model (1) using inpatient/outpatient and prescription drug data, respectively, confirm the graphical evidence. In the overall sample, we observe a 1.5 percentage point (14 percent) decrease in the likelihood of a mother having an inpatient or outpatient visit for childbirth-related complications (Table 4, Panel A). We also find a 1.9 percentage point (11 percent) decline in the likelihood of any antibiotic prescription in the first six months after childbirth (Table 5, Panel A). When it comes to mental health, we observe a marginally significant reduction in the likelihood of any anti-anxiety prescription drug during this time period of 0.3 percentage points (26 percent). These effects are larger in both absolute and relative terms for mothers with pre-birth medical histories (see Panel B in each table). We do not find any significant impacts on inpatient or specialist outpatient visits for mental health reasons, external causes, or medical advice, or on the other prescription drugs that we consider.

Timing of effects. We next explore the timing of the effects on paternity leave use and maternal health. Figure 6 plots the RD-DD treatment coefficients scaled by the dependent variable means (i.e., such that the magnitudes can be interpreted as percent changes relative to sample means) and corresponding 95% confidence intervals from regression models that use outcomes measured in the periods since childbirth denoted on the x-axis of each graph. Sub-figure (a) demonstrates that most of the effect on the likelihood of any post-baseline leave use among fathers occurs in the first six months after childbirth, with a stronger relative impact in the first three months. Sub-figure (b) shows that the decline in maternal inpatient

[37] When examining inpatient and outpatient visits separately, we find that the reduction is more pronounced for outpatient visits (results available upon request). We aggregate the two types of visits into one indicator to increase our statistical power.

and outpatient visits for childbirth-related complications is most pronounced in months four through six postpartum, although the confidence intervals overlap across all of the time periods we consider. In sub-figure (c), we find that the reduction in anti-anxiety prescriptions is particularly large and statistically significant at the 5% level during the first three months post-childbirth, while there is no significant effect in the subsequent time periods. Sub-figure (d) shows that the reduction in antibiotic prescriptions is of similar magnitude for the first nine months postpartum. These results underscore the idea that the ability of the household to flexibly choose to keep the father at home alongside the mother, if need be, in the first few months post-childbirth, has large and nearly immediate impacts on multiple measures of maternal postpartum health.

Mechanisms. Our theoretical framework suggests that households choose to keep the father at home on days when the marginal benefit of doing so is particularly high. This is consistent with the fact that the magnitudes of our estimated effects on maternal health are large when compared to the modest increase in the total number of leave days that fathers use. The reduction in inpatient and specialist outpatient visits, as well as in prescription drugs, suggests that fathers' ability to take a day or two of paid leave when this is especially needed may avert maternal health complications that require medical intervention.[38]

We next examine whether, conditional on a mother needing medical care, the father takes leave on days when she has a health care encounter. Appendix Table A3 presents results from the RD-DD model, in which the outcome is an indicator for whether the father takes leave on a day that overlaps with when the mother has either an inpatient or outpatient visit or fills a drug prescription. For families with mothers who have a pre-birth medical history (Panel B), we find a 1.6 percentage point (26 percent) increase in the likelihood of this event occurring. This result points to the possibility that in families in which mothers are particularly vulnerable to postpartum health issues, the "Double Days" reform grants fathers the flexibility to take leave and stay home with their infants on days when mothers need medical care.

In addition, we analyze whether the effects of the "Double Days" reform differ across

[38]As noted in Section 4, we do not have data on primary care visits. Thus, it is possible that the "Double Days" reform allows fathers to take leave so that mothers seek prompt primary care and thereby avoid more serious complications that would have required specialist visits or hospitalizations.

families who do and do not have at least one grandparent aged 74 years or less residing in the same county.[39] Fathers' ability to take full-time leave in the postpartum period may be especially important for families who do not have another family member—such as the child's grandparent—who can step in to help when a mother experiences health issues. As such, we expect the impacts of the "Double Days" reform on maternal health to be stronger in families without a relatively young grandparent residing in close proximity. Appendix Tables A4, A5, and A6 report the results of this heterogeneity analysis for the paternity leave, maternal inpatient/outpatient, and maternal prescription drug outcomes, respectively. Interestingly, we find that the impacts on paternity leave use are similar for families with and without a grandparent in the same county, suggesting that the reform induced fathers in both groups to take post-baseline leave. However, the impacts on maternal physical health—as measured by inpatient and outpatient visits for childbirth-related complications and antibiotic prescriptions in the 6 months post-childbirth—appear larger for families without a grandparent in the same county. These results are consistent with the hypothesis that fathers' ability to take full-time paid leave in the postpartum period is particularly important when no other potential caregivers are available to help mothers recover and rest. We do not detect significant effects on anti-anxiety prescription drugs in the 6 months post-childbirth in either sub-sample, possibly due to power concerns from reducing the sample size.

Lastly, it is possible that in the pre-reform period, when fathers are restricted to only ten baseline days during which they can take full-time paid parental leave at the same time as mothers, fathers rely on other benefits to stay home if necessary. While Sweden does not provide any family leave benefits to care for adult family members (i.e., postpartum mothers), it is possible that fathers rely on own sick leave benefits for these purposes. Since sick leave data are only available at an annual level, we compare the annual number of sick leave days used by fathers of firstborn singleton children born in January-March 2011 and January-March 2012 in Appendix Table A7. We do not detect any statistically significant differences either in the average number of sick leave days or in the share of fathers with any sick leave across the two groups, suggesting that substitution from sick leave toward parental leave is not biasing our main estimates. Unfortunately, we do not have data on other benefits such as vacation

[39] The age restriction on grandparents is due to a data constraint as we only observe demographic information including county of residence for individuals aged 74 or less in our data.

days. However, in Sweden, vacation benefits are not very temporally flexible, as vacation time has to be scheduled with the employer in advance (moreover, employees are typically required to take at least a portion during the summer months). Thus, vacation benefits are far less flexible than sick leave benefits, which we do observe. Nonetheless, if anything, substitution from other time off to paid parental leave among fathers would imply that our effects of fathers' workplace flexibility on maternal health are attenuated.

Sensitivity analysis. While our main analysis uses an RD-DD design in order to account for seasonal differences in births, we also present results for our main outcomes from standard RD specifications. Specifically, we start with data on all firstborn singleton births in 2008-2015, and then estimate RD models with local linear polynomials that compare births before and after January 1, 2012 and use different optimal bandwidth algorithms to select the bandwidths of the number of days used on each side of the cutoff.[40] We include the same vector of controls $\mathbf{x}_i$ as in model (1).

The results for any paternal post-baseline leave use in the two months post-birth, any paternal post-baseline leave use in the six months post-birth, maternal physical health measured in the first six months post-childbirth (any inpatient or outpatient visit for childbirth-related complications and any antibiotic prescription drug), and any maternal anti-anxiety prescription drug in the first three months post-childbirth are presented in Appendix Tables A8, A9, A10, A11, and A12, respectively. Our estimates are mostly statistically significant and reasonably robust across the different bandwidths. The discontinuity in paternity leave use becomes small (and at times insignificant) with very narrow bandwidths, which is consistent with the fact that parents of children born shortly before the reform become eligible for "Double Days" at the time of the reform.

To account for the timing pattern of treatment, we calculate the share of days between

[40] The optimal bandwidth algorithms are: (1) one common mean squared error (MSE)-optimal bandwidth selector for both sides of the cutoff; (2) two different MSE-optimal bandwidth selectors (below and above the cutoff); (3) one common MSE-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (4) minimum of (1) and (3); (5) median of (1), (2), and (3) for each side of the cutoff separately; (6) one common coverage error rate (CER)-optimal bandwidth selector; (7) two different CER-optimal bandwidth selectors (below and above the cutoff); (8) one common CER-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (9) minimum of (6) and (8); (10) median of (6), (7), and (8) for each side of the cutoff separately. We follow Calonico et al. (2014), Calonico et al. (2018a), and Calonico et al. (2018b) in using these algorithms. We report the number of days used in the left and right-hand bandwidths in each model at the bottom of the table.

the child's first and 60th day of life that parents are eligible for the "Double Days." Thus, a child who is born on December 31, 2011 gets a value of $\frac{59}{60}$, while a child born on November 3, 2011 gets a value of $\frac{1}{60}$. We analogously calculate the share of days between the child's first and 180th day of life that the parents are eligible for the "Double Days". We then estimate a version of model (1) in which we replace the main interaction term, $R_i \times \mathbf{1}[d \geq c]$, with this variable. The rest of the variables included in the model are the same as before. We can interpret the coefficient on the new treatment variable as the effect of moving from 0 to 100 percent eligibility during the relevant time frame. The results for our main outcomes are reported in Panels A and B of Appendix Table A13. The magnitudes of the impacts on fathers' post-baseline leave use are larger than those from our main RD-DD specifications, consistent with the idea that we are now more accurately capturing eligibility for families in the weeks and months leading up to the reform. For instance, we now find that moving from 0 to 100 percent eligibility for "Double Days" during the child's first six months of life increases the likelihood of any paternal post-baseline leave use during that time period by 20.2 percentage points, or 83 percent. We also find that the total number of days of leave used by fathers increases by 6.4 days. When it comes to maternal health, we find that moving from 0 to 100 percent eligibility in the first six months post-birth reduces the likelihood of an inpatient or outpatient visit for childbirth complications by 3.9 percentage points (38 percent) and the likelihood of an antibiotic prescription by 5.2 percentage points (31 percent). We also document that moving from 0 to 100 percent eligibility in the first two months post-birth decreases the likelihood of a maternal anti-anxiety prescription in the first three months after birth by 0.3 percentage points (45 percent, marginally significant).

Lastly, as Figure 3 suggests that most of the increase in post-baseline leave use among fathers of children born pre-reform occurs in the few weeks immediately before it, we estimate our main RD-DD models, dropping all December births. The results are presented in Panel C of Appendix Table A13, and are similar to those from our main specifications. Taken together, our sensitivity tests suggest that the impacts of the "Double Days" reform on paternity leave use and maternal postpartum health are robust across various modeling choices.

7 Conclusion

When a woman gives birth to a child, much of the attention is typically placed on the health and well-being of the newborn baby. There are many medical and social policy interventions targeting infants, and a plethora of research has been dedicated to understanding the causes and consequences of early-life health (see, e.g., Currie, 2011; Almond and Currie, 2011; Chen et al., 2016; Almond et al., 2017; Persson and Rossin-Slater, 2018). New mothers, who undergo a significant physical and emotional transition after childbirth, are comparably under-discussed and under-studied.

A recent influential medical study in *The Lancet* journal has raised awareness about the state of maternal postpartum health by documenting that the United States has experienced a disturbing increasing trend in maternal mortality in the last several decades (Kassebaum et al., 2016). A lot of the resulting discussion has centered around the role of the health care system in delivering prenatal and postpartum care.[41] But the mother's environment at home can have significant influence on her well-being during the often emotional and overwhelming months of new parenthood. In fact, in recent commentary about the rise in maternal mortality in the U.S., Dr. Neel Shah, a leading maternal health expert at the Harvard Medical School, argues:

> "*What's important to understand is that most maternal deaths happen after women have the baby and the fundamental failure is not unsafe medical care but lack of adequate social support…a lot of the risks around childbirth happen after the baby is born during that vulnerable time when you're trying to care for an infant while also taking care of your household and doing all the things we expect of moms.*"[42]

Our paper attempts to isolate the effect of a key factor in the mother's postpartum home environment: the presence (or absence) of the child's father in the weeks and months immediately following childbirth. To study this question, we take advantage of linked Swedish

[41] For examples of these discussions in the press, see: https://www.vox.com/science-and-health/2017/6/26/15872734/what-no-one-tells-new-moms-about-what-happens-after-childbirth
https://www.npr.org/2017/05/12/528098789/u-s-has-the-worst-rate-of-maternal-deaths-in-the-developed-world
https://www.npr.org/2017/05/12/527806002/focus-on-infants-during-childbirth-leaves-u-s-moms-in-danger.

[42] See: `https://www.pbs.org/newshour/show/whats-behind-americas-rising-maternal-mortality-rate`.

administrative data and quasi-experimental variation from a social insurance reform in January 2012, which granted fathers the flexibility to take up to 30 days of paid leave on an intermittent basis alongside the mother. Using an RD-DD design, we document that this reform leads to 50 and 24 percent increases in the likelihoods of fathers using any post-baseline leave in the first two and six months after childbirth, respectively.

Then, we present consistent evidence that fathers' access to workplace flexibility improves maternal postpartum health. We find a 14 percent decrease in the likelihood of a mother having an inpatient or specialist outpatient visit for childbirth-related complications, and 11 and 26 percent reductions in the likelihoods of her getting any antibiotic and anti-anxiety prescription drugs, respectively, in the first six months post-birth. Moreover, we show that the decline in anti-anxiety medications is especially pronounced in the first three months after childbirth. The effects on maternal health are larger in both absolute and relative terms for mothers with a pre-birth medical history, who may be particularly vulnerable and thus benefit the most from a policy that grants fathers the flexibility so stay home from work in the postpartum period. These large effects are consistent with our simple theoretical framework, in which households use their private information to optimally choose to keep the father at home on precisely the days when his presence is especially valuable.

In addition to informing questions about determinants of maternal postpartum health, our findings have important implications for debates about workplace flexibility and paid family leave (PFL) policies. The United States remains the only high-income country without a national PFL policy, although six states and Washington, D.C., have either implemented or passed PFL legislation that provides partially paid parental leave to both mothers and fathers.[43] Just as in other countries that have had paid parental leave policies for decades, fathers in states with PFL programs take much less leave than mothers do.[44] While discussions about encouraging men to take paternity leave typically focus on policies that promote

[43]These are: California (in 2004), New Jersey (in 2009), Rhode Island (in 2014), New York (in 2018), D.C. (will go into effect in 2020), Washington state (will go into effect in 2020), and Massachusetts (will go into effect in 2021).

[44]Bartel et al. (2018) estimate that the introduction of California's 6-week PFL program only increased fathers' leave duration from about 1 to 1.5 weeks on average. Bana et al. (2018) document that only 12 percent of eligible new fathers in California made a PFL claim in 2014, ten years after the introduction of the program. In contrast, in the same year, 47 percent of eligible new mothers made a PFL claim. Moreover, while fathers in California are eligible for 6 weeks of paid leave, over three-quarters of those who take leave take less than the maximum amount.

sequential and consolidated leave use (such as “Daddy Month”-style programs), our findings imply that policies that restrict fathers’ flexibility in being able to take leave at the same time as mothers on an intermittent basis could have negative spillover effects on maternal health.

Our study further contributes to discussions about whether paid leave programs should incorporate family care (i.e., leave to care for an ill family member who is not a newborn child). While paid leave for new parents is nearly ubiquitous in most of the developed world (with the important exception of the United States), family care leave is much less common (e.g., Sweden does not have such a program).[45] Our results suggest that the availability of such leave—which fathers could use to care for mothers in the immediate postpartum period—could have important and previously uncalculated benefits for families.

An important limitation of our study is that we do not observe any information about parent-child interactions. Future research may explore how the “Double Days” reform affects not only paternal time spent in childcare (as in studies on “Daddy Month”-style policies, e.g., Ekberg et al., 2013; Patnaik, 2016), but also how it may impact mother-child relationships, which are likely mediated by maternal postpartum well-being.

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[45] See Addati et al. (2014) and Olivetti and Petrongolo (2017) for more information on family leave policy details in countries around the world.

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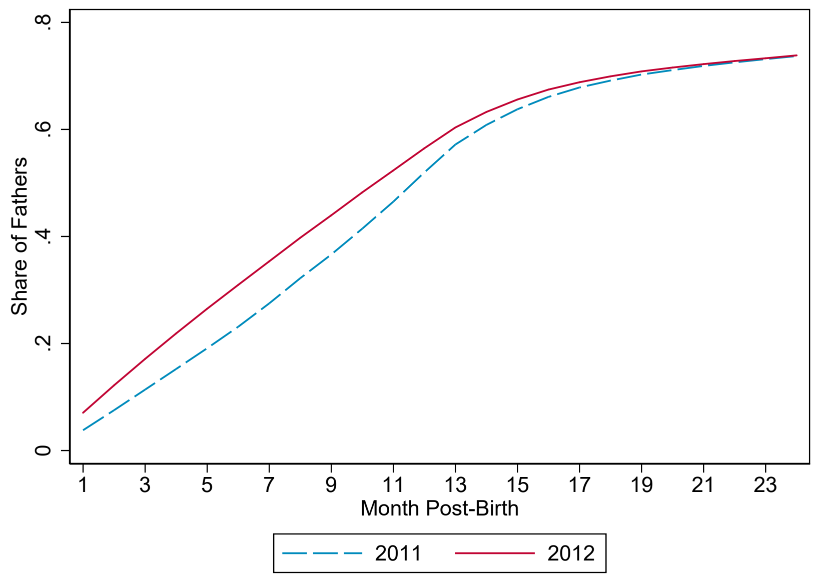
8 Figures

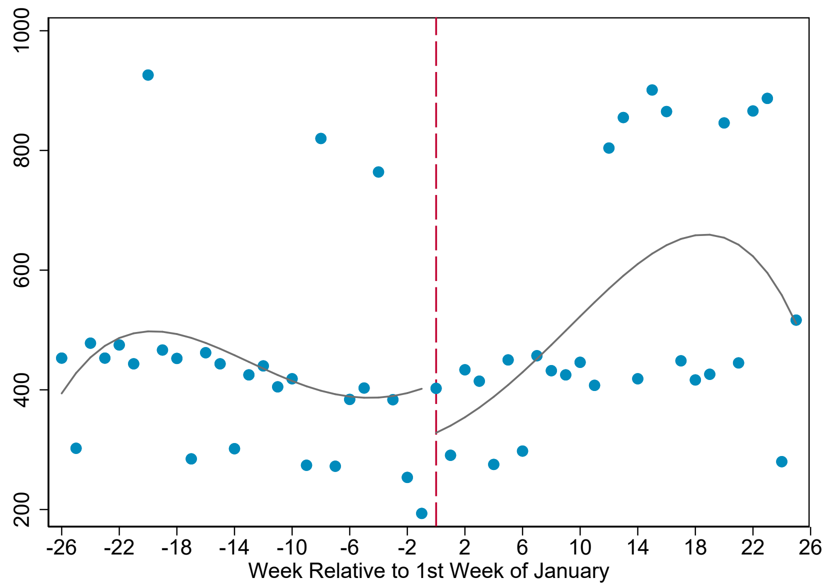
Figure 1: Cumulative Distribution of Initiation of Post-Baseline Paternity Leave for Fathers of Firstborn Children Born in 2011 and 2012



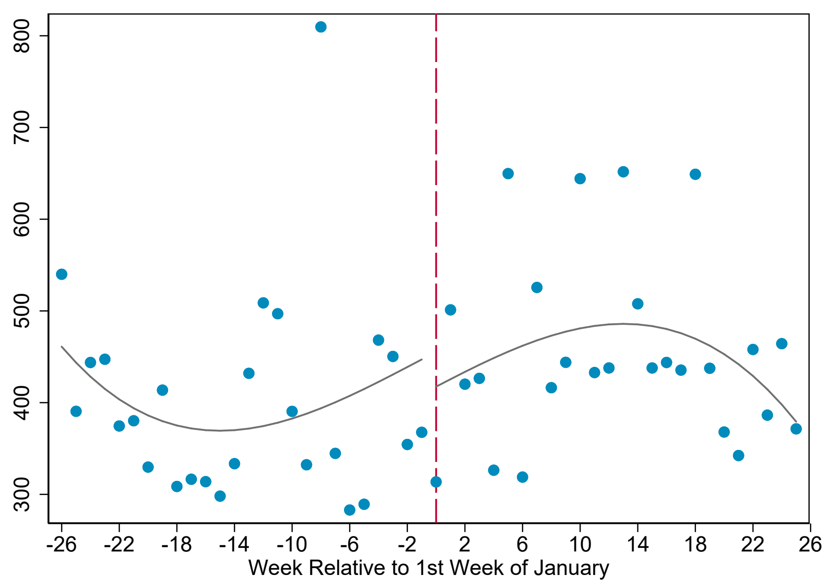
Note: The figure plots the cumulative distribution of the initiation of post-baseline parental leave among fathers by month following childbirth separately for fathers of firstborn singleton children born in 2011 and 2012.

Figure 2: Number of Births by Birth Month in Reform and Non-Reform Samples

(a) Reform Sample



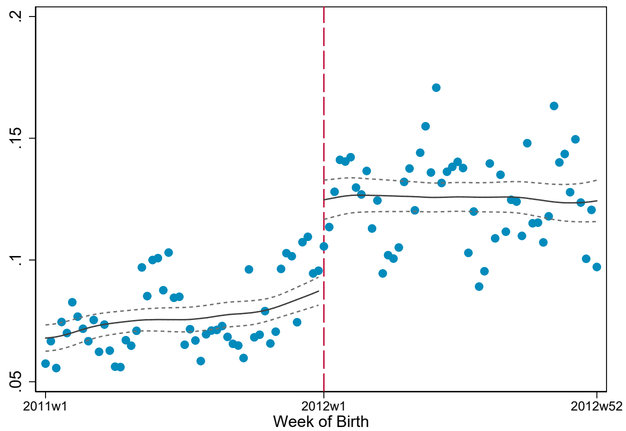
(b) Non-Reform Sample



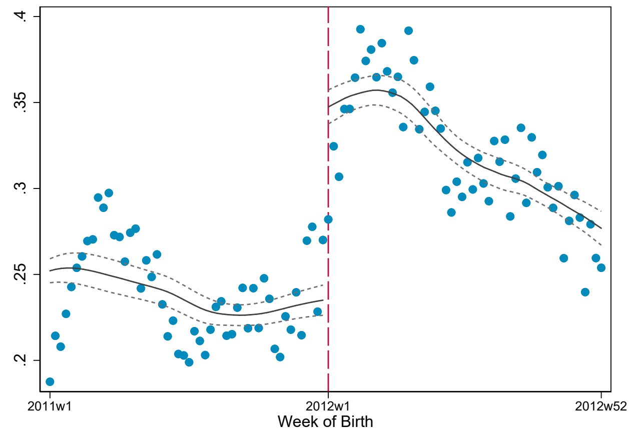
Note: The sample includes all firstborn singleton children born in 2008-2012 with information on exact date of birth. Sub-figure (a) plots the total number of births by birth week in the reform sample with a 6-month bandwidth (July 2011 - June 2012). Sub-figure (b) plots the average of the total number of births by birth week across all years in the non-reform sample with the same bandwidth (July 2008 - June 2011). The fitted lines are predicted from 4th order polynomial models. We follow Lee and Lemieux (2010) by selecting the model with the smallest Akaike Information Criterion (AIC) value.

Figure 3: Effects of 2012 "Double Days" Reform on Paternity Leave Take-Up

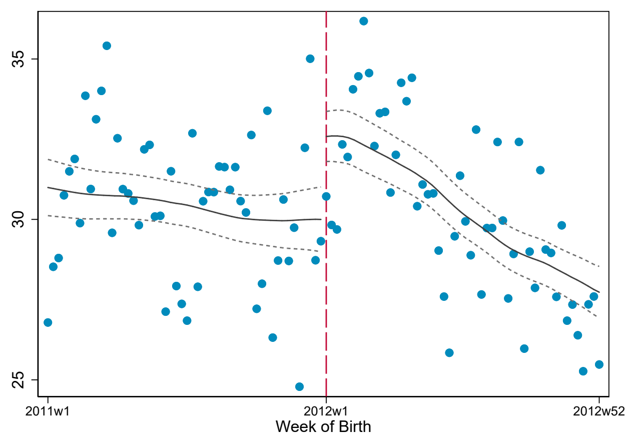
(a) Any Post-Baseline Leave in First 60 Days



(b) Any Post-Baseline Leave in First 180 Days



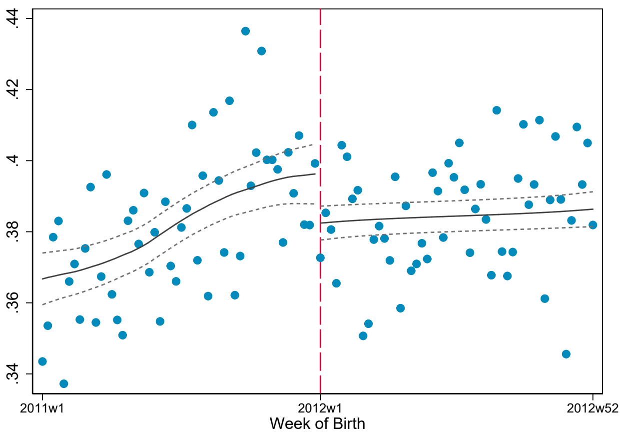
(c) Tot. # Leave Days in First 180 Days



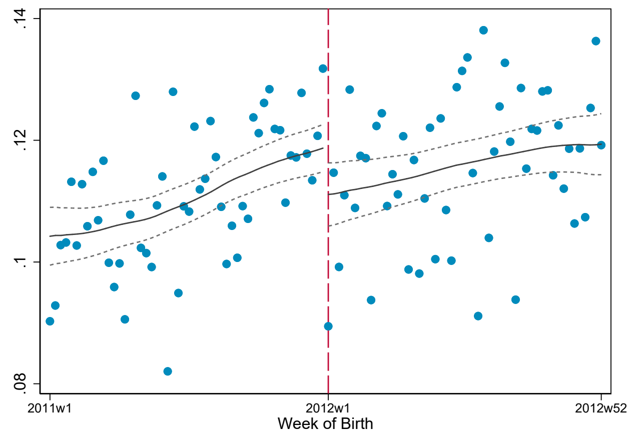
Note: The sample includes all firstborn singleton children born in 2011-2012 with information on exact date of birth. The figures display the means of outcome variables by the child's birth week. The 2012 reform is denoted with a vertical red dashed line. The fitted curves and 95% confidence intervals are predicted from local linear polynomial models on each side of the cut-off. The paternity leave outcomes are listed in the sub-figure headings. The total number of leave days in first 180 days post-childbirth (sub-figure c) includes both baseline and post-baseline leave.

Figure 4: Effects of 2012 "Double Days" Reform on Maternal Health Outcomes in First 180 Days Post-Childbirth, Inpatient and Outpatient Data

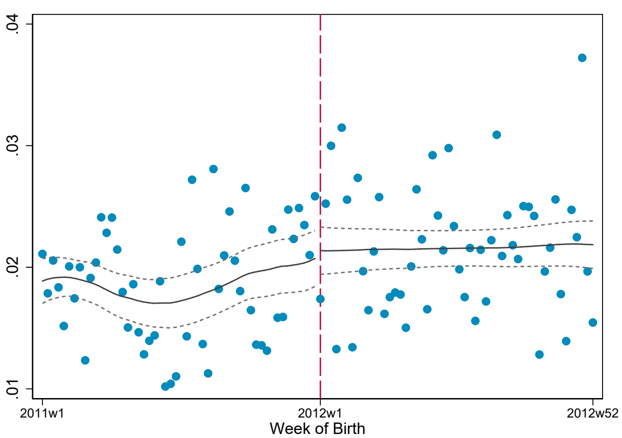
(a) Any Inpatient/Outpatient Visit



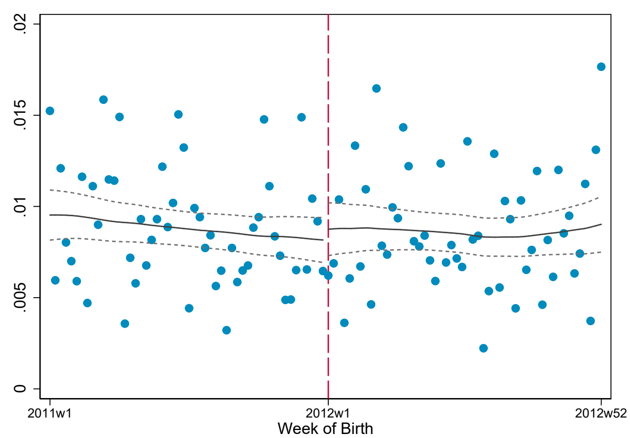
(b) Visit for Childbirth Complications



(c) Visit for Mental Health Issues



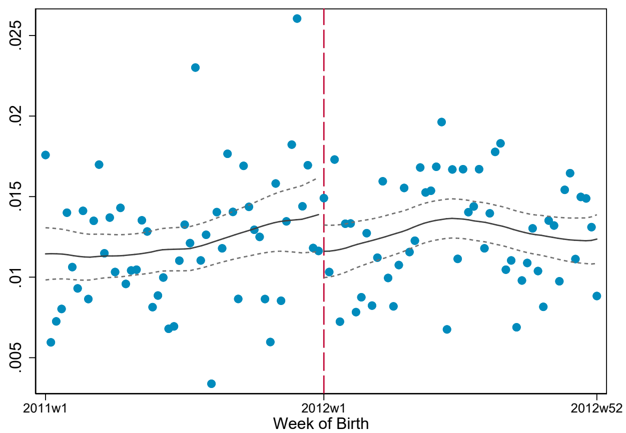
(d) Visit for External Causes or Counseling



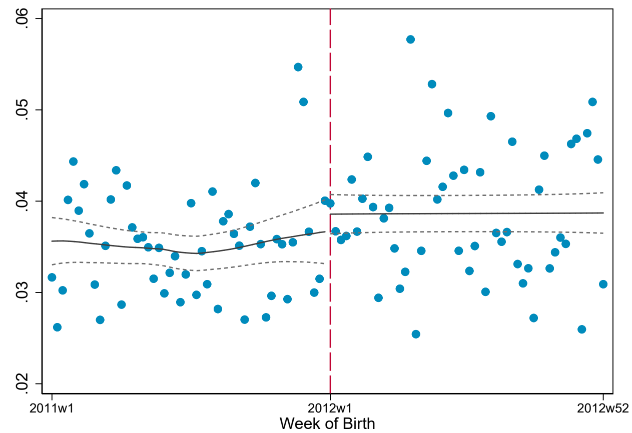
Note: The sample includes all firstborn singleton children born in 2011-2012 with information on exact date of birth. The figures display the means of outcome variables by the child's birth week. The 2012 reform is denoted with a vertical red dashed line. The fitted curves and 95% confidence intervals are predicted from local linear polynomial models on each side of the cut-off. The outcomes are measured using inpatient and specialist outpatient records data. See Appendix B for more details on the exact ICD codes for outcomes.

Figure 5: Effects of 2012 "Double Days" Reform on Maternal Health Outcomes in First 180 Days Post-Childbirth, Prescription Drug Data

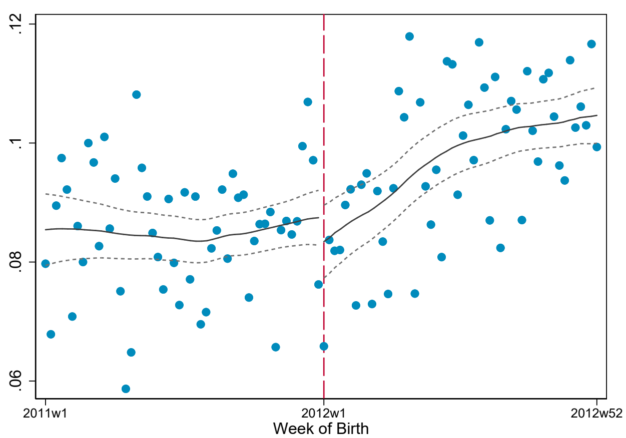
(a) Any Anti-Anxiety Drug



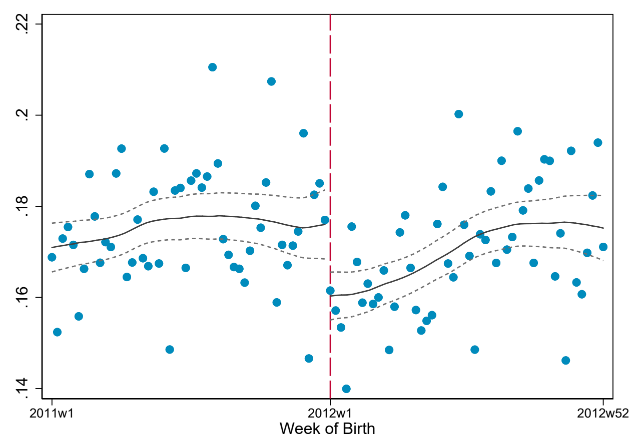
(b) Any Anti-Depressant Drug



(c) Any Painkiller Drug



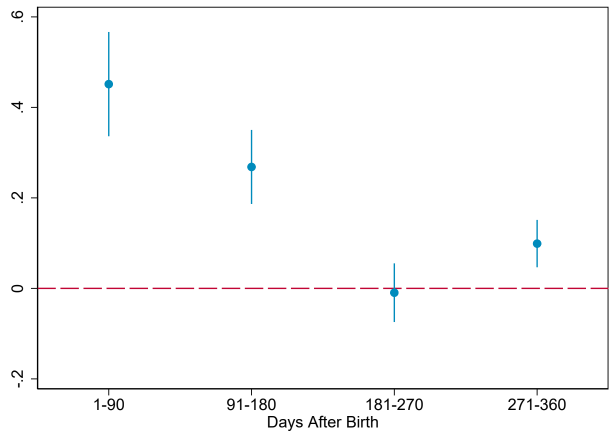
(d) Any Antibiotic Drug



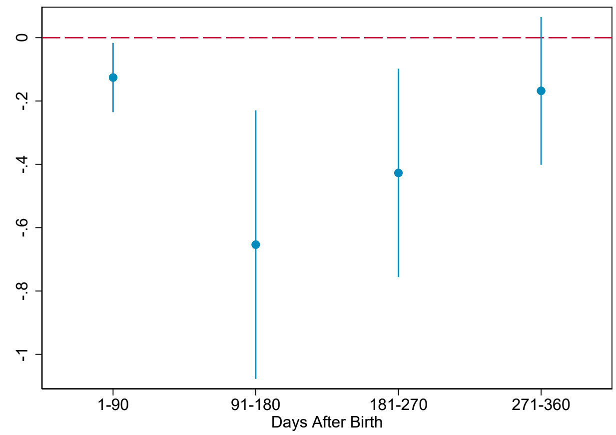
Note: The sample includes all firstborn singleton children born in 2011-2012 with information on exact date of birth. The figures display the means of outcome variables by the child's birth week. The 2012 reform is denoted with a vertical red dashed line. The fitted curves and 95% confidence intervals are predicted from local linear polynomial models on each side of the cut-off. The outcomes are measured using prescription drug records data. See Appendix B for more details on the exact ATC codes for outcomes.

Figure 6: Timing of Effects of 2012 "Double Days" Reform on Paternity Leave and Maternal Health Outcomes

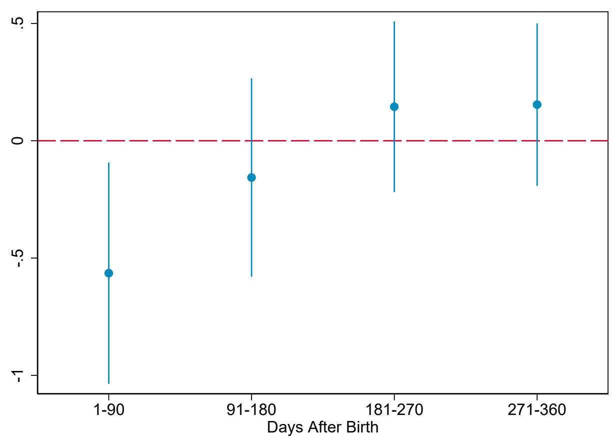
(a) Fathers: Any Post-Baseline Leave



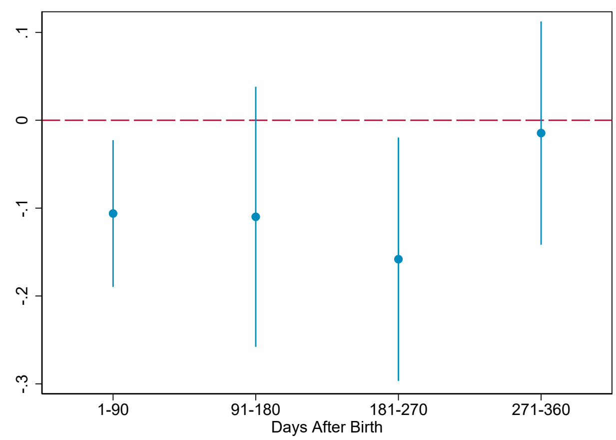
(b) Mothers: Visit for Childbirth Complications



(c) Mothers: Any Anti-Anxiety Drug



(d) Mothers: Any Antibiotic



Note: The figures plot the RD-DD treatment coefficients divided by the dependent variable means (i.e., the magnitudes can be interpreted as percent changes relative to the sample means) and 95% confidence intervals from regression models that use outcomes measured in the periods since childbirth denoted on the $x-$axis of each graph. The outcomes are listed in the sub-figure headings. See Appendix B for more details on the exact ICD and ATC codes for outcomes. See notes under Table 3 for more details on the specifications and controls.

9 Tables

Table 1: Means of Background Characteristics and Maternal Health Outcomes

	All	Exact DOB	RD-DD Sample	Med. History
Mother low education	0.45	0.45	0.45	0.53
Father low education	0.57	0.57	0.57	0.62
Mother age	28.83	28.79	28.85	28.63
Father age	31.90	31.86	31.91	31.61
Mother income (1000s)	207.78	206.94	205.53	179.17
Father income (1000s)	275.26	274.22	273.32	258.54
Mother foreign-born	0.21	0.21	0.21	0.18
Father foreign-born	0.22	0.22	0.22	0.20
Any inpatient	0.05	0.05	0.05	0.06
Any specialist outpatient	0.33	0.35	0.34	0.43
Any visit for childbirth complications	0.10	0.10	0.10	0.12
Any visit for mental health	0.02	0.02	0.02	0.05
Any visit for external causes/medical counseling	0.01	0.01	0.01	0.01
Any anti-anxiety/anti-depressant drug	0.04	0.04	0.04	0.12
Any painkiller drug	0.08	0.09	0.08	0.12
Any antibiotic drug	0.16	0.17	0.17	0.21
Observations	233981	222638	88502	25454

Notes: This table reports the means of selected parental background characteristics and maternal health outcomes measured in the first 180 days post-childbirth. Column (1) includes all firstborn singleton children born in 2008-2012. Column (2) limits the sample to children with information on exact date of birth. Column (3) uses our primary RD-DD analysis sample, which consists of firstborn singleton children with information on exact dates of birth born in the months of October-December of 2008-2011 and January-March of 2009-2012. Column (4) limits the RD-DD analysis sample to children of mothers who have a pre-birth medical history, which we define as either having any inpatient visit in months 1-24 before childbirth or any specialist outpatient visit for mental health reasons in months 1-60 before childbirth or any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth. See text for more details. Appendix B provides more details on the exact ICD and ATC codes for maternal health outcomes.

Table 2: McCrary Test Using Different Polynomials in Week of Birth

	1st	2nd	3rd	4th	5th	6th
Reform × Birth Jan-June	36.00	36.00	36.00	36.00	36.00	36.00
	(61.02)	(60.62)	(60.24)	(59.52)	(59.69)	(59.92)
Reform	36.91	36.91	36.91	36.91	36.91	36.91
	(43.15)	(42.86)	(42.59)	(42.09)	(42.21)	(42.37)
Dummy for Birth Jan-June	1.302	1.302	-78.52	-78.52	-41.85	-41.85
	(68.25)	(67.80)	(85.92)	(84.90)	(100.4)	(100.8)
Observations	104	104	104	104	104	104
AIC	1349.8	1349.4	1349.0	1347.4	1348.9	1350.6

Notes: Each column reports coefficients from separate regressions. The data are collapsed into week-of-birth bins, with the outcome being the total number of firstborn singleton births. The reform sample includes births in July 2011 - June 2012, while the non-reform sample includes births in July 2008 - June 2011. We report results from models that use 1st through 6th order polynomials in the running variable, which is the week of birth normalized relative to the first week of January in each year. We report the Akaike Information Criterion (AIC) values in the bottom row. Robust standard errors in brackets.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 3: Effects of "Double Days" Reform on Paternity Leave Take-Up

	Any Post-Baseline (Days 1-60)	Any Post-Baseline (Days 1-180)	Tot # Days (Days 1-180)
A. All first births			
Reform × Birth Jan-Mar	0.0388***	0.0594***	1.887**
	[0.00470]	[0.00705]	[0.825]
Dep. var mean	0.0783	0.244	31.43
N	82558	82558	82558
B. Mothers with medical history			
Reform × Birth Jan-Mar	0.0487***	0.0664***	1.112
	[0.00933]	[0.0132]	[1.647]
Dep. var mean	0.0971	0.260	34.52
N	23935	23935	23935

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are: (1) indicator for any post-baseline paternity leave in days 1-60 after childbirth, (2) indicator for any post-baseline paternity leave in days 1-180 after childbirth, and (3) total number of paternity leave days (including baseline leave) in days 1-180 after childbirth. The reported coefficients are from the RD-DD model. We compare the differences in outcomes for fathers of firstborn singleton children born in January-March 2012 and October-December 2011 ("reform sample"), relative to the difference in outcomes in the same months in the previous three years (January-March 2009, 2010, and 2011 versus October-December 2008, 2009, and 2010, "non-reform sample"). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. All regressions include controls for child gender and for the following family characteristics measured in the year before birth: maternal and paternal earnings (in 1000s of SEK), indicators for each parent's age groups (<20, 20-24, 25-34, 35+), indicators for each parent's education levels (high school or less, some college, university degree or more), an indicator for the parents being married, indicators for each parent being foreign-born. We also include birth year fixed effects. Robust standard errors in brackets. Panel A reports results for the whole analysis sample, while Panel B limits the sample to mothers with a pre-birth medical history, which we define as either having any inpatient visit in months 1-24 before childbirth or any specialist outpatient visit for mental health reasons in months 1-60 before childbirth or any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 4: Effects of “Double Days” Reform on Maternal Health Outcomes in Inpatient and Outpatient Data

	Any	Diagnosis Categories		
		Childbirth Comp.	Mental	External/Counseling
A. All first births				
Reform × Birth Jan-Mar	-0.00764	-0.0148***	0.00310	0.000829
	[0.00779]	[0.00507]	[0.00223]	[0.00149]
Dep. var mean	0.366	0.103	0.0182	0.00900
N	82558	82558	82558	82558
B. Mothers with medical history				
Reform × Birth Jan-Mar	-0.0171	-0.0343***	0.00604	0.00105
	[0.0147]	[0.0101]	[0.00664]	[0.00300]
Dep. var mean	0.461	0.128	0.0516	0.0127
N	23935	23935	23935	23935

Notes: Each column in each panel reports coefficients from separate regressions. The maternal health outcomes are measured in days 1-180 after childbirth. The outcomes are: (1) indicator for any inpatient or specialist outpatient visit, (2) any visit for childbirth complications, (3) any visit for mental health reasons, and (4) any visit for external causes or counseling. The reported coefficients are from the RD-DD model. See notes under Table 3 for more details on the specifications. Robust standard errors in brackets. Panel A reports results for the whole analysis sample, while Panel B limits the sample to mothers with a pre-birth medical history, which we define as either having any inpatient visit in months 1-24 before childbirth or any specialist outpatient visit for mental health reasons in months 1-60 before childbirth or any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth. Appendix B provides more details on the exact ICD and ATC codes for maternal health outcomes.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 5: Effects of “Double Days” Reform on Maternal Health Outcomes in Prescription Drug Data

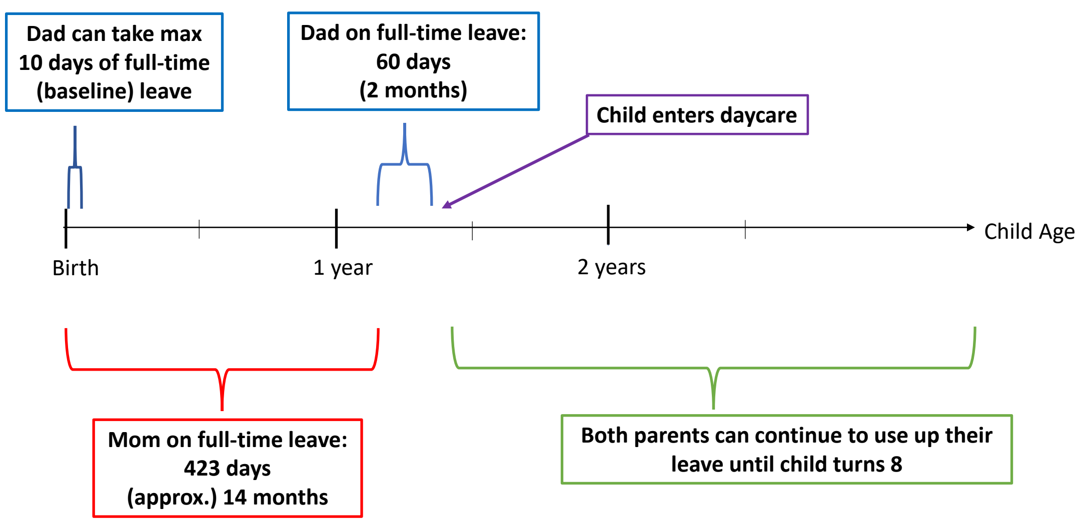
	Any Anti-Anxiety	Any Anti-Depressant	Any Painkiller	Any Antibiotic
A. All first births				
Reform × Birth Jan-Mar	-0.00290*	0.000669	-0.00461	-0.0193***
Mar	[0.00176]	[0.00299]	[0.00445]	[0.00602]
Dep. var mean	0.0112	0.0338	0.0831	0.170
N	82558	82558	82558	82558
B. Mothers with medical history				
Reform × Birth Jan-Mar	-0.00863*	0.000661	-0.00322	-0.0301**
	[0.00486]	[0.00906]	[0.00965]	[0.0120]
Dep. var mean	0.0274	0.102	0.123	0.213
N	23935	23935	23935	23935

Notes: Each column in each panel reports coefficients from separate regressions. The maternal health outcomes are measured in days 1-180 after childbirth. The outcomes are indicators for: (1) any anti-anxiety drug, (2) any anti-depressant drug, (3) any painkiller drug, and (4) any antibiotic drug. The reported coefficients are from the RD-DD model. See notes under Table 3 for more details on the specifications. Robust standard errors in brackets. Panel A reports results for the whole analysis sample, while Panel B limits the sample to mothers with a pre-birth medical history, which we define as either having any inpatient visit in months 1-24 before childbirth or any specialist outpatient visit for mental health reasons in months 1-60 before childbirth or any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth. Appendix B provides more details on the exact ICD and ATC codes for maternal health outcomes.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

ONLINE APPENDIX

A Additional Results

Figure A1: How Parents Allocate Leave: The Case of the Median Household, 2008-2011



Note: The figure represents how the median family in Sweden allocates leave between parents, using data on parents of firstborn singleton children born in 2008-2011. The number of days on full-time leave for each parent (423 days for mothers and 60 days for fathers) are the medians of the two respective distributions in the data.

Table A1: The 2012 “Double Days” Reform and Parental Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	M. Low Ed	F. Low Ed	M. F-born	F. F-born	M. Age	F. Age	M. Inc	F. Inc
Reform × Birth Jan-Mar	-0.00307	0.000424	-0.00289	-0.00377	-0.0941	-0.0624	5369.0**	5788.4
	[0.00782]	[0.00779]	[0.00651]	[0.00656]	[0.0806]	[0.0995]	[2420.3]	[4286.4]
Dep. var mean	0.448	0.570	0.215	0.218	28.82	31.89	204867.3	271989.0
Indiv. obs.	85954	85954	85954	85954	85954	85954	84253	83875
F-Statistic: 1.57 P-value: 0.13								

Notes: Each column reports coefficients from separate regressions. The dependent variables are the following parental characteristics measured in the year before the child’s birth: indicators for the mother having a low education level, the father having a low education level, the mother being foreign-born, the father being foreign-born, the mother’s age in years, the father’s age in years, the mother’s income (1000s of SEK), and the father’s income (1000s of SEK). The reported coefficients are from the RD-DD model, excluding the controls for parental characteristics. We compare the differences in characteristics of parents of firstborn singleton children born in January-March 2012 and October-December 2011 (“reform sample”), relative to the difference in characteristics in the same months in the previous three years (January-March 2009, 2010, and 2011 versus October-December 2008, 2009, and 2010, “non-reform sample”). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. Robust standard errors in brackets. In the bottom row, we report the F-statistic and associated p-value from a joint test of significance of all the coefficients using a seemingly unrelated regression model.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A2: The 2012 “Double Days” Reform, Birth Outcomes, and Maternal Pre-Birth Medical History Indicators

	Birth Outcomes								Maternal Pre-Birth Medical History			
	(1) Bweight	(2) LBW	(3) Gest.	(4) Preterm	(5) Apgar<7	(6) SGA	(7) Induced	(8) C-section	(9) Inp	(10) Outp	(11) Drug	(12) Any
Reform × Birth Jan-Mar	11.21 [8.689]	-0.00490 [0.00310]	0.0314 [0.0296]	-0.000684 [0.00368]	-0.00204 [0.00368]	-0.00371 [0.00282]	-0.00150 [0.00568]	-0.00176 [0.00601]	-0.00264 [0.00597]	0.00406 [0.00440]	0.00279 [0.00571]	-0.000669 [0.00728]
Dep. var mean	3448.4	0.0398	39.85	0.0584	0.0585	0.0317	0.141	0.181	0.165	0.0749	0.144	0.287
Indiv. obs.	85856	85954	85954	85954	85954	85954	85954	85954	84640	84640	84640	84640
F-Statistic: 0.60 P-value: 0.85												

Notes: Each column reports coefficients from separate regressions. The dependent variables include the following birth outcomes: birth weight (in grams), indicator for low-birth-weight (<2,500g), gestation length (in weeks), indicator for preterm birth (<37 weeks), indicator for Apgar score <7, indicator for small-for-gestational-age, indicator for induction of labor, and indicator for delivery by cesarean section. In the last four columns we use as the dependent variables the following maternal pre-birth medical history indicators: any inpatient visit in months 1-24 before childbirth, any specialist outpatient visit for mental health reasons in months 1-60 before childbirth, any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth, as well as an indicator for any of these three conditions holding (i.e., our indicator for the mother having a pre-birth medical history). The reported coefficients are from the RD-DD model, excluding the controls for parental characteristics. We compare the differences in outcomes for firstborn singleton children born in January-March 2012 and October-December 2011 (“reform sample”), relative to the difference in outcomes in the same months in the previous three years (January-March 2009, 2010, and 2011 versus October-December 2008, 2009, and 2010, “non-reform sample”). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. Robust standard errors in brackets. In the bottom row, we report the F-statistic and associated p-value from a joint test of significance of all the coefficients using a seemingly unrelated regression model.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A3: Effect of "Double Days" Reform on the Likelihood of Father Taking Leave on Days When Mother Needs Medical Care

	Dad Leave During Mom Medical Care
A. All first births	
Reform × Birth Jan-Mar	0.00436
	[0.00344]
Dep. var mean	0.0420
N	82558
B. Mothers with medical history	
Reform × Birth Jan-Mar	0.0159**
	[0.00751]
Dep. var mean	0.0618
N	23935

Notes: Each coefficient in each panel is from a separate regression. The outcome is an indicator that is equal to 1 if a father takes at least one day of leave on the same day as a mother has an inpatient or specialist outpatient visit or fills a prescription during days 1-180 after childbirth. The reported coefficients are from the RD-DD model. See notes under Table 3 for more details on the specifications. Robust standard errors in brackets. Panel A reports results for the whole analysis sample, while Panel B limits the sample to mothers with a pre-birth medical history, which we define as either having any inpatient visit in months 1-24 before childbirth or any specialist outpatient visit for mental health reasons in months 1-60 before childbirth or any anti-anxiety or anti-depressant prescription drug in months 1-36 before childbirth.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A4: Heterogeneity in Effects on Paternity Leave Take-Up by Grandparent Proximity

	Any Post-Baseline (Days 1-60)	Any Post-Baseline (Days 1-180)	Tot # Days (Days 1-180)
A. 1+ grandparent lives in mother's county in year before birth			
Reform × Birth Jan-Mar	0.0436***	0.0648***	1.839**
	[0.00537]	[0.00803]	[0.915]
Dep. var mean	0.0813	0.254	32.23
N	65244	65244	65244
B. No grandparent lives in mother's county in year before birth			
Reform × Birth Jan-Mar	0.0215**	0.0401***	2.081
	[0.00960]	[0.0145]	[1.891]
Dep. var mean	0.0667	0.204	28.43
N	17314	17314	17314

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are: (1) indicator for any post-baseline paternity leave in days 1-60 after childbirth, (2) indicator for any post-baseline paternity leave in days 1-180 after childbirth, and (3) total number of paternity leave days (including baseline leave) in days 1-180 after childbirth. The reported coefficients are from the RD-DD model. See notes under Table 3 for more details on the specifications. Robust standard errors in brackets. Panel A reports results for families in which at least one grandparent aged 74 or less lives in the mother's county of residence in the year before birth, while Panel B reports results for families with no grandparents aged 74 or less living in the mother's county of residence in the year before birth.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A5: Heterogeneity in Effects on Maternal Health Outcomes in Inpatient and Outpatient Data by Grandparent Proximity

	Any	Diagnosis Categories		
		Childbirth Comp.	Mental	External/Counseling
A. 1+ grandparent lives in mother's county in year before birth				
Reform × Birth Jan-Mar	-0.00123	-0.0105*	0.00318	-0.0000252
	[0.00875]	[0.00564]	[0.00254]	[0.00171]
Dep. var mean	0.365	0.101	0.0191	0.00906
N	65244	65244	65244	65244
B. No grandparent lives in mother's county in year before birth				
Reform × Birth Jan-Mar	-0.0314*	-0.0306***	0.00292	0.00406
	[0.0171]	[0.0115]	[0.00460]	[0.00302]
Dep. var mean	0.371	0.114	0.0147	0.00878
N	17314	17314	17314	17314

Notes: Each column in each panel reports coefficients from separate regressions. The maternal health outcomes are measured in days 1-180 after childbirth. The outcomes are: (1) indicator for any inpatient or specialist outpatient visit, (2) any visit for childbirth complications, (3) any visit for mental health reasons, and (4) any visit for external causes or counseling. The reported coefficients are from the RD-DD model. See notes under Table 3 for more details on the specifications. Robust standard errors in brackets. Panel A reports results for families in which at least one grandparent aged 74 or less lives in the mother's county of residence in the year before birth, while Panel B reports results for families with no grandparents aged 74 or less living in the mother's county of residence in the year before birth.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A6: Heterogeneity in Effects on Maternal Health Outcomes in Prescription Drug Data by Grandparent Proximity

	Any Anti-Anxiety	Any Anti-Depressant	Any Painkiller	Any Antibiotic
A. 1+ grandparent lives in mother's county in year before birth				
Reform × Birth Jan-Mar	-0.00271	-0.000500	-0.00445	-0.0168**
	[0.00206]	[0.00350]	[0.00491]	[0.00675]
Dep. var mean	0.0118	0.0364	0.0792	0.169
N	65244	65244	65244	65244
B. No grandparent lives in mother's county in year before birth				
Reform × Birth Jan-Mar	-0.00334	0.00598	-0.00478	-0.0283**
	[0.00313]	[0.00530]	[0.0104]	[0.0133]
Dep. var mean	0.00884	0.0240	0.0976	0.177
N	17314	17314	17314	17314

Notes: Each column in each panel reports coefficients from separate regressions. The maternal health outcomes are measured in days 1-180 after childbirth. The outcomes are indicators for: (1) any anti-anxiety drug, (2) any anti-depressant drug, (3) any painkiller drug, and (4) any antibiotic drug. The reported coefficients are from the RD-DD model. See notes under Table 3 for more details on the specifications. Robust standard errors in brackets. Panel A reports results for families in which at least one grandparent aged 74 or less lives in the mother's county of residence in the year before birth, while Panel B reports results for families with no grandparents aged 74 or less living in the mother's county of residence in the year before birth.

Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A7: Fathers' Sick Leave Use: Jan-Mar 2011 vs. Jan-Mar 2012 Births

	Jan-Mar 2011	Jan-Mar 2012	P-value
Days of Sick Leave	2.707	2.652	0.844
Any Sick Leave	0.045	0.043	0.543
Observations	11353	11509	

Notes: This table reports the means of the annual number of sick leave days and the share of fathers who use any sick leave separately for fathers of firstborn singleton children born in January-March 2011 and January-March 2012. The last column reports the p-values from testing the differences between the values in the previous two columns.

Table A8: Robustness to Using Different Optimal Bandwidth Algorithms: Any Post-Baseline Paternity Leave in Days 1-60 Post-Childbirth

	MSE	MSE-2	MSE-Sum	Min-MSE	Med-MSE	CER	CER-2	CER-Sum	Min-CER	Med-CER
RD Estimate	0.0209***	0.0242***	0.0297***	0.0209***	0.0243***	0.0216**	0.0196**	0.0221***	0.0216**	0.0211**
	[0.00635]	[0.00626]	[0.00568]	[0.00635]	[0.00616]	[0.00876]	[0.00861]	[0.00788]	[0.00876]	[0.00849]
Left BW	203.3	229.3	251.3	203.3	229.3	107.5	121.2	132.9	107.5	121.2
Right BW	203.3	191.7	251.3	203.3	203.3	107.5	101.4	132.9	107.5	107.5
Num. Obs.	48070	49881	59799	48070	51243	24622	25538	30712	24622	26276

Notes: Each coefficient is from a separate regression. The outcome for all regressions is an indicator for any post-baseline paternity leave in days 1-60 after childbirth. We use an RD model with local linear polynomials, comparing births before and after January 1, 2012, using different optimal bandwidth algorithms to select the bandwidths of the number of days used on each side of the cutoff. We include the same controls as in Table 3. The optimal bandwidth algorithms are: (1) one common mean squared error (MSE)-optimal bandwidth selector for both sides of the cutoff; (2) two different MSE-optimal bandwidth selectors (below and above the cutoff); (3) one common MSE-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (4) minimum of (1) and (3); (5) median of (1), (2), and (3) for each side of the cutoff separately; (6) one common coverage error rate (CER)-optimal bandwidth selector; (7) two different CER-optimal bandwidth selectors (below and above the cutoff); (8) one common CER-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (9) minimum of (6) and (8); (10) median of (6), (7), and (8) for each side of the cutoff separately. We follow Calonico et al. (2014), Calonico et al. (2018a), and Calonico et al. (2018b) in using these algorithms. We report the number of days used in the left and right-hand bandwidths in each model at the bottom of the table. Robust standard errors in brackets.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A9: Robustness to Using Different Optimal Bandwidth Algorithms: Any Post-Baseline Paternity Leave in Days 1-180 Post-Childbirth

	MSE	MSE-2	MSE-Sum	Min-MSE	Med-MSE	CER	CER-2	CER-Sum	Min-CER	Med-CER
RD Estimate	0.0553***	0.0781***	0.109***	0.0553***	0.0739***	0.0108	0.0330**	0.0611***	0.0108	0.0314**
	[0.0115]	[0.00931]	[0.00795]	[0.0115]	[0.00945]	[0.0160]	[0.0128]	[0.0110]	[0.0160]	[0.0130]
Left BW	129.7	289.6	267.1	129.7	267.1	68.60	153.1	141.3	68.60	141.3
Right BW	129.7	143.5	267.1	129.7	143.5	68.60	75.89	141.3	68.60	75.89
Num. Obs.	29929	51234	63572	29929	48541	15490	26677	32871	15490	25129

Notes: Each coefficient is from a separate regression. The outcome for all regressions is an indicator for any post-baseline paternity leave in days 1-180 after childbirth. We use an RD model with local linear polynomials, comparing births before and after January 1, 2012, using different optimal bandwidth algorithms to select the bandwidths of the number of days used on each side of the cutoff. We include the same controls as in Table 3. The optimal bandwidth algorithms are: (1) one common mean squared error (MSE)-optimal bandwidth selector for both sides of the cutoff; (2) two different MSE-optimal bandwidth selectors (below and above the cutoff); (3) one common MSE-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (4) minimum of (1) and (3); (5) median of (1), (2), and (3) for each side of the cutoff separately; (6) one common coverage error rate (CER)-optimal bandwidth selector; (7) two different CER-optimal bandwidth selectors (below and above the cutoff); (8) one common CER-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (9) minimum of (6) and (8); (10) median of (6), (7), and (8) for each side of the cutoff separately. We follow Calonico et al. (2014), Calonico et al. (2018a), and Calonico et al. (2018b) in using these algorithms. We report the number of days used in the left and right-hand bandwidths in each model at the bottom of the table. Robust standard errors in brackets.

Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A10: Robustness to Using Different Optimal Bandwidth Algorithms: Any Maternal Inpatient/Outpatient Visit for Childbirth Complications

	MSE	MSE-2	MSE-Sum	Min-MSE	Med-MSE	CER	CER-2	CER-Sum	Min-CER	Med-CER
RD Estimate	-0.0152***	-0.0166***	-0.0152***	-0.0152***	-0.0152***	-0.0174***	-0.0184**	-0.0174***	-0.0174***	-0.0174***
	[0.00473]	[0.00524]	[0.00477]	[0.00477]	[0.00477]	[0.00656]	[0.00727]	[0.00661]	[0.00661]	[0.00661]
Left BW	382.0	318.0	376.4	376.4	376.4	202.0	168.2	199.0	199.0	199.0
Right BW	382.0	304.8	376.4	376.4	376.4	202.0	161.2	199.0	199.0	199.0
Num. Obs.	89839	73988	88683	88683	88683	47832	38691	47147	47147	47147

Notes: Each coefficient is from a separate regression. The outcome for all regressions is an indicator for any maternal inpatient or specialist outpatient visit for childbirth complications in days 1-180 after childbirth. We use an RD model with local linear polynomials, comparing births before and after January 1, 2012, using different optimal bandwidth algorithms to select the bandwidths of the number of days used on each side of the cutoff. We include the same controls as in Table 3. The optimal bandwidth algorithms are: (1) one common mean squared error (MSE)-optimal bandwidth selector for both sides of the cutoff; (2) two different MSE-optimal bandwidth selectors (below and above the cutoff); (3) one common MSE-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (4) minimum of (1) and (3); (5) median of (1), (2), and (3) for each side of the cutoff separately; (6) one common coverage error rate (CER)-optimal bandwidth selector; (7) two different CER-optimal bandwidth selectors (below and above the cutoff); (8) one common CER-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (9) minimum of (6) and (8); (10) median of (6), (7), and (8) for each side of the cutoff separately. We follow Calonico et al. (2014), Calonico et al. (2018a), and Calonico et al. (2018b) in using these algorithms. We report the number of days used in the left and right-hand bandwidths in each model at the bottom of the table. Robust standard errors in brackets.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A11: Robustness to Using Different Optimal Bandwidth Algorithms: Any Maternal Antibiotic Prescription Drug

	MSE	MSE-2	MSE-Sum	Min-MSE	Med-MSE	CER	CER-2	CER-Sum	Min-CER	Med-CER
RD Estimate	-0.0204***	-0.0224***	-0.0200***	-0.0200***	-0.0210***	-0.0191**	-0.0191**	-0.0205**	-0.0205**	-0.0187**
	[0.00585]	[0.00593]	[0.00650]	[0.00650]	[0.00613]	[0.00811]	[0.00821]	[0.00904]	[0.00904]	[0.00851]
Left BW	344.3	459.3	279.9	279.9	344.3	182.1	242.9	148.0	148.0	182.1
Right BW	344.3	246.9	279.9	279.9	279.9	182.1	130.6	148.0	148.0	148.0
Num. Obs.	81585	83480	66538	66538	74375	42960	44104	34552	34552	38882

Notes: Each coefficient is from a separate regression. The outcome for all regressions is an indicator for any maternal antibiotic prescription drug in days 1-180 after childbirth. We use an RD model with local linear polynomials, comparing births before and after January 1, 2012, using different optimal bandwidth algorithms to select the bandwidths of the number of days used on each side of the cutoff. We include the same controls as in Table 3. The optimal bandwidth algorithms are: (1) one common mean squared error (MSE)-optimal bandwidth selector for both sides of the cutoff; (2) two different MSE-optimal bandwidth selectors (below and above the cutoff); (3) one common MSE-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (4) minimum of (1) and (3); (5) median of (1), (2), and (3) for each side of the cutoff separately; (6) one common coverage error rate (CER)-optimal bandwidth selector; (7) two different CER-optimal bandwidth selectors (below and above the cutoff); (8) one common CER-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (9) minimum of (6) and (8); (10) median of (6), (7), and (8) for each side of the cutoff separately. We follow Calonico et al. (2014), Calonico et al. (2018a), and Calonico et al. (2018b) in using these algorithms. We report the number of days used in the left and right-hand bandwidths in each model at the bottom of the table. Robust standard errors in brackets.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A12: Robustness to Using Different Optimal Bandwidth Algorithms: Any Maternal Anti-Anxiety Prescription Drug

	MSE	MSE-2	MSE-Sum	Min-MSE	Med-MSE	CER	CER-2	CER-Sum	Min-CER	Med-CER
RD Estimate	-0.00333***	-0.00358***	-0.00333***	-0.00333***	-0.00333***	-0.00458**	-0.00409**	-0.00458**	-0.00458**	-0.00458**
	[0.00127]	[0.00133]	[0.00127]	[0.00127]	[0.00127]	[0.00180]	[0.00189]	[0.00180]	[0.00180]	[0.00180]
Left BW	384.2	402.3	384.1	384.1	384.2	203.2	212.7	203.1	203.1	203.2
Right BW	384.2	294.0	384.1	384.1	384.1	203.2	155.5	203.1	203.1	203.1
Num. Obs.	90541	82658	90541	90541	90541	48070	43389	48070	48070	48070

Notes: Each coefficient is from a separate regression. The outcome for all regressions is an indicator for any maternal anti-anxiety prescription drug in days 1-90 after childbirth. We use an RD model with local linear polynomials, comparing births before and after January 1, 2012, using different optimal bandwidth algorithms to select the bandwidths of the number of days used on each side of the cutoff. We include the same controls as in Table 3. The optimal bandwidth algorithms are: (1) one common mean squared error (MSE)-optimal bandwidth selector for both sides of the cutoff; (2) two different MSE-optimal bandwidth selectors (below and above the cutoff); (3) one common MSE-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (4) minimum of (1) and (3); (5) median of (1), (2), and (3) for each side of the cutoff separately; (6) one common coverage error rate (CER)-optimal bandwidth selector; (7) two different CER-optimal bandwidth selectors (below and above the cutoff); (8) one common CER-optimal bandwidth selector for the sum of regression estimates (as opposed to difference thereof); (9) minimum of (6) and (8); (10) median of (6), (7), and (8) for each side of the cutoff separately. We follow Calonico et al. (2014), Calonico et al. (2018a), and Calonico et al. (2018b) in using these algorithms. We report the number of days used in the left and right-hand bandwidths in each model at the bottom of the table. Robust standard errors in brackets.

Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A13: Effects on Main Outcomes Using Alternative Specifications

	Fathers' Post-Baseline Leave			Maternal Health		
	Any, Days 1-60	Any, Days 1-180	Tot # Days (Days 1-180)	Childbirth Comp.	Antibiotic	Anti-Anxiety
A. Share Days Eligible in Days 1-60 Post-Birth						
Share Days Eligible in Days 1-60 Post-Birth	0.0527***	0.0840***	2.632***	-0.0149**	-0.0195***	-0.00268*
	[0.00546]	[0.00834]	[0.993]	[0.00614]	[0.00730]	[0.00158]
B. Share Days Eligible in Days 1-180 Post-Birth						
Share Days Eligible in Days 1-180 Post-Birth	0.128***	0.202***	6.414***	-0.0388**	-0.0519***	-0.00607
	[0.0133]	[0.0207]	[2.444]	[0.0153]	[0.0182]	[0.00387]
C. Drop December Births (N=69953)						
Reform $\times$ Birth Jan-Mar	0.0479***	0.0753***	2.651***	-0.0157***	-0.0215***	-0.00301**
	[0.00513]	[0.00772]	[0.920]	[0.00566]	[0.00674]	[0.00150]
Dep. var mean	0.078	0.244	31.4	0.103	0.170	0.006

Notes: Each coefficient is from a separate regression. Indicators for maternal inpatient/outpatient visits for childbirth-related complications and antibiotic prescriptions are measured in the first 180 days post-childbirth, while the indicator for anti-anxiety prescriptions is measured in the first 90 days post-childbirth. Panel A uses specifications in which the main treatment variable is the share of days between the child's first and 60th day of life that parents are eligible for the "Double Days". Panel B uses specifications in which the main treatment variable is the share of days between the child's first and 180th day of life that parents are eligible for the "Double Days". We uses this treatment variables instead of the interaction term between the reform sample dummy and the indicator for a birth in January-March. The rest of the variables are the same as in our main RD-DD specification. Panel C uses our main RD-DD specifications, but drops all December births. See notes under Table 3 for more details about specifications and control variables. Robust standard errors in brackets.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

B Definitions of Health-Related Outcomes

Diagnosis (ICD) codes For all mothers, we obtain comprehensive inpatient and outpatient medical records. We create indicators for visits associated with the following diagnosis codes (ICD-10) within different time periods from the birth of the child (in the inpatient records, we exclude the visit associated with the birth itself):

- Conditions related to or aggravated by the pregnancy, childbirth, or by the puerperium (maternal causes or obstetric causes) (O00-O99)
- Mental, behavioral and neurodevelopmental disorders (F00-F98)
- External causes and medical counseling
  - Injury, poisoning and certain other consequences of external causes (S00-S99, T00-T32, T66-T78)
  - Assault (X92-Y09)
  - Factors influencing health status and contact with health services (Z00-Z99)

Prescription drug (ATC) codes Prescription drugs are classified according to the Anatomical Therapeutic Chemical Classification System (ATC). To associate certain prescription drugs to certain diagnoses, we use the classification system below:

- Anti-anxiety: ATC code begins with “N05B”
- Anti-depressant: ATC code begins with “N06A”
- Antibiotic: ATC code begins with “J01”
- Painkiller (analgesic): ATC code begins with “N02”