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The Effect of Prenatal Maternity Leave on Short and Long-term Child Outcomes†

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Abstract

Maternity leave policies are designed to safeguard the health of pregnant workers and their unborn children. However, little is known about the impact of existing policies, which are not evidence-based. We evaluate a maternity leave extension in Austria, which increased mandatory leave from 6 to 8 weeks prior to birth. We exploit that the eligibility for the extended leave was determined by a cutoff due date. Our estimates capture a reduction of *in utero* exposure to maternal stress caused by work in the third trimester of pregnancy. We find no evidence for significant effects of this extension on children's health at birth or long-term health and labor market outcomes. Subsequent maternal health and fertility are also unaffected. We conclude that, for workers without problems in pregnancy, mandatory maternity leave should not start prior to the 35th week of gestation.

JEL Classification: J13, I18, J28, I13, J83, J88.
Keywords: Maternity leave, fetal origins hypothesis, infant health, birth outcomes, birth weight, long-term child outcomes, fertility.

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I. Introduction

Developed countries have special regulations in place to address the safety and health of pregnant workers and their unborn children. One 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 pre- and postnatal durations.[2] In this paper, we are interested in *prenatal* ML. We evaluate the impact of maternal employment during pregnancy on child and maternal outcomes. Despite the popular belief that prenatal ML is beneficial to the infant and mother, empirical evidence on the impact of prenatal ML is extremely scarce and existing policies are not evidence-based.[3]

In particular, we evaluate a prenatal ML extension in Austria. Until 1973 statutory ML prohibited employment in the period from 6 weeks before to (usually) 6 weeks after the delivery. A reform in the year 1974 increased both mandatory pre- and postnatal ML from 6 to 8 weeks (or by 33 percent). 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.

Our research design has a number of interesting features. First, since our IV reflects a policy change, our estimated LATE is equivalent to a *policy-relevant treatment effect* (Heckman and Vytlacil, 2001). This is a well-defined parameter answering a policy-relevant question. Second, in our research design assigned and not assigned mothers, while having different prenatal ML durations, were both entitled to the *same postnatal* ML duration and the same parental leave. This allows us to cleanly identify the effect of variation in prenatal ML, not only on birth outcomes, but also on post-birth outcomes. Third, the timing of the reform enables us to study its effects on children and mothers in the long-run. This is important, since the fetal origins hypothesis stresses that (health) effects of prenatal events may remain latent for many years (Almond and Currie, 2011*a*,*b*). Fourth, to check the robustness of our results, we can additionally use information on unaffected non-working mothers, who are not eligible for ML.

[1]The leave that often follows ML and 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).

This second source of exogenous variation extends our RDD with a difference-in-differences (DiD) approach. The DiD component differences out potential seasonal effects and accounts for any unobserved characteristics that follow a seasonal pattern between children born in different months. Thus, the combination of these two sources of exogenous variation ensures a clean identification of treatment effects. Fifth, we can rely on high-quality administrative data sources covering the universe 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 children's long-term human capital outcomes (up to 40 years of age) and maternal mortality. For a subsample of observations, we also have data on long-term health outcomes (i.e, health care utilization between 25 and 40 years of age). Sixth, the institutional setting promotes a clear interpretation of our results. Our LATE captures the reduction of *in utero* exposure to maternal stress caused by work in the 33rd and 34th week of gestation for a group of mothers without major problems in this stage of pregnancy. We consider this estimate to be informative not only for the Austrian case, but also for designing prenatal ML policies in other places, such as the United States.

There are several potential mechanisms through which extended prenatal ML could improve the health of pregnant workers and their unborn children. First, the extended job-protection and the absence from work should reduce the mother's psychological and physiological stress level. Certain groups of workers could also benefit from a reduction in specific occupational exposures.[4] 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. Finally, the modified allocation of time (i.e., substituting work with leisure) may also lead to healthier behavior. Expecting mothers may have more time to rest, to follow a healthy diet, or to do all necessary prenatal check-ups.

The existing literature provides evidence for the importance of these mechanisms. The fetal origins hypothesis and supporting empirical evidence emphasize a number of factors in the prenatal environment that are important for later child and adult outcomes (Almond and Currie, 2011*a*,*b*). Maternal stress is one important factor. Most studies distinguish the effects of prenatal stress by pregnancy trimester of exposure. The reform we consider in this paper has the potential to reduce maternal stress in the third trimester (more specifically in the weeks 33 and 34). Multiple studies provide evidence that prenatal stress has adverse effects for birth outcomes

[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 cannot be ruled out that the counterfactual home environment is less beneficial for some women. In this case, an increase in prenatal ML may have even negative effects.

throughout pregnancy. For instance, Black et al. (2016) find negative effects of stress induced by the death of the mother's parent during pregnancy on birth outcomes with similar effects across all trimesters of exposure.[6] Persson and Rossin-Slater (2018), studying an equivalent treatment with a focus on long-run mental health outcomes, confirm this pattern. Based on these findings we consider reduced stress in the third trimester as one important causal channel of our treatment. Regarding healthier behavior during pregnancy, a large number of factors (such as nutrition and physical activity) are discussed. While causal evidence is lacking for some of these determinants, the importance of prenatal checkups is documented in design-based studies. For example, Evans and Lien (2005) exploit a 1992 bus strike in Pennsylvania, which led to a sharp decline in prenatal care visits among women pregnant at that time. They conclude that prenatal checkups reduce maternal smoking and enhance birth weight.[7]

To our surprise, we find no evidence for an impact 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 subsamples 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 health outcomes in the short-run, we also do not find any evidence for significant effects on long-run health and labor market outcomes. Treated and untreated children have statistically indistinguishable labor market and health outcomes up to the age of 40. Thus, there is also no evidence for latent effects that manifest later in life. Our analysis of subsequent maternal fertility neither reveals any significant effects of the reform. 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 year survival rates. We therefore conclude that the reform had no measurable effects on children and mothers.

The political justification for this reform was to improve the health of pregnant workers and their children. Our evaluation provides no evidence for any impact of the extension from 6 to 8 weeks of prenatal ML. In contrast, the reform has clear cost. It has increased public spending on transfer payments by one-third and additional cost for firms cannot be ruled out. Importantly, some women may prefer to work during this period, but are not allowed to. While our results must be interpreted within the scope of the Austrian setting, we conclude more generally that mandatory prenatal ML starting in the 35$^{\text{th}}$ week of gestation is sufficient for pregnant workers without problems in pregnancy. It should be emphasized that we do not interpret our results as a general argument against (mandatory) prenatal ML. Quite the contrary, we consider our finding to be valuable for designing an optimal prenatal ML policy.[8]

[6] This finding is consistent with previous studies on the effects of prenatal exposure to stressful events such as armed conflicts (Mansour and Rees, 2012) or hurricanes (Currie and Rossin-Slater, 2013). Earlier papers using terrorist attacks landmine explosions (Camacho, 2008) and a large earthquake (Torche, 2011) find the strongest effects in the first trimester.

[7] Sonchak (2015) finds similar effects of prenatal care on birth weight for disadvantaged white mothers.

[8] 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 across 17 OECD countries over time. We find no evidence for an 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.[9] 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) in 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 firm coverage. 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 six weeks of paid ML lowered rates of low birth weight and pre-term births by around 3 and 7 percent, respectively. In contrast to Rossin (2011), the effects were driven by disadvantaged African American and unmarried mothers. Wüst (2015) uses Danish data to study the effect of maternal employment during pregnancy on birth outcomes. She focuses on the pregnancy weeks 12 and 30. To account for selection into employment she exploits variation across pregnancies and compares outcomes of mothers' consecutive children. She finds that mothers, who are employed (in either week 12 or 30) are *less* likely to have a preterm birth. As a potential explanation for this finding she discusses maternal stress caused by *not* working.

The remainder of the paper is organized as follows: In Section II, we present our research design. We first provide details on the ML system, the reform in the year 1974, and other relevant aspects of the institutional setting. We describe our data sources and present our estimation strategy. In Section III, we discuss the estimation results along with a number of robustness checks. In Section IV, we briefly discuss complementary evidence from a 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 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. Finally, we describe changes in the public prenatal care program over time.

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

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.[10]

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 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 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 from 6.3 to 8.1 weeks.

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

[Figure 1]

The lower Panel of Figure 1 depicts 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.[11]

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 until the child's first birthday. The eligibility criteria for parental leave and the associated transfer payments did not differ for not assigned and assigned mothers.

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 system in place which provided almost universal access to high-quality healthcare.[12] 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.[13]

[11] 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.

[12] Patients hold mandatory health insurance administered through 9 *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.

[13] 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.

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. The ASSD also allows us to construct outcome variables in the domains of subsequent fertility, human capital outcomes, and mortality. Furthermore, we use mothers' labor market histories 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. One drawback of the ABR is that we do not observe information on parity for the early birth cohorts we consider. We will use subsamples of young versus older mothers to approximate a comparison between first births and higher-order parities.

Finally, we use information provided in the *Upper Austrian Sickness Fund* database to construct long-term health outcomes for children. This database includes information on healthcare expenditures for all private employees and their dependents in Upper Austria starting in the year 1998. It covers roughly one million members representing 75 percent of the population in Upper Austria (see also footnote 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. 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, $A_i = 0$. Note that, since we do not observe the expected due date, we use the actual birth date as a proxy. Thus, our assignment variable potentially has some measurement error. However, this should be negligible, since the variable is binary and possible misclassifications are unlikely.[14]

[14]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 44.6 weeks. The second one describes cases of extreme preterm births, with gestational lengths of at most 35.4 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 0.03 percent were

We disregard mothers who gave birth in May where the reform was phased in and 28 cases of multiple births.

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. 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 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 1.589, implying that assignment increases the average prenatal ML duration by 1.6 weeks or 11 days. The estimated coefficient is highly statistically significant with an F-statistic of about 756. This coefficient is stable across subsamples (see Table A.1 in the Web Appendix).[15]

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.[16] The bill was submitted on February 5, 1974. 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

such extreme postterm births.

[15] The largest difference is observed between very young (< 21) and older mothers (≥ 29), for whom we obtain coefficients of 1.34 and 1.72, respectively.

[16] 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 discussing maternity leave. We found a total of five articles.

effective on April 1, 1974.[17] 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.

[Figure 2]

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.

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 September 1974.

[Figure 3]

Statistical power of the research design Does our research design have sufficient statistical power? Consider the outcome birth weight. The number of observations required under a randomized controlled trial to achieve a minimum detectable effect size of a quarter of a sample standard deviation (equal to 131 grams) is 404.[18] To derive the valid minimum required sample size (henceforth MRSS) for our research design using an RDD, we have to account for imperfect compliance (Schochet, 2008). Perfect compliance would imply that all non-assigned mothers had a prenatal ML duration of 6 weeks, while all assigned mothers had 8 weeks. In contrast, we have sample means of 6.33 and 7.92 weeks, respectively. This gives a difference in treatment of 1.59 weeks (instead of 2 weeks as in the case of full compliance). Adjusting for this extent of incomplete compliance we compute an MRSS of 640. Since our number of observations is 7,350, the minimum detectable effect size is considerably smaller (about 28 grams). Thus, our research design has sufficient statistical power to detect even very small treatment effects.

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 observable characteristics) from working mothers, they are useful since they were never eligible for ML.[19] The reform had by definition no impact on non-working mothers, hence they

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

[18]To calculate this minimum required sample size we assume a power level of 0.8, a significance level of 0.05, and we use the sample mean of not assigned mothers (3,267.40), as well as the standard deviations (524.34 and 532.96) of both groups.

[19]Non-working mothers were on average 2.8 years older at the time of birth and more likely to be married and Catholic. Table A.2 in the Web Appendix provides descriptive statistics for working and non-working mothers, who gave birth in April or June 1974.

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. This differences out any potential seasonal effects between children born in April and June.[20] To translate this combined approach into a regression framework, we extend our first stage estimation with a binary variable, W, capturing the mother's 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 substitute 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 long-term outcomes for both the child and the mother. We are interested in health at birth outcomes, as well as long-term labor market and health outcomes of the child. To infer on effects on the mother, we examine her subsequent fertility and health outcomes. In Table 1, we provide descriptive statistics for our main outcome variables and covariates, separately for assigned and non-assigned mothers.

[20] There is some evidence for the U.S. (Buckles and Hungerman, 2013) that children born at different times of the year are born to mothers with significantly different characteristics. Working mothers who gave birth in April and June 1973 could in principle serve as an alternative additional control group. Unfortunately, we have incomplete information to link observations across time for cohorts before 1974.

[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. According to the medical literature, intrauterine growth retardation can either start early or late in pregnancy leading to symmetrically or asymmetrically growth restricted newborns, respectively.[21] Since the ML reform altered the situation of pregnant women in gestation week 33 and 34, we examine on asymmetric growth restriction. We construct a binary variable combining the birth weight and the child's *Ponderal* index, $PI = kg/m^3$ (Landmann et al., 2006). We define growth as asymmetrically restricted if birth weight is lower than 2,500 grams and the Ponderal index is in the lowest quartile of its sample distribution. In our sample, about 4% of children have an asymmetric growth restriction according to this definition. Finally, we generate a variable for premature births. This information had not been recorded in the ABR until 1983, but we can infer it from the mother's postnatal ML duration. In 1974 postnatal leaves were stipulated to last 12 weeks for mothers who experienced a preterm birth. Accordingly, we assume a preterm birth if a mother took 12 weeks or more of postnatal ML. We find 6% of all births to be premature in our data.

Children's long-term outcomes We consider children's long-term labor market and health outcomes. About one-third of all children in our sample can be uniquely matched to their mother in the ASSD and can be included in our 2SLS estimations.[22] Fortunately, the availability of the data link seems to be idiosyncratic. It is not correlated with our IV and should therefore not bias our results: The share of children for whom we have information on labor market outcomes is similar for assigned (32.7%) and non-assigned (32.4%) mothers. However, since we observe the assignment status A_i and long-term outcomes for all children, we can provide a reduced form estimate based on the full sample. We analyze employment, occupation, and wages between 25 and 40 years of age.

Moreover, using the database from the *Upper Austrian Sickness Fund*, we are able to construct long-term 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

[21] Symmetric growth restricted fetuses have a proportionally small body, with small weight and length. The causes are genetic factors, maternal diseases, infections or other toxic environmental effects occurring in early gestation. Asymmetric growth restriction is associated with small weight but normal length and is typically caused by risk factors in the last phase of gestation (after week 32). Common risk factors are poor maternal nutrition, placental insufficiency, preeclampsia or chronic hypertension in late pregnancy (Lin and Santolaya-Forgas, 1998; Valsamakis et al., 2006). Approximately 70-80 percent of growth restricted newborns can be classified as asymmetrically growth restricted (Lin and Santolaya-Forgas, 1998).

[22] Two-thirds of children cannot be uniquely matched to their mother in the ASSD. This link in the administrative data was not comprehensively available for early cohorts. For these children we do not observe their mothers' employment status W_i nor their treatment M_i.

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 long-term outcomes are provided in Table A.3 in the Web Appendix.

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, as well as the number of subsequent births. Finally, we study mortality to capture effects on the mother's health. We construct two binary variables indicating whether the mother survived at least 20 and 40 years after giving birth. The average survival rates at these two points in time are 99% and 92%, respectively.

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.1 in the Web Appendix. As outcome variables we consider the birth weight in logs, a low birth weight indicator, asymmetric growth restriction, birth length in logs, and an indicator for a premature birth in columns (1) to (5). Across outcomes we find no significant effects of the reform. All point estimates are very close to zero and precisely estimated. This finding is not sensitive to the sample choice with respect to the window around the cutoff due date (see Figure A.2 in the Web Appendix).

[Table 2]

For comparison we provide naïve OLS estimates in Panel B of Table 2. Assuming the prenatal ML duration be exogenous, we simply regress each outcome on the mother's actual leave duration in weeks. To avoid capturing the reform's effect with our OLS estimates, we restrict the sample to births in the pre-treatment period. The OLS estimates comprise a potential causal effect as well as different sources of endogeneity. Regarding the latter, it is useful to distinguish between an early start date and a late end date of ML. An early start may either reflect a curative intervention due to diagnosed health problems in pregnancy, or a preventative intervention by risk-averse mothers and doctors. The former would lead to a negative association between ML duration and health at birth outcomes, while the effect of the latter is ambiguous. Assuming that health-conscious mothers have better health outcomes, the association would be positive. A late end date captures a mechanical relationship between a longer ML and a longer gestation. This contributes to a positive association between ML duration and health at birth.

We find statistically significant positive OLS estimates for all outcome variables except the asymmetric growth restriction. The source of this positive correlation between longer ML and health at birth is unclear and should not be interpreted causally. Since our RDD estimates show no significant effects, the OLS estimates are likely driven by endogeneity.

III.1.1. Interpretation of estimation results

When interpreting these results, we have to keep in mind that we cannot observe how assigned mothers in fact spend the additional free time obtained through the ML extension. This is not unique to our research design. Every paper analyzing ML (or parental leave) reforms shares the feature that estimated effects have a reduced-form character in this respect. It is informative to interpret the results in regards to the Austrian institutional setting. In our specific case of a *mandatory* leave, the problem is alleviated to some degree, since we should have only one-sided non-compliance (to borrow the language of RCTs). We expect assigned mothers to be compliant, in the sense that they do not work in the 33^{rd} and 34^{th} week of gestation. In contrast, non-assigned mothers may be non-compliant and do not work.

The institutional setting offers two possibilities for non-compliance. First, mothers can 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 an occupational physician of the *Labour Inspectorate*. This source of non-compliance is captured by our estimation strategy, since we use the assignment to instrument for the *actual* ML duration. Thus, this does not complicate the interpretation of the results. Second, expecting mothers — as any other employee — are always entitled to sick leave if supported by a medical certificate. This source of non-compliance is not captured by our estimation strategy, and potentially complicates the interpretation of our results. In an extreme scenario, a non-assigned mother could go on sick leave in her 33^{rd} week of pregnancy until her prenatal ML starts. Thus, she would *de facto* stop working 8 weeks before her due date, just as an assigned mother. Fortunately, we can assess the importance of sick leaves prior to prenatal ML for all blue-collar workers.[23] It turns out that only 5.1% of all non-assigned mothers were on sick leave prior to their ML. Most importantly, this share was very comparable for assigned mothers (4.2%). The difference between these two shares is not statistically significant (p = 0.1684, n = 3,962).

This clarifies the interpretation of our results. First, our LATE is most likely driven by coun-

[23]Sick workers receive their compensation from two sources: First, workers continue to receive their salaries from firms. Second, after a certain period, they receive also public sickness benefits. The ASSD contains only information on sick leaves once the public sickness benefits are paid (Halla et al., 2015). Until September 1974, blue-collar workers received public sickness benefits already after 5 days of sick leave. This allows us to observe their sick leave with little error. In contrast, white-collar workers received public sickness benefits, depending on their tenure, only after several weeks. Thus, for white-collar workers we observe only long sick leave spells.

terfactual comparisons of mothers without major problems in this stage of pregnancy. Second, treated mothers were (in contrast to non-treated mothers) indeed *not* exposed to work in the 33rd and 34th week of gestation. Thus, our LATE captures a reduction in the *in utero* exposure to maternal stress caused by work in the 33rd and 34th week of gestation for a group of mothers without major problems in pregnancy. We consider this *policy-relevant treatment effect* (Heckman and Vytlacil, 2001) to be informative not only for designing an optimal prenatal ML policy in Austria but also in other countries.

III.1.2. Complier characteristics and treatment effect heterogeneity

In this subsection, we first examine characteristics of those mothers who comply with our IV, and then study whether treatment effects vary along the socioeconomic spectrum. In Table 3, we summarize the average characteristics of the compliers (i.e., those mothers who increase their prenatal ML duration because of being assigned) along with the average characteristics of the full sample as comparison. We follow Angrist and Fernández-Val (2013) and compute ratios of first stages of subsamples that have the given characteristic to the overall first stage. Across all variables we find that compliers tend to have somewhat more favorable characteristics. For instance, compliers are more likely to be married at birth (by 2.6%), be above 24 years of age (5%), have a high income (7.3%), and be white-collar workers (9.6%).

In a next step, we explore potential treatment effect heterogeneity. We stratify our sample according to different characteristics of mothers 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 29 years and older) is not only interesting *per se*, but also allows us to infer on parity to some extent. 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 subsamples, along with our baseline estimates.[24] We focus on three outcome variables (birth weight, length, and premature birth). The general finding is a zero effect in each stratum. The same holds for the other outcome variables (not shown). The estimated treatment effects are (with one exception) all statistical insignificant. The widths of the 95% confidence intervals vary somewhat and are, as expected, larger for smaller subsamples. We conclude that the reform had no beneficial effects,

[24] Corresponding first stage estimates are summarized in Table A.1 in the Web Appendix.

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.[25] This allows us to subtract the estimated RDD effect for non-working mothers from the equivalent effect for working mothers, which will difference out any seasonal effects. The means of our observables are very similar across the two subsamples (see Table A.2 in the Web Appendix). Panel C of Table 2 summarizes estimation results from this alternative specification. The estimated effects are similar to those obtained by our RDD estimations above (see Panel A). All point estimates are precisely estimated and statistically insignificant.[26]

[Figure 5]

Panel D of Table 2 presents estimation results from a simple DiD specified 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.[27] In Figure 5 we compare all three different estimates graphically.

III.2. Children's long-term outcomes

In a next step we examine children's long-term outcomes. Despite the lack of evidence that the reform improved children's birth outcomes, one should not jump to the conclusion that the reform had no impact on children at all. Several medical and epidemiological studies postulate the subtle idea that events *in utero* might indeed affect the infant, but these effects remain latent for many years (Almond and Currie, 2011*b*). Panel A of Table 4 summarizes our RDD estimates for long-term human capital and health outcomes. In columns (1) to (3), we consider labor market outcomes at the age of 40. At this point in time, about 84% were in a regular employment, and among those, about 70% were employed as a white-collar worker. Average daily wages amounted to € 119. In the remaining columns we consider effects on health. In particular, we examine aggregate spending in the outpatient sector (column 4, in € 1,000), and

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

[26]The estimated first stage coefficient from equation (3), $\hat{\theta}_3$, is 1.586 (0.058). The Kleinbergen-Paap *rk* Wald F-statistic is 755.75, and the partial r^2 is 0.029.

[27]Note that 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.

the aggregate days spent in hospital (column 5). Both variables refer to the period in 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.

[Table 4]

The analysis of long-term outcomes confirms our conclusion derived from the analysis of birth outcomes. Across outcomes, we do not observe any economically or statistically significant effects of the reform. This applies to human capital outcomes as well as to health outcomes. To assess the robustness of these findings we also examine labor market outcomes at various stages over the life cycle. Figure 6 summarizes RDD estimates for our three outcome variables at the ages of 25, 30, 35 and 40. Again, neither estimate is economically or statistically significant.

[Figure 6]

As explained in Section II.3.2 (see footnote 22), we have fewer observations available for children's long-term outcomes due to a missing link between the ASSD and the ABR. Although our estimates are derived from sufficiently strong first stages (see Panel A of Table 4), we additionally provide reduced form estimates based on the full population of children in the ASSD. In Panel B of Table 4 we compare children born in April 1974 with those born in June 1974. For the analysis of labor market outcomes, we have now at least 11,000 observations. For our health outcomes, the number of observations has increased to almost 3,300. Across outcomes we find economically and statistically insignificant reduced form estimates. This analysis confirms our findings from Panel A.

III.3. Maternal outcomes

So far, we have provided evidence that the ML extension had no significant effects on children's outcomes, neither at the time of birth nor in the long-run. However, it is still possible that extended ML, while having no discernible effects on children, has improved the physiological or psychological well-being of mothers. A reduction in maternal stress prior to birth might have altered pregnancy and birth experiences for mothers, which in turn may have reduced the number of pregnancy complications, obstetric labor complications, or health problems in the postpartum period. Since we do not observe maternal health at the time of birth, we focus on long-term outcomes. We examine two informative outcome dimensions. First, we consider mothers' subsequent fertility behavior. If the extended leave has improved well-being of mothers, we would see an increased quantum or tempo of fertility. Mothers may either be more willing or more able to conceive and deliver a further child. Accordingly, we estimate the effect of the reform on completed fertility, and — conditional on having another birth — on the duration until the next birth. Second, we examine mothers' long-term health and study their mortality. In particular, we consider mothers' survival 20 and 40 years after birth.

[Table 5]

Panel A of Table 5 summarizes our RDD estimates for these maternal outcomes. Panel B provides OLS estimates for comparison, where the sample is again restricted to the pre-treatment period. In columns (1) to (3), we focus on fertility. About half of *all* mothers have at least one further birth, with an average duration to their next birth of about 4.36 years. The average total number of subsequent births is 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. For 20-year survival, we find a clear zero-effect. For 40-year survival, we obtain a marginally significant negative effect. This suggests that an additional week of ML decreases the probability of being alive after 40 years by 0.7 percentage points corresponding to a reduction of roughly 0.76% 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 7]

In Figure 7, we additionally study 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 births by roughly 0.1 due to the reform. Overall, we conclude that the ML reform in 1974 had no effects on maternal long-term health and fertility.

IV. Complementary cross-country evidence

We have shown that the increase in prenatal ML duration from 6 to 8 weeks in Austria had no discernible impact on children and mothers, neither at birth nor later on. Clearly, our findings must be interpreted taking into consideration the Austrian institutional setting. Austria has a comprehensive social security system and extensive employment protection. Nevertheless, we regard our estimates as informative for designing an ML system in general. Still, we provide additional evidence for the external validity of our findings. We complement our micro-data analysis with a cross-country study. If this exercise provided evidence for significant effects of ML durations beyond six weeks, this would question the external validity of our findings based on Austrian data. Section B of the Web Appendix discusses the cross-country study in detail, below we provide a brief summary.

We construct a sample of 17 countries that experienced one or more reforms in prenatal ML at different points in time between 1970 and 2010. Prenatal leave durations range from 0 to 8.7 weeks, with an average of about 5 weeks. Exploiting the variation in timing and extent of ML reforms across countries, we estimate country-level DiD models for a number of child and maternal outcomes (such as perinatal, neonatal, and maternal mortality, as well as fertility). Across outcomes, we find evidence for parallel trends before ML reforms. None of our DiD estimations yield statistically and economically significant effects for prenatal ML durations above 6 weeks. Our estimates are robust to various functional form specifications and sample stratifications. Thus, our cross-country study confirms the findings obtained by our micro-analysis based on Austrian data and supports its external validity.

V. Conclusions

We have analyzed the impact of a reform of the Austrian ML legislation in the year 1974, which has increased the compulsory prenatal ML duration from 6 to 8 weeks. The political justification for this reform was to improve the health of pregnant workers and their children. Extended leave was determined by a cutoff due date, which allows us to implement an RDD. Our LATE captures a reduction of the *in utero* exposure to reduced maternal stress caused by work in the 33rd and 34th week of gestation for a group of mothers without major problems in pregnancy. To our surprise, we find no evidence for a significant effect on children's health at birth or on their long-term health and human capital outcomes. Subsequent maternal health and fertility also remain unaffected. The estimated treatment effects are statistically insignificant and precisely estimated zero effects. This finding is consistent across subsamples of mothers. Thus, this reform has increased public spending on transfer payments by one-third and has restricted female workers in their freedom to work without producing any measurable benefits.

While our findings must be interpreted taking into consideration the prevailing Bismarckian healthcare system in Austria, we conclude more generally that mandatory prenatal ML starting in the 35th week of gestation is sufficient for pregnant workers without problems in pregnancy. It should be emphasized that we do not interpret our results as a general argument against (mandatory) prenatal ML. Quite the contrary, we consider our finding to be valuable for designing an optimal prenatal ML policy in places without comprehensive legislation in place. Finally, we suggest reassessing existing ML legislations with long compulsory durations and to reduce either the extent of obligation or the duration.

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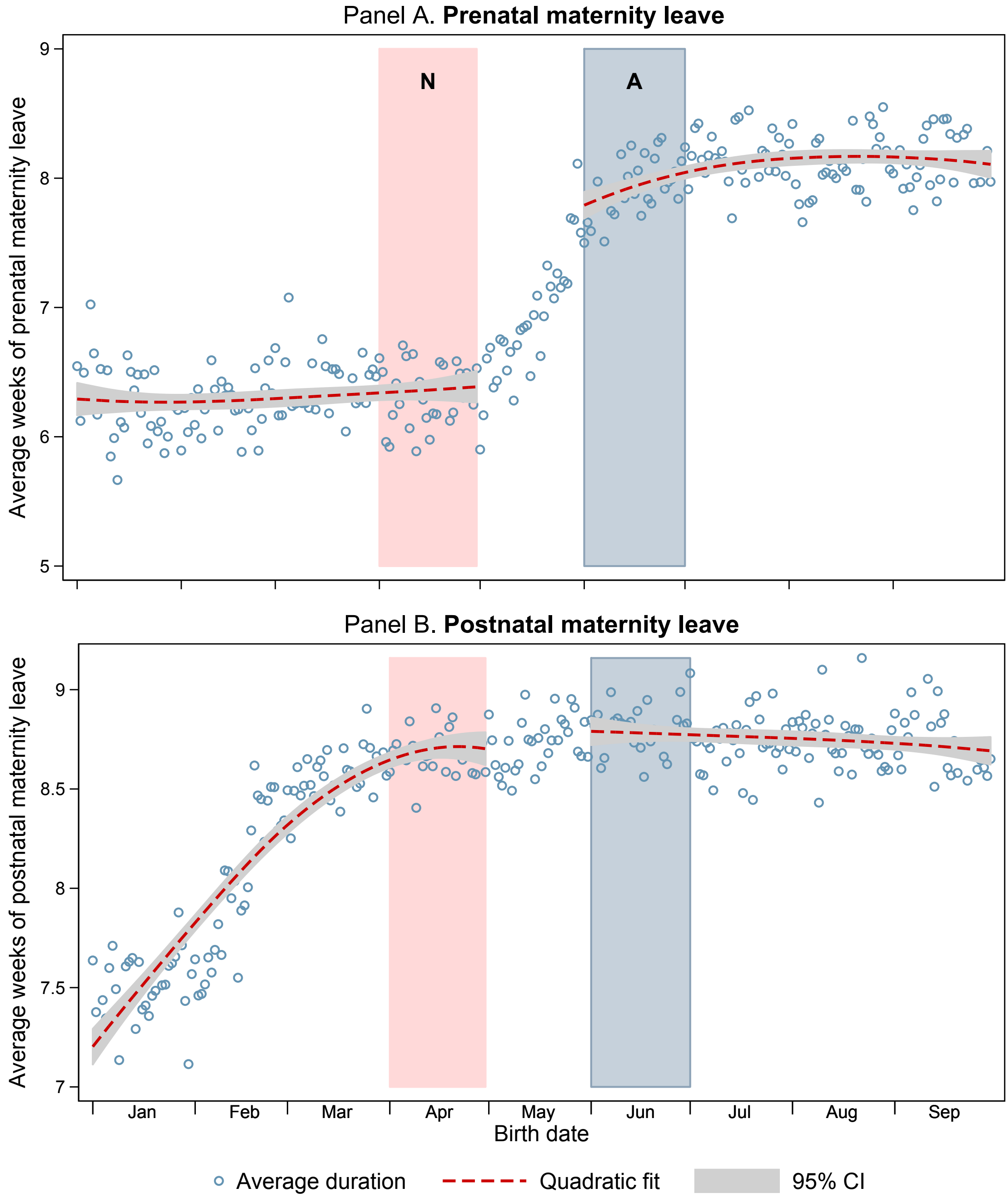
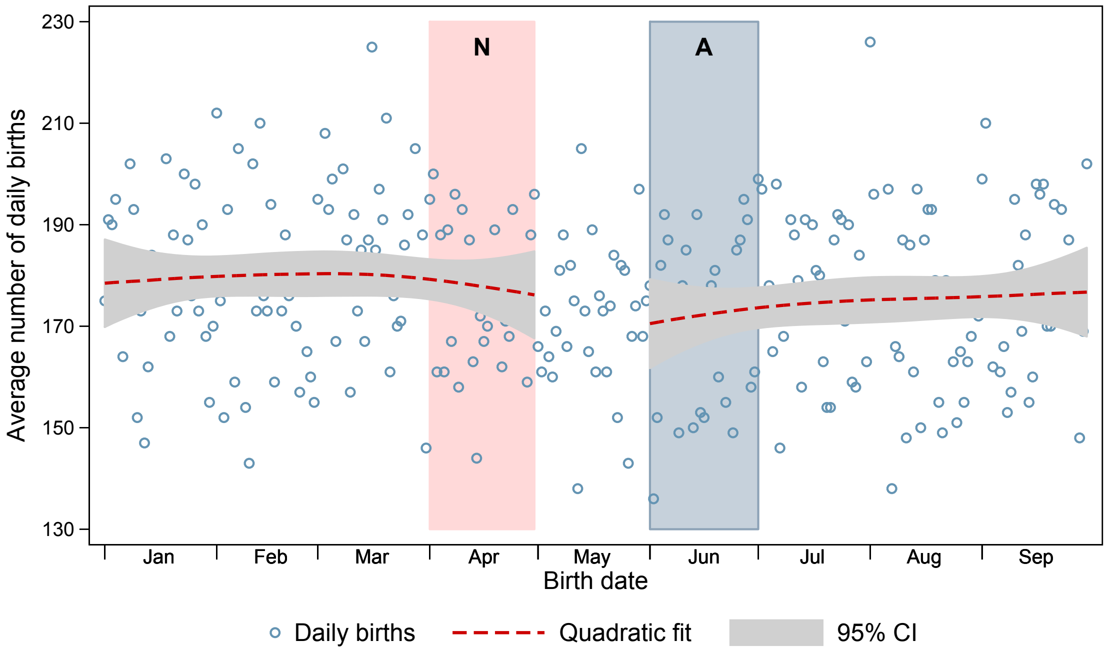
VI. Figures and tables (to be placed in the paper)

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



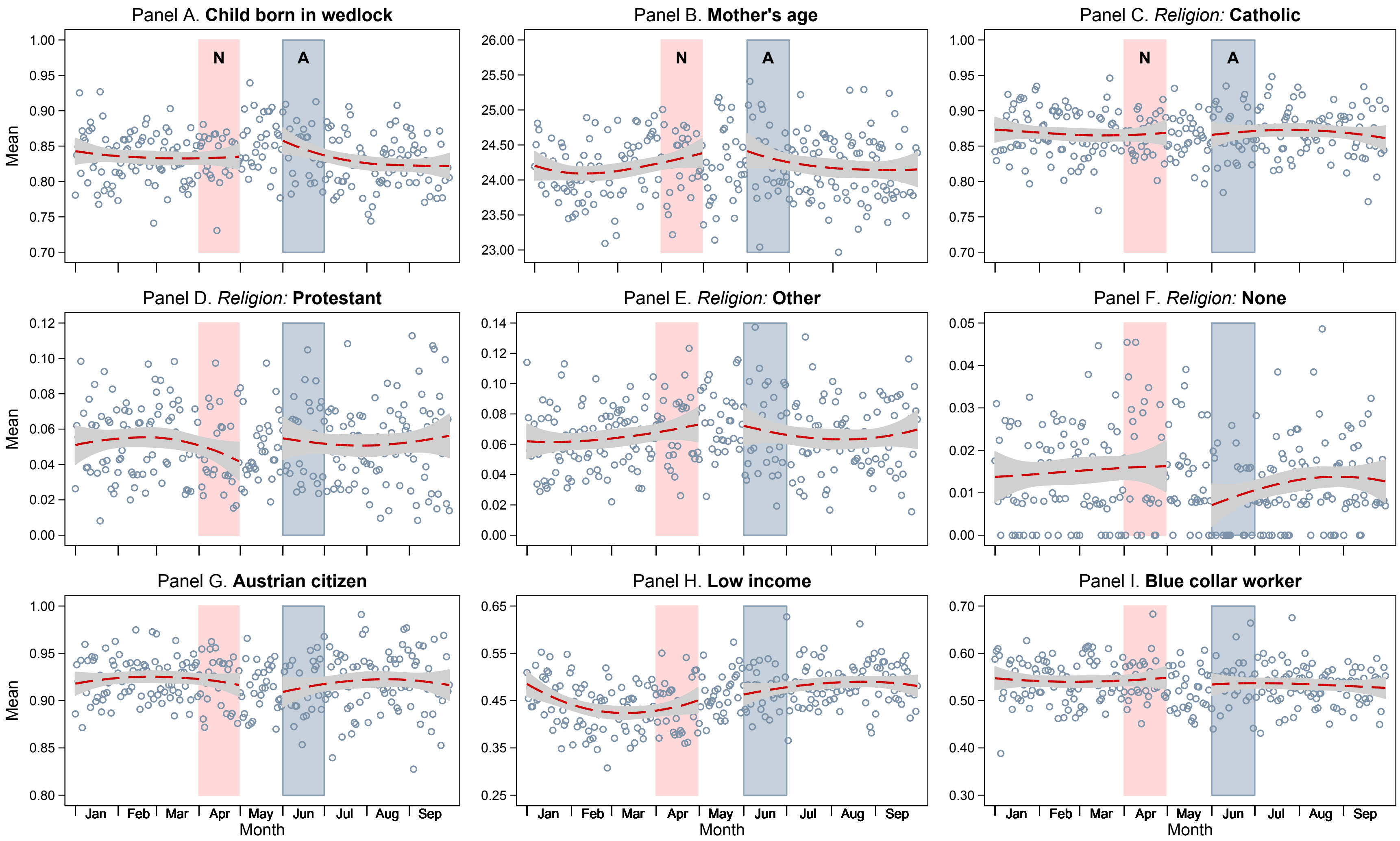
Notes: These graphs depict the average prenatal (Panel A) and postnatal (Panel B) ML durations by birth date of the child between January and September 1974. Separate quadratic fits for the pre-treatment (until April 30, 1974) and the post-treatment period (starting with June 1, 1974) are depicted by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

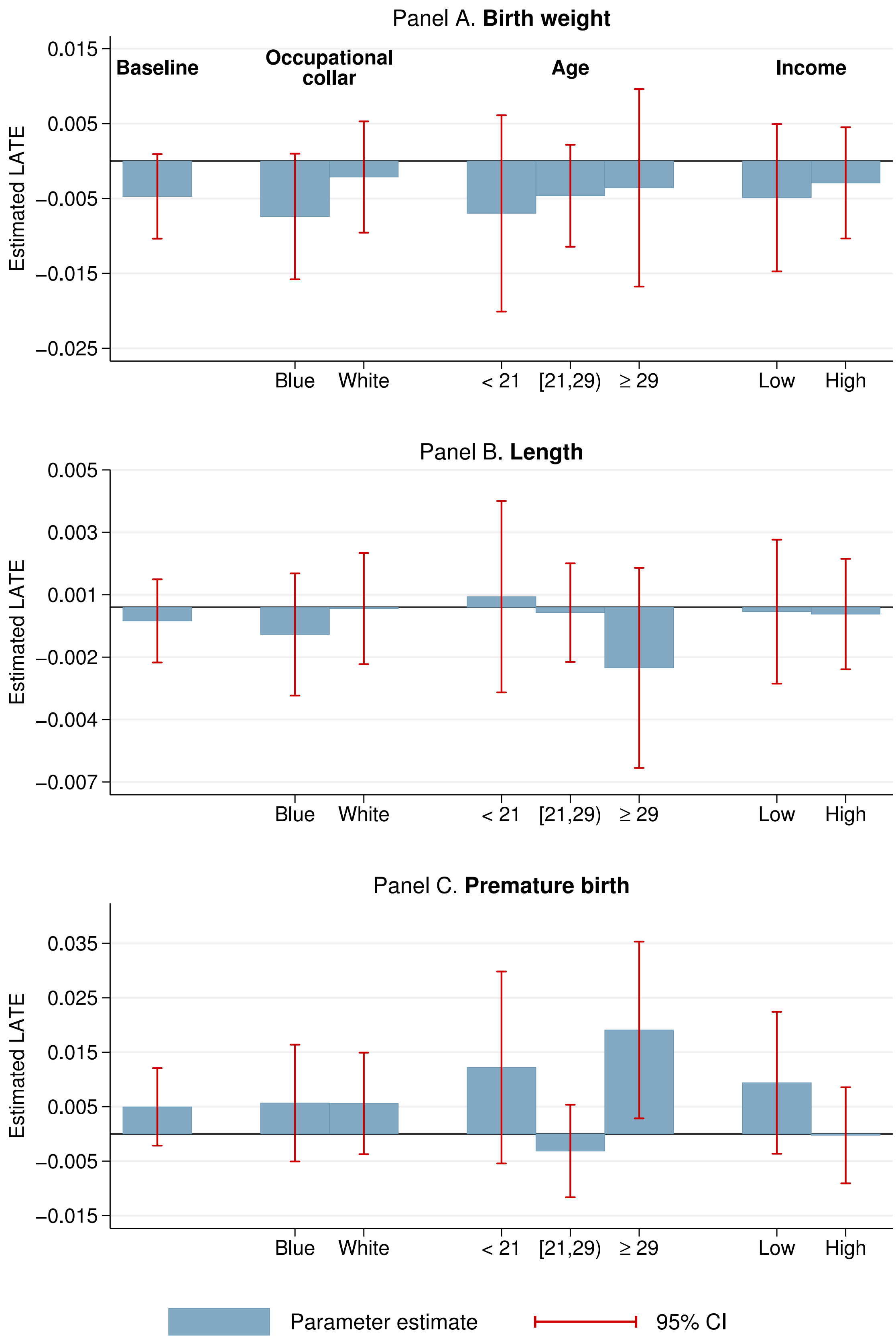
Table 1 — Descriptive statistics, assigned vs. non-assigned mothers.

	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
Asymmetric 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	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	0.91		0.0	1.0	3721	0.92		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.

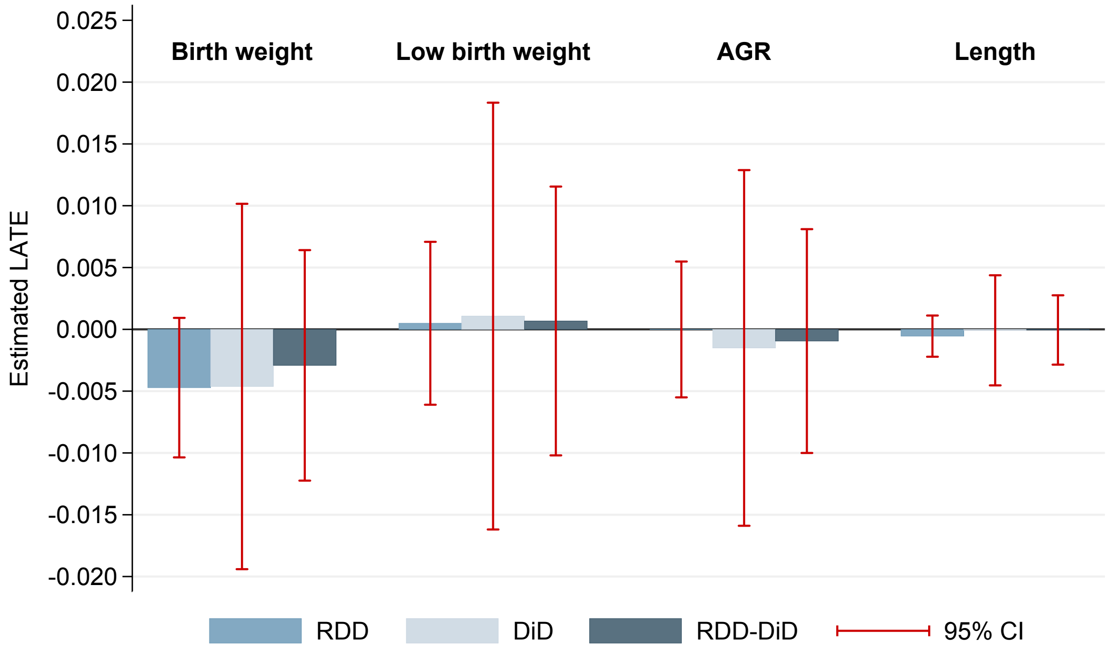
[a] Asymmetric growth restriction is defined as having low birth weight *and* a low Ponderal index ($PI = kg/m^3$).

FIGURE 4 — Heterogeneous treatment effects for 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 subsamples. 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.1 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 LATEs of extending the compulsory ML duration 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' and 'AGR' are binary variables indicating the probability of having birth weight below 2,500 grams and the probability of growth being asymmetrically restricted (i.e., low birth weight and Ponderal index being in the lowest quartile of its sample distribution), hence estimates can be interpreted as percentage point increases or decreases in the outcome induced by the treatment. Note that RDD and RDD-DiD coefficients correspond to a one week increase in ML, while DiD estimates indicate the LATE of a two week increase in ML.

Table 2 — Estimated treatment effects on health at birth outcomes

	(1) Birth weight	(2) Low birth weight	(3) Asymmetric growth restr.	(4) Length	(5) Premature birth†
Panel A. RDD					
Prenatal maternity leave	−0.005	0.000	−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	0.23	0.19	0.06	0.24
Kleinbergen-Paap rK Wald F-statistic	756.45	756.45	756.45	756.45	756.45
Panel B. OLS (only pre-treatment period)					
Prenatal maternity leave	0.006***	−0.006***	−0.002	0.001**	−0.011***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
No. of observations	3,721	3,721	3,721	3,721	3,721
Mean of outcome	5.77	0.06	0.04	3.92	0.06
Std. dev. of outcome	0.19	0.23	0.19	0.06	0.24
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	0.22	0.18	0.06	
Kleinbergen-Paap rK Wald F-statistic	755.75	755.75	755.75	755.75	
Panel D. DiD					
Assigned × working	−0.005	0.001	−0.002	−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	0.22	0.18	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 that extended compulsory leave by two weeks. Corresponding first stage estimates are summarized in Table A.1 in the Web Appendix. Panel B summarizes OLS estimates for the pre-treatment period, where the prenatal ML duration is used as an explanatory variable. Panel C are difference-in-differences estimates which compare pre- and post-reform outcomes between working and non-working mothers. In Panel D we combine these two sources of exogenous variation in regression discontinuity difference-in-differences estimators. In Panel A the sample consists of working mothers giving birth in April and June 1974, in panel B the sample is restricted to women giving birth in April 1974. In panels C and D we extend the sample from A with non-working mothers giving birth in April and June 1974. Each cell represents a separate estimation. The outcomes 'birth weight' and 'length' (columns 1 and 4) are continuous variables specified in logs, while 'low birth weight' (column 2), 'asymmetric 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 sample 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 the 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). Note that coefficients in panels A, B, and C correspond to a one week increase in ML, while coefficients in panel D indicate the LATE of a two week increase in ML. Robust standard errors are in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 — Complier characteristics

Characteristic	**Sample mean**	**Complier ratio**	**95% CI**
Born in wedlock	0.84	1.026	(1.003,1.048)
Catholic	0.87	1.015	(0.998,1.032)
Age at birth > 24	0.41	1.050	(1.007,1.091)
Austrian citizen	0.92	1.013	(1.000,1.026)
High income	0.50	1.073	(1.028,1.119)
White collar worker	0.45	1.096	(1.046,1.146)

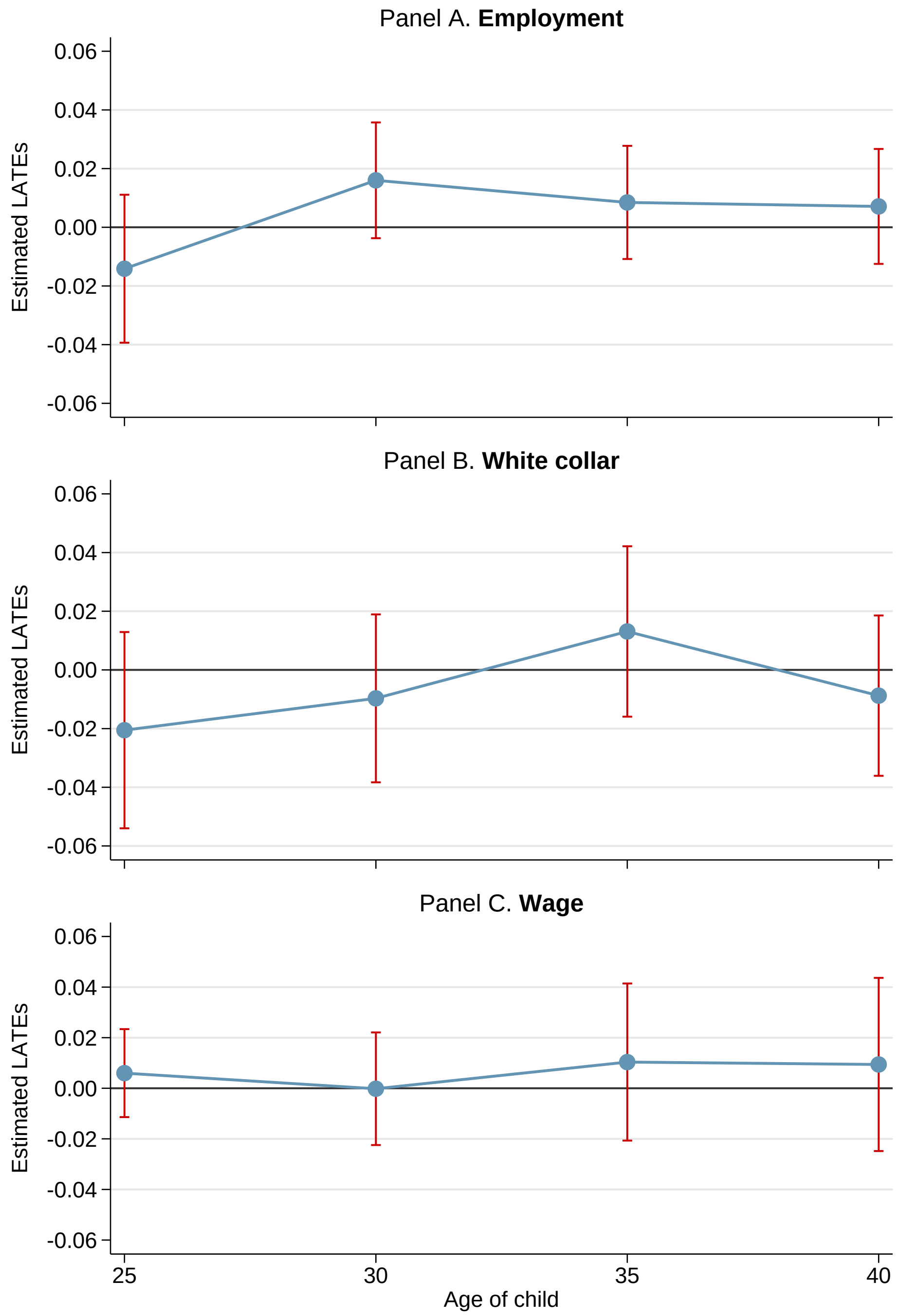
Notes: This table presents observed complier characteristics based on calculations proposed by Angrist and Fernández-Val (2013). The complier ratio is the relative likelihood that a complier has the given characteristic. It is derived as the ratio of the first stage for mothers with the given characteristic to the overall first stage (as in equation 1). We use here a first stage with a binarized running variable, which is equal to 1 if maternity leave was longer than 7 weeks (and 0 else). A ratio larger than 1 indicates that compliers are more likely to have the given characteristic. Standard errors are bootstrapped with 99 repetitions.

Table 4 — Estimated treatment effects on children's long-term outcomes

	Labour market outcomes at age 40			**Health outcomes between age 25 and 40**	
	(1)	(2)	(3)	(4)	(5)
	Employed	White collar	Wage	Outpatient expenses	Hospital days
Panel A. RDD LATEs					
Prenatal maternity leave	0.007	−0.006	0.009	−0.030	−1.381
	(0.010)	(0.014)	(0.017)	(0.130)	(1.382)
No. of observations	2,395	2,002	1,559	511	511
Mean of outcome	0.84	0.69	1.19	1.83	9.19
Std. dev. of outcome	0.37	0.46	0.52	2.34	23.90
Kleinbergen-Paap rK Wald F-statistic	206.11	177.66	131.32	49.39	49.39
Panel B. Reduced form estimates					
Born in June 1974	0.005	0.001	−0.001	0.092	−1.167
	(0.005)	(0.008)	(0.008)	(0.076)	(1.206)
No. of observations	15,450	13,838	11,023	3,287	3,287
Mean of outcome	0.90	0.54	1.02	2.30	13.43
Std. dev. of outcome	0.31	0.50	0.49	2.30	35.19

Notes: This table presents fuzzy RDD estimates of extending ML duration by two weeks on long-term child outcomes in Panel A., 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, 'white collar' is a binary variable indicating whether the child worked in a white-collar job at age 40, 'wage' is the daily wage in € 100 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. Additionally, in Panel B, we present results for the entire population of children born in April or June 1974 observed at age 40 in our data, irrespective of whether we can match them to their mothers as in Panel A. Regressing the respective outcome on a binary variable indicating whether the child was born in June 1974 (as opposed to April in the same year) gives us a reduced form estimate of the reform. In Panel B we only control for the child's sex, and whether the child could be identified in the birth register. Robust standard errors are in parentheses, stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 6 — Estimated treatment effects on children's labour market outcomes over the life cycle



Notes: This figure presents fuzzy RDD estimates of extending ML duration by two weeks on long-term labor market outcomes over the child's life cycle (at ages 25, 30, 35, and 40); where the respective outcome is regressed on prenatal ML duration (in weeks), instrumented by a reform assignment indicator. Each dot represents a separate regression. The sample in each regression 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 in Panel A is 'employed,' a binary variable indicating whether the child was in employment at a certain age. The outcome in Panel B is 'white collar,' a binary variable indicating whether the child worked in a white-collar job at a certain age. The outcome in Panel C is 'wage,' the daily wage in €100 at a certain age. 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). Error bars indicate the 95 percent confidence interval and are based on robust standard errors.

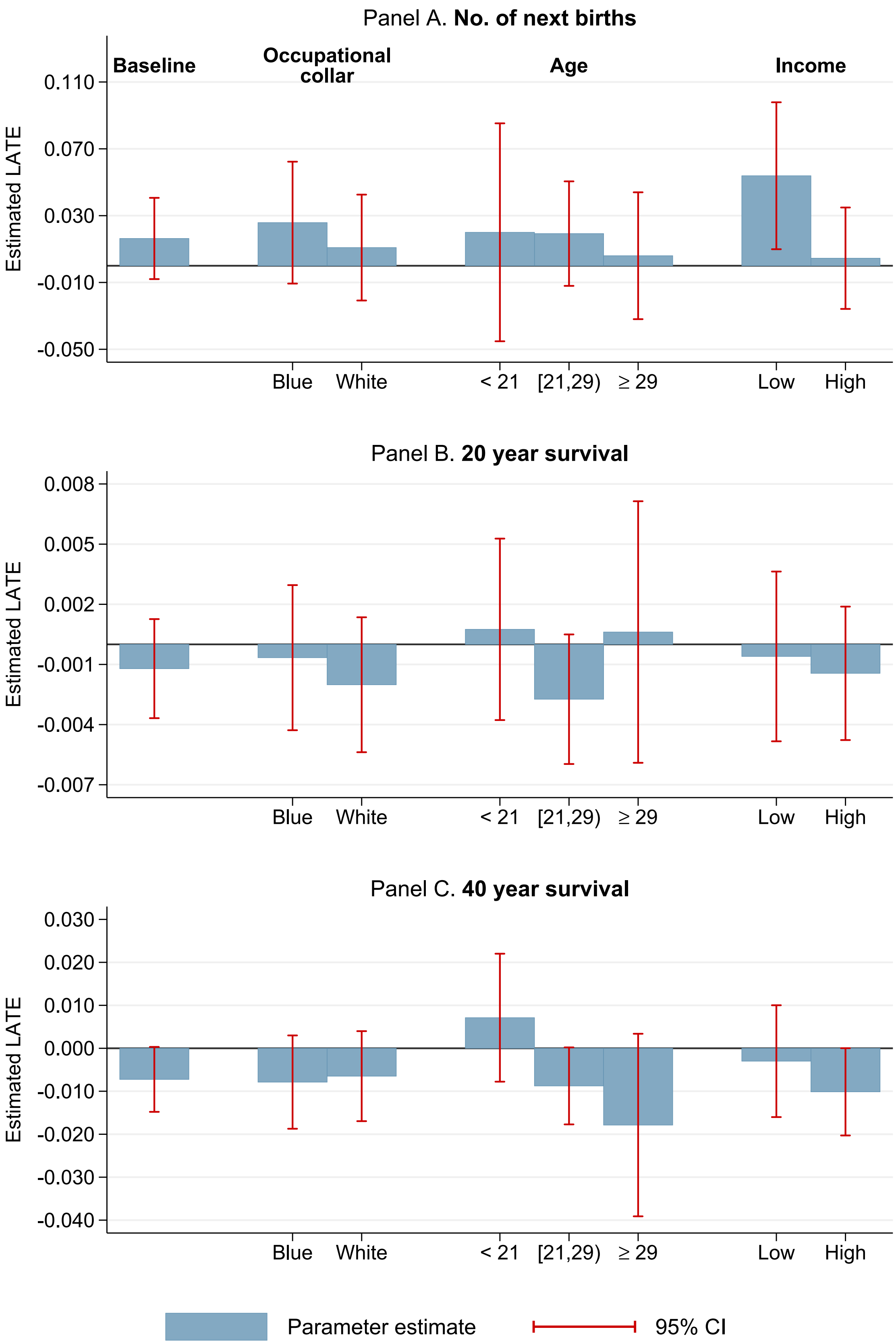
Table 5 — Estimated treatment effects on subsequent maternal outcomes

	(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	0.016	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	0.50	0.73	0.09	0.27
Kleinbergen-Paap rK Wald F-statistic	756.45	756.45	366.15	756.45	756.45
Panel B. OLS (only pre-treatment period)					
Prenatal maternity leave	0.003	0.003	−0.010	−0.000	−0.001
	(0.005)	(0.003)	(0.008)	(0.000)	(0.002)
No. of observations	3,721	3,721	1,816	3,721	3,721
Mean of outcome	0.69	0.49	7.11	0.99	0.93
Std. dev. of outcome	0.87	0.50	0.74	0.08	0.26

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 in Panel A consists of working mothers giving birth in April and June 1974, in panel B the sample is restricted to women giving birth in April 1974. 'No. of next births' (column 1) is a count variable measuring the number of children the mother has given birth to subsequently, 'further birth' (column 2) is a binary variable indicating whether the mother gave birth at least one more time, and 'time to next birth' (column 3) is the number of days passed until the mother gave birth again in logs, conditional on having another child. The outcomes '20 year survival' and '40 year survival' (columns 4 and 5) are binary variables indicating whether the mother was still alive 20 and 40 years after birth, 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, 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 7 — Heterogeneous treatment effects for 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 subsamples. 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.1 in the Web Appendix. Further details are provided in the notes to Table 5.

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. Appendix A provides additional tables and figures, and appendix B covers further details on our cross-country analysis.

A. Additional Tables and Figures

Table A.1 — First-stage regressions

	Coef.	Std. err.	F-statistic†	Shea's $r^{2\ddagger}$
Baseline	1.589***	(0.058)	756.4	0.093
Restricted samples				
Mother's age				
< 21	1.340***	(0.106)	160.9	0.077
$[21, 29)$	1.670***	(0.079)	444.4	0.101
≥ 29	1.723***	(0.140)	152.5	0.097
Income				
Low	1.503***	(0.081)	340.8	0.086
High	1.685***	(0.074)	521.9	0.109
Occupation				
Blue collar	1.525***	(0.079)	373.2	0.088
White collar	1.675***	(0.086)	375.4	0.100

Notes: This table 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.2 — 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
Asymmetric 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	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	0.92		0.0	1.0	3074	0.98		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.

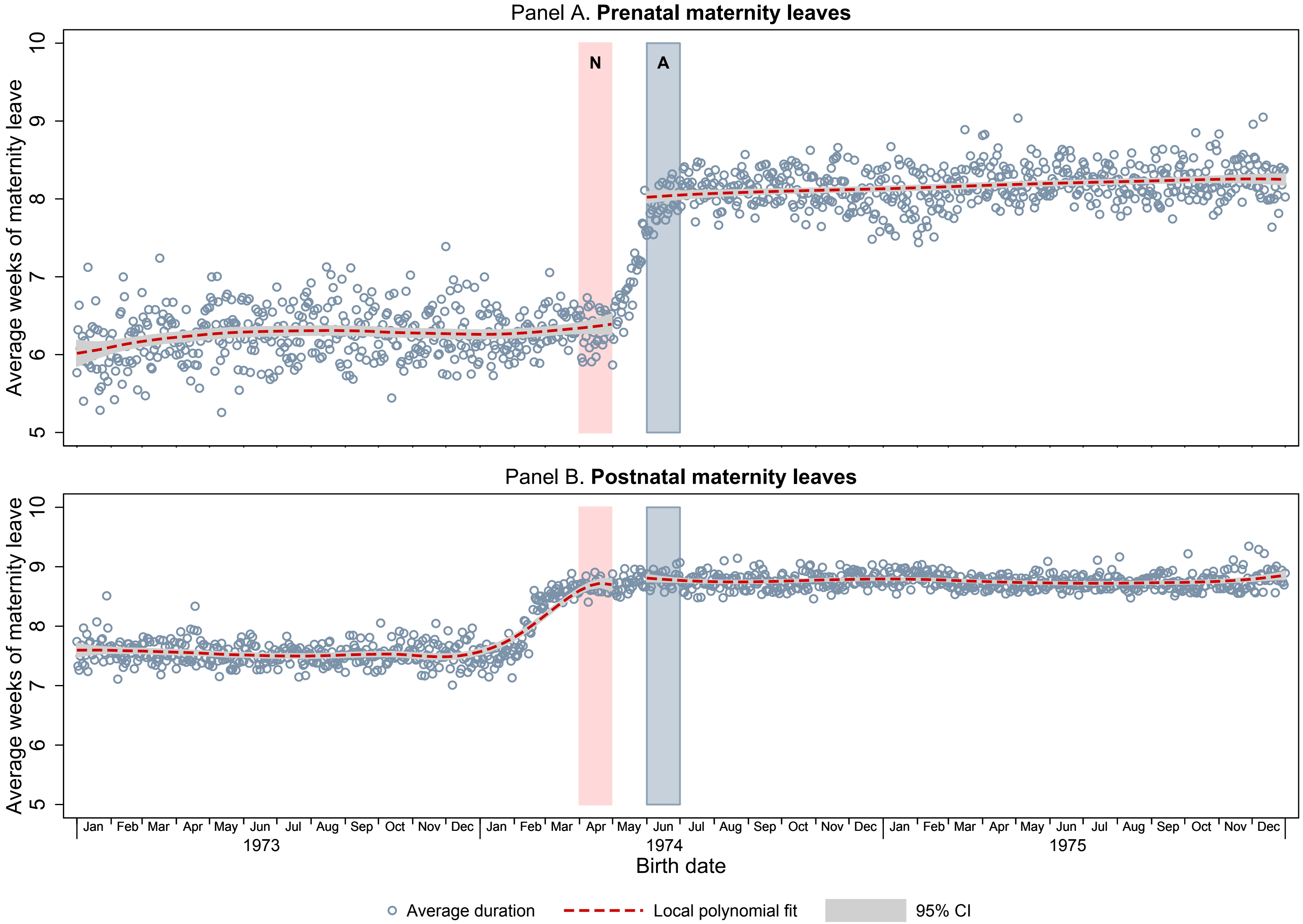
[a] ML duration is assumed to be 40 weeks for non-working mothers. The specific value chosen has no impact on the estimation results.
[b] Asymmetric growth restriction is defined as the probability of having low birth weight *and* having a low Ponderal index ($PI = kg/m^3$).

TABLE A.3 — Summary statistics for children's long-term outcomes

	Assigned mothers		**Non-assigned mothers**	
	Mean	N	Mean	N
Employed at age 40	0.84	1189	0.83	1206
White collar employee at age 40	0.69	1000	0.69	1002
Daily wage at age 40 (in € 100)	1.20	781	1.19	778
Agg. physician expenses b/w age 25–40 (in € 1,000)	1.81	255	1.86	256
Agg. hospital days b/w age 25–40	8.13	255	10.26	256

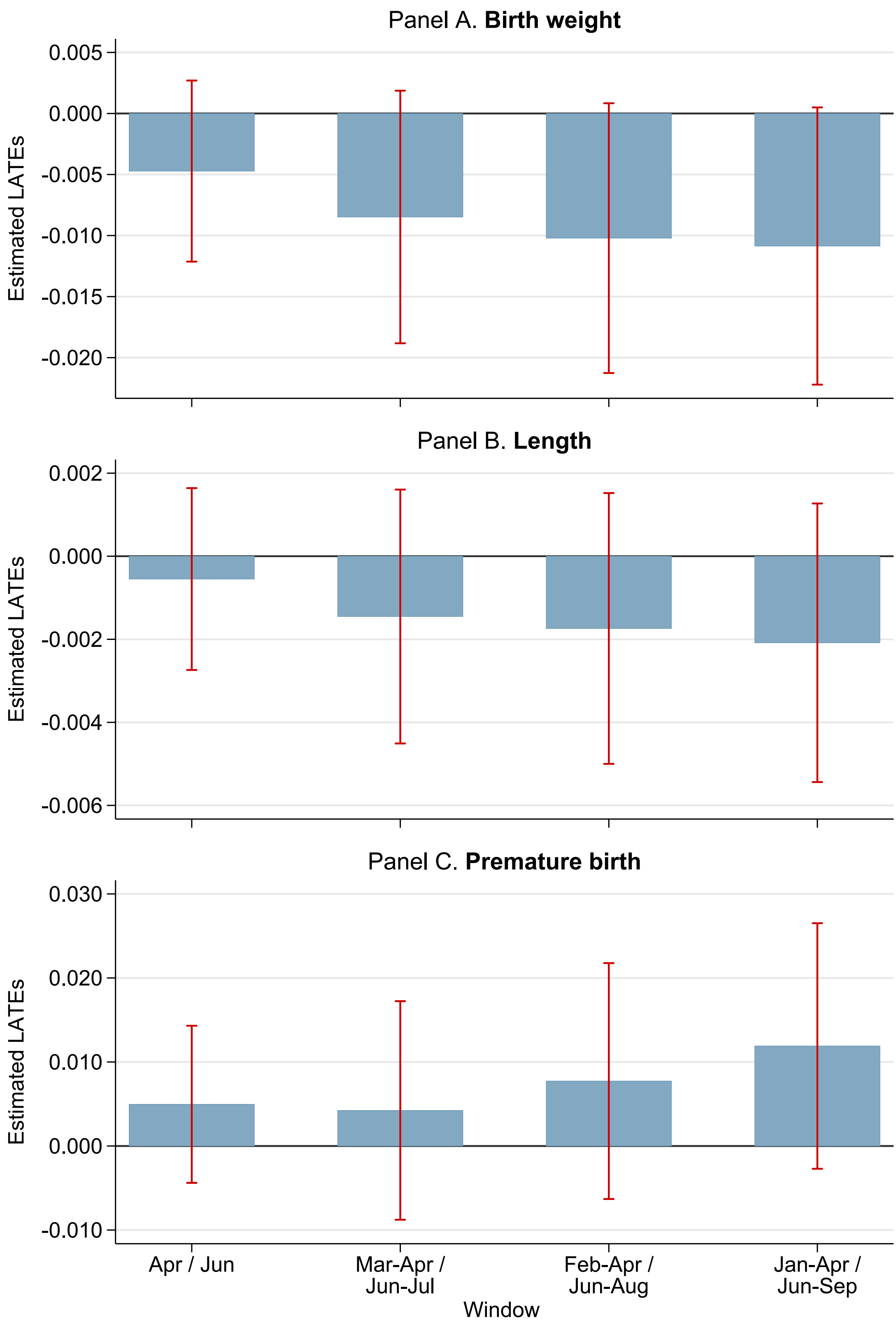
Notes: This table provides summary statistics for our long-term child outcomes, separately for children of assigned mothers (born in June 1974) and non-assigned mothers (born in April 1974). The samples for each variable consist of children of mothers who were working at time of birth, who could uniquely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

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 not assigned births ('N'), which we use in our estimation analysis. These mothers were eligible for 6 weeks of prenatal ML duration. The framed blue-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. 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.

FIGURE A.2 — Health at birth RDD estimates with wider windows around the cutoff due date.



Notes: In this graph we summarize the results of performing our RDD estimations on different samples based on the window around the cutoff due date we consider. The leftmost bar (*'Apr / Jun'*) resembles our baseline results from Table 2, where we essentially compare mothers giving birth in June 1974 to mothers giving in April 1974. For the estimate depicted by the second bar from the left (*'Mar–Apr / Jun–Jul'*) we extend the window by one month on each side of the cutoff, comparing mothers giving birth in June and July 1974 to those giving birth in March and April 1974. In order to obtain the estimates depicted by the next two bars, we continue to extend the window by another 1 and 2 months, respectively, on each side of the cutoff. Similar graphs for other outcomes we consider are available upon request.

B. Cross-country analysis

In the paper 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 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. Austria has a comprehensive social insurance system with very good health care and extensive employment protection. In case of health problems during pregnancy, working women are entitled to sick leave or early ML. A second drawback of our micro analysis is that we are only able to compare effects of 8 relative to 6 weeks of prenatal leave. We cannot easily generalize our findings to countries with other initial durations of prenatal leave, 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.

B.1. Data

We compile country-level data from the *Organisation for Economic Cooperation and Development Family Database* (OECD, 2016), the *Comparative Family Policy Database* (Gauthier, 2011) and the *Penn World Table Database* (Feenstra et al., 2015). We consider 17 countries that experienced a reform of 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 B.1, we plot the evolution of prenatal leave durations for each country separately over time. The notes to Figure B.1 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.

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.[1] We also check for any effects of prenatal ML on the level of fertility and consider the total fertility rate as an additional dependent variable. Information on exact definitions of these variables is provided along with summary statistics in Table B.1. 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, female and male labor force participation, share of employment across sectors, GDP per capita, average education, share of labor compensation in GDP, and the average hours worked (see Table B.1).

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

B.2. DiD framework and estimation results

B.2.1. Flexible DiD framework

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

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

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. 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}.[2]

Our parameter of main interest is $\varphi_{\mathrm{cc},r}$, 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 B.2 plots the estimated coefficients on $\varphi_{\mathrm{cc},r}$, 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 country-specific cubic time trends (i. e., we set $p = 3$ in model 6). For the outcome variables perinatal, neonatal and maternal mortality as well as the fertility rate, we do not find differences in the pre-treatment trends before reforms ($-9 < r < 0$), neither for ML expansions nor for ML reductions. Studying these outcomes, we feel confident in imposing the parallel trend assumption. However, in the sample of ML expansions, pre-treatment trends in low birth weight are negative and statistically significant already 6 years prior to the reforms, indicating a violation of the parallel trend assumption.

Figure B.2 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

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

narrow in the first couple of years after the reform and widen a bit thereafter. The only outcome for which we find significant effects is low birth weight. We find that the share of children born with low birth weight decreases after leave expansions. These estimates have to be interpreted with caution since we observe a downward trend already in the pre-reform period. We therefore cast doubt on the validity of the parallel trend assumption for this particular outcome.

B.2.2. Alternative specifications

In Table B.2, we explore further specifications. 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 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 effects on child health and maternal health. 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 binary variables indicating whether the country has a high (above the median) or low (below or at the median) female labor force participation rate. We do not find any robust evidence for an impact on health outcomes.[3] In Panels D and E, we apply equivalent specifications 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.[4] The only outcome variable for which we observe some significant coefficients, is the total fertility rate (see Panels B and C, column 5). These effects are quantitatively of minor importance and hard to rationalize.[5]

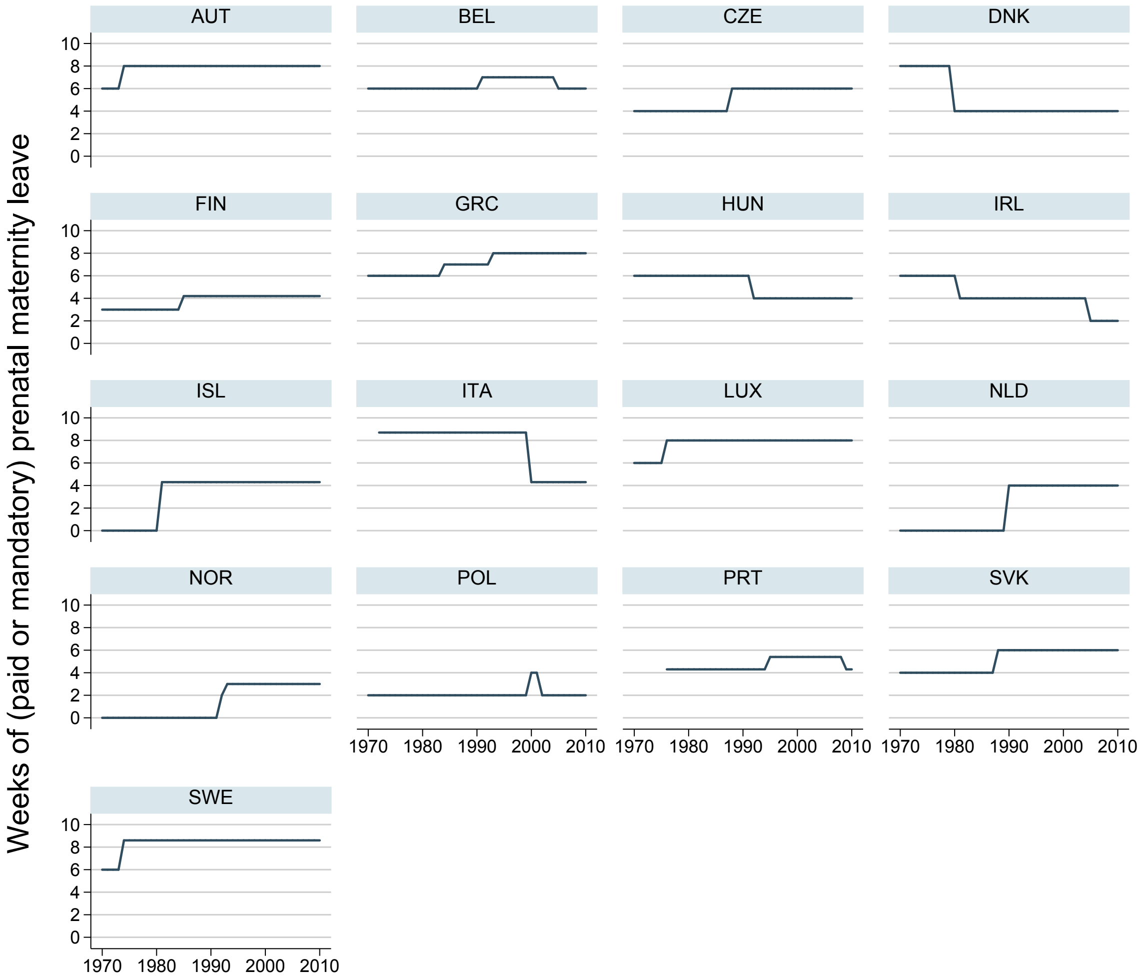
B.3. Corresponding Figures and Tables

[3]In countries with a female LFP above the median, effects are statistically zero throughout. For countries with a female LFP below or at the median, 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 about 0.03 standard deviations. Thus, the economic relevance of this estimate is negligible.

[4]When stratifying countries by GDP per capita, prenatal ML seems to reduce the number of children born with low birth weight by 0.073 percentage points. This effect is small and, as discussed above, the parallel trend assumption might be violated for this particular outcome.

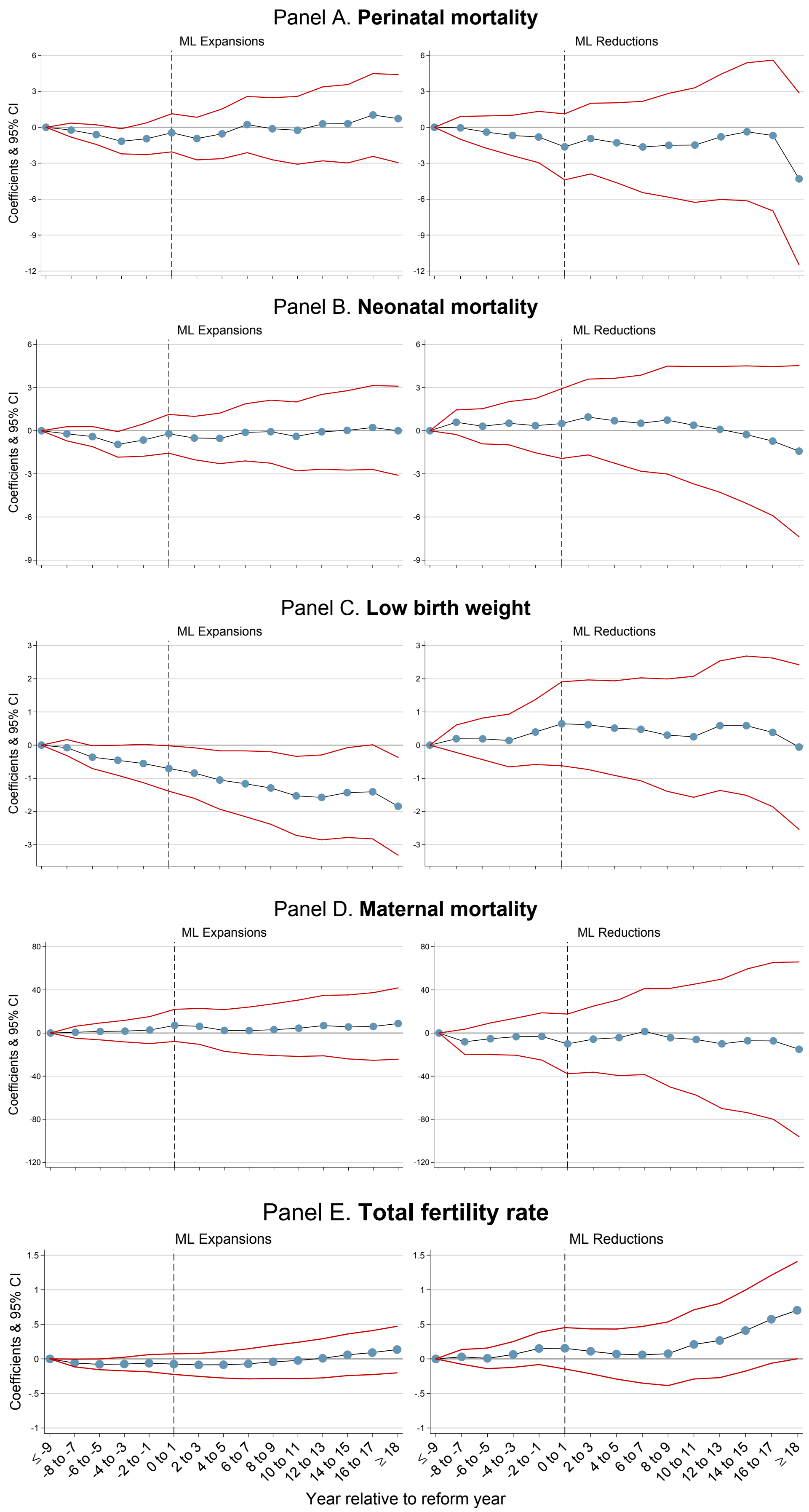
[5]In Panel B, we estimate a 0.086 reduction in children per woman for prenatal ML durations above 6 weeks, and a 0.115 reduction for durations below 6 weeks. This suggests that fertility is highest if the prenatal ML duration is *exactly* six weeks. In Panel C, we find that in countries with a low female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

Figure B.1 — 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 as follows. *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 was 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 B.2 — Cross-country analysis: DiD estimation results with pre-treatment trends



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

Table B.1 — 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 $\leq$ 14	Share of inhabitants aged 0-14	689	0.20	0.04	0.1	0.3
Population aged $\geq$ 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. Data are drawn from the Organisation for Economic Cooperation and Development Family Database (OECD, 2016), the Comparative Family Policy Database (Gauthier, 2011) and the Penn World Table Database (Feenstra et al., 2015).

Table B.2 — 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 D. 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 E. 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 B.1, 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.