Mortgage Choices and Housing Speculation*

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Abstract

We show why both speculators and those who lend to them prefer mortgages with backloaded payments, in particular interest-only mortgages (IOs), over traditional mortgages when there is speculation in the housing market and lenders cannot determine which borrowers are speculators. This insight is used to assess the extent to which speculation drove house prices during the recent housing boom. We find that IOs were used sparingly in cities where elastic housing supply precludes speculation from arising. In cities with inelastic supply, where speculation is possible, there was heavy use of IOs, but only in cities which exhibited boom-bust cycles. Peak IO usage predicts rapid appreciations that cannot be explained by standard correlates and this variable is more robustly correlated with rapid appreciations than other mortgage characteristics, including sub-primes, securitization and leverage. Where IOs were popular, their use does not appear to have been a response to houses becoming more expensive. Indeed their use anticipated future appreciation. Finally, consistent with the reason why lenders prefer IOs, these mortgages are more likely to be repaid earlier. Combined, this evidence suggests that speculation was an important factor in cities with boom-bust housing cycles after 2000.

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1 Introduction

The financial crisis of 2007 has refocused attention on the housing market and its apparent vulnerability to boom-bust cycles in which house prices appreciate dramatically over a relatively short time period and then collapse. As evident from the U.S. experience, such cycles have the potential to severely disrupt the functioning of the financial sector given its exposure to house price risk, which in turn can affect real economic activity. Consequently, economists and policymakers have sought to understand when and why boom-bust cycles can arise in the housing market. Are such price movements driven by fundamentals, or do they reflect speculation in which prices increasingly drift away from the expected value of the services the underlying assets can offer? Are there any indicators that can predict where such boom-bust episodes might occur if policymakers wish to intervene before they develop?

This paper examines whether data from the mortgage market can help to address these questions. Our focus on the mortgage market is motivated by theoretical work that suggests credit markets can play a key role in allowing for speculation, e.g. Allen and Gorton (1993) and Allen and Gale (2000). These papers show that if traders finance their asset purchases with borrowed funds, they are willing to pay more for a risky asset than its expected value. This is because they can default on their creditors should their gamble fail. Lenders would naturally be reluctant to finance such speculative activity that comes at their expense. But if lenders are unable to distinguish speculators from safe, profitable borrowers, they may end up financing such speculative purchases after all.

If credit markets indeed play a role in allowing for speculation as implied by these models, then if at least some of the boom-bust cycles in the housing market reflect speculation, credit market data might be relevant for predicting the occurrence of such episodes. For example, if borrowers temporarily bid up prices above their true value because they can default should their speculative purchases fail, boom-bust cycles might be more likely to emerge if and when borrowers are able to leverage themselves to a greater extent and thus default against a larger share of the assets they purchase. Indeed, previous work by Lamont and Stein (1999) has already argued that house prices tend to be more volatile in cities where a larger proportion of mortgages are highly leveraged.[1]

[1] More precisely, Lamont and Stein (1999) show that in cities with a large share of mortgages with a loan-to-value ratio of over 80%, house prices respond more to income shocks than in cities with a small share of such mortgages. Their work was not motivated by interest in speculation, but by work in Stein (1995) on down-payment constraints. In Stein's model, house prices reflect fundamentals. However, down-payment constraints impede the efficient allocation of houses and make the fundamentals more volatile, similarly to Kiyotaki and Moore (1997), which implies more volatile house prices. This hypothesis is distinct from, but not mutually exclusive of, the model that motivates our analysis.

While previous work has been concerned with leverage, here we consider other mortgage market characteristics that are motivated by the work of Barlevy (2009). That paper argues that if lenders cannot avoid lending to speculators, they would have an incentive to offer particular types of contracts to influence the behavior of these speculators. Here, we build on this insight by focusing specifically on mortgage contracts, and argue that when lenders know that some of those they lend to are buying overvalued assets to speculate, it will be possible to make both lenders and speculators better off using contracts with backloaded payments, i.e. contracts where the initial payments stipulated in the contract are low while later payments are onerously high. Lenders prefer these contracts because they preclude the borrowers from gambling at their expense for too long given that speculators will be forced to sell the asset once payments rise (or else refinance with another borrower if possible). At the same time, borrowers prefer these contracts because they can defer building up equity in what they know is a risky asset, leaving them with the option to default on all of the principal they borrowed should the prices collapse early. Thus, these contracts effectively get the borrower to commit to settling his debt earlier than he would under a traditional mortgage contract. We further show that the fact that backloaded contracts can make both parties better off is intimately related to the fact that the asset is the target of speculation; if it were not, then absent any other frictions, it would no longer be possible for backloaded contracts to make both the lender and the borrower better off.

These results lead us to look at whether markets with boom-bust cycles also relied on backloaded contracts. We find that the use of backloaded payments, specifically interest-only mortgages (IOs), was highly concentrated in cities that experienced boom-bust cycles.[2] In particular, we find that IOs were used only sparingly in areas with few restrictions on the supply of housing, i.e. cities where the supply response would prevent speculation from ever arising. But in cities where geographical and regulatory restrictions could have inhibited supply, so that speculation would have been possible under our theoretical setup, such contracts were used quite prevalently, but only in cities that experienced boom-bust cycles.

To convey the spirit of our findings, consider two cities: Phoenix, Arizona and Laredo, Texas. Laredo is a low income border city in a state with little regulation and vast open spaces on which new homes can be built. As such, we would expect that if house prices in Laredo were ever to rise above their fundamental value, the supply of housing would

[2] Contemporaneous work by Amromin, Huang, and Sialm (2010) also finds that the contracts we focus on were associated with high price appreciation in the boom phase, although they emphasize the complexity of these contracts rather than their backloaded nature. The fact that house price appreciation was concentrated in areas using backloaded contracts was also noted in congressional testimony by Sandra Thompson of the FDIC back in September of 2006 during a hearing regarding nontraditional mortgage products, although this testimony points to a statistical pattern without studying it rigorously.

quickly rise in response and drive prices back down. This would limit the growth of housing prices to the growth of fundamentals. By contrast, although Phoenix also has plenty of open space, it also ranks fairly high on the Wharton Residential Land Use Index complied by Gyourko, Saiz, and Summers (2008). These restrictions could have prevented home builders from responding quickly if house prices exceeded fundamentals, so that house prices could have grown faster than fundamentals. Figure 1 shows the Federal Housing Finance Agency (FHFA) house price index for these two cities, deflated by the Consumer Price Index. In Laredo, real house prices grew at a steady rate of roughly 2.5% per year between 2000 and 2008. In Phoenix, house price growth was indeed much higher, on average 9.5% per year between 2001 and 2006 and 36% in 2005 alone. House prices then declined sharply, reverting to their 2001 levels by 2010. The fact that cities with geographical and regulatory restrictions on housing supply have more volatile housing prices has been pointed out before; see, for example, Krugman (2005) and Glaeser, Gyourko, and Saiz (2008). But Panel A of Figure 1 also shows that home buyers in the two cities relied on different types of mortgage contracts to finance their purchases: IOs grew to over 40% of all mortgages for purchase in Phoenix as home prices climbed, but accounted for at most 2% of mortgages for purchase in any given quarter in Laredo.

The association between extensive use of interest-only contracts and rapid house price appreciation evident in these two cities remains when we look at a cross-section of over 200 cities, and is robust to controlling for various city-level characteristics. In particular, the peak share of IOs among new mortgages turns out to be a better indicator of rapid house price appreciation than other variables that have been shown to be useful in predicting unusually high house price appreciation, e.g. restrictions on housing supply and whether the city previously experienced boom-bust cycles in housing prices. The peak share of IOs also does better than other characteristics of mortgages we consider, including the share of mortgages with high leverage ratios and the share of mortgages privately securitized within a year of origination. Even more noteworthy, we find no relationship between house price appreciation at the city level and the peak share of subprime mortgages. This result, which may seem surprising at first, reflects the fact that subprime mortgages were more common in low income cities while boom-bust cycles were more common in middle income and wealthy cities. Panel B of Figure 1 is consistent with this pattern: Laredo, a lower-income city, had one of the largest shares of submprime mortgages during this period, peaking at over 27%. By contrast, the share of subprime mortgages in Phoenix peaked at 11% of all home purchases. Thus, subprime borrowers do not appear to have played an important role in accounting for the boom-bust pattern in housing prices observed at the metropolitan level. This does not deny a role for subprime lending in explaining the rise in home ownership over the period, as argued by Chambers, Garriga, and Schlagenhauf (2009), or the subsequent default wave, as argued by Corbae and Quintin (2010).

One concern about our findings is that backloaded mortgages could just reflect rapid house price appreciation if price appreciation forces borrowers to turn to these mortgages for reasons of affordability. We offer evidence against this interpretation. First, we find that in cities where backloaded mortgages were popular, the use of backloaded contracts took off before house prices. In other words, the use of these contracts anticipated the growth in housing prices rather than the other way around. This pattern can be seen in Panel A of Figure 1: The use of IOs in Phoenix began in early 2004, while house prices only took off in late 2004 and early 2005. We also find that the peak share of IOs continues to predict house price appreciation even after controlling for the level of the median house price at the peak and a measure of housing affordability. Finally, although other affordable mortgage products such as longer-term and hybrid mortgages were more common in cities with high price appreciation, the share of IOs turns out to be a much better predictor of which cities experience rapid house price appreciation.

Our findings offer several new insights for understanding boom-bust cycles in the housing market. First, they provide evidence that the boom-bust cycles in the housing market could have reflected speculation given that the contracts which are strictly preferred in the presence of speculation are more heavily used in cities that exhibited these cycles. In particular, the evidence we provide does not involve comparing the level of house prices to the true fundamental value of housing, but behavioral patterns that we show should be observed when assets are targets of speculation. Although our evidence does not definitively prove that boom-bust cycles were driven by speculation, it does point to a pattern that any theory of boom-bust cycles must explain, namely that in cities where such cycles occurred, home buyers took out backloaded contracts in anticipation of the appreciation of house prices and not in response to them. On the face of it, it might seem puzzling that lenders in cities with rapid price appreciation would be willing to expose themselves to more housing risk by letting borrowers avoid building equity in the homes they purchase. Although IOs did charge a premium, the premium was typically small, and lenders would collect less on these mortgages early on then on conventional mortgages with a slightly lower rate. Yet our theory can explain this pattern – it argues that lenders agreed to these contracts because they force borrowers to repay more quickly. Lastly, since our results suggest that backloaded contracts were used *before* house prices appreciated, policymakers might be able to use the type of contracts used to finance home purchases to anticipate boom-bust cycles. While IOs appeared to have been the relevant contract for forecasting house price appreciation during the recent episode, more generally our model suggests a preference for backloaded contracts, and in future episodes the contracts used to achieve backloading may differ in details from those of this episode.

The paper is organized as follows. In the next section, we discuss the theoretical environment that motivates us to look at backloaded mortgages as a predictor of boom-bust cycles. In Section 2, we describe the data we use. Section 3 documents the cross-sectional relationship between house price appreciation and mortgage characteristics. Section 4 shows that in cities where backloaded contracts were common, the use of backloaded mortgages anticipated rather than followed house price appreciation. Section 5 examines whether borrowers with backloaded contracts do indeed repay their debt more quickly, which in our model is the reason lenders prefer these contracts. Section 6 concludes.

2 Theory

Consider a model in which agents can buy assets that pay a stream of dividends $\{d_t\}_{t=1}^{\infty}$. Agents are assumed to be risk neutral and share a common discount rate β. For reference, define the fundamental value of the asset at date t as the expected value of its dividends the asset offers after date t:

$$f_t \equiv E_t\left[\sum_{s=t+1}^{\infty} \beta^{s-t} d_s\right] \tag{1}$$

We find it convenient to work with assets whose dividends are structured like a lottery ticket that pays off at a random date: The asset is either a "winner" and yields a perpetuity or else is worthless, and the perpetuity begins paying off only when the asset is revealed to be a winner or not, which occurs at random. Formally, let τ denote the date at which the true value of the ticket is revealed. If the asset yields a perpetuity, it begins to pay off at date $\tau + 1$. We assume there is some finite date $T + 1$ at which point the true value of the asset must be revealed. But in dates $t = 1, ..., T$, there is a constant probability q that the true value of the asset will be revealed if it has not yet been revealed. Formally,

$$\Pr(\tau = t | \tau \geq t) = \begin{cases} q & \text{if } t = 1, 2, ..., T \\ 1 & \text{if } t = T + 1 \end{cases}$$

Below, we assume that T is also the term of mortgages agents take out to buy the asset, although it is not essential that the two be equal. The dividends d_t are given as follows:

$$\begin{aligned} & d_t = 0 & & \text{for } t = 1, .., \tau \\ & d_t = \begin{cases} 0 & \text{with prob } 1-\epsilon \\ (1-\beta)\, D/\beta & \text{with prob } \epsilon \end{cases} & & \text{for } t = \tau + 1 \\ & d_t = d_{t-1} & & \text{for } t = \tau+2, \tau+3, ... \end{aligned} \tag{2}$$

Using (1), the fundamental value f_t for $t \geq \tau$ is either D or 0. For dates $t < \tau$, the fundamental f_t is increasing in t, reaching a value of ϵD at date T. Thus, even if asset prices reflect fundamentals, they can still appreciate over time. However, since this appreciation is due to discounting, the growth rate of f_t before date τ is bounded by the discount rate, i.e. $f_{t+1}/f_t \leq \beta^{-1}$. The key features we require of the asset are that its value is uncertain and that this uncertainty be resolved at a time that is itself uncertain. These features are what allow a speculative price dynamics to emerge, i.e. they allow the asset to trade at a price that increasingly deviates from the fundamental value f_t with time.

The dividend structure in (2) is arguably more appropriate for equity than housing, e.g. an equity share in a firm with a patent that may not pan out. However, this setup can also be viewed as a crude description of a housing market in which the value of housing services depends on some event about which agents are uncertain. For example, migration might make living in certain neighborhoods valuable due to agglomeration effects, but migration into the city is uncertain. Of course, for housing it is more natural to set d_t to a positive value at all dates. But we find it more convenient to use the structure above.

In what follows, we assume agents purchase assets using funds secured through mortgage contracts. As discussed in Allen and Gale (2000), agents who buy assets with risky dividends using borrowed funds might be willing to pay more for assets than the fundamental value f_t. This is because buyers can default if the payoff on the asset is low and so do not bear all of the losses if the asset fails to pay off. Of course, lenders would try to avoid financing such traders. But if lenders cannot distinguish speculators intent on buying risky assets from safe borrowers to whom it is profitable to lend, they might be willing to lend if they believe there are enough safe potential borrowers.[3] Thus, lenders who cannot tell when their borrowers are speculating create the potential for agents to trade assets for more than their

[3] One example of safe borrowers to whom it would be profitable to lend are cash-constrained individuals with guaranteed future income flows who wish to buy houses that are distinct from the houses that serve as vehicles for speculation. If these individuals value the particular houses they buy for more than their selling price, e.g. if they derive more housing services as owners than can be generated from renting the house, they would be willing to pay a positive amount for the right to borrow and buy the home, and would pay back their debt to retain these houses.

fundamental value f_t.

Whether assets trade above f_t or not depends crucially on relative supply and demand for the asset. In particular, speculation requires that there be some constraints on supply: If any potential buyer willing to pay f_t for a house could always purchase one at that price, the price could not exceed f_t without resulting in excess supply.[4] Thus, if the number of home buyers is small relative to the stock of housing, or if there are no impediments to new construction, the equilibrium price of the asset p_t will equal f_t at all dates t:

$$p_t = f_t \tag{3}$$

By contrast, if those who are willing to pay more than f_t for the asset can't always buy one at the price f_t, the asset will trade above its fundamental. This result hinges crucially on leverage: if buyers use their own funds to buy assets, they would never be willing to pay more than f_t for the asset. Under additional assumptions about the supply of assets and the number of potential buyers, one can show that the equilibrium asset price will correspond to a stochastically bursting "bubble" as in Blanchard and Watson (1982), namely

$$p_t = f_t + b_t \tag{4}$$

where $b_0 > 0$ and

$$b_t = \begin{cases} (1+g)\, b_{t-1} & \text{for } t = 1, ..., \tau - 1 \\ 0 & \text{for } t = \tau, \tau + 1, ... \end{cases} \tag{5}$$

for some growth rate $g > \beta^{-1} - 1$. That is, the price rises for as long as the true value of the asset remains uncertain, during which time it grows at a rate g that exceeds the growth rate of f_t. Such dynamics emerge when demand in any one period falls short of the total supply of the asset, but cumulative demand up to date T can exceed total supply. Intuitively, if price growth survives to date T, there will be enough demand for the asset from all those who wish to use it to earn speculative profits to drive the price above fundamentals. But if agents expect the asset might trade for its fundamental in the future, they would not sell it for its fundamental value at earlier dates: They can always get at least the fundamental value for an asset, and by waiting they might be able to sell it for more. This explains why $b_0 > 0$. At the same time, if demand in any one period is not enough to exceed supply, some people who own the asset will have to hold on to it into the next period. Since they risk having the asset value collapse while they wait, they need to be compensated for waiting. This is why the price of the asset must grow faster than the discount rate if the price does

[4] Relatedly, the existence of speculation requires constraints on short sales; otherwise, the supply of assets would be perfectly elastic. Since in practice houses are not sold short, the relevant supply constraints are mainly geographic and regulatory.

not collapse – to reward those who hold on to the asset with a capital gain if price continues to rise. In what follows, we assume that if there is speculation, prices evolve according to (5) without specifying the arrival rate of buyers that support this path.[5]

Our empirical work builds on the assumption that the expansion of credit after 2000 raised demand for housing. In cities with constraints on housing supply, this increase could have made speculative price dynamics possible, specifically if total demand could have conceivably exceeded supply at prices equal to fundamentals. But in cities without such constraints, these price dynamics could not have emerged even if demand was expected to be high. Our interest is in which mortgage contracts agents choose depending on whether or not speculative price dynamics emerge.

To analyze this question, we consider a variation on the Barlevy (2009) model that allows for mortgage contracts. We assume agents who buy the asset do so without a down payment and instead borrow the full amount p_t. Loans are assumed to be non-recourse, i.e. if an agent defaults, he only has to surrender the asset but not other sources of income.[6] Since lenders need to cover their expected losses from defaults, the interest rate r on loans would have to exceed the discount rate, i.e. $1+r > \beta^{-1}$. The only contracts we allow are mortgage contracts that stipulate debts be paid back in installments over a term T. Let m_t denote the amount the borrower is obligated under the contract to pay at date t to retain rights to the asset. A traditional fixed rate mortgage implies m_t is constant over time and given by

$$m_t = \frac{r\,(1+r)^T}{(1+r)^T - 1} L \tag{6}$$

A fixed-rate IO instead backloads payments.[7] In particular, the borrower does not have to pay back any principal for the first T_0 periods of the contract, where T_0 is specified in advance, so initial payments are low. But from date $T_0 + 1$ on, he must repay his loan according to a standard fixed-rate mortgage schedule with term $T - T_0$. This payment schedule can be

[5] The price might drop even without information on the value of dividends. For example, news that future demand for housing can be met when prices are equal to fundamentals after all, say because of tighter credit conditions that limit the flow of new buyers, would also the price to drop.

[6] For a discussion on recourse and non-recourse loans, see Ghent and Kudlyak (2009). They argue that even in states with recourse, lenders often find it unprofitable to go after the borrower's other sources of income.

[7] As can be seen in our summary statistics later on, most interest-only loans issued during the period we study stipulated a time-varying adjustable rate. We focus on fixed rate mortgages for simplicity. Since rates were unusually low by historical standards when most of these contracts originated, adjustable rates may have further encouraged buyers to sell the asset early before rates would have likely increased.

summarized as follows:

$$m_t^0 = \begin{cases} rL & \text{if } t = 1, .., T_0 \\ \\ \dfrac{r(1+r)^{T-T_0}}{(1+r)^{T-T_0}-1}L & \text{if } t = T_0+1, .., T \end{cases} \tag{7}$$

We allow for only these two contracts, and consider only the preferences of those who contract at date 0.

Let L_t denote the principal amount the agent still owes the lender by the end of date t if the loan remains current (i.e. is not in default). L_t is defined recursively as follows:

$$\begin{aligned} L_0 &= L = p_0 \\ L_{t+1} &= (1+r) L_t - m_t \end{aligned}$$

We assume the agent earns an exogenous income stream ω_t which he can use to meet the scheduled payments m_t in (6) i.e. $\omega_t \geq m_t$. In what follows, we ignore the utility that comes from the exogenous income ω_t. This is because the borrower receives this income would be earned regardless of what he does, so it has no effect on his choices. To capture the notion that the high payments stipulated under backloaded contracts were often onerous for borrowers, we assume that the high payment under the backloaded contract exceeds his income, i.e. $\omega_t < m_t^0$ for $t > T_0$. In principle, borrowers might be able to save part of their income before date T_0 to keep making payments for some time after T_0, although they would likely have to default eventually. For simplicity, we assume savings cannot be used to meet even the first payment, so default is immediate.

Borrowers could in principle refinance to a longer term mortgage with lower payments once the higher payments kick in. In what follows, we assume borrowers cannot refinance. If we allowed refinancing, lenders would be perfectly content if a backloaded contract induced a speculator to refinance with another lender. This is because lenders in our environment already prefer backloaded contracts that encourage borrowers to sell their assets and repay their loans. Inducing borrowers to refinance in order to repay their loan achieves the same purpose. The main difficulty with introducing refinancing is that lenders would naturally try to make inference about borrowers from the terms of their previous mortgage, and it is problematic to construct a model where speculators would be able to refinance despite the fact that they previously chose a contract that appeals more to speculators. But if backloaded contracts did encourage refinancing, that should only strengthen their appeal to lenders.

The mortgage is structured so that if the borrower ever fails to make his stipulated payments, i.e. defaults, the lender takes possession of the asset. The lender receives the dividend d_t in the period of default, and can then choose what to do with the asset. We allow for foreclosure costs by assuming the lender can only recoup a fraction $\theta \in [0, 1]$ of the revenue the asset generates, so he earns θd_t while he holds on to the asset and θp_t if he sells it.

To determine what type of mortgage the borrower and lender each prefer, we first need to solve for the borrower's optimal strategy under each contract. Once the true value of the asset is revealed, the optimal strategy is the same under either contract. If the asset is worthless, the borrower should immediately default on any remaining debt. If the asset does pay out a positive flow, then since D must be greater than $(1 + r) L$ for the borrower to have agreed to buy the asset in the first place, the borrower would strictly prefer to avoid default. Moreover, since $1 + r > \beta^{-1}$, the borrower will prefer to sell the asset and repay his debt immediately than to continue paying in installments.

We next turn to the optimal strategy before the true value of the asset is revealed. Let V_t denote the expected value from owning the asset at date t given the true value of the asset is still uncertain at date t and before knowing whether dividends will be revealed at date t, i.e. conditional on $\tau \geq t$. We can derive V_t by backwards induction. In particular, at $t = T + 1$, if the borrower still owns the asset, he must have paid off his debt, i.e. $L_T = 0$. Since dividends are revealed at this date with certainty but only start accruing at date $T+2$, we have $V_{T+1} = \epsilon D$.

Next, consider $t \leq T$. Under the traditional mortgage, if the agent still owns the asset, the fact that $\omega_t \geq m_t$ implies he always has three options: (1) he can sell the asset and repay his remaining debt obligation; (2) he can hold on to the asset and pay the required payment to retain the asset; (3) he can default and give up his right to the asset, including to the date-t dividend. The expected utility from selling the asset and discharging one's debt is $p_t + d_t - (1 + r) L_{t-1}$. The expected utility from keeping the loan current is $\beta V_{t+1} + d_t - m_t$. The expected utility from default is 0. Once the agent learns whether $\tau = t$ and the price p_t, he will choose optimally and secure the payoff $\max [p_t + d_t - (1 + r) L_{t-1}, \beta V_{t+1} + d_t - m_t, 0]$. Since the only uncertainty is whether $\tau = t$ or $\tau > t$, and since $d_t = 0$ when $\tau \geq t$, we have

$$V_t = q\epsilon (D - (1 + r) L_{t-1}) + (1 - q) \max [p_t - (1 + r) L_{t-1}, \beta V_{t+1} - m_t, 0] \tag{8}$$

Given the boundary condition $V_{T+1} = \epsilon D$, we can compute V_t recursively for any date t.

Under the interest-only contract, the only difference is that at date T_0+1, our assumption that his income is not enough to cover m_{T_0+1} requires the borrower to either sell the asset or default. The expected value from owning the asset at date $T_0 + 1$ conditional on $\tau \geq T_0 + 1$

is then

$$V_{T_0+1} = q\epsilon \left(D - (1+r) L_{T_0}\right) + (1-q) \max \left[p_t - (1+r) L_{T_0}, 0\right]$$

For all earlier periods $t = 1, .., T_0$. the value V_t will continue to satisfy equation (8), and so can be constructed recursively from V_{T_0+1}. Since expected utility at date 0 is equal to βV_1, determining which mortgage contract the borrower prefers requires us to compare V_t at $t = 1$ for the two mortgage contracts.

To determine which contract the lender prefers, let Π_t denote the revenue that the lender can expect to collect from date t on before dividends at date t are known and assuming the borrower has remained current. For $t = T + 1$, any outstanding loan would have already been paid off, and so

$$\Pi_{T+1} = 0$$

For $t = 1, ..., T$, the value Π_t satisfies the equation

$$\Pi_t = (1-q)\,\pi_t + q\epsilon\,(1+r)\,L_{t-1}$$

where π_t denotes the expected profits if the dividend is not revealed at date t. This depends on what the borrower chooses. If the borrower chooses to sell the asset and pay off his loan, $\pi_t = (1+r)\,L_{t-1}$. If the borrower chooses to keep current on his payments, $\pi_t = m_t + \beta\Pi_{t+1}$. If the borrower chooses to default, $\pi_t = \theta d_t + \max\left(\theta p_t, \beta FC_{t+1}\right)$, where FC_{t+1} denotes the expected value to the lender from owning the asset at date $t+1$ given $\tau \geq t+1$. To derive FC_{t+1}, note that for $t = T$, we have

$$FC_{T+1} = \theta\epsilon D$$

i.e. with probability ϵ the asset turns out to be worth D, and the seller recovers a fraction θ of this. For any $t = 1, ..., T$, this value satisfies the equation

$$FC_t = q\theta\epsilon D + (1-q)\max\left(\theta p_t, \beta FC_{t+1}\right)$$

Given the boundary conditions on Π_{T+1} and FC_{T+1}, we can recursively compute Π_t for all t. If the opportunity cost of funds for the lender is to earn an expected return of β on the asset, as would be the case for a competitive lending market, expected profits from lending to a speculator are $\beta\Pi_1 - L$.

The model does not yield a simple characterization as to when borrowers and lenders prefer certain contracts. But the model is trivial to solve numerically. One insight that emerges from solving the model for different parameter values is that speculators usually benefit less from holding on to an overvalued asset than the expected losses they inflict on lenders. That is, the expected gains to the borrower from additional price appreciation are smaller than the expected losses the lender would incur should the asset be revealed as worthless and the borrower defaults. This implication arises not just under plausible parameterizations, but is in fact a restriction that must hold in equilibrium in the model of Barlevy (2009). Intuitively, the original owners of the asset must have been willing to sell it for speculative behavior to be an equilibrium phenomenon. Since, by assumption, the original owners did not borrow to buy the asset, it follows that unleveraged agents would want to sell the asset below the equilibrium price. This in turn implies that the joint interests of borrower and lender are maximized if they sell the asset immediately, since collectively they have no debt obligations. Yet under the traditional mortgage contract, the borrower has an incentive to hold on to the asset for some time.

The above result implies there is scope for mutual gains between borrower and lender if they could agree to sell the asset earlier than would occur under the traditional mortgage. A properly designed interest-only contract can capture these gains: On the one hand, it forces the borrower to sell the asset by date $T_0 + 1$, so as long as $T_0 + 1$ is earlier than when the borrower would have chosen to sell the asset under the traditional contract the contract achieves a beneficial outcome. At the same time, it compensates the borrower for forcing him to sell the asset early by allowing him to avoid building equity in the asset and thus to default on a larger amount should the asset value collapse. Thus, the interest-only contract redistributes the gains from selling the asset early so that both parties are made better off.[8]

As an illustration, consider the following parameterization. Set the mortgage term $T = 30$ and $T_0 = 5$, in line with the modal interest-only period on mortgages during the period we study. We set $\beta = 0.97$, implying a discount rate of 3% per year. We set the real interest rate $r = 0.04$ to exceed the discount rate. Next, we set $\epsilon = 0.02$, which implies the ratio of the maximal payoff on the asset to its expected payoff is $1/\epsilon = 50$. The higher this ratio, the more appreciation is possible in equilibrium. We normalize $D = 50$ to set $\epsilon D = 1$. For q we chose 0.4, which implies the average duration of speculative price dynamics is 2.5 years, on par with the length of the period of rapid house appreciation in our sample. For now, we abstract from foreclosure costs and consider the case where $\theta = 1$. To allow for speculation, we set $b_0 = 0.15$, implying the asset will start out about 15% overvalued, and let the excess value term grow at a rate $g = 0.2$. The implied growth rate in the price of the asset p_t turns out to be pretty modest early on, no more than 5% in the first five years and no more than

[8] Note that there are other ways to compensate the borrower for selling the asset beyond allowing him to avoid building equity. For example, a borrower who repays his debt early may be charged a lower interest rate, as in Barlevy (2009). The adjustable rate option may have achieved the same purpose given the historically low rates by 2003 would have been expected to rise over time, and in this regard it is noteworthy that most interest-only contracts at this time specified adjustable rates.

9% in the first ten years. By comparison, the average growth rate of real housing prices in the cities we study was 4.6%. One can confirm that for these parameter values, the borrower and the lender both prefer the IO over the traditional mortgage, i.e. both V_t and Π_t are higher for the IO at date $t = 1$.

The fact that both parties specifically prefer the interest-only option with $T_0 = 5$ is sensitive to the parameters we choose. For example, suppose we increased q to 0.5. Since this raises the probability speculation comes to an end early, a long interest-only contract becomes less appealing for the lender, since it lets the borrower wait too long for the asset to sell. However, we should still be able to find a shorter interest-only term that both parties prefer. Indeed, when $q = .5$, both parties will prefer a contract with an interest-only period of $T_0 = 4$ to the traditional mortgage product. Similar results obtain when we vary the other parameters.

If both the borrower and lender prefer some interest-only contract to a traditional mortgage with the same interest rate, we would expect that in equilibrium speculators will not resort to traditional mortgages to buy assets. We confirm this in the Appendix, where we characterize the equilibrium given the borrower and the lender both prefer the IO for a fixed r. We show that lenders will offer both types of mortgages, and safe borrowers choose the traditional mortgage while speculators choose the IO. Of course, in practice the separation of types will be imperfect, and some non-speculators may have reasons to prefer a backloaded mortgage that our model doesn't capture. We further show that interest-only loans carry higher interest rates in equilibrium. For example, for our numerical example above, given $r = 0.04$, the equilibrium interest rate on traditional mortgages will be 0.036, i.e. 40 basis points lower. This is on par with the empirical interest rate spread for using the interest-only option.[9]

Next, we turn to the case where there is no speculation. When the asset was the target of speculation, we argued that agents were collectively better off selling it than holding on to it, so any contract that encourages selling the asset earlier represents a Pareto improvement. This argument breaks down when asset prices equal fundamentals: The price accurately reflects the value of holding on to the asset, so there is no gain from selling it early. This suggests interest-only contracts will no longer be mutually beneficial. The following proposition confirms this to be true when there are no foreclosure costs, i.e. when $\theta = 1$:

Proposition: Suppose $p_t = f_t$ for all dates t and $\theta = 1$. Then $V_t + \Pi_t = E[f_t | \tau \geq t]$ for

[9] For example, Lacour-Little and Yang (2008) cite a spread of 25 basis points from lender pricing sheets. Jack Guttentag also looks at wholesale prices on mortgages in 2006 and reports a somewhat larger spread ranging between 37.5 to 100 basis points. See http://www.mtgprofessor.com/A%20-%20Interest%20Only/how_much_more_does_interest-only_cost.htm.

any mortgage contract $\{m_t\}_{t=1}^T$. Hence, if a mortgage contract makes one party better off relative to some benchmark contract, it must make the other party worse off.

The equilibrium is thus essentially indeterminate when there is no speculation: at most one party will prefer the interest-only contract, and whether it gets its way in equilibrium depends on other features of the environment. The model thus has little to say on what type of contracts should be observed when there is no speculation. That said, there are forces we ignore that may favor one type of contract or the other in this situation. For example, some conditions may favor IOs even without speculation. One example is if borrowers are financially constrained early on in the life-cycle and were unable to meet the mortgage payment m_t under the traditional contract but could meet both the low and high payments m_t^0 under the IO. In this case, both parties would prefer the backloaded contract. Another force that may favor backloaded contracts even without speculation is the option of refinancing. As long as the asset is risky, lenders would prefer to palm off the speculator on other lenders. This is because even when the asset is priced at its fundamental value, the lender still risks default if the fundamentals decline. Borrowers would be willing to go along with this as long as they could refinance at no cost. However, if these contracts are primarily aimed at borrowers who are betting that the fundamentals of the asset will rise, lenders may be reluctant to refinance borrowers who previously chose these contracts.

Conversely, there are forces that make traditional contracts more attractive in the absence of speculation. In particular, suppose foreclosure were costly. Then both parties may prefer the traditional mortgage contract, since interest-only contracts can increase the probability of default relative to an IO. In particular, under our assumptions, an agent who chose to borrow and buy a risky asset with $p_t = f_t$ would keep making payments until the true value of the asset were announced, and then default only if the asset was worthless. By contrast, under the IO, he may default at date $T_0 + 1$. One can confirm that for our parameterization, if we set $b_0 = 0$, one can show that both parties prefer the traditional contract to the interest-only contract with $T_0 = 5$ for $\theta = 0.75$ or lower.[10] The borrower prefers to hold on to the asset until its value is revealed, and while the lender is happy to foreclose on the asset before the true value is revealed, the amount he can recover through foreclosure turns out to be low enough that he prefers the traditional contract.

To summarize, our theoretical setup suggests the following empirical patterns. First, cities with few constraints on the supply of housing should exhibit no evidence of speculation. In cities with constraints on supply, speculation could develop. Since under speculation prices

[10]For comparison, Campbell, Giglio, and Pathak (2009) estimate the average cost of foreclosure at 28% of the value of the property.

grows faster than fundamentals, cities where speculation emerges should exhibit higher price appreciation while the prices are growing, and a subsequently large price decline if and when prices drop. In these, we should see clear evidence of backloaded contracts. In cities where there is no rapid appreciation or price crashes, the model has little to say on the use of either type of contracting feature. In addition, since the model predicts backloaded contracts would have been used in anticipation of price appreciation due to speculation, we could observe these contracts in cities with speculation even before asset prices start to rise. Finally, since our model suggests lenders agree to extend backloaded mortgages only because they encourage borrowers to repay their debt more quickly, we should observe that IOs have higher prepayment rates. The remainder of the paper explores these predictions.

3 Data

We now describe the data we use to explore the implications of our model, although we defer some of the details to a separate data appendix. For house prices, we use the Federal Housing Finance Agency (FHFA) house price index, previously known as the OFHEO house price index. The FHFA house price index is compiled quarterly from home prices in mortgages purchased or securitized by Fannie Mae and Freddy Mac, agencies that are subject to FHFA oversight. The FHFA house price index has several advantages. First, it is a repeat-sales index based on the change in price for the same home over time. This makes it less vulnerable to changes in the composition of which houses get sold over time than indices based on the median transaction price. Second, it tracks a large number of cities over a long time period. However, the FHFA house price index also has some well-known shortcomings. For example, since it only includes mortgages purchased or securitized by Fannie Mae and Freddy Mac, it excludes homes that were financed with non-conforming mortgages such as jumbo or subprime loans. Such mortgages are used in computing the rival Case-Shiller index. However, the Case-Shiller index is only publicly available for 20 cities. We did confirm that for these 20 cities, the rate of price appreciation during the boom phase was similar under both indices. This is comforting, since our analysis mostly involves this phase. When we recompute our measure of house price appreciation for each city using the Case-Shiller index, the statistics were similar in magnitude to those using the FHFA index, and the correlation between the two measures is 0.98.[11]

[11] We also explored two other issues concerning the FHFA home price index. First, the index relies on both market transactions and appraised home values from refinancings. Since the FHFA also reports a purchase-only house price index for 25 cities based solely on transaction prices, we compared our price growth measures for those cities where both indices are reported and found they were almost perfectly correlated. Second, the FHFA index uses a simple average of house price appreciation rather than weighting by house value.

Recall that in our model, cities with speculative price dynamics exhibit faster real price growth until the price collapses. To get at this notion empirically, we first deflated the FHFA index by the consumer price index, and then identified the peak real price between 2000:Q1 and 2008:Q4 for each city. We then computed the maximum 4-quarter log real price growth between 2000:Q1 and the city-specific peak date. Thus, for each city we measure price appreciation during the boom phase using the fastest rate at which real house prices before they reach their peak. However, we also experimented with the average price appreciation between 2000:Q1 and the peak. As we discuss below, the share of IOs is highly correlated with this measure as well, but this measure also seems more correlated with other characteristics of the mortgage market. Our reason for preferring the maximum 4-quarter growth rate is that it emphasizes especially rapid house price growth concentrated over a short time period. That is, given two cities with the same average growth rate up to the peak, this measure ranks a city in which house prices grow slowly at first but then surge above a city that grows at a stable rate. As a result, the maximal 4-quarter price growth seems to better identify those cities that are most singled out for the boom-bust cycle they experienced. For example, the two cities with the highest maximal 4-quarter price growth are Las Vegas and Phoenix, respectively, yet they rank only 53rd and 57th among all cities in terms of their average price appreciation from 2000 to their peak.

For data on mortgages, we turned to the Lender Processing Services (LPS) Applied Analytics dataset, previously known as the McDash dataset. The data consists of information on mortgages collected from the servicers that process payments and monitor the status of mortgages on behalf of lenders. The LPS dataset includes data from 9 out of the 10 top mortgage servicers, and has fairly broad coverage of all mortgages, some 60% of the mortgage market by value.[12] However, the dataset is not meant to be a representative sample of mortgages. Indeed, it tends to underrepresent mortgages held by banks on their portfolios, since smaller and mid-size banks often service their own loans and do not report their data to LPS. The dataset also appears to undersample subprime mortgages, which again tend not to be serviced by those outfits that report to LPS. However, since our analysis uses variation across cities, this selection would only compromise our analysis if this selection varies systematically across cities. While we do not have data on the universe of mortgages to address how selection varies across cities, we did compare the foreclosure rates on mortgages in LPS to annual foreclosure rates across a large set of cities published by RealtyTrac in 2007-2009. The latter are based on public records such as default notices and court filings.

We therefore looked at the Conventional Mortgage House Price Index, which is essentially a value-weighted version of the FHFA index available for the same large set of cities. Again, we found that our price growth measures were nearly identical to those based on the FHFA index.

[12]This estimate of the coverage is reported in Foote, Gerardi, Goette, and Willen (2009).

The foreclosure rates we compute were consistent with those complied by RealtyTrac, and we found no evidence of systematic bias across cities. Since the model suggests speculators are both likely to take out backloaded mortgages and default if the price collapses, the absence of systematic bias in foreclosure rates across cities is reassuring.

Given that our model suggests speculation would tend to favor backloaded contracts, we need a measure of how pervasive such contracts are in each city. For this, we first need to take a stand on which mortgages should count as backloaded. There are several mortgage products that involve unambiguously backloaded payments. One such mortgage is the graduated payment mortgage, first introduced in the 1970s. As suggested by its name, this mortgage offered payments that gradually increased over the duration of the loan, often during the first five years. However, these mortgages were rarely used in the period we look at. Another contract with backloaded payments is the IO we discussed in the previous section, in which the borrower only pays interest for some specified period and only then repays both principal and interest. In contrast to the graduated payment mortgage, this product was used quite extensively, at least in certain cities. Finally, another popular but less widely used product is the option-ARM mortgage, in the borrower has the option each month to pay both principal and interest, pay only the interest portion, or pay less than the required interest and add to his total principal, at least up to some maximum amount set forth in the contract. Table 1 reports some characteristics for interest-only and option-ARM mortgages from the LPS dataset, as well as non-backloaded fixed-rate and adjustable rate mortgages for comparison.

Although both interest-only and option-ARM are essentially backloaded contracts, we chose to focus on IOs in our empirical work. We do this for two reasons. First, the LPS dataset only began to identify mortgages as interest-only or option-ARM mortgages from 2005 on. Mortgages that both originated and terminated before January 2005 were not classified. However, we can directly identify which of these mortgages were interest-only using the scheduled payment, since the scheduled payment would exactly equal the interest rate times the loan amount for IOs. Unfortunately, there is no analogous way to identify option-ARMs, since the scheduled payment can reflect any of the options available to the borrower. Thus, we trust the time series on IOs more than we do the series on option-ARMs.[13] Second, as suggested by Table 1, the two types of mortgages appear to have served

[13]In private correspondence, Paul Willen pointed out to us an additional data issue stemming from the fact that option-ARM mortgages before 2003 were largely held in portfolio because lenders were attracted to the fact that these mortgages were readjusted monthly, thus providing a better hedge against interest-rate movements than conventional ARM mortgages that adjusted more slowly and whose changes were capped. Since loans held in portfolio are underrepresented in the LPS, the data is likely to missrepresent the time series pattern for these mortgages. Willen's points are mirrored in press releases from Golden

somewhat different purposes. In particular, option-ARMs were associated with an extremely high rate of prepayment penalties. This is inconsistent with the notion that backloaded contracts were designed to induce early repayment of the loan.[14] By contrast, the fraction of IOs with prepayment penalties is only a little higher than the fraction of all mortgages with such penalties. Thus, IOs seem more similar to the type of backloaded contract that would be mutually preferred according to the model. However, as a robustness exercise we also considered using only option-ARM mortgages and both types of mortgages combined. The results were qualitatively similar.

To measure the extent to which IOs were used in each city, we first computed for each city the fraction of all first-lien mortgages for purchase (as opposed to refinancing) that involved an interest-only feature. We also considered the share of IOs weighted by loan size, but this ratio turned out to be very similar to the simple share. This is because even though the average loan size for IOs in Table 1 is considerably larger than the average loan size for all mortgages, this is driven by the fact that IOs were more common in more expensive cities. Within cities, the average loan size for interest-only was more closely related to traditional mortgages. To remain consistent with our approach of measuring house price appreciation using the maximal 4-quarter growth rate, we summarize the use of IOs in each city using the maximum share of these mortgages over the sample period. We constructed similar statistics for other mortgage characteristics, specifically the share of 30-year hybrid mortgages (2/28 and 3/27 mortgages with a short fixed-rate component of 2-3 years followed by an adjustable rate), the share of mortgages with a term of 30 years or more, the share of subprime mortgages, the share of mortgages that were privately securitized one year of origination, and the share of mortgages from non-occupant investors. In each case, we use the maximum share of such mortgages in each city over the sample period. An additional mortgage characteristic of interest we already mentioned is leverage. However, given the growing custom of using second "piggyback" loans to cover down payments, measuring the true extent of leverage for our sample period requires knowing the combined loan-to-value (CLTV) of *all* loans against a given property. Unfortunately, LPS does not match first and second liens taken against the same property, and even if it did, the second lien would not be reported if it was serviced by a servicer who did not report to LPS. In lieu of this, we turned to the LoanPerformance ABS database on non-prime privately securitized mortgages. This

West Financial Corp, one of the leading issuers of option-ARM mortgages prior to 2003, on their website, www.goldenwestworld.com. See, in particular, the note "History of the Option ARM" posted on the site.

[14]Prepayment penalties come in two varieties; hard penalties, which penalize any early repayment, and soft penalties, which waive the penalty if the house is sold. Anecdotal evidence suggests that penalties on option-ARMs were increasingly shifted towards the soft variety, i.e. lenders were increasingly allowing borrowers to sell the asset without penalty. But soft prepayment penalties are still puzzling for our model, since one would expect lenders to want speculators to refinance.

data is reported by trustees of privately securitized mortgage pools rather than by servicers. Since CLTV data is highly relevant for investors, such data is reported for all mortgages, even when the second lien is not directly in the dataset. Thus, we have some data on leverage for certain types of mortgages, but for a different set of mortgages than for our other mortgage variables. Following Lamont and Stein (1999), we consider the share of mortgages with a CLTV exceeding 80%. Since the subprime mortgage market collapsed soon after the crisis began, the time series does not span the same period as in the LPS dataset. We therefore chose to use the average share of mortgages with CLTV above 80% instead of the maximum share of such mortgages. Table 2 reports descriptive statistics for how the various mortgage variables we use vary across cities in our sample.

Lastly, we compiled various data on factors that should presumably affect house prices and have been used in previous work which has attempted to account for variation in house price appreciation, e.g. Case and Shiller (2003), Himmelberg, Mayer, and Sinai (2005), Glaeser, Gyourko, and Saiz (2008). Our list of variables includes real per capita income, unemployment, and population, which ought to affect demand for housing; the share of land in each city that is undevelopable because of bodies of water or land terrain that is too steep, which we take from Saiz (2010), and the Wharton Residential Land Use Index that is reported in Gyourko, Saiz, and Summers (2008), variables which ought to affect supply of housing; and property tax rates, which affect both supply and demand.

4 Cross-Sectional Evidence

We begin our analysis by looking at the evidence on the relationship between the use of backloaded contracts, specifically IOs, and rapid house price appreciation across cities. Recall that our approach presumes speculative behavior could have emerged in the early 2000s, but only in cities where housing supply was somehow constrained, and we want to explore whether participants in the mortgage market respond differently in cities where speculation could emerge and those where it cannot.

To distinguish between cities that are and are not vulnerable to speculation, we ranked each city by the share of undevelopable land and by the value of their land use index. We view cities those which rank in the bottom half of both measures as unlikely to experience speculation, and those which rank in the top half of both measures as cities where speculation could have occurred in principle. Figure 2 plots house price appreciation against the maximal share of IOs for both groups. Not surprisingly, cities with few restrictions on supply exhibit low rates of house price appreciation. This was essentially already demonstrated in Glaeser, Gyourko, and Saiz (2008). However, Figure 2 also shows that these cities tended to forgo IOs. For cities where there are some restrictions on supply, there is wide variation in both house price appreciation and the use of IOs. That is, not all cities that were prone to speculation by our classification exhibited a boom-bust cycle. However, house price appreciation and the use of interest-only contracts does appear to be strongly correlated for these cities. This pattern anticipates one of our findings below, namely that the share of IOs is a better predictor of house price appreciation than data on supply constraints. This because of absence of geographic and regulatory constraints tells us that these cities will exhibit lower rates of house price appreciation, but the presence of constraints does not guarantee that a city will exhibit high rates of house price appreciation. By contrast, knowing that a city tended to rely on IOs quite heavily is a good predictor of whether that city experienced high price appreciation. Consequently, data on the share of these mortgages renders information on supply constraints redundant in predicting house price appreciation.

Figure 2 is meant to be illustrative. A more rigorous analysis should draw on data for all cities, including those that do not rank in either the top of bottom half of both supply measures, as well as control for other observable city characteristics. This analysis is reported in Table 3. The first column shows that the positive relationship between house price appreciation and interest-only contracts survives when we expand our sample to all cities for which we have complete data for all our variables. The coefficient on the share of IOs is statistically significant at the 1% level. To help interpret the coefficient 0.416, note that the difference in the share of IOs between the city with smallest such share in our data and the city with the largest such share is equal to $0.609 - 0.017 = 0.592$. Multiplying this by 0.416 implies that the largest log price appreciation we observe in the data should be 0.246 higher than the smallest log price appreciation we observe, which implies that the maximum 4-quarter growth rate in the city with the largest share of backloaded mortgages will exceed the growth rate in the city with smallest share by $\exp(0.246) - 1 = 27.9\%$. This is comparable to the difference in peak growth rates between Phoenix (36%) and Laredo (7.8%) in Figure 1.

Of course, some of the variation in house price appreciation across cities might be due to differences in other factors that help determine the value of housing services in different cities. The second column in Table 3 includes only these factors, both in levels and in annualized changes from the beginning of our sample and the peak date in each city. The change in population growth, unemployment, and property tax rates all enter significantly with the expected signs, as do the two variables affecting the supply of housing. Interestingly, the R^2 for these variables combined is not much larger than for the share of IOs. In the third column of Table 3, we use these variables as controls when looking at the relationship between house price appreciation and the share of IOs. The coefficient on the share of IOs is smaller than in the first column, although we cannot reject that the two coefficients are equal at the 5% level. More importantly, the coefficient remains tightly estimated and significantly different from zero. Hence, the share of IOs is significantly related to the residual variation in house price appreciation across cities that cannot be explained by the set of typical covariates used to predict house price appreciation. As anticipated earlier, accounting for the share of IOs renders the two variables that capture constraints on housing supply statistically insignificant; given information on which cities rely on IOs, these variables provide little additional information to help predict which cities experience house price booms. In the last column, we add state fixed effects so that our identification relies on variation from cities in the same state. The coefficient falls to 0.225, but remains highly significant.

Table 3 confirms that home buyers in cities with unusually high price appreciation tended to rely more on IOs. However, this correlation may arise for reasons that have nothing to do with speculation. For example, cities with faster house price appreciation may have faster income growth, and individuals who expect their income to grow may prefer backloaded mortgages for liquidity reasons even when assets are priced at their fundamental value. The fact that the correlation survives even when we control for growth in per capita income across cities should discount this particular explanation.[15] But an even more mundane explanation for the correlation is that as houses become expensive, borrowers may be forced to resort to mortgage products that remain affordable. IOs have the advantage that they offer very low payments during the interest-only period, and so they offer at least temporary affordability. By this view, the use of IOs simply mirrors the rapid appreciation of housing prices for whatever reason and need not reflect a strategic response to the presence of speculation. One way to check this is to include the level of housing prices as a control variable to see if it drives out the correlation between house price appreciation and the use of IOs. More precisely, for each city we first took the median price of single family homes reported by the National Association of Realtors at a common date, 2000:Q1. We then used the rate of real price appreciation in the FHFA to compute the implied price level in each city at its peak.

[15] A related hypothesis is that income is more volatile in cities with faster house price appreciation, and backloaded contracts allow borrowers to avoid default if they suffer a negative income shock early on. We tried to control for this in two ways. First, we added the variance of annual growth in real log per-capita income between 1969 and 2000 as an additional variable. The coefficient on this variable was positive but not statistically significant, and including it had virtually no effect on the coefficient on IOs or its standard error. However, this variable only captures the volatility of aggregate income. We therefore also tried to control for employment shares by industry for 8 categories (agriculture and mining, construction, manufacturing, transportation and utilities, trade, finance and real-estate, services, and government) since income volatility varies by industry. Even after adding these variables, the share of IOs remained significant at the 1% level.

The first two columns in Table 4 show the effect of adding the log of the price level at the peak.[16] The other control variables in the regression, which are not reported, correspond to the set of explanatory variables in Table 3. When we include the log peak price by itself, this variable has a positive and statistically significant effect, confirming that places with high appreciation ended up as relatively expensive cities compared with price levels in other cities. However, when we add the share of IOs, the coefficient on log peak price becomes statistically insignificant, while the coefficient on the share of IOs remains highly significant, lower than in Table 3 but the difference is not statistically significant. We also considered the ratio of the peak price to per-capita income in the year of the peak as an alternative affordability measure. These are reported in the third and fourth columns of Table 4. The results are the same. Finally, we also examined whether cities that experienced previously high rates of house appreciation might have been more willing to embrace IOs because they were more attuned to issues of affordability. In particular, we focused on the period between 1985 and 1989 when once again there were simultaneous boom-bust cycle in real housing prices in many U.S. cities.[17] Again, we find that on its own, higher price appreciation in that previous episode is significantly correlated with price appreciation in the more recent period. But this correlation turns insignificant (and with the wrong sign) once we control for the share of IOs, which remains highly statistically significant. While it is true that cities with rapid house price appreciation from 2000 on tended to be places that were ultimately more expensive and which experienced rapid house price appreciation previously, these features are not as good at predicting which particular cities experience rapid price appreciation from 2000-2007 as information on whether borrowers in that city tended to use IOs. For example, some expensive cities like New York and Newark had only modest house price appreciation from 2000 even as relatively inexpensive cities like Boise, Idaho had unusually high house price appreciation. But the latter city tellingly had a higher share of IOs. Essentially, there are too many examples of relatively inexpensive cities with both high price appreciation and shares on IOs and relatively expensive cities with little price appreciation or IOs that our findings can be attributed to affordability.

Table 4 shows that the share of IOs is related to housing prices only through contemporaneous house price appreciation rather than the level of house prices or previous price appreciation. This suggests that there is a connection between the rate of house price appreciation and mortgage choice. But since our discussion so far has focused only on IOs, it is not obvious that backloadedness is indeed the most prominent mortgage characteristic

[16] Using the peak price instead of log peak price yielded similar results.

[17] We also examined house price appreciation between 1985 and 1999 to capture whether cities had recently become expensive. This rate did worse in predicting price appreciation after 2000, and also had no affect the share of IOs.

associated with more rapid house price appreciation. To address this question, we consider several alternative mortgage characteristics that were already summarized in Table 2. The first two mortgage characteristics we consider, the share of hybrid mortgages and the share of mortgages with a term of at least 30 years, are meant to capture alternative affordability products. The next three correspond to various theories that have been proposed for the boom-bust cycle in the housing market: the share of subprime borrowers, the share of mortgages that were privately securitized soon after origination, and the share of highly leveraged mortgages (with a CLTV of at least 80%). Lastly, we also consider the share of mortgages taken out by investors, i.e. individuals who do not plan to occupy the property they are borrowing against. Recent work by Robinson and Todd (2010) suggests that the share of investors in the mortgage pool played a significant role in explaining the rise of foreclosures when house prices began to decline, and possibly the rate of house price appreciation for some states in the boom phase. It is also noteworthy from Table 1 that investors were over-represented among backloaded mortgages, so we want to explore whether our backloaded mortgage measure is really a proxy for the presence of more investors in cities with rapidly rising house prices.

The results for including these mortgage variables are reported in Table 5. We examine the effect of adding each of these alternatives by itself, as well as combining all in a single regression. As evident from the first row of the table, including any one of these variables by itself has no significant impact on the coefficient on the share of IOs. However, when we combine all of these measures, our measured coefficient falls, although it remains statistically significant at the 1% level. Moreover, since the standard error on this variable doubles when we combine all of these alternative measures, we cannot reject that the coefficient is the same as in our benchmark specification. The two affordability measures, the share of hybrids and the share of long term mortgages, are not significant at the 5%, although both are highly significant and positive when we omit the share of IOs. The share of subprime mortgages is statistically significant when it is the only variable we use in addition to the share of IOs. However, the sign of the coefficient is the opposite of what we might expect: cities where there were more subprime borrowers had lower house price appreciation. Moreover, the significance is not robust to including other mortgage characteristics, and the share comes in insignificant when we only use the share of subprime mortgages and drop the share of IOs. As noted above, the negative correlation in column (4) most likely reflects the prevalence of subprime mortgages in lower income cities, while house price appreciation was mostly concentrated in medium and high income cities. Thus, the expansion of the subprime market does not seem to be relevant for explaining why some cities experienced large boom-bust cycles in housing prices while others did not. However, this does not mean that the expansion of the subprime market was unimportant. It may have played an important role in the dramatic rise in foreclosures when house prices declined, and these may be as important if not a more important concern for policymakers as the boom-bust cycle in housing prices.

Turning to the share of mortgages that were privately securitized shortly after origination, we again find it is not statistically significant for explaining house price appreciation once we control for the share of IOs. However, on its own this variable is significant and positively to house price appreciation. The average share of mortgages with a CLTV of over 80% is statistically significant, but again comes with a surprising sign: cities in which where borrowers were more leveraged tended to experience less house price appreciation during the boom phase. One explanation for this is that in cities where there was house price speculation, lenders tried to protect themselves by insisting that borrowers put up larger equity stakes. However, this correlation is not robust, and when we combine all mortgage characteristics together, this variable is no longer statistically significant. Finally, the share of mortgages taken out by investors is significantly correlated with price appreciation, and this correlation is robust to adding other variables. Interestingly, the share of investor mortgages is only weakly correlated with the share of IOs. Thus, the share of investors appears to be a distinct factor that accounts for rapid house price appreciation than the one captured by the share of IOs.

As noted earlier, we also considered the average house price appreciation between 2000:Q1 and the peak in each city. Most of the results were qualitatively similar for this measure: The share of IOs is statistically significant at the 1% level in predicting this measure of house price appreciation, and including the share of IOs as an explanatory variable renders insignificant the share of undevelopeable area in a city, the Wharton regulatory index, the level of house prices, and the rate of house price appreciation between 1985 and 1989. However, adding other mortgage characteristics produced somewhat different results for this price measure than are reported in Table 5. In particular, the share of mortgages that were privately securitized is statistically significant at the 5% level both when we add it by itself as well as when we include all of our mortgage variables. Moreover, when we include all of our alternative mortgage characteristics together in the same regression, the share of IOs only enters significant at the 5% level rather than at the 1% level, and its coefficient fall by half. Thus, we continue to find a strong correlation between the share of IOs and house price appreciation for this measure, but there appears to be some other factor, possibly associated with private securitization, that also varies with average house price appreciation. This factor also appears to be correlated with the share of IOs, since including all of the factors appears to mitigate the importance of IOs even if it doesn't eliminate it.

So far, our results show that the use of IOs appears to be strongly associated with rapid house price appreciation before house prices peak. However, the model would suggest that the use of these mortgages would also be associated with rapid declines in house prices if and when house prices collapse. To investigate this feature, we constructed measures of house price decline following the peak in an analogous way to the way we constructed measures of price appreciation before the peak. In particular, for each city we measure the decline as the largest 4-quarter decline between the peak and the end of our sample, i.e. 2008:Q4. In 31 cities, the highest price recorded occurred in the last quarter in the sample, 2008:Q4, so there were no period after the peak to analyze. In another 12 cities, the peak occurred sometime in 2008, so the period of decline was not long enough to compute a 4-quarter growth rate. Among the remaining cities in which the peak price occurred before 2008, the largest 4-quarter price declined occurred in the same period in 85% of the cities, between 2007:Q3 and 2008:Q3, even though these cities peaked at different dates, some as early as 2003. Nearly all registered their sharpest decline starting in 2007. In what follows, we adopt of the convention of using the negative of the price change for losses, so bigger decline corresponds to a larger number.

Given that many of the cities with rapid house price appreciation also experienced significant subsequent price declines, it is not surprising that we find a similarly strong correlation between the share of IOs and the decline in house prices following the peak to the one we found for house price appreciation. This correlation remains highly significant after controlling for the level of home prices at the peak and the rate of house price appreciation between 1985 and 1989, as in Table 4. As with house price appreciation, the share of IOs renders variables like geographical constraints and previous house price appreciation redundant for forecasting large price declines. However, as was the case when we used our alternative measure of average price appreciation before the peak, there appears to be an additional factor that can predict which cities have large house price declines and which appears to be correlated with the share of IOs. We show this in Table 6, which reproduces Table 5 using the maximum 4-quarter decline following the peak, and where the values of population growth, unemployment, per capita income, and property tax rates are taken for the period following the peak. We now find that almost all of the mortgage characteristics we consider are statistically significant at the 5% level, and some are significant at the 1% level; only the share of mortgages with terms of 30 years or more fails to enter significantly. When we include all of these explanatory variables, only the share of hybrid mortgages and the share of investors are statistically significant at the 5% level. The share of IOs remains significant when we include these additional features, but only at the 5% level rather than the 1% level. Moreover, the coefficient on the share of IOs is less than half the size as when we only control for this mortgage characteristic.

Whatever the additional factor that appears to be associated with rapid declines in prices, then, it seems correlated with most of the other mortgage characteristics we consider, including variables like the share of subprime mortgages which appeared unrelated to house price appreciation before the peak. Since previous work has argued that the growth of the subprime market contributed to the increase in foreclosures, one reasonable candidate for the missing factor is the foreclosure rate once house prices began to decline. In our model, the foreclosure rate would contain no additional information beyond the share of IOs, since the only home buyers who default in our model are those who speculate. But in practice, there does appear to be significant variation in foreclosure rates that cannot be explained by differences in the propensity to use IOs. As an example, consider Washington DC and Stockton, CA. The two cities have similar shares of IOs at their peak, 47% and 49%, respectively. They also experiences similar rates of house price appreciation, where the maximum 4-quarter growth in house prices was 22.5% and 28.2%, respectively. But the two differ dramatically in terms of foreclosures. Stockton had a foreclosure rate of 9.5% in 2008 according to RealtyTrac, a firm which records foreclosure rates from public records and court notices, the highest in the country. In Washington DC, the foreclosure rate was less than a third, at 3.0%. Consistent with this, house prices fell much more in Stockton than in Washington DC when housing prices generally started to decline: The largest 4-quarter price decline after the peak are quite different in the two cities: 17.3% in Washington DC, but 42.3% in Stockton. Although differences in foreclosure rates are outside the model, there are various reasons for why they might differ. For example, if defaults tend to be more concentrated in certain areas in some cities, they are more likely to drive down the value of neighboring properties facing abandoned buildings. Campbell, Giglio, and Pathak (2009) provide evidence of such spillover effects.

To explore whether the additional mortgage characteristics we consider are significant because they help to predict foreclosure rates, we considered the maximum share of outstanding mortgages each quarter that entered into foreclosure for each city. We then regressed this measure on the maximum share of IOs. The residual from this regression represents the rate of foreclosure that could not be predicted based on the use of IOs. When we add this variable in addition to the list of characteristics we control for in Table 6, the share of hybrid mortgages is no longer significant, and the coefficients on the remaining mortgage characteristics are pushed towards zero. The one mortgage characteristic that does enter significantly is the share of nonprime mortgages with a CLTV of over 80%, but it enters with the opposite sign than when we control for the variable by itself. Moreover, when we tried an F-test all six mortgage characteristics, we could not reject the hypothesis that they were all zero. The share of IOs is now significant at the 1% level. Thus, the factors that seem to best predict which cities will experience large price declines following their peak are whether these cities relied on IOs, even though these were largely taken out before house prices peaked, and whether these cities experienced an unexpectedly large number of foreclosures[18]

5 Time Series Analysis

In the previous section we demonstrated that use of IOs is a powerful indicator of whether a city experienced rapid house price appreciation in the period 2000-2008. Our hypothesis is that this reflects the contract choices of house buyers and mortgage lenders amidst house price speculation. However, it could be that backloaded mortgages are mechanically associated with house price appreciation, because as houses become expensive, borrowers choose such mortgages for affordability reasons. We have already provided some evidence from the cross-section that rejects this interpretation. In this section, we offer additional evidence based on the time series information in our panel of cities.

Our strategy for doing this involves testing for Granger-causality. If use of IOs is driven by past house price appreciation then this appreciation should Granger-cause increased use of IOs. If anticipated house price appreciation leads agents to choose IOs then increased use of these mortgages should Granger-cause house price appreciation. In this latter case we would not interpret Granger-causality as meaning there literally is a causal connection between current use of IOs and future house price appreciation. Rather, consistent with our theory, we would interpret it as meaning simply that increased use of IOs *anticipates* future growth in house prices. This pattern is clearly evident for the case of Phoenix described in the introduction. Is it true more generally?

Based on our theory we do not expect use of IOs to Granger-cause house price appreciation in *all* cities. We only expect this in cities where there was an added incentive to use IOs due to speculation. In these cities we expect to see that these mortgages attained a relatively high share of the market. Therefore in this section we focus on a subset of cities in which the peak share of interest-only mortages was relatively high, at least 40 percent. This leaves us with a sample of 29 cities. Using 40 percent as the cut-off is clearly arbitrary. Nevertheless,

[18] As a robustness check, we also looked at whether the maximum foreclosure rate was useful for predicting price appreciation before the peak. Adding this variable did not have much impact on the coefficient on the share of IOs, and while the foreclosure rate was statistically significant at the 5% level when it was the only mortgage variable other than the share of IOs, it was not statistically significant when we included it together with the other six mortgage characteristics we considered.

it has the advantage that the dynamics of house prices and mortgages in these cities appears relatively homogeneous. For example, all cities in this sample experience both relatively large price appreciations and peak usage of IOs in the middle quarters of the sample period. Such homogeneity justifies assuming that the coefficients in the statistical models we use to test for Granger-causality are common across these cities thereby increasing the power of these tests. In addition to restricting our sample of cities, we focus attention on only the period of house price appreciation leading up to 2006q4. This quarter is the peak of the national FHFA real price.[19] We focus on the period of house price appreciation instead of the entire boom-bust period since our theory has little to say about use of IOs after prices collapse. In summary, our dynamic analysis is based on a balanced panel of the 29 cities with the largest usage of IOs running from 2000q1 to 206q4.

Some simple correlations demonstrate that the dynamics of IO use and house price appreciation evident for Phoenix is widespread in our sample of 29 cities. Figure 3 displays dynamic correlations between changes in the share of IOs at date $t + j$, Δio_{t+j}, with log changes in real house prices at date t, Δp_t, for $j = -4, -3, \ldots, 4$. The key features of Figure 3 are that the correlations for $j \leq 2$ are positive, the correlations between changes in the IO share and future house price appreciation are larger than the corresponding correlations with past appreciation, and the peak correlation is at $j = -2$. In the language of time series analysis, this pattern of correlations indicates that increased use of IOs *leads* house price appreciation in cities which end up with relatively high shares of IOs. This leading relationship is consistent with IO use rising in anticipation of house price appreciation.

To test for Granger-causality we estimate simple statistical models of house price appreciation and changes in use of IOs including from one to four lags of these variables. For simplicity we focus on models where the number of included lags is the same for each variable. Table 7 and Table 8 report coefficient estimates, specification tests, and tests of Granger-causality based on estimating the models in two ways, motivated by different underlying assumptions regarding the degree of homogeneity among the cities in our sample. Assuming that housing market dynamics are well-approximated by models with identical constant and slope coefficients, then Ordinary Least Squares (OLS) is appropriate. OLS estimates are reported Table 7. To accommodate city-specific constants, that is "fixed effects," with homogeneous slope coefficients, we use the System-GMM estimator developed by Arellano and Bover (1995) and Blundell and Bond (1998). Our GMM estimates are reported in Table 8.

[19]We have redone our analysis using the first three quarters of 2006 as the end-date of the sample and obtain similar results.

Consider the OLS estimates in Table 7. Maintaining the homogeneity hypothesis, the results based on OLS are valid only if the estimated residuals are serially uncorrelated. Otherwise the estimates are inconsistent. The tables report p-values for Arellano and Bond (1991) tests of the null hypothesis that the residuals exhibit no serial correlation of order one through four, $AR(j), j = 1, 2, 3, 4$. Using conventional significance levels, these tests indicate that it is not possible to reject serial correlation for at least value of j when predicting house price appreciation or changes in the share of IOs, even with four lags of each variable. We have increased the number of lags to six and serial correlation is still present. The presence of serial correlation is a strong indication that fixed effects are present in our data and that our estimates and standard errors based on OLS are inconsistent.

Since the tests of serial correlation do not have perfect power it is still worth examining the tests for Granger-causality with OLS. Table 7 indicate we can reject the null hypothesis that house price appreciation does not Granger-cause future use of IOs with three lags at the 3 percent level of significance and with two lags at the 4 percent level. (The F-statistics associated with this hypothesis are displayed in the row labelled "F-stat" with the associated p-values below.) In both the two and three lag cases we cannot reject the hypothesis that the coefficients on house price appreciation sum to negative numbers. (The sum of coefficients with associated standard errors are reported in the rows labelled with summations.) Furthermore, the only individual coefficients that are significant are negative. So, while there is some evidence of Granger-causality running from prices to IOs, it is of the wrong sign. Table 7 also indicates the null hypothesis that changes in use of IOs do not Granger-cause house prices is strongly rejected in all cases. While a couple of the IO coefficients are negative, the magnitudes are small, and we cannot reject the hypothesis that the sum of interest-only coefficients is positive in all four models. Combined these results indicate that increased use of IOs is probably not driven by past house price appreciation and suggest that use of IOs anticipates future house price appreciation, and

In addition to rendering the estimates inconsistent, serial correlation in the OLS residuals suggests fixed effects are present. With fixed effects OLS suffers from dynamic panel bias.[20] Our GMM estimates address this phenomenon. System-GMM involves estimating the coefficients of interest using a system of two equations. One equation involves differencing the original equation to remove the fixed effects and then using lagged variables as instruments. Instrumental variables are necessary in this case because differencing induces correlation between lagged dependent variables and the differenced error term. The sec-

[20] Whether or not fixed effects are included in the regression, if the data indeed have fixed effects, estimation by OLS mechanically induces correlation between the regression error and any included lagged dependent variable.

ond equation involves using lagged differences of the variables as instruments in the original equation. The validity of the instruments in this case is based on the assumption that the differenced variables are orthogonal to the fixed effects. Our GMM estimates in Table 8 are based on using lags three and four of both variables as instruments.[21] The validity of our instruments with GMM depends on the lack of serial correlation in the estimation errors for the differenced equation of order three and higher.[22] Table 8 indicates four lags are necessary for this condition to be satisfied when forecasting house price appreciation. Table 8 indicates two or more lags satisfy the serial correlation criterion when forecasting changes in the use of IOs.[23]

For the three and four lag models of IOs estimated by GMM we fail to reject the hypothesis that prices do not Granger-cause mortgages at greater than the 29 percent significance level. In the two lag case there is modest evidence that the null of no Granger-causality is rejected, at the 7 percent level. Notice, however, that in the two lag case the only coefficent that is significant is negative. Moreover, the sum of coefficients is negative in all three cases and significantly so at the 10 percent level with two lags. So, at best, house price appreciation predicts lower future use of IOs. With the four lag model of house price appreciation estimated using GMM the hypothesis that mortgages do not Granger-cause prices is easily rejected. Furthermore, the two coefficients on lagged IOs in the four lag model that are statistically significant are positive and the estimated sum of all these coefficients is positive, large and statistically significant. Since lags three and four of IOs are not significant we have explored estimating this model with just two lags on mortgages. We obtain similar results. So, the evidence points toward our hypothesis that increased use of IOs occurs in anticipation of, and not because of previous, house price appreciation.

It is common in dynamic panel data analysis to include dummies for each time period

[21] All lags at least as large as three are valid instruments. Including all valid lags can lead to a very large number of instruments, which is problematic in small samples. For example, the p-value of the Hansen-Sargan test of the overidentifying restrictions is biased toward unity. Our implementation of System-GMM follows the convention of including orthogonality conditions that are valid at each date. That is, the expectations are evaluated over cities at each date. Consequently, even with just two lags as instruments the instrument count is quite large. While we fail to reject the J test of the overidentifying restrictions with a p-value of unity in all the models estimated by GMM, the actual values of the test statistic are relatively small. This indicates that the non-rejection of the overidentifying restrictions is not driven by noise in the data. Evaluating expectations over cities and dates dramatically reduces the number of instruments. There does not appear to be agreement in the empirical literature on the best way to proceed. However, our findings are robust to following this approach. Our findings are also robust to using just one lag as an instrument.

[22] Differencing induces first order serial correlation and the fact that the original equations already involve first differences suggests up to second order serial correlation is possible.

[23] We have explored using the lag selection criterion for dynamic panel data models introduced by Andrews (1999). For both house price appreciation and change in use of IOs this leads to the one lag model being chosen.

in the estimation. Such a practise is typically justified by the concern that predictability at the individual level is not a direct manifestation of individual decisions, but is driven by an aggregate relationship. In our sample of 29 cities the run-up in house prices occurred in all cities around the same time, so including time dummies renders the other regressors insignificant. It is conceivable that innovation in credit provision is a leading indicator of the business cycle and house prices are a lagging indicator. If it just so happened that IOs were a product of such innovation then the relationship we observe at the city level could just reflect business cycle dynamics. We are skeptical of this hypothesis because it does not account for why use of IOs was concentrated in those cities that had the most house price appreciation. Nevertheless, we have considered including lags of log GDP growth and the Federal Funds interest rate in our statistical models and it does not change our findings.

6 Mortgage Pre-payment and Foreclosure

Our theory implies that lenders prefer IOs over traditional mortgages because they encourage borrowers who may be speculators to re-pay their mortgages sooner rather than later. An obvious check on our theory, then, is to assess whether the IOs in our sample are pre-paid at a faster rate than other kinds of mortgages. However confirmation of this feature of our model it does not prove our theory. For example, it could be that households who intend to move relatively soon choose IO mortgages. Still, if we were to find that IOs are pre-paid at a slower rate than other mortgages it would be evidence against our theory. Our theory also predicts that when prices collapse holders of IO mortgages who are speculators will default. This suggests looking at foreclosure in our data as well.

We estimate pre-payment and foreclosure rates using the Kaplan-Meier estimator. For simplicity we focus on pre-payment, but the same approach applies to foreclosures. Let $S(t)$ be the probability that payments on a particular kind of mortgage originated at a fixed date continue at least until date t. For a sample from this population of size N, let the observed times until pre-payment be t_i, $i = 1, 2, \ldots, N$. Corresponding to each t_i is the number of mortgages that are at risk of being pre-paid at date $t_i - 1$, n_i, and the number of pre-payments d_i. The Kaplan-Meier estimator is the nonparametric maximum likelihood estimate of $S(t)$

$$\hat{S}(t) = \prod_{t_i \leq t} \frac{n_i - d_i}{n_i}$$

Our estimator of the pre-payment rate is $1 - \hat{S}(t)$. Note that we include refinances in our classification of pre-paid since lenders are indifferent between pre-payment due to selling the house and refinancing. An important advantage of the Kaplan-Meier estimator is that the method takes into account right-censoring, which occurs if a mortgage leaves our sample before pre-payment is observed. This can happen when the mortgage is sold to a servicer outside the sample or the mortgage is defaulted on, i.e. foreclosed. Unemployment durations are typically estimated using the Kaplan-Meier approach.

Figure 4 displays estimated pre-payment and foreclosure rates for two types of mortgage: IO and non-backloaded. By "non-backloaded" we mean all mortgages that are neither IOs or Option-ARMs. The estimates are based on mortgages originated in 2005q1 for our sample of 29 cities with maximum share of IOs exceeding or equal to .4. This has the advantage of including a relatively large sample of IOs. Our estimates are similar for other quarters of origination in 2005 and 2004. They are less similar for data in 2003 where the sample is small. The figure indicate that the point estimates for pre-payment and foreclosure rates are higher for IOs than non-backloaded mortgages. After 8 quarters 40 percent of IOs have been pre-paid while less than 30 percent of non-backloaded mortgages are pre-paid. The difference in pre-payment rates stays roughly equal through 20 quarters. The foreclosure rates are indistinguishable through 8 quarters after origination and then start to diverge in during 2007. By 2009q2 the difference in foreclosure rates is more than 10 percentage points. Using the log rank test of equality we confirm that the differences between the survival functions, $\hat{S}(t)$, are statistically different at very high levels of significance. We conclude that our data is consistent with the theory along these dimensions.

7 Conclusion

In this paper we have argued that both speculators and those who lend to them prefer mortgages with backloaded payments, in particular interest-only mortgages, over traditional mortgages when there is speculation in the housing market and lenders cannot determine which borrowers are speculators. This insight motivated our analysis of house prices and mortgages for a sample of US cities over the period 2000-2008. Our main findings are that interest-only usage is a robust predictor of which cities experienced high price appreciation; subprimes, securitization, leverage do not seem important for predicting price appreciation; that IOs do not seem to be used because of high house prices, but that IOs anticipate future price appreciation; following periods of high IO origination, IOs pre-paid and foreclosed at higher rates than mortgages without backloaded payments. We think our findings represent compelling evidence that speculation was an important reason for the rapid acceleration in house prices in some cities over the period 2003 to 2006.

Some may interpret our findings to mean that backloaded mortgages such as interest-only loans were essential for sustaining speculation, and so policymakers concerned about speculation should forbid these contracts. Our analysis does not allow us to say what would happen counterfactually if such a rule were in place. However, we note a few things. First, backloaded contracts do serve a useful purpose in some cases, such as helping those expecting future income growth (young people starting their careers). Second, backloaded contracts may be the "canary in the coal mine" in terms of anticipating future price increases, as our empirical evidence suggests. Finally, Barlevy (2009) suggests forbidding backloaded contracts may not prevent speculation, and since backloaded contracts encourage agents to sell, eliminating them may end up causing asset prices to be even more overvalued.

Appendix A: Proofs of Theoretical Results

Equilibrium Contracting with Bubbles: In this section, we show that if there exists an IO contract that both borrower and lender prefer, then (1) both types of contracts will be offered in equilibrium, with speculators choosing the interest-only contract and safe borrowers choosing the traditional mortgage; (2) interest-only contracts will carry a higher interest charge in equilibrium.

First, we argue that speculators must receive interest-only contracts in equilibrium. For suppose not, i.e. they receive a traditional mortgage contract with interest rate r^*. The interest rate r^* must exceed $\beta^{-1} - 1$. Otherwise, since safe borrowers can always choose this contract, the amount they must repay will not be enough to cover the amount they borrow. But then the lender will make a loss, since the expected value of lending to a speculator is negative, requiring positive profits on safe borrowers.

Now, consider a lender who offers the same set of contracts in equilibrium, but in addition offers an interest-only contract with interest rate r^*. Safe borrowers will not choose this contract, since given $1 + r^* > \beta^{-1}$, the present discounted value they would have to pay under the traditional contract is lower. Instead, they will stick with whatever contract they were originally choosing in equilibrium. However, both borrowers and lenders are better off under the interest-only contract. Since lenders earn zero profits in equilibrium, this implies the lender who offers the interest-only contract at rate r^* will earn a strictly positive profit. But then this could not have been an equilibrium.

Next, we argue that safe borrowers will take the traditional mortgage in equilibrium. For suppose in equilibrium all borrowers took the IO with interest rate r^*. Consider a lender who offers a traditional mortgage at rate r^*. Such a lender would not attract speculators, since by assumption they prefer the interest-only contract. It will, however, attract safe borrowers, since as argued above $r^* > \beta^{-1} - 1$, and so safe borrowers prefer the traditional mortgage at a given interest rate. Since lending to safe borrowers at an interest rate that exceeds the discount rate yields positive profits, such a lender will earn a strictly positive profit. But then this could not have been an equilibrium.

Hence, both contracts will be offered in equilibrium. We now argue that in equilibrium, speculators must be indifferent between the types of mortgages contracts in equilibrium. For suppose not, i.e. speculators striclty prefer the interest-only contract. Consider a lender who offers only the traditional mortgage contract offered in equilibrium, but lowering the interest rate by ε. Given speculators strictly prefer the interest-only contract, there exists an ε such that they would still prefer the interest-only contract. Traditional borrowers will prefer this contract. But there exists an ε small enough that the lender offering this contract and attracting only the safe borrowers will earn a strict profit.

Since speculators prefer the interest-only contract with rate r^* to the traditional mortgage contract with rate r^*, the only way to ensure speculators are indifferent between the two contracts is to charge a lower rate on the traditional mortgage in order to make it more attractive. Hence, the equilibrium rate on the traditional mortgage contract will be lower than on the interest-only contract. To solve for this rate, we need to find the interest rate such that V_1 at this rate is equal to V_1 under the interest-only contract with rate r^*.

Proof of Proposition: The proof is by induction. We begin by characterizing $E[f_t|\tau \geq t]$. For $t = T+1$, we have

$$E[f_{T+1}|\tau \geq T+1] = \epsilon D$$

For $t \leq T$, we can use (1) to derive the recursive relationship

$$E[f_t|\tau \geq t] = q\epsilon D + (1-q)\beta E[f_{t+1}|\tau \geq t+1]$$

Below we make use of two additional results. First, p_t corresponds to the fundamental value f_t conditional on either $\tau \leq t$ or $\tau \geq t+1$, i.e. the price is determined after nature decides whether to reveal the value of dividends at date t or not. Second, we use the fact that when $p_t = f_t$ and $\theta = 1$, the value of foreclosure $FC_{t+1} = E[f_{t+1}|\tau \geq t+1]$.

We now establish the claim by backwards induction. For $t = T+1$, we have

$$V_{T+1} + \Pi_{T+1} = \epsilon D = E[f_{T+1}|\tau \geq T+1]$$

Next, suppose $V_s + \Pi_s = E[f_s|\tau \geq s]$ for $s = t+1, ..., T+1$. We want to show that the result holds for date $s = t$ as well. Adding up the equations for V_t and Π_t and using the fact that $d_t = 0$ if $\tau \geq t$ yields

$$V_t + \Pi_t = q\epsilon D + (1-q)(\pi_t + \max[p_t - (1+r)L_{t-1}, \beta V_{t+1} - m_t, 0])$$

We consider different possibilities for which argument maximizes the latter term.

First, suppose the borrower prefers to pay off the loan. In this case,

$$\max[p_t - (1+r)L_{t-1}, \beta V_{t+1} - m_t, 0] = p_t - (1+r)L_{t-1}$$

and $\pi_t = (1+r)L_{t-1}$, and so $V_t + \Pi_t = q\epsilon D + (1-q)p_t$. But here p_t is conditional on the value of dividends not being revealed at date t, and so $p_t = E[f_t|\tau \geq t+1]$. But if $\tau \geq t+1$, then $d_{t+1} = 0$ with certainty. Hence, using (1), we have

$$\begin{aligned} E[f_t|\tau \geq t+1] &= E\left[\sum_{s=t+1}^{\infty} \beta^{s-t} d_s \middle| \tau \geq t+1\right] \\ &= \left[\sum_{s=t+2}^{\infty} \beta^{s-t} d_s \middle| \tau \geq t+1\right] \\ &= \beta E\left[\sum_{s=t+2}^{\infty} \beta^{s-t-1} d_s \middle| \tau \geq t+1\right] \\ &= \beta E[f_{t+1}|\tau \geq t+1]. \end{aligned}$$

Substituting back in yields $V_t + \Pi_t = q\epsilon D + (1-q)\beta E[f_{t+1}|\tau \geq t+1] = E[f_t|\tau \geq t]$.

Next, suppose the borrower prefers to make his payment. In this case,

$$\max[p_t - (1+r)L_{t-1}, \beta V_{t+1} - m_t, 0] = \beta V_{t+1} - m_t$$

and $\pi_t = m_t + \beta\Pi_{t+1}$, and so $V_t + \Pi_t = q\epsilon D + (1-q)\,\beta\,(V_{t+1} + \Pi_{t+1})$. But since $V_s + \Pi_s = E\left[f_s | \tau \geq s\right]$ for $s = t+1, ..., T+1$, this expression reduces to $q\epsilon D + (1-q)\,\beta E\left[f_{t+1}|\tau \geq t+1\right]$. But this is exactly $E\left[f_t|\tau \geq t\right]$.

Finally, suppose the borrower prefers to default. In this case, $\max\left[p_t - (1+r)\,L_{t-1}, \beta V_{t+1} - m_t, 0\right] = 0$ and $\pi_t = \max\left(p_t, \beta FC_{t+1}\right)$, and so $V_t + \Pi_t = \max\left(p_t, \beta FC_{t+1}\right)$. But above we argued that $FC_{t+1} = E\left[f_{t+1}|\tau \geq t+1\right]$ and $p_t = \beta E\left[f_{t+1}|\tau \geq t+1\right]$. Hence, we have $V_t + \Pi_t = q\epsilon D + (1-q)\,\beta E\left[f_{t+1}|\tau \geq t+1\right]$. But this is exactly $E\left[f_t|\tau \geq t\right]$. ■

Appendix B: Data

In this appendix, we describe the data sources we used and the way we constructed the data.

B1. House Price Data

Our primary data source for housing prices is the Federal Housing Finance Agency (FHFA) house price index for metropolitan areas. ****Discuss metropolitan areas and metro division way of organizing the data****. To arrive at a measure of changes in real house prices over time, we divide the FHFA index by the consumer price index for urban consumers for all items, as reported by the Bureau of Labor Statistics.

Because of data limitations for mortgage data before 2000, we begin our dataset in the first quarter of 2000, even though the FHFA index is available earlier. However, we do use earlier data to construct a measure of real house price appreciation between 1985:Q1 and 1989:Q4. Given limitations on some of the explanatory variables we use, such as real income per capita, we only consider data through the end of 2008. To simplify the notation, we index quarters by t and refer to the initial date, 2000:Q1, as $t = 1$ and the terminal date, 2008:Q4, as $t = T$.

For each city i, we identify the quarter where the real price of housing reaches its peak. That is, if p_{it} denotes the real house price index in city i at date t, then

$$t_i^* = \arg\max_t \{p_{it}\}$$

To simplify notation, we will henceforth omit the reference to city i. As our measure of real house price appreciation before the peak, we use the highest 4-quarter growth in real house prices up date t^*. That is, the rate of house price appreciation in the boom phase is given by

$$\max\left\{\ln\left(\frac{p_t}{p_{t-4}}\right)\right\}_{t=5}^{t^*}$$

for all cities where $t^* \geq 5$.

****Add explanations for average annualized real house price appreciation prior to peak and the largest 4-quarter decline after the peak ****

Formulae:

$$\frac{4}{t^* - 1} \ln \frac{p_{t^\&}}{p_1}$$

$$\max \left\{ \ln \left(\frac{p_{t-4}}{p_t} \right) \right\}_{t=t^*}^{T}$$

where $T \geq t^* + 4$.

Since the FHFA is an index, we need to turn to alternative sources for data on price levels. We chose to use the median sales price of single family homes in each city as reported by the National Association of Realtors in 2000:Q1. Denote this price as P_1^{NAR} to reflect that we only use this price from date $t = 1$. To compute the level of the peak price in each city in a way that would not be vulnerable to changes in the composition of housing that are brought to the market, we use the appreciation rate in the FHFA index. That is, the peak price in each city is given by

$$P_{t^*} = P_1^{NAR} \times \frac{p_{t^*}}{p_1}$$

When we use this price as a control in Table 2, we use $\ln P_{t^*}$.

B2. Mortgage Data

Our primary data source for mortgage information is the Lender Processing Services (LPS) mortgage performance data, reported by loan servicers for the mortgages they service.

One issue we have to grapple with in this dataset is that different servicers begin to report to LPS at different dates. When a servicer first begins to report, they report on all of their outstanding loans. For loans that were originated before the servicer began to report, the relevant data is backfilled, i.e. data for quarters before the servicer began to report are entered not in real time but retrospectively. This pattern of reporting gives rise to survivorship bias, since the only loans that are reported retrospectively are those which survive until the reporting period. One approach that some researchers have taken to address this, e.g. Foote, Gerardi, Goette, and Willen (2009), is to only include mortgages that are reported within a short time of when they were originated, e.g. a few months or one year. However, restricting attention to data that is reported in real time can give rise to composition bias if the servicers that report to LPS later tend to specialize in certain types of loans. In particular, throwing out backfilled data appears to undercount the share of backloaded mortgages earlier on in our sample, since these mortgages appear to have been disproportionately originated by services who report to LPS at later dates. Since we only use data starting in 2000:Q1, we opted to use backfilled data on mortgages to avoid composition bias. This restriction has little bearing on our cross-sectional analysis, which mostly relies on origination in the peak years of 2005 and 2006 when most services were already reporting to McDash. The restriction does matter for the time-series analysis. However, since we find that interest-only mortages were less likely to survive, the survivorship bias in our data biases against our findings. Looking only at surviving mortgages will tend to underrepresent the fraction of mortgages originating in previous years that would have involved a backloaded payment pattern.

B3. Other Data

Finally, we compile data on cities from various sources that we can use to control for differences across cities that may directly account for differences in prices. To be completed...

WRI – from Gyourko, Saiz, and Summers (2008).

Undevelopeable area – from Saiz (2010)

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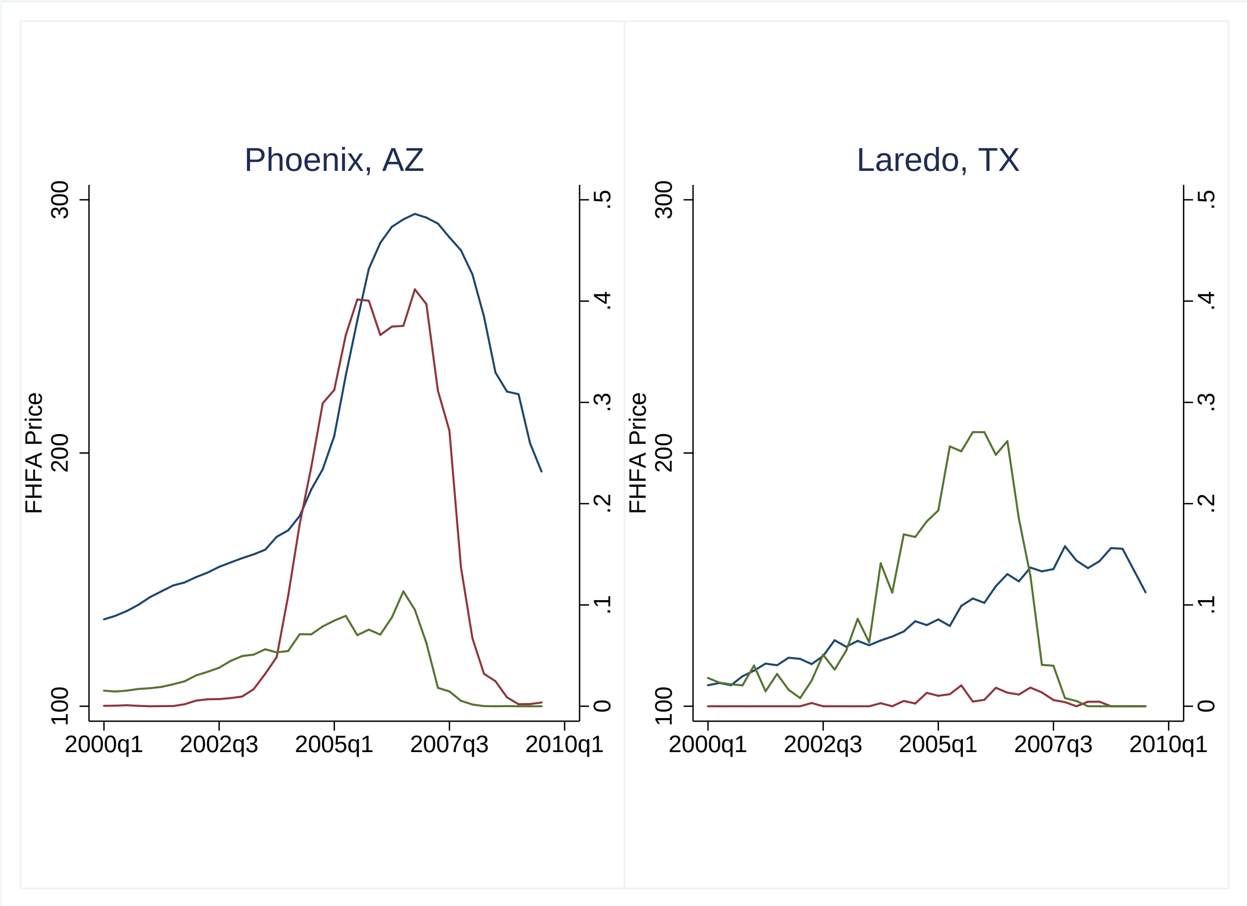
Figure 1: House Prices and Mortgage Use in Two Cities



Note: Blue lines – Real Price, Red lines – IO Share, Green lines – Sub-prime Share.

Figure 2: Maximum 4 Quarter Appreciation versus Maximum IO Share

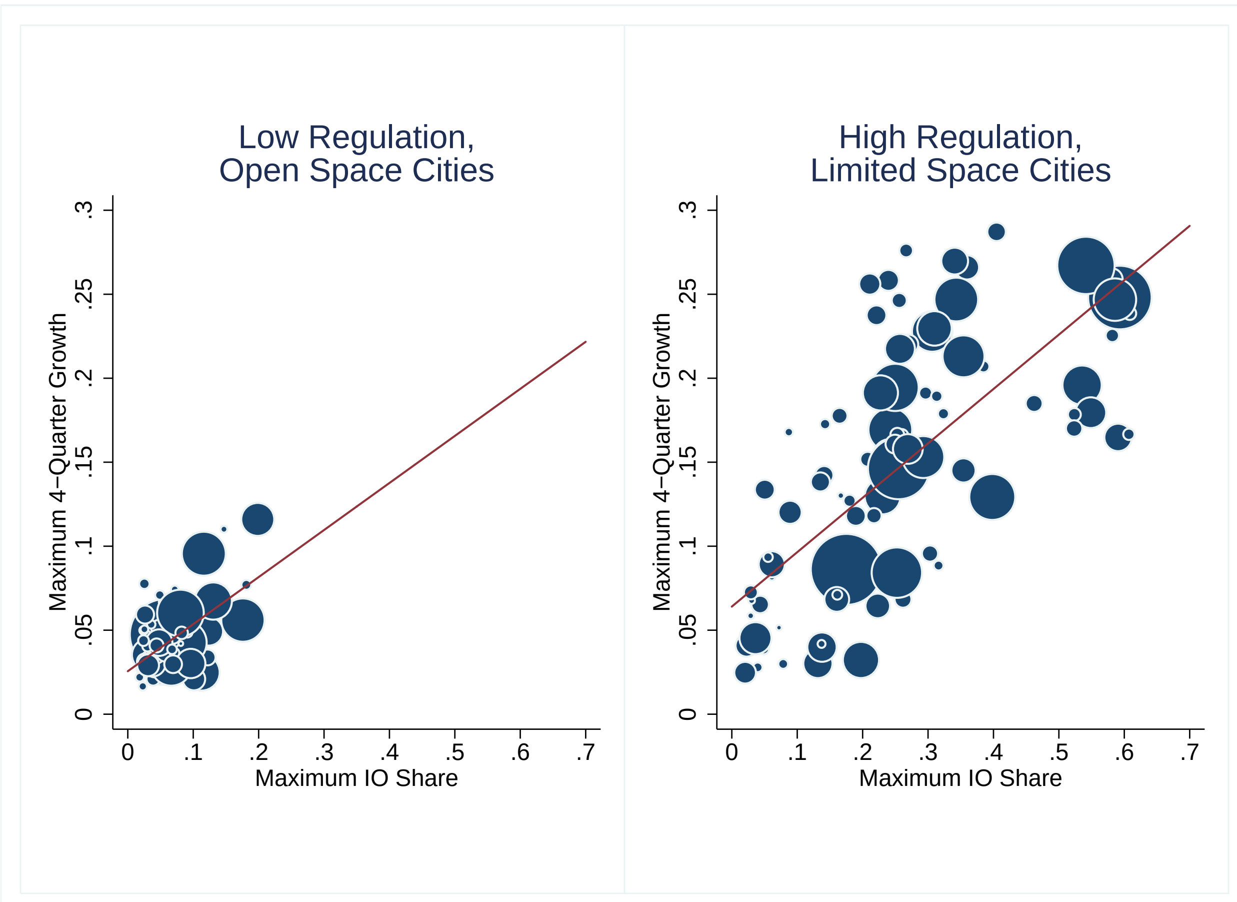


Figure 3: Correlations between Δio_{t+j} and Δp_t

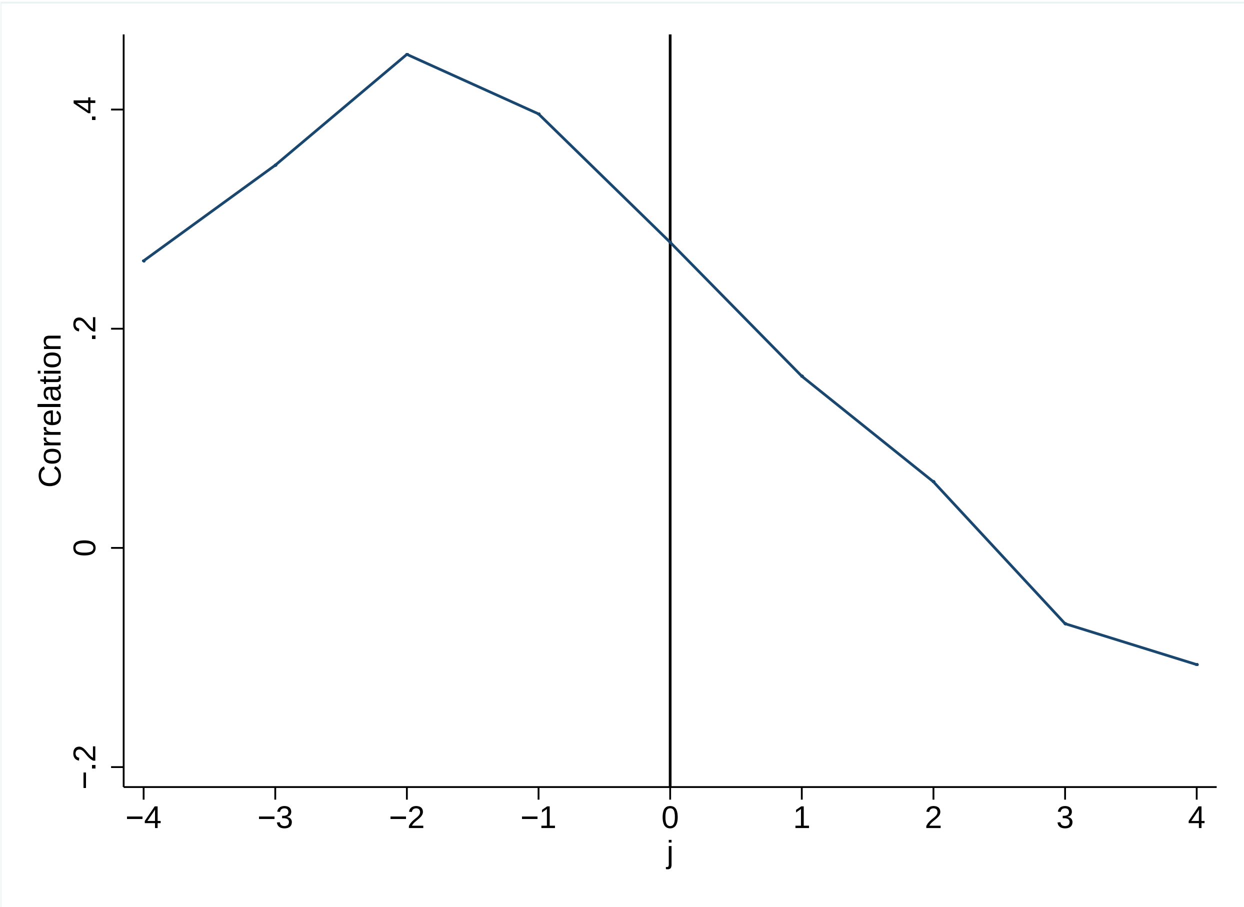
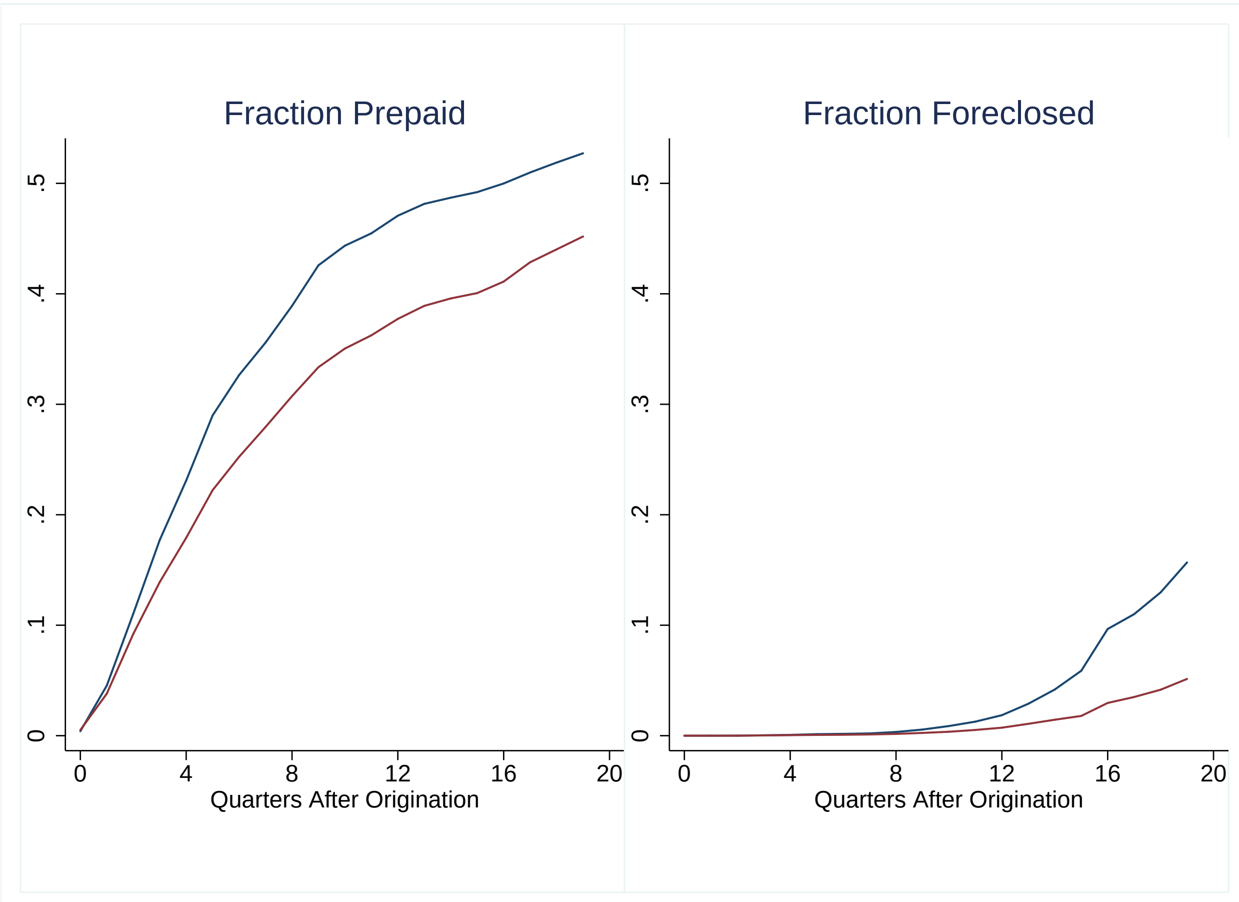


Figure 4: Pre-Payment and Foreclosure of Mortgages Originated in 2005q1



Note: Blue lines – IO Mortgages, Red lines – Non-Backloaded Mortgages.

Table 1: Mortgage Characteristics

Type	Year	Mean Amount	All	Owner	Investor	Long Term	Priv. Sec.	Sub-prime	Pre Pay Penalty	ARM
IO	2000	451.0	0.1	97.1	2.9	58.9	58.7	0.2	0.3	92.8
	2005	322.3	19.7	88.6	11.4	99.4	52.5	9.3	17.8	91.8
Option ARM	2000	192.4	1.1	97.3	2.7	98.6	N/A*	1.2	31.9	100.0
	2005	342.2	8.2	84.7	15.3	99.9	58.4	24.3	64.2	100.0
Fixed	2000	120.9	85.2	95.8	4.2	90.9	9.4	0.6	4.5	0.0
(Not IO)	2005	185.5	57.3	92.6	7.4	91.3	14.3	2.1	2.0	0.0
ARM	2000	217.9	10.6	98.1	1.9	94.1	17.6	7.6	14.5	100.0
(Not Backloaded)	2005	215.7	14.2	90.4	9.6	99.8	45.0	29.8	28.0	100.0
Other	2000	138.7	3.0	99.9	0.1	99.1	23.7	0.0	0.0	0.0
	2005	195.6	0.5	96.4	3.6	98.4	4.2	9.8	0.3	0.0
All	2000	132.6	100.0	96.1	3.9	91.5	10.7	1.3	5.7	11.7
	2005	225.6	100.0	90.9	9.1	94.9	29.8	9.3	13.9	40.5

Note: Entries are percent of indicated type of mortgage except for "Mean Amount" which is in units of thousands of current dollars.

Table 2: Summary Statistics for Price and Mortgage Variables

	Mean	St. Dev.	Min	25p	Median	75p	Max
Prices							
Max 4Q Growth	0.10	0.07	0.02	0.04	0.07	0.15	0.33
Mean Growth	0.05	0.04	0.01	0.02	0.03	0.07	0.14
Max 4Q Decline	0.10	0.10	0.00	0.05	0.07	0.11	0.65
Mean Decline	0.05	0.06	0.00	0.02	0.03	0.06	0.35
Mortgages							
Max IO Share	0.15	0.14	0.02	0.05	0.09	0.22	0.61
Max Hybrid Share	0.14	0.06	0.04	0.10	0.13	0.16	0.45
Max Long Term Share	0.97	0.02	0.90	0.96	0.97	0.98	1.00
Max Sub-prime Share	0.11	0.05	0.03	0.08	0.10	0.14	0.32
Max Securitiezed Share	0.28	0.10	0.12	0.21	0.25	0.32	0.64
Mean 80+ Share	0.68	0.08	0.38	0.63	0.69	0.73	0.85
Max Investor Share	0.11	0.04	0.04	0.08	0.10	0.14	0.38

Table 3: Baseline Models of Maximum 4 Quarter Price Appreciation in Boom Phase

	(1)	(2)	(3)	(4)
Max IO Share	.42		.35	.22
	(.03)***		(.03)***	(.04)***
Population Growth		2.76	1.35	.86
		(.64)***	(.36)***	(.27)***
Population		.008	-.002	.001
		(.006)	(.004)	(.002)
Per Capita Income Growth		.5	.87	.21
		(.44)	(.32)***	(.15)
Per Capita Income		-.04	-.1	-.06
		(.04)	(.03)***	(.01)***
Property Tax		-.04	.02	-.006
		(.01)***	(.01)*	(.01)
Property Tax Change		-.85	-.58	-.64
		(.2)***	(.1)***	(.1)***
Unemployment		.001	-.005	.001
		(.004)	(.003)	(.002)
Unemployment Growth		-.42	-.3	-.06
		(.11)***	(.08)***	(.08)
Regulation		.02	.004	-.004
		(.01)**	(.008)	(.005)
Undevelopable Land		.09	.02	.02
		(.03)***	(.02)	(.01)
State Fixed Effects	No	No	No	Yes
Observations	237	237	237	237
R^2	.65	.74	.87	.97

Note: OLS regressions of Maximum 4 Quarter Price Appreciation on indicated variables. Most variables are mean values from 2000q1 to quarter of peak real house price. Property taxes are for 2000 and the change between 2000 and the year of the peak price. The Regulation variable is the Wharton Regulation Index and the Undevelopable Land variable is from Saiz (2010). ***, **, and * denote significance at the 1, 5 and 10 percent levels respectively.

Table 4: Controlling for Affordability and Past Price Appreciation

	(1)	(2)	(3)	(4)	(5)	(6)
Max IO Share		.28		.29		.39
		(.07)***		(.07)***		(.04)***
Price at Peak	.13	.04				
	(.02)***	(.04)				
Price/Income at Peak			.13	.03		
			(.02)***	(.03)		
Appreciation 1985-1989					.32	-.19
					(.13)**	(.13)
Observations	105	105	105	105	171	171
R^2	.86	.9	.86	.9	.76	.88

Note: OLS regressions of Maximum 4 Quarter Price Appreciation on indicated variables plus the variables in Table 3 excluding Regulation, Undevelopable Land and State Fixed Effects. ***, **, and * denote significance at the 1, 5 and 10 percent levels respectively.

Table 5: Controlling for Additional Mortgage Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Max IO Share	.35	.4	.33	.36	.35	.31	.34	.25
	(.03)***	(.04)***	(.04)***	(.03)***	(.05)***	(.04)***	(.02)***	(.07)***
Max Hybrid Share		-.14						.01
		(.09)						(.17)
Max Long Term Share			.43					.43
			(.24)*					(.24)*
Max Sub-prime Share				-.19				-.2
				(.09)**				(.21)
Max Securitized Share					.008			.1
					(.06)			(.06)
Mean 80+ Share						-.15		-.06
						(.07)**		(.07)
Max Investor Share							.62	.58
							(.13)***	(.12)***
Observations	237	237	237	237	237	237	237	237
R^2	.87	.88	.87	.88	.87	.88	.9	.91

Note: OLS regressions of Maximum 4 Quarter Price Appreciation on indicated variables plus the variables in Table 3 excluding Regulation, Undevelopable Land and State Fixed Effects. ***, **, and * denote significance at the 1, 5 and 10 percent levels respectively.

Table 6: Controlling for Additional Mortgage Characteristics With Price Declines

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Max IO Share	.59	.43	.6	.59	.39	.7	.58	.31	.35
	(.08)***	(.08)***	(.08)***	(.07)***	(.09)***	(.08)***	(.07)***	(.12)***	(.08)***
Max Hybrid Share		.51						.6	.26
		(.15)***						(.3)**	(.24)
Max Long Term Share			-.33					-.64	-.17
			(.49)					(.53)	(.36)
Max Sub-prime Share				.47				-.39	-.27
				(.15)***				(.3)	(.23)
Max Securitized Share					.32			.27	.12
					(.09)***			(.1)***	(.08)
Mean 80+ Share						.39		.27	-.13
						(.15)**		(.14)*	(.11)
Max Investor Share							.37	.56	-.04
							(.2)*	(.19)***	(.15)
Foreclosure									12.3
									(1.9)***
N	188	188	188	188	188	188	188	188	188
R^2	.84	.85	.84	.85	.85	.85	.84	.88	.93

Note: OLS regressions of Maximum 4 Quarter Price Declines on indicated variables plus the variables in Table 3 excluding Regulation, Undevelopable Land and State Fixed Effects. ***, **, and * denote significance at the 1, 5 and 10 percent levels respectively.

Table 7: Granger-Causality Based on OLS Regressions

	Δp_t				Δio_t			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Δp_{t-1}	.50	.37	.37	.42	.04	-.02	.05	.04
	(.04)***	(.05)***	(.05)***	(.04)***	(.06)	(.07)	(.07)	(.07)
Δp_{t-2}		.14	.10	.12		-.14	-.03	.008
		(.04)***	(.04)**	(.04)***		(.07)*	(.08)	(.08)
Δp_{t-3}			.15	.27			-.21	-.18
			(.04)***	(.05)***			(.08)***	(.08)**
Δp_{t-4}				-.31				-.04
				(.04)***				(.07)
Δio_{t-1}	.17	.13	.15	.13	.43	.32	.33	.31
	(.03)***	(.03)***	(.03)***	(.03)***	(.05)***	(.05)***	(.05)***	(.05)***
Δio_{t-2}		.12	.13	.12		.29	.32	.34
		(.03)***	(.03)***	(.03)***		(.05)***	(.05)***	(.05)***
Δio_{t-3}			-.07	-.07			-.10	-.07
			(.04)**	(.04)**			(.06)	(.07)
Δio_{t-4}				.02				-.11
				(.03)				(.06)*
$\sum \Delta p$	0.50	0.51	0.63	0.50	0.04	-0.16	-0.19	-0.17
	(.04)***	(.04)***	(.05)***	(.06)***	(.06)	(.07)**	(.09)**	(.10)*
$\sum \Delta io$	0.17	0.25	0.21	0.20	0.43	0.61	0.54	0.46
	(.03)***	(.03)***	(.03)***	(.04)***	(.05)***	(.05)***	(.06)***	(.06)***
AR(1)	0.00	0.58	0.00	0.01	0.00	0.36	0.02	0.07
AR(2)	0.20	0.60	0.00	0.00	0.00	0.01	0.65	0.00
AR(3)	0.00	0.00	0.00	0.09	0.40	0.62	0.87	0.04
AR(4)	0.00	0.00	0.00	0.71	0.60	0.09	0.42	0.14
F Stat	36.2	29.6	23.0	15.4	0.47	3.21	3.12	1.42
Prob	0.00	0.00	0.00	0.00	0.49	0.04	0.03	0.23
Observations	754	725	696	667	754	725	696	667

Note: OLS regressions of log price growth or change in IO share on indicated variables. ***, **, and * denote significance at the 1, 5 and 10 percent levels respectively. "$\sum x$" denotes sum of coefficients associated with variable x. "$AR(j)$" indicates the p-value of the Arellano and Bond (1991) test for serial correlation in the residuals of order j. "F Stat" is the test statistic for the null that the non-regressor lag coefficients are all zero with the p-value below.

Table 8: Granger-Causality Based on System-GMM

	Δp_t				Δio_t			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Δp_{t-1}	.45	.33	.35	.40	.02	-.003	.10	.16
	(.05)***	(.05)***	(.05)***	(.06)***	(.08)	(.11)	(.09)	(.11)
Δp_{t-2}		.12	.12	.09		-.22	-.10	-.17
		(.03)***	(.04)***	(.06)		(.10)**	(.11)	(.17)
Δp_{t-3}			.20	.26			-.24	-.37
			(.06)***	(.07)***			(.14)*	(.22)*
Δp_{t-4}				-.32				-.19
				(.09)***				(.20)
Δio_{t-1}	.22	.16	.21	.17	.60	.50	.52	.45
	(.02)***	(.04)***	(.03)***	(.04)***	(.05)***	(.08)***	(.09)***	(.10)***
Δio_{t-2}		.12	.12	.11		.22	.25	.24
		(.03)***	(.03)***	(.04)***		(.08)***	(.08)***	(.09)***
Δio_{t-3}			-.10	-.07			-.15	-.13
			(.05)**	(.06)			(.07)**	(.07)*
Δio_{t-4}				.05				-.02
				(.05)				(.08)
$\sum \Delta p$	0.45	0.45	0.67	0.43	0.02	-0.22	-0.24	-0.58
	(.05)***	(.06)***	(.08)***	(.17)**	(.08)	(.12)*	(.18)	(.44)
$\sum \Delta io$	0.22	0.28	0.23	0.26	0.60	0.72	0.62	0.55
	(.02)***	(.03)***	(.04)***	(.06)***	(.05)***	(.06)***	(.05)***	(.14)***
J-stat	28.9	28.7	28.2	28.4	29.0	28.9	28.7	26.6
dof	140	138	134	126	140	138	134	126
AR(1)	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
AR(2)	0.99	0.01	0.37	0.68	0.001	0.14	0.39	0.54
AR(3)	0.00	0.00	0.01	0.37	0.05	0.43	0.85	0.77
AR(4)	0.00	0.00	0.00	0.52	0.78	0.95	0.89	0.79
F Stat	83.8	62.6	35.4	16.8	0.07	2.87	1.29	0.89
Prob	0.00	0.00	0.00	0.00	0.80	0.07	0.30	0.48
Observations	754	725	696	667	754	725	696	667

Note: System-GMM estimates of log price growth or change in IO share on indicated variables. ***, **, and * denote significance at the 1, 5 and 10 percent levels respectively. See Table 7 for descriptions of "$\sum x$," "$AR(j)$" and "F-stat". "J-stat" indicates Hansen-Sargan test statistic for the overidentifying restrictions, where "dof" is the degrees of freedom of the test. In all cases the p-value is 1.