

What Works for Working Mothers? A Regular Schedule Lowers the Child Penalty*

Ludovica Ciasullo Martina Uccioli

April 17, 2023

Abstract

Which work arrangements do mothers prefer, and how do these work arrangements affect the child penalty they experience? The Australian 2009 Fair Work Act explicitly entitled parents of young children to request a (reasonable) change in work arrangements. Leveraging variation in the timing of the law, timing of childbirth, and the bite of the law across different occupations and industries, we establish two main results. First, if allowed to request a change in work arrangements, new mothers ask for regularity in their schedule. Second, with regular schedules, working mothers' child penalty declined from a 47 percent drop in hours worked to a 40 percent drop. For the most exposed mothers, the Fair Work Act led to both a doubling in schedule regularity, and a 30% decrease in the child penalty in hours of work.

*Ciasullo: NYU, lc4177@nyu.edu; Uccioli: MIT, muccioli@mit.edu.

We are grateful to Daron Acemoglu, Amy Finkelstein, Simon Jäger, Petra Moser, Debraj Ray for guidance and advice throughout this project. We thank Josh Angrist, David Autor, Mattia Bertazzini, Alden Cheng, Jonathan Cohen, Viola Corradini, Angela Crema, Marc de la Barrera i Bardalet, Enrico de Magistris, Isabel Di Tella, Joel Flynn, Michael Gilraine, Claudia Goldin, Jonathan Gruber, Ahmet Gulek, Clemence Idoux, Chitra Marti, Matías Morales, Claudia Olivetti, Abby Ostricker, Helena Pedrotti, James Poterba, Charlie Rafkin, Martin Rotemberg, Ursina Schaede, Frank Schilbach, Garima Sharma, Anna Stansbury, Teresa Steininger, Diana Sverdrin Lisker, Marco Tabellini, Nagisa Tadjfar, Ishaana Talesara, Sharon Traberman, Sean Wang, Samuel Young, Whitney Zhang, and all the participants to MIT Labor Lunch and Coffee, MIT Public Finance Lunch, NYU Applied Micro Lunch and Breakfast, and the NBER GITE Mentoring Workshop for helpful comments. Uccioli is grateful to Jerry Hausman and the NBER Retirement and Disability Research Center for financial support. The research reported herein was performed pursuant to grant RDR18000003 from the US Social Security Administration (SSA) funded as part of the Retirement and Disability Research Consortium. The opinions and conclusions expressed are solely those of the author(s) and do not represent the opinions or policy of SSA, any agency of the Federal Government, or NBER. Neither the United States Government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of the contents of this report. Reference herein to any specific commercial product, process or service by trade name, trademark, manufacturer, or otherwise does not necessarily constitute or imply endorsement, recommendation or favoring by the United States Government or any agency thereof.

1 Introduction

In virtually all countries in the world, women experience a large drop in labor supply and labor earnings when becoming parents, while men do not (Kleven, 2022; Kleven et al., 2022). Although this child penalty today accounts for a very large fraction of the gender earnings gap in many developed countries,¹ existing research has not fully identified its causes yet. Many potential explanations have been tested, none of which can completely explain the observed patterns. These explanations broadly fall in two categories: individual characteristics (biology, comparative advantage, norms)² and lack of direct parental support from government (such as paid parental leave, childcare)³; however, the role of the workplace that awaits for mothers as they return to work has received less attention.

Previous literature suggests that work arrangements are key in determining maternal labor supply (e.g. Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016), and policy makers seem to hold a similar belief. For example, as of August 2022 all European Union member states must apply the “Directive on work-life balance for parents and carers”,⁴ which establishes rights “such as the right to request flexible working arrangements, which will help people develop their careers and family life without having to sacrifice either”⁵; at the same time, the US Congress is debating the “Schedules That Work Act”,⁶ introduced on the premise that “Employees across the nation are forced to juggle the dual demands of home and work. However, too few workplaces provide work schedules that allow their employees to succeed at both.”⁷ Despite this widespread belief, we lack causal evidence linking work arrangements with the child penalty, likely because it is hard to find natural experiments that exogenously shift work arrangements. This paper represents a first step towards filling this gap.

In this paper, we exploit the Fair Work Act, a law introduced in Australia in 2009 that explicitly entitled parents of young children to request a change in work arrangements. We show two main results: first, if allowed to request a change in work arrangements, new mothers ask for schedule regularity in their schedule; second, if granted regularity, they work more and their child penalty in labor supply is smaller than it would have been otherwise.

Work arrangements are what determines when, where and how a job is expected to be performed. For example, work arrangements include whether a worker has a predictable schedule or is on call; whether the job has to be performed in a specific location, such as the office or the home or a combination of the two; whether hours of work are rigid or can

¹For example, the child penalty is estimated to account for 70% of gender earnings gap in the 2010s in the US (Cortes and Pan, Forthcoming), 80% in Denmark (Kleven et al., 2019), 80% in Austria (Kleven et al., 2021a).

²e.g. in Kleven et al. (2021b); Andresen and Nix (2022); Kleven (2022); Boelmann et al. (2021)

³e.g. in Schönberg and Ludsteck (2014); Havnes and Mogstad (2011); Kleven et al. (2021a)

⁴Directive (EU) 2019/1158 of the European Parliament and the Council, link.

⁵Source: European Commission press release, 2 August 2022. url: https://ec.europa.eu/commission/presscorner/detail/en/IP_22_4785

⁶House bill H.R.6670, reintroduced on 02/09/2022 (originally introduced in 2015), link.

⁷Source: Congresswoman Rosa DeLauro (CT-03) press release, 20 June 2017. url: <https://delauro.house.gov/media-center/press-releases/delauro-and-warren-lead-members-reintroducing-schedules-work-act>

be modified at the employees' will; whether one person's full time job can be carried out by two people working part-time.

The Australian Fair Work Act entitled parents of children below school age to request a change in work arrangements, thus creating the setting for two related but distinct pieces of analysis. First, the wording of the law is deliberately vague: it does not specify which work arrangements parents are entitled to request changes to. This allows us to study in a revealed-preference fashion which work arrangements parents choose to change when allowed to do so, and which ones they do not. Second, this law acts as a shifter to a certain subset of work arrangements, and we can use this natural experiment as quasi-exogenous variation to shed light on the effect of relaxing constraints on work arrangements on maternal labor supply.

The *Household, Income and Labour Dynamics in Australia* survey allows us to study the effect of workplace arrangements on the child penalty thanks to the richness of the information provided. In this longitudinal survey started in 2001, we can directly measure many work arrangements, through questions on scheduling (e.g. work Monday to Friday, on weekends, vary from week to week) and shifts (e.g. work on regular daytime schedules, in the evenings, on call), as well as workplace entitlements (e.g. entitlement to flexible start and finish times) and direct questions on work arrangements (e.g. hours of work carried out at home). We can also observe when children are born, and in each year whether and how many hours the parents worked and for what pay, thus allowing us to measure the child penalty in labor supply and earnings.

Using a flexible differences-in-differences framework, we show that the 2009 Fair Work Act did impact a specific type of work arrangements for new mothers: it increased schedule regularity. Although the law is gender-neutral, and encompasses parents of children below school age – it expanded in 2013 to include school-aged children – we posit that its effect might differ by gender, parity, by age of the child, and over time, and we allow for such treatment effect heterogeneity. We find that women who became mothers after the passage of the law experienced a sharp increase in schedule regularity, measured as working a regular daytime schedule from Monday to Friday, and that this increase was persistent. This also holds if we measure regularity simply as working a regular daytime schedule, as not working on weekends, and as not being on call. Instead, we do not find significant effects on other work arrangements, including flexible start and finish times, and results regarding working from home are mixed. We do not find any significant effect for men.

Since the difference-in-differences analysis above, and some of the analysis on the child penalty that follows, rely on the assumption that mothers who have their first child earlier in the sample are comparable to mothers who have their first child later in the sample, we provide two pieces of evidence in support of this assumption. First, we show that the fraction of women who become mothers any given year does not appear to change systematically around 2009. Second, we test whether pre-motherhood characteristics differ systematically between women who have their first child before or after 2009, and show that the two groups are balanced on observables.

We then turn to our main outcome of interest, the child penalty in labor supply, which at baseline is large for women and very small for men. The child penalty is defined as the change in a variable in the years following the birth of one's first child relative to before. We show that at baseline women experience large and persistent child penalties in the

labor market, while men don't: in the five years following the birth of their first child, on average women face a drop in labor force participation by 39 percentage points, while the corresponding figure for men is 2 percentage points. Even new mothers who continue working work on average 18 fewer hours per week, a figure driven by the fact that about two thirds of them switch from full-time to part-time work after giving birth to their first child.

However, the child penalty in both intensive and extensive labor supply decreased systematically for women who become mothers after 2009, suggesting a role of the change in work arrangements brought about by the Fair Work Act in mothers' labor supply decisions. We compare early cohorts of mothers, whose first child was born before 2009, and late cohorts of mothers, whose first child was born after 2009. Both on the extensive, but especially on the intensive margin, we observe an upward shift of maternal labor supply trajectories: both cohorts of mothers show a drop in labor supply after childbirth, but the drop is smaller at all horizons for the late one. The difference is sizable: in the five years following the birth of the first child, the drop in labor force participation is 7 percentage points smaller for late relative to early cohorts (from 39 to 32 percentage points) and the drop in weekly hours is 3 hours smaller (from a drop of 18 hours per week to one of 15 hours per week).

Comparing early and late cohorts of mothers is suggestive, but could be capturing a time trend: by estimating the child penalty in labor supply separately by year of birth of the first child, we show that there is a sharp, discontinuous, and robust change in 2009, in particular for the intensive margin. To test for this, we split women into cohorts defined by the year in which they become mothers, and for each cohort we compute the child penalty in labor supply in the first five years from childbirth. When we plot these child penalties against the year of first childbirth, we see a relatively stable trend in the years leading up to 2009, and then a very sharp discontinuity in the direction of greater labor supply (smaller child penalty) in 2009, which remains stable afterwards. We confirm with placebo tests that the break is indeed in 2009, especially for the child penalty in intensive margin labor supply (weekly hours and log weekly hours of work).

To further investigate whether the increase in regularity brought about by the Fair Work Act caused the decrease in child penalty, we construct a measure of exposure to the law, based on the level of schedule regularity within occupation-by-industry cells before 2009. The basic idea is the following: there are certain jobs for which schedules were already fully regular before the passage of the law, and hence the law did not have any bite on them; on the other hand, there are some jobs that cannot be made regular for technological reasons, and the law did not have bite on them either (the law allows parents to request a change in work arrangements, but this request can be denied "on reasonable business grounds"). The jobs for which we observe that, in the years before 2009, some people were working regular schedules, and some people were not, are those where we infer there were no technological barriers to schedule regularity and there was room for increasing it. Thus, if we rank jobs according to the within-job fraction of regularity, we expect the law to not impact the extremes, but to impact jobs in the middle.

Using this measure of exposure, we find that the increase in regularity and the mirror decrease in child penalty in intensive-margin labor supply are entirely concentrated in the group of women with jobs in the middle tercile of the regularity distribution, those most

exposed to the law. We assign women the level of regularity of the occupation-by-industry they were in two years before childbirth, and we track the evolution over time of the child penalty, separately for the group of women with pre-birth jobs in the first, second, and third tercile of the regularity distribution. We find that the level of regularity only increased for the mothers in the middle tercile; correspondingly, the entire increase in intensive-margin labor supply (decrease in child penalty) observed around 2009 is concentrated in the same group. The magnitude is sizeable: in the middle tercile, late cohorts experience a child penalty that is 32% smaller than early cohorts, while this figure is 8% and 9% (neither statistically different from zero) for the top and the bottom tercile. This split, however, does not explain the change in child penalty in labor force participation, as the three groups evolve largely in parallel along this dimension.

A potential main confounder to our analysis is the 2010 introduction of government-sponsored Paid Parental Leave (PPL): we show that our results are robust to accounting for it. We leverage the fact that, before the introduction of PPL, about 55% of working women were eligible for employer-provided paid maternity leave, an entitlement that we can observe in our data: we make the assumption that these women were not treated by the introduction of PPL. We find that not only accounting for PPL does not change our exposure-based results described above, but also that PPL *per se* does not have a significant impact on the child penalty in labor supply. This result is unsurprising, given the pre-existing institutional setting featuring unpaid leave with job protection plus a “baby bonus” payment, and is in line with similar findings in other contexts (e.g. see review in Olivetti and Petrongolo, 2017, and literature review below).

Related Literature This work is at the intersection of a large and growing literature on the determinants of the child penalty, and a recent but well established literature on the role of work arrangements on the gender earnings gaps.

Contrary to common wisdom, the size of the child penalty does not seem to be affected by parental leave policies (Schönberg and Ludsteck, 2014; Dahl et al., 2016; Kleven et al., 2021a), while the evidence on childcare is mixed (positive in Gelbach, 2002 and Lefebvre and Merrigan, 2008, null in Havnes and Mogstad, 2011 and Kleven et al., 2021a).⁸ Similarly, neither biology nor comparative advantage are able to explain the drop in labor earnings women – and not men – face when becoming parents (Andresen and Nix, 2022; Kleven et al., 2021b). Instead, gender norms seem to be a strong predictor of the size of the child penalty: Boelmann et al. (2021) show that, when moving to west Germany, eastern German women born between 1946 and 1994 have a lower child penalty than their new neighbours, carrying with them east Germany pro-work attitudes; Kleven (2022) shows that child penalties correlate strongly with measures of gender progressivity, both over time and across countries and US states, and that, when moving to the US or within the US, migrants experience child penalties that are strongly correlated with those of their country or state of origin. For a recent review of the literature on the child penalty, see Cortes and Pan (Forthcoming).

We contribute to this literature by focusing on the role played by the characteristics of

⁸The effects of family policies on female labor market outcomes are reviewed in Olivetti and Petrongolo (2017).

the workplace and work arrangements that women face upon returning to work, and more specifically flexibility and schedule regularity/predictability, which have been shown to be a key determinant in gender earnings gap (Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016). Two papers we are aware of have addressed a similar question: Bütkofer et al. (2018) compute child penalties for different professions in Norway for mothers who have their first birth between 1989 and 2000, and show that the child penalty is smaller in more flexible professions; Bang (2021) shows that child penalties in the US between 1979-2016 are decreasing in own and spouse's flexibility. We build on and complement these descriptive patterns by leveraging an exogenous change in work arrangements to establish two novel facts: first, when given the possibility to change work arrangements, Australian mothers choose schedule regularity (which can be considered a form of flexibility, in Goldin, 2014's definition of the term that both these papers build on); second, that there is indeed a causal link between regularity and the size of the child penalty.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the data. Section 3 shows that the Fair Work Act shifted some work arrangements (schedule regularity) but not others (flexibility). Section 4 formally defines the child penalty, our main object of interest, and documents its evolution over the last twenty years in Australia, showing a discontinuous decrease around 2009. Section 5 ties together the previous two sections, by showing that the decrease in child penalty is entirely concentrated in the group of mothers in occupations most exposed to the changes in regularity brought by the Fair Work Act. Section 6 rules out the role of Paid Parental Leave as a potential confounder, and Section 7 concludes.

2 Background and Setting

The Australian 2009 Fair Work Act (FWA) entitled parents of young children to request alternative work arrangements. Work arrangements are a multidimensional concept, spanning from the possibility of working from home to the definition of the working schedule, and more. To be more precise, by work arrangements we indicate the job characteristics that define when and where a job is expected to be performed. The Fair Work Act represents a quasi-exogenous shift in ability to change work arrangements, which we will leverage both to study which work arrangements mothers seek to change, and to make causal statements on the link between work arrangements and the child penalty.

2.1 Institutions: the Fair Work Act (2009)

The Australian 2009 Fair Work Act establishes, among other things, that parents of children under school age are entitled to request a “change in working arrangements” and employers can refuse only “on reasonable business ground” (Fair Work Act 2009, Act No. 28, Section 65). The legislator provides examples of the requests that can be made under the scope of this law, such as the adoption of an 8-to-4 workday as opposed to a 9-to-5

one, or a split of the 35 hours per week in 4 days rather than 5⁹. However, the language is quite general, and does not restrict to a specific type of working arrangement: Section 3 of this paper studies precisely *which* work arrangements were impacted by the passage of this law. Examples of “reasonable business ground” that can lead to denied requests include the requested arrangements being too costly or that other employees’ work arrangements cannot be changed to accommodate the request.¹⁰

This law, which has been fairly salient and present in the public discourse,¹¹ includes several provisions aimed at making work “more fair”, but the greatest majority of them apply to all workers and not just to parents. In our analysis, we leverage the parents-non parents comparison to control for more general labor market impacts of this legislation. The other provisions include: defining 38 hours a week the maximum weekly hours, above which an employee has to be paid overtime pay; establishing an entitlement of 4 weeks per year of paid annual leave; setting a minimum number of days of notice before firing. The law also extends *unpaid* parental leave from 12 to 24 months, but take-up for this provision was low (O’Neill, 2012), and, if anything, we would expect it to play against our results (Olivetti and Petrongolo, 2017). Paid Parental Leave, which was introduced the following year, is discussed in detail in Section 6.

With the Fair Work Amendment Act of 2013, the right to request a change in work arrangements was extended to parents of all school-aged children – the flexible specification we employ in our analysis will account for this.

2.2 Data: HILDA

The Household, Income and Labour Dynamics in Australia (HILDA) Survey covers a representative panel of the Australian population and combines information on family and work, including hours worked as well as workplace entitlements. This rich dataset allows us to identify people who become parents as well as their occupations (and hence their exposure to the Fair Work Act) and their labor supply around childbirth and over time.

HILDA is a household-based ongoing longitudinal survey, started in 2001, designed to be representative of the Australian population. For each of the approximately 8,000 sampled household, everyone above 15 years old is interviewed, but basic demographics are also collected for younger members of the household. This gives us information on family structure, arrival of new babies, and partners (both married couples and *de facto* partners). At the time of the analysis, data up to 2019 are available to researchers – hence our sample covers the years 2001-2019.

Variables Construction HILDA asks a variety of questions on workplace entitlements, job characteristics, scheduling, and job location that we can use to measure work arrangements. We primarily focus on three types of work arrangements: schedule regularity, flex-

⁹Examples from the government-run Fair Work website: <https://www.fairwork.gov.au/tools-and-resources/best-practice-guides/flexible-working-arrangements>

¹⁰Fair Work Website, see footnote 9

¹¹According to Google searches, interest for this law is substantially larger than that for other labor-related queries such as “Trade Unions”, “Unfair dismissal”, or “Disability Discrimination Act” – see Appendix Figure A.1.

ibility, and work from home. Our main measure of regularity is called “Regular schedule” and is a dummy equal to one if the respondent has a Monday to Friday weekly schedule and a regular daytime schedule. We occasionally refer separately to the variables “Regular weekly schedule (Monday to Friday)” and “Regular daytime schedule”, which jointly determine our main measure; and to “On call”, which is a dummy equal to one if the respondent reports working on call or irregular shifts. We measure flexibility with two different variables: the first is a dummy indicating agreement with the statement “My working times can be flexible”, the other one is a dummy for whether the employee reports being entitled to flexible start and finish times. Finally, respondents are asked how many hours a week they work from home and also whether they would be entitled to home-based work if they wanted it, and we use both measures. Details on variable construction can be found in Appendix B.

Sample Restrictions We restrict our sample to people aged 15 to 65. Since our analysis focuses on how outcomes evolve around childbirth, from Section 4 onward we only keep parents that have their first child between 2003 and 2017, in order to observe at least two years before and after childbirth. For these parents, we only keep observations in the range [-6, +10] years relative to the first childbirth, in line with Kleven et al. (2019). In studying the effect of the Fair Work Act on the child penalty, when we compare early and late cohorts (non-treated and treated by the Fair Work Act, respectively), we exclude the cohort of parents who have their first child exactly in 2009, since this cohort is partially treated. We keep non-parents and individuals who don’t have a first child during the sample years, and they act as controls in all our specifications.

Descriptive Statistics Table 1 shows the basic summary statistics, for our entire sample, split by gender. Table 2 zooms in to will-be mothers, and presents a snapshot two years before childbirth. Appendix Table A.1 shows the distribution of working mothers, again two years before childbirth, across the different occupations and industries.

Australian mothers in our sample have a fairly strong labor market presence before childbirth: two years before childbirth, 93% of them are in the labor force, working roughly 39 hours per week. Among the employed, 20% of them work part time, but almost half of them work over the standard full time amount of 38 hours per week and roughly 10% over 50 hours per week. More than 70% of those who work have a permanent contract (as opposed to being a casual or fixed-term employee); about half of them are in supervisory roles and 18% of them received a promotion in the previous year. Along all of these dimensions, will-be mothers are roughly comparable to men in the full sample.¹² In Section 4, we will see that all these numbers change dramatically once they have their first child.

Will-be mothers are on average more educated than the full sample, work on slightly more predictable schedules and have marginally less flexible jobs. 38% of will-be mothers have a bachelor degree, as opposed to 27% for the full sample of women and 22% for the full sample of men. Roughly 60% of women work on a regular Monday to Friday daytime

¹²The most notable differences are that men in the full sample are twice as likely to work more than 50 hours a week, and half as likely to be promoted – some of it could also be driven by the different age composition of will-be mothers versus the full sample of men.

schedule two years before their first childbirth, while on average 42% of women and 53% of men do, and only 6% of will-be mothers reports working on call and irregular shifts, while this figure is 10% for both men and women in the full sample. While about 40% of working women agree with the statement “My working times can be flexible” two years before becoming mothers, this is lower than the 50% figure for the full sample.

3 The Fair Work Act (2009) Shifted Work Arrangements

In this section, we show that the Fair Work Act did have bite, and in a specific way: it increased schedule regularity, but not flexibility, for a subset of treated mothers; thus representing an exogenous shift in a specific working arrangement we will leverage in subsequent analysis. We also show that the law did not observably affect fertility or selection into motherhood, thus supporting the assumption that women becoming mothers before and after the law can be meaningfully compared.

3.1 The Effect of the Fair Work Act on Work Arrangements

3.1.1 Empirical Strategy

Given the wording of the law, the most natural way to study the effect of the 2009 Fair Work Act is through a standard difference-in-differences design, but we want to allow for treatment effect heterogeneity. The Fair Work Act entitles parents of children under school age to request a change in work arrangements, and thus the most natural specification would be one that includes all parents of children who are younger than five in the treated group (after 2009) and everybody else, both non-parents and parents of older children, in the control group. However, we have the prior that the law would impact different demographics differently: for example, if women are those who disproportionately care for the children, a law which is explicitly aimed at improving work-family balance¹³ might disproportionately affect them. Similarly, we might hold the prior of path dependence in maternal labor supply: we could think that labor supply choices are made within the first year from the birth of the first child, and then re-optimized only in case of large changes to the environment. In our main specification we allow for enough flexibility to account and test for those priors.

We run the following specification, separately by gender:

$$Y_{it} = \sum_{a \in A} \left(\sum_{j \neq 2009} \beta_{FC_{a,j}} \times \mathbb{1}\{FC(i,t) \in a\} \times \mathbb{1}\{t = j\} + \beta_{FC_a} \times \mathbb{1}\{FC(i,t) \in a\} \right) + \\ + \sum_{a \in A} \left(\sum_{j \neq 2009} \beta_{YC_{a,j}} \times \mathbb{1}\{YC(i,t) \in a\} \times \mathbb{1}\{t = j\} + \beta_{YC_a} \times \mathbb{1}\{YC(i,t) \in a\} \right) + \quad (1) \\ + \alpha_i + \delta_t + \gamma_{h(i)} + \epsilon_{it}$$

¹³Section 3 of the Law describes its objective as “to provide a balanced framework for cooperative and productive workplace relations that promotes national economic prosperity and social inclusion for all Australians by [...] assisting employees to balance their work and family responsibilities by providing for flexible working arrangements”

where Y_{it} is the outcome of interest for individual i at time t , $\mathbb{1}\{FC(i, t) \in a\}$ is an indicator function that takes the value of one if i 's *first* (oldest) child in year t is in age range a , and $\mathbb{1}\{YC(i, t) \in a\}$ is an analogous function but based on the age of the *youngest* child. The age ranges $a \in A$ used in the estimation are 0-2, 3-5, 6-8, 9-10, 10+. $\mathbb{1}\{t = j\}$ is an indicator function for the year being j , and $\alpha_i, \delta_t, \gamma_{h(i)}$ are individual, time and age fixed effects respectively. The coefficients of interest are the $\beta_{FC_{a,j}}$'s and the $\beta_{YC_{a,j}}$'s, which tell us how differently at any point in time parents of first/youngest children in age range a behave from analogous parents in 2009 (controlling for individual, time and age of the parent fixed effects).

The underlying assumption for the validity of this analysis is that individuals who choose to become parents at different points over the sample period are comparable: we show evidence in support of this claim for mothers (on whom our analysis focuses) in Section 3.2.

3.1.2 Results

Regularity Increases for Mothers The main result of the strategy just described is summarized in Figure 1. In this figure, we plot the estimated $\beta_{FC_{a,j}}$'s and $\beta_{YC_{a,j}}$'s from equation (1) where the outcome variable is “Regular Schedule”, a dummy for working from Monday to Friday on a regular daytime schedule¹⁴, and in which we restrict the sample to only women. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$'s, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$'s, for child's age ranges 0-2, 3-5, 6-8. The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and so on).

The fraction of mothers on regular schedules increases substantially after 2009, but with considerable heterogeneity according to age and parity of the child. From the first panel in Figure 1 we learn that, in 2009, 26% of working mothers who had given birth to their oldest child in the previous two years worked on a regular schedule, and this figure is similar for new mothers of 0-2 year old children in the preceding years. Starting in 2010, this fraction starts increasing by 10 and up 20 percentage points, constantly remaining at a higher level afterwards. In the second panel of the top row, where we display coefficients on the dummy for the oldest child being between 3 and 5 years old, we notice a very similar pattern, but with the increase starting after 2013, the year in which the first children born after the passage of the Fair Work Act turned three. Remarkably, in the third panel, which displays coefficients on the dummy for the oldest child being between 6 and 8 years old, a series of estimated zeros is followed by a sharp and permanent increase, but only after 2016, when the first post-Fair Work Act children turned six.

While we observe an increase in schedule regularity for mothers whose first child was born after the passage of the Fair Work Act, this does not appear to be the case when we split mothers based on the age of the youngest child. The bottom panels in Figure 1 mirror the top panels, but they display the estimated coefficients on the dummies based on the age of the youngest child. In all three panels, all the coefficients estimated are statistically

¹⁴More details on variable construction are in Section 2.2 and Appendix B.

indistinguishable from zero at all horizons.

Regularity Increases for Mothers, Flexibility Does Not and Working From Home Is Unclear Informed by the patterns just described, and in order to gain precision and facilitate comparisons, we estimate a restricted version of equation (1), where we only include the dummies based on the age of the first child, and we replace the year-by-year interactions with mobile “post” dummies. In particular, we interact the dummy for the first child being between 0 and 2 with an indicator for the year being equal or after 2010; the dummy for the first child being between 3 and 5 with an indicator for the year being equal or after 2013; the dummy for the first child being between 6 and 8 with an indicator for the year being equal or after 2016. These mobile “post” dummies are meant to capture the point at which mothers whose first child was born after the passage Fair Work Act start entering the indicated group, in line with the evidence presented above.

The first four columns of Table 3 confirm that the result on regularity holds regardless of how we define it: whether we define it as Regular schedule (column 1), as either of its two components - Monday to Friday schedule (column 2) or Regular daytime schedule (column 3), or as the inverse of being on call (column 4). The table also allows us to gauge magnitudes: after the passage of the law, “Regular schedule” increases by almost 40% among mothers whose first child is between 0-2 years old relative to their earlier counterpart, driven both by an increase in Monday to Friday scheduling by 33%, and increase in regular daytime schedule by 10% .

Flexibility, which was explicitly targeted by the Fair Work Act, does not appear to have been affected by it, at least on average. Columns 5 and 6 of Table 3, which report the coefficient on the restricted specification above for the outcome variables indicating agreement with the statements “My working times can be flexible” (column 5) and “I am entitled to flexible start and finish times” (column 6), display coefficients that are small and mostly not significantly different from zero. Appendix Figure A.2, which replicates Figure 1 but using agreement with the statement “My working times can be flexible” as outcome variable, confirms that this estimated zero is not an artifact of the restricted specification, and that flexibility does not seem to be changing for mothers of young children around the passage of the Fair Work Act.

The analysis reveals a mixed picture regarding work from home. The entitlement to be working from home – which is the only aspect that the law can directly target – didn’t significantly change (column 10 of Table 3). Actual hours worked from home (column 7) increased by about 0.5-1 hours per week, with differing combinations of changes on the extensive margin (fraction of women doing any work from home, column 8) and on the intensive margin (hours worked from home conditional on working from home at all, column 9) across child’s age group. Appendix Figure A.3 makes this mixed picture visually clear: not much seems to be happening to mothers of a 0-2 year old child in terms of hours worked from home; mothers of whose first child is between 3 and 5 do experience an increase in hours worked from home, after 2013, but it is noisy and short-lived; mothers of 6-8 year old first children follow the same pattern, but shifted by 3 years. So we cannot say with confidence whether the Fair Work Act impacted work from home.

No Working Arrangement Changes for Fathers We replicate the same analysis on men, and show that nothing seems to be happening for fathers of young children around 2009. Appendix Figure A.4 replicates Figure 1 for regularity on the sample of men. We notice that a much larger percentage of working fathers than of mothers tend to have a regular schedule (60%), and that this fraction does not show any systematic changes, neither when we compare fathers based on the age of their first child (top row), nor when we do it based on the age of their youngest child (bottom row). All panels show long lines of estimated zeros at all horizons. Appendix Table A.2, which replicates Table 3 on the sample of men, shows that nothing changes for fathers around 2009 also with respect of the other work arrangements considered.

Interpretation We interpret the evidence as supporting a few key facts, which inform the subsequent analysis. First, the Fair Work Act has had a statistically significant impact only on the work arrangements of mothers, and none on those of fathers. This is consistent with government reports, which indicate that the greatest majority of requests made under the Fair Work Act come from women (76% of them, according to survey evidence in O'Neill, 2012). This insight, together with evidence we will present in Section 4.1 of the child penalty being a female-only issue, motivates our focus on women. Secondly, we show that the entirety of the effect is concentrated among women whose first child was born after the passage of the law, and was persistent. This suggests the following pattern: women rearrange their working life and re-optimize soon after they become mothers, based on the constraints they face in that moment, and this arrangements are sticky; so that mothers of three year old children, although technically treated by the law, did not change their arrangements when the Fair Work Act was introduced. This supports our choice in subsequent analysis of defining treatment and exposure to the law based on the year of birth of the first child.

3.2 The Fair Work Act Did Not Observably Alter Fertility or Selection Into Parenthood

In order to support the assumption that early and late cohorts of mothers (women becoming mothers before and after the passage of the Fair Work Act) can be meaningfully compared, we show that the fraction of women having their first child each year (fertility rate) did not change around 2009, and that the two groups of mothers are similar on observable characteristics measured two years before childbirth.

Australian women did not become mothers at different rates around 2009. Figure 2 plots the fraction of Australian women in our sample (aged between 15 and 65) who give birth to their first child by year, and superimposes best-fit lines separately before and after 2009. Although there is a slight upward trend in first births, it does not seem to change systematically around 2009, supporting the claim that the law did not have a direct impact on rates of first births. The graph looks very similar if we restrict the sample to women aged 20 to 45 (although the range of the y-axis understandably changes).

Women who became mothers before and after 2009 were not observationally different when compared before childbirth. Table 2 presents means and standard deviations of

key labor market variables for mothers who had their first child between 2003 and 2008 (early cohorts) and for mothers who had their first child between 2010 and 2017 (late cohorts), measured two years before their first childbirth. The last two columns present the between-groups difference, and standard indications for whether the difference is significantly different from zero. None of the variables displays a significant difference across the two groups, except for year of first birth (by construction), and wage and income variables, which is explained by inflation. In Appendix Table A.1 we show that “early” and “late” mothers were also working in similar industries and occupations before childbirth.

4 The Child Penalty in Labor Supply Decreased Following the Fair Work Act

In this section we define and compute the child penalty in labor supply, the change in labor supply following the birth of the first child relative to before, and show that it is large and negative for women and close to null for men. We further show that the child penalty in labor supply for women changed discontinuously around the passage of the Fair Work Act, in the direction of greater labor supply, suggesting a role of the law on maternal labor supply choices; in the next section we will show further evidence that this link is causal.

4.1 The Child Penalty at Baseline

4.1.1 Definition

The child penalty is defined as the distance between actual outcomes of parents and the counterfactual that would have realized absent childbirth. More specifically, we define as “child penalty in Y , k years since childbirth” the estimates of coefficients γ_k from the following event study regression:

$$Y_{it} = \alpha_i + \delta_t + \beta_{h(i)} + \sum_{\substack{k=-5, \\ k \neq -2}}^{10} \left\{ \gamma_k \times \mathbb{1}\{t - E_i = k\} \right\} + \epsilon_{it}, \quad (2)$$

where E_i represents the year of birth of i 's first child, and α_i , δ_t , and $\beta_{h(i)}$ are individual, time, and age fixed effects respectively. The coefficients of interest are the γ_k 's, representing the effect on the outcome Y of being k periods from childbirth. Never-parents and individuals who don't have their first child in our sample years are kept in the sample and act as controls. In order to account for heterogeneous treatment effects across cohorts, we estimate the equation above using the Sun and Abraham (2020) estimator. We cluster standard errors at the individual level.

4.1.2 The Child Penalty in Labor Market Outcomes by Gender

The child penalty, as defined above, is gender neutral, and can be computed for any outcome of interest: here we focus on labor market outcomes, and we compute the child penalties separately by gender. Here we want to present baseline child penalties, net of

any potential effect of the Fair Work Act, and hence we only estimate it on parents whose first child was born before 2009.

Labor Supply and Earnings Having a child is followed by a very large drop in labor supply and labor earnings for mothers, but by very little change for fathers. Figure 3 shows the estimates of the γ_k 's from equation (2), in stereotypical blue for men and pink for women. Panel (a) shows that on the year of their first childbirth, women are 52 percentage points less likely to work, relatively to two years earlier (when 93% of them were working). Labor force participation recovers in the year following childbirth, but never fully: ten years after becoming mothers, women are still 30 percentage points less likely to be in the labor force, relatively to two years before childbirth and to non-parents. Even mothers who do remain in the labor force experience a large drop in labor supply: intensive-margin labor supply falls by 19 hours of work per week, among those who remain employed, and never recovers (panel (b)). The log of weekly hours spent in employment permanently drops by 0.8 (panel (a) in Appendix Figure A.5). The drop in labor supply corresponds to a proportional drop in labor income (panel (d)), given that hourly wages don't show systematic changes (panel (c)).¹⁵ None of this happens to fathers, for whom the child penalty is tiny at all horizons – the point estimates are very small in magnitude (the post-chilbirth drop is of about 2 percentage points on the extensive margin, an about 1 hour a week on the intensive margin labor supply), and most confidence intervals include zero, for all labor market outcomes considered.

This baseline analysis showing large child penalties for women in all labor market outcomes, and none for men, further motivates the focus on women in the rest of the paper.

A small aside on wording. The post-birth changes in labor supply that we estimate for women are large and negative; hence, according to the definition given above we should say that the child penalty in labor supply for women is negative. However, the word has always been used with the opposite meaning – “we observe a large child penalty in earnings for women” generally means that we observe a large *drop* in earnings after childbirth. We always try to be as specific as possible, but when comparing child penalties across groups this might result in awkward writing. In these instances, we will use “child penalty” in labor supply and earnings with the meaning it commonly has – to indicate a drop in labor supply and earnings.

Other Labor Market Outcomes Child penalties in labor supply and earnings are a good summary of the main labor market changes faced by women as they become mothers, but our data allows us to paint a much richer picture. Appendix Figure A.5 expands on Figure

¹⁵Hourly wage is computed as the ratio of weekly wage and weekly hours. Given the wording and placement in the survey of the questions priming these variables, there might be a mismatch in the reference week. This is unlikely to be a problem in general, but could be a problem the year of childbirth, when hours of work might approach zero, but the mother might still be receiving payments for work carried out in the past, or she might interpret maternity payments as wages. This is probably the cause for the spike in hourly wage we observe for women the year of first childbirth. As such, we interpret it as noise. We discuss this variable further in Appendix B.

3 by including other labor market outcomes; panel A of Appendix Table A.5 summarizes the child penalties for women in a variety of labor market outcomes. From these exhibits, we learn that the observed drop in hours of work is driven by a *four-fold* increase in the fraction of women working part-time – an increase by more than 60 percentage points, off a pre-childbirth base of 22% – and by a zeroing of overtime work, both if measured as working more than the government-defined standard of 38 hours/week (a drop by about 40 percentage points, off a base of 44%) and if measured as working more than 50 hours/week (a drop by more than 10 percentage points, off a base of 11%).

Also the type of contract and the type of roles change. While in the years before childbirth, more than 70% of working women have a permanent contract, this figure drops by 20 and up to 30 percentage points in the years following the birth of the first child: even among the women who do retain employment, many of them switch from permanent to casual and fixed-term employment, and the fraction of women who do the switch increases as the child grows up (rather than stabilizing around year one, as most of the other labor market outcomes do). After becoming mothers, women are 30% less likely to be in a supervisory role (drop of about 15 pp, off a base of 50%) and half as likely to be promoted (drop of about 8 pp, off a base of 16%).

Child Penalty in Work Arrangements and Job Characteristics Many work arrangements change for women when they become mothers. Note that here we are doing an exercise that is different from what we presented in Section 3: there, we were discussing how mothers in certain years compared to mothers of similarly-aged children in previous years. Here, we are discussing how mother's work arrangements compare to those they themselves had before their first childbirth; focusing only on women who became mothers before 2009. Panel A of Appendix Table A.6 shows that mothers are substantially less likely to be in jobs with a regular schedule than their pre-motherhood selves (columns 1-4), that they are more likely to agree with the statement “my working times can be flexible” (column 5), and that they are more likely to work from home but for fewer hours, so that on average the number of hours worked from home doesn't change (column 7-9). The child penalty in some of these arrangements was affected by the Fair Work Act - panels B offers a preview, which we will discuss more in detail in later sections.

Mothers tend to work in more flexible jobs than their pre-motherhood selves, when we define job flexibility based on Goldin (2014). Panel A in Appendix Table A.7 displays the child penalty in different measures of inflexibility. The outcomes in columns 1 through 4 are variables based on the O*NET characteristics that underlie Goldin's measure of inflexibility: Time pressure, Importance of relationship, Unstructured work, Freedom to make decisions. “Inflexible job” in column 5 is the index of inflexibility that averages these four measures. We transpose these variables to HILDA using questions that get at these dimensions, in order to maintain time and within-occupation variation, but if we merge in Goldin's original measure from O*NET through occupation we get a similar picture. Appendix B describes the construction of these variables in detail; all the variables are measured in z-scores. All these measures of inflexibility of one's job decrease after the birth of the first child, suggesting that mothers make their jobs more flexible when they have a child to care for.

Focusing on specific job characteristics of “Freedom in decision making” and “Time pressure, complex and stressful job”, as identified via principal component analysis, confirm the same story. The outcomes in columns 6 and 7 of Appendix Table A.7 are the first two components from carrying out Principal Component Analysis on the very large set of job characteristics we can observe. They are both described in Appendix B, and they are both measures in z-scores. “Freedom in decision making”, in particular, is associated with managerial roles, while “Time pressure, complex and stressful job” is distributed similarly across different occupations. After the birth of their first child, women’s job are characterized by one third standard deviation less freedom in decision making, and two thirds standard deviation less time pressure and stress.

A large part of the observed changes after motherhood come from within-job changes, but the across-job changes (arrangements and characteristics varying after childbirth relative to pre from changing job) are non-trivial, especially along certain dimensions. In Table A.8, we proxy jobs by occupation-by-industry-by-education cells (since we can’t directly observe employer) and compute averages of the variables we are interested in studying by these cells. We then assign individuals these job averages, and compute the child penalty using as outcome these averages. While computing the child penalty directly on a given variable recovers a combination of across and within job changes, this exercise isolates the change coming exclusively from changing job: by comparing the two, we can learn how much each component explains.

From comparing Table A.8 with tables A.6 and A.7, we learn that the largest part of the child penalty in work arrangements and job characteristics comes from within-job changes, and only a small fraction from actually changing occupation and/or industry, but this differs according to the specific variable under examination. For example, in column 1 we see that there is some decrease in regularity coming from moving to less predictable occupations and industries, but the estimated change from changing job is only about one tenth than the overall estimated child penalty in regularity. At the other end of the spectrum, for “Freedom in decision making” – usually associated with managerial occupations, about two thirds of the observed child penalty come from changing occupation or industry.

4.2 The Reduction in Child Penalty Following the Fair Work Act (2009)

Here we show that the child penalty in labor supply for mothers changed discontinuously after the passage of the Fair Work Act, in the direction of greater labor supply of new mothers. We do it in two ways: first, we compare the child penalty of early and late cohorts of mothers (who had their first child before and after 2009, respectively), and show that late cohorts of mothers experience a smaller child penalty in labor supply. Second, we compute the child penalty by year of birth of the first child, and we show that the change is sharp around 2009, rather than reflecting a time trend, and that this result is robust to permutation inference.

4.2.1 Early vs Late Cohorts: Late Cohorts Experienced a Smaller Child Penalty

Empirical Strategy We start by estimating the child penalty for each year relative to childbirth, separately for early and late cohorts of mothers. In particular, we estimate

equation (2) separately for the two groups, keeping the non-mothers as controls in both, and we plot the resulting estimates in the same graph, to visually see the difference.

We also estimate the child penalty for the two groups of cohorts jointly, in a more succinct specification, in order to formally test for their difference. In this specification, we group years relative to childbirth in 0 to 5 and 6 to 10: this preserves power, and it is justified in light of the previous results in Figures 3 and 4, which show great persistence in the child penalty for several years after the birth of the first child. Formally, we estimate the following model:

$$\begin{aligned}
 Y_{it} = & \alpha_i + \delta_t + \beta_{h(i)} + \gamma_{0-5} \times \mathbb{1}\{(t - E_i) \in [0, 5]\} + \\
 & + \gamma_{6-10} \times \mathbb{1}\{(t - E_i) \in [6, 10]\} + \\
 & + \gamma_{0-5,post} \times \mathbb{1}\{(t - E_i) \in [0, 5]\} \times \mathbb{1}\{E_i > 2009\} + \\
 & + \gamma_{6-10,post} \times \mathbb{1}\{(t - E_i) \in [6, 10]\} \times \mathbb{1}\{E_i > 2009\} + \epsilon_{it}
 \end{aligned} \tag{3}$$

where $\mathbb{1}\{(t - E_i) \in [0, 5]\}$ is an indicator function for i 's first child being between 0 and 5 years old, $\mathbb{1}\{(t - E_i) \in [6, 10]\}$ is an indicator function for i 's first child being between 6 and 10 years old, and $\mathbb{1}\{E_i > 2009\}$ is an indicator function taking value of one if i 's first child was born after 2009 – meaning, an indicator for i being part of the late cohorts. The 2009 cohort is partially treated, and we exclude it from this analysis. In this specification, the estimates of γ_{0-5} and γ_{6-10} summarize the child penalty in Y for the early cohorts, which we have discussed in Section 4.1; the object of interest and discussion in this section are $\gamma_{0-5,post}$ and $\gamma_{6-10,post}$, the differences in child penalty for late cohorts relative to early cohorts.

Main Outcomes: Labor Supply and Earnings The year-on-year child penalties in labor supply for late cohorts display a parallel vertical shift upwards relative to that of early cohorts, in the direction of greater labor supply (smaller child penalties); this corresponds to a proportional smaller drop in earnings. We can see it in Figure 4, where green circles display the estimates of γ_k 's from equation (2) estimated only on mothers whose first child was born between 2005 and 2008 (and non-mothers, as controls), and red squares display the same estimates but on the sample of mothers whose first child was born between 2010 and 2013. We show the results of formally testing for the difference in Table 4, where we report the coefficient estimates from equation (3). In this section, “panels” refers to Figure 4 and “columns” refer to Table 4.

In panel (a) we observe that later cohorts of women experience a drop in labor force participation upon childbirth similar to the one of the earlier cohorts, but recover faster and by a greater amount: on average, in the first five years after the birth of their first child, the drop in labor force participation is 7 percentage points smaller for later cohorts relative to earlier cohorts (corresponding to a 18% lower drop), and even in years 6 to 10 from the first childbirth, the gap between the cohorts is still of about 5 percentage points, suggesting persistence (although for longer horizons the coefficients are less precisely estimated). The precise numbers are in column 1.

On the intensive margin labor supply, the parallel vertical shift is even starker than on the extensive margin, as we can see from panel (b) and columns 2 and 3. The drop in weekly hours of work following childbirth for later cohorts is constant at about 15

hours/week in the first ten years from the birth of the child, a drop which is three hours smaller (18% smaller) than for earlier cohorts. In log terms, the drop for later cohorts is on average 0.15 smaller than the 0.8 drop of early cohorts. As for early cohorts, also for late cohorts hourly wages are not affected by childbirth (panel (c)), and hence the smaller drop in labor earnings faced by the late cohorts of mothers is proportional to the smaller drop in labor supply (panel (d)).

Changes to Child Penalty in Work Arrangements In Section 3 we described how the Fair Work Act changed certain work arrangements for the mothers of young children, but only for mothers whose kids were born after the passage of the law. The same analysis can be recast as changes in the child penalty in these work arrangements: many work arrangements change systematically for women when they become mothers, but for women that became mothers after 2009 some of them changed systematically less relative to earlier cohorts of mothers. More specifically: Appendix Table A.11 shows that when women become mothers, they move to less regular jobs (a decrease by 44 percentage points in the fraction of women working Monday to Friday, regular daytime schedule, for early cohorts), but this is systematically less true for later cohorts (36 percentage point, a 19% change), which is driven both by fewer women switching out of Monday to Friday jobs, and fewer women switching out from a regular daytime schedule and into on call and irregular shifts.

Other work arrangements do change when women have children, but were not impacted by the Fair Work Act; or the other way around. Flexibility, as measured by agreement with the question “My working times can be flexible”, increases by 16 percentage points when a woman becomes mother (off a base of 41%), but this is not systematically different between early and late cohorts. Average number of hours worked from home does not systematically change when women become mothers in the early cohorts, but it noisily increases by half hour among later cohorts (though the difference across cohorts is only significant at the 10% level). This confirms the results discussed in Section 3.

The direct measures of inflexibility do not seem to change systematically, but there is some mild evidence that the Fair Work Act brought with it fewer job switches. In Appendix Table A.13, recall that the top two rows estimate the early-cohorts child penalty in a given variable from moving occupation: for example, in early cohorts mothers experience a drop in “Freedom in decision making” by 0.2 standard deviation simply from moving to an occupation or industry with a lower average value of this variable (in addition to a within-job component, which goes in the same direction). The next two rows estimate the difference in child penalty for late relative to early cohorts. All the coefficients for the differences are positive: given that the baseline coefficients are negative, this indicates fewer job switches. For some variables the estimated difference between early and late cohorts is not significant, but for “Inflexible job” and “Freedom in decision making” it is. This is particularly notable because “Freedom in decision making” is high among more prestigious occupations, such as managers; and this indicates that, after 2009, fewer mothers are switching out from these prestigious occupations.

4.2.2 It Is Not Just a Time Trend: The Discontinuous Change in 2009

While the results just presented show that the child penalties along various labor market margins are systematically different between early and late cohorts of mothers, they could just be capturing a time trend. Here we show that, instead, we observe a sharp change, to an otherwise pretty stable pattern of child penalties, precisely around 2009.

Empirical Strategy Specifically, we expand on equation (3) by estimating a separate coefficient by cohort:

$$Y_{it} = \alpha_i + \delta_t + \beta_{h(i)} + \sum_c \left\{ \gamma_{0-5,c} \times \mathbb{1}\{(t - E_i) \in [0, 5]\} \times \mathbb{1}\{c = cohort(i)\} + \right. \\ \left. + \gamma_{6-10,c} \times \mathbb{1}\{(t - E_i) \in [6, 10]\} \times \mathbb{1}\{c = cohort(i)\} \right\} + \epsilon_{it} \quad (4)$$

where $cohort(i)$ is the cohort i belongs to. In our preferred specification, it corresponds to E_i (year of first childbirth); for certain outcomes we might define cohorts based on pairs of years of childbirth to preserve power. Here, $\gamma_{0-5,c}$ is the 0-5 year child penalty (meaning, the average child penalty in the first five years from the birth of the first child) for mothers belonging to cohort c . The sequence of these coefficients shows us the evolution of the child penalty.

We also estimate a more restrictive model, in which we assume that child penalties follow a linear trend based on the year of first childbirth, but allowing slope and intercept to differ between early and late cohorts. In the graphs, we superimpose it on the coefficient estimates from the previous exercise, as it is helpful to parse trends for some particularly noisy outcomes.

Main Outcomes: The Evolution of the Child Penalty in Labor Supply and Earnings
Figure 5 shows that the child penalties in labor supply, earnings, and other labor market outcomes are relatively stable across early cohorts and across late cohorts of mothers, but that there was a discontinuous jump in child penalty in 2009, in the direction of greater labor supply and labor earnings. The graphs read as follows: year of first childbirth is on the x-axis, and the corresponding $\gamma_{0-5,c}$, from estimating equation (4), is on the y-axis. The straight lines come from the restricted model.

Earlier in this section we showed that the child penalty in labor force participation, in the first five years after childbirth, is 18% lower for post-2009 relative to pre-2009 first time mothers. Panel (a) of Figure 5 shows that this estimated decrease in the child penalty in labor force participation is not driven by a slow improvement over time, but rather by a sharp change in 2009: if anything, the child penalty in labor force participation was growing *larger* in the years leading up to 2009. For example, the graph shows that for women whose first child was born in 2003, labor force participation drops by 37 percentage points in the first five years after childbirth: this figure is 38 percentage points for women who had their first child in 2005, and 41 pp for women who became mothers in 2008 (39 pp is the pre-2009 average). Instead, women who become mothers in 2010 or 2011 experience a much smaller drop in labor force participation by 30 percentage point, and the figure remains similar for first-time mothers in subsequent years.

The pattern looks even more striking when the outcome is labor supply on the intensive margin (panel (b)): the child penalty in working hours increases in magnitude for the birthing cohorts 2003 to 2008, before discontinuously decreasing after 2009. The figure shows that a woman experiences a drop in hours of work in the five years after childbirth by 16 hours per week if her first child is born in 2003, by 17 if born in 2004 and by 18 if born in 2008 (18 hours/week is the pre-2009 average), but the child penalty in weekly hours of work is 15 hours per week if a woman becomes mother in 2010 or 2012. The sharp change is even more evident in logs (in Appendix Figure A.7). Consistent with the evidence presented above, the child penalty in hourly wages (panel (c)) does not show any systematic pattern, and all confidence intervals include zero: it is therefore unsurprising that the pattern of child penalties in earnings (panel (d)) closely follows that of labor supply.

Other Labor Market Outcomes This exercise is quite revealing also when looking at other outcomes, and helps painting a nuanced picture. Panel (c) in Appendix Figure A.7 shows that the fraction of working women moving to part-time after becoming mothers is increasing in the years between 2003 and 2008, and it starts decreasing after 2009, although not in a sharp manner. This mirrors the hours margin in the first half of the sample, but not in the second half, when also other factors must have been at play. Panel (d) shows a fairly sharp result for what concerns the type of contract: the fraction of working women who move from a permanent position to casual or fixed term employment is fairly stable, around 23 percentage points, for mothers who give birth to their first child between 2003 and 2008, but it drops to about 10 percentage points for mothers who give birth in 2010 and subsequent years.

Persistence In earlier sections we have suggested the hypothesis that a woman's labor supply choices are optimized soon after the birth of her first child, usually within the first year or two, and they tend to be sticky afterwards; here we want to bring some evidence supporting it. First, the event-study graphs shown earlier, in Figure 4, lend support to this hypothesis: for both early and late cohorts, child penalties two or three years after the birth of the first child are very similar to child penalties seven or eight years after childbirth (even though they are quite different between cohorts). Second, in Appendix Figure A.8, we confirm this in a more systematic way. We plot the 0-5 year child penalties in the main labor market outcomes by year of birth of first child (the estimates of the $\gamma_{0-5,c}$'s from equation (4)), as in Figure 5, along with the 6-10 year child penalties (the estimates of the $\gamma_{6-10,c}$'s), meaning the average difference in a given outcome when the first child is between six and ten years old, relative to before childbirth.

The figures support the hypothesis of persistence, pretty strikingly. In panel (a) we see that, although on average the child penalty in labor force participation when the oldest child is between 6 and 10 (in orange) is 8 percentage points lower than when the child is between 0 and 5 (in blue; labor force participation is 8 pp higher when the kid is older), the former very closely tracks the latter, including its ups and down. The child penalty in hours of work conditional on working is extremely persistent: in panel (b), the orange line of estimates of the 6-10 year child penalty perfectly overlaps the blue line of estimates of

the 0-5 year child penalty; and both show a sharp increase in labor supply for post-2009 first births. Panel (c) confirms that also at longer horizons the child penalty in hourly wages is zero.¹⁶ Finally, like in the rest of previous analysis, in panel (d) we see that the child penalty in earnings closely tracks the child penalty in labor supply, in both pattern and persistence.

4.2.3 Robustness Check: Permutation Test

In order to formally test for whether the changes we observe in the child penalty happen indeed around 2009, we perform a permutation test based on Ganong and Jäger (2018). The idea behind this test is simple: re-estimate the basic model “pretending” that the structural break in the time-series of child penalties happened in different years, and compare the true estimates with the placebo estimates; if the true estimate is more extreme than the placebo estimates, you can say with a certain degree of confidence to have found the true structural break.

The 2009 break “passes” the test for most outcome variables, as much as it is possible within this setting. We carry out this permutation test, described in Ganong and Jäger (2018), on all our main outcomes, using as placebo discontinuities all the years in the range 2005 to 2015 (the largest range allowed given the data). Appendix Figure A.9 and Appendix Table A.14 present the results: for the child penalties in labor supply and earnings, the discontinuity between 2009 and 2010 always yields a more extreme estimate than the placebo discontinuities. This is reflected in a p-value of .18 (which is the smallest possible, given the two-sided test and the small number of placebo years). For the child penalty in labor force participation, the 2009-10 discontinuity yields an estimate that is extreme but not the most extreme, with a p-value of .36 in the two-sided test. For the child penalty in hourly wages, for which we don’t see anything happening around 2009-10, the estimate at this break sits comfortably in the middle of the distribution of placebo estimates.

5 Connecting the Dots: Exposure to the Fair Work Act via Occupation and Industry

To investigate whether the increase in regularity brought about by the Fair Work Act did cause the change in labor supply, we construct a measure of occupational exposure to the law based on the ex-ante variation in regularity of jobs. We show that the mothers in the most exposed jobs are both those for whom regularity increased the most and those for whom the child penalty in labor supply decreased the most.

¹⁶Moreover, the absence of a large outlier in 2005 for the 6-10 year child penalty reassures that the peak we see for the 0-5 year child penalty is indeed driven by measurement error.

5.1 A Measure of Occupational Exposure

The Fair Work Act increased regularity, but it could only do so in jobs where there was scope for a change. We thus develop a measure of exposure of a job to this law based on the within-job level of regularity before 2009: our hypothesis is that jobs at the top and the bottom of this distribution were not exposed to the law, while only jobs in the middle were. Our definition of “job” is an occupation-by-industry cell.

The intuition behind our exposure measure is the following: jobs in which the schedule was either already fully regular, or technologically (“naturally”) impossible to make regular, were not affected by the Fair Work Act; only jobs “in the middle” were. Recall that we define regularity as working from Monday to Friday on a regular daytime schedule, but the reasoning and the analysis also go through for alternative definitions of regularity, such as not being on call or on an irregular schedule. There are certain jobs that, as of 2009, were already fully regular, thus a law increasing regularity could have no impact on them. A clerk working in an office only open on weekdays from 9 to 5 would fall into this category. There are other jobs, on the other end of the spectrum, that could not possibly be made regular: for example, doctors or bodyguards are often needed at night, during weekends, or in unforeseeable moments. These jobs too would not be impacted by the Fair Work Act, which explicitly states that parents’ requests for a change in work arrangements can be refused by an employer on “reasonable business grounds”.

We define “job” as an occupation-by-industry cell, and we measure job regularity using pre-2009 observations. In our definition, “job” is an occupation-by-industry cell, where both occupations and industries are identified at the 2-digit level. In our view, this strikes a good balance between cells that are sufficiently granular that jobs performed are sufficiently similar, while retaining a sufficient sample size for accurately estimating the level of regularity. Our measure of job regularity is the fraction of all worker-years in that occupation-by-industry cell that report having a regular schedule. Our results are robust to using observations only for men for the whole sample period (under the assumption that men are not treated by the law, both measures should proxy for underlying job “technology”).

We split the jobs (occupation-by-industry cells) in terciles of regularity; Figure 6 depicts where occupations and industries fall in this classification. Each occupation is present in multiple industries, and each industry contains multiple occupations, which is why each bar contains multiple colors. From the figure, we see that the most regular occupations are corporate managers and math and engineering professionals, and public administration is the industry in which the largest fraction of employees are on a regular schedule. At the other end of the spectrum, security guards, salespersons, and workers at hotels and restaurants are those least likely to work on a regular schedule. In the middle tercile of regularity distribution we find office clerks, teaching professionals and other associate professionals, working in education, and scattered around other industries. It is reasonable to imagine that office clerks working 9 to 5 jobs coexist with clerks that are required to work on call, and that while some teachers work the same hours every week, others are expected to be available if a colleague is sick. These occupations and industries are neither already fully regular nor impossible to be made regular, and are those in which we expect the Fair Work Act to have the most bite.

5.2 Empirical Strategy

In order to test whether the Fair Work Act did increase regularity, and improved the child penalty in labor supply as a consequence, we assign mothers a level of regularity based on their jobs, and then follow the evolution over time of regularity and of the child penalty of the groups of more and less exposed mothers.

Women are classified in terciles of regularity distribution as follows. We split jobs (occupation-by-industry cell) in terciles of regularity, based on the pre-2009 measure of regularity described above. Note that this assignment is fixed over the sample period for each job. Mothers are then assigned the tercile of the job they were in two years before their first childbirth; for non-mothers (which we keep as controls), we randomly draw fake childbirth years in order to assign the tercile of regularity in an analogous fashion. The assigned level of exposure is constant within individuals.

The first analysis we carry out consists in showing the evolution over time of the child penalty, separately for the three terciles of job regularity to whom mothers belong. In order to do so, we fully interact equation (4) with tercile-of-job-regularity dummies:

$$Y_{it} = \alpha_i + \sum_{d \in \{1, 2, 3\}} \left\{ \delta_{t,d} + \beta_{h(i),d} + \sum_c \left\{ \gamma_{0-5,c,d} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{c = cohort(i)\} + \right. \right. \\ \left. \left. + \gamma_{6-10,c,d} \times \mathbb{D}_{it}^{6-10} \times \mathbb{1}\{c = cohort(i)\} \right\} \right\} \times \mathbb{1}\{d = tercile(i)\} + \epsilon_{it} \quad (5)$$

where $\mathbb{D}_{it}^{0-5} = \mathbb{1}\{(t - E_i) \in [0, 5]\}$ is an indicator function that takes value of one if i 's first child is aged 0 to 5 and $\mathbb{D}_{it}^{6-10} = \mathbb{1}\{(t - E_i) \in [6, 10]\}$ is an indicator function for i 's first child being between 6 and 10 years old. Here the coefficients of interest are $\gamma_{0-5,c,d}$, which indicate the 0-5 years child penalty in outcome Y for mothers who have had their first child in year c and who belong to tercile d . For power reasons, in this specification cohorts of mothers are biannual (we group together mothers having their first child in 2003-04, 2005-06, etc.).

The exercise just described recovers the time series of the child penalty for the three groups of mothers defined by their regularity tercile; here we present the relative difference-in-differences setup. The estimating equation is a little more involved than a standard diff-in-diff because we are not interested in estimating the treatment effect on Y (e.g. labor force participation), but on the pre-post birth *change* in Y (e.g. the child penalty in labor force participation). We formalize it as follows:

$$\begin{aligned}
Y_{it} = & \gamma_a \times \mathbb{D}_{it}^{0-5} + \\
& + \gamma_{a,2} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{\text{tercile}(i) = 2\} + \\
& + \gamma_{a,1} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{\text{tercile}(i) = 1\} + \\
& + \gamma_b \times \mathbb{D}_{it}^{0-5} \times \text{Post}(i) + \\
& + \gamma_{b,2} \times \mathbb{D}_{it}^{0-5} \times \text{Post}(i) \times \mathbb{1}\{\text{tercile}(i) = 2\} + \\
& + \gamma_{b,1} \times \mathbb{D}_{it}^{0-5} \times \text{Post}(i) \times \mathbb{1}\{\text{tercile}(i) = 1\} + \\
& + \alpha_i + \sum_{d \in \{1,2,3\}} \{\delta_{t,d} + \beta_{h(i),d}\} \times \mathbb{1}\{\text{tercile}(i) = d\} + \\
& + [...] + \epsilon_{it}
\end{aligned} \tag{6}$$

where $\text{Post}(i)$ indicates that i 's first child was born after 2010 (and hence i belongs to the late cohorts, using the terminology from previous sections); and the penultimate line specifies that time and age fixed effects are estimated separately by tercile. Here γ_a is the baseline 0-5 years child penalty for the top tercile, meaning that it is the difference in Y in the five years following the birth of their first child relative to the five years before for couples (or individuals) in the reference group, as explained in the paragraph above. $\gamma_{a,2}$ is the baseline difference in this child penalty for the treated group (middle tercile) relative to the reference group (top tercile), and $\gamma_{a,1}$ is analogous for the placebo group (bottom tercile); these baselines are estimated off the early cohorts, meaning couples who had their first child between 2003 and 2008. The coefficient γ_b is the difference in 0-5 years child penalty for the late cohorts relative to the early cohorts for the reference group, and $\gamma_{b,2}$ is the difference-in-differences coefficient of interest, which captures the difference in child penalty for late versus early cohorts of couples in the treated group, relative to the same difference for the reference group. $\gamma_{b,1}$ is the difference-in-differences coefficient for the placebo group. The notation [...] indicates that analogous terms are included for the child penalty 6 to 10 years from the birth of i 's first child for completeness. They are always included in the estimation; however, the estimates are generally noisy since the number of observations is more limited, and hence these estimates are generally not displayed in the main tables (but reported in appendix tables).

5.3 Results

The entire change in regularity and the entire improvement in intensive labor supply are concentrated among mothers with jobs in the middle tercile of regularity distribution, supporting the hypothesis that the increase in regularity brought about by the Fair Work Act is responsible for the improvement in maternal labor supply (decrease in child penalty).

Figure 7 shows that the entire post-2009 decrease in the child penalty in intensive-margin labor supply and increase in regularity documented in Section 4.2 is concentrated among mothers with pre-birth jobs in the middle tercile of regularity distribution. Figure 7 replicates Figure 4, which shows the child penalty separately for early and late cohorts of mothers (whose first child is born between 2005-2008 and 2010-2013 respectively), separately by terciles of regularity. While panel (a) of Figure 7 shows that the child penalty

in labor force participation decreases similarly over time for mothers in the three terciles, panel (b) shows very clearly that the child penalty in hours of work only changes between early and late cohorts for women in the middle tercile of regularity distribution, and the same is true for the probability of being on a regular schedule in panel (c).

Figure 8 displays the parallel trends in the evolution of the child penalty of the three groups of job regularity and the sharp change in 2009 for the middle tercile. This figure basically replicates Figure 5 but separately for the three terciles of pre-birth job regularity. More formally, it displays the estimated $\gamma_{0-5,c,d}$'s from equation (5): the dotted blue line plots the evolution over time of the 0-5 years child penalty for mothers with pre-birth jobs in the top tercile of regularity distribution, the dashed yellow line for mothers with pre-birth jobs in the middle tercile of regularity distribution, and the solid red line for mothers with pre-birth jobs in the middle tercile of regularity distribution. Note that the result for regularity is also cast in terms of child penalty for easier comparison: panel (d) in Figure 8 does not display average regularity by cohort, but average drop in regularity after childbirth relative to before, by cohort. (As an aside, the *pre-birth levels* of regularity are fairly constant across the cohorts, hence the patterns shown for the child penalty are fully driven by *post-birth* variations in regularity, which strengthens our interpretation. Note that this statement is true for all the outcome variables we consider. The graphs showing raw averages of variables of interest, pre and post birth, by cohort, are available upon request).

The child penalties in regularity and intensive-margin labor supply are fairly constant over the sample period for the top and bottom terciles, while they display sharp changes around 2009 - in the direction of greater regularity and greater labor supply - for the middle tercile. Panel (d) of Figure 8 confirms that indeed the child penalty in regularity changed sharply after 2009, but only for mothers working in the jobs most exposed to the law: the jobs less exposed to the law show no change in the child penalty in regularity over the sample period, reassuringly. Panels (b) and (c) display the evolution of child penalty in labor supply on the intensive margin, for weekly hours in paid employment and log weekly hours in paid employment respectively. In both panels we see that the blue dotted line and the red solid line, plotting the coefficients for the top and bottom terciles respectively, are fairly flat over the sample period; while the yellow dashed line of the middle tercile shows a sharp and permanent increase in the intensive margin labor supply (a decrease in the child penalty) after 2009, with a timing that tracks almost perfectly the changes in regularity. Finally, panel (a) shows that this heterogeneity does not explain the changes in the child penalty in extensive margin labor supply, as the three groups closely track each other along this dimension.

In Table 5 we confirm the significance, or lack thereof, of the patterns just described. In the even-numbered columns of this table we report the coefficient estimates from equation (5): the first row reports the estimates of γ_a (baseline child penalty for low exposure group), the second those of $\gamma_{a,2}$ (baseline difference in child penalty between the top and the middle tercile of job regularity), the fourth those of γ_b (late-early cohort difference for the top tercile) and the fourth those of $\gamma_{b,2}$ (difference-in-difference coefficient, our coefficient of interest); the odd-numbered columns report the relative coefficient from a basic specification that doesn't distinguish between terciles.

The child penalty in regularity (columns 7 and 8) changed systematically for late co-

horts of mothers relative to early cohorts of mothers for mothers in the middle tercile; relative to mothers in the top tercile, for whom it didn't change at all. Column 8 tells us that at baseline, women in the top tercile of regularity are 53 percentage points less likely to have a regular schedule after they become mothers relative to before, and this figure is 10 percentage points larger (but not significantly different) for women in the middle tercile. It also tells us that there is virtually no difference in the child penalty in regularity between late and early cohorts in the top tercile, but, crucially, the difference-in-differences coefficient indicates that the change for the middle tercile is large (15 percentage points) and significant. The difference-in-differences for the bottom tercile – a placebo exercise, since we don't expect this group to be impacted by the law – instead is not significantly different from zero. This confirms that the passage of the Fair Work Act did have a large impact on the regularity of mothers of young children in the middle tercile of the regularity distribution, and none at the top and at the bottom.

Correspondingly, the child penalty in intensive margin labor supply shrank dramatically in the late cohorts of mothers relative to the early cohorts for mothers in the middle tercile; relative to top and bottom tercile where it basically did not change. Column 4 in Table 5 shows that at baseline women in the top tercile suffer a child penalty of 16 hours of work per week, and women in the middle tercile of about 21 hours a week. Among women in the late cohorts (giving birth after 2009), those in the top tercile suffer a very similar child penalty as early cohort (their post-birth labor supply is about 1.3 hours higher, but not significantly different), but those in the middle tercile experience an increase in post-birth labor supply (a decrease in the child penalty) of about 5.4 hours a week, relative to the late-early comparison for the top tercile. The difference-in-differences for the bottom tercile is reassuringly zero. In summary, women in the middle tercile after the passage of the law experience a child penalty in intensive margin labor supply that is about 32% smaller than before the Fair Work Act, while for the top and bottom terciles this difference is 8% and 9% respectively and neither statistically different from zero.

These effects on the child penalty in hours of work are driven by fewer women switching to part-time jobs in the high-exposure group, and also hold in the longer term. Appendix Table A.16 shows that, in the middle tercile, the share of women moving to part-time after they become mothers is 18 percentage points smaller for late cohorts relative to early cohorts, relative to the top tercile. While Table 5 only presents the coefficients on the 0-5 year child penalty, meaning the child penalty estimated on the first five years from the birth of the first child (for clarity of exposition), Appendix Table A.15 extends it to include coefficients for the 6-10 year child penalty, meaning the child penalty estimated in the 6 to 10 years after the birth of the first child (relative to before childbirth, and to non-mothers). In columns 2 and 4, we can see that both the estimated increase in regularity and the estimated increase in intensive-margin labor supply (decrease in child penalty) for more exposed mothers still hold when we look at the longer term, although more noisily.

Finally, exposure to the Fair Work Act as we measure it does not explain the observed change in the child penalty in labor force participation. Column 1 of Table 5 summarizes what we have shown in Section 4.2.2: that there is a change in the child penalty in labor force participation between early and late cohorts of mothers (although statistical significance for this variable is less clear-cut than for intensive-margin labor supply, as we discuss in that section). Column 2, though, shows that the three terciles have very similar child

penalties in labor force participation both at baseline and after the passage of the law: thus, exposure to the Fair Work Act does not explain the early-late cohort difference in child penalty in labor supply we observe.

To conclude: we presented the results using the tercile split for clarity of exposition, but they also hold if we use the measure of regularity directly. We visualize this in Appendix Figure A.11, where we plot on the x-axis our measure of regularity (fraction on regular schedule in pre-2009 observations), and on the y-axis the reduction in child penalty in hours of work between late and early cohorts (higher numbers mean higher post-childbirth labor supply – hence smaller child penalty – in late cohort relative to early cohorts). Both are computed at the level of occupation-by-industry and “binscattered” here. We see a clearly quadratic relationship between the two variables: for high and low values of job regularity, the change in the child penalty between early and late cohorts is essentially zero; for middle levels of regularity, late cohorts experience a child penalty in labor supply which is up to 10 hours smaller than early cohorts.

6 Robustness Check: Paid Parental Leave Is Not Driving the Results

We have shown in the previous section that the large drop in child penalty in labor supply that occurs around 2009 is entirely driven by the increase in workplace regularity. Here we rule out an important potential confounder: the 2010 introduction of Paid Parental Leave (PPL). We show that our results are unaffected when we account for Paid Parental Leave and that Paid Parental Leave *per se* does not seem to have significantly affected the child penalty in labor supply, a result in line with previous literature, and which is not surprising given the pre-existing institutional context.

6.1 Institutional Setting

Paid Parental Leave was introduced a year after the Fair Work Act, and could thus act as a confounder. However, the pre-existing presence of unpaid leave with job guarantee for up to one year, and a cash transfer for new babies, implies that this new piece of legislation did not substantially change incentives for new mothers.

Paid parental leave was signed into law in 2010 (and applies since January 1, 2011), and it allows one of the parents to take up to 18 weeks off from work, paid at the national minimum wage, after the birth of a child. PPL could not be combined with the Baby Bonus, an unconditional and tax-exempt cash transfer that was available to all families in which new babies were born. While PPL is nominally more generous, the interaction with the tax and transfer system (including Family Tax Benefits, a set of transfers specifically for lower income families) meant that for a fraction of families it was less advantageous than the already existing Baby Bonus, and for another set the two were close substitutes. The remaining group, relatively wealthier mothers, were facing a relatively lower replacement rate (since PPL is paid at the minimum wage regardless of pre-birth earnings) and were

more likely to be eligible for employer-provided paid maternity leave, thus it is unclear whether this policy changed their choice set either.

In addition, paid leave guarantees job protection – and this is the aspect that really matters for labor force attachment of new mothers, according to the review article by Olivetti and Petrongolo (2017). However, up to one year of unpaid job-protected leave upon the birth of a child was already granted to all Australian workers since 1979, so the introduction of PPL didn't change this aspect either. In conclusion, it seems that the introduction of PPL did not significantly change the incentives faced by new parents, and therefore we do not expect it to lead to significant changes in their labor supply choices. For a more detailed explanation of family benefits in Australia, and how they have changed over the past twenty years, see Bassford and Fisher (2016); Kalb (2018); de Gendre et al. (2021).

6.2 Empirical Strategy

In order to study whether Paid Parental Leave affected the child penalty in labor supply, we leverage the fact that before the introduction of PPL, 55% of working women were already eligible for employer-provided paid maternity leave. Under the assumption that the introduction of national, state-funded, Paid Parental Leave does not affect women who already can access this benefit through their employer, the evolution of the child penalty for non-eligible women relative to that of eligible ones estimates the effect of introducing paid parental leave.

First, we study the effect of PPL on maternal labor supply by comparing the evolution of the child penalty for treated and non-treated women. We define women as being treated by PPL if, two years before the birth of their first child, they were *not* eligible for employer-provided PPL; for non-mothers (which we keep as controls) we randomly draw fake childbirth years to assign treatment analogously. Then, we run a specification analogous to equation (6), where we replace the dummy of middle tercile with a dummy for being treated by PPL (and don't include a dummy for the bottom tercile).

Second, we test whether PPL might be the omitted variable driving our results from the previous sections. If the jobs that offer employer-provided paid leave are systematically more or less likely to be exposed to the changes of the Fair Work Act, we might attribute the change in the child penalty to the Fair Work Act when in fact the observed effect is driven by the correlation between job regularity and likelihood of being offered paid maternity leave. We address this issue by augmenting the specification above to include

both controls for high-exposure to Fair Work Act and for being treated by PPL, as follows:

$$\begin{aligned}
Y_{it} = & \gamma_a \times \mathbb{D}_{it}^{0-5} + \\
& + \gamma_{a,H} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{\text{exposure}(i) = \text{high}\} + \\
& + \gamma_{a,nP} \times \mathbb{D}_{it}^{0-5} \times \text{nPPL} + \\
& + \gamma_b \times \mathbb{D}_{it}^{0-5} \times \text{Post}(i) + \\
& + \gamma_{b,H} \times \mathbb{D}_{it}^{0-5} \times \text{Post}(i) \times \mathbb{1}\{\text{exposure}(i) = \text{high}\} + \\
& + \gamma_{b,nP} \times \mathbb{D}_{it}^{0-5} \times \text{Post}(i) \times \text{nPPL} + \\
& + \alpha_i + \sum_{p \in \{\text{yes}, \text{no}\}} \sum_{d \in \{\text{high}, \text{low}\}} \{\delta_{t,d,p} + \beta_{h(i),d,p}\} \\
& \times \mathbb{1}\{d = \text{exposure}(i)\} \times \mathbb{1}\{p = \text{PPL-treated}(i)\} + \\
& + [\dots] + \epsilon_{it}
\end{aligned} \tag{7}$$

where $\mathbb{1}\{\text{exposure}(i) = \text{high}\}$ is a dummy indicating that the woman's pre-birth job is in the middle tercile of regularity distribution as opposed to top or bottom, as defined in Section 5, and nPPL indicates that individual i was *not* offered paid maternity leave by her employer two years before childbirth, and hence is treated by the law introducing paid parental leave. The last couple of lines specify that time and age fixed effects are estimated separately for the four groups defined by the interaction of the two classifications. In this specification, the coefficient $\gamma_{b,nP}$ is the effect of PPL on the child penalty in Y , controlling for exposure to the Fair Work Act, and $\gamma_{b,H}$ is the effect of exposure to the Fair Work Act on the child penalty in Y , controlling for PPL.

6.3 Results

By leveraging access to employer-provided paid leave to proxy for treatment by PPL, as described above, here we show that we cannot reject the null hypothesis that PPL did not have an effect on maternal labor supply, and that the main results of the paper presented in the section above are robust to controlling for PPL.

PPL does not have a statistically significant impact on the child penalty in labor supply, although the point estimates suggest that it might have helped increase labor force participation of new mothers. From column 3 in Table 6 we learn that, among mothers whose first child was born before 2009, those not eligible for employer-provided paid parental leave were 6 percentage points less likely to return to work after childbirth, and the difference between the two groups disappears among mothers whose first child was born after 2010; however, neither the "diff" nor the "diff-in-diff" coefficients are significantly different from zero. The child penalty in log hours in paid employment (column 7) is basically identical for mothers who were and were not eligible for employer-provided paid leave throughout the sample period.

Controlling for PPL does not change the results based on exposure to the Fair Work Act presented in Section 5. In column 2 [6] of Table 6 we replicate the analysis from Section 5 on the sample in which we can observe eligibility to employer-provided paid parental leave (and hence assign PPL-treatment); on column 4 [8] we report the coefficients from estimating equation (7): the only difference between the two is the inclusion of controls for

PPL-treatment status in the latter. In columns 2 and 4 the outcome variable is labor force participation, while in columns 6 and 8 it is the log of weekly hours in paid employment. In both instances, the inclusion of the controls for PPL does not change the coefficients on being exposed to the Fair Work Act. This confirms that the results in Section 5, that attribute the decrease in child penalty to exposure to the Fair Work Act, were not driven by the contemporaneous passage of Paid Parental Leave.

7 Conclusion

In this paper we have studied the role of work arrangements in the child penalty in labor supply, the large drop in labor supply women face when becoming mothers. By leveraging the introduction of the 2009 Australian Fair Work Act, which entitled parents of young children to a change in work arrangements, we established two sets of facts. First, when allowed to request not better defined “changes in work arrangements”, mothers of young children choose to increase the regularity of their schedules, rather than their flexibility. Second, an increase in regularity is associated with a smaller child penalty on the intensive margin labor supply: when their schedules are more predictable, mothers of young children work longer hours.

We have provided further support for the hypothesis that the observed changes in the child penalty were caused by the changes in regularity brought by the Fair Work Act in two ways. First, we used a measure of exposure to the Fair Work Act, based on terciles of pre-2009 regularity in occupation-by-industry cells, to show that the entire increase in regularity and the full decrease in the child penalty in labor supply are concentrated in the group of women whose pre-motherhood jobs was in the middle tercile of regularity distribution and who were thus most exposed to the law. Second, we have provided evidence that our results are not driven by the contemporaneous introduction of Paid Parental Leave.

We conclude the paper opening the way for further research in this area. First, our paper shows the existence of a “child penalty” in work arrangements, and that when these work arrangements are allowed to change, labor supply changes as well. Further research is warranted to explore further both which work arrangements are predictive of the size of the child penalty, and what we can learn from the way work arrangements change around childbirth. Second, changing labor supply around childbirth is part of an overall rearrangement of tasks within the household. Studying how time is reallocated within person, and how tasks are allocated across partners in a couple, is going to help shed more light on the root causes of the child penalty in labor market.

References

- Martin Eckhoff Andresen and Emily Nix. What Causes the Child Penalty? Evidence from Adopting and Same-Sex Couples. *Journal of Labor Economics*, 40(4):971–1004, October 2022. Publisher: The University of Chicago Press. 2, 5
- Minji Bang. Job Flexibility and Household Labor Supply: Understanding Gender Gaps and the Child Wage Penalty. page 53, 2021. 6
- Micaela Bassford and Hayley Fisher. Bonus babies? the impact of paid parental leave on fertility intentions. 2016. 28
- Marianne Bertrand, Claudia Goldin, and Lawrence F. Katz. Dynamics of the gender gap for young professionals in the financial and corporate sectors. 2, 2010. 2, 6
- Barbara Boelmann, Anna Raute, and Uta Schönberg. Wind of Change? Cultural Determinants of Maternal Labor Supply. *SSRN Electronic Journal*, 2021. 2, 5
- Aline Bütikofer, Sissel Jensen, and Kjell G. Salvanes. The role of parenthood on the gender gap among top earners. *European Economic Review*, 109:103–123, 2018. 6
- Patricia Cortes and Jessica Pan. Children and the Remaining Gender Gaps in the Labor Market. *Journal of Economic Literature*, Forthcoming. 2, 5
- Gordon B. Dahl, Katrine V. Løken, Magne Mogstad, and Kari Vea Salvanes. What Is the Case for Paid Maternity Leave? *Review of Economics and Statistics*, 98(4):655–670, 2016. 5
- Alexandra de Gendre, John Lynch, Aurélie Meunier, Rhiannon Pilkington, and Stefanie Schurer. Child health and parental responses to an unconditional cash transfer at birth. 2021. 28
- Peter Ganong and Simon Jäger. A Permutation Test for the Regression Kink Design. *Journal of the American Statistical Association*, 113(522):494–504, April 2018. 21
- Jonah B Gelbach. Public Schooling for Young Children and Maternal Labor Supply. *American Economic Review*, 92(1):307–322, 2002. 5
- Claudia Goldin. A grand gender convergence: Its last chapter. *American Economic Review*, 104(4):1091–1119, 2014. 2, 6, 15
- Claudia Goldin and Lawrence F Katz. A most egalitarian profession: pharmacy and the evolution of a family-friendly occupation. *Journal of Labor Economics*, 34(3):705–746, 2016. 2, 6
- Tarjei Havnes and Magne Mogstad. Money for nothing? Universal child care and maternal employment. *Journal of Public Economics*, 95(11), 2011. 2, 5
- Guyonne Kalb. Paid parental leave and female labour supply: A review. *Economic Record*, 94(304):80–100, 2018. 28

Henrik Kleven. The Geography of Child Penalties and Gender Norms: Evidence from the United States, June 2022. URL <https://www.nber.org/papers/w30176>. 2, 5

Henrik Kleven, Camille Landais, and Jakob Egholt Søgaard. Children and Gender Inequality: Evidence from Denmark. *American Economic Journal: Applied Economics*, 11(4): 181–209, 2019. 2, 8

Henrik Kleven, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation. Technical Report w28082, National Bureau of Economic Research, 2021a. 2, 5

Henrik Kleven, Camille Landais, and Jakob Egholt Søgaard. Does Biology Drive Child Penalties? Evidence from Biological and Adoptive Families. *American Economic Review: Insights*, 3(2):183–198, 2021b. 2, 5

Henrik Kleven, Camille Landais, and Gabriel Leite Mariante. The Child Penalty Atlas. 2022. 2

Pierre Lefebvre and Philip Merrigan. Child-Care Policy and the Labor Supply of Mothers with Young Children: A Natural Experiment from Canada. *Journal of Labor Economics*, 26(3), 2008. doi: 10.1086/587760. 5

Julie McMillan, Adrian Beavis, and Frank L Jones. The ausei06: A new socioeconomic index for australia. *Journal of Sociology*, 45(2):123–149, 2009. 81

Claudia Olivetti and Barbara Petrongolo. The economic consequences of family policies: lessons from a century of legislation in high-income countries. *Journal of Economic Perspectives*, 31(1):205–30, 2017. 5, 7, 28

Bernadette O'Neill. General Manager's Report into the operation of the provisions of the National Employment Standards relating to requests for flexible working arrangements and extensions of unpaid parental leave 2009-2012. Technical report, Fair Work Australia, 2012. 7, 12

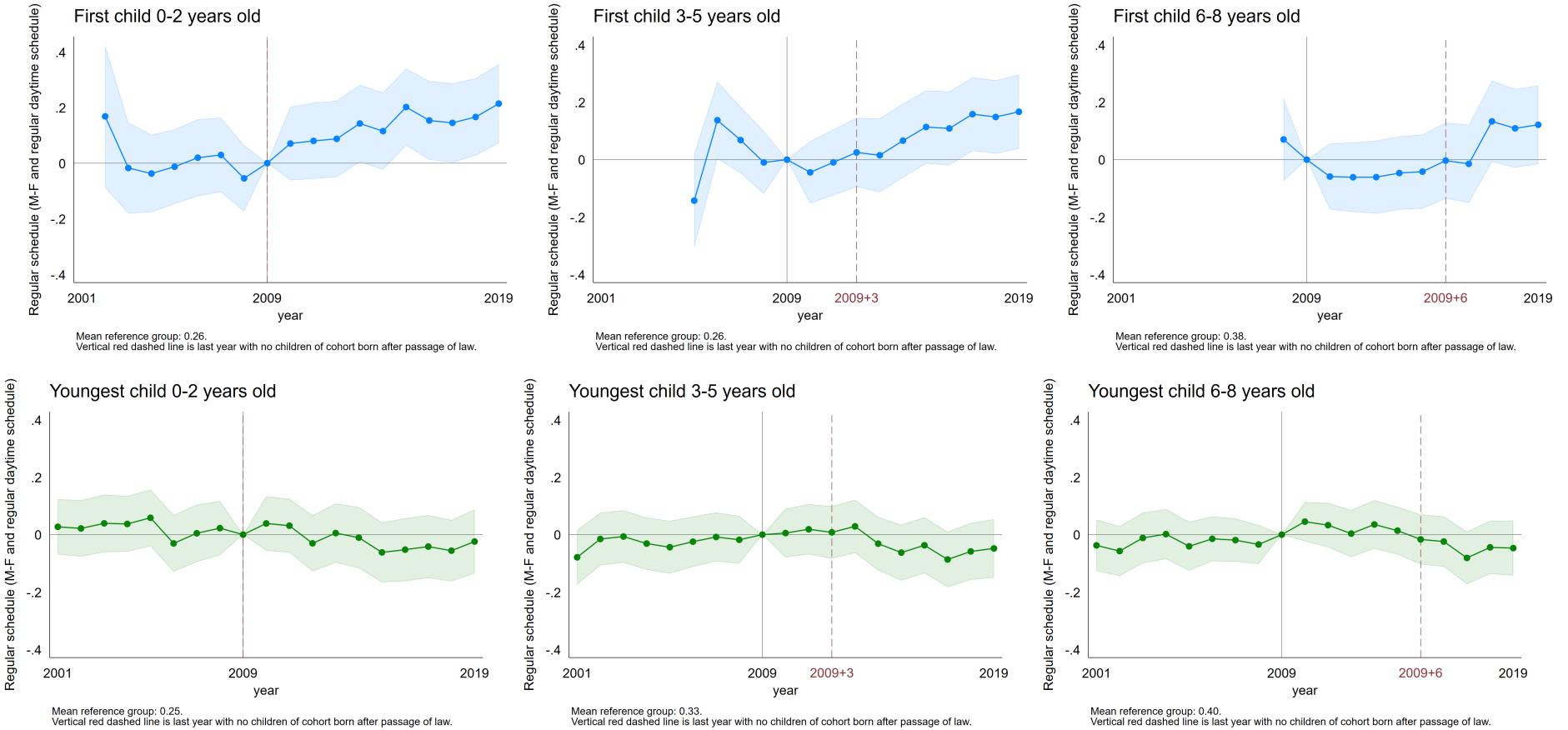
Uta Schönberg and Johannes Ludsteck. Expansions in Maternity Leave Coverage and Mothers' Labor Market Outcomes after Childbirth. *Journal of Labor Economics*, 32(3): 469–505, 2014. 2, 5

Michelle Summerfield, Brooke Garrard, Markus Hahn, Yihua Jin, Roopa Kamath, Ninette Macalalad, Nicole Watson, Roger Wilkins, and Mark Wooden. Hilda user manual-release 19. *Melbourne Institute of Applied Economic and Social Research, University of Melbourne*, 2019. 81

Liyang Sun and Sarah Abraham. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 2020. 13, 36, 40, 55

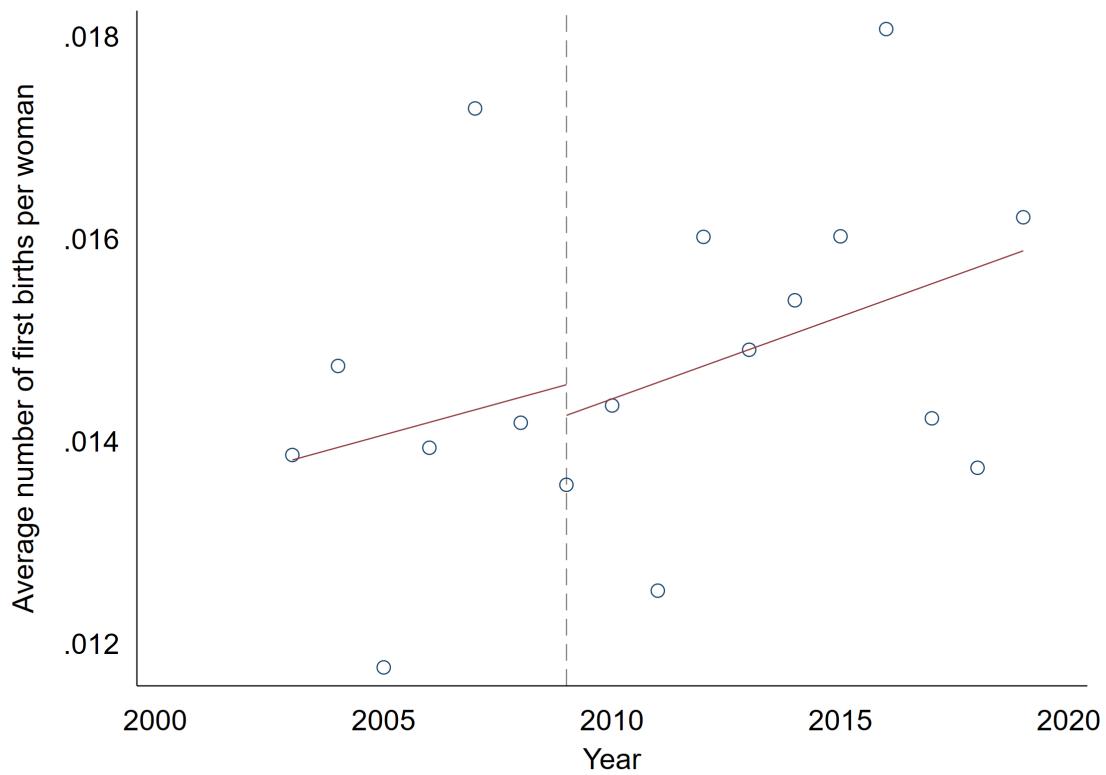
8 Figures

Figure 1: “Regular Schedule” for Mothers Relative to Non-Mothers, by Year and Age of Child



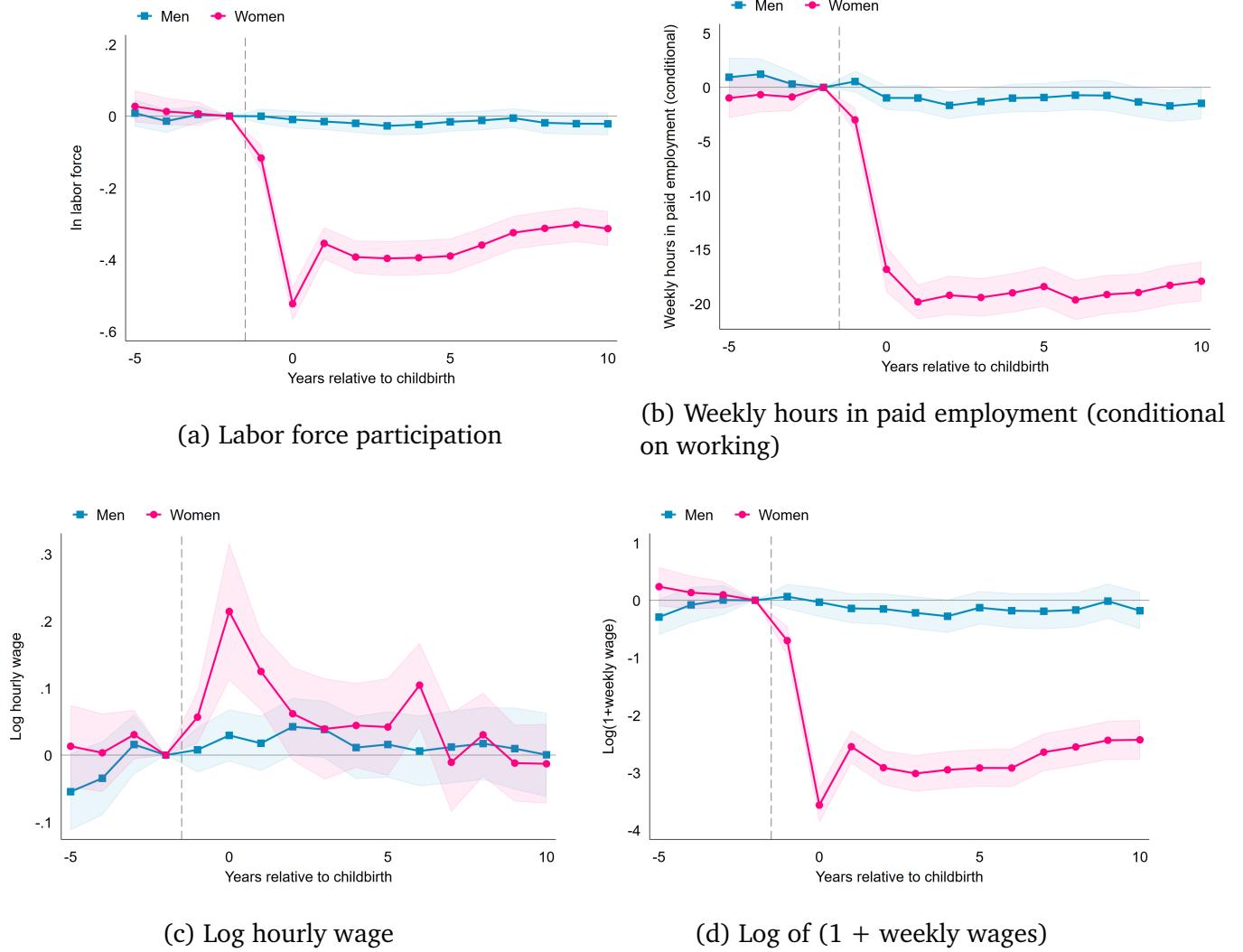
Note: This figure represents the evolution over time of regularity for women over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is “Regular Schedule”, a dummy for working from Monday to Friday on a regular daytime schedule. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimates for different ranges of child’s age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure 2: Fertility Did Not Change Discontinuously Around 2009



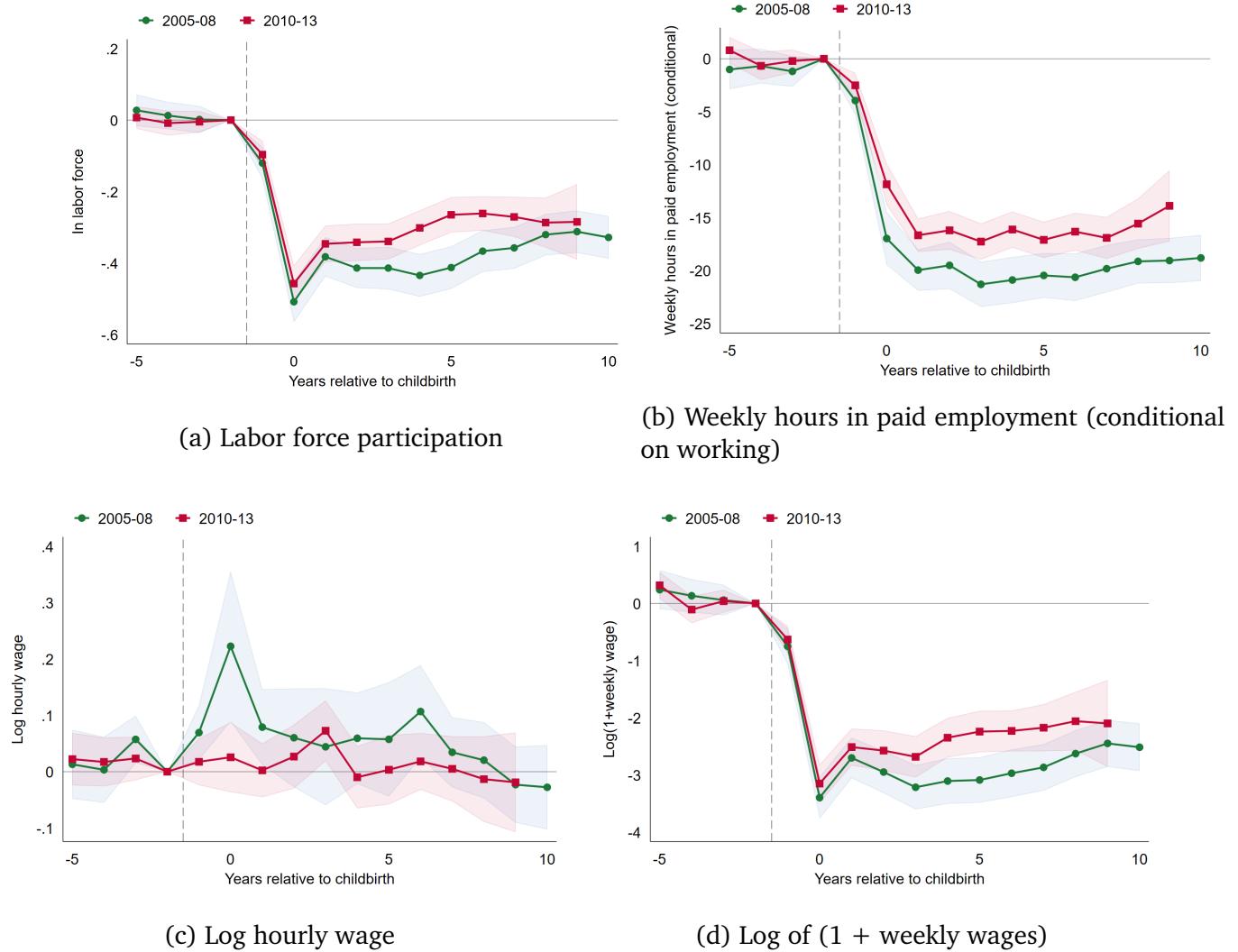
Note: This figure plots the fertility rate (for first births) in our sample over the sample period. Each dot represents the total number of women giving birth to their first child in the corresponding year divided by the total number of women in our sample in that year. The vertical dotted line indicates the year 2009, in which the Fair Work Act was introduced.

Figure 3: Child Penalty by Gender: Main Labor Market Outcomes



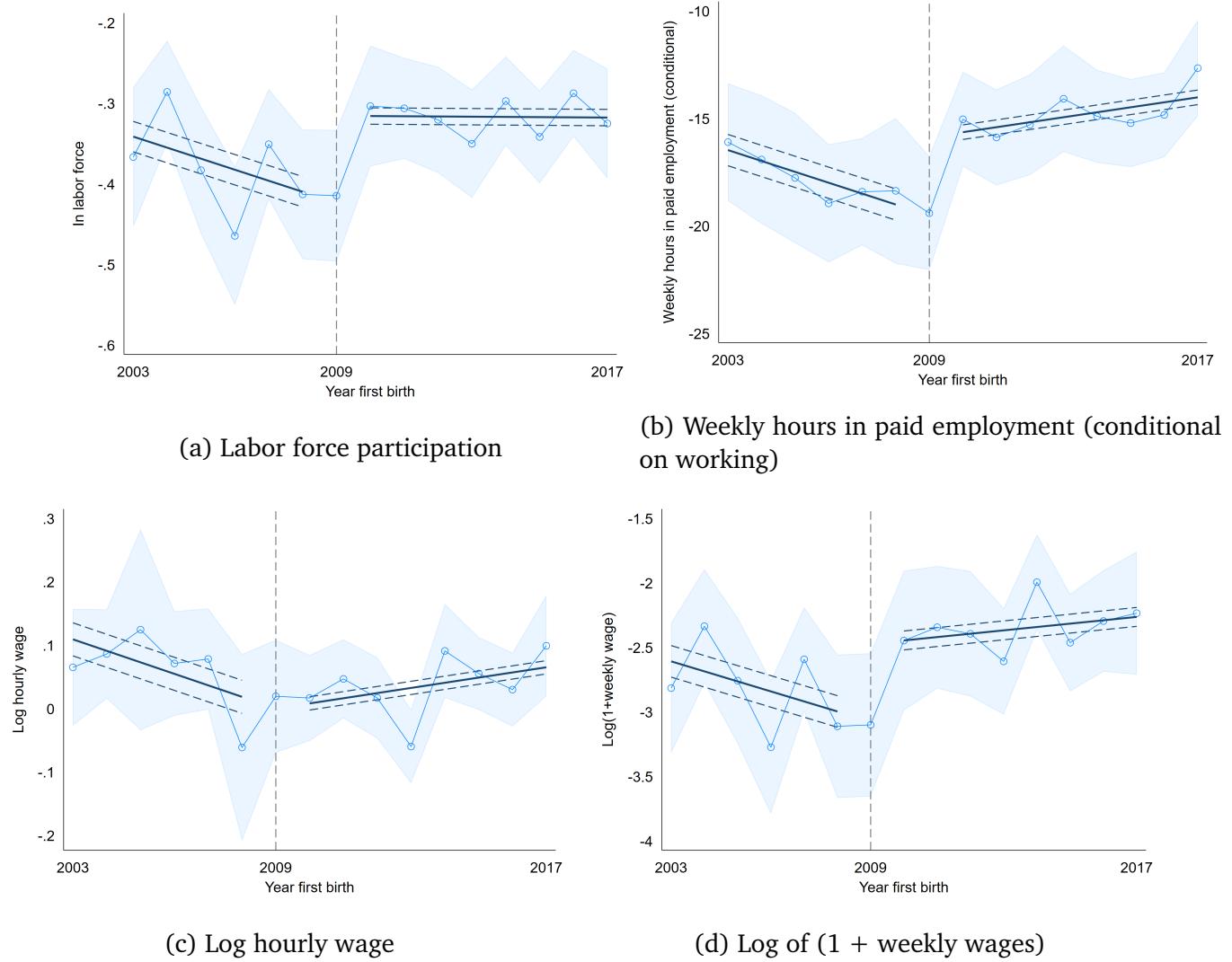
Note: This figure shows the child penalty in labor supply, wages, and earnings by year relative to childbirth for both men and women, as described in Section 4.1.1. Namely, the figure plots the coefficients γ_k 's from estimating equation (2), which includes individual, time and age fixed effects. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is the natural logarithm of hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. In all panels, the blue squares are estimates from a regression that includes only men, while the pink circles are estimates from a regression that includes only women. The shades around them are 95% confidence intervals. We only include parents who have their first child between 2003 and 2008, and we include non-parents as controls - see Section 4.1.1 for details. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level.

Figure 4: Change in the Child Penalty for Women: Early Versus Late Cohorts (Pre vs Post 2009)



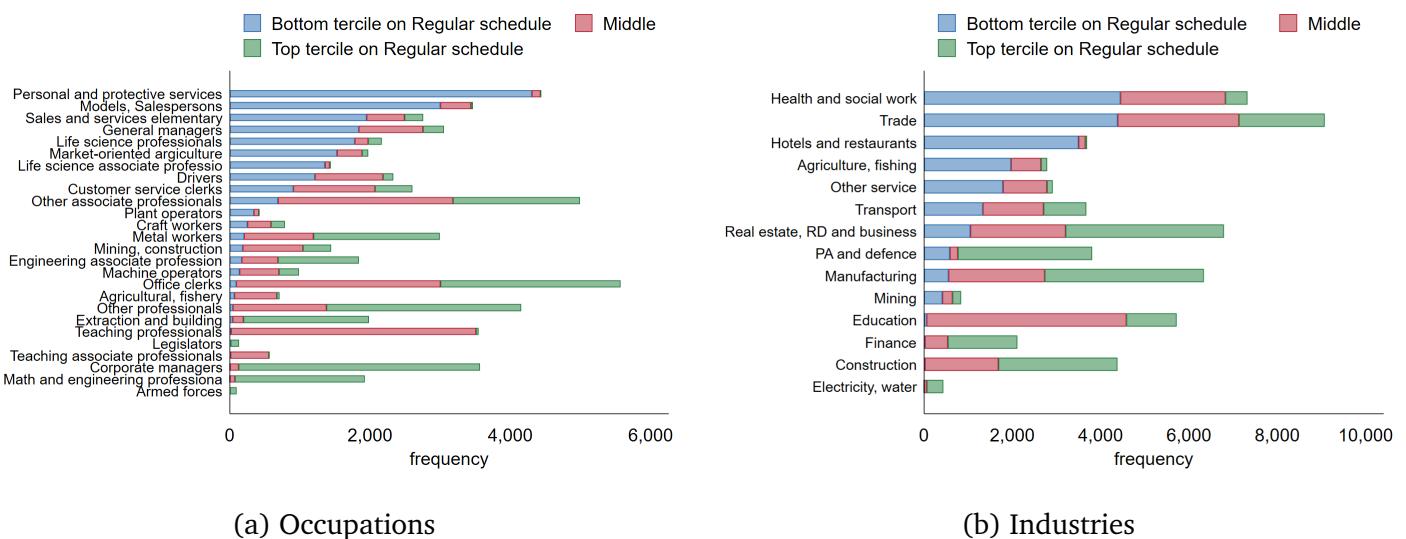
Note: This figure shows how the child penalty in labor supply, wages, and earnings for women has changed over the sample period. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is the natural logarithm of hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. All panels plot the child penalty in labor supply by year relative to childbirth (the coefficients γ_k 's in equation (2)) for women, estimated separately for early cohorts (first childbirth between 2005 and 2008, green circles) and late cohorts (first childbirth between 2010 and 2013, red squares). Women without children are included in both regressions as controls, and women having children in other sample years are excluded from both regression. See note to Figure 3 for further details.

Figure 5: Time Evolution of the Child Penalty for Women



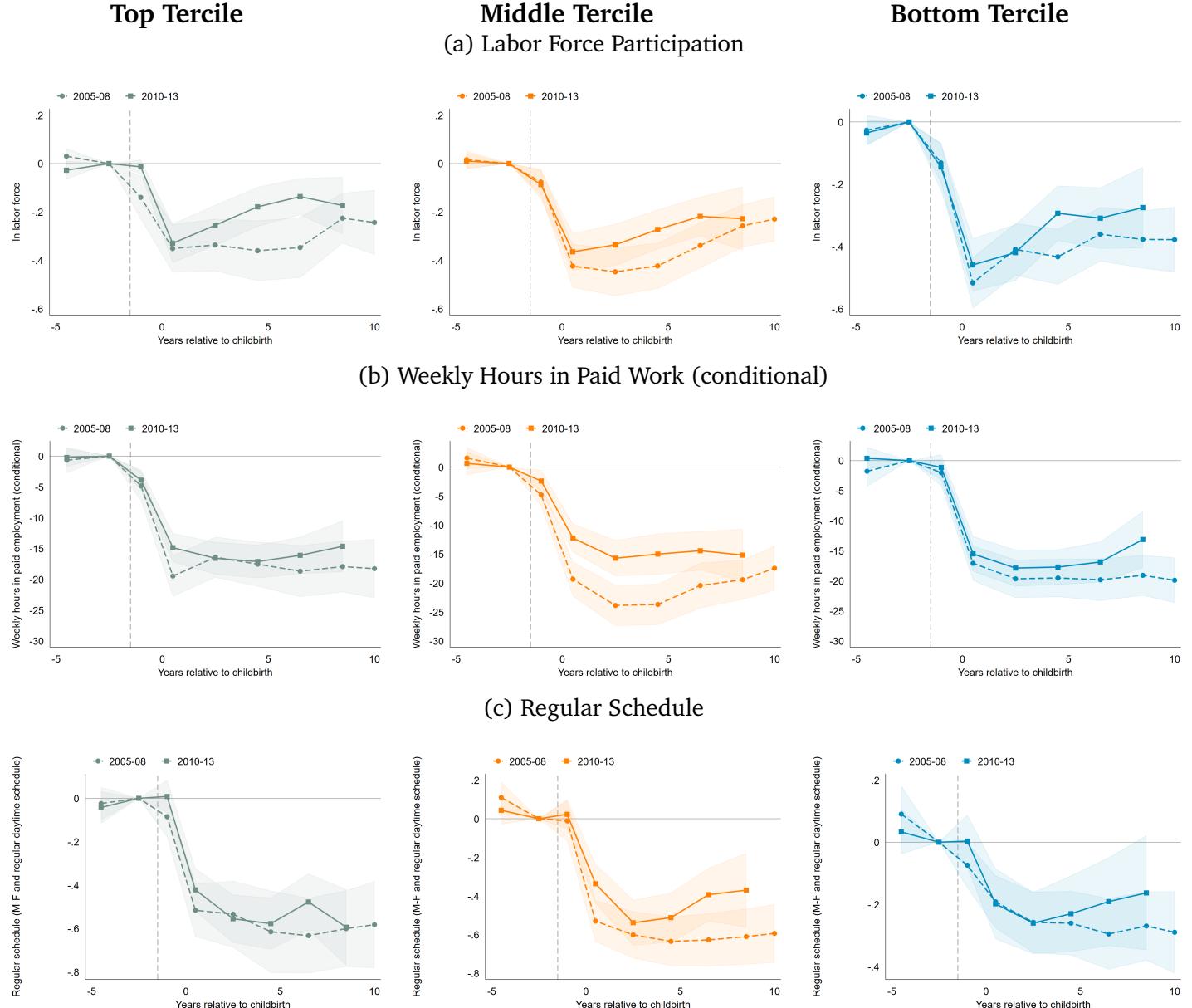
Note: This figure shows how the child penalty in labor supply, wages, and earnings for women has changed over the sample period. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is the natural logarithm of hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of $1 + \text{weekly wages}$, again winsorised. All panels display the 0-5 years child penalty (average of the child penalty from zero to five years after the birth of the first child) for women by year of birth of the first child. That is, they plot the estimates of $\gamma_{0-5,c}$ from equation (4). The blue circles are the estimates yearly cohort-by-yearly cohort, and the shades around them are 95% confidence intervals. The solid lines are the linear trends estimated for childbirth cohorts 2003-2008 and 2010-2017, and the dashed lines are the corresponding 95% confidence intervals, as described in Section 4.2.2. We cluster standard errors at the individual level.

Figure 6: Exposure to the Fair Work Act: Occupations and Industries by Terciles of Regularity



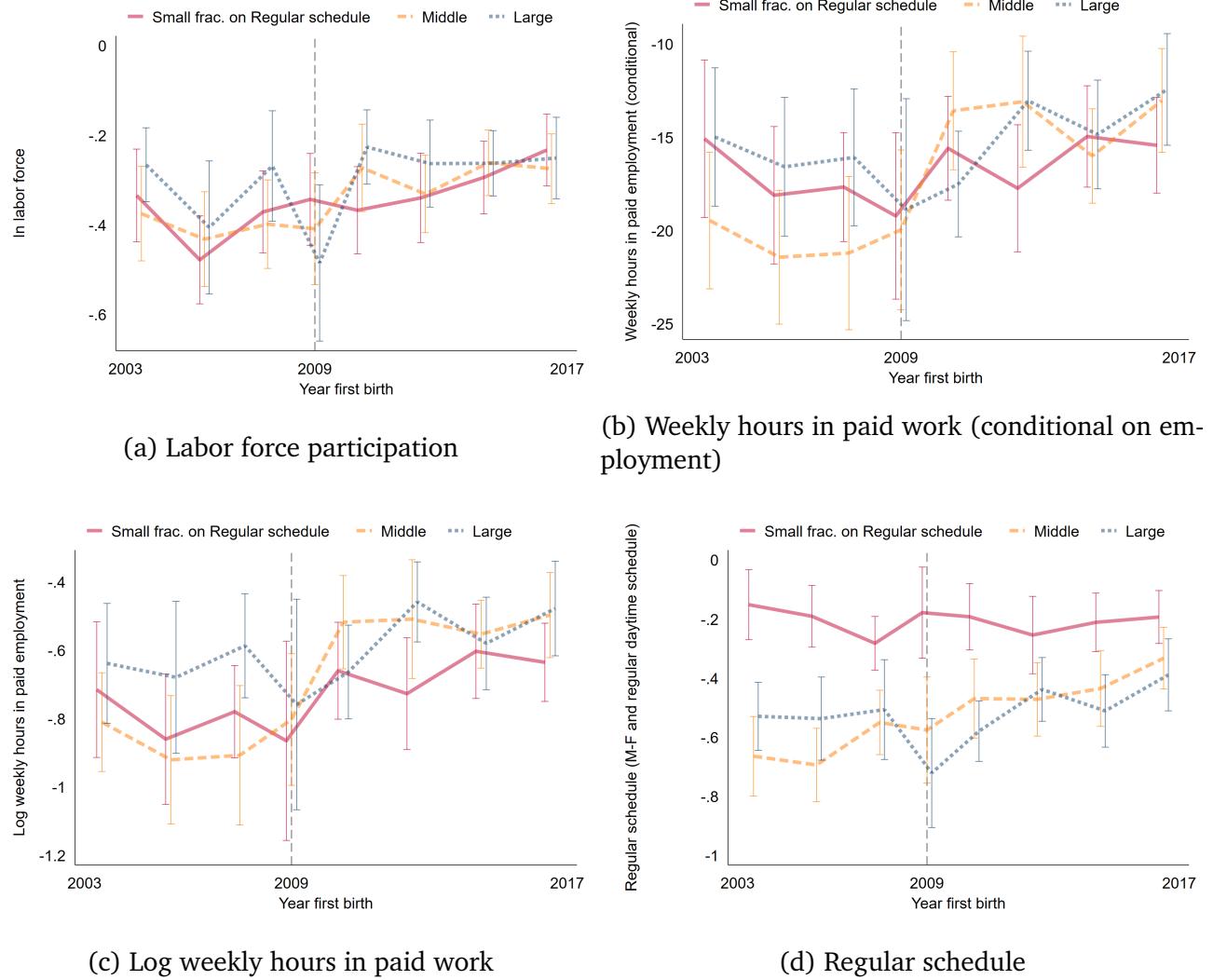
Note: For each occupation (left panel) and industry (right panel) in our sample, this figure plots the frequency of observations by terciles of regularity. Since regularity is defined at the industry-by-occupation level, each industry and each occupation can include jobs with different values of regularity. Jobs (occupation-by-industry cells) are weighted by the number of pre-2009 observations, including both women and men.

Figure 7: Child penalty for Women, Before and After Reform by Treatment Status



Note: This figure shows the child penalty for women by early and late cohorts and by terciles of regularity distribution. In each panel, we report estimates of γ_k 's from estimating a version of equation (2) that pools adjacent years in pairs for clarity (i.e. the relative-year dummies are bi-annual as opposed to annual). Each set of coefficient estimates comes from separate regressions, all of which include individual, time, and age fixed effects. In the top row, the outcome is labor force participation; in the second row, the outcome is weekly hours spent in paid employment conditional on working; in the bottom row, the outcome is a dummy for being on a regular schedule. The leftmost panels restrict the sample to women whose job two years before their first childbirth was in the top tercile of regularity distribution; the middle panels to women whose job two years before their first childbirth was in the middle tercile of regularity distribution; the rightmost panels to women whose job two years before their first childbirth was in the bottom tercile of regularity distribution - see text for details. Within each panel, the circles connected via dashed lines are estimates when restricting the sample to women who had their first child between 2005 and 2008, while the squares connected via solid lines are coefficient estimates when restricting the sample to women whose first child was born between 2010 ad 2013. Non-mothers are included as controls: they are assigned a random fake childbirth year in order to assign them to a tercile in the same way as we do for mothers. The shades around the point estimates are 95% confidence intervals. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level.

Figure 8: Time Evolution of Child Penalty by Job Regularity



Note: This figure shows the evolution of the child penalty in labor supply and regularity for women over the sample period, separately by levels of exposure to the Fair Work Act, as described in Section 5. Specifically, exposure of a job is defined as the standard deviation in regularity using pre-2009 observations; the exposure of a woman is that of the job (occupation-by-industry) she had two years before childbirth. The blue dotted lines are the estimates for the low-exposure group, women in jobs with below-median standard deviation of regularity; the red solid lines plot the same estimates for the high exposure groups, women in jobs with above-median standard deviation of regularity. More precisely, dots represent the estimates for $\gamma_{0-5,c,d}$ from equation (5). Cohorts are biannual (i.e. we pull together women whose birth was in 2003 and in 2004, in 2005 and in 2006, etc; except for 2009, which is kept separate). The vertical segments represent 95% confidence intervals. Women without children are included as controls, and exposure is assigned based on the job they were in two years before a randomly-drawn fake childbirth year. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is the natural logarithm of weekly hours in paid employment; in panel (d) the outcome is the regular schedule dummy (equal to 1 if the respondent has a Monday to Friday schedule and a regular daytime schedule). We cluster standard errors at the individual level.

9 Tables

Table 1: Summary Statistics

	N	Mean	SD	Min	Max
Women					
Age	132391	38.8	14.3	15	65
In labor force	126624	0.72	0.45	0	1
Weekly hours in paid employment (conditional)	85707	31.4	14.2	1	144
Log(1+weekly wage)	132391	4.09	3.18	0	8.36
Hourly wage	85707	25.2	27.1	0	2000
Part-time if employed	85790	0.48	0.50	0	1
Permanent employment if employed (vs casual and fixed-term)	76564	0.63	0.48	0	1
Overtime (more than 38 hours/week)	132391	0.19	0.39	0	1
Overtime (more than 50 hours/week)	132391	0.053	0.22	0	1
Promoted in the past year (conditional on current employment)	74091	0.091	0.29	0	1
Normally supervise work of other employees	85906	0.41	0.49	0	1
Occupational status scale	85876	52.3	22.6	0	100
Regular schedule (M-F and regular daytime schedule)	85880	0.42	0.49	0	1
M-F	85885	0.45	0.50	0	1
Regular daytime schedule	85906	0.75	0.44	0	1
On call and irregular shifts	85906	0.10	0.30	0	1
My working times can be flexible (yes-no)	63040	0.49	0.50	0	1
Entitled to flexible start/finish times	67070	0.56	0.50	0	1
Has bachelor degree	126588	0.27	0.44	0	1
Men					
Age	125761	38.5	14.4	15	65
In labor force	116100	0.84	0.36	0	1
Weekly hours in paid employment (conditional)	91809	41.7	14.4	1	150
Log(1+weekly wage)	125761	4.87	3.19	0	8.84
Hourly wage	91809	26.8	22.9	0	1611
Part-time if employed	91890	0.17	0.38	0	1
Permanent employment if employed (vs casual and fixed-term)	74868	0.72	0.45	0	1
Overtime (more than 38 hours/week)	125761	0.47	0.50	0	1
Overtime (more than 50 hours/week)	125761	0.19	0.39	0	1
Promoted in the past year (conditional on current employment)	76366	0.098	0.30	0	1
Normally supervise work of other employees	91975	0.50	0.50	0	1
Occupational status scale	91926	47.2	23.7	0	100
Regular schedule (M-F and regular daytime schedule)	91966	0.53	0.50	0	1
M-F	91978	0.57	0.49	0	1
Regular daytime schedule	91981	0.75	0.43	0	1
On call and irregular shifts	91981	0.10	0.30	0	1
My working times can be flexible (yes-no)	64191	0.52	0.50	0	1
Entitled to flexible start/finish times	68387	0.60	0.49	0	1
Has bachelor degree	116056	0.22	0.41	0	1

Note: The table shows summary statistics for the full sample from HILDA, separately by gender. The dataset covers the years 2001-2019. We restrict the sample to people aged 15 to 65, and exclude individuals who have their first child in the first and last two years of the sample (2001-02 and 2018-19). For the individuals who have their first child in the years 2003-2017, we exclude observations earlier than six years prior to the childbirth and later than ten years afterwards. All variables are defined in Appendix B.

Table 2: Balance Table: Early vs Late Cohorts of Mothers-to-be

	Early cohort (2003-2008)			Late cohort (2010-2017)			Diff.	SE
	N	Mean	SD	N	Mean	SD		
Year first birth	434	2005.54	(1.73)	800	2013.80	(2.20)	8.25***	(0.12)
Age at first birth	434	28.74	(6.09)	800	29.04	(5.50)	0.30	(0.34)
In labor force	428	0.93	(0.26)	797	0.93	(0.25)	0.00	(0.02)
Weekly hours in paid employment (conditional)	375	39.19	(10.86)	700	38.17	(10.61)	-1.03	(0.68)
Log(1+weekly wage)	434	5.46	(2.41)	800	5.74	(2.52)	0.29	(0.15)
Log(1+annual labor income)	434	8.78	(3.58)	800	9.68	(3.08)	0.90***	(0.19)
Hourly wage	375	18.39	(8.52)	700	25.90	(11.80)	7.51***	(0.69)
Part-time if employed	375	0.21	(0.40)	700	0.20	(0.40)	-0.00	(0.03)
Permanent employment if employed (vs casual and fixed-term)	353	0.71	(0.45)	668	0.75	(0.43)	0.03	(0.03)
Overtime (more than 38 hours/week)	434	0.47	(0.50)	800	0.43	(0.50)	-0.04	(0.03)
Overtime (more than 50 hours/week)	434	0.12	(0.33)	800	0.09	(0.29)	-0.03	(0.02)
Promoted in the past year (conditional on current employment)	275	0.18	(0.38)	612	0.18	(0.38)	0.00	(0.03)
Normally supervise work of other employees	375	0.51	(0.50)	700	0.47	(0.50)	-0.04	(0.03)
Occupational status scale	375	54.72	(23.14)	699	57.91	(22.53)	3.19*	(1.46)
Regular schedule (M-F and regular daytime schedule)	375	0.59	(0.49)	700	0.63	(0.48)	0.04	(0.03)
M-F	375	0.62	(0.49)	700	0.65	(0.48)	0.02	(0.03)
Regular daytime schedule	375	0.80	(0.40)	700	0.83	(0.38)	0.03	(0.02)
On call and irregular shifts	375	0.06	(0.23)	700	0.05	(0.22)	-0.01	(0.01)
My working times can be flexible (yes-no)	110	0.37	(0.49)	609	0.45	(0.50)	0.07	(0.05)
Entitled to flexible start/finish times	243	0.53	(0.50)	555	0.59	(0.49)	0.06	(0.04)
Has bachelor degree	428	0.38	(0.49)	797	0.39	(0.49)	0.01	(0.03)

Note: The table shows the summary statistics for the sample of will-be mothers. All variables except for age are measured two years before childbirth. Early cohorts include women who had their first childbirth between 2003 and 2008, and late cohorts include women whose first child was born between 2009 and 2017. We restrict the sample to people aged 15 to 65. For the descriptions of the variable see Appendix B. Stars indicate statistical significance (+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

Table 3: Changes in work arrangements for Mothers

	Regularity				Flexibility		Work from home			
	(1) Regular schedule (M-F and regular daytime schedule)	(2) M-F	(3) Regular daytime schedule	(4) On call and irregular shifts	(5) My working times can be flexible (yes-no)	(6) Entitled to flexible start/finish times	(7) Hours work from home	(8) Any hours worked from home	(9) Hours work from home conditional on any	(10) Entitled to home- based work
First child 0-2	0.104*** (0.0235)	0.0975*** (0.0239)	0.0768*** (0.0200)	-0.0288 ⁺ (0.0157)	-0.0615* (0.0303)	0.00500 (0.0236)	0.508 ⁺ (0.294)	-0.0379 ⁺ (0.0221)	2.900** (1.024)	-0.0268 (0.0232)
× First birth after 2010										
First child 3-5	0.0578* (0.0266)	0.0633* (0.0267)	0.0737*** (0.0217)	-0.0450** (0.0170)	-0.0106 (0.0282)	0.0205 (0.0256)	0.880* (0.387)	0.0319 (0.0230)	1.779 ⁺ (0.989)	0.0161 (0.0250)
× First birth after 2013										
First child 6-8	0.0906** (0.0310)	0.0757* (0.0317)	0.0708** (0.0230)	-0.0265 (0.0173)	-0.0120 (0.0299)	0.0305 (0.0266)	1.248** (0.440)	0.0563* (0.0254)	1.831 (1.155)	0.0156 (0.0252)
× First birth after 2016										
Pre-period mean:										
First child aged 0-2	0.266	0.294	0.753	0.102	0.587	0.636	2.574	0.313	8.372	0.353
First child aged 3-5	0.282	0.300	0.759	0.123	0.579	0.638	2.823	0.310	9.265	0.381
First child aged 6-8	0.299	0.333	0.784	0.0902	0.556	0.617	3.140	0.329	9.647	0.329
Observations:										
N	83526	83532	83553	83553	61095	64853	88671	88671	17679	60302
N Individuals	9780	9781	9781	9781	8257	8489	10530	10530	3116	8244

Standard errors in parentheses

⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimates for the coefficients of a restricted version of equation (1), where we only include the dummies based on the age of the first child (meaning, we drop all the dummies based on the age of the youngest child), and we replace the year-by-year interactions with mobile “post” dummies. In particular, we interact the dummy for the first child being between 0 and 2 with an indicator for the year being equal or after 2010; the dummy for the first child being between 3 and 5 with an indicator for the year being equal or after 2013; the dummy for the first child being between 6 and 8 with an indicator for the year being equal or after 2016. These coefficients are reported in the first three lines. The regressions also include the dummies based on the age of the first child (without interactions) for age groups 0-2, 3-5, 6-8, 9-10, more than 10; as well as individual, time and age fixed effects; these coefficients are not reported. The bottom panel includes the pre-period means of the outcome variables, namely averages for the relevant group of the years for which the mobile “post” dummy is zero. Standard errors are clustered at the individual level. The outcomes considered are measures of regularity (for columns 1-4), flexibility (for columns 5 and 6), and work from home (for columns 7-10). All variables are defined in Appendix B.

Table 4: Child Penalties for Women: Main Labor Market Outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-5=1	-0.386*** (0.0159)	-18.05*** (0.585)	-0.773*** (0.0283)	3.877** (1.345)	0.0547** (0.0203)	-3.404*** (0.148)
First child aged 6-10=1	-0.303*** (0.0177)	-17.55*** (0.702)	-0.735*** (0.0310)	1.465 (1.035)	-0.00215 (0.0206)	-3.518*** (0.189)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-5=1 × First birth after 2010=1	0.0692*** (0.0193)	3.239*** (0.693)	0.187*** (0.0334)	-1.787 (1.559)	-0.0180 (0.0235)	0.780*** (0.187)
First child aged 6-10=1 × First birth after 2010=1	0.0465+ (0.0247)	2.620** (0.931)	0.126** (0.0406)	-0.368 (1.391)	0.00631 (0.0269)	0.722** (0.267)
Pre-birth mean:						
Early cohorts	0.887	38.22	3.578	19.09	2.884	8.878
Late cohorts	0.909	37.20	3.537	24.69	3.143	9.375
Observations:						
N	116955	77959	77959	77959	72074	122094
N Individuals	12209	9501	9501	9501	9159	12879
N New Parents	1539	1386	1386	1386	1354	1539

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table reports the estimates for the child penalty in various labor market outcomes, both as baseline, for the cohorts of women who became mother for the first time before the Fair Work Act (early cohorts), and as change (difference between late cohorts and early cohorts). More formally, the displayed coefficients are estimated as in equation (3): the top panel reports estimates for the baseline child penalty (coefficients γ_{0-5} and γ_{6-10}), while the bottom panel reports estimates for the change in child penalty (coefficients $\gamma_{0-5,post}$ and $\gamma_{6-10,post}$). All outcome variables are defined in Appendix B.

Table 5: Time Evolution of Child Penalty by Job Regularity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In labor force	In labor force	Weekly hours in paid work	Weekly hours in paid work	Log weekly hours in paid work	Log weekly hours in paid work	Regular schedule	Regular schedule
Child penalty 0-5	-0.381*** (0.0188)	-0.315*** (0.0366)	-17.97*** (0.676)	-15.79*** (1.157)	-0.773*** (0.0321)	-0.629*** (0.0541)	-0.449*** (0.0242)	-0.525*** (0.0465)
* tercile = 2			-0.0911+ (0.0484)	-4.898** (1.679)		-0.248** (0.0786)		-0.0973 (0.0606)
* tercile = 1			-0.0835+ (0.0473)	-1.456 (1.584)		-0.165* (0.0752)		0.298*** (0.0567)
Child penalty 0-5 * post-2010 first birth	0.0949*** (0.0225)	0.0619 (0.0419)	3.172*** (0.778)	1.254 (1.328)	0.196*** (0.0370)	0.0806 (0.0621)	0.0755* (0.0294)	0.0459 (0.0533)
* tercile = 2			0.0561 (0.0561)	5.381** (1.914)		0.270** (0.0898)		0.153* (0.0718)
* tercile = 1			0.0284 (0.0563)	0.240 (1.839)		0.0647 (0.0876)		-0.0289 (0.0672)
Early (pre-2010) cohorts: Mean Y pre-birth	0.941		38.45		3.588		0.591	
- tercile 3		0.960		41.52		3.692		0.822
- tercile 2		0.961		40.22		3.655		0.768
- tercile 1		0.913		34.90		3.462		0.287
Late (post-2010) cohorts: Mean Y pre-birth	0.961		37.36		3.547		0.604	
- tercile 3		0.981		39.84		3.648		0.812
- tercile 2		0.971		38.01		3.574		0.732
- tercile 1		0.935		34.53		3.431		0.296
Early cohorts: New parents	373		369		369		369	
- tercile 3		102		102		102		102
- tercile 2		118		117		117		117
- tercile 1		153		150		150		150
Late cohorts: New parents	678		676		676		676	
- tercile 3		200		200		200		200
- tercile 2		227		227		227		227
- tercile 1		251		249		249		249
Tot observations	56790	56790	47627	47627	47627	47627	47715	47715

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the estimates for the child penalty in various labor market outcomes (columns 1 to 6) and in schedule regularity (columns 7 and 8), separately by terciles of ex-ante job regularity. In columns (3) and (4), the outcome is weekly hours in employment conditional on working. More formally, even-numbered columns report coefficients that are estimated from equation (5), i.e. γ_a (first row), $\gamma_{a,2}$ (second row), $\gamma_{a,1}$ (third row), γ_b (fourth row), $\gamma_{b,2}$ (fifth row) and $\gamma_{b,1}$. Odd-numbered columns report estimates from a more basic specification that does not distinguish between exposure groups, i.e. the analogous of the first and third rows from Table 4, but restricted to the subsample of respondents who answer the question about regularity in their schedule. All outcome variables are defined in Appendix B.

Table 6: The Interaction between Paid Parental Leave and Regularity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In labor force	In labor force	In labor force	In labor force	Log weekly hours in paid employment			
First child aged 0-5	-0.351*** (0.0230)	-0.348*** (0.0283)	-0.321*** (0.0278)	-0.312*** (0.0351)	-0.784*** (0.0399)	-0.739*** (0.0464)	-0.763*** (0.0472)	-0.701*** (0.0517)
× Middle Tercile Regularity		-0.00976 (0.0488)		-0.0225 (0.0502)		-0.142 (0.0872)		-0.161+ (0.0881)
× not PPL eligible			-0.0635 (0.0463)	-0.0664 (0.0479)			-0.0502 (0.0825)	-0.0759 (0.0825)
First child aged 0-5 × First birth after 2010	0.0906*** (0.0270)	0.0886** (0.0335)	0.0517 (0.0331)	0.0439 (0.0416)	0.213*** (0.0458)	0.143** (0.0536)	0.226*** (0.0538)	0.142* (0.0595)
× Middle Tercile Regularity		0.00917 (0.0568)		0.0227 (0.0581)		0.212* (0.0995)		0.222* (0.100)
× not PPL eligible			0.0782 (0.0546)	0.0811 (0.0561)			-0.0475 (0.0967)	-0.0199 (0.0964)
Observations	35269	35269	35269	35269	30379	30379	30379	30379
N Individuals	2461	2461	2461	2461	2459	2459	2459	2459
N New Parents	671	671	671	671	670	670	670	670

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the estimates for the child penalty in labor supply at the extensive margin (columns 1-4) and at the intensive margin (columns 5-8), separately by exposure to the Fair Work Act and to the Paid Parental Leave introduction. Columns 1 and 5 report simple estimates that do not control for exposure to any of the two measures; columns 2 and 6 account for exposure to the Fair Work Act by estimating the child penalty separately for mothers who were in the middle tercile of regularity distribution versus top and bottom; columns 3 and 7 account for exposure to the introduction of Paid Parental Leave by estimating the child penalty for parents who were and were not already eligible; columns 4 and 8 account for both.

Table 7: Child Penalties in Time Use for Women

	(1) Playing with children (hrs/week)	(2) Housework (hours/week)	(3) Paid employment (hrs/week)	(4) Other activities (hrs/week)	(5) Physical activity (hrs/week)	(6) Social activity (hrs/week)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-2=1	48.08*** (0.948)	9.183*** (0.443)	-24.38*** (0.658)	-2.814*** (0.200)	-0.343*** (0.0637)	0.650*** (0.107)
First child aged 3-5=1	42.67*** (1.019)	11.38*** (0.514)	-23.56*** (0.754)	-2.171*** (0.236)	-0.482*** (0.0729)	0.549*** (0.113)
First child aged 6-8=1	34.85*** (0.983)	11.35*** (0.575)	-22.24*** (0.827)	-2.058*** (0.278)	-0.278*** (0.0807)	0.423*** (0.125)
First child aged 9-10=1	32.49*** (0.988)	10.32*** (0.565)	-21.01*** (0.905)	-1.811*** (0.331)	-0.255** (0.0902)	0.361** (0.130)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-2=1 × First birth after 2010=1	0.228 (1.221)	-1.593** (0.532)	2.815*** (0.815)	0.150 (0.259)	-0.113 (0.0804)	-0.210 (0.132)
First child aged 3-5=1 × First birth after 2010=1	0.184 (1.286)	-2.069** (0.634)	3.557*** (0.934)	0.0109 (0.327)	-0.0476 (0.0950)	-0.300* (0.143)
First child aged 6-8=1 × First birth after 2010=1	2.511 ⁺ (1.411)	-2.495*** (0.738)	3.409** (1.083)	0.354 (0.428)	-0.216 ⁺ (0.118)	-0.259 (0.170)
First child aged 9-10=1 × First birth after 2010=1	3.918 ⁺ (2.180)	-0.286 (1.346)	3.214 (1.965)	-0.197 (0.948)	0.0619 (0.227)	-0.188 (0.312)
Pre-birth mean:						
Early cohorts	0.448	9.319	31.77	3.540	2.568	3.659
Late cohorts	0.395	7.707	32.45	4.509	2.657	3.374
Observations:						
N	92991	98187	96944	122094	105587	104702
N Individuals	10973	11125	11087	12879	11624	11595
N New Parents	1504	1513	1510	1539	1518	1517

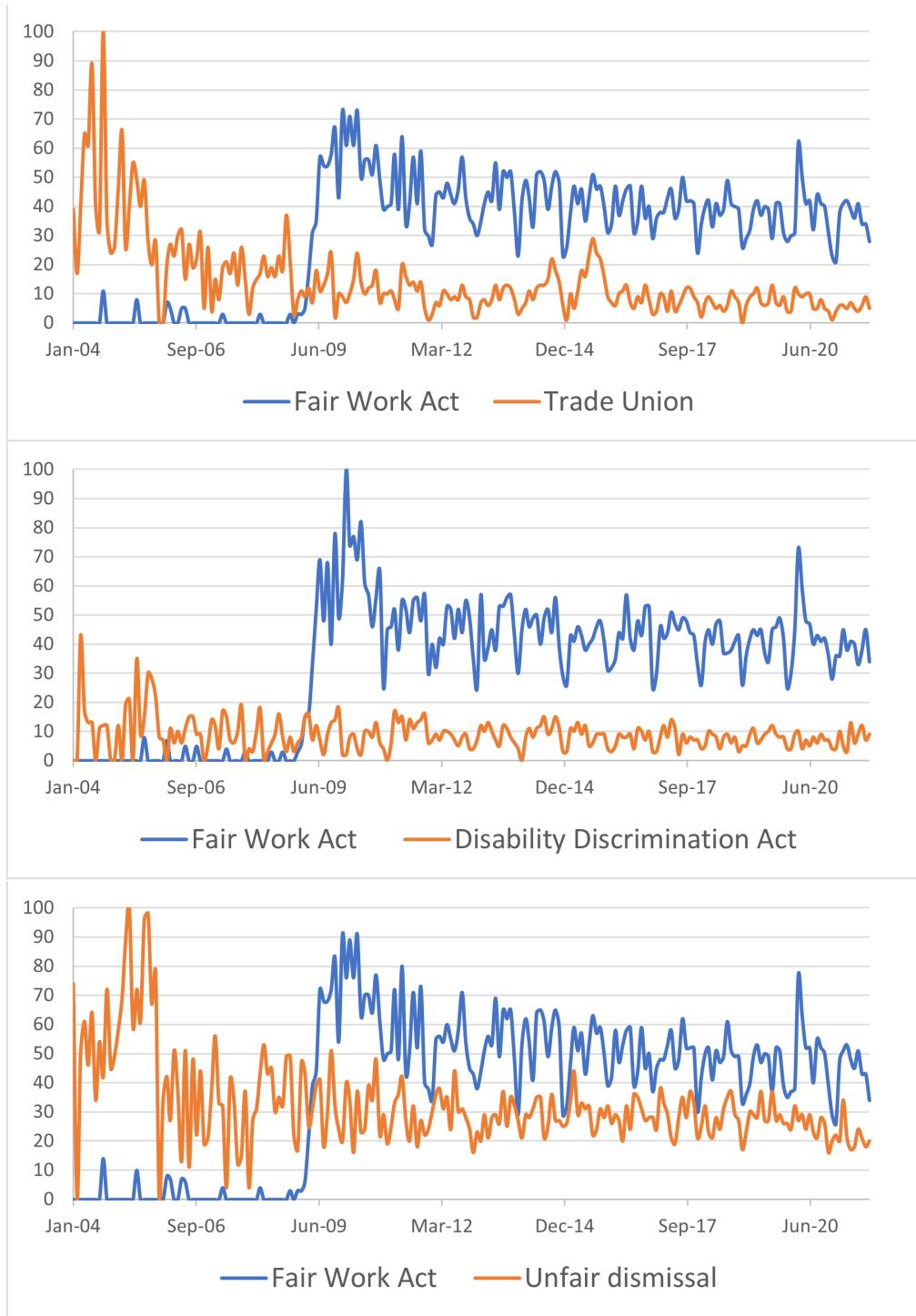
Standard errors in parentheses; ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the outcome variables are weekly hours spent in various non-labor market activities, and in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10. Similarly to Table 4 we are showing the child penalty both at baseline and in change. All outcome variables are defined in Appendix B.

A Appendix

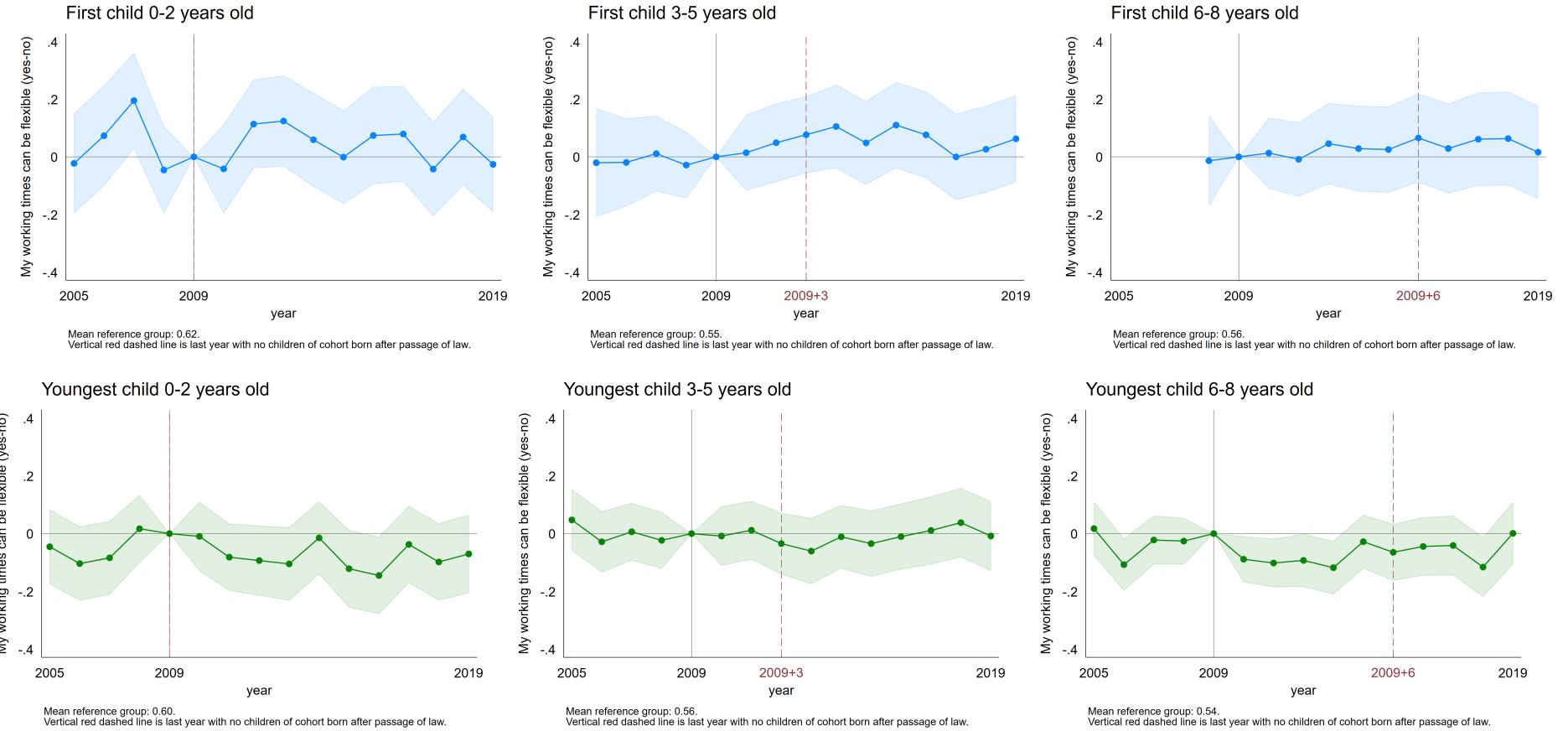
A.1 Appendix figures

Figure A.1: Google searches



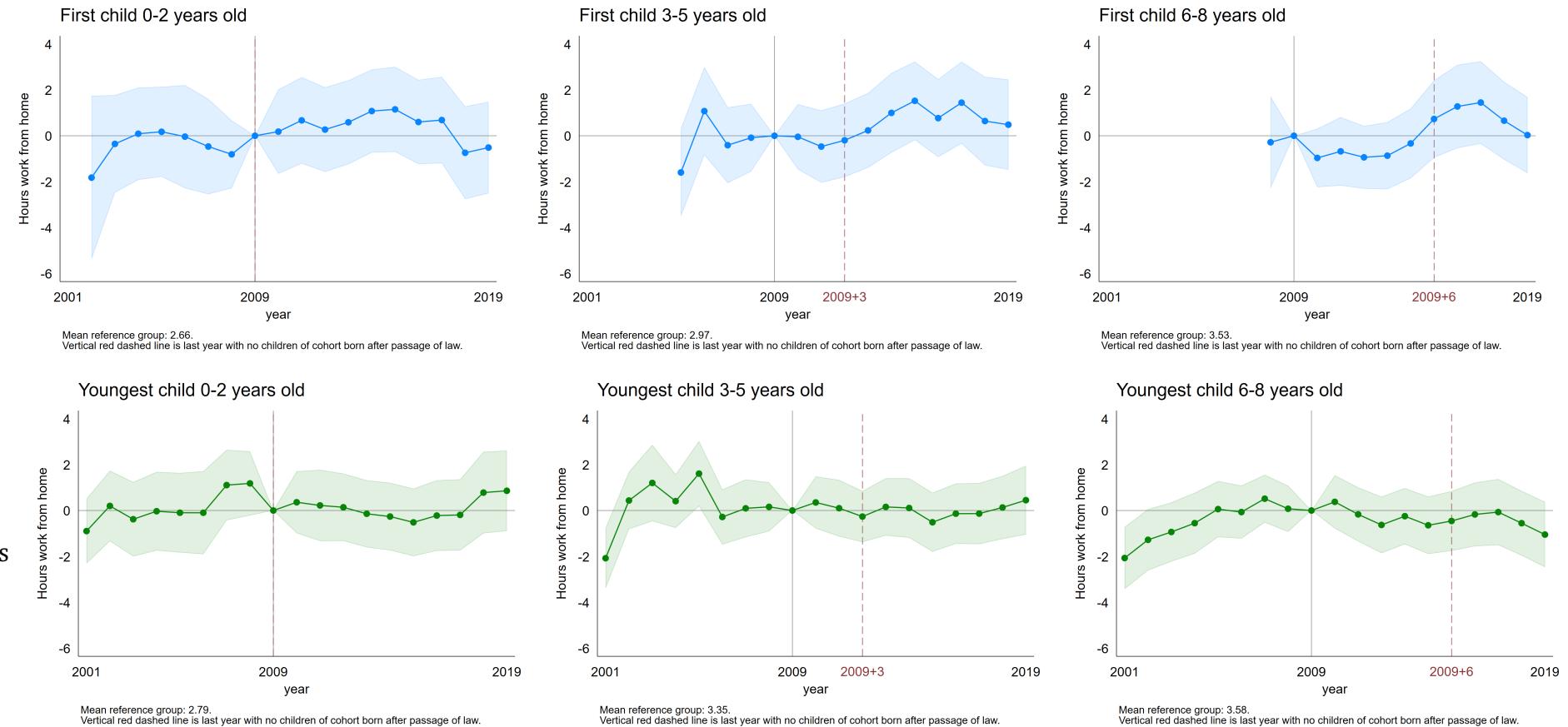
Note: This figure shows the frequency of Google Searches for the query "Fair Work Act", compared with the corresponding frequencies for other labor-market related queries – "Trade Union", "Disability Discrimination Act" and "Unfair dismissal", respectively. The series are scaled to be 100 on the day with the maximum number of queries, for the series in which this peak is higher. The time horizon covers the years from 2004 to 2020 (the Fair Work Act was approved in July 2009).

Figure A.2: “I am entitled to flexible start and finish times” for mothers relative to non-mothers, by year and age of child



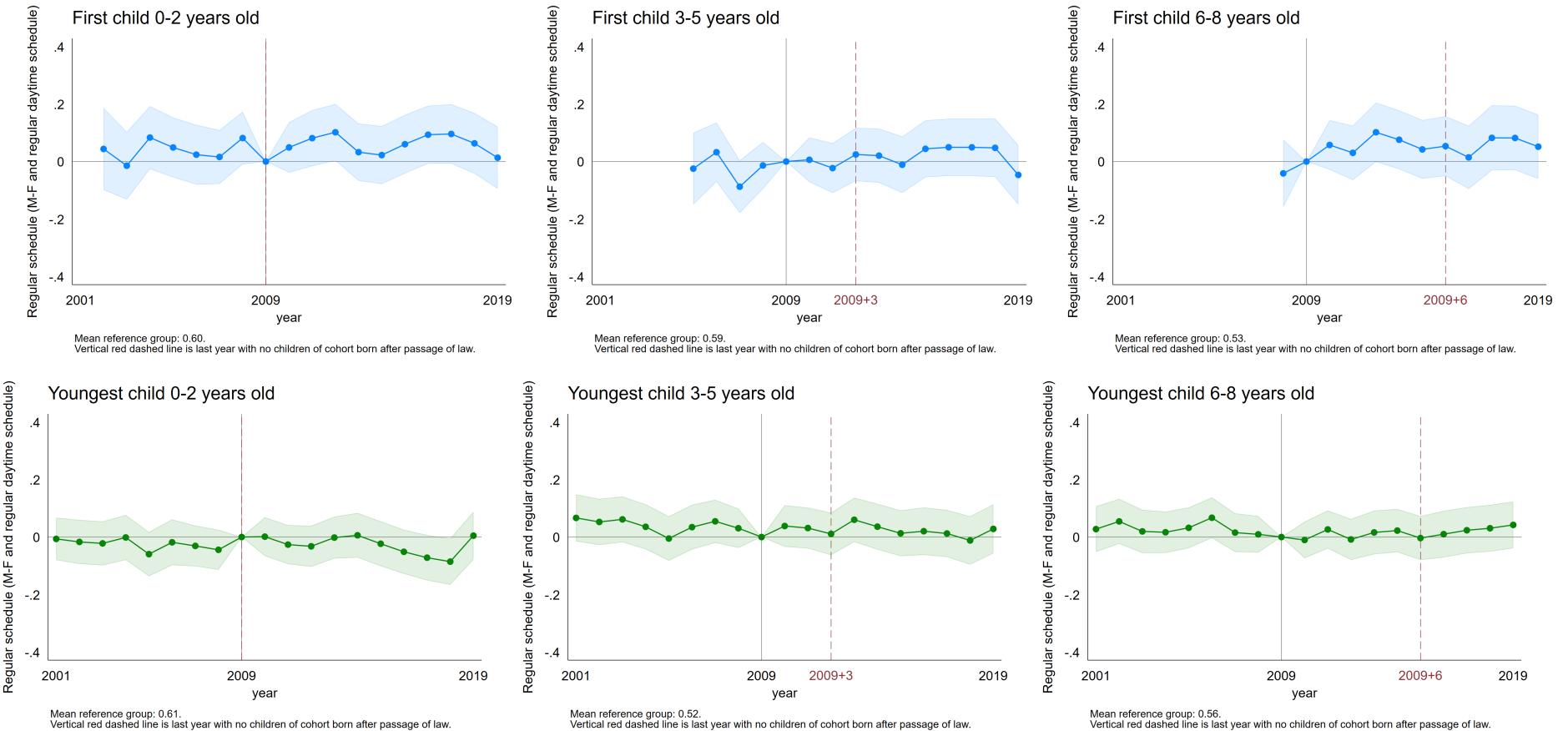
Note: This figure represents the evolution over time of flexibility for women over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is “I am entitled to flexible start and finish times”. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimes for different ranges of child’s age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure A.3: Hours worked from home, for mothers relative to non-mothers, by year and age of child



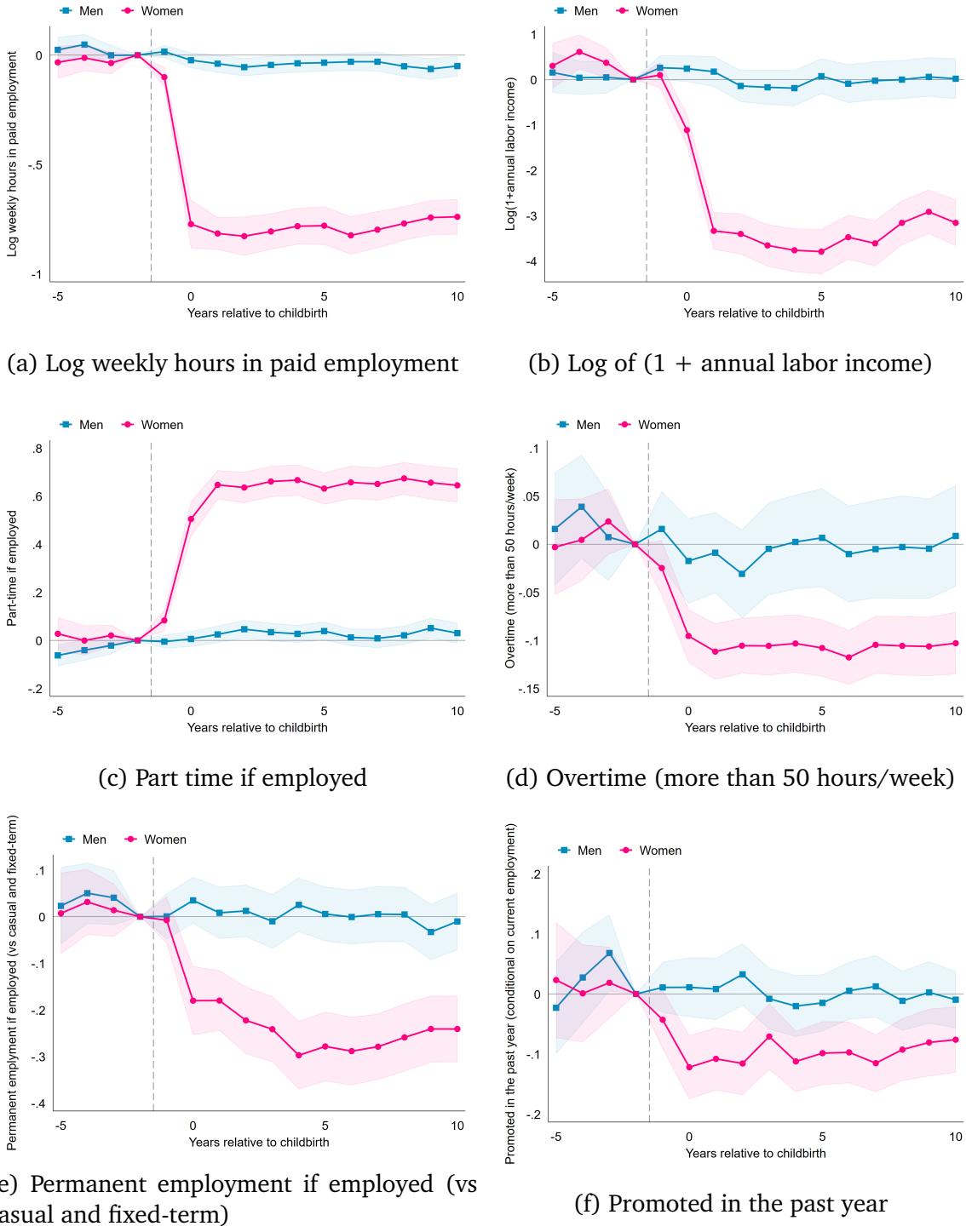
Note: This figure represents the evolution over time of hours worked from home for women over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is the number of hours worked from home, conditional on being employed. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimates for different ranges of child's age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure A.4: “Regular schedule” for fathers relative to non-fathers, by year and age of child



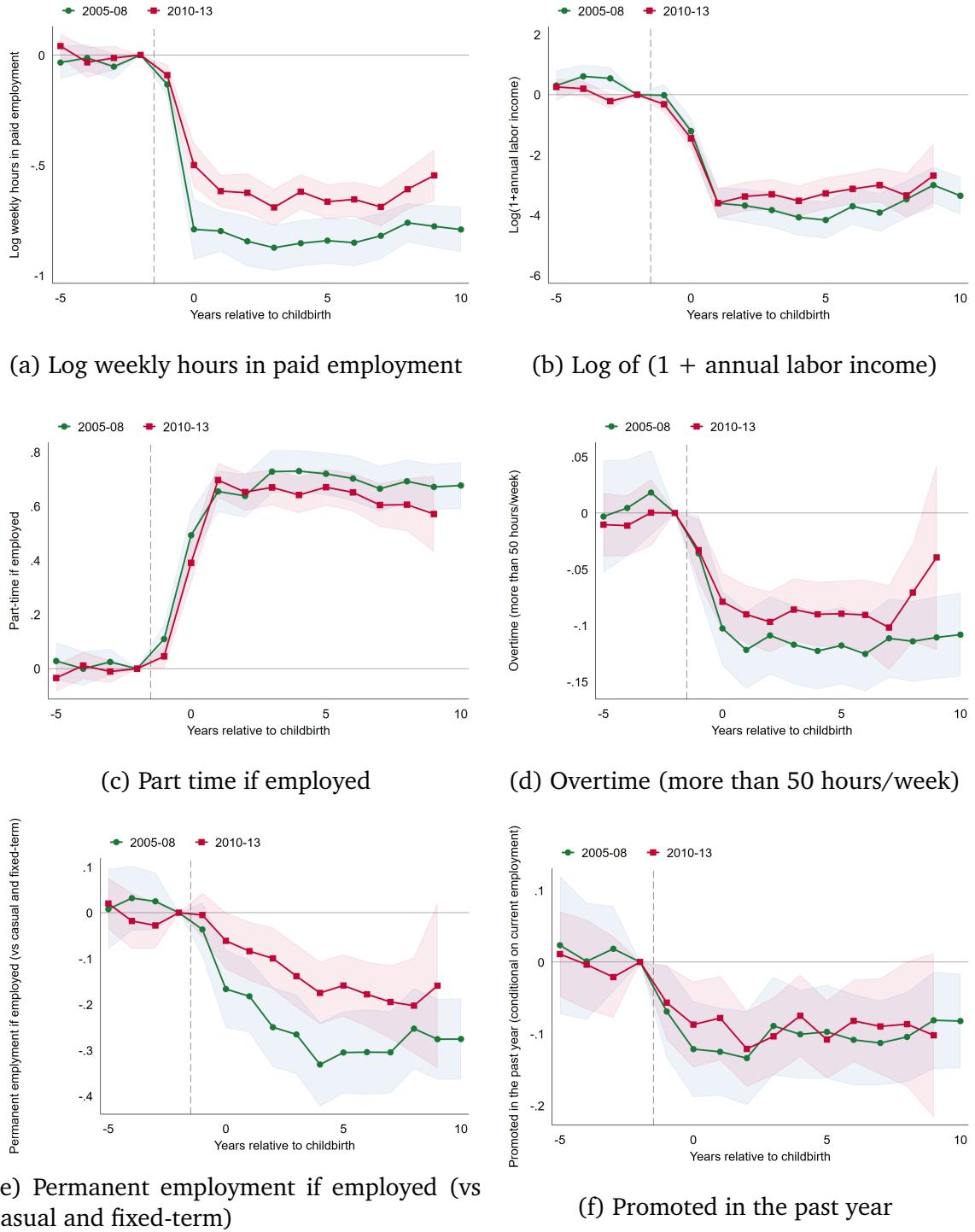
Note: This figure represents the evolution over time of regularity for men over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is “Regular Schedule”, a dummy for working from Monday to Friday on a regular daytime schedule. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimates for different ranges of child’s age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure A.5: Child penalty by gender: other labor market outcomes



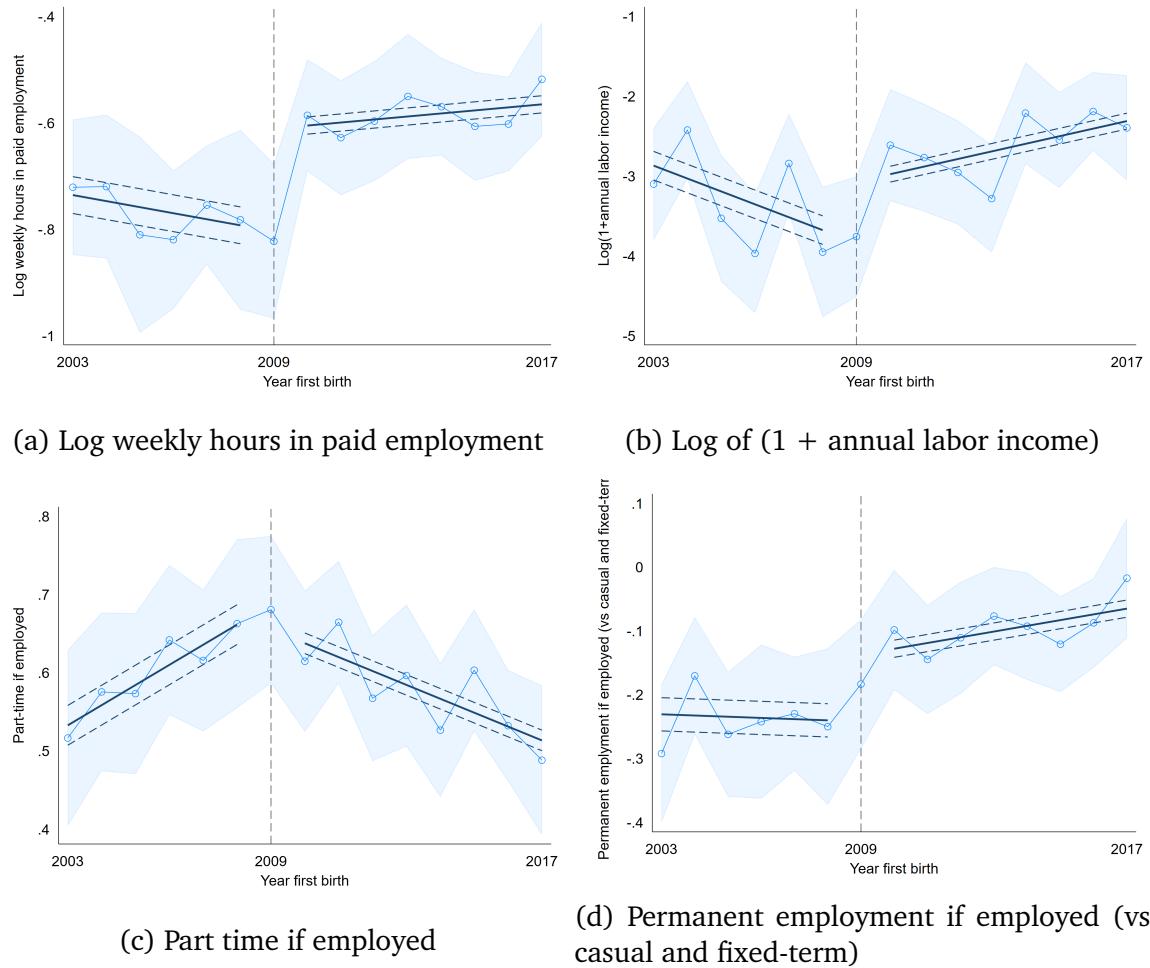
Note: This figure shows the child penalty in various labor market outcomes, as described in Section 4.1.1. Namely, the figure plots the coefficients γ_k 's from estimating equation (2), which includes individual, time and age fixed effects. In all panels, the blue squares are estimates from a regression that includes only men, while the pink circles are estimates from a regression that includes only women. The shades around them are 95% confidence intervals. We only include parents who have their first child between 2003 and 2008, and we include non-parents as controls - see Section 4.1.1 for details. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level. The outcome variables considered are defined in Appendix B.

Figure A.6: Change in the child penalty for women: other labor market outcomes



Note: This figure shows how the child penalty in various labor market outcomes has changed, for women, over the sample period. All panels plot the child penalty in labor supply by year relative to childbirth (the coefficients γ_k 's in equation (2)) for women, estimated separately for early cohorts (first childbirth between 2005 and 2008, green circles) and late cohorts (first childbirth between 2010 and 2013, red squares). Women without children are included in both regressions as controls, and women having children in other sample years are excluded from both regression. See note to Figure 3 for further details. The outcome variables considered are defined in Appendix B.

Figure A.7: Time evolution of the child penalty for women: other labor market outcomes



Note: This figure shows how the child penalty in various labor market outcomes has changed, for women, over the sample period. All panels display the 0-5 years child penalty (average of the child penalty from zero to five years after the birth of the first child) for women by year of birth of the first child. That is, they plot the estimates of $\gamma_{0-5,c}$ from equation (4). The blue circles are the estimates yearly cohort-by-yearly cohort, and the shades around them are 95% confidence intervals. The solid lines are the linear trends estimated for childbirth cohorts 2003-2008 and 2010-2017, and the dashed lines are the corresponding 95% confidence intervals, as described in Section 4.2.2. We cluster standard errors at the individual level. The outcome variables considered are defined in Appendix B.

Figure A.8: Time evolution of the child penalty for women (persistence)



Note: This figure shows how the child penalty in labor supply, wages, and earnings for women has changed over the sample period. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is the natural logarithm of hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. All panels display the 0-5 and the 6-10 years child penalty (average of the child penalty from zero to five years, and from six to ten years, after the birth of the first child) for women by year of birth of the first child. That is, they plot the estimates of $\gamma_{0-5,c}$ and $\gamma_{6-10,c}$ from equation (4). The circles are the estimates yearly cohort-by-yearly cohort, and the shades around them are 95% confidence intervals. We cluster standard errors at the individual level.

Figure A.9: Permutation test

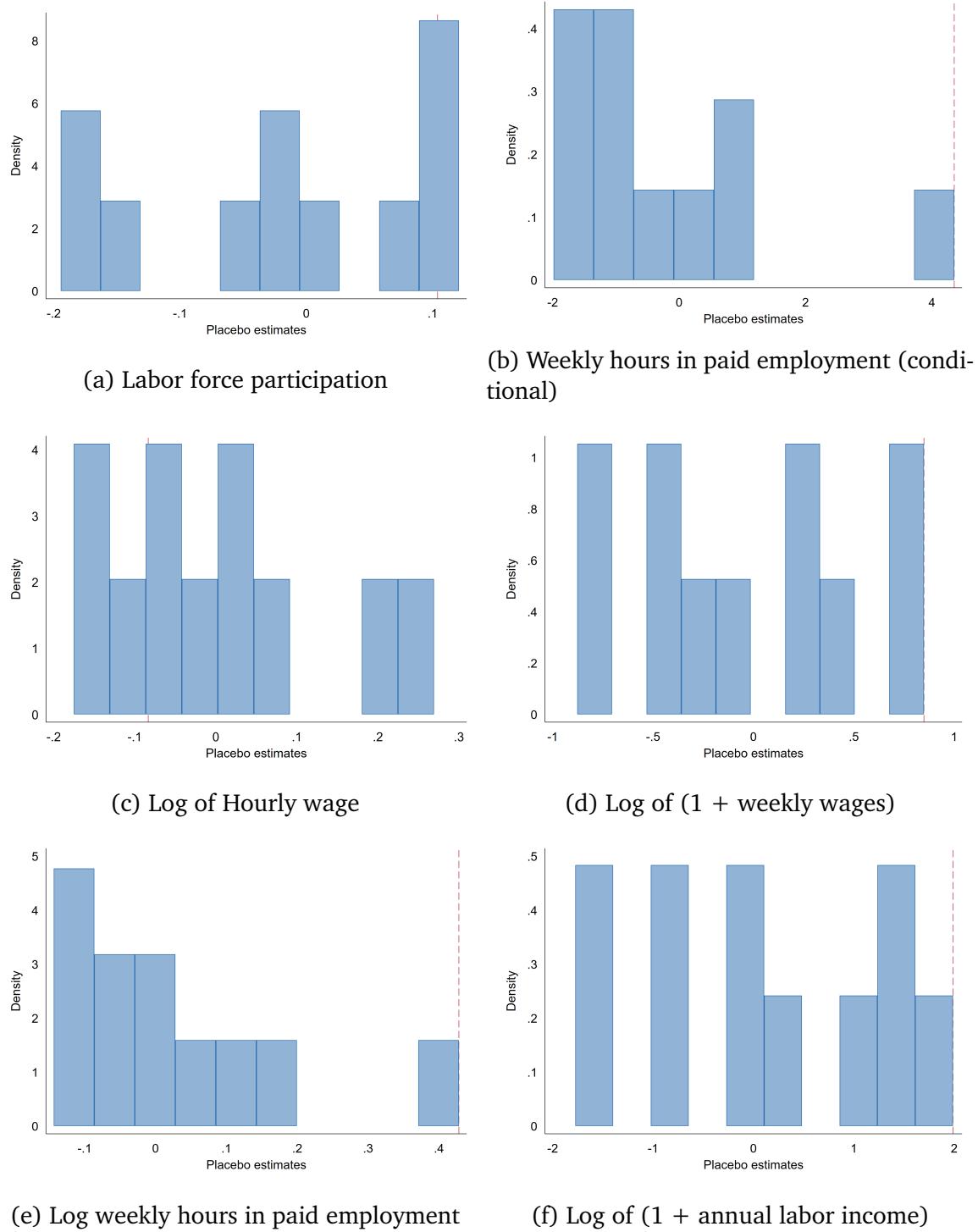
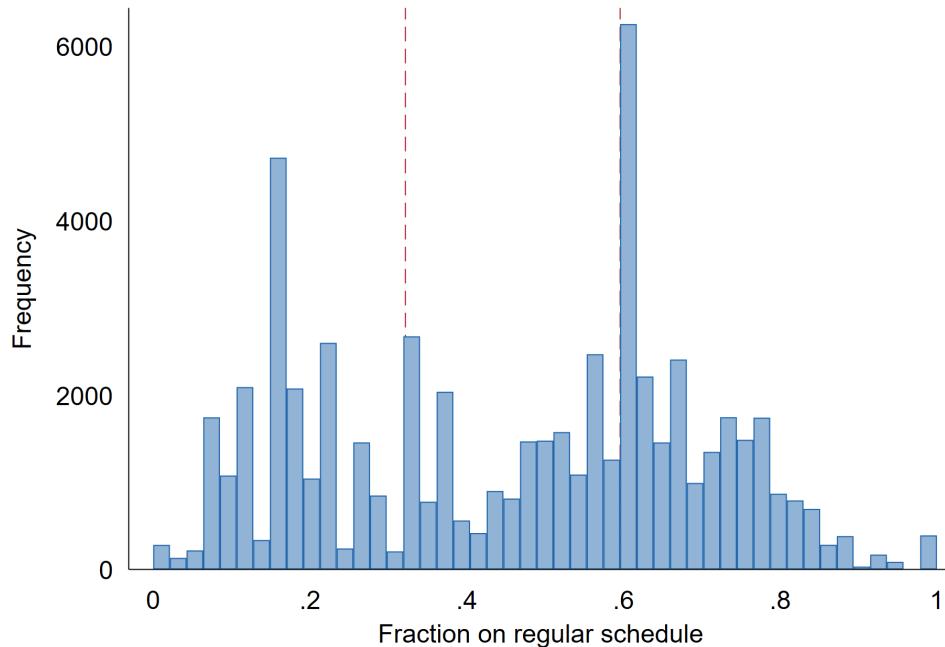
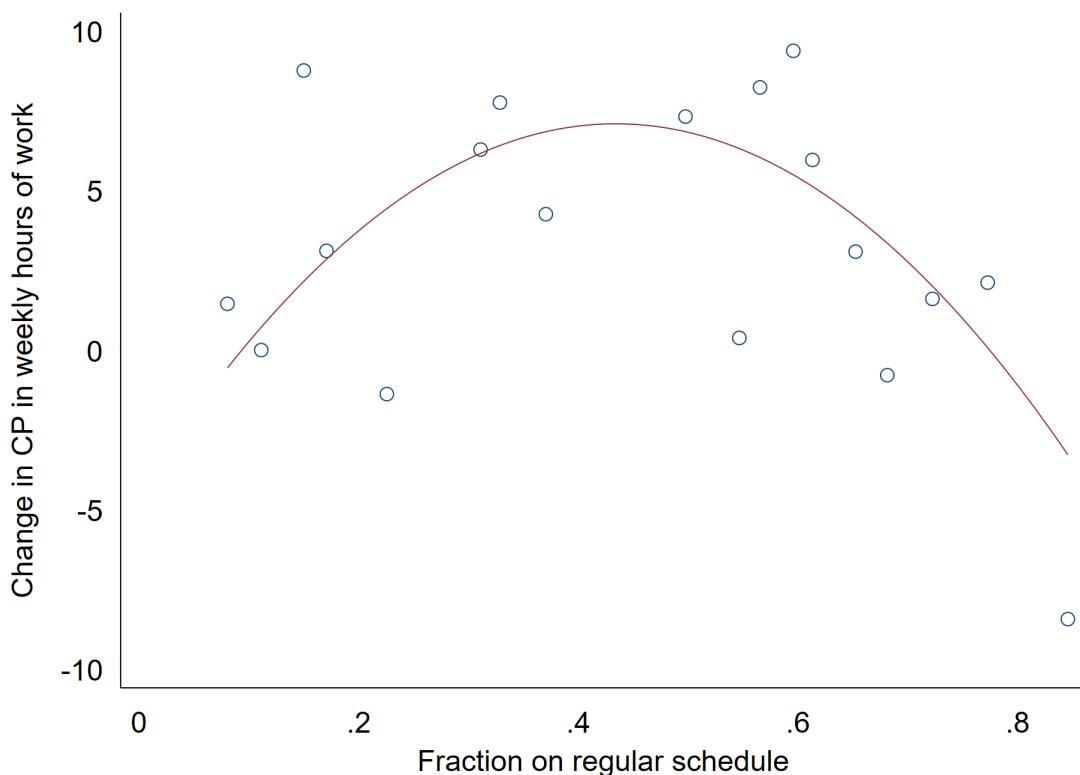


Figure A.10: Distribution of regularity



The vertical dashed lines correspond to the 33rd and the 66th percentile

Figure A.11: The change in child penalty is non-monotonic in regularity



Note: This figure displays the relationship between the standard deviation of regularity (our measure of exposure to the Fair Work Act) and the change in child penalty in the intensive margin of labor supply (weekly hours in paid employment). The latter is defined as the coefficient $\gamma_{0-5,post}$ in equation (3).

A.2 Appendix tables

Table A.1: Balance Table: Occupation and Industry of Early vs Late Cohorts of Mothers

	Early cohort (2003-2008)			Late cohort (2010-2017)			Diff.	SE
	N	Mean	SD	N	Mean	SD		
<i>Occupations</i>								
Managers	375	0.11	(0.32)	699	0.11	(0.32)	-0.00	(0.02)
Professionals	375	0.33	(0.47)	699	0.37	(0.48)	0.05	(0.03)
Technicians and Trade Workers	375	0.06	(0.25)	699	0.04	(0.20)	-0.02	(0.01)
Community and Personal Service Workers	375	0.12	(0.32)	699	0.14	(0.35)	0.02	(0.02)
Clerical and Admin Workers	375	0.19	(0.39)	699	0.20	(0.40)	0.01	(0.03)
Sales Workers	375	0.13	(0.33)	699	0.09	(0.28)	-0.04*	(0.02)
Machinery Operators and Drivers	375	0.01	(0.09)	699	0.00	(0.07)	-0.00	(0.00)
Labourers	375	0.05	(0.22)	699	0.04	(0.19)	-0.02	(0.01)
<i>Industries</i>								
Agriculture, Forestry and Fishing	373	0.01	(0.10)	696	0.01	(0.10)	-0.00	(0.01)
Mining	373	0.01	(0.10)	696	0.01	(0.11)	0.00	(0.01)
Manufacturing	373	0.06	(0.24)	696	0.04	(0.19)	-0.02	(0.01)
Electricity, Gas, Water, Waste	373	0.01	(0.07)	696	0.00	(0.07)	-0.00	(0.00)
Construction	373	0.02	(0.13)	696	0.02	(0.15)	0.01	(0.01)
Wholesale Trade	373	0.02	(0.14)	696	0.02	(0.15)	0.01	(0.01)
Retail Trade	373	0.13	(0.33)	696	0.09	(0.28)	-0.04*	(0.02)
Accommodation and Food Services	373	0.09	(0.29)	696	0.08	(0.27)	-0.02	(0.02)
Transport, Postal, Wharehousing	373	0.02	(0.13)	696	0.02	(0.12)	-0.00	(0.01)
Information Media and Telecommunications	373	0.03	(0.17)	696	0.02	(0.15)	-0.01	(0.01)
Financial and Insurance Services	373	0.05	(0.21)	696	0.05	(0.23)	0.01	(0.01)
Rental, Hiring, Real Estate	373	0.02	(0.13)	696	0.02	(0.13)	0.00	(0.01)
Professional, Scientific and Technical Services	373	0.09	(0.29)	696	0.10	(0.30)	0.01	(0.02)
Admin and Support Services	373	0.02	(0.15)	696	0.04	(0.19)	0.02	(0.01)
Public Admin and Safety	373	0.07	(0.26)	696	0.07	(0.25)	-0.00	(0.02)
Education and Training	373	0.13	(0.34)	696	0.14	(0.35)	0.01	(0.02)
Healthcare and Social Assistance	373	0.18	(0.38)	696	0.21	(0.41)	0.04	(0.03)
Arts and Recreation Services	373	0.02	(0.15)	696	0.02	(0.13)	-0.00	(0.01)
Other Services	373	0.03	(0.18)	696	0.04	(0.19)	0.00	(0.01)

Note: See note to Table 2. Each raw represents a dummy variable taking the value of one if the individual works in the given occupation / industry.

Table A.2: Changes in work arrangements for fathers

	Predictability				Flexibility		Work from home			
	(1) Regular schedule (M-F and regular daytime schedule)	(2) M-F	(3) Regular daytime schedule	(4) On call and irregular shifts	(5) My working times can be flexible (yes-no)	(6) Entitled to flexible start/finish times	(7) Hours work from home	(8) Any hours worked from home	(9) Hours work from home conditional on any	(10) Entitled to home- based work
First child 0-2	0.0154	0.0177	0.00520	0.00375	0.0273	0.0249	0.0123	-0.0136	0.606	0.0216
× First birth after 2010	(0.0184)	(0.0182)	(0.0156)	(0.0110)	(0.0217)	(0.0207)	(0.257)	(0.0160)	(0.848)	(0.0181)
First child 3-5	0.0151	0.0176	0.00928	-0.00981	0.0189	-0.0444*	-0.191	-0.000137	-1.054	-0.0185
× First birth after 2013	(0.0215)	(0.0213)	(0.0175)	(0.0124)	(0.0229)	(0.0219)	(0.337)	(0.0189)	(0.947)	(0.0214)
First child 6-8	-0.00850	-0.00643	0.00631	-0.00306	-0.00566	0.000884	0.0148	0.0165	-0.676	0.0152
× First birth after 2016	(0.0259)	(0.0252)	(0.0210)	(0.0136)	(0.0254)	(0.0251)	(0.442)	(0.0216)	(1.059)	(0.0225)
Pre-period mean:										
First child aged 0-2	0.555	0.604	0.790	0.0789	0.510	0.587	2.446	0.251	9.826	0.252
First child aged 3-5	0.589	0.625	0.802	0.0867	0.519	0.645	2.880	0.274	10.67	0.309
First child aged 6-8	0.618	0.652	0.798	0.0863	0.581	0.665	3.475	0.288	12.17	0.314
Observations:										
N	89605	89617	89621	89621	62311	66164	98520	98520	20737	61043
N Individuals	10063	10063	10065	10065	8200	8397	11182	11182	3494	8130

Standard errors in parentheses

* $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows estimates analogous to those in Table 3, for the subsample of fathers. See note to Table 3.

Table A.3: Child penalties for men: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-2=1	-0.0132 (0.00833)	-1.782*** (0.390)	-0.0573*** (0.0128)	0.961 ⁺ (0.547)	0.0424** (0.0140)	-0.0439 (0.115)
First child aged 3-5=1	-0.0217* (0.00984)	-1.505** (0.485)	-0.0525*** (0.0148)	1.376 ⁺ (0.740)	0.0370* (0.0167)	-0.219 (0.146)
First child aged 6-8=1	-0.0133 (0.0111)	-1.783*** (0.533)	-0.0629*** (0.0171)	2.199* (1.020)	0.0357 ⁺ (0.0198)	-0.209 (0.167)
First child aged 9-10=1	-0.0248 ⁺ (0.0129)	-2.395*** (0.626)	-0.0830*** (0.0195)	2.861* (1.165)	0.0296 (0.0239)	-0.112 (0.186)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-2=1 × First birth after 2010=1	-0.00863 (0.0103)	0.436 (0.504)	0.0109 (0.0161)	0.549 (0.745)	-0.0166 (0.0179)	-0.108 (0.149)
First child aged 3-5=1 × First birth after 2010=1	0.00519 (0.0121)	-0.332 (0.610)	-0.0210 (0.0190)	0.228 (1.013)	-0.0140 (0.0218)	0.0343 (0.186)
First child aged 6-8=1 × First birth after 2010=1	-0.0187 (0.0156)	-0.770 (0.790)	-0.0403 (0.0247)	-0.600 (1.418)	-0.0229 (0.0264)	-0.0867 (0.242)
First child aged 9-10=1 × First birth after 2010=1	0.0181 (0.0242)	-0.857 (1.201)	-0.0301 (0.0329)	0.0930 (2.838)	-0.0465 (0.0572)	-0.0864 (0.373)
Pre-birth mean:						
Early cohorts	0.952	44.35	3.737	20.52	2.963	9.220
Late cohorts	0.952	43.55	3.720	27.85	3.277	9.627
Observations:						
N	107225	84018	84018	84018	74203	116055
N Individuals	11732	9802	9802	9802	9279	12748
N New Parents	1338	1302	1302	1302	1281	1338

Standard errors in parentheses; ⁺ $p < 0.10$, ^{*} $p < 0.05$, ^{**} $p < 0.01$, ^{***} $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the subsample consider is fathers, and in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10. See note to Table 4 for additional details.

Table A.4: Child penalties for women: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-2=1	-0.398*** (0.0167)	-18.01*** (0.609)	-0.777*** (0.0303)	5.524** (2.003)	0.0909*** (0.0236)	-2.843*** (0.140)
First child aged 3-5=1	-0.369*** (0.0188)	-18.19*** (0.702)	-0.772*** (0.0336)	2.099 (1.384)	0.0159 (0.0249)	-4.098*** (0.191)
First child aged 6-8=1	-0.309*** (0.0187)	-18.07*** (0.734)	-0.758*** (0.0331)	1.841 (1.150)	0.0124 (0.0225)	-3.777*** (0.202)
First child aged 9-10=1	-0.281*** (0.0202)	-16.99*** (0.778)	-0.709*** (0.0340)	0.339 (1.132)	-0.0378 (0.0234)	-3.364*** (0.210)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-2=1 × First birth after 2010=1	0.0608** (0.0206)	4.015*** (0.729)	0.225*** (0.0361)	-3.600 (2.220)	-0.0547* (0.0272)	0.419* (0.179)
First child aged 3-5=1 × First birth after 2010=1	0.0936*** (0.0232)	2.138* (0.840)	0.136*** (0.0397)	0.0482 (1.676)	0.0167 (0.0291)	1.064*** (0.244)
First child aged 6-8=1 × First birth after 2010=1	0.0639* (0.0256)	2.727** (0.965)	0.131** (0.0429)	-0.745 (1.522)	-0.0103 (0.0293)	0.846** (0.279)
First child aged 9-10=1 × First birth after 2010=1	0.0331 (0.0452)	3.540* (1.500)	0.168** (0.0567)	-0.741 (2.091)	0.0104 (0.0458)	0.770 (0.472)
Pre-birth mean:						
Early cohorts	0.887	38.22	3.578	19.09	2.884	8.878
Late cohorts	0.909	37.20	3.537	24.69	3.143	9.375
Observations:						
N	116955	77959	77959	77959	72074	122094
N Individuals	12209	9501	9501	9501	9159	12879
N New Parents	1539	1386	1386	1386	1354	1539

Standard errors in parentheses; ⁺ $p < 0.10$, ^{*} $p < 0.05$, ^{**} $p < 0.01$, ^{***} $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the subsample consider is mothers, and in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10. See note to Table 4 for additional details.

Table A.5: Child penalties for women: other labor market outcomes

	(1) Part-time if employed	(2) Overtime (more than 38 hours/week)	(3) Overtime (more than 50 hours/week)	(4) Permanent employment if employed (vs casual and fixed-term)	(5) Normally supervise work of other employees	(6) Promoted in the past year (conditional on current employment)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-2=1	0.600*** (0.0219)	-0.392*** (0.0157)	-0.103*** (0.00946)	-0.194*** (0.0228)	-0.124*** (0.0225)	-0.104*** (0.0153)
First child aged 3-5=1	0.632*** (0.0243)	-0.407*** (0.0182)	-0.106*** (0.0109)	-0.273*** (0.0255)	-0.154*** (0.0240)	-0.0802*** (0.0161)
First child aged 6-8=1	0.634*** (0.0271)	-0.402*** (0.0193)	-0.107*** (0.0109)	-0.274*** (0.0278)	-0.152*** (0.0252)	-0.0819*** (0.0161)
First child aged 9-10=1	0.619*** (0.0297)	-0.379*** (0.0213)	-0.104*** (0.0119)	-0.245*** (0.0290)	-0.142*** (0.0287)	-0.0624** (0.0195)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-2=1 × First birth after 2010=1	-0.0611* (0.0269)	0.0548** (0.0197)	0.0233* (0.0111)	0.125*** (0.0272)	0.0494+ (0.0277)	0.0178 (0.0185)
First child aged 3-5=1 × First birth after 2010=1	-0.00293 (0.0303)	0.0396+ (0.0226)	0.0232+ (0.0131)	0.131*** (0.0319)	0.0448 (0.0310)	-0.00992 (0.0198)
First child aged 6-8=1 × First birth after 2010=1	-0.0437 (0.0373)	0.0575* (0.0269)	0.0298* (0.0146)	0.0978** (0.0371)	0.0539 (0.0360)	0.00906 (0.0237)
First child aged 9-10=1 × First birth after 2010=1	-0.0667 (0.0601)	0.0743 (0.0474)	0.0734* (0.0345)	0.0662 (0.0755)	0.0392 (0.0650)	-0.0340 (0.0470)
Pre-birth mean:						
Early cohorts	0.221	0.440	0.110	0.716	0.498	0.159
Late cohorts	0.226	0.399	0.0808	0.701	0.446	0.157
Observations:						
N	78031	122094	122094	69088	78142	67308
N Individuals	9501	12879	12879	8846	9504	8719
N New Parents	1386	1539	1539	1342	1387	1340

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table A.4 but considers different labor market outcomes. All variables are defined in Appendix B.

Table A.6: Child penalties for women: work arrangements (continues in next page)

	Regularity				Flexibility	
	(1) Regular schedule (M-F and regular daytime schedule)	(2) M-F	(3) Regular daytime schedule	(4) On call and irregular shifts	(5) My working times can be flexible (yes-no)	(6) Entitled to flexible start/finish times
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-2=1	-0.427*** (0.0222)	-0.421*** (0.0227)	-0.116*** (0.0188)	0.0752*** (0.0152)	0.152*** (0.0295)	0.0316 (0.0219)
First child aged 3-5=1	-0.462*** (0.0250)	-0.461*** (0.0254)	-0.142*** (0.0212)	0.101*** (0.0164)	0.163*** (0.0309)	0.0378 (0.0248)
First child aged 6-8=1	-0.475*** (0.0270)	-0.458*** (0.0279)	-0.124*** (0.0211)	0.0815*** (0.0152)	0.161*** (0.0333)	0.0337 (0.0257)
First child aged 9-10=1	-0.427*** (0.0296)	-0.419*** (0.0307)	-0.134*** (0.0222)	0.0761*** (0.0157)	0.154*** (0.0355)	0.0233 (0.0289)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-2=1 × First birth after 2010=1	0.0933*** (0.0276)	0.0886** (0.0278)	0.0700** (0.0219)	-0.0380* (0.0174)	-0.0463 (0.0338)	0.00973 (0.0264)
First child aged 3-5=1 × First birth after 2010=1	0.0679* (0.0322)	0.0730* (0.0323)	0.0876*** (0.0254)	-0.0550** (0.0197)	-0.0162 (0.0357)	0.0218 (0.0298)
First child aged 6-8=1 × First birth after 2010=1	0.130** (0.0397)	0.115** (0.0403)	0.0797** (0.0284)	-0.0369+ (0.0210)	-0.00705 (0.0421)	0.0414 (0.0332)
First child aged 9-10=1 × First birth after 2010=1	0.0949 (0.0647)	0.0906 (0.0660)	0.120** (0.0413)	-0.0637* (0.0285)	0.000206 (0.0637)	0.0770 (0.0539)
Pre-birth mean:						
Early cohorts	0.597	0.625	0.803	0.0659	0.411	0.563
Late cohorts	0.599	0.627	0.802	0.0567	0.433	0.590
Observations:						
N	78115	78121	78140	78140	57256	60698
N Individuals	9503	9504	9504	9504	7979	8220
N New Parents	1387	1387	1387	1387	1290	1289

Standard errors in parentheses; + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.6: Child penalties for women: work arrangements (continues from previous page)

	Work from home			
	(7) Hours work from home	(8) Any hours worked from home	(9) Hours work from home conditional on any	(10) Entitled to home-based work
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>				
First child aged 0-2=1	-0.0837 (0.266)	0.0476* (0.0198)	-3.204*** (0.944)	0.0360+ (0.0200)
First child aged 3-5=1	0.164 (0.364)	0.0549** (0.0207)	-2.258* (1.020)	0.0420+ (0.0232)
First child aged 6-8=1	0.285 (0.377)	0.0511* (0.0220)	-2.077+ (1.196)	-0.00630 (0.0243)
First child aged 9-10=1	0.566 (0.445)	0.0462+ (0.0239)	-1.400 (1.179)	-0.0185 (0.0266)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>				
First child aged 0-2=1 × First birth after 2010=1	0.492 (0.329)	-0.0285 (0.0244)	2.413* (1.138)	-0.0283 (0.0247)
First child aged 3-5=1 × First birth after 2010=1	0.771+ (0.449)	0.0103 (0.0270)	1.797 (1.301)	0.000375 (0.0285)
First child aged 6-8=1 × First birth after 2010=1	1.106+ (0.582)	0.0564+ (0.0319)	1.651 (1.710)	0.0178 (0.0335)
First child aged 9-10=1 × First birth after 2010=1	1.388 (1.163)	0.0927 (0.0638)	2.098 (2.013)	0.0829 (0.0536)
Pre-birth mean:				
Early cohorts	1.462	0.176	8.503	0.230
Late cohorts	1.506	0.183	8.382	0.270
Observations:				
N	83118	83118	16667	56410
N Individuals	10251	10251	2991	7973
N New Parents	1395	1395	593	1273

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Tables A.4 and A.5, but considers as outcomes various work arrangements: measures of regularity (columns 1 to 4), of flexibility (columns 5 and 6), and work from home (columns 7 to 10). All variables are defined in Appendix B.

Table A.7: Child penalties for women: index measures of (in)flexibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Time pressure	Importance of relationships	Unstructured work	Freedom to make decisions	Inflexible job	Freedom in decision making	Time pressure, complex and stressful job
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>							
First child aged 0-2=1	-0.122*** (0.0357)	-0.249*** (0.0451)	-0.0784* (0.0397)	-0.105** (0.0393)	-0.146*** (0.0286)	-0.242** (0.0904)	-0.658*** (0.0699)
First child aged 3-5=1	-0.120** (0.0422)	-0.309*** (0.0482)	-0.0730+ (0.0434)	-0.121** (0.0392)	-0.155*** (0.0290)	-0.310** (0.0961)	-0.654*** (0.0788)
First child aged 6-8=1	-0.0859+ (0.0444)	-0.305*** (0.0506)	-0.135** (0.0473)	-0.121** (0.0418)	-0.182*** (0.0292)	-0.377*** (0.105)	-0.564*** (0.0863)
First child aged 9-10=1	-0.126** (0.0462)	-0.284*** (0.0576)	-0.118* (0.0502)	-0.161*** (0.0476)	-0.171*** (0.0331)	-0.366** (0.116)	-0.560*** (0.0994)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>							
First child aged 0-2=1 × First birth after 2010=1	0.0282 (0.0409)	0.0991+ (0.0556)	0.0746 (0.0476)	0.0292 (0.0457)	0.0457 (0.0337)	0.134 (0.108)	0.114 (0.0886)
First child aged 3-5=1 × First birth after 2010=1	-0.0233 (0.0482)	0.0899 (0.0621)	0.0769 (0.0534)	0.0568 (0.0476)	0.0289 (0.0358)	0.204+ (0.121)	-0.00463 (0.101)
First child aged 6-8=1 × First birth after 2010=1	-0.0300 (0.0542)	0.108 (0.0723)	0.129* (0.0619)	0.0608 (0.0547)	0.0557 (0.0401)	0.358* (0.144)	0.0193 (0.120)
First child aged 9-10=1 × First birth after 2010=1	0.00518 (0.0829)	0.0787 (0.130)	0.203+ (0.108)	0.146 (0.0931)	0.102 (0.0740)	0.409+ (0.237)	-0.188 (0.219)
Pre-birth mean:							
Early cohorts	0.179	0.0737	-0.0330	-0.0489	0.0271	-0.404	-0.0194
Late cohorts	0.184	-0.0310	-0.131	-0.00959	0.00146	-0.596	0.0523
Observations:							
N	69604	78142	69608	69598	79406	67843	67843
N Individuals	8914	9504	8916	8915	9603	8777	8777
N New Parents	1337	1387	1337	1337	1397	1323	1323

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10.

Table A.8: Child penalties for women: change in flexibility from changing job

	(1) Regular schedule (job average)	(2) Flexible working times (job average)	(3) Entitlement to flex entry/exit times (job average)	(4) Inflexible job (job average)	(5) Freedom in decision making (job average)	(6) Time pressure (job average)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-2=1	-0.0346*** (0.00864)	-0.00874 (0.0140)	-0.00814 (0.00663)	-0.0455*** (0.00894)	-0.165*** (0.0367)	-0.122*** (0.0238)
First child aged 3-5=1	-0.0534*** (0.00943)	-0.00354 (0.0168)	-0.0129 (0.00820)	-0.0541*** (0.0101)	-0.225*** (0.0422)	-0.176*** (0.0263)
First child aged 6-8=1	-0.0581*** (0.0105)	-0.0332+ (0.0189)	-0.0258** (0.00935)	-0.0726*** (0.0121)	-0.301*** (0.0479)	-0.195*** (0.0302)
First child aged 9-10=1	-0.0591*** (0.0114)	-0.0388+ (0.0205)	-0.0214* (0.0104)	-0.0748*** (0.0126)	-0.318*** (0.0523)	-0.206*** (0.0333)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-2=1 × First birth after 2010=1	0.0108 (0.0102)	0.00760 (0.0172)	0.000783 (0.00802)	0.0237* (0.0111)	0.0570 (0.0448)	0.0547+ (0.0285)
First child aged 3-5=1 × First birth after 2010=1	0.0185 (0.0117)	0.0140 (0.0215)	0.00818 (0.0101)	0.0226+ (0.0129)	0.0925+ (0.0531)	0.0596+ (0.0340)
First child aged 6-8=1 × First birth after 2010=1	0.0114 (0.0147)	0.0409 (0.0256)	0.0131 (0.0126)	0.0446** (0.0156)	0.187** (0.0647)	0.0562 (0.0425)
First child aged 9-10=1 × First birth after 2010=1	0.0124 (0.0240)	0.000752 (0.0509)	0.000300 (0.0247)	0.0476+ (0.0274)	0.226* (0.0969)	0.0779 (0.0793)
Pre-birth mean:						
Early cohorts	0.448	-0.00871	0.581	0.0138	-0.245	-0.0814
Late cohorts	0.453	-0.000979	0.585	0.0116	-0.219	-0.0755
Observations:						
N	75995	75459	75723	75995	75894	75894
N Individuals	9435	9403	9418	9435	9427	9427
N New Parents	1377	1376	1377	1377	1377	1377

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table reports the coefficient estimates from a version of equation (3), in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10.

Table A.9: Child penalties for men: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-5=1	-0.0172* (0.00844)	-1.654*** (0.393)	-0.0549*** (0.0128)	1.145+ (0.587)	0.0400** (0.0139)	-0.123 (0.120)
First child aged 6-10=1	-0.0173 (0.0110)	-2.016*** (0.527)	-0.0698*** (0.0169)	2.412* (0.970)	0.0339+ (0.0195)	-0.160 (0.161)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-5=1 × First birth after 2010=1	-0.00252 (0.0102)	0.141 (0.497)	-0.000725 (0.0160)	0.379 (0.784)	-0.0151 (0.0177)	-0.0389 (0.151)
First child aged 6-10=1 × First birth after 2010=1	-0.0130 (0.0152)	-0.505 (0.762)	-0.0295 (0.0238)	-0.758 (1.405)	-0.0227 (0.0267)	-0.122 (0.232)
Pre-birth mean:						
Early cohorts	0.952	44.35	3.737	20.52	2.963	9.220
Late cohorts	0.952	43.55	3.720	27.85	3.277	9.627
Observations:						
N	107225	84018	84018	84018	74203	116055
N Individuals	11732	9802	9802	9802	9279	12748
N New Parents	1338	1302	1302	1302	1281	1338

Standard errors in parentheses; ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 4, but applies to the subsample of men

Table A.10: Child penalties for women: other labor market outcomes

	(1) Part-time if employed	(2) Overtime (more than 38 hours/week)	(3) Overtime (more than 50 hours/week)	(4) Permanent employment if employed (vs casual and fixed-term)	(5) Normally supervise work of other employees	(6) Promoted in the past year (conditional on current employment)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-5=1	0.614*** (0.0205)	-0.398*** (0.0160)	-0.105*** (0.00968)	-0.229*** (0.0212)	-0.138*** (0.0209)	-0.0923*** (0.0141)
First child aged 6-10=1	0.622*** (0.0262)	-0.391*** (0.0191)	-0.106*** (0.0106)	-0.254*** (0.0259)	-0.145*** (0.0246)	-0.0758*** (0.0160)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-5=1 × First birth after 2010=1	-0.0398 (0.0250)	0.0504* (0.0197)	0.0234* (0.0113)	0.133*** (0.0256)	0.0499+ (0.0260)	0.00391 (0.0168)
First child aged 6-10=1 × First birth after 2010=1	-0.0486 (0.0361)	0.0534* (0.0266)	0.0321* (0.0146)	0.0895* (0.0355)	0.0516 (0.0351)	0.000212 (0.0235)
Pre-birth mean:						
Early cohorts	0.221	0.440	0.110	0.716	0.498	0.159
Late cohorts	0.226	0.399	0.0808	0.701	0.446	0.157
Observations:						
N	78031	122094	122094	69088	78142	67308
N Individuals	9501	12879	12879	8846	9504	8719
N New Parents	1386	1539	1539	1342	1387	1340

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 4, but applies to different labor market outcomes. All variables are defined in Appendix B.

Table A.11: Child penalties for women: work arrangements (continues in next page)

	Regularity				Flexibility	
	(1) Regular schedule (M-F and regular daytime schedule)	(2) M-F	(3) Regular daytime schedule	(4) On call and irregular shifts	(5) My working times can be flexible (yes-no)	(6) Entitled to flexible start/finish times
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-5=1	-0.442*** (0.0213)	-0.439*** (0.0218)	-0.128*** (0.0177)	0.0872*** (0.0138)	0.156*** (0.0275)	0.0341+ (0.0205)
First child aged 6-10=1	-0.451*** (0.0258)	-0.438*** (0.0268)	-0.126*** (0.0200)	0.0774*** (0.0140)	0.154*** (0.0319)	0.0281 (0.0248)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-5=1 × First birth after 2010=1	0.0848** (0.0265)	0.0847** (0.0267)	0.0796*** (0.0204)	-0.0470** (0.0159)	-0.0347 (0.0312)	0.0145 (0.0245)
First child aged 6-10=1 × First birth after 2010=1	0.115** (0.0381)	0.104** (0.0387)	0.0860** (0.0267)	-0.0370+ (0.0194)	-0.00775 (0.0404)	0.0460 (0.0321)
Pre-birth mean:						
Early cohorts	0.597	0.625	0.803	0.0659	0.411	0.563
Late cohorts	0.599	0.627	0.802	0.0567	0.433	0.590
Observations:						
N	78115	78121	78140	78140	57256	60698
N Individuals	9503	9504	9504	9504	7979	8220
N New Parents	1387	1387	1387	1387	1290	1289

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.11: Child penalties for women: work arrangements (continues from previous page)

	Work from home			
	(7) Hours work from home	(8) Any hours worked from home	(9) Hours work from home conditional on any	(10) Entitled to home-based work
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>				
First child aged 0-5=1	0.0284 (0.263)	0.0503** (0.0178)	-2.773** (0.843)	0.0380* (0.0191)
First child aged 6-10=1	0.358 (0.373)	0.0468* (0.0207)	-1.987+ (1.101)	-0.0135 (0.0231)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>				
First child aged 0-5=1 × First birth after 2010=1	0.579+ (0.323)	-0.0131 (0.0224)	2.053+ (1.049)	-0.0167 (0.0233)
First child aged 6-10=1 × First birth after 2010=1	0.982+ (0.587)	0.0563+ (0.0310)	1.457 (1.581)	0.0234 (0.0320)
Pre-birth mean:				
Early cohorts	1.462	0.176	8.503	0.230
Late cohorts	1.506	0.183	8.382	0.270
Observations:				
N	83118	83118	16667	56410
N Individuals	10251	10251	2991	7973
N New Parents	1395	1395	593	1273

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table A.6, but the child's age is split in coarser groups (two as opposed to four): first child aged 0-5 and 6-10.

Table A.12: Child penalties for women: index measures of (in)flexibility

	(1) Time pressure	(2) Importance of relationships	(3) Unstructured work	(4) Freedom to make decisions	(5) Inflexible job	(6) Freedom in decision making	(7) Time pressure, complex and stressful job
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>							
First child aged 0-5=1	-0.120*** (0.0354)	-0.276*** (0.0420)	-0.0757* (0.0370)	-0.113** (0.0347)	-0.150*** (0.0253)	-0.276*** (0.0824)	-0.654*** (0.0671)
First child aged 6-10=1	-0.0994* (0.0422)	-0.290*** (0.0493)	-0.129** (0.0449)	-0.137*** (0.0415)	-0.176*** (0.0284)	-0.369*** (0.101)	-0.557*** (0.0834)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>							
First child aged 0-5=1 × First birth after 2010=1	0.00752 (0.0399)	0.100 ⁺ (0.0522)	0.0747 ⁺ (0.0442)	0.0432 (0.0406)	0.0395 (0.0301)	0.170 ⁺ (0.100)	0.0631 (0.0840)
First child aged 6-10=1 × First birth after 2010=1	-0.00770 (0.0516)	0.104 (0.0704)	0.128* (0.0592)	0.0797 (0.0536)	0.0575 (0.0392)	0.355* (0.138)	0.0137 (0.115)
Pre-birth mean:							
Early cohorts	0.179	0.0737	-0.0330	-0.0489	0.0271	-0.404	-0.0194
Late cohorts	0.184	-0.0310	-0.131	-0.00959	0.00146	-0.596	0.0523
Observations:							
N	69604	78142	69608	69598	79406	67843	67843
N Individuals	8914	9504	8916	8915	9603	8777	8777
N New Parents	1337	1387	1337	1337	1397	1323	1323

Standard errors in parentheses; ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.13: Child penalties for women: change in flexibility from changing job

	(1) Regular schedule (job average)	(2) Flexible working times (job average)	(3) Entitlement to flex entry/exit times (job average)	(4) Inflexible job (job average)	(5) Freedom in decision making (job average)	(6) Time pressure (job average)
<i>Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)</i>						
First child aged 0-5=1	-0.0434*** (0.00798)	-0.00650 (0.0135)	-0.0104 (0.00664)	-0.0495*** (0.00840)	-0.193*** (0.0349)	-0.147*** (0.0222)
First child aged 6-10=1	-0.0568*** (0.00987)	-0.0362* (0.0180)	-0.0239** (0.00903)	-0.0724*** (0.0114)	-0.302*** (0.0459)	-0.193*** (0.0294)
<i>Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)</i>						
First child aged 0-5=1 × First birth after 2010=1	0.0155 (0.00947)	0.00991 (0.0167)	0.00409 (0.00790)	0.0240* (0.0104)	0.0769+ (0.0428)	0.0614* (0.0268)
First child aged 6-10=1 × First birth after 2010=1	0.0123 (0.0141)	0.0388 (0.0246)	0.0102 (0.0121)	0.0463** (0.0150)	0.196** (0.0624)	0.0649 (0.0417)
Pre-birth mean:						
Early cohorts	0.448	-0.00871	0.581	0.0138	-0.245	-0.0814
Late cohorts	0.453	-0.000979	0.585	0.0116	-0.219	-0.0755
Observations:						
N	75995	75459	75723	75995	75894	75894
N Individuals	9435	9403	9418	9435	9427	9427
N New Parents	1377	1376	1377	1377	1377	1377

Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.14: Permutation test

	P-value asymptotic	P-value random
In labor force	.290	.36
Weekly hrs in paid emp (cond)	.004	.18
Log weekly h in paid emp (cond)	.036	.18
Log(1+weekly wage)	.026	.18
Log(1+annual labor income)	.002	.18
Log (hourly wage)	.546	.73

Table A.15: Time evolution of child penalty by job regularity (short and long run, 1/2)

	(1) Weekly hours in paid employment (conditional)	(2) Weekly hours in paid employment (conditional)	(3) Regular schedule (M-F and regular daytime schedule)	(4) Regular schedule (M-F and regular daytime schedule)
Child penalty 0-5	-17.97*** (0.676)	-15.79*** (1.157)	-0.449*** (0.0242)	-0.525*** (0.0465)
* tercile = 2		-4.898** (1.679)		-0.0973 (0.0606)
* tercile = 1		-1.456 (1.584)		0.298*** (0.0567)
Child penalty 0-5 * post-2010 first birth	3.172*** (0.778)	1.254 (1.328)	0.0755* (0.0294)	0.0459 (0.0533)
* tercile = 2		5.381** (1.914)		0.153* (0.0718)
* tercile = 1		0.240 (1.839)		-0.0289 (0.0672)
Child penalty 6-10	-17.70*** (0.804)	-16.23*** (1.524)	-0.462*** (0.0300)	-0.540*** (0.0581)
* tercile = 2		-2.583 (2.036)		-0.0929 (0.0766)
* tercile = 1		-1.518 (2.020)		0.287*** (0.0716)
Child penalty 6-10 * post-2010 first birth	3.333** (1.032)	2.409 (1.794)	0.130** (0.0437)	0.0969 (0.0749)
* tercile = 2		3.274 (2.482)		0.189+ (0.104)
* tercile = 1		-0.0212 (2.623)		-0.0289 (0.104)
Early (pre-2010) cohorts: Mean Y pre-birth	38.45		0.591	
- tercile 3		41.52		0.822
- tercile 2		40.22		0.768
- tercile 1		34.90		0.287
Late (post-2010) cohorts: Mean Y pre-birth	37.36		0.604	
- tercile 3		39.84		0.812
- tercile 2		38.01		0.732
- tercile 1		34.53		0.296
Early cohorts: New parents	369		369	
- tercile 3		102		102
- tercile 2		117		117
- tercile 1		150		150
Late cohorts: New parents	676		676	
- tercile 3		200		200
- tercile 2		227		227
- tercile 1		249		249
Early cohorts: Observations	3886		3891	
- tercile 3		1134		1136
- tercile 2		1276		1277
- tercile 1		1476		1478
Late cohorts: Observations	6019		6031	
- tercile 3		1940		1942
- tercile 2		2020		2022
- tercile 1		2059		2067
Tot observations	47627	47627	47715	47715

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.16: Time evolution of child penalty by job regularity (short and long run, 2/2)

	(1) Part-time if employed	(2) Part-time if employed	(3) Promoted in the past year (conditional on current employment)	(4) Promoted in the past year (conditional on current employment)
Child penalty 0-5	0.607*** (0.0239)	0.581*** (0.0435)	-0.0951*** (0.0169)	-0.136*** (0.0403)
* tercile = 2		0.139* (0.0577)		0.0222 (0.0481)
* tercile = 1		-0.0596 (0.0594)		0.0809+ (0.0461)
Child penalty 0-5 * post-2010 first birth	-0.0226 (0.0282)	0.0338 (0.0493)	0.0135 (0.0196)	0.0297 (0.0451)
* tercile = 2		-0.181** (0.0668)		0.0390 (0.0544)
* tercile = 1		0.00954 (0.0697)		-0.0651 (0.0528)
Child penalty 6-10	0.631*** (0.0303)	0.632*** (0.0541)	-0.0799*** (0.0179)	-0.114** (0.0415)
* tercile = 2		0.0898 (0.0744)		0.0528 (0.0510)
* tercile = 1		-0.0835 (0.0746)		0.0314 (0.0482)
Child penalty 6-10 * post-2010 first birth	-0.0666 (0.0409)	-0.0519 (0.0678)	0.0286 (0.0265)	0.0852+ (0.0514)
* tercile = 2		-0.133 (0.0968)		-0.0428 (0.0722)
* tercile = 1		0.0552 (0.101)		-0.115+ (0.0642)
Early (pre-2010) cohorts: Mean Y pre-birth	0.218		0.162	
- tercile 3		0.0752		0.238
- tercile 2		0.143		0.159
- tercile 1		0.378		0.116
Late (post-2010) cohorts: Mean Y pre-birth	0.220		0.158	
- tercile 3		0.0969		0.211
- tercile 2		0.189		0.130
- tercile 1		0.358		0.139
Early cohorts: New parents	369		357	
- tercile 3		102		100
- tercile 2		117		115
- tercile 1		150		142
Late cohorts: New parents	676		660	
- tercile 3		200		195
- tercile 2		227		222
- tercile 1		249		243
Early cohorts: Observations	3889		3321	
- tercile 3		1135		951
- tercile 2		1277		1116
- tercile 1		1477		1254
Late cohorts: Observations	6026		5414	
- tercile 3		1941		1766
- tercile 2		2022		1844
- tercile 1		2063		1804
Tot observations	47676	47676	41871	41871

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 5, but it includes also estimates for the long-run (6 to 10 years after childbirth). The coefficients for the short run are exactly the same as in Table 5 because the estimated equation also includes the long run coefficients, which are however not displayed in that table.

Table A.17: The interaction between Paid Parental Leave and regularity (other variables)

	(1) Weekly hours in paid employment (condi- tional)	(2) Weekly hours in paid employment (condi- tional)	(3) Weekly hours in paid employment (condi- tional)	(4) Weekly hours in paid employment (condi- tional)	(5) Log(1+weekly wage)	(6) Log(1+weekly wage)	(7) Log(1+weekly wage)	(8) Log(1+weekly wage)
First child aged 0-5	-18.93*** (0.845)	-17.82*** (0.980)	-19.57*** (1.065)	-18.31*** (1.204)	-3.042*** (0.161)	-2.872*** (0.195)	-2.986*** (0.192)	-2.761*** (0.241)
× Middle Tercile Regularity			-3.412 ⁺ (1.848)		-3.298 ⁺ (1.878)		-0.493 (0.345)	-0.559 (0.354)
× not PPL eligible				1.423 (1.732)	0.924 (1.744)			-0.0992 (0.324)
First child aged 0-5 × First birth after 2010	3.849*** (0.969)	2.508 [*] (1.127)	5.078*** (1.197)	3.629** (1.353)	0.814*** (0.193)	0.597 [*] (0.235)	0.593 [*] (0.234)	0.303 (0.290)
× Middle Tercile Regularity			4.063 ⁺ (2.114)		3.806 ⁺ (2.143)		0.653 (0.406)	0.748 ⁺ (0.415)
× not PPL eligible				-3.052 (2.024)	-2.572 (2.027)			0.438 (0.388)
Observations	30379	30379	30379	30379	35594	35594	35594	35594
N Individuals	2459	2459	2459	2459	2462	2462	2462	2462
N New Parents	670	670	670	670	671	671	671	671

Standard errors in parentheses

⁺ $p < 0.10$, ^{*} $p < 0.05$, ^{**} $p < 0.01$, ^{***} $p < 0.001$

Note: This table is analogous to Table 6, but estimates pertain to different labor market outcomes.

B Variables construction

Demographics and labor market outcomes “Age” is the age of the respondent. “In labor force” is a dummy equal to one if the individual is either employed or unemployed but looking for work, and equal to zero if the individual is not in the labor force; it can be missing if the individual is not administered this part of questionnaire - see Summerfield et al. (2019) for further details. “Weekly hours in paid employment” is conditional on hours being strictly positive. “Log weekly hours in paid employment” is the natural logarithm of the variable above. “ $\text{Log}(1 + \text{weekly wage})$ ” is the log of 1 plus weekly wages, in contemporaneous Australian dollars, and is winsorized at 0.5% level, by year and gender. “ $\text{Log}(1 + \text{annual labor income})$ ” is the log of one plus yearly earnings, similarly winsorized.

The way hourly wages are measured and computed in Hilda might not be reliable in periods of large job changes (such as after childbirth for example) because of the way the questions are worded. In fact, hourly wages are computed as the ratio of weekly wages to the number of hours spent in paid employment every week. However, weekly wages are computed from a question that asks “What was the total amount of your most recent pay” (suggesting to take out a pay slip to answer accurately) while weekly hours come from a question that asks “Including any paid or unpaid overtime, how many hours per week do you usually work in all your jobs?”. Moreover, they are asked in two different parts of the survey (questions about income come much after other questions on current employment). Therefore, while for a person that has not experienced large job changes we can expect the answers to the two questions to be consistent, we cannot have the same expectation for people who are experiencing large job changes, such as new mothers. For example, we might expect a new mother to answer the wage question based on the pay slip she received when she last worked while the hours questions based on the hours she works now. Or, alternatively, she might be counting the amount she receives for paid leave as wages. In any case, at least for the first year of life of the child hourly wage is not a reliable measure. Therefore we follow the suggestion from Summerfield et al. (2019) and construct our variable “Hourly wage” using a measure of weekly wages that combines current weekly gross wages salary in all jobs (winsorised as described above). “Part-time if employed” is a dummy variable equal to one if the individual is employed and working part-time, and equal to zero if the individual is working full-time (it is missing for unemployed individuals). “Permanent employment if employed” is a dummy equal to one if the respondent has a permanent contract, and equal to 0 if they have casual or fixed-term contracts (it is missing for unemployed individuals). The two “Overtime” variables are dummies equal to one if the respondent works more than 38 or 50 hours a week (respectively), and zero otherwise (it is missing for unemployed individuals). “Promoted in the past year” is a dummy equal to one if the individual is currently employed and has been promoted in the previous year, and zero otherwise (it is missing for unemployed individuals). “Normally supervise work of other employees” is a dummy variable equal to one if the respondent has supervisory responsibilities (it is missing for unemployed individuals). “Occupational status scale” is the Australian Socioeconomic Index 2006 from McMillan et al. (2009), ranging from 1 to 100. “Regular schedule (M-F and regular daytime schedule) is a dummy equal to one if the individual has both a regular Monday-Friday weekly schedule and a regular daytime daily schedule. “M-F” and “Regular daytime schedule” are the two corresponding dummies. “On call and irregular shifts” is a dummy equal to 1 if the individual’s current work schedule is either call-based or irregular. “My working times can be flexible” is built based on a categorical variable denoting how strongly respondents agree, on a scale from 1 to 7, with the statement “My working times can be flexible”. We discretize it by indicating as agreement any answer above 4 and disagreement otherwise. “Entitled to flexible start/finish times” is a dummy variable equal to 1 if the respondent answers yes to the question “Would you be entitled to flexible start/finish times if you were to need it?”. “Has bachelor degree” is a dummy variable equal to one if the respondent holds a bachelor degree, a graduate diploma or a postgraduate one.

Workplace flexibility The first measure of workplace flexibility we adopt is a dummy indicating agreement with the statement "My working times can be flexible". This is built based on a categorical variable denoting how strongly respondents agree, on a scale from 1 to 7, with the statement "My working times can be flexible". We discretize it by indicating as agreement any answer above 4 and disagreement otherwise. Results are unchanged if we use the categories separately.

The second measure of workplace flexibility we adopt is the answer to the question "Would you be entitled to flexible start/finish times if you were to need it?".

Schedule Regularity There are mainly two questions that refer to regularity. The first one, which we label "Regular weekly schedule" asks "On which days of the week do you usually work in your main job?", and possible answers are "Monday to Friday", "Nine day fortnight", that their days vary from week to week, and that they vary from month to month.

The second question is "Regular daily schedule", and respondents are asked "Which of these best describes your current work schedule in your main job?". Possible answers are listed below.

1. A regular daytime schedule
2. A regular evening shift
3. A regular night shift
4. A rotating shift (changes from days to evenings to nights)
5. Split shift (two distinct periods each day)
6. On call
7. Irregular schedule
8. Other

Our measure, which is called "Regular schedule" is a dummy variable equal to 1 if the answer to the first question is "Monday to Friday" and the answer to the second question is "A regular daytime schedule", and equal to 0 for any other combination of the two answers.

Work from home The variable "Hours worked from home" is based on the question "Approximately how many hours each week do you usually work at home?". This is missing for unemployed respondents. The variable "Any hours worked from home" is a dummy variable equal to one if the variable defined above is strictly positive. The variable "Hours worked from home conditional on any" is equal to "Hours worked from home" for those who work from home a strictly positive number of hours, and missing otherwise. The variable "Entitled to home-based work" is a dummy variable equal to one if the respondent answers yes to the question "Would you be entitled to home-based work if you were to need it?"

Time use For most of our time use variables we rely on direct questions of the form "How much time would you spend on each of the following activities in a typical week?" This is true for the following measures:

- Playing with your children
- Housework
- Paid employment

- Other (which aggregates commuting, volunteering, looking after other people's children, caring for disabled spouse)

The variable "Social activity" is constructed from the question "In general, about how often do you get together socially with friends or relatives not living with you?" with the following possible answers:

- Every day
- Several times a week
- About once a week
- 2 or 3 times a month
- About once a month
- Once or twice every three months
- Less often than once every three months

We assume that each "get together" occasion lasts for about two hours and therefore we recode the above as follows:

- Every day – 14 hours per week
- Several times a week – 6 hours per week
- About once a week – 2 hours per week
- 2 or 3 times a month – 1.25 hours per week
- About once a month – 0.5 hours per week
- Once or twice every three months – 0.15 hours per week
- Less often than once every three months – 0 hours per week

For "Physical activity" we proceed similarly. The questionnaire asks "In general, how often do you participate in moderate or intensive physical activity for at least 30 minutes?", with the following possible answers:

- Not at all
- Less than once a week
- 1 to 2 times a week
- 3 times a week
- More than 3 times a week
- Every day

We assume that each sports session lasts one hour and we recode the variable as follow:

- Not at all – 0 hours per week
- Less than once a week – 0.8 hours per week

- 1 to 2 times a week – 1.5 hours per week
- 3 times a week – 3 hours per week
- More than 3 times a week – 4.5 hours per week
- Every day – 7 hours per week