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THE IMPACT OF PAID PARENTAL LEAVE ON FERTILITY INTENTIONS

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Abstract

Paid parental leave is an important part of family policy in OECD countries. Australia's Paid Parental Leave (PPL) scheme was introduced in 2011 and provides 18 weeks of leave paid at the full time minimum wage for the primary carer of a child. We estimate the effect of access to paid parental leave on women's fertility intentions by exploiting the differential impact of the scheme for women working in the public and private sectors. We find that the scheme's announcement had no impact on fertility intentions at the extensive margin but that, conditional on intending to have at least one (more) child, the number of children intended increases by 0.34, a 16% increase. This effect is concentrated among highly educated women. As it has been shown that fertility intentions predict fertility outcomes, these results suggest that even modest paid parental leave programs can increase the fertility of working women and so moderate the declines in fertility rates seen in many developed countries.

JEL codes: J13, J18

Keywords: Parental leave; fertility; fertility intentions; family policy

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1 Introduction

Since 1970, paid parental leave policies have increased in importance in OECD countries, growing in generosity from an average of 17 weeks in 1970 to over one year in 2014 (OECD 2015). A broad body of research has linked these paid leave policies to increased female employment, improved maternal health, and benefits for children (Adema and Frey 2015). The costs of these schemes vary considerably across countries depending on leave length and payment generosity, from as much as 0.5% of GDP in Nordic and Eastern European countries to 0.07% in New Zealand (Adema and Frey 2015).

Publicly-funded paid parental leave provides a financial transfer to working women at the birth of a child and an improved ability to combine work with early childcare responsibilities. This eases constraints associated with fertility choices and so is expected to increase intended levels of fertility. Previous studies estimating the effect of access to paid parental leave on fertility have mainly focused on countries with generous paid leave schemes, and found modest positive effects of increased generosity, either in terms of leave duration or payment rates.[1] Less is known about the effects of more modest paid parental leave policies.

In this paper, we examine whether the provision of paid parental leave affects the fertility intentions of working women in Australia. Australia first introduced a federal Paid Parental Leave (PPL) scheme in 2011, providing 18 weeks of paid leave to a newborn's primary carer paid at the minimum wage, equivalent to a 41% replacement rate at the average wage – a relatively modest scheme in an international context. Prior to this, any paid leave was provided either through collective bargaining agreements, through state or federal legislation for public sector employees, or at the discretion of employers. The new scheme was intended to improve health and development outcomes for mothers and children, increase mothers' labour force participation, and better enable work-family balance (Martin et al. 2014a). However, it is important to examine if the scheme's introduction increased fertility to fully understand its costs and implications. This paper contributes an estimate of the impact of a modest paid parental leave scheme on fertility intentions.[2]

Stated fertility intentions have been show to have predictive value for eventual realised fertility. Using US data, Morgan (2001) shows that average intended parity is "...*relatively* stable and *frequently* provides good/useful estimates of mean completed parity." Schoen et al. (1999) shows the additional predictive power of fertility intentions for future

[1] For example, Lalive and Zweimüller (2009) find that an increase in leave duration from one to two years in Austria caused a 15% increase in fertility, and Malkova (2018) finds that the introduction of one year of paid maternity leave in Russia led to a 5% increase in fertility rates, driven by higher parity births. Studies of recent German reforms that reduced the duration of paid leave from two years to one year but linked payments to earnings and so increased the value of the leave for higher earning mothers have found that a 1000 € increase in the value of the leave increased fertility by 1.2% (Raute 2019; Stichnoth 2014; Cygan-Rehm 2016).

[2] As explained in Section 4, due to the sample size of the survey data we use we are unable to estimate the effects on realised fertility.

fertility using a different US dataset. For the UK, Berrington (2004) shows that women's fertility intentions are strongly predictive of completed fertility even after controlling for other observable characteristics. Fan and Maitra (2011) confirm this is also the case for Australia, highlighting that women's preferences are more strongly predictive of realised fertility than men's.

We use data on employer-provided paid maternity leave[3] and fertility intentions from the Household, Income and Labour Dynamics in Australia (HILDA) Survey for working women aged 21 to 45, and estimate how the substantial expansion in paid parental leave provision induced by the scheme's introduction affects fertility intentions.

We exploit the fact that the introduction of the PPL scheme increased access to any paid parental leave by 54 percentage points for women working in the private sector, but only 12 percentage points for women working in the public sector. This generates exogenous variation in access to paid leave and allows us to identify the causal effect of leave access on fertility intentions, eliminating bias resulting from women selecting into jobs with paid leave on the basis of their fertility intentions. We have two measures of fertility intentions: whether a woman expects to have a child in the future; and, conditional on expecting to have at least one (more) child, how many children the woman expects to have. This allows us to consider both the extensive and intensive margins of fertility intentions.

We find that, on average, there is no significant difference in fertility intentions for women with and without access to paid parental leave after controlling for observable characteristics. Exploiting the introduction of the PPL scheme, we find that having access to paid parental leave has no significant effect on the extensive margin of fertility intentions. Our primary result is the impact of access to paid leave on the intensive margin of fertility intentions: conditional on an intention to have children, access to paid parental leave causes an increase in the intended number of 0.34, a 16% increase. OLS estimates of the effect of paid leave on the number of intended children are biased downwards as career-oriented women are both more likely to be in jobs with a generous set of benefits, including paid leave, and also have a lower intended number of children than less career-focused women.

A series of robustness checks show that these results are not driven by uncertainty about access to paid parental leave before the scheme's introduction, or by changes in employment status and sector induced by the scheme's announcement. Placebo tests show no statistically significant differences in trends in leave access or fertility intentions between women working in the public and private sectors before the scheme's introduction. We also find that the effects are driven by women with more than high school education.

These results demonstrate that fertility intentions are influenced by family policy.

[3]In this paper we will use the terms 'maternity leave' and 'parental leave' interchangeably.

Coupled with evidence that fertility intentions predict fertility outcomes, and assuming that the relationship between intended and realised fertility is stable over time, these results suggest that even modest paid parental leave programs can increase fertility among working women and so help to moderate the declines in fertility rates seen in many developed countries, including Australia. A corollary of this is that the expected cost of such programs needs to account for these fertility responses.

2 The Australian Paid Parental Leave scheme

Australia's statutory PPL scheme was announced in May 2009 as part of the majority Labor government's budget proposals, and came into force on the 1st of January 2011 (Department of Social Services 2014). Whilst a national paid parental scheme was discussed by the Australian Labor Party during the 2007 election campaign, no details on funding, eligibility or the level of support were discussed and substantial uncertainty remained until the 2009 announcement (Maiden 2008). The scheme provides eighteen weeks of minimum wage pay (40.6% of average earnings for full-time workers (OECD 2015)) to the primary carer of a new child (born or adopted), which can be taken at any time within the first year of a child's birth or adoption (Baird and Whitehouse 2012). Prior to its implementation, Australia was one of only two OECD countries to remain without a national PPL program (OECD 2015). The Australian scheme is the most recently introduced national paid leave scheme in the OECD, and is modest in comparison to the generosity of other OECD countries' schemes.

The stated objectives were to normalise taking time out of paid employment for new parents and to promote gender equality and work-family life balance (Martin et al. 2014a). To be eligible for the PPL scheme recipients must work at least three hundred and thirty hours in ten of the thirteen months prior to birth or adoption with no gap larger than eight weeks, and earn $150,000 or less in the year prior to the birth.[4] Given the generosity of these tests, the scheme is accessible for a large majority of working women. In the HILDA sample described below, 94% of working women satisfy these tests, with ineligibility typically due to low usual work hours.[5] Payments under the scheme are funded from general taxation and paid via the employer. Although the scheme provides gender neutral 'parental' pay, it is only available to the primary carer of a newborn and is usually taken by women: in the first full year of the scheme, 99.4% of recipients were mothers (Martin et al. 2013).[6]

[4]There are also residence and visa requirements.

[5]Women who are not working are not eligible for the PPL scheme, and so the proportion of total births covered is much lower than this.

[6]Recognising that up-take of the scheme was primarily by mothers, the government later added an additional two weeks of non-transferable 'Dad and Partner Pay' (DAPP) component with payments commencing in 2013.

One concern with our analysis below is that the scheme's introduction may have led women with lower labour force attachment to increase their hours of work to become eligible for paid leave. However, until 2014 the PPL scheme existed alongside Australia's Baby Bonus payment. Mothers from households with annual income below $75,000 could elect to take either the $5,000 Baby Bonus (paid in instalments over thirteen weeks), or their PPL entitlements. For mothers working less than full time, the Baby Bonus could be the more generous payment, and indeed the PPL evaluation found that the minority of PPL eligible mothers who elected instead for the Baby Bonus had lower labour market attachment (Martin et al. 2013).[7]

Prior to the introduction of the PPL scheme there was no legislated general entitlement to paid parental leave. Around one half of Australia's female workforce received some form of paid maternity leave from their employer with an average duration of six to eleven weeks (Baird, Whelan, and Page 2009). Public sector workers accounted for a large proportion of these women: the Commonwealth government and each State and Territory introduced legislation covering paid maternity leave policy for employees, ranging from four to twelve weeks of paid leave availability (Risse 2006b). However, all employees (both casual and permanent) with twelve months of continuous service had a legislated entitlement to 52 weeks of job-protected *unpaid* parental leave. These provisions remain in place, providing job protection and unfair dismissal rights, and are complemented by the PPL scheme.

3 Related literature

There is a substantial and growing interdisciplinary literature examining how family policies including financial incentives, childcare and parental leave affect fertility intentions and outcomes in developed countries (Gauthier 2007). These studies cover a wide variety of policies implemented in many different countries at multiple points in time: this is reflected in the range of conclusions reached. The novelty of this paper is that it provides further international evidence on the impact of a recently implemented policy that is modest in scale compared to other studies of parental leave policies.

Studies of the impact of paid parental leave on fertility exploiting changes in policy have found a modest positive impact. Longer leave entitlements (in the order of one to two years) have been linked to increases in higher order fertility in Austria (Lalive and Zweimüller 2009) and in Russia (Malkova 2018). Recent reforms to paid leave in Germany which shortened the length of leave whilst increasing payment generosity have also been linked to modest increases in fertility, especially among more highly educated women (Raute 2019; Stichnoth 2014). Comparably, an increase in the payment rate for parental leave in Quebec has been linked to increased fertility (Ang 2014). There is also evidence of

[7] We thank an anonymous referee for this insight.

women delaying pregnancy to take advantage of the anticipated introduction of paid leave schemes (Lichtman-Sadot 2014). Cross-country studies are more equivocal, suggesting no relationship between the length or payment generosity of a country's parental leave policy and fertility rates (Gauthier and Hatzius 1997). Access to unpaid job-protected parental leave has also been linked to higher fertility in the United States (Averett and Whittington 2001; Cannonier 2014).

Most relevant to this paper is an earlier attempt to evaluate the impact of paid and unpaid parental leave on pregnancy in Australia. Risse (2006b) uses the HILDA survey to examine employer-provided maternity leave in 2003, before any legislated entitlement to paid leave, finding that unpaid leave access increases pregnancy among women aged under 35. However, paid leave is only associated with increases in pregnancy among women aged under 25. The drawback of this study is that it is unable to distinguish women selecting into jobs or choosing the timing of their pregnancy based on leave availability.

At this stage, there is limited evidence of the effects of the Australian PPL scheme. A sample of mothers who gave birth before and after the Scheme's introduction shows that the PPL scheme delayed mothers' return to work over the six months post-birth, but increased labour force participation a year later, especially returns to the pre-birth job (Broadway et al. 2016). This is in line with international evidence of the labour supply effects of paid parental leave (see, for example, Ruhm (1998), Ulker and Guven (2011), Hanel (2013), Rossin-Slater, Ruhm, and Waldfogel (2013), and Bergemann and Riphahn (2015)). Improvements in the health of mothers and an increase in breastfeeding duration were also found (Martin et al. 2014b), again in line with international evidence (Ruhm 2000; Tanaka 2005; Avendano et al. 2015; Broadway et al. 2017; Huang and Yang 2015; Stearns 2015). This paper extends and complements the formal evaluations by considering the effects of the scheme on *potentially eligible* women rather than a selected sample of new mothers.

This paper also contributes to a literature examining the determinants of fertility intentions. Previous research drawing on data from a range of countries has shown that fertility intentions are not fixed across the life course, with age, relationship status transitions and fertility events being important determinants (Berrington 2004; Liefbroer 2009; Morgan and Rackin 2010; Iacovou and Tavares 2011). Evidence for the adjustment of fertility intentions in response to changes in employment status, occupation and education has been much weaker (Heiland, Prskawetz, and Sanderson 2008).

For Australia, a range of studies have used data from the HILDA survey to examine fertility intentions. For example, Fan and Maitra (2011) demonstrate the predictive value of fertility intentions for realised fertility, and that women's intentions are more important than men's, Gray, Evans, and Reimondos (2013) highlight the importance of age and relationship status as determinants of fertility intentions, Keygan (2017) focuses on the determinants of men's fertility intentions, and Testa and Bolano (2019) show the enduring predictive content of fertility intentions in the HILDA survey even after accounting for a range of characteristics and life events. Lifecycle-related changes in fertility intentions in Australia have also been demonstrated in other data sources as reported by Gray, Qu, and Weston (2008) and Holton, Fisher, and Rowe (2011).

A growing body of research evaluates how fertility intentions respond to changes in family policy and public policy in general. Most relevant to this paper is an analysis of an expansion of paid maternity leave in Switzerland which, consistent with our results, finds an increase in fertility intentions (Barbos and Milovanska-Farrington 2019). For Australia, the effects of the Baby Bonus on fertility intentions have been analysed in a number of studies, with Risse (2010) and Drago et al. (2011) finding that the non-means tested maternity payment increased fertility intentions.[8] Looking beyond family policy, Yu, Kippen, and Chapman (2007) finds no impact of the Australian student income-contingent loans scheme (HECS) on fertility intentions.

4 Fertility in Australia

The primary empirical analysis presented in this paper concerns stated fertility *intentions* and not realised fertility. It is nevertheless informative to examine trends in realised fertility in Australia across the same time period to provide context for the main estimates below, and to inspect for evidence of any response in realised fertility after the announcement and implementation of the federal PPL scheme.

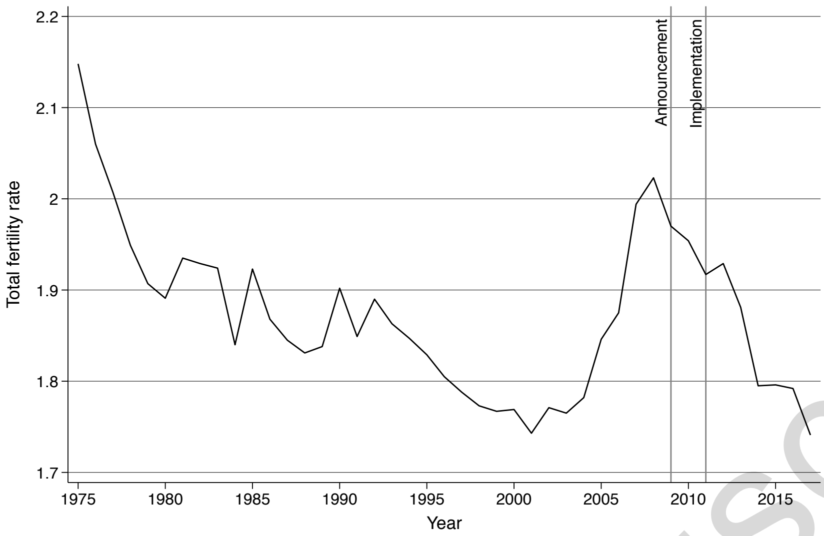
Figure 1 graphs Australia's total fertility rate (TFR) from 1975 to 2017 and shows the decline in realised fertility from the 1970s through to the early 2000s.[9] The mid to late 2000s saw a strong increase in the TFR (coinciding with the 2004 introduction of the Baby Bonus), peaking at just over two in 2008. The subsequent decline in the TFR occurred against the background of the Global Financial Crisis and at the same time as changes in the structure of public child care subsidies.

The Figure also indicates the 2009 announcement and 2011 implementation of the PPL scheme; there is no clear change in the fertility rate at these dates. We cannot, however, conclude directly that the PPL scheme had no effect on realised fertility, as we do not observe the counterfactual fertility trend in the absence of the scheme's introduction: the decline in fertility observed after 2011 may have been even more dramatic in the absence of the scheme. Changes in the fertility rate may also reflect changes in the age pattern of fertility across birth cohorts, with a decline in the total fertility rate reflecting, for example, an increase in age at first birth in more recent birth cohorts.

[8] There is mixed evidence of the effect of the Baby Bonus on realised fertility (Lattimore and Pobke 2008; Parr and Guest 2011; Sinclair, Boymal, and De Silva 2012a; Sinclair, Boymal, and De Silva 2012b).

[9] The TFR represents how many children would be born to each woman if she gives birth in alignment with prevailing age-specific fertility rates and lives to the end of her child-bearing years.

Figure 1: Total fertility rate



Note: Based on ABS fertility data from ABS.Stat

To motivate the empirical strategy outlined in the following section, it is helpful to express age-specific fertility rates ($fertility_{at}$: the fertility rate of women aged a in year t) as a function of an age effect α, a cohort effect γ, and an underlying time effect τ:

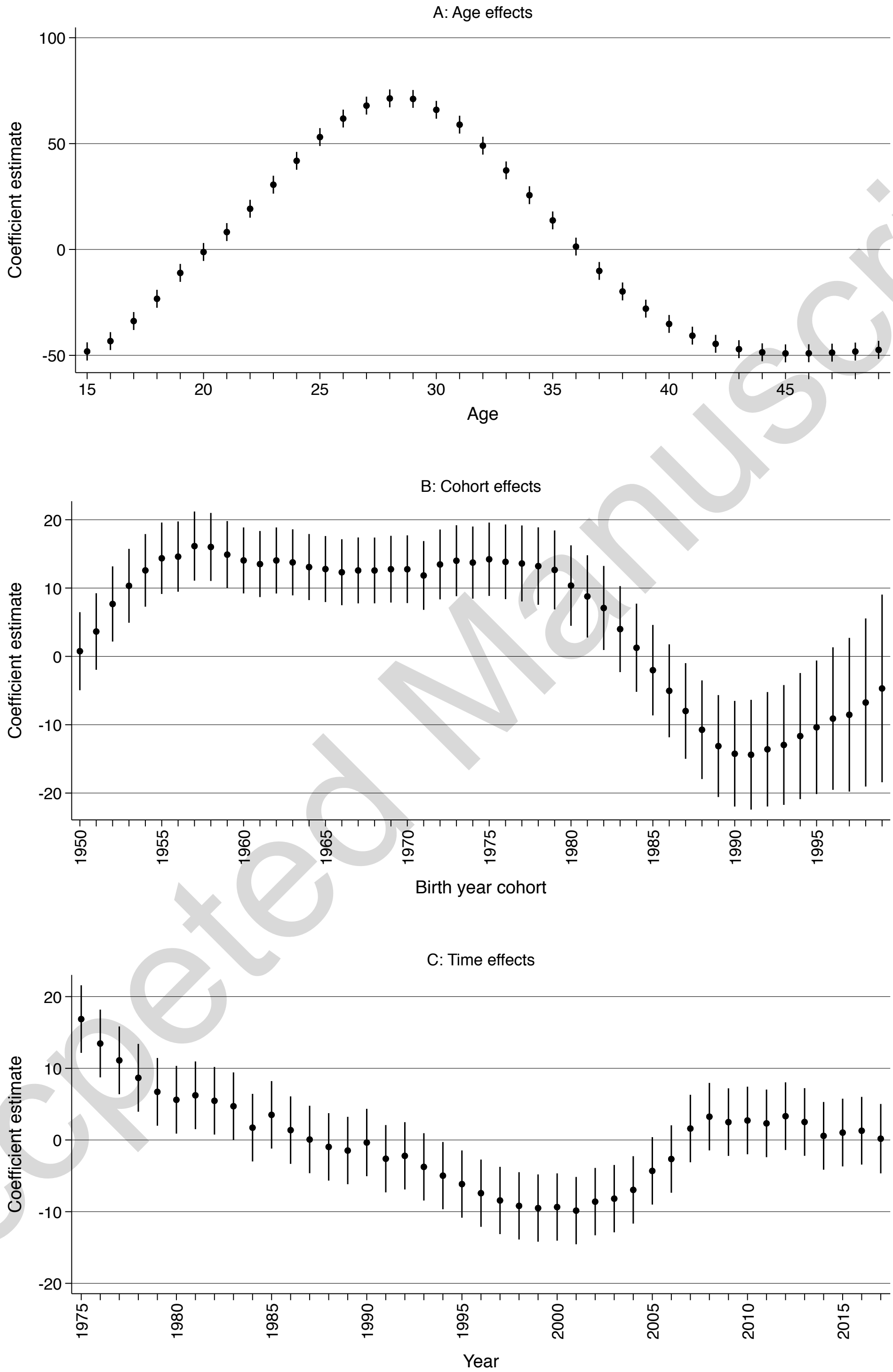
$$fertility_{at} = \mu + \alpha_a + \gamma_c + \tau_t + \varepsilon_{at}$$

This approach acknowledges the influences of the established age profile of fertility and changes across birth cohorts, which may, for example, be linked to changes in education policy. Time-based variation is captured separately. These time or period fixed effects capture all changes in fertility driven by relevant policy changes or contemporaneous economic conditions; this would include changes in fertility driven by the PPL scheme.

Isolating the time effects in the above relationship is not straightforward due to the collinearity of age, year of birth (cohort), and year of observation (time); this issue has been addressed extensively in the fields of demography and epidemiology, with one common approach being the intrinsic estimator. The intrinsic estimator constrains the sum of each set of coefficients to be equal to zero (Yang, Fu, and Land 2004).[10] Using age-specific annual fertility rates for the Australian population, we implement the intrinsic estimator to gain insight into the relative size of estimated age, time and cohort effects in the Australian context. The results are illustrated in Figure 2, with the coefficient estimates provided in Appendix Table B1.

[10]Other approaches typically set one coefficient equal to zero, for example assuming that the first two time effects are equal. It has been shown that results of these analyses are extremely sensitive to the choice of normalisation (Yang, Fu, and Land 2004).

Figure 2: Age, cohort and time effects



Note: Graphs illustrate coefficient estimates and 95% confidence intervals from the intrinsic estimator of age, time and cohort effects using age-specific annual fertility rates sourced from ABS.Stat

Panel A of Figure 2 illustrates the age effects, showing a clear hump-shaped fertility profile peaking at age 28-29; Panel B illustrates the estimated cohort effects, showing a clear decline in fertility for women born from the mid 1970s onwards. Panel C illustrates the estimated time effects, capturing the clear increase in fertility starting around 2004. There is an indication of declining fertility from 2013 onward, though this is not precisely identified. There is not clear evidence of a change in fertility in response to the PPL scheme from 2011 onwards. However, this analysis has not attempted to disentangle the effect of the scheme's introduction from other contemporaneous influences on fertility.

Establishing the importance of age, cohort and time effects in explaining trends in Australian fertility brings into focus the challenge of selecting an appropriate control group to identify the effect of the PPL scheme. Given the clearly established age pattern of fertility, it is necessary to compare groups of women at the same age: comparing younger women to older women would risk overestimating any true impact on fertility. One potential approach would be to compare the fertility of a group of women aged 20-30 in 2001, with a group of the same age after the PPL scheme was introduced in 2011, ensuring that the age profile is comparable between the two groups. However, this approach would also compare different birth cohorts and different time periods. This means that any effect found would also reflect cohort and time effects, which the above analysis suggests are large.

Ideally, we wish to compare the fertility rates of two groups of women of the same age and cohort at the same time, but with different exposure to the PPL scheme. As the scheme was extended to all working women at the same time, there is not a clear natural control group. However, the differential prior access to paid leave between women working in the private and public sectors (established in Section 6 below) generates a differential impact of the federal scheme among women of the same age and cohort at the same time. We use this differential exposure in our empirical strategy outlined below.

The above discussion has addressed realised fertility rates. Using our chosen empirical strategy to evaluate the effect of the PPL scheme on fertility requires data that include information on fertility, demographic information, and labour force participation including sector of work, at a frequency that allows us to examine the change in fertility before and after the introduction of the PPL scheme. The rich data required means that the population-level birth measures used above are insufficient, and so we turn to data from the HILDA Survey, described in detail in Section 6 below.

The limitation of the HILDA Survey data is its sample size: in the main analysis sample used below, a maximum (minimum) of 77 (37) births per year are observed, with a birth rate of around 3-4% of sample women. These data do not have sufficient power to identify even large changes in realised fertility in this empirical design. Power calculations following the methods outlined in Burlig, Preonas, and Woerman (2019) suggest a minimum detectable effect of at least 2.2 percentage points, a 55% increase in the fertility rate.[11]

Due to the limitations of the survey data in being able to address realised fertility, we instead consider stated fertility *intentions*. The benefit of focusing on fertility intentions is that all women of childbearing age are asked to respond, substantially increasing the power of the survey data to identify the effect of the PPL scheme. As outlined in Section 3, there is a large body of research demonstrating the predictive power of stated fertility intentions for realised fertility. So, if we find that the PPL scheme's introduction has an effect on fertility intentions, it is reasonable to infer that there may be an eventual effect on realised fertility.

The value of this approach relies on the assumption that the relationship between fertility intentions and realised fertility is constant over time. If there has been a contemporaneous change in how fertility intentions map to fertility outcomes then the results presented below may not have real implications for fertility or for the cost of the PPL scheme, and the results should be interpreted with this in mind.

5 Empirical strategy

To evaluate the relationship between access to paid parental leave and fertility intentions, we first use OLS regressions, comparing fertility preferences of employed women with and without access to paid parental leave. Specifically, we estimate:

$$pref_{it} = \beta_0 + \beta_1 leave_{it} + \beta_2 prisec_{it} + \beta_3 X_{it} + \theta_t + \varepsilon_{it} \tag{1}$$

where $pref_{it}$ is the relevant fertility intention, $leave_{it}$ indicates whether woman i has either access to employer-provided PPL or anticipated access to the PPL scheme at time t, and $prisec_{it}$ is an indicator for the woman working in the private sector. X_{it} is a vector of control variables that includes age, education, number of children, marital status, indigenous status, country of birth and income, all of which have been shown to influence fertility intentions (Berrington 2004; Hagewen and Morgan 2005; Liefbroer 2009; Iacovou and Tavares 2011; Gray, Evans, and Reimondos 2013). In addition, this vector of controls includes indicators for whether the woman meets the eligibility criteria for the new leave scheme, an indicator for being in employed in a casual position, and measures of firm size and industry as these may correlate with employer provision of paid maternity leave. Finally, θ_t is a set of time dummies to capture trends in fertility preferences, and ε_{it} is an

[11] In line with the analysis sample below, this assumes a sample size of 1400 per wave, 70% of sample 'treated', five pre- and four post-treatment waves, an outcome variable with variance 0.04, and reflects minimum detectable effect sizes with power 0.8 and for positive serial correlation of errors. Simulation exercises also indicate that a test for a two percentage point increase in the fertility rate of women working in the private sector – that is, a 50% increase – would have power of around 0.75.

error term, clustered at the individual level.

A crucial concern associated with this approach is that women may select into jobs based on their fertility intentions and other characteristics. For example, women who intend to have a child may select into jobs that provide paid maternity leave, meaning that any positive estimate of β_1 reflects this selection. On the other hand, women who already have children (and as a result may have lower future fertility preferences) may select into jobs with more family-friendly work policies, including paid maternity leave, biasing the estimate of β_1 downwards. Furthermore, more career-oriented women may be more likely to have jobs with generous benefits packages including paid maternity leave whilst also having lower fertility preferences, once again biasing β_1 downwards.

To avoid these selection concerns, we exploit exogenous variation in access to PPL generated by the introduction of Australia's national PPL scheme. Since the scheme applied to almost all working women, the time series variation will capture other changes affecting fertility preferences at the same time. However, the scheme's introduction affected public and private sector workers' access to PPL differently: based on our sample from HILDA, prior to the scheme only 35% of workers in the private sector had access to employer-provided paid maternity leave, compared to 80% of workers in the public sector. So, the introduction of the scheme had a much larger impact on access to paid parental leave for private sector workers than for public sector workers. By comparing the response in intentions between these groups, the impact of an extension in PPL availability is identified.

Because the outcomes of interest are fertility *intentions*, they are likely to respond to the announcement of the PPL scheme rather than its implementation: women will take into account the future option-value of the PPL from the point at which they become aware of its existence; there is no reason to expect women to wait until the scheme's implementation date to adjust their intentions. Our measure of leave access therefore includes current access to employer-provided paid leave and *anticipated* access to the public scheme from the time of announcement in 2009 where a woman is eligible.[12]

We implement an instrumental variables strategy using two-stage least squares. The first stage predicts women's access to paid parental leave using an indicator for working in the private sector after the announcement of the scheme ($prisec_{it} * post_{it}$), implementing a difference-in-difference estimator of the effect of the scheme on access to paid leave:

$$leave_{it} = \gamma_0 + \gamma_1(prisec_{it} * post_{it}) + \gamma_2 prisec_{it} + \gamma_3 X_{it} + \theta_t + u_{it} \quad (2)$$

Since we include a control for private sector employment and a set of time dummies

[12] As will be shown in Section 7.3, where intentions respond, they do so after the announcement date and there is no further impact of the Scheme's implementation.

(which subsume a $post_{it}$ indicator), γ_1 estimates the differential effect of the PPL scheme on paid leave access between the two sectors. Predicted probabilities of access to leave from this estimation are then used in the second stage regression in place of observed leave access:

$$pref_{it} = \beta_0 + \beta_1 \widehat{leave}_{it} + \beta_2 prisec_{it} + \beta_3 X_{it} + \theta_t + \varepsilon_{it} \tag{3}$$

In implementing the two-stage least squares strategy, the vector of control variables X_{it} is common to equations (2) and (3), X_{it} to ensure that predicted leave access from equation (2) is uncorrelated with the error term in equation (3). In this specification, β_1 will provide a causal estimate of the effect of access to paid parental leave on fertility intentions if a number of assumptions hold. First, the instrument (being in the private sector after the scheme's announcement or implementation) must strongly predict access to leave. We show that this is the case in Section 7.

Second, the instrument must only affect fertility intentions through its effect on access to paid leave, conditional on the regressors included in X_{it}. There are three key threats to this assumption. First, the PPL scheme may have caused women to change employment sector: they now receive paid parental leave in any job. For example, if women with strong future fertility intentions initially select into a public sector job due to paid parental leave availability, the announcement of the PPL scheme may have encouraged some of these women to seek employment opportunities in the private sector, thus causing fertility intentions to increase in the private sector due to sector switching rather than leave availability. We investigate this further in Section 7.2.

The second threat to this assumption is that there must be common trends in access to paid leave for public and private sector workers before the scheme's introduction. This ensures that our first stage regression captures the differential change in paid leave access caused by the scheme. If differential trends exist, some of the increase in leave availability we attribute to the scheme's announcement may reflect differences in the growth rates of employer-provided paid parental leave. The third threat is that differential trends in fertility intentions across the two groups before the scheme's announcement would bias our results. Given a number of years of data from before the scheme's announcement, we are able to test for the presence of common trends in leave access and fertility intentions, and cannot reject the hypothesis of common trends in the pre-scheme period. These results are shown in Section 7.3.

Finally, the instrument must satisfy monotonicity: it must (weakly) increase access to leave for all women. One concern is that employers who provided paid maternity leave prior to the scheme's introduction may remove this entitlement in response to the publicly funded scheme, leaving some previously covered women (either working few hours or earning in excess of the threshold) without paid parental leave access. Employer surveys from before and after the scheme's introduction find that some employers modify their parental leave benefits in response to the new scheme, but that this generally takes the form of redesigning their benefits to complement or top-up the statutory scheme. No employer in the survey reports removing their paid leave provision (Martin et al. 2014a). This suggests that the monotonicity assumption is reasonable.

6 Data

We use data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. Wave 1 of HILDA was collected in 2001 and surveyed 19,914 people from 7,682 households. These respondents have been interviewed annually since 2001, answering questions covering a broad range of demographic, economic and social topics.[13] All children in households in the initial sample 'age in' to responding to the survey, meaning that the average age of our sample does not increase over time. Our estimation sample consists of all observations within the survey of employed women aged 21 to 45 who answer a set of questions about their fertility intentions and their access to employer-provided paid maternity leave, and covers responses for the period 2002-2013.

Working individuals are allocated to employment sector based on their description of their employer. Those reporting that their employer is a 'Government business enterprise or commercial statutory authority', or an 'Other government organisation, such as a public service department, local councils, schools and universities' are allocated as public sector workers. All other workers, including those working for a not-for-profit organisation, are allocated as private sector workers.

Fertility intentions are measured by responses to two questions. First, respondents are asked to indicate how likely they are to have a child or more children in the future on a scale from 0 to 10. This is our measure of fertility intentions at the extensive margin. If the response to this question is 6 or higher, the respondent is then asked how many (more) children they intend to have – the *intended number* of children. Our focus on the response to these questions means that we exclude data from waves 5, 8 and 11 due to differences in the criteria used to determine who is asked these questions and the order in which they are asked. We do, however, use responses from waves 5, 8 and 11 to exclude women who report infertility or sterilisation. Appendix A describes these data issues in detail and provides a range of related robustness tests.

Table 1 shows our measures of fertility intentions over time by sector of employment. We see that private sector workers have consistently higher average fertility intention scores across our sample period compared to women working in the public sector, with an

[13]An extension sample was included from 2011 – we exclude these individuals from our analysis as this is after the announcement and implementation of the PPL scheme.

upward trend over time observed for women in both sectors. There is no notable change in trend around the 2009 announcement of the paid leave scheme or its 2011 implementation. The pattern in the reported intended number of children is less clear. For women working in the public sector, there is a decline in the number of intended children beginning from 2006, whereas for women working in the private sector there is a broadly upward trend from this point on. The analysis below will examine these different trends, controlling for observable characteristics and accounting for prior access to paid leave, to establish whether the announcement of the new paid leave scheme affected fertility intentions.

Table 1: Fertility intentions data, by year and sector of employment

	2002	2003	2004	2006	2007	2009	2010	2012	2013
Fertility intentions score (ten-point Likert scale)									
Public sector	3.80	4.05	4.34	4.66	4.94	5.04	5.07	5.17	5.21
Private sector	3.97	4.30	4.63	5.26	5.41	5.46	5.52	5.60	5.36
Intended number of children									
Public sector	2.22	2.13	2.08	2.19	2.16	2.05	2.01	1.99	2.04
Private sector	2.11	2.21	2.13	2.10	2.11	2.17	2.11	2.19	2.15

Fertility intentions are average scores reported on a ten-point Likert scale in response to being asked how likely the respondent is to have a child or more children in the future. Intended number is the number of children the respondent intends to have, conditional on planning to have at child or more children. Respondents must answer 6 or higher on the fertility intention question to be asked the number of children they intend to have.

A further critical response for our analysis is whether the woman is able to access paid maternity leave from her employer. This question is part of a battery of questions about employer-provided entitlements.[14] From wave 2 onward, these questions ask whether "you, or other employees working at a similar level to you at your workplace" would be able to use the entitlement if needed. In contrast, in wave 1 the question is limited to whether the respondent would personally have access to the entitlement, and so wave 1 is excluded from our analysis. Moreover, not all respondents report knowing whether they (or other similar employees) have access to paid maternity leave: 17% of respondents report that they don't know. For our main analysis, we exclude these 'don't know' responses, and in Section 7.1 we provide robustness checks including these 'don't know' responses under various assumptions.[15]

Table 2 provides summary statistics for the analysis sample, in aggregate and split into those working in the public or private sector. After excluding observations with

[14]We use responses to the question about maternity leave, and not to a later question in the same battery referring to parental leave.

[15]Survey data from the Australian Bureau of Statistics (ABS) have a similar proportion of 'don't know' responses (16% in 2007), and ABS analysis suggests that these responses are not disproportionately found across a range of demographic and labour market characteristics (Australian Bureau of Statistics 2008). It is therefore suggested that the 'don't know' responses can be inferred to have a similar distribution to those who responded yes or no to the question (Productivity Commission 2009). In contrast, Risse (2006a) finds differences in the observable characteristics of women who do and don't know their leave entitlements, but finds that this does not generate selection bias.

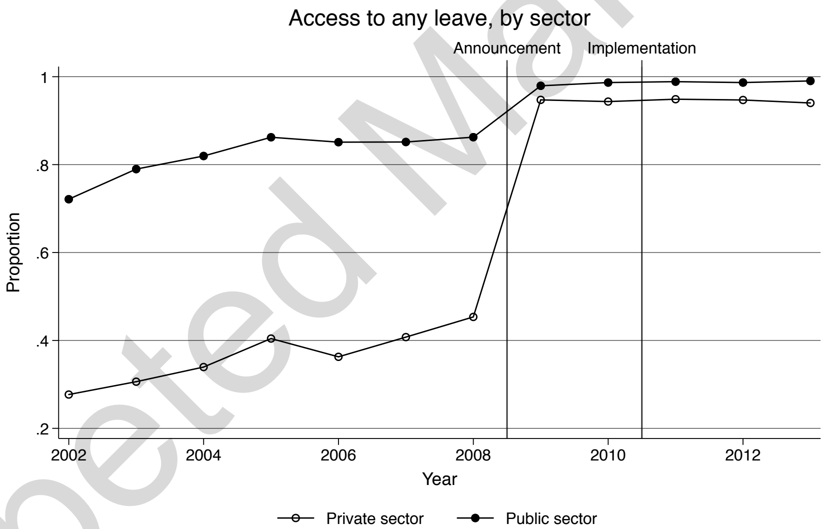
Table 2: Summary statistics

	All women	Private sector	Public sector
Age	32.04	31.54	33.16
	(7.04)	(7.02)	(6.96)
Number of children	0.88	0.87	0.91
	(1.14)	(1.14)	(1.15)
Childless	0.55	0.55	0.54
	(0.50)	(0.50)	(0.50)
Education			
Bachelor degree or higher	0.41	0.33	0.61
	(0.49)	(0.47)	(0.49)
Vocational education	0.27	0.30	0.20
	(0.44)	(0.46)	(0.40)
Year 12	0.19	0.21	0.13
	(0.39)	(0.41)	(0.33)
Year 11 or lower	0.13	0.16	0.07
	(0.34)	(0.36)	(0.26)
Marital status			
Married	0.43	0.43	0.45
	(0.50)	(0.49)	(0.50)
De facto	0.25	0.25	0.25
	(0.43)	(0.43)	(0.43)
Separated/Divorced/Widowed	0.07	0.06	0.07
	(0.25)	(0.24)	(0.26)
Never married	0.25	0.26	0.23
	(0.44)	(0.44)	(0.42)
Aboriginal or Torres Strait Islander	0.02	0.01	0.03
	(0.13)	(0.11)	(0.17)
Country of birth			
Australia	0.83	0.82	0.86
	(0.37)	(0.38)	(0.35)
Other English speaking	0.06	0.06	0.06
	(0.24)	(0.25)	(0.24)
Other non-English speaking	0.10	0.11	0.08
	(0.30)	(0.31)	(0.27)
Individual income ($000)	52.09	49.45	58.08
	(43.10)	(45.54)	(36.25)
Household income ($000)	121.62	119.21	127.09
	(89.17)	(93.26)	(78.85)
Ineligible: hours	0.05	0.05	0.03
	(0.21)	(0.23)	(0.18)
Ineligible: income	0.01	0.02	0.01
	(0.12)	(0.12)	(0.09)
Casual worker	0.29	0.37	0.11
	(0.46)	(0.48)	(0.32)
Employer paid maternity leave	0.54	0.41	0.85
	(0.50)	(0.49)	(0.36)
Observations	10430	7238	3192
Individuals	3548	2854	1165

Standard deviations in parentheses. Education categories: vocational education represents vocational education and training completed after high school that is not part of a bachelor degree or higher; completion of year 12 can be considered equivalent to high school completion or graduation; year 11 or below reflects dropping out of high school before completion. Note that number of unique individuals in private and public sectors does not add up to the total number due to individuals being observed working in both sectors.

missing data, we have 10440 observations for 3548 individuals. Public sector workers in the sample are on average slightly older, more educated, have higher incomes, are less likely to be in casual employment, and have more children than their private sector counterparts. Access to employer-provided paid maternity leave is substantially higher for workers in the public sector than in the private sector on average, with a 45 percentage point difference in reported access. However, reported access for public sector workers is not universal, at 85%. Prior to the introduction of the scheme, the main predictors of reporting not having access to paid leave for public sector workers included being in a casual position, working in a very small organisation (fewer than five employees), and having low income. There is no significant change in the share of casual public sector workers and those working in very small organisations after the scheme's implementation. There is, however, an increase in average incomes for public sector workers after the scheme's announcement in 2009.[16] We control for income, casual employment status and employer size in the analysis presented below.

Figure 3: Anticipated access to paid parental leave, by sector of employment



Our sample gives estimates of coverage that are broadly in line with estimates from Australian Bureau of Statistics (ABS) survey data: estimates reported in the Productivity Commission's Inquiry Report suggest that paid maternity leave was available to 41% of working women in 2002, 44% in 2004 and 54% in 2007 (Productivity Commission 2009); our comparable estimates are 42%, 50% and 58%, where we have excluded women known to be infertile and so do not have a representative sample. Figure 3 illustrates these trends in access to paid maternity leave, either employer provided or anticipated access to the public scheme, by employment sector. Prior to the scheme's announcement, public sector workers are around twice as likely to have access to paid maternity leave than private

[16]Results of this analysis are available on request from the authors.

sector workers, and this difference is persistent over time. After the scheme is announced, almost all women anticipate having access to paid leave, though some remain ineligible through low hours of work or high income. The divergence in trends in this anticipated access at the scheme's announcement, with the large increase in paid parental leave access for women working in the private sector, illustrates the variation we use to identify the impact of paid parental leave on fertility intentions.

7 Results

Results from the OLS and instrumental variables specifications for our main sample are shown in Table 3. The table presents results for two sets of control variables: odd-numbered columns include state and wave fixed effects and controls for age, education, marital status, income and industry; controls for the number of children are added in the even-numbered columns. We include OLS and IV results with both sets of control variables for the two measures of fertility intentions.

Columns (1) to (4) of Table 3 show results for fertility intentions. Columns (1) and (2) suggest that there is no significant relationship between extensive margin fertility intentions and access to paid leave. The IV results in columns (3) and (4), whilst less precise, reinforce this result. We also see that the expectation of having more children declines quadratically with age: at age 25, each additional year reduces the score on the ten-point scale by around 0.19, with the magnitude of the effect increasing to 0.37 at age 40. Here the quadratic term in age dominates any linear effect. Fertility intentions are strongest for married couples compared to their unmarried counterparts. The current number of children is also an important determinant of fertility intentions, with fewer children (and particularly childlessness) being associated with the highest fertility intentions.[17] Controlling for the number of children is particularly important in our analysis as women's stated fertility intentions are likely to change if they give birth in the analysis period. For example, a woman working in the private sector may increase her fertility intentions in response to the PPL scheme's announcement in 2009. If she then has a child in 2011 when the scheme has been implemented, her fertility intentions are then likely to fall, attenuating the observed effect on fertility intentions. On the other hand, if she does not have that child until 2013, the effect on fertility intentions will be sustained.

Columns (5) to (8) of Table 3 give results for the effect on the intended number of children. Whilst having access to paid leave is weakly correlated with intending to have more children, this relationship is explained by differences in the number of children a

[17] The inclusion of the 'childless' dummy acknowledges that the decision or intention to have a first child may be differently motivated to the decision to have additional children (Dommermuth, Klobas, and Lappegård 2011). However, the results presented are robust to the exclusion of this indicator variable. Results are also robust to alternative ways of controlling for the number of children and the age of the youngest child.

woman already has. However, the instrumental variables results in columns (7) and (8) reveal a different result: exploiting the fact that the statutory PPL scheme had a much larger impact on paid leave access for women working in the private sector than those working in the public sector we see that having access to paid parental leave increases the intended number of children by 0.34, even after controlling for the number of existing children.

This result represents a 16% increase in the intended number of children, and implies that access to PPL increases fertility intentions at the intensive margin. This suggests that the OLS result in column (6) is biased downwards. One explanation for this is that the null relationship is a spurious result driven by "career-oriented" women selecting into jobs offering a suite of benefits that include PPL. These career-oriented women are comparable to others in terms of whether they want to have *any* children, but plan to have *fewer* children, and this creates a spurious negative correlation between access to PPL and the intended number of children. This is in line with recent demographic trends showing a convergence in the rate of childlessness across educational groups alongside continued higher family sizes for women with less education.[18]

The validity of the IV results depends on the assumptions discussed above. The first stage of the IV regressions allows us to test whether our instrument is strong. Table 3 reports first stage F-statistics for these regressions ranging from 350 to 700, indicating a strong instrument. The full first-stage regressions are reported in Appendix Table B2, and show that being in the private sector after the announcement of the PPL scheme is linked to a highly statistically significant 42 percentage point increase in the likelihood of access to any paid leave, consistent across specifications. Appendix Table B2 further shows that employment sector is a particularly strong predictor of access to paid leave in the sample alongside being in casual employment, with comparatively little variation by education.

The results described above are for the full sample of working women aged 21 to 45. Table 4 splits the sample by education level to gain further insight into which group is driving the effects of paid parental leave access on the intensive margin of fertility intentions. The first line presents IV results for both outcomes with the full set of controls for women with Year 12 education or less. The IV estimates are statistically significant in this subsample, and the point estimate for the intended number is low. In contrast, the second line shows estimates for women with education beyond Year 12, and shows that the effect of paid leave access on the intensive margin of fertility intentions occurs among these more highly educated women.

Previous research suggests that lower income women increase their labour supply most in response to paid leave schemes, especially with modest schemes that represent a higher

[18]See, for example, Hazan and Zoabi (2015) and Pew Research Center (2015) for the US, and Miranti et al. (2009) and Heard and Arunachalam (2015) for Australia.

Table 3: Effect of paid parental leave access on fertility intentions

	Intentions				Intended number			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leave access	-0.032	-0.039	0.553	0.326	0.058	0.053	0.453**	0.335**
	(0.102)	(0.095)	(0.372)	(0.337)	(0.040)	(0.038)	(0.140)	(0.130)
Private sector	0.225^{+}	0.155	0.309*	0.207	0.036	0.036	0.089^{+}	0.074
	(0.127)	(0.119)	(0.136)	(0.127)	(0.047)	(0.047)	(0.047)	(0.047)
Age	0.052	0.109^{+}	0.053	0.109^{+}	0.002	0.037	-0.000	0.036
	(0.060)	(0.057)	(0.059)	(0.056)	(0.028)	(0.026)	(0.029)	(0.027)
Age^2	-0.007**	-0.006**	-0.007**	-0.006**	-0.001*	-0.001**	-0.001*	-0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Education *(reference group: less than Year 12)*								
$\geq$ Bachelors	1.043**	0.654**	1.031**	0.648**	0.265**	0.176**	0.274**	0.183**
	(0.158)	(0.146)	(0.158)	(0.146)	(0.056)	(0.052)	(0.057)	(0.052)
Vocational	0.514**	0.358*	0.516**	0.360**	0.172**	0.117*	0.185**	0.126*
	(0.152)	(0.140)	(0.152)	(0.139)	(0.057)	(0.052)	(0.059)	(0.053)
Year 12	0.831**	0.606**	0.830**	0.605**	0.262**	0.207**	0.271**	0.214**
	(0.161)	(0.148)	(0.161)	(0.148)	(0.058)	(0.053)	(0.059)	(0.053)
Marital status *(reference group: married)*								
Cohabiting	0.411**	-0.266*	0.407**	-0.267*	0.073*	-0.077*	0.067^{+}	-0.081*
	(0.117)	(0.119)	(0.117)	(0.119)	(0.035)	(0.034)	(0.035)	(0.034)
Separated/widowed	-1.134**	-1.036**	-1.145**	-1.044**	0.077	0.010	0.052	-0.008
	(0.155)	(0.148)	(0.155)	(0.148)	(0.083)	(0.082)	(0.083)	(0.083)
Never married	-0.561**	-1.496**	-0.582**	-1.509**	0.252**	0.044	0.233**	0.031
	(0.122)	(0.123)	(0.122)	(0.123)	(0.045)	(0.043)	(0.046)	(0.043)
Casual worker	-0.237*	-0.026	-0.125	0.043	-0.056	-0.014	0.022	0.042
	(0.101)	(0.096)	(0.125)	(0.116)	(0.038)	(0.037)	(0.049)	(0.047)
Ineligible: hours	-0.229	0.055	-0.020	0.184	-0.170**	-0.054	-0.012	0.059
	(0.168)	(0.162)	(0.208)	(0.198)	(0.060)	(0.056)	(0.076)	(0.071)
Ineligible: income	-1.058**	-0.426	-0.914**	-0.337	-0.179	0.034	-0.104	0.087
	(0.324)	(0.299)	(0.334)	(0.308)	(0.117)	(0.109)	(0.127)	(0.113)
No. of children		-2.905**		-2.914**		-0.242**		-0.257**
		(0.268)		(0.267)		(0.051)		(0.053)
No. of children^2		0.382**		0.384**		0.031**		0.033**
		(0.053)		(0.052)		(0.007)		(0.008)
Childless		-1.533**		-1.544**		0.500**		0.482**
		(0.310)		(0.308)		(0.061)		(0.062)
Observations	10430	10430	10430	10430	5010	5010	5010	5010
Individuals	3548	3548	3548	3548	2137	2137	2137	2137
First stage F			700.689	699.643			355.579	350.669

Standard errors clustered at the individual level in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Intended number of children is conditional on wanting at least one more child. All regressions include a full set of year fixed effects, state fixed effects, indicators for country of birth, Aboriginal/Torres Strait Islander status, a set of 19 industry indicators for ANSZIC 2006 Division codes, a set of 10 indicators for the number of employees at the woman's workplace, and linear and quadratic terms in personal and household income.

Table 4: Results by education group

	Intentions	Intended number
Year 12 or less	1.248	0.122
	(0.827)	(0.333)
Observations	3316	1442
Individuals	1400	762
First stage F-statistic	138.621	66.453
Beyond Year 12	0.357	0.375*
	(0.380)	(0.149)
Observations	7114	3568
Individuals	2368	1507
First stage F-statistic	507.616	267.239

Standard errors clustered at the individual level in parentheses. * $p < 0.05$. Full set of controls included as in column (2) of Table 3.

replacement rate for lower income women (Rossin-Slater, Ruhm, and Waldfogel 2013; Broadway et al. 2016). The conclusion that the significant intensive margin response is concentrated among more educated women is not in line with these findings.[19] One explanation for this is that more educated women are those with the most malleable fertility intentions: they are formulating plans to combine career progression with fertility. For these women, the introduction of a national paid leave scheme brings an ability to manage a career and access paid parental leave regardless of employer transitions. Women with less education may not be considering joint career and fertility decisions as carefully over a longer time period, or alternatively may face additional constraints such as childcare costs which mean that access to paid parental leave does not trigger a change in fertility intentions.

7.1 Including women uncertain about leave availability

Our main results presented above exclude observations where women respond 'don't know' when asked whether they, or others in their position at their employer, have access to paid maternity leave. This allows us to infer the impact of the PPL scheme on leave access. However, if women who do not know whether they have access to paid leave have systematically different fertility intentions compared to women who know their leave status then our results will be biased. For example, women who do not intend to have children may not be aware of their eligibility for paid maternity leave and other family-friendly workplace benefits, and their fertility intentions are unlikely to respond to the availability of paid leave. If these unaware and uninterested women are concentrated in

[19] There is, however, evidence that higher income households are more responsive to cash incentives for fertility in Germany (Riphahn and Wiynck 2017).

the private sector and do not have access to paid maternity leave, we may underestimate the impact of the PPL scheme's announcement on leave access in the private sector and at the same time overestimate the responsiveness of fertility intentions. This will lead our IV estimates of the impact of paid leave access to be biased upwards.

Table 5: Robustness tests

	Intentions	Intended number
A: including women who 'don't know' if they have access to paid leave		
Don't know = yes	0.437	0.467**
	(0.436)	(0.176)
First stage F-statistic	485.670	234.313
Don't know = no	0.296	0.311**
	(0.295)	(0.116)
First stage F-statistic	846.751	441.077
Observations	12691	6064
Individuals	3945	2401
B: fixing pre-announcement sector of employment		
Fixed sector	0.086	0.236+
	(0.409)	(0.141)
First stage F-statistic	445.220	272.148
Observations	10430	5010
Individuals	3548	2137

Standard errors clustered at the individual level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$. Full set of controls included as in column (2) of Table 3.

To test whether our results are robust to including women who answer 'don't know' when asked about paid maternity leave, we include them under two scenarios. First, we assume that all women answering 'don't know' do in fact have access to paid maternity leave, minimising the impact of the statutory PPL scheme. We then assume that these women don't have access to leave. Estimates of our main results under these two assumptions then provide estimates reflecting two extremes of measurement error.

Panel A of Table 5 reports these results. For both outcomes the two extreme assumptions generate estimates that bound the point estimates of our main results. The significant impact of access to leave on the intensive margin of fertility intentions is retained under both assumptions: access to paid parental leave increases the intended number of children by between 0.31 and 0.47, compared to our main estimate of 0.33.

7.2 Sector switching and participation decisions

A further threat to these estimates is that they could be driven by women changing their sector of work, or entering or leaving the labour market, in response to the PPL scheme's announcement. For example, if public sector workers with high fertility preferences move to private sector jobs in response to the PPL scheme's introduction, this would increase fertility intentions in the private sector and reduce fertility intentions in the public sector resulting in a positive estimated effect of paid parental leave access under our empirical strategy that does not reflect a true increase in fertility intentions. Similarly, if the PPL scheme causes higher fertility preference non-working women to enter the labour force and disproportionately choose private sector jobs, the same problem may arise. In our data, we see 26% of pre-announcement public sector workers switching to the private sector at some point in the post-announcement period, and 6% of private sector workers making the opposite move. It is therefore important to assess whether these sector changes explain the results estimated above.

We first reestimate our results fixing the woman's sector of employment as its pre-announcement category. This eliminates the effects of any switches between sectors. The results are shown in Panel B of Table 5. The point estimate for the extensive margin measure is much smaller and remains statistically insignificant. The magnitude of the intensive margin effect is also reduced, but is significant at the 10% level. This gives some reassurance that sector switching is not driving the intensive margin result we find.

The limitation of this test is that it does not account for women changing their participation decision in response to the scheme's announcement: some women may enter the labour force in response to the scheme. We therefore perform a series of tests of whether women's stated fertility intentions in 2007 (the last wave of data before the PPL scheme's announcement) are correlated with the propensity to change sectors, or to enter or exit the labour force. We distinguish between 'sector leavers' and 'sector joiners' for each sector, including women who either join or leave the labour force. For a given sector, we test whether future sector leavers and joiners have significantly different fertility intentions in 2007 relative to those remaining in the same sector. Any significant differences would indicate the possibility of bias in the results presented above as the average fertility preferences in the two sectors would have changed due to the leavers and joiners.

Table 6 presents results for these tests. We consider a woman a sector leaver if she is *ever* observed not working in that sector in the post-announcement waves (either not working, or working in the other sector). Comparably, a woman is a sector joiner if she does not work in the sector in 2007 (pre-announcement), but is *ever* observed working in that sector after the scheme's announcement.[20] For each of our outcome variables, we

[20] We reach the same conclusions if we instead consider the proportion of observations the woman is observed to be a sector leaver or joiner.

estimate whether there is a difference between average fertility preferences in the original group and the group of leavers or joiners. Any significant difference would indicate that the leavers and joiners changed average fertility intentions in the sector, contaminating the above results.[21]

Table 6: Tests for difference in fertility preferences of sector leavers and joiners

	Intentions	Intended number
Sector leavers		
Private sector	1.155**	-0.057
	(0.342)	(0.110)
Observations	799	434
Public sector	0.289	0.110
	(0.408)	(0.159)
Observations	297	149
Sector joiners		
Private sector	-0.005	0.103
	(0.430)	(0.179)
Observations	882	481
Public sector	0.383	-0.158
	(0.588)	(0.234)
Observations	345	174

Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$. All observations from wave 7, with sector joiners and leavers determined by subsequent observations. Controls as in columns (4) and (8) of Table 3.

These results indicate that private sector workers who either leave the labour force, or switch to a public sector job have higher extensive margin fertility intentions than private sector workers who remain. So, the workers leaving the sector will, other things being equal, reduce average fertility intentions in the private sector. With no other significant differences in sector leavers and joiners for these outcomes, this suggests that the estimate of no impact of paid parental leave access on extensive margin fertility expectations reported above is a lower bound for this impact: if all workers stayed in the sector, these reported preferences would be higher. For the intended number of children, we see no significant difference in the average conditional-on-positive number for workers leaving or joining either the public or private sector. This supports the interpretation of the above estimate as the causal impact of access to paid parental leave on the number of children a woman intends to have.

[21] We consider fertility intentions in wave 7 as any changes after the announcement of the PPL scheme could be driven by the scheme's announcement. For example, women entering the labour market and taking a private sector job in 2010 could have higher fertility intentions than existing private sector workers and these higher intentions could have been caused by the scheme's introduction.

7.3 Placebo tests

The validity of the results presented above relies upon there being no differences in pre-existing trends in either access to paid maternity leave or fertility intentions. Since our sample spans 2002-2013, we can use the pre-announcement data to perform placebo tests – in effect, to test whether there is any difference in these outcomes between public and private sector workers in the years prior to the 2009 announcement.

To test for pre-trends, and to examine the temporal nature of any estimated effects, we estimate reduced form regressions for leave access and our measures of fertility intentions. These regressions interact a set of indicator variables for the pre-announcement waves ($\tau_t,\ t = 3, 4, 6, 7$) with the private sector indicator $prisec_{it}$ – these are the 'placebo' regressors – as well as taking an event-study approach to post-announcement waves (τ_t, $t = 9, 10, 12, 13$)

$$\begin{aligned} outcome_{it} =& \beta_0 + \gamma_3\tau_3 * prisec_{it} + \gamma_4\tau_4 * prisec_{it} + \gamma_6\tau_6 * prisec_{it} + \gamma_7\tau_7 * prisec_{it} \\ &+ \gamma_9\tau_9 * prisec_{it} + \gamma_{10}\tau_{10} * prisec_{it} + \gamma_{12}\tau_{12} * prisec_{it} + \gamma_{13}\tau_{13} * prisec_{it} \\ &+ \beta_2 prisec_{it} + \beta_3 X_{it} + \theta_t + \varepsilon_{it} \end{aligned}$$

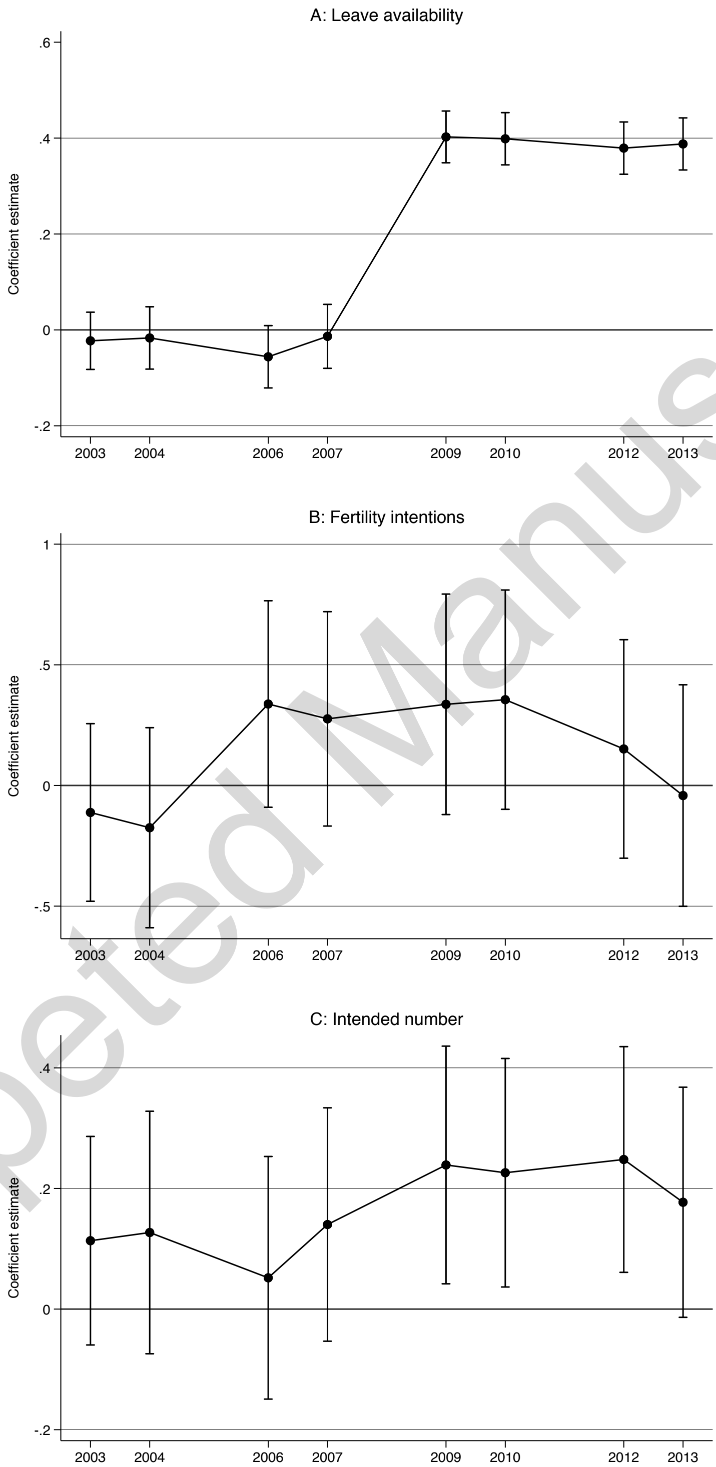
Here θ_t is the full set of time dummies and X_{it} is the full set of control variables as used in the results in Table 3. If workers in the public and private sectors have statistically indistinguishable trends in leave availability and fertility intentions before the scheme's announcement, the set of γ_t coefficients prior to the 2009 announcement will be insignificantly different from zero.

Figure 4 illustrates the estimated coefficients of interest from these regressions.[22] They show that for all outcomes, the pre-announcement coefficients are not significantly different from zero. Panel A shows that the difference in leave availability between the two employment sectors only emerges due to the scheme's announcement, and panel B shows that there is never a statistically significant difference in fertility intentions at the extensive margin.

Panel C reveals the emergence of the differential trend in the intended number of children only after the scheme is announced, providing confidence that the significant impact of paid parental leave access on the intended number of children is identified by exogenous variation in paid leave access driven by the PPL scheme's differential impact on workers in the public and private sectors. Panel C also clearly illustrates that the intensive margin response occurs after announcement, and does not only occur after the scheme's implementation in 2011, providing evidence that these fertility intentions respond in advance of the scheme's benefits being available.

[22] Estimates are presented in Appendix Table B3.

Figure 4: Placebo test illustrations



Point estimates of coefficients with 95% confidence intervals illustrated.

8 Conclusion

The existing body of research on the fertility impact of paid parental leave largely examines national policy changes in countries with existing, generous paid leave schemes (Lalive and Zweimüller 2009; Malkova 2018; Raute 2019; Stichnoth 2014; Cygan-Rehm 2016). In this paper, we consider the introduction of a modest paid leave scheme and examine its impact on fertility intentions. We exploit the fact that the scheme's introduction had a large impact on paid parental leave access for women working in the private sector, but comparatively little effect on having *any* leave access for public sector workers. We find that access to any PPL has no significant effect on fertility intentions at the extensive margin. Conditional on expecting to have at least one more child, access to paid leave increases the number intended by 0.34, a 16% increase. These results are not driven by changes in employment status or sector in response to the scheme's announcement.

Our results are driven by the fertility intentions of more highly educated women. These women are more likely to be concerned with planning how to combine work and childbearing. Moreover, this is a local average treatment effect: it is the effect of access to any paid leave for working women who gained access as a result of being in the private sector after the scheme's announcement. That is, it is the average effect among women working in the private sector who did not have employer-provided paid parental leave. This is an estimate of particular interest if we wish to consider the introduction of such a scheme in an alternative setting.

We interpret these results as reflecting career-oriented women selecting into jobs offering a rich package of employment benefits that include paid parental leave, but at the same time having lower intensive margin fertility intentions than their less career-oriented counterparts. Failing to account for this selection leads to an underestimation of the impact of paid parental leave on fertility intentions at the intensive margin. Given that the scheme's stated objectives include improving work-family balance and increasing the labour force participation of mothers, the concentration of the effect on fertility intentions among highly educated women reinforces the success of the scheme in meeting its aims.

If the established relationship between stated fertility intentions and realised fertility (documented in Section 3) is stable over the time period in question, it is reasonable to expect that the increase in fertility intentions identified in this paper will be followed by a future increase in realised fertility.[23] Thus, despite not being intended as a pronatalist policy, the PPL scheme may eventually lead to increased fertility. Since Australia's total fertility rate is currently below replacement, this can be interpreted as an added bonus rather than an unwanted side effect of the program.

[23] As outlined in Section 4, the HILDA Survey does not have sufficient power to detect changes in realised fertility. Undertaking analysis of realised fertility using administrative data presents a fruitful avenue for future research.

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Appendices

A Fertility intentions data description

Our analysis of fertility intentions is based on two measures: a rating of how likely an individual reports being to have a child or more children in the future on a ten-point Likert scale, and, if the response is rated as 6 or more, the number of additional children they intend to have. These survey questions are included consistently across most waves of HILDA. However, in waves 5, 8 and 11 a rotating module on 'fertility and family' appeared in the HILDA survey, and incorporated the fertility intention questions among a wider range of questions. This made responses from these waves inconsistent with others for four reasons. First, in waves 5, 8 and 11, before the module respondents were warned that the questions were personal and they were free to refuse to answer at any time. Consequently there are more refusals in these waves. Second, the fertility intention questions were only asked to respondents who reported that they and their partner had not been sterilised or were otherwise infertile. By excluding sterilised and infertile women, those who do answer have disproportionately high fertility intentions compared with other waves. Third, the intended number of children question is asked to all fertile respondents in waves 5, 8 and 11 and included the option of answering 'zero', instead of being restricted to those answering 6 or higher to the fertility intentions question. Finally, in waves 5, 8 and 11, the two questions were asked in the opposite order, potentially affecting responses.[24]

This range of differences in the question positioning and who was asked make these three waves quite different to others, and so our preferred sample excludes them. However, the information contained in waves 5, 8 and 11 brings additional information: a report on whether respondents are infertile or have been sterilised. For our main analysis sample, we use this data by dropping observations for women who report they are infertile or have been sterilised for all subsequent waves, following the approach of Risse (2010).

There are a range of different approaches that can be taken to either include or exclude data from these three waves. To demonstrate that our particular approach of (a) excluding observations from waves 5, 8 and 11, and (b) excluding women who report infertility from that point onward, does not drive the results we find, we present results based on two alternative approaches. The results are shown in Table A1.

Panel A of Table A1 shows the estimates of interest for our preferred sample, as in the first row of Table 3. Panel B does not exclude women reporting infertility, making no use of any data from waves 5, 8 and 11. Panel C instead includes data from the three otherwise-excluded waves, whilst dropping women reporting infertility. To make the fertility intentions responses as comparable as possible, only those reporting 6 or

[24]Keygan (2017) shows that men's reported fertility intentions are significantly higher in waves 5, 8 and 11 than in other waves, based on a consistent sample of men.

higher on the extensive margin intensive questions are included in the intensive margin intended number analysis, and any reports of zero children that remain are dropped.

The three different sample selection approaches deliver qualitatively similar results, showing no effect of paid leave access on extensive margin fertility intention, but a positive and significant effect of around 0.3 on the intended number of children. This gives confidence that our sample selection is not driving our results.

Table A1: Main results: robustness checks with varying sample

	Intentions				Intended number			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Preferred sample								
Leave access	-0.032	-0.039	0.553	0.326	0.058	0.053	0.453^{**}	0.335^{**}
	(0.102)	(0.095)	(0.372)	(0.337)	(0.040)	(0.038)	(0.140)	(0.130)
Observations	10430	10430	10430	10430	5010	5010	5010	5010
Individuals	3548	3548	3548	3548	2137	2137	2137	2137
First stage F			700.689	699.643			355.579	350.669
B: Do not exclude infertile								
Leave access	-0.038	-0.037	0.542	0.318	0.053	0.045	0.399^{**}	0.285^{*}
	(0.090)	(0.083)	(0.330)	(0.296)	(0.038)	(0.037)	(0.130)	(0.121)
Observations	13121	13121	13121	13121	5305	5305	5305	5305
Individuals	4246	4246	4246	4246	2291	2291	2291	2291
First stage F			960.040	955.809			438.834	433.187
C: Exclude infertile; include waves 5, 8 and 11 with appropriate adjustment								
Leave access	0.063	0.044	0.411	0.176	0.064	0.057	0.409^{**}	0.274^{*}
	(0.094)	(0.088)	(0.355)	(0.323)	(0.035)	(0.034)	(0.135)	(0.125)
Observations	13535	13535	13535	13535	6603	6603	6603	6603
Individuals	3710	3710	3710	3710	2296	2296	2296	2296
First stage F			730.363	729.621			369.022	364.305

Standard errors clustered at the individual level in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. All regressions include a full set of controls as in Table 3.

B Tables

Table B1: Coefficient estimates: intrinsic estimator of age, year and cohort effects in Australian fertility rates

Age		Year				Cohort					
15	-48.16	1975	16.87	2010	2.73	1926	-17.70	1961	13.51	1996	-9.10
	(2.19)		(2.41)		(2.41)		(13.97)		(2.47)		(5.32)
16	-43.28	1976	13.46	2011	2.32	1927	-15.60	1962	14.04	1997	-8.54
	(2.16)		(2.41)		(2.41)		(9.89)		(2.47)		(5.74)
17	-33.80	1977	11.11	2012	3.32	1928	-13.91	1963	13.76	1998	-6.75
	(2.16)		(2.41)		(2.41)		(8.09)		(2.47)		(6.28)
18	-23.29	1978	8.68	2013	2.51	1929	-12.28	1964	13.08	1999	-4.69
	(2.16)		(2.41)		(2.41)		(7.02)		(2.47)		(7.01)
19	-11.07	1979	6.72	2014	0.58	1930	-10.92	1965	12.78	2000	-2.84
	(2.16)		(2.41)		(2.41)		(6.29)		(2.47)		(8.08)
20	-1.19	1980	5.61	2015	1.04	1931	-9.58	1966	12.31	2001	-0.62
	(2.16)		(2.41)		(2.41)		(5.76)		(2.46)		(9.89)
21	8.22	1981	6.23	2016	1.29	1932	-8.87	1967	12.58	2002	1.33
	(2.16)		(2.41)		(2.41)		(5.34)		(2.46)		(14.42)
22	19.23	1982	5.47	2017	0.18	1933	-8.30	1968	12.58		
	(2.16)		(2.41)		(2.47)		(5.01)		(2.46)		
23	30.60	1983	4.72			1934	-7.93	1969	12.76		
	(2.16)		(2.41)				(4.73)		(2.49)		
24	41.87	1984	1.72			1935	-7.61	1970	12.76		
	(2.16)		(2.40)				(4.50)		(2.53)		
25	53.09	1985	3.51			1936	-8.17	1971	11.85		
	(2.15)		(2.40)				(4.30)		(2.57)		
26	61.84	1986	1.38			1937	-8.60	1972	13.46		
	(2.15)		(2.40)				(4.13)		(2.61)		
27	67.95	1987	0.07			1938	-9.22	1973	14.00		
	(2.15)		(2.40)				(3.97)		(2.65)		
28	71.36	1988	-0.96			1939	-9.89	1974	13.73		
	(2.15)		(2.40)				(3.84)		(2.69)		
29	71.11	1989	-1.47			1940	-10.87	1975	14.21		
	(2.15)		(2.40)				(3.71)		(2.74)		
30	65.97	1990	-0.35			1941	-12.32	1976	13.84		
	(2.15)		(2.40)				(3.60)		(2.79)		
31	58.96	1991	-2.61			1942	-13.28	1977	13.60		
	(2.15)		(2.40)				(3.50)		(2.84)		
32	49.03	1992	-2.21			1943	-13.61	1978	13.22		
	(2.15)		(2.40)				(3.41)		(2.89)		
33	37.34	1993	-3.75			1944	-14.24	1979	12.66		
	(2.15)		(2.40)				(3.32)		(2.95)		
34	25.65	1994	-4.97			1945	-13.30	1980	10.37		
	(2.15)		(2.39)				(3.24)		(3.01)		
35	13.73	1995	-6.15			1946	-11.99	1981	8.78		
	(2.15)		(2.39)				(3.17)		(3.07)		
36	1.36	1996	-7.42			1947	-10.94	1982	7.08		
	(2.15)		(2.39)				(3.10)		(3.14)		
37	-10.13	1997	-8.44			1948	-7.21	1983	3.99		
	(2.15)		(2.39)				(3.03)		(3.21)		
38	-19.83	1998	-9.19			1949	-3.62	1984	1.26		
	(2.15)		(2.39)				(2.97)		(3.29)		
39	-27.96	1999	-9.50			1950	0.76	1985	-2.02		
	(2.15)		(2.39)				(2.91)		(3.38)		
40	-35.17	2000	-9.35			1951	3.64	1986	-5.03		
	(2.16)		(2.40)				(2.86)		(3.47)		
41	-40.71	2001	-9.85			1952	7.68	1987	-7.99		
	(2.16)		(2.40)				(2.81)		(3.57)		
42	-44.58	2002	-8.60			1953	10.33	1988	-10.73		
	(2.16)		(2.40)				(2.76)		(3.68)		
43	-47.09	2003	-8.18			1954	12.59	1989	-13.12		
	(2.16)		(2.40)				(2.71)		(3.81)		
44	-48.57	2004	-6.96			1955	14.36	1990	-14.24		
	(2.16)		(2.40)				(2.67)		(3.94)		
45	-49.08	2005	-4.31			1956	14.61	1991	-14.39		
	(2.16)		(2.40)				(2.62)		(4.10)		
46	-49.03	2006	-2.65			1957	16.14	1992	-13.60		
	(2.16)		(2.40)				(2.58)		(4.27)		
47	-48.73	2007	1.60			1958	16.02	1993	-12.96		
	(2.16)		(2.40)				(2.54)		(4.47)		
48	-48.21	2008	3.25			1959	14.90	1994	-11.66		
	(2.16)		(2.40)				(2.50)		(4.71)		
49	-47.42	2009	2.50			1960	14.05	1995	-10.38		
	(2.17)		(2.40)				(2.46)		(4.99)		

Table B2: First stage. Dependent variable: (anticipated) access to any paid parental leave

	Full sample		Intended number sample	
	(1)	(2)	(3)	(4)
Private sector*post	0.413**	0.413**	0.402**	0.402**
	(0.015)	(0.015)	(0.021)	(0.021)
Private sector	-0.329**	-0.329**	-0.332**	-0.332**
	(0.018)	(0.018)	(0.024)	(0.024)
Age	0.000	0.000	0.007	0.007
	(0.006)	(0.006)	(0.012)	(0.012)
Age^2	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Education *(reference group: less than Year 12)*				
Bachelors or higher	0.013	0.012	-0.026	-0.026
	(0.016)	(0.016)	(0.024)	(0.024)
Vocational education	-0.013	-0.013	-0.037	-0.037
	(0.015)	(0.015)	(0.023)	(0.023)
Completed Year 12	-0.004	-0.004	-0.024	-0.024
	(0.016)	(0.016)	(0.024)	(0.024)
Marital status *(reference group: married)*				
Cohabiting	0.004	0.004	0.009	0.009
	(0.010)	(0.010)	(0.013)	(0.013)
Separated/widowed	0.015	0.016	0.060	0.060
	(0.019)	(0.019)	(0.049)	(0.049)
Never married	0.037**	0.037**	0.044**	0.045**
	(0.011)	(0.012)	(0.016)	(0.016)
Casual worker	-0.189**	-0.189**	-0.194**	-0.194**
	(0.011)	(0.011)	(0.016)	(0.016)
Ineligible: hours	-0.353**	-0.353**	-0.399**	-0.400**
	(0.023)	(0.023)	(0.035)	(0.034)
Ineligible: income	-0.269**	-0.271**	-0.226**	-0.228**
	(0.049)	(0.049)	(0.078)	(0.078)
No. of children		0.025		0.040
		(0.018)		(0.029)
No. of children^2		-0.006		-0.008*
		(0.004)		(0.004)
Childless		0.022		0.033
		(0.022)		(0.033)
Observations	10430	10430	5010	5010
Individuals	3548	3548	2137	2137

Standard errors clustered at the individual level in parentheses. $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Full set of controls as in Table 3.

Table B3: Placebo tests

	(1) Leave access	(2) Intentions	(3) Intended number
Pre-announcement			
Private sector*wave 3	-0.023	-0.112	0.113
	(0.030)	(0.188)	(0.088)
Private sector*wave 4	-0.017	-0.175	0.127
	(0.033)	(0.211)	(0.103)
Private sector*wave 6	-0.056^{+}	0.338	0.052
	(0.033)	(0.218)	(0.103)
Private sector*wave 7	-0.013	0.276	0.140
	(0.034)	(0.227)	(0.099)
Post-announcement			
Private sector*wave 9	0.403**	0.336	0.239*
	(0.028)	(0.233)	(0.100)
Private sector*wave 10	0.399**	0.356	0.226*
	(0.028)	(0.232)	(0.097)
Private sector*wave 12	0.379**	0.151	0.248**
	(0.028)	(0.231)	(0.095)
Private sector*wave 13	0.388**	-0.042	0.177^{+}
	(0.028)	(0.234)	(0.097)
Obeservations	10430	10430	5010
Individuals	3548	3548	2137

Standard errors clustered at the individual level in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Full set of controls included as in column (2) of Table 3.