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ABSTRACT

The Effect of Prenatal Maternity Leave on Short and Long-Term Child Outcomes*

Maternity leave policies are presumed to be essential to ensure the health of pregnant workers and their unborn children. However, little is known about the optimal duration of prenatal maternity leave and existing policies are not evidence-based. We evaluate a substantial maternity leave extension in Austria, which increased mandatory leave prior to birth from six to eight weeks. Our estimation strategy exploits that the eligibility for the extended leave was determined by a cutoff due date. As an additional source of exogenous variation, we use information on non-working mothers, who are not eligible for maternity leave. Across estimations, we consistently find no evidence for significant effects of this extension on children's health at birth, subsequent maternal health and fertility, and longterm human capital outcomes of children. Our finding is confirmed by a supplementary cross-country panel analysis.

JEL Classification: J13, I18, J28, I13, J83, J88

Keywords: maternity leave, infant health, birth outcomes, birth weight, fertility

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NON-TECHNICAL SUMMARY

While there is large literature studying the effect of maternal employment after childbirth, surprisingly little evidence exists on the effects of employment during pregnancy. Nevertheless, prenatal maternity leave (ML) policies are presumed to be essential to ensure the health of pregnant workers and their unborn children. To fill this gap, we evaluate a substantial prenatal ML extension in Austria. In 1974, mandatory leave prior to birth had been increased from six to eight weeks. We exploit that the eligibility for the extended leave was determined by a cutoff due date. A nice feature of our research design is that assigned and not assigned mothers, were both entitled to the same postnatal ML duration. This allows us to cleanly identify the effect of variation in prenatal ML, not only on outcomes at birth, but also on post-birth outcomes. For our analysis we can rely on high-quality administrative data sources covering the universe of all births in Austria. We find no evidence for significant effects of the prenatal ML extension on children's health at birth. The estimated treatment effects are statistically insignificant and precisely estimated zero effects. This finding is consistent across selected sub-samples of mothers who are expected to be more vulnerable. We also do not find any evidence for significant effects on long-run labor market outcomes. Our analysis of subsequent maternal fertility does not reveal any significant effects of the reform either. Treated and untreated mothers do not significantly differ in the timing of subsequent births or in their completed fertility. The same holds true for their 20 and 40 years survival rates. We conclude from our micro-data analysis that the reform had no measurable effects on children's or on mothers' outcomes. This suggests that six weeks of mandatory prenatal ML are sufficient. Of course, our results have to be interpreted within the scope of the Austrian setting, where expecting mothers (before and after the reform) are always entitled to sick leave if supported by a medical certificate. To provide some evidence for the external validity of our findings, we complement our micro-analysis with a cross-country study. Applying a DiD approach, we exploit the variation in prenatal ML duration within 17 OECD countries over time. In this analysis, we also find no evidence for a beneficial impact of a longer duration of prenatal ML. We conclude that existing ML legislations with long compulsory durations should be re-assessed and one may either reduce the extent of obligation or the duration.

I. Introduction

Developed countries have special regulations in place to address the safety and health of pregnant workers and their unborn children. An important element of these regulations is *maternity leave* (ML). This is the temporary employment-protected period of absence for women around the time of childbirth and should be distinguished from parental leave.[1] There is considerable variation in the ML arrangements across countries in terms of income support, obligation, and in pre- and postnatal duration.[2] In this paper, we are interested in the optimal *prenatal* ML duration. Thus, we are interested in the impact of maternal employment during pregnancy on maternal and child outcomes. Despite the popular belief that prenatal ML is beneficial to the infant and mother, empirical evidence on the optimal prenatal ML duration is extremely scarce and existing policies are not evidence-based.[3]

We evaluate a substantial ML extension in Austria. Until 1973 statutory ML prohibited employment in the period from six weeks before to (usually) six weeks after the delivery. A reform in the year 1974 increased mandatory pre- and postnatal ML to eight weeks, respectively. All other aspects of the ML regulations (such as the associated transfer payments) remained unaffected by the reform. Our estimation strategy exploits that the eligibility for the extended leave was determined by a cutoff due date. This gives rise to a fuzzy regression discontinuity design (RDD), which we translate into an instrumental variable (IV) approach. This provides us with a local average treatment effect (LATE) that identifies the causal effect of an extended *prenatal* ML duration due to being assigned to the new regulations. A nice feature of our research design is that assigned and not assigned mothers, while having different prenatal ML durations, were both entitled to the *same postnatal* ML duration. This allows us to cleanly identify the effect of variation in prenatal ML, not only on outcomes at birth, but also on post-birth outcomes. We study also the reform's effects on children's and mothers' long-run outcomes. To check the robustness of our results, we additionally use information on unaffected non-working mothers, who are not eligible for ML. This second source of exogenous variation extends our RDD with a difference-in-differences (DiD) approach. The DiD component differences out any potential seasonal effects. The combination of these two sources of exogenous variation ensures a clean identification of treatment effects.

For our analysis we can rely on high-quality administrative data sources covering the uni-

[1]The leave that often follows ML and that allows one or both parents to remain home to care for young children is usually called parental leave (see *OECD Family database*, "Child-related leave: PF2.1 Key characteristics of parental leave systems," updated: March, 2017). We follow this semantic convention throughout the paper.

[2]Currently, 32 states have ratified the *Maternity Protection Convention* issued by the *International Labour Organization* (ILO), which mandates among others at least 14 weeks of ML and an entitlement to cash and medical benefits.

[3]In contrast, the effect of maternal employment after childbirth and during first years of a child's life is extensively studied. In particular, there are a number of design-based papers on the effect of different postnatal maternity and parental leave durations on child outcomes available (Liu and Skans, 2010; Baker and Milligan, 2010; Rasmussen, 2010; Baker and Milligan, 2015; Dustmann and Schönberg, 2012; Carneiro et al., 2015; Dahl et al., 2016; Danzer et al., 2017).

verse of all births in Austria. The *Austrian Social Security Database* (ASSD) provides information on the mother's eligibility for ML, her actual leave duration, and her return to work behavior. The *Austrian Birth Register* comprises a number of outcomes to assess children's health at birth, and enables us to closely track subsequent maternal fertility. The ASSD further allows to assess maternal mortality and children's longterm human capital outcomes (up to 40 years of age). For a subset of observations, we have also data on post-birth health care utilization.

There are several potential mechanisms through which extended prenatal ML could improve the health of pregnant workers and their unborn children. The extended job-protection and the absence from work should reduce the mother's psychological (and in the case of manual jobs, also the physiological) stress level. Certain groups of workers could also benefit from the reduction in specific occupational exposures.[4] The fetal origins hypothesis and supporting empirical evidence highlights the importance of the prenatal environment on later child and adult outcomes (Almond and Currie, 2011*a*,*b*). Thus, for women whose counterfactual home environment is healthier than their job environment, an extended prenatal ML has the highest potential payoff.[5] In our research design we can abstract from self-selection into ML with respect to the relative quality of the work versus home environment, since ML is mandatory. The modified allocation of time (i.e., substituting work with leisure) may also lead to a healthier behavior: Expecting mothers may have more time to rest, to follow a healthy diet, or to do all necessary prenatal check-ups.

We find no evidence for significant effects of the prenatal ML extension by two weeks on children's health at birth. The estimated treatment effects are statistically insignificant and precisely estimated zero effects. This finding is consistent across selected sub-samples of mothers who are expected to be more vulnerable (such as blue-collar workers or older mothers). In line with this zero effect on children's outcomes in the short run, we also do not find any evidence for significant effects on long-run labor market outcomes. Treated and untreated children have statistically indistinguishable employment rates and earnings up to the age of 40. Our analysis of subsequent maternal fertility does not reveal any significant effects of the reform either. Treated and untreated mothers do not significantly differ in the timing of subsequent births or in their completed fertility. The same holds true for their 20 and 40 years survival rates. We conclude from our micro-data analysis that the reform had no measurable effects on children's or on mothers' outcomes. This suggests that six weeks of mandatory prenatal ML are sufficient. Our results have to be interpreted within the scope of the Austrian setting, where expecting mothers (before and after the reform) are always entitled to sick leave if supported by a medical

[4] Examples are second-hand tobacco smoke in the hospitality industry (Bharadwaj et al., 2014), chemicals in certain branches of manufacturing (Chen et al., 2000; Snijder et al., 2012), anaesthetic gases and antineoplastic drugs in the medical sector (Lawson et al., 2012), low levels of radiation in the aviation industry, or shift work (Bonzini et al., 2011) and noise.

[5] At the same time, it can not be ruled out that the counterfactual home environment is for some women less beneficial. In this case, an increase in prenatal ML may have negative effects.

certificate.

To provide some evidence for the external validity of our findings, we complement our micro-analysis with a cross-country study. Applying a DiD approach, we exploit the variation in prenatal ML duration within 17 OECD countries over time. Between 1970 and 2010, we observe 22 reforms of prenatal ML duration (14 increases and 8 decreases). We consider a potential effect of these reforms on different measures of infant mortality, the incidence of low birth weight, maternal mortality, and the fertility rate. The value added of this analysis is three-fold. First, we are able to provide evidence beyond the variation from six to eight weeks of leave duration. Our cross-country sample offers a range of prenatal ML durations with a minimum of zero weeks and a maximum of nine weeks. Second, we are able to analyze not only increases, but also decreases in prenatal ML duration. Third, our evidence is not solely based on data from Austria — a country with a generous welfare state and a long tradition of labor protection — in which the health of pregnant women is protected well beyond ML. In our cross-country analysis, we also find no evidence for a beneficial impact of a longer duration of prenatal ML.

Our findings add to the scarce stock of empirical evidence on this topic. So far, only a handful of design-based papers provide evidence on the effects of prenatal ML.[6] With regards to the U.S., there are two studies available. Rossin (2011) evaluates the effects of twelve weeks unpaid ML introduced by the *The Family Medical Leave Act* (FMLA) of 1993. This policy allowed mothers to take a leave during their pregnancy and/or after childbirth. The author's identification is based on variation in FMLA policies across states and variation in which firms are covered by FMLA provisions. She finds that unpaid ML led to small increases in birth weight, decreases in the likelihood of a premature birth, and substantial decreases in infant mortality. These effects are present only for children of highly educated and married mothers, who were most able to take advantage of unpaid leave. Stearns (2015) evaluates the effect of state-based access to paid ML on health at birth outcomes. She exploits the fact that five states were required to start providing wage replacement benefits to pregnant women in the year 1978 through their *Temporary Disability Insurance* (TDI) programs. Eligible women could access this *de facto* paid ML in the period immediately before or after birth. Based on state-level data she implements a difference-in-differences approach, which suggests that access to these six weeks of paid ML lowered rates of low birth weight and pre-term births by 5 and 8 percent, respectively. In contrast to Rossin (2011), the effects were driven by disadvantaged African-American and unmarried mothers. Finally, Wüst (2015) uses Danish data to study the effect of maternal employment during pregnancy on birth outcomes. She focuses on the pregnancy weeks 12 through 30. To account for selection into employment she exploits variation across pregnancies and compares outcomes of mothers' consecutive children.

The remainder of the paper is organized as follows: In Section II, we present the research design of our micro-analysis. We first provide details on the ML system, the reform in the year

[6]The evidence from observational studies on the effect of working conditions on pregnancy outcomes is summarized by two meta-analyses (Mozurkewich et al., 1999; Palmer et al., 2013).

1974, and other relevant aspects of the institutional setting. We also describe our data sources. In Section III, we present the estimation results along with a number of robustness checks. In Section IV, we present our complementary cross-country analysis. Section V concludes the paper and discusses potential policy implications.

II. Research design

II.1. Institutional background

In this section, we summarize the relevant institutional background and describe the ML system before and after the 1974 reform. To enhance the understanding of the context, we first provide information on female labor force participation rates. Then we discuss in detail the reform of the ML system. Finally, we describe changes in the public prenatal care program over time.

II.1.1. Female labor force participation

Throughout the 1970s, labor force participation rates remained quite constant. Among women between 15 and 60 years of age the rate was around 55 percent. The equivalent male rate amounted to roughly 85 percent. The highest female participation rate among all age groups in 1971 was 62.4 percent for those aged 20 to 29 (Butschek, 1974). Our estimation sample is dominated by this age-group, which accounts for about 66 percent of our sample. In comparison, the rate for women aged 30 to 39 was only 50.9 percent (Butschek, 1974). This significant reduction was due to women leaving the labor force when they married or had their first child.

II.1.2. Maternity leave system and its reform in 1974

In 1957, Austria introduced a legislation which mandated 12 weeks of paid job-protected ML. This prohibited pregnant women from working 6 weeks before and 6 weeks after birth. The beginning of the prenatal leave was determined based on the doctor's estimation of the date of delivery. The prenatal leave could be started earlier, if the mother's or the child's health was at risk due to the work environment. The latter had to be certified by either the chief medical officer of the *Regional Health Insurance Fund* or by an occupational physician of the *Labour Inspectorate*. Postnatal leave was regularly extended for all nursing mothers to 8 weeks and for nursing mothers with premature births to 12 weeks.[7]

The last major reform of the ML system took place in 1974, which extended the compulsory ML duration to 16 weeks. Since then, pregnant women are prohibited from working 8 weeks before the delivery and usually 8 weeks after the delivery. Eligibility for the extended ML was determined by the expected due date. Pregnant women with an expected due date until April 1974 were still covered by the old regime and assigned to 6 weeks of prenatal leave. Mothers who expected to give birth on June 1, 1974 were the first to be covered by the full

[7]Since 1962, *all* mothers experiencing a premature birth were eligible for 12 weeks postnatal leave.

implementation of the reform and were assigned to 8 weeks of prenatal leave. Mothers whose expected date was in May 1974 were phased stepwise into the program.

The upper Panel of Figure 1 depicts the relationship between assignment to the reform and the actual length of the prenatal ML. We use the actual birth date as a proxy for the expected due date, since we cannot observe the latter. The figure plots the average prenatal leave duration (measured in days) by birth date. Until the end of April, we observe a constant mean of about 6.3 weeks (or 44.2 days). Throughout May, we see a steady increase in the average prenatal leave duration, which reflects the stepwise increase as specified by the reform. Starting from June, when the reform starts to be in full effect, we observe an average prenatal leave duration of about 8.1 weeks (or 56.6 days). In our estimation analysis below, we will focus on children born in April and June, which represent the groups of 'not assigned' (N) and 'assigned' (A) mothers, respectively. We disregard mothers who gave birth in May. Thus, we focus on the jump in the average prenatal ML duration (between end of April to the beginning of June) from 6.3 to 8.1 weeks.

[Figure 1]

The lower Panel of Figure 1 focuses on the *postnatal* ML duration. The reform had been implemented such that all women who gave birth from April onward were assigned to the extended postnatal leave duration. We can see that average duration is constant at about 8.8 weeks (or 61.5 days) starting from April. Thus, assigned and not assigned mothers — while having differential average *prenatal* ML durations — do not differ in their *postnatal* ML durations. This feature of the reform allows us to cleanly identify the effect of variation in the prenatal ML duration also in the case of post-birth outcomes.[8] During ML, mothers receive a transfer payment that amounts to 100 percent of the average net earnings of the preceding 13 weeks (*Wochengeld*). Furthermore, they cannot be dismissed by their employer until 4 months after delivery. After ML most mothers were eligible for parental leave (PL) until the child's first birthday. The eligibility criteria for PL and the associated transfer payments did not differ for 'not assigned' (N) and 'assigned' (A) mothers.[9]

II.1.3. Public prenatal care

In the early 1970s, infant mortality was comparably high in Austria, amounting to about 25 deaths of infants under the age of 1 per 1,000 live births. This was slightly above the U.S. figures and well above those in Scandinavian countries (own calculations based on data from the World Bank). This is somewhat surprising, since Austria already had a Bismarckian welfare

[8] Figure A.1 in the Web Appendix plots the average prenatal and postnatal ML duration for a wider window, ranging from January 1973 to December 1975. It shows that both durations have been constant before and after the reform.

[9] During the PL period, eligible mothers received a monthly transfer of either 2,000 or 3,000 Austrian schillings (356.9 or 535.4 in 2018 euros), depending on whether they were married or not (Dirschmied, 2000).

system in place which provided almost universal access to high-quality healthcare.[10] In order to improve perinatal health outcomes, the *Austrian Federal Ministry of Health* launched the first nationwide prenatal screening program in 1974. This so-called *Mother-Child-Pass Examination Program* (MCPEP) initially advocated pregnant mothers to participate in four prenatal screenings (in pregnancy weeks $\leq$ 16, 19, 27 and 37) and one postnatal examination (in the first week after birth). Over time, the aim and scope of the MCPEP has expanded substantially (Halla et al., 2016). Before the introduction of the MCPEP, women could consult their gynaecologist for the same medical examinations. The essential feature of the MCPEP was the newly introduced financial incentive, along with an information campaign. Mothers received 8,000 Austrian schillings (1,427.7 in 2018 euros) if they participated in at least one prenatal and the one postnatal examination. All mothers in our estimation sample were already exposed to the MCPEP and its financial incentives were offered equally to assigned and not-assigned mothers.[11]

II.2. Data sources

We construct our main data set by combining three administrative data sources. The *Austrian Social Security Database* (ASSD) includes administrative records to verify pension claims and is structured as a matched employer–employee data set. For each individual we observe on a daily basis where she is employed, along with her occupation, experience, and tenure. Information on earnings is provided per year and per employer. The limitations of the data are top-coded wages and the lack of information on (contracted) working hours (Zweimüller et al., 2009). We draw information from the ASSD to measure eligibility, assignment and treatment status. We further obtain information to construct outcome variables in the domains of subsequent fertility, human capital outcomes, and mortality. Second, we use the available information on mothers labor market history to construct sample stratification variables. The *Austrian Birth Register* (ABR) includes all live births in Austria with individual-level information on birth characteristics such as date, place of birth, birth weight, and birth length. This information is complemented by maternal socioeconomic characteristics such as age, marital status, occupation, and religious denomination. Third, we use information provided in the database from the *Upper Austrian Sickness Fund* to construct longterm health outcomes for children. This database includes information on healthcare expenditure for all private employees and their dependents in

[10]Patients hold mandatory health insurance administered through nine *regional health insurance funds* ("Gebietskrankenkassen"), which cover private employees and their dependents, and 16 social security institutions that provide health insurance for specific occupational groups such as farmers, civil servants, and self-employed persons.

[11]The only difference which has to be noted is that assigned mothers were already in pregnancy week 19 at time of the introduction of the MCPEP. The first prenatal screening according to the MCPEP was already scheduled for week 16. Thus, it is possible that assigned mothers were more likely to participate in this first prenatal screening. There is no data on the actual participation rates in this prenatal screening available for this period.

Upper Austria starting in the year 1998.[12]

II.3. Estimation strategy

Our treatment variable is the actual prenatal ML duration in weeks M. Assignment into treatment, A, depends on the expected cutoff due date, which we proxy with the actual birth date. We consider all eligible women who gave birth in June 1974 as assigned, $A_i = 1$, and those who gave birth in April 1974 as not assigned, $A_i = 0$. Thus, our assignment variable potentially has some measurement error. However, this should be negligible, since the variable is binary and possible misspecifications are unlikely.[13] We disregard mothers who gave birth in May where the reform was phased in and cases of multiple births (51 children born to 28 mothers).

While we have seen before that the relationship between assignment and treatment is strong, it is not fully deterministic. Hence, we set up a *fuzzy* RDD, where assignment into treatment is used as an IV for the endogenous treatment variable. This design can be translated into a *two-stage least squares* (2SLS) setup with the following first stage estimation of the prenatal ML duration:

$$M_i = \alpha_0 + \alpha_1 A_i + \mathbf{x}_i \boldsymbol{\gamma}' + \eta_i, \tag{1}$$

where $\mathbf{x}$ is a vector of control variables comprising information on the mother's age, citizenship, religious denomination, place of residence, and the child's legitimacy status; and η is a stochastic error term. In the second stage, we then use the exogenous variation $\widehat{M}$ to explore its effect on the respective outcome variable Y:

$$Y_i = \beta_0 + \varphi_{\text{rdd}} \cdot \widehat{M}_i + \mathbf{x}_i \boldsymbol{\delta}' + \varepsilon_i. \tag{2}$$

II.3.1. Identifying assumptions

Three conditions need to hold for $\hat{\varphi}_{\text{rdd}}$ to be informative about the causal effect of two additional weeks of prenatal ML. First, assignment to the increased prenatal ML duration A must predict actual take-up M. Second, mothers must not precisely manipulate their child's expected date of

[12]It covers roughly one million members representing 75 percent of the population in Upper Austria (see also footnote 10).

[13]There are two potential mistakes we could make by using the actual birth date (instead of the expected due date) to generate our assignment variable. First, we could erroneously assume that a woman was assigned (since her actual birth date was June 1 or later), while she was in fact not assigned (when her expected due date was on April 30 or earlier). Second, we could erroneously assume that a women was not assigned (since her actual birth date was on April 30 or earlier), while she was in fact assigned (when her expected due date was on June 1 or later). The first scenario describes cases of extreme postterm births, with gestational lengths of at least 44 weeks. The second one describes cases of extreme preterm births, with gestational lengths of at most 34 weeks. Since 1984, the ABR provides information on gestational length; these data allows us to check the relative importance of these two scenarios and to assess the potential measurement in our assignment variable. Using the years 1984 through 1994, we find that only 1.7 percent of all births were such extreme preterm births, and 0.02 percent were such extreme postterm births.

birth around the eligibility cutoff. Third, assignment must not be correlated with any outcome-determining factor. The first condition is testable. We have already shown the distinctive jump in the takeup rate at the cutoff (see upper Panel of Figure 1). This condition also holds in our regression framework, where we obtain an $\hat{\alpha}_1$ of 1.587, implying that assignment increases the average prenatal ML duration by 1.6 weeks or 12 days. The estimated coefficient is highly statistically significant with an F-statistic of about 757. This coefficient is stable across subsamples (see Table A.3 in the Web Appendix).[14]

[Figure 2]

The inability to precisely manipulate assignment into treatment is the key identifying assumption behind any RDD. Public discussion about the potential reform of the ML system started in December 1973. The earliest media coverage we found is a newspaper article published on December 13, 1973. This reports that the Socialist-led government plans to extend maternity leave without providing any details.[15] The bill was submitted on February 5. The legislative proposal underwent a preliminary deliberation by the *Committee on Social Affairs* of the *National Council* on February 22, 1974. The bill was then passed by the National Council on March 6, 1974 and approved by the Federal Council on March 14, 1974. It became effective on April 1, 1974.[16] This timing rules out that parents adjusted their conception behavior. This is confirmed by Figure 2, which shows that the average number of births per day does not vary around the cutoff date. More formally, the density-based manipulation test suggested by McCrary (2008) confirms this. We cannot reject the hypothesis that there is a shift in the discontinuity at the birthday cutoff: test statistic = 0.023, standard error = 0.023 (bin size = 0.68, default bandwidth calculation, bandwidth = 104.08). Thus, there is no evidence of manipulations of the birth date.

[Figure 3]

Whether assignment is correlated with any outcome-determining factor is not fully testable; however, it is reassuring that none of our covariates changes discontinuously around the cutoff. Figure 3 plots the daily averages of all covariates and other pre-determined variables between January and December 1974.

Non-working mothers, an additional control group To check the robustness of our results, we use information on unaffected non-working mothers. While these mothers clearly differ (in their

[14]The largest difference is observed between very young (< 21) and older mothers (≥ 29), for whom we obtain coefficients of 1.337 and 1.715, respectively.

[15]We have scanned four major newspapers (*Neue Kronen Zeitung*, *Die Presse*, *Salzburger Nachrichten*, *Oberösterreichische Nachrichten* in the period from November 1973 through March 1974 for all articles relating maternity leave. We found a total of five articles.

[16]The signed law was published in the Federal Law Gazette on March 29, 1974 (see *Bundesgesetzblatt* 59/1974).

observable characteristics) from working mothers, they are useful since they were never eligible for ML.[17] The reform had by definition no impact on non-working mothers and they could serve as an additional control group. This second source of exogenous variation can either be used to complement or substitute our RDD approach.

In the case where we use non-working mothers to extend our RDD approach we gain a difference-in-differences (DiD) component, which differences out any potential seasonal effects. To translate this combined approach into a regression framework, we extend our first stage estimation with a binary variable, W, capturing the mothers employment status at the time of birth, and an interaction between this variable and the assignment variable A:

$$M_i = \theta_0 + \theta_1 A_i + \theta_2 W_i + \theta_3 (A_i \times W_i) + \mathbf{x}_i \boldsymbol{\zeta}' + u_i, \tag{3}$$

where the latter is again equal to one for all women who gave birth in June, irrespective of their employment status, and zero otherwise. For non-working mothers, the ML reform did not affect allocation of time. We impute $M_i = 40$ if i was not working at time of birth; the specific value chosen has no impact on the estimation results. Instead of using the assignment variable A as an exclusion restriction, in this approach we use the interaction term $A \times W$ to identify the effect of the ML extension on the respective outcome Y in the second stage estimation:

$$Y_i = \rho_0 + \varphi_{\text{rdd-did}} \cdot \widehat{M}_i + \rho_1 A_i + \rho_2 W_i + \mathbf{x}_i \boldsymbol{\iota}' + v_i, \tag{4}$$

where our alternative treatment effect of interest is $\hat{\varphi}_{\text{rdd-did}}$. In the case of using this approach to substituting our RDD analysis, we identify the effects of the reform solely based on the DiD component (thus, we do no exploit the RDD in a 2SLS setup). Now the identification strategy is identical to a simple DiD approach,

$$Y_i = \lambda_0 + \lambda_1 A_i + \lambda_2 W_i + \varphi_{\text{did}} \cdot (A_i \times W_i) + \mathbf{x}_i \boldsymbol{\omega}' + w_i, \tag{5}$$

where the treatment effect of interest is equal to $\hat{\varphi}_{\text{did}}$. The identifying assumption is that the trends in the outcome variables would have been the same for these two groups of mothers (working and non-working) in the absence of the reform.

II.3.2. Outcome variables

We consider various short- and longterm outcomes for both the child and the mother. Primarily we are interested in health at birth outcomes of the child, and we augment these with subsequent maternal fertility and health outcomes of the mother, as well as longterm labor market and health outcomes of the child. In Table 1, we provide descriptive statistics for our main outcome

[17] These mothers were on average 2.8 years older at the time of birth, more likely married and Catholic. Table A.1 in the Web Appendix provides descriptive statistics for working and non-working mothers, who gave birth in April or June 1974.

variables and covariates, separately for assigned and non-assigned mothers.

[Table 1]

Health at birth To construct health at birth outcomes we use information on birth weight and length. We consider both as continuous variables measured in logs. Additionally we construct a binary variable indicating a low birth weight. This is equal to one if birth weight is lower than 2,500 grams, and zero else. Average birth weight in the sample is 3,256 grams, and roughly 6% of all children had a low birth weight. We also consider another binary variable indicating whether growth of the child is symmetrically restricted. This measures combines the birth weight and the child's *Ponderal* index ($PI = kg/m^3$) and indicates whether a child has developed slowly from an early stage of gestation. We define growth as symmetrically restricted if birth weight is lower than 2,500 grams and the Ponderal index is in the lowest quartile of its distribution. In our sample, about 4% of children have a symmetrical growth restriction.

Finally, we generate a proxy for premature births. This information had not been recorded in the Austrian Birth Register until 1983, but we can infer it from the mother's postnatal ML duration. In 1974, postnatal leaves were stipulated to last a maximum of 12 weeks, unless the mother experienced a preterm birth (or a fetal death). Accordingly, we assume a preterm birth if a mother took 12 weeks or more of postnatal ML. Based on this proxy, we find 6% of all births to be premature in our data.

Maternal outcomes For mothers, our outcome variables include measures of subsequent fertility and health. In the former domain, we consider a potential effect of the reform on the tempo and quantum of fertility. To capture the tempo, we consider the time until the mother's next birth in logs. About half of the mothers had at least one further birth. The average duration until the next birth was 4.36 years. We employ two measures for completed fertility. First, we use a binary variable indicating whether the mother gave birth at least once more, and zero else; as well as the number of subsequent births. To capture any effects on health, we study mortality. We construct two binary variables indicating whether the mother survived at least 20 and 40 years after giving birth, and zero else. The average survival rates at these two points in time are 99% and 92%, respectively.

Children's longterm outcomes Finally, we consider children's longterm labor market and health outcomes. Labor market outcomes are available for about one-third of all children.[18] For these children we analyze employment, occupation, and wages at age 40. Using the database

[18]The remaining two-thirds of children cannot be uniquely tracked over this long period of time. This part of the administrative data was not comprehensively available for early cohorts. Fortunately, the availability of information on labor market outcomes seems to be idiosyncratic. It is not correlated with our IV, and should therefore not bias our results: The share of children we have information on labor market outcomes is similar for assigned (32.7%) and non-assigned (32.4%) mothers.

from the *Upper Austrian Sickness Fund*, we are able to construct longterm health outcomes for all children employed in the private sector in Upper Austria in the period between 1999 and 2014. Over this period (during which children were between 25 and 40 years of age) we aggregate health care spending in the outpatient sector, and the days spent in hospital for the 511 children remaining in our sample. Descriptive statistics for all children's longterm outcomes are provided in Table A.4 in the Web Appendix. Across outcomes, we do not observe any statistically significant differences in the sample means between the groups of assigned and not-assigned mothers.

III. Estimation results

III.1. Health at birth outcomes

We present our main estimation results in Table 2. In Panel A, we summarize our RDD estimates $\hat{\varphi}_{\text{rdd}}$, which exploit the eligibility cutoff to estimate LATEs of the 1974 reform. Each coefficient in Panel A is obtained by estimating the fuzzy RDD outlined in section II.3 via 2SLS. Corresponding first stage estimates are summarized in Table A.3 in the Web Appendix. As outcome variables we consider the birth weight in logs, a low birth weight indicator, the symmetric growth restriction, birth length in logs, and an indicator for a premature birth in columns (1) to (5). Across outcomes we find no significant causal effects of the reform. All point estimates are very close to zero and precisely estimated. The only exception is the coefficient on birth weight, which is marginally significant. An increase in ML duration by two weeks is estimated to reduce birth weight by 1%, which amounts to a decrease from 329 to 326 grams at the sample mean. This is a very small effect in economic terms, which turns statistically insignificant in alternative specifications that are discussed below.

[Table 2]

For comparison we provide naïve OLS estimates in Panel B of Table 2, where we assume the duration of prenatal ML duration be exogenous. Here we find statistically significant effects on all outcome variables. The robust finding is that longer prenatal leave is positively correlated with better health at birth.[19] It suggests that it is important to account for endogeneity. Ignoring the positive selection into longer prenatal ML would lead to the erroneous conclusion that longer leave durations causally improve health at birth. In contrast, our RDD estimates show that an increase form six to eight weeks has, *on average,* no significant effect on health at birth.

III.1.1. Interpretation of estimation results

Clearly, these results have to be interpreted in regards to the Austrian institutional setting. Expecting mothers — before and after the reform — have always been entitled to sick leave if

[19] This can partly be explained by a longer gestational length in these cases

supported by a medical certificate. They can even start ML early whenever the mother's or the child's health is at risk due to work. The latter has to be certified by either the chief medical officer of the *Regional Health Insurance Fund* or by a occupational physician of the *Labour Inspectorate*. Thus, we can expect that untreated mothers were also not working until the sixth week before the expected due date if they had health problems in pregnancy. This means that our LATE estimates are most likely driven by counterfactual comparisons of mothers without problems in pregnancy, who spend six versus eight weeks on prenatal ML. In a next step, we explore potential treatment effect heterogeneity.

III.1.2. Treatment effect heterogeneity

We now stratify our sample according to different mother characteristics and repeat our estimation analysis for each subsample. We distinguish mothers by occupational collar, age, and labor income. The occupational collar (blue versus white collar) is highly correlated with the job task (manual labor versus office work). One might expect women performing manual labor to benefit more from an increase in prenatal leave duration. The stratification by age (less than 21 years, between 21 and 28, and over 28 years of age) is not only interesting *per se*, but also allows us to infer on parity to some extent.[20] In the subsample of the youngest mothers, the vast majority of cases are presumably first births. A sample split by earnings (below versus above the sample median) considers more general differences between socioeconomic backgrounds.

[Figure 4]

Figure 4 graphically summarizes our RDD estimates for these seven subsamples, along with our baseline estimates.[21] We focus here on three outcome variables (birth weight, length, and premature birth). The general finding is a zero-effect in each strata. The same holds for the other outcome variables (not shown). The estimated treatment effects are (with one exception) all statistical insignificant. The size of the 95% confidence intervals varies somewhat and is, as expected, larger for smaller subsamples (e. g., younger and older mothers). We conclude that the reform had no beneficial effects; not even for children born to more vulnerable women, or to those exposed to more exhausting working conditions.

III.1.3. Robustness checks using non-working mothers as a control group

To check robustness of our results on health at birth, we augment our fuzzy RDD model from above with a DiD component. As in equation (4), we introduce non-working mothers who were unaffected by the reform as a control group.[22] This essentially allows us to subtract the estimated

[20] Information on parity is not available for the early birth cohorts we consider.

[21] Corresponding first stage estimates are summarized in Table A.3 in the Web Appendix.

[22] Note that we cannot perform this analysis on the premature birth outcome, since this variable can only be constructed for working mothers.

RDD effect for non-working mothers from the equivalent effect for working mothers, which will difference-out any seasonal effects. In Table A.1 (Web Appendix), we provide descriptive statistics separately for working and non-working mothers; the means of our observables are very similar across the two subsamples. Panel A of Table 3 summarizes estimation results from this alternative specification. The estimated effects are highly similar to those obtained by our RDD estimations above (see Table 2). The only notable difference is that the effect on birth weight, which was marginally significant before, is now insignificant as well. Thus, all point estimates are precisely estimated and statistically insignificant.[23]

[Table 3]

Panel B of Table 3 presents estimation results from a simple DiD specification as in equation (5), where we compare pre- and post-reform effects of working and non-working mothers, but do not exploit the discontinuity. This specification shows also economical and statistical zero-effects across all outcomes.[24] In Figure 5, we compare all three different estimates graphically.

[Figure 5]

III.2. Maternal outcomes

So far, we have provided evidence that the ML extension had no significant effects on children's health at the time of birth. In a next step, we consider effects on mothers. It is possible that the reform, while having no discernible effects on children, has improved the physiological or psychological well-being of mothers. While we do not have comparable health indicators for mothers at time of birth, we examine two other informative outcome dimensions. First, we consider mothers' subsequent fertility behavior. If the extended leave has improved physiological or psychological well-being of mothers, we could see an increased quantum or tempo of fertility. Accordingly, we estimate the effect of the reform on completed fertility, and — conditional on having another birth — on the duration to the next birth. Second, we examine mothers' longterm health and study their mortality. In particular, we consider mothers' survival 20 and 40 years after birth.

[Table 4]

Panel A of Table 4 summarizes our RDD estimates for these maternal outcomes. Panel B provides equivalent OLS estimates for comparison. In columns (1) to (3), we focus on fertility

[23] Here the estimated first-stage coefficient from equation (3), $\hat{\theta}_3$, is -1.584 (0.058). The Kleinbergen-Paap *rk* Wald F-statistic is 755.47, and the partial r^2 is 0.029.

[24] Note, the DiD estimator captures the average treatment effect of the reform, which extended compulsory prenatal ML duration by two weeks. In contrast, the LATE estimates capture the effect of one additional week prenatal ML due to assignment. To ensure arithmetic comparability of these two estimates, the LATE estimate has to be multiplied by a factor of two.

behavior. About half of *all* mothers have at least one further birth, with an average duration to next birth of about 4.36 years. The average total number of subsequent births is about 0.7. Across columns and estimation methods, we do not find evidence for any significant effects of the reform. The point estimates are neither statistically nor economically significant. In columns (4) to (5), we examine mortality. Exactly 20 and 40 years after giving birth, about 99% and 92% of all mothers are still alive, respectively. For the former outcome, we find a clear zero-effect, while for the latter we obtain a marginally significant negative effect. This suggests that the reform has decreased the probability of being alive after 40 years by 1.4 (= $0.7 \cdot 2$ weeks) percentage points. This amounts to a reduction of roughly 1.5 percent in terms of the sample mean. Given that any harmful effect of the reform for mothers' health is hard to rationalize; it should be emphasized that the estimated effect is only significant at the 10 percent level.

[Figure 6]

In Figure 6, we additionally check whether certain socioeconomic groups respond differently to the reform. Again we stratify mothers by their occupational collar, age, and income. With one exception, most of these subgroups resemble the baseline, with coefficients being close to zero and insignificant. In terms of fertility, however, we find that low income mothers experience an increase in the number of further children by roughly 0.1 due to the reform.

III.3. Children's longterm outcomes

In a final step of our micro analysis, we examine children's longterm outcomes. Table 5 summarizes our corresponding RDD estimates. In columns (1) to (3), we consider labor market outcomes at the age of 40. At this point in time, about 83 percent were in a regular employment, and among these about 70 percent were employed as a white collar worker. In the remaining columns, we consider two health outcomes. In particular, we consider aggregate spending in the outpatient sector (column 4), and the aggregate days spent in hospital (column 5). Both variables refer to the period during which children were between 25 and 40 years of age. We observe an average spending in the outpatient sector of about € 1,830 and an average of 9.2 days spent in hospital. Again, across outcomes we do not observe any economically or statistically significant effects of the reform.

[Table 5]

IV. Complementary cross-country evidence

So far, we have shown that — in Austria — the increase in prenatal ML duration from 6 to 8 weeks had no discernible impact on children and mothers, neither at birth nor later on. We now complement these micro-data estimates with a cross-country analysis. This helps us to overcome two main obstacles to the external validity of our findings. First, Austria has a comprehensive social insurance system with very good health care and extensive employment protection. In case of health problems during pregnancy, expecting women may always take sick leave or even start ML early. In addition to ML, pregnant women have enjoyed multiple other protection measures since 1957, such as the ban of job tasks that involve long standing, heavy lifting, or piecework with high working speed. Consequently, we may not find any effects of extended leave in light of this specific institutional setting. Another caveat of our micro analysis is that we are only able to compare effects of 8 relative to 6 weeks of prenatal leave. Thus, we cannot generalize our findings to countries with other initial prenatal leave durations, or to ones without ML institutions at all. In our cross-country analysis, we therefore consider ML reforms across different countries and estimate their effects on average child health, maternal mortality rates, and fertility in a DiD setting.

IV.1. Data

We compile country-level data from the *Organisation for Economic Cooperation and Development* (OECD) Family Database (OECD, 2016) and (Gauthier, 2011). In total, we consider 17 countries that experienced a change in prenatal ML duration in the period between 1970 and 2010. ML regulations are quite heterogenous across countries. Systems differ with respect to whether the leave is mandatory or optional, whether the leave is paid or just job protected, and — in cases of paid leave — with respect to the amount of the remuneration. In Figure 7, we plot the evolution of prenatal leave durations for each country separately over time. The notes to Figure 7 provide details on each reform. We observe both extensions and reductions in leave duration with a total of 22 reforms. In our sample, we have plenty of variation in leave durations ranging between 0 and 8.7 weeks, with an average of about 5 weeks.

[Figure 7]

As outcome measures we use perinatal and neonatal mortality, the share of children with low birth weight (i. e., below 2,500 grams), and maternal mortality. We also check for any effects of reforms on the level of fertility and consider the total fertility rate as an additional dependent variable.[25] Information on exact definitions of these variables are provided along with summary statistics in the upper Panel of Table 6. We have collected a large array of demographic and economic control variables. These comprise information on the total population, its age distribution, mean age at childbirth, marriage rate, GDP per capita, female and male labor force participation, share of employment across sectors, average education, share of labor compensation in GDP, and the average hours worked (see lower Panel of Table 6).

[Table 6]

[25] We abstain from analyzing infant mortality rate, since this outcome is heavily influenced by the postnatal leave duration.

IV.2. DiD framework and estimation results

We use these data to set up a flexible DiD framework, which translates into the following panel fixed effects model:

$$Y_{jt} = \sum_{r} \varphi_{\text{cc},r} \cdot R_{jt,r} + \sum_{j} \Xi_j \cdot C_j + \sum_{t} \Delta_t \cdot T_t + \sum_{j} \sum_{p} \Pi_{jp} \cdot \left(\mathbf{1}\{C = j\} \times \tau_j^p\right) + \mathbf{x}_{jt}\boldsymbol{\eta}' + \mu_{jt}, \quad (6)$$

where Y is the outcome of interest for country j in year $t = 1970, \ldots, 2010$. The effect of prenatal ML duration reforms is captured by R. This denotes a series of binary variables equal to one if a country has changed prenatal ML duration r years ago. Below we will vary the definition of R, and, for instance, distinguish between reductions and extensions in ML durations. Furthermore, we control for fixed effects at the country and year level (Ξ_j and Δ_t), as well as for country-specific time trends of polynomial p, Π_{jp}. Finally, $\mathbf{x}_{jt}$ is a vector of control variables and the stochastic error term is denoted by μ_{jt}.[26]

[Figure 8]

Our parameter of main interest is $\varphi_{\text{cc},r}$, which is identified by variations in prenatal ML durations due to the 22 reforms. The advantage of this flexibel DiD specification is that it does not impose any functional form assumption on the effects of the reforms, and traces out the full adjustment path of the respective outcome. In particular, we include lags up to 18 years following the reforms. Crucial for identification of the DiD model is that the average change in Y in the comparison group represents the counterfactual change in the treatment group in the absence of the reform. While this so-called *parallel-trend assumption* is untestable, it is instructive to examine pre-reform years. Therefore, we extend our specification above and include leads up to 9 years before the reform. Figure 8 plots the estimated coefficients on $\varphi_{\text{cc},r}$, where we distinguish between reforms that extended prenatal ML durations (left side), and those that reduced prevailing durations (right side). In this specification, we allow for state-specific cubic time trends (i. e., we set $p = 3$ in model 6). For none of our outcomes we find differences in the pre-treatment trends before reforms ($-9 < r < 0$). This applies to extensions as well as reduction. Thus, we feel confident in imposing the parallel trend assumption.

Figure 8 also provides the estimated effects of the reforms ($0 < r \leq 18$). Across outcomes, we find almost no significant effects. Perinatal and neonatal mortality, maternal mortality, and the total fertility rate all remain constant after changes in stipulated leave durations. This applies to reductions and to extensions of leave durations. The estimated point coefficients are all close to zero and statistically insignificant. The estimated 95 percent confidence intervals are quite narrow in the first couple of years after the reform and widen a bit thereafter. The only outcome for which we find some significant effects is low birth weight. We find that the share of children

[26]We focus on unweighted estimation results. Population-weighted estimations lead to the same conclusions. Detailed estimation output is available upon request.

born with low birth weight decreases after leave expansions. However, these estimates must be interpreted with some caution. First, the effect is only marginally significant. Second, we observe already some downward trend in the pre-reform period for this outcome. The latter fact casts some doubt on the validity of the parallel trend assumption for this particular outcome. In sum, these results corroborate the findings from our micro-data analysis.

IV.2.1. Alternative specifications

In Table 7, we explore further specifications across panels. In a first step, we discuss the estimation results for the health related outcomes (see columns 1 to 4). In Panel A, we present results from a simple fixed effects model, where the treatment parameter is captured with a linear specification of the prevailing prenatal ML duration measured in weeks. Across outcomes we do not find any significant effects. In Panel B, we compare ML regimes with either below or above six weeks of prenatal ML to ones with exactly six weeks (base group). The latter resembles the reform situation in our microanalysis. Again, we do not find any significant differences in child or mother health outcomes across the different leave duration. In Panel C, we account for the fact that ML legislation is only relevant for working mothers. We suggest a specification, where we interact the prevailing prenatal ML duration measured in weeks with a binary variable equal to one in the case of a low and high female labor force participation rate, respectively. Again, we do not find any robust evidence for an impact on health outcomes.[27] In Panels D and E, we apply an equivalent specification for mean age at birth and GDP per capita, respectively. Thus, we allow for a situation where ML duration may matter more for older women or in economically weaker countries. In neither case, we find any robust evidence for a significant effect of prenatal ML duration.

[Table 7]

The only outcome variable for which we observe some significant effects, is the total fertility rate (see column 6). The specifications summarized in Panel B and D show statistically significant effects. However, both are quantitatively of minor importance and hard to rationalize.[28]

[27]In countries with a female LFP above the sample mean, effects are statistically zero throughout. For countries with a female LFP below the sample mean, we obtain one marginally significant coefficient. This suggests that an additional week of prenatal leave, increases neonatal mortality by 0.142 children per 1,000 live births or 0.02 standard deviations. Thus, also the economic relevance of these estimates is negligible, and we suggest not to overinterpret this single estimate.

[28]In Panel B, we estimate a 0.086 reduction in children per woman for prenatal ML durations above 6 weeks, and a 0.115 reduction for durations below 6 weeks. This suggests that fertility is highest if the prenatal ML duration is *exactly* six weeks. In Panel D, we find that in country-years with a low female labor force participation an increase in prenatal ML decreases the TFR by 0.018 children or 0.04 standard deviations.

V. Conclusions

We have analyzed the effect of different prenatal maternity leave durations on children's health at birth, subsequent maternal health and fertility, and longterm human capital outcomes of children. Our main analysis exploits an Austrian reform in the year 1974, which increased the compulsory prenatal maternity leave duration from six to eight weeks. Extended leave was determined by a cutoff due date, which allows us to implement a regression discontinuity design. We find no evidence for a significant effect of this prenatal maternity leave extension. The estimated treatment effects are statistically insignificant and precisely estimated zero effects. This finding is consistent across sub-samples of mothers. Our findings have to be interpreted in regards to the prevailing Bismarckian healthcare system, which provided sickness benefits before and after the reform. Thus, our estimated treatments are most likely driven by counterfactual comparisons of mothers without major problems in pregnancy, who spend six versus eight weeks on prenatal leave. A supplementary cross-country panel analysis, drawing on data from 17 OECD countries, with varying institutional settings confirms the results from our main analysis. We suggest to re-assess maternity leave legislations with long compulsory durations and to either reduce the extent of obligation or the duration.

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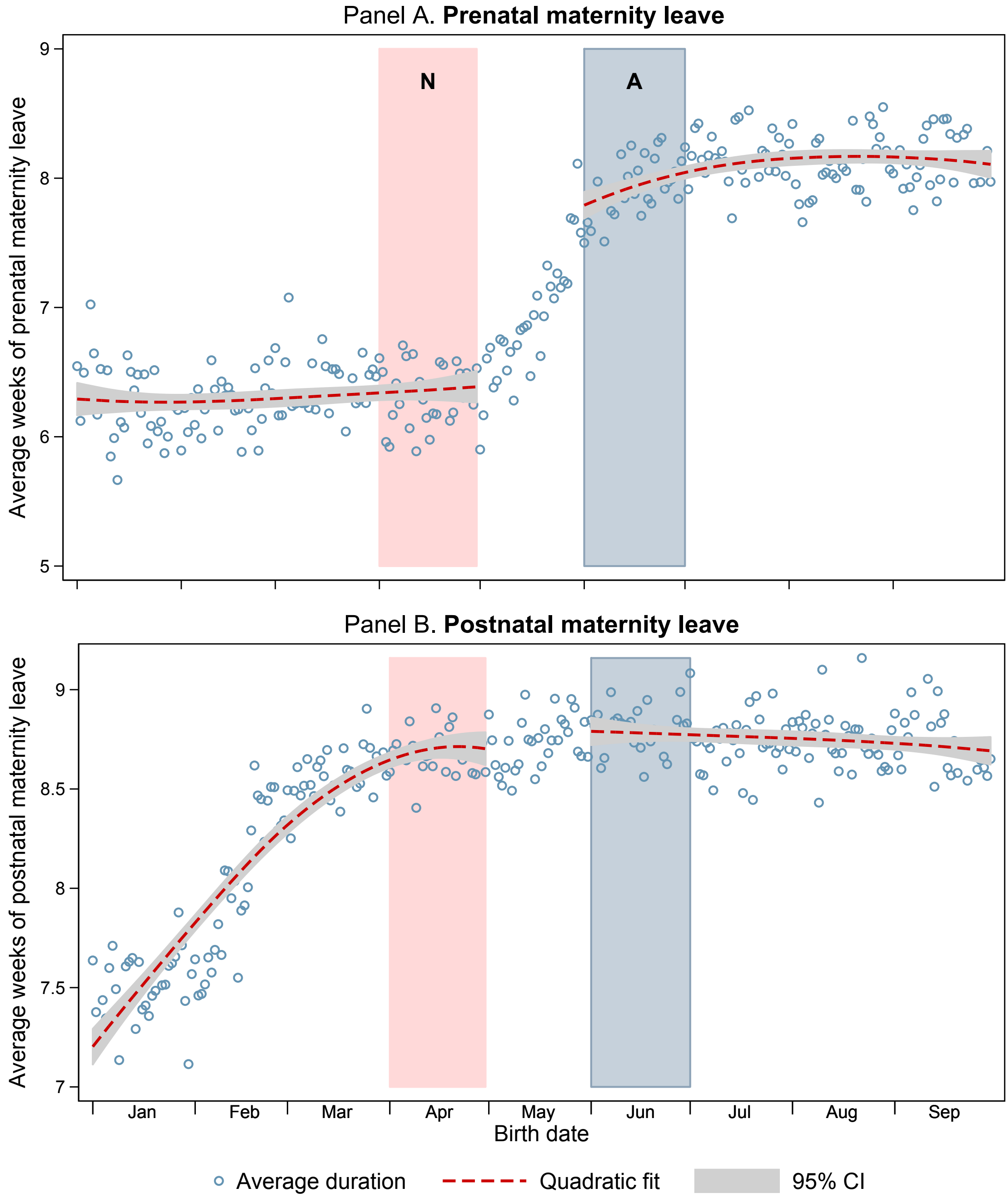
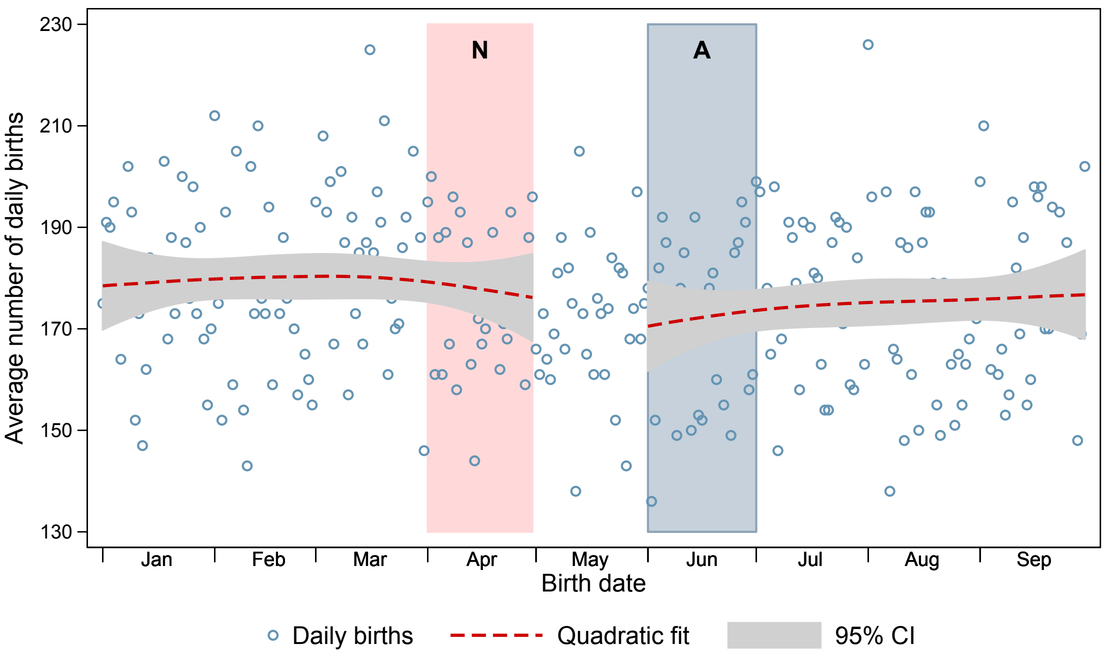
VI. Figures and tables (to be placed in the paper)

Figure 1 — Average pre- and postnatal ML durations by birth date of the child.



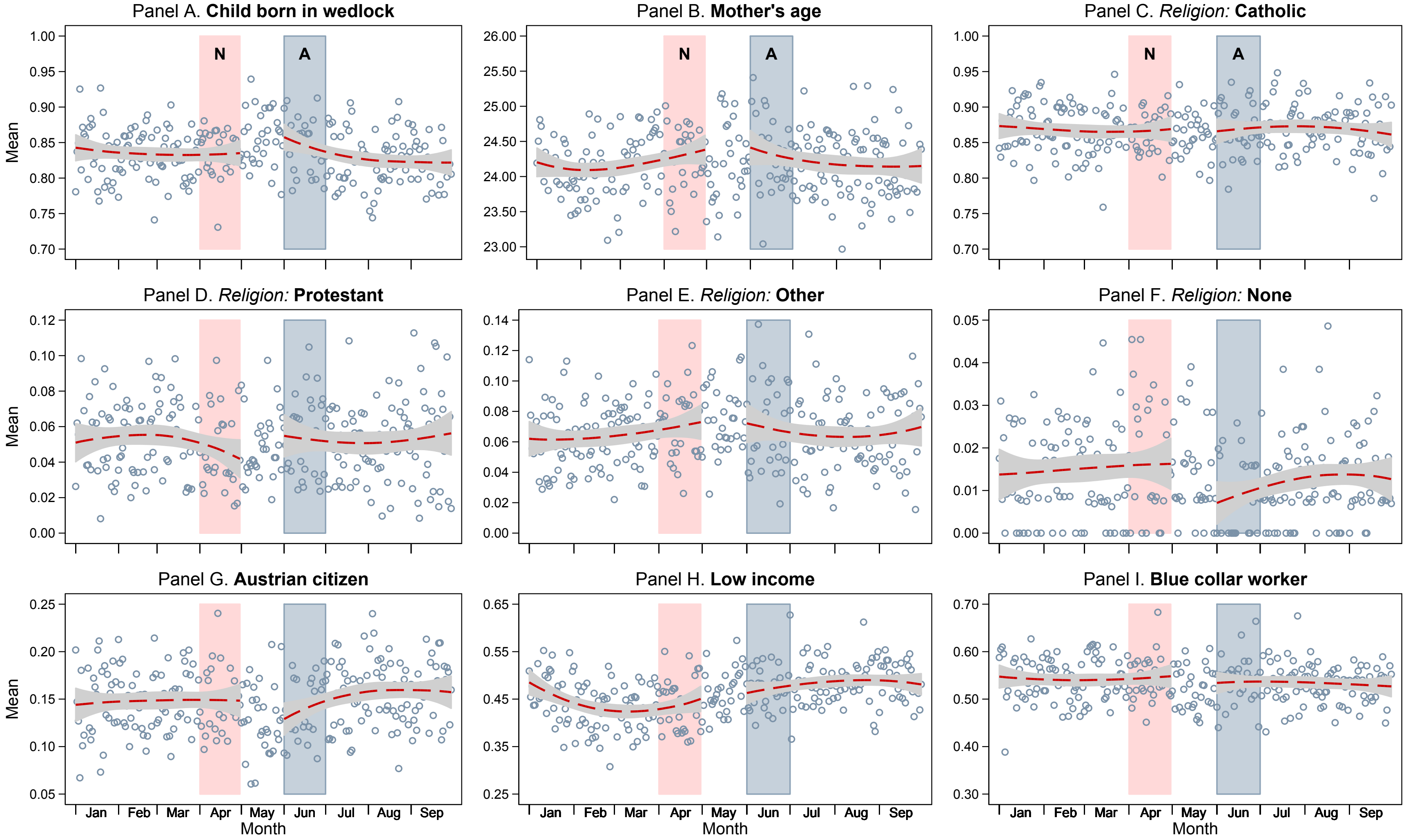
Notes: These graphs depict the average prenatal (Panel A) and postnatal (Panel B) ML durations by birth date of the child between January and September 1974. Separate quadratic fits for the pre-treatment (until April 30, 1974) and the post-treatment period (starting with June 1, 1974) are depicted by the scattered line. The red-shaded area highlights the subset of assigned births ('A'), which we use in our estimation analysis. These mothers were eligible for 8 weeks of prenatal ML duration. The framed blue-shaded area highlights the subset of not assigned births ('N'), which we use in our estimation analysis. These mothers were eligible for 6 weeks of prenatal ML duration. Both groups of mothers were eligible for 8 weeks of postnatal ML duration. Mothers who gave birth in May (during which the reform was phased-in) are excluded form our estimation analysis.

FIGURE 2 — Density of assignment variable (number of daily births).



Notes: This figure depicts the average number of daily births between January and September 1974. Separate quadratic fits for the pre-treatment (until April 30, 1974) and the post-treatment period (starting with June 1, 1974) are depicted by the scattered line. The red-shaded area highlights the subset of assigned births ('A'), which we use in our estimation analysis. These mothers were eligible for 8 weeks of prenatal ML duration. The framed blue-shaded area highlights the subset of not assigned births ('N'), which we use in our estimation analysis. These mothers were eligible for 6 weeks of prenatal ML duration. Mothers who gave birth in May (during which the reform was phased-in) are excluded form our estimation analysis.

FIGURE 3 — Daily averages of covariates and sample stratification variables.



Notes: In these graphs we plot daily averages for several covariates and sample stratification variables in our data. Separate quadratic fits for the pre-treatment (until April 30, 1974) and the post-treatment period (starting with June 1, 1974) are depicted by the scattered line. The red-shaded area highlights the subset of assigned births ('A'), which we use in our estimation analysis. These mothers were eligible for 8 weeks of prenatal ML duration. The framed blue-shaded area highlights the subset of not assigned births ('N'), which we use in our estimation analysis. These mothers were eligible for 6 weeks of prenatal ML duration. Mothers who gave birth in May (during which the reform was phased-in) are excluded form our estimation analysis. We observe no indications of significant discontinuities at the cutoffs in May 1974.

Table 1 — Descriptive statistics, assigned vs. non-assigned mothers.

	Assigned mothers (A)					**Non-assigned mothers** (N)				
	N	Mean	Std. dev.	Min.	Max.	*N*	Mean	Std. dev.	Min.	Max.
Prenatal maternity leave (in weeks)	3629	7.92	2.43	0.3	33.3	3721	6.33	2.53	0.3	31.6
Health at birth outcomes										
Birth weight (in grams)	3629	3246.29	532.96	500.0	5300.0	3721	3267.40	524.34	400.0	5200.0
Birth weight is below 2,500 grams	3629	0.06		0.0	1.0	3721	0.06		0.0	1.0
Symmetric growth restriction[a]	3629	0.04		0.0	1.0	3721	0.04		0.0	1.0
Length (in cm)	3629	50.41	2.82	29.0	61.0	3721	50.44	2.71	27.0	59.0
Premature birth[b]	3629	0.07		0.0	1.0	3721	0.06		0.0	1.0
Maternal outcomes										
Number of next births	3629	0.71	0.90	0.0	8.0	3721	0.69	0.87	0.0	7.0
Probability of having another child	3629	0.50		0.0	1.0	3721	0.49		0.0	1.0
Time to next birth (in years)	1803	4.34	3.43	0.8	22.8	1816	4.37	3.42	0.5	23.5
20 year survival probability	3629	0.99		0.0	1.0	3721	0.99		0.0	1.0
40 year survival probability	3629	0.92		0.0	1.0	3721	0.93		0.0	1.0
Sample stratification variables										
Blue collar worker	3604	0.54		0.0	1.0	3681	0.54		0.0	1.0
Below median income in 1973	3340	0.48		0.0	1.0	3422	0.44		0.0	1.0
Covariates										
Age at birth	3629	24.31	5.18	15.0	47.0	3721	24.27	5.21	15.0	45.0
Child born in wedlock	3629	0.85		0.0	1.0	3721	0.84		0.0	1.0
Religion										
Catholic	3629	0.87		0.0	1.0	3721	0.87		0.0	1.0
Protestant	3629	0.05		0.0	1.0	3721	0.05		0.0	1.0
Other religion	3629	0.07		0.0	1.0	3721	0.07		0.0	1.0
No religion	3629	0.01		0.0	1.0	3721	0.02		0.0	1.0
Mother is Austrian citizen	3629	0.14		0.0	1.0	3721	0.15		0.0	1.0

Notes: This table presents summary statistics for our treatment (ML duration); as well as our outcome, sample stratification, and control variables. Statistics are provided separately for both assigned mothers (i.e., mothers giving birth in June 1974) and non-assigned mothers (giving birth in April 1974). The population includes only mothers who had been working at time of birth.

[a] Symmetric growth restriction is defined as having low birth weight *and* a low Ponderal index ($PI = kg/m^3$).

[b] This variable is only available for working mothers. It is calculated using the postnatal ML duration.

Table 2 — Estimated treatment effects on health at birth outcomes

	(1) Birth weight	(2) Low birth weight	(3) Symmetric growth restr.	(4) Length	(5) Premature birth†
Panel A. RDD					
Prenatal maternity leave	−0.005*	0.001	0.000	−0.001	0.005
	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)
No. of observations	7,350	7,350	7,350	7,350	7,350
Mean of outcome	5.77	0.06	0.04	3.92	0.06
Std. dev. of outcome	0.19			0.06	
Kleinbergen-Paap rK Wald F-statistic	756.93	756.93	756.93	756.93	756.93
Panel B. OLS					
Prenatal maternity leave	0.009***	−0.010***	−0.006***	0.002***	−0.020***
	(0.001)	(0.002)	(0.001)	(0.000)	(0.002)
No. of observations	7,350	7,350	7,350	7,350	7,350
Mean of outcome	5.77	0.06	0.04	3.92	0.06
Std. dev. of outcome	0.19			0.06	

Notes: This table summarizes estimated effects of extending compulsory prenatal ML duration on health at birth. Panel A summarizes fuzzy RDD estimates (obtained via 2SLS), where the duration of prenatal ML is instrumented by the assignment to a reform, which extended compulsory leave by two weeks. Corresponding first stage estimates are summarized in Table A.3 in the Web Appendix. Panel B summarizes OLS estimates, where the prenatal ML duration is used as an explanatory variable. Each cell represents a separate estimation. The sample consists of working mothers giving birth in April and June 1974. The outcomes 'birth weight' and 'length' (columns 1 and 4) are continuous variables specified in logs, while 'low birth weight' (column 2), 'symmetric growth restriction' (column 3), and 'premature birth' (column 5) are binary variables indicating whether birth weight is below 2,500 grams, whether both birth weight is low and the Ponderal index is in the lowest quarter of its distribution, and whether the child was born prematurely, respectively. In each specification we control for a binary variable indicating whether the child was born in wedlock, the mother's religion, whether the mother is an Austrian citizen, the province a mother lives in, and very flexibly for mother's age (separate dummies for every value of age between 20 and 34, and two additional categories indicating whether age is lower than 20 or higher than 34). Robust standard errors are in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 — Comparison of estimated treatment effects on health at birth outcomes obtained via different estimators

	(1) Birth weight	(2) Low birth weight	(3) Symmetric growth restr.	(4) Length	(5) Premature birth[†]
Panel A. RDD					
Prenatal maternity leave	−0.005*	0.001	0.000	−0.001	0.005
	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)
No. of observations	7,350	7,350	7,350	7,350	7,350
Mean of outcome	5.77	0.06	0.04	3.92	0.06
Std. dev. of outcome	0.19			0.06	
Kleinbergen-Paap rK Wald F-statistic	756.93	756.93	756.93	756.93	756.93
Panel B. DiD					
Assigned × working	−0.005	0.001	−0.001	−0.000	
	(0.008)	(0.009)	(0.007)	(0.002)	
No. of observations	10,424	10,424	10,424	10,424	
Mean of outcome	5.78	0.05	0.03	3.92	
Std. dev. of outcome	0.19			0.06	
Panel C. RDD-DiD					
Prenatal maternity leave	−0.003	0.001	−0.001	−0.000	
	(0.005)	(0.006)	(0.005)	(0.001)	
No. of observations	10,424	10,424	10,424	10,424	
Mean of outcome	5.78	0.05	0.03	3.92	
Std. dev. of outcome	0.19			0.06	
Kleinbergen-Paap rK Wald F-statistic	755.47	755.47	755.47	755.47	

Notes: This table presents compares estimated local average treatment effects of extending compulsory ML duration by two weeks on health at birth outcomes obtained via different estimators. Each cell represents a separate regression. The sample in panel A consists of working mothers giving birth in April and June 1974, in panels B and C we extend the sample with non-working mothers giving birth in the same months. The outcomes 'birth weight' and 'length' (columns 1 and 4) are continuous variables specified in logs, while 'low birth weight' (column 2), 'symmetric growth restriction' (column 3), and 'premature birth' (column 5) are binary variables indicating whether birth weight is below 2,500 grams, whether both birth weight is low and the Ponderal index is in the lowest quartile of its distribution, and whether the child was born prematurely, respectively. In each specification we control for a binary variable indicating whether the child was born in wedlock, the mother's religion, whether the mother is an Austrian citizen, the province a mother lives in, and very flexibly for age of the mother (separate dummies for every value of age between 20 and 34, and two additional categories indicating whether age is lower than 20 or higher than 34). Panel A presents fuzzy RDD estimates obtained via 2SLS where duration of ML is instrumented by assignment to the reform (here coefficients have to be multiplied by two in order to make them comparable). Panel B are difference-in-differences estimates which compare pre- and post-reform outcomes between working and non-working mothers. In Panel C, we combine these two sources of exogenous variation in regression discontinuity difference-in-differences estimators. Robust standard errors are in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[†] The premature birth dummy is only available for working mothers, thus we cannot include it in our difference-in-differences and regression discontinuity difference-in-differences frameworks.

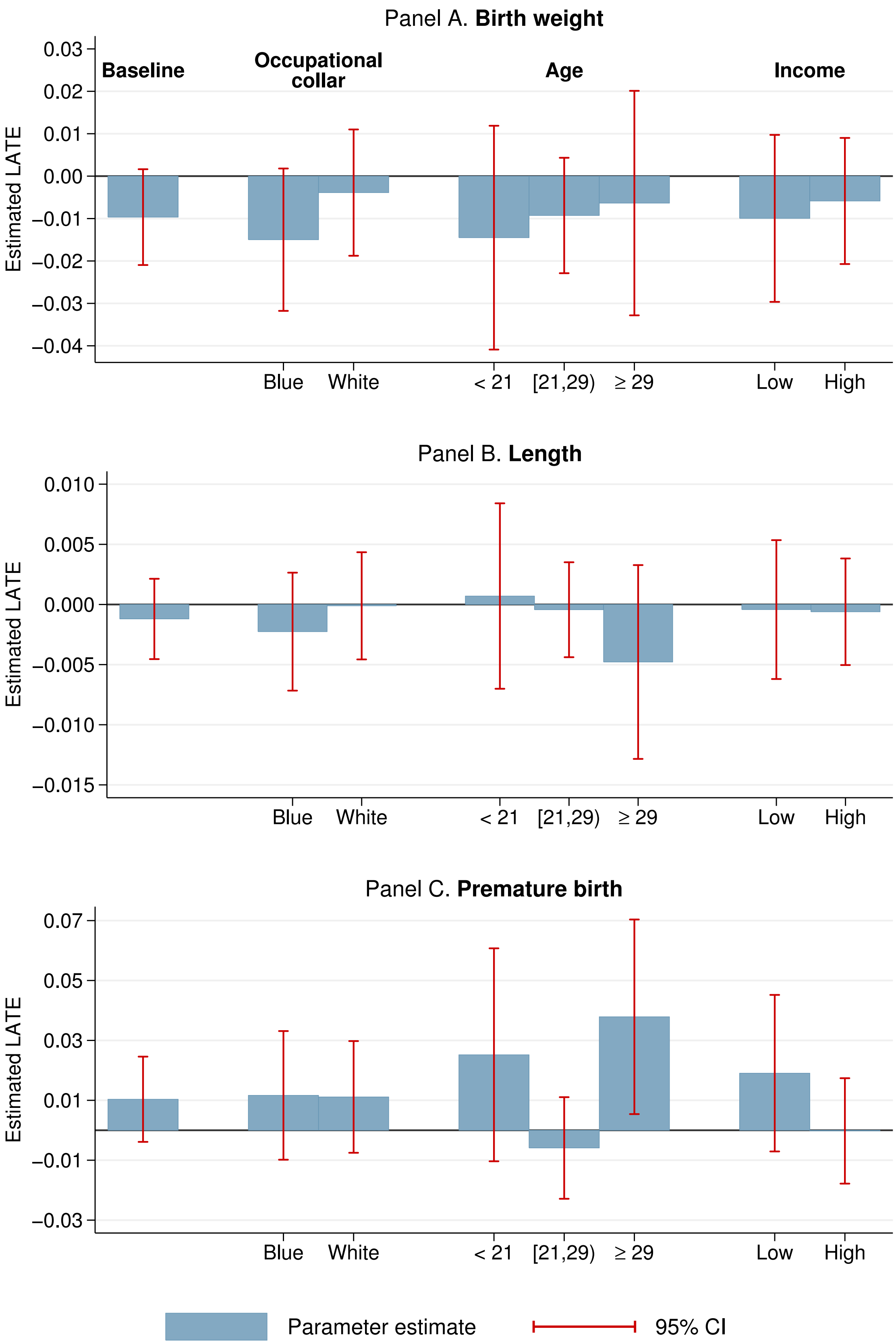
TABLE 4 — Estimated treatment effects on subsequent maternal outcomes

	(1) No. of next births	(2) Further birth	(3) Time to next birth†	(4) 20 year survival	(5) 40 year survival
Panel A. RDD					
Prenatal maternity leave	0.015	0.007	−0.002	−0.001	−0.007*
	(0.012)	(0.007)	(0.016)	(0.001)	(0.004)
No. of observations	7,350	7,350	3,619	7,350	7,350
Mean of outcome	0.70	0.49	7.10	0.99	0.92
Std. dev. of outcome	0.88		0.73		
Kleinbergen-Paap *rK* Wald *F*-statistic	756.93	756.93	366.27	756.93	756.93
Panel B. OLS					
Prenatal maternity leave	0.002	0.003	−0.006	0.000	−0.000
	(0.004)	(0.002)	(0.005)	(0.000)	(0.001)
No. of observations	7,350	7,350	3,619	7,350	7,350
Mean of outcome	0.70	0.49	7.10	0.99	0.92
Std. dev. of outcome	0.88		0.73		

Notes: This table presents estimated treatment effects of extending compulsory ML duration by two weeks on different subsequent maternal fertility outcomes. Each cell represents a separate regression. The sample consists of working mothers giving birth in April and June 1974. The outcomes '20 year survival' and '40 year survival' (columns 1 and 2) are binary variables indicating whether the mother was still alive 20 and 40 years after birth, respectively. 'No. of next births' is a count variable measuring the number of children the mother has given birth to subsequently, 'further birth' is a binary variable indicating whether the mother gave birth at least one more time, and 'time to next birth' is the time is the number of days passed until the mother gave birth again in logs, conditional on having another child. In each specification we control for a binary variable indicating whether the child was born in wedlock, the mother's religion, whether the mother is an Austrian citizen, the province a mother lives in, and very flexibly for age of the mother (separate dummies for every value of age between 20 and 34, and two additional categories indicating whether age is lower than 20 or higher than 34). Panel A presents fuzzy RDD estimates obtained via 2SLS where duration of ML is instrumented by assignment to the reform, panel B are simple OLS estimates where ML duration is used as an explanatory variable. Compulsory ML was extended by two weeks due to the reform, hence coefficients in panel A have to be multiplied by the same factor as well. Robust standard errors are in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

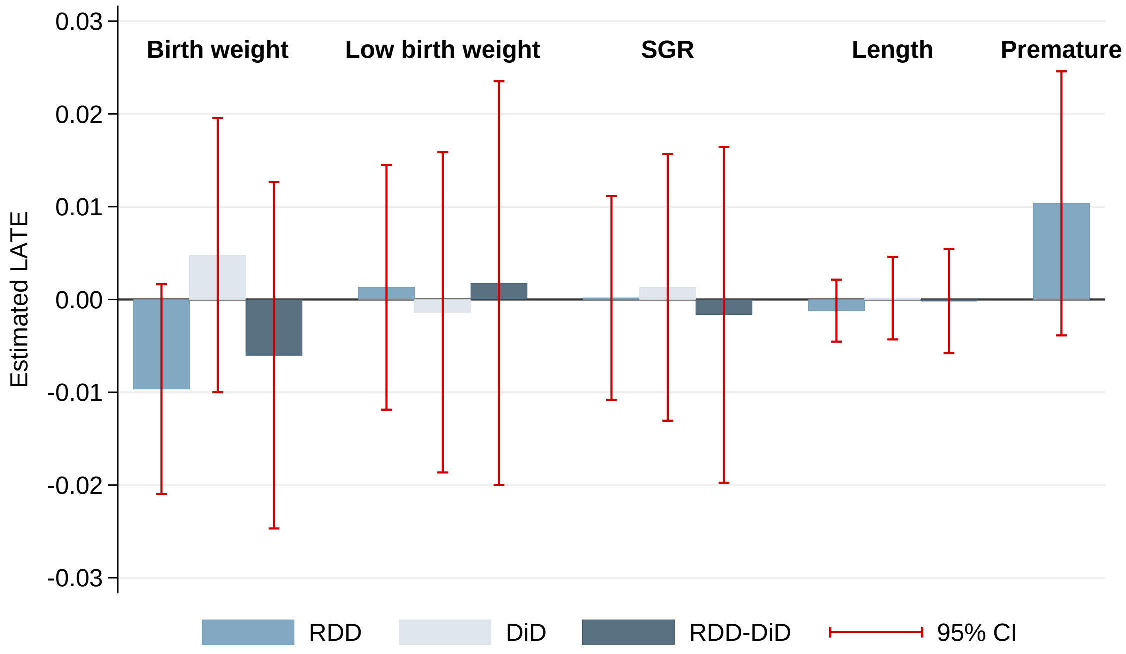
† Time to next birth is conditional on giving birth again, thus the samples includes only mothers who had another child.

FIGURE 4 — Heterogeneous treatment effects for different health at birth outcomes.



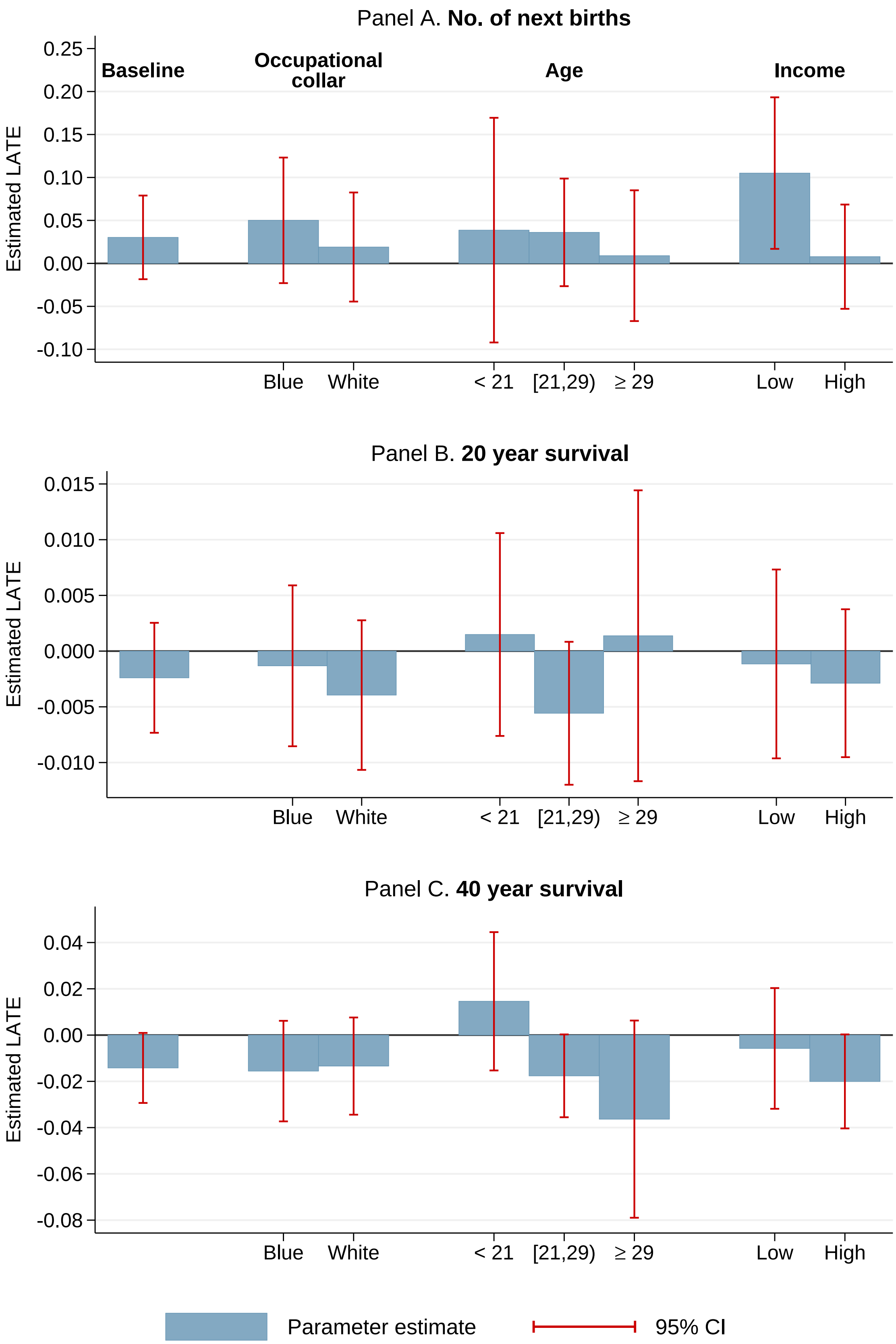
Notes: This figure summarizes fuzzy RDD estimates (obtained via 2SLS) of extending compulsory prenatal ML duration on health at birth for different (sub)samples. The duration of compulsory prenatal ML is instrumented with the assignment to a reform, which extended compulsory leave by two weeks. Corresponding first stage estimates are summarized in Table A.3 in the Web Appendix. Further details are provided in the notes to Table 2.

Figure 5 — Comparison of estimated treatment effects on health at birth outcomes obtained by different estimators.



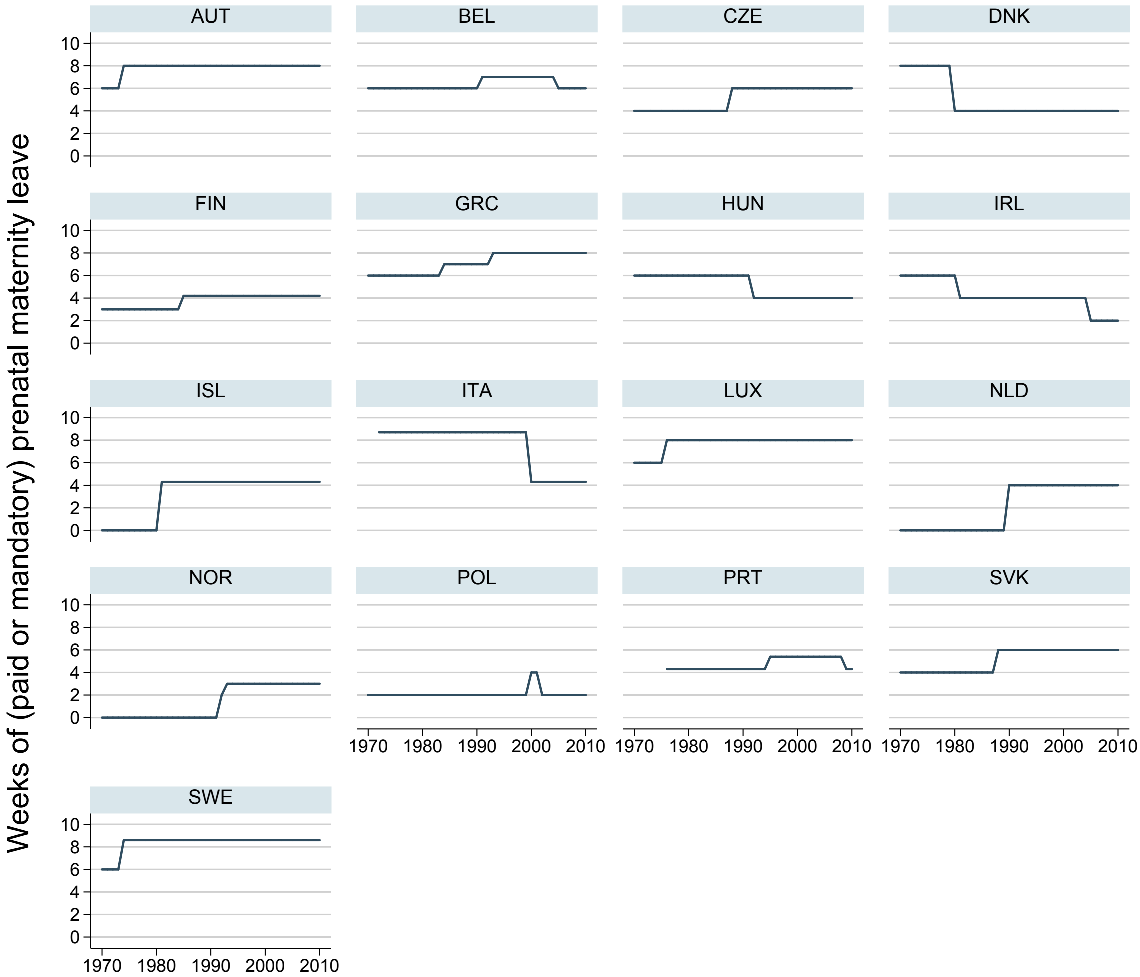
Notes: This graph compares estimated local average treatment effects of extending compulsory ML duration by two weeks on different health at birth outcomes. The sample used for RDD estimations consists of 7,350 working mothers giving birth in April and June 1974, while the sample for DiD and RDD-DiD estimations includes also non-working mothers giving birth in the same months, which amounts to a total of 10,424 mothers. For each outcome we present estimated treatment effects obtained by three different estimators. First, the *RDD* bars represent our baseline regression discontinuity treatment effects. Second, the *DiD* bars plot treatment effects from a difference-in-differences estimator, where we compare outcomes of working and non-working mothers before and after the eligibility cutoff. Third, the *RDD-DiD* bars plot effects from a regression discontinuity difference-in-differences estimator, which combines these two sources of variation. Our health at birth outcomes are defined as follows: 'Birth weight' and 'length' are continuous measures specified in logs; hence when multiplied by 100, estimated effects can be interpreted as percentage increases or decreases in the respective outcome induced by the treatment. 'Low birth weight,' 'SGR,' and 'premature' are binary variables indicating the probability of having birth weight below 2,500 grams, the probability of growth being symmetrically restricted (i.e., low birth weight and Ponderal index being in the lowest quartile of the distribution), and the probability of having a premature birth, respectively; hence estimates can be interpreted as percentage point increases or decreases in the outcome induced by the treatment.

FIGURE 6 — Heterogeneous treatment effects for different subsequent maternal outcomes.



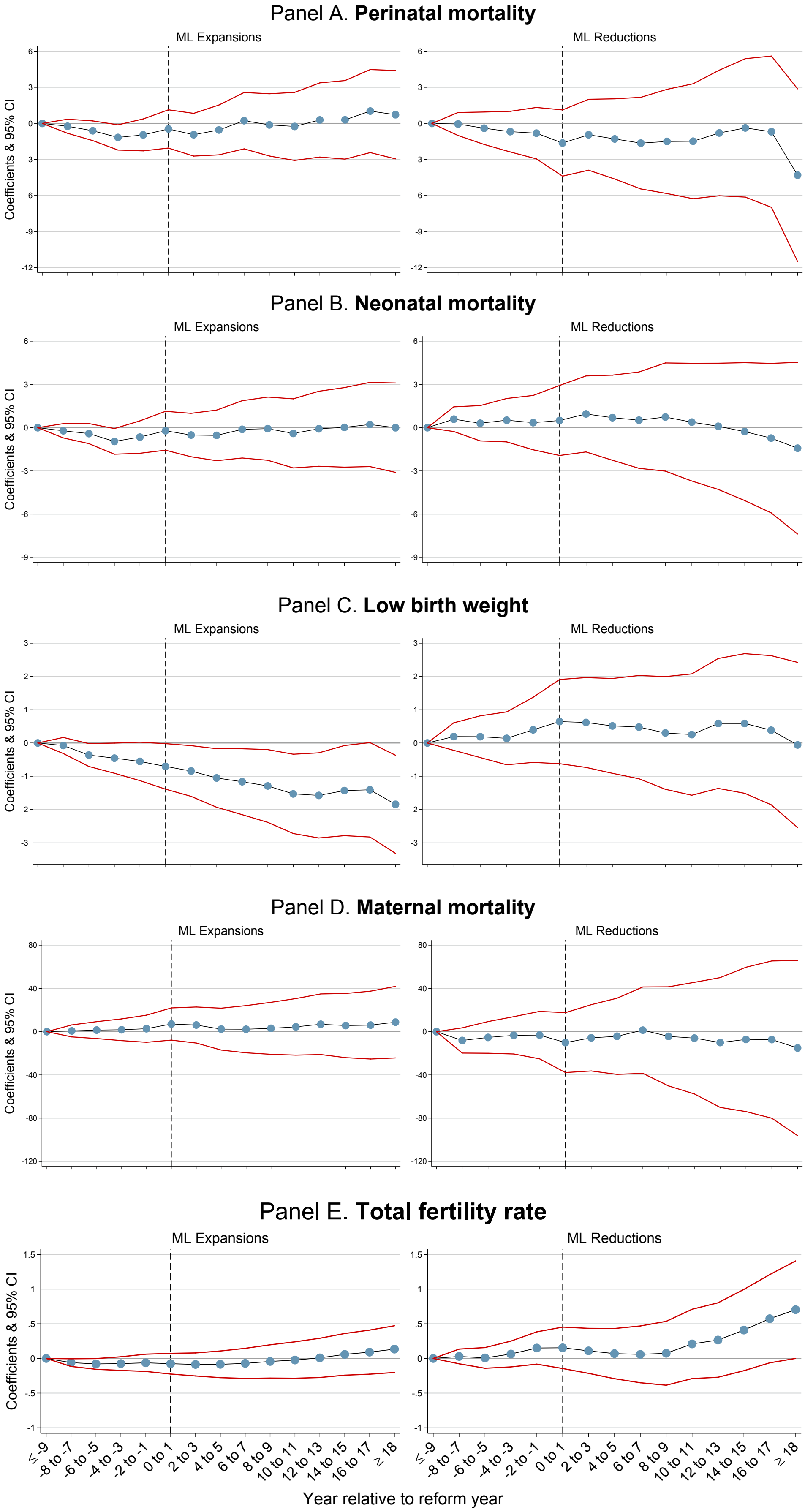
Notes: This figure summarizes fuzzy RDD estimates (obtained via 2SLS) of extending compulsory prenatal ML duration on subsequent maternal fertility and mortality for different (sub)samples. The duration of compulsory prenatal ML is instrumented with the assignment to a reform, which extended compulsory leave by two weeks. Corresponding first stage estimates are summarized in Table A.3 in the Web Appendix. Further details are provided in the notes to Table 4.

Figure 7 — Cross-country analysis: Reforms in prenatal ML durations



Notes: This figure shows the development of prenatal ML duration for all OECD countries, which experienced a reform of the mandatory or paid prenatal ML duration between 1970 and 2010. The information is based on Gauthier (2011) and OECD (2016). Reforms taking place between January and June are considered in the respective reform year, while new regulations are considered not until the subsequent year if the reform took place between July and December. Details on the reforms are summarized in the following, *AUT:* mandatory and fully paid leave increased in 4/1974 from 6 to 8 weeks; *BEL:* paid ML increased from 6 to 7 weeks in 1/1991 with one week being mandatory, in 7/2004 one week of leave was reallocated to the period after birth; *CZE:* paid prenatal leave increased from usually 4 to usually 6 weeks in 7/1987 in the former Czechoslovakia; *DNK:* non job-protected, paid leave were 8 weeks and decreased to 4 mandatory weeks before birth in 6/1980 (accompanied by an increase in the total leave duration from 14 to 18 weeks); *FIN:* paid and job-protected leave was extended from 3 to 4.2 weeks (25 weekdays) in 2/1985; *GRC:* paid and job-protected leave was increased from 6 to 7 weeks in 2/1984 and further extended to 8 mandatory weeks in 6/1993; *HUN:* paid and job-protected leave was decreased from 6 to 4 prenatal weeks in 3/1992 (while total leave increased from 12 up to 24 weeks); *IRL:* paid leave decreased from 6 to 4 paid, job-protected and compulsory weeks in 4/1981 (while total leave increased from 12 to 14 weeks), the 4 mandatory weeks decreased to 2 in 10/2004 (with 18 weeks total leave); *ISL:* in 1/1981 one month of prenatal job-protected and paid leave was introduced (in addition to one month after birth and one month shareable with the father); *ITA:* from 1/1972 five job-protected and paid months of ML were compulsory, with two of them prior to childbirth, from 3/2000 the mandatory leave could start one month prior to birth; *LUX:* paid and job-protected prenatal leave was increased from 6 to 8 weeks in 7/1975 and became mandatory in 1998; *NLD:* paid and job-protected leave from 1976 was 12 weeks after birth and increased in 3/1990 to 16 weeks with 4 prenatal weeks becoming mandatory; *NOR:* in 7/1991 paid and job-protected parental leave increased to 40 weeks with 2 weeks becoming mandatory for the mother to be taken prior to birth, this prenatal leave was increased in 4/1993 to 3 weeks; *POL:* possible paid prenatal leave was increased from 2 to 4 in 1/2000 and reduced to 2 again in 1/2002; *PRT:* from 1976 paid and job-protected leave was 90 days, up to 30 of which possibly be taken before birth, the possible prenatal leave duration was increased to 38 days in 6/1995 and reduced to 30 days again in 5/2009; *SVK:* see CZE; *SWE:* paid and job-protected prenatal leave was extended from 6 to 8.6 possible weeks.

Figure 8 — Cross-country analysis: DiD estimation results with pre-treatment trends



Notes: This figure plots coefficients for the effect of expanding (left-hand side graphs) and reducing (right-hand side) the prenatal maternity leave duration from the flexible DiD model discussed in section IV.2.

TABLE 5 — Estimated treatment effects on children's longterm outcomes

	(1) Employed	(2) White collar	(3) Wage	(4) Outpatient expenses	(5) Hospital days
Prenatal maternity leave	0.007	−0.013	0.014	−0.038	−1.431
	(0.010)	(0.014)	(0.017)	(0.132)	(1.395)
No. of observations	2,395	2,002	1,559	511	511
Mean of outcome	0.84	0.69	1.20	1.83	9.19
Std. dev. of outcome			0.52	2.34	23.90
Kleinbergen-Paap *rK* Wald *F*-statistic	204.82	176.63	151.20	48.91	48.91

Notes: This table presents fuzzy RDD estimates of extending ML duration by two weeks on longterm child outcomes, where the respective outcome is regressed on prenatal ML duration (in weeks), instrumented by a reform-assignment indicator. Each column represents a separate regression. The sample in each column consists of children born to working mothers giving birth in April and June 1974, who could uniquely be tracked in the our administrative data and for whom we had data on the respective outcome variable. The outcome 'employed' is a binary variable indicating whether the child was in employment at age 40, 'wage' is the daily wage in € 100 at age 40, 'white collar' is a binary variable indicating whether the child worked in a white collar job at age 40, 'outpatient expenses' are aggregated physician expenses between age 25 and 40 in € 1,000, and 'hospital days' is the aggregate number of days spent in hospital between age 25 and 40. In each specification we control for a binary variable indicating whether the child was born in wedlock, the mother's religion, whether the mother is an Austrian citizen, the province a mother lives in, and very flexibly for age of the mother (separate dummies for every value of age between 20 and 34, and two additional categories indicating whether age is lower than 20 or higher than 34). Compulsory ML was extended by two weeks due to the reform, hence coefficients have to be multiplied by the same factor as well. Robust standard errors are in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 — Cross-country analysis: Variable description and summary statistics

	Description	N	Mean	Std. dev.	Min.	Max.
Prenatal maternity leave	Maximum number of weeks of (mandatory or paid) maternity leave prior to childbirth	689	5.08	2.42	0.0	8.7
Outcomes						
Perinatal mortality	Number of fetal deaths (27 weeks/1,000 grams) plus deaths within first week per 1,000 total births	649	11.40	6.50	2.6	34.9
Neonatal mortality	Number of deaths within first 28 days per 1,000 live births	666	7.02	5.23	0.9	28.7
Low birth weight	Number of children with a birth weight of below 2,500 grams as percent of total live births	574	5.89	1.62	2.9	11.7
Maternal mortality	Number of maternal deaths per 100,000 live births	634	9.38	10.06	0.0	75.3
Total fertility rate	Number of children per women aged 15 to 49 years old	689	1.80	0.42	1.1	4.0
Control variables						
Total population	Number of inhabitants (/100,000)	689	11.66	13.69	0.2	60.5
Population aged ≤ 14	Share of inhabitants aged 0-14	689	0.20	0.04	0.1	0.3
Population aged ≥ 65	Share of inhabitants aged 65+	689	0.14	0.02	0.1	0.2
Age at birth	Mean age of women at childbirth	669	28.02	1.64	24.5	31.4
Marriage rate	Marriages per 1,000 inhabitants	689	5.95	1.42	3.5	12.8
Female LFP	Civilian labor force as percent of population aged 15-64, females	568	59.63	13.77	31.0	96.5
Male LFP	Civilian labor force as percent of population aged 15-64, males	568	81.39	8.18	63.6	122.7
Agricultural share	Employment in primary sector as percent of total employment	584	0.09	0.07	0.0	0.4
Manufacturing share	Employment in secondary sector as percent of total employment	573	0.31	0.06	0.2	0.5
Service share	Employment in tertiary sector as percent of total employment	573	0.60	0.10	0.3	0.8
GDP per capita	Real GDP per capita, 2011 USD, chained PPP (/1,000)	649	25.47	12.71	5.3	87.7
Schooling years	Average years of education in the population aged 25+	689	9.49	1.80	3.1	13.2
Labor share	Share of labor compensation in GDP	689	0.60	0.06	0.4	0.7
Hours worked	Average annual hours worked by population engaged (/1,000)	619	1.81	0.22	1.4	2.4

Notes: Statistics are based on a sample of 17 countries (Austria, Belgium, Czech Republic, Denmark, Finland, Greece, Hungary, Ireland, Iceland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovakia, Sweden) observed from 1970 to 2010.

TABLE 7 — Cross-country analysis: Further estimates

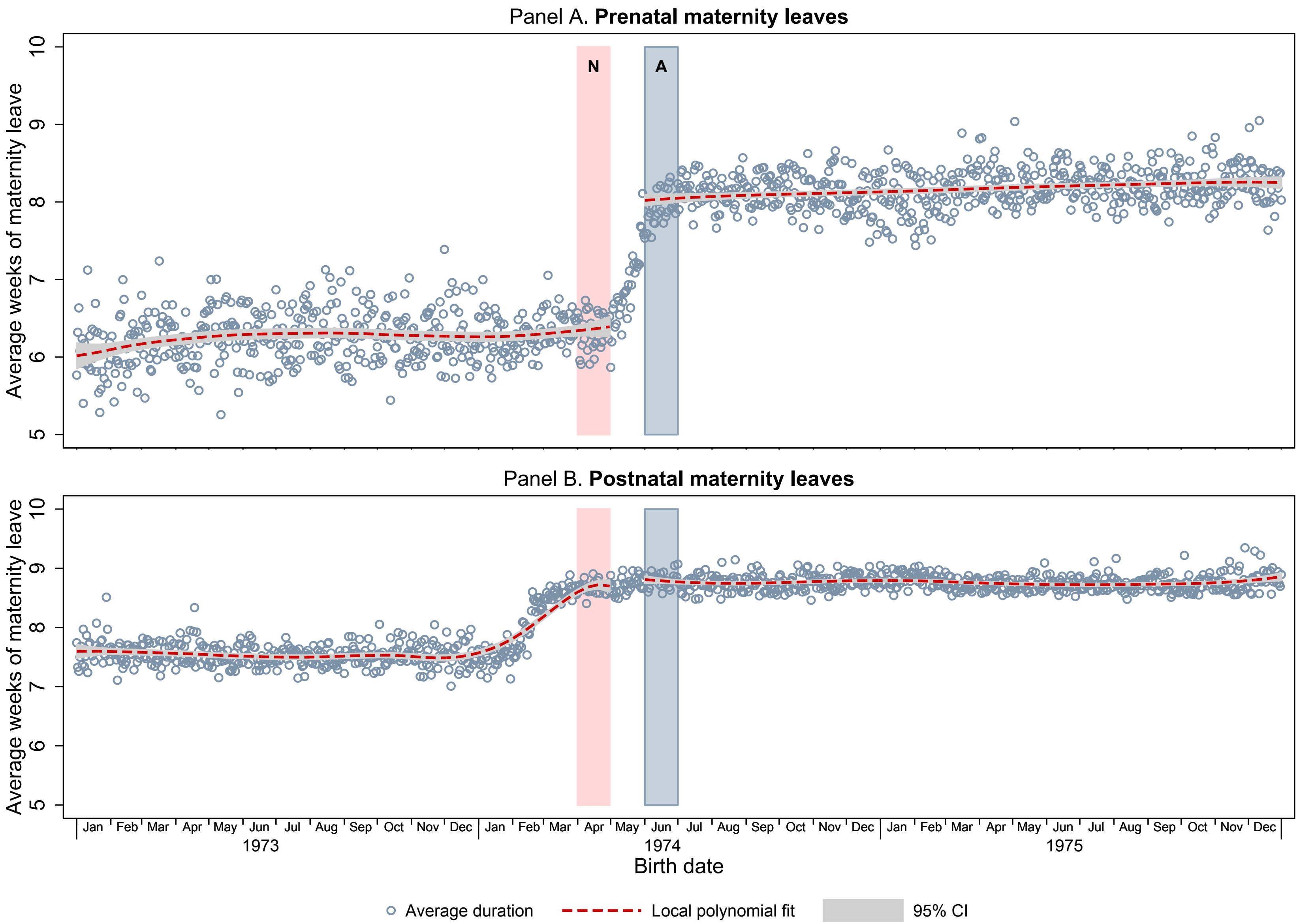
	(1) Perinatal mortality	(2) Neonatal mortality	(3) Low birth weight	(4) Maternal mortality	(5) Total fertility rate
Panel A. Linear Model					
Prenatal maternity leave	0.047	0.019	−0.041	0.444	−0.004
	(0.065)	(0.053)	(0.026)	(0.534)	(0.006)
Panel B. Effects relative to 6 weeks of prenatal ML					
Below 6 wks	−0.267	−0.011	0.163	0.563	−0.115***
	(0.331)	(0.274)	(0.136)	(2.807)	(0.032)
Above 6 wks	−0.126	0.061	0.065	2.914	−0.086***
	(0.282)	(0.240)	(0.120)	(2.481)	(0.027)
Prob > F	0.695	0.810	0.496	0.476	0.398
Panel C. Heterogenous effects by female LFP					
Prenatal ML x high female LFP	−0.029	−0.104	−0.023	0.768	0.010
	(0.090)	(0.075)	(0.033)	(0.828)	(0.009)
Prenatal ML x low female LFP	0.127	0.142*	−0.067	0.195	−0.018**
	(0.093)	(0.075)	(0.041)	(0.722)	(0.009)
Prob > F	0.228	0.021	0.408	0.608	0.028
Panel E. Heterogenous effects by mean age at birth					
Prenatal ML x high age at birth	0.052	0.048	−0.042	0.652	−0.005
	(0.085)	(0.070)	(0.035)	(0.737)	(0.008)
Prenatal ML x low age at birth	0.040	−0.029	−0.039	0.171	−0.003
	(0.108)	(0.090)	(0.039)	(0.853)	(0.011)
Prob > F	0.932	0.516	0.962	0.682	0.888
Panel F. Heterogenous effects by GDP per capita					
Prenatal ML x high GDP	0.046	0.014	−0.013	1.015	−0.011
	(0.084)	(0.070)	(0.035)	(0.731)	(0.008)
Prenatal ML x low GDP	0.049	0.026	−0.073*	−0.253	0.005
	(0.104)	(0.085)	(0.038)	(0.810)	(0.010)
Prob > F	0.984	0.915	0.247	0.254	0.220
No. of observations	649	666	574	634	689
Mean of outcome	11.40	7.02	5.89	9.38	1.80
Std. dev. of outcome	6.50	5.23	1.62	10.06	0.42

Notes: Regressions include all control variables given in Table 6, respective missing dummies, country fixed-effects, year fixed-effects as well as country-specific cubic time trends. Standard errors are given in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Web Appendix

This Web Appendix (not for publication) provides additional material discussed in the unpublished manuscript 'The Effect of Prenatal Maternity Leave on Short and Long-term Child Outcomes' by Alexander Ahammer, Martin Halla, and Nicole Schneeweis.

FIGURE A.1 — Average pre- and postnatal ML durations between 1973 and 1975.



Notes: These graphs depict the average prenatal (Panel A) and postnatal (Panel B) ML durations by birth date of the child between January 1973 and December 1975. Separate quadratic fits for the pre-treatment (until April 30, 1974) and the post-treatment period (starting with June 1, 1974) are depicted by the scattered line. The red-shaded area highlights the subset of assigned births ('A'), which we use in our estimation analysis. These mothers were eligible for 8 weeks of prenatal ML duration. The framed blue-shaded area highlights the subset of not assigned births ('N'), which we use in our estimation analysis. These mothers were eligible for 6 weeks of prenatal ML duration. Both groups of mothers were eligible for 8 weeks of postnatal ML duration. Mothers who gave birth in May 1974 (during which the reform was phased-in) are excluded form our estimation analysis.

Table A.1 — Descriptive statistics, working vs. non-working mothers.

	Working mothers					Non-working mothers				
	N	Mean	Std. dev.	Min.	Max.	*N*	Mean	Std. dev.	Min.	Max.
Prenatal maternity leave (in weeks)[a]	7350	7.11	2.60	0.3	33.3	3074	40.00	0.00	40.0	40.0
Health at birth outcomes										
Birth weight (in grams)	7350	3256.98	528.68	400.0	5300.0	3074	3351.40	505.65	700.0	5200.0
Birth weight is below 2,500 grams	7350	0.06		0.0	1.0	3074	0.04		0.0	1.0
Symmetric growth restriction[b]	7350	0.04		0.0	1.0	3074	0.03		0.0	1.0
Length (in cm)	7350	50.42	2.77	27.0	61.0	3074	50.94	2.58	31.0	60.0
Premature birth[c]	7350	0.06		0.0	1.0					
Maternal outcomes										
Number of next births	7350	0.70	0.88	0.0	8.0	3074	0.71	1.01	0.0	8.0
Probability of having another child	7350	0.49		0.0	1.0	3074	0.44		0.0	1.0
Time to next birth (in years)	3619	4.36	3.43	0.5	23.5	1364	4.22	3.37	0.8	20.8
20 year survival probability	7350	0.99		0.0	1.0	3074	0.99		0.0	1.0
40 year survival probability	7350	0.92		0.0	1.0	3074	0.92		0.0	1.0
Sample stratification variables										
Blue collar worker	7285	0.54		0.0	1.0					
Below median income in 1973	6762	0.46		0.0	1.0					
Covariates										
Age at birth	7350	24.29	5.19	15.0	47.0	3074	27.07	5.83	14.0	46.0
Child born in wedlock	7350	0.84		0.0	1.0	3074	0.92		0.0	1.0
Religion										
Catholic	7350	0.87		0.0	1.0	3074	0.92		0.0	1.0
Protestant	7350	0.05		0.0	1.0	3074	0.05		0.0	1.0
Other religion	7350	0.07		0.0	1.0	3074	0.02		0.0	1.0
No religion	7350	0.01		0.0	1.0	3074	0.01		0.0	1.0
Mother is Austrian citizen	7350	0.14		0.0	1.0	3074	0.07		0.0	1.0

Notes: This table presents summary statistics for our treatment (ML duration); as well as our outcome, sample stratification, and control variables. The sample is comprised of mothers giving birth in April and June 1974. Statistics are provided separately for both working and non-working mothers, where working status is assessed at time of birth.

[a] ML duration is assumed to be 40 weeks for non-working mothers. The specific value chosen has no impact on the estimation results.
[b] Symmetric growth restriction is defined as the probability of having low birth weight *and* having a low Ponderal index ($PI = kg/m^3$).
[c] This variable is only available for working mothers. It is calculated using the postnatal ML duration.

Table A.2 — Full estimation output for RDD estimation

	Health at birth outcomes					Subsequent maternal fertility outcomes				
	(1) Birth weight	(2) Low birth weight	(3) Symmetric growth restr.	(4) Length	(5) Premature birth	(6) No. of next births	(7) Further birth	(8) Time to next birth	(9) 20 year survival	(10) 40 year survival
Prenatal maternity leave	−0.005*	0.001	0.000	−0.001	0.005	0.015	0.007	−0.002	−0.001	−0.007*
	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)	(0.012)	(0.007)	(0.016)	(0.001)	(0.004)
Child born in wedlock	0.055**	−0.047	−0.020	0.017**	−0.081**	0.100	0.061	0.082	0.005	0.002
	(0.027)	(0.029)	(0.023)	(0.008)	(0.035)	(0.084)	(0.047)	(0.128)	(0.009)	(0.023)
Mother's religion										
Protestant	−0.023*	0.003	0.004	−0.008*	0.004	−0.086**	−0.071***	0.045	0.000	−0.020
	(0.014)	(0.013)	(0.011)	(0.004)	(0.014)	(0.041)	(0.025)	(0.062)	(0.005)	(0.016)
Other religion	−0.005	−0.007	−0.009	−0.003	0.027*	−0.053	−0.082***	0.151***	0.003	0.007
	(0.010)	(0.011)	(0.009)	(0.003)	(0.014)	(0.044)	(0.024)	(0.055)	(0.003)	(0.012)
No religion	−0.003	0.010	0.003	−0.009	0.022	0.025	0.012	0.064	−0.011	−0.045
	(0.025)	(0.026)	(0.020)	(0.007)	(0.029)	(0.074)	(0.048)	(0.097)	(0.015)	(0.034)
Mother is Austrian citizen	0.024	−0.010	0.007	0.011	−0.046	0.214**	0.083*	0.190	0.002	−0.031
	(0.027)	(0.031)	(0.024)	(0.008)	(0.037)	(0.090)	(0.049)	(0.131)	(0.009)	(0.025)
Intercept	5.754***	0.075*	0.033	3.904***	0.107**	0.814***	0.586***	6.969***	0.995***	0.988***
	(0.036)	(0.039)	(0.031)	(0.010)	(0.047)	(0.133)	(0.074)	(0.172)	(0.016)	(0.041)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mother's age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	7,350	7,350	7,350	7,350	7,350	7,350	7,350	3,619	7,350	7,350
Mean of outcome	5.77	0.06	0.04	3.92	0.06	0.70	0.49	7.10	0.99	0.92
Std. dev. of outcome	0.19			0.06		0.88		0.73		
Kleinbergen-Paap *rK* Wald *F*-statistic	756.93	756.93	756.93	756.93	756.93	756.93	756.93	366.27	756.93	756.93

Notes: In this table, we present the full estimation output of our fuzzy RDD estimates summarized in Tables 2 and 4 in the paper.

TABLE A.3 — Full estimation output for first-stage regression

	Coef.	Std. err.	F-statistic†	Shea's $r^{2\ddagger}$
Baseline	1.587***	(0.058)	756.9	0.093
Restricted samples				
Mother's age				
< 21	1.337***	(0.106)	159.6	0.076
$[21, 29)$	1.670***	(0.079)	444.5	0.101
≥ 29	1.715***	(0.137)	156.3	0.097
Income				
Low	1.486***	(0.081)	338.3	0.079
High	1.707***	(0.074)	536.9	0.119
Occupation				
Blue collar	1.525***	(0.079)	373.3	0.088
White collar	1.670***	(0.086)	376.1	0.100

Notes: This tables gives first-stage statistics for RDD regressions used in the main paper. The overall sample consists of working mothers giving birth in April and June 1974. We present first-stages for both the baseline results and all restricted samples we use for other estimations (e.g., to estimate effects on certain outcomes or heterogeneous effects). We provide the first-stage coefficient (obtained from a regression of ML duration on the assignment variable date of birth) along with its standard error, the overall F-statistic of the first-stage, and the partial r^2 of the first-stage. Each row represents a separate first-stage regression.

† Kleinbergen-Paap rK Wald F-Statistic
‡ Shea's partial R^2

TABLE A.4 — Summary statistics for children's longterm outcomes

	Assigned mother		**Non-assigned mother**			
	Mean	N	Mean	N	Diff.	p-value
Employed at age 40	0.84	1189	0.83	1206	–0.01	0.50
Daily wage at age 40 (in € 100)	1.21	774	1.19	785	–0.02	0.41
White collar employee at age 40	0.68	1000	0.70	1002	0.02	0.37
Agg. physician expenses b/w age 25–40 (in € 1,000)	1.81	255	1.86	256	0.05	0.80
Agg. hospital days b/w age 25–40	8.13	255	10.26	256	2.13	0.31

Notes: This table provides summary statistics for our longterm child outcomes, separately for children of assigned mothers (born in June 1974) and non-assigned mothers (born in April 1974). The samples for each variable consist of children of mothers who were working at time of birth, who could uniquely be tracked in our administrative data, and for whom we have data on the respective variable.