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The Effects of a Paid Parental Leave Policy Change on Maternal Labor Market Outcomes: Evidence From Germany

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Abstract

This paper documents maternal wage dynamics around childbirth and its heterogeneity by education using German administrative data. Women with low education experience a smaller drop and a faster recovery of their wages than women with higher levels of education. These differences are largely explained by the fact that women with low education have their first child at a younger age. To investigate whether public policies can influence these wage dynamics, Virágh exploits a paid family leave policy change in 2007 to set up a differences-in-regression-discontinuities design. The policy change shortened the duration of monthly benefit receipt, increased the amount of monthly transfers, and encouraged secondary caregivers to spend at least two months on leave. She finds suggestive evidence that the policy did not have an effect on wage loss after childbirth for women with high education. Women with low education earn a larger share of their pre-birth wage in the second year after childbirth under the new policy regime. This is most likely because they returned to work faster. Virágh's results suggest that the policy change only influenced labor market behavior of mothers with economic constraints.

Introduction

Despite convergence over the past several decades, differences in labor market participation and wages between men and women still exist (Olivetti & Petrongolo, 2016). Among the traditional explanations are the fact that women spend more time on non-paid household activities and child care, they prefer more flexible work arrangements, and work in different occupations than men (Cortes & Pan, 2020). More recently, this literature has been focusing on the role of children in explaining the remaining gender gaps. After the arrival of children, mothers, compared to men or to childless women, experience a sizeable drop in earnings, as well as a decrease in hours worked and employment. This is often called the "child penalty" and has been well established across a wide range of countries (Aguilar-Gomez et al., 2019; Andresen & Nix, 2021; Bertrand et al., 2010; Kleven, Landais, & Søgaard, 2019; Kleven, Landais, Posch, et al., 2019). An important question is whether and to what extent public policies can mitigate the effects of having children on maternal labor market outcomes.

Parental leave policies can support parental employment after childbirth or adoption. They provide time off of work to care for children and at the same time, they allow caregivers to stay employed and to return to their pre-child jobs once their leave ends. Many countries pay benefits to caregivers on leave to protect their income. While parental leave policies often allow either parent to take time off of work, mothers are more likely to take leave (Bana et al., 2018; Bünning, 2015).

There is a large literature on the effects of parental leave policies on female labor market attachment (recent summaries are provided by Olivetti & Petrongolo, 2017; Rossin-Slater, 2018). In general, papers find a relatively small short-term effect on maternal employment (0-2 years after childbirth), and no change in labor market behavior on average in the long-term (starting 2-3 years after having children and observing behavior up to 10 years). Both extensions of job protected leave and increased benefit payments encouraged women to return to the labor market later, but the effects were small in magnitude in Austria (Lalive et al., 2014; Lalive & Zweimüller, 2009), in Germany (Schönberg & Ludsteck, 2014), and in Sweden (Ginja et al., 2020). Moreover, parental leave policy changes did not reduce the "child penalty" in Austria (Kleven et al., 2020) or in Norway (Andresen & Nix, 2022).

Two potential explanations for the small and null effects of paid leave policy changes on maternal labor market outcomes are (1) strong preferences about time allocation between market work and home production; and (2) the presence of heterogeneous effects. Most papers do not analyze policy effects on sub-groups of mothers, and while they find no impact on average, there might be differences in labor market responses based on budgetary or institutional constraints families face. For example, higher-income mothers may face lower pressure to return to the labor market to protect household income, or lower-income mothers could have lower access to child care which prevents them from working outside the home. There is some evidence that mothers with different education and pre-birth income levels respond to policies differently. In a cross-country analysis of a sample of Organisation for Economic Co-operation and Development (OECD) members, Olivetti and Petrongolo (2017) find that correlations between the number of weeks of job protected leave and female employment rate are only statistically significantly different from 0 for women with the lowest level of education. However, other papers find similar responses in labor market behavior to paid leave extensions across income groups (Ginja et al., 2020; Lalive & Zweimüller, 2009).

The current paper expands our understanding about the heterogeneous effects of childbirth and paid leave policy changes on maternal labor market outcomes. I investigate maternal wage dynamics around childbirth and analyze the impact of a 2007 policy change in Germany on maternal wages by level of education. Multiple aspects of the policy supported female labor force participation following childbirth, but it influenced families differently based on pre-birth income (Huebener et al., 2016). The new policy regime reduced the length of transfer receipt from 24 to 12 months. It also increased the transfer amount. The prior regime provided a means-tested 300-euro monthly transfer to eligible families, while the new regime introduced a 2/3 replacement of pre-birth earnings and stopped means-testing. This meant that lower-income families were made worse off and higher income families were made better-off in terms of income during leave (Huebener et al., 2019). These differences may have led to heterogeneous effects by pre-birth income. Since family income and education are highly correlated, I use maternal education at birth as a proxy for income.

I first document maternal wage dynamics around childbirth using administrative data from Germany. I replicate trends from prior papers that used survey data for the overall population (Kleven, Landais, Posch, et al., 2019), and provide new findings on sub-groups by educational attainment. My dataset covers a 2% random sample of individuals who paid social security contributions, which is about 2 million people, and includes their complete labor market histories since 1975. I identify mothers with a first birth between 2003-2007 and use an event-study design to describe their wages 5 years before to 10 years after childbirth. Mothers experience a 53% drop in their raw earnings in the first year after childbirth compared to the year right before childbirth, and they only recover about half of this loss by 10 years after childbirth. However, once I account for age and year effects, this recovery disappears, and maternal wages remain at around 60% of their pre-birth levels throughout the observed 10-year post-birth period. Mothers with no vocational training or university diploma (mothers with low education) have a faster recovery of earnings. They start to earn as much as they did before childbirth by 8 years after childbirth. Mothers with at least a university degree (mothers with high education) and mothers who have vocational training (mothers with mid-level education) have similar wage dynamics to that of the overall sample. Post-birth dynamics including age and year effects are similar for all four groups (all mothers, and mothers with all three levels of education).. This indicates that the increasing wage post-childbirth is mainly due to age and economic growth. The faster recovery for mothers with low levels of education is mostly explained by the fact that they are younger when they have children.

In the second half of the paper, I analyze whether a 2007 change in the paid parental leave policy of Germany had an effect on maternal wage dynamics around childbirth. The new policy came into effect on January 1, 2007. I employ a differences-in-regression-discontinuities framework to analyze the effect of the policy on maternal wages. I compare mothers who gave birth in the last quarter (October-December) of 2006, who were subject to the old policy regime, to mothers who gave birth in the first quarter of 2007 (January-March), who were eligible for the new benefits. Because of the seasonality of births around January (Buckles & Hungerman, 2013; Currie & Schwandt, 2013), I use the same two quarters in prior years (2003, 2004, and 2005) to account for seasonal differences in demographic characteristics of mothers who gave birth at the end of the year versus those in the beginning of the year. My main outcome of interest is the change in earnings loss mothers experience following childbirth. I find suggestive evidence that maternal wages fall by less under the new policy regime than they did under the old policy regime, but the point estimates are imprecise. This downward trend is true up to 15 months after the arrival of children. The results are driven by mothers with low education. I find no effect of the policy on wage loss of mothers who have a high level of education.

This paper contributes to two strands of the literature. First, it adds to our understanding about maternal wage dynamics around childbirth (often called the "child penalty"). Several papers across a range of countries document a large and immediate decrease in female wages, hours worked, and participation following childbirth, with no convergence between men and women even several years after the birth of children (Aguilar-Gomez et al., 2019; Andresen & Nix, 2021; Kleven, Landais, Posch, et al., 2019; Sandler & Szembrot, 2019). Kleven et al. (2019) estimate a long-run child penalty of 61% for women in Germany using the Socio-Economic Panel survey and births between 1985-2003. I bring more descriptive evidence on maternal wage dynamics in Germany by using an administrative data source for births between 2003-2007. This dataset provides a larger sample size and is less susceptible to misreporting of income. I also document trends for women with different levels of education. It is important to understand heterogeneities in wage dynamics as they can help understand differences in policy impacts.

Second, my paper extends analyses of parental leave policy effects on maternal labor market participation by focusing on women with different levels of education. In general, parental leave shorter than 1 year encourages women to stay employed and is beneficial for female labor force participation. Longer leave tends to reduce long-term employment of women (Rossin-Slater, 2018). Extension of leave duration and increase in the number of months parents receive transfers for while on leave lengthens the amount of time mothers stay home in Austria, but only to a small extent and in the short-run (Lalive et al., 2014; Lalive & Zweimüller, 2009). Women have similar behaviors regardless of whether their incomes are below or above the median before childbirth. Furthermore, only 20-25% of women return to work when their leave ends in the Austrian sample (Lalive & Zweimüller, 2009). Findings are qualitatively similar in Germany. Schonberg & Ludsteck (2014) estimate a 1-month extension of time at home after childbirth when benefit receipt increased by 4 months, and a 3-month extension with a 16-month increase in benefit receipt. These papers suggest that it is difficult to change maternal labor market behavior by paid leave policies, at least on the aggregate level. Mothers may have relatively strong preferences about market work and home production that cannot be altered by policy changes, or social norms could be highly internalized around caregiving. Lack of child care to enable parental work outside the home can also be an explanation for the small effects of paid leave on employment.

An active strand of the “child penalty” literature investigates whether public policies like parental leave or the provision of child care can influence parental wage changes around childbirth. Parental leave policy changes have been found to have no effect on maternal earnings in Austria (Kleven et al., 2020). In Norway, a family leave policy change that encouraged both mothers and fathers to take time off of work to care for their children had no effect on parental earnings, either (Andresen & Nix, 2022). In this Norwegian context, the provision of public child care led to a reduction in the child penalty for mothers in the short-run (Andresen & Nix, 2022). In Germany, mothers who lived in counties with low public child care provision experienced a larger drop in their earnings following childbirth than mothers in counties with a high provision of child care (Chhaochharia et al., 2020). The current paper contributes to this literature on policy effects on the “child penalty” the following ways. First, I generate the outcome of wage loss after childbirth at a monthly frequency. Most prior papers use annual data, which can mask important insights into wage dynamics. Monthly wage dynamics may be especially important given that prior papers found changes in return to the labor market at the monthly level. I also analyze whether the policy change had heterogeneous effects on wage loss by education. Women with different levels of education tend to experience different career and family life trajectories, as well as different constraints, so paid leave policies could have different impacts on their wage dynamics. It is important to understand these differences to refine existing policies to better serve families with young children.

Institutional background

In Germany, maternity leave is available for mothers, which starts 6 weeks before the due date of the child and lasts for 8 weeks after childbirth (Bergemann & Riphahn, 2022). The leave is job protected and mothers are only allowed to work in the weeks prior to childbirth with explicit written consent. They are not allowed to work in the 8 weeks after childbirth (Huebener et al., 2019). Employed mothers receive their full pay during their leave.

Next to the maternity leave, job protected parental leave has also been available in Germany for either parent. The 2007 reform called the *Elterngeld* changed the benefit structure associated with the existing parental leave policy (Bergemann & Riphahn, 2022). The policy change aimed (1) to increase family income in the first year of a child's life; (2) to encourage mothers to participate on the labor market; and (3) to increase gender equality by encouraging fathers to take part in family life (Huebener et al., 2016, 2019). The new system went into effect on January 1, 2007. All parents whose children were born on or after this date were eligible for the new benefits. The government coalition decided on the reform in May 2006 and parliament agreed in September 2006 (Kluve & Tamm, 2013), which means that parents whose children are born around January 1, 2007 did not know about this reform when they decided to have children.

This policy changed several aspects of the prior system (Huebener et al., 2016). First, it changed the amount of paid time parents could take off work to care for their child. Before 2007, one parent could stay home for 24 months and receive some transfers. The leave was almost always taken by the mother, only 3.5 percent of fathers took any leave in 2006 (Bünning, 2015). The reform cut this time in half to 12 months. Second, under the prior scheme, there were no specific incentives in place for the second caregiver to stay home. The reform allowed 2 extra months, bringing the total to 14, if both parents took at least 2 months. These 2 months are referred to as the "partner months". Parents could allocate the 14 months however they saw fit if one parent took at least 2.[1] Third, the reform also changed financial incentives. Under the prior policy, the monthly transfer was 300 euros for the duration of the leave, which was means-tested. In 2006, 77% of families received the payments for 6 months and about 50% from month 7 on (Ehlert, 2008). The new policy incorporated earnings replacement, with parents receiving about 2/3 of their net monthly income for the duration of their leave. The minimum payment remained 300 euros and a cap of 1,800 euros was imposed. Those who were not employed prior to childbirth also received the 300-euro minimum transfers. 300 euros is about 11% of the average net household income pre-birth for the years 2005-2008 (Huebener et al., 2019).

By design, the policy had heterogeneous effects on family income based on pre-child earnings. Families who had higher pre-child earnings received more transfers from the state than families with lower earnings. Families who were eligible for the 300-euro transfer under the prior policy regime saw no change in their transfers in the first 12 months after the birth of their child, but they lost the monthly 300 euros for the second 12 months. There were some families who were eligible for the 300-euro transfer prior to the reform but had higher monthly earnings, so the transfers they received in the first 12 months were higher. They also lost the 300-euro transfers for the second 12 months. Families who were ineligible for transfers prior to the policy change became eligible for at most 1,800 euros per months for the first 12 months after childbirth. These differences in transfers by pre-child family income can lead to heterogeneous effects on wage

[1] Single parents are allowed to take all 14 months.

dynamics. I cannot calculate pre-child household income, so I use maternal pre-birth education as a proxy for the income category. I analyze the effects of the policy on mothers with no vocational training or university degree (mother with low education) and on mothers with at least a university diploma (mothers with high education).

Conceptual background on the effects of the Elterngeld on maternal wages

Labor force participation for mothers with young children has been relatively low in Germany. In 2006, 61.2% of women between the ages of 15-64 with at least one child (aged 0-14) were employed, compared to the average of 65.3% of the Organization of Economic Cooperation and Development (OECD, 2020). The paid parental leave reform changed the benefit structure with the specific aim to influence female labor force participation. The policy could achieve this goal via several pathways. First, it encouraged women to work prior to having children, so that the transfers they received while on leave were higher. Second, under the new policy regime more women, specifically those who had higher incomes pre-child, received transfers from the state in the first 12 months after childbirth. This could have led some women to take longer leaves given that their incomes were guaranteed at 2/3 of their pre-child earnings up to 12 months. Kluve and Tamm (2013) found a 6 percent reduction in the share of mothers who worked during the first year after giving birth induced by the reform. This pathway could lead to a larger loss in wages for mothers in the cohorts eligible for the reform in the first year after childbirth compared to previous cohorts, because while previous cohorts may have returned to their pre-child earnings before 12 months, the eligible cohorts would stay on leave and hence have lower wages for a longer time period. This pathway is expected to be more pronounced for women with higher pre-child earnings.

The third pathway of the policy influencing female labor force participation is through encouraging mothers to return to the labor force earlier. During the prior policy regime, eligible mothers received transfers up to 24 months, which under the new regime was reduced to 12 months. This implies that mothers have to go back to work earlier to retain their monthly income. This pathway is expected to apply especially to women with lower pre-child earnings as they would have received transfers during both systems. Bergemann & Riphahn (2022) found that those mothers who received transfers under both systems returned to the labor force 10 months earlier at the median after the reform. Mothers who were not eligible for transfers before 2007 returned 8 months earlier. Maternal earnings in cohorts eligible for the reform should be higher in the months when they work instead of staying at home than earnings of mothers in non-reform eligible cohorts. This would mean that the policy increased wages for at least some months between 12-24 after childbirth. Given the shorter disruption of employment, mothers post-reform might advance in their careers faster than the pre-reform cohort, which could lead to higher wages, as well. Thus, the policy could have reduced wage loss after children starting in the second year after childbirth. This reduction is likely more pronounced for women with lower pre-child earnings.

Post-child earnings of women and gender gaps in earnings is also influenced by paternal leave taking and fathers' participation in both family life and in the labor force. While parental leave for fathers was technically available prior to the reform, only 3.5% of fathers took any leave in 2006 (Bünning, 2015). The reform encouraged fathers to take time off work on two accounts. First, the 2/3 earnings replacement provided a higher income for many fathers during the months they were on leave. Second, if each parent took at least 2 months of leave, the total allowance increased from 12 to 14 months. In line with this incentive, the share of fathers who took leave increased to 34% by 2014 (Huebener et al., 2016). Fathers whose partners had a higher education were twice as likely to take time off as fathers with partners who had a lower education (Huebener et al., 2019). These changes in paternal behavior could have a direct effect on their wages, which is expected to decrease for the duration of their leave (as the replacement rate is 2/3 with cap). This by design makes wage loss after children larger for reform-eligible fathers. This could have an indirect effect on maternal wages if in turn women are encouraged to work during those months when the father is on parental leave to increase monthly family income. However, 55% of fathers took leave simultaneously with their partner (Bünning, 2015), and anecdotal evidence suggest that families used this time to take a long vacation together.

Paternal leave taking can influence maternal wages through an indirect channel if the change in paternal behavior around child care is persistent and leads to changes in norms around family life participation. Policies that incentivize fathers to stay home for some time following the birth of their children were found to increase time spent with children and on household duties in several countries (Haas & Hwang, 2008; Kotsadam & Finseraas, 2011; Nepomnyaschy & Waldfogel, 2007; Tanaka & Waldfogel, 2007). Bünning (2015) and Tamm (2019) both analyze the *Elterngeld* and find that fathers reduced their working hours in the short-term, and increased the time they spend with child care, which is persistent even after their return to work. If social norms around female labor force participation after having children change and women are more likely to continue working after having children, maternal wage loss after childbirth can be reduced in the longer-run. However, these changes in social norms are relatively slow, and even if fathers spend more time with their children when they are not at work, they may not change their behavior in the labor force.

While I am not able to measure gender gaps in earnings in the Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021) the policy change could have an effect on gender gaps in wage loss following children. The definition of the child penalty provided by Kleven, Landais, & Søgaard (2019) is "the percentage by which women fall behind men due to children" (p.182). Since this number includes both the changes in maternal and paternal wages, both a smaller penalty for women and a larger penalty for men would result in a smaller overall gender gap. As outlined above, the policy by design could increase paternal child penalty in the short-run (for those fathers who took leave), and maternal child penalty is expected to increase in the short-run and decrease in the long-run. Depending on the magnitudes of the increases in paternal and maternal child penalties in the short-run, the gender gap could either decrease, increase, or stay the same. In the longer run, the gender gap could decrease if maternal child penalty decreases or if social norms change in a way encourage fathers to substitute work time to more family time.

To summarize, I expect to see an increase in wage loss (a larger child penalty) for mothers in the first year following childbirth, especially those who had higher earnings pre-child. After the first 12 months, I hypothesize a decrease in wage loss (a smaller child penalty), more pronounced for women with lower earnings pre-child. I do not expect to see a change in wage loss in the long-term.

The *Elterngeld* specifically aimed to increase equality on the labor market between men and women and to encourage higher female labor force participation. If families faced no constraints when they made decisions about allocating time for market work, childcare, housework, and leisure, the outcome of gender equality in the labor force would be a desirable outcome without any qualifiers. However, given time and monetary constraints, as well as personal preferences around spending time with children and market work, even if the policy led to smaller child penalties for women and a more equal labor market, it might have reduced family welfare.

The effect of Elterngeld on household income and child development

The literature on the effects of the *Elterngeld* has focused on two other outcomes: household income and child development. Concerning household income, the policy influenced some, mostly low-earner, households negatively. Those families who would have been eligible for 24 months of transfers in the old policy regime but were only eligible for 12 months under the new regime could have experienced a decrease in their household income in the second year. Indeed, while household income in the first year remained largely unchanged for previously eligible families, it decreased by about a 1000 euros in the second year after childbirth (Huebener et al., 2019). Families who were ineligible during the prior reform (in general, higher-earner households) benefited from the reform. Huebener et al. (2019) estimate that household income for these families increased by about 7500 euros during the first year after childbirth. Changes in household income in the first few years after childbirth could indirectly influence decisions parents make about when and how much to participate in the labor market.

Child development can be influenced on three accounts: (1) shorter overall time with parents; (2) longer time spent with fathers; and (3) changes in household income. Changes in household income are relatively small for most households especially in the long-term (Huebener et al., 2019). In a model of children's cognitive development Del Boca et al. (2014) show that time spent with both parents matters more for cognitive development than financial investments. These points make it more likely that overall time with parents and time with fathers are the more important channels for child development. The effect of these changes would depend on alternative care arrangements and the quality of both parental time and non-parental child care. Without directly measuring the quantity and quality of the types of care children receive, it is difficult to know how the potential changes in the amount of maternal, paternal, and non-parental care are going to influence child development.

Two papers analyze the effect of *Elterngeld* on child development. Huber (2019) finds that infant socio-emotional development was negatively affected by the reform. This effect is driven by families who became worse off following the reform. When looking at an index of physical health mostly relying on doctor's visits and whether children have medical problems, they find no changes in outcomes following the reform. Huebener et al. (2019) use data from school entry examinations to assess the effect of the policy on different child development indicators. These consist of four indicator variables of whether the child lags behind in language development (whether they can use prepositions, build plural words, or repeat pseudo-words); whether they lag behind in gross motor development (whether they can stand and jump on one feet); whether they have socio-emotional problems (whether they receive medical or psychological treatment); and whether they are ready for school overall assessed by a pediatrician. They find very small effects of the reform on these indicators. They also look at whether maternal employment, paternal leave taking, or the availability of child care in the county of residence mediates the effects, but they find no significant impacts. They conclude that the reform had no effect on children.

Dataset and Measures

I use the Sample of Integrated Labour Market Biographies (SIAB) from the Integrated Employment Biographies of the Institute for Employment Research (IAB) (Berge et al., 2021). The SIAB is produced by the Research Data Centre (FDZ) of the Federal Employment Agency at the IAB. The SIAB is an administrative dataset that includes labor market histories of individuals who paid social security contributions, which is about 80% of the working population. The dataset does not include civil servants and self-employed workers. A 2% sample of this population is available for research use, which covers 1,940,692 individuals and their employment biographies (Dorner et al., 2010). These biographies are recorded continuously, so indicators can be created up to daily frequency. The dataset includes time periods (spells) of employment, as well as periods when people receive unemployment or other benefits in accordance with the Social Code Book III and Social Code Book II. Periods of registered job search are also included. The data are available for Western Germany since 1975 and for Eastern Germany since 1992. I use labor market histories for the years 1998-2019.

One limitation of the SIAB dataset is that it does not contain specific information on childbirth for individuals. Women start maternity leave 6 weeks prior to their due date, which is recorded in the dataset. It is possible to impute children's date of birth using this information for women. It is not possible to infer when men become fathers using the SIAB only. Fathers would also deregister from employment when they go on parental leave, however, as there is no universal leave taking at a specific time relative to children's birth date, birth dates of children cannot be inferred for men.

I use the strategy and code provided by Müller, Filser & Frodermann (2022) to generate an imputed birth date for women who go on maternity leave. They use the "grund" variable to identify when someone deregisters from employment due to "wage compensation from a statutory health insurance", or code 151. The same code is used when someone goes on long-term sick leave, so they only impute childbirth for women who are at most 40 years old when they start their leave. The imputed birth date of children is the date when women start maternity leave plus 42 days (6 weeks). I use mothers' wages 2 months before giving birth as the baseline wage in my analyses below to account for the fact that they are in general already on leave in the month before giving birth. There are several cleaning steps, too. If someone stays on leave for fewer than 98 days (maternity protection lasts 6 weeks prior to giving birth and 8 weeks after birth), women are considered to be on sick-leave and not on maternity leave. The number of days between 2 potential childbirths is also checked to make sure that sufficient time passes between two maternity leave periods. This method can identify about 50% of all births (Müller et al., 2022), because (1) the dataset does not include civil servants and self-employed workers; (2) the method cannot differentiate between live- and stillbirths; (3) it cannot identify twin births; and (4) it cannot account for multiple births if the mother is not employed between subsequent children.

Employment histories are reported continuously as spells in the SIAB (Dorner et al., 2010). It means that one observation (for employment spells) is a period between two specific dates when a person is employed in the same position.[2] Their wages are reported as a daily wage during that period. It is also indicated whether they work part-time or full-time. I generate monthly wages using the following procedure. I first adjust wages for inflation using 2015 as the base year for the consumer price index (CPI), following Dauth & Eppelsheimer (2020). Then, for each day in each month between January 1998 and December 2019 I check whether the employment spell contains that specific day. If it does, I assign the daily wage to that day. Then, I sum the daily wages by month. This creates a monthly panel for the period January 1998 to December 2019. If an individual has multiple jobs, wages from all jobs are added up.

The SIAB contains a relatively limited set of demographic variables. I use birth year to measure people's age, and the information on country of birth to generate an indicator variable for whether one was born in Germany or in a different country. I also generate three variables for the highest level of education relying on the imputed education variable from the FDZ (following

[2] Benefit Recipient Histories while one is on unemployment or other benefits and Jobseeker Histories when people are registered to be on job search are also reported in the dataset (Dorner et al., 2010).

Dauth & Eppelsheimer (2020)): (1) no vocational training or university diploma (or "low education"); (2) completed vocational training (or "mid-level education"); (3) degree from a university (or "high education")[3]. I use these education categories to proxy for pre-birth household income. Monthly earnings of women in the high education category are 2,872 euros on average, monthly earnings of women in the middle education category are 1,797 euros on average, and monthly wages of women with low levels of education is 1,214 euros on average. In robustness checks, I also generate "high-income" and "low-income" categories using pre-birth earnings. Women who earn below the median are in the "low-income" category and women who earn above the median are in the "high-income" category.

My sample includes women who had a first birth between 2003-2007. The policy came into effect on January 1, 2007 and I use a differences-in-regressions-discontinuities design to measure its impact. As I outline in section 5.1 below, for this identification strategy I need information not just on women who gave birth around the cut-off date, but also on women who gave birth in the same period in prior years. I chose to include only first births because the change from no children to one child is likely different than the changes families experience with subsequent children. I further restrict my sample to include women who are between 18-40 years old when they have their first child. The imputation strategy for children's birth dates limits maternal age at 40, and I use 5 years of employment and wage history before childbirth. Women would be too young to work if I included mothers below 18. My analytic sample includes n = 22,909 women with a first childbirth in 2003-2007 and age 18-40 at the time of first childbirth. For the regression discontinuity design, I only use births in the first (January-March) and last

[3] The imputed variable considers data errors and reporting differences across years. The sample size is similar if I use the raw education data provided in the dataset.

quarters (October-December) of the years.[4] This sub-sample with only Q1 and Q4 births includes 8,811 mothers.

Table 1 includes summary statistics for the sample. Column 1 includes women with a first birth in 2003-2007, column 2 includes women with only Q1 and Q4 births, and column 3 further restricts the sample to include only women with Q1 and Q4 births who also have baseline wage data 2 months before giving birth. Women are 29 years old on average when they have their first child, and about 9% of them were not born in Germany. 13.4% of them have a university degree and another 13.4% of them have low levels of education (no vocational training or university diploma). Mothers earn on average about 2,500 euros before giving birth. There are no statistically significant differences between the samples in column 1 and column 2. Column 3 (the sample with a baseline wage in $t = -2$) is slightly older (by 0.4 years) and better educated on average (1 percentage point fewer people have low education). These differences in sample means are statistically significant at 5%.

Outcome variable: wages

The main outcome variable of interest is monthly wage. When women are on maternity leave, they are still employed but their wages are set to 0 as they do not earn their monthly income from employment. If they do not return to work once their maternity leave ends, they disappear from the dataset. In general, when someone is not employed or is not actively looking for a job, they are absent from the SIAB. If they are employed in a civil servant position or they are self-employed, there are also no observations. For mothers a typical employment history looks the

[4] In 2003 I only use the last quarter and in 2007 I only use the first quarter, so this sub-sample includes births in October-December 2003, January-March 2004, October-December 2004, January-March 2005, October-December 2005, January-March 2006, October-December 2006, and January-March 2007.

following: (1) employment with non-zero wages; (2) maternity leave with 0 wages; (3) no observations for a period (when they stay out of the labor force to spend time with their children); (4) employment with non-zero wages (if they return to a non-civil-servant or non-self-employed position). For the purposes of my analyses, there is important information missing in periods when mothers are not observed in the dataset. Hence, I generate a balanced panel where I impute all months without observation in the dataset between January 1998 and December 2019 with 0 wages. This method will set some women's wages as 0 who are in effect earning wages as civil servants or are self-employed. I run the analyses both with and without the imputed zeros. Wages are top-coded as there is a ceiling for social security contributions. If one earns more than the contribution ceiling, their wage is reported as the ceiling. About 2.7% of the observed wages are top-coded in my sample of mothers.

In graphs and regressions describing wage dynamics of mothers around childbirth I use an annual frequency. I index the year of childbirth as $t = 0$. I generate the mean of monthly wages over the year for 5 years before and 10 years after childbirth as the outcome variable in these analyses.

In analyses investigating whether the paid leave policy change influenced maternal wages, I use monthly wages. I generate a *wage loss after childbirth* variable to capture by how much maternal wages change after childbirth compared to pre-child wages. I use $t = 0$ to index childbirth. To define wage loss in period $t + 1$, I use the following formula: $\frac{y_{base} - y_{t+1}}{y_{base}}$, where y_{t+1} is an individual's salary 1 month after childbirth, and y_{base} is the individual's salary 2 months before childbirth (as mothers are in general on leave the month before birth and their wages are 0 by design). For example, if someone earned 1800 euros 2 months before childbirth, and 300 euros

1 month after childbirth, the child penalty in period $t+1$ is $\frac{1800-300}{1800} = 0.83$. This number means that the individual earns 83% less after childbirth than what they earned before giving birth. If 10 months after childbirth they earn 1200 euros, the child penalty in period $t+10$ is $\frac{1800-1200}{1800} = 0.33$, meaning that they earn 33% less 10 months after childbirth than what they earned before childbirth. I generate this monthly *wage loss* variable for 60 months (5 years) after childbirth for each mother individually.

Notes on interpretation: I analyze whether the paid leave policy change influenced wage loss. If the reform decreased wage loss, the group affected by the reform should retain a higher percentage of their pre-child wages, making their wage loss outcome smaller. Staying with the previous example, let's say an individual in before-reform era earned 1800 euros before giving birth and 300 after giving birth, earning 83% less after childbirth. In a counterfactual scenario, the individual in the reform era earned more than 300, say 500. Wage loss for the counterfactual scenario would be $\frac{1800-500}{1800} = 0.72$. This means that wage loss in the reform era is smaller. The difference in wage loss is 0.72-0.83= -0.11. We would interpret this as the policy had an effect of -11 percentage points. Hence, if the policy decreased wage loss, effect sizes are going to be negative. From the point of view of the policy's goals a lower wage loss is a "positive outcome". Thus, a negative coefficient is interpreted as a "positive outcome". A positive coefficient would mean that the policy increased wage loss, so it is a "negative outcome".

How to interpret the coefficients if someone earns more after giving birth than before? Let's say they earn 1800 right before giving birth and 2500 after giving birth. Then their earnings difference would be $\frac{1800-2500}{1800} = -0.38$. This means a 38% increase in salary. If in the counterfactual scenario of the reform era they earn 2000 post-child (instead of 2500), their "wage loss" would be $\frac{1800-2000}{1800} = -0.11$ (still an increase, but of a smaller 11 percentage points). The difference between the two counterfactual outcomes is -11-(-38) = 27 percentage points, which is bigger than zero, so we can interpret it the same way as above: the policy had a negative effect on earnings (because the individual is earning 2000 as opposed to 2500 post-child). If in a different counterfactual scenario the wage went up to 3000, the "wage loss" variable would show $\frac{1800-3000}{1800} = -0.66$, or a 66% increase in wages from pre- to post-child. Here, the effect of the policy would be -66-(-38) = -28 (instead of earning 2500 the individual earns 3000 post-child compared to the 1800 base). This difference is smaller than 0, and the interpretation is the same as in the other cases: the policy had a "positive effect" on wages.

Descriptive statistics

Panel B of Table 1 reports descriptive statistics for a set of outcome variables. Wages 3 months after childbirth are between 700-900 euros per month across the samples, but only a small percentage of mothers have non-zero wages this close to childbirth. If we impute all missing observations as 0, wages drop to between 200-300 euros. About 30% of mothers are reported to work part-time 3 months after birth, but again, a large share of the sample has missing information. A year after giving birth, maternal wages are around 1,200 euros per month, which is about half of pre-child wages. Around 55% of all mothers in the sample have missing wage data 13 months after childbirth. 50% of those who work are employed part-time. Their wages increase somewhat to about 1,400 euros per month by month 24 (2 years after childbirth). 45% of mothers have no wage information 2 years after childbirth. Close to 60% of mothers work part-time 2 years after childbirth, but this information is only available for about 55% of the sample.

Documenting maternal wage dynamics around childbirth

In this section, I document maternal wage dynamics around childbirth in Germany for mothers whose first children were born in 2003-2007. I show that mothers experience a large and persistent drop in monthly wages right after childbirth, both overall and by education level. Monthly wages do not reach pre-birth levels for mothers in my analytic sample whom I follow for 10 years after childbirth, except for the group with the lowest level of education. However, once I account for the effect of age and economic growth in wages, the differences between the groups disappear.

I use event study regressions to document wage dynamics around childbirth. The event is the birth of the child. I set the year when the child is born to $t = 0$. Wages in each year are indexed to the event of childbirth. I report wages annually for 5 periods before and 10 periods after childbirth. To describe maternal mean wages before and after children I run the following regression:

$$(1)\ Y_{ist} = \sum_{j \neq -1} \alpha_j \, \mathbf{I}[j = t] + \varepsilon_{ist}$$

where Y_{ist} is the average monthly wage in year s for individual i in event time t. $\mathbf{I}[j = t]$ depicts a full set of event time dummies. I use the year before childbirth as the baseline wage, hence omit $t = -1$ from the regression. The $\widehat{\alpha_j}$ coefficients are the mean wages for each event time compared to the year before childbirth. I run the same regression for sub-samples of mothers with low levels of education, mid-level education, and mothers with high levels of education. Standard errors are clustered by individual.

Women earn on average 2,296 euros per month in the year before childbirth. Their wages decrease to 1,088 euros per month on average in the year after childbirth, which is a (1,088-

2,296)/2,296*100 = 53% drop. They continue to earn less than before childbirth during the 10-year period following childbirth. Even 10 years after the arrival of children mothers earn on average 23% less than before children. Women with high levels of education (at least a university diploma) earn 3,498 euros per month on average in the year before childbirth, which drops by 43% to 1,980 in the year after childbirth. Mothers in this group earn 16% less 10 years after childbirth than they did pre-child. Women with low levels of education (no vocational training or university diploma) experience a similarly large decrease in earnings after childbirth, a 46% drop (from 1,244 euros to 669 euros). However, their raw earnings recover faster than wages of women in the two other groups. They reach their pre-child earnings 8 years after childbirth and have higher monthly wages in years 9 and 10 after childbirth than they did pre-child. Women with mid-level education (completed vocational training) earn on average 2,358 euros per months before childbirth, which drops by 28% to 1,687 after childbirth. They earn 28% less 10 years after childbirth than what they were earning before having children. Figure 1 Panel A shows maternal wages 5 years before to 10 years after childbirth as percentages of the baseline wage right before childbirth for all four groups.

The means reported in the previous paragraph do not account for the fact that many mothers exit the labor force when they have children at least for some time. For example, 13 months after childbirth only 46% of women have any wage data reported in the dataset. To correct for this selection, I produce similar estimates using a balanced panel where all missing wages are imputed as zero. This provides a lower bound for the estimates of maternal wages around childbirth. The imputed mean wage in the period before childbirth for the full analytic sample is 2,144 euros per month. Imputed maternal wages drop by 77.5% to 483 euros. Even 10 years after childbirth mothers earn 38% less than they did before childbirth once I account for the missing wages. Women with low levels of education experience a similarly large drop as the overall sample (75%), but their wages recover faster. They do not reach pre-child levels of imputed wages, though. 10 years after childbirth women in this group earn 20% less than before children. Imputed wages of women with high levels of education decrease by 67.5% right after childbirth. They earn 33.5% less 10 years after childbirth. Imputed wages of women with mid-level education drop by 51%, and they earn 41% less 10 years after childbirth than they did before having children. Figure 1 Panel B shows imputed wage dynamics for all four groups of women.

These differences in maternal wage dynamics by educations groups around childbirth can be explained by several factors. For example, age can drive the results. Women have children at different ages, and women with lower levels of education have children earlier on average. In my sample, women with low education are 25 years old on average when they have their first child, and women with at least a university degree have their first child on average at age 32. Wages also tend to be higher when one is older, which contributes to the different levels of wages mothers have before childbirth. Women at different ages are also at different stages of their careers, which likely contributes to the differences in wage dynamics. To control for these age-related life-cycle effects in wages, I run event-study regressions with including not only the event time dummies, but also indicator variables for each observed age in the sample. The model I estimate is the following:

$$(2)\ Y_{ist} = \sum_{j \neq -1} \alpha_j \, \mathbf{I}[j = t] + \sum_k \beta_k \mathbf{I}[k = age_{is}] + \varepsilon_{ist}$$

For each group of women, I estimate equation (2) separately, and I omit the indicator variable for the mean age (29 for the full sample, 25 for the low-education group, 29 for the mid-level education group, and 32 for the high-education group). I also omit event time -1. The coefficient on my omitted variable (the constant in the regression) is an estimate of the average wage in $t = -1$ net of age effects. This can be interpreted as estimating the $\hat{\alpha}_j$ coefficients as if everyone in my sample had the mean age.

Figure 2 shows wage dynamics around childbirth adjusting for age effects. Panel A uses observations with non-missing wage data. After controlling for the effect of age in wages, mothers in all three groups experience very similar dynamics. Their wages drop by about 50% in the year after childbirth, and then stay relatively flat. Women with low education experience an increase in their wages, but even 10 years after childbirth they earn 32% less than before childbirth. Dynamics are qualitatively similar if I use imputed wages. The drop in wages after childbirth is very large, between 70-80% across the four groups. Women with low levels of education again recover some of their lost wages, but they still earn only about half of what they did pre-child.

A second factor I consider is year effects. Maternal wages are likely influenced by economic growth and events such as the Great Recession of 2007-2009, with heterogeneous effects on groups by levels of education. To control for such events, I include a set of year dummies in my event-study regressions next to the event time and the age dummies, making the full specification of my model the following:

$$(3)\ Y_{ist} = \sum_{j \neq 1} \alpha_j \, \mathbf{I}[j = t] + \sum_{k} \beta_k \mathbf{I}[k = age_{is}] + \sum_{y} \gamma_y \mathbf{I}[y = s] + \varepsilon_{ist}$$

I omit the mean of the year variable, 2008, to make the $\hat{\alpha}_j$ coefficients comparable to estimates from the other equations. I calculate the percentage change in wages by event time using the coefficient on the omitted variable as the baseline.

Figure 3 shows the $\hat{\alpha}_j$ coefficients as a percentage of the baseline wage. Controlling for macroeconomic factors makes the estimated wage-drop larger. The recovery that women experience over time disappears, which can be interpreted that it was mostly driven by economic growth (real wage growth). Women with low levels of education experience the lowest drop in their earnings, they earn about 40% of their pre-child wage throughout the observed period. Women in the full sample and those with higher levels of education experience a 55% drop that increases to 60-65% during the 10 years after first childbirth.

The effect of the policy change on maternal wages

In this section of the paper, I investigate whether the paid family leave policy change influenced the wage drop mothers experience around childbirth. As outlined in section 2.1, I expect to see an increase in wage loss in the first year following childbirth (the drop in wages is going to be larger under the new policy regime than what it was under the old regime). I expect this increase in wage loss to influence especially those who had higher earnings pre-child. After the first 12 months, I hypothesize a decrease in wage loss: the drop from pre-child to post-child wages in the 2nd year after childbirth to be smaller in the new regime than what they were in the old regime. I expect this effect to be more pronounced for women with lower earnings pre-child. I do not expect to see a change a change in wage loss in the long-term.

Identification strategy

This section answers the research question of whether the paid family leave policy change had an effect on labor market outcomes. The ideal experiment would randomly assign future potential parents to either the old (control group) or to the new policy regime (treatment group) and would look at the differences in average outcomes between these two groups. Since this ideal experiment is not feasible, I rely on the date when the policy became effective to allocate people to treatment and control groups. Parents whose children were born on or after January 1st, 2007 were eligible for the new paid parental leave benefits (treatment group), and people whose children were born on or before December 31st, 2006 stayed under the old regime (control group). This set-up would make a regression discontinuity (RD) design possible.

The identifying assumption for the RD design is that the potential outcomes are continuous with respect to the assignment variable, or in other words, there are no discrete changes in the outcome variables except due to the treatment. In this specific case, this means that in the absence of the policy change we would not expect to see any jumps or discontinuous changes in labor market outcomes of parents whose children are born in December compared to parents whose children are born in January. This also means that all observed and unobserved characteristics are expected to be continuous (or "vary smoothly") around the cut-off (Lee & Lemieux, 2010).

In Table 2, panel A, I compare mothers who gave birth in the year 2006 to mothers who gave birth in the year 2007. Their observable characteristics are mostly similar. Both are around 29 years old when they have their first child, about 9% of them were born not in Germany, 14.5% have at least a bachelors degree, 13% have no vocational training or university education. They earn around 2200 euros a month before giving birth. The pre-birth wages including the imputed zeros of mothers who gave birth in 2006 are 1951 euros, versus 2013 for the mothers who gave birth in 2007, which is the only statistically significant difference between the two groups.

To analyze the effect of the policy using a 12-month bandwidth around the cut-off date, I specify the following relationship between the variables of interest:

$$(4)\ y_i = \beta_1 T_i + f(m) + f(m)T + \gamma \boldsymbol{X}_i + \varepsilon_i,$$

where y_i refers to the outcomes of individual i (wage loss in period 1, 2, etc., each of which is a separate outcome in a separate equation). T is an indicator variable =1 for the year 2007, $f(m)$

refers to the function of the running variable m. More specifically, it indexes the month of birth relative to the cut-off of January 1st, so December is -1, January is 1, February is 2, etc. I allow this function to be different after the cut-off in T. $\boldsymbol{X}$ contains a set of demographic control variables (age at first birth, being foreign-born, indicators for low and high education, baseline wage 2 months prior to giving birth, part-time employment 2 months prior to giving birth), and ε is the error-term. I estimate this regression using Ordinary Least Squares.

Prior research has documented a seasonality in births in the United States, which would make the identifying assumption invalid. Women who give birth in January tend to be less educated, younger, and are less likely to be married (Buckles & Hungerman, 2013; Currie & Schwandt, 2013). These findings could bias my results the following way. In general, people with fewer years of education tend to earn lower wages. If more people with lower wages give birth in January, the average wages in January are going to be lower. Attributing all of the differences in wages between December and January to the policy could overestimate the effect of the policy.[5]

To overcome this limitation, I use a differences-in-regression-discontinuities design to estimate the effect of the policy change on maternal labor market outcomes, which is a popular method to study the effects of family leave policy changes on parental labor market and health outcomes (Lalive et al., 2014; Lalive & Zweimüller, 2009; Persson & Rossin-Slater, 2021; Schönberg & Ludsteck, 2014). For the RD set-up, I use October-December births in 2006 just before the policy as the control group, and January-March births in 2007 as the treatment group. I

[5] Misreporting of birth month could be another concern. Torun & Tumen (2017) show that in Turkey 20% more births are reported for January than for other months driven by families with lower-socioeconomic status due to geography and institutional reasons. This fact is relevant for my project because 3% of the nationally representative German Socio-Economic Panel survey sample report a Turkish nationality, which is the largest ethnic group after German (86%). However, the strategy I use to infer birth dates relies on administrative data on when mothers deregister from employment to go on maternity leave, so misreporting of birth dates is not of concern for this project.

call these births together the "reform sample". I also use the same time periods in the three preceding years to control for seasonality, which constitute the "non-reform" sample. I use births from October 2003-March 2004, October 2004-March 2005, October 2005-March 2006 as the "non-reform sample", with October-December births as the "control group" and January-March births as the "treatment group", and October 2006-March 2007 births as the "reform sample", with the same treatment-control set-up. I use a robust data-driven estimation to select the optimal bandwidth around the cut-off date (Calonico et al., 2014, 2017). The optimal number of days is 84 around the January 1st introduction date. Since this is very close to using three months on each side, I keep the three-month bandwidth in the below analyses.

I specify the following equation to describe the relationship of interest:

$$(5)\ y_i = \beta_1 Q_{1i} + \beta_2 R_i + \beta_3 Q_{1i} R_i + f(m) + f(m)Q_1 + \gamma \boldsymbol{X}_i + \sigma_p + \varepsilon_i,$$

where y_i refers to the outcomes of individual i (wage loss in period 1, 2, etc., each of which is a separate outcome in a separate equation). Q_1 is an indicator variable =1 for the months of January-March, R is an indicator variable =1 for the reform sample (people whose children was born between October 2006 and March 2007), $f(m)$ refers to the function of the running variable m. More specifically, it indexes the month of birth relative to the cut-off of January 1st, so December is -1, January is 1, October is -2, etc. I allow this function to be different after the cut-off in Q_1. $\boldsymbol{X}$ contains a set of demographic control variables (age at first birth, being foreign-born, indicators for low and high education, baseline wage 2 months prior to giving birth, part-time employment 2 months prior to giving birth), σ_p is period fixed-effects, and ε is the error-term. I estimate this regression using Ordinary Least Squares. β_3 is the coefficient of interest, $\hat{\beta}_3$ is the estimate of the effect of the policy change on the reform sample if the identifying assumptions hold.

I build up my analyses to the full specification in (4) step-by-step. First, I estimate the RD models separately for the reform and non-reform groups by adding each term sequentially. I start with only the Q_1 dummy, then add control variables, then add the trends:

$$(6)\ y_i = \beta_1 Q_{1i} + f(m) + f(m)Q_1 + \gamma \boldsymbol{X}_i + \varepsilon_i,$$

then I pool the samples and add the "differences" (the reform dummy and the interaction term between reform and quarter 1) with the period fixed-effects.

Table 2 shows differences in means of baseline variables by reform/non-reform group status. There are no statistically significant differences between observable characteristics of women who gave birth in October-December 2006 (reform sample, control group, n = 1054) and women who gave birth in January-March 2007 (reform sample, treatment group, n = 1026). Women who gave birth in January-March of 2004, 2005, or 2006 (non-reform sample, treatment group, n = 3289) are 0.6 years older on average than women who gave birth in October-December 2003, 2004, 2005 (non-reform sample, control group, n = 3442). Women in the non-reform treatment group are 2 percentage points more likely to have at least a university degree and 3 percentage points less likely to have low education.

Manipulation of birth dates of children is a concern in this institutional set-up. Some women may have tried to give birth in the new year so that they become eligible for the new benefits. This would be possible by delaying labor inductions for example. As long as individuals are not able to precisely manipulate the date of the birth of their children, there would be a source of randomized variation in the treatment status very close to the threshold, which would make the RD strategy theoretically valid (Lee & Lemieux, 2010). It is not possible to precisely control the start and the length of all births, so if the sample had enough births one minute before midnight and one minute after midnight, the two groups of mothers would be expected to be very similar to each other. However, researchers often extend the time period around the cut-off to include parents of children who are born in the few weeks or months on the two sides of the threshold to increase sample size. As one increases the time period considered around the policy introduction date, it becomes less likely that the treatment and control groups stay comparable to each other.

Looking at a 7-day period around the January 1, 2007 threshold, papers have shown that about 1000 births, which account for about 8% of births in the general population, were shifted from the last week of December to the first week of January (Neugart & Ohlsson, 2013; Tamm, 2013). This shift was driven by working women. On average about 40% of births are a result of C-section or a labor induction on a working day, and 25% on a weekend or public holiday. Jürges (2017) shows that 80% of the "missing" births in December and 90% of the excess births in January can be explained by delayed elective C-sections and labor inductions. This means that the majority of the births are still expected to fall randomly on either side of the threshold. I could over- or underestimate the effect of the policy if people who achieved the delayed C-sections and labor inductions have observable characteristics that correlate with potential outcomes. However, I use the date when mothers go on maternity leave 6 weeks prior to their due date to infer the birth date of children. As long as mothers do not adjust the first day of their maternity leave systematically, I will infer non-manipulated dates of birth. This will allow me to estimate intent-to-treat effects.

Looking at the number of births by week compared to the January 1, 2007 cut-off would provide some evidence that the manipulation of birth is not of concern in my set-up. Figure 4 graphs the number of births by week for one year before and one year after the cut-off date. While there appears to be more weeks with a higher number of births after the reform, there is no discrete jump at the cut-off date. The local linear smoothing shows somewhat fewer births in the first week of January (which is the opposite of the expected direction if women delay their births), but the 95% confidence intervals are highly overlapping.

As a last threat to identification, the policy could have induced some families to have (more) children. The government coalition decided on the reform in May 2006, and parliament agreed in September 2006 (Kluve & Tamm, 2013), which means that parents whose children were born around the January 1, 2007 date did not know about the policy change when they decided to have children. This implies that changes in fertility close to the cut-off date do not threaten the identifying assumptions.

Results: The effect of the policy change on maternal wage loss

I first run the Regression Discontinuity analyses of maternal wage loss (the difference in earnings between pre- and post-child) on giving birth in the first quarter of the year. I define a separate outcome for each month after childbirth. I show results for 3 outcomes: wage loss in t=3 (3 months after childbirth), wage loss in t=13 (13 months, or a little over a year), and wage loss in t=24 (2 years after childbirth). I also use imputed wages for these time periods (where I include wage observations of mothers who are not in the dataset in a specific time period as having 0 wages for that period).

Figure 5 shows the change in wage loss in t = 13 (panels A and B) and wage loss in t =24 (panels C and D) around the cut-off date with local linear smoothing and 95% confidence intervals. Panels A and C show the reform sample (when the policy was introduced) and Panels B and D show the same period in prior years (with no change in policy). Wage loss 13 months after childbirth is lower after the cut-off date in the reform sample, while there is no discontinuous change in the non-reform years. Wage loss 24 months after childbirth looks similar in both reform and non-reform years.[6]

Table 3 shows regression coefficients for the RD models. Panel A reports coefficients with a 12-month bandwidth, Panel B reports coefficients for the Reform sample with a 3-month bandwidth, and Panel C reports coefficients for the Non-reform sample with a 3-month bandwidth. There is no statistically significant difference between wage loss 3, 13, or 24 months after childbirth for women who gave birth in 2006 versus 2007, or between women who gave birth in Q4 and women who gave birth in Q1. This is true both for the reform and non-reform sample. There are significant differences in imputed wage loss 13 and 24 months after childbirth. Women who gave birth in 2007 experience a larger wage drop 3 months after childbirth than women who gave birth in 2006. Women who gave birth in 2007 also experience a smaller wage drop 13 and 24 months after childbirth than women who gave birth in 2006. Women in the reform sample who gave birth in January experience a 3 percentage point lower wage drop between their pre-child wages and wages 13 months after birth than women who gave birth in December based on the model with baseline control variables and time trends. The drop is statistically significantly different in the other specifications, too (without controls and/or time trends), but the coefficient changes a lot between the specifications. The coefficients on the 24-month-post-birth wage loss variables also indicate a decrease in wage loss as a result of the policy. However, the point estimates are very large: 10 and 26 percentage points, respectively, in models with controls and with time trends. A change in imputed wage loss but not in the raw wage loss indicates that fewer women have zero wages among those who give birth in Q1 compared to those who give birth in

[6] I cannot produce similar graphs for wage loss 3 months after childbirth because the sample size for non-missing wage information by week is too small.

Q4. The fact that there are statistically significant differences in wage outcomes between women who gave birth in Q4 and Q1 in the non-reform sample, too, makes it less credible that the changes in the reform sample are due to the policy itself. Hence, I turn to the full specification of the model where I check whether the differences in the regression discontinuity estimates are statistically significantly different from zero.

Table 5 reports regression results from the full model specification with a 3-month bandwidth from equation (5) for the same set of outcomes: wage loss 3, 13, and 24 months after childbirth, and imputed wage loss 3, 13, and 24 months after childbirth. Panel A uses the full sample, panel B the sample of mothers who have no vocational training or university degree, and panel C uses the sample of mothers with at least a university degree at the time of childbirth.

I also generate wage loss outcomes for each month up to 60 months (5 years) after childbirth and graph the coefficients on the "treated" dummy for the 12-month bandwidth, and on the interaction term for the 3-month bandwidth with the 95% confidence intervals. Figures 6, 7, and 8 show these estimated coefficients for the full sample, for the low education sample, and for the high education sample, respectively. The estimated coefficients on the figures for periods 3, 13, and 24 are the same as the estimated coefficients in Table 5.

For estimates using a 12-month bandwidth, the wage loss is larger in the initial periods under the new policy regime, but then becomes smaller under the new regime after month 4. This is true both for the raw wages and for the imputed wages in the full sample. For women with low education, the initially higher wage loss than smaller wage loss under the new regime is apparent for the raw wages, but not for the imputed wages. The confidence intervals for women with high levels of education are very wide and include zero for all periods.

In estimates using a 3-month bandwidth, for all three samples, confidence intervals are quite wide for all periods and with very few exceptions contain 0. This means that we cannot reject the null hypotheses that the policy had no effect on wage loss for any period. For the full sample, the point estimates are negative for periods 5-20 for raw wage loss and 9-25 for imputed wage loss, which would be interpreted as wage loss decreasing as an effect of the policy (the desirable outcome from the policy's standpoint). However, these are not statistically significantly different from zero.

The only group where the point estimates show a consistent pattern with my hypotheses is women with low levels of education. The sample size is relatively small, so the estimates are noisy, but they imply that wage loss decreased in each period in the first two years after childbirth. The imputed wage loss starts to decrease after 8 periods. The policy stopped transfers to this group after the first 12 months, encouraging mothers to return to work after the transfers stop to retain monthly earnings. If mothers in the old regime stayed home for the second year, but under the new regime went back to work, we would expect their wage loss 12-24 months after childbirth to be smaller in the new regime. The data support this story. The point estimates on imputed wages (Figure 8, Panel B) suggest that the gap between maternal wages pre-child and 15 months post-child decreased by about 50 percentage points (the average gap is about 70%). The 95% confidence intervals are very wide, but they do not contain 0 for 15-18 months after childbirth.

Considering the sample of women with high levels of education, the coefficients in the first 3 years after childbirth are all very close to zero, with relatively tight confidence intervals (the coefficients are not precisely estimated as the sample size is relatively small). This could be interpreted as the policy had no effect on wage loss for women with high levels of education. No significant change is indicated for the imputed wage loss variable either, which implies that they also did not change their responses on the extensive margin. These findings mean that wage dynamics of women in this group did not change as a result of the policy. They experience a similar drop in earnings compared to their pre-child levels for all periods under the new regime than what they experienced under the old regime. The policy change encouraged this group of women to work before having children (so they have higher transfers during their leave), and to return to work after the transfers stop, however, the data does not support a change in their behavior. I do not test whether levels of wages change, only if the difference changes. It is possible that someone earned 1000 in the old regime in t-2 and 600 in t+3, which is a 40% drop. In the counterfactual scenario, in the new regime, they could have earned 1100 in t-2 and 660 in t+3, which is the same 40% drop. Their earnings are higher both pre- and post-child, but the gap is still 40%, so my analyses would show no effect of the policy. In terms of wage loss after children, their outcome is unchanged.

Conclusion

Women experience a large and persistent drop in their wages after childbirth (Aguilar-Gomez et al., 2019; Andresen & Nix, 2021; Kleven, Landais, Posch, et al., 2019; Sandler & Szembrot, 2019). I document a 53% decrease in monthly wage one year after childbirth compared to their earnings in the year right before childbirth for German mothers who had a first birth between 2003-2007. Their wages are still 23% less 10 years after childbirth than what they were before having children. This increase of 30 percentage points between year 1 and year 10 after childbirth is explained by age effects and economic growth. Women with high and low education levels have similar dynamics, although women with low education experience a smaller drop in their earnings in percentage terms than women with high education.

The decision of how much market work, housework, and child care to provide after children arrive depends on many factors including personal preferences and social norms, as well as institutional, financial, and time constraints. Family and child policies, like paid family leave, can directly or indirectly influence these factors. The German paid family leave policy change in 2007 reduced the number of months parents received transfers for but increased the amount they received. Low-income households received transfers under both regimes, for two years under the prior regime, and for one year after the new regime. High-income households received no transfers under the old regime, and 2/3 of their income in the new regime (Huebener et al., 2019). I use education levels as a proxy to sort women into these two groups.

One specific aim of the policy was to encourage mothers to return to work earlier, which could also lead to a reduction in gender inequality in the labor market. If mothers indeed returned to work earlier under the new regime, their earnings should recover faster after childbirth. I find suggestive evidence that the paid family leave policy change of 2007 reduced maternal wage loss after childbirth in the second year after children were born. This change is driven by mothers with low levels of education. Wage loss of highly educated mothers is not influenced by the policy change. Even though prior research shows that women in both groups returned to work several months earlier under the new policy (Bergemann & Riphahn, 2022), I do not find evidence of this in my dataset and sample. Highly educated mothers who gave birth in 2006 and 2007 have a similarly long period of about 2 years of no market work after the arrival of their children. Mothers with low education return to market work about 3 months earlier in 2007 than in 2006. This suggests that policy incentives influence mothers with lower household income more. They are less likely to be able to stay home with their children in the absence of the transfers. High-income mothers seem to have more flexibility in whether they return to work or stay home; their decision is not so strongly influenced by the paid leave policy.

Paid family leave policy changes with incentives for secondary caregivers to take time off of work are promising tools to increase gender equality. Discussions about how to achieve gender equality often focus on enabling women to have similar labor market outcomes to men or on reducing the impact of children on maternal labor market experiences. However, gender equality can be achieved not just by a change in women's behavior. Policies such as the *Elterngeld* have the potential to change paternal choices around market work, child care, and housework, which could lead to more equitable outcomes between genders both in the short- and in the long-term. In this project, I am only able to study women and their labor market response to the policy change. Future research should focus on the effects of family policies on household decision-making to understand the channels that can lead to gender equality.

Tables

Table 1.1: Descriptive Statistics

Panel A: Baseline variables

	(1)	(2)	(3)
sample size	**22,909**	**8,811**	**7,665**
Age	29.1 (4.9)	29.1 (4.95)	29.4 (4.8)
Born abroad	9.2%	9.1%	8.5%
High education	13.6%	13.4%	13.7%
Low education	13.4%	13.4%	12.2%
Wage in t = -2	2,247.3 (1262.1)	2,249.50 (1262.95)	2,249.50 (1262.95)
Imputed wage t = -2	1,973.0 (1392.7)	1,956.9 (1400.1)	2,249.50 (1262.95)
Works part-time t = -2	19.8%	19.9%	19.9%

Panel B: Outcome variables

	(1)	(2)	(3)
sample size	**22,909**	**8,811**	**7,665**
Wage in t = 3	920.0 (1359.7)	723.5 (1201.9)	728.1 (1207.9)
Imputed wage t = 3	291.2 (876.5)	230.7 (757.7)	263.5 (806.4)
Wage in t = 13	1,240.80 (1308.5)	1,264.70 (1306.1)	1,296.50 (1319.1)
Imputed wage t = 13	578.10 (1086.7)	563.80 (1075.0)	636.50 (1128.9)
Wage in t = 24	1,415.20 (1258.1)	1,411.50 (1254.9)	1,451.90 (1266.2)
Imputed wage t = 24	753.50 (1158.2)	745.20 (1152.3)	828.90 (1196.5)

Notes: Sample includes mothers who gave birth between 01-01-2003 and 12-31-2007 and are between ages 18-40 at the time of first childbirth. Column (1) includes the full sample. Column (2) includes only women who gave birth in 2003 October-2004 March, 2004 October-2005 March, 2005 October-2006 March. Column (3) incudes the same women as column (2) who also have baseline wage data in t = -2. Table reports means (standard deviations) of demographic characteristics (age, country of birth, education) at the time of childbirth. Baseline labor market indicators are reported for the period 2 months before childbirth, and outcome variables are reported for periods 3, 13, and 24 months after childbirth. Data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). "Low education" refers to the group of mothers who have no vocational training or a university diploma. "High education" refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. To generate imputed wages, all missing wages are set to 0.

Table 1.2: Differences in means of baseline variables (balance checks)

Panel A: 2006 and 2007 births

	2007 birth "treated"	2006 birth "control"	diff (2006-2007)	p-value
sample size	**4442**	**4297**	**8739**	
Age at first birth	29.21	29.11	-0.10	0.31
Foreign born	8.58%	9.00%	0.41	0.51
High education	14.58%	14.53%	-0.05	0.95
Low education	13.44%	13.05%	-0.39	0.61
Wage in t = -2	2193.88	2170.80	-23.08	0.41
Imputed wage t = -2	2013.11	1951.04	-62.07	0.03
Works part-time t = -2	20.01%	21.28%	1.20	0.19

Notes: Table reports means of demographic characteristics (age, country of birth, education) at the time of childbirth for women who gave birth in 2007 (treated group) and women who gave birth in 2006 (control group). Data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). “Low education” refers to the group of mothers who have no vocational training or a university diploma. “High education” refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. To generate imputed wages, all missing wages are set to 0.

Table 1.3: Differences in means of baseline variables (balance checks, quarters)

Panel A: Reform sample (birth in 2006 October-2007 March)

	Q1 birth "treated"	Q4 birth "control"	diff (Q1-Q4)	p-value
sample size	**1026**	**1054**	**2080**	
Age at first birth	29.17	28.91	0.26	0.21
Foreign born	8.04%	7.87%	0.17	0.9
High education	13.98%	12.37%	0.02	0.33
Low education	14.27%	13.54%	0.01	0.67
Wage in t = -2	2160.66	2154.67	5.99	0.92
Imputed wage t = -2	1960.6	1958.42	2.18	0.97
Works part-time t = -2	19.27%	22.13%	-0.03	0.13

Panel B: Non-reform sample (birth in 2003 October-2004 March, 2004 October-2005 March, 2005 October-2006 March)

	Q1 birth "treated"	Q4 birth "control"	diff (Q1-Q4)	p-value
sample size	**3289**	**3442**	**6731**	
Age at first birth	29.30	28.78	0.59	<0.001
Foreign born	9.20%	9.66%	-0.005	0.53
High education	14.58%	12.64%	0.02	0.04
Low education	11.45%	14.53%	-0.03	<0.001
Wage in t = -2	2290.45	2269.31	21.14	0.52
Imputed wage t = -2	1956.18	1956.14	0.04	0.99
Works part-time t = -2	18.63%	16.27%	-0.02	0.08

Notes: Table reports means of demographic characteristics (age, country of birth, education) at the time of childbirth for women who gave birth in January-March (Q1, or treated group) and women who gave birth in October-December (Q4, or control group). Data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). “Low education” refers to the group of mothers who have no vocational training or a university diploma. “High education” refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. To generate imputed wages, all missing wages are set to 0.

Table 1.4: Regression results for the Regression Discontinuity

Panel A: 12-month bandwidth

	Raw wage			Imputed wage		
	t=3	t=13	t=24	t=3	t=13	t=24
2007 birth	0.055 (0.033)	-0.073 (0.054)	-0.063 (0.048)	0.042*** (0.010)	-0.041 (0.028)	-0.052* (0.030)
N obs.	2230	3945	4764	7935	7935	7935
2007 birth + controls	0.069** (0.032)	-0.057 (0.054)	-0.065 (0.050)	0.044*** (0.011)	-0.037 (0.030)	-0.053* (0.032)
N obs.	2187	3758	4496	7394	7394	7394
2007 birth + controls + trends	0.049 (0.062)	-0.155 (0.101)	-0.166* (0.094)	-0.004 (0.023)	-0.159*** (0.060)	-0.187*** (0.066)
N obs.	2187	3758	4496	7394	7394	7394

Panel B: 3-month bandwidth, reform sample

	Raw wage			Imputed wage		
	t=3	t=13	t=24	t=3	t=13	t=24
Q1 birth	-0.0085 (0.048)	-0.07 (0.041)	-0.019 (0.048)	0.018 (0.016)	-0.054** (0.025)	-0.053 (0.033)
N obs.	531	947	1132	1889	1889	1889
Q1 birth + controls	0.011 (0.045)	-0.068 (0.043)	-0.081 (0.049)	-0.011 (0.018)	-0.090*** (0.030)	-0.102*** (0.037)
N obs.	518	843	978	1634	1634	1634
Q1 birth + controls + trends	-0.141 (0.127)	-0.092 (0.136)	-0.095 (0.158)	-0.187*** (0.058)	-0.03*** (0.098)	-0.257** (0.123)
N obs.	518	843	978	1634	1634	1634

Panel C: 3-month bandwidth, non-reform sample

	Raw wage			Imputed wage		
	t=3	t=13	t=24	t=3	t=13	t=24
Q1 birth	-0.016 (0.02)	-0.015 (0.046)	-0.056 (0.062)	-0.017** (0.009)	0.002 (0.024)	-0.063* (0.037)
N obs.	2242	2811	3238	5770	5770	5770
Q1 birth + controls	-0.015 (0.02)	-0.039 (0.046)	-0.061 (0.066)	-0.046*** (0.008)	-0.046* (0.025)	-0.099** (0.043)
N obs.	2218	2668	3003	5217	5217	5217
Q1 birth + controls + trends	0.002 (0.054)	0.143 (0.127)	0.176 (0.138)	-0.112*** (0.029)	-0.081 (0.066)	-0.036 (0.085)
N obs.	2218	2668	3003	5217	5217	5217

Notes: Each coefficient is from a separate regression. The sample includes mothers who gave birth between 2003-2007, and are between ages 18-40 at the time of first childbirth. The Reform sample includes women who gave birth in October 2006-March 2007. The Non-reform sample includes women who gave birth in 2003 October-2004 March, 2004 October-2005 March, 2005 October-2006 March. Control variables are age at first birth, indicator variables for level of education, indicator variable for being born not in Germany, baseline wage in t=-2, and indicator for part-time employment in t=-2. Robust standard errors are in parentheses. *p<0.1, **p<0.05, ***p<0.001

Table 1.5: Regression results for differences-in-regression-discontinuity design

Full sample

	Raw wage			Imputed wage		
	t=3	t=13	t=24	t=3	t=13	t=24
Coeff on RxQ1	0.027 (0.052)	-0.038 (0.066)	-0.025 (0.083)	0.033 (0.21)	-0.048 (0.040)	-0.005 (0.057)
N. obs	2736	3511	3981	6851	6851	6851

Women with low education

	Raw wage			Imputed wage		
	t=3	t=13	t=24	t=3	t=13	t=24
Coeff on RxQ1	-0.106 (0.153)	-0.235 (0.210)	-0.012 (0.286)	0.071 (0.070)	-0.240 (0.137)	-0.199 (0.183)
N. obs	343	350	399	837	837	837

Women with high education

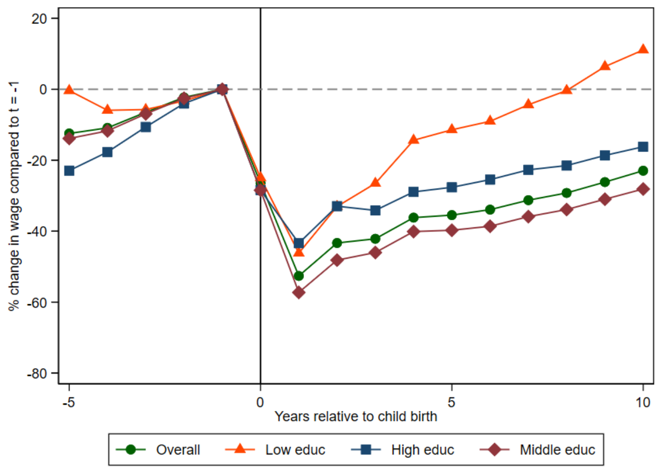
	Raw wage			Imputed wage		
	t=3	t=13	t=24	t=3	t=13	t=24
Coeff on RxQ1	0.078 (0.125)	0.015 (0.098)	-0.037 (0.127)	0.034 (0.057)	0.006 (0.081)	-0.004 (0.100)
N. obs	418	560	587	936	936	936

Notes: Each coefficient is from a separate regression. Regression equation is specified in equation (4) of the main text. The sample includes mothers who gave birth between 2003-2007, and are between ages 18-40 at the time of first childbirth. “Low education” refers to the group of mothers who have no vocational training or a university diploma. “High education” refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. To generate imputed wages, all missing wages are set to 0. Robust standard errors are in parentheses. *p<0.1, **p<0.05, ***p<0.001

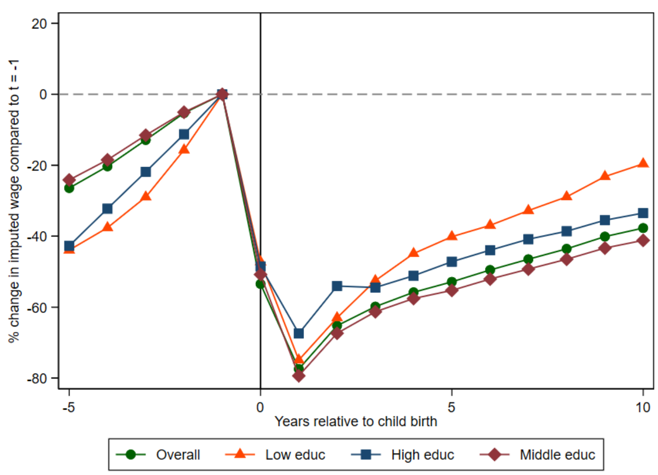
Figures

Figure 1.1: Maternal wage dynamics around childbirth

Panel A: Raw wages



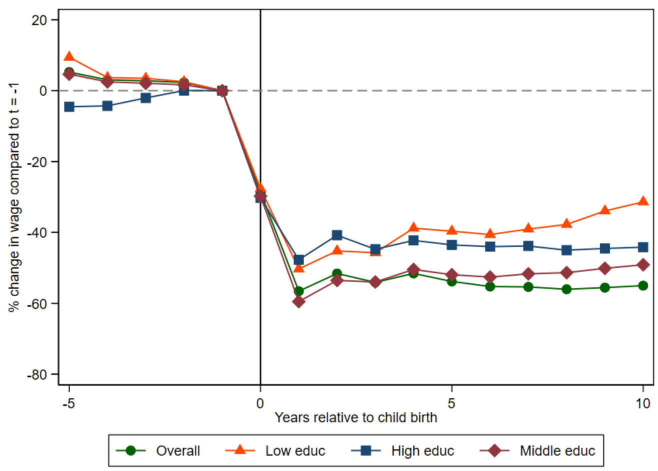
Panel B: Imputed wages



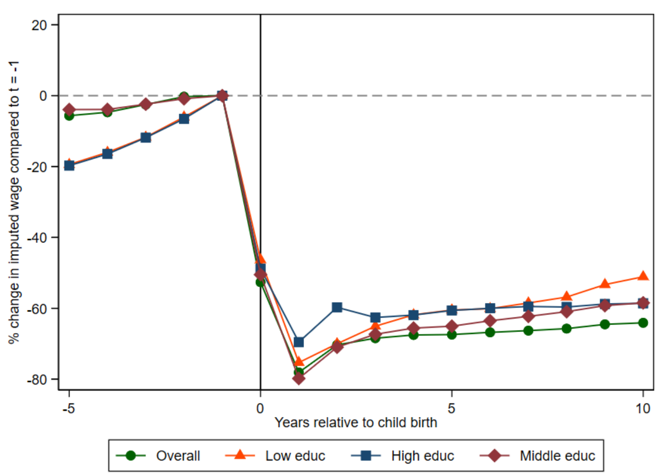
Notes: Figures show mean monthly wages annually for 5 years before and 10 years after childbirth. The analytic sample includes mothers with a first childbirth between 2003-2007 who were between the ages of 18-40 at childbirth. Wage data is only considered for women who are at least 16. Mean wage in the year right before childbirth ($t = -1$) is set as the baseline and all other periods are calculated as a percentage of this baseline wage. Wage data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). “Low education” refers to the group of mothers who have no vocational training or a university diploma. “Middle education” refers to the group of mothers who have a completed vocational training, but no university diploma. “High education” refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. Panel B shows wage dynamics with imputed wages, where all missing wages are set to 0.

Figure 1.2: Maternal wage dynamics around childbirth without age effects

Panel A: Raw wages



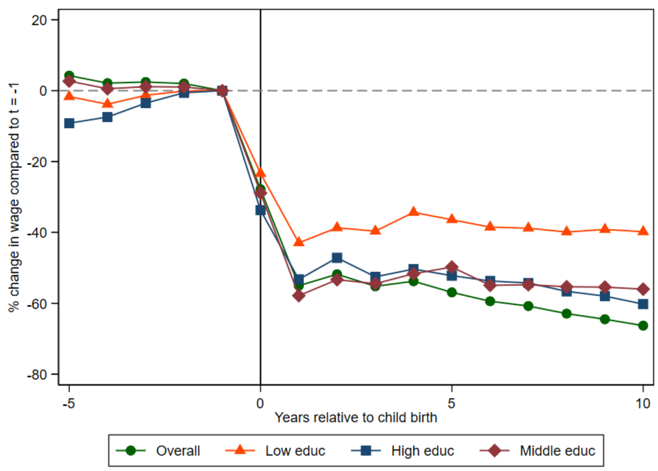
Panel B: Imputed wages



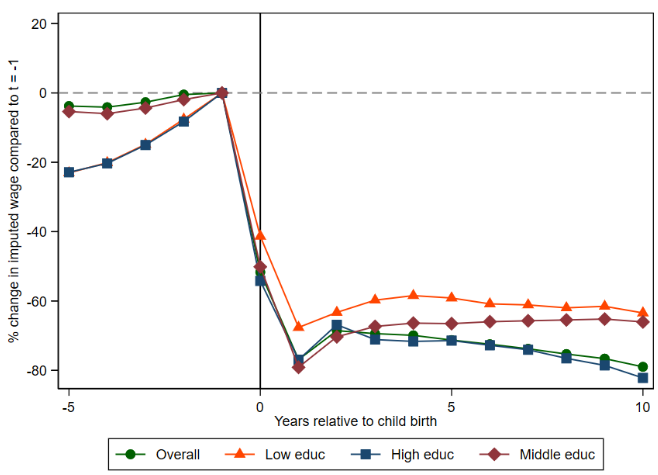
Notes: Figures show coefficients on the event time dummies from regressions of wages on event time dummies and age dummies. The graph can be interpreted as showing mean monthly wages annually for 5 years before and 10 years after childbirth keeping age constant. The analytic sample includes mothers with a first childbirth between 2003-2007 who were between the ages of 18-40 at childbirth. Wage data is only considered for women who are at least 16. Mean wage in the year right before childbirth ($t = -1$) and mean age for each group (29 for the full sample, 25 for the low-education group, 32 for the high-education group) is set as the baseline. All other periods are calculated as a percentage of this baseline wage. Wage data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). "Low education" refers to the group of mothers who have no vocational training or a university diploma. "Middle education" refers to the group of mothers who have a completed vocational training, but no university diploma. "High education" refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. Panel B shows wage dynamics with imputed wages, where all missing wages are set to 0.

Figure 1.3: Maternal wage dynamics around childbirth without age and year effects

Panel A: Raw wages

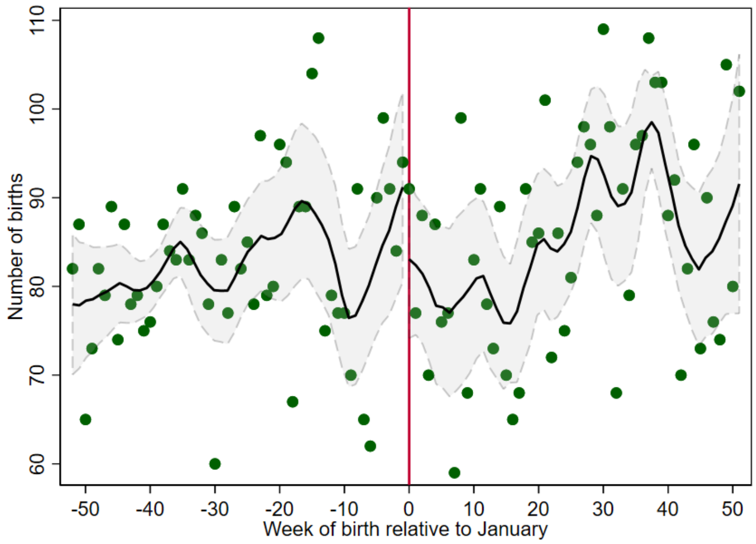


Panel B: Imputed wages



Notes: Figures show coefficients on the event time dummies from regressions of wages on event time dummies, age dummies, and year dummies. The graph can be interpreted as showing mean monthly wages annually for 5 years before and 10 years after childbirth keeping age and economic conditions constant. The analytic sample includes mothers with a first childbirth between 2003-2007 who were between the ages of 18-40 at childbirth. Wage data is only considered for women who are at least 16. Mean wage in the year right before childbirth ($t = -1$) , mean age for each group (29 for the full sample, 25 for the low-education group, 32 for the high-education group), and the mean year (2008) is set as the baseline. All other periods are calculated as a percentage of this baseline wage. Wage data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). "Low education" refers to the group of mothers who have no vocational training or a university diploma. "Middle education" refers to the group of mothers who have a completed vocational training, but no university diploma. "High education" refers to the group of mothers with at least a university diploma. When mothers are not working for pay, when they work in a civil servant job or are self-employed, they have no observations in the dataset. Panel B shows wage dynamics with imputed wages, where all missing wages are set to 0.

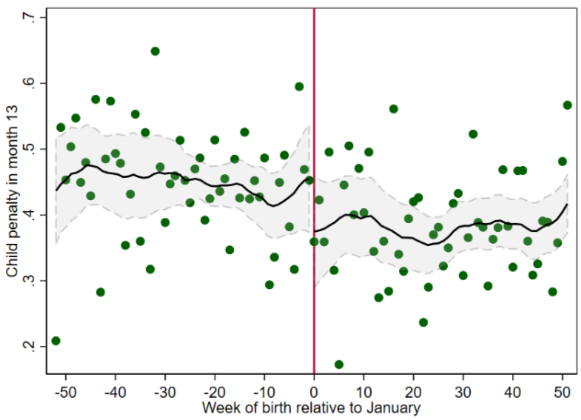
Figure 1.4: Smoothness of birth around policy introduction



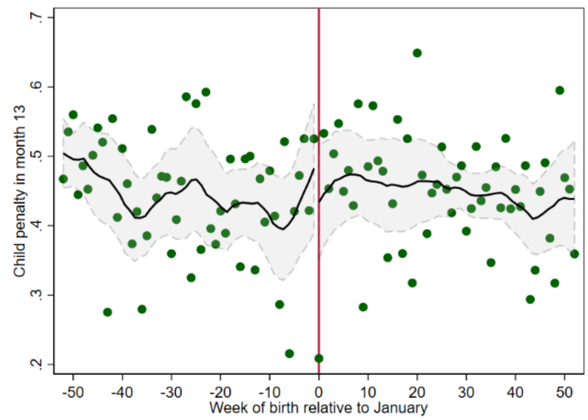
Notes: Graph shows the number of births by week relative to January 1, 2007, which is the date of the policy introduction analyzed in the paper. Data come from the German Sample of Integrated Labour Market Biographies (SIAB) (Berge et al., 2021). Birth date of children is inferred based on the date when mothers go on maternity leave, which is registered in the SIAB. Maternity leave starts 6 weeks prior to one's due date. Local mean-smoothing with 95% confidence intervals is also displayed on the graphs.

Figure 1.5: Wage loss after childbirth by week of childbirth

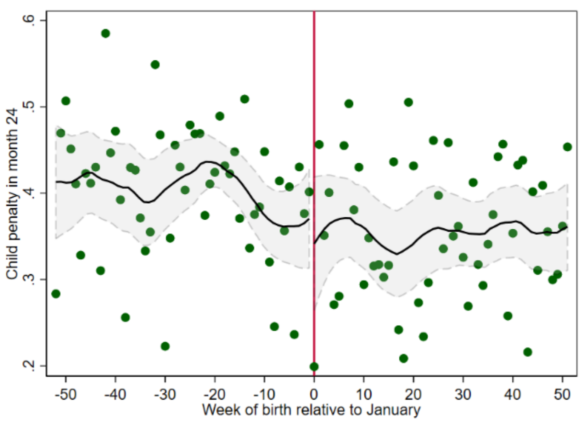
Panel A: t = 13, year 2007 (reform)



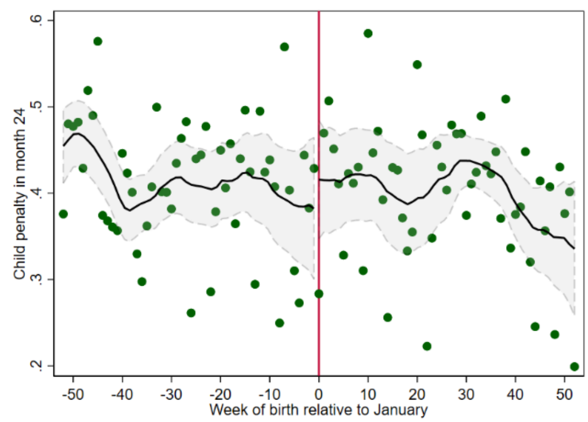
Panel B: t = 13, year 2004-2006 (non-reform)



Panel C: t = 24, year 2007 (reform)



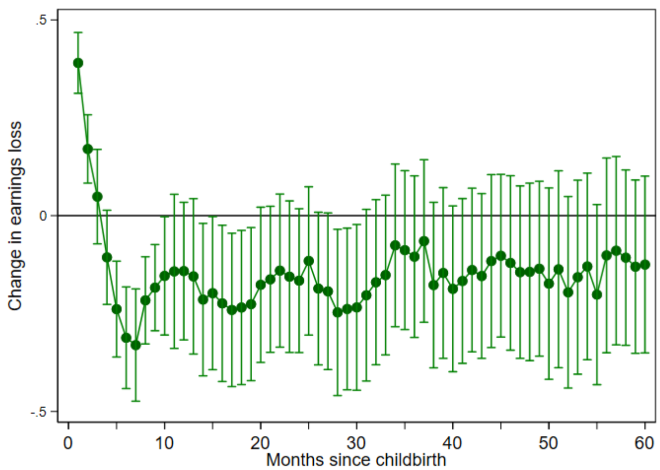
Panel D: t = 24, year 2004-2006 (non-reform)



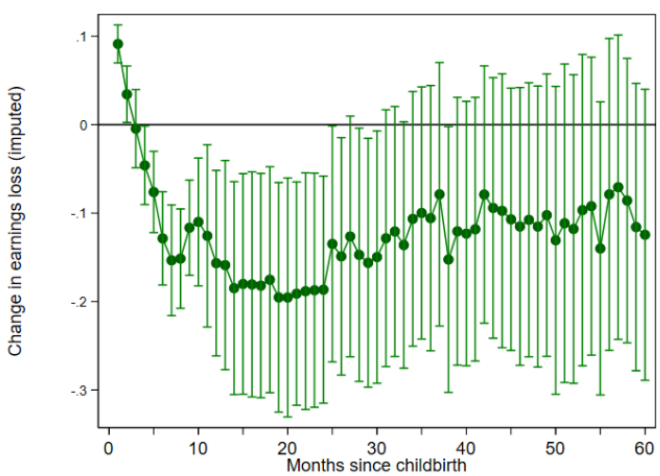
Notes: Figures show mean wage loss after childbirth (the "child penalty") compared to wages 2 months before childbirth, by week of birth. Panels A and B show the wage loss variable 13 months after childbirth, and panels C and D show wage loss 24 months after childbirth. Panels A and C use 2007, the reform year, and panels B and D use 2004-2006, the non-reform years pooled. The vertical red line indicates January 1st. Local mean-smoothing with 95% confidence intervals is also displayed on the graphs.

Figure 1.6: The effect of the paid leave policy change on maternal wage loss

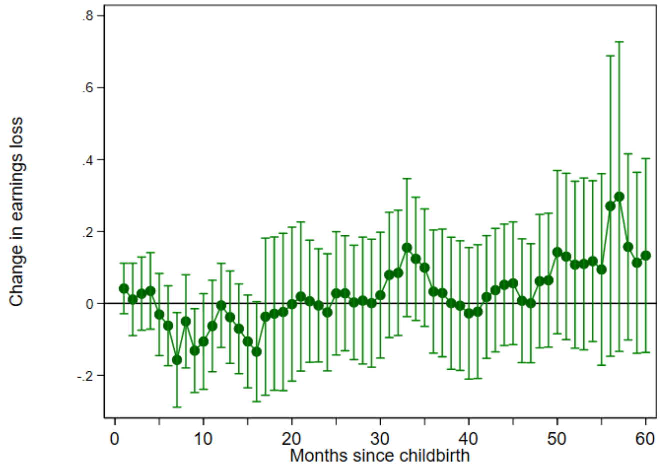
Panel A: Raw wages, 12-month bandwidth



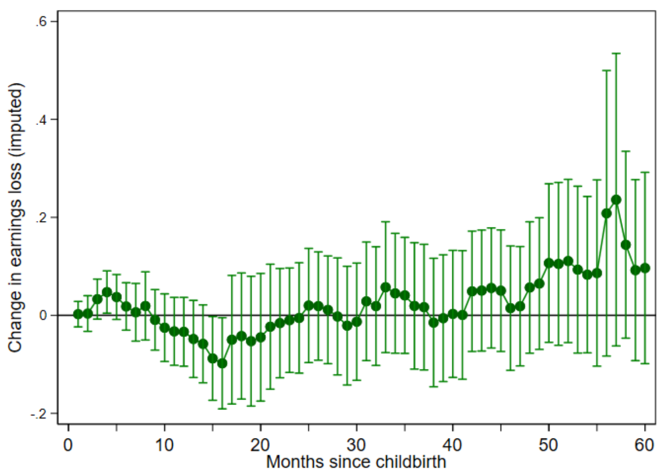
Panel B: Imputed wages, 12-month bandwidth



Panel C: Raw wages, 3-month bandwidth



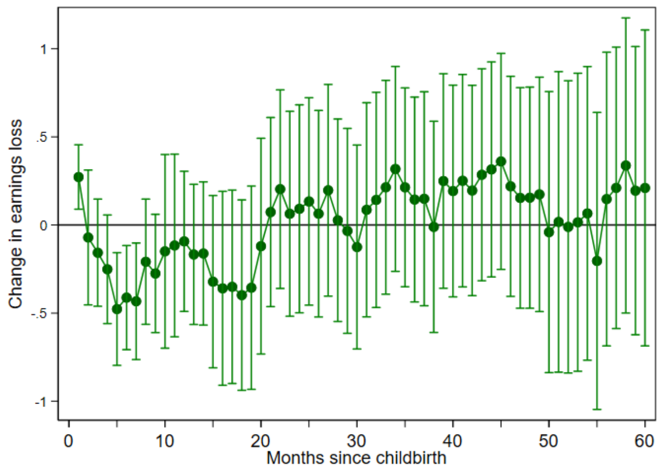
Panel D: Imputed wages, 3-month bandwidth



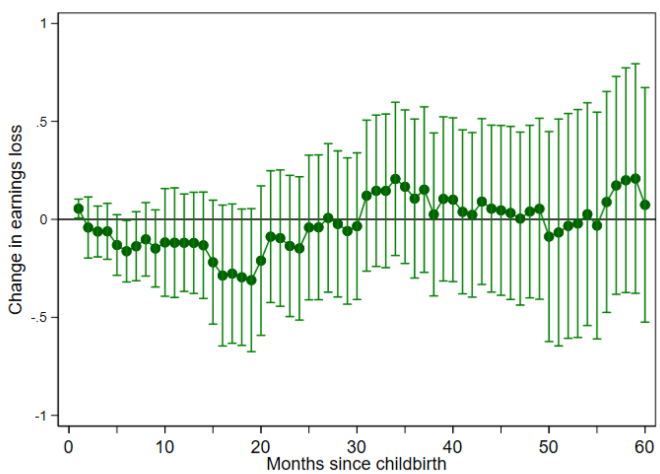
Notes: Figures show estimated coefficients from regressions in the fully specified form of equations (1) (Panel A and B) and equation (5) (Panel C and D). Point estimates and 95% confidence intervals come from separate regressions where the outcome is wage loss t months after childbirth (t = 1,2,..,60) compared to pre-birth wages. The coefficients should be interpreted as percentage point differences in wage loss as a result of the policy. Coefficients below 0 mean that wage loss became smaller, so the drop in wages between pre-child and post-child periods is smaller after the policy change. Panels A and B use sample of mthers who gave birth in 2006 and 2007. Panels C and D use the full sample of mothers who gave birth in 2003Q4, 2004Q1, 2004Q4, 2005Q1, 2005Q4, 2006Q1, 2006Q4, and 2007Q1. Panels A and C show raw wages as they appear in the dataset. Panels B and D use imputed wages where all missing observations are set to 0.

Figure 1.7: The effect of the paid leave policy change on maternal wage loss: mothers with low education

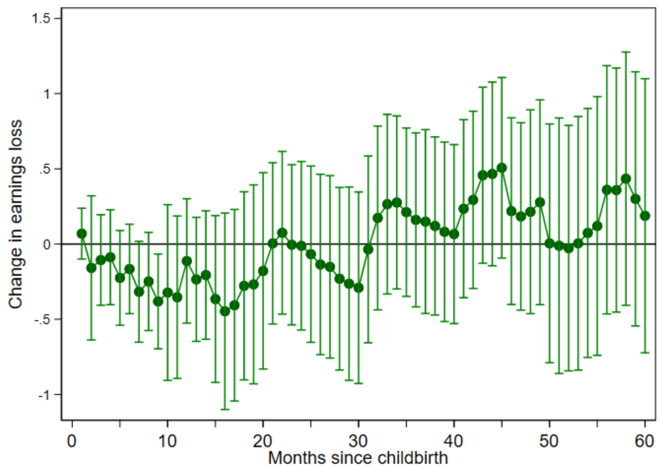
Panel A: Raw wages, 12-month bandwidth



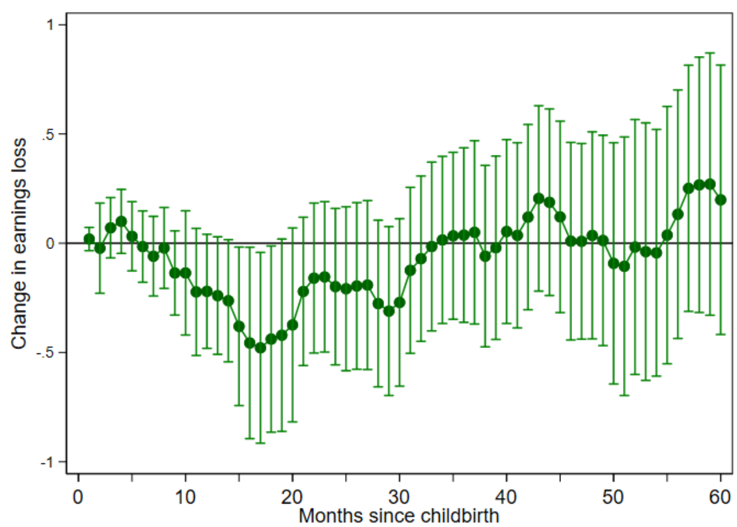
Panel B: Imputed wages, 12-month bandwidth



Panel C: Raw wages, 3-month bandwidth



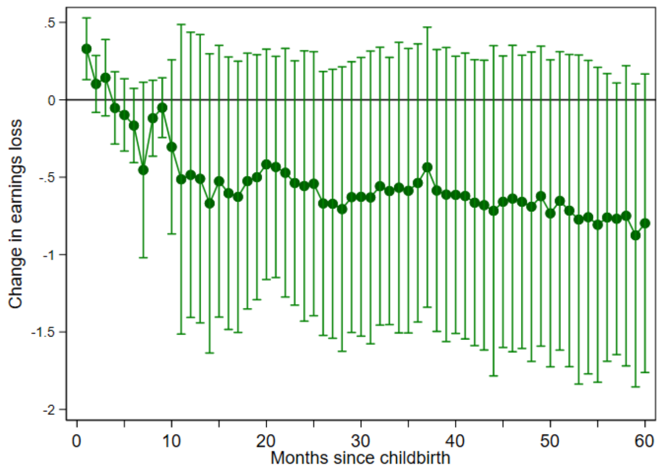
Panel D: Imputed wages, 3-month bandwidth



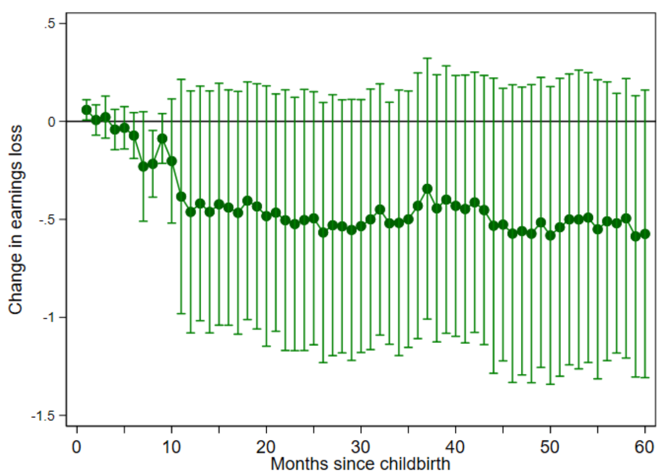
Notes: Figures show estimated coefficients from regressions in the fully specified form of equations (1) (Panel A and B) and equation (5) (Panel C and D). Point estimates and 95% confidence intervals come from separate regressions where the outcome is wage loss t months after childbirth (t = 1,2,..,60) compared to pre-birth wages. The coefficients should be interpreted as percentage point differences in wage loss as a result of the policy. Coefficients below 0 mean that wage loss became smaller, so the drop in wages between pre-child and post-child periods is smaller after the policy change. The graphs show estimates for mothers who did not have any vocational training or university degree at time of childbirth. Panels A and B use sample of mthers who gave birth in 2006 and 2007. Panels C and D use the full sample of mothers who gave birth in 2003Q4, 2004Q1, 2004Q4, 2005Q1, 2005Q4, 2006Q1, 2006Q4, and 2007Q1. Panels A and C show raw wages as they appear in the dataset. Panels B and D use imputed wages where all missing observations are set to 0.

Figure 1.8: The effect of the paid leave policy change on maternal wage loss: mothers with high education

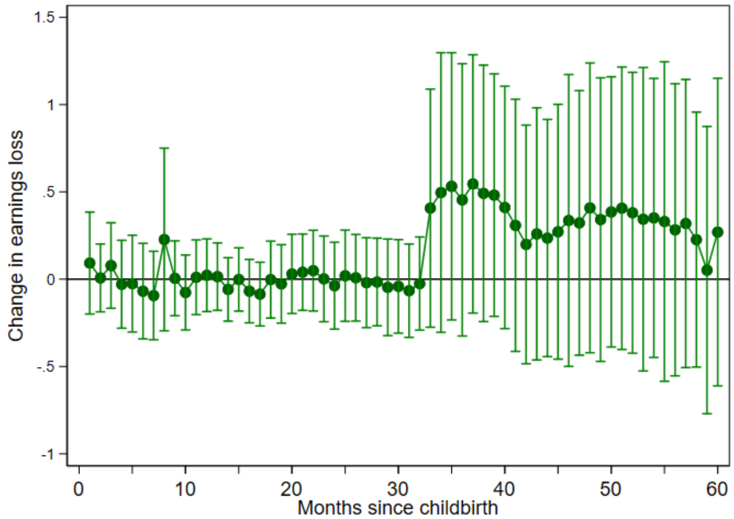
Panel A: Raw wages, 12-month bandwidth



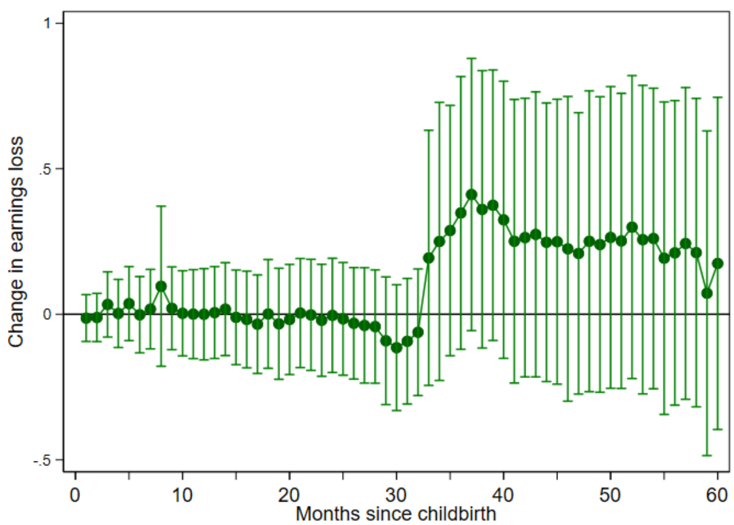
Panel B: Imputed wages, 12-month bandwidth



Panel C: Raw wages, 3-month bandwidth



Panel D: Imputed wages, 3-month bandwidth



Notes: Figures show estimated coefficients from regressions in the fully specified form of equations (1) (Panel A and B) and equation (5) (Panel C and D). Point estimates and 95% confidence intervals come from separate regressions where the outcome is wage loss t months after childbirth (t = 1,2,..,60) compared to pre-birth wages. The coefficients should be interpreted as percentage point differences in wage loss as a result of the policy. Coefficients below 0 mean that wage loss became smaller, so the drop in wages between pre-child and post-child periods is smaller after the policy change. The graphs show estimates for mothers who had at least a university degree at time of childbirth. Panels A and B use sample of mthers who gave birth in 2006 and 2007. Panels C and D use the full sample of mothers who gave birth in 2003Q4, 2004Q1, 2004Q4, 2005Q1, 2005Q4, 2006Q1, 2006Q4, and 2007Q1. Panels A and C show raw wages as they appear in the dataset. Panels B and D use imputed wages where all missing observations are set to 0.

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