Children and the Gender Earnings Gap: Evidence for Australia

Elif Bahar, Natasha Bradshaw, Nathan Deutscher and Maxine Montaigne[1,2]

[1] Macroeconomic Analysis and Policy Division, Macroeconomic Group, The Treasury, Langton Crescent, Parkes ACT 2600, Australia. Correspondence: nathan.deutscher@treasury.gov.au. We thank Rebecca Cassells, Mark Cully, Pauline Grosjean, Kristen Sobeck, colleagues in the Social Policy Division, and seminar participants at the Treasury, the Australian Conference of Economists 2022, and the e61 Institute for helpful feedback and suggestions.

[2] The views expressed in this paper are those of the authors and do not necessarily reflect those of The Australian Treasury or the Australian Government.

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The paper uses data from the Australian Taxation Office (ATO) Longitudinal Information Files (ALife), specifically the 2019 release of the individuals' file. The findings and views reported are those of the authors and should not be attributed to the ATO or the Australian Government.

Abstract

We estimate the impact of children on the gender earnings gap in Australia using an event study approach. We show the arrival of children has a large and persistent impact on the gender earnings gap, reducing female annual earnings by 53 per cent, on average, in the first 5 years of parenthood. We attribute the gap in earnings to lower participation rates and reduced working hours among mothers, including a shift to part-time work. Although the decline in earnings for women is similar regardless of their breadwinner status prior to children, women with greater access to workplace flexibility are more likely to remain employed after having children.

JEL Classification Numbers: J13, J16, J22, J31
Keywords: children, gender earnings gap, labour supply, wage differential, norms, discrimination, workplace policies

1. Introduction

Despite convergence in recent decades, there remain large disparities in earnings between men and women in Australia, as in many other countries. International literature has shown that the differential impacts of children on men's and women's earnings is the primary driver of gender earnings gaps (Kleven et al. 2019a) and has been documented in varying magnitudes in a number of countries (Kleven et al. 2019b). This divergence in earnings following entry into parenthood is often termed the 'motherhood penalty'.

In this paper, we examine the impact of children on the gender earnings gap in Australia, identify the motherhood penalty in Australia up to a decade following the arrival of the first child, and compare the results to international estimates. We define earnings as annual wage and salary income, rather than using broader measures of income that include capital income, or narrower measures such as the commonly discussed 'gender pay [wage rate] gap' (WGEA 2023), which abstract from important differences in employment and hours worked. This allows us to provide a holistic picture of diverging labour market experiences following parenthood and is consistent with existing literature on the motherhood penalty.

We follow Kleven et al.'s (2019a) approach of using an event study to show that the arrival of children has a large and persistent impact on the earnings of women, but not of men. Using data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, we show that women's annual earnings fall behind men's by an amount averaging 53 per cent in the first 5 years after entry into parenthood, while men's are unchanged. To test for longer-run effects, we extend this analysis using administrative tax data which shows a similar gap averaging 47 per cent persists for the first 10 years following parenthood. Precise estimates of the 'motherhood penalty' vary across datasets, years and sample selection rules but the general patterns remain the same. We attribute the gap in earnings to lower participation rates and reduced working hours amongst mothers, and to a lesser extent weaker growth in hourly wages. We show that men's earnings are not significantly affected by parenthood.

We then show, through a gender earnings gap decomposition, that the share of gender inequality attributable to the motherhood penalty has increased substantially since the early 2000s. In 2001, the motherhood penalty explained around half of all gender earnings inequality; by 2019, this had increased to over four-fifths. The increase is likely due to a decline in non-child related drivers of inequality, such as the rise in female educational attainment or decline in employment discrimination.

We also contribute to the literature by investigating several potential drivers of the motherhood penalty in Australia that have emerged from the international literature. Health issues related to birth and biological differences have largely been ruled out as a cause through studies which compare gender differences in outcomes of same-sex and adoptive parents (Anderson & Nix 2022; Kleven et al. 2021). Some earlier studies have also pointed to the role of employer discrimination (Blau & Kahn 2000; Neumark et al. 1996).

One explanation with mixed evidence is that the reduction in a mother's earnings may result from choices within households based on each parent's comparative advantage in the labour market. Recent Australian work (Siminski & Yetsenga 2022) examines HILDA Survey data and finds little to no evidence that comparative advantage plays a role in the sexual division of labour in households. Internationally, Anderson & Nix (2022) rule out fathers' labour market advantage as a cause of the motherhood penalty in Norway by showing that child penalties experienced by women are significantly larger in heterosexual couples compared to same-sex couples, and that the contrast to same-sex couples remains even controlling for several measures of pre-determined relative labour productivity differences between spouses. In contrast, using Swedish administrative data,

Angelov et al. (2016) show that gender gaps following children are lower, or even negative, when the mother has higher earnings and education relative to the father. In our paper, we show that the motherhood penalty is the same regardless of a woman's breadwinner status prior to children. We find that the penalty is significant even in households where the woman earns double or more what her partner does, implying that relative earnings prior to children have little influence on the intrahousehold allocation of paid work following children.

We then investigate workplace outcomes and the role of flexibility. Following parenthood women are more likely to work part-time and variable days, with an increase in casual status and underemployment over time. Reflecting time out of the labour force, women's progress along the job ladder also appears to slow, as they are also less likely to be promoted or to change jobs. We also provide suggestive evidence that women working in more flexible occupations before parenthood are more likely to remain employed after the arrival of children. However, for women who remain employed, the hourly wage penalty is larger in more flexible occupations, which may reflect foregone promotion opportunities. This is consistent with other research that finds flexible work can restrict mothers' earnings in industries or occupations that disproportionately penalise short or flexible working hours (Cortes & Pan 2019; Goldin 2019). Importantly, we also show that women with less access to flexibility are more likely to exit the labour force entirely. These results suggest flexibility can be a double-edged sword, allowing some mothers to remain engaged in work, but potentially at the cost of a lower hourly wage than they might otherwise earn.

A final potential explanation for the reduction in women's earnings relative to men's after children could be differences in preferences for providing care. While such preferences likely play a substantial role in determining the allocation of work and care responsibilities between parents, we provide suggestive evidence that other factors are also at play. We observe a fall in women's satisfaction with their employment opportunities after the arrival of children. We also observe a rise in work-life conflict, including for fathers, who are more likely to report that work affects their family life after the arrival of children. This suggests potential gains from more equal division of caring responsibilities.

Lastly, we discuss the role of broader gender norms, which have been previously shown to have strong implications for labour market outcomes and family dynamics (Avdic & Karimi 2018; Bertrand et al. 2015; Bittman et al. 2003; Kuziemko et al. 2020). Kleven et al. (2019a) show that in Denmark child penalties are transmitted through generations, from parents to daughters (but not sons), as girls growing up in more traditional families experience larger motherhood penalties. Kleven et al. (2019b) also show a strong correlation between stated gender norms and the motherhood penalty, with more conservative countries experiencing larger penalties. We further this strand of the literature by placing Australia within the international landscape, showing Australia has both a high motherhood penalty and is relatively conservative in its gender norms.

Overall, our results highlight the strong labour supply implications of entry into parenthood for women. These results are consistent with international studies suggesting these differences in labour market outcomes do not purely reflect different preferences between the genders, but also constraints that may be driven by workplace and social norms, institutions and policy settings. This suggests a role for policy in reducing these constraints, to allow new parents to share work and care responsibilities more evenly. To our knowledge, we are the first paper to provide causal evidence of the motherhood penalty in both survey and tax data for Australia.

2. Background

Gendered pay inequity in Australia is rooted in historical industrial practices and views of men's and women's roles. As early as 1912, women's wages were set at 54 per cent of men's, based on the 'breadwinner assumption' that men needed to be paid extra to provide for their family, since married women generally did not work outside of the home. This difference only applied in female-dominated occupations to ensure women's lower wages did not put men out of work, creating a strong wage-based incentive for gendered occupational segregation (Pocock 1999). Important legislative changes including the equal work for equal pay decision in 1969 and the equal pay for work of equal value decision in 1972 were significant in narrowing the gender pay gap in Australia throughout the 1970s, putting Australia's pay equity ahead of our international counterparts.

As in many other countries, women's labour force participation rates have increased significantly over the past several decades, and the gender pay gap has gradually narrowed over time. These gains coincided with a significant long-term increase in the level of women's educational attainment, allowing women to enter skilled professions and access better paid jobs. At the time of writing, the gender pay gap in weekly ordinary time earnings for full-time employees is 13.0 per cent and reflects the persistent effects of industrial and occupational segregation, women's time out of the workforce and higher rates of part-time work, and discrimination and bias (WGEA 2023). This gap is substantially higher when overtime and other earnings, and part-time employees are included. Australia has relatively high rates of female workforce participation compared to other OECD countries, but Australian women are much more likely to work part-time, and the female participation rate remains around 9 percentage points below men's (ABS 2022a). Survey evidence suggests that care responsibilities are the primary reason for women not participating in the workforce or working part-time, as women continue to take on the bulk of unpaid work in Australian households (ABS 2020a).

Expectations about the roles of men and women in Australia also have deep historical roots (Grosjean & Khattar 2019), although they continue to evolve. Our analysis using the Household, Income and Labour Dynamics in Australia (HILDA) survey indicates that the share of Australians who agree with the statements "mothers who don't need the money really shouldn't work" and "a pre-school child is likely to suffer if his/her mother works full-time" declined by 15 percentage points between 2005 and 2019.[3] However, a sizeable minority of Australians (26 and 17 per cent, respectively) continued to agree with the statements.

Some of the policy levers available to address pay inequity for mothers are child care and paid parental leave settings. In 2011, Australia became the second-last OECD country to implement a national paid parental leave scheme. The current scheme offers 20 weeks of leave to be shared flexibly to eligible parents, paid at the national minimum wage, and is funded entirely from public funds. Child care in Australia operates primarily in a market-based system, and the Australian Government pays a means-tested subsidy to providers that substantially reduces out-of-pocket child care costs, particularly for lower- and middle-income families. Over the past several decades, there has been a substantial increase in the use of formal child care in Australia, and in average weekly hours used (ABS 2018). However, at the end of the 2010s average out of pocket costs remained relatively high in Australia, compared to other OECD countries (OECD 2020).

[3] Survey respondents are asked in a self-completion questionnaire to state how much they agree with each statement, on a scale of one to seven. We define 'agree' as selecting a number greater than 4.

3. Data

We use two balanced, longitudinal datasets to examine the role of children in the gender earnings gap in Australia – drawn from the Household, Income and Labour Dynamics in Australia (HILDA) survey and the Australian Taxation Office (ATO) Longitudinal Information Files (ALife). Here we briefly describe both datasets, their strengths and limitations in this context, and provide some descriptive information about the analysis samples.

The Household, Income and Labour Dynamics in Australia (HILDA) survey

HILDA is an annual longitudinal survey of Australian households from 2001 onwards; we use the 2021 release but restrict attention to the pre-COVID-19 period (waves 1-19). Each year HILDA collects information on just under 10,000 households, with most surveys conducted from August through to October. Respondents (and their households) are asked questions about their income, employment and educational characteristics. In addition, HILDA includes information about satisfaction with life and work, which are helpful in exploring the mechanisms that might underlie the motherhood penalty.

We identify our key event, the arrival of a child, from changes in response to the question concerning the number of children ever had (`tchad`). We identify the first year of parenthood, time zero in our event study, as the year in which the number of children a respondent has ever had increases from zero to a positive number.

Our key outcome variable is financial year gross wages and salary (`wsfei`), which will cover all jobs held through the year. This will capture the influence of changes in employment, hours worked and hourly wage rates. To explore these margins of adjustments we use current broad labour status (`esbrd`), hours per week usually worked in all jobs (`jbhruc`), and weekly gross wages and salary in all jobs (`wscei`). From these we can construct an hourly wage rate, which we windsorise at the top and bottom percentiles.[4] We describe other outcome variables as they are drawn on in the analysis.

The detail available in HILDA allows us to paint a relatively rich picture of the motherhood penalty in Australia. Nonetheless, the sample sizes in HILDA limit our ability to precisely estimate the motherhood penalty over longer periods of time and may constrain other more data-intensive explorations of the motherhood penalty in the future. With this in mind, we turn to administrative tax data.

The ATO Longitudinal Information Files (ALife)

ALife is a longitudinal administrative dataset linking individual tax returns from the 1991 income year onwards (Carter et al. 2021).[5] We use the 2018 release, which is a random 10 per cent sample of taxpayers lodging returns over the 28-year period from the 1991 to 2018 income years. The data includes information from the tax returns and client records of these taxpayers. As such, ALife has high quality data on individual incomes and captures core demographic information such as age and gender.

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4 These choices of outcome variables are all consistent with recommended approaches in the HILDA User Manual (Summerfield et al 2021).

5 The individual is the primary unit of taxation in Australia (though there are few return items subject to couple income tests). Income years run from 1 July through to 30 June; we refer to income years with reference to the year in which they end.

To identify when people become parents we need to make two restrictions to our ALife sample. First, we focus on the period from 2000 on to benefit from improvements in the data collected. Second, we require individuals to have lodged a tax return in the years immediately before and the year of parenthood in order to isolate the parenthood event. The latter of these is likely to result in a sample that is somewhat positively selected.

The choices above reflect the key drawback of ALife data—children are only observed when recorded on the tax return, and the requirement to do this has varied over time. Individuals are only required to report the number of dependent children for income test purposes for Medicare levy reductions, private health insurance rebates or other tax offsets. However, individuals are able to report dependent children even if not required to, and the reporting of dependent children has increased over time.[6] We focus on income years from 2000 onwards to draw on the ALife derived variable for the number of dependent children (`c_depend_child`). We identify the first year of parenthood, time zero in our event study, as the year in which the number of dependent children increases from zero to either one or two. We exclude any larger jumps in numbers of dependent children as given the drawbacks of the tax data, they may reflect misreporting rather than genuine multiple births.

Earnings data in ALife is more comprehensive than data on children. Our key outcome variable is once again based on financial year wage and salary income, across all jobs (`i_salary_wage`). This allows for comparison with the HILDA analysis. The standard ALife dataset only consists of tax return data, which raises the question of how to treat individuals in years when they do not lodge a tax return.[7] We first supplement the data by linking it with the non-lodger module, which contains 3rd party reported wage and salary income for individuals regardless of whether they lodged a tax return or not. In years in which an individual still has no data, we set earnings equal to zero. Finally, we exclude years where an individual worked overseas for all or part of the year (which can be inferred from responses to `a_part_year_months`). Given this, the only years in which someone is not observed in our ALife sample are those years when they are working overseas for part of the year. All other years have earnings reported (by the individual or a 3rd party) or imputed to zero.

In our baseline ALife analysis we consider a balanced panel from the resulting dataset. This requires only that the individuals became parents between 2003 and 2008 (inclusive) and hence are in the time period covered by our panel (2000 to 2018 inclusive) for 3 years before and 10 years after parenthood; and that they are not identified as being overseas in any of those 14 years.

Sample selection and summary statistics

For the estimation of the motherhood penalty, we focus on a balanced panel of individuals observed for 3 years prior to parenthood in both datasets, and for 5 years following in HILDA and 10 years following in ALife. 'Observation' here means, respectively, that the individual has completed the HILDA survey questionnaire containing the outcome variable in question or that, in ALife, they were not working overseas for part of the year. This leaves us with 1,399 people in HILDA observed over 9 years (12,591 person-years), and 40,707 people observed over 14 years in ALife (569,898 person-years).

In Table 1 we provide some summary statistics in the year prior to parenthood. While there is some evidence of positive selection in the ALife sample, it is relatively modest. Median individual age prior to parenthood is slightly higher in the ALife sample, with a difference of two years for women and one year for men. HILDA earnings are higher, but this will largely reflect economic growth given this sample is typically being observed later in the 2000s. The proportion with zero earnings is broadly

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6 See summary statistics provided at https://alife-research.app/.

7 We thank an anonymous referee for the comment that led to us bringing in the non-lodger module, and a more careful examination of sample selection issues in the ALife dataset more broadly.

similar although it is smaller in ALife than in HILDA. Consistent with the idea that we may be missing some zero- or low-wage women in ALife who do not file a tax return either side of parenthood, the proportion of women with zero earnings is slightly smaller in ALife than in HILDA, and we observe more men than women in the ALife sample, while the reverse is true in HILDA.

In the analysis that follows we use the HILDA data unless otherwise stated. The ALife data is used purely for the estimation of the long-run motherhood penalty over the first decade of parenthood. While outside the scope of this paper, the ALife data may also provide an avenue for future work on, for example, finely grained geographic variation in the motherhood penalty (as in Kleven 2022), while noting potential remaining sources of bias in the data.

Table 1: Summary statistics

	Women	Men	All
HILDA sample			
N	735	664	1,399
Age	29.0	32.1	30.5
	[29.0]	[31.0]	[30.0]
	(6.5)	(8.3)	(7.6)
Hours worked	30.4	40.2	35.2
	[38.0]	[40.0]	[40.0]
	(17.4)	(16.9)	(17.8)
Earnings (2022-23)	64,289	83,348	73,555
	[62,263]	[75,683]	[68,561]
	(47,537)	(65,846)	(57,944)
Prop. zero earnings (%)	10.1	12.1	11.1
ALife sample			
N	22,026	22,300	40,707
Age	31.5	33.5	32.6
	[31.0]	[31.5]	[31.0]
	(8.2)	(8.9)	(8.6)
Earnings (2022-23)	54,100	67,866	61,641
	[51,453]	[63,830]	[57,572]
	(39,902)	(59,580)	(52,072)
Prop. zero earnings (%)	7.6	12.8	10.4

Notes: Statistics reported for year before parenthood for our balanced panels of parents in the HILDA (waves 1-19) and ALife (income years 2000-2018) datasets. To be included in the panel, individuals were required to be observed 3 years prior to parenthood in both datasets, and for 5 years following in HILDA and 10 years following in ALife. Population weights have been used for the HILDA sample. For age, hours worked and earnings we report means, medians (square brackets) and standard deviations (round brackets). Earnings has been CPI adjusted to 2022–23 dollars.
Source: Authors' calculations using HILDA Release 21.0, ALife 2018 and ABS (2022b).

For some HILDA analysis, where noted, we will restrict to those individuals who had a responding partner in the year prior to children. In these cases, we include all partnered individuals, regardless of whether the relationship continued following children. While of significant interest, relationship dynamics following children are not easily captured in the event study framework we use given their potential endogeneity with our other outcomes of interest.

4. Children and the Gender Earnings Gap

Event study methodology

To identify the impact of parenthood on outcomes for mothers and fathers we adopt the event study methodology introduced in Kleven et al. (2019a). For ease of comparison, we use the same notation as in that paper. The approach exploits sharp changes in outcomes around the arrival of the first child, after controlling flexibly for lifecycle and time trends.

The event study methodology exploits differences in the timing of children among parents, rather than comparing people who have children to people without children. Among those who do have children, most unobserved determinants of outcomes would be expected to be relatively smooth over time, while entry into parenthood creates sharp changes in these outcomes. The identifying assumptions are nonetheless strong, particularly that birth timing needs to be as good as randomly assigned, conditional on age and year fixed effects. Here we rely on analysis by Kleven et al. (2019a) showing that the event study methodology produces almost identical results to even more rigorous approaches.[8] This provides strong evidence that an event study methodology drawing on within-person variation can causally identify the impacts of children on labour market and other outcomes.

Our baseline event study model adopts the following form:

$$Y_{ist}^{g} = \Sigma_{j\neq -1}\alpha_j^g . I[j = t] \; + \; \Sigma_k \, \beta_k^g . I[k = age_{is}] + \; \Sigma_y \, \gamma_y^g . I[y = s] + \; \varepsilon_{ist}^g \qquad (1)$$

where the dependent variable Y_{ist}^{g} is the observed outcome for individual *i* of gender *g*, in year *s* and at event time *t*. For each individual, event time *t* is zero in their first year as a parent. We run the regression separately for men (g=m) and women (g=w). The first term on the right-hand side is a full set of event time fixed effects α_j^g, excluding the year before parenthood (i.e. *j*=-1). We also include a full set of age (β_k^g) and year (γ_y^g) fixed effects. The age effects control for underlying lifecycle trends in earnings, while the year effects control for time trends such as the business cycle and inflation.

With this approach there are three different estimates of the effect of children that will be of interest. We will draw on each through the remainder of the paper and describe them in turn.

First, there are the estimated event study coefficients $\hat{\alpha}_t^g$, from equation (1). These capture the *level* effect of children on the relevant outcome of interest for a given gender g and event time t. For many variables such as employment, hours worked, and time use this has a clear and concrete meaning.

Second, we can express the estimated event study coefficients in *relative* terms as a percentage of the counterfactual outcome in the absence of children. To estimate this counterfactual we take the predicted values $\tilde{Y}_{ist}^{g}$ from equation (1) setting the event time effects to zero, and average the result for the sample observed at each event time t to generate $E[\tilde{Y}_{ist}^{g}|t]$. The resulting P_t^g capture the percentage effect of children on the relevant outcome of interest for a given gender g and event time t. For variables such as earnings this may be of more interest.

$$P_t^g \equiv \frac{\hat{\alpha}_t^g}{E[\tilde{Y}_{ist}^{g}|t]} \qquad (2)$$

[8] Kleven et al. (2019a) show several robustness tests estimating their results using difference-in-differences and IV estimators, which produce results that are almost perfectly aligned with the event study approach.

Third, we can look at the difference between the event study coefficients for men and women as a percentage of the counterfactual outcome for women in the absence of children. This measures the percentage by which women are falling behind men due to children at event time *t*.

$$P_t \equiv \frac{\hat{\alpha}_t^m - \hat{\alpha}_t^w}{E[\bar{Y}_{ist}^w | t]} \quad (3)$$

The above provides a comparative view of how women differ in their experiences of parenthood relative to men. We typically present the raw or relative event study coefficients as our main results, and in these cases will use the gender neutral term 'child penalty'. At times, we will also provide in the text the calculated P_t as an estimate of the 'motherhood penalty' for comparison with the international literature.

As noted in the preceding section, we focus on a balanced panel of individuals observed for 3 years prior to parenthood in both datasets, and for 5 years following parenthood in HILDA and 10 years following parenthood in ALife. By focusing on a balanced panel we follow the same individuals over time and remove any variation driven by idiosyncratic differences in the composition of our panel with event time. This idiosyncratic variation is a potential source of uncertainty and bias in our estimates. On the other hand, the smaller sample size and non-random attrition in a balanced panel will add to potential uncertainty and bias. In HILDA we can address non-random attrition by weighting the sample using the relevant longitudinal weights, whereas attrition is less of a concern in our administrative ALife data.[9]

Event study results

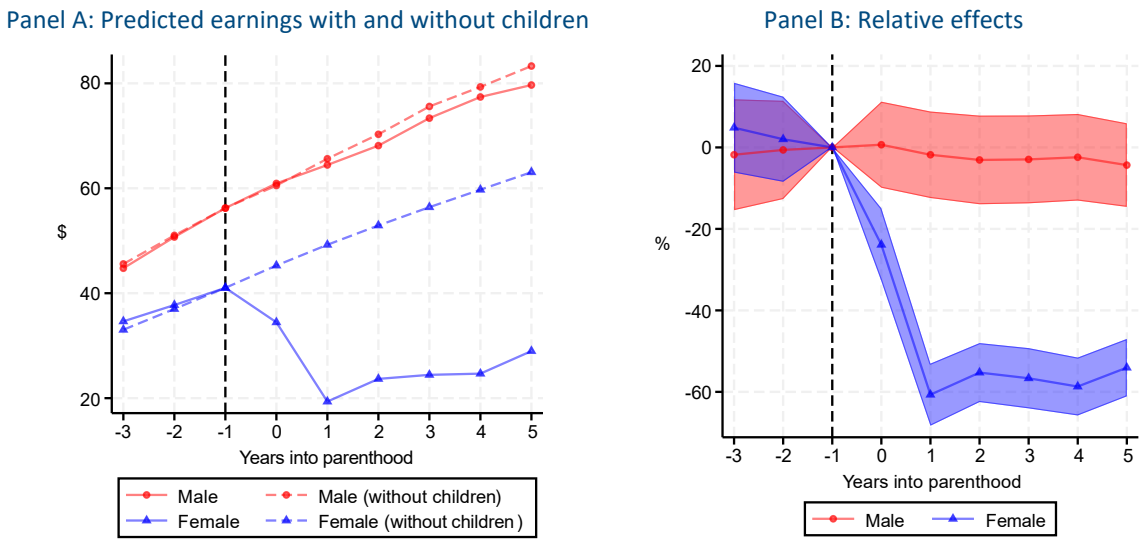
Figure 1 illustrates the gender-specific effects of children on earnings over time. In Panel A we show average predicted nominal earnings for men and women from equation (1), including the counterfactual without children. The gap between these two series for each gender corresponds to the estimated level effects, the event study coefficients $\hat{\alpha}_t^g$. In Panel B we show these effects as a percentage of the counterfactual earnings, the relative effects P_t^g from equation (2). Both the level and relative effects are constrained to be zero in the year before parenthood.

Children have radically different effects on the earnings of men and women. In the 3 years prior to parenthood, the earnings of men and women move in a similar way. Following the arrival of the first child, the earnings of men and women diverge, with a significant and persistent earnings penalty for women but not for men. The motherhood penalty in annual earnings, the percentage P_t by which women fall behind men due to children from equation (3), averages 53 per cent over the first 5 years (excluding the birth year). In Appendix Figure A1 we show a very similar pattern of results, with a motherhood penalty of 55 per cent, arising if instead we use an unbalanced panel.

Extending our sample using the ALife dataset shows a slightly smaller, but still persistent, motherhood penalty in the longer run (see Appendix B for further detail). Our estimated long run penalty (defined as the average penalty from 5 to 10 years after entry into parenthood) is 47 per cent, similar to the United Kingdom (44 per cent), larger in magnitude than estimates for Denmark (21 per cent), Sweden (26 per cent) and the United States (31 per cent), but smaller than estimates for Austria (51 per cent) and Germany (61 per cent) (Kleven et al. 2019b).

[9] Weighting the analysis is not without drawbacks, as identified in Solon et al (2015), but makes sense in the context of non-random attrition in the survey data.

Figure 1: Effects of children on earnings, by sex



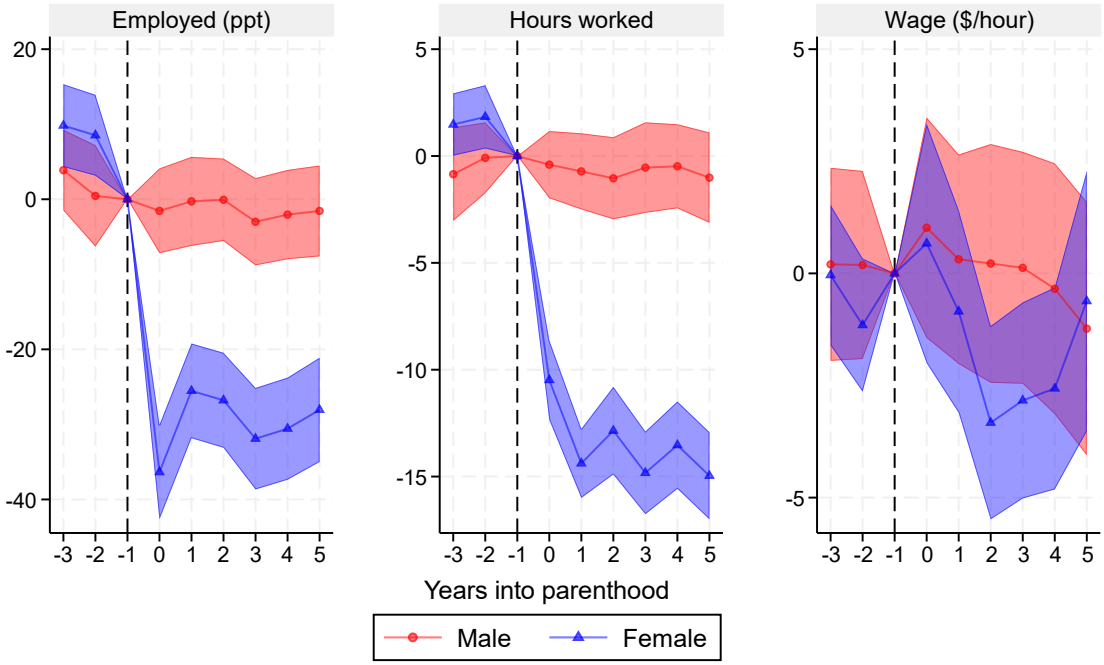
Notes: Panel A shows the predicted values from equation (1) with and without the estimated event study coefficients $\hat{\alpha}_t^g$ and averaged over the sample. These coefficients, the level effect of children on earnings, are thus the gap between the dashed and solid lines. Panel B shows the estimated relative effects P_t^g from equation (2), effectively the gap between the respective lines in Panel A as a percentage of the dashed line. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level and relative effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Our main results include effects not only from the first child, but any additional children born during the observation window. To test the effect without multiple children, we estimate our original result separately for women who go on to have a second or multiple children over the observation period, and those who only have one child in the observation window. Appendix Figure A2 shows that the child penalty for women with one child is smaller than the penalty estimated for multiple children but remains economically and statistically significant up to 10 years after birth. Importantly, for women who only have one child, there is no significant recovery in earnings at year 5—the age at which children generally become school-age. The motherhood penalties average 56 per cent (multiple children) and 31 per cent (one child only) in the first five years of parenthood.

A child penalty in earnings can arise from three margins – probability of employment and, for the employed, hours of work and the hourly wage rate. Figure 2 explores all three, highlighting particularly large effects for employment and hours of work relative to their counterfactual trajectories. There is a sharp drop in the employment rate for women in the first year of parenthood (of about 36 percentage points), with minimal recovery 5 years after birth (first panel). For women who remain employed, hours worked falls significantly following birth and does not recover over the period (second panel). Finally, there is suggestive evidence of an hourly wage penalty for women who remain employed (third panel), though our measure for the hourly wage is quite imprecise. The motherhood penalties over the first 5 years average 30 per cent for employment, 34 per cent for hours worked and an imprecise 5 per cent for the hourly wage. The differences that emerge are driven entirely by women, with men largely unaffected. These effects highlight the strong labour supply implications of entry into parenthood for women.[10]

[10] In Appendix Table A1 we provide summary statistics on men and women's labour force status in HILDA to support Figure 2. The share of women working full-time decreases after parenthood, and the share

Figure 2: Level effects of children on earnings margins, by sex



Notes: Panels show the estimated event study coefficients $\widehat{\alpha}_t^g$ from equation (1) with outcomes as follows: employment; hours worked for the employed; hourly wage rate for the employed. Hourly wages are windsorised, namely top and bottom coded at the top and bottom percentiles respectively, to reduce the influence of measurement error. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Wage effects will capture any impact of women sorting into occupations and industries with lower pay or pay growth following birth, however, our estimates do not capture anticipatory sorting effects more than 3 years prior to entry into parenthood. Consistent with recent studies, women may make work or study decisions early in their careers in anticipation of the requirement for more flexibility following entry into parenthood or may invest less in their education because anticipated shorter careers or more career breaks would result in lower returns (Adda et al. 2017; Wasserman 2022).

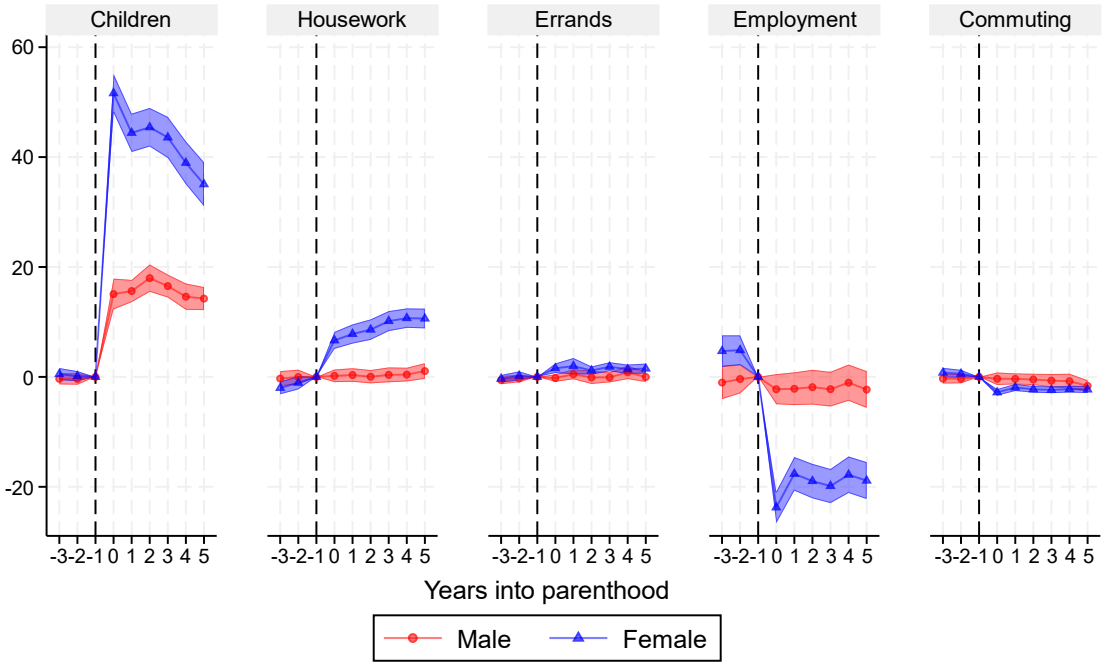
A broader perspective on experiences following parenthood can be found in time use data.[11] In Figure 3 we show the impact of children on hours per week spent in various household and work activities. Again, these are relative to counterfactual trajectories in the absence of children. For both genders there are sharp changes following the arrival of children. Women see hours per week spent with children—playing with, helping, teaching or transporting them—jump up by around 50 hours a week. This then declines with time. Men have a more modest increase of around 15 hours. Women also see increases in housework and errands after parenthood, unlike men; parenthood exacerbates existing

working part-time or leaving the labour force increases, while men's labour force status remains largely unchanged.

11 We thank an anonymous referee for this suggestion. We focus here on the top five activities, by average time use, for our sample. In total, time spent in these activities averages 70 hours a week, or 42 per cent of total weekly hours.

inequalities in time spent on housework and errands, as shown in Appendix Table A2. Consistent with earlier findings we see a fall in hours in paid employment for women (and in commuting).[12]

Figure 3: Level effects of children on weekly hours spent in select activities, by sex



Notes: Panels show the estimated event study coefficients $\hat{\alpha}_t^g$ from equation (1) with outcomes the amount of time spent on the selected activities in a typical week. Activities are: 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 (Children); housework, such as preparing meals, washing dishes, cleaning house, washing clothes, ironing and sewing (Housework); household errands, such as shopping, banking, paying bills, and keeping financial records (Errands); paid employment (Employment); and travelling to and from a place of paid employment (Commuting). Prior to parenthood time spent with own children averages less than an hour a week, but not zero, which may reflect measurement error in identifying parenthood or errors in responses to this question (e.g. including time spent with other children, which is asked separately). The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

5. Decomposition of the Gender Earnings Gap

Before turning to explanations for the motherhood penalty we explore just how much of the gender earnings gap it can explain. We do this by estimating the unconditional percentage difference in earnings between men and women, and isolating the portion of this earnings inequality that can be explained by children and the motherhood penalty. While inspired by a similar exercise in Kleven et al. (2019a) we take a simpler approach, but also extend the decomposition to highlight the role of the motherhood penalty as experienced at different points of parenthood.[13]

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12 The time use estimates will include both extensive and intensive margins. As a result, the effect here is larger than the hours worked (conditional on employment) measure in the middle panel of Figure 2. Note that while in level terms the fall in commuting hours for women is small, in relative terms it is around the same as that observed for employment hours—both fall by around 50 per cent following children.

13 In particular, unlike Kleven et al (2019a) we do not nest our decomposition in an Oaxaca-Blinder approach to estimate the component due to differences in educational attainment.

We begin by expanding our focus from the balanced panel of parents used in estimating the motherhood penalty to all HILDA survey respondents aged 25–64 years old. Based on this sample, we estimate the gender earnings gap as the difference between average male and female earnings, as a percentage of male earnings.[14] This is shown in the top line in Figure 3 below. The gap begins at 48 per cent in the early 2000s before falling to 38 per cent by the late 2010s.[15]

The next step of the decomposition estimates what men and women in our sample would have earned if they did not have children. For all individuals in our sample we identify if they ever had children and the years since the arrival of their first child. For fathers, in line with our earlier results, we assume they suffered no earnings penalty from parenthood—what they would have earned equals what they did earn. For mothers, we scale up their earnings in the years following parenthood according to our estimated earnings penalty from the last section. For example, if the estimated child penalty for women in a given year into parenthood is 50 per cent, we assume they would have earned double what we actually observe in the absence of children. We can then recalculate the gender earnings gap based on these counterfactual earnings without any motherhood penalty, and subtract it from the observed gender earnings gap to arrive at the child-related gender earnings gap—the second line in Figure 4.

A key sensitivity in this exercise is what we assume about the child penalty beyond the first 5 years. One approach would be to assume it remains fixed at around 50 per cent, given the strong persistence in the child penalty for women. However, the HILDA data does suggest a gradual attenuation of the penalty over time, as apparent in Figure 1. To take a more conservative approach we fit a linear trend to the penalty from years one to 5 and extrapolate it out. This trend implies a child penalty that eventually closes, but many decades after entry into parenthood, when most women will have retired.

The child-related gender earnings gap can itself be decomposed into components stemming from those at various points of parenthood. We do this by recalculating the child-related gender earnings gap as above but assuming either no motherhood penalty from 6 years into parenthood on—to get the component for those 0-5 years into parenthood, or no motherhood penalty from 21 years into parenthood on—to get the additional component for those 6-20 years into parenthood (the remaining difference with the overall child-related gender earnings gap being driven by those 21 years or more into parenthood).

Figure 4 illustrates the results of this exercise. In 2001, women earned around 48 per cent less than men. Around half of this, 25 percentage points, could be explained by the motherhood penalty as observed and extrapolated above, represented by the blue shaded areas. By 2019, women were earning around 38 per cent less than men, of which over four-fifths, 33 percentage points, could be explained by the motherhood penalty. This echoes Danish findings that an increasing share of the gender earnings gap is due to the motherhood penalty (Kleven et al. 2019a). While increasing female educational attainment and success in tackling explicit discrimination have helped ameliorate the overarching gender earnings gap, the motherhood penalty remains.

Most of the contribution of the motherhood penalty to the gender earnings gap comes well after the early years of parenthood. The penalty from the first 5 years of parenthood is only one-fifth of the current gender earnings gap (light blue), growing to three-fifths 20 years in (mid blue). In a way this is quite intuitive: most women are not 'new' mothers, but many have been mothers at some point, and it is the long shadow that parenthood casts on their labour market outcomes that drives these results.

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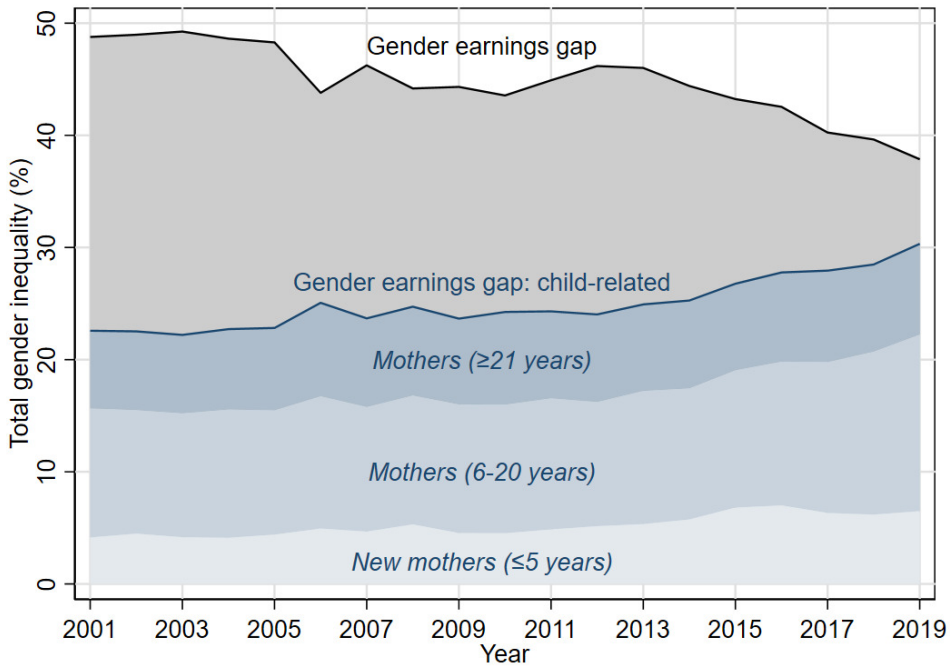
14 To arrive at these averages we weight the sample using the cross-sectional responding person weights.

15 Note this is larger than the commonly reported gender pay gap, which does not account for differences in participation rates or hours of work between men and women. Nonetheless, this too has been narrowing, falling from 17.2 per cent in February 2014 to 13.3 per cent in February 2023 according to the Workplace Gender Equality Agency (WGEA 2023).

This long shadow may reflect foregone opportunities earlier in parenthood, but may also capture the continued, evolving caring responsibilities as children age. This includes outside school hours care, transport to and from school and extracurricular activities, and more general care and support. Outside school hours care, like early childhood education and care, is eligible for government subsidies in Australia. However, caps on these subsidies can create high effective marginal tax rates for some women and an incentive for part-time employment.

Figure 4: Decomposition of gender earnings gap



Notes: Shows the unconditional gender earnings gap for 25–64 year olds in the HILDA survey data, and the proportion of this accounted for by the motherhood penalty. Interpretation is as described in the text.
Source: Authors' calculations using HILDA Release 21.0.

Finally, this exercise assumes by necessity that the motherhood penalty has not changed over time.[16] The relatively short and small panel of births in HILDA precludes a precise test of this assumption, as do the changes in the coverage of parenthood by the ALife dataset over time. Using Danish registry data, Kleven et al. (2019a) find only a modest decline in the motherhood penalty over time. Examining trends in the motherhood penalty, even over the first few years of parenthood, would be a worthwhile topic for further research.

[16] While the size of the motherhood penalty does not change over time in this exercise, the magnitude of its effect depends not just on the size of the penalty, but also on the counterfactual earnings of women relative to men. Because non-child related gender differences in earnings have narrowed over the period, child-related gender inequality appears to increase in Figure 4, because the penalty is operating on a larger base.

6. Why the Gap? Potential Explanations of the Motherhood Penalty

The estimates of the motherhood penalty imply that children have large effects on the careers of women, relative to men. However, it is unclear whether this reflects optimal allocations of time based on the relative earnings of parents, preferences, or constraints driven by workplace norms or institutions. In this section we discuss potential explanations of the motherhood penalty.

Intrahousehold allocations of time

One explanation for the presence of a motherhood penalty may be that couples make choices about intrahousehold allocations of work and care based on relative earnings. Namely, it may be more financially optimal for the partner with the lower income to take time off work to care for children given their lower opportunity cost of staying at home.

This household allocation would be consistent with the theoretical "unitary model" of the household proposed by Cortes and Pan (2020), where a child penalty for mothers, but not fathers, only occurs when both parents have the same preferences and productivities at home, but different market wages.

To test whether our results are driven by financial decisions, we narrow our focus from all those entering parenthood, to those who also have a partner captured in HILDA in the year prior to children. We then estimate the child penalty separately for women who are the primary earner in a couple before parenthood, and for women who are not (i.e. secondary earners). We include all partnered women, regardless of whether the relationship continued following children. Around one-third of the partnered mothers in our sample are the primary earner prior to children.

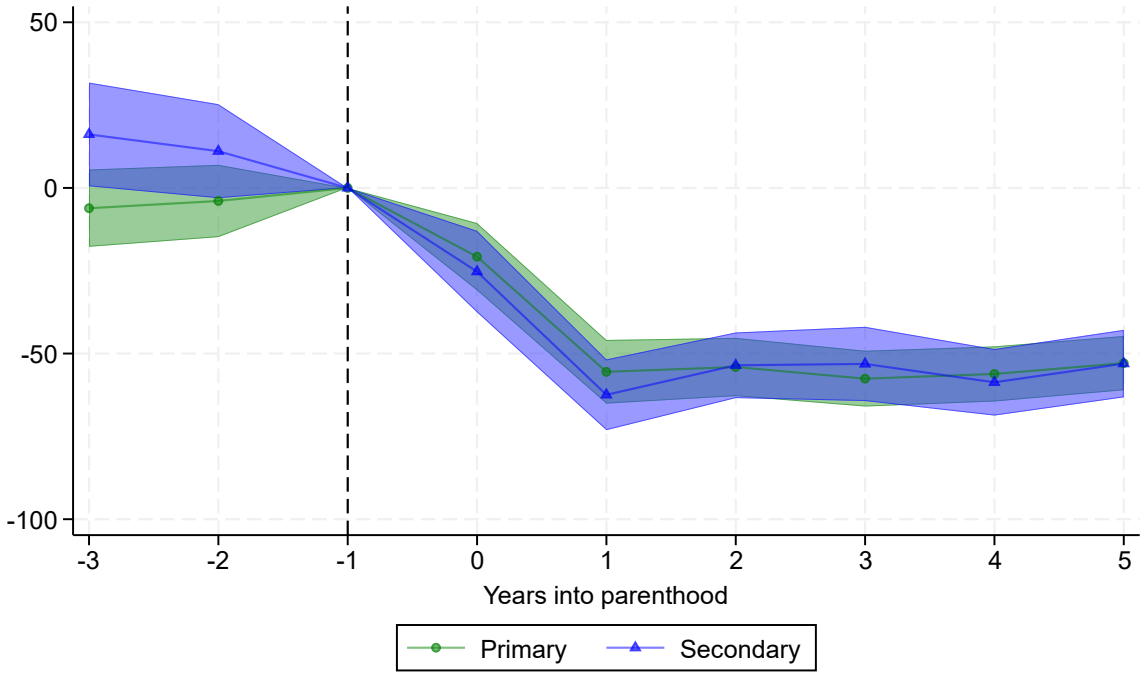
We find no difference in the child penalty depending on earner status (Figure 4); mothers who were primary earners before parenthood experience a similar decline in earnings as mothers who were secondary earners within their household. This result is consistent with earlier literature that shows intrahousehold allocations of time are not always based on financial considerations, with female breadwinners spending more time on unpaid work than their partner (Bertrand et al. 2015; Bittman et al. 2003; Cortes & Pan 2020). In Appendix Figure A3 we also show that the child penalty holds even for women who earn double or more what their partner earns.

To complement this analysis, we also study the child penalty by education, and find that highly educated women experience a similar drop in earnings after the arrival of children, compared to other women (Appendix Figure A4). This drop in earnings is contrary to the significant investments highly educated women make in their human capital. This may reflect the fact that educated women are more likely to work in more flexible occupations (as discussed below), or that they are more likely to be partnered with highly educated and high earning partners due to assortative matching. Having a higher income spouse may create a disincentive to work full-time due to high effective marginal tax rates, and could also make dropping out of the labour force more financially viable. Literature has also shown that more educated women are likely to spend more time with their children, despite their higher opportunity cost of doing so (Guryan et al. 2008).

These two results suggest that household choices around work and care are not based on financial considerations alone. This is consistent with theoretical "non-cooperative" bargaining models, where partners have different preferences and household bargaining is unbalanced or inequitable as proposed by Lundberg. and Pollak (1994), and with models where households face a utility penalty from deviating from traditional gender norms (see, for example, Akerlof and Kranton, 2000). It is also

consistent with the "non-cooperative" model of the household proposed more recently by Cortes and Pan (2020).

Figure 5: Effect of children on women's earnings (%), by primary earner status prior to parenthood



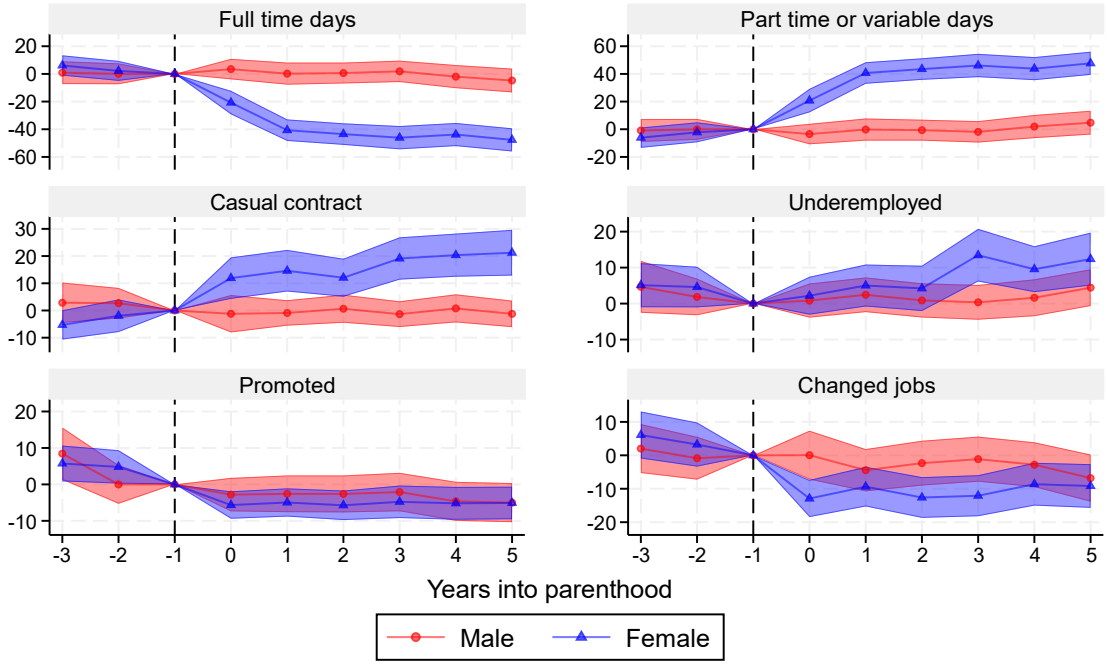
Notes: Shows the estimated effects of children on women's earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Restricted to women with partner information and estimated separately for those who were primary earners and those who were secondary earners in the year prior to parenthood. Those with identical earnings to their partner are excluded. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Workplace outcomes and flexibility

A potentially important mechanism for the motherhood penalty is the flow on consequences from career breaks and requiring flexibility in working hours. If job design carries unanticipated – perhaps unnecessary – penalties for these then women may face additional challenges to achieving a satisfying balance between work and family life.

In Figure 6 we show the effect of children on the incidence of a variety of job outcomes. Following parenthood, conditional on employment, women experience a 40 percentage point fall in the likelihood of working full-time days and a commensurate rise in part-time or variable days. Over time this also sees women more likely to be employed on a casual basis. While part-time and flexible work arrangements may allow many women to balance work and care responsibilities, parenthood is also associated with an increase in underemployment. The probability of women saying they would prefer more hours jumps up by a little over 10 percentage points from the 3rd full year of parenthood onwards. This is suggestive that care-related barriers are causing mothers to work less hours than they would like and are available to work. Across all four outcomes, there is no evidence of parenthood having any effect on men.

Figure 6: Level effects of children on incidence of selected job outcomes, by sex (percentage points)



Notes: Panels show the estimated event study coefficients $\hat{\alpha}_t^g$ from equation (1) with outcomes as follows, percentage probability of: working five days a week in main job; working less than five or variable days a week in main job; being on a casual contract in main job; being underemployed; being promoted at work in the past year; changing jobs in the past year. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line .
Source: Authors' calculations using HILDA Release 21.0.

The final row of Figure 6 highlights a broader issue with the reduced labour force attachment of women following parenthood—there is suggestive evidence that women are less likely to be moving along or up the job ladder. The probability of promotion falls by around 5 percentage points and the probability of changing jobs falls by around 10 percentage points. There is some evidence that men and women begin to diverge a little earlier, in the year prior to parenthood, which may could arise if women are less likely to change jobs or apply for promotions during pregnancy, or face discrimination in hiring and promotion processes during this time. The results following parenthood could reflect time out of the labour force or a lower propensity to apply for promotion or new jobs. Regardless of the cause, the reduced vertical and horizontal labour mobility of women following parenthood may further weigh on earnings later in life.[17]

To provide more evidence on the impact of flexibility we now examine how the child penalty varies by the flexibility or otherwise of an individual's main job in the year prior to parenthood. We use a question in HILDA that asks respondents whether flexible start or finish times are available as an entitlement in their workplace. We take the average by 2-digit occupation across all years to construct mean flexibility scores (by occupation across all years). We then assign occupations as high (low) flexibility if they score above (below) the overall median across all occupations. Appendix Table A3 shows occupations with the most availability of flexible work entitlements include managers and

[17] See Deutscher (2019) for a discussion of the role of job switching in wages growth in Australia.

professionals, while those with the least (on this measure) include educational professionals, carers and aides, and various machinery and factory operators – occupations with more rigid schedules.[18]

In Appendix Figure A5 we show a similar child penalty regardless of the level of flexibility offered in jobs. This result is robust to different definitions of flexibility, such as the availability of permanent part time work. In Appendix Figure A6 we show that, for our partnered subsample, the occupational flexibility of partners does not change the child penalty. This contrasts with previous literature which found within couples where the father works flexibly, mothers have less labour supply adjustment (Bang 2021). We posit that this may in part be due to the high cost of child care in Australia,[19] differences in norms or institutions, which we reflect on in the next section, or different measures of workplace flexibility capturing different effects.[20]

Although the aggregate penalty is the same regardless of the level of flexibility on offer prior to parenthood, we now explore whether the extensive or intensive margins of the penalty are affected by workplace conditions. That is, we explore whether workplace flexibility impacts mothers' employment, hours worked or hourly wage. We find that women with less job flexibility before parenthood are more likely to exit the labour force after having children, whereas women with greater flexibility are somewhat more likely to experience a weaker wage growth trajectory (Figure 7).

Whilst our evidence for this latter result is noisy, it does suggest that mothers working in flexible occupations may attract lower wages, potentially reflecting weaker wage growth because of forgone promotion opportunities.[21] This result is also important as it suggests the availability (or lack thereof) of flexible work conditions may be one potential driver of a mother's decision to exit the labour force.

These results also suggest a role for workplace settings, particularly around the availability of flexibility, in potentially mitigating the child penalty in employment and hours worked, but potentially at the cost of hourly wages. Similar findings in Kleven et al. (2019a) point to parenthood disproportionately leading women to be more likely to be in a flexible workplace but less likely to be in management. Further, Goldin (2014) argues that disproportionate rewards to long hours in some occupations results in larger gender pay gaps that could be ameliorated with more considered job design. This would benefit women, but also men, seeking greater flexibility at work. Recent Australian causal work on the changes in the motherhood penalty in response to the availability of workplace flexibility also suggests potential gains from reforms in this area (Ciasullo & Uccioli 2023).

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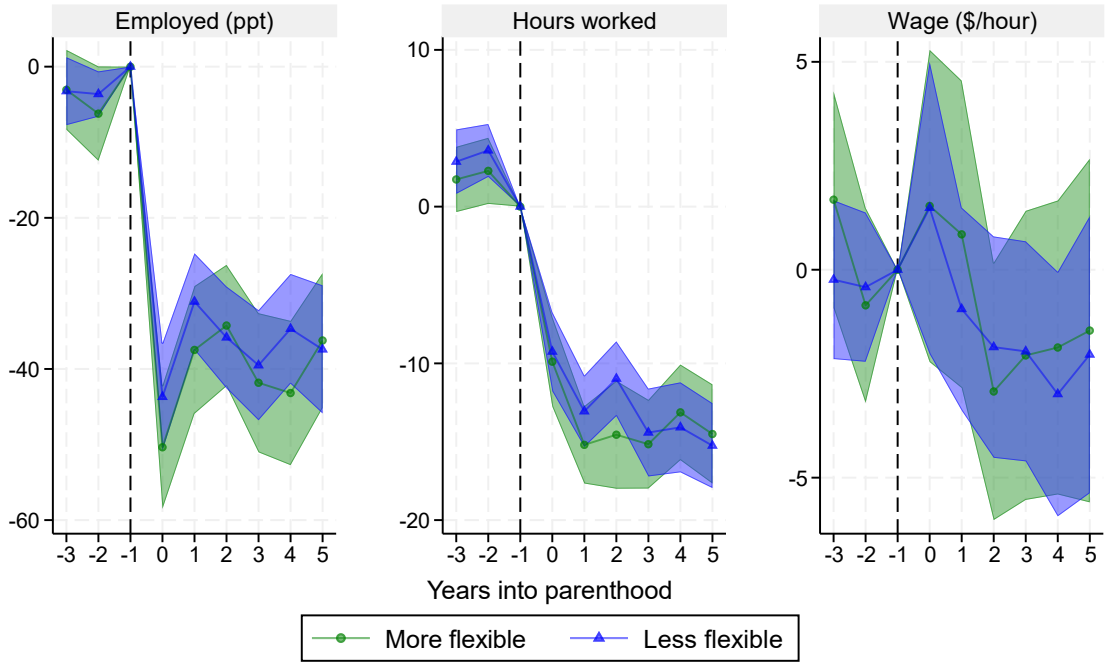
18 It has previously been shown that certain flexible characteristics are associated with higher wages for some workers, rather than lower wages due to a compensating differential (He et al. 2019; Maestas et al. 2018; Mas & Pallais 2017). This may be due to higher skilled workers sorting into more flexible jobs, or employers combining wage and non-wage benefits to attract employees (Mas & Pallais 2020).

19 Wood et al. (2020) find that for most women, there is no financial benefit to working more than 3 days a week. The high cost of child care in Australia is likely a major factor driving the need for couples to specialise in either paid work or care.

20 As per Goldin (2014), "workplace flexibility is a complicated, multidimensional statement. Differences in the empirical findings and appropriate frameworks seem to depend on the definition of flexibility applied and the particular type of non-wage amenity considered."

21 Another potential explanation (for which we thank an anonymous referee) is that less flexible occupations are also characterised by a high share of award wages, and compressed wage distributions. In this case, a smaller wage penalty for less flexible occupations may reflect that the counterfactual wage growth trajectory is relatively flat, reducing the potential impact of motherhood on wage growth.

Figure 7: Level effects of children on women's earnings margins, by workplace flexibility prior to parenthood



Notes: Panels show the estimated event study coefficients $\widehat{\alpha}_t^g$ from equation (1) with outcomes as follows: employment; hours worked for the employed; hourly wage rate for the employed. Hourly wages are windsorised, namely top and bottom coded at the top and bottom percentiles respectively to reduce the influence of measurement error. Estimated separately for those in an occupation prior to parenthood that is above or below the median level of flexibility, based on the availability of flexible start and finish times. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Satisfaction with current arrangements

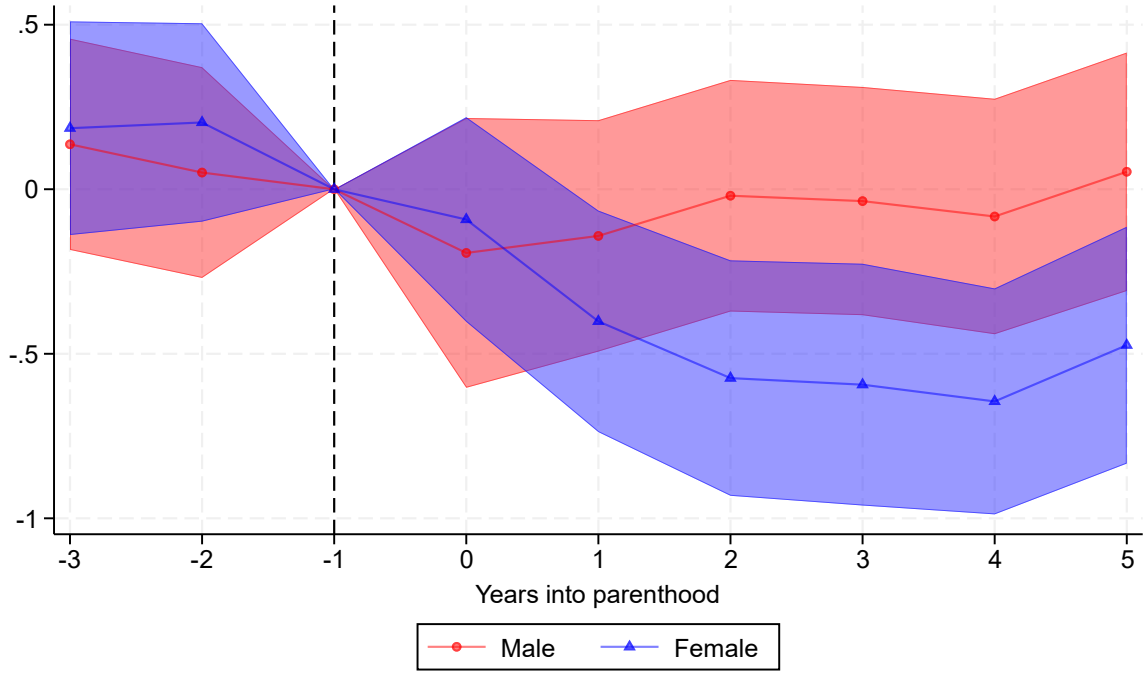
Even if household choices regarding labour supply are financially suboptimal, households may still be making choices consistent with their preferences. To analyse parents' preferences, we study questions in HILDA around parents' satisfaction with their employment opportunities and work-family life. While preferences are likely to play a substantial role in allocations, dissatisfaction with current arrangements would suggest that other factors are also at play.

We first consider parents' satisfaction with their employment opportunities. This is based on a survey question which asks parents to rate their satisfaction with their employment opportunities on a scale of zero to 10, with zero indicating the least satisfaction with employment opportunities and 10 the most satisfaction. We then use the same event study design described in the previous section to measure the impacts of children.

Figure 8 shows that women's satisfaction with their employment opportunities begins to fall the year prior to parenthood and becomes significant one year after, indicating they may pre-empt reduced work opportunities prior to the arrival of children. Women's satisfaction falls by around half a point on the scale, which corresponds to roughly 0.25 of a standard deviation in the sample. Interestingly, women's satisfaction troughs later than their employment outcomes (Figure 2), implying that the longer-term impacts of children may be unanticipated and that there are significant challenges in re-

engaging in the labour market. This is consistent with previous literature which finds women underestimate the difficulty of parenthood before children and often adopt more negative views about working after children (Kuziemko et al. 2020). It is also consistent with mothers facing discrimination in the labour market, due to employers' disapproval of working mothers, or their expectation that working mothers will have a more intermittent labour market attachment than other workers (see Cortes and Pan 2020 for a discussion of relevant literature). In contrast, fathers' satisfaction with their employment opportunities does not change significantly over time.

Figure 8: Level effect of children on satisfaction with employment opportunities, by sex



Notes: Shows the estimated event study coefficients $\hat{\alpha}_t^g$ from equation (1) with outcome satisfaction with employment opportunities. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

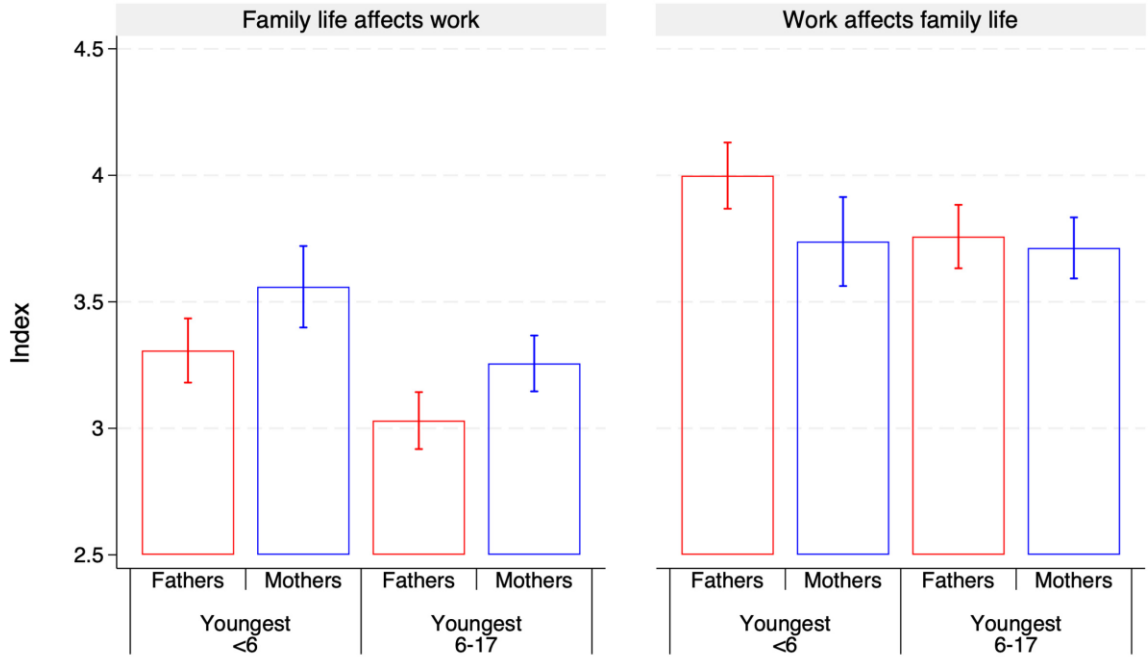
Next, we construct indexes to measure parents' experiences of work-family conflict, based on survey questions in the HILDA dataset (see Appendix C for further detail). The mean scores of the indexes are shown in Figure 9, with higher means indicating more agreement on a scale of 1-7 with the statement in the title. The left-hand panel shows that mothers are more likely than fathers to indicate that family life affects their work. While the situation improves for both sexes as children age, a gap remains. The right-hand panel shows that fathers with young children are more likely than mothers to report work affecting family life. As these questions are only asked of parents balancing work and care, they exclude parents (predominantly women) who exit the labour force after entering parenthood.[22]

Combined, these measures suggest that parents may be facing constraints when choosing how to allocate their time between paid employment and caring. Of course, there are likely costs and benefits

[22] Furthermore, as these questions are only asked of parents, we cannot use the event study design as we do not have data from individuals before parenthood.

to parenthood in other domains of life satisfaction. For example, Baetschmann et al. (2016) points to increases in overall life satisfaction for both men and women on entering parenthood.[23]

Figure 9: Mean level of agreement with statements concerning work-family conflict, by sex and child age



Notes: Indexes constructed using a combination of questions in HILDA that ask respondents about their work-family balance. Further details in Appendix C. Higher mean values indicate more agreement with the statement in the title, with error bars providing a 95 per cent confidence interval. Sample only includes parents who are currently employed.
Source: Authors' calculations using HILDA Release 21.0.

Gender norms

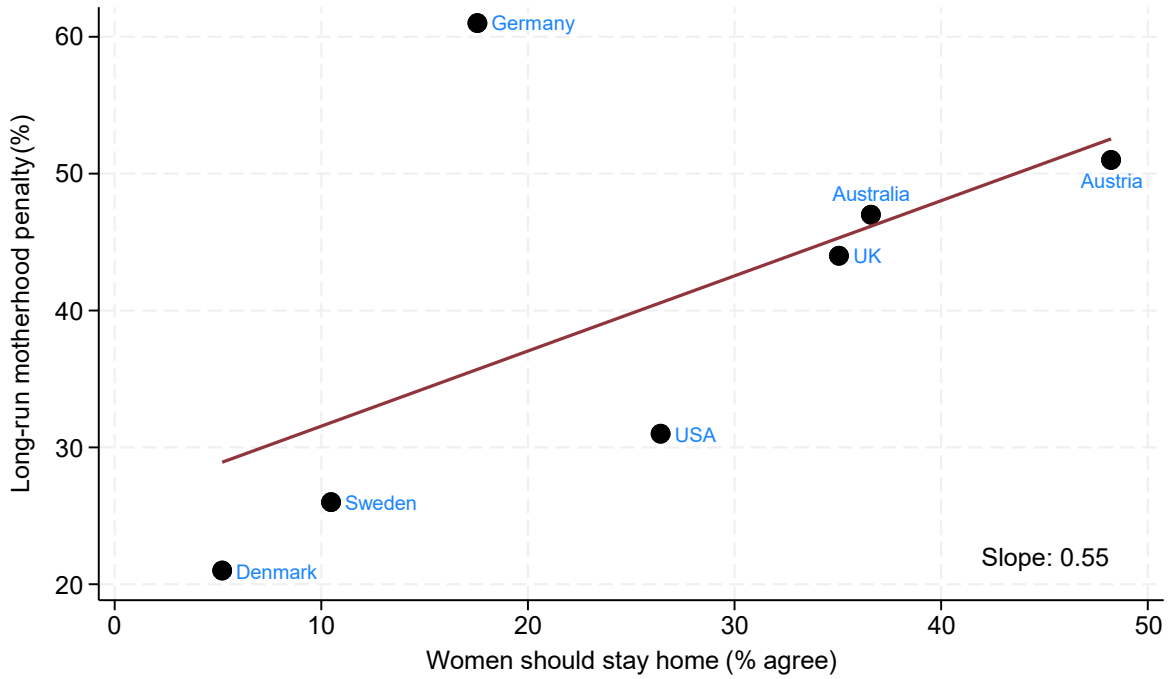
Another potential explanation for the motherhood penalty could be gender norms, and the institutional or policy settings that sustain these norms. In this final subsection we use our results and the International Social Survey Programme (ISSP) to place Australia on Figure 4 from Kleven et al. (2019b), which compares estimated child penalties with stated gender norms across countries. As mentioned above, our estimated long run motherhood penalty from the ALife dataset (47 per cent) is similar to the United Kingdom (44 per cent), larger in magnitude than estimates for Denmark (21 per cent), Sweden (26 per cent) and the United States (31 per cent), but smaller than estimates for Austria (51 per cent) and Germany (61 per cent) (Kleven et al. 2019b). These cross-country differences may be driven by differences in gender norms, as well as institutional and policy settings across countries.

Our Australian results are consistent with the positive correlation found in Kleven et al. (2019b) between countries' long run child penalties and the fraction of respondents in ISSP who think women should stay at home (i.e. not work) when they have young children (Figure 10). Australia displays

[23] The same pattern is not apparent in HILDA. Life satisfaction falls for women (not men) after parenthood. The one area where women appear to do a little better than men is satisfaction with their local community, which would be consistent with this become a more common source of support during parenthood.

relatively conservative norms about men's and women's roles, and a relatively high motherhood penalty, compared with the other countries surveyed. Furthermore, we test the robustness of this exercise and also look at the share of respondents agreeing with the statement "men's job is at work, women's job is at home", and find a similar association (Appendix Figure A7). For both measures, countries with larger motherhood penalties display more conservative views about mothers working.

Figure 10: Long-run motherhood penalties and gender norms



Notes: A modified version of Figure 4 from Kleven et al. (2019b), with Australia added. Long-run motherhood penalty for Australia is this paper's ALife estimate of 47 per cent. Other countries' estimates come from Kleven et al (2019b). We plot this against the proportion of respondents who say that women should stay at home when their youngest child is either under school age or in school. For simplicity and a more up-to-date view on gender norms we have used only the latest 2012 ISSP data, rather than average as they do across waves running back to 1988. Line of best fit shown in red.
Source: Kleven et al. (2019b), authors' calculations using ISSP 2012 and ALife 2019.

Gender norms surrounding family choices are likely influenced by multiple factors, including historical attitudes towards women working. Previous literature has showed that historical settings can influence modern gender norms and attitudes (Grosjean & Khattar 2019). Parental leave policies and the norms around fathers taking leave likely also drive cross-country differences in motherhood penalties. For instance, past work has found that countries with more generous parental leave policies, particularly schemes with dedicated leave for fathers, have a more gender equal balance of work and care.[24] Furthermore, other work for the United Kingdom found gender norms held by peers can influence mothers' employment decisions (Cavapozzi et al. 2021). Future work could assess more rigorously the role of gender norms in shaping the gender pay gap in Australia.

[24] These countries typically have parental leave policies with designated, 'use-it-or-lose-it' leave for fathers. This type of leave policy has been previously shown to encourage greater take-up amongst fathers and a more even balance of care responsibilities at home (Kotsadam & Finseraas 2011; Patnaik 2019). Literature for Denmark has shown an increase in the earmarked portion of leave for fathers results in an increase in women's labour supply (Druedahl et al. 2019), while other work has even shown that life satisfaction, particularly for mothers, increases with enrolment into earmarked paternal leave (Korsgren & van Lent 2022).

7. Conclusion

Considerable gender inequality remains in Australia, and in this paper, we show the majority of this inequality can be explained by children. The arrival of a child reduces women's annual earnings by around 53 per cent in the first 5 years, while men's earnings are not affected. This earnings gap remains high for at least 10 years into parenthood, even for women who only have one child. Our results can be explained by women exiting the labour force and reducing their hours worked, and to a lesser extent, a weaker wage growth trajectory. We take our research as strong evidence of a large and persistent long run motherhood penalty in Australia. We provide, for the first time, causal evidence that children have a significant impact on the long run labour market outcomes of Australian women.

Further, we discuss several potential explanations of the motherhood penalty. We provide evidence to suggest that the motherhood penalty is not purely a financial decision; female breadwinners experience the same penalty as their secondary female earning counterparts. We also show that the motherhood penalty is unlikely to be driven purely by preferences, with mothers' satisfaction with their work opportunities falling, and fathers reporting that work impacts their family life post-children.

We also provide evidence for the role of norms in two areas. First, we provide suggestive evidence that norms around working flexibly may be correlated with the penalty. We show mothers who work in more flexible occupations before children are less likely to exit the labour force once parents. However, these same women may be penalised for this flexibility by lower hourly wages growth. Second, we show how norms around gender roles are associated with the motherhood penalty at a national level. We argue here that countries (including Australia) with more conservative attitudes towards mothers working have higher penalties. Taking these results together, we conclude that both workplace settings around flexible working and broader gender norms likely explain the persistent penalty, though further research, especially for Australia, would be beneficial.

Australia's motherhood penalty has important implications for both gender equality and aggregate productivity. On average, Australian women are now more educated than Australian men (ABS 2020b). Improving the utilisation of women's skills would increase the returns on investments made in women's human capital. Past literature has also found that there are potentially large productivity gains from diversifying the workforce within firms and sectors (Criscuolo et al. 2021; Ostry et al. 2018).

Our results have strong implications for targeting policy to address the motherhood penalty and reducing barriers to a more equal allocation of responsibilities within Australian households. Australia can learn from the experiences of other countries, which have reduced the cost of child care and increased paternal take-up of parental leave. Both measures may help to increase fathers' involvement in family life and improve mothers' labour force participation, helping to reduce the motherhood penalty in the long run.

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Appendix A: Additional figures and tables

Table A1: Labour force status, by sex

Sex	Years into parenthood	Full-time	Part-time	Unemployed	Not in labour force
Male	-3	82.2	8.5	4.4	5.0
	-2	81.8	6.3	4.6	7.3
	-1	83.8	6.1	3.7	6.4
	0	85.3	5.0	1.6	8.2
	1	83.8	7.3	2.5	6.3
	2	81.7	8.0	1.6	8.7
	3	81.3	8.0	2.8	7.9
	4	82.8	6.6	2.7	7.9
	5	82.6	7.9	1.9	7.6
Female	-3	69.0	18.9	3.5	8.6
	-2	71.6	16.8	4.5	7.1
	-1	67.1	15.6	1.5	15.8
	0	21.2	25.9	0.7	52.2
	1	16.3	46.0	1.8	35.8
	2	20.1	42.2	1.9	35.8
	3	17.6	43.1	1.7	37.7
	4	19.9	45.3	2.4	32.5
	5	21.1	49.0	2.4	27.5

Notes: Shows per cent share of sample population in each labour force status group by sex and years into parenthood. Population weights have been used. Based on a balanced panel of parents in HILDA (waves 1-19). To be included in the panel, individuals were required to be observed 3 years prior to parenthood and for 5 years following.
Source: Authors' calculations using HILDA Release 21.0.

Table A2: Mean hours in select activities in year before children, by sex

	Children	Housework	Errands	Employment	Commuting
Females	0.9	9.8	3.8	31.6	3.7
Males	1.3	5.9	3.3	42.2	4.8
Difference	-0.4	3.8	0.5	-10.7	-1.1

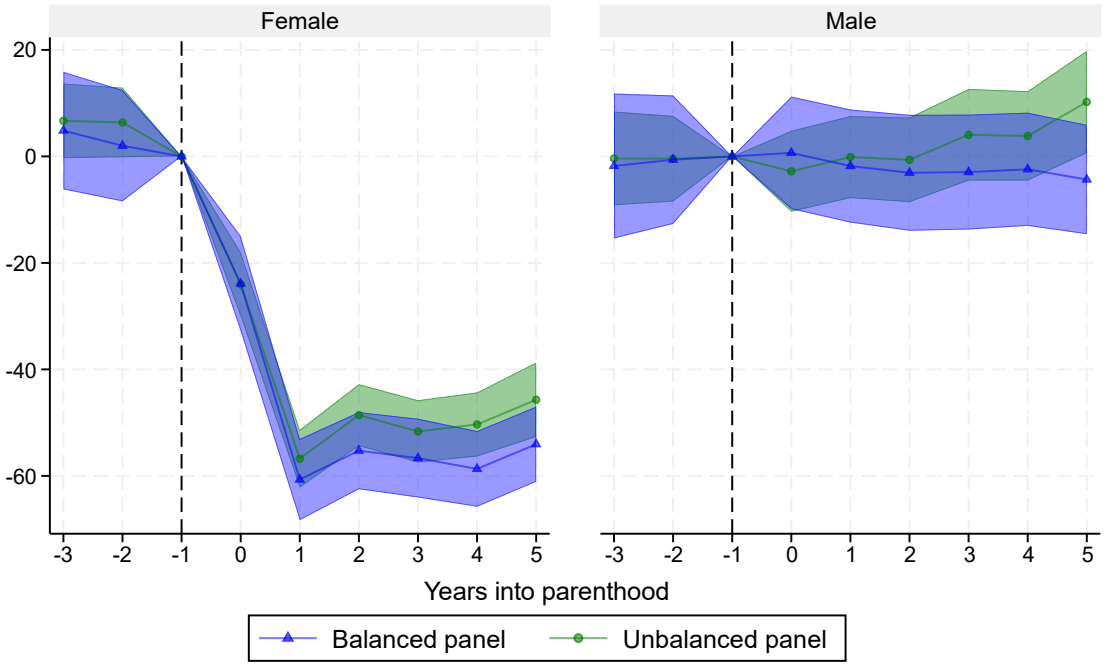
Notes: Shows the mean hours in select activities in the year before children (t=-1) by sex, and their difference. Activities are: playing with your children, helping them with personal care, teaching, coaching or actively supervising them, or getting them to child care, school and other activities (Children); housework, such as preparing meals, washing dishes, cleaning house, washing clothes, ironing and sewing (Housework); household errands, such as shopping, banking, paying bills, and keeping financial records (Errands); paid employment (Employment); and travelling to and from a place of paid employment (Commuting).

Table A3: Bottom and top occupations by workplace flexibility

Lowest Flexibility Entitlements	Highest Flexibility Entitlements
Education professionals (24 per cent)	Managers (96 per cent)
Machinery Operators and Drivers (26 per cent)	Professionals (93 per cent)
Carers and Aides (34 per cent)	ICT Professionals (90 per cent)
Machine and Stationary Plant Operators (34 per cent)	Clerical and Administrative Workers (84 per cent)
Factory Process Workers (35 per cent)	Business, Human Resource and Marketing Professionals (84 per cent)

Notes: Lists the top and bottom 5 occupations by availability of flexible start and finish times. Percentage of HILDA respondents reporting the availability of the entitlement is in parentheses. Based on an unbalanced, weighted panel.
Source: Authors' calculations using HILDA Release 21.0.

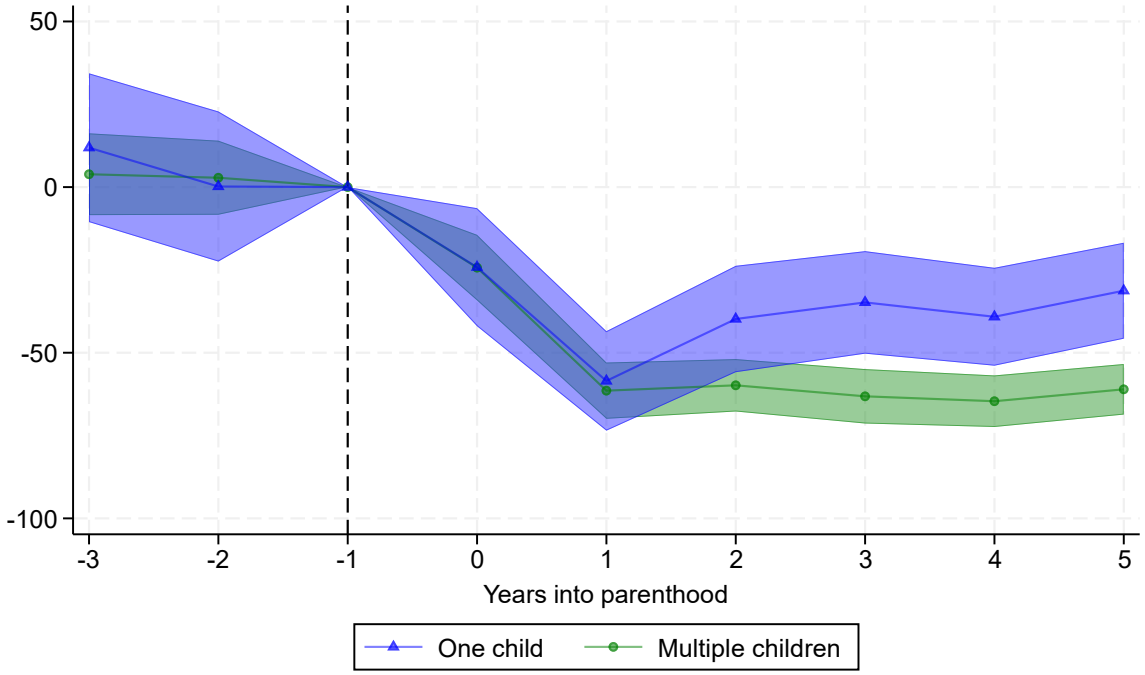
Figure A1: Effect of children on earnings (%), by sex and HILDA panel



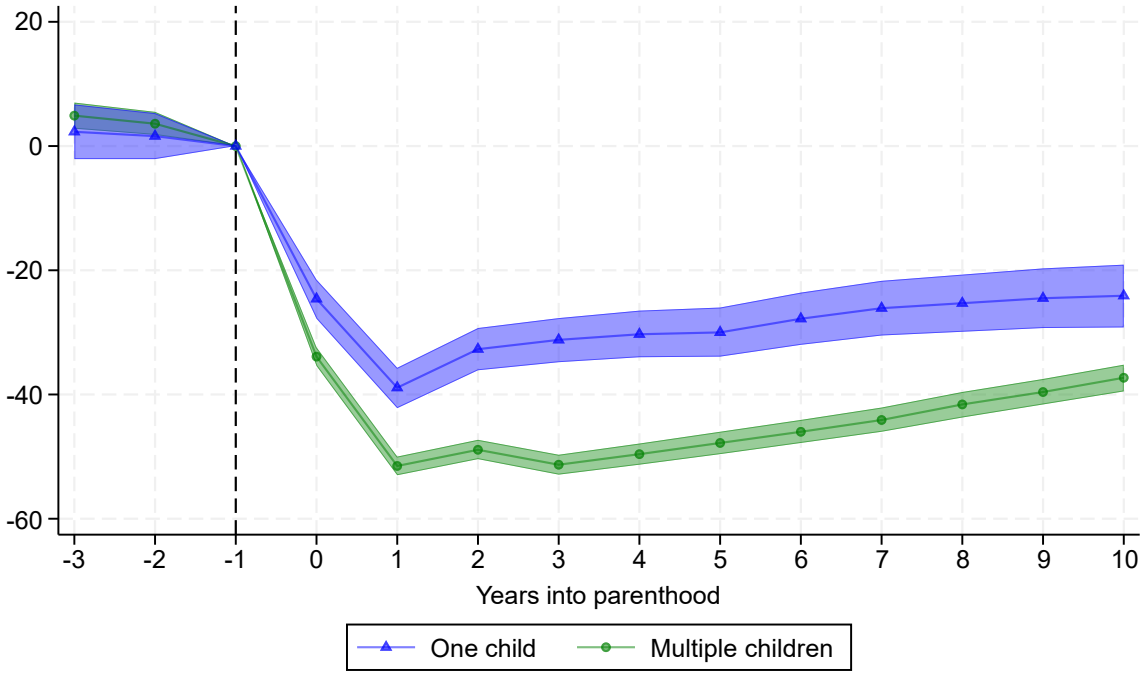
Notes: Shows the estimated effects of children on earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Estimated separately on a balanced and unbalanced panel. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level and relative effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line. Note that while the relative effects for women (left panel) here are smaller for the unbalanced panel, the differences with the results for men (right panel) are such that the unbalanced panel gives rise to a larger comparative motherhood penalty from equation (3).
Source: Authors' calculations using HILDA Release 21.0.

Figure A2: Effect of children on women's earnings (%), by number of children

Panel A: Short-run penalty (HILDA)

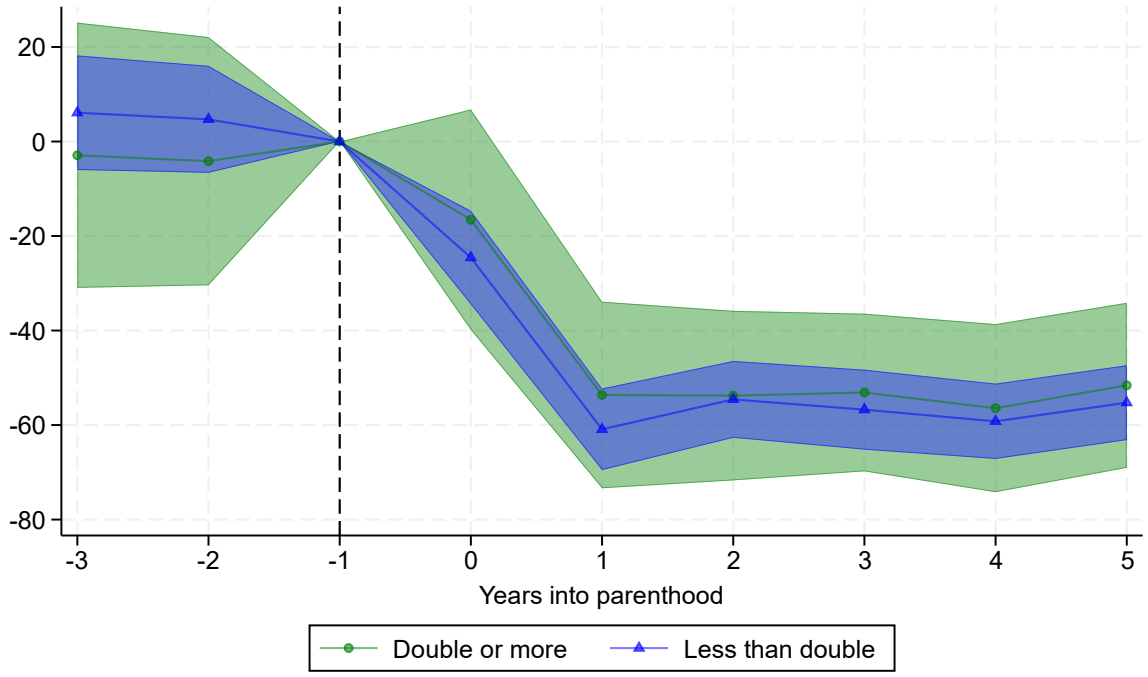


Panel B: Long-run penalty (ALife)



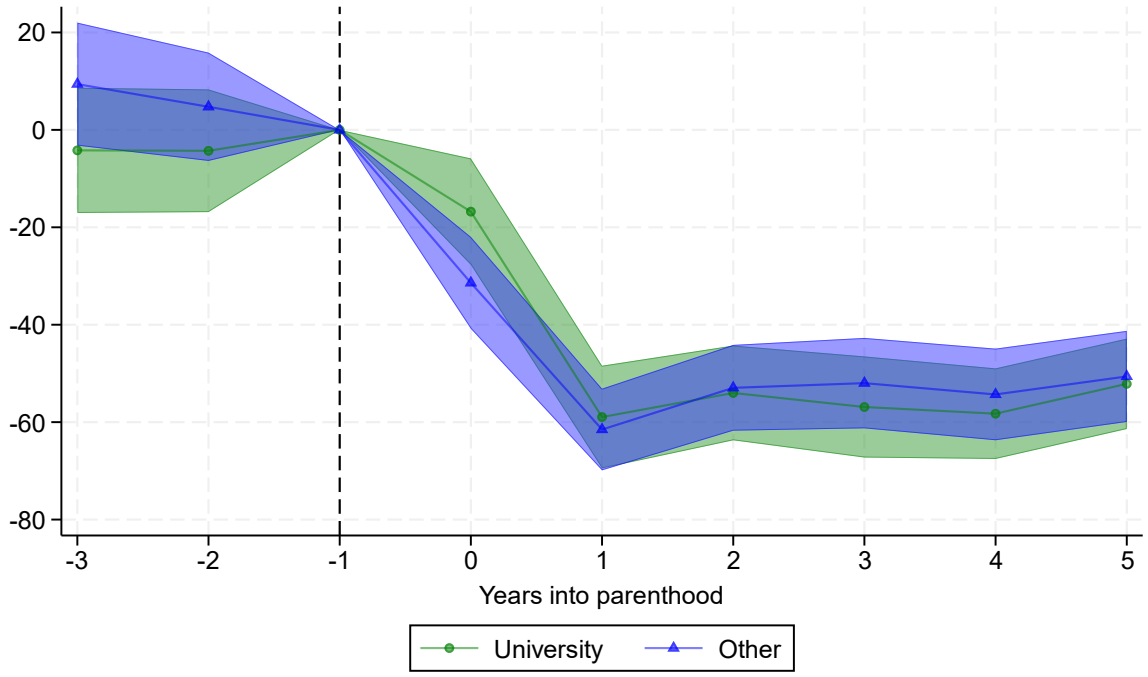
Notes: Shows the estimated effects of children on women's earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Estimated separately for those with only one child versus multiple children over the window of observation. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level and relative effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0 (Panel A) and ALife 2019 (Panel B).

Figure A3: Effect of children on women's earnings (%), by women's earnings relative to partner prior to parenthood



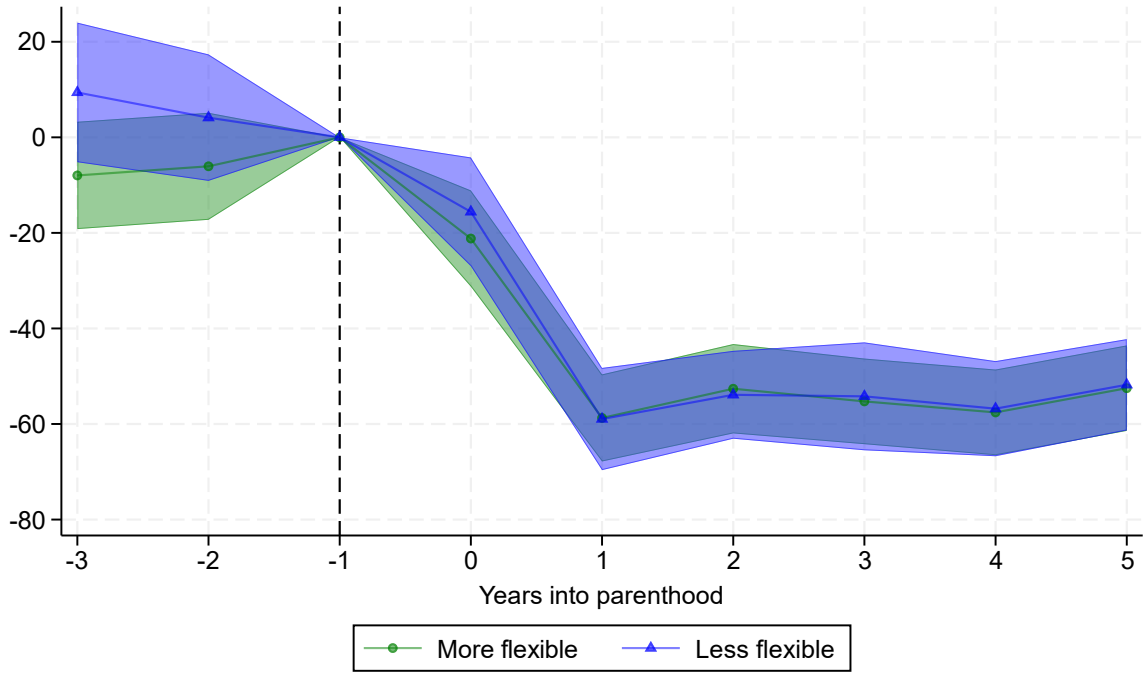
Notes: Shows the estimated effects of children on women's earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Estimated separately for those who earned double or more what their partner earned prior to birth, and all other women who were partnered prior to birth. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Figure A4: Effect of children on women's earnings (%), by highest education level prior to parenthood



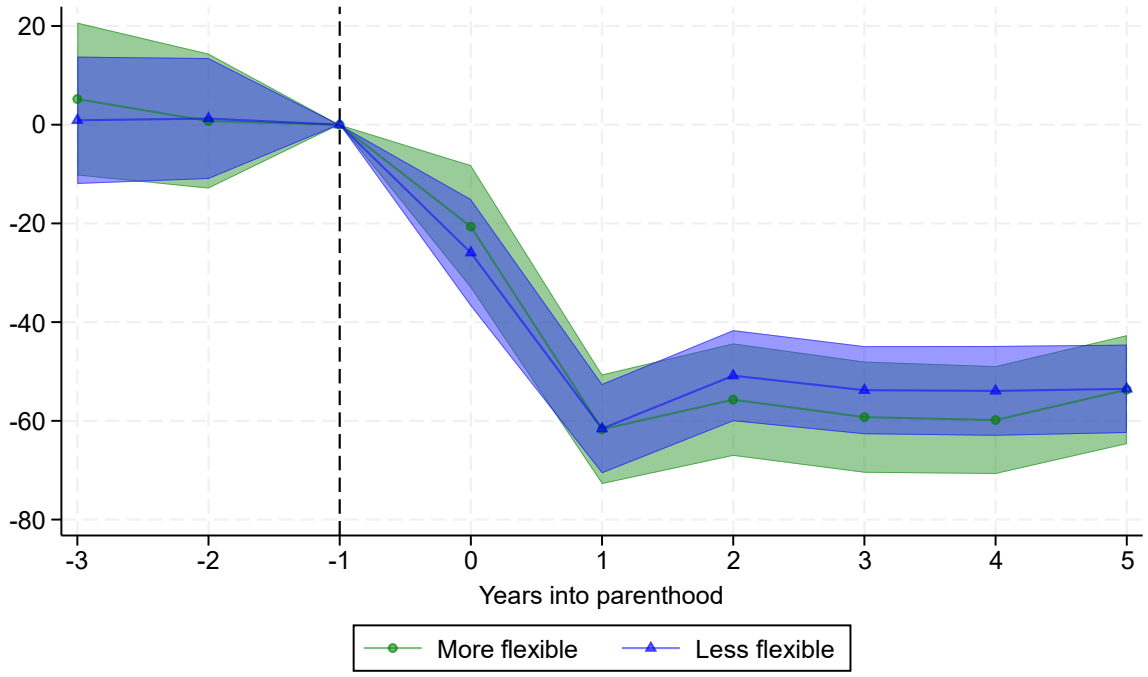
Notes: Shows the estimated effects of children on women's earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Estimated separately for those whose highest level of education was university prior to parenthood and all other women. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Figure A5 : Effect of children on women's earnings (%), by occupational flexibility prior to parenthood



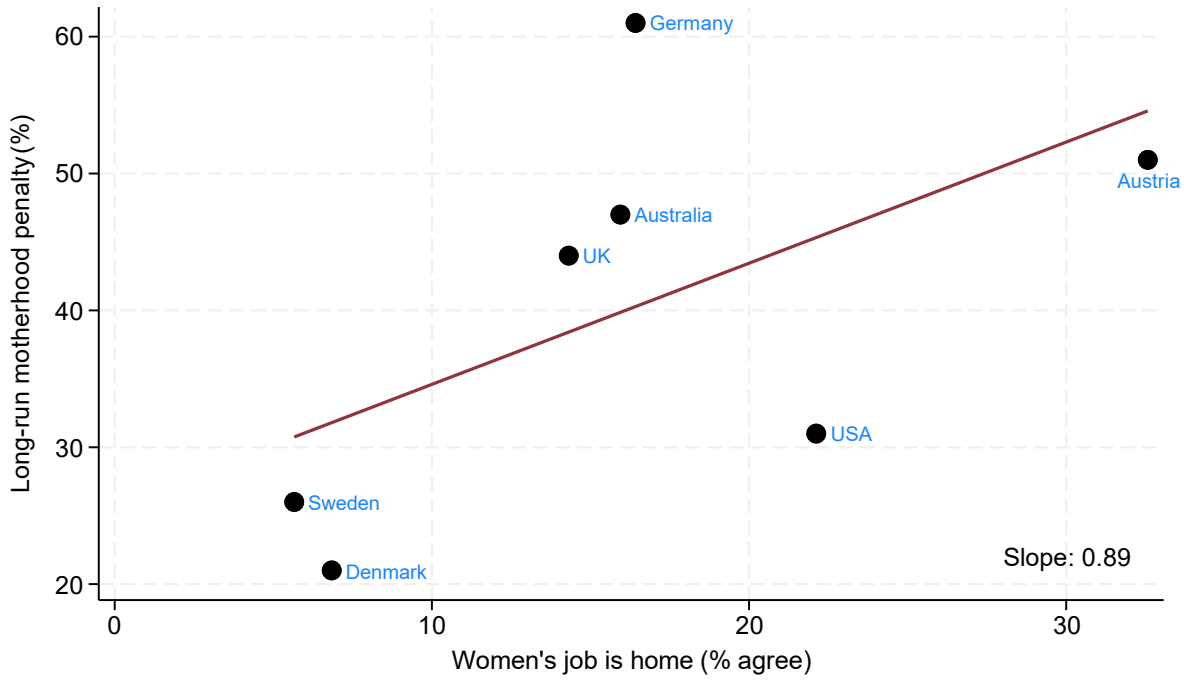
Notes: Shows the estimated effects of children on women's earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Estimated separately for those in an occupation prior to parenthood that is above or below the median level of flexibility, based on the availability of flexible start and finish times. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Figure A6: Effect of children on women's earnings (%), by partner's occupational flexibility prior to parenthood



Notes: Shows the estimated effects of children on women's earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). Estimated separately for those with partners in an occupation prior to parenthood that is above or below the median level of flexibility, based on the availability of flexible start and finish times. The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using HILDA Release 21.0.

Figure A7: Long-run motherhood penalties and gender norms



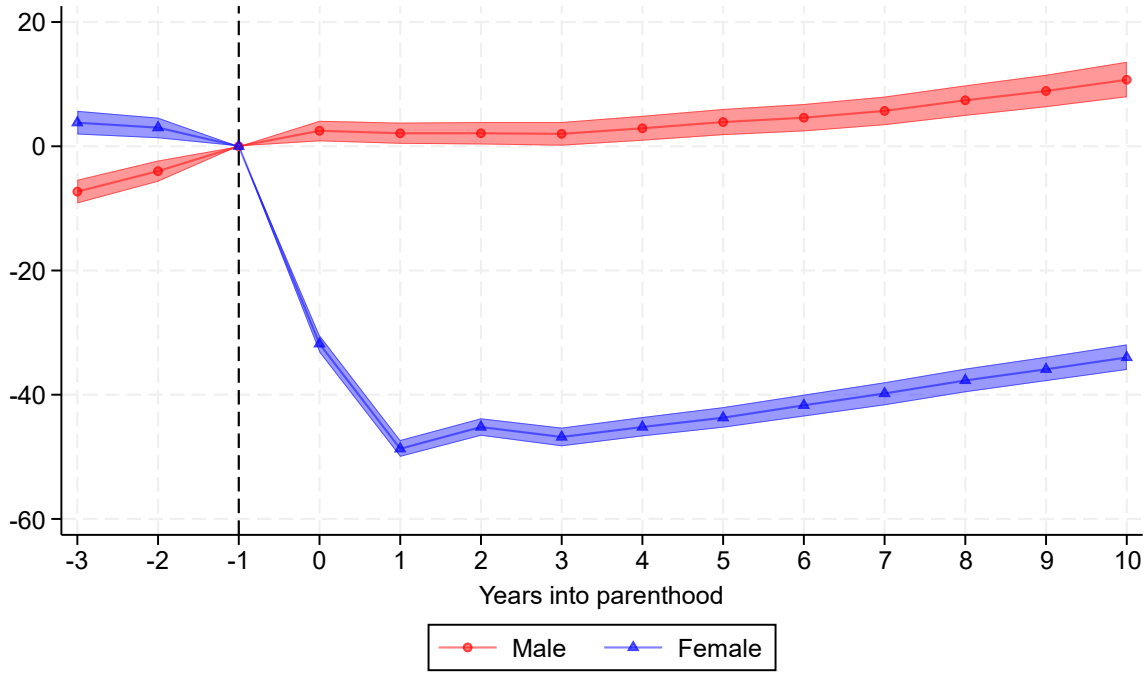
Notes: A modified version of Figure 4 from Kleven et al. (2019b), with Australia added. Long-run motherhood penalty for Australia is this paper's ALife estimate of 47 per cent. Other countries' estimates come from Kleven et al (2019b). We plot this against the proportion of respondents who agree that "men's job is at work, women's job is at home". For simplicity and a more up-to-date view on gender norms we have used only the latest 2012 ISSP data, rather than average as they do across waves running back to 1988. Line of best fit shown in maroon.
Source: Kleven et al. (2019b), authors' calculations using ISSP 2012 and ALife 2019.

Appendix B: Penalties using ALife data

As described in the data section, our ALife sample consists of longitudinal tax data from the 2000 income year onwards. It is more than an order of magnitude larger than our HILDA sample and allows us to extend our analysis to follow individuals for 10 years after entry into parenthood. However, we are only able to observe parenthood for individuals lodging tax returns in both the year before and the year of parenthood, which introduces a positive selection into the sample and may bias our estimates.

Figure B1 plots the gender-specific effects of children on earnings over time, as a percentage of counterfactual earnings in the absence of children. These are the relative effects P_t^g from equation (2) which we presented estimates of for the HILDA sample in Figure 1B. Unlike HILDA, the earnings profiles for men and women prior to birth are not parallel. The positive trend in men's earnings prior to parenthood is in line with other papers in the literature and indicates some earnings growth preceding parenthood above age profiles. In contrast, the small decline in women's earnings the year before parenthood could reflect the impacts of pregnancy, including potential anticipatory effects, and pregnancy-related leave which is not fully covered by paid parental leave. It may also capture miscarriage or other reproductive health challenges (and treatments, such as *in-vitro* fertilisation) that may precede observed births but still have meaningful impacts on labour force participation and other outcomes. A final possibility is specification or measurement error, as if children are observed with a lag in ALife then some of the child penalty may be observed prior to the event. While small, we should be cautious of these pre-trends when interpreting the results in the ALife dataset.

Figure B1: Long run impact of children on earnings, by sex



Notes: Shows the estimated effects of children on earnings as a percentage of counterfactual earnings in the absence of children: P_t^g from equation (2). The shaded areas show a 95% confidence interval, based on robust standard errors. By construction the level and relative effects are zero at event time t=-1, the year before parenthood, which is marked by the vertical line.
Source: Authors' calculations using ALife 2018.

Following children, women's earnings begin to significantly decline. While the penalty on female earnings improves slightly over time, it remains persistent for a decade. In contrast, male earnings grow slightly.[25] The motherhood penalty in earnings, the percentage P_t by which women fall behind men due to children from equation (3), averages 49 per cent over the first 5 years. It is comforting given the differences in data sources and evidence of positive selection that this is very close to the 53 per cent estimated in the HILDA sample. The long-run motherhood penalty from years 5-10 of parenthood is a slightly smaller 47 per cent.

Effect of sample selection

To identify the parenthood event our ALife sample is restricted to those who lodge tax returns in both the year before and the year of parenthood. Our balanced panel only requires that the individuals became parents between 2003 and 2008 (inclusive) and hence are in the time period covered by our panel (2000 to 2018 inclusive) for 3 years before and 10 years after parenthood; and that they are not identified as being overseas in any of those 14 years. Our unbalanced panel will capture people becoming parents early and (especially) later, from 2001 to 2018; and will only drop individuals in the year in which they are overseas.

A strength of ALife is that we capture not only earnings from tax returns, but also as reported by 3rd parties (employers). It is because the data is comprehensive in earnings that we are confident imputing zero earnings when individuals are missing from return or 3rd-party data.

What would the ALife motherhood penalty look like if we imposed more restrictions on our sample? For example, if we treated missing data as missing, and required individuals to have return or 3rd-party data in each of the 14 years we follow them? What if we required individuals to have return data in each of the 14 years we follow them? This is the focus of the remainder of this section.

In Table B1 we show how our long-run penalty varies with the restrictiveness of our ALife sample. Our baseline balanced panel follows 40,707 people and results in a long-run motherhood penalty of 47.0 per cent. If we focus in on a smaller balanced panel of individuals with return or 3rd party reported data in every year the penalty falls to 45.8 per cent; if we hone in on only those with return data in every year then it falls further to 43.4 per cent. Intuitively, by limiting the panel in this way we remove one mechanism – non-employment – which helps drive the motherhood penalty.

An unbalanced panel brings in more individuals again relative to the balanced panel, but also has a lower motherhood penalty of 37.2 per cent. The reasons behind this difference are unclear. One possibility is that the motherhood penalty may have been falling over time in Australia (as suggested by Ciasullo and Uccioli (2023)). As noted above, the balanced panel is restricted to individuals becoming parents from 2003 to 2008. The unbalanced panel will include some earlier and many later births. If we restrict the unbalanced panel to those becoming parents from 2003 to 2008 the motherhood penalty rises to 43.4 per cent, much closer to that seen for the original balanced panel. There could be other issues at play in the unbalanced panel that drive differences, including the potential for small compositional differences across event time to have a bearing on the results.

[25] This growth in male earnings is not robust to sample selection choices explored later, so we caution against placing too much weight on it.

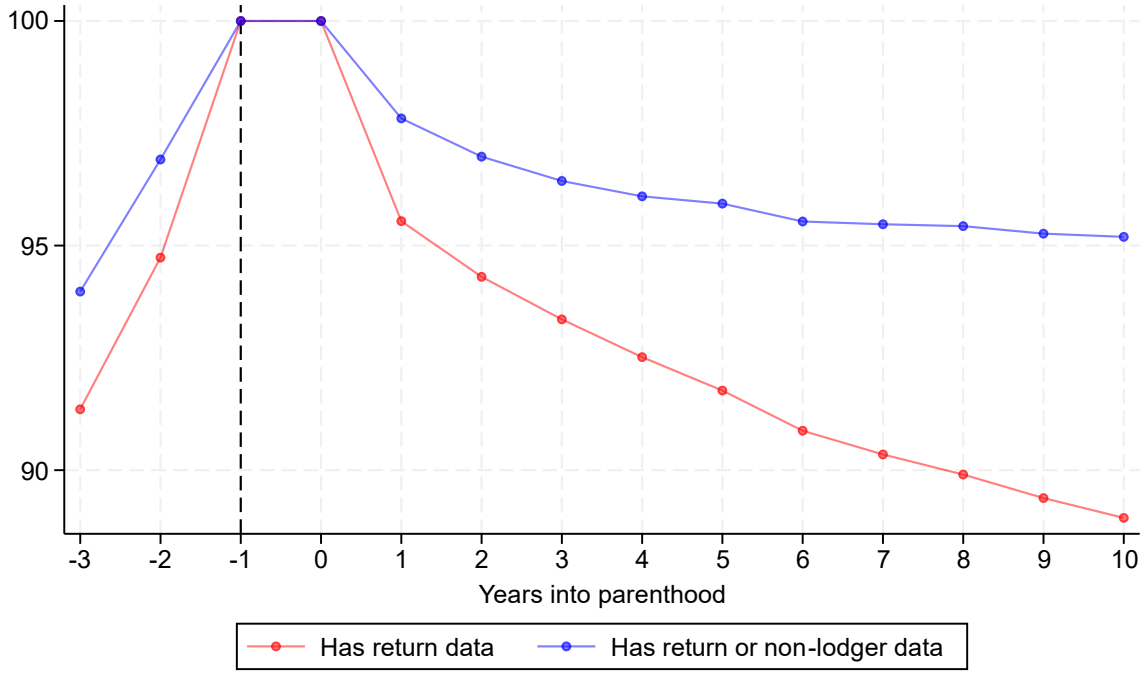
Table B1: Motherhood penalty and sample size, by ALife panel

Panel	Motherhood penalty (5-10 years)	N
Balanced	47.0	40,707
Balanced, return or 3rd party data every year	45.8	33,690
Balanced, return data every year	43.4	29,981
Unbalanced	37.2	83,529
Unbalanced, 2003-08 births	43.4	43,068

Notes: Shows the estimated long-run motherhood penalty – an average over years 5-10 of P_t from equation (3) – for an unbalanced panel, our baseline balanced panel and two more restrictive balanced panels that require either some tax data or some tax return data in every year. The sample sizes report the number of individuals in the panel – a constant in each year of event time for the balanced panels or an average over event time years for the unbalanced panel.
Source: Authors' calculations using ALife 2018.

Finally, we provide a visual illustration of the attrition out of having return or 3rd party reported data in Figure B2. By construction, all individuals in our baseline ALife sample have return data in the years either side of the event, as return data is used to identify entry into parenthood. There is meaningful attrition further out however, with more than 10 per cent of individuals not having tax return data in their 10th full year of parenthood. The inclusion of the non-lodger module roughly halves this attrition, with a little under 5 per cent of individuals not having any return or 3rd party reported data in the 10th year.

Figure B2: Percentage of baseline ALife sample with data



Notes: Shows the percentage of our baseline ALife sample with data by event time, years into parenthood. By construction, this is 100 per cent in the years either side of the event, as return data is needed to identify entry into parenthood.
Source: Authors' calculations using ALife 2018.

Appendix C: Work-family balance indexes

In HILDA, survey participants are given statements relating to work-family balance and asked to state how much they agree on a measured 7-point Likert scale (where 1 is 'strongly disagree' and 7 is 'strongly agree'). Using these questions, we construct two indexes to measure work-family conflicts.

Index 1: Work's impact on family life

- Because of the requirements of my job, I miss out on home or family activities that I would prefer to participate in
- Because of the requirements of my job, my family time is less enjoyable and more pressured
- Working leaves me with too little time or energy to be the kind of parent I want to be
- Working causes me to miss out on some of the rewarding aspects of being a parent

Index 2: Family's impact on work life

- Because of my family responsibilities, the time I spend working is less enjoyable and more pressured
- Because of my family responsibilities, I have to turn down work activities or opportunities that I would prefer to take on
- I worry about what goes on with my children while I'm at work
- Thinking about the children interferes with my performance at work

To construct the indexes, we sum the individual's answers to each question, and divide by 4. We make an adjustment for people who only answer 3 of the questions and exclude anyone who answers less than 3.