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The Impact of Paid Maternity Leave on Maternal Health[†]

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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, including BMI, blood pressure, pain, and mental health. The reform also increased health-promoting behaviors, such as exercise and not smoking. 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. (*JEL* I12, J13, J16, J32)

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

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

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 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 maternal health 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 and increase household income, particularly in cases where the leave is partially paid or unpaid, which may improve health. There may be further effects on health to the extent that maternity leave impacts subsequent labor market attachment and earnings. 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 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, Løken, and Salvanes (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 in the medium and long term. Various aspects of metabolic health around age 40 improved for mothers who were eligible for the reform, including BMI, blood pressure, and a summary index that aggregates the metabolic health measures. The reform significantly decreased the probability of experiencing chronic pain, improved self-reported mental and general health, and increased health-promoting behaviors, such as exercise and not smoking, around age 40. The effects are robust to adjusted inference for multiple hypothesis testing. We find the reform had larger effects on mothers who experienced complications at delivery, first-time mothers, and low-resource mothers (e.g., single mothers and those with below-median household income).

Due to limited data during the reform period, our ability to explore mechanisms underlying the health improvements around age 40 is constrained. In particular, we do not observe short-term health outcomes or behaviors for the full sample. We do, however, provide evidence that women who had their first child after July 1, 1977 experienced fewer health problems before and during their next pregnancy, suggestive of short-term health improvements. We also use information on maternal income around when the mother gave birth to examine whether the reform affected unpaid leave. The reform did not crowd out unpaid leave and did not significantly impact maternal income, implying 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 that includes many low-resource mothers.

We then briefly explore how a series of paid parental leave expansions that occurred 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, improved health. 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 leave generates larger benefits than expansions, at least at the levels we consider. Our finding of a nonmonotonic relationship between paid maternity leave length and maternal health improvements complements the literature that finds (i) short and moderate leave durations are associated with higher female employment while longer leaves may adversely impact employment and (ii) the introduction of short leave programs can improve children's outcomes, but extensions in leave have no impact on children's outcomes (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, Humlum, and Vejlin 2016; Guertzgen and Hank 2018).[2] First, we analyze many aspects of health using data that 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). Biomarkers predict well a variety of future health conditions and allow us to learn more about the mechanisms through which maternity leave affects maternal health than do 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 giving 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 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.[3] Last, we mainly focus on

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

[3] Our findings may not generalize well to mothers of low socioeconomic status given that no paid leave proposals under serious consideration in the United States offer full wage replacement, and disadvantaged women may be less able to afford partially paid leave.

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.

Our findings complement the documented improvements in children's outcomes that result from the introduction of maternity leave programs (Rossin-Slater 2017). Carneiro, Løken, and Salvanes (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 I reviews the related literature. In Section II, we provide background on the 1977 maternity leave reform. Section III describes the data, and Section IV presents our empirical strategy. We discuss our results in Section V and perform a variety of sensitivity analyses in Section VI. We provide a brief conclusion in Section VII.

I. Related Literature

A. *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.[4] Another line of literature analyzes the impacts of maternity leave on maternal employment and earnings.[5] 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.[6]

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 six 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 postpartum. They use variation in state maternity leave policies to instrument for leave length and find longer maternity leave (paid or unpaid) is

[4] 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, Løken, and Salvanes (2015); Dahl et al. (2016).

[5] See, for example, Baker and Milligan (2008a), Lalive and Zweimüller (2009), Lalive et al. (2014), Schönberg and Ludsteck (2014), and Stearns (2017).

[6] Several studies in the public health literature focus on postpartum employment and maternal health, and some 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, McGovern, and Dowd (2014), as well as Staehelin, Bertea, and Stutz (2007); and Aitken et al. (2015) for reviews of the literature.

significantly associated with decreased depressive symptoms.[7] In related work, Chatterji and Markowitz (2012) consider maternal health nine months after giving birth in 2001. Using cross-sectional variation in local labor market conditions, child care costs, and state maternity leave policies as instruments for leave length, they find longer leave is associated with decreased depressive symptoms and improved 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 at age 50 and over using data from the Survey of Health, Ageing and Retirement in Europe. 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. Bullinger (2019) finds suggestive improvements in maternal self-reported mental health within a year postpartum after California's implementation of partially paid family leave in 2004.[8]

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, Humlum, and Vejlin (2016) focus on a reform in Denmark in 2002 that increased the length 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 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 3 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.[9]

[7] 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.

[8] 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.

[9] They attribute their findings to the expansion particularly inducing those with poor prebirth health to reenter the labor market. Carneiro, Løken, and Salvanes (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 prebirth income.

Our paper is also related to Carneiro, Løken, and Salvanes (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, Løken, and Salvanes (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 six 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, and divorce.[10] Neither Carneiro, Løken, and Salvanes (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.

B. *Postpartum Health*

A number of studies in the public health literature document the frequency and duration of health problems after childbirth.[11] These studies show that postpartum health problems are common, with some concluding that full recovery from childbirth can take more than six months. The long-term effects of postpartum health problems are not well-established as studies generally do not consider health beyond one or two years postpartum.

[10] The expansions were substantially smaller than the 1977 reform, increasing paid leave by two to four weeks.

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

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, Brown, and West 2003), and up to 20 percent of women retain 5 kilograms or more 6 to 18 months postpartum (Gunderson and Abrams 1999). Another common concern is postpartum mental health, particularly depression. Meta-analyses suggest the prevalence of postpartum depression is 13 to 19 percent (O'Hara and Swain 1996, Gavin et al. 2005). In a US 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).

II. Maternity Leave Reform

In 1956, maternity leave benefits were granted to women in Norway for the first time.[12] The benefits provided eligible mothers with up to 12 weeks of unpaid 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 prebirth 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 one 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.[13]

For our empirical strategy, it is important that mothers could not change their eligibility status after the reform was announced. As explained in Carneiro, Løken, and Salvanes (2015), the reform was largely unexpected and introduced at the end of the sitting Parliament's term along with several other legislative changes.[14] The

[12] Our discussion of maternity leave in Norway and the 1977 reform follows from Carneiro, Løken, and Salvanes (2015).

[13] 10,000 NOK in 1977 is approximately $5,600 in 2018.

[14] 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

government report on the reform was made official on April 15, 1977 and approved on June 13 of that year. National newspapers did not report on the reform prior to June 1977 (Carneiro, Løken, and Salvanes 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 it difficult to change eligibility status in the short term.

III. Data

We use the Norwegian Registry data (Statistics Norway 1967–2017; Statistics Norway 1970–2017), a linked administrative dataset that covers the Norwegian population. 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.

A. *Birth Data*

The data on births are obtained from the Medical Birth Registry of Norway (Norwegian Institute of Public Health 1967–2017), which contains records for all births as long as the gestation period was at least 16 weeks. The records include information on the exact date 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.[15] The data also contain 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.

B. *Health Data*

The data on mothers' health come from the Cohort of Norway (CONOR) data (Norwegian Institute of Public Health 1994–2003) and the National Health Screening Service's Age 40 Program data (Norwegian Institute of Public Health 1988–2003), 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. 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.

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, Løken, and Salvanes 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.

[15] We have day-of-birth data up to 1980, and month-of-birth data thereafter. Thus, when we consider the subsequent leave extensions, we only have information on month of birth.

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.[16]

The data allow us to analyze self-reported health measures 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 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). 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, Liebman, and Katz (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.[17] Aggregating the measures in this way improves statistical power (Kling, Liebman, and Katz 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 create a summary standardized index for self-reported mental health. Individuals are asked separate questions about how nervous, anxious, depressed, irritated, lonely, calm, and happy they were during the last two weeks. They could respond with $\{\text{no}, \text{a little}, \text{quite a bit}, \text{a lot}\}$. We follow Black, Devereux, and Salvanes (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

[16] Black, Devereux, and Salvanes (2015) provide a detailed description of the health data and representativeness of the sample.

[17] We follow the approaches of Kling, Liebman, and Katz (2007) and Hoynes, Schanzenbach, and Almond (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.

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.[18] We also include a summary index for self-reported general health consisting of two 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 three 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 four mutually exclusive options: (i) sedentary activities like reading and watching television; (ii) light activities like walking and cycling; (iii) moderate activities and sports like running, swimming, and cross-country skiing; (iv) vigorous activities like hard exercise and competitive sports. We create an exercise score that takes on values $\{1, 2, 3, 4\}$ with higher values indicating increased physical activity. We also consider an indicator for any active exercise, corresponding to categories (ii) through (iv).

C. *Earnings Data*

Earnings data are obtained from the tax registry (Statistics Norway 1970–2017). Earnings are measured as annual earnings for taxable income, and include labor earnings, taxable sickness benefits, unemployment benefits, and parental leave payments.[19] They are not top-coded.

D. *Determining Leave Eligibility and Take-up*

We cannot measure employment in the ten 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, Løken, and Salvanes (2015) and define eligibility status based on whether the woman earned at least 10,000 NOK in the

[18] We only consider the mental health index and not its individual components. 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).

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

calendar year before giving birth. Given the law additionally based eligibility on employment in the 10 months prior to childbirth, we may overstate the number of eligible mothers.[20]

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, Løken, and Salvanes (2015) claim that take-up of the reform was 100 percent for eligible mothers, meaning they took the full 4 months of paid leave. They provide four pieces of evidence, which we recap 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 four 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, Løken, and Salvanes 2015; Dahl et al. 2016). In addition, women were likely well-informed from the start of the policy. Local and national newspaper coverage of the reform in late June 1977 involved large ads that described the policy, including leave length and eligibility, and instructed individuals to contact their local social security office for more details.[21]

E. *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 nonreform years (1975, 1978, and 1979) and are observed in the health datasets. To gain a sense of the representativeness of our sample, in online 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

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

[21] See, for example, *Verdens Gang*, June 30, 1977.

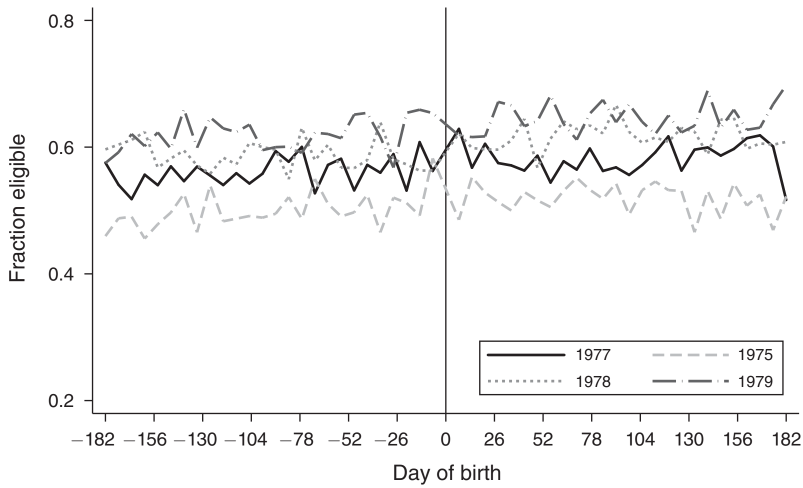

Figure 1. Proportion of Mothers Eligible for Paid Maternity Leave

Notes: The figure shows the fraction of eligible mothers (i.e., those with earnings of at least 10,000 NOK in the calendar year before giving birth) among all mothers observed in the health datasets by birth week of the child in 1975, 1977, 1978, and 1979. The vertical line denotes July 1 (normalized to zero).

mothers only. Figure 1 shows the proportion of eligible mothers in our sample by birth week of the child in 1975, 1977, 1978, and 1979. Figure 2 displays the number of children born in 1975, 1977, 1978, and 1979 to eligible mothers in our sample by week of birth. There is no unusually large spike in eligibility or the number of children born to eligible women after July 1, 1977. We take this as evidence that eligibility and delivery date manipulation are not serious issues in our data. In Section V, 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.

IV. Empirical Strategy

We estimate the medium- and long-term impacts of the 1977 maternity leave reform on maternal health by comparing the health of eligible mothers who had children immediately before and 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, Løken, and Salvanes (2015) and we use their notation. Let E_i denote whether woman i was entitled to paid leave benefits, which is a deterministic function of the date she gave birth X_i:

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

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.

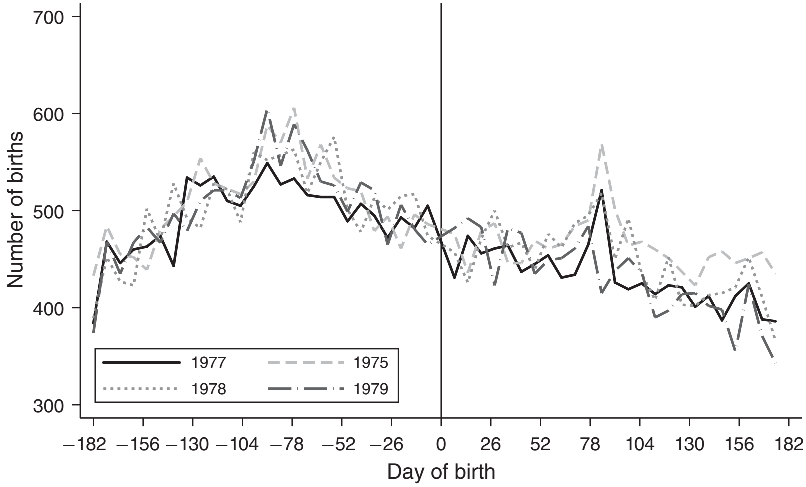

Figure 2. Number of Children Born to Eligible Mothers

Notes: The figure shows the number of children born in 1975, 1977, 1978, and 1979 to eligible mothers (i.e., those with earnings of at least 10,000 NOK in the calendar year before giving birth) observed in the health datasets by week of birth. The vertical line denotes July 1 (normalized to zero).

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

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

where $y_i(1)$ is the health outcome of woman i in the presence of the reform, and $y_i(0)$ is her health outcome in the absence of the reform. If $E\big[y_i(1)\,|X_i = c\big]$ and $E\big[y_i(0)\,|X_i = c\big]$ are continuous in x (more importantly, there is continuity at $x = c$), we can estimate

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

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, Todd, and Van der Klaauw (2001) with the triangle kernel, a bandwidth of 90 days, and separate trends on each side of the discontinuity. We use heteroskedastic-robust standard errors as suggested in Lee and Lemieux (2010).[22]

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 (RD-DD) 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 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).[23] The RD-DD design incorporates any outcome discontinuity that occurs for mothers who gave birth in July in these nonreform 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 nonreform 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 nonreform years, and then identifying the difference in the threshold breaks for the two groups.

It is important to note that because we do not have information on leave taken, we estimate an intent-to-treat (ITT) effect among mothers exposed to the reform. Given the arguments above that take-up of the reform was likely close to 100 percent, the ITT effect is a good estimate of the treatment effect on the treated (TOT). If anything, the ITT may underestimate the TOT to the extent that our eligibility definition overstates the number of eligible mothers and there were mothers who did not take up the paid leave, perhaps because they were unaware of the policy (though we believe this group is small).

V. Results

A. *Balance of Treatment and Control Groups*

We first show how observable prereform 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 day on which they gave birth. We do this to check for balance between the treatment and control groups. A lack of balance suggests some mothers may have manipulated their delivery date. The results of this check are shown in Figure 3. We plot the unrestricted weekly means and the fitted values from a local linear regression applied to each side of the cutoff. We find

[22] We do not cluster standard errors by date of birth because Kolesár and Rothe (2018) show that standard errors clustered by a discrete running variable have poor coverage properties.

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

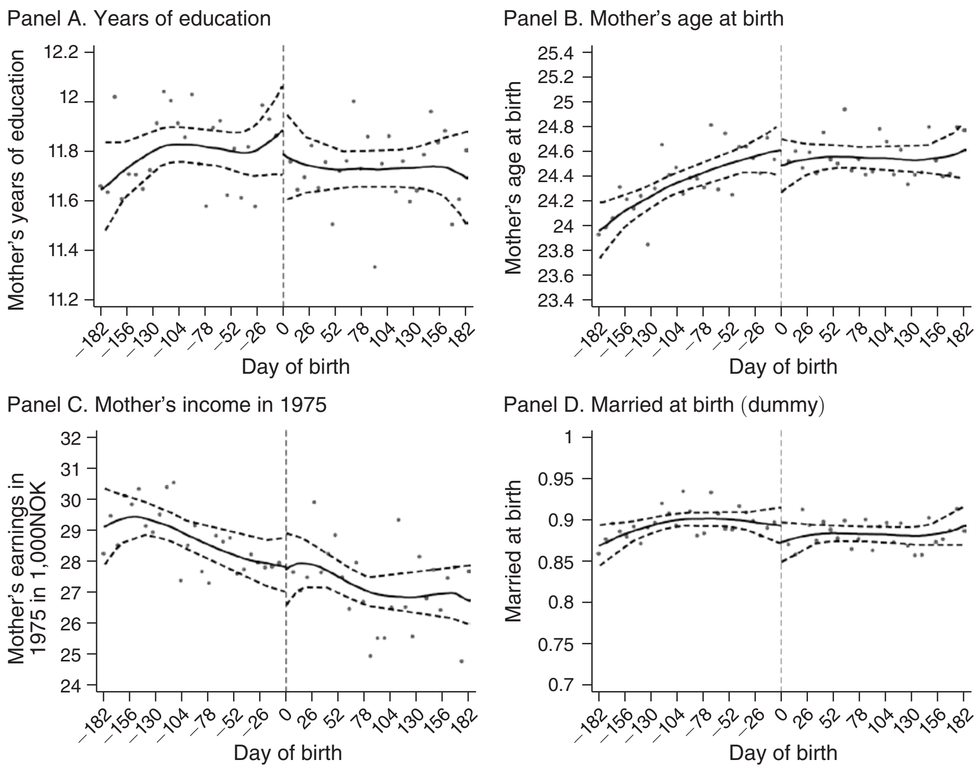

Figure 3. Mothers' Prereform Characteristics

Notes: The figure plots prereform 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-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). 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.

the characteristics are stable across birth months and there is no discontinuity after July 1, 1977.[24] 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.

[24] 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 1, 1977 in either case. We also find no discontinuity at the cutoff in the probability that eligible mothers are observed in the health surveys. Results are available upon request.

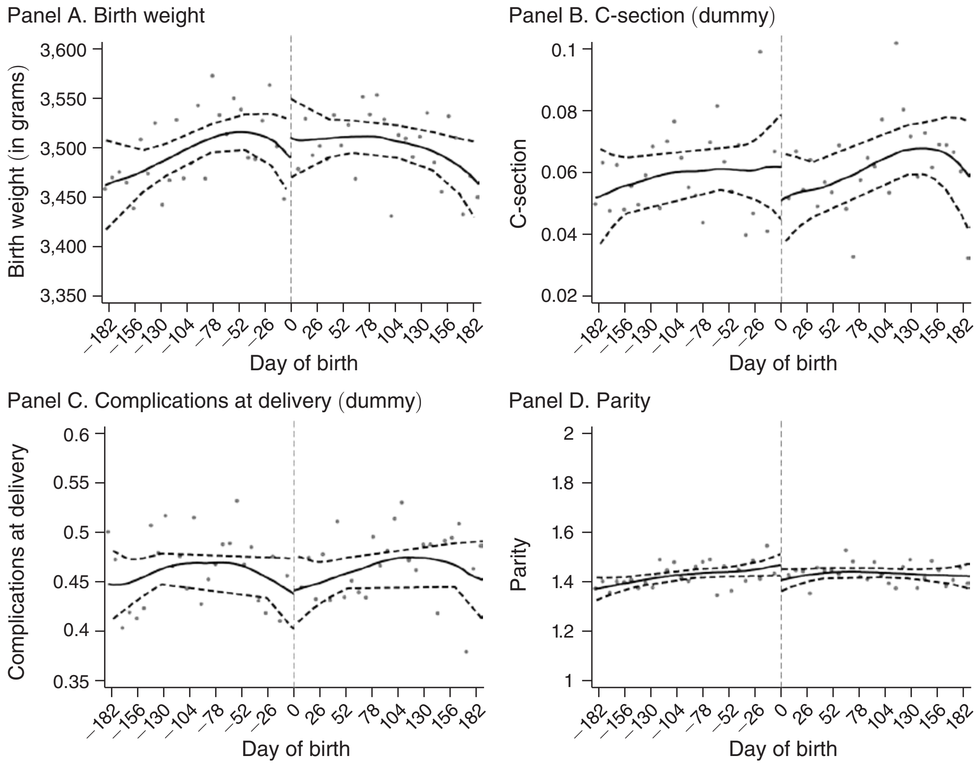

Figure 4. Mothers' Birth Experience Characteristics

Notes: The figure plots birth 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-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). 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.

B. *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, and prereform means for the outcomes are displayed in the bottom row. For the sake of comparison, we show results from five 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 RD estimates using mothers who gave birth in 1977 and a 90-day bandwidth. In panel D, we show the estimates from the RD-DD specification where we use mothers who gave birth in 1975, 1978, and 1979 as a control group. In panel E, we present RD-DD 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 RD and RD-DD 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 Figures 5 to 8, we present graphically the RD results, and in online Appendix Figures A1 to A5, we present graphs corresponding to the RD-DD results.

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.

The reform led to significant improvements in the metabolic health of mothers around age 40 (Table 1 and Figure 5). BMI decreased by 0.6 to 0.9 kg/m^2, a 2.5 to 3.7 percent decline relative to prereform mean BMI, and the probability of being obese declined by about 3 percentage points (except in one RD-DD specification), a 39 percent decrease. In the left (right) panel of online Appendix Figure A6, we plot the BMI density functions for women who gave birth in June and July 1977 (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 those giving birth in 1979. The Kolmogorov-Smirnov test statistic reveals that we can reject the null hypothesis of equality of the 1977 distributions 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 similar to those in Courtemanche (2011), who finds after 7 years, a $1 increase in the price of gasoline in the United States 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 1 to 2.2 millimeters of mercury (mmHg) in response to the reform, a 1.3 to 2.9 percent decline. We also find the reform decreased the probability of experiencing hypertension by about 3 percentage points, a 10 percent reduction (results available upon request). To put the results in perspective, in the RAND Health Insurance Experiment in the United States, 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 0.3 to 0.6 percentage points, but there were no significant effects on cardiac risk. The reform led to about a 0.2 standard deviation improvement in the metabolic syndrome index, which aggregates the metabolic health measures. The effects on BMI, blood

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

	BMI (1)	Obese (2)	Diabetes (3)	Blood pressure (4)	Cholesterol risk (5)	Cardiac risk (6)	Index (7)
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	2,430	2,434	2,431	2,428	2,434	2,434	2,424
Panel B							
DD	*−0.576*	−0.029	−0.007	−0.995	−0.006	−0.002	*−0.162*
	(0.148)	(0.010)	(0.004)	(0.394)	(0.002)	(0.003)	*(0.044)*
Observations	9,644	9,665	9,650	9,650	9,665	9,665	9,629
Panel C							
RD	*−0.813*	*−0.028*	−0.005	*−1.815*	−0.003	−0.002	*−0.212*
	(0.112)	*(0.009)*	(0.002)	*(0.103)*	(0.001)	(0.004)	*(0.025)*
Observations	7,150	7,160	7,154	7,147	7,160	7,160	7,138
Panel D							
RD-DD	*−0.940*	−0.010	−0.003	*−2.244*	−0.002	−0.001	*−0.164*
	(0.109)	(0.008)	(0.003)	*(0.291)*	(0.002)	(0.002)	*(0.028)*
Observations	29,585	29,638	29,590	29,597	29,638	29,638	29,546
Panel E							
RD-DD (1975 only)	*−0.910*	−0.025	−0.001	*−2.177*	−0.003	0.002	*−0.224*
	(0.101)	(0.011)	(0.004)	*(0.440)*	(0.002)	(0.003)	*(0.042)*
Observations	13,859	13,882	13,866	13,865	13,882	13,882	13,843
Prereform mean	24.275	0.077	0.006	75.745	0.005	0.005	0.001

Notes: 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 90 days, 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 prereform 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 percent level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors.

pressure, and the metabolic syndrome index survive the adjustments for multiple hypothesis testing.

Our estimates show that the reform improved self-reported health, generating about a 0.1 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 6). These impacts are significant 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, Løken, and Salvanes 2019). Mothers with access to longer leave may have had more time to attend these meetings and obtain services

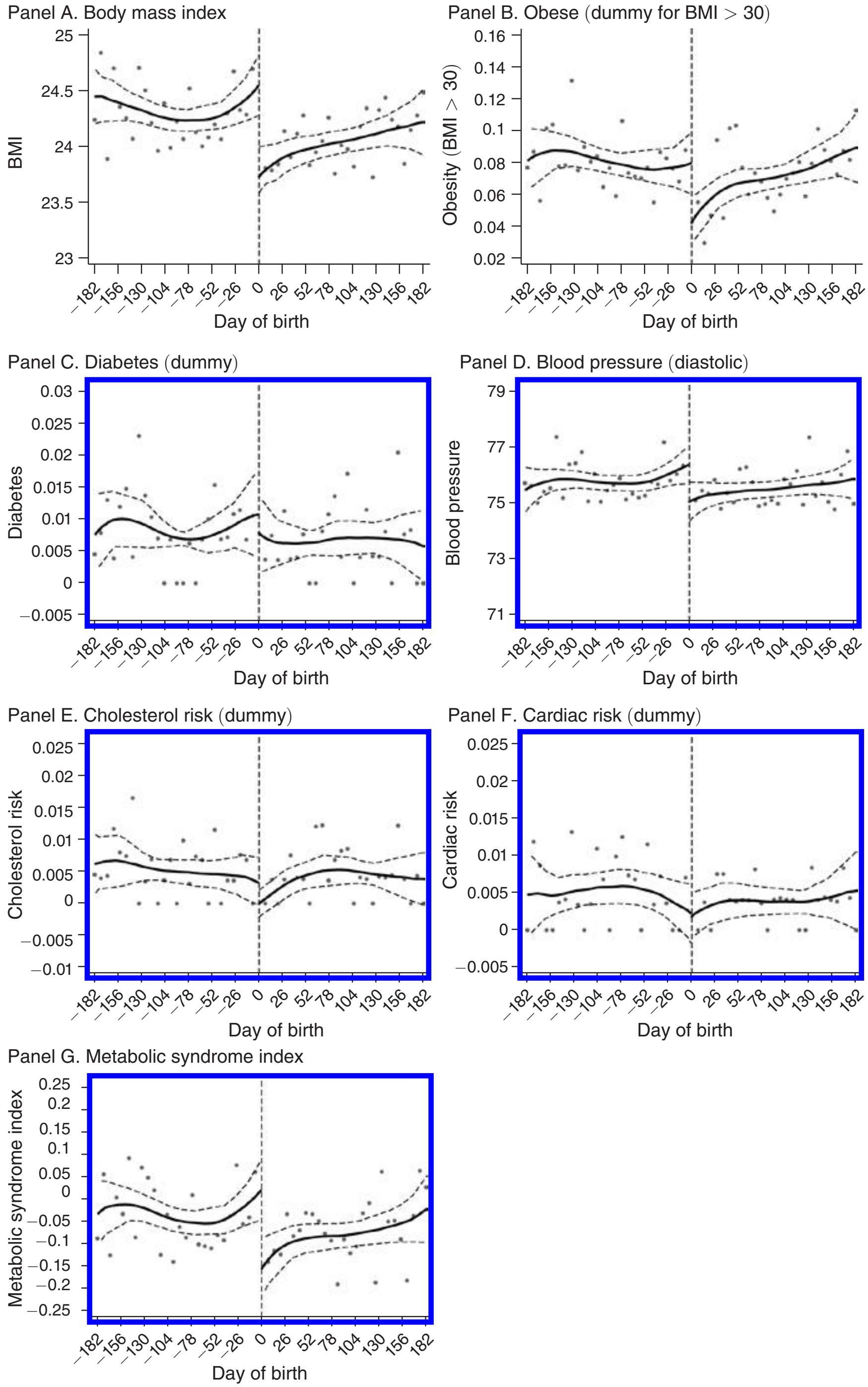

Figure 5. Impact of the Reform on Mothers' Metabolic Health

Notes: The figure plots metabolic health outcomes around age 40 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-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). 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 2—IMPACT OF THE REFORM ON SELF-REPORTED HEALTH OF MOTHERS

	Mental health index (1)	General health index (2)
Panel A		
Single difference	*−0.087*	*−0.116*
	(*0.031*)	(*0.031*)
Observations	2,434	2,434
Panel B		
DD	*−0.099*	*−0.121*
	(*0.036*)	(*0.035*)
Observations	9,763	9,763
Panel C		
RD	*−0.101*	*−0.123*
	(*0.012*)	(*0.025*)
Observations	7,160	7,160
Panel D		
RD-DD	*−0.114*	*−0.054*
	(*0.023*)	(*0.014*)
Observations	29,638	29,638
Panel E		
RD-DD (1975 only)	*−0.113*	*−0.129*
	(*0.034*)	(*0.021*)
Observations	13,882	13,882
Prereform mean	−0.002	0.005

Notes: 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 90 days, 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 prereform 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 percent level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors.

from the health care centers, which could explain, in part, the mental health effects. Furthermore, Chatterji and Markowitz (2005, 2012) find longer maternity leave (in the United States) is associated with decreased depressive symptoms. Given they study mental health up to two 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 3.7 to 4.8 percentage points, a 16 to 20 percent decline relative to the prereform mean, with the improvements driven by decreases in neck and shoulder and back

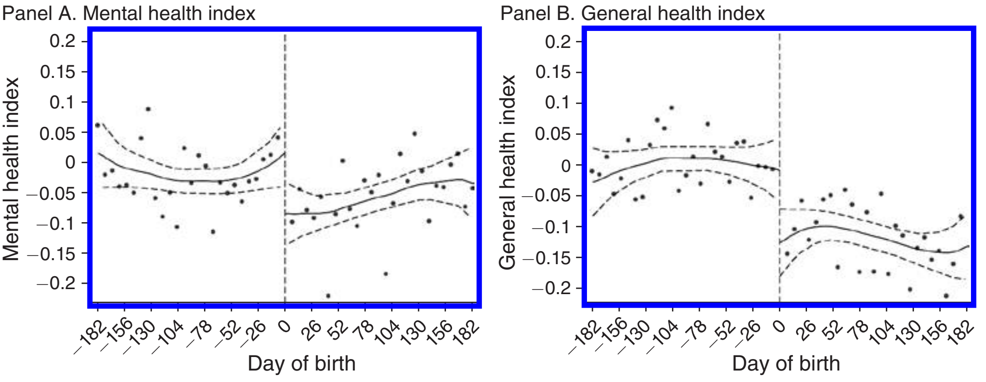

Figure 6. Impact of the Reform on Mothers' Self-Reported Health

Notes: The figure plots self-reported health outcomes around age 40 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-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). 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.

pain (Table 3 and Figure 7). The effects on any, neck and shoulder, and in some cases back pain survive the adjustments for multiple hypothesis testing.[25]

We also consider the impact of the reform on health behaviors. The reform decreased the probability of daily smoking around age 40 by 4.9 to 5.7 percentage points, a 16 to 18 percent decrease relative to the prereform mean (Table 4 and Figure 8) The exercise score increased by about 0.2, and the probability of any active exercise increased by 8.5 to 12 percentage points, a 14 to 20 percent increase. The impacts on smoking are statistically significant after accounting for multiple hypothesis testing across all specifications, but the effects on exercise survive only in some cases.[26] The changes in these health-promoting activities may reflect increased efforts by mothers 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 during the postpartum period (Bellows-Riecken and Rhodes 2008). If expanded leave delayed the return to work, this may have allowed mothers to engage in or return to regular exercise, and such behavior may persist well after the postpartum period.

[25] The declines in musculoskeletal pain could be explained, in part, by the BMI declines, 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 RD-DD specifications (see online Appendix Table A2).

[26] As a sensitivity check, we adjusted p-values for multiple hypothesis testing without grouping outcomes into separate families. In both the RD and RD-DD analyses, the effects on BMI, blood pressure, the metabolic syndrome index, the mental health index, pain (any), and smoking survive at the 10 percent level. The effects on obesity, the general health index, neck and shoulder pain, back pain, and any active exercise additionally survive in the RD analysis.

TABLE 3—IMPACT OF THE REFORM ON PAIN OF MOTHERS

	Any (1)	Neck/shoulder (2)	Arm (3)	Back (4)	Chest (5)	Leg/hip (6)
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	2,647	2,647	2,434	2,647	2,434	2,434
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	10,494	10,494	9,763	10,494	9,763	9,763
Panel C						
RD	*−0.037*	*−0.045*	*−0.019*	*−0.039*	0.002	−0.013
	(*0.008*)	(*0.013*)	(*0.005*)	(*0.012*)	(0.005)	(0.007)
Observations	7,752	7,752	7,160	7,752	7,160	7,160
Panel D						
RD-DD	*−0.048*	*−0.020*	−0.014	−0.014	0.000	−0.015
	(*0.012*)	(*0.007*)	(0.006)	(0.007)	(0.003)	(0.007)
Observations	31,645	31,645	29,638	31,645	29,638	29,638
Panel E						
RD-DD (1975 only)	*−0.047*	*−0.036*	−0.018	−0.021	0.001	−0.011
	(*0.008*)	(*0.010*)	(0.009)	(0.010)	(0.005)	(0.010)
Observations	14,529	14,529	13,882	14,529	13,882	13,882
Prereform mean	0.234	0.074	0.048	0.058	0.010	0.052

Notes: 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 90 days, 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 percent level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors.

C. *Mechanisms*

Our results suggest the 1977 maternity leave reform generated significant medium- and long-term improvements in maternal health. Due to limited data around the time of the reform, we are constrained in our ability to explore the mechanisms mediating these improvements. In particular, we cannot comprehensively examine whether maternal health improved in the short term as we do not observe health measures prior to age 40 for the full sample. We do, however, observe some maternal health information in the birth registry data, which we use to provide suggestive evidence of short-term health improvements among a subset of women who had another child after the reform. Furthermore, the health 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.

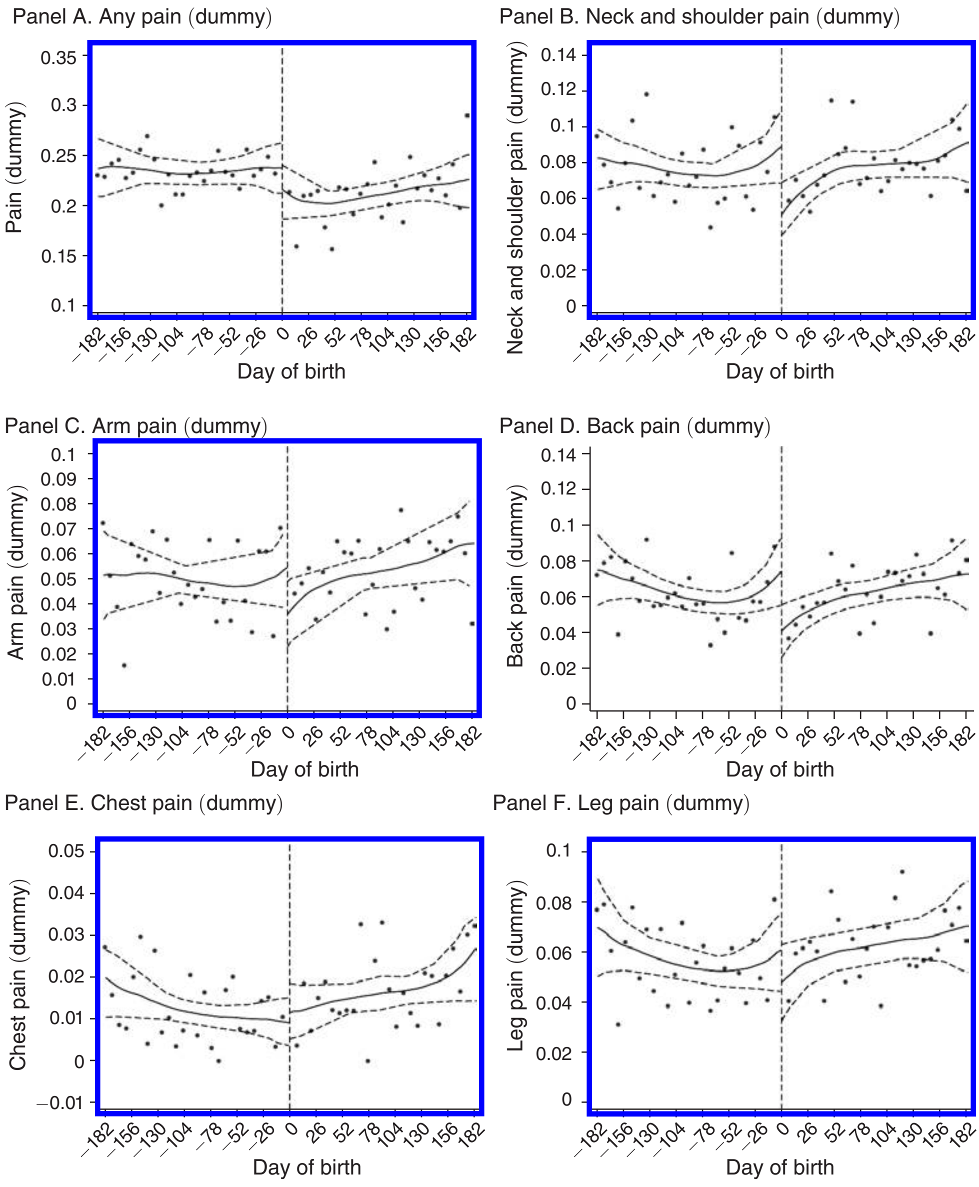

Figure 7. Impact of the Reform on Mothers' Pain

Notes: The figure plots the probability of having pain around age 40 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-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). 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.

Short-Term Health Effects.—We use the limited information in the birth registry to 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 major medical diagnosis prior to one's

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

	Smoking (dummy) (1)	Exercise score (2)	Any active exercise (3)
Panel A			
Single difference	*−0.052*	0.165	0.085
	(*0.018*)	(0.091)	(0.038)
Observations	2,522	2,516	2,516
Panel B			
DD	*−0.054*	0.154	0.087
	(*0.021*)	(0.083)	(0.039)
Observations	10,090	10,079	10,079
Panel C			
RD	*−0.052*	*0.170*	*0.098*
	(*0.014*)	(*0.060*)	(*0.031*)
Observations	7,506	7,506	7,506
Panel D			
RD-DD	*−0.049*	0.179	*0.120*
	(*0.013*)	(0.069)	(*0.042*)
Observations	30,654	30,654	30,654
Panel E			
RD-DD (1975 only)	*−0.057*	*0.198*	0.107
	(*0.019*)	(*0.059*)	(0.043)
Observations	14,196	14,196	14,196
Prereform mean	0.310	3.174	0.605

Notes: 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 90 days, 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 percent level after adjusting for multiple hypothesis testing. Numbers in parentheses are heteroskedastic-robust standard errors.

second pregnancy, including asthma, hypertension, kidney disease, heart disease, and diabetes; any medical diagnosis prior to the second pregnancy, including the above-mentioned conditions as well as others like pain and skin problems; and diabetes and hypertension during the second pregnancy. The results are presented in online Appendix Table A3. Although these health problems are somewhat rare, the reform significantly decreased the probability of experiencing such problems. For example, the probability of experiencing hypertension during the second pregnancy declined by 0.2 to 0.4 percentage points, a 6 to 12 percent decrease. These estimates are consistent with the reform generating short-run health improvements, which may have persisted into a woman's forties.

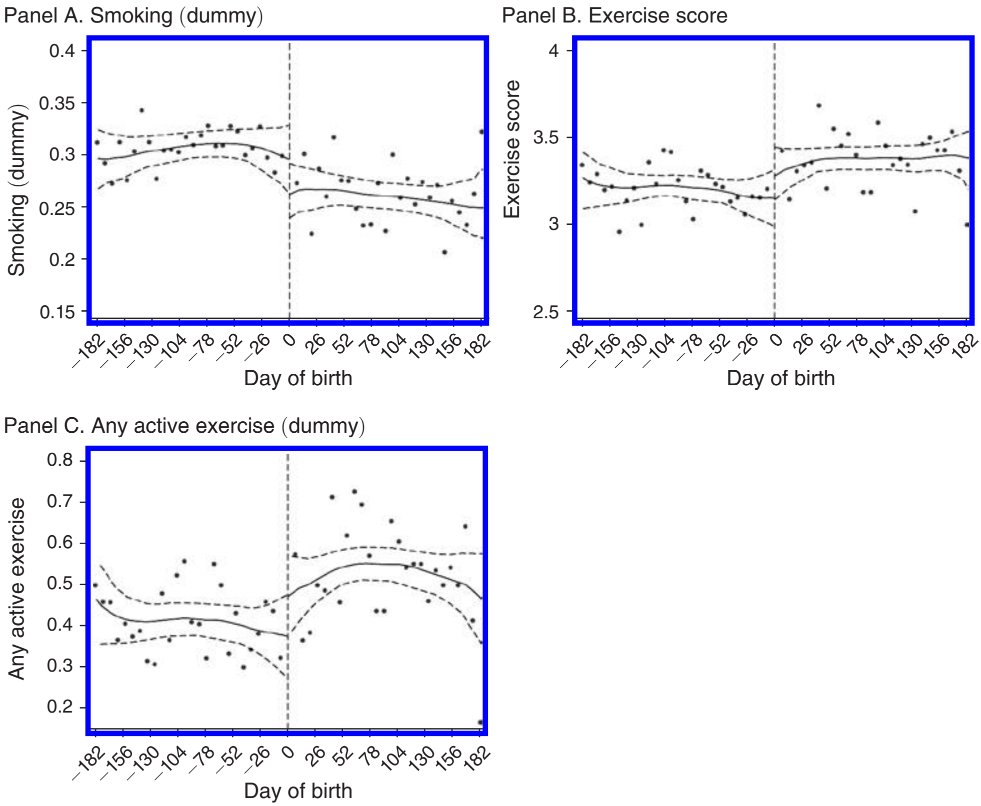

Figure 8. Impact of the Reform on Mothers' Health Behaviors

Notes: The figure plots health behaviors around age 40 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-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). 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.

Time at Home, *Income*, *and Employment Effects*.—Earlier we reviewed the evidence suggesting 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, we can 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, Løken, and Salvanes (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 prebirth earnings are a good approximation for post-birth inflation-adjusted potential earnings as well as full take-up of the four months of paid leave.[27] The intuition is as follows. If a woman's income increased (decreased) after childbirth, this suggests a decrease (increase) in the amount of unpaid leave taken. 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, with about 15 percent of women taking 3 months or less.[28]

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, Løken, and Salvanes (2015), we find no significant effect on unpaid leave, and given the 95 percent confidence intervals, we can rule out changes of more than half a month.[29] Thus, the reform did not crowd out unpaid leave, but rather increased the total amount of time a woman spent at home by about four months.

The 1977 reform allowed women to return to work up to 16 months after giving birth. This extended job protection may have strengthened the labor market attachment of mothers, which may have increased maternal income. Such employment and income changes 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 results are presented in columns 2 to 7 of Table 5. We find no significant impact of the reform on short-, medium-, or long-term income or employment, with point estimates that are very small.[30] Taken together, these results suggest more time at home, not income effects, led to the maternal health improvements.

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).[31]

Breastfeeding.—One potential channel underlying the health improvements that is consistent with mothers spending more time at home is breastfeeding. The economics literature suggests paid maternity leave introductions and expansions

[27] We divide earnings in 1976 by 12 to obtain prebirth monthly income. We then calculate total earnings in 1977 to 1979 and divide by prebirth monthly income, yielding a predicted number of months of unpaid leave during the first 24 months after childbirth.

[28] 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.

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

[30] 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. These results are in line with Carneiro, Løken, and Salvanes (2015) who also find no significant impact of the 1977 reform on women's employment or income. In other country settings, changes in maternity leave policy have generated changes in maternal employment (see the summaries in Olivetti and Petrongolo 2017, Rossin-Slater 2017).

[31] Carneiro, Løken, and Salvanes (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.

Table 5—Impacts of the Reform on Unpaid Leave, Income, and Employment of Mothers

		Income			Employed		
	Unpaid leave (months) (1)	2 years after birth (2)	5 years after birth (3)	10 years after birth (4)	2 years after birth (5)	5 years after birth (6)	10 years after birth (7)
Panel A							
RD	−0.050	−13.3	24.0	45.2	−0.002	−0.001	−0.000
	(0.207)	(12.9)	(18.3)	(30.4)	(0.002)	(0.002)	(0.006)
Observations	7,160	7,160	7,160	7,160	7,160	7,160	7,160
Panel B							
RD-DD	−0.019	−13.7	11.1	10.9	0.002	−0.002	−0.006
	(0.183)	(45.8)	(10.5)	(17.1)	(0.002)	(0.002)	(0.009)
Observations	29,638	29,638	29,638	29,638	29,638	29,638	29,638
Prereform mean	9.267	24,523	36,122	81,758	0.756	0.767	0.891

Notes: 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 90 days, 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.

significantly increase breastfeeding duration, but not initiation, across a variety of country settings (Baker and Milligan 2008b; Huang and Yang 2015; Kottwitz, Oppermann, and Spiess 2016). We do not have data on breastfeeding during this time, and therefore, cannot analyze breastfeeding behavior. However, given the findings in the literature, it is reasonable to believe the reform changed breastfeeding duration for some mothers.[32]

Whether breastfeeding affects maternal health is an open question. Biologically, during pregnancy, fat stores accumulate, and insulin resistance and lipid and trigylceride levels increase to support the fetus and in anticipation of lactation. According to the reset hypothesis, breastfeeding mobilizes these energy stores and may "reset" maternal metabolism. If the mother does not breastfeed or does so for a short duration, the fat stores are retained and the effects of pregnancy on glucose and lipid metabolism may persist for a longer period, potentially increasing the risk of metabolic disease (Stuebe and Rich-Edwards 2009). Most studies in the public health literature rely on observed variation in breastfeeding across mothers. They generally find longer breastfeeding duration is associated with lower maternal weight and blood pressure as well as reduced risk of breast cancer, ovarian cancer, postpartum depression, diabetes, and cardiovascular disease (see the summaries in Ip et al. 2007, American Academy of Pediatrics 2012, Chowdhury et al. 2015). However, there are likely unobserved confounders that impact breastfeeding behavior and maternal health, biasing the effects in these observational studies.

Due to ethical concerns, there is little experimental evidence on the maternal health effects of breastfeeding. The PROBIT study was a large randomized trial

[32] Liestøl, Rosenberg, and Walløe (1988) document trends in breastfeeding from 1860 to 1984 in Norway using data from three 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.

conducted in Belarus in the 1990s, in which some mothers received breastfeeding guidance and support and some did not. At 11.5 years postpartum, the longer breastfeeding duration and exclusivity induced by the intervention led to small but statistically insignificant improvements in maternal blood pressure and adiposity (e.g., BMI, body fat) (Oken et al. 2013). A much smaller study in Honduras randomized some women to continue exclusive breastfeeding from 4 to 6 months postpartum and other women to introduce complementary foods.[33] Women who breastfed exclusively for 6 compared to 4 months lost significantly more weight (0.6 kg) and BMI (0.4 kg/m^2) by 6 months postpartum (Dewey et al. 2001).

In sum, the health improvements we estimate are consistent with the breastfeeding benefits found in the nonexperimental public health literature and the Honduras study. However, given the lack of causal evidence on breastfeeding and maternal health, we simply speculate that breastfeeding could be an important channel and believe this is an avenue for future work.

D. *Heterogeneous Effects*

Next, we examine whether the effects of the reform varied with characteristics of mothers and their birth experience. For brevity, we only show the RD estimates, but results from the RD-DD 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.[34]

The results are presented in Tables 6 to 9. For mothers who experienced complications at birth, the reform had a stronger effect on the metabolic health measures, mental health, pain, and smoking compared to mothers without complications. 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 syndrome 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).[35]

First-time mothers were more affected by the reform relative to non-first-time mothers, but only with respect to metabolic health measures, particularly

[33] The World Health Organization recommends exclusive breastfeeding for six months with continued breastfeeding along with complementary foods up to two years of age.

[34] 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.

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

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

	BMI (1)	Obese (2)	Diabetes (3)	Blood pressure (4)	Cholesterol risk (5)	Cardiac risk (6)	Index (7)
Panel A. Complications at birth							
RD	−0.736	−0.030	−0.005	−1.449	−0.006	−0.002	−0.215
	(0.077)	(0.003)	(0.002)	(0.067)	(0.001)	(0.001)	(0.016)
Interaction term	−0.240	−0.015	−0.002	−0.514	−0.003	−0.001	−0.050
	(0.065)	(0.002)	(0.001)	(0.052)	(0.002)	(0.001)	(0.013)
Panel B. C-section							
RD	−0.812	−0.029	−0.010	−1.096	−0.002	−0.001	−0.212
	(0.082)	(0.006)	(0.002)	(0.094)	(0.002)	(0.002)	(0.015)
Interaction term	0.186	0.032	0.008	−0.787	−0.002	0.001	0.151
	(0.084)	(0.006)	(0.007)	(0.300)	(0.003)	(0.001)	(0.026)
Panel C. First child							
RD	−0.815	−0.035	−0.006	−1.303	−0.005	−0.004	−0.242
	(0.082)	(0.006)	(0.002)	(0.086)	(0.002)	(0.002)	(0.050)
Interaction term	−0.077	−0.022	−0.002	−0.318	−0.001	−0.003	−0.022
	(0.010)	(0.005)	(0.003)	(0.098)	(0.002)	(0.002)	(0.008)
Panel D. Single mothers							
RD	−0.839	−0.020	−0.009	−1.802	−0.000	−0.002	−0.148
	(0.262)	(0.005)	(0.002)	(0.069)	(0.004)	(0.002)	(0.039)
Interaction term	−0.095	−0.021	−0.001	−0.056	−0.004	−0.003	−0.132
	(0.014)	(0.006)	(0.002)	(0.042)	(0.002)	(0.002)	(0.034)
Panel E. Below median household earnings in 1975							
RD	−0.391	−0.035	−0.007	−1.199	−0.001	0.002	−0.180
	(0.056)	(0.004)	(0.001)	(0.088)	(0.001)	(0.001)	(0.014)
Interaction term	−0.374	−0.014	−0.005	−0.695	−0.000	−0.005	−0.128
	(0.051)	(0.006)	(0.002)	(0.071)	(0.003)	(0.002)	(0.018)
Panel F. $\geq$15 years between birth and survey							
RD	−0.798	−0.041	−0.005	−1.308	−0.002	−0.005	−0.286
	(0.114)	(0.009)	(0.000)	(0.132)	(0.002)	(0.001)	(0.045)
Interaction term	−0.016	−0.011	−0.004	−0.284	−0.001	−0.001	0.066
	(0.044)	(0.010)	(0.000)	(0.299)	(0.002)	(0.001)	(0.042)
Panel G. $\leq$3 months unpaid leave							
RD	−0.848	−0.034	−0.005	−1.034	−0.002	−0.005	−0.297
	(0.094)	(0.006)	(0.002)	(0.057)	(0.001)	(0.001)	(0.015)
Interaction term	−0.327	−0.020	−0.004	−0.324	−0.003	0.001	−0.042
	(0.055)	(0.002)	(0.001)	(0.056)	(0.002)	(0.002)	(0.007)
Observations	7,150	7,160	7,154	7,147	7,160	7,160	7,138

Notes: 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 90 days, 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.

BMI, obesity, blood pressure, and the metabolic syndrome 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

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

	Mental health index (1)	General health index (2)
Panel A. Complications at birth		
RD	−0.064	−0.064
	(0.011)	(0.009)
Interaction term	−0.087	0.001
	(0.008)	(0.009)
Panel B. C-section		
RD	−0.107	−0.064
	(0.012)	(0.012)
Interaction term	−0.054	−0.014
	(0.061)	(0.012)
Panel C. First child		
RD	−0.111	−0.072
	(0.012)	(0.010)
Interaction term	0.004	0.045
	(0.008)	(0.019)
Panel D. Single mothers		
RD	−0.163	−0.066
	(0.022)	(0.010)
Interaction term	0.040	−0.025
	(0.033)	(0.007)
Panel E. Below median household earnings in 1975		
RD	−0.093	−0.041
	(0.015)	(0.005)
Interaction term	−0.060	−0.038
	(0.011)	(0.004)
Panel F. $\geq$15 years between birth and survey		
RD	−0.118	−0.046
	(0.004)	(0.003)
Interaction term	−0.014	−0.025
	(0.004)	(0.010)
Panel G. $\leq$3 months unpaid leave		
RD	−0.164	−0.060
	(0.010)	(0.011)
Interaction term	−0.069	−0.078
	(0.010)	(0.029)
Observations	7,160	7,160

Notes: 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 90 days, 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.

paid leave scheme. If the effect of exposure to paid maternity leave accumulates, that may also explain the heterogeneous results by birth parity.

Single mothers experienced larger improvements in some metabolic health measures, the general health index, pain (overall), and exercise compared to women

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

	Any (1)	Neck/shoulder (2)	Arm (3)	Back (4)	Chest (5)	Leg/hip (6)
Panel A. Complications at birth						
RD	−0.024	−0.032	−0.035	−0.030	−0.001	−0.015
	(0.012)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Interaction term	−0.020	−0.011	−0.011	−0.006	−0.001	−0.009
	(0.004)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)
Panel B. C-section						
RD	−0.040	−0.032	−0.020	−0.039	0.004	−0.013
	(0.007)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Interaction term	0.003	0.002	−0.034	−0.011	−0.003	0.004
	(0.006)	(0.015)	(0.016)	(0.011)	(0.004)	(0.015)
Panel C. First child						
RD	−0.065	−0.042	−0.019	−0.042	0.003	−0.010
	(0.008)	(0.004)	(0.003)	(0.002)	(0.003)	(0.002)
Interaction term	0.006	0.005	−0.006	0.003	−0.003	−0.005
	(0.007)	(0.004)	(0.005)	(0.003)	(0.002)	(0.003)
Panel D. Single mothers						
RD	0.003	−0.015	−0.021	−0.070	0.030	−0.020
	(0.007)	(0.009)	(0.010)	(0.005)	(0.011)	(0.009)
Interaction term	−0.031	−0.011	−0.016	−0.011	0.012	0.005
	(0.002)	(0.006)	(0.006)	(0.006)	(0.010)	(0.006)
Panel E. Below median household earnings in 1975						
RD	−0.010	−0.001	−0.011	−0.028	0.002	−0.010
	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Interaction term	−0.027	−0.031	−0.008	−0.016	−0.004	−0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)
Panel F. ≥15 years between birth and survey						
RD	−0.044	−0.022	−0.019	−0.013	−0.002	−0.015
	(0.002)	(0.005)	(0.004)	(0.006)	(0.001)	(0.003)
Interaction term	−0.005	−0.014	−0.008	−0.011	−0.005	−0.003
	(0.008)	(0.010)	(0.010)	(0.010)	(0.004)	(0.006)
Panel G. ≤3 months unpaid leave						
RD	−0.042	−0.022	−0.009	−0.046	0.001	−0.015
	(0.009)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)
Interaction term	−0.019	−0.013	−0.017	−0.006	−0.004	−0.003
	(0.009)	(0.003)	(0.003)	(0.008)	(0.003)	(0.003)
Observations	7,752	7,752	7,160	7,752	7,160	7,160

Notes: 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 90 days, 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.

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.[36]

[36] 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.

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

	Smoking (dummy) (1)	Exercise score (2)	Any active exercise (3)
Panel A. Complications at birth			
RD	−0.031	0.173	0.059
	(0.004)	(0.054)	(0.028)
Interaction term	−0.020	0.080	0.020
	(0.003)	(0.057)	(0.019)
Panel B. C-section			
RD	−0.030	0.202	0.079
	(0.005)	(0.021)	(0.021)
Interaction term	0.019	0.052	0.004
	(0.013)	(0.068)	(0.022)
Panel C. First child			
RD	−0.061	0.169	0.061
	(0.002)	(0.018)	(0.018)
Interaction term	−0.006	0.010	0.007
	(0.008)	(0.009)	(0.019)
Panel D. Single mothers			
RD	−0.047	0.138	0.058
	(0.013)	(0.013)	(0.019)
Interaction term	−0.013	0.111	0.022
	(0.015)	(0.018)	(0.010)
Panel E. Below median household earnings in 1975			
RD	−0.017	0.120	0.044
	(0.002)	(0.030)	(0.020)
Interaction term	−0.035	0.080	0.031
	(0.009)	(0.038)	(0.018)
Panel F. $\geq$15 years between birth and survey			
RD	−0.058	0.272	0.077
	(0.003)	(0.028)	(0.020)
Interaction term	−0.015	−0.015	0.003
	(0.010)	(0.013)	(0.019)
Panel G. $\leq$3 months unpaid leave			
RD	−0.036	0.122	0.066
	(0.004)	(0.025)	(0.024)
Interaction term	−0.038	0.055	0.012
	(0.010)	(0.098)	(0.021)
Observations	7,506	7,506	7,506

Notes: 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 90 days, 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.

Thus, the heterogeneity analyses suggest the reform had stronger effects on low-resource mothers.

The reform had a relatively larger impact on diabetes and the general health index for women with more than 15 years between giving birth and taking the health survey.[37] These results are consistent with some health improvements being 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 taken. In principle, unpaid leave could be affected by the reform and we should not condition on it. However, 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 three months or less of unpaid leave versus more than three months. Results are shown in panel G in Table 6 to Table 9. Across most of the outcomes, the reform had larger effects on women who took less unpaid leave. Earlier, we established that the reform led to more time at home. This additional time appears to have been especially valuable for women who in the absence of the reform would have taken little (unpaid) leave.[38]

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 were about 5 percentage points less likely to be married at the time of birth and their incomes were about 6,000 NOK lower on average. Thus, the 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 consistent with Carneiro, Løken, and Salvanes (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.[39]

E. *Subsequent Reforms*

A series of expansions in parental paid leave occurred in Norway between 1987 and 1992. Like the 1977 reform, they provided 100 percent wage replacement, and to be eligible, women had to work 6 of the 10 months immediately preceding

[37] 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.

[38] A concern regarding interpretation of the heterogeneous effects is they may reflect systematic differences in mothers' age at birth. 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 discussed above, suggesting mothers' age does not drive the effect heterogeneity. In addition, the baseline effects are robust to the inclusion of this covariate and, if anything, are more precisely estimated. Results are available upon request.

[39] Although we find no evidence of income or employment effects in the full sample, such effects may exist for subgroups of mothers. In online 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. We sometimes find statistically significant effects on income and employment, but effect sizes are tiny, implying employment changes of less than 1 percentage point and income changes of less than 100 NOK. Income and employment effects are, therefore, unlikely to explain the heterogeneous maternal health improvements.

childbirth and have annual income that exceeded a "substantial gainful activity" threshold. Some of the weeks could be shared among both parents, but very few fathers took any leave (Dahl et al. 2016).[40] Women were still entitled to up to one 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 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 extensions did not crowd out unpaid leave or change family income. We estimate the effects of these expansions on maternal health around age 40 using the health survey data and exploiting the policy cutoff dates.

Some caveats are worth noting. First, the sample is 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 extensions provided fewer 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. Fourth, we only estimate RD models because the expansions occurred in consecutive years, making it difficult to find control years for the RD-DD analysis. Last, we only have month-of-birth data (rather than day-of-birth) for the years covering the extensions; therefore, the running variable has fewer points of support.[41]

The results are presented in online Appendix Tables A5 to A8. We find some significant beneficial effects of the first two expansions (and occasionally the third and fourth) that 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 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 maternal health effects of expansions in maternity leave from already generous levels.[42]

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

[41] We 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.

[42] One possible explanation for the lack of subsequent reform effects is women's baseline health improved over time, leaving less room for improvements. We use information from the birth registry on the small set of health conditions ever experienced by women prior to pregnancy and 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).

VI. Robustness Analyses

We present the results of several robustness checks in the online 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 A9 to A12). 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).[43]

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 A13 to A16).

We perform placebo analyses assuming the reform occurred on July 1 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 A17 to A20). 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 first of a different month (not just July). To allow for a 90-day 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 larger in magnitude (i.e., larger negative or larger positive 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 A21, 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, chest pain, and the exercise score, providing further confidence that the estimated effects are driven by the reform, not month-of-birth variation.

In the RD-DD 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 the results are quantitatively similar to our baseline estimates. Results from this exercise are available upon request.

Last, we show the RD results for different bandwidth choices as suggested in Lee and Lemieux (2010). Figures A7 to A10 in the online Appendix display the estimates of the impact of the reform as well as 95 percent confidence intervals for bandwidths ranging from 30 to 150 days. Generally, the point estimates are not very sensitive to different bandwidth values, but they are less precise when smaller bandwidths are chosen.

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

VII. 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 four 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 United States. 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 improvements were driven by more time at home after childbirth, not changes in income. The additional time at home was especially valuable for disadvantaged mothers, including single and low-income mothers and women who would have taken little unpaid leave in the absence of the reform.

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 policymakers 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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