The Impact of Paid Maternity Leave on Maternal Health*

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Abstract

We examine the impact of the introduction of paid maternity leave in Norway in 1977 on maternal health in the medium- and long-term. Using administrative data combined with survey data on the health of women around age 40, we find the reform improved a range of maternal health outcomes and increased health-promoting behaviors. The effects were larger for first-time and low-resource mothers and women who would have taken little unpaid leave in the absence of the reform. We study the health effects of subsequent expansions in paid maternity leave and find evidence of diminishing returns to leave length.
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1 Introduction

Across OECD countries, there is substantial variation in maternity leave benefits. In the United States, the Family and Medical Leave Act of 1993 guarantees 12 weeks of unpaid leave for eligible mothers, but no paid leave.[1] In contrast, in most other high-income countries, there has been an increase in paid maternity leave benefits over the last several decades. For example, prior to 1977, only 12 weeks of unpaid leave were available to working mothers in Norway, but currently, women are entitled to almost a full year of paid leave and an additional year of unpaid job protection after the birth of a child. To comprehensively assess maternity leave policies and determine the case for expanded paid leave, one must consider the impact of these policies on the outcomes of children, mothers, and families.

There is a large literature that estimates the effects of maternity leave reforms on maternal employment and earnings as well as a variety of short and long-term outcomes of children, such as health and cognitive development. However, there is little evidence on the causal effects of maternity leave on maternal health, which is surprising given one of the main motivations for maternity leave provisions is to allow women

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[1] There are no federally-funded paid leave entitlements in the US, though some states provide paid leave benefits.

to recover from childbirth. A priori, the effect is unclear. On one hand, returning to work shortly after giving birth may have negative effects on the health of mothers if employment increases stress or detracts from time the woman spends caring for herself and recovering from the physical effects of childbirth. On the other hand, employment may bring psychic benefits to the mother and increase household income, which may improve health. The potential endogeneity of maternity leave uptake and length with respect to maternal health makes this a difficult question to answer. For example, there may be unobservable attributes that impact both maternal health and a woman's maternity leave uptake and duration decisions, or there may be a reverse causality problem if postpartum health influences the return-to-work decision.

We overcome these challenges by examining the impact of a reform that introduced paid maternity leave benefits in Norway in July 1977. Before the reform, mothers were eligible for 12 weeks of unpaid leave and no paid leave. Mothers giving birth after July 1, 1977 were entitled to 4 months of paid leave and 12 months of unpaid leave. We combine Norwegian birth registry data with survey data containing both medically-documented and self-reported health measures of mothers around age 40, including body mass index (BMI), blood pressure, cholesterol levels, diabetes, self-reported pain, and self-reported physical and mental health, as well as health behaviors like smoking and exercise. We estimate the impact of the 1977 reform on medium- and long-term maternal health using a regression discontinuity design, comparing outcomes of mothers who had children just after and just before July 1, 1977. We also use data on women who gave birth in the years around the maternity leave change and employ a difference-in-regression discontinuity design as in Carneiro et al. (2015) to address concerns about potential differences in the outcomes of mothers who gave birth in June and July 1977 that are unrelated to the reform (i.e. month-of-birth effects).

We find the 1977 reform was protective of maternal health. Various aspects of metabolic health improved for mothers who were eligible for the reform, including BMI, blood pressure, and a summary index that aggregates the measures of metabolic health. The reform decreased the probability of experiencing pain around age 40, particularly musculoskeletal pain. We find significant improvements in self-reported mental and general health as well as increases in health-promoting behaviors, such as exercise and not smoking. The effects are robust to adjusted inference for multiple hypothesis testing. We then analyze whether there were heterogeneous effects across various subgroups of women. The reform had larger effects on mothers who experienced complications at delivery, first-time mothers, and low-resource mothers (single mothers and those with below-median household income).

Using information on maternal income in the years around when the mother gave birth, we explore whether unpaid leave changed in response to the reform. We find the reform did not crowd out unpaid leave and did not significantly impact maternal income. This implies more time at home after childbirth, not income effects, drives the health improvements. Furthermore, the improvements were larger for women who would have taken little unpaid leave in the absence of the reform, a group which includes many low-resource mothers. Thus, the additional time at home was particularly beneficial for disadvantaged women who could not afford to take much unpaid time off work after childbirth.

Consistent with the idea that more time at home is an important channel, we hypothesize that increased breastfeeding duration plays a role in generating the health improvements. There is a large public health literature documenting associations between breastfeeding and various maternal health benefits, such as decreased risk of breast and ovarian cancer, diabetes, postpartum depression, and cardiovascular disease (see for example Ip et al. 2007, Eidelman et al. 2012), as well as studies that estimate a causal link between maternity leave and breastfeeding duration (Baker and Milligan 2008b, Huang and Yang 2015, Kottwitz et al. 2016). We cannot explicitly examine changes in breastfeeding due to lack of data. However, we consider the impact of the reform on sickness absence and find a decrease in absence taken due to breast and ovarian cancers among women in their fifties. Thus, we find some support for the idea that breastfeeding may be an important channel.

We then briefly explore how a series of expansions in paid parental leave that occurred in Norway between 1987 and 1992 impacted maternal health around age 40. We find weak evidence that the first few expansions, which each increased paid leave by 2 weeks, generated improvements in most of the outcomes that were impacted by the 1977 reform. The later expansions led to no further improvements. These results suggest there are diminishing returns to paid maternity leave length and that the introduction of paid maternity leave generates larger benefits than expansions, at least at the levels we consider. Our finding of a non-monotonic relationship between paid maternity leave length and maternal health improvements complements the literature that finds short and moderate leave durations are associated with higher female employment while longer leave may adversely impact female employment (Olivetti and Petrongolo 2017, Rossin-Slater 2017).

We contribute to the sparse literature that estimates the causal relationship between maternity leave and maternal health in a variety of ways (see for example Chatterji and Markowitz 2005, 2012, Baker and Milligan 2008b, Beuchert et al. 2016, Guertzgen and Hank 2018).[2] First, our data contains a large and comprehensive set of health outcomes, including self-reported measures as well as biomarkers from medical examinations (e.g., blood pressure, cholesterol). Thus, we analyze the impact of maternity leave on many aspects of health. The biomarkers we consider predict well a variety of future health conditions, and they allow us to learn more about the mechanisms through which maternity leave affects maternal health than other studies. Second, we observe the health of mothers around age 40, which allows us to analyze the effects of maternity leave potentially several years after the woman gave birth. Our results are informative, therefore, for understanding the medium- and long-term effects of paid maternity leave. For the most part, the prior literature has focused on maternal health shortly after childbirth. Third, our sample includes mothers of all types (first time, non-first time, single, married, etc.) who gave birth in Norway during the time frame we consider. Prior studies often focus on selected samples of mothers such as new mothers, married mothers, or currently employed mothers. We overcome some of the limited generalizability of these studies. Fourth, parental leave policies are currently under debate in the United States, and the reform we consider changed maternity leave benefits when they were at a very low level, similar to benefits in the United States today. Our results, therefore, may inform the current debate over family leave policy. Last, we mainly focus on the 1977 reform, but we also consider expansions in paid leave, enabling us to study differential effects of the introduction versus the expansion of maternity leave in the same country setting.

[2] We discuss this literature in detail in the next section.

Our findings complement the documented improvements in children's outcomes that result from the introduction of maternity leave programs (Rossin-Slater 2017). Carneiro et al. (2015) find the 1977 Norwegian reform had no impact on mothers' short- or long-term employment or income, but it improved children's educational attainment and earnings at age 30. They attribute their findings to increased early time investments by mothers in their children. We find mothers were physically and mentally healthier as a result of the reform, which may have allowed them to make even more time investments and/or make higher-quality investments. Thus, there may be important effects of maternity leave on children that occur through improved maternal health.

The paper proceeds as follows. Section 2 reviews the related literature. In Section 3, we provide background on the 1977 maternity leave reform. Section 4 describes the data and Section 5 presents our empirical strategy. We discuss our results in Section 6 and perform a variety of sensitivity analyses in Section 7. We provide a brief conclusion in Section 8.

2 Related Literature

2.1 Maternity Leave and Maternal Health

Several studies examine the effects of maternity leave reforms on children's outcomes, such as health, cognitive development, and educational attainment, across a variety of countries and institutional settings.[3] Another line of literature analyzes the impact of maternity leave on maternal employment and earnings.[4] However, in the economics literature, there are few studies that estimate the effects of maternity leave on maternal health, and the results are mixed. We briefly summarize these studies below.[5]

Chatterji and Markowitz (2005) examine how maternity leave length affects maternal health in the United States using a sample of women who returned to work within 6 months after giving birth in 1988. They consider self-reported measures of depression 6 to 24 months after giving birth and outpatient visits during the first 6 months after childbirth. To address the potential endogeneity of leave length, they exploit variation in state-level maternity leave policies.[6] They find longer maternity leave (paid or unpaid) is significantly associated with decreased depressive symptoms. In related work, Chatterji and Markowitz (2012) examine the association between maternity leave length and mental and physical health 9 months after giving birth in 2001. When they control for the potential endogeneity of leave length using cross-sectional variation in local labor market conditions, costs of child care, and state maternity leave

[3]See for example Ruhm (2000), Tanaka (2005), Baker and Milligan (2008b, 2010), Liu and Skans (2010), Rasmussen (2010), Rossin (2011), Dustmann and Schönberg (2012), Carneiro et al. (2015), and Dahl et al. (2016).

[4]See for example Baker and Milligan (2008a), Lalive and Zweimüller (2009), Lalive et al. (2014), Schönberg and Ludsteck (2014), and Stearns (2016).

[5]There are several studies in the public health literature that focus on postpartum employment and maternal physical and mental health. Within this literature, some studies specifically analyze the relationship between maternity leave duration and maternal health. However, they are largely correlational studies using very narrow samples and results are mixed. See for example Gjerdingen et al. (1993), Hyde et al. (1995), Saurel-Cubizolles et al. (2000), and Dagher et al. (2014) as well as Staehelin et al. (2007) and Aitken et al. (2015) for reviews of the literature.

[6]The authors acknowledge that most of the maternity leave variation in their sample is small, which makes it difficult to evaluate substantial changes in leave policy, such as the reform we consider.

policies as instruments, they find taking more than 12 weeks of total leave is associated with a decrease in depressive symptoms, and taking more than 8 weeks of paid leave is associated with an improvement in overall self-reported health.

Baker and Milligan (2008b) find an increase in paid maternity leave from a maximum of 25 weeks to 50 weeks in Canada had no impact on self-reported health, depression, or other postpartum problems (e.g., hemorrhage, infection, hypertension) among mothers 7 to 24 months after giving birth. Avendano et al. (2015) exploit changes in maternity leave benefits over time within a subset of European countries (excluding Norway) to study the impact of such policies on maternal mental health in older age using data from the Survey of Health, Ageing and Retirement in Europe (SHARE). They focus on first-time mothers aged 16 to 25 when they gave birth, and find more generous leave policies are associated with reduced depressive symptom scores later in life (i.e. at ages 50 and over).[7]

Two recent studies exploit sharp changes in access to maternity leave benefits (as we do) to analyze the impact of paid leave expansions on maternal health. Beuchert et al. (2016) focus on a reform in Denmark in 2002 that increased the number of weeks of parental leave with full benefit compensation. They examine how maternity leave length impacted outcomes related to health care utilization 1 to 5 years after childbirth, including inpatient hospital admissions, outpatient hospital visits, emergency department visits, and antidepressant prescriptions. Mothers benefited from increased leave in terms of fewer inpatient and outpatient hospital admissions, but the remaining outcomes were unaffected. The average length of maternity leave prior to the Danish reform was 244 days and it increased by about 32 days after the reform. Thus, they consider an expansion in leave from a baseline level that was already quite generous, which may explain the limited beneficial impacts on maternal health. Guertzgen and Hank (2018) estimate the impact of an expansion in paid leave in Germany from 2 to 6 months in 1979 on mothers' long-term sickness absence (i.e. spells greater than 6 weeks) up to three decades after childbirth. They find mothers who were impacted by the extension and returned to work had a higher incidence of sickness absence compared to unaffected mothers 3 years after childbirth, but no evidence of significant medium- or long-run effects.[8]

Our paper is also related to Carneiro et al. (2015) and Dahl et al. (2016), which exploit Norwegian maternity leave reforms as exogenous sources of variation in maternity leave length in a regression discontinuity framework. Carneiro et al. (2015) focus on the outcomes of children born to mothers affected by the 1977 reform and find the reform led to a decline in children's high school dropout rates and an increase in their wages at age 30. Dahl et al. (2016) consider the 6 expansions in paid leave that occurred between 1987 and 1992 and find other than mothers' time spent at home after childbirth, the expansions had little effect on a variety of outcomes, including parental earnings and labor market participation, completed fertility, marriage,

[7] Liu and Skans (2010) study how the duration of paid parental leave affects children's academic performance in Sweden using a reform that extended leave benefits in 1988. To understand the underlying mechanisms, they analyze the effects of leave on intermediate outcomes including maternal mental health as measured by mental health-related hospital admittances. They find the reform did not significantly affect such admittances.

[8] They attribute their findings to the expansion particularly inducing those with poor pre-birth health to reenter the labor market. Carneiro et al. (2015) point out that the German reform was less generous than the 1977 Norwegian reform because the benefit payments in the expansion period (from the third to the sixth month after childbirth) corresponded, on average, to only one-third of average pre-birth income. As a result, there was only a small decrease in maternal labor supply due to the reform.

and divorce.[9] Neither Carneiro et al. (2015) nor Dahl et al. (2016) examine maternal health effects.

Our paper contributes to and expands this strand of literature in several ways. First, we consider an array of health outcomes, including self-reported measures and medically-documented biomarkers, which allow us to analyze the effect of paid maternity leave on many dimensions of maternal health. Having information on biomarkers is unique and enables us to explore the mechanisms through which maternity leave affects health at a more detailed level than other studies. Second, given that we observe mothers' health around age 40, we examine medium- and long-term effects of leave benefits. Though there are some exceptions (Avendano et al. 2015, Guertzgen and Hank 2018), prior work has predominantly focused on short-term health effects. Third, the administrative data we use includes mothers of all types who gave birth during the time period we consider. The above-mentioned studies often focus on very selected samples of mothers. Chatterji and Markowitz (2005) only consider mothers who returned to work within 6 months postpartum; Baker and Milligan (2008b) do not include single mothers; Chatterji and Markowitz (2012) and Avendano et al. (2015) only consider new mothers; and Guertzgen and Hank (2018) focus on employed mothers. We improve upon the limited generalizability of these prior studies. Last, most prior work considers expansions in paid leave from an already generous level. We focus on a reform that introduced paid maternity leave. We also explore the subsequent expansions in paid leave considered in Dahl et al. (2016), allowing us to estimate the maternal health effects of the introduction and expansions of paid maternity leave in one institutional setting.

2.2 Postpartum Health

A number of studies in the public health literature document the frequency and duration of physical and mental health problems after childbirth.[10] These studies show that postpartum health problems are common, with some concluding that full recovery from childbirth can take more than 6 months. Generally, studies do not consider health beyond 1 or 2 years postpartum. Thus, the long-term effects of postpartum health problems are not well studied.

Cheng and Li (2008) review 22 studies and document the prevalence of various postpartum health conditions. They find most women encounter at least one health problem within a year after childbirth, with fatigue being one of the most frequent and persistent conditions experienced. They also find that many women experience backache, headache, and pain associated with a cesarean section.

Postpartum weight retention is a common health concern for mothers, especially given the medical conditions associated with being overweight or obese. Average postpartum weight retention ranges from 0.5 to 3 kilograms (Gore et al. 2003), and some studies find up to 20 percent of women retain 5 kilograms or more 6 to 18 months postpartum (Gunderson and Abrams 1999). High gestational weight gain is an important risk factor for high postpartum weight retention in the short- and long-term (Siega-Riz et al. 2009, Nehring et al. 2011). There is a widely held belief that breastfeeding promotes postpartum weight loss, but the evidence is inconsistent (see for example Olson et al. 2003, Baker et al. 2008, Neville et al. 2014). This is due in part to the fact that

[9] The expansions were substantially smaller than the 1977 reform, increasing paid leave by 2 to 4 weeks.

[10] See for example Gjerdingen et al. (1993), Brown and Lumley (1998, 2000), Albers (2000), Saurel-Cubizolles et al. (2000), Thompson et al. (2002), and Woolhouse et al. (2014).

many studies on breastfeeding and postpartum weight change are observational or prospective cohort studies that often fail to control for confounding factors such as age and pre-birth weight. The results also depend on the duration and intensity of breastfeeding and when during the postpartum period mothers are observed.

Postpartum mental health, particularly depression, has been widely studied. O'Hara and Swain (1996) perform a meta-analysis and conclude that the prevalence of postpartum depression is 13 percent. Another meta-analysis finds that 19 percent of women have a depressive episode during the first 3 months postpartum (Gavin et al. 2005). In a US national survey, over a third of women who gave birth in the past year reported suffering some depressive symptoms in the 2 weeks prior to the survey, with about 20 percent reporting that they consulted a health professional regarding their mental well-being since giving birth (Declercq et al. 2014). Those who experience a postpartum depressive episode have a higher likelihood of depression recurrence (Miller 2002).

3 1977 Maternity Leave Reform

In 1956, maternity leave benefits were granted to women in Norway for the first time.[11] The benefits provided eligible mothers with up to 12 weeks of unpaid maternity leave. Hence, women were entitled to the same level of leave currently granted by the Family and Medical Leave Act of 1993 in the United States, which provides up to 12 weeks of job-protected unpaid leave to individuals working for at least one year at a firm with 50 or more employees.

Paid maternity leave was instituted in Norway on July 1, 1977. The new law gave parents universal right to 18 weeks of paid leave with job protection before and after childbirth. The income replacement rate was 100 percent (of pre-birth income from wages) for 18 weeks. Of the 18 weeks, 6 had to be taken by the mother, and the remaining weeks could be shared between mothers and fathers. However, almost no fathers took leave (Rønsen and Sundström 2002). In addition to providing paid leave benefits, the 1977 reform increased unpaid leave, allowing parents to take up to 1 year of job-protected unpaid leave. Whether a mother was eligible for leave benefits depended on her work and income history. Women who earned more than 10,000 Norwegian kroner (NOK) annually and worked at least 6 of the 10 months immediately prior to childbirth were eligible.[12]

For our empirical strategy, it is important that mothers could not change their eligibility status after the reform was announced. As explained in Carneiro et al. (2015), the reform was largely unexpected and introduced at the end of the sitting Parliament's term along with several other legislative changes.[13] The government report on the reform was made official on April 15, 1977 and approved on June 13 that year. National newspapers did not report on the reform prior to June 1977 (Carneiro et al. 2015). Thus, women who gave birth immediately after the reform went into effect were already pregnant when the law was announced. Furthermore, eligibility required working 6 of the 10 months prior to giving birth, making

[11] Our discussion of maternity leave in Norway and the 1977 reform follows from Carneiro et al. (2015).

[12] 10,000 NOK in 1977 is approximately $5,600 in 2018.

[13] We examined the other legislative changes that occurred in 1977 during the end of Parliament's term and did not identify any that may have also impacted maternal health. During this general period, we identified one relevant legislative change—an abortion law that went into effect on January 1, 1976 that made it easier for women to have an abortion within 12 weeks of conception. The first cohort of children affected by this reform was born around July 1976 (Carneiro et al. 2015). For this reason, we do not include women who gave birth in 1976 in the control group in our difference-in-regression discontinuity specifications.

it difficult to change eligibility status in the short-term.

4 Data

We use the Norwegian Registry data, a linked administrative dataset that covers the Norwegian population up to 2012. The data provide information about labor market status, educational attainment, and demographics. We merge this data to the datasets described below using personal identification numbers.

4.1 Birth Data

The data on births are obtained from the Medical Birth Registry of Norway, which contains records for all births as long as the gestation period was at least 16 weeks. The records include information on year and month of birth, age of the mother, and other variables related to infant health and the birth experience, such as whether there were complications at birth or a cesarean section was performed.

4.2 Health Data

The data on mothers' health come from the Cohort of Norway (CONOR) data and the National Health Screening Service's Age 40 Program data, two population-based and nationwide surveys carried out from 1988 to 2003 by the National Institute of Public Health. The information contained in both surveys was gathered through questionnaires and short health examinations. For the most part, the same information was collected in both surveys. In particular, questions were asked about general health, specific diseases, pharmaceutical use, physical activity, and smoking and drinking habits. The health examination component was conducted by medical professionals and provides us with detailed biomarker information, including data from blood tests.

The goal of the Age 40 Program was to survey all men and women aged 40 to 42 between 1988 and 1999. It covered all counties in Norway except Oslo, with a response rate between 55 and 80 percent, yielding 374,090 observations. The CONOR survey was carried out between 1994 and 2003 and included Oslo, Norway's capital and largest city. This dataset includes 56,863 respondents from a somewhat wider set of age groups. We include individuals from the CONOR survey who were between 39 and 42 years old at the time of the survey.[14]

The data allow us to analyze self-reported health measures of mothers as well as biomarkers, such as weight, blood pressure, and cardiac and cholesterol risk. Biomarkers are correlated with higher stress levels, are useful in detecting deteriorations in health before specific diseases or conditions present themselves, and are predictive of a variety of future health conditions (Evans and Garthwaite 2014). Observing both self-reported health measures and biomarkers allows us to comprehensively estimate the effect of paid maternity leave on mothers' health.

We analyze several health measures and biomarkers related to "metabolic syndrome," including obesity, diabetes, diastolic blood pressure, and cardiac and cholesterol risk. An individual is defined as obese if his or her body mass index (BMI) is higher than 30 kilograms per meter squared (kg/m^2). We create an indicator for whether an individual has diabetes (either type 1 or 2). Cardiac risk is an indicator for whether a woman's triglyceride (a type of fat found in blood) level is above 2.3 millimoles per liter (mmol/L).

[14] Black et al. (2015) provide a detailed description of the health data and representativeness of the sample.

Cholesterol risk is an indicator for whether her total serum cholesterol level is above 6.2 mmol/L. These cutoffs are based on international health guidelines. Obesity, diabetes, high blood pressure, high cholesterol, and high triglycerides are major risk factors for heart disease and cardiac events. High blood pressure is also predictive of stroke and kidney failure.

We consider each measure of metabolic health separately. In addition, we follow Kling et al. (2007) and aggregate the variables related to BMI, blood pressure, diabetes, and cardiac and cholesterol risk into a summary standardized index, which we refer to as the metabolic syndrome index. This index is an average across standardized z-score measures of each health outcome or biomarker. The z-score is calculated by subtracting the mean and dividing by the standard deviation.[15] Aggregating the measures in this way improves statistical power (Kling et al. 2007). All of the components of the metabolic syndrome index are 'bads' (e.g., diabetes, cardiac risk). Hence, a decrease in the index indicates an improvement in metabolic health.

We also consider measures of self-reported health. We create a summary standardized index for mental health. Individuals are asked separate questions about how nervous, anxious, depressed, irritated, lonely, calm, and happy they were during the last 2 weeks. They could respond with {no, a little, quite a bit, a lot}. We follow Black et al. (2016) and for nervous, anxious, depressed, irritable, and lonely, code the answers as {1, 2, 3, 4}, respectively. For calm and happy, we code the answers as {4, 3, 2, 1}. Thus, higher values for each component of the index imply poorer mental health, and a decrease in the index indicates an improvement in mental health.[16] We also include a summary index for self-reported general health consisting of 2 components. Individuals are asked to assess their overall health and can respond with {poor, not so good, good, very good}. Individuals are also asked about satisfaction with their health and can respond on a 0 to 10 scale, with higher numbers indicating more satisfaction. For ease of comparison with the other indices, we code the components of the general health index such that a decrease in the index reflects an improvement in general health. Analogous to the metabolic syndrome index, both indices are an average across standardized z-score measures of each outcome included in the index.

Both health surveys include questions about whether respondents faced pain or stiffness that lasted at least 3 months and where the pain occurred. We create an indicator for reporting any pain around age 40 as well as indicators for certain types of pain, such as back pain.

Finally, we analyze health behaviors around age 40. We create an indicator for whether a woman reports that she smokes daily. Individuals are asked about weekly physical activity they engage in during leisure time and select from the following 4 mutually exclusive options: (1) sedentary activities like reading and watching

[15] We follow the approaches of Kling et al. (2007) and Hoynes et al. (2016) for randomized and quasi-experimental settings and use the control group mean and standard deviation when calculating the z-scores. That is, we use the mean and standard deviation of mothers who gave birth before July 1, 1977. In the difference-in-regression discontinuity specifications where we additionally include mothers who gave birth in the years surrounding the reform, we use the mean and standard deviation of each birth year's equivalent "control" group. For example, for mothers who gave birth in 1975, we use the mean and standard deviation of the mothers who gave birth before July 1, 1975.

[16] We only consider the mental health index and not its individual components. We do this because most measures of mental health, such as the CES-D scale, are aggregate measures constructed from several symptoms. The mental health index constructed from the health survey data has been shown to correlate highly with previously validated mental health indices such as the Hopkins Symptom Checklist (HSCL-10) and the Hospital Anxiety and Depression Scale (HADS) (Søgaard et al. 2003).

television; (2) light activities like walking and cycling; (3) moderate activities and sports like running, swimming, and cross-country skiing; (4) vigorous activities like hard exercise and competitive sports. We create an exercise variable that takes on values $\{1, 2, 3, 4\}$ with higher values indicating increased physical activity.

4.3 Sickness Absence Data

Sickness insurance in Norway covers all individuals who have been employed at the same employer for at least 4 weeks. The replacement rate is 100 percent up to an amount of 6G (approximately $85,000 in 2013) from the first day of absence up to 1 year.[17] For absences lasting more than 3 days, medical certification is required. The employer covers the first 16 days of sickness absence, and from day 17 onwards, the Social Security Administration covers the benefits.

The data on sickness leave is reported by the Social Security Administration. For all certified sickness-related absence spells, we have information on the start and end dates from 1995 to 2014. For each absence spell, we observe the main diagnosis from ICPC-2 codes. We consider sickness absences related to cardiovascular, musculoskeletal, and psychological diagnoses as well as breast and ovarian cancer.[18,19] We examine the effect of the reform on the probability of taking certified sickness leave at some point between ages 40-45 and ages 50-55.[20] Focusing on absences between ages 40-45 allows us to explore effects among a group similar to those who took the health surveys, and observing absences between ages 50-55 allows us to explore effects at later ages, which we cannot do with the health survey data.[21]

4.4 Earnings Data

Earnings data are obtained from the tax registry. Earnings are measured as annual earnings for taxable income, and they include labor earnings, taxable sickness benefits, unemployment benefits, and parental leave payments.[22] They are not top-coded.

4.5 Determining Leave Eligibility and Take-up

We cannot measure employment in the 10 months prior to childbirth directly as our data only contain yearly earnings. We, therefore, rely on an imperfect measure of leave eligibility. We follow Carneiro et al. (2015) and define eligibility status based on whether the woman earned at least NOK 10,000 in the calendar year before giving birth. Given the law additionally based eligibility on employment in the 10 months prior to childbirth,

[17] G is an inflation-adjusted unit for calculation of social benefits in Norway.

[18] We use crosswalks between ICPC-2 codes and ICD-10 codes and follow the classification of diagnoses in prior studies that are based on ICD-10 codes. We group ICPC-2 diagnoses that correspond to ICD-10 codes I00-I99 as cardiovascular; M00-M99 as musculoskeletal; and F01-F99 as psychological. Within these categories, we exclude diagnoses related to congenital disorders, disorders developed early in life, and disorders that are irrelevant for the age group we consider (e.g., dementia). A full list of included diagnoses codes is available upon request.

[19] ICPC-2 codes X76 and X77 correspond to breast and ovarian cancer. We consider these types of cancer because they have been strongly linked to breastfeeding, which we explore as a potential mechanism.

[20] We consider sickness leave among women who work since one must be employed to access such benefits.

[21] We do not have authority to merge the sickness absence and health survey data. Thus, we cannot study sickness leave among the exact same women observed in the health survey data. Instead, we focus on sickness absence among the same cohorts of women observed in the health survey data.

[22] We use "income" and "earnings" interchangeably, referring to the income sources captured in the tax registry earnings variable.

we may overstate the number of eligible mothers by using only annual earnings to determine eligibility.[23]

Furthermore, there are no direct measures of leave-taking during this time period; thus, we do not have information about the use of leave before or after the 1977 reform. Carneiro et al. (2015) conjecture that the take-up of the reform was 100 percent for eligible mothers, meaning they took the full 4 months of paid leave. They provide various pieces of evidence to support this claim. We recap them here. First, using data from the Norwegian Family and Occupation Survey of 1988, Rønsen and Sundström (1996) show very few mothers who gave birth in Norway between 1968 and 1988 returned to work within 4 months of childbirth. Second, in a survey about fertility behavior conducted in 1977 by Statistics Norway, 60 percent of respondents thought mothers should stay home for the first 2 years after childbirth. Third, the reform provided women with 100 percent wage replacement for 4 months, which is a strong incentive for take-up. Last, leave-taking data is available from 1992 on, and take-up of a reform that extended maternity leave by 3 weeks in 1992 is estimated to be close to 100 percent (Carneiro et al. 2015, Dahl et al. 2016).

4.6 Sample Selection and Descriptive Statistics

Our main sample includes eligible mothers who gave birth in 1977 and are observed in either the CONOR or Age 40 Program data, where eligible means they had earnings of at least NOK 10,000 in the calendar year before giving birth. In some analyses, we additionally include women who gave birth in nearby non-reform years (1975, 1978, and 1979) and are observed in the health datasets. To gain a sense of the representativeness of our sample, in Appendix Table A1, we compare the characteristics of all eligible and ineligible mothers who gave birth in the first half of 1977 to the characteristics of mothers observed in the health surveys. In general, the mothers in the health survey data are quite similar to the full sample of mothers. Given women were around the age of 40 when they took the health surveys and the surveys were conducted from 1988 to 2003, the women in our sample who gave birth in 1977 were between 16 and 33 years old at the time of birth. Thus, eligible mothers in our sample were younger on average at the time of birth relative to the full sample of eligible mothers. The average age of eligible mothers in our sample who gave birth in the first half of 1977 is 24.5 (compared to 25.6 in the full sample).

In our sample of mothers who gave birth in 1977, 57 percent were eligible for the reform according to our eligibility definition. In our analysis, we focus on eligible mothers only. Figure 1 shows the proportion of eligible mothers in our sample by birth month of the child from 1975 to 1979. There is no unusually large spike in July 1977. Figure 2 displays the number of children born to eligible mothers in our sample by birth month. There were very similar numbers of births in June and July 1977. We take this as evidence that eligibility and delivery date manipulation are not serious issues in our data. In Section 6, we confirm that the characteristics of mothers who gave birth before and after the reform were virtually identical, further alleviating concerns that mothers may have manipulated their delivery date.

[23] We considered alternative definitions of eligibility, such as a weighted average of 1976 and 1977 earnings where the weights were determined by the month in which the woman gave birth in 1977. Our results are nearly identical using these alternative eligibility definitions.

5 Empirical Strategy

We estimate the medium- and long-term impacts of the 1977 maternity leave reform on maternal health. To do so, we compare the health of eligible mothers who had children immediately before and immediately after July 1, 1977. These women should be similar except those who gave birth after July 1, 1977 were entitled to paid leave benefits.

Our empirical strategy follows that of Carneiro et al. (2015) and we use their same notation. We let E_i denote whether woman i was entitled to paid leave benefits, which is a deterministic function of the date when she gave birth X_i:

$$E_i = 1\{X_i \geq c\} \tag{1}$$

where c is the cutoff date of July 1, 1977. Mothers who gave birth after c may have taken up the new maternity leave benefits and are the treatment group, and those who gave birth before c make up the control group.

Denote α the effect of interest (i.e. the effect of the reform on eligible mothers' health). We estimate α via regression discontinuity (RD). The estimator is given by:

$$\alpha_{RD} = E[y_i(1)|X_i = c] - E[y_i(0)|X_i = c] \tag{2}$$

where $y_i(1)$ is the health outcome of woman i in the presence of the reform, and $y_i(0)$ is the health outcome in the absence of the reform.

If $E[y_i(1)|X_i = c]$ and $E[y_i(0)|X_i = c]$ are continuous in x (more importantly, there is continuity at $x = c$), we can estimate:

$$\alpha_{RD} = \lim_{x \downarrow c} E[y_i|X_i = x] - \lim_{x \uparrow c} E[y_i|X_i = x], \tag{3}$$

the difference between two regression functions at the boundary point: one for women who gave birth on or after July 1, 1977 and one for women who gave birth before July 1, 1977. The RD design can be implemented by estimating the following equation:

$$y_i = \eta + \beta(X_i - c) + \tau E_i + \gamma(X_i - c)E_i + \varepsilon_i, \tag{4}$$

where α_{RD} is estimated as $\hat{\tau}$. We estimate this equation on eligible women who gave birth in 1977 using local linear regression as in Hahn et al. (2001) with the triangle kernel, a bandwidth of 3 months, and separate trends on each side of the discontinuity. We use heteroskedastic-robust standard errors as suggested in Lee and Lemieux (2010).[24] It is important to note that because we do not have information on leave taken by mothers, we estimate an intent-to-treat effect among mothers exposed to the reform.

Some studies find evidence of systematic differences in maternal characteristics by season of birth. To minimize concerns that the RD estimator captures month-of-birth effects, we employ a difference-in-regression discontinuity design. That is, we augment our RD sample and include women who gave birth in nearby years (in which no reform took place) to control for differences in outcomes between mothers who gave birth in June versus July that are unrelated to the reform. Specifically, we create a control group that includes eligible

[24] We do not cluster our standard errors by the running variable (i.e. month of birth) because Kolesár and Rothe (2018) show that clustering by the running variable performs poorly (relative to heteroskedastic-robust standard errors) when the running variable only takes a moderate number of distinct values.

mothers who gave birth in 1975, 1978, and 1979, where eligible means they would have qualified for paid leave given the 1977 reform eligibility criteria (i.e. they earned at least 10,000 NOK the calendar year before giving birth).[25] The difference-in-RD design incorporates any outcome discontinuity that occurs for mothers who gave birth in July in these non-reform control years. Under the mild assumptions that month-of-birth effects do not vary across years and do not interact with the true reform effect, the effect of the reform is the difference between the outcome discontinuity for mothers giving birth in 1977 and the discontinuity for mothers giving birth in the nearby non-reform years. This approach, therefore, accounts for month-of-birth effects. Intuitively, this strategy amounts to estimating the RD specification on women who gave birth in 1977 and in the nearby non-reform years and then identifying the difference in the threshold breaks for the two groups.

6 Results

6.1 Balance of Treatment and Control Groups

We first show how observable pre-reform characteristics of eligible mothers who gave birth in 1977, such as education, age at birth, income in 1975, and marital status at the time of birth, vary with the month in which they gave birth. We do this to check for balance between the treatment and control groups. A lack of balance (i.e. the characteristics of eligible mothers who gave birth immediately before and after the reform differ) suggests some mothers may have manipulated their delivery date. The results of this check are shown in Figure 3. We plot the unrestricted monthly means and the estimated monthly means using local linear regression applied to each side of the cutoff (i.e. the fitted values). We find the characteristics are stable across birth months and there is no discontinuity after July 1, 1977.[26] We examine other characteristics and birth experiences of mothers such as the child's birth weight, whether there were complications at birth, whether the birth involved a cesarean section, and the parity of the birth. The results are shown in Figure 4. We again find no discontinuity at the July 1, 1977 cutoff. The lack of a discontinuity in the probability of a cesarean section is particularly important as it provides evidence that women did not strategically delay delivery by changing the date of their procedure.

6.2 Health Outcomes and Behaviors

We present estimates of the impact of the 1977 reform on maternal health outcomes and behaviors in Tables 1 to 4.[27] For the sake of comparison, we show results from 5 estimation strategies. In Panel A, we show results from a simple comparison of average health outcomes of eligible mothers who gave birth in June versus July 1977 (i.e. the single difference in outcomes). In Panel B, we show results from a simple difference-in-differences estimator where we additionally include mothers who gave birth in June and July in 1975, 1978, and 1979 to control for month-of-birth effects. In Panel C, we present the regression discontinuity estimates using mothers

[25] As mentioned earlier, we do not include women who gave birth in 1976 in the difference-in-RD specification because of the abortion law that went into effect in January 1976.

[26] Given our eligibility definition (and hence, sample restriction) is based on income in 1976, we additionally checked for balance in 1976 income in both our eligible mothers sample as well as the sample that does not condition on eligibility status. We find no evidence of a discontinuity after July 1977 in either case. Results are available upon request.

[27] Pre-reform means for the outcomes are displayed in the bottom row of the tables.

who gave birth in 1977 and a 3-month bandwidth. In Panel D, we show the estimates from the difference-in-regression discontinuity specification where we use mothers who gave birth in 1975, 1978, and 1979 as a control group. In Panel E, we present difference-in-regression discontinuity estimates only using mothers who gave birth in 1975 as a control group to address concerns about using post-reform control years. Our preferred estimates are those in Panels C and D. We prefer the regression discontinuity and difference-in-regression discontinuity models because they use the observed trends in the outcomes on each side of the discontinuity to construct the appropriate counterfactual for the treatment group in the absence of the reform, while the first difference and difference-in-differences models assume the potential outcome curves are flat. In Appendix Figures A1 to A5, we present graphically the regression discontinuity results (i.e. those from Panel C).

Given we analyze many outcomes, we test whether the effects survive after adjusting p-values for multiple hypothesis testing. We use the method described in Romano and Wolf (2005), which is an iterative procedure that controls for the type I error rate within a family of outcomes at a fixed level of significance. We group variables into a family if they measure conceptually similar health outcomes. For example, measures of metabolic health comprise one family, and types of self-reported pain comprise another family. In the tables, the estimates marked in italics are significant at the 10 percent level when adjusted for multiple hypothesis testing.

We find the reform led to significant improvements in the metabolic health of mothers around age 40 (Table 1 and Figures A1 and A2). BMI decreased by 0.6 to 1.1 kg/m^2, and the probability of being obese declined by 3 to 4 percentage points (except in the difference-in-RD specifications). In the left panel of Appendix Figure A6, we plot the BMI density functions for women who gave birth in June and July 1977, and in the right panel, we plot the distributions for women giving birth in June and July 1979. The figures make clear that there was a shift left in the BMI distribution around age 40 for mothers who gave birth in July 1977 compared to June 1977 and no such shift for mothers giving birth in 1979. The test statistic from a Kolmogorov-Smirnov test reveals that we can reject the null hypothesis that the June and July 1977 distributions are equal at the 1 percent level. This suggests the reform did not just decrease BMI on average, but may have shifted the whole distribution. The declines in BMI likely reflect an increased likelihood of returning to pre-pregnancy weight, making it difficult to compare them to the impacts of policies aimed at reducing weight, such as taxes on sugary drinks. Such interventions tend to have little or no impact on adult BMI. Our results are quite similar to those in Courtemanche (2011), who finds after 7 years, a \$1 increase in the price of gasoline in the US reduces average BMI by 0.7 to 0.8 kg/m^2 and reduces the probability of being obese by 3 to 4 percentage points.

Diastolic blood pressure fell by about 1 to 2.5 millimeters of mercury (mmHg) in response to the reform. We also find the reform decreased the probability of experiencing hypertension by 1 to 1.5 percentage points (results available upon request). To put the blood pressure results into perspective, in the RAND Health Insurance Experiment in the US, individuals randomized to health insurance policies that provided free care versus cost-sharing plans experienced a 0.8 mmHg average reduction in diastolic blood pressure, with a 1.9 mmHg decrease among hypertensives (Keeler et al. 1985). We find weak evidence that the reform decreased the probability of having diabetes. The probability of having risky cholesterol levels fell by 0.2 to 0.6 percentage points, but there were no significant effects on cardiac risk. The reform led to a 0.1 to 0.25 standard deviation improvement in the metabolic health index, which aggregates the metabolic health measures. The effects on BMI, blood pressure, and the metabolic health index survive the adjustments for multiple hypothesis testing.

Our estimates show that the reform improved self-reported mental and general health. The reform generated a 0.1 to 0.2 standard deviation improvement in the mental health index and a 0.05 to 0.1 standard deviation improvement in the general health index (Table 2 and Figure A3). Except in one case, these impacts are significant even after accounting for multiple hypothesis testing. It is not obvious that the reform would generate mental health improvements. For those with prior mental health problems, structured time may be important and longer leave may be harmful. However, mothers who gave birth during this time had universal and free access to mother and child health care centers as well as mother-group meetings (Bütikofer et al. 2016). Mothers with access to longer leave had more time to attend these meetings and obtain services from the health care centers, which may explain, in part, the mental health effects. Furthermore, Chatterji and Markowitz (2005, 2012) find longer maternity leave (in the US) is associated with decreased depressive symptoms. Given they study mental health up to 2 years after childbirth and we observe women around age 40, potentially several years after childbirth, our results suggest the improvements in mental health persist.

We find the reform decreased the probability of reporting pain around age 40 by 4 to 6 percentage points (a 15 to 24 percent decline relative to the pre-reform mean), with the improvements driven by decreases in neck and shoulder, leg and hip, and back pain (Table 3 and Figure A4). The effects on any, neck and shoulder, and in some cases back pain survive the adjustments for multiple hypothesis testing. Overall, our results indicate that the 1977 maternity leave reform was protective of maternal health. We outline potential mechanisms through which these improvements occur later in this section.

In addition to health outcomes, we consider the impact of the reform on health behaviors.[28] We find the reform decreased the probability of daily smoking among mothers around age 40 by 3 to 7 percentage points (a 10 to 23 percent decrease relative to the pre-reform mean) and increased the exercise index by 0.1 to 0.2 (Table 4 and Figure A5). The impacts on smoking are statistically significant after accounting for multiple hypothesis testing across all specifications, but the effects on exercise survive the adjustment only in some cases.[29] Given that exercise and not smoking are health-promoting behaviors, the changes in these activities may reflect increased efforts by mothers who were impacted by the reform to preserve their improved health. On the other hand, increased exercise may play a role in generating the health improvements. Lack of time and fatigue are the most commonly cited barriers to physical activity among mothers during the postpartum period (Bellows-Riecken and Rhodes 2008). If expanded leave delayed the return to work, this may have allowed them to engage in or return to regular exercise, and such behavior may persist well after the postpartum period.

[28]We also find the reform decreased the probability of experiencing sleep problems around age 40. However, less than 3 percent of the sample reports having such problems.

[29]As a sensitivity check, we adjusted p-values for multiple hypothesis testing without grouping outcomes into separate families. In the RD and difference-in-RD analyses, the effects on BMI, blood pressure, the metabolic health index, mental health, pain (any), and back pain survive at the 10 percent level.

6.3 Mechanisms

6.3.1 Time at Home, Income and Employment Effects

Our results suggest the 1977 maternity leave reform generated significant medium- and long-term improvements in maternal health. These improvements could be driven by more time spent at home after childbirth and/or income effects (i.e. changes in family income). Using the data available to us, we attempt to understand the relative importance of these mechanisms.

Earlier, we reviewed the evidence suggesting that take-up of the 1977 reform was close to 100 percent among eligible mothers. Thus, it is reasonable to assume that women took the full 4 months of paid leave (with 100 percent wage replacement). However, the reform may have changed the amount of unpaid leave taken by mothers. While we do not have information on leave-taking, it is possible to estimate how much unpaid leave (or more generally, time off work) was taken by analyzing a woman's income before and after giving birth. We follow Carneiro et al. (2015) and Dahl et al. (2016) and impute the number of months of unpaid leave from information on yearly earnings (which include maternity benefits) from 1977 to 1979. The assumptions underlying the imputation are that pre-birth earnings are a good approximation for post-birth inflation-adjusted potential earnings as well as full take-up of the 4 months of paid leave provided by the reform.[30] The intuition is as follows. If a woman's income increased after childbirth, this suggests a decrease in the amount of unpaid leave taken, and if her income fell, unpaid leave may have increased. If there was no change in income, the reform did not affect unpaid leave. According to our calculations, average unpaid leave was 8.8 months for women who gave birth in the first half of 1977, a relatively high amount. However, there is substantial heterogeneity in unpaid leave taken, with about 15 percent of women taking 3 months or less.[31]

Column 1 of Table 5 shows the estimated impact of the reform on the predicted number of months of unpaid leave taken. Consistent with Carneiro et al. (2015), we find no significant effect of the reform on unpaid leave, and given the 95 percent confidence intervals, we can rule out changes of more than 1 month.[32] Thus, the reform did not crowd out unpaid leave, but rather increased the total amount of time a woman spent at home by about 4 months.

The 1977 reform provided substantial job protection to mothers, allowing them to return to work up to 16 months after giving birth. The labor market attachment of mothers may have strengthened as a result of this extended job protection. If labor market attachment increased, this may have led to increased maternal income, which could also explain the health improvements. We explore whether the reform impacted maternal income as well as the probability of being employed 2, 5, and 10 years after giving birth. The

[30] We divide earnings in 1976 by 12 to obtain pre-birth monthly income. We then calculate total earnings in 1977 to 1979 and divide by pre-birth monthly income, yielding a predicted number of months of unpaid leave during the first 24 months after the mother gave birth.

[31] Our unpaid leave estimate in 1977 includes any unpaid sickness leave a woman took around the time of birth. Paid sickness leave was introduced in Norway in July 1978, and sickness leave taken by mothers after that change does not contribute to our unpaid leave estimate. Average unpaid leave is 0.5 months smaller among women who gave birth in 1979 (and had access to paid sickness leave) compared to women who gave birth in the first half of 1977 (and had access to unpaid sickness leave). Thus, a small fraction of our unpaid leave estimate in 1977 may reflect unpaid sickness leave.

[32] For brevity, here and in subsequent analyses, we present RD estimates and difference-in-RD estimates using 1975, 1978, and 1979 as control years.

results are presented in columns 2 to 7 of Table 5. We find no impact of the reform on short-, medium-, or long-term income or employment.[33] Taken together these results suggest more time at home, not income effects, led to the maternal health improvements.

6.3.2 Breastfeeding

One particular channel through which the reform may have generated health improvements that is consistent with mothers spending more time at home is breastfeeding. Breastfeeding is associated with several maternal health benefits, which we describe below. We do not have data on breastfeeding around the time of the reform, and therefore, cannot explicitly analyze whether breastfeeding behavior changed.[34] However, we outline arguments for why breastfeeding was likely impacted by the reform and may be an important mechanism below. In the next subsection, we consider the impact of the reform on sickness absence and those results corroborate the idea that breastfeeding is a likely channel.

Returning to work has been documented as an important reason for stopping breastfeeding or never starting. For example, Berger et al. (2005) find children of mothers in the US who returned to work within 12 weeks were about 8 percentage points less likely to be breastfed and were breastfed for 4 to 5 fewer weeks. Studies in the economics literature have found a significant causal relationship between maternity leave length and breastfeeding. Baker and Milligan (2008b) find an increase in paid maternity leave in Canada from a maximum of 25 weeks to 50 weeks significantly increased breastfeeding duration by about 1 month during the first year postpartum and increased the proportion of women who exclusively breastfed for 6 months by 8 to 9 percentage points.[35,36] Huang and Yang (2015) estimate the probability of exclusively breastfeeding through the first 3 and 6 months postpartum increased by 3 to 5 percentage points, and the probability of breastfeeding (overall) through the first 3, 6, and 9 months increased 10 to 20 percentage points after the introduction of up to 6 weeks of paid maternity leave in California in 2004. Kottwitz et al. (2016) examine a parental leave reform in Germany in 2007 in which most parents received more financial support than they would have prior to the reform and find an increase in breastfeeding duration. None of the above-mentioned studies find increases in breastfeeding initiation, only duration changes. Overall, the literature suggests maternity leave expansions from modest and generous baseline levels increase breastfeeding duration, giving us good reason to believe the 1977 reform changed the breastfeeding behavior of some mothers.

Breastfeeding has been linked to various maternal health benefits, such as reduced risk of breast cancer, ovarian cancer, postpartum depression, and diabetes (particularly among women without a history of

[33]We estimated the impact of the reform on income and employment 1 to 10 years after giving birth, and find no significant effects over the full horizon. We also find no effects on log (rather than level) income.

[34]Liestøl et al. (1988) document trends in breastfeeding from 1860 to 1984 in Norway using data from 3 maternity hospitals. In the late 1970s, about 75 percent of mothers breastfed for 3 months, 50 percent for 6 months, and 25 percent for 9 or more months.

[35]The World Health Organization recommends exclusive breastfeeding for 6 months with continued breastfeeding along with complementary foods up to 2 years of age.

[36]However, they do not find significant improvements in maternal health 7 to 24 months postpartum, which could be due to the fact that the increase in leave was from an already large baseline level.

gestational diabetes).[37,38] There is also evidence that breastfeeding is associated with cardiovascular benefits later in life, such as reduced risks of hypertension, hyperlipidemia, and cardiovascular disease (Schwarz et al. 2009). The improvements in metabolic and mental health we estimate are consistent with the notion that the reform increased breastfeeding.

The effects of breastfeeding on some health outcomes, such as postpartum weight loss, are less clear. Breastfeeding could affect weight loss via changes in energy metabolism and calories burned during lactation. Several studies in the public health literature find women who breastfeed, particularly those who exclusively breastfeed for the first 6 months postpartum, retain less weight after pregnancy than mothers who do not breastfeed. However, these studies generally fail to control for other factors that may impact weight loss, such as baseline BMI, age, birth parity, ethnicity, and diet. For a review of this literature, see Neville et al. (2014). We find the reform decreased BMI; breastfeeding may be the mechanism through which the weight changes occur.

Some studies find longer breastfeeding duration decreases the risk of developing rheumatoid arthritis, but others find no significant impact on long-term changes in bone mineral densities.[39] We find the reform decreased the probability of experiencing musculoskeletal pain, which could be explained, in part, by our BMI results, as such pain is correlated with body weight. To test whether the improvements in pain are mediated through the reduction in weight, we reestimated the pain specifications controlling for BMI at the time of the health survey. The estimated impacts of the reform decrease in magnitude but are still statistically significant in most of the RD and difference-in-RD specifications (see Appendix Table A2). If breastfeeding indeed improves bone mineralization, that may explain the improvements in pain we observe that are not mediated through changes in weight.

The decline in smoking in response to the reform may also be linked to breastfeeding. Various public health studies document a negative correlation between maternal smoking (including postpartum relapse) and breastfeeding initiation and duration,[40] though we are unaware of any that establish a causal link between the two.

6.3.3 Short-Term Health Effects

The birth registry data contains information on a small set of health conditions ever experienced by women prior to pregnancy as well as during pregnancy, such as hypertension and diabetes. To gain a sense of whether maternal health improved in the short-term, we examine whether the reform impacted the health of mothers who gave birth for the first time in 1977 (or 1975, 1978, and 1979) before and during their next pregnancy. We consider the probability of experiencing: a metabolic health-related diagnosis prior to one's second pregnancy, including asthma, hypertension, kidney disease, heart disease, and diabetes; any chronic health diagnosis prior to the second pregnancy, including the above-mentioned conditions as well as others like pain and skin

[37]The discussion of the relationship between maternal health and breastfeeding is based on the reviews of the public health literature in Ip et al. (2007) and Eidelman et al. (2012).

[38]The impact on diabetes is attributed to the fact that lactation has a positive impact on glucose and lipid metabolism as well as insulin sensitivity.

[39]Breastfeeding may impact these outcomes because calcium and bone metabolism are substantially impacted during pregnancy and lactation.

[40]See for example Ratner et al. (1999), Scott et al. (2006), and Higgins et al. (2010).

problems; diabetes during the second pregnancy; and, hypertension during the second pregnancy. The results are presented in Appendix Table A3. Although these health problems are somewhat rare, we generally find the reform significantly decreased the probability of experiencing such problems before and during a woman's second pregnancy. Thus, the reform appears to have generated health improvements in the short-run, which led to improved health during subsequent pregnancies and likely persisted into women's forties.

We explored other mechanisms through which the reform may have impacted health, such as changes in completed fertility, birth spacing, and marital stability. We find no significant impact of the reform on these outcomes (results are available upon request).[41]

6.4 Heterogeneous Effects

Next, we examine whether the effects of the reform varied with characteristics of mothers and their birth experience. We consider heterogeneous effects by whether there were complications at delivery, whether the mother had a cesarean section, birth parity, the mother's marital status at birth, household income in 1975, and time between giving birth and the health survey. For brevity, we only show the RD estimates, but results from the difference-in-RD specifications are quantitatively similar and available upon request. Specifically, we augment our baseline RD framework by including a subgroup indicator (for whether there were complications at birth, whether a cesarean section was performed, whether the birth was a first birth, whether the mother was single at birth, whether the household had below-median income in 1975, and whether the time between giving birth and the health survey was greater than 15 years), an interaction term between the subgroup indicator and an indicator for having access to paid leave, as well as interactions between the subgroup indicator and the trends on each side of the cutoff.[42]

The results are presented in Tables 6 to 9. For mothers who experienced complications at birth, the reform had a stronger effect on all of the metabolic health measures, mental health, pain, and smoking compared to mothers without complications. Such complications often make postpartum recovery more difficult, and some studies have found that they are associated with the development of postpartum depression (O'Hara and Swain 1996). The reform may have been especially important for these mothers in that it provided them more time to recover from the physical and mental stress of a difficult birth. For mothers who had a cesarean section, we find smaller effects of the reform on BMI, obesity, and the metabolic health index. These results could be explained by the fact that overweight women are at a greater risk for a cesarean section (Chu et al. 2007, Poobalan et al. 2009).[43]

First-time mothers were more affected by the reform relative to non-first-time mothers, but only with

[41] Carneiro et al. (2015) also find no impact of the 1977 reform on completed fertility or marital stability, and Dahl et al. (2016) find no effect of the leave extensions on these outcomes. In addition, we find no evidence among first-time mothers that the reform impacted completed fertility or subsequent birth spacing, suggesting changes in fertility do not explain the short-term health improvements.

[42] We also explored heterogeneity by whether the woman had a low birth weight baby (less than 2,500 grams) and by whether the woman experienced a chronic health diagnosis prior to her pregnancy. We find no differential reform effects, which could be due in part to these subgroups being small, leading to a lack of statistical power.

[43] We do not have information about the mother's weight when she gave birth. Nevertheless, we find a significant positive correlation between having a cesarean section in 1977 and obesity around age 40.

respect to metabolic health measures, particularly BMI, obesity, blood pressure, and the metabolic health index. It may be that non-first-time mothers were already experienced with childbirth and better able to cope with the subsequent physical effects and stress. In addition, non-first-time mothers had their prior children during periods of less generous maternity leave, while first-time mothers in 1977 had subsequent children under the more generous paid leave scheme. If the effect of exposure to paid maternity leave accumulates, that may also explain the heterogeneous results by birth parity.

We find single mothers experienced larger improvements in some metabolic health measures, the general health index, pain (overall), and exercise compared to women who were married when they gave birth. For mothers with household income below the median in 1975, the reform had larger effects on most metabolic health measures, the mental and general health indices, pain, smoking, and exercise.[44] Thus, the heterogeneity analyses by marital status and household income suggest the reform had stronger effects on low-resource mothers (single mothers and those with below-median household income).

The reform had a relatively larger impact on diabetes and the mental and general health indices for women with more than 15 years between giving birth and taking the health survey.[45] These results are consistent with the idea that some health improvements are more pronounced in the long-run. However, these women were also younger when they gave birth in 1977, and another interpretation is that the reform had a larger impact on some dimensions of health for younger mothers. Unfortunately, because women gave birth in 1977 and were around age 40 when they took the health survey, we cannot distinguish between these interpretations.

Last, we explore heterogeneity by the amount of predicted unpaid leave women took. In principle, unpaid leave could be affected by the reform and we should not condition on it. However, earlier we found the reform had no significant impact on unpaid leave. Thus, we can analyze whether the effects of the reform differ by the amount of unpaid leave a woman would have taken in the absence of the reform. Specifically, we examine heterogeneity by whether women took 3 or fewer months of unpaid leave versus more than 3 months. Results are shown in Panel G in Tables 6 to 9. Across most of the health outcomes and behaviors, the reform had larger effects on women who took less unpaid leave, sometimes on the order of 1.5 to 2 times larger. Earlier, we established that the reform led to more time at home. This additional time at home appears to have been especially valuable for women who in the absence of the reform would have taken little (unpaid) leave.

These results are consistent with our hypothesis that changes in breastfeeding are an important mechanism. Women who would have returned to work shortly after childbirth are likely to have had short breastfeeding durations. The 4 months of paid leave provided by the reform may have allowed them to continue breastfeeding. Furthermore, we found larger effects of the reform among low-resource mothers, who may have been least able to afford lengthy unpaid leave. Indeed, relative to women who took more than 3 months of unpaid leave, those who took less leave were about 5 percentage points less likely to be married at the time of birth and their incomes were about NOK 6,000 lower on average. Thus, the

[44] We also explored heterogeneous effects by whether household income in 1975 was in the lowest quintile versus all other quintiles. The effects of the reform were larger for those in the bottom quintile.

[45] Among women in our sample who gave birth in 1977, the average (median) time between giving birth and taking the health survey is 15.5 (15) years.

reform was valuable for low-resource mothers, in part, because they often took little unpaid leave, and the reform allowed them to spend more time at home after childbirth. These results are also consistent with Carneiro et al. (2015), which finds the effects of the reform on children's later-life outcomes were larger for those whose mothers would have taken very low levels of unpaid leave in the absence of the reform.

Although we find no evidence of income or employment effects in the full sample, it is possible such effects existed for subgroups of mothers. In Appendix Table A4, we present the heterogeneous effects of the reform on months of unpaid leave as well as income and employment 2, 5, and 10 years after giving birth for the groups of mothers for whom, a priori, we expect income effects could be likely. We find no evidence of heterogeneous impacts of the reform on unpaid leave taken. First-time mothers see increases in employment and income up to 5 years after giving birth, but the effects on income are small. For single mothers, the reform increased income and the probability of being employed 10 years after giving birth, and for women who would have taken low levels of unpaid leave in the absence of the reform, both income and employment increased over the 10 years since giving birth. Thus, women of all types spent more time at home after giving birth. For some subgroups, labor market attachment may have strengthened, leading to increased income. However, the income effects are modest in size (NOK 200 to 2,400), and therefore unlikely to fully explain the heterogeneous maternal health improvements.

One concern regarding interpretation of the effect heterogeneity is that the mother's age at birth is correlated with the dimensions of heterogeneity we consider, and the differential effects may simply reflect systematic differences in age at birth across the subgroups. Indeed, low-income mothers, single mothers, and mothers who take little unpaid leave are younger on average than their counterparts. We reestimated the heterogeneity specifications controlling for the woman's age at birth. The estimates are quantitatively similar to those that do not control for the mother's age at birth, suggesting mothers' age does not drive the effect heterogeneity. Results of this exercise are available upon request.[46]

6.5 Sickness Absence

An advantage of the sickness absence data is that we observe women at various ages, not only around age 40. We consider the effect of the reform on the probability of ever taking certified sickness leave between ages 40-45 as well as 50-55 to examine whether there were impacts at later ages. Results are shown in Appendix Table A5. Generally, the reform did not significantly affect sickness absence, implying the health improvements did not translate into reductions in sickness absence in the age ranges we consider. However, we find some evidence that the reform decreased the probability of taking sickness leave related to breast and ovarian cancers between ages 50-55. Given several studies have found associations between breastfeeding duration and ovarian and breast cancer risk (Ip et al. 2007, Eidelman et al. 2012), we cautiously interpret the decline in sickness absence related to these cancers as supportive evidence for our hypothesis that breastfeeding behavior changed in response to the reform.

[46]Since age at birth is collinear with time between giving birth and the health survey, this exercise also addresses the concern that the effect heterogeneity is driven by systematic differences in years since giving birth across the subgroups.

6.6 Subsequent Reforms

A series of expansions in parental paid leave occurred in Norway between 1987 and 1992. They were similar to the 1977 reform in several ways. They provided 100 percent wage replacement, and to be eligible, women had to work 6 of the 10 months immediately preceding childbirth and have annual income that exceeded a threshold indicating "substantial gainful activity." Some of the weeks could be shared among both parents, but very few fathers took any leave (Dahl et al. 2016).[47] Women were still entitled to up to 1 year of unpaid leave on top of the paid leave.

The first expansion allowed eligible mothers who gave birth after May 1, 1987 to take 20 weeks of paid leave (compared to the 18 weeks provided by the 1977 reform). The cutoffs and expansions for the subsequent reforms were as follows: July 1, 1988 (2 additional weeks); April 1, 1989 (2 additional weeks); May 1, 1990 (4 additional weeks); July 1, 1991 (4 additional weeks); and April 1, 1992 (3 additional weeks). Dahl et al. (2016) show that similar to the 1977 reform, the subsequent reforms did not crowd out unpaid leave or change family income. They also had little effect on a host of child and family outcomes, such as children's schooling, parental earnings and employment, completed fertility, and marriage. We estimate the effects of these expansions on maternal health around age 40 via regression discontinuity using the health survey data and exploiting the policy cutoff dates.[48]

Before proceeding to the results, some caveats are worth noting. First, given we observe women's health around age 40, the mothers in our sample are increasingly older and closer to age 40 at the time of birth as we consider later expansions. The results should, therefore, be interpreted as the impacts of leave expansions on older mothers. Second, we are limited to analyzing health effects generated over a shorter time horizon compared to the 1977 reform. Third, the expansions occurred in consecutive years, making it difficult to find control years for the difference-in-RD analysis. As a result, we only estimate RD models. Last, the extensions provided a substantially smaller number of additional paid leave weeks compared to the 1977 reform, which is important to keep in mind when comparing the effects of the extensions to the effects of the introduction of paid leave.

The results are presented in Appendix Tables A6 to A9. We find some significant beneficial effects of the first two expansions (and occasionally the third and fourth) on many of the outcomes that were impacted by the 1977 reform, such as BMI, blood pressure, the metabolic health index, self-reported mental and general health, neck and shoulder pain, smoking, and exercise. The effects tend to be smaller in magnitude than the 1977 reform effects. In Panel G, we present the cumulative effects of all the expansions from 1987 to 1992. Only the cumulative impacts on the general health index and smoking are significant at the 5 percent level. However, none of the estimates in Tables A6 to A9 survive after adjustments for multiple hypothesis testing. Thus, we find some, albeit weak, evidence that expansions in paid maternity leave improve maternal health up to a point, and then have little to no further effect, consistent with the notion of diminishing returns to maternity leave length. These results are also consistent with prior studies that have found zero or small

[47] More details about these expansions can be found in Dahl et al. (2016).

[48] We again use local linear regression with triangular weights, a 3-month bandwidth, and separate trends on each side of the discontinuity. We only consider mothers whose income exceeds the eligibility threshold.

maternal health effects of expansions in maternity leave from already generous levels.

One possible explanation for the lack of subsequent reform effects is women's baseline health improved over time, leaving less room for further improvements. We explore this possibility using the information from the birth registry on the small set of health conditions ever experienced by women prior to pregnancy, such as asthma, hypertension, kidney disease, diabetes, thyroid problems, and arthritis. We find no significant differences in the probability of experiencing such conditions for all mothers giving birth in 1977 and 1987 through 1992, as well as no significant differences among older mothers (those over 30 at the time of birth), which suggests differences in baseline maternal health are not responsible for the lack of subsequent reform effects.

7 Robustness Analyses

We present the results of several robustness checks in the Appendix. First, we examine whether the 1977 reform impacted the health of fathers. While it is possible changes in time spent at home by mothers could affect fathers' health, we expect such effects to be second-order relative to the effects on mothers. In general, the reform did not significantly affect the health of fathers (see Tables A10 to A13). The main exception is that we find significant increases in fathers' blood pressure. However, the reform did not impact their probability of experiencing hypertension (results not shown).[49]

We analyze whether the reform impacted the health of mothers who were ineligible for the paid leave benefits (i.e. those who earned less than 10,000 NOK the year before giving birth). We generally find no significant effects of the reform on this group of mothers (see Tables A14 to A17).

We perform placebo analyses assuming the reform occurred on July 1st in a year other than 1977. We find no significant effect of the placebo reform regardless of whether it is defined to occur in 1975, 1978, or 1979 (see Tables A18 to A21). We also conduct a more rigorous placebo analysis to address any remaining concerns that our estimates reflect unobserved differences between mothers who gave birth in different months. We estimate our RD specifications redefining the reform cutoff to be the 1st of a different month (not just July). To allow for a 3-month bandwidth, we consider reform cutoffs from April to October of 1975, 1977, 1978, and 1979, yielding 27 placebo effects (excluding the July 1977 effect). We calculate the proportion of times the placebo estimates are less (i.e. larger negative numbers) than the actual 1977 reform estimate, which represent the p-values of the null hypothesis that any other month-of-birth comparison would generate the same pattern of effects. In Table A22, we show the results of this exercise. We reject the above-mentioned null hypothesis at the 1 percent level for all outcomes except cholesterol risk and chest pain, providing further confidence that the estimated effects are driven by the reform, not month-of-birth variation.

In the difference-in-RD specifications, it is possible some women appear in the sample more than once if they had multiple births. To address this issue, in cases where a woman gave birth more than once between 1975 and 1979 (excluding 1976), we randomly include only one of her births and reestimate our specifications. We repeat this exercise, bootstrapping 100 times, and find our results are quantitatively similar to our baseline estimates. Results from this exercise are available upon request.

[49] Fathers are only included in the sample if the mother was eligible for the leave benefits.

Earlier we mentioned the heterogeneous effects are robust to controlling for the mother's age at birth. The full-sample baseline effects are also robust to the inclusion of this covariate, and if anything, are more precisely estimated. Results including mother's age are available upon request.

Last, we show the regression discontinuity results for different bandwidth choices as suggested in Lee and Lemieux (2010). Figures A7 to A11 in the Appendix display the estimates of the impact of the reform as well as 95 percent confidence intervals for bandwidths ranging from 1 to 5 months. Generally, the point estimates are not very sensitive to different bandwidth values, but they are less precise when smaller bandwidths are chosen.

8 Conclusion

We exploit a reform in Norway in 1977 to estimate the impact of the introduction of paid maternity leave on maternal health. Under the new policy, mothers who gave birth after July 1, 1977 were eligible for 4 months of paid leave plus a year of unpaid job protection. Mothers who gave birth prior to this date were eligible for 12 weeks of unpaid leave, similar to leave benefits provided under the Family and Medical Leave Act in the US. Using regression discontinuity and difference-in-regression discontinuity designs, we examine the impact of the reform on a range of maternal health outcomes and behaviors around age 40.

Our results imply that the introduction of paid maternity leave had important medium- and long-term health benefits. The reform generated improvements in metabolic health, pain, and self-reported mental and overall health of eligible mothers. In addition, health-promoting behaviors such as exercise and not smoking increased. We provide evidence that the health improvements were driven by more time at home after childbirth, not changes in income, and we speculate that changes in breastfeeding behavior are likely an important mechanism through which maternity leave impacts health.

The effects of the reform differed across various subgroups of women. In particular, the effects were larger for single mothers and low-income mothers, suggesting the benefits of paid maternity leave are greater for low-resource mothers. The impacts were also larger for women who would have taken little unpaid leave in the absence of the reform, a group which includes many low-resource mothers. Thus, the additional 4 months at home after childbirth were especially valuable for disadvantaged mothers.

We find limited evidence that expansions in paid leave further improved maternal health. Caution should be exercised in interpreting these results since we are limited to analyzing the effects of the expansions on women who were closer to age 40 when they gave birth. Nevertheless, it appears there are diminishing returns to maternity leave length. The differential effects of introductions versus expansions in paid leave are important for policy-makers to consider when designing family leave policies.

Our findings may shed light on the documented benefits of maternity leave programs for children. Mothers who are physically and mentally healthier may be better able to invest in their children. Improved maternal health may, therefore, complement the increased time mothers spend with children as a result of leave provisions, leading to better child outcomes.

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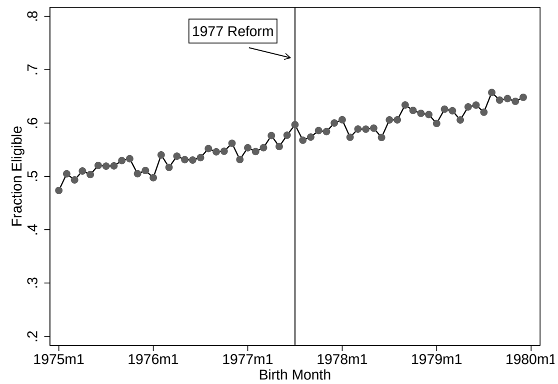
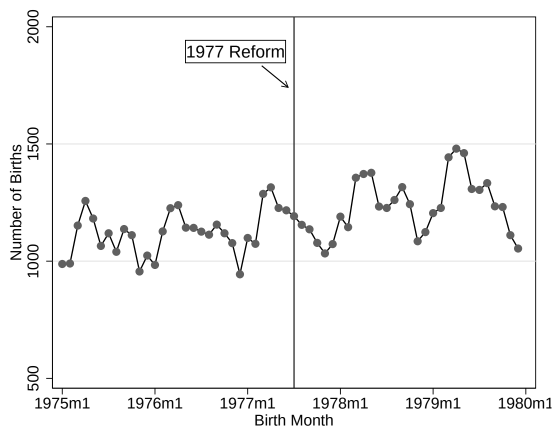
9 Tables and Figures

Figure 1: Proportion of Mothers Eligible for Paid Maternity Leave



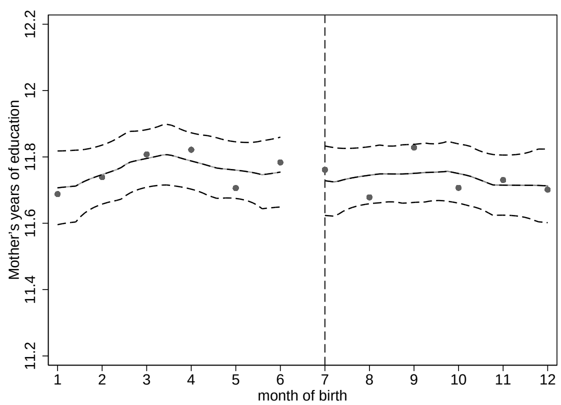
Note: The figure shows the fraction of eligible mothers (i.e. those with earnings of at least NOK 10,000 in the calendar year before giving birth) among all mothers we observe in the health datasets by birth month of the child from January 1975 to December 1979.

Figure 2: Number of Children Born to Eligible Mothers

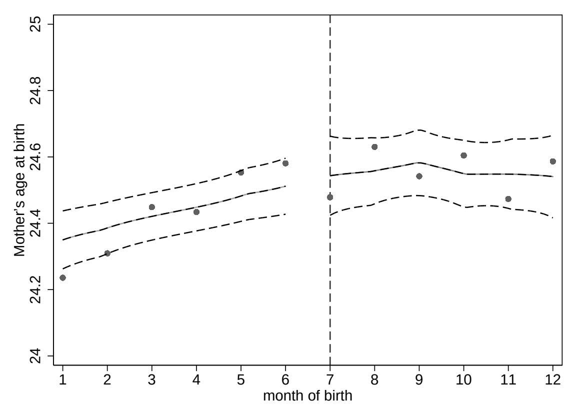


Note: The figure shows the number of children born to eligible mothers (i.e. those with earnings of at least NOK 10,000 in the calendar year before giving birth) who we observe in the health datasets by month of birth from January 1975 to December 1979.

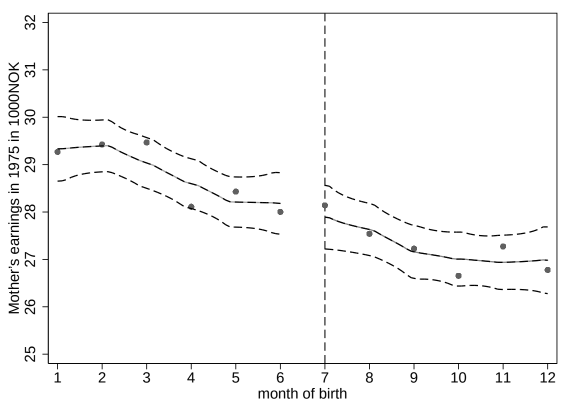
Figure 3: Mothers' Pre-reform Characteristics



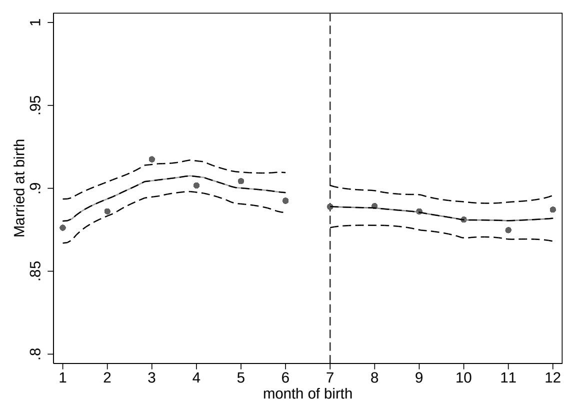
(a) Years of Education



(b) Mother's Age at Birth



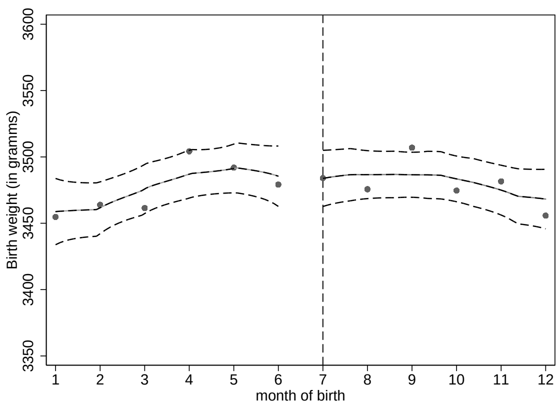
(c) Mother's Income in 1975



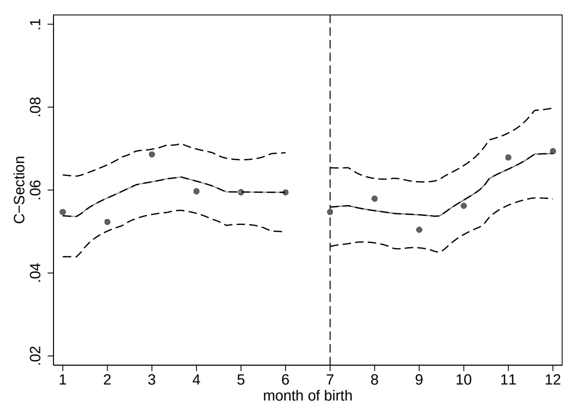
(d) Married at Birth (Dummy)

Note: The figure plots pre-reform characteristics of mothers giving birth in the vicinity of the reform date. The sample consists of eligible mothers that we observe in the health datasets. Each data point corresponds to the average value of each outcome, organized according to date of birth (in one-month bins). Dashed vertical lines denote the reform cutoff of July 1, 1977. The solid line represents fitted values from a local linear regression where the window includes all eligible mothers who gave birth in 1977. The dashed lines mark the 95 percent confidence interval.

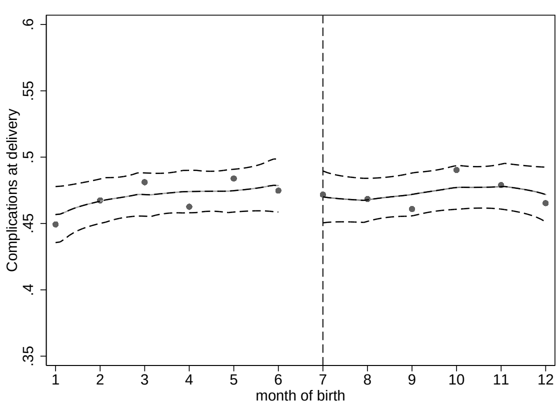
Figure 4: Mothers' Birth Experience Characteristics



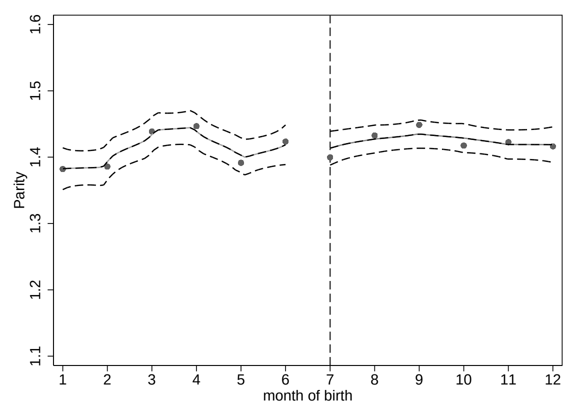
(a) Birth Weight



(b) C-Section (Dummy)



(c) Complications at Delivery (Dummy)



(d) Parity

Note: The figure plots delivery experience characteristics of mothers giving birth in the vicinity of the reform date. The sample consists of eligible mothers that we observe in the health datasets. Each data point corresponds to the average value of each outcome, organized according to date of birth (in one-month bins). Dashed vertical lines denote the reform cutoff of July 1, 1977. The solid line represents fitted values from a local linear regression where the window includes all eligible mothers who gave birth in 1977. The dashed lines mark the 95 percent confidence interval.

Table 1: Impact of the Reform on Metabolic Health of Mothers

	(1) BMI	(2) Obese	(3) Diabetes	(4) Blood Pressure	(5) Cholesterol Risk	(6) Cardiac Risk	(7) Index
				Panel A			
Single difference	*-0.624****	*-0.027***	-0.005	*-1.045****	-0.004**	-0.002	*-0.162****
	(0.148)	*(0.010)*	(0.004)	*(0.394)*	(0.002)	(0.002)	*(0.038)*
Observations	2430	2434	2431	2428	2434	2434	2424
				Panel B			
DD	*-0.576****	-0.029**	-0.007*	-0.995**	-0.006**	-0.003	*-0.156****
	(0.173)	(0.012)	(0.004)	(0.457)	(0.003)	(0.003)	*(0.044)*
Observations	9742	9763	9747	9748	9763	9763	9727
				Panel C			
RD	*-0.843****	*-0.038****	*-0.010****	*-1.780****	-0.002**	-0.005	*-0.251****
	(0.088)	*(0.008)*	*(0.001)*	*(0.064)*	(0.001)	(0.004)	*(0.018)*
Observations	5993	6002	5997	5991	6002	6002	5982
				Panel D			
RD-DD	*-0.753****	-0.010	-0.003	*-1.518****	-0.003***	-0.001	*-0.096****
	(0.140)	(0.010)	(0.003)	*(0.365)*	(0.001)	(0.002)	*(0.036)*
Observations	24750	24794	24750	24763	24794	24794	24720
				Panel E			
RD-DD (1975 only)	*-1.070****	-0.018	-0.004	*-2.526****	-0.002	0.002	*-0.174****
	(0.153)	(0.011)	(0.004)	*(0.408)*	(0.003)	(0.010)	*(0.040)*
Observations	11587	11607	11592	11593	11573	11607	11607
Pre-reform mean	24.298	0.076	0.005	75.782	0.004	0.005	0.002

Note: Panel A shows the coefficients from a regression of each of the variables on an indicator for giving birth in July 1977 where the sample includes only women who gave birth in June and July of 1977. For Panel B, we added to the sample women who gave birth in June and July of 1975, 1978, and 1979, and we regressed each of the variables on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter. In Panels C, D, and E, each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. The estimates in Panel C are from the sample of eligible mothers who gave birth in 1977, whereas the RD-DD estimates in Panel D additionally include eligible mothers who gave birth in 1975, 1978, and 1979. The RD-DD estimates in Panel E include only mothers who gave birth in 1975 as an additional control group. The pre-reform mean of the metabolic index is standardized to be zero with a standard deviation of one. Coefficient estimates marked in italics are significant at the 10% level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Impact of the Reform on Self-Reported Health of Mothers

	(1) Mental Health Index	(2) General Health Index
	Panel A	
Single difference	*-0.087****	*-0.116****
	(0.031)	*(0.031)*
Observations	2434	2434
	Panel B	
DD	*-0.099****	*-0.121****
	(0.036)	*(0.035)*
Observations	9763	9763
	Panel C	
RD	*-0.140****	*-0.078****
	(0.011)	*(0.005)*
Observations	6002	6002
	Panel D	
RD-DD	*-0.189****	-0.055**
	(0.028)	(0.022)
Observations	24794	24794
	Panel E	
RD-DD (1975 only)	*-0.116****	*-0.138****
	(0.031)	*(0.019)*
Observations	12548	12548
Pre-reform mean	0.002	0.003

Note: Panel A shows the coefficients from a regression of each of the variables on an indicator for giving birth in July 1977 where the sample includes only women who gave birth in June and July of 1977. For Panel B, we added to the sample women who gave birth in June and July of 1975, 1978, and 1979, and we regressed each of the variables on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter. In Panels C, D, and E, each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. The estimates in Panel C are from the sample of eligible mothers who gave birth in 1977, whereas the RD-DD estimates in Panel D additionally include eligible mothers who gave birth in 1975, 1978, and 1979. The RD-DD estimates in Panel E include only mothers who gave birth in 1975 as an additional control group. The pre-reform means of the indexes are standardized to be zero with a standard deviation of one. Coefficient estimates marked in italics are significant at the 10% level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Impact of the Reform on Pain of Mothers

	(1) Any	(2) Neck/Shoulder	(3) Arm	(4) Back	(5) Chest	(6) Leg/Hip
			Panel A			
Single difference	*-0.047****	*-0.031****	-0.010	*-0.029****	0.003	-0.004
	(0.016)	*(0.010)*	(0.009)	*(0.009)*	(0.004)	(0.009)
Observations	2647	2647	2434	2647	2434	2434
			Panel B			
DD	*-0.038***	*-0.039****	-0.018*	*-0.032****	0.001	-0.013
	(0.019)	*(0.011)*	(0.010)	*(0.010)*	(0.005)	(0.011)
Observations	10494	10494	9763	10494	9763	9763
			Panel C			
RD	*-0.052****	*-0.037****	*-0.023****	*-0.042****	0.003**	-0.012***
	(0.008)	*(0.003)*	*(0.003)*	*(0.002)*	(0.001)	(0.003)
Observations	6668	6668	6002	6668	6002	6002
			Panel D			
RD-DD	*-0.059****	*-0.025****	-0.014*	-0.014**	-0.002	-0.015**
	(0.011)	*(0.006)*	(0.008)	(0.006)	(0.003)	(0.006)
Observations	26756	26756	24794	26756	24794	24794
			Panel E			
RD-DD (1975 only)	*-0.039****	*-0.029****	-0.014	-0.019**	0.001	-0.007
	(0.011)	*(0.010)*	(0.009)	(0.009)	(0.004)	(0.009)
Observations	12251	12251	12251	12251	12251	12251
Pre-reform mean	0.249	0.078	0.050	0.059	0.010	0.058

Note: Panel A shows the coefficients from a regression of each of the variables on an indicator for giving birth in July 1977 where the sample includes only women who gave birth in June and July of 1977. For Panel B, we added to the sample women who gave birth in June and July of 1975, 1978, and 1979, and we regressed each of the variables on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter. In Panels C, D, and E, each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. The estimates in Panel C are from the sample of eligible mothers who gave birth in 1977, whereas the RD-DD estimates in Panel D additionally include eligible mothers who gave birth in 1975, 1978, and 1979. The RD-DD estimates in Panel E include only mothers who gave birth in 1975 as an additional control group. Coefficient estimates marked in italics are significant at the 10% level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impact of the Reform on Health Behaviors of Mothers

	(1) Smoking (Dummy)	(2) Exercise
	Panel A	
Single difference	*-0.052***	*0.165**
	(0.018)	*(0.091)*
Observations	2522	2516
	Panel B	
DD	*-0.054***	0.154*
	(0.021)	(0.083)
Observations	10090	10079
	Panel C	
RD	*-0.032***	*0.215***
	(0.006)	*(0.020)*
Observations	6293	6287
	Panel D	
RD-DD	*-0.075***	0.180***
	(0.013)	(0.073)
Observations	26398	26375
	Panel E	
RD-DD	*-0.063***	0.122*
	(0.019)	(0.071)
Observations	11919	11919
Pre-reform mean	0.317	3.217

Note: Panel A shows the coefficients from a regression of each of the variables on an indicator for giving birth in July 1977 where the sample includes only women who gave birth in June and July of 1977. For Panel B, we added to the sample women who gave birth in June and July of 1975, 1978, and 1979, and we regressed each of the variables on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter. In Panels C, D, and E, each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. The estimates in Panel C are from the sample of eligible mothers who gave birth in 1977, whereas the RD-DD estimates in Panel D additionally include eligible mothers who gave birth in 1975, 1978, and 1979. The RD-DD estimates in Panel E include only mothers who gave birth in 1975 as an additional control group. Coefficient estimates marked in italics are significant at the 10% level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Impacts of the Reform on Unpaid Leave, Income, and Labor Force Participation of Mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Income			Employed	
	Unpaid Leave Months	2 Years After Birth	5 Years After Birth	10 Years After Birth	2 Years After Birth	5 Years After Birth	10 Years After Birth
				Panel A			
RD	-0.205	-69.3	32.3	597.4	0.005	-0.003	-0.004
	(0.223)	(171.2)	(245.1)	(329.3)	(0.009)	(0.009)	(0.010)
Observations	6002	6002	6002	6002	6002	6002	6002
				Panel B			
RD-DD	0.017	-377.9	184.8	483.2	0.002	-0.009	-0.003
	(0.288)	(92.4)	(132.9)	(217.3)	(0.006)	(0.006)	(0.012)
Observations	24794	24794	24794	24794	24794	24794	24794
Pre-reform mean	9.613	26,789	39,569	85,333	0.761	0.773	0.882

Note: In Panels A and B, each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. The estimates in Panel A are from the sample of eligible mothers who gave birth in 1977, whereas the RD-DD estimates in Panel B additionally include eligible mothers who gave birth in 1975, 1978, and 1979. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Heterogeneous Impacts of the Reform on Metabolic Health of Mothers

	(1) BMI	(2) Obese	(3) Diabetes	(4) Blood Pressure	(5) Cholesterol Risk	(6) Cardiac Risk	(7) Index
Panel A: Complications at Birth							
RD	-0.649***	-0.035***	-0.004***	-1.432***	-0.004***	-0.002***	-0.198***
	(0.053)	(0.006)	(0.001)	(0.027)	(0.001)	(0.001)	(0.010)
Interaction	-0.332***	-0.024***	-0.002**	-0.338***	-0.003***	-0.002**	-0.071***
term	(0.061)	(0.002)	(0.001)	(0.058)	(0.000)	(0.001)	(0.017)
Panel B: C-Section							
RD	-0.870***	-0.041***	-0.010***	-1.737***	-0.000	-0.004***	-0.259***
	(0.081)	(0.006)	(0.000)	(0.090)	(0.000)	(0.000)	(0.016)
Interaction	0.359***	0.033***	0.008	-0.651	-0.002	0.001***	0.166***
term	(0.102)	(0.006)	(0.007)	(0.682)	(0.003)	(0.000)	(0.026)
Panel C: First Child							
RD	-0.874***	-0.044***	-0.007***	-1.322***	-0.007***	0.007***	-0.244***
	(0.084)	(0.005)	(0.00—)	(0.084)	(0.001)	(0.001)	(0.020)
Interaction	-0.049***	-0.018***	-0.002	-0.293**	-0.001	-0.003	-0.022**
term	(0.006)	(0.004)	(0.003)	(0.114)	(0.002)	(0.002)	(0.008)
Panel D: Single Mothers							
RD	-0.834**	-0.020***	-0.009***	-1.771***	-0.000	-0.002	-0.148***
	(0.262)	(0.005)	(0.002)	(0.069)	(0.004)	(0.002)	(0.044)
Interaction	-0.096***	-0.018***	-0.000	-0.057	-0.004**	-0.002**	-0.123***
term	(0.011)	(0.004)	(0.002)	(0.042)	(0.002)	(0.001)	(0.031)
Panel E: Below Median Household Earnings in 1975							
RD	-0.324***	-0.040***	-0.007***	-1.199***	-0.001	0.002*	-0.143***
	(0.056)	(0.004)	(0.001)	(0.088)	(0.001)	(0.001)	(0.014)
Interaction	-0.358***	-0.003	-0.005**	-0.683***	-0.000	-0.005***	-0.143***
term	(0.051)	(0.006)	(0.002)	(0.096)	(0.003)	(0.001)	(0.008)
Panel F: More Than 15 Years Between Birth and Survey							
RD	-0.725***	-0.037***	-0.004***	-1.681***	-0.003**	-0.005***	-0.258***
	(0.114)	(0.009)	(0.000)	(0.132)	(0.001)	(0.001)	(0.025)
Interaction	-0.017	-0.008	-0.004***	-0.093	-0.001	-0.001	0.010
term	(0.044)	(0.010)	(0.000)	(0.243)	(0.002)	(0.001)	(0.037)
Panel G: $\leq$ 3 Months Unpaid Leave							
RD	-0.842***	-0.034***	-0.006***	-1.512***	-0.001**	-0.005***	-0.230***
	(0.094)	(0.006)	(0.000)	(0.058)	(0.000)	(0.001)	(0.016)
Interaction	-0.333***	-0.020***	-0.004***	-0.500***	-0.003**	0.001	-0.041***
term	(0.057)	(0.002)	(0.001)	(0.052)	(0.002)	(0.001)	(0.007)
Observations	5993	6002	5997	5991	6002	6002	5982

Note: In all panels, we show the estimated discontinuity in the outcomes as a result of the maternity leave reform as well as the coefficient on the interaction term between the reform and the subgroup indicator. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. We allowed the trends to differ across subgroups. The estimates are from the sample of eligible mothers who gave birth in 1977. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Heterogeneous Impacts of the Reform on Self-Reported Health of Mothers

	(1) Mental Health Index	(2) General Health Index
	Panel A: Complications at Birth	
RD	-0.089***	-0.064***
	(0.011)	(0.009)
Interaction term	-0.082***	0.002
	(0.004)	(0.004)
	Panel B: C-Section	
RD	-0.122***	-0.064***
	(0.012)	(0.012)
Interaction term	-0.053	-0.047
	(0.061)	(0.055)
	Panel C: First Child	
RD	-0.152***	-0.069***
	(0.012)	(0.010)
Interaction term	0.004	0.039**
	(0.008)	(0.019)
	Panel D: Single Mothers	
RD	-0.123***	-0.064***
	(0.022)	(0.009)
Interaction term	0.039	-0.025***
	(0.033)	(0.006)
	Panel E: Below Median Household Earnings in 1975	
RD	-0.103***	-0.056***
	(0.015)	(0.005)
Interaction term	-0.036**	-0.019***
	(0.011)	(0.004)
	Panel F: More Than 15 Years Between Birth and Survey	
RD	-0.110***	-0.043***
	(0.004)	(0.003)
Interaction term	-0.041***	-0.027**
	(0.004)	(0.010)
	Panel G: $\leq$ 3 Months Unpaid Leave	
RD	-0.162***	-0.058***
	(0.010)	(0.011)
Interaction term	-0.082***	-0.098**
	(0.006)	(0.029)
Observations	6002	6002

Note: In all panels, we show the estimated discontinuity in the outcomes as a result of the maternity leave reform as well as the coefficient on the interaction term between the reform and the subgroup indicator. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. We allowed the trends to differ across subgroups. The estimates are from the sample of eligible mothers who gave birth in 1977. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Heterogeneous Impacts of the Reform on Pain of Mothers

	(1) Any	(2) Neck/Shoulder	(3) Arm	(4) Back	(5) Chest	(6) Leg/Hip
			Panel A: Complications at Birth			
RD	-0.030**	-0.033***	-0.029***	-0.030***	-0.001	-0.014***
	(0.012)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Interaction term	-0.019***	-0.011***	-0.011***	-0.006**	-0.000	-0.009***
	(0.004)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)
			Panel B: C-Section			
RD	-0.044***	-0.035***	-0.021***	-0.039***	0.004***	-0.014***
	(0.007)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Interaction term	0.005	0.000	-0.034	-0.012	-0.003	0.004
	(0.006)	(0.015)	(0.016)	(0.011)	(0.004)	(0.015)
			Panel C: First Child			
RD	-0.067***	-0.040***	-0.021***	-0.049***	0.003	-0.010***
	(0.008)	(0.004)	(0.003)	(0.002)	(0.003)	(0.002)
Interaction term	0.008	0.005	-0.008	0.003	-0.002	-0.005
	(0.007)	(0.004)	(0.005)	(0.003)	(0.002)	(0.003)
			Panel D: Single Mothers			
RD	0.001	-0.017	-0.021	-0.073***	0.030**	-0.020
	(0.007)	(0.017)	(0.016)	(0.012)	(0.011)	(0.013)
Interaction term	-0.058***	-0.015	-0.016	-0.011	0.014	0.005
	(0.002)	(0.019)	(0.016)	(0.016)	(0.010)	(0.016)
			Panel E: Below Median Household Earnings in 1975			
RD	-0.030***	-0.001	-0.010***	-0.028***	0.002	-0.011***
	(0.008)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Interaction term	-0.027***	-0.033***	-0.008**	-0.018***	-0.004	-0.002
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)
			Panel F: More Than 15 Years Between Birth and Survey			
RD	-0.044***	-0.022***	0.018***	-0.019	-0.002	-0.014***
	(0.002)	(0.005)	(0.004)	(0.006)	(0.001)	(0.003)
Interaction term	-0.007	-0.014	-0.008	-0.012	-0.005	-0.002
	(0.018)	(0.010)	(0.010)	(0.010)	(0.004)	(0.008)
			Panel G: $\leq$ 3 Months Unpaid Leave			
RD	-0.046***	-0.032***	-0.019***	-0.046***	0.001	-0.015***
	(0.009)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)
Interaction term	-0.012	-0.018***	-0.017***	-0.007	-0.004	-0.003
	(0.009)	(0.003)	(0.003)	(0.008)	(0.003)	(0.003)
Observations	6595	6595	6002	6595	6002	6002

Note: In all panels, we show the estimated discontinuity in the outcomes as a result of the maternity leave reform as well as the coefficient on the interaction term between the reform and the subgroup indicator. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. We allowed the trends to differ across subgroups. The estimates are from the sample of eligible mothers who gave birth in 1977. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Heterogeneous Impacts of the Reform on Health Behaviors of Mothers

	(1) Smoking (Dummy)	(2) Exercise
	Panel A: Complications at Birth	
RD	-0.033**	0.173**
	(0.004)	(0.054)
Interaction term	-0.021***	-0.080
	(0.003)	(0.057)
	Panel B: C-Section	
RD	-0.029***	0.209***
	(0.005)	(0.021)
Interaction term	0.018	0.054
	(0.013)	(0.068)
	Panel C: First Child	
RD	-0.061***	0.164***
	(0.002)	(0.018)
Interaction term	-0.006	0.011
	(0.008)	(0.009)
	Panel D: Single Mothers	
RD	-0.047***	0.180***
	(0.013)	(0.003)
Interaction term	-0.013	0.190***
	(0.015)	(0.018)
	Panel E: Below Median Household Earnings in 1975	
RD	-0.019***	0.120***
	(0.002)	(0.030)
Interaction term	-0.030**	0.083**
	(0.009)	(0.038)
	Panel F: More Than 15 Years Between Birth and Survey	
RD	-0.058***	0.269***
	(0.003)	(0.028)
Interaction term	-0.015	-0.016
	(0.010)	(0.013)
	Panel G: $\leq$ 3 Months Unpaid Leave	
RD	-0.036***	0.123***
	(0.004)	(0.025)
Interaction term	-0.038**	0.055
	(0.010)	(0.098)
Observations	6353	6353

Note: In all panels, we show the estimated discontinuity in the outcomes as a result of the maternity leave reform as well as the coefficient on the interaction term between the reform and the subgroup indicator. We used local linear regressions including triangular weights, a bandwidth of 3 months, and separate trends on each side of the discontinuity. We allowed the trends to differ across subgroups. The estimates are from the sample of eligible mothers who gave birth in 1977. Numbers in parentheses are heteroskedastic-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$