Does Earlier Return to Work Help Mothers' Career? Evidence via the Substitution Effect of Parental Leave*

Anikó Bíró[1], Lili Márk[2], Tímea Laura Molnár[3], and Zsigmond Pálvölgyi[4]

[1]HUN-REN Centre for Economic and Regional Studies (Hungary)
[2]Central European University (Austria)
[3]Central European University (Austria) and IZA
[4]University of Michigan (United States)

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Abstract

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. 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 and 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, without further differences in their working propensities. We find significant effects on mothers' wages and on their firms' average wage premium 3–5 years after giving birth for those who gave birth at an older age and live in the capital – these results are due to eligible mothers being more likely to stay at the same firm and being less likely to switch 'down' to lower-paying firms. We also find that eligible mothers sort 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

*Bíró: biro.aniko@krtk.hun-ren.hu. Márk: mark_lili@phd.ceu.edu. Molnár: molnartl@ceu.edu. Pálvölgyi: palvolgz@umich.edu. We thank Michele Belot, Zoltán Hermann, Hedvig Horváth, Melvin Stephens, Andrea Weber, Rudolf Winter-Ebmer, Basit Zafar, and seminar and conference audiences at CEU, the HUN-REN Centre for Economic and Regional Studies, the University of Michigan, the 2024 Vienna Applied Micro Workshop, the 2024 Austrian Labor Economics Workshop, and the 2024 Conference of the Hungarian Society of Economics for helpful questions, comments and discussions.

1 Introduction

Career interruptions due to giving birth 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., 2024). At the same time, the literature is inconclusive about whether the length of career interruptions itself affects women's labor market trajectories in wages and earnings 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 negatively affect 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 earnings 1-3 years after childbirth, while Bailey et al. (2025) do not find evidence that the same policy increased employment or boosted earnings in the long run. Schönberg and Ludsteck (2014) analyze a series of expansions of maternity leave duration in Germany and find that each expansion led to a delay in the return to work of mothers, however, the expansions did not have an effect on employment rates and earnings 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 workforce, but it did not affect employment or earnings in the long run. Lalive et al. (2014) and Kleven et al. (2024) 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 the first childbirth has positive long-run effect on wages.[1]

One potential reason for the inconclusive findings on whether the length of career interruptions affect women's long-run labor trajectories is that parental leave reforms may also affect workforce composition, thereby affecting long-run wage estimates through compositional changes, differently across prior studies. In this paper, we leverage 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. Building on linked employer-employee data, and using an Event Study research design comparing the careers of women eligible and ineligible for the return-to-work policy, with women without births as comparison group, we find that mothers in eligible cohorts were by 3 percentage points (30 percent) more likely to work, but only in the narrow window of 19–24 months after childbirth. We not only find a large effect on mothers' propensity to work exactly in the period when they have the option to work and keep the parental leave benefit at the same time, but also verify that this policy did not have an effect on subsequent fertility. 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 across cohorts exist.

With the unique Hungarian policy setting, to the best of our knowledge, we provide the first clean

[1]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.

estimates on the *timing* of mothers' return-to-work on mothers' long-run labor market trajectories—in terms of wages, the firm-specific wage premium and occupation characteristics—3–5 years after childbirth when there are no employment effects anymore without the contaminating effect of changing workforce composition. And while there is extensive literature on mothers' occupational sorting after return-to-work post-childbirth (*e.g.*, Bertrand et al., 2010; Cortés and Pan, 2023; Boinet et al., 2024), we present first evidence on how *earlier* return-to-work affects such occupational sorting years after childbirth. We find significant effects on mothers' wages and on their firms' average wage premium 3–5 years after giving birth for those who gave birth at an older age and live in the capital – these results are due to eligible mothers being more likely to stay at the same firm and less likely to switch 'down' to lower-paying firms. We also find that already in the 3^{rd} year after childbirth, eligible mothers have sorted into occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure, and the difference stays relatively stable up to years 5 after childbirth – suggesting important, previously undocumented, persistence in occupational sorting of mothers with young children.

As a falsification test, we confirm zero reduced-form estimates for likely "always worker" mothers. Thus, our main estimates are due to the effects of earlier return to work after childbirth on the complier mothers' long-term outcomes, who returned earlier due to being eligible to the analyzed policy.

Our second contribution is to identify the substitution effect of paid parental leave on mothers' long-term employment propensities. Following Autor and Duggan (2007), (i) the substitution effect arises due to the fact that paid parental leave recipients face an incentive not to work since working would mean sacrificing the benefits, while (ii) 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. The substitution effect is distortionary and causes deadweight loss, because it distorts the relative price of non-work (Autor and Duggan, 2007; Chetty, 2008). While the distinction of the two effects is a central topic in the literature on the labor market effects of disability insurance and unemployment insurance (Autor and Duggan, 2007; Chetty, 2008; Deuchert and Eugster, 2019; Krekó et al., 2024, among others), we are not familiar with studies that distinguish either of the two channels related to paid parental leave. At the same time, identification of the substitution effect is key and policy-relevant, because it shows to what extent paid parental leave distorts the incentives to work and for whom.

Our unique policy setting allows us to identify the substitution effect of paid parental leave. In our setting, before the reform, early return-to-work had a cost in the form of sacrificing the benefits; then, the reform removed this "cost", or, in other words, this "implicit tax" of working. Existing studies exploit paid parental leave duration cuts or extensions (e.g. Schönberg and Ludsteck (2014); Dahl et al. (2016); Lalive et al. (2014); Kleven et al. (2024)), where both the duration of parental leave and the corresponding parental leave benefit payment change, thus they measure together the income and substitution effect of parental leave on the labor market participation of mothers. We find that mothers in eligible cohorts were more likely to work by 3 percentage points (30 percent), but only in the narrow window of 19–24 months after childbirth. We find that our point estimate on the propensity to work is driven by mothers who give birth to their first child at a relatively older age, and by those who are high-earners pre-birth.

In what follows, Section 2 introduces the parental leave system in Hungary. Section 3 gives a conceptual framework for the identification of substitution effects in paid parental leave. Section 4 describes the data, and Section 5 our empirical strategy. Section 6 presents and discusses our results. Section 7 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 amount of childcare benefit is capped at 140% of the minimum wage. To reach this cap mothers need 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.

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.

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 plus wage") reform in 2014, mothers had to forgo their childcare benefit if they decided to return to work before the 2^{nd} birthday of their youngest children. Similarly, mothers could only keep their childcare allowance if they did not work more than 30 hours per week. These work restrictions and the income provided by the parental leave benefits acted as disincentives 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 and wage protection by the Hungarian Labor Code (Act I of 2012); the regulation of mothers' job protection did not change during our study period. It is a common belief among both employers and employees that courts greatly favor returning mothers in case of job protection or wage disputes. 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 to unpaid and fully protected leave between the birth and 3^{rd} birthday of their youngest child, during which time they accumulate vacation days. During maternity and parental leave, mothers can return to work at any time they would like to. Within 30 days, employers have to offer them a similar position they held before giving birth, even if they previously hired a replacement for mothers' pre-birth occupation. Compared to their pre-birth wage, returning mothers are entitled to a similar wage raise that other employees in similar roles received during the parental leave. Upon request, employers also need to accommodate mothers who would like to work part-time until the 3^{rd} birthday of their youngest child.

Returning mothers' contracts can only be terminated for cause until the 3^{rd} birthday of their youngest child, which provides another level of job protection. The only uncertainty returning mothers could face arises if their pre-birth role is ceased because of a reorganization, in which case no employee works at their pre-birth occupation. Only in this specific situation can employers terminate returning mothers' contracts. However, in this situation, mothers could decide to stay on paid parental leave until the 3^{rd} birthday of their child instead of returning to an employer that ceased their previous role. Meanwhile, because of potential disputes in courts, employers are also motivated to avoid this situation by communicating clearly the lack of positions for returning mothers, or by offering similar positions mothers hold pre-birth.

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 Childcare Benefit Plus Wage ($CB + W$) Reform

Starting with January 1, 2014, the Hungarian government implemented a childcare benefit reform ("GYED-extra" reform, in Hungarian), 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 to receive 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,[2] 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 the future fertility decisions of these mothers. By eliminating the crowding out of current maternity leave benefits, Pillar II could have motivated mothers to decrease the time between subsequent births. In this way Pillar II could have acted as a disincentive for mothers to return earlier to work, which was one of the main aims of the reform's Pillar I. As later we show, overall the reform increased maternal employment during parental leave, which suggests that Pillar I's financial incentives for returning to work exceeded the disincentives Pillar II created. Meanwhile, we did not find evidence that the reform affected mothers' future fertility (for details see section 6.3) , 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 plus wage") – the abolishment of work restrictions after the first birthday of the child. Hungary is 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 caps benefits if wage income exceeds 50% of the parental leave benefits (Bičáková and Kalíšková, 2022; OECD, 2024).[3] In Hungary, the *CB+W* reform creates a temporary income jump, which could increase the pre-birth income by as much as 70%.

[2] We can illustrate the policy change through an example of a mother who receives childcare benefits 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.

[3] Some other OECD countries (France and Slovenia) allow parents to work part-time during parental leave and receive time-adjusted parental leave benefits, while others (Belgium, Germany, and Sweden) give more flexibility to parents to

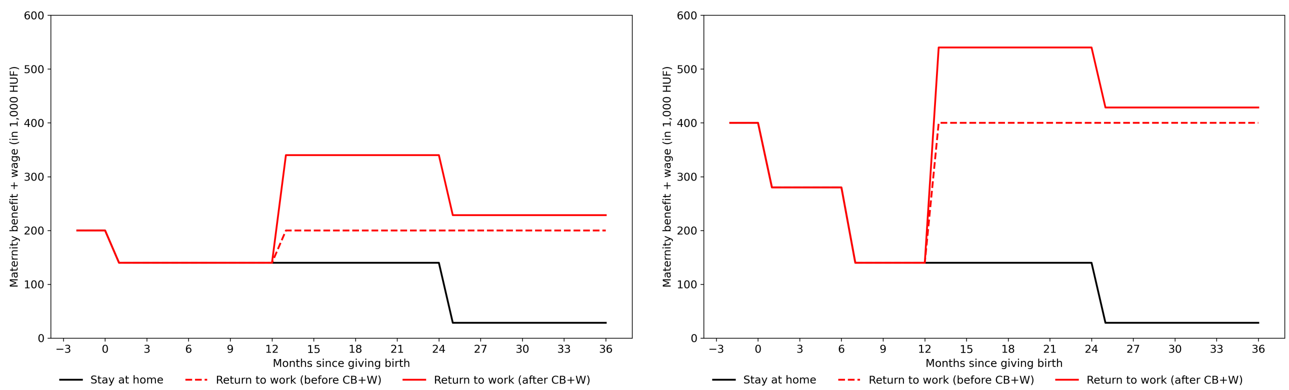
2.3 Financial Incentives of the *CB+W* Reform: A Numerical Example

We illustrate the effect of the *CB+W* reform through a numerical example.

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 on the first birthday of her youngest child. In panel (a) 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

(a) Full-Time Salary: Below the Cap (b) Full-Time Salary: Above the Cap



Notes: In this figure we illustrate 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 youngest child. In panel (a) this women's pre-birth income is twice the minimum wage, meaning that her replacement rate is 70%. In panel (b) 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%. The minimum wage is set to 100,000 Hungarian forints (HUF) – the exact value of monthly minimum wage was 98,000 HUF in 2013 and 101,500 HUF in 2014.

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 panel (a) 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 an 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 2^{nd} birthday of her child by returning to full-time

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

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 Identifying the Substitution Effect of Paid Parental Leave

Paid parental leave decreases mothers' labor supply through two channels, that are rarely discussed in the context of parental leave: through the substitution effect and the income effect. Below we discuss both conceptually, and explain how our unique policy setting helps us to identify the substitution effect.

The substitution effect represents the distortionary effect on work incentives in that mothers lose the parental leave benefit if they start to work, and the loss of the benefit acts as an "implicit tax" on working. As a result, mothers decide not to work even if they would otherwise have positive labor supply at the income level provided by the parental leave benefit. The income effect, on the other hand, refers to the impact of an increase in non-wage income and decreases labor supply simply because at a higher level of income, mothers might prefer staying at home with their child.

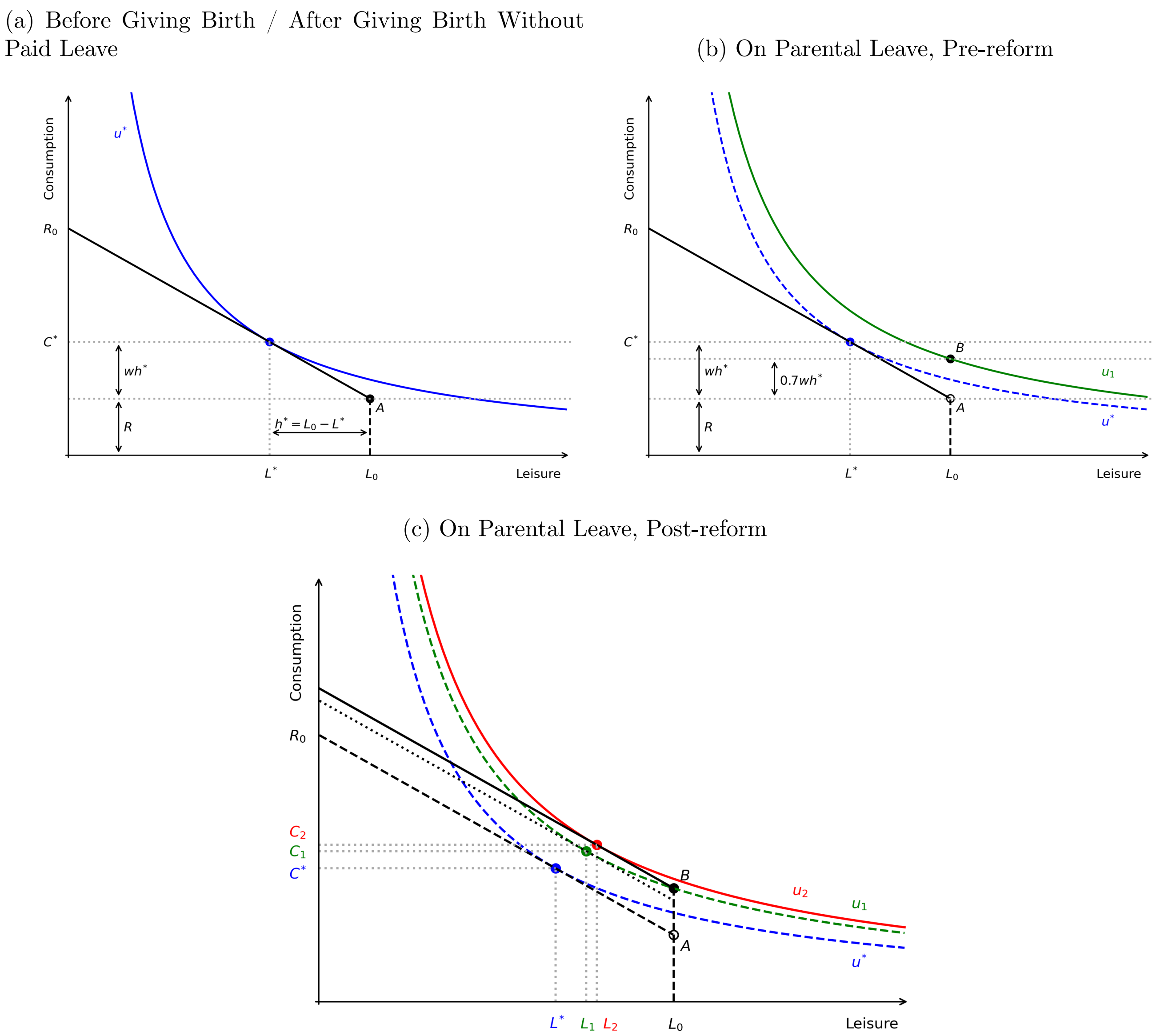
We illustrate both effects in Figure 2, using comparative statics of the labor supply decision under three scenarios in a standard neoclassical model of labor supply: (a) before giving birth (or after giving birth but without any parental leave benefit); (b) after giving birth under the pre-reform paid parental leave scheme; (c) the post-reform paid parental leave scheme that abolished return-to-work restrictions.

In panel (a), we show the labor supply decision of a woman before giving birth. She chooses the level of leisure (L) and consumption (C) that maximizes her utility, subject to the budget constraint. We use the term "leisure" to follow the terminology of the literature but after giving birth, that term also includes childcare time. R_0 denotes her potential income, L_0 is the maximum amount of time to allocate between work and leisure, R is her non-wage income, and w is the hourly wage. At her optimum, she spends L^* hours on leisure, and works $h^* = L^0 - L^*$ hours, and consumes C^*. In what follows, we focus on mothers who have non-zero labor supply before giving birth, so that they are all eligible for paid parental leave.

In panel (b) of Figure 2, we show the budget constraint after giving birth in the pre-reform parental leave scheme: a mother can earn 70% of her previous wage—on top of her non-wage income, R—but only if she does not work at all. In the case we represent here, the mother stops working after giving birth so that she can keep the parental leave benefit. The first potential reason behind her decision to stop working is due to the income effect: at income level $R + 0.7wh^*$ without any work, her marginal rate of substitution at point B (MRS_B), or, equivalently, her reservation wage, may be higher than w. The second potential reason behind her decision to stop working is due to the substitution effect: even if she wanted to work for wage w on the labor market (i.e., her $w > MRS_B$) she does not do so, because moving from L_0 amount of leisure (0 hours of work) to any positive hours of work she would have to give up parental leave benefit in the pre-reform parental leave scheme.

The relative importance of the two channels is policy-relevant: if the substitution effect drives the decrease in mothers' labor supply between scenarios (a) and (b) (corresponding to panels (a) and (b)), there is a deadweight loss (Autor and Duggan, 2007) in the paid parental leave scheme, distorting the incentives to work: mothers, who would otherwise prefer to work, will stay at home because they get paid for staying at home. On the other hand, the income effect captures what policy makers aim for with paid parental leave, that is to provide mothers with sufficient income to allow them to stay at home with their child while keeping their job.

Figure 2: The Labor Supply Decision of Mothers in Three Policy Scenarios



(a) Before Giving Birth / After Giving Birth Without Paid Leave

(b) On Parental Leave, Pre-reform

(c) On Parental Leave, Post-reform

Notes: This figure illustrates the labor supply decision of mothers in different states of the world in the standard neoclassical static model of labor supply. The figures on panel (b) and (c) show the labor market decision of mothers at a given month after giving birth.

In panel (c) of Figure 2, we illustrate what happens after the *CB+W* policy is introduced. In this case, those mothers whose non-participation in the labor market was driven by the second reason (substitution effect) in the pre-reform phase, will decide to work due to a 'negative substitution effect', since the implicit tax on labor market participation is gone (for them point B in panel (c) is no longer optimal, the optimal choice is L_2 & C_2). At the same time, mothers who stayed out of the labor force due to the first reason, due to the income effect, will stay out even after the restriction on work while receiving the parental leave benefit is abolished. Note, that the income effect also affects the hours of work of mothers who switch to working after the reform, and it is represented by the difference between L_1 to L_2 on the figure.

Importantly, in Figure 2 we illustrate the labor supply decision of *complier* mothers, i.e., mothers who do not work after childbirth in the pre-reform setting but would work in the post-reform setting. In Table 2, we summarize the different types of mothers defined by their labor market participation decision in the different policy environments, and the income and substitution effect that is present for them, as a result of the *CB+W* reform. Never-workers decide not to work when offered childcare benefit irrespective of the restriction on working, thus for them there is no income and substitution effect. Compliers switch to work as a response to the *CB+W* reform that is due to the substitution effect, but they work somewhat fewer hours due to the income effect from the childcare benefit. Always-workers might reduce their working hours as a response to the reform, but they would never stop working, since that option with the same income was available for them even before the reform, it did not maximize their utility, and now the budget constraint just loosened compared to that. In sum, the participation decision and thus any change in the observed employment rate of mothers, is only due to the 'negative substitution effect' of the reform.

Table 2: Types of Mothers by Their Participation Decision in Different Policy Schemes

	Participation decision		
Policy	Never-worker	Complier	Always-worker
No *CB*	Yes	Yes	Yes
CB	No	No	Yes
CB+W	No	Yes	Yes
Income effect of *CB+W*	-	Decrease hours	Decrease hours
Substitution effect of *CB+W*	-	Switches to work	-

In the analysis of the income and substitution effects we assumed away some important features that determine the participation decision of mothers in reality, however these simplifications do not alter the main message from our illustration. First, we abstract from the fact that having children might lead to different preferences regarding work, leisure and consumption, thus the labor market decision before giving birth and after giving birth without childcare benefit is not necessarily the same. Second, we do not incorporate the cost of childcare (institutional childcare or private nanny) if a mother decides to work that might have an effect on the budget constraint. Third, paid parental leave comes with job protection. Job protection, just like any other dynamic feature of the labor market decision of mothers, e.g., the role of experience or gaps in employment, cannot be incorporated in the static labor supply model.

4 Data

4.1 Data Sourcse

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 anonymized 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 are identified with International Classification of Diseases (ICD-10) 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).

4.2 Samples: Birth Sample and Control Sample (Women Never Giving Birth)

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 first child was between 13 and 24 months old:

[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.

(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, i.e., who were 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 (pseudo birth 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*. The event dates of the *control sample* fall between 07/2012–12/2012 and 07/2011–12/2011, i.e., at the intervals of the first births of women in the *birth sample*. ($N = 10,138$)

4.3 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 woman 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 sample divided by the 2014 average wage of women in the control sample). 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 (firms with at least 100 employees and smaller firms under special conditions) 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, 2016, 2018), we include the quadratic and cubic age, and year dummies, in X (but no firm-level variables),[5] and the vector of $\hat{\phi}$ is the "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) Standard Occupational Classification (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, capturing to what extent: (1) the job is achievement-oriented (as in: it requires establishing and maintaining personally challenging goals and exerting effort toward mastering tasks, it requires persistence in the face of obstacles, and it requires a willingness to take on responsibilities); (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 Empirical Strategy

5.1 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 (birth in the *birth sample*, random pseudo birth date in the *control sample*), 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. The Event Study regression model is the following:

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

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

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:

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

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 sample (for whom by definition the outcome is zero) and replace the individual FEs with a vector of control variables of age effects, 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 FEs since the outcome (birth of a subsequent child) has little time variation, without any time variation for those mothers who did not have a second birth in our observation period.

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

Here, we restrict the sample to months 6–60 after the birth of the first child. The outcome is an indicator of giving birth in the given month. The coefficients of interest θ_k, and we make the normalization $\theta_6 = 0$.

5.2 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 we estimate 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 showed that *CB+W*-eligibility affects employment. We look at the logarithm of hourly wage, estimated AKM firm effect, firm size, white-collar employment, and O*NET scores. We also estimate the policy effect on work hours.

We estimate equation (3) with the job quality indicators as outcome variable. We restrict the sample to event times -36 to -13 and 25 to 60, and to those women who were employed at least for 12 months over event time 25 to 60. This additional restriction makes the sample quasi-balanced, i.e., we do not include those women in the pre-event period whom we do not observe to work at least for a year in the post-event period (3–5 years after the event). We also estimate the average effect of the reform on job quality 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$_{it}$).

5.3 Robustness: Event Study Research Design on Matched Sample

There exists an 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 3). To verify that our estimates are not affected by the baseline imbalance—a level difference—we apply a matching strategy.

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; the binary indicator of the mode of employment 13–36 months before birth being 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.

6 Results

6.1 Effects on Employment

Panels (a)–(b) of Figure 3 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 sample, for whom the pseudo 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. As a reminder, mothers in the eligible cohort gave birth in the same part of the year as mothers in the ineligible cohort, only a year later, thus, these two cohorts of mothers very likely were affected by the same aggregate-level shocks.

Figure 3 reveals three interesting facts. First, based on the full (baseline) sample (panel (a)), the employment rate slowly and steadily increases over time in the control sample. Second, mothers have higher employment rate before birth than women who do not give birth, which difference, reassuringly, almost disappears in the matched sample (panel (b)). Third, 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 policy.

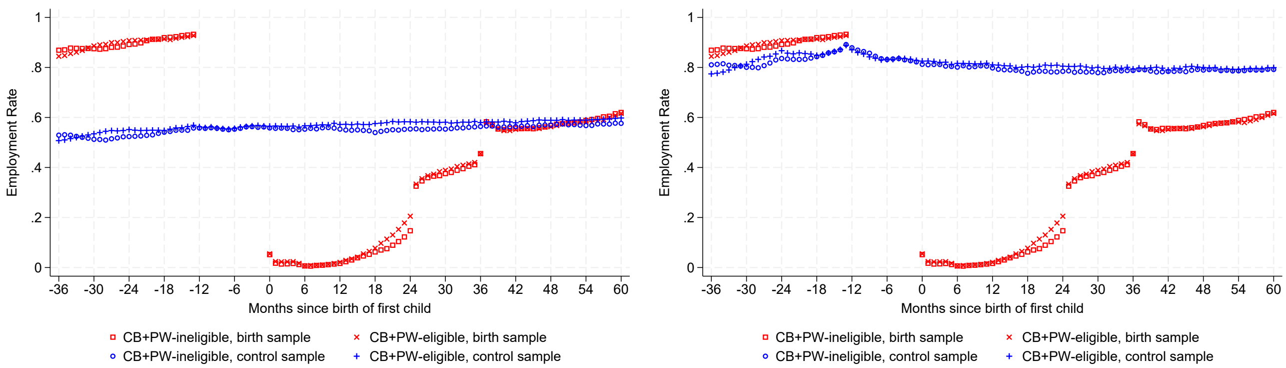
Figure 4 shows the Event Study coefficient estimates for the birth sample and the control sample separately (in panels (a) and (b), respectively), so that the differences between the estimated coefficients at each event time can be seen in panel (c). Specifically, in panels (a) and (b) we show the estimated $\tilde{\beta}_k$'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 sample (women never giving birth).[6] Panel (c) shows the estimated δ_k coefficients

[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 (eligible) group is observed in 2008 (2009) at event time -36 and in 2009 (2010) at event time -24, between which periods the employment rate decreased (levelled off) in Hungary.

from equation (3) on the baseline sample, and panel (d) shows the same on the matched sample.

Figure 3: Employment Rate Over Event Time

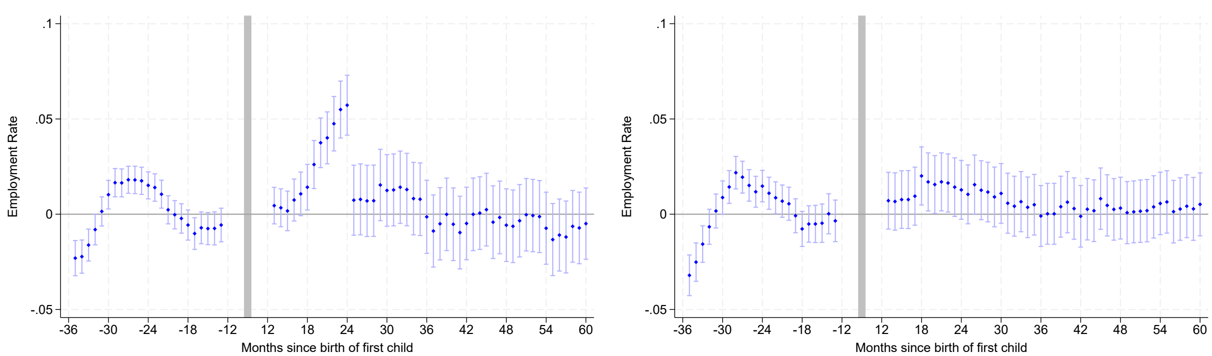
(a) Baseline Sample (b) Matched Sample



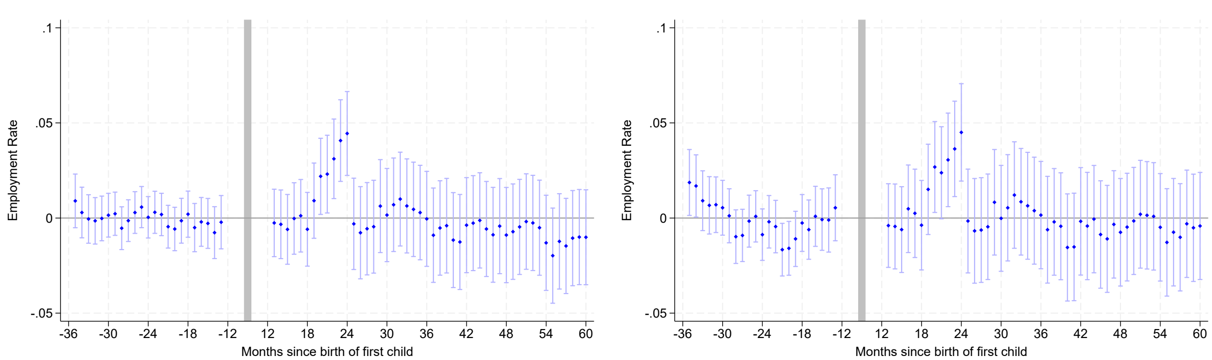
Notes: Sample is as described in Section 4.2. The "birth" sample" consists of women whom we observe giving birth. The "control sample" consists of women whom we do not observe giving birth.

Figure 4: 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 4.2. 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 is in panel (c). In (a) and (b) we show the estimated $\tilde{\beta}_k$'s from model (2). In (a), the sample is women giving birth; in (b), the sample is 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.

We find that eligibility to 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 increase is 2.2, 2.3, 3.1, 4.1, and 4.4 percentage points in months 20–24, respectively. As panel (d) reveals, 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 that can be seen in panel (a).

Table 3: 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 3^{rd} and 2^{nd} years before the event 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 before the event. Baseline wage and AKM firm FE refer to 13–24 months before the event. Robust standard errors in brackets. Number of individuals: 21,922. Number of observations: 657,619. *** p<0.01, ** p<0.05, * p<0.1.

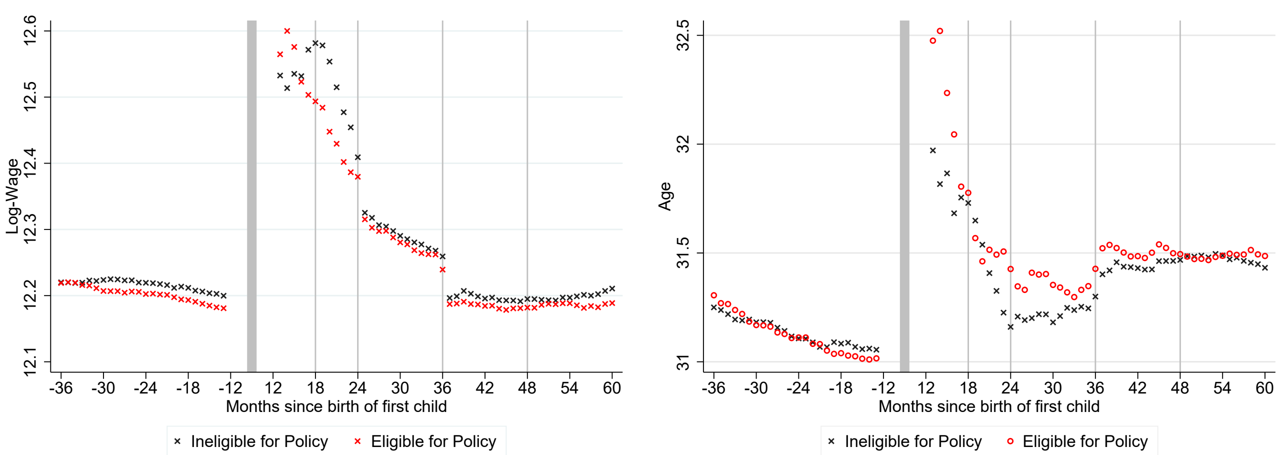
In Table 3 we investigate the heterogeneity of the *CB+W* policy's effect on employment by pre-birth characteristics. The top row indicates that on average, the *CB+W* policy increased the employment rate by 2.8 percentage points (3.0 percentage points in the matched sample, or about 30 percent) 19–24 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). Yet, the differences along these 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.

In sum, the effect of the policy on the propensity to work is driven – at least in terms of the point estimates – by women who are above 30 years old when giving birth, and with above-median pre-wage.

Figure 5 shows the composition of the mothers' workforce at different points in time, relative to the birth of their first child. Reassuringly, there are no major differences between the average wage 1 year before the birth of their first child of those working mothers who are in the labor force 36-to-12 months before the birth in the eligible and ineligible cohorts (panel (a)), and the same holds for their average age (panel (b)). (The gray crosses in months 12-to-24 after birth represent the averages for mothers who work in the child's 2^{nd} year of age even if the financial incentives by the policy was not provided for them.)

Figure 5: Composition of Mothers' Workforce Before and After Birth

(a) Average Pre-Birth Wage of Those Working (b) Average Age at Birth of Those Working



Notes: Sample only contains women who gave birth, in the eligible and ineligible cohorts, as described in Section 4.2.

In Figure 4, we have seen that the policy did not affect mothers' propensity to work differently in the eligible vs. ineligible cohorts 3–5 years after the birth of the first child. Consistent with this result, in Figure 5 there are no compositional differences in the workforce of the two cohorts 3–5 years after the birth of the first child either. Thus, the estimates for the policy effect on mothers' job quality outcomes 3–5 years after birth are not tainted by compositional differences in the workforce of the two—eligible vs. ineligible—cohorts. Before analyzing these, we assess the average characteristics of the compliers.

6.2 Characteristics of Complier Mothers Responding to the Policy

We follow the method proposed by Almond and Doyle (2011) to analyze the characteristics of compliers.

The endogenous variable we consider is returning to work in months 19–24 after giving birth (W). Thus, compliers in such setting are, by definition, mothers who work in months 19–24 after giving birth if they are eligible for the policy, but who would not work had they been ineligible for the policy. The average characteristics of the compliers can be estimated using the following formula:

$$E\left(X|W_1=1,W_0=0\right)=\frac{\pi_C+\pi_A}{\pi_C}\left[E\left(X|W=1,D=1\right)-\frac{\pi_A}{\pi_C+\pi_A}E\left(X|W=1,D=0\right)\right], \tag{6}$$

where $W_1=1$ denotes working 19–24 months after giving birth if being in the eligible cohort (eligible for the policy, $D=1$), $W_0=0$ denotes not working 19–24 months after giving birth if being in the ineligible cohort (ineligible for the policy, $D=0$), π_A is the share of always-takers and π_C is the share of compliers.

Using the independence assumption between D and W, and assuming away defiers by the monotonicity condition, the share of always-takers and never-takers, π_A and π_N, can be estimated by the empirical counterpart of $Prob(W_0=1)$ and $Prob(W_1=0)$; we estimate $\hat{\pi}_A=0.154$ and $\hat{\pi}_N=0.781$. Thus, the estimated complier share is $\hat{\pi}_C=0.065$.

Table 4 shows the estimated characteristics of compliers for various X's, and whether those significantly differ from averages in the *birth sample*. Consistent with results in Table 3, complier mothers are significantly older and have a higher pre-birth wage than mothers in the overall sample and, albeit the differences are not significantly different from zero at the 5 percent level, are more likely to live in the capital, are more likely to work in white-collar occupations before giving birth, and their pre-birth firm has, on average, higher estimated AKM firm effect reflecting more ability to pay higher wages.

Table 4: Average Characteristics of Compliers and of Overall Sample

	Compliers	Full Sample	p-value of Diff
In Capital (Budapest)	0.2087	0.1930	0.2313
White-Collar	0.7460	0.7117	0.0843
Age-at-Birth (Years)	30.753	30.295	0.0009
Base Log Hourly Wage	12.308	12.161	0.0000
Base AKM Firm FE	0.0650	0.0486	0.1327

Notes: Table shows average characteristics of compliers, estimated with equation (6), average characteristics of the full *birth sample* as defined in Section 4.2. The last column shows the p-values of the equality tests between the compliers and the full sample.

6.3 Effects on 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 analyzed reform have opposite effect on the timing of subsequent births. By itself, the *CB+W* reform could have motivated mothers to delay future pregnancies 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).

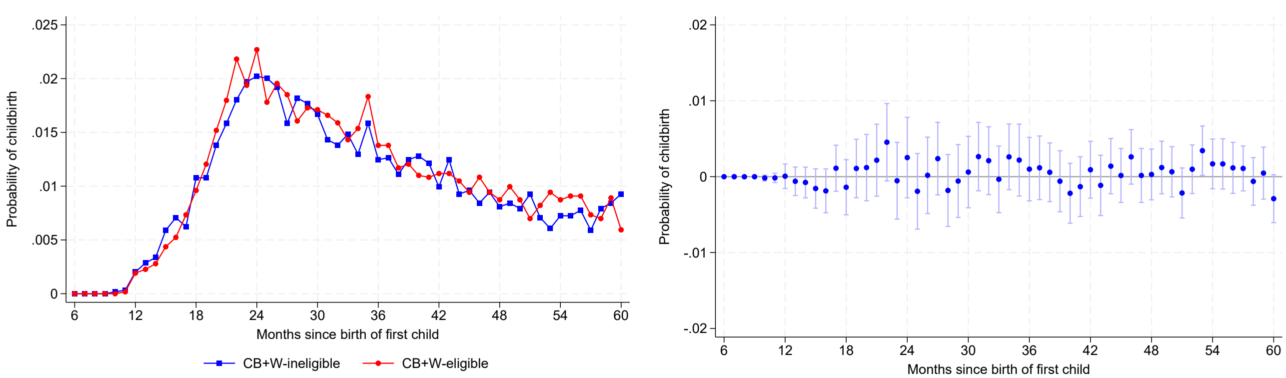
Despite these differences between the eligible and ineligible cohorts' incentives to have a 2^{nd} child, panel (a) of Figure 6 suggests that there is no systematic difference in the timing of second birth between our chosen two cohorts of mothers. We test the statistical significance of these raw differences in fertility by estimating equation (5) on childbirth rate. Panel (b) of Figure 6 confirms that there is no statistically significant difference in future fertility between eligible and ineligible mothers.

Our findings on fertility suggest that 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 6: Effect of the $CB{+}W$ Policy on Childbirth Rate (Further Child)

(a) Descriptive

(b) Difference-In-Differences; Birth Sample



Notes: Panel (a) shows childbirth rate after the birth of the first child. Panel (b) shows estimated θ_k coefficients from equation (5). Bars are 95 percent confidence intervals based on robust standard errors. Sample is as described in Section 4.2, excluding the control sample (women never giving birth).

In sum, the policy not only did not affect mothers' propensity to work in the eligible vs. ineligible cohorts 3–5 years after the birth of the first child, it also did not affect mothers' subsequent fertility behavior. Thus, the estimates for the policy effect on mothers' labor market outcomes 3–5 years after birth are not tainted by compositional differences in the workforce or in motherhood of the two cohorts.

6.4 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, except one notable difference: mothers in eligible cohorts were by 3 percentage points more likely to work 19–24 months after giving birth. We show the $\hat{\delta}$ estimates from equation (3) in Figure 7.[7] We present descriptive time patterns and DiD estimates from equation (2) in Figures A2–A6. All estimates are conditional on working.

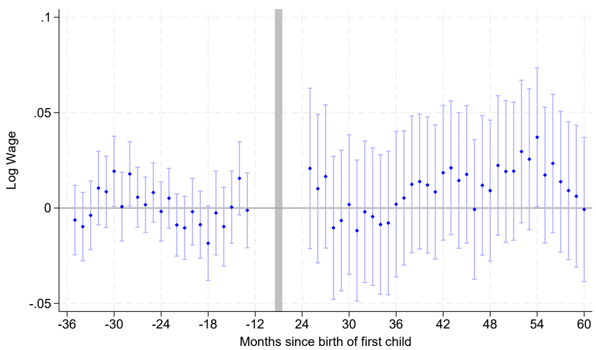
Panels (a), (c) and (d) of Figure 7 suggests small positive but statistically insignificant effect of $CB{+}W$-eligibility on hourly wage, firm size decile, and the probability of employment in a white-collar

[7] Appendix Figure A1 shows that these results are robust to restricting the estimation sample to the matched sample.

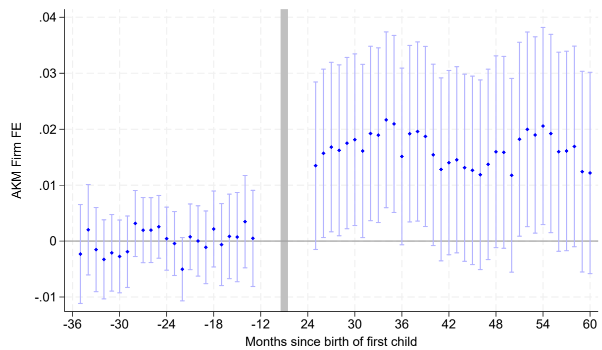
job. Panel (b) indicates that $CB{+}W$-eligibility increases the AKM firm FE of the employer by up to 0.02, corresponding to a 2 percent increase of the firm wage premium. Looking at working hours, panel (e) of Figure 7 shows that in the third year after childbirth, the weekly hours worked increase by 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 3^{rd} year of age of the child.[8]

Figure 7: Effect of $CB{+}W$-Eligibility on Job Characteristics

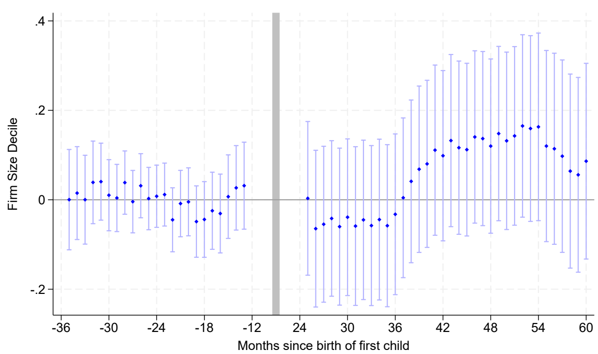
(a) Log Hourly Wage



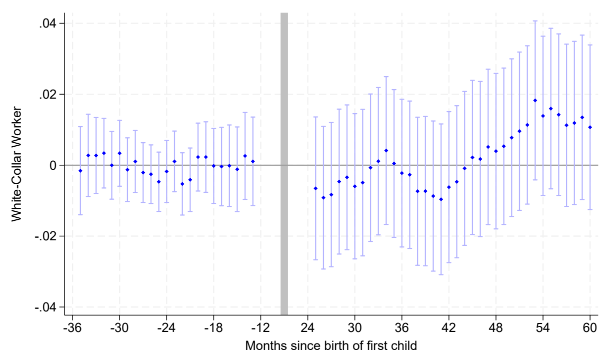
(b) AKM Firm FE



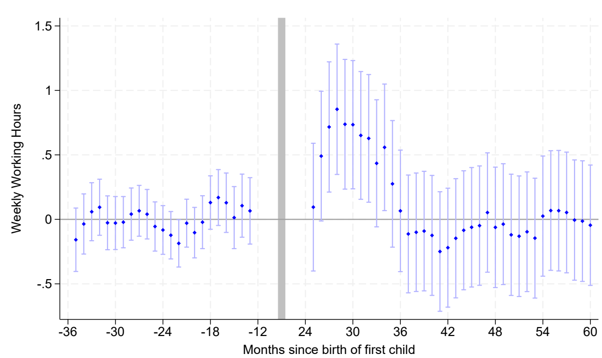
(c) Firm Size Decile



(d) Employment in White-Collar Job



(e) Weekly Hours of Work



Notes: Figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2.

[8]The reform component abolishing the 30-hour working restriction for PL benefit recipients explains this: some of the ineligible group (whose child turns 13-months-old in 07/2012—12/2012) are eligible only for fewer months (e.g. a mother whose child turns 2-years-old in 07/2013 is eligible for the full-time work only for 6 months, in 01/2014–06/2014) 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.

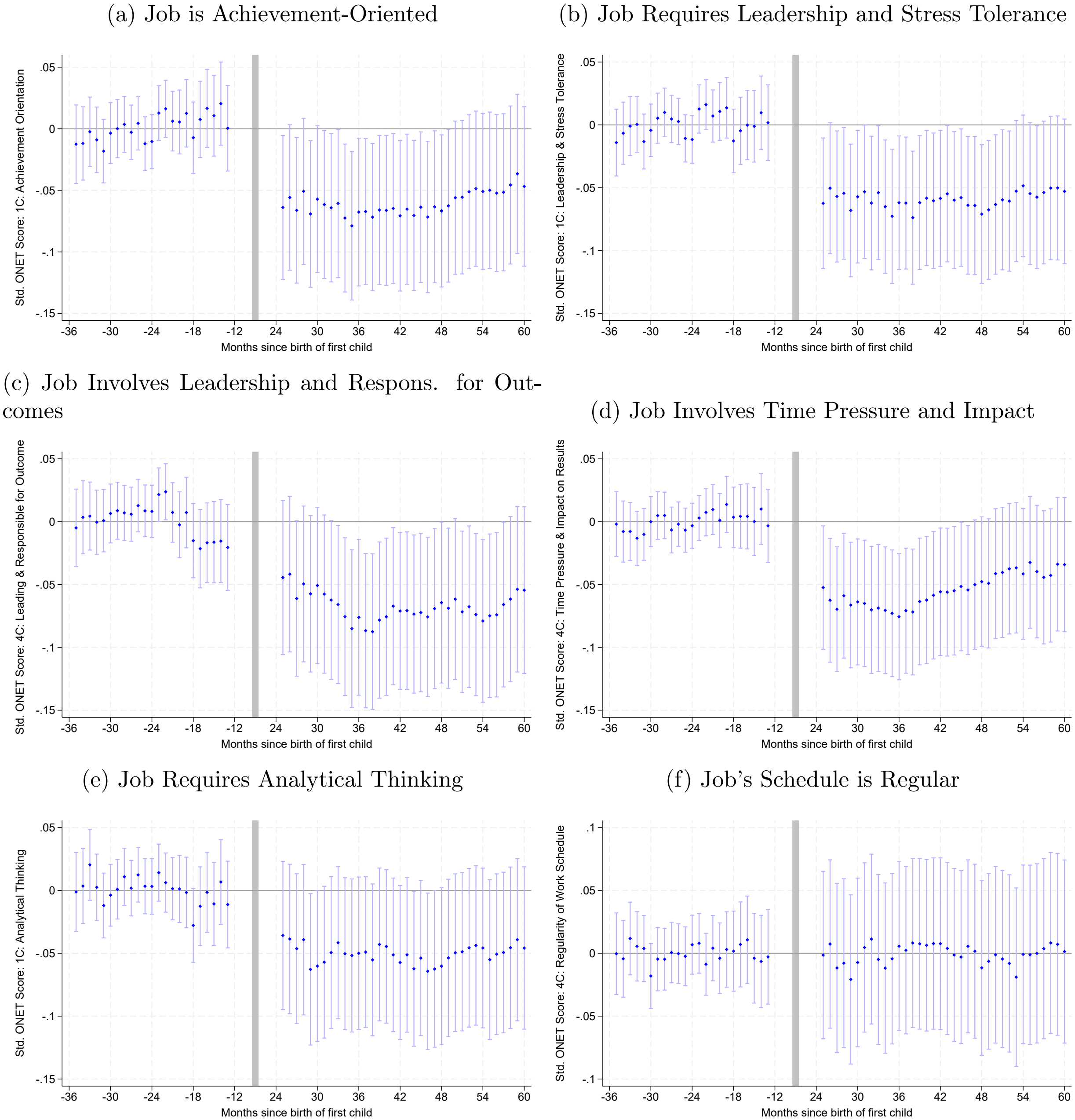
Then, in the first panel of Table 5, we show the average effects of $CB{+}W$-eligibility in the 3^{rd} to 5^{th} year after giving birth, for all. The first column shows that wages are, on average, by 1.2 percent higher after giving birth, for the $CB{+}W$-eligible vs. non-eligible mothers vs. non-mothers, but these differences are not statistically significant at the usual levels. Column (2) of the first panel reveals that the estimated AKM firm effect – capturing the firm-specific wage premium – is significantly higher by 1.6 percent in the 3^{rd} to 5^{th} year (which estimate is significant at the 5 percent level); this result indicates that $CB{+}W$-eligible mothers sorted into higher-paying, and presumably better, firms 3–5 years after giving birth, relative to non-eligible mothers (and all relative to non-mothers). Differences between eligible and non-eligible mothers in hours worked, firm size and being in a white-collar occupation are not significant.

Table 5: Heterogeneity in the Effect of the $CB{+}W$ Policy on Job Characteristics

		(1)	(2)	(3)	(4)	(5)
		Log. of Hourly Wage	AKM Firm FE	Decile of Firm Size	White-Collar	Hours
All	afterY3-5 × B × D	0.012	0.016**	0.066	0.004	0.135
		[0.014]	[0.008]	[0.089]	[0.010]	[0.201]
By Age at Childbirth						
Age 25-30	afterY3-5 × B × D	-0.019	0.002	-0.100	0.014	0.369
		[0.020]	[0.012]	[0.126]	[0.015]	[0.292]
Age 31+	afterY3-5 × B × D	0.043**	0.030***	0.252**	-0.005	-0.018
		[0.020]	[0.010]	[0.124]	[0.014]	[0.273]
By Location						
Rural	afterY3-5 × B × D	-0.002	0.007	-0.004	0.009	0.220
		[0.016]	[0.008]	[0.100]	[0.012]	[0.218]
Capital	afterY3-5 × B × D	0.071**	0.048**	0.287	-0.017	-0.095
		[0.034]	[0.020]	[0.196]	[0.020]	[0.493]
By Baseline Occupation						
Blue-Collar	afterY3-5 × B × D	-0.012	0.007	0.074	0.013	0.178
		[0.024]	[0.013]	[0.150]	[0.023]	[0.446]
White-Collar	afterY3-5 × B × D	0.025	0.018*	0.055	0.000	0.167
		[0.017]	[0.010]	[0.111]	[0.010]	[0.215]
By Baseline Wage						
Below Median	afterY3-5 × B × D	-0.000	0.011	0.167	-0.020	-0.172
		[0.022]	[0.013]	[0.172]	[0.019]	[0.408]
Above Median	afterY3-5 × B × D	0.020	0.017*	0.045	0.017	0.280
		[0.018]	[0.009]	[0.106]	[0.011]	[0.206]
By Baseline AKM Firm FE						
Below Median	afterY3-5 × B × D	-0.017	0.017	0.126	-0.014	-0.267
		[0.021]	[0.012]	[0.285]	[0.016]	[0.348]
Above Median	afterY3-5 × B × D	0.026	0.013	0.081	0.015	0.429*
		[0.019]	[0.010]	[0.101]	[0.013]	[0.222]

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 3^{rd} and 2^{nd} years before the event as reference period (equation (4), but allowing the treatment effect to vary across 12-month periods, and estimating average effect for years 3 to 5 after birth). For each heterogeneity variable a separate (split-sample) regression is estimated. 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. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. Number of individuals in the full sample: 13,570. Number of observations in the full sample: 663,427. Outcomes are in the column titles.

Figure 8: Effect of $CB{+}W$-Eligibility on Occupation Characteristics



Notes: Figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)), where the outcomes are standardized O*NET scores that describe the the extent of which a job has the characteristic described in the panel titles. Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2.

Further panels of Table 5 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. For instance, wages of mothers in Budapest are by 7.1 percent higher in the 3^{rd} to 5^{th} year after giving birth, for the $CB{+}W$-eligible vs. non-eligible mothers vs. non-mothers, and these differences are significant, whereas we do not find any impact for mother outside Budapest. Similarly, the estimated AKM firm effects for those living in the capital are by 4.8 percent higher. Wages of mothers who were at least 31 years old when giving birth are by 4.3 percent higher in the long-run, and their estimated AKM firm effects are also higher on average by 3 percent. The results on the AKM firm effects are also rather driven by white-collars and with above-median baseline wages.

Column (3) of Table 5 reveals that the overall effect on firm size decile is driven by those who were 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. Column (5) of Table 5 reveals that the overall effect on hours in the 3^{rd} year after giving birth is driven by those with above-median baseline wages and above-median baseline AKM firm effect.

Turning to occupation characteristics, Figure 8 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). In Table 6, we show the effect of $CB{+}W$-eligibility on the standardized occupation score, relative to the baseline score in the 2^{nd} and 3^{rd} year before childbirth. Already in the 3^{rd} year after childbirth, eligible mothers have sorted into occupations that require less analytical thinking, stress tolerance, leadership and willingness to take on responsibilities, and involve less time pressure, and the difference stays relatively stable up to years 5 after childbirth. This result suggests important persistence in occupational sorting of mothers with young children.

Table 6: Effect of $CB{+}W$-Eligibility on Standardized Occupation Scores

	Std. Analytical O*NET Score	Std. Achievement O*NET Score	Std.Lead-Stress O*NET Score	Std.Lead-Resp. O*NET Score	Std. Time Pressure O*NET Score
afterY3 $\times$ B $\times$ D	-0.0488*	-0.0659**	-0.0624**	-0.0599**	-0.0677***
	[0.0294]	[0.0291]	[0.0258]	[0.0305]	[0.0244]
afterY4 $\times$ B $\times$ D	-0.0550*	-0.0698**	-0.0649**	-0.0742**	-0.0572**
	[0.0303]	[0.0300]	[0.0266]	[0.0309]	[0.0249]
afterY5 $\times$ B $\times$ D	-0.0497	-0.0560*	-0.0594**	-0.0703**	-0.0415
	[0.0316]	[0.0314]	[0.0279]	[0.0321]	[0.0259]

Notes: Table shows average effects of the $CB{+}W$ policy on occupation characteristics (O*NET scores) (equation (4), but allowing the treatment effect to vary across 12-month periods). Baseline is the 3^{rd} and 2^{nd} years before the event. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. Number of individuals: 13,274. Number of observations: 651,127. Outcomes are in the column titles.

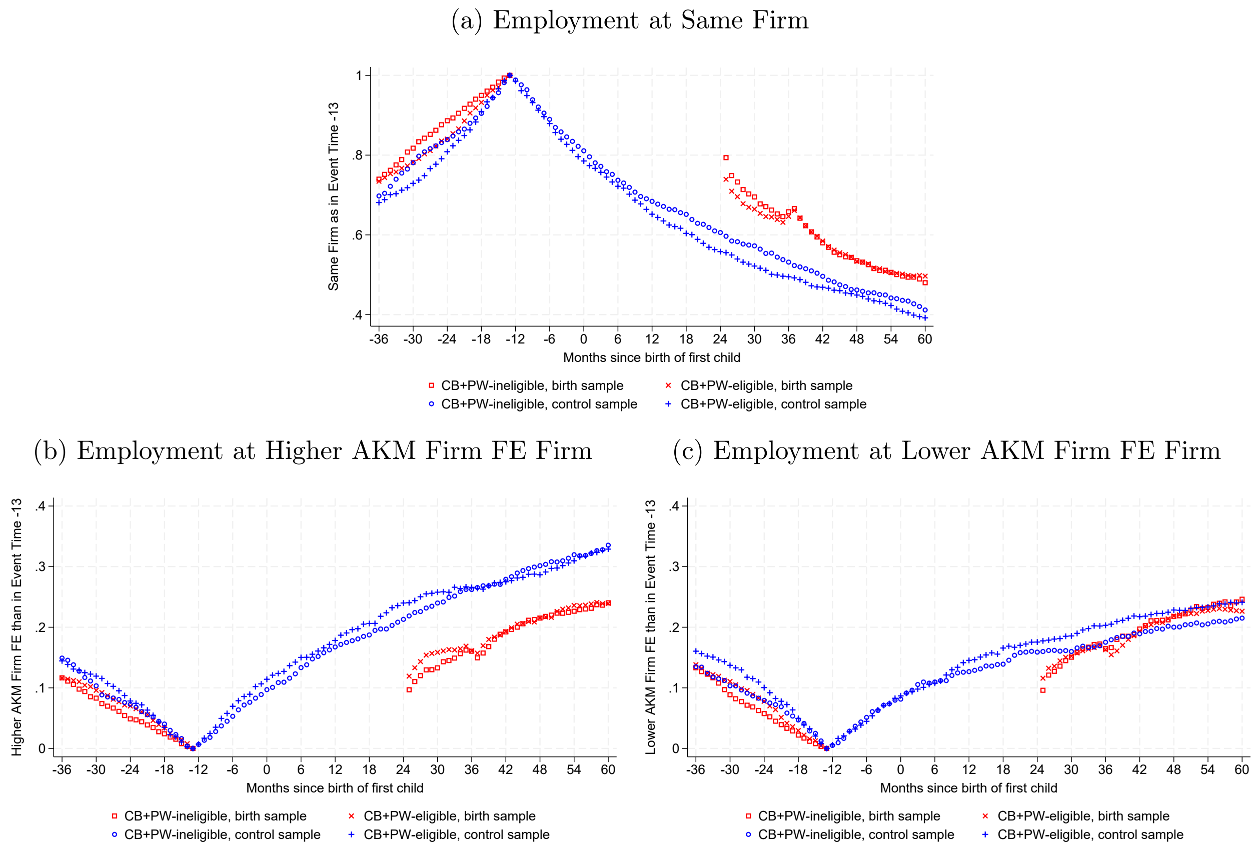
6.5 Mechanisms

6.5.1 Staying at the Same Employer or Moving to Different Employers

In this section, we investigate what patterns of transitioning between employers can explain the above presented estimated effects of earlier return to work, primarily on wages and the AKM firm effect. For this, we first define a binary indicator variable, indicating if the worker works at the same firm in a given month as at event time -13. Second, conditional on working at a different firm in a given month as at event time -13, we define further indicator variables, indicating if the worker this month works at a firm that has a higher or lower AKM firm effect than the firm had at event time -13.

Panel (a) of Figure 9 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 a firm that has a higher (panel (b)) or lower (panel (c)) AKM firm FE, than their firm had at event time -13. Here, panel (c) suggests that 3–5 years after childbirth, the propensity to have moved to a firm with lower AKM firm effect is relatively lower among those mothers who were $CB{+}W$-eligible. We now turn to regression estimates, presented in Table 7, to quantify these differences, and to see if they are significantly different from 0.

Figure 9: Descriptive Patterns of Staying at the Same Firm and Transitions Between Employers



Notes: Figure shows the share of workers employed at the same firm (panel (a)), at a higher (panel (b)), and at a lower quality firm (panel (c)) as in event time -13. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2.

In Table 7, we show the effects of the policy on the propensity to (i) be employed at the same firm as one year before the event, (ii) be employed by a firm with a lower AKM firm effect (and lower firm-specific wage premium), and (iii) be employed by a firm with a higher AKM firm effect (and higher firm-specific wage premium). The row for 'All' in *Panel A* reveals that the eligibility to the policy increased mothers' propensity to stay at the same firm by around 3.2 percentage points, and, conditional on switching, it decreased the propensity to switch 'down' to a firm with a lower AKM firm effect by around 3 percentage points across years in the 3^{rd}–5^{th} year after childbirth (these estimates are significant at the unusual 15 percent level). The row for 'All' in *Panel B* reveals that these estimates are roughly constant across years after childbirth. (These estimates are robust for using the matched sample described in section 5.3.)

Subsequent rows show that the negative effect on switching down to a firm with lower AKM firm effect is entirely driven by mothers who are 31 years or older when giving birth (see the -5.7 percentage points estimates in columns (3) in *Panel A*, and point estimates of -4.4, -5.9, and -6.2 percentage points in the 3^{rd}, 4^{th} and 5^{th} year after giving birth in column (3) of *Panel B*, respectively). At the same time, the increased propensity of staying at the same firm is driven by women in Budapest, the capital of Hungary (with point estimates of 8.5 percentage points in column (1) in *Panel A*).

Results from Tables 5 and 7 suggest that for women in the capital, earlier return to work due to the policy may have helped to be more likely to be promoted internally at the same firm (given that women in the capital also experience positive wage effects of the policy). At the same time, for older women, the positive wage effects seem to be driven by a lower likelihood of switching down to lower-paying firms.

Table 7: Effect of *CB+W*-Eligibility on Transitions Across Firms Compared to One Year Before the Event

		1: At Same Firm	1: At a Firm With... Higher AKM	Lower AKM
		(1)	(2)	(3)
Panel A: Averaging Across Y3-5				
All	afterY3-5 × B × D	0.0323	-0.0006	-0.0298
		[0.0233]	[0.0185]	[0.0187]
By Age at Childbirth				
Age 25-30	afterY3-5 × B × D	0.0225	-0.0209	-0.0004
		[0.0311]	[0.0250]	[0.0255]
Age 31+	afterY3-5 × B × D	0.0394	0.0175	-0.0572**
		[0.0324]	[0.0256]	[0.0255]
By Location				
Rural	afterY3-5 × B × D	0.0166	0.0039	-0.0265
		[0.0254]	[0.0203]	[0.0203]
Capital	afterY3-5 × B × D	0.0851*	-0.032	-0.0245
		[0.0477]	[0.0377]	[0.0394]
		1: At Same Firm	1: At a Firm With... Higher AKM	Lower AKM
		(1)	(2)	(3)
Panel B: Averaging By Y3,Y4,Y5				
All	afterY3 × B × D	0.0274	-0.0027	-0.0260
		[0.0240]	[0.0188]	[0.0190]
	afterY4 × B × D	0.0299	0.0004	-0.0274
		[0.0242]	[0.0195]	[0.0197]
	afterY5 × B × D	0.0272	0.0072	-0.0317
		[0.0247]	[0.0206]	[0.0208]
By Age at Childbirth				
Age 31+	afterY3 × B × D	0.0195	0.022	-0.0437*
		[0.0332]	[0.0260]	[0.0257]
	afterY4 × B × D	0.0389	0.0183	-0.0591**
		[0.0336]	[0.0269]	[0.0269]
	afterY5 × B × D	0.0491	0.0147	-0.0621**
		[0.0344]	[0.0282]	[0.0284]
By Location				
Capital	afterY3 × B × D	0.0557	-0.0307	0.0001
		[0.0498]	[0.0387]	[0.0399]
	afterY4 × B × D	0.1019**	-0.0377	-0.0344
		[0.0500]	[0.0402]	[0.0417]
	afterY5 × B × D	0.079	-0.0165	-0.0327
		[0.0505]	[0.0418]	[0.0440]

Notes: Table shows average effects of the *CB+W* policy on the probability of working in specific categories of firms, compared to the employer one year before the event (equation (4), but allowing the treatment effect to vary across 12-month periods). Baseline is the 3^{rd} and 2^{nd} years before the event. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. Number of individuals: 8,630. Number of observations: 417,381. Outcomes are in the column titles.

6.5.2 Compliers and Always-Workers

Finally, we estimate the Local Average Treatment Effect (LATE) of early return to work after childbirth, due to eligibility to the *CB+W* policy, on the wage and AKM firm FE of the complier mothers, 25 to 60 months after their childbirth. In this Instrumental variable (IV) setup, the endogenous variable is R_i, a binary indicator of early return, being equal to one if the mother works at least for one month in months 13 to 24 after childbirth, and is zero otherwise, and the IV is the binary indicator of *CB+W*-eligibility. To the extent the exclusion restriction holds and *CB+W*-eligibility is related to mothers' wages and their firm's AKM firm effects in the longer run only through early return (R_i) and not through other direct channels (such as the income effect of the policy, that is due to the extra income for mothers working before their child turns 2-years-old), this IV approach captures the effect of early return on job quality among the compliers, i.e., among mothers who returned early to work due to *CB+W*-eligibility.

Return to work after childbirth is defined only for women giving birth, thus we cannot include women never giving birth in the IV regression. However, to ensure that our results are not driven by aggregate time trends, and to make the exclusion restriction more likely to hold, we estimate time effect parameters on the sample of women never giving birth separately for log wage and AKM firm FE, and use these estimates to net out time effects also from the log wage and AKM firm FE data of mothers.[9] We use these net-of-time-effects wage and AKM firm FE values as outcomes. The IV estimates are in Table 8, which shows that women who returned to work in their children's 2^{nd} year of age only because they were eligible for the policy have, on average, 2.73 percent higher wage and 8.81 percent higher AKM firm effect 3-5 years after childbirth, all else equal (where the second point estimate is significant at the 5 percent level). The first-stage relationship is very strong with point estimate of 8.5 percentage point.

Table 8: Effect of Early Return to Work After Childbirth on Wages and AKM Effect; LATE Estimates

	Log Wage	AKM Firm FE
R_i: return to work in M13-24	0.0273	0.0881**
	[0.0871]	[0.0430]
Number of observations	207,792	263,329
First-Stage Results	Return to work in M13-24	
CB+W-eligibility	0.0849***	
	[0.0106]	

Notes: Table shows IV regression estimates on the effect of returning to work 13–24 months after giving birth on log wages and AKM firm FE 25-60 months after childbirth. Control variables include: full set of age FEs, full set of baseline AKM firm effect decile FEs, baseline wage, baseline occupation, and county FEs, where the baseline period corresponds to eventtime −13. Cluster robust standard errors in brackets (clustered at the individual level). *** p<0.01, ** p<0.05, * p<0.1. Outcomes are in the column titles.

Next, we check to what extent the estimated effect of *CB+W*-eligibility on wages and AKM firm FE are driven by always-workers, i.e., by mothers who return to work even if they are not eligible for the *CB+W* policy. As we discussed in Section 3, *CB+W*-eligibility could affect the work effort of always-workers even if their participation decision is unaffected. Since a woman is either eligible or ineligible to the policy, one certainly cannot directly identify always-worker mothers in the sample. Instead, we focus on those women

[9] Specifically, on the sample of women never giving birth, and using the notation of equation (2), we estimate the regression $\Omega_{it} = \alpha + \sum_k \alpha_k \mathbb{1}[e_{it} = k] + \sum_k \beta_k \mathbb{1}[e_{it} = k] \times D_i + \varepsilon_{it}$. We use the estimated α and β parameters to net out time effects from the wage and AKM firm FE observations of women giving birth.

who would have returned early to work with high probability even in the pre-reform period. Specifically, on the sample of *CB+W*-ineligible mothers, we regress the binary indicator of returning to work 13–24 months after childbirth on age at birth, living in the capital city, baseline occupation categories (mode of occupation 13–36 months before childbirth), and wages and the firm fixed effect in the 2^{nd} year prior childbirth being above their median. We use the predicted probability of early return to categorize mothers as "always workers" – we consider those mothers as such, for whom the predicted probability is above the 75th percentile or the 90th percentile of its distribution (i.e., it is above 29.2% or 32.7%, respectively). Restricting the sample to the so defined "always worker" mothers, we estimate a reduced form model for the relation between *CB+W*-eligibility and log wage or AKM firm FE, controlling for the same variables as in equation (5), and using the net of time effects values of wage and AKM firm FE, as explained above.

We report the reduced form estimates for "always worker" mothers in Table 9, which shows no reduced-form effect for these women, no matter which cutoff is used. Thus, our main estimates are indeed interpretable as the effects of earlier return to work after childbirth on mothers' long-term labor outcomes.

Table 9: Effect of *CB+W*-Eligibility on Wages and Firm Wage Premium on "Always-Worker" Mothers

	Log Wage		AKM firm FE	
	Top 90 percentile	Top 75 percentile	Top 90 percentile	Top 75 percentile
CB+W-eligible	-0.0130	-0.0093	-0.0034	0.0037
	[0.0225]	[0.0155]	[0.0087]	[0.0060]
Observations	18,737	46,091	28,898	68,406

Notes: Table shows estimates of equation (5), with log wage or AKM firm FE as dependent variable, restricting the sample to "always worker" mothers, as defined in the main text. Cluster robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

7 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.

We use an Event Study research design comparing the careers of women eligible and ineligible for the policy, with women without births as comparison group, based on 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 3 percentage points or 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.

The estimated impact on employment is due to the response of mothers who previously would not have worked due to the substitution effect, that is the distortional effect, of paid parental leave. This finding suggests that the majority of mothers who decide not to work until their child's 2^{nd} birthday (90% of all mothers before the reform) do so due to the income effect of paid parental leave or changed preferences compared to pre-birth preferences that prioritize staying at home with the child.

Second, we assess whether earlier return-to-work affects mothers' job quality outcomes. We find significant effects on mothers' wages and on their firms' average wage premium 3–5 years after giving birth for those who gave birth at an older age and live in the capital – these results are due to eligible mothers being more likely to stay at the same firm and less likely to switch 'down' to lower-paying firms.

We also find that eligible mothers sort 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, and suggest important persistencies in young mothers' occupational sorting behavior.

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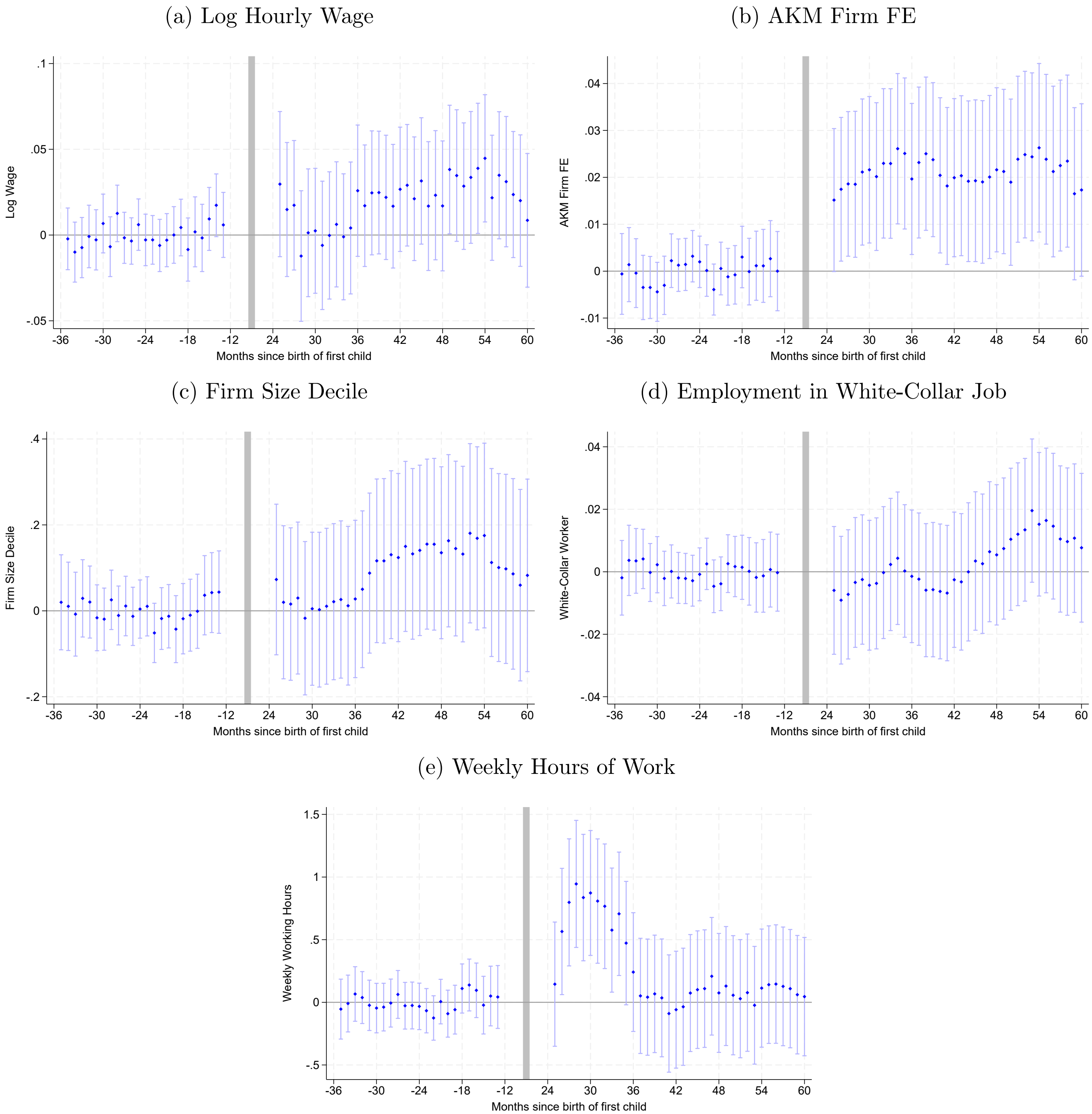
A Appendix: Additional Figures and Tables

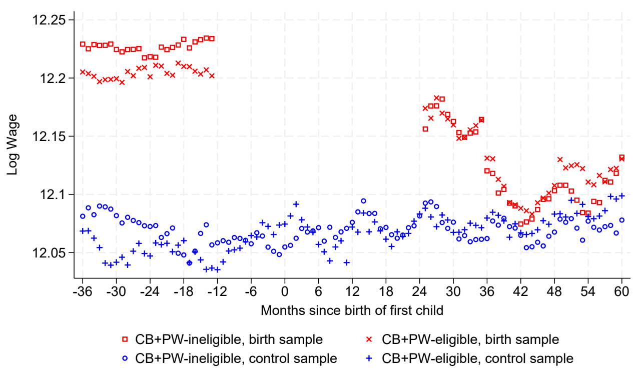
Appendix Figure A1: Effect of $CB{+}W$-Eligibility on Job Characteristics, Matched Sample



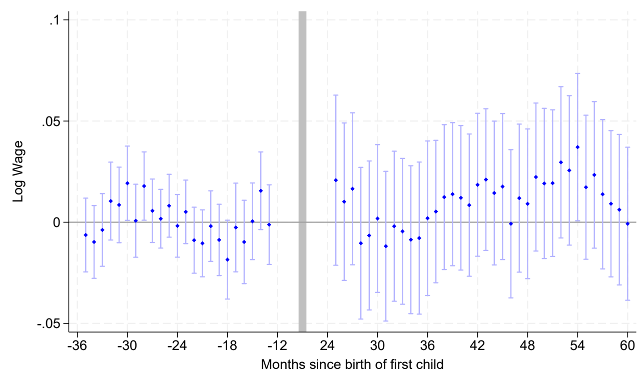
Notes: Figure shows the Event Study coefficient estimates (δ_k coefficients from equation (3)). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the matched sample, as described in Section 4.2, with further sample restrictions described in Section 5.2.

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

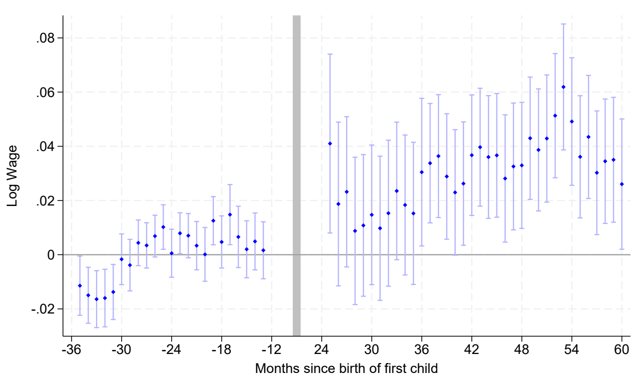
(a) Descriptive



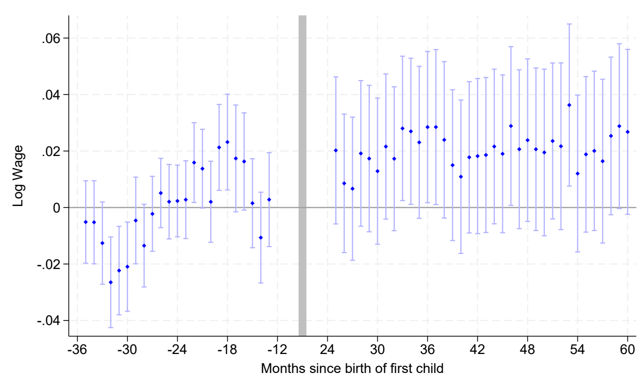
(b) Triple Differences (DDD)



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



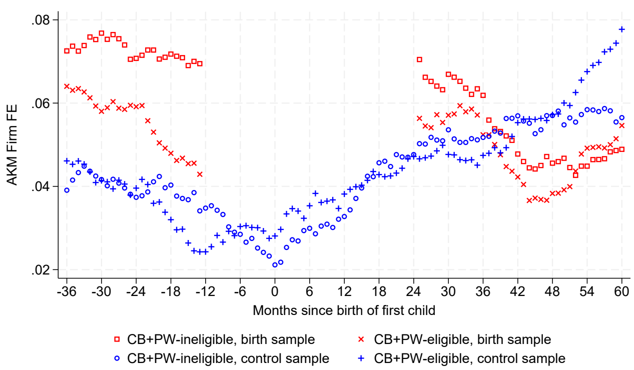
(d) Difference-in-differences (DD); control sample



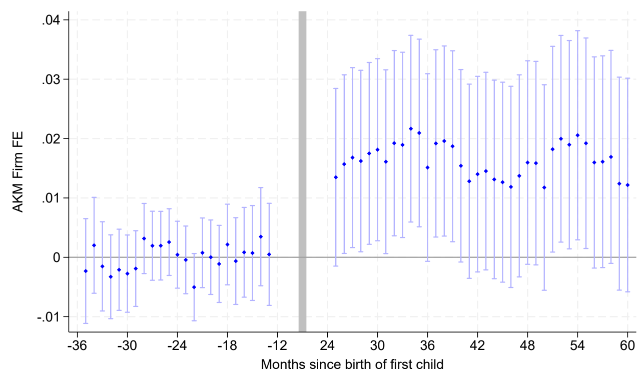
Notes: Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control sample (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Section 4.2 and 5.2.

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

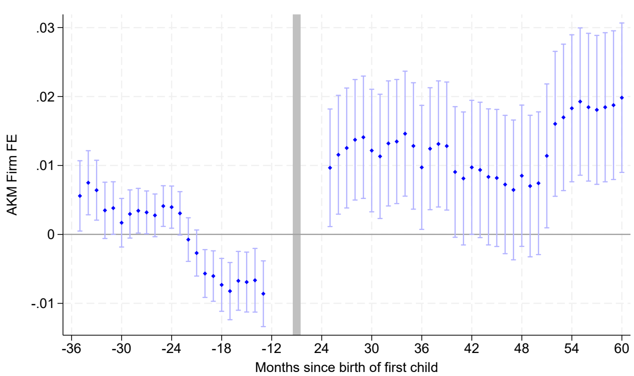
(a) Descriptive



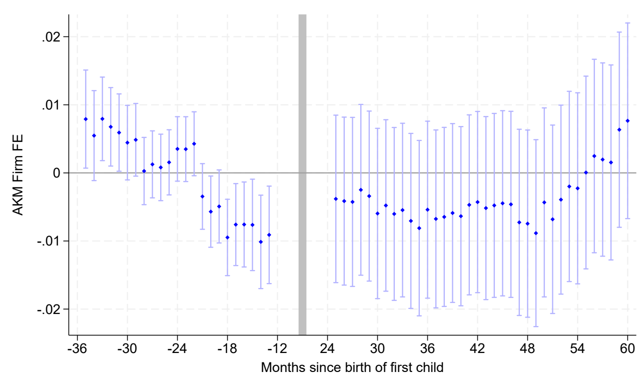
(b) Triple Differences (DDD)



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



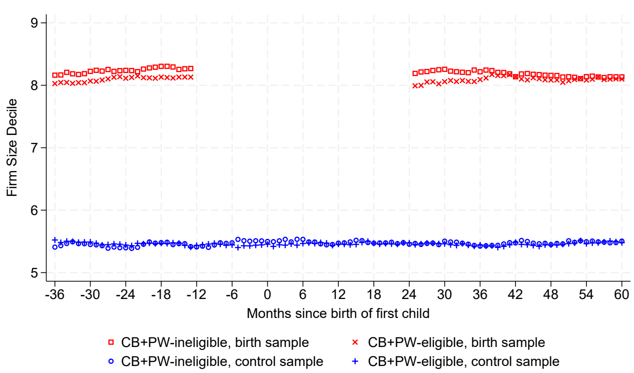
(d) Difference-in-differences (DD); control sample



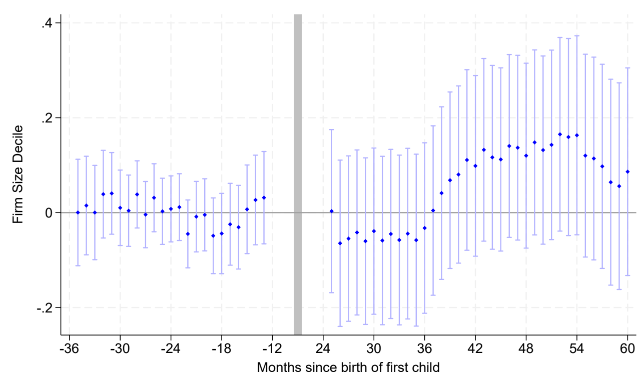
Notes: Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta_k}$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control sample (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Section 4.2 and 5.2.

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

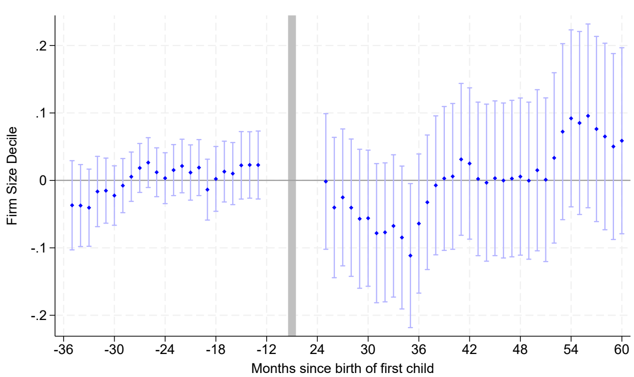
(a) Descriptive



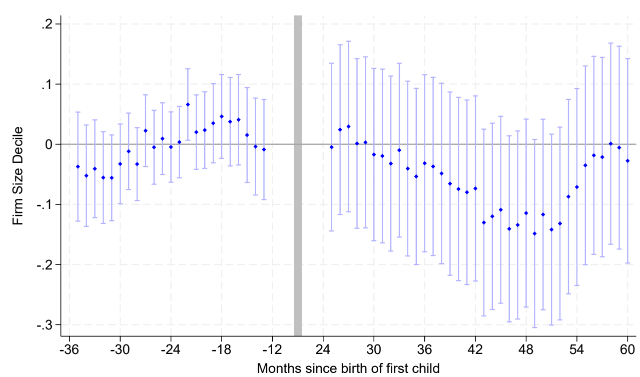
(b) Triple Differences (DDD)



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



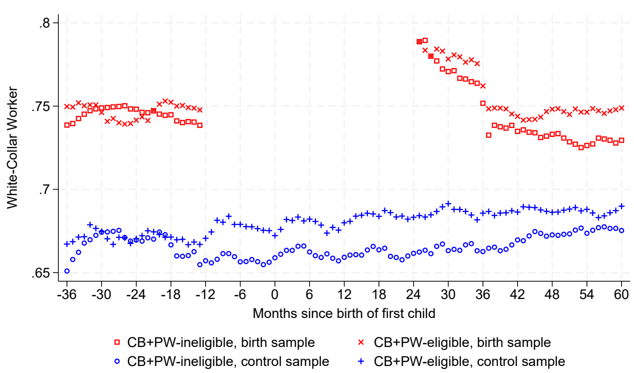
(d) Difference-in-differences (DD); control sample



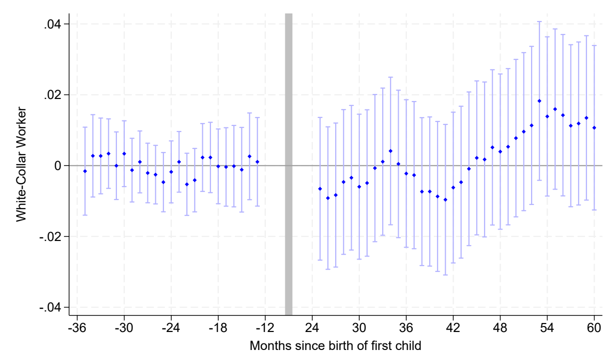
Notes: Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta_k}$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control sample (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Section 4.2 and 5.2.

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

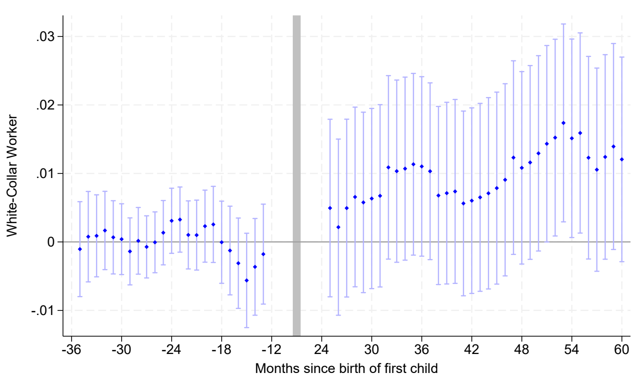
(a) Descriptive



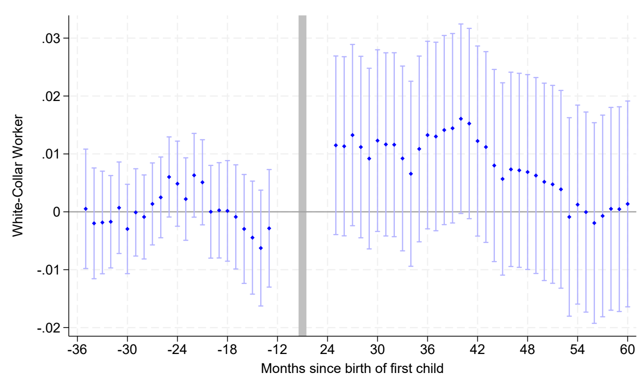
(b) Triple Differences (DDD)



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



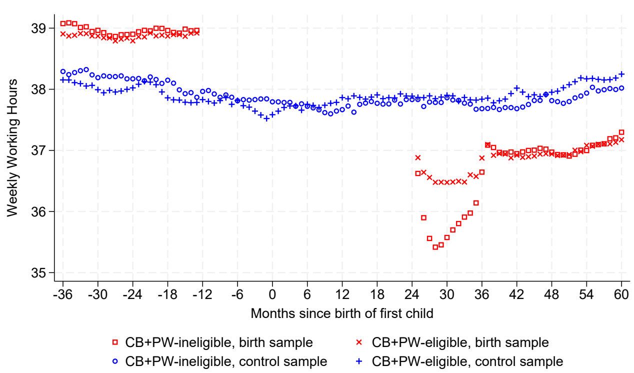
(d) Difference-in-differences (DD); control sample



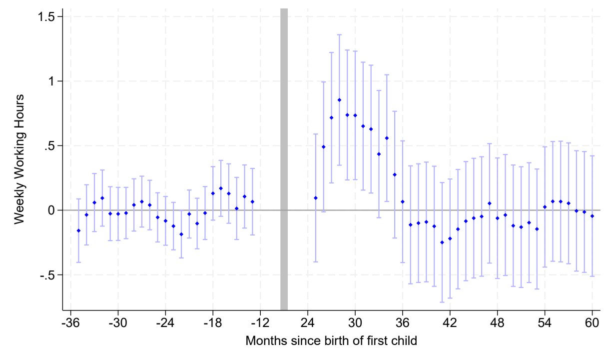
Notes: Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta_k}$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control sample (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Section 4.2 and 5.2.

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

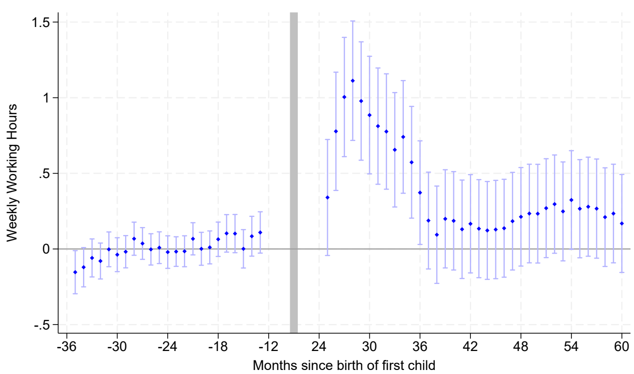
(a) Descriptive



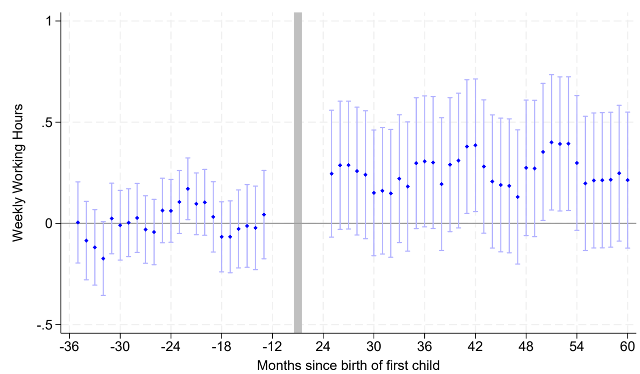
(b) Triple Differences (DDD)



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



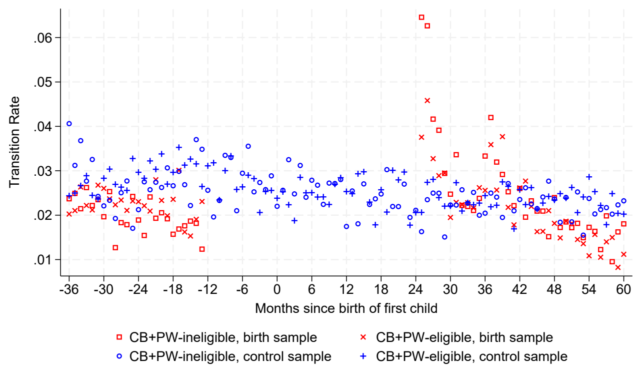
(d) Difference-in-differences (DD); control sample



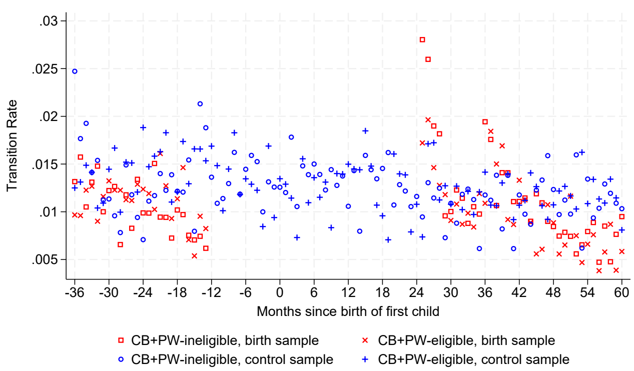
Notes: Panel (a) shows averages by groups over time. Panels (b) and (c) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta_k}$'s from model (2). In panel (b), the sample is restricted to women giving birth; in panel (c), the sample is restricted to the control sample (women never giving birth). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Section 4.2 and 5.2.

Appendix Figure A7: Transition Rate Between Employers

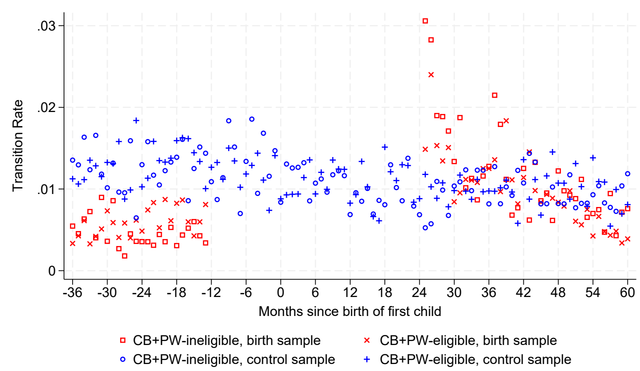
(a) Any Transition



(b) Transition to Higher AKM Firm FE Firm



(c) Transition to Lower AKM Firm FE Firm



Notes: Figure shows transition rates between employers. Transitions are defined by comparing the current and the last observed employer, conditional on the last employment being observed at most 48 months before. Sample is the baseline sample with employment restrictions as described in Sections 4.2 and 5.2.