When Dad Stays Home: Paternity Leave and Maternal Health *

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July 23, 2018

PRELIMINARY AND INCOMPLETE

Abstract

This paper studies how increased father presence in the household shortly after childbirth affects maternal postpartum health. Using variation from Sweden's two most recent "Daddy Month" reforms, which extended earmarked parental leave for fathers, we show that paternity leave reduces maternal inpatient and specialist outpatient visits in the months following childbirth. We also find a reduction in the consumption of prescription painkillers among mothers who had labor or delivery complications, and suggestive evidence for a decrease in anti-depressant use among mothers with pregnancy risk factors. Our results suggest that father presence in the household could be critical for maternal postpartum recovery and health. They further imply that simply making paternity leave available may not be sufficient for households to reap these gains, and that policies that constrain households' leave-taking choice sets may raise welfare.

*We thank Marcella Alsan, Claudia Goldin, Caroline Hoxby, and Isaac Sorkin for helpful comments. We are grateful to Lindsey Uniat for excellent research assistance.

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1 Introduction

A society's development is often measured by its ability to keep babies alive and healthy in the first year of life. A wealth of research has been dedicated to exploring 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), with evidence suggesting marked improvements in infant health over the last several decades in the United States (Currie and Schwandt, 2016a,b) and around the world.[1] Yet new mothers—whose postpartum health is an important determinant of child development (see, e.g., Cheng et al., 2006; Stewart, 2007; Kingston and Tough, 2014 for some overviews)—have received comparably less attention in these discussions.

Understanding the causal drivers of maternal postpartum health is important in light of the high prevalence of a variety of physical and mental conditions. Studies from multiple countries document that between 23 and 83 percent of new mothers experience pain in various parts of their bodies (including the perineum, cesarean-section incisions, the back or the head) in the months following childbirth, and that the majority of mothers report substantial fatigue up to one year after childbirth (see Cheng et al., 2006 and the citations therein). Postpartum mental health issues are also common, as women undergo a significant transition in their identity and experience a variety of hormonal changes. Recent estimates suggest that about one in nine women in the United States report symptoms of postpartum depression (Ko et al., 2017). In Sweden, the setting for our paper, around 11 and 14 percent of new mothers are found to have depressive symptoms based on the Edinburgh Postnatal Depression Scale (EPDS) at two months and one year post-childbirth, respectively (Rubertsson et al., 2005), while about 0.6 percent are hospitalized specifically for the condition (Silverman et al., 2017). Maternal death after childbirth—although certainly rare when compared to just a century ago (Centers for Disease Control and Prevention, 1999)—is nevertheless existent even in high-income countries, with the United States witnessing an alarming increasing trend over the last twenty five years (Kassebaum et al., 2016).

While discussions about maternal health often center around the role of the healthcare

[1] According to the World Health Organization, the global infant mortality rate has decreased from 64.8 deaths per 1,000 live births in 1990 to 30.5 deaths per 1,000 live births in 2016. See: `http://www.who.int/gho/child_health/mortality/neonatal_infant_text/en/`.

system,[2] we instead focus on the mother's postpartum environment *at home*. Specifically, we ask whether the presence of the child's father in the home affects maternal physical and mental health. To study this question, we take advantage of linked administrative population-level data from Sweden together with quasi-experimental variation from the implementation of the two most recent "Daddy Month" reforms in 2002 and 2016. These reforms expanded upon the original 1995 "Daddy Month" policy, which earmarked one month of parental leave specifically to fathers. The 2002 and 2016 reforms extended this dedicated paternity leave to two and three months, respectively.

Increased father presence in the home could impact maternal health in a number of ways. Fathers on paternity leave may be more likely to share in childcare duties, enabling postpartum women to get more rest and supporting their physical recovery from childbirth. This channel could be especially important for women who have complications during labor or delivery. Fathers may also play a role in reducing maternal loneliness, fatigue, and stress, which have all been found to be related to the onset of postpartum depression and anxiety symptoms (Chan et al., 2002; Honey et al., 2003; Corwin et al., 2005). Indeed, correlational evidence suggests that stress and lack of social support account for a significant share of the positive association between single-mother status and postpartum depression (Cairney et al., 2003), implying a potentially important role for fathers in improving maternal mental health.

Alternatively, paternity leave could negatively affect maternal well-being, especially if the quality of the parental relationship is low. Fatigue and stress associated with having a newborn may exacerbate conflict between parents. Studies show that intimate partner violence (IPV) and psychological abuse can escalate during pregnancy and the postpartum period (Cheng and Horon, 2010; Brownridge et al., 2011), leading to maternal physical injuries and mental health problems (Romito et al., 2009).

To provide a comprehensive analysis of maternal health, we leverage administrative in-patient, outpatient, and prescription drug register data. The outpatient and prescription drug data have only been collected in Sweden since the 2000s, thus motivating our focus on the two most recent "Daddy Month" reforms rather than the original 1995 policy. We

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

use data on mothers with first births of singleton children in the months surrounding the two reforms, and implement a Regression Discontinuity Difference-in-Differences (RD-DD) research design. Our preferred specifications compare the outcomes of parents of children born in the 3 months before and after each reform, relative to the analogous difference in either the preceding or following year. Our empirical strategy thus exploits the sharp change in paternity leave eligibility for fathers of children born shortly after each reform, while differencing out other sources of variation in family outcomes between October-December and January-March births (e.g., due to seasonality or school starting-age laws, see Buckles and Hungerman, 2008; Currie and Schwandt, 2013; Black et al., 2011).

For the analysis of the 2002 reform, we are able to link our births to data on parental leave claims that are currently available to us through 2010, and show that it increases the average duration of paternity leave—which can be taken in multiple spells until the child turns 8 years old—by approximately 19 days. This result is consistent with prior studies of this reform (Duvander and Johansson, 2012, 2014; Avdic and Karimi, 2018). More relevant to our focus on the postpartum period, however, we also provide novel evidence on take-up on the extensive margin and on the timing of leave: We observe a 1.7 percentage point (2 percent at the sample mean) rise in the take-up of any paternity leave, a 2.9 percentage point (5 percent) increase in paternity leave use within the first year after childbirth, and a 2.3 percentage point (31 percent) increase in paternity leave use in the month of or the month following childbirth. These estimates show that the 2002 reform increased paternal presence in the home both immediately after childbirth—when the mother is recovering—and during the child's first year of life, which corresponds to the time period during which most of maternity leave is taken.[3]

Across both reforms, we find evidence that extended earmarked paternity leave reduces mothers' interactions with the non-primary-care healthcare system for reasons due to physical complications after childbirth and those plausibly related to mental or emotional well-being. We document that the 2002 "Daddy Month" leads to a 3.2 percentage point (38 percent) reduction in the likelihood of the mother being admitted to the hospital in the year after

[3]We are in the process of obtaining the most recent parental leave claims data to study the effects of the 2016 reform on paternity leave take-up, duration, and timing. To the best of our knowledge, no prior studies have examined the impacts of the 2016 reform.

childbirth, driven by a 0.9 percentage point (22 percent) decrease in inpatient visits for conditions related to pregnancy, childbirth, or the puerperium period and a 1.9 percentage point (158 percent) decline in visits for medical advice or counseling.[4] We also see an analogous 1.9 percentage point (186 percent) reduction in outpatient visits for the purposes of counseling or medical advice. Interestingly, while the impacts on counseling-related inpatient and outpatient visits are present across multiple sub-groups of mothers, the reduction in hospitalizations for childbirth-related problems is only apparent for mothers who experienced labor or delivery complications. Moreover, when we examine prescription drug use measured 5 to 7 years post-childbirth (the time horizon over which we observe all mothers in the 2002 reform analysis sample), we see a 2.3 percentage point (13 percent) decline in the consumption of painkillers for this sub-group. Since data show that among mothers who consume at least one prescription painkiller in the year after childbirth, 40 percent also take one 5 to 7 years later, our result is consistent with a possible reduction in the severity or duration of pain in the postpartum period.

However, while the average effect of the 2002 reform seems beneficial for maternal health, we do observe one potentially adverse impact for mothers with risky prenatal behaviors (e.g., those who smoked during pregnancy): For this sub-group, we find an increase in outpatient visits with mental health related diagnoses. This result is consistent with the possibility that paternity leave may exacerbate family conflict or stress, especially among disadvantaged families.

When we study the 2016 "Daddy Month" reform, we observe a 2.0 percentage point (31 percent) decline in hospital admissions in the 6 months post-childbirth, driven by a 2.0 percentage point (162 percent) reduction in visits with codes associated with some type of counseling. We also observe marginally significant declines in hospitalizations for childbirth-related complications for mothers with pregnancy risk factors (e.g., diabetes or hypertension). Analysis of outpatient data points to a 4.7 percentage point (10 percent) reduction in non-primary-care visits in the 6 months following childbirth, driven by 3.8, 0.6, and 1.7

[4]Specifically, we see a reduction in visits with ICD-10 codes that begin with the letter *Z*, which are described as "Factors influencing health status and contact with health services." As we discuss further in Section 5, these codes are typically used in circumstances when there is a health encounter for an issue that does not have a specific diagnosis, but may involve counseling or medical advice.

percentage point (24, 25, and 9 percent) declines in visits associated with childbirth-related complications, mental health diagnoses, and counseling, respectively. Lastly, we observe a 2.5 percentage point (11 percent) decline in the use of painkillers among mothers with labor or delivery complications, as well as a marginally significant reduction in anti-depressant use among mothers with pregnancy risk factors.

Our paper contributes to a growing literature on the consequences of "Daddy Month"-type reforms across several countries, including Sweden (Duvander and Johansson, 2012; Ekberg et al., 2013; Duvander and Johansson, 2014, 2015; Avdic and Karimi, 2018), Norway (Rege and Solli, 2013; Dahl et al., 2014; Cools et al., 2015; Dahl et al., 2016), Iceland (Eydal and Gislason, 2008), Germany (Schober, 2014; Bünning, 2015), Canada (Patnaik, 2016), and Spain (Luna and Farré, 2017). By encouraging fathers to take paternity leave, these policies aim to reduce gender inequalities in both the household and the labor market. Sweden is commonly seen as having one of the most gender egalitarian parental leave policies among high-income countries (Ray et al., 2010). But evidence on the impacts of Swedish "Daddy Month" reforms on household and labor market outcomes has been mixed. 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).[5] In fact, Avdic and Karimi (2018) actually find evidence that the original 1995 Swedish "Daddy Month" reform raised the probability of parental separation between one and two years after childbirth, driven by parents who would have otherwise separated a few years later. If increased paternal time at home intensifies conflict in the family that eventually causes parents to separate, then we may expect minimal or even adverse effects of the original reform on maternal health. By contrast, Avdic and Karimi (2018) report no effects of the 2002 reform on the likelihood or timing of parental separation, implying that paternity leave can have positive impacts on maternal health among couples

[5]In particular, Ekberg et al. (2013) study the first 1995 Swedish "Daddy Month" reform with a regression discontinuity design, finding no effect on the division of leave taken to care for sick children or fathers' and mothers' long-term wages and employment. Interestingly, Duvander and Johansson (2015) show some evidence that mothers actually reduce their sick day usage following the "Daddy Month" reform, but this reduction is not offset by any increases among fathers.

where conflict is not as prevalent.

We also build on some evidence that *maternity* leave benefits are associated with improvements in mothers' health outcomes (Hyde et al., 1995; Staehelin et al., 2007; Chatterji and Markowitz, 2012; Aitken et al., 2015; Avendano et al., 2015; Butikofer et al., 2017; Hewitt et al., 2017; Heymann et al., 2017; Jou et al., 2018). To the best of our knowledge, our paper provides the first comprehensive quasi-experimental analysis of the impacts of *paternity* leave on maternal mental and physical health.[6] While much of the discourse about incentivizing men to take paternity leave is rooted in a desire to change gender norms, our study demonstrates a more immediate possible benefit: Encouraging fathers to take leave, especially shortly after childbirth, can be instrumental for maternal postpartum recovery and health.

More broadly, our research has implications for the design of paid family leave (PFL) policies. In the presence of stigma associated with paternity leave use, simply making leave available to fathers may not be enough to promote its use or reap its benefits. Then, policies that go further—and explicitly constrain households' leave-taking choice sets—may potentially be welfare-improving.

2 Institutional Setting

Sweden implemented its gender-neutral paid parental leave policy in 1974, which replaced the previous maternity leave system that only covered mothers. The program is largely funded through employer social security contributions. In the 1990s, the program featured a per-child benefit of 12 months of wage-replaced leave, as well as an additional 3 months

[6] 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 "Daddy Month" reforms in an RD-DD design.

of leave with a low flat-rate benefit.[7] During the time period covered in our analysis, the replacement rate has been 75 to 90 percent of prior gross earnings, up to a ceiling.[8] 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.[9] Moreover, the benefits can be claimed on a part-time basis.[10]

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.[11] Both parents can take full-time leave at the same time for a maximum of 30 days, and this joint leave must be taken during the child's first year of life. Additionally, both parents can be simultaneously on leave part-time while also working part-time, as long as the total amount of leave claimed by the two parents does not exceed the equivalent of a full-time job.

Important to our setting, there is one exception to the rule about parents not being able to take full-time leave jointly for more than 30 days. During our entire sample period, fathers have been entitled to ten extra days of wage-replaced leave that can only be taken during the first 60 days of the child's life.[12] These days can be taken as the same time as the mother claims full-time parental leave.

"Daddy Month" reforms. In the original Swedish parental leave system, the 12 months of wage-replaced leave and 3 months of flat-rate leave were completely transferrable between

[7] In the 1990s, the flat-rate benefit was 60 SEK per day (Ekberg et al., 2013). By the mid-2000s, the flat-rate increased to 180 SEK per day, and in 2016 it was 250 SEK per day (Duvander et al., 2017).

[8] 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 (Duvander et al., 2017). Individuals who do not meet this employment requirement receive the lower flat-rate benefit only.

[9] 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. In practice, the vast majority of all leave days are taken before a child's second birthday (Duvander et al., 2017).

[10] 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).

[11] 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.

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

parents, i.e., they could share the leave in any way they wanted. In practice, the vast majority of the leave was taken by mothers: 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. In an effort to promote a more gender-equitable division of parental leave, the Swedish government introduced a reform that earmarked one month of the wage-replaced leave to each parent. In other words, the family would lose one month of wage-replaced paid leave if either parent fails to take his/her earmarked leave. Since virtually all mothers were already taking more than one month of leave, this reform was in actuality only binding for fathers (and therefore colloquially referred to as the "Daddy Month" reform). The reform applied to all fathers of children born on January 1, 1995 or later.

The second reform went into effect on January 1, 2002, earmarking a second month of wage-replaced leave to each parent. At the same time, the total number of months of wage-replaced leave was increased from 12 to 13 months. The third reform went into effect on January 1, 2016, earmarking a third month of wage-replaced leave to each parent, but leaving the total number of months of parental leave unchanged (at 13 months of wage-replaced leave and 3 months of flat-rate leave).

Thus, the first and third reforms simply imposed constraints on families, which may or may not have been binding depending on the division of parental leave that the parents would have chosen in the absence of the reforms. The second reform not only imposed a constraint, but also "compensated" families by adding one more month of wage-replaced leave. This means that after the second reform, even if the added constraint were binding for fathers, mothers could still take the same amount of leave as they would have without the reform (of course, a mother's optimal duration of leave may change in response to a change in the father's duration on leave; but, the constraint on the mother's side was left unchanged).

As we mentioned in Section 1, our analysis of maternal health focuses on the second and third "Daddy Month" reforms in 2002 and 2016, respectively. Figure 1 displays trends in the timing and duration of parental leave taken by fathers before and after the 2002 reform. Sub-figure (a) plots the share of fathers taking any paternity leave in the month of or the month

following childbirth.[13] Sub-figures (b) and (c) display the average number of wage-replaced compensation days taken in months 0-12 post-childbirth and in total (i.e., until the child's 8th birthday), respectively. There appear to be jumps in all three variables at the time of the reform. The fact that the second reform appears to raise the likelihood of paternity leave use shortly after childbirth is particularly relevant for our analysis of maternal postpartum health: Many fathers only eligible for the first "Daddy Month" allocated most of their leave to follow the mother's leave period (i.e., after the child's first year of life); the second "Daddy Month" encouraged some fathers to also take leave immediately after their child is born. We cannot construct analogous figures for the 2016 reform, as we (presently) do not have data on parental leave take-up beyond 2010; moreover, to the best of our knowledge, there exists no (other) research analyzing the impact of the 2016 reform on the take-up of parental leave or any other outcomes.

3 Data

Our analysis uses multiple Swedish administrative data sets: 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 to measure maternal health outcomes.

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 2010. For each spell, the data contain the exact start and end dates, as well as information about the type of compensation, as described in Section 2 above.

We define the following measures of parental leave: First, we create an indicator for any

[13] We do not have children's exact birth dates in our data (only the month and year), and therefore cannot examine the timing of paternity leave take-up relative to childbirth at a daily or weekly level.

parental leave use. Second, to capture fathers who are on paternity leave at the same time as mothers shortly after childbirth, we create an indicator for any parental leave use in the month of childbirth or the following month. Third, to measure paternity leave take-up over the same time horizon as many of our maternal health outcomes (described further below), we create an indicator for any parental leave use in months 0-12 post-childbirth. Fourth, the number of wage-replacement (WR) days on leave capture the utilization of days with the largest amount of compensation to the household. We define WR days as the total number of days for which wage-replacement compensation is claimed, regardless of whether the day was claimed as a full-time day of leave or a part-time day of leave. We calculate the number of WR days taken in months 0-1, 0-12, and ever post-childbirth.

Unfortunately, the child identifier in the parental leave claims data is not the same as the child identifier in the data from Statistics Sweden. However, the parent identifiers can be linked across the two data sets. We therefore match children in the parental leave data to their year and month of birth in the Statistics Sweden data using the parent identifier. This procedure, described in detail in Appendix B, enables us to match parental leave data to 99 percent of the children born within the time horizon of the sample that we use to analyze the 2002 reform. While we are restricted to the matched sample in our analysis of leave use, we can study maternal health outcomes for both reforms using the full samples of births. For the 2002 reform, we find nearly identical results across the matched and full samples, which is consistent with the fact that there are very few children whom we are unable to match to parental leave claims data.

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 1987 to 2016, specialist outpatient records for 2001 to 2016, and prescription drug records for 2005 to 2016. 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 in 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 primarily 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) diagnosis codes,[14] and the length of stay (for inpatient data only). For each occasion when a prescription drug was bought, the prescription data contain information about the drug name, active substance, average daily dose, and the drug's exact Anatomical Therapeutic Chemical (ATC) code.[15] The ATC classification allows us to link the drugs to the conditions they are most commonly used to treat.

We measure our outcomes over different time horizons for the two reforms because of data constraints. For the 2002 reform, we study maternal inpatient and outpatient visits in months 0 through 12 after the month of childbirth. For the 2016 reform, we can only observe inpatient and outpatient visits through month 6 following childbirth in our primary analysis sample. Since the prescription drug data begin in 2005, we measure the effects of the 2002 reform on medication take-up at any point between 5 and 7 years after childbirth; for the 2016 reform, we are able to study prescription drug use during months 0 through 12 post-childbirth.

Using the inpatient data, we define indicators for any inpatient 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, and (iii) factors influencing health status and contact with health services. The third group of 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

[14] ICD-10 codes begin in 1997, so we use them in the analysis of both inpatient and outpatient records when studying the 2002 and 2016 reforms.

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

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).[16] We create analogous indicators in the outpatient data.[17]

In the prescription drug data, we create indicators for any prescription drug claim, any drug claim in the psycholeptics or psychoanaleptics category, and indicators for two particularly relevant sub-categories of psycholeptics and psychoanaleptics: anti-anxiety medications and anti-depressants. We also create an indicator for any prescription painkiller (analgesic) drug claim.[18]

Finally, to examine mothers across different sub-groups, we add detailed information from birth records, which are available for years 2000-2016. For each mother in our sample, we obtain variables related to the mother's pregnancy and delivery. We create three sub-groups of mothers:[19] (1) mothers with risky prenatal behaviors: late initiation of prenatal care (defined as initiation after the 1st trimester) or tobacco use either at the first prenatal visit or in weeks 30-32;[20] (2) mothers with at least one high-risk condition during pregnancy: urinary infection, kidney disease, diabetes, epilepsy, asthma, hypertension, Ulcerative Colitis, Crohn's disease, or Systemic Lupus Erythematosus (SLE);[21] and (3) mothers with labor or delivery complications: induction of labor, c-section delivery, vaginal delivery with the use of forceps or suction, episiotomy, or uterine ruptures of 3rd or 4th degree.[22]

[16] See Appendix C for exact ICD codes for these conditions.

[17] We also explored effects on the total number of inpatient and outpatient visits, as well as the total number of visits related to any of the diagnoses groups specified above, and found that our estimated effects are driven entirely by extensive margin responses (results for total visits are available upon request).

[18] See Appendix C for exact ATC codes for these conditions.

[19] In supplementary analysis, we also examined a 4th sub-group of mothers of children with poor birth outcomes (e.g., low birth weight or pre-term delivery), finding that the results are similar to those in the overall sample.

[20] Tobacco use includes smoking and the use of *snus*, a powder tobacco product placed under the upper lip. Information about tobacco use is recorded in the electronic medical record that we observe, and is self-reported by the mother both at the first prenatal care visit and in the visit closest to weeks 30-32. Mothers are incentivized to report tobacco use truthfully as tobacco cessation programs are an integral part of prenatal care in Sweden.

[21] Ulcerative Colitis and Crohn's disease are inflammatory bowel diseases that cause long-lasting inflammation and ulcers (sores) in the digestive tract. SLE is a chronic autoimmune disease.

[22] In Sweden, uterine ruptures are classified according to a four-grade scale, with ruptures of degree three and four being the most severe. These include ruptures of the cervix, sphincter, or rectum.

Summary statistics. The top panel of Appendix Table A1 presents sample means of the parental leave variables for the 2002 reform only, while the bottom panel presents sample means of some of our family background characteristics for both reforms. As described in detail below, to study the 2002 reform, our primary specification compares the outcomes of mothers and fathers of first-born children born in January-March 2002 and October-December 2001, relative to the difference across these months in the following year. For the 2016 reform, we compare the outcomes of families with first-born children born in January-March 2016 and October-December 2015, relative to the difference in the previous year.

Columns (1) and (2) of Appendix Table A1 display means for the post- (January-March) and pre- (October-December) sub-groups of the 2002 reform sample, while the 3rd and 4th columns display the analogous sub-groups of the 2002 non-reform sample. In the post-group in the reform sample, where fathers are incentivized to take more parental leave, 91 percent of all fathers take any leave, 8 percent take any leave in months 0-1, 54 percent take any leave in months 0-12, while the average number of WR days taken in months 0-1, 0-12, and ever is 2.4, 27.8, and 76.8, respectively. Compared to the pre-group in the 2002 reform sample, these treated fathers are both more likely to take any leave and to take it soon after childbirth, and also have a higher number of WR days over all the time horizons considered. All mothers take at least some leave. Mothers in families treated by the 2002 reform take slightly more total WR days than mothers in the pre-reform group, which makes sense since the 2002 reform (in contrast to the first one in 1995) gave treated households one additional month of parental leave. These patterns are not apparent in the non-reform sample; if anything, parents in the post-group in the non-reform sample (column 3) have lower parental leave use rates than those in the pre-group (column 4). The disparity in leave use between the pre- and post-groups in the non-reform sample (in which all families are exposed to the 2002 "Daddy Month") highlights the importance of accounting for seasonal differences across January-March and October-December births in an RD-DD design.

The bottom panel of Appendix Table A1 shows some differences in parental characteristics across the post- and pre- sub-groups of both reform and non-reform samples. For instance, compared to parents of children born in October-December, parents of children born in January-March have higher incomes and are slightly older. Again, these patterns suggest that

an RD-DD design that nets out seasonal differences in parental characteristics is appropriate.

4 Empirical Methods

Our goal is to examine the causal link between paternity leave and maternal health. The key empirical challenge is that paternity leave take-up, timing, and duration are not randomly distributed, and instead likely correlated with unobserved family characteristics. For example, men married to women with higher levels of education tend to take longer parental leave than their counterparts (Statistics Sweden, 2016) and, more generally, there are stark differences in paternity leave take-up across the socioeconomic spectrum. It is therefore difficult to isolate the casual effects of paternal time at home on maternal health from the influences of other family characteristics.

We address this challenge by exploiting the natural experiments stemming from the two most recent Swedish "Daddy Month" reforms on January 1, 2002 and 2016, respectively. Our analysis essentially compares individuals whose children are born very close to, but on opposite sides of, these reform dates, and we difference out seasonality effects using parents of children born in the same months but in other non-reform years.

Specifically, for the 2002 reform, our primary specification compares the outcomes of mothers and fathers of first-born singleton children born in January-March 2002 and October-December 2001 ("2002 reform sample"), relative to the difference in outcomes in the following 12 months (January-March 2003 versus October-December 2002, "2002 non-reform sample"). We choose the following year as the non-reform group because the outpatient data only begin in 2001, meaning that we cannot study those outcomes for a group of births from a year prior to the reform. When studying the 2016 reform, our main model compares parents of first-born singleton children born in January-March 2016 and October-December 2015 ("2016 reform sample"), relative to the difference in the preceding 12 months (January-March 2015 versus October-December 2014). We choose the preceding year as the non-reform group as our inpatient and outpatient data end in 2016.

Our primary regression model, which uses the child's month of birth as the running

variable, t, can be expressed as follows:

$$y_{it} = \alpha + \beta_1 \mathbf{1}[t \geq c] + \beta_2 R_i + \beta_3 R_i \times \mathbf{1}[t \geq c] + f(t-c) + \mathbf{1}[t \geq c] \times f(t-c) + \mathbf{x'}_i \kappa + \varepsilon_{it} \quad (1)$$

for each family of first-born singleton child i born in month t. y_{it} is an outcome of interest, such as an indicator for a maternal inpatient visit in the months following childbirth. c denotes January, the reform month. R_i is an indicator set to 1 for children who are in each of our reform samples (i.e., October 2001 - March 2002 for the 2002 reform and October 2015 - March 2016 for the 2016 reform), and 0 otherwise. The dummy variable $\mathbf{1}[t \geq c]$ is set to 1 for children born in January-March in any year. $f(t-c)$ is a linear function of the running variable, month of birth centered around January, which we allow to have different slopes on opposite sides of the threshold in all samples.

The vector $\mathbf{x}_i$ includes the following family control variables, measured in the year before birth: maternal and paternal earnings (in 1000s of real 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, and indicators for each parent being foreign-born. ε_{it} 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, $\mathbf{1}[t \geq c]$, and is denoted by β_3. 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 extended earmarked parental leave—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 "Daddy Month" reforms.

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

Prior literature provides evidence in support of the identifying assumption for the 1995 and 2002 "Daddy Month" reforms (in particular, see Ekberg et al., 2013 and Avdic and Karimi, 2018). Here, we perform RD-DD versions of the McCrary (2008) tests using our samples of the data for the 2002 and 2016 reforms. Specifically, we collapse our data into month-of-birth bins using 6-month bandwidths surrounding each of the reform (non-reform) cutoffs in January 2002 (2003) and 2016 (2015), respectively. Tables 1 and 2 report coefficients from RD-DD models that use 1st through 6th order polynomials in the running variable, with number of births as the outcome. We also report the Akaike Information Criterion (AIC) in the bottom row of each table. While there are statistically significant differences in the number of births in January-June relative to July-December in all years, the discontinuity is not different across the reform and non-reform years (i.e., the coefficient on the interaction between the post-January dummy and the reform sample dummy is insignificant in both tables). Figure 2 presents analogous graphical evidence, where we plot the number of births by birth month in the reform and non-reform samples (as noted in the sub-figure headings). The fitted lines are predicted from 6th (5th) order polynomial models for the 2002 (2016) reform and non-reform samples; we follow Lee and Lemieux (2010) by selecting the models with the smallest AIC values.

We next check for any discontinuities in pre-determined covariates at the reform date. Appendix Tables A2 and A3 report results from estimating versions of model (1), omitting the controls in vector $\mathbf{x}_i$ and instead using the following parental characteristics as outcomes:

indicators for the mother and father having a low education, indicators for the mother and father being foreign-born, maternal and paternal age, and maternal and paternal income. We also construct a summary index by regressing an indicator for any inpatient visit post-childbirth on the displayed parental characteristics, and then using the predicted outcome as the dependent variable in the last column.

Reassuringly, even though Appendix Table A2 displays differential discontinuities in a few individual covariates in our 2002 analysis sample, the RD-DD coefficient when using the summary index outcome is not significant, suggesting that there is little room for concern of systematic timing of birth in the data (as also demonstrated by Avdic and Karimi, 2018). In our 2016 analysis sample, the RD-DD coefficient when using the summary index outcome is significant at the 5% level. This relationship is largely driven by the significant discontinuity in the fraction of births by foreign-born mothers in 2016, following the large immigration wave to Sweden in the fall of 2015.[23] Indeed, if we exclude all births by foreign-born mothers, the RD-DD coefficient for the summary index becomes smaller and only marginally significant. To address the changing share of births by foreign-born mothers over time, we control for the mother being foreign-born in all our regressions in our main specifications. Moreover, as we show in Panel A of Appendix Table A5, our main results for the 2016 reform remain if we exclude all births by foreign-born mothers.

5 Results

Effects of 2002 reform on paternity leave use. Before presenting our results on the effects of the "Daddy Month" reforms on maternal health, we briefly discuss our estimates of the impacts of the 2002 reform on paternity leave take-up, timing, and duration. As already noted in Section 2, Figure 1 displays graphical evidence for discontinuities in the share of fathers taking any leave in months 0-1 following childbirth, the average number of WR days taken in the year following childbirth, and the average number of WR days taken overall (until the child's 8th birthday) at the time of the reform, respectively.

Table 3 presents results from estimating equation (1) using the paternity leave variables

[23]For more information about the immigration wave, see, e.g.: https://www.scb.se/hitta-statistik/sverige-i-siffror/manniskorna-i-sverige/in-och-utvandring/fordjupning.

described in Section 3 as outcomes. Consistent with prior studies (Duvander and Johansson, 2012, 2014; Avdic and Karimi, 2018), the last column shows that the implementation of the 2002 reform led to an increase in total paternity leave duration of approximately 19 days. More pertinent to our study of maternal postpartum health, the remainder of the table presents new results on take-up on the extensive margin and on the timing of leave. Columns (1), (2), and (3) show 1.7, 2.3, and 2.9 percentage point increases in any paternity leave take-up, paternity leave use in months 0-1 after childbirth, and paternity leave use in months 0-12 after childbirth, respectively, which represent 2, 31, and 5 percent effect sizes when evaluated at the sample means. Columns (4) and (5) show increases in the average number of WR days taken in months 0-1 and 0-12 of 0.8 and 2.7, respectively, corresponding to 41 and 10 percent effects at the sample means.

Effects of the 2002 "Daddy Month" on maternal health. Tables 4, 5, and 6 report estimates from model (1) for the 2002 reform, using inpatient, outpatient, and prescription drug data, respectively. In each table, we show results for the whole sample in Panel A, and for each of our sub-samples of mothers described in Section 3 in Panels B, C, and D. In panel A of Table 4, we document that the 2002 reform results in a 3.2 percentage point (38 percent) decrease in the likelihood of the mother having any inpatient visit in the 12 months after childbirth (column 1), which is driven by a 0.9 percentage point (22 percent) reduction in visits for pregnancy or childbirth complications (column 2) and a 1.9 percentage point (158 percent) decline in visits with diagnosis codes that begin with Z for "factors influencing health status and contact with health services" (column 4). There are no statistically significant impacts on visits with diagnoses for mental, behavioral, or neurodevelopmental disorders (column 3). When we examine in more detail the diagnoses driving the result in column (4), we find that the reduction in visits with Z code diagnoses is entirely explained by one code: *Z39*, which is used for "encounters for maternal postpartum care and examination". Since routine postpartum care is performed in primary care clinics in Sweden, an inpatient record with this code suggests a more serious issue that involves counseling, medical advice, or admission for observation (but does not lead to a formal diagnosis of a health condition). Interestingly, while the impacts on counseling-related inpatient visits are present across mul-

tiple sub-groups of mothers, the decrease in hospitalizations for childbirth-related problems is only apparent for mothers who experienced labor or delivery complications (column 2 of Panel D in Table 4).

In Table 5, we consider the same outcomes in the outpatient data. In the overall sample and across all sub-samples of mothers, we again see a decline in visits with specific *Z* codes: *Z71* ("Persons encountering health services for other counseling and medical advice, not elsewhere classified") and *Z76* ("Persons encountering health services in other circumstances"). The fact that there is a decline in these types of visits in non-primary-care outpatient settings—to which mothers must be referred by their primary care providers—suggests that paternity leave leads to a reduced likelihood that a new mother requires medical counseling from a specialist provider. However, among mothers with risky prenatal behaviors, there appears to be an offsetting increase in outpatient visits with mental health related diagnoses (column 3 of Panel B in Table 5), suggesting that paternity leave can have some adverse consequences for maternal well-being in this relatively less advantaged sub-group.

Table 6 examines the prescription drug outcomes, which, as we discuss in Section 3, we measure 5-7 years after childbirth when analyzing the 2002 reform. While there are no significant impacts on mental health related prescription drugs, we do see a 2.3 percentage point (13 percent) decrease in the consumption of painkillers among mothers with labor/delivery complications. Since data show that among mothers who consume at least one prescription painkiller in the year after childbirth, about 40 percent also take one 5 to 7 years later,[24] our result is consistent with a possible reduction in the severity or duration of pain in the postpartum period for this sub-group.

Lastly, sub-figures (a), (b), and (c) of Figure 3 present graphical evidence for the impact of the 2002 reform on some of the key maternal health outcomes: inpatient visits for childbirth-related complications, inpatient visits with a *Z39* code, and outpatient visits with a *Z71* code, respectively. We plot the raw means of the outcome variables by the child's birth month using a 12-month bandwidth surrounding the reform, with linear fits estimated separately on each side of the cut-off. Consistent with the regression results just discussed, the figures suggest

[24]Using data on all 2006 births, we examine mothers who are observed with at least one painkiller prescription drug claim in the 12 months post-childbirth. Among these mothers, we find that 40 percent have at least one painkiller prescription drug claim when their child is 5, 6, or 7 years old.

declines in these health encounters at the time of the reform.

Effects of the 2016 "Daddy Month" on maternal health. We turn to studying the impacts of the 2016 reform on maternal inpatient, outpatient, and prescription drug outcomes in Tables 7, 8, and 9, respectively. As before, results for the whole sample and for each of the three sub-samples are in Panel A through D of each table. Similar to the 2002 reform, we observe a 2.0 percentage point (31 percent) decline in hospital admissions in the 6 months post-childbirth, driven entirely by a 2.0 percentage point (162 percent) reduction in visits with *Z* codes. Analysis within the *Z* category shows that this decline is due to a reduction in visits coded with *Z76*. We also find a marginally significant decline in hospitalizations for childbirth-related complications for mothers with pregnancy risk factors (column 2 of panel C in Table 7). For this sub-group, there is a partially offsetting increase in visits with *Z39* codes, although the overall effect on visits with any *Z* code is still negative.

We also observe declines in outpatient visits in months 0-6 postpartum. Panel A of Table 8 shows that in the overall sample, there is a 4.7 percentage point (10 percent) reduction in non-primary-care visits, driven by 3.8, 0.6, and 1.7 percentage point (24, 25, and 9 percent) declines in visits for childbirth-related complications, mental health diagnoses, and *Z* code diagnoses, respectively. The estimated effects are stronger among mothers with risky prenatal behaviors and those with labor/delivery complications (panels B and D, respectively). For mothers with high-risk pregnancy conditions in panel C, there is an increase in visits with a *Z39* code, and a decline in visits with a *Z76* code, resulting in a net zero impact on total *Z* code visits.

Analysis of prescription drug data in Table 9 suggests that there are no statistically significant effects on drug consumption in the year following childbirth in the overall sample. However, we observe a 2.5 percentage point (11 percent) decline in the use of painkillers among mothers with labor/delivery complications in panel D, as well as marginally significant reductions in the likelihood of any drug claim in the psycholeptics or psychoanaleptics category among mothers with risky prenatal behaviors (column 2 of Panel B) and in anti-depressant use among mothers with pregnancy risk factors (column 4 of Panel C).

Sub-figures (d), (e), and (f) of Figure 3 show graphs for three of the maternal outpatient

visit outcomes for the 2016 reform: outpatient visits with a *Z71* code, for childbirth-related complications, and for mental health diagnoses, respectively. Sub-figures (g), (h), and (i) of Figure 3 present graphs for prescription drug outcomes for the sub-groups where we see some significant effects: painkiller drugs among mothers with labor/delivery complications, drugs in the psycholeptics or psychoanaleptics category among mothers with risky prenatal behaviors, and anti-depressants among mothers with pregnancy risk factors. Again, there seem to be reductions in these health encounters and drug purchases following the reform.

Sensitivity analysis and timing of effects. We explore the sensitivity of our main results to alternative specifications in Appendix Tables A4 and A5 for the 2002 and 2016 reforms, respectively. For the 2002 reform, we focus on the following outcomes: (1) any inpatient admission for childbirth-related complications, (2) any inpatient admission with code *Z39*, (3) any outpatient admission with code *Z71*, and (4) any consumption of painkillers among the sub-group of mothers with labor/delivery complications. For the 2016 reform, we examine: (1) any inpatient admission with code *Z76*, (2) any outpatient admission for childbirth-related complications, (3) any outpatient admission with a mental health diagnosis, (4) any outpatient admission with code *Z71*, and (5) any consumption of painkillers among the sub-group of mothers with labor/delivery complications.

In Panel A (B) of Appendix Table A4 (A5), we simplify our baseline regression (1) to a difference-in-differences (DD) model by omitting controls for the running variable (month of birth). In Panel B (C) of Appendix Table A4 (A5), we extend the RD-DD model to include an expanded sample of births in a 6-month bandwidth (i.e., we compare January-June births relative to July-December births). In Panel C (D) of Appendix Table A4 (A5), we present results from maternal fixed effects (FE) models: When studying the 2002 reform, we use a sample of mothers of two or more children born in January 2001 through December 2004; for the 2016 reform, we use mothers of siblings born in July 2013 through June 2016. The treatment variable in the FE model is an indicator for a child being born in the post-reform period, and in addition to maternal fixed effects, we control for the following time-varying characteristics: number of months between the focal child and the previous birth (0 for firstborns), maternal and paternal earnings, indicators for each parent's age groups,

indicators for each parent's education levels, an indicator for the parents being married, birth month fixed effects, and fixed effects for the child's birth order. Standard errors in Panel C are clustered on the mother.[25] Additionally, Panel A of Appendix Table A5 presents results for the 2016 reform when dropping foreign-born mothers to account for the significant discontinuity in this characteristic due to a large immigration wave to Sweden in late 2015 (as discussed in Section 4 above).

Our results are broadly consistent across specification and sample choices. The maternal FE model does not deliver statistically significant coefficients for the outpatient outcomes in the 2016 reform analysis, although the signs of the coefficients for visits with mental health diagnoses and *Z71* codes are the same as in the main results. The FE model uses a selected sample of sibling births, and does not account for possible spillover effects across siblings, making it our less preferred specification.

Finally, Figure 4 explores the timing of the effects on some of the main inpatient and outpatient outcomes for both reforms and prescription drug outcomes for the 2016 reform. We plot the RD-DD treatment coefficients and 95% confidence intervals from regression models that use outcomes measured in the months since childbirth denoted on the $x-$axis of each graph. The outcomes are: (a) any inpatient admission for childbirth-related complications (2002 reform), (b) any inpatient admission with code *Z39* (2002 reform), (c) any outpatient admission with code *Z71* (2002 reform), (d) any outpatient admission with code *Z71* (2016 reform), (e) any outpatient admission for childbirth-related complications (2016 reform), (f) any outpatient admission with a mental health diagnosis (2016 reform), (g) any painkiller prescription drug claim among mothers with labor/delivery complications, (g) any drug claim in the psycholeptics or psychoanaleptics category among mothers with risky prenatal behaviors, and (h) any anti-depressant drug claim among mothers with pregnancy risk factors. For nearly all inpatient and outpatient outcomes (except for mental health outpatient visits following the 2016 reform) we observe that the effects materialize primarily in the months 0-3 following childbirth. This pattern makes sense as most postpartum healthcare encounters happen soon after childbirth rather than months later. Given that we find a significant in-

[25]For studying mothers with labor/delivery complications for the analysis of painkiller take-up using maternal fixed effects, we use mothers who had labor/delivery complications at the time of the first birth in the sample.

crease in the likelihood that fathers use paternity leave in the month of or the month following childbirth as a result of the 2002 reform (Table 3), our results are consistent with the idea that greater paternal presence in the home can improve maternal health in the immediate postpartum period. The timing of the 2016 reform results for prescription drug claims in the months following childbirth are less precise; we cannot rule out that the effects are the same across the months considered in the graphs.

Discussion and implications for the design of family leave policies. Our documented positive impacts of the two most recent Swedish "Daddy Month" reforms on maternal postpartum health raise a key question: Why does *adding* constraints to households' decision problems—in this case, imposing lower bounds on fathers' leave duration—improve household welfare? In most standard models of household behavior, adding constraints on the choice set cannot make the household better off (see, e.g., Browning and Chiappori, 1998 and Persson, 2014). In the setting that we analyze, however, we find that the imposition of constraints has positive consequences, even though parents who adjust their take-up of parental leave in response to a "Daddy Month" reform end up with a division of parental leave that existed in their choice set pre-reform as well.

While this behavior may result from parents having downward biased prior beliefs about the benefits of paternity leave, a more policy-relevant potential explanation relates to the presence of stigma.[26] There may exist a real or perceived career cost from taking parental leave as it may signal that an individual is less motivated at his/her job. If such signaling concerns are equally important for men and women, then this cost would cause parents to take up less leave than they optimally would choose in the absence of leave-related stigma but it would not lead households to alter their *distribution* of leave across parents. If, however, the signaling cost is higher for men, then parents will choose a division of parental leave that is tilted away from paternal time at home, relative to their optimal choice in the absence of stigma. Dahl et al. (2014) discuss the presence of stigma against fathers who take paternity

[26] Patnaik (2016) discusses an alternative, more "behavioral" interpretation of the fact that a paternity leave reform in Canada resulted in men taking more leave than in the pre-reform period. She argues that simply labeling certain weeks as "daddy leave" plays an important role for encouraging program participation. However, as the Canadian reform analyzed in the paper not only reserved certain weeks for the father, but also raised the wage-replacement rate and expanded eligibility, it is challenging to separate any impact of the label from the impact of the program becoming more generous.

leave in Norway, and offer an illustrative quote from a 1995 white paper from the Norwegian government on this topic: "Fathers are concerned that both employers and coworkers will perceive them as less invested in their careers if they exhibit a large commitment to family." The report also states: "One reason fathers fail to exercise their rights may also be fear of deviating from the usual pattern in the workplace."

In the presence of male-biased stigma against parental leave take-up, policies that simply make paternity leave available may have limited effects on actual paternal time in the household. Consistent with this point, when Sweden first introduced shared parental leave in 1974, fathers took a mere 0.5 percent of all parental leave days (Duvander and Johansson, 2012), and those who took any leave were derisively referred to as "velvet dads".[27] Policies that explicitly encourage paternity leave take-up by making it costly for households to *not* use it (e.g., by removing the possibility to transfer some of the father's unused leave to the mother) may therefore be welfare-improving. By inducing a large number of men to take up more paternity leave, such policies change the norm and thus reduce leave-associated stigma, generating a positive externality for all men who choose to take leave, along with their families.

6 Conclusion

When a woman gives birth to a child, much of the attention is typically placed on the health and well-being of the newborn baby. There are many healthcare and social policy interventions targeting infants, and a plethora of research has been dedicated to understanding the causes and consequences of early-life health. 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 healthcare system in delivering postpartum care.[28] But the mother's environment at home can have

[27] See: `https://www.nytimes.com/2010/06/10/world/europe/10iht-sweden.html`.

[28] For examples of these discussions in the press, see: https://www.vox.com/science-and-

significant influence on her well-being during the often emotional and overwhelming months of new parenthood. Our paper focuses on a key factor: the presence (or absence) of the child's father at home in the months after childbirth.

To isolate the causal effects of paternal time at home on maternal health, we take advantage of linked Swedish administrative data and quasi-experimental variation from the introduction of the two most recent "Daddy Month" reforms in 2002 and 2016, which extended dedicated paternity leave to two and three months, respectively. Using an RD-DD design, we first confirm prior evidence that the 2002 reform increased average paternity leave duration by about 19 days, and provide new results on take-up on the extensive margin and on the timing of leave. Most relevant to our analysis of maternal postpartum health, we show that the 2002 reform resulted in a 2.3 percentage point (31 percent) increase in paternity leave use in the month of or the month following childbirth.

Then, we present consistent evidence that each of the two reforms has led to a decline in inpatient and non-primary-care outpatient visits among new mothers on average. We demonstrate that the 2002 reform is associated with a 3.2 percentage point (38 percent) decrease in the likelihood of any inpatient visit in the year after childbirth, driven by reductions in visits for pregnancy or childbirth complications and in visits for medical advice or counseling. We similarly observe a decrease in outpatient visits for medical advice or counseling. The effect on counseling-related visits is apparent across different sub-groups of mothers, while the decline in hospitalizations for childbirth-related problems is only present for mothers who had labor/delivery complications. Analysis of prescription drug data further shows a reduction in the consumption of painkillers 5-7 years after childbirth for this sub-group. We do find one potentially adverse effect of the 2002 reform on maternal health, however: Among mothers with risky prenatal behaviors, we observe an increase in outpatient visits with mental health related diagnoses. Thus, while the average impact of the 2002 "Daddy Month" reform appears to be beneficial for maternal health, disadvantaged mothers—such as those who engage in risky behaviors during pregnancy—may experience some unintended consequences due to

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.

exacerbated family conflict or stress.

We also show that the 2016 reform leads to a 2.0 percentage point (31 percent) decrease in inpatient visits in the 6 months after childbirth, largely driven by visits for medical advice or counseling. In the outpatient data, we see reductions in visits for childbirth-related complications, mental health diagnoses, and counseling. And, similar to the 2002 reform, we find a decrease in painkiller use among mothers with labor/delivery complications, which we can measure in months 0-12 after childbirth. Lastly, we observe marginally significant reductions in mental health-related drug purchases among some of the sub-groups of mothers in our data.

In addition to informing questions about determinants of maternal postpartum health, our findings have important implications for debates about paid family leave. 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.[29] 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.[30] While most discussions about encouraging men to take paternity leave focus on the consequences for men's and women's careers and gender norms,[31] our study demonstrates that encouraging fathers to take leave, especially immediately after childbirth, could be critical for maternal postpartum recovery and health. Moreover, our research underscores that it may not be sufficient to simply make leave available to fathers; if there is stigma associated with paternity leave use, then gender-neutral PFL policies may only have limited impacts on take-up among fathers.[32] Policies that go further—and explicitly impose costs on households in which the father does not take leave—can thus be potentially welfare-improving.

[29]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).

[30]For example, Bana et al. (2018) estimate that only 12 percent of eligible new fathers in California made a PFL claim in 2014, ten years after the introduction of the program. In 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.

[31]See, e.g.: `http://time.com/4483627/paternity-leave/`

[32]To this point, 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.

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

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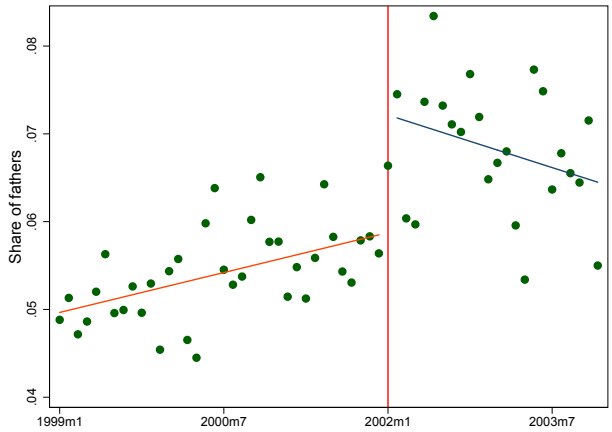
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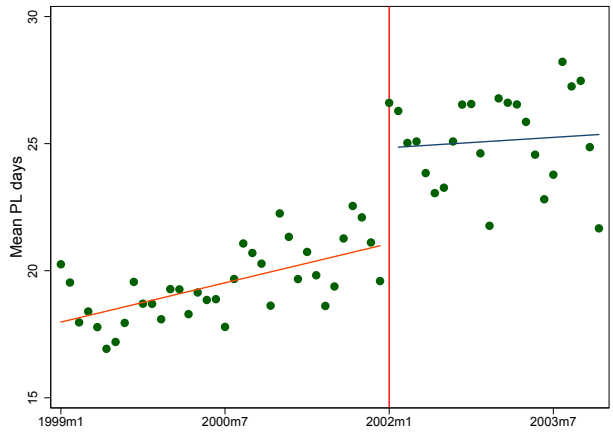
7 Figures

Figure 1: Paternity Leave Use Before and After **2002** "Daddy Month" Reform

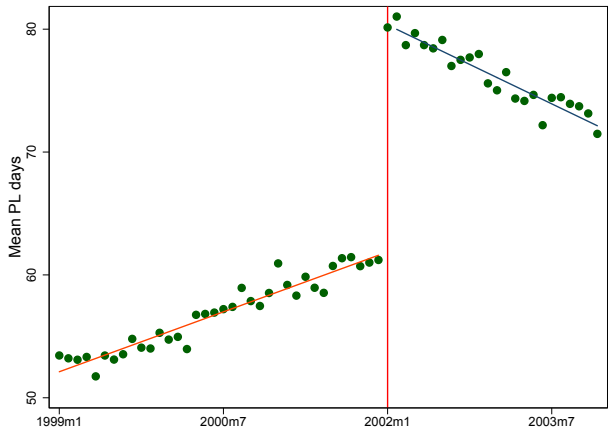
(a) Share of Fathers Using Any Leave in Mos 0-1



(b) Fathers' Average Number of WR Days in Mos 0-12



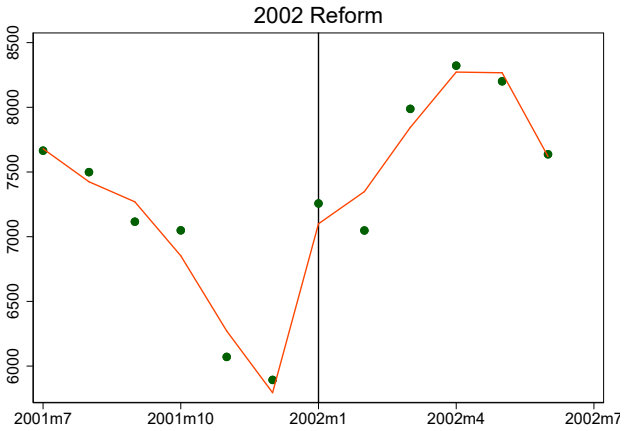
(c) Fathers' Average Number of WR Days Ever Taken



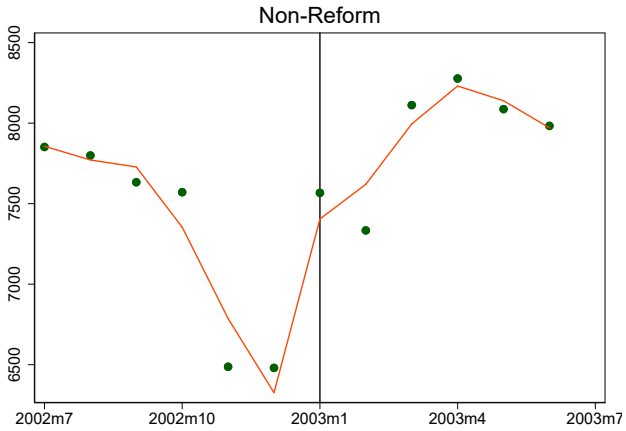
Note: The figures plot trends in paternity leave variables by the child's birth month. Sub-figure (a) displays the share of fathers taking any paternity leave in the month of or the month following childbirth, sub-figure (b) shows the average number of wage-replaced compensation days in months 0-12 post-childbirth, while sub-figure (c) shows the average number of wage-replaced compensation days ever taken post-childbirth. The 2002 reform is denoted using vertical red lines. The fitted lines are predicted from linear models on each side of the cut-off.

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

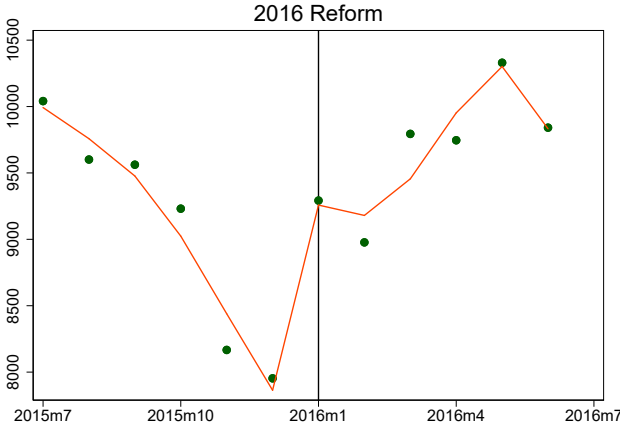
(a) **2002** Reform



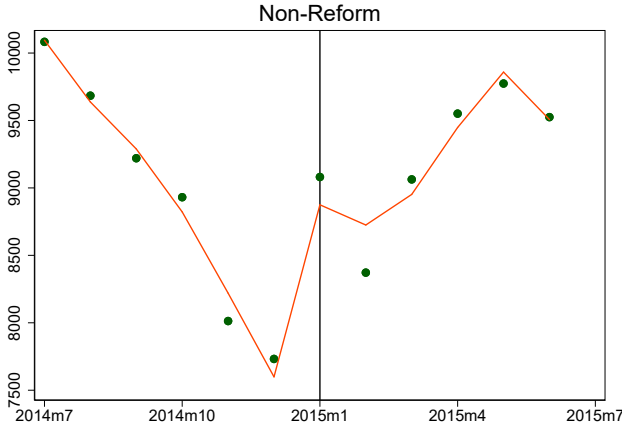
(b) **2002** Non-Reform



(c) **2016 Reform**



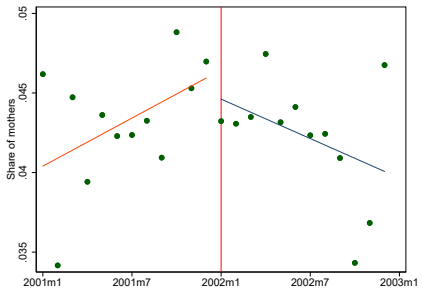
(d) **2016** Non-Reform



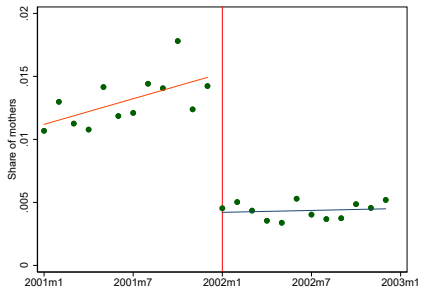
Note: The figures plot the total number of births by birth month in the reform and non-reform samples (as noted in the sub-figure headings). The fitted lines are predicted from 6th (5th) order polynomial models for the 2002 (2016) reform and non-reform samples. We follow Lee and Lemieux (2010) by selecting the models with the smallest Akaike Information Criterion (AIC) values.

Figure 3: Graphical Evidence of Effects of the 2002 and 2016 "Daddy Month" Reforms on Maternal Health Outcomes

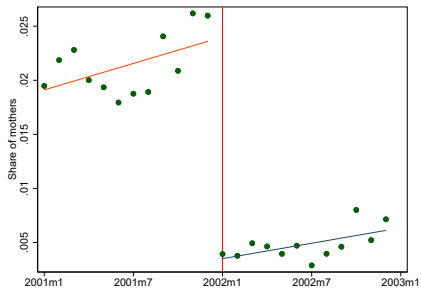
(a) **2002**, Inpatient, Preg Comp.



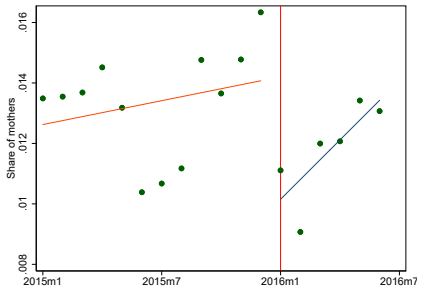
(b) **2002**, Inpatient, Z39



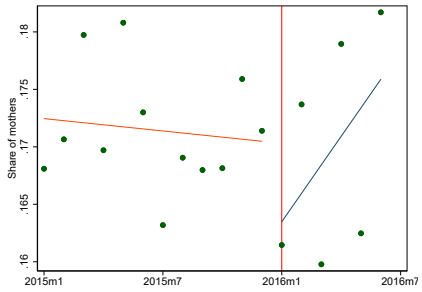
(c) **2002**, Outpatient, Z71



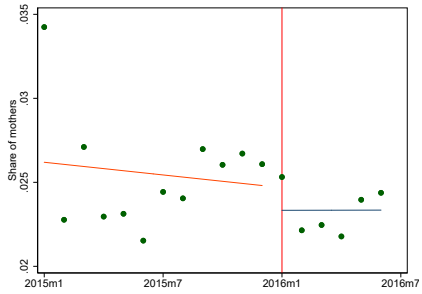
(d) **2016**, Outpatient, Z71



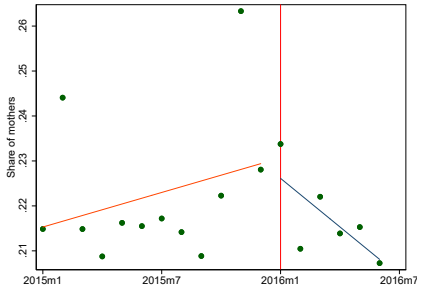
(e) **2016**, Outpatient, Preg Comp.



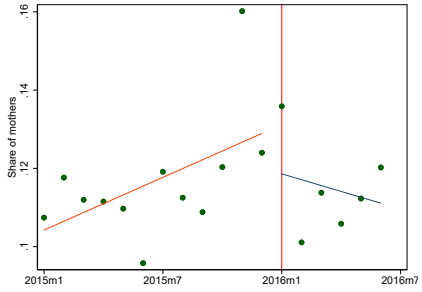
(f) **2016**, Outpatient, Mental



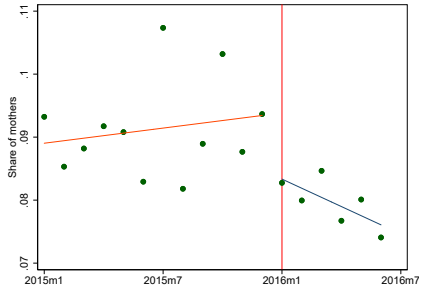
(g) **2016**, Painkillers, Moms with Labor/Delivery Complications



(h) **2016**, N05/N06 Drugs, Moms with Risky Prenatal Behaviors



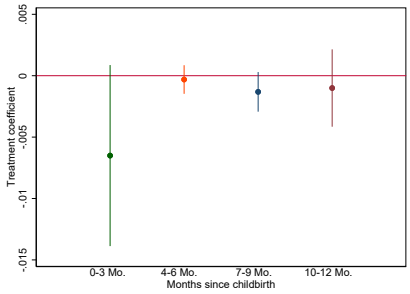
(i) **2016**, Anti-Depressants, Moms with High-Risk Pregnancies



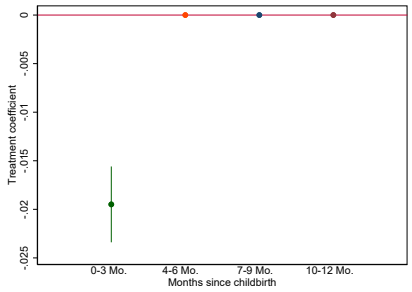
Note: The figures display the means of outcome variables by the child's birth month. The 2002 and 2016 reforms are denoted using vertical red lines. The fitted lines are predicted from linear models on each side of the cut-off. The outcomes are listed in the sub-figure headings. See Appendix C for more details on the exact ICD and ATC codes for outcomes.

Figure 4: Timing of Effects of the 2002 and 2016 "Daddy Month" Reforms on Maternal Health Outcomes

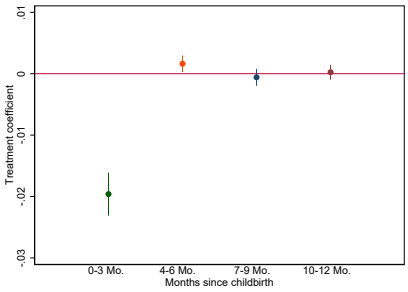
(a) **2002**, Inpatient, Preg Comp.



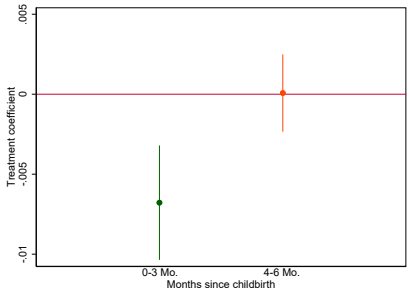
(b) **2002**, Inpatient, Z39



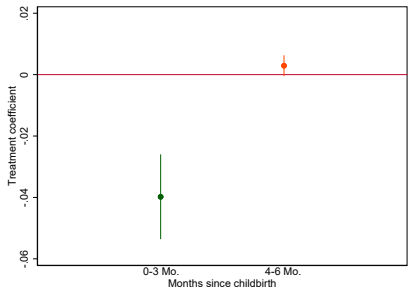
(c) **2002**, Outpatient, Z71



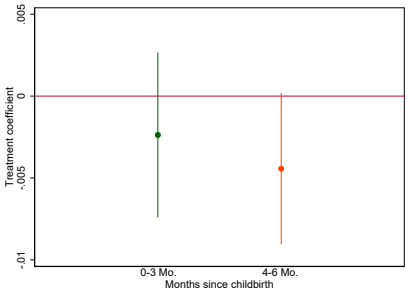
(d) **2016**, Outpatient, Z71



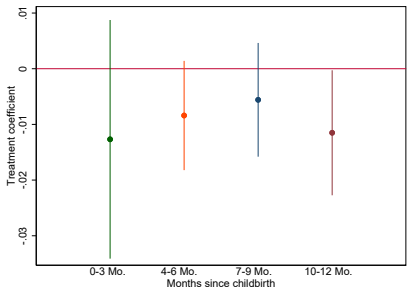
(e) **2016**, Outpatient, Preg Comp.



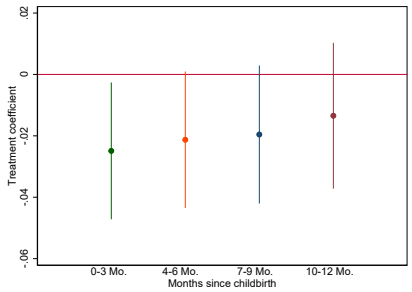
(f) **2016**, Outpatient, Mental



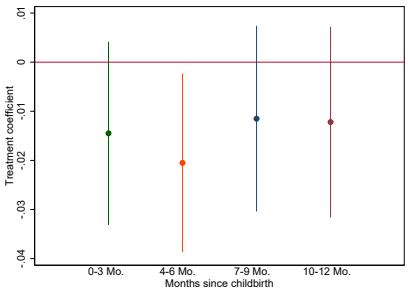
(g) **2016**, Painkillers, Moms with Labor/Delivery Complications



(h) **2016**, N05/N06 Drugs, Moms with Risky Prenatal Behaviors



(i) **2016**, Anti-Depressants, Moms with High-Risk Pregnancies



Note: The figures plot the RD-DD treatment coefficients and 95% confidence intervals from regression models that use outcomes measured in the months 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 Tables 4 and 7 for more details on the specifications and controls.

8 Tables

Table 1: McCrary Test: **2002** "Daddy Month" Reform

	(1) Poly 1	(2) Poly 2	(3) Poly 3	(4) Poly 4	(5) Poly 5	(6) Poly 6
Reform x Birth Jan-June	270.3	270.3	270.3	270.3	270.3	270.3
	(462.3)	(326.1)	(332.7)	(196.3)	(197.8)	(182.8)
Reform	-421.5	-421.5*	-421.5*	-421.5***	-421.5***	-421.5***
	(326.9)	(230.6)	(235.3)	(138.8)	(139.8)	(129.3)
Dummy for Birth Jan-June	1184.2**	1184.2***	1013.3*	1013.3***	1196.2***	1196.2***
	(521.3)	(367.7)	(489.6)	(288.8)	(358.6)	(331.5)
Observations	24	24	24	24	24	24
AIC	376.8	360.7	362.3	337.5	338.3	334.9

Notes: Each column reports coefficients from separate regressions. The data are collapsed into month-of-birth bins, with the outcome being the total number of births. The 2002 reform sample includes births in July 2001 -June 2002, while the non-reform sample includes births in July 2002 - June 2003. We report results from models that use 1st through 6th order polynomials in the running variable. We report the Akaike Information Criterion (AIC) values in the bottom row. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 2: McCrary Test: **2016** "Daddy Month" Reform

	(1) Poly 1	(2) Poly 2	(3) Poly 3	(4) Poly 4	(5) Poly 5	(6) Poly 6
Reform x Jan-June	287.0	287.0	287.0	287.0	287.0	287.0
	(561.1)	(290.5)	(298.5)	(205.1)	(175.4)	(179.9)
Reform	148.7	148.7	148.7	148.7	148.7	148.7
	(396.8)	(205.4)	(211.1)	(145.0)	(124.0)	(127.2)
Dummy for Birth Jan-June	1075.0	1075.0***	1134.0**	1134.0***	1621.6***	1621.6***
	(632.7)	(327.6)	(439.3)	(301.8)	(318.1)	(326.3)
Observations	24	24	24	24	24	24
AIC	386.1	355.2	357.1	339.6	332.6	334.1

Notes: Each column reports coefficients from separate regressions. The data are collapsed into month-of-birth bins, with the outcome being the total number of births. The 2016 reform sample includes births in July 2015 -June 2016, while the non-reform sample includes births in July 2014 - June 2015. We report results from models that use 1st through 6th order polynomials in the running variable We report the Akaike Information Criterion (AIC) values in the bottom row. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 3: Effects of the **2002** "Daddy Month" on Paternity Leave Take-Up, Timing, and Duration

	(1) Any	(2) Any, Mos 0-1	(3) Any, Mos 0-12	(4) WR Days, Mos 0-1	(5) WR Days, Mos 0-12	(6) WR Days, Tot
Reform x Birth Jan - Mar	0.0173***	0.0226***	0.0289***	0.783***	2.738***	19.23***
	(0.00566)	(0.00544)	(0.0104)	(0.293)	(0.978)	(1.319)
Dep. var mean	0.918	0.0730	0.543	1.915	27.07	75.98
Indiv. obs.	36229	36229	36229	36229	36229	36229

Notes: Each column reports coefficients from separate regressions. The outcomes are: (1) indicator for any paternity leave, (2) indicator for any paternity leave taken in months 0-1 after childbirth, (3) indicator for any paternity leave taken in months 0-12 after childbirth, (4) total number of WR days taken in months 0-1 after childbirth, (5) total number of WR days taken in months 0-12 after childbirth, and (6) total number of WR days taken in total (until the child's 8th birthday). The reported coefficients are from the RD-DD model for the analysis of the 2002 reform. We compare the differences in outcomes for fathers of first-born children born in January-March 2002 and October-December 2001 ("2002 reform sample"), relative to the difference in outcomes in the following year (January-March 2003 versus October-December 2002, "2002 non-reform sample"). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. All regressions include controls for 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, and indicators for each parent being foreign-born. Robust standard errors in parentheses.

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

Table 4: Effects of the **2002** "Daddy Month" Reform on Maternal Health Outcomes in **Inpatient** Data

	(1) Any	(2) Preg. Comp.	(3) Ment.	(4) Z	(5) Z39	(6) Z76
A. Full Sample						
Reform x Birth Jan - Mar	-0.0315***	-0.00927**	0.0000321	-0.0187***	-0.0195***	0.000196
	[0.00575]	[0.00419]	[0.00107]	[0.00226]	[0.00206]	[0.000203]
Dep. var mean	0.0835	0.0424	0.00265	0.0118	0.00970	0.000107
Indiv. obs.	37307	37307	37307	37307	37307	37307
B. Mothers with Risky Prenatal Behaviors						
Reform x Birth Jan - Mar	-0.0378***	-0.0112	0.000643	-0.0179***	-0.0171***	0.000347
	[0.0124]	[0.00927]	[0.00263]	[0.00477]	[0.00423]	[0.000347]
Dep. var mean	0.0990	0.0525	0.00406	0.0131	0.0104	0.000107
Indiv. obs.	9361	9361	9361	9361	9361	9361
C. Mothers with High-Risk Pregnancy Conditions						
Reform x Birth Jan - Mar	-0.0371***	-0.00749	-0.00110	-0.0165***	-0.0181***	-0.0000151
	[0.0124]	[0.00884]	[0.00225]	[0.00454]	[0.00405]	[0.000614]
Dep. var mean	0.0968	0.0468	0.00285	0.0117	0.00933	0.000220
Indiv. obs.	9108	9108	9108	9108	9108	9108
D. Mothers with Labor/Delivery Complications						
Reform x Birth Jan - Mar	-0.0350***	-0.0128**	-0.000828	-0.0171***	-0.0180***	0.000607*
	[0.00859]	[0.00624]	[0.00157]	[0.00342]	[0.00306]	[0.000351]
Dep. var mean	0.0919	0.0461	0.00281	0.0132	0.0105	0.000165
Indiv. obs.	18159	18159	18159	18159	18159	18159

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for any inpatient admission in months 0-12 after childbirth (excluding the actual childbirth). We consider indicators for any visit for any reason (column 1), as well as indicators for any visits associated with the following diagnosis codes: pregnancy or childbirth-related physical health complications (column 2); mental health (column 3); factors influencing health status and contact with health services (ICD-10 codes that begin with "Z", columns 4-6). See Appendix C for more details on the exact ICD codes for these conditions. The reported coefficients are from the RD-DD model for the analysis of the 2002 reform. We compare the differences in outcomes for fathers of first-born children born in January-March 2002 and October-December 2001 ("2002 reform sample"), relative to the difference in outcomes in the following year (January-March 2003 versus October-December 2002, "2002 non-reform sample"). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. All regressions include controls for 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, and indicators for each parent being foreign-born. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 5: Effects of the **2002** "Daddy Month" Reform on Maternal Health Outcomes in **Outpatient** Data

	(1) Any	(2) Preg. Comp.	(3) Ment.	(4) Z	(5) Z39	(6) Z71	(7) Z76
A. Full Sample							
Reform x Birth Jan - Mar	0.00831	-0.00499	0.00162	-0.00980	0.00195	-0.0185***	-0.00112
	[0.0103]	[0.00598]	[0.00187]	[0.00719]	[0.00416]	[0.00215]	[0.000748]
Dep. var mean	0.434	0.0920	0.00826	0.139	0.0418	0.00994	0.00129
Indiv. obs.	37307	37307	37307	37307	37307	37307	37307
B. Mothers with Risky Prenatal Behaviors							
Reform x Birth Jan - Mar	0.0193	-0.0117	0.00913**	-0.00631	0.00119	-0.0204***	-0.00315*
	[0.0207]	[0.0123]	[0.00451]	[0.0151]	[0.00827]	[0.00453]	[0.00169]
Dep. var mean	0.462	0.0980	0.0121	0.156	0.0412	0.0111	0.00160
Indiv. obs.	9361	9361	9361	9361	9361	9361	9361
C. Mothers with High-Risk Pregnancy Conditions							
Reform x Birth Jan - Mar	0.0280	0.00497	-0.000488	-0.000704	0.0121	-0.0237***	-0.000938
	[0.0210]	[0.0126]	[0.00436]	[0.0153]	[0.00922]	[0.00486]	[0.00135]
Dep. var mean	0.485	0.101	0.0109	0.158	0.0507	0.0130	0.00110
Indiv. obs.	9108	9108	9108	9108	9108	9108	9108
D. Mothers with Labor/Delivery Complications							
Reform x Birth Jan - Mar	-0.00906	-0.00667	0.00147	-0.0237**	0.00392	-0.0209***	-0.00288**
	[0.0148]	[0.00929]	[0.00287]	[0.0113]	[0.00747]	[0.00327]	[0.00123]
Dep. var mean	0.487	0.111	0.00936	0.175	0.0682	0.0117	0.00171
Indiv. obs.	18159	18159	18159	18159	18159	18159	18159

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for any outpatient admission in months 0-12 after childbirth. We consider indicators for any visit for any reason (column 1), as well as indicators for any visits associated with the following diagnosis codes: pregnancy or childbirth-related physical health complications (column 2); mental health (column 3); factors influencing health status and contact with health services (ICD-10 codes that begin with "Z", columns 4-7). See Appendix C for more details on the exact ICD codes for these conditions. The reported coefficients are from the RD-DD model for the analysis of the 2002 reform. See notes under Table 4 for more details on the specification and controls. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 6: Effects of the **2002** "Daddy Month" Reform on Maternal Health Outcomes in **Prescription Drug** Data

	(1) Any	(2) N05/06	(3) Anti-anx	(4) Anti-dep	(5) Pain
A. Full sample					
Reform x Birth Jan - Mar	-0.00682	-0.00758	-0.00354	-0.00631	-0.0154*
	[0.00678]	[0.00766]	[0.00503]	[0.00667]	[0.00786]
Dep. var mean	0.877	0.165	0.0630	0.119	0.175
Indiv. obs.	37307	37307	37307	37307	37307
B. Mothers with Risky Prenatal Behaviors					
Reform x Birth Jan - Mar	0.00671	-0.0134	-0.0129	-0.00860	-0.0153
	[0.0132]	[0.0169]	[0.0119]	[0.0149]	[0.0169]
Dep. var mean	0.884	0.213	0.0919	0.154	0.212
Indiv. obs.	9361	9361	9361	9361	9361
C. Mothers with High-Risk Pregnancy Conditions					
Reform x Birth Jan - Mar	-0.0171	0.0130	0.00359	0.00253	-0.00559
	[0.0116]	[0.0169]	[0.0113]	[0.0148]	[0.0172]
Dep. var mean	0.916	0.207	0.0800	0.149	0.215
Indiv. obs.	9108	9108	9108	9108	9108
D. Mothers with Labor/Delivery Complications					
Reform x Birth Jan - Mar	-0.00910	-0.0114	-0.00297	-0.00569	-0.0233**
	[0.00949]	[0.0111]	[0.00727]	[0.00966]	[0.0115]
Dep. var mean	0.883	0.171	0.0645	0.122	0.185
Indiv. obs.	18159	18159	18159	18159	18159

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for any prescription drug claim observed for the mother when her firstborn child is between ages 5 and 7. We consider indicators for any prescription drug claim (column 1), any psycholeptic or psychoanaleptic drug claim (ATC codes that begin with "N05" or "N06", column 2), any anti-anxiety drug claim (column 3), any antidepressant drug claim (column 4), any pain-related drug claim (column 5), any birth control drug claim (column 6). See Appendix C for exact ATC codes for these conditions. The reported coefficients are from the RD-DD model for the analysis of the 2002 reform. See notes under Table 4 for more details on the specification and controls. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 7: Effects of the **2016** "Daddy Month" Reform on Maternal Health Outcomes in **Inpatient** Data

	(1) Any	(2) Preg. Comp.	(3) Ment.	(4) Z	(5) Z39	(6) Z76
A. Full Sample						
Reform x Birth Jan - Mar	-0.0195***	-0.00369	0.000598	-0.0197***	0.000319	-0.0210***
	[0.00468]	[0.00345]	[0.000934]	[0.00212]	[0.00133]	[0.00152]
Dep. var mean	0.0624	0.0329	0.00234	0.0121	0.00479	0.00615
Indiv. obs.	42788	42788	42788	42788	42788	42788
B. Mothers with Risky Prenatal Behaviors						
Reform x Birth Jan - Mar	-0.0219*	-0.00534	-0.00146	-0.0267***	0.00273	-0.0313***
	[0.0117]	[0.00848]	[0.00245]	[0.00570]	[0.00348]	[0.00420]
Dep. var mean	0.0751	0.0380	0.00317	0.0167	0.00621	0.00901
Indiv. obs.	8212	8212	8212	8212	8212	8212
C. Mothers with High-Risk Pregnancy Conditions						
Reform x Birth Jan - Mar	-0.0200*	-0.0139*	0.00191	-0.00784*	0.00764**	-0.0169***
	[0.0109]	[0.00797]	[0.00231]	[0.00472]	[0.00307]	[0.00323]
Dep. var mean	0.0702	0.0362	0.00291	0.0127	0.00538	0.00583
Indiv. obs.	8921	8921	8921	8921	8921	8921
D. Mothers with Labor/Delivery Complications						
Reform x Birth Jan - Mar	-0.0164**	-0.00475	0.00126	-0.0167***	0.00216	-0.0193***
	[0.00734]	[0.00543]	[0.00149]	[0.00326]	[0.00221]	[0.00215]
Dep. var mean	0.0720	0.0380	0.00271	0.0135	0.00623	0.00573
Indiv. obs.	19892	19892	19892	19892	19892	19892

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for any inpatient admission in months 0-6 after childbirth (excluding the actual childbirth). We consider indicators for any visit for any reason (column 1), as well as indicators for any visits associated with the following diagnosis codes: pregnancy or childbirth-related physical health complications (column 2); mental health (column 3); factors influencing health status and contact with health services (ICD-10 codes that begin with "Z", columns 4-6). See Appendix C for more details on the exact ICD codes for these conditions. The reported coefficients are from the RD-DD model for the analysis of the 2016 reform. We compare the differences in outcomes for fathers of first-born children born in January-March 2016 and October-December 2015 ("2016 reform sample"), relative to the difference in outcomes in the previous year (January-March 2015 versus October-December 2014, "2016 non-reform sample"). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. All regressions include controls for 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, and indicators for each parent being foreign-born. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 8: Effects of the **2016** "Daddy Month" Reform on Maternal Health Outcomes in **Outpatient** Data

	(1) Any	(2) Preg. Comp.	(3) Ment.	(4) Z	(5) Z39	(6) Z71	(7) Z76
A. Full Sample							
Reform x Birth Jan - Mar	-0.0464***	-0.0385***	-0.00642**	-0.0167**	0.00244	-0.00690***	-0.00435***
	[0.00960]	[0.00714]	[0.00305]	[0.00758]	[0.00542]	[0.00218]	[0.000812]
Dep. var mean	0.451	0.164	0.0258	0.190	0.0864	0.0127	0.00178
Indiv. obs.	42788	42788	42788	42788	42788	42788	42788
B. Mothers with Risky Prenatal Behaviors							
Reform x Birth Jan - Mar	-0.0405*	-0.0553***	-0.0172**	-0.0263	-0.000298	-0.0111**	-0.00739***
	[0.0220]	[0.0162]	[0.00867]	[0.0174]	[0.0120]	[0.00532]	[0.00220]
Dep. var mean	0.450	0.159	0.0415	0.191	0.0799	0.0142	0.00268
Indiv. obs.	8212	8212	8212	8212	8212	8212	8212
C. Mothers with High-Risk Pregnancy Conditions							
Reform x Birth Jan - Mar	-0.0184	-0.0182	-0.0117	0.00212	0.0266**	-0.00600	-0.00445***
	[0.0211]	[0.0163]	[0.00775]	[0.0173]	[0.0125]	[0.00532]	[0.00169]
Dep. var mean	0.508	0.183	0.0352	0.210	0.0962	0.0158	0.00157
Indiv. obs.	8921	8921	8921	8921	8921	8921	8921
D. Mothers with Labor/Delivery Complications							
Reform x Birth Jan - Mar	-0.0673***	-0.0570***	-0.00330	-0.0242**	-0.00128	-0.00770**	-0.00636***
	[0.0141]	[0.0115]	[0.00469]	[0.0123]	[0.00952]	[0.00364]	[0.00136]
Dep. var mean	0.536	0.208	0.0285	0.251	0.130	0.0167	0.00231
Indiv. obs.	19892	19892	19892	19892	19892	19892	19892

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for any outpatient admission in months 0-6 after childbirth. We consider indicators for any visit for any reason (column 1), as well as indicators for any visits associated with the following diagnosis codes: pregnancy or childbirth-related physical health complications (column 2); mental health (column 3); factors influencing health status and contact with health services (ICD-10 codes that begin with "Z", columns 4-7). See Appendix C for more details on the exact ICD codes for these conditions. The reported coefficients are from the RD-DD model for the analysis of the 2002 reform. See notes under Table 7 for more details on the specification and controls. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 9: Effects of the **2016** "Daddy Month" Reform on Maternal Health Outcomes in **Prescription Drug** Data

	(1) Any	(2) N05/06	(3) Anti-anx	(4) Anti-dep	(5) Pain
A. Full Sample					
Reform x Birth Jan - Mar	-0.0111	-0.00786	-0.00431	-0.00463	-0.00358
	[0.00827]	[0.00546]	[0.00303]	[0.00482]	[0.00691]
Dep. var mean	0.756	0.0893	0.0251	0.0676	0.152
Indiv. obs.	42788	42788	42788	42788	42788
B. Mothers with Risky Prenatal Behaviors					
Reform x Birth Jan - Mar	-0.0253	-0.0259*	-0.0124	-0.0197	-0.0159
	[0.0184]	[0.0143]	[0.00828]	[0.0125]	[0.0161]
Dep. var mean	0.770	0.121	0.0356	0.0869	0.158
Indiv. obs.	8212	8212	8212	8212	8212
C. Mothers with High-Risk Pregnancy Conditions					
Reform x Birth Jan - Mar	-0.00206	-0.0159	-0.00476	-0.0198*	0.0161
	[0.0157]	[0.0135]	[0.00761]	[0.0118]	[0.0166]
Dep. var mean	0.835	0.118	0.0334	0.0865	0.194
Indiv. obs.	8921	8921	8921	8921	8921
D. Mothers with Labor/Delivery Complications					
Reform x Birth Jan - Mar	-0.0168	-0.00367	-0.00577	0.00361	-0.0251**
	[0.0112]	[0.00847]	[0.00475]	[0.00753]	[0.0118]
Dep. var mean	0.806	0.101	0.0288	0.0774	0.226
Indiv. obs.	19892	19892	19892	19892	19892

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for any prescription drug claim observed for the mother in months 0-12 after childbirth. We consider indicators for any prescription drug claim (column 1), any psycholeptic or psychoanaleptic drug claim (ATC codes that begin with "N05" or "N06", column 2), any anti-anxiety drug claim (column 3), any antidepressant drug claim (column 4), any pain-related drug claim (column 5), any birth control drug claim (column 6). See Appendix C for exact ATC codes for these conditions. The reported coefficients are from the RD-DD model for the analysis of the 2016 reform. See notes under Table 7 for more details on the specification and controls. Robust standard errors in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

ONLINE APPENDIX

A Additional Results

Table A1: Means of Parental Leave Variables and Background Characteristic Variables

	2002 Reform		2002 Non-Reform		2016 Reform		2016 Non-Reform	
	01-03'02	10-12'01	01-03'03	10-12'02	01-03'16	10-12'15	01-03'15	10-12'14
Parental Leave Variables								
Any leave (father)	.91	.89	.91	.91				
Any leave, months 0-1 (father)	.08	.07	.07	.08				
Any leave, months 0-12 (father)	.54	.50	.54	.53				
WR days, months 0-1 (father)	2.43	1.85	1.79	2.01				
WR days, months 0-12 (father)	27.84	22.96	28.92	26.40				
WR days, total (father)	76.79	59.87	78.83	79.65				
Any leave (mother)	1.00	1.00	1.00	1.00				
WR days, total (mother)	264.56	258.92	260.39	262.71				
Observations	10057	9031	10062	9773				
Background Characteristics								
Mother income (1000s)	189.07	183.69	193.90	192.34	238.13	226.68	228.84	219.93
Father income (1000s)	262.63	245.90	262.17	256.54	315.21	294.26	301.94	281.69
Mother age	28.80	28.24	29.12	28.29	29.47	28.76	29.39	28.72
Father age	31.64	31.20	31.91	31.20	32.40	31.78	32.28	31.75
Mother married	.33	.27	.33	.25	.33	.24	.33	.25
Mother low education	.56	.55	.54	.54	.41	.42	.43	.42
Father low education	.63	.62	.61	.62	.55	.57	.56	.55
Observations	9938	8893	10132	9644	11537	10963	11124	10838

Notes: This table reports the means of parental leave variables for fathers and mothers for the 2002 reform, as well as family background characteristics for both reforms. We present means separately for each of our pre- (October-December) and post- (January-March) groups in the reform and non-reform samples. See text for more details.

Table A2: The **2002** "Daddy Month" 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	(9) Pred. Index
Reform x Birth Jan - Mar	0.00737	0.0118	0.00192	0.000901	-0.263***	-0.277**	3822.8	11108.9**	-0.000229
	[0.0101]	[0.00988]	[0.00739]	[0.00754]	[0.101]	[0.121]	[2808.9]	[4443.0]	[0.000318]
Dep. var mean	0.546	0.622	0.156	0.163	28.63	31.50	189941.4	257179.8	0.0835
Indiv. obs.	38607	38607	38607	38607	38607	38607	37955	37839	37307

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 last column shows results where we use a predicted index of parental characteristics, which we compute by regression an indicator for any inpatient visit in months 0-12 post childbirth month on these characteristics and estimating the predicted value, which we use as the outcome. The reported coefficients are from the RD-DD model for the analysis of the 2002 reform. We compare the differences in outcomes for fathers of first-born children born in January-March 2002 and October-December 2001 ("2002 reform sample"), relative to the difference in outcomes in the following year (January-March 2003 versus October-December 2002, "2002 non-reform sample"). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. Robust standard errors in parentheses.

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

Table A3: The **2016** "Daddy Month" 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	(9) Pred. Index
Reform x Birth Jan - Mar	-0.0174*	-0.0204**	0.0210***	0.00622	0.0368	0.0960	2491.6	712.9	-0.000444**
	[0.00937]	[0.00942]	[0.00806]	[0.00816]	[0.0932]	[0.118]	[3041.7]	[3899.4]	[0.000185]
Dep. var mean	0.421	0.558	0.236	0.245	29.09	32.06	228642.1	298658.8	0.0624
Indiv. obs.	44462	44462	44462	44462	44462	44462	43520	43562	42788

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 last column shows results where we use a predicted index of parental characteristics, which we compute by regression an indicator for any inpatient visit in months 0-6 post childbirth month on these characteristics and estimating the predicted value, which we use as the outcome. The reported coefficients are from the RD-DD model for the analysis of the 2016 reform. We compare the differences in outcomes for fathers of first-born children born in January-March 2016 and October-December 2015 ("2016 reform sample"), relative to the difference in outcomes in the preceding year (January-March 2015 versus October-December 2014, "2016 non-reform sample"). See equation (1) in the text for more details. We report the coefficient and standard error on the interaction between being born in January-March and being in the reform sample. Robust standard errors in parentheses.

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

Table A4: Effects of the **2002** "Daddy Month" Reform on Maternal Health Outcomes, Robustness

	(1) Preg. Comp. (IN)	(2) Z39 (IN)	(3) Z71 (OUT)	(4) Pain (DRUGS)
A. DD Oct - Mar				
Reform x Birth Jan - Mar	-0.00925**	-0.0195***	-0.0185***	-0.0234**
	[0.00419]	[0.00206]	[0.00215]	[0.0115]
Dep. var mean	0.0424	0.00970	0.00994	0.185
Indiv. obs.	37307	37307	37307	18159
B. RD-DD Jul - Jun				
Reform x Birth Jan - June	-0.00232	-0.0197***	-0.0188***	-0.0190**
	[0.00289]	[0.00139]	[0.00142]	[0.00797]
Dep. var mean	0.0421	0.00928	0.00928	0.184
Indiv. obs.	77465	77465	77465	37562
C. Maternal FE				
Post Jan 2002	-0.0106**	-0.00866***	-0.0122***	0.00127
	[0.00429]	[0.00200]	[0.00208]	[0.0100]
Dep. var mean	0.0370	0.0103	0.00956	0.183
Indiv. obs.	82339	82339	82339	35319

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for: (1) any inpatient admission in months 0-12 after childbirth for pregnancy or childbirth-related complications, (2) any inpatient admission in months 0-12 after childbirth with ICD-10 code Z39, (3) any outpatient admission in months 0-12 after childbirth with ICD-10 code Z71, and (4) any consumption of painkillers 5-7 years post-childbirth among mothers with labor/delivery complications. See Appendix C for more details on the exact ICD and ATC codes for outcomes. Panel A presents coefficients from a DD model, using data on births in October through March only. Panel B presents coefficients from an RD-DD model using an expanded sample of births in a 6-month bandwidth: we compare the differences in outcomes for fathers of first-born children born in January-June 2002 and July-December 2001, relative to the difference in outcomes in the following year (January-June 2003 versus July-December 2002). Panel C presents coefficients from a maternal fixed effects model that uses data on all siblings born in January 2001 through December 2004. See the text for more details. Regressions in Panels A and B include controls 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, and indicators for each parent being foreign-born. Panel C includes controls for the following time-varying characteristics: number of months between the focal child and the previous birth (0 for firstborns), maternal and paternal earnings, indicators for each parent's age groups, indicators for each parent's education levels, an indicator for the parents being married, birth month fixed effects, and fixed effects for the child's birth order. Panels A and B use robust standard errors; while standard errors in Panel C are clustered on the mother.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A5: Effects of the **2016** "Daddy Month" Reform on Maternal Health Outcomes, Robustness

	(1) Z76 (IN)	(2) Preg. Comp. (OUT)	(3) Ment. (OUT)	(4) Z71 (OUT)	(5) Pain (DRUGS)
A. RD-DD Oct - Mar, No Foreign-Born Moms					
Reform x Birth Jan - June	-0.0214***	-0.0362***	-0.00825**	-0.00704***	-0.0286**
	[0.00171]	[0.00802]	[0.00360]	[0.00247]	[0.0135]
Dep. var mean	0.00622	0.162	0.0285	0.0130	0.227
Indiv. obs.	33617	33617	33617	33617	15363
B. DD Oct - Mar					
Reform x Birth Jan - Mar	-0.0210***	-0.0384***	-0.00644**	-0.00689***	-0.0249**
	[0.00152]	[0.00714]	[0.00305]	[0.00217]	[0.0118]
Dep. var mean	0.00615	0.164	0.0258	0.0127	0.227
Indiv. obs.	42788	42788	42788	42788	15363
C. RD-DD Jul - Jun					
Reform x Birth Jan - June	-0.0217***	-0.0319***	-0.00364*	-0.00209	-0.0157*
	[0.00105]	[0.00494]	[0.00207]	[0.00150]	[0.00807]
Dep. var mean	0.00620	0.164	0.0248	0.0128	0.219
Indiv. obs.	89336	89336	89336	89336	41559
D. Maternal FE					
Post Jan 2016	-0.00649***	0.00493	-0.00250	-0.00335	0.00618
	[0.000943]	[0.00650]	[0.00206]	[0.00207]	[0.00576]
Dep. var mean	0.00207	0.118	0.0151	0.00983	0.0748
Indiv. obs.	103636	103636	103636	103636	41238

Notes: Each column in each panel reports coefficients from separate regressions. The outcomes are indicators for: (1) any inpatient admission in months 0-6 after childbirth with ICD-10 code Z76, (2) any outpatient admission in months 0-6 after childbirth for pregnancy or childbirth-related complications, (3) any outpatient admission in months 0-6 after childbirth with a mental health diagnosis, (4) any outpatient admission in months 0-6 after childbirth with ICD-10 code Z71, (5) any consumption of painkillers in months 0-12 after childbirth among mothers with labor/delivery complications. See Appendix C for more details on the exact ICD and ATC codes for outcomes. Panel A presents coefficients from our baseline RD-DD model, but using a sample that drops all foreign-born mothers. See notes under Appendix Table A4 for more details on specifications and controls in the other panels. Panels A, B, and C use robust standard errors; while standard errors in Panel D are clustered on the mother.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

B More Details about Matching Families to Parental Leave Claims

The data on parental leave claims from the Swedish Social Insurance Agency uses a different child identifier than the administrative data on family demographic variables from Statistics Sweden. However, the parent identifiers are consistent between the two data sets. In order to match each parental leave claim to a specific child in the family demographics data, we used the following matching algorithm. First, we identify each parent in the parental leave claims data as either a mother or a father. For each mother, we keep the first leave spell made for each child and sort them by the start date. From the family demographics data we determine the birth order of each of the motherâs children. If the mother had only one child, her very first parental leave spell is attributed to that child. If she has more than one child, the very first leave spell is attributed to her first child if it starts no later than 2 months before the birth of the second child. Otherwise, we attribute a mother's i^{th} first parental leave spell to child of birth order x if leave spell i starts earlier than two months before the birth of child number $x+1$ and either (1) leave spell $i-1$ is assigned to child number $x-1$, or (2) leave spell i starts no earlier than 2 months before the birth of child x. We do not attempt to match non-singleton children (i.e., twins, triplets, etc.) but account for their birth order. We repeat the same procedure for fathers. In cases where the child match made on the mother differs from that made on the father, we keep only the child match made on the mother. Additionally, for our analysis sample, we drop all children of parents who ever had a child born outside of Sweden. We match 96 percent of all children born in 1993-2003 in Sweden in families whose first child was born in or after 1993 and who never gave birth to a child outside of Sweden ($\frac{696{,}027}{724{,}960}$). Restricting the sample further to only first-born children in the reform and non-reform samples for the analysis of the 2002 reform, we match 99 percent of the firstborn children born in October 2001 - March 2003 ($\frac{39{,}071}{39{,}421}$).

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 month windows 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)
- Factors influencing health status and contact with health services (Z00-Z99); we find the following codes to be affected:
  - Encounter for maternal postpartum care and examination (Z39)
  - Persons encountering health services for other counseling and medical advice, not elsewhere classified (Z71)
  - Persons encountering health services in other circumstances (Z76)

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:

- Psycholeptics and psychoanaleptics: ATC code begins with "N05" or "N06"
- Depression: ATC code begins with "N06A"
- Anxiety: ATC code begins with "N05B"
- Pain (analgesic): ATC code begins with "N02"