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 by employing local projection methods. We find that monetary tightening increases both real and nominal rents. A 25 basis point increase in the 30-year fixed rate mortgage raises real (nominal) rents by 1.7 (1.4) percent 12-24 months following the monetary policy shock. The effect is driven by a shift in household demand from the owner-occupied market to the rental market. The increase in demand for rentals is accommodated by real-estate investors who capitalize on the higher rents. Our results highlight the distributional effects of monetary policy and call into question its capacity to effectively curb inflation.

JEL-Codes: E31, E52, G51, R21, R28, R31.

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1 Introduction

Contractionary monetary policy has long been a cornerstone of inflation control, from the rate hikes following World War I to Volcker's campaign against double digit inflation in the early 1980s. By raising borrowing costs, monetary tightening aims to dampen demand for goods and services and thereby to curb inflation. Yet a rich empirical literature, from Sims (1992) and Eichenbaum (1992) to more recent "price puzzle" refinements, shows that inflation can sometimes rise in response to monetary tightening. The post-COVID19 surge in inflation and subsequent rate hikes have reignited the public debate over the efficacy of monetary policy.

The effect of monetary policy on inflation and on welfare crucially depends on how monetary policy affects housing rents. Rents constitute approximately 35% of the Consumer Price Index (CPI) and are the single largest expense for renter households in the United States.[1] The relationship between monetary policy and rents is not ex-ante 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, by making credit more expensive, an increase in the interest rate might shift household demand from the owner-occupier market to the rental market (Gete and Reher, 2018; Ringo, 2024; Dias and Duarte, 2024; Greenwald and Guren, 2025; Han, Ngai and Sheedy, 2025) and put upward pressure on rents.

This paper studies the effects of monetary policy on rents. We begin by constructing a new repeat-rent index using a national database of rental listings. Our repeat-rent index provides high-frequency measures of rent inflation at an unprecedented micro-geographic scale. The granularity of our rent index is key for estimating the effects of monetary policy. While pioneering work by Dias and Duarte (2019) uses national rent indices to study the effect of monetary policy on rents, it also points out that granular rent data is needed for obtaining statistically precise estimates. Equipped with our rent inflation measures, we estimate the impulse responses of rents to monetary policy shocks by employing local projection methods (Jordà, 2005).

Our main finding is that contractionary monetary policy increases both real and nominal rents. A 25 basis point unexpected increase in the 30-year fixed rate mortgage raises real (nominal) rents by 1.7 (1.4) percent 12-24 months following the monetary policy shock. We find that the effect is driven primarily by a shift in household demand from the owner-occupied market to the rental market. Using transaction level data, we show that when monetary policy tightens, renters are less likely to become homeowners and the home-ownership rate drops. This then crowds in the rental market and increases rents. We find that the

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

increase in demand for rentals is accommodated by real-estate investors who capitalize on the higher rents by buying houses from owner-occupiers and renting them in out the rental market.

Our results have important policy implications. First, we shed new light on the distributional effects of monetary policy. The finding that contractionary monetary policy makes renting relatively more expensive implies that it might disproportionately harm lower-income households who tend to be renters. Second, from a positive perspective, our results can help explain the so-called "price puzzle". If contractionary monetary policy increases rents, which are the single largest component of the CPI, this limits its ability to effectively lower aggregate inflation.

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 2022. 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 rent inflation in real terms. 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 index is representative of rent inflation in the U.S. At the national level, the ADH-RRI aligns closely with the CPI-NTRR, an national repeat-rent index based on nationally representative rental data (Adams et al., 2024). It also aligns well with other widely used rent indices such as Zillow's ZORI index (Clark, 2022) and the Marginal Rent Index (Ambrose, Coulson and Yoshida, 2023). The ADH-RRI and these alternative indices tend to lead the official CPI-Rent measure, since they reflect rent changes faced by new tenants rather than by all occupants.

The ADH-RRI's key advantage is that it is both nationally representative and geographical granular. To the best of our knowledge, our ADH-RRI is the most granular high-frequency rent index to date. Alternative indices are either computed a the national level (CPI-NTRR and ACY-MRI), the CBSA level (CPI-Rent), or the zipcode level (ZORI). Our ADH-RRI is not only more geographically granular, but is also more geographically comprehensive. For example, it covers substantially more zipcodes compared to ZORI. The granularity and comprehensiveness of our inflation measures are key for the statistical precision of our analysis. They also allow us to study the heterogeneous effects of monetary policy across local housing markets and to shed light on the mechanisms that underlie the empirical results. More broadly, by providing high-frequency measures of rent inflation at the neighborhood level, our ADH-RRI allows researchers to study rental markets in the U.S. at an unprecedented granular level.

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 contractionary (expansionary) monetary policy shocks increase (decrease) both real and nominal rents. A 25 basis point increase in the interest rate on a 30-year fixed rate mortgage leads to a 1.6% (1.4%) increase in real (nominal) rents 12-24 months following the monetary policy shock. 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 indices. However, since these alternatives are less granular and comprehensive, the estimation is substantially less statistically precise and effects are largely indistinguishable from zero. This illustrates that granular rent data is imperative for the question at hand.

Next we turn to the mechanisms underlying our main results. We find that monetary tightening shifts household demand from the owner-occupied market to the rental market. Using data on the universe of housing transactions in the U.S., we document that monetary tightening reduces the volume of purchases made by owner-occupier households, in particular by first-time buyers. In contrast, the volume of purchases made by buy-to-rent investors is unaffected by monetary policy shocks. Overall, this implies that the homeownership rate declines in response to contractionary monetary policy. The result is intuitive. Higher borrowing costs deter financially constrained households from buying homes, but deep-pocketed real-estate investors are less impacted by the cost of debt. One might still expect investors to pull out of the housing market as alternative investments become more attractive. However, the increase in rents makes investment in housing more attractive. Overall, the economy settles in a new equilibrium where the volume of buy-to-rent investor activity is unaffected.

Consistent with the shift in household demand, we find that the impact of monetary policy on rents is more pronounced in the single-family rental market relative to the multi-family market. Indeed, if monetary tightening prevents renters from becoming homeowners, rents should increase more prominently in segments of the rental market that are more substitutable to the owner-occupied market. Finally, we find no evidence that mortgage lock-in in the owner-occupied market drives rent responses. In particular, the impact of monetary policy on rents does not depend on the share of housing units with outstanding mortgages, implying that any supply reduction from lock-in is offset by a corresponding drop in purchase demand from the same group of existing homeowners.

Related Literature

This paper is one of the first to study the effects of monetary policy on rents. A large literature evaluates monetary policy's impact on house prices (Case and Shiller, 1989; Kuttner, 2014; Williams et al., 2015; Aastveit and Anundsen, 2022; Gorea, Kryvtsov and Kudlyak, 2022) and on housing search (Badarinza, Balasubramaniam and Ramadorai, 2024), but evidence on rents remains scarce and mixed. Dias and Duarte (2019) and Dias and Duarte (2024) use national-level rent indices and 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) and Groiss and Syrichas (2025) find the opposite result. A key challenge that this growing literature faces is the lack of granular quality-adjusted rent data. Granular rent data is essential for obtaining sufficient statistical power to precisely detect the effects of monetary policy on rents. Quality-adjusted rent data is necessary for obtaining unbiased estimates the effects of monetary policy on rents.

Our paper bridges this gap by constructing the most granular quality-adjusted rent index to date. While other work uses national or regional rent indices, our ADH-RRI is constructed at the hyper-local neighborhood level. This granularity is imperative for precisely estimating the effects of monetary policy on rents. We illustrate this by showing that the effect of monetary policy on rents is statistically indistinguishable from zero if we use our national or regional ADH-RRI, but is highly statistically significant when we use our neighborhood level ADH-RRI. The granularity of our index also enables us to use the local projection estimation framework, while other work typically uses the vector auto-regression (VAR) framework. The main appeal of the local projection 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) and are more robust to misspecification (Jordà, 2005). The granularity of our index also allows us to study the heterogeneous effects of monetary policy across local housing markets and to shed light on the mechanisms that underlie the empirical results. More broadly, by providing high-frequency measures of rent inflation at the neighborhood level, our ADH-RRI allows researchers to study rental markets in the U.S. at an unprecedented granular level. Finally, our micro data allows us to construct a repeat-rent index, while other papers use hedonic indices. A repeat-rent index provides a quality-constant measure of rent growth, while hedonic indices cannot fully control for the quality of rented units and might be biased due to changes in the composition of rented units (Bailey, Muth and Nourse, 1963).

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; Garriga, Kydland and Šustek, 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). We 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 explain the well-known "price puzzle" (Eichenbaum, 1992). The 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).

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. Monetary policy can impact rents even absent mortgage lock-in. For example, by increasing borrowing costs, contractionary monetary policy can prevent first-time buyers from becoming homeowners (Ringo, 2024), which might crowd in the rental market (Gete and Reher, 2018). Our analysis suggests that mortgage lock-in plays a limited role in the transmission of monetary policy shocks to the rental market. This does not necessarily contrast previous findings on the effect of mortgage lock-in, but rather implies that aggregate monetary policy shocks affect rents primarily through other channels.

Finally, our empirical results complement a literature that develops equilibrium models with both housing markets and rental markets and uses these models to study the role of rental markets for the transmission of credit shocks in the housing market (Greenwald and Guren, 2025; Rotberg and Steinberg, 2024; Castellanos, Hannon and Paz-Pardo, 2024; Han, Ngai and Sheedy, 2025). In these models, credit-driven demand shocks in the housing market filter to the rental market because of real-estate investors who are active in both markets. By documenting the role of real-estate investors in the transmission of monetary policy shocks, our results provide empirical support to these models. In line with these models, we find that when credit becomes more expensive due to monetary tightening, household demand shifts from the owner-occupier market to the rental market, and this shift in demand is accommodated by real-estate investors who capitalize on the higher rents.

The remainder of the article is organized as follows. Section 2 describes our data. In Section 3, we construct our repeat-rent index. Section 4 presents our empirical strategy for evaluating the effect of monetary policy on rents. Section 5 examines the underlying mechanisms. 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 2022. Altos compiles a national database of rental listings from online listing platforms and from Multiple Listings Services (MLS) platforms. Updated on a weekly basis, Altos 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.

Of course, in reality, 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, we drop units that are likely to differ in their quality but that are undistinguishable based on their address, number of beds and number of baths. Namely, 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]

[2]We drop units that correspond to tuples of address, number of beds and number of baths for which we observe a 4-week (52-

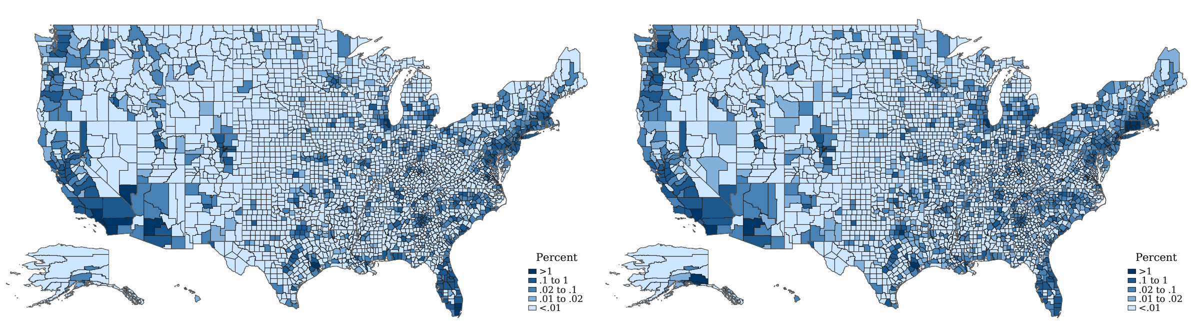
Our main analysis is at the monthly frequency. To obtain a monthly panel of listing prices at the unit level, we collapse all listings of the same unit that appear within the same month (e.g. due to the unit being listed on multiple platforms or being listed for several weeks within a month), to one 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 30.3 million monthly observations of listed rents. It comprises 6.5 million rental units. Each rental units is observed on average for approximately 4.7 months during the sample. The average time 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 in our data that are located in that particular county. Counties colored in lighter (darker) shades are counties where we observe a relatively small (large) number of 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 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 between 2015 and 2019 that are located in that county. Panel (b) displays, for each county, the share of all rental units in the 2015-2019 ACS data that are located in that county. Darker colors correspond to higher shares.

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.

Summary statistics

Table 1 compares summary statistics from our data to summary statistics computed from AHS, from the ACS, and from Zillow. The AHS is a nationally representative survey of the housing stock in the U.S. Since it was redesigned in 2015, we compute the AHS summary statistics based on the 2015, 2017, 2019, and 2021 waves. For consistency, summary statistics from our Altos data, ACS and Zillow are computed based on the same time period. 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 2015 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. Overall, our data over-represents higher tier rental units relative to the nationally representative AHS and ACS. The average rent in our data is higher than the average rent in the U.S, and units in our data are larger and somewhat more likely to be single-family dwellings. Our data aligns with Zillow in terms of the median rent.[3]

Table 1: Summary Statistics

	Rent ($)	**Year Built**	**Bedrooms (#)**	**Bathrooms (#)**	**Sqft**	**Single-family (%)**
	(1)	(2)	(3)	(4)	(5)	(6)
ACS	927	1984	.	2	.	30
AHS	967	1974	2	1	875	35
Altos	1400	1981	3	2	1405	36
Zillow	1387	.	.	.	.	.

Note: This table presents summary statistics from ACS, AHS, our Altos data, and Zillow. The first column reports the median rent (in 2015 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 ACS surveys between 2015 and 2021. AHS statistics are computed from the 2015, 2017, 2019, and 2021 biennial surveys. Altos statistics are computed based on Altos data between 2015 and 2021. The median Zillow rent is computed as the median national ZORI between 2015-2021.

One plausible explanation for the discrepancy between our data and the AHS and ACS is that our data records listed rents, while the AHS and ACS record contract rents, which might be lower. Note that Zillow data is also based on listed rents (see Section 3.1 for a more detailed discussion of the Zillow data). A second explanation is selection. It might be that higher-quality rental units are more likely to be advertised on online listings platforms and therefore disproportionally more likely to be observed in our data.

A potential concern with the repeat-rent index that we construct in Section 3 is that, as illustrated by Table 1, it is based on a sample of rental units that is not representative of the U.S. rental market. We address this concern in Section 3.1 by showing that our repeat-rent index is representative of rent *inflation*. Despite differences in underlying samples, we demonstrate that our index is consistent with alternative

[3]Zillow does not provide summary statistics on the characteristics of rental units.

repeat-rent indices that are based on nationally representative rental data. As discussed in Section 3.1, the advantage of our index is its broader and more granular geographical coverage.

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.[4] The publicly available series includes only monetary policy surprises measured around FOMC meetings. In our baseline specification, we limit the analysis to shocks occurring prior to the Covid-19 pandemic.[5] Our results are robust to including also post-pandemic shocks.

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 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

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

[5] Bauer and Swanson (2023*b*) recommend against using the monetary policy shocks that are measured during the Covid-19 pandemic since these shocks tend to be extreme in magnitude.

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 use 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.[6]

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 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 (Bailey, Muth and Nourse, 1963):

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

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

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 exponent 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 rent inflation in real terms. The real index is computed by first deflating nominal rents by the non-shelter CPI index. 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 AD-RRI at multiple geographical levels - from the hyper-local census tract level, through the zipcode and CBSA levels, up to the national level.[7] 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.

[7] We construct the national index as a weighted average of the zipcode level index, with zipcodes weights corresponding to their aggregate rental stock value. That is, 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, calculated from the 2019 5-year ACS. This approach follows the method used to construct the national Case-Shiller house price index (https://www.spglobal.com/spdji/en/documents/methodologies/methodology-sp-corelogic-cs-home-price-indices.pdf) and ensures that zipcodes with a larger and more expensive rental stock are over-represented in the national index.

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 that our ADH-RRI aligns well 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 index and the CPI-NTRR are both constructed from the BLS Housing Survey data. The 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 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.

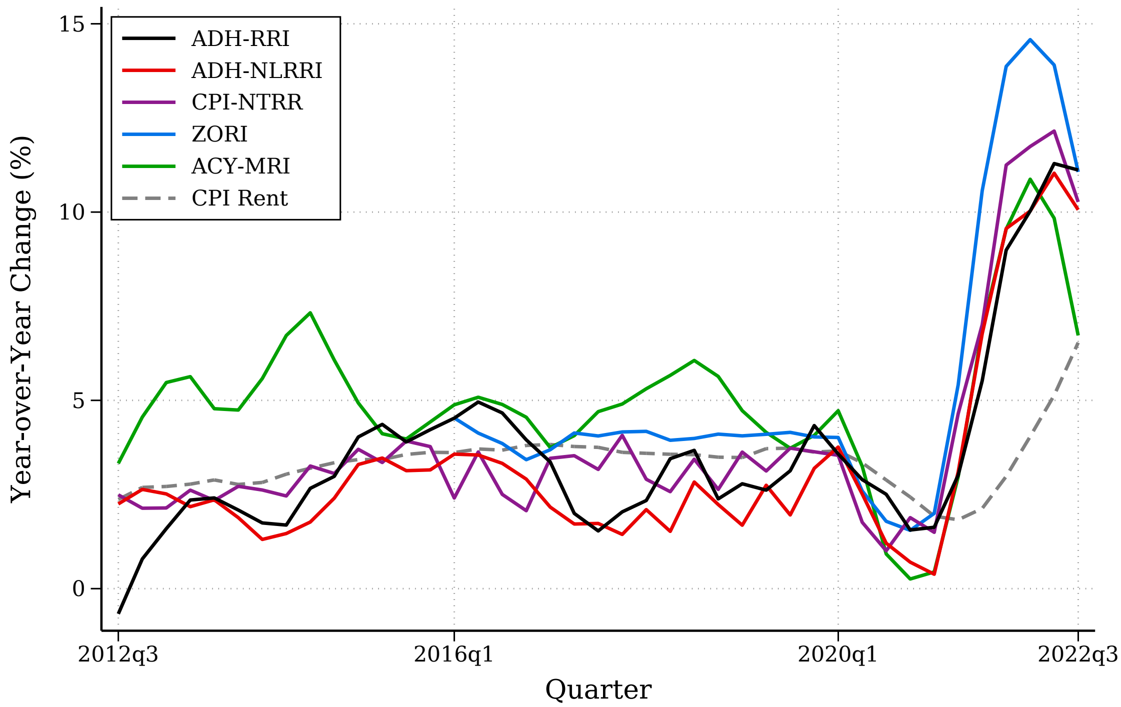
Index Comparison

This section compares our ADH-RRI to alternative rent indexes. Our index aligns well with the popular alternatives discussed above. Importantly, the ADH-RRI is consistent with the nationally representative CPI-NTRR. This suggests that our ADH-RRI is representative of rent inflation in the U.S.

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, the ACY-MRI, and, most importantly, the nationally representative CPI-NTRR. The five indexes clearly capture common rental market dynamics. Our ADH-RRI and ADH-NRRI lead the CPI-rent index, as do the CPI-NTRR, ZORI, and the ACY-MRI. 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.

Table 2 further validates our repeat-rent index by presenting the pairwise correlation coefficients between the year-over-year rent inflation in alternative rent indexes. Correlations between the ADH-RRI, ADH-NRRI, CPI-NTRR, ACY-MRI and CPI-rent are computed based on the quarters between 2012q3 and 2022q3. The correlations with ZORI are computed based on the quarters between 2016q1 and 2022q3. Our ADH-RRI and ADH-NRRI are highly correlated with the nationally representative CPI-NTRR and with ZORI. The correlation between the ADH-RRI or ADH-NRRI and the ACY-MRI is also high. The correlation

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 is computed for each quarter between 2012q3 and 2022q3. 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.

between all indexes and the CPI-rent index, which measured rent growth for both new and existing tenants, is lower. Overall, Figure 2 and Table 2 suggest that the ADH-RRI and ADH-NRRI are representative measures of rent inflation in the U.S. The consistency between the ADH-RRI and ADH-NRRI and alternative rent indexes is in line with previous studies that also document a strong correlation between alternative repeat-rent indexes (Ambrose, Coulson and Yoshida, 2015; Adams et al., 2024).

Advantage of the ADH-RRI

The main advantage of our ADH-RRI is its granular geographical coverage. To the best of our knowledge, our ADH-RRI is the most granular high-frequency rent index to date. The ADH-RRI is constructed at the census tract level, while alternative indices are either computed a the national level (CPI-NTRR and ACY-MRI), the CBSA level (CPI- Rent), or the zipcode level (ZORI). This granularity is key for obtaining the statistical power needed to precisely estimate the effects of monetary policy on rents. More broadly,

Table 2: Correlation between Alternative Rent Indexes

	ADH-RRI	ADH-NRRI	CPI-NTRR	ZORI	ACY-MRI	CPI-Rent
ADH-RRI	1.00	0.93	0.90	0.91	0.66	0.65
ADH-NRRI	0.93	1.00	0.95	0.96	0.73	0.52
CPI-NTRR	0.90	0.95	1.00	0.98	0.75	0.49
ZORI	0.91	0.96	0.98	1.00	0.88	0.37
ACY-MRI	0.66	0.73	0.75	0.88	1.00	0.41
CPI-Rent	0.65	0.52	0.49	0.37	0.41	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, CPI-NTRR, ACY-MRI and CPI-Rent are computed based on the quarters between 2012q3 and 2022q3. The correlations with ZORI are computed based on the quarters between 2016q1 and 2022q3.

by providing high-frequency measures of rent inflation at the neighborhood level, our ADH-RRI allows studying rental markets in the U.S. at an unprecedented granular level.

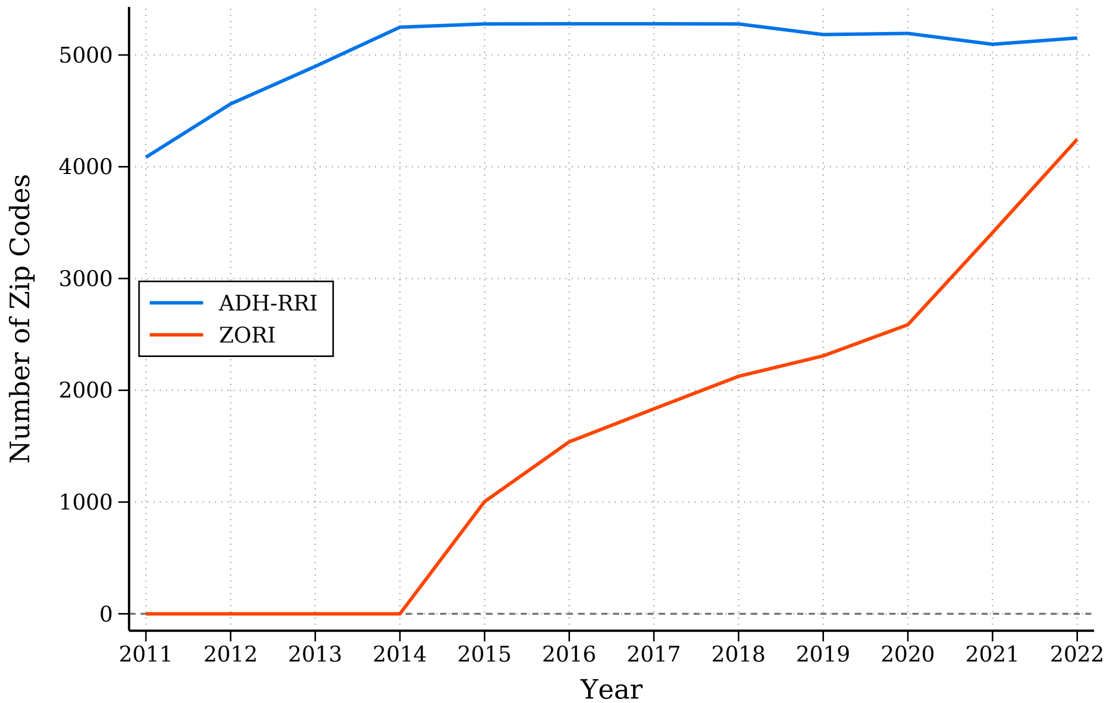
Our ADH-RRI is not only more geographically granular, but is also more geographically comprehensive. For example, it covers substantially more zipcodes compared to ZORI. This is illustrated in Figure 3, which plots the number of zipcodes covered by our zipcode level ADH-RRI and by ZORI across time. First, the ADH-RRI is available starting from 2011, while ZORI is only available starting from 2015. Second, even when ZORI becomes available in 2015, the ADH-RRI provides measures of rent inflation for more than 5 times as many zipcodes. As time passes, ZORI expands it coverage, but even in 2022 our ADH-RRI covers approximately 1,000 zipcodes more than ZORI. Figure B.1 in the Appendix illustrates which zipcodes are covered by each index. The geographical comprehensiveness of the ADH-RRI allow us to study the heterogeneous effects of monetary policy across local housing markets and to shed light on the mechanisms that underlie our empirical results.

4 Effect of Monetary Policy on Rent

In this section, we use our repeat-rent index to evaluate the effects of monetary policy on rents. 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:

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

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 2022.

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 which 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 4 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.[8] 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. However, due to the large size of our dataset, this is not the case in our application.

4.1 Results

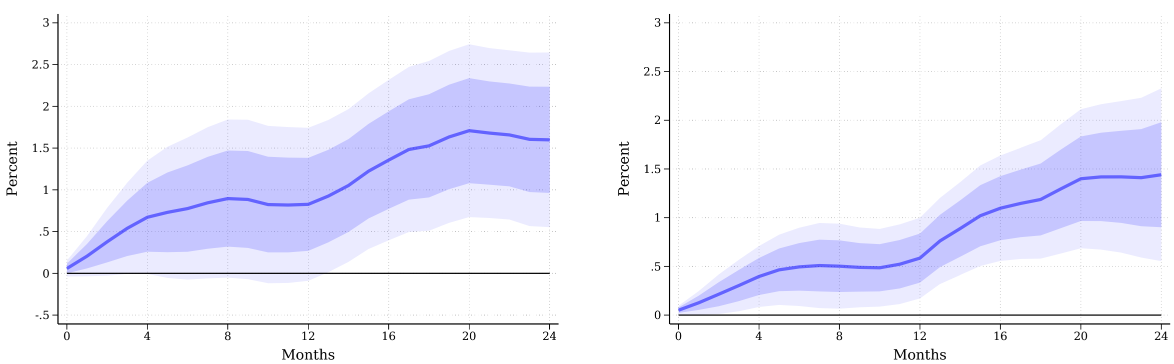
Our baseline analysis is at the zipcode level. Figure 4 illustrates our main results. Panel (a) plots the impulse response function of real rent inflation to an exogenous 25 basis point increase in the interest rate on a 30-

[8] In particular, we include 12 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.

year fixed rate mortgage. The dark (light) shaded areas correspond to the 68% (90%) confidence intervals. We find that monetary tightening increases real rents. A 25 basis point increase in the interest rate on a 30-year fixed rate mortgage leads to a 1% (1.7%) increase in real rents 12 (24) months following the monetary policy shock.

Panel (b) plots the impulse response function of *nominal* rent inflation to the same exogenous increase in interest rate. Contractionary monetary policy shocks not only increase real rents, but also nominal rents. A 25 basis point increase in the interest rate on a 30-year fixed rate mortgage leads to a 0.7% (1.4%) increase in nominal rents 12 (24) months following the monetary policy shock. This result might help explain the so-called "price puzzle" (Sims, 1992; Eichenbaum, 1992). If contractionary monetary policy increases rents, which account for more than 35 percent of the CPI, this substantially limits the ability of contractionary monetary policy to curb inflation.

Figure 4: Effect of Monetary Policy on Rents



(a) Real Rents (b) Nominal Rents

Note: Panel (a) (Panel (b)) displays the impulse response function of real (nominal) 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 4 yields consistent estimates of the effect of the 30-year fixed rate mortgage rate on rent growth. 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 B.2 in the Appendix plots the effective F-statistic of the first-stage of Equation 4 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 $h = \{0, 1, ..., 24\}$. As illustrated by the Figure, the F-statistic is consistently 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.[9] Orthogonalizing is done against the set of controls $X_{z,t-1}$. As illustrated in Table B.1, The R-square in these regressions ranges between 0.003 and 0.008, suggesting that the instrument is unforecastable by lags of the dependent variable.

4.2 Robustness

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

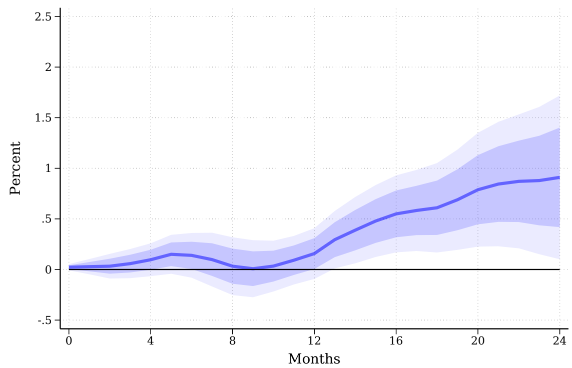
Alternative monetary policy shocks. First, the choice of monetary policy surprises s_t can be important for the estimated effects of monetary policy (Ramey, 2016). We therefore replicate our empirical analysis using a host of alternative monetary policy shocks that have been used in the literature. In particular, we re-estimate Equation 4 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.[10] Figure 5 shows the impulse response function of nominal rent inflation for each of these alternative specifications. Figure B.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 indices. As a second robustness test, we replicate our analysis using a host of alternative rent indices. We begin by re-estimating Equation 4 using three alternative indices: the ZORI index, our new-listing repeat-rent index (the ADH-NRRI), and a hedonic rent index that we construct from our listings data. The construction of the hedonic index is discussed in Appendix A.1. Figure B.4 plots the impulse response functions of rent inflation to an exogenous 25 basis point increase in the 30-year fixed rate mortgage rate for each of these alternative indices. The results are qualitatively and quantitatively consistent with our

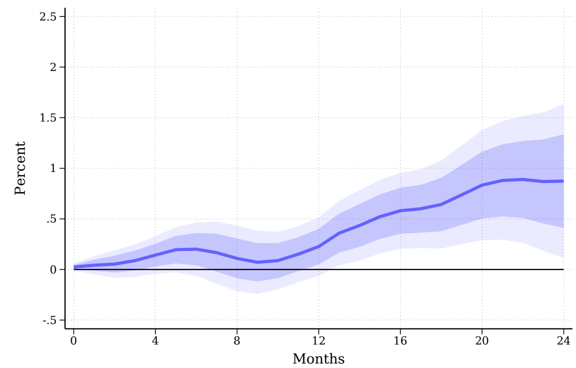
[9] As discussed in Stock and Watson (2018), the requirement that s_t is uncorrelated with future shocks follows directly from the definition of shocks as unanticipated structural disturbances.

[10] 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).

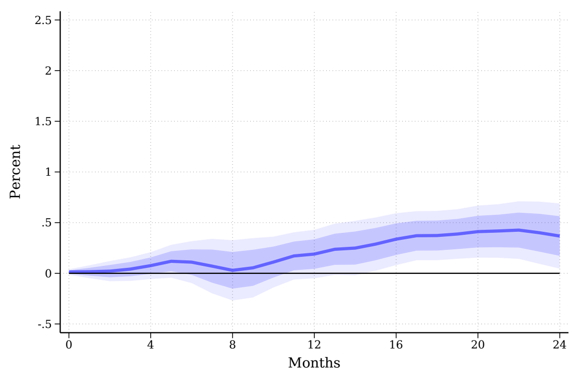
Figure 5: Alternative Monetary Policy Shocks



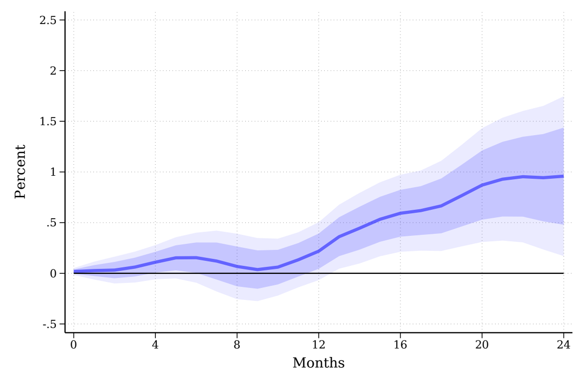
(a) Gürkaynak Sack Swanson (2005)



(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.

baseline results.

We also replicate the analysis using the CPI-rent index, the CPI-NTRR index,and the ACY-MRI. Since these indices 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 4 at the national level and quarterly frequency.[11] Figure B.5 plots the impulse response functions of rent inflation to an exogenous monetary policy shock for each of these alternative indices. The results are qualitatively consistent with our baseline, but are imprecisely measured. This is not surprising given the limited granularity of these indices, which requires estimating the LP-IV equation at the national level. Indeed, as illustrated in Figure B.6, the Olea and Pflueger

[11]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.

(2013) first-stage effective F-statistic is substantially lower than the rule of thumb cutoff for weak instruments recommended by Staiger and Stock (1997) and Andrews, Stock and Sun (2019), suggesting that the monetary policy shocks are a weak instrument at the national level. These results emphasize that granular rent data is needed for precisely estimating the effects of monetary policy on rents.

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 4 using each of these alternative monetary policy indicators as our instrumented variable. As illustrated in Figure B.7, the results are qualitatively and quantitatively consistent with our baseline specification.

5 Mechanisms

Why does monetary tightening increase rents? In this section we explore the mechanisms driving our main results. Our analysis suggests that higher interest rates shift household demand from the owner-occupied market to the rental market. The rise in demand for rentals is accommodated by real-estate investors who capitalize on the higher rents by buying houses from owner-occupiers and renting them in out the rental market.

5.1 Sales in the Housing Market

We begin by investigating how monetary policy shocks affect the volume and composition of sales in the housing market. We show that a contractionary shock lowers the volume of houses that are bought by owner-occupier households but does not impact the volume of sales to real-estate investors. Overall, the home-ownership rate drops. This suggests that household demand shifts to the rental market.

Data. To analyze the impact of monetary policy on housing sales, we use Corelogic data. Corelogic 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. Properties are identified by their Assessor's Parcel Number (APN), which allows tracking the same property across subsequent transactions. We limit our sample to apartments, single family residences, condominiums, and duplexes, and to arms-length transactions.

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 buyers based on whether or not they are cash-buyers. A buyer is defined 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 Corelogic 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 further compute, for each county, the average number of quarterly transactions where the buyer is a non-occupier, and restrict the analysis to counties that are above the median[12].

Empirical Analysis. 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 4, we estimate the following LP regression via two-stage least squares:

$$\log Sales_{c,t+h} - \log Sales_{c,t-1} = \beta^{(h)} i_t + \Gamma^{(h)} X_{c,t-1} + u^{(h)}_{c,t+h}, \tag{3}$$

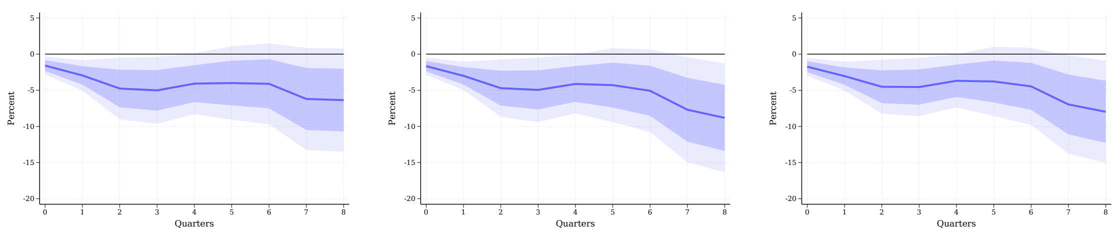
for each horizon $h = \{0, 1, ..., 8\}$. The dependent variable is the cumulative change in the number of transactions in county c between quarter $t-1$ and quarter $t+h$. For the number of transactions, we will consider the overall number of transactions, the number of transactions where the buyer is an owner-occupier, where the buyer is an investor, where the buyer is a first-time buyer, where the buyer is a cash-buyer, and where the buyer is a corporation. We will also consider the number of transactions where the buyer is an owner-occupier and the seller is an investor (which we refer to as "owner-to-investor" transactions), where

[12] The final sample is a panel of 918 counties and 89,964 quarterly observations.

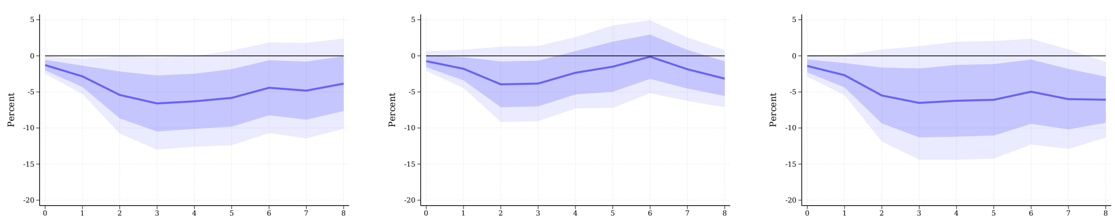
the buyer is an owner-occupier and the seller is an owner-occupier ("owner-to-owner"), where the buyer is an investor and the seller is an investor ("investor-to-investor"), and where the buyer is an investor and the seller is an owner-occupier ("investor-to-owner"). Following Gertler and Karadi (2015), 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.[13]

Results. Panel (a) of Figure 6 plots the impulse response function of the total volume of housing transactions to an exogenous 25 basis point increase in the interest rate on a 30-year fixed rate mortgage. A contractionary monetary policy shock leads to a drop in transaction volume - a hypothetical 25 basis point increase in the 30-year fixed rate mortgage lowers the volume of sales by 4% (7%) one (two) years following the contractionary shock.

Figure 6: Effect of Monetary Policy on Housing Transactions



(a) Transactions (b) Owner-Occupiers (c) First-Time Buyers



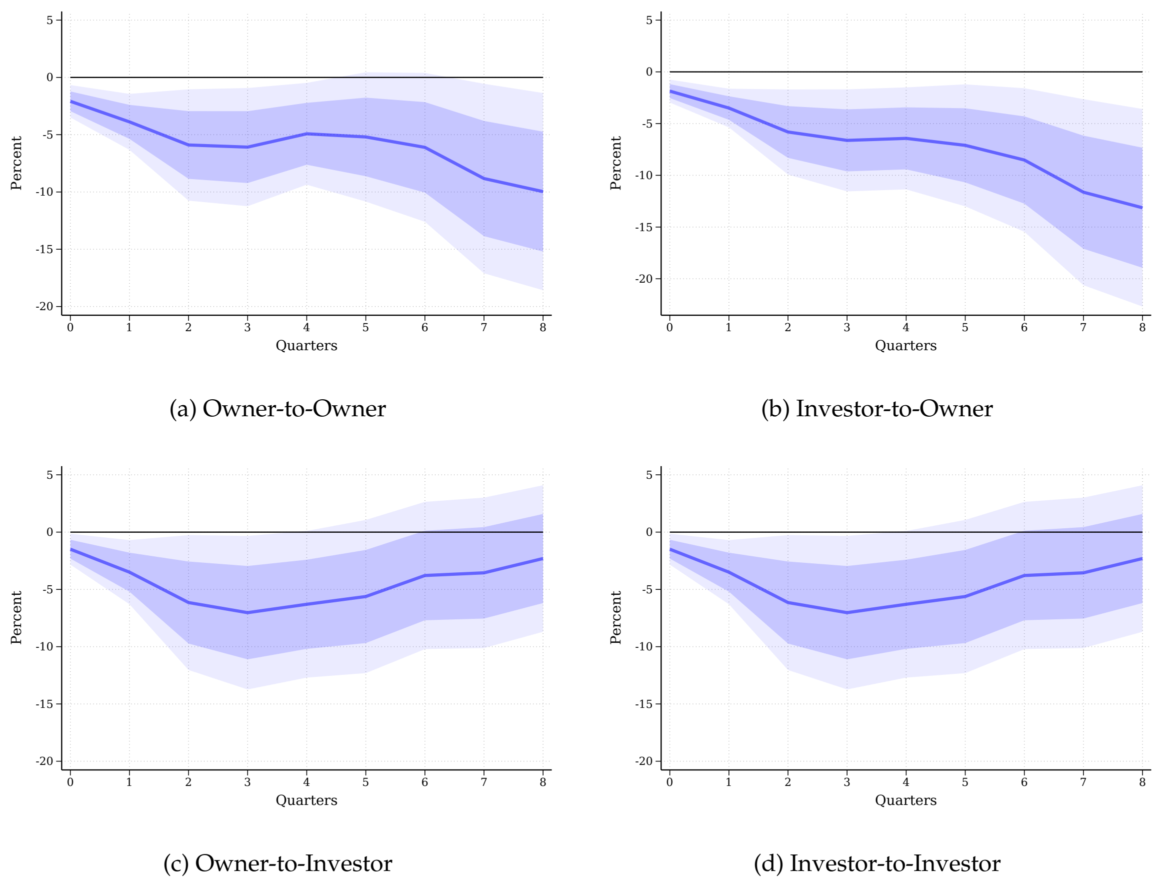
(d) Non-Occupiers (e) Cash Buyers (f) Corporate Buyers

Note: Panel (a) displays the impulse response function of the change in the volume of housing transactions to an exogenous 25 basis point increase in the 30-year fixed rate mortgage. Panel (b) (Panel (c), Panel (d), Panel (e), Panel (f)) displays the impulse response function of the change in the volume of housing transactions where the buyer is an owner-occupier (first-time buyer, investor, cash-buyer, corporation). 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.

[13]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.

The drop in the volume of transactions is driven by a decrease in the number of purchases made by owner-occupiers and first-time buyers. The volume of transactions where the buyer is an owner-occupier (panel (b)) or first-time buyer (panel (c)) drops following a contractionary shock. At the same time, the volume of transactions where the buyer is an investor - defined as either a non-occupier (panel (d)), cash-buyer (panel (e)) or corporation (panel (f)) - remains unaffected (as illustrated by impulse responses that are not statistically distinguishable from zero). The result is intuitive. Higher borrowing costs deter financially constrained households from buying homes, but deep-pocketed real-estate investors are less impacted by borrowing costs. One might still expect investors to pull out of the housing market as alternative investments (for example money market funds) become more attractive. However, the fact that rents increase in response to a contractionary monetary shock makes investment in housing more attractive. Overall, the volume of investor activity in the housing market is unaffected by the increase in interest rate.

Figure 7: Effect of Monetary Policy on Ownership Composition



(a) Owner-to-Owner

(b) Investor-to-Owner

(c) Owner-to-Investor

(d) Investor-to-Investor

Note: This figure displays the impulse response function of the change in the volume of "owner-to-owner" transactions (Panel (a)), "investor-to-owner" transactions (Panel (b)), "owner-to-investor" transactions (Panel (c)) and "investor-to-investor" transactions (Panel (d)) to an exogenous 25 basis point 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.

The finding that owner-occupiers buy less houses while real-estate investors' activity is unaffected by the contractionary shock does not immediately suggest that household demand has shifted from the owner to the rental market. For example, if owner-occupiers also sell less houses and investors sell more houses, the share of housing owned by households might not change. To more directly examine how monetary policy shapes the home-ownership rate, Figure 7 plots the impulse responses of the volume of "owner-to-owner", "owner-to-investor", "investor-to-owner", and "investor-to-investor" transactions. While the number of houses that are sold by investors to owner-occupiers drops (Panel (b)), the number of houses that transition from owner-occupiers to investors is unaffected by the monetary policy shocks (as illustrated in Panel (c) by effects that are statistically indistinguishable from zero). Overall, these results suggest that a higher interest rate shifts household demand from the owner-occupied market to the rental market and lowers the home-ownership rate. This shift in demand is accommodated by investors who capitalize on the higher rents.

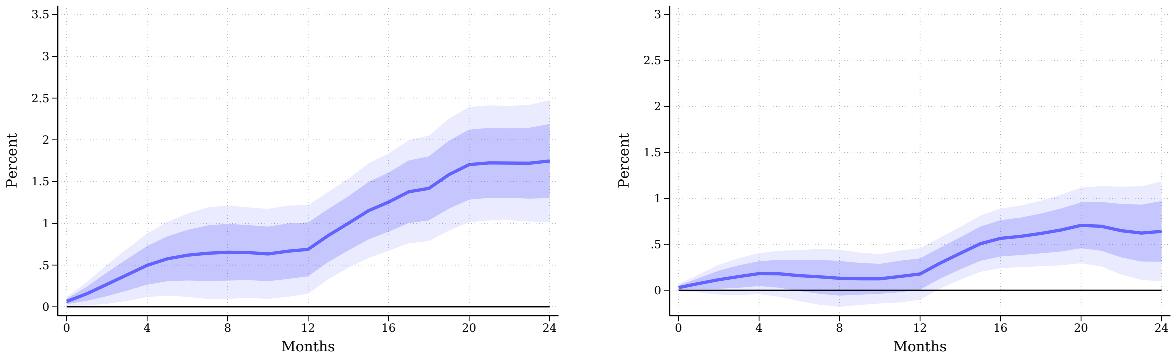
5.2 Heterogeneity Analysis

If monetary tightening prevents renters from becoming homeowners, as suggested by Section 5.1, we would expect demand for rental units to increase particularly in segments of the rental market that are more substitutable to the owner-occupied market. Consistent with this conjecture, we find that the effect of monetary policy on single-family rents is more pronounced relative to the effect on multi-family rents. This is illustrated by Figure 8, which plots the impulse response function of rent inflation in the single-family market (panel (a)) and in the multi-family market (panel (b))to a 25bps increase in the 30-year fixed rate mortgage. These impulse responses are obtained by re-estimating Equation 4 using rent inflation measured by the single-family ADH-RRI and the multi-family ADH-RRI as the dependent variable.

5.3 Mortgage Lock-In

Part of the housing demand shift toward rental may stem from mortgage lock-in. When mortgage rates rise, existing homeowners 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). This lock-in reduces the supply of houses for sale, which can potentially 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). In this section, we evaluate the role of the mortgage lock-in channel in the transmission of monetary policy shocks to the rental market.

Figure 8: Effect of Monetary Policy on Rents: Single-Family vs. Multi-Family



(a) Single-Family

(b) Multi-Family

Note: Panel (a) (Panel (b)) displays the impulse response function of nominal single-family (multi-family) 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.

Data. Our data for this apparatus is the universe of mortgage originations in the U.S. compiled by Corelogic. For each mortgage originated between January 1990 and September 2024, the data records, for example, the date of origination, the mortgage term, 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 a transaction or represents refinancing, and whether the borrower is a corporation. We limit our sample to conventional 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 new first mortgages and refinances.

Empirical Strategy. To study the role that mortgage lock-in plays in the transmission of monetary policy to rents, we compare local housing markets that differ in the extent to which an aggregate monetary policy shock "locks-in" existing homeowners. In particular, for each zipcode and month, we compute the share of housing units with an outstanding mortgage, and ask whether the effect of monetary policy on rents depends on this measure.[14] Intuitively, the same contractionary monetary policy shock locks-in relatively more homeowners in housing markets where a larger share of homeowners have outstanding mortgages. If the mortgage lock-in channel plays an important role in the transmission of monetary policy, we would expect the effect of monetary policy shocks on rents to be more pronounced when a larger share of homeowners have outstanding mortgages. To examine whether this is the case, we estimate the following LPIV

[14]A mortgage is classified 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. The total number of housing units in each zipcode is computed annually from 5-year ACS data and is assumed to be constant within each calendar year.

specification:

$$\log ADHRRI_{z,t+h} - \log ADHRRI_{z,t-1} = \beta^{(h)} i_t \times LockIn_{z,t} + \gamma_t + \Gamma^{(h)} X_{z,t-1} + u^{(h)}_{z,t+h}, \tag{4}$$

for each horizon $h = \{0, 1, ..., 24\}$. The dependent variable is the nominal rent inflation in zipcode z between month $t-1$ and month $t+h$, measured based on our ADH-RRI. i_t is the interest rate on a 30-year fixed rate mortgage. $LockIn_{z,t}$ is the share of housing units in zipcode z in month t with an outstanding mortgage, which we normalize to a unit variance measure for ease of interpretation. $\beta^{(h)}$ is the coefficient of interest which captures how the effect of a higher mortgage rate on rent inflation depends on the share of housing units with an outstanding mortgage. γ_t is a time fixed effect that controls for the aggregate economic environment (including the mortgage rate) at time t. $X_{z,t-1}$ is the set of controls in Equation 4, in addition to one lag of $LockIn_{z,t}$ and the median age of heads of households (computed annually from 5-year ACS data and assumed to be constant within each calendar year). We instrument $i_t \times LockIn_{z,t}$ with the Bauer and Swanson (2023*b*) monetary policy shocks interacted with $LockIn_{z,t}$.

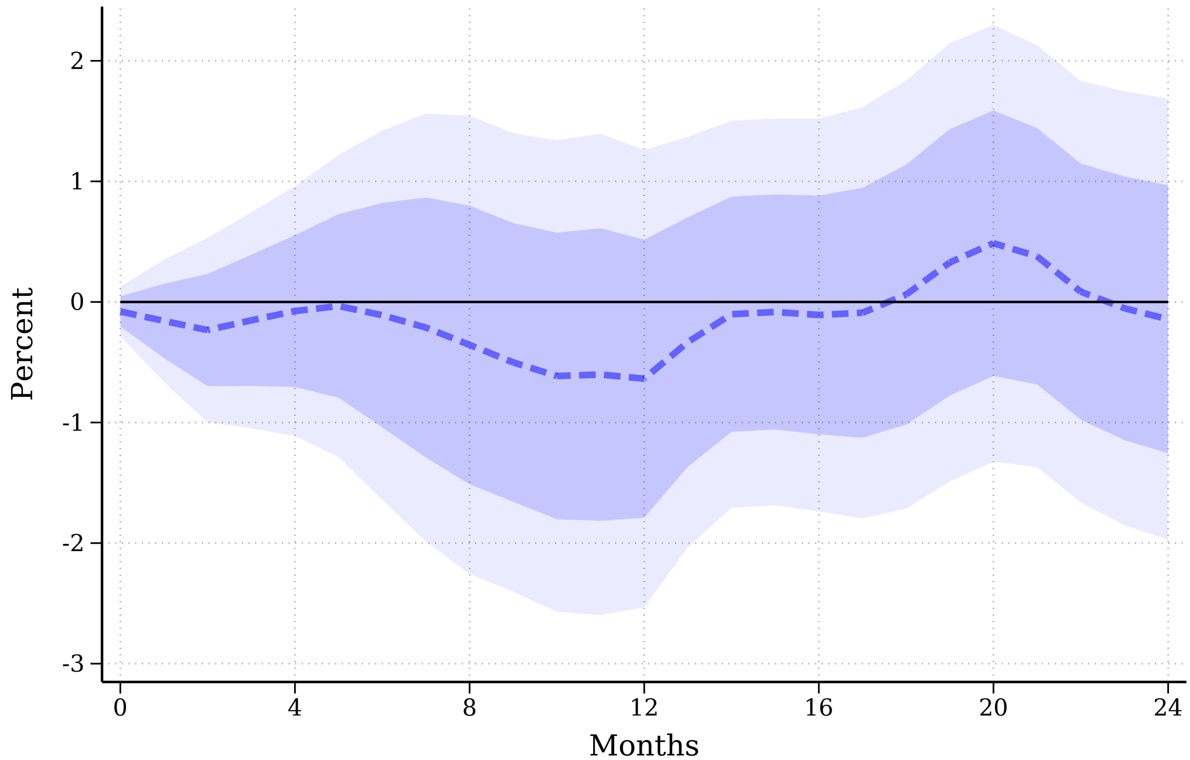
Results. Figure 9 plots the differential effect of an exogenous 25 basis point increase in the 30-year fixed rate mortgage on rent inflation that is induced by a one standard deviation increase in the share of housing units that have outstanding mortgages. We find that the effect of monetary policy shocks on rents is not more pronounced when a larger share of homeowners have outstanding mortgages. This suggests that the mortgage lock-in channel plays a limited role in the transmission of monetary policy. This does not necessarily contrast previous findings on the effect of mortgage lock-in, but rather implies that aggregate monetary policy shocks affect rents primarily through other channels.

6 Conclusion

This paper provides new causal estimates of the effects of monetary policy on housing rents. To do so, 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 high-frequency rent index to date. Employing standard local projections methods, we find that contractionary monetary policy increases both real and nominal rents. Using housing transactions data, we show that the result is driven by a drop in households demand for owner-occupied housing, which crowds in the rental market.

Our results have important normative and positive implications. Normatively, they underscore the unintended consequences of contractionary monetary policy: by elevating rents, monetary tightening can

Figure 9: The Lock-In Channel of Monetary Policy



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 housing units that have outstanding mortgages. 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.

exacerbate the affordable housing problem. Positively, our results carry important implications for the transmission of monetary policy to inflation. The fact that monetary tightening increases rent, which is the single largest component of the Consumer Price Index (CPI), limits the extent to which it can effectively curb inflation.

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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.[15] 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 street 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 have missing number of beds or missing number of baths.

Seventh, as discussed in Section 2, for the purpose of constructing a repeat-rent index, we identify units

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

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 tuples.[16]

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.[17] 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.

Table A.1: Sample Selection

#	**Filter**	**# Listings in Millions (% Raw)**
0	-	443.8 (100.0)
1	Exclude Missing Address/Rent/Date	443.2 (99.9)
2	#1 + Exclude Incomplete Standardized Address	422.7 (95.4)
3	#2 + Exclude Missing or Non-Unique Geocodes	417.9 (94.3)
4	#3 + Exclude Short-Term/Mobile/Commercial/Rooms/Missing Type	417.2 (94.1)
5	#4 + Exclude Duplicates of {Week,Address,Beds,Baths,Price}	365.5 (82.5)
6	#5 + Exclude Multi-Family with Missing Beds/Baths	318.4 (71.8)
7	#6 + Exclude Units Non-Distinguishable Based on {Address,Beds,Baths}	140.2 (31.6)
8	#7 + Exclude Outliers (Extreme Rent/Rent Fluctuations)	120.4 (27.2)
9	#8 + Truncate Constant Rent Listings $>$ 90th percentile of Spell Duration	103.4 (23.3)

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

[16] Namely, we drop all observations associated with tuples of address, number of beds, and number of baths for which we observe, at least in two occasions throughout our sample period, the weekly rent fluctuating between two rents within a 5-week period.

[17] For each of the years 2015, 2017, 2019, and 2021, 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 (deflated) percentiles computed from the most proximate AHS survey.

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 15 units are listed and zipcodes that are observed in less than 70 months throughout our sample. Our final panel data contains 30.3 million monthly observations of listed rents. It comprises 6.5 million rental units across 5,092 zipcodes. Each rental units is observed on average for approximately 4.7 months during the sample. The average time 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 indices (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 15 observations and zipcodes that are observed in less than 70 months throughout our sample. Our final panel data for the hedonic regression contains 68.9 million monthly observations of listed rents across 6,940 zipcodes.

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

$$\log P_{i,z,t} = \alpha_z + \gamma_{z,t} + \Gamma_z X_{i,t} + \varepsilon_{i,z,t}, \tag{5}$$

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 property size, as well as two-way interactions between the property type and all other characteristics. We also include a month-of-year fixed effect to control for seasonality. 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 $\frac{1}{3}\Sigma_{k=0}^{2}100\exp(\gamma_{z,t-k})$.

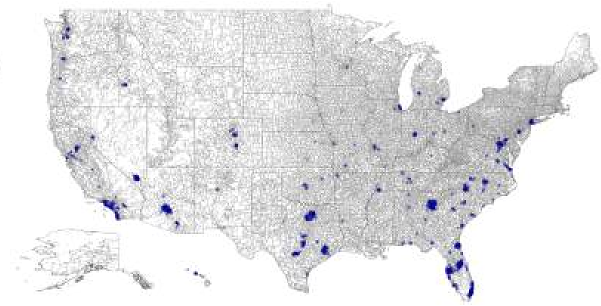
B Additional Figures and Tables

Figure B.1: ADH-RRI vs. ZORI: 2011-2015

(a) ADH-RRI, 2011 (4,085 zipcodes)

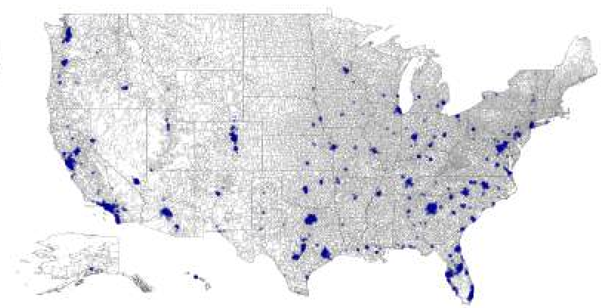
(b) ZORI, 2011 (0 zipcodes)

(c) ADH-RRI, 2015 (5,278 zipcodes)



(d) ZORI, 2015 (1,004 zipcodes)

(e) ADH-RRI, 2019 (5,183 zipcodes)



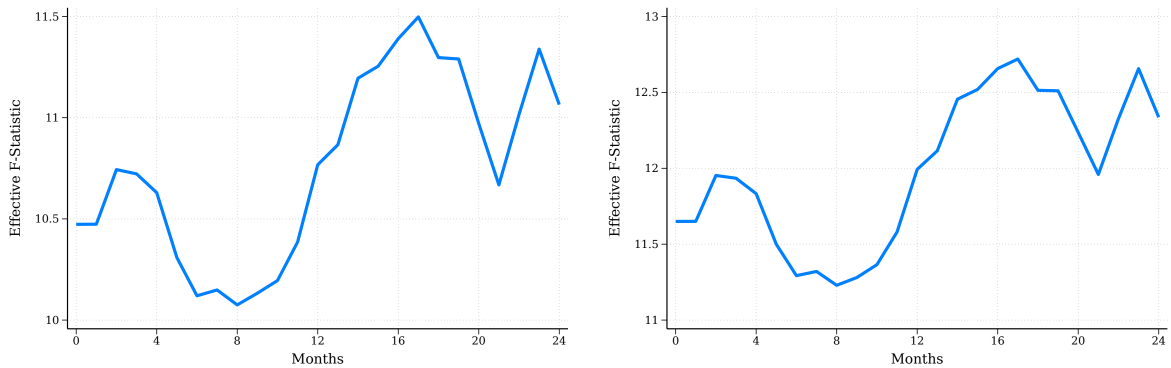
(f) ZORI, 2019 (2,308 zipcodes)

(g) ADH-RRI, 2022 (5,152 zipcodes)

(h) ZORI, 2022 (4,246 zipcodes)

Note: This Figure displays, in blue, the zipcodes that are covered by the ADH-RRI and by ZORI in 2011, 2015, 2019 and 2022. The number of covered zipcodes is in parenthesis.

Figure B.2: First-Stage F-Statistic



(a) Real Rent

(b) Nominal Rent

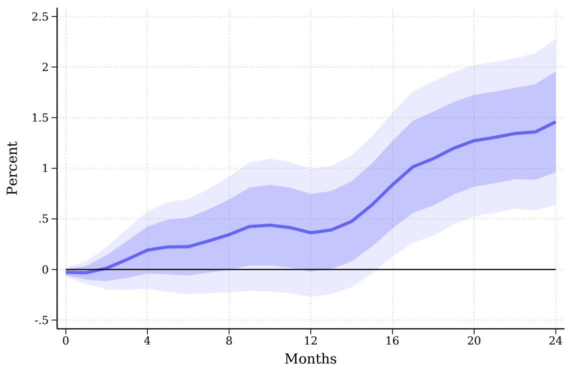
Note: This figure displays the Olea and Pflueger (2013) effective F-statistic of the first-stage of Equation 4 for the case where the outcome is real rent growth (Panel (a)) and for the case where the outcome is nominal rent growth (Panel (b)), for each horizon $h = \{0, 1, ..., 24\}$. The F-statistic may differ between Panel (a) and Panel (b) since the controls in Equation 4 include lags of the outcome variable.

Table B.1: Lag Exogeneity

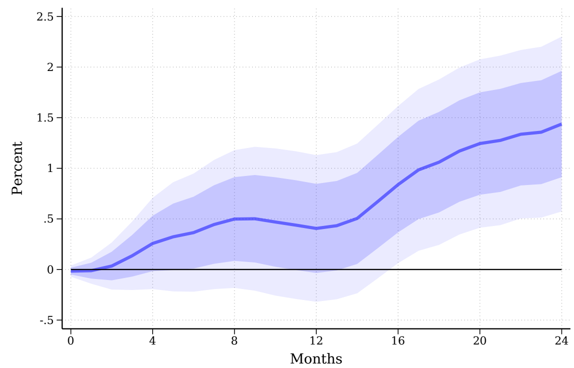
	$Z_t^{\perp}$	
	(1)	(2)
$\{\Delta \log ADH^{\perp}_{z,t-k}\}_{k=1}^{12}$	✓	.
$\{\Delta \log ADH^{\perp}_{z,t-k}\}_{k=1}^{24}$	.	✓
R-squared	0.003	0.008
Observations	216,046	155,266

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. Orthogonalizing is done against the set of controls $X_{z,t-1}$.

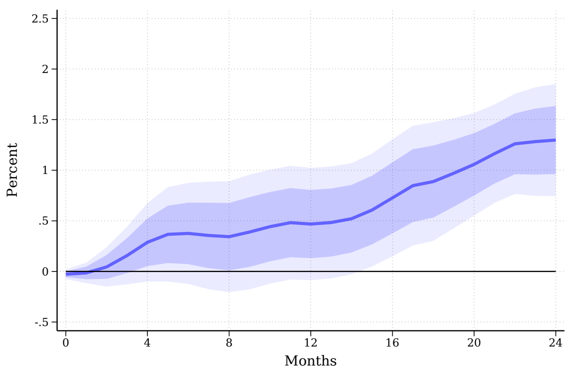
Figure B.3: Alternative Monetary Policy Shocks



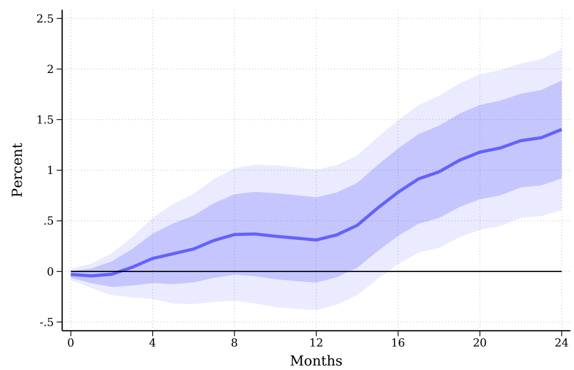
(a) Gürkaynak Sack Swanson (2005)



(b) Nakamura Steinsson (2018)



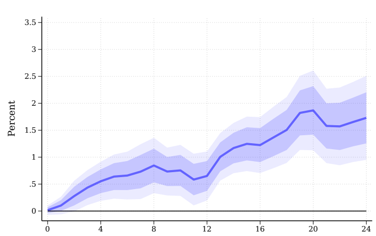
(c) Swanson (2021)



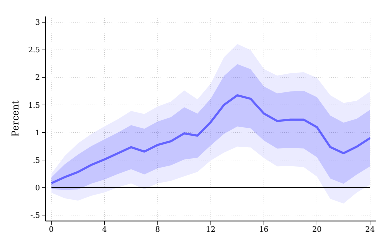
(d) Bauer Swanson, Non-Orthogonalized (2023)

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.

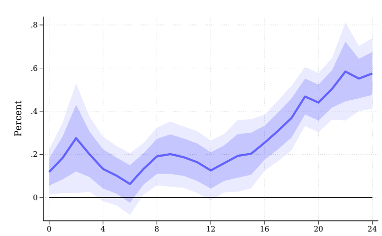
Figure B.4: Alternative Rent Indices: ADH-NRRI, Hedonic, ZORI



(a) ADH-NRRI



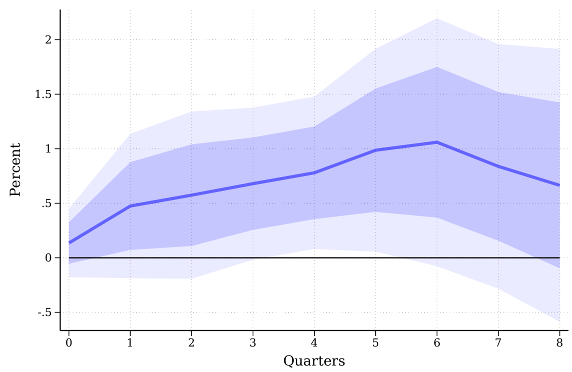
(b) ADH-Hedonic



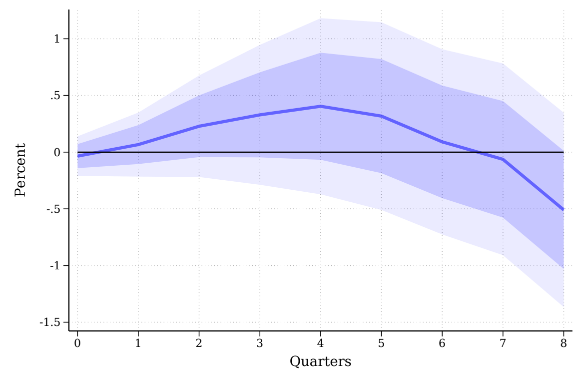
(c) ZORI

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 indices. Panel (a) corresponds to the ADH-NRRI, panel (b) corresponds to the hedonic ADH index, and panel (c) corresponds to ZORI. 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.

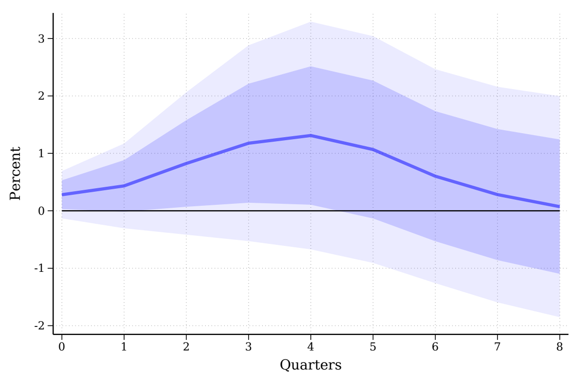
Figure B.5: National Rent Indices: CPI-NTRR, CPI-Rent, ACY-MRI



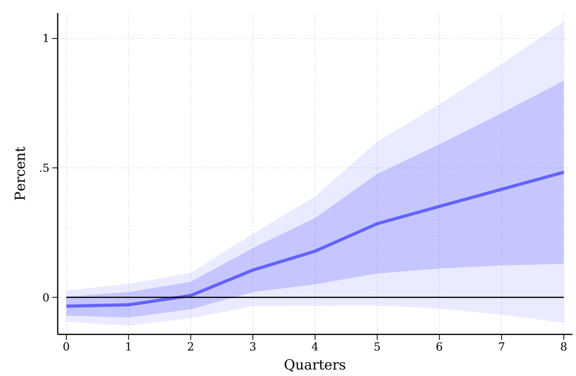
(a) ADH-RRI



(b) CPI-NTRR



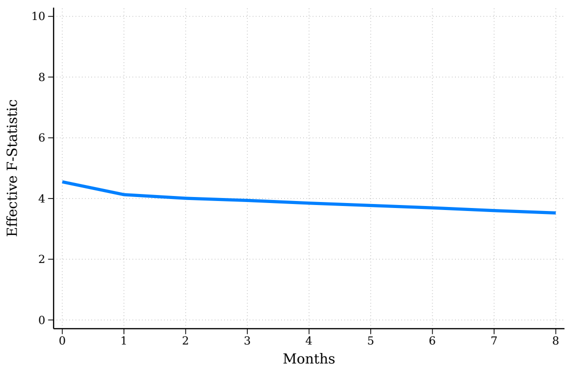
(c) ACY-MRI



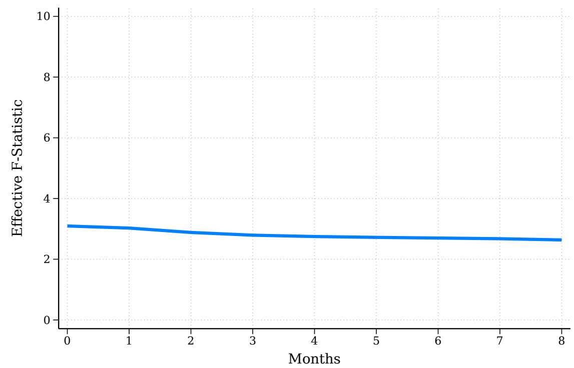
(d) CPI-Rent

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 (national) rent indices. Panel (a) corresponds to the national ADH-RRI, panel (b) corresponds to the CPI-NTRR, panel (c) corresponds to the ACY-MRI, and panel (d) corresponds to the CPI-Rent index. Dark (light) shaded areas represent 68% (90%) confidence intervals based on Newey and West (1987) standard errors.

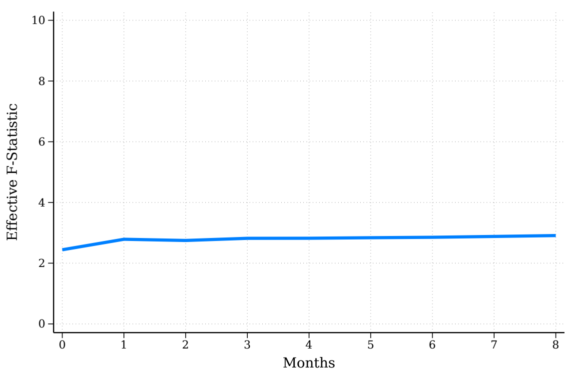
Figure B.6: First-Stage F-Statistic: National Rent Indices



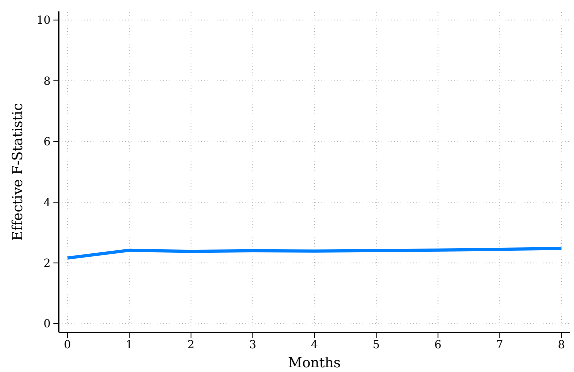
(a) ADH-RRI



(b) CPI-NTRR



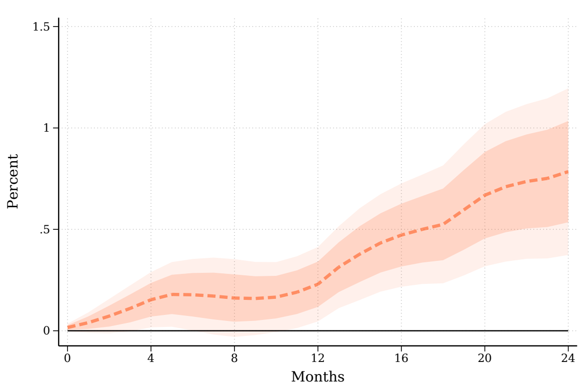
(c) ACY-MRI



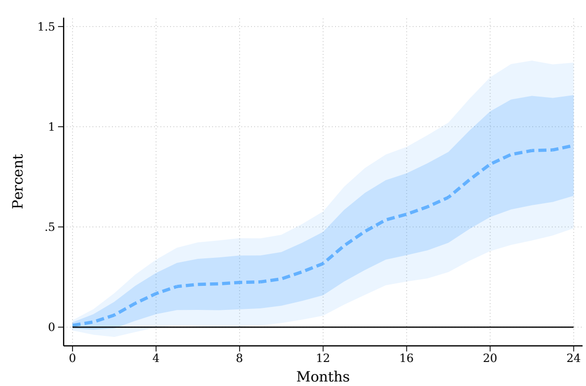
(d) CPI-Rent

Note: This figure displays the Olea and Pflueger (2013) effective F-statistic of the first-stage of Equation 4 for the case where the outcome is quarterly national nominal rent inflation measured by the national ADH-RRI (panel (a)), the CPI-NTRR (panel (b)), the ACY-MRI panel (c)), and the CPI-Rent index (panel (d)).

Figure B.7: 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.