Do Housing Investors Pass-through Changes in Their Interest Costs to Rents?

Declan Twohig, Jonathan Hambur, and Anirudh Yadav

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Author: hamburj@rba.gov.au

Media Office: rbainfo@rba.gov.au

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Abstract

Interest rates and rents often move together. Some have argued that this positive relationship is evidence that higher interest rates have been a key driver of increases in rents over the past few years, due to leveraged housing investors passing through increases in their interest costs to their tenants. This article uses anonymised tax return data to estimate the direct pass-through of interest cost changes to housing investors' rental income. It finds small pass-through on average, even when interest rates are rising. The largest estimate suggests that direct pass-through results in rents increasing by $25 per month when interest payments increase by $850 per month (the median monthly increase for leveraged investors between April 2022 and January 2024). While passthrough is slightly stronger when vacancies rates are low, it remains small. Our findings with more aggregated data are similair. Higher rates can lead to higher advertised rents, but only after around two or three years, suggesting an affect via dwelling investment and housing supply, not passthrough of rates to rents.

JEL Classification Numbers: E3; E5; R3

Keywords: Rents; interest rates; inflation

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1. Introduction

Understanding the effect of interest rates on rents is important for the RBA. Rent is the second largest component of the Consumer Price Index (CPI), and so how rents respond when interest rates change will have a large mechanical bearing on the overall inflation response. Around one-third of Australian households rent their home. In 2022, the median renter spent 25 per cent of their disposable income on rent, with low-income households tending to have the highest rent-to-income ratios (Agarwal, Gao and Garner 2023). As such, changes in rents have significant implications for households' spending power and financial wellbeing.

A view that is often put forward is that higher interest rates push up rents in the short term by raising costs for indebted housing investors, which they in turn will pass on to tenants.[1] This is intuitive, and at first glance, appears consistent with the aggregate data – interest rates and growth in rents often move together.[2]

By contrast, standard economic theory suggests that rents reflect the balance of demand for, and supply of, available housing. This standard view is embedded in models of the housing market that the RBA uses, such as the Saunders and Tulip (2019) model. In these models, the balance of demand and supply of housing is typically summarised by the vacancy rate, which also tracks movements in rent growth (Graph 2). In this framework, higher interest rates have little immediate *direct* effect on rents as the overall supply of housing in the economy is essentially fixed in the short run. But higher rates should reduce rents indirectly by lowering incomes and therefore housing demand.[3]

Pinning down the relationship between interest rates and rents is tricky because both will tend to move together with the economic cycle. For example, a strong economy will mean higher income growth, which in turn will mean increased demand for rental properties. This will put upward pressure on rents. At the same time, interest rates may be raised to reduce inflationary pressures. So the observation that rates and rents move together may be a case of correlation, rather than higher rates causing higher rents.

One way researchers have tried to better understand this relationship is to trace out the response of rents to higher interest rates but strip out the effect of the economic cycle on both. In principle, this approach should capture any direct pass-through of higher rates to rents, alongside indirect effects higher rates may have on rents by affecting incomes and, over the medium term, housing construction.[4] Overseas work taking this approach finds mixed results from changes in monetary policy on rent inflation (Liu and Pepper 2023; Albuquerque and Lenney 2023; Dias and Duarte 2019). Similar work in Australia finds higher rates have little effect on rents (Moore 2023).

1 Examples where this view has been expressed are Malo (2023) and Kelly (2023).

2 This view implicitly thinks of interest costs as a cost of providing housing services, rather than as the cost of financing an asset purchase. The idea is similar to the "cost channel" of monetary policy, where higher borrowing rates raise firms' marginal costs (e.g. of inventories), which are then passed on to consumers (IMF 2003, Gaiotti and Secchi 2006).

3 In the medium term, lower dwelling investment may offset some of this decline.

4 A drawback of this approach is that it can be sensitive to the exact approach used. It can also be hard to test for asymmetries, such as whether the effects differ when rates are rising or falling, due to short sample periods.

This article focuses on a different approach to specifically study the direct pass-through of interest costs to rents in the short term. To do so, we use detailed anonymised tax microdata. These data are well suited to studying this question because we can compare rental outcomes for investors who have different levels of debt, while controlling for economic conditions that might influence rents and interest payments for all investors. The downside is that our approach implicitly assumes that there is limited spillover from highly indebted investors' rents to other less-indebted investors' rents. We think this is a reasonable assumption, particularly given the fragmented nature of ownership of the Australian rental market, but if such spillovers do exist, we may understate the pass-through of rates to rents.

Taking this approach, overall, we find that there is some pass-through of higher interest rate payments for landlords to rents, but it is very small. Our largest estimates suggest that a $1 increase in interest payments is associated with a 3 cent increase in rents. Putting this in the context of the post-COVID interest rate hiking cycle, the median monthly increase in interest payments for leveraged investors between April 2022 and January 2024 was $850. This would translate to a $25 per month increase in rents, or around $7 per week.

We consider several extensions. First, we test whether pass-through is stronger when rates are rising, as potentially landlords only pass-through increases. While we find stronger pass-through, it remains small. Second, we consider whether pass-through is stronger in tight housing markets, as landlords may have more power to pass through interest rate increases. Again, we find some support for this mechanism, with passthrough tending to increase as vacancy rates fall. But the magnitude remains small. Overall, we find little evidence to support the notion that higher rates are passed through to materially higher rents by landlords.

We also supplement these results using the standard shock-based approach used in the literature. We find no evidence that contractionary policy shocks lead to higher rents in the short run. There is some evidence that they increase advertised, but not CPI, rents after around 2-3 years. However, this timing is more consistent with contractionary policy influencing dwelling investment, and therefore housing supply in the medium term, as captured in models such as Saunders and Tulip (2019).

2. Literature review

There is a sizable literature empirically examine the relationship between rates and rents. As noted above, much of this uses measures of exogenous monetary policy shocks and traces out the effects on rents.

As noted, this literature has mixed results on the effect of rates on rents. Liu and Pepper (2023) find that contractionary monetary policy lowers rents in the US using a standard local projections approach and the shock measure of Bauer and Swanson (2022), while Duarte and Dias (2019) find the opposite using an instrumental variable structural vector autoregression and a similair but slightly different shock measure. Albuquerque and Lenney (2023) use local projections and find that higher rates lead to higher rents in the short-run for the UK as a whole, but also for most regions of the UK. Regarding Australian studies, Moore (2023) finds that rising rates have not had a significant effect on rents as measured in the CPI.

While this is the most common approach, some other work has tried to get at this question empirically in other ways. Hughes (2024) uses US linked property-level asking rents and mortgage data, and explores what happens to rents around refinancings, exploiting some quais-exogenous variation. He finds that when landlords refinance their interest payments tend to fall, and they respond by raising asking rent. This is inconsistent with the pass-through theory, and the author suggests that it relates to landlord taking the opportunity of the refinancing to reassess the optimal rental price.

3. Macroeconomic regressions

We start by re-visiting the standard monetary policy shock-based approach used in much of the literature. This allows us to assess the macroeconomic effect of policy. But as discussed above, it is heavily reliant on the shock measure being exogenous.

3.1 Method and data

We employ a local projections approach (Jordà 2005) of the following form to trace out the effect of monetary policy shocks ($shock_t$) over a number of horizons (h = 0,...,12).

$$R_{i,t+h} - R_{i,t} = \alpha_h + \beta_h * shock_t + \sum_{j=1}^{4} \boldsymbol{\gamma}_{h,j} \boldsymbol{X}_{t-j,i} + \varepsilon_{i,t+h}$$

We consider two different approaches. First, we examine aggregate rent data. Specifically, we use CPI rents and quarter-average advertised hedonic rents from Cotality. In this case we allow for heteroskedasticity in the standard errors. We also consider a version where we do the regressions at a local (SA4) level, looking at quarter-average rents in each area i. Here we cluster the errors for each period, allowing cross-sectional correlation across areas. This is important given the variable of interest is the same across all households for a given period (Cameron and Miller 2015). We do not allow for serial correlation as this is addressed by the lagged variables.

The vector $\mathbf{X}_{t-j}$ is a set of control variables, including lags of GDP and CPI, the shock itself, rents and, in the local level regressions, the vacancy rate.[5] As the shocks are exogenous, controls are not strictly needed, but they help improve the precision of our estimates, particularly the firm-level controls.

Our measure of monetary policy shocks is the measure constructed in Beckers (2020), which covers the sample period up to December quarter 2019. This is a Romer and Romer (2004) style shock that measures the shock as a deviation from a Taylor Rule, augmented with measures of financial conditions and financial market participants' expectations.

In particular, Beckers (2020) estimates an augmented Taylor rule that includes forecasts for economic conditions, as well as a number of indicators of financial conditions (e.g. bond spreads, option-implied volatility). The shocks are then constructed as the deviation of the actual policy rate from that implied by the rule. He produces two main measures: a preferred measure that also

5 The results are generally robust to including more lags of the right-hand side variables, as well as including the contemporaneous controls which imposes the implicit assumption that monetary policy cannot affect current conditions (Ramey 2016).

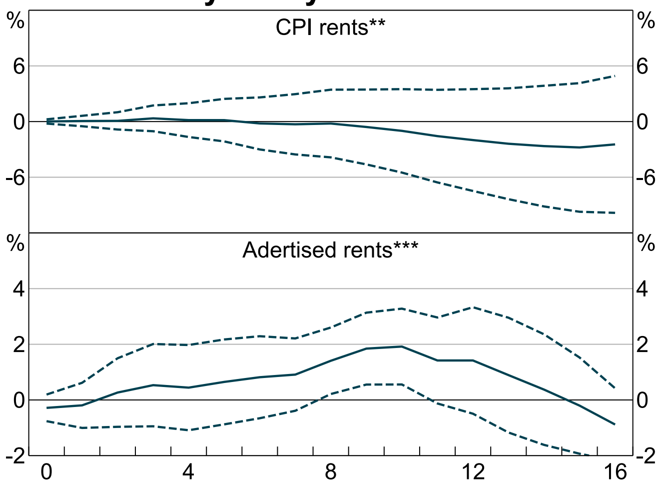
accounts for market expectations for the policy rate, and another version that does not. We use the preferred measure for our analysis.

We choose the Beckers measure as our preferred measure as it has been shown to resolve the so-called price puzzle in Australian data: that contractionary monetary policy is often estimated to lead to higher prices. We allow the shock to enter the model directly, as in other papers like Durante *et al* (2022), rather than using it as an instrument for changes in the cash rate. As such, we are implicitly taking the measure to be a true estimate of the shock, rather than a noisy estimate.

3.2 Results for average effect

Figure 1 shows the results of the regression with national data. First, focusing on CPI rents we find no evidence that increases in interest rates affect rents, consistent with Moore (2023). Turning to advertised rents, which represent the flow of new rent prices, rather than the stock as with CPI, there is some evidence that rents increase following a contractionary monetary policy shock. The peak increase is around 2 per cent. However, the response is quite delayed, only occurring after around 10 quarter, or 2 ½ years.

Figure 1
Effect of 100 Basis Point Monetary Policy Shock on Rents*



* Solid line is point estimate. Dotted line is 95 per cent confidence interval.
** Sample 1995 to 2019. Approximately 90 observations
*** Quarter-ended rents. Sample 2006 to 2019. Approximately 50 observations
Sources: ABS; Beckers (2020); Cotality.

Given changes in policy rates are likely to pass on to borrowers quite quickly, reflecting the variable rate nature of borrowing in Australia, it seems unlikely that these results reflect direct pass-through of higher interest costs into rent. In fact, given we are looking at the effect of a one-off shock to rates, interest rates will have returned back to their near initial level after 10 quarters. Rather, the increase in rents is more likely to reflect the fact that contractionary monetary policy can lower dwelling investment, and therefore the dwelling stock and supply in the medium term. This lower supply can push up rents. This mechanism is captured by models such as Saunders and Tulip (2019).

One thing to note is that the CPI data are available for a longer sample than the advertised rent data. If we instead estimate the CPI rents regression over a shorter sample we do find some evidence that contractionary policy leads to higher CPI rents (Figure A1). However, this again occurs after around 3 years, suggesting that the finding reflects more standard dwelling supply channels, rather than rates passing directly through to rents.

Finally, we also explore the results using a regression with SA4 level data. Doing so provides a very similair pattern to the national data, though the effect of policy on rents is not significant (Figure A2).

3.3 Results by market tightness

While there is little evidence of direct pass-through of rates to rents in aggregate, one natural question is whether this is also the case for regions where rental markets are tight? It may be that when markets are tight, landlords have more pricing power to passthrough higher interest rates.

To explore this with the macro data we run an extension of the above model:

$$R_{i,t+h} - R_{i,t} = \alpha_h + \beta_h * shock_t + \rho_h * shock_t * vacancy\ rate_{i,t} + \sum_{j=1}^{4} \boldsymbol{\gamma}_{h,j} \boldsymbol{X}_{t-j,i} + \varepsilon_{i,t+h}$$

For this model we are allowing the effect of the monetary policy shock to differ, based on how low the vacancy rate was in the SA4 was at the time of the shock. If coefficient $\rho_h < 0$ then the shock has a larger effect when markets are tighter (vacancy rates are lower).[6]

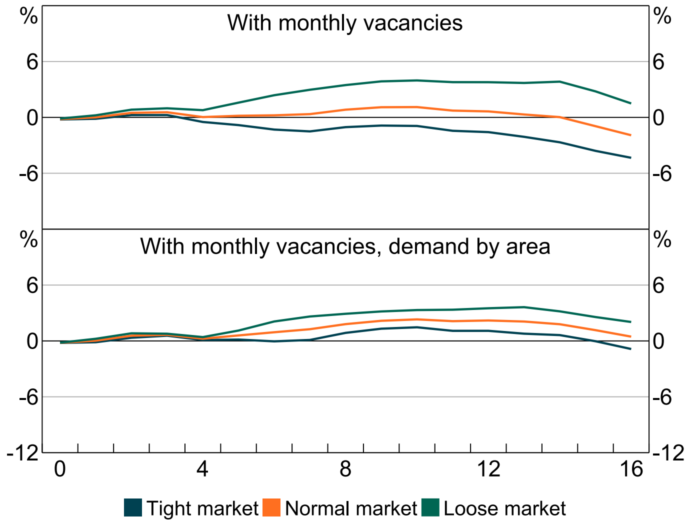
We consider two different ways of looking at the vacancy rate. The first is to let the vacancy rate enter directly. This will identify effects both off changes in market tightness within areas, but also differences in tightness across markets. The second is to demean vacancy rates for each SA4. This focuses of variation within markets, and abstracts from differences across markets. This would be preferred if there are potentially some omitted variables that both influences how monetary policy affects rents, and leads to systematically higher or lower vacancy rates. One example might be how elastic housing supply is in the area.

Figure 2 plots the point estimate of the effect of monetary policy on rents across a tight, normal, and loose market, where we define these defined as the 25th percentile (around 1.5 per cent), median (around 2.25 per cent), and 75th percentile of the vacancy distribution (around 3.5 per cent), respectively. We can see that if anything, the evidence suggests that the effects on rents are smaller in tighter markets. That said, when focusing on our preferred demeaned specification the differences are not statistically significant, suggesting that the differential effect reflects some other, location-specific factors. In either case, these differences again are only evident after around 2 years, which is later than might be expected if they reflected direct passthrough of rates to rents, particularly given we are focusing on the flow of new advertised rents, not the stock of rents.

6 We also include the vacancy rate directly I the control vector $\boldsymbol{X}_{t-j,i}$

Overall, putting these results together there is little evidence in the macro data to support the notion that higher rates are directly passed on to higher rents. However, as noted there are significant limitations with this approach. As such we now investigate the issues using administrative microdata.

Figure 2
Effect of 100 Basis Point Monetary Policy Shock on Rents*
By market tightness



* Point estiamtes only. Tight market is 25th percentile of vacnacy rates. Normal is median. Loose is 75th percentile. Monthly vacancy rates. SA4 level regression. Sample 2006 to 2019.

Sources: ABS; Beckers (2020); Cotality.

4. Property-level regressions

Given the potential issues with the above approach we now attempt to isolate direct pass through by using person level administrative data and fixed effects to strip out macroeconomic and local factors which may affect the relationship between rising interest costs and rental inflation. Ultimately, our approach tried to isolate the effect by looking at the differential responses of more and less levered landlords.

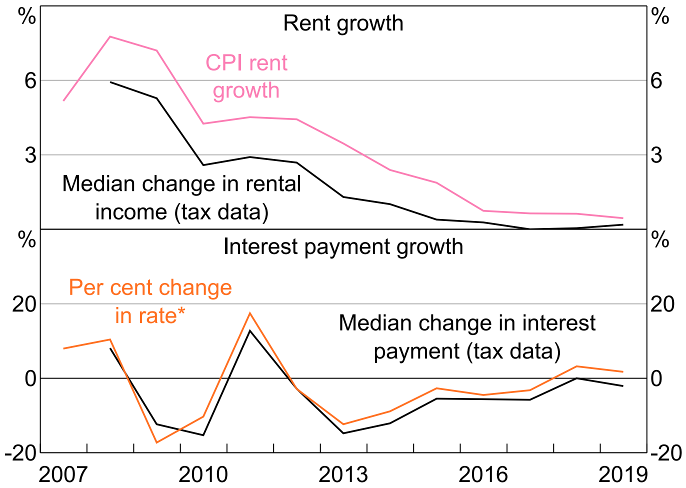
4.1 Data

Our dataset covers every investor that filed a personal income tax return in Australia from 2006/07 to 2018/19. The data are annual. We observe an investor's rental income and their mortgage interest deductions, along with their location, total income, age and other demographic characteristics.

The rental incomes and interest payments coming out of the dataset follow sensible patterns. Median growth in rental income in our dataset closely tracks the trend in CPI rent inflation

(Figure 3). And median growth in interest payments closely tracks percentage changes in the indicator lending rate for investors.[7]

Figure 3
Growth in Rents and Interest Payments
By financial year

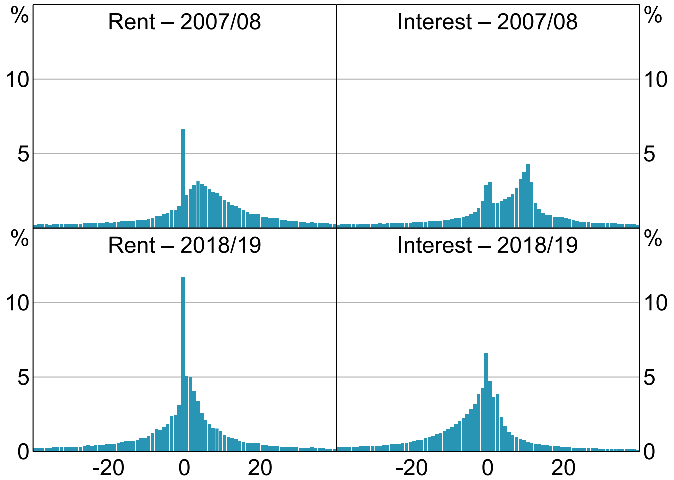


* Per cent change in indicator lending rate for investors.
Sources: ABS; RBA.

Nevertheless, there is a huge amount of variation in both annual rental income growth and changes in interest payments at the individual level (Graph 4). Much of this variation likely reflects investor-specific factors. For example, if an investor sells their property halfway through the year and pays off their mortgage, both rent and interest costs will halve, even though interest rates may not have changed. These kind of housing transactions will introduce a spurious positive correlation between investors' interest costs and rental income: it looks like interest costs and rents move together, but this is not because of pass-through of interest costs to rents. We discuss our approach to dealing with this below.

7 We use the standard variable rate for investors from Statistical Table F5, splice it backwards using the standard variable owner-occupier rate, and then compute the average rate for each financial year. The resulting series closely tracks movements in the cash rate.

Figure 4
Distribution of Annual Changes in Rental Income and Interest Payments*
Selected financial years



* Share of leveraged investors.
Sources: ABS; RBA.

4.2 Method

To test whether investors pass-through changes in their interest costs to their tenants, we compare changes in rental incomes for investors with different levels of debt. Ideally, we would be able to observe rental growth for two identical properties with different levels of associated debt. Then when interest rates changed, we could compare rental growth for the more indebted property to the less indebted one and confidently learn something about pass-through. For example, suppose there are two fictional investors, A and B, that own identical investment properties next door to each other – the only difference being that A has a large mortgage on their investment property and B owns the property outright. If A increases their rent by more than B when interest rates increase, we would conclude that the difference reflects the pass-through of A's higher interest costs to the rent that they charge. However, if A and B's rental incomes grow similarly after a change in interest rates, then we would conclude that there is limited pass-through.

In reality, we do not observe identical properties with different levels of debt. Instead, we approximate this by comparing rental income growth for investors in the same local area, and investigate whether it varies across investors depending on the change in their interest costs. In doing so, we control for other factors that may drive both rates and rents for all investors, such as local economic conditions.

Our regression approach exploits variation in the indebtedness of investors within the same local area to estimate the pass-through of interest costs to rents. We start with a hypothetical rental pricing model and build up to our regression specification. Suppose a leveraged investor *i* sets their rent in year *t* according to:

$$Rent_{it} = p_{it} + \beta Interest_{it}$$

Here, *p* is the (unobserved) 'competitive' annual rental price for *I*s property, *Interest* is their annual mortgage interest payment, and β is the pass-through parameter that we want to estimate. The per cent change in *I*s rent from year *t-1* to *t* is then:

$$\Delta\%Rent_{it} = \frac{\Delta p_{it}}{Rent_{it-1}} + \beta\frac{\Delta Interest_{it}}{Rent_{it-1}}$$

Two identical properties should have the same value for the first term on the right-hand-side, which is close to the per cent change in the unobserved competitive rental price. Since we can't observe identical properties, our approach is to soak-up this term using location-by-time fixed effects. In other words, we assume all investors in the same local housing market (SA4) experience the same per cent change in the competitive rental price for their property each year. This assumption allows us to learn about β by comparing rental growth for investors in the same SA4 but who experience different changes in their interest costs. More indebted investors should experience larger changes in their interest costs when interest rates change. Putting this all together, we arrive at our regression:

$$\Delta\%Rent_{it} = \alpha_{SA4,t} + \beta\frac{\Delta Interest_{it}}{Rent_{it-1}} + \Gamma'X_{it} + e_{it}$$

To account for other differences in the properties and landlords, we include two types of additional controls in *X*. First, we include a control for lagged quintile of *Interest*/*Rent*. This is to account for the fact that investors with higher debt (as measured by their reported interest costs) tend to have systematically higher rental income growth over our sample period, potentially reflecting the different nature of the properties they hold (Figure A3).[8] All regressions also include age-group and income quintile dummies. These auxiliary controls are not needed for identifying *β*, but may help with precision by absorbing residual variation in rental income growth.

As mentioned above, transactions and similair large changes in rates and rents could bias our estimates of passthrough. We explore two approaches to accounting for this issues. First, we remove observations with very large changes in rental income and, for levered investors, we remove observations where the change in interest payment does not broadly line up with the change in the indicator lending rate.

We try three different levels of trimming: narrow, medium and wide (Table 1). The 'narrow window' excludes observations if annual rental income growth is above 30 per cent or below −10 per cent, or if interest payment growth lies outside a ± 5 percentage point range around the per cent change in the indicator lending rate over the corresponding financial year. Figure 5 gives a visual representation of how this range for interest payment growth works. This window should remove most observations where there is a transaction, but may exclude investors who are well into their mortgage term. These more seasoned mortgagors tend to have rapidly declining mortgage principals, meaning that interest payments will decline quickly. The 'medium window' tries to capture more of these seasoned mortgagors by lowering the bottom threshold for interest payment growth. Finally, the 'wide window', includes observations with rental income growth and

8 Excluding this control does not substantially change the results.

interest payment growth below 50 per cent in absolute terms. The wide window undoubtedly includes many property purchases/sales, so we use it mostly for illustrative purposes when looking at the estimates.

Table 1: Data Trimming

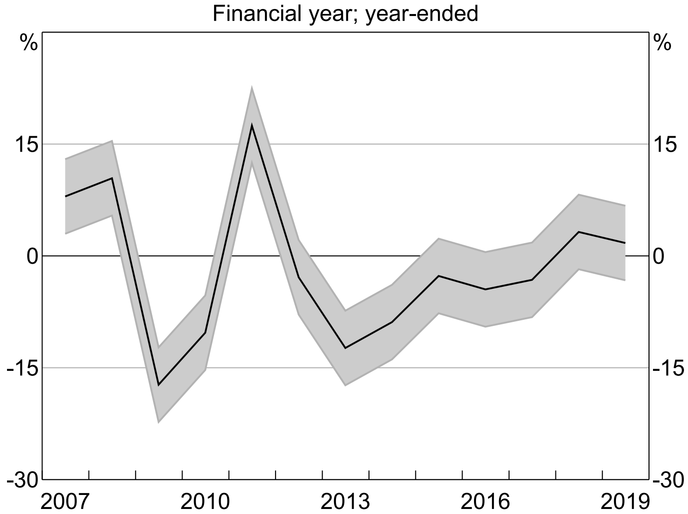
	Range for annual rental income growth (per cent)	Range for annual interest payment growth around the per cent change in the indicator lending rate[(a)] (ppt)
Narrow window	-10 to 30	-5 t0 5
Medium window	-10 to 30	-15 to 5
Wide window	-50 to 50	-50 to 50

Note: (a) We include non-mortgagors in our main regressions even if they fall outside these windows. But our results are robust to excluding non-mortgagors.

Figure 5

Narrow Window Around Changes in the Indicator Lending Rate

Financial year; year-ended



* Grey shaded area shows a +/-5 percentage point range.

Source: RBA.

Second, we also try an instrumental variables (IV) approach, which tries to isolate changes in interest payments that are due solely to changes in aggregate lending rates, and not due to mortgage transactions or other factors. To construct the instrument, we first impute an investor's level of debt in year $t-1$ by dividing their reported interest payment in that year by the indicator lending rate. Our instrument then multiplies this lagged imputed debt level by the observed change in the indicator lending rate over year $t-1$ to t:

$$Z_{it} = \frac{\widetilde{D}_{it-1}}{Rent_{it-1}} \times \Delta r_t, \quad \text{where } \widetilde{D}_{it-1} = \frac{Interest_{it-1}}{r_{t-1}}$$

Here Z is our instrument, r is the indicator lending rate, and $\widetilde{D}_{it-1}$ is the investor's imputed level of debt.

4.3 Baseline results

Table 2 outlines our baseline results. Focusing on OLS estimates first, using the wide window suggests a strong positive relationship between changes in interest costs and changes in rental incomes. However, once we remove the extreme changes, which likely reflect purchases and sales of properties, these effects go away. Using the narrow and medium windows this goes away we find evidence of statistically significant, but economically insignificant pass-through: a one dollar increase in mortgage interest payments leads to a 1 cent increase in rents for the year.[9]

The results re similar for the IV models. Here, even using the wide-window but removing these large idiosyncratic changes via the instrument we find no economically significant pass-through. As shown in the Appendix, our instrument is a very strong, suggesting we do no need to be worried that these results are driven by weak instrumentation (Table A1).

Table 2: Effect of Change in Interest Payment on Change in Rental Income
OLS and IV models

	Narrow window	Medium window	Wide window
OLS: $\Delta Interest/Rent_{t-1}$	0.009**	0.010***	0.316***
(s.e.)	(0.004)	(0.002)	(0.027)
IV: $\Delta Interest/Rent_{t-1}$	0.005	0.006	0.019*
(s.e.)	(0.006)	(0.004)	(0.010)
Observations	2,795,198	3,683,960	6,785,979

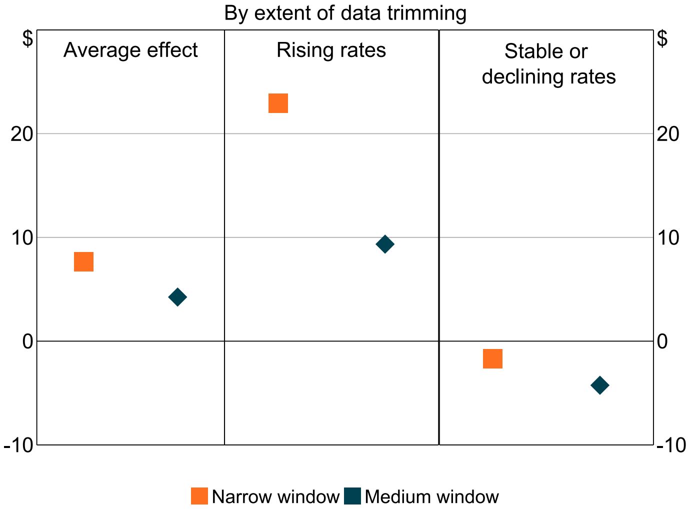
Note: This table reports OLS and IV estimates of the effect on changes in interest payments on changes in rental income for different levels of data trimming as defined in Table 1. All regressions include non-mortgagors. They also include control variables for the individual and year-*SA*4 fixed effects as discussed in text.. Year-clustered standard errors are in parentheses. ***, **, and * statistical significance at the 1, 5 and 10 per cent levels, respectively.

To put this effect in context, the median monthly interest payment for leveraged investors increased by around $850 between April 2022 and January 2024.[10] Our estimate suggests that this $850 increase in interest costs would have raised rents by less than $10 per month, or just over $2 per week (Graph 5, left panel). This increase in rent equates to around 0.4 per cent of the median monthly rent in January 2024.

9 Including a lag and so allowing for some time to passthrough rate changes do not change the result. The coefficient on the lag is very low.

10 This statistic is from the RBA's Securitisation Dataset. For detail on this dataset, see Hughes (2024) and Fernandes and Jones (2018).

Figure 6
Effect of an $850 Increase in Monthly Interest Costs on Monthly Rent*
By extent of data trimming



* These figures multiply our point estimates in Table A.1 by 850. The 'narrow window' excludes observations if annual rental income growth is above 30 per cent or below 10 per cent, or if interest payment growth lies outside a +/- 5 percentage point range around the per cent change in the indicator lending rate over the corresponding financial year. The medium window uses a lower bottom threshold for interest payment growth. See Table A.1 for details. In our sample period, interest rates for investors were rising in 2008/09, 2011/12, 2017/18 and 2018/19. Interest rates were not rising in 2009/10 and 2012/13-2016/17.

Sources: ABS; RBA.

4.4 Asymmetry for rate rises

One concern with the above results might be that passthrough is asymmetric. It might be that landlords pass through increases in their interest payments to renters, but not decreases. To examine this, we allow the coefficient on interest rates to differ based on whether interest rates were increasing in that year or not. In our sample the indicator lending rate rose in 2008/09 and 2011/12, and had small increases in 2017/18 and 2018/19 (mainly driven by lending spreads). We abstract from the wide window given the above identified issues.

We can see that there is some evidence of asymmetry: changes in rates pass through more strongly onto rents in years when rates are rising. But the magnitude is still small, at around 3 cents for every dollar. Taking the same calculation as above, this would have contributed a bit over $20 dollars per month, or $5 per week, to rents over the recent hiking cycle. The results are similair using IV approach (Table A1).

Table 3: Effect of Change in Interest Payment on Change in Rental Income
Asymmetry model, OLS

	Narrow window	Medium window
$\Delta Interest/Rent_{t-1}$	-0.002	0.005*
(s.e.)	(0.003)	(0.003)
$\Delta Interest/Rent_{t-1} \times \mathbf{1}[\Delta r_t > 0]$	0.029**	0.016*
(s.e.)	(0.011)	(0.008)
Observations	2,795,198	3,683,960

Note: This table reports OLS estimates of the effect on changes in interest payments on changes in rental income for different levels of data trimming as defined in Table 1. It allows the coefficient to differ in years where rates are rising All regressions include non-mortgagors. They also include control variables for the individual and year-*SA*4 fixed effects as discussed in text.. Year-clustered standard errors are in parentheses. ***, **, and * statistical significance at the 1, 5 and 10 per cent levels, respectively.

4.5 Passthrough by vacancy rate

One final question is, do these results differ significantly in a tight rental market? This might be the case if landlords have more market power to pass on higher rates when the rental market is very tight.

This is an important question, as it could lead to time-variation in the passthrough of rates. So while we find little evidence of passthrough in our sample, it might be that this dynamic would only become important when rental markets are very tight, as was the case over the early 2020s.

To consider this, we incorporate year-average vacancy rates at the SA4 level into our model. We first allow these to enter the model directly, in place of the SA4-by-time fixed effects, to test whether they have the expected effect on rents. We then interact them with the pass-through coefficient to test whether passthrough is higher where vacancy rates are lower. Specifically, we run the following regressions:

$$\Delta\%Rent_{iyt} = \alpha_{SA4,t} + \beta\frac{\Delta Interest_{it}}{Rent_{it-1}} + \gamma\frac{\Delta Interest_{it}}{Rent_{it-1}} * vacancy_{yt} + \Gamma'\mathrm{X}_{it} + e_{it}$$

If passthrough is stronger went rental markets are tight, γ should be negative. We allow vacancies to enter linearly, but also try other parametric assumptions and non-parametric approaches.

Table A2 summarises the results. We focus on the medium window and note that the results are of a similair sign, but less significant using the narrow window. First, from column 1 we can see that there is a strong negative relationship between vacancy rates and rental growth. This is consistent with theory, and indicates that vacancy rates are effectively capturing market tightness.

Moving to the models where we interact passthrough with vacancy rates, we do see a negative coefficient on the interaction term – so passthrough tends to be stronger when vacancy rates are lower. This is the case where we take vacancy rates directly (column 2), and where we demean each local areas vacancy rate and so just focus on differences in market tightness within each area over time (column 3). However, the differences remain quite small. If we compare passthrough for a tight market (25th percentile vacancy rate of 1.75) and a soft market (75th percentile of 3.75) passthrough to rents is estimated to be 1 cent per dollar of interest, or 0.2 cents per dollar, respectively. Even focusing at very tight markets with near zero vacancy rates leads to estimates of passthrough a little under 2 cents, so not significantly large than our earlier findings.

Table 4: Effect of Change in Interest Payment on Change in Rental Income
Vacancy models, medium window

	Direct vacancy model	Vacancy passthrough model	Demeaned vacancy passthrough model
$\Delta Interest/Rent_{t-1}$	0.008**	0.017***	0.009***
(s.e.)	(0.003)	(0.004)	(0.002)
$\Delta Interest/Rent_{t-1} \times vacancy_{yt}$		-0.004***	-0.005***
(s.e.)		(0.001)	(0.001)
$vacancy_{yt}$	-0.001***		
(s.e.)	(0.000)		
Observations	3,681,106	3,681,106	3,681,106

Note: This table reports OLS estimates of the effect on changes in interest payments on changes in rental income for different levels of data trimming as defined in Table 1. It allows the coefficient to differ in years where rates are rising. All regressions include non-mortgagors. They also include control variables for the individual and year-*SA*4 fixed effects as discussed in text, apart from column 1 which excludes SA4-time effects. ***, **, and * statistical significance at the 1, 5 and 10 per cent levels, respectively.

One concern may be that the effects of market tightness are understated because we assume a linear relationship between vacancies and passthrough. It might be that passthrough only increases when markets are very tight. To test this we run a series of robustness test allowing for non-linear effects, including by allowing by interacting passthrough with a linear trend or dummy for vacancy rates below the median or 5th and 1st percentile (2, 1 and 0.5 per cent respectively). We also try parametric non-linear models including a quadratic, cubic and inverse function. None provides evidence of substantially more variation in passthrough. Similarly we explore whether variation is larger when rates are rising, and again find no real evidence.

Overall, it does appear that passthrough of rates to rents is stronger when markets are tight. However, the evidence suggests that even when rental markets are very tight passthrough remains very low. As such, direct passthrough of higher rates into rates appears unlikely to be economically significant.

5. Limitations and extensions

There are a few limitations of our approach that are important to acknowledge. As described above, our regression tries to infer the extent of pass-through by looking at whether, following a change in interest rates, investors with higher debt change their rent by more than investors with less debt. In doing so, we are effectively ruling out the possibility of 'spillovers' between the rent-setting decisions of highly indebted investors and the rent-setting decisions of less-indebted investors. If investors with big mortgages increase their rents due to an increase in interest costs, and less-indebted investors observe this and follow suit, then our approach would incorrectly infer limited pass-through of interest costs to rents. Given the nature of Australian housing markets, with lots of individual landlords all competing for renters, this 'no spillovers' assumption may be reasonable. But others may believe it is a strong assumption.

While difficult to verify, future data sources could potentially allow us to test aspects of this issue. For example, one mechanism through which this type of activity could occur is via property managers who may manager various different properties and recommend consistent increases across them. Rental property schedules being integrated into PLIDA as part of the WHAM pilot contain information on property manager expenses, which could be used to infer whether the property is managed (Bradshaw *et al* 2025). This could allow us to explore whether these properties behave differently.

These same data could also be used to more precisely account for the location of the property. Currently for both vacancies and other controls we are using the location of the landlord. But in some cases the landlord may own a property in a different area. This would help refine the results but seem unlikely to change them materially, particularly given testing suggests that the SA4-time controls do not affect the results substantially (suggesting that broader economic trends are more important than local ones). Finally, these data could also be used to better identify sales and purchases, allowing for a more precise approach to trimming. Other data, such as rental bond schedules, could allow us to examine weekly rents, which would also allow us to more precisely account for these dynamics, as well as vacant periods.

6. Conclusion

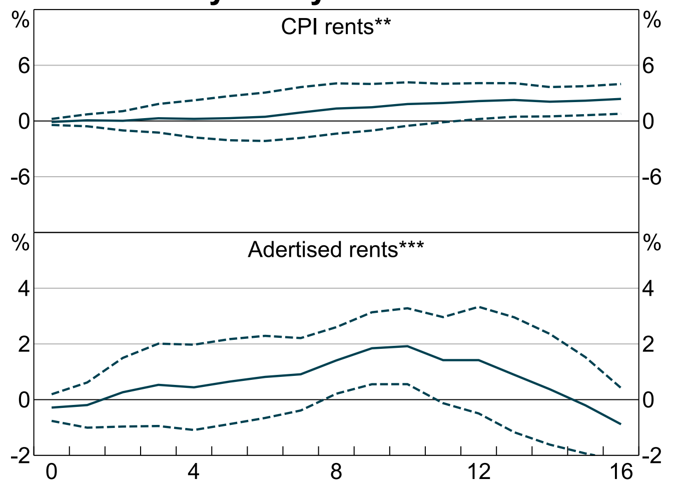
Overall, we find no evidence that investors pass through changes in their interest costs materially into the rents that they charge. This is the case both when rental markets are very tight, and when interest rates are increasing.

These findings are consistent with the standard view that the level of housing demand relative to the stock of properties available is the key driver of rents (Hunter 2024; Saunders sand Tulip 2019). This has important implications for the conduct of monetary policy, as it suggests that higher rates will not translate into higher rents in the short-term, though they may increase rents in the medium-term by influencing dwelling investment. These findings also have implication for the way practitioners build models of the Australian macroeconomy and housing markets, as they suggest that standard approaches should be sufficient to capture market dynamics.

These results also highlight the power of new integrated administrative datasets to answer key questions around the housing market. Future data integration will allow us to refine this work, as well as open up new and important directions for research.

Appendix A: Additional results

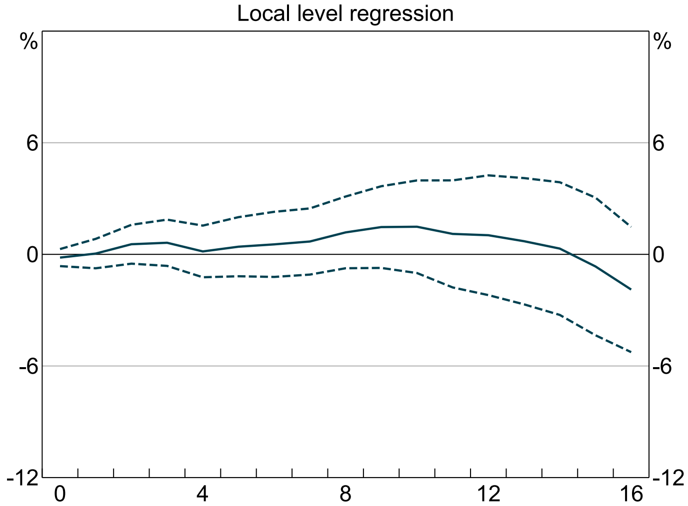
Figure A1
Effect of 100 Basis Point
Monetary Policy Shock on Rents*



* Solid line is point estimate. Dotted line is 95 per cent confidence interval.
** Sample 2006 to 2019. Approximately 50 observations
*** Quarter-ended rents. Sample 2006 to 2019. Approximately 50 observations

Sources: ABS; Beckers (2020); Cotality.

Figure A2
Effect of 100 Basis Point
Monetary Policy Shock on Rents
Local level regression

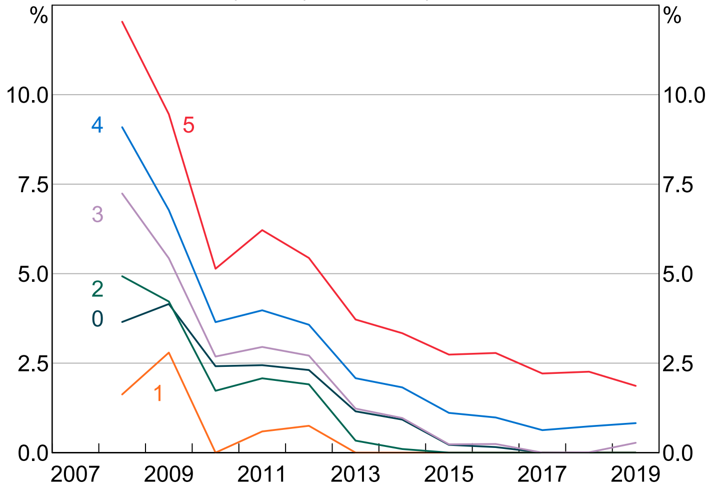


* Solid line is point estiamte. Dotted line is 95 per cent confidence interval. Sample is 2006 to2019

Sources: ABS; Beckers (2020); Cotality.

Figure A3
Median Growth in Rental Income

Financial year; by interest payment quintiles



Sources: ABS; RBA.

Table A1: IV First Stage Regression Estimates

	Narrow window	Medium window	Wide window
First-stage coefficient: $\tilde{D}_{it-1} \times \Delta r_t \,/Rent_{t-1}$	1.009***	0.988***	0.720***
(s.e.)	(0.036)	(0.043)	(0.078)
First-stage *F*-Statistic	796	532	85
Observations	2,795,198	3,683,960	6,785,979

Note: This table reports the first-stage estimates of the effect of our instrument for different levels of data trimming as defined in Table 1. All regressions include non-mortgagors. They also include control variables for individuals and year-*SA4* fixed effects as discussed earlier in the paper. Year-clustered standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5 and 10 per cent levels, respectively.

Table A2: Effect of Change in Interest Payment on Change in Rental Income

Asymmetry model, IV

	Narrow window	Medium window
$\Delta Interest/Rent_{t-1}$	-0.013**	-0.007
(s.e.)	(0.004)	(0.005)
$\Delta Interest/Rent_{t-1} \times \mathbf{1}[\Delta r_t > 0]$	0.046***	0.041**
(s.e.)	(0.012)	(0.014)
Observations	2,795,198	3,683,960

Note: This table reports IV estimates of the effect on changes in interest payments on changes in rental income for different levels of data trimming as defined in Table 1. It allows the coefficient to differ in years where rates are rising All regressions include non-mortgagors. They also include control variables for the individual and year-*SA4* fixed effects as discussed in text.. Year-clustered standard errors are in parentheses. ***, **, and * statistical significance at the 1, 5 and 10 per cent levels, respectively.

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