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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.

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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. These days could be taken on an intermittent basis. Importantly, the reform did not alter the total duration of leave available to households. Thus, fathers were granted more flexibility to choose, on a day-to-day basis, whether to claim a paid leave benefit to stay home with the mother and child or whether to save the benefit for the family's future use.

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 theoretical analysis of the flexibility reform. Based on four parsimonious assumptions about the benefits and costs of parental leave, our dynamic model describes how parents divide a household's allocation of

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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.

parental leave days, taking into account the evolution of the labor market costs and household benefits of the presence of each parent. We first derive parents' optimal division of leave when they are *not* allowed to take leave simultaneously. This characterization is highly consistent with actual parental leave use in Sweden in the pre-reform period, which underscores the model's applicability to our setting. We then introduce a reform that relaxes the restriction on simultaneous leave. 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. For example, additional support for the mother may be more valuable to the household on days when she is not feeling well (e.g., because she is coming down with an infection), is fatigued or stressed, or is having mental health issues.

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 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.[5]

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

[5] 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).

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 postpartum 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. We show that the decline in health care visits is entirely driven by *unplanned* rather than scheduled appointments, which is consistent with an improvement in underlying maternal health as opposed to a sub-optimal decline in health care utilization. With regard to maternal 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 at the 5% level) 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.[6]

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 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

[6]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.

first place.[7]

Our study contributes to a large literature on parental leave (for some overviews, see: Olivetti and Petrongolo, 2017; Rossin-Slater, 2018; Rossin-Slater and Uniat, 2019). However, unlike most studies that identify the impacts of program implementation or extensions, our paper instead provides insights into the details of program *design*. 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 policy-makers 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.[8] While the evidence on the potential (bonding or labor market) benefits of such inflexibility is mixed,[9] our study demonstrates that doing the opposite—letting fathers take leave *intermittently* and *jointly* with the mother—could be critical to maternal postpartum recovery.[10]

Our results further 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-

[7] 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.

[8] Indeed, nearly all existing studies of paternity leave focus on the consequences of so-called "Daddy Month" 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; Olafsson and Steingrimsdottir, 2019; Andresen and Nix, 2019.

[9] 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).

[10] 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.

reform, the average father takes just a few additional days of leave alongside the mother out of the full 30 days that they are allowed. This limited response likely stems from the fact that parents incur the marginal cost of taking a "Double Day" by foregoing the option to take an additional parental leave day in the future. This feature makes the policy potentially less costly than other interventions that could be used to support mothers during the postpartum period, such as nurse home visiting programs. By leveraging families' private information about when it is most desirable to stay home relative to the cost of missed time at work, workplace flexibility allows households to ensure that they reap large benefits relative to the number of leave days used.

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.[11] More broadly, our results contribute to our understanding of how policy influences maternal postpartum health. While discussions about maternal health often center around the role of the medical system,[12] 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.[13] This paper emphasizes the importance of a particular aspect of a new mother's home environment: the presence of the father.

[11]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.

[12]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`.

[13]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 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).

## 2 Institutional Setting

Sweden implemented its gender-neutral paid parental leave policy in 1974, replacing the previous maternity leave system that only covered mothers.[14] 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.[15] 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.[16] Moreover, the benefits can be claimed on a part-time basis.[17]

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.[18]

Additionally, although leave in the original system was completely transferrable between parents, the vast majority of the leave days was taken by mothers.[19] In an effort to promote 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

[14] 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.

[15] 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).

[16] 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.

[17] 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).

[18] 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.

[19] Duvander and Johansson (2012) report that men used 0.5 percent of all parental leave days at the time of the program's inception in 1974, and this number rose only slightly over the next two decades.

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 parental 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.[20] 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) 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

[20] 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.

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.

**Other benefits.** In the pre-reform period, when fathers were restricted to only ten baseline days during which they could take full-time paid parental leave at the same time as mothers, fathers could in principle 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 relied on own sick leave benefits for these purposes. In addition, if a mother claims her sick leave benefit instead of her parental leave benefit on a given day, then the father can claim a full-time parental leave benefit on that same day. However, sick leave benefits are reimbursed at a lower rate than parental leave benefits for most parents, making this a potentially unappealing option for families. Nevertheless, if parents were using sick leave for these purposes before the "Double Days" reform, we would expect there to be a decline in sick leave use among both mothers and fathers in the post-reform period.

As sick leave data are only available at an annual level, we compare the annual number of sick leave days used by parents of firstborn singleton children born in January-March 2011 and January-March 2012 in Appendix Table A1. We do not detect any statistically significant differences either in the average number of sick leave days or in the share of parents with any sick leave across the two groups, suggesting that substitution from sick leave toward parental leave is not affecting the interpretation of our main estimates.

Unfortunately, we do not have data on other benefits such as vacation 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.

# 3 A Model of Household Parental Leave Use

We develop a framework of parental leave use that describes how parents divide a household's allocation of parental leave days, taking into account the labor market costs as well as the household benefits of the presence of each parent. We start from a set-up that mimics Sweden's parental leave system before the introduction of "Double Days," and then examine how this reform alters the allocation of parental leave and household wellbeing.

## 3.1 General Notation

Consider a household consisting of a child, mom $m$, and dad $d$. Let $t$ denote discrete time (in days), with childbirth at $t = 0$. Time is divided into two intervals, before and after publicly-provided childcare becomes available.[21] Specifically, there exists some $\bar{t} > 0$, such that:

- For $t < \bar{t}$, public childcare is not available. We refer to these days as "core" days.
- For $t \geq \bar{t}$, public childcare is generally available, except on some days (e.g., school holidays). We refer to days without childcare during this period as "miscellaneous" days.

The total number of parental leave days available to the family is $T > \bar{t}$. The total number of core and miscellaneous days exceeds $T$.[22]

[21] Children are eligible for publicly-provided childcare at age 1. In practice, most childcare slots open up in August (when all children are "shifted" one year forward). Thus, many children do not gain access to a desired childcare slot until August in the year after they turn one year old.

[22] Consistent with this conjecture, parents generally exhaust their leave days. (Recall that parental leave can be claimed until the child turns 8 years old; thus, the period $t \geq \bar{t}$ essentially lasts until the child's eighth

Let $B_p(t)$ and $C_p(t)$ denote the benefit and cost of a leave day taken (alone) by parent $p \in \{m, d\}$, respectively, on a day before childcare is available (i.e., during a core day $t < \bar{t}$). The corresponding benefit and cost of taking leave on a miscellaneous day during $t \geq \bar{t}$ is given by $b_p(t)$ and $c_p(t)$, respectively.[23] Let the value of parental leave be strictly positive, $B_p - C_p > 0$ and $b_p - c_p > 0$, on days without childcare; and negative otherwise.

## 3.2 Assumptions

We assume that household decisions are efficient, and (for simplicity) abstract away from discounting.[24] The general household problem of choosing an allocation of leave days among the large set of permissible ones is complex and dynamic. To obtain specific predictions for how parents divide the leave, we need to impose more structure. We make four parsimonious assumptions about the benefits and costs of parental leave. They are not meant to reflect the reality of all families, but simply to be plausible for the "typical" family in our data.

The first two assumptions concern the benefits of parental leave. We define the difference between the benefit of the mom and the benefit of the dad taking leave on core and miscellaneous days, respectively, as: $\Delta_B(t) \equiv B_m(t) - B_d(t)$ and $\Delta_b(t) \equiv b_m(t) - b_d(t)$.

**Assumption 1** (Early care)**.** *$B_p(t)$ is strictly decreasing and converges to $b_p(t) = b_p > 0$.*

Intuitively, the benefit of parental care is the largest immediately after childbirth, and then gradually falls to $b_p$, the benefit of a miscellaneous day.

**Assumption 2** (Maternal advantage)**.** *$\Delta_B(t)$ is positive, strictly decreasing, and converges to $\Delta_b(t) = \Delta_b \geq 0$.*

The relative advantage of the mother staying home being decreasing over time is consistent with, for example, the fact that breastfeeding is usually concentrated in the beginning of a child's life.

birthday.)

[23]These benefits and costs pertain to those subjectively "perceived" by the family. To the extent that they differ from the true benefits and costs (i.e., their perceptions may be wrong), it is the perceived benefits and costs that matter for our analysis because they drive parental leave choices.

[24]As discussed in footnote 14, Sweden's parental leave program grants benefits to both parents of a child regardless of their marital or cohabitation status. In our model, we refer to the mom and dad as residing in one household; strictly speaking, however, we only require that parents are able to make efficient joint decisions.

The next two assumptions concern the costs of parental leave. Let $C_p(t) \equiv (1-\alpha)w_p + \kappa(\tau_p)$, where $w_p$ is the (constant) current wage, $\alpha$ is the wage replacement rate, $\kappa(\tau_p)$ is a future career cost, and $\tau_p$ is total number of core leave days taken by parent $p$ (up to $t$). By contrast, assume that leave taken on miscellaneous days does not have any long-term career consequences, i.e., $c_p(t) \equiv (1-\alpha)w_p$.

**Assumption 3** (Parental income difference)**.** $w_d > w_m$.

Consistent with this assumption, the intra-household median earnings difference (father minus mother earnings) in our analysis sample is positive.[25]

**Assumption 4** (Career cost)**.** *Let* $\kappa > 0$ *and* $\frac{\bar{t}}{2} < \tau^c < \bar{t}$ *such that*

$$\kappa(\tau_p) = \begin{cases} \kappa & \textit{if } \tau_p \geq \tau_c \\ 0 & \textit{otherwise} \end{cases}$$

Intuitively, this assumption captures the idea that absence from the labor market *for an extended period of time* (longer than $\tau^c$) comes with a career cost. While we use a simple step function for tractability only, the idea that career costs are particularly pronounced when a parent takes a long period of leave is consistent with empirical evidence.[26] Here, the critical time threshold $\tau^c$ is chosen such that the career cost can be avoided if and only if the core days are (suitably) shared by both parents.[27]

## 3.3 Parental Leave System Before the "Double Day" Reform

We start by defining a "basic parental leave system" as one in which parents can freely divide the total allowance $T$, but where leave cannot be taken simultaneously by both parents. This

[25] This fact is also true at the mean in our data. As can be seen in Table 1, the mean of mothers' earnings is approximately 75 percent of the mean of fathers' earnings. Note that we do not observe wages, only earnings (i.e., wage $\times$ hours).

[26] Multiple studies document negative labor market impacts of prolonged leave (Lalive and Zweimüller, 2009; Lequien, 2012; Schönberg and Ludsteck, 2014; Bičáková and Kalíšková, 2016; Canaan, 2017). In general, cross-country comparisons suggest that provisions of leave of up to one year in length have zero or positive impacts on maternal employment, whereas longer leave entitlements can negatively affect women's long-term labor market outcomes (Ruhm, 1998; Blau and Kahn, 2013; Thévenon and Solaz, 2013; Olivetti and Petrongolo, 2017; Rossin-Slater, 2018).

[27] This is likely true in the typical Swedish setting, where the core period often extends beyond the child's first birthday (as discussed in footnote 22), while the literature documents career costs associated with leave entitlements longer than a year (as discussed in footnote 26).

represents a simplified version of Sweden's parental leave system before 1995 (when the first earmarked month of leave was introduced) and, more generally, is akin to typical parental leave systems around the world in which parents can divide up a total "budget" of leave days.

**Corollary 1** (Basic system)**.** *Under the basic parental leave system, leave is taken during the entire core period, with residual leave days used in the miscellaneous period. Either mom takes all leave days, or mom takes all leave days except for a single interval of leave days taken by dad at the end of the core period.*

*Proof.* See Appendix B. □

This allocation intuitively reflects the above assumptions: Parental leave is concentrated at the start of a child's life due to the importance of early care (Assumption 1). Further, leave is taken predominantly, if not exclusively, by moms because of maternal advantages in childrearing and parental income differences (Assumptions 2 and 3); a countervailing effect is that extended leave by one parent negatively affects that parent's future career (Assumption 4). Thus, dad may take some core days when doing so allows the household to avoid the maternal career cost.

In Sweden, under the basic parental leave system (prior to 1995) only a small share of all fathers chose to take any leave (Duvander and Johansson, 2012)—this low rate of paternal leave use was in fact the motivation for introducing the first "Daddy Month." In light of the model, this pattern suggests either that parents' income differences were so large that not even career costs could overcome them, or that income differences were modest but career costs were not substantial enough to neutralize them.

Next, we add earmarked leave. Specifically, out of the family's total allowance of $T$ leave days, $E < T$ days are earmarked for each parent (but leave days still cannot be taken simultaneously). This structure resembles Sweden's parental leave system right before the "Double Day" reform that we study, when Sweden had implemented two "Daddy Months" (in 1995 and 2002). We assume that $T - E > \bar{t}$; that is, the household is able to cover the core period with only one parent taking leave.[28]

[28]This assumption reflects the Swedish system at the time of the "Double Day" reform: $T$ was 16 months, $E$ was 2 months, and childcare eligibility occurred at 12 months.

**Corollary 2** (Earmarked leave and the value of a miscellaneous day)**.** *In a basic parental leave system with earmarked leave, if dad takes leave, then he takes it at the end of the core period or during the miscellaneous period. The magnitude of a household's response to the introduction of earmarked leave reflects the household's valuation of a miscellaneous day.*

*Proof.* See Appendix B. □

Intuitively, earmarking affects households in which the dad would have otherwise taken less than $E$ leave days by raising the opportunity cost of *not* taking a paternity leave day—without earmarking the mother can stay home instead; with earmarking, the day is lost. A father induced to take leave allocates it either to the end of the core period (when it can reduce maternal career costs) or during the miscellaneous period (when the household benefit differential is the smallest).

Corollaries 1 and 2 are important for two reasons. First, they provide the model's prediction about parental division of leave before the introduction of "Double Days": Mothers take leave starting at childbirth and for the majority of the core period, while fathers take leave at the end of the core period or during (a subset of the) miscellaneous days. To gauge the plausibility of the model's predictions, we can use data on *actual* parental leave use in the pre-reform period. Appendix Figure A1 illustrates that Corollaries 1 and 2 are highly consistent with actual parental leave use in Sweden in the period before the "Double Days" reform, underscoring the model's applicability to our empirical setting.

Second, the last statement in Corollary 2 links a household's response to the introduction of earmarking to its valuation of a miscellaneous day. While we do not empirically analyze the impact of earmarking in our paper, this result provides an important link between existing evidence on earmarking and the model's predicted household responses to the reform that we study. In particular, multiple studies have documented that Sweden's earmarking reforms substantially increased paternity leave take-up (Duvander and Johansson, 2012; Ekberg et al., 2013; Duvander and Johansson, 2014, 2015; Avdic and Karimi, 2018). By Corollary 2, this finding implies that households place a high valuation on a miscellaneous day.[29] This, in turn, has important implications for our analysis because, as we show in Section 3.4 below, a

[29] Intuitively, as we show in the Proof of Corollary 2, when earmarking induces a father to take an extra leave day (that he otherwise would not have taken), the household gains one miscellaneous day.

household's benefit from using a "Double Day" is *directly related to a household's valuation of a miscellaneous day.* Thus, Corollary 2 provides a theoretical link between existing studies on earmarking and the findings that we present in this paper. We explain this in detail below.

### 3.4 "Double Days" Reform

The "Double Days" reform relaxes the assumption that parents cannot take leave at the same time by allowing "double days." During the core period, parents can now take leave on the same day, using two units of leave. However, "double day" units do not count toward earmarked units.[30]

To capture the value of taking a double day, we introduce some additional notation. Let $B_{pp'}(t)$ capture the direct benefit of parent $p$ taking leave to join parent $p'$ at home on day $t$. Let $C_p(t)$ be the corresponding direct cost.

**Assumption 5** (Flexibility and the value of a "double day")**.** $B_{pp'}(t)$ *contains a stochastic element. The double-day decision can be made flexibly, at time* $t$*, when the daily realization of* $B_{pp'}(t)$ *is observed.*

In principle, $B_{pp'}(t)$ may encompass benefits to parent $p$ who takes the additional leave (e.g., joy of leisure or domestic work), benefits to parent $p'$ from having the second parent at home (e.g., help with household chores or emotional support), and benefits to the child from being home with two parents as opposed to one. We let this aggregate household benefit contain a stochastic element to capture the fact that it may be subject to domestic shocks that necessitate a flexible response. For example, 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.[31]

[30] This structure closely resembles Sweden's reform, which allowed the use of "double days" before the child's first birthday (and thus before the child is eligible for public childcare), and which did not allow for "double days" to count toward either parent's earmarked allowance.

[31] In principle, another example of a domestic shock that could affect $B_{pp'}(t)$ in this general set-up is child illness. However, since one parent is already at home during the core days—and thus able to flexibly respond to unexpected child health shocks by, for example, taking the infant to the doctor—the marginal value of the second parent also staying home in response to a child health shock is likely to be low. Consistent with this conjecture, we find no empirical evidence of effects of the "Double Days" reform on measures of child health available in our data (specialist outpatient and inpatient visits as well as prescription drugs like antibiotics).

Further, for simplicity, we assume that the number of potential double days to be used is strictly smaller than $T - E - \bar{t}$. This simplifies our analysis as it ensures that use of a double day will not preclude use of a later (desired) double day.[32]

**Prediction 1** (Double days)**.** *A double day is used if and only if*

$$B_{pp'}(t) > b_m + (1-\alpha)(w_p - w_{p'}). \tag{1}$$

*Proof.* See Appendix B. □

Prediction 1 contains two insights that are important for our empirical analysis. First, households choose to take a double day on days when the direct household benefit from parent $p$ joining $p'$ exceeds the threshold in (1). Thus, when parents have the flexibility to decide when to take joint leave on a day-to-day basis, the optimal response is to remove the additional parent from the labor force only on days when the benefit of doing so is perceived to be sufficiently high.

Second, the right-hand side of condition (1) formalizes the notion of "sufficiently high." Intuitively, a double day has a shadow cost beyond the foregone wage of parent $p$: it eliminates a future miscellaneous leave day that could be taken by mom.[33] This makes the overall opportunity cost of taking a double day potentially large. Specifically, for a double day taken by the dad to join the mom at home, condition (1) becomes

$$B_{dm}(t) > b_m + (1-\alpha)\Delta_w$$

where $\Delta_w = w_d - w_m > 0$ is the wage difference between the dad and the mom. That is, the added benefit of dad joining mom on a core day allocated to mom would have to exceed the

[32] This assumption is made for convenience and can be relaxed. If relaxed, the household will be more conservative in its use of a double day (relative to the case when this assumption holds); consequently, the right-hand side of equation (1) is the lower bound of the direct benefit that must be obtained from taking a double day.

[33] Corollary 1 and 2 together imply that any miscellaneous day taken by the father are taken in response to earmarking; thus, they count toward the father's earmarked allowance. Because double days do not count toward the earmarked allowance, a double day (taken by any parent) replaces a miscellaneous day taken by the mother in the future. See the Proof of Proposition 1 for a more formal treatment.

gross benefit of mom taking leave on a future miscellaneous day without childcare, plus the difference in the non-replaced wage income.[34]

Thus, the higher is the household's valuation of a future miscellaneous day, the higher is the cutoff in (1) at which the household decides to take a double day. Further, a higher cutoff in (1) implies fewer days taken as double days, and a higher perceived household benefit of each claimed double day. This relates to our above discussion of Corollary 2: The strong response in paternity leave take-up to Sweden's earlier earmarking reforms suggests that the value of a miscellaneous leave day is high. We thus obtain a clear prediction: the "Double Days" reform (i) induces a relatively small average increase in the number of double days taken, but (ii) ensures that the claimed double days are associated with substantial benefits to the household.

# 4 Data

Our empirical 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

[34] Similarly, for a double day in which the mom joins dad at home, condition (1) becomes $B_{md}(t) > b_m$ (without career costs). In practice, however, as illustrated in Appendix Figure A1, the typical mother's first spell extends beyond the time period when double days can be used.

frame, and to calculate the children's exact dates of birth using information on gestational age and expected due date.[35]

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

**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 from 2001 to 2016, and prescription drug records from 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)

[35] 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.

diagnosis codes, the length of stay (for inpatient data only), whether the visit originated in the emergency room, and whether the visit was planned (i.e., scheduled in advance) or unplanned (i.e., originated in the emergency room or due to a same-day appointment or an immediate referral from primary care). 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.[36] 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 neurodevelopmental disorders,[37] and (iii) external causes and medical counseling.[38]

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 C lists the exact ICD and ATC codes for all of our outcomes.[39]

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-

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

[37] Note that inpatient and outpatient visits with a mental health diagnosis are generally associated with severe and/or chronic mental illness. Milder or more temporary cases of mental health issues may instead show up in our data in the form of prescription drug treatment. To that point, one does not need to have a formal mental health diagnosis in order to be prescribed anti-anxiety or anti-depressant medications.

[38] 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).

[39] 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).

depressant prescription drug in months 1-36 before childbirth.[40]

**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.[41] 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 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 five 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

[40]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.

[41]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.

visit during the same time frame. Ten percent of mothers have an inpatient or outpatient visit for childbirth-related complications, two percent have a visit with a mental health diagnosis, while one percent have a visit for external causes or medical counseling. Consistent with the idea that one does not need to have a formal mental health diagnosis in order to be prescribed a mental health-related medication (see footnote 37), we observe that four percent of new mothers have an anti-anxiety or anti-depressant drug prescription, which is double the share of women with a diagnosis. Eight and 16 percent of new mothers 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:

$$\begin{aligned} y_{idp} &= \alpha + \beta_1 \mathbf{1}[d \geq c] + \beta_2 R_i \times \mathbf{1}[d \geq c] \\ &+ f(d-c) + \mathbf{1}[d \geq c] \times f(d-c) + \mathbf{x'}_i \kappa + \theta_p + \varepsilon_{idp} \end{aligned} \tag{2}$$

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, $\theta_p$.[42]

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. $\varepsilon_{idp}$ 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 $\beta_2$. It represents an estimate of the difference in parental outcomes between January-March and October-December births in the reform sample, relative to the 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

[42] 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.

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 (2) 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 1 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 A2 and A3 report results from estimating versions of model (2), 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 2 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 (2) 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.

To explore the impacts of the reform on the distribution of post-baseline leave days taken by fathers in the first 6 months post-childbirth, Figure 3 plots the RD-DD treatment coefficients and 95% confidence intervals from separate regression models that use as outcomes indicator variables for fathers taking different numbers of post-baseline leave days denoted in bins on the $x-$axis of each graph. We show results for the overall sample in sub-figure (a), and for families with mothers who have a medical history in sub-figure (b). Consistent with the estimates in Table 3, we observe significant extensive margin effects—in both samples, there are large reductions in the shares of fathers who take zero post-baseline leave days. In the overall sample, fathers are both more likely to take one to five days of leave and 11 or more days of leave.[43] In the sample of families with mothers who have a medical history, we only see statistically significant increases in the shares of fathers taking 11-20, 21-30 or 31+ days of post-baseline leave. Thus, it appears that the one to two day increase in the total number of leave days taken on average is driven both by some fathers being more likely to take a few days of leave and a (small) share of fathers—concentrated in families where mothers may be most prone to health problems—taking a more extended period of time off.

Importantly, our theoretical framework in Section 3 highlights that households may reap gains from a reform that grants flexibility in the use of simultaneous parental leave, even if fathers, *ex post*, end up shifting only a few extra days of leave to the immediate postpartum period. The availability of simultaneous 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 the impacts of such leave on maternal postpartum health.

[43] We see a statistically significant increase in the share of fathers taking 31 or more days of post-baseline leave. Recall that this is possible because while families are limited to at most 30 "double days" in the first year post-childbirth, fathers can take additional post-baseline leave days if mothers do not claim paid leave benefits on the same days.

**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.[44] 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 6, which present estimates from model (2) 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 6, 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 with mental health diagnoses (i.e., representing more chronic and/or severe mental health issues), driven by external causes, or for medical advice, or on the other prescription drugs that we consider.

Are the observed reductions in health care visits and prescription drugs consistent with an improvement in maternal health or do they instead reflect a (potentially welfare-reducing)

[44] 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.

decline in health care utilization? To shed light on this question, we analyze whether the effects on inpatient and outpatient visits are driven by those that are scheduled in advance or those that are unplanned (either because they originate in the emergency room or because they involve a same-day appointment or immediate referral from primary care) in Table 5. We find that the reduction in health care visits is entirely due to *unplanned* rather than scheduled appointments, suggesting that underlying maternal health becomes better as a result of the "Double Days" reform.

**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 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.[45]

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 A4 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 families who do and do not have at least one grandparent aged 74 years or less residing in the same county.[46] 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 A5, A6, and A7 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-

[45]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.

[46]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.

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.[47]

**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. We follow the RD methodological literature (Calonico et al., 2014b,a, 2018a,b), and use triangular kernels and robust bias-corrected inference procedures in all models.[48] In Panel A of each table, we show results that include the same vector of controls $\mathbf{x}_i$ as in model (2); while Panel B presents results without covariates.[49]

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

[47]When we examine at maternal anti-anxiety prescription drugs in the 90 days post-childbirth, we find a statistically significant decline among families with a grandparent in the same county. However, this is likely due to the larger sample size in that sub-sample than in the sub-sample of families without a grandparent. We cannot reject that the two coefficients are the same across the two groups.

[48]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. (2014b), 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.

[49]See Calonico et al. (2018b) for a discussion of RD models with and without covariates.

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 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 (2) 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 2 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 wellbeing 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; Chen et al., 2019). 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.[50] 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.*"[51]

[50] 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.

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

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 administrative data and quasi-experimental variation from a social insurance reform in January 2012, which granted fathers the flexibility to take 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 flexible leave in the postpartum period improves maternal 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 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 the design of 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.[52] 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.[53] While discus-

[52] 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).

[53] 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

sions about encouraging men to take paternity leave typically focus on policies that promote 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.

Finally, our results suggest that workplace flexibility for fathers may be a highly cost-effective way of improving maternal postpartum health, when compared with other public programs such as nurse home visiting. The "Double Days" reform does not change the total number of days of leave allocated to the household; rather, it grants parents agency to allocate their leave in a way that maximizes the household's benefits. The medical and psychological literature suggests that these benefits may be long-lasting—maternal postpartum health issues have important consequences for the mother's long-term wellbeing as well as the family's welfare overall (see Meltzer-Brody and Stuebe, 2014 and Saxbe et al., 2018 for some overviews). Thus, our finding of short-term benefits for maternal health may underestimate the total value of paternal access to workplace flexibility.

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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.

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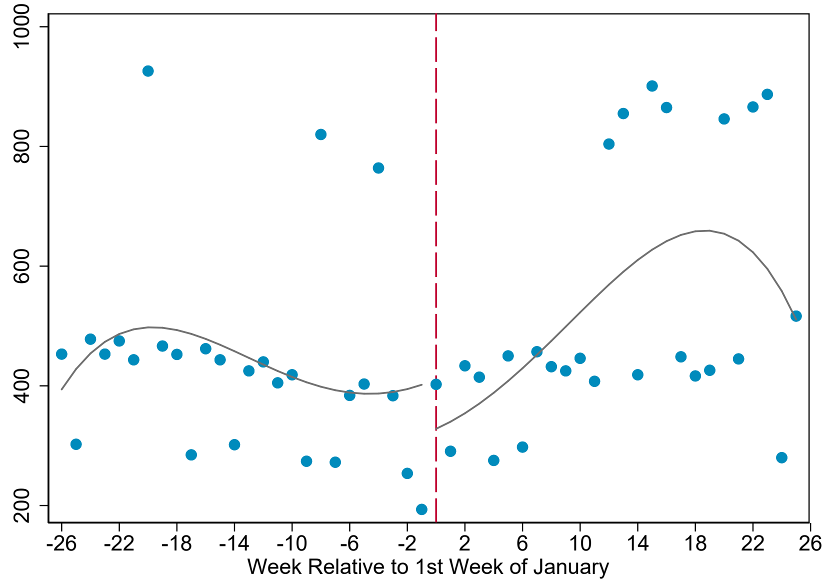
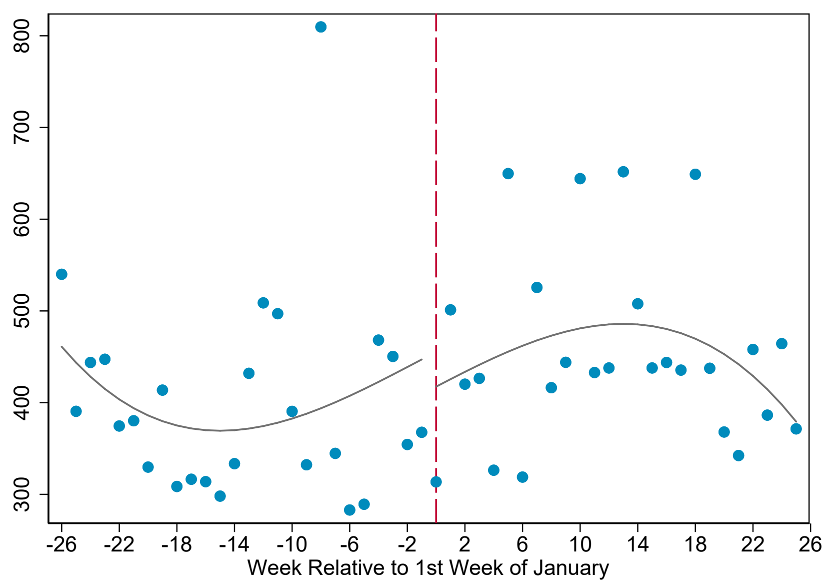
# 8 Figures

Figure 1: Number of Births by Birth Month in Reform and Non-Reform Samples

(a) Reform Sample



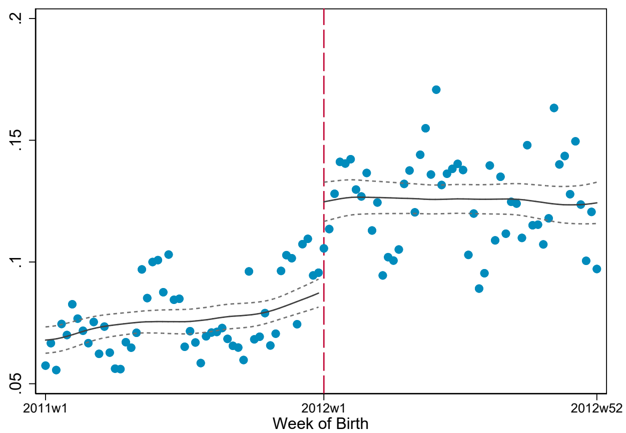

(b) Non-Reform Sample



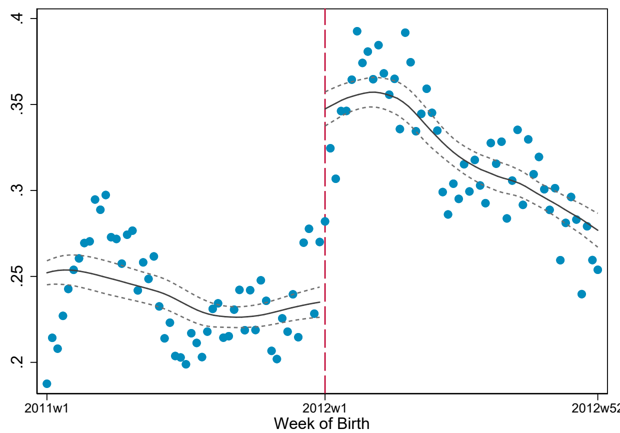

*Note:* The sample includes all firstborn singleton children born in 2008-2012 with information on exact date of birth. Sub-figure (a) plots the total number of births by birth week in the reform sample with a 6-month bandwidth (July 2011 - June 2012). Sub-figure (b) plots the average of the total number 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 2: Effects of 2012 "Double Days" Reform on Paternity Leave Take-Up

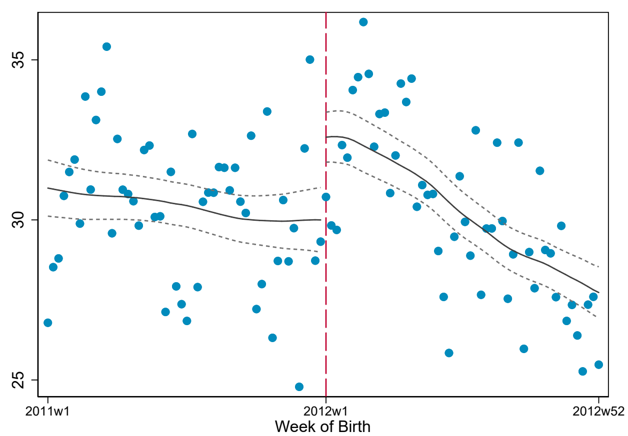
(a) Any Post-Baseline Leave in First 60 Days




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



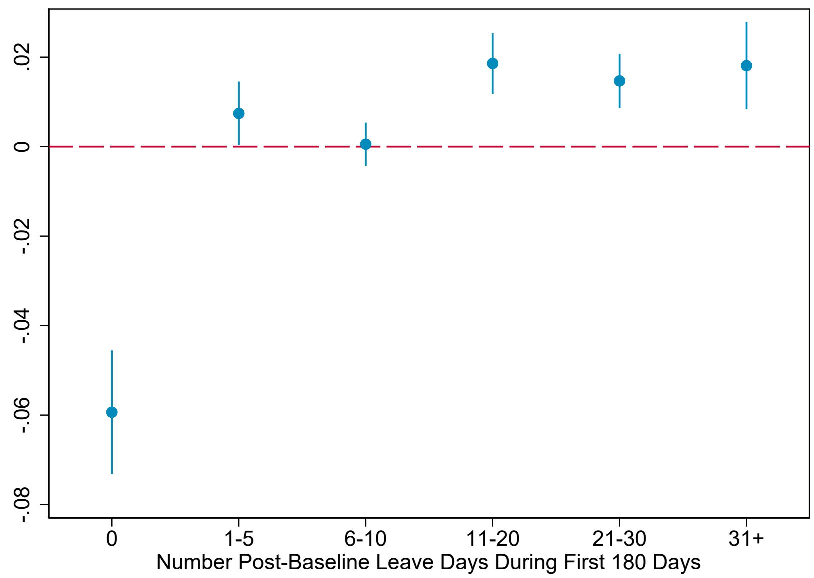

(c) Tot. # Leave Days in First 180 Days



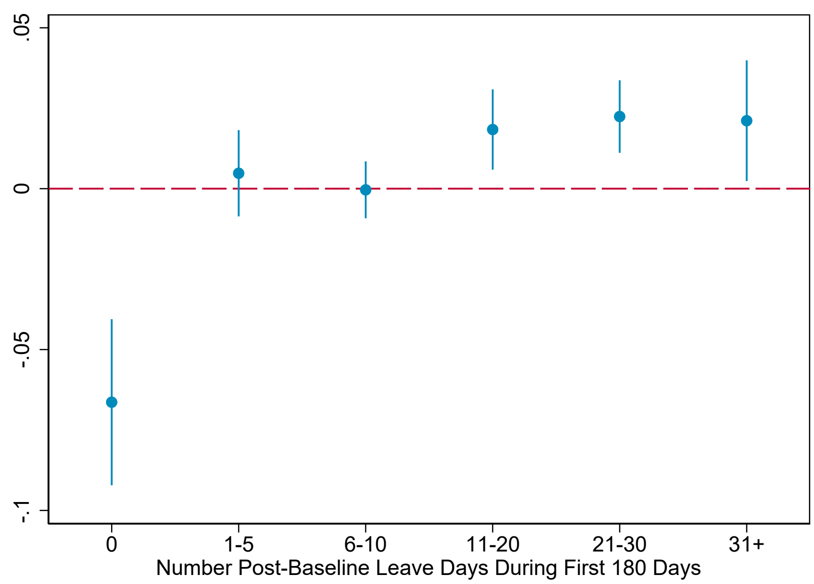

*Note:* The sample includes all firstborn singleton children born in 2011-2012 with information on exact date of birth. The figures display the means of outcome variables by the child's birth week. The 2012 reform is denoted with a vertical red dashed line. The fitted curves and 95% confidence intervals 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 3: Effect of 2012 "Double Days" Reform on Distribution of Post-Baseline Leave Days Taken by Fathers During First 180 Days

(a) RD-DD Sample (All Families)

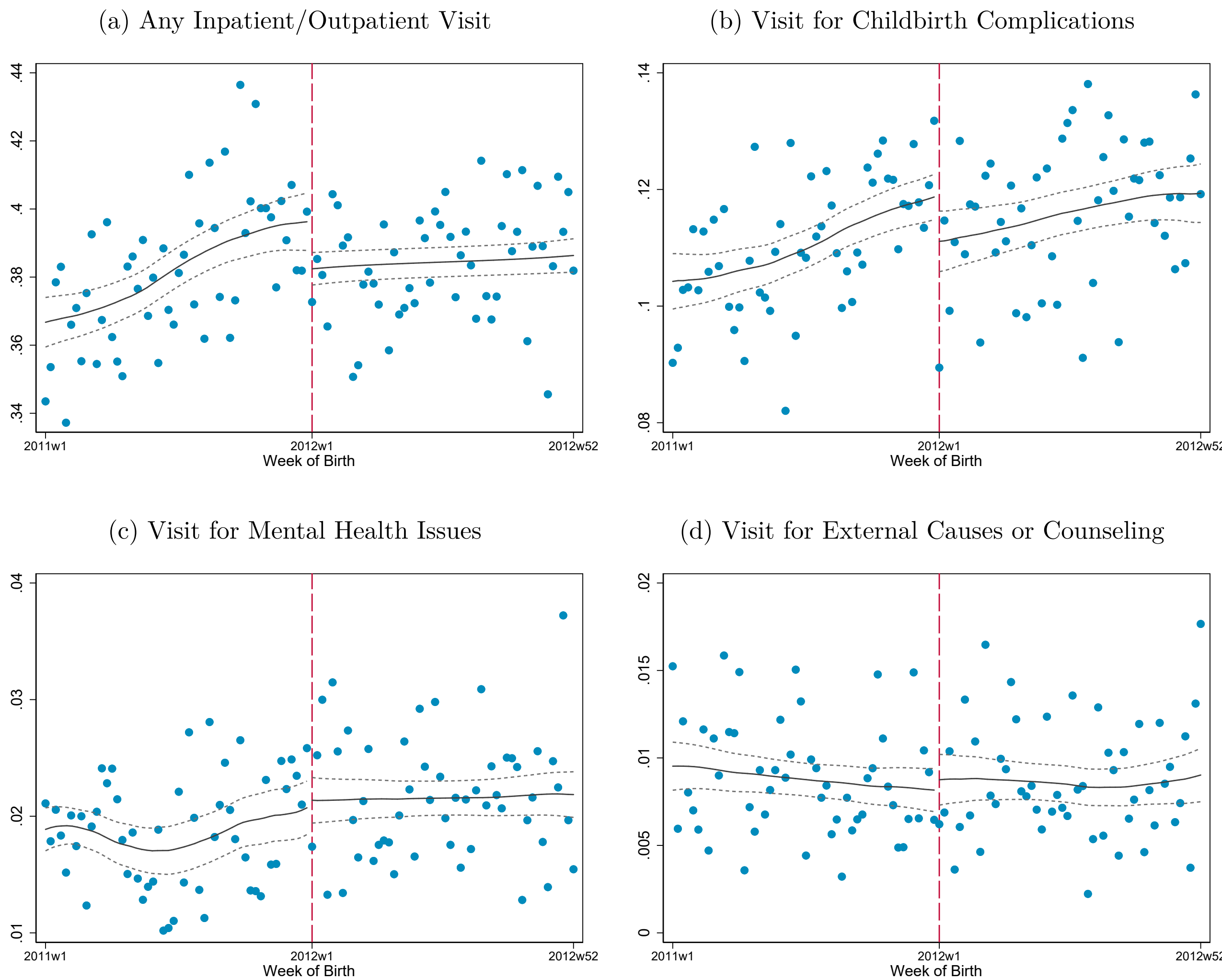



(b) Sample: Mothers with Medical History




*Note:* The figures plot the RD-DD treatment coefficients and 95% confidence intervals from separate regression models that use as the outcome an indicator for the father taking the number of post-baseline leave days denoted in bins on the $x-$axis of each graph. Sub-figure (a) uses our primary RD-DD analysis sample, while sub-figure (b) limits the RD-DD analysis sample to children of mothers who have a pre-birth medical history. See notes under Table 3 for more details on the specifications and controls.

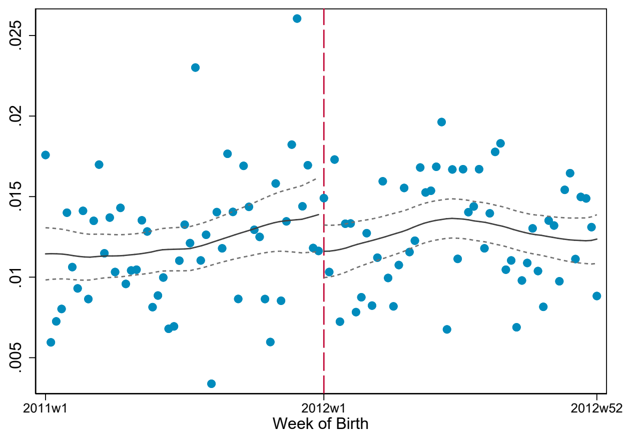
Figure 4: Effects of 2012 “Double Days” Reform on Maternal Health Outcomes in First 180 Days Post-Childbirth, Inpatient and Outpatient Data



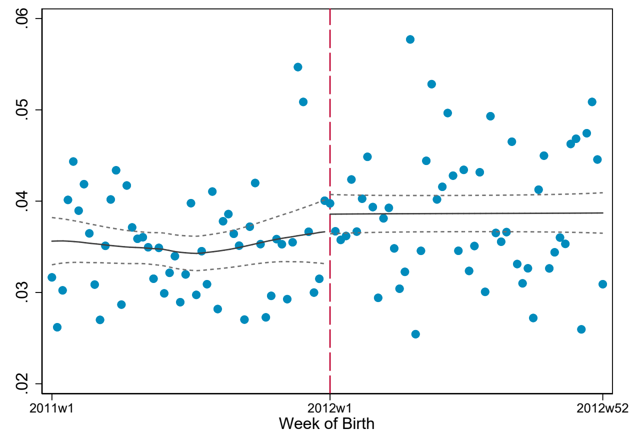

*Note:* The sample includes all firstborn singleton children born in 2011-2012 with information on exact date of birth. The figures display the means of outcome variables by the child’s birth week. The 2012 reform is denoted with a vertical red dashed line. The fitted curves and 95% confidence intervals 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 C 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

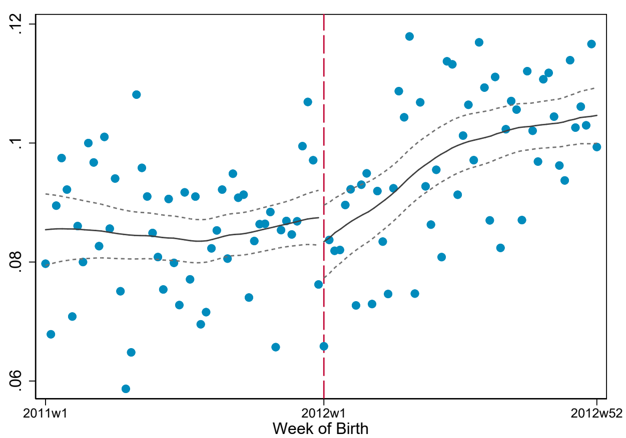
(a) Any Anti-Anxiety Drug



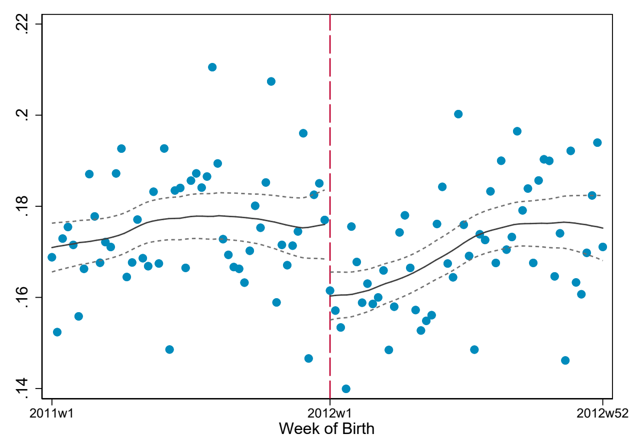

(b) Any Anti-Depressant Drug




(c) Any Painkiller Drug



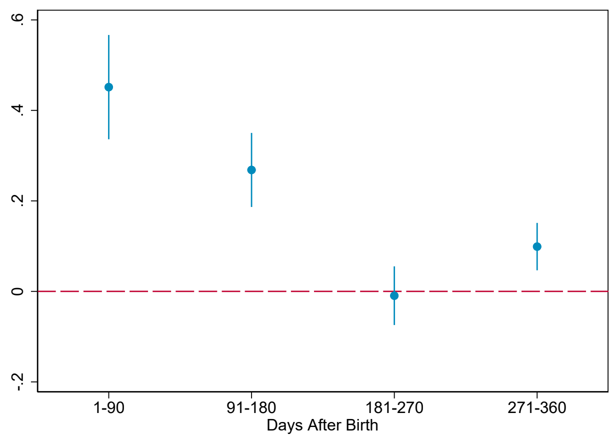

(d) Any Antibiotic Drug



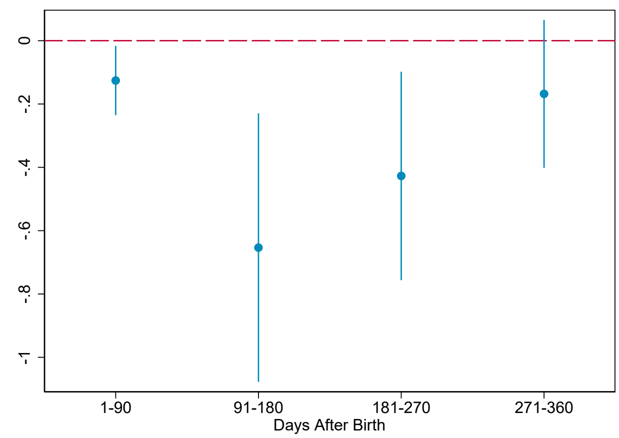

*Note:* The sample includes all firstborn singleton children born in 2011-2012 with information on exact date of birth. The figures display the means of outcome variables by the child's birth week. The 2012 reform is denoted with a vertical red dashed line. The fitted curves and 95% confidence intervals 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 C 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

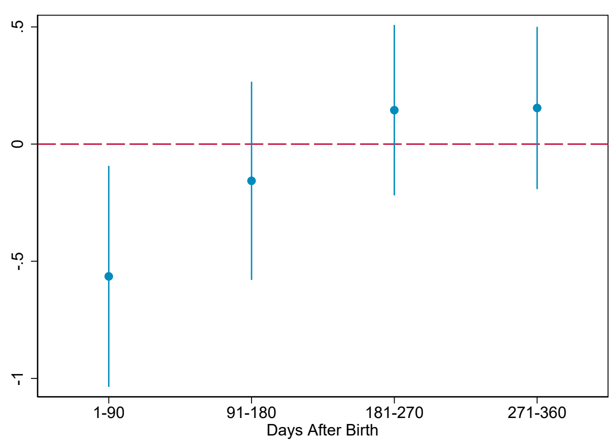
(a) Fathers: Any Post-Baseline Leave



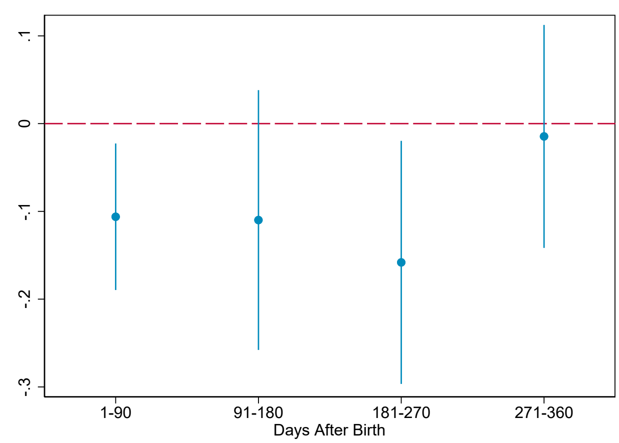

(b) Mothers: Visit for Childbirth Complications




(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 C 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 C 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

| | $1^{st}$ | $2^{nd}$ | $3^{rd}$ | $4^{th}$ | $5^{th}$ | $6^{th}$ |
|---|---|---|---|---|---|---|
| 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 (2) 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 indicators for: (1) 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 C 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 Inpatient and Outpatient Visits: Emergency, Planned, or Unplanned

| | Visit Type | | | | |
|---|---|---|---|---|---|
| | Emergency | Planned Inpatient | Unplanned Inpatient | Planned Outpatient | Unplanned Outpatient |
| **A. All first births** | | | | | |
| Reform × Birth Jan-Mar | -0.0000170 | -0.00157 | 0.00315 | -0.00376 | -0.0117** |
| | [0.00243] | [0.00138] | [0.00342] | [0.00715] | [0.00585] |
| Dep. var mean | 0.0210 | 0.00681 | 0.0423 | 0.259 | 0.154 |
| N | 82558 | 82558 | 82558 | 82558 | 82558 |
| **B. Mothers with medical history** | | | | | |
| Reform × Birth Jan-Mar | -0.00119 | -0.00258 | 0.00293 | 0.000133 | -0.0396*** |
| | [0.00519] | [0.00306] | [0.00722] | [0.0140] | [0.0117] |
| Dep. var mean | 0.0293 | 0.0109 | 0.0583 | 0.343 | 0.200 |
| N | 23935 | 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 visit that originates in the emergency room, (2) any planned (i.e., scheduled in advance) inpatient visit, (3) any unplanned inpatient visit (includes visits that originate in the emergency room and those that are same-day appointments or immediate referrals from primary or outpatient care), (4) any planned (i.e., scheduled in advance) specialist outpatient visit, (5) any unplanned specialist outpatient visit (including same-day appointments and immediate referrals from primary care). 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 C 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 6: Effects of “Double Days” Reform on Maternal Health Outcomes in Prescription Drug Data

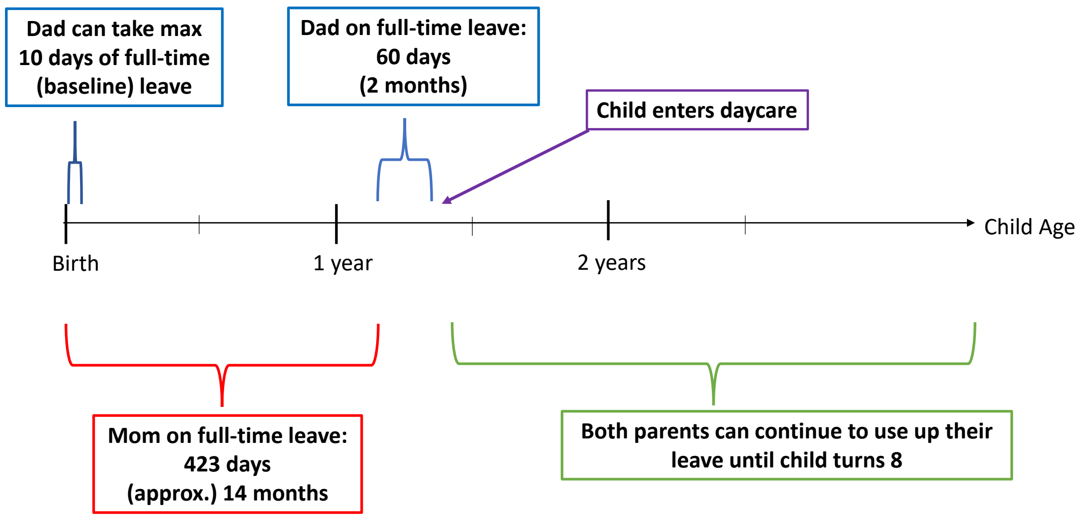
| | Any Anti-Anxiety | Any Anti-Depressant | Any Painkiller | Any Antibiotic |
|---|---|---|---|---|
| **A. All first births** | | | | |
| Reform × Birth Jan-Mar | -0.00290* | 0.000669 | -0.00461 | -0.0193*** |
| Mar | [0.00176] | [0.00299] | [0.00445] | [0.00602] |
| Dep. var mean | 0.0112 | 0.0338 | 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 C 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: Parental Sick Leave Use: Jan-Mar 2011 vs. Jan-Mar 2012 Births

| | Jan-Mar 2011 | Jan-Mar 2012 | P-value |
|---|---|---|---|
| **A. Fathers** | | | |
| Days of Sick Leave | 2.707 | 2.652 | 0.844 |
| Any Sick Leave | 0.045 | 0.043 | 0.543 |
| **B. Mothers** | | | |
| Days of Sick Leave | 6.181 | 6.619 | 0.132 |
| Any Sick Leave | 0.202 | 0.206 | 0.499 |
| Observations | 11353 | 11509 | |

Notes: This table reports the means of the annual number of sick leave days and the share of parents who use any sick leave for parents of firstborn singleton children born in January-March 2011 and January-March 2012. Panel A presents the statistics for fathers, while Panel B for mothers. The last column reports the $p$-values from testing the differences between the values in the previous two columns.

Table A2: The 2012 “Double Days” Reform and Parental Characteristics

| | (1) M. Low Ed | (2) F. Low Ed | (3) M. F-born | (4) F. F-born | (5) M. Age | (6) F. Age | (7) M. Inc | (8) 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 (2) 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: 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 (2) 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 A4: 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 A5: 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 A6: 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 A7: 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 $\times$ 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 $\times$ 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 A8: RD Estimates 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 |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. With Controls** | | | | | | | | | | |
| 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 |
| **B. No Controls** | | | | | | | | | | |
| RD Estimate | 0.0230*** | 0.0241*** | 0.0231*** | 0.0230*** | 0.0230*** | 0.0225*** | 0.0232*** | 0.0226*** | 0.0225*** | 0.0226*** |
| | [0.00615] | [0.00599] | [0.00614] | [0.00615] | [0.00615] | [0.00850] | [0.00830] | [0.00849] | [0.00850] | [0.00849] |
| Left BW | 209.7 | 208.7 | 210.4 | 209.7 | 209.7 | 110.7 | 110.2 | 111.0 | 110.7 | 110.7 |
| Right BW | 209.7 | 233.8 | 210.4 | 209.7 | 210.4 | 110.7 | 123.4 | 111.0 | 110.7 | 111.0 |
| Num. Obs. | 51591 | 54602 | 51831 | 51591 | 51717 | 26418 | 28060 | 26686 | 26418 | 26552 |

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 estimate an RD model with local linear polynomials, triangular kernels, and robust bias-corrected inference procedures, 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. Panel A includes the same controls as in Table 3, Panel B omits the controls. 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. (2014b), 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: RD Estimates 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 |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. With Controls** | | | | | | | | | | |
| 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 |
| **B. No Controls** | | | | | | | | | | |
| RD Estimate | 0.0930*** | 0.0838*** | 0.115*** | 0.0930*** | 0.101*** | 0.0448*** | 0.0404*** | 0.0698*** | 0.0448*** | 0.0536*** |
| | [0.00914] | [0.00919] | [0.00772] | [0.00914] | [0.00844] | [0.0127] | [0.0127] | [0.0107] | [0.0127] | [0.0117] |
| Left BW | 198.4 | 286.9 | 275.8 | 198.4 | 275.8 | 104.7 | 151.4 | 145.6 | 104.7 | 145.6 |
| Right BW | 198.4 | 146.7 | 275.8 | 198.4 | 198.4 | 104.7 | 77.43 | 145.6 | 104.7 | 104.7 |
| Num. Obs. | 48805 | 53173 | 68243 | 48805 | 58348 | 24857 | 27728 | 35251 | 24857 | 30177 |

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 estimate an RD model with local linear polynomials, triangular kernels, and robust bias-corrected inference procedures, 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. Panel A includes the same controls as in Table 3, Panel B omits the controls. 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. (2014b), 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: RD Estimates 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 |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. With Controls** | | | | | | | | | | |
| 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 |
| **B. No Controls** | | | | | | | | | | |
| RD Estimate | -0.0142*** | -0.0156*** | -0.0142*** | -0.0142*** | -0.0142*** | -0.0167** | -0.0181** | -0.0167** | -0.0167** | -0.0167** |
| | [0.00474] | [0.00520] | [0.00477] | [0.00477] | [0.00477] | [0.00657] | [0.00721] | [0.00661] | [0.00661] | [0.00661] |
| Left BW | 366.8 | 309.5 | 361.9 | 361.9 | 361.9 | 193.6 | 163.4 | 191.0 | 191.0 | 191.0 |
| Right BW | 366.8 | 300.8 | 361.9 | 361.9 | 361.9 | 193.6 | 158.7 | 191.0 | 191.0 | 191.0 |
| Num. Obs. | 89894 | 75504 | 88824 | 88824 | 88824 | 47593 | 39203 | 46837 | 46837 | 46837 |

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 estimate an RD model with local linear polynomials, triangular kernels, and robust bias-corrected inference procedures, 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. Panel A includes the same controls as in Table 3, Panel B omits the controls. 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. (2014b), 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: RD Estimates 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 |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. With Controls** | | | | | | | | | | |
| 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 |
| **B. No Controls** | | | | | | | | | | |
| RD Estimate | -0.0202*** | -0.0212*** | -0.0196*** | -0.0196*** | -0.0201*** | -0.0192** | -0.0180** | -0.0206** | -0.0206** | -0.0191** |
| | [0.00595] | [0.00585] | [0.00648] | [0.00648] | [0.00618] | [0.00827] | [0.00813] | [0.00902] | [0.00902] | [0.00860] |
| Left BW | 320.0 | 443.3 | 271.4 | 271.4 | 320.0 | 168.9 | 234.0 | 143.3 | 143.3 | 168.9 |
| Right BW | 320.0 | 249.7 | 271.4 | 271.4 | 271.4 | 168.9 | 131.8 | 143.3 | 143.3 | 143.3 |
| Num. Obs. | 78997 | 85161 | 67229 | 67229 | 73159 | 41129 | 44838 | 34757 | 34757 | 38046 |

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 estimate an RD model with local linear polynomials, triangular kernels, and robust bias-corrected inference procedures, 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. Panel A includes the same controls as in Table 3, Panel B omits the controls. 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. (2014b), 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: RD Estimates 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 |
|---|---|---|---|---|---|---|---|---|---|---|
| **A. With Controls** | | | | | | | | | | |
| 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 |
| **B. No Controls** | | | | | | | | | | |
| RD Estimate | -0.00243** | -0.00356*** | -0.00335*** | -0.00335*** | -0.00335*** | -0.00438*** | -0.00422** | -0.00440** | -0.00440** | -0.00440** |
| | [0.00108] | [0.00130] | [0.00124] | [0.00124] | [0.00124] | [0.00153] | [0.00183] | [0.00176] | [0.00176] | [0.00176] |
| Left BW | 501.6 | 374.7 | 387.4 | 387.4 | 387.4 | 264.7 | 197.8 | 204.5 | 204.5 | 204.5 |
| Right BW | 501.6 | 332.1 | 387.4 | 387.4 | 387.4 | 264.7 | 175.3 | 204.5 | 204.5 | 204.5 |
| Num. Obs. | 123174 | 87107 | 94972 | 94972 | 94972 | 65445 | 45697 | 50278 | 50278 | 50278 |

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 estimate an RD model with local linear polynomials, triangular kernels, and robust bias-corrected inference procedures, 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. Panel A includes the same controls as in Table 3, Panel B omits the controls. 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. (2014b), 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 | 0.0527*** | 0.0840*** | 2.632*** | -0.0149** | -0.0195*** | -0.00268* |
| in Days 1-60 Post-Birth | [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 | 0.128*** | 0.202*** | 6.414*** | -0.0388** | -0.0519*** | -0.00607 |
| in Days 1-180 Post-Birth | [0.0133] | [0.0207] | [2.444] | [0.0153] | [0.0182] | [0.00387] |
| **C. Drop December Births (N=69953)** | | | | | | |
| Reform × 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 Mathematical Proofs

## B.1 Proof of Corollary 1

First, we show that the dad under the "basic parental leave system" does not take leave on any miscellaneous days, but may take leave on core days. Under the assumptions in Section 3.2, we have that $\Delta_c(t) = c_m(t) - c_d(t) < 0$ while $\Delta_b \geq 0$; thus, if a miscellaneous leave day is taken, then it is taken by mom. Under the assumptions in Section 3.2, we also have that $\Delta_C(t) = C_m(t) - C_d(t)$ can be positive on days when mom would incur a career cost; thus, dad may take leave on core days when this allows the household to avoid the maternal career cost.

Second, we show that it is optimal for the household to claim leave during the entire core period. By Assumption 1, it is generally optimal to fill up core days before allocating leave to miscellaneous days. While the career cost can make taking more than $\tau^c$ of core leave days by one parent expensive, the family as a whole would always find it optimal to cover any remaining core days using the other parent (rather than have no one stay at home). This follows from the following two observations: (i) Mom and dad can allocate leave between them in a way that enables them to cover core days without incurring any career costs ($\frac{\bar{t}}{2} < \tau^c < \bar{t}$). (ii) Absent career costs, the household strictly prefers to take leave during a core day over not taking leave ($B_p - (1-\alpha)w_p > b_p - (1-\alpha)w_p > 0$).

Third, we show that, if dad takes leave, then it is taken as a single interval of leave days at the end of the core period. Within the core period, it follows directly from Assumptions 2 and 3 that it is optimal to allocate at least $\tau^c$ of core leave days to mom. If $(1-\alpha)w_m + \kappa - (1-\alpha)w_d \equiv \Delta_C^c > 0$, then it is potentially optimal to allocate some core leave days to dad.

- Specifically, on core days where $\Delta_B - \Delta_C^c < 0$, dad takes leave.
- Given $\Delta_C^c$, the left-hand side is smaller for higher $t$, because $\Delta_B$ is smaller for higher $t$ by Assumption 2. Hence, if dad takes any leave days, those will form a single interval at the end of the core period.

Fourth, we show that, once the core period is accounted for, any remaining leave days will be taken as miscellaneous days (by mom, as per the first argument in this proof). Because

$b_m - (1-\alpha)w_m > 0$, the household prefers to use any miscellaneous day over not using it.

## B.2 Proof of Corollary 2

First, for the $T - E$ days that mom can use without any impact on the total allowance, the same arguments apply as under the basic parental leave system (see proof of Corollary 1 in Section B.1 above). Given that $T - E > \bar{t}$, the above arguments imply that the core period will be covered under any allocation of leave in the presence of earmarking.

Second, the residual question is what the household does with the $E$ days earmarked for dad. If dad takes more than $E$ days under the basic parental leave system, then the earmarking reform does not affect the household's allocation of leave (described in Corollary 1). We thus henceforth focus on the case in which dad takes less than $E$ leave days under the basic system. It is useful to note that, in this case, if dad had to take more leave days, then he would optimally take those extra days either during the miscellaneous period (because the benefit differential is smallest there, $\Delta_b \leq \Delta_B$), or towards the end of the core period (where, while the differential may be larger, he can reduce career costs for the mom).

Third, we show that if dad takes less than $E$ leave days under the basic system, then the earmarking reform will strengthen his incentives to take more leave days. This is because the earmarking reform raises the household's opportunity cost of dad not taking a day of leave (up to $E$ days): under the basic system, mom can take the day of leave instead; under earmarking, the household loses the leave benefit on that day. To see this, consider the following:

- Under the basic system, suppose dad considers taking a leave day. Since under the basic system, all $T$ days are always used, this would effectively replace mom on that leave day who would have taken that leave day otherwise. If the candidate day is a late-period core day, then the marginal value of dad replacing mom on that day is

  $$\Delta_B - \Delta_C^c,$$

  and if the candidate day is a miscellaneous day, then the marginal value is

  $$\Delta_b - \Delta_c.$$

- Now, suppose dad considers using an *earmarked* day to replace mom on the above candidate days. Because he uses an earmarked day, the family allowance effectively grows; that is, mom being replaced on that day means that she can allocate the "freed up" allowance to another miscellaneous day (all core days are filled). So, the marginal benefit of dad using an earmarked day to replace mom on a late-period core day is

$$\Delta_B - \Delta_C^c + [b_m(t) - (1-\alpha)w_m],$$

and to replace mom on a miscellaneous leave day is

$$\Delta_b - \Delta_c + [b_m(t) - (1-\alpha)w_m].$$

When comparing these to the analogous conditions under the basic system, we see that the term $[b_m(t) - (1-\alpha)w_m]$ is the added incentive that earmarking creates for dads to take more leave: the value of an additional miscellaneous leave day taken by mom.

## B.3 Proof of Prediction 1

First, we show that the use of a double day always reduces the number of miscellaneous leave days. Recall that, under any allocation, the core period will be fully covered. Hence, if the use of double days reduces the total number of covered days, then the reduction will always come out of the set of miscellaneous days.

Second, it is useful to note the following on the take-up of miscellaneous days: Because $\Delta_b - \Delta_c < 0$, non-earmarked miscellaneous leave days are not taken by dad. Thus, any miscellaneous leave days taken by dad are earmarked for dad. All other miscellaneous leave days are taken by mom.

Third, we show that when a double day is taken, then it replaces one of mom's miscellaneous leave days.

- When all miscellaneous leave days are taken by mom, the use of a double day will replace one of mom's miscellaneous leave days.
- When some miscellaneous leave days are taken by dads, the use of a double day will

(still) replace one of mom's miscellaneous leave days. This is because double days cannot be counted against earmarked days; hence, if a double day is used, eliminating a dad's miscellaneous leave day (which, by step 2 of this argument, is an earmarked day) does not prevent that a mom-only miscellaneous leave day is taken away. To see this, let $\hat{T}$ denote the total number of leave units taken, some possibly already on double days. Suppose $T - E < \hat{T} \leq T$, i.e., dad uses some but not more than his earmarked days (this is the necessary condition for dad to take miscellaneous leave days). Now suppose that the family decides to take another double day. To do this, the use of a unit of leave on another day must be eliminated. One could eliminate the use of another unit earmarked for dad, but this would reduce the number of allowed units $\hat{T}$ by one unit, so that the need to eliminate another, non-earmarked, unit in response to the added double day remains. As per previous arguments, if a non-earmarked unit must be eliminated and dad only uses earmarked days, then it is optimal to eliminate one of mom's miscellaneous leave days (rather than one of mom's core days).

Fourth, by the preceding arguments, a double day is taken when the value of "doubling up" exceeds the loss of a mom's miscellaneous leave day, i.e., $B_{pp'}(t) - (1-\alpha)w_p > b_m - (1-\alpha)w_m$.

# C 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"