Rising Interest Rates, Mortgage Rate Lock, and House Price Fluctuations *

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Abstract

How do non-assumable fixed-rate mortgages affect the transmission of interest rates into house prices? Higher rates increase the value of existing fixed-rate debt, create an asymmetry between buyers and existing owners. We use administrative data to study the effect of this asymmetry on existing-home prices and sales during the 2021-23 tightening cycle. Using unexpected increases in the long-term Treasury rate, we find that existing owners value low-rate mortgages: lower fixed rates reduce sales, discourage moves from owning to renting, and increase existing owners' willingness to accept. This causes higher price growth in local housing markets where existing mortgages become more valuable during tightening. For variation in the local mortgage distribution, we develop new instruments based on family size shocks that cause moves in periods with different long-term rates. Our estimates imply that eliminating the value of existing fixed-rate mortgages would reverse 30% of 2021-23 house price growth. We estimate a structural model of dynamic housing demand to measure the net price effect of higher rates, which both increase the value of existing fixed-rate debt and discourage homeownership. Existing models without fixed-rate mortgages predict a 20-37% price decline due to 2021-23 tightening. We predict a 4% decrease. Fixed-rate mortgages thus attenuate negative price effects of rate hikes, but do not explain 2021-23 price *growth*.

Keywords: fixed-rate mortgages, monetary policy, house prices, household finance

JEL classification: G51, E21, R21, R31, L78

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Financial arrangements around homeownership are central to consumer housing choices, which shape wealth accumulation (Badarinza *et al.* 2016), resilience to economic shocks (Sodini *et al.* 2023), and aggregate economic fluctuations (Leamer 2007 2015). A large body of research from the 2000s housing cycle shows how the financial environment affects housing demand for new buyers (e.g. Greenwald and Guren 2024). There is less work on existing owners' resale supply decisions. This distinction matters when changing financial conditions create a gap in the incentives faced by buyers and sellers of existing housing units.

This paper studies the impact of a policy-induced change in the financial environment – an increase in long-run borrowing rates due to monetary tightening – on housing market equilibrium. We focus on house prices due to their central importance to both housing and monetary policy. In the US, rising interest rates create asymmetries between new buyers and existing owners with non-assumable mortgages, 92% of which have fixed rates. When interest rates rise, fixed-rate loans enable existing owners to borrow cheaply, while new borrowing and homeownership becomes less attractive. But cheap borrowing is tied to continued ownership, because mortgages are non-assumable and must be repaid at face value on sale.[1] This creates a "rate lock" effect by making existing owners reluctant to sell their home, lose a valuable mortgage asset, and recognize a present-value loss.

While rate lock may magnify the negative impact of rising rates on transaction volume, effects on prices are not obvious. One view is that rate lock is *price neutral* because fewer sellers translates into fewer buyers, reducing supply and demand in equal proportion.[2] An alternative view is that rate lock changes relative prices between some segments of the housing market by affecting moves between them. This can impact aggregate owner-occupied house prices, relative to rents, if rate lock reduces moves from owner-occupied to rental housing.

We provide theory and evidence to evaluate these views and estimate the price effect of financial tightening given rate lock. First, we present a parsimonious model clarifying how incomplete segmentation of owner-occupied and rental markets can lead rate lock to impact prices relative to rents in equilibrium. Next, we use administrative data to support the model's predictions. To quantify the effect of low fixed rates on sales decisions and the choice to move from owning to renting, we use an event study from November 2016, the largest one-month 10-year Treasury yield increase after the 2008 recession. We develop new instruments for the local

[1] Lenders have long used "due-on-sale" clauses to prevent buyers from assuming existing mortgages. Many states made these clauses unenforceable when rates rose in the 1960s. Federal law preempted those rules to allow enforcement for federally-chartered institutions in 1976 and most others in 1982 (Murdock 1984). Some government mortgages (USDA, FHA) allow assumption, with 1-12,000 per year ($\sim$0.5% rate) (Park 2022).

[2] Olivier Blanchard summarized this viewpoint in a November 2023 post on Twitter (now X): "Correcting a frequent incorrect conclusion: Yes, non transferable fixed mortgages lead people to keep their house and not sell. This decreases supply. But it has no obvious effect on the price, because the same people do not buy, and it decreases demand by the same amount" (Blanchard 2023).

outstanding mortgage distribution based on family size shocks to quantify how this behavior impacts equilibrium price growth during 2021-23 tightening. Finally, we quantify the impact of rate lock on equilibrium prices during tightening with a new model of dynamic housing demand. We estimate the model with moments from the 2016 event study, and show that price predictions align with untargeted cross-sectional estimates from our new instruments. Previous statistical (e.g. Federal Reserve 2024) or structural (e.g. Amromin and Eberly 2023) models that do not consider rate lock predict a 20-37% decrease in real house prices from 2021-23 tightening. Our model predicts a 4% decline, showing that while rate lock significantly attenuates the negative price effects of tightening, it does not explain recent price growth.

The first section presents a simple model in which households with non-assumable fixed-rate mortgages decide whether to own or rent their home. The model shows how fixed-rate mortgages affect standard "user-cost" formulas used to give intuition for the impact of interest rates on price-rent ratios (Himmelberg *et al.* 2005, Glaeser *et al.* 2012, Samuels 2024).[3] While specialized to housing markets, the model's supply and demand forces apply to equilibrium in any durable goods market where ownership with fixed-rate financing is tied to usage. We introduce the notion of "mortgage value" embedded in non-assumable fixed-rate mortgages, equal to the present-value benefit of the ability to borrow at below-market rates. Mortgage value rises with market rates, attenuating the negative price effect of rate hikes if it discourages moves from owner-occupied to rental housing. This dynamic effect is proportional to the relationship between mortgage value and prices in the cross-section of local housing markets. Mortgage value only affects prices if mortgages are non-assumable, and so hereafter references to fixed-rate mortgages implicitly restrict to those requiring repayment on sale.

The second section provides new causal estimates of these household-level and market-wide relationships. To do so, we use both credit bureau records merged to loan-level mortgage term and repayment data and a separate panel of property transactions.

At the household level, we estimate effects of fixed mortgage rates on existing-home sales and own-to-rent moves using an event study in November 2016 when 10-year Treasury yields jumped by 72 bps over two weeks. We compare households who originally purchased a home just before the rate increase, and obtained a lower fixed mortgage rate, to those moving in just after. Since moving is frictional and hard to time around high-frequency rate changes, the increase did not induce selection on observable household characteristics for originations in a narrow window around the yield change. Estimates imply a 1pp increase in existing owners' fixed mortgage rates increase the probability of a sale over the next four years by 33% (3.1pp).

This effect could reflect that households with a lower fixed rate lack liquidity to cover the

[3] Our baseline model focuses on price-rent ratios, but mortgage value might also affect equilibrium rental costs. An extension of our model with endogenous rents shows how this impacts our results.

per-period cost of a new, higher-rate mortgage, or because they face a higher wealth cost to prepay their below-market mortgage at face value. We find evidence that forward-looking wealth effects dominate. First, we do not find larger effects on own-to-rent moves, which avoid borrowing at higher rates. Second, effects are larger for households with high mortgage loan-to-value ratios, for whom mortgage value is large relative to total wealth, but does not vary with debt-to-income ratios, a proxy for liquidity. Third, existing owners with relatively lower mortgage rates sell their homes at higher prices relative the local market. The price difference is a lower bound on the effect of mortgages on owner willingness-to-accept, because integrated demand for properties within local markets shades transaction prices towards the market average. Estimates imply an annual financial discount rate of below 9.4%, meaning household decisions are consistent with a forward-looking calculation. This seller-specific effect is consistent with, but not the same as, a local market-wide equilibrium price effect, because it depends on the elasticity of substitution between properties *within* a market. This tends to be high because highly-localized markets are much less segmented.

To estimate market-wide equilibrium price effects, we study the impact of mortgage value growth on average price growth across local housing markets defined by zip codes. We focus on year-end 2021-23, as this period is the only tightening cycle with significant mortgage value growth for which we have detailed data. Our analysis compares zip codes within cities defined by Core-Based Statistical Areas (CBSAs) to limit geographic differences across markets.

Within CBSAs, mortgage value depends on loan-to-value ratios and mortgage interest rates, which themselves depend on borrower characteristics and mortgage origination timing. For example, markets with many households who recently moved will have high mortgage value, due to low rates in the 2010s and the fact that recent mortgage originations have higher loan-to-value ratios. Both borrower characteristics and mortgage origination timing may correlate with unobserved drivers of 2021-23 house price growth, biasing estimates from a regression of price growth on mortgage value. In the 2021-23 period, the bias could go in either direction. Markets with positive local demand shocks will have recent moves, creating spurious positive correlation between mortgage value and price growth. On the other hand, markets with high churn will also have recent moves, but such markets, typically in high-density, urban areas, experienced a negative demand shock related to the Covid-19 pandemic, and hence had low price growth from 2021-23.

We first present estimates using instruments that address the endogeneity of borrower characteristics, but are still endogenous to origination timing. The instruments only use variation from differences in the fraction of outstanding mortgages originated at different levels of the 30-year Treasury rate. This uses similar variation as other studies that estimate effects of rate lock on labor mobility (Liebersohn and Rothstein 2024, Fonseca and Liu 2024).

To overcome remaining endogeneity due to origination timing, we develop new instruments based on family size shocks from having children, a main non-financial reason for moving. Our preferred approach uses unexpected shocks due to twin births. We estimate that having twins rather than one child locks in a different path of moves over the next fifteen years. At the local market level, this means different *changes* in the twin birth *rate* generate moves at different levels of the Treasury. We show that within CBSAs, twin birth rate changes are consistent with meaningful binomial finite sample variation, producing quasi-random variation in moves. We predict local outstanding mortgage rates using the co-movement between national mortgage rates and local moves predicted from past twin birth rates. Our predictions use 1995-2005 twin birth rate changes to avoid direct effects on housing markets due to moves, which would violate the exclusion restriction. To summarize our identifying variation, changes in twin birth rates between 1995-2005 generate moves over 1995-2020, a period with meaningful Treasury rate variation, leading to different outstanding mortgage rates across local markets within CBSAs.

Estimates that use the twin instrument to address endogenous origination timing are three times larger than IV estimates that only address endogenous borrower characteristics. Linearly aggregating two-stage least squares estimates from the twins instrument implies that eliminating below-market fixed-rate mortgage debt would reverse 30% of realized 2021-23 house price growth. Over that period, variation in mortgage value growth across markets can explain 78% of cross-sectional variation in price growth. In robustness exercises, we show these estimates are not driven by variation in IVF births nor affected by controlling for the local *expected* twin birth rate change, which ensures that estimation only uses finite sample variation.

We find quantitatively similar estimates using two other sources of variation. First, we form instruments using predicted moves based on pre-2005 changes in first maternal births, a proxy for family formation, rather than the twin birth rate. Second, we control for latent origination quarter effects in a specification using a version of the initial instruments. That all three approaches, which use different variation, give similar results builds confidence that the twins instrument does not estimate a non-representative local average treatment effect. Instruments based on the twin birth rate and family formation can be constructed using public data, and serve as general-purpose instruments for local mobility.

These estimates show that different exposure to mortgage value impacts relative price growth from 2021-23 when rates rose. But higher rates also discouraged homeownership in all markets by raising the opportunity cost of capital. We estimate an empirical model of housing demand to determine the net price effect of these two forces. Each period, credit constrained, risk averse households, differentiated by age, financial wealth, homeownership status, and mortgage characteristics, make dynamic consumption and housing choices under uncertainty. Consumption is financed out of wealth that earns the time-varying risk-free rate.

Households choose whether to stay in their current unit, switch to a new owner-occupied unit, or switch to a rental unit, with unobserved, persistent preference heterogeneity parameterized with state-dependent switching costs. Owner-occupied units are financed with non-assumable loans with a fixed interest rate based on the risk-free rate, endogenously generating rate lock when rates rapidly rise. Equilibrium house prices equate net home sales and purchases.

We estimate the key parameters that control how rate lock affects existing-home sales and own-to-rent substitution by matching moments from the 2016 event study. The estimated model matches various untargeted household behaviors, such as effects of rate lock on moving by age. Crucially, the model matches the untargeted IV estimate of the effect of mortgage value on 2021-23 price growth, even though price moments are not targeted in estimation. This gives confidence that the model captures the impact of rate lock on price fluctuations.

We use the estimated model to predict the net impact of 2021-23 rate hikes on house prices, holding fixed non-financial preferences for owner-occupied units. While real house prices rose by 5.6% from year-end 2021-23, the model predicts a 4% decline. This is much closer than the Federal Reserve (2024) prediction of a 20% decline and the Amromin and Eberly (2023) prediction of a 37% decline. Rate lock thus significantly attenuates the negative price effects of rate hikes. However, that the model predicts a decrease suggests changes in non-financial housing preferences or unmodeled supply factors explain recent net price growth.

Our work builds on several important literatures. First, we contribute to an extensive literature on how the financial environment and the mortgage market interact to influence consumers' homeownership choice and house prices. One strand of this literature focuses on the role of collateral constraints, using applied theory (e.g. Stein 1995, Campbell and Hercowitz 2005, Kaplan *et al.* 2020, Favilukis *et al.* 2017) or quasi-experimental cross-sectional variation in credit supply (e.g. Loutskina and Strahan 2015, Di Maggio and Kermani 2017, Mian and Sufi 2022). Another strand studies the impact of mortgage interest rates on new borrowing (Himmelberg *et al.* 2005, Glaeser *et al.* 2012, Kuttner 2014, Adelino *et al.* 2025).

Our contribution is to focus on existing owners' resale supply choices and their equilibrium implications. Fixed interest rates on existing mortgages grant owners the ability to borrow cheaply when rates rise. This is similar to an expansion in housing collateral tied to existing owners' current units, increasing current unit value relative to substitutes. That this effect influences house prices is an economic application of the Fostel and Geanakoplos (2016) notion of an asset's "collateral value," where equilibrium asset prices increase if their ownership facilitates borrowing on favorable terms.

Crucial for policy, our finding that existing mortgage rates impact house prices introduces a channel for path dependence in monetary policy. Low rates in the 2010s mean most borrowers have historically cheap mortgages. While Berger *et al.* (2021) argue this reduces room for future monetary stimulus, we show that it also dampens the house price effect of tightening.

Second, we contribute to a literature studying various types of housing lock-in that affect moves, including "equity lock" when falling house prices put owners underwater on their mortgages (Ferreira *et al.* 2010, Andersson and Mayock 2014, Bernstein and Struyven 2022) and other transactions costs (Quigley 2002).[4] We analyze the price impact of existing-home supply and demand flows, providing a framework to study equilibrium effects of these frictions.

Previous work has studied the impact of rate lock specifically with a focus on mobility (Bonanno 1971, Quigley 1987, Goodman and Bai 2017, Fount and Oundee 2020, Fonseca and Liu 2024, Batzer *et al.* 2024, Abel 2024, Liebersohn and Rothstein 2024). Relative to these papers, our rich data on mortgage contract structure and focus on a well-identified event study allows us to credibly estimate the impact of rate lock on moves between segmented housing markets and identify its drivers as primarily due to effects on net worth, not liquidity.

Contemporaneous work by Gerardi *et al.* (2024), Fonseca *et al.* (2024), and Amromin and Eberly (2023) present structural models to study how rate lock impacts equilibrium prices. These papers model rate lock as a reduced-form transaction cost in perfect foresight, steady-state equilibria. We use new instruments to deliver model-free estimates of the causal effect of rate lock on local house price growth. Our model features endogenous rate lock in an equilibrium with rate fluctuations.[5] This allows us to study the fact that rate lock only occurs when long-term interest rates rise, which discourages ownership by increasing the opportunity cost of capital. These features also ensure that the implicit transaction cost from rate lock is endogenous to both the current rate environment and expectations for how it will change. Because our model can simulate dynamic moments based on rate changes, we can estimate its parameters with identified substitution patterns and evaluate its predictions using our IV estimates. Endogenizing rate lock is therefore important for our model to realistically quantify the price effect of financial tightening given fixed-rate mortgages.

Finally, we contribute to a literature on models of dynamic housing demand. Existing models feature rich detail on the non-financial determinants of housing choice with little focus on capital structure (e.g. Bayer *et al.* 2016) or vice versa (e.g. Landvoigt *et al.* 2015). Our model combines richness along both dimensions, which is possible to estimate because our data allow us to connect detailed housing choices to individual owners' capital structure.

Sections 1-6, respectively, present the conceptual model to organize our empirical analysis; data; reduced-form evidence on borrower behaviors and market-wide equilibrium price effects; the empirical model, its estimation, and counterfactuals; and the conclusion.

[4]The price effects of broker commissions are of particular policy importance, given recent litigation surrounding National Association of Realtors price fixing (Akcam *et al.* 2024, Kim 2024).

[5]Abel (2024)'s model has these features, but "is silent on home prices."

1 Valuing houses with fixed-rate mortgages

This section presents a framework to illustrate the impact of fixed-rate mortgages on equilibrium prices and transaction volume. We make a series of simplifying assumptions to show conceptually how fixed-rate mortgages affect the intuition from "user-cost" formulas of price-rent ratios as in Glaeser *et al.* (2012) and Himmelberg *et al.* (2005). These formulas guide our reduced-form analysis in Sections 3 and 4. For quantitative exercises in Section 5, we incorporate empirically-realistic features of consumer choice in housing markets into this framework, such as credit constraints, mortgage contract details, uncertainty, option value, and leverage.

Setup. A city has a fixed population of infinitely-lived consumers who each occupy one unit of housing. There is a fixed supply of two types of units: owner-occupied and rental housing.

A fraction μ is endowed with owner-occupied units financed with fixed-rate perpetuity mortgages. These mortgages have balance M and interest rate r_F, so that living in an owner-occupied unit costs $r_F M$ each period. Remaining consumers live in rental housing units owned by absentee landlords who set prices competitively. The per-period lease cost ℓ_t is therefore pinned down by landlords' flow cost to keep an installed unit of rental housing livable.[6] This flow cost grows at rate g, so that lease costs in period $\ell_{t+k} = (1+g)^{k-1}\ell_t$.

The equilibrium price of owner-occupied housing P_t clears the spot market each period, equating the mass of existing owners who sell with the mass of existing renters who buy. Finally, consumers face a constant per-period risk-free interest rate r_t.

Since we treat lease costs as effectively exogenous, equilibrium prices are implicitly price-rent ratios. Appendix Appendix A.8 describes an extension that endogenizes lease costs and housing supply, and reaches the same qualitative conclusions as in this model.

Consumer problem. Consumers can own at most one unit at a time, and make discrete housing choices to maximize utility from the present-value of housing consumption less its present-discounted cost. Housing utility across units may differ due to preferences over the average characteristics of each type of unit, such as a preference for increased space and privacy in typically owner-occupied single-family units, or because of preferences over the contract structure, such as a preference to customize a home. We assume that households discount financial costs using the interest rate, implicitly reflecting their opportunity cost of capital. We conjecture that consumers make a once-and-for-all location choice in period t, derive equilibrium price implications, and confirm that this choice is optimal for all $t+k$ in Appendix A.2.

Markets are partially segmented, in that fraction γ_o of owners choose between owning their

[6] A micro-foundation is that the city contains a large mass of low-quality, unlivable units which can be made livable at cost ℓ_t. The threat of low-quality unit owner entry pins prices to cost.

current property and renting, while $1-\gamma_o$ choose between owning their own property another owner-occupied one. Similarly, γ_ℓ of renters choose between their current unit and owning.

Current renters on the own-rent margin compare the present-discounted cost of purchasing a home, P_t,[7] against their financial willingness-to-pay, $WTP_t \equiv \sum_{k\geq 1} \frac{\ell_{t+k}}{(1+r_t)^k} = \frac{\ell_t}{r_t-g}$. The indirect utility from a rent-to-own move is:

$$v_{it,\ell\to o} = h_{i\ell} + \alpha \cdot (WTP_t - P_t) \tag{1}$$

where $h_{i\ell} \sim H_\ell$ is the present-value of the net housing consumption benefit from living in owner-occupied rather than rental housing. Because $\frac{\partial v_{it,\ell\to o}}{\partial r_t} < 0$, higher interest rates depress renter's demand for owner-occupied housing.

Current owners on the own-rent margin compare the present-discounted cost of continued ownership and a switch from owning to renting. Current ownership requires repaying fixed-rate mortgage debt, with present-value cost $MV_t \equiv M - \sum_{k\geq 1} \frac{r_F M}{(1+r_t)^k} = \frac{(r_t - r_F)M}{r_t}$. A switch from owning to renting earns net sales revenue $P_t - M$ and incurs present-value lease costs of WTP_t. The indirect utility from an own-to-rent move is therefore:

$$v_{it,o\to\ell} = -h_{io} + \alpha \cdot (P_t - WTA_t) \text{ where } WTA_t \equiv WTP_t + MV_t \tag{2}$$

where $h_{io} \sim H_o$ follows a different distribution from $h_{i\ell}$ to allow that own-vs-rent preferences for current owners and renters may differ, and WTA_t reflects owners' willingness-to-accept.

Owner willingness-to-accept depends on two terms. The first is WTP_t, symmetric to the renter problem. However, there is a second "mortgage value" term which reflects the present-value cost of repaying a fixed-rate mortgage at face value. This term is higher when the interest rate on existing mortgages, r_F, is lower, and increases as r_t rises. When $r_t > r_F$, the mortgage value equals the discounted value of the implicit positive dividend the owner receives from a mortgage granting the right to borrow at below-market rates. Due-on-sale clauses require an owner to repay their mortgage upon sale, meaning they must be compensated for this lost asset through a higher sale price. As in the Fostel and Geanakoplos (2016) theory of "collateral value," the connection between financing and asset ownership means they must be compensated for this lost asset through a higher sales price.

There are three cases where mortgage value would not impact existing owners' choices: (i) if mortgage balanaces were zero; (ii) if households had variable rate mortgages, implying $r_F = r_t$; and (iii) if mortgages were marketable, rather than due-on-sale.[8] Cases (ii) and (iii)

[7] The present-value cost is the sum of a down payment, $P-M$, and the present-value cost of interest payments on a fixed-rate mortgage with interest rate r_t and balance M, equal to M.

[8] In that case, the term $P_t - WTA_t$ in equation (2) would become $P_t + MV - WTA_t = P_t - WTP_t$.

show that two frictions – mortgages are *both* fixed-rate, *and* tied explicitly to asset ownership – are necessary for mortgage value to impact household behavior.

Finally, for owners on the own-own margin, the indirect utility of a move is $v_{it,0\to o} = -h_{io} - \alpha MV_t$, which declines when r_F falls or r_t rises.

Housing market equilibrium. House price P_t equates net supply and demand for owner-occupied housing:

$$\gamma_o \mu \Pr\big(v_{it,o\to\ell} \geq 0\big) = \gamma_\ell (1-\mu) \Pr\big(v_{it,\ell\to o} \geq 0\big) \tag{3}$$

Total transaction volume is $Q_t^S = \mu\big[\gamma_o \Pr\big(v_{it,o\to\ell} \geq 0\big) + (1-\gamma_o)\Pr\big(v_{it,0\to o}\big)\big]$.

Without consumer heterogeneity ($h_{io} = h_{i\ell} = 0$), Equation (3) reduces to $P_t = WTP_t = WTA_t$. Without mortgage value ($MV_t = 0$), $P_t = \frac{\ell_t}{r_t - g}$, a simplified version of the pricing equation from no-arbitrage formulas where price equates the user cost of housing, $(r_t - g)P_t$, and the flow cost of renting, ℓ_t. In such models, higher interest rates significantly depress equilibrium house prices: if $r_t - g = 4\%$, a 1pp increase in r_t will reduce house prices by 20%. However, with mortgage value ($MV_t > 0$), no equilibrium trade occurs because $WTP_t < WTA_t$. The asymmetry between buyers and sellers introduced by fixed-rate mortgages means that the price at which renters will buy exceeds the price at which owners will sell.

Consumer preference heterogeneity enables trade. Appendix A derives three features of the equilibrium. First, an increase in r_F increases the probability that an existing homeowner moves, and the probability of a move-to-rent by an amount depending on market segmentation:

$$\frac{\partial \Pr(Move)_{ot}}{\partial r_F} = \underbrace{\frac{\partial \Pr(Move)_{ot}}{\partial WTA_t}}_{<0} \underbrace{\frac{\partial MV_t}{\partial r_F}}_{<0} > 0, \quad \frac{\partial^2 \Pr(Move, o \to \ell)_{ot}}{\partial \gamma_o \partial r_F} > 0 \tag{4}$$

Therefore, the effect of fixed rates on moves indicates how much consumers value fixed rate mortgages, and the magnitude of the effect on own-to-rent moves indicates the degree of market segmentation. If H_o is uniform, the ratio of $\frac{\partial \Pr(Move, o\to\ell)_{ot}}{\partial r_F}$ and $\frac{\partial \Pr(Move)_{ot}}{\partial r_F}$ equals γ_o.

Second, an increase in mortgage value MV_t increases prices:

$$\frac{\partial P_t}{\partial MV_t} \propto -\frac{\partial \Pr(Move, o \to \ell)_{ot}}{\partial r_F} > 0, \quad \frac{\partial^2 P_t}{\partial \gamma_o \partial MV_t} > 0 \tag{5}$$

An increase in MV_t effectively shifts inwards the level of existing-home supply at each price level, increasing the equilibrium price, all else equal. As the fraction on the margin between owning and renting increases, the effect of mortgage value on price grows, because a given shift in mortgage value has a larger level effect on the fraction of homes available. Importantly, this cross-partial requires that γ_o changes, holding γ_ℓ fixed. If $\gamma_o = \gamma_\ell$, so the same fraction are on the margin between owning and renting as renting and owning, γ_o does not affect prices.

Third, mortgage value offsets the negative house price effects of higher interest rates:

$$\frac{\partial P_t}{\partial r_t} = \underbrace{\frac{\partial WTP_t}{\partial r_t}}_{<0} + \underbrace{\frac{\partial P_t}{\partial MV_t}\frac{\partial MV_t}{\partial r_t}}_{>0} \tag{6}$$

Mortgage value unambiguously dampens the negative price effects of rate hike. The magnitude of the dampening relates to the cross-sectional effect of mortgage value on house prices, which itself depends on the degree of market segmentation, observable from own-to-rent substitution.

These three predictions organize the paper's empirical analysis. We first study the effect of r_F on overall moves and moves from own-to-rent as in equation (4) to estimate how much value consumers place on fixed-rate mortgages and identify the degree of market segmentation. Next, we analyze the cross section of equilibrium prices as in equation (5), and find that local markets with higher mortgage value growth also experienced greater price growth during 2021-23 tightening. These estimates are not directly applicable for estimating $\frac{\partial P_t}{\partial r_t}$ in equation (6), which depends on r_t changing, not r_F. To accomplish this translation, we specify a dynamic housing demand model, estimate its parameters using empirical estimates of equation (4), and determine price impacts of tightening under different mortgage value distributions.

Extensions. Appendix A.6 studies mortgage value with complete owner-occupied and rental market segmentation in a model where owner-occupants only move between differentiated owner-occupied markets. The predictions are the same, except that (i) price predictions in levels become price predictions in cross-market differences, and (ii) there is greater price growth in the market that experiences a greater change in mortgage value.

Appendix A.7 presents a version with an explicit "housing ladder," with renters, starter homes, mature homes, and moves only between adjacent rungs. The predictions for relative prices across owner-occupied housing types align with the model in Appendix A.6, and predictions for price levels align with the model in the main text.

Appendix A.8 shows a version that endogenizes per-period lease costs in a market with investors who engage in costly arbitrage across the owner-occupied and rental housing markets. The qualitative conclusions of the model in the main text are the same, although the effect of mortgage value on house prices is attenuated.

2 Data sources

We use two administrative data sources to connect consumer behavior and housing market outcomes to outstanding mortgages.

The first is the Equifax Credit Risks Insight Servicing panel provided to the Federal Reserve system, hereafter referred to as CRISM. CRISM is a borrower-level monthly panel starting in 2005 that merges mortgage servicer data from ICE, McDash®, covering about two-thirds of residential installment loans, with credit bureau data on other consumer debts from Equifax. ICE, McDash®provides details about mortgage contract terms and property features that are unavailable in standard credit bureau data, such as mortgage interest rates, whether a loan is fixed or variable, appraised property value, origination loan-to-value ratios, and origination debt-to-income ratios. Fields from Equifax's credit bureau files add information on other mortgage and non-mortgage debt, as well as granular information on demographics and mobility such as household age, monthly zip codes of residence, and the date of address changes.

We construct two datasets from CRISM: a borrower panel to study household behaviors, and a local housing market panel at the zip-code level to study market-wide outcomes. The borrower panel is based on a 2.5% sample of the full monthly panel. In this dataset, we infer existing-home sales and relate the decision to sell to mortgage contract features, borrower demographics, and the financial environment. The local housing market panel is derived from a 10% sample of year-end snapshots. In this dataset, we estimate average market-wide mortgage value and relate it to local house price growth. Appendix Table C.3, panel (a) provides summary statistics; see Appendix B.1 for dataset construction details.

As a second data source, we use property-level real estate public records data provided by CoreLogic. For most US residential properties, we observe the history of transactions dating back until around 2005, including purchase price, purchase date, and the identity of buyers and sellers. We also see limited information about mortgages associated with purchases, including mortgage balance, origination date, and term, as well as whether the mortgage has a fixed or variable interest rate. CoreLogic also provides information on property characteristics, such as address and home size. We use CoreLogic data to construct a dataset that relates the sale price of a housing unit to the inferred characteristics of outstanding mortgages on the property. Appendix Table C.4 presents summary statistics, and Appendix B.2 provides details.

We supplement these datasets with publicly-available data on local demographics and housing market characteristics, which we will introduce where relevant. Appendix B.3 describes these ancillary data sources.

3 Effect of rate lock on household behaviors

This section studies Section 1's predictions for the effect of rate lock on borrower behavior, and provides moments used to estimate our empirical model in Section 5. We show that low interest fixed-rate mortgages discourage existing home sales and own-to-rent moves, implying incomplete own-rent segmentation and creating scope for equilibrium price effects. This behavior appears driven by forward-looking wealth costs of losing low-rate financing, rather than liquidity constraints making households unable to meet higher post-move monthly payments.

3.1 Empirical strategy

An ideal experiment would randomly assign fixed mortgage rates to borrowers and compare their existing-home sales decisions, holding both the economic environment and time since home purchase constant. However, fixed mortgage rates depend on mortgage spreads and long-term interest rates at origination, both endogenous to subsequent home sale choices.

The endogeneity of mortgage spreads means naively comparing borrowers with different fixed rates would overstate the rate lock effect. Borrowers with lower or less stable income are are likely younger and more mobile due to changing job prospects or family size. Furthermore, borrowers who unobservably anticipate staying in their current house for longer may shop for mortgages more intensively, purchase mortgages points, or pay fixed refinancing costs, thereby lowering their mortgage rate. These factors create spurious positive correlation between fixed mortgage rates and future existing-home sales.

On the other hand, higher long-term interest rates raise monthly payments and may discourage moves by lower income and more mobile borrowers, creating selection due to credit demand. Moreover, high-frequency fluctuations in long-term rates often reflect changes in the Treasury market that may reduce lenders' risk appetite and screen out more mobile borrowers, creating selection due to credit supply.[9] This creates spurious negative correlation between fixed mortgage rates and subsequent existing-home sales.

To overcome these challenges, we compare existing-home sales choices of borrowers who originally move into a home immediately around sharp increases in long-term interest rates. Because of the pass-through of the long-term rate into fixed mortgage rates, those who move in before the increase will have a lower fixed rate than those who move in just after. We focus on

[9] In general, monetary policy surprises lower market risk appetite, as measured by the S&P 500 VIX and option-adjusted spreads on fixed-rate, high-yield debt (Bauer *et al.* 2023). For example, during the 2013 "taper tantrum" episode after the Fed announced the eventual end of quantitative easing, the 10-year Treasury yield increased by 100 basis points primarily due to greater monetary policy uncertainty (Sinha and Smolyansky 2022). Mortgage spreads immediately increased by 50 bps (Gordon 2023), possibly due to lower liquidity supply by Treasury broker-dealers raising mortgage option-adjusted spreads (Goldberg 2020).

rate increases rather than general rate changes, because households who move in after rates drop face very similar mortgage-driven existing-home sales incentives as those who move in before due to the latter's option to refinance.

Using movements in aggregate long-term interest rates removes endogeneity due to borrower specific mortgage spreads. Comparing borrowers who move in immediately around sharp rate fluctuations reduces concerns over selection due to credit demand, as moving is frictional and exact move-in dates are typically the result of months of planning.[10] To limit credit supply selection concerns, we focus on an event study in November 2016, during a period of otherwise stable economic conditions, when 10-year Treasury yields jumped in response to Donald Trump's surprise presidential election victory (Wagner *et al.* 2018).[11] As shown in Appendix Figure C.2, this episode caused the largest month-over-month increase in the 10-year Treasury yield covered by our data outside of a recession. As this increase was not driven by credit market stress, it is less likely to induce credit supply-driven selection. Our focus on a specific event to identify causal relationships aligns with the "narrative approach" to studying effects of monetary policy (Romer and Romer 1989, 2023).

Formally, we estimate coefficients in the following regression specification for outcome y_i, which is either the fixed mortgage rate or the probability of moving over a fixed horizon:

$$y_i = \beta_0 + \beta_1 I(T_i > T_0) + \beta_2(T_i - T_0) + \beta_3(T_i - T_0) \times I(T_i > T_0) + \eta' X_i + \varepsilon_i \tag{7}$$

where T_i is the origination date for borrower i's initial purchase mortgage, $I(T_i > T_0)$ is an indicator for whether the purchase mortgage origination date is after the November 2016 rate increase, and X_i are other controls. The coefficient of interest is β_1, which compares the level of y_i for borrowers with mortgages originated just before and just after the increase. Estimating effects at the boundary ensures moving frictions plausibly constrain selection on borrower characteristics. When the outcome is the probability of an existing-home sale over a fixed horizon, doing so also ensures that estimates flexibly hold fixed the effect on existing-home sales of the path of financial environment, time since initial home purchase, and any interaction between them. The identifying assumption is that unobserved drivers of y_i in ε_i do not shift discontinuously after the interest rate increase at date T_0.

We use the CRISM borrower panel to estimate coefficients in equation (7). We restrict to first-lien, fixed-rate, owner-occupied purchase mortgage originations that align with a bor-

[10]Most online guides recommend at least 3-6 months for finding a new house, selling an existing one, and arranging moving logistics (example here).

[11]According to minutes from the Fed's December 2016 meeting: "[s]urveys of market participants indicated that revised expectations for government spending and tax policy following the U.S. elections in early November were seen as the most important reasons, among several factors, for the increase in longer-term Treasury yields."

rower move. This screens out mortgages made to real estate investors who may time origination around high-frequency rate fluctuations and exhibit different sales choices than owner-occupants. We restrict to borrowers with mortgage origination dates within 6 months of the event window. Appendix Table C.3, panel (b) shows summary statistics for the restricted sample.

3.2 The effect of low fixed rates on existing-home sales

Figure 1 provides a visual overview of the identifying variation and November 2016 event study results. Panel (a) shows the effect of the increase in 10-year Treasury yields on mortgage rates. Over the last three weeks of November 2016, the 10-year treasury yield increased about 70 bps. This passed through almost one-to-one to posted rates on new 30-year fixed rate mortgages from Freddie Mac's Primary Mortgage Market Survey, shown in green. Consequently, rates increased on mortgages with origination dates after November 2016, shown in red and calculated from the CRISM borrower panel. Rates in CRISM lag Freddie Mac by about six weeks, reflecting that many lenders allow borrowers to lock in a rate for 30-60 days.[12] Therefore, we omit originations in the six weeks after the rate increase when estimating equation (7), and compute event time $T_i - T_0$ relative to six weeks post-increase for $T_i > T_0$.

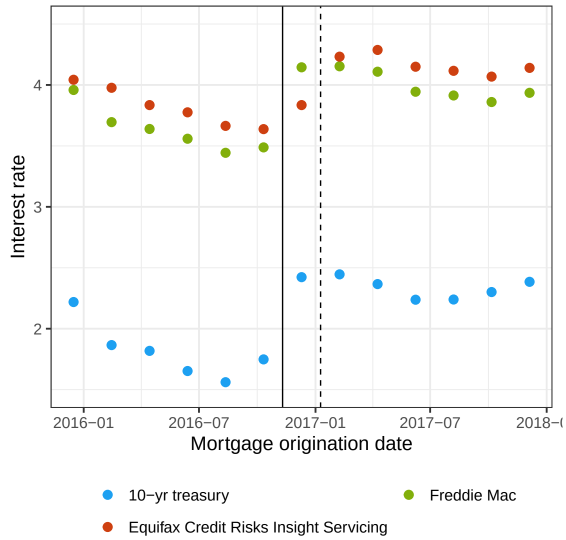
The leading threat to identification is selection on borrower characteristics – borrowers who buy just after the rate increase have different move rates for reasons aside from a higher fixed mortgage rate. A related concern is that higher rates caused borrowers to purchase cheaper homes, leading to lower match quality and higher subsequent mobility. Appendix Figure C.3 gives evidence against these concerns. There is no discontinuous change in age, income, credit score, or appraised property value at the boundary. This is inconsistent with selection on borrower characteristics or direct effects on the type of home purchased. For additional evidence against selection on characteristics, Appendix Figure C.4 shows that 2016 origination volumes closely align with previous and subsequent years.

Panel (b) of Figure 1 shows that rather than induce selection, the primary effect of higher origination rates is a higher monthly mortgage payment. Fixing the financial environment and time since initial home purchase, this implies the willingness-to-accept for an owner with a higher-rate mortgage is lower than the willingness-to-accept for an owner with a lower-rate mortgage.

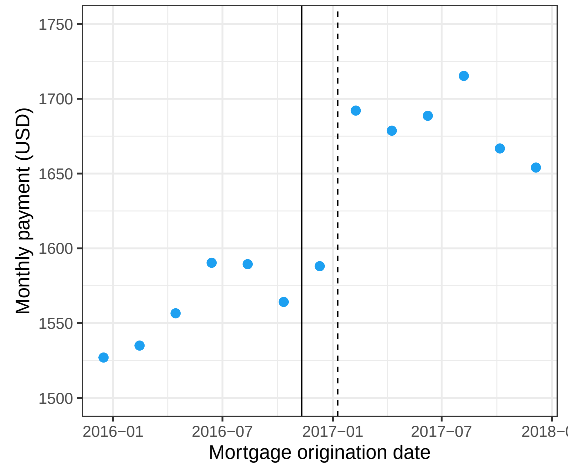
Panel (c) shows that, as predicted by the model in Section 1, this decreased willingness-to-accept increases the probability that a borrower sells their home within four years by about

[12]Freddie Mac rates are slightly lower than in CRISM because Freddie Mac asks lenders to quote the interest rate on a prime 80% LTV mortgage. The market is not that creditworthy on average, implying higher spreads.

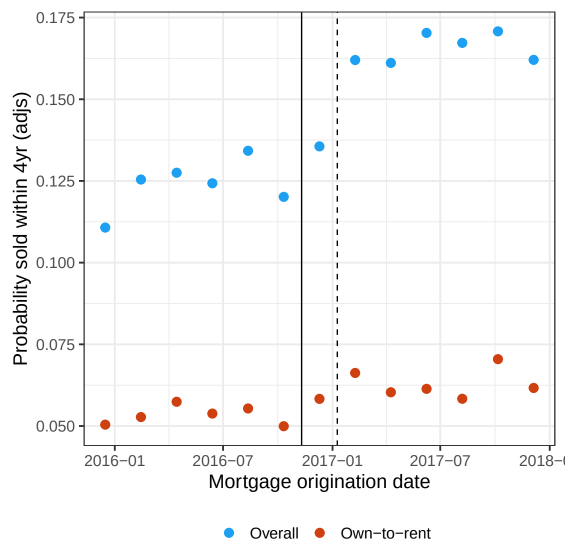
Figure 1: Effect of Nov 2016 rate increase on existing-home supply.
(Source: Equifax Credit Risks Insight Servicing)



(a) Rates by mortgage origination date



(b) Mortgage payment by origination date



(c) Existing-home sales by origination date

Source: Equifax Credit Risks Insight Servicing and FRED. Each figure plots outcomes in 60-day bins relative to November 10, 2016, when long-term interest rates started to increase following the 2016 presidential election. Panel (a) plots the average 10-year Treasury yield, the Freddie Mac Primary Mortgage Market Survey 30-year fixed rate, and the average rate in CRISM for first-lien purchase mortgages originated at move-in. Panel (b) plots monthly principal and interest mortgage payments. Panel (c) plots the probability of an existing-home sale, both unconditionally (blue) and conditional on an own-to-rent move (red).

3pp, or 25%.[13] The probability of an own-to-rent move also increases by about 1pp, or 20%.

[13] This is the longest horizon for which we can track outcomes for borrowers who move in a year after the event without truncation.

As explained in Section 1, this indicates incomplete market segmentation, with moves between owning and renting impacted by rate lock, creating scope for equilibrium house price effects.

Main regression estimates and heterogeneity analysis. Table 1 presents estimates of β_1 from equation (7), restricting to a 6-month window before and after the Treasury rate increase, with controls for age-by-income quintile fixed effects.[14] Column (1) shows that mortgage rates increased by 72 bps. As shown in Columns (2) and (3), the resulting decrease in willingness-to-accept increases the probability of an existing-home sale within 4 years by 3.1pp, or 24%, and the probability of a sale resulting in an own-to-rent move by 1.3pp, or 25%. This implies a semi-elasticity of total and own-to-rent moves with respect to the origination rate of 33% and 36%, respectively.

Table 1: November 2016 event study: Main estimates.
(Source: Equifax Credit Risks Insight Servicing)

Dependent Variables: Model:	Rate (1)	Pr(Moved in 4 yr) (2)	Pr(Moved to rent in 4yr) (3)
Variables			
Post	0.7164***	0.0309***	0.0132**
	(0.0180)	(0.0099)	(0.0067)
Trend width	6mo	6mo	6mo
Fixed-effects			
Age quintile-Inc quintile	Yes	Yes	Yes
Fit statistics			
Observations	32,350	32,350	32,350
R^2	0.41907	0.07677	0.06125
Within R^2	0.35885	0.01884	0.00621

Clustered (Person & Move in date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing. The table presents estimates of β_1 in equation (7). Outcomes are fixed mortgage rate in Column (1), the probability of a move resulting in an existing-home sale within 4 years in Column (2), and the probability of an own-to-rent move resulting in an existing-home sale within 4 years in Column (3).

What types of existing-home sales does rate lock disrupt, and for whom? Appendix Table C.5 explores heterogeneity by distance of post-sale move and age at purchase. In Column (2), the outcome is whether a borrower moves within-county after selling their home. This effect is about 86% of the magnitude of the full effect, and proportionally higher than the full sample estimate. This suggests that rate lock mostly disrupts local moves. This makes sense: shocks large enough to trigger a distant move, like a new job requiring relocation, are probably big enough to still be worth a move even given the net worth cost of losing a fixed rate mortgage.

Column (3) interacts the treatment indicator with age quartile. Rate lock matters for young and old households, and is less important in between. As shown in the model in Section 5, this

[14] Because there is no selection on observables at the boundary, estimates are similar without controls.

may reflect an endogenous feature of lifecycle wealth accumulation. Young households are more sensitive to monthly payment differences because they have low wealth. Older households with high wealth and low future income discount costs at close to the risk-free rate, and are more sensitive to the present-value cost of face value prepayment. These forces matter less for middle-aged borrowers. Appendix Table C.6 shows similar results for own-to-rent moves.

Mechanisms: Wealth effects, not liquidity constraints, drive behavior. In Section 1, variation in the fixed mortgage rate has a larger effect on subsequent existing-home sales for households with large mortgage balances. This is because higher balances increase the wealth cost of prepayment at face value in forward-looking, present-value terms. Column (1) of Table 2 verifies this prediction empirically by interacting $I(T_i > T_0)$ with an indicator for whether the loan-to-value (LTV) ratio on i's mortgage is above or below median. Rate lock primarily impacts households with high LTV ratios.

An alternative hypothesis is that borrowers make static choices based on current liquidity, and rate lock occurs because some households cannot afford higher monthly payments once interest rates rise. This constraint should bind more for households with a high ratio of debt payments to income (DTI). Column (2) finds no heterogeneity in the rate lock effect based on DTI. These results are overall more consistent with rate lock occurring due to forward-looking net worth calculations, rather than myopic concerns about monthly housing costs.

Table 2: November 2016 event study: Heterogeneity by LTV and DTI.
(Source: Equifax Credit Risks Insight Servicing)

Dependent Variable:	Pr(Moved in 4yr)	
Model:	(1)	(2)
Variables		
Post × LTV≤ median	0.0174	
	(0.0129)	
Post × LTV> median	0.0492***	
	(0.0147)	
Post × DTI≤ median		0.0397**
		(0.0193)
Post × DTI> median		0.0368**
		(0.0176)
Trend width	6mo	6mo
Fixed-effects		
Age quintile-Inc quintile	Yes	Yes
Observations	31,829	19,309
R^2	0.10076	0.12433
Within R^2	0.01903	0.01913

Clustered (Person & Move in date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing. The table presents estimates of β_1, with the same outcome as in Column (2) of Table 1, interacted with indicators for whether the purchase mortgage is above or below median LTV (Column (1)) or median DTI (Column (2)).

Robustness checks. Appendix C.1 presents several robustness checks. First, we verify that seasonal patterns do not drive patterns in Figure 1 by running a placebo test on originations around November 2014. There is no jump in existing home sales within four years for originations around this boundary. Second, we present estimates from an alternative "difference-in-differences" design that uses origination date-of-year trends in 2014-2015 as a control group for the treated 2016-2017 sample. Results are similar. Third, we show that our main estimates are unchanged if we exclude moves to areas to and from the Washington, DC area, ensuring our results are not driven by moves directly related to political churn.

3.3 How much do borrowers value rates on fixed-interest mortgages?

This section calibrates a lower bound on the value that borrowers place on low-interest, fixed rate mortgages. We use revealed preferences from existing-home sales prices, comparing resale prices for properties sold in the same market at the same time, but purchased with different mortgage rates. An internal rate of return analysis shows that valuations imply that households make a forward-looking calculation. We close with evidence suggesting that household discount rates correlate with the risk-free return on saving.

Empirical approach. We compare resale prices for properties sold in the same zip code and year-month that were purchased in the same year-quarter, but have different origination mortgage rates due to variation in the exact date of purchase within-quarter. Where i indexes property, T_i mortgage origination date, t resale month, j property zip, and q calendar quarter, we estimate:

$$\log Price_{it} = \beta r_{T_i} + \gamma_{j(i),t} + \omega_{j(i),q(T_i)} + \eta'_{q(t)} X_i + \varepsilon_{it} \tag{8}$$

In this equation, $Price_{it}$ is the resale price of property i in month t, r_{T_i} is the Freddie Mac average origination interest rate at date T_i,[15] $\gamma_{j(i),t}$ are zip code-by-resale month fixed effects, $\omega_{j(i),q(T_i)}$ are zip code-by-origination quarter fixed effects, and X_i are property characteristics with resale-quarter specific coefficients. The zip code-by-resale month fixed effects control for unobserved time-varying demand or supply factors that influence prices for all zip code properties, such as employer entry or exit or local construction. Zip code-by-origination quarter effects control for unobserved, priced differences in the composition of buyers or purchased properties that might correlate with resale price.

The coefficient of interest is β, the semi-elasticity of resale price to average mortgage rate at

[15] This is calculated from Freddie Mac Single-Family Loan-Level dataset, rather than the Primary Mortgage Market Survey. See Appendix B.3 for details.

origination.[16] The identifying assumption is that variation in within-quarter purchase mortgage origination timing is uncorrelated with unobserved but priced property or seller characteristics, conditional on zip code and sale year-month. Our analysis in the previous section, where we found that household moving frictions limit high-frequency selection due to changes in long-term interest rates, supports the assumption that unobserved *initial purchase* characteristics do not confound estimates.

Our estimated semi-elasticity is a lower bound on the value borrowers place on their mortgage for two reasons. First, incomplete housing market segmentation means the true coefficient β is attenuated towards zero relative to the true effect on seller willingness-to-accept. If housing markets were fully integrated at the zip code-by-month level, with a single price for all transactions, then variation in seller willingness-to-accept within a zip-month would not impact transaction prices. In reality, Kotova and Zhang (2020) find substantial price dispersion in local markets, attributed to search-and-matching frictions which allow realized prices to partly capitalize idiosyncratic seller willingness-to-accept. We thus expect to estimate a non-zero, but attenuated, effect. This also suggests we will estimate a smaller effect when restricting to comparisons for more similar properties, since buyers may treat them as closer substitutes, reducing sellers' ability to price on their own willingness-to-accept.

Second, that low rates discourage sales, as we found in Section 3.2, implies that conditional on sales month, sellers with a lower fixed-rate mortgage likely have lower unobserved willingness-to-accept (e.g. are more likely to be forced sellers). Since sellers with a lower willingness-to-accept will transact at lower prices (Campbell *et al.* 2011), this direct effect of rate lock on seller characteristics will bias down fixed effects estimates of β.

Results. Table 3 presents estimates of equation (8) using property-level data from CoreLogic. Column (1) only includes zip code-by-resale month fixed effects. A 1pp increase in a borrowers' fixed mortgage rate, which lowers their willingness-to-accept, reduces resale price by 1.9%.

Two pieces of evidence support that this estimate is driven by variation fixed mortgage rates rather than unobserved confounds. First, Column (2) shows the estimate is essentially unchanged after including zip code-by-quarter of origination fixed effects. Since this implies that origination timing across quarters does not correlate with unobserved idiosyncratic priced characteristics, it is unlikely that within-quarter origination timing does. Second, Appendix Figure C.5 shows relationship between price and origination interest rate demeaned at the zip code-by-resale month level. A *higher* value on the horizontal axis implies that there is a *smaller* difference between the origination rate and the current market rate. The relationship

[16] We would ideally use r_i, the property-level fixed rate, as the regressor, and instrument for it using the national average. We adopt this "reduced form" specification because we lack property-level mortgage rates in the CoreLogic data used to estimate this specification.

Table 3: Effect of origination mortgage rate on resale price.

Dependent Variable:	Log resale price		
Model:	(1)	(2)	(3)
Variables			
Avg interest rate at orig	-0.0192***	-0.0186***	-0.0125***
	(0.0013)	(0.0040)	(0.0037)
Fixed-effects			
Zip-Resale month	Yes	Yes	Yes
Zip-Mtg orig qtr		Yes	Yes
Resale month-Bed x bath bin			Yes
Fit statistics			
Observations	2,681,335	2,661,893	2,661,877
R^2	0.51259	0.56462	0.62609
Within R^2	0.00011	9.26×10^{-6}	4.85×10^{-6}

Clustered (Zip & Resale month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: CoreLogic. The table presents estimates of β in equation (8). Column (1) includes fixed effects for zip code-by-resale month. Column (2) adds zip code-by-origination quarter fixed effects. Column (3) adds resale month-by-bedroom bin-by-bathroom bin fixed effects.

is non-linear: once origination rates are high enough, variation does not affect price. This makes sense if fixed rates drive the relationship. Because of the option to refinance, the owner can borrow at the same (market) rate regardless of whether they sell, meaning variation in the origination borrowing rate should not impact their willingness-to-accept. It is not obvious what confound would produce this non-linearity.

Column (3) of Table 3 show results adding time-varying controls for interactions between bins in number of bedrooms (0-2, 3-4, 4+) and bathrooms (0-2, 3-4, 4+). As expected, given that this implicitly compares price differences for more substitutable properties, the coefficient declines somewhat.

Implied discount rates. To interpret our estimates, we calculate the risk-neutral discount rate that would make a typical seller indifferent between a 1.9% higher resale price and the present-value of annual difference in payments due to a 1pp higher mortgage rate. We account for the impact of moving and prepayment using estimates of mortgage duration from ICE, McDash®. Since the resale price effect is a *lower* bound on the effect on willingness-to-accept, the procedure gives a *upper* bound on discount rates. Appendix Section C.2 gives details.

Estimates imply that borrowers discount future costs at a risk-neutral rate of at most 9.4%. This confirms that borrowers account for future costs when evaluating the financial consequences of giving up a house that allows cheap borrowing.

Is the implicit discount rate impacted by the risk-free rate, as in the model in Section 1? To investigate, we correlate the 2-year Treasury rate with a precise revealed-preference measure of relative borrower discounting based on bunching at the 80% LTV threshold for conventional loans. Conventional loans with LTVs over 80% are required to pay for private mortgage insurance (PMI), which adds an insurance premium to monthly payments and discretely increases the present value of future costs. This makes LTVs just above 80% dominated by an LTV of exactly 80% until the benefit from additional liquidity from putting less money down today equals the discrete increase in the present value of future costs. Borrowers who would have chosen an LTV in this dominated region without PMI requirements instead bunch at 80% exactly. The size of this region – and hence the amount of bunching – depends on borrower discount rates. When discount rates are low, bunching is high because future costs loom large relative to current liquidity, and vice versa with discount rates are high.

Appendix Figure C.6 correlates an estimate of the amount of bunching at an 80% LTV with the two-year Treasury yield. There is a strong negative relationship (R-squared of 17.8%), consistent with a *positive* correlation between borrower discount rates and the risk-free rate.

4 Effect of rate lock on local house price growth

The previous section established that borrowers value low interest, fixed-rate mortgages in a way that disrupts moves across segmented markets and hence can influence equilibrium market-level prices. We also found a correlation between rate lock and transaction prices in the cross section of properties, but as discussed above, this *property-level* estimate depends on market clearing frictions and hence does not necessarily relate to the effect of rate lock in the cross-section of local housing markets (see Minton and Mulligan (2024) for further discussion).

In this section, we quantify how the value borrowers place on their fixed-rate mortgages impact equilibrium house prices. We do so using variation in exposure to rate lock across local housing markets during 2021-2023 financial tightening. See Appendix D for derivations.

4.1 Estimation framework and approach

Estimating equation. A first-order expansion around equilibrium prices in the market clearing expression (3) from Section 1 yields:

$$\%\Delta P_{jt} = \beta \cdot \Delta MVP_{jt} + \eta'_{c(j)} \cdot X_{jct} + \omega_{c(j)} + \epsilon_{jt} \tag{9}$$

where $\%\Delta P_{jt}$ is the percent change in equilibrium house prices in market j; ΔMVP_j is the change in mortgage value, scaled by the initial price; X_j is a vector of observable housing market and demographic characteristics, including rent-price ratios; and $\omega_{c(j)}$ is a fixed effect representing shocks to owner-occupied housing demand common to all markets in city $c = c(j)$.

Equation (9) suggests estimating the following equation in the cross section of local housing markets defined by zip codes j in core-based statistical areas (CBSAs) c, where changes are relative to two years prior:

$$\%\Delta\overline{P}_{j,2023} = \beta\Delta\overline{MVP}_{j,2023} + \eta_c' X_{j,2023} + \omega_{c(j)} + \varepsilon_{j,2023} \tag{10}$$

where $\%\Delta\overline{P}_{j,2023}$ is average house price growth from year-end 2021-23, $\omega_{c(j)}$ is a CBSA fixed effect, $\overline{MVP}_{j,2023}$ is average mortgage value, scaled by initial price, for mortgages outstanding in j in 2021 given interest rates in t, and $\Delta\overline{MVP}_{j,2023} \equiv \overline{MVP}_{j,2023} - \overline{MVP}_{j,2021}$.[17]

The coefficient of interest is β, which quantifies how outstanding mortgage value is capitalized into equilibrium house prices. We estimate $\Delta\overline{P}_{j,2023}$ using a repeat-sales house price index, and $\overline{MPV}_{jt}$ as the within-zip average of property-level scaled mortgage value.

Measuring mortgage value. Mortgage value equals the present-value cost of prepaying a fixed-rate mortgage. This is the difference between the mortgage's face value and the sum of remaining fixed payments, discounted at expected market rates. In Section 1 model with fixed-rate perpetuity mortgages and no discount rate uncertainty, scaled mortgage value equals $MVP_{jt} = \left(M_{F,t_0} - \frac{r_{F,t_0} M_{F,t_0}}{r_t}\right)/P_{j,t_0}$, where t_0 is an initial purchase date. Our empirical measure of scaled mortgage value accounts for balance and interest rate heterogeneity, finite repayment horizon, and interest rate uncertainty. We estimate the scaled mortgage value for property i, with annual fixed payments m_i, outstanding end-of-year balance two years prior M_i, remaining loan term two years prior n_i, and appraisal value P_{ia}, as:

$$MVP_{it} \equiv \left(M_i - E_t \sum_{q \leq n_i} \frac{m_i}{(1 + r_{f,t+q})^q}\right)/P_{ia} \tag{11}$$

where expectations are taken over future discount rates based on period t information. $\overline{MVP}_{jt}$ is the equal-weighted average of MVP_{it} for mortgages i outstanding in zip code j in year $t-2$.[18]

Sources of mortgage value variation and housing market confounds. For a 30-year, fully amortizing fixed rate mortgage with no prepayments or refinancing, Appendix Section D.2 shows that $MVP_{it} = MVP\left(r_{m,i0}, LTV_{i0}, n_i, \boldsymbol{y}_t\right)$, a function of fixed mortgage APR $r_{m,i0}$, origination LTV_{i0}, remaining term n_i, and current nominal yield curve expectations $\boldsymbol{y}_t \equiv \{y_{t,t+q}\}_q$.[19]

MVP_{it} is the present value of the ability to borrow at rate $r_{m,i0}$ an amount depending on LTV and remaining term. It is higher when $r_{m,i0}$ is lower, because lower fixed mortgage rates imply

[17] This setup is consistent with market clearing every two years, as in the setup to our empirical model in Section 5. The market-clearing price in t depends on the mortgage value of existing owners present in the market in period t, consisting of owners in $t-2$.

[18] We use equal-weighted averages because we estimate $\%\Delta\overline{P}_{j,2023}$ with equal-weighted FHFA indices.

[19] y_{t+q} is the continuously compounded period t yield on a zero-coupon unit bond that pays out in $t+q$.

lower monthly payments and hence a lower present-value of future mortgage costs. Borrowers have low $r_{m,i0}$ due to low mortgage spreads based on risk premia or the purchase of mortgage points, or because they borrow at a time of low long-term interest rates. MVP_{it} also increases as yields rise by depressing the present-value of future mortgage costs. Origination LTV_{i0} and remaining term n_i scale MVP_{it} in absolute value, increasing it if the fixed mortgage rate is low relative to the rate implied by the forward yield curve.

Why does $\Delta\overline{MVP}_{j,2023}$ vary across local housing markets? $\overline{MVP}_{jt}$ increased from 2021-23 as yields rose. The increase is higher in markets where more borrowers have low-rate, high-LTV mortgages with a long remaining term in 2021 relative to two years before. The outstanding mortgage distribution in 2021 is a function of mortgage risk premia and LTVs, which depend on local borrower composition, and co-movement between past mortgage origination and the long-term borrowing rate, which depend on dynamics of local housing market churn.

Both local borrower composition and housing market churn dynamics likely correlate with unobserved drivers of house price growth, biasing fixed effects estimates $\hat{\beta}_{FE}$ in equation (10). While biases point in both directions, they mostly suggest $\hat{\beta}_{FE} < \beta$ for 2021-23.

Local borrower composition likely biases $\hat{\beta}_{FE}$ downwards due to negative correlation between mortgage value and ε_j. For example, borrowers with less stable income likely pay higher mortgage rates due to a higher risk premium. Markets with many such borrowers have lower mortgage value, but are likely more exposed to aggregate housing demand or credit supply shocks that influence prices. While in principle this could lead to either positive or negative correlation with price growth, the 2021-23 period is characterized by positive shocks to both (Delgado and Gravelle 2023, Gamber *et al.* 2023).

Housing market churn dynamics could bias $\hat{\beta}_{FE}$ due to both general and episode-specific factors. In general, variation across local housing markets in the co-movement of mortgage origination and interest rates bias $\hat{\beta}_{FE}$ downwards. Local markets more exposed to aggregate economic conditions, and with mortgage demand less sensitive to financial conditions, may have more mortgage originations when aggregate growth, and hence interest rates, are high. This could reflect more moves by locals with rising incomes or inflows because of moves for work. Such markets have lower mortgage value, since a greater fraction of borrowers got their mortgage when rates were high. However, these markets likely have higher price growth from 2021-23, as they are more affected by higher aggregate housing demand and less affected by financial tightening. Such dynamics lead $\hat{\beta}_{FE}$ to underestimate β.

Churn dynamics specific to the 2021-23 episode also produce directionally ambiguous bias in $\hat{\beta}_{FE}$. Borrowers with originations closer to 2021 have lower rates and higher mortgage value, both due to falling rates in the early 21st century and 2021 monetary stimulus due to the Covid-19 pandemic. This biases $\hat{\beta}_{FE}$ *upwards* if recent moves reflect unobserved local demand shocks that would increase prices regardless of mortgage value. However, this could also bias estimates *downwards*, because high-density urban housing markets, which suffered negative demand shocks from 2021-23 due to the Covid-19 pandemic (Frost 2023), tend to have higher churn and hence more recent moves (Henning-Smith *et al.* 2023).

Instruments to address confounds. We estimate β in a series of specifications using distinct instruments that progressively remove different sources of bias. Section 4.2 describes instruments that remove bias due to borrower composition, but are still endogenous due to housing market churn dynamics. Section 4.3 presents our preferred approach and introduces instruments to address this endogeneity.

Our preferred approach identifies family size shocks as a leading non-financial reason for moving, and correlates predicted moves driven by these shocks with the Treasury rate to predict the outstanding mortgage distribution. We focus on *unexpected* family size shocks due to twin births, which fluctuate due to finite sample variation within small geographies and hence are unlikely to relate to unobserved 2021-23 house price growth. We find similar results when predicting moves using broader family formation measures, or when controlling directly for housing market churn dynamics. All three approaches deliver similar estimates of β that are three times as large as estimates confounded by endogenous housing market churn.

4.2 Borrower composition instruments.

The primary borrower-level confounds are variation in mortgage spreads due to risk premia and mortgage points and variation in origination LTV. We use two instruments to remove variation from each in turn.

Instrument construction. Our first instrument removes variation in $\overline{MVP}_{jt}$ due to heterogeneity in borrower mortgage spreads. We re-calculate average scaled mortgage value, assuming that all borrowers got the 30-year Treasury rate as of their origination date. We call this the "LTV-by-Treasury rate" instrument, because variation is driven by borrower LTV, the aggregate interest rate, and origination timing. Where I_{jt} is the set of borrowers with mortgages in j in period $t-2$, we estimate: $MVP_{jt}^{\text{LTV x T-rate}} \equiv E\left[MVP\left(r_{f,T(i)}, LTV_{i0}, n_i, \boldsymbol{y}_t\right)|i \in I_{jt}\right]$. We then use $\Delta MVP_{j,2023}^{\text{LTV x T-rate}}$ to instrument for $\Delta\overline{MVP}_{j,2023}$.

Our second instrument strips out variation in borrower LTV by only using variation in origination timing. We refer to this as to as the "Treasury rate" instrument. Define the share of mortgages in zip code j outstanding as of end-of-year t originated in year-month τ as $w_{j\tau,t}^{O}$. The origination timing-driven rate on outstanding mortgages in year t is $r_{jt}^{O} \equiv \sum_{\tau\leq t} w_{j\tau,t}^{O} r_{m,\tau}$, where $r_{m,\tau}$ is the average mortgage rate for loans originated at date τ. The instrument recalculates average scaled mortgage value by assuming all borrowers in j had rate r^O_{jt}, average origination $\overline{LTV}_{t0}$, and average term remaining $\overline{n}_t$: $MVP^{\text{T-rate}}_{jt} \equiv MVP\left(r^O_{jt}, \overline{LTV}_{t0}, \overline{n}_t, \boldsymbol{y}_t\right)$. To improve efficiency, we use separate instruments for each part of $\Delta\overline{MVP}_{j,2023}$.[20]

This uses similar variation as the mobility instruments in Liebersohn and Rothstein (2024), Fonseca and Liu (2024), and Batzer *et al.* (2024). Relative to these papers, we focus on prices, not mobility, and instrument for a theory-driven estimate of mortgage value, rather than average outstanding rates, to properly scale effects of the outstanding mortgage rate distribution.

Results. Panel (a) of Table 4 presents estimates.[21] Column (1) shows $\hat{\beta}_{FE}$, our non-IV fixed effects estimate of equation (10), which implies that house price growth increases by 0.17pp due to a 1pp increase, in units of appraised value, in the average cost of prepaying an outstanding mortgage at face value.

Columns (2)-(3) present two-stage least-squares estimates using the LTV-by-Treasury rate instrument and Treasury rate instruments, respectively. That estimates increase when moving from Column (1) to Column (3) confirms that borrower composition biases $\hat{\beta}_{FE}$ downwards. The bias is large: the coefficient using just origination timing variation is about three times larger than the fixed effects estimate.

4.3 Instruments addressing origination timing endogeneity.

The previous IV estimates are confounded by the endogeneity of origination timing to housing market churn dynamics. This section describes our two approaches to address this. Section 4.3.1 gives our preferred approach of using family size shocks to predict moves. Section 4.3.2 presents an alternative that controls for expected confounds directly.

4.3.1 Origination timing instruments.

Instrument framework. We predict pre-2021 household moves due to shocks that are unrelated to 2021-23 house price growth, and forms instruments based on the co-movement of predicted moves and the aggregate interest rate.

Rates on outstanding mortgages depend on whether current residents got their mortgage when average mortgage rates were high or low, which is shaped by the cumulative effect of

[20] For example, origination timing may be a better predictor of mortgage rates in periods where borrowers anticipate greater rate fluctuations, and hence purchase fewer points. This is more important when we predict origination timing based on predicted moves, but is introduced here for consistency.

[21] Estimates include rent-price controls suggested by the model from Section 1. Appendix Table D.14 adds other CBSA-by-demographic controls, including for 2021 log income, log population, log unit value, homeownership rate, population density, average age, average origination LTV ratio, average origination DTI ratio, the fixed-rate mortgage share, the nonwhite share, the rent-price ratio, and the ratio of 2023 rents to 2021 prices.

past moving and refinancing choices. Households move for many reasons – due to new job opportunities or local amenities, the birth of a child, health events, or new home construction.

Our origination timing instruments identify shocks that create local housing market churn by causing some households to move, but do not otherwise directly impact price growth from 2021-23. We use these shocks to predict moves in each local housing market in each year prior to 2021. If more of such moves happen at times with low interest rates, they will accumulate to a *stock* of lower interest rates by 2021. We thus predict the outstanding mortgage rate by correlating the time series of predicted moves and average mortgage yields.

Consider a household shock $S_{j\tau}$ in year τ which potentially effects mobility in years $t \geq \tau$. For each local market, we estimate predicted moves based on the history of these shocks $\boldsymbol{S}_{jt} \equiv \{S_{j\tau}\}_{\tau<t}$, and then correlate predicted moves with the national average interest rate to predict outstanding mortgage rates in year t, r^S_{jt}. Our instruments for scaled mortgage value, MVP^S_{jt}, then use r^S_{jt} in place of r^O_{jt}, with $MVP^S_{jt} \equiv MVP(r^S_{jt}, \overline{LTV}_{t0}, \overline{n}_t, \boldsymbol{y}_t)$.

Formally, let $v^S_{j\tau}(\boldsymbol{S}_{j\tau})$ be a linear combination of $\boldsymbol{S}_{j\tau}$ that predicts moves in year τ. Due to moves and refinancing activity, a move in year τ will have a diminishing impact on the outstanding mortgage distribution as time progresses. The predicted impact of moves in τ on the mortgage distribution in t is thus $K(t-\tau) \cdot v^S_{j\tau}$, where $K(a)$ gives the probability that a mortgage originated a years prior remains outstanding. The shock-driven average mortgage rate outstanding in year t is then $r^S_{jt} \equiv \sum_{\tau \leq t} w^S_{j\tau,t} r^m_\tau$ where $w^S_{j\tau,t} \equiv \frac{K(t-\tau)\cdot v^S_{j\tau}}{\sum_{\tau' \leq t} K(t-\tau) \cdot v^S_{j\tau'}}$.

This procedure removes variation due to level differences in $v^S_{j\tau}$, which result from level differences in S. Therefore, the identifying assumption is that *changes* in the shock are conditionally mean independent of unobserved drivers of house price growth from 2021-23:

Proposition 1 (Origination timing identifying assumption.)**.** *The two-stage least squares estimate of β in equation (10) using MVP^S_{jt} is consistent if $\log S_{j\tau} - \log S_{j,\tau-1}$ is mean-independent of ε_{jc}, conditional on $\omega_{c(j)}$, X_{jt}.*

Selecting household shocks. A primary non-financial reason that households move is a family size shock due to having kids. We focus on *unexpected* family size shocks due to twin births. The decision to have children might relate to current housing market conditions in a way correlated with 2021-23 house price growth, but having twins rather than one child likely does not. Below, we show that having twins locks in different moving patterns than having one child that has persistent effects over childhood and early adolescence, and *within a CBSA*, differences in the *change* in the fraction of births that are twins across local housing markets are consistent with binomial finite sample variation. Changes in the twin birth rate therefore produce quasi-random variation in moves, which accumulate over many years to impact the outstanding mortgage rate distribution.

For these reasons, origination timing instruments formed using the history of local twin birth rates as shocks $\boldsymbol{S}_{jt}$ satisfy Proposition 1's identifying assumption. To avoid direct effects of the twin birth rate on local housing markets, we restrict to the twin birth rate prior to 2005. Therefore, a sufficient identifying assumption is that *changes* in the local twin birth rate from 1995-2005 are conditionally uncorrelated with unobserved drivers of within-CBSA house price growth from 2021-23.

We also consider moves due to family formation, proxied by the number of births to first-time mothers, conditional on the population and age distribution. This instrument has higher power, but raises identification concerns. On the one hand, economic conditions impact family formation and fertility (Autor *et al.* 2019), meaning that local housing markets with increased family formation when interest rates are high or low may have either experienced a boom or bust with persistent effects or have different exposure to national economic conditions. Either possibility could affect house price growth from 2021-23. Mitigating this concern, family formation many years prior affect moves leading up to 2021, because variation in birth cohorts locks in a path of moves as children age. While factors that affect the decision to have kids may correlate with contemporaneous housing market conditions, they are less likely to correlate with housing market conditions many years later. We therefore restrict our analysis to pre-2005 first births.

The rest of this section focuses on the construction, exclusion, and relevance of the twin birth rate instrument. We also provide an overview of the family formation instrument. See Appendix Sections D.5 and D.8 for details.

Twin birth rate variation – instrument exclusion. About 3% of US births are twins. The *realized* twin birth rate varies across markets and years for two reasons. First, whether a particular maternity produces twins is stochastic, so identical markets will have different *realized* twin birth rates each year due to finite sample variation. The law of large numbers means this variation falls with the number of births, but it is important in our context because even markets with reasonably large populations have a relatively small number of annual births. Second, markets may differ in expected twin birth rates due to differences in maternal characteristics.

Our instrument uses variation from county-level changes in the twin birth rate from 1995-2005, conditional on CBSA and observed demographic characteristics.[22] We find that this variation can be explained by finite sample variation, implying that differences in maternal characteristics are not its primary driver. This supports the assumption that twin birth rate changes are conditionally uncorrelated with unobserved drivers of 2021-23 house price growth.

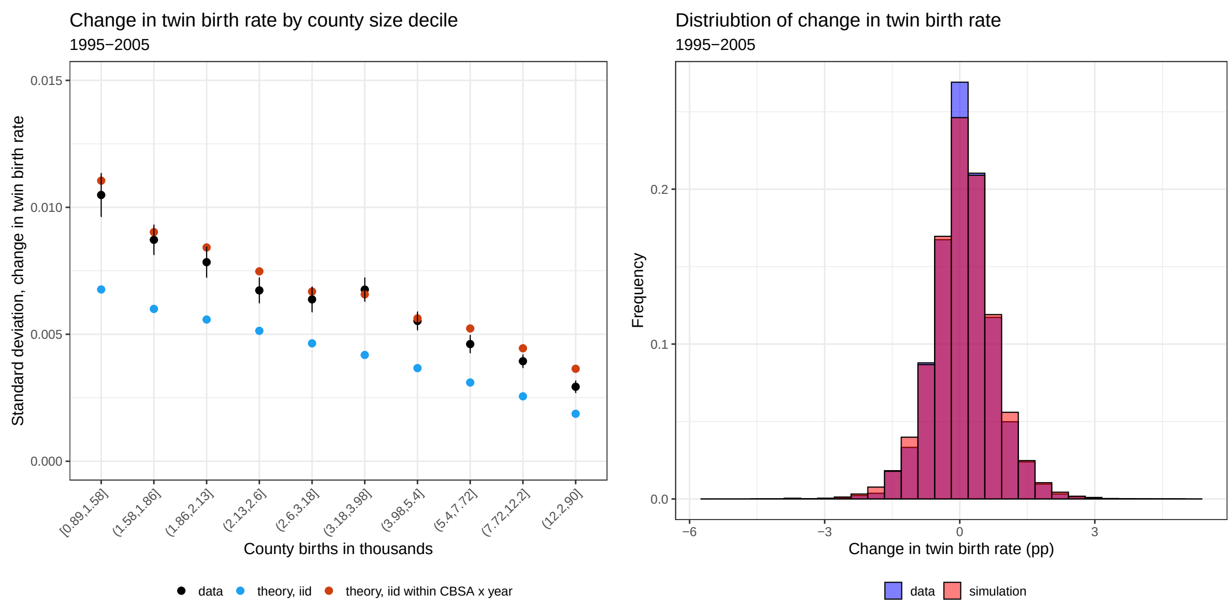
To understand variation in the change in the twin birth rate, the left panel of Figure 2 com-

[22] This is because $\Delta \log \Pr(Twins) \approx (\Pr(Twins) - 1) - (\Pr(Twins)_{-1} - 1) = \Delta \Pr(Twins)$.

pares observed variation to theoretical benchmarks that would satisfy conditions in Proposition 1.[23] We bin county-years into deciles based on the number of births, and plot the empirical and theoretical standard deviation of the change in the twin birth rate. Theoretical benchmarks are calculated by assuming that county-level twin births are iid draws from national (blue) or CBSA-by-year specific (red) binomial distributions. (see Appendix Section D.6 for details).

Finite sample variation accounts for about two-thirds of the unconditional standard deviation across the county size distribution. The remaining variation can be explained by differences in expected twin birth rates across CBSAs and years. As shown in Appendix Section D.4, the CBSA fixed effects absorb these sources of variation.

Figure 2: Actual and theoretical variation in twin birth rate



Source: Author's calculations using CDC natality data from 1995-2005. The right panel plots the standard deviation of the year-over-year change in the twin birth rate (on a scale from 0 to 1) within county birth deciles, in the data and under different distributional assumptions. Spikes plot 95% confidence intervals calculated using the Delta method. Appendix Section D.6 provides details. The left panel uses CDC natality data from 1995-2005 to compare the empirical and simulated change in the twin birth rate. A two-sample Kolomogorov-Smirnov test fails to reject the null that observations are drawn from the same distribution at a 5% significance threshold, with p-value = 0.12. Appendix Section D.6 provides details.

Finite sample variation is high enough to explain within-CBSA variation in twin birth rate changes. Does the empirical distribution appear to come from finite sample variation? The right panel of Figure 2 plots the empirical distribution of the year-to-year change in the twin birth rate against simulations using iid binomial draws from by CBSA-by-year distributions. Given local birth cohort sizes, there is significant year-to-year variation in the empirical twin birth rate, with frequent changes above 1pp (∼30% of the mean). Moreover, the simulated

[23] We show that these distributions satisfy Prop. 1 in Example 1 in Appendix Section D.4. Appendix Figure D.8 presents a version of the left panel but in levels, rather than changes, of the twin birth rate.

and empirical distributions align (p-value of two-sample Kolomogorov-Smirnov test = 0.12)[24]

Markets may also differ in expected twin birth rates due to maternal characteristics, potentially confounding estimates. There is no variation in the expected twin birth rate for the one-quarter to one-third of twin births that are monozygotic ("identical").[25] The remaining births are dizygotic ("fraternal"), which occur naturally due to excess secretion of certain reproductive hormones that control the release of eggs from ovarian follicles. Fraternal twinning runs in families – with sisters and daughters of mothers with fraternal twins about twice as likely to have twins – due to high heritability of the factors behind secretory drive in the hypothalamic-pituitary system. The fraternal twinning rate also rises with age as follicle density declines (Hoekstra *et al.* 2008). In-vitro fertilization (IVF) can result in fraternal twins if multiple embryos are transferred. However, IVF is rare, accounting for less than 1.2% of births over our study period,[26] and explains almost none of the cross-sectional variation in the twin birth rate.[27] Public health research attributes the recent increase in the twin birth rate to better maternal health and increased age rather than IVF (Hoekstra *et al.* 2008, Tandberg *et al.* 2007).

After conditioning on CBSA, we find that *changes* in the twin birth rate are not explained by local demographics, suggesting that maternal characteristics do not confound estimates. If finite sample variation drives within-CBSA twin birth rate variation, then within CBSA, local demographics should not predict the twin birth rate. Table D.11 verifies this, both in levels and first differences. None of the local demographic or financial characteristics are statistically significant predictors of the twin birth rate at the 5% level (across specifications, two in 40 are significant at the 10% level). Most within-CBSA variation relevant for the identifying assumption is not explained by these characteristics, given the within-CBSA R-squared of 0.21%. This is likely because maternal characteristics across geography that might create differences in the *level of* the twin birth rate are sticky, and so cannot explain *changes*. Rather, these results are consistent with stochasticity driving within-CBSA changes.

Two robustness exercises described in Appendix Section D.9 verify that maternal charac-

[24]In both panels of Figure 2, the theoretical variance appears slightly higher than the data. There are two reasons why. First, the theoretical benchmarks have (unplotted) standard errors, because the twin birth rate at the CBSA-by-year level is estimated. Second, we find that the twin birth rate is mean reverting. This is to be expected; if the genetic component of twinning is iid across the population, then after a year with an unusually high number of twins, the remaining population has an unusually low propensity to have twins. Appendix Section D.9.2 presents estimates robust to county-level mean reversion.

[25]"There are no clear associations between m[ono]z[ygotic] twinning and maternal, environmental or genetic factors and the mechanisms have not been identified" (Hoekstra *et al.* 2008).

[26]As detailed below, we use data on twin births from 1995-2005. In 1995, 0.5% of births used IVF (Guyer *et al.* 1997, Wright *et al.* 2006). In 2004, 1.2% of births used IVF (for Disease Control *et al.* 2004, Hamilton *et al.* 2006).

[27]Changes in Assistive Reproductive Technology (ART) utilization, of which IVF is a subset, only explain 0.13% of the variation in changes to the twin birth rate. This is possibly due to longstanding CDC guidance recommending single embryo transfer (Practice Committee of the Society for Assisted Reproductive Technology 2006). See Appendix Section D.5.1 for details.

teristics do not drive our results. First, we exclude twin births to older women, which strips out 75% of IVF births. Second, we control for the local change in the expected twin birth rate directly, which we prove ensures only finite sample variation drives estimates.

Twin birth rate predicts moves – instrument relevance. Having twins rather than a single child is a large shock to family size. For a given family, having twins causes moves around important childhood events, such as birth and starting school. For a local housing market, a large twin birth cohort creates predictable housing market churn around these events.

We estimate the effect of changes in twin birth rates on housing market churn by estimating:

$$\Pr(Move)_{jt} = \alpha_j + \omega_{c(j),t} + \sum_{a \in A} \beta_a p_{j,t-a} + \varepsilon_{jt} \tag{12}$$

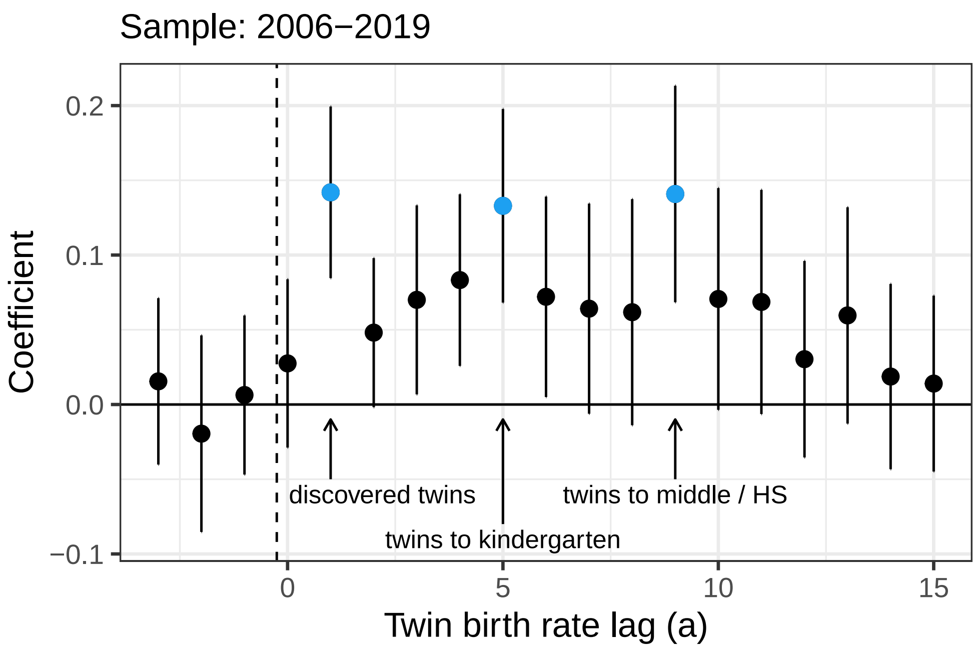
where $\Pr(Move)_{jt}$ is the probability that a household in zip code j moves in year t, α_j is a zip code fixed effect, $\omega_{c(j),t}$ is a time-varying CBSA effect, and $p_{j,t-a}$ is the twin ("plurality") birth rate in zip code j in calendar year $t-a$.[28] The coefficient of interest, β_a, is the effect of the twin birth rate a years prior on moves in the current year, when twins are aged a. Assuming that ε_{jt} is conditionally uncorrelated with $p_{j,t-a}$ – true if p_{jt} is an iid draw from a CBSA-by-year specific distribution – we can estimate β_a with a fixed effects regression.

Figure 3 plots estimates of β_a from equation (12), with $A = [-3, 15]$. Estimated lead effects are statistically insignificant, supporting that twin births are unexpected for households. Having twins generally increases local moves, consistent with parents moving to accommodate an unexpectedly larger family. The increase in moves aligns with meaningful milestones in twins' life – when twins are born and parents realize their family is larger than expected; when twins enter kindergarten at age 5; and when twins turn 9, around the start of middle school. The effect of twin births dissipates after 15 years, consistent with children finishing high school and leaving the house. Appendix Section D.7.1 conducts a calibration exercise to assess the magnitude of the estimated coefficients. Importantly, our estimates imply that local twin births have spillover effects on moves by households who do not have twins by leaving vacancies filled with moves by new households, who leave their own vacancies that induce moves, and so on. Our calibrated spillover effect is similar to recent estimates by French and Gilbert (2023) and Anenberg and Ringo (2022).

Based on these estimates, we only use variation in the twin birth rate until 2005. After 2005, the twin birth rate has a direct effect on moves during 2021-23, which impacts effective existing home supply by freeing up more units. Because this plausibly impacts prices as well,

[28] Data on twin births is only available at the county level. We map counties to zip codes using the procedure described in Appendix Section B.3.

Figure 3: Twin birth rate predicts moves during twins' childhood.



Source: Author's calculations using CDC natality data from 1995-2019 and Equifax Credit Risks Insight Servicing data from 2006-2019. The figure plots fixed effects estimates of β_a from equation (12), along with 95% robust confidence intervals clustered at the county level.

it violates the exclusion restriction.

Twin instrument construction and power. To predict the outstanding mortgage rate distribution in year t, we first predict moves from 1995 to t using twin birth rates from 1995-2005. We then convert these predicted moves into predicted origination shares for each year using estimated mortgage duration. Finally, we correlate predicted origination shares shares with the average mortgage rate in each year to predict the interest rates on outstanding mortgages.

Using the notation of Proposition 1, we predicted moves by summing the product of the lagged twin birth rate and its estimated effects on moves in τ:[29]

$$v_{j\tau}^{twin} \equiv \sum_{a=\tau-2005}^{\tau-1995} \hat{\beta}_a p_{j,\tau-a} \tag{13}$$

We form $r_{jt}^{twin} = \sum_{\tau \leq t} w_{j\tau,t}^{twin} r_\tau^m$ where $w_{j\tau,t}^{twin} \equiv \frac{K(t-\tau)v_{j\tau}^{twin}}{\sum_{\tau'<t} K(t-\tau')v_{j\tau'}^{twin}}$, and $K(a)$ is the probability that a mortgage originated a years prior remains outstanding. We estimate $K(\cdot)$ using microdata on mortgage duration from ICE, McDash®, described in Appendix Section D.5.3. The estimation of $K(\cdot)$ is national to avoid confounds from differential repayment rates by zip code.

[29]To improve power in our instrument, we estimate β_a by parameterizing β in equation (12) as a fifth-order polynomial with fixed effects for years one, five, and nine – see Appendix Section D.5.2. Results are essentially identical if we use fixed effects estimates shown in Figure 3 instead.

Appendix Figure D.16 shows the steps in construction of predicted shares $w^{twin}_{j\tau,2021}$ for zip codes at the 20th, 50th, and 80th percentiles of $r^{twin}_{j,2021}$. The top-left panel plots the twin birth rate from 1995-2006, along with the average 30-year fixed mortgage rate from 1995-2023. High or low average values of the twin birth rate do not clearly map to high or low values of the instrument, illustrating how variation in the twin-driven predicted interest rate is based on *changes* in twin birth rates, rather than *level* differences. The top-right panel plots predicted moves $v^{twin}_{j\tau}$ using the history of twin birth rates for each year $\tau \in \{1995, 2021\}$ (demeaned by year for readability). The bottom-left down-weights earlier moves using estimated $K(\cdot)$, and plots $K(2021-\tau)\cdot v^{twin}_{j\tau}$. The bottom-right panel eliminates variation due to level differences by re-normalizing predicted moves to sum to one, and plots the resulting predicted origination shares $w^{twin}_{j\tau,2021}$. Zip codes with low predicted outstanding interest rates in 2021, plotted in blue, have low predicted origination shares in the early 2000s, when interest rates were high, and high predicted origination shares in the mid-2010s, when interest rates were low. Zip codes with high predicted outstanding rates in 2021, plotted in green, show the opposite pattern.

Predicted twin-driven origination shares explain a meaningful, although small, fraction of observed variation in origination timing. The left panel in Appendix Figure D.17 plots the annual cross-sectional standard deviation of $w^{twin}_{j\tau,2021}$, in both levels (blue) and as a percent of the actual cross-sectional standard deviation of origination year shares, $w^{O}_{j\tau,2021}$. Predicted variation is between 1-2% of observed variation. Appendix Table D.12, Column (1) regresses $w^{O}_{j\tau,2021}$ on $w^{twin}_{j\tau,2021}$, and confirms that twin-driven origination shares both (i) positively predict actual origination shares, and (ii) explain a little less than 2% of within-CBSA variation in origination timing.

Variation in predicted origination timing generates meaningful, although small, variation in predicted outstanding interest rates. Appendix Figure D.18 plots the distribution of predicted $r^{twin}_{j,2021}$ across zip codes, which spans about 15 basis points. This variation is of the same order of magnitude as previous research estimating causal effects of interest rate variation on equilibrium prices.[30] This difference is not due to small changes in all household's interest rates, but rather by changes in the fraction of households with very low (below 2.9%) or high (above 5.5%) interest rates (see Appendix Section D.7.2). The small average change therefore belies a significant shift in economic incentives for a portion of the population.

The twin-driven predicted outstanding interest rate is a statistically strong predictor of the actual average outstanding interest rate. Column (2) of Appendix Table D.12 regresses $r^{O}_{j,2021}$

[30] Adelino *et al.* (2025) identify cross-sectional price effects from a change in average borrowing costs of 6-8 basis points due to the GSE subsidy relative to jumbo mortgages at the non-conforming loan threshold. Loutskina and Strahan (2015) estimate that a 1pp increase in the fraction of local borrowers at the non-conforming loan threshold – that is, a 1pp increase in the fraction of borrowers receiving this 6-8 bps mortgage subsidy – causes 0.5% higher price growth at the CBSA level.

on $r^{twin}_{j,2021}$. The coefficient is statistically equal to one with high F and Wald test statistics.[31]

Finally, twin-driven moves affect the mortgage rate for a meaningful fraction of existing homeowners. It is therefore plausible that rate lock impacts incentives for enough households to move equilibrium house prices. To illustrate, we select zip codes with $r^{twin}_{j,2021}$ 1.96 standard deviations above the average, and compare average predicted origination shares $w^{twin}_{j\tau,2021}$ for each year with zip codes with $r^{twin}_{j,2021}$ 1.96 standard deviations below the average. The right panel in Appendix Figure D.17 plots the results. As expected, zip codes with a high predicted outstanding interest rate had relatively fewer originations in recent years, when mortgage rates were relatively low, and relatively more originations in earlier years, when mortgage rates were relatively high. In a given year, the difference in origination probability is at most 0.08%.

Crucially, the figure illustrates that what is relevant for the outstanding distribution in 2021 is not the difference in any *one* year, but the *cumulative* difference over 25 years. Over 25 years, twin-driven moves reshuffle origination timing for close to 1% of households. Previous research on housing supply and speculative activity shows that variation impacting about 1% of local housing units moves equilibrium house prices.[32] Therefore, changing existing-home sales incentives for 1% of the population should have detectable effects on local house prices. Appendix Section D.7.2 provides additional details.

First births instrument overview. We also construct an instrument using variation in family formation timing. We call this the "first births" instrument, as it measures family formation using the number of local births corresponding to mothers' first maternity.

There are two challenges to predicting moves using first births rather than the twin birth rate. First, first births are tightly related to the local age distribution, which is directly related to moves. Second, first births mechanically negatively correlate with future move rates, since new families increase the number of households and so reduces moves as a fraction of households.

We address the first issue by flexibly controlling for the local age distribution, and the second by estimating the effect of first births on moves in *levels*, rather than *rates*. This affects how we predict moves relative to the twins instrument. We model moves as:

$$Moves_{jt} = \alpha_j + \omega_{c(j),t} + \sum_{a\in A}\beta_a B_{j,t-a} + \delta_t Pop_{jt} + \sum_{l\in L}\pi_{lt}\Pr(Age_{it} = l|i \in jt) + \varepsilon_{jt} \tag{14}$$

[31] While the F and Wald test statistics clear common rules-of-thumb to rule out weak instrument problems, we do not emphasize this fact here because $r^{O}_{j,2021}$ is not our endogenous regressor.

[32] Single-family housing unit completions are typically 0.5-1% of the residential housing stock (take the ratio of COMPU1USA to ETOTALUSQ176N from the St. Louis Fed), and variation in supply elasticities across geography causes easily measurable house price effects (e.g. Baum-Snow and Han 2024). Additionally, Mian and Sufi (2022) find that activity by the 1.4% of the population that are housing speculators explained 100% of increased lending financed by non-core deposits in the lead-up to the 2008 housing crisis, which caused a 19.1% increase in MSA-level prices (moving from the bottom to top quartile of local exposure to non-core deposit financed lending).

where B_{jt} is the number of twin births in zip code j in year t, Pop_{jt} is the local population, and $\Pr(Age_{it} = l | i \in j)$ is the fraction of households in j of age l in year t.

Appendix Figure D.19 shows fixed effects estimates of β_a. Consistent with some family planning, some of the estimated effects up to and including zero are statistically positive. Moves then fall for several years, implying challenges of moving with an infant. There are statistically positive effects around years 4 and 7. This is consistent with moves around subsequent child births and median birth spacing of 3-4 years (Martinez and Daniels 2023), or moves around the start of kindergarten and elementary school. The effect dies out after 15 years.

We predict moves as:[33]

$$v_{j\tau}^{births} \equiv \sum_{a=\tau-2005}^{\tau-1995} \hat{\beta}_a B_{j,\tau-a} + \overline{Moves}_j \tag{15}$$

where $\overline{Moves}_j$ is the average in zip code j from 2006-2019. Including average moves appropriately re-centers level differences across location; formally, the identifying assumption is that the change in first births as a fraction of average moves is mean independent of ε_{jt}.[34]

Given $v_{j\tau}^{births}$, we form r_{jt}^{births} and MVP_{jt}^{births} as with the twins instrument. Appendix Figure D.20 shows the distribution of r_{jt}^{births} for $t = 2019, 2021$. Relative to r_{jt}^{twins}, there is more variation in the instrument, with a standard deviation slightly more than twice as high. Appendix Table D.13 regresses the actual average outstanding interest rate in 2021 against $r_{j,2021}^{births}$, with Column (4) directly comparable to Column (2) in Appendix Table D.12. The coefficient on $r_{j,2021}^{births}$ is statistically equal to one, and the within-CBSA R-squared is 6.9% (compared to 1.3% for the twins instrument). We should therefore expect the first births instrument to have slightly more power, despite some remaining identification concerns as discussed above.

4.3.2 Origination timing controls.

We compare estimates using our origination timing instruments to an approach that attempts to address expected origination timing confounds directly. This approach modifies the analysis using the Treasury rate instrument described in Section 4.2 in two ways.

First, we add controls to remove confounds due to differences in how local housing market churn co-moves with the interest rate. Specifically, we control for the fraction of originations from each calendar quarter q: $\sum_q \nu_q \sum_{\tau \in q} w_{j\tau,2021}^{O}$. This ensures the instrument only uses the within-quarter correlation between origination shares and the aggregate interest rate. Ap-

[33] As with the twins instrument, we slightly increase power by estimating β_a as a high-dimensional polynomial with indicators at meaningful milestones. See Appendix Section D.8.2 for details.

[34] See Appendix Section D.8.1 for how this fits into in the framework for Proposition 1 and a derivation of the identifying assumption.

pendix Section D.3 shows that this approach accounts for an unobserved origination quarter fixed effect for property-level price growth, and explains how the resulting estimating equation relates to the motivating model in Section 1.

Second, we construct the Treasury rate instrument so that it does not use variation from 2021-23. This removes confounds related to correlation between recent demand shocks and outstanding mortgage rates. This "lagged Treasury rate" instrument replaces the share of mortgages originated in years after 2019 with pre-2019 averages, holding fixed the number of years since origination and re-normalizing so that shares sum to one. Specifically, we replace the share of mortgages in 2021 originated in 2020 with the pre-2019 average share of mortgages outstanding in year t originated in $t-1$.[35] This gives lagged origination shares $w^L_{j\tau,t}$, used to form rates $r^L_{jt} \equiv \sum_{\tau \leq t} w^L_{j\tau,t} r_{m,\tau}$ and instruments $MVP^{\text{Lag T-rate}}_{jt}$ with r^L_{jt} in place of r^O_{jt}.

4.3.3 Results.

Panel (b) of Table 4 compares estimates of β using approaches that account for origination timing with estimates from Section 4.2 where origination timing is endogenous. Column (1)-(3), respectively, show two-stage least squares estimates using the twins instrument, the first births instrument, and the lagged Treasury rate instrument combined with origination quarter controls. Instruments are strong, with first-stage F-statistics of at least 20.

Our preferred specification in Column (1) uses variation from the twin birth rate. The estimates indicate that house price growth increases by 2pp due to a 1pp increase in average scaled mortgage value. This is more than three times larger than IV estimates from Panel (a), Column (3) with endogenous origination timing, and ten times larger than fixed effects estimates in Panel (a), Column (1). Endogenous origination timing therefore puts downwards bias on the estimate of β, and accounting for this endogeneity is quantitatively important.

Our three approaches to address origination timing all give similar estimates. Panel (b) in Appendix Table D.14 shows these estimates are robust to including additional CBSA-by-demographic controls. This builds confidence that our methodologies remove confounding variation due to origination timing and estimate β consistently.

Average scaled mortgage value in 2023 was 2.9%. Linearly aggregating the estimate in Column (1) of Table 3, panel (b), this implies that eliminating mortgage value would reduce house prices by 6.9%, reversing 28% of house price growth over 2021-23. Furthermore, variation in scaled mortgage value growth can account for about 78% of cross-sectional variation

[35]Formally, for τ up to 2019, we set $v^L_{j\tau,2021} = w^O_{j\tau,2021}$, and thereafter let $v^L_{j\tau,2021} = (N_{19})^{-1} \sum_{t \leq 2019} \sum_{\tau':y(\tau')=t+y(\tau)-2021} w^O_{j\tau',t}$ where N_{19} is the number of year-months before 2019 and $y(\tau)$ is the year of year-month τ. We follow similar steps for t other than 2021.

in house price growth over the 2021-23 period.[36]

We present results from three additional robustness exercises in Appendix D.9. Section D.9.1 excludes twin births to older mothers, where IVF use is more common. Section D.9.2 present twin instrument estimates using a methodology that controls for local market-specific expected changes in the twin birth rate, ensuring identifying variation is exclusively finite sample variation relative to the expectation. Section D.9.3 describes another instrument that uses predicted moves based on the lagged age distribution, using the fact that households are differentially likely to move at certain ages.[37] Estimates from the alternative twin instruments are very similar to main estimates from the twin instrument, and estimates from the age distribution are similar to those in Panel (b).

Table 5 presents specifications where the outcome is the probability that a household moves from the local owner-occupied housing market from 2022-2023, with Appendix Table D.15 adding controls. In all specifications, coefficients are negative, more so for our IV specifications that estimate greater price effects.

Table 4: Effect of rate lock on local equilibrium prices.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

	Panel (a). Endogenous origination timing.			Panel (b). Origination timing instruments.		
Dependent Variable:	%Δ HPI, 21-23			%Δ HPI, 21-23		
Model:	(1)	(2)	(3)	(1)	(2)	(3)
Variables						
Δ MVP, 2021-23	0.1658***	0.1925***	0.6148***	2.048***	2.196***	2.338***
	(0.0525)	(0.0623)	(0.2186)	(0.7180)	(0.5833)	(0.6735)
Fixed-Effects						
CBSA	Yes	Yes	Yes	Yes	Yes	Yes
Rent/price x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demo x CBSA controls?	No	No	No	No	No	No
Origination qtr controls?	No	No	No	No	No	Yes
Instrument?		T-rate x LTV	T-rate	Twin	First birth	Lag T-rate
Fit statistics						
Observations	6,701	6,701	6,701	5,044	5,336	6,701
R^2	0.64317	0.64314	0.63332	0.42677	0.40443	0.49056
Within Adjusted R^2	0.00356	0.00346	-0.02394	-0.46791	-0.54976	-0.43302
F-test (1st stage), Δ MVP, 2021-23		19,697.9	210.69	19.626	31.957	26.542
Wald (1st stage), Δ MVP, 2021-23		6,182.4	51.501	7.1452	11.493	16.917

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Author's calculations using Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC natality data. Each column reports an estimate of β in equation (10) using a different methodology. Panel (a) reports fixed effects and IV estimates with endogenous origination timing as described in Section 4.2. Panel (b) reports IV estimates that address endogenous origination timing, as described in Section 4.3.

[36]This comes from calculating $\sqrt{\beta \text{Var}(\Delta MVP_{2021})/\text{Var}(\%\Delta HPI_{2021})} = 0.78$.

[37]Specifically, we use pre-2019 data to estimate the effect of the full age distribution – the fraction in each single-year age bin – in year $\tau - 1$ in zip code τ on period-τ moves. We obtain fitted values for predicted year-τ moves based on the local year $\tau - 1$ age distribution, set these as $v_{j\tau}^{\text{age}}$, and proceed as with other instruments.

Table 5: Effect of rate lock on local outflows
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

	Panel (a). Endogenous origination timing.			Panel (b). Origination timing instruments.		
Dependent Variable:	Pr(move), 2022			Pr(move), 2022		
Model:	(1)	(2)	(3)	(1)	(2)	(3)
Variables						
Δ MVP, 2021-23	-0.0307**	-0.0384**	-0.1116*	-0.5459**	-0.5208***	-0.6896***
	(0.0157)	(0.0169)	(0.0595)	(0.2532)	(0.1473)	(0.2110)
Fixed-Effects						
CBSA	Yes	Yes	Yes	Yes	Yes	Yes
Rent/price x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demo x CBSA controls?	No	No	No	No	No	No
Origination qtr controls?	No	No	No	No	No	Yes
Instrument?		T-rate x LTV	T-rate	Twin	First birth	Lag T-rate
Fit statistics						
Observations	6,701	6,701	6,701	5,044	5,336	6,701
R^2	0.30216	0.30212	0.29676	0.02201	0.04213	0.13691
Within Adjusted R^2	0.00093	0.00086	-0.00681	-0.31409	-0.29004	-0.26374
F-test (1st stage), Δ MVP, 2021-23		19,697.9	210.69	19.626	31.957	26.542
Wald (1st stage), Δ MVP, 2021-23		6,182.4	51.501	7.1452	11.493	16.917

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Author's calculations using Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC natality data. The table has the same format as Table 4, except the outcome variable is the probability of moving from 2022-23. See notes to Table 4 for details.

4.4 Mechanisms

The model in Section 1 predicts that the effect of mortgage value on price should be higher when γ_o is higher, where γ_o is the fraction of owner-occupants choosing whether to exit the local owner-occupied housing market. This is because higher γ_o increases the impact of existing owners' choices on the number of available housing units. To test this prediction, we proxy for the effect of γ_o in two ways. First, we use heterogeneity in *physical* housing supply elasticity, which *reduces* the relevance of existing homeowner's choices on available housing units. Second, we use heterogeneity in predicted *effective* existing-home supply, based on predicted 2023 moves driven by the 2019 age distribution. Prices should be especially sensitive to rate lock in places where the number of existing homes available would be high absent disruption due to mortgage value – for example, places where many households neared retirement in 2023, and would move away absent rate lock. We estimate β_3 in the following specifications, where $\hat{\gamma}_{jt}$ is either an estimate of the local housing supply elasticity or predicted 2023 outflows:

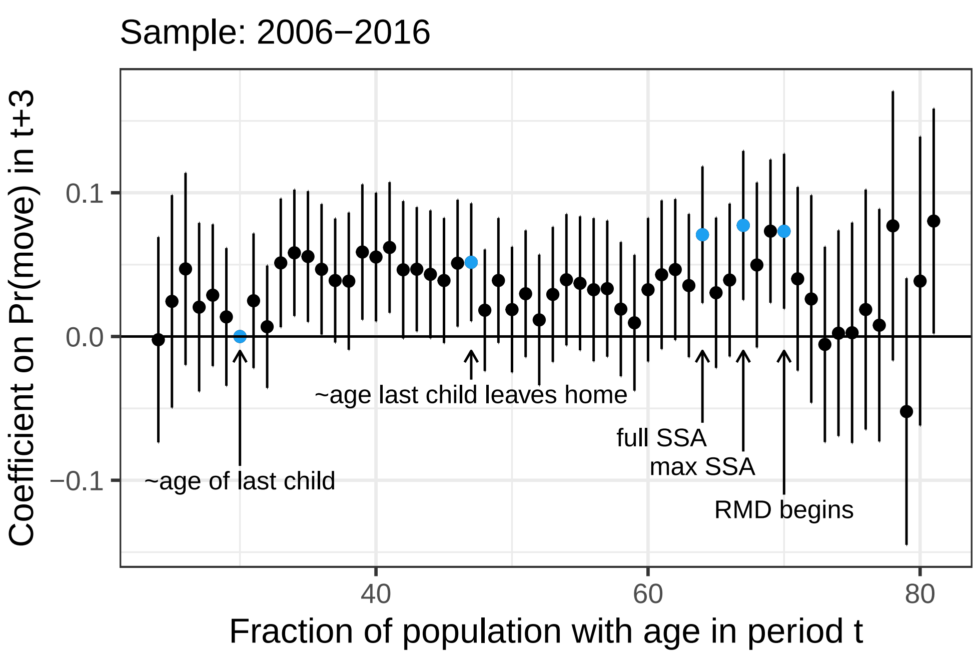
$$\begin{aligned}\%\Delta\overline{P}_{j,2023} = \beta_1 \Delta\overline{MVP}_{j,2023} + \beta_2 \hat{\gamma}_{j,2023} + \beta_3 \hat{\gamma}_{j,2023} \times \Delta\overline{MVP}_{j,2023} \\ + \eta'_{c(j)} X_{j,2023} + \omega_{c(j)} + \varepsilon_{j,2023}\end{aligned} \tag{16}$$

Measuring physical supply elasticity and expected outflows. We take the owner-occupied housing-unit weighted average of estimated Census-tract level long run new unit supply elasticities from Baum-Snow and Han (2024) to estimate the local physical supply elasticity.

We predict outflows from 2022-23 using the local age distribution in 2019 and the pre-2019 historical relationship between three-period ahead outflows and the local age distribution. We first estimate coefficients in the following specification, estimated on pre-2019 data:

$$\Pr(Move)_{j,t+3} = \alpha_j + \omega_{c(j),t} + \sum_{l \in L} \pi_l \Pr(Age_{it} = l | i \in jt) + \varepsilon_{j,t+3} \tag{17}$$

Figure 4: Effect of age distribution on moves.



Source: Author's calculations using Equifax Credit Risks Insight Servicing data. The figure plots fixed effects estimates of π_l in equation (17).

Figure 4 plots coefficient estimates. Places with many households early adulthood and early retirement see higher outflows three years later. A higher fraction aged 30 predicts lower moves three years later, consistent with 33 as the median parental age at last child and lower mobility when households have young children. Moves occur around age-related retirement milestones, when a higher fraction are currently aged 64 (64+3=67 is the full SSA retirement age), 67 (67+3=70 is the max SSA retirement age), and 69-70 (avg of 69-to-70+3=72.5, the age when required minimum distributions from tax-deferred savings accounts begin.

We predict 2022-23 outflows using 2019 ages as $\sum_{l \in L} \pi_l \Pr\big(Age_{i,2019} = l | i \in j, 2019\big)$. Appendix Figure D.21 shows that there significant variation in predicted outflows that can be used to estimate effects.

Instruments and results. We instrument for $\Delta\overline{MVP}_{j,2023}$ and $\hat{\gamma}_{j,2023} \times \Delta\overline{MVP}_{j,2023}$ in equation (16) with our LTV-by-Treasury rate instruments and their interaction with $\hat{\gamma}_{j,2023}$. We use the LTV-by-Treasury rate instrument because it has the greatest power, and here we aim to test what relationships hold directionally rather than assess magnitudes.

Table 6 presents results. Column (1) shows results proxying for $-\hat{\gamma}_{j,2023}$ with the physical supply elasticity. A one-standard deviation increase in supply elasticity reduces the marginal effect of scaled mortgage value on price growth by about two-thirds. This suggests that rate lock primarily has a meaningful price effect due to low supply elasticities in some markets.

Column (2) shows results that proxy for $\hat{\gamma}_{j,2023}$ using predicted outflows from the lagged age distribution. This specification also includes direct controls for $\Pr(Age_{it} = l | i \in jt)$, so that estimates are driven only by the correlation between the age distribution and outflows not the direct effect of the age distribution on prices. Higher predicted outflows reduce price growth, as is intuitive as demand falls. Consistent with the theory from Section 1, the coefficient on the interaction term is positive. This means rate lock impacts prices the most in markets that would have experienced higher outflows in its absence. A one standard deviation increase in predicted outflows implies a 55% increase in the marginal price effect of higher scaled mortgage value.

Table 6: Cross-sectional price effects, heterogeneity.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

Dependent Variable:	%Δ HPI, 21-23	
Model:	(1)	(2)
Δ MVP	0.36^{***}	0.22^{***}
	(0.057)	(0.071)
New unit supply elast, std	0.065^{***}	
	(0.014)	
Δ MVP x new unit supply elast, std	-0.24^{***}	
	(0.0556)	
Pred. 2023 outflows, std		-0.031^{**}
		(0.013)
Δ MVP x Pred 2023 outflows, std		0.12^{**}
		(0.055)
Fixed-Effects		
CBSA and Demo x CBSA controls?	Yes	Yes
Instrument?	Δ 30-year T-rate	Δ 30-year T-rate
Age dist controls?	No	Yes
Observations	5,211	6,446

Source: Author's calculations using Equifax Credit Risks Insight Servicing. The table presents coefficient estimates for equation (16) using the LTV-by-Treasury rate instrument. Column (1) proxies for $-\hat{\gamma}_{j,2023}$ using physical supply elasticities. Column (2) proxies for $\hat{\gamma}_{j,2023}$ using predicted outflows based on the three-period lagged age distribution.

5 Empirical housing demand model with fixed-rate mortgages

The previous section establishes that local housing markets relatively more exposed to rate lock experienced higher price growth when interest rates rose from 2021-23. Higher interest rates also had direct effects in all markets during this period that affected housing demand. Renters considering a home purchase may have faced liquidity constraints preventing them from purchasing a home at new, higher rates. Furthermore, the opportunity cost of capital increased for existing owners and renters, depressing owner-occupied housing demand.

This section presents an empirical model of housing demand to estimate the net effect of these forces on equilibrium house prices. The model introduces quantitatively important features of housing demand, such as uncertainty about the future economic environment, credit constraints, a lifecycle household profile, mobility frictions, and transactions costs. Sections 5.1-5.3, respectively, describe and parameterize the model; estimate parameters using moments from the November 2016 event study described in Section 3; and show the model matches important untargeted moments, including the cross-sectional effect of scaled mortgage value on price growth from Section 4. Section 5.4 simulates the model-predicted impact of the 2021-23 rate increase on equilibrium house prices, holding fixed other factors influencing housing demand. Estimates are preliminary and may change as the paper is finalized.

5.1 Model description.

A heterogeneous, fixed population chooses between otherwise undifferentiated owner-occupied and rental housing units in fixed supply. Households are differentiated by their age, financial wealth, housing debt, and current unit type.

In each period, credit-constrained, risk-averse households who occupy exactly one housing unit at a time make two dynamic choices under uncertainty. First, they choose non-housing consumption, financed through current income and financial wealth. Second, they make a discrete housing choice of whether to stay in their current unit, switch to a different owner-occupied unit, or switch to a different rental unit. Purchases of owner-occupied units are financed with non-assumable, amortizing fixed-rate mortgages. Households face uncertainty over the interest rate, per-period rental unit costs, and owner-occupied house prices. Households are also uncertain over how their needs and preferences for units changes over time. Households live to age $\overline{A}$, and have preferences over bequests left after death.

In each period, the price of owner-occupied housing adjusts to equate net moves between the owner-occupied and rental housing market. This price depends on the household state distribution, the realized risk-free rate, and realized rents. Changes in the risk-free rate affect housing demand by changing the rate on saving and borrowing. Outstanding fixed-rate mortgages are valuable when rates rise because they allow households to borrow at below-market rates, attenuating the negative effect of rising rates on housing demand. The model is partial equilibrium, in that while owner-occupied house prices are endogenous, interest rates and rents follow an exogenous process.

Household problem overview. In each discrete period t, households i make discrete housing choice d_{it} and continuous non-housing consumption choice c_{it}. Housing choice is between the current unit $d_{it} = 0$, a different rental unit $d_{it} = \ell$, and a different owner-occupied unit $d_{it} = o$.

Housing delivers flow utility $h(a_{it}, d_{it}, k_{it}, \boldsymbol{\varepsilon}_{it}, \chi_{it})$, a function of age a_{it}, current housing tenure $k_{it} \in \{o, \ell\}$ of either $o$wn or ℓease, and idiosyncratic household preferences $(\boldsymbol{\varepsilon}_{it}, \chi_{it})$, detailed below. Utility depends on both current status k_{it} and housing choice d_{it} to encode moving costs. Non-housing consumption delivers flow utility $u(c_{it})$. Households have preferences $v^b(W_{i,t+1})$ over bequests any financial wealth $W_{i,t+1}$ remaining after liquidation of any housing assets. The sequence of choices are assumed to maximize the lifetime expected present discounted value of additively-separable flow utility from housing and non-housing consumption:

$$E_t \left[\sum_{q \geq t}^{t+\bar{A}-a_{it}} \delta^{q-t} \left[h(a_{iq}, d_{iq}, k_{iq}, \boldsymbol{\varepsilon}_{iq}, \chi_{iq}) + u(c_{iq}) \right] + \delta^{\bar{A}-a_{iq}+1} v^b(W^b_{i,\bar{A}-a_{iq}+1}) \right] \tag{18}$$

Choices depend on the household state s_{it}, aggregate market conditions θ_t, and idiosyncratic household preferences $(\boldsymbol{\varepsilon}_{it}, \chi_i)$. The household state vector s_{it} includes a_{it}, k_{it}, financial wealth W_{it}, and if a household is an owner-occupant mortgage balances M_{it} and mortgage payment m_{it}. The vector of aggregate market conditions θ_t includes the risk-free return on savings r_t, lease costs ℓ_t, and P_t, the purchase price of owner-occupied housing.

Households may place different value on the characteristics of owner-occupied and rental units. Some households might prefer owner-occupied units for their larger size, location in suburban markets with family-oriented amenities, and greater customization, while others might prefer the urban amenities and low sweat equity of rental units. Given that it is likely persistent, an ideal approach would model heterogeneous preferences for each choice as a Markov process, adding at least one unobserved state variable for each housing choice. We adopt a tractable alternative that retains some of the important features of the more realistic approach.

Specifically, the iid binomial variable χ_{it} encodes whether i considers a move in period t, while $\boldsymbol{\varepsilon}_{it} = \{\varepsilon_{idt}\}_d \overset{\text{iid}}{\sim} G_{k_{it}}$ describes the unobserved value household i places on choice d, conditional on considering a move. This conceptually represents the arrival of some change in household circumstances – for example, marriage, divorce, birth of a child, or health shocks – that leads households to consider a move. The formal structure produces persistence in household choice without having to track additional state variables. A high draw of ε_{idt} will have persistent effects in future periods so long as $\chi_{iq} = 0$ for all $q > t$.

The model is in real dollars, with the exception of interest rates, which are nominal. We assume a constant rate of inflation ι.

Mortgage contracts, household state transitions, and constraints. Households finance home purchases using a fixed-payment mortgage with origination loan-to-value ratio of $1-\phi$. For a mortgage originated in period t:

$$M_{it} = (1-\phi)P_t, \quad m_{it} = A(r_{mt}, (1-\phi)P_t) \tag{19}$$

where r_{mt} is the mortgage interest rate and $A(r, M) \equiv M \cdot \frac{r_{mt}(1+r_{mt})^n}{(1+r_{mt})^n - 1}$ is the amortization formula for n the number of payments. Thereafter:

$$(1+\iota) \cdot M_{i,t+1} = M_{it} - \nu \cdot m_{it}, \quad (1+\iota) \cdot m_{i,t+1} = \mathbb{1}(M_{i,t+1} > 0) \cdot m_{it} \tag{20}$$

The mortgage balance transition depends on ν, rather than the origination interest rate. This eliminates a state variable, at the cost of slightly changing the convexity of the mortgage repayment schedule.[38] We pick ν so that a borrower with a fully-amortizing 30-year mortgage rate will repay within 30 years at the steady-state of the interest rate process, detailed below.

Aside from mortgage debt, households face credit constraints that prevent borrowing, so that financial wealth $W_{it} \geq 0$. Households use their financial wealth to finance housing and non-housing consumption, and save the residual in a risk-free asset. Financial wealth after borrowers make their housing choice is:

$$W'_{it} = \underbrace{W_{it} + y_a}_{\text{resources}} - \underbrace{\mathbb{1}_{\text{own},t} \cdot (m_{it} + \tau_{ft}) - \mathbb{1}_{\text{lease},t} \cdot \ell_t}_{\text{per-period housing costs}} + \underbrace{\mathbb{1}_{\text{sell},t} \cdot ((1-\tau_s)P_t - M_{it}) - \mathbb{1}_{\text{buy},t} \cdot \phi P_t}_{\text{capital gains net down pmt}} \tag{21}$$

Household resources include financial wealth and age-specific income, y_a. Per-period housing costs include property taxes and maintenance τ_{ft}. If a household decides to sell their current house, they receive the market price of their home less an ad valorem transactions cost τ_s representing fees paid to real estate agents, and must repay their outstanding mortgage balance at face value. Finally, a household makes a down payment when they buy a new home. Financial

[38]This change also impacts loan term, but in practice the change in term is less than our calibrated time step.

wealth earns r_t after households choose consumption:

$$W_{i,t+1} = (1 + r_{t+1} - \iota) \cdot \left(W'_{it} - c_{it}\right) \tag{22}$$

Define s'_{it} as equal to s_{it}, with W_{it} replaced with W'_{it}. In the terminal period when $a_{it} = \bar{A}$, bequests are $W^b_{it} \equiv \max(0, W_{i,t+1} + (1 - \tau_s)P_{t+1} - M_{i,t+1})$.

Location k_{it} transitions based on d, with $k_{i,t+1} = k_{it}$ if $d_{it} = 0$ and $k_{i,t+1} = d_{it}$ otherwise.

Timing and recursive formulation of household problem. When the period begins, households observe state $(s_{it}, \theta_t, \boldsymbol{\varepsilon}_{it}, \chi_{it})$. They make housing choices if $\chi_{it} = 1$, taking expectations over the evolution of aggregate state variables, which households expect to follow a first-order Markov process (detailed below). θ_{t+1} is realized, and households make non-housing consumption choices, taking expectations over future realizations of idiosyncratic preferences.[39] Finally, the period advances and $\chi_{i,t+1}, \boldsymbol{\varepsilon}_{i,t+1}$ are drawn.

The optimal choice of d and c satisfy the following Bellman equation, where the value function $V(\cdot)$ represents the present-value of expected lifetime utility:

$$V(s_{it}, \theta_t, \boldsymbol{\varepsilon}_{it}, \chi_{it}) = \begin{cases} h(a_{it}, d_{it} = 0, k_{it}, \boldsymbol{\varepsilon}_{it}) + E_\theta \left[V^c(s'_{it}, \theta_{t+1}) | s_{it}, \theta_t\right] & \text{if } \chi_{it} = 0 \\ \max_d \; h(a_{it}, d, k_{it}, \boldsymbol{\varepsilon}_{it}) + E_\theta \left[V^c(s'_{it}, \theta_{t+1}) | s_{it}, \theta_t, d\right] & \text{if } \chi_{it} = 1 \end{cases} \tag{23}$$

$$\text{s.t. } W'_{it} \geq 0, \text{ (19), (20), (21);}$$

$$V^c(s'_{it}, \theta_{t+1}) = \begin{cases} \max_{c \geq 0} \; u(c) + E_{\varepsilon,\chi} \left[V(s_{i,t+1}, \theta_{t+1}, \boldsymbol{\varepsilon}_{it}, \chi_{i,t+1}) | s'_{it}, c\right] & \text{if } a_{it} < \bar{A} \\ \max_c \; u(c) + v^b(W^b_{it}) & \text{if } a_{it} = \bar{A} \end{cases} \tag{24}$$

$$\text{s.t. } W_{i,t+1} \geq 0, \; a_{i,t+1} = a_{it} + 1, \text{ (22).}$$

Let $d^*(s, \theta, \boldsymbol{\varepsilon}, \chi)$ denote the optimal location choice.

Equilibrium. The risk-free rate r_t and lease costs ℓ_t follow a correlated first-order Markov process. Owner-occupied house prices adjust in equilibrium to equalize total flows to and from owner-occupied and rental housing. Let N_o, N_ℓ denote the fixed number of owner-occupied and rental units; $\pi_{dk}(P; s, r, \ell) \equiv \Pr_{\varepsilon,\chi}[d = d^*(s, \theta, \boldsymbol{\varepsilon}, \chi)]$ denote the probability of choosing alternative d from location k given state s, and G_{kt} the distribution of household states in

[39] θ_{t+1} is realized after housing but before non-housing consumption choices are made so that households don't know their origination interest rate when making housing choices. This is so that the identifying assumption behind the November 2016 event study, which we will use to estimate preference parameters, holds in the model.

location k. Equilibrium prices satisfy:

$$\underbrace{\sum_{k\in\{o,\ell\}} N_k \int_s \pi_{ok}(P_t; s, r_t, \ell_t) dG_{kt}}_{\text{owner-occupied inflows}} = \underbrace{\sum_{d\neq 0} N_o \int_s \pi_{do}(P_t; s, r_t, \ell_t) dG_{ot}}_{\text{owner-occupied outflows}} \tag{25}$$

Discussion. In our model, households react to a change in the risk-free rate for several reasons. First, higher rates increase the return on savings, making investment in owner-occupied housing less attractive. Second, higher rates increase per-period payments on new mortgages. This discourages new purchases. Third, higher rates reduce the present-value of future income, a negative wealth effect. And finally, higher rates increase the value of existing fixed-rate mortgages, by depressing the present-value of their payments.

Crucially, households care about these effects to the extent they impact the present value of future non-housing consumption. Non-housing consumption in our model endogenously depends on income, liquidity, age, and uncertainty over returns to housing. This makes the model able to realistically capture *how much* households care about impacts on future non-housing consumption across the state distribution and as financial conditions shift.

5.2 Model parametrization and solution.

To solve for household policy functions, we parameterize preferences and expectations.

Housing consumption. Flow utility from housing consumption is given by:[40]

$$h(a, d, k, \boldsymbol{\varepsilon}, \chi) = h_{k(d)} + \mathbb{1}_{d\neq 0} \cdot \lambda_0 + \mathbb{1}_{d\neq 0,k} \cdot \lambda_s + \mathbb{1}_{\chi=1} \cdot \varepsilon_d \tag{26}$$

In this expression, $h_{k(d)} \in \{h_o, h_\ell\}$ is the mean value of owner-occupied or rental housing units, which depends on post-decision location $k(d)$; λ_0 is the net benefit of a move; and λ_s is the net benefit of switching housing types (from owning to renting or vice versa). We assume that $\varepsilon_d \overset{\text{iid}}{\sim}$ nested logit, with $d = 0$ in one nest and $d \neq 0$ in another and nesting parameter σ_k.[41] Finally, we assume that χ_{it} is distributed Bernoulli with success probability χ.

When $\chi_{it} = 0$, equation (26) equals $h_{k_{it}}$. Otherwise, the value of each housing alternative is state dependent, with λ_0 meant to capture any persistent value of a household's own unit relative to others of the same type k, and λ_s the persistent value of a household's current unit type k relative to the alternative. A positive value of λ indicates a persistent preference

[40]In future versions, this parametrization will depend on age a, although it does not here.

[41]For $\sigma_k = 1$, ε is perfectly correlated across alternatives within nests; for $\sigma_k = 0$, ε follows a type-I extreme value distribution.

for the current relative to alternative type, while a negative value indicates a household is "unmatched" from their current arrangement when the option to move arrives.

Non-housing consumption. We assume that flow utility over non-housing consumption and bequest motives take the following constant relative risk aversion forms:

$$u(c) = \alpha \frac{c^{1-\gamma}}{1-\gamma}, \quad v^b(W) = \varphi_1 \left(\frac{W + \varphi_2}{1-\gamma} \right)^{1-\gamma} \tag{27}$$

The functional form for v^b follows De Nardi (2004). The parameter α controls the relative value of housing relative to non-housing consumption. When α is high, households become more sensitive to the relative price of owner-occupied and rental units. It is therefore central in determining the impact of higher rates on housing demand.

Aggregate state variables and expectations. The nominal interest rate r_t follows an exogenous single-factor Cox, Ingersoll, and Ross (CIR) process. We assume that mortgage rates equal the implied 30-year fixed rate implied by CIR estimates, plus a fixed spread: $r_{mt} \equiv r_{t,30} + \Delta$. Rental cost growth follows an exogenous process that correlates with the level of and shocks to the interest rate. Households further expect that house price growth follows a first-order Markov process correlated with the level of the interest rate as well as shocks both interest rates and rental cost growth. Appendix Section E.1 describes expectations in more detail.

Our approach to modeling expectations for future house prices departs from full information rational expectations, because as shown in the market clearing equation (A.3), house prices depend on the full household state distribution. As in Landvoigt *et al.* (2015), our model does not focus on expectations, and so we simplify here to enable more detailed modeling of more central pieces of the household problem. Given the high computational burden that processing the full state distribution would place on households, we view this simplification as behaviorally realistic. As in other papers that simplify by modeling expectations as first-order Markov (e.g. Kaplan *et al.* 2020, Guren *et al.* 2021, Jeon 2022), we ensure that expectations are consistent with the correlations that households have previously observed.

Model solution. We solve the household problem via backwards iteration. For each age a, we first solve the household consumption problem, which gives $V^c(\cdot)$ from equation (24). Given optimal consumption, we form choice-specific value functions $v_{do}(s, \theta_t) \equiv h_{k(d)} + \mathbb{1}_{d \neq 0} \cdot \lambda_0 + \mathbb{1}_{d \neq 0,k} \cdot \lambda_s + E_\theta \left[V^c(s', \theta_{t+1}) | d, s, \theta_t \right]$. Given choice-specific value functions, we can solve for equilibrium choice probabilities by integrating over the joint distribution of $\chi_{it}, \boldsymbol{\varepsilon}_{it}$, with $\pi_{do}(\cdot) = E[\chi_i] \cdot \Pr\big(d = \max_{d'} v_{d',o}(\cdot) + \varepsilon_{i,d',o}\big)$.

We efficiently solve for optimal non-housing consumption using an Euler equation that accounts for future expected moves. Due to the embedded discrete choice over housing and liquidity constraints, the Euler equation is necessary, but not sufficient, for optimal policy. To overcome this, we use an extension of the endogenous grid point method described in Druedahl and Jørgensen (2017), which inverts the Euler equation at endogenous *segments* and takes the upper envelope to solve for optimal non-housing consumption at each state.

5.3 Estimation and validation.

We first calibrate and estimate a number of parameters offline. We then estimate the preference parameters that are central to the household response to rate lock in a rising-rate environment using full-solution minimum distance (Rust 1987, Gourinchas and Parker 2002, Laibson *et al.* 2024). To ensure our estimates are credible, we choose parameters so that the model replicates the substitution patterns observed in the November 2016 event study from Section 3.2. Finally, we verify that the model's predictions for household behavior and equilibrium prices align with untargeted moments.

Offline calibration and estimation. We set a time step t equal to two years, meaning that r_t is the nominal interest rate on a 2-year Treasury. Appendix Table E.16 summarizes parameters. We calibrate preference parameters $\gamma, \delta, \varphi_1, \varphi_2$, and σ_k, along with the boundaries of the household age profile (with $a \in [22, 82]$), transactions costs τ_s, per-period owner-occupied housing costs τ_{ft}, mortgage spreads Δ, and origination LTVs ϕ. We use the 2013-2019 Survey of Consumer Finances to estimate the age profile of real income $\{y_a\}$, the household state distribution G_{kt}, N_{kt}. Finally, we estimate state transition parameters using data on the nominal 2-year Treasury, rental cost growth, and house price growth. We deflate all nominal variables besides the interest rate to 2019 dollars using the BLS CPI-U.

Appendix equation (E.2) shows estimates for how interest rates, rental cost growth, and house price growth correlate. The nominal 2-year Treasury has a steady-state value of 3.9% and autocorrelation coefficient of 0.9. Rental cost and house price growth correlate negatively with the interest rate, with a greater effect on house prices. Positive shocks to rental cost growth and nominal interest rates also increase house price growth. This is consistent with the identification concern from Section 4 that high rates coincide with high housing demand.

Minimum distance estimation. We normalize $h_\ell = 0$ and estimate the remaining household preference parameters $\psi \equiv (h_o, \lambda_0, \alpha, \lambda, \chi)$ using just-identified minimum distance. We estimate ψ as the value that makes the model exactly match five empirical moments related to the November 2016 event study.

The first two moments target the probability that a household purchasing a home before

November 2016 moves to either (i) another owner-occupied home within 4 years; or (ii) a rental unit within 4 years. The probability of an own-to-own move is most informative about λ_0, as the net benefit of a move is the only average non-financial differentiator across properties. The probability of an own-to-rent move is most informative about h_o, the average value of an owner-occupied relative to rental unit.

The second two moments target the impact of higher mortgage rates on the probability of (i) all moves and (ii) own-to-rent moves within 4 years. The impact on all moves is most informative about α, which controls the relative importance of financial and non-financial factors in housing demand. The impact on own-to-rent moves has information about λ_s; conditional on the other parameters, a higher value of λ_s implies that substitution driven by changing costs of ownership is more likely to induce a change in tenure.

Finally, we target the variance of the total move rate from 2011-2019 to pin down χ. Conditional on other parameters, χ scales responsiveness of household behavior to changes in the economic environment. When χ is high, aggregate moves respond aggressively to changing house prices, interest rates, and rents, and vice versa when χ is low.

Formally, where Ω is the vector of empirical moments and $g(\psi, \Omega)$ a distance function where each element is the difference between the modeled and empirical moment given parameters ψ, the minimum-distance estimate is:[42]

$$\hat{\psi} \equiv \arg\min_{\psi} \; g(\psi, \Omega)' g(\psi, \Omega) \tag{28}$$

Appendix Table E.17 presents $\hat{\psi}$, along with the description and empirical value of the moment primarily used for identification, recognizing that all moments are relevant because parameters are estimated jointly. Estimates indicate that households prefer owner-occupied to rental housing on average, since $\hat{h}_o > 0$. Households also experience a utility cost when they change units, given that $\hat{\eta}_0 < 0$. Our estimate of $\hat{\chi} = 0.37$ indicates that on average, households consider a move once every 5.3 years.

To quantify the value households place on moving costs and owner-occupied housing, we consider the wealth transfer that would make the average 2019 owner indifferent between their current situation and various forced moves. Compensating an owner for a forced move to another owner-occupied unit would require a transfer equal to 2.1% of total non-housing wealth (the sum of financial wealth and the present-value of future income). Compensation for a forced move to a rental unit would require a 4.6% transfer.

[42] Since our system is just-identified and we match targeted moments exactly, a weighting matrix is redundant.

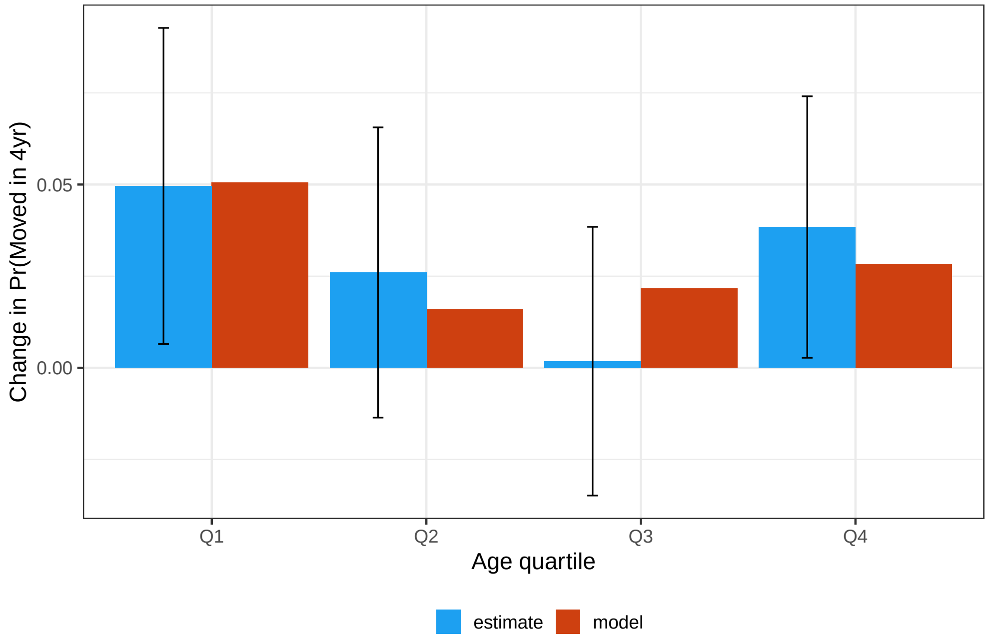
Validation with untargeted moments. To validate our estimates, we compare our model's predictions untargeted household behaviors and equilibrium house price effects. The predictions for untargeted price effects are especially important, both because our main outcome of interest is predicted price growth during 2021-23 tightening and because we did not directly target house prices or price growth anywhere in our estimation procedure. Price predictions depend on modeled impacts for how housing choice responds to economic conditions, which are not mechanically connected to the untargeted price growth moments we consider below.

First, Figure 5 shows that the model reproduces the heterogeneous impact of rate lock on existing-home sales by age. We simulate the November 2016 event study for households that originally purchase a home at different ages, and compare model predictions to estimates from Appendix Table C.5. Even though parameterized flow utility from housing consumption does not depend on age, the model endogenously replicates lifecycle patterns for the effects of rate lock, with greater impacts for younger and older households. This is both because of the lifecycle structure of the model – income depends on age, and households' decision horizon changes as they approach the end of their life – and because of the empirical correlation between age and financial wealth. As explained in Section 3.2, younger households, who are relatively more liquidity constrained, are sensitive to the liquidity effects of rate lock, while older households are more sensitive to its net worth effects. This builds confidence that the model captures the main economic forces determining the impact of rate lock on housing choice.

Second, Figure 6 shows the model predicts cross-sectional price effects of scaled mortgage value on equilibrium price growth from 2021-23 that align with IV estimates from Section 4. We simulate the model's predictions for equilibrium house price growth due to the 2021-23 increase in the 2-year Treasury rate, given the household state distribution in 2021. We then perturb the 2021 distribution of outstanding mortgages to increase or decrease rate lock, and plot equilibrium house price growth from 2021-23 as a function of scaled mortgage value growth. The blue dashed line plots fitted values from a regression of modeled house price growth on scaled mortgage value. For comparison, the red and green lines show the fixed effects and twin IV estimates, respectively, from Table 4. The model's predictions align almost exactly with IV estimates.

Finally, Figure 7 compares model-implied to actual real price growth during the last rate tightening cycle, from 2013-2019. We simulate the model-implied equilibrium price for 2013, and show how prices change due to a move to the 2019 rental cost, 2-year Treasury rate, and state distribution, changing one at a time. From 2013-2019, real rents increased, the 2-year Treasury rate rose by 1.6pp, and households grew older and wealthier on average. These changes accumulated to a predicted 9.4% increase in house prices. In reality, house prices increased by 7.2%. That our model slightly overshoots price growth is consistent with trends

Figure 5: Model predicts heterogeneous impact of rate lock by age.
(Source: Equifax Credit Risks Insight Servicing)



Source: Equifax Credit Risks Insight Servicing and author's calculations. The blue bars plot estimates and 95% confidence intervals of the impact of rate lock on subsequent exiting-home sales from the November 2016 event study presented in Appendix Table C.5. The red bars show predictions of the event study estimates based on simulations of estimated model.

in housing preferences, with more recent birth cohorts favoring urban areas (Frost 2023). The model would interpret this preference shift as a decrease in h_o from 2013-2019, a parameter our model holds fixed.

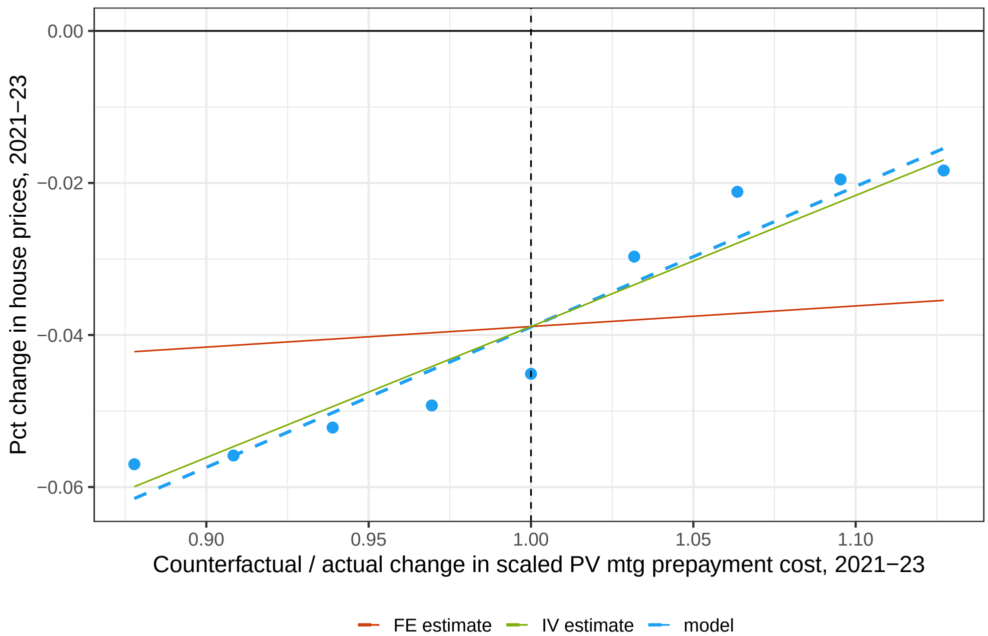
5.4 The net effect of 2021-23 tightening on equilibrium house prices.

This section uses the estimated model to simulate the effects of the 2021-23 increase in the 2-year Treasury on equilibrium house prices. We hold the household state distribution fixed at 2021 levels,[43] and calculate the change in equilibrium prices due to the 3.6pp increase in the 2-year Treasury rate combined with the observed change in real rents.

The model predicts a 4.0% decrease in real house prices, holding fixed preferences for owner-occupied housing consumption and the household state distribution. The negative impact on housing demand from higher rates therefore outweighs the attenuating effect of rate lock. This decrease contrasts with the 5.6% *increase* in prices actually observed. This indicates that other factors, such as an increase in the value of housing consumption or changes in household financial wealth, are responsible for price growth from 2021-23.

[43]We do not consider an impact of a change in the household state distribution because the 2024 Survey of Consumer Finances, which estimates liquid wealth positions for households in 2023, has not yet been released.

Figure 6: Model predicts price effects of mortgage value aligned with IV estimates. (Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)



Source: Equifax Credit Risks Insight Servicing, ICE, McDash®, and author's calculations. Blue dots plot model simulations for the impact of 2021-23 2-year Treasury increases on equilibrium house price growth as a function of the change in scaled mortgage value, where the change is plotted as a fraction of the model-implied 2021-23 change. The blue dashed line plots the line of best fit through the model predictions. The red and green lines plot, respectively, predictions based on fixed effects and IV estimates of the impact of scaled mortgage value on 2021-23 house price growth from Table 4.

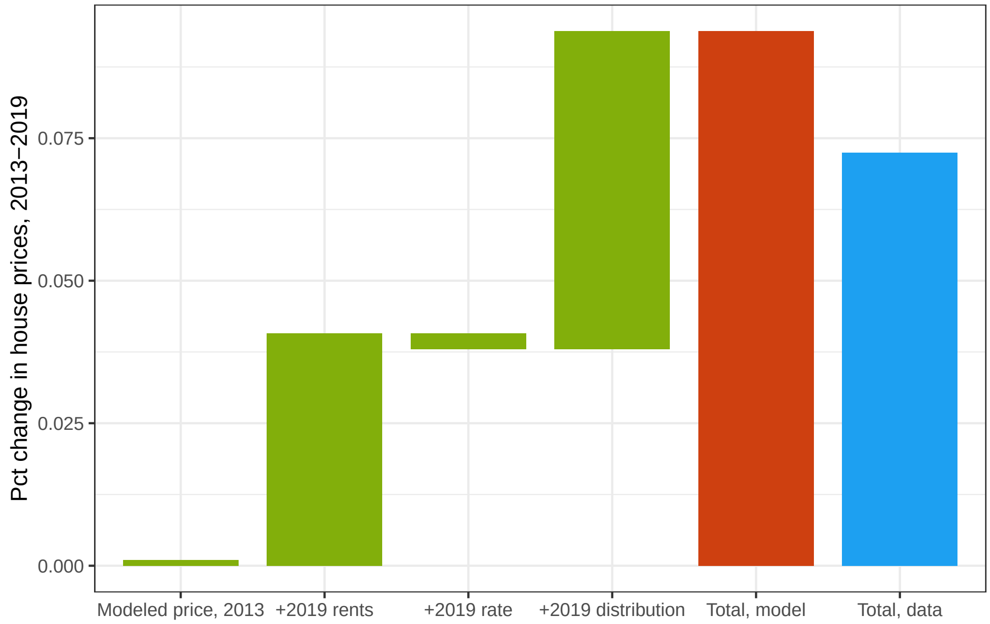
Although our model predicts that prices fall, it predicts a much smaller decrease than alternative approaches that consider tightening without rate lock. Figure 8 compares our estimates to two other approaches – one that uses the historical relationship between house prices and interest rates, and another that calibrates a lifecycle model of housing market equilibrium with variable-rate mortgages. The Federal Reserve (2024)'s April Financial Stability report used a statistical model of the historical relationship between house prices, the real 10-year Treasury yield, and market rents to predict equilibrium valuations. The estimates imply that prices should have fallen 20% during tightening.[44] Amromin and Eberly (2023) calibrate the impacts of tightening on perfect-foresight steady-state equilibrium house prices in an overlapping generations lifecycle model with variable rate mortgages, and predict that 2021-23 tightening should decrease house prices by 37%.[45] Therefore, accounting for rate lock is able to close 63-78% of the gap between actual and predicted price appreciation.[46]

[44]Figure 1.18 shows that market over-valuation relative to the model increased from $\sim 5\%$ in 2020-21 to $\sim 30\%$ by 2024, a $\sim 25\%$ increase. Since real house prices actually rose by $\sim 5\%$ over this period, the Fed's study implicitly predicts a $\sim 20\%$ drop in real house prices.

[45]See Table 6, row labeled "House price appreciation relative to peak."

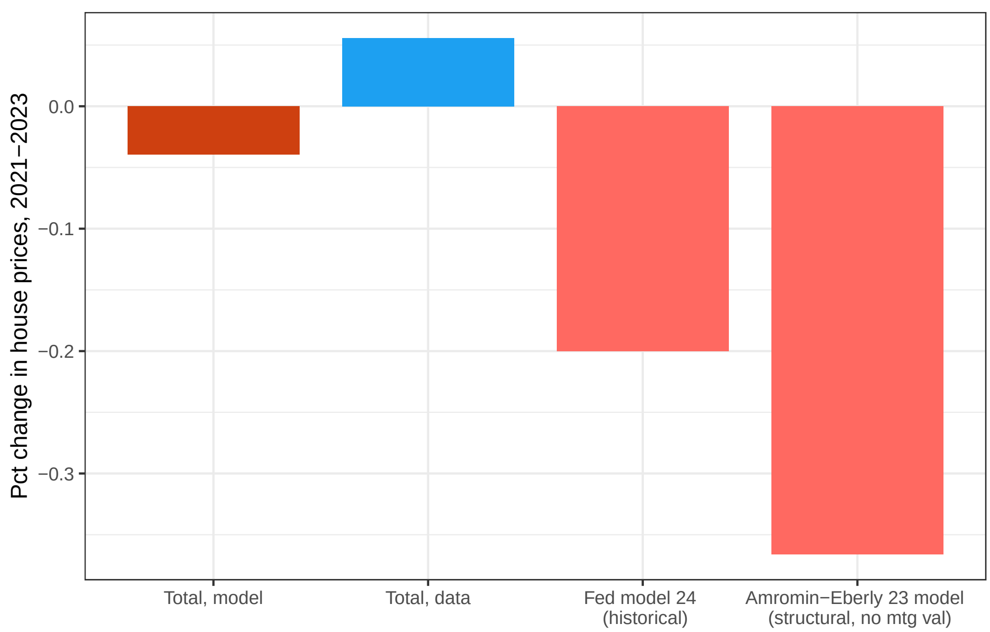
[46]Appendix Figure E.22 shows the differences across models are unlikely to be driven by different ways the models account for changes in rental costs, since accounting for rent growth does not impact our model's equilib-

Figure 7: Model-predicted vs. actual house price growth from 2013-2019



Source:Author's calculations. The green bars show how changing rents, 2-year Treasury rates, and the household state distribution one at a time from 2013 to 2019 levels impacts price growth. The red bar shows the model-implied total change in price growth. The blue bar plots the empirical change in real house prices, calculated using FHFA all-transactions repeat-sales price indices.

Figure 8: Model-predicted effects of 2021-23 tightening on equilibrium prices



Source: Author's calculations. The first bar shows model predictions of the impact of tightening on real equilibrium house prices. The second bar shows the actual change over the relevant model periods. The third and fourth bars present estimates from Federal Reserve (2024) and Amromin and Eberly (2023), respectively.

rium predictions much. Although there was substantial nominal rental cost growth from 2022-23, the cumulative change in real rents from 2021-23 was 2.7%.

6 Conclusion

This paper provides theory and evidence to study how the presence of fixed-rate, non-assumable mortgages affects the housing market response to financial tightening. At the household level, we provide causal evidence that rate lock discourages existing owners from exiting the market for owner-occupied homes. At the local housing market level, we develop new instruments for household mobility to show a causal effect of these household behaviors on equilibrium house price growth during tightening. We present an empirical model of dynamic housing demand to understand the impact of rate lock on aggregate market equilibrium. We estimate model parameters using household-level substitution patterns, and verify that the model generates an effect of rate lock in the cross section that aligns with our IV estimates.

We have two main findings. First, rate lock does not explain recent price increases, as the model predicts that 2021-23 tightening should have reduced real house prices by 4%, all else equal. Second, rate lock has a significant impact on equilibrium prices. Our reduced-form estimates imply that rate lock can explain almost 80% of the cross-sectional variation in market-level house price growth over the 2021-23 period. Furthermore, our model-based estimates indicate rate lock significantly attenuates the effect of financial tightening. Estimates that do not include rate lock predict a much larger 20-37% decrease in real house prices due to tightening. Our findings imply that fixed-rate mortgages make it more challenging for monetary policy to reduce house prices. This makes both monetary policy and housing affordability path dependent: a long era of low rates makes rapid tightening less effective in rapidly curbing house price growth.

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Online appendix to: "Rising Interest Rates, Mortgage Rate Lock, and House Price Fluctuations"

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Table of Contents

A Derivations for Section 1

A.1 Definitions and full expressions.

Define:

$$\Pr(Move)_{ot} = \Pr\big(v_{it,o\to\ell} \geq 0\big) + \Pr\big(v_{it,0\to o} \geq 0\big) \tag{A.1}$$

$$\Pr(Move, o \to \ell)_{ot} = \Pr\big(v_{it,o\to\ell} \geq 0\big) \tag{A.2}$$

where it is understood that $\Pr(Move)_{ot}$ and $\Pr(Move, o \to \ell)_{ot}$ are functions of equilibrium price P_t. In period t, equilibrium prices prices satisfy:

$$\underbrace{\gamma_o \mu H_o(\alpha[P_t - WTA_t])}_{\text{existing-home supply}} = \underbrace{\gamma_\ell(1-\mu)(1-H_\ell(\alpha(P_t - WTP_t)))}_{\text{existing-home demand}} \tag{A.3}$$

A.2 Verifying equilibrium.

Start with P_t that solves the general market-clearing equation (3) in period t. We will show that P_t solves equation (3) in period $t+1$.

First, consider the owners and renters who do not switch: $Stay_o \equiv \{i : v_{it,o\to\ell} < 0\}$ and $Stay_\ell \equiv \{i : v_{it,\ell\to o} < 0\}$. For $i \in Stay_o$, $WTP_{t+1} = WTP_t$, and $MV_{t+1} = MV_t$. Therefore, if $P_{t+1} = P_t$, then $v_{i,t+1,o\to\ell} < 0$. Similarly for $i \in Stay_\ell$, $v_{i,t+1,\ell\to o} < 0$. Therefore, at price P_t in period $t+1$, no current renters or owners who did not switch in period t want to switch in period $t+1$.

Next, consider the renters who switched to become owners: $Switch_\ell \equiv \{i : v_{it,\ell\to o} \geq 0\}$. These owners have no mortgage value in period $t+1$, and $WTP_{t+1} = WTP_t$. This means that if $P_{t+1} = P_t$, for $i \in Switch_\ell$, $v_{i,t+1,\ell\to o} = v_{it,\ell\to o} \geq 0$. Therefore, at price P_t in period $t+1$, no current owners who switched from renting to owning in period t strictly want to switch back from owning to renting in period $t+1$.

Finally, consider the owners who switched to become renters: $Switch_o \equiv \{i : v_{it,o\to\ell} \geq 0\}$. These renters *had* mortgage value in period t, but no longer do. Therefore, if $P_{t+1} = P_t$, it must be the case that for $i \in Switch_o$, $v_{i,t+1,o\to\ell} > v_{it,o\to\ell} \geq 0$. Therefore, at price P_t in period $t+1$, no current renters who switched from owning to renting in period t strictly want to switch back from renting to owning in period $t+1$.

The above results imply that in period $t+1$, after period t transitions, no existing owners strictly want to switch to become renters, and no renters strictly want to switch to become owners, if the market price is P_t. Some transactions may occur between households who switched in period t despite indifference. Iterating forwards, this implies that $P_{t+k} = P_t$ is an equilibrium price as well.

This result relies on shocks h_{io}, h_{il} being drawn once in period t and not switching once households transition between owning and renting. It can therefore be viewed as an approximation to an economy with more frequent shock realizations.

A.3 Derivation of equation (4).

From equation (A.1), the probability of a move for an existing owner is:

$$\Pr(Move)_{ot} = \gamma_o \cdot \Pr\left(h_{io} \leq \alpha\left[P_t - WTA_t\right]\right) + (1-\gamma_o) \cdot \Pr\left(h_{io} \leq \alpha MV_t\right) \tag{A.4}$$

$$= \gamma_o H_o\left(\alpha\left[P_t - WTA_t\right]\right) + (1-\gamma_o) H_o\left(-\alpha MV_t\right) \tag{A.5}$$

Therefore:

$$\frac{\partial \Pr(Move)_{ot}}{\partial r_F} = \underbrace{-\alpha\left(\gamma_o H_o'(\alpha\left[P_t - WTA_t\right]) + (1-\gamma_o) H_o'(-\alpha MV_t)\right)}_{\equiv \frac{\partial \Pr(Move)_{ot}}{\partial MV}} \frac{\partial MV_t}{\partial r_F} \tag{A.6}$$

The first term $\frac{\partial \Pr(Move)_{ot}}{\partial MV_t} < 0$, and $\frac{\partial MV_t}{\partial r_F} = -r_t^{-1} < 0$, so $\frac{\partial \Pr(Move_{ot})}{\partial r_F} > 0$, as in the text.

Furthermore:

$$\frac{\partial \Pr(Move, o \to \ell)_t}{\partial r_F} = -\alpha \gamma_o H_o'(\alpha[P_t - WTA_t]) \frac{\partial MV_t}{\partial r_F} \tag{A.7}$$

Therefore:

$$\frac{\partial^2 \Pr(Move, o \to \ell)_t}{\partial \gamma_o \partial r_F} = -\alpha \frac{\partial MV_t}{\partial r_F} H_o'(\cdot) > 0 \tag{A.8}$$

The sign follows because $\frac{\partial MV_t}{\partial r_F} < 0$. Note that there is no $\frac{\partial P_t}{\partial \gamma_o}$ term because the partial derivative $\frac{\partial \Pr(Moe, o \to \ell)_t}{\partial r_F}$ is implicitly evaluated at a fixed equilibrium price P_t.

If $H_o \sim$ uniform with density σ^{-1}, then:

$$\frac{\partial \Pr(Move)_{ot}}{\partial MV} = -\frac{\alpha}{\sigma} \tag{A.9}$$

$$\frac{\partial \Pr(Move, o \to \ell)_t}{\partial r_F} = \gamma_o \cdot \frac{\partial \Pr(Move)_{ot}}{\partial r_F} \implies \tag{A.10}$$

$$\gamma_o = \left(\frac{\partial \Pr(Move, o \to \ell)_t}{\partial r_F}\right)\left(\frac{\partial \Pr(Move)_{ot}}{\partial MV}\right)^{-1} \tag{A.11}$$

as in the text.

A.4 Derivation of equation (5).

Differentiate equation (A.3) with respect to MV_t:

$$\frac{\partial P_t}{\partial MV_t} = \underbrace{\frac{\gamma_o \mu H_o'(\cdot)}{\gamma_o \mu H_o'(\cdot) + \gamma_\ell (1-\mu) H_\ell'(\cdot)}}_{\equiv \omega \in [0,1]} \cdot \underbrace{\frac{\partial WTA_t}{\partial MV_t}}_{=1} \tag{A.12}$$

Expression (A.12) has two properties. First, it is clear from equation (A.7) that $\omega \propto \frac{\partial \Pr(Move, o \to \ell)_t}{\partial MV_t}$, which is increasing in γ_o. Therefore, $\frac{\partial^2 P_t}{\partial \gamma_o \partial MV_t} > 0$, as in the text. Second, if $\gamma_o = \gamma_\ell$, meaning owner-occupied and rental markets are symmetrically segmented, then the degree of market segmentation does not impact prices.

A.5 Derivation of equation (6).

Differentiating the market clearing expression (A.3) with respect to r_t:

$$\frac{\partial P_t}{\partial r_t} = (1-\omega)\frac{\partial WTP_t}{\partial r_t} + \omega \frac{\partial WTA_t}{\partial r_t} \tag{A.13}$$

$$= \underbrace{\frac{\partial WTP_t}{\partial r_t}}_{<0} + \omega \underbrace{\frac{\partial MV_t}{\partial r_t}}_{>0} \tag{A.14}$$

Substituting $\omega = \frac{\partial P_t}{\partial MV_t}$ as in equation (A.12) gives the expression in the text.

A.6 Extension: Two owner-occupied housing markets

This section extends the baseline own-vs-rent model to a setting where consumers choose across segmented owner-occupied housing markets. The setup is the same as in Section 1 unless otherwise noted.

Setup. A city has two owner-occupied housing markets with a fixed supply of units, a and b. For $j \in \{a, b\}$, fraction μ_j, with $\sum_j \mu_j = 1$, of consumers are initially endowed with an owner-occupied unit in market j financed with a fixed-rate perpetuity mortgage. Mortgages have balance M_j and r_{Fj}.

The equilibrium prices of owner-occupied housing, P_{at} and P_{bt}, clear spot markets each period by equating the mass of owners in a who buy in b with the mass of owners in b who buy in a.

Consumer problem. Markets are partially segmented: fraction γ_j of owners in j choose between market j and market j', with the remaining $1-\gamma_j$ choosing whether to remain in their current unit or occupy a different one in j.

For all consumers, the present-value cost of a new purchase in j is P_{jt}, and the present-value cost of repaying outstanding fixed-rate mortgage debt at face value is $MV_{jt} \equiv \frac{(r_t - r_{Fj})M_j}{r_t}$.

Let $h_{ij} \sim H_j$ be the present-value of the net housing consumption benefit from living in market j rather than an alternative unit. For consumers on the margin between market j and $j' \neq j$, the alternative unit is one in market j'. For consumers on the margin between an incumbent and alternative unit in market j, the alternative unit is a different unit in j. The indirect utility from a switch from j to j' is:

$$v_{it,j\to j'} = -h_{ij} + \alpha\left(P_{jt} - P_{j't} - MV_{jt}\right) \tag{A.15}$$

For consumers on the margin within market j, this collapses to $v_{it,0\to j} = -h_{ij} - \alpha MV_{jt}$ because the market price of the current and alternative units are the same.

Market equilibrium. The overall probability of moving is given by:

$$\Pr(Move)_{jt} = \underbrace{\gamma_j H_j\left(\alpha\left(P_{jt} - P_{j't} - MV_{jt}\right)\right)}_{\equiv \Pr(Move, j\to j')_t} + (1-\gamma_j)H_j(-\alpha MV_{jt}) \tag{A.16}$$

Relative prices $\Delta P_{jt} \equiv P_{jt} - P_{j't}$ equate the flow of moves from j to j' with the flow of moves from j' to j:

$$\mu_j \gamma_j H_j\left(\alpha\left[\Delta P_{jt} - MV_{jt}\right]\right) = \mu_{j'}\gamma_{j'}H_{j'}\left(\alpha\left[-\Delta P_{jt} - MV_{jt}\right]\right) \tag{A.17}$$

Equilibrium properties. First, lower fixed rates in market j imply less overall existing-home sales, with the magnitude of the effect on $j \to j'$ moves informative about the degree of market segmentation:

$$\frac{\partial \Pr(Move)_{jt}}{\partial r_{Fj}} = \underbrace{-\alpha\left(\gamma_j H_j'(\cdot) + (1-\gamma_j)H_j'(\cdot)\right)}_{\equiv \frac{\partial \Pr(Move)_{jt}}{\partial MV_{jt}}} \cdot \frac{\partial}{\partial MV_{jt} r_{Fj}} > 0 \tag{A.18}$$

where the sign follows because $\frac{\partial \Pr(Move)_{jt}}{\partial MV_{jt}} < 0$ and $\frac{\partial MV_{jt}}{\partial r_{Fj}} < 0$. Furthermore:

$$\frac{\partial \Pr(Move, j \to j')_t}{\partial r_{Fj}} = -\alpha\gamma_j H_j'(\cdot)\frac{\partial MV}{\partial r_{Fj}} \geq 0 \tag{A.19}$$

where $\frac{\partial^2 \Pr\left(Move, j \to j'\right)_t}{\partial \gamma_j \partial r_{Fj}} = -\alpha H_j'(\cdot)\frac{\partial MV_{jt}}{\partial r_{Fj}} > 0$ because $\frac{\partial MV_{jt}}{\partial r_{Fj}} < 0$.

Second, relative mortgage value affects relative house prices. Totally differentiating the market clearing expression (A.17):

$$d\Delta P_{jt} = \omega_j dMV_{jt} - (1-\omega_j)dMV_{j't}, \quad \omega_j \equiv \frac{\mu_j\gamma_j H_j'(\cdot)}{\mu_j\gamma_j H_j'(\cdot) + \mu_{j'}\gamma_{j'}H_{j'}'(\cdot)} \tag{A.20}$$

As in the main text, relative prices do not depend on the degree of segmentation if $\gamma_j = \gamma_j'$. Equation (A.20) implies that:

$$\frac{\partial \Delta P_{jt}}{\partial MV_{jt}} = \omega_j > 0 \tag{A.21}$$

where ω_j is increasing in γ_j, all else equal, and proportional to $\frac{\partial \Pr\left(Move, j \to j'\right)_t}{\partial r_{Fj}}$.

Third, an increase in market rates r_t will cause relative price growth depending on the relative change in mortgage value across markets. From equation (A.20):

$$\frac{\mathrm{d}\Delta P_{jt}}{\mathrm{d}r_t} = \omega_j \frac{\partial MV_{jt}}{\partial r_t} - (1-\omega_j)\frac{\partial MV_{j't}}{\partial r_t} \tag{A.22}$$

If j and j' are symmetric with respect to population ($\mu_j = \mu_j'$), market segmentation ($\gamma_j = \gamma_j'$), and relative preferences $H_j = H_j'$, then $\omega_j = \frac{1}{2}$, and $\frac{\mathrm{d}\Delta P_{jt}}{\mathrm{d}r_t} = \frac{1}{2}\left(\frac{\partial MV_{jt}}{\partial r_t} - \frac{\partial MV_{j't}}{\partial r_t}\right)$. This means relative prices will increase if the increase in mortgage value due to an increase in r_t is higher in j than j', and decrease otherwise.

A.7 Extension: Housing ladder.

This section combines the baseline own-vs-rent model with the model in Section A.6 to understand price implications in an environment with a housing ladder.

Setup. A city has three types of differentiated housing units in fixed supply: owner-occupied housing units of type a or b, and rental housing. Owner-occupied units of type a are "starter homes" with smaller square footage. They are the only type of unit available for purchase by current renters. Owner-occupied units of type b are "upgrade homes" with larger square footage. They are available for purchase for current occupants of starter homes.

For $j \in \{\ell, a, b\}$, a fraction $\mu_j, \sum_j \mu_j = 1$ of consumers is endowed with occupancy in units of type j. All owner-occupied units are financed via fixed-rate perpetuity mortgages, with the notation for mortgage value following Section A.6. Rental units require per-period lease costs, with assumptions and notation as in Section 1.

Consumer problem. Markets are partially segmented and arranged on a housing ladder, with some households on each rung considering a move up or down to an adjacent one.

Among current renters, a fraction γ_ℓ chooses between continued renting and ownership in a, with the remainder choosing between renting their current unit or an alternative rental unit.

Osccupants of type a units are either "downsizers" or "upgraders." A fraction μ_{aD} choose between staying in their starter home or "downsizing" to a rental unit. A fraction μ_{aU}, with $\mu_{aU}+\mu_{aD} \leq 1$, choose between staying in their starter home or "upgrading" to an upgrade home in market b. The remainder choose between their current home and an alternative home in a.

Among current occupants of units of type b, a fraction γ_{bD} are "downsizers" who choose between their current unit and a unit in market a. The remainder choose between their current home and an alternative home in b.

This setup links the problem in the baseline model with the problem in Section A.6 via the market for starter homes. Starter homes are priced relative to exogenous rents via households on the margin between owning in a and renting. The indirect utility from a switch from renting to owning in a is the same as the problem for renters in the baseline model. The indirect utility from a switch from owning in a to renting is the same as the problem for owner-occupants in the baseline model. Upgrade homes are priced relative to starter homes via households on the margin between owning in a and b. The indirect utility for each of these switches is the same as in the model in Section A.6.

Market equilibrium. Equilibrium prices P_{at}, P_{bt} satisfy the following two market-clearing equations:

$$\underbrace{\gamma_{aD}\mu_a H_a\left(\alpha\left[P_{at}-\frac{\ell}{r_t-g}-MV_{at}\right]\right)}_{a\to\ell \text{ flows}}=\underbrace{\gamma_\ell\mu_\ell\left(1-H_\ell\left(\alpha\left[P_{at}-\frac{\ell}{r_t-g}\right]\right)\right)}_{\ell\to a \text{ flows}} \tag{A.23}$$

$$\underbrace{\mu_b\gamma_{bD}H_b\left(\alpha\left[P_{bt}-P_{at}-MV_{bt}\right]\right)}_{b\to a \text{ flows}}=\underbrace{\mu_a\gamma_{aU}H_a\left(\left[P_{at}-P_{bt}-MV_{at}\right]\right)}_{a\to b \text{ flows}} \tag{A.24}$$

Equilibrium properties. The baseline model analyzes P_{at} relative to ℓ_t, and the model in Section A.6 analyzes $P_{at}-P_{bt}$. Therefore, we focus on implications of the new setup for price *levels* of P_{bt} relative to ℓ_t when r_t rises.

Since $P_{bt} = P_{at} + \Delta P_{bt}$:

$$\frac{\partial P_{bt}}{\partial r_t} = \frac{\partial P_{at}}{\partial r_t} + \frac{\partial \Delta P_{bt}}{\partial r_t} = \frac{\partial WTP_t}{\partial r_t} + \omega_{\ell,D}\frac{\partial MV_{at}}{\partial r_t} + \omega_b \frac{\partial MV_{bt}}{\partial r_t} - (1-\omega_b)\frac{\partial MV_{at}}{\partial r_t} \tag{A.25}$$

$$= \frac{\partial WTP_t}{\partial r_t} + \left(\omega_{\ell,D} + \omega_b - 1\right)\frac{\partial MV_{at}}{\partial r_t} + \omega_b \frac{\partial MV_{bt}}{\partial r_t} \tag{A.26}$$

$$\omega_{\ell,D} \equiv \frac{\mu_a \gamma_{aD} H_a'(\cdot)}{\mu_a \gamma_{aD} H_a'(\cdot) + \mu_\ell \gamma_\ell H_\ell'(\cdot)}, \quad \omega_b \equiv \frac{\mu_b \gamma_{bD} H_b'(\cdot)}{\mu_b \gamma_{bD} H_b'(\cdot) + \mu_a \gamma_{aU} H_a'(\cdot)}$$

The first term is negative, reflecting the negative pressure that higher interest rates put on the price of units in a, which reduces the price of units in b assuming relative prices do not change. The third term is positive, reflecting the offsetting increase in prices due to higher mortgage value in b.

The middle term may be positive or negative, depending on the degree of market integration. Since $\omega_b = 1 - \omega_a$, the coefficient on $\frac{\partial MV_{at}}{\partial r_t}$ equals $\omega_{\ell,D} - \omega_a$. This is positive when more occupants in a are on the margin between owning and renting relative to being on the margin between staying in a and upgrading to b. In this case, the presence of a housing ladder *magnifies* the upwards pressure that mortgage value puts on equilibrium prices during tightening. Intuitively, rate lock disrupts more own-to-rent moves than starter home-to-upgrade unit moves, which leads to positive net demand for owner-occupied units that filters to the top of the market. The coefficient on $\frac{\partial MV_{at}}{\partial r_t}$ is negative when more occupants in a are on the margin between starter and upgrade homes relative to being on the margin between owning and renting. In this case, the housing ladder *attenuates* upwards pressure on equilibrium prices from mortgage value. Intuitively, rate lock disrupts more starter-to-upgrade moves than own-to-rent moves, depressing the relative value of upgrade homes without putting sufficient offsetting upwards pressure on absolute owner-occupied prices relative to rental units.

A.8 Extension: Endogenous rents.

This section extends the model in Section 1 so that rents are set in equilibrium. The main findings are that (i) endogenous supply partly offsets house price effects of rate lock; and (ii) some of its equilibrium effects affect rents.

Setup. We modify the model in the main text in two ways.

First, we add a mass N_I of investors. Investors can purchase owner-occupied units from owners and sell them as rental units to absentee landlords, or alternatively purchase rental units and sell them to owner-occupants. This makes N_{ot}, $N_{\ell t}$ endogenous.

Second, we assume that a representative landlord decides how many units $N_{\ell t}$ to hold to maximize the present-value of their profits. Each unit delivers present-value revenue $k(r_t)\ell_t$, where $k(r)$ is a decreasing function that capitalizes future revenue, and costs $C(N)$, with $C', C'' > 0$.

Landlord problem. Landlords are price takers in ℓ_t and choose N to maximize present-value profits:

$$\max_N N \cdot k(r_t)\ell_t - C(N) \implies \ell_t = C'(N)/k(r_t) \tag{A.27}$$

An increase in ℓ_t leads to more rental unit supply N, and an increase in r_t, if $k' < 0$, leads to a decrease in N.

Investor problem. Investors can buy and sell owner-occupied units for price P_t, and buy and sell rental units for price $k(r_t)\ell_t$. Heterogeneous investors face transaction costs $c \sim F, c \geq 0$ applied to at most one unit. Investors buy an owner-occupied unit to sell to landlords if $k(r_t)\ell_t - P_t \geq c$, and buy a leased unit to sell to owner-occupants if $P_t - k(r_t)\ell_t \geq 0$. That $c \geq 0$ implies that in any period t, there are never simultaneous own-to-rent and rent-to-own conversions.

Define a function $g(k(r_t)\ell_t - P_t)$ that describes the net mass of own-to-rent conversions (where a negative value indicates the mass of rent-to-own conversions):

$$g(k(r_t)\ell_t - P_t) = N_I \cdot (\Pr(k(r_t)\ell_t - P_t \geq 0) - \Pr(P_t - k(r_t)\ell_t > 0)) \tag{A.28}$$

$$= N_I \cdot \begin{cases} 1 - F(k(r_t)\ell_t - P_t) & \text{if } k(r_t)\ell_t - P_t \geq 0 \\ F(P_t - k(r_t)\ell_t) - 1 & \text{if } k(r_t)\ell_t - P_t < 0 \end{cases} \tag{A.29}$$

Housing market equilibrium. Rental markets clear to pin down ℓ_t:

$$k(r_t)\ell_t = C'(N_{\ell t}) \tag{A.30}$$

Owner-occupied housing markets clear to pin down the ratio of P_t and ℓ_t. This is the same as equation (A.3), but includes net transactions from investors:

$$\underbrace{\gamma_o \mu H_o(\alpha[P_t - WTA_t])}_{\text{own-to-rent moves}} = \underbrace{\gamma_\ell (1-\mu)(1 - H_\ell(\alpha(P_t - WTP_t)))}_{\text{rent-to-own moves}} + \underbrace{g(k(r_t)\ell_t - P_t)}_{\text{own-to-rent conversions}} \tag{A.31}$$

Finally, the initial-period rental units plus own-to-rent conversions equal current-period rental units:

$$N_{\ell t} = N_{\ell,0} + g(k(r_t)\ell_t - P_t) \tag{A.32}$$

Equilibrium implications. How do these new features impact the cross-sectional relationship between mortgage value, the rent-price ratio, price levels, and rent levels? Differentiating equation (A.31) with respect to MV_t, holding fixed ℓ_t, we have:

$$\frac{\partial P_t}{\partial MV_t} = \underbrace{\frac{\alpha\mu\gamma_o H_o'(\cdot)}{\alpha\mu\gamma_o H_o'(\cdot) + \alpha(1-\mu)\gamma_\ell H_\ell'(\cdot) + g'(\cdot)}}_{\equiv \omega_o} \tag{A.33}$$

where $g'(\cdot) > 0$. Since $g'(\cdot) > 0$, this effect is lower than the one in the main text. Therefore the presence of investors reduces the effect of mortgage value on prices, holding rents fixed. This is intuitive – as owner-occupied prices rise relative to rents, investors find it profitable to buy rental units and sell them to renters, increasing the fraction of consumers living in owner-occupied units.

How does this impact equilibrium rents? Holding fixed rents, the transactions from investors will reduce the stock of rental housing, which by the landlord first order condition will reduce ℓ_t. This will tend to put additional downwards pressure on P_t, meaning the overall impact on price levels is also attenuated.

What about dynamic effects of tightening? Holding fixed ℓ_t, we have:

$$\frac{\partial P_t}{\partial r_t} = \omega_\ell \frac{\partial WTP}{\partial r_t} + \omega_o \frac{\partial WTA}{\partial r_t} + \omega_I k'(r_t)\ell_t \tag{A.34}$$

where $\omega_\ell \equiv \frac{\alpha(1-\mu)\gamma_\ell H_\ell'(\cdot)}{\alpha\mu\gamma_o H_o'(\cdot)+\alpha(1-\mu)\gamma_\ell H_\ell'(\cdot)+g'(\cdot)}$ and $\omega_I \equiv \frac{g'(\cdot)}{\alpha\mu\gamma_o H_o'(\cdot)+\alpha(1-\mu)\gamma_\ell H_\ell'(\cdot)+g'(\cdot)}$ with $\omega_o + \omega_\ell + \omega_I = 1$. The new third term reflects that when interest rates rise, the price of rental properties falls, which makes investors more willing to purchase rental units and sell them to prospective owner-occupants. This pushes down the rent-price ratio relative to the version of the model in the main text. Furthermore, the presence of $g' > 0$ in the denominator of ω_o implies that the presence of investors attenuates the impact of mortgage value on prices during tightening.

The effect on equilibrium rents is ambiguous. First, the effect of tightening on demand for rental units, holding prices fixed, is ambiguous given the presence of mortgage value. Unless the increase in mortgage value is very high, demand for renting is likely to increase, which puts upwards pressure on l_t. In general, higher mortgage value attenuates this. Second, higher r_t depresses the present-value of rental units $k(r_t)\ell_t$, which from the first order condition requires an increase in ℓ_t, holding rental demand fixed. In general, it is likely that higher rates will increase rents.

Endogenous rents attenuates the effect of rates on the price-rent ratio, but tends to put upwards pressure on rents, implying ambiguous effects for price levels. Therefore, the effect of endogenous rents on price predictions is ambiguous.

As a final comment, the above discussion focuses on comparative statics for small changes in variables. For larger changes, equilibrium forces might significantly change the local density of, for instance, H_ℓ and H_o, which potentially impacts how the comparative statics in this section relate to those in the main text.

B Data construction details

B.1 CRISM and ICE, McDash®details.

We use an individual-level panel for analysis in Section 3, and a local market-level dataset for analysis in Section 4. We also use data from ICE, McDash®on mortgage duration for ancillary analysis and instrument construction.

B.1.1 Individual-level panel.

We randomly draw a 2.5% sample of individuals from the CRISM monthly panel from January 2012 to December 2023. We restrict to households for whom we observe the purchase of an owner-occupied home with a fixed-rate, 30-year, first-lien mortgage during this period. To ensure an observed mortgage origination corresponds to a home purchase, we require that the mortgage origination date occurs within 180 days of a persistent Equifax address change, where a persistent change is one that does not revert within 6 months, and that the property's zip code of origination aligns with the destination zip code of the persistent address change.

We do not directly observe home sales in CRISM. We infer them based on a combination of mortgage prepayment and moving activity. Specifically, we infer an existing-home sale if three conditions are met: (i) a borrower terminates a purchase mortgage; (ii) a persistent address change occurs within 180 days of mortgage termination; and (iii) the zip code of the property that secured the terminated mortgage aligns with the origin zip code of the persistent address change. To screen out real estate investors and especially distressed sales, we restrict to households whom we infer sell their home within 1 year of purchase.

We classify an existing-home sale as an own-to-rent move if it meets two additional conditions: (i) households have no outstanding first-lien mortgage balances in the credit records six months after the move; and (ii) a borrower had at least 50% of the origination balance still outstanding on the original purchase loan when the move occurred. The first condition indicates that the borrower did not take out a new mortgage after a move, and the second condition screens out borrowers with enough home equity to purchase a new property in cash without massively downsizing.

The data contains complete Equifax credit bureau information for the full Jan 2012-Dec 2023 period. However, Equifax waits a year to match loans with ICE, McDash®, which means the most recent matches are from Dec 2022 originations. Since our algorithm for identifying existing-home sales and own-to-rent moves relies on observing activity potentially 6 months after a move, we track activity for up to four years after move-in for our main sample for the

2016 event study. This ensures that originations from December 2017 have a round number of years without truncation issues.

B.1.2 Market-level dataset.

We randomly draw a 10% sample of December observations for individuals in the CRISM monthly panel from 2005-2023. We use this sample to calculate, at the zip code level: (i) churn; (ii) the age distribution; (iii) credit scores; (iv) average appraisal amount, average LTV, average DTI, and ARM share; and (v) scaled mortgage value.

We calculate (i)-(iii) using averages for local residents, regardless of mortgage characteristics. we restrict the sample to borrowers living in residential addresses to screen out borrowers with zips linked to non-residential business mailing addresses. We estimate churn based on the fraction with a year-over-year change in zip code. We estimate the age distribution by calculating borrower-level age as the difference between year and borrower year of birth, and taking the fraction within each single-year age bucket. Finally, we estimate credit scores by taking the average of borrower Risk Score 3.0.

We calculate (iv) and (v) using averages for local mortgages. We restrict to first-lien mortgages to owner-occupants, excluding mortgages with exotic repayment terms (e.g. weekly or balloon payments) and graduated payment schedules. We assume that the scaled mortgage value of an adjustable-rate mortgage is zero, a simplification both because adjustable-rate mortgages have fixation periods which confer present-value prepayment costs, and because our current mortgage value calculations omit a credit spread relative to the 30-year rate.

The above steps deliver two zip code-by-year panels, which we merge together for our cross-sectional regressions. We then make two sample restrictions. First, we require that a zip code has at least 100 sampled borrowers, both to reduce measurement error in zip-level averages and for data privacy reasons. Second, we drop zip codes with average appraised property values below the 5th or above the 95th percentile. This is because we use FHFA repeat sales indices to estimate price growth. These indices are based on data from mortgages secured by Fannie Mae and Freddie Mac. The indices do not include properties purchased with non-agency jumbo loans or loans backed by the government directly (e.g. FHA, VA loans). Restricting to zip codes with average property values in the middle 90% ensures that price indices are broadly representative of typical transactions in the market.

B.1.3 ICE, McDash®details.

For some calculations, we also use information from the non-matched ICE, McDash®sample, which has a longer history than CRISM. Specifically, we take a 0.5% sample of loans from

1992Q1-2024Q1. The data has information on loan origination date, loan termination date, contract characteristics (mortgage balance, LTV ratios, DTI ratios, appraised value, interest rates, fixed-vs-variable rate, monthly payments). We use this information to calculate mortgage duration. Our calculations restrict to first-lien, fixed-rate purchase mortgages originated to owner-occupants.

B.2 CoreLogic details.

We pull transaction, mortgage, and property characteristic records for all residential properties in CoreLogic. We then identify purchases and sales by owner-occupants with a mortgage where an initial purchase occurred between Jan 2012 and Jan 2018.

We follow several sample selection criteria to focus on purchases by owner-occupants with a mortgage. First, we merge mortgage origination records to property transactions. To match a mortgage record with a property transaction, we require that (i) the transaction record has a flag indicating it was recorded with a mortgage; (ii) the buyer name in the transaction file matches the borrower name in the mortgage file; and (iii) the deed is recorded no more than 10 days before and no more than 21 days after the mortgage is recorded. The fuzzy match in (ii) requires that the Jaro-Winkler edit distance between the two strings is less than 0.2. Second, we drop transactions where the buyer is a corporation or trust, the buyer has a sufficiently different name (to eliminate non-arms length transfers to spouses, children, and other family members), or the buyer mailing address has a different zip code than the property, indicating an investment property or non-primary residence. Third, we restrict to transactions where the matched mortgage is a fixed-rate purchase loan.

Once we have identified valid purchases, we search for the next valid sale. Our main criteria for a valid sale is that it is arms-length, not an incorporation as a trust or a transfer to a spouse or family member. To screen out the former, we screen out sales to trusts. To screen out the latter, we require that (i) all sellers and buyers associated with each transaction have sufficiently different names; and (ii) the name of the current seller is different than the name of the next seller.

As mentioned in the main text, the CoreLogic mortgage data mostly lack information on interest rates. We impute mortgage rates using the average rate on Freddie Mac purchase mortgages with first payment dates within the same month as the month of first payment for CoreLogic mortgages. We calculate monthly mortgage rates using the Freddie Mac Single Family Loan-Level dataset. Note this is different than the rates from the Primary Market Mortgage Survey (PMMS), because the PMMS gives posted rates, which may differ from origination rates due to borrowers locking in different rates, and because the PMMS holds fixed borrower characteristics (credit score, LTV) over time.

B.3 Supplemental data sources.

We draw on a variety of publicly-available sources.

American Community Survey. We use annual estimates from the 5-year American Community Survey for zip-code level demographics. Estimates are available annually from 2011-2022, with the exception of 2020, when ACS did not publish due to the impacts of the Covid-19 pandemic (Census 2021). Where relevant, we impute 2020 values using 2021 estimates. We use the following tables to construct control variables:

- total population (`B01001_001E`);
- total number of households (`B25003_001E`);
- total number of owner-occupied households (`B25003_002E`);
- median family income (`B19113_001E`);
- median owner-assessed housing unit value (`B25077_001E`);
- median gross rent (`B25111_001E`);
- total white population (`B02009_001E`).

Population density. We construct population density as the ratio of ACS population and zip-code land area in meters. Land area in meters comes from ESRI and are sourced from ArcGIS Data and Maps (ESRI 2023).

House price indices. We use FHFA zip-level all-transactions annual house price indices to estimate average market-wide house price growth.

Twin and first births. For data on twin and first births, we use natality information published by the Centers for Disease Control. Our main dataset is drawn from the CDC's WONDER portal, which covers 1995-2023. For our analysis of first births, we supplement the main dataset with information from 1985 and 1987-1994 provided by the NBER.

The CDC's natality data is itself derived from the National Center for Health Statistics' (NCHS) Vital Statistics Cooperative Program. Through this program, all 50 states and DC send 100% of birth records to the NCHS for processing. The CDC publishes aggregated tables based on this information for each year. The CDC only publishes public-use natality data at the county level for counties with a population of at least 100,000 as of the most recent decennial census. When forming instruments that use natality data, we restrict to counties with continuous natality coverage from 1995-present. This reduces the number of zip codes in our sample by 21%.

We use four fields from the natality dataset. First, we use the annual number of total live births. Second, we use the total number of multiple births, which include twins, triplets, and higher (higher-order births account for only 0.1-0.2% of total births). Third, we use the number of births that are first in maternal birth order (i.e. are “first births.”). Finally, we use total population, which is drawn from intercensal population estimates from the Census Bureau. We calculate each of these fields by mother’s county of residence, which we map to zip codes using the methodology described below.

Local housing supply elasticity. We seek a zip code-level estimate of long-term housing supply elasticities. To measure this, we aggregate census tract level estimates from Baum-Snow and Han (2024) as of September 2023.

Baum-Snow and Han (2024) estimates heterogeneous long-run new unit supply elasticities, where heterogeneity is parameterized as a function of distance to the MSA central business district, fraction of the tract developed, and fraction of tract land area surrounded by flat topography. To address endogeneity, the paper instruments for labor demand using a Bartik instrument based on the local industry composition and shocks to demand in that industry.

We use estimates from the paper’s linear IV-finite mixture model that allows for heterogeneous coefficients for two different latent classes in the model describing drivers of supply elasticity heterogeneity. In the replication packet, these estimates are called `gamma01a_newunits_FMM`. To aggregate these estimates, we use Housing and Urban Development crosswalk files mapping from 2000 census tract to zip code. We assign each tract to a unique zip code based on the zip that contains the majority of tract housing units.

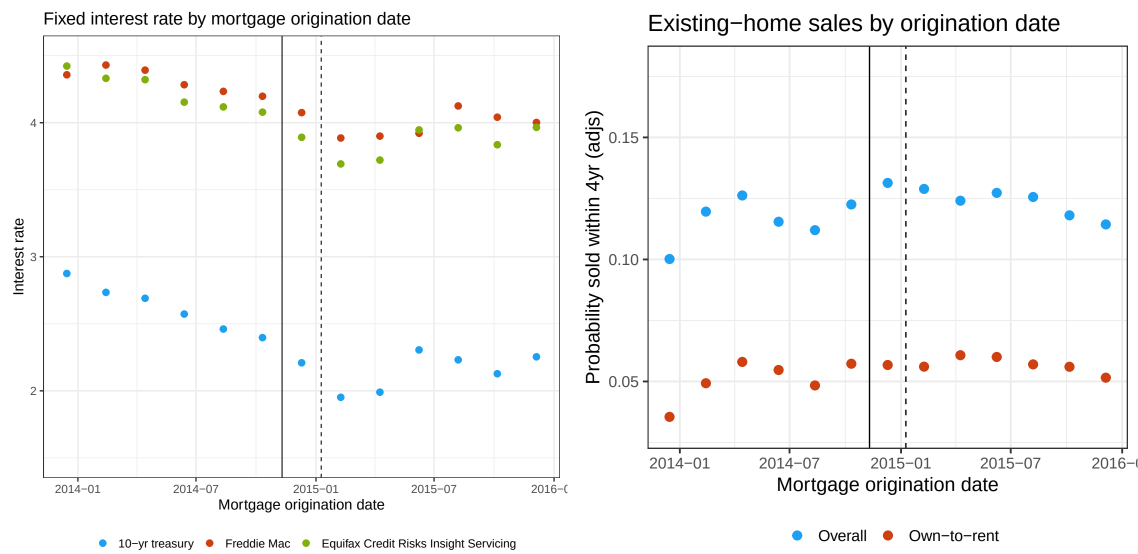
Mapping counties to zipcodes. We use 2010 county definitions. We map 2010 counties to zip codes using Housing and Urban Development crosswalk files. For years between 2010-2023, we load Q4 county-to-zip crosswalks. Each zip code is assigned to the unique county where the highest fraction of housing units are located. HUD does not provide data for years before 2010. For those years, we use the 2010 crosswalk files.

C Section 3 details.

C.1 November 2016 event study: Robustness checks

Placebo test: Patterns around November 2014. To verify our results are not driven by seasonal trends, we replicate in Figure C.1 panels (a) and (c) of Figure 1 two years prior to the November 2016 event. The left panel shows no sharp increase in mortgage rates for originations just before vs. just after November 2014 (although rates do fall slightly). The right panel shows that with no big decrease in household willingness-to-accept, the probability of an existing-home sale overall, or a sale resulting in an own-to-rent move, does not change for originations around November 2014. This placebo test builds confidence that our results are not driven by differential moving patterns for households who move in at different points in the year.

Figure C.1: Placebo: November 2014 change in rates and existing-home sales.
(Source: Equifax Credit Risks Insight Servicing)



Source: Equifax Credit Risks Insight Servicing and FRED. The figure replicates panels (a) and (c) of Figure 1 for origination dates between Jan 1, 2014 and Dec 31, 2015. Observations are grouped into 60-day bins relative to Nov 10, 2014. See notes to Figure 1 for details.

Difference-in-difference design. We can use observed seasonal patterns over 2014-2015 to control for any origination-date-of-year effects in the interest rate or four-year existing-home sales rate using an approach similar to difference-in-differences designs. Define $\tau_i \in \{0, 1\}$ as an indicator for whether i is in the "treatment group," with origination date between Jan 1, 2016 and Dec 31, 2017, rather than the "control group," with origination date between Jan 1, 2014 and Dec 31, 2015. Define $Post_i \in \{0, 1\}$ as an indicator for whether i's origination date is after Nov 10, 2014 (for $\tau_i = 0$) and after Nov 10, 2016 (for $\tau_i = 1$). We estimate the equation:

$$y_i = \beta_0 + \beta_1 \tau_i \times Post_i + \beta_2 \tau_i + \beta_3 Post_i + \eta' X_i + \varepsilon_i \tag{C.1}$$

where the coefficient of interest, β_1, gives the difference in outcome y_i before and after the treatment date for the treatment relative to the control group. As in the main text, we exclude originations in the 60 days after November 10 of the reference year for both the treatment and control groups.

Table C.1 shows estimates of β_1 where the outcome is mortgage rate at origination in Column (1), the unconditional probability of an existing-home sale in Column (2), and the probability of an existing-home sale resulting in an own-to-rent move in Column (3). Estimates are similar to those in the main text.

Table C.1: November 2016 event study: Difference-in-differences estimates (Source: Equifax Credit Risks Insight Servicing)

Dependent Variables:	Rate	Pr(Moved in 4yr)	Pr(Moved to rent in 4yr)
Model:	(1)	(2)	(3)
Variables			
Treated x post	0.6425***	0.0380***	0.0067**
	(0.0178)	(0.0045)	(0.0030)
Treated	-0.4674***	0.0087***	0.0003
	(0.0152)	(0.0031)	(0.0021)
Post	-0.3108***	0.0061**	0.0042**
	(0.0100)	(0.0029)	(0.0021)
Fixed-effects			
Age quintile-Inc quintile	Yes	Yes	Yes
Fit statistics			
Observations	114,391	114,391	114,391
R^2	0.24281	0.04934	0.03257
Within R^2	0.18089	0.01706	0.00561

Clustered (Borrower & Move in date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing. The table presents coefficient estimates for equation (C.1). Outcomes are fixed mortgage rate in Column (1), the probability of a move resulting in an existing-home sale within 4 years in Column (2), and the probability of an own-to-rent move resulting in an existing-home sale within 4 years in Column (3).

Excluding DC. It is possible that some post-November 2016 reflect churn from political appointees moving into or out of the Washington, DC area due to Trump's election. These households might have different mobility patterns than the general population, accounting for some of the increase in existing-home sales rates for households who move in after November 2016.

To investigate, I re-estimate the specification from the main text excluding all moves to and from counties within the Washington-Arlington-Alexandria, DC-VA-MD-WV CBSA. Results are in Table C.2, and are essentially identical to those in the main text.

Table C.2: November 2016 event study: Main estimates, excluding DC
(Source: Equifax Credit Risks Insight Servicing)

Dependent Variables:	Rate	Pr(Moved in 4 yr)	Pr(Moved to rent in 4yr)
Model:	(1)	(2)	(3)
Variables			
Post	0.7199***	0.0300***	0.0149**
	(0.0182)	(0.0104)	(0.0069)
Trend width	6mo	6mo	6mo
DC?	No	No	No
Fixed-effects			
Age quintile-Inc quintile	Yes	Yes	Yes
Fit statistics			
Observations	30,771	30,776	30,774
R^2	0.41859	0.07562	0.05867
Within R^2	0.36029	0.01905	0.00631

Clustered (Person & Move in date standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing. The table replicates Table 1, but excludes all moves to or from the Washington-Arlington-Alexandria, DC-VA-MD-WV CBSA. See notes in Table 1 for details.

C.2 Implicit borrower discount rate

We calculate the implicit risk-neutral constant discount rate that a representative borrower uses to discount the future using the following formula:

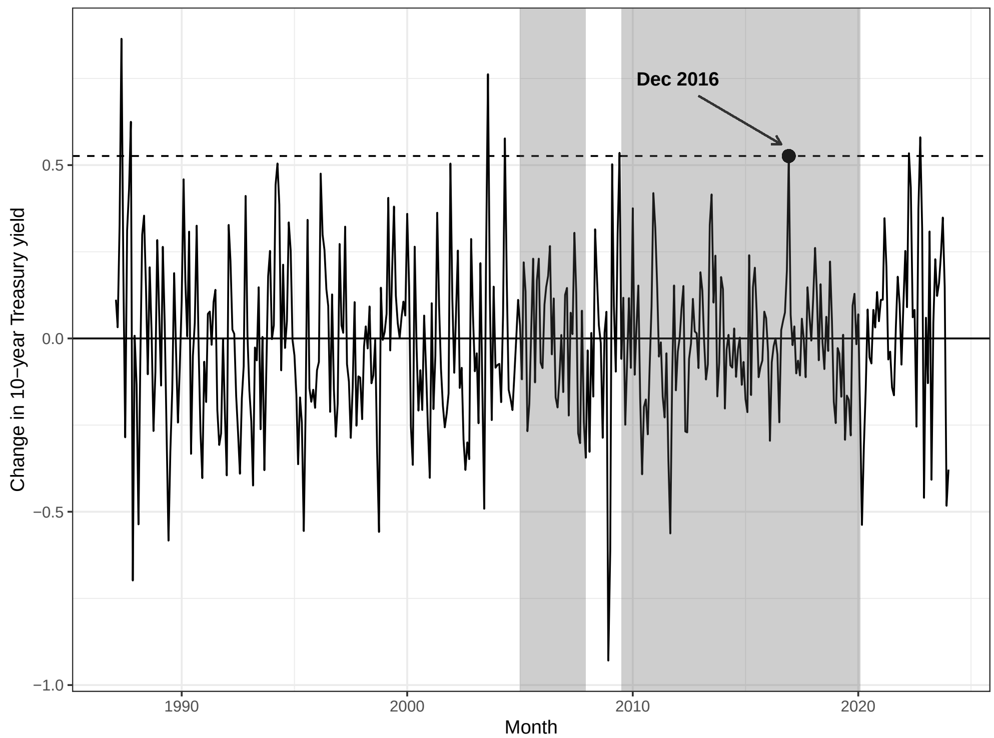
$$\Delta P = \sum_{a=1}^{\tau} \text{Pr(active in } a) \cdot \frac{\Delta m}{(1+r)^a} \tag{C.2}$$

where ΔP is the price premium, Δm is the difference in annual costs, r is the risk neutral rate the representative borrower uses to discount future costs, τ is the number of years remaining until repayment, Pr(active in a) is the probability that a loan outstanding will remain active in a years.

We consider Δm implied on the average loan for a 1pp difference in mortgage origination rate, and use our estimates to predict the implied ΔP. We use estimates of $K(a)$ from Appendix Section D.5.3.

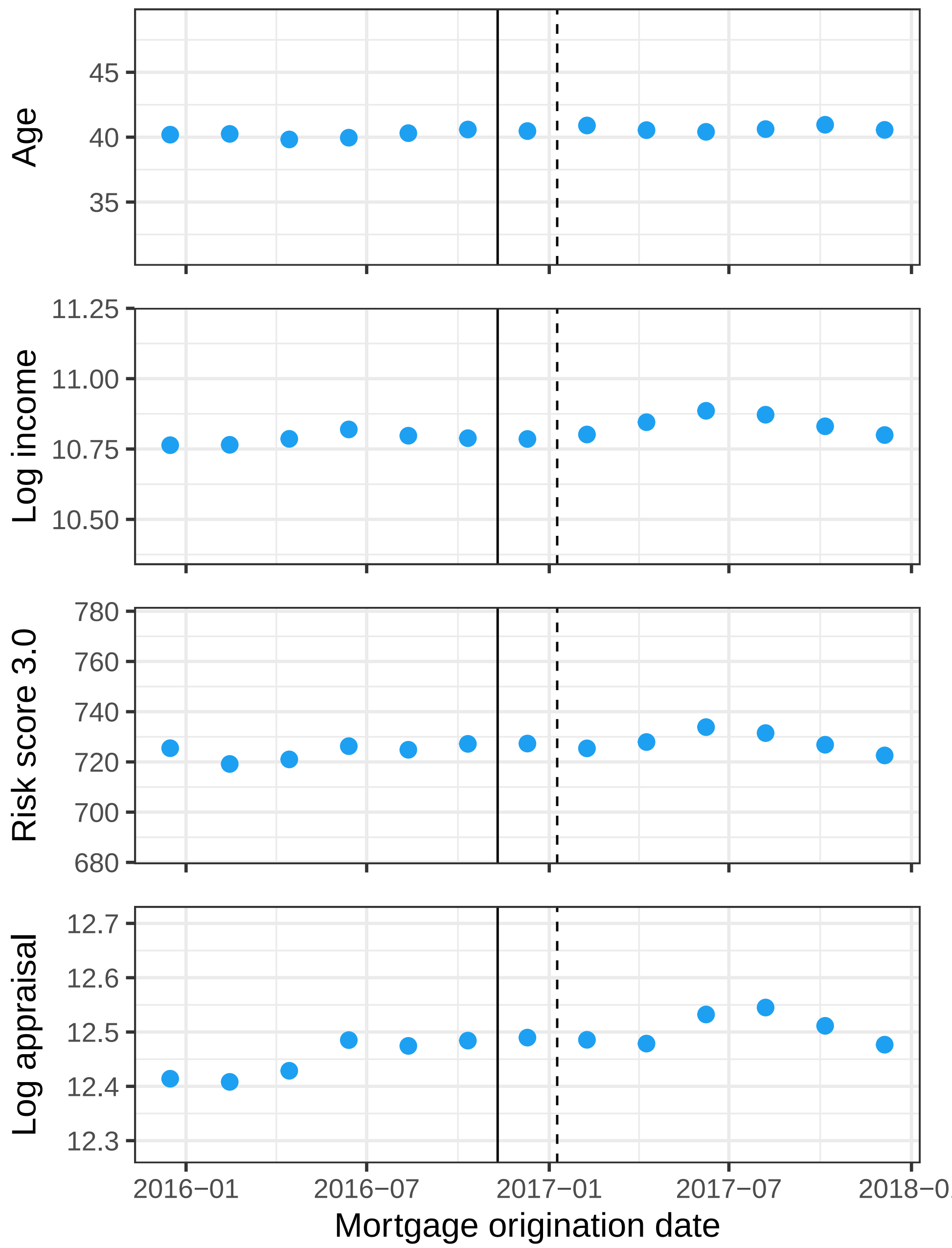
C.3 Additional figures

Figure C.2: The largest in-sample rate increase was around Nov 2016.



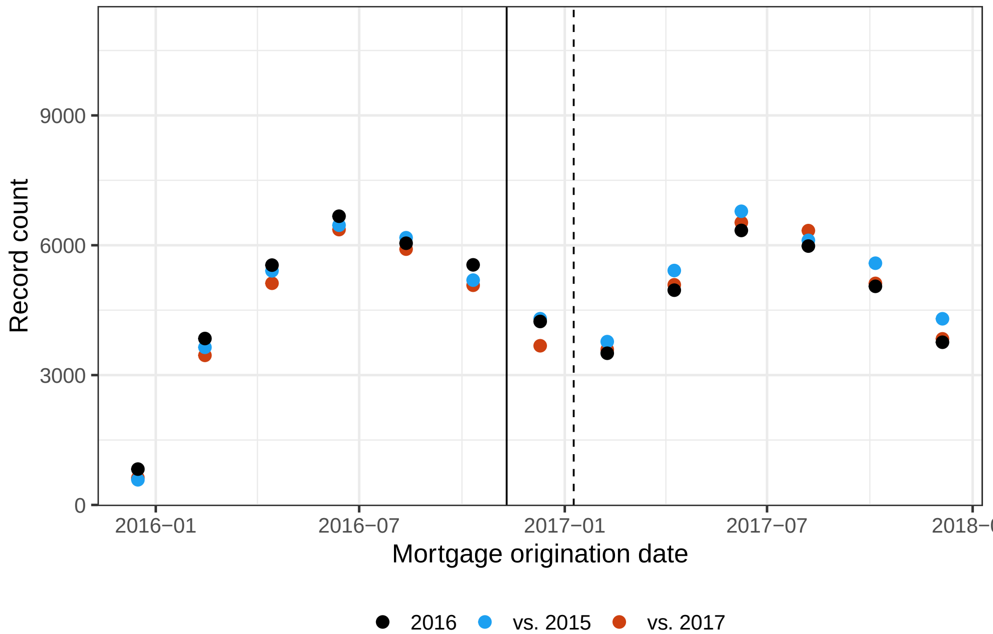
Source: FRED 10-year Treasury yield (series `DGS10`. The figure calculates the average Treasury yield in each month and plots the month-over-month change. Shaded areas reflect sample period, excluding the 2008 recession and periods after the start of the Covid recession using NBER business cycle dates.

Figure C.3: Covariate balance: 2016 event study.



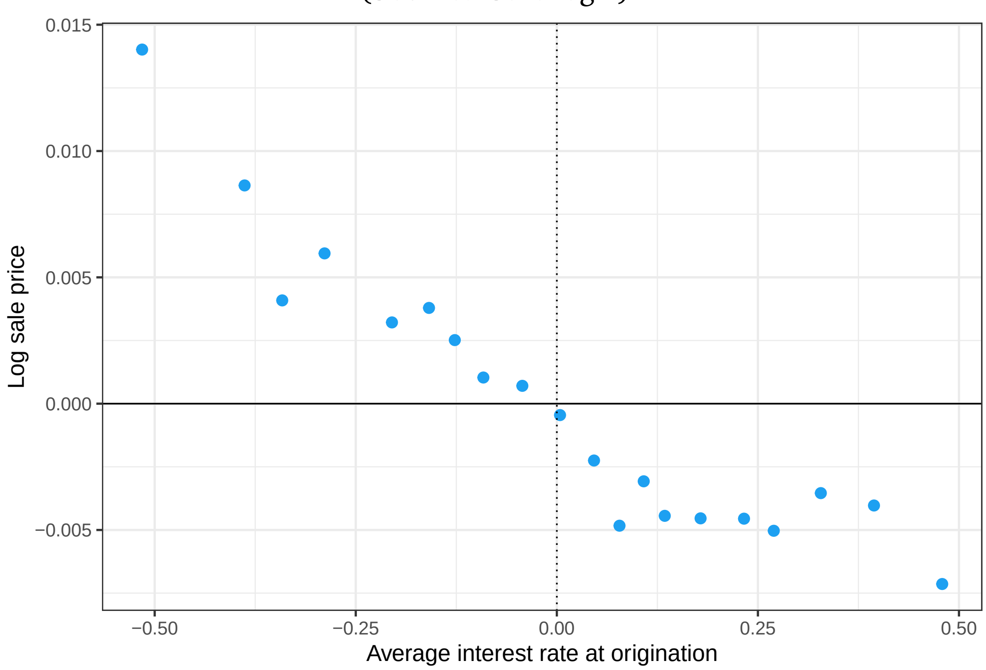
Source: Equifax Credit Risks Insight Servicing. The figure plots average age, log income, Vantage Risk Score 3.0, and log appraised value of the property securing the mortgage within 60-day buckets around Nov 10, 2016.

Figure C.4: Origination density, November 2016 event study.



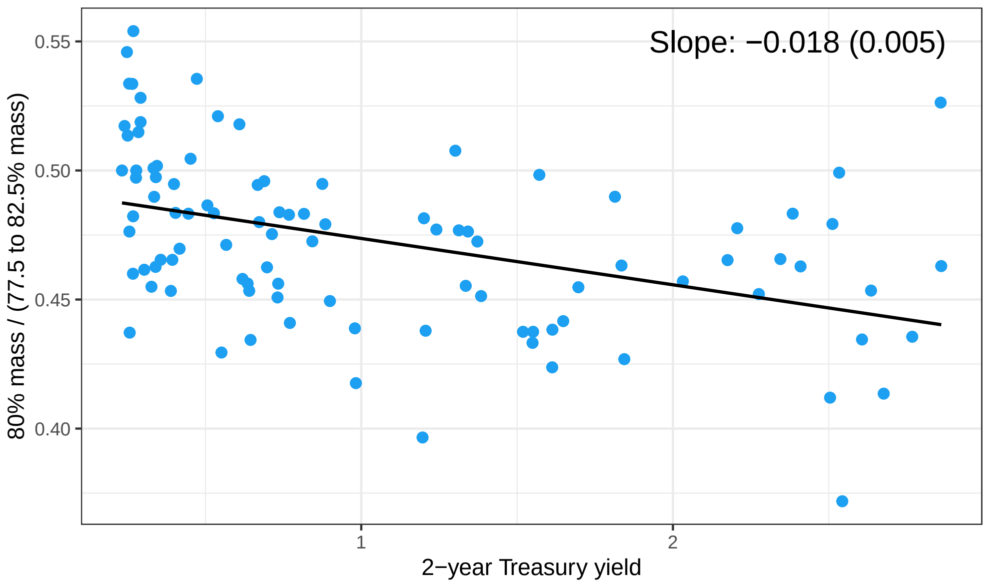
Source: Equifax Credit Risks Insight Servicing. The figure plots raw record counts in the 2.5% CRISM sample within 60-day buckets. The black dots plot origination counts for 2016-17; the blue dots plot origination counts for 2015-16 at equivalent day-of-year times; and the red dots plot origination counts for 2017-2018 at equivalent day-of-year times.

Figure C.5: Higher origination interest rate predicts higher resale price, within zip code-by-resale month.
(Source: CoreLogic)



Source: CoreLogic and Freddie Mac. The figure plots the log of resale price against the average mortgage interest rate as of the date of a property's purchase mortgage, where both resale price and origination mortgage rate are residualized on zip code-by-resale month fixed effects. Each dot represents a data ventile.

Figure C.6: Bunching at 80% LTV correlates with the risk-free rate.
(Source: Equifax Credit Risks Insight Servicing)



Source: Equifax Credit Risks Insight Servicing and FRED. The figure restricts to conventional 30-year fixed rate purchase mortgages originated from 2012-2019. Each dot represents a month. The vertical axis gives the fraction of loans with a purchase LTV of exactly 80% relative to the total mass of loans with purchase LTV of between 77.5% and 82.5%. The horizontal axis gives the average monthly 2-year Treasury yield.

C.4 Additional tables

Table C.3: CRISM summary statistics.
(Source: Equifax Credit Risks Insight Servicing)
(a) Full sample.

Statistic	N	Mean	St. Dev.
Age (yrs)	280,785	40.301	12.769
Income (USD)	163,977	60,979.310	72,231.930
Appraised purchase price (USD)	281,214	314,810.500	339,263.100
Equifax Risk Score 3.0	209,431	724.910	76.194
Origination interest rate (%)	281,561	4.044	0.615
Origination mortgage balance	281,561	264,519.100	188,321.500
Monthly payment (USD)	281,045	1,596.363	1,293.144
LTV (%)	280,725	88.717	23.701
DTI (%)	164,007	35.372	9.807
Pr(Moved in 4 yrs)	281,561	0.113	0.316
Pr(Moved to rent in 4 yrs)	281,561	0.048	0.213
Pr(Moved outside county in 4 yrs)	281,561	0.039	0.194
Pr(Moved outside state in 4 yrs)	281,561	0.021	0.142

(b) Nov 2016 event study sample.

Statistic	N	Mean	St. Dev.
Age (yrs)	32,285	40.501	12.599
Income (USD)	19,923	63,952.880	64,408.330
Appraised purchase price (USD)	32,335	331,031.300	267,909.400
Equifax Risk Score 3.0	25,343	727.500	72.298
Origination interest rate (%)	32,350	3.936	0.452
Origination mortgage balance	32,350	277,269.900	193,018.700
Monthly payment (USD)	32,299	1,637.453	1,244.295
LTV (%)	32,261	88.412	53.951
DTI (%)	19,923	35.338	9.599
Pr(Moved in 4 yrs)	32,350	0.129	0.336
Pr(Moved to rent in 4 yrs)	32,350	0.051	0.221
Pr(Moved outside county in 4 yrs)	32,350	0.046	0.209
Pr(Moved outside state in 4 yrs)	32,350	0.025	0.155

Table C.4: CoreLogic summary statistics.
(Source: CoreLogic)

Statistic	N	Mean	St. Dev.
Years since purchase | sale	3,029,199	4.886	1.974
Resale price | sale	3,029,199	331,009.500	1,191,519.000
Purchase price | sale	3,029,199	241,131.800	2,474,139.000
Purchase interest rate | sale	3,029,199	4.036	0.284
Initial mortgage amount | sale	3,029,167	207,004.900	144,191.200
Origination LTV | sale	3,029,167	0.906	0.641

Table C.5: November 2016 event study: Heterogeneity by age and geography, overall. (Source: Equifax Credit Risks Insight Servicing)

Dependent Variables:	Pr(Moved in 4yr)	Pr(Moved in 4yr), in cnty	Pr(Moved in 4yr)
Model:	(1)	(2)	(3)
Variables			
Post	0.0305***	0.0265***	
	(0.0099)	(0.0084)	
Post × Age quartile 1			0.0496**
			(0.0220)
Post × Age quartile 2			0.0260
			(0.0202)
Post × Age quartile 3			0.0018
			(0.0187)
Post × Age quartile 4			0.0384**
			(0.0182)
Trend width	6mo	6mo	6mo
Pre avg	0.118	0.075	
Fixed-effects			
Age quintile-Inc quintile	Yes	Yes	Yes
County-ever refi-Age quartile			Yes
Fit statistics			
Observations	32,357	32,357	31,050
R^2	0.07673	0.06370	0.13869
Within R^2	0.01886	0.01269	0.02053

Clustered (Person & Move in date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing. The table presents estimates of β_1 in equation (7). Column (1) reproduces Column (2) of Table 1. Column (2) replaces the outcome variable with the probability of a within-county move resulting in an existing-home sale within 4 years. Column (3) interacts coefficients in equation (7) with age quartile indicators and shows the interactions with β_1, where the outcome is the same as in Column (1).

Table C.6: November 2016 event study: Heterogeneity by age and geography, own-to-rent. (Source: Equifax Credit Risks Insight Servicing)

Dependent Variables:	Pr(Moved to rent in 4yr)	Pr(Moved in 4yr), in cnty	Pr(Moved to rent in 4yr)
Model:	(1)	(2)	(3)
Variables			
Post	0.0132**	0.0118**	
	(0.0067)	(0.0054)	
Post × Age quartile 1			0.0287*
			(0.0156)
Post × Age quartile 2			0.0011
			(0.0133)
Post × Age quartile 3			-0.0049
			(0.0123)
Post × Age quartile 4			0.0110
			(0.0116)
Trend width	6mo	6mo	6mo
Pre avg	0.05	0.029	
Fixed-effects			
Age quintile-Inc quintile	Yes	Yes	Yes
County-ever refi-Age quartile			Yes
Fit statistics			
Observations	32,355	32,355	31,048
R^2	0.06125	0.05334	0.12183
Within R^2	0.00622	0.00394	0.00654

Clustered (Person & Move in date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing. The table presents estimates of β_1 in equation (7). Column (1) reproduces Column (3) of Table 1. Column (2) replaces the outcome variable with the probability of a within-county own-to-rent move resulting in an existing-home sale within 4 years. Column (3) interacts coefficients in equation (7) with age quartile indicators and shows the interactions with β_1, where the outcome is the same as in Column (1).

D Section 4 details.

D.1 Derivation of Equation (9), with and without demographic controls

Consider the model described in Section 1, with market-clearing expression (A.3). Define $\bar{h}_{ot} \equiv \int h_{io} dH_{ot}$, the average non-financial preference for owner-occupied housing among owner-occupants in t. A first-order Taylor expansion around price P_0 yields:

$$P' = P_0 + \frac{\partial P_0}{\partial MV_0}\left(MV' - MV_0\right) + \frac{\partial P_0}{\partial WTA_0}\left(WTP' - WTP_0\right) + \frac{\partial P_0}{\partial \bar{h}_{o0}}\left(\bar{h}'_o - \bar{h}_{o0}\right) \tag{D.1}$$

Applying this approximation to local market j in city c and rearranging, equation (D.1) implies:

$$\frac{P_{cj,t+k} - P_{cj,t}}{P_{cj,t}} = \beta_1\left(\frac{MV_{jc,t+k} - MV_{jc,t}}{P_{jc,t}}\right) + \beta_2\left(\frac{WTP_{jc,t+k} - WTP_{jc,t}}{P_{jc,t}}\right) + \beta_3\left(\frac{\bar{h}_{ojc,t+k} - \bar{h}_{ojc,t}}{P_{jc,t}}\right) \tag{D.2}$$

Assume that the expected per-period rental cost growths at a constant rate across markets j within city c: $g_{jc,t} = g_{ct}$. Furthermore, write growth in $\bar{h}_{ojc}$ as a proportion of initial prices: $\bar{h}_{ojc,t+k} - \bar{h}_{ojc,t} = (\tilde{\omega}_c + \tilde{\xi}_{jc})P_{jc,t}$.

We can write the scaled difference in WTP as a linear combination of the period-t rent-price ratio and the ratio of period $t+k$ rents to period t prices with city-specific coefficients:

$$\frac{WTP_{jc,t+k} - WTP_{jc,t}}{P_{jc,t}} = \underbrace{\left(\left[r_{t+k} - g_{c,t+k}\right]^{-1}\right)}_{\equiv \tilde{\eta}_{1c}} \frac{\ell_{jc,t+k}}{P_{jc,t}} \underbrace{-\left(\left[r_t - g_{ct}\right]^{-1}\right)}_{\equiv \tilde{\eta}_{2c}} \frac{\ell_{jc,t}}{P_{jc,t}} \tag{D.3}$$

The scaled difference in $\bar{h}_{ojc}$ becomes: $\frac{\bar{h}_{ojc,t+k} - \bar{h}_{ojc,t}}{P_{jc,t}} = \left(\tilde{\omega}_c + \tilde{\xi}_{jc}\right)$. Let $\omega_c \equiv \beta_3 \cdot \tilde{\omega}_c$, $\xi_{jc} \equiv \beta_3 \cdot \tilde{\xi}_{jc}$, and $(\eta_{1c}, \eta_{2c}) \equiv \beta_2 \cdot (\tilde{\eta}_{1c}, \tilde{\eta}_{2c})$.

Parameterize ξ_{jc} as a linear combination of observed and unobserved components:

$$\xi_{jc} \equiv \eta'_{\xi,c} X_{\xi,jc,t} + \epsilon_{jc} \tag{D.4}$$

Equation (D.2) becomes:

$$\%\Delta P_{jc,t,t+k} = \beta_1 \cdot \Delta MVP_{jc,t,t+k} + \eta'_c \cdot X_{jc,t,t+k} + \omega_c + \epsilon_{jc} \tag{D.5}$$

the expression in the text, with $\eta_c \equiv (\eta_{1c}, \eta_{2c}, \eta_{\xi,c})'$ and $X_{jc,t,t+k} \equiv \left(\frac{\ell_{jc,t+k}}{P_{jc,t}}, \frac{\ell_{jc,t}}{P_{jc,t}}, X_{\xi,jc,t}\right)'$. If

$\epsilon_{jc} \perp \Delta MVP_{jc,t,t+k} | X_{jc,t,t+k}, \omega_c$, then a fixed-effects regression will consistently estimate β_1 in equation (D.5). Estimation without additional demographic controls assumes this assumption holds with $X_{\xi,jc,t} = \varnothing$.

D.2 Derivation of $MVP(\cdot)$ formula

This section gives a formula for MVP, defined in equation (11). We assume a mortgage has a fixed rate for the life of the loan and amortizes over 30 years with monthly payments, no default, and no curtailments.

Let $LTV, M_0, r_0^m, n_i, \{y_k\}_k$ denote, respectively, the origination LTV, initial mortgage balance, monthly fixed interest rate on mortgage balances, number of years remaining, and the nominal yield curve, where y_k is the continuously compounded nominal yield on a zero-coupon bond that pays k years hence.

Using the amortization formula, annual mortgage payments are $m_i = 12 \cdot M_0 \cdot \frac{r_0^m \cdot (1+r_0^m)^{360}}{(1+r_0^m)^{360}-1}$. The outstanding balance in year t, assuming no curtailment or default, is:

$$M_t = M_0 \cdot (1 + r_0^m)^{12\cdot(30-n_i)} - \frac{m_i}{12} \cdot \frac{(1+r_0^m)^{12\cdot(30-n_i)} - 1}{r_0^m} \tag{D.6}$$

$$= M_0 \left((1+r_0^m)^{12\cdot(30-n_i)} - \frac{r_0^m \cdot (1+r_0^m)^{360}}{(1+r_0^m)^{360}-1} \cdot \frac{(1+r_0^m)^{12\cdot(30-n_i)} - 1}{r_0^m} \right) \tag{D.7}$$

where the second line follows from substituting in the formula for annual payments and factoring out M_0.

Plugging in m_i and M_i into equation (11) and factoring like terms delivers:

$$MVP = \frac{M_0}{P_a} \cdot \frac{r_0^m \cdot (1+r_0^m)^{360}}{(1+r_0^m)^{360}-1} \cdot \left(\frac{(1+r_0^m)^{360}-1}{r_0^m \cdot (1+r_0^m)^{12n_i}} - \frac{(1+r_0^m)^{12\cdot(30-n_i)}-1}{r_0^m} - 12 \cdot E_t \sum_{q \le n_i} (1+r_q)^{-q} \right) \tag{D.8}$$

By definition, $E_t(1+r_q)^{-q} = \exp\big(-y_q \cdot q\big)$. Therefore:

$$MVP(LTV_0, r_0^m, n_i, \{y_k\}_k) \equiv LTV_0 \cdot R^1(r_0^m) \cdot \left(R^2(r_0^m, n_i) - 12 \cdot \sum_{q \le n_i} \exp\big(-y_q \cdot q\big) \right) \tag{D.9}$$

$$R^1(r_0^m) \equiv \frac{r_0^m \cdot (1+r_0^m)^{360}}{(1+r_0^m)^{360}-1}, \quad R^2(r_0^m, n_i) \equiv \frac{(1+r_0^m)^{360}-1}{r_0^m \cdot (1+r_0^m)^{12n_i}} - \frac{(1+r_0^m)^{12\cdot(30-n_i)}-1}{r_0^m}$$

D.3 Origination quarter fixed effects controls.

We first show that from an econometric perspective, controlling for $\sum_q \nu_q \sum_{\tau\in q} w^O_{j\tau}$ removes correlation between the instrument and property-level origination quarter fixed effects that affect property-level price growth.

We then show these controls also appear in an extension of the model in Section 1 allowing for origination timing heterogeneity.

Econometric argument.

Proof. Assume that equation (9) holds at the property level with an origination quarter fixed effect:

$$\%\Delta P_{ijc,t,t+k} = \beta \cdot \Delta MVP_{ijc,t,t+k} + \eta_c' \cdot X_{ijc,t,t+k} + \omega_c + \nu_{q(i)} + \epsilon_{ijc} \tag{D.10}$$

where $q(i)$ is the origination quarter for property i. Taking expectations yields:

$$\%\Delta\overline{P}_{jc,21\to 23} = \beta \Delta\overline{MVP}_{jc,21\to 23} + \eta_c' X_{jc,21\to 23} + \omega_c + \sum_q \nu_q \Pr(q(i)=q) + \overline{\epsilon}_{jc} \tag{D.11}$$

where $\overline{\epsilon}_{jc} = E[\epsilon_{ijc}]$. Suppose for one of our timing-based instruments z_{jc} that $(z_{jc} \perp \overline{\epsilon}_{jc})|X_{jct}, \omega_c$. For the unobserved component ε_{jc} in equation (10):

$$\varepsilon_{jc} = \overline{\epsilon}_{jc} + \sum_q \nu_q \Pr(q(i)=q) \tag{D.12}$$

Therefore, $(z_{jc} \not\perp \varepsilon_{jc})|X_{jct}, \omega_c$ because origination timing, reflected in $\Pr(q(i)=q)$, affects fixed mortgage rates and hence z_{jc}.

Including linear controls for $\left\{\sum_{\tau\in q} w^O_{j\tau}\right\}$ fixes this problem, since $\sum_{\tau\in q} w^O_{j\tau} = \Pr(q(i)=q)$. Specifically, $(z_{jc} \perp \epsilon_{jc})|X_{jct}, \omega_c, \left\{\sum_{\tau\in q} w^O_{j\tau}\right\}$ as required. □

Economic argument. It is not obvious that equation (D.10) comes from an equilibrium model. We now show that its result – equation (D.11), approximates an extension of the model from Section 1 where owner-occupants have different relative preferences for owner-occupied and rental housing depending on when they originally moved into a unit.

Proof. Assume that in the Section 1, $h_{io} \sim H_{qo}$ for household i having their mortgage originated in quarter q. The market clearing expression from equation (A.3) becomes:

$$\gamma_o \mu \left(\sum_q \Pr(q(i) = q) H_{qo} \left(\alpha(P_t - WTA_{qt}) \right) \right) = \gamma_\ell (1 - \mu) (1 - H_\ell(\alpha(P_t - WTP_t))) \tag{D.13}$$

Assume that the distribution of consumer preferences are linear, with $H_{qo}(x) = h_{qo} + \sigma_o^{-1} x$ and $H_\ell(x) = h_\ell + \sigma_\ell^{-1} x$. This can be seen as a local approximation to a more flexible distribution.

Equation (D.13) becomes:

$$\begin{aligned} \left(\gamma_o \mu \sum_q \Pr(q(i) = q) \cdot h_{qo} \right) + \gamma_o \mu \sigma_o^{-1} \alpha \left(P_t - \overline{WTA}_t \right) \\ = \gamma_\ell (1 - \mu) \left(1 - h_\ell - \sigma_\ell^{-1} \alpha (P_t - WTP_t) \right) \end{aligned} \tag{D.14}$$

where $\overline{WTA}_t \equiv E[WTA_q]$. Solving for P_t:

$$\begin{aligned} P_t = P_0 - \sum_q \Pr(q(i) = q) \cdot h_{qo} \frac{\mu \gamma_o}{\alpha \left(\mu \gamma_o \sigma_o^{-1} + (1 - \mu) \gamma_\ell \sigma_\ell^{-1} \right)} \\ + \frac{\mu \gamma_o \sigma_o^{-1}}{\mu \gamma_o \sigma_o^{-1} + (1 - \mu) \gamma_\ell \sigma_\ell^{-1}} \overline{WTA}_t + \frac{(1 - \mu) \gamma_\ell \sigma_\ell^{-1}}{\mu \gamma_o \sigma_o^{-1} + (1 - \mu) \gamma_\ell \sigma_\ell^{-1}} WTP_t \end{aligned} \tag{D.15}$$

Parameterizing $h_{qo} \equiv \tilde{\nu}_q \cdot P_t$, defining $\nu_q \equiv \tilde{\nu}_q \cdot \frac{\mu \gamma_o}{\alpha \left(\mu \gamma_o \sigma_o^{-1} + (1 - \mu) \gamma_\ell \sigma_\ell^{-1} \right)}$, and following steps as in Appendix Section D.1 delivers an estimating equation like (D.11). □

D.4 Proof of Proposition 1

Let $\chi_{jt} \equiv (\omega_{c(j)}, X_{jt})$ collect observable local housing market characteristics. Define the instrument as $MVP_{jt}^S = z_t(r_{jt}^S)$. In this section, unlike in the main text, $\Delta \log S_{jt}$ refers to the one-period (rather than two-period) difference in $\log S_{jt}$.

Formally, Proposition 1 claims that:

$$E\left[\varepsilon_{jt} z_t(r_{jt}^S) | \chi_{jt} \right] = 0 \tag{D.16}$$

if ε_{jc} is mean-independent of $\Delta \log S_{j\tau}$:

$$E\left[\varepsilon_{jt} | \Delta \log S_{j\tau}, \chi_{jct} \right] = E\left[\varepsilon_{jt} | \chi_{jt} \right] \quad \forall j\tau \tag{D.17}$$

Proof. First, that ε_{jt} is mean-independent of $\Delta \log S_{j\tau}$ implies:

$$E\left[\varepsilon_{jt} G\left(\Delta \log S_{j\tau}\right) | \chi_{jt}\right] = 0 \quad \forall G(\cdot),\ j\tau \tag{D.18}$$

Second, note that where τ_0 is the first-observed τ:

$$S_{j\tau} = S_{j\tau_0} \exp\left(\sum_{q>\tau_0} \Delta \log\left(S_{jq}\right)\right) \tag{D.19}$$

By linearity of $v^S_{j\tau}$, we can therefore write:

$$v^S_{j\tau}(\boldsymbol{S}_{j\tau}) = S_{j\tau_0} v'^S_{\tau}\left(\Delta \log \boldsymbol{S}_{j\tau}\right) \tag{D.20}$$

where $\Delta \log \boldsymbol{S}_{j\tau} \equiv \{\Delta \log S_{jt}\}_{t<\tau}$. An implication is that the predicted origination shares are only a function of $\Delta \log S_{jt}$:

$$w^S_{j\tau,t} = \frac{K(t-\tau)\cdot v^S_{j\tau}}{\sum_{\tau'\leq t} K(t-\tau)\cdot v^S_{j\tau}} = \frac{S_{j\tau_0} K(t-\tau)\cdot v'^S_{\tau}(\Delta \log \boldsymbol{S}_{j\tau})}{S_{j\tau_0} \sum_{\tau'\leq t} K(t-\tau)\cdot v'^S_{j\tau}(\Delta \log \boldsymbol{S}_{j\tau})} = w^S_{\tau,t}(\Delta \log \boldsymbol{S}_{j\tau}) \tag{D.21}$$

Third, equation (D.21) implies that:

$$r^S_{jt} = \sum_{\tau\leq t} w^S_{\tau,t}(\Delta \log \boldsymbol{S}_{jc\tau})\cdot r^m_{\tau} = r^S_t\left(\Delta \log \boldsymbol{S}_{jt}, \{r^m_{\tau}\}_{\tau}; w^S_{\tau,t}(\cdot)\right) \tag{D.22}$$

The claim that equation (D.16) holds follows from defining $G \equiv z_t \circ r^S_t$ and applying equation (D.18). □

Example 1 (Twin birth rate iid draw from city-by-year distribution.). *Suppose that the twin birth rate $m_{jt} \sim^{iid} \tilde{G}_{c(j),t}$, where $\tilde{G}_{ct}$ is a CBSA-by-year specific distribution. Then $\Delta \log m_{jt} \sim^{iid} G_{c(j),t}$, a distribution based on $\tilde{G}_{c(j),t}$ and $\tilde{G}_{c(j),t-1}$.*

Conditioning on χ_{jt} includes conditioning on CBSA $c(j)$, which implicitly conditions on the history of $G_{c(j),t}$. We can therefore write $\chi_{jt} \equiv \left(X_{jt}, \left\{G_{c(j),q}\right\}_{q\geq\tau_0}, u_{c(j)}\right)$, where $u_{c(j)}$ include unobserved characteristics of CBSA $c(j)$ besides the twin birth rate distribution history.

For each $j\tau$:

$$E\left[\varepsilon_{jt} | \Delta \log m_{j\tau}, X_{jt}, \left\{G_{c(j),q}\right\}_{q\geq\tau_0}, u_c(j)\right] = E\left[\varepsilon_{jt} | X_{jt}, \left\{G_{c(j),q}\right\}_{q\geq\tau_0}, u_{c(j)}\right] \tag{D.23}$$

because the realized draw contains no information above the distribution itself. This is equivalent to ε_{jt} being mean-independent of $\Delta \log m_{j\tau}$ for all $j\tau$ (equation (D.17) being satisfied), which means Proposition 1 holds.

Proposition 1 also holds in two special cases considered in the main text:

- *Twin birth rates drawn from a constant distribution:* $G_{ct} = G$ *for all* ct.
- *Twin birth rates drawn from a time-invariant CBSA-specific distribution:* $G_{ct} = G_c \; \forall ct$.

D.5 Twin birth rate instrument construction.

D.5.1 Variation in IVF usage does not explain much variation in the twin birth rate.

This section provides additional analysis demonstrating that variation in IVF usage is not a major driver of variation in the twin birth rate. We focus on data from after 2016 – while not in our sample period, this is the first year for which the CDC publishes detailed natality data that links the use of assistive reproductive technology (ART, of which IVF is a subset) to multiple births.

First, IVF births are not a significant contributor to the twin birth rate. In 2016, 76,892 infants were born using ART, accounting for 1.8% of all births and 16.4% of all multiple birth infants (Sunderam 2019).

Second, IVF usage rates does not explain much *variation* in the twin birth rate across geography. Table D.7 regresses the twin birth rate on the fraction of births using ART from 2016-2023, the only years for which data are available. Column (1) shows results in levels, and Column (2) shows results in first differences. Higher ART usage does translate into higher twin birth rates. However, most variation in the twin birth rate is not explained by ART. In levels, variation in ART only explains about 2% of variation in the twin birth rate. In differences – using variation relevant for the identifying assumption– changes in ART usage explain only 0.13% of the change in the twin birth rate.

D.5.2 Estimating β_a.

As mentioned in the main text, we slightly increase power by parameterizing β_a in equation (12) as a fifth-order polynomial with fixed effects at ages 1, 5, and 9. Specifically, we let:

$$\beta_a = \sum_{p=1}^{5} \pi_a \cdot a^p + \sum_{a' \in \{1,5,9\}} \tilde{\beta}_a \mathbb{1}(a = a') \tag{D.24}$$

and estimate π_a and $\tilde{\beta}_a$ via OLS. Figure D.7 shows estimates.

D.5.3 Estimating repayment rates.

We estimate the repayment kernel $K(a)$ using borrower-level records from ICE, McDash®.

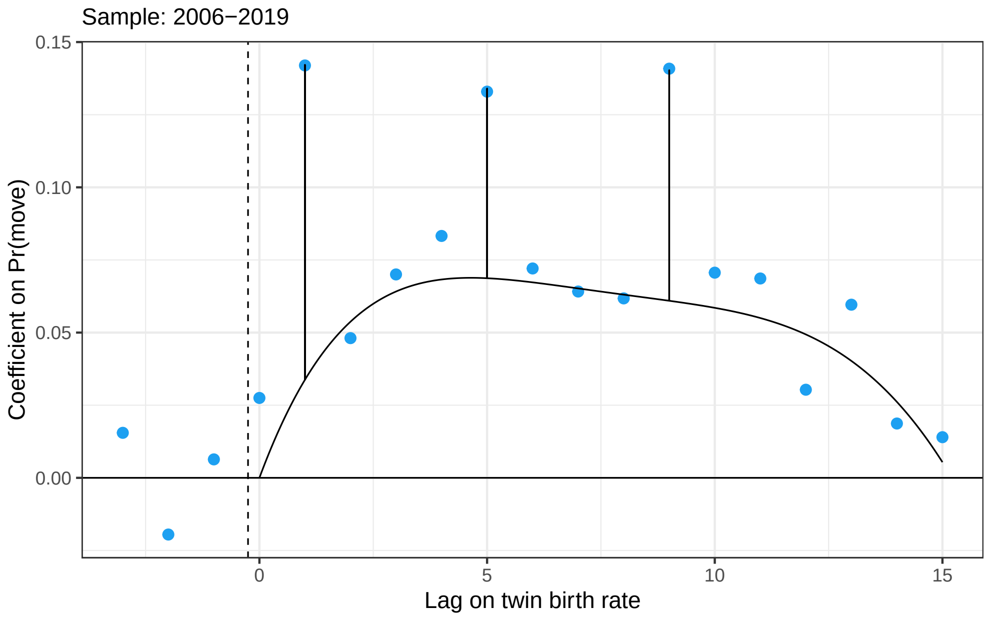
Table D.7: Assistive Reproductive Technology usage explains little variation in twin birth rate.

Dependent Variables:	Pr(Twin)	Δ Pr(Twin)
Model:	(1)	(2)
Variables		
Constant	0.0244***	-0.0004***
	(0.0003)	(7.04×10^{-5})
Pr(ART)	0.0980***	
	(0.0211)	
Δ Pr(ART)		0.0810*
		(0.0426)
Fit statistics		
Observations	4,560	3,990
R^2	0.01989	0.00155
Adjusted R^2	0.01967	0.00129

Clustered County and Year standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Author's calculations using CDC Natality data from 2016-2023. The table shows estimated coefficients from a regression of the county twin birth rate (twins / total births) on the county ART utilization rate (ART births / total births). The first column estimates in levels, and the second column estimates in first differences. Robust standard errors are clustered by county and calendar year.

Figure D.7: Polynomial estimates of the effect of the twin birth rate on local housing market churn.
(Source: Equifax Credit Risks Insight Servicing)



Source: Equifax Credit Risks Insight Servicing. The figure plots fitted values of β_a from equation (D.24). See text for details.

We model repayments, as a fraction of remaining balances, as additively separable in calendar time and tenure effects:

$$\frac{\Delta M_{it}}{M_{i,t-1}} = \gamma_t + \sum_a \lambda_a I(t - T_i = a) + \epsilon_{it} \tag{D.25}$$

where T_i is the origination year of mortgage i. A main source of average ΔM_{it} is borrower full repayment due to moves or refinancing. If the outcome were specified in either levels or as a fraction of initial balances, full repayments would have different effects on estimated coefficients depending on when repayment occurs. This implicitly introduces a remaining balance-by-tenure effect, which the specification in equation (D.25) avoids.

We calculate the fraction of balances remaining a years after origination recursively as:

$$K(0) = 1 \tag{D.26}$$

$$K(a) = K(a-1) \cdot (1 - \hat{\lambda}_a) \tag{D.27}$$

$K(a)$ gives the fraction of balances remaining a years after origination. Assuming a constant inflow of mortgages, the outstanding distribution of origination dates is a weighted average of mortgages originated at each horizon a, with weights proportional to $K(a)$.

D.6 Standard deviation calculations in Figure 2.

This section describes how we form theoretical benchmarks for the standard deviations shown in Figure 2.

Notation. Let j, c, and t index county, CBSA, and year respectively. Let n_{jt}, p_{jt} denote the number of total births and the twin birth rate in county j, respectively. The estimated number of twins born is then $p_{jt} \cdot n_{jt}$.

Twin birth rate distributions satisfying Proposition 1. Assume that $p_{jt} \sim^{iid} \tilde{G}_{c(j),t}$, where $\tilde{G}_{ct}$ is a CBSA-by-year specific distribution. Then $\Delta \log p_{jt} \sim^{iid} G_{c(j),t}$, where G_{ct} is some distribution based on $\tilde{G}_{ct}$ and $\tilde{G}_{c,t-1}$.

Levels. First, consider the case of iid binomial national distribution: $p_{jt} \cdot n_{jt} \sim^{\text{iid}} \text{Binom}(\overline{p}, n_{jt})$, where $\overline{p}$ is the national twin birth rate. We estimate $\overline{p}$ as the county-weighted average of p_{jt}, $\widehat{\overline{p}} \equiv N^{-1} \sum_{j,t} p_{jt}$, where N is the number of county-years from 1995-2005. For each jt, the expected standard deviation is $\frac{\overline{p}(1-\overline{p})}{n_{jt}}$. For each size bin $\mathscr{B}_k$, we estimate the theoretical benchmark as:

$$\frac{1}{N_k} \sum_{jt \in \mathscr{B}_k} \frac{\widehat{\overline{p}}(1 - \widehat{\overline{p}})}{n_{jt}} \tag{D.28}$$

where $N_k \equiv |\mathscr{B}_k|$ is the number of county-years in size bin $\mathscr{B}_k$.

Next, consider the case of iid binomial draws from a CBSA-specific binomial distribution:

$p_{jt} \cdot n_{jt} \sim^{\text{iid}} \text{Binom}(\overline{p}_c, n_{jt})$. By the law of total variance:

$$\begin{aligned}\text{Var}\left(p_{jt}\right) &= E\left[\text{Var}\left(p_{jt}\right)\middle|c(j)\right] + \text{Var}\left(E\left[p_{jt}\middle|c(j)\right]\right) \\ &= E\left[\frac{\overline{p}_{c(j)} \cdot (1-\overline{p}_{c(j)})}{n_{jt}}\right] + \text{Var}\left(\overline{p}_{c(j)}\right) \end{aligned} \tag{D.29}$$

We then estimate $\text{Var}(p_{jt})$ by taking the sum of the sample counterparts of the terms in equation (D.29) within each size bin. We first estimate $\widehat{\overline{m}}_c \equiv N_c^{-1} \sum_{j,t} p_{jct}$, where N_c is the number of county-years in CBSA c. Next, we estimate the expected conditional variance as an equal weighted mean within size bin: $N_k^{-1} \sum_{jt \in \mathscr{B}_k} \frac{\widehat{\overline{p}}_{c(j)} \cdot (1-\widehat{\overline{p}}_{c(j)})}{n_{jt}}$. Finally, we estimate the cross-CBSA variance of the expectation for observations within each size bin using the sample variance, $\widehat{\text{Var}}_k(\widehat{\overline{p}}_c)$.

Finally, in the case of iid binomial draws from a CBSA-by-year specific binomial distribution, $p_{jt} \cdot n_{jt} \sim^{\text{iid}} \text{Binom}(\overline{p}_{c(j),t}, n_{jt})$. By the law of total variance:

$$\text{Var}\left(p_{jt}\right) = E\left[\frac{\overline{p}_{c(j),t} \cdot (1-\overline{p}_{c(j),t})}{n_{jt}}\right] + \text{Var}\left(\overline{p}_{c(j),t}\right) \tag{D.30}$$

Similar to the CBSA-specific case, we sum the sample counterparts of equation (D.30) within each size bin, where we estimate $\widehat{\overline{p}}_{ct} \equiv N_{ct}^{-1} \sum_{j \in c} p_{jt}$, where N_{ct} is the number of county-level observations in year t in CBSA c.

Estimates are in Figure D.8 below.

First differences (right panel). In the case where twin births are modeled as iid draws from a national binomial distribution, the theoretical variance is $\text{Var}(p_{jt} - p_{j,t-1}) = \overline{p}(1-\overline{p}) \cdot (n_{jc}^{-1} + n_{j,t-1}^{-1})$. We estimate the theoretical benchmark within each size bin by taking the average of the estimated variance:

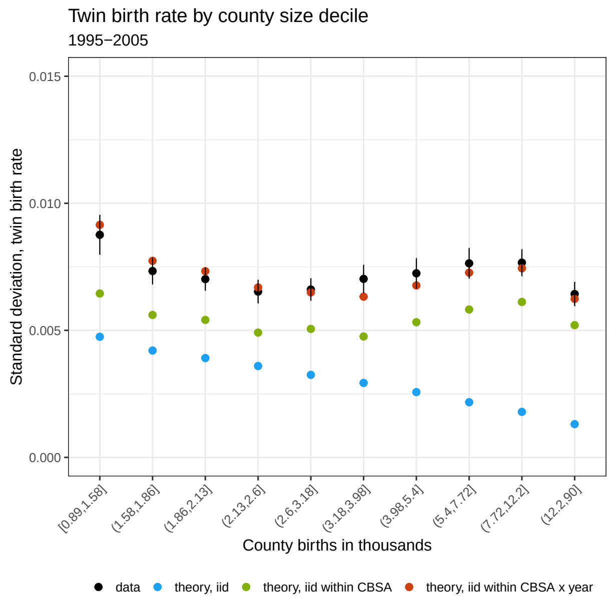
$$\frac{1}{N_k} \sum_{jt \in \mathscr{B}_k} \widehat{\overline{p}} \cdot \left(1 - \widehat{\overline{p}}\right) \cdot \left(\frac{1}{n_{jt}} + \frac{1}{n_{j,t-1}}\right) \tag{D.31}$$

In the case where twin births are modeled as iid draws from a CBSA-by-year specific binomial distribution, the law of total variance implies that:

$$\text{Var}(\Delta p_{jt}) = E\left[\frac{\overline{p}_{c(j),t} \cdot (1-\overline{p}_{c(j),t})}{n_{jt}} + \frac{\overline{p}_{c(j),t-1} \cdot (1-\overline{p}_{c(j),t-1})}{n_{j,t-1}}\right] + \text{Var}\left(\Delta \overline{p}_{c(j),t}\right) \tag{D.32}$$

We estimate this benchmark by summing the sample counterparts of equation (D.32) within each size bin as in the case with levels. Note that we omit the case where twin births are

Figure D.8: Actual and theoretical variation in twin birth rate



Source: Author's calculations using CDC natality data from 1995-2005. The figure plots the standard deviation of the twin birth rate (on a scale from 0 to 1) within county birth deciles, in the data and under different distributional assumptions.

iid draws from a binomial CBSA (but not calendar time) specific distribution, because in that case, the second term in equation (D.32) would equal zero and the associated estimate would converge to the same value as the estimate in equation (D.31).

D.7 Calibrating twin instrument relevance and power.

D.7.1 Twin instrument relevance.

This section analyzes coefficient magnitudes for the estimated effect of the twin birth rate on total moves, as plotted in Figure 3. We focus on the impact of the twin birth rate in year t on moves in $t+1$, which at the family level are moves the year after twins are born rather than one child. Recall that we estimate moves among owner-occupants with a mortgage.

The coefficient on the twin birth rate (where the rate runs from 0 to 1) is:

$$\Delta\Pr(\text{Move}|\text{twins}) = \Big(\Pr(\text{Has kid}) \times \Delta\Pr(\text{Move}|\text{twins}) \tag{D.33}$$

$$+ \Pr(\text{Twin-related}) \times \Delta\Pr(\text{Move}|\text{twin-related})\Big) \tag{D.34}$$

$$\times E\left[\text{Vacancy chain length}\right] \tag{D.35}$$

This expression has three important terms. The first, in line (D.33), shows the *direct* effect of twin births on moves among families that have multiple births rather than singletons. The first part, Pr(Has kid), is the birth rate among owner-occupied households with a mortgage. A rough estimate is the equal-weighted average ratio across zip codes of total births to owner-occupied households with a mortgage, equal to 0.11 in the 2014 ACS.[47] The second part, $\Delta\,\mathrm{Pr}(\mathrm{Move}|\mathrm{twins})$, reflects how much having twins rather than one child impacts moves. Twins are unexpected for most families, and increase the number of expected children by a huge amount – by 100% (first births), 50% (second births), or 33% (third births). We calibrate this term as equal to 0.5 based on the results of an informal Reddit survey (where about half of respondents on the forum r/parentsofmultiples said they moved when they found out they were having twins).

The second part of the formula, in line (D.34), reflects *indirect* moves due *directly* to twin births, outside of the nuclear family where the twin birth occurs. Anecdotally, many parents of multiples ask their parents or siblings to relocate to help with caregiving. Furthermore, many local twins may create congestion for childcare, kindergarten, and pre-K, leading to relocation. Absent good data, we set this term to zero and consider our calibration a lower bound.

The third part of the formula, in line (D.35), reflects *spillover* effects on the local housing market due to twin-driven moves. When having twins causes one family to move, they both leave their previous unit vacant and, if they purchase a previously owner-occupied unit, free up their new unit's previous owner to move. Their previous vacancy is filled by another family, who creates their own vacancy. In this manner, twin-driven moves create housing market churn that impacts the timing of moves for a broader set of families unrelated to the one having twins.

It is hard to get precise estimates for how much churn such moves will create. A recent paper by French and Gilbert (2023) studies effective vacancies generated by low-density suburban housing, and tabulates when these vacancies occur by length of "vacancy chain." Estimates from this paper are likely to be a generous lower bound in our context. Rather than moves to new construction in low-density suburban markets specifically, likely driven by new family formation and hence unlikely to leave vacancies, we study moves among existing owner-occupants. Results provided by the authors (not in the working paper) imply that the average vacancy emerges after 0.92 moves. Including the initial move, this implies a lower bound of 1.92 moves. We round this to 2. This is slightly lower than the 2.5 "transactions multiplier effect" estimated due to increased housing demand in "hot" markets in Anenberg and Ringo

[47]This assumes that 60% of owner-occupied households have a mortgage and that all children are born to owner-occupied households with a mortgage. The former is probably an under-estimate of the equal-weighted average owner-occupant mortgage rate (because the owner-occupant mortgage rate is likely higher in less populous zip codes) and the latter is probably an over-estimate (because not all children are born into owner-occupied households with mortgages). Furthermore, the overall birth rate (births / population) was higher during the earlier years of our sample where much of the variation comes from, but for which we lack small-area data from surveys such as the ACS. This also would increase the above ratio.

(2022), which the authors describe as a lower bound on effects on moves.[48] '

Putting everything together, we should expect that, as a lower bound:

$$\Delta \Pr(\text{Move}|\text{twins}) = 0.11 \times 0.5 \times 2 = 0.11 \in [0.08, 0.20] \tag{D.36}$$

which is well within the 95% confidence interval for the coefficient estimate of β_1.

D.7.2 Twin instrument power.

Simulated variation in churn. The main text analyzes the degree of empirical housing market churn generated by the twin birth rate instrument. We argue that most of this churn comes from finite sample variation in realized twin birth rates. This section conducts simulations to both (i) further demonstrate this directly; and (ii) quantify more clearly how many households are affected by churn due to finite sample variation in the twin birth rate.

We start by considering a county with 5,000 annual births, at about the 40th percentile of the county size distribution. Such a county has about 57,000 households, and a twin birth rate of about 3.6%. We run 10,000 simulations to produce the analysis that follows.

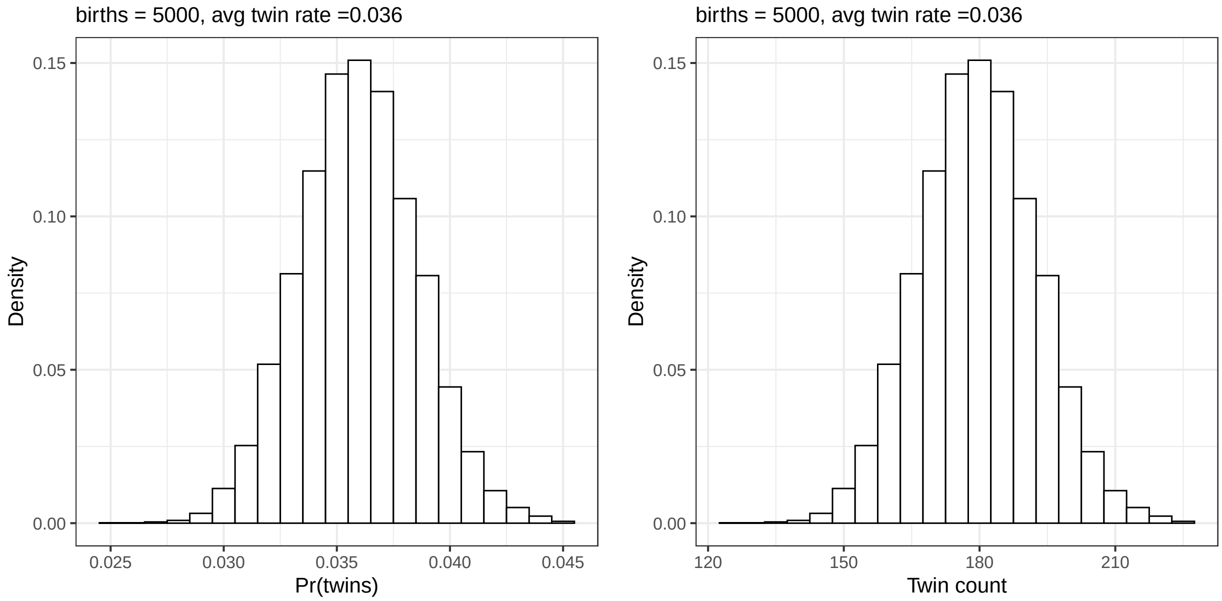
First, there is significant variation in the number of twins born given this county size. Figure D.9 plots the distribution of the annual twin birth rate (left panel, SD = 0.26%) and the distribution of the number of annual twin births (right panel, SD = 13.2).

Second, the cumulative effect of variation in the twin birth rate from 1995-2005 creates significant cross-sectional differences in 2021 in origination timing. To show this, we simulate twin births from 1995-2005, and run the simulated path through our instrument construction methodology.[49] We then compare annual twin-driven predicted originations outstanding in 2021 for a simulation with an instrument value 1.96 SD above the mean to those of a simulation with an instrument value 1.96 SD below the mean, similar to in Figure 3. Figure D.10 plots the resulting differences, both as a percentage of the 2021 population (left panel) and in levels (right panel). Draws with a high value of the instrument have relatively more originations in earlier years and relatively fewer in later years, consistent with a downwards trend in the mortgage rate. As in the main text, the key point is that even though the difference in origination in any one year is small, the small differences accumulate to sizable ones taken over 1995-2020. In these simulations, variation in the twin birth rate reshuffles origination

[48] The unit of analysis in this paper is housing market transactions, not household moves. In Appendix A.1, the authors find that their estimate for the probability that the previous owner of a sold unit repurchases another one is lower than external estimates of the probability that a household who sells property purchases another property, which itself is different (and likely lower) than the probability that a household who sells their primary residence purchases another primary residence.

[49] These figures use a triangular kernel rather than an empirical one, and so slightly overstate churn. This will be fixed in later iterations.

Figure D.9: Simulated distribution of twin birth rates.



Source: Author's calculations. The figure shows the distribution of annual twin birth rates (left) and the annual number of twin births (right) across 10,000 simulations for a county with 5,000 annual births and a twin birth rate of 3.6%.

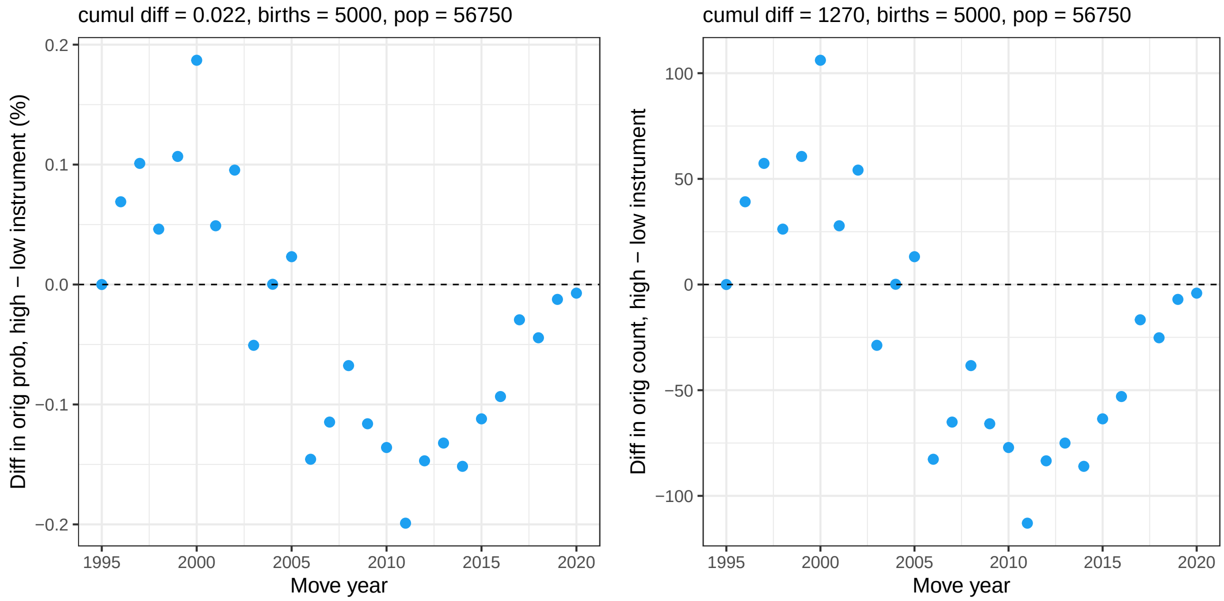
timing for 1,270 households with a mortgage in 2021, or 2.2% of the local population. A big change in the existing home sales incentives of this group – due to, for instance, rate lock – will easily have a detectable impact on existing home prices.

How does the impact of twin-based churn vary based on population size? Figure D.11 runs the same analysis for counties with different populations, and plots the fraction (left) or number (right) of households with mortgages outstanding in 2021 who have origination timing reshuffled.

Empirical variation in incentives. The predicted average interest rate outstanding in 2021 due to twin-driven moves, $r^{twin}_{j,2021}$ might have small variance for two reasons. First, all households could have mortgages with very small differences in origination rates. In this case, all households would face very similar incentives, and the scope for equilibrium price effects of rate lock would be small. Second, the fraction of households with very significant differences in interest rates might vary. In this case, equilibrium price effects are plausible, since across locations, the fraction of households affected by large rate lock incentives varies.

Empirically, the evidence supports the latter interpretation: $r^{twin}_{j,2021}$ varies across markets because of changes in the fraction of households who are significantly impacted by rate lock incentives. For each market j, we predict the fraction of households who had their mortgage

Figure D.10: Simulated difference in twin-predicted origination probability, zips +/- 1.96 SD of avg instrument.



Source: Author's calculations. The figure shows the difference in annual origination probability (left) or count (right) for each year for mortgages outstanding in 2021 between simulation draws where simulated instruments are 1.96 standard deviations above vs. 1.96 standard deviations below the mean. Values are calculated across 10,000 simulations for a county with 5,000 annual births and a twin birth rate of 3.6%.

originated when the interest rate was in each 30-year Treasury rate quintile:

$$\Pr\left(T_{ijt} \in \mathcal{T}_q\right) = \sum_{\tau \leq 2021} w_{j\tau,t}^{twin} \mathbb{1}(\tau \in \mathcal{T}_q) \tag{D.37}$$

where $\mathcal{T}_q$ is the set of calendar years for which the 30-year Treasury is within the qth quintile.

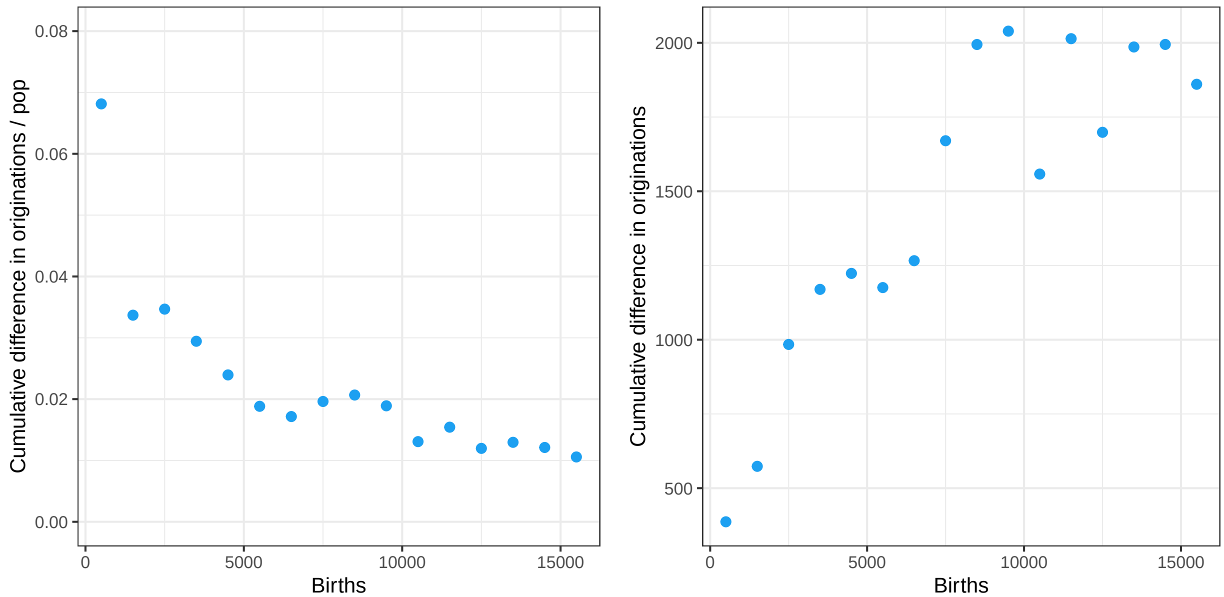
Figure D.18 plots estimates of $\Pr\left(T_{ijt} \in \mathcal{T}_q\right)$ for markets at the 10th percentile, median, and 90th percentile of the instrument. Markets with a low instrument – i.e. a low average predicted outstanding interest rate in 2021 – have more originations when interest rates are below 2.9% and fewer when interest rates are above 5.4%. This indicates a greater predicted fraction of households who are severely "locked." Markets with a higher instrument are less likely to have "locked" households.

D.8 First births instrument construction details.

D.8.1 Identifying assumption given framework for Proposition 1.

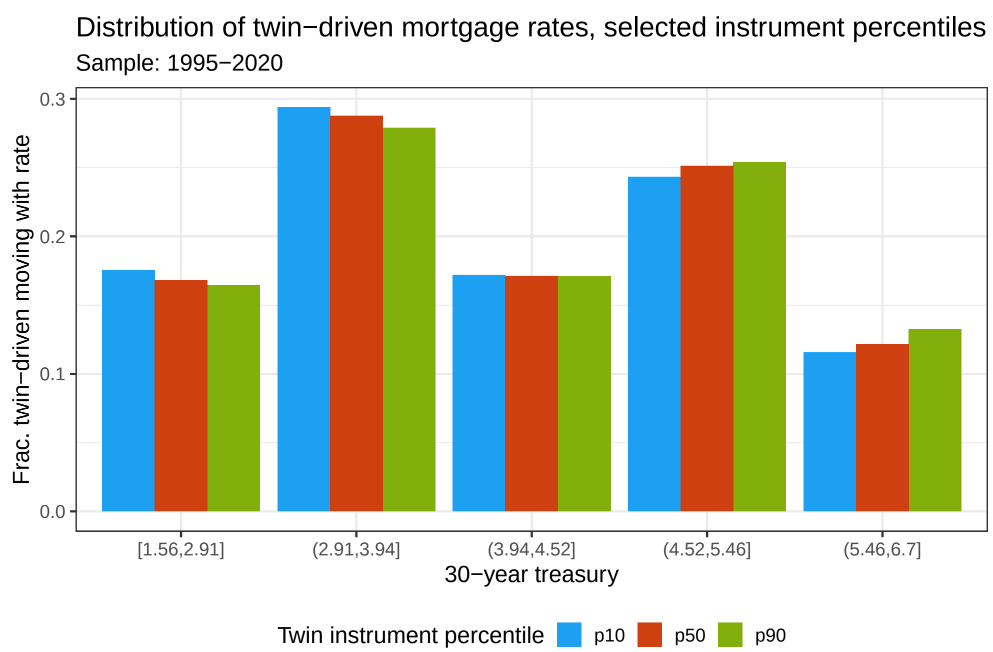
This subsection describes how the equation for predicted first-birth driven moves in (15) relates to the framework for Proposition 1.

Figure D.11: Cumulative difference in simulated twin-driven origination timing by county births.



Source: Author's calculations. The figure plots the cumulative difference in origination timing for households with mortgages outstanding in 2021, as a fraction of the population (left) or in levels (right). Each point shows results from simulations with the number of births indicated on the horizontal axis and a twin birth rate of 3.6% with 2,500 draws. For each simulation, we draw iid paths of the twin birth rate from 1995-2005 using a binomial distribution; calculate a distribution of predicted twin-driven mortgage rates; calculate the annual absolute value of the difference in originations for simulation draws with predicted mortgage rates +/- 1.96 SD of the mean; and summed these absolute values over the period 1995-2020 to get a cumulative estimate.

Figure D.12: Distribution of origination timing.



Source: Author's calculations using CDC natality data, Equifax Credit Risks Insight Servicing data, and ICE, McDash®data. The figure plots the twin-driven predicted probability that a given household is originated a mortgage in a year when the average mortgage rate equals the reference value.

Let $S_{jt} = \left(B_{jt} + \frac{\overline{Moves}_j}{\sum_a \beta_a}\right)$. Then:

$$v_{j\tau} = \sum_a \beta_a \left(B_{j,t-a} + \frac{\overline{Moves}_j}{\sum_a \beta_a}\right) \tag{D.38}$$
$$= \sum_a \beta_a B_{j,t-a} + \overline{Moves}_j \tag{D.39}$$

Clearly, S_{jt} defined as such can be factored out of $v_{j\tau}$ as required for the proof of Proposition 1 in Section D.4.

The identifying assumption behind the first births instrument is therefore that ΔS_{jt} is mean-independent of ε_{jt}. This essentially requires that the change in first births, as a fraction of average moves, is uncorrelated with ε_{jt}. To see this, note that:

$$\Delta S_{jt} = \log\left(B_{jt} + \frac{\overline{Moves}_j}{\sum_a \beta_a}\right) - \log\left(B_{j,t-1} + \frac{\overline{Moves}_j}{\sum_a \beta_a}\right) \tag{D.40}$$
$$= \log\left(\frac{\sum_a \beta_a}{\overline{Moves}_j} B_{jt} + 1\right) - \left(\frac{\sum_a \beta_a}{\overline{Moves}_j} B_{jt} + 1\right) \tag{D.41}$$
$$\approx \sum_a \beta_a \frac{\Delta B_{jt}}{\overline{Moves}_j} \tag{D.42}$$

where the second line follows by factoring out $\frac{\overline{Moves}_j}{\sum_a \beta_a}$ and the third line follows by applying the approximation $\log(1 + x) \approx x$. This approximation should be good because $\frac{\Delta B_{jt}}{\overline{Moves}_j} << 1$ (it is less than the number of births to number of moves ratio, which is the same as the birth rate to move rate ratio, which is about 1.5% / 8%) and $\sum_a \beta_a << 1$.

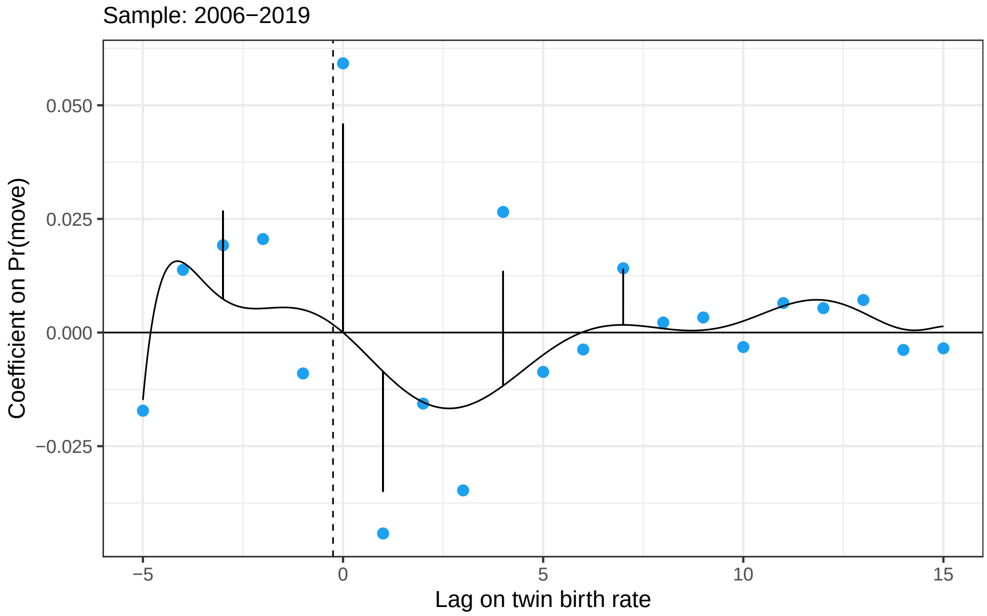
D.8.2 Estimating β_a.

As mentioned in the main text, we slightly increase power by parameterizing β_a in equation (12) as a tenth-order polynomial with fixed effects at ages -3, 1, 2, 4, and 7. Specifically, we let:

$$\beta_a = \sum_{p=1}^{10} \pi_a \cdot a^p + \sum_{a' \in \{-3,1,2,4,7\}} \tilde{\beta}_a \mathbb{1}(a = a') \tag{D.43}$$

and estimate π_a and $\tilde{\beta}_a$ via OLS. Figure D.13 shows estimates.

Figure D.13: Polynomial estimates of the effect of the twin birth rate on local housing market churn.
(Source: Equifax Credit Risks Insight Servicing)



Source: Equifax Credit Risks Insight Servicing. The figure plots fitted values of β_a from equation (D.43). See text for details.

D.9 Origination timing instrument robustness: twins and age distribution.

D.9.1 Twins: Addressing IVF-driven variation.

Appendix Section D.5.1 shows that cross-sectional variation in IVF usage is not an important driver of variation in in the twin birth rate due to its rarity. This subsection provides evidence that variation in IVF use does not appear to drive our twin IV estimates.

We cannot remove twin births due to IVF directly, because the CDC only started reporting IVF-by-plurality birth cross tabs in public datasets starting in 2016. In 2016, the data show that restricting to births to mothers under the age of 35 removes about 75% of IVF births. We therefore re-construct our instrument using the twin birth rate for mothers under 35.

Columns (1) and (2) of Table D.9 show results, with and without CBSA-by-demographic controls. Point estimates are nearly identical to those in the main results, although estimates are slightly more precise. This suggests that variation in IVF usage does not drive our estimates.

D.9.2 Twins: Controlling for changes in county-specific expected twin birth rate.

A sufficient condition for the estimates in the main text to consistently identify β in equation (10) is if changes in the twin birth rate are iid draws from a CBSA-by-time specific distribution.

In this section, we relax this assumption, and allow that the twin birth rate is an iid draw from a local market-specific, time-varying, autocorrelated binomial distribution. As in Borusyak and Hull (2023), we show that including a linear control for the expected value of the instrument given this process consistently identifies β by ensuring that all identifying variation comes exclusively from finite sample variation.

Estimating local twin birth rate process. We model the twin birth rate p_{jt} in location j in period t as a draw from a binomial distribution with success rate $\alpha m_{j,t-1} + \gamma_t + \rho_j$ and number of births n_{jt}, where γ_t is a calendar time trend, ρ_j is a location-specific level, and α is an autoregressive coefficient:

$$p_{jt} \cdot n_{jt} \sim \text{Binom}\left(n_{jt}, \alpha p_{j,t-1} + \gamma_t + \rho_j\right) \tag{D.44}$$

This process implies that:

$$p_{jt} = \alpha p_{j,t-1} + \gamma_t + \rho_j + \epsilon_{jt} \tag{D.45}$$

In this specification, $p_{j,t-1}$ is correlated with $\epsilon_{j,t+k}$ for $k \geq 1$, because $p_{j,t-1}$ impacts p_{jt}, which in turn impacts the distribution from which $\epsilon_{j,t+1}$ is drawn, and so on. To avoid lookahead bias,[50] we follow a two-step procedure. First, we estimate α in a first differences regression:

$$\Delta p_{jt} = \alpha \Delta p_{j,t-1} + \Delta \gamma_t + \Delta \epsilon_{jt} \tag{D.46}$$

Then, we estimate γ_t in a specification that residualizes out $p_{j,t-1}$:

$$p_{jt} - \hat{\alpha} p_{j,t-1} = \gamma_t + \rho_j + \epsilon_{jt} \tag{D.47}$$

Finally, we estimate $\hat{\rho}_j = E[p_{jt} - \hat{\alpha} p_{j,t-1}] - E[\hat{\gamma}_t]$. Table D.8 presents parameter estimates for α and γ_t. The autoregressive coefficient $\hat{\alpha}$ is negative, indicating that the twin birth rate mean reverts. This makes sense. Suppose the local population has a distribution of latent genetic predisposition towards having twins. If more people with twin genetics give birth in year t, they are less likely to give birth in $t+1$, so the twin birth rate should fall.

[50] Indeed, the fixed effect estimate of α is lower than the estimate reported below, as expected given lookahead bias and a true negative coefficient as we estimate.

Table D.8: Twin autoregression estimates
(Source: Equifax Credit Risks Insight Servicing)

Dependent Variables:	Δ Pr(Twins)	Pr(twins) - $\alpha\times$ (Lag Pr(Twins))
Model:	(1)	(2)
Variables		
Lag Δ Pr(Twins)	-0.4962***	
	(0.0162)	
Constant	0.0016***	
	(0.0003)	
Year = 1998	0.0007*	0.0040***
	(0.0004)	(0.0004)
Year = 1999	0.0005	0.0061***
	(0.0005)	(0.0004)
Year = 2000	-0.0011**	0.0066***
	(0.0004)	(0.0004)
Year = 2001	-0.0004	0.0078***
	(0.0004)	(0.0004)
Year = 2002	-0.0004	0.0090***
	(0.0004)	(0.0004)
Year = 2003	-0.0007*	0.0099***
	(0.0004)	(0.0004)
Year = 2004	-0.0004	0.0110***
	(0.0004)	(0.0004)
Year = 2005	-0.0018***	0.0108***
	(0.0004)	(0.0004)
Year = 1997		0.0016***
		(0.0003)
Fixed-effects		
County		Yes
Sample	1997-2005	1996-2005
Fit statistics		
Observations	3,598	4,051
R^2	0.25096	0.76401
Within R^2		0.34422

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing and CDC natality data. Column (1) shows coefficient estimates for equation (D.46), and Column (2) shows estimates for equation (D.47).

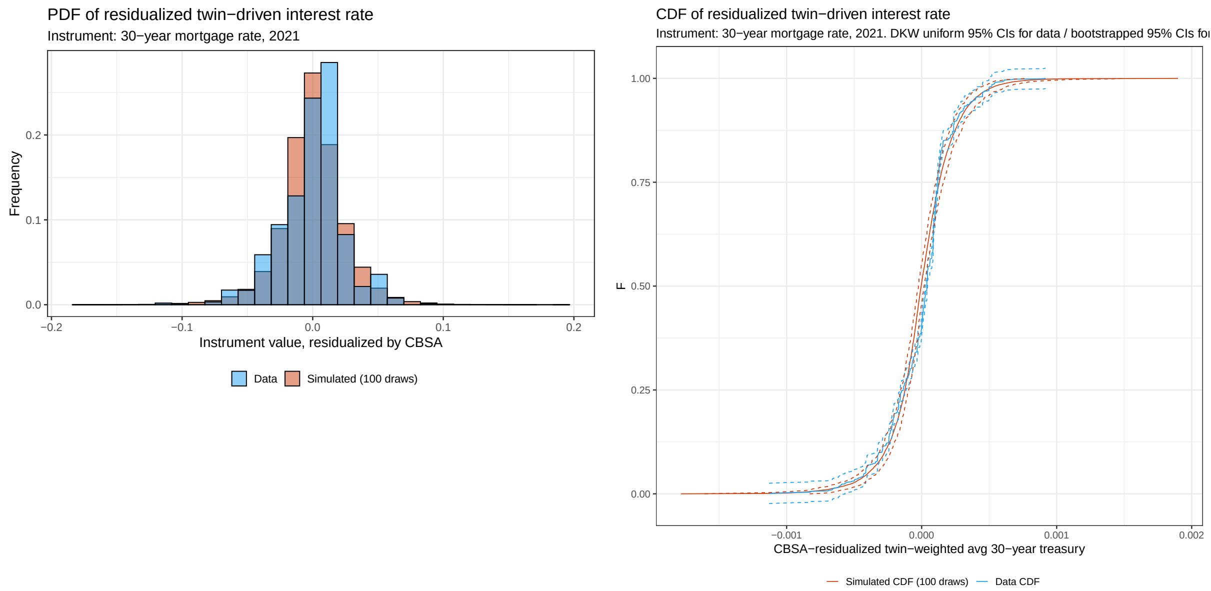
Forming the expected instrument. Using simulated draws from the estimated distribution, we form many counterfactual twin-driven average outstanding mortgage rates, $r_{jt}^{twins,k}$. The estimated distribution fits the observed distribution of predicted twin-driven mortgage rates well. Figure D.14 compares the pdf and cdf of the empirical and simulated distributions, both residualized at the CBSA level to capture the identifying variation used in estimation of equation (10). The two closely align.

Using $r_{jt}^{twins,k}$, we also form simulated nonlinear instruments $MVP_{jt}^{twins,k}$.

Estimation using only finite sample variation. For notational convenience, let $z_{jt} = \Delta MVP_{jt}^{twins}$ denote the twin instrument, and let $z_{jt}^{\perp}$ denote its value residualized on fixed effects and controls in equation (10).

We can write $z_{jt}^{\perp} = f_{jt}(\omega; \alpha, \gamma_t, \rho_t, m_{0t})$, where ω represents the vector of randomly drawn

Figure D.14: Simulated vs. actual distribution of $r_{j,2021}^{twin}$, county-specific, autocorrelated twin birth rate process.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)



Source: Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC Natality. The figure compares the PDF (left panel) and CDF (right panel) of the CBSA-residualized estimate of $r_{j,2021}^{twin}$ for the data (blue) and values of the instrument based on simulated draws (red) from the estimated process in equation (D.44).

shocks due to realized finite sample variation. The identifying assumption is:

$$(\omega \perp \varepsilon_{jt})|\rho_j, \alpha, m_{0t}, \gamma_t \tag{D.48}$$

where ε_{jt} is the error in equation (10). Importantly, ε_{jt} may correlate with ρ_j, m_{0t}, or γ_t.

Define $\mu_{jt} \equiv E_\omega\left[f_{jt}(\omega; \alpha, \gamma_t, \rho_t, m_{0t})\right]$. The expected cross-sectional covariance between the instrument and ε_{jt} is:

$$E\left[N^{-1}\sum_j z_{jt}^{\perp}\epsilon_{jt}\right] = N^{-1}\sum_j E\left[z_{jt}^{\perp}\epsilon_{jt}\right] \tag{D.49}$$

$$= N^{-1}\sum_j E\left[E\left[z_{jt}^{\perp}\epsilon_{jt}|\alpha, \gamma_t, \rho_t, m_{0t}\right]\right] \tag{D.50}$$

$$= N^{-1}\sum_j E\left[\mu_{jt}E\left[\varepsilon_{jt}|\alpha, \gamma_t, \rho_t, m_{0t}\right]\right] = E\left[N^{-1}\sum_j \mu_{jt}\varepsilon_{jt}\right] \tag{D.51}$$

where the first line follows from linearity of expectations, the second from the law of iterated expectations, the third from the identifying assumption in (D.48), and the final line from another application of the law of iterated expectations.

This implies that after projecting $z_{jt}^{\perp}$ on μ_{jt}, the residualized instrument will have no expected covariance with ε_{jt}, making the TSLS estimate of β consistent. This is achieved by including μ_{jt} as a TSLS control.

Columns (3) and (4) of Table D.9 do this by controlling for $K^{-1}\sum_k MVP_{jt}^{twins,k}$ across $K = 1,000$ simulations, with and without CBSA-by-demographic controls. The estimates of β are almost identical to the main text.

D.9.3 Additional instrument: Age distribution.

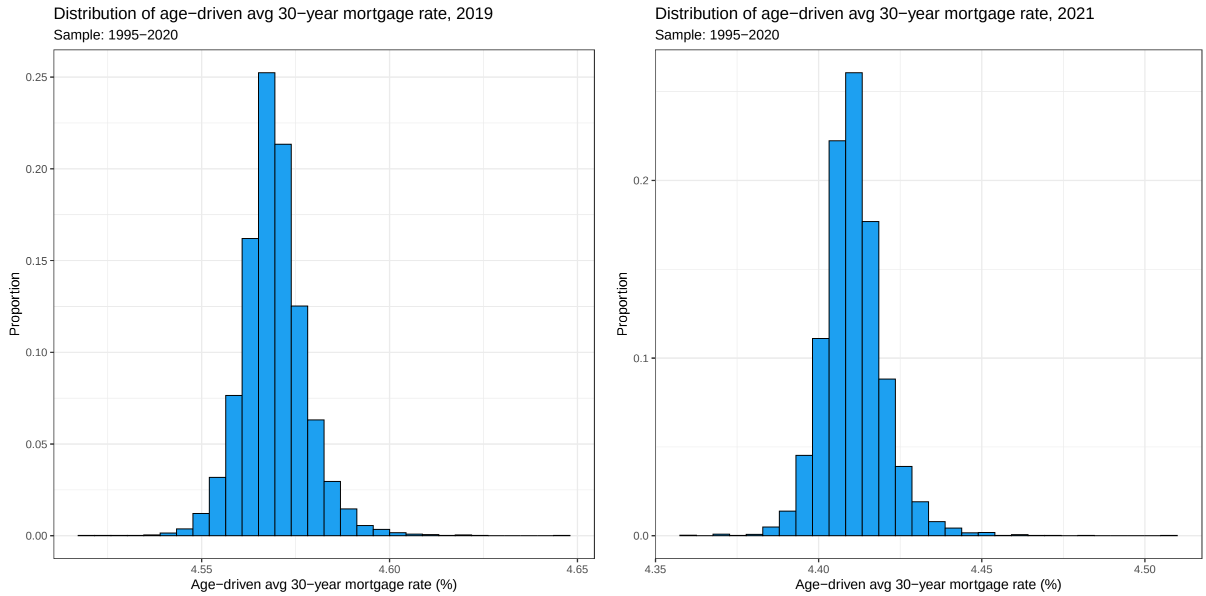
We also consider an additional instrument based on predicted moves based on the lagged age distribution. We first use data from 2006-2019 to estimate coefficients π_l in the following specification:

$$\Pr(Move)_{j,t+1} = \alpha_j + \omega_{c(j),t} + \sum_{l \in L} \pi_l \Pr(Age_{it} = l | i \in jt) + \varepsilon_{j,t+1} \tag{D.52}$$

We predict moves in τ as $v_{j\tau}^{age} \equiv \sum_{l \in L} \hat{\pi}_l \Pr\big(Age_{i,\tau-1} = l | i \in j\tau\big)$. Given $v_{j\tau}^{age}$, we form w_{jt}^{age}, r_{jt}^{age}, and MVP_{jt}^{age} as in the main text. The left and right panels of Figure D.15 show the distribution of r_{jt}^{age} for $t = 2019$ (left panel) and $t = 2021$ (right panel).

The instrument uses variation from changes in the age distribution. Recent changes in the age distribution will affect the outstanding distribution in 2021, which might directly relate to unobserved determinants of price growth from 2021-23 (to the extent that those unobserved factors are driven by the outstanding age distribution). We therefore directly control for a CBSA-specific quadratic in average zip code age as of 2021 in all specifications that use the age instrument. This ensures we only use variation from the age distribution to the extent it impacts mobility.

Figure D.15: Distribution of age distribution avg 30-year mortgage rate. (Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)



Source: Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC Natality. The panels plot the distribution of the lagged-age driven mortgage rates. The left panel plots the distribution for outstanding mortgages in 2019, and the right panel plots the distribution for outstanding mortgages in 2021.

Table D.9: Cross-sectional price effects, robustness (Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

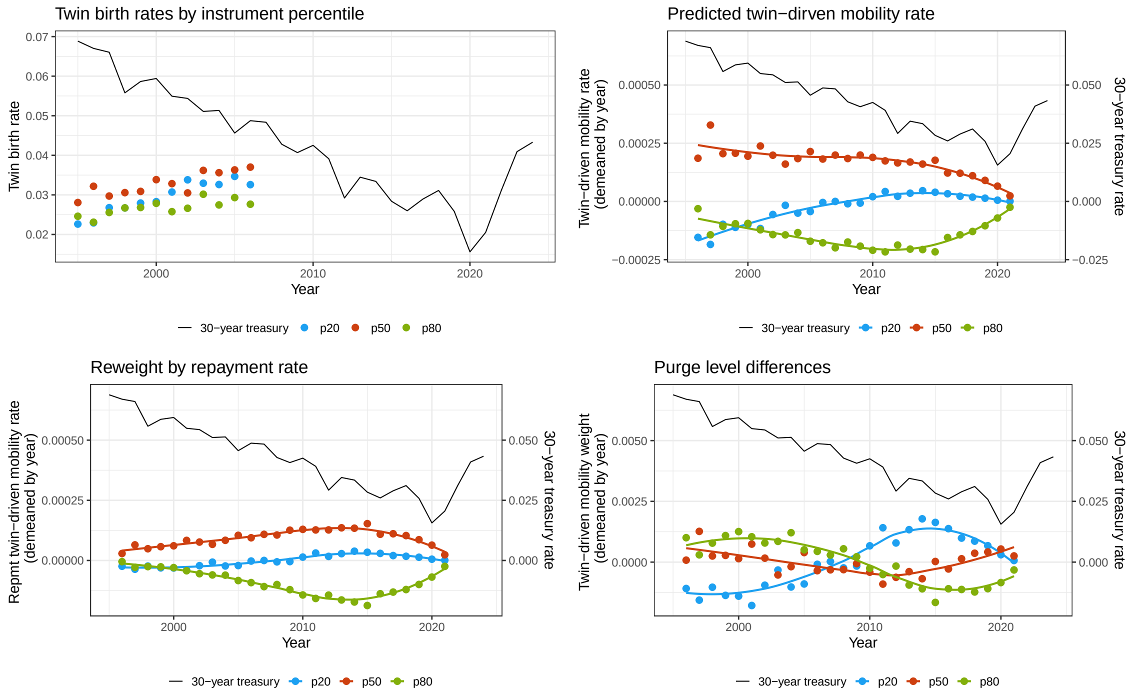
Dependent Variable:			%Δ HPI, 21-23			
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Δ MVP, 2021-23	1.987***	2.062**	1.692***	2.058***	1.431***	2.955***
	(0.7648)	(0.8446)	(0.6226)	(0.7765)	(0.3449)	(1.062)
Fixed-Effects						
CBSA	Yes	Yes	Yes	Yes	Yes	Yes
Rent/price x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demo x CBSA controls?	No	Yes	No	Yes	Yes	Yes
Expected IV controls?	No	No	Yes	Yes	No	No
Instrument?	Twin < 35	Twin < 35	Twin	Twin	Age dist	Age dist
Fit statistics						
Observations	5,044	5,047	5,044	5,047	6,697	6,697
F-test (1st stage), Δ MVP, 2021-23	21.260	20.926	20.298	17.390	69.252	12.911
Wald (1st stage), Δ MVP, 2021-23	7.6766	6.7726	8.0870	7.0329	28.570	6.2002

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing, ICE, McDash® and CDC natality data. Each column presents TSLS estimates of β in equation (10) using a different set of instruments as indicated. Columns (1) and (2) construct the twin-driven outstanding interest rate excluding twin births to mothers over 35 as described in Section D.9.1, with and without demographic controls. Columns (3) and (4) control for the expected value of the instrument given a county-specific process for the expected twin birth rate, with and without controls, as described in Section D.9.2. Columns (5) and (6) construct an instrument based on moves given the lagged age distribution, with and without controls, as described in Section D.9.3.

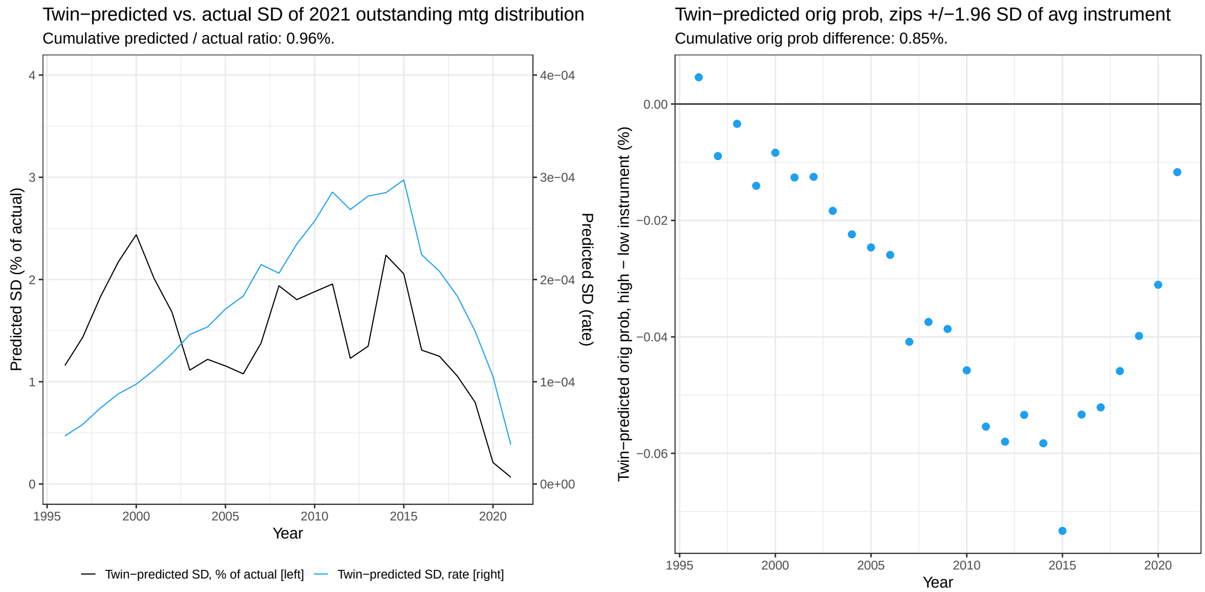
D.10 Additional figures.

Figure D.16: Visual construction of $\overline{r}_j^{twin}$.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)



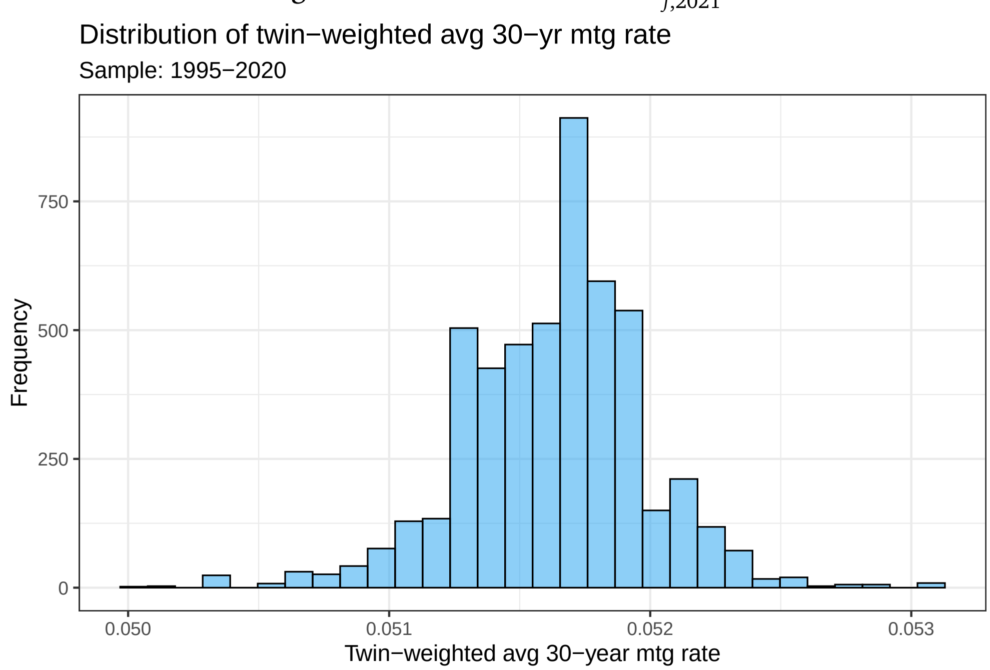
Source: Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC Natality. The figure plots steps for constructing $w_{j\tau,2021}^{twin}$ for zip codes at the 20th, 50th, and 80th percentiles of the $r_{j,2021}^{twin}$ distribution. The top-left panel shows raw twin birth rates for 1995-2005 along with the 30-year Treasury rate. The top-right panel shows $v_{j\tau}$, demeaned for each τ to show relative differences. The bottom-left panel shows $K(2021-\tau)\cdot v_{j\tau}$. The bottom-right panel shows $w_{j\tau,2021}^{twin}$. See main text for details.

Figure D.17: Variation in twin-predicted moves by year.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)



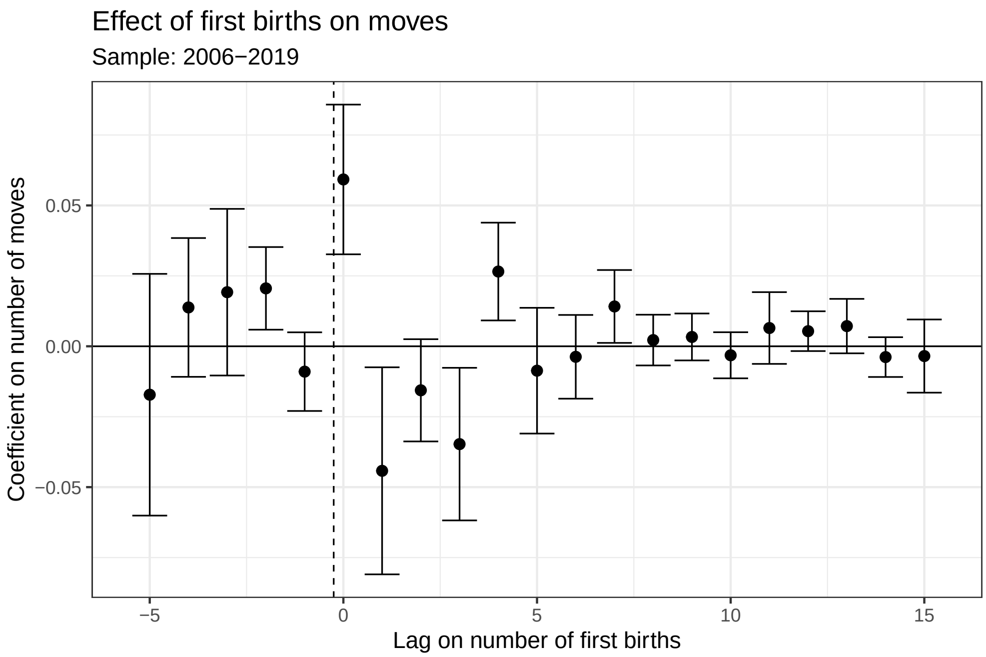
Source: Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC Natality. The left panel plots the cross-sectional standard deviation of the twin-predicted probability an outstanding mortgage in 2021 was originated in each previous year τ. The blue line, plotted on the right axis, shows the standard deviation of $w_{j\tau,2021}$ for each year τ. The black line, plotted on the left axis, takes the ratio between the standard deviation of the prediction and the standard deviation of the actual cross-section of origination shares across years, $w^{O}_{j\tau,2021}$. The right panel takes zip codes 1.96 standard deviations above and below the mean of $r^{twin}_{j,2021}$, calculates difference in $w_{j\tau,2021}$ for each year τ, and plots the difference, along with the sum of the absolute value of the difference across years. See main text for details.

Figure D.18: Distribution of $r^{twin}_{j,2021}$.



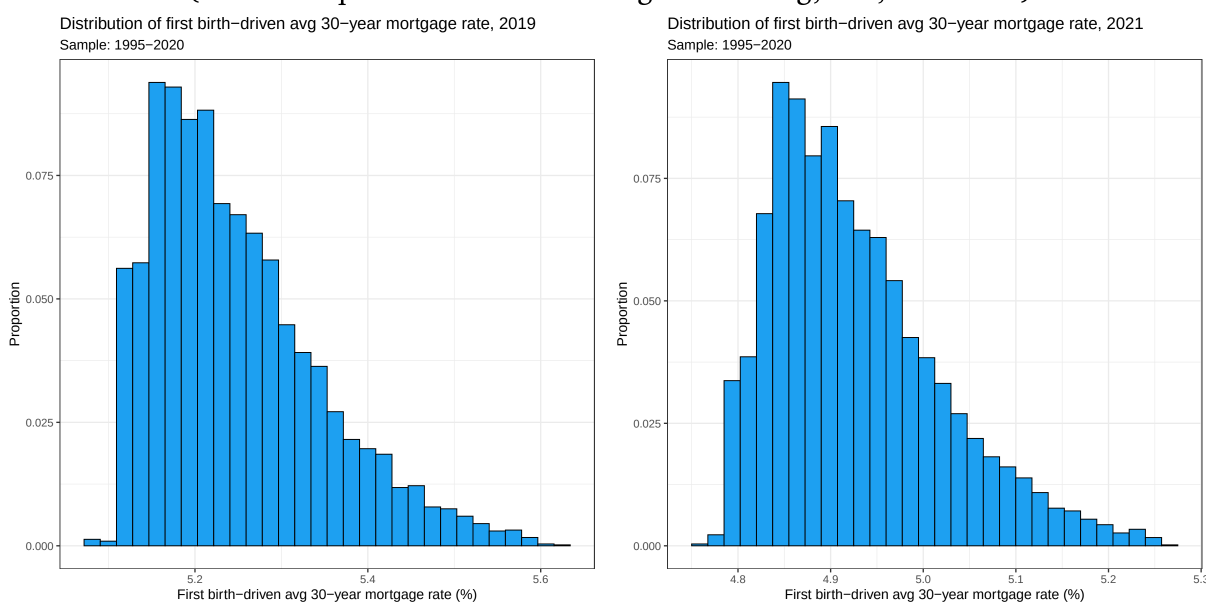
Source: Author's calculations using CDC natality data, Equifax Credit Risks Insight Servicing data, and ICE, McDash® data. The figure plots the pdf of $r^{twin}_{j,2021}$.

Figure D.19: Effect of first births on moves.



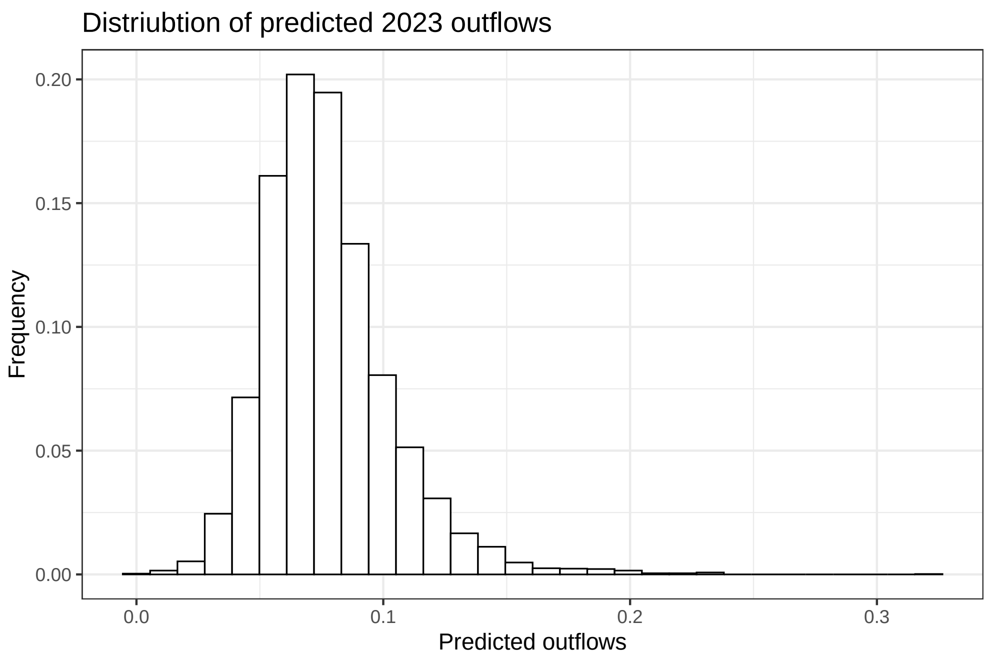
Source: Author's calculations using CDC natality data and Equifax Credit Risks Insight Servicing data. The figure plots fixed effects estimates of β_a in equation (14). The point estimates are multiplied by 10 because CRISM is a 10% random sample.

Figure D.20: Distribution of first birth-driven avg 30-year mortgage rate. (Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)



Source: Equifax Credit Risks Insight Servicing, ICE, McDash®, and CDC Natality. The panels plot the distribution of first birth driven mortgage rates. The left panel plots the distribution for outstanding mortgages in 2019, and the right panel plots the distribution for outstanding mortgages in 2021.

Figure D.21: Predicted outflows in 2022-23 based on 2019 age distribution. (Source: Equifax Credit Risks Insight Servicing)



Source: Equifax Credit Risks Insight Servicing. The figure plots the distribution of predicted outflows based on coefficient estimates in equation (17) and the 2019 age distribution.

D.11 Additional tables.

Table D.10: Local housing market summary statistics.
(Source: Equifax Credit Risks Insight Servicing)

Statistic	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
%Δ HPI, 21-23	6,701	0.248	0.087	0.190	0.241	0.300
MVP, 2019	6,701	−0.211	0.028	−0.229	−0.211	−0.192
MVP, 2021	6,701	0.029	0.019	0.019	0.031	0.041
Δ MPV, 2021	6,701	0.240	0.027	0.222	0.239	0.257
Pr(Move next year), 2022-23	6,701	0.047	0.021	0.033	0.044	0.057
Median family income, 2021 (USD)	6,701	91,696.710	29,255.060	69,990	86,672	108,627
Unit value, 2021 (USD)	6,701	257,874.000	118,090.600	167,600	233,000	326,600
Rent to base price ratio, 2021	6,701	0.067	0.022	0.052	0.064	0.078
Homeowner rate, 2021	6,701	0.686	0.148	0.594	0.704	0.797
Population density (pop / sq km)	6,701	942.819	1,523.833	133.275	466.735	1,236.178
Avg age (yrs), 2021	6,701	52.378	2.838	50.727	52.150	53.729
Frac white, 2021	6,701	0.784	0.195	0.708	0.847	0.925
Avg LTV ratio, 2021 (%)	6,701	82.963	383.114	73.810	78.719	82.758
Avg DTI ratio, 2021 (%)	6,701	33.161	2.463	31.528	33.165	34.742
Fixed-rate mtg share, 2021	6,701	0.974	0.017	0.966	0.977	0.986
Avg Risk Score 3.0, 2021	6,701	756.809	22.840	741.313	758.547	774.477

Source: Equifax Credit Risks Insight Servicing and ACS. The table shows summary statistics at the zip code level for the sample used to estimate coefficients in equation (10). House price growth is calculated from zip code-level FHFA all-transaction repeat sale price indices. MVP variables, the probability of moving between 2022-23, average age, average LTV ratio, average DTI ratio, the fixed-rate mortgage share, and the average Vantage Risk Score 3.0 are calculated from the 10% CRISM sample. Remaining variables are calculated from ACS. See Section B for details.

Table D.11: Demographics do not explain twin birth rate variation.
(Source: Equifax Credit Risks Insight Servicing)

Dependent Variables:	Pr(twins) (%), 2019			Δ Pr(twins) (%), 2020		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Pr(twins) (%), 2018	0.1005*		0.0950			
	(0.0603)		(0.0603)			
Δ Pr(twins) (%), 2019				-0.4071***		-0.4098***
				(0.0569)		(0.0569)
Log inc, 2019, sd		0.0329*	0.0266		0.0141	0.0003
		(0.0177)	(0.0166)		(0.0254)	(0.0232)
Log unit val, 2019, sd		0.0085	0.0075		-0.0478	-0.0486
		(0.0307)	(0.0309)		(0.0465)	(0.0408)
Rent / price, 2019, sd		0.0046	0.0029		-0.0115	-0.0168
		(0.0106)	(0.0109)		(0.0156)	(0.0134)
Homeowner rate, 2019, sd		-0.0038	-0.0016		-0.0014	0.0068
		(0.0092)	(0.0088)		(0.0112)	(0.0097)
Pop density, 2019, sd		-0.0113	-0.0109		0.0010	-0.0020
		(0.0161)	(0.0153)		(0.0158)	(0.0146)
1-Nonwhite share, 2019, sd		-0.0091	-0.0057		-0.0157	-0.0051
		(0.0120)	(0.0116)		(0.0161)	(0.0137)
Age, 2019, sd		-0.0033	-0.0027		-0.0049	-0.0036
		(0.0082)	(0.0079)		(0.0112)	(0.0100)
FRM share, 2019, sd		0.0030	0.0035		0.0043	0.0076
		(0.0092)	(0.0091)		(0.0112)	(0.0101)
Origination LTV ratio, 2019, sd		3.38×10^{-5}	1.64×10^{-5}		0.0002	0.0001
		(0.0001)	(0.0001)		(0.0003)	(0.0002)
Origination DTI rtio, 2019, sd		-0.0020	-0.0029		-0.0131	-0.0176*
		(0.0079)	(0.0080)		(0.0120)	(0.0103)
Equifax Risk Score 3.0, 2019, sd		-0.0215	-0.0224		0.0233	0.0107
		(0.0185)	(0.0182)		(0.0251)	(0.0209)
Fixed-effects						
CBSA	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	5,600	5,599	5,599	5,600	5,599	5,599
R^2	0.73199	0.73039	0.73365	0.67645	0.60578	0.67754
Within R^2	0.01376	0.00756	0.01959	0.18103	0.00210	0.18374

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing, CDC natality data, and ACS. The table shows estimated coefficients from a regression of the levels of and first differences in the twin birth rate on local demographics. The outcome in Columns (1)-(3) is the 2019 twin birth rate, and the outcome in Columns (4)-(6) is the 2019-2020 change in the twin birth rate.

Table D.12: Twin-predicted moves predict moves + outstanding rate in 2021. (Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

Dependent Variables:	Pr(Originate)	Avg rate on outstanding mtgs, 2021
Model:	(1)	(2)
Variables		
Twin-predicted Pr(Originate)	0.3321***	
	(0.0204)	
Twin-predicted rate on outstanding mtgs, 2021		1.640***
		(0.3700)
Fixed-effects		
CBSA	Yes	Yes
Fit statistics		
Observations	115,456	5,047
R^2	0.01937	0.29824
Within Adjusted R^2	0.01768	0.01294
F-test (projected)	2,074.7	63.847
Wald (joint nullity)	266.12	19.642

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing, CDC natality data, and ACS. Column (1) shows coefficients from a regression of origination shares ($w^O_{j\tau,2021}$) on twin-predicted origination shares ($w^{twin}_{j\tau,2021}$). Column (2) shows results of a regression of the average origination-timing driven outstanding 30-year fixed mortgage rate in 2021 ($r^O_{j,2021}$) on twin-driven mortgage rates ($r^{twin}_{j,2021}$).

Table D.13: Predicted vs. actual mortgage rate, first births IV. (Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

Dependent Variables:	Avg mtg rate, 2019	Avg mtg rate, 2021	Avg mtg rate at orig, 2019	Avg mtg rate at orig, 2021
Model:	(1)	(2)	(3)	(4)
Variables				
First birth-driven rate, 2019	0.8961***		0.8998***	
	(0.1236)		(0.0978)	
First birth-driven rate, 2021		1.073***		1.097***
		(0.1285)		(0.1073)
Fixed-Effects				
CBSA	Yes	Yes	Yes	Yes
Fit statistics				
Observations	5,339	5,339	5,339	5,339
R^2	0.33070	0.33483	0.29402	0.33611
Within R^2	0.05164	0.05841	0.06128	0.06867

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Equifax Credit Risks Insight Servicing, CDC natality data, and ACS. Columns (1) and (2) regress the 2019 and 2021 average outstanding mortgage rates in 2019 and 2021 on their twin-driven analogues, respectively. Columns (3) and (4) repeat the exercise, except replace as the outcome variable r^O_{jt}, the origination-timing driven mortgage rate.

Table D.14: Effect of rate lock on local equilibrium prices, with CBSA-by-demographic controls.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

	Panel (a). Endogenous origination timing.			Panel (b). Origination timing instruments.		
Dependent Variable:	%Δ HPI, 21-23			%Δ HPI, 21-23		
Model:	(1)	(2)	(3)	(1)	(2)	(3)
Variables						
Δ MVP, 2021-23	0.1853***	0.2348***	0.3738**	2.372***	2.156***	1.752**
	(0.0640)	(0.0746)	(0.1671)	(0.7347)	(0.5332)	(0.7127)
Fixed-Effects						
CBSA	Yes	Yes	Yes	Yes	Yes	Yes
Rent/price x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demo x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Origination qtr controls?	No	No	No	No	No	Yes
Instrument?		T-rate x LTV	T-rate	Twin	First birth	Lag T-rate
Fit statistics						
Observations	6,704	6,704	6,704	5,047	5,339	6,704
R^2	0.68234	0.68227	0.68106	0.47804	0.51857	0.61977
Within Adjusted R^2	0.00369	0.00341	-0.00037	-0.53575	-0.44243	-0.19929
F-test (1st stage), Δ MVP, 2021-23		12,396.9	422.52	21.371	51.884	29.151
Wald (1st stage), Δ MVP, 2021-23		4,968.8	82.929	7.4210	16.791	16.519

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Author's calculations using Equifax Credit Risks Insight Servicing.

Table D.15: Effect of rate lock on local outflows, with demographic controls.
(Source: Equifax Credit Risks Insight Servicing; ICE, McDash®)

	Panel (a). Endogenous origination timing.			Panel (b). Origination timing instruments.		
Dependent Variable:	Pr(move), 2022			Pr(move), 2022		
Model:	(1)	(2)	(3)	(1)	(2)	(3)
Variables						
Δ MVP, 2021-23	-0.0544***	-0.0600***	-0.3220***	-0.6775***	-0.4644***	-0.4870*
	(0.0203)	(0.0232)	(0.0564)	(0.2302)	(0.1178)	(0.2510)
Fixed-Effects						
CBSA	Yes	Yes	Yes	Yes	Yes	Yes
Rent/price x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demo x CBSA controls?	Yes	Yes	Yes	Yes	Yes	Yes
Origination qtr controls?	No	No	No	No	No	Yes
Instrument?		T-rate x LTV	T-rate	Twin	First birth	Lag T-rate
Fit statistics						
Observations	6,704	6,704	6,704	5,047	5,339	6,704
R^2	0.40343	0.40341	0.35915	0.12611	0.26669	0.34416
Within Adjusted R^2	0.00282	0.00279	-0.07120	-0.37845	-0.16239	-0.13817
F-test (1st stage), Δ MVP, 2021-23		12,396.9	422.52	21.371	51.884	29.151
Wald (1st stage), Δ MVP, 2021-23		4,968.8	82.929	7.4210	16.791	16.519

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Source: Author's calculations using Equifax Credit Risks Insight Servicing.

E Section 5 details.

E.1 Household expectations

Expectations. Households expect that aggregate state variables $\theta_t = (r_t, \ell_t, P_t)$ follow a correlated first-order Markov process:

$$E\left[\log \theta_{t+1} | \theta_t\right] = \log \theta_t + \boldsymbol{\mu} + \boldsymbol{\rho} \cdot r_t + \boldsymbol{\Sigma}(r_t) \cdot \boldsymbol{\omega}_{t+1} \tag{E.1}$$

where $\boldsymbol{\omega}_{t+1} \equiv (\omega^r_{t+1}, \omega^\ell_{t+1}, \omega^P_{t+1})' \overset{\text{iid}}{\sim} N(0, I)$. In this expression, $\boldsymbol{\mu} \equiv (\mu_r, \mu_\ell, \mu_P)'$ encodes average growth and $\boldsymbol{\rho} \equiv (\rho_r, \rho_\ell, \rho_P)'$ the correlation between growth and the short rate. The matrix $\boldsymbol{\Sigma}(r_t)$ is lower diagonal, and determines households' expectations for how innovations co-vary. It depends on r_t because the variance of innovations to the short rate depend on its current level given the Cox, Ingersoll, Ross parametrization.

Estimation. We estimate parameters for the Cox, Ingersoll, Ross process using monthly data on the 2-year Treasury yield from January 1987 to July 2024 using maximum likelihood. Our estimation routine adapts the Matlab code from Kladívko (2007).

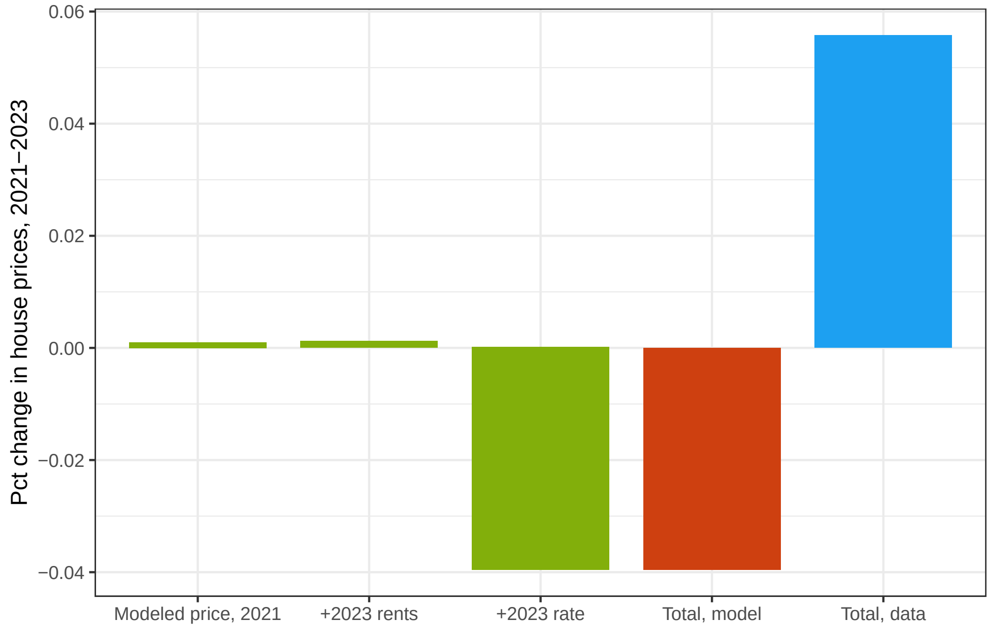
Nominal rental cost growth comes from the CPI-U's rent of primary residence series, and nominal house price growth comes from the FHFA's all-transactions annual house price index. We choose these indices over alternatives because they have the longest time series. We use data from 1976-2019, and deflate nominal values to 2019 dollars using the CPI-U.

To estimate the non-CIR coefficients in (E.1), we first estimate ω^r_{t+1} from the 2-year nominal Treasury series and estimated CIR coefficients. We then estimate $\mu_\ell, \mu_P, \rho_\ell, \rho_P$ using seemingly unrelated regressions, and form estimates of $\boldsymbol{\omega}_{t+1}$. To estimate the non-zero coefficients in $\Sigma(r)$, where the ijth coefficient is σ_{ij}, we use a lower triangular Cholesky decomposition of the estimated covariance matrix of $\boldsymbol{\omega}$. Estimates are presented in equation (E.2) below.

$$\begin{pmatrix} r^s_{t+1} \\ \log \frac{l_{t+1}}{l_t} \\ \log \frac{P_{t+1}}{P_t} \end{pmatrix} = \underbrace{\begin{pmatrix} 0.0041 \\ 0.015 \\ 0.066 \end{pmatrix}}_{\equiv \mu} + \underbrace{\begin{pmatrix} 0.90 \\ -0.15 \\ -0.79 \end{pmatrix}}_{\equiv \rho} r_t + \underbrace{\begin{pmatrix} 0.077\sqrt{r_t} & 0 & 0 \\ -0.0095 & 0.024 & 0 \\ 0.033 & 0.028 & 0.056 \end{pmatrix}}_{\equiv \Sigma(r_t)} \begin{pmatrix} \omega^r_{t+1} \\ \omega^\ell_{t+1} \\ \omega^P_{t+1} \end{pmatrix} \tag{E.2}$$

E.2 Additional figures.

Figure E.22: Model-predicted effects of 2021-23 tightening on equilibrium prices.



Source: Author's calculations. The green bars show how changing rents and the 2-year Treasury rate one at a time from 2021-2023 levels impacts price growth. The red bar shows the cumulative model-predicted change, while the blue bar plots the empirical change.

E.3 Additional tables.

Table E.16: Offline calibrated and estimated parameters

Symbol	Description	Value	Source
δ	Discount factor	0.90	Calibrated
(φ_1, φ_2)	Bequest motive	$(\hat{\alpha}, 0)$	
(σ_o, σ_ℓ)	Nesting parameter	$(0.7, 0.7)$	
γ	Coef. of relative risk aversion	4	
(A_{min}, A_{max})	Min / max age	$(22, 82)$	
τ_s	Transaction cost	0.03	Industry sources
τ_{ft}	Maintenance + insurance + net taxes	$0.015 \cdot P_t$	
Δ	Mortgage spread	0.02	Calibrated / SCF
ϕ	Origination LTV	0.2	
$\{y_a\}$	Income by age		SCF (2013-2019)
G_{kt}, N_{kt}	Household state distribution		SCF (2013-2022)
$\boldsymbol{\mu}, \boldsymbol{\rho}, \boldsymbol{\Sigma}(r)$	Agg. state transitions		See Section E.1

Source: Author's calculations. The table shows parameters calibrated and estimated offline. See Section 5.3 for details.

Table E.17: Minimum distance estimates

Symbol	Description	Parameter estimate	Identifying empirical moment	Moment value
h_o	Mean flow util of owner-occ housing	19.8	Pr(Moved to own in 4 yr), 2016	0.075
λ_0	Net util benefit of move	-0.79	Pr(Moved in 4 yr), 2016	0.125
α	Non-housing consumption util coef	309.4	$\uparrow$ Pr(Moved in 4 yr), 2016 event study	0.031
λ_s	Net util benefit of tenure change	23.5	$\uparrow$ Pr(Moved to rent 4 yr), 2016 event study	0.013
χ	Prob of considering move	0.37	Variance in Pr(Move), 2011-2019	0.015

Source: Author's calculations. The table shows parameters estimated with the minimum distance procedure described in Section 5.3. The first two columns describe parameters to be estimated. The third column present minimum distance estimates. The fourth and fifth column describe and present the empirical moment primarily used to identify each parameter.