Monetary Policy and Rents *

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Abstract

This paper studies the effects of monetary policy on housing rents. We provide comprehensive measures of rent inflation at a micro-geographic scale by constructing a new repeat-rent index. Using our rent index, we estimate the impulse responses of rents to monetary policy shocks. We find that, on average, monetary tightening increases rents. The effect is driven by a shift in demand from the owner-occupied market to the rental market. Areas where household borrowing constraints are more binding, where renter and owner markets are more segmented, and where landlords are more levered experience greater rent increases following the same contractionary shock.

JEL-Codes: E31, E52, G51, R21, R28, R31.
Keywords: monetary policy, rents, repeat-rent index.

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1 Introduction

Rent is the single largest component of the Consumer Price Index (CPI) and the single largest expense for renter households in the United States.[1] The effect of monetary policy on inflation and on welfare therefore crucially depends on how monetary policy affects rents. Establishing the effect of monetary policy on rents is far from obvious. On the one hand, renting is a form of consumption, and standard intertemporal substitution considerations suggest that an increase in the interest rate lowers demand for and prices of consumption goods. On the other hand, tighter credit conditions might prevent households from becoming homeowners and shift demand to the rental market (Gete and Reher, 2018; Ringo, 2024), placing upward pressure on rents. Moreover, there are substantial heterogeneities across local housing markets (Piazzesi and Schneider, 2016), which means that the effect of monetary policy on rents could differ across neighborhoods and cities within a country.

This paper studies how monetary policy impacts rents and how the effect differs across local housing markets. We begin by constructing a new repeat-rent index using a national database of rental listings. The key advantage of our repeat-rent index relative to alternative indexes is that it provides the most granular and the most comprehensive geographical coverage of rental markets in the U.S. This granularity and broad geographical coverage are essential for studying the heterogeneous effects of monetary policy on rents. While a small but growing literature studies the relationship between monetary policy and rents (e.g., Dias and Duarte (2019, 2024)), these papers are limited to studying the aggregate effects of monetary policy. As a result, they abstract from the tremendous variation that exists across local housing markets. Equipped with our granular rent inflation measures, we estimate the impulse responses of rents to monetary policy shocks, and how these responses vary across housing markets, by employing standard local projection methods (Jordà, 2005).

We find that, on average, contractionary monetary policy increases both real and nominal rents. A 25 basis point unexpected increase in the 30-year fixed rate mortgage rate raises real (nominal) rents by 0.7 (1) percent 12 months following the monetary policy shock. We attribute the effect to a shift in household demand from the owner-occupied market to the rental market. When monetary policy tightens, the homeownership rate drops and the inventory of renter-occupied housing increases. The increase in both rents and the quantity of housing in the rental market suggests that contractionary monetary policy shocks act as positive rental demand shocks.

[1] See www.bls.gov/cpi and www.bls.gov/cex.

The average effect of monetary policy on rents masks tremendous heterogeneity across local housing markets. We focus on three sources of variation across housing markets. First, we explore how the effect of monetary policy depends on household credit constraints. As highlighted by Greenwald (2018), when borrowers are constrained by debt-to-income (DTI) underwriting limits, an increase in interest rates can prevent them from qualifying for a mortgage. Using data on the universe of mortgage originations in the U.S., we find that monetary policy shocks have a substantially greater impact on rents in markets where DTI constraints are more binding. Second, we explore how the effect of monetary policy on rents depends on landlords' exposure to credit. When the cost of credit increases, financing rental properties becomes more expensive and the rental supply curve can shift upwards. Using transaction level data, we show that contractionary monetary policy shocks have a greater impact on rents when a higher share of landlords rely on credit as a source of financing. Third, we examine how the responsiveness of rents to monetary policy depends on the degree of segmentation between owner-occupied and renter-occupied markets. As highlighted by Greenwald and Guren (2025), the effect of shocks that influence the relative demand for homeownership on rents and house prices depends on the elasticity of the rental supply curve, which in turn depends on how easy it is to convert housing units between owner-occupied and renter-occupied markets. We find that monetary tightening has a larger effect on rents in areas where owner and renter markets are more segmented.

Our results have important policy implications. The finding that contractionary monetary policy, which is the central bank's primary tool for taming inflation, tends to increase rents, which are the single largest component of the CPI, presents a challenge to policymakers. At the same time, the finding that the responsiveness of rents depends on housing market characteristics implies that monetary policy can be more effective in curbing inflation under certain conditions. Namely, when household borrowing constraints are lax enough and when landlords are sufficiently unlevered, contractionary monetary policy can lower rents and hence be more effective in taming inflation. Our results also shed new light on the distributional effects of monetary policy. By making renting more expensive relative to all other goods, monetary tightening can disproportionately harm renter households. Renters in markets where prospective homeowners are more credit constraints, where owner and renter markets are more segmented, and where landlords rely more heavily on credit, are especially exposed to this unintended consequence of monetary tightening.

We begin by developing a new repeat-rent index, which we refer to as the ADH-RRI. The main data source for this apparatus is listing data compiled by Altos Research between 2011 and 2025. Altos compiles a national database of rental listings from online listing platforms and from Multiple Listings Services (MLS). Updated on a weekly basis, the data provides a snapshot of the listings that are observed every week. For each listing, the data records the listed monthly rent, the date in which the listing is observed, the property address, as well as physical characteristics of the listed unit such as the number of beds and baths, the floor size, the year built and the property type. We identify rental units in our data based on their address and physical characteristics.

To construct the repeat-rent index, we adapt the well-known repeat-sales methodology of Bailey, Muth and Nourse (1963) and Case and Shiller (1989) to the rental market. The core idea is to compare rents listed for the same unit over time to construct a quality-constant measure of rent inflation. We construct and analyze several specifications, including a nominal ADH-RRI, which measures inflation of nominal rents, and a real ADH-RRI, which measures how rents grow relative to all other prices in the economy. We construct our ADH-RRI at multiple geographic levels – from the census tract level, through the zipcode level, and up to the national level – and at both monthly and quarterly frequencies. Our rent index is representative of rent inflation in the U.S. At the national level, the ADH-RRI aligns closely with the CPI-NTRR, a national repeat-rent index that is based on nationally representative sample of rental units and that measures inflation of contractual rents (Adams et al., 2024). It also aligns well with other widely used rent indexes such as Zillow's ZORI index (Clark, 2022). The ADH-RRI and these alternative indexes lead the official CPI-Rent index since they reflect rent growth faced by new tenants rather than by both new and existing tenants.

The ADH-RRI has two key advantages relative to alternative rent indexes. First, to the best of our knowledge, it is the most granular high-frequency rent index to date. Alternative indexes are either computed a the national level (CPI-NTRR), the CBSA level (CPI- Rent), or the zipcode level (ZORI). Second, the ADH-RRI provides the most comprehensive geographical coverage of rental markets in the U.S. It covers considerably more zipcodes compared to ZORI. The granularity and comprehensiveness of our inflation measures are key for studying the heterogeneous effects of monetary policy across local housing markets. They enhance our ability to precisely estimate how the responsiveness of rent to monetary policy shocks varies with local housing market characteristics. More broadly, by providing comprehensive high-frequency measures of rent inflation at the neighborhood level, our ADH-RRI is a powerful tool for many other applications. It allows studying rental markets in the U.S. at an unprecedented granularity and breadth.

We use the ADH-RRI as the basis for examining the effects of monetary policy on rents. We estimate the dynamic effects of monetary policy shocks on rents using the standard local projection instrumental variable (LP-IV) framework (Jordà, 2005; Jordà, Schularick and Taylor, 2015; Ramey, 2016; Stock and Watson, 2018). In our baseline specification, we use the 30-year fixed rate mortgage as the (instrumented) monetary policy indicator and we identify exogenous shocks to monetary policy using the Bauer and Swanson (2023*b*) monetary policy surprises series. Bauer and Swanson (2023*b*) identify exogenous shocks to monetary policy by measuring high-frequency interest rate changes around Federal Open Market Committee (FOMC) meetings and around press conferences, speeches, and testimonies made by the Federal Reserve chair, and then orthogonalizing these monetary policy surprises against economic and financial variables that predate the meetings and announcements. By doing so, they capture the component of changes to interest rates around monetary policy events that is ex-ante unpredictable. We verify that these monetary surprises satisfy the validity conditions of Stock and Watson (2018) as an instrument for the 30-year mortgage rate.

We find that, on average, contractionary (expansionary) monetary policy shocks increase (decrease) both real and nominal rents. The results are robust to a host of alternative specifications. First, because the choice of monetary policy shock can influence estimated pass-through effects (Ramey, 2016), we replicate our analysis using several alternative shock series, including those of Gürkaynak, Sack and Swanson (2005), Nakamura and Steinsson (2018), and Swanson (2021), and obtain virtually identical results. Second, our findings remain unchanged when using different monetary policy indicators such as the effective federal funds rate or the 2-year treasury yield. Third, our results continue to hold when instead of using our ADH-RRI, we use alternative rent indexes such as the CPI-NTRR, ZORI, as well as a hedonic rent index that we construct from our listings data. The effect of monetary policy on the CPI-rent index, which tracks rent inflation faced by both new and existing tenants, lags relative to the effect on other indexes.

We attribute the effect of monetary on rents to a shift in household demand from the owner-occupied market to the rental market. Using national level data on house prices, homeownership, and the inventory of renter-occupied housing units, we show that contractionary monetary policy lowers the homeownership rate and house prices, and increases the inventory of renter-occupied dwellings. A 25 bps increase in the 30-year fixed rate mortgage rate leads to a 0.4 percent drop in the homeownership rate, a 1.3 percent drop in house prices, and a 0.7 percent increase in the number of renter-occupied housing units 12 month following the shock. Taken together, the increase in both rents and the quantity of renter-occupied housing suggest that contractionary monetary policy shocks act as a positive rental demand shock. When credit becomes more expensive, household demand shifts from the owner-occupier market to the rental market, which lowers the homeownership rate and house prices. The increase in the rental inventory suggests that rental supply adjusts to accommodate the increased demand for rental housing.

A key contribution of this paper is to study the differential effects of monetary policy across local housing markets. We focus on three dimensions of geographical heterogeneity. First, we explore the role of debt-to-income (DTI) constraints. When borrowers are subject to DTI underwriting limits, higher interest rates can prevent prospective homeowners from qualifying for mortgages (Greenwald, 2018), and as a result induce an outward shift in the rental demand curve. To examine the role of DTI constraints for the transmission of monetary policy, we exploit the fact that borrowers in the U.S. are subject to institutional DTI underwriting limits (Greenwald, 2018; DeFusco and Mondragon, 2020; Bosshardt et al., 2024). Using the Home Mortgage Disclosure Act (HMDA) data, we document substantial cross-sectional and temporal variation in the share of borrowers whose DTI is just below the institutional limit. We use this share as our proxy for the extent to which DTI constraints bind and estimate how the impulse response of rents to monetary policy shocks vary across time and space as a function of this local market characteristic. We find that the same contractionary shock leads to a 0.6 percent higher rent inflation in markets (times) where (when) the share of DTI-constrained borrowers is one standard deviation higher. While the extent to which DTI constraints bind might correlate with other drivers of the effect of monetary policy on rents, we offer various arguments that support our interpretation that DTI constraints are an important driver of the differential effect across housing markets.

Second, contractionary monetary policy shocks can induce not only a shift in the rental demand curve, but also a shift in the rental supply curve. When the cost of credit increases, financing rental properties becomes more expensive and the rental supply curve can shift upwards. We show that the effect of monetary policy on rents is greater in markets where landlords tend to take on more leverage. Using data on the universe of housing transactions in the U.S., we measure the share of buy-to-rent acquisitions where the investor is a cash-buyer. We use this share as our main proxy for landlords credit exposure, and estimate how the impulse response of rents to monetary policy shocks vary with it. We find that the effect of monetary policy on rents is greater in markets where more landlords rely on credit. The same contractionary shock leads to a 0.45 percent higher rent inflation 12 months following the shock when the share of buy-to-rent investors who are cash buyers is one standard deviation lower. We obtain similar results when using alternative proxies for landlord credit exposure in a local market.

Third, we explore the role of housing market segmentation. As highlighted by Greenwald and Guren (2025), the effect of shocks that influence the relative demand for homeownership on rents and house prices depends on the elasticity of the rental supply curve, which in turn depends on the degree of segmentation between owner and renter markets - the ease in which housing units can be converted between owner-occupied and renter-occupied markets. We proxy the degree of housing segmentation in a local housing market with the share of single-family housing units in the market. This is based on the idea that a main source of segmentation is the suitability of different properties for rentals, and that multi-family units can be maintained and managed as rentals more effectively than single-family homes (Halket, Nesheim and Oswald, 2020). We find that the effect of monetary policy on rents is greater in areas where owner and renter markets are more segmented. When properties are less suitable to be converted to rentals, the supply of rentals is less elastic and a demand shock induced by monetary tightening translates to a larger increase in rents.

Related Literature

This paper is one of the first to study the effects of monetary policy on rents. A large literature evaluates how monetary policy and interest rates impact house prices (Case and Shiller, 1989; Kuttner, 2014; Williams et al., 2015; Aastveit and Anundsen, 2022; Gorea, Kryvtsov and Kudlyak, 2022; Adelino, Schoar and Severino, 2025), homeownership (Ringo, 2024; Dias and Duarte, 2024), and housing search (Badarinza, Balasubramaniam and Ramadorai, 2024; Han, Ngai and Sheedy, 2025), but evidence on rents remains scarce and mixed. Dias and Duarte (2019) and Dias and Duarte (2024) find that rents in the U.S. rise in response to contractionary monetary policy shocks, while Cloyne, Ferreira and Surico (2020) find that households in the U.S. and U.K. report higher rent payments after expansionary shocks. In the EU, Corsetti, Duarte and Mann (2022) document that rents increase in response to contractionary monetary policy shocks, while Koeniger, Lennartz and Ramelet (2022) find the opposite result.

A main contribution of this paper is to study how the effects of monetary policy on rents differ across local housing markets. While the aforementioned literature uses national level rent indexes and is hence confined to studying the aggregate effects of monetary policy, the comprehensiveness and granularity of our rent index allows us to document substantial heterogeneities across local housing markets. Indeed, we show that while on average contractionary monetary policy increases rents, the sign of the effect varies based on local economic conditions. Given that rent is the single largest component of the CPI, documenting this heterogeneity is particularly important for understanding the circumstances under which monetary policy is more or less effective in stabilizing inflation. Perhaps closest to our paper is a working paper by Groiss and Syrichas (2025), who construct regional rent indices to study the effect of monetary policy on rents in Germany. Our paper differs from theirs along several dimensions. First, our rent index is constructed at the hyper-local neighborhood level, while they construct a coarser index at the district level (roughly equivalent to a U.S. county or MSA). As a result, we are able to more precisely estimate heterogeneous effects. Second, our heterogeneity analysis focuses on financial constraints and housing segmentation, while theirs focuses on regional rent control policies. Third, while we find that contractionary monetary policy increases aggregate rents, they find the opposite result. The difference might reflect institutional and cultural differences between the U.S. and Germany, where the intertemporal substitution channel seems to dominate.

Our work relates to the vast literature that studies the role of the mortgage market in the transmission of monetary policy (Scharfstein and Sunderam, 2016; Bhutta and Keys, 2016; Garriga, Kydland and Šustek, 2017; Di Maggio et al., 2017; Beraja et al., 2019; DeFusco and Mondragon, 2020; Di Maggio, Kermani and Palmer, 2020; Berger et al., 2021; Fuster et al., 2021; Eichenbaum, Rebelo and Wong, 2022; Benetton, Gavazza and Surico, 2024; Bosshardt et al., 2024; Anenberg, Scharlemann and Van Straelen, 2025). Our contribution is to emphasize that the rental market also plays a key role in the transmission of monetary policy. By increasing the cost of debt, monetary tightening increases demand in the rental market and as a result raises rents. This limits the capacity of contractionary monetary policy to curb inflation and might help in explaining the well-known "price puzzle" of monetary policy (Sims, 1992; Eichenbaum, 1992). Our results also underscore that monetary tightening can exacerbate rental housing affordability and contribute more broadly to the literature on the distributional effects of monetary policy (Doepke, Schneider and Selezneva, 2015; Coibion et al., 2017; Kaplan, Moll and Violante, 2018; Auclert, 2019; Cloyne, Ferreira and Surico, 2020; Luetticke, 2021; Holm, Paul and Tischbirek, 2021; Amberg et al., 2022; Andersen et al., 2023).

Finally, our paper also relates to a growing literature on the effects of mortgage lock-in on housing markets. This literature establishes that rising mortgage rates reduce mobility rates of existing homeowners who have locked-in low mortgage rates (Quigley, 1987; Ferreira, Gyourko and Tracy, 2010; Fonseca and Liu, 2024; Batzer et al., 2024; Aladangady, Krimmel and Scharlemann, 2024; Liebersohn and Rothstein, 2025). This can in turn lower the supply of houses for sale, increase house prices (Mabille, Liu and Fonseca, 2024; Gerardi, Qian and Zhang, 2024), and ultimately increase demand in the rental market and drive up rents (De la Roca, Giacoletti and Liu, 2024). While these papers focus on mortgage lock-in, we study the effects of monetary policy more broadly. Higher interest rates can raise rents even absent mortgage lock-in, for example by preventing households from becoming homeowners (Ringo, 2024), which might crowd in the rental market (Gete and Reher, 2018; Castellanos, Hannon and Paz-Pardo, 2024; De Stefani, 2025).

The remainder of the article is organized as follows. Section 2 describes our data. In Section 3, we construct our repeat-rent index. We estimate the aggregate effects of monetary policy on rents in Section 4. We analyze how the effects differ across local housing markets in Section 5. Section 6 concludes.

2 Data

This section describes our data. We begin by discussing the rental listing data that we use to construct our repeat-rent index. We then describe our measures of monetary policy shocks as well as the instrumental and control variables that we use for estimating the effects of monetary policy on rents.

2.1 Rent Prices

Our main data source is rental listing data compiled by Altos Research between January 2011 and September 2025. Altos compiles a national database of rental listings from online listing platforms and from Multiple Listings Services (MLS) platforms. Updated on a weekly basis, the data provides a snapshot of listings that are observed during the week. For each listing, the data records the listed monthly rent, the date in which the listing is observed, the street address, zipcode, and geocodes of the unit being listed, as well as physical characteristics of the listed unit: the number of beds and baths, floor size, property type, year built, and whether the property features amenities such as air-conditioning and in-unit washer-dryer.

Sample Selection

We focus on listings of multifamily units and single family homes and exclude short-term and vacation rentals, commercial properties, mobile homes, and listings of individual rooms. We drop listings where the rent, date, number of beds or number of baths is missing, as well as listing with incomplete information on the unit address. To avoid outliers, we drop listings with listed rents that exceed the 97.5 percentile or are below the 2.5 percentile of contract rents in the American Housing Survey (AHS). Sample selection is discussed in more detail in Appendix A.

Identifying Rental Units

In Section 3, we use our rental data to construct a repeat-rent index. To facilitate the construction of a repeat-rent index, one must first identify listings of the same unit across time. This is because, as discussed in more detail in Section 3, a repeat-rent index is constructed by comparing rents on the same unit across time. Since our data does not provide a unit identifier, our strategy is to identify units by their street address, number of beds and number of baths. That is, we assume that listings within the same building that have the same number of beds and the same number of baths correspond to the same unit.

A potential concern with this strategy is that multifamily buildings might feature multiple different units that have the same number of beds and baths. However, for the purpose of constructing a repeat-rent index, which is a quality-constant measure of rent growth, this is problematic only if these units differ in their quality. In other words, if units within the same building that have the same number of beds and baths are also of the same quality, then comparing a rent listed on one unit to a rent listed on another unit later in time indeed provides a quality-constant measure of rent growth.

If, in contrast, units within the same building that have the same number of beds and baths do differ in their quality, then comparing rents listed on one unit to a rent listed on another unit later in time does not provide a quality-constant measure of rent growth and would bias the repeat-rent index. Therefore, to be conservative, we drop units that likely differ in their quality but that are undistinguishable based on their address, number of beds and number of baths. Namely, we drop units that correspond to tuples of address, number of beds and number of baths for which we observe within a same week multiple listings with different prices, or for which we observe statistically extreme rent fluctuations within a one-month or a one-year period.[2]

Our main analysis is at the monthly frequency. To obtain a monthly panel of listing prices at the unit level, we collapse all listings of the same unit that appear within the same month (due to the unit being listed on multiple platforms or being listed for several weeks within a month), to one observation. Namely, we keep the last listing observed within the month. Units that are not listed in more than one month are excluded, since they do not inform the repeat-rent index. Our final panel data contains 48.1 million monthly observations of listed rents. It comprises 10.0 million rental units. Each rental unit is observed on average for approximately 4.8 months during the sample. The average time

[2]Specifically, we drop units that correspond to tuples of address, number of beds and number of baths for which we observe a 4-week (52-week) rent fluctuation (in absolute value) that exceeds the 95th percentile (99th percentile) of the 4-week (52-week) rent fluctuation distribution in the data.

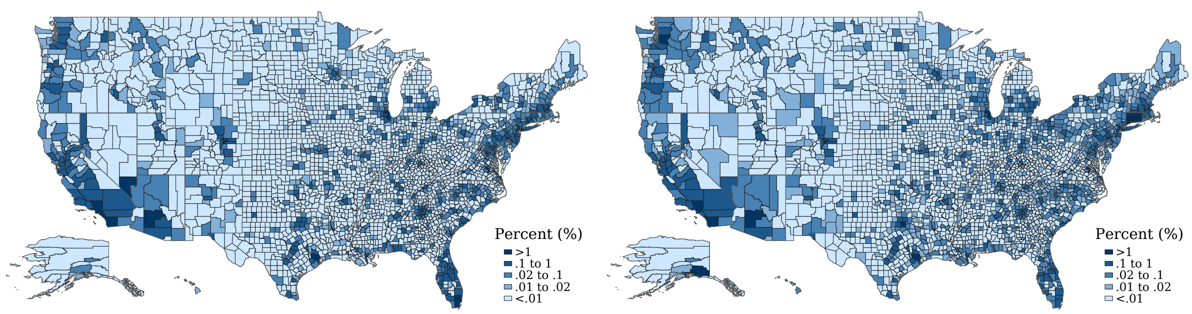
on market (i.e., the consecutive number of months a unit is being listed) is 2 months.

Geographical Coverage

Panel (a) of Figure 1 illustrates the geographical coverage of our data. For each county, we compute the percentage of all rental units observed in our data in 2019 that are located in that particular county. Counties colored in lighter (darker) shades are counties where we observe relatively few (many) rental units. Not surprisingly, we observe more rentals units in more densely populated areas of the U.S., for example the two coasts.

Panel (b) of Figure 1 illustrates the geographical distribution of rental units in the U.S., as measured from the nationally representative 2019 American Community Survey (ACS). Counties colored in lighter (darker) shades are counties with a relatively small (large) number of rental units. Reassuringly, comparing both panels shows that the geographical coverage of our data aligns well with the geographical distribution of rental units in the U.S. This suggests that our data is representative of the U.S. rental market in terms of its geographical coverage.

Figure 1: Geographical Coverage



(a) Altos (b) ACS

Note: Panel (a) displays, for each county, the percentage of all rental units observed in our rent data during 2019 that are located in that county. Panel (b) displays, for each county, the share of all rental units in the 2019 ACS data that are located in that county. Darker colors correspond to higher shares.

Summary statistics

Table 1 compares summary statistics from our data to summary statistics computed from the AHS, the ACS, and Zillow. The AHS is a nationally representative survey of the housing stock in the U.S. To maintain consistency with our data, as well as with Zillow, we focus on new renters within the ACS and AHS samples. The first column compares the median rent across the different datasets, reported in 2019 U.S. dollars. Columns 2-5 compare physical characteristics of the median unit across datasets, and Column 6 reports the share of rental units that are single-family. For all datasets, summary statistics are computed for 2019. It is evident that the average rent in our data is higher than the average rent in the AHS and ACS and that units in our data are larger on average. Our data aligns well with Zillow in terms of the median rent.

Table 1: Summary Statistics

	Rent ($)	**Year Built**	**Bedrooms (#)**	**Bathrooms (#)**	**Sqft**	**Single-family (%)**
	(1)	(2)	(3)	(4)	(5)	(6)
ACS	1000	1984	2	.	.	29
AHS	1100	1974	2	1	875	32
Altos	1500	1980	2	2	1400	28
Zillow	1437	.	.	.	.	.

Note: This table presents summary statistics from ACS, AHS, our Altos data, and Zillow. The first column reports the median rent (in 2019 dollars), columns 2-5 compare physical characteristics of the median unit, and Column 6 reports the share of rental units that are single-family. ACS statistics are computed from the 1-year 2019 ACS survey. AHS statistics are computed from the 2019 biennial survey. Altos statistics are computed based on Altos data between January and December 2019. The median Zillow rent is computed as the median national ZORI between between January and December 2019.

One plausible explanation for the discrepancy between our data and the AHS and ACS is that our data (as well as Zillow) records listed rents, while the AHS and ACS record contract rents which might be lower. To evaluate the extent to which listed rents differ from contract rents, we use Multiple Listing Services (MLS) data from Cotality (previously Corelogic) between 2011 and 2025. While the MLS data covers only a subset of rental listing (typically those posted by real estate brokers), its advantage is that it records both listed and contract rents. For each MLS listing, we compute the difference between the listed rent and the contract rent. Table C.1 reports, for each year, the average and median difference between listed and contract rents, as well as the percentage of listings for which the listed rent is exactly equal to contract rent. The analysis shows that listed rents closely and consistently track contractual rents. The average difference between listed and contract rents is never higher than 1.4 percent and the median difference is always zero. The share of units for which there is no difference between listed and contract rents is between 70 and 80 percent. Based on this evidence, we argue that listed rents are a good proxy for contractual rents.

A more plausible explanation for the discrepancy between our data and the AHS and ACS is selection. It is likely that higher-quality rental units are over-represented on online listings platforms and therefore disproportionally more likely to be observed in our data. Regardless of the reason for the discrepancy, a potential concern with the repeat-rent index that we construct in Section 3 is that it is based on a sample that is not representative of rent levels in the U.S. We address this concern in Section 3.1 by showing that our repeat-rent index is representative of rent *inflation* in the U.S. Specifically, we show that, in terms of growth rates, our index closely tracks alternative rent indexes that are based on nationally representative samples of rental units and that measure inflation of contractual rents.

2.2 Monetary Policy Shocks

In our main empirical specification, we measure exogenous shocks to monetary policy using the Bauer and Swanson (2023*b*) monetary policy surprises series. Bauer and Swanson (2023*b*) identify monetary policy shocks in two steps. First, they measure high-frequency changes in interest rates around Federal Open Market Committee (FOMC) meetings and around press conferences, speeches, and testimonies made by the Federal Reserve chair. Second, they orthogonalize these monetary policy surprises by regressing them on economic and financial variables that predate the FOMC meetings and Federal Reserve chair announcements, and take the residuals. We download the monthly Bauer and Swanson (2023*b*) monetary policy surprises from the Federal Reserve Bank of San Francisco data portal.[3] The publicly available series includes only monetary policy surprises measured around FOMC meetings.

Recent work has shown that high-frequency changes to interest rates around FOMC meetings might not be exogenous. For example, Cieslak (2018), Miranda-Agrippino and Ricco (2021), and Bauer and Swanson (2023*a*) show that these changes are correlated with publicly available macroeconomic and financial indicators that predate these announcements. If high-frequency changes to interest rates around monetary policy events are not exogenous, they are not a valid instrument for estimating the effects of monetary policy (Stock and Watson, 2018). Bauer and Swanson (2023*b*) address this concern by regressing the changes in interest rates around monetary policy announcements on economic and financial data that predate these announcements. Their monetary policy surprises, which are the residuals from this regression, capture the component of changes to interest rates around monetary policy events that is ex-ante unpredictable.

The choice of monetary policy shock can be important for the estimated effects of monetary policy (Ramey, 2016). We therefore replicate our empirical analysis for a host

[3]See https://www.frbsf.org/research-and-insights/data-and-indicators/monetary-policy-surprises/.

of alternative monetary policy shocks that have been used in the literature. We use monetary policy shocks from Gürkaynak, Sack and Swanson (2005), Gertler and Karadi (2015), Nakamura and Steinsson (2018), and Swanson (2021) and show that are results are robust to these alternative shocks. In line with Bauer and Swanson (2023*b*), we construct a monthly series for each of these alternative shocks (which are provided at a daily frequency) by summing all daily shocks within each month.

2.3 Instrument and Control Variables

In Section 4, we employ a local projection instrumental variable approach (LP-IV) to evaluate the effects of monetary policy (Ramey, 2016; Stock and Watson, 2018). Here, we briefly describe the instrumented variables and controls that are used in the estimation. As the instrumented monetary policy indicator, we use the Freddie Mac 30-year fixed mortgage rate, downloaded from FRED (series: MORTGAGE30US). Bauer and Swanson (2023*b*) use the interest rate on two-year US Treasury bonds as their instrumented variable, but since our focus is on the housing market, we use the mortgage rate as our relevant monetary policy variable (as is common in the literature on the effects of monetary policy on housing markets, e.g. Aastveit and Anundsen (2022)). Bauer and Swanson (2023*b*) document a statistically and economically significant relationship between their monetary policy shocks and the 30-year treasury yield. In Section 4, we confirm that the Bauer and Swanson (2023*b*) monetary policy surprise series is a valid instrument to the 30-year fixed mortgage rate. We also show that our results are robust to using the interest rate on two-year US Treasury bonds (downloaded from the Federal Reserve Board website, series: SVENY02) or the effective federal funds rate (series: EFFR) as the instrumented variable. For controls, we include lags of Core PCE inflation and lags of unemployment rates at the county level, as well as lags of monetary shocks and the monetary policy indicator. PCE (series: PCEPILFE) is downloaded from FRED. County-level unemployment rates are downloaded from the BLS.[4]

3 Repeat-Rent Index

We construct a repeat-rent index using our rental listings data. Hereafter, we refer to this index as the ADH-RRI. Introduced by Bailey, Muth and Nourse (1963), the repeat-sales method provides a quality-constant measure of price growth. In particular, it uses

[4]See https://download.bls.gov/pub/time.series/la/.

repeated sales of the same housing unit to control for observed and unobserved time-invariant quality components. Popularized by Case and Shiller (1989), repeat-sales indexes have become the gold standard of house price indexes. Starting with Ambrose, Coulson and Yoshida (2015), repeat-*rent* indexes have also been used by applying the repeat-sales method to the rental market (Clark, 2022; Adams et al., 2024).

Consider a rental unit that is observed in our listing data in both time s and time $t > s$. Assume that the data consists of listings observed in times $\{1, ..., N\}$. The repeat-rent index is constructed by estimating the following regression:

$$\log P_{i,t} - \log P_{i,s} = \gamma_1 D_{i,1} + \gamma_2 D_{i,2} + ... + \gamma_N D_{i,N} + \varepsilon_{i,t}, \tag{1}$$

where $P_{i,s}$ is the listed rent on unit i at time s and $P_{i,t}$ is the listed rent on the same unit i at a later time t. $D_{i,k} = 1$ if the second observation in the pair took place in time k, $D_{i,k} = -1$ if the first observation in the pair took place in time k, and $D_{i,k} = 0$ otherwise. The estimated parameters $\{\gamma_1, ...\gamma_N\}$ represent the percentage change in listed rents relative to the base (omitted) period. The exponents of these estimates constitute the repeat-rent index, where we normalize the value of the index in the base period to 100. To minimize noise, we follow Clark (2022) and smooth the index using a three month moving average. That is, the ADH-RRI in time t is given by $ADHRRI_t = 100\Sigma_{k=-1}^{1} \exp(\gamma_{t+k})$.

The error term in Equation 1 is likely heteroskedastic due to variation in the time-gap between pairs of repeated listings (Case and Shiller, 1989). To address this, we follow Calhoun (1996). First, we estimate Equation 1 by OLS. Second, we regress the residuals from this regression on a constant, the time-gap between observations and the square of the time-gap between observations, and store the predicted values. Third, we estimate a weighted least squares version of Equation 1 using the inverse of the square roots of these predicted values as weights. In line with other work (Clark, 2022; Adams et al., 2024), we find that our RRI is practically unchanged due to this adjustment.

We construct and analyze various specifications of our ADH-RRI. First, we construct both a nominal ADH-RRI which measures nominal rent inflation, and a real ADH-RRI which measures how rents grow relative to all other prices in the economy. Specifically, the real rent index is computed by first deflating nominal rents by the non-shelter CPI index and then estimating Equation 1. Second, we construct both an "all listings" index and a "new listings" index, which we refer to as the ADH-NRRI. The latter is based only on new listings that come on the market (i.e. listings which were not observed in the previous period) while the former is based on all observed listings. Third, we construct our ADH-RRI at multiple geographical levels - from the hyper-local census tract level, through the zipcode and CBSA levels, up to the national level.[5] Fourth, we consider both monthly and quarterly indexes. Finally, we construct a repeat-rent index for single-family rental units, an index for for multifamily rental units, and an index for for all rental units. For each index, we retain only geographies for which the repeat-rent index is constructed for at least 90% of the time periods between the first and last period the index is constructed for.

3.1 Comparison to Alternative Rent Indexes

This section compares our ADH-RRI to popular alternative rent indexes - the Zillow Observed Rent Index (ZORI, Clark (2022)), the CPI-rent index, the Marginal Rent Index (ACY-MRI, Ambrose, Coulson and Yoshida (2023)), and the CPI-NTRR (Adams et al. (2024)). We begin by briefly describing the alternative indexes. We then show how our ADH-RRI aligns with these indexes and demonstrate that it is representative of rent inflation in the U.S. Finally, we discuss the advantages of our index, namely its broader and more granular geographical coverage.

Alternative Indexes

The CPI-rent and CPI-NTRR indexes are both constructed from the BLS Housing Survey data and are based on contractual rents. The BLS Housing Survey is a nationally representative panel of renter-occupied housing units. For each unit, the survey records the contract rent, the utilities included, unit characteristics and tenants' move-in date. The BLS sample is divided into six panels, and each rental unit is surveyed every six months. For a detailed discussion of the CPI-rent index and the CPI-NTRR, see Verbrugge and Poole (2010) and Adams et al. (2024). Below, we provide we brief summary.

The CPI-rent index is constructed at the monthly frequency. Rent growth is measured by first calculating the average six-month rent growth across the units in the panel that is surveyed in that month, and then taking the sixth root of that average. The CPI-rent index measures the rent growth faced by all tenants, regardless of their occupancy tenure. It adjusts for aging, structure changes, and changes in utilities included in rent. The most granular geographical level for which the CPI-rent index is constructed is the Core Based

[5] We construct the national index as a weighted average of the zipcode level index, with zipcodes weights corresponding to their aggregate rental stock value. Specifically, the national index is constructed as $ADHRRI_t = \sum_z ADHRRI_{z,t}\omega_z / \sum_z \omega_z$, where $ADHRRI_{z,t}$ is the ADH-RRI for zipcode z at time t and ω_z is the aggregate contract rent in zipcode z, between 2015 and 2019. This approach follows the method used to construct the national Case-Shiller house price index and ensures that zipcodes with a larger and more expensive rental stock are over-represented in the national index.

Statistical Area (CBSA).

The CPI-NTRR (Adams et al., 2024) is a repeat-rent index that measures the rent growth faced by *new* tenants. This is in contrast to the CPI-rent index, which measures rent growth faced by both new and continuing tenants. By limiting the BLS sample only to observations where occupants are new tenants, the CPI-NTRR measures the rent growth that a new renter would face had she signed a new rent contract every period. A main advantage of the CPI-NTRR (and of the CPI-rent) is that it is based on a representative sample of U.S. rental units. The main limitation of the CPI-NTRR is that the sample size of BLS Housing Survey is relatively small. For this reason, the CPI-NTRR is constructed only at the quarterly frequency and only at the national level.

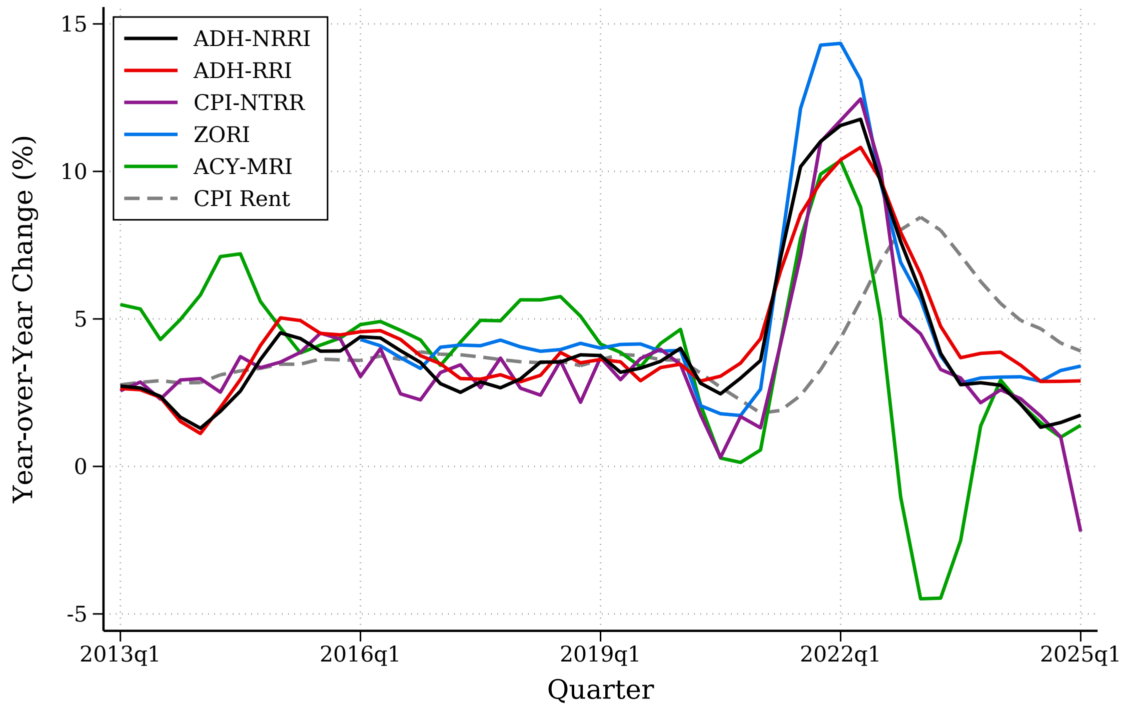
The Zillow Observed Rent Index (ZORI) is a repeat rent index that is constructed from Zillow's proprietary rental listings data and from MLS listing data. As the CPI-NTRR and our ADH-RRI, ZORI measures rent growth faced by new renters. ZORI is based on a sample of rental units that are listed online and is therefore not necessarily representative of the U.S. stock of rental units. For example, as illustrated by Table 1, units listed on Zillow and on MLS are of higher quality relative to the average rental unit in the country. ZORI is constructed at the monthly frequency and at the zipcode level. As we discuss below, the geographical and temporal coverage of ZORI is limited compared to our ADH-RRI. For a detailed discussion of ZORI, see Clark (2022).

The ACY-MRI (Ambrose, Coulson and Yoshida, 2023) is a rent index that measures rent growth faced by tenants in large multifamily buildings. It is constructed in two steps. First, a net rent index (NRI) is computed as the product of the Real Capital Analytics' (RCA) multifamily capitalization rate and the RCA commercial property price index (CPPI), which is a quality-adjusted repeat-sale index of multifamily properties. Second, the ACY-MRI is constructed by rescaling the NRI so that its mean and volatility match the mean and volatility of a previous rent index constructed by (Ambrose, Coulson and Yoshida, 2015). The ACY-MRI is constructed only at the national level.

Index Comparison

Figure 2 compares the year-over-year rent inflation implied by our ADH-RRI and ADH-NRRI to the rent inflation implied by the CPI-rent index, the CPI-NTRR, ZORI, and the ACY-MRI. Rent inflation is measured at the national level. The figure illustrates that, despite differences in the underlying rental data and index construction methods, our ADH-RRI and ADH-NRRI closely track ZORI and, most importantly, the nationally representative CPI-NTRR. The four indexes clearly capture common rental market dynamics. This is an indication that our ADH-RRI is representative of rent inflation in the U.S.

Figure 2: Rent Inflation in Alternative Rent Indexes



Note: This figure plots the year-over-year rent inflation in alternative rent indexes. Year-over-year inflation for ADH-RRI, ADH-NRRI, ACY-MRI, CPI-NTRR, and CPI Rent is computed for each quarter between 2013q1 and 2025q1. Year-over-year inflation for ZORI is computed for each quarter between 2016q1 and 2025q1. The CPI-NTRR is downloaded from `https://www.bls.gov/pir/new-tenant-rent.htm` at a quarterly frequency. ZORI is downloaded from `https://www.zillow.com/research/data/`, ACY-MRI is downloaded from `https://sites.psu.edu/inflation/`, and CPI-Rent is downloaded from `https://fred.stlouisfed.org/series/CUSR0000SEHA`, all at a monthly frequency. All monthly indexes are converted to quarterly indexes by averaging across months within the quarter.

Our ADH-RRI and ADH-NRRI lead the CPI-rent index, as do the CPI-NTRR and ZORI. This is because the CPI-rent index measures the rent growth faced by both new and existing tenants, while the other indexes measure the rent growth faced only by *new* tenants. Since rents on existing leases fluctuate less than rents on new leases, the CPI-rent index is less volatile and lags the other indexes. The ACY-MRI index, which measures rent inflation for a selected segment of the rental market, appears to be more volatile and tends to deviate from the other indexes.

To provide further validation for our repeat-rent index, Table 2 reports the pairwise correlation coefficients between the year-over-year rent inflation according to our index and the year-over-year rent inflation according to alternative rent indexes. Correlations between the ADH-RRI, ADH-NRRI, ACY-MRI, CPI-NTRR and CPI-rent are computed based on the quarters between 2013q1 and 2025q1. The correlations with ZORI are computed based on the quarters between 2016q1 and 2025q1. Our ADH-RRI and ADH-NRRI are highly correlated with the nationally representative CPI-NTRR and with ZORI, while the correlation between all indexes and the CPI-rent index is lower. Overall, Figure 2 and Table 2 show that the ADH-RRI and ADH-NRRI closely track the nationally representative CPI-NTRR, suggesting they are representative of rent inflation in the U.S. The consistency between the ADH-RRI and ADH-NRRI and some of the alternative indexes is in line with prior work documenting a strong correlation between alternative repeat-rent indexes (Ambrose, Coulson and Yoshida, 2015; Adams et al., 2024). In Table C.2, we provide further validation of our ADH-RRI. We show that it aligns very closely with ZORI also at the more granular zipcode level and at the higher monthly frequency.

Table 2: Correlation between Alternative Rent Indexes

	ADH-RRI	ADH-NRRI	CPI-NTRR	ZORI	ACY-MRI	CPI-Rent
ADH-RRI	1.00	0.97	0.88	0.93	0.23	0.35
ADH-NRRI	0.97	1.00	0.91	0.96	0.37	0.22
CPI-NTRR	0.88	0.91	1.00	0.95	0.50	0.25
ZORI	0.93	0.96	0.95	1.00	0.61	0.09
ACY-MRI	0.23	0.37	0.50	0.61	1.00	-0.54
CPI-Rent	0.35	0.22	0.25	0.09	-0.54	1.00

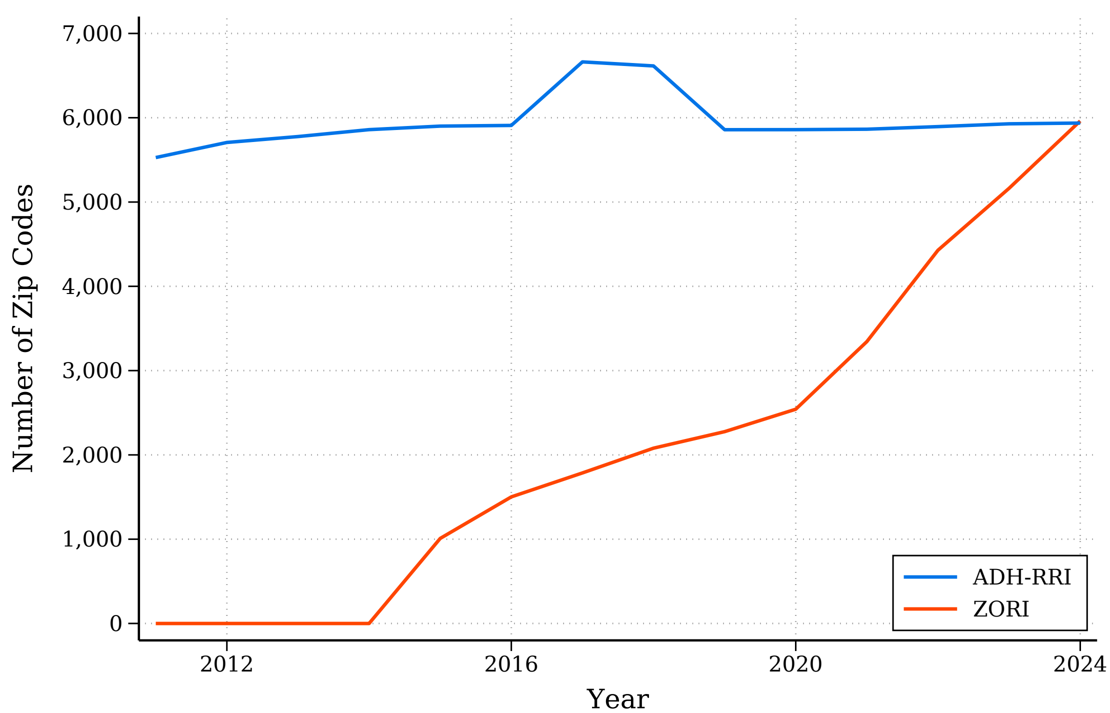
Note: This table reports the pairwise correlation coefficients between year-over-year rent inflation in alternative rent indexes. The correlations between ADH-RRI, ADH-NRRI, ACY-MRI, CPI-NTRR and CPI-Rent are computed based on the quarters between 2013q1 and 2025q1. The correlations with ZORI are computed based on the quarters between 2016q1 and 2025q1.

Advantage of the ADH-RRI

The ADH-RRI has two key advantages relative to alternative rent indexes. First, to the best of our knowledge, the ADH-RRI is the most granular high-frequency rent index to date. The ADH-RRI is constructed at the census tract level, while alternative indexes are either computed a the national level (CPI-NTRR and ACY-MRI), the CBSA level (CPI-Rent), or the zipcode level (ZORI). Second, the ADH-RRI provides the most comprehensive geographical coverage of rental markets in the U.S. It covers substantially more zipcodes compared to ZORI, which is currently the rent index with the broadest geographical coverage. This is illustrated in Figure 3, which plots the number of zipcodes covered by our ADH-RRI and by ZORI across time. While ZORI is available only starting in 2015, the ADH-RRI is available starting from 2011. When ZORI becomes available in 2015, the ADH-RRI covers roughly 6 times as many zipcodes. As time passes, ZORI expands it coverage and by the end of our sample its coverages is similar to that of the ADH-RRI. Appendix Figure C.1 illustrates the geographical distribution of the zipcodes covered by both indexes.

The granularity and broad geographical coverage of the ADH-RRI are key for studying the heterogeneous effects of monetary policy on rents. While a small but growing literature studies the relationship between monetary policy and rents (Dias and Duarte, 2019, 2024; Corsetti, Duarte and Mann, 2022; Koeniger, Lennartz and Ramelet, 2022), these papers are limited to studying the aggregate effects of monetary policy. As a result, they abstract from the tremendous variation that exists across local housing markets. Our ADH-RRI allows us to fill this gap. Indeed, as discussed in Section 5, we find substantial heterogeneity in the responsiveness of rents to common monetary policy shocks across local housing markets. More broadly, our ADH-RRI is a powerful tool for many other applications - it allows studying rental markets at an unprecedented level of granularity and geographical breadth.

Figure 3: Coverage - ADH-RRI and ZORI



Note: This figure plots number of zipcodes covered by the ADH-RRI and by ZORI for every year between 2011 and 2024.

4 Aggregate Effects of Monetary Policy

In this section, we use our repeat-rent index to evaluate the aggregate effects of monetary policy on rents. In Section 5, we turn to examine the heterogeneous effects across local housing markets. We estimate the dynamic effects of a monetary policy shock on rents using the standard local projection instrumental variable (LP-IV) framework (Jordà, Schularick and Taylor, 2015; Ramey, 2016). The LP-IV framework combines the Jordà (2005) local projection approach (LP) with instrumental variable (IV) methods and is discussed in detail in Stock and Watson (2018). To perform an LP-IV estimation, we estimate the following LP regression via two-stage least squares:

$$\log ADHRRI_{z,t+h} - \log ADHRRI_{z,t-1} = \beta^{(h)} i_t + \Gamma^{(h)} X_{z,t-1} + u^{(h)}_{z,t+h}, \tag{2}$$

for each horizon $h = \{0, 1, ..., 24\}$. The dependent variable is the cumulative rent inflation in geography z between month $t-1$ and month $t+h$, measured based on our ADH-RRI. i_t is a monetary policy indicator. Since our context is the housing markets, in our baseline specification we use the 30-year fixed rate mortgage as our monetary policy indicator (Aastveit and Anundsen, 2022; Gorea, Kryvtsov and Kudlyak, 2022). $\beta^{(h)}$ is the coefficient of interest and captures how a change in the monetary policy indicator impacts rent inflation going forward. $X_{z,t-1}$ is a set of controls which we include to ensure that the LP-IV estimation satisfies the Stock and Watson (2018) instrument validity conditions. We discuss the controls and validity conditions in more detail below. The error term, $u_{z,t+h}$, is a linear combination of all 'structural shocks' up to time $t+h$, excluding a directly observable contemporaneous monetary policy shock denoted by s_t (Ramey, 2016; Stock and Watson, 2018). Standard errors for the estimated coefficients are clustered by both geography (to account for potential serial correlation of errors across time) and by time period (to account for potential heteroskedasticity of errors across geographies) and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

In the first stage, the monetary policy indicator i_t, which is likely endogenous, is instrumented with a directly observed measure of exogenous monetary policy shock, s_t. As explained by Stock and Watson (2018), monetary policy surprises measured from high-frequency interest rate changes around FOMC meetings might capture only part of the true underlying (and unobserved) monetary policy shock and might be measured with error. They are therefore instruments for the true monetary policy shock, not the shock itself. Since the true monetary policy shock is unobserved, the instrumented variable in the LP regression is the observed monetary policy indicator i_t.

For the observed monetary policy shock s_t to be a valid instrument for the monetary policy indicator, it must satisfy three conditions (Stock and Watson, 2018). First, the standard relevance condition must hold. That is, conditional on the controls $X_{z,t-1}$, the observed monetary policy shock s_t must be correlated with the monetary policy indicator i_t. Second, the standard exogeneity condition must hold. That is, conditional on controls, s_t must be uncorrelated with all other contemporaneous structural shocks. Third, the lead-lag exogeneity condition must hold. That is, conditional on controls, s_t must be uncorrelated with all structural shocks at all leads and lags.

The set of controls in Equation 2 are chosen to ensure that the instrument validity conditions hold. They include geography level controls, namely geography month-of-year fixed effects which control for seasonality at the geography level, lags of the growth rate of rent, and lags of the change in the unemployment rate, as well as macro level controls, namely lagged PCE inflation, lagged changes in the mortgage rate i_t, and lagged monetary policy shocks.[6] In Section 4.1, we show that the relevance condition and the lead-lag exogeneity condition hold. The exogeneity condition relies on the identification of truly exogenous monetary policy surprises. In our main specification, we use the Bauer and Swanson (2023*b*) monetary surprise series. For robustness, we also consider a host of alternative monetary policy shocks used in the literature.

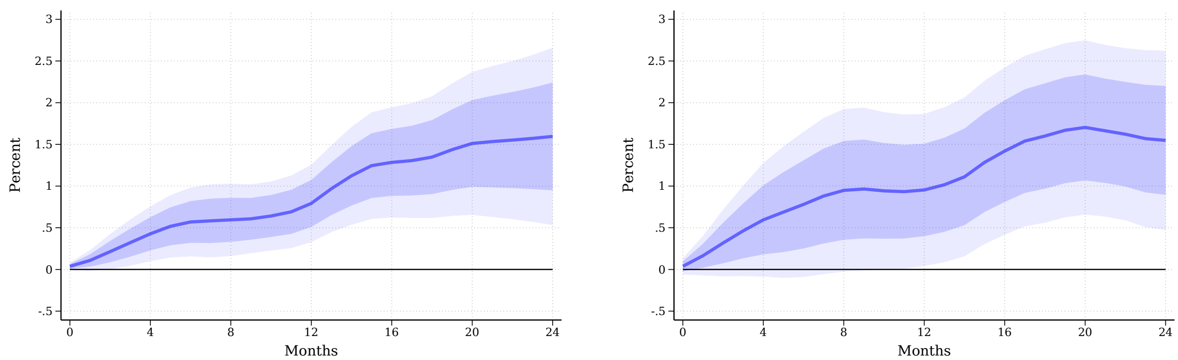
The main appeal of the local projection framework relative to the vector auto-regression (VAR) framework is that it allows for non-linear effects of monetary policy shocks without modeling them as a system (Jordà, Schularick and Taylor, 2015; Ramey, 2016). Local projection is also more robust to misspecification (Jordà, 2005). Since the local projection framework imposes less restrictions, it often results in more erratic and less precisely estimated impulse response functions. It is useful to note that the geographical breadth and granularity of our panel data does not enhance the precision of the *aggregate* impulse response function estimated from Equation 2. This is because panel local projections with aggregate shocks are equivalent to synthetic time series local projections with an appropriately aggregated dependent variable (Almuzara and Sancibrián, 2024). However, the breadth and granularity of our index is key for precisely estimating the *heterogeneous* effects of monetary policy across housing markets (Section 5), where the source of variation is an aggregate shock interacted with a geographical time-varying market characteristic.

[6] In particular, we include 6 lags of the monthly geography level growth rate of the ADH-RRI, one lag of the year-over-year change in the unemployment rate at the county level, one lag of the year-over-year PCE inflation, one lag of the instrument, and 4 lags of the monthly change in the monetary policy indicator.

4.1 Results

Our baseline analysis is at the zipcode level. Figure 4 illustrates our results. Panel (a) plots the impulse response function of *nominal* rent inflation to an exogenous 25 basis point increase in the interest rate on a 30-year fixed rate mortgage. The dark (light) shaded areas correspond to the 68% (90%) confidence intervals. A 25 basis point increase in the interest rate on a 30-year fixed rate mortgage leads to a 0.7 (1.5) percent increase in nominal rents 12 (24) months following the monetary policy shock. Panel (b) plots the impulse response function of *real* rent inflation to the same exogenous increase in interest rate. A 25 basis point increase in the interest rate on a 30-year fixed rate mortgage leads to a 1 (1.6) percent increase in real rents 12 (24) months following the monetary policy shock. Overall, the takeaway is that contractionary monetary policy shocks increase nominal rents and makes renting more expensive relative to other goods in the economy.

Figure 4: Effect of Monetary Policy on Rents



(a) Nominal Rents

(b) Real Rents

Note: Panel (a) (Panel (b)) displays the impulse response function of nominal (real) rent inflation to a 25bps increase in the 30-year fixed rate mortgage. Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

Instrument validity. If the observed monetary policy shock satisfies the three instrument validity conditions specified in Stock and Watson (2018), then estimating Equation 2 yields consistent estimates of the effect of the 30-year fixed rate mortgage rate on rent inflation. Here, we show that the relevance condition and the lead-lag exogeneity condition, which are testable, hold. The exogeneity condition relies on the (Bauer and Swanson, 2023*b*) monetary policy surprises being exogenous and is not directly testable.

To test the relevance condition, we compute the Olea and Pflueger (2013) first-stage effective F-statistic. Figure C.2 in the Appendix plots the effective F-statistic of the first-stage of Equation 2 for the case where the outcome is real rent growth (Panel (a)) and for the case where the outcome is nominal rent growth (Panel (b)), for each horizon $h = \{0, 1, ..., 24\}$. The F-statistic is above 10, the rule of thumb cutoff for weak instruments recommended by Staiger and Stock (1997) and Andrews, Stock and Sun (2019). To test the lead-lag exogeneity condition, we follow Stock and Watson (2018) and regress the orthogonalized monetary policy shock on orthogonalized lags of the dependent variable.[7] Orthogonalizing is done against the set of controls $X_{z,t-1}$. As illustrated in Table C.3, The R-square in these regressions is virtually zero, suggesting that the instrument is unforecastable by lags of the dependent variable.

4.2 Robustness

We consider a host of robustness tests for our baseline result.

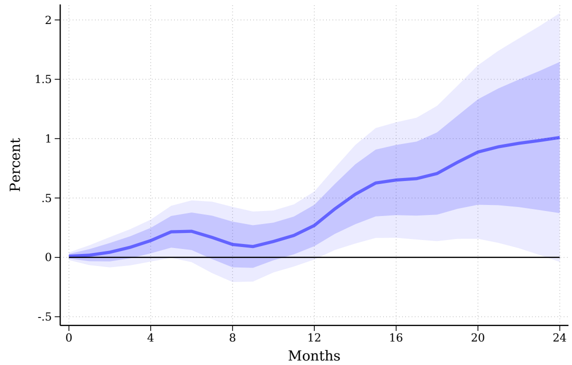
Alternative monetary policy shocks. First, the choice of monetary policy surprises s_t can be important for the estimated effects of monetary policy (Ramey, 2016). We therefore replicate our empirical analysis using a host of alternative monetary policy shocks that have been used in the literature. In particular, we re-estimate Equation 2 using monetary policy shocks from Gürkaynak, Sack and Swanson (2005), Nakamura and Steinsson (2018), Swanson (2021), and the non-orthogonalized shocks from Bauer and Swanson (2023*b*) as alternative instruments.[8] Figure 5 shows the impulse response function of nominal rent inflation for each of these alternative specifications. Figure C.3 in the appendix replicates this exercise for real rents. Reassuringly, the results are qualitatively and quantitatively in line with the baseline specification.

Alternative rent indexes. As a second robustness test, we replicate our analysis using a host of alternative rent indexes. We re-estimate Equation 2 using six alternative indexes: the CPI-rent index, the CPI-NTRR index, the ACY-MRI, the ZORI index, our new-listing repeat-rent index (the ADH-NRRI), and a hedonic rent index that we construct from our

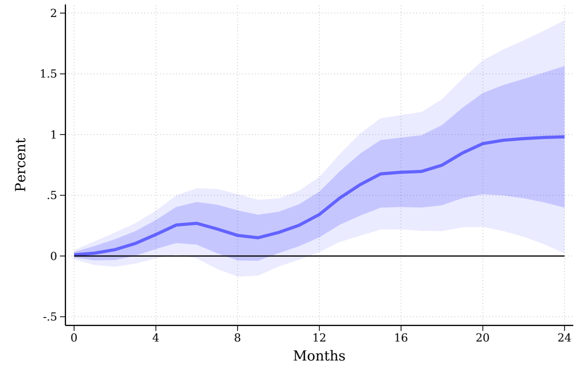
[7] As discussed in Stock and Watson (2018), the requirement that s_t is uncorrelated with future shocks follows directly from the definition of shocks as unanticipated structural disturbances.

[8] In these specifications, we also include the controls used by Bauer and Swanson (2023*b*) to orthogonalize high-frequency changes in interest rates around FOMC meetings and around Federal Reserve chair announcements. As pointed out by Cieslak (2018) and Miranda-Agrippino and Ricco (2021), high-frequency changes to interest rates around these events might not be exogenous. Bauer and Swanson (2023*b*) address this concern by regressing changes in interest rates around monetary policy announcements on economic and financial data that predate these announcements. These controls include the surprise component of the most recent nonfarm payrolls release from (Bauer and Swanson, 2023*b*), employment growth over the past year as constructed by (Cieslak, 2018), the change in the slope of the yield curve from 3 months before to one day before the FOMC announcement (measured as the second principal component of 1-to-10-year zero-coupon Treasury yields by Gürkaynak, Sack and Wright (2007)), the log change in the Bloomberg Commodity Spot Price Index over the same period, the log change in the S&P 500 index over the same period, and the option-implied skewness of the 10-year Treasury yield from Bauer and Chernov (2024).

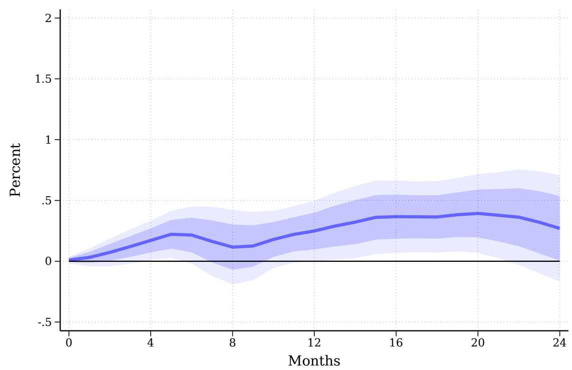
Figure 5: Alternative Monetary Policy Shocks



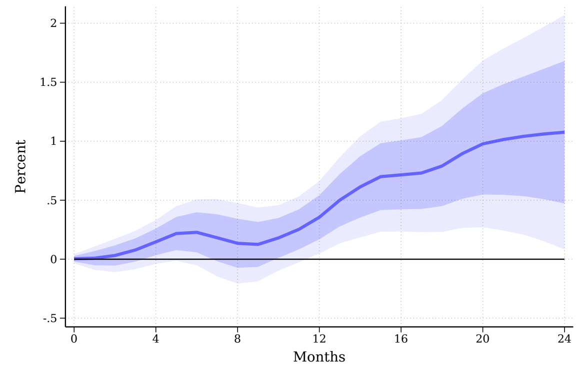
(a) Gürkaynak Sack Swanson (2005)



(b) Nakamura Steinsson (2018)



(c) Swanson (2021)



(d) Bauer Swanson, Non-Orthogonalized (2023)

Note: This figure displays the impulse response function of nominal rent inflation to a 25bps increase in the 30-year fixed rate mortgage using alternative monetary policy shocks. Panel (a) corresponds to the Gürkaynak, Sack and Swanson (2005) shocks, where we use both the surprise changes in the federal funds rate and the surprise changes in forward guidance as instruments for the monetary policy indicator. Panel (b) corresponds to the Nakamura and Steinsson (2018) shocks. Panel (c) corresponds to the Swanson (2021) shocks, where we use surprise changes in the federal funds rate, in forward guidance, and in large-scale asset purchases (LSAPs) as instruments for the monetary policy indicator. Panel (d) corresponds to the non-orthogonalized shocks from Bauer and Swanson (2023*b*). Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

listings data. The construction of the hedonic index is discussed in Appendix A.1. Since the CPI-rent, the CPI-NTRR, and the ACY-MRI indexes are not available at the zipcode level, and since some are available only at the national level and the quarterly frequency, we re-estimate Equation 2 at the national level and quarterly frequency.[9] Figure C.4 plots the impulse response functions of rent inflation to an exogenous 25 basis point increase

[9]Following Gertler and Karadi (2015), we compute the quarterly monetary policy shock by first summing, for each of the three months within the quarter, the monetary surprises over the preceding three months, and then averaging this sum across the three months. The monetary policy indicator is computed as the average 30-year fixed rate mortgage. Controls include quarter-of-year fixed effects, one lag of the year-over-year core PCE inflation and of the year-over-year change in the unemployment rate, one lag of the change in monetary policy indicator, and four lags of the dependent variable.

in the 30-year fixed rate mortgage rate for each of these alternative indexes. The results are qualitatively and quantitatively consistent with our baseline results. As expected, the effect on CPI-Rent, which tracks rent inflation faced by both new and existing tenants, is lagged relative to the effect on other indexes.

Alternative monetary policy indicators. Since our focus is on the housing market, in our baseline specification we use the 30-year fixed mortgage rate as the instrumented monetary policy indicator. We assess the sensitivity of our results to alternative policy indicators that are often used in the literature, namely the interest rate on the two-year US Treasury bond and the effective federal funds rate (Gertler and Karadi, 2015; Bauer and Swanson, 2023*a*; Jordà and Taylor, 2025). To do so, we re-estimate Equation 2 using each of these alternative monetary policy indicators as our instrumented variable. As illustrated in Figure C.5, the results are qualitatively and quantitatively consistent with our baseline specification.

4.3 Prices, Homeownership, and Inventory of Renter-Occupied Units

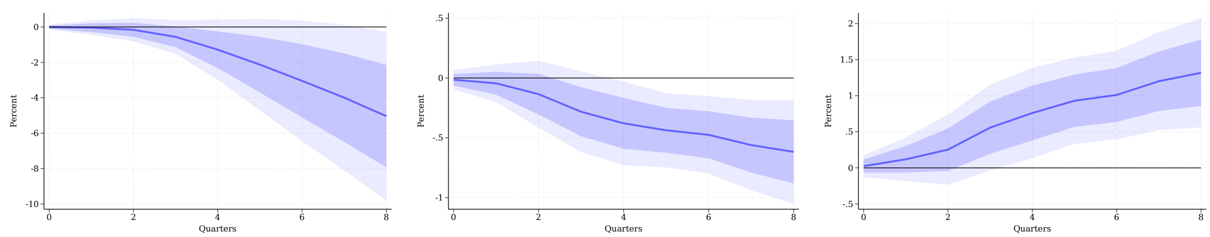
In this section we study how monetary policy impacts homeownership, house prices, and most importantly the inventory of renter-occupied housing. This is useful for understanding the mechanisms that underlie the aggregate effect of monetary policy on rents. Our measures for these outcomes are at the national level and quarterly frequency. House prices are measured using the Case-Shiller house price index, downloaded from FRED (series: CSUSHPISA). The homeownership rate and the inventory of renter-occupied housing are downloaded from the Census Bureau.[10] We estimate Equation 2 at the national level and quarterly frequency. The set of controls include quarter-of-year fixed effects, lags of the growth rate of the dependent variable, lagged PCE inflation, lagged unemployment rate, lagged changes in the mortgage rate, and lagged monetary policy shocks.

We find that contractionary monetary policy increases the inventory of renter-occupied housing, lowers the homeownership rate, and decreases house prices. A 25 bps increase in the 30-year fixed rate mortgage rate leads to a 0.7 percent increase in the number of renter-occupied housing, a 0.4 percent drop in the homeownership rate, and a 1.3 percent drop in house prices 12 month following the shock. Taken together, the increase in both rents and the quantity of renter-occupied housing suggests that contractionary monetary policy acts as a positive rental demand shock. When credit becomes more expensive,

[10] See https://www.census.gov/housing/hvs/data/histtabs.html.

household demand shifts from the owner-occupier market to the rental market, which lowers the homeownership rate and house prices and places upward pressure on rents. In Appendix B, we show that the increase in the inventory of renter-occupied housing is facilitated by real-estate investors who capitalize on the higher rents by buying houses from owner-occupiers and renting them out in the rental market. Namely, using transaction level data, we find that contractionary monetary policy leads to an increase in the volume of housing sold from owner-occupiers to real estate investors.

Figure 6: Alternative Housing Market Outcomes



(a) House Price (b) Homeownership Rate (c) Renter-Occupied Housing

Note: This figure displays the impulse response function of house prices (Panel (a)), the homeownership rate (Panel (b)) and the inventory of renter-occupied housing (Panel (c)) to a 25bps increase in the 30-year fixed rate mortgage. Dark (light) shaded areas represent 68% (90%) confidence intervals based on Newey and West (1987) standard errors.

5 Heterogeneous Effects of Monetary Policy

This section studies the heterogeneous effects of monetary policy across local housing markets. Given the tremendous variation across local housing markets, the aggregate effect of monetary policy on rents might mask substantial heterogeneity. Documenting this heterogeneity is important for understanding the circumstances under which monetary policy is more or less effective in stabilizing rent inflation as well as the distributional effects of monetary policy. The granularity and broad geographical coverage of our repeat-rent index allows us to shed light on these heterogeneous effects, which have so far been unexplored. We focus on three important sources of variation across housing markets: household credit constraints, landlords exposure to credit, and the degree of segmentation between owner-occupied and renter-occupied markets.

To study the heterogeneous effects of monetary policy across local housing markets, we estimate LP-IV regressions of the following form:

$$\log ADHRRI_{z,t+h} - \log ADHRRI_{z,t-1} = \beta^{(h)} i_t \times q_{z,t-1} + \gamma_t + \Gamma^{(h)} X_{z,t-1} + u^{(h)}_{z,t+h}, \quad (3)$$

for each horizon $h = \{0, 1, ..., 24\}$. As in Equation 2, the dependent variable is rent inflation in geography z between month $t-1$ and month $t+h$ (measured based on our ADH-RRI), and i_t is the interest rate on a 30-year fixed rate mortgage. $q_{z,t-1}$ is a time-varying measure of ex-ante heterogeneity across geographies, which we normalize to a unit variance measure for ease of interpretation. The coefficient of interest, $\beta^{(h)}$, measures how the effect of a monetary policy shock at time t varies with $q_{z,t-1}$. γ_t is a time fixed effect that controls for the aggregate economic environment (including the interest rate) at time t. $X_{z,t-1}$ is the set of geography level controls in Equation 2. We instrument $i_t \times q_{z,t-1}$ with the Bauer and Swanson (2023*b*) monetary policy shocks interacted with $q_{z,t-1}$.

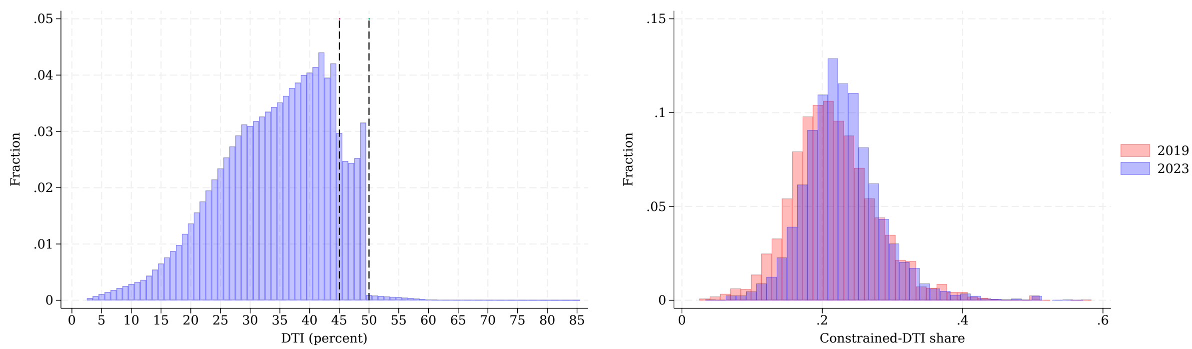
5.1 Debt-to-Income Constraints

We begin by exploring the role of household credit constraints for the transmission of monetary policy to rents. When interest rates increase, prospective homeowners might not qualify for mortgages and as a result crowd in the rental market. This is particularly true when borrowers are subject to debt-to-income (DTI) constraints - which cap the ratio of monthly debt payments to income. As highlighted by Greenwald (2018), higher mortgage rates directly increase mortgage payments, which raises DTI ratios towards underwriting limits. We therefore expect monetary policy shocks to have a greater impact on rental demand, and consequently on rents, when DTI constraints are more binding.

Borrowers in the U.S. are subject to institutional DTI underwriting limits (Greenwald, 2018; DeFusco, Johnson and Mondragon, 2020; Bosshardt et al., 2024). To illustrate this, Panel (a) of Figure 7 plots the distribution of DTI ratios on newly issued mortgages in the U.S., which we compile from the Home Mortgage Disclosure Act (HMDA) data. HMDA records the universe of mortgage originations across the country. For each mortgage it reports, for example, the year of origination, the DTI ratio, and the address of the underlying property. We focus on conventional first-lien purchase loans for single-family, owner-occupied, site-built properties. We restrict the sample period to 2018-2025 since the DTI ratio is not reported in earlier years. The DTI distribution exhibits sharp discontinuities at 45% and 50%. These thresholds correspond to underwriting limits for loans acquired by the government-sponsored enterprises (GSEs) Fannie Mae and Freddie Mac. As discussed in prior work, the 45% threshold appears to be a self-imposed GSE limit and the 50% threshold is an explicit strict limit for GSE loans (Greenwald, 2018; Bosshardt et al., 2024).

There is substantial cross-sectional variation across housing markets in the degree to which these institutional DTI limits bind. To illustrate this, we compute, for each zipcode and year, the share of borrowers whose DTI is just below the 45% limit - namely above 40% and below 45%. This share is our main proxy for how binding DTI constraints are in a given market at a given time. Intuitively, markets where a higher share of borrowers have a DTI just below the institutional limit are markets where DTI constraints likely bind for relatively more borrowers. Panel (b) of Figure 7 plots the distribution of this share, which we refer to as the "constrained-DTI share", across zipcodes in 2019 (in red) and in 2023 (in blue). It is evident that markets differ in how binding DTI constraints are: in some markets more than one in three borrowers are "DTI-constrained", while in some markets less than 5 percent of borrowers appear to be constrained. Comparing the 2023 distribution to the 2019 distribution shows that the share of DTI-constrained borrowers varies across time and is higher when interest rates are higher.

Figure 7: Debt-to-Income



(a) DTI

(b) Constrained-DTI share

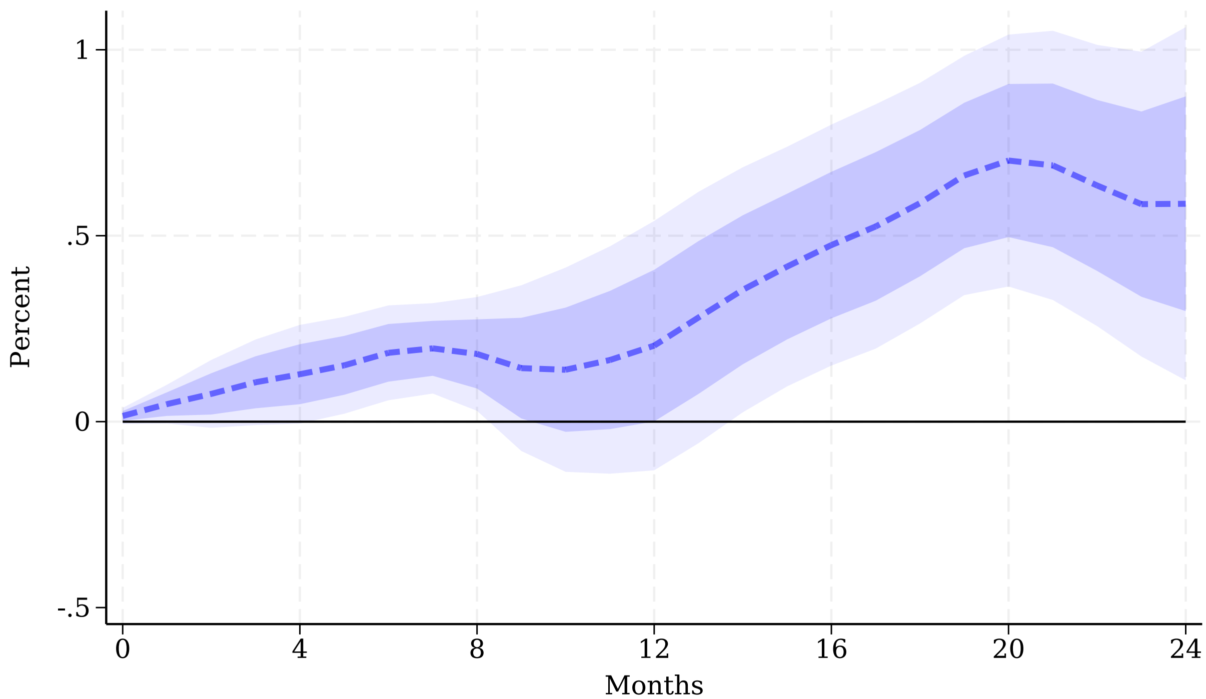
Note: Panel (a) displays the distribution of DTI ratios on newly issued loans across the U.S. measured based on HMDA data. See text for sample selection criteria. Panel (b) plots the distribution of the share of "constrained-DTI" borrowers across zipcodes in 2019 (in red) and in 2023 (in blue).

To explore how the effect of monetary policy varies with how binding DTI constraints are across local housing markets, we estimate Equation 3 where $q_{z,t-1}$ is the "constrained-DTI share" in zipcode z at month $t-1$.[11] Figure 8 illustrates the results. It plots the differential effect of an exogenous 25 basis point increase in the interest rate on a 30-year fixed rate mortgage that is associated with a one standard deviation increase in the share of DTI-constrained borrowers. We find that contractionary monetary policy shocks lead to a more pronounced increase in rents when and where DTI constraints are more binding. The same aggregate contractionary shock leads to a 0.6 percent higher rent inflation 24 months following the shock when the share of DTI-constrained borrowers is one standard deviation higher. A similar result is obtained when instead of using the share of

[11]Since the HMDA data tracks only the year of mortgage origination, we assign $q_{z,t-1}$ as the share of "constrained-DTI share" in the calendar year prior to month t.

borrowers whose DTI is just below the 45% limit, we use the share of borrowers whose DTI just below the institutional limit of 50% (namely between 45% and 50%) as our proxy for how binding DTI constraints (see Appendix Figure C.6). When more borrowers are ex-ante constrained by DTI limits, monetary tightening likely causes a more pronounced shift in household demand towards the rental market, resulting in a more pronounced upward pressure on rents.

Figure 8: Heterogeneous Effects - DTI Constraints



Note: This figure plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation, for each horizon h, associated with a one standard deviation increase in the share DTI-constrained borrowers. Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

A potential concern is that the extent to which DTI constraints bind in a local housing market correlates with other determinants of the effect of monetary policy on rents, such as the employment rate or the racial composition of households. This could confound the interpretation of DTI constraints as driving the differential effect documented in Figure 8 . While we cannot perfectly isolate variation in the share of constrained-DTI borrowers, we offer various arguments that support our interpretation that DTI constraints are an important driver of the effect of monetary policy on rents. First, we control for all time-invariant market-level characteristics across housing markets by including zipcode fixed effects in Equation 3. Second, we control for a host of time-varying zipcode-level characteristics that are included in $X_{z,t-1}$. Third, we consider three placebo tests. Specifically, we estimate Equation 3 for the case where $q_{z,t}$ is defined as (1) the average DTI ratio in zipcode z at month t, (2) the share of borrowers whose DTI ratio is between 37% and 41%, and (3) the share of borrowers whose DTI ratio is between 38% and 42%. The results, illustrated in Appendix Figure C.7, show no statistically distinguishable differential effect associated with these alternative DTI measures. Overall, the effect of monetary policy on rents varies with the share of DTI-constrained borrowers but not with other moments of the DTI distribution. This serves to show that a local housing market characteristic that correlates with local measures of DTI is unlikely to confound the interpretation of DTI constraints as driving the differential effect - unless it specifically correlates with the share of borrowers with DTI just below the institutional limit.

5.2 Landlord Credit Exposure

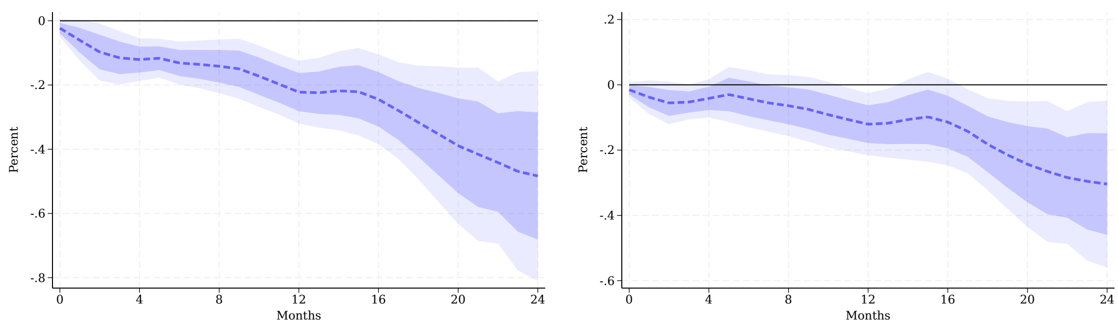
Monetary policy shocks can induce not only a shift in the rental demand curve, but also a shift in the rental supply curve. When the cost of credit increases, financing rental properties becomes more expensive and the rental supply curve can shift upwards. In this section, we evaluate how differences in local landlords' credit exposure matters for the transmission of monetary policy to rents. We show that areas where landlords rely more heavily on credit as a source of financing experience greater rent increases following the same contractionary shock.

To evaluate how landlord exposure on credit matters for the effect of monetary policy on rents, we use data from Cotality. Cotality compiles the universe of housing transactions in the U.S. For each transaction, the data records, for example, the property address, the mailing address and full names of the buyers and sellers, the date of the transaction, and whether the acquisition was financed with a mortgage. We limit our sample to apartments, single family residences, condominiums, and duplexes, and to arms-length transactions.

We consider various classifications of buyers. First, for each transaction, we classify the buyer as either an owner-occupier or as an investor. A buyer is classified as an owner-occupier if her mailing address is the same as the property address and as an investor otherwise. Second, we classify buyers based on whether or not they are cash-buyers. A buyer is classified as a cash-buyer if she does not take a mortgage to finance the acquisition. Third, buyers are classified by Cotality as corporate buyers if their name indicates that they are likely a corporation.

Our main proxy for landlord exposure to credit in a given market is the share of investor acquisitions where the investor is a cash-buyer. In markets where this share is higher, landlords rely less on credit to finance their rental properties and are hence less sensitive to fluctuations in the cost of credit. As a second proxy for landlords reliance on credit, we compute the share of corporate acquisitions where the corporate buyer is a cash buyer. We compute both these shares for each zip and year between 2011 and 2024. We then estimate Equation 3 for the case where $q_{z,t-1}$ is the share of cash buyers among investors in zipcode z during the calendar year prior to month t, and similarly for the case where $q_{z,t-1}$ is the share of cash buyers among corporate buyers in zipcode z during the calendar year prior to month t.

Figure 9: Heterogeneous Effects - Landlord Exposure to Credit



(a) Share investors who are cash-buyers (b) Share corporate buyers who are cash-buyers

Note: Panel (a) (Panel (b)) plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation, for each horizon h, associated with a one standard deviation increase in share cash buyers among investors (corporate buyers). Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

Figure 9 plots the results. A contractionary monetary policy shock leads to a less pronounced increase in rents when and where landlords rely less heavily on credit to finance rental properties. The same contractionary shock leads to a 0.45 (0.3) percent lower rent inflation 12 months following the shock when the share of investors (corporate buyers) who are cash buyers is one standard deviation higher. When less landlords use debt as a source of financing, a contractionary monetary policy shock likely induces a less pronounced upward shift in the rental supply curve, which can explain why it leads to a lower increase in rents.

5.3 Segmentation

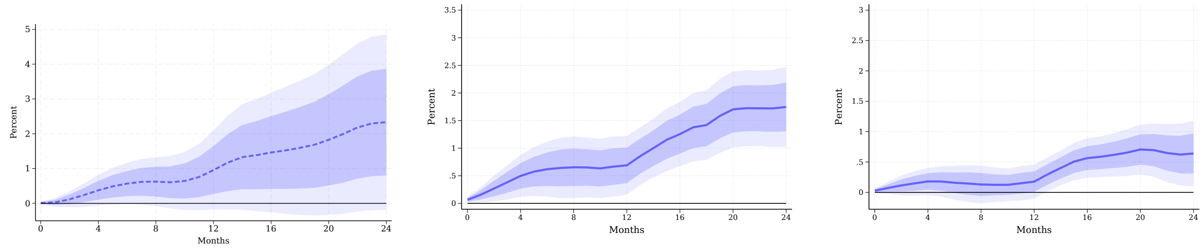
The results in Section 4 suggest that contractionary monetary policy shocks shift household demand from the owner occupier market to the rental market. The responsiveness of rents to a shock that influences relative demand for homeownership depends on the elasticity of the rental supply curve (Rotberg and Steinberg, 2024; Greenwald and Guren, 2025). As highlighted by Greenwald and Guren (2025), the rental supply elasticity in turn depends on the degree of segmentation in housing markets - the ease in which housing units can be converted between owner-occupied and renter-occupied markets. The more segmented owner and renter markets are, the less elastic the rental supply curve, and the stronger the pass-through of the shock to rents. We therefore expect monetary policy shocks to have a greater impact on rents in areas (times) where (when) owner-occupied and renter-occupied housing markets are more segmented.

A main source of housing segmentation is the suitability of different properties for rentals (Halket, Nesheim and Oswald, 2020; Greenwald and Guren, 2025). In particular, multifamily units can be maintained and managed as rentals more effectively than single-family homes that tend to depreciate faster, are less standardized, and are more prone to moral hazard concerns. Based on this rational, we proxy the degree of segmentation in a local market with the share of single family housing units in that market. To examine how differences in segmentation matter for the responsiveness of rents to monetary policy shocks, we estimate Equation 3 where $q_{z,t-1}$ is the share of single family units in zipcode z at month $t-1$, measured from ACS data. Since the ACS data is provided at an annual frequency, we assign $q_{z,t-1}$ to be the share of single-family housing units in the calendar year prior to month t.

Panel (a) of Figure 10 illustrates the results. It plots the differential effect of an exogenous 25 basis point increase in the interest rate on a 30-year fixed rate mortgage that is associated with a one standard deviation increase in our segmentation proxy. A contractionary monetary policy shock leads to a more pronounced increase in rents when and where local housing markets are more segmented. The same contractionary shock leads to a 1 (2) percent higher rent inflation 12 (24) months following the shock when the share of single family housing units is one standard deviation higher. When properties are less suitable to be converted to rentals, the supply of rentals is less elastic and a demand shock induced by monetary tightening translates to a larger increase in rents.

To further explore the role of segmentation, we separately estimate the effect of monetary policy on single-family rent inflation and on multi-family rent inflation. That is, we estimate Equation 2 for the case where rent inflation is measured based on our single-family (multi-family) ADH-RRI. If segmentation matters for the effect of monetary policy on rents, we would expect the effect to be greater in the single-family rental market, where it is more costly to convert homes between the owner-occupied and renter-occupied markets. As illustrated in panels (b) and (c) of Figure 10, we find that this is indeed the case.

Figure 10: Heterogeneous Effects - Segmentation



(a) Heterogeneous Effects - SF Share (b) SF (c) MF

Note: Panel (a) plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation, for each horizon h, associated with a one standard deviation increase in share of single family homes. Panel (b) (Panel (c)) plots the impulse response function of single-family (multi-family) rent inflation to a 25bps increase in the 30-year fixed rate mortgage. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

5.4 Mortgage Lock-In

Part of the increase in demand for rentals following a contractionary monetary policy shock may stem from mortgage lock-in. When mortgage rates rise, existing homeowners who locked into low mortgage rates are less likely to move because selling their home and buying a new one would require prepaying their outstanding loan balance and remortgaging at a higher rate (Quigley, 1987; Ferreira, Gyourko and Tracy, 2010; Fonseca and Liu, 2024; Batzer et al., 2024; Liebersohn and Rothstein, 2025; Aladangady, Krimmel and Scharlemann, 2024). This lock-in can reduce the supply of houses for sale, which can potentially increase house prices (Mabille, Liu and Fonseca, 2024; Gerardi, Qian and Zhang, 2024) and as a result increase demand in the rental market and drive up rents (De la Roca, Giacoletti and Liu, 2024). In this section, we evaluate the role of mortgage lock-in for the transmission of monetary policy shocks to the rental market.

Our data for this apparatus is the universe of mortgage originations in the U.S. compiled by Cotality. For each mortgage originated between January 1990 and September 2024, the data records, for example, the date of origination, the mortgage terms, the type of property the mortgage was issued against, a property identifier that allows tracking the same property across subsequent mortgage originations, the property address, whether the mortgage is a fixed or adjustable rate mortgage, whether the mortgage is associated with an arms-length transaction, refinancing, or a non-arms-length transaction, and whether the borrower is a corporation. We limit our sample to conventional 30-year fixed-rate mortgages that are originated against apartments, single family residences, condominiums, or duplexes, and where the borrower is not a corporation. We include both arms-length transactions and refinances.

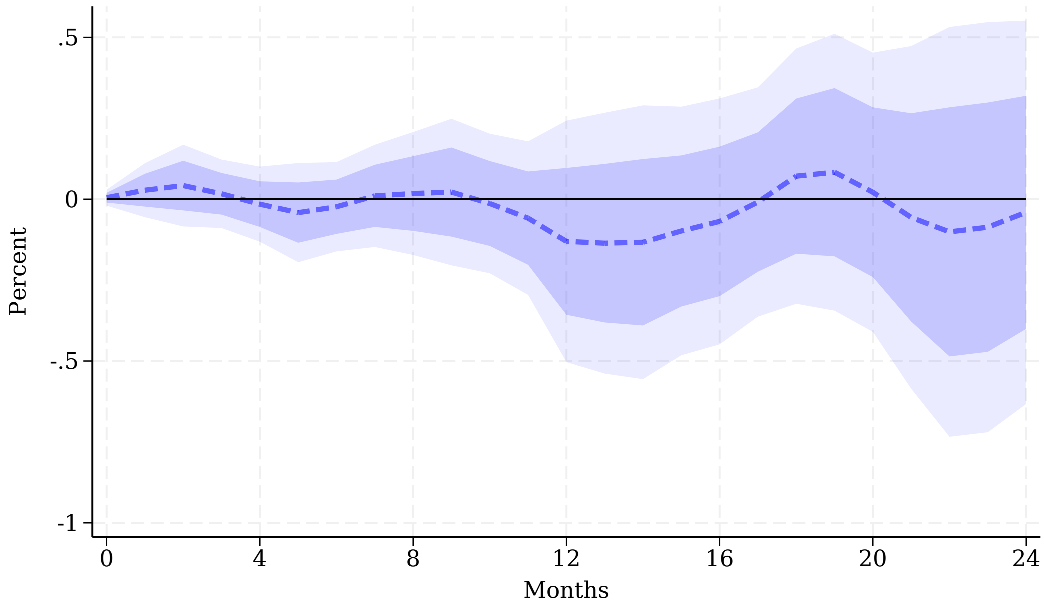
To study the role that mortgage lock-in plays in the transmission of monetary policy to rents, we compute, for each zipcode and month, the share of homeowners who are "locked in". We define a homeowner as "locked-in" if she has an outstanding mortgage and if the difference between the prevailing market mortgage rate and the mortgage rate she locked at origination (which Fonseca and Liu (2024) term the "mortgage delta") is negative. The idea is that for locked-in homeowners (but not for non locked-in homeowners) the financial cost of moving includes the increase in mortgage payments associated with prepaying the current mortgage and remortgaging at a higher rate. Since these additional remortgaging costs increase with interest rates, the same aggregate contractionary monetary policy increases moving costs for relatively more incumbent homeowners in markets where more homeowners are locked-in. If monetary policy shocks impact rents by affecting moving costs for locked-in homeowners, we would expect the effect of monetary policy on rents to be more pronounced in markets where more owners are locked-in.

We estimate Equation 3 for the case where $q_{z,t-1}$ is the share of homeowners who are locked-in. To construct this lock-in measure, we proceed as follows. We classify a mortgage as outstanding as long as it has not reached its maturity and as long as another senior mortgage on the same property was not originated. We classify a mortgage as having a negative "mortgage delta" if the mortgage rate upon origination is lower than the prevailing market mortgage rate. We proxy the prevailing market mortgage rate with the Freddie Mac 30-year fixed mortgage rate. Similarly, we proxy the mortgage rate upon origination with the Freddie Mac 30-year fixed mortgage rate at the time of origination.[12] We then divide the number of outstanding mortgages with a negative mortgage delta by the total number of housing units in each zipcode, which is measured annually from the ACS data and is assumed to be constant within each calendar year.

Figure 11 plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation that is induced by a one standard deviation increase in the share of locked-in homeowners. We find that the effect of monetary policy shocks on rents is not more pronounced in markets where a larger share of homeowners are locked-in. This suggests that the mortgage lock-in channel plays a limited role in the transmission of monetary policy to rents. It is worth emphasizing that this result does not necessarily contradict previous findings on the effect of mortgage lock-in on mobility, house prices, and rents. Rather, it implies that aggregate monetary policy shocks affect rents primarily through other channels.

[12] Cotality records information on loan-specific mortgage rates but for the vast majority of loans this field is missing in the data. Our mortgage delta measure is hence best interpreted as the "aggregate mortgage delta" in Fonseca and Liu (2024).

Figure 11: Heterogeneous Effects - Mortgage Lock-In



Note: This figure plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation, for each horizon h, induced by a one standard deviation increase in the share of locked-in homeowners. Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the county and quarter level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

6 Conclusion

This paper provides new causal estimates of the effects of monetary policy on housing rents. We construct comprehensive measures of rent inflation at a micro-geographic scale and at a high frequency. Our repeat-rent index is the most granular and most comprehensive high-frequency rent index to date. Employing standard local projections methods, we find that , on average, contractionary monetary policy increases both real and nominal rents. However, the aggregate effect masks tremendous heterogeneity across local housing markets. Contractionary shocks have greater impact in markets (times) where (when) households are more constrained by debt-to-income limits, renter and owner markets are more segmented, and landlords rely more heavily on credit as a source of financing.

Our results have important policy implications. The fact that monetary tightening typically increases rent, which is the single largest component of the Consumer Price Index (CPI), presents a challenge to policymakers. However, a silver lining is that the effect of monetary policy on rents can flip under certain economic conditions. Monetary policy can be more effective in curbing inflation in times when household borrowing constraints are less binding, when there is less segmentation between renter and owner markets, and when landlords are less levered.

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Internet Appendix

A Data Sample Construction

We apply several filters to the raw Altos data to arrive at our final sample. Appendix Table A.1 summarizes the number of observations remaining after each of these filters. First, we drop observations where the address, the date, or the listed rent are missing. Second, we standardize street addresses and drop observations for which the standardized address is incomplete. The address standardization process involves standardizing street suffix abbreviations based on the United States Postal Service (USPS) abbreviations dictionary, standardizing house number suffixes, and truncating unit numbers.[13] Once street addresses have been standardized, we keep only addresses that have complete information on the street name and house number. Third, we drop addresses for which the geocodes are missing or for which the geocodes do not uniquely identify a street address in the data. Fourth, we exclude short-term and vacation rentals, commercial properties, mobile homes, listings of individual rooms, as well as listings for which the unit type is missing.

Fifth, we standardize the number of beds and number of baths. The standardization process involves assigning missing values to the number of beds (baths) in cases where the number of beds (baths) is larger than 10 or is not a multiple of 0.5 (0.25). We then collapse all listings that appear within the same week and have the same address, same number of beds and baths, and same listed rent to one observation. These cases likely reflect duplicate listings for the same unit across different listing platforms or within the same listing platform. While it might be that multiple different units within the same building that feature the same number of beds and baths are listed for rent during the same week, the fact that these units are listed for the same price suggests they are of the same quality. As such, they do not contribute differentially to the repeat-rent index.

Sixth, for each building address, we infer whether the structure is a single-family house or a a multi-family building. We categorize addresses as corresponding to single-family houses if (1) more than half of the listings associated with the address specify that the unit type is "house" and (2) we never observe multiple listings associated with the address within the same week that list different rents. These conditions suggest that there is only one housing unit in that address. Remaining addresses are classified as corresponding to multi-family units. We drop observations of multi-family buildings that

[13] For some listings, the data does record the unit number, but this field is typically missing.

have missing number of beds or missing number of baths.

Seventh, for the purpose of constructing a repeat-rent index, we identify units by their street address, number of beds and number of baths. The validity of our repeat-rent index as a quality-constant measure of rent growth therefore relies on units within the same building that have the same number of beds and baths also being of the same quality. We therefore drop units that likely differ in their quality but that are undistinguishable based on their address, number of beds and number of baths. We begin by identifying all tuples of address, number of beds and number of baths for which we observe, within a same week, multiple listings with different prices. We drop observations associated with these tuples. We then identify tuples of address, number of beds and number of baths for which the (unique) weekly rent often fluctuates between consecutive weeks, and drop all observations associated with these tuples.[14]

Eight, we drop outliers. We drop listings with rents that are above the 97.5 percentile or below the 2.5 percentile of contract rents in the AHS.[15] We also drop tuples of address, number of beds and number of baths for which we observe a 4-week (52-week) rent fluctuation, in absolute value, that exceeds the 95th percentile (99th percentile) of the 4-week (52-week) rent fluctuation distribution in the data.

Ninth, we identify and drop listings that likely do not correspond to vacant units. In particular, we define a listing spell as the number of weeks a listing consecutively appears on the market without a break that is longer than 4 weeks. We then truncate the duration of spells where the listed rent is constant throughout the spell to the 90th percentile of spell durations throughout the sample. These cases likely reflect listings that were not taken off the market despite the underlying unit being rented.

In addition to the filters we apply to the raw data, we construct a monthly panel of listed rents, identified at the unit level by collapsing all listings of the same unit that appear within the same month to one observation. Namely, we keep the latest observation within the month. Units that are not listed in more than one month are excluded, since they do not inform the repeat-rent index. We drop zipcodes-months in which less than 5 units are listed. Our final panel data contains 48.1 million monthly observations of listed rents. It comprises 10.0 million rental units across 14,459 zipcodes. Each rental units is observed on average for approximately 4.8 months during the sample. The average time

[14]Namely, we drop all observations associated with tuples of address, number of beds, and number of baths for which we observe, at least in three occasions throughout our sample period, the weekly rent fluctuating between two rents within a 4-week period.

[15]For each of the years 2015, 2017, 2019, 2021, and 2023, we compute the 97.5 and 2.5 percentile in the corresponding AHS survey, and drop all listings with rents that are above the 97.5 percentile or below the 2.5 percentile in that year. For all remaining years, in which AHS data is unavailable, we use the (inflation-adjusted) percentiles computed from the most proximate AHS survey.

Table A.1: Sample Selection

#	Filter	# Listings in Millions (% Raw)
0	-	708.9 (100.0)
1	Drop missing address/rent/date	708.2 (99.9)
2	#1 + Drop incomplete standardized address	688.3 (97.1)
3	#2 + Drop missing or non-unique geocodes	681.0 (96.1)
4	#3 + Drop short-term/mobile/commercial/room/missing type	680.1 (95.9)
5	#4 + Drop duplicates of {week,address,beds,baths,price}	560.9 (79.1)
6	#5 + Drop multi-family with missing beds/baths	505.2 (71.3)
7	#6 + Drop units non-distinguishable by {address,beds,baths}	209.5 (29.6)
8	#7 + Drop outliers (extreme rent/rent fluctuations)	169.1 (23.9)
9	#8 + Drop non-vacant units	147.1 (20.8)

Note: This table reports the number and share of listings remaining after each data filter.

on market (i.e. the consecutive number of months a unit is being listed) is 2 months.

A.1 Hedonic Rent Index

Using our listings data, we construct a hedonic rent index. The advantage of the hedonic rent index over the repeat-rent index is that, since it does not require identifying rental units, it is based on more observations and is constructed for more zipcodes. The downside is that it does not provide a quality-constant measure of rent growth. We use the hedonic index to assess the robustness of our results to alternative rent indexes (Section 4.2). This section discusses the construction of the hedonic index.

The sample used for the construction of the hedonic index is less restrictive than the sample used for constructing the ADH-RRI. In particular, we do not apply filter #7 in Table A.1 since the hedonic index does not require identifying rental units across the sample. For the same reason, filter #8 is less restrictive - we do not drop tuples of address, number of beds and number of baths for which we observe a 4-week (52-week) rent fluctuation, in absolute value, that exceeds the 95th percentile (99th percentile) of the 4-week (52-week) rent fluctuation distribution in the data.

Finally, we collapse all listings that have the same address, number of beds and baths that appear within the same month to one observation. This is meant to avoid double-counting the same listings multiple times within a month. We drop zipcodes-months in which there are less than 5 observations. Our final panel data for the hedonic regression contains 112.4 million monthly observations of listed rents across 15,550 zipcodes.

We construct the hedonic index by estimating the following hedonic regression for each zipcode z:

$$\log P_{i,z,t} = \alpha_z + \gamma_{z,t} + \Gamma_z X_{i,t} + \varepsilon_{i,z,t}, \tag{4}$$

where $P_{i,z,t}$ is the rent for listing i in zipcode z at month t, α_z is a constant, and $X_{i,t}$ is a vector of unit quality controls. As controls, we include an indicator for the property type (whether the property is single family or multi-family), indicators for the number of bedrooms and baths, for the age of the property and for the unit size (in square footage), and a month-of-year fixed effect to control for seasonality. We also include two-way interactions between the property type and all other characteristics (bedrooms, baths, age, and size); a two-way interaction between the number of beds and baths; a three-way interaction between the number of beds, number of baths, and property type; a three-way interaction between the number of beds, number of baths, and size; a three-way interaction between the number of beds, number of baths, and age; and a three-way interaction between property type, age, and size. The estimated parameters $\gamma_{z,t}$ represent the percentage change in listed rents relative to the base (omitted) month, controlling for all observable characteristics. The exponent of these estimates constitute the hedonic rent index, where we normalize the value of the index in the base period to 100 and smooth the index by taking a 3-month moving average. That is, the hedonic ADH is given by $\frac{1}{3}\Sigma_{k=0}^{2} 100 \exp(\gamma_{z,t-k})$.

B Effect of Monetary Policy on Housing Transactions

This section explores how monetary policy shocks affect transactions in the housing market. We find that monetary tightening leads to a pronounced shift in the composition of housing transactions. The share of acquisitions where the buyer is a real-estate investor and the seller is an owner-occupier increases, while the share of acquisitions where the buyer is an owner-occupier and the seller is an investor drops. This net increase in the volume of housing sold from owner-occupiers to investors can explain the increase in the inventory of renter-occupied housing following contractionary monetary policy shocks (Section 4.3).

To analyze the impact of monetary policy on housing sales, we use data on the universe of housing transactions in the U.S. compiled by Cotality. Section 5.2 describes the data and sample selection criteria. We consider various classifications of buyers and sellers. First, for each transaction, we classify the buyer as either an owner-occupier or non-occupier. A buyer is defined as an owner-occupier if her mailing address is the same as the property address and as a non-occupier otherwise. Similarly, a seller is defined as an owner-occupier if her mailing address at the time she bought the property was the same as the property address and as a non-occupier otherwise. Second, we classify a buyer as a cash-buyer if she does not take a mortgage to finance the acquisition. Third, we classify buyers as first-time buyers if they have not bought or sold a property in the same county in the past. Finally, buyers are classified by Cotality as corporate buyers if their name indicates that they are likely a corporation.

Due to the infrequent nature of housing transactions, our analysis of housing transactions is conducted at the quarter frequency and at the county level. For each county and quarter between 2000Q1 and 2024Q2, we compute the total number of transactions and the composition of these transactions based on the classification of the buyer and sellers. To mitigate noise, we restrict our sample to counties that historically have a sufficiently high volume of transactions. For each county, we compute the average number of quarterly transactions across the sample and restrict the analysis to counties that are above the median.

We estimate the dynamic effects of a monetary policy shock on housing sales using the LP-IV framework described above. In similar fashion to Equation 2, we estimate the following LP regression via two-stage least squares:

$$Y_{c,t+h} - Y_{c,t-1} = \beta^{(h)} i_t + \Gamma^{(h)} X_{c,t-1} + u^{(h)}_{c,t+h}, \tag{5}$$

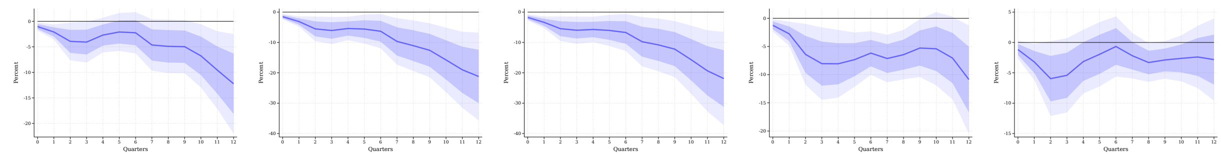
for each horizon $h = \{0, 1, ..., 8\}$. The dependent variable is either the cumulative growth rate of the *volume* of different types of transactions in county c between quarter $t-1$ and quarter $t+h$ or the cumulative percentage point change in the *share* of different types of transactions. For both volumes and shares, we consider transactions where the buyer is an owner-occupier, an investor, a first-time buyer, a cash-buyer, and a corporation. We also consider transactions where the buyer is an owner-occupier and the seller is an investor (which we refer to as "owner-to-investor" transactions), where the buyer is an owner-occupier and the seller is an owner-occupier ("owner-to-owner"), where the buyer is an investor and the seller is an investor ("investor-to-investor"), and where the buyer is an investor and the seller is an owner-occupier ("investor-to-owner"). Following Gertler and Karadi (2015), we compute the quarterly monetary policy shock by first summing, for each of the three months within the quarter, the Bauer and Swanson (2023*b*) monetary surprises over the preceding three months, and then averaging this sum across the three months. The monetary policy indicator is computed as the average 30-year fixed rate mortgage within the quarter. $X_{c,t-1}$ is a set of controls which include county quarter-of-year fixed effects, lags of the dependent variable, lags of the change in the county unemployment rate, lagged PCE inflation, and lagged changes in the monetary indicator i_t.[16]

Figure B.1 plots the impulse response function of the *volume* of different types of housing transactions to an exogenous 25 basis point increase in the interest rate on a 30-year fixed rate mortgage. As illustrated in Panel (a), a contractionary monetary policy shock leads to a drop in the overall transaction volume - a hypothetical 25 basis point increase in the 30-year fixed rate mortgage lowers the volume of sales by approximately 5% two years following the contractionary shock. Panels (b)-(j) suggest that the overall drop in transaction volume masks important compositional effects. The volume of all types of housing transactions drops when monetary policy tightens, but the drop is not uniform across the different types of transactions.

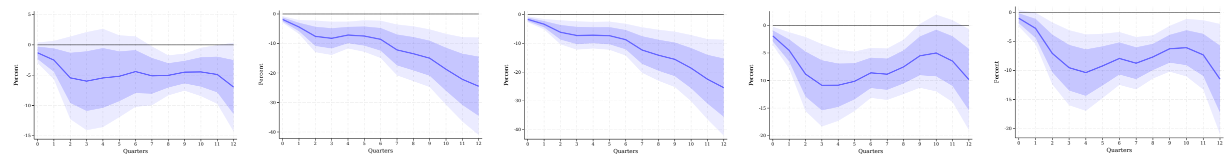
To quantify these compositional effects, Figure B.2 plots the impulse response function of the *shares* of different types of housing transactions to an exogenous 25 basis point increase in the interest rate on a 30-year fixed rate mortgage. As illustrated in panels (a) and (b), the share of transactions where the buyer is an owner-occupier or a first-time buyer drops in response to monetary tightening. At the same time, the share of transactions where the buyer is an investor (panel (c)), a cash-buyer (panel (d)), or a corporation (panel (e)) sharply increases. For example, a 25 basis point increase in the 30-year fixed rate

[16] We include 4 lags of the quarterly growth rate of the number of transactions, one lag of the year-over-year change in the quarterly unemployment rate at the county level, one lag of the year-over-year quarterly PCE inflation, and one lag of the quarterly change in the monetary policy indicator.

Figure B.1: Effect of Monetary Policy on Transaction Volume



(a) Overall (b) Owner-Occupier (c) First-Time Buyer (d) Investor (e) Cash Buyer



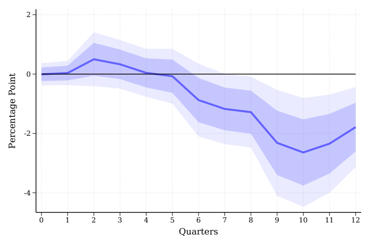
(f) Corporate Buyer (g) Owner-to-Owner (h) Investor-to-Owner (i) Owner-to-Investor (j) Investor-to-Investor

Note: This figure plots the impulse response function of the percent change in the volume of different types of housing transactions to an exogenous 25 basis point increase in the 30-year fixed rate mortgage. Panel (a) is for total transactions, Panel (b) is for transactions where the buyer is an owner-occupier, panel (c) is for transactions where the buyer is an first-time buyer, panel (d) is for transactions where the buyer is an investor, panel (e) is for transactions where the buyer is an cash buyer, panel (f) is for transactions where the buyer is a corporate buyer, panel (g) is for transactions where the buyer is an owner-occupier and the seller is an owner-occupier, panel (h) is for transactions where the buyer is an owner-occupier and the seller is an investor, panel (i) is for transactions where the buyer is an investor and the seller is an owner-occupier, and panel (j) is for transactions where the buyer is an investor and the seller is an investor. Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the county and quarter level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

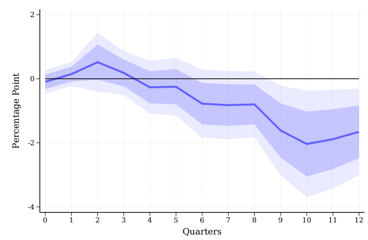
mortgage lowers (increases) the share of owner-occupier (investor) purchases by roughly 2 percentage points 2 years following the shock. This result is intuitive. Higher borrowing costs deter financially constrained households from buying homes, while deep-pocketed real-estate investors are less impacted by borrowing costs.

The finding that owner-occupiers buy relatively less houses compared to real-estate investors in response to contractionary shocks does not immediately suggest that the inventory of rental housing increases. For example, if owner-occupiers also sell relatively less houses compared to real-estate investors in response to contractionary shocks, then the share of housing units owned by investors might not change. To more directly examine how monetary policy shapes the inventory of rental housing, Panels (f)-(i) of Figure B.2 plot the impulse responses of the share of "owner-to-owner", "owner-to-investor", "investor-to-owner", and "investor-to-investor" transactions. The main takeaway is that the share of transactions where the buyer is an owner-occupier and the seller is an investor drops in response to contractionary shocks (panel (g)), while the share of transactions where the buyer is an investor and the seller is an owner-occupier increases (panel (h)). Overall, these results suggest that monetary tightening shifts household demand from the owner-occupied market to the rental market, and that this shift in demand is accommodated by real-estate investors who increase their rental inventory to capitalize on the higher rents.

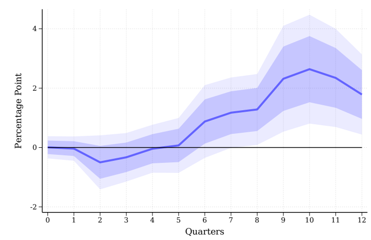
Figure B.2: Effect of Monetary Policy on Transaction Shares



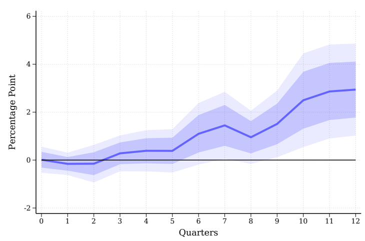
(a) Owner-Occupier



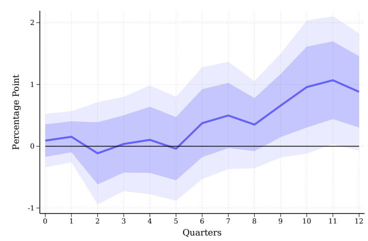
(b) First-Time Buyer



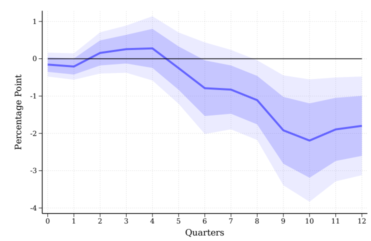
(c) Investor



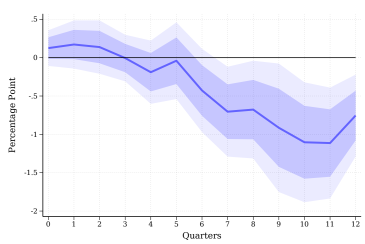
(d) Cash Buyer



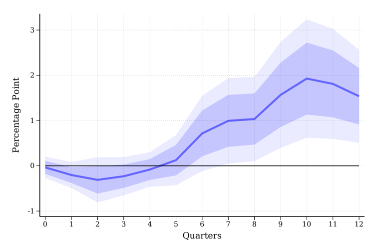
(e) Corporate Buyer



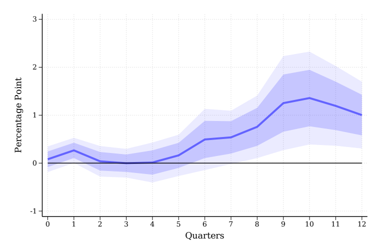
(f) Owner-to-Owner



(g) Investor-to-Owner



(h) Owner-to-Investor



(i) Investor-to-Investor

Note: This figure displays the impulse response function of the percentage point change in the share of different types of housing transactions to an exogenous 25 basis point increase in the 30-year fixed rate mortgage. Panel (a) is for transactions where the buyer is an owner-occupier, panel (b) is for transactions where the buyer is an first-time buyer, panel (c) is for transactions where the buyer is an investor, panel (d) is for transactions where the buyer is an cash buyer, panel (e) is for transactions where the buyer is a corporate buyer, panel (f) is for transactions where the buyer is an owner-occupier and the seller is an owner-occupier, panel (g) is for transactions where the buyer is an owner-occupier and the seller is an investor, panel (h) is for transactions where the buyer is an investor and the seller is an owner-occupier, and panel (i) is for transactions where the buyer is an investor and the seller is an investor. Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the county and quarter level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

C Additional Figures and Tables

Table C.1: Listed and Contract Rents

Year	Average Difference (%)	Median Difference (%)	Share Match
2011	1.39	0.00	68.2
2012	1.00	0.00	72.2
2013	0.78	0.00	75.2
2014	0.65	0.00	77.8
2015	0.56	0.00	79.0
2016	0.51	0.00	78.9
2017	0.48	0.00	79.7
2018	0.44	0.00	80.2
2019	0.38	0.00	80.2
2020	0.39	0.00	80.1
2021	0.02	0.00	81.3
2022	0.08	0.00	80.7
2023	0.24	0.00	80.6
2024	0.26	0.00	80.4
2025	0.18	0.00	81.2

Note: The second (third) column reports the average (median) percentage difference, in absolute value, between listed and contract rents for each year between 2011 and 2025. The fourth column reports the share of listings where the listing and contract rent are equal. Tabulations are based on MLS data.

Table C.2: ADH-RRI vs. ZORI

	OLS			
	Log ADH-RRI		Δ_{YoY} Log ADH-RRI	
	(1)	(2)	(3)	(4)
Log ZORI	0.868***	0.889***	—	—
	(0.003)	(0.002)		
Δ_{YoY} Log ZORI	—	—	0.605***	0.593***
			(0.005)	(0.005)
Zip Code FEs	No	Yes	No	Yes
R-squared	0.917	0.969	0.464	0.521
Observations	359,332	359,332	294,805	294,785

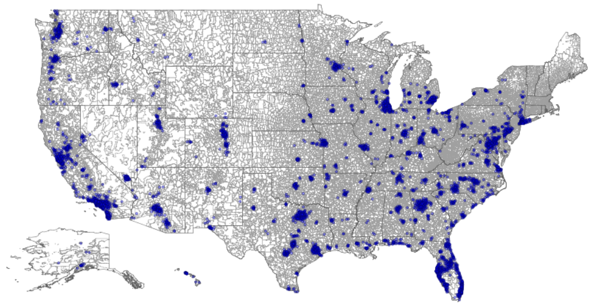
Note: This table reports coefficients from regressing log ADH-RRI on log ZORI (columns 1-2) and year-over-year changes in log ADH-RRI on year-over-year changes in log ZORI (columns 3-4). Both indexes are at the zipcode level and are normalized to 100 in the first month both are available. Even-numbered columns include zipcode fixed effects.

Table C.3: Lag Exogeneity

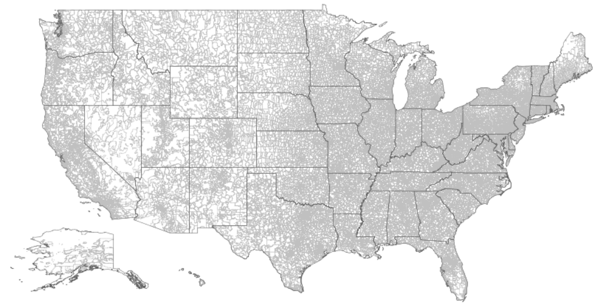
	$Z_t^{\perp}$	
	(1)	(2)
$\{\Delta \log ADH^{\perp}_{z,t-k}\}_{k=1}^{12}$	✓	.
$\{\Delta \log ADH^{\perp}_{z,t-k}\}_{k=1}^{24}$	.	✓
R-squared	0.001	0.006
Observations	413,223	338,965

Note: The first (second) column of this table reports the R-square from regressing the orthogonalized monetary policy shock on 12 (24) orthogonalized lags of the dependent variable in Equation 2, for the case where the dependent variable is nominal rent growth. Orthogonalizing is done against the set of controls $X_{z,t-1}$.

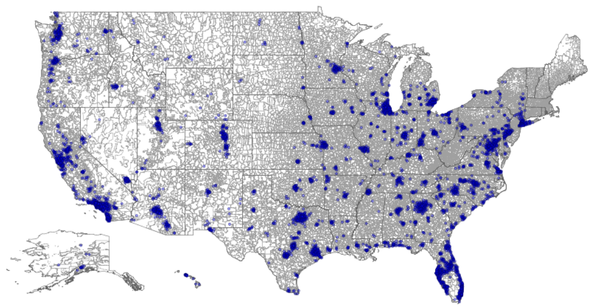
Figure C.1: ADH-RRI vs. ZORI: 2012-2024



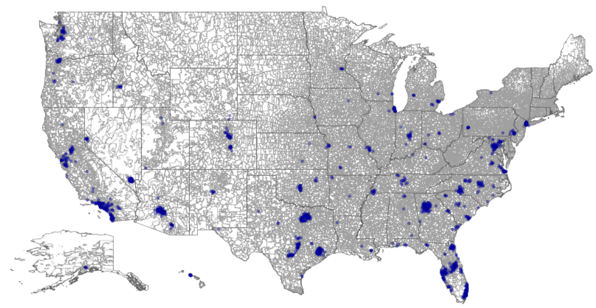
(a) ADH-RRI, 2012 (5,423 zipcodes)



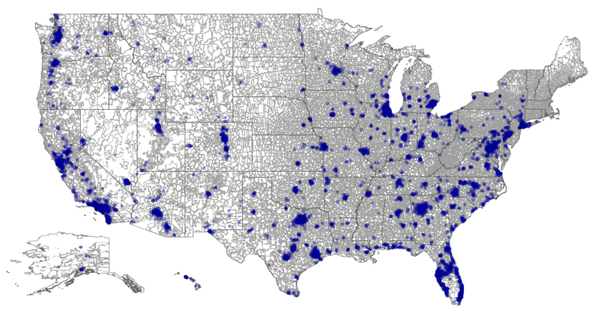
(b) ZORI, 2012 (0 zipcodes)



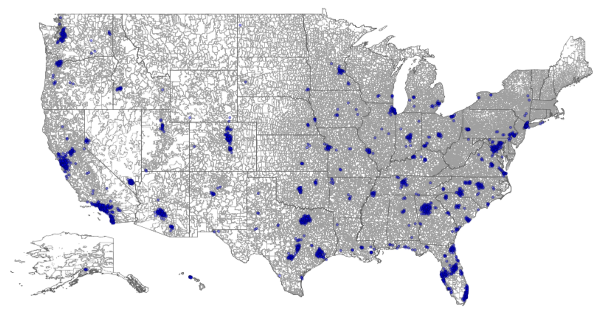
(c) ADH-RRI, 2016 (5,527 zipcodes)



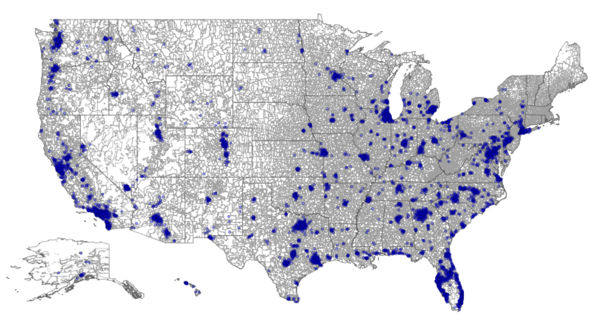
(d) ZORI, 2016 (1,502 zipcodes)



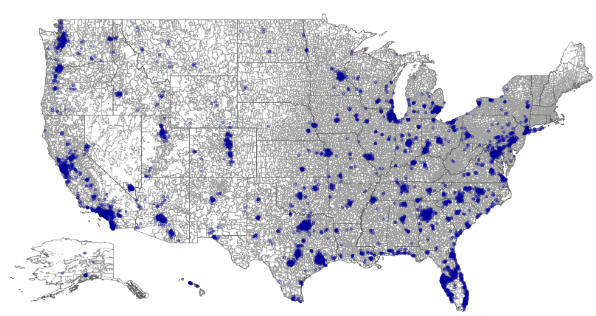
(e) ADH-RRI, 2020 (5,533 zipcodes)



(f) ZORI, 2020 (2,542 zipcodes)



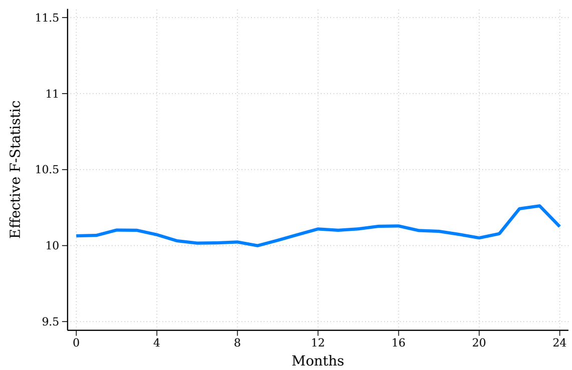
(g) ADH-RRI, 2024 (5,528 zipcodes)



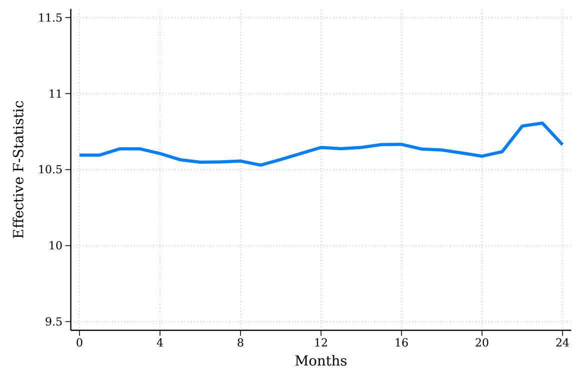
(h) ZORI, 2024 (5,962 zipcodes)

Note: This Figure displays, in blue, the zipcodes that are covered by the ADH-RRI and by ZORI in 2012, 2016, 2020 and 2024. The number of covered zipcodes is in parenthesis.

Figure C.2: First-Stage F-Statistic



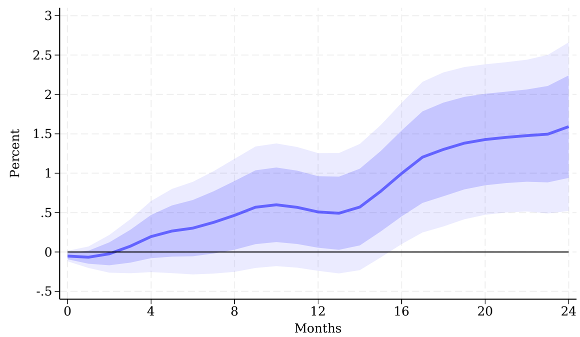
(a) Real Rent



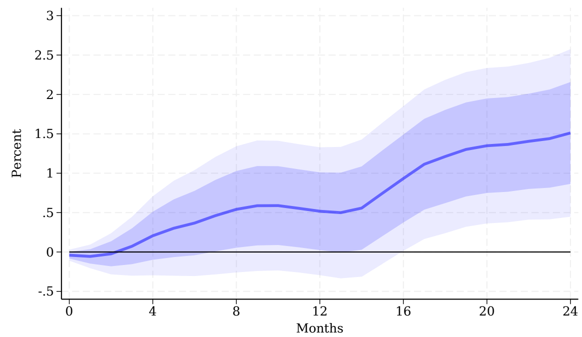
(b) Nominal Rent

Note: This figure displays the Olea and Pflueger (2013) effective F-statistic of the first-stage of Equation 2 for the case where the outcome is real rent growth (Panel (a)) and for the case where the outcome is nominal rent growth (Panel (b)), for each horizon $h = \{0, 1, ..., 24\}$. The F-statistic may differ between Panel (a) and Panel (b) since the controls in Equation 2 include lags of the outcome variable.

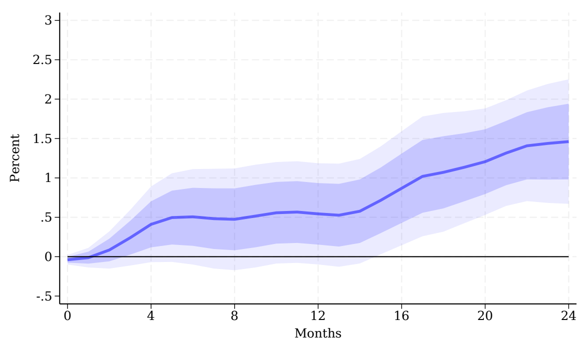
Figure C.3: Alternative Monetary Policy Shocks



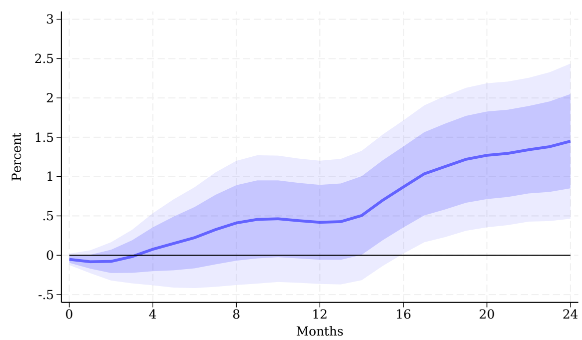
(a) Gürkaynak Sack Swanson (2005)



(b) Nakamura Steinsson (2018)



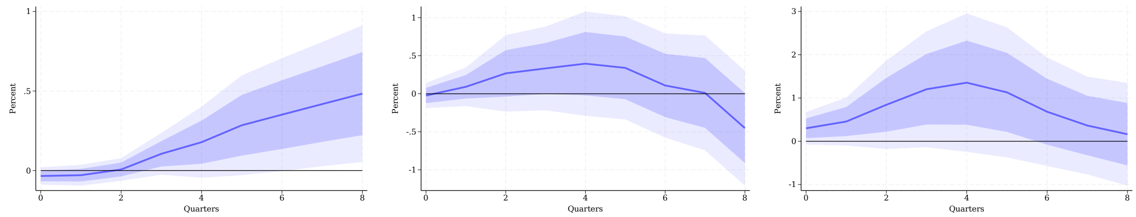
(c) Swanson (2021)



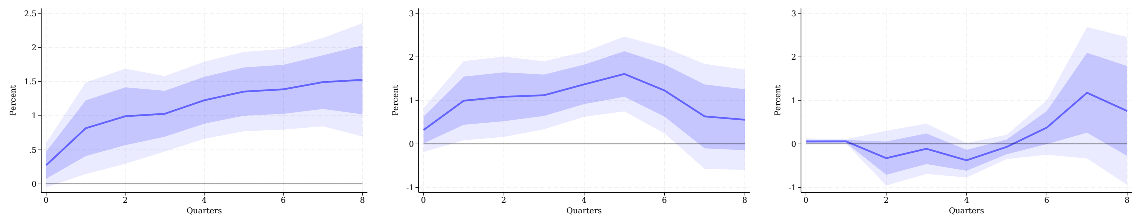
(d) Bauer Swanson, Non-Orthogonalized (2023)

Note: This figure displays the impulse response function of real rent inflation to a 25bps increase in the 30-year fixed rate mortgage using alternative monetary policy shocks. Panel (a) corresponds to the Gürkaynak, Sack and Swanson (2005) shocks, where we use both the surprise changes in the federal funds rate and the surprise changes in forward guidance as instruments for the monetary policy indicator. Panel (b) corresponds to the Nakamura and Steinsson (2018) shocks. Panel (c) corresponds to the Swanson (2021) shocks, where we use surprise changes in the federal funds rate, in forward guidance, and in large-scale asset purchases (LSAPs) as instruments for the monetary policy indicator. Panel (d) corresponds to the non-orthogonalized shocks from Bauer and Swanson (2023*b*). Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

Figure C.4: Alternative Rent indexes



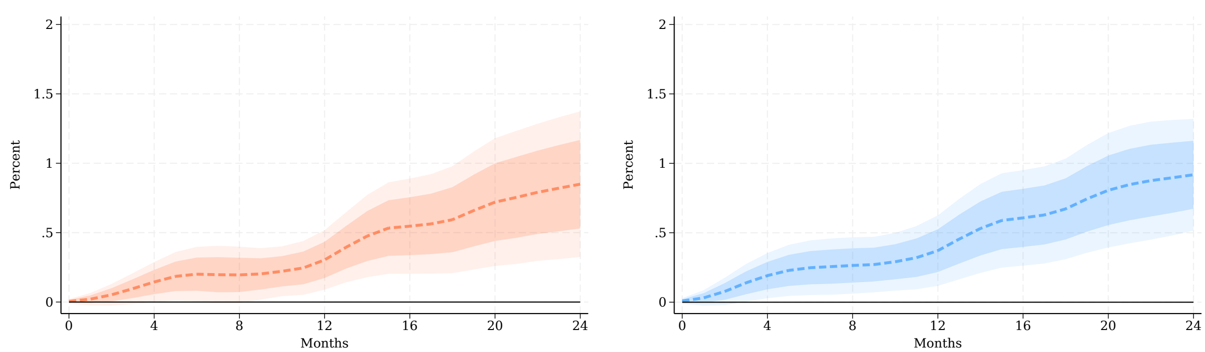
(a) CPI-Rent (b) CPI-NTRR (c) ACY-MRI



(d) ADH-NRRI (e) ADH-Hedonic (f) ZORI

Note: This figure displays the impulse response function of nominal rent inflation to a 25bps increase in the 30-year fixed rate mortgage using alternative rent indexes. Panel (a) corresponds to the CPI-Rent index, panel (b) corresponds to the CPI-NTRR, panel (c) corresponds to the ACY-MRI, panel (d) corresponds to the ADH-NRRI, panel (e) corresponds to the hedonic ADH index, and panel (f) corresponds to ZORI. Dark (light) shaded areas represent 68% (90%) confidence intervals based on Newey and West (1987) standard errors.

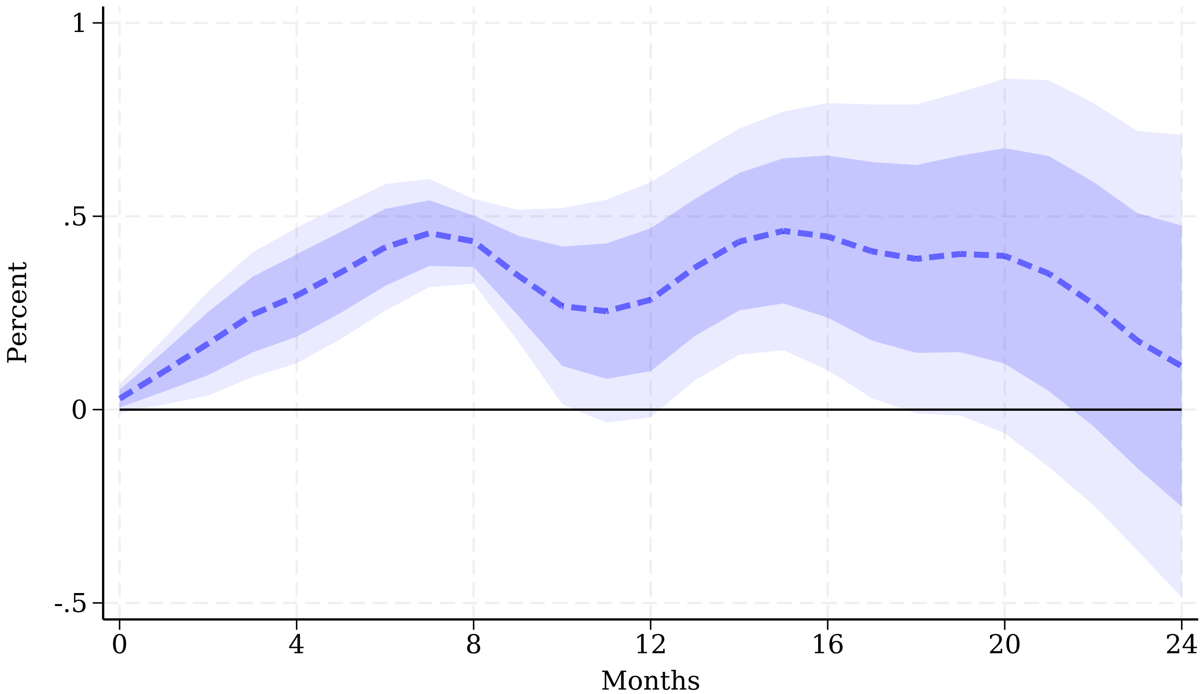
Figure C.5: Alternative Monetary Policy Indicators



(a) Two-Year Treasury Yield (b) Effective Federal Funds Rate

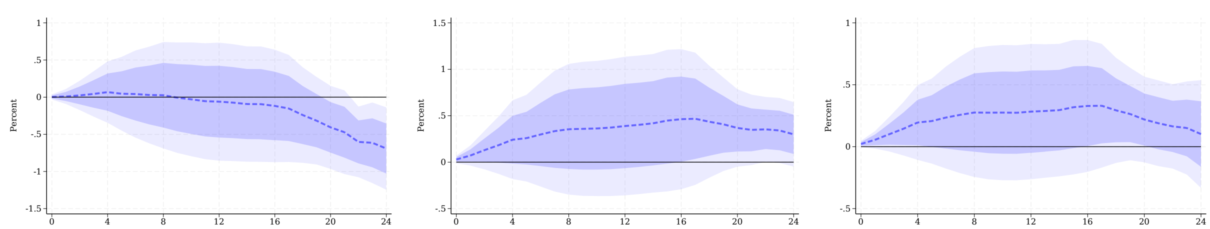
Note: Panel (a) (Panel (b)) displays the impulse response function of nominal rent inflation to a 25bps increase in the two-year US Treasury bond yield (effective federal funds rate). Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

Figure C.6: Heterogeneous Effects - DTI $\in [45, 50)$



Note: This figure plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation, for each horizon h, associated with a one standard deviation increase in the share of borrowers who debt-to-income ratio is greater or equal than 45% and below 50%. Dark (light) shaded areas represent 68% (90%) confidence intervals. Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.

Figure C.7: Heterogeneous Effects - Alternative DTI Measures



(a) Average DTI (b) Share with DTI $\in [37\%, 41\%]$ (c) Share with DTI $\in [38\%, 42\%]$

Note: This figure plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation, for each horizon h, associated with a one standard deviation increase in average DTI (Panel (a)), in the share of borrowers with DTI between 37% and 41% (Panel (b)), and in the share of borrowers with DTI between 38% and 42% (Panel (c)). Standard errors are clustered at both the zipcode and month level and are estimated using the Cameron, Gelbach and Miller (2011) multi-way clustering estimation method.