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

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Abstract

Does earlier return to work after childbirth affect mothers' labor market trajectories? In this paper, we exploit a unique Hungarian policy reform that allowed mothers to keep maternity benefits even if they resumed work before their child turned two, creating a financial incentive to return earlier without reducing benefit levels. Such unique setting enables us to isolate the Substitution Effect inherent in Paid Parental Leave and to provide its first estimates. Using an Event Study design and administrative linked employer-employee data, we find a 2.8 percentage point (28 percent) rise in employment 19–24 months after childbirth, with no impact on later births or long-term employment propensities. 3–5 years after childbirth, eligible mothers tend to sort into firms with higher wage premiums, but into roles with fewer leadership responsibilities, lower time pressure or consequences of error, and less need for analytical thinking. Our results are consistent with mothers having an income target and trading off wages with job amenities, suggesting that earlier return to work shapes their careers. We find wage increases for some – mothers who give birth at an older age and live in Budapest – despite sorting into less managerial jobs, while those initially in lower-paying firms tend to move up, albeit into less managerial, but predictable jobs with a regular schedule.

Keywords: parental leave; return-to-work; substitution effect
JEL codes: J13; J18; J22

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1 Introduction

Many mothers may wonder if it is worth it for them to *return earlier to work* after giving birth and being away on parental leave: will they have better careers – higher wages, better jobs, potentially at higher-paying firms – if they return earlier? Even though existing studies are clear that career interruptions due to childbirth affect gender pay gaps (e.g., Bertrand et al., 2010; Angelov et al., 2016; Kleven et al., 2019, 2024a; Aaronson et al., 2020; Casarico and Lattanzio, 2023; Bensnes et al., 2023; Adams-Prassl et al., 2024) and that parental leave policies affect the length of career breaks (Kluve and Tamm, 2013; Canaan, 2022; Bergemann and Riphahn, 2023; Kleven et al., 2024b),[1] they give little guidance on whether the *length* and the *timing* of career breaks affect mothers' careers. Parental leaves shape mothers' careers through various channels: the contemporaneous "incapacitation" channel (Adams et al., 2025) when mothers do not work and earn at all, the labor market experience channel when mothers do not accumulate experience (and their human capital may even depreciate), and also due to mothers altering their work tendencies to balance family and career, thereby potentially sorting into particular roles and firms. However, whether *earlier return to work* affects these channels is an outstanding empirical question.

In many countries, mothers receive benefits while on parental leave as a replacement for foregone earnings. Then, paid parental leave disincentivizes work through both the Substitution Effect (SE) and the Income Effect (IE): the former arises due to the fact that working would mean sacrificing the benefits, while the latter captures that given the parental leave benefits, beneficiaries may prefer leisure to labor, even if working is possible while receiving the benefits (Autor and Duggan, 2007). The SE is distortionary and causes deadweight loss, because it distorts the relative price of non-work (Autor and Duggan, 2007; Chetty, 2008); then, whether mothers' careers is affected through the SE channel is the primary policy-relevant question. However, it is challenging to disentangle the SE from the IE, as policy changes typically affect just the duration or the generosity of the benefits (thereby the IE channel), without touching the condition that mothers need to sacrifice the benefits if they work (the SE channel), or they simultaneously affect both.

In this paper, we leverage a unique Hungarian reform implemented in 2014, which affected only the possibility of working – abolishing the return-to-work restriction – during the receipt of parental leave benefits before the child turns two, while keeping the overall benefit amounts and duration unchanged. Building on rich linked employer-employee data, we compare the careers of women eligible and ineligible for the return-to-work policy, with women without births as the comparison group, in an Event Study research design. We find that the reform increased the

[1] The main theme from this literature, as summarized by Olivetti et al. (2024), is that men and women typically face different constraints on the labor market and different trade-offs between family and career, with most of the gender pay gap being due to differential job sorting and the structure of work (Goldin, 2014; Cortés and Pan, 2019). Differences in preferences and psychological traits seem to play a minor role (Hyde, 2014; Blau and Kahn, 2017) and maternal biology and productivity differences are unlikely to drive persistent gaps (Azmat and Ferrer, 2017; Kleven et al., 2021; Andresen and Nix, 2022), except that women may be less productive when working from home due to more frequent childcare interruptions (Adams et al., 2025).

employment rate by 2.8 percentage points (28 percent), but only in the narrow window of 19–24 months after childbirth. Our point estimate is driven by mothers who give birth to their first child at an older age, and by those who are high-earners before giving birth. We estimate the share of compliers – those who returned to work early, before the child's 2^{nd} birthday, only because of the financial incentive and would not have returned in the absence of it – to be around 7 percent. The compliers are typically white-collar workers who give birth at an older age, are high-earners before giving birth, but who work at firms with lower ability to pay higher wages pre-birth.

Building on the policy effects on earlier return to work before the child turns two, we then ask whether earlier return to work affects mothers' labor market trajectories 3–5 years after childbirth, with a focus on not only wages, but also sorting into specific firms and specific roles (occupations).

The existing literature is inconclusive about whether the length of career interruption itself affects women's wages and earnings in the long run. On the one hand, 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.[2] For instance, Rossin-Slater et al. (2013) and Baum and Ruhm (2016) analyze the introduction of a paid leave program in California and find that it did not 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 earnings in the long run. Gregg et al. (2007) and Baker and Milligan (2008) find some increase in labor market attachment, when analyzing the introduction of short parental leave schemes in the United Kingdom and in Canada, respectively. Schönberg and Ludsteck (2014) analyze a series of expansions of maternity leave duration in Germany and find that each led to a delay in the return to work of mothers, however, there is no 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. (2024b) arrive at similar conclusions from a series of reforms on parental leave duration in Austria, and Corekcioglu et al. (2024) find that longer parental leave did not increase mothers' earnings in Norway.[3]

On the other hand, other studies find effects of the length of career interruption on earnings, suggesting that work experience soon after birth is rewarded with steeper returns in the longer run. Kuka and Shenhav (2024) show US evidence that those who had stronger work incentives

[2]Earlier cross-country studies on parental leave reforms found positive employment effects of shorter parental leave duration, but negative effects of longer parental leave duration (Olivetti and Petrongolo, 2017; Canaan, 2022).

[3]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 decrease the labor supply of mothers on both margins, but only in the short-run. Looking at the effect on health outcomes, while Bütikofer et al. (2021) show that the introduction of paid maternity leave in Norway had positive effects on maternal health, Chuard (2023) and Ahammer et al. (2023) show that longer parental leave in Austria led to worse mental health.

immediately after childbirth, rather than years later, accumulate more years of experience and higher earnings years later. In the same vein, Lequien (2012) find that a French reform inducing mothers to work less within the first 3 years after childbirth led to long-term wage decreases for 10 years, and Ejrnæs and Kunze (2013) find similar effects in Germany (and neither of them analyze other outcomes). Timpe (2024) finds that expansion to paid maternity leave in the '60s-'70s in the US led to lower hourly wages, employment, and family income.

One potential reason for the inconclusive findings on whether the length of career interruptions affects 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 our setting, we not only find a large effect on mothers' propensity to work exactly in the period when they had 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 mothers' propensity to have subsequent children and their timing. Thus, the 2014 Hungarian 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.

Our first contribution to the literature is to provide clean estimates on the *timing* of mothers' return-to-work on mothers' labor market trajectories – wages, the firm-specific wage premium, and job characteristics – 3–5 years after childbirth, when there are no employment effects anymore, and no contaminating effect of changing workforce composition. And while there is extensive evidence in the literature on mothers' occupational sorting after return-to-work post-childbirth,[4] and several field and lab experiments support women's –and particularly mothers' – different preferences for specific job amenities and reservation wages,[5] we present novel evidence on how *earlier* return-to-work affects such occupational sorting years after childbirth. Similarly, while some studies do analyze mothers' sorting across firms – such as Casarico and Lattanzio (2023) and Gallen (2023), who document a reallocation of women into lower-productivity firms post-childbirth – we present

[4]For instance, Kleven et al. (2019) find that young mothers are less likely to hold managerial roles, and are more likely to move to the public sector and to firms led by female managers who are themselves parents, thereby trading off wages for desirable job amenities (such as family-friendly firm policies); consistent with this, Tannenbaum and Timpe (2025) document that mothers are less likely than fathers to switch to leadership-intensive occupations after childbirth. Goldin (2014) shows that occupations with the greatest remuneration for long and particular hours have the largest gender pay gap, leading to substantial gender pay gaps in legal, corporate, and financial occupations (Wood et al., 1993; Bertrand et al., 2010; Azmat and Ferrer, 2017). Consistently with this, Wasserman (2022) finds that women medical residents are more likely to enter specialties with capped working hours, and Cortés and Pan (2019) find that low-skilled immigration inflows – with whom women can substitute their time out in home production – induce young women to enter occupations with higher returns to overwork.

[5]For instance, Wiswall and Zafar (2017) find that women express larger Willingness-to-Pay (WTP) for flexibility in working hours than men, Mas and Pallais (2017) find that women, especially those with young children, express a higher WTP for flexible scheduling and working from home than men, and Le Barbanchon et al. (2020) and Cortés et al. (2023) estimate lower reservation wage for women (some part of it being related to children). Philippe and Skandalis (2024) document that jobless mothers are more selective on wage and non-wage amenities. Consistently with the importance of non-wage amenities, Felfe (2012) finds that when young mothers switch employers, they tend to adjust different aspects of their work schedule (hours, irregular shifts, or flexible schedule) and the level of stress. Hotz et al. (2017) estimate that mothers value workplaces with higher shares of female co-workers.

novel evidence on how *earlier* return-to-work affects such sorting across firms years after childbirth.

We find positive effects, on average, on mothers' wage for some groups (for those who gave birth at an older age and who live in the capital), and a clear positive effect on their firms' average firm-specific wage premium 3–5 years after giving birth; 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. For instance, mothers' estimated firm-specific wage premium is significantly higher by 1.6 percent in the 3^{rd} to 5^{th} year after giving birth, indicating that mothers eligible for the policy sorted into higher-paying, and presumably better, firms 3–5 years after giving birth. Or, 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 firm-specific wage premium is also higher on average by 3 percent. We also find that as a consequence of the reform, already in the 3^{rd} year after childbirth, mothers have sorted into occupations with fewer leadership responsibilities, lower time pressure, and less need for analytic thinking, and the difference remains for 5 years after childbirth. Our results are consistent with mothers – at least in the first 5 years after childbirth – having an income target in mind and trading off wages with job amenities, and suggest important, previously undocumented, persistence in mothers' occupational sorting patterns when they return to work earlier.

As a falsification test, we confirm zero reduced-form estimates for likely "always-worker" mothers, i.e., for mothers who would have returned to work earlier even in the absence of the reform. 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 for the policy.

Our second contribution is to identify to what extent mothers' careers is affected through the Substitution Effect (SE) of paid parental leave alone. While the distinction of the SE and the IE 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, identifying the SE is key and policy-relevant, because it shows to what extent paid parental leave distorts the incentives to work and for whom. 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, this 'implicit tax' of working. Existing studies exploit paid parental leave duration cuts or extensions (e.g., Schönberg and Ludsteck, 2014; Lalive et al., 2014; Dahl et al., 2016; Kleven et al., 2024b), thus they measure together the IE and SE of parental leave on the labor market participation of mothers. Compared to them, we also look at further outcomes: wages, firm-specific wage premium, and occupation characteristics.

In what follows, Section 2 describes the Hungarian parental leave system and analyzes the "Childcare Benefit Plus Wage Reform". Section 3 describes the Hungarian linked employer-employee data that is unique in that it contains monthly wages, work hours, fine occupation codes, and the exact month of birth. Then, Section 4 describes our Event Study research design, Section 5 shows the results, and Section 6 provides concluding remarks and a discussion.

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 collectively 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 after birth, 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 after birth.

The baby-care allowance and the childcare benefit are social insurance benefits, based on prior employment and earnings. Parents can receive these benefits by accumulating at least 365 days of social insurance coverage during the 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, the 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

Note: The new regulations from 2014 and 2016 applied also to children born before 2014 and 2016, respectively.

Table 1 shows the regulations on paid work during parental leave for mothers who are eligible for baby-care allowance and childcare benefit. Before the *CB+W* ("Childcare Benefit Plus Wage") reform in 2014, mothers had to forgo the childcare benefit if they returned to work before the 2^{nd} birthday of their youngest child. Similarly, mothers could only keep the childcare allowance if they did not work more than 30 hours per week. These work restrictions and the parental leave benefits disincentivized mothers to return to work before the 2^{nd} birthday of their youngest child, and resulted in sudden jumps in the share of mothers working at 24 and 36 months of their children's age (when mothers stop receiving the childcare benefit and allowance, respectively).[6]

Mothers enjoy strong job and wage protection by the Hungarian Labor Code (Act I of 2012), and the regulation of mothers' job protection did not change during our study period. Starting from the 12^{th} week of pregnancy, women can announce officially their pregnancy to their employers, after which employers cannot lay them off. Mothers are entitled to unpaid and fully protected leave between the birth and 3^{rd} birthday of their child, during which they accumulate vacation days. Mothers can return to work at any time they prefer, and within 30 days, employers have to offer them a similar position they held before giving birth, even if they previously hired a replacement. Compared to their pre-birth wage, mothers are entitled to a similar wage raise that other employees in similar roles received during their leave. Upon request, employers also need to accommodate mothers who want 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. Employers can only terminate returning mothers' contracts if mothers' pre-birth role is ceased because of a reorganization, in which case no employee works at their previous occupation. However, in this situation, mothers could decide to stay on paid parental leave until the 3^{rd} birthday of their youngest 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 held 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 meal costs for their children (around 10% of the 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, 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

[6] Act LXXXIV of 1998 established restrictions on doing paid work while receiving maternity benefits.

goal of the reform was to increase the number of births and help mothers return 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,[7] and this change intended to motivate families to decrease the gap between childbirths by terminating the previous 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. Its details were first announced 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. The changes introduced by the act took effect on January 1, 2014, less than a month after the vote.

The policy change was unexpected for families due to the short time window between the introduction and the implementation of the reform. 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. 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.

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

[7] We can illustrate the policy change through an example of a mother who receives childcare benefits because of her firstborn. If this mother gives birth to a second child when her firstborn is 18 months old, then both before and after the reform, she would have received baby-care allowance for her second child. However, before the reform, she would have lost the childcare benefit for her first child because the higher baby-care allowance for the second child had crowded out the childcare benefit for the first child. As the reform abolished this crowd-out effect, after the reform this mother would have received both the baby-care allowance and the childcare benefit.

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. We did not find evidence that the reform affected mothers' future fertility (in Section A.1), which suggests that Pillar II's effect on the timing of future births was moderate.

In this paper, we focus on Pillar I of the Act CCXXIV of 2013, referred to as *CB+W* ("Childcare Benefit Plus Wage"), which abolished work restrictions after a child's first birthday. Hungary is one of the few OECD countries (along with Czechia, Latvia, and Slovakia) which allows parents to keep all parental leave benefits in case of returning to work (Bičáková and Kalíšková, 2022; OECD, 2024).[8] In Hungary, the *CB+W* reform increases the pre-birth income by as much as 70%.

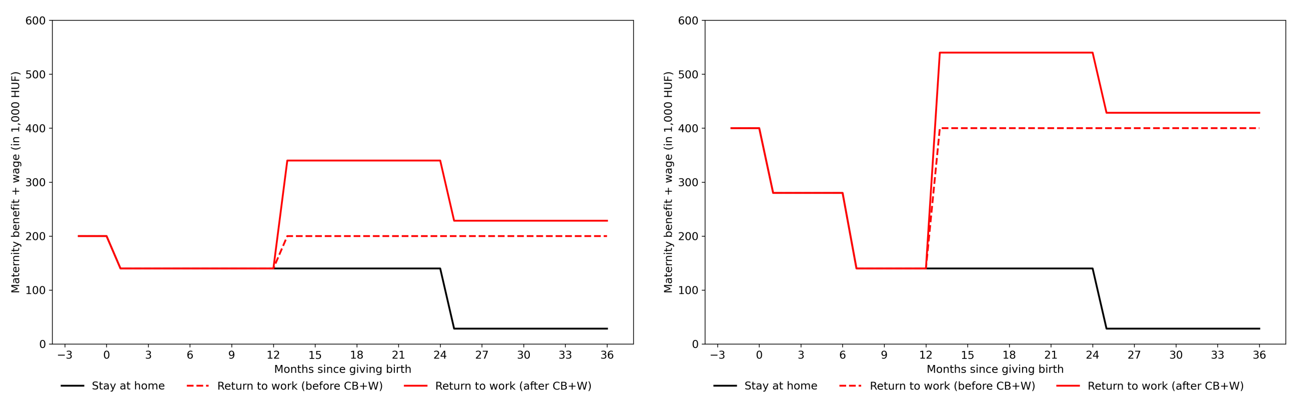
2.3 Financial Incentives of the *CB+W* Reform: A Numerical Example

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

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: This figure illustrates how the reform affected the decision of a mother who is eligible for childcare benefit and needs to decide whether to stay at home or to return to work at the first birthday of her youngest child. In panel (a) the woman's pre-birth income is twice the minimum wage, her replacement rate is 70%, and we assume she either works full-time or stays home. 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%. Here, we also assume that if she works she works full-time. The minimum wage is set to 100,000 Hungarian forints (HUF) – the exact value of the monthly minimum wage was 98,000 HUF in 2013 and 101,500 HUF in 2014.

Figure 1 depicts how the reform affected the income of a hypothetical mother who is eligible for childcare benefit and must decide whether to stay at home or return to work on the first birthday of her youngest child. In panel (a) the pre-birth salary is assumed to be twice the minimum wage, which is exactly the cutoff of the capped childcare benefit, the childcare benefit is 70% of

[8] Estonia caps benefits if wage income exceeds 50% of the parental leave benefits. 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 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.

the pre-birth wage, and we assume that the mother works full-time when she returns to work. Meanwhile, in panel (b) of Figure 1, the 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%. Again, we assume the mother works full-time after she returns from parental leave.

Before the reform, higher-income mothers had stronger financial incentives to return earlier to work than lower-income mothers. Lower-income mothers gained less additional income by an earlier return, due to their higher replacement rate. In our 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 full-time (such as childcare, transportation, etc.). In contrast, because of her lower replacement rate, for the higher-income mother returning to work would increase income by 65% of her pre-birth wage. Hence, relative to their pre-birth income, before the reform, 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, and 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 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 strong incentives to return to work before the child turn two, given mothers could keep the childcare benefit while working. Given 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 with a higher replacement rate.

Paid parental leave decreases mothers' labor supply through two channels, that are rarely discussed in the context of parental leave: through the Substitution Effect (SE) and the Income Effect (IE). In Appendix B we discuss how our unique policy setting helps us identify the SE.

3 Data and Measurement

3.1 Data Source

Our main dataset is an employer-employee linked panel dataset, covering 50 percent of the Hungarian population with a social security number in 2003; besides information on labor outcomes, it also contains administrative data on medical records and drug expenditures, for years 2009–2017.[9]

The employment-related data contains the anonymized identity of the employer, type of employment, wage, occupation, and work hours (provided by the *Hungarian Central Administration of National Pension Insurance*). It contains all sources of income liable for paying social security contributions 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.

Healthcare use data is available from the *Hungarian National Health Insurance Fund Administration* starting from 2009. Births are identified with International Classification of Diseases (ICD-10) codes O80-O84.

For each individual, we observe gender, age, the amount of unemployment-, child-related, and pension benefits, and also the number of days in hospital by month, broken down by the cause of hospitalization, including births. Upon employment in a given month, we observe monthly wage income, monthly hours worked, occupation, industry, and firm identifier.

Regarding firms, we observe financial data, employment, industry, and foreign ownership from the *Hungarian National Tax and Customs Administration*, for all double-entry bookkeeping firms, reported by the firms annually (in their balance sheets and profit and loss statements).

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

3.2 Samples: Birth Sample and Control Sample

We construct our *birth sample* the following way:

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

2. To ensure that we keep women who give birth for the 1^{st} time, we keep mothers in the sample who did not receive any parental benefits before 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:

    (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;

[9]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 ELTE Centre for Economic and Regional Studies in Hungary.

(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 eligible and ineligible cohorts are affected by similar aggregate shocks (i.e., the time of their childbirth is close), and give birth in the same season (therefore, we have a six-month gap between the cohorts). ($N = 14,626$)

5. We keep mothers who were eligible for parental leave benefit, i.e., who worked for at least 12 months in the two years preceding birth. Due to measurement errors in the employment indicator during pregnancy, we restrict the sample to 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 so 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 07/2013–12/2013 and 07/2012–12/2012, i.e., at the intervals of the first births of women in the *birth sample*. ($N = 10,138$)

3.3 Definitions of the Outcome Variables

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 essentially substitutes for unemployment in Hungary), and months when a woman is on maternity leave for at least 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 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 $\ln(w_{hour})$ to missing for the non-employed (as per our definition) in a given month.

We need to account for one data issue with the wage variables: 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 (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 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). For its estimation, 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 logarithm of 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 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 firm-specific wage premium and firms' abilities to pay higher wages, 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),[10] 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. 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 four variables, capturing to what extent: (1) the job involves leadership roles (it requires leadership, stress tolerance, responsibility for outcomes, establishing and maintaining personally challenging goals and exerting effort toward mastering tasks, persistence in the face of obstacles, and a willingness to take on responsibilities); (2) the job involves time pressure and consequences of error; (3) the job requires analytical thinking; and (4) the job's schedule is regular.

4 Empirical Strategy

4.1 Effect of the Reform on Employment and Fertility

To build intuition for our preferred specification, we begin with a difference-in-differences model that compares the outcome trajectories of *CB+W*-eligible and *CB+W*-ineligible women over time.

We estimate model (2) separately on the sample of mothers and of women never giving birth:

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

where Ω_{it} is the outcome variable for individual i at monthly date t, e_{it} denotes the months relative to the event (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 $\sum_{k=-36}^{-13} \tilde{\beta_k} = 0$.

[10] 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.

In model (2), the estimated effect of $CB{+}W$-eligibility may be confounded with time trends. Thus, 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 who never gave birth as control group. The Event Study regression model is the following:

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

where 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 working over 19–24 months after childbirth:

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

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

In the model for fertility (equation (5)), we do not include the women who do not give birth, and replace the individual FEs with a vector of control variables[11] 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), and county of living 12 months before childbirth.

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

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 coefficient of interest is θ_k, $\theta_6 = 0$.

4.2 Effect of the Reform on Job Quality

To investigate the effect of $CB{+}W$-eligibility on job quality, we focus only on the 3^{rd}, 4^{th}, and 5^{th} years after giving birth, in years in which 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

[11] 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.

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

We estimate equation (3) with job quality outcomes. 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. Thus, 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 afterY3-Y5$_{it}$, to estimate the average effect across 3–5 years after childbirth.

4.3 Robustness: Event Study Research Design on Matched Sample

An imbalance exists in the pre-birth (or pre-pseudo birth) employment rate of women ever giving birth ($B_i = 1$) and with no birth ($B_i = 0$) (Panel (a) of Figure 2). To verify that our estimates are not affected by this baseline (level) imbalance, we apply a matching strategy.

We create a sample that includes all women of the *birth sample* ($B_i = 1$), and create cells based on 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 the number of women who never gave birth is at least the number of 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 *matched sample* avoids the over-representation of $B_i = 0$ with different labor market history than the $B_i = 1$ women.

5 Results

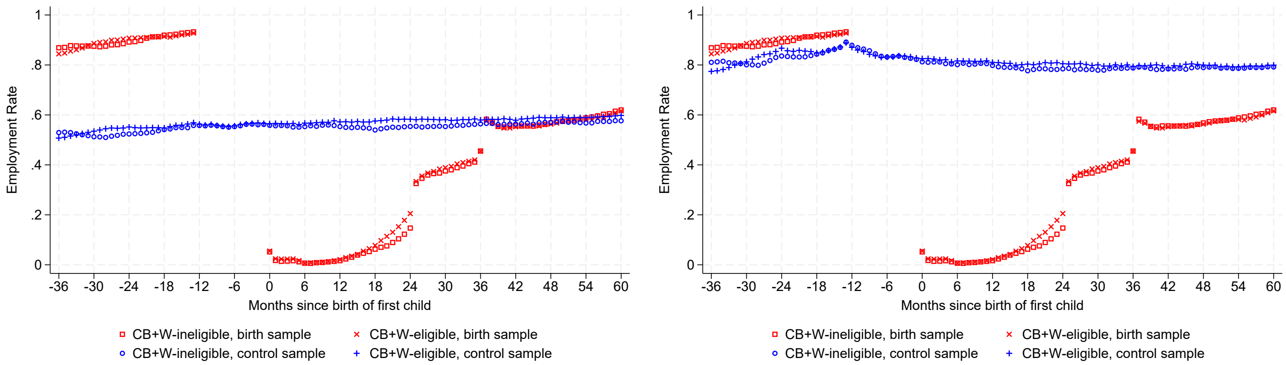
5.1 Outcome: Employment

In this section, we first present descriptives and regression estimates for the outcome of being employed. Next, we show the composition of the mothers' workforce at different times, relative to the birth of their first child. Finally, we analyze the characteristics of compliers. In Appendix Section A.1, we show that reform did not change families' fertility decisions and subsequent births.

Descriptives and Regression Estimates. Panels (a)–(b) of Figure 2 show the evolution of the employment rate for the 4 groups in 2 dimensions (eligible/ineligible women; giving birth/not giving birth) in the baseline and matched samples, respectively. Women who do not give birth are in the control sample, for whom the pseudo time of birth – determining event time – is randomly drawn so that they can also be classified as eligible vs. ineligible. 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 were likely affected by the same seasonal factors.

Figure 2: Employment Rate Over Event Time

(a) Baseline Sample (b) Matched Sample



Notes: Sample is as described in Section 3.2. 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 2 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-month marks in their children's age. The gap between eligible and ineligible mothers widens exactly between 18 and 24 months of their children's age: eligible mothers' employment rate is higher exactly in the second half of the child's 2^{nd} year of age, when return to work was strongly incentivized by the policy.

Figure 3 shows the Event Study coefficient estimates for the birth sample and the control sample separately, 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 women giving birth, and in panel (b), the sample is the control sample (women never giving birth).[12] Panel (c) shows the estimated δ_k coefficients from equation (3) on the baseline sample, and panel (d) shows the same on the matched sample.

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 with the control group – women without giving birth – we difference out the pre-trends that can be seen in panel (a).[13]

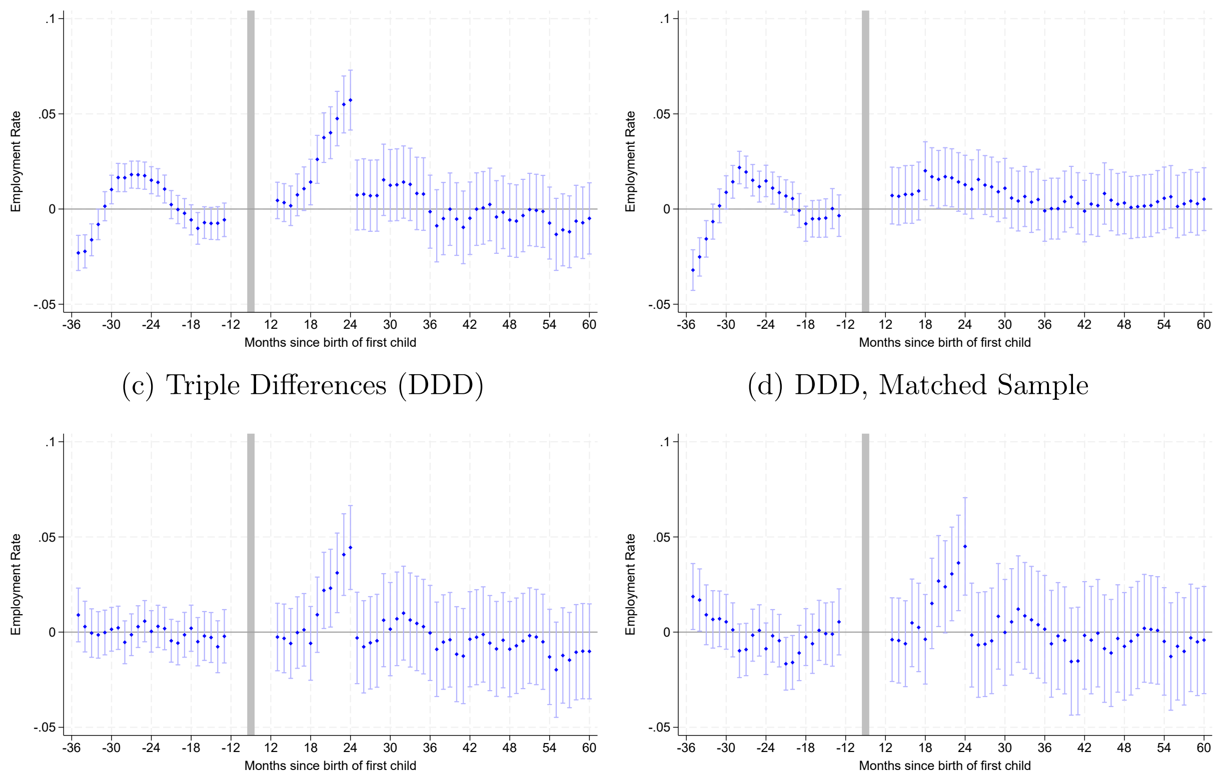
[12]The pre-trend in the second and third year before birth is due to aggregate labor market trends. For example, the *CB+W*-ineligible (eligible) group is observed in 2008 (2009) at event time -36 and in 2009 (2010) at event time -24, between which periods the employment rate decreased (levelled off)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

[13]Figures A11 and A12 show that the increased employment between months 20–24 is due to the rising return to

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

(a) Difference-in-Differences (DiD); Birth Sample (b) DiD; Control Sample

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



Notes: Sample is as described in Section 3.2. This figure shows the Event Study coefficient estimates for the birth sample and the control sample separately (in panels (a) and (b), respectively), so that their difference 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.

Table 2 presents heterogeneities in the policy effect on employment by pre-birth characteristics. The top row indicates that, on average, the $CB{+}W$ policy increased the employment rate by 2.8 percentage points (3.0 percentage points in the matched sample) 19–24 months after childbirth.

We find that the point estimates for the employment effects are higher for women who have their first child at age 31–40, and, in terms of the corresponding *p-values*, this is the strongest heterogeneity we find in the employment effects. We also see bigger employment effects for mothers whose baseline wage was above the median before childbirth, but also for mothers whose employer's AKM firm effect was below the median (the baseline wage and AKM firm effect refer to 13–24 months prior childbirth). 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

full-time jobs, which require at least 35 hours of weekly work. Besides this earlier return to full-time work, we also find that the $CB{+}W$ policy likely had a positive effect on full-time employment during the 25–36 months period after childbirth. The $CB{+}W$ policy and the abolition of working hours restrictions on employment in the 25–36 months following childbirth motivated 3 to 5 percent of mothers to switch from part-time to full-time employment.

return to work, than on higher-income mothers. We also analyze the heterogeneity of the employment effects by location, specifically, by living in the capital city (Budapest) or not – the rationale for looking at this dimension is that on average, the employment rate and earnings are higher in the capital, and there are more nursery places.[14] The employment effect of the *CB+W* policy is slightly stronger in the capital, but not significantly different from the effect in the rural areas.

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

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

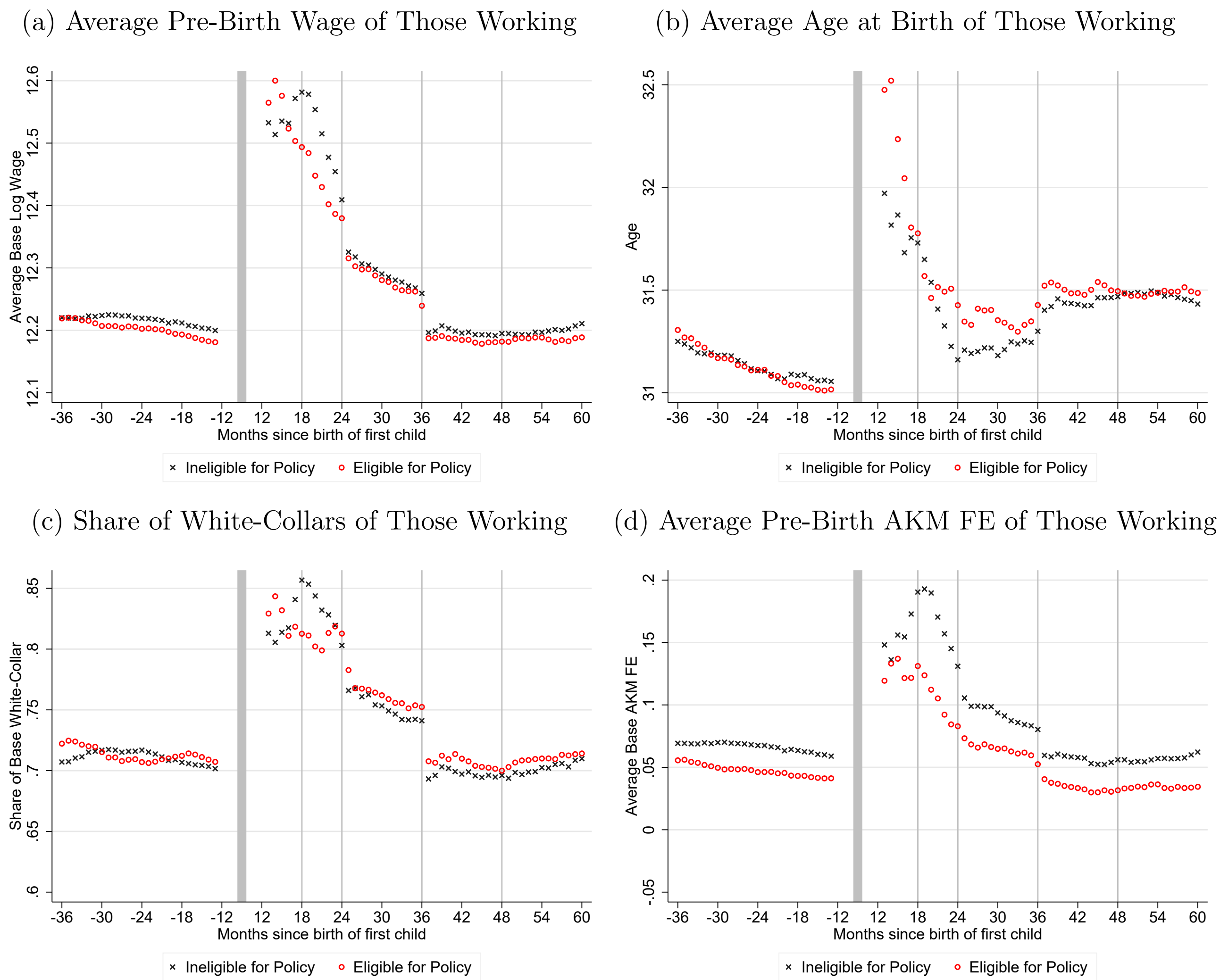
Notes: Table shows average effect of the *CB+W* policy on employment probability 19–24 months after childbirth, using the 3^{rd} and 2^{nd} years before the event as reference period (equation (4)). In the models with heterogeneity terms, all regressors are interacted with the heterogeneity term. Occupation 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.

[14] According to the T-STAR regional statistics of the Hungarian Central Statistical Office, in 2013, the per capita annual income was 1,799 thousand HUF in rural areas while 2,627 thousand HUF in Budapest; the unemployment rate was 7.6% in rural areas and 3.7% in Budapest; and the number of available nursery places relative to the population aged 0–2 years old was 0.20 in rural areas and 0.25 in Budapest.

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

Composition. Figure 4 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 in observable pre-birth characteristics before the birth of their first child of working mothers who are in the labor force 36-to-12 months before the birth in the eligible and ineligible cohorts.

Figure 4: Composition of Mothers' Workforce Before and After Birth



(a) Average Pre-Birth Wage of Those Working

(b) Average Age at Birth of Those Working

(c) Share of White-Collars of Those Working

(d) Average Pre-Birth AKM FE of Those Working

Notes: Sample only contains women who gave birth, in the eligible and ineligible cohorts, as described in Section 3.2.

In Figure 3, 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 4 there are no compositional differences in the workforce of the two cohorts 3–5 years after the birth of the first child either. Here, we show the (a) average pre-birth wage, (b) average age, (c) share of white-collar workers, and (d) average pre-birth AKM firm FE, only for those who are in the workforce at that month before and after the event, separately for the ineligible (gray) and eligible (red) cohorts. Before giving birth and 3–5 years after giving birth, in all panels, the averages either overlap or are parallel with similar differences. In the 2^{nd} year after giving birth, the gray crosses indicate that the always-takers – those who work before the child's 2^{nd} birthday even without the policy – typically have higher wage and AKM firm FE before giving birth. 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 cohorts.

Characteristics of Compliers, Always-Takers and Never-Takers. 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 this 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 in months 19–24 had they been ineligible. The average characteristics of the compliers are then estimated using the following formula:

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

where $W_1 = 1$ denotes working 19–24 months after giving birth if being in the eligible cohort ($D = 1$), $W_0 = 0$ denotes not working 19–24 months after giving birth if being in the ineligible cohort ($D = 0$), 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- and never-takers, π_A and π_N, is estimated by the empirical counterpart of $Prob(W_0 = 1)$ and $Prob(W_1 = 0)$, respectively: $\hat{\pi}_A = 0.1601$ and $\hat{\pi}_N = 0.7724$. Thus, the estimated complier share is $\hat{\pi}_C = 0.0674$, or around 6.74 percent of mothers.

Table 3: Average Characteristics of Compliers, of Always-Takers, and of Never-Takers

Average Among/In:	(1) Compliers	(2) Overall Sample	(3) p-value of Difference (1)-(2)	(4) Always-Takers	(5) Never-Takers
in Budapest	0.270	0.193	0.094	0.184	0.187
white collar	0.777	0.666	0.023	0.764	0.626
age-at-birth (years)	31.16	30.29	0.076	30.60	30.25
base wage (logs)	12.25	12.16	0.260	12.40	12.08
base AKM firm FE	-0.025	0.049	0.052	0.115	0.021

Table 3 shows the average characteristics of compliers for various X's, and whether those significantly differ from the overall sample's average characteristics. Consistent with results in Table 2, columns (1)-(3) indicate that complier mothers are more likely to work in white-collar occupations; and, albeit the differences are not all significantly different from zero at the 5 percent level, complier mothers are more likely to live in the capital city (Budapest), are older at the time of the birth, and their pre-birth firm has, on average, lower estimated AKM firm effect reflecting their ability to pay higher wages. Note that complier women have a higher pre-birth wage, on average, than the overall sample, but the difference is not significantly different from zero.

Column (4) indicates that always-takers are more likely to work in white-collar occupations, are younger at the time of the birth, and have higher pre-birth wage and pre-birth AKM firm FE (where all differences are significant at the 5 percent level). Finally, column (5) indicates that never-takers are less likely to work in white-collar occupations and have lower pre-birth wage and pre-birth AKM firm FE (where all differences are significant at the 5 percent level).

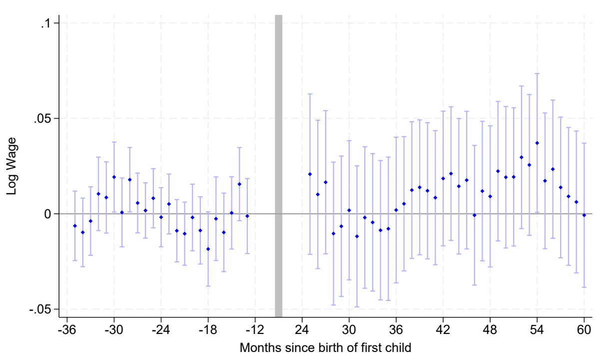
In sum, compliers are typically white-collars, more likely to live in Budapest, who gave birth at age 31-40 and were working at lower-paying firms prior to birth (with a slightly higher wage).

5.2 Outcomes: Job Characteristics

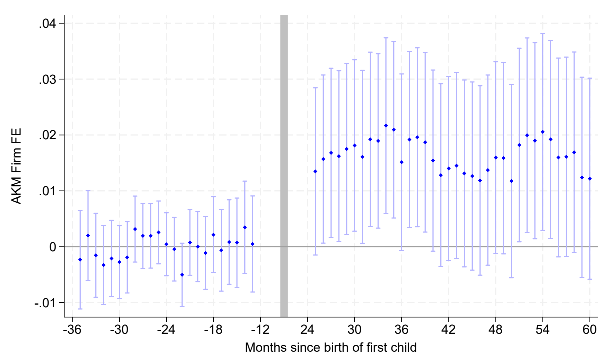
Now we turn to the analysis of job characteristics 3–5 years after giving birth, when there are no differences between the two cohorts of mothers – eligible and ineligible for the analyzed policy – in their employment and fertility behaviors, except that mothers in the eligible cohorts were 2.8 percentage points more likely to work 19–24 months after giving birth.

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

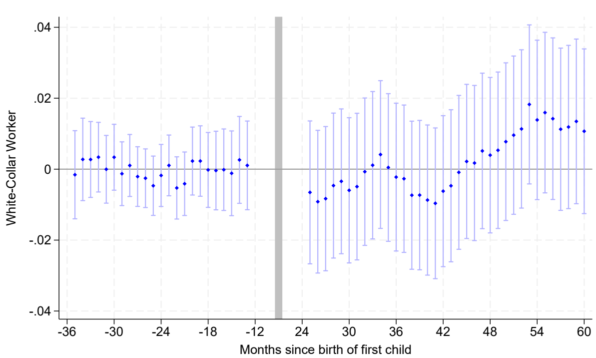
(a) Log Hourly Wage



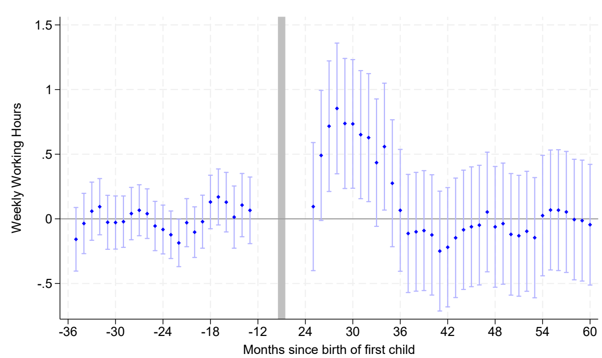
(b) AKM Firm FE



(c) Employment in White-Collar Job



(d) 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. The sample is the baseline sample with employment restrictions as described in Sections 3.2 and 4.2.

Wage, AKM Firm Effect, White-Collar Occupation, Work Hours. We show the $\hat{\delta}$ estimates from equation (3) in Figure 5 on these outcomes.[15,16] All estimates are conditional on working. Reassuringly, there are no significant pre-trends for any of the outcomes.

Table 4: Heterogeneity in the Effect of the *CB+W* Policy on Job Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Std. O*NET Score			
	Log. of Hourly Wage	AKM Firm FE	White-Collar	Work Hours	Leadersh.	Pressure	Analytical	Reg.Sched.
All	0.012	0.016**	0.004	0.135	-0.057**	-0.048**	-0.046	-0.002
	[0.014]	[0.008]	[0.010]	[0.201]	[0.023]	[0.024]	[0.029]	[0.032]
By Age at Childbirth								
Age 25-30	-0.019	0.002	0.014	0.369	-0.053	-0.013	-0.092**	-0.007
	[0.020]	[0.012]	[0.015]	[0.292]	[0.033]	[0.034]	[0.042]	[0.047]
Age 31+	0.043**	0.030***	-0.005	-0.018	-0.059*	-0.084**	0.002	0.010
	[0.020]	[0.010]	[0.014]	[0.273]	[0.032]	[0.034]	[0.040]	[0.044]
By Location								
Rural	-0.002	0.007	0.009	0.220	-0.047*	-0.043	-0.053	-0.004
	[0.016]	[0.008]	[0.012]	[0.218]	[0.026]	[0.027]	[0.032]	[0.036]
Capital	0.071**	0.048**	-0.017	-0.095	-0.100**	-0.073	-0.014	-0.002
	[0.034]	[0.020]	[0.020]	[0.493]	[0.051]	[0.051]	[0.065]	[0.072]
By Baseline Occupation								
Blue-Collar	-0.012	0.007	0.013	0.178	-0.058**	-0.050*	-0.052	0.007
	[0.024]	[0.013]	[0.023]	[0.446]	[0.027]	[0.026]	[0.034]	[0.038]
White-Collar	0.025	0.018*	0.000	0.167	-0.039	-0.025	-0.049	-0.045
	[0.017]	[0.010]	[0.010]	[0.215]	[0.043]	[0.051]	[0.055]	[0.059]
By Baseline Wage								
Below Median	-0.000	0.011	-0.020	-0.172	-0.095**	-0.065	-0.058	0.019
	[0.022]	[0.013]	[0.019]	[0.408]	[0.038]	[0.043]	[0.047]	[0.052]
Above Median	0.020	0.017*	0.017	0.280	-0.030	-0.037	-0.033	-0.019
	[0.018]	[0.009]	[0.011]	[0.206]	[0.029]	[0.028]	[0.037]	[0.041]
By Baseline AKM Firm FE								
Below Median	-0.017	0.017	-0.014	-0.267	-0.062*	-0.057	-0.065	0.091*
	[0.021]	[0.012]	[0.016]	[0.348]	[0.034]	[0.038]	[0.043]	[0.048]
Above Median	0.026	0.013	0.015	0.429*	-0.053*	-0.038	-0.028	-0.072
	[0.019]	[0.010]	[0.013]	[0.222]	[0.031]	[0.031]	[0.040]	[0.045]

Notes: Table shows average effect of the *CB+W* policy on job characteristics in the 3^{rd}–5^{th} year after giving birth (for variable afterY3-5 $\times$ B $\times$ D), 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 to childbirth. Baseline wage and AKM firm FE refer to 13–24 months prior childbirth. The sample is the baseline sample with employment restrictions as described in Sections 3.2 and 4.2. 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.

Panels (a) and (c) of Figure 5 suggest a small positive, but statistically insignificant effect of *CB+W*-eligibility on hourly wage and the probability of employment in a white-collar job. Panel (b) indicates that *CB+W*-eligibility increases the AKM firm FE of the employer by up to 0.02, corresponding to a 2 percent increase in the firm-specific wage premium. Looking at working hours, panel (d) of Figure 5 shows that in the third year after childbirth, the weekly hours worked

[15] Appendix Figure A2 shows that these results are robust to estimating them on the matched sample.

[16] We present descriptive time patterns and DiD estimates from equation (2) in Figures A3–A6.

increase by up to 0.9 hours, and this effect is statistically significant. This increase is likely due to the direct impact of the $CB{+}W$ reform, which eliminated the 30-hour weekly working time restriction for the 3^{rd} year of age of the child.[17]

Next, in the first row of Table 4 in columns (1)–(4), we show the average effects of $CB{+}W$-eligibility on job characteristics in the 3^{rd} to 5^{th} year after giving birth, for all; here, each point estimate (with the accompanying Standard Error estimate) stems from a different regression.

The first column of Table 4 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 this difference is 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 and being in a white-collar occupation are not significant at the usual levels (although the point estimates are positive).

Table 4 also reveals that the wage and AKM firm wage premium effects are driven by women aged 31 and above 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 mothers outside Budapest. Similarly, the estimated AKM firm effects for those living in the capital are by 4.8 percent higher. These heterogeneities might be due to the better employment options in Budapest (see the statistics reported in footnote 14). Wages of mothers who were at least 31 years old when giving birth are 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-collar and with above-median baseline wages. There are no groups with a significant effect on working in white-collar occupations (column (3)). Column (4) of Table 4 reveals that the overall effect on hours in the 3^{rd} to 5^{th} years after giving birth is driven by those with above-median baseline AKM firm effect.

As a falsification test, in Appendix Section A.3 we confirm zero reduced-form estimates for the outcomes of hourly wages and the AKM firm effect, for likely "always-worker" mothers, i.e., for mothers who would have returned to work earlier even in the absence of the reform. 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 for the policy.

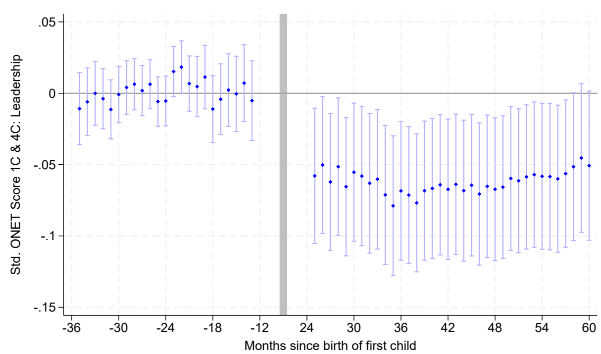
[17] The abolition of the 30-hour restriction allowed mothers in both the eligible and ineligible groups to have a full-time job and keep their childcare allowance. However, since the children of the mothers in the ineligible group were already 2 years old at the time of this change, they had less time to adjust their working hours compared to mothers in the eligible group.

Occupation Characteristics. Next, we turn to occupation characteristics as outcomes. Figure 6 shows that eligible mothers, on average, sorted into occupations with fewer leadership responsibilities, lower time pressure and less impact on results, and less need for analytic thinking (relative to ineligible mothers and their own pre-birth occupation characteristics).[18] We do not see any effect on whether eligible mothers sorted into occupations in which the work schedule is regular. Reassuringly, there are no significant pre-trends for any of the occupation (O*NET) outcomes.

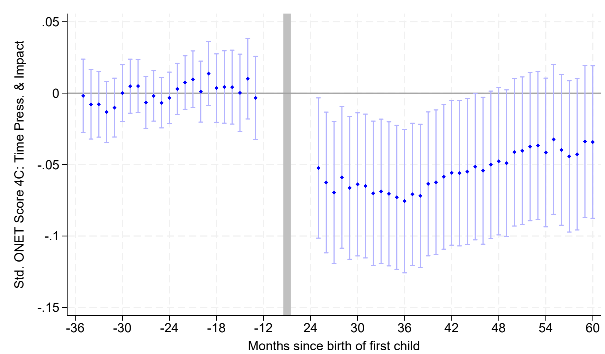
In the first row and in columns (5)-(8) of Table 4, we show the effect of $CB{+}W$-eligibility on all mothers' standardized occupation scores, relative to the baseline score in the 2^{nd}–3^{rd} year before childbirth: 3–5 years after giving birth, eligible mothers sort into occupations that have fewer leadership responsibilities and involve lower pressure: eligible mothers sort into jobs that have, on average, a 5.7 (4.8) percent of a standard deviation lower O*NET "Leadership" ("Pressure") Scores (relative to occupations they were in before birth, and to the control women with no birth); the point estimate on the "Analytical Thinking" Score is negative (-4.6) but insignificant.

Figure 6: Effect of $CB{+}W$-Eligibility on Occupation Characteristics

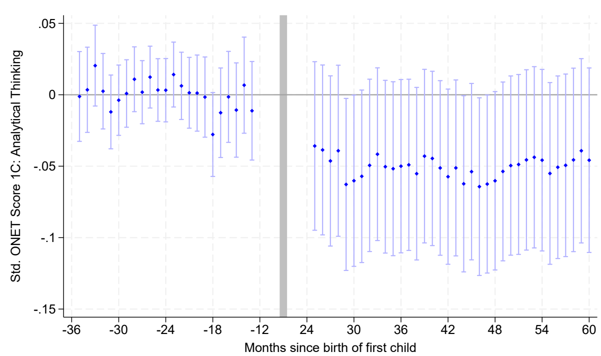
(a) Leadership in Job



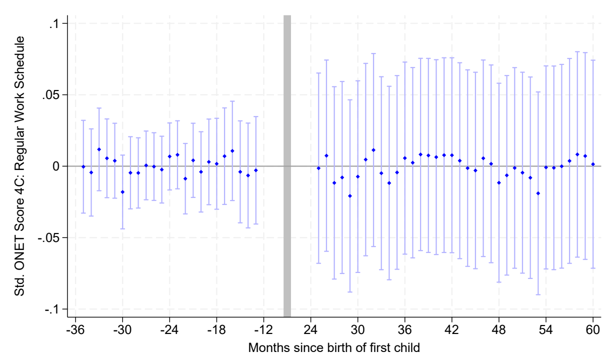
(b) Time Pressure and Impact on Results



(c) Job Requires Analytical Thinking



(d) Job's Schedule is Regular



Notes: Figure shows the estimated δ_k's 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 3.2 and 4.2.

These results are consistent with mothers having an income target in mind – taking advantage

[18] We present descriptive time patterns and DiD estimates from equation (2) in Figures A7–A10.

of the generous parental leave benefit, but not sorting into stressful and managerial occupations with much time pressure and responsibility once they reached their income target – and trading off wage with job amenities, albeit at higher-paying firms. This strategy may also be important for them by keeping the development of their children in mind; indeed, it may happen that they do not (yet) sort into demanding occupations while the child is in the critical period of child development (Cunha and Heckman, 2007, 2008; Cunha et al., 2010), but they will later when the child is older. The results also suggest important, previously undocumented, persistence in mothers' occupational sorting patterns when they return to work earlier. The persistence in occupational sorting is to some extent similar to "lock-in" patterns in other contexts; e.g., Collischon et al. (2024) show lock-in of Minijobs[19] in Germany or De Quinto and González (2024) find that mothers who are more attached to the labor market are more likely stay in part-time jobs and earn less due to a part-time reform in Spain.[20] Lock-in into specific occupations (in our case) may indicate misallocation, and policies that help mothers to avoid (disincentivize) lock-in can improve allocative efficiency.

The heterogeneity analyses reported in Table 4 show some key differences among mothers' labor market trajectories. Some eligible mothers have higher wages and work at higher-paying firms despite sorting into less managerial occupations. For instance, eligible mothers who give birth at an older age than 31, have around 4.3 percent higher wage and 3 percent higher AKM firm effect, despite them sorting into jobs that have, on average, a 5.9 (8.4) percent of a standard deviation lower O*NET "Leadership" ("Pressure") Scores (relative to occupations they were in before birth, and to the control women with no birth). Similarly, eligible mothers in the capital, have around 7.1 percent higher wage and 4.8 percent higher AKM firm effect, despite them sorting into jobs that have, on average, a 10 (7.3) percent of a standard deviation lower O*NET "Leadership" ("Pressure") Score (albeit the later is insignificant at the usual significance levels). But, eligible mothers who work at lower-paying firms prior to giving birth, have a large positive point estimate for the AKM firm effect (0.17), large negative point estimates on the leadership and pressure O*NET scores (0.06), and a large positive point estimate on the regular schedule score (0.09), suggesting that it is important for mothers who work at lower-paying firm prior to giving birth to eventually work at higher-paying firms, even if in less managerial but more predictable jobs.

Finally, note that earlier return to work implicitly implies that, all else equal, an early-returner woman at the same age will have accumulated more tenure in the labor market than a late-returner woman. Then, one might ask whether the positive wage and AKM firm effect are solely driven by more tenure at a given month after childbirth? Appendix Figure A13 does not suggest so: if we define the event as return to work and not as the time of the birth, then the DiD Event Study coefficient estimates for log-wages are still large and significantly positive.

[19] Minijob or "gering-fügige Beschäftigung" in Germany is an employment relationship with a low absolute level of earnings (max. 520 euros) or of short duration; for more details, *see* the definition of the Federal Employment Agency of Germany at https://www.arbeitsagentur.de/lexikon/minijob.

[20] Yet, Baertsch and Malte (2024) do not find lock-in of a policy incentivizing return before age 1 part-time.

5.3 Outcomes: Staying at the Same Employer or Moving Up/Down

In this subsection, we investigate the patterns of transitioning between employers that explain the above presented estimated effects of earlier return to work. For this, we first define a binary variable, indicating if the worker works at the same firm in a given month as at event time −13.[21]

Table 5: Effect of *CB+W*-Eligibility on Transitions Across Firms Compared to One Year Before the Event

	1: At Same Firm (1)	1: At a Firm With... Higher AKM (2)	Lower AKM (3)
All	0.040**	0.011	-0.032***
	[0.020]	[0.012]	[0.011]
By Age at Childbirth			
Age 25-30	0.030	0.013	-0.032**
	[0.027]	[0.017]	[0.015]
Age 31+	0.049*	0.012	-0.032**
	[0.028]	[0.017]	[0.016]
By Location			
Rural	0.024	0.016	-0.033***
	[0.022]	[0.013]	[0.012]
Capital	0.091**	-0.0083	-0.025
	[0.041]	[0.026]	[0.024]
By Baseline Occupation			
Blue-Collar	0.052	-0.001	-0.025
	[0.032]	[0.017]	[0.016]
White-Collar	0.035	0.025	-0.036**
	[0.025]	[0.017]	[0.016]
By Baseline Wage			
Below Median	0.021	0.009	-0.020
	[0.030]	[0.020]	[0.018]
Above Median	0.050**	0.035*	-0.055***
	[0.026]	[0.019]	[0.018]
By Baseline AKM Firm FE			
Below Median	0.032	0.017	-0.0289
	[0.031]	[0.020]	[0.018]
Above Median	0.041	0.035*	-0.049**
	[0.026]	[0.019]	[0.019]

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. The sample is the baseline sample with employment restrictions as described in Sections 3.2 and 4.2. 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.

First, we look at the shares of mothers in the eligible and ineligible cohorts who, in at least one

[21] Panel (a) of Figure A14 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.

month in a given year after birth, work at the same firm as in event time −13 (among women in the workforce). In the eligible cohort, the shares in years 1–5 after giving birth are 88.9, 85.4, 74.4, 67.4, and 56.4 percent, respectively; in the ineligible cohort, the same shares are 88.1, 81.4, 75.6, 65.1, and 55.3 percent. In Table 5, we show the effects of the policy on the propensity to (i) be employed at the same firm as one year before the event in at least 1 month in a given event-year, (ii) be employed by firms with a lower AKM firm effect (and lower firm-specific wage premium), and (iii) be employed by firms with a higher AKM firm effect (and higher firm-specific wage premium), in a given event-year. One observation corresponds to one individual in an event-year.

The first row for "All" of Table 5 reveals that the eligibility to the policy increased mothers' propensity to stay at the same firm by around 4 percentage points, and decreased the propensity to switch "down" to a firm with a lower AKM firm effect by 3.2 percentage points across years in the 3^{rd}–5^{th} year after childbirth (which poin estimates are significant at the 5 percent level). The point estimate for switching "up" in column (2) is positive, but insignificant.

Subsequent rows show that the positive effect on staying at the same firm is driven by mothers who are 31 years or older when giving birth (with a point estimate of 4.88 in column (1)), whereas the point estimates for the propensity to switch "down" do not differ by age at birth. At the same time, the increased propensity of staying at the same firm is driven by women in Budapest, the capital of Hungary (with a point estimate of 9.1), and by high-earners (with a point estimate of 0.05). Also, the negative effect on switching down to a lower AKM firm is driven by white-collar women who worked at a firm with a higher AKM and were high-earners before giving birth.

In sum, results in Tables 4–5 suggest that for women in the capital, for those who gave birth at an older age, and for initially high-earners earlier return to work due to the policy may have helped through increasing the probability of internal promotion at the same firm (given that women in the capital and those who gave birth at an older age also experience positive wage effects).

6 Conclusion and Discussion

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 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 2 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 if they returned to work 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 the 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 their 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 parental leave. This finding suggests that the majority of mothers who decide not to work until their child's 2^{nd} birthday (85% of all mothers before the reform) do so due to reasons other than the substitution effect, including the income effect of parental benefits, the availability of childcare, or social norms.

Second, we assess whether earlier return-to-work affects mothers' job quality outcomes. We find that 3–5 years after childbirth, mothers eligible for a policy that allowed to keep maternity benefits while working, tend to sort into firms with higher wage premiums, but into roles with fewer leadership responsibilities, lower time pressure or consequences of error, and less need for analytical thinking. We find wage increases for some – mothers who give birth at an older age and live in Budapest – despite sorting into less managerial jobs, while those initially in lower-paying firms tend to move up to higher-paying firms (or are less likely to move further down), albeit into less managerial but predictable jobs with a regular schedule. Our results point to the importance of outcomes other than wages, such as firms' and occupations' characteristics, after giving birth to a first child, and suggest important persistencies in mothers' occupational sorting.

The persistence in occupational sorting is consistent with "lock-in" patterns in other contexts; e.g., Collischon et al. (2024) show lock-in into Minijobs in Germany or De Quinto and González (2024) show lock-in to part-time jobs in Spain. Our results indicate that a policy incentivizing early return after giving birth results lock-in into occupations with lower responsibilities, at least in the first five years, with allocative efficiency implications (and no lock-in into part-time jobs).

Overall, our results are consistent with mothers having an income target in mind – taking advantage of the generous Hungarian parental leave benefit, but not sorting into stressful and managerial occupations with much time pressure and responsibility once they reached their income target – and trading off wage with job amenities (Rosen, 1986; Mas and Pallais, 2017), albeit at higher-paying firms. This strategy may also be important for them by keeping the development of their children in mind; indeed, it may happen that they do not (yet) sort into demanding occupations while the child is in the critical period of child development (Cunha and Heckman, 2007, 2008; Cunha et al., 2010) and the quality of parenting time and the parent–child interactions is of primary importance (Kim et al., 2018; Cobb-Clark et al., 2019; Seror, 2022), but they will when the child is older. We leave the study of such longer-term occupational sorting and child outcomes for future research, requiring data with longer time horizon.

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A Appendix: Further Empirical Results

A.1 Effects on Fertility

In this subsection, we test whether the reform changed families' birth planning behavior.

As 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 incentivized families to reduce the time between births. In our sample, eligible mothers (whose child turns 13 months old in 07/2013–12/2013) are eligible to keep their childcare benefits for additional 7–12 months if they return to work, and they could also receive maternity benefits for their first and second child for potentially a year longer than ineligible mothers (whose child turns 13 months old in 07/2012–12/2012).

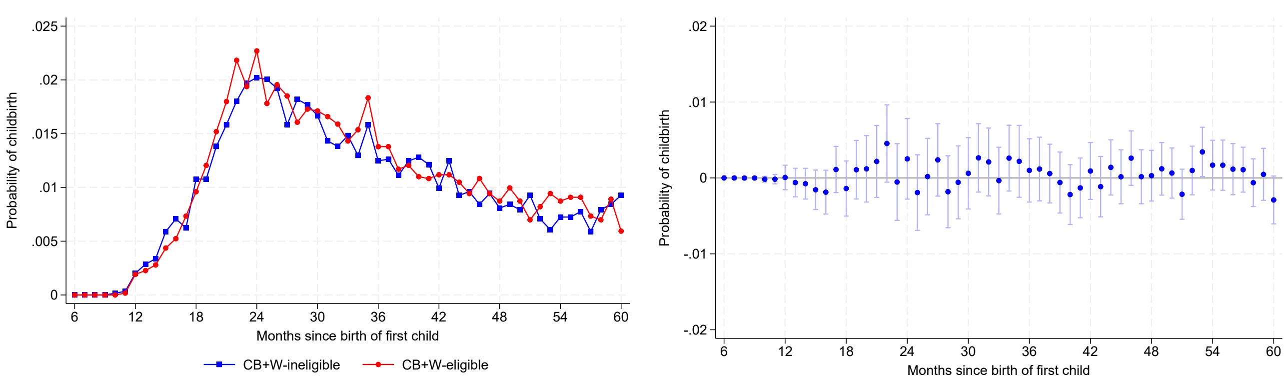
Despite the differences between the eligible and ineligible cohorts' incentives to have a second child, panel (a) of Figure A1 suggests that there is no systematic difference in the timing of second birth between the two cohorts. We test the statistical significance of these raw differences in fertility by estimating equation (5) on childbirth rate. Panel (b) of Figure A1 confirms that there is no significant difference in later fertility between eligible and ineligible mothers.

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.

Appendix Figure A1: Effect of the *CB+W* Policy on Childbirth Rate (Further Child)

(a) Descriptive

(b) Difference-In-Differences; Birth Sample



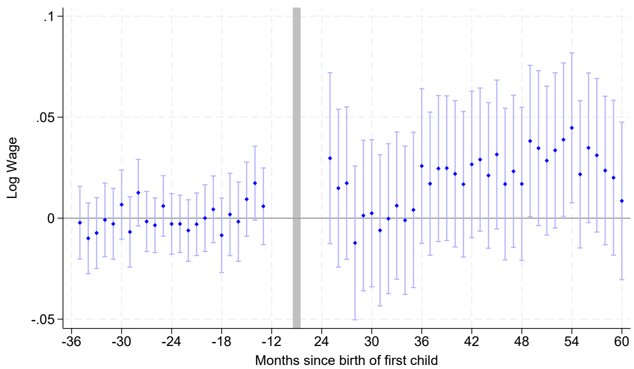
Notes: Panel (a) shows childbirth rate after the birth of the first child. Panel (b) shows estimated θ_k coefficients from equation (5). Bars are 95 percent confidence intervals based on robust standard errors. Sample is as described in Section 3.2, excluding the control sample (women never giving birth).

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 (as shown in Section 5.1), it also did not affect mothers' subsequent fertility. Thus, the estimates for the policy effect on mothers' labor market outcomes 3–5 years after birth we provide in Section 5.1 are not tainted by compositional differences in the workforce or in the number and timing of children of the two cohorts.

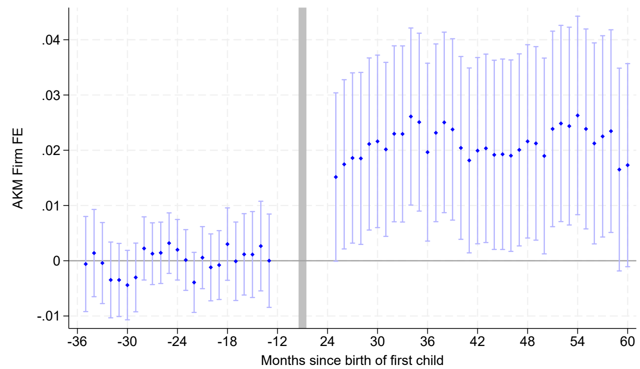
A.2 Additional Figures and Tables

Appendix Figure A2: Effect of $CB{+}W$-Eligibility on Job Characteristics, Matched Sample

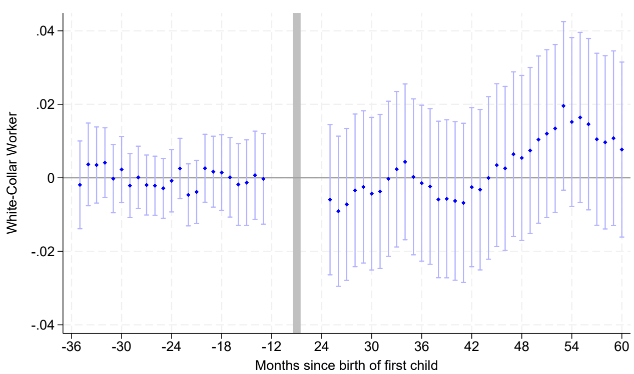
(a) Log Hourly Wage



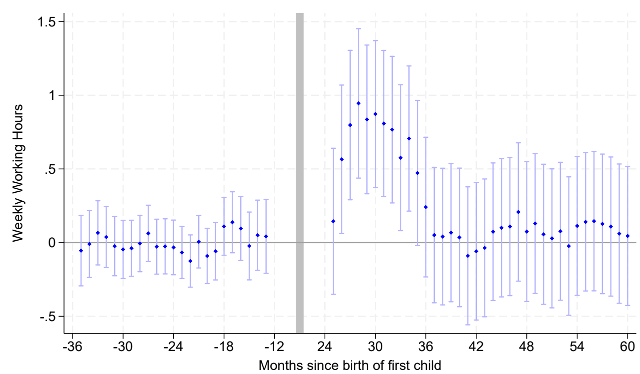
(b) AKM Firm FE



(c) Employment in White-Collar Job

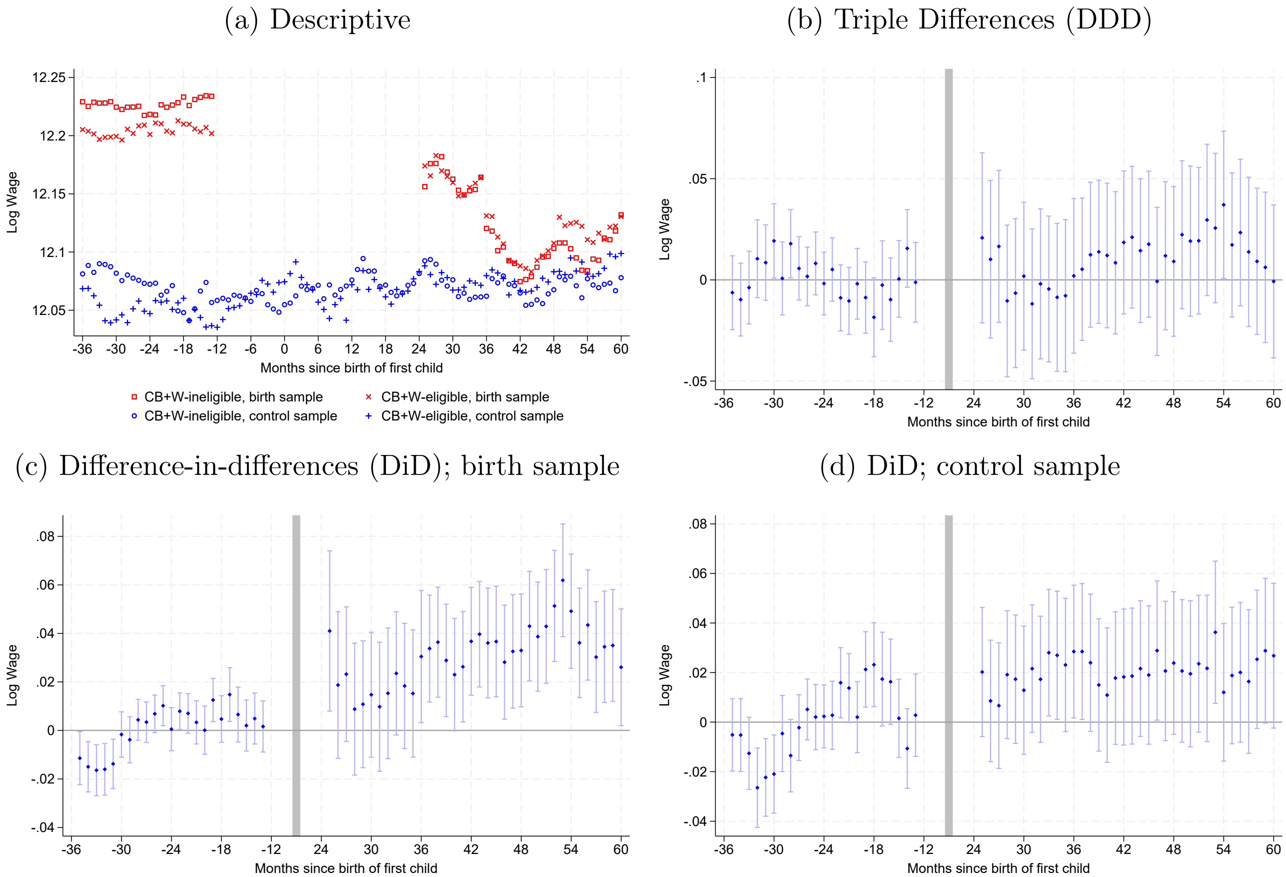


(d) Weekly Hours of Work



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 matched sample, as described in Section 3.2, with further sample restrictions described in Section 4.2.

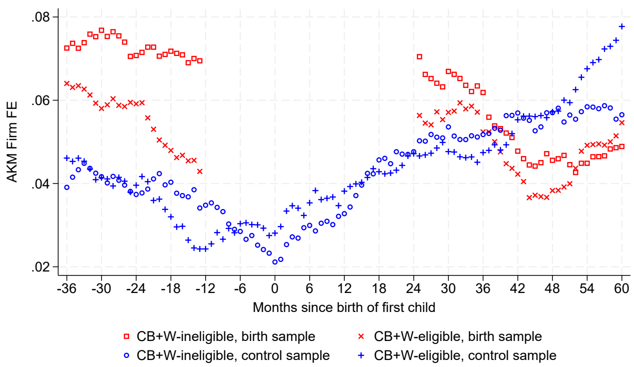
Appendix Figure A3: Logarithm of Hourly Wage: Descriptive Patterns and DD Estimates



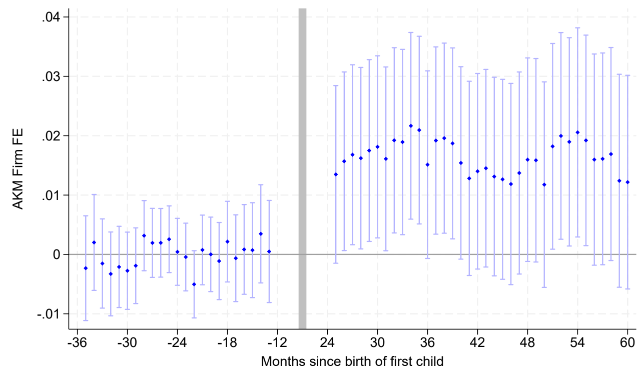
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

Appendix Figure A4: AKM Firm FE: Descriptive Patterns and DD Estimates

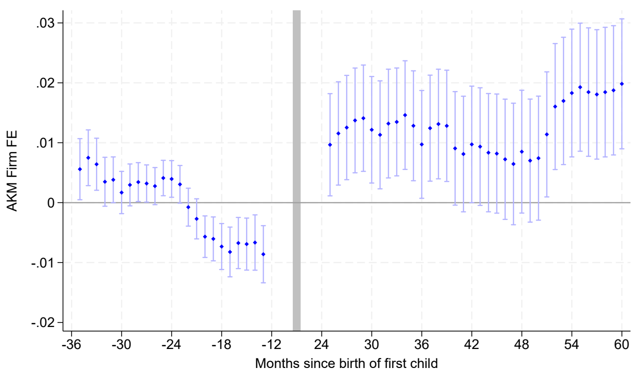
(a) Descriptive



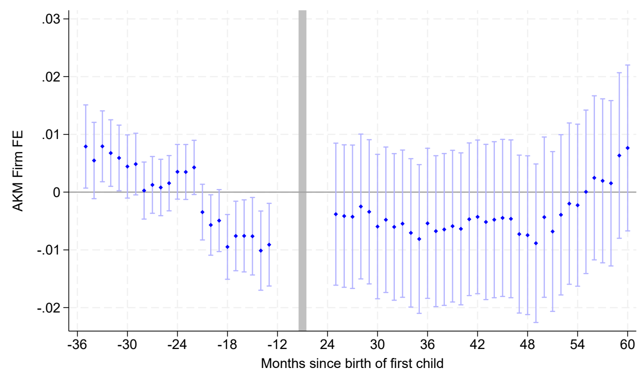
(b) Triple Differences (DDD)



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



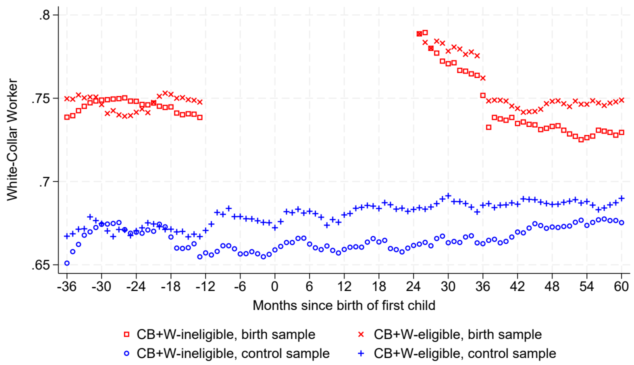
(d) DiD; control sample



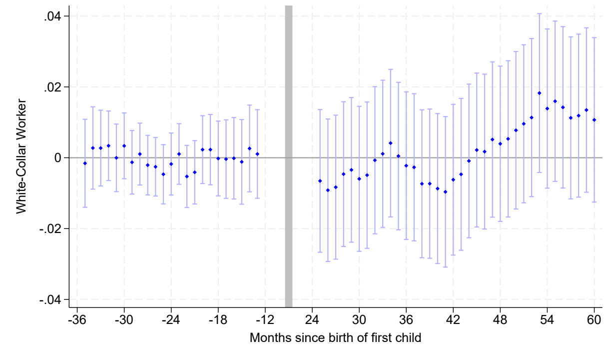
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

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

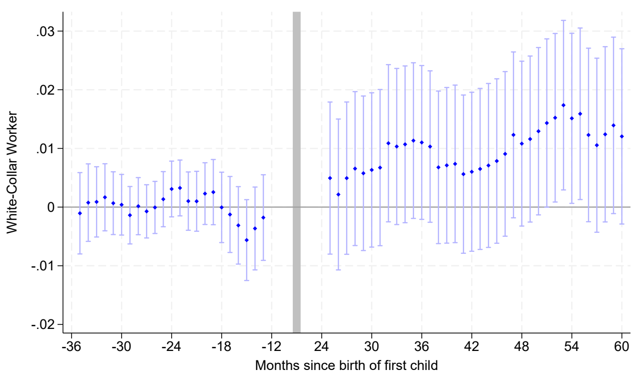
(a) Descriptive



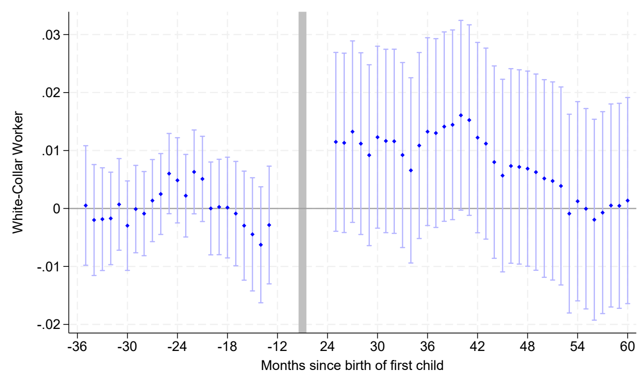
(b) Triple Differences (DDD)



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



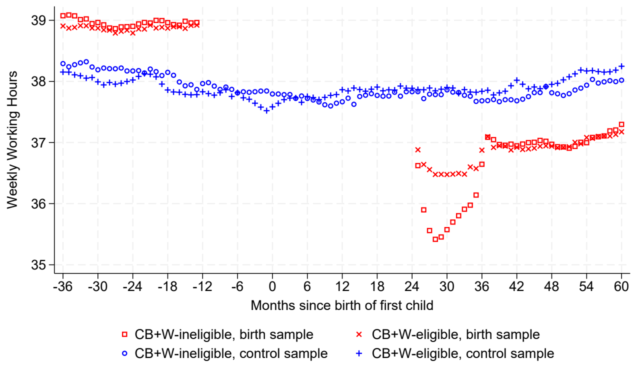
(d) DiD; control sample



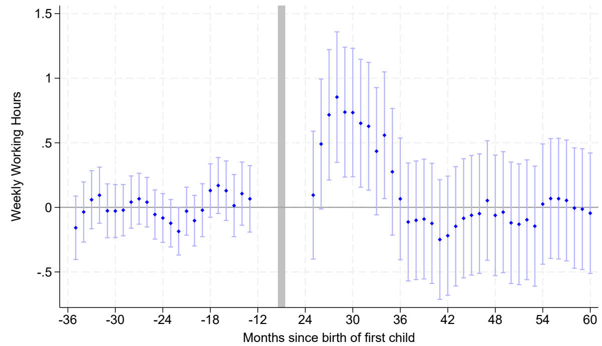
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

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

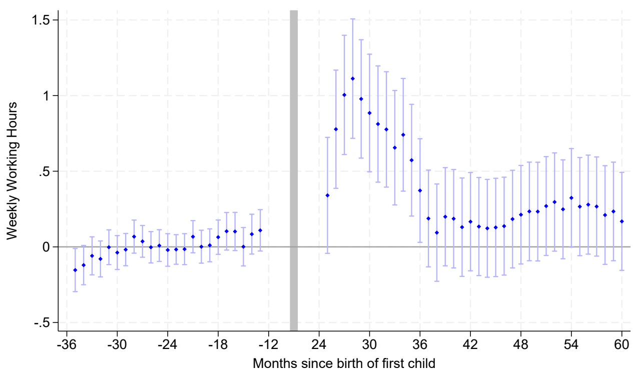
(a) Descriptive



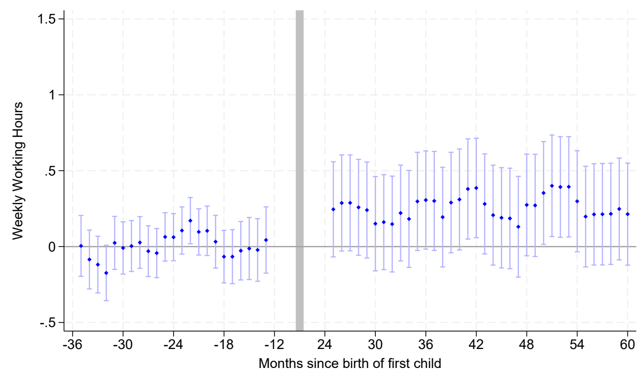
(b) Triple Differences (DDD)



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



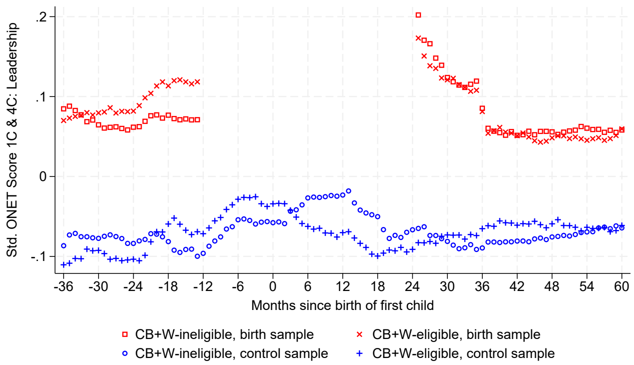
(d) DiD; control sample



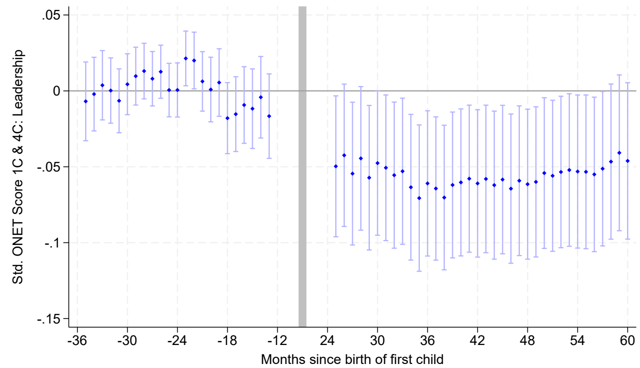
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

Appendix Figure A7: O*NET – Leadership in Job: Descriptive Patterns and DD Estimates

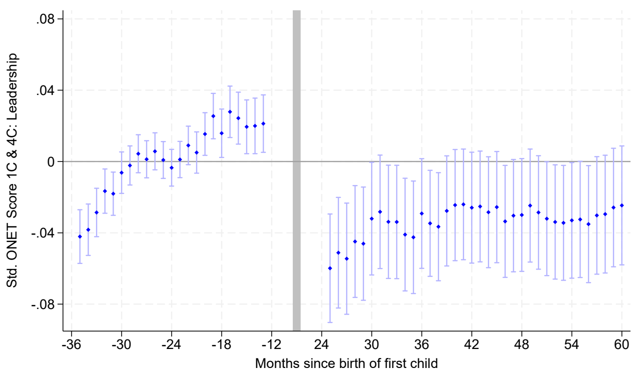
(a) Descriptive



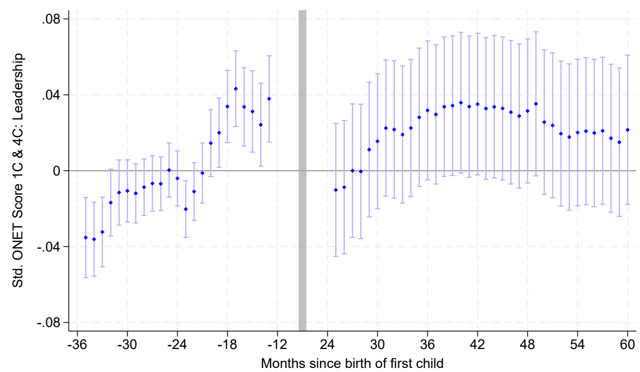
(b) Triple Differences (DDD)



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



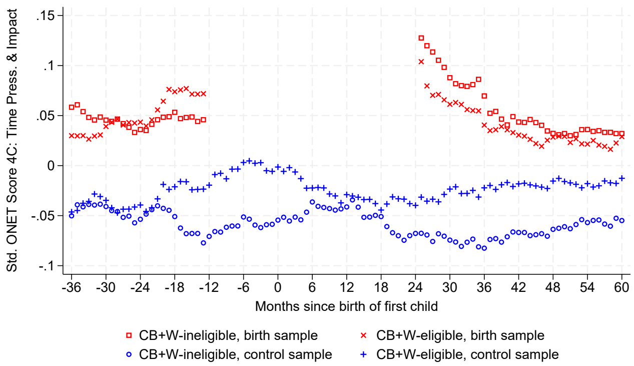
(d) DiD; control sample



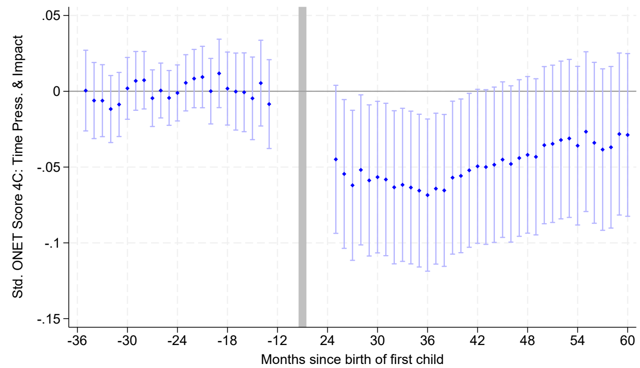
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

Appendix Figure A8: O*NET – Job Involves Time Pressure and Impact on Results: Descriptive Patterns and DD Estimates

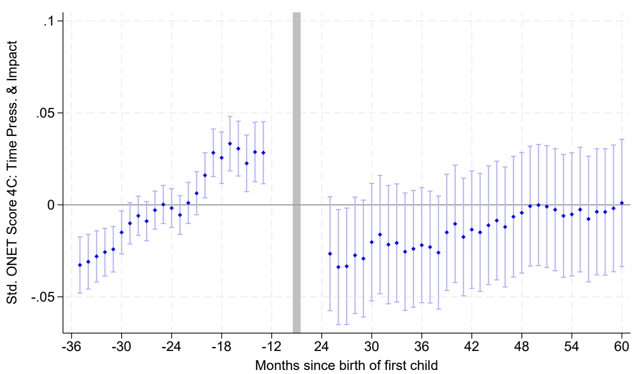
(a) Descriptive



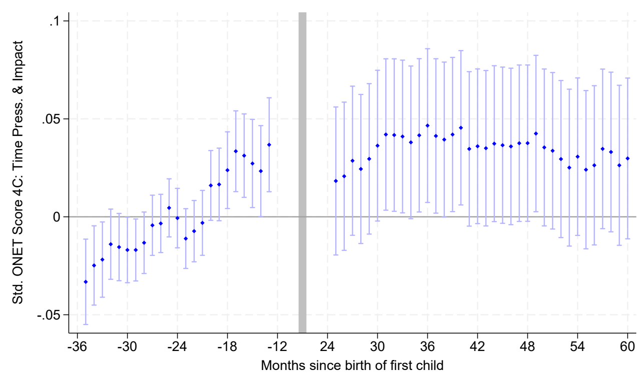
(b) Triple Differences (DDD)



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

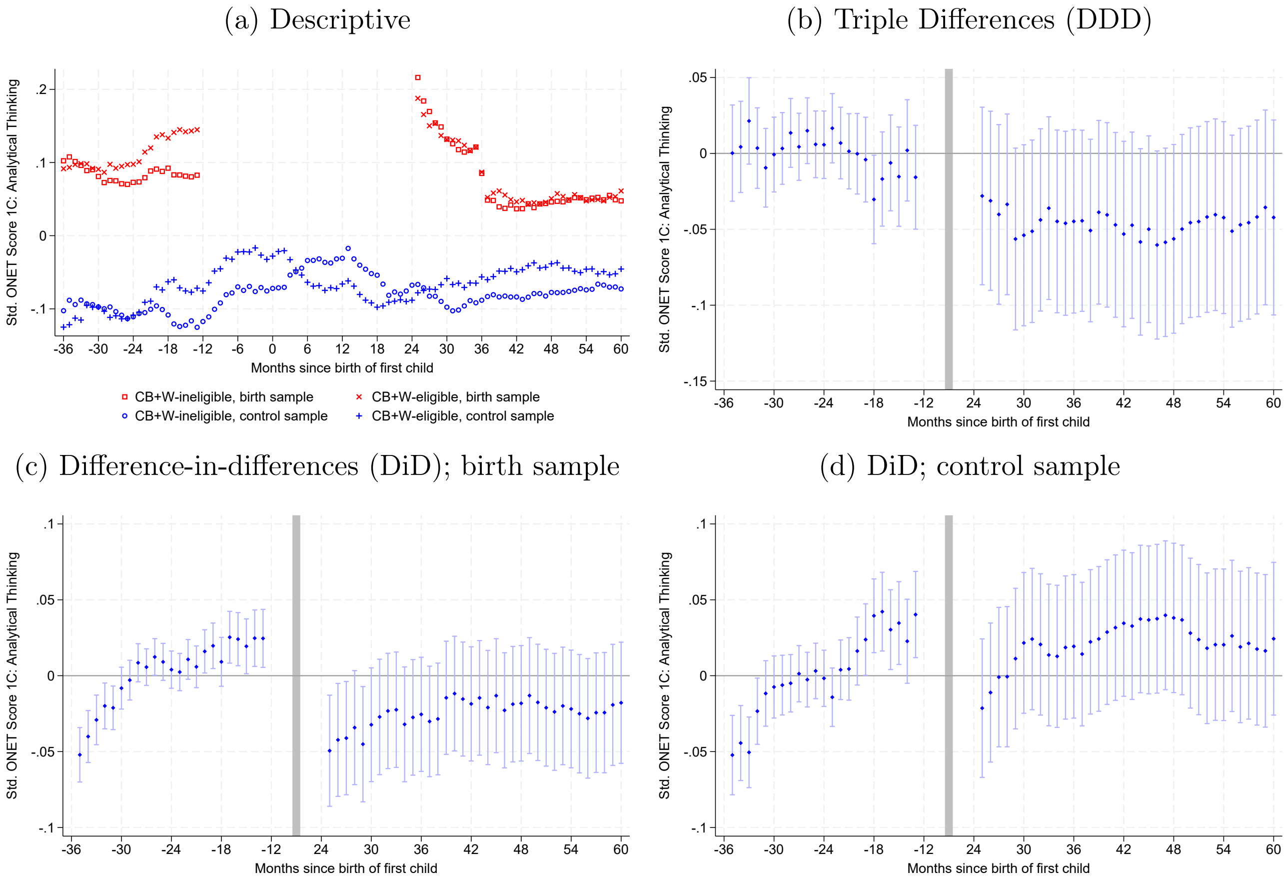


(d) DiD; control sample



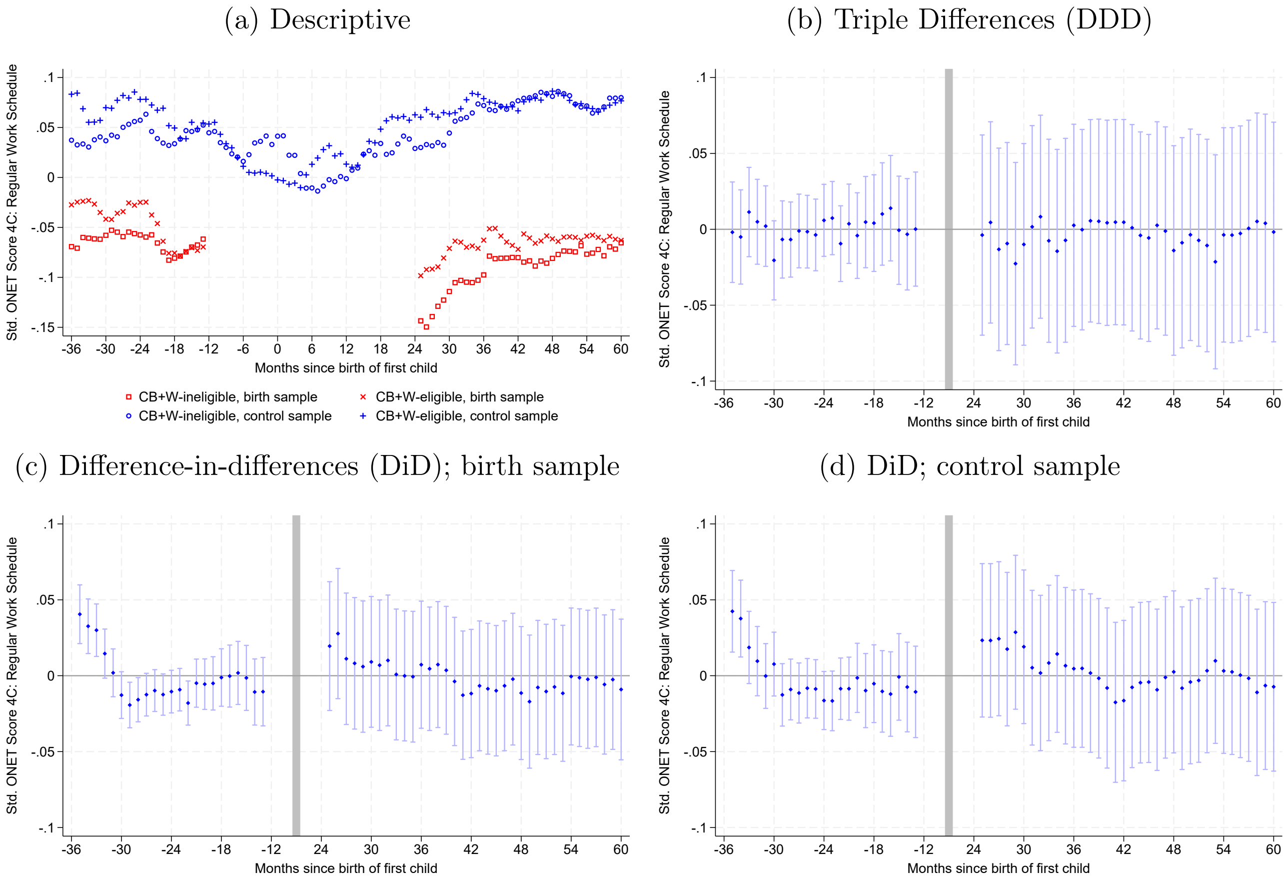
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta_k}$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

Appendix Figure A9: O*NET – Job Requires Analytical Thinking: Descriptive Patterns and DD Estimates



Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) 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 (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

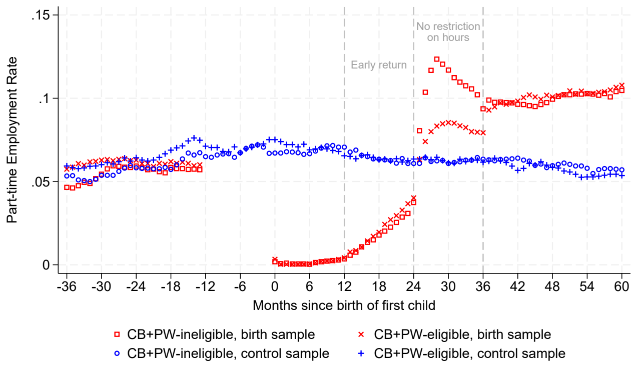
Appendix Figure A10: O*NET – Job's Schedule is Regular: Descriptive Patterns and DD Estimates



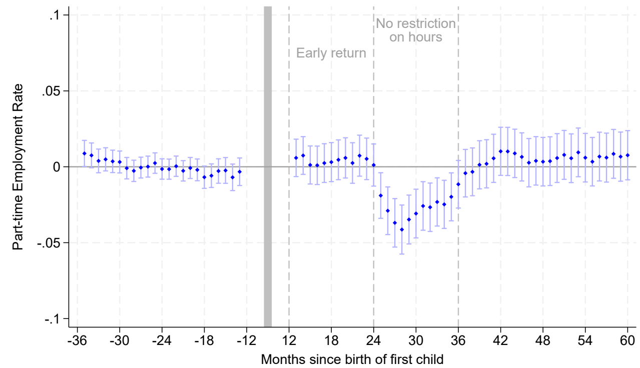
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta_k}$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

Appendix Figure A11: Effect of *CB+W* Eligibility on Part-time Employment

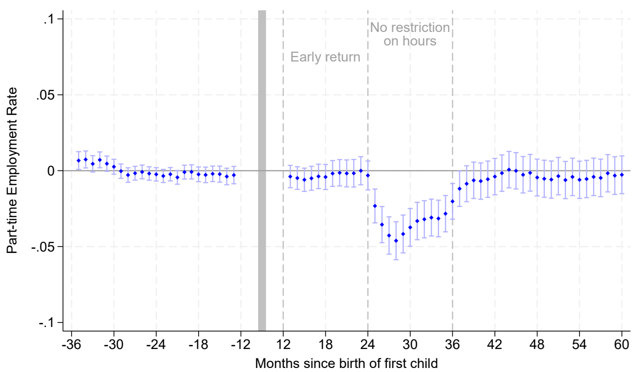
(a) Descriptive



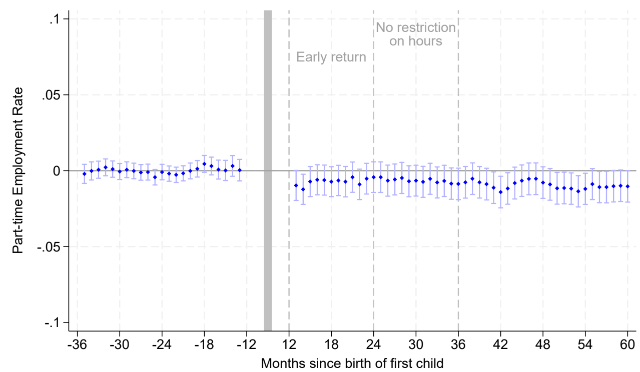
(b) Triple Differences (DDD)



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

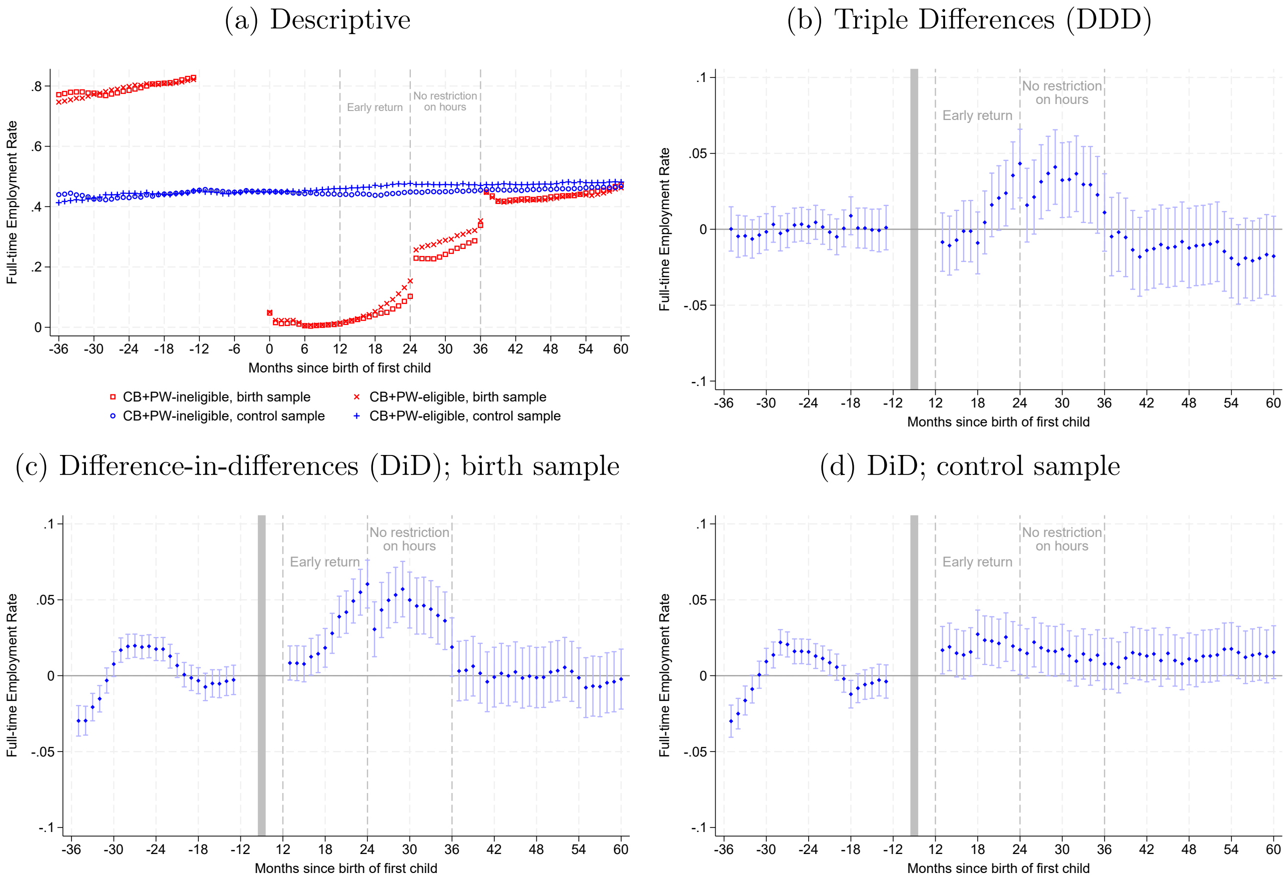


(d) DiD; control sample



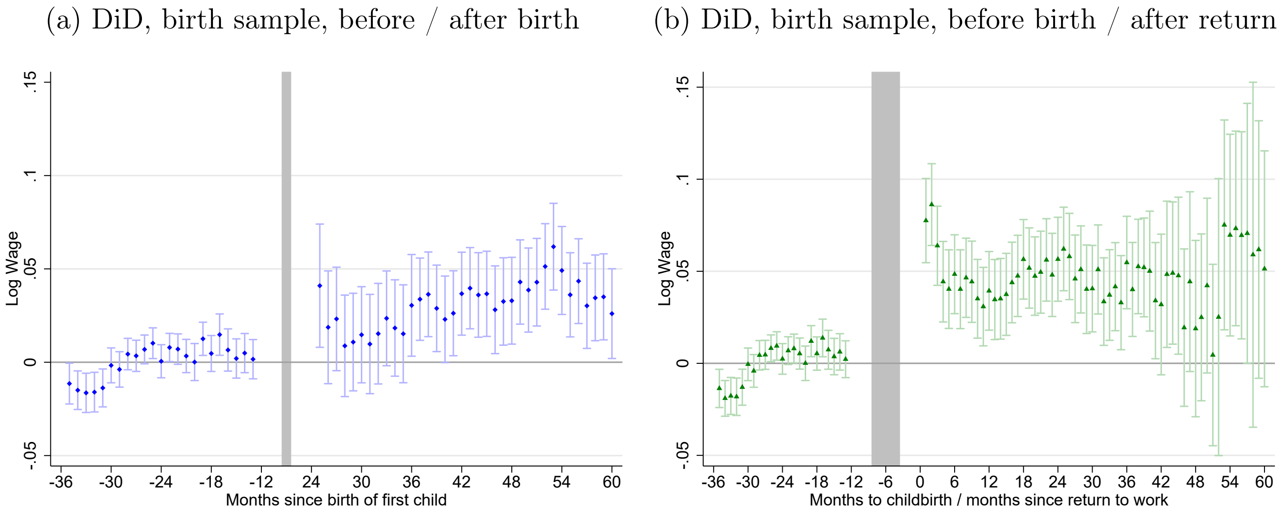
Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) the Event Study coefficient estimates for the birth sample and the control sample separately. Specifically, we show the estimated $\tilde{\beta}_k$'s from model (2). In panel (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

Appendix Figure A12: Effect of $CB+W$ Eligibility on Full-time Employment



Notes: Panel (a) shows averages by groups over time. Panels (c) and (d) 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 (c), the sample is restricted to women giving birth; in panel (d), 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 3.2 and 4.2.

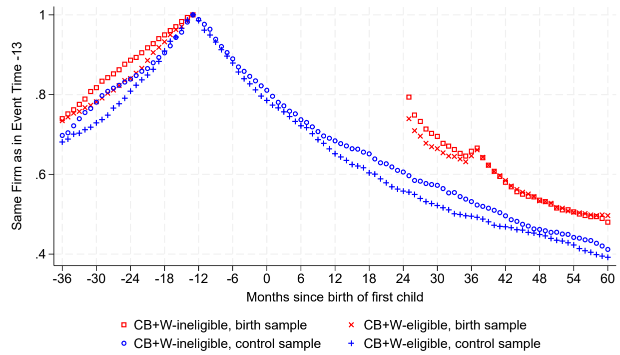
Appendix Figure A13: Earlier Return-to-Work vs. More Tenure at Work



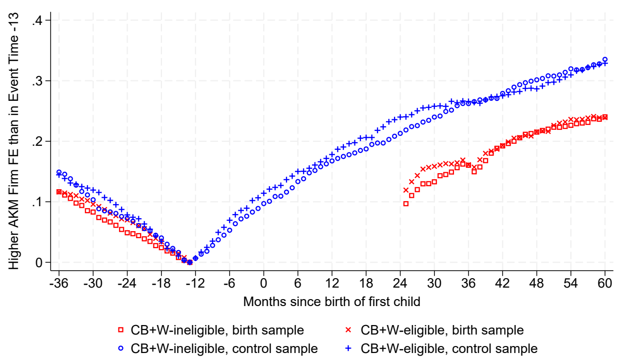
Notes: Figure shows the estimated $\tilde{\beta}_k$'s from model (2). In panel (b) the event time afer birth is the months since return to work, instead of the months since the birth of the first child (as in panel (a)). Bars are 95 percent confidence intervals based on robust standard errors. Sample is the baseline sample with employment restrictions as described in Section 3.2 and 4.2, restricting the sample to women giving birth.

Appendix Figure A14: Descriptive Patterns of Staying at the Same Firm and Transitions Between Employers

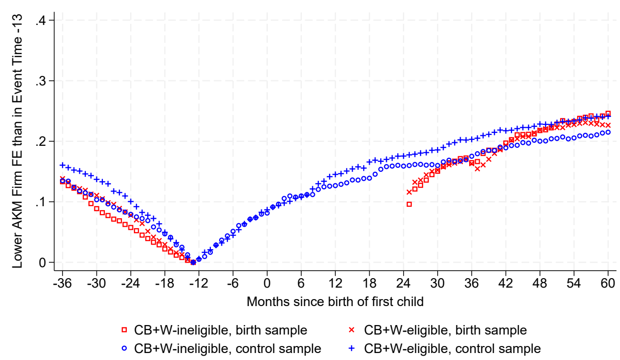
(a) Employment at Same Firm



(b) Employment at Higher AKM Firm FE Firm



(c) Employment at Lower AKM Firm FE Firm



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 3.2 and 4.2.

A.3 Compliers and Always-Workers

In this subsection, 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.[22] We use these net-of-time-effects wage and AKM firm FE values as outcomes. The IV estimates are in Table A1, 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.

Appendix Table A1: 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.

[22]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.

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 explain in Appendix B, *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, we cannot directly identify always-worker mothers in the sample. Instead, we focus on those women 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 75^{th} percentile or the 90^{th} percentile of its distribution (i.e., 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-workers" in Table A2, which shows no reduced-form effect, no matter which cutoff is used. Thus, our main estimates are interpretable as the effects of earlier return to work after childbirth on mothers' long-term labor outcomes.

Appendix Table A2: 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.

B Appendix: The Substitution Effect of Parental Leave

Paid parental leave decreases mothers' labor supply through two channels, that are rarely discussed in the context of parental leave: the Substitution Effect (SE) and the Income Effect (IE). Below we discuss both conceptually, and explain how we isolate the SE in our unique policy setting.

The SE represents the distortionary effect on work incentives in that mothers lose the parental leave benefit if they 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 IE is due to higher non-wage income and decreases labor supply because at a higher level of income, mothers might prefer staying at home.

We illustrate both effects in Figure A15, 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) after giving birth under the post-reform paid parental leave scheme that abolished return-to-work restrictions.

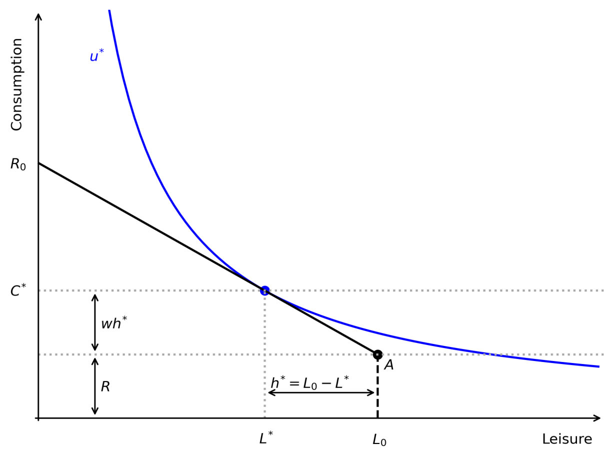
In panel (a), we show the labor supply decision of a woman before giving birth. She chooses the level of leisure (L) and consumption (C) that maximizes her utility, subject to the budget constraint. We use the term "leisure" to follow the terminology of the literature but after giving birth, that term 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^*. The blue line plots the corresponding indifference curve. 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 A15, 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 the parental leave benefit in the pre-reform parental leave scheme.

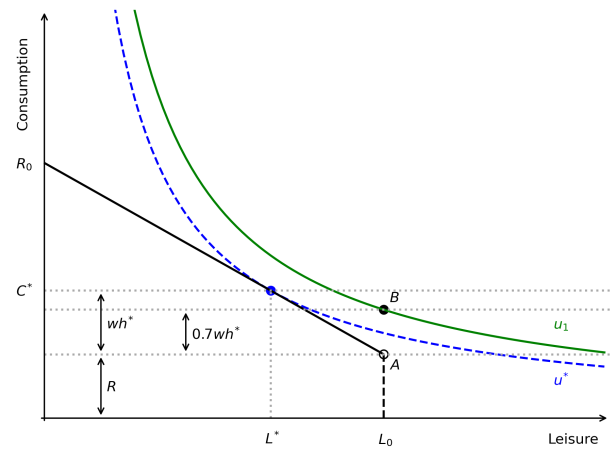
The relative importance of the two channels is policy-relevant: if the SE drives the decrease in mothers' labor supply between scenarios (a) and (b) (corresponding to panels (a) and (b)), there is a deadweight loss in the paid parental leave scheme (as in Autor and Duggan, 2007 for disability insurance), 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 IE 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.

Appendix Figure A15: The Labor Supply Decision of Mothers in Three Policy Scenarios

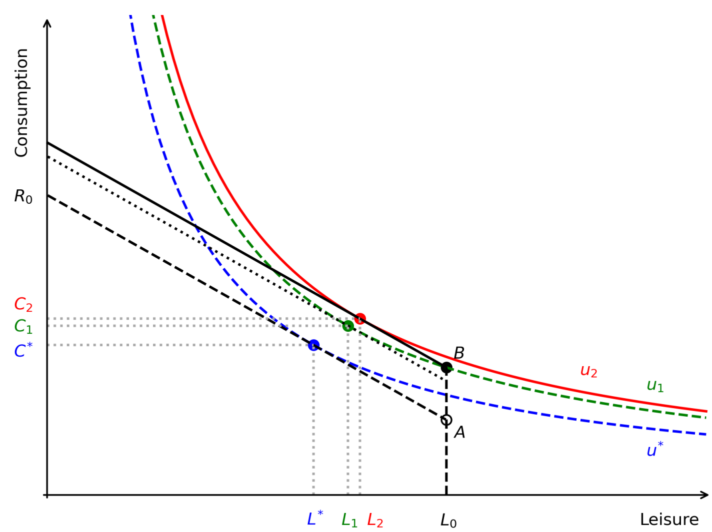
(a) Before Giving Birth / After Giving Birth Without Paid Leave



(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. Panels (b) and (c) show the labor market decision of mothers at a given month after giving birth. The blue dashed lines in panels (b) and (c) are copies of the blue line (indifference curve) of panel (a). The dashed green line in panel (c) is a copy of the green line of panel (b).

In panel (c) of Figure A15, 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 SE 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). Mothers who stayed out of the labor force due to the IE, 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 A15 we illustrate the labor supply decision of *complier* mothers, i.e., mothers who would decide not to work after childbirth in the pre-reform setting but would work in the post-reform setting. In Table A3, 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 has just loosened. In sum, the participation decision and thus any change in the observed employment rate of mothers, is only due to the "negative SE" of the reform.

Appendix Table A3: 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) that might have an effect on the budget constraint if a mother decides to work. 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.