Does Earlier Return to Work Enhance Mothers' Labor Trajectories? – Evidence on Substitution Effects in Paid Parental Leave*

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Abstract

In this paper, we identify the substitution effect in paid parental leave ('PL'), by exploiting a Hungarian reform that affected only the possibility of employment during the receipt of PL benefits, while keeping the overall benefit amounts unchanged. Hungarian mothers whose child turned into their 2^{nd} year of age after 2014 were provided strong incentives to return to the labor market earlier than previous cohorts, since they did not have to give up PL benefits while earning their wage income during their children's 2^{nd} year of age. Using an Event Study research design with women without births in the control group and Hungarian linked employer-employee data, we find that mothers in eligible cohorts were by 3 percentage points (30 percent) more likely to work 19–24 months after giving birth – but, no further differences exist in their working propensities or its timing. Our results suggest no effects, on average, on mothers' wages but positive effects on their firms' average wage premium 3-5 years after giving birth, and that eligible mothers sorted to occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure.

Keywords: Parental leave, Maternal labor supply, Fertility, Substitution effect
JEL codes: J13, J16, J18, J22

1 Introduction

Career interruptions due to giving birth to children are known to contribute to the gender pay gap (Bertrand et al., 2010; Angelov et al., 2016; Kleven et al., 2019), and parental leave policies have been found to strongly affect the length of career interruptions around childbirth (Kleven et al., 2023). At the same

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time, the literature is inconclusive about whether the length of career interruptions itself affect women's labor market trajectories in the long-run. Earlier cross-country studies on parental leave policies tend to find positive employment effects of short parental leave duration, but negative effects of longer parental leave duration (Olivetti and Petrongolo, 2017), while more recent micro-level studies, exploiting parental leave reforms, suggest that the duration of maternity leave does not affect negatively the employment or earnings of mothers in the long run. For instance, Rossin-Slater et al. (2013) analyze the effects of the introduction of paid leave program in California and find that it did not significantly affect employment but had positive effect on hours worked and wage incomes 1-3 years after childbirth (while Bailey et al. (2024) do not find evidence that the same policy increased employment or boosted earnings in the longer run). Schönberg and Ludsteck (2014) analyze a series of expansions of maternity leave duration in Germany and find that each expansion led to some delay in the return to work of mothers, however, the expansions did not have an effect on employment rates and labor market income 3–6 years after childbirth. Dahl et al. (2016) show that the expansion of paid maternity leave in Norway increased the time spent out of the labor market, but it did not affect employment or earnings in the long run. Lalive et al. (2014) and Kleven et al. (2023) arrive at similar conclusions based on a series of reforms affecting the parental leave duration in Austria. Yet, Kuka and Shenhav (2024) show evidence from the US that increasing employment beyond the first year after a first childbirth has positive long-run effect on wages.[1,2]

One potential reason for the inconclusive findings on whether the length of career interruptions affect women's long-run labor trajectories is that the two effects inherent in paid parental leave—the substitution effect and the income effect—may have different relative importance across previous studies. Following Autor and Duggan (2007), the substitution effect arises due to that paid parental leave recipients face an incentive not to work as employment would mean sacrificing the benefits, while the income effect captures that given the parental leave benefits, beneficiaries may prefer leisure to labor, even if working is possible while receiving the benefits. Parental leave reforms may also affect workforce composition, thereby affecting long-run wage estimates through compositional changes, differently across prior studies.

Our first contribution is to identify the substitution effect of paid parental leave on mothers' long-term employment propensities, by leveraging a Hungarian reform that affected only the possibility of working—essentially abolished the return-to-work restriction—during the receipt of parental leave benefits in the child's 2^{nd} year of age, while keeping the overall benefit amounts unchanged. We estimate the reform's effect on employment until the 5^{th} years after giving birth. While the distinction of the two effects is a central topic in the literature on the labor market effects of DI and UI (Autor and Duggan, 2007; Chetty, 2008; Deuchert and Eugster, 2019; Krekó et al., 2022, among others), we are not familiar with studies that distinguish either of the two channels related to paid parental leave. Identification of the substitution effect is important because it shows to what extent paid parental leave distorts the incentives to work.

Using an Event Study research design with women without births in the control group and Hungarian

[1] Looking at the effect on health outcomes, while Bütikofer et al. (2021) show that the introduction of paid maternity leave in Norway had positive effects on maternal health, Chuard (2023) and Ahammer et al. (2023) show that lengthening the duration of parental leave in Austria led to worse mental health.

[2] There is a smaller literature on the labor market effects of parental leave benefit levels. Bana et al. (2020) estimate the effect of maternity benefit amount on labor market outcomes, applying a regression kink design on maternity benefits in California; they find that higher maternity benefits do not affect the duration of maternity leave and increase the probability of returning to the pre-leave employer instead of switching to a new firm. Ginja et al. (2020) use a similar empirical strategy to estimate the effect of parental leave benefit levels in Sweden, and find that higher parental leave levels imply a reduction in the labor supply of mothers both in the extensive and intensive margin, but only in the short-run.

linked employer-employee data, we find that mothers in eligible cohorts were by 3 percentage points (30 percent) more likely to work 19–24 months after giving birth, which estimate shows the substitution effect of paid parental leave in Hungary – but, no further differences exist in their working propensities or its timing. We find the large effect on the propensity to work exactly in the period when they have the option to be employed and keep the parental leave benefit at the same time. Looking at potential heterogeneities, we find that this estimate are driven by mothers who give birth to their first child after the age of 30.

Our second contribution is to provide clean estimates on the consequences on mothers' career trajectory in terms of wages, firm-specific wage premium and occupation characteristics in the 3^{rd}–5^{th} year after giving birth, when there are no employment effects anymore – thus, our estimates on job characteristics are not confounded by selection concerns. We also verify that this policy did not have an effect on subsequent fertility. We find that the wage of eligible mothers do not differ from that of ineligible mothers in the 3^{rd}, 4^{th} and 5^{th} year after giving birth, relative to the control women, but eligible women's firm quality (measured by the firm-specific wage premium) and occupation characteristics may have been affected.

In what follows, Section 2 introduces the parental leave system in Hungary, and illustrates how the reform abolished the return-to-work restriction during the receipt of parental leave benefits. Section 3 describes the data and measurement of key variables, and Section 4 describes our empirical strategy. Section 5 presents and discusses our preliminary results. Finally, Section 6 concludes.

2 Institutional Background

2.1 Paid Parental Leave in Hungary

Act LXXXIII of 1997 of the Hungarian Labor Code establishes three main types of parental benefits: (1) baby-care allowance, (2) childcare benefit, and (3) childcare allowance. Since most of the beneficiaries of the Hungarian parental benefits are women, we refer to the three types of parental benefits jointly as "maternity benefit", and we refer to recipients as mothers. The baby-care allowance is payable for twenty-four weeks, the childcare benefit is payable after baby-care allowance for up to two years, and the childcare allowance is payable after baby-care allowance and childcare benefit (or immediately after giving birth if the mother is not eligible for baby-care allowance and childcare benefit) for up to three years.

The baby-care allowance and the childcare benefit are social insurance benefits, based on prior employment and earnings. Parents could gain access to these benefits by accumulating at least 365 days of social insurance coverage during two years preceding childbirth. Both the baby-care allowance and childcare benefit provides 70% of the average pre-birth wage, but the maximum amount of childcare benefit is capped at 140% of the minimum wage. To reach this cap mothers needs to earn at least 200% of the minimum wage before giving birth. Because of the capped benefit, the replacement rate for higher-earning mothers is lower than 70%. Meanwhile, childcare allowance is a fixed allowance that is available regardless of labor market history. The amount of childcare allowance equals the minimum old-age pension, irrespective of the pre-childbirth wage of the mother.

Act LXXXIV of 1998 established restrictions on doing paid work while receiving maternity benefit; we provide an overview of the regulations on paid work during parental leave in Table 1. Until the $CB{+}W$ ("childcare benefit + wage") reform in 2014, mothers had to forgo their childcare benefit if they decided to return to work before the $2nd$ birthday of their youngest children. Similarly, mothers could only keep their

Table 1: Restrictions on Paid Work During Parental Leave

	Age of child			
	0-6 months	7-12 months	13-24 months	25-36 months
2007-2013	No paid work allowed	No paid work allowed	No paid work allowed	At most 30 hours per week, no restrictions on paid work at home
2014-2015	No paid work allowed	No paid work allowed	No restrictions	No restrictions
2016-2017	No paid work allowed	No restrictions	No restrictions	No restrictions

Notes: We present the restrictions on paid work for mothers eligible for parental leave benefit and childcare benefit. The new regulations from 2014 and 2016 applied also to children born before 2014 and 2016, respectively.

childcare allowance in case they did not work more than 30 hours per week. These work restrictions acted as a disincentive for mothers to return to work, which caused the sudden jumps in maternal employment right at the running out of childcare benefit (at 24 months) and childcare allowance (at 36 months).

Regarding job protection, mothers enjoy strong job protection by the Hungarian Labor Code. Starting from the 12^{th} week of pregnancy, women can announce officially their pregnancy to their employers, from which point employers cannot lay them off. Mothers are entitled for unpaid and protected leave between the birth and 3rd birthday of their youngest child, during which time they accumulate vacation days. During the maternity and parental leave mothers can return to work at any time they would like to, and within 30 days employers have to offer them a similar position and wage they held before giving birth. Upon request employers also need to accommodate mothers who would like to continue to work in a part-time position until the 3rd birthday of their youngest child.

Regarding mothers' opportunities to leave their children in facilities upon return to work, their children are eligible for attending early child care centers ('nurseries'), which are state-funded and run by local governments. Nearly 14% of children under age 3 attended one of these centers in 2013, which is lower than the European average of 29% (Makay, 2015). The tuition is usually progressive and heavily subsidized in these centers: most families only need to cover the dining cost of their children (around 10% of monthly minimum wage), while even for the highest earning families the tuition is no more than 15-20% of the monthly minimum wage (Makay, 2012). In theory, children can start early child care education at any time of the year, but because of the excess demand for early child care, most spots open up in September, when elder children start kindergarten. Families need to apply for these spots between March and May, decisions are made during May and June, usually just before the 3-6 week-long summer break. Due to the cyclical nature of new openings, many children start their early child care education in September or October, making it more likely that mothers will return to work in the second half of the calendar year.

2.2 The $CB+W$ Reform

Starting with January 1, 2014, the Hungarian government implemented the "Childcare Benefit Extra" ("GYED Extra") reform, which is governed by the Act CCXXIV of 2013. The goal of the reform was to increase the number of births and help mothers returning to work from maternity leave by easing some restrictions of the previous parental leave system. The reform had the following four main pillars:

I. *Decreasing work restrictions during maternity leave:* The reform abolished all work restrictions after the first birthday of the child, independently from the year the child was born. This change made the biggest impact on mothers receiving childcare benefits, who could return to paid work one year after giving birth without forgoing their benefits. Similarly, this pillar allowed mothers receiving childcare allowance and work at full-time jobs at the same time. In 2016 the government lowered the age threshold from 12 to 6 months.

II. *Sibling benefits/allowance:* For children born on/after January 1, 2014, families receive childcare benefit/allowance for all their children below the age of 3. Before the reform, the birth of a younger child crowded out benefits of older siblings,[3] and this change intended to motivate families to decrease the gap between childbirths by terminating the previously prevailing financial disincentives.

III. *Twin childcare benefits:* Eligible mothers with twins born on or after January 1, 2014, could have received childcare benefits for 3 instead of 2 years.

IV. *Childcare benefits for college and graduate students*: Active students with at least one year of college education became eligible for receiving childcare benefits if their children are born on or after January 1, 2014. Before the reform they received only the lower childcare allowance.

The reform was unexpected and sudden. The raw details of the reform were first communicated following a cabinet meeting of the Hungarian Government on November 7, 2013, and the draft bill was published on November 18. The Hungarian National Assembly voted on the draft on December 9, and the final version of Act CCXXIV of 2013 was published on December 18, 2013. Finally, the changes the act introduced started with January 1, 2014, less than a month after it was voted on.

Because of the short time window between the introduction and implementation of the reform, the policy change was truly unexpected for families. This is especially true for mothers giving birth just before the January 1, 2014 cutoff, which made them ineligible for Pillars II-IV of the reform. The quick creation and implementation of the reform meant that women with small children at early 2014 had no knowledge of the reform during their pregnancy, and the reform could not affect their timing of having children.

However, Pillar II of the reform could have affected future fertility decisions of these mothers. By eliminating the crowding out of current maternity leave benefits, Pillar II could have motivated mothers to decrease time between subsequent births. In this way Pillar II could have acted as a disincentive for mothers to return to earlier to work, which was one of the main aim of the reform's Pillar I. As later we show, overall the reform increased maternal employment during parental leave, which suggests that Pillar I' financial incentives for returning to work exceeded the disincentives Pillar II created. Meanwhile, we did not find evidence on that the reform affected mothers' future fertility, which suggests that Pillar II's effect on timing of future births was moderate.

In this paper, we focus on Pillar I of the Act CCXXIV of 2013, which we refer to as $CB{+}W$ ("childcare benefit + wage") – the abolishment of work restrictions after the first birthday of the child. Hungary is

[3] We can illustrate the policy change through an example of a mother who receives childcare benefit after the birth of her first child. If this mother gives birth to her second child when her firstborn is 18 months-old, then both before and after the reform she would have received parental leave benefit. But, before the reform she would have lost the childcare benefit of his first children, because the higher parental leave benefit of the first child crowded out the childcare benefit of the second child. As the reform abolished this crowd out effect, after the reform this mother would have received both the parental leave benefit and the childcare benefit as well.

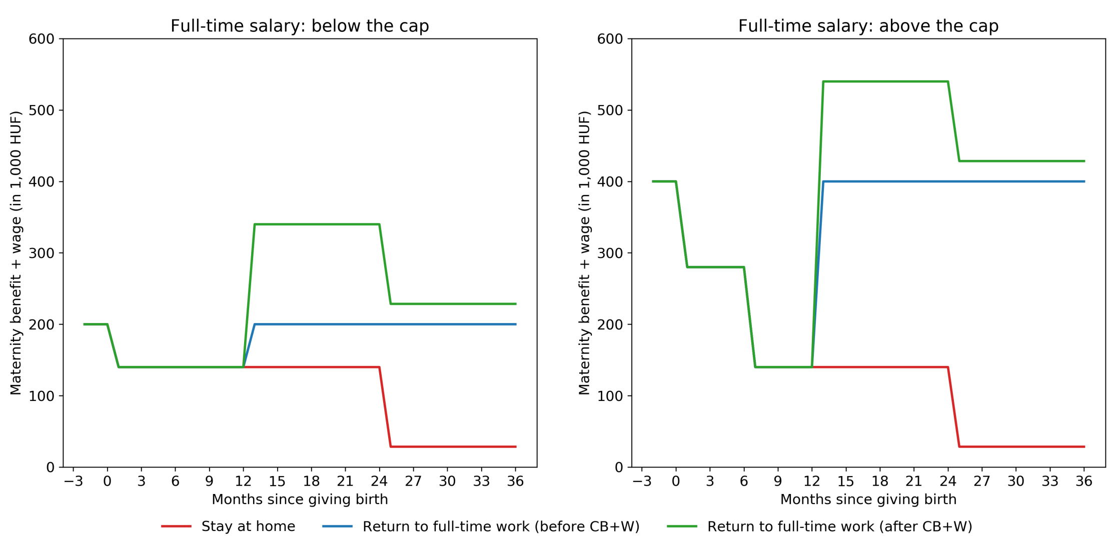
one of the few OECD countries (among Czechia, Latvia, and Slovakia) which allows parents to keep all parental leave benefits in case of returning to work, while Estonia cap benefits if wage income exceeds 50% of the parental leave benefits (Bičáková and Kalíšková, 2022, OECD, 2024). Some other OECD countries (France and Slovenia) allows parents to work part-time during parental leave and receive time-adjusted parental leave benefits, while others (Belgium, Germany, and Sweden) gives more flexibility to parents to allocate their total available parental benefits in case of part-time employment. However, in these parental leave systems the total income of parents during parental leave cannot exceed the pre-birth wage in case of returning to work, while in Hungary the $CB{+}W$ reform creates a temporary income jump, which could increase the pre-birth income by as much as 70%.

2.3 Financial Incentives of the $CB{+}W$ Reform: A Numerical Example

We illustrate the effect of the $CB{+}W$ reform through a numerical example, which shows that the reform created stronger incentives for lower-income women to return to work before the child's 2^{nd} birthday.

In both panels of Figure 1, we depict how the reform affected the income of a hypothetical childcare benefit eligible mother's decision on whether to stay at home or return to work at the first birthday of her youngest child. In the left panel the women's pre-birth salary is assumed to be twice the minimum wage, which is exactly the cutoff of the capped childcare benefit; her childcare benefit is 70% of her pre-birth wage. Meanwhile, in the right panel of Figure 1, our hypothetical mother's pre-birth wage income is assumed to be four times the minimum wage. This higher-income mother receives the maximum amount of childcare benefit (140% of the minimum wage) and her replacement rate is 35%.

Figure 1: The effect of $CB{+}W$ reform on mothers' income



Notes: Figure illustrates how the reform affected a childcare benefit eligible women who needs to decide whether stay at home or return to work at the 1st birthday of her younges child. In the left panel this women's pre-birth income is twice the minimum wage, meaning that her replacement rate is 70%. In the right panel the pre-birth income of the mother is four times the minimum wage, which is over the childcare benefit cutoff. She receives the maximum amount of childcare benefit (140% of minimum wage), and thus her replacement rate is 35%.

Compared to lower-income mothers, before the $CB{+}W$ reform, higher-income mothers had stronger financial incentives to return earlier. Because of their higher replacement rate, lower-income mothers gained less additional income by an earlier return. In our numerical example, the lower-earning mother in the left panel would gain 30% of her pre-birth income in case of returning work, which might be only slightly higher than the extra costs of working in a full-time position (such as childcare, transportation, etc.). In contrast, because of her lower replacement rate, for the higher-income mother returning to work would increase her income by 65% of her pre-birth wage. Hence, relative to their pre-birth income, before the policy change higher-income mothers could gain more by early return to work.

The *CB+W* reform allowed mothers to keep both their benefits and wage income in case of returning to work before the child's 2^{nd} birthday, which increased the opportunity cost of staying at home. Counter to the previous parental leave system, the *CB+W* reform created higher financial-incentives to return to work for lower-income mothers. In our numerical example, following the reform the lower-income mother could receive 170% of her pre-birth income until the 2nd birthday of her child by returning to full-time work. Thus, compared to pre-reform, the income gain of returning to work for this women increased from 30% to 100% measured in pre-birth income. In contrast, the higher-income mother in our example receives 135% of her pre-birth wage in case of returning to work after the *CB+W* reform, and for her the reform increased the income gain of returning to work from 65% to 100%. Thus, compared to their pre-birth income, the policy change created an opportunity for a temporary 70% income boost for the lower-income woman, which was only 35% for the higher-income mother in our example.

In sum, the *CB+W* reform created a strong financial incentive to return to work before the child's 2^{nd} birthday, given mothers could keep the childcare benefit while working. Considering the income gain of returning to work after the reform *relative* to the gain before the reform, the reform provided incentives to return to work earlier particularly for lower-earning mothers because of their high replacement rate.

3 Data

3.1 Data Source

Our main dataset is an employer-employee linked panel dataset, covering 50 percent of the Hungarian population with a social security number in 2003, which, besides information on labor outcomes, also contains administrative data on medical records and drug expenditures, for years 2009–2017.[4]

The employment-related data, containing the identity of the employer (firm), the type of employment, wage, occupation, and working hours, is provided by the *Hungarian Central Administration of National Pension Insurance*. It contains all sources of income liable for paying social security contribution and all employment counted in the length of service as a base for pension. Information on transfers is from the *Hungarian Central Administration of National Pension Insurance* and the *Hungarian National Health Insurance Fund Administration* for maternal benefit, disability benefit and pension, and from the *Hungarian Ministry of Finance* for unemployment benefit.

Data on inpatient and outpatient episodes, and spending on prescribed medication is available from 2009, from the *Hungarian National Health Insurance Fund Administration*. Births and miscarriages can be

[4]The administrative database used in this paper is a property of the National Health Insurance Fund Administration, the Central Administration of National Pension Insurance, the National Tax and Customs Administration, the National Employment Service, and the Educational Authority of Hungary. The data was processed and provided by the Databank of the HUN-REN Centre for Economic and Regional Studies in Hungary.

identified using International Classification of Diseases (ICD) codes O80-O84 and O00-O08, respectively.

For each individual we observe gender, age, amount of unemployment and child-related and pension benefits, number of days in hospital by month broken down by the cause of hospitalization (births, miscarriages, *etc.*), number of prescriptions by month, and monthly amount spent by drug category (antibiotics, antidepressants, *etc.*). To the extent the individual works in that month, we also observe monthly wage income, monthly hours worked, occupation, industry, and firm identifier.

On the firms' side, the data includes financial data, employment, industry, and foreign ownership data from the *Hungarian National Tax and Customs Administration*, for all double-entry bookkeeping firms in Hungary, reported by the firms annually (in balance sheets, and profit and loss statements).

Finally, we link the Occupation Information Network Database (O*NET) to our data, which describes the task content of occupations (*see* O*NET Resource Center).

3.2 Samples: Birth Sample and Control Sample

To construct our *birth sample*, we follow the steps below:

1. We identify women with birth ($N = 262,168$), and their month of *first birth* in $2009 - 2017$.

2. To ensure that we observe women who give birth for the 1^{st} time, we keep mothers in the sample who did not receive any parental benefits preceding the observed *first birth* ($N = 166,873$).

3. We keep mothers who were $25 - 40$-years-old at the time of birth ($N = 124,737$).

4. To see the effect of the *CB+W* policy change introduced in 2014 January, we select two cohorts, based on when their child was between 13 and 24 months old:

   (a) *CB+W*-eligible for $7 - 12$ months (*i.e.*, for most of the child's 2^{nd} year of age): mothers whose child turns 13 months old in the time frame 07/2013–12/2013;

   (b) *CB+W*-ineligible: mothers whose child turns 13 months old in 07/2012–12/2012.

   We select these two cohorts to ensure that the *CB+W*-eligible and *CB+W*-ineligible cohorts are affected by similar aggregate shocks (*i.e.*, the time of their childbirth is close to each other), and give birth in the same season (therefore we have the six-month gap between the cohorts). ($N = 14,626$)

5. We keep mothers who were eligible for parental leave benefit and childcare benefit, *i.e.*, being employed for at least 12 months in the two years preceding the month of birth. Since there are measurement errors in the employment indicator during pregnancy, we restrict the sample to women whom we observe to receive childcare benefit at least for a month before the child turns two years old ($N = 11,667$).

For the *control sample*, unaffected by the reform, we select women who never had a child or a diagnosis related to pregnancy, for whom the event date is chosen randomly in a way that the age distribution of the control group at the event date is the same as that of women in the *birth sample* ($N = 10,138$).

3.3 Variable Definitions

— *Employment.* Employment (or working) is defined as, for a given month, having non-zero wage income. We exclude employment under the public work scheme (which is essentially a substitute for unemployment in Hungary), and those months when a women is on maternity leave at least for a day.
— *Hourly wage.* We measure (the logarithm of) deflated hourly wage, for a given month, as: $\ln(w_{hour}) = \ln\left(\frac{\text{wage income}_m}{\frac{\text{days in month}}{7}\text{working hours}_w}/\text{defl}_y\right)$, where wage income$_m$ contains all the monthly income which are used to calculate social security contributions. For a given month m, working hours$_w$ contain the weekly working hours, and defl$_y$ is the yearly deflator (year-specific average wage of women in the control group divided by the 2014 average wage of women in the control group). We set the value of the hourly wage to missing if the individual is not employed (as per our definition) in a given month.

One data issue with the wage variables needs to be accounted for: after 2014 at workplaces that are eligible to pay out the parental leave benefit and childcare benefit (benefits payable until the child is two years old), the amount of such benefits may be recorded as part of the wage income. To account for this issue, we do not use the wage income variable for mothers who are less than two years after giving birth.
— *Estimated AKM Firm Effect as a Measure of Firm Quality.* To capture firm quality, which we approximate with the extent to which a given firm is able to pay higher wages, we estimate firm-specific wage premiums, following the tradition of Abowd, Kramarz and Margolis (AKM, Abowd et al., 1999).

Consider the equation for worker i at firm j at time t:

$$\ln w_{ijt} = X'_{ijt}\beta + \vartheta_i + \phi_j + \varepsilon_{ijt}, \tag{1}$$

where $\ln w_{ijt}$ is the logarithmic wage of worker i at firm j at time t, X_{ijt} is a vector of time-varying observable characteristics with a $k \times 1$ vector of returns β, ϑ_i is time-invariant worker ability (or worker type), ϕ_j is the time-invariant firm-specific wage premium, and ε_{ijt} is the time-varying error term.

To get an estimate for a time-invariant firm quality measure, capturing the wage premium, ϕ_j, we estimate the model in (1) using the entire sample of the linked employer-employee data, for years 2003-2017. Following Card et al. (2013), Card et al. (2016), and Card et al. (2018), we include the quadratic and cubic age, and year dummies, in X (but no firm-level variables).[5] After estimating model (1), we take the estimated coefficient vector of $\hat{\phi}$, and call it "estimated AKM firm (fixed) effect". Then, for each year, on the entire sample, we flag firms that have above-median $\hat{\phi}$, and form the deciles, too.
— *Occupation Characteristics.* For this,, we use the O*NET data. We build on Hardy et al. (2018) in assigning O*NET (US-specific) SOC-based occupation categories to European ISCO (International Standard Classification of Occupations) classification and to Hungarian occupation codes ("FEOR" codes). Based on the 2011 February O*NET edition, we create six variables, based on to what extent: (1) the job is achievement-oriented (as in: it requires establishing and maintaining personally challenging achievement goals and exerting effort toward mastering tasks, it requires persistence in the face of obstacles, and it requires a willingness to take on responsibilities and challenges); (2) the job requires leadership and stress tolerance; (3) the job involves leadership and taking responsibility for outcomes; (4) the job involves time pressure; (5) the job requires analytical thinking; and (6) the job's schedule is regular.

[5] Card et al. (2013), Card et al. (2016), and Card et al. (2018) all also include the interaction of these variables with education dummies, but due to data limitations, we are unable to do that.

4 Empirical Strategy

4.1 Effect of the Reform on Employment and Fertility

To build intuition for our preferred specification, we begin with a difference-in-differences model that compares the outcome trajectories of $CB{+}W$-eligible and $CB{+}W$-ineligible women over time. We estimate model (2) separately on the sample of mothers and of women never giving birth.

$$\Omega_{it} = \tilde{\alpha} + \sum_k \tilde{\alpha_k} \mathbb{1}[e_{it} = k] + \sum_k \tilde{\beta_k} \mathbb{1}[e_{it} = k] \times D_i + \tilde{\nu_{it}}^{age} + \tilde{\mu_i} + \tilde{\varepsilon_{it}}, \tag{2}$$

where Ω_{it} is the outcome variable for individual i at monthly date t, e_{it} denotes the months relative to the event, and $\mathbb{1}[e_{it} = k]$ is a set of indicators for i being observed k periods after the event (where negative k's refer to pre-event periods). D_i is a binary indicator which equals one if a women is $CB{+}W$-eligible and zero if she is $CB{+}W$-ineligible. $\tilde{\nu_{it}}^{age}$ denotes age effects, $\tilde{\mu_i}$ denotes individual fixed effects. The coefficients of interest are the $\tilde{\beta_k}$ parameters. We make the normalization $\sum_{k=-36}^{-13} \tilde{\beta_k} = 0$.

In model (2), the estimated effect of $CB{+}W$-eligibility may be confounded with time trends. Because of this concern, our preferred baseline specification is the Event Study regression model (3), comparing the outcome trajectories of $CB{+}W$-eligible and $CB{+}W$-ineligible mothers over time, and including women never giving birth as control group. Then, we also estimate the average policy effect on employment probability 19-24 months after childbirth, using the 3^{rd} and 2^{nd} years preceding childbirth as reference period, and interact it with various baseline characteristics of women.

The Event Study regression model is the following:

$$\begin{aligned} \Omega_{it} = \alpha + \sum_k \alpha_k \mathbb{1}[e_{it} = k] + \sum_k \beta_k \mathbb{1}[e_{it} = k] \times D_i + \\ + \sum_k \gamma_k \mathbb{1}[e_{it} = k] \times B_i + \sum_k \delta_k \mathbb{1}[e_{it} = k] \times B_i \times D_i + \nu_{it}^{age} \times B_i + \mu_i + \varepsilon_{it}, \end{aligned} \tag{3}$$

where we use the same notation as in equation (2). B_i is a binary indicator of ever giving birth. $\nu_{it}^{age} \times B_i$ denotes age effects interacted with the binary indicator of ever giving birth, μ_i denotes individual fixed effects. The coefficients of interest are the δ_k parameters. We make the normalization $\sum_{k=-36}^{-13} \delta_k = 0$.

We also estimate the average effect of the reform on employment over 19-24 months after childbirth:

$$\begin{aligned} \Omega_{it} = \alpha + \alpha_1 \, \text{afterM19-24}_{it} + \beta \, \text{afterM19-24}_{it} \times D_i + \gamma \, \text{afterM19-24}_{it} \times B_i + \\ \delta \, \text{afterM19-24}_{it} \times B_i \times D_i + \nu_{it}^{age} \times B_i + \mu_i + \varepsilon_{it}, \end{aligned} \tag{4}$$

where the afterM19-24$_{it}$ dummy variable equals zero in the 3^{rd} and 2^{nd} years before childbirth, and equals one 19-24 months after childbirth. The coefficient of interest is the δ parameter.

In the model for fertility (equation (5)), we do not include the control group and replace the individual fixed effects with a vector of control variables of age, age squared, log of average wage 7-24 months before childbirth, deciles of average AKM firm effect 7-24 months before childbirth (missing values as a separate category), mode of one-digit occupation 7-24 months before childbirth (missing values as a separate category), county of living 12 months before childbirth. Here we do not include individual fixed effects because the outcome variable has little time variation, without any time variation for those mothers who did not have a second birth in our observation period.

$$\Omega_{it} = \lambda + \sum_k \lambda_k \mathbb{1}[e_{it} = k] + \sum_k \theta_k \mathbb{1}[e_{it} = k] \times D_i + X'_{it}\eta + \epsilon_{it}. \tag{5}$$

In this specification, we restrict the sample to months 6 to 60 after the birth of the first child. The outcome variable is the binary indicator of giving birth in the given month. The coefficients of interest are the θ_k parameters. We make the normalization $\theta_6 = 0$.

4.2 Effect of the Reform on Job Quality

To investigate the effect of $CB{+}W$-eligibility on job quality, we focus only on the 3^{rd}, 4^{th} and 5^{th} years after giving birth, in years in which there are no employment effects anymore. By definition, job quality is observed only for the employed, and to avoid selection bias, we do not estimate the effect of $CB{+}W$-eligibility on job quality in the first two years after childbirth, when we show that $CB{+}W$-eligibility affects employment. As job quality indicators, we use the logarithm of hourly wage, estimated AKM firm effect, firm size deciles, employment in a white-collar job, and O*NET occupation scores. We also estimate the effect of $CB{+}W$-eligibility on working hours.

We estimate equation (3) with the job quality indicators as outcome variable. We restrict the sample to event times -36 to -12 and 25 to 60. We also estimate the average effect of the reform on job quality indicators and working hours in the 3^{rd}, 4^{th}, and 5^{th} year after childbirth, by using an analogous version of equation (4), but replacing afterM19-24$_{it}$ with a vector of (afterY3$_{it}$, afterY4$_{it}$, afterY5$_{it}$), denoting the 3^{rd}, 4^{th}, and 5^{th} year after childbirth, respectively.

4.3 Robustness: Event Study Research Design on Matched Sample

Our results show imbalance in the pre-birth (or pre-pseudo birth) employment rate of women ever giving birth ($B_i = 1$) and not giving birth ($B_i = 0$) (Panel (a) of Figure 2). To check and to verify that our estimates are not affected by the baseline imbalance—which is essentially a level difference—we apply a matching strategy described below.

We create a matched sample that includes all women of the *birth sample* ($B_i = 1$). We create cells based on the age at birth; employment at 13, 24 and 36 months before birth; a binary indicator of the mode of employment 13-36 months before birth is in a white collar job; and the binary indicator of $CB + W$-eligibility. In each cell, we ensure that we have at most as many women never giving birth as women in the *birth sample*, selecting women in the $B_i = 0$ group randomly if their number exceeds the number of $B_i = 1$ women. This is our matched sample that avoids the over-representation of those $B_i = 0$ women who have very different labor market history than the $B_i = 1$ women.

5 Results

5.1 Employment

Panels (a)–(b) of Figure 2 show the evolution of the employment rate for the 4 relevant groups in 2 dimensions (eligible / ineligible women; giving birth / not giving birth) in the baseline and matched samples, respectively. The group of women who do not give birth we define as the control group, for whom the time of birth – that also determines event time – has been randomly drawn so that they can be also classified to be in the eligible *vs.* ineligible group. The employment rate slowly and steadily increases over time in the control group. Mothers have higher employment rate before birth than women who do not give birth, which difference almost disappears in the matched sample.

Figure 2 also shows that for women giving birth the employment rate starts to increase again only 12 months after giving birth, with discontinuous jumps at the 24 and 36 months mark in their children's age. The gap between the eligible–ineligible mothers widens exactly between 18 and 24 months of their children's age: eligible mothers' employment rate is higher exactly in the 2^{nd} half of the child's 2^{nd} year of age, when return to work was strongly incentivized by the analyzed policy.

Figure 3 shows the Event Study coefficient estimates for the birth sample and the control sample separately (in panels (a) and (b), respectively), so that their difference could be seen in panel (c). Specifically, in panels (a) and (b) we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (a), the sample is restricted to women giving birth; in panel (b), the sample is restricted to the control group (women never giving birth).[6] Panel (c) shows the estimated δ_k coefficients from equation (3) on the baseline sample, and panel (d) shows the estimated δ_k coefficients from equation (3) on the matched sample.

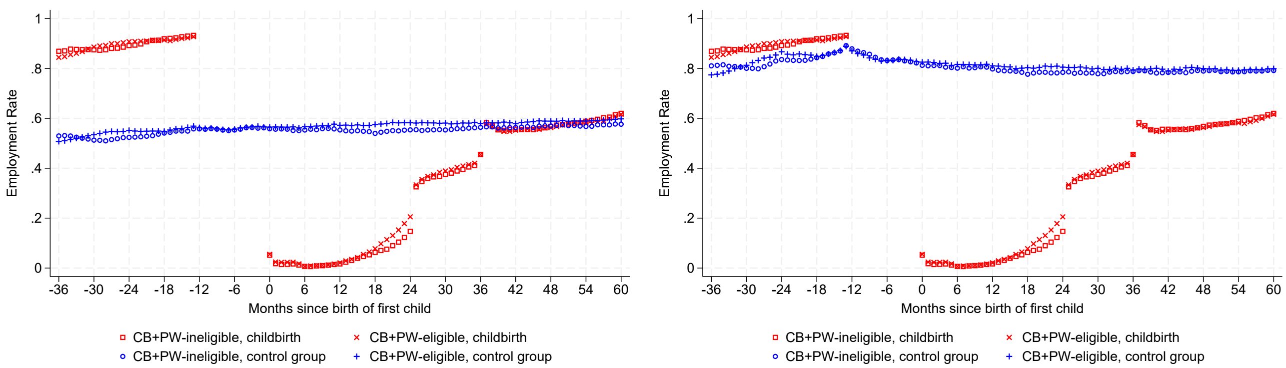
We find that the *CB+W* policy increased the propensity of being employed by 0.9 percentage point in the 19^{th} month after giving birth, and this difference is 2.2, 2.3, 3.1, 4.1, and 4.4 percentage points in months 20–24, respectively. Based on panel (d), these results are robust to the matching which takes care of the baseline imbalance. Reassuringly, there are no pre-trends in any of the months prior to the event in either panels (c) or (d), thus the use of a control group – women without giving birth – is helpful to difference out the pre-trends in panel (a).

In Table 2 we investigate the heterogeneity of the *CB+W* policy's effect on employment by pre-birth characteristics. The top row indicates that on average, the *CB+W* policy increased the employment rate by 2.8 percentage points (3.0 percentage points in the matched sample) 19-24 months after childbirth. We find that the point estimates for the employment effects are higher for women who have their first child at age 31-40, and, in terms of the corresponding *p-values*, this is the strongest heterogeneity we find in the employment effects. We also see bigger employment effects for mothers whose baseline wage was above the median before childbirth, but also for mothers whose employer's AKM firm effect was below the median (the baseline wage and AKM firm effect refer to 13-24 months prior childbirth). However, the differences along these heterogeneity dimensions are not statistically significant. Therefore, our results do not confirm the expectations based on the changing relative financial incentives, that the reform would have had higher effect on lower-income mothers to return to work, than on higher-income mothers.

[6] The pre-trend in the second and third year before birth is due to aggregate labor market trends. For example, the *CB+W*-ineligible group is observed in 2008 at event time -36 and in 2009 at event time -24, between which periods the employment rate decreased in Hungary. On the other hand, the *CB+W*-eligible group is observed in 2009 at event time -36 and in 2010 at event time -24, between which periods the employment rate levelled off in Hungary.

Figure 2: Employment Rate Over Event Time

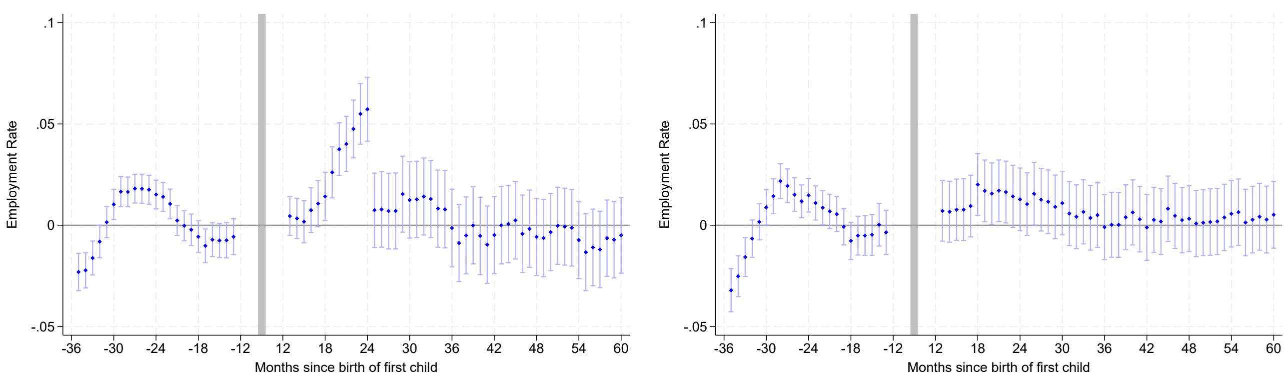
(a) Baseline Sample (b) Matched Sample



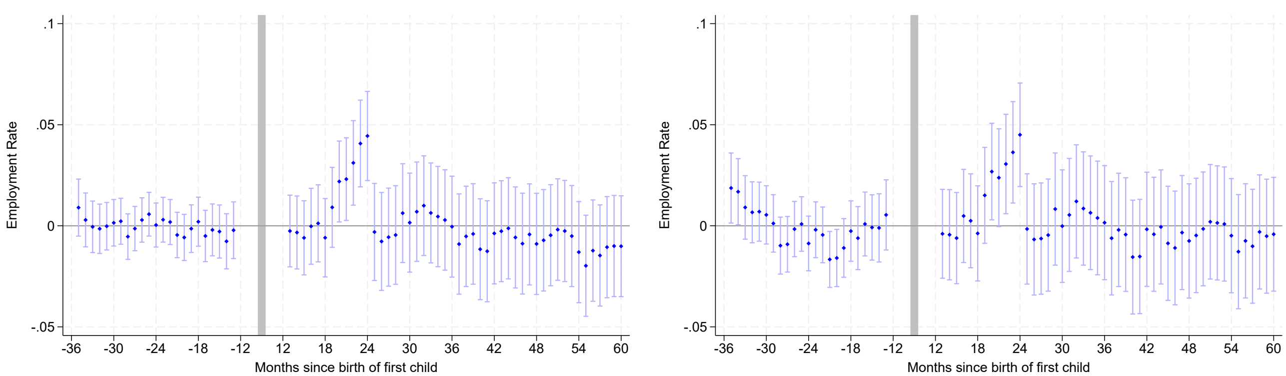
Notes: Sample is as described in Section 3.2. The "childbirth" sample consists of women whom we observe giving birth. The "control group" consists of women whom we do not observe giving birth.

Figure 3: Effect of $CB+W$-Eligibility on Employment

(a) Difference-in-differences (DD); Birth Sample (b) Difference-in-differences (DD); Control Sample



(c) Triple differences (DDD) (d) Triple differences (DDD), Matched Sample



Notes: Sample is as described in Section 3.2. This figure shows the Event Study coefficient estimates for the birth sample and the control sample separately (in panels (a) and (b), respectively), so that their difference could be seen in panel (c). Specifically, in panels (a) and (b) we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (a), the sample is restricted to women giving birth; in panel (b), the sample is restricted to the control group (women never giving birth). Panel (c) shows the estimated δ_k coefficients from equation (3) on the baseline sample, and panel (d) shows the estimated δ_k coefficients from equation (3) on the matched sample. Bars are 95 percent confidence intervals based on robust standard errors.

Table 2: Heterogeneity in the Effect of the *CB+W* Policy on Employment

		Full Sample	Matched Sample	*Baseline Mean*
All	afterM19-24 × B × D	0.028***	0.030***	*0.101*
		[0.010]	[0.012]	
By Age at Childbirth				
Age 25-30	afterM19-24 × B × D	0.013	0.005	*0.093*
		[0.014]	[0.017]	
Age 31+	afterM19-24 × B × D	0.044***	0.056***	*0.109*
		[0.013]	[0.016]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.110*	*0.028*	
By Location				
Rural	afterM19-24 × B × D	0.027**	0.030**	*0.102*
		[0.011]	[0.013]	
Capital	afterM19-24 × B × D	0.033	0.035	*0.098*
		[0.022]	[0.026]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.830*	*0.842*	
By Baseline Occupation				
Blue-Collar	afterM19-24 × B × D	0.018	0.027	*0.057*
		[0.015]	[0.021]	
White-Collar	afterM19-24 × B × D	0.025*	0.030**	*0.121*
		[0.014]	[0.014]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.750*	*0.898*	
By Baseline Wage				
Below Median	afterM19-24 × B × D	0.011	0.018	*0.058*
		[0.018]	[0.019]	
Above Median	afterM19-24 × B × D	0.034**	0.037**	*0.141*
		[0.015]	[0.015]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.326*	*0.419*	
By Baseline AKM Firm FE				
Below Median	afterM19-24 × B × D	0.030*	0.045**	*0.079*
		[0.017]	[0.018]	
Above Median	afterM19-24 × B × D	0.020	0.019	*0.125*
		[0.016]	[0.016]	
	p-value of $H_0 : \delta_1 = \delta_0$	*0.679*	*0.286*	

Notes: Table shows average effect of the *CB+W* policy on employment probability 19-24 months after childbirth, using the 3rd and 2nd years preceding childbirth as reference period (equation (4)). In the models with heterogeneity terms, all regressors are interacted with the heterogeneity term. Occupation category refers to the mode of occupation 13-36 months prior childbirth. Baseline wage and AKM firm FE refer to 13-24 months prior childbirth. Robust standard errors in brackets. Number of individuals: 21,922. Number of observations: 657,619. *** denotes significant at the 1 percent level, and ** denotes significant at the 5 percent level.

5.2 Fertility

Next, we test whether the reform changed families' birth planning behavior: as we discussed in Section 2.2, the *CB+W* and Pillar II of the reform have opposite effect of the timing of subsequent births. By itself, the *CB+W* reform could have motivated mothers to delay future pregnancy to be able to return to work during parental leave. Meanwhile, by eliminating the crowd-out effect of maternity benefits, Pillar II of the reform incentivized families to reduce the time between births. In our sample, *CB+W*-eligible mothers (whose child turns 13 months old in the time frame 07/2013–12/2013) are not only eligible to keep their childcare benefits for additional 7-12 months in case of returning to work, but they could also receive maternity benefits for their first and second child for potentially a year longer than *CB+W*-ineligible mothers (whose child turns 13 months old in 07/2012–12/2012).

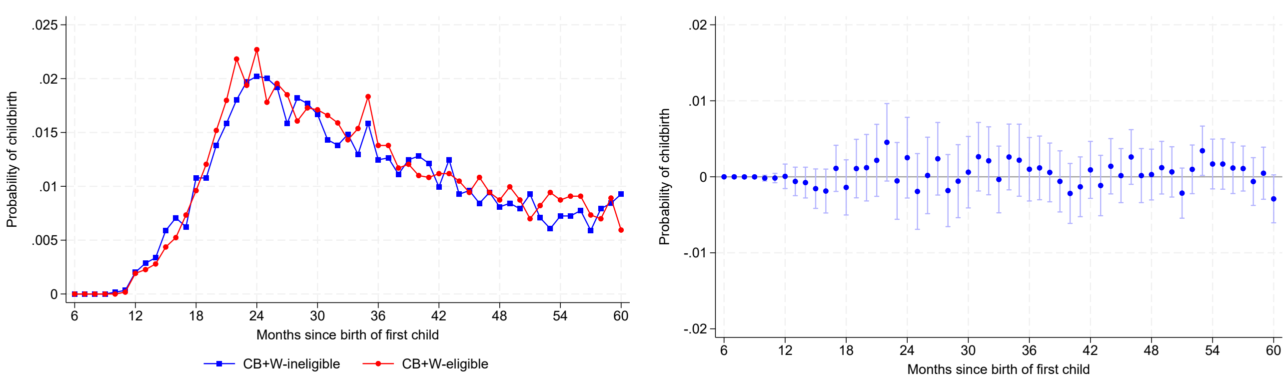
Despite these differences, panel (a) of Figure 4 suggests that there is no systematic difference in the timing of second birth between our chosen two cohorts of mothers. We test the statistical significance of these raw differences in fertility by estimating equation (5) on childbirth rate. Panel (b) of Figure 4 confirms that there are no statistically significant difference in future fertility between eligible and ineligible mothers.

Our findings on fertility suggest the effect of Pillar II of the reform was moderate, as the financial incentives created by Pillar II was not able to offset the *CB+W* to decrease the time between subsequent births. Overall, the reform did not change families' fertility decisions, which also means that the positive effect on maternal employment we document does not have negative spillovers on fertility.

Figure 4: Effect of the *CB+W* Policy on Childbirth Rate (Further Child)

(a) Descriptive

(b) Difference-in-differences (DD); birth sample



Notes: Sample is as described in Section 3.2, excluding the control group (women never giving birth). Panel (b) shows estimated θ_k coefficients from equation (5). Bars are 95 percent confidence intervals based on robust standard errors.

5.3 Job Characteristics

Now we turn to the analysis of job characteristics 3–5 years after giving birth, when there are no differences between the eligible and ineligible mothers in their employment and fertility behaviors, but there remains one notable difference: mothers in eligible cohorts were by around 3 percentage points more likely to work 19–24 months after giving birth (and there are no further differences).

We show the $\hat{\delta}$ estimates from eq. (3) in Figure 5. Appendix Figure A1 shows that these results are robust to restricting the estimation sample to the matched sample. We present descriptive time patterns and DiD estimates from eq. (2) in Figures A2–A6. All presented estimates are conditional on working.

Panels (a), (c) and (d) of Figure 5 suggests small positive but statistically insignificant effect of $CB+W$-eligibility on hourly wage, firm size decile, and the probability of employment in a white-collar job. Panel (b) indicates that $CB+W$-eligibility increases the AKM firm FE of the employer by up to 0.02, corresponding to a 2 percent increase of the firm wage premium. Looking at working hours, panel (e) of Figure 5 shows that in the third year after childbirth, the weekly hours worked increase by up to 0.9 hour, and this effect is statistically significant. This increase is due to the direct impact of the $CB+W$ reform, which eliminated the 30-hour weekly working time restricting for the 3rd year of age of the child.[7]

Then, in the first panel of Table 3, we show the average effects of $CB+W$-eligibility in the 3^{rd}, 4^{th} and 5^{th} year after giving birth, for all. The first column shows that wages are by 0.8 and 1.7 percent higher in the 4^{th} and 5^{th} year after giving birth, respectively, for the $CB+W$-eligible *vs.* non-eligible mothers *vs.* non-mothers, but these differences are not significant at the usual significance levels. At the same time, the second column shows that the estimated AKM firm effect – capturing the firm-specific wage premium – is significantly higher by 1.4, 1.3 and 1.6 percent in the 3^{rd}, 4^{th} and 5^{th} year, respectively. Hours worked is significantly higher by 0.41 hours only in the 3^{rd} year after giving birth, but otherwise differences in firm size and being in a white-collar occupation are not significant at the usual levels.

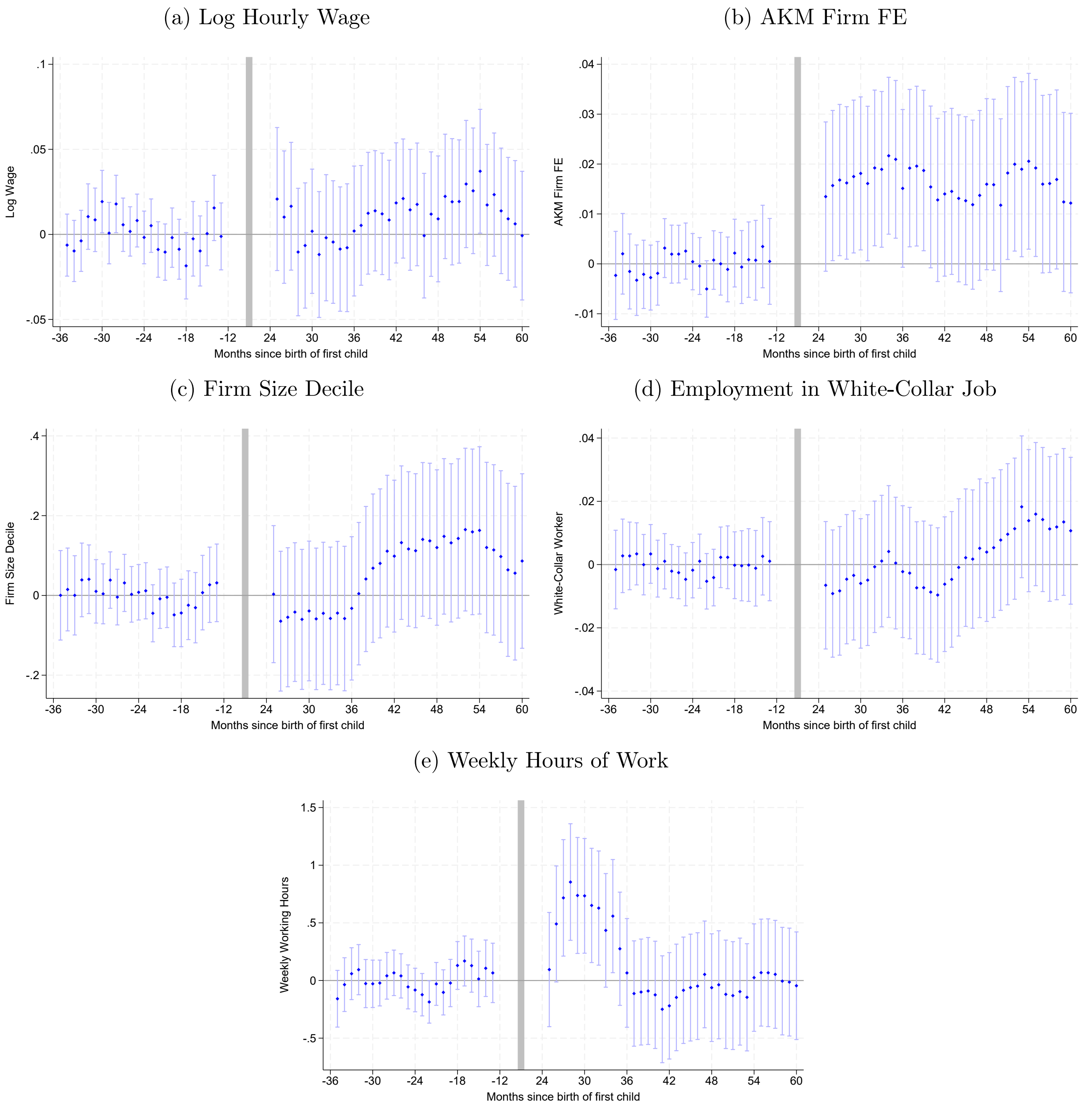
Further panels reveal that the wage and AKM firm wage premium effects are driven by women who were relatively older (31 years old or older) at the time of the event, as well as those who live in the capital city, Budapest. For instance, the first column shows that wages are by 9 and 8.4 percent higher in the 3^{rd} and 4^{th} year after giving birth, respectively, for the $CB+W$-eligible *vs.* non-eligible mothers *vs.* non-mothers, and these differences are significant at the 5 percent level. Similarly, the second column shows that the estimated AKM firm effects for those living in Budapest are by 4.1–4.5 percent higher, and they are also rather driven by those in white-collar occupations and with above-median baseline wages.

Column (3) reveals that the overall effect on hours in the 3^{rd} year after giving birth is driven by those in white-collar occupations and with above-median baseline wages and above-median baseline AKM firm effect. Column (4) reveals that the overall effect on firm size decile is driven by those who were relatively older (31 years old or older) at the time of the event, as well as those who live in the capital city, Budapest. There are no groups with a significant effect on the propensity to work in white-collar occupations.

Finally, Figure 6 suggests that eligible mothers sorted into occupations that are less achievement-oriented and require less stress tolerance, analytical thinking, leadership and responsibility, and involve less time pressure (we also checked and did not find any substantial heterogeneities for these outcomes).

[7]The reform component abolishing the 30-hour working restriction for PL benefit recipients explains this: some mothers of the ineligible group (whose child turns 13-months-old in 07/2012–12/2012) are eligible only for fewer month, while in the eligible group (whose child turns 13-months-old in 07/2013–12/2013) everyone is eligible for the full length in year 3.

Figure 5: Effect of $CB{+}W$-Eligibility on Job Characteristics



Notes: Sample is the baseline sample as described in Section 3.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)). Bars are 95 percent confidence intervals based on robust standard errors.

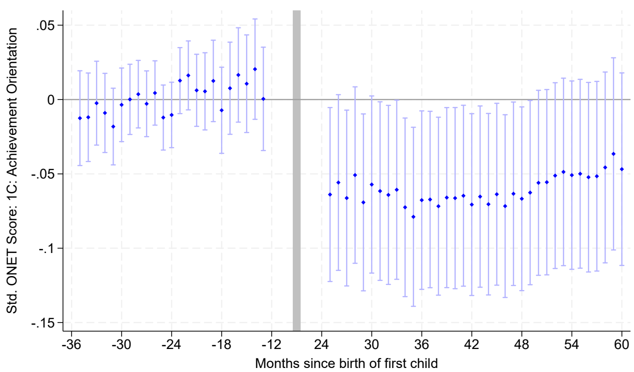
Table 3: Heterogeneity in the Effect of the *CB+W* Policy on Job Characteristics

		log. of hourly wage	AKM firm FE	hours	decile of firm size	1: white collar
All	afterY3 × B × D	-0.004	0.014**	0.407*	-0.03	-0.006
		[0.015]	[0.007]	[0.211]	[0.081]	[0.009]
	afterY4 × B × D	0.008	0.013*	-0.100	0.076	-0.006
		[0.015]	[0.008]	[0.208]	[0.087]	[0.010]
	afterY5 × B × D	0.017	0.016**	-0.058	0.098	0.008
		[0.016]	[0.008]	[0.212]	[0.094]	[0.011]
By Age at Childbirth						
Age 31+	afterY3 × B × D	0.028	0.023**	0.237	0.092	-0.013
		[0.021]	[0.009]	[0.291]	[0.114]	[0.013]
	afterY4 × B × D	0.03	0.024**	-0.134	0.252**	-0.007
		[0.021]	[0.010]	[0.285]	[0.121]	[0.014]
	afterY5 × B × D	0.041*	0.028**	-0.076	0.315**	0.004
		[0.022]	[0.011]	[0.290]	[0.133]	[0.014]
By Location						
Budapest	afterY3 × B × D	0.090***	0.045**	0.334	0.111	-0.018
		[0.035]	[0.018]	[0.511]	[0.175]	[0.018]
	afterY4 × B × D	0.084**	0.043**	-0.551	0.383**	-0.025
		[0.036]	[0.020]	[0.517]	[0.188]	[0.020]
	afterY5 × B × D	0.058	0.041*	-0.265	0.28	-0.011
		[0.039]	[0.022]	[0.524]	[0.204]	[0.021]
By Baseline Occupation						
White-Collar	afterY3 × B × D	0.016	0.023***	0.471**	0.034	
		[0.016]	[0.009]	[0.224]	[0.107]	
	afterY4 × B × D	0.021	0.019*	-0.103	0.178	
		[0.016]	[0.010]	[0.222]	[0.115]	
	afterY5 × B × D	0.026	0.014	-0.093	0.166	
		[0.017]	[0.011]	[0.226]	[0.125]	
By Baseline Wage						
Above Median	afterY3 × B × D		0.017*	0.450**	-0.019	0.009
			[0.009]	[0.221]	[0.099]	[0.010]
	afterY4 × B × D		0.016*	0.111	0.052	0.013
			[0.009]	[0.217]	[0.106]	[0.011]
	afterY5 × B × D		0.013	0.132	0.038	0.027**
			[0.010]	[0.223]	[0.116]	[0.012]
By Baseline AKM Firm FE						
Above Median	afterY3 × B × D	0.006		0.691***	0.025	0.01
		[0.020]		[0.236]	[0.095]	[0.012]
	afterY4 × B × D	0.03		0.158	0.066	0.005
		[0.021]		[0.234]	[0.102]	[0.013]
	afterY5 × B × D	0.024		0.316	0.084	0.018
		[0.022]		[0.238]	[0.110]	[0.014]

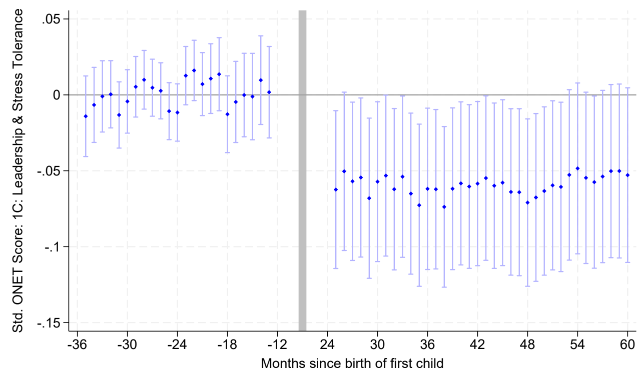
Notes: Table shows average effect of the *CB+W* policy on job characteristics in the 3^{rd}–5^{th} year after giving birth, using the 3rd and 2nd years preceding childbirth as reference period (equation (4)). In the models with heterogeneity terms, all regressors are interacted with the heterogeneity term. Occupation category refers to the mode of occupation 13-36 months prior childbirth. Baseline wage and AKM firm FE refer to 13-24 months prior childbirth. Robust standard errors in brackets. *** denotes significant at the 1 percent level, ** denotes significant at the 5 percent level, and * denotes significant at the 10 percent level.

Figure 6: Effect of $CB{+}W$-Eligibility on Occupation Characteristics (The Extent of Which...)

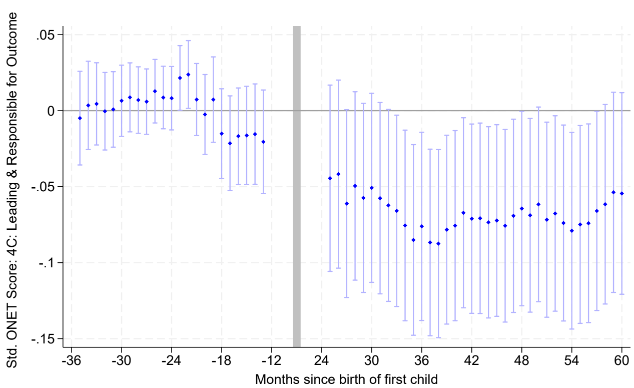
(a) Job is Achievement-Oriented



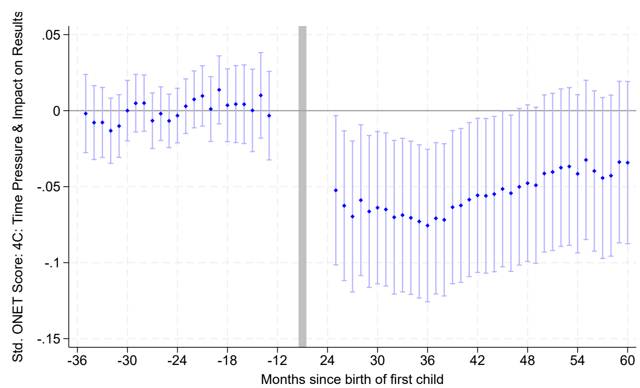
(b) Job Requires Leadership and Stress Tolerance



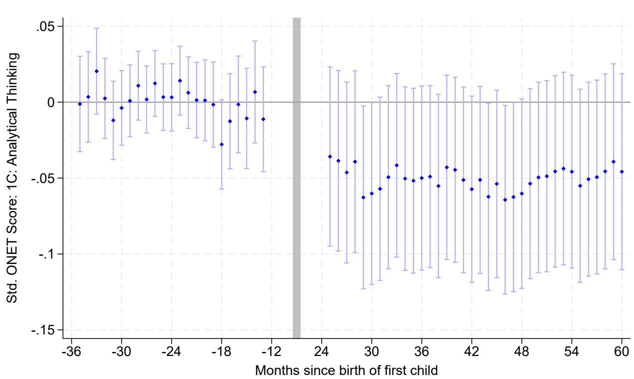
(c) Job Involves Leadership and Respons. for Outcomes



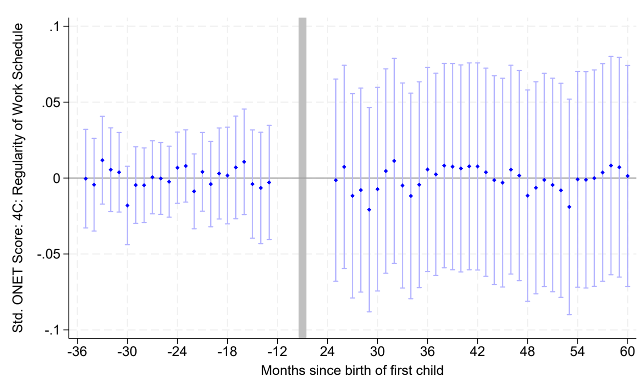
(d) Job Involves Time Pressure and Impact



(e) Job Requires Analytical Thinking



(f) Job's Schedule is Regular



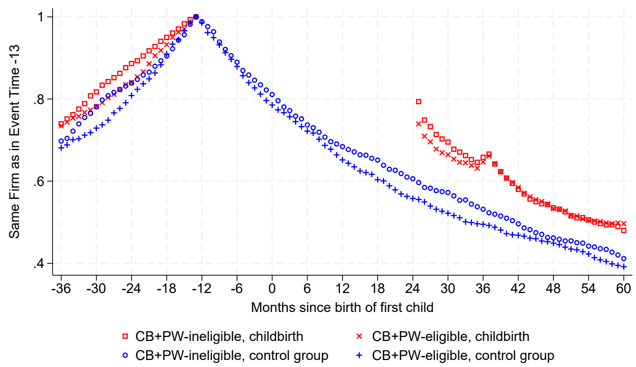
Notes: Sample is the baseline sample as described in Section 3.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)). Bars are 95 percent confidence intervals based on robust standard errors.

5.4 Transitions Between Employers

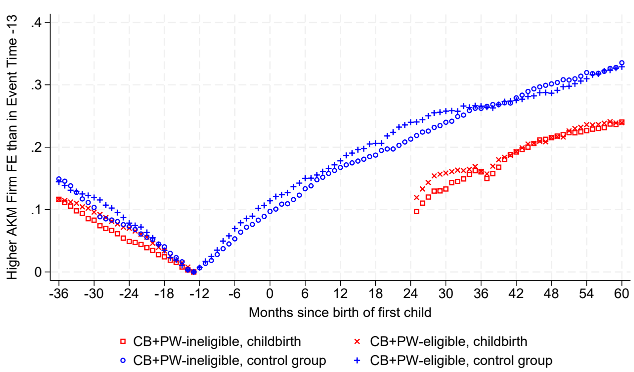
Panel (a) of Figure 7 depicts the ratio of workers who are employed at the same firm as in event time -13. We do not see clear evidence that the difference between the *CB+W*-eligible and ineligible mothers would differ from the same difference in the control group (women never giving birth). Panels (b)–(c) show the ratio of workers who are employed at higher (panel (b)) or lower (panel (c)) AKM firm FE firm than at event time -13. Here, panel (c) suggests that 3-5 years after childbirth, employment probability at lower AKM firm FE firm is relatively lower among those mothers who were *CB+W*-eligible.

Figure 7: Employment at Same Firm, and Higher or Lower Quality Firm as in Event Time -13

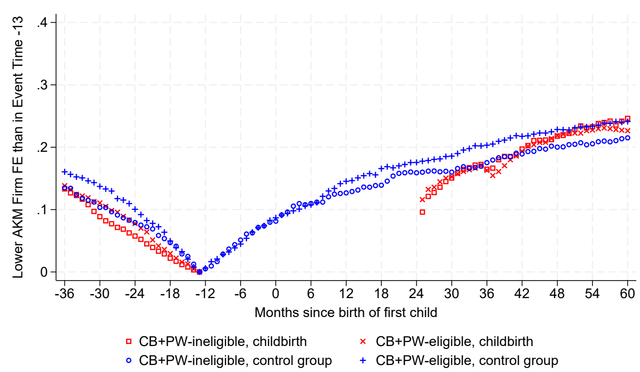
(a) Employment at Same Firm



(b) Employment at Higher AKM Firm FE Firm



(c) Employment at Lower AKM Firm FE Firm



Notes: Sample is the baseline sample as described in Section 3.2, restricted to employed individuals.

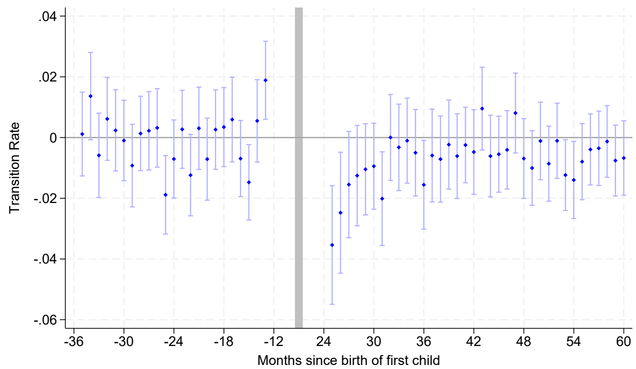
In Figure 8 we show our event study estimates for the transition rates between employers. We estimate equation (3). In panel (a), the binary outcome is one if the current employer is different from the last observed employer (conditional on the last employment being observed at most 48 months before), zero otherwise. In panels (b) and (c) the binary outcome is one if the AKM firm FE of the current firm is higher (lower) than the AKM firm FE of the last employer.[8] The estimation results indicate that *CB+W*-eligibility decreases the transition rate between employers in the 3rd and 5th years after childbirth. These patterns are reinforced by the twelve-monthly average effects reported in Table 4, moreover, the decreasing transition rate to lower quality firms is slightly more pronounced than the decreasing transition rates to

[8]We present descriptive plots for the time patterns of these outcomes in Figure A7.

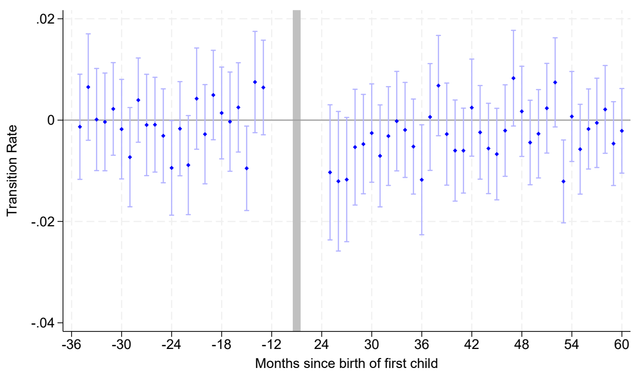
higher quality firms in the 3rd year after childbirth, and especially in the 5th year after childbirth.

Figure 8: Effect of $CB{+}W$-Eligibility on Transitions Between Employers

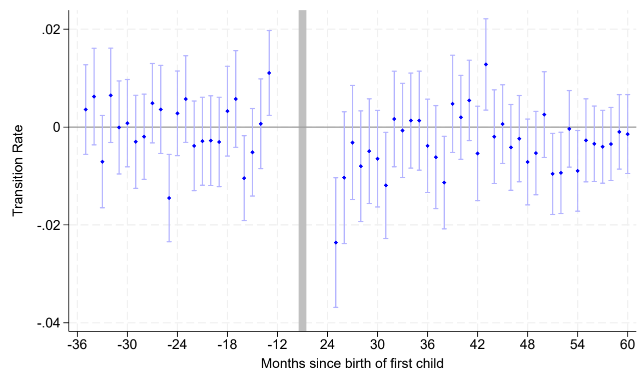
(a) Any Transition



(b) Transition to Higher AKM FE Firm



(c) Transition to Lower AKM FE Firm



Sample is the baseline sample as described in Section 3.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)). Bars are 95 percent confidence intervals based on robust standard errors.

Table 4: Twelve-Monthly Effect of the $CB{+}W$ Policy on Transition Between Employers

	Any transition	Transition to higher AKM FE firm	Transition to lower AKM FE firm
beforeY3 × B × D	0.0019	0.0015	-0.0000
	[0.0030]	[0.0021]	[0.0020]
afterY3 × B × D	-0.0100***	-0.0046**	-0.0052**
	[0.0034]	[0.0022]	[0.0022]
afterY4 × B × D	-0.0015	0.0001	-0.0012
	[0.0032]	[0.0021]	[0.0020]
afterY5 × B × D	-0.0054*	-0.0008	-0.0040**
	[0.0031]	[0.0020]	[0.0020]

Notes: Table shows average effect of the $CB{+}W$ policy on the probability of transition between employees (equation (4), but allowing the treatment effect to vary across 12-month periods). Baseline is the second year before childbirth. Robust standard errors in brackets. Number of individuals: 10,969. Number of observations: 475,715.

6 Conclusions

In this paper, we exploit a unique paid parental leave (PL) reform in Hungary that affected only the possibility of employment during the receipt of PL benefits, while keeping the overall benefit amounts unchanged, to identify the substitution effect in paid PL on the timing of mothers' return to work, their long-run employment propensity, and subsequent labor market trajectories. Specifically, Hungarian mothers whose child turned into their 2^{nd} year of age after 2014 were provided strong incentives to return to the labor market earlier than previous cohorts, since they did not have to give up PL benefits while earning their wage income during their children's 2^{nd} year of age.

Using an Event Study research design with women without births in the control group and Hungarian linked employer-employee data, we first establish that the policy increased mothers' propensity to work exactly when it provided strong financial incentives, thereby inducing eligible mothers to return earlier to the labor market, in months 19–24 after giving birth. We find that mothers in eligible cohorts were by 30 percent more likely to work 19-24 months after giving birth – but, no further differences exist in their working propensities or its timing. Thus, this policy provides a unique setting to study mothers' long-run labor market trajectories when a subset of them was incentivized to bring forward their return-to-work, but otherwise no differences exist in terms of subsequent employment and fertility behavior.

Second, we assess whether earlier return-to-work affects mothers' job quality outcomes. We find no effects, on average, on mothers' wages but positive effects on their firms' average wage premium 3-5 years after giving birth, and that eligible mothers sorted to occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure. Our results point to the importance of analyzing also other outcomes than wages, such as the quality of the firms and the characteristics of the occupations mothers sort into, after giving birth to their first child.

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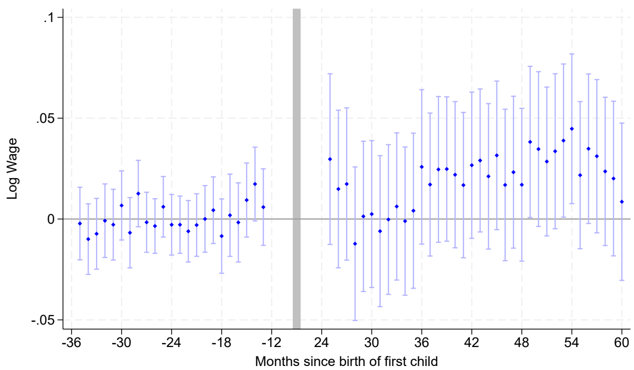
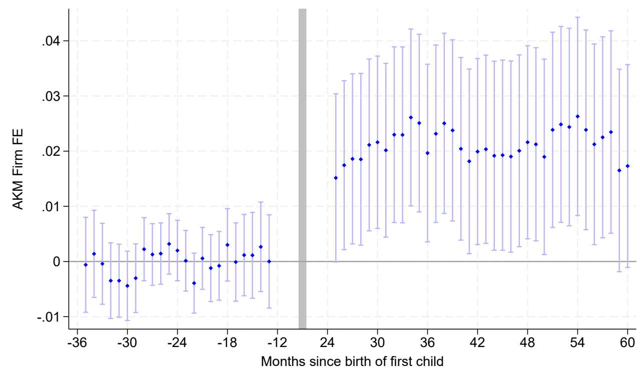
A Appendix: Additional Figures and Tables

Appendix Figure A1: Effect of $CB{+}W$-Eligibility on Job Characteristics, Matched Sample

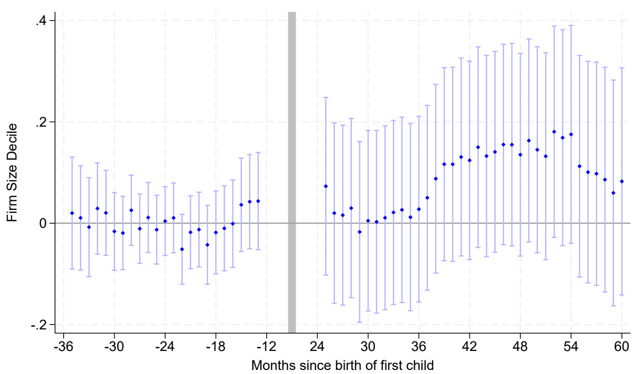
(a) Log Hourly Wage



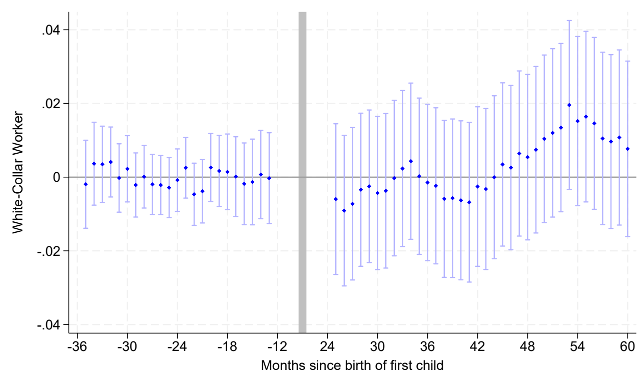
(b) AKM Firm FE



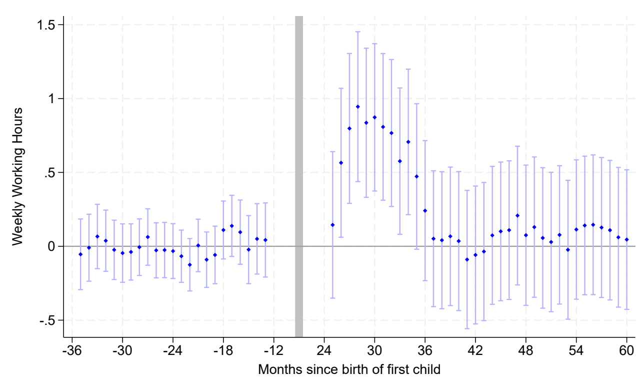
(c) Firm Size Decile



(d) Employment in White-Collar Job



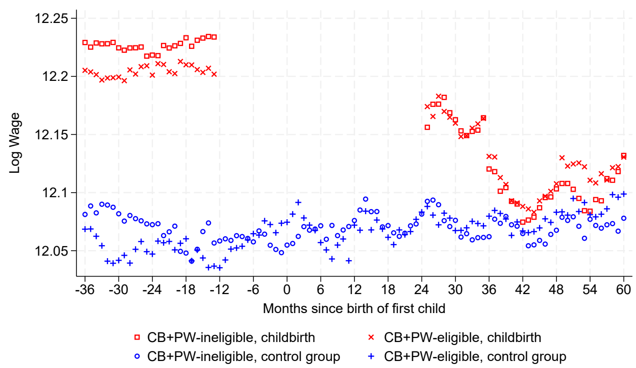
(e) Weekly Hours of Work



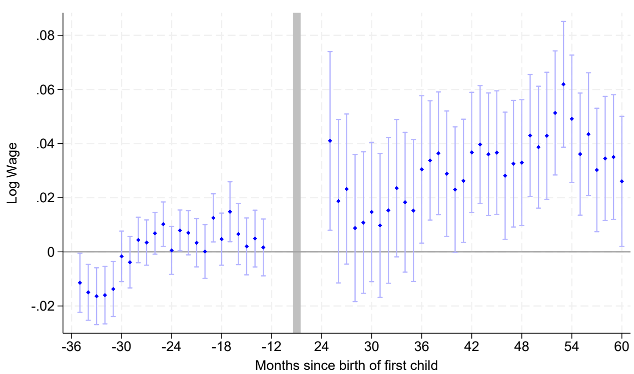
Notes: Sample is the matched sample, as described in Section 3.2. This figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)). Bars are 95 percent confidence intervals based on robust standard errors.

Appendix Figure A2: Logarithm of Hourly Wage: Descriptive Patterns and DD Estimates

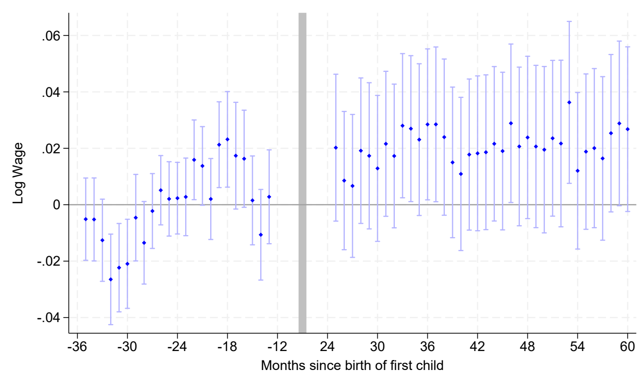
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



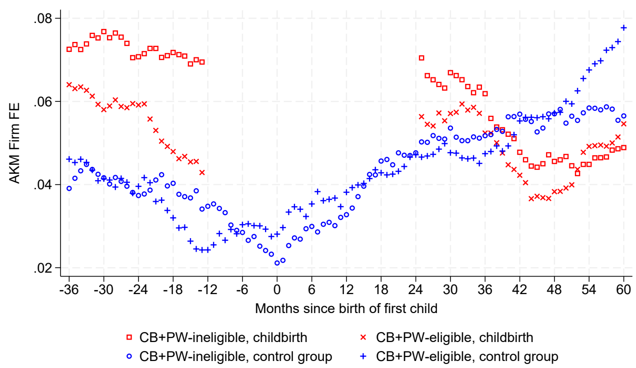
(c) Difference-in-differences (DD); control sample



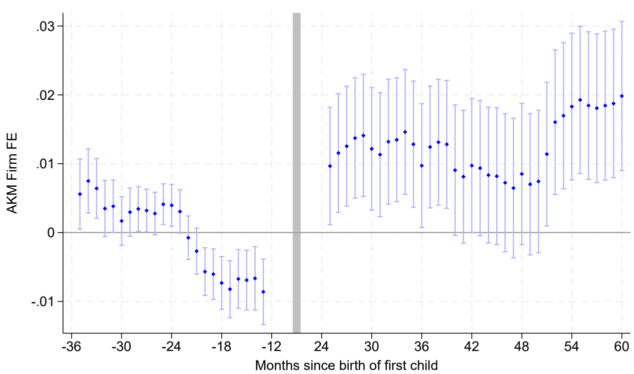
Notes: Sample is the baseline sample as described in Section 3.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\widehat{\beta}_k$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Appendix Figure A3: AKM Firm FE: Descriptive Patterns and DD Estimates

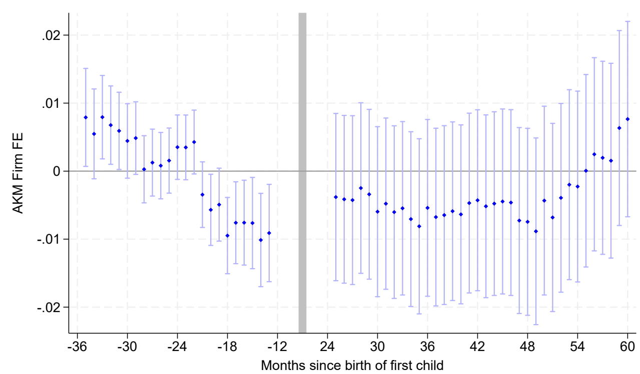
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



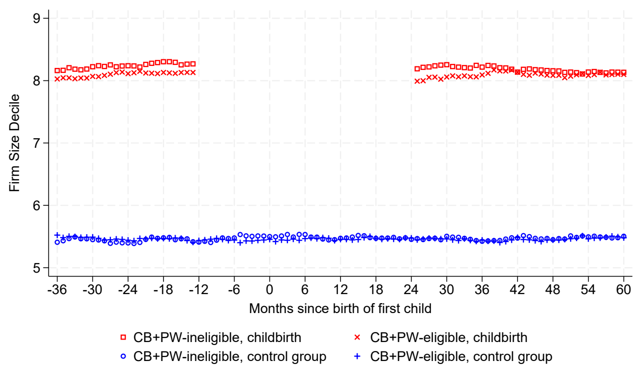
(c) Difference-in-differences (DD); control sample



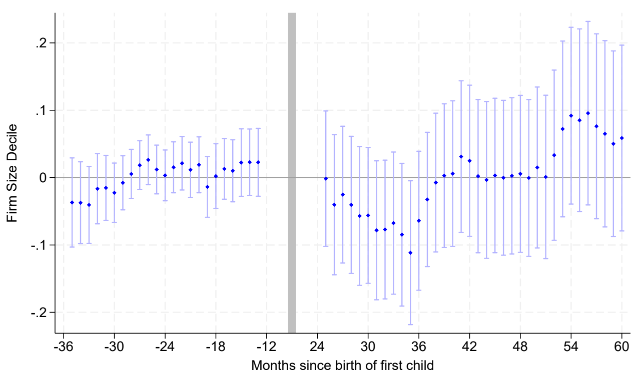
Notes: Sample is the baseline sample as described in Section 3.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\widehat{\beta_k}$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Appendix Figure A4: Firm Size Decile: Descriptive Patterns and DD Estimates

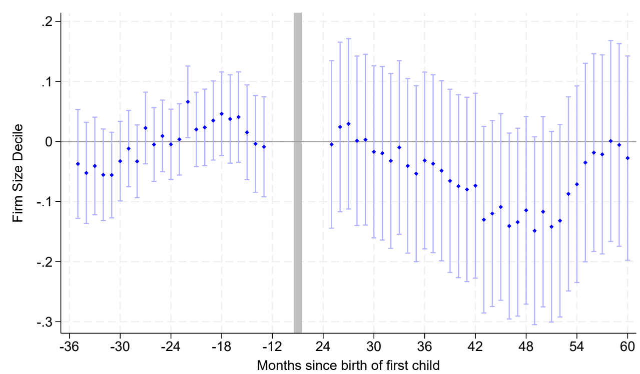
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



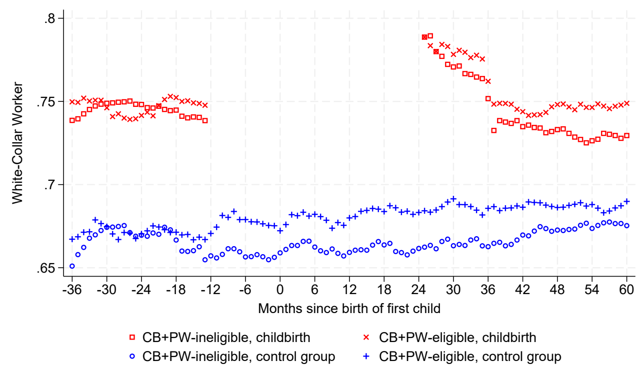
(c) Difference-in-differences (DD); control sample



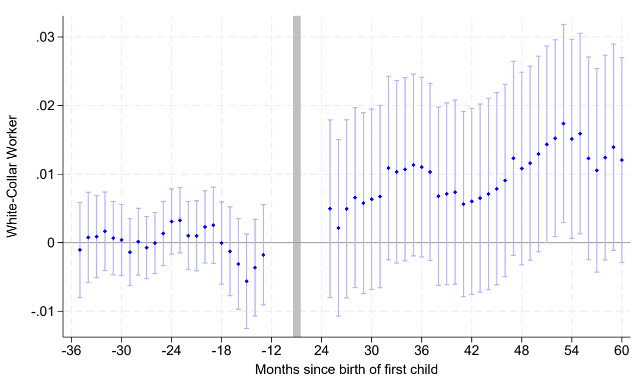
Notes: Sample is the baseline sample as described in Section 3.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\widehat{\beta_k}$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Appendix Figure A5: Employment in White Collar Job: Descriptive Patterns and DD Estimates

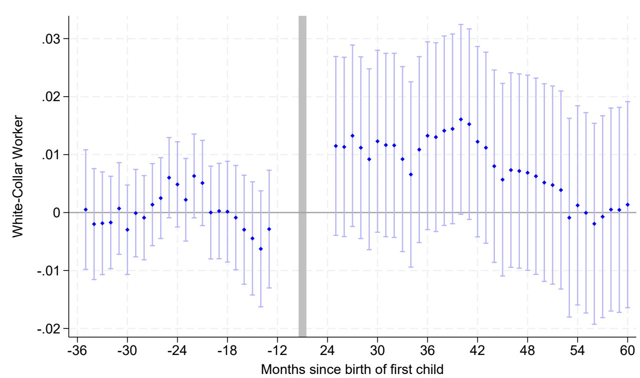
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



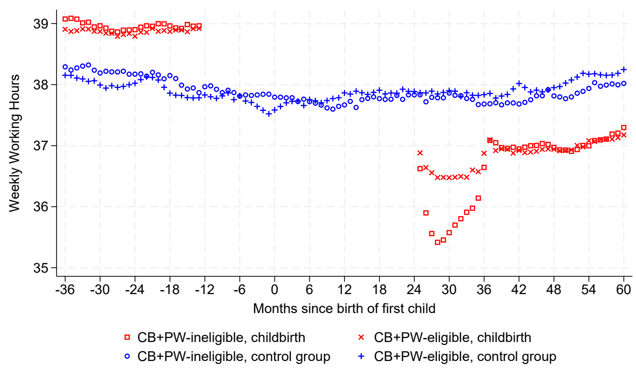
(c) Difference-in-differences (DD); control sample



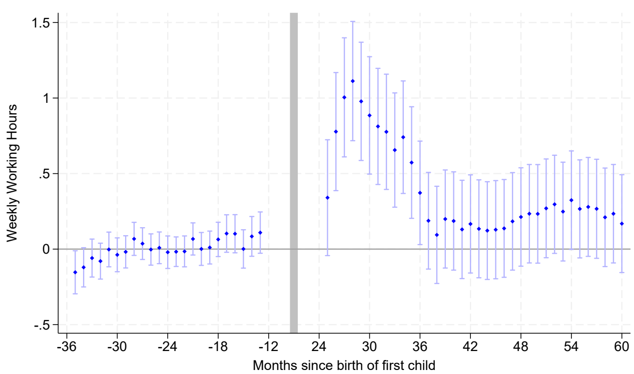
Notes: Sample is the baseline sample as described in Section 3.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\widehat{\beta}_k$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Appendix Figure A6: Weekly Working Hours: Descriptive Patterns and DD Estimates

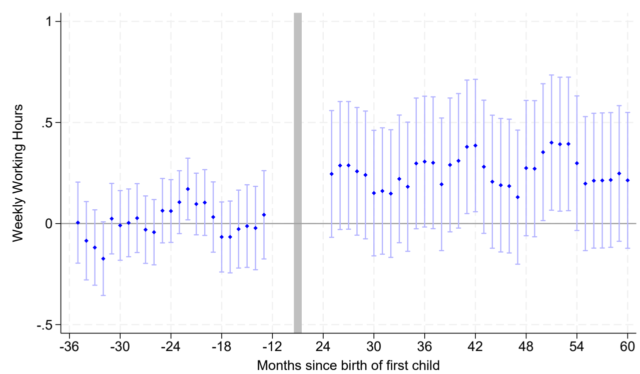
(a) Descriptive



(b) Difference-in-differences (DD); birth sample



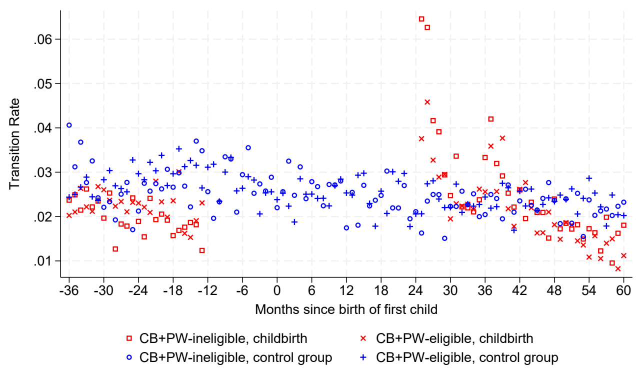
(c) Difference-in-differences (DD); control sample



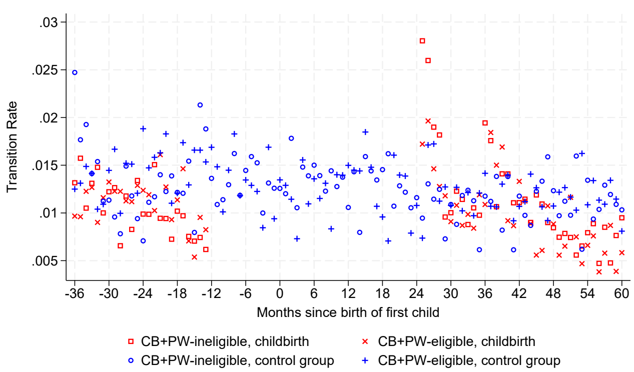
Notes: Sample is the baseline sample as described in Section 3.2. Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\widehat{\beta}_k$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control group (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors.

Appendix Figure A7: Transition Rate Between Employers

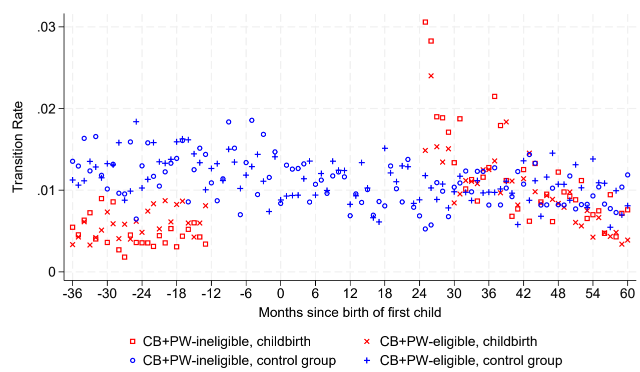
(a) Any Transition



(b) Transition to Higher AKM Firm FE Firm



(c) Transition to Lower AKM Firm FE Firm



Sample is the baseline sample as described in Section 3.2.