Get the Wombs to Work: The Missing Impact of Maternal Employment on Newborn Health

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Abstract

Parental leave policies across the globe become much more generous than they used to be. This is also true for prenatal maternal leave. While this may be costly in the short run, little is known about the effects of maternal labor market participation during pregnancy on newborn health. In this paper I exploit three policy changes on the duration of paid parental leave in Austria that affect the share of mothers who work during pregnancy up to the 32nd pregnancy week. Specifically, an increase in the duration of parental leave for the first child, discontinuously decreases the share of mothers who work during pregnancy with their second child. Using administrative data from Austria on the working history of women, that can be linked to the full Austrian birth register in a regression discontinuity framework, allows to identify the effects of prenatal employment on the exposed offspring. Maternal employment during pregnancy reacts strongly to the policy changes. The share of employed mothers increases in 1990 by 27%, declines in 1996 by 11% and increases again by 12% in 2000. None of these changes in prenatal employment translate into effects on newborn health measured via birth weight and gestational length. This effect pattern suggests that prenatal employment does not causally affect the fetus for measures visible at birth.

Keywords: newborn health, maternal labor market status, pregnancy conditions, parental leave;
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1 Introduction

Most high-income countries have seen a significant and steady increase in female labor force participation over the last decades. Therefore, women today are much more likely to work while pregnant. At the same time, family policies have become much more generous over the past century with multifold goals such as gender equity, higher fertility, and child development (Olivetti and Petrongolo, 2017). One of the main instruments of family policy is prenatal maternity leave, showing a substantial variation in duration across Europe from 0 to 11 weeks (Jurviste et al., 2016). This variation across countries mirrors the discrepancy among policy makers on how to optimally design such programs. The understanding of the effects of maternal employment during pregnancy on newborn health is crucial. In the fetal origins hypothesis literature several pregnancy conditions have already been identified as key influencing factors on a variety of long-term outcomes of children. If prenatal maternal employment is among them, long-term benefits for the children may offset the costs of prenatal maternity leave. Therefore, understanding the effects of maternal employment during pregnancy on newborn health is key for policy makers designing policies such as prenatal maternity leave.

There is a large literature estimating the effects of pregnancy conditions on newborn health and long-term outcomes of children (Almond and Currie, 2011; Almond et al., 2017). However, there is surprisingly little evidence on the effect of prenatal maternal employment on newborn health. Even though the prenatal maternal employment status combines several major aspects of pregnancy conditions such as stress, physical activity, disease environment, income and others. Thus in theory the impact of prenatal employment is ambiguous. On the one hand, maternal employment during pregnancy may be stressful for the mother or may correlate with exposure to pollutants and diseases. These influences have been shown to be detrimental to the unborn baby (Aizer et al., 2015; Currie and Schwandt, 2013, 2016; Schwandt, 2017). On the other hand, employment may also increase a mother's income or may be a joyful activity itself, which therefore could improve newborn health.

In this paper, I analyze the effect of maternal labor force participation during pregnancy on newborn health. I exploit three reforms in Austria which affect mothers' likelihood to work during pregnancy with their second child. These three reforms on the duration of parental leave allow to employ a regression discontinuity setting. In order to empirically analyze the impact of prenatal maternal employment I use administrative data from the Austrian Social Security Database (ASSD), containing the full working history for private sector employees. I link the ASSD to the Austrian Birth Register (ABR), covering all births with several indicators on newborn health and characteristics about mothers.

Parental leave policies in Austria consist of both a flat benefit and job protection. Beginning in 1990 there have been several reforms affecting the duration of parental leave. In 1990 parental leave got extended by 1 year, from 12 to 24 months. In 1996 this was partially reversed to 18 months, and increased again to 30 months in 2000. These changes lead to an easier access to an automatic extension for another parental leave spell when giving birth to an additional child, the so called *grace period* rule. This rule exempts mothers from a work requirement and therefore reduces a mother's probability to work during pregnancy with the second child. The three reforms made access to another parental leave spell without working requirements discontinuously easier at the cutoff dates, which I exploit in the following.

I find strong evidence across all policy reforms, that the duration of parental leave significantly affects the mother's employment status during pregnancy with her second child. The effect for the largest reform in July 1990, increasing parental leave by 12 months to 24 months, corresponds to a 27% decline in the share of mothers working during pregnancy at the mean. This effect is equally spread over the entire first seven months of a pregnancy and varies by characteristics of the mother, such as her marital status and whether she is a blue- or a white-collar worker.

The reform-induced changes in employment during pregnancy do not translate into significant effects on newborn health. More specifically, the discontinuous jump in maternal employment during pregnancy does not lead to a significant discontinuous jump in the birth measures. Furthermore, all covariates vary smoothly around the cutoffs of the policy reforms and there are no other detectable changes in the composition of mothers giving birth. Therefore, the evidence suggests that prenatal employment up to the seventh month of a pregnancy does not affect newborn health, measured via birth weight and gestational length. These results are robust to the inclusion of covariates and several different specifications.

I contribute to the literature that estimates the relationship between maternal employment during pregnancy and newborn health in various ways. My paper is the first to provide clear evidence of the effect of maternal employment up to and including the seventh month of a pregnancy on the health of the newborn. This setting combined with previous literature helps in understanding when in pregnancy time off might be most beneficial. Using the described reforms for exogenous variation in prenatal maternal employment up to the 32nd pregnancy week generates a large and representative sample of compliers. This complements the existing literature on maternal employment during pregnancy on newborn health. Rossin (2011) evaluates the introduction of a total of 12 weeks of unpaid maternity leave (for pre- and post-birth combined) in the US and exploits county-level variation in eligibility. Her results therefore point to the effect at the very end of a pregnancy for high-wage mothers, while I can analyze the first 32nd weeks of a pregnancy. Wüst (2015) employs survey data and estimates her effect based on the specific sample of mothers who change their employment status from one pregnancy to another based on educational choices or mothers who choose to have closely spaced siblings.

Second, compared to the previous literature I am able to use a large administrative dataset on an individual level basis, that allows me to exactly identify the exposed mothers and their offspring. Based on this dataset I can calculate the exact number of days a mother is working during pregnancy. Therefore, I can analyze the impact of prenatal employment on two margins. The extensive margin of mother's who choose to work or not during pregnancy, and the intensive margin of working mothers, as I can factor in unusually detailed information on maternal employment histories. The richness of the data, also makes it possible to analyze varying effects by different types of mothers. This allows to say something about heterogeneous effects by work environment.

Third, I can combine the rich data with a diverse policy setting. I explore the impact of prenatal employment in three different point in times, that allow me to both look at the effects of increases as well as decreases in employment during pregnancy. This variation in both directions allows to study asymmetries in an already generous leave setting with strong changes in parental leave duration across the studied time period.

Finally, this study also relates to the literature studying the effects of parental leave on other types of outcomes such as maternal labor market outcomes, fertility, child and maternal health, and cognitive development of children. In the Austrian context, Lalive and Zweimüller (2009) and Lalive et al. (2013), for example, study the effect of the same reforms on maternal labor market outcomes and fertility, while Danzer and Lavy (2018) and Danzer et al. (2017) focus in their studies on cognitive outcomes of the affected children. Studying the effect of prenatal employment therefore adds to a complete picture of analyzing the impact of parental leave policies and thus helps to guide policy makers.

Overall, my results show that large changes in prenatal employment do not imply significant changes of newborn health. This suggests, that parental leave policies should rather focus on the time after birth, where time spent with parents especially in the very early childhood has been shown to be beneficial (Carneiro et al., 2015; Rossin-Slater, 2017; Heckman, 2007).

The paper is organized as follows: Section 2 describes the Austrian parental leave system and its reforms, it also develops testable hypotheses. Section 3 discusses the data. Section 4 presents the empirical framework of the study. Section 5 provides an overall assessment of the results and sensitivity analyses, and a conclusion follows in Section 6.

2 Background

This section provides the institutional background of the Austrian parental leave system and discusses how the reforms may have affected newborn health via the various channels.

2.1 The Austrian parental leave system

The Austrian family policy rules consist of two types of policies that cover the period around birth. The first policy, mandatory maternity leave (ML), prohibits work 8 weeks pre- and post-birth, and pays the average wage a mother earned during the last quarter prior to giving birth. As such, ML promises a generous environment in order to protect both the mother's and the baby's health. After the expiration of the mandatory maternity leave, eligible mothers can choose to take parental leave (PL). This second policy consists of two pillars: a flat benefit and job protection. The policy changes that I will use in this paper affect parental leave, which will be explained in more detail in what follows.

First-time mothers above the age of 25 are eligible for parental leave if they have worked for at least 52 weeks within the 2 years prior to giving birth.[1] This requirement is reduced to 20 weeks[2] within the year prior to giving birth for higher-order births. Furthermore, there is a so called *grace period* rule, that allows mothers with a relatively short spacing of births an automatic extension for the next birth. More specifically, the grace period exempts mothers from a work requirement if they give birth to an additional child no later than 3.5 months after the expiration of the parental leave of the previous child.

Since 1990, there have been several reforms to the parental leave system, which are illustrated in Figure (1). Political debate about the introduction of paternal leave at the costs of maternal leave, led to the compromise of extending the parental leave duration. Furthermore, in the early 90s formal child care institutions were scarce in Austria. This made maternal employment in the early childhood of her descendant difficult and could possibly even deter mothers from the labor market in the long run. An extension in parental leave duration was expected to improve this situation for mothers.

The reforms were structured as follows. While parental leave lasted up to the first birthday of the child until June 1990, this was extended to 24 months after birth in July 1990. The reform in July 1996, targeting the cash benefits leaving job protection unchanged, reserved 6 of these added months to fathers. This actually reduced parental leave duration to 18 months after birth as almost no fathers were taking it up by that time. The July 2000 reform, again only targeting cash transfers, increased parental leave to 36 months after birth, reserving 6 months for fathers. Therefore, it essentially increased parental leave duration from 18 to 30 months after birth.[3]

The changes in parental leave duration had strong implications for the likelihood of mothers to give birth to another child within the grace period. Prior to June 1990, a mother had to conceive the next child no later than 5.5 months after giving birth to her first child

[1] Work requirements are shorter for younger mothers.

[2] 26 weeks after July 1996

[3] For further and more detailed information on the amount of cash benefits, eligibility criteria, announcement of the policy reforms, and the reforms itself see Lalive and Zweimüller (2009) and Lalive et al. (2013).

in order to meet the requirements for automatic extension, which is biologically difficult.[4] The 1990 reform extended this to 17.5 months, the July 1996 reform partially reversed it to 11.5 months, while the July 2000 reform extended it again to 23.5 months. All of these time windows are biologically feasible and desirable.[5]

Figure (A1) shows how the percentage of children born within parental leave and the grace period evolves over time. The discontinuities depicted in Figure (A1) are exploited in this paper. While prior to July 1990 only 10% of all second born get born within the period of parental leave extended by the grace period, this increases to around 40% for the period from July 1990 to June 1996. In July 1996 this fraction drops again to 0.2 with the shorter parental leave duration for these cohorts and increases again with the very generous parental leave system in July 2000. Furthermore it can be seen, that it is especially the changes in the duration of the parental leave that drive these changes over time. The fraction of births occurring during the grace period stays rather constant and explains only most of the births in the least generous system before July 1990.

Figure (2) looks at pregnancies with second children for mothers that gave birth to their first child each 2 years pre- and post-policy reform. It shows that this simplification in meeting the requirements for the grace period in July 1990 goes hand in hand with fewer women working during pregnancy with their second child. These effects are equally spread over the entire first 32 weeks of pregnancy. Thereafter, mothers go on mandatory maternity leave and do not have to work under either regime. The decline in duration in July 1996 leads to more women working during the entire pregnancy duration, while the increase in July 2000 again translates into fewer women working during pregnancy with their second child.

[4]Mothers who exclusively breastfeed have a 98% protection from pregnancy in the first six months (Kennedy et al., 1989).

[5]Short (often defined as less than 18 months) and very long (more than 59 months) interpregnancy intervals are associated with adverse perinatal outcomes (see Conde-Agudelo et al. (2006) for a meta-analysis on birth spacing).

2.2 Hypotheses and related literature

Health at birth is an important predictive factor for individual long-term outcomes, such as educational attainment, adult labor market outcomes, and health status (Almond, 2006; Black et al., 2014; Schwandt, 2017). Economists show increasing interest in understanding the pathways from pregnancy conditions to early childhood health. Many such influencing conditions have already been identified, such as nutritional shocks (Almond et al., 2015; Scholte et al., 2015), diseases (Almond, 2006; Currie and Schwandt, 2013; Schwandt, 2017), tobacco and alcohol consumption (Aizer and Stroud, 2010; Nilsson, 2017), pollution exposure (Currie et al., 2009; Currie and Schwandt, 2016), maternal stress (Black et al., 2016; Aizer et al., 2015), etc. However, the general effects of working during pregnancy have, to my knowledge, not been extensively investigated so far. There are few related studies showing contrasting evidence on the effects of prenatal labor market participation on newborn health.

Wüst (2015) looks at Danish survey data focusing on mothers who change their employment status for consecutive children due to reasons unrelated to health. The effect is therefore identified for mothers who switch between employment and education or those who have closely spaced consecutive births. She documents positive effects of working during pregnancy in reducing the probability of a preterm birth. Wüst (2015) explains her findings by the special setting in Denmark with high female employment rates, where it might seem to be stressful to be excluded from employment.

Rossin (2011) analyzes the impact of unpaid maternity leave provisions in the US on infant health outcomes and finds positive effects. She documents small increases in birth weight and a reduction in premature birth and infant mortality. While the leave is unpaid and lasts in total (both pre- and post-birth) only for 12 weeks, her focus is on the very last weeks of a pregnancy and mostly on early infancy. Furthermore, the fact that the leave is unpaid leads to a specific sample of compliers, namely those who can afford to take unpaid leave.

Similarly to Rossin (2011), Stearns (2015) analyzes time off from work during late pregnancy in the US. In her study leave is, however, paid due to a temporary disability insurance program and therefore especially beneficial for unmarried and black mothers for which the share of low birth weight births reduces significantly.

Ginja et al. (2017) analyze the effect of a speed premium in Sweden, which is analogous to the grace period in Austria. In their study the effect of maternal employment on newborn health is not directly addressed. However, they also document as a result of the speed premium a slight decline in maternal employment during pregnancy with the second child and no effects on outcomes measured at birth.[6]

Finally, a study by Ahammer et al. (2018) adds to this literature by analyzing a reform that extended prenatal maternity leave from six to eight weeks in Austria in 1974 . They find no evidence for significant effects on newborn health, subsequent maternal health, fertility, and long-term human capital outcomes of children. Combining the reform analyzed by Ahammer et al. (2018), which affects the weeks 33 to 34 of pregnancy and the one analyzed in this paper, which affects the weeks 0-32 of pregnancy, allows to say something more general about the mother's labor market status during pregnancy. Therefore, the two papers complement each other in order to give a broader picture on prenatal employment.

Not only does the previous literature on the impact of maternal employment on newborn health leads to inconclusive evidence but the impact is also theoretically ambiguous. There are several opposing factors that could affect the newborn health via a mother's labor market status during pregnancy. The effect of employment during pregnancy on newborn health is therefore a priori not clear.

First, working itself could be stressful for the mother. Existing empirical literature on the effects of maternal stress on infant health is generally limited to studying the effects of very severe but often unique stress factors (like domestic violence (Aizer, 2011), hurricanes (Currie and Rossin-Slater, 2013), political uprising (Lee, 2014), death of a relative (Black et al., 2016; Persson and Rossin-Slater, 2018), and terrorism (Quintana-Domeque and Ródenas-Serrano, 2017; Camacho, 2008)). While all these studies show a negative impact on newborn health, it is less clear what the effects of a more mild version of stress are, as, for example, induced by a factor like work. As recently summarized by Almond et al. (2017), relatively mild shocks in

[6] The effect size of the analyzed policy reform on maternal labor market participation is with -0.013 substantially smaller than in my study with 0.191 (Ginja et al., 2017).

early or prenatal life can, however, have substantial negative impact on child development. Additionally, it is unclear whether mental and physical stress affect the fetus similarly and whether there might be a difference of these two forces early and late in pregnancy. Aizer et al.'s (2015) research, for example, suggests that cortisol measured in the third trimester matters for newborn health measures, while Camacho (2008) finds that psychological stress is more important in the first trimester of a pregnancy.

Second, working during pregnancy might expose the mother to pollutants and diseases at work or while commuting. These influences have been shown to be negative for the fetus (Almond, 2006; Schwandt, 2017; Currie et al., 2009; Currie and Schwandt, 2013, 2016). Both Almond (2006) and Schwandt (2017) even show that flu exposure during pregnancy has long-term effects on children's educational attainment and wages.

Third, being on PL and therefore not working during pregnancy is also related to changes in income. The intensity of this effect depends on a mother's location in the income distribution. In fact, PL benefits are flat over the entire PL duration and amounted to roughly 340 euros per month. This, therefore, describes an income reduction for high income mothers only. However, the income effect on newborn health is almost exclusively identified for low income mothers in the literature. Hoynes et al. (2015), for example, show that the Earned Income Tax Credit (EITC) in the US reduces the incidence of low birth weight and increases mean birth weight. Almond et al. (2011) exploit monthly variation in the introduction of the Food Stamp Program (FSP) in the US and find positive effects for birth outcomes especially at the lower tail of the birth weight distribution. Both papers focus on programs designed for low income people. Furthermore, evidence from conditional cash transfers in developing countries also show a positive impact on several birth outcomes (Barber and Gertler, 2008; Amarante et al., 2016). However, as PL benefits are flat and therefore only negatively impact high wage mothers, the effect of income in this setup is less clear. I will investigate this issue in a heterogeneity analysis, where I assume blue-collar workers to not be affected by the income channel.

Fourth, a side effect of the extension of the grace period could also be shorter birth spacing between siblings. The medical literature argues that short spacing (most often defined as less than 18 months in age difference) leads to adverse infant health outcomes (see

Conde-Agudelo et al. (2006) for a meta-analysis).

Finally this paper also relates to a large body of literature that studies the effect of post-birth parental leave reforms on child development (Baker and Milligan, 2008; Beuchert et al., 2016; Carneiro et al., 2015; Dahl et al., 2016; Danzer and Lavy, 2018; Dustmann and Schönberg, 2012; Ruhm, 2000; Tanaka, 2005; Rasmussen, 2010). The general take away from this literature is that an introduction of parental leave significantly improves child development in both the short and long run while extensions in the duration of parental leave often do not lead to significant changes in child development. However, the latter might be a result of an average treatment effect canceling each other out for different heterogeneous groups. Danzer and Lavy (2018) show, for example, that a 1 year extension in parental leave duration to 2 years is beneficial for boys of highly educated mothers only, while it actually harms boys of low educated mothers.

3 Data

This project is based on two administrative datasets. The primary source of data for the determination of a mother's work status during pregnancy is the Austrian Social Security Database (ASSD). For the analysis of newborn health outcomes, the ASSD is linked to the Austrian Birth Register (ABR).

The Austrian Social Security Database. The ASSD stores the full working history of private sector employees and is used to verify pension claims. The ASSD also records the date of all live births after entering the labor market and maternity and parental leave spells if taken. As a result, I observe detailed information on a daily basis for each woman after first entry into the labor market. Detailed labor market information consists of the employer, along with information on occupation, experience, and tenure. Information on earnings is provided per year and per employer.

The Austrian Birth Register. Information about newborn health measures is coming from the ABR, that includes the universe of live births in Austria. Each birth entry consists of individual-level information on birth characteristics such as date, place, gender, plurality, gestational length, birth weight, and length. Furthermore, for every birth maternal socio-economic characteristics such as age, educational attainment, marital status, and origin complement the individual-level information.

The two sources of data are matched based on characteristics I observe in both datasets, such as the date of birth, the date of precedent birth for higher-order births, location of birth and age of the mother.[7] For my main analysis I restrict my sample to private sector dependent employees who are PL eligible[8], aged 15-45 in line with demographic research, and who are giving birth to a single child. Furthermore, I restrict my main sample to the period of 1984 to 2007, as gestational length, one of my key variables, is only recorded after 1984. Altogether, this results in approximately 60,000 observed births per year of a total of around 85,000 births in Austria.

Finally, I construct several variables that are key for my analysis. From all birth entries per mother I calculate the parity for every birth. Furthermore, I calculate the days a mother has been working during pregnancy and based on this I create a dummy for the work status, defined as 1 if she worked a positive number of days during pregnancy.[9]

In addition to days worked, days sick during pregnancy can be calculated in the same manner. From this, I create a sickness dummy equaling 1 if a positive amount of days sick is reported. This sickness dummy will be used as an additional explanatory variable in the OLS regression and allows to compare my results to the study by Wüst (2015).

[7] This results in 61% of matches of all births, corresponding to roughly 80% of all births observed in the ASSD. Once a mother leaves the labor market and gives birth to additional children, these birth dates will not be recorded in the ASSD any longer. However, based on information about the characteristics of the mother herself and the date of birth of the precedent child, I am able to recover some births that are only observed in the Austrian Birth Register. The mother's unique social security number can be added to these recovered births. This method allows to add 87,362 births to the linked dataset.

[8] For PL eligibility I follow the definition of Lalive and Zweimüller (2009). In order to construct the eligible sample, the work history 2 years prior to giving birth to the first child is considered. Women who show any form of employment or were eligible to collect unemployment benefits are considered eligible. This very generous form of eligibility results in 95% of women being PL eligible.

[9] I count both normal as well as marginal employment as working days and use the terms worked and employed interchangeably. For the construction of the number of days a mother worked, gestational length in weeks (multiplied by 7 days a week) is subtracted from the exact date of birth and working days (MO-FR) are being calculated from the day of conception to the last day of pregnancy.

For the analysis of the impact on newborn health, I use two measures of birth outcomes since these have already been shown to be linked to later-life outcomes (Almond and Currie, 2011). First, I include a dummy for low birth weight (below 2500 grams) as a general measure. Second, I construct a dummy for prematurity equaling 1 if gestational length is below 37 weeks.[10] Additionally, I will show results in the appendix for birth weight and gestational length.

Lastly, I control for the following maternal characteristics: a dummy for marital status, an indicator of foreign origin, 5-year age brackets, and an indicator combining wage and educational data.[11] For the heterogeneity analysis I further add a dummy for having worked in a blue-collar job during the first pregnancy.

Summary Statistics. Table (1) presents the descriptive statistics for the full matched and eligible sample. While Column (1) describes the full sample, Columns (2)-(4) restrict the sample to second born children with older siblings born in the vicinity of the policy reform thresholds.[12] When compared to the full sample, second born with older siblings born around a policy reform are less likely to be born preterm and with low birth weight. Their mothers are more likely to be married and older, as expected. Therefore, as I will focus on higher-order born children, potentially being affected by their mother's longer (or shorter) parental leave stay with the first born child, the sample will be selected positively with respect to newborn health. However, as 78% of Austrian families are having more than 1 child in my sample, this implies that my analysis is based on a non-negligible fraction of births.

In the restricted samples (Columns (2)-(4)), the average birth weight ranges from 3,381-3,419 grams. The average occurrence of a preterm birth is between 3.2-3.6% and the one of

[10]Both thresholds of weight (low birth weight) and gestational age (preterm) are defined by the World Health Organization.

[11] Following Danzer et al. (2017) I construct this indicator variable by defining all mothers as low SES who completed compulsory schooling or who completed apprenticeship training or intermediate vocational school and additionally have low wage. For the assignment to high and low wage, I use average wage data in the two years prior to a mother's first birth. I classify a women as low wage if her wage is below or equal the median wage in that specific birth year for all women in my sample. Mothers who have completed at least higher school or who finished apprenticeship training or intermediate vocational school and earn a high wage are defined as high SES mothers.

[12]The chosen bandwidth in my baseline analysis is 24 months pre- and post policy reform.

a low birth weight 3.2-3.3%. The largest fraction of mothers is married (0.72-0.80) and gives birth to their second child at an age of 25-29 years (0.38-0.45). On average about 60% of the mothers work during pregnancy with their second child and spend 63-69 days at work.

4 Empirical design

This paper focuses on identifying the causal effect of work during pregnancy on newborn health. Specifically, consider the following baseline model:

$$Y_i = \beta_0 + \beta_1 work_i^m + \beta_2 X_i^c + \beta_3 X_i^m + \tau_t + \epsilon_i, \tag{1}$$

where for each individual i, Y_i is the outcome of interest, being born preterm or with low birth weight. $work_i^m$ represents a dummy for the work status of the mother during pregnancy or the days worked. X_i^c are child-level characteristics such as gender. The vector X_i^m of maternal characteristics includes controls for 5-year age brackets, marital status, origin and low SES. τ_t is a vector controlling for year of birth and month of birth fixed effects and ϵ_i is a random vector of unobservable individual characteristics. β_1 is the coefficient of interest. The coefficient β_1 describes the effect of prenatal employment on newborn health. However, one issue with estimating Equation (1) is omitted variable bias.

Omitted variable bias occurs if there are unobserved variables that are both correlated with newborn health and a mother's labor market participation during pregnancy. One such example is the mother's own health status. A mother might choose not to work because she is in bad health, which directly influences also the health of her own child. Leaving out the mother's own health status in the model, will then lead to overestimating the effect of work status. Other unobserved variables that could threaten the validity of a standard OLS approach, are, for example, income and stress.

Regression Discontinuity Design. To overcome the issue of omitted variable bias, I base my analysis on the described reforms of the parental leave system that generate quasi-experimental variation in the likelihood of mothers to work during pregnancy. More specifically, I use three separate regression discontinuities for the three reforms in July 1990, July 1996, and July 2000. The RD method and its use in economics is extensively summarized by Imbens and Lemieux (2008); and Lee and Lemieux (2010). The intuition behind it, is that all mothers giving birth to their first child just before or just after a policy change do not differ discontinuously in their characteristics but face two different policies.

Imagine two mothers, mother A and B. Mother A gives birth to her first child in June 1990 and can stay on parental leave up to the first birthday of her child. She has to conceive her second baby latest by December 1990, so that she does not have to go back to work during pregnancy with her second child. Mother B, instead, gives birth to her first child in July 1990, only one month later than mother A. She can stay on parental leave for two years and must conceive latest by January 1992 in order to be exempt from the work requirement. Mother A and B are unlikely to differ in characteristics beforehand. However, mother B is much more likely to give birth to another child within the grace period. This discontinuity in the likelihood of working during pregnancy with the consecutive child stemming from an exogenous policy reform is what I will exploit in the following.

Estimation. Following Jacob et al. (2012) and Lee and Lemieux (2010), I estimate local linear regressions in samples around the cutoffs. I estimate separate OLS regressions with a rectangular kernel for each cutoff date and choose in my context a meaningful bandwidth of 24 months. Following Lee and Lemieux (2010) the choice of kernel typically has little impact in practice. I also conduct sensitivity analysis with different bandwidths chosen by the method of Imbens and Kalyanaraman (2012) or Calonico et al. (2016).[13] I will report graphical evidence of the robustness to different bandwidths in the Appendix.

I start by documenting the discontinuity of prolonged parental leave on prenatal employment for mothers having a second child. I estimate the following first stage regression:

$$work_i^m = \gamma_0 + \gamma_1 T_i + \gamma_2 R_i + \gamma_3 T_i * R_i + \gamma_4 X_i^m + \upsilon_i. \tag{2}$$

[13]The bandwidth selection according to Imbens and Kalyanaraman (2012) is implemented in Stata by the command rdcv choosing ik as method. The bandwidth selection according to Calonico et al. (2016) is implemented in Stata by the command rdbwselect choosing the default method mserd.

The variable T_i describes an indicator variable for having an older sibling being born in the post policy reform period. R_i is the so called rating variable, indicating the number of months from the older sibling's birth date to the date of policy change, and υ_i is an idiosyncratic error term. $work_i^m$ and X_i^m are specified as above. The parameter of interest in Equation (2) is γ_1. γ_1 describes the size of the change in the outcome $work_i^m$ at the date of the policy reforms and therefore highlights the discontinuity in the share of mother's working during pregnancy with their second child.

In a second step, I examine the effect of the policy reform on newborn health using the following reduced form equation:

$$Y_i = \delta_0 + \delta_1 T_i + \delta_2 R_i + \delta_3 T_i * R_i + \delta_4 X_i^m + \delta_5 X_i^c + \tau_t + \nu_i, \tag{3}$$

where Y_i describes the various newborn health outcomes, such as the dummies for being born with low birth weight or preterm. The remainder of the variables are as defined before. This reduced form equation provides estimates of the net effect of parental leave reforms on baby health.

Finally, I report instrumental variables estimates of the effect of working during pregnancy on newborn health using the discontinuity as the excluded instrument. This allows on the one hand to compare the magnitude of my estimated effects to those already reported in the literature, and on the other hand to see what kind of effect sizes my data provide evidence against. In this final step I estimate the following regression:

$$Y_i = \theta_0 + \theta_1 \widehat{work_i^m} + \theta_2 R_i + \theta_3 T_i * R_i + \theta_4 X_i^m + \theta_5 X_i^c + \tau_t + \eta_i, \tag{4}$$

where I include for the work status of the mother the prediction from Equation (2).

Identification. The identifying assumptions for inference using the RDD are: (1) the probability of being treated must be discontinuous at the cutoff and (2) there should be no discontinuity in potential outcomes at the cutoff (Lee and Lemieux, 2010). The second statement requires that no observable nor unobservable factors exhibit any discontinuities at the cutoffs. This is likely fulfilled if there is no precise manipulation to be on either side of the cutoff.

While assumption (1) holds by construction of the policy reform, there is no definitive test for proofing that assumption (2) holds. However, I can conduct standard tests for asserting the validity of assumption (2). The timing of the policy announcement guarantees that individuals cannot perfectly sort at the policy reform threshold. As described by Lalive and Zweimüller (2009), the policy reform in 1990, for example, was only announced three months before implementation and therefore made a perfect birth planning impossible. However, pregnant mothers could still influence the timing of a birth via a Caesarian section within a short time window, which I will address in the robustness analysis.

Additionally, I can visualize the relationship between the covariates and the rating variable. In the context of possible confounders, these variables should evolve smoothly across the cutoff in order for the RD estimate to generate consistent estimates. Furthermore, I will report a Covariate Balance Test, where I check in regressions for discontinuities in my observable variables. If the smoothness of all observable variables is fulfilled, this is likely the case for the unobservables.

Sample Selection. While the regression discontinuity design circumvents the problem of omitted variables bias described before, it does not solve the problem of sample selection. Sample selection in this context can arise as I only observe newborn health measures for those women who choose to become mothers. If the policy changes in the duration of paid parental leave affect fertility decisions directly, this might influence the sample of mothers for which I observe the newborn's health. Lalive and Zweimüller (2009) and Lalive et al. (2013) show in their analysis direct fertility effects of the policy reform in 1990, but no such effects for the other two reforms in 1996 and 2000.[14] In order to address the issue of sample selection, I follow the approach described by Dong (2017) and Kim (2016) in a robustness analysis.

The approach is based on estimating treatment bounds in the presence of sample se-

[14] I re-estimate the fertility effect for the reform in 1990 with my own sample and find that the policy leads to 2.5 additional children per 100 women within three years. With an estimated effect size of the first stage of 19.1 women per 100 women who additionally don't work after the change in the policy, the effect of the sample selection seems relatively small.

lection, leaving the formal specification unchanged as in Equations (2) and (3). Compared to other approaches that deal with sample selection, the chosen one does not require specifying any selection mechanism. The only additional assumption for identification is (3) monotonicity, which implies that observability of outcomes is only affected in one direction due to treatment assignment. In the described context, this means that all mothers who gave birth to an additional child in the less generous parental leave duration regime would also give birth under the new rules. After the policy reform, additional mothers join the sample who are only induced to give birth to another child under the more generous policy regime. Monotonicity would be violated if there are mothers who would only give birth to an additional child in the less generous pre-reform policy period. Here, the assumption of monotonocity is reasonable as mothers who give birth after the policy reform could still go back to work after 1 year and face the same opportunities as pre-policy reform.

The idea of the treatment bounds is to estimate the share of additional mothers in the after-reform sample (∼5%) and restrict this sample to a very favorable group (excluding the lower 5% in the respective outcome distribution) and a very unfavourable group (excluding the upper 5% in the respective outcome distribution).[15]

5 Results

This section starts by discussing the OLS effects of working during pregnancy on newborn health. I will then show, graphically and empirically, RD estimates of the effects of changing PL duration on maternal employment during pregnancy with her second child and newborn health. Based on this, I present instrumental variables estimates of the effect of working during pregnancy on newborn health. Finally, several sensitivity analyses in order to test the robustness of the results will be discussed.

5.1 Baseline OLS estimates

Table (2) presents baseline OLS results for estimates of Equation (1). All columns are estimated on the pooled sample of the three RD 24 months bandwidth sample with second

[15]Standard erros in this approach are calculated via bootstrapping.

born children. This allows to compare the OLS estimates later on with the pooled IV estimates. Columns (1) and (3) include only child-level characteristics such as a gender dummy, year, and month of birth fixed effects. In Columns (2) and (4), referred to as the full control model, I additionally control for mother-level characteristics: 5-year age dummies,[16] an indicator of foreign origin, a dummy for the marital status, and a dummy for low SES.

Table (2) is split into Panel A, looking at the effect of the work status, and Panel B, looking at the effect of days worked on newborn health. Both a mother's work status and the days worked during pregnancy are positively related with newborn health outcome measures. The full control model suggests, that children of mothers who work during pregnancy are at the mean 6.3% less likely to be born preterm and 7.0% less likely to be of low birth weight, respectively. The effect sizes are even bigger in the full control model for the days worked. A baby of a mother who works 65 days (the average of the three RD samples) versus that of a mother who doesn't work at all, is on average 19.4% less likely to be born preterm and 19.9% less likely to be of low birth weight, everything else equal. Estimates on the two continuous outcome variables of birth weight and gestational length are reported in Table (A1) and show the same direction of correlations.

The sickness dummy is negatively correlated with all newborn health measures, as reported in Panel A and B in Table (2). The full control model shows that being sick leads at the mean to an increase in preterm birth and low birth weight of 91.0-93.4% and 91.1-93.3%, respectively. To give some idea of the magnitude of this effect, I can compare this finding with the one of Wüst (2015). She reports an increase in a preterm birth of roughly 43% at the mean if the mother reports being sick during pregnancy.[17]

Controlling for mother's characteristics only marginally affects the size of the coefficients of interest.[18] However, controlling for a broad set of observables, is not sufficient to completely rule out endogeneity concerns. Therefore, I will exploit the policy reforms in the next

[16] I also test for other specifications of the age variable commonly used in the literature, such as age and age squared. My main coefficients of interest are, however, unaffected.

[17] However, Wüst (2015) estimates this effect on the full sample. If I re-run the OLS regression on my full sample as described in Table (1) Column (1) including all parities and not only second born, I get an increase of 46-52% in a preterm birth if a mother is sick which is comparable in size to the one reported by Wüst (2015).

[18] Table (A2) reports the entire set of coefficients for mother characteristics.

sections in a regression discontinuity set-up to infer the impact of prenatal employment on newborn health.

5.2 First stage estimates

Graphical evidence and estimation results. In this section I test the first stage described in Equation (2). In particular I focus on the extensions of parental leave duration for the first child and the impact on a mother's employment status during pregnancy with her second child. Figure (3) graphically represents the results. Dots refer to monthly averages, to which linearly fitted values and confidence intervals are added. There are significant discontinuities around the cutoff dates. The 1990 reform, for example, leads to an approximate decline of 18.5 percentage points (pp) in the share of mothers employed during pregnancy with the second child giving birth to their first child right after July 1990 compared to mothers that gave birth just before the policy reform. The discontinuities in Panel B and C for the reforms in 1996 and 2000 highlight the same pattern. When parental leave for the first child declines, as in 1996, the share of employed mothers during pregnancy with their second child increases by approximately 7 pp, while it declines by 6 pp with the extension of parental leave duration in the year 2000. The intensive margin of days worked corroborates these patterns, where as an example a 14% drop in 1990 of a total of potentially 160 days worked[19] results in a reduction of 22 working days.

Table (3) presents regression estimates of Equation (2). Each regression is estimated with a chosen bandwidth of 24 months. Columns (1), (3), and (5) show regression results with child-level controls only, while Columns (2), (4), and (6) add mother's characteristics. All estimates corroborate the graphical evidence found in Figure (3) and confirm the statistical significance of the discontinuities in mother's work behavior at the cutoff. In terms of magnitude, the 1990 reform has the strongest impact in the full control model on maternal employment during pregnancy with her second child, decreasing the%age of mothers employed and the days employed by 19.1 pp and 23.0 days, respectively. The corresponding

[19]The 160 days are the product of mothers working 32 weeks during pregnancy times 5 days per week. The 32 weeks are the result of an average pregnancy lasting 40 weeks minus 8 weeks mothers have to spend on maternity leave.

numbers for the 1996 reform are an increase of 7.2 pp and 9.5 days in the employment share and the days worked. The 2000 reform leads to a decline in the%age of mothers working of 6.4 pp and a decline in the days worked of 6.6 days. Altogether, the documented effects are significant, precisely estimated and economically important.

The point estimates of the effect of having an older sibling being born post policy reform on the work status and the days worked does not vary with the inclusion of mother's controls. This robustness to the inclusion of additional controls confirms the validity of the RDD in this setting. Furthermore, the regression results are also robust to other choices of bandwidths (see Figure (A2)) and functional forms (see Table (A3)).

Heterogeneity. Table (4) reports the first stage effect of the 1990 reform for different heterogeneous groups. This helps to understand whether mothers with different background characteristics react differently to the policy change. The heterogeneous groups shown in Table (4) are with respect to the following characteristics: marital status, blue- versus white-collar, origin, and gestational length of the precedent birth.[20]

The regression estimates demonstrate, that married mothers (Panel A) are significantly less likely to be employed during pregnancy with their second child compared to unmarried mothers. Married mothers have an increased propensity to not work during pregnancy with their second child of 22.1 pp, while unmarried mothers are only 7.2 pp more likely to not work during pregnancy. Blue-collar mothers (Panel B) are less likely to react to the policy change compared to white-collar mothers (17.4 pp vs 21.1 pp). I find no difference across mothers with different origin (Panel C), showing that both Austrian and foreign mothers decrease their probability of being employed during pregnancy with their second child after the policy reform by a comparable magnitude. Finally, mothers with a preterm first birth (Panel D) are significantly marginally more likely to be employed during pregnancy with their second child than mothers with no such history (19.4 pp vs 19.5 pp).

[20]Information about mother's characteristics refer to the first pregnancy to mitigate possible endogeneity concerns.

5.3 Reduced form estimates

Graphical evidence and estimation results. This section reports the reduced form estimates of Equation (3). In particular I analyze the impact of the extension of parental leave duration for the first child on several health outcomes for the newborn child, such as being born preterm or with low birth weight.[21] These reduced form estimates can be interpreted as the net effect of the policy reforms on newborn health. As the first stage estimates were generally large and statistically significant, we would expect to see sizeable effects on newborn health if there exists a relationship between the latter and a mother's work status.

Figure (4) shows the effects of the policy changes on newborn health. Dots represent monthly averages and solid lines correspond to values from a linear fit. There is no significant discontinuity visible. This holds true for any of the outcome variables and also for all three policy reforms. Linking these results with the discontinuous jump found for the first stage results of prenatal employment, leads to the conclusion that my estimates are consistent with a null hypothesis of no effect of maternal employment during pregnancy on newborn health.

The graphical results are complemented with regression outputs estimating Equation (3) in Table (5). All columns include a gender dummy, year of birth and month of birth fixed effects. Columns (1), (3), and (5) show regression results with child-level controls only, while Columns (2), (4), and (6) add mother's characteristic controls. All estimates support the graphical evidence found in Figure (4). For all three reforms and all newborn health outcomes I document statistically insignificant and generally small effects. For example in 1990, the estimates allow to rule out an increase of more than 0.6%age points and a decline of more than 0.2%age points in the probability of being born with low birth weight. Furthermore, the point estimates are not affected by the inclusion of mother's characteristic controls. Overall, these results indicate that the net impact of the extension of parental leave duration for the first child and the accompanying significant reduction in prenatal employment during pregnancy with the second child on newborn health is negligible.

[21] Results on birth weight and gestational length are reported in the Appendix; Figure (A3) and Tables (A4)-(A5).

Heterogeneity. Table (6) reports the results for the different heterogeneous groups looked at in Section 5.2, namely marital status, blue- versus white-collar, origin and previous birth outcomes. For all heterogeneous groups reported in Table (6) effects are insignificant for the policy reform in 1990 and do not vary by marital status, blue- versus white-collar, origin, nor gestational length of the precedent child.[22] While, for example, the labor market effects were 3 times as high for married women than for unmarried women, no such differences can be seen for the newborn health measures.

The insignificant effects by a worker's type deserve to be looked at more closely. Both blue- and white-collar mothers show sizeable and significant first stage effects, although blue-collar workers react less strongly in maternal employment to the policy reforms. While blue-collar workers earn significantly less on average, these mothers are much less likely to be affected by a possible negative income channel due to the flat benefit scheme of the parental leave system. Therefore, the effects reported for this type of workers result mostly from the work channel. As however no differences in newborn health are detectable, it is reasonable to assume that employment during pregnancy does not affect child outcomes. Furthermore, the distinction between blue- and white-collar workers is also interesting to understand the effects of physical versus mental stress during the first seven months of a pregnancy. The missing impact of prenatal employment for both groups suggests, that preventive measures for pregnant women in Austria seem to work and that the fetus of none of these women is neither negatively nor positively impacted by the mother's work status. Overall, these results are consistent with the effects for the full sample. Specifically, the extension of the parental leave duration for the first child does not lead to significant differences in health outcomes for the second child.

5.4 Instrumental variables estimates

To put the magnitude of the reduced form estimates into perspective with the related literature, I report instrumental variables (IV) estimates in Table (7). All IV estimates are

[22] I also test for other heterogeneous groups such as gender of the child, above median age of the mother, different sectors the mother works in, and the precedent child born with low birth weight. None of them show significant differences.

statistically insignificant. In comparison to the OLS estimates reported in Table 2 they are in general bigger in absolute terms but most of the time of the same sign for both OLS and IV, meaning that working during pregnancy is positively related to newborn health outcomes. However, IV estimates are often relatively imprecisely measured.

Due to the relatively imprecisely measured effects and the largest sample size in the pooled version, I will concentrate on column (8), the full control model, when interpreting the effect sizes. In economic terms, the effect of the mother's work status during pregnancy on the three newborn health measures is relatively large. The estimate on low birth weight is -0.017 with a standard error of 0.013. The effect of working on the incidence of a preterm birth is -0.007 with a standard error of 0.013. Both estimates are not significantly different from 0 and therefore suggest that a mother's work status does not affect the fetus.

At the 5% level, I am able to reject all null hypotheses involving an effect size larger than 0.004 for the incidence of low birth weight. This is based on a one-sided test with an alternative hypothesis that the effect is smaller than the null. Thus, for low birth weight, this analysis provides evidence against an effect larger than an increase of 13% evaluated at the mean. Comparing this effect size with other factors occurring during pregnancy that increase the incidence of low birth weight in the literature, I can rule out that prenatal employment is as bad as maternal influenza hospitalization (+100% stated by Schwandt (2017)), the announcement of 500 job losses in a mother's county (+16% stated by Carlson (2015)), or exposure to the death of a close relative (+20% stated by Persson and Rossin-Slater (2018)).[23] As it is especially the possible negative impact of prenatal employment on infant health that is relevant for policy makers, this is good news for the design of maternity leave policies. However, the imprecision in my estimates cautions against strong interpretation.

5.5 Robustness tests

In this section I test the robustness of my results. I start by showing that covariates vary smoothly across the thresholds. For additional robustness tests, I will only focus on the reform in 1990. However, all conclusions remain unchanged for the other reforms.

[23]For this comparison I rely on all references made by (Almond et al., 2017) in Panel A of Table 6 that significantly increase the incidence of low birth weight.

Covariate Balance Testing. In Figure (5) I examine the relationship between nonoutcome variables and the rating variable, the birth month of the first child. According to the theory, these additional control variables should not be affected by the treatment. If manipulation or selection into second motherhood around the cutoff occurs, the validity of the RDD might be violated. In that case I would observe discontinuities in covariates that could harm the causal interpretation of the reduced form estimates.

In the first four graphs (a)-(d) per Panel in Figure (5) no discontinuities in the observable characteristics of mothers having a second child are detectable around the cutoffs. This suggests that there are no changes in the composition of mothers giving birth to their second child. These patterns are also confirmed by balancing tests reported in Table (8). Graph (e) per Panel addresses the issue that the policy reform might have affected the age difference to the first child, which could violate the interpretation of the causal pathway. While the age difference, measured in months from first to second born, varies smoothly with no significant jump around the cutoff in the year 1990, there is a small but significant drop of less than 1 month (-0.7 month) in 1996 and an increase of less than 1 month (+0.6 month) in 2000.

For a better understanding of the age difference between first and second born, I decompose the effect into respective age categories implied by the three policy reforms. Table (9) reports the results. An extension of parental leave duration as in 1990 and 2000 reduces the likelihood of very small age differences, while a reduction in parental leave duration increases the likelihood of small age differences. The effects for the medically relevant time window of less than 18 months are significant for the 1990 and 1996 reform. If a short spacing would harm the health of the second born child, this would bias the effect on newborn health upwards in 1990 and downwards in 1996. However, in terms of magnitude both effects are small.

Different birth weight and gestational age thresholds. In Figure (6) I report the reduced form effect for separate regressions with different choices of birth weight and gestational age cutoffs. While low birth weight, defined as less than 2500 grams, and being born preterm, defined as less than 37 weeks of gestational age, are relatively arbitrary chosen cutoffs, I show that the results are robust to different choices for these outcome variables. It can be seen, that no matter what birth weight and gestational age threshold is chosen, the effect of the policy reforms on these birth outcomes is statistically insignificant.

Donut estimations. Panel A of Table (10) shows results of Donut estimations, creating a hole in the middle of the sample. Here I exclude data points from second born with older siblings that are born within a week on both sides of the cutoff. As all policy reforms have been announced only shortly before the actual implementation, selection into motherhood and therefore a perfect planning of birth into a specific policy regime can be ruled out. However, the choice of a Caesarean section and therefore timing of birth might still be possible up to a short time window of around 1 week. Lalive and Zweimüller (2009) investigate the issue of this narrow-window timing, and argue that although there is a steady increase in births on a day-to-day basis from June to July, there is no discontinuity in the reported births on July 1. Also in my analysis, results are robust to this adjustment and all conclusions remain exactly the same.

Adding third born. In Panel B of Table (10) I add the sample of third born with an older sibling born 24 months around the cutoff of a policy reform. While the sample size and therefore also the precision increase, Panel B detects no major changes to the conclusions drawn before. Mothers are significantly less likely to work during pregnancy with their higher-order child if facing longer parental leave duration for their older child. Effects on newborn health measures for their consecutive child are, tough, not significant. This highlights that the results are not specific to second born only.

Sample selection. Panel C of Table (10) addresses the previously raised concern of selection into second motherhood by reporting bounds on treatment effects in presence of possible selection. Applying the monotonicity assumption, yields the bounds reported in Panel C in Table (10), where again I can draw the same conclusions as before. Maternal employment during pregnancy with the second child decreases significantly with no significant effects on newborn health for the second child.

Placebo reform 1988. In Panel D of Table (10) I show estimation results of a placebo regression assuming a policy reform in July 1988. As expected this placebo treatment in a non-reform year shows no significant effects for any of the outcome variables. Interestingly, as this is known to be a true zero effect, I can compare the standard errors of this specification with the results reported for the three reforms in Table (5). The size of the standard errors is very similar for the placebo and the reform regressions.

Finally, in Figure (7) I put the coefficient estimates on newborn health measures of the robustness regression into comparison with the estimates for the different policies and heterogeneous groups. This graph shows the robustness of the different specifications, and highlights that the insignificant effect on newborn health outcomes is precisely estimated.

6 Conclusion

This paper analyzes the effect of maternal employment during pregnancy on newborn health. I use data on all Austrian births and working histories of the respective mothers. To overcome the endogeneity of employment decisions I exploit three policy reforms of the duration of parental leave. I find no evidence that prenatal employment affects the probability of being born preterm, nor the probability of being born with low birth weight. For the largest reform in 1990, extending parental leave from one to two years, I document a significant 19.1 pp decline in the share of mothers employed during pregnancy with their second child and insignificant effects for all newborn health measures. Even though prenatal employment varies substantially by marital status and the worker's type, I find no evidence of any effect on health outcomes at birth even in these sub-populations.

Altogether, this study shows that the net effect for the second child of changes in parental leave policies are insignificant. This result is robust to the inclusion of covariates and several different specifications. The zero effect of prenatal employment adds to the scarce and contrasting literature on maternal employment during pregnancy on newborn health. With increasing employment rates for pregnant women, it is crucial to understand the mechanisms of prenatal employment on newborn health.

The literature on the effects of pregnancy conditions on long-term outcomes identifies the second trimester of a pregnancy as the trimester with the strongest neural brain development (Schwandt, 2017; Black et al., 2014). This could mean, that the effect of exposure to maternal labor market participation during pregnancy could only be detectable in the long run and may not be visible at the day of birth. As such, linking prenatal employment with long-term outcomes such as educational attainment and labor market participation of the child itself could be explored in future studies.

Overall, the results of this paper suggest that maternal employment (to the end of the seventh month) has no significant effect on the health of newborns. This finding is especially relevant for the design of labor market and parental leave policies. Unlike post-natal child rearing, the duty of pregnancy cannot be shared among partners. However, this should not lead to discrimination towards women, as there is no evidence that the working status during pregnancy leads to bad health outcomes for the unborn baby. As a result of this, parental leave policies should continue to be designed towards families instead of women. There is no need to design more generous maternal leave policies in order to protect unborn children which ultimately might limit woman's participation in workplace out of a firm's concern of the yet-to-be-conceived child.

Furthermore, the results of this paper help women to optimally allocate their free time given by parental leave policies. Several countries in the world design their parental leave policies in such a way, that women can choose how to divide a certain amount of weeks pre- and post-birth. With the results at hand, women are safe to take the majority of their parental leave time after giving birth with beneficial impact on their child's development.

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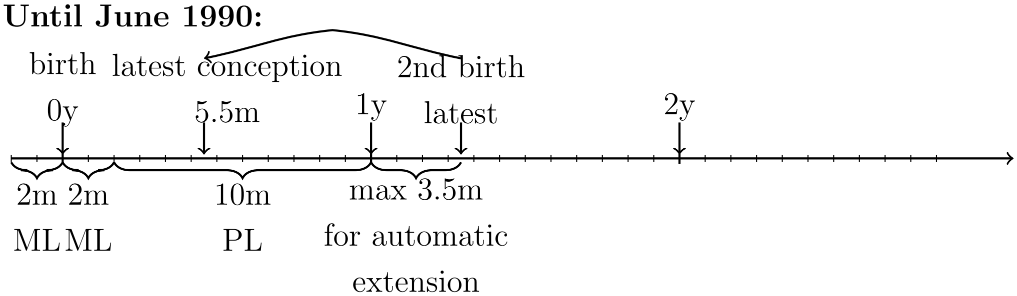
7 Figures and Tables

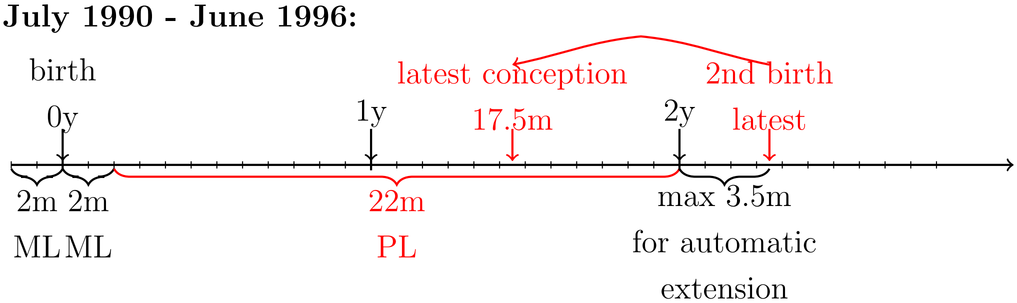
Figure 1: Overview policy changes

Until June 1990:



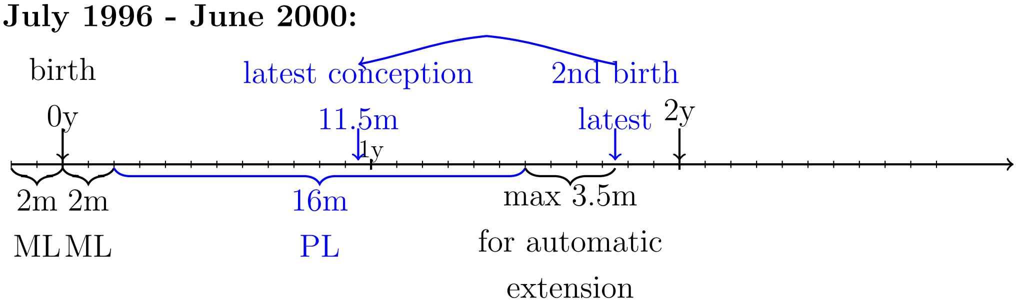
- PL up to age of 12m for both duration of cash benefits and job protection

July 1990 - June 1996:



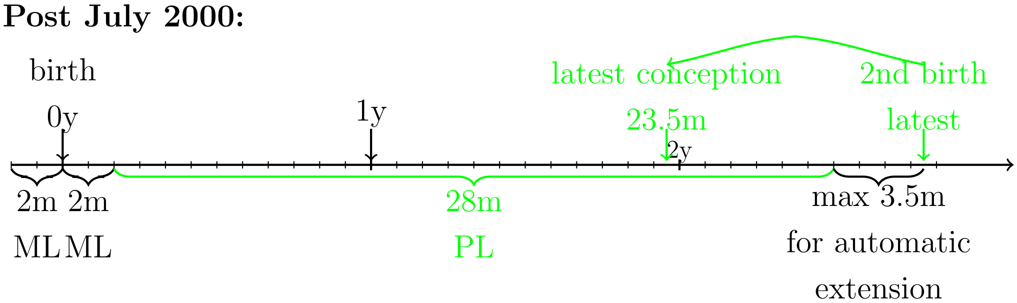
- PL up to age of 24m for both duration of cash benefits and job protection

July 1996 - June 2000:



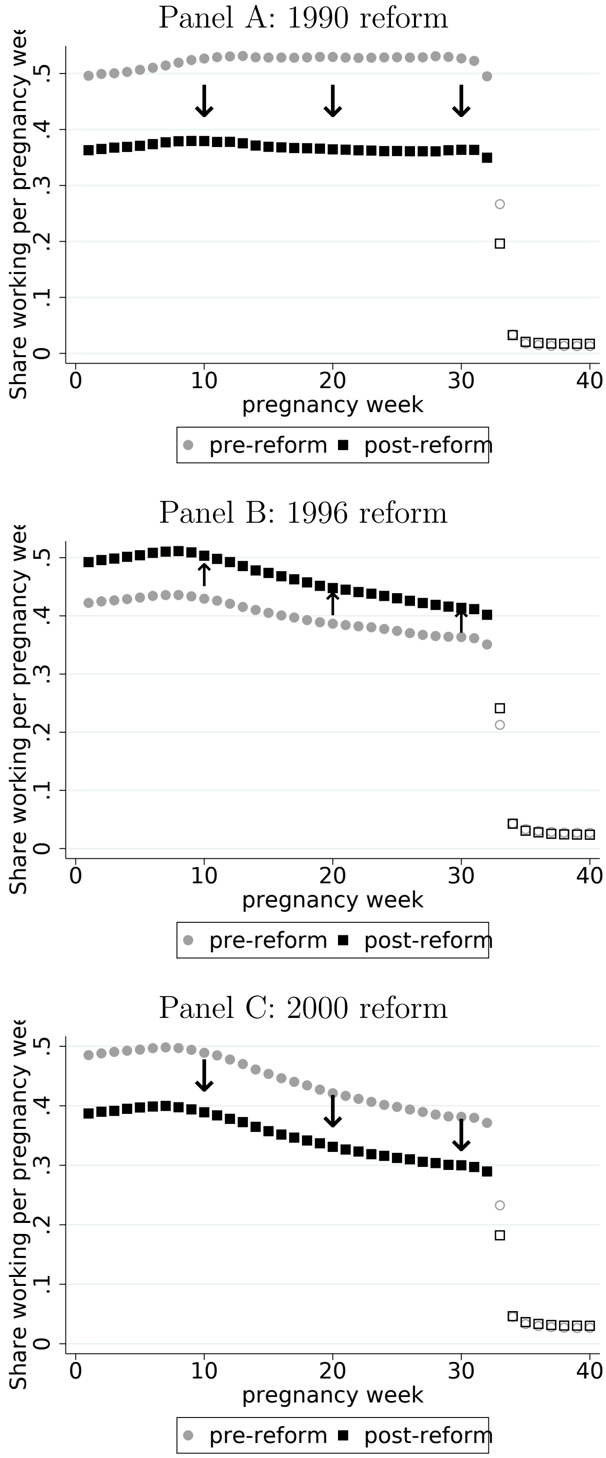
- PL up to age of 18m for duration of cash benefits; job protection unchanged (24m)

Post July 2000:



- PL up to age of 30m for duration of cash benefits; job protection unchanged (24m)

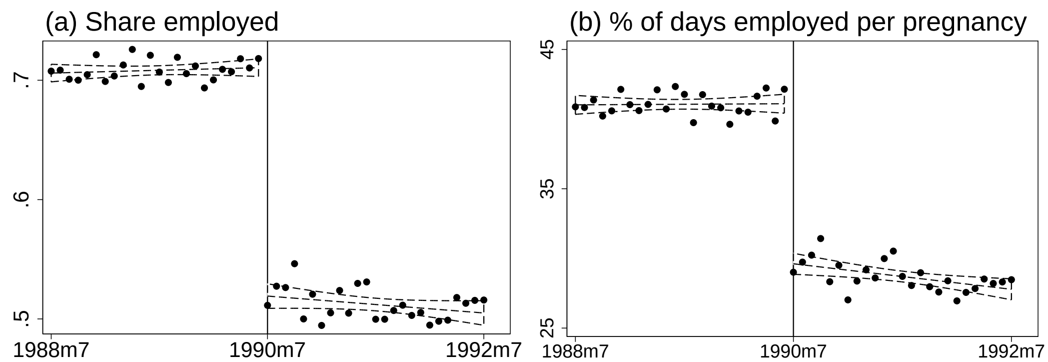
Figure 2: Work status of mother during pregnancy with second child



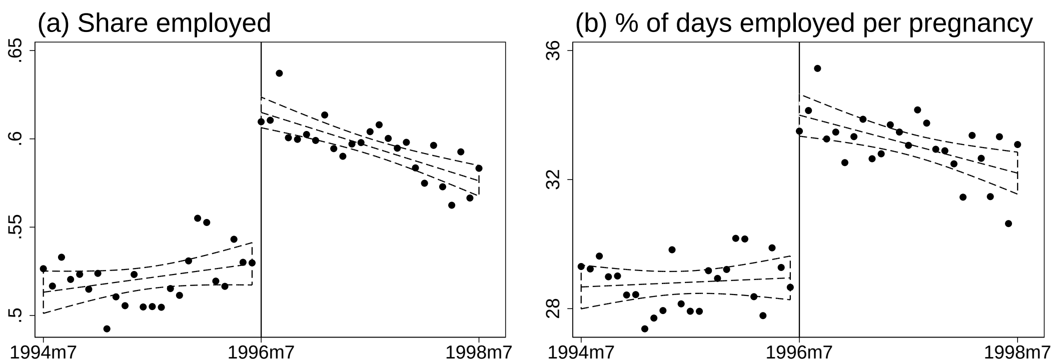
Notes: This Figure reports the share of women who work during pregnancy with their second child. Mothers considered are all women pregnant with their second child who gave birth to their first child 24 months before and after the policy changes in July 1990, July 1996 and July 2000. Hollow squares and circles refer to weeks where mothers are supposed to be on maternity leave (after week 32). The sample consists of all matched and eligible mothers that are still pregnant at a given week of pregnancy (i.e. with preterm births the sample gets smaller moving from week 0 to week 40).

Figure 3: RDD plots - prenatal employment

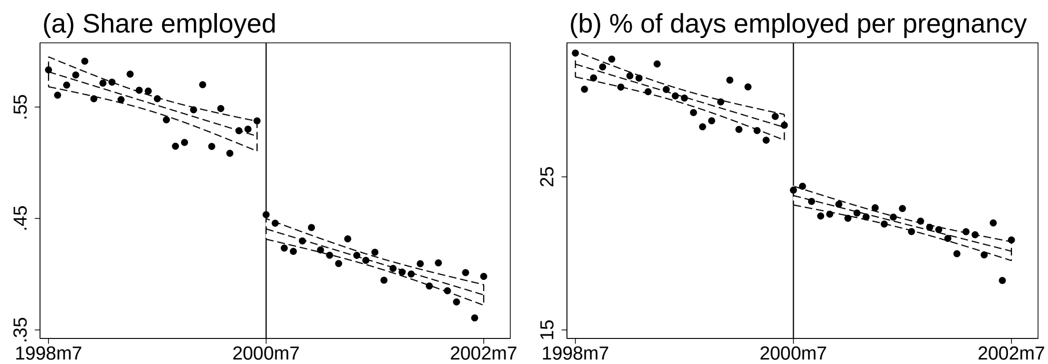
Panel A: 1990 reform



Panel B: 1996 reform

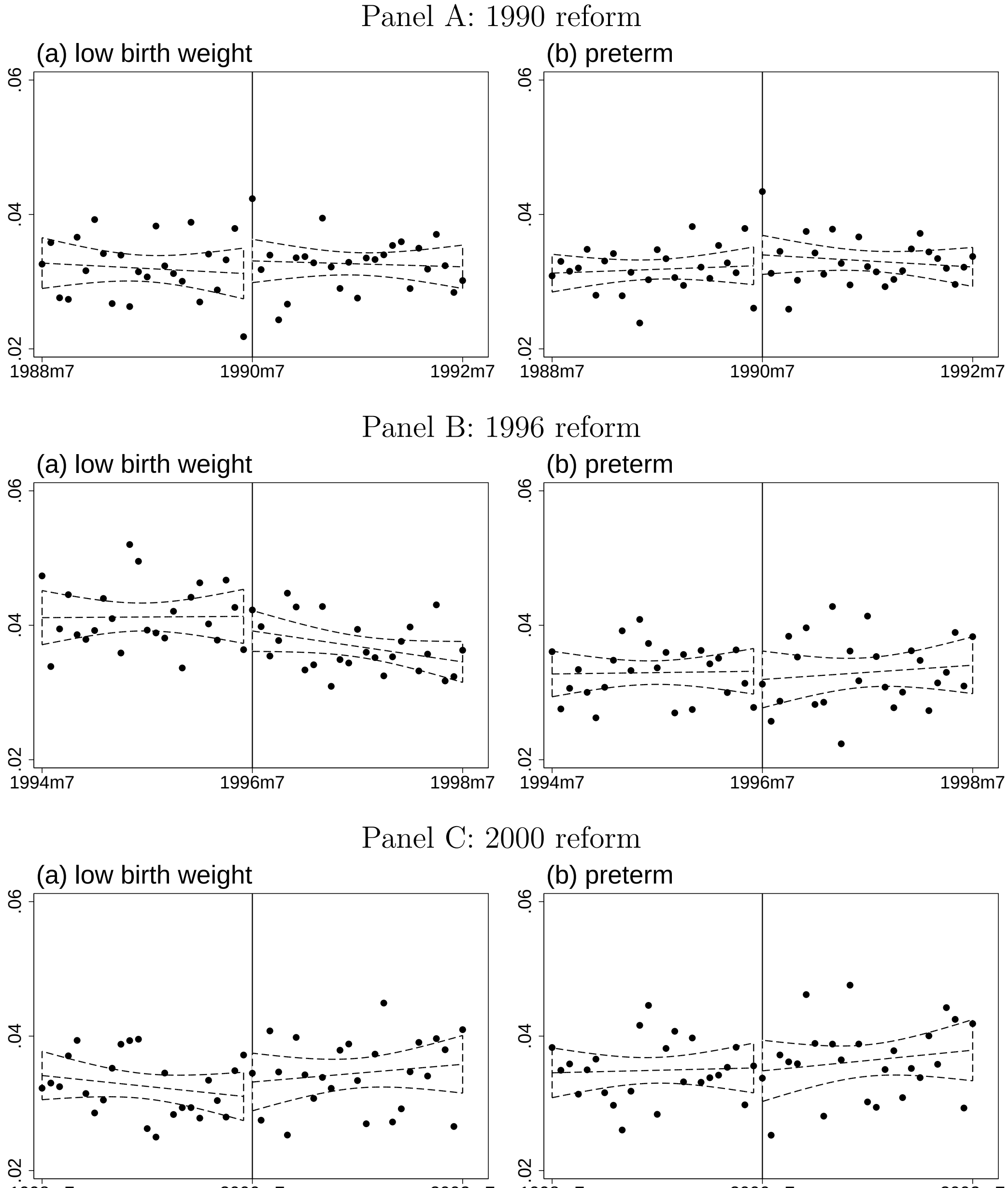


Panel C: 2000 reform



Notes: This Figure reports the fraction of mothers working during pregnancy with their second child and the average in% of days employed per pregnancy by month of birth of the first child. Panel A and C refer to the increase in parental leave duration in July 1990 and July 2000, respectively. Panel B shows the policy reform in July 1996, where parental leave duration declined. All panels were estimated using the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

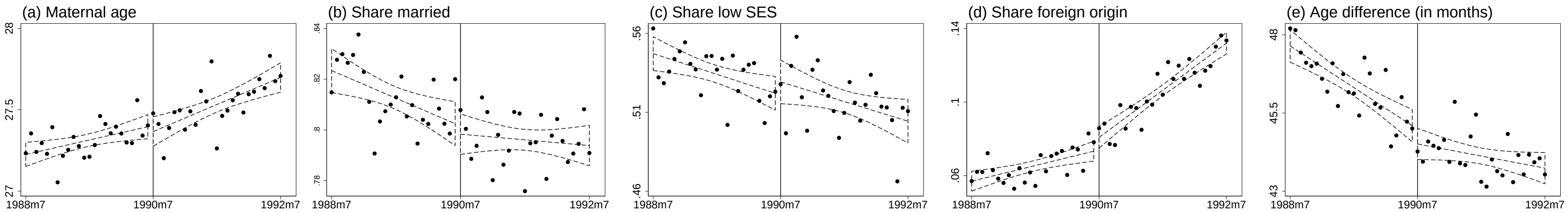
Figure 4: RDD plots - newborn health



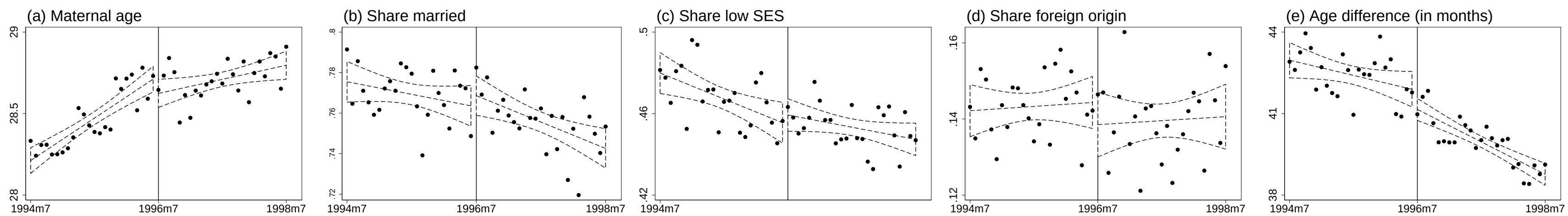
Notes: This Figure reports the average share of second children being born with low birth weight and preterm by month of birth of the first child (their older sibling). Panel A and C refer to the increase in parental leave duration in July 1990 and July 2000, respectively. Panel B shows the policy reform in July 1996, where parental leave duration declined. Note that I introduce a shifter for the first figure (a) in Panel B. Birth weight has been reported in hectograms up to December 1998. From January 1999 birth weight is measured in decagrams. This switch goes hand in hand with a discontinues increase in birth weight and decrease in the probability of low birth weight most likely due to a rounding down previous to 1999. I correct for it in these graphs by multiplying birth weight observed after the shift with the change in yearly average values from 1998 to 1999. All panels were estimated using the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

Figure 5: RDD plots - covariates

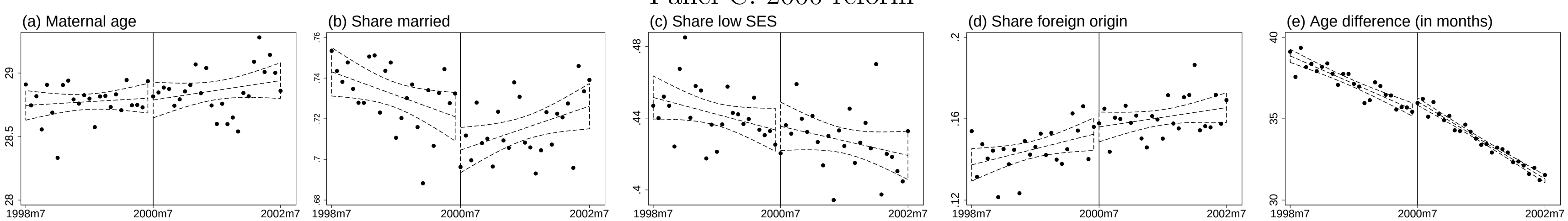
Panel A: 1990 reform



Panel B: 1996 reform



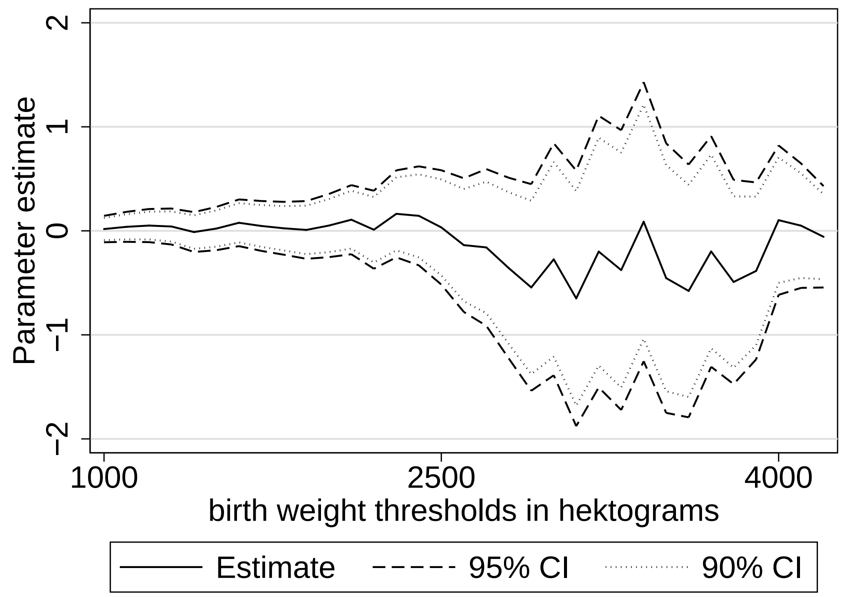
Panel C: 2000 reform



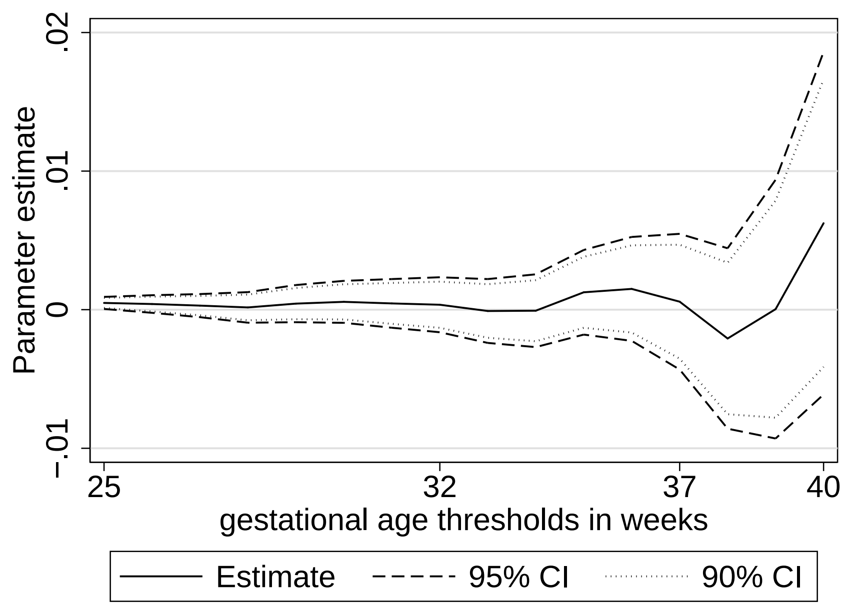
Notes: This Figure reports the composition of mothers giving birth to their second child by month of birth of the first child and the age difference in months from first to second born. Panel A and C refer to the increase in parental leave duration in July 1990 and July 2000, respectively. Panel B shows the policy reform in July 1996, where parental leave duration declined. All panels were estimated using the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

Figure 6: RDD estimates for different thresholds - 1990 reform

Panel A: Birth weight



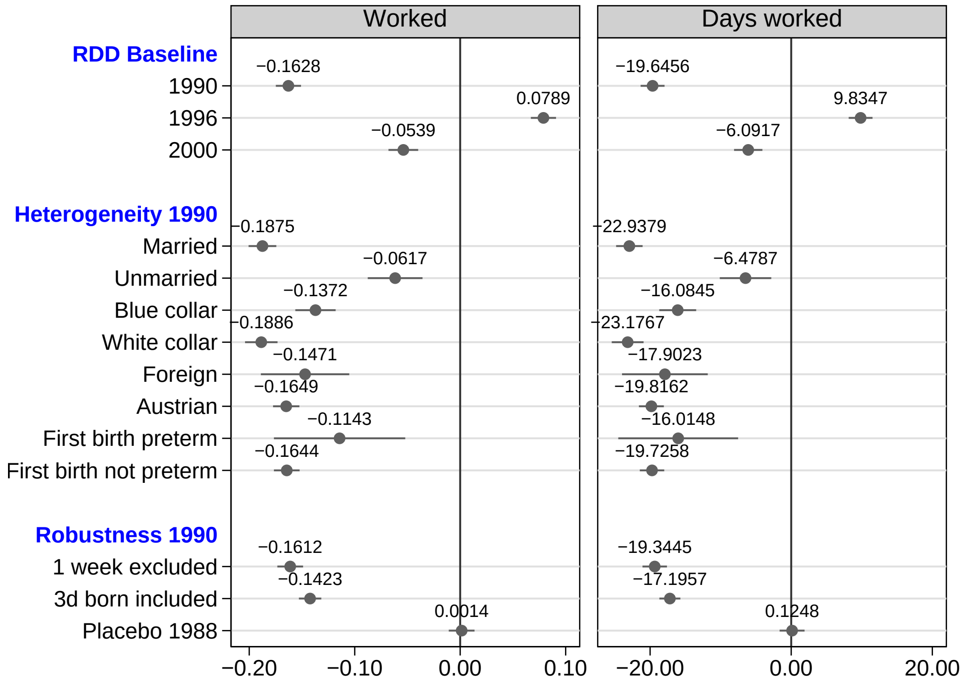
Panel B: Gestational age



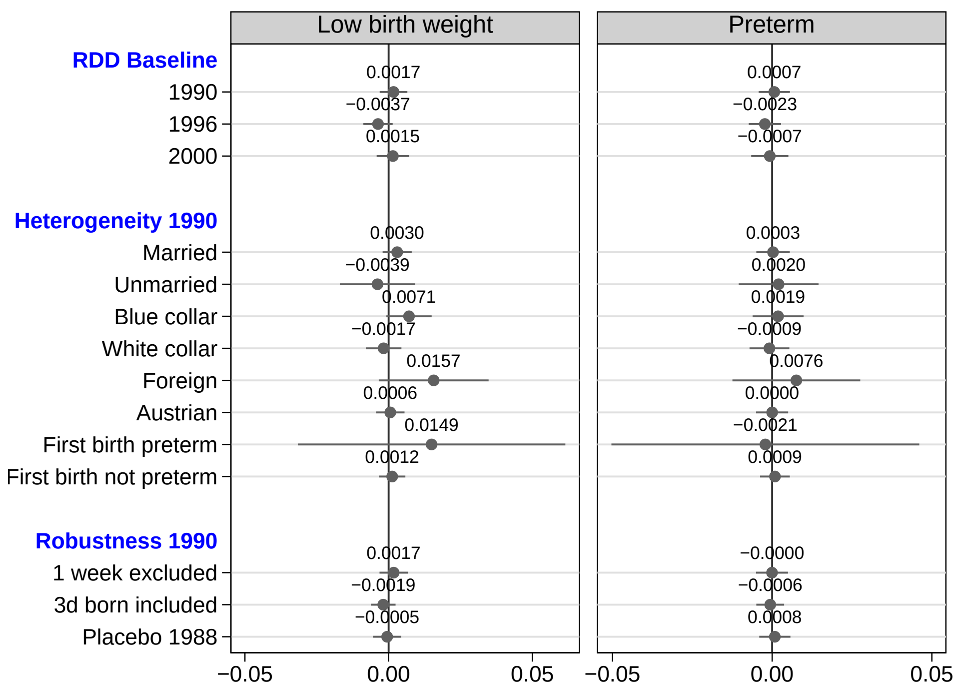
Notes: This Figure reports parameter estimates for the coefficient δ_1 in Equation (3). In Panel A (B) for every 100 grams (week) a separate regression is estimated, corresponding to another birth weight (gestational age) threshold than the one used in the paper of 2500 grams (37 weeks). 95% confidence intervals are shown by the dashed line, and 90% confidence intervals by dotted lines. All estimates are based on regressions including the full set of controls. The sample consists of all matched and eligible mothers that gave birth to their first child not more than 24 months apart from the policy reform in July 1990.

Figure 7: RDD estimates in comparison

Panel A: Work estimates



Panel B: Newborn health estimates



Notes: This Figure reports in Panel A (B) parameter estimates for different specifications for the coefficient δ_1 of having an older sibling born post policy reform on work status and days worked (low birth weight and preterm). Regression coefficients with a 95% confidence interval are displayed. The sample consists of all matched and eligible mothers that gave birth to their first child not more than 24 months apart from the respective policy reform.

Table 1: Descriptive statistics

	Full sample	1990 sample	1996 sample	2000 sample
Birth weight (in gram)	3,320.028	3,381.319	3,410.534	3,419.033
	(519.768)	(500.795)	(505.488)	(503.999)
Birth length (in cm)	50.494	50.699	50.785	50.746
	(2.641)	(2.507)	(2.559)	(2.572)
Gestational length (in weeks)	39.686	39.797	39.757	39.665
	(1.776)	(1.625)	(1.678)	(1.710)
Preterm birth	0.043	0.032	0.033	0.036
Low birth weight	0.046	0.032	0.032	0.033
Female baby	0.487	0.489	0.486	0.487
Mother married	0.688	0.804	0.763	0.724
Mother foreign	0.121	0.087	0.140	0.152
Mother aged 15-19	0.056	0.013	0.009	0.009
Mother aged 20-24	0.278	0.248	0.174	0.178
Mother aged 25-29	0.356	0.445	0.412	0.375
Mother aged 30-34	0.221	0.230	0.312	0.323
Mother aged 35-39	0.077	0.058	0.086	0.103
Mother aged 40-45	0.012	0.006	0.008	0.011
Mother of low SES	0.516	0.526	0.462	0.434
Worked	0.759	0.611	0.607	0.563
Days worked	95.881	69.826	68.847	63.528
Observations	1,279,374	87,566	77,279	63,481

Notes: The full sample covers the universe of births occurring from 1984 to 2007 to matched and eligible mothers. Columns (2)-(4) restrict the full sample to second born children with an older sibling that is born 24 months pre- and post a policy reform. The policy reforms considered happen in July 1990 for Column (2), July 1996 for Column (3) and July 2000 for Column (4).

Table 2: OLS results on preterm and low birth weight

Dependent Variable	(1)	(2)	(3)	(4)
	Preterm		Low birth weight	
Panel A: Work status				
Worked	-0.0020	-0.0021	-0.0027	-0.0023
	(0.0008)**	(0.0008)***	(0.0008)***	(0.0008)***
Sick	0.0322	0.0313	0.0314	0.0305
	(0.0018)***	(0.0018)***	(0.0018)***	(0.0018)***
Panel B: Days worked				
Days worked	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0000)***	(0.0000)***	(0.0000)***	(0.0000)***
Sick	0.0312	0.0305	0.0305	0.0298
	(0.0018)***	(0.0018)***	(0.0018)***	(0.0018)***
Mother Controls	No	Yes	No	Yes
Mean Dep. Var.	0.0335	0.0335	0.0327	0.0327
Observations	226,824	226,824	226,824	226,824

Notes: This Table is estimated on the pooled sample of the three RDD regression windows. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Additional controls included in all columns are year and month of birth FE, and a gender dummy. Mother's characteristic controls are dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

Table 3: RDD effects on maternal employment during pregnancy

	1990		1996		2000	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable employment status						
1{Post policy reform}	-0.191	-0.191	0.069	0.072	-0.064	-0.064
	(0.006)***	(0.006)***	(0.007)***	(0.007)***	(0.008)***	(0.008)***
Comparison Mean	0.714	0.714	0.587	0.587	0.600	0.600
Panel B: Dependent variable days worked						
1{Post policy reform}	-23.105	-23.011	9.147	9.493	-6.712	-6.636
	(0.924)***	(0.891)***	(0.980)***	(0.954)***	(1.083)***	(1.059)***
Comparison Mean	82.720	82.720	65.607	65.607	67.369	67.369
Additional Mother Controls	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	77,279	77,279	63,481	63,481

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. Panel A estimates the effects on employment status and Panel B on the number of days worked during pregnancy with the second child.

Table 4: RDD heterogeneous effects on maternal employment during pregnancy - 1990 reform

Dependent Variable	Worked		Days worked	
	(1)	(2)	(3)	(4)
Panel A: Source of heterogeneity - marital status				
1{Post policy reform}	-0.075	-0.072	-8.220	-7.678
	(0.015)***	(0.015)***	(2.131)***	(2.009)***
1{Post policy reform}	-0.145	-0.149	-18.517	-19.118
*1{Married}	(0.017)***	(0.016)***	(2.363)***	(2.240)***
Panel B: Source of heterogeneity - type of worker				
1{Post policy reform}	-0.215	-0.212	-26.534	-26.228
	(0.008)***	(0.008)***	(1.243)***	(1.206)***
1{Post policy reform}	0.044	0.038	6.674	6.029
*1{Blue collar}	(0.013)***	(0.013)***	(1.894)***	(1.834)***
Panel C: Source of heterogeneity - origin				
1{Post policy reform}	-0.194	-0.192	-23.339	-23.096
	(0.007)***	(0.007)***	(0.965)***	(0.928)***
1{Post policy reform}	0.031	0.023	3.323	1.988
*1{Foreign}	(0.023)	(0.023)	(3.334)	(3.341)
Panel D: Source of heterogeneity - first birth preterm				
1{Post policy reform}	-0.195	-0.195	-23.459	-23.377
	(0.007)***	(0.006)***	(0.945)***	(0.909)***
1{Post policy reform}	0.001	0.001	0.090	0.093
*1{First birth preterm}	(0.000)***	(0.000)***	(0.045)**	(0.045)**
Comparison Mean	0.714	0.714	82.720	0.714
Additional Mother Controls	No	Yes	No	Yes
Observations	87,566	87,566	87,566	87,566

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reform in July 1990. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The $1\{\textit{post policy reform}\}$ coefficient estimate reports the impact of a first birth just after the policy reform versus just before. The $1\{\textit{post policy reform}\} * 1\{\textit{heterogenous group}\}$ interaction term, reports the estimate with respect to the heterogeneity reported. Panel A estimates the heterogeneous effects with respect to marital status, Panel B with type of worker, Panel C with origin and Panel D with whether the first birth was preterm or not.

Table 5: RDD effects on newborn health of second born

	1990		1996		2000	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable low birth weight						
1{Post policy reform}	0.002	0.002	-0.004	-0.004	0.001	0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Comparison Mean	0.031	0.031	0.034	0.034	0.031	0.031
Panel B: Dependent variable preterm						
1{Post policy reform}	0.001	0.001	-0.002	-0.002	-0.001	-0.001
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Comparison Mean	0.034	0.032	0.033	0.033	0.035	0.035
Additional Mother Controls	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	77,279	77,279	63,481	63,481

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. All columns include a gender dummy, year of birth and month of birth fixed effects. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. Panel A estimates the effects on low birth weight and Panel B on being born preterm.

Table 6: RDD heterogeneous effects on newborn health - 1990 reform

Dependent Variable	Low birth weight		Preterm	
	(1)	(2)	(3)	(4)
Panel A: Source of heterogeneity - marital status				
1{Post policy reform}	-0.004	-0.004	0.002	0.002
	(0.007)	(0.007)	(0.006)	(0.006)
1{Post policy reform}	0.007	0.007	-0.002	-0.001
*1{Married}	(0.007)	(0.007)	(0.007)	(0.007)
Panel B: Source of heterogeneity - type of worker				
1{Post policy reform}	-0.002	-0.002	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)
1{Post policy reform}	0.008	0.009	0.003	0.003
*1{Blue collar}	(0.005)	(0.005)*	(0.005)	(0.005)
Panel C: Source of heterogeneity - origin				
1{Post policy reform}	0.001	0.001	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.003)
1{Post policy reform}	0.014	0.013	0.008	0.007
*1{Foreign}	(0.010)	(0.010)	(0.010)	(0.010)
Panel C: Source of heterogeneity - first birth preterm				
1{Post policy reform}	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
1{Post policy reform}	0.000	0.000	-0.000	-0.000
*1{First birth preterm}	(0.000)	(0.000)	(0.000)	(0.000)
Comparison Mean	0.031	0.031	0.032	0.032
Additional Mother Controls	No	Yes	No	Yes
Observations	87,566	87,566	87,566	87,566

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reform in July 1990. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. All columns include a gender dummy, year of birth and month of birth fixed effects. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The $1\{\textit{post policy reform}\}$ coefficient estimate reports the impact of a first birth just after the policy reform versus just before. The $1\{\textit{post policy reform}\} * 1\{\textit{heterogenous group}\}$ interaction term, reports the estimate with respect to the heterogeneity reported. Panel A estimates the heterogeneous effects with respect to marital status, Panel B with type of worker, Panel C with origin and Panel D with whether the first birth was preterm or not.

Table 7: IV estimates of work status on newborn health of second born

	1990		1996		2000		Pooled	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent variable low birth weight								
worked	-0.010	-0.010	-0.047	-0.045	-0.028	-0.027	-0.017	-0.017
	(0.015)	(0.015)	(0.033)	(0.033)	(0.053)	(0.054)	(0.013)	(0.013)
Comparison Mean	0.031	0.031	0.034	0.034	0.031	0.031	0.032	0.032
Panel B: Dependent variable preterm								
worked	-0.004	-0.004	-0.029	-0.026	0.014	0.014	-0.007	-0.007
	(0.015)	(0.015)	(0.033)	(0.033)	(0.055)	(0.055)	(0.013)	(0.013)
Comparison Mean	0.032	0.032	0.033	0.033	0.035	0.035	0.033	0.033
Additional Mother Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	77,279	77,279	63,481	63,481	226,824	226,824

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. All columns include a gender dummy, year of birth and month of birth fixed effects. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. Panel A estimates the effects on low birth weight and Panel B on being born preterm.

Table 8: Covariate balance test

	1990 (1)	1996 (2)	2000 (3)
Panel A: Dependent variable maternal age			
1{Post policy reform}	-0.032	-0.096	-0.004
	(0.059)	(0.062)	(0.072)
Comparison Mean	27.412	28.735	28.822
Panel B: Dependent variable married			
1{Post policy reform}	-0.003	0.006	-0.014
	(0.005)	(0.006)	(0.007)**
Comparison Mean	0.801	0.763	0.719
Panel C: Dependent variable share low SES			
1{Post policy reform}	0.008	0.005	0.001
	(0.007)	(0.007)	(0.008)
Comparison Mean	0.522	0.456	0.432
Panel D: Dependent variable share foreign origin			
1{Post policy reform}	0.007	-0.006	0.003
	(0.004)*	(0.005)	(0.006)
Comparison Mean	0.073	0.144	0.152
Panel E: Dependent variable age difference to first born (in months)			
1{Post policy reform}	-0.445	-0.716	0.581
	(0.438)	(0.357)**	(0.258)**
Comparison Mean	44.905	41.823	35.425
Observations	87,566	77,279	63,481

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. Panel A estimates the effects on maternal age, Panel B on marital status, Panel C on the share of low SES, Panel D on a mother's origin, and Panel E on the age difference to the first born in months.

Table 9: Age differences in detail

	1990 sample (1)	1996 sample (2)	2000 sample (3)
0-18 months	-0.040	0.039	-0.006
	(0.005)***	(0.004)***	(0.005)
0-36 months	0.021	0.002	-0.007
	(0.006)***	(0.007)	(0.008)
0-120 months	0.004	0.004	
	(0.002)*	(0.001)***	
0-16 months	-0.030		
	(0.004)***		
17-28 months	0.057		
	(0.006)***		
29-120 months	-0.023		
	(0.007)***		
0-22 months		0.046	
		(0.006)***	
23-28 months		-0.050	
		(0.005)***	
29-120 months		0.009	
		(0.007)	
0-22 months			-0.031
			(0.007)***
23-34 months			0.018
			(0.007)**
35-120 months			0.013
			(0.008)*
Observations	87,413	76,943	63,134

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. This table reports the $1\{\textit{post policy reform}\}$ parameter estimate on the respective age difference between first and second born child. All columns include mother's characteristic controls for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

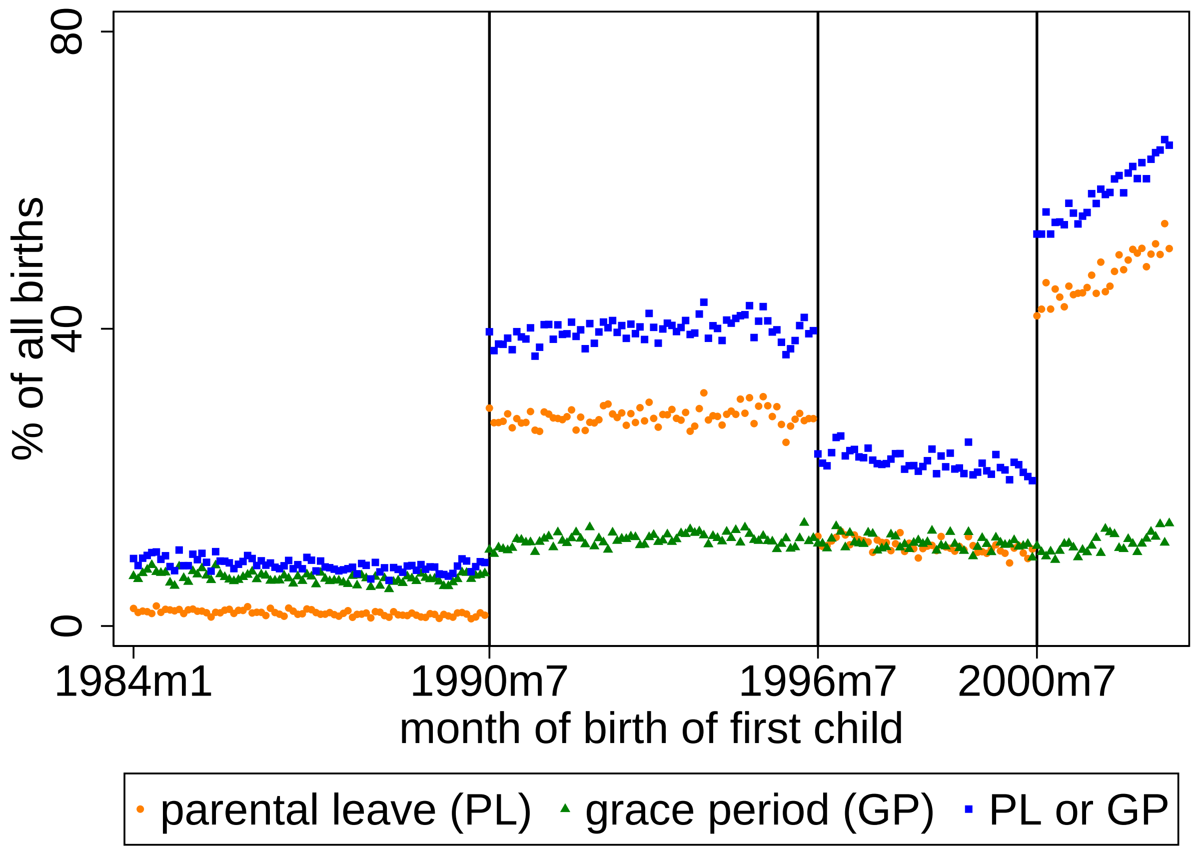
Table 10: Robustness - 1990 reform

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Worked		Days worked		Low birth weight		Preterm	
Panel A: excluding 1 week around cutoff								
1{Post policy reform}	-0.191	-0.190	-22.961	-22.848	0.002	0.002	-0.000	-0.000
	(0.007)***	(0.006)***	(0.942)***	(0.908)***	(0.003)	(0.003)	(0.003)	(0.003)
Observations	86,702	86,702	86,702	86,702	86,702	86,702	86,702	86,702
Panel B: adding third born								
1{Post policy reform}	-0.167	-0.166	-20.050	-19.958	-0.002	-0.002	-0.001	-0.001
	(0.006)***	(0.006)***	(0.811)***	(0.785)***	(0.002)	(0.002)	(0.002)	(0.002)
Observations	113,294	113,294	113,294	113,294	113,294	113,294	113,294	113,294
Panel C: Bounds on Treatment Effects								
Treatment Bounds	[-0.194, -0.127]		[-23.726, -15.663]		[-0.003, 0.008]		[-0.002, 0.008]	
Panel D: Placebo cutoff July 1988								
1{Post policy reform}	0.000	0.001	-0.243	-0.123	-0.001	-0.001	0.001	0.001
	(0.006)	(0.006)	(0.947)	(0.920)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	80,754	80,754	80,754	80,754	80,754	80,754	80,754	80,754
Comparison Mean	0.714	0.714	82.720	82.720	0.031	0.031	0.032	0.032
Comparison Mean incl. 3 born	0.684	0.684	78.625	78.625	0.034	0.034	0.034	0.034
Comparison Mean 1988	0.708	0.708	82.331	82.331	0.032	0.032	0.029	0.029
Additional Mother Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: This Table is estimated on the matched and eligible sample of second born (and 3d born in Panel B) with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Standard errors in the treatment bound estimation reported in Panel C are calculated via bootstrapping. Columns (5-10) include a gender dummy, year of birth and month of birth fixed effects. In Panel B Columns (5-10) additionally include birth order dummies. Additional Mother Controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first (or previous in Panel B) birth just after the policy reform versus just before. In Panel A births that happen within around 1 week of a policy change are excluded. Panel B additionally also includes births of third born, Panel C implements bounds on the treatment effect under selection into second motherhood, and Panel D shows Placebo estimates for an imaginary reform in July 1988.

Appendix

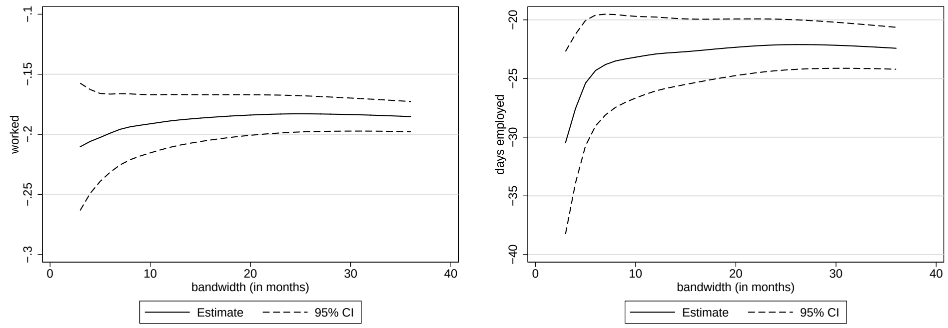
Figure A1: Percent of second children born within parental leave and grace period



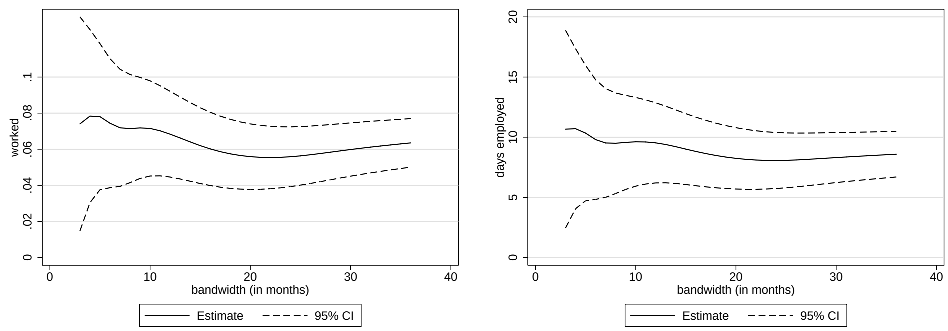
Notes: This Figure shows the fraction of first born in families with at least two children that have a younger sibling that is born within the parental leave and grace period. Orange dots refer to the%age of second children born within the parental leave period of the first child. Green triangles refer to the%age of second children born within the grace period after the parental leave period of the first child. Blue squares refer to the sum of both. The solid vertical lines refer to the three policy changes, increasing the latest consecutive birth date to 27.5 months in July 1990, decreasing it to 21.5 months in July 1996 and raising it again to 33.5 months in July 2000. The data consists of the full matched and eligible sample of mothers with at least two children.

Figure A2: Robustness to different choices of bandwidths - work status and days employed

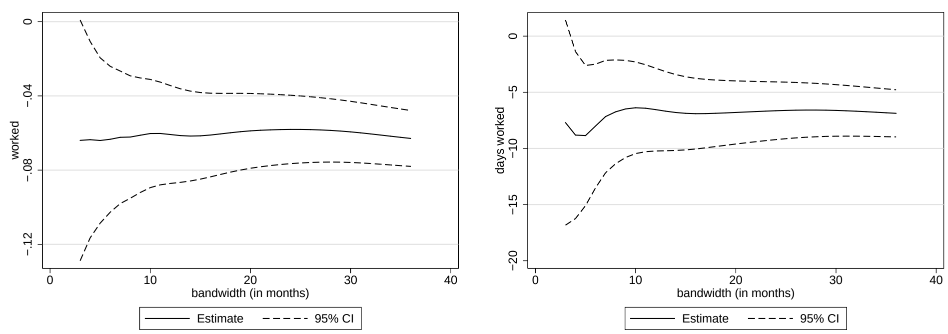
Panel A: 1990



Panel B: 1996



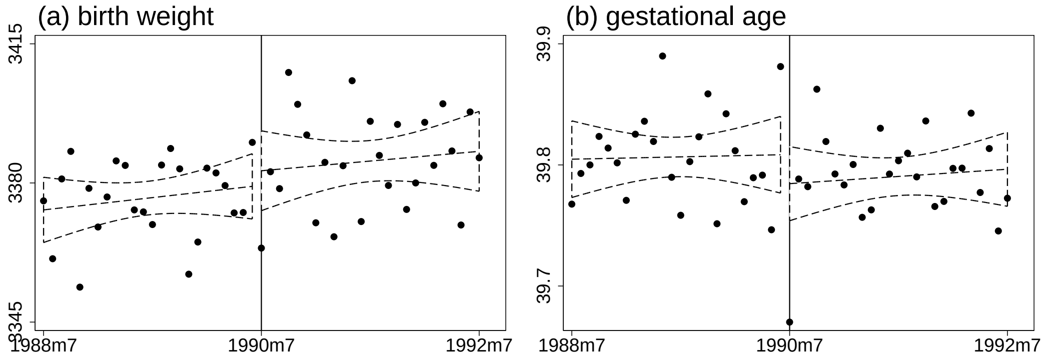
Panel C: 2000



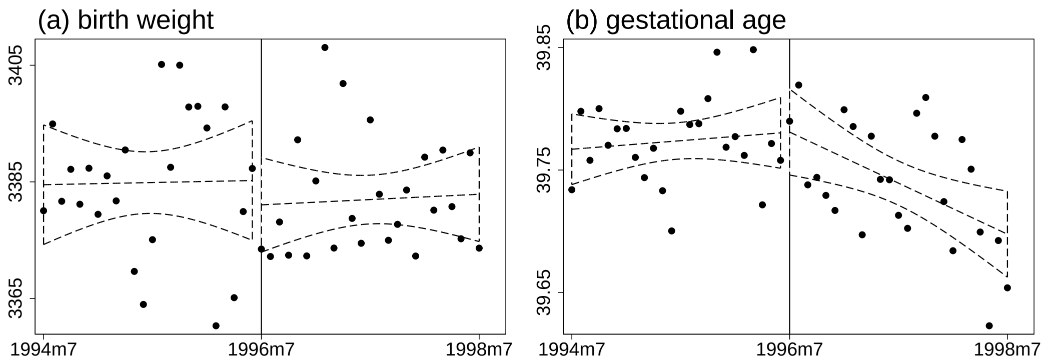
Notes: This Figure reports the robustness of the RDD estimate on the work status and the days employed from Equation (2) to different choices of bandwidths. The solid line refers to the point estimate of separate equations, and the dashed lines indicate the corresponding 95% confidence interval. Panel A and C refer to the increase in parental leave duration in July 1990 and July 2000, respectively. Panel B shows the policy reform in July 1996, where parental leave duration declined. All panels were estimated using the full sample of matched and eligible mothers.

Figure A3: RDD plots - newborn health

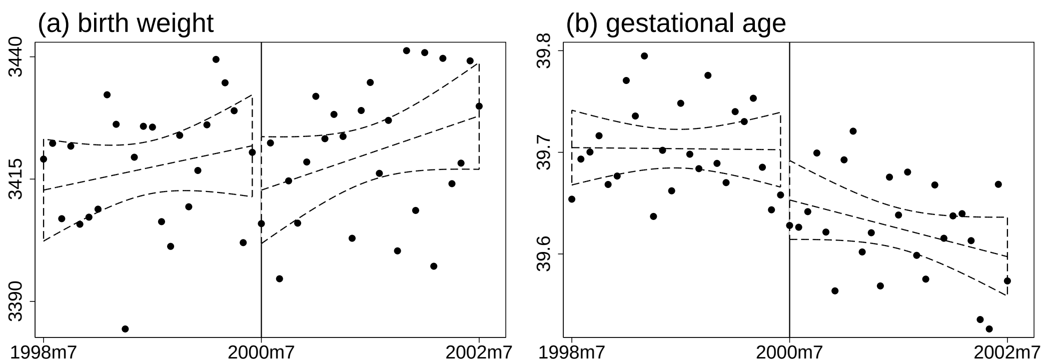
Panel A: 1990 reform



Panel B: 1996 reform



Panel C: 2000 reform



Notes: This Figure reports the average birth weight and gestational age of second children by month of birth of the first child (their older sibling). Panel A and C refer to the increase in parental leave duration in July 1990 and July 2000, respectively. Panel B shows the policy reform in July 1996, where parental leave duration declined. Note that I introduce a shifter for the first figure (a) in Panel B. Birth weight has been reported in hectograms up to December 1998. From January 1999 birth weight is measured in decagrams. This switch goes hand in hand with a discontinues increase in birth weight most likely due to a rounding down previous to 1999. I correct for it in these graphs by multiplying birth weight observed after the shift with the change in yearly average values from 1998 to 1999. All panels were estimated using the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

Table A1: OLS results on birth weight and gestational length

	(1)	(2)	(3)	(4)
Dependent Variable	Birth weight		Gestational length	
Panel A: Work status				
Worked	11.3360	6.3355	0.0136	0.0285
	(2.1728)***	(2.2083)***	(0.0073)*	(0.0074)***
Sick	-81.5409	-80.1422	-0.3414	-0.3484
	(4.4193)***	(4.4512)***	(0.0163)***	(0.0165)***
Panel B: Days worked				
Days worked	0.1721	0.1530	0.0007	0.0008
	(0.0152)***	(0.0154)***	(0.0000)***	(0.0000)***
Sick	-78.7184	-78.1331	-0.3294	-0.3373
	(4.4173)***	(4.4437)***	(0.0163)***	(0.0164)***
Mother Controls	No	Yes	No	Yes
Mean Dep. Var.	3,401.5743	3,401.5743	39.7474	39.7474
Observations	226,824	226,824	226,824	226,824

Notes: This Table is estimated on the pooled sample of the three RDD regression windows. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Additional controls included in all columns are year and month of birth FE, and a gender dummy. Mother's characteristic controls are dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

Table A2: OLS results on preterm and low birth weight - Full set of mother characteristic controls

	(1)	(2)	(3)	(4)
Dependent Variable	Preterm		Low birth weight	
Panel A: Work status				
Worked	-0.0020	-0.0021	-0.0027	-0.0023
	(0.0008)**	(0.0008)***	(0.0008)***	(0.0008)***
Sick	0.0322	0.0313	0.0314	0.0305
	(0.0018)***	(0.0018)***	(0.0018)***	(0.0018)***
Married		-0.0056		-0.0094
		(0.0010)***		(0.0010)***
Foreign		0.0047		-0.0003
		(0.0013)***		(0.0012)
Aged 20-24		-0.0185		-0.0184
		(0.0047)***		(0.0048)***
Aged 25-29		-0.0202		-0.0214
		(0.0047)***		(0.0048)***
Aged 30-34		-0.0149		-0.0173
		(0.0048)***		(0.0048)***
Aged 35-39		-0.0027		-0.0027
		(0.0050)		(0.0051)
Aged 40-45		0.0139		0.0110
		(0.0074)*		(0.0074)
Low SES		0.0034		0.0044
		(0.0008)***		(0.0008)***
Panel B: Days worked				
Days worked	-0.0001	-0.0001	-0.0001	-0.0001
	(0.0000)***	(0.0000)***	(0.0000)***	(0.0000)***
Sick	0.0312	0.0305	0.0305	0.0298
	(0.0018)***	(0.0018)***	(0.0018)***	(0.0018)***
Married		-0.0054		-0.0092
		(0.0010)***		(0.0010)***
Foreign		0.0045		-0.0005
		(0.0013)***		(0.0012)
Aged 20-24		-0.0176		-0.0177
		((11.0632))***		(0.0048)***
Aged 25-29		-0.0185		-0.0201
		(0.0047)***		(0.0048)***
Aged 30-34		-0.0126		-0.0155

		(0.0048)***		(0.0048)***
Aged 35-39		-0.0001		-0.0008
		(0.0050)		(0.0051)
Aged 40-45		0.0165		0.0130
		(0.0074)**		(0.0074)*
Low SES		0.0032		0.0043
		(0.0008)***		(0.0008)***
Mother Controls	No	Yes	No	Yes
Mean Dep. Var.	0.0335	0.0335	0.0327	0.0327
Observations	226,824	226,824	226,824	226,824

Notes: This Table is estimated on the pooled sample of the three RDD regression windows. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Additional controls included in all columns are year and month of birth FE, and a gender dummy. Mother's characteristic controls are dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

Table A3: Robustness to different functional forms - 1990 reform

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent variable work status								
1{Post policy reform}	-0.191	-0.190	-0.191	-0.191	-0.191	-0.190	-0.185	-0.183
	(0.006)***	(0.006)***	(0.006)***	(0.006)***	(0.006)***	(0.006)***	(0.010)***	(0.009)***
Comparison Mean	0.713	0.713	0.714	0.714	0.712	0.712	0.716	0.716
Panel B: Dependent variable days worked								
1{Post policy reform}	-22.965	-22.802	-23.105	-23.011	-23.042	-22.933	-22.244	-22.030
	(0.919)***	(0.885)***	(0.924)***	(0.891)***	(0.922)***	(0.888)***	(1.401)***	(1.349)***
Comparison Mean	82.104	82.104	82.720	82.720	82.400	82.400	82.825	82.825
Additional Mother Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	87,566	87,566	87,566	87,566	87,566	87,566
Spline	Linear	Linear	Linear Interaction	Linear Interaction	Quadratic	Quadratic	Quadratic Interaction	Quadratic Interaction

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth on maternal employment just after the policy reform versus just before. The models that are tested are linear, linear interaction, quadratic, and quadratic interaction as described by Jacob et al. (2012).

Table A4: RDD effects on newborn health of second born

	1990		1996		2000	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable birth weight						
1{Post policy reform}	4.019	4.076	-0.896	-1.178	-7.379	-7.345
	(6.894)	(6.882)	(7.274)	(7.255)	(8.038)	(8.021)
Comparison Mean	3,380.155	3,380.155	3,410.033	3,410.033	3,422.336	3,422.336
Panel B: Dependent variable gestational length						
1{Post policy reform}	-0.016	-0.015	0.013	0.011	-0.032	-0.032
	(0.023)	(0.023)	(0.024)	(0.024)	(0.028)	(0.027)
Comparison Mean	39.778	39.810	39.783	39.783	39.705	39.705
Additional Mother Controls	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	77,279	77,279	63,481	63,481

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. All columns include a gender dummy, year of birth and month of birth fixed effects. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. Panel A estimates the effects on birth weight and Panel B on gestational length.

Table A5: RDD heterogeneous effects on newborn health - 1990 reform

Dependent Variable	Birth weight		Gestational length	
	(1)	(2)	(3)	(4)
Panel A: Source of heterogeneity - marital status				
1{Post policy reform}	4.971	5.380	0.021	0.026
	(16.427)	(16.396)	(0.056)	(0.056)
1{Post policy reform}	-0.742	-1.413	-0.046	-0.051
*1{Married}	(17.987)	(17.958)	(0.061)	(0.061)
Panel B: Source of heterogeneity - type of worker				
1{Post policy reform}	8.993	9.734	-0.017	-0.016
	(9.055)	(9.045)	(0.029)	(0.029)
1{Post policy reform}	-7.168	-9.014	0.007	0.006
*1{Blue collar}	(14.054)	(14.035)	(0.046)	(0.046)
Panel C: Source of heterogeneity - origin				
1{Post policy reform}	4.067	4.048	-0.002	-0.002
	(7.115)	(7.105)	(0.023)	(0.023)
1{Post policy reform}	1.536	3.240	-0.161	-0.156
*1{Foreign}	(27.124)	(27.026)	(0.092)*	(0.092)*
Panel C: Source of heterogeneity - first birth preterm				
1{Post policy reform}	5.620	5.652	-0.017	-0.016
	(6.882)	(6.872)	(0.022)	(0.022)
1{Post policy reform}	-0.472	-0.470	0.000	-0.000
*1{First birth preterm}	(0.435)	(0.433)	(0.002)	(0.002)
Comparison Mean	3,380.155	3,380.155	39.810	39.810
Additional Mother Controls	No	Yes	No	Yes
Observations	87,566	87,566	87,566	87,566

Notes: This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reform in July 1990. Robust standard errors are reported in parentheses; *p<0.10, **p<0.05, ***p<0.01. All columns include a gender dummy, year of birth and month of birth fixed effects. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. The 1{*post policy reform*} ∗ 1{*heterogenous group*} interaction term, reports the estimate with respect to the heterogeneity reported. Panel A estimates the heterogeneous effects with respect to marital status, Panel B with type of worker, Panel C with origin and Panel D with whether the first birth was preterm or not.