Peak-Bust Rental Spreads

Marco Giacoletti*

University of Southern California

Christopher A. Parsons†

University of Washington

Landlords appear to use stale information when setting rents. Among over 43,000 California rental houses in 2018-2019, those last purchased during 2005-2007 (the peak) rent for 2-3% more than those purchased during 2008-2010 (bust). Neither house nor landlord characteristics explain this "peak-bust rental spread." To clarify the mechanism, we test cross-sectional predictions from a simple theory of rent-setting. We find empirical support for both *anchoring* and *prospect theory*. In the first, past sales prices distort landlords' current estimates of house values/rents. In the second, monthly payments establish (recurring) reference points, against which gains or losses are measured.

First version: February 12, 2019

This version: November 12, 2019

Keywords: Anchoring, learning from experiences, prospect theory, liquidity constraints, residential rents, house prices, behavioral biases in real estate

JEL Classification: D40, G00, G40, R31

*Marshall School of Business, University of Southern California. **Email:** mgiacole@marshall.usc.edu

†Foster School of Business, University of Washington. **Email:** cap1975@uw.edu

1 Introduction

Consider a landlord who has just received a notice to vacate from the existing resident. How does she determine what rent to ask of a new potential tenant? Like any retailer, she faces a key tradeoff: a higher rent offers more revenue in the event of a transaction, but due to uncertain demand, also increases the probability of the house not renting. The optimal rent balances these marginal effects.

However, a number of unique features of real estate imply potential departures from the fully rational benchmark described above. First, about half of U.S. rentals are owned by "mom and pop" investors who may lack the sophistication, information, or cognitive resources to set rents efficiently.[1] Second, unlike financial markets, in which rational traders can bet against others' mistakes, real estate markets are comparatively inefficient. Segmentation, high transactions costs, and the inability to short-sell limit arbitrage opportunities available to rational investors. Third, since no two properties are exactly alike, and because information on the cross-section of prices is not readily accessible, landlords prone to valuation errors, or who use non-informative heuristics, may have a hard time realizing any errors they might make.

In this paper, we explore whether stale information — a landlord's exposure to aggregate house prices over a decade ago — influences the rent she sets today.[2] Among online rental listings for over 43,000 houses in California from December 2018 to March 2019, we find that landlords having last purchased their houses at the peak of the real estate boom (2005-2007) charge rents 2-3% higher than landlords who acquired their houses in the ensuing bust (2008-2010). This difference, which we refer to as the *peak-bust rental spread*, suggests that 5-10% of the price difference between peak and bust sales prices (25-40% in California) remains durably imprinted in market rents.

[1] https://www.huduser.gov/portal/pdredge/pdr-edge-frm-asst-sec-061118.html

[2] Personal experiences have been linked to a wide variety of financial/economic decisions and outcomes. See Nagel and Malmenadier (2011, 2012, 2016), Bernile, Bhagwat, and Raghavendra (2017), Nagel, Malmenadier, and Yan (2018) and Bailey, Cao, Kuchler, Stroebel, and Wong (2018).

Two immediate questions arise. The first is whether historical house prices really are stale, or whether they tell us about current house quality, services provided by landlords, or other attributes that matter to tenants. The second regards the mechanism. Even if the timing of historical purchases is orthogonal to rental services, why do landlords appear to take them into account at all?

Starting with the first question, a key aspect of our empirical design is that the measure of staleness does not use the actual historical sales price of individual houses. In a seminal paper studying loss aversion among condominium sellers in Boston, Genesove and Mayer (2001) describe the econometric challenges associated with using actual purchase prices as reference points against which gains and losses are measured. In particular, sales prices will reflect durable, but unobservable aspects of housing quality. Thus, were we to estimate a cross-sectional regression of current (t) house (i) rents $R_{i,t}$ on observable characteristics $X_{i,t}$ and historical ($\tau < t$) sales prices $p_{i,\tau}$, the concern is that past sales prices may proxy for street noise, views, micro-environment, or other attributes not captured by hedonic characteristics X.[3]

In hopes of minimizing this issue, our main analysis features cross-sectional regressions of rents on a sequence of "acquisition vintage" dummy variables (in addition to standard hedonic attributes), and thus exploits only fluctuations in house price *index values* as our source of variation in purchase prices. While still possible that residual differences in unobserved house quality exist across vintages, the size of any bias is limited to that caused by unobserved heterogeneity between large, diversified groups containing thousands of houses each.

We address remaining cross-vintage heterogeneity through a number of subsequent tests. Oster (2016), building on Altonji, Elder, and Taber (2005), develops and describes a methodology to assess the importance of omitted variable bias. The procedure compares the

[3]Genesove and Mayer (2001) characterize both the size and direction of the bias introduced by unobserved house characteristics in estimates of loss aversion among sellers. Though simulations suggest that the absolute size of such bias is small, their analysis also accounts for unobserved quality by including past sales residuals (in logarithms) in regressions of current asking prices. For robustness, we also employ this approach (see Table 5).

coefficient(s) of interest — here, the difference in average rents across acquisition vintages — when increasingly informative sets of controls are added to the regression. This cross-regression sensitivity to observables (i.e., how much the estimated coefficient declines for a unit increase in R^2) is then used to place bounds on the true, bias-free effect. Intuitively, the more stable the estimated coefficient, particularly with a substantial increase in R^2, the less likely that unobservables represent a significant source of bias. Applying this methodology to our setting, we find that the peak-bust rental spread is virtually constant across specifications, despite the fully-controlled regressions achieving explanatory power over 85%. To explain the peak-bust rental spread, remaining unobservables would have to exhibit sensitivity to rents over six times higher compared to that involving observables.

While house characteristics do not appear to explain much of the peak-bust rental spread, perhaps differences in landlords and/or the quality of services offered by landlords vary across acquisition vintages in a way consistent with the observed patterns. Indeed, we do find that the composition of landlords is strongly related to acquisition vintage, with bust-acquired houses being more likely to be purchased by corporate entities and/or professional investors. However, this variation goes the wrong way toward explaining the result, because all else equal, investors tend to ask higher, rather than lower, rents. In any event, the magnitude of any effect is negligible; controlling for landlord type has almost no impact on our main estimate (2.4%).

To further address this issue, we conduct a placebo test using Texas. Although data on landlords is much sparser, price-to-rent ratios were and remain much lower in Texas rather than in California, and thus, would likely have been equally (or more) attractive to investors "reaching for yield" in the wake of the crisis. Critically however, Texas did not experience the same house price volatility as did California through the early 2000s (see Figure 4). Finding a complete absence of a comparable peak-bust rental spread thus suggests that realized historical price volatility, rather than the changing composition of investors, is the key determinant of the observed patterns.

The remainder of the paper addresses the second question, i.e., the specific reason(s) why fluctuations in real estate prices appear to influence rents over a decade later. We explore three possibilities. The first is *anchoring*, whereby irrelevant information — aggregate real estate prices a decade ago — are nevertheless regarded as informative by landlords, and thus lead to pricing distortions relative to fundamentals.[4] In contrast to the alternatives featuring reference-dependent utility described below, anchoring makes landlords strictly worse off compared to a fully rational benchmark. We demonstrate this using a simple model of rent-setting adapted from Lazear (1986). Landlords anchored to high (low) historical values ask too much (little) in rents, which leads to both a lower (higher) probability of renting, and lower expected revenue (in both cases). Indeed, analysis of time-on-market confirms this prediction: houses acquired at the peak sit in inventory about 8% longer than those acquired during the bust.

The second and third possibilities rely not on landlords making valuation errors *per se*, but on reference-dependent utility. Prior work on house sales demonstrates that sellers expecting to realize a nominal loss, relative to the purchase price, set higher asking prices during sales (Genesove and Mayer (2001)).[5] Our analysis does not involve house sales, but rather the analog in the rental market. If landlords regard periodic expenses as reference points, then to the extent that historical prices affect monthly payments,[6] similar considerations may cause past sales to exert a nearly *continuous* influence on rental markets, in addition to their punctuated impact on sales.

[4]Similar "memory" effects have been observed among renters in Bordalo, Gennaioli, and Shleifer (2019), which shows that tenants moving to a less (more) expensive metropolitan area tend to rent more (less) expensive apartments compared to non-moving local renters with similar income.

[5]Investors of financial assets also appear unwilling to sell stocks which have experienced nominal price declines since purchase (i.e., losers). For examples of this "disposition effect" in stocks, see Odean (1998), Frazzini (2006) and more recent evidence by Birru (2015). For examples of reference-dependent preferences in other settings, see for example Pope and Simonsohn (2011), Lacetera, Pope, and Sydnor (2012) and Allen, Dechow, Pope, and Wu (2016).

[6]For several reasons, mortgages and property taxes being the most important, how much a landlord paid for a house influences the periodic expenses she faces in the future. In California, property taxes are not marked to market annually, but per Proposition 13 of 1978, are instead limited to appreciate no more than 2% annually. Consequently, current taxes depend on historical prices, and given volatility in aggregate prices, the time since purchase.

The two broad ways in which monthly payments can influence landlord's rent-setting decisions are through *prospect theory preferences* (see Kahneman and Tversky (1979, 1991, 1992)), and through what we call *liquidity management*.[7] In the first, the slope of a landlord's utility function, with respect to rent, is particularly steep for values near, but below, her monthly payment. In the second, landlords with limited cash-on-hand may find it costly to finance deficits between rent and monthly expenses, perhaps by delaying consumption[8] or liquidating other assets.

To establish empirical predictions, we return to the rent-setting model, but rather than anchoring the landlord's perceived home value to purchase prices, purchase prices determine reference points, against which gains and losses are measured. Rent falling below a landlord's reference point causes a utility loss. As we show, the shape of the loss function is crucial, leading to sharp empirical predictions depending on the type of reference dependence. Prospect theory preferences lead landlords to be loss averse, which we parametrize by a loss function that increases, but at a decreasing rate as the rent-cost deficit increases (i.e., losses exhibit diminishing sensitivity). The liquidity management loss function also increases with this deficit, but at an increasing rate: although landlords don't like having to pay \$500 out-of-pocket to maintain a rental, paying \$1,000 is more than twice as painful.

The key insight that emerges is that when a landlord expects rent to not exceed her reference point (i.e., she expects a loss), the optimal rent depends on two competing effects. On the one hand, a higher rent reduces the size of the cost-rent deficit if the house rents, but also leads to a lower probability of renting, which exposes the landlord to an even larger loss. Under prospect theory, the first effect dominates, while with liquidity management, the second effect is larger. Consequently, loss-averse landlords always set rents *above* the risk-neutral benchmark — a specific manifestation of prospect theory's general prediction of

[7] In the sales market, Stein (1995) and Genesove and Mayer (1997) characterize the impact of home equity on asking prices. Liquidity constrained sellers facing losses may be unable to move, or forced to "move down," due to inadequate down payments for subsequent mortgages.

[8] Argyle, Nadauld, and Palmer (2017) present evidence that monthly-payment considerations impact the demand for automobiles.

risk-seeking in the domain of losses — whereas liquidity-constrained landlords choose *lower* rents, attempting to hedge against an even larger loss incurred in the no-rent state.

To test this idea, we seek to compare two groups of landlords who own similar houses, but face different monthly expenses. For this analysis, our main comparison is between landlords that choose different leverage at time-of-purchase, while controlling for purchase price. The control group consists of landlords employing little to no leverage, with loan-to-value (LTV) ratios below 50%. For this group, the sum of all monthly obligations would typically be substantially lower than market rents, and thus, the loss function would have a minimal impact on rent-setting. In the intent-to-treat group are landlords with LTV ratios exceeding 95%, among which we expect most landlords to face paper losses on a monthly basis.[9] Among these landlords, the loss function is expected to play a role, and with a direction that reveals the mechanism.

The comparison of rents asked by the high- and low-LTV groups points strongly to prospect theory, rather than liquidity management. Controlling for house quality, landlords with little to no leverage ask almost 5% less than landlords in the high-LTV group. As further evidence against liquidity management, we show that the peak-bust spread is present also for properties that are more likely to be held by deep pocketed investors, for whom liquidity concerns are likely less important. For this test, we split our sample based on zip code-level income and/or the level of monthly rent, finding a peak-bust rental spread at least as large in wealthy areas and for expensive houses, which is inconsistent with liquidity management playing a primary role.

Importantly, while the results above appear supportive of a prospect theory interpretation, they do not rule out anchoring to historical prices also playing a role in rent-setting. To address this possibility, what was the control group in the prior test (low-LTV borrowers) now

[9] We use intent-to-treat to describe the high-LTV cohort because beyond LTV at time-of-purchase, we lack further data on landlords' monthly expenses, and therefore cannot precisely measure gains and losses for individual landlords. To the extent that the high-LTV group contains landlords not experiencing losses, the estimated magnitudes will be biased downward.

constitutes the sample of interest. Because we expect a large majority of low-LTV landlords to be cash-flow-positive, the loss function is largely irrelevant, implying that sensitivity to acquisition vintages would indicate anchoring. Indeed, we find that low-LTV landlords manifest the same peak-bust rental spread behavior as that observed in the full sample. Together, these results suggest that both anchoring, as well as prospect theory considerations, likely play a role in rent-setting in the residential real estate market.

The rest of the paper proceeds as follows. Section 2 introduces a simple model illustrating the biases and distortions that can lead to past purchase prices to affect current rents set by landlords. The last part of the section formalizes the empirical predictions we aim to test in the data. Section 3 presents our dataset of rental listings, and the key empirical results on the effects of historical purchase prices (i.e., acquisition vintages) on house rents. Section 4 studies differences in time-on-market for peak- and bust-acquired rental houses. Section 5 provides tests intended to disentangle the different economic mechanisms driving our results. Section 6 concludes.

2 A Simple Model of Rent Setting

We begin with a stylized, one-period model, based on Lazear (1986), involving a landlord attempting to rent out a house she owns.[10] The purpose of the model is to fix ideas for the empirical tests that follow, which relate the price a landlord paid (proxied using the year it was purchased) to its asking rent, as well as to the length of time it sits on the market.

As a benchmark, we first characterize in Section 2.1 the optimal rent when the landlord: 1) holds correct beliefs about the distribution of her house's value, and 2) maximizes only expected revenue. Then, we allow for stale information to become relevant by weakening each of these assumptions. In the first case (Section 2.2), the landlord comes to hold potentially

[10] While our model is stylized, it captures the key economic tradeoff at the core of studies of rent-setting behavior in the residential market. See, for example, Stull (1978) and Allen, Rutherford, and Thomson (2009).

incorrect beliefs because she anchors to historical purchase prices. Specifically, a landlord who paid a high (low) price is optimistic (pessimistic) about her house's value, and thus sets a rent that is too high (low). In the second case (Section 2.3), although maximizing expected rent remains a consideration, she also cares about rent exceeding a pre-specified reference level, which depends on stale information (i.e., the historical purchase price).

2.1 Benchmark Case

There is a single landlord, whose house has value $v \sim U[0,1]$ to any potential tenant. The landlord, knowing only the distribution of v but not its realization, posts a take-it-or-leave-it rent R, for which the tenant can rent the house ($v \geq R$), or if not ($v < R$), search for another one. There is a single period, all parties are risk-neutral, and the reservation utility for landlords and tenants are both zero. The landlord's problem is:

$$\max_R (1 - F[R])R = \max_R (1 - R)R, \tag{1}$$

which gives $R^* = \frac{1}{2}$, and expected revenue of $\frac{1}{4}$. The landlord's ex-ante lack of information about v means that she cannot price discriminate, and thus trades off the probability of renting $(1 - R)$ against the rent conditional on a successful match (R). The expected surplus from trade is $\frac{1}{4} + \int_{R^*}^{1} (v - R^*)dv = \frac{3}{8} < E[v] = \frac{1}{2}$. The efficiency loss obtains from situations where $R > v$, but nevertheless, the house fails to rent.

Note that the maximization problem ignores any costs the landlord may face, and thus equates expected revenue with expected profit. For determining R^*, this makes no difference for costs that do not depend on rent, such as periodic maintenance, taxes, or mortgage service. On the other hand, the solution will generally differ if the cost of providing rental services is not constant across potential tenants, or if rent acts as a screening device.

2.2 Anchoring to Historical Prices

Now consider that the landlord of house i believes (mistakenly) that $v \sim U[\Delta, 1+\Delta]$, where $\Delta \in \{-X, X\}; 0 \leq X \leq \frac{1}{2}$. The first case, $\Delta = -X$, corresponds to undervaluation, whereas the second case, $\Delta = X$, implies a landlord who is positively biased about the distribution of v. The landlord will now set

$$R^* = \frac{1+2\Delta}{2}, \tag{2}$$

which is either too high, or too low, by X.

If the house is undervalued by the landlord, the expected revenue is $\frac{1}{4} - X^2$, and the landlord's expected revenue is strictly worse than in the benchmark case. Although the house now rents more often than before, this is more than offset by lower rent in the event of a successful match. The tenant is, of course, better off, with an expected utility of $\int_{R^*}^{1}(v - R^*)dv = \frac{1}{2}(X + \frac{1}{2})^2 > \frac{1}{8}$. The total surplus is $\frac{3}{8} + \frac{X}{2} - \frac{X^2}{2}$, increases as the landlord becomes more pessimistic about her house's value: as X increases, R^*, and consequently the probability of the house not renting, both approach zero. In the limit of $X = \frac{1}{2}$, the house always rents ($R^* = 0$), and the tenant captures the entire surplus.

On the other hand, when the house is overvalued, the landlord sets $R^* = \frac{1+2X}{2}$. The landlord's expected revenue remains $\frac{1}{4} - X^2$, and the tenant's expected utility is $\frac{1}{8} - \frac{X}{2} + \frac{X^2}{2}$. In contrast to the prior case, the landlord's bias now reduces the total surplus. Now, as X approaches $\frac{1}{2}$, $R \to 1$, the combined surplus goes to zero, as the probability of renting the house diminishes to zero.

2.3 Reference-Dependent Utility

As an alternative, return to the benchmark case, where the landlord holds correct beliefs about v. However, in addition to maximizing expected profit, the landlord is influenced by a reference point $C > 0$, against which rent R is directly compared. If $R \geq C$, the landlord's objective function is as above, i.e., she maximizes expected revenue as in Equation

(1). However, for $R < C$, the landlord experiences an additional utility loss $g(C - R) > 0$, where $g'(\cdot) > 0$.

There are two primary ways to motivate the existence of reference point C, and the landlord's loss function around it, $g(\cdot)$. One is through *liquidity management*, whereby C constitutes the landlord's periodic (e.g., monthly) expenses related to the rental property. To the extent that the rent R fails to exceed C, the landlord must fund the deficit from other sources. Here, g captures the frictions associated with such cash-flow management, such as liquidating other assets (i.e., transactions costs), delaying other consumption, or the opportunity cost of the landlord's time. Under the liquidity-management hypothesis, we specify convexity in the loss function, or $g'' > 0$. With this formulation, we wish to capture the intuition that modest rent-cost deficits are relatively easy for landlords to accommodate, but grow increasingly costly with the size of the shortfall.

Reference-dependence can also arise through *prospect theory preferences* (Kahneman and Tversky (1979), (1991), (1992)). A key attribute of prospect theory is "loss aversion," whereby the loss function g is particularly steep for small, positive values of $C - R$. In contrast to the loss function motivated by liquidity management considerations, the loss function is concave ($g'' < 0$) under prospect theory.[11]. Although the landlord always prefers smaller deficits (recall that $g' > 0$ in all cases), under prospect theory, she is particularly concerned with minimizing the chance of incurring even small losses.

Under both liquidity management and prospect theory, the reference point, C, may depend on the price a landlord paid for her house. One reason, which applies to every property in our sample, is due to peculiarities in the California tax code. Proposition 13, passed by the state legislature in 1978, mandated that property taxes on newly sold property be no more than 1% of the purchase price; furthermore, subsequent percentage increases in taxes were limited to 2%. The combination of these factors means that property tax bills remain closely tied to

[11] Of course, because R enters with a negative as the argument, g exhibits convexity with respect to R, consistent with traditional formulations of prospect theory utility functions.

original acquisition prices, even decades later. A second reason is due to interest service on mortgages. In general, higher purchase prices translate to larger mortgages, and therefore higher monthly payments.

With reference-dependent utility, the landlord's optimization problem becomes:

$$\max_R (1-R)R - (1-R)g(C-R) - Rg(C), \tag{3}$$

which we present in expanded form to highlight the difference with the benchmark case, represented by the second and third terms. The last captures the additional loss in expected utility if the house does not rent. Given that $g'(.) > 0$, and that $g(0) = 0$, it follows that $g(C)$ is the maximum utility loss the landlord can experience, which occurs with probability R. The second (middle) term captures the landlord's loss in the event that the house rents, $g(C-R)$, scaled by $1-R$, the chance the house rents at posted rent R.

The optimal rent is now:

$$R^* = \begin{cases} \frac{1-g(C)}{2} & \text{if } C \leq \hat{C} \\ 1 - \frac{1+g(C)-g(C-R^*)}{2+g'(C-R^*)} & \text{otherwise,} \end{cases}$$

where $\hat{C}$ is defined by

$$g'(0)[\hat{C}-1] + 2\hat{C} + g(\hat{C}) - 1 = 0. \tag{4}$$

The cutoff $\hat{C}$ characterizes the value of the reference point below which, in equilibrium, $R^* > C$. In this region, C still plays a role in the landlord's decision, but only insofar as she reduces the rent, in order to avoid the maximum loss, $-g(C)$, which is experienced if the house fails to rent. This leads to our first proposition.

Proposition 1 *For* $\hat{C} >> C \approx 0$*,* $R^* \approx \frac{1}{2}$*.*

When the landlord's reference point is small, the optimal rent is close to the no-reference point benchmark, which follows from $g(0) = 0$. In our later empirical analysis, we will develop empirical proxies for C, which generate cross-sectional dispersion in the size of landlords' reference points. For sufficiently small values of these proxies, the rents we observe constitute our approximation to the benchmark case with no reference dependence.

Our second proposition pertains to higher values for C. For $C > \hat{C}$, the landlord will experience a utility loss not only if the house doesn't rent, $-g(C)$, but also a (smaller) loss if it does, $-g(C-R)$. This additional consideration leads to an upward, discontinuous rent shift at $\hat{C}$. Crucially, the size of this shift, as well as how R^* varies with further increases in C, depend on the shape of $g(\cdot)$.

Proposition 2 *For $C > \hat{C}$, under prospect theory, $R^* > \frac{1}{2}$, the benchmark with no reference dependence. Likewise, in the liquidity management case, $R^* < \frac{1}{2}$.*

To build some intuition for Proposition 2, consider first the linear case, $g''(\cdot) = 0$, where $g(C-R) = K(C-R)$, where $K > 0$. With a linear specification, changes in R have equal, but opposite effects on the utility loss experienced by the landlord. On the one hand, a lower value for R decreases her chance of experiencing the maximum possible loss $(-KC)$, but at the same rate, increases her chance of experiencing a smaller loss, $-K(C-R)$, with the marginal effects with respect to C exactly offsetting. Thus, although the presence of the reference point unambiguously decreases the landlord's expected utility, K is an affine transformation of the expected revenue with no reference dependence, the specific value of C plays no role, and R^* equals the benchmark value of $\frac{1}{2}$.

This is not true when the loss function $g(\cdot)$ exhibits curvature. Unlike the case in which $g''(\cdot) = 0$, both a concave (corresponding to prospect theory) or convex loss function (liquidity management) lead to an unambiguous relation with the benchmark case with no reference dependence. Provided that $C > \hat{C}$, then

$$R^* > \frac{1}{2} \Leftrightarrow g'(C-R^*) > 2[g(C) - g(C-R^*)],$$

which can be written as

$$\frac{g'(C-R^*)}{\frac{g(C)-g(C-R^*)}{R^*}} > 2R^*.$$

Observing that the left hand side is the ratio of the local slope of g evaluated at $C-R^*$, compared to the average slope from C to $C-R^*$, it follows directly that $R^* > \frac{1}{2} \Leftrightarrow g''(\cdot) < 0$, and $R^* < \frac{1}{2} \Leftrightarrow g''(\cdot) > 0$.

Intuitively, Proposition 2 illustrates a key trade-off between the two effects of reference dependence on the optimal rent-setting behavior of landlords. When the loss function is fairly flat around the reference point, but steepening as the $R-C$ deficit widens ($g''(\cdot) > 0$), landlords are mostly concerned about renting the house and avoiding large losses, even if this requires dropping the rent, and accepting a modest loss. Such modest losses are, of course, exactly what landlords with prospect theory preferences ($g''(\cdot) < 0$) find least tolerable on the margin. For them, the diminishing sensitivity of the loss function creates an incentive for risk taking in the loss region, a key implication of prospect theory. In our context, such risk-taking is operationalized via higher rent: even though this reduces the probability of renting (and reduces expected revenue), it is offset by the benefit of narrowing $C-R$, in the event that the house successfully rents.

2.4 Discussion

The model delivers several testable implications. The main set of predictions focuses on how rents may reflect stale information, to the extent that historical purchase prices distort landlords' views about their houses' values, or establish a reference point around which utility depends. Our central claim is that our measure of staleness — the year a house was most recently purchased — exhibits minimal correlation with fundamentals, both observed and unobserved. That is, in the cross-section of landlords indexed by i, we take the year of purchase as positively correlated with Δ_i and/or C_i, but crucially, not with v_i. A major component of our analysis establishing the main empirical findings (Section 3) is to rule out correlation between acquisition timing and v_i.

Subsequent analysis focuses on disentangling the key economic mechanisms driving distortions in house rents. In particular, Section 5 explores the implications of Propositions 1 and 2. Within the reference-dependence framework, we seek to distinguish between prospect theory and liquidity management. We develop further tests to isolate the effects of anchoring, independent of reference-dependent preferences.

A further set of predictions — which arise with both anchoring and reference dependence — is centered around the insight that rents overpricing should be associated with a lower probability of renting, which we proxy using the length of time a house sits on the market (Section 4). For this analysis in particular, it is important to emphasize that the model is highly stylized, specifically with regard to the timing (one period). In reality, houses that don't rent in one period have additional chances thereafter, though we believe that ignoring dynamics is largely innocuous in our context. As in the analysis of retail pricing in Lazear (1986), when the landlord can test the market over multiple periods, she sets a high initial rent, and gradually drops it over time. Though a multi-period setting will change the level of posted rents relative to the single-period benchmark, the key distortions introduced by anchoring or reference-dependent utility will still arise, thus allowing stale information to still influence rents.

Finally, note that in the model posted rents are take-it-or-leave-it offers which cannot be negotiated. While tenants may occasionally be able to successfully negotiate for lower rent, conversations with listing agents suggests that with current market conditions in California's major cities (where the vast majority of our houses are located), concessions are unusual, and when they do occur, modest. In any event, our main findings are impacted only to the extent that such off-listing bilateral negotiations differ depending on the purchase year of the property.

3 Estimation of the Peak-Bust Rental Spread

3.1 Rental Listings Data

Our dataset is constructed by collecting rental listings from major online rental listings services. The final dataset of advertised rents is based on listings for houses located in the state of California, collected from December 2018 through March 2019. We exclude from the dataset houses and apartments for which the number of bedrooms, the number of bathrooms, size or information on the last sale date are not available. We also exclude houses last purchased before 1980, houses that have a last purchase date more recent than their construction date (presumably a data error), and listings that have been online for more than 300 days at the time of collection.

Our main dataset consists of 44,237 rental listings which, in our analysis, we pool and treat as a single cross-section. Figure 1 shows the geographic distribution of these listings across California zip codes, while Table 1 reports summary statistics of the data. The majority of the properties are single family residencies (SFRs). Roughly 17.7% of properties are condo or apartments, 7.7% are townhouses, and 5.5% are multi-family residencies. Less than 1% are studios. Slightly less than 40% of listings are posted by real estate agents, suggesting that roughly 60% correspond to smaller, non-institutional investors, consistent with the overall fraction of mom-and-pop investors observed in national surveys. At the time of data collection, on average, a typical listing has been online for 39 days (median 23 days). The average monthly rent in the dataset is slightly above $3,500 (median $2,750), with a standard deviation of $3,400.

3.2 Purchase Vintages and Current House Rents

Our key empirical exercise consists of comparing otherwise similar houses purchased at different points during the peak-bust cycle that occurred from the mid 2000s through the early 2010s. Figure 2 shows some preliminary patterns. For 2-bedroom homes (single family residences, townhouses or apartments) in the city of Los Angeles, we plot the average advertised rent/square foot for a sequence of six, non-overlapping acquisition vintages: 2002-2004, 2005-2007, and so on, until 2017-2019. The blue bars report average rent per square foot for each purchase vintage, and for comparison, the red line is the S&P Case-Shiller index value for Los Angeles over the same years. The correlation is easily apparent, with rents being higher for houses purchased at the peak (2005-2007) and/or during the recovery (after 2013), and lower for houses purchased during the bust (2008-2013).

An immediate concern is that houses acquired at different times may differ in terms of quality, location, services, or other relevant attributes. We thus generalize the patterns above in a regression framework that includes controls for various dimensions of house quality:

$$\log(R_i) = \Gamma I_{p,i} + \mathcal{B}_{ctrl} X_{ctrl,i} + a_z + e_i, \tag{5}$$

where R_i is the monthly rent for listing i, $I_{p,i}$ is a vector of acquisition vintage dummies, $X_{ctrl,i}$ is a vector of house i's characteristics, and a_z is a family of zip code fixed effects. To keep the number of categories manageable (as we will report the coefficients for each vintage in our tables), and to focus more precisely on the specific years of interest, all properties acquired prior to 1990 are lumped together, as are properties purchased from 1990-1994 and 1995-1998. Starting in 1999, we group vintages into 3-year increments: 1999-2001, 2002-2004, and continuing through 2017-2019. The latter group is the reference category in our estimates.

The set of house characteristics, $X_{ctrl,i}$, is extensive, and includes dummies for the number of bedrooms and bathrooms in the property, as well as indicators for properties that offer only street parking (no garage or parking slot), for properties that have shared laundry, for townhouses, condos or apartments and studios. Other characteristics include the log square feet size of the property, age and age squared, and dummies for rental properties that forbid pets, that do not have air conditioning, and that provide a refrigerator, a dishwasher, hardwood flooring, and/or forced air heating and central air conditioning. For condos and apartments, we also include the floor on which the apartment is located. In some specifications, we also control for the log number of days the property has been listed online (log number of days-in-inventory), as well as whether the listing was posted by a real estate agent.

Columns 1, 2, 3 and 4 of Table 2 report parameter estimates for different specifications of Equation (5), depending on which control variables are included. Relative to the 2017-2019 acquisition vintage, individual estimates of Γ are negative and statistically significant, indicating that houses most recently acquired rent for the highest amounts. One potential explanation for this is that purchase dates may correspond with renovations, so that houses acquired more recently may have more, and/or more contemporary, updates not captured by $X_{ctrl,i}$. Consequently, if the historical price trend were monotonic, it would be difficult to disentangle the "stale price" effect from that related to recent renovations.

California represents a near-ideal setting for making the distinction. Because prices dropped so precipitously from 2008-2010 compared to the three years prior (2005-2007), comparing rents between these particular vintages represents a lower bound on the stale-price effect, since the older vintage is also the most (historically) expensive. Starting with the first column, the difference between peak (2005-2007) and bust (2008-2010) vintage is $5.5 - 3.0 = 2.5\%$, and $5.1. - 3.0 = 2.1\%$ when compared to the subsequent three years (2011-2013). Both of these differences are statistically significant at the 1% level.[12]

Figure 3 provides a graphical representation of the vintage-to-vintage patterns, plotting the individual elements of Γ based on the fully controlled specification in column 4 of Table 2 (top panel), against changes in the historical price index over the same years (bottom panel).[13] When compared, the two panels of Figure 3 show a clear positive co-movement.

[12]The reader might be concerned that part of the difference between the 2005 to 2007 and the 2008 to 2010 dummy is driven by houses that last transacted in 2008. At that point, the housing market had not yet reached its bottom, and the purchase prices for some rental properties might still have been (relatively) high. In Table A.1 in Appendix A we repeat the analysis excluding all properties last sold in 2008. The results are virtually identical to the ones reported in Table 2.

[13]Whereas Figure 2 uses the Case-Shiller Index for Los Angeles, Figure 3 calculates a similar index, replacing the dependent variable (log rent) with the log purchase price of each listed property i in Equation

Returning to Table 2, the last two columns show the results when, instead of using vintage dummies to capture extreme highs and lows of the historical price index at time-of-purchase, we use these index values directly. In column 5, the acquisition dummy indicators are replaced with a single variable, the logarithm of $p_{last,zip}$, which corresponds to zip code-specific price indices (ZHVI) published and maintained by Zillow.[14] Column 6 uses the logarithm of $p_{last,CA}$, the average of the Case-Shiller values for San Diego, Los Angeles and San Francisco from January 1987 through June 2019. In both cases, with zip code fixed effects, the coefficients can be interpreted as within-zip elasticities, i.e., the percent rent by which two houses in the same zip code would differ on average, for every percent change in the price index prevailing when they were respectively acquired.

Interestingly, the coefficients in columns 5 and 6 are almost identical, with an implied elasticity of 0.054, and t-statistics exceeding ten. Observing that average prices in California dropped by 25%-40% (depending on location) from the peak in Summer 2006 through the bottom in Fall 2011, the estimated peak-bust rental spread of 2.3% implies that 5-10% of historical price fluctuations remain imprinted in current market rents, even a decade later.

3.3 Unobserved Quality

An important concern is that the cross-sectional patterns we document might be driven, at least in part, by unobserved heterogeneity across housing units. If either house quality or services vary in the same way as do fluctuations in historical prices, beyond what is captured by controls, then the relation between stale prices and rents will be spurious. We begin in Section 3.3.1, describing the key identification problem, as well as how our main source of

(5). This allows us to use the entire sample, including houses not in major metropolitan areas, for which Case-Shiller estimates are not generated. Parameter estimates are reported in Table A.2 in nominal (columns 1 and 3) and real (columns 2 and 4) terms, the latter using December 2018 as the reference date. The figure looks nearly identical if we use an equal- or weighted-average of Case-Shiller index values for all major cities in California.

[14] ZVHI values are based on hedonic, proprietary machine-learning algorithms. The reported frequency is monthly, and because ZHVI is not uniformly populated, coverage can vary by zip code. The longest available series start in April 1996.

price variation (acquisition years) offers important advantages for minimizing the concern. The following two sections then present additional tests intended to address remaining heterogeneity in house quality (3.3.2) and landlords/services quality (3.3.3).

3.3.1 Why Acquisition Vintages Rather than Historical Prices?

Recall that our key empirical strategy, shown in Equation (5), relates asked rents to the year(s) in which a property was purchased, not to the original purchase price. By focusing on fluctuations in past index values, our hope is to isolate price variation largely orthogonal to unmeasured quality attributes, and thus identify an effect on rent-setting that is not due to omitted heterogeneity in quality.

To see the advantage of using index values rather than historical prices, note that rental houses are financial assets, allowing us to value them using standard techniques. At any time t, the price of financial asset i, $p_{i,t}$, is the sum of its future cash flows $C_{i,t+1}$, $C_{i,t+2}$,..., each deflated by a discount rate r_{t+1}, r_{t+2},... that compensates investors for time preferences and risk. For simplicity, assume that, at time t, prices are formed with the expectation of a constant growth rate in cash flows, $\frac{C_{t+1}}{C_t} = \bar{g}_t$, and a constant discount rate $\bar{r}_t$. With these assumptions, the price of housing unit i at time t can be written as

$$p_{i,t} = \frac{E[C_{i,t+1}]}{\bar{r}_t - \bar{g}_t} = \frac{\rho E[R_{i,t+1}]}{\bar{r}_t - \bar{g}_t}, \tag{6}$$

where $\bar{r}_t$ and $\bar{g}_t$ are the discount and growth rates prevailing at time t, which are assumed to be identical for all houses in the cross-section.[15] While possible that financial risk differs across the state of California, most of this variation should be captured by zip code fixed effects, leaving only risk differences between houses within a few miles of each other, which we presume to be small.[16]

[15]Numerous studies find that growth rates in rents are small and stable, on the order of 0.5-1% above inflation. See Shiller (2006), Campbell, Morris, Gallin, and Martin (2009), and Giglio, Maggiori, and Stroebel (2015).

[16]For example, it is plausible that house values are more sensitive to the macroeconomy in large cities.

$R_{i,t+1}$ is the expected rent, of which fraction $1-\rho$ is lost to taxes and other costs. Let $log\,(R_{i,t+1}) = \phi_{i,t} + \beta_t X_{i,t}$, where $\beta_t X_{i,t}$ represents a mapping of observable characteristics onto the flow of rental services, and $\phi_{i,t}$ a multiplier representing quality attributes not spanned by $X_{i,t}$. Rearranging terms, we have:

$$\epsilon_{i,t} = log(p_{i,t}) - \beta_t X_{i,t} = log(\rho) + \phi_{i,t} - log(\bar{r}_t - \bar{g}_t). \tag{7}$$

The left hand side represents the pricing error from a hedonic regression of sales prices on observable house characteristics. These residuals can be attributed to differences in taxes and/or operating efficiency (ρ), unobservable quality (ϕ), and capitalization ("cap") rates $(\bar{r} - \bar{g})$.[17]

Equation (7) clarifies the reason why including the historical price of house i, $p_{i,\tau<t}$, in a time-t cross-sectional regression of rents is potentially problematic. Because $p_{i,\tau}$ is a function of $\phi_{i,\tau}$, a positive correlation with $\phi_{i,t}$ means that variation in $p_{i,\tau}$ will also capture variation in current quality, invalidating the interpretation of $p_{i,\tau}$ as a measure of staleness. Fortunately, the same decomposition also indicates a natural solution.

Since the nature of the identification problem is *cross-sectional* — house j has a higher historical price residual than house i because it is better in some way — achieving identification from the *time series* offers an appealing alternative. As indicated by Equation (7), fluctuations in capitalization rates $(r - g)$ induce fluctuations in asset prices, even when the house's cash flow characteristics, particularly ϕ, do not change. Indeed, an important goal of commercially published real estate indices, such as Case-Shiller or Zillow's "Home Value Index" (ZHVI), is to account for both observable and unobservable dimensions of house quality, and thus

Standard asset pricing models would indicate that investors demand a risk premium for holding assets with such higher exposure, and should be reflected in the discount rate.

[17] In real estate, it is common to quote prices as multiples of *net operating income (NOI)*, which approximate the cash flows available to equity investors (debt costs are not part of NOI). The ratio $\frac{NOI}{p_t}$ is equal to the capitalization rate, or "cap rate." Whereas NOI does not perfectly correspond to free cash flow (the numerator in Equation (6)) — for example ignoring investment costs — this discrepancy makes little difference for the discussion here.

capture time-series variation in capitalization rates, rather than compositional changes.[18]

Accordingly, in hopes of breaking the linkage between historical prices and unobservable quality, our analysis focuses on historical fluctuations in index pricing which, because such fluctuations exhibit such a distinct temporal signature in California, can also be inferred from examining year-of-purchase directly. Indeed, recall from the last two columns of Table 2 that the estimated rent-index sensitivities are nearly identical to those implied by the cross-vintage comparisons.

Although vintage-level unobservable quality may vary, a number of factors suggests that such heterogeneity is likely too small to meaningfully affect our results. Most importantly, the main source of variation is limited to that differing, on average, between acquisition vintages containing thousands of houses each. Moreover, for our estimates to be biased upward, any such vintage-level average differences must be negatively correlated with the capitalization rate prevailing at time t, in order to generate a positive correlation with historical prices. The following two sections show that accounting for cross-vintage heterogeneity usually goes in the opposite direction than what would be needed to account for the peak-bust rental spread in the data. Consequently, omitted house (or landlord) characteristics do not appear capable of providing a credible alternative to the interpretation of stale prices impacting current rents.

3.3.2 House Heterogeneity

Mean differences among observables. To the extent that unobservable heterogeneity between vintages is correlated with differences in observable characteristics, comparing house characteristics may help alleviate the concern that peak-acquired houses are better than bust-acquired ones. Table 3 reports various means of house characteristics for the four acquisition vintages 2005-2007, 2008-2010, 2011-2013, and 2014-2016. In general, differences

[18] The Case-Shiller index is constructed from repeated sales. Zillow's proprietary algorithm is inherently cross-sectional, and may include measures of quality not incorporated in standard hedonic analysis.

are minimal, and when they are significant, often go the opposite way of the main finding. For example, bust-acquired houses tend to be more newly constructed (about two years) than either of the immediately following vintages in 2011-2013 and 2014-2016, despite having lower rents. The main comparison of interest, however, is between the peak- and bust-acquired houses, between which we fail to find any statistically significant difference in size, number of bedrooms, number of bathrooms, likelihood of being a condominium/apartment (versus a single family home), age, laundry services (for apartments) and parking policy.

Statistical diagnosis of unobservables. More formal treatment of unobservable variable bias is developed by Altonji, Elder, and Taber (2005), and subsequently extended in Oster (2016). The goal is to place plausible bounds on the size of omitted variable bias, given structural assumptions about the effects of both unobservable and observable factors on the outcome variable of interest (here rents). The typical procedure is to estimate regressions with progressively more controls, measuring: 1) how much the R^2 increases, and 2) how much the coefficient of interest (here the coefficients on purchase/acquisition vintage dummies) is reduced. The final step is to extrapolate to a hypothetical regression that controls for all relevant factors, i.e., those both observed by the econometrician, as well as those not observed. In this hypothetical estimation, which involves the maximum possible explanatory power, R^2_{max}, the question is whether we would still estimate a significant effect for the coefficient(s) of interest, and if so, its magnitude.[19]

The answer depends on the extent to which unobservables behave similarly to observables, in terms of their marginal impact on the estimated treatment effect. If this sensitivity, δ, is equal to one, then the effect of unobservables is identical to that of the observables, and the extrapolation to R^2_{max} respects the "slope" between the long and short regressions. For example, if control variables increase the R^2 from 30% to 50%, with β declining from 1 to 0.7, one would hypothesize that in a theoretical regression with $R^2_{max} = 90\%$ –an increase twice that observed from the short to long regressions ($2 \times 20\% = 40\%$)– the coefficient of interest

[19] R^2_{max} might still be less than 1 due to, for example, measurement error, but is still expected to be very high.

would suffer an additional decline of $2 \times (1 - 0.7) = 0.6$, for an estimated bias-free treatment effect of 0.1. Likewise, the cases $\delta < 1$ and $\delta > 1$ correspond, respectively, to unobservables having a smaller and larger impact on the treatment effect than observables.

Because δ is itself unobservable, a common implementation of this procedure is to ask, for a given long-short regression pair, and a hypothetical R^2_{max}, how large would δ need to be in order to fully explain the observed effect. Denoting this threshold $\bar{\delta}$, high values imply a required sensitivity that is larger (in absolute value) than the one with respect to changes in observable characteristics. For more details on the methodology developed by Oster (2016), see Appendix B.

Table 4 presents two variants of this calculation. In column 1, the treatment variable is the "bust" indicator, taking a value of one if the house was purchased at the depth of the crisis (2008-2010). The short regression includes only zip code fixed effects and the other (non-bust) acquisition dummy variables ($R^2 = 62.7\%$), while the inclusion of house characteristics in the long regression increases the R^2 to 85.9%. The negative value for $\bar{\delta} = -8.22$ at $R^2_{max} = 100\%$ reflects the observation that the estimated treatment effect becomes larger (in absolute value), relative to the other acquisition vintages, in the long versus short regression. That is, for the bias-free treatment effect to be zero, not only would unobservables have to display roughly eight times the sensitivity to the treatment effect as do observables, but with opposite sign. Absent such unusual correlation structure, the estimates here suggest that the rent reduction observed among bust-acquired houses is unlikely to be explained by unobserved heterogeneity in housing quality.

The second column adds the peak-vintage coefficient in relief. In addition to a dummy for the bust (2008-10), we also include an indicator for the period that spans both these years, and the peak years immediately proceeding (2005-2010). With both variables in the regression together (along with vintages outside the 2005-2010 window), a significant coefficient on the bust-vintage dummy indicates that, relative to 2005-2007, the years 2008-2010 are associated with lower rent. Column 2 shows this is the case, both in the short (3.0%) and long (2.4%)

regressions. Unlike in column 1, where we observed a strengthening of the effect, column 2 indicates a decline, but the effect is very modest: even if the R^2_{max} were 100%, unobservables would have to exert an influence on the treatment effect over six times larger than that involving observable factors.

Lagged hedonic residuals. As an additional test of the importance of unobservables on the acquisition-vintage fixed effects, we implement the empirical strategy introduced by Genesove and Mayer (2001) and Beggs and Graddy (2009), which uses past sales residuals from hedonic regressions to measure quality attributes not spanned by observable characteristics. We first estimate a regression similar to Equation (5):

$$\log(p_{i,last}) = \mathcal{B}_{ctrl} X_{ctrl,i} + a_z + a_y + v_i, \tag{8}$$

where $p_{i,last}$ is the last purchase price of the house, $X_{ctrl,i}$ is the same vector of controls used in column 4 of Table 2, a_z is a zip code fixed effect, and a_y a year-of-last-sale fixed effect.[20] Denoting $\pi_{i,last}$ as the hedonic estimate from Equation (8) for house $i's$ most recent sale, we then estimate several variants of Equation (5) that incorporate the residual $\log(p_{i,last}) - \pi_{i,last} = v_i$.

Table 5 presents the results. In the first column, note that although the coefficient on the difference in log residuals from past sales $\log(p_{i,last}) - \pi_{i,last}$, is strongly and positively associated with current rent, its inclusion has almost no impact on the sequence of acquisition vintage coefficients. In particular, the estimate of the peak-bust rental spread remains stable at 2.5%, with a p-value less than 1%. Column 2 replaces the acquisition vintage indicators with the logarithm of the last sales price, along with the lagged residual.

Our preferred specification eschews actual historical sales prices due to their potential correlation with unobserved quality. However, the specification in column 2 includes lagged residuals, along with contemporaneous house characteristics, so that the estimate of the

[20] Virtually identical results obtain including separate fixed effects for each zip code $\times$ year-of-last-sale pair.

coefficient on $\log(p_{i,last})$ is driven more by changes in index prices, rather than cross-sectional heterogeneity.

Recall from column 4 of Table 2 that a peak-bust rental spread of 2.3% implies a rent elasticity to historical prices of about 0.054 — i.e., a 10% drop in index prices corresponds to a 0.54% drop in rents. This is similar to, although somewhat lower than, the estimated coefficient on $\log(p_{i,last})$, from column 2, which is 0.083 ($t = 16.7$).[21] Controlling for house age and time-on-market leaves the results unchanged (column 3), as does interacting lagged residuals with purchase vintages, which allows the value of unobservable characteristics to appreciate with the overall price index (column 4).

3.3.3 Landlord Heterogeneity

Rather than peak- and bust-acquired houses differing in terms of their property characteristics, in this section we consider that systematic differences in owners and/or landlords may be responsible for the observed differences. For example, some landlords may provide superior service for their rental properties, and consequently, charge higher rent. A second possibility is that holding costs for empty properties may differ across landlords, and thus, alter the tradeoff between higher asking rents and a lower probability of matching with a tenant.[22] In this section, we consider the joint hypothesis that: 1) the types of owners acquiring rental property varies through the housing cycle, and 2) the predicted direction on asking rents parallels that of the peak-bust rental spread.

Controlling for landlord type. To address this issue, we begin by supplementing our set of rental listings with data from Corelogic deed files, which identifies the name of the most recent

[21] One possible reason, as discussed in Genesove and Mayer (2001) is that in addition to capturing changes in the housing index, and quality attributes not spanned by controls, transactions prices may reflect over- or underpayment relative to fundamentals. See the discussion on pages 1238-1241 of Genesove and Mayer (2001) for a detailed discussion of this issue, and the potential biases introduced.

[22] Note that this case corresponds directly to the version of the model presented in Section 2.3, but where $C < \hat{C}$. In this region, the landlord experiences no loss disutility in the event that the house rents, and thus, the reference point matters only insofar as it changes her reservation utility to $-g(C)$. In this case, rents strictly decrease with $g(C)$.

buyer. Linking each individual listing to the Corelogic data requires an exact property match, resulting in 34,473 listings (78% of the data) where the property owner can be identified. For 25,000 of these properties we can observe the full set of house and rental characteristics. Two designations in the deed records are significant: those for absentee and corporate owners. The former are directly flagged in the deed record files. For the latter, Corelogic identifies a number of these directly, which we supplement by searching for key words in buyer names, such as “LLC,” “corporate,” “investment,” “INC,” and other words frequently associated with corporate entities.

Regarding the first part of the joint hypothesis above, owner/landlord composition varies strongly with aggregate house prices. The fraction of houses with absentee owners at the time of purchase increased sharply during the bust. Whereas 25.5% of listings last purchased in the years from 2005-2007 were originally absentee owned, this increases to 38.3% for properties last purchased from 2008-2010, and again to 53.6% for acquisitions from 2011-2013. Similar patterns are observed for corporations, increasing from 2.5% for 2005-2007, to 11.2% for 2008-2010, and finally to 14% for 2011-2013.[23]

We then return to our benchmark specification in Equation (5), but now control for landlord type. Table 6 presents our results. To ensure that the sample containing landlord information is comparable to the full dataset, the first two columns show the results of re-estimating our baseline specifications from prior analysis, without new variables. The estimated peak-bust rental spread remains very similar (2.2%, $p < 0.01$), both with (column 2) and without (column 1) the lagged hedonic residual included.

We then augment the specification with indicators for absentee and/or corporate owners. Column 3 shows that accounting for absentee owners makes virtually no difference in the rent spread between peak- and bust-acquired houses (2.2%, $p < 0.01$). Moreover, absentee owners — which increased in frequency during the bust — are associated with significantly *higher*

[23]Similar patters in the bust and post-crisis period for several residential real estate markets in the United States are discussed also in Xiao and Xiao (2019).

rather than lower rents, the opposite pattern implied by the peak-bust rental spread. The final column adds an indicator for corporate entities, which we assume are absentee owners, and thus, constitute a subset of the absentee owner group. This distinction, shown in column 4, indicates that of the absentee owners, it is corporations who are most responsible for the higher estimated rent. Yet, despite the inclusion of controls for landlord type, the peak-bust rental spread remains stable at 2.3%, significant at the 1% level.[24]

A placebo. We replicate our analysis by linking acquisition vintages to rents in Texas. Geographically large, populous, growing, and demographically diverse, the U.S. state with the second largest economy represents arguably the best single-state comparison to California (the largest economy), though admittedly important differences remain. One is that relative to California, Texas real estate is considerably less expensive. As of 2019, a market report by *Cushman and Wakefield* indicates that every property class in both commercial and residential (multi-family) real estate has a higher price-to-rent ratio in California's major cities compared to those in Texas, despite prices in the latter having more than doubled over the last decade.[25] Unfortunately, because Corelogic's data on deed records is less detailed compared to California, we cannot explicitly control for landlord characteristics as before. However, given that current income (rather than appreciation) is the prime consideration for corporate real estate investors, it seems reasonable that plummeting bond yields during the crisis — the rate on 10-year U.S. Treasury bonds was over 5% in June 2007 but less than 2.5% in December of 2008 — would have also tilted the composition of Texas's landlords toward having more investors, as observed in California.[26]

Another important difference is that the extreme price volatility observed in California did not occur in Texas. Whereas Los Angeles and San Francisco saw prices rise by 121% and

[24]We have also interacted both landlord types with the acquisition vintage indicators. Though the peak-bust rental spread is present across both landlord groups, the estimates are smaller, noisier and less significant for absentee landlords and corporate landlords.

[25]See http://www.cushmanwakefield.com/en/research-and-insight/2019/us-q1-2019-marketbeat.

[26]Evidence of real estate investors reaching for yield in the U.S. is provided by Glaeser (2013) and Duca and Ling (2018). Korevaar (2019) provides evidence that exploits an historical setting from the 17th and 18th century in the Netherlands.

91% respectively from January 2000 to January 2005, only to fall by 21% and 27% over the following five years, Dallas's Case-Shiller index indicates a rise of only 16% from 2000-2005. More importantly, for all major cities in Texas, prices *rose* from 2005-2010, including Dallas, where real estate values increased by a modest 0.5%. Comparable price changes, based on the "All Transactions Price Data" from the St. Louis Federal Reserve Bank, for Houston were 26% (2000-2005) and 14% (2005-2010), for Austin 21% and 23%, and for San Antonio 25% and 20%.

Together, these observations suggest that if stale prices influence rents, we would not expect to find a comparable peak-bust rental spread in the Lone Star State, whereas if compositional changes in landlord/owners were responsible for the peak-bust spread observed in California, we likely would. Figure 4 reports the results. As with the analogous figure for California, we plot the estimated coefficients for a sequence of purchase/acquisition vintage dummies. The top panel corresponds to a cross-sectional regression of current rents, and the bottom panel to a panel regression, where the dependent variable is the logarithm of the acquisition price for each house. Together, the analysis in Texas shows a fairly smooth trajectory in historical prices, and a complete absence of a peak-bust rental spread between 2005-2007 compared to 2008-2010. In fact, the estimated coefficient for the latter vintage is higher than that for the former, although jointly, no coefficient on any of the acquisition vintage dummies is significant.

4 Time on Market

To this point, we have focused only on the impact of stale information on the rents asked by landlords. However, recall that in the model in Section 2, the probability of a house renting is inversely proportional to its rent. Thus, to the extent that acquisition years distort asking rents relative to fundamental values — through either anchoring or reference dependence — we would also expect to find distortions when examining the time a house sits on the market before renting. More specifically, controlling for quality, houses purchased during the peak (at high prices) are expected to rent more slowly than houses acquired during the bust (at low prices).

To examine this issue, we estimate the number of days each listing was posted online before renting. We make three assumptions. The first is that when a house listing disappears online, this implies that the house has been rented. Although a house's failure to rent may result in it being put up for sale, or in cases of proposed rentals that are currently owner-occupied, simply taken off the rental market, we believe that such situations are rare. The second assumption stems from the way the data set was assembled. We crawled the online portal over consecutive two-week intervals, and thus observe whether a listing disappeared between collection dates, rather than the exact date it rented. When a listing disappears, we assume that it rented halfway between the collection dates. This means that each listing will be mis-measured by seven days on average, with a minimum (maximum) of one (thirteen) day(s). The final assumption is that the online portal occasionally features "zombie" listings, where the house has rented, but the advertisement has not been removed. We therefore drop the roughly 4% of observations with listing times exceeding 300 days. This filter has a minimal impact on the results, and alternative cutoffs give similar estimates.

Overall, the California rental market during the data collection period appears very tight, with over one-third of houses leaving the market within two weeks. Our main interest is whether, conditioned on observables, peak-acquired houses take longer to rent than bust acquired houses. On average, the difference is relatively modest, with the former taking 45 days to rent versus 41 for the latter. Although a relatively small difference, a Kolmogorov-Smirnov statistic rejects the hypothesis that the peak and bust groups are drawn from the same distribution ($p < 0.05$), both of which are plotted in Figure 5. While the difference is small for much of the range, houses acquired during the peak (blue dots) become overrepresented starting at about 50 days, and continuing through the right tail of the distribution.

Regression analysis confirms a small, but statistically significant disparity in the time-to-rent. We estimate:

$$\log(days_i) = \beta_r \log(R_i) + \mathcal{B}_{ctrl} X_{ctrl,i} + a_z + e_i, \tag{9}$$

where $days_i$ is the number of days on market. Although most of our observations correspond to completed spells, approximately 25% remain outstanding as of our final collection date (mid-March 2019). Consequently, in Table 7, we present maximum-likelihood estimates from an accelerated failure time (AFT) model that accounts for right-censoring.[27] $X_{ctrl,i}$ represents the same vector of listing characteristics used in Equation (5), excluding the log of inventory days, and a_z is a zip code fixed effect.

The first two columns of Table 7 indicate that across the entire sample, after controlling for characteristics, more expensive houses take longer to rent. With the logarithm of rent by itself (column 1), the estimate suggests that in the cross-section, a house with 5% higher rent will take 3% longer to clear the market, or about 1.3 days on average. Column 2 shows that this effect is concave, so that a 5% higher rent would lead to a 2.8 days delay on average.

Column 3 focuses specifically on the difference in time-on-market between the peak-and bust-acquired vintages, with only houses acquired from 2005-2010 in the estimation. After controls for house characteristics and zip code, the point estimate is 6%, although with marginal statistical significance ($p < 0.10$). Note that while of the same order of magnitude, the estimate in column 3 (6.3%) is two to four times larger than that implied by either columns 1 or 2. One possible explanation is that despite the attributes spanned by observable characteristics, and those captured by lagged hedonic residuals, rents in the cross-section nevertheless contain information about house quality. However, if this is *not* true for rent disparities caused by stale information — i.e., if this represents pure rent dispersion unaffiliated with quality — then we would expect to see a larger effect on inventory times (as we do).

[27] The residual e_i is assumed to have an extreme value distribution, and the baseline hazard function is Weibull with shape parameter κ. The shape parameter determines whether the hazard rate in the model is increasing ($\kappa > 1$), decreasing ($\kappa < 1$) or constant ($\kappa = 1$) in the number of days on market.

The results in the last column of Table 7, along with the average size of the peak-bust rental spread, can be used to produce a back-of-the-envelope estimate of the loss in expected revenue for a landlord acquiring her house at the bust, compared to one acquiring her house at the peak. At an average rent of $3,524 (the sample average), a 2.3% reduction implies a revenue loss of $972 a year. Although on average the house will rent 6% faster (3 days), this expediency is offset by the lower rent only 3 months into the lease term. Thus, the total, annualized difference in revenue between peak- and bust-acquired rental houses is about $700 on average. While we believe this rough calculation is useful for benchmarking the all-in distortion implied by stale information the rental market, we reiterate that whereas the effects of stale prices on rents appear to be estimated relatively precisely across a variety of specifications, due to the measurement error discussed above, we are less confident in the analogous spread for time-on-market.

5 Why Does Stale Information Influence Rents?

The model in Section 2 suggests two potential reasons why, holding house quality constant, landlords having paid more for their houses might charge higher rent today: reference-dependence (Section 5.1) and anchoring (Section 5.2). In this section, we conduct cross-sectional tests intended to indicate whether one, or both, contribute to the peak-bust rental spread differential.

5.1 Reference Dependence

One reason why stale prices can matter is through reference-dependent utility, whereby landlords experience periodic (e.g., monthly) disutility when the cash flow received from a rental property falls short of a predetermined threshold. A natural candidate for such a threshold is the sum of the landlord's recurring expenses. The most important of these are both linked to the past purchase price: property taxes and mortgage service.[28] The impact of prices on debt service is straightforward, because more expensive houses will, in general, require larger mortgages, with higher periodic payments. The role played by property taxes is specific to California, due to legislation (Proposition 13) that limits increases to 2% annually. Thus, a San Francisco house last purchased in January 2000 for $500,000, if it followed the Case-Shiller Index would be worth about $1,335,000 in October 2019, but for tax purposes, would be valued at $1.02^{19.8} \times \$500,000 \approx \$740,000$. A landlord having purchased the same house in 2000 would pay about $500 less in monthly taxes, compared to one that purchased it recently for current market value.

The goal of this section is to test Proposition 2, which indicates that reference-dependence may either increase, or decrease, asking rents, depending on the curvature of the loss function. Provided that the reference point C is not too small, landlords face a tradeoff associated with setting a high rent: although the realized loss is smaller if the house rents, the probability of the house not renting is larger, which exposes the landlord to an even greater loss. When landlords are loss averse ($g'' < 0$), the steepness of the loss function in the neighborhood of C dominates the landlord's rent-setting decision, and R is set fairly close to C, even though this increases the chance of the house not renting. On the other hand, when landlords are more concerned about insuring against large losses ($g'' < 0$), lower rents are optimal, since they minimize the chance that the house fails to rent. Thus, by examining whether variation in C leads to more, or less, aggressive rent-setting by landlords, we can infer whether loss-aversion, or liquidity management, appears to more accurately capture landlords' preferences.

Fortunately, the data offer a suitable way to generate cross-sectional dispersion in reference points C, while holding other features of the property constant. For a subset of the properties in our sample, Corelogic reports the original amount borrowed by buyers who used a mortgage to finance their house purchase. For example, for the same San Francisco house above, a

[28] Ratnadiwakara and Yerramilli (2017) show that property taxes and mortgage amounts affect listing prices in house sales. However, the authors use property taxes and mortgage amounts as proxies for the total historical sunk cost of a property at the time of sale. They do not study house rents, and consequently do not explore the channel determined by reference-dependent utility.

buyer that uses 80% mortgage financing in 2019 would borrow 80% $\times$ \$1.335M = \$1.07M. At 4% for 30 years, the corresponding monthly payment would be \$6,100. On the other hand, the monthly payment using only 50% debt for the purchase would be only \$3,800. The analysis here controls for the purchase price, and instead focuses on variation in the loan-to-value (LTV) ratio.[29]

We first compare rents, controlling for historical purchase prices, between landlords with low-LTV (50% or less) and high-LTV (95% or higher) ratios. The low-LTV group represents the control group, for which we expect C to play little if any role in the landlord's rent-setting decisions. Recall from Proposition 1 that if C is small, the optimal rent is close to the benchmark without reference dependence.[30] The high-LTV group represents the intent-to-treat group. Among these landlords, we assume that the high interest costs — in addition to taxes and other expenses — tip the balance such that the typical highly-levered landlord cannot avoid a monthly "loss" ($R < C$) at current rents.

Although we lack detailed data on maintenance expenses for each property, back-of-the-envelope calculations suggests that the LTV cutoffs used above are reasonable for identifying landlords likely to experience monthly gains or losses. The average rent in our sample is \$3,500 per month. Allocating 20% to property maintenance and other miscellaneous expenses (e.g., exterior maintenance), the average rental house yields perhaps \$2,800 monthly before taxes and mortgage service. As described above, property taxes in California depend on purchase, not market, prices. Using the average historical purchase price of \$590,000, the monthly property tax bill will be about \$550 per month, leaving \$2,250 prior to mortgage service. Using a 30-year term, and an interest rate of 4%, the LTV which generates a monthly

[29] We exclude from the data observations for which the LTV ratio at the time of the last purchase is equal to zero. We make this choice because $LTV = 0$ may imply that the property was purchased with cash, or that the buyer obtained financing through a channel alternative to the mortgage market. Such alternative forms of debt financing are not recorded in the Corelogic data.

[30] Specifically, recall that if $C < \hat{C}$, then in equilibrium, the house rents for more than the reference point (i.e., $R > C$), and $g(R - C) = 0$. Although the model predicts that rents for $0 < C < \hat{C}$ will be lower than rent in the no-reference-dependence case, we assume that for very low LTV values, C is small enough that this is negligible.

payment (principal and interest only) equal to the rent is 77%.[31] We thus select LTV cutoffs roughly equidistant from this threshold, in hopes of grouping landlords into those likely to face a monthly deficit versus surplus.[32]

Among the approximately 4,000 landlords with LTV ratios either below 50% or above 95%, we estimate:

$$\begin{aligned}\log(R_i) = \phi_{LTV,high} I_{LTV_{i,last}>0.95} + \phi_p \log(p_{i,last}) + \rho(\log(p_{i,last}) - \pi_{i,last}) + \\ + \mathcal{B}_{ctrl} X_{ctrl,i} + a_z + u_i,\end{aligned} \tag{10}$$

where $LTV_{i,last}$ is the loan-to-value (LTV) ratio for the house at the time of purchase. The key covariate is the dummy transformation of LTV, $I_{LTV_{i,last}>0.95}$, which takes a value of one when the LTV is greater than 0.95, leaving the low-LTV group as the reference category. Importantly, note that in contrast to our benchmark estimates, we now control explicitly for lagged purchase prices, $\log(p_{i,last})$, and instead focus on variation in monthly payments through variation in LTV. As a control for unobserved heterogeneity in house quality, we also include the lagged hedonic residual from Equation (8), $\log(p_{i,last}) - \pi_{i,last}$.

The first column of Table 8 indicates a positive and significant coefficient estimate for $\phi_{LTV,high}$. Holding purchase price constant, landlords having borrowed 95% or more set rent 4.5% ($p < 0.01$) higher than those with LTV ratios less than 50%. Column 2 adds interactions between lagged hedonic residuals ($\log(p_{i,last}) - \pi_{i,last}$) and acquisition vintages, allowing the rental value of unobserved heterogeneity to appreciate with the housing index since purchase. However, consistent with our primary findings that include the lagged residual, this inclusion makes virtually no difference for rent-setting.

The next three columns show the results when we include all observations for which

[31] For this calculation, we use 4%, which corresponds to (approximately) the minimum rate available to investors over the last 15 years. By doing so, we assume that landlords having initially borrowed when rates were higher would have subsequently refinanced, and thus, lowered their monthly payments.

[32] A landlord borrowing 95% of the purchase price would face a monthly payment of $2,880, requiring the landlord to contribute $630 to cover the shortfall. In contrast, a landlord with 50% LTV would have a monthly surplus of about the same amount.

we have LTV data, not just those at the extremes of the distribution. Columns 3 and 4 maintain the dummy variable specification, with the reference category now being houses for which $0.50 < LTV \leq 0.95$. The estimates here, which parallel the controls employed in columns 1 and 2, indicate that the rent-LTV relation is fairly uniform across the distribution. Moving from the high-LTV group to the middle-LTV group is associated with a reduction in asking rents of 2.25%, with another drop of 2.34% when transitioning to the low$-LTV$ group. Column 5 shows the result when the raw value of LTV is included as a covariate. With a t-statistic of nine, the coefficient on LTV suggests that an increase in the LTV ratio of 0.25 — similar to moving from the middle category to either extreme in columns 1 and 2— leads to a rent increase of about 2%.

In addition to the analysis involving monthly payments in Table 8, we present two additional pieces of evidence in Tables A.3 and A.4 in the Appendix. While we lack wealth and/or income data on landlords, particularly for non-corporations for which we expect loss averse preferences to be most likely, we use average house rents (Table A.3) and zip code-level income (Table A.4) as a crude proxy for the financial position of landlords.[33] Here, the idea is that the maximal per-period loss possible – the house not renting – would likely have a smaller utility impact on deep-pocketed versus poorer and/or financially constrained landlords. If so, then according to Proposition 2, reference-dependence based on liquidity management would predict an *inverse* relation between rent and index values at time of purchase – the opposite of what we observe – and in addition, attenuating as landlord wealth increases. Instead, what both tables indicate is that the peak-bust rental spread is not only present among zip codes with higher average income and for more expensive houses, but is stronger than in the complementary samples. Together with the evidence on monthly payments, these findings are consistent with loss averse landlords minimizing cost-rent deficits provided that the house rents, and contrary to liquidity-constrained landlords attempting to hedge large losses by

[33]We provide separate estimates of the peak-bust rental spread for listings: with monthly rent above and below median, and: from zip codes with income above/below median, based on estimates from the 2016 SOI Individual Income Tax Statistics published by the Internal Revenue Services.

dropping rents.

5.2 Anchoring

A second reason that acquisition vintages could influence rents is through anchoring. In textbook treatments of asset pricing and investment decisions, only forward-looking information regarding expected cash flows and opportunity costs should matter. However, several decades of experimental and field studies indicate evidence that investors allow stale and/or otherwise irrelevant information — anchors — to enter financial decisions.[34] Applied to pricing and valuation decisions, Ariely, Loewenstein, and Prelec (2003) note that although the "vast majority of anchoring experiments in the psychological literature have focused on how anchoring corrupts subjective judgment, not subjective valuation or preference....because **valuation typically involves judgment**....it is not surprising that valuation, too, can be moved up or down by the anchoring manipulation [Emphasis added]."

The classic anchoring experiments intentionally involve signals that are irrelevant to the task at hand, and are *understood* as irrelevant by the subjects.[35] In our context, the irrelevance of the anchor — historical index values at the time a house is purchased — may be less obvious to landlords. If so, then what looks like "pure" anchoring may, in fact, be landlords using irrelevant signals from the past to infer current fundamentals, but simply not recognizing their lack of information content.

Of course, this begs the question of why landlords appear to regard such non-informative, stale information as relevant for their pricing decisions. One possibility is simply that they fail to adequately distinguish between cross-sectional and time-series price dispersion, perhaps because cross-sectional variation is more familiar or salient, compared to changes in index

[34] Kahneman and Tversky (1974) were the first to document anchoring in the laboratory. For a review of the literature on anchoring in financial decisions, see Shiller (1999). For more recent contributions, see Beggs and Graddy (2009), Bucchianeri and Minson (2013), Maniadis, Tufano, and List (2014) and Dougal, Engelberg, Parsons, and Van Wesep (2015).

[35] For example, Ariely, Loewenstein, and Prelec (2003) demonstrate a strong, positive association between willingness-to-pay for items like Belgian chocolates and cordless keyboards with the last two digits of a person's social security number, which became salient after writing them down.

values over time. Recall from the discussion in Section 3.3.1 that index-level variation (i.e., across acquisition vintages) is due mostly to discount rates,[36] whereas cross-sectional price variation (i.e., between two houses at a given point in time) reflects mostly differences in quality. Conditional on observables, this means that in the cross-section, houses with high price residuals are expected to have high rent residuals as well, both due to unobservable attributes like street noise, views, micro-location attributes like trees, sun exposure, and so forth.

While speculative, it seems to us plausible that a rule-of-thumb, derived from cross-sectional experiences could, when conflated with the time series, generate distorted beliefs in the way described. For example, a landlord paying attention to the local market would likely browse nearby listings, visit open houses, or otherwise obtain real-time information about the market. Such activities inherently involve cross-sectional comparisons, and may give rise to a heuristic such as "better houses (i.e., those with more desirable features) both sell and rent for more." That is, of course, true, and follows from Equation (7). However, this rule-of-thumb is less appropriate when making time-series comparisons, e.g., "My house sold for a lot several years ago, so it must be a better house, and accordingly should rent for more." Thus, landlords who erroneously attribute discount rate variation to unobservable quality may come to hold distorted beliefs about their house's value, the key assumption in Section 2.2.

Observationally, while sharing the key prediction of classical anchoring — irrelevant information being treated as relevant — the behavior described here may be more accurately characterized as "anchoring via misattribution," in the sense that landlords' reliance on irrelevant signals stems from a misunderstanding of the information structure, rather than from alternative psychological foundations for anchoring.[37]

[36]This is analogous to evidence from the stock market. Cochrane (2008) argues that because price-to-dividend ratios do not reliably predict dividend growth, that variation in expected rates of return (risk premia) represents the most important source of stock market volatility.

[37]See, for example, Mussweiler and Strack (2001), Epley and Gilovich (2006), Bergman, Ellingsen, Johannesson, and Svensson (2010) and other contributions mentioned in the literature review by Furham and Boo

This interpretational caveat notwithstanding, we attempt to separate the effects of anchoring from reference-dependence again relying on the LTV data, but for a different purpose. Now, in Table 9, rather than comparing high-LTV and low-LTV borrowers, we focus only on the latter, re-analyzing the peak-bust rental spread for only this subset. Column 1 shows the results for the $LTV \leq 50\%$ cohort,[38] whereas column 2 uses a cutoff of 75%. In both cases, for which we expect loss aversion to play a minimal role in rent-setting, the peak-bust rental spread is at least as large as our benchmark estimates. Columns 3 and 4 repeat the analysis using the zip code-level index at the time of purchase. In these cases as well, historical index prices appear strongly related to current rents, suggesting that even when landlords expect positive cash flows from renting, stale information over a decade ago continues to impact rent-setting decisions.

6 Conclusion

Among approximately 43,000 rental houses in California, we ask whether stale signals — historical house prices over a decade prior — influence the rent-setting behavior of landlords today. A summary of our findings is:

- Landlords of houses acquired when aggregate prices were high (2005-2007) set rents 2-3% higher than landlords having bought houses during the ensuing correction (2008-2010). Alternatively, current rents are strongly related to historical values of the Case-Shiller index (or other similar measures) at the time of purchase.

- Virtually none of this effect is explained by observable house quality or landlord characteristics, despite the explanatory power (in rent regressions) from the latter being large. This likewise suggests that unobserved sources of heterogeneity are unlikely to explain the key finding.

(2011).

[38] Estimates of the purchase/acquisition vintage dummies for this cohort are noisier than in the previous tables, due to the limited sample size.

- Houses acquired at the peak take longer to rent.
- Part of the mechanism works through a desire for rent to exceed monthly payments. Controlling for purchase price, landlords who borrowed more heavily to finance their purchases ask higher rent. This finding is consistent with prospect theory (loss aversion), and inconsistent with liquidity constraints.
- Part of the mechanism is independent of monthly payments. Even for houses purchased using very little debt, such that rent will far exceed monthly payments, rents remain strongly related to historical index prices. This finding is consistent with anchoring.

On the empirical side, studying rents rather than prices represents, in our view, an important contribution to the existing behavioral literature. As Barberis notes when describing the challenges of taking prospect theory to the data, "In any given context, it is often unclear how to define precisely what a gain or loss is, not least because Kahneman and Tversky (1992) offered relatively little guidance on how the reference point is determined." For this reason, it is perhaps unsurprising that most studies of reference dependence in financial markets study consecutive transactions in the same market — e.g., buying and selling a house (Genesove and Mayer (2001)), buying and selling a stock (Odean (1998)), issuing debt at different times (Dougal, Engelberg, Parsons, and Van Wesep (2015)), and so forth. The evidence presented in this paper suggests that in addition to these "intramarket" effects, reference points appear capable of *spanning* markets: those established in one context (sales) can influence outcomes in another (rents).

To the extent that this generalizes, the impact of reference points is potentially much larger than previously recognized. For example, do landlords that buy when aggregate prices are high spend more on maintenance expenses? Or, even beyond real estate, when short-selling, do institutions having bought their shares for high prices (relative to the current market) charge higher fees when lending their shares? In such settings, reference points may exert a near-continuous "flow" influence, in addition to their saltatory impact at time-of-sale.

For assessing magnitudes, this is particularly important for real estate: whereas only about 6% of U.S. homes sell every year (see Piazzesi and Schneider (2009)), almost half of housing units in California are rented. Future work might quantify the extent to which rigidity in rental markets — even in aggregate — can be attributed to the type of stale information effects described herein.

On the theoretical side, although simple, the model develops clear, testable implications that shed light onto how landlord utility functions incorporate reference points. Whereas loss averse preferences prescribe that landlords will set high rents when faced with high reference points, the opposite prediction obtains (lower rents) for reference dependence arising from liquidity constraints. Finding evidence for the former hypothesis thus provides empirical support for one of prospect theory's central predictions: risk-seeking in the domain of losses. It is worth emphasizing the specialness of the real estate market for identifying such risk-taking behavior, since uncertainty about demand plays a crucial role. Without this feature — consider stocks or credit markets, where finding a buyer is virtually costless — there would be no trade off between prices (rents) and the probability of a transaction occurring. In this respect, the limited depth of the rental market offers an econometric advantage for assessing landlord's willingness to accept higher risk (the house not renting) in exchange for the chance of narrowing any loss.

References

Allen, Eric J., Patricia M. Dechow, Devin G. Pope, and George Wu, 2016, Reference-Dependent Preferences: Evidence from Marathon Runners, *Management Science*.

Allen, Marcus T., Ronald C. Rutherford, and Thomas A. Thomson, 2009, Residential Asking Rents and Time on the Market, *Journal of Real Estate Finance and Economics*.

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber, 2005, Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools, *Journal of Political Economy*.

Argyle, Bronson, Taylor Nadauld, and Christopher Palmer, 2017, Monthly Payment Targeting and the Demand for Maturity, *Working Paper*.

Ariely, Dan, George Loewenstein, and Drazen Prelec, 2003, Coherent Arbitrariness: Stable Demand Curves without Stable Preferences, *Quarterly Journal of Economics*.

Bailey, Michael, Ruiqing Cao, Theresa Kuchler, Johannes Stroebel, and Arlene Wong, 2018, The Economic Effects of Social Networks: Evidence from the Housing Market, *Journal of Political Economy*.

Beggs, Alan, and Kathrin Graddy, 2009, Anchoring Effects: Evidence from Art Auctions, *American Economic Review*.

Bergman, Oscar, Tore Ellingsen, Magnus Johannesson, and Cicek Svensson, 2010, Anchoring and Cognitive Ability, *Economics Letters*.

Bernile, Gennaro, Vineet Bhagwat, and Rau P. Raghavendra, 2017, What Doesn't Kill You Will Only Make You More Risk-Loving: Early-Life Disasters and CEO Behavior, *Journal of Finance*.

Birru, Justin, 2015, Confusion of Confusions: A Test of the Disposition Effect and Momentum, *Review of Financial Studies*.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer, 2019, Memory and Reference Prices: an Application to Rental Choice, *American Economic Review Papers and Proceedings.*

Bucchianeri, Grace W., and Julia A. Minson, 2013, A homeowner's dilemma: Anchoring in residential real estate transactions, *Journal of Economic Behavior and Organization.*

Campbell, Sean D., Davis A. Morris, Joshua Gallin, and Robert F. Martin, 2009, What Moves Housing Markets: A Variance Decomposition of the Rent-Price Ratio, *Journal of Urban Economics.*

Cochrane, John, 2008, The Dog That Did Not Bark: A Defense of Return Predictability, *Review of Financial Studies.*

Dougal, Casey, Joseph Engelberg, Christopher A. Parsons, and Edward D. Van Wesep, 2015, Anchoring on Credit Spreads, *Journal of Finance.*

Duca, John V., and David C. Ling, 2018, The Other (Commercial) Real Estate Boom and Bust: The Effects of Risk Premia and Regulatory Capital Arbitrage, *Journal of Banking and Finance.*

Epley, Nicholas, and Thomas Gilovich, 2006, The Anchoring-and-Adjustment Heuristic, *Psychological Science.*

Frazzini, Andrea, 2006, The Disposition Effect and Underreaction to News, *Journal of Finance.*

Furham, Adrian, and Hua Chu Boo, 2011, A Literature Review of the Anchoring Effect, *The Journal of Socio-Economics.*

Genesove, David, and Christopher Mayer, 1997, Equity and Time to Sale in the Real Estate Market, *American Economic Review.*

Genesove, David, and Christopher Mayer, 2001, Loss Aversion and Seller Behavior: Evidence from the Housing Market, *Quarterly Journal of Economics.*

Giglio, Stefano, Matteo Maggiori, and Johannes Stroebel, 2015, Very Long-Run Discount Rates, *Quarterly Journal of Economics*.

Glaeser, Edward L., 2013, A Nation of Gamblers: Real Estate Speculation and American History, *American Economic Review*.

Kahneman, Daniel, and Amos Tversky, 1974, Judgement under Uncertainty: Heuristics and Biases, *Science*.

Kahneman, Daniel, and Amos Tversky, 1979, Prospect Theory: An Analysis of Decision under Risk, *Econometrica*.

Kahneman, Daniel, and Amos Tversky, 1991, Loss Aversion in Riskless Choice: A Reference-Dependent Model, *Quarterly Journal of Economics*.

Kahneman, Daniel, and Amos Tversky, 1992, Advances in Prospect Theory: Cumulative Representation of Uncertainty, *Journal of Risk and Uncertainty*.

Korevaar, Matthijs, 2019, Reach for Yield and Real Estate: The Impact of Investor Demand on House Prices, *Working Paper*.

Lacetera, Nicola, Devin G. Pope, and Justin R. Sydnor, 2012, Heuristic Thinking and Limited Attention in the Car Market, *American Economic Review*.

Lazear, Edward, 1986, Retail Pricing and Clearance Sales, *American Economic Review*.

Maniadis, Zacharias, Fabio Tufano, and John A. List, 2014, One Swallow Doesn't Make a Summer: New Evidence on Anchoring Effects, *American Economic Review*.

Mussweiler, Thomas, and Fritz Strack, 2001, The Semantics of Anchoring, *Organizational Behavior and Human Decision Processes*.

Nagel, Stefan, and Ulrike Malmenadier, 2011, Depression Babies: Do Macroeconomic Experiences Affect Risk Taking?, *Quarterly Journal of Economics*.

Nagel, Stefan, and Ulrike Malmenadier, 2012, Macroeconomic Experiences and Expectations, *Federal Reserve Board (Academic Consultants Meeting)*.

Nagel, Stefan, and Ulrike Malmenadier, 2016, Learning from Inflation Experiences, *Quarterly Journal of Economics*.

Nagel, Stefan, Ulrike Malmenadier, and Zhen Yan, 2018, The Making of Hawks and Doves, *Working Paper*.

Odean, Terrance, 1998, Are Investors Reluctant to Realize Losses?, *Journal of Finance*.

Oster, Emily, 2016, Unobservable Selection and Coefficient Stability: Theory and Evidence, *Journal of Business Economics and Statistics*.

Piazzesi, Monika, and Martin Schneider, 2009, Momentum Traders in the Housing Market: Survey Evidence and a Search Model, *American Economic Review*.

Pope, Devin G., and Uri Simonsohn, 2011, Round Numbers as Goals: Evidence from Baseball, SAT Takers, and the Lab, *Psychological Science*.

Ratnadiwakara, Dimuthu, and Vijay Yerramilli, 2017, Sunk-Cost Fallacy and Seller Behavior in the Housing Market, *Working Paper*.

Shiller, Robert, 2006, Long-Term Perspectives on the Current Boom in Home Prices, *The Economist's Voice*.

Shiller, Robert J., 1999, Chapter 20: Human Behavior and the Efficiency of the Financial System, (in *Handbook of Macroeconomics*,).

Silverman, B., 1984, *Density Estimation for Statistics and Data Analysis*. (Chapman and Hall).

Stein, Jeremy, 1995, Prices and Trading Volume in the Housing Market: A Model with Downpayment Constraints, *Quarterly Journal of Economics*.

Stull, William J., 1978, The Landlord's Dilemma: Asking Rent Strategies in a Heterogeneous Housing Market, *Journal of Urban Economics*.

Xiao, Steven, and Serena Xiao, 2019, Market Power and Consumer Welfare: Evidence from Home Rental Markets, *Working Paper*.

Tables

	N. Obs.	Mean	Median	Std. Dev.
size (sqft)	44,237	1,753	1,548	1,052
1 bed	44,237	0.0564	0	0.2308
2 bed	44,237	0.2410	0	0.4277
3 bed	44,237	0.3915	0	0.4881
4-plus bed	44,237	0.3111	0	0.4629
1 bath	44,237	0.1636	0	0.3699
2 bath	44,237	0.3776	0	0.4848
3 bath	44,237	0.3323	0	0.4710
4-plus bath	44,237	0.1266	0	0.3326
condo/apt.	44,237	0.1765	0	0.3813
townhouse	44,237	0.0770	0	0.2667
multi-family	44,237	0.0549	0	0.2277
studio	44,237	0.0068	0	0.0823
age	38,648	41.53	38	25.85
listing by agent	44,237	0.3944	0	0.4887
inventory days	42,811	39.44	23	48.05
monthly rent ($)	44,237	3,524	2,750	3,409
last purchase price ($)	42,938	590,092	408,000	702,820
last purchase year	44,237	2010.6	2013	7.12

Table 1: Summary statistics for the rental listings dataset.

	(1) Log Rent	(2) Log Rent	(3) Log Rent	(4) Log Rent	(5) Log Rent	(6) Log Rent
$\log(p_{last,ZIP})$					0.0536***	
					(0.00511)	
$\log(p_{last,CA})$						0.0539***
						(0.00461)
dummy 1980s	-0.129***	-0.104***	-0.105***	-0.0945***		
	(0.0151)	(0.00913)	(0.0122)	(0.0123)		
dummy 1990;1994	-0.0927***	-0.0802***	-0.0742***	-0.0657***		
	(0.0130)	(0.00777)	(0.00994)	(0.00999)		
dummy 1995;1998	-0.0510***	-0.0630***	-0.0538***	-0.0487***		
	(0.0100)	(0.00622)	(0.00758)	(0.00770)		
dummy 1999:2001	-0.0510***	-0.0649***	-0.0575***	-0.0543***		
	(0.00875)	(0.00574)	(0.00699)	(0.00698)		
dummy 2002:2004	-0.0381***	-0.0506***	-0.0425***	-0.0400***		
	(0.00715)	(0.00471)	(0.00523)	(0.00522)		
dummy 2005:2007	-0.0280***	-0.0341***	-0.0288***	-0.0275***		
	(0.00786)	(0.00456)	(0.00518)	(0.00517)		
dummy 2008:2010	-0.0554***	-0.0580***	-0.0536***	-0.0518***		
	(0.00678)	(0.00407)	(0.00450)	(0.00453)		
dummy 2011:2013	-0.0503***	-0.0486***	-0.0452***	-0.0442***		
	(0.00615)	(0.00399)	(0.00432)	(0.00431)		
dummy 2014:2016	-0.0174***	-0.0168***	-0.0154***	-0.0161***		
	(0.00557)	(0.00366)	(0.00389)	(0.00389)		
age			-0.00154***	-0.00149***	-0.00146***	-0.00147***
			(0.000416)	(0.000398)	(0.000414)	(0.000399)
age-sq			9.77e-06**	9.66e-06***	9.72e-06**	9.68e-06***
			(3.88e-06)	(3.67e-06)	(3.86e-06)	(3.68e-06)
log(inventory days)			0.0167***	0.0166***	0.0171***	0.0167***
			(0.00165)	(0.00158)	(0.00164)	(0.00158)
log(size)		0.380***	0.378***	0.372***	0.376***	0.372***
		(0.0173)	(0.0187)	(0.0185)	(0.0191)	(0.0185)
agent listing		-0.0101***	-0.0123***	-0.0143***	-0.0155***	-0.0144***
		(0.00240)	(0.00266)	(0.00266)	(0.00273)	(0.00269)
shared laundry		-0.0554***	-0.0530***	-0.0553***	-0.0574***	-0.0553***
		(0.00779)	(0.00833)	(0.00791)	(0.00820)	(0.00789)
townhouse		-0.0966***	-0.107***	-0.106***	-0.105***	-0.106***
		(0.00574)	(0.00648)	(0.00634)	(0.00649)	(0.00635)
condo		-0.110***	-0.120***	-0.123***	-0.122***	-0.122***
		(0.00748)	(0.00884)	(0.00877)	(0.00900)	(0.00877)
multi-family		-0.136***	-0.146***	-0.140***	-0.143***	-0.141***
		(0.00757)	(0.00877)	(0.00863)	(0.00898)	(0.00866)
street parking		-0.0354***	-0.0400***	-0.0521***	-0.0552***	-0.0539***
		(0.0109)	(0.0124)	(0.0123)	(0.0128)	(0.0123)
studio		-0.380***	-0.407***	-0.404***	-0.404***	-0.401***
		(0.0304)	(0.0352)	(0.0346)	(0.0354)	(0.0346)
refrigerator				0.0235***	0.0236***	0.0232***
				(0.00390)	(0.00400)	(0.00390)
dishwasher				-0.00123	-0.00142	-0.00145
				(0.00288)	(0.00293)	(0.00288)
no pets				-0.0188***	-0.0186***	-0.0186***
				(0.00273)	(0.00281)	(0.00273)
hardwood floor				0.0317***	0.0319***	0.0317***
				(0.00276)	(0.00282)	(0.00275)
forced heat				0.0123***	0.0126***	0.0125***
				(0.00292)	(0.00293)	(0.00293)
no AC				-0.0267***	-0.0260***	-0.0261***
				(0.00804)	(0.00777)	(0.00804)
central AC				0.0301***	0.0300***	0.0303***
				(0.00384)	(0.00381)	(0.00383)
floor (condo)				0.00297**	0.00272**	0.00287**
				(0.00126)	(0.00125)	(0.00124)
bedrm dummies	NO	YES	YES	YES	YES	YES
bathrm dummies	NO	YES	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES	YES	YES
F-stat 05:07 - 08:10	10.49	24.65	20.29	20.08	-	-
p-value 05:07 - 08:10	0.0012	0.0000	0.0000	0.0000	-	-
F-stat 05:07 - 11:13	7.87	9.92	9.76	10.51	-	-
p-value 05:07 - 11:13	0.0051	0.0017	0.0018	0.0012	-	-
Obs	43,138	43,138	36,555	36,555	34,748	36,447
R-sq	0.629	0.858	0.857	0.859	0.858	0.859

Table 2: Impact of acquisition vintage on current asked rents (see Equation (5)). In columns from 1 to 4, the Table also reports the F-statistics and relative p-values for two tests. The first one is the test for the null that the dummy coefficient for properties last purchased from 2005 to 2007 and the dummy coefficient for properties last purchased from 2008 to 2010 are equal. The second test is for the null that the coefficients for the 2005 to 2007 and the 2011 to 2013 dummies are equal. In column 5, we replace the acquisition vintage dummies with the log of the ZHVI price index in the month when the house was last purchased and in the zip code where the house is located ($\log(p_{last,zip})$). In column 6, we replace the acquisition vintage dummies with the log of the California house price index constructed by the authors, in the month when the house was last purchased ($\log(p_{last,CA})$). Standard errors are reported in parenthesis and are clustered by zip code.

	2005:2007	2008:2010	2011:2013	2014:2016
size (sqft)	1755.5	1723.8	1736.7	1754.2
	[-1.5674]	-	[-0.7595]	[-1.7650]
fraction 1 bed	0.0600	0.0500	0.0566	0.0644
	[-1.9191]	-	[-1.5135]	[-3.2931]
fraction 2 beds	0.2507	0.2307	0.2277	0.2503
	[-2.0405]	-	[0.3770]	[-2.4754]
fraction 3 beds	0.3900	0.4066	0.3954	0.3741
	[1.4833]	-	[1.1972]	[3.6199]
fraction 4+ beds	0.2993	0.3126	0.3203	0.3111
	[1.2553]	-	[-0.8647]	[0.1752]
fraction 1 bath	0.1618	0.1576	0.1599	0.1625
	[-0.4965]	-	[-0.3266]	[-0.7136]
fraction 2 baths	0.3951	0.3771	0.3887	0.3568
	[-1.6127]	-	[-1.2534]	[2.2821]
fraction 3 baths	0.3239	0.3448	0.3314	0.3422
	[1.9310]	-	[1.4853]	[0.3052]
fraction 4+ plus baths	0.1192	0.1204	0.1199	0.1385
	[0.1677]	-	[0.0841]	[-2.8932]
fraction condo/apt.	0.1702	0.1606	0.1709	0.2032
	[-1.1336]	-	[-1.4548]	[-5.9103]
age	38.61	38.79	40.68	40.26
	[0.3010]	-	[-3.7169]	[-2.8338]
fraction street parking	0.0138	0.0097	0.0107	0.0118
	[-1.6848]	-	[-0.5373]	[-1.0934]
fraction shared laundry	0.0588	0.0496	0.0547	0.0548
	[-1.7868]	-	[-1.1805]	[-1.2562]
Obs	3,331	4,434	7,172	8,721

Table 3: Mean of house characteristics for rental houses last purchased in four different vintages: years from 2005 to 2007, from 2008 to 2010, from 2011 to 2013 and from 2014 to 2016. We report in square brackets t-statistics for a test of the the difference in means between the 2008 to 2010 vintage and each one of the other three vintages.

	(1) Equation (5)	(2) Equation (5) with 2005:2010 dummy
Treatment Coefficient	dummy 2008:2010	dummy 2008:2010
$\bar{\delta}$	-8.2173	6.3474
Coeff. Short	-0.0412	-0.0299
R-square Short	0.627	0.627
Coeff. Long	-0.0518	-0.0238
R-square Long	0.859	0.859
Vintage Dummies in Short Reg.	YES	YES
zip code FE in Short Reg.	YES	YES

Table 4: Statistical diagnosis of unobservables: The Table reports the value of $\bar{\delta}$ that would make the unbiased estimate of the treatment coefficient equal to zero (see Section 3.3.2 and Appendix B). The Table also reports estimates of the treatment coefficients and R-squares for the "short" and "long" regressions. The short regressions include only acquisition vintage dummies and zip code fixed effects in the conditioning information. The long regressions include all controls from column 4 of Table 2. In column 1 the specification of the year of purchase dummies is the same as in Equation (5), and the treatment of interest is the dummy coefficient for properties last purchased in the years from 2008 to 2010. In column 2 the specification is changed. We replace the dummy for the 2005 to 2007 vintage with a dummy equal to one for all houses purchased from 2005 to 2010. In this new specification, the treatment of interest is the dummy for houses purchased from 2008 to 2010, which directly measures the peak-bust rental spread.

	(1) Log Rent	(2) Log Rent	(3) Log Rent	(4) Log Rent
dummy 1980s	-0.0884*			
	(0.0500)			
dummy 1990:1994	-0.0752***			
	(0.0253)			
dummy 1995:1998	-0.0616***			
	(0.0124)			
dummy 1999:2001	-0.0652***			
	(0.00873)			
dummy 2002:2004	-0.0394***			
	(0.00644)			
dummy 2005:2007	-0.0252***			
	(0.00595)			
dummy 2008:2010	-0.0505***			
	(0.00489)			
dummy 2011:2013	-0.0437***			
	(0.00457)			
dummy 2014:2016	-0.0153***			
	(0.00408)			
$\log(p_{last})$		0.0829***	0.0788***	0.0788***
		(0.00495)	(0.00501)	(0.00501)
$\log(p_{last}) - \pi_{last}$	0.148***	0.0655***	0.0696***	0.0427**
	(0.0128)	(0.0118)	(0.0117)	(0.0173)
$(\log(p_{last}) - \pi_{last})$ ×dummy 1980s				0.143**
				(0.0558)
$(\log(p_{last}) - \pi_{last})$ × dummy 1990:1994				-0.0517
				(0.0814)
$(\log(p_{last}) - \pi_{last})$ × dummy 1995:1998				0.109**
				(0.0536)
$(\log(p_{last}) - \pi_{last})$ × dummy 1999:2001				0.0872**
				(0.0388)
$(\log(p_{last}) - \pi_{last})$ × dummy 2002:2004				0.0930*
				(0.0485)
$(\log(p_{last}) - \pi_{last})$ × dummy 2005:2007				0.123***
				(0.0370)
$(\log(p_{last}) - \pi_{last})$ × dummy 2008:2010				-0.0106
				(0.0261)
$(\log(p_{last}) - \pi_{last})$ × dummy 2011:2013				0.0260
				(0.0225)
$(\log(p_{last}) - \pi_{last})$ × dummy 2014:2016				0.0380*
				(0.0211)
age	-0.00124***		-0.000877***	-0.000886***
	(0.000307)		(0.000301)	(0.000306)
age-sq	7.58e-06***		4.90e-06*	4.82e-06*
	(2.63e-06)		(2.53e-06)	(2.61e-06)
log(inventory days)	0.0164***		0.0160***	0.0160***
	(0.00172)		(0.00171)	(0.00171)
additional controls	YES	YES	YES	YES
bedrm dummies	YES	YES	YES	YES
bathrm dummies	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES
F-stat 05:07 - 08:10	14.21	-	-	-
p-value 05:07 - 08:10	0.0002	-	-	-
F-stat 05:07 - 11:13	8.60	-	-	-
p-value 05:07 - 11:13	0.0034	-	-	-
Obs	29,640	29,640	29,640	29,640
R-sq	0.859	0.858	0.860	0.860

Table 5: Impact of acquisition vintage on current asked rent, after controlling for unobservable characteristics, using the approach in Genesove and Mayer (2001). p_{last} is the last purchase price of the rental property, while π_{last} is the hedonic estimate of the log price based on regression Equation (8). The *additional controls* include all the controls used in column 4 (excluding age, age squared and log inventory days) of Table 2. Standard errors are reported in parenthesis and are clustered by zip code.

	(1) Log Rent	(2) Log Rent	(3) Log Rent	(4) Log Rent
dummy 1980s	-0.0373	-0.0576	-0.0554	-0.0584
	(0.0285)	(0.0445)	(0.0447)	(0.0448)
dummy 1990:1994	-0.0866***	-0.0788***	-0.0765***	-0.0777***
	(0.0268)	(0.0260)	(0.0260)	(0.0261)
dummy 1995:1998	-0.0588***	-0.0558***	-0.0538***	-0.0552***
	(0.00818)	(0.00801)	(0.00782)	(0.00782)
dummy 1999:2001	-0.0570***	-0.0553***	-0.0533***	-0.0544***
	(0.00691)	(0.00680)	(0.00658)	(0.00655)
dummy 2002:2004	-0.0407***	-0.0377***	-0.0360***	-0.0365***
	(0.00636)	(0.00636)	(0.00623)	(0.00621)
dummy 2005:2007	-0.0336***	-0.0306***	-0.0292***	-0.0296***
	(0.00600)	(0.00615)	(0.00609)	(0.00608)
dummy 2008:2010	-0.0565***	-0.0523***	-0.0515***	-0.0530***
	(0.00464)	(0.00479)	(0.00474)	(0.00473)
dummy 2011:2013	-0.0460***	-0.0435***	-0.0438***	-0.0453***
	(0.00473)	(0.00475)	(0.00479)	(0.00475)
dummy 2014:2016	-0.0145***	-0.0139***	-0.0131***	-0.0134***
	(0.00490)	(0.00478)	(0.00478)	(0.00476)
absentee			0.00691**	0.00391
			(0.00347)	(0.00363)
corporation				0.0178***
				(0.00683)
$(\log(p_{last}) - \pi_{last})$		0.178***	0.179***	0.178***
		(0.0113)	(0.0115)	(0.0115)
age	-0.00119***	-0.00101***	-0.00102***	-0.00102***
	(0.000387)	(0.000379)	(0.000378)	(0.000378)
age-sq	1.23e-05***	1.09e-05***	1.09e-05***	1.09e-05***
	(3.22e-06)	(3.22e-06)	(3.22e-06)	(3.22e-06)
log(inventory days)	0.0157***	0.0146***	0.0146***	0.0145***
	(0.00171)	(0.00157)	(0.00156)	(0.00157)
bedrm dummies	YES	YES	YES	YES
bathrm dummies	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES
F-stat 05:07 - 08:10	16.75	14.56	15.52	16.93
p-value 05:07 - 08:10	0.0000	0.0001	0.0001	0.0000
Obs	24,992	24,672	24,672	24,672
R-sq	0.869	0.876	0.876	0.876

Table 6: Impact of acquisition vintage on current rents, controlling for both house characteristics and landlord type. We control for landlord type by adding to the specification of Equation (5) dummies equal to one if the landlord was absentee or a corporate entity at the time of purchasing the property. Corporate entities are a subset of absentee landlords. Estimates are based on the sample of rental listings matched with Corelogic deed files records. Standard errors are reported in parenthesis and are clustered by zip code.

	(1) Log Inv. Days AFT	(2) Log Inv. Days AFT	(3) Log Inv. Days AFT 2005:2010
log(R)	0.597***	1.324***	
	(0.0409)	(0.404)	
log(R)-sq		-0.0430*	
		(0.0235)	
dummy 2005:2007			0.0633*
			(0.0334)
additional controls	YES	YES	YES
bedrm dummies	YES	YES	YES
bathrm dummies	YES	YES	YES
zip code FE	YES	YES	YES
Weibull Par. 95% C.I.	[1.39,1.42]	[1.39,1.42]	[1.55,1.64]
Obs	36,331	36,331	6,061

Table 7: Effect of asked rents on time on market, based on the accelerated failure time (AFT) model in Equation (9). The dependent variable is the log of the number of days the house remains listed. In column 3 the sample is restricted to homes last purchased between January 2005 and December 2010. Standard errors are reported in parenthesis and are clustered by zip code.

	(1) Log Rent	(2) Log Rent	(3) Log Rent	(4) Log Rent	(5) Log Rent
	High or *Low* LTV_{last}				
$LTV_{last} > 0.95$	0.0446***	0.0452***	0.0223***	0.0225***	
	(0.00871)	(0.00858)	(0.00407)	(0.00406)	
$LTV_{last} \leq 0.50$			-0.0234***	-0.0238***	
			(0.00682)	(0.00674)	
LTV_{last}					0.0867***
					(0.00969)
$\log(p_{last})$	0.0837***	0.0863***	0.0683***	0.0681***	0.0717***
	(0.0114)	(0.0110)	(0.00520)	(0.00517)	(0.00520)
age	-0.000769	-0.000682	-0.000904**	-0.000881**	-0.000924**
	(0.000961)	(0.000879)	(0.000418)	(0.000411)	(0.000411)
age-sq	6.08e-06	5.09e-06	9.78e-06***	9.50e-06***	9.83e-06***
	(8.63e-06)	(7.89e-06)	(3.57e-06)	(3.52e-06)	(3.51e-06)
log(inventory days)	0.0120***	0.0121***	0.0150***	0.0150***	0.0150***
	(0.00294)	(0.00296)	(0.00173)	(0.00174)	(0.00174)
$\log(p_{last}) - \pi_{last}$	0.0892***	0.0747	0.146***	0.156***	0.155***
	(0.0293)	(0.0873)	(0.0121)	(0.0349)	(0.0352)
$\log(p_{last}) - \pi_{last} \times$ vintage	NO	YES	NO	YES	YES
bedrm dummies	YES	YES	YES	YES	YES
bathrm dummies	YES	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES	YES
Obs	3,953	3,953	19,741	19,741	19,741
R-sq	0.894	0.895	0.872	0.872	0.872

Table 8: Effect of initial leverage (LTV) at the time of house purchase on current rents. LTV_{last} is the loan-to-value ratio for the mortgage on the rental property at the time of purchase, p_{last} is the last purchase price of the rental property, and π_{last} is the hedonic estimate of the log purchase price based on regression Equation (8). Estimates are based on the sample of rental listings matched with Corelogic deed files records. In columns 1 and 2 the sample is restricted to properties with original LTV_{last} greater than 0.95 or smaller or equal than 0.5 (*High* or *Low* LTV_{last}). Standard errors are reported in parenthesis and are clustered by zip code.

	(1) Log Rent $LTV_{last} \leq 0.50$	(2) Log Rent $LTV_{last} \leq 0.75$	(3) Log Rent $LTV_{last} \leq 0.50$	(4) Log Rent $LTV_{last} \leq 0.75$
$log(p_{last,zip})$			0.0970***	0.0416***
			(0.0252)	(0.00928)
dummy 1980s		0.439***		
		(0.0326)		
dummy 1990:1994	0.286	0.0754		
	(0.453)	(0.115)		
dummy 1995:1998	-0.147***	-0.0539***		
	(0.0524)	(0.0204)		
dummy 1999:2001	-0.113***	-0.0603***		
	(0.0331)	(0.0131)		
dummy 2002:2004	-0.0232	-0.0213*		
	(0.0284)	(0.0121)		
dummy 2005:2007	-0.0110	-0.0230*		
	(0.0384)	(0.0119)		
dummy 2008:2010	-0.0876***	-0.0607***		
	(0.0294)	(0.00855)		
dummy 2011:2013	-0.0467	-0.0531***		
	(0.0329)	(0.00755)		
dummy 2014:2016	-0.0290	-0.0144*		
	(0.0248)	(0.00794)		
	(0.0555)	(0.0209)	(0.0511)	(0.0203)
age	-0.00116	-0.00141**	-0.00130	-0.00154**
	(0.00190)	(0.000669)	(0.00193)	(0.000683)
age-sq	5.50e-06	1.18e-05**	7.00e-06	1.32e-05**
	(1.77e-05)	(5.68e-06)	(1.81e-05)	(5.82e-06)
log(inventory days)	0.0145*	0.0156***	0.0133*	0.0155***
	(0.00741)	(0.00247)	(0.00724)	(0.00249)
$\log(p_{last}) - \pi_{last}$	0.234***	0.221***	0.218***	0.214***
	(0.0555)	(0.0209)	(0.0511)	(0.0203)
additional controls	YES	YES	YES	YES
bedrm dummies	YES	YES	YES	YES
bathrm dummies	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES
Obs	1,195	8,431	1,183	8,336
R-sq	0.885	0.871	0.888	0.870

Table 9: Evidence of anchoring behavior: Effect of acquisition vintage on current rents for homes that were purchased with a loan-to-value ratio (LTV_{last}) below 0.5 (columns 1 and 3), or below 0.75 (columns 2 and 4). In columns 3 and 4, we replace the acquisition vintage dummies with the log of the ZHVI price index in the month when the house was last purchased and in the zip code where the house is located ($\log(p_{last,zip})$). Standard errors are reported in parenthesis and are clustered by zip code.

Figures

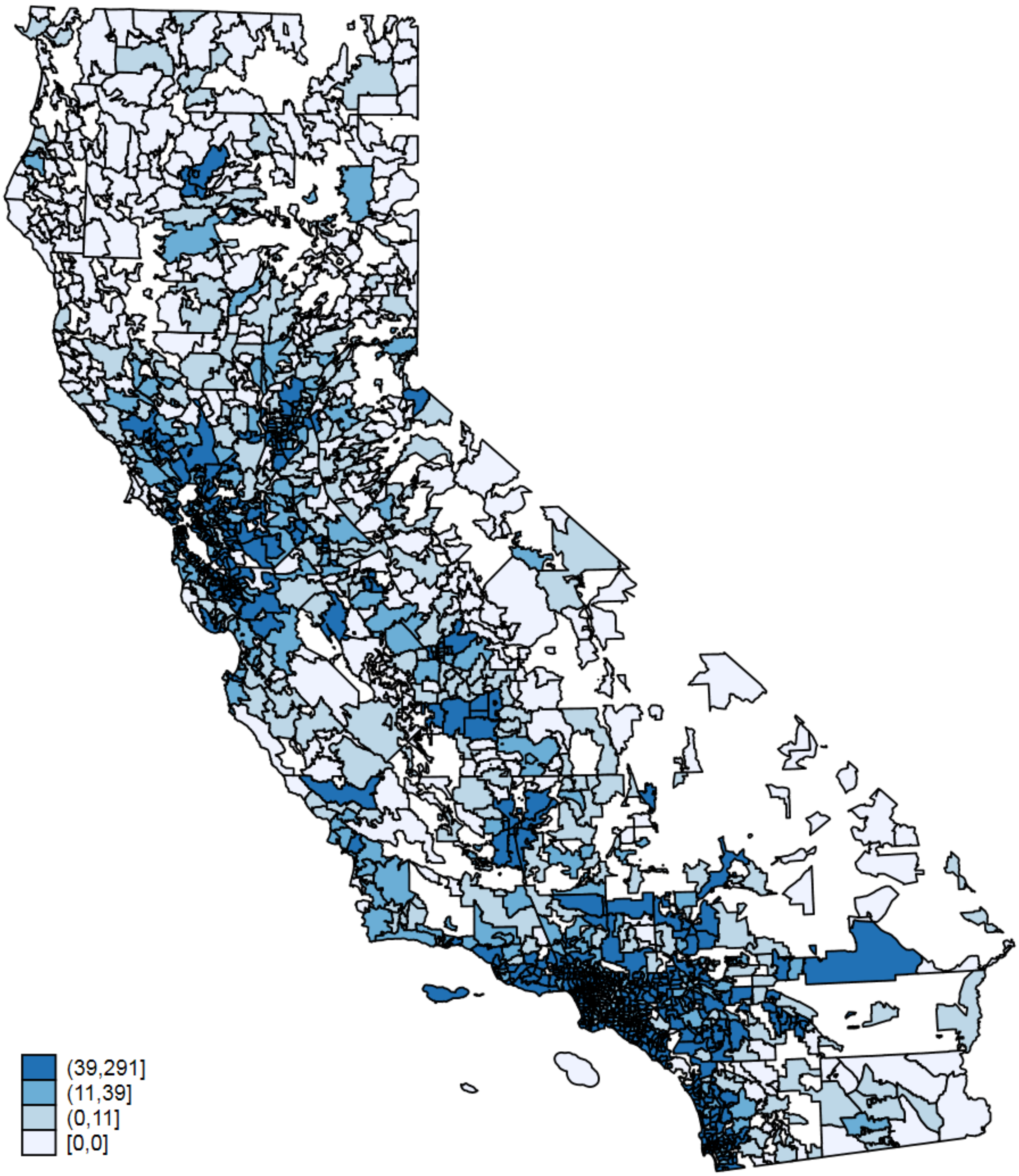

Figure 1: Number of rental listings per zip code in the dataset.

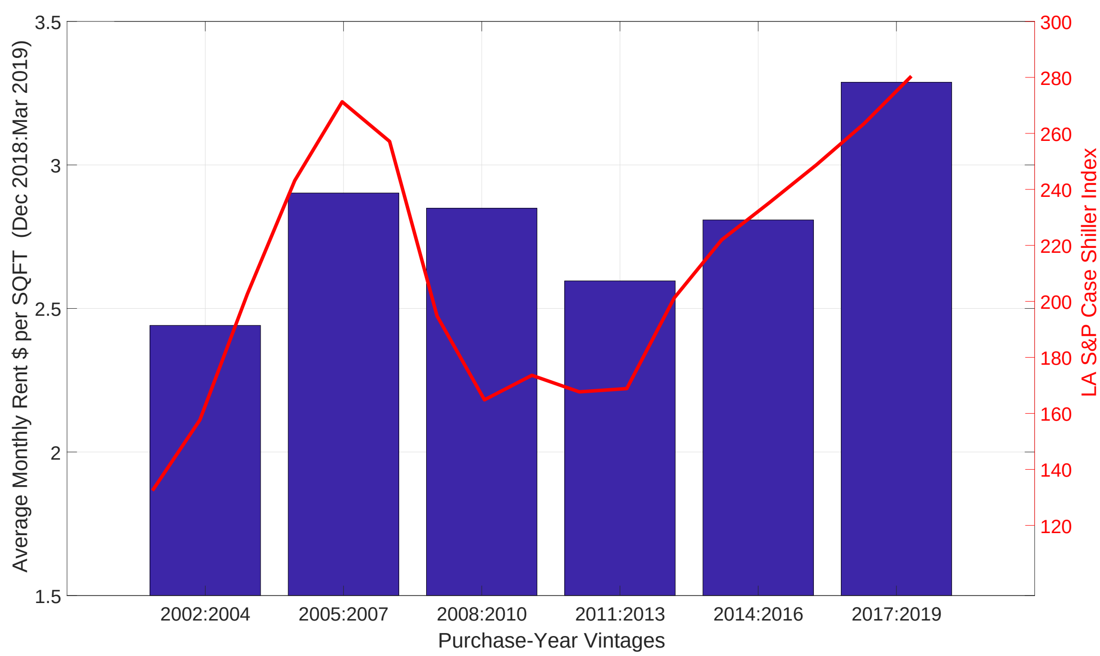

Figure 2: The blue bars show the average monthly rent per square foot for listings of 2 bedroom houses in the city of Los Angeles, by vintage of house purchase. Note that the sample of rental listings has been collected over the period from December 2018 to March 2019. We compare the pattern in the cross-section of rents (blue bars) against the time series evolution of aggregate house prices across acquisition vintages, which is measured using the S&P Case-Shiller repeated sale index for Los Angeles (the red line). The index is set equal to 100 in January 2000.

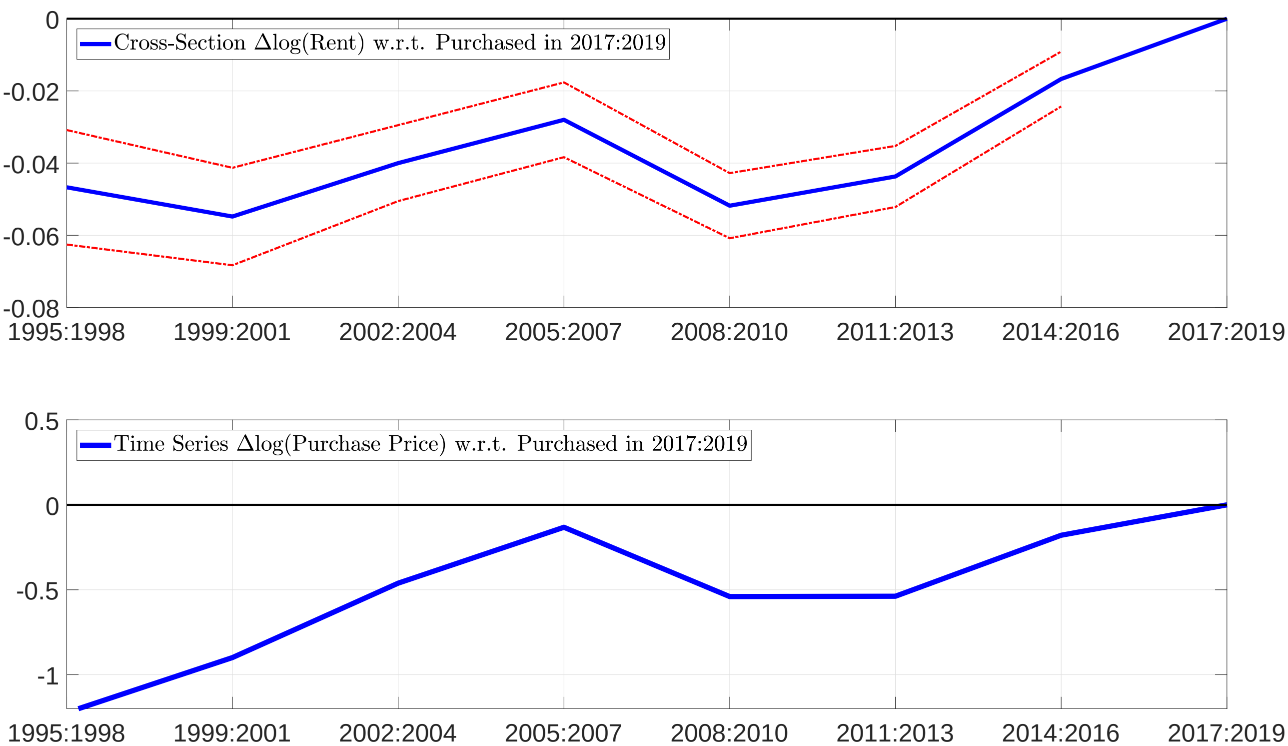

Figure 3: The top panel shows estimates of purchase/acquisition vintage dummies from the specification of Equation (5) in column 4 of Table 2. The dependent variable is log monthly rent, and the dummies measure the log differences in the cross-section of current rents between the rent asked by houses purchased in each vintage and the rent asked by houses purchased in 2017-2019. The bottom panel shows purchase/acquisition vintage year dummies from the regression specification in column 3 of Table A.2. The dependent variable is the log of the last purchase price of the house, and the estimates of the dummies in this second panel can be interpreted as an historical house price index, showing the log difference between purchase prices in each vintage with respect to the 2017-2019 vintage. All estimates are based on rental listings from the state of California, collected over the period from December 2018 through March 2019.

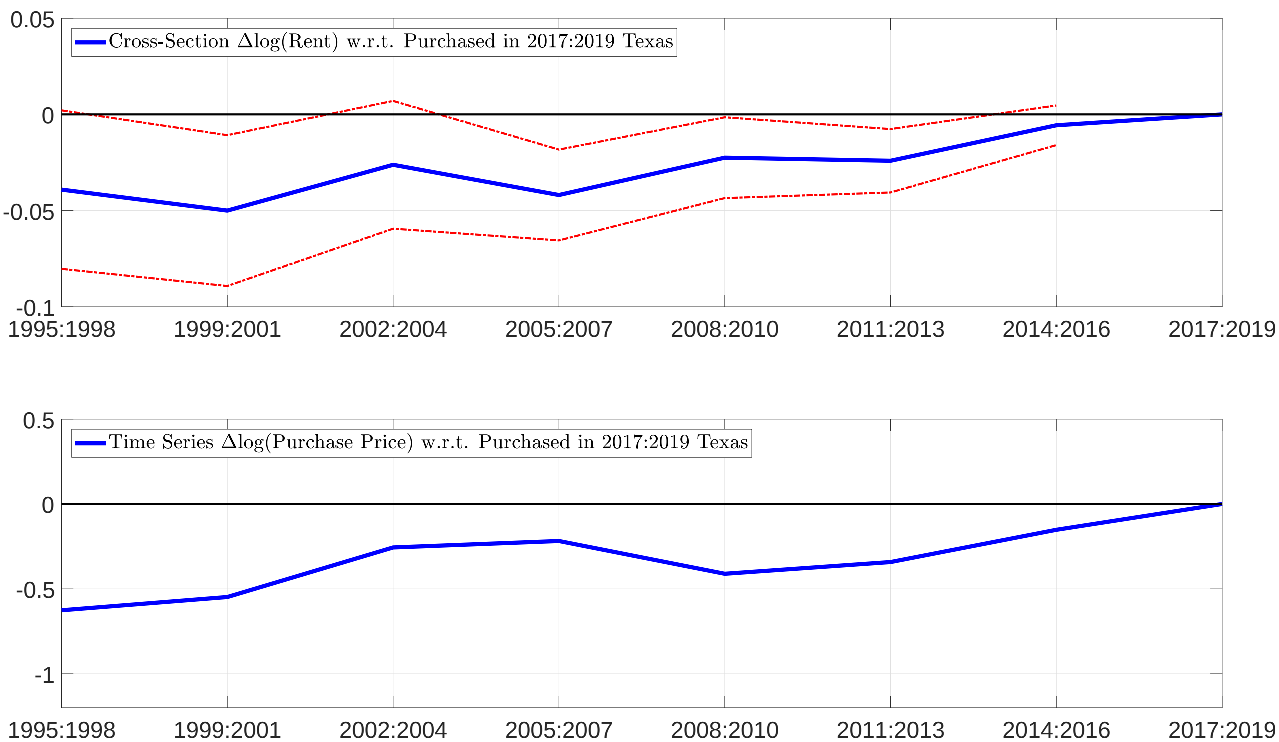

Figure 4: While Figure 3 reports evidence based on California rental listings, the current Figure reports evidence based on rental listings from the state of Texas, collected over the period from December 2018 through March 2019. Similar to Figure 3, the top panel shows estimates of purchase/acquisition vintage dummies from the specification of Equation (5) in column 4 of Table 2. The dependent variable is log monthly rent, and the dummies measure the log differences in the cross-section of current rents between the rent asked by houses purchased in each vintage and the rent asked by houses purchased in 2017-2019. The bottom panel shows purchase/acquisition vintage year dummies from the regression specification in column 3 of Table A.2. The dependent variable is the log of the last purchase price of the house, and the estimates of the dummies in this second panel can be interpreted as an historical house price index, showing the log difference between purchase prices in each vintage with respect to the 2017-2019 vintage.

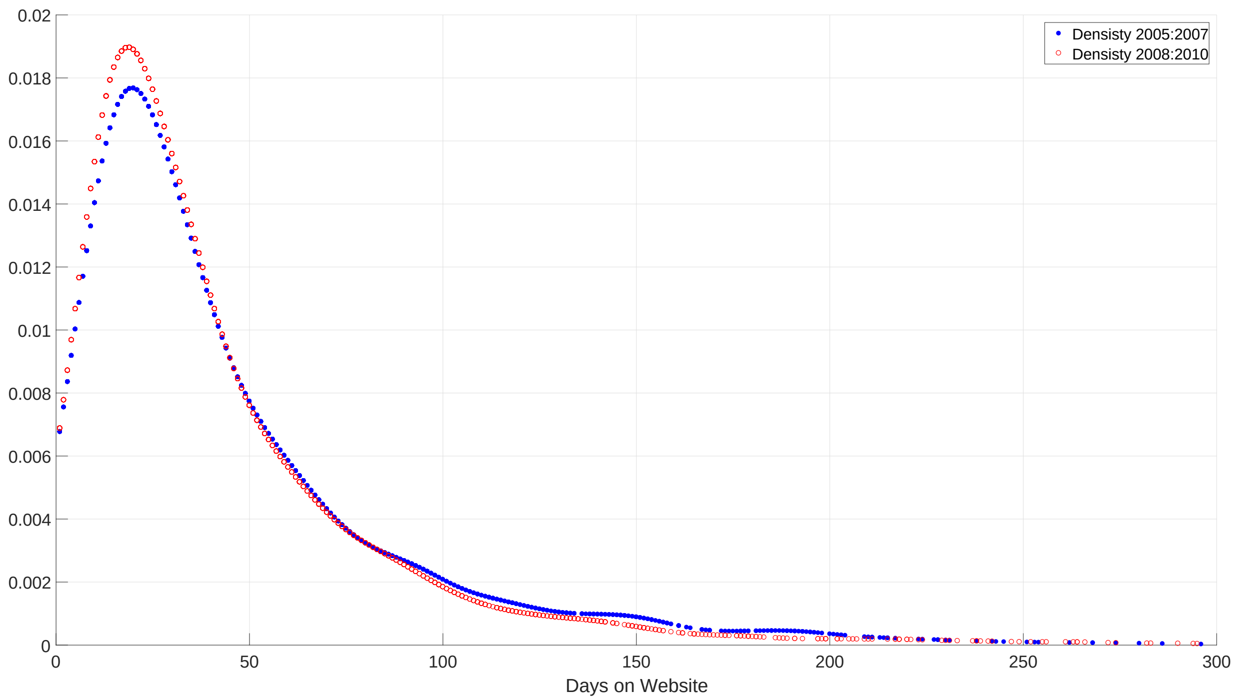

Figure 5: Distribution of time on market (inventory days) for houses purchased at the peak (2005-2007) and the bust (2008-2010) of California housing markets. The Figure shows kernel density estimates for the number of days until the listing was removed, or the number of days the listing had been available till our last collection date, for houses that were still listed at that point. Densities are estimated using a Normal kernel with optimal bandwith chosen according to the methodology in Silverman (1984).

Appendix

A Additional Tables

	(1)	(2)	(3)	(4)
	Log Rent	Log Rent	Log Rent	Log Rent
	Excluding Houses Last Purchased in 2008			
dummy 1980s	-0.128***	-0.104***	-0.103***	-0.0926***
	(0.0149)	(0.00912)	(0.0122)	(0.0123)
dummy 1990:1994	-0.0916***	-0.0798***	-0.0738***	-0.0656***
	(0.0129)	(0.00782)	(0.00996)	(0.00996)
dummy 1995:1998	-0.0508***	-0.0620***	-0.0520***	-0.0470***
	(0.0101)	(0.00655)	(0.00784)	(0.00795)
dummy 1999:2001	-0.0495***	-0.0650***	-0.0582***	-0.0551***
	(0.00861)	(0.00562)	(0.00678)	(0.00677)
dummy 2002:2004	-0.0380***	-0.0502***	-0.0423***	-0.0400***
	(0.00711)	(0.00474)	(0.00528)	(0.00528)
dummy 2005:2007	-0.0302***	-0.0344***	-0.0297***	-0.0283***
	(0.00784)	(0.00457)	(0.00520)	(0.00517)
dummy 2008:2010	-0.0649***	-0.0587***	-0.0545***	-0.0526***
	(0.00771)	(0.00475)	(0.00514)	(0.00515)
dummy 2011:2013	-0.0506***	-0.0481***	-0.0449***	-0.0439***
	(0.00608)	(0.00395)	(0.00426)	(0.00425)
dummy 2014:2016	-0.0197***	-0.0176***	-0.0161***	-0.0167***
	(0.00549)	(0.00358)	(0.00381)	(0.00380)
age			-0.00152***	-0.00145***
			(0.000418)	(0.000398)
age-sq			9.79e-06**	9.60e-06***
			(3.90e-06)	(3.67e-06)
log(inventory days)			0.0163***	0.0162***
			(0.00164)	(0.00157)
log(size)		0.389***	0.389***	0.382***
		(0.0167)	(0.0179)	(0.0177)
agent listing		-0.00881***	-0.0109***	-0.0128***
		(0.00243)	(0.00269)	(0.00270)
shared laundry		-0.0587***	-0.0567***	-0.0582***
		(0.00809)	(0.00861)	(0.00816)
townhouse		-0.0946***	-0.105***	-0.104***
		(0.00578)	(0.00646)	(0.00633)
condo		-0.106***	-0.116***	-0.119***
		(0.00754)	(0.00892)	(0.00886)
multi		-0.129***	-0.139***	-0.133***
		(0.00752)	(0.00862)	(0.00848)
street parking		-0.0351***	-0.0395***	-0.0514***
		(0.0111)	(0.0125)	(0.0125)
studio		-0.370***	-0.392***	-0.390***
		(0.0304)	(0.0351)	(0.0345)
refrigerator				0.0232***
				(0.00395)
dishwasher				-0.000921
				(0.00287)
no pets				-0.0185***
				(0.00282)
hardwood floor				0.0316***
				(0.00282)
forced heat				0.0106***
				(0.00301)
no AC				-0.0268***
				(0.00807)
central AC				0.0316***
				(0.00400)
floor (condo)				0.00323**
				(0.00126)
bedrm dummies	NO	YES	YES	YES
bathrm dummies	NO	YES	YES	YES
zip code FE	YES	YES	YES	YES
F-stat 05:07 - 08:10	12.85	19.38	16.25	16.28
p-value 05:07 - 08:10	0.0004	0.0000	0.0001	0.0001
Obs	42,930	42,930	36,486	36,486
R-sq	0.625	0.857	0.856	0.858

Table A.1: Robustness of the effect of acquisition vintage on current rents. We replicate the analysis in the first 4 columns of Table 2, after removing from the data rental listings for houses last purchased in 2008. The Table also shows the F-statistic and the relative p-value of a test for the null that the dummy coefficient for properties last purchased from 2005 to 2007 and the dummy coefficient for properties last purchased from 2008 to 2010 are equal. Standard errors are reported in parenthesis and are clustered at the zip code level.

	(1) Log Purchase Price	(2) Log Purchase Price Dec 2018 $	(3) Log Purchase Price	(4) Log Purchase Price Dec 2018 $
dummy 1980s	-1.475***	-1.447***	-0.684***	-0.656***
	(0.0361)	(0.0382)	(0.0341)	(0.0351)
dummy 1990:1994	-1.236***	-1.244***	-0.713***	-0.719***
	(0.0215)	(0.0227)	(0.0219)	(0.0233)
dummy 1995:1998	-1.204***	-1.222***	-0.785***	-0.803***
	(0.0156)	(0.0146)	(0.0157)	(0.0146)
dummy 1999:2001	-0.877***	-0.900***	-0.533***	-0.556***
	(0.0141)	(0.0129)	(0.0140)	(0.0128)
dummy 2002:2004	-0.444***	-0.462***	-0.163***	-0.182***
	(0.0126)	(0.0118)	(0.0126)	(0.0118)
dummy 2005:2007	-0.124***	-0.134***	0.0987***	0.0882***
	(0.0153)	(0.0135)	(0.0155)	(0.0136)
dummy 2008:2010	-0.550***	-0.542***	-0.394***	-0.386***
	(0.0126)	(0.0112)	(0.0127)	(0.0112)
dummy 2011:2013	-0.545***	-0.538***	-0.437***	-0.431***
	(0.0109)	(0.00976)	(0.0109)	(0.00977)
dummy 2014:2016	-0.174***	-0.180***	-0.120***	-0.127***
	(0.00837)	(0.00719)	(0.00844)	(0.00721)
age		-0.00437***		-0.00432***
		(0.000824)		(0.000805)
age-sq		3.31e-05***		3.27e-05***
		(8.09e-06)		(7.90e-06)
log(size)		0.533***		0.534***
		(0.0209)		(0.0211)
shared laundry		0.0648***		0.0667***
		(0.0184)		(0.0184)
townhouse		-0.180***		-0.179***
		(0.0107)		(0.0108)
condo		-0.177***		-0.178***
		(0.0112)		(0.0112)
multi-family		-0.174***		-0.175***
		(0.0152)		(0.0152)
street parking		0.00958		0.00754
		(0.0269)		(0.0269)
studio		0.309***		0.310***
		(0.0573)		(0.0574)
bedrm dummies	NO	YES	NO	YES
bathrm dummies	NO	YES	NO	YES
zip code FE	YES	YES	YES	YES
Obs	41,882	36,574	41,561	36,264
R-sq	0.642	0.764	0.612	0.747

Table A.2: Effect of acquisition vintage on the last purchase price of the rental properties in our sample. In columns 1 and 3, the dependent variable is the nominal purchase price of the property, while in columns 2 and 4 the dependent variable is the real purchase price (expressed in terms of December 2018 dollars). Standard errors are reported in parenthesis and are clustered by zip code.

	(1) Log Rent	(2) Log Rent ≥ Median Rent	(3) Log Rent	(4) Log Rent	(5) Log Rent < Median Rent	(6) Log Rent
dummy 1980s	-0.109***	-0.101***	-0.0923***	-0.0958***	-0.0978***	-0.0863***
	(0.0148)	(0.0189)	(0.0191)	(0.00972)	(0.0142)	(0.0143)
dummy 1990:1994	-0.0925***	-0.0870***	-0.0789***	-0.0677***	-0.0564***	-0.0468***
	(0.0111)	(0.0141)	(0.0140)	(0.0104)	(0.0125)	(0.0125)
dummy 1995:1998	-0.0680***	-0.0596***	-0.0549***	-0.0503***	-0.0373***	-0.0313***
	(0.00964)	(0.0112)	(0.0113)	(0.00696)	(0.00825)	(0.00828)
dummy 1999:2001	-0.0690***	-0.0636***	-0.0611***	-0.0544***	-0.0426***	-0.0383***
	(0.00791)	(0.00957)	(0.00961)	(0.00712)	(0.00819)	(0.00808)
dummy 2002:2004	-0.0464***	-0.0368***	-0.0357***	-0.0530***	-0.0454***	-0.0414***
	(0.00745)	(0.00792)	(0.00794)	(0.00542)	(0.00621)	(0.00612)
dummy 2005:2007	-0.0269***	-0.0193**	-0.0183**	-0.0428***	-0.0408***	-0.0376***
	(0.00692)	(0.00775)	(0.00768)	(0.00547)	(0.00617)	(0.00615)
dummy 2008:2010	-0.0507***	-0.0474***	-0.0470***	-0.0630***	-0.0583***	-0.0552***
	(0.00645)	(0.00695)	(0.00701)	(0.00498)	(0.00543)	(0.00545)
dummy 2011:2013	-0.0442***	-0.0420***	-0.0416***	-0.0514***	-0.0476***	-0.0452***
	(0.00660)	(0.00703)	(0.00700)	(0.00447)	(0.00466)	(0.00466)
dummy 2014:2016	-0.00787	-0.00523	-0.00621	-0.0253***	-0.0251***	-0.0251***
	(0.00576)	(0.00605)	(0.00606)	(0.00414)	(0.00435)	(0.00440)
age		-0.00239***	-0.00221***		-0.00124***	-0.00128***
		(0.000448)	(0.000448)		(0.000412)	(0.000400)
age-sq		2.09e-05***	1.96e-05***		5.81e-06*	6.24e-06*
		(3.88e-06)	(3.80e-06)		(3.51e-06)	(3.39e-06)
log(inventory days)		0.0164***	0.0161***		0.0145***	0.0149***
		(0.00235)	(0.00223)		(0.00217)	(0.00211)
log(size)	0.513***	0.513***	0.503***	0.239***	0.230***	0.227***
	(0.0225)	(0.0249)	(0.0248)	(0.0169)	(0.0180)	(0.0179)
agent listing	-0.0105***	-0.0130***	-0.0151***	-0.00840***	-0.0102***	-0.0127***
	(0.00352)	(0.00385)	(0.00392)	(0.00289)	(0.00323)	(0.00327)
shared laundry	-0.0714***	-0.0623***	-0.0628***	-0.0442***	-0.0465***	-0.0498***
	(0.0111)	(0.0116)	(0.0110)	(0.00956)	(0.0102)	(0.00958)
townhouse	-0.0938***	-0.100***	-0.0982***	-0.0834***	-0.0954***	-0.0954***
	(0.00810)	(0.00914)	(0.00891)	(0.00725)	(0.00769)	(0.00765)
condo	-0.101***	-0.105***	-0.107***	-0.101***	-0.113***	-0.119***
	(0.0114)	(0.0135)	(0.0134)	(0.00800)	(0.00956)	(0.00967)
multi	-0.124***	-0.126***	-0.121***	-0.132***	-0.148***	-0.141***
	(0.0108)	(0.0129)	(0.0126)	(0.0105)	(0.0112)	(0.0110)
street parking	-0.0300*	-0.0429**	-0.0569***	-0.0418***	-0.0441***	-0.0512***
	(0.0167)	(0.0180)	(0.0179)	(0.0131)	(0.0151)	(0.0150)
studio	-0.288***	-0.306***	-0.304***	-0.416***	-0.438***	-0.437***
	(0.0450)	(0.0506)	(0.0496)	(0.0355)	(0.0413)	(0.0408)
refrigerator			0.0186***			0.0219***
			(0.00555)			(0.00451)
dishwasher			-0.00146			0.000119
			(0.00467)			(0.00327)
no pets			-0.0155***			-0.0212***
			(0.00410)			(0.00365)
hardwood floor			0.0282***			0.0332***
			(0.00401)			(0.00337)
forced heat			0.00152			0.0191***
			(0.00438)			(0.00355)
no AC			-0.0346***			0.00196
			(0.0107)			(0.00788)
central AC			0.0423***			0.0158***
			(0.00619)			(0.00404)
floor (condo)			0.00229*			0.00425**
			(0.00134)			(0.00177)
bedrm dummies	YES	YES	YES	YES	YES	YES
bathrm dummies	YES	YES	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES	YES	YES
F-stat 05:07 - 08:10	6.65	8.01	8.62	14.95	10.96	10.66
p-value 05:07 - 08:10	0.0101	0.0048	0.0034	0.0001	0.0010	0.0011
Obs	22,188	19,181	19,181	21,956	18,259	18,259
R-sq	0.824	0.824	0.827	0.822	0.818	0.821

Table A.3: Effect of acquisition vintage on current rents, across housing units demanding rents above and below median. We replicate the analysis in columns 2, 3 and 4 of Table 2 within two subsamples of the data. In columns 1, 2 and 3 the sample is restricted to listings that ask monthly rent greater or equal than the median. In columns 4, 5 and 6 the sample is restricted to listings that ask monthly rent below the median. The Table also shows the F-statistic and p-value of a test for the null that the dummy coefficient for properties last purchased from 2005 to 2007 and the dummy coefficient for properties last purchased from 2008 to 2010 are equal. Standard errors are reported in parenthesis and are clustered by zip code.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Rent	Log Rent	Log Rent	Log Rent	Log Rent	Log Rent
	$\geq$ Median zip code Income			$<$ Median zip code Income		
dummy 1980s	-0.117***	-0.118***	-0.112***	-0.0526***	-0.0418***	-0.0327***
	(0.0149)	(0.0185)	(0.0190)	(0.00817)	(0.00978)	(0.00978)
dummy 1990:1994	-0.0847***	-0.0816***	-0.0757***	-0.0594***	-0.0497***	-0.0415***
	(0.0123)	(0.0154)	(0.0153)	(0.00815)	(0.00951)	(0.00962)
dummy 1995:1998	-0.0702***	-0.0648***	-0.0625***	-0.0334***	-0.0241***	-0.0185***
	(0.00986)	(0.0111)	(0.0113)	(0.00510)	(0.00616)	(0.00606)
dummy 1999:2001	-0.0720***	-0.0693***	-0.0679***	-0.0371***	-0.0261***	-0.0223***
	(0.00799)	(0.00943)	(0.00944)	(0.00501)	(0.00613)	(0.00598)
dummy 2002:2004	-0.0532***	-0.0470***	-0.0468***	-0.0368***	-0.0299***	-0.0257***
	(0.00716)	(0.00777)	(0.00777)	(0.00379)	(0.00436)	(0.00427)
dummy 2005:2007	-0.0375***	-0.0309***	-0.0315***	-0.0259***	-0.0224***	-0.0194***
	(0.00712)	(0.00804)	(0.00800)	(0.00398)	(0.00442)	(0.00439)
dummy 2008:2010	-0.0568***	-0.0545***	-0.0556***	-0.0414***	-0.0371***	-0.0338***
	(0.00668)	(0.00741)	(0.00750)	(0.00354)	(0.00373)	(0.00371)
dummy 2011:2013	-0.0415***	-0.0396***	-0.0409***	-0.0367***	-0.0329***	-0.0305***
	(0.00573)	(0.00609)	(0.00608)	(0.00294)	(0.00313)	(0.00311)
dummy 2014:2016	-0.00769	-0.00723	-0.00900	-0.0226***	-0.0215***	-0.0209***
	(0.00561)	(0.00597)	(0.00599)	(0.00304)	(0.00321)	(0.00322)
age		-0.00106**	-0.000945**		-0.00195***	-0.00198***
		(0.000417)	(0.000419)		(0.000360)	(0.000345)
age-sq		1.06e-05***	9.98e-06***		9.72e-06***	9.94e-06***
		(3.73e-06)	(3.72e-06)		(3.43e-06)	(3.28e-06)
log(inventory days)		0.0175***	0.0169***		0.00494***	0.00543***
		(0.00198)	(0.00191)		(0.000954)	(0.000937)
log(size)	0.532***	0.530***	0.522***	0.149***	0.140***	0.139***
	(0.0223)	(0.0244)	(0.0245)	(0.0119)	(0.0128)	(0.0127)
agent listing	-0.0152***	-0.0173***	-0.0187***	-0.00523***	-0.00526***	-0.00691***
	(0.00361)	(0.00395)	(0.00401)	(0.00195)	(0.00204)	(0.00202)
shared laundry	-0.0502***	-0.0471***	-0.0499***	-0.0316***	-0.0320***	-0.0347***
	(0.00963)	(0.0103)	(0.0101)	(0.00482)	(0.00515)	(0.00503)
townhouse	-0.0854***	-0.0885***	-0.0868***	-0.0502***	-0.0585***	-0.0593***
	(0.00808)	(0.00920)	(0.00904)	(0.00443)	(0.00470)	(0.00470)
condo	-0.0954***	-0.0950***	-0.1000***	-0.0692***	-0.0807***	-0.0805***
	(0.0113)	(0.0131)	(0.0131)	(0.00494)	(0.00555)	(0.00575)
multi	-0.0957***	-0.0956***	-0.0911***	-0.0998***	-0.114***	-0.110***
	(0.0137)	(0.0154)	(0.0154)	(0.00568)	(0.00656)	(0.00647)
street parking	-0.0293*	-0.0436**	-0.0560***	-0.0239**	-0.0245**	-0.0280**
	(0.0163)	(0.0172)	(0.0171)	(0.0108)	(0.0122)	(0.0119)
studio	-0.109**	-0.128**	-0.136**	-0.367***	-0.375***	-0.371***
	(0.0529)	(0.0598)	(0.0590)	(0.0221)	(0.0251)	(0.0250)
refrigerator			0.0217***			0.00369
			(0.00473)			(0.00308)
dishwasher			-0.00286			0.00246
			(0.00459)			(0.00226)
no pets			-0.00900**			-0.0240***
			(0.00401)			(0.00225)
hardwood floor			0.0234***			0.0239***
			(0.00373)			(0.00229)
forced heat			-0.00550			0.0166***
			(0.00414)			(0.00238)
no AC			-0.0333***			0.00507
			(0.00999)			(0.00635)
central AC			0.0324***			0.00860***
			(0.00519)			(0.00249)
floor (condo)			0.00333*			0.000326
			(0.00190)			(0.000775)
bedrm dummies	YES	YES	YES	YES	YES	YES
bathrm dummies	YES	YES	YES	YES	YES	YES
zip code FE	YES	YES	YES	YES	YES	YES
F-stat 05:07 - 08:10	9.58	11.03	12.01	14.02	7.49	7.67
p-value 05:07 - 08:10	0.0021	0.0010	0.0006	0.0002	0.0064	0.0058
Obs	22,262	19,623	19,623	21,740	17,662	17,662
R-sq	0.769	0.773	0.776	0.798	0.798	0.802

Table A.4: Effect of acquisition vintage on current rents, across housing units in zip codes with average household income above and below median. We replicate the analysis in columns 2, 3 and 4 of Table 2 within two subsamples of the data. In columns 1, 2 and 3 the sample is restricted to listings for houses located in zip codes with average income greater or equal than the median (based on 2016 zip code-level average income calculated by the IRS). In columns 4, 5 and 6 the sample is restricted to listings for houses located in zip codes with average income below the median. The Table also shows the F-statistic and p-value of a test for the null that the dummy coefficient for properties last purchased from 2005 to 2007 and the dummy coefficient for properties last purchased from 2008 to 2010 are equal. Standard errors are reported in parenthesis and are clustered by zip code.

B Unobservable Selection and Coefficient Stability

An abbreviated version of the analysis in Oster (2016) is sketched below. Consider the following regression equation:

$$y_i = \beta x_i + w_{1,i} + w_{2,i} + e_i$$

where x_i is the treatment of interest for observation i, $w_{1,i}$ is a scalar capturing the observed characteristics of i and $w_{2,i}$ is a scalar capturing unobserved quality of i. Assume that $w_{1,i} = \psi \tilde{W}_{1,i}$, where $\tilde{W}_{1,i}$ is a vector of observable characteristics. Also assume that w_1 and w_2 are orthogonal, i.e. w_2 captures variation unobserved quality that is not spanned by the observable characteristics. There will always be a scalar δ such that:

$$\delta \frac{\sigma_{1,x}}{\sigma_1^2} = \frac{\sigma_{2,x}}{\sigma_2^2}$$

where $\sigma_{1,x} = Cov(w_1, x)$, $\sigma_{2,x} = Cov(w_2, x)$, $\sigma_1^2 = Var(w_1)$ and $\sigma_2^2 = Var(w_2)$. Now, consider a "short" regression, including only the treatment x_i as a control; β° and R° are the regression coefficient and the R^2 from the short regression. Consider then the "long" regression including both x_i and $w_{1,i}$, with output $\hat{\beta}$ is $\hat{R}$. Oster (2016) shows that, under some restrictive assumptions:

$$\beta^* - \hat{\beta} \approx \delta \left(\hat{\beta} - \beta^\circ\right) \frac{1 - \hat{R}}{\hat{R} - R^\circ},$$

where β^* is the unbiased estimator of the population value of β. The bias is increasing in the difference in the slope estimates between the "short" to the "long" regression, and is decreasing in the change of R. The exact representation in the equation above holds only under the assumption that the relative contributions of each observable control (each element of $\tilde{W}_{1,i}$) to x is the same as its contribution to y. This assumption does not hold in general in the data. However, Oster (2016) shows that even after removing the restrictive assumptions, a consistent estimator of β^* can still be found. This estimator will retain the key properties and intuition of the basic case. Moreover, Oster (2016) shows that her framework can be further extended to a case where the short regression includes in the conditioning information not only x, but also some of the observable controls. This last case is the one we implement in our study.