

What Works for Working Couples? Work Arrangements, Maternal Labor Supply, and the Division of Home Production*

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November 12, 2024

Abstract

We provide the first causal evidence that changes to work arrangements – in the form of greater schedule regularity – can reduce the child penalty in earnings for women. The Australian 2009 Fair Work Act explicitly entitled parents of young children to request a 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 three main results. First, new mothers used the Fair Work Act to maintain a regular schedule while reducing hours upon childbirth. Second, thanks to increased regularity, working mothers' child penalty declined from a 47 to a 38 percent drop in hours worked. Third, while this increase in maternal labor supply implies a significant shift towards equality in the female- and male-shares of household income, we do not observe any changes in the female share of home production.

Keywords: Child Penalty, Work Arrangements, Division of Home Production

JEL: J16, J22, J18, J81

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1 Introduction

Women earn less than men. In 2022, across OECD countries, women spent 34% fewer hours in the labor market and were paid 11% less than men for every hour worked.¹ This disparity can be mostly attributed to parenthood: while men and women exhibit similar work and earnings trajectories upon entering the labor market, a notable divergence occurs when they become parents. This divergence results in persistent and widening participation and earnings gaps – the so-called child penalty (Kleven, Landais, and Leite-Mariante, Forthcoming). Why parenthood is associated with a labor market penalty for mothers remains largely unexplained.²

Academics and policymakers agree that control over one's time at work is crucial for reducing the gender participation and earnings gap. Yet, causal evidence remains scant. Goldin's seminal work indicates that the gender pay gap has narrowed most in occupations where workers have gained more control over their schedules, highlighting the importance of the structure of work and work arrangements in addressing the gap (Goldin, 2014; Goldin and Katz, 2016). Lawmakers in Europe and in the US advocate for giving workers more control over their schedules on the grounds that it helps balance work and family, thus helping mothers' careers: several pieces of legislation have been passed on both sides of the Atlantic under this premise.³ However, we lack causal evidence that giving workers more control over their schedule reduces the gender earnings and participation gap.

We are the first to demonstrate a causal link between work arrangements and the child penalty in labor supply and earnings. In this paper, we show that, when given the possibility of keeping a regular schedule upon reducing work hours after childbirth, new mothers increase their labor supply by 54%; this translates into a 17% smaller child penalty and a 19% lower earnings gap relative to fathers. We show that changing the structure of work such that workers have more control over their time – and, in particular, giving them a regular and predictable schedule – does make new mothers more likely to return to work and work longer hours.

Importantly, the discussion around whether work arrangements can help *mothers* bal-

¹Authors' calculation based on OECD data (OECD, 2022a,b,c).

²A substantial and growing body of literature has examined potential determinants of the child penalty—such as biological differences, comparative advantage, and paid parental leave—yielding largely inconclusive results. Gender norms seem to be an important driver, yet they do not explain it fully. For a comprehensive review of existing findings, please refer to the Related Literature paragraph below.

³The US Congress is debating the “Schedules That Work Act” (House bill H.R.6670) 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.” (Congresswoman DeLauro press release, 6/20/17). At the same time, several US states and localities have been introducing predictive scheduling laws (<https://www.hrdive.com/news/a-running-list-of-states-and-localities-with-predictive-scheduling-mandates/540835/>). In Europe, as of August 2022 all European Union member states must apply the “Directive on work-life balance for parents and carers” (Directive (EU) 2019/1158) 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” (European Commission press release, 8/2/22).

ance a family and a career takes as given that the time demands of child-rearing disproportionately fall upon women (Ferrant, Pesando, and Nowacka, 2014). In the second part of the paper, we show that this gendered allocation of tasks within the household does not change despite a substantial increase in the female share of household income.

The 2009 Fair Work Act and the richness of the available data make Australia the perfect setting to provide causal answers to our research questions. The Australian 2009 Fair Work entitled parents to request a change in work arrangements, thus generating quasi-exogenous variation in the work arrangements available to mothers – most notably, schedule regularity – that we will exploit in our analysis. The *Household, Income, and Labour Dynamics in Australia* (HILDA) dataset provides very rich longitudinal data on work arrangements, time use, and family linkages for over 20 years: this allows us to directly observe the work environment and time allocation of both partners in the couple, and how they change upon the arrival of a child.

Parenthood in Australia is characterized by a traditional allocation of tasks, similarly to most other countries, but work structure heightens the tradeoffs mothers face. At baseline, upon becoming parents, women substantially reduce their hours of work (-28 hours per week) in order to increase home production (housework and childcare) by a staggering 53 hours per week; fathers do not change their time use, except for spending about two hours per day with the newborn. Before the Fair Work Act, reducing hours below full-time often entailed switching from a permanent to a casual contract, characterized by an irregular schedule: indeed, among women who gave birth before the law and continued working after childbirth, we observe a “child penalty” in the probability of being on a permanent contract and having a regular schedule by 25 and 43 percentage points respectively. In what follows, we argue that this schedule irregularity amplified substantially the child penalty in labor supply upon motherhood.

We leverage the Australian 2009 Fair Work Act as a natural experiment to make a causal statement on the impact of work arrangements on maternal labor supply. This law changed a very specific work arrangement: it strengthened the ability of mothers to reduce hours of work within their permanent contracts, which are associated with a regular schedule, instead of moving to a casual contract and an irregular schedule. By expanding access to less-than-full-time jobs with a regular schedule (“permanent reduced hours”), the passage of the Fair Work Act allows us to study the impact of this work arrangement on maternal labor supply. We document that these institutional changes are indeed reflected in our data, and in particular that the probability of new mothers having a permanent job with reduced hours more than doubled after the Fair Work Act.

Exploiting the variation introduced by the Fair Work Act, we show that having access to a job with a permanent contract (thus a regular and predictable schedule) and reduced hours dramatically lowers the child penalty in labor supply for treated mothers. We further show that, while the treated mothers work more and do fewer hours of housework, their partners’ time use is unaffected.

We obtain these results through two complementary empirical strategies. First, we leverage variation in the timing of childbirth to show the aggregate changes the Fair Work Act caused. Second, we exploit additional variation from differential occupational exposure to the law to argue that the changes in mothers’ work arrangements do directly cause the observed changes in the child penalty, and to zoom into the couple-level analysis.

In the first empirical strategy, we compare the child penalty of women who became mothers in the four years preceding the Fair Work Act with that of those who became mothers in the four years after the Fair Work Act. Under the assumption – for which we provide supporting evidence – that the late cohorts of mothers would have behaved like the early ones absent the law, this comparison recovers the causal estimate of the impact of the Fair Work Act on the child penalty. Using this strategy, we document a large increase in the post-childbirth probability of maintaining a permanent contract, as opposed to transitioning to a casual contract, and a contemporaneous increase in maternal labor supply (decrease in the child penalty), but no change in time spent directly with the child.

In the second empirical strategy, we leverage additional variation from women's pre-birth occupations to directly link the changes in work arrangements and in the child penalty. The intuition is the following. The Fair Work Act decreased the probability of transitioning from a permanent to a casual contract. This did not have bite for jobs that only offered permanent contracts even before the law (e.g. public administration). At the other end of the spectrum, jobs that only offered casual contracts (e.g. hospitality) presumably did so for "technological" reasons and thus were not affected by the law either (the law allowed employers to refuse requests "on reasonable business grounds"). The set of jobs most exposed to the law is the middle group, where there was both scope for change and limited technological constraints against it.

Thus, this strategy is based on the idea that different jobs were differentially exposed to the Fair Work Act, that the pre-2009 share of casual contracts in a job ("casual prevalence") can efficiently summarize such exposure, and that mothers were differentially exposed to the Fair Work Act depending on the exposure of the job they held before childbirth. In practice, we define treated mothers as those with pre-birth jobs with intermediate levels of casual prevalence: low enough for the law to have room to change the pervasiveness of permanent contracts, but high enough for this arrangement to be feasible.

Using this second empirical strategy, we confirm that the aggregate results found using the first strategy are entirely driven by mothers in the group most exposed to the law, those with a pre-birth job in the middle tercile of casual prevalence. For women in the bottom tercile of casual prevalence – namely those in pre-birth jobs with a very high fraction of permanent contracts – we don't observe significant changes in the child penalty in contract type and time use between pre- and post-Fair Work Act cohorts of mothers. Instead, for those in the middle tercile, the probability of transitioning from a permanent to a casual contract upon childbirth goes from 37 percentage points to zero. For these most exposed mothers, this is associated with an increase in labor supply (paid work and commute) by 11 hours per week and a decrease in housework by 5 hours per week, with no changes in the time spent directly with the child.

Once we have established the causal link between access to regular-schedule, less-than-full-time jobs and mothers' time allocation, we ask how this reflects in the intra-household allocation of tasks. Given that treated mothers spend more hours working in the labor market, their contribution to household income increases dramatically, yet we find no significant changes in the female share of home production.⁴ In other words, we find that a

⁴Housework represents a small share of maternal home production in the presence of young children in the household, thus the documented decrease in housework for treated mothers does not translate into a

sizable increase in the female share of income leads to no decrease in the disproportionate share of home production women shoulder. This suggests that relative income is not the only determinant of how tasks are split within the couple, and that something else, possibly in gender norms, plays an important role.

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

The literature on the child penalty, recently reviewed by Cortés and Pan (2023), has explored several potential determinants, such as parental leave policies, childcare, biology, and comparative advantage, finding mostly null results.⁵ We contribute to this literature by focusing on the role played by the work arrangements available to mothers upon returning to work, and, more specifically, the ability to work less than full-time while maintaining a regular schedule. Differently from many public policies analyzed in this literature, the one we study, aimed at changing the structure of work in a way that increases individual control over time use, does yield an impact on the child penalty.⁶

Work arrangements have been shown to be a key determinant of gender earnings gaps (Bertrand, Goldin, and Katz, 2010; Goldin, 2014; Goldin and Katz, 2016). Goldin (2014) pioneered the idea that the organization of the workplace is crucial in determining the size of the gender earnings gap of different occupations. Her definition of flexibility includes “a multitude of temporal matters including the number of hours, precise times, predictability and ability to schedule one’s own hours.” [p. 1104] The dimension of work arrangements we study (the possibility of reducing work hours while keeping a permanent contract, and thus a regular and predictable schedule) is therefore part of what she calls “flexibility” (though we will avoid using this term because in our setting it might create confusion).

Providing quasi-experimental evidence on the impact of “Goldin’s flexibility” on the child penalty – and thus on the gender earnings gap – is a novel contribution to the literature. Two papers we are aware of investigate the relationship between the child penalty and “Goldin’s flexibility”: Bütkofer, Jensen, and Salvanes (2018) compute child penalties for different professions in Norway, and show that the child penalty is smaller in more flexible professions; Bang (2021) shows that child penalties in the US are decreasing in own and spouse’s flexibility. While this descriptive evidence is suggestive, it does not exclude the possibility of this correlation being driven by sorting – it could be the case that women

significant decrease in female share of home production.

⁵Contrary to common wisdom, the size of the child penalty does not seem to be affected by parental leave policies (Dahl, Løken, Mogstad, and Salvanes, 2016; Kleven, Landais, Posch, Steinhauer, and Zweimüller, 2024; Schönberg and Ludsteck, 2014), 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., 2024). The effects of family policies on female labor market outcomes are reviewed in Olivetti and Petrongolo (2017). Similarly, biology and comparative advantage don’t explain the observed child penalty (Andresen and Nix, 2022; Kleven, Landais, and Søgaard, 2021). On the other hand, gender norms are a strong predictor of its size (see, for example, Boelmann, Raute, and Schönberg, Forthcoming, and Kleven, 2022, among others).

⁶Beware though that similar policies might backlash, as happened when Spain introduced the right to part-time work for parents (Fernández-Kranz and Rodríguez-Planas, 2021). We don’t find evidence of backlash in our context; we expand on the difference between the Australian and the Spanish settings in Appendix C.4.

who are more career-oriented (and thus would be more likely to return to their job after childbirth anyway) also select more flexible occupations and spouses with more flexible jobs.⁷ We are the first to provide causal evidence of the link between work arrangements and the child penalty.

Our story hinges on two components of the interaction between work arrangements and parenthood: on the one hand, working mothers value predictability and regularity in their schedules; on the other hand, they often need to reduce working hours. In support of the first fact there is experimental evidence by Mas and Pallais (2017), who find that women, especially mothers of young children, place a higher than average value on avoiding irregular work schedules.⁸ In support of the second, there is extensive descriptive evidence,⁹ as well as causal evidence by Paule-Paludkiewicz (2024), who finds that the take-up of part-time by mothers in Germany significantly increased by 2.8 percentage points following a law granting all employees the right to part-time work.¹⁰ We add to this literature by showing the importance of the coexistence of these two work arrangements (shorter working hours and regular schedules).¹¹ Indeed, when comparing our results to those of Paule-Paludkiewicz (2024), we find that granting schedule regularity (in addition to reduced hours) has a positive impact on maternal labor supply which is at least twice as large, and possibly several times larger, than only granting the right to work part-time.¹² This point is also present in Del Boca, Pasqua, and Pronzato (2009), who find that the diffusion of part-time work is positively associated with women employment only in countries in which part-time jobs offer the same stability as full-time work.

Our final set of results, which shows the lack of changes in the intra-household organization of home production following the reduction in child penalty we document, speaks to a large literature on the question of whether men and women split housework and parenting tasks based on labor income. A recent contribution, using our same data, is Siminski and Yetsega (2022): they find that that comparative advantage plays little or

⁷Bütikofer et al. (2018) explicitly mention in their introduction that they “cannot rule out differences due to selection into certain educations or professions.” Similarly, Bang (2021) “document[s] the empirical relationship between occupational flexibility and married couples’ labor adjustment around childbirth” but does not claim an absence of endogeneity in occupational choice.

⁸Further evidence is provided by Bolotnyy and Emanuel (2022) who, using administrative time-card data on bus and train operators, find that female operators, especially those with dependents, pursue schedule conventionality, predictability, and controllability more than male operators.

⁹Virtually all papers about the child penalty document a reduction in hours of work upon motherhood, e.g. Kleven, Landais, and Søgaard, 2019, in Denmark, de Quinto, Hospido, and Sanz, 2021, in Spain, Cortés and Pan, 2023, in the US.

¹⁰Additional causal evidence that women value relatively shorter schedules is provided by Wasserman (2023), who finds that when a medical specialty reduces its weekly hours, more women enter the specialty, whereas there is little change in men’s entry.

¹¹Note that in our setting there is a very strong correlation between having a permanent contract and being on a regular schedule, which we document in detail in Section 3; thus, we refer to them almost interchangeably here.

¹²On the extensive margin, we find an average effect of the Fair Work Act that ranges from 4 percentage points the year after childbirth to 11 percentage points six years afterward, while in Paule-Paludkiewicz (2024) the corresponding figures are 1 and 4 percentage points respectively. Paule-Paludkiewicz (2024) does not observe the intensive margin – only that there is an increase in the probability of working part-time when the child is young, while we find a 19% reduction of the child penalty in intensive-margin labor supply even the year following the birth of the first child.

no role in the gendered division of labor within couples, and they document that Australian women do much more housework than males at every percentile of the relative wage distribution.¹³ However, this distribution is likely to be endogenous to choices made before marriage and childbirth (like education and job sorting). Instead, we rely on a quasi-exogenous change in the relative earnings distribution within the family, provided by the reduction in the child penalty in labor supply brought about by the Fair Work Act. Our paper documents that a quasi-exogenous change in the female-share of household income does not lead to a corresponding adjustment of home production task allocation, thus adding causal evidence to the mostly correlational patterns already present in the literature.¹⁴

The remainder of the paper is organized as follows. Section 2 describes the data and the sample selection. Section 3 first describes the institutional setting and then discusses the impact of the Fair Work Act on work arrangements, in particular on the prevalence of less-than-full-time permanent contracts. Section 4 describes the status quo, namely the child penalty in contract type and time use before the Fair Work Act. Section 5 documents the impact of the Fair Work Act on the child penalty and on the intra-household allocation of time, first through an early versus late cohort comparison (Section 5.1) and second through the exposure design (Section 5.2). Section 6 concludes.

2 Data: The HILDA Survey

The Household, Income, and Labor Dynamics in Australia (HILDA) Survey (Department of Social Services and Melbourne Institute of Applied Economic and Social Research, 2020) contains a rich set of information on a representative sample of Australian households since 2001. For each of the approximately 8,000 sampled households,¹⁵ everyone above 15 years old is interviewed, but basic demographics are also collected for younger members of the household. Individuals within families are followed over time, and family linkages are codified, which allows us to match each individual with their spouse or *de facto* partner and to identify when children are born. The design of the HILDA Survey is detailed in Watson and Wooden (2012). We limit the sample to the years 2001-2019 to avoid confounders from COVID.

The distinctiveness of the HILDA Survey lies in the fact that it provides information on work characteristics and time use for all adult household members. This allows us to test whether the Fair Work Act impacted work arrangements and study, in the same sample, how this affected the time use of both the treated mothers and their partners.

¹³A similar pattern is found in other settings, such as the US (Bertrand, Kamenica, and Pan, 2015; Hancock, Lafortune, and Low, 2024) and Germany (Jessen, Schweighofer-Kodritsch, Weinhardt, and Berkes, 2024).

¹⁴See also Ichino, Olsson, Petrongolo, and Skogman-Thoursie (Forthcoming) for related causal evidence in the context of Sweden.

¹⁵The sample was expanded in 2011 with the addition of 2,000 households. We include them in our analysis, but all results are robust to restricting data to the original sample.

Variables Definition Our analysis relies on measures of work arrangements and of time use. By work arrangements we indicate what determines when, where, and how a job is expected to be performed. This includes the number of working hours, but also how the hours are arranged in a schedule. Key variables in our analysis will be the type of contract (whether casual or permanent), whether a schedule is regular (working from Monday to Friday or four weekdays on a regular daytime schedule) or irregular (being on call), whether the job is full-time (35 hours per week or more), less-than-full-time (1-34 hours per week), or “reduced hours” (between 25 and 34 hours per week). Usual work arrangements are elicited also for individuals on paid leave, who are considered employed. This mitigates concerns of selectively missing information, as well as concerns of possibly overestimating the child penalty raised by Adams, Fjællegaard Jensen, and Petrongolo (2024).

We measure three dimensions of time use: paid work, housework, and parenting. “Paid work” measures the number of hours per week spent in paid employment or commuting; “Housework” is a combination of housework, outdoor tasks, and errands; “Parenting” is time spent playing with or caring for own children. We define “Home production” as the sum of housework and parenting, and we call “Total active time” the sum of home production and paid work, that is, weekly hours spent not in leisure or sleep.¹⁶

We are also interested in how time is allocated within the couple. To this end, we define “Female share of home production” as the number of weekly hours a woman spends in home production divided by the sum of weekly hours she and her male partner spend in home production. “Female share of paid work” is defined analogously. “Female share of total income” is the fraction of total annual household labor income earned by the woman. These “share” variables are only defined for heterosexual couples, and only when we observe the underlying time use and income variable for both partners in the couple. Details on variables definition can be found in Appendix A.

Sample Restrictions We make two sets of sample restrictions: one is individual-based, and one is couple-based.

At the individual level, we focus on people aged 15 to 65. When we study parents (from Section 4 onwards) we only keep parents whose first child was born between 2003 and 2017, in order to observe at least two years before and two years after the first childbirth. For parents, we only keep observations from 4 years before childbirth to 7 years after, in order to have a balanced panel when we restrict to the cohorts close to the Fair Work Act. In the pre-post Fair Work Act comparisons, we exclude the cohort of parents who had their first child exactly in 2009, since this cohort is partially treated.

The couple-based restrictions are germane to our study of intra-household task allocation and apply to Section 5.2.¹⁷ We focus on cohabiting heterosexual couples, both married and *de facto* partners. This excludes 9% of first births from single mothers and 1% of first births from lesbian couples (not enough to adequately study them separately, which would

¹⁶The questionnaire specifies not to count any activity twice, and caps the total number of indicated hours at 168.

¹⁷All the results in the sections preceding this one replicate in this smaller sample and are available upon request.

be desirable if sample size allowed). Partnerships and marriages don't last forever: some women who report having a partner the year of their first childbirth no longer do in subsequent years (6% and 11%, 2 and 5 years after first childbirth respectively), and some change partner (0% and 3%, 2 and 5 years after first childbirth respectively). This is not systematically different between couples who have their first child before and after the Fair Work Act. In our primary sample, for each year we match each woman to the partner she indicates that year, regardless of whether he is the partner in the year of her first childbirth. In our secondary sample, we match each woman to the partner she indicates the year of her first childbirth, regardless of whether they are no longer or not yet a couple. Results are practically identical. We only report results on the primary sample, but results on the secondary sample are available upon request.

Given our focus on couples, and how time is split in the couple, we restrict the analysis to couples for which we observe variables of interest for both the man and the woman. If a given variable in a given year is missing for the man, we set the corresponding variable to missing for his partner as well, and vice-versa. This allows us to make sure that results on men and women are comparable, that is, they come from the same couples. In practice this restriction doesn't significantly impact our results, but it allows for a cleaner interpretation.

Since the exposure design relies on a woman's job before childbirth, for the analysis of Section 5.2 we restrict the sample to couples in which we can observe it. Namely, we keep women (and their partners) if they work and report an occupation and an industry at least once in the five years prior to the birth of their first child. This is not very restrictive: 89% of mothers in our sample satisfy this criterion.

We keep non-parents and parents with older children as controls, meaning we drop individuals who enter the survey with children aged five or younger. Our results are robust to dropping all individuals who are already parents when they enter the survey, and keeping only the non-parents as controls. In the sample of non-mothers, we assign random fake childbirth years, and we perform symmetrical matching with partner and cleaning procedures as for the mothers.

Descriptive Statistics The summary statistics in Table 1 show clear gender differences in work characteristics and time use in the Australian population. 72% of women are in the labor force, as opposed to 84% of men, and, conditional on working, women work 31 hours per week on average, while men 42. Conditional on working, 26% of women are in casual contracts and 63% have a permanent contract, while these figures for men are 19% and 72% respectively. Women do the home shift: on average, they spend 31 hours per week in home production, while among men this figure is 18 hours. These aggregates mask extensive heterogeneity by parental status and cohorts, which will be the focus of the rest of the analysis.

3 Institutional Setting: Casual Jobs and the Fair Work Act

In this section, we describe the institutional setting before the 2009 Fair Work Act and the change in work arrangements the Act brought. The Fair Work Act empowered mothers who wanted to reduce hours of work to do so within their permanent contracts, which are

generally associated with a regular schedule. Prior to the Act, switching to irregular casual contracts was the main route to reduce work hours below full-time. We document that this change is reflected both in the text of the awards (sector-specific labor regulations) and in the data: the probability of new mothers having a permanent job with reduced hours more than doubled after the Fair Work Act. In subsequent sections, we will study the impact of this large change in work arrangements on the child penalty.

3.1 Before 2009: Less-Than-Full-Time Jobs Were Mostly Casual

Before the Fair Work Act, it used to be uncommon to work less than full-time in permanent contracts, and individuals who wanted to reduce hours – primarily mothers – often turned to casual contracts, characterized by very irregular schedules, or quit.

There are three types of contracts in the Australian labor market. The most common type is permanent (covering 68% of workers in 2019), which does not have a termination date and includes a prescribed number of weekly hours. Fixed-term contracts, although much less prevalent (11%), are very similar to permanent jobs, the only difference being that they have an end date. Finally, a peculiarity of the Australian setting is the existence and widespread use of casual contracts.

Casual contracts are zero-hours contracts with no commitments on either part, without many entitlements, but which, in principle, pay higher wages. Casual contracts are relatively common in Australia (more than 20% in 2019), and they are disciplined by the awards (pieces of regulation that outline the minimum pay rates and conditions of employment, separately for different industries): even when the award includes protections for the workers who are hired under permanent contracts, special clauses allow for workers to be employed under certain forms of casual employment that are exempted from the standard provisions (Pocock, Buchanan, and Campbell, 2004). Casual contracts are characterized by the absence of legal entitlements such as paid leave (annual leave and sick leave), paid public holidays, minimum periods of notice of termination, and severance pay; as compensation, casual employees are usually entitled to a pay loading, which was around 20% before 2010, and was set to progressively increase up to 25% (Lass and Wooden, 2019).¹⁸ Thus, for the average worker, casual contracts are not necessarily inferior to permanent contracts.

However, casual contracts are substantially more likely than permanent to be associated with an irregular schedule. Appendix Figure B.1 shows this association across jobs: occupations and industries with a higher share of casual contracts also have a greater fraction of workers who report being on call and a lower fraction who report having a regular schedule. This is also true within jobs: when controlling for occupation-by-industry fixed effects, people who work under permanent contracts are still 17 percentage points (p.p.) more likely to have a regular schedule (Appendix Table B.1), highlighting the fact that the more irregular nature of work arrangements for people under casual contracts is a characteristic of the contract itself and not a simple by-product of different occupations and industries. In fact, while some industries and occupations are dominated by one type

¹⁸Note that by making casual contracts more attractive, this provision, if anything, plays against the results we find.

of contract or the other, many see workers in either regime. For example, 51% of people working as service and sales workers have a casual contract, as do 24% of teaching associates.

Casual employment is most common among people working less-than-full-time jobs, and there is a gender component: Pocock and Masterman-Smith (2005) comment on the institutional setting before the Fair Work Act and highlight that Australian women are more likely than the OECD average to work part-time, and much more likely than Australian men to work under casual contracts.

The existence of this trade-off between reducing working hours and giving up predictability is also summarized in Pocock, Skinner, and Ichii (2009), commenting on the pre-Fair Work Act landscape. They argue that “workers may seek out casual work [...] as a strategy to reduce their time commitment to paid work. However, our findings suggest that they are likely to get the same work-life benefits just by working shorter hours as a permanent or fixed-term employee. Unfortunately, finding part-time work in the current Australian labour market often requires a shift to casual work – with effects on the quality of work, income, careers, retirement savings and so on.” More specifically addressing how this trade-off has gender-related consequences, the authors highlight how “in pursuing part-time work, many women find that they have to accept casual employment.”

All this evidence coherently indicates that the Australian labor market is quite segmented: on the one hand, there are regular jobs, whose schedules are predictable and mostly full-time and which are under permanent contracts; on the other hand, there are irregular jobs, which are generally part-time and performed under casual contracts.

3.2 The Shift: The Fair Work Act Facilitates Shorter Hours in Permanent Contracts

The Fair Work Act responded to calls for “permanent part-time”: it empowered mothers to reduce hours below full-time without giving up a permanent contract and the regular schedule that comes with it. Qualitatively, we find that indeed the wording of the awards (industry-specific labor regulations that discipline minimum standards of employment) changed to include permanent less-than-full-time work. Quantitatively, we find that the distribution of hours worked under permanent contract shifted to include a previously missing mass of “reduced hours” (25-34 hours per week).

3.2.1 The Wording of the Law and its Interpretation

While the Fair Work Act allows a generic “change in working arrangements”, it was understood in the media as well as by legal scholars as introducing a right to request “permanent part-time”, namely the option to remain in a permanent contract – with the regular schedule associated with it – while reducing hours below full-time.

The Fair Work Act establishes that “An employee who is a parent [...] may request the employer for a change in working arrangements to assist the employee to care for the child if the child [...] is under school age.” Importantly, “The employer may refuse the request

only on reasonable business grounds.”¹⁹ Requests that are protected under this provision “include changes in hours of work, changes in patterns of work and changes in location of work.”²⁰ The list of work arrangements that can be made under the protection of this provision includes “changing from full-time to part-time”.²¹ This is not the only protected request, but we can argue that the notion of “permanent part-time”, a work arrangement that combines the security and regularity of a permanent contract with reduced hours, is an important component of the reform we are considering: Pocock et al. (2009), for example, while commenting on the Fair Work Act “right to request”, cite the option of permanent part-time as an important tool in facilitating work-life balance of workers.

3.2.2 Qualitative Evidence of the “Success” of the Law

We find evidence of the Fair Work Act increasing access to less-than-full-time permanent contracts both in the text of the awards (industry-specific regulations that prescribe minimum employment conditions and pay rates), which explicitly allow for this possibility after the Fair Work Act, and in the distribution of hours worked under permanent contracts, which shifts after the Fair Work Act to include a previously missing mass of “reduced hours” (25-34 hours a week).

In the edition of their “Labour Law” textbook published right after the passage of the Fair Work Act, Creighton and Stewart (2010) argue that “until recently many awards did not provide for part-time workers to be engaged on anything but a casual basis. But with changing attitudes in the union movement, and legislative reforms, the concept of permanent part-time employment has become well accepted.”

For example, the 2010 Modern Award for Private Hospital Employees mentions that “A part-time employee has reasonably predictable hours of work. Before commencing employment, the employer and employee will agree in writing on a regular pattern of work including the number of hours to be worked each week, days of the week the employee will work and starting and finishing times each day.” This is in contrast to the corresponding pre-Fair Work Act award, which did not consider regular part-time positions at all. This and other examples of change in the wording of the awards for different occupations and industries are included in Appendix Table B.2.

The importance of the Fair Work Act in allowing women to maintain permanent positions while reducing working hours emerges also in the case study by Cooper and Baird (2015), who interviewed managers and employees from two major Australian firms. The authors find that the typical request made under the protective umbrella of the Fair Work Act involved a reduction of working hours below full-time. In particular, the majority of the employees interviewed “were in reduced hours permanent roles and each of these interviewees were mothers returning from a period of maternity leave, having previously

¹⁹With the Fair Work Amendment Act of 2013, the right to request a change in working arrangements was extended to parents of school-aged children – the flexible specification we employ in our analysis in Section 3.3 will account for this.

²⁰Fair Work Act 2009, Section 65, available at this link: <https://www.legislation.gov.au/C2009A00028/2009-07-01/text>

²¹<https://www.fairwork.gov.au/tools-and-resources/best-practice-guides/flexible-working-arrangements>

worked in standard full-time role.”

Coherently with this narrative around the Fair Work Act and with the institutional evidence presented in this section, we find suggestive evidence, in our data, that the Fair Work Act did introduce permanent contracts with reduced hours. We see an increase of permanent contracts among mothers working less than 35 hours/week (Appendix Figure B.2) and a shift in the distribution of hours worked by mothers under permanent contracts (Appendix Figure B.3) that “fills” the missing mass that we could observe, before 2009, on the immediate left of full-time jobs.

The wording of the law and the interpretation given by the Australian government, together with the qualitative evidence described in this paragraph, allow us to conclude that reduced hours and types of contract – with the regularity that comes with them – are the key work arrangements to study when evaluating the effects of the Fair Work Act.

3.2.3 Other Relevant Institutional Features

Australian workers have been entitled to 12 months of job-protected unpaid parental leave since 1979.²² This was extended with the Fair Work Act, which allowed parents to request up to 12 additional months, subject to the employer’s approval. This extension had a very low take-up – we discuss this change, together with the other policies that were approved as part of the Fair Work Act, in Appendix E.2.

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. The paid leave 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, which existed since 2002. This introduction does not impact our results (which is consistent with incentives not changing dramatically, given the pre-existing combination of job-protected leave and a cash transfer upon childbirth). We describe the reform and address its impacts in detail in Appendix F.

Pre-school education, for children aged 3 to 5 years old, is not compulsory, and responsibility for its management is shared between the Australian Government and the state or territory governments. At the time of the Fair Work Act introduction, enrollment was between 60 and 80% in most States (cumulatively for government- and privately-run institutions), for the year before the start of formal schooling (Dowling and O’Malley, 2009). Government subsidies are available to cover portions of the childcare costs and are decreasing in household income. Formal schooling begins at 6 years old and is compulsory.

3.3 First Stage: Quantitative Evidence of the Effect of the Law on Work Arrangements

We employ a difference-in-differences methodology (allowing for a large set of treatment effect heterogeneities) to show that the Fair Work Act did have bite: after the passage of the law, new mothers were twice as likely to be in jobs with a permanent contract and

²²See <https://www.fairwork.gov.au/about-us/workplace-laws/fair-work-system/australias-industrial-relations-timeline>

reduced hours, and 41% more likely to report having a regular schedule. This is a large shift in work arrangements we will leverage in subsequent sections.

3.3.1 Empirical Strategy

Our strategy is a variation of the standard difference-in-differences: instead of simply comparing treated individuals (parents of children under school age) to everyone else, before and after the law, we augment the specification by allowing the changes in the outcome variable to depend flexibly on the child's age.

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 six in the treated group (after 2009) and everybody else, both non-parents and parents of older children, in the control group. However, it is reasonable to expect that the law would impact different demographics differently, based on gender and children's age. First, we run the analysis separately by gender: if women are those who disproportionately care for the children, a law which is explicitly aimed at improving work-family balance²³ might disproportionately affect them. Second, there could be path-dependence in maternal labor supply: for example, 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.

We account for potential treatment effect heterogeneity and persistence as follows. Instead of including one dummy for a parent's child being aged 0-5, we include several dummies: one for the first child being aged 0-2, one for the first child being aged 3-5, 6-8, and so on. This accounts, for example, for the fact that a law changing labor market opportunities for women might have a larger effect if passed when a woman's first child is just born and she is reoptimizing her labor supply, rather than later when certain choices (of leaving the labor market for example) might be hard to reverse.²⁴ For the same reason, we use dummies based on the age of the first child, rather than on the age of the youngest child.²⁵ Similarly, if a work arrangement has been in place for a few years, there might be no incentives to change it when the child turns six, which is why we also include dummies for older children.²⁶

²³Section 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”.

²⁴Kuka and Shenhav (2024) document this pattern for the US: they find that single mothers exposed to the expansion of the Earned Income Tax Credit immediately after a first birth have 3 to 4 p.p. higher employment in the five years after a first birth than single mothers exposed 3 to 6 years after a first birth.

²⁵Including dummies based on the age of the youngest child yields no changes in the coefficient estimates on the dummies for the age of oldest child, and, when controlling for age of oldest child, dummies on the age of youngest child yield coefficient estimates of zero.

²⁶In 2013, the Fair Work Act Amendment expanded the right to request a change in working arrangements to parents of all school-aged children, which further rationalizes the choice of separately controlling the presence of older children.

We run the following specification, separately by gender:

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

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

Threats to Identification The difference-in-differences strategy just described relies on a parallel trends assumption between mothers and non-mothers; namely, that mothers of young children before and after 2009 would have shown similar work arrangements in the absence of the law, relative to non-mothers. We provide supporting evidence for this assumption in several ways. In addition to showing the absence of pre-trends in the figures below, in Appendix C we show that there was no systematic change in fertility or selection into fertility around 2009, that the financial crisis (which was mild in Australia, not even a formal recession) does not play a role in these results, and that there was no employer response on the hiring margin (no backlash through decreased hiring).

3.3.2 Results

Our main finding is that new mothers are more likely to be in permanent contracts with reduced hours after the Fair Work Act than mothers with similarly aged young children before 2009. Correspondingly, they are more likely to have a regular schedule, but they do not experience any change in flexibility. For fathers, we don't observe any change in work arrangements.

Reduced-hours Permanent Contracts and Schedule Regularity Increase for Mothers
The fraction of mothers with a reduced-hours permanent contract more than doubled after 2009, but with considerable heterogeneity according to age of the child. From the left panel in Figure 1 we learn that, in 2009, only 3% of mothers who had given birth to their oldest child in the previous two years worked under a reduced-hours permanent contract, where reduced hours indicate 25 to 34 hours per week (approximately 80% of full-time); 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, more than tripling in the first three years and constantly remaining at a higher level afterward. In the right panel, 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. Appendix Figure D.1 shows that this pattern holds identically when conditioning on working women, which

clarifies that the extensive margin of labor force participation is not driving the observed pattern; and it also shows that, correspondingly, the probability of having a regular schedule jumps up for mothers of young children after 2009 and remains persistently higher.

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 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 years old with an indicator for the year being equal to or after 2010, and the dummy for the first child being between 3 and 5 years old with an indicator for the year being equal to or after 2013. 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. Estimates are reported in Table 2.

Other Work Arrangements: While Schedule Regularity Increases for Mothers, Flexibility Does Not Table 2 confirms the visual evidence that the increase in reduced-hours permanent contracts was accompanied by an increase in the probability of being on a regular schedule for mothers, while other work arrangements, such as flexibility and work from home, either did not change or give inconclusive results. Column (1) summarizes the findings in Figure 1 relative to reduced-hours permanent contract (unconditional variable, equal to 0 for unemployed people), and column (2) confirms that they hold also conditionally on working, as already shown in Appendix Figure D.1: the probability for a young mother to be on a reduced-hours permanent contract conditional on working doubles after the Fair Work Act. While columns (1) and (2) emphasize that the large increase is of mothers who work right below full-time, column (3) reports a 47% increase in the more general probability of working less-than-full-time (1 to 34 hours per week) on a permanent contract. This is accompanied by a 41% increase in the probability of being on a regular schedule (column 4) and a 28% decrease in the probability of being on call (column 5).

Other work arrangements, while in principle covered by the Fair Work Act, did not change systematically and permanently, highlighting the importance of understanding the public discussion around this law rather than simply its words. Flexibility, measured as agreeing with the statement “My working times can be flexible”, decreases for mothers of very young children (column 6 of Table 2), consistently with a switch from casual to permanent contracts (as casual contracts are associated with greater schedule flexibility) rather than with the acquisition of the right to request flexibility. The analysis regarding work from home is inconclusive: the coefficient estimates in column (7) range from negative to positive, are mostly insignificant, and the corresponding figure (not shown) displays a lot of volatility in the estimated coefficients.

No Changes in Work Arrangements for Fathers We replicate the same analysis on men, and show that there are no systematic changes for fathers of young children around 2009. Appendix Table D.1 replicates Table 2 on the sample of men. Reduced-hours permanent contracts are very rare among men (only 2% of working fathers have this arrangement), who tend to work full-time, and this does not change with the Fair Work Act. For fathers,

the probability of being on a regular schedule was almost double that of mothers before the Fair Work Act (59% probability for fathers versus 32% probability for mothers of 0 to 2-year-olds) and was not affected by the law, along with the other work arrangements.

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 comes from women (76% of them, according to survey evidence in O'Neill, 2012). This insight, together with the evidence we will present in Section 4 of the child penalty being an issue only for mothers, motivates the primary 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 at that moment, and these 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 the subsequent analysis of defining treatment and exposure to the law based on the year of birth of the first child.

4 The Status Quo: Child Penalties Before The Fair Work Act

We now set the ground for the remainder of the analysis by describing the child penalty before the Fair Work Act. Women who became mothers before the Fair Work Act experienced a large decrease in labor supply upon childbirth, a massive increase in home production, and, consistently with the institutional setting, a sizeable transition from permanent to casual employment. This changed sharply after the reform, as did women's time allocation, as we will show in the next section.

4.1 Measuring the Child Penalty

We define the “child penalty” as the change in an outcome of interest in the seven years following the birth of an individual’s first child, relative to the four years before the child-birth, and relative to a counterfactual trend based on similarly aged childless individuals and individuals with (much) older children. We present it here as an event study. Note that we define the child penalty as the gap between a woman who becomes a mother and her counterfactual self had she not had children, rather than the gap between mothers and fathers. Using the male-female gap would not alter the conclusions of our analysis, since we do not find any changes around childbirth in most variables for men, neither before nor after the Fair Work Act.

Our main object of interest is how work arrangements and time use change around the birth of their first child for women and men. Formally, we are interested in the coefficients

γ_k from the following event study regression:

$$Y_{it} = \sum_{\substack{k=-4, \\ k \neq -2}}^7 \left\{ \gamma_k \times \mathbb{1}\{t - E_i = k\} \right\} + \alpha_i + \delta_t + \beta_{h(i)} + \epsilon_{it}, \quad (2)$$

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. We run this regression separately for men and women, and for couples as a single entity where appropriate. The coefficients of interest, γ_k 's, represent the change in outcome Y k periods after childbirth, relative to two years before – this is the child penalty in outcome Y , k years from childbirth. Never-parents and individuals who don't have a child aged five or younger when they enter the survey 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.²⁷ We cluster standard errors at the individual level.

4.2 Evidence on the Child Penalty Before the Fair Work Act

In this section, we show that women who became mothers before the Fair Work Act experienced a stark transition from permanent to casual employment at the time of childbirth (-25 percentage point probability of being in a permanent contract conditional on working). This was accompanied by a substantial reallocation of time from paid work (-28 hours per week) to home production (+53 hours per week) and a large decrease in leisure and sleep (-24 hours per week). Men did not change their employment contract upon fatherhood; they only mildly changed labor supply and time spent on housework (about two hours a week of less paid work and more housework) and spent about a third of the time with the newborn relative to women, decreasing leisure and sleep about half as much relative to women.

The Transition from Permanent to Casual Contracts for Mothers Before the Fair Work Act, women moved to casual jobs after becoming parents, while men did not. As explained in Section 3, before the Fair Work Act it used to be very hard to reduce hours below full-time while keeping a permanent job. Instead, in order to reduce hours, people often had to move to casual contracts, which don't require any commitment and are associated with irregular hours. Unsurprisingly, motherhood, with its large increase in time demands, was associated with a sharp transition from permanent to casual jobs. This is evident in panel (a) of Figure 2, which plots the estimate of the child penalty in the probability of being on a permanent contract conditional on working, separately for women (in pink circles) and men (in blue squares). From the figure, we see that, while the pre-birth prevalence of permanent contracts among working men and women was the same (73%), for women we observe a large drop (almost 20 percentage points) in the probability of being on a permanent contract upon motherhood, which increases over time to nearly 30 percentage points in the seven subsequent years (the average over this time horizon is 25 p.p.). Recall

²⁷Accounting for treatment effect heterogeneity is a novelty relative to the current practice in the literature on the child penalty, as emphasized by Melentyeva and Riedel (2023).

that this is only among women who are employed, so the drop is not explained by women dropping out of the labor force. Instead, we see no changes in contract type for men who become fathers.

Partners Specialize and This has Long Term Consequences We document a post-childbirth decrease in leisure and sleep of both partners, that hides very heterogeneous changes in time use. Panels (c) and (d) of Figure 2 show the change in time use around the birth of their first child, for women and men respectively. In panel (d) we see that, for men, time in paid work decreases by a small amount (on average, 2.6 hours per week), while time spent in household work increases marginally (1.4 hours per week), and basically the entire increase in active time (14 hours per week) comes from time spent parenting. For women, in panel (c), the story is very different: in order to make space for the 42 hours a week spent parenting and the 11 hours a week of increased housework, they decrease time in paid work and commute by 28 hours per week (a 77% decrease relative to their pre-child work hours). On net, women decrease their leisure and sleep twice as much as men: panel (b) shows that post-childbirth leisure and sleep decrease by 21% for women (-24 hours per week), and by 12% for men (-14 hours per week). This indicates that the large increase in maternal labor supply we document in the next section did not come from women having extra leisure time.

Choices of time use made when the first child is born have long-lasting consequences. From panel (c) of Figure 2, we see that, while parenting time needs decrease over time, women do not increase time in paid work symmetrically, suggesting that work choices made at a time of high pressure – the arrival of a baby – might be hard to reverse even after the direct pressure decreases. This translates into a permanently lower income for mothers. If there isn't perfect income pooling, or if bargaining power in a couple depends on income, this might have adverse consequences for women's welfare in the long run.

5 The Impact of Work Arrangements on the Child Penalty and on the Intra-household Allocation of Time

Exploiting the quasi-experiment brought about by the Fair Work Act, we show that having access to a job with a permanent contract and reduced hours dramatically lowers the child penalty in labor supply and housework for treated mothers, while their partners' time use is unaffected. We show this using two complementary empirical strategies. The first exploits variation in the timing of childbirth, while the second adds to it variation in exposure to the law at the occupation-by-industry level, defining individual exposure from the job mothers held before childbirth. We find that women who had their first child after the Fair Work Act are 70% less likely to move out of a permanent contract after childbirth and work 54% more hours per week relative to their earlier counterparts. The entirety of the effect is concentrated among women in jobs most exposed to the law. While treated mothers do reduce time spent on housework (but not on parenting), their partners do not pick up the slack. Since parenting is the biggest component of home production, women's reduction of housework is not enough to tilt the household task sharing towards equality,

and the female share in home production does not change significantly, despite a large and significant increase in the female share of household income.

5.1 Strategy 1: Early versus Late Cohorts

In this section, we compare women who became mothers in the four years preceding the Fair Work Act (early cohorts) and those who became mothers in the four years after the Fair Work Act (late cohorts): we find that later cohorts of mothers are only 30% as likely as the early cohorts to move from permanent to casual contracts after childbirth, and this is accompanied by a 22% lower child penalty in labor supply. Under the assumption that the late cohorts of mothers would have behaved like the early ones absent the law, this comparison recovers the causal estimate of the change in work arrangement brought by the Fair Work Act on the child penalty. We support this assumption by showing that mothers in the early and late cohorts were similar on observables before childbirth and that the change in the child penalty happened sharply around the 2009 cohort (so the early versus late comparison is not capturing a smooth time trend), as well as by addressing possible contemporaneous confounders.

5.1.1 Empirical Strategy

We compare the child penalties in contractual form, labor supply, and home production for early and late cohorts of mothers, where early cohorts are defined as women who had their first child between 2005 and 2008 (before the 2009 Fair Work Act) and late cohorts are women who became mothers between 2010 and 2013.

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 and mothers of older children as controls in both, and we plot the resulting estimates in the same graph, to gauge the difference visually.

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 particular, we estimate the following model:

$$Y_{it} = \gamma \times \mathbb{D}_{it} + \gamma_{post} \times \mathbb{D}_{it} \times Post(i) + \alpha_i + \delta_t + \beta_{h(i)} + \epsilon_{it} \quad (3)$$

where $\mathbb{D}_{it} \equiv \mathbb{1}\{(t - E_i) \in [0, 7]\}$ is an indicator function for i 's first child being between 0 and 7 years old (which are all the post-childbirth observations we keep for new parents), and $Post(i) \equiv \mathbb{1}\{E_i > 2009\}$ 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. Similarly, the year right before childbirth is also partially treated (some women stop working during pregnancy, for example), therefore we also drop it in this analysis. In this specification, the estimate of γ summarizes the child penalty in Y for the early cohorts, while γ_{post} is the difference in child penalty for late cohorts relative to early cohorts, which is the object of interest discussed in this section.

5.1.2 Results

Late (post-Fair Work Act) cohorts of mothers are 70% less likely to transition from permanent to casual contracts after childbirth, relative to early (pre-Fair Work Act) cohorts. They are also eight percentage points more likely to be in the labor force post-childbirth and they work four more hours per week conditional on working. While they decrease the number of hours in housework, time spent directly with the child is unaffected.

Figure 3 shows the change in the child penalty for women brought by the Fair Work Act, for all the outcomes of interest. In this figure, the green circles connected by dashed lines display the estimates of γ_k 's from Equation (2) estimated only on mothers whose first child was born between 2005 and 2008, and the red squares connected by solid lines display the same estimates but on the sample of mothers whose first child was born between 2010 and 2013. Table 3 reports the estimates of Equation (3), where we test formally for the difference in child penalty between late and early cohorts of mothers.

Lower Probability of Transitioning to Casual Employment and to an Irregular Schedule After Childbirth Post-Fair Work Act cohorts of mothers experienced a substantially lower probability of transitioning to casual employment, consistently with the interpretation of the law and the evidence presented in Section 3. Panel (a) of Figure 3 shows the change in the probability of being on a permanent contract conditional on working around motherhood. The green circles replicate the pink line in Figure 2(a) and show that for pre-Fair Work Act cohorts of mothers this probability dropped by 20 to 30 percentage points, with the magnitude of the drop increasing over time. The red squares show that instead for post-Fair Work Act cohorts of mothers this probability dropped far less (about 70% less): women who became mothers after the Fair Work Act did use their new right to remain in permanent and regular employment, instead of switching into casual employment, when moving to below full-time after childbirth.

The documented decrease in the child penalty in permanent contracts hold also unconditionally, and it is accompanied by a corresponding decrease in the child penalty in regular schedule. Table 3 shows that the change in the child penalty in permanent contract is comparable when considering the unconditional probability of having a permanent contract (column 1) as to when using the conditional probability as outcome (column 2) – thus ruling out a systematic exit of mothers who would have moved to a causal contract post-childbirth as the driver of the conditional result. Consistently with the institutional context, the child penalty in the probability of having a regular schedule decreases alongside that of having a permanent contracts (column 3), highlighting the strong correlation between contract type and schedule regularity. Note that the increase in the unconditional probability of having a permanent contract is almost twice as large as the increase in employment (column 6), thus showing that the documented intensive-margin changes in work arrangements cannot be fully explained by extensive-margin changes–though greater entry in the labor market because of more suitable work arrangements is part of the story too.

Treated Mothers Work Longer Hours This change in type of contract and schedule regularity was accompanied by a significant increase on the extensive and, in particular, on the intensive margin. Panel (c) of Figure 3 shows that, on average, late cohorts of mothers returned to work at a higher rate relative to the earlier cohorts, and in particular, they are eight percentage points more likely to return; the figure is the same both considering labor force participation and employment (columns 5 and 6 in Table 3), indicating that unemployment does not change systematically. Even more starkly, panel (b) of Figure 3 shows that, among working mothers, late cohorts worked on average four more hours per week, which corresponds to a decrease in the child penalty by 22%. This is a very large change, especially in light of the current near-consensus in the literature finding public policies aiming at improving maternal labor supply mostly ineffective (Cortés and Pan, 2023; Olivetti and Petrongolo, 2017).²⁸

With Tighter Time Constraints, Mothers Favor Parenting Over Housework This large decrease of the child penalty in labor supply was accompanied by a decrease of the child penalty in housework, but no change in the time spent directly with the child. Panels (d) and (e) of Figure 3 shows that later cohorts of mothers experienced a smaller increase in time spent on housework relative to their earlier counterparts (+9 hours a week instead of +11 hours a week), but they spent exactly the same time playing with and caring for the child (+42 hours a week). This suggests that in their optimization, mothers who work more cut first on less valuable forms of home production (housework), thus mitigating concerns that working mothers invest less in their children.

This is consistent with analogous results found for the US by Bastian and Lochner (2022): better labor market opportunities translate into a greater fraction of time spent in paid work and a smaller fraction in home production. However, the first kind of home production that is substituted away is housework, which is presumably considered less important than parenting, time for which is not affected at all by the large increase in labor supply.

5.1.3 Threats to Validity and Robustness Checks

The interpretation of our results of this early versus late cohorts comparison as causal relies on the assumption that absent the Fair Work Act late cohorts of mothers would have behaved, after childbirth, in the same way as the early cohorts. We support this assumption

²⁸ Differently from other countries studied in the literature (e.g. Denmark in Kleven et al., 2019, Austria in Kleven et al., 2024, US in Cortés and Pan, 2023), in Australia there is no child penalty in hourly wages, neither before nor after the Fair Work Act. This is consistent with a labor market characterized by highly regulated wages, with a comparatively high national minimum wage, and awards that cover most employees and include tables with pay rates by detailed type of job (Creighton and Stewart, 2010, p. 352; Lass and Wooden, 2019). This is also not in contradiction with the changing composition of contracts: while permanent contracts could, in principle, be associated with higher “status” and higher wages within a job, casual workers are mandated by law to be paid with a 20-25% premium. This is called casual loading, and is part of the minimum wage legislation, summarized here <https://www.fairwork.gov.au/tools-and-resources/fact-sheets/minimum-workplace-entitlements/minimum-wages>. Thus, there is no reason to expect that the post-motherhood transition to casual contracts we observe at baseline, or the changes in this transition brought about by the Fair Work Act, would induce a mechanical increase or decrease in hourly wage.

in several ways, by discussing and addressing three sets of concerns. First, we show that women in early and late cohorts were similar on observables before becoming mothers, ruling out selection into childbearing as a concern. Second, we show that the change in the child penalty happened sharply around the 2009 cohort rather than being a slow-moving time trend. Finally, we rule out potential confounders such as Paid Parental Leave (women affected by this other reform are not driving our results) and other provisions in the Fair Work Act (which either turned out to be quantitatively irrelevant or did not affect parents differently relative to non-parents and are thus captured by our controls). These last sets of robustness checks are detailed in Appendices E and F.

First, we address selection into childbearing. This selection could drive our results if the women who had children after the Fair Work Act are systematically different from their earlier counterparts. As highlighted before, and as we discuss further in Appendix C, we find no systematic change in the fertility trend around 2009, nor do we find systematic differences in pre-birth observables between women who became mothers before and after the Fair Work Act. For example, both early and late cohorts of women have their first child at 29 years of age on average, 93% of both groups are in the labor force two years before childbirth, and in both groups, two years before childbirth, they work roughly 39 hours per week conditional on working (Appendix Table C.1). This supports the assumption that women becoming mothers before and after the Fair Work Act can be meaningfully compared.

Second, a simple secular trend in maternal labor supply may explain our results. If the child penalty did not change discontinuously around 2009 but rather slowly evolved, we cannot attribute the observed changes to the Fair Work Act. Section 5.1.4 discusses – and dismisses – this eventuality.

Third, something else may have happened around the same time to explain the discontinuous change in the child penalty we document. First, we address the hypothesis that the financial crisis might have increased labor supply of mothers through the added-worker effect. In Appendix E.1, we show that our results are unchanged, and if anything stronger, when we control for a woman's partner's earnings and labor supply. This is unsurprising, given that the financial crisis was mild in Australia (not even a formal recession; see Borland, 2011) and controlling for it does not affect our results on work arrangements.²⁹ Second, in Appendix E.2 we discuss other provisions of the Fair Work Act and argue that none of them differentially impacted mothers relative to other women, and thus do not violate our identification assumption, given that we always keep non-mothers as controls. Finally, we consider the introduction of federal Paid Parental Leave in Australia in 2010, which may in principle explain why mothers in the late cohorts have better labor market outcomes. We exploit the fact that approximately half of the women in our sample already had access to Paid Leave through their employer even before the 2010 provision, and we show that our results also hold in this group of women not affected (or affected less) by the introduction of employer-provided Paid Parental Leave. Appendix F describes this in detail.

²⁹Appendix C.3 shows robustness of the work arrangement results to accounting for the financial crisis.

5.1.4 It is Not a Time Trend

So far, we have shown that women who became mothers after the Fair Work Act experienced a systematically smaller child penalty in labor supply and housework relative to earlier counterparts. Here we confirm that this is driven by a sharp change in 2009 and not by a slow-moving time trend. We do so by estimating the child penalty separately by cohorts, where cohort is now defined by the year of birth of the first child: for all the outcomes of interest, this time series of child penalties is relatively flat before 2009, then shows a discrete jump in 2009, and continues being flat at the new level afterward.

Empirical Strategy More formally, we expand on Equation (3) by estimating a separate coefficient by cohort:

$$Y_{it} = \sum_c \left\{ \gamma_c \times \mathbb{D}_{it} \times \mathbb{1}\{c = cohort(i)\} \right\} + \alpha_i + \delta_t + \beta_{h(i)} + \epsilon_{it} \quad (4)$$

where \mathbb{D}_{it} is a dummy function for i 's first child being between 0 and 7 years old (as above) and $cohort(i)$ is the cohort i belongs to. We define cohorts based on pairs of years of childbirth to preserve statistical power. Here, γ_c is the average child penalty in the first seven years after the birth of the first child, for mothers belonging to cohort c . The sequence of these coefficients shows us the time evolution of the child penalty.

Results Figure 4 shows that the time series of the child penalties are relatively flat up to 2009, then display a discrete jump around the 2009 cohort, and finally remain flat at a higher level. In these graphs, the year of first childbirth is on the x-axis, and the corresponding γ_c , from estimating Equation (4), is on the y-axis.

This pattern of child penalties being stable before the 2009 cohort, displaying a jump exactly at 2009, and then remaining stable afterwards is common among both the type of contract and labor supply. Panel (a) shows that a woman who gave birth to her first child in 2003 or 2004 was 21 percentage points less likely to be on a permanent contract conditional on working after childbirth; and this number for a woman who had her first child in 2007 or 2008 was -25 percentage points. Instead, for a woman who became a mother in 2010 or 2011, the decrease in the probability of being in a permanent contract after childbirth was only 9 percentage points, and remained similarly small for women who became mothers in the following years.

This pattern looks similarly striking when the outcome is labor supply. On the intensive margin (panel b), the magnitude of the child penalty in working hours slightly increases, over time, for the birthing cohorts 2003 to 2008, before discontinuously decreasing after 2009. The figure shows that a woman experienced a drop in hours of work in the seven years after childbirth by 16 hours per week if her first child was born in 2003 or 2004 and by 17 if born between 2005 and 2008, but the child penalty in weekly hours of work was 14 hours per week or less for women who became mothers in 2010 or later. The extensive margin (panel c) shows a similar sharp change exactly around the 2009 cohort, in the direction of greater labor supply (smaller child penalty).

Home production is more nuanced, consistent with the pre-post analysis above. Panel (d) shows that the child penalty in housework displays a shift downward for post-2009

cohorts, but the shift is less pronounced than for labor supply. In panel (e), we can see that time spent parenting is pretty much constant in the years around the Fair Work Act.

5.2 Strategy 2: Exposure by Occupation and Industry

Despite the fact that the early versus late cohorts comparison presented in Section 5.1 is robust to a series of checks, our results might, in principle, still be driven by something systematically changing for mothers in 2009 we have not accounted for. Here we address this concern by adding a further level of variation: exposure to the law, determined by the characteristics of the job women held before childbirth. We find that indeed the entirety of the effect on contract type (lower likelihood to transition from permanent to casual contract after childbirth for post-Fair Work Act cohorts of mothers), labor supply (lower drop in labor supply), and housework (lower spike in housework) is concentrated among mothers in the most exposed jobs. Adding this level of variation also allows us to zoom into the household division of labor of the most exposed couples, and test for changes we might not detect at the aggregate level. We find that male partners of treated mothers do not pick up the slack in housework, and thus, while female share in paid work and household income increases substantially, female share in household home production does not change.³⁰

5.2.1 A Measure of Occupational Exposure

Our empirical strategy is based on the idea that different jobs are differentially exposed to the Fair Work Act and that the pre-2009 share of casual contracts in a job (“casual prevalence”) can efficiently summarize such exposure.

Intuition We rely on the fact that employers could refuse requests made under the Fair Work Act on “reasonable business grounds”. This implies that the reform did not affect some jobs, in which irregular and casual employment is likely inherent to the nature of the job (e.g. hospitality). Conversely, other jobs only offered permanent contracts even before the law (e.g. public administration), thus the Fair Work Act likely did not affect them either. This creates three categories of jobs: jobs with a high share of casual contracts were unaffected by the law for technological reasons, jobs with a very low share of casual contracts were unaffected because there was no scope for change in the direction of more permanent contracts (and more regular schedules), while an intermediate share of casual contracts indicates that there was scope for improvement, without technological reasons preventing it. Therefore, we conjecture that women in this intermediate group of jobs would be most exposed to the reform.

The Intuition in Practice We define a “job” as an occupation-by-industry cell, and we measure casual prevalence as the share of casual contracts within a job using pre-2009

³⁰Since housework represents a much smaller share than parenting in the total time spent in home production, a small decrease in housework for mothers does not translate into a significant change in female share of home production.

observations. 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 people within the same cell do sufficiently similar jobs, while retaining a sufficient sample size for accurately estimating the level of casual prevalence.³¹ Our measure of casual prevalence is the fraction of all worker-years in a given occupation-by-industry cell that report having a casual contract, restricting to observations prior to 2009. The resulting distribution of casual prevalence is displayed in Appendix Figure G.1. Our results are robust to measuring casual prevalence using observations only for men for the whole sample period (under the assumption that men are not treated by the law, both these measures should proxy for underlying job “technology”).

Terciles of Casual Prevalence We split the jobs (occupation-by-industry cells) in terciles of casual prevalence; Figure 5 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 occupations with the lowest casual prevalence, meaning the occupations characterized by the highest fraction of permanent jobs, are corporate managers and various types of professionals, and public administration is the industry in which the smallest fraction of employees are on a casual contract. At the other end of the spectrum, security guards, salespersons, and workers at hotels and restaurants are those most likely to be on a casual contract. In the middle tercile of casual prevalence, we find teaching professionals, office clerks, and life science professionals, working in education and health and social work. It is reasonable to imagine that full-time permanent teachers coexist with casual teachers called in if a colleague is sick, and similarly for nurses. These occupations and industries have both a large fraction of permanent workers and a sizeable fraction of casuals, meaning that there is both scope for the law to bring a change in the direction of more permanent contracts, and no technological constraints against it: these are the occupations and industries in which we expect the Fair Work Act to have the most bite.

The bottom and middle terciles of casual prevalence include jobs that are quite similar, and women who make similar fertility choices, while the top tercile is fairly different: this is why we will mostly focus on the bottom-versus-middle comparison. In Appendix G.2 we discuss the similarity and differences between the terciles. The main takeaway is that jobs in the bottom and middle terciles are similar in terms of weekly hours of work, pay, and educational requirements, while for the top tercile these figures are much lower. Women in these jobs tend to make similar choices in terms of fertility too: women in jobs in the bottom and in the middle tercile of casual prevalence have their first child at 32 and 31 respectively, around three years later relative to women with jobs in the top tercile of the casual prevalence distribution; they also have similar realized fertility three, five, and seven years after the birth of their first child. This is why, in our empirical analysis, we will emphasize the comparison between the bottom and middle tercile, as the bottom tercile provides a more credible counterfactual for the middle tercile than the top.

³¹This is also a practical choice, since 2-digit is the highest level of granularity afforded by the general release of the dataset.

5.2.2 Empirical Strategy

We group mothers by exposure to the Fair Work Act depending on the casual prevalence of the job they held before childbirth, and we test whether the child penalty evolved differently after 2009 for more and less exposed groups of women.

We split women in terciles of casual prevalence and we carry out three related exercises. Each woman is assigned the tercile of casual prevalence of the modal job she held in the five years prior to childbirth.³² First, we replicate the early versus late cohort comparison of Section 5.1 separately by tercile, to show that the observed changes are concentrated in the middle (most exposed) tercile. Second, we estimate the time series of the child penalty separately by tercile, to show parallel pre-trends in the child penalty between more and less exposed terciles. This is carried out in Appendix I.1. Third, we estimate the relative difference-in-differences for the child penalties (triple-difference), as explained below.

Estimating the difference-in-differences for the child penalties 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). In practice, we augment Equation (3) by including the interaction with indicators for terciles:

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

where $\mathbb{D}_{it} = \mathbb{1}\{(t - E_i) \in [0, 7]\}$ is an indicator function that takes value of one if i 's first child is aged 0 to 7; the dummy $\text{Post}(i)$ indicates that i 's first child was born on or after 2010 (and hence i belongs to the late cohorts, using the terminology from previous sections); and the bottom line specifies that time and age fixed effects are estimated separately by tercile. Here γ_a is the baseline child penalty for the bottom tercile of casual prevalence, meaning that it is the difference in Y in the seven years following the birth of their first child relative to the four years before, for individuals in the bottom tercile of casual prevalence, which we take as reference group. $\gamma_{a,2}$ is the baseline difference in the child penalty for the treated group (middle tercile) relative to the reference group (bottom tercile), and $\gamma_{a,3}$ is analogous for the placebo group (top tercile); these baselines are estimated off the early cohorts, meaning mothers who had their first child between 2003 and 2008. The coefficient γ_b is the difference in 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

³²We do the same for the controls (non-mothers and mothers who enter the sample with old children) based on a randomly assigned fake childbirth year.

mothers in the treated group, relative to the same difference for the reference group. $\gamma_{b,3}$ is the difference-in-differences coefficient for the placebo group.

5.2.3 Results for Mothers

We confirm that the aggregate results presented earlier are concentrated among mothers in the jobs most exposed to the Fair Work Act, supporting the hypothesis that the greater likelihood of being on a permanent contract and on a regular schedule brought by the Fair Work Act was responsible for the observed increase in maternal labor supply (decrease in the child penalty).

Figure 6 replicates Figure 3 (early versus late cohort comparison) but split by terciles of exposure. As before, the estimates for the early cohorts of mothers (that is, women who became mothers between 2005 and 2008, before the Fair Work Act) are represented by circles connected by dashed lines, while the ones for the late cohorts of mothers (women whose first childbirth was between 2010 and 2013) are represented by squares connected by solid lines. The panels on the left, in blue, are estimated on women with pre-birth jobs in the bottom tercile of casual prevalence: these women were in jobs where it was already the norm for everyone to be on a permanent contract, and thus were likely not affected by the Fair Work Act. The panels in the middle, in orange, are estimated on women with pre-birth jobs in the middle tercile of casual prevalence: in these jobs, not everyone was already on permanent contracts – and thus there was scope for improvement, but also not everyone was on casual contracts, thus suggesting the absence of “technological” constraints against permanent contracts. Finally, the panels on the right, in grey, are estimated on mothers with pre-birth jobs in the top tercile of casual prevalence: here, casual contracts are presumably part of the “technology” of the job, and it’s likely that employers can refuse requests made under the Fair Work Act on “reasonable business grounds”: we do not expect this group to be impacted by the Fair Work Act as much as the middle group (if at all).

Probability of Transitioning Out of a Permanent Contract Upon Childbirth: from 37 Percentage Points to Zero for Most Exposed Mothers Women with pre-birth jobs in the middle tercile of “casual prevalence” experienced a stark decrease in the probability of transitioning to a casual contract after childbirth after the Fair Work Act, while women with jobs in the other two terciles did not. Panel (a) of Figure 6 shows the child penalty in the probability of being on a permanent contract, conditional on working. For women with jobs in the bottom tercile of casual prevalence (blue, on the left), relatively few women transitioned from permanent to casual contract after childbirth at baseline, before the Fair Work Act (9 percentage points); this number is virtually zero for post-Fair Work Act cohorts of mothers, and the difference between cohorts is not statistically significant (11 p.p., with a standard error of 7 p.p.). Mothers in the top tercile (grey, on the right) experienced a similar decrease in the probability of being in a permanent contract both before and after the Fair Work Act. Women with pre-birth jobs in the middle tercile of casual prevalence – the most exposed – (orange, in the middle) are the ones for which we observe the sharpest change: while the early cohorts experienced a massive drop in

the probability of remaining in a permanent contract after childbirth (-37 p.p.), the late cohorts in this group experienced no change in the probability of remaining in a permanent contract after childbirth. The difference is large and significant at the 0.1% level (40 p.p., S.E.: 8 p.p.).

Exposure to Fair Work Act Increased Labor Supply and Decreased Housework, But Not Parenting The large change in maternal labor supply (decrease in child penalty) between pre- and post-Fair Work Act cohorts of mothers is fully concentrated in the middle tercile, as is the change in housework; parenting does not change for any group. By looking at Figure 6, it is evident that the middle tercile (in orange), which experienced the largest relative increase in the probability of being on a permanent contract (panel a), is also the same group that experienced the largest relative increase in labor supply (panel b, +11 hours per week, with a standard error of 2.6 hours/week) and decrease in housework (panel d, -5.5 hours per week, S.E.: 2 hours/week). Parenting (panel c) is unchanged even in this group (+1.5 hours per week, S.E.: 3 hours per week), despite the massive increase in labor supply; again confirming that mothers exposed to the Fair Work Act did not increase their labor supply at the expense of time spent directly with the child.

The triple-difference coefficient estimates are consistent with the patterns visualized in the graphs. The coefficients of interest ($\gamma_{b,2}$) from running Specification (5), namely those on the interaction between post-childbirth (“Child Penalty 0-7”), post-Fair Work Act cohort (“Post-2009 First Birth”), and pre-birth job in middle tercile of casual prevalence (Tercile = 2), are highlighted in blue in Table 4. They convey the same message as Figure 6; numbers can be slightly different because this specification uses all available cohorts (birthing cohorts from 2003 to 2017, excluding 2009) as opposed to focusing only on the cohorts closest to the Fair Work Act.³³

Using this more succinct specification, we show that the conditional result on permanent contracts is not driven by selective exit. If it was driven by a systematic exit of mothers with a greater propensity to move to casual contracts, the *unconditional* probability of holding a permanent contract would not change for treated mothers, given that both exits and casual contracts are coded as zero in the unconditional variable. Instead, the triple-difference coefficient of interest for the unconditional probability of having a permanent contract (column 2 of Table 4) is slightly larger than the corresponding coefficient on the conditional probability (column 4), thus ruling out selection through exit as the driver of the observed increase in the conditional probability of having a permanent contract (decrease in the child penalty).

Finally, we show that the increase in the probability of being on a permanent contract and the increase in the probability of working on a regular schedule are basically overlapping for the most exposed mothers. In column (4) of Table 4, the outcome is a dummy variable for having a permanent contract, conditional on working, while in column (6) the outcome is a dummy for working a regular schedule, also conditional on working. The triple-difference coefficients, highlighted in blue, are virtually identical.

³³In the event study graphs we restrict to cohorts 2005-2008 and 2010-2013 in order to have a balanced panel for both early and late cohorts of parents.

5.2.4 Labor Market Outcomes For Mothers: Consequences and Mechanisms

After showing that the aggregate results presented in Section 5.1 are indeed driven by the mothers most exposed to the Fair Work Act, here we dig further into the labor market consequences of this new time allocation – which are mostly mechanical, with no statistically significant impact on career progression (though noisily pointing to a positive effect). We also look into some of the mechanisms, establishing that the compliers are NOT mothers that would have counterfactually worked full-time (and thus would have been induced by the law to reduce hours) and ruling out the explanation that exposed mothers are simply closer to their desired level of hours after the reform.

Exposure to the Fair Work Act Increased Employment and Hours, But Not Hourly Wages

The increase in labor supply (decrease in the child penalty) for the mothers most exposed to the Fair Work Act came both from the extensive and intensive margin, and was not accompanied by a change in hourly wages, thus translating into a large increase in annual labor earnings. Table 5 reports the coefficient estimates from running the triple-difference specification (5) on labor market outcomes; again, the coefficients of interest ($\gamma_{b,2}$) are highlighted in blue. These estimates show that the large increase in unconditional hours of work for the most exposed mothers in the middle tercile documented above came both from the extensive margin (+11 percentage points, column 2) and from the intensive margin (+3.7 hours per week, column 4). This increase in labor supply was not accompanied by a change in hourly wages - the coefficient of interest in column (6), albeit positive, is very small and not statistically different from zero. As discussed extensively in footnote 28, this is consistent with the high degree of wage regulation in the Australian labor market. Thus, the high and persistent increase in labor supply translates into a large and persistent increase in earnings (+14,890 2021 AUD per year, column 8), corresponding to a 41% smaller child penalty in annual labor earnings for the mothers in pre-birth jobs most exposed to the Fair Work Act.

Exposure to the Fair Work Act Has Noisily Positive, But Not Statistically Significant, Effects on Career Progression

The large increase in labor supply of exposed mothers translates into a suggestive noisy improvement in career progression, though the aggregate numbers are not significantly different from zero. In Appendix H.1, we study the impact of the Fair Work Act on the probability of being promoted and on the probability of having a supervisory role. For both variables, the average child penalty in the seven years following the birth of their first child is not statistically different for most exposed mothers (Tercile 2) in post-Fair Work Act cohorts relative to earlier cohorts and to less exposed mothers (Appendix Table H.1). On the other hand, Appendix Figure H.1 shows that for the mothers with pre-birth jobs most exposed to the Fair Work Act there is a significant increase in the probability of being promoted in the year following the first childbirth. In addition, it shows that, for the exposed group (middle tercile), the progressive shifting away from supervisory roles after childbirth evident in the early (pre-Fair Work Act) cohorts is no longer happening for the post-Fair Work Act cohorts. While the evidence from the event-studies is suggestive, it is extremely noisy and in both cases only significant in a single

relative-event year, thus we are hesitant to draw strong conclusions.

The Compliers Would Have Not Worked Full-Time, And The Gap Between Actual and Desired Hours Does Not Close In Appendix H.2 we address and rule out two concerns: (i) that mothers that would have counterfactually remained full-time are driven by the Fair Work Act into working part-time; and (ii) that the Fair Work Act “simply” allowed mothers to choose a number of work hours closer to their optimum. In principle, a reform that makes less-than-full-time hours less costly (interpreting irregularity as a cost) might also entice women who would have worked full-time to work part-time. In fact, we do not see it happening: there is no decrease in the probability of working full-time; if anything, the point estimates point to the opposite direction (see Appendix Table H.2 and Appendix Figure H.1). Another possible concern is that casual contracts offer suboptimally low hours, and the new reduced-hours permanent contracts simply offer hours that are closer to the optimal number of part-time hours mothers desire. In fact, the gap between actual and desired hours does not close for the exposed group (the point estimates, though insignificant, go in the opposite direction; see Appendix Table H.2). Intuitively, when the only contract available is a casual contract with irregular hours, mothers *want* fewer hours, because each hour is more costly with an irregular schedule. With the regular schedule granted by permanent contracts, mothers want (and get) more hours. So we can rule out the explanation that the permanent contracts increase hours simply by getting mothers closer to their desired level of hours.

5.2.5 Results for Couples and Intra-household Time Allocation

We do not find any evidence that increased labor supply and earnings of mothers impact how the time burden of children is shared among partners, or that men’s time use is impacted at all.

In what follows, treatment status for men is defined as their female partner’s treatment status, and treatment status for a couple is defined as the woman’s treatment status. That is, men (and couples) in “bottom tercile” are partners of women with pre-birth jobs in the bottom tercile of casual prevalence, and analogously for the other two groups, regardless of their own job.

(Lack of) Changes in Men’s Time Use Partners of treated mothers don’t change their labor supply, nor their time spent in home production, despite the large changes in their female partner’s time use. As we can see from panel (a) of Figure 7, men do not change their labor supply around childbirth, and this is true for all groups and cohorts. Panel (c) shows that the same is true for housework, except for a small and not significant increase in housework for the late cohorts of fathers in the middle tercile. In panel (b) we see a small increase in parenting for partners of most exposed mothers (middle panel, in orange), but the increase is not statistically significant and does not significantly alter how home production is shared between the partners, to which we turn next. Consistently, the triple-difference coefficient estimates reported in blue in Table 6 (columns 2, 4, and 6) show no significant change in time use for the partners of most exposed mothers.

Women Contribute More to Household Income But No Less to Home Production

Most exposed mothers, who increase their labor supply (thanks to greater access to regular schedules and permanent contracts granted by the Fair Work Act) increase their contribution to total household income, but this does not lead to a change in the relative distribution of home production. Panel (a) of Figure 8 displays the child penalty in “Female Share of Household Income”, computed as the fraction of total household annual labor income earned by the woman. For all groups and for all cohorts we observe a decrease in the female contribution to total household income after childbirth, consistent with the post-childbirth decrease in labor supply for women but not for men documented previously. However, for the middle tercile (most exposed to the Fair Work Act), the late cohorts experience a drop in this measure half the size that of their earlier counterparts (while we don’t observe similarly sharp differences between the two cohorts for women in bottom and top terciles). This is a direct consequence of the fact that most exposed mothers increased their contribution to the total labor supply of the household, as shown in panel (b).³⁴ Despite the substantial increase in the female share of household income and of the female share in paid work for most exposed mothers, we do not observe a significant decrease in the female share of home production (panel c). Since housework represents a small share of home production when there is a young child to take care of, the decrease in women’s time spent in housework is compatible with the lack of change in the relative distribution of total time in home production.

This lack of reallocation of home production among partners, despite a substantial increase in the female share of household income, suggests that women’s labor market opportunities are not the main reason we observe such an unbalanced allocation of the time burden of children. In other words, task allocation within the household is not simply a function of relative labor market earnings. The reason for the observed unequal division of home production is to be looked for elsewhere, possibly in gender norms.

5.2.6 Threats to Validity and Robustness Checks

Our job exposure strategy relies on a triple-difference version of the parallel trend assumption: that the *child penalties* of mothers in the treated tercile would have evolved, over time, similarly to the ones of the other two groups. We support this assumption in several ways. First, we show parallel pre-trends in child penalties across terciles. Second, we show no selection into childbirth: early and late cohorts of mothers in the three groups are similar in terms of observables in the pre-birth years, and fertility trends are similar across terciles. Third, we rule out selection into occupation by showing that our results are robust to assigning women the exposure of the job they had before the Fair Work Act. Fourth, we show that fertility is not affected and that possible differential fertility timing is not driving the results. We discuss these four sets of checks in detail in Appendix I.

In the first check, carried out in Appendix I.1, we show that the child penalties in the outcomes of interest (probability of being on a permanent contract, labor supply, house-

³⁴Here the outcome variable is the fraction of female time spent in paid work over total household time spent in paid work: $\frac{\text{Woman's weekly hours in paid work}}{\text{Woman's weekly hours in paid work} + \text{Man's weekly hours in paid work}}$.

work) for the bottom and middle terciles were evolving in parallel before the 2009 cohort. We do this by displaying the evolution over cohorts of the child penalty in our main outcomes of interest, separately for the three terciles of casual prevalence to whom mothers belong. Namely, we replicate Figure 4 separately for the three terciles of casual prevalence. The figures clearly show that the child penalties for the bottom and middle terciles were evolving in parallel, though on different levels, before 2009, with the middle tercile displaying bigger penalties. They also show that there was a sharp change in 2009 for the middle tercile and that after 2009, the child penalties for the bottom and middle tercile continued to evolve in parallel, but much closer to each other, or, in certain cases, on the same level.

The second set of checks shows that will-be mothers are similar on pre-birth observables in each tercile. In Appendix I.2 we replicate the balance table in pre-birth characteristics between early and later cohorts of mothers (Appendix Table C.1) separately by terciles. In all three terciles, will-be mothers whose first child was born between 2003 and 2008 are similar in pre-birth characteristics such as hours worked, hourly pay, and female share of household labor income to will-be mothers whose first child was born between 2010 and 2017. This lends support to the assumption that earlier cohorts of mothers in similar pre-birth jobs are a good counterfactual for later cohorts of mothers.

Third, we show that selection into occupation is not a concern in this setting. If more career-oriented women endogenously switched to jobs in the middle tercile, for example, this would be an interesting story but would change the interpretation of our results. We rule this out with two observations. First, most of our analysis focuses on a short time span (four-year childbirth cohorts around 2009), and it's unlikely that workers in their late twenties would make a dramatic occupation switch in this small window. Second, in Appendix I.3, we show formally that our results are unchanged when defining exposure on jobs that were chosen before the Fair Work Act: we limit the sample to two-year cohorts around the Fair Work Act and assign women the exposure of the job they had three years before childbirth (thus the job they had before the Fair Work Act); our results are the same as with the primary strategy.

Finally, in Appendix I.4, we show both that fertility was not affected by the Fair Work Act and that differential fertility was not the channel that mediated the estimated effect. Running our triple-difference specification on measures of birth/adoption and of completed fertility, we show that mothers exposed to the Fair Work Act did not display a systematically different fertility pattern relative to less exposed mothers. Consistently, our main results replicate very closely on the sample of mothers with a completed fertility of one, although the estimates are noisier due to the smaller sample size. Lastly, we discuss how women with pre-birth jobs in the bottom and in the middle tercile of casual prevalence (our preferred comparison) are very similar in terms of education level, age at first birth, and fertility patterns (and this does not change systematically with the law), thus indicating that the issues of differential age at first birth and differential birth spacing by education raised by Adams et al. (2024) for general estimation of child penalties are not a concern in this context.

6 Conclusion

This paper studies the role of work arrangements in explaining the child penalty in labor supply, defined as the large drop in working hours that women face when becoming mothers. Leveraging the variation introduced by the Fair Work Act, which entitled parents of young children to request a change in their work arrangements, we establish three sets of facts. First, the Fair Work Act was used by new mothers to reduce their weekly working hours without renouncing their permanent contract, hence maintaining a regular schedule. In the institutional context in place in Australia at the time of the reform, permanent contracts provided the most predictable hours, but such contracts were almost exclusively available for full-time jobs. The shift introduced by the Fair Work Act allowed mothers to work under permanent contracts while reducing their working hours, and therefore enjoying the predictability of a regular schedule while being able to work less-than-full-time hours.

The second part of our analysis documents that this shift led to a decrease in the child penalty in working hours. Our interpretation is that, when given the option to work the regular schedule afforded by a permanent contract, mothers were able to work longer hours than what they would have done with the irregular schedule associated with a casual contract. Using our measure of occupational exposure to the Fair Work Act as a further source of identification, we find that the most exposed women, for whom the probability of staying in a permanent contract after childbirth increased by 50%, are also the group for whom the child penalty in the hours of work dropped by more than 30%.

The final set of results we present discusses the effect on intra-household repartition of tasks of the above-mentioned improvement: while the changes in mothers' working hours imply a shift towards equality in the male- and female-shares of household income, we do not observe any changes in the female disproportionate share of home production.

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: most notably, women switch out of their permanent contract at the time of childbirth, in order to be able to reduce their labor supply on the intensive margin. 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, one would expect that changing labor supply around childbirth would be part of an overall rearrangement of tasks within the household. We have argued that this is not the case, and that improving women's share of household income does not map into a more equal distribution of home production. Exploring why the reallocation did not happen and understanding what in certain cases prevents labor market conditions from impacting the intra-household allocation of tasks seems like a fruitful avenue for future research.

Disclaimers

This paper uses unit record data from Household, Income and Labour Dynamics in Australia Survey [HILDA] conducted by the Australian Government Department of Social Services (DSS). The findings and views reported in this paper, however, are those of the author[s] and should not be attributed to the Australian Government, DSS, or any of DSS' contractors or partners. DOI: 10.26193/3QRFMZ.

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.

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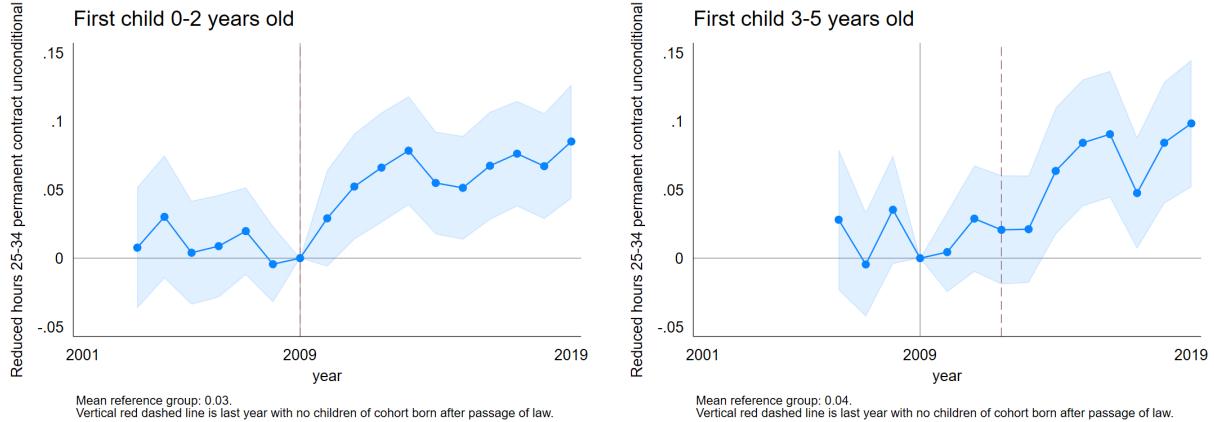
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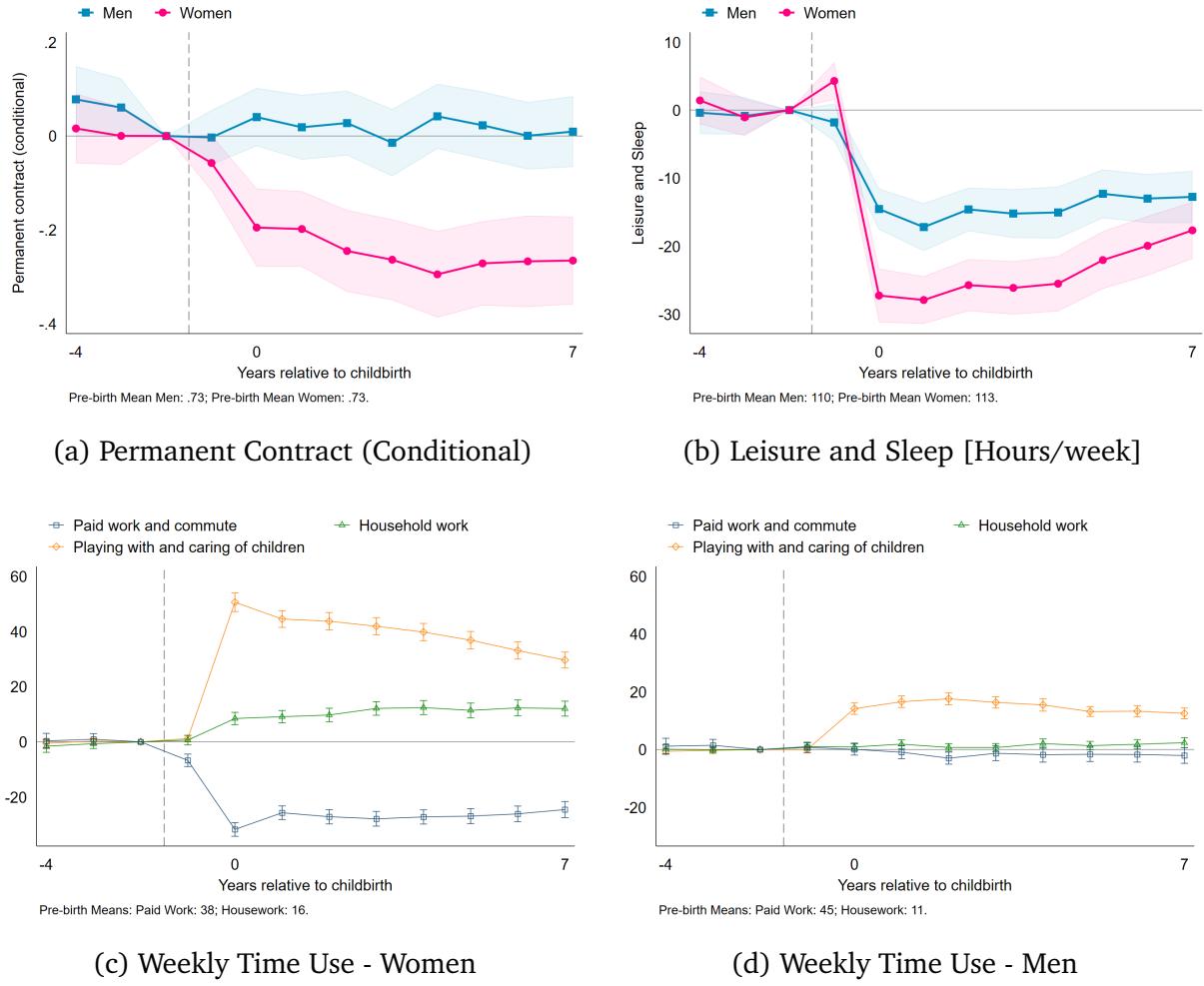
7 Figures

Figure 1: Reduced-hours Permanent Contract for Mothers Relative to Non-Mothers, by Year and Age of Child



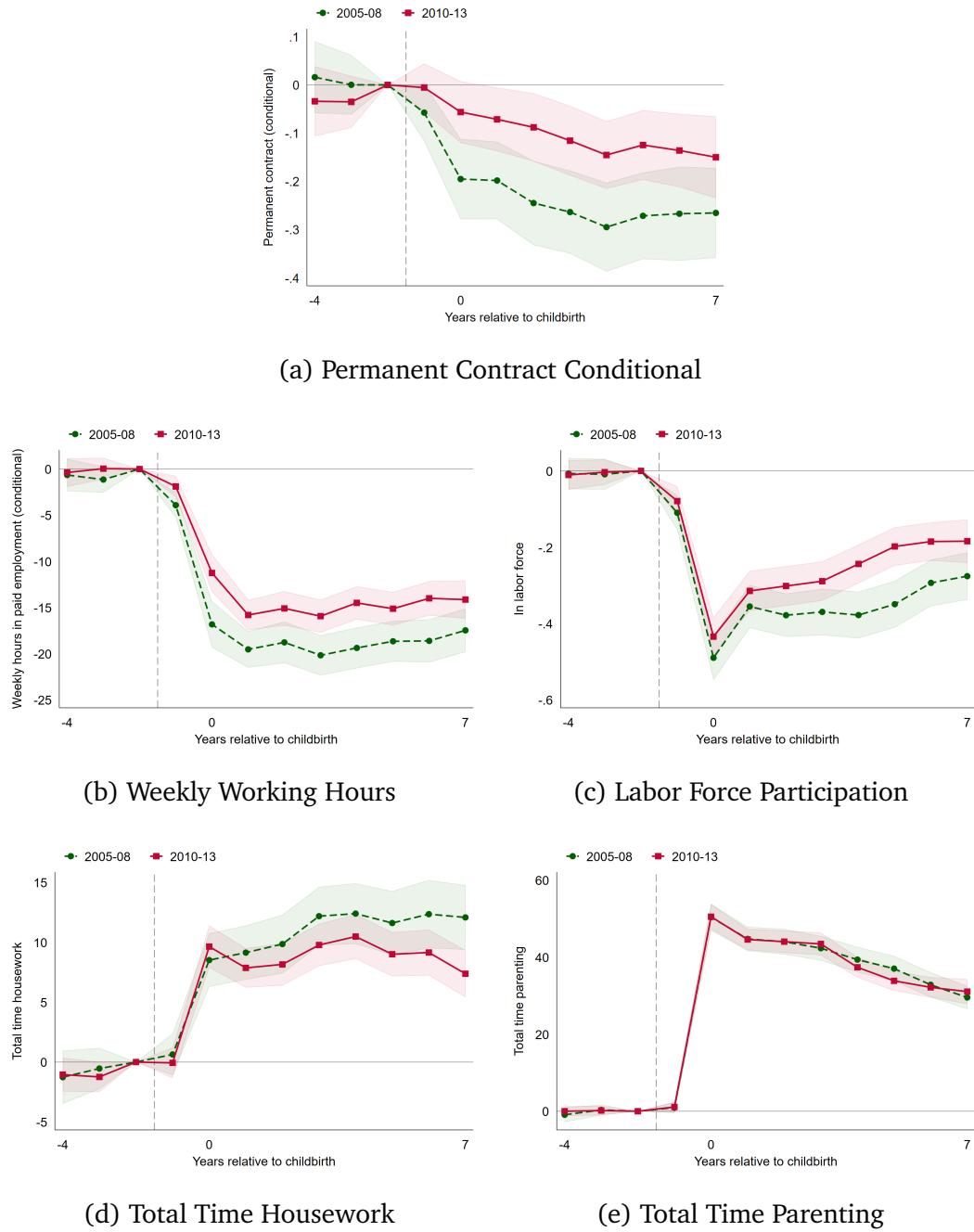
Note: This figure shows the change in the unconditional probability of having a reduced-hours permanent contract for mothers relative to non-mothers over time. More formally, we are plotting the estimates of $\beta_{a,j}$ from Equation (1), where the outcome variable is reduced-hours permanent contract, i.e. a dummy for working between 25 and 34 hours per week under a permanent contract (unconditional). The left panel displays the estimates for the coefficients on the dummy of the first child being aged 0-2 interacted with calendar years, the right panel 3-5. 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). Standard errors are clustered at the individual level.

Figure 2: Child Penalties Before the Fair Work Act



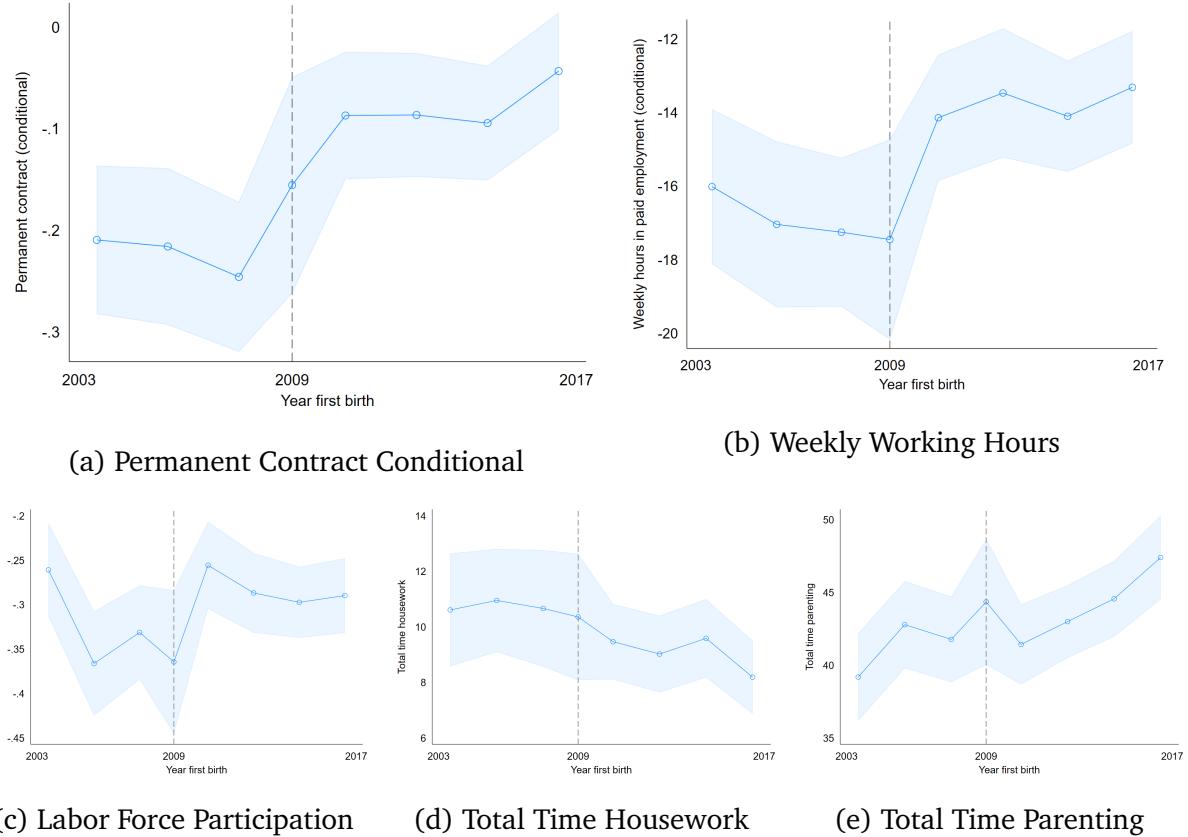
Note: This figure shows the child penalty in the probability of having a permanent contract conditional on working (panel a), in leisure and sleep (panel b), and in different dimensions of time use (panels c and d). Namely, the figure plots the coefficients γ_k 's from estimating Equation (2), which includes individual, time, and age fixed effects. In panels (a) and (b) the blue squares are the estimates for men, the pink circles are the estimates for women, and the shades around them are 95% confidence intervals; the outcomes are, respectively, a dummy for being on a permanent contract conditional on working (panel a) and the number of weekly hours spent in leisure and sleep (panel b). Panels c and d plot the estimates for different dimensions of time use, separately for women (panel c) and men (panel d). The blue squares are estimates for the outcome variable “weekly hours spent in paid work and commuting”, the green triangles for weekly hours spent in housework, and the yellow diamonds for weekly hours spent playing with or caring for the child. We only include parents who had their first child between 2005 and 2008, and we include non-parents and parents of older children as controls - see Section 4.1 for details. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level.

Figure 3: Change in the Child Penalty for Women: Early versus Late Cohorts (Pre- versus Post-Fair Work Act)



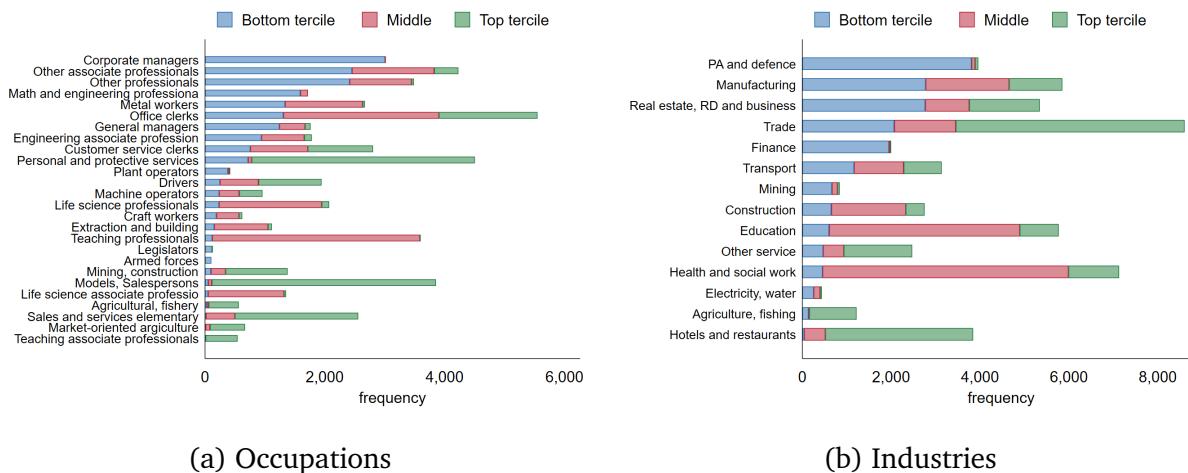
Note: This figure shows how the child penalty in permanent contract and time use for women has changed after the passage of the Fair Work Act. In panel (a) the outcome is a dummy for having a permanent contract conditional on working; in panel (b) the outcome is weekly hours in paid employment conditional on working; in panel (c) the outcome is a dummy for whether the individual is in the labor force; in panel (d) the outcome is weekly hours spent doing housework, and in panel (e) weekly hours spent playing with and taking care of the child. All panels plot the child penalty 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 connected by dashed lines) and late cohorts (first childbirth between 2010 and 2013, red squares connected by solid lines). Women without children and with older children are included in both regressions as controls, and women having children in other sample years are excluded from both regression. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level.

Figure 4: Time Evolution of the Child Penalty for Women



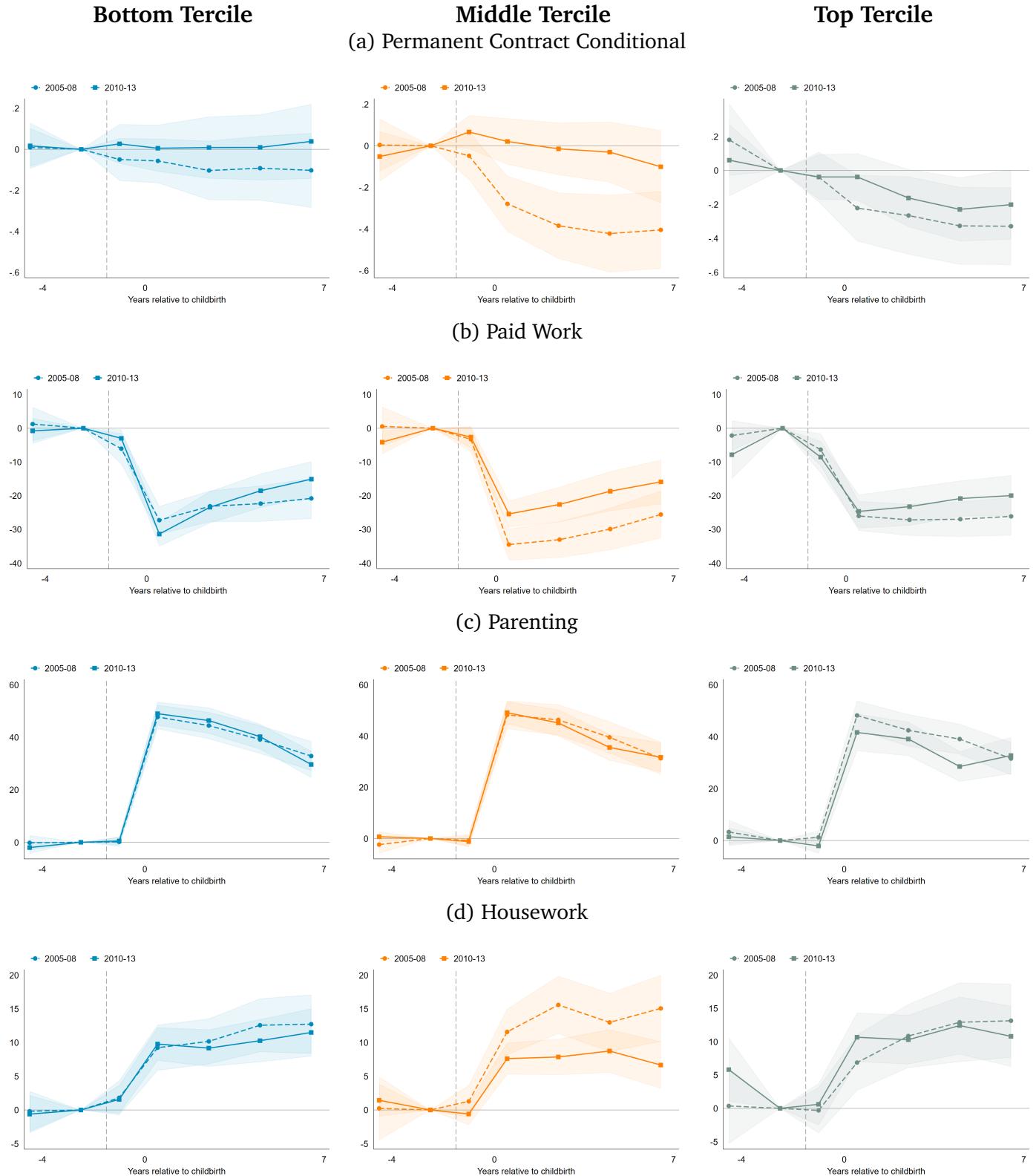
Note: This figure shows how the child penalty in permanent contract and time use for women has changed over the sample period. All panels plot the estimates of γ_c from Equation (4); that is, they display the average of the child penalty from zero to seven years after the birth of their first child for women (on the y-axis) by year of birth of their first child (on the x-axis). The blue circles are the estimates for each biyearly cohort, and the shades around them are 95% confidence intervals. We cluster standard errors at the individual level.

Figure 5: Exposure to the Fair Work Act: Occupations and Industries by Terciles of Casual Prevalence



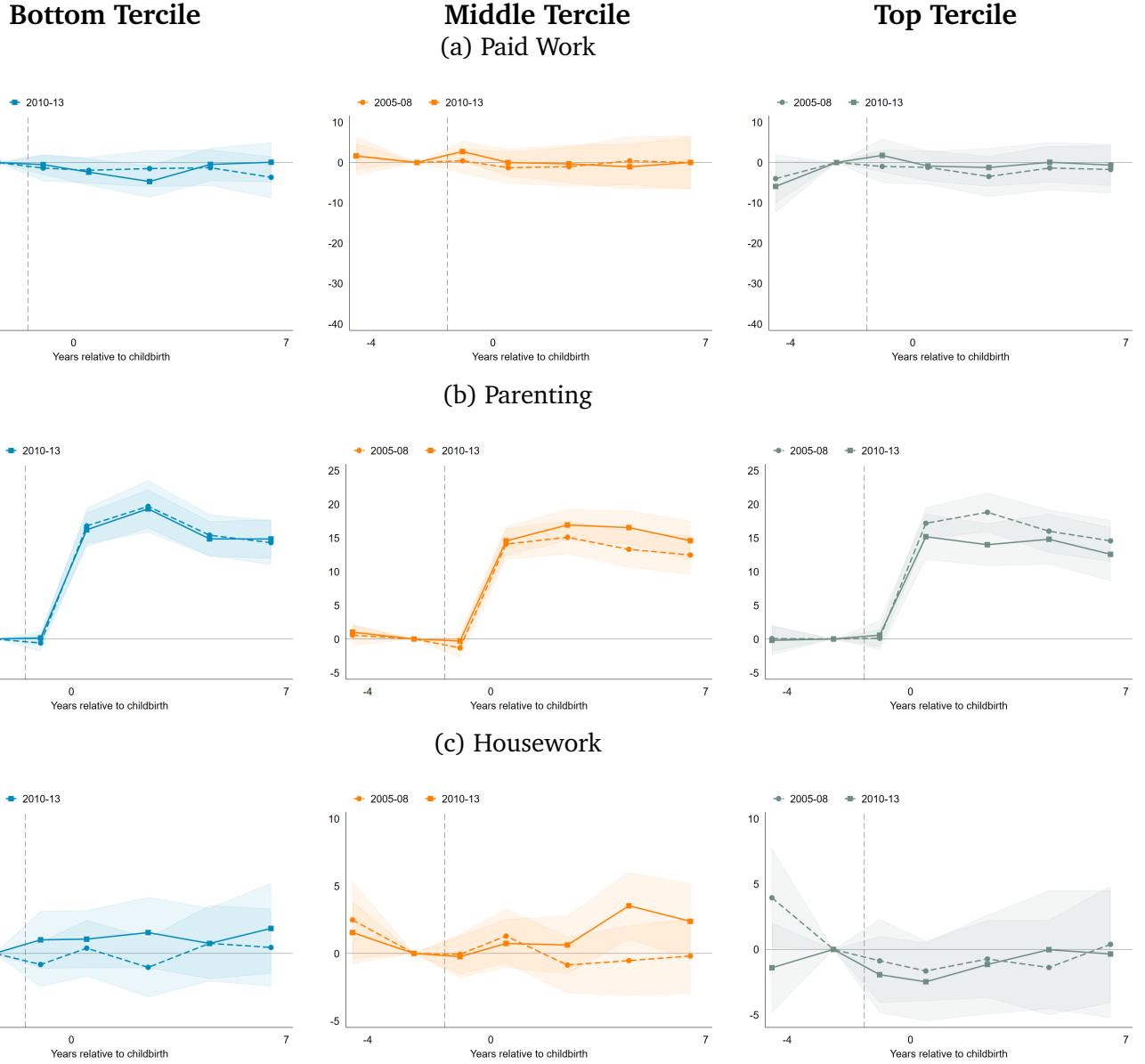
Note: For each occupation (left panel) and industry (right panel) in our sample, this figure plots the frequency of observations by terciles of casual prevalence. Since casual prevalence (share of workers with a casual contract) is defined at the industry-by-occupation level, each industry and each occupation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

Figure 6: Contract Type and Time Use of Women around Childbirth, Before and After Reform, by Exposure to the Fair Work Act



Note: This figure shows the child penalty in permanent contract and time use for women by early and late cohorts and by terciles of casual prevalence. The leftmost panels restrict the sample to women whose modal job in the five years before their first childbirth was in the bottom tercile of casual prevalence (jobs with the highest share of permanent contracts); the middle panels to women whose modal job in the five years before their first childbirth was in the middle tercile of casual prevalence (the most exposed to the Fair Work Act); the rightmost panels to women whose modal job in the five years before their first childbirth was in the top tercile of casual prevalence (jobs with the highest share of casual contracts) - see text for details. Further details on estimation in note to Figure 7.

Figure 7: Time Use of Men around Childbirth, Before and After Reform, by their Partner's Exposure to the Fair Work Act

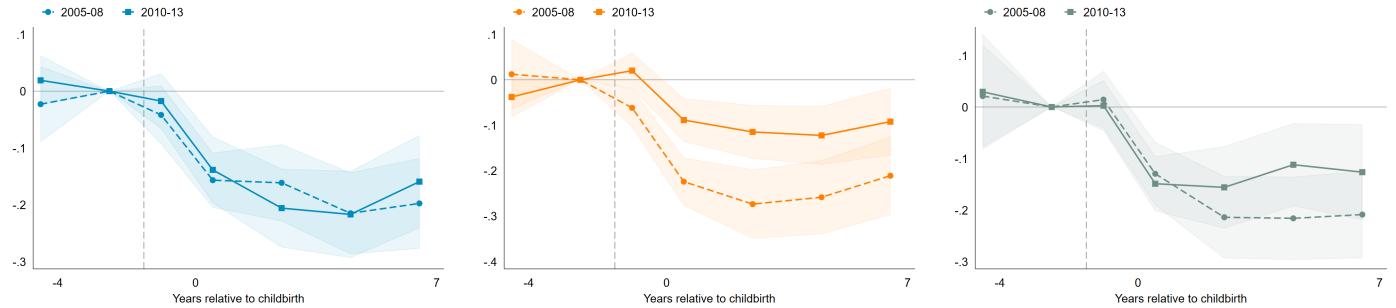


Note: This figure shows the child penalty in time use for men by early and late cohorts and by their female partner's terciles of casual prevalence; that is, it replicates the time use part of Figure 6 for men, but based on their female partner's treatment status. In each panel, we report estimates of γ_k 's from estimating a version of Equation (2) that pools adjacent years in pairs for power (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 weekly hours spent in paid work and commute; in the second row, the outcome is weekly hours spent in household work (housework and errands); in the bottom row, the outcome is weekly hours spent parenting (playing with and caring for the child). Within each panel, the circles connected via dashed lines are estimates restricting the sample to male partners of women who had their first child between 2005 and 2008, while the squares connected via solid lines are coefficient estimates restricting the sample to male partners of women whose first child was born between 2010 and 2013. 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. Further details in note to Figure 6.

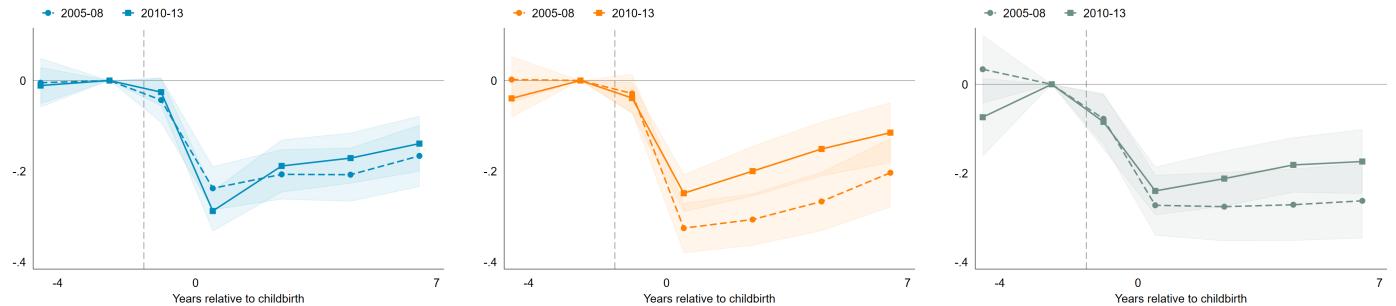
Figure 8: Female Share of Household Income, Working Time, and Home Production around Childbirth, Before and After Reform, by Exposure to the Fair Work Act

Bottom Tercile

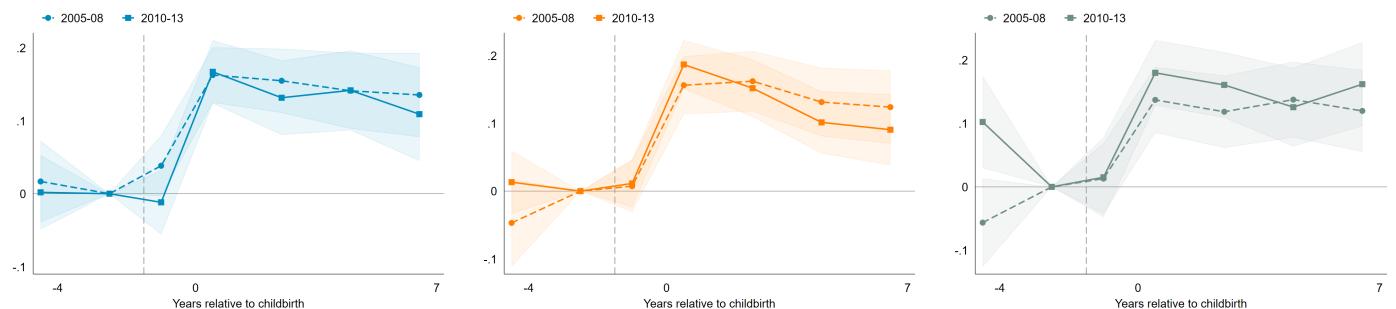
Middle Tercile
(a) Female Share of Household Income



(b) Female Share of Household Time in Paid Work



(c) Female Share of Household Time in Home Production



Note: This figure shows the child penalty in female share of household income, female share in total household time spent in paid work, and female share in total household time spent in home production (sum of housework and parenting) by early and late cohorts and by terciles of casual prevalence. The treatment status of the household is based on the woman's treatment status. See notes to Figures 6 and 7 for details.

8 Tables

Table 1: Descriptive Statistics

	N	Mean	SD	P1	P99
Women					
Age	132421	38.8	14.3	15	65
In labor force	126654	0.72	0.45	0	1
Employed	126654	0.68	0.47	0	1
Weekly hours in paid employment (conditional)	85734	31.4	14.2	3	67
Annual labor income, 2021 AUD (thousands)	132421	35.0	42.1	0	162.9
Hourly wage, 2021 AUD	85734	31.3	32.3	0	99.6
Total time work	106033	23.8	20.6	0	70
Total time housework	106282	21.4	17.3	0	80
Total time parenting	106138	9.61	18.8	0	84
Total time home production	106314	31.0	28.6	0	121
Total time leisure and sleep	104962	109.8	31.2	28	166
Regular schedule (M-F and regular daytime schedule)	85907	0.42	0.49	0	1
On call and irregular shifts	85933	0.10	0.30	0	1
My working times can be flexible (yes-no)	63065	0.49	0.50	0	1
Hours work from home	91716	2.31	7.15	0	38
Any hours worked from home	91716	0.21	0.41	0	1
Permanent contract (conditional)	76380	0.63	0.48	0	1
Casual contract (conditional)	76380	0.26	0.44	0	1
Fixed-term contract (conditional)	76380	0.10	0.30	0	1
Reduced hours (25-34) permanent contract unconditional	126654	0.064	0.24	0	1
Has bachelor degree or above	126618	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
Employed	116100	0.79	0.41	0	1
Weekly hours in paid employment (conditional)	91809	41.7	14.4	5	80
Annual labor income, 2021 AUD (thousands)	125761	60.4	67.4	0	286.2
Hourly wage, 2021 AUD	91809	33.3	28.3	0	116.5
Total time work	93825	37.2	22.7	0	84
Total time housework	94047	13.6	12.0	0	60
Total time parenting	94013	4.60	9.28	0	40
Total time home production	94065	18.2	16.5	0	78
Total time leisure and sleep	93204	110.6	28.5	43	166.5
Regular schedule (M-F and regular daytime schedule)	91966	0.53	0.50	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
Hours work from home	101659	2.48	7.94	0	42
Any hours worked from home	101659	0.22	0.41	0	1
Permanent contract (conditional)	74624	0.72	0.45	0	1
Casual contract (conditional)	74624	0.19	0.39	0	1
Fixed-term contract (conditional)	74624	0.092	0.29	0	1
Reduced hours (25-34) permanent contract unconditional	116100	0.014	0.12	0	1
Has bachelor degree or above	116056	0.22	0.41	0	1

Note: The table shows descriptive statistics (number of observations, mean, standard deviation, 1st and 99th percentile) for the variables in the analysis from the full sample, separately by gender. The dataset covers the years 2001-2019. We restrict the sample to people aged 15 to 65. All variables are defined in Appendix A.

Table 2: Changes in Work Arrangements for Mothers

	(1) Reduced- hours Permanent (Uncond.)	(2) Reduced- hours Permanent (Cond.)	(3) Less-than- Full-Time Permanent (Uncond.)	(4) Regular Schedule	(5) On call	(6) My working times can be flexible (yes /no)	(7) Any hour worked from home
First child 0-2 × Post-2009	0.0501*** (0.00889)	0.0845*** (0.0169)	0.0725*** (0.0147)	0.131*** (0.0245)	-0.0285 ⁺ (0.0158)	-0.0613* (0.0303)	-0.0380 ⁺ (0.0221)
First child 3-5 × Post-2012	0.0499*** (0.0111)	0.0778*** (0.0199)	0.0767*** (0.0177)	0.0857** (0.0272)	-0.0447** (0.0170)	-0.0101 (0.0281)	0.0323 (0.0230)
Pre-period mean:							
First child aged 0-2	0.0367	0.0837	0.154	0.323	0.103	0.588	0.314
First child aged 3-5	0.0529	0.107	0.191	0.352	0.123	0.579	0.310
Observations:							
N	123930	74013	123930	83553	83580	61120	88698
N Individuals	12506	9118	12506	9782	9783	8259	10532

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 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 and the dummy for the first child being between 3 and 5 with an indicator for the year being equal or after 2013. These coefficients are reported in the first two lines. The regressions also include the dummies based on the age of the first child for age groups 0-2, 3-5, 6-8, 9-10, more than 10; as well as the interaction between 6-8 and post-2016, and 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. Here the sample is restricted to women only. The outcomes considered are: in column (1) a dummy for working reduced hours (25-34 hours per week) on a permanent contract, unconditional; in column (2) same as column (1) but conditional on working a positive number of hours; in column (3) a dummy for working less than full time (1-34 hours per week) on a permanent contract, unconditional; in column (4) a dummy for being on a regular schedule; in column (5) a dummy for being on call or working an irregular schedule; in column (6) a dummy for agreeing with the statement “My working times can be flexible”; in column (7) a dummy for working from home at least one hour a week on a regular basis. All variables are defined in Appendix A.

Table 3: Child Penalty for Early and Late Cohorts of Mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Perma-nent contract (uncondi-tional)	Perma-nent contract (condi-tional)	Regular schedule	Weekly hours in paid em-ployment (condi-tional)	In labor force	Employed	Total time house-work	Total time parenting
Child Penalty 0-7	-0.400*** (0.0245)	-0.252*** (0.0310)	-0.426*** (0.0308)	-18.37*** (0.868)	-0.375*** (0.0223)	-0.362*** (0.0220)	11.27*** (0.829)	41.72*** (1.186)
Child Penalty 0-7 * Post-2009 First Birth	0.142*** (0.0335)	0.177*** (0.0394)	0.121** (0.0408)	3.992*** (1.083)	0.0795** (0.0288)	0.0813** (0.0290)	-1.784+ (1.007)	0.952 (1.528)
Pre-birth mean:								
Early cohorts	0.586	0.727	0.652	38.89	0.920	0.860	14.52	0.340
Late cohorts	0.619	0.736	0.677	37.63	0.928	0.873	13.09	0.267
Observations:								
N	84594	49993	56229	56116	84594	84594	71447	66524
N Individuals	9625	6858	7369	7368	9625	9625	8717	8554
N New Parents	776	636	662	662	776	776	752	746

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 several outcomes of interest, both at baseline, namely for the cohorts of women who became mothers 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 estimates from running Specification (3): the top row reports estimates for the baseline child penalty (γ), while the bottom row reports estimates for the change in child penalty (γ_{post}). For consistency with Figure 3, we restrict to four-year cohorts around the Fair Work Act (2005-08 and 2010-13) and we drop the year right before childbirth (partially treated due to pregnancy). All outcome variables are defined in Appendix A.

Table 5: Changes in Child Penalty by Exposure to the Fair Work Act - Women (2/2 - Labor Market Outcomes)

	(1)	(2)	(3) Weekly hours in paid em- ployment (condi- tional)	(4) Weekly hours in paid em- ployment (condi- tional)	(5) Log hourly wage	(6) Log hourly wage	(7) Annual labor income, 2021 AUD (thou- sands)	(8) Annual labor income, 2021 AUD (thou- sands)
Employed	Employed	Employed	Weekly hours in paid em- ployment (condi- tional)	Weekly hours in paid em- ployment (condi- tional)	Log hourly wage	Log hourly wage	Annual labor income, 2021 AUD (thou- sands)	Annual labor income, 2021 AUD (thou- sands)
Child Penalty 0-7	-0.346*** (0.0191)	-0.291*** (0.0305)	-16.87*** (0.697)	-14.37*** (0.996)	0.0811*** (0.0235)	0.0632+ (0.0362)	-33.13*** (1.588)	-35.26*** (3.289)
* Tercile = 2		-0.0776+ (0.0442)		-5.251*** (1.578)		0.0651 (0.0534)		-3.028 (4.141)
* Tercile = 3		-0.0823+ (0.0475)		-2.312 (1.719)		-0.0409 (0.0597)		10.05* (3.944)
Child Penalty 0-7 * Post-2009 First Birth	0.0749*** (0.0222)	0.00592 (0.0370)	4.294*** (0.797)	2.458* (1.203)	-0.0226 (0.0268)	-0.0445 (0.0441)	7.386*** (1.810)	0.946 (3.732)
* Tercile = 2		0.109* (0.0514)		3.715* (1.796)		0.00800 (0.0627)		14.89** (4.614)
* Tercile = 3		0.0828 (0.0584)		2.671 (2.076)		0.0810 (0.0694)		3.859 (4.583)
Early (pre-2010) cohorts: Mean Y pre-birth	0.923		40.20		2.947		59.73	
- tercile 1		0.951		41.89		2.993		73.85
- tercile 2		0.949		40.87		3.040		64.04
- tercile 3		0.858		36.80		2.743		37.56
Late (post-2010) cohorts: Mean Y pre-birth	0.933		39.20		3.268		62.53	
- tercile 1		0.972		40.81		3.401		78.93
- tercile 2		0.959		39.49		3.267		62.59
- tercile 3		0.828		35.63		3.021		38.54
Early cohorts: New parents	415		365		335		426	
- tercile 1		134		128		121		137
- tercile 2		138		127		117		141
- tercile 3		143		110		97		148
Late cohorts: New parents	734		670		617		782	
- tercile 1		234		222		211		250
- tercile 2		296		283		257		313
- tercile 3		204		165		149		219
Tot observations	31792	31792	23520	23519	19155	19154	35105	35105

Standard errors in parentheses

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

Note: This table reports the estimates of the child penalty in labor market outcomes for women and its interaction with treatment status (defined by the tercile of casual prevalence of a woman's modal job in the five years before the birth of her first child). More details in the note to Table 4. The outcome in columns (1) and (2) is a dummy variable for being in employment; in columns (3) and (4) it is the number of hours of work conditional on being in employment. The outcome in columns (5) and (6) is the logarithm of hourly wages, and in columns (7) and (8) it is annual labor income in thousands of 2021 Australian Dollars. A detailed variable description is in Appendix A.

Appendix

List of Appendices

A Variables' Construction	57
B Characteristics of Casual Contracts and Qualitative Evidence on the Effect of the Fair Work Act	61
C Threats to the Identification of the Direct Effect of the Fair Work Act on Work Arrangements	66
D Further Evidence on the Effect of the Fair Work Act on Work Arrangements	77
E Threats to Identification in the Early versus Late Cohort Design	79
F Paid Parental Leave	85
G Further Details on the Exposure Design	89
H Additional Evidence from the Exposure Design	92
I Threats to Identification in the Exposure Design	99

A Variables' Construction

Demographics and Employment 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, Garrard, Hahn, Jin, Kamath, Macalalad, Watson, Wilkins, and Wooden (2019) for further details. “Employed” is a dummy equal to one if the respondent is employed; this includes also respondents on parental leave. “Weekly hours in paid employment” is conditional on hours being strictly positive.

Income and Wages “Annual labor income” and “Hourly wage” are yearly earnings and hourly wages, both in 2021 Australian dollars. As instructed in the HILDA User Manual (Summerfield et al., 2019), the raw hourly wage is computed as the ratio between “Imputed current weekly gross wages and salary in all jobs” (which we winsorize at 0.5% level, by year and gender) and “Combined hours per week usually worked in all jobs”, conditional on both variables being positive and the individual being employed. “Log hourly wage” is the natural logarithm of the raw hourly wage.

The way hourly wages are measured and computed in HILDA can introduce noise and might not be reliable in periods of large job changes (such as after childbirth for example); thus we avoid to emphasize the results on hourly wages. Hourly wages are computed as the ratio of weekly wages to the number of hours spent in paid employment every week. However, weekly wages come 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 currently works; since we are taking the ratio, small mistakes can turn into large and systematic noise. Thus, we don't necessarily think that hourly wage is a reliable measure in the first year of life of the child, and we avoid to emphasize results for this variable.

Contract Type and Weekly Hours “Permanent contract (conditional)” is a dummy equal to one if the respondent has a permanent contract, and equal to zero if they have casual or fixed-term contracts (it is missing for individuals not in employment). “Casual contract (conditional)” is similarly defined. “Permanent contract (unconditional)” is defined as its conditional counterpart except that it takes the value of zero (instead of missing) for individuals not in employment. The variable “Reduced hours” is a dummy equal to 1 if the weekly working hours of the respondent are between 25 and 34. “Full-time” refers

to working 35 hours per week or more, and “Less-than-full-time” is defined as working between 1 and 34 hours per week. Throughout the paper, we specify whether they are “conditional”, meaning that they are conditional on working a positive amount of hours (thus missing for individuals not in employment), or “unconditional”, meaning that they take a value of 0 for individuals not in employment.

Regular Schedule and On Call There are mainly two questions that refer to work schedules. Note that these questions are explicitly asked also to individuals currently on paid leave. 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, that they vary from month to month, or “other”. If the respondent replies the latter, they are then asked to indicate which days of the week they usually work. 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”, or if the respondent indicates four weekdays after answering “other” (e.g. Monday to Thursday), and the answer to the second question is “A regular daytime schedule”, and equal to 0 for any other combination of the two answers. We define someone as being “On Call” if they answer “On Call ” or “Irregular Schedule” to the second question.

Workplace Flexibility The main 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 of 4 or above and disagreement otherwise. Results are unchanged if we use the categories separately, or alternative variables, such as the answer to the question “Would you be entitled to flexible start/finish times if you were to need it?”. Note that also in this case this question is explicitly asked also to individuals currently on paid leave.

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 but not for individuals on paid leave.

Education and Other Labor Market Outcomes "Has bachelor degree" is a dummy variable equal to one if the respondent holds a bachelor degree, a graduate diploma or a postgraduate one. "Occupational status scale" is the Australian Socioeconomic Index 2006 from McMillan, Beavis, and Jones (2009), ranging from 1 to 100. "Promoted in the past year" is a dummy variable that takes the value of one if the respondent replies "yes" to the question "Did any of these happen to you in the past 12 months? [...] Promoted at work". This can be "conditional on current employment", that is, conditional on being employed at the time of answering (and missing otherwise), or "unconditional", which does not impose this restriction. "Supervise other employees" is a dummy variable that takes the value of one if answering "yes" to the question "As part of your job, do you normally supervise the work of other employees?". This can also be conditional on employment or unconditional; in the latter case it takes value of zero for individuals who are unemployed or not in the labor force.

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 the case for the following measures:

- **Playing with and caring for your children**
 - "Playing with your children, helping them with personal care, teaching, coaching or actively supervising them, or getting them to child care, school and other activities "
- **Housework** is the sum of
 - "Housework, such as preparing meals, washing dishes, cleaning house, washing clothes, ironing and sewing"
 - "Household errands, such as shopping, banking, paying bills, and keeping financial records (but do not include driving children to school and to other activities)"
 - "Outdoor tasks, including home maintenance (repairs, improvements, painting, etc.), car maintenance or repairs and gardening"
- **Paid employment and commute** is the sum of
 - "Paid employment"
 - "Travelling to and from a place of paid employment"
- **Total active time** is the sum of
 - The six items listed above

- “Looking after other people’s children (aged under 12 years) on a regular, unpaid basis”
- “Volunteer or charity work (for example, canteen work at the local school, unpaid work for a community club or organisation)”
- “Caring for a disabled spouse or disabled adult relative, or caring for elderly parents or parents-in-law”

Home Production is the sum of Housework and Playing with your children. **Leisure and Sleep** is calculated using the total number of hours in a week (168) and subtracting the number of hours of Total Active Time (following Jarosz, Matysiak, and Osiewalska, 2023). The form explicitly asks not to count any activity twice; at the end, it asks to sum the hours indicated for all activites and specifies the following: “This cannot exceed 168 hours and typically will not be greater than 120. If it is, please re-think your answers.” This mitigates potential worries that our measures of home production and total active time might include double-counting of hours.

The details of the cleaning are the following:

- If an individual reports time use for all activities except a few (e.g. they report positive hours for employment and housework but don’t report any hours for volunteering), we assume they devote zero hours to them;³⁵
- If an individual reports zero hours on all activities, we set them as missing;³⁶
- When we group variables (e.g. work and commute) this is non-missing if at least one of the underlying categories is non-missing;
- Total active time (and thus Leisure and Sleep) is non-missing only if *all* underlying variables are non-missing.³⁷

Female Share “Female share of home production” is the number of weekly hours a woman spends in home production divided by the sum of weekly hours she and her male partner spend in home production. “Female share of paid work” is defined analogously. “Female share of total income” is the fraction of total annual gross household labor income earned by the woman. This is defined only for the individuals for which we also observe the time use variables (but results are unchanged without this restriction). These “share” variables are only defined for heterosexual couples, and only when we observe the underlying time use and income variable for both partners in the couple.

³⁵ 0.8% of observations (individual-year) reporting positive hours for other activities don’t report any hour for housework, 1.4% for employment, 7.8% for volunteering.

³⁶ 1.3% of observations with non-missing hours report zero hours on all activities.

³⁷ All underlying variables are non-missing for 98.9% of observations that report hours.

B Characteristics of Casual Contracts and Qualitative Evidence on the Effect of the Fair Work Act

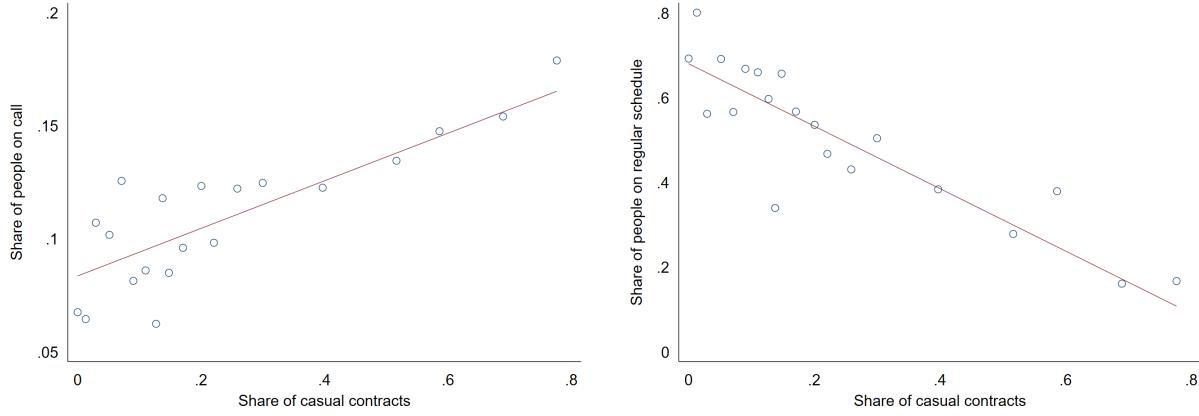
B.1 Casual Contracts Are Associated With Irregular Schedules

Casual contracts are characterized by irregular schedules. This is true both across jobs, as can be seen by the scatterplots in Figure B.1, and within jobs, as can be inferred from the coefficient on “permanent” in Table B.1 remaining high and highly significant after controlling for occupation-by-industry fixed effects.

Figure B.1 shows the negative correlation between casual contracts and schedule regularity across jobs. In this figure, for each job (2-digit industry-by-occupation cell) we compute the fraction of workers with a casual contract (x-axis, both panels), the share of workers who report being on call (panel a) and the share of workers who report having a regular schedule (panel b), using observation before the Fair Work Act. From these bin-scatters, the negative correlation between casual contracts and having a regular schedule is very clear across jobs.

Table B.1 shows that this negative correlation between having a casual contract and having a regular schedule—and the corresponding positive correlation between having a permanent contract and having a regular schedule—also holds within jobs and within people who change contract during their careers. In this table we report the coefficient estimates from regressing the dummy for having a regular schedule on the dummy for having a permanent contract, without any further controls (column 1), controlling for jobs (occupation-by-industry) fixed effects (column 2), and controlling for individual fixed effects (column 3). The positive correlation remains high and statistically significant also when including these controls (17 p.p. and 18.4 p.p. respectively), thus indicating that this correlation is driven by the nature of the casual/permanent contract itself rather than by other characteristics of jobs it is associated to.

Figure B.1: Correlation between Casual Contract and (Ir)regular Schedule



(a) Share of Casual Contracts and Fraction of Workers on Call (b) Share of Casual Contracts and Fraction of Workers on a Regular Schedule

Note: This figure shows a binscatter between share of casual contracts and two measures of (ir)regular schedule. The unit of observation is a job (occupation-by-industry, 2-digit), weighted by the fraction of workers, all measured before 2009. Variables construction is detailed in Appendix A.

Table B.1: Probability of Having a Regular Schedule by Type of Contract

	(1) Regular schedule	(2) Regular schedule	(3) Regular schedule
Permanent contract (conditional)	0.289*** (109.88)	0.170*** (66.28)	0.184*** (66.24)
Constant	0.362*** (167.33)	0.440*** (215.31)	0.436*** (204.94)
Observations	150955	147482	146227
Included fixed effects	None	Occ-by-ind (2d)	Individual

t statistics in parentheses

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

Note: The table shows the estimated coefficients from a regression of a dummy variable for being on a regular schedule on a dummy variable for having a permanent contract (as opposed to casual or fixed term, conditional on working). Column 1 includes no additional controls, column 2 includes occupation-by-industry (2-digit) fixed effects, and column 3 includes individual fixed effects.

B.2 Qualitative Evidence on the Effect of the Fair Work Act

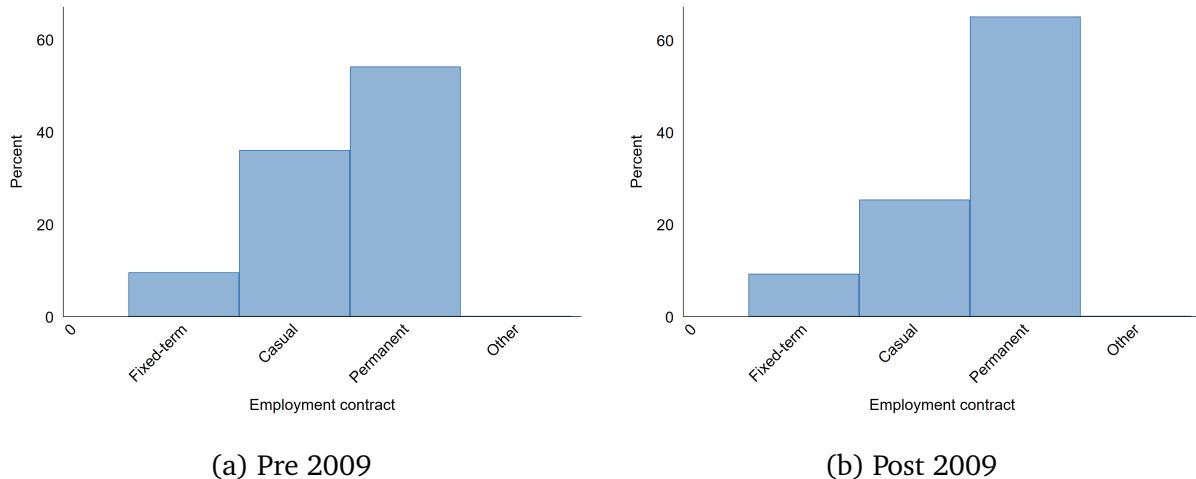
The advent of “permanent reduced hours”, meaning the arrangement to work right below full-time but on a permanent contract, is evident both in the wording of the awards in the raw data. Table B.2 shows how the wording of the awards was changed to introduce and expand on the notion of permanent part-time. Figure B.2 shows the type of contract for mothers who work less than 35 hours per week: the giant post-2009 increase in the fraction of permanent contracts among less than full-time workers is clear. Figure B.3 shows the distribution of hours worked by mothers under permanent contracts: the clear missing mass of hours right below full-time in the pre-2009 data disappears post-2009.

Table B.2: Comparing Pre-2009 Awards to Modern Awards

Before Fair Work Act			After Fair Work Act		
Name	Year	Provision	Name	Year	Provision
Private Hospital Employees (State) Award (AN120434)	2006	(i) A permanent part-time employee is one who is permanently appointed to work a specified number of hours which are less than those prescribed for a full-time employee. Permanent part-time employees shall be paid an hourly rate calculated on the basis of [...] (Section 8)	Health Professionals and Support Services Award (MA000027)	2010	A part - time employee is an employee who is engaged to work less than the full - time hours of an average of 38 hours per week and who has reasonably predictable hours of work. Before commencing employment, the employer and employee will agree in writing on a regular pattern of work including the number of hours to be worked each week, the days of the week the employee will work and the starting and finishing times each day. (Section 10.3)
Community Colleges Tutors (State) Award (AN120151)	2006	"Part-Time Tutor" means a tutor other than a casual tutor who is engaged to work regularly, but has a lesser tutoring load than a full-time tutor. [...] A part-time tutor shall be entitled to the same conditions as a full-time tutor and shall be entitled to all conditions on a pro-rata basis. (Section 2.6 and 3.2.2)	Educational Services (Post-Secondary Education) Award (MA000075)	2010	A part-time employee is an employee employed for less than the normal ordinary hours specified for a full-time employee, or in the case of a teaching staff member for less than the face-to-face teaching load of a full-time teaching staff member at that workplace, for which all award entitlements are paid or calculated on a pro rata basis by reference to the time worked. Before engagement the employer and the part-time employee will agree in writing on a regular pattern of work, including the number of hours to be worked each week, the days of the week the employee will work, the starting and finishing times each day where hours are not averaged and any periods during a year when work by the employee will not be required. Changes in hours may only be made by agreement in writing between the employer and the employee. (Section 10.3)
Restaurant Keepers Award (AN170086)	2006	A part-time employee shall be entitled to all provisions of a full-time employee on a pro rata basis. (Section 27)	Fast Food Industry Award (MA000003)	2010	A part-time employee is an employee who works less than 38 hours per week and has reasonably predictable hours of work. (Section 12.1)

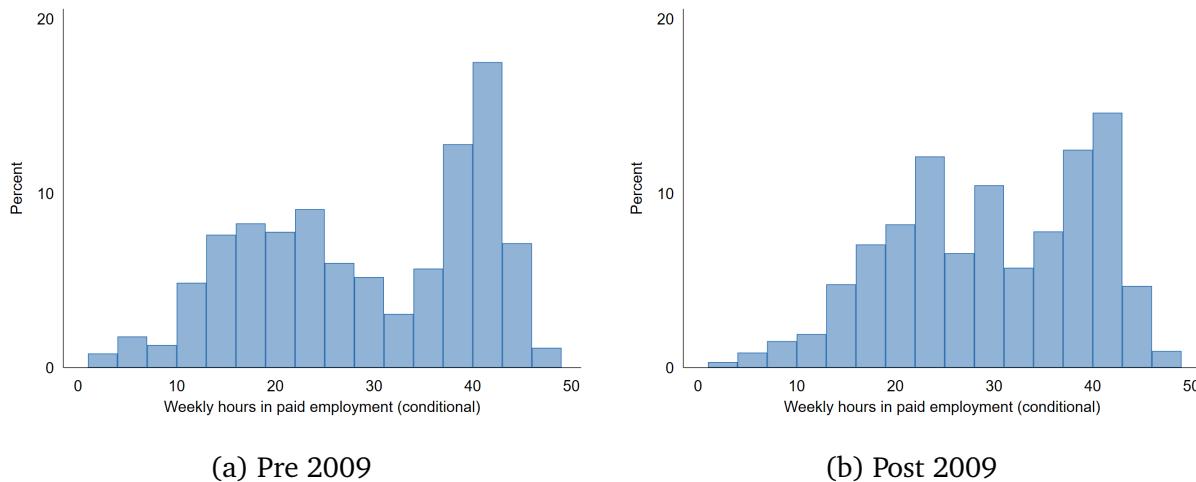
Note: The table shows quotes from different Awards, before and after the Fair Work Act, to illustrate how provisions concerning part-time employment changed. The correspondence between pre-2009 Awards and the post-Fair Work Act relevant contract was downloaded here: <https://www.fwc.gov.au/agreements-awards/awards/awards-research> (date of download: February 5 2024). All the texts from the Awards can be downloaded from [https://www.fwc.gov.au/document-search\(dateofdownload:March26,2024\)](https://www.fwc.gov.au/document-search(dateofdownload:March26,2024)).

Figure B.2: Type of Contracts among Mothers Working Less Than Full-Time, Before and After the Fair Work Act



Note: This figure shows the distribution of types of contracts for mothers working less than full-time (1 to 34 hours per week), before and after the 2009 Fair Work Act.

Figure B.3: Number of Hours Worked by Mothers in Permanent Jobs, Before and After the Fair Work Act



Note: This figure shows the distribution of hours worked under permanent contracts by mothers, before and after the 2009 Fair Work Act.

C Threats to the Identification of the Direct Effect of the Fair Work Act on Work Arrangements

Section 3.3 quantifies the effect of the Fair Work Act on work arrangements. These findings rely on the assumption that new mothers of young children before and after 2009 would have shown similar work arrangements in the absence of the law, relative to non-mothers. In this Appendix, we address potential threats to identification. First, we show that fertility does not change systematically around 2009 (Section C.1), and that women who became mothers before and after 2009 are similar on pre-birth observables (Section C.2). Second, we address the concern that a potential “added-worker effect” induced by the global financial crisis could be driving our results, showing that results are robust to controlling for partner’s labor market status (Section C.3). Finally, we demonstrate that there was no employer backlash in hiring in response to the reform, by showing that there are no gender differences in the trend of employment, even for the childbearing age bracket (Section C.4).

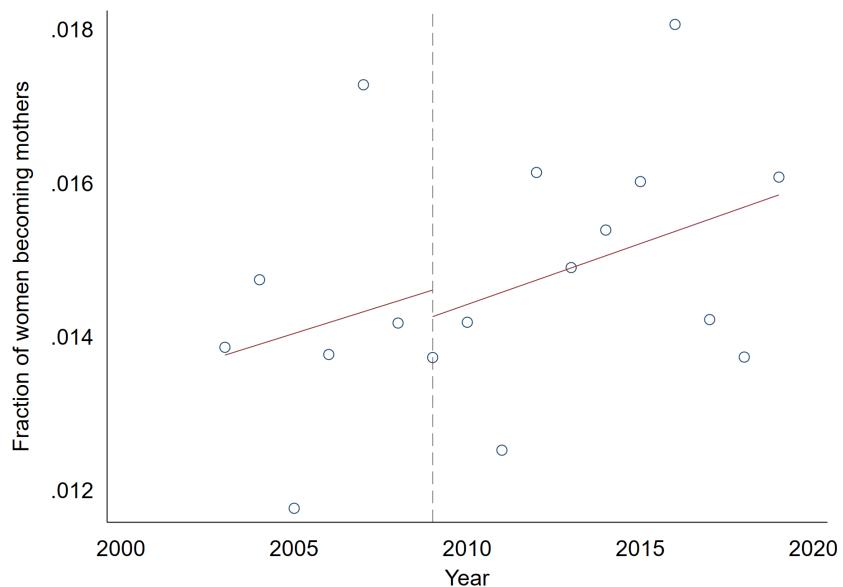
C.1 No Systematic Changes in Fertility

Australian women did not become mothers at different rates around 2009. Figure C.1 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 directly impact 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).

C.2 Early and Late Cohorts of Mothers are Similar on Pre-Birth Observables

Women who became mothers before and after 2009 were not observationally different on most dimensions when compared before childbirth. This lends support to the hypothesis that they would have behaved similarly also after childbirth, had institutional conditions not changed thanks to the Fair Work Act. Table C.1 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. The few significant differences could be by chance (multiple testing) and, if anything, play against the results we find. Table C.2 shows that “early” and “late” mothers also worked in similar industries and occupations before childbirth.

Figure C.1: 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.

Table C.1: Balance Table: Early versus Late Cohorts of Mothers-to-be

	Early cohort (2003-2008)			Late cohort (2010-2017)			Diff.	SE
	N	Mean	SD	N	Mean	SD		
Age at first birth	433	28.72	(6.09)	800	29.05	(5.51)	0.32	(0.34)
In labor force	427	0.93	(0.26)	797	0.93	(0.25)	0.00	(0.02)
Employed	427	0.88	(0.33)	797	0.88	(0.33)	0.00	(0.02)
Weekly hours in paid employment (conditional)	374	39.18	(10.87)	700	38.19	(10.63)	-0.99	(0.69)
Annual labor income, 2021 AUD (thousands)	433	48.64	(38.71)	800	56.27	(41.02)	7.63**	(2.40)
Hourly wage, 2021 AUD	374	29.20	(13.65)	700	30.51	(13.81)	1.31	(0.88)
Total time work	307	38.18	(18.06)	684	38.42	(17.41)	0.24	(1.21)
Total time housework	308	15.73	(13.05)	687	13.36	(12.27)	-2.37**	(0.86)
Total time parenting	308	0.50	(3.21)	687	0.25	(2.25)	-0.25	(0.18)
Total time home production	308	16.23	(13.88)	687	13.61	(12.60)	-2.63**	(0.89)
Total time leisure and sleep	306	112.69	(21.42)	679	114.84	(19.17)	2.15	(1.37)
Regular schedule	374	0.64	(0.48)	700	0.69	(0.46)	0.04	(0.03)
On call and irregular shifts	374	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	0.44	(0.50)	0.07	(0.05)
Hours work from home	380	1.75	(5.93)	703	1.57	(5.20)	-0.18	(0.35)
Any hours worked from home	380	0.18	(0.38)	703	0.20	(0.40)	0.02	(0.03)
Permanent contract (conditional)	352	0.71	(0.45)	667	0.75	(0.43)	0.04	(0.03)
Casual contract (conditional)	352	0.18	(0.39)	667	0.12	(0.33)	-0.06**	(0.02)
Fixed-term contract (conditional)	352	0.11	(0.31)	667	0.13	(0.34)	0.03	(0.02)
Reduced hours (25-34) permanent contract unconditional	427	0.05	(0.21)	797	0.06	(0.23)	0.01	(0.01)
Has bachelor degree or above	427	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 variable descriptions, see Appendix A. Stars indicate statistical significance (+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

C.3 Accounting for the Financial Crisis

The global financial crisis was particularly mild in Australia, which never went into formal recession (Barrett, 2018; Borland, 2011; Reserve Bank of Australia, n.d.). Thus, it is unlikely to be driving our results.

Here we show formally that the financial crisis is unlikely to be the driver of our results in Section 3.3. In the tables below, we show that, controlling for their male partner's employment and earnings, our results on the effect of the Fair Work Act on mothers' work arrangements remain virtually identical. Tables C.3, C.4, and C.5 replicate columns (1)-(3) of Table 2 respectively, and progressively add controls for whether a woman's male partner is employed or how many hours per week he works and his labor earnings. The coefficients of interest in the first two rows are practically unchanged from adding these controls. This indicates that the results on the effect of the Fair Work Act on mothers' work arrangements presented in Section 3.3 are not driven by women rearranging their labor supply due to their partner's reduced labor earnings from the financial crisis.

Table C.3: Robustness of Effect of Fair Work Act on Work Arrangements to Accounting For The Financial Crisis: Permanent Reduced Hours (Unconditional)

	(1) Reduced hours (25-34) permanent contract un- conditional (woman)	(2) Reduced hours (25-34) permanent contract un- conditional (woman)	(3) Reduced hours (25-34) permanent contract un- conditional (woman)	(4) Reduced hours (25-34) permanent contract un- conditional (woman)	(5) Reduced hours (25-34) permanent contract un- conditional (woman)	(6) Reduced hours (25-34) permanent contract un- conditional (woman)	(7) Reduced hours (25-34) permanent contract un- conditional (woman)
First child 0-2 × First birth after 2009	0.0633*** (0.0121)	0.0629*** (0.0122)	0.0627*** (0.0122)	0.0629*** (0.0122)	0.0637*** (0.0128)	0.0636*** (0.0128)	0.0636*** (0.0128)
First child 3-5 × First birth after 2012	0.0586*** (0.0152)	0.0593*** (0.0155)	0.0589*** (0.0155)	0.0593*** (0.0154)	0.0623*** (0.0162)	0.0620*** (0.0163)	0.0624*** (0.0162)
Employed (man)		0.0109+ (0.00581)	0.00663 (0.00586)	-0.000256 (0.00679)			
Hours per week usually worked in all jobs (man)					0.000108 (0.000154)	0.0000398 (0.000158)	0.0000680 (0.000155)
Weekly labor earnings (000) (man)			0.00445* (0.00225)			0.00461+ (0.00239)	
Log(1+weekly labor earnings) (man)				0.00202* (0.000795)			0.00191* (0.000855)
Pre-period mean:							
First child aged 0-2	0.0312	0.0313	0.0313	0.0313	0.0333	0.0333	0.0333
First child aged 3-5	0.0470	0.0474	0.0474	0.0474	0.0490	0.0490	0.0490
Observations:							
N	48702	45135	45135	45135	37882	37882	37882
N Individuals	6225	5858	5858	5858	5141	5141	5141

Standard errors in parentheses

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

Note: The table replicates column (1) of Table 2 but includes controls for mothers' male partner's employment status, hours of work, and earnings. The outcome variable is a dummy for working between 25 and 34 hours a week under a permanent contract (unconditional). Note that the coefficients without controls (column 1) are marginally different from the table in the main text because, in order to carry out a meaningful exercise, here we restrict the sample to women with male partners.

Table C.4: Robustness of Effect of Fair Work Act on Work Arrangements to Accounting For The Financial Crisis: Permanent Reduced Hours (Conditional on Working)

	(1) Reduced hours (25-34) permanent contract (woman)	(2) Reduced hours (25-34) permanent contract (woman)	(3) Reduced hours (25-34) permanent contract (woman)	(4) Reduced hours (25-34) permanent contract (woman)	(5) Reduced hours (25-34) permanent contract (woman)	(6) Reduced hours (25-34) permanent contract (woman)	(7) Reduced hours (25-34) permanent contract (woman)
First child 0-2 × First birth after 2009	0.114*** (0.0205)	0.114*** (0.0206)	0.113*** (0.0206)	0.114*** (0.0206)	0.113*** (0.0212)	0.113*** (0.0212)	0.113*** (0.0212)
First child 3-5 × First birth after 2012	0.0862** (0.0264)	0.0897*** (0.0268)	0.0891*** (0.0268)	0.0897*** (0.0268)	0.0928*** (0.0276)	0.0921*** (0.0276)	0.0929*** (0.0276)
Employed (man)		-0.00426 (0.0109)	-0.0123 (0.0113)	-0.0172 (0.0132)			
Hours per week usually worked in all jobs (man)					0.000343 (0.000240)	0.000192 (0.000247)	0.000290 (0.000242)
Weekly labor earnings (000) (man)			0.00774* (0.00376)			0.00965* (0.00395)	
Log(1+weekly labor earnings) (man)				0.00224+ (0.00136)			0.00228 (0.00141)
Pre-period mean:							
First child aged 0-2	0.0650	0.0652	0.0652	0.0652	0.0672	0.0672	0.0672
First child aged 3-5	0.0945	0.0949	0.0949	0.0949	0.0979	0.0979	0.0979
Observations:							
N	29630	27488	27488	27488	25010	25010	25010
N Individuals	4524	4237	4237	4237	3969	3969	3969

Standard errors in parentheses

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

Note: The table replicates column (2) of Table 2 but includes controls for mothers' male partner's employment status, hours of work, and earnings. The outcome variable is a dummy for working between 25 and 34 hours a week under a permanent contract, conditional on working. Note that the coefficients without controls (column 1) are marginally different from the table in the main text because, in order to carry out a meaningful exercise, here we restrict the sample to women with male partners.

Table C.5: Robustness of Effect of Fair Work Act on Work Arrangements to Accounting For The Financial Crisis: Permanent Below 35 hours (Unconditional)

	(1) Permanent below 35 (uncondi- tional) (woman)	(2) Permanent below 35 (uncondi- tional) (woman)	(3) Permanent below 35 (uncondi- tional) (woman)	(4) Permanent below 35 (uncondi- tional) (woman)	(5) Permanent below 35 (uncondi- tional) (woman)	(6) Permanent below 35 (uncondi- tional) (woman)	(7) Permanent below 35 (uncondi- tional) (woman)
First child 0-2 × First birth after 2009	0.0689*** (0.0199)	0.0710*** (0.0201)	0.0707*** (0.0201)	0.0710*** (0.0201)	0.0717*** (0.0212)	0.0714*** (0.0212)	0.0716*** (0.0212)
First child 3-5 × First birth after 2012	0.0801*** (0.0243)	0.0824*** (0.0244)	0.0817*** (0.0244)	0.0825*** (0.0244)	0.0873*** (0.0256)	0.0868*** (0.0256)	0.0874*** (0.0256)
Employed (man)		0.0315*** (0.00789)	0.0239** (0.00800)	0.0150 ⁺ (0.00898)			
Hours per week usually worked in all jobs (man)					0.0000940 (0.000225)	-0.0000106 (0.000230)	0.0000373 (0.000226)
Weekly labor earnings (000) (man)			0.00797* (0.00336)			0.00710 ⁺ (0.00363)	
Log(1+weekly labor earnings) (man)				0.00299** (0.000990)			0.00272* (0.00107)
Pre-period mean:							
First child aged 0-2	0.180	0.179	0.179	0.179	0.188	0.188	0.188
First child aged 3-5	0.214	0.214	0.214	0.214	0.220	0.220	0.220
Observations:							
N	48702	45135	45135	45135	37882	37882	37882
N Individuals	6225	5858	5858	5858	5141	5141	5141

Standard errors in parentheses

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

Note: The table replicates column (3) of Table 2 but includes controls for mothers' male partner's employment status, hours of work, and earnings. The outcome variable is a dummy for working less than 35 hours a week under a permanent contract (unconditional). Note that the coefficients without controls (column 1) are marginally different from the table in the main text because, in order to carry out a meaningful exercise, here we restrict the sample to women with male partners.

C.4 No Backlash in Hiring

When giving rights to a specific group of workers, employers might react by not hiring them: we show here that this was not the case for the 2009 Australian Fair Work Act, which is consistent with the law giving a “right to request” rather than a “hard” right. Note that this might be different in different contexts, as we discuss below. Following Fernández-Kranz and Rodríguez-Planas (2021), we formally test whether women of childbearing age were less likely to be in employment, worked systematically fewer hours, or were paid systematically less after the Fair Work Act relative to men and relative to older women. We don’t find support for this backlash in any dimension.

The specification we use to test for backlash in hiring is a triple-difference, which compares men and women, of childbearing age versus not, before and after the passage of the 2009 Fair Work Act. In Table C.6, we report coefficient estimates from regressing a dummy for being employed (columns 1-2), number of hours conditional on working (columns 3-4), and the logarithm of hourly wages (columns 5-6) on an indicator for being a woman, an indicator for being of childbearing age (20-40), an indicator for the year being after 2009 (the year of the Fair Work Act), all the pairwise interactions and the triple interaction. We also control for age and year fixed effects. The coefficient of interest is the coefficient on the triple interaction, reported in the first row. For all outcomes, we run the regression both for individuals aged 20-65 (odd-numbered columns) and also restricting to individuals aged 25-50 (even-numbered columns). Since the second age range is smaller, we expect it to group individuals who are more comparable to each other.

After the Fair Work Act, women of childbearing age were not less likely to be employed, did not work for fewer hours, and were not paid less relative to men and older women. If the Fair Work Act had caused a backlash from employers, making them less likely to hire women of childbearing age, this would show up as a negative coefficient on the triple interaction. In all the specifications, the coefficients are not statistically different from zero and the point estimates are positive for employment, hours of work, and hourly wages, indicating that such employer backlash did not materialize.

A Case of Employer Backlash: Spain While we do not find any evidence of backlash to the right granted by the Fair Work Act to Australian workers, a related piece of legislation in Spain granting parents the right to request part-time did cause an employer backlash, as documented by Fernández-Kranz and Rodríguez-Planas (2021). The difference is likely due both to the differential strength of the right granted and to the very different labor markets in Australia and in Spain at the time of the passage of the laws.

For what concerns the strength of the right given by the law, the Australian 2009 Fair Work Act grants parents a right to request a change in work arrangements, request that employers can refuse on reasonable business grounds. The Spanish 1999 *Law to Promote the Reconciliation of Work and Family Life* studied by Fernández-Kranz and Rodríguez-Planas (2021) is also a right to request, but in practice the employer is very rarely allowed to refuse the request.³⁸ In addition, the Australian Fair Work Act did not allow the employee

³⁸The only instance in which the employer is allowed to refuse the request is when the latter concerns hours that are outside the employee’s usual shift. Even in this instance, there have been cases in which the

to appeal against the employer's decision in court in the time frame of our analysis, while the Spanish law allows for appeals. These two differences imply that the Spanish legislation is much harder on employers, generating potential costs for them that they may pass-through to their employees, while this is much less true in Australia.

Moreover, the laws were passed in countries experiencing very different labor markets: the unemployment rate in Spain in 1999 was 15.5%, while it was 5.6% in Australia in 2009.³⁹ This suggests a potentially different bargaining power of employers, with Spanish employers with a potential scope for backlash that Australian employers likely did not have.

Thus, the very different context and the different strength of the Australian Fair Work Act and of the Spanish Law to Promote the Reconciliation of Work and Family Life likely explain the difference between the results we present here and the findings in Fernández-Kranz and Rodríguez-Planas (2021).

decision has been appealed, and the court has ruled in favor of the worker, since the law forefronts the child's interest.

³⁹Source: International Labour Organization. "Labour Force Statistics database (LFS)" ILOSTAT. Accessed October 16, 2024. ilo.org/statistics.

Table C.6: No Backlash in Hiring

	(1) Employed	(2) Employed	(3) Weekly hours in paid employment (condi- tional)	(4) Weekly hours in paid employment (condi- tional)	(5) Log hourly wage	(6) Log hourly wage
Childbearing age (20-40) × Woman × Post-2009	0.00689 (0.0168)	0.00310 (0.0223)	0.730 (0.623)	1.009 (0.830)	0.0219 (0.0236)	0.0450 (0.0324)
Childbearing age (20-40) × Woman	0.0469*** (0.0127)	-0.00513 (0.0164)	-0.353 (0.467)	-2.318*** (0.610)	0.00904 (0.0177)	-0.0847*** (0.0238)
Woman × Post-2009	0.0335*** (0.00366)	0.0315*** (0.00440)	1.793*** (0.137)	2.088*** (0.163)	-0.00964 ⁺ (0.00529)	-0.0151* (0.00634)
Childbearing age (20-40) × Post-2009	-0.0549*** (0.0122)	-0.00508 (0.0163)	-0.669 (0.437)	-0.451 (0.578)	-0.00798 (0.0166)	0.0129 (0.0228)
Woman	-0.156*** (0.00282)	-0.169*** (0.00334)	-11.58*** (0.106)	-12.64*** (0.124)	-0.0923*** (0.00409)	-0.0908*** (0.00483)
Constant	0.828*** (0.00131)	0.890*** (0.00158)	43.09*** (0.0469)	44.55*** (0.0560)	3.294*** (0.00183)	3.324*** (0.00219)
Observations	217344	130098	164073	105292	148302	95953
Age Range	20-65	25-50	20-65	25-50	20-65	25-50

Standard errors in parentheses

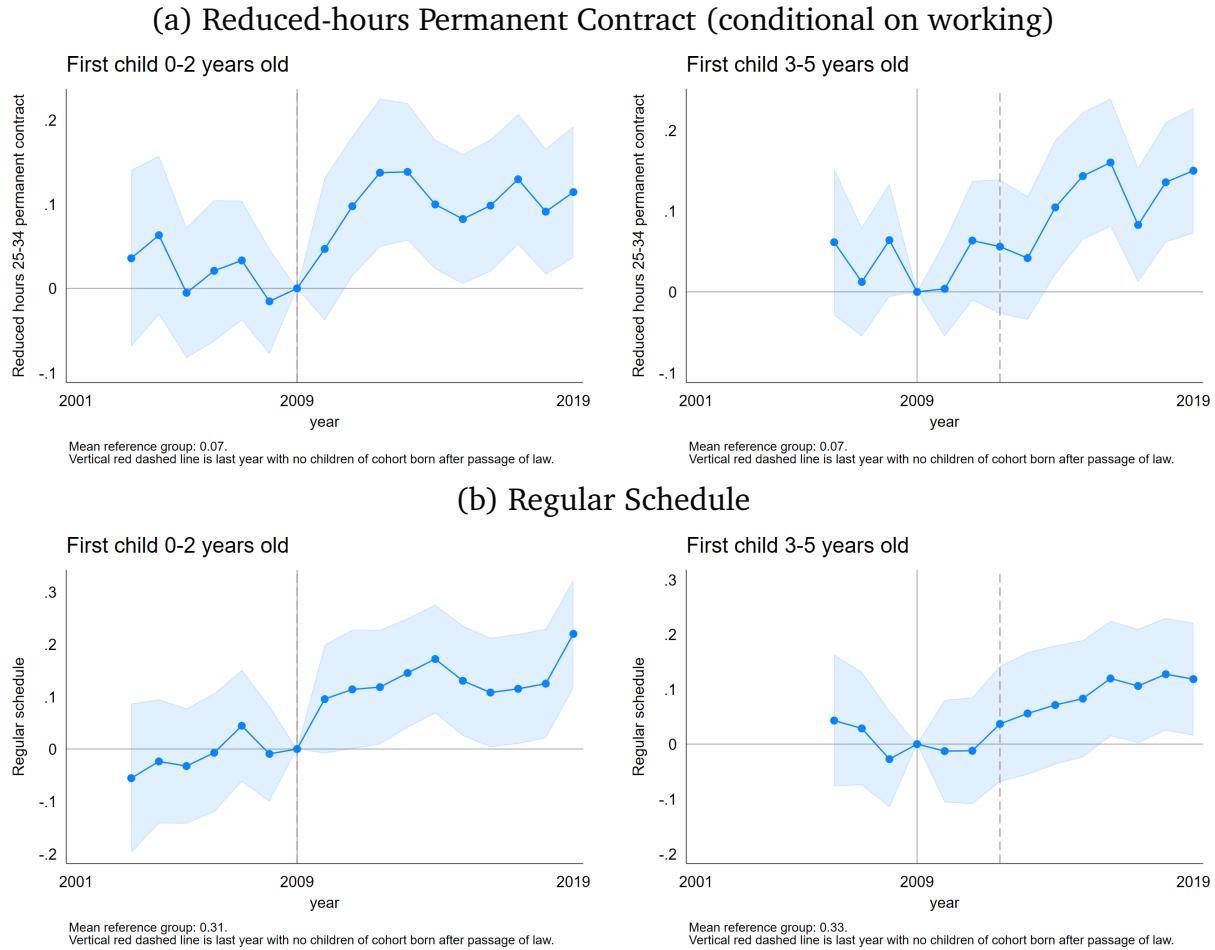
⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The table reports coefficient estimates from regressing a dummy for being employed (columns 1-2), number of hours conditional on working (columns 3-4), and the logarithm of hourly wages (columns 5-6) on an indicator for being a woman, an indicator for being of childbearing age (20-40), an indicator for the year being after 2009 (the year of the Fair Work Act), all the pairwise interactions and the triple interaction. We also control for age and year fixed effects. In odd-numbered columns the sample is comprised of individuals aged 20 to 65; in even-numbered columns of individuals aged 25 to 50.

D Further Evidence on the Effect of the Fair Work Act on Work Arrangements

D.1 Visual Evidence of Change in Work Arrangements of Mothers

Figure D.1: Change in Work Arrangements for Mothers Relative to Non-Mothers, by Year and Age of Child



Note: This figure shows the change in work arrangements for mothers relative to non-mothers over time. In panel (a) (top row), the outcome variable is the probability of having a reduced-hours permanent contract conditional on working; in panel (b) (bottom row), the outcome is a dummy for having a regular schedule. As in Figure 1, we are plotting the estimates of $\beta_{a,j}$ from Equation (1). The left panels display the estimates for the coefficients on the dummy of the first child being aged 0-2 interacted with calendar years, the right panel 3-5. 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).

D.2 No Effects of the Fair Work Act on Fathers' Work Arrangements

Here we report the result of running the same empirical analysis of Section 3.3 on men. Although the Fair Work Act is gender-neutral and its provisions apply to both parents, we don't find any changes in men's work arrangements: all the coefficients in Table D.1 are small in magnitude and not significantly different from zero.

Table D.1: Changes in Work Arrangements for Fathers

	(1) Reduced- hours Permanent (Uncond.)	(2) Reduced- hours Permanent (Cond.)	(3) Less-than- Full-Time Permanent (Uncond.)	(4) Regular Schedule	(5) On call	(6) My working times can be flexible (yes /no)	(7) Any hour worked from home
First child 0-2 × Post-2009	-0.00202 (0.00440)	-0.00478 (0.00577)	-0.00656 (0.00590)	0.0217 (0.0180)	0.00288 (0.0110)	0.0273 (0.0217)	-0.0136 (0.0161)
First child 3-5 × Post-2012	0.000870 (0.00641)	-0.00197 (0.00859)	-0.00484 (0.00759)	0.0123 (0.0213)	-0.00914 (0.0124)	0.0189 (0.0229)	0.000570 (0.0189)
Pre-period mean:							
First child aged 0-2	0.0149	0.0197	0.0231	0.586	0.0791	0.510	0.251
First child aged 3-5	0.0188	0.0251	0.0246	0.623	0.0853	0.519	0.274
Observations:							
N	113454	72264	113454	89605	89621	62311	98520
N Individuals	11995	8852	11995	10063	10065	8200	11182

Standard errors in parentheses

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

Note: This tables replicates Table 2 for men. See note to Table 2 for details.

E Threats to Identification in the Early versus Late Cohort Design

Section 5.1 quantifies the change in the child penalties around the time of the Fair Work Act. Under the assumption that the late cohorts of mothers would have behaved like the early ones absent the law, this comparison recovers the causal estimate of the change in work arrangement brought by the Fair Work Act on the child penalty. In this Appendix, we address potential threats to this assumption. First, we address the possibility that the financial crisis might have increased labor supply of mothers through the added-worker effect (Appendix E.1). Second, we discuss other provisions of the Fair Work Act and argue that none of them differentially impacted mothers relative to other women, and thus do not violate our identification assumption, given that we always keep non-mothers as controls (Appendix E.2).

E.1 Accounting for the Financial Crisis (Continued)

In this Appendix we show that controlling for men's employment and earnings, our estimates for the early-versus-late cohorts comparison remain almost identical (if anything, they become stronger), supporting the fact that the results shown in Section 5.1 are not driven by women's increasing supply in response to their male partner's losing their job (added-worker effect).

Tables E.1 and E.2 show that the estimated effect of the Fair Work Act on the child penalty in the probability of having a permanent contract and in hours of work is practically unchanged or becomes stronger when including controls for male partner's labor supply. The estimated effect for child penalty in labor force participation becomes slightly smaller, but remains large and significant, as shown in Table E.3.

Table E.1: Robustness of Early versus Late Cohort Comparison to Partner's Labor Supply and Income (Added-Worker Effect): Permanent Contract

	(1) Permanent contract (condi- tional) (woman)	(2) Permanent contract (condi- tional) (woman)	(3) Permanent contract (condi- tional) (woman)	(4) Permanent contract (condi- tional) (woman)	(5) Permanent contract (condi- tional) (woman)	(6) Permanent contract (condi- tional) (woman)	(7) Permanent contract (condi- tional) (woman)
Child Penalty 0-7	-0.253*** (0.0316)	-0.222*** (0.0369)	-0.222*** (0.0369)	-0.222*** (0.0369)	-0.237*** (0.0373)	-0.237*** (0.0374)	-0.237*** (0.0373)
Child Penalty 0-7 * Post-2009 First Birth	0.176*** (0.0400)	0.207*** (0.0448)	0.207*** (0.0448)	0.207*** (0.0448)	0.218*** (0.0450)	0.218*** (0.0450)	0.218*** (0.0450)
Employed (man)		0.0573*** (0.0143)	0.0533*** (0.0152)	0.0403* (0.0174)			
Hours per week usually worked in all jobs (man)					0.000617 ⁺ (0.000364)	0.000586 (0.000380)	0.000565 (0.000371)
Weekly labor earnings (000) (man)			0.00385 (0.00644)			0.00202 (0.00713)	
Log(1+weekly labor earnings) (man)				0.00295 (0.00190)			0.00214 (0.00207)
Pre-birth mean:							
Early cohorts	0.729	0.791	0.791	0.791	0.811	0.811	0.811
Late cohorts	0.738	0.787	0.787	0.787	0.785	0.785	0.785
Observations:							
N	49304	23865	23865	23865	21540	21540	21540
N Individuals	6783	3659	3659	3659	3409	3409	3409
N New Parents	625	518	518	518	507	507	507

Standard errors in parentheses

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

Note: The table replicates column (1) of Table 3 (and is similarly run on women only) but includes controls for their male partner's employment status, hours of work and earnings.

Table E.2: Robustness of Early versus Late Cohort Comparison to Partner's Labor Supply and Income (Added-Worker Effect): Weekly Hours of Work Conditional on Working

	(1) Weekly hours in paid employment (condi- tional) (woman)	(2) Weekly hours in paid employment (condi- tional) (woman)	(3) Weekly hours in paid employment (condi- tional) (woman)	(4) Weekly hours in paid employment (condi- tional) (woman)	(5) Weekly hours in paid employment (condi- tional) (woman)	(6) Weekly hours in paid employment (condi- tional) (woman)	(7) Weekly hours in paid employment (condi- tional) (woman)
Child Penalty 0-7	-18.72*** (0.878)	-17.95*** (1.019)	-17.95*** (1.018)	-17.96*** (1.019)	-18.13*** (1.038)	-18.12*** (1.033)	-18.15*** (1.037)
Child Penalty 0-7 * Post-2009 First Birth	4.347*** (1.094)	5.115*** (1.237)	5.124*** (1.235)	5.122*** (1.237)	5.482*** (1.257)	5.523*** (1.251)	5.503*** (1.257)
Employed (man)		0.706+ (0.362)	0.939* (0.393)	0.995* (0.460)			
Hours per week usually worked in all jobs (man)					0.111*** (0.0143)	0.121*** (0.0147)	0.113*** (0.0144)
Weekly labor earnings (000) (man)			-0.232 (0.149)			-0.711*** (0.161)	
Log(1+weekly labor earnings) (man)				-0.0518 (0.0486)			-0.127* (0.0505)
Pre-birth mean:							
Early cohorts	38.95	40.78	40.78	40.78	41.01	41.01	41.01
Late cohorts	37.49	39.64	39.64	39.64	39.65	39.65	39.65
Observations:							
N	55275	27762	27762	27762	25227	25227	25227
N Individuals	7291	4051	4051	4051	3786	3786	3786
N New Parents	651	551	551	551	537	537	537

Standard errors in parentheses

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

Note: The table replicates column (4) of Table 3 (and is similarly run on women only) but includes controls for their male partner's employment status, hours of work and earnings.

Table E.3: Robustness of Early versus Late Cohort Comparison to Partner's Labor Supply and Income (Added-Worker Effect): Labor Force Participation

	(1) In labor force (woman)	(2) In labor force (woman)	(3) In labor force (woman)	(4) In labor force (woman)	(5) In labor force (woman)	(6) In labor force (woman)	(7) In labor force (woman)
Child Penalty 0-7	-0.380*** (0.0226)	-0.328*** (0.0264)	-0.327*** (0.0264)	-0.328*** (0.0265)	-0.333*** (0.0262)	-0.332*** (0.0261)	-0.333*** (0.0262)
Child Penalty 0-7 * Post-2009 First Birth	0.0801** (0.0292)	0.0689* (0.0324)	0.0694* (0.0323)	0.0691* (0.0324)	0.0665* (0.0322)	0.0675* (0.0321)	0.0667* (0.0322)
Employed (man)		0.133*** (0.0114)	0.144*** (0.0119)	0.144*** (0.0133)			
Hours per week usually worked in all jobs (man)					0.000790** (0.000274)	0.00104*** (0.000279)	0.000823** (0.000275)
Weekly labor earnings (000) (man)			-0.0107* (0.00421)			-0.0167*** (0.00438)	
Log(1+weekly labor earnings) (man)				-0.00190 (0.00128)			-0.00160 (0.00130)
Pre-birth mean:							
Early cohorts	0.920	0.955	0.955	0.955	0.959	0.959	0.959
Late cohorts	0.927	0.954	0.954	0.954	0.960	0.960	0.960
Observations:							
N	83613	39542	39542	39542	32661	32661	32661
N Individuals	9543	5175	5175	5175	4483	4483	4483
N New Parents	767	680	680	680	648	648	648

Standard errors in parentheses

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

Note: The table replicates column (5) of Table 3 (and is similarly run on women only) but includes controls for their male partner's employment status, hours of work and earnings.

E.2 Other National Employment Standards Provisions

The Fair Work Act established a list of provisions that all awards must include, i.e. minimum employment standards that must be provided to all employees, called National Employment Standards (NES). This Appendix lists them and discusses why all of them (except for the provision we study) are either not expected to impact mothers differentially relative to other women, and therefore their potential impact is controlled for by the inclusion of non-mothers as controls, or they are quantitatively irrelevant.

The list of National Employment Standards provisions is the following⁴⁰:

1. maximum weekly hours
2. requests for flexible working arrangements
3. parental leave and related entitlements
4. annual leave
5. personal/carer's leave and compassionate leave
6. community service leave
7. long service leave
8. public holidays
9. notice of termination and redundancy pay
10. Fair Work Information Statement.

The provision we are studying is the second one. The third one (parental leave and related entitlements) is the only one that could, in principle, impact our analysis since it potentially impacts mothers differently than anyone else, but it turns out to be quantitatively irrelevant, as we argue below. The cap on maximum weekly hours was already present in Australian legislation⁴¹, and the Fair Work Act only strengthened the related protections to employees. This provision, together with the ones regulating community service leave and public holidays, applies broadly to all workers, and we don't expect them to impact mothers and non-mothers differentially. The Fair Work Information Statement is a publication that contains, among others, information about the National Employment Standards, the modern Awards, the role of Fair Work Act, and that needs to be distributed to workers when hired.

Annual (vacation) leave, personal/carer's leave and compassionate leave, and notice of termination and redundancy pay apply to all workers except casual employees, but they apply to both men and women, and to both parents and non-parents, in the same way. Therefore, these are "controlled for" in all our specifications by the inclusion of non-parents as controls in our analysis.

⁴⁰The full text of the Fair Work Act, as approved in 2009, can be found at this link: <https://www.legislation.gov.au/C2009A00028/2009-07-01/text>

⁴¹The 38 hours workweek was introduced in 1983, as described on this government website: <https://www.fairwork.gov.au/about-us/workplace-laws/fair-work-system/australias-industrial-relations-timeline>

E.2.1 Parental Leave and Related Entitlements

This provision gives workers the right to request up to 12 months of extension to the 12 months of unpaid parental leave they are entitled to, for a total of 24 months.⁴²

While the first twelve months are an entitlement, the extension is not: the employee needs to send a request to his or her employer, who has to agree to the extension. The extension cannot be longer than 12 months and needs to immediately follow the end of the available parental leave period.

On ending unpaid parental leave, an employee is entitled to return to the pre-parental leave position, or, if that job no longer exists, “an available position for which the employee is qualified and suited nearest in status and pay to the pre-parental leave position.”

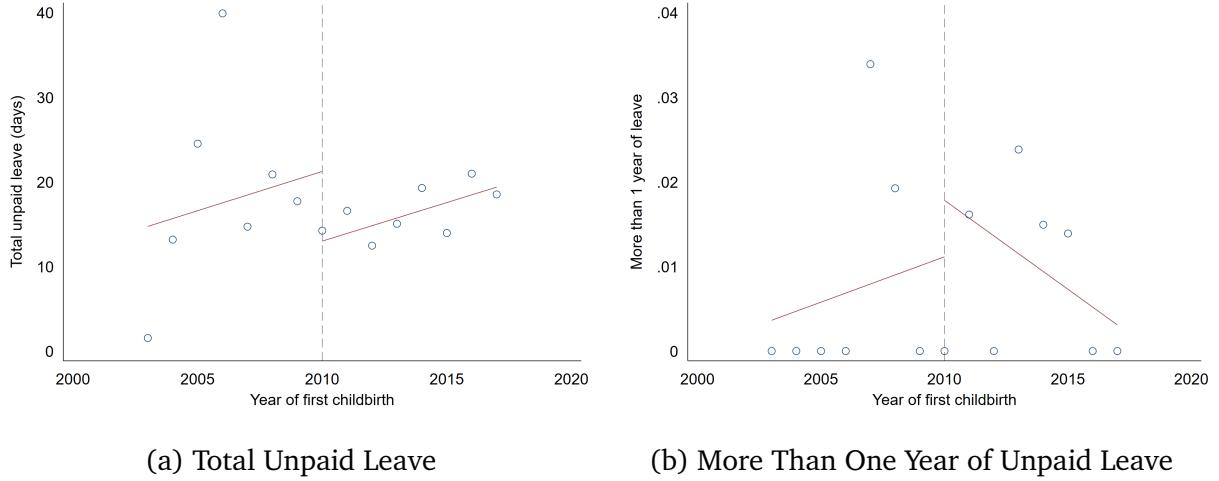
Hence, in order to understand whether this provision can confound our results, we verify whether it led to an increase in average leave length, beyond the 12 months parents were already entitled to. First, we note that the government report that evaluates this aspect of the Fair Work Act (O’Neill, 2012) finds that a very small fraction (less than one-quarter of a percent) of all employees surveyed were considered to have made a request for an extension of unpaid parental leave under the National Employment Standards beyond the 12 months entitlement.

In order to confirm the irrelevance of this legislative change for working mothers in our data, we compute the total number of unpaid leave days taken in the year of first childbirth, and the two subsequent years. Figure E.1 shows the time evolution of the total days of unpaid leave in this three-year window for the subsample of women who did not have a second child in the two years after the first one⁴³. The left panel shows the total number of days of unpaid leave, and we can see that the average is around 20 days, far below the maximum leave length that can be taken without the employer’s agreement and which was available before the Fair Work Act. The right panel shows the share of women taking more than one year of leave: the numbers are very small (and actually zero for most of the years), further validating the irrelevance, for our results, of the above-described provision.

⁴²The entitlement to the first 12 months of job-protected unpaid leave has been available to Australian workers covered by the award system since 1979, as highlighted in the timeline of Australian industrial relations here: <https://www.fairwork.gov.au/about-us/workplace-laws/fair-work-system/australias-industrial-relations-timeline>

⁴³The share of women who have a second child within two years from the year in which they gave birth for the first time is unchanged around 2009, as shown in Appendix I.4.

Figure E.1: No Variation in Unpaid Leave Take-Up



Note: This figure plots the average number of days of unpaid leave (panel a) and the probability that leave taken is longer than one year (panel b) in the year of first birth and in the following two years, by year of the birth of the first child.

F Paid Parental Leave

In this Appendix, we rule out the possibility that the introduction of a federal Paid Parental Leave in Australia in 2010 may have been the cause of the effects we observe. In order to do this we rely on the fact that approximately half of the Australian workforce (and of our sample) had already access to paid parental leave through their employer before the federal reform. Running our analysis on this sample we find unchanged results. The absence of a major effect of Paid Parental Leave on the child penalty in labor supply is consistent with findings in the literature (Olivetti and Petrongolo, 2017), and reasonable in a setting in which Paid Parental Leave replaced a previously existing job-protected unpaid leave and a cash transfer for newborns (thus not dramatically changing incentives for new mothers).

F.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 newborns, 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 it applies since January 1, 2011), allowing 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. The Paid Parental Leave could not be combined with the Baby Bonus, an unconditional and tax-exempt cash transfer that was available to all families who gave birth to or adopted a child. While the Paid Parental Leave 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 Paid Parental Leave is paid at the minimum wage regardless of pre-birth earnings) and were more likely to be already 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 (as mentioned also in Appendix E.2), so the 2010 reform did not change this aspect either. In conclusion, it seems that this provision 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 (2020); de Gendre, Lynch, Meunier, Pilkington, and Schurer (2021); Kalb (2018).

F.2 Empirical Strategy and Results

In order to study whether Paid Parental Leave affected the change in child penalty we document, we leverage the fact that before its introduction, 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, any change we observe for this group of women can be attributed to the Fair Work Act (and not Paid Parental Leave).

We repeat the analyses of Tables 3 and 4 on the subsample of women who, two years before childbirth, were entitled to employer-provided paid parental leave. Results are presented in Tables F.1 and F.2 respectively: while we lose some statistical power since we are working on a smaller sample (especially for the second table, where we are looking at heterogeneities by exposure to the law), all coefficients have the same sign and similar magnitudes.

Table F.1: Child Penalty for Early and Late Cohorts of Mothers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Perma-nent contract (unconditional)	Perma-nent contract (condi-tional)	Regular schedule	Weekly hours in paid em-ployment (condi-tional)	In labor force	Employed	Total time house-work	Total time parenting
Child Penalty 0-7	-0.417*** (0.0422)	-0.186*** (0.0428)	-0.485*** (0.0502)	-18.69*** (1.277)	-0.313*** (0.0337)	-0.341*** (0.0341)	12.21*** (1.334)	44.04*** (1.886)
Child Penalty 0-7 * Post-2009 First Birth	0.171** (0.0550)	0.139** (0.0530)	0.157** (0.0593)	3.786* (1.499)	0.0583 (0.0406)	0.0850* (0.0412)	-3.263* (1.538)	0.654 (2.280)
Pre-birth mean:								
Early cohorts	0.766	0.789	0.744	41.32	0.981	0.977	13.90	0.190
Late cohorts	0.749	0.796	0.722	39.08	0.980	0.961	13.14	0.240
Observations:								
N	14677	12376	12831	12814	14677	14677	13414	12355
N Individuals	1069	1057	1066	1066	1069	1069	1067	1066
N New Parents	244	240	241	241	244	244	243	243

Standard errors in parentheses

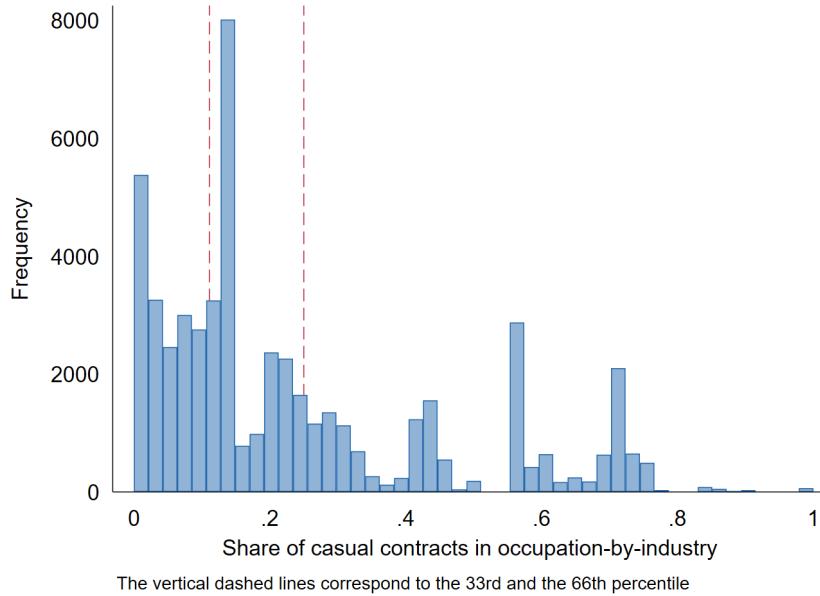
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table replicates Table 3 on the subsample of women who, two years before childbirth, were entitled to employer-provided paid parental leave.

G Further Details on the Exposure Design

G.1 Distribution of Casual Prevalence

Figure G.1: Distribution of Casual Prevalence



G.2 Characteristics of Jobs in the Three Terciles

In this appendix, we discuss similarities and differences of the jobs in the three terciles of casual prevalence. Including only data until 2008, we notice two main characteristics. First, jobs with lower casual prevalence are associated with higher pay, longer hours, and greater occupational status. Second, while there are differences between jobs in the bottom and in the middle tercile, they are substantially smaller than the differences with jobs in the top tercile. This is particularly true when we narrow the focus to women and to their fertility choices. This lends justification to our choice of using women with pre-birth job in the bottom tercile of casual prevalence as our preferred comparison group for treated women in the middle tercile.

Occupational Characteristics and “Greediness” of Jobs The top panel of Table G.1 shows that wages, occupational prestige, and weekly work hours are highest in the bottom tercile of casual prevalence, and monotonically decrease in casual prevalence. We also observe a similar pattern for the probability of being on a regular schedule – which is unsurprising, given the strong correlation between a permanent contract and the probability of having a regular schedule.

We also construct four measures to summarize the “greediness” (or inflexibility) of jobs, mirroring Goldin (2014); they indicate that lower casual prevalence is correlated with

higher greediness. The four characteristics are Time pressure, Establishing and maintaining interpersonal relationships, Structured vs unstructured work, and Freedom to make decisions.⁴⁴ For each of these job characteristics, we identify in the HILDA Survey questions that match the O*NET question used by Goldin (2014)⁴⁵. The variable “Flex Score” averages them all. Following Goldin (2014), each of these has been coded so that a higher value of the variables denotes a job that has less flexibility, more interpersonal contact, less independence in decision-making, and so on, and thus is “greedier”. Each of these characteristics should produce the convex relationship between hours and earnings that Goldin (2014) identifies as an explanation for the residual gender earnings gap. In all of these measures, except for time pressure, jobs at the bottom of the distribution of casual prevalence score higher, and jobs at the top have the lowest score.

These characteristics depict a picture of jobs in the bottom tercile of casual prevalence as being career-oriented jobs, with extended hours and possibly convex returns to hours, comparable to Goldin’s “greedy” jobs. Jobs in the middle tercile appear as good white-collar jobs, similar to those in bottom tercile except for the greediness. Finally, jobs in the top tercile of casual prevalence are irregular, low-hours and low-pay jobs.

The reform we study does not fundamentally change the nature of greedy jobs; instead, it introduces a change in one specific aspect of work arrangements that relates to the ability to combine family and career. The Fair Work Act did not make greedy jobs less greedy, it did not linearize convex returns to hours, to maintain Goldin’s terminology. It instead acted where there was room for improvements, in jobs that could in principle be more mother-friendly but were not due to institutional practices. Thus, it should not be surprising to notice that the most affected jobs were not the greediest, i.e. the ones of the bottom tercile, where the contractual arrangements probably did not matter, but the intermediate ones of the middle tercile.

Comparability of Mothers Between Bottom and Middle Tercile The bottom panel of Table G.1 shows that women in jobs in the bottom and in the middle tercile of casual prevalence make similar fertility choices, thus mitigating potential concerns of lack of comparability between these two groups. Women with jobs in the bottom tercile of casual prevalence on average have their first child at 32, close to the 31 of women with jobs in the middle tercile, but much older than the 28.6 of the women with jobs in the top tercile of casual prevalence. They also have similar fertility patterns: the average number of children three, five, and seven years after the first is not statistically different between any of these groups. While the similarity of women with pre-birth jobs in the bottom and in the middle tercile of casual prevalence is not required by our empirical strategy (our design only requires a triple-difference version of parallel trends, as detailed in Sections 5.2.2 and 5.2.6), this similarity is indeed reassuring on the validity of our strategy.

⁴⁴Goldin (2014) also includes Contact with others, for which no equivalent question was found in the HILDA Survey.

⁴⁵When comparing our measures with the O*NET ones, applied to our data by merging the occupation codes, we find very high correlation for Freedom in decision-making and Structured vs unstructured, somehow lower for the other two.

Table G.1: Job Characteristics by Tercile

	Bottom tercile		Middle tercile		Diff Middle-Bottom		Top tercile		Diff Top-Bottom	
	Mean	SD	Mean	SD	Diff.	SE	Mean	SD	Diff.	SE
<i>All Employees</i>										
Avg hourly wage (2021 dollars)	37.22	(33.50)	31.69	(29.49)	-5.53***	(0.30)	22.57	(24.47)	-14.65***	(0.29)
Avg occupational status	60.34	(19.60)	54.02	(24.04)	-6.32***	(0.21)	30.55	(12.74)	-29.78***	(0.16)
Has bachelor degree or above	0.35	(0.48)	0.32	(0.47)	-0.03***	(0.00)	0.08	(0.27)	-0.27***	(0.00)
Avg weekly working hours	42.75	(12.71)	37.72	(14.46)	-5.04***	(0.13)	30.95	(17.91)	-11.80***	(0.15)
Avg tenure with current employer	7.36	(8.09)	7.22	(8.15)	-0.15	(0.08)	4.92	(7.48)	-2.45***	(0.08)
Share of casual contracts	0.05	(0.21)	0.17	(0.37)	0.12***	(0.00)	0.51	(0.50)	0.46***	(0.00)
Share of regular schedule	0.62	(0.48)	0.48	(0.50)	-0.14***	(0.00)	0.25	(0.43)	-0.38***	(0.00)
Share of on call	0.10	(0.30)	0.10	(0.30)	0.00	(0.00)	0.16	(0.36)	0.06***	(0.00)
Share on flexible start/finish times	0.69	(0.46)	0.48	(0.50)	-0.20***	(0.01)	0.55	(0.50)	-0.14***	(0.01)
Share of women	0.36	(0.48)	0.54	(0.50)	0.18***	(0.00)	0.54	(0.50)	0.18***	(0.00)
Avg Freedom Decision	0.22	(0.33)	0.04	(0.31)	-0.19***	(0.00)	-0.27	(0.39)	-0.50***	(0.00)
Avg Unstructured work	0.22	(0.39)	-0.00	(0.30)	-0.22***	(0.00)	-0.20	(0.41)	-0.41***	(0.00)
Avg Importance of Relationships	0.22	(0.42)	-0.00	(0.33)	-0.23***	(0.00)	-0.23	(0.27)	-0.46***	(0.00)
Avg Time Pressure	-0.01	(0.19)	0.05	(0.21)	0.07***	(0.00)	-0.06	(0.18)	-0.05***	(0.00)
Avg Flex Score	0.17	(0.27)	0.01	(0.21)	-0.16***	(0.00)	-0.21	(0.20)	-0.38***	(0.00)
<i>Women in occupational tercile two years before childbirth</i>										
Avg age at first birth (women)	31.98	(4.85)	31.03	(4.77)	-0.95***	(0.15)	28.59	(5.05)	-3.39***	(0.15)
Avg num of children 3 years after first (women)	1.60	(0.54)	1.68	(0.58)	0.07	(0.08)	1.59	(0.55)	-0.02	(0.08)
Avg num of children 5 years after first (women)	1.88	(0.58)	2.01	(0.63)	0.12	(0.08)	1.85	(0.56)	-0.04	(0.08)
Avg num of children 7 years after first (women)	2.00	(0.60)	2.11	(0.74)	0.11	(0.09)	1.95	(0.65)	-0.05	(0.09)

Note: This table shows means and differences in means for various characteristics of jobs, divided into the three terciles of casual prevalence as defined in Section 5.2. In the top panel we include all employees (with no distinction of gender) and all observations between 2001 and 2008. In the bottom panel we restrict to women who were in a given occupational tercile two years before the birth of their first child, also between 2001 and 2008. The definition of the main variables is detailed in Appendix A; Goldin's flexibility characteristics (Freedom Decision, Unstructured Work, Importance of Relationship, Time Pressure, and Flex Score) are described in the text above.

H Additional Evidence from the Exposure Design

In this appendix, we provide further evidence on the (weakly positive) career's impact of the Fair Work Act and highlight a specific mechanism at play in the transition from casual to permanent reduced hours contracts, which rules out a simple explanation of exposed mothers simply getting closer to their desired number of hours of work. In particular, we do not document statistically significant changes to careers' trajectories of most exposed mothers, but some specifications might point to a gradual improvement. In terms of mechanisms, we show that the increase in hours of work of the most exposed mothers does not bring them closer to their desired number of hours - the earlier cohorts of mothers, with a casual contract, *wanted* to work the fewer hours they did because, with an irregular schedule, every hour worked was more costly (e.g. every hour actually worked corresponding to more than one hour being on call).

H.1 Career's (Lack of) Impact of the Fair Work Act

Here we present and discuss the following piece of evidence: despite the permanent positive effect of the law on maternal labor supply, the Fair Work Act does not seem to have had large effects on careers conditional on working; at least not large enough to be captured with survey data, though there is some noisy suggestive evidence consistent with a positive effect.

The self-reported probability of having been promoted in the previous 12 months does not change systematically for the most exposed mothers in the seven years following the birth of their first child; however, the event-study noisily suggests a positive effect in the first year after childbirth. The triple-difference coefficient of interest (highlighted in blue in Table H.1) shows that on average, in the seven years following the first childbirth, the child penalty in the probability of being promoted is not systematically different for the most exposed mothers (Tercile 2) relative to the less exposed mothers (Tercile 1), who gave birth after the passage of the law relative to before. This holds both conditional on current employment (column 2) and unconditionally (column 4). However, there is one noticeable caveat: the event-study in panel (a) of Figure H.1 shows that for the most exposed mothers (middle tercile, the central panel in orange) there is a significant difference in the probability of being promoted in the year following the first childbirth, with later (post-Fair Work Act) cohorts significantly more likely to report no post-childbirth change in the probability of promotion. This might be economically meaningful, but since the estimates are noisy, we do not want to over-interpret it.

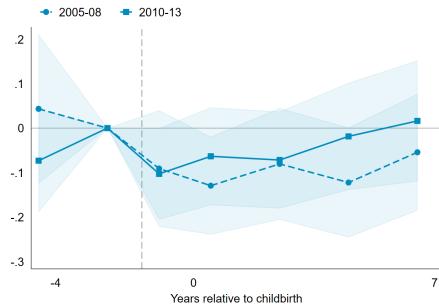
The probability of supervising other employees does not change on average for the most exposed mothers, but the event-study suggests a dynamic effect, in the direction of greater probability of being in a supervisory role for the mothers exposed to the Fair Work Act. In Table H.1, the point estimates from the triple-difference specification suggest that the child penalty in the probability of having a supervisory role in the seven years following the first childbirth does not change differentially for the more and less exposed mothers, neither conditional on working (column 6), nor unconditionally (column 8). Panel (b) of Figure H.1 though, shows that this average might be hiding a dynamic effect: for the most

exposed group (middle tercile, in orange in the middle panel), a progressive shifting away from supervisory roles after childbirth evident in the early (pre-Fair Work Act) cohorts is no longer happening for the post-Fair Work Act cohorts. While the evidence is suggestive, the difference is only statistically significant in a single year (five years from first childbirth), and thus we are hesitant to extrapolate a trend from it.

Figure H.1: Work Hours and Career Progression of Women around Childbirth, Before and After Reform, by Exposure to the Fair Work Act

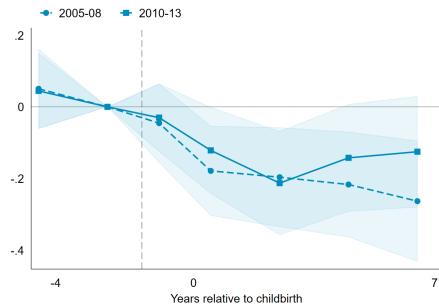
Bottom Tercile

(a) Promoted in The Past Year (Conditional on Current Employment)



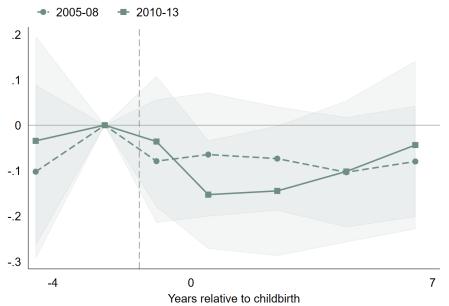
Middle Tercile

(b) Supervise Other Employees (Conditional)

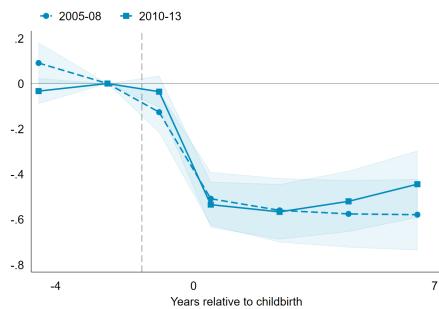


Top Tercile

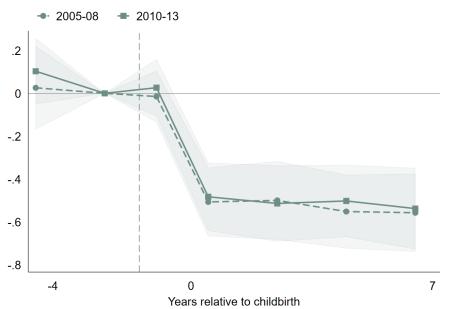
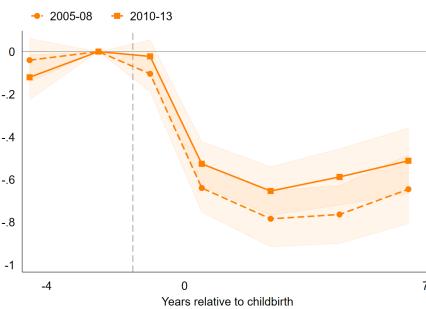
(c) Full-time (35+) Conditional



(c) Full-time (35+) Conditional



Years relative to childbirth



Years relative to childbirth

Note: This figure shows the child penalty in the probability of having been promoted in the prior year (panel a), in the probability of supervising other employees at work (panel b), and in the probability of working full-time (35 hours per week or more) (panel c) by early and late cohorts and by terciles of casual prevalence. All outcomes are conditional on current employment. See notes to Figures 6 and 7 for details.

Table H.1: Changes in Child Penalty by Exposure to the Fair Work Act - Women (Career)

	(1) Promoted in the past year (conditional on current employment)	(2) Promoted in the past year (conditional on current employment)	(3) Promoted at work in the past year (un- conditional)	(4) Promoted at work in the past year (un- conditional)	(5) Supervise other employees (conditional)	(6) Supervise other employees (conditional)	(7) Supervise other employees (uncondi- tional)	(8) Supervise other employees (uncondi- tional)
Child Penalty 0-7	-0.0588** (0.0189)	-0.0618 ⁺ (0.0325)	-0.0774*** (0.0155)	-0.0693* (0.0288)	-0.154*** (0.0250)	-0.175*** (0.0412)	-0.264*** (0.0218)	-0.280*** (0.0386)
* Tercile = 2		0.00337 (0.0453)		-0.0116 (0.0390)		0.0558 (0.0589)		0.0307 (0.0539)
* Tercile = 3		0.00215 (0.0462)		-0.00522 (0.0374)		0.00799 (0.0653)		0.0268 (0.0544)
Child Penalty 0-7 * Post-2009 First Birth	-0.0102 (0.0218)	0.0159 (0.0383)	-0.00487 (0.0179)	-0.00840 (0.0331)	0.0575 ⁺ (0.0295)	0.0297 (0.0476)	0.0917*** (0.0260)	0.0478 (0.0446)
* Tercile = 2		-0.0193 (0.0521)		0.0159 (0.0444)		0.0175 (0.0664)		0.0623 (0.0611)
* Tercile = 3		-0.0604 (0.0552)		-0.00808 (0.0442)		0.0781 (0.0827)		0.0555 (0.0685)
Early (pre-2010) cohorts: Mean Y pre-birth	0.160		0.148		0.553		0.501	
- tercile 1		0.205		0.187		0.621		0.586
- tercile 2		0.147		0.151		0.509		0.472
- tercile 3		0.113		0.0970		0.515		0.432
Late (post-2010) cohorts: Mean Y pre-birth	0.167		0.157		0.493		0.457	
- tercile 1		0.211		0.205		0.552		0.537
- tercile 2		0.148		0.143		0.451		0.429
- tercile 3		0.130		0.113		0.479		0.391
Early cohorts: New parents	340		401		366		415	
- tercile 1		122		133		128		134
- tercile 2		118		133		127		138
- tercile 3		100		135		111		143
Late cohorts: New parents	625		698		671		734	
- tercile 1		212		226		223		234
- tercile 2		262		284		283		296
- tercile 3		151		188		165		204
Tot observations	20052	20051	27135	27135	23605	23604	31786	31786

Standard errors in parentheses

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

Note: This table reports the estimates of the child penalty in career progression for women and its interaction with treatment status (defined by the tercile of casual prevalence of a woman's modal job in the five years before the birth of her first child). More details in the note to Table 4. The outcome in columns (1) through (4) is a dummy for having been promoted in the previous 12 months, both conditionally on being employed at the time of answering (columns 1 and 2) and unconditionally (columns 3 and 4). The outcome in columns (5) through (8) is a dummy for having a supervisory role at work, both conditionally on working (columns 5 and 6) and unconditionally (columns 7 and 8). A detailed variable description is in Appendix A.

H.2 Further Evidence on Compliers and on Mechanisms

Here we emphasize two findings: (i) that mothers who would have counterfactually remained full-time after childbirth are not driven by the Fair Work Act into working less than full-time; and (ii) that the law does not act through moving mothers closer to their desired level of hours.

First of all, we show that the compliers to the law—who work on permanent reduced-hours after the Fair Work Act—are NOT mothers who would have counterfactually worked full-time. After the Fair Work Act, reducing hours is less costly, since it is no longer associated with an irregular schedule: thus, in principle, we could observe women working part-time who would have worked full-time absent the Fair Work Act. Instead, we do not see it happening: there is no decrease in the probability of working full-time; if anything, the point estimates point to the opposite direction. This can be seen in columns (2) and (4) of Table H.2, where we report the estimates of the triple-difference coefficients from Equation (5) (and the coefficients of interest are highlighted in blue), as well as in panel (c) of Figure H.1 (where the most exposed tercile is in orange in the middle).

Secondly, we provide evidence against the hypothesis that permanent contracts, by supposedly offering mothers more hours than the equivalent casual contracts, simply bring them closer to their optimal level of part-time hours. In this piece of analysis, we find that the gap between actual and desired number of hours is not systematically different for mothers in the middle tercile between early and late cohorts; if anything, the point estimate in the triple-difference points to the opposite direction. Similarly, when looking at whether they work too little (the gap between actual and desired hours is negative), we see that mothers in the middle tercile do not differ systematically between the early and late cohorts, and again the point estimate of the triple-difference points to an (insignificant) *increase* in the probability working too little relative to desire rather than a decrease, which would be implied by the aforementioned hypothesis. So, we can rule out the explanation that the permanent contracts increase mothers' hours simply by getting them closer to their desired level of hours.

We interpret these results as indicative of a change in mothers' perceived cost of each hour worked part-time. Before the reform, when the main option was a casual contract with irregular hours, mothers optimal level of hours (i.e., the desired number of hours) was lower, because each hour was more costly with an irregular schedule. After the Fair Work Act, when permanent contracts became more accessible, mothers' desired level of work hours increased, alongside actual worked hours.

Table H.2 makes these points. Columns (5) and (6) display the coefficient estimates from estimating equation (5) with the difference between actual and desired weekly hours of work as outcome variable, and columns (7) and (8) with a dummy that equals one when this difference is negative as outcome, meaning people report working less than they would like. Note that both these variables are only defined on people working. If the Fair Work Act acts by bringing mothers closer to their preferred number of hours, which is greater than what the casual contracts were affording them before, we would expect a positive coefficient in the triple-interaction term in column (6) (meaning new mothers work more relative to desired hours post-Fair Work Act) and a negative coefficient in the triple-interaction term in column (8) (meaning there are fewer mothers that report working

too little relative to desire). Instead, the triple-interaction coefficients, highlighted in blue, have point estimates of the opposite signs, which are not statistically different from zero. This suggests that the availability of permanent contracts does not increase maternal labor supply through offering hours that are closer to the desired amount, but it does so via some other channels, namely schedule regularity, that changes both the actual and the desired number of hours.

Table H.2: Changes in Child Penalty by Exposure to the Fair Work Act - Women (Compliers and Mechanisms)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full-time (35+) conditional	Full-time (35+) conditional	Full-time (35+) unconditional	Full-time (35+) unconditional	Gap between actual and desired weekly working hours (conditional)	Gap between actual and desired weekly working hours (conditional)	Too little work	Too little work
Child Penalty 0-7	-0.575*** (0.0247)	-0.532*** (0.0410)	-0.596*** (0.0209)	-0.599*** (0.0347)	-7.775*** (1.705)	-9.905*** (2.170)	0.270*** (0.0546)	0.293*** (0.0704)
* Tercile = 2			-0.122* (0.0567)	-0.0591 (0.0495)		1.664 (3.619)		-0.0214 (0.120)
* Tercile = 3		0.00753 (0.0645)		0.0973+ (0.0517)		13.02* (5.110)		-0.198 (0.175)
Child Penalty 0-7 * Post-2009 First Birth	0.0584* (0.0285)	0.0177 (0.0461)	0.0616* (0.0245)	0.00351 (0.0389)	4.643* (2.018)	7.873** (2.613)	-0.127+ (0.0681)	-0.182* (0.0882)
* Tercile = 2			0.110+ (0.0645)	0.111* (0.0558)		-3.123 (4.138)		0.0874 (0.143)
* Tercile = 3		0.0268 (0.0763)		0.0818 (0.0625)		-16.52* (6.449)		0.315 (0.225)
Early (pre-2010) cohorts: Mean Y pre-birth	0.846		0.767		10.22		0.112	
- tercile 1		0.923		0.872		12.26		0.0526
- tercile 2		0.838		0.782		12.77		0.0426
- tercile 3		0.745		0.618		2.333		0.333
Late (post-2010) cohorts: Mean Y pre-birth	0.849		0.780		10.97		0.154	
- tercile 1		0.919		0.892		13.75		0.0562
- tercile 2		0.852		0.813		11.03		0.139
- tercile 3		0.715		0.559		4.286		0.429
Early cohorts: New parents	365		415		122		122	
- tercile 1		128		134		51		51
- tercile 2		127		138		40		40
- tercile 3		110		143		31		31
Late cohorts: New parents	670		734		164		164	
- tercile 1		222		234		57		57
- tercile 2		283		296		75		75
- tercile 3		165		204		32		32
Tot observations	23520	23519	31792	31792	3749	3741	3749	3741

Standard errors in parentheses

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

Note: This table reports the estimates of the child penalty in various aspect of work hours for women and its interaction with treatment status (defined by the tercile of casual prevalence of a woman's modal job in the five years before the birth of her first child). More details in the note to Table 4. The outcome in columns (1) through (4) is a dummy for working full-time (≥ 35 hours per week), both conditionally on working (columns 1 and 2) and unconditionally (columns 3 and 4). The outcome in columns (5) and (6) is the difference between actual weekly hours of work and self-reported desired weekly hours of work. This is only defined on employed individuals and takes the value of zero when actual and desired weekly hours of work coincide. In columns (7) and (8), the outcome variable is a dummy that takes value of one if actual number of hours are less than desired number of hours and zero if they are the same or more.

I Threats to Identification in the Exposure Design

Our job exposure strategy from Section 5.2 relies on a triple-difference version of the parallel trend assumption: that the child penalties of mothers in the treated tercile would have evolved, over time, similarly to the ones of the other two groups. We lend support to this assumption in several ways. First, In Section I.1, we show parallel pre-trends in child penalties across terciles. Second, in Section I.2, we show that there isn't any selection into childbirth: early and late cohorts of mothers in the three groups are similar in terms of observables in the pre-birth years. Third, in Section I.3, we rule out selection into occupations by showing that our results are robust to narrowing the sample to two-year cohorts around the Fair Work Act and assigning women the exposure of the job they had three years before childbirth (thus the job they had before the Fair Work Act). Fourth, in Section I.4, we show that the Fair Work Act did not have an effect on fertility, and thus differential fertility is not a channel for the observed effect on labor supply.

I.1 Parallel Pre-trends

Here we show the evolution over time of the child penalty, separately for the three terciles of casual prevalence to whom mothers belong. The figures clearly show that the child penalties for bottom and middle terciles were evolving in parallel, though on different levels, before 2009; that there was a sharp change in 2009 for the middle tercile; and that after 2009, the child penalties for the bottom and middle tercile continue to evolve in parallel, at a closer level (in some cases identical) relative to before.

Empirically, we do the following: we fully interact Equation (4) with tercile-of-casual-prevalence dummies:

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

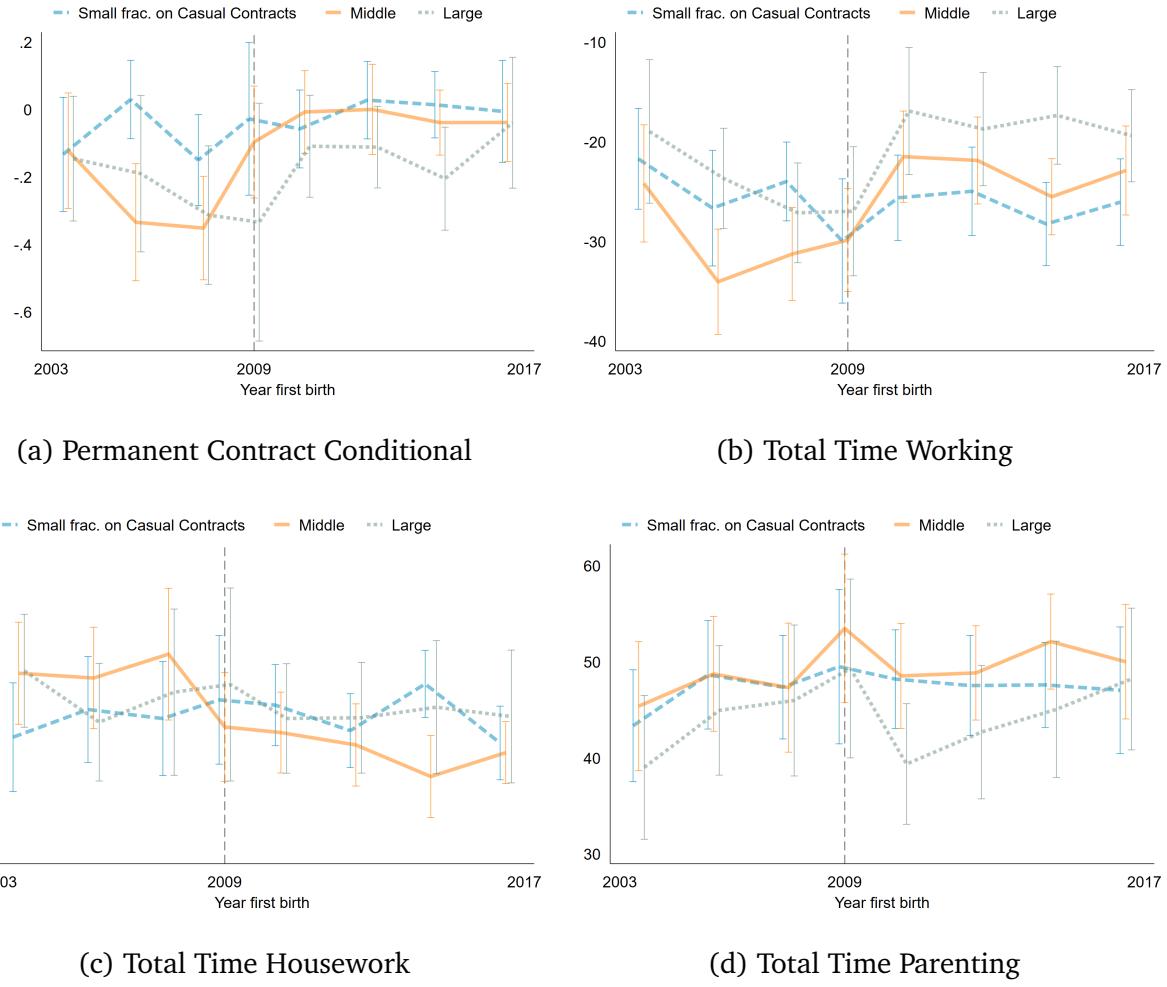
where $\mathbb{D}_{it} = \mathbb{1}\{(t - E_i) \in [0, 7]\}$ is an indicator function that takes value of one if i 's first child is aged 0 to 7. Here the coefficients of interest are $\gamma_{c,d}$, which indicate the child penalty in outcome Y for mothers who have had their first child in year c and who belong to tercile d . For power reasons, in this specification cohorts of mothers are biannual as in Section 5.1.4 (we group together mothers having their first child in 2003-04, 2005-06, etc.).

Figure I.1 replicates Figure 4 but separately by terciles of casual prevalence. In each panel, the blue dashed line shows the evolution of the child penalty for women with a pre-birth job belonging to the bottom tercile of casual prevalence (main control group), the orange solid line for women with pre-birth jobs in the middle tercile of casual prevalence (treated group), and the dashed grey line for the top tercile. Recall that the bottom and middle tercile are jobs that are closest in characteristics, and thus the orange-versus-blue (bottom versus middle tercile) is the most meaningful comparison.

The figure shows that the child penalties in permanent contract, labor supply, and housework for the bottom and middle tercile of casual prevalence were evolving in par-

allel until 2009; that for the middle tercile there was a sharp change in 2009; and that the child penalties for these two groups continued to evolve in parallel, at a closer level, afterward. Panel (a) shows the evolution of the child penalty in the probability of having a permanent contract. The blue dashed line shows us that this penalty was close to zero throughout the sample period for women in jobs with a very low share of casuals to start with: mothers who gave birth in 2005 and had a pre-birth job in the bottom tercile of casual prevalence were as likely to be in permanent contract after childbirth as before (conditional on working), and the same is true for women who became mothers in 2013 and had a pre-birth job in the bottom tercile of casual prevalence. The orange line shows that instead, for women who gave birth in 2005 or 2007 and had a job in the middle tercile of casual prevalence, there was a sizeable child penalty in this measure (around 30 percentage points), which was relatively stable; there was a sharp decrease in this child penalty around the 2009 cohort, and women with pre-birth jobs in this middle tercile who gave birth from 2010 onwards basically no longer experienced a child penalty in this dimension, exactly like women in the bottom tercile. The child penalties in labor supply (panel b) and time spent in housework (panel c) display analogous patterns; total time spent parenting (panel d) is very similar for the bottom and middle terciles and does not change around 2009, consistently with the results presented in the main text.

Figure I.1: Time Evolution of Child Penalty by Exposure to Fair Work Act



Note: This figure shows the evolution of the child penalty in contract type and in time use for women over the sample period, separately by levels of exposure to the Fair Work Act, as described in Section 5.2. Specifically, exposure of a job is defined as the share of casual contracts using pre-2009 observations; the exposure of a woman is that of the modal job (occupation-by-industry) she had in the five years before childbirth. The blue dashed lines are estimates for the bottom tercile (lowest casual prevalence), the orange solid lines are estimates for the middle tercile (the most exposed group), the grey dotted lines for the top tercile. More precisely, dots represent the estimates for $\gamma_{c,d}$ from Equation (6). 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 and with older children are included as controls, and exposure is assigned based on the modal job they had in the five years before a randomly-drawn fake childbirth year. We cluster standard errors at the individual level.

I.2 Early and Late Cohorts are Similar on Pre-Birth Observables in All Terciles

Here we show that the Fair Work Act did not induce selection into fertility, and thus the estimated effects of the Fair Work Act on the child penalties are not driven by a different selection of women who become mothers.

Table I.1 shows that early and late cohorts of will-be mothers are similar on pre-birth observables in all terciles. It also shows that women in the bottom and middle terciles are similar before childbirth. Women whose modal job in the five years before childbirth was in the bottom tercile of casual prevalence on average became mothers at 31 years old both in the early and late cohorts. Furthermore, two years before childbirth, they worked more than 40 hours a week, both in the early and late cohorts, and earned almost 50% of household income, again both in the early and late cohorts. Women whose modal pre-birth job was in the middle tercile of casual prevalence are very similar pre-birth, both between early and late cohorts and compared to women in the bottom tercile. Women in the bottom tercile are again very similar between early and late cohorts, but they are systematically different from will-be mothers of the other two terciles (for example, they become mothers at 28 and have significantly lower hourly wages).

Fertility trends are similar across terciles, and in particular between bottom and middle tercile. When regressing the fraction of women who become mothers on time fixed effects and tercile dummies, the trend in the middle and bottom tercile is identical both before and after the Fair Work Act (not shown, available upon request).

I.3 Robustness to Pre-Fair Work Act Job Choice

Our results are not driven by endogenous job switching in response to the Fair Work Act. The empirical strategy presented in Section 5.2 relies on grouping women in terciles of exposure to the Fair Work Act, based on their pre-birth job. Our main specification assigns each woman the modal job she had in the five years prior to the birth of her first child, and women's exposure is measured as the casual prevalence associated with such job. However, if will-be-mothers endogenously switch jobs in response to the Fair Work Act, this might bias our results. Here, we show that this is not the case and that our results are robust to assigning mothers jobs they had chosen before the Fair Work Act.

Restricting to jobs chosen before the Fair Work Acts yields results that are almost identical to our main specification. In Table I.2, we restrict the sample to women who became mothers in 2007-08 and 2010-11. In columns (1)-(4), we replicate the main columns of Table 4 on this sample. Here, we use the same treatment assignment as in the main text: each woman is assigned the modal job she holds in the five years prior to the birth of her first child, and she is assigned the tercile of casual prevalence relative to this job. In columns (5)-(8), instead, we base the tercile assignment on the job she held precisely three years before childbirth. This guarantees that all jobs used in the assignment are chosen before 2009, and thus could not be impacted by the Fair Work Act. Comparing column (2) with column (6) and column (4) with column (8) we see that the two specifications yield basically identical results. In addition, the coefficients of interest (highlighted in blue), are, if anything, larger in magnitude than the corresponding coefficients in the main table, indicating that endogenous job switching is not driving our main results.

Table I.2: Robustness to Only Using Jobs Chosen Before Fair Work Act

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main Treatment Definition				Alternative Treatment Definition			
	Permanent contract (uncond)	Permanent contract (uncond)	Total time work	Total time work	Permanent contract (uncond)	Permanent contract (uncond)	Total time work	Total time work
Child Penalty 0-7	-0.388*** (0.0371)	-0.323*** (0.0662)	-28.42*** (1.301)	-25.28*** (2.034)	-0.388*** (0.0371)	-0.333*** (0.0694)	-28.44*** (1.301)	-25.30*** (2.072)
* Tercile = 2			-0.162+ (0.0863)	-8.245** (2.915)		-0.152+ (0.0877)		-8.640** (2.927)
* Tercile = 3		-0.00468 (0.0982)		-0.277 (3.114)		0.00937 (0.0971)		-0.444 (3.102)
Child Penalty 0-7 * Post-2009 First Birth	0.186*** (0.0531)	0.0540 (0.0852)	6.284*** (1.854)	0.599 (2.772)	0.186*** (0.0531)	0.0218 (0.0869)	6.315*** (1.854)	0.188 (2.699)
* Tercile = 2			0.283* (0.118)	10.39** (3.894)		0.351** (0.118)		10.70** (3.884)
* Tercile = 3		0.0992 (0.138)		7.954 (5.330)		0.117 (0.140)		7.895 (5.223)
Early (pre-2010) cohorts: Mean Y pre-birth	0.737		42.70		0.737		42.70	
- tercile 1		0.861		44.31		0.833		43.89
- tercile 2		0.787		43.50		0.806		43.25
- tercile 3		0.505		39.39		0.555		40.81
Late (post-2010) cohorts: Mean Y pre-birth	0.747		41.93		0.747		41.93	
- tercile 1		0.860		44.40		0.836		44.52
- tercile 2		0.718		40.75		0.752		41.16
- tercile 3		0.597		39.70		0.575		38.14
Early cohorts: New parents	112		108		112		108	
- tercile 1		34		33		33		31
- tercile 2		42		41		40		38
- tercile 3		36		34		39		39
Late cohorts: New parents	116		108		116		108	
- tercile 1		41		39		41		40
- tercile 2		46		46		42		42
- tercile 3		29		23		33		26
Tot observations	17437	17434	14974	14971	17445	17444	14984	14981

Standard errors in parentheses

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

Note: This table replicates the primary columns of Table 4 on a smaller sample (focusing on cohorts of women who had their first child in 2007-08 and 2010-11) and using alternative definitions of exposure. Columns (1)-(4) use the definition of exposure we use in the main specification (each woman is assigned the modal job she had in the five years prior to the birth of her first child and the casual prevalence associated with it), while columns (5)-(8) assign each woman the job she had three years before the birth of her first child. This ensures that even in the post-Fair Work Act cohorts, women are assigned a job they held before the Fair Work Act.

I.4 No Effects on Fertility or Via Fertility

Here we show that the Fair Work Act did not induce differential fertility, and thus the estimated effects are not mediated by exposed mothers having more or fewer children in response to the law.

No Effect on Fertility Table I.3 shows that the Fair Work Act did not impact fertility choices. The table reports the coefficients estimated from running the triple-difference specification (5) on various measures of fertility. As in the main table, the coefficients of interest, those on the interaction between post-childbirth (“Child Penalty 0-7”), post-Fair Work Act cohort (“Post-2009 First Birth”), and pre-birth job in middle tercile of casual prevalence (Tercile = 2) are highlighted in blue in the table. Columns (4) and (6) show that mothers exposed to the Fair Work Act are not differentially likely to have a birth or adoption or to be pregnant. Consequently, total fertility is not impacted, neither when using as outcome variable the time-varying “Total number of children ever had” (column 2), nor when looking at specific points in time: three, five, and seven years after childbirth (column 8, 10, and 12 respectively).

No Effect Via Fertility Consistently, our results replicate when conditioning on a completed fertility of one. When restricting the sample to mothers whom we do not observe having any further child after the first, our results replicate closely, albeit more noisily, since only about 20% of mothers in our sample satisfy this restriction (results not shown, available upon request).

Differential Birth Timing is Not a Concern The evidence presented above should mitigate concerns that differential birth timing across skill groups, and thus across terciles of casual prevalence, would drive the estimated results. In particular, Adams et al. (2024) raise the issue that more educated women tend to have children later and with shorter birth spacing, and that, if periods of leave are counted as non-employment, this might overstate the child penalty for this group. These concerns do not apply in this setting for several reasons. First of all, as discussed above, we find no effect on fertility, and thus differential fertility cannot be the channel through which the Fair Work Act impacts exposed mothers. Second, most of these differences that might generate bias in the estimation of the child penalty are in fact held constant in our preferred comparison, that between mothers with pre-birth jobs in the bottom and in the middle tercile of casual prevalence: as highlighted in Appendix G, these occupational groups tend to have a similar share of employees with bachelor degrees (35% and 32% respectively, versus jobs in the top tercile of casual prevalence where this figure is 8%), similar age at first birth for women (32 and 31, versus 28.6 in the top tercile) and similar fertility patterns for mothers. Finally, periods of leave are counted as employment, thus differential leave taking would not impact our estimated child penalty either.

