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Labor Supply Effects of Monetary Policy: Evidence from Australian Mortgage Holders

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ABSTRACT: Macroeconomic models largely preclude a labor supply response to monetary policy shocks, and this view of monetary policy is reflected in explicit statements by major central banks. Our paper contributes to an emerging literature that challenges this view by providing evidence of a labor supply channel in monetary transmission. We study how Australian workers adjust labor supply in response to the Reserve Bank of Australia's 2022–23 monetary policy tightening, exploiting administrative data covering the universe of employed workers. Because most Australian mortgages are floating-rate, higher policy rates quickly translate into higher mortgage repayments, allowing us to measure household exposure to the tightening using pre-tightening debt service ratios. We find that highly exposed individuals respond to higher interest payments by increasing labor supply, with sizable effects on employment probabilities, the number of jobs held, and labor earnings. The effects are strongest among those without children, consistent with childcare constraints limiting labor supply responses, but the discrepancy diminishes following a policy reform that increased the generosity of childcare subsidies, highlighting an interaction between fiscal policy and monetary transmission. Together, these findings provide causal evidence that liquidity pressures from higher mortgage repayments can transmit monetary policy to the labor market through house-hold labor supply decisions.

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WORKING PAPERS

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1 Introduction

How households respond to monetary policy shocks is a key determinant of the monetary transmission mechanism. While the role of households as consumers who reduce their spending in response to an interest rate hike is well understood and a key consideration for policymakers, the decision to potentially adjust employment and job-holding behavior when interest rates move has received much less attention. In fact, a common working assumption for central banks is that, in line with the workhorse models in the literature, labor supply is not significantly affected by monetary policy. For example, in its February 2024 quarterly statement on monetary policy, the Reserve Bank of Australia commented: "Monetary policy has little direct effect on labor supply or structural features of the job market, and so generally takes the current level of full employment as given".[1]

A small number of recent papers have started to question this assumption, arguing that contractionary monetary policy can lead to increased labor supply (Cantore et al. (2022); Graves et al. (2024)). If this is the case, it has important implications for both the interpretation and forecasting of macroeconomic conditions, as well as for our understanding of the transmission of monetary policy; an increase in labor supply following an interest rate hike would dampen the effect of contractionary policy on output, while potentially amplifying its effect on inflation through downward pressure on wages and, in turn, prices. Moreover, such responses may have distributional and welfare consequences, particularity if the labor supply response is more evident among certain household types.

In this paper, we test this assumption in the context of Australia. We do so by using administrative data to study labor supply dynamics at the individual taxpayer level at the time of the Reserve Bank's rapid policy rate increases that began in May 2022, following years of low rates.

To establish a causal relationship between monetary policy shocks and labor supply, we follow a strategy along the lines of a continuous treatment local projection difference-in-difference (Jordà, 2023) and compare outcomes for households who are more exposed to monetary policy shocks against households who are less exposed, in what amounts to a local projection differences-in-differences approach. Given the high prevalence of variable rate mortgages in Australia, we can measure each individual's likely exposure to policy rate changes by its debt-service-to-income ratio before the rate change. We can then compare labor market outcomes of individuals with high ex-ante debt service ratios against those of households with low ex-ante debt service ratios. This approach differs from previous papers, which focused on tracing out heterogeneous responses to monetary policy shocks, but did

[1] See *Statement on Monetary Policy*, Section 4.2 "The role of monetary policy in achieving full employment";. Accessed at:https://www.rba.gov.au/publications/smp/2024/feb/in-depth-full-employment.html; see also footnote 2 for parallel statement from the Federal Reserve.

not exploit differing exposures to monetary policy directly.

We make use of information on household characteristics, including mortgage debt service, from the 2021 Census covering all 9.3 million Australian households and 25.5 million people, including 3.3 million mortgage-holding households, and match it with monthly administrative data on income and employment covering the population of employees. This rich and comprehensive dataset allows us not only to quantify the average labor supply response to the rate hike, but also to examine the heterogeneity in responses across households with different characteristics. Identifying this heterogeneity is important for uncovering the underlying causal mechanisms and allows observation of the distributional consequences of monetary policy. In particular, we group households by debt-service quintile; evidence in favor of a liquidity channel of monetary policy would be supported if differential responses are concentrated among the most indebted households.

A priori, a key concern about our identification strategy is whether, absent the policy rate hike, outcomes of high and low debt service households would have followed parallel trajectories. While labour demand remained unusually strong during this tightening cycle relative to historical episodes, the variation in household mortgage exposure provides cross-sectional identification that is largely orthogonal to aggregate labour market conditions. Here, again, we benefit from the size and richness of the Australian administrative data set which allows us to use detailed geographic and demographic variables to control for confounding determinants of employment outcomes. Moreover, the high data frequency allows us to map the timing of individual responses.

One year after the first rate hike in May 2022, high-debt-service individuals experience a large and statistically significant relative increase in employment, job holding, and labor earnings. In the full sample, the probability of employment rises by 0.93 percentage points relative to low-debt individuals, the number of jobs increases by 1.6 per 100 workers, and labor earnings increase by 3.1 percent. These effects are persistent and remain significant at the one percent level three years after the onset of the tightening cycle. Notably, over the first two years of the tightening cycle, the effects are confined to individuals in the top quintile, with no differential labor supply response observed for quintiles two through four.

The effects are especially pronounced among high-debt individuals who are plausibly better positioned to adjust their labor supply. Among those who were not working full-time prior to the onset of monetary tightening, the increase in the probability of employment and the number of jobs is approximately three times larger than in the full sample. The response is significant for individuals who were not in the labor force, pointing to a substantial extensive margin adjustment. At the same time, labor earnings rise sharply among those who were employed part-time in 2021, indicating the presence of both extensive and intensive

margin responses. The effects are also larger for high-debt individuals without children, consistent with lower labor supply responsiveness among parents facing childcare constraints.

We interpret these results as evidence of a liquidity channel, whereby liquidity constrained individuals seek additional income in response to higher interest payment obligations. Consistent with this interpretation, we also find some evidence that high-debt households with mortgages issued in 2020-21, many of which had *fixed* rates at the beginning of the tightening cycle, exhibit a weaker employment response. This attenuation suggests that the response is not driven solely by debt levels, but by exposure to rising required payments.

We further reinforce this interpretation by exploiting a natural experiment that relaxes labor supply constraints for parents through an increase in the generosity of government childcare subsidies. Following the introduction of the Cheaper Child Care reform in July 2023, the differential employment response among high-debt households without children diminishes. This pattern underscores the role of labor supply constraints in shaping household responses to monetary tightening and highlights an important interaction between monetary and fiscal policy in influencing labor supply decisions.

Our findings are robust to a range of identification concerns. The results are also robust to alternative samples and controls, including restricting the sample to households that have lived in their home for at least five years to mitigate selection into high debt, and excluding states still affected by COVID-related restrictions. Finally, we show that the effects are unlikely to be driven purely by broader cost-of-living pressures. The timing of the response aligns with household expectations of interest rate increases and is more closely related to changes in interest rates relative to inflation. Consistent with this interpretation, we do not observe a differential labor supply response among renters, who face similar price-level pressures but do not experience higher mortgage payments.

Related literature: This paper relates to three strands of the literature: (i) the effects of monetary policy on labor markets, which has focused primarily on labor demand; (ii) recent work examining labor supply responses to monetary policy; and (iii) a broader literature on labor supply responses to balance sheet and liquidity shocks. We contribute to the second and third strands by providing causal evidence of a liquidity-driven labor supply response to monetary tightening.

A large literature studies the impact of monetary policy on labor markets through firms' labor demand. Seminal work by Christiano et al. (1996) shows that contractionary monetary policy raises firms' borrowing costs, reducing investment, employment, and output. Subsequent models with nominal rigidities emphasize that monetary tightening depresses aggregate demand, leading firms to reduce labor demand in the presence of sticky wages and prices (Blanchard and Gali, 2010). Related work highlights the role of credit mar-

ket frictions, whereby tighter monetary policy constrains firms' access to external finance and disproportionately reduces labor demand among credit-constrained firms (Gertler and Gilchrist, 1994). In this literature, the effects of monetary policy on employment operate primarily through the demand for labor.

Consistent with this focus, standard macroeconomic models typically abstract from a meaningful labor supply response to monetary policy shocks and imply a low marginal propensity to earn. This reflects both functional form assumptions in household preferences and the presence of wage rigidities that limit income effects (see, for example, Blanchard and Gali (2010), Wolf (2023), and Auclert et al. (2023)). This view is echoed by monetary authorities. For instance, in a speech in November 2022 about the underlying causes of the shortfall in labor supply during the recovery from the pandemic, Chairman of the Federal Reserve Jerome Powell pointed to the health shock and weak growth, noting that "Policies to support labor supply are not the domain of the Fed: Our tools work principally on demand."[2]

More recently, a growing literature has begun to challenge this assumption by documenting labor supply responses to monetary policy shocks. Graves et al. (2024) analyze labor market flows in the United States and show that contractionary monetary policy increases transitions from non-participation into employment and reduces job-to-job quits, suggesting an active labor supply response. Our paper is perhaps most similar to Cantore et al. (2022), who document heterogeneous labor supply responses to monetary policy across the income distribution in the United States and the United Kingdom. While aggregate hours worked decline following monetary tightening, they show that lower-income workers increase labor supply, with higher labor income driven primarily by the intensive margin (i.e., hours worked), and not by the extensive margin (the labor earnings).

This interpretation is consistent with a broader literature documenting labor supply responses to shocks that affect household balance sheets. For example, positive wealth shocks relax budget constraints and reduce labor supply along both intensive and extensive margins (Golosov et al., 2024; Georgarakos et al., 2025), while tighter credit conditions or borrowing constraints increase labor supply as households substitute labor income for reduced access to funds (Bui and Ume, 2020; Dasgupta and Mason, 2025). Although these shocks are not monetary in nature, they highlight important related channels: changes in liquidity, cash flow, or borrowing capacity can operate similarly to the higher mortgage debt service induced by monetary tightening in our setting. Evidence in Cloyne et al. (2020) similarly emphasizes that monetary transmission is amplified among highly indebted households through

[2]See *Inflation and the Labor Market*, speech by Chairman Jerome H. Powell. Accessed at:https://www.federalreserve.gov/newsevents/speech/powell20221130a.htm; likewise, see footnote 1 referencing a quote from the RBA.

repayment exposure under variable-rate contracts.

Our paper complements and extends this literature in several important respects. First, we use administrative data covering the universe of Australian workers, allowing us to study labor supply responses without the sampling and non-response concerns inherent in survey data, which are particularly acute in the tails of the earnings distribution. Second, we exploit quasi-experimental variation in exposure to monetary tightening based on pre-determined mortgage debt service, enabling a causal interpretation of the labor supply response. Third, by focusing on mortgage debt holders over a short and well-defined tightening cycle, our analysis isolates a liquidity channel of monetary policy, whereby higher required debt service induces households to increase labor supply. In contrast to broader measures of heterogeneity, which may conflate multiple transmission channels, our approach allows for a more precise identification of the mechanism linking monetary policy to household labor supply decisions. Finally, the richness of the administrative data allows us to explore channels of transmission through sub-sample analysis, and a natural experiment on childcare generosity provides additional causal evidence that labor supply constraints shape the response to monetary tightening.

The remainder of this paper is organized as follows: Section 2 presents context on the Australian labor market, the tightening episode starting in May 2022, and discusses the dataset used in this paper. Section 3 discusses the empirical strategy, Section 4 presents the main findings, reports the results of sub-group analysis, and tests of robustness, Section 5 discusses results from the childcare natural expeirment, Section 6 undertakes a quantification exercise, while Section 7 concludes.

2 Context and Data

2.1 Context

We study the period of monetary tightening in Australia beginning in May 2022 and extending through June 2025, during which the Reserve Bank of Australia (RBA) increased the policy cash rate by a cumulative 4.25 percentage points over 13 consecutive rate hikes.[3] This represents both the largest cumulative increase and the longest tightening cycle in Australia's history; see Figure 1a.[4] The scale and speed of this episode provide an unusually sharp and well-defined shift in monetary conditions, creating an ideal quasi-experiment to assess the effects of monetary policy tightening on household behavior.[5]

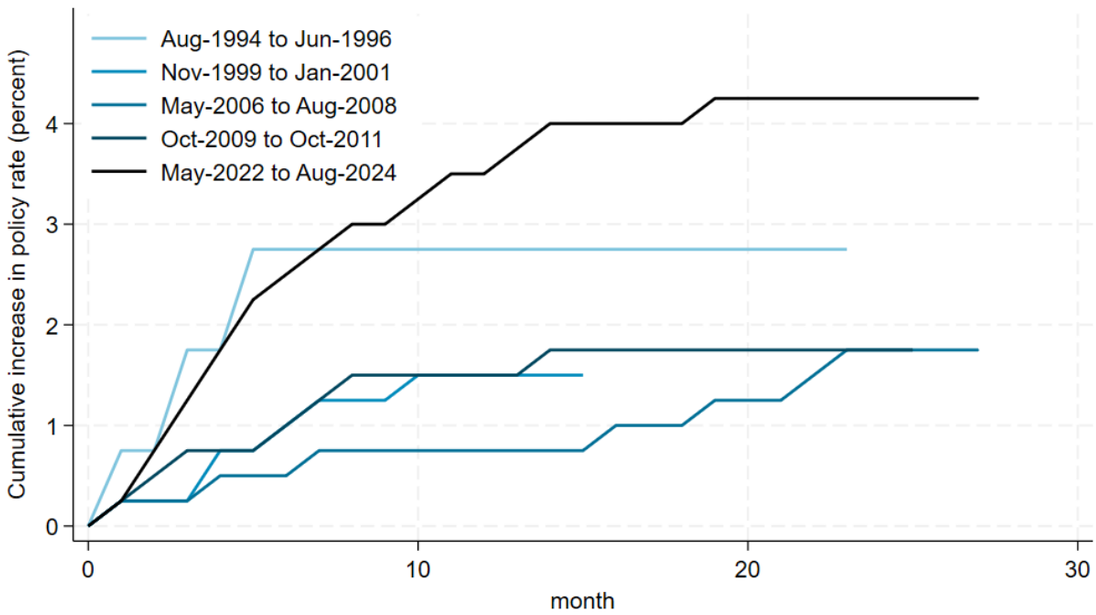
[3]There was a 25 basis points rate cut in February 2025.

[4]The previous largest cumulative increase in a single tightening cycle was 2.75 percentage points (IMF, 2024a).

[5]Despite the magnitude of the shock and inflation rising more than in previous cycles, macroeconomic dynamics over the tightening period have been broadly comparable to earlier episodes (IMF, 2024a). This

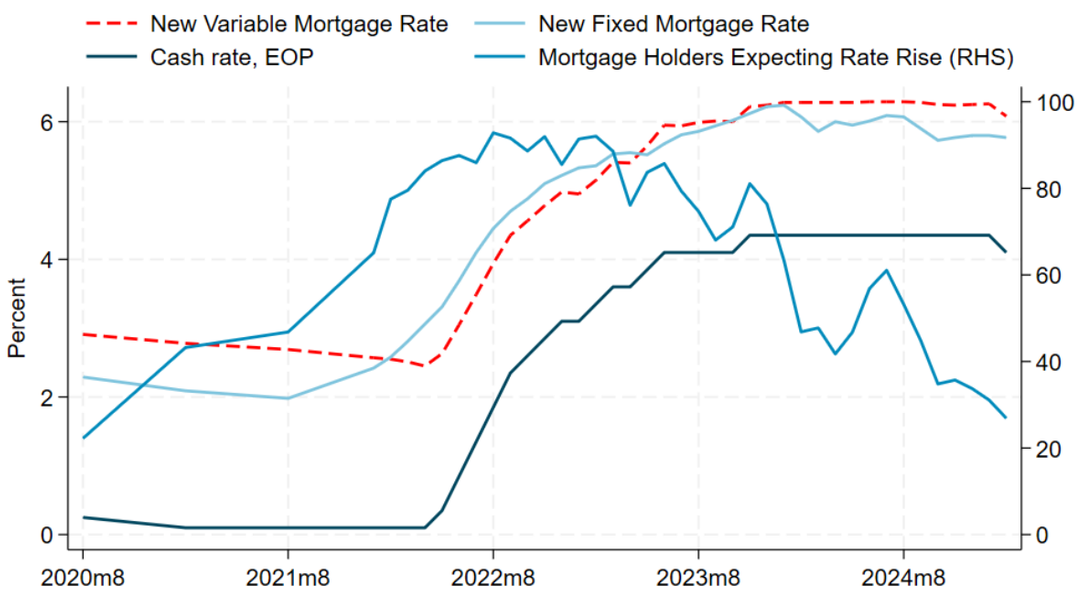
Figure 1: Interest Rates

(a) Policy Rate



Source: Petrescu 2024, IMF Australia Selected Issues Paper, Chapter 1

(b) Timing of Interest Rate Rise and Expectations



Source: Australian authorities and authors calculations.

Transmission from monetary policy to mortgage holders in Australia likely began even prior to the initial increase in the cash rate. As shown in Figure 1b, interest rates on newly issued fixed-rate mortgages began to rise in January 2022, several months before the first policy rate increase. This shift coincided with a rise in the share of individuals expecting higher interest rates, as measured by Westpac-Melbourne Institute Household Survey.[6]

Transmission operates quickly in Australia. As shown in Figure 1b, new fixed and variable rate mortgages rose quickly in line with the cash rate. Moreover, the share of Australian households with variable-rate mortgages (typically indexed directly to the policy cash rate) is substantially higher than in other advanced economies, accounting for around 70 percent of outstanding mortgages in 2022 (RBA, 2023), compared with less than 20 percent in the United Kingdom and under 5 percent in the United States. The combination of a high prevalence of variable-rate borrowing and elevated household debt levels implies that interest rate changes can generate large and immediate effects on household cash flows (a mechanism often referred to as the cash-flow channel (IMF, 2024b; Kent, 2023)). While these effects are partially offset in the aggregate by the presence of net creditor households and widespread use of floating-rate deposits, rate increases can nevertheless have sizable and rapid macroeconomic effects, particularly for households with binding liquidity constraints.

Over the tightening cycle, there have been increased pressures on household budgets and squeezed disposable income (Figure 2). The tightening episode also occurred against a backdrop of unusually strong labour market conditions in Australia, with unemployment near multi-decade lows and labour demand remaining elevated relative to historical tightening cycles (Hunter (2026)). This reflected a combination of higher prices, higher tax burden (due to bracket creep), and higher interest and mortgage payments, particularly for net borrowers. While the overall increase in *net* interest payable was small, the underlying data indicate that gross mortgage payments increased from around 7 per cent of household income to around 10 per cent. Moreover, these increases apply to a little under half of the population (who hold mortgages), so the actual affect on the average mortgage-holder, let alone a highly indebted household, is much higher. So while broad cost of living pressures from price rises have been significant, for mortgage holders rising rates led a large additional drain on disposable income.

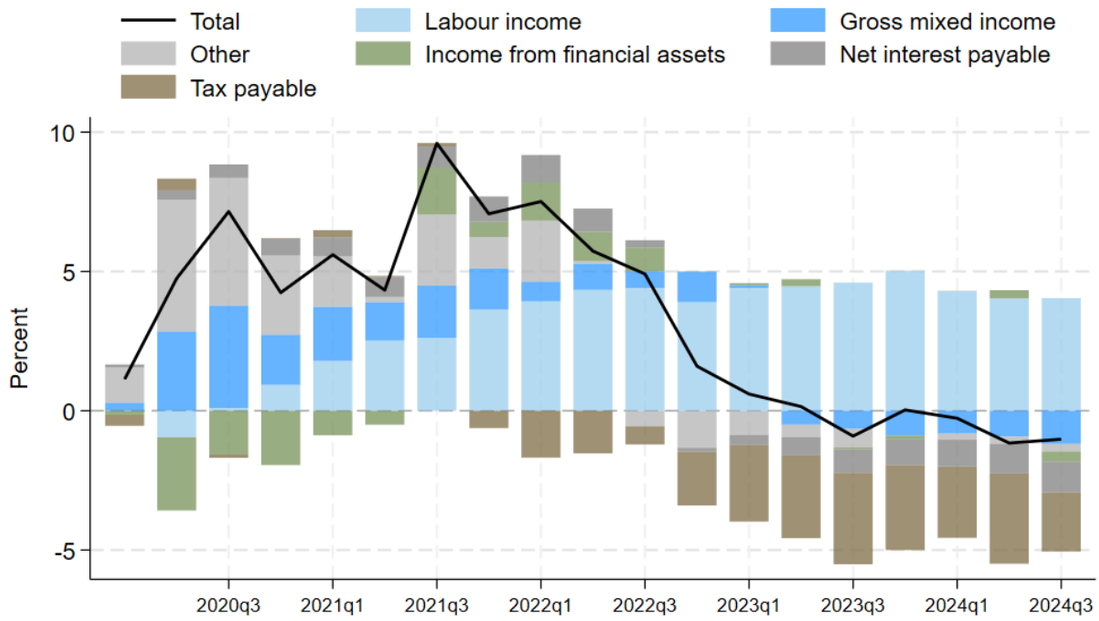
Labor markets have remained tighter than historical norms, reflected in rising participation rates, an increasing number of people holding multiple jobs, and persistently low

similarity supports the external validity of our setting for understanding the effects of monetary tightening in other periods.IMF (2024a) compares macroeconomic outcomes in this cycle with four previous tightening episodes, finding that public demand and private investment were broadly similar, GDP and household consumption slightly weaker, and labor market conditions notably tighter. Overall, these differences are not sufficiently large to undermine the relevance of this episode for past or future tightening cycles.

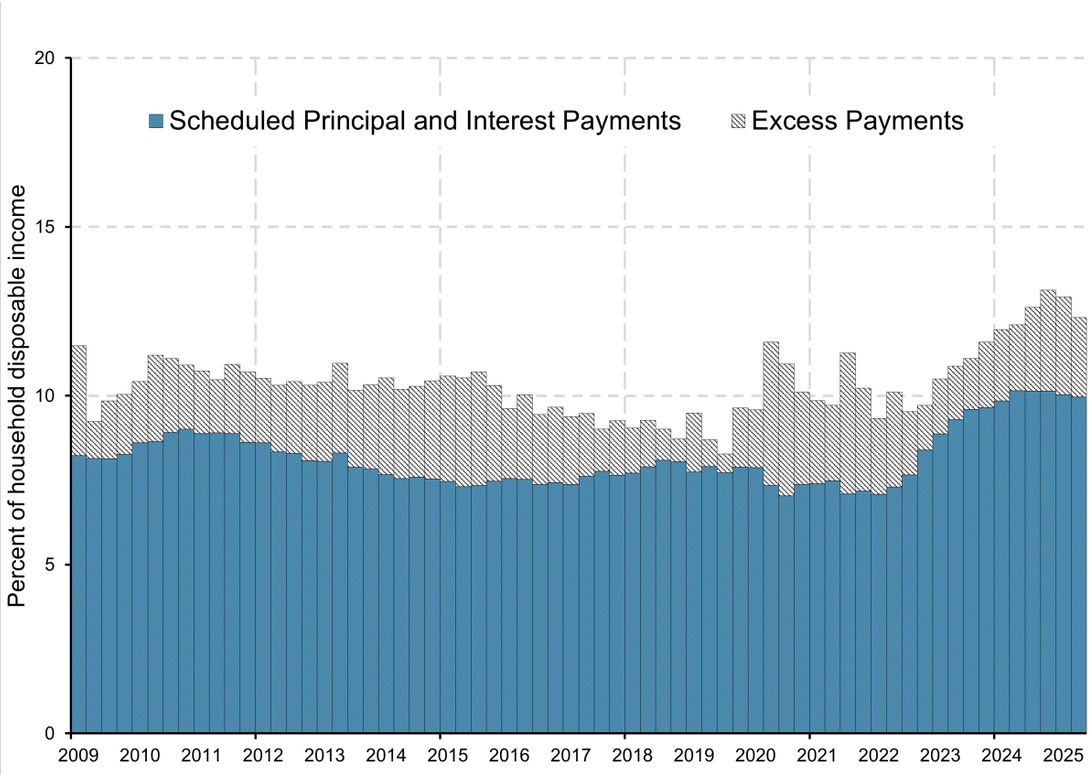
[6]The shift in expectations happened despite then Governor Lowe stating in March 2022 that "[t]he Board is committed to maintaining highly supportive monetary conditions to achieve its objectives of a return to full employment in Australia."

Figure 2: Role of Interest Rates in Household Disposable Income and Mortgage Payments

(a) Real Household Disposable Income Growth



(b) Mortgage Payments to Household Disposable Income (ratio)



Sources: RBA and IMF staff calculations

unemployment. As shown in Figure 3a, the share of individuals working more than one job has increased by 0.4 percentage points since the beginning of the rate-hike cycle, which is equivalent to more than 100,000 additional workers. Meanwhile, the participation rate (Figure 3b) has reached a historic high of around 67 per cent, up from its pre-COVID level of roughly 66 per cent (ABS, 2024). Although both series exhibit long-run upward trends,[7] the recent acceleration in labor supply above trend coincides with the period of monetary tightening.[8]

2.2 Data

We use administrative data from Australia Bureau of Statistics' Person Level Integrated Data Asset (PLIDA) dataset. This dataset integrates diverse administrative data sources, encompassing information on health, education, government payments, income, taxation, employment, and demographic variables. In particular, we utilize employment data collected by the Australian Tax Office (ATO) for the universe of employees in Australia, which provides monthly information on individual and household employment starting in 2021. These data are sourced from the ATO's Single Touch Payroll (STP) system, through which employers report payroll information directly to the ATO each pay cycle. We also use data from the 5-yearly Census, which provides detailed demographic information, as well as information on total household income, mortgage repayments and rental costs as of mid-2021. These data are used to construct debt service ratios as the ratio of mortgage payments to household (weekly) income prior to the start of the tightening cycle. Finally other tax data from the ATO are used to measure (annual) household and individual total incomes in earlier years.

The dataset includes 25.5 million individuals, across 9.3 million households, of which 3.3 million are mortgage holders.

We take the following steps to assemble the dataset for our analysis. By construction, our sample begins with those individuals were who were already in Australia as per the Census data in mid-2021 and, among them, we focus on the working age population between 18 and 65. Those respondents who never report employment income in the three years leading up to the analysis are removed so we can differentiate them from retired workers and from those unable to work for other reasons such as disability. We also remove those who are self-employed in the Census, as their employment income cannot be observed in the STP data. Due to the computational burden of working with the universe of employees in the STP, we use a 10 per cent random sample that amounts to some 800,000 unique workers. Our main results exclude those who we never observe as employed in STP. This exclusion makes near

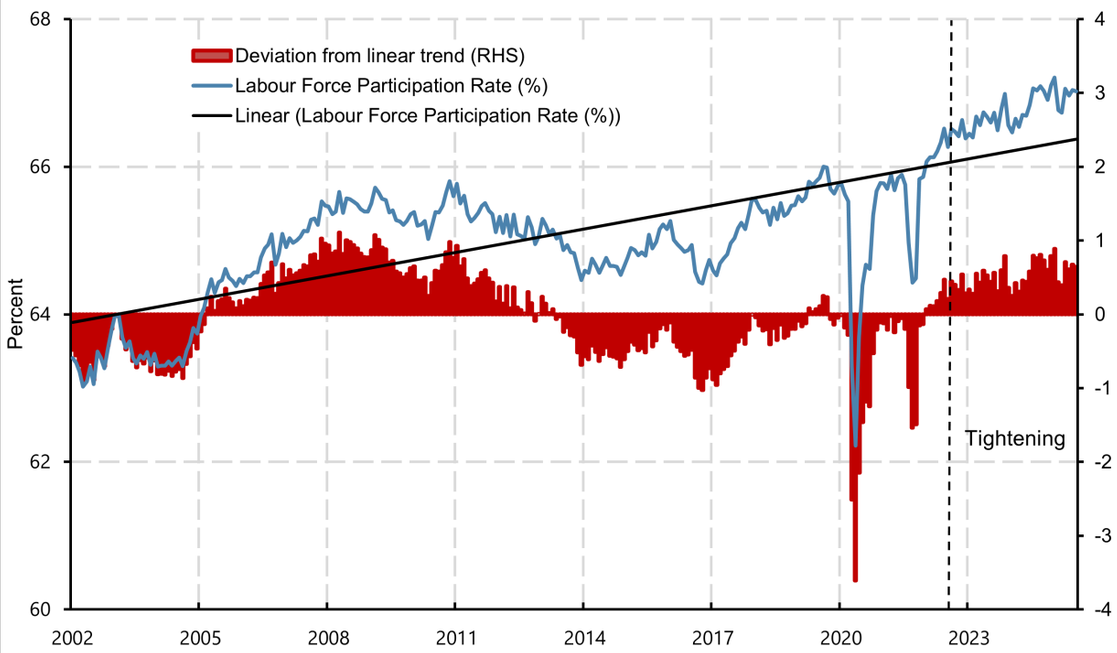
[7] This is a trend observed in many advanced economies reflecting the role of improved health of older workers, increased female labor force participation, and migrant participation IMF (2025)

[8] Over the same period the unemployment rate rose only modestly and remains well below its pre-pandemic level.

no difference to the headline results.[9][10] The results we report are not materially affected when we make slightly different choices, such as including self-employed as at mid-2021.

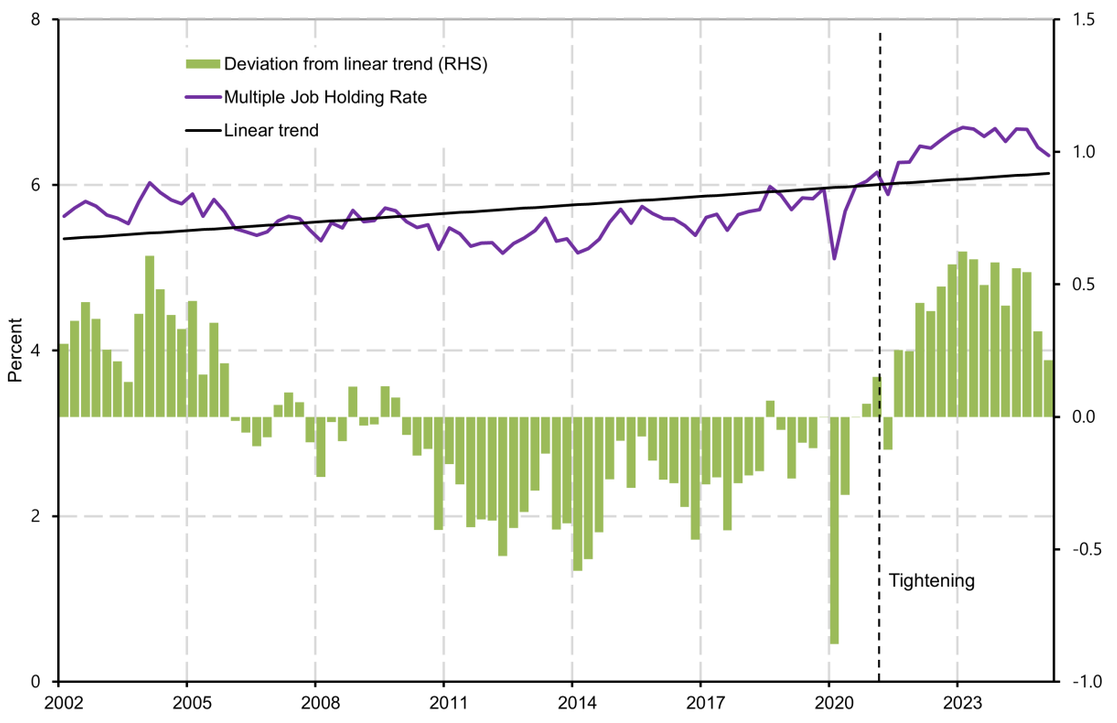
Figure 3: Labor Force Participation Rate

(a) Multiple Job Holding Rate



Sources: ABS; RBA

(b) Multiple Job Holding Rate



Sources: ABS and authors calculations

[9]We make this cleaning decision given the arbitrary nature of any choice we would make to try to exclude people who are retired, have left Australia, or who otherwise have no scope to work.

[10]One may be more concerned that this could be an issue in the sub-sample analysis focused on those not in the labor force. For this group, we show robustness to including those never observed in STP in the Online Appendix, finding qualitatively similar results.

In constructing the pre-tightening DSR ratios we can use household income from either the annual ATO tax data, or weekly data from the Census as of mid-2021. As the two measures of income are highly correlated in practice, we are guided by the following considerations in choosing between them. We use the Census to construct the DSR because it provides the most contemporary snapshot of the DSR pre-rate rise. Furthermore, drawing mortgage payments and household income from the same source ensures consistency of the timing and measurement of the data. However, we use the ATO measure of household income to construct income-related control variables. This allows us to capture longer-term income profiles by using several previous years of ATO data. Moreover, using ATO income in the controls but not the DSR limits the co-linearity that could be caused by using the same varialbe in the DSR and controls. Nevertheless, the results are robust when we use the ATO income to construct the DSR.

To allow for flexible heterogeneity in exposure to interest rate changes, we group individuals into quintiles based on their pre-tightening DSR. This choice reflects the highly non-linear relationship between interest rate increases and household cash-flow stress. At low DSR levels, higher interest rates can often be absorbed with limited adjustment, whereas at high DSR levels even modest rate increases may bind liquidity constraints and induce discrete changes in labor supply.[11] Quintiles provide a transparent way to capture this non-linearity without imposing a functional form, while ensuring sufficient sample size and statistical power within each group.

In considering the results, it is important to note that we will capture only labour income for employees. In cases where people in the sample gain employment by starting a business, or being engaged as a contractor outside of the STP system (e.g. a gig worker) this will not be captured. And similarly if employed workers are gaining additional hours and labour income through such roles this will not be captured. That said, such income will be captured in the DSR calculations.

2.3 Summary Statistics

The means and standard deviations of key variables are presented in Table 1 broken down by debt-service quintile groups, as well as renters and others. As one might expect, the moments of some variables are not uniformly balanced across the debt service groupings. Most noticeably, more indebted individuals and households (those in higher debt service quintiles) have lower average incomes and a lower probability of having a bachelor's degree. Renters are also likely to earn less, are on average younger, and less likely to have a bachelor's

[11] For example Phillips (2023) uses a micro-simulation model to model household mortgage stress in Australia over the period. They showed that for those in the lowest income quintile, mortgage payments as a share of disposable income rose from around 35 per cent to around 55 per cent from 2021 to 2023. For the next income quintile, the rise was smaller from 22 per cent to 33 per cent.

education. Other individuals, which includes those who own their house outright, tend to be older. However, we find nearly no statistical difference in age or gender across debt quintiles. As discussed in the next section, we include controls for observable characteristics to mitigate concerns about violations of the parallel trends assumption.

3 Baseline Regression

To study the dynamic response of labor supply by level of indebtedness, we estimate the following Local Projection-style regressions (following Jordà (2005)) as our baseline for individual i in month t:

$$Y_{i,t} - Y_{i,t_0} = \sum_{q=2}^{5} \beta_{q,t} \mathbb{1}(DSR_i \in Q_q) + X_i' \gamma_t + u_{i,t} \tag{1}$$

where Y represents the outcomes of interest, including a dummy variable for being employed, a count variable for the number of jobs held, and total labor income.[12] In all cases we compare outcomes at time t with the initial period t_0 = July 2021. The treatment variable is an indicator for the quintile of individual's debt-service ratio (DSR) in the 2021 Census (i.e., prior to the start of the tightening cycle). The first (lowest) DSR quintile is omitted and thus serves as the reference group.[13] X is a vector of controls including: age; age-squared; gender; education, the (log) individual and household income in 2019, 2020 and 2021; number of children; type of dwelling (i.e. apartment or house); number of bedrooms in the dwelling; number of people in the home; whether or not the person has moved in the past 1 or 5 years; and detailed geographic area. We use standard errors robust to heteroskedasticity.

Rather than using a single panel regression, we estimate the regression month by month, tracing out the response every month for different DSR groups. As such, we are effectively interacting each control with a time dummy, allowing them to affect labor outcomes differently in each period. This flexibility strengthens identification by ensuring that differential trends in observables are not spuriously attributed to differential outcomes across other observables over time.

[12] More precisely, for labor income we calculate the approximate percentage change in labor income, using growth rates boded from -2 to 2 that a common in the firm dynamism literature.

[13] It is important that we utilize pre-shock DSR values to avoid endogeneity as is conventional in Bartik (1991) type instruments.

4 Results

4.1 Main results

Headline results are presented in Table 3, which reports the differential change in the three main outcome variables for individuals in the 5th DSR quintile relative to those in the 1st quintile. The regression results indicate that more indebted individuals experience a statistically significant and persistent increase in all three labor supply measures over the tightening cycle.

Focusing first on Column (i), the differential effect on the probability of employment becomes apparent in March 2022, coinciding with the sharp rise in expectations of policy rate increases shown in Figure 1b and preceding the first increase in the cash rate. Following the onset of monetary tightening, the magnitude of the effect increases steadily as higher interest rates place growing pressure on household balance sheets. The effect peaks in March 2024, implying that individuals in the 5th DSR quintile experience a 0.94 percentage point higher probability of employment relative to those in the 1st quintile two years after the tightening cycle begins. These responses occurred in a labour market that remained relatively tight throughout the tightening cycle, suggesting that the ability to increase labour supply partly reflected strong underlying labour demand. This differential remains statistically significant at the 1 percent level for up to three years after the initial rate increase.

Turning to Column (ii), the differential impact on the growth in the number of jobs emerges contemporaneously with the start of the tightening cycle, becoming statistically significant at the 10 percent level in June 2022. The effect accelerates rapidly, peaking one year after the onset of tightening, with a differential increase of 1.6 jobs per 100 workers in June 2023. This effect remains statistically significant at the 1 percent level through to the end of the sample period. The differential response of labor income growth (Column (iii)) is observable from late 2021 and continues to strengthen over time, peaking at 4.3 percent differential increase three years after the start of the tightening cycle.

Figure 4 plots the β coefficients for DSR quintiles 2 through 5 from equation (1). Panel (a) presents estimates without controls and suggests that, unconditionally, higher indebtedness is associated with an increased probability of employment around the period when expectations of monetary tightening rise. Once the full set of controls is included in Panel (b), the differential effects for quintiles 2 through 4 are no longer statistically significant, indicating that the labor supply response is concentrated among the most highly indebted households once confounding factors are held constant. A similar pattern is evident for the other two outcome variables shown in Panels (c) and (d). Unlike the employment margin, there is a modest but statistically significant increase in both job holding and labor income growth for

Figure 4: Full Sample Headline Results

Note: Figures plot OLS β coefficients from specification 1. All results are relative to the omitted category of the lowest Debt Service Ratio Group. The outcome variable is a dummy for being employed. Panels (c) and (d) run regressions on the sub-sample of individuals who reported being employed full-time and part-time in the 2021 Census, prior to the start of the tightening cycle.

the 4th quintile toward the end of the sample period, consistent with liquidity constraints becoming more binding as household financial buffers are gradually depleted.

Taken together, these results suggest that the increase in interest rates in Australia induced a labor supply response among households most exposed to rising debt servicing costs. The concentration of effects in the top DSR quintile points to an important role for liquidity constraints, with highly indebted households increasing labor supply to offset declines in disposable income and smooth consumption in the face of higher interest payments.

4.2 Sub-Group Analysis

In the next section, we explore the mechanisms underlying the baseline effects by examining subgroups of individuals who, *a priori*, are more able to adjust their labor supply: non–full-time workers, those Not in the Labor Force (NILF), and individuals without young children.

Rather than estimating heterogeneous effects through interactions in a pooled regression, we conduct the analysis separately by subgroup. We take this approach because these groups plausibly face fundamentally different economic environments and constraints, such that the relationship between labor supply, income, and other covariates may differ across groups. Estimating separate models allows all controls—including time effects and other

covariates—to have group-specific coefficients, avoiding potentially restrictive homogeneity assumptions implicit in pooled specifications.

Table 4 presents differential effects on the three outcome variables for the most indebted individuals, including the full set of control variables for subsamples defined by labor market status at the time of the census.

For each outcome, the sample is split based on whether individuals were employed full-time prior to the onset of the tightening cycle. Among those who were already working full-time, we find no statistically significant differential change in the probability of employment, number of jobs, or pay by DSR group at any point during the sample period (Columns (i), (iv) and (vii)). This suggests that the employment effects are not driven by already full-time employed individuals taking on additional jobs or by lower levels of attrition for this group. It also indicates that there is no evidence of full-time workers responding to cost-of-living pressures by shifting to higher payer jobs.

By contrast, Columns (ii), (v), and (viii) show that the increase in employment is entirely driven by individuals who were not employed full-time prior to tightening. For this group, the magnitude of the effect is approximately three times larger than that observed in the full sample. Columns (iii), (vi) and (ix) show that the effects are driven by part time and those not in the labor force. This aligns with the finding that those in the unemployed group do not exhibit a significant treatment effect (formally documented in the Online Appendix).

Table 5 further decompose between the NILF and part-time workers to allow observation of extensive- and intensive-margin responses. In Columns (i) and (ii), the extensive employment effect is most pronounced among the NILF subgroup.[14] Qualitatively similar patterns are observed in Columns (iii) and (iv), which reports results for the number of jobs held. [15]

This pattern is intuitive. Individuals who are already employed part-time can only adjust along the extensive margin by avoiding labor force exit. In contrast, individuals who were NILF can respond to higher interest rates by entering the labour force and gaining employment.

Columns (v) and (vi) examine subgroup responses in total labor income, capturing both extensive and intensive margin adjustments. Column (v) shows that among part-time workers, highly indebted individuals experience a differential increase in labor earnings, consistent with an increase in hours worked. This effect emerges contemporaneously with the start of monetary tightening and peaks at approximately 3.3 percent, significant at the 1 percent

[14]Coefficient magnitudes in Columns (v) and (vi) are lower than in Column (iv), which may seem counterintuitive. We interpret this difference as reflecting the controls having group-specific effects, due to the heterogeneous impact of macro shocks across subgroups. Note that for the NILF sample we use a 100 per cent sample to increase power due to the smaller number of NILF people in the sample.

[15]The results are also qualitatively similar for the NILF cohort if we include people who worked some time in the past 3 years, but do not work at all during the sample, though the magnitudes are slightly lower. These are not our preferred results as choices we make around the relevant never working but able to work population (e.g. worked in past one or three years) are arbitrary.

level. This is consistent with an intensive margin response.[16]

Finally, Table 6 investigates heterogeneity in employment responses by parental status. In the classic family labor-supply model of Blau and Robins (1988), higher formal childcare costs raise the effective cost of employment for parents, since each additional hour worked requires purchasing an additional hour of care. Childcare therefore acts as a shadow tax on labor, lowering the net return to work and dampening labor supply responses on the margin.

Column (ii) shows that highly indebted adults without children experience an increase in employment probability that is 1.5–2 times larger than that observed in the full sample, following monetary tightening. In contrast, highly indebted adults with older children (aged 6–12) exhibit no statistically significant increase in employment at the start of the tightening cycle, with effects becoming positive and statistically significant at the 10 percent level only by March 2024. Even then, the magnitude of the effect is roughly one-third of that in the full sample. For adults with young children (aged below 5), we find no statistically detectable differential increase in employment probability over the sample period. These differences across groups based on the presence of children are statistically significant.

Tables 7 and 8 replicate this analysis for the number of jobs and total pay, respectively, and reveal qualitatively similar patterns. In both cases, labor supply responses are substantially attenuated for individuals with children (particularly young children and those with two or more children under the age of two) relative to the full sample. While some estimates become positive and statistically significant toward the end of the sample period, they remain considerably smaller in magnitude. In Section 5, we investigate this pattern further by examining how a relaxation of childcare constraints interacts with labor supply responses to monetary tightening in mid 2023.

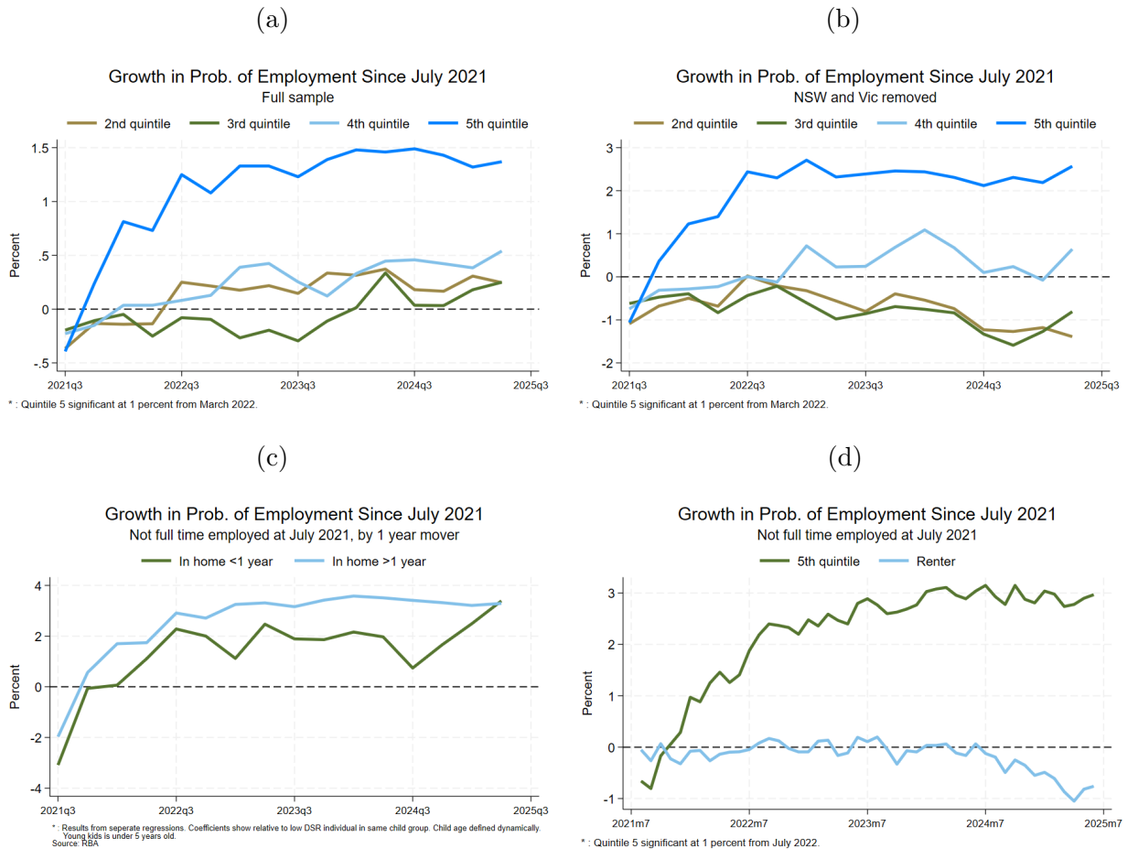
4.3 Threats to identification and robustness

There are three main threats to our interpretation of the results.

First, one may be concerned that the parallel trends assumption does not hold, given that selection into DSR quintiles is non-random. Reassuringly, our finding that the responses are concentrated among non-full-time workers (particularly for total pay) helps to address concerns that our controls do not adequately account for individuals' earnings trajectories, or that households select into high debt in anticipation of future income growth. If such selection were driving the results, we would expect to observe similar responses among full-time workers, but we do not. In this sense, the heterogeneity by employment type acts as an implicit triple-difference test that supports the causal interpretation.

[16] Somewhat surprisingly there is limited evidence of an affect for pay for the NILF group. This appears to reflect significant volatility in the recorded pay of these workers, many of which would be on zero or low labour income at the start of the sample by construction.

Figure 5: Robustness Results



Note: Figures plot OLS β coefficients from specification 1. All results are relative to the omitted category of the lowest Debt Service Ratio Group. The outcome variable is a dummy for being employed. Panels (c) and (d) run regressions on the sub-sample of individuals who reported being employed full-time and part-time in the 2021 Census, prior to the start of the tightening cycle.

We conduct two additional robustness exercises to further address concerns around pre-trends and self-selection. Results are reported in Table 9 and plotted in Figure 5. First, we restrict the sample to households that have lived in their home for five or more years, and thus have occupied the same dwelling since at least 2016. It is unlikely that these households took on high-DSR mortgages in 2016 in anticipation of income gains observed in 2021. Consistent with limited scope for self-selection in this group, the results are very similar to the baseline estimates (Panel (a), Column (ii)).

A related concern is that lockdowns in New South Wales and Victoria during mid-2021 may have temporarily depressed reported income at the time of the Census. Although households were instructed to report their "normal" income, any downward bias could mechanically inflate measured DSRs. At the same time, these households may have experienced a subsequent recovery in income as restrictions eased. To address this possibility, we re-estimate the models excluding New South Wales and Victoria. The results remain largely unchanged (Panel (b), Column (iii)).

A second concern is that liquidity constraints may not be the primary driver of the differential response observed among households in the top DSR quintile. To support this mechanism, we examine whether highly indebted individuals with fixed-rate mortgages, who

are less exposed to short-run changes in debt servicing costs, respond differently from those with variable-rate mortgages. Although mortgage interest rate type is not directly observed in the data, we proxy for this distinction using recent home purchases, as households that moved in 2020 and 2021 were more likely to originate mortgages with an initial fixed-rate period given the unusually low fixed rates at the time, and hence experienced delayed pass-through of rate hikes to service payments.[17] Columns (iv) and (v) split the sample along this dimension and show that the estimated effects are weaker among highly indebted households who took out their mortgages more recently and are therefore plausibly less liquidity constrained. This is despite the fact that, as noted before, there may be some selection into high-DSR in anticipation of higher income for this group, which should bias us towards finding a larger response for recent movers. Notably, by the end of the sample period, the estimated treatment effects for the two groups converge, consistent with recently originated fixed-rate mortgages rolling off and households becoming exposed to similar liquidity constraints. Results are plotted in Panel (c).

The third main concern is that the estimated effects may be driven not by interest rate increases, but by differential exposure to rising prices across DSR quintiles. For example, conditional on income, households with higher DSRs may have lower liquidity and thus be more affected by increases in the cost of living.[18]

Evidence that higher prices are not the primary driver of the differential response is shown in Figure 5(d), where we include renters as an additional control group.[19] Unlike mortgage holders, renters are not directly exposed to increases in mortgage interest costs but are fully exposed to changes in the cost of living. Across all outcomes and periods, we find no differential labor supply response among renters.

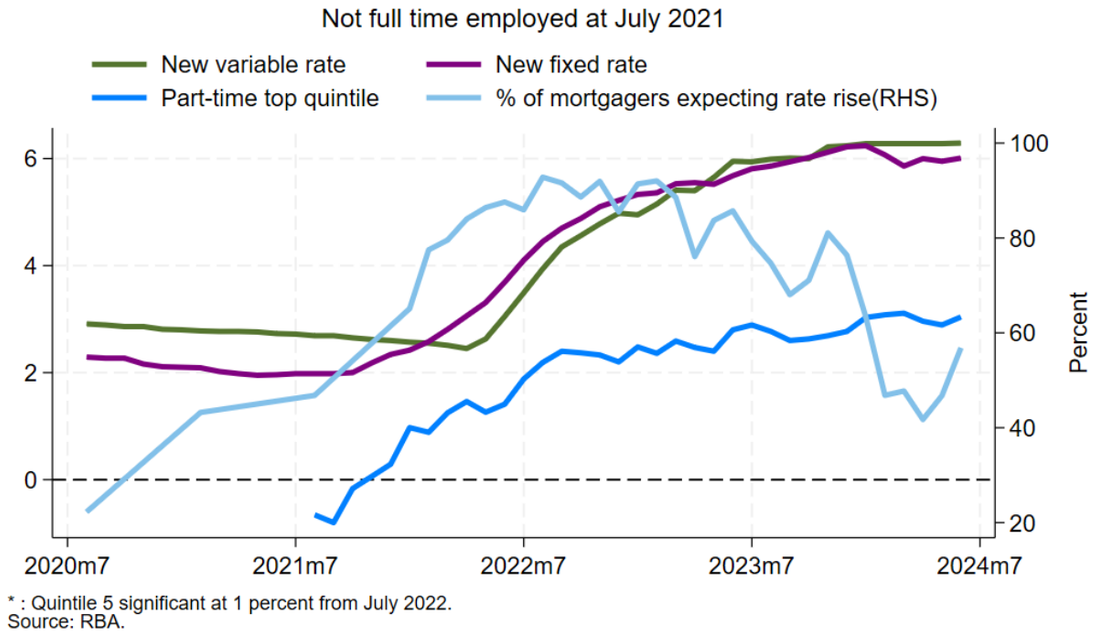
Further evidence comes from the timing of the estimated effects. Figure 6 shows that the labor supply response coincides with the period of rising interest rates' expectations and rises in fixed and variable rate mortgages, consistent with the proposed mechanism. By contrast, we do not observe differential labor supply responses moving in line with inflation. We formally test for this in Tables 12, 13, 14, which include interaction terms between the fifth quintile and interest rates variously alongside controls for inflation and underlying trends. As shown in columns (i) and (ii), there is a statistically significant correlation between changes in interest rates and employment growth. Indeed, a 100 basis point increase in both the fixed and variable rates is associated with a 0.0038 percentage point increase in employment growth for the most indebted individuals. When inflation interacted with fifth quintile is

[17] In Australia, borrowers choose between fixed and variable interest rates at mortgage origination, with fixed-rate contracts typically locking in repayments for two to three years before reverting to variable rates.

[18] Including income as a control mitigates this concern, as we are comparing households with the same nominal income and thus facing similar proportional real income losses from higher prices.

[19] Results are restricted to individuals not working full time at 2021 Census, consistent with our main findings.

Figure 6: Growth in Jobs Per Hundred People Since July 2021



added to the regression (Columns (vii) and (viii)), the coefficient on interest rates marginally diminishes but remains statistically significant.[20]

Finally, even if part of the estimated response reflects higher prices rather than higher interest rates, this does not undermine the core mechanism. Both channels operate through declines in real disposable income, and greater binding of liquidity constraints, prompting households to increase labor supply in order to smooth consumption. To the extent that our estimates capture a combination of higher borrowing costs and higher prices, this primarily affects the interpretation of magnitudes rather than the underlying behavioral response.

5 Natural Experiment: Interest Rates and Childcare

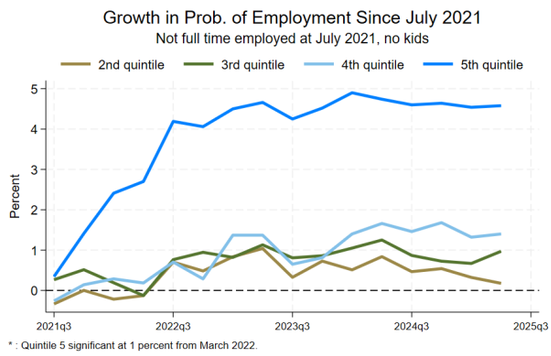
As shown in Section 4.2, highly indebted workers with children exhibit small (or even zero) labor supply responses to tightening interest rates, in contrast to those without children. This is consistent with a long-standing literature showing that higher formal childcare costs raise the effective cost of employment for parents, because each additional hour of work often requires purchasing an additional hour of care (Blau and Robins, 1988). Accordingly, when interest rates rise, the labor supply response among parents may be muted. By contrast, childcare subsidies can enhance labor mobility.[21]

[20] Results are more mixed when using the price level, which closely co-moves with interest rates. We view inflation as the more relevant metric, as the price level remains elevated even after real incomes recover.

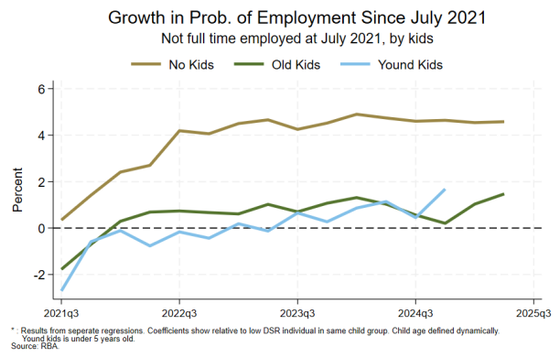
[21] Recent models, such as Berlinski et al. (2020) and Ho and Pavoni (2016), formalize this intuition in general-equilibrium settings, highlighting the tension between substitution effects—where rising childcare costs make market work relatively less attractive—and income effects, where higher costs erode disposable income, prompting additional work to maintain consumption. Because parents' own caregiving and paid childcare are imperfect substitutes (e.g., due to non-pecuniary benefits of direct care), empirical evidence suggests substitution effects dominate. In Australia, Andrews et al. (2014) show that government childcare subsidies explain a substantial share of post-1990s gains in female labor force participation; U.S. studies

Figure 7: Childcare Natural Experiment

(a)



(b)



Note: Figures plot OLS β coefficients from specification 1. All results are relative to the omitted category of the lowest Debt Service Ratio Group. The outcome variable is a dummy for being employed. Panels (c) and (d) run regressions on the sub-sample of individuals who reported being employed full-time and part-time in the 2021 Census, prior to the start of the tightening cycle.

Childcare subsidies are particularly relevant in Australia, where government-administered childcare support (CCS) historically constitutes a significant share of family disposable income, especially for lower-income households. At the start of the pandemic, over 90 percent of families used childcare, with CCS covering 85 percent of fees for low-income households and tapering with income (AIFS, 2023). Net childcare costs for working parents with children in full-time childcare, as a proportion of household income ranged from 12-19 percent in 2020 for a range of family types (Noble and Hurley, 2021).[22] This underscores that changes in subsidy generosity can materially influence labor supply decisions.

In 2022, rising childcare costs became a national concern, prompting the government to increase childcare subsidies from July 2023 onward to ease household financial strain. The reform expanded the higher subsidy rate for families with two or more children under six and lowered the income threshold for eligibility, extending support to more households (Australian Parliament, 2023). Appendix A provides further details on the reform.

The 2023 subsidy reform thus provides a quasi-experimental setting to examine how labor supply responses varied among families within the top DSR quintile.

Figure 7 presents a graphical analysis of monetary tightening and changes in the childcare subsidy. Consistent with Section 4.2, the labor supply response to higher interest rates is largest among those without children. Panel (b) shows growth in employment probability for fifth-quintile DSR individuals with no children, children aged 6–12, and children aged 5 or younger, based on separate regressions. Individuals without children experience a large, significant increase in employment, whereas those with older children show smaller effects, and those with young children exhibit no significant change. Following the childcare subsidy

find employment elasticities with respect to childcare costs of roughly -0.3 to -0.4 (Blau and Robins, 1988; Berlinski et al., 2020).

[22] A single parent earning 67 percent of average wage with two children pays 12 percent, while a couple with two children, both earning average wage pays 19 percent. This suggests the means-tested nature of the CCS was working.

reform in mid-2023, there appears to be a gradual increase in employment probability, particularly for households with young children, while the control group of childless individuals remains flat.

To formally test whether the childcare subsidy relaxed labor supply constraints for highly indebted parents, we estimate a difference-in-differences specification for fifth-quintile DSR individuals who were not full-time employed.[23]

$$Y_{i,t} - Y_{i,t_0} = \boldsymbol{\beta}' \left(\text{Post}_t \times \mathbf{Children}_i \right) + \text{Post}_t + X_i' \gamma + u_{i,t} \tag{2}$$

where Post_t is an indicator equal to one for periods after June 2023; $\mathbf{Children}_i$ is a vector of indicators for the presence of young and older children, with individuals with no children forming the omitted reference group; and $\boldsymbol{\beta}$ is the corresponding vector of difference-in-differences coefficients. The vector of outcome variables Y are set in long differences from $t_0 =$ June 2023. The vector X contains the same set of controls as previously.

As shown in Table 15, the childcare subsidy is associated with a 0.12 percentage point increase in the probability of employment and a 0.14 increase in the number of jobs per 100 workers for individuals with young children, relative to to those with no children, both significant at the 1 percent level. Individuals with older children also exhibit an increase in the number of jobs, significant at the 5 percent level, though the coefficient is smaller. Other outcomes are not distinguishable from zero.

These policies may have two offsetting effects. By reducing out-of-pocket costs, households experience higher disposable income, which could reduce labor supply (income effect). At the same time, the effective cost of working is lower, encouraging substitution from home care to paid work (substitution effect). Consistent with the literature, the substitution effect appears to dominate.

In the context of our paper, childcare is a clear illustration of fiscal-monetary policy interaction in determining the supply of labor. Subsidies for childcare, a fiscal policy, can either offset or exacerbate the income effects of higher debt servicing costs from tighter monetary policy if they are respectively increased or reduced, because both operate the same way on household disposable income.

6 Quantification

We use the estimated quintile-specific treatment effects from Equation 1 to construct an implied aggregate response at the national level. For each outcome Y, the aggregate effect at time t, relative to the baseline period t_0, is obtained by weighting the estimated coefficients by

[23] We focus on this group as they were most likely to benefit from the subsidy.

the population share of each debt-service-ratio (DSR) quintile[24] and expressing the resulting effect in per-capita terms:

$$\frac{\widehat{\Delta Y}_t^{\text{agg}}}{N_t} = \sum_{q=2}^{5} s_{q,t}\hat{\beta}_{q,t}, \tag{3}$$

where N_t denotes the working-age population at time t, $s_{q,t}$ is the population share in DSR quintile q, and $\hat{\beta}_{q,t}$ is the estimated change in outcome Y for individuals in quintile q, relative to the bottom quintile.

Equation (3) provides a transparent mapping from micro-level heterogeneity in labor supply responses to an implied aggregate contribution. The resulting object should be interpreted as a reduced-form equilibrium estimate for the realized 2022–23 tightening episode: the estimated coefficients incorporate the wage and labor demand adjustments that occurred during this period, but the aggregation does not model counterfactual general-equilibrium feedbacks that would arise under alternative policy magnitudes or debt distributions.

A maintained assumption underlying this aggregation is that individuals in the bottom DSR quintile—the omitted category—do not exhibit a systematic labor supply response to monetary tightening. Under this assumption, the weighted sum in Equation (3) can be interpreted as an implied aggregate effect rather than a purely relative effect across debt groups. This assumption is consistent with a large empirical literature documenting limited average labor supply responses to contractionary monetary policy shocks, particularly along the extensive margin (Cantore et al., 2022).

In practice, the aggregate effect is driven largely by households in the top DSR quintile, as estimated effects for quintiles 2–4 are small and statistically indistinguishable from zero across most outcomes and time periods. Nevertheless, we construct aggregates using all quintiles to avoid imposing zero responses outside the top of the debt distribution.

Results for all three outcome variables are presented in Figure 8 using estimated effects from the full sample (Table 3). When including effects for quintiles 1-4, Panel (a) shows that one year after the start of the tightening cycle, monetary tightening is associated with a 0.10 percentage point increase in the participation rate (0.093 percentage points when restricting the aggregation to just the top DSR quintile).[25] This effect rises to 0.15 percentage points at the end of the sample period, three years after tightening begins, corresponding to approximately 31,000 additional employed individuals nationwide. Turning to Panel (b), we find an increase of 0.0020 jobs per working-age individual one year after tightening, rising to 0.0024 jobs per person by mid-2025, implying an additional 50,000 jobs in the aggregate. Finally, Panel (c) shows that labor income is 0.31 percent higher one year after the onset of

[24] Approximately 50 percent of households in the sample are mortgage holders, so each DSR quintile corresponds to roughly 10 percent of the working-age population.

[25] Note that we also assume that unemployment is unimpacted in line with results discussed in Section 4, so the participation rate increase is equal to the increase in the employment-to-population ratio.

tightening, rising to 0.76 percent by June 2025.

Note that if we instead take a minimalist approach, and use estimated treatment effects from the sub-sample groups, then the aggregate impacts are much smaller. For instance, if we assume that the only group to have a positive impact on probability of employment is the NILF group (Table **??**, Column (vi)), then the number of new employed individuals would be estimated to increase by just 8300 or a 0.04 percentage points increase in the participation rate.

In the absence of a fully specified heterogeneous-agent model, these estimates should be interpreted with caution. Taken at face value, however, they suggest that "added worker" effects associated with monetary tightening contributed meaningfully to the increase in labor supply observed in recent years. To the extent that these responses reflect temporary adjustments to elevated cost pressures, some of this effect may unwind as financial conditions normalize.

7 Conclusion

In this paper, we have tested and rejected the hypothesis that labor supply is unresponsive to monetary policy settings. While our results are derived from the specific context of post-Covid Australia, they hold lessons for advanced economies more generally, notably those where variable rate mortgages are more prevalent. The paper shows that labor supply is an important margin of adjustment for households in the face of interest rate shocks, a mechanism that is typically abstracted from in monetary policy frameworks. Beyond the positive findings regarding the impact on labor supply, the results also suggest that liquidity constraints at the household level have important welfare implications that need to be considered by policymakers.

Our results raise a number of questions that merit further exploration. First, to what extent the interest rate shock has affected intra-household decision making. That is, whether high-debt couples with a non-working partner have seen that partner enter the labor force in response to the interest rate shock; and whether such a labor supply increase depends on the number of children or interact with childcare subsidy policies. Second, to what extent the interest shock has had longer-term consequences by affecting decisions related to household formation and having children. Third, to what extent the labor supply response has been limited to particular sectors (e.g., the gig economy).

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Table 1: Summary Statistics (full sample)

		Debt Service Grouping				
		1	2	3	4	5
2021 ATO Income	Mean	89019	87094	84147	79177	71595
	St. Dev	(95747)	(75311)	(70903)	(76112)	(102818)
2021 ATO Household Income	Mean	137516	138738	133850	122162	101099
	St. Dev	(137913)	(115220)	(108060)	(109030)	(132981)
Census Income	Mean	1697	1697	1601	1428	1118
	St. Dev	(1112)	(1028)	(962)	(878)	(840)
Census HH Income	Mean	4319	3902	3380	2805	2057
	St. Dev	(1979)	(1619)	(1486)	(1307)	(1381)
Age	Mean	42	41	40	41	42
	St. Dev	(14)	(12)	(11)	(11)	(11)
Males	Share	0.50	0.50	0.50	0.49	0.48
Higher Degree	Share	0.10	0.10	0.10	0.10	0.10
Bachelor's Degree	Share	0.26	0.26	0.27	0.25	0.23
Children	Share	0.85	1.01	1.12	1.19	1.29
Young Children	Share	0.18	0.28	0.35	0.37	0.35
Part Time	Share	0.24	0.23	0.24	0.25	0.27
Unemployed	Share	0.08	0.07	0.07	0.08	0.10
Married	Share	0.69	0.76	0.78	0.75	0.64
Student	Share	0.12	0.11	0.11	0.10	0.11
Debt Service Ratio	Ratio	0.06	0.12	0.17	0.23	0.47

Notes: Census income is weekly, ATO is annual; part time is less than 35 hours per week. Debt Service Ratio is reported as monthly repayment over weekly income divided by four.

Table 2: Summary Statistics - Not Full Time

		Debt Service Grouping				
		1	2	3	4	5
2021 ATO Income	Mean	57283	55743	53968	51120	47249
	St. Dev	(70670)	(55901)	(54610)	(46875)	(68990)
2021 ATO Household Income	Mean	106912	111111	109827	101464	78487
	St. Dev	(125518)	(107366)	(104024)	(97164)	(111211)
Census Income	Mean	1045	1055	994	891	683
	St. Dev	(909)	(839)	(778)	(700)	(612)
Census HH Income	Mean	3891	3637	3142	2610	1754
	St. Dev	(1977)	(1582)	(1429)	(1219)	(1253)
Age	Mean	41	39	39	40	41
	St. Dev	(15)	(13)	(13)	(12)	(13)
Males	Share	0.35	0.31	0.29	0.30	0.35
Higher Degree	Share	0.07	0.07	0.08	0.07	0.08
Bachelor's Degree	Share	0.22	0.24	0.25	0.24	0.21
Children	Share	0.37	0.50	0.57	0.59	0.56
Young Children	Share	0.15	0.24	0.30	0.32	0.28
Part Time	Share	0.63	0.65	0.64	0.62	0.52
Unemployed	Share	0.20	0.21	0.20	0.20	0.20
Married	Share	0.63	0.69	0.73	0.73	0.63
Student	Share	0.20	0.18	0.17	0.17	0.16
Debt Service Ratio	Ratio	0.05	0.12	0.17	0.23	0.54

Notes: Census income is weekly, ATO is annual; part time is less than 35 hours per week; Not full time excludes those who were working more than 35 hours a week and includes part time, unemployed, and those not in the labor market. Debt Service Ratio is reported as monthly repayment over weekly income divided by four.

Table 3: Headline results:
Effects for the 5th DSR (relative to the 1st DSR)

	Full Sample		
	Probability of Employment	Number of Jobs	Percentage Effect on Pay
12/31/2021	-0.000254	0.000950	0.00898*
	(0.00149)	(0.00250)	(0.0044)
3/31/2022	0.00405*	0.00272	0.0167***
	(0.00171)	(0.00275)	(0.0048)
6/30/2022	0.00420*	0.00547	0.0235***
	(0.00178)	(0.00293)	(0.0049)
9/30/2022	0.00956***	0.0119***	0.0268***
	(0.00188)	(0.00299)	(0.0050)
12/31/2022	0.00733***	0.00765*	0.0360***
	(0.00195)	(0.00307)	(0.0052)
3/31/2023	0.00867***	0.0131***	0.0247***
	(0.00202)	(0.00313)	(0.0053)
6/30/2023	0.00931***	0.0157***	0.0307***
	(0.00204)	(0.00315)	(0.0054)
9/30/2023	0.00853***	0.0112***	0.0356***
	(0.00214)	(0.00320)	(0.0054)
12/31/2023	0.00825***	0.0112***	0.0398***
	(0.00218)	(0.00319)	(0.0054)
3/31/2024	0.00941***	0.0106**	0.0421***
	(0.00221)	(0.00324)	(0.0055)
6/30/2024	0.00882***	0.0132***	0.0439***
	(0.00222)	(0.00328)	(0.0055)
9/30/2024	0.00775***	0.0100**	0.0367***
	(0.00226)	(0.00331)	(0.0056)
12/31/2024	0.00769***	0.00987**	0.0402***
	(0.00230)	(0.00335)	(0.0057)
3/31/2025	0.00809***	0.0104**	0.0374***
	(0.00233)	(0.00335)	(0.0057)
6/30/2025	0.00906***	0.0136***	0.0434***
	(0.00235)	(0.00338)	(0.0057)
Observations	831,900	831,900	831,900

Notes: This table reports relative difference in the three outcome variables at different time periods for the 5th DSR relative to the 1st DSR as estimated in repeated cross sections outlined in equation 1. The regressions include a full set of control variables. Regressions are run in repeated cross sections with each line showing results from a single regression on the labeled month. Standard errors are reported in parentheses. Columns correspond to employment categories as classified in the Census prior to the start of the tightening cycle. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Subgroup Results (Full time, Not full time, PT/NILF): 5th DSR Relative to 1st DSR

	Probability of Employment			Number of Jobs			Percentage Effect on Pay		
	Full time	Not full time	PT/NILF	Full time	Not full time	PT/NILF	Full time	Not full time	PT/NILF
12/31/2021	0.00052	0.003	-0.004	0.002	0.003	-0.003	-0.002	0.027***	0.015*
	(0.001)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.008)	(0.009)
3/31/2022	0.00008	0.013***	0.007**	-0.002	0.011**	0.006	-0.008	0.049***	0.037***
	(0.002)	(0.004)	(0.004)	(0.003)	(0.005)	(0.006)	(0.005)	(0.009)	(0.010)
6/30/2022	0.00033	0.014***	0.008**	0.001	0.015***	0.008	0.007	0.044***	0.029***
	(0.002)	(0.004)	(0.004)	(0.003)	(0.005)	(0.006)	(0.005)	(0.009)	(0.010)
9/30/2022	0.002	0.024***	0.018***	0.003	0.026***	0.020***	-0.00098	0.062***	0.054***
	(0.002)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.005)	(0.009)	(0.010)
12/31/2022	-0.00017	0.022***	0.016***	0.001	0.021***	0.017***	0.011*	0.066***	0.054***
	(0.002)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.006)	(0.010)	(0.011)
3/31/2023	-0.00054	0.026***	0.022***	0.004	0.028***	0.024***	-0.007	0.064***	0.061***
	(0.002)	(0.004)	(0.004)	(0.003)	(0.006)	(0.006)	(0.006)	(0.010)	(0.011)
6/30/2023	-0.001	0.028***	0.024***	0.003	0.036***	0.033***	-0.001	0.068***	0.062***
	(0.002)	(0.004)	(0.004)	(0.003)	(0.006)	(0.007)	(0.005)	(0.010)	(0.011)
9/30/2023	-0.00027	0.026***	0.021***	0.002	0.030***	0.026***	0.00006	0.080***	0.074***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)
12/31/2023	-0.002	0.028***	0.022***	-0.00028	0.033***	0.029***	0.004	0.082***	0.071***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)
3/31/2024	-0.002	0.031***	0.026***	-0.003	0.035***	0.032***	0.006	0.085***	0.074***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)
6/30/2024	-0.002	0.030***	0.026***	0.002	0.035***	0.033***	0.002	0.094***	0.087***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)
9/30/2024	-0.003	0.028***	0.023***	-0.002	0.031***	0.029***	-0.003	0.085***	0.081***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)
12/31/2024	-0.003	0.028***	0.025***	-0.002	0.032***	0.031***	0.003	0.084***	0.081***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.011)
3/31/2025	-0.001	0.027***	0.025***	-0.00001	0.031***	0.032***	-0.00004	0.082***	0.083***
	(0.002)	(0.004)	(0.005)		(0.006)	(0.007)	(0.004)	(0.010)	(0.011)
6/30/2025	-0.001	0.030***	0.026***	0.00095	0.038***	0.038***	0.007	0.084***	0.082***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.010)	(0.012)
Observations	489,400	342,500	267,700	489,400	342,500	267,700	489,400	342,500	267,700

Notes: Each cell reports the estimated effect for the 5th DSR relative to the 1st DSR from repeated cross-sectional regressions (equation 1). All specifications include the full set of controls. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Subgroup Results (Part-Time and NILF): 5th DSR Relative to 1st DSR

	Probability of Employment		Number of Jobs		Percentage Effect on Pay	
	Part time	NILF	Part time	NILF	Part time	NILF
12/31/2021	-0.005*	-0.005	-0.004	-0.007	0.010	-0.020**
	(0.003)	(0.004)	(0.005)	(0.005)	(0.009)	(0.008)
3/31/2022	0.001	-0.001	-0.002	-0.003	0.021**	-0.018**
	(0.003)	(0.004)	(0.006)	(0.005)	(0.010)	(0.008)
6/30/2022	-0.001	0.0009	0.00025	-0.004	0.00099	-0.018**
	(0.003)	(0.004)	(0.006)	(0.005)	(0.010)	(0.009)
9/30/2022	0.006*	0.005	0.007	0.007	0.024**	-0.007
	(0.004)	(0.004)	(0.007)	(0.005)	(0.011)	(0.009)
12/31/2022	0.003	0.002	0.001	0.001	0.026**	-0.009
	(0.004)	(0.004)	(0.007)	(0.005)	(0.011)	(0.009)
3/31/2023	0.005	0.006	0.004	0.006	0.031***	-0.007
	(0.004)	(0.004)	(0.007)	(0.005)	(0.011)	(0.009)
6/30/2023	0.005	0.008*	0.011	0.010*	0.022**	-0.002
	(0.004)	(0.004)	(0.007)	(0.005)	(0.011)	(0.009)
9/30/2023	0.004	0.003	0.008	0.003	0.033***	-0.007
	(0.004)	(0.005)	(0.007)	(0.005)	(0.011)	(0.009)
12/31/2023	0.003	0.008*	0.008	0.009*	0.026**	0.008
	(0.004)	(0.005)	(0.007)	(0.005)	(0.011)	(0.009)
3/31/2024	0.005	0.012**	0.007	0.015***	0.021*	0.011
	(0.004)	(0.005)	(0.007)	(0.005)	(0.011)	(0.009)
6/30/2024	0.003	0.011**	0.006	0.014***	0.030***	0.011
	(0.004)	(0.005)	(0.007)	(0.005)	(0.012)	(0.009)
9/30/2024	-0.00043	0.006	0.003	0.009	0.030**	-0.003
	(0.004)	(0.005)	(0.007)	(0.005)	(0.012)	(0.009)
12/31/2024	0.00056	0.008*	0.003	0.010*	0.028**	0.004
	(0.004)	(0.005)	(0.007)	(0.005)	(0.012)	(0.009)
3/31/2025	0.0008	0.009**	0.003	0.011**	0.029**	0.005
	(0.004)	(0.005)	(0.007)	(0.005)	(0.012)	(0.009)
6/30/2025	0.0003	0.010**	0.006	0.012**	0.028**	0.013
	(0.004)	(0.005)	(0.007)	(0.005)	(0.012)	(0.009)
Observations	209,700	575,400	209,700	575,400	209,700	575,400

Notes: Each cell reports the estimated effect for the 5th DSR relative to the 1st DSR from repeated cross-sectional regressions (equation 1). All specifications include the full set of controls. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Childcare subgroup:
Effect on probability of employment for the 5th DSR (relative to the 1st DSR), Not Full Time Employed

	All	No children	Old children	Young children	2+ young children
12/31/2021	0.00288	0.0141**	-0.00706	-0.00590	0.00563
	(0.0032)	(0.0045)	(0.0057)	(0.0076)	(0.0144)
3/31/2022	0.0125***	0.0241***	0.00291	-0.00111	0.0160
	(0.0035)	(0.0050)	(0.0062)	(0.0093)	(0.0160)
6/30/2022	0.0141***	0.0270***	0.00688	-0.00769	0.0148
	(0.0037)	(0.0053)	(0.0064)	(0.0093)	(0.0159)
9/30/2022	0.0240***	0.0419***	0.00735	-0.00166	0.0145
	(0.0038)	(0.0056)	(0.0067)	(0.0098)	(0.0165)
12/31/2022	0.0220***	0.0406***	0.00667	-0.00437	0.00947
	(0.0040)	(0.0058)	(0.0070)	(0.0097)	(0.0166)
3/31/2023	0.0259***	0.0450***	0.00608	0.00185	0.0339
	(0.0041)	(0.0059)	(0.0068)	(0.0109)	(0.0175)
6/30/2023	0.0280***	0.0466***	0.0102	-0.00129	0.0114
	(0.0041)	(0.0060)	(0.0068)	(0.0108)	(0.0170)
9/30/2023	0.0260***	0.0425***	0.00699	0.00649	0.0213
	(0.0042)	(0.0061)	(0.0071)	(0.0112)	(0.0178)
12/31/2023	0.0277***	0.0452***	0.0107	0.00270	0.0139
	(0.0042)	(0.0062)	(0.0072)	(0.0113)	(0.0174)
3/31/2024	0.0311***	0.0490***	0.0131	0.00861	0.0231
	(0.0043)	(0.0062)	(0.0069)	(0.0127)	(0.0196)
6/30/2024	0.0304***	0.0474***	0.0103	0.0114	0.0263
	(0.0043)	(0.0063)	(0.0070)	(0.0124)	(0.0189)
9/30/2024	0.0278***	0.0460***	0.00561	0.00449	0.0243
	(0.0043)	(0.0064)	(0.0070)	(0.0125)	(0.0193)
12/31/2024	0.0281***	0.0464***	0.00203	0.0168	0.0414*
	(0.0044)	(0.0065)	(0.0070)	(0.0127)	(0.0197)
3/31/2025	0.0274***	0.0454***	0.0103		
	(0.0044)	(0.0065)	(0.0062)		
6/30/2025	0.0297***	0.0458***	0.0147*		
	(0.0044)	(0.0065)	(0.0062)		
Observations	342,500	198,277	75,900	68,300	22,000

Notes: This table reports the relative difference in probability of employment for the 5th DSR relative to the 1st DSR among not full-time employed individuals, broken down by household composition. t-statistics are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Blank cells correspond to missing data.

Table 7: Childcare subgroup:
Effect on number of jobs for the 5th DSR (relative to the 1st DSR), Not Full Time Employed

	All	No children	Old children	Young children	2+ young children
12/31/2021	0.00342	0.0138*	-0.00136	-0.00514	-0.00340
	(0.0048)	(0.0068)	(0.0091)	(0.0101)	(0.0189)
3/31/2022	0.0111*	0.0238**	0.00285	-0.00865	0.0198
	(0.0052)	(0.0074)	(0.0098)	(0.0124)	(0.0208)
6/30/2022	0.0150**	0.0250**	0.0105	-0.00266	0.0304
	(0.0055)	(0.0078)	(0.0104)	(0.0127)	(0.0214)
9/30/2022	0.0265***	0.0401***	0.0209*	-0.00649	0.0120
	(0.0056)	(0.0081)	(0.0106)	(0.0132)	(0.0226)
12/31/2022	0.0212***	0.0365***	0.00294	-0.00321	0.00998
	(0.0058)	(0.0082)	(0.0109)	(0.0134)	(0.0227)
3/31/2023	0.0281***	0.0399***	0.0102	0.0194	0.0552*
	(0.0058)	(0.0084)	(0.0103)	(0.0147)	(0.0239)
6/30/2023	0.0365***	0.0468***	0.0223*	0.0172	0.0383
	(0.0059)	(0.0084)	(0.0105)	(0.0148)	(0.0239)
9/30/2023	0.0301***	0.0387***	0.0223*	0.00798	0.0182
	(0.0060)	(0.0085)	(0.0107)	(0.0151)	(0.0243)
12/31/2023	0.0329***	0.0417***	0.0219*	0.0200	0.0307
	(0.0059)	(0.0085)	(0.0106)	(0.0147)	(0.0234)
3/31/2024	0.0351***	0.0438***	0.0253*	0.0215	0.0469
	(0.0060)	(0.0085)	(0.0103)	(0.0169)	(0.0264)
6/30/2024	0.0350***	0.0418***	0.0214*	0.0287	0.0551*
	(0.0060)	(0.0086)	(0.0104)	(0.0167)	(0.0261)
9/30/2024	0.0314***	0.0380***	0.0175	0.0236	0.0406
	(0.0061)	(0.0087)	(0.0104)	(0.0167)	(0.0267)
12/31/2024	0.0321***	0.0408***	0.0148	0.0335*	0.0701**
	(0.0061)	(0.0088)	(0.0105)	(0.0168)	(0.0267)
3/31/2025	0.0312***	0.0396***	0.0228*		
	(0.0062)	(0.0088)	(0.0089)		
6/30/2025	0.0376***	0.0427***	0.0340***		
	(0.0062)	(0.0089)	(0.0089)		
Observations	342,500	198,300	75,900	68,300	22,000

Notes: This table reports the relative difference in number of jobs for the 5th DSR relative to the 1st DSR among not full-time employed individuals, broken down by household composition. t-statistics are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Blank cells correspond to missing data.

Table 8: Childcare subgroup:
Percentage effect on pay for the 5th DSR (relative to the 1st DSR), Not Full Time Employed

	All	No children	Old children	Young children	2+ young children
12/31/2021	0.0270**	0.0553***	-0.00344	0.0232	0.0155
	(0.0084)	(0.0120)	(0.0156)	(0.0190)	(0.0352)
3/31/2022	0.0489***	0.0715***	0.0297	0.0460*	0.0683
	(0.0090)	(0.0128)	(0.0164)	(0.0224)	(0.0388)
6/30/2022	0.0436***	0.0798***	0.0232	0.00800	0.0288
	(0.0093)	(0.0133)	(0.0169)	(0.0229)	(0.0389)
9/30/2022	0.0620***	0.103***	0.0307	0.0224	0.0444
	(0.0095)	(0.0137)	(0.0173)	(0.0229)	(0.0396)
12/31/2022	0.0656***	0.0991***	0.0568**	0.0284	0.0377
	(0.0097)	(0.0140)	(0.0179)	(0.0237)	(0.0405)
3/31/2023	0.0640***	0.112***	0.0226	0.0339	0.0951*
	(0.0098)	(0.0142)	(0.0171)	(0.0257)	(0.0413)
6/30/2023	0.0675***	0.112***	0.0342*	0.0368	0.0276
	(0.0099)	(0.0144)	(0.0172)	(0.0259)	(0.0412)
9/30/2023	0.0803***	0.124***	0.0386*	0.0393	0.0405
	(0.0100)	(0.0144)	(0.0173)	(0.0262)	(0.0422)
12/31/2023	0.0816***	0.123***	0.0429*	0.0505	0.0527
	(0.0101)	(0.0145)	(0.0177)	(0.0264)	(0.0418)
3/31/2024	0.0846***	0.118***	0.0605***	0.0420	0.0239
	(0.0101)	(0.0145)	(0.0169)	(0.0296)	(0.0460)
6/30/2024	0.0941***	0.131***	0.0608***	0.0656*	0.0583
	(0.0102)	(0.0148)	(0.0171)	(0.0295)	(0.0452)
9/30/2024	0.0851***	0.133***	0.0398*	0.0442	0.0490
	(0.0102)	(0.0147)	(0.0171)	(0.0295)	(0.0450)
12/31/2024	0.0836***	0.131***	0.0302	0.0639*	0.0597
	(0.0104)	(0.0149)	(0.0174)	(0.0297)	(0.0459)
3/31/2025	0.0824***	0.125***	0.0453**		
	(0.0104)	(0.0149)	(0.0149)		
6/30/2025	0.0835***	0.123***	0.0498***		
	(0.0105)	(0.0151)	(0.0150)		
Observations	342,500	198,300	75,900	68,300	22,000

Notes: This table reports the relative percentage difference in pay for the 5th DSR relative to the 1st DSR among not full-time employed individuals, broken down by household composition. t-statistics are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Blank cells correspond to missing data.

Table 9: Robustness table:
Effect on probability of employment for the 5th DSR (relative to the 1st DSR)

	(1) All	(2) Moved more than 5 years ago	(3) Exclude Vic and NSW	(4) Recent mover	(5) Not recent mover
12/31/2021	0.003	0.002	0.004	-0.00063	0.006*
	(0.003)	(0.002)	(0.005)	(0.013)	(0.003)
3/31/2022	0.013***	0.008***	0.012**	0.00065	0.017***
	(0.004)	(0.002)	(0.006)	(0.013)	(0.004)
6/30/2022	0.014***	0.007***	0.014**	0.011	0.017***
	(0.004)	(0.002)	(0.006)	(0.014)	(0.004)
9/30/2022	0.024***	0.013***	0.024***	0.023	0.029***
	(0.004)	(0.002)	(0.006)	(0.014)	(0.004)
12/31/2022	0.022***	0.011***	0.023***	0.020	0.027***
	(0.004)	(0.003)	(0.006)	(0.015)	(0.004)
3/31/2023	0.026***	0.013***	0.027***	0.011	0.033***
	(0.004)	(0.003)	(0.006)	(0.015)	(0.004)
6/30/2023	0.028***	0.013***	0.023***	0.025*	0.033***
	(0.004)	(0.003)	(0.006)	(0.015)	(0.004)
9/30/2023	0.026***	0.012***	0.024***	0.019	0.032***
	(0.004)	(0.003)	(0.007)	(0.015)	(0.004)
12/31/2023	0.028***	0.014***	0.025***	0.019	0.034***
	(0.004)	(0.003)	(0.007)	(0.015)	(0.004)
3/31/2024	0.031***	0.015***	0.024***	0.022	0.036***
	(0.004)	(0.003)	(0.007)	(0.015)	(0.004)
6/30/2024	0.030***	0.015***	0.023***	0.020	0.035***
	(0.004)	(0.003)	(0.007)	(0.015)	(0.004)
9/30/2024	0.028***	0.015***	0.021***	0.007	0.034***
	(0.004)	(0.003)	(0.007)	(0.016)	(0.005)
12/31/2024	0.028***	0.014***	0.023***	0.017	0.033***
	(0.004)	(0.003)	(0.007)	(0.016)	(0.005)
3/31/2025	0.027***	0.013***	0.022***	0.025	0.032***
	(0.004)	(0.003)	(0.007)	(0.016)	(0.005)
6/30/2025	0.030***	0.014***	0.026***	0.034**	0.033***
	(0.004)	(0.003)	(0.007)	(0.016)	(0.005)
Observations	342,500	444,100	146,900	52,400	290,100

Table 10: Robustness table:
Effect on number of jobs for the 5th DSR (relative to the 1st DSR)

	(1) All	(2) Moved more than 5 years ago	(3) Exclude Vic and NSW	(4) Recent mover	(5) Not recent mover
12/31/2021	0.003	0.003	0.004	0.002	0.008
	(0.005)	(0.003)	(0.007)	(0.019)	(0.005)
3/31/2022	0.011**	0.007*	0.012	0.011	0.017***
	(0.005)	(0.003)	(0.008)	(0.020)	(0.005)
6/30/2022	0.015***	0.008**	0.013	0.015	0.020***
	(0.005)	(0.004)	(0.009)	(0.021)	(0.006)
9/30/2022	0.026***	0.013***	0.025***	0.035*	0.034***
	(0.006)	(0.004)	(0.009)	(0.021)	(0.006)
12/31/2022	0.021***	0.008**	0.022**	0.024	0.029***
	(0.006)	(0.004)	(0.009)	(0.021)	(0.006)
3/31/2023	0.028***	0.016***	0.029***	0.010	0.037***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
6/30/2023	0.036***	0.018***	0.032***	0.026	0.044***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
9/30/2023	0.030***	0.015***	0.028***	0.032	0.037***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
12/31/2023	0.033***	0.016***	0.032***	0.043**	0.040***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
3/31/2024	0.035***	0.014***	0.030***	0.032	0.041***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
6/30/2024	0.035***	0.018***	0.026***	0.027	0.041***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
9/30/2024	0.031***	0.017***	0.028***	0.029	0.038***
	(0.006)	(0.004)	(0.009)	(0.022)	(0.006)
12/31/2024	0.032***	0.015***	0.030***	0.032	0.038***
	(0.006)	(0.004)	(0.010)	(0.023)	(0.006)
3/31/2025	0.031***	0.015***	0.022**	0.032	0.038***
	(0.006)	(0.004)	(0.010)	(0.023)	(0.006)
6/30/2025	0.038***	0.017***	0.034***	0.051**	0.043***
	(0.006)	(0.004)	(0.010)	(0.023)	(0.006)
Observations	342,500	444,100	146,900	52,400	290,100

Table 11: Robustness table:
Percentage effect on pay for the 5th DSR (relative to the 1st DSR)

	(1) All	(2) Moved more than 5 years ago	(3) Exclude Vic and NSW	(4) Recent mover	(5) Not recent mover
12/31/2021	0.027***	0.009	0.027**	0.028	0.049***
	(0.008)	(0.006)	(0.013)	(0.031)	(0.009)
3/31/2022	0.049***	0.024***	0.065***	0.062*	0.067***
	(0.009)	(0.006)	(0.014)	(0.034)	(0.009)
6/30/2022	0.044***	0.029***	0.052***	0.070**	0.062***
	(0.009)	(0.006)	(0.015)	(0.034)	(0.010)
9/30/2022	0.062***	0.030***	0.071***	0.089**	0.083***
	(0.009)	(0.006)	(0.015)	(0.035)	(0.010)
12/31/2022	0.066***	0.038***	0.080***	0.119***	0.085***
	(0.010)	(0.007)	(0.015)	(0.036)	(0.010)
3/31/2023	0.064***	0.030***	0.070***	0.078**	0.087***
	(0.010)	(0.007)	(0.015)	(0.036)	(0.010)
6/30/2023	0.068***	0.040***	0.059***	0.113***	0.086***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.010)
9/30/2023	0.080***	0.044***	0.074***	0.103***	0.102***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.010)
12/31/2023	0.082***	0.050***	0.089***	0.100***	0.107***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.010)
3/31/2024	0.085***	0.055***	0.080***	0.104***	0.104***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.010)
6/30/2024	0.094***	0.059***	0.085***	0.085**	0.115***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.011)
9/30/2024	0.085***	0.052***	0.079***	0.070*	0.108***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.011)
12/31/2024	0.084***	0.052***	0.084***	0.107***	0.103***
	(0.010)	(0.007)	(0.016)	(0.037)	(0.011)
3/31/2025	0.082***	0.047***	0.093***	0.103***	0.100***
	(0.010)	(0.007)	(0.016)	(0.038)	(0.011)
6/30/2025	0.084***	0.055***	0.085***	0.115***	0.099***
	(0.010)	(0.007)	(0.016)	(0.038)	(0.011)
Observations	342,500	444,100	146,900	52,400	290,100

Table 12: Inflation vs. interest rate horse race:
Full Sample Period Regressions

	Employment growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top quintile × Variable rate	0.0038		–		0.003		0.003**	
	(1.81)		()		(1.20)		(2.04)	
Top quintile × Fixed rate		0.0038*		0.004*		0.004		0.003**
		(2.04)		(1.91)		(1.54)		(2.06)
Top quintile × Linear trend	N	N	Y	Y	N	N	N	N
Top quintile × Price level	N	N	N	N	Y	Y	N	N
Top quintile × YoY inflation	N	N	N	N	N	N	Y	Y
Observations	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500

Notes: Regressions use quarterly observations and include the full set of controls from the main specification, along with interactions with interest rates or inflation as indicated. t-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 13: Inflation vs. interest rate horse race:
Full Sample Period Regressions

	Job growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top quintile × Variable rate	0.003		0.008***		0.009**		0.004*	
	(1.30)		(2.63)		(2.37)		(1.97)	
Top quintile × Fixed rate		0.004*		0.008***		0.010**		0.004*
		(2.24)		(2.70)		(2.49)		(1.90)
Top quintile × Linear trend	N	N	Y	Y	N	N	N	N
Top quintile × Price level	N	N	N	N	Y	Y	N	N
Top quintile × YoY inflation	N	N	N	N	N	N	Y	Y
Observations	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500

Notes: Regressions use quarterly observations and include the full set of controls from the main specification, along with interactions with interest rates or inflation as indicated. t-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 14: Inflation vs. interest rate horse race:
Full Sample Period Regressions

	Pay growth							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top quintile × Variable rate	0.008**		0.005		0.001		0.008**	
	(-2.18)		(-1.03)		(-0.21)		(-2.32)	
Top quintile × Fixed rate		0.008**		0.007		0.006		0.009**
		(2.24)		(1.45)		(0.86)		(2.43)
Top quintile × Linear trend	N	N	Y	Y	N	N	N	N
Top quintile × Price level	N	N	N	N	Y	Y	N	N
Top quintile × YoY inflation	N	N	N	N	N	N	Y	Y
Observations	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500	1,332,500

Notes: Regressions use quarterly observations and include the full set of controls from the main specification, along with interactions with interest rates or inflation as indicated. t-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

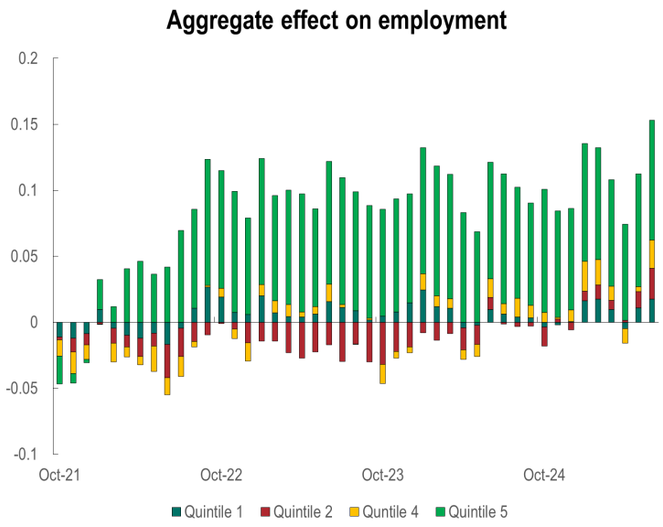
Table 15: Childcare natural experiment:
Effects for the 5th DSR Relative to the 5th DSR with No Children (Post June 2023)

	Probability of Employment		Number of Jobs		Pay	
	Old children	Young children	Old children	Young children	Old children	Young children
Post June 2023	0.00175	0.0120**	0.00846*	0.0137**	-0.00387	0.00404
	(0.74)	(3.29)	(2.48)	(2.67)	(-0.65)	(0.44)
R^2	0.022	0.022	0.017	0.017	0.036	0.036
Observations	2,740,000	2,740,000	2,740,000	2,740,000	2,558,900	2,558,900

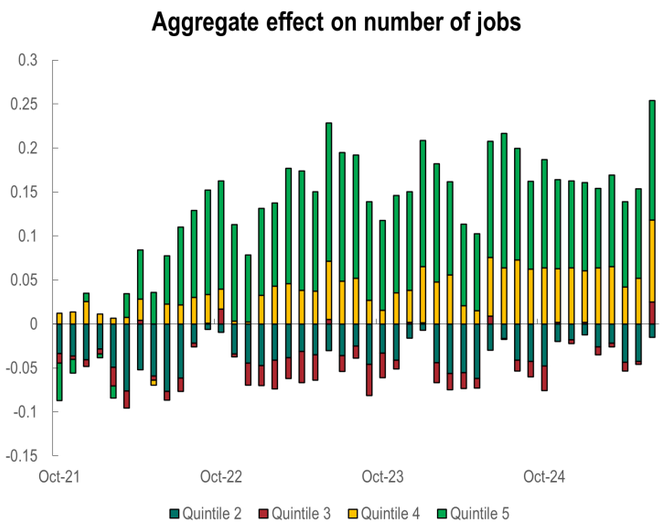
Notes: This table reports post–June 2023 effects for individuals in the 5th DSR quintile relative to individuals in the 5th DSR quintile with no children, restricting the sample to not full-time employed individuals. t-statistics are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 8: Aggregate impacts

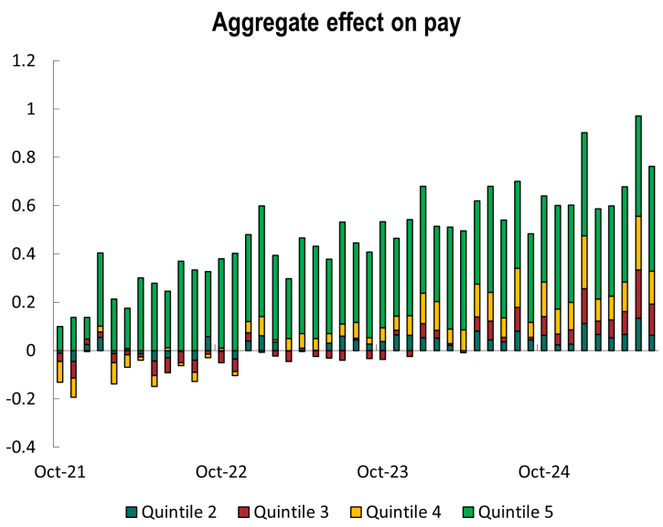
(a)



(b)



(c)



Note: Figures plot the aggregation of OLS β coefficients from specification 3.

A Childcare subsidy

- Eligibility
  - Families must care for a child aged 13 or younger who is not attending secondary school, unless an exemption applies
  - Families need to use an approved child care service and be responsible for paying child care fees
  - There are residence and immunisation requirements.

- Subsidy rate
  - The subsidy amount is determined by a family's income and activity level
  - Families with higher income may receive a lower subsidy rate.

- Change in 2023
  - The maximum subsidy rate increased to 90% for families earning up to $80,000, and the income limit for CCS increased to $530,000.
  - Made less progressive decreasing the effective marginal tax rate

Figure 9: Change in Childcare Subsidy

Family income	Current CCS %	New CCS% from July 23	Possible savings at $140 fee per day
$70,000	85.0%	90.0%	$7.00
$80,000	82.5%	90.0%	$10.50
$90,000	79.2%	88.0%	$12.32
$100,000	75.8%	86.0%	$14.28
$120,000	69.2%	82.0%	$17.92
$140,000	62.5%	78.0%	$31.70
$160,000	55.8%	74.0%	$25.48
$180,000	50.0%	70.0%	$28.00
$200,000	50.0%	66.0%	$22.40
$220,000	50.0%	62.0%	$8.00
$240,000	50.0%	58.0%	$11.12
$260,000	48.9%	54.0%	$7.14
$280,000	42.3%	50.0%	$10.78
$300,000	35.6%	46.0%	$14.56
$350,000	20.0%	36.0%	$22.40
$400,000	0.0%	26.0%	$36.40
$450,000	0.0%	16.0%	$22.40
$500,000	0.0%	6.0%	$8.40

B Disclaimer

The results of these studies are based, in part, on data supplied to the ABS under the Taxation Administration Act 1953, A New Tax System (Australian Business Number) Act 1999, Australian Border Force Act 2015, Social Security (Administration) Act 1999, A New Tax System (Family Assistance) (Administration) Act 1999, Paid Parental Leave Act 2010 and/or the Student Assistance Act 1973. Such data may only used for the purpose of administering the Census and Statistics Act 1905 or performance of functions of the ABS as set out in section 6 of the Australian Bureau of Statistics Act 1975. No individual information collected under the Census and Statistics Act 1905 is provided back to custodians for administrative or regulatory purposes. Any discussion of data limitations or weaknesses is in the context of using the data for statistical purposes and is not related to the ability of the data to support the Australian Taxation Office, Australian Business Register, Department of Social Services and/or Department of Home Affairs' core operational requirements.

Legislative requirements to ensure privacy and secrecy of these data have been followed. For access to PLIDA and/or BLADE data under Section 16A of the ABS Act 1975 or enabled by section 15 of the Census and Statistics (Information Release and Access) Determination 2018, source data are de-identified and so data about specific individuals has not been viewed in conducting this analysis. In accordance with the Census and Statistics Act 1905, results have been treated where necessary to ensure that they are not likely to enable identification of a particular person or organisation.

PUBLICATIONS

Labor Supply Effects of Monetary Policy: Evidence from Australian Mortgage Holders

Working Paper No. WP/2026/071