The Winner's Curse in Housing Markets*

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Abstract

This paper tests for a winner's curse in housing markets by examining the subsequent performance of bidding war transactions relative to non-bidding war transactions. We develop a model in which homebuyers who purchase their house in a bidding war experience lower annualized returns and a higher likelihood of mortgage default. Consistent with the model, we find that homebuyers in bidding wars experience average total unlevered returns that are 10.5 percentage points lower, equivalent to an overpayment of 8.2 percent, and are 1.9 percentage points more likely to default. These winner's curse effects are more pronounced among socioeconomically vulnerable homebuyers.

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1 Introduction

In competitive housing markets, much of a property's value is determined by shared fundamentals, such as neighborhood quality, market conditions, and comparable sales, that shape buyers' expectations about future resale values. When these common-value components predominate, competition can produce a winner's curse, where the winning bidder overpays due to incomplete information or over-optimism about the asset's value (Wilson, 1977; Milgrom and Weber, 1982; Kagel and Levin, 1986). In housing, this often takes the form of bidding wars. This paper tests for a winner's curse by comparing the subsequent performance of houses purchased through bidding wars with those bought in non-bidding war transactions.

We begin by presenting a model of bidding wars wherein multiple risk-neutral homebuyers compete to purchase a house after receiving independent signals about the house value (Milgrom and Weber, 1982; Levin and Smith, 1994). The model, which incorporates institutional details about the residential home buying process, has two key empirical predictions. First, the model predicts that homebuyers who pay above the list price are, on average, overpaying for their house. We test this prediction by examining whether bidding war transactions experience lower house price appreciation than non-bidding war transactions. Second, the model predicts that homebuyers who overpay for their house are more likely to default on their mortgage. We test this prediction by examining whether homebuyers who purchase their house in a bidding war are more likely to default than those who purchase their house in a non-bidding war transaction.

One of the primary challenges in testing for a winner's curse in housing markets is the difficulty of identifying bidding wars in transaction data. Following Han and Strange (2014, 2015, 2016), we define a bidding war as any transaction where the sale price exceeds the list price. The underlying assumption is that competition from many bidders causes a bidding war that pushes the sales price above the list price. We offer new theoretical and empirical evidence to further support this definition. Theoretically, we demonstrate that expected sale prices stay below list prices in rational equilibrium. In contrast, in limited rationality equilibrium, expected sales prices can exceed list prices, indicating that buyers who pay above list prices are generally overpaying. Empirically, we perform a systematic textual analysis to measure the frequency with which bidding war-related text (e.g., mentions of deadlines, highest and best, or multiple offers) appears in agents' written descriptions of the houses for sale. We find the bidding

war-related text is substantially more likely to appear in transactions we classify as bidding wars than in those we do not. Moreover, our results remain robust across multiple definitions of bidding wars.

We test for and find evidence of a winner's curse using over 14 million housing transactions across 30 states from 2000 through 2018. On average, homebuyers who purchase their house in a bidding war experience a 1.3 percentage point (pp) lower annualized return than those who purchase their house in non-bidding war transactions. Using the average holding period of 6.3 years in the data, this translates to a 10.5pp lower total unlevered return for bidding war transactions. Recognizing that most homebuyers use mortgage financing to purchase their house, we also examine the levered returns of bidding war transactions. We estimate that homebuyers who purchase their house in a bidding war experience, on average, a 6.9pp lower levered annualized return than those who do not purchase their house in a bidding war.

Because mortgage financing exposes homebuyers to the risk of default, we also examine the effect of bidding wars on homebuyers' subsequent default behavior. Once again, we find strong evidence of a winner's curse. Buyers who purchase their house in bidding wars are 1.9pp more likely to default on their mortgages than similar buyers in non-bidding war transactions. Consistent with our model, we show that these winner's curse effects (i.e., lower price appreciation and higher default rate) are more pronounced in periods and regions where bidding wars are more prevalent.

A natural concern is selection into bidding wars. Buyers who win bidding wars may differ along unobserved dimensions - such as credit access, liquidity, patience, or preferences - that also influence purchase and resale outcomes. These differences could generate lower annualized returns if such buyers are less patient when buying and selling or underinvest in maintenance, independent of any winner's curse mechanism. We address these selection concerns in three ways. First, where possible, we directly test and rule out these alternative explanations. For example, we show that buyers who purchase their house in bidding wars exhibit similar holding periods and patience at resale (measured by time on market) as buyers in non-bidding war transactions. Likewise, our results remain largely unchanged after incorporating controls for time-varying house quality, indicating that capital improvements or the underinvestment in maintenance are unlikely to fully explain the lower returns observed for bidding war transactions.

Second, we employ the sensitivity frameworks of Oster (2019) and Cinelli and Hazlett (2020) to assess the impact that unobservables would need to exert to overturn our estimates. The Oster implied strength of unobserved selection far exceeds conventional benchmarks even after including hundreds of qualityârelated controls, and the Cinelli-Hazlett robustness values indicate that only confounders several times more predictive than the benchmark covariates (e.g., investor purchases for returns, CLTV for default) could fully attenuate the coefficients. Confounders of such magnitude are implausible in either context, suggesting that selection on unobservables cannot plausibly explain the observed differences in returns and default risk among bidding war winners.

Third, we apply a judge-style instrumental variables (IV) research design (Dobbie et al., 2017; Stevenson, 2018) that instruments for bidding wars using listing agent fixed effects. This strategy leverages systematic differences in agents' propensity and ability to generate bidding wars, creating quasi-experimental variation in buyers' exposure. We document substantial cross-sectional dispersion in agents' bidding war propensities that is not mechanically driven by experience or scale, providing strong first stage relevance. The design is also plausibly exogenous: buyers choose neighborhoods but face a limited set of properties for sale and have little control over which agent lists a given home. We show that within-zip code listing agent concentration is generally low and bidding war dispersion is generally high. This evidence supports the plausibility of the exclusion restriction by ruling out concerns about local agent concentration and uniform listing strategies. The IV estimates mirror our main results, indicating that adverse buyer outcomes arise from the bidding war mechanism itself rather than from selection into bidding wars.

After establishing strong evidence of a winner's curse effect, we focus on identifying those most impacted, finding that socioeconomically vulnerable homebuyers are particularly susceptible to the winner's curse effects linked to bidding wars. We find bidding wars are more prevalent, and their effects are more pronounced in neighborhoods with higher proportions of minority, lower-income, and less-educated populations. Using a subset of transactions where the homebuyer's race, ethnicity, and income can be reliably identified, we provide additional evidence that winning a bidding war is more likely and its negative consequences are more severe for lower-income, Black, and Hispanic homebuyers.

Our study contributes to several strands of literature at the intersection of finance, economics, and real estate, with the most direct link to studies on the winner's curse. The concept of a winner's curse was first identified in Capen et al. (1971), which documents how aggressive bidding in oil lease auctions frequently leads to overpayment. Building on these insights, Milgrom and Weber (1982) formalize the phenomenon within auction theory, showing how common values and bidder asymmetries can systematically bias bids upward. Since then, canonical auction experiments, such as Kagel and Levin (1986) and Lind and Plott (1991), document a winner's curse in controlled laboratory settings using induced common values and experimental trials.

Empirical research has tested for a winner's curse in a variety of settings, including government auctions (Hendricks et al., 1987; Athey et al., 2011), corporate acquisitions and takeovers (Roll, 1986; Giliberto and Varaiya, 1989; de Bodt et al., 2018), and financial markets (Rock, 1986; Levis, 1990; Keloharju, 1993; Riggs et al., 2020). Although a few real estate studies exist, they focus on a winner's curse in formal auctions, such as open-outcry condo sales, public land auctions, or sealed-bid tenders (Tse et al., 2011; Chow and Ooi, 2014). We instead examine informal auctions, or bidding wars, where buyers submit simultaneous, private offers to a seller. In recent years, bidding wars have comprised as much as half of all U.S. residential transactions.[1] Yet, the existence and consequences of the winner's curse in U.S. housing markets remain largely unexplored. This omission is significant given the size of the asset class, the high stakes of home purchases, and the potential that overpayment may cause increased mortgage default risk.

Gargano and Giacoletti (2026) examine the effects of underquoting - the practice of setting listing prices below sellers' reservation values - on housing auctions in Australia, finding that strategically low list prices intensify competition and raise final sale prices, while regulatory efforts to curb underquoting lead to higher list prices but lower sale prices and sale probabilities. Their findings highlight how seller-side strategic choices can meaningfully alter bidding intensity in a formal auction environment, an insight that motivates our listing agent IV design. Turning to bidding wars more directly, a small but growing body of work documents their prevalence, underlying drivers, and implications for market volatility (Han and Strange, 2014, 2016; Liu and Smith, 2023; Arefeva, 2025). We contribute to this literature by providing the first empirical test for a winner's curse

[1] In April 2021, 72% of home purchase offers submitted by Redfin agents faced competition. It was the twelfth consecutive month in which more than half of all listings resulted in bidding wars (Katz, 2021).

in housing markets. Our results show that the consequences of bidding wars persist well beyond the initial transaction, leading to lower annualized returns and a higher likelihood of default - both of which constitute clear evidence of a winner's curse.

As bidding wars have become increasingly common in the postâpandemic housing market, policymakers and the public have grown concerned about how intensified competition shapes affordability and market outcomes. We show that lower-income, less educated, and minority buyers are disproportionately vulnerable to winner's curse effects, consistent with a broader literature on socioeconomic and racial disparities in housing market outcomes (Kermani and Wong, 2026; Kahn, 2024). By identifying which buyers face the greatest overpayment risk, our study demonstrates how bidding wars can amplify market inefficiencies and exacerbate persistent inequities in homeownership.

2 Model of Bidding Wars in the Housing Market

In this section, we construct a model of bidding wars in the housing market, where multiple risk-neutral buyers compete to purchase a house in a setting with incomplete information. We compare fully rational equilibrium with an equilibrium in which bidders with limited rationality may suffer from the so-called winner's curse. As we note throughout the paper, the limited rationality environment should not be interpreted as implying that homebuyers necessarily behave irrationally in this equilibrium. Instead, we interpret the outcomes in the limited rationality environment as being driven by a lack of financial sophistication, limited exposure to homeownership, or some other constraint.[2]

We assume that a house on the market has a reservation price $\underline{\theta} \geq 0$, i.e., the lowest price a seller is willing to accept. In other words, the seller is better off keeping the house than selling it for less than $\underline{\theta}$. We note that a reservation price is different from a list price, which is not binding, a distinction we will revisit when discussing bilateral bargaining.

We begin with a scenario where $n \geq 2$ potential buyers, or bidders hereinafter, compete for the house. The intrinsic value v of the house is the same for all bidders, who do not observe v directly. Instead, each bidder i obtains a noisy private signal θ_i, which is drawn

[2]While the purpose of this study is not to uncover the behavioral mechanisms behind the limited rationality equilibrium, there is a rich economics literature that provides support for less informed decision-making arising due to limited access to financial information and lack of homeownership experience. See, for example, Deltas and Engelbrecht-Wiggans (2005); Landier and Thesmar (2008); Charles et al. (2009).

independently from the uniform distribution on the interval $[\underline{\theta}, \bar{\theta}]$. The house value v is assumed to be the average of the bidders' signals:

$$v = \frac{1}{n} \sum_{i=1}^{n} \theta_i. \tag{2.1}$$

In other words, the home value v is drawn from the Bates distribution with the mean

$$\mu = \frac{\underline{\theta} + \bar{\theta}}{2},$$

and each signal θ_i is positively correlated with v. The signals are private information, i.e., bidder i knows θ_i, but does not observe θ_{-i}, where θ_{-i} denotes the vector of signals observed by the other $n-1$ bidders. The distribution of v and signals θ_i, for $i = 1...n$, is common knowledge. The house will be sold to the highest bidder, provided the winning bid exceeds $\underline{\theta}$.

One interpretation of our setup is that the value of a house is determined by a number of difficult-to-evaluate characteristics, such as location, quality, cost of materials and appliances, practicality of the floor plan, uniqueness of the architecture, the size and usability of the lot, and so on. For simplicity, we assume that each bidder can only observe one of the characteristics. A more general setup will not change the key empirical predictions of our model.

2.1 Rational Equilibrium

We now derive the optimal bidding strategies in a symmetric Bayesian Nash equilibrium, i.e., bidding strategies that maximize the expected payoff for each bidder properly taking into account all the available information and the strategies employed by the other bidders. The expected payoff π_i^R of rational bidder i is given by

$$\pi_i^R = \Pr(b_i > b_{-i}) E\left[v - b_i | \theta_i, b_i > b_{-i}\right], \tag{2.2}$$

where $\Pr(b_i > b_{-i})$ is the probability of winning the auction, and $E\left[v - b_i | \theta_i, b_i > b_{-i}\right]$ is the expected utility gain of buying the house conditional on the private signal θ_i and fact that the winning bid b_i is the highest.

We start by conjecturing that the equilibrium strategy of each bidder is a linear function of their private signal of the following form:

$$b_i^R = \underline{\theta} + \alpha(\theta_i - \underline{\theta}), \tag{2.3}$$

for some $\alpha > 0$. The next proposition characterizes the symmetric equilibrium with full rationality.

Proposition 1. *In a fully rational symmetric Bayesian Nash equilibrium, the optimal bidding strategies are given by*

$$b_i^R = \underline{\theta} + a^R(\theta_i - \underline{\theta}), \tag{2.4}$$

where

$$\alpha^R = \frac{(n+2)(n-1)}{2n^2}. \tag{2.5}$$

The expected price P_n^R for a listing with n bidders increases with the number of bidders n, but never exceeds μ, and is equal to

$$P_n^R = \mu - \frac{\bar{\theta} - \underline{\theta}}{n(n+1)}. \tag{2.6}$$

Proof. *See Appendix C.1.*

Proposition 1 establishes a fully rational benchmark that we will use to evaluate equilibrium outcomes with limited rationality.

2.2 Equilibrium with Limited Rationality

In this subsection, we analyze an equilibrium, in which bidders fail to take into account all available information. As in the rational equilibrium, bidders with limited rationality make their offers conditional on their private signal. However, they ignore the information about the other bidders' signals. While the other signals are not observable, a rational bidder knows that winning the auction means that all the other bidders observed less favorable signals than theirs. Here, we assume that bidder i with limited rationality ignores this important piece of information about the house value and

instead assumes that the other bidders perceive the house in a similar way. Specifically, bidder i incorrectly assumes that

$$E\left[\theta_j|\theta_i\right] = \theta_i,$$

and, as a consequence,

$$E\left[v|\theta_i\right] = \theta_i.$$

Thus, bidder i computes their expected payoff π_i^L as follows:

$$\pi_i^L = \Pr(b_i > b_{-i})\left(\theta_i - b_i\right). \tag{2.7}$$

We conjecture that the equilibrium strategy of each bidder with limited rationality is a linear function of their private signal:

$$b_i^L = \underline{\theta} + \alpha(\theta_i - \underline{\theta}), \tag{2.8}$$

for some $\alpha > 0$. In the equilibrium with limited rationality, each bidder maximizes their perceived payoff as given by (2.7), considering that their competitors bid according to (2.8). The next proposition characterizes the symmetric equilibrium with limited rationality.

Proposition 2. *In a symmetric equilibrium with limited rationality, the optimal bidding strategies are given by*

$$b_i^L = \underline{\theta} + \alpha^L(\theta_i - \underline{\theta}), \tag{2.9}$$

where

$$\alpha^L = \frac{n-1}{n}. \tag{2.10}$$

The expected price P_n^L paid at the auctions increases with the number of bidders n, and is equal to

$$P_n^L = \mu - \frac{(3-n)(\bar{\theta} - \underline{\theta})}{2(n+1)}. \tag{2.11}$$

Proof. *See Appendix C.2.*

The rational and irrational equilibria share several common features. In both equilibria, bids (2.4) and (2.9) are increasing with private signals. In addition, the average prices (2.6) and (2.11) paid by the auction winners are increasing in the number of bidders n. This is expected. Private signals are positively correlated with the house value, while more competition results in a higher winning bid.

2.3 Winner's Curse

To highlight important differences between the two equilibria, we define a *strong winner's curse* as a situation where the winning bid exceeds the intrinsic value of the house. We define a *weak winner's curse* as a situation where the winning bid is higher than that in the rational equilibrium but does not necessarily exceed the intrinsic value of the house.[3]

We define the differences between the bidding strategies and expected prices as follows $\Delta b_i \equiv b_i^L - b_i^R$ and $\Delta P_n \equiv P_n^L - P_n^R$. Using equations (2.4) - (2.6) and (2.9) - (2.11), we can write

$$\Delta b_i = \frac{(n-1)(n-2)}{2n^2}(\theta_i - \underline{\theta}), \tag{2.12}$$

$$\Delta P_n = \frac{(n-1)(n-2)}{2n(n+1)}(\bar{\theta} - \underline{\theta}). \tag{2.13}$$

Inspecting equations (2.11) and (2.13) yields the following proposition.

Proposition 3. *The bidding strategies and expected prices are the same when $n = 2$, but are strictly higher in the equilibrium with limited rationality when $n > 2$. Moreover, the differences in (2.12) and (2.13) are increasing with n.*

Bidders with limited rationality experience a weak winner's curse when $n = 3$, and a strong winner's curse when $n > 3$. The magnitudes of both weak and strong winner's curses are proportional to $(\bar{\theta} - \underline{\theta})$.

Proof. *See Appendix C.3.*

[3]The internet appendix presents a numerical example highlighting the salient differences between the two equilibria.

2.4 Theoretical Hypotheses

Our model shows that home sales above the list price are associated with the winner's curse. Indeed, the average sale price in the rational equilibrium is always below the list price, while the average sale price in the equilibrium with limited rationality can exceed the list price. In other words, our model predicts that buyers who pay above the list price are, on average, overpaying for their house.

To simplify our terminology, we will refer to houses sold above the list price as *bidding war transactions* and those sold at or below the list price as *non-bidding war transactions*. In addition, we will refer to buyers who pay above the list price as *bidding war winners*.

Due to the difficulty in precisely determining a house's fair market value based on observable characteristics, we test this prediction using a repeat-sales approach. The resale price should, on average, reflect the intrinsic value of the house. Hence, buyers who overpay for their house will experience lower house price appreciation in the future. As a result, we have the first prediction of the model.

Prediction 1. *Bidding war transactions have lower unlevered annualized returns than non-bidding war transactions.*

The unlevered annualized return measures house price appreciation over time, allowing us to determine if homebuyers overpay for their house and suffer from a winner's curse. Recognizing that most homebuyers purchase their house using mortgage financing, we also examine whether buyers who purchase their house in a bidding war experience lower returns on their equity investment in the house.

Prediction 2. *Bidding war transactions have lower levered annualized returns than non-bidding war transactions.*

Because most homebuyers use mortgage financing, another direct consequence of the winner's curse is that it can lead to higher default rates among bidding war winners. Indeed, paying a higher price not only results in taking on a larger mortgage but also leads to a higher effective loan-to-value (LTV) ratio, which is based on the most likely resale price rather than the transaction price. For example, if a bidding war winner who makes a 3.5% down payment overpays by 10%, their mortgage amount will be 7.5% above the resale value of the house, encumbering their ability to pay off the loan. As

a result, this buyer is more likely to default than someone who makes a similar down payment but pays the fair market price. This leads to Prediction 3.

Prediction 3. *Bidding war winners are more likely to default on their mortgages.*

The winning bid increases with the number of bidders n. Thus, the winner's curse should be stronger when more people compete to buy a house. Since we do not observe the number of bidders directly, we use the likelihood of a bidding war as another proxy for the number of bidders. In particular, when bidding wars are more common in a given location, bidders who lose one bidding war are likely to bid again on another house, increasing the overall number of bids. Thus, we have

Prediction 4. *The winner's curse effects – lower home appreciation rates and higher default rates among bidding war winners – are more pronounced for bidding war transactions during periods and in locations where bidding wars are more common.*

The definition of irrationality used in our theory model is strictly technical. A buyer with limited rationality overestimates the value of the house and overpays for it by failing to account for the fact that other potential buyers had lower estimates of the house's value. This is likely to be driven by a lack of financial sophistication or some other constraint (e.g., time, information, experience, etc.).

Because we do not observe each buyer's level of financial sophistication, we instead use income and education as proxies for financial sophistication. Accordingly, we expect the winner's curse to be stronger for low-income buyers as well as those without a college degree. Likewise, while we do not observe the specific constraints faced by homebuyers, we recognize that exposure to such constraints varies across demographic groups. For example, Section E.7 of the internet appendix presents evidence that minority households are less likely to have prior experience or exposure to homeownership than white households. These points lead to the following prediction.

Prediction 5. *The probability of a bidding war is higher, and the winner's curse effects—such as lower home appreciation rates and higher default rates among bidding war winners—are stronger for the following homebuyer groups: (a) lower income, (b) less educated, and (c) minority.*

3 Data and Measurement

3.1 Housing Transaction Data

We examine the subsequent performance of bidding war transactions using single-family detached housing transaction data from January 2000 through June 2023. We combine multiple listing service (MLS) data with ownership transfer (i.e., deeds) data from Cotality. We restrict our analysis to arms-length transactions (sales between two unrelated parties) in counties where historical data is available back to at least 2003. Although non-arms-length transactions are excluded from our analyses, we rely on them to determine each buyer's holding period and default outcomes before removing them from the sample. This unfiltered dataset includes 18.8 million arms-length housing transactions.

We further restrict the data to include transactions from January 2000 through December 2018, providing a minimum of 30 months to observe the subsequent performance of each transaction in the data. We apply several additional filters to remove outliers and miskeyed information.[4] After applying the filters, the sample we use in the empirical analysis includes just under 14.2 million transactions across 136 counties in 30 states.

The housing transaction data includes information about the physical characteristics of the house (e.g., square feet living area, number of bathrooms), location of the house (e.g., street address, zip code), transaction (e.g., list price, sales price, sale date), as well as an unstructured public remark (i.e., written description) about the house. We use the text in the public remarks field to validate our bidding war measure. The data also includes a unique identifier for each house, allowing us to track its performance over time.

We supplement the housing transaction data with mortgage financing information from Cotality, which includes the fields necessary to link with the Home Mortgage Disclosure Act (HMDA) data. The HMDA data provides information about the buyer, including their race, ethnicity, and income at the time of purchase. We link the HMDA data using the following fields: year, census tract, loan amount, and lender name.

[4]The internet appendix provides a detailed description of each filter and tracks the number of observations the filter removes.

3.2 Identifying Bidding Wars

Identifying whether a house sold via a bidding war is complicated by the fact that bidding wars are not flagged (i.e., they are unobservable) in most datasets. Moreover, even when the number of bids is observable, it is still often unclear if the bids were received sequentially or simultaneously. Although we do not observe the number of bidders involved or bids received in our data, we can observe both the sales price and list price for every housing transaction. Accordingly, we define any transaction whose sales price exceeds its list price as a bidding war.[5] Our definition of bidding wars is similar to previous research on the topic (Han and Strange, 2014, 2015, 2016).

The underlying assumption of our bidding war classification strategy is that competition (or at least the threat of competition) from many bidders causes a bidding war that pushes the sales price above the list price. We test the validity of this assumption by performing a systematic textual analysis of listing agents' public remarks to measure the frequency with which bidding war-related keywords appear in the written description of the houses. We examine whether the comparative frequency of bidding war-related keywords is substantially higher in the public remarks for transactions we classify as bidding wars than those we classify as non-bidding wars. Because no definitive list of bidding war-related keywords exists, we use a modified version of the computer-assisted keyword discovery method in King et al. (2017) to identify relevant bidding war-related keywords. We consider a keyword relevant if it ranks high in terms of its ability to discriminate between a bidding war and a non-bidding war transaction in our dataset.

We calculate the comparative frequency for each keyword as the proportion of bidding war transactions that contain the keyword relative to the proportion of non-bidding war transactions that contain the keyword. We perform the calculation by partitioning the full set of agent remarks S into our target set T of bidding war transactions and nontarget set $S \setminus T$ of non-bidding war transactions. After pre-processing the remarks to remove punctuation, stop words, and non-alphanumeric characters, we mine all the keywords in S with a minimum document frequency of 50. For each keyword k in either set, let $n_{k,T}$ and $n_{-k,T}$ denote the number of remarks in T that do and do not include keyword k, respectively. Similarly, let $n_{k,S\setminus T}$ and $n_{-k,S\setminus T}$ denote the number of remarks in $T \setminus S$ that

[5] We run a series of robustness checks using alternative definitions of bidding wars. Our main findings remain the same using the alternative definitions.

do and do not include keyword k, respectively. Denote the total number of remarks in T as $N_T = n_{k,T} + n_{-k,T}$ and $S \setminus T$ as $N_{S\setminus T} = n_{k,S\setminus T} + n_{-k,S\setminus T}$. Using the notation above, we calculate the comparative frequency of each token as $\frac{n_{k,T}}{N_T} \div \frac{n_{k,S\setminus T}}{N_{S\setminus T}}$.

Table 1 displays five unigram (one-word) and bigram (two-word) keywords with comparative frequencies that are substantially higher for bidding war transactions than non-bidding war transactions. The table lists the keyword, its comparative frequency, and an example of the keyword in context (kwic). The kwic column provides a short snippet of the text where the keyword is mentioned in the remark. Panel A displays five unigram keywords indicating the presence of a bidding war. The keywords indicate that the listing has received multiple competing offers. The agent has then updated the remark to set a *deadline* for offers (such as *tuesday* at 5 pm), notify interested parties when the *received* offers will be *reviewed*, or ask the interested parties for their *highest* and best offer.

The high comparative frequencies in Panel A of Table 1 imply the keywords are substantially more likely to appear in bidding war transactions than non-bidding war transactions. For example, the keyword *deadline* is 7 times more likely to appear in a bidding war transaction than a non-bidding war transaction. This finding implies that our classification strategy effectively captures bidding war transactions because agents do not need to notify buyers of a *deadline* when negotiating with a single buyer (i.e., sequential bargaining). Many other similar bidding war keywords have high comparative frequencies. For example, *monday* (5.81) and *wednesday* (5.02) also have high comparative frequencies, indicating that agents often set their deadline for offers early in the week - presumably after potential buyers have had the opportunity to view the house over the weekend.

The five examples in Panel A of Table 1 also highlight the difficulty of selecting relevant bidding war-related keywords. If one were to manually select keywords based on the text in the kwic column in Panel A, the keyword *offers* seems like a reasonable choice because it appears in all five remark snippets. However, we find the keyword *offers* has a relatively low comparative frequency of 1.07, implying that it appears in bidding war and non-bidding war transactions at roughly the same rate.[6] A review of non-bidding war transactions shows that agents often mention a “backyard that *offers* lots of privacy" or an “open floor design that *offers* room to grow." In other cases, the agent remarks

[6]The keyword *offer* also has a low comparative frequency of 0.97. The findings we present in this section are similar when we stem or lemmatize the remarks.

indicate a "motivated seller open to *offers*." In these three instances, the keyword *offers* does not imply a bidding war is taking place, which explains why the unigram keyword has a relatively low comparative frequency and is not included in Panel A of Table 1.

Panel B of Table 1 displays five bigram keywords indicating the presence of a bidding war. By design, the bigram keywords provide more context than unigram keywords, which helps address the difficulties mentioned above. The additional context results in higher comparative frequencies for the bigram keywords in Panel B than the unigram keywords in Panel A. The high comparative frequencies in Panel B imply that these bigram keywords are substantially more likely to appear in bidding war transactions than in non-bidding war transactions. Once again, this finding implies that our classification strategy effectively captures bidding war transactions.

Although not displayed in Table 1, we find many unigram and bigram keywords with high comparative frequencies that mention a location where bidding wars frequently occur. For example, the unigram keywords *miraloma* (6.36), *westbrae* (6.10), and *ardenwood* (5.76), along with the bigram keywords *outer parkside* (7.82), *mt davidson* (7.62), and *monterey market* (6.59) are a few of many locations in San Francisco, California, that have high comparative frequencies. Similarly, the unigram keyword *bart* - which is an acronym for the Bay Area Rapid Transit system that connects San Francisco to the East Bay, Oakland, and San Jose - also has a high comparative frequency. The high comparative frequency of these keywords aligns with reports of bidding wars in San Francisco dating back to the late 1990s.[7] Taken together with the unigram and bigram bidding war keywords in Table 1, the location keywords provide evidence consistent with the underlying assumption of our bidding war classification strategy that competition from many bidders results in a bidding war that pushes the sales price above the list price.

Table 1 demonstrates that agents are more likely to update the remarks field to set a *deadline* or request bidders' *highest and best* offers in transactions identified as bidding wars. However, it is important to note that agents are not obligated to update the remarks field after listing a property on the MLS and may have less incentive to do so if they already have multiple offers. As a result, the comparative frequencies shown in Table 1 are likely understated, making the text in the remarks field insufficient as a standalone strategy for identifying bidding wars. Instead, we use this textual information to (a) validate our measure and (b) demonstrate that our results remain

[7] See examples of these reports here, here, here, and here.

robust across several alternative bidding war classification methods that incorporate the text in the remarks field.

3.3 Measuring Subsequent Performance

We examine the subsequent performance of bidding war transactions using three distinct measures: unlevered returns, levered returns, and default. All three measures utilize consecutive transactions (i.e., repeat sales) for each property. This section describes how we construct the three measures of subsequent performance.

Our first measure of subsequent performance examines unlevered realized returns for housing earned in the period from purchase to sale. Our approach is similar to Goldsmith-Pinkham and Shue (2023), who examine housing returns by gender. We identify the unlevered annualized return for house i in sale year s as $r_{is} = (\frac{P_{is}}{P_{ib}})^{\frac{1}{(s-b)}} - 1$, where P_{ib} is the purchase price for the house in year b and P_{is} is the subsequent sale price in year s. When calculating the holding period for each transaction, we allow years b and s to be nonintegers. Based on our theoretical prediction in Prediction 1, we expect to find that buyers who purchase their house in bidding wars experience lower unlevered annualized returns (i.e., winner's curse) than buyers who purchase their house in non-bidding war transactions.

Our second measure of subsequent performance addresses the fact that most homebuyers in the United States use leverage (i.e., take out a mortgage) to purchase a house. We find many of the fields (i.e., mortgage type, term, interest rate, and downpayment) necessary to calculate the levered return are not reliably populated in the data. Following Goldsmith-Pinkham and Shue (2023), we address this data issue by computing hypothetical levered returns based on the most common mortgage type in the data: a 30-year fixed rate loan with an initial LTV ratio of 80%.

We estimate the downpayment D_{ib} and mortgage amount M_{ib} for each transaction based on the purchase price and the aforementioned 80% LTV assumption. Using the 30-year fixed-rate mortgage interest rate from Freddie Mac in the month and year of the purchase ρ_{ib}, we calculate the amount of the principal paid down (i.e., amortized) at every monthly duration horizon. Assuming no refinancing, we use the amortization schedule to determine the remaining mortgage balance (i.e., the outstanding principal) when the house is sold, denoted as M_{is}. We then estimate the homeowner's equity

reversion (i.e., cash remaining from the sale after paying off their mortgage) at the time of sale as $Equity_{is} = max\{P_{is} - M_{is}, 0\}$. Next, we estimate the present value of the homeowner's equity investment as the sum of the downpayment plus the discounted value of the principal paydown payment as $Equity_{ib} = D_{ib} + \sum_{\tau=b}^{s} W_{i\tau}/(1+\rho_{ib})^{\tau-b}$. Finally, we estimate the levered annualized return as $r_{is}^{lev} = (\frac{Equity_{is}}{Equity_{ib}})^{\frac{1}{(s-b)}} - 1$. When $Equity_{is} = 0$, the formula incorrectly calculates the buyer's levered annualized return as -100%. We address this issue by straight-lining the buyer's 100% loss over their holding period.

Our third measure of subsequent performance examines whether a buyer subsequently defaults on their loan resulting in a distressed transaction. We construct the subsequent default measure by identifying whether the transaction following the buyer's purchase is distressed. The subsequent default variable equals 1 for transactions where the subsequent transaction is either a short sale or real estate owned (REO) transaction and 0 otherwise. We construct the subsequent default variable and then remove distressed transactions from the data. We initially assume that the buyer did not default if no subsequent transaction exists. For example, if a buyer purchases a house in 2008 and there is no subsequent transaction in the data as of June 2023, we assume the buyer did not default and still owns the house. We relax this assumption in later analysis.

3.4 Summary Statistics

Panel A of Table 2 displays summary statistics for just under 14.2 million transactions where we can credibly identify whether a buyer subsequently defaulted on their mortgage. We limit the subsequent default sample in Panel A to include transactions from 2000 through 2018, which provides a 30-month post-purchase default measurement window for transactions at the tail end of the sample. As noted above, we initially assume that all purchases without a subsequent transaction did not default. Our key empirical results do not change when we further restrict the sample to only include purchases with a subsequent transaction (i.e., the returns sample in Panel C of Table 2).

Panel A of Table 2 reports the average list price, purchase price, and subsequent default rate separated by transaction type (bidding war versus non-bidding war). We refrain from interpreting differences in raw purchase prices in the summary statistics table because houses purchased via bidding wars may be materially different than houses not purchased via bidding wars. We address this concern in the empirical analysis by

focusing on housing returns and subsequent default, both of which hold the property constant. We also refrain from interpreting differences in the subsequent default rates in the summary statistics table because houses purchased via bidding wars may differ in terms of how they are financed compared to houses not purchased via bidding wars. The summary statistics in Panel A of Table 2 validate this concern, showing that a larger share of non-bidding war transactions either involved cash buyers (where default is impossible) or had unknown financing.

Panel B of Table 2 displays summary statistics after we filter out cash purchases as well as purchases with unknown financing. The resulting dataset includes over 8.8 million transactions where we can reliably identify the buyer's equity position at the time of purchase. Panel B displays the average combined loan-to-value (CLTV) ratio at origination by transaction type. The summary statistics in Panel B indicate that bidding war transactions have a slightly higher average CLTV than non-bidding war transactions. We also find that, on average, bidding war transactions default at a higher rate than non-bidding war transactions (11.42% versus 8.28%).

Panel C of Table 2 displays summary statistics for 6.3 million transactions with a subsequent transaction in the data (i.e., a repeat sale) and a minimum holding period of at least six months. We require a subsequent transaction because it allows us to estimate the purchaser's unlevered and levered returns. In doing so, we drop all observations without a subsequent transaction, which explains why the subsequent default rates in Panel C are substantially higher than in Panels A and B. Still, the summary statistics in Panel C indicate that conditional on a subsequent transaction occurring during the study period, bidding war transactions are, on average, more likely to subsequently default than non-bidding war transactions (25.07% versus 16.87%).

Panel D displays summary statistics after we filter out transactions with either unknown financing or cash purchases from the returns sample in Panel C. The summary statistics in Panel D indicate that, on average, bidding war transactions have lower unlevered and levered annualized returns than non-bidding war transactions. Moreover, buyers who purchase their houses via a bidding war hold their house for almost half a year less, on average, than buyers who purchase their houses via non-bidding war transactions (6.27 years versus 6.73 years).

4 Main Results

4.1 Estimation Approach

We test the theoretical hypotheses in Section 2.4 using a linear regression framework that compares the subsequent performance of bidding war transactions relative to non-bidding war transactions. Our empirical analysis takes two forms. The first set of analyses focuses on unlevered and levered annualized returns for bidding war transactions relative to non-bidding war transactions:

$$r_{is} = BiddingWar_{ib}\beta_1 + Hold_{ib}\beta_2 + X'_{ib}\beta_X + \epsilon_{ib} \tag{4.1}$$

where $BiddingWar_{ib}$ is an indicator variable for a bidding war transaction at time b, r_{is} is the annualized return, $Hold_{ib}$ is the holding period length in years, and X'_{ib} is a vector of control variables. The control variables in Equation 4.1 include an indicator variable identifying investor purchases and time-by-location fixed effects. The investor purchase indicator variable equals 1 if the buyer is flagged as a corporate buyer in the ownership transfer data or the occupancy type is recorded as investment property in either the ownership transfer or HMDA data; and 0 otherwise. The location-by-time fixed effects include zip code-by-year-by-quarter fixed effects for the initial purchase transaction and zip code-by-year-by-quarter fixed effects for the sale transaction. Including the fixed effects allows us to estimate the average difference in returns by transaction type in the same zip code and time period of purchase and sale.

The second set of analysis examines the probability that the homebuyer subsequently defaults, resulting in a distressed transaction:

$$Distress_{is} = BiddingWar_{ib}\beta_1 + X_{ib}\beta_X + \epsilon_{ib} \tag{4.2}$$

where $Distress_{is}$ is an indicator identifying if the subsequent transaction is either a short sale or REO, $BiddingWar_{ib}$ is an indicator variable for a bidding war transaction at time b, and X_{ib} is a vector of control variables. The control variables in Equation 4.2 include housing characteristics, financing, investor purchase, as well as time and location controls. We allow the housing characteristics to have non-linear effects on price and create indicator variables for 100-square-foot-wide

bins of living space ($SQFT_{500-599,nt}, SQFT_{600-699,nt}, ...$), 10-year-wide bins of house age ($AGE_{0-9,nt}, AGE_{10-19,nt}, ...$), number of bedrooms, number of bathrooms, and number of acres in lot size. We also include indicators for the presence of a garage and a pool. Recognizing leverage is an important determinant of default, we include indicator variables identifying the CLTV ratio for each transaction.[8] The time and location controls include zip code-by-year-by-quarter fixed effects for the time of purchase.

4.2 Unlevered Returns

Table 3 displays results from Equation 4.1 examining the annualized returns of bidding war transactions relative to non-bidding war transactions. Column 1 of Panel A examines homebuyers' unlevered annualized returns by transaction type using the returns sample in Panel C of Table 2. The coefficient estimate for bidding war transactions in column 1 of Panel A is negative and statistically significant, indicating homebuyers who purchase their house in a bidding war experience 1.3 percentage points (pp) lower unlevered annualized returns than those who purchase their house in a non-bidding war transaction. This coefficient is economically significant as the average annualized unlevered return in the sample is 5.1pp. Consistent with the prediction in Prediction 1, this finding implies that homebuyers who purchase their house in a bidding war experience a 10.5pp lower total return than those who transact in a non-bidding war, assuming a 6.3-year holding period. This is equivalent to an 8.2pp overpayment for houses purchased in bidding wars.[9]

Later in this section, we present results showing that homebuyers who purchase their house via bidding wars are more likely to default. Column 2 of Panel A in Table 3 filters the returns sample in the preceding column to only include transactions that did not subsequently default. Even after removing transactions whose buyers subsequently defaulted, we continue to find that the coefficient estimate for bidding war transactions in column 2 is negative and statistically significant. Conditional on not subsequently defaulting, we find homebuyers who purchase their house in a bidding war experience 1.0pp lower annualized unlevered returns than those who purchase their house in a

[8] CLTV buckets: CLTV=0 (i.e., cash purchases); $0 < \text{CLTV} \leq 60$; $60 < \text{CLTV} < 80$; CLTV=80; $80 < \text{CLTV} \leq 90$; $90 < \text{CLTV} \leq 95$; $95 < \text{CLTV} \leq 100$; CLTV > 100; CLTV is unknown.

[9] Assuming a 6.3-year holding period, 5.3pp unlevered annualized returns on non-bidding war transactions, and 4.0pp unlevered annualized returns on bidding war transactions, we compute the overpayment as follows $(1.053/1.040)^{6.3} - 1 = 0.082$

non-bidding war transaction. This coefficient is economically significant as the average annualized unlevered return for this non-distressed returns subsample is 7.8pp. We find similar results in columns 3 and 4 of Panel A when we filter the returns sample to remove transactions without financing information and cash purchases (i.e., the financed returns sample in Panel D of Table 2).

Overall, the results in Panel A of Table 3 provide evidence consistent with the empirical prediction in Prediction 1 that homebuyers who purchase their house in a bidding war overpay and suffer from a winner's curse. Complementary analyses in the internet appendix provide further support for this interpretation. Using a hedonic model with rich housing characteristics and location-by-time fixed effects, we construct a contemporaneous predicted value for each property and measure the buyer's overpayment relative to fundamentals. We find bidding war transactions exhibit significantly larger overpayments than nonâbidding war transactions in the same neighborhood and market conditions, providing additional evidence that bidding war winners overpay at the time of purchase.[10]

4.3 Levered Returns

Recognizing that most houses are purchased using leverage, Panel B of Table 3 examines the levered returns of bidding war transactions relative to non-bidding war transactions. Column 1 of Panel B examines homebuyers' levered annualized returns by transaction type using the returns sample in Panel C of Table 2. The coefficient estimate for bidding war transactions is negative and statistically significant, indicating homebuyers who purchase their house in a bidding war experience 6.9pp lower levered annualized returns than those who purchase their house in a non-bidding war transaction. This coefficient is economically significant as the average annualized levered return in the sample is 25.7pp.

Column 2 filters the returns samples in column 1 to only include transactions that did not subsequently default. Even after removing transactions whose buyers subsequently defaulted, we continue to find that the coefficient estimate for bidding war transactions in column 2 is negative and statistically significant. Conditional on not subsequently

[10]Our main results focus on subsequent returns rather than contemporaneous overpayment because overpayment is not, by itself, sufficient to establish a winner's curse. A buyer may pay above fundamentals at purchase yet avoid a winner's curse if the transaction price becomes a salient reference point (Andersen et al., 2022; Giacoletti and Parsons, 2023) in future negotiations or market assessments. In such cases, the buyer's realized return need not suffer even if they initially overpaid.

defaulting, we find homebuyers who purchase their house in a bidding war experience 6.5pp lower annualized levered returns than those who purchase their house in a non-bidding war transaction. This coefficient is economically significant as the average annualized levered return for this non-distressed returns subsample is 34.1pp. We find similar results in columns 3 and 4 of Panel B when we filter the returns sample to remove transactions without financing information and cash purchases. Once again, these results provide evidence consistent with the prediction in Prediction 2 that homebuyers who purchase their house in a bidding war overpay and suffer from a winner's curse.

4.4 Subsequent Default

The lower annualized returns for bidding war transactions provide evidence consistent with the predictions generated by Prediction 1 and 2 that bidding war winners overpay for their houses. This section tests Prediction 3, which predicts that homebuyers who purchase their houses in bidding wars are more likely to default on their mortgage than those who purchase their house in non-bidding war transactions. If homebuyers overbid and their houses appreciate less or decrease in value, they are more likely to be underwater, which increases their likelihood of default. Table 4 displays bidding war coefficient estimates from Equation 4.2, examining the probability that buyers in bidding war transactions subsequently default relative to buyers in non-bidding war transactions.

Column 1 of Table 4 uses the subsequent default sample in Panel A of Table 2 that assumes homebuyers without a repeat sale transaction in the data still own their house and did not default on their mortgage. The coefficient estimate for bidding war transactions is positive and statistically significant, indicating homebuyers who purchase their house in a bidding war are 1.9pp more likely to default on their mortgages than buyers who purchase their house in a non-bidding war transaction. This coefficient is economically significant as the mean probability of default in the sample is 8.3pp.

The housing transaction sample in column 1 of Table 4 includes cash purchases (where default is impossible unless the buyer obtains financing later) as well as transactions where financing information is unavailable. Column 2 re-estimates Equation 4.2 after dropping these transactions (i.e., the financed subsequent default sample in Panel B of Table 2). The coefficient estimate for bidding war transactions in column 2 is positive and statistically significant, indicating homebuyers who purchase their house via a bidding war are 1.5pp more likely to default on their mortgages than those who purchase their

house in a non-bidding war transaction. This coefficient is economically significant as the average probability of default in the sample is 8.9pp.

The subsequent default coefficient estimates in columns 1 and 2 of Table 4 may be biased because we assume any homeowner who did not sell their house did not eventually default. Column 3 of Table 4 limits the sample to all transactions with a subsequent transaction in the sample (i.e., the returns sample in Panel C of Table 2). The coefficient estimate for bidding war transactions is positive and statistically significant, indicating homebuyers who purchase their house in a bidding war are 3.4pp more likely to default on their mortgages than those who purchase their house in a non-bidding war. This coefficient is economically significant as the average probability of default in the sample is 18.3pp. Overall, these results provide evidence consistent with the prediction in Prediction 3 that homebuyers who purchase their house in a bidding war are more likely to default on their mortgage, thereby suffering from a winner's curse.

4.5 Alternative Bidding War Measures

This section demonstrates that our main results in the preceding sections are robust to several alternative bidding war classifications. In column 1 of Table 5, we classify a transaction as a bidding war if it sells for at least $1,000 more than its list price. In column 2, we classify a transaction as a bidding war if it sells for more than 1% above its list price. In column 3, we classify a transaction as a bidding war if its sales price exceeds its list price or its public remark contains one of the 41 unigram bidding war keywords with a comparative frequency greater than 5. In column 4, we classify a transaction as a bidding war if its sales price exceeds its list price or its public remark contains one of the 449 bigram bidding war keywords with a comparative frequency greater than 5. Regardless of how we define a bidding war in Table 5, we continue to find that homebuyers who purchase their house via a bidding war experience significantly lower unlevered and levered returns and are significantly more likely to subsequently default compared to homebuyers who do not purchase their house via a bidding war.

4.6 Bidding War Concentration

Figure 1 shows that bidding war activity is highly cyclical: it hovered around 15-25% pre-crisis (2000-2007), fell below 10% during the heart of the financial crisis (2008-2011),

returned to pre-crisis levels by the late 2010s, and surged to nearly 50% in 2021 before moderating in 2023. Prediction 4 predicts that winner's curse effects should be stronger when bidding wars are more prevalent. Motivated by the cyclicality in Figure 1, Table 6 re-estimates our main specifications across the boom (2000-2007), bust (2008-2012), and recovery (2013-2018) periods.

Across all three subperiods, the results in Table 6 broadly align with the full sample findings. During the boom (Panel A) and the recovery (Panel C), houses purchased via bidding wars earn significantly lower annualized returns than comparable non-bidding war transactions. During the bust (Panel B), when bidding wars were relatively rare, the return differentials are smaller and concentrated among borrowers who later default; conditional on no subsequent default, return differences are muted. Consistent with this pattern, bidding war winners are 1-2 pp more likely to default during the bust, whereas default rates do not differ significantly during the recovery, when rising prices allowed distressed owners to sell rather than default.

To further test Prediction 4, we construct a bidding war concentration measure defined as the annual share of transactions in a zip code purchased via bidding wars. We then assign each transaction to a bidding war concentration quartile, where quartile 1 includes transactions in zip codes with the lowest levels, and quartile 4 includes transactions in zip codes with the highest levels of bidding war activity that year. We interact these quartiles with the bidding war indicator in modified versions of Equations 4.1 and 4.2.

Table 7 shows that the adverse return effects associated with bidding wars are largest in zip codes with higher bidding war concentration, consistent with Prediction 4. Both unlevered and levered annualized returns decline monotonically with the bidding war intensity quartiles. Bidding war winners are more likely to default overall, though the quartile interaction is non-monotonic: default risk initially rises in quartile 2 but then decreases in quartile 4 as the concentration of bidding war activity increases.

5 Selection

Because assignment to the bidding war winner group is not random, unobserved buyer characteristics could confound our estimates. We address this concern in three ways. First, we discuss and test several alternative explanations. Second, we apply the methodologies of Oster (2019) and Cinelli and Hazlett (2020) to assess the robustness

of our estimates to selection on unobservables. Third, we implement a judgeâstyle IV strategy that exploits listing agent fixed effects as instruments for bidding wars.

5.1 Alternative Explanations

Bidding war winners may differ along unobserved dimensions - such as credit access, liquidity, patience, or preferences - that independently influence returns and default risk. Several mechanisms could therefore generate lower annualized returns or higher default risk, independent of any winner's curse effect. First, buyers who stretch their credit access or liquidity to win a bidding war may subsequently underinvest in maintenance or lack the capacity to undertake capital improvements, resulting in lower observed returns. Second, bidding war winners may be less patient when purchasing or reselling, leading them to overpay to close quickly or accept lower offers to exit faster. Third, bidding war winners may have a higher valuation for the property because it better matches their preferences, in which case the overpayment reflects match quality rather than a winner's curse.

Importantly, these alternative mechanisms complement rather than contradict our winner's curse interpretation. Several are direct consequences of overpaying to win a bidding war rather than independent explanations for lower returns. For example, if buyers who stretch their credit to win a bidding war subsequently underinvest in maintenance, financial constraints would help explain part of the return differential. However, those constraints are themselves a product of overpayment, reinforcing rather than undermining the winner's curse.

5.1.1 Credit Access and Liquidity

Homebuyers who overpay in bidding wars may subsequently underinvest in maintenance or lack the financial capacity to undertake capital improvements, given the premium paid at purchase. If not properly accounted for, quality improvements made by non-bidding war buyers could be misinterpreted as higher annualized returns. We address this potential omitted-variable bias using text analysis (Nowak and Smith, 2017; Liu et al., 2020). Specifically, we identify and include indicator variables for a set of quality-related tokens extracted from real estate agents' written remarks about the property at the time of both the purchase and resale.

We identify and select the time-varying quality tokens using the double-selection LASSO procedure described in Nowak and Smith (2020). It is essential to include only tokens that convey new information about house quality and exclude redundant tokens that indicate a bidding war transaction, such as those displayed in Table 1. Accordingly, we (a) remove unigram and bigram tokens with comparative frequencies greater than five and (b) use the computer-assisted text cleaning algorithm in Mehrotra et al. (2024) to identify and remove spurious predictors of bidding wars in the remarks. Table 8 presents results from Equations 4.1 and 4.2 with and without quality-related tokens as additional controls. Panel A includes 695 unigram tokens, while Panel B includes 679 bigram tokens in the post-double-selection estimations as controls.

The quality-adjusted coefficients in the even-numbered columns of Table 8 are consistently smaller than their unadjusted counterparts in the odd-numbered columns, indicating that non-bidding war buyers are more likely to undertake capital improvements, less likely to defer maintenance, or both. This pattern aligns with the concern raised at the outset of this subsection: buyers who stretch their credit access or liquidity to win a bidding war may be more financially constrained afterward, limiting their ability to invest in property quality. Such constraints are themselves a consequence of overpaying - and thus should be viewed as part of the winner's curse. Even so, after controlling for time-varying house quality, we continue to find strong evidence that bidding war buyers suffer from a winner's curse (i.e., quality-adjusted lower returns and higher default rate).

5.1.2 Patience

If bidding war winners face higher search costs, they may be willing to overpay in order to close quickly. We evaluate this alternative mechanism by comparing time to close, measured as the number of days between the contract date and the sale date, for buyers who purchase in a bidding war versus those who do not. The identifying assumption is that if bidding war winners have higher search costs, they may be more willing to pay a premium to close quickly. Under this hypothesis, bidding war transactions should have shorter time to close durations than comparable nonâbidding war transactions.

Panel A of Table 9 tests this hypothesis by estimating a modified version of Equation 4.2, where we regress time to close (in days) on the bidding war indicator while controlling for the full set of house, financing, and investor characteristics, along

with zip code-by-quarter-by-year fixed effects for the purchase period. Across all four specifications in Panel A of Table 9, the bidding war coefficient is positive and statistically significant, indicating that the average bidding war transaction takes longer to close, roughly 0.7 to 1.8 days on a 36.3-day mean, than an otherwise comparable non-bidding war transaction. This suggests that high search costs are unlikely to be the primary driver of the observed overpayment, which is intuitive for two reasons. First, it is unlikely that buyers with high search costs would intentionally seek out properties where they would face competition, and second, they would be expected to overpay (relative to low search cost buyers) regardless of whether they face competition.

If bidding war winners are less patient when reselling, they may accept lower offers at resale in order to sell quickly, which would result in lower observed annualized returns. To evaluate this alternative mechanism, we compare resale time on market, measured as the number of days from the resale listing date to its contract date, for houses originally purchased in bidding wars versus those purchased in non-bidding wars.[11] The identifying assumption is that if bidding war winners are less patient, their subsequent resale listings should exhibit shorter time on market durations.

Panel B of Table 9 tests this hypothesis by estimating a modified version of Equation 4.2, where we regress resale time on market on the bidding war indicator, the full set of house, financing, and investor controls, and zipâcodeâbyâquarterâbyâyear fixed effects for the resale period. In Columns 1 and 3, the bidding war coefficient is positive and statistically significant, indicating that the average house purchased in a bidding war takes longer to resell, roughly 0.5 to 0.6 days on a 51.9-day mean, than a comparable house purchased in a non-bidding war transaction. However, these specifications include homebuyers who subsequently defaulted - cases in which the lender or servicer, rather than the homeowner, determines the time on market. Once we exclude these lender-managed resales, the coefficient becomes negative in Columns 2 and 4 and statistically insignificant in Column 4. Taken together, the results in Panels A and B indicate that neither high search costs nor impatience at resale is the primary driver of the lower returns experienced by bidding war winners.

[11] We find similar results when we measure resale time on market as the number of days from the resale listing date to its sale date.

5.1.3 Preferences

A homebuyer may overbid for a house because they receive a bad signal, thereby suffering a winner's curse. Alternatively, the homebuyer may bid more because the house better matches their preferences, and they place a higher value on it. In this case, the overpayment is rational and does not reflect a winner's curse. We examine this alternative explanation by comparing the holding periods of buyers who purchase their houses in a bidding war to those who do not. The underlying assumption is that if bidding war winners overpay because the house is a better match, they should exhibit longer holding periods, as a stronger preference match reduces the incentive to sell and trade up/down.

Panel C of Table 9 tests this hypothesis by estimating a modified version of Equation 4.2, regressing buyer holding period (in days) on the bidding war indicator, the full set of house, financing, and investor controls, and zip-code-by-quarter-by-year fixed effects for the purchase period. The coefficient on bidding war indicator is negative and statistically significant across all four columns. In Columns 1 and 3, the estimates imply that buyers who purchase their house in bidding wars hold them for 53 to 60 fewer days, on average, than buyers in non-bidding war transactions. After excluding buyers who subsequently default, the estimate remains negative, but its magnitude declines. Overall, the results in Panel C reject match quality as an alternative explanation. Bidding war winners do not hold their homes longer, suggesting they do not purchase because the house is a better match. Instead, the shorter holding periods are more consistent with the idea that homebuyers overbid due to receiving limited information signals, leaving them vulnerable to the winner's curse.

5.2 Omitted Variable Bias

In the internet appendix, we evaluate the potential influence of unobserved confounding using the sensitivity frameworks of Oster (2019) and Cinelli and Hazlett (2020). Across all outcomes, both approaches show that unobservables would need to be far more predictive than the extensive set of observed covariates to eliminate the estimated effects. The Oster implied strength of unobserved selection is extremely large in the baseline specifications and remains well above conventional thresholds even after adding hundreds of quality-related controls. The Cinelli-Hazlett robustness values

similarly indicate that only confounders several times stronger than the most predictive benchmark covariates (e.g., investor purchases for returns, the set of CLTV indicators for default) could fully attenuate the coefficients. Such confounding magnitudes are implausible given the richness of the observed controls, implying that omitted variable bias cannot fully account for the lower returns and higher default rates among bidding war winners.

5.3 Listing Agent IV Estimation

To further address concerns that bidding war winners face adverse outcomes for reasons unrelated to the bidding process itself, we implement a judge-style IV research design (Dobbie et al., 2017; Stevenson, 2018) that introduces variation in bidding wars unrelated to the buyer's unobservable characteristics. Specifically, we instrument for bidding wars using listing agent fixed effects. Our approach leverages the fact that some listing agents are more likely and capable of employing listing strategies that generate bidding wars.

Figure 2 documents this heterogeneity by plotting each agent's average bidding war share against their total listing volume (winsorized at 2,000 for agents with a minimum of 10 listings). The fitted red line in Figure 2 captures the average relationship between listing volume and bidding war incidence across agents. The red line is relatively flat, indicating that bidding war propensity does not mechanically increase with agent experience or scale. Instead, the figure reveals substantial dispersion in bidding war shares across agents at all levels of listing volume. This wide cross-sectional variation confirms that agents differ meaningfully in their ability to induce bidding wars, providing the relevance needed for our IV strategy.[12]

We argue these instruments satisfy the exclusion restriction because although buyers can choose the neighborhood in which to live, they face a limited set of available properties for sale (Bayer et al., 2008; Han and Strange, 2015) and have little control over which agent lists a given property in the neighborhood. The exclusion restriction would be violated if either (i) a single listing agent dominated sales in an area or (ii) all agents in a given area uniformly used bidding wars.

Figure 3a addresses the first concern by plotting the distribution of listing activity across agents within each zip code. For each zip code, we compute the share of listings

[12]Additional results in the internet appendix show that our IV results are largely unchanged when we drop transactions by listing agents in the top 5% of listing volume within each state.

for every agent and construct a zip code-level Herfindahl-Hirschman Index based on these market shares. The distribution indicates that agent concentration at the zip code level is generally low. In most zip codes, no single agent accounts for more than a small fraction of listings, and only a few zip codes contain an agent with more than 10 percent of the market. In the internet appendix, we show that our IV results are unchanged when we exclude high-concentration zip codes in which an agent exceeds a 10 percent share.

Figure 3b addresses the second concern by testing whether agents within a zip code uniformly adopt similar bidding war strategies, which would undermine the identifying variation in agentâlevel bidding war propensities. For each zip code, we compute the within-zip code standard deviation of agent-level bidding war propensities. The distribution of this dispersion measure shows substantial heterogeneity within most zip codes, inconsistent with a zip code-wide, uniform listing strategy. A robustness check in the internet appendix shows that our IV results are unchanged when we exclude low-dispersion zip codes with a standard deviation less than 0.20.[13]

Table 10 reports the IV estimation results using a subsample of transactions for listing agents with at least ten transactions in the data. Columns 1, 3, and 5 of Table 10 present OLS estimates, which are directly comparable to column 1 of Table 3 and column 3 of Table 4. Columns 2, 4, and 6 report IV estimates using listing agent-level fitted bidding war propensities. We estimate these propensities using a linear probability model with property controls, zip code-by-quarter-by-year fixed effects, and listing agent ID indicators. We then use the fitted listing agent ID coefficients from this first stage as an instrument.[14] Our approach isolates cross-agent variation in bidding war propensity net of house, time, and neighborhood factors. Crucially, the IV results support our central finding that adverse buyer outcomes (i.e., winner's curse) stem from bidding wars themselves rather than selection into them.

6 The Winner's Curse and Housing Inequality

Prediction 5 predicts a higher probability of a bidding war and stronger winner's curse effects for (a) lower income, (b) less educated, and (c) minority homebuyers. This

[13] Additional robustness checks in the internet appendix show that our IV estimates remain unchanged when all the figures and specifications in this section are re-estimated at the census tract level.

[14] The cluster-robust F-statistic from the first-stage regression is 305.7. Results are robust to an alternative first-stage estimation using a modified leave-one-out procedure.

section tests these predictions using neighborhood-level demographic data. Table 11 examines how the probability of a bidding war varies with zip code-level demographics from the 2011 American Community Survey. Each column displays coefficient estimates from a modified version of Equation 4.2, where the dependent variable equals 1 if the transaction is a bidding war and 0 otherwise. The regressions compare quartiles 2, 3, and 4 of the given neighborhood demographic relative to quartile 1.

The results in Table 11 show that the probability of a bidding war transaction increases with the median household income of the neighborhood and the fraction of Black and less educated (i.e., no college degree) households in the neighborhood. These results provide evidence that is mostly consistent with Prediction 5, the only conflict being that the probability of a bidding war increases as the median household income of the neighborhood increases. We explore this result in more detail using transaction-level household income from HMDA in the next section.

Table 12 examines how the subsequent performance of bidding war transactions varies with zip code-level demographics from the 2011 American Community Survey. Table 12 reports the bidding war coefficient estimate for a subsample of transactions in each quartile. The subsample analyses in Panel A (unlevered annualized returns) and Panel B (levered annualized returns) use Equation 4.1, whereas the subsample analyses in Panel C (subsequent default) use Equation 4.2.

Panels A and B of Table 12 display (mostly) similar patterns for unlevered and levered returns across the zip code-level demographic quartiles. We find that the magnitude of the lower average annualized returns for bidding war transactions decreases with income and education, but increases with the fraction of Black households in the neighborhood. Although the magnitude decreases, the lower annualized returns (relative to non-bidding war transactions) in the higher income and education quartiles remain economically and statistically significant.

In Panel C of Table 12, we find that the probability that buyers who purchase their house in a bidding war are more likely to subsequently default decreases with income and education, but increases with the fraction of Black households in the neighborhood. Although the probability of subsequent default decreases with income and education, the bidding war coefficient estimates in the higher quartiles remain economically and statistically significant. Overall, these results align with the prediction in Prediction 5

that the winner's curse effects we document are stronger for minority, lower-income, and less educated homebuyers.

The internet appendix presents additional analyses using transaction-level demographic and financial information from HMDA. Consistent with Prediction 5, we find that lower-income, Black, and Hispanic homebuyers are disproportionately likely to win bidding wars and to experience more adverse consequences when they do. Across non-bidding war transactions, minority and lower-income buyers experience lower annualized returns and exhibit higher default rates than White and higher-income buyers. Bidding wars further widen these disparities: buyers of all races and income levels earn lower returns when purchasing in a bidding war, but the interaction estimates show that Black, Hispanic, and lower-income buyers experience disproportionately larger reductions in both unlevered and levered returns, as well as higher subsequent default probabilities. Together, these results indicate that bidding wars not only occur more frequently among socioeconomically vulnerable buyers but also amplify preexisting racial and income gaps in realized housing outcomes.

7 Conclusion

This paper presents the first empirical evidence of a winner's curse in the context of informal bidding wars, a phenomenon that has become increasingly prevalent in U.S. residential housing markets. Using a theoretical framework tailored to the institutional details of the homebuying process and a rich dataset of over 14 million transactions, we demonstrate that buyers who win bidding wars systematically overpay. Consistent with our model's predictions, these buyers experience significantly lower subsequent house price appreciation and higher mortgage default rates relative to buyers in non-bidding war transactions. These findings hold across multiple definitions of bidding wars, persist after extensive robustness checks, and are strongest in periods and locations where bidding wars are more common. Moreover, estimates from a listing agent IV design align with our main results, showing that the bidding war mechanism, and not selection into bidding wars, drives the adverse buyer outcomes.

We also show that the incidence and severity of the winner's curse in housing are unevenly distributed across buyers. Lower-income, less educated, and minority households are not only more likely to win bidding wars but also more likely to

bear the adverse winner's curse effects. These patterns are consistent with existing evidence on racial and socioeconomic disparities in housing returns (Kahn, 2024; Kermani and Wong, 2026) but reveal a previously overlooked mechanism: intensified bidding competition. In the context of the current U.S. affordability crisis, this mechanism carries important policy implications. If bidding wars disproportionately harm socioeconomically disadvantaged buyers, their growing prevalence may exacerbate wealth inequality and diminish the long-term benefits of homeownership for these groups.

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Tables and Figures

Table 1. Comparative frequency of bidding war keywords in agent remarks

Keyword (1)	Comp Freq (2)	Keyword in Context (3)
Panel A: Unigrams		
deadline	7.16	*deadline* for offers end of day august 10th
tuesday	5.97	multiple offers sellers will review offers submitted by *tuesday* at 5pm
reviewed	5.73	all offers will be *reviewed* june 17th at 100pm
highest	5.20	sellers requested *highest* and best offers no later than 2pm on monday
received	5.12	*received* several offers will continue to accept offers until noon on monday
Panel B: Bigrams		
deadline offers	15.10	*deadline* [for] *offers* is monday at 5pm
due tuesday	14.77	received mult offers best *due tuesday* by 2pm
offer deadline	12.76	*offer deadline* noon on monday
highest best	11.84	several offers recvd *highest* [and] *best* due no later than 5pm cst on october 6th
multiple offers	11.31	*multiple offers* in hand

Note: Table 1 provides an example of five unigram (one word) and bigram (two word) keywords in the MLS public remarks field with comparative frequencies that are substantially higher for bidding war transactions than non-bidding war transactions. We remove common stop words (e.g., in, and, at) when running our computer-assisted keyword discovery process but include them in the keyword in context (i.e., column 3) to facilitate readability.

Table 2. Summary Statistics

	Bidding War	Non-bidding War	Total
	(1)	(2)	(3)
Panel A: Subsequent Default Sample			
Log(List Price)	12.4085	12.4475	12.4406
Log(Purchase Price)	12.4327	12.3881	12.3960
Cash Purchase	0.0655	0.1000	0.0940
Unknown Financing	0.2442	0.2916	0.2832
Investor Purchase	0.1222	0.1429	0.1393
Subsequent Default	0.1113	0.0764	0.0825
Observations	2,497,683	11,697,651	14,195,334
Panel B: Financed Subsequent Default Sample			
Log(List Price)	12.4581	12.5039	12.4950
Log(Purchase Price)	12.4819	12.4514	12.4573
Investor Purchase	0.1389	0.1689	0.1630
CLTV	87.4608	86.3088	86.5335
Subsequent Default	0.1142	0.0828	0.0889
Observations	1,724,019	7,116,838	8,840,857
Panel C: Returns Sample			
Log(List Price)	12.3176	12.4066	12.3911
Log(Purchase Price)	12.3405	12.3508	12.3490
Cash Purchase	0.0553	0.0899	0.0839
Unknown Financing	0.2590	0.2999	0.2928
Investor Purchase	0.1070	0.1279	0.1243
Subsequent Default	0.2507	0.1687	0.1829
Annualized Unlevered Return	0.0385	0.0530	0.0505
Annualized Levered Return	0.2130	0.2662	0.2569
Holding Period (Years)	6.3354	6.7942	6.7146
Observations	1,104,862	5,264,532	6,369,394
Panel D: Financed Returns Sample			
Log(List Price)	12.3667	12.4600	12.4422
Log(Purchase Price)	12.3891	12.4101	12.4061
Investor Purchase	0.1263	0.1555	0.1499
CLTV	88.8509	85.2729	85.9557
Subsequent Default	0.2597	0.1832	0.1978
Annualized Unlevered Return	0.0291	0.0371	0.0356
Annualized Levered Return	0.1555	0.1649	0.1631
Holding Period (Years)	6.2722	6.7315	6.6439
Observations	757,668	3,212,701	3,970,369

Note: Table 2 reports summary statistics for the samples used in the empirical analysis, split by transaction type (bidding war versus non-bidding war) and also pooled. Panel A includes all transactions from January 2000 through December 2018 where we can credibly identify whether a buyer subsequently defaulted on their loan. Panel B further filters out transactions with either unknown financing or cash purchases. Panel C includes a subsample of transactions with a repeat sale and a minimum holding length of six months. Panel D further filters the data in Panel C to remove transactions with either unknown financing or cash purchases.

Table 3. Annualized Returns

	(1)	(2)	(3)	(4)
Panel A: Unlevered Returns				
Bidding war	−0.013***	−0.009***	−0.007***	−0.004***
	(0.0003)	(0.0004)	(0.0002)	(0.0002)
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.268	0.234	0.481	0.352
Panel B: Levered Returns				
Bidding war	−0.063***	−0.058***	−0.027***	−0.024***
	(0.003)	(0.003)	(0.001)	(0.002)
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.177	0.194	0.246	0.238
Hold Period	✓	✓	✓	✓
Investor Control	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓
Subsequent Defaults	✓		✓	
Data Sample	Panel C	Panel C	Panel D	Panel D

*p<0.1; **p<0.05; ***p<0.01

Note: Table 3 examines the annualized unlevered and levered returns of houses purchased in bidding war transactions relative to those purchased in non-bidding war transactions. Panel A displays coefficient estimates from Equation 4.1 examining the unlevered returns. Panel B displays coefficient estimates from Equation 4.1 examining the levered returns. Column 1 uses the returns transaction sample in Panel C of Table 2. Column 3 uses the financed returns transaction sample in Panel D of Table 2. Columns 2 and 4 further restrict the returns sample in the preceding columns to only include transactions that did not subsequently default. Standard errors are clustered by zip code.

Table 4. Subsequent Default

	(1)	(2)	(3)	(4)
Bidding war	0.019***	0.015***	0.034***	0.024***
	(0.0004)	(0.0004)	(0.001)	(0.001)
Observations	14,195,334	8,840,857	6,369,394	3,970,369
Adjusted R^2	0.179	0.214	0.251	0.288
House Characteristics	✓	✓	✓	✓
Financing Controls	✓	✓	✓	✓
Investor Control	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓
Data Sample	Panel A	Panel B	Panel C	Panel D

*p<0.1; **p<0.05; ***p<0.01

Note: Table 4 examines the subsequent loan performance of houses purchased in bidding war transactions using the four distinct nationwide housing transaction samples in Table 2. Each column displays bidding war coefficient estimates from Equation 4.2 examining the probability that buyers in bidding war transactions subsequently default relative to buyers in non-bidding war transactions. Every column includes the full set of housing characteristics, investor, and financing controls, as well as zip-by-quarter-by-year fixed effects. The data sample row identifies the corresponding panel of summary statistics in Table 2. Standard errors are clustered by zip code.

Table 5. Alternative Bidding War Measures

	Above List Plus $1K	Above List Plus 1%	Above List Plus Unigram	Above List Plus Bigram
	(1)	(2)	(3)	(4)
Panel A: Unlevered Returns				
Bidding war	−0.014***	−0.012***	−0.012***	−0.012***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Observations	6,369,394	6,369,394	6,369,394	6,369,394
Adjusted R^2	0.268	0.268	0.268	0.268
Panel B: Levered Returns				
Bidding war	−0.068***	−0.056***	−0.061***	−0.057***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	6,369,394	6,369,394	6,369,394	6,369,394
Adjusted R^2	0.177	0.177	0.177	0.177
Panel C: Subsequent Default				
Bidding war	0.049***	0.051***	0.045***	0.045***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	6,369,394	6,369,394	6,369,394	6,369,394
Adjusted R^2	0.234	0.234	0.235	0.235

*p<0.1; **p<0.05; ***p<0.01

Note: Table 5 examines the subsequent performance of bidding war transactions relative to non-bidding war transactions using several alternative definitions of a bidding war transaction. Panels A and B, which examine annualized unlevered and levered returns, display coefficient estimates for the alternative bidding war variables from Equation 4.1 that include holding period and investor controls, as well as zip-code-by-quarter-by-year fixed effects for the purchase and resale periods. Panel C, which examines subsequent default, displays coefficient estimates for the alternative bidding war variables from Equation 4.2 that includes house, financing, and investor controls, and zip-code-by-quarter-by-year fixed effects for the purchase period. In column 1, a transaction is classified as a bidding war if it sells for $1,000 more than its list price. In column 2, a transaction is classified as a bidding war if its sale price is more than 1% above its list price. In column 3, a transaction is classified as a bidding war if its sales price exceeds its list price or the agents' public remark includes a high comparative frequency unigram keyword. In column 4, a transaction is classified as a bidding war if its sales price exceeds its list price or the agents' public remark includes a high comparative frequency bigram keyword. Every column uses the returns transaction sample from Panel C of Table 2. Standard errors are clustered by zip code.

Table 6. Subperiod Analyses

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Housing boom (2000-2007)						
Bidding war	-0.012^{***}	-0.008^{***}	-0.048^{***}	-0.044^{***}	0.037^{***}	0.047^{***}
	(0.0003)	(0.0004)	(0.002)	(0.003)	(0.001)	(0.001)
Observations	3,823,526	2,775,425	3,823,526	2,775,425	6,488,944	3,823,526
Adjusted R^2	0.443	0.364	0.282	0.301	0.138	0.212
Panel B: Housing bust (2008-2012)						
Bidding war	-0.004^{***}	-0.0002	-0.022^{***}	-0.004	0.009^{***}	0.020^{***}
	(0.001)	(0.001)	(0.007)	(0.007)	(0.001)	(0.001)
Observations	988,583	905,045	988,583	905,045	2,284,942	988,583
Adjusted R^2	0.497	0.554	0.591	0.650	0.058	0.106
Panel C: Housing recovery (2013-2018)						
Bidding war	-0.012^{***}	-0.012^{***}	-0.080^{***}	-0.079^{***}	-0.0004^{***}	-0.0001
	(0.001)	(0.001)	(0.006)	(0.006)	(0.0001)	(0.0003)
Observations	1,557,285	1,523,878	1,557,285	1,523,878	5,421,448	1,557,285
Adjusted R^2	0.344	0.344	0.305	0.305	0.009	0.029
Investor Control	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel A	Panel C

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Note: Table 6 displays subperiod analyses examining the subsequent performance of bidding war transactions relative to non-bidding war transactions. Panel A uses transactions from the housing boom (2000-2007), Panel B uses transactions from the housing bust (2008-2012), and Panel C uses transactions from the housing recovery (2013-2018). Columns 1 and 2 display bidding war coefficient estimates from Equation 4.1 examining the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions. Columns 3 and 4 display bidding war coefficient estimates from Equation 4.1 examining the annualized levered returns of bidding war transactions relative to non-bidding war transactions. Columns 1 through 4 use the returns sample in Table 2, although columns 2 and 4 remove homebuyers who subsequently default. Columns 5 and 6 display bidding war coefficient estimates from Equation 4.2 examining the probability that homebuyers in bidding war transactions subsequently default relative to those in non-bidding war transactions. Column 5 uses the subsequent default sample in Table 2, whereas column 6 uses the returns sample. Standard errors are clustered by zip code.

Table 7. Bidding War Concentration

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	−0.006***	0.001	−0.0001	0.018	0.021***	0.038***
	(0.001)	(0.001)	(0.010)	(0.011)	(0.001)	(0.002)
Quartile 2	0.010	0.015	0.079	0.120	0.013	0.006
	(0.011)	(0.014)	(0.080)	(0.107)	(0.024)	(0.043)
Quartile 3	0.016	0.020	0.140	0.178	0.009	−0.002
	(0.012)	(0.015)	(0.087)	(0.113)	(0.027)	(0.049)
Quartile 4	0.003	0.004	0.054	0.061	0.0001	−0.014
	(0.015)	(0.017)	(0.108)	(0.138)	(0.028)	(0.051)
Bidding war × Quartile 2	−0.007***	−0.007***	−0.055***	−0.061***	0.004***	0.007***
	(0.001)	(0.001)	(0.011)	(0.013)	(0.001)	(0.002)
Bidding war × Quartile 3	−0.008***	−0.010***	−0.071***	−0.081***	0.001	−0.001
	(0.001)	(0.001)	(0.011)	(0.012)	(0.001)	(0.002)
Bidding war × Quartile 4	−0.009***	−0.014***	−0.080***	−0.105***	−0.005***	−0.014***
	(0.001)	(0.001)	(0.010)	(0.012)	(0.001)	(0.002)
Observations	6,369,394	5,204,348	6,369,394	5,204,348	14,195,334	6,369,394
Adjusted R^2	0.268	0.234	0.177	0.194	0.179	0.251
Investor Control	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel A	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table 7 examines the subsequent performance of bidding war transactions relative to non-bidding war transactions in zip codes with varying degrees of bidding war activity. We measure bidding war activity by calculating the fraction of transactions in each zip code that were purchased via a bidding war that year. Quartile 1 includes transactions in zip codes with low levels of bidding war activity that year, whereas Quartile 4 includes transactions in zip codes with high levels of bidding war activity that year. Columns 1 and 2 display bidding war coefficient estimates from Equation 4.1 examining the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions. Columns 3 and 4 display bidding war coefficient estimates from Equation 4.1 examining the annualized levered returns of bidding war transactions relative to non-bidding war transactions. Columns 5 and 6 display bidding war coefficient estimates from Equation 4.2 examining the probability that homebuyers in bidding war transactions subsequently default relative to those in non-bidding war transactions. Standard errors are clustered by zip code.

Table 8. Quality-Adjusted Estimates

	Unlevered Returns		Levered Returns		Subsequent Default	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unigram Tokens						
Bidding war	−0.013***	−0.009***	−0.064***	−0.045***	0.035***	0.025***
	(0.0004)	(0.0003)	(0.003)	(0.003)	(0.001)	(0.001)
Observations	5,453,370	5,453,370	5,453,370	5,453,370	5,453,370	5,453,370
Adjusted R^2	0.254	0.301	0.172	0.190	0.251	0.376
Panel B: Bigram Tokens						
Bidding war	−0.013***	−0.010***	−0.064***	−0.050***	0.035***	0.030***
	(0.0004)	(0.0003)	(0.003)	(0.003)	(0.001)	(0.001)
Observations	5,453,370	5,453,370	5,453,370	5,453,370	5,453,370	5,453,370
Adjusted R^2	0.254	0.287	0.172	0.185	0.251	0.321
Investor Control	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table 8 examines the subsequent performance of bidding war transactions relative to non-bidding war transactions using a subsample of repeat-sales transactions with public remarks exceeding ten characters in length. The odd-numbered columns present raw, unadjusted coefficient estimates, while the even-numbered columns present quality-adjusted estimates. In Panel A, quality adjustments in the even columns are based on unigram tokens. In Panel B, they are based on bigram tokens. Columns 1 and 2 display bidding war coefficient estimates from Equation 4.1 examining the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions. Columns 3 and 4 display bidding war coefficient estimates from Equation 4.1 examining the annualized levered returns of bidding war transactions relative to non-bidding war transactions. Columns 5 and 6 display bidding war coefficient estimates from Equation 4.2 examining the probability that homebuyers in bidding war transactions subsequently default relative to those in non-bidding war transactions. Standard errors are clustered by zip code.

Table 9. Buyer Patience and Preferences

	(1)	(2)	(3)	(4)
Panel A: Purchase Time to Close				
Bidding war	1.668***	1.846***	0.713***	0.788***
	(0.053)	(0.060)	(0.039)	(0.045)
Observations	5,973,192	4,877,799	3,717,831	2,977,956
Adjusted R^2	0.207	0.214	0.241	0.248
Panel B: Resale Time on Market				
Bidding war	0.472***	−0.190***	0.643***	−0.100
	(0.073)	(0.070)	(0.085)	(0.078)
Observations	6,180,361	5,034,515	3,849,714	3,077,403
Adjusted R^2	0.170	0.209	0.177	0.226
Panel C: Holding Period				
Bidding war	−60.497***	−14.537***	−53.870***	−19.327***
	(1.942)	(2.405)	(2.208)	(2.735)
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.166	0.193	0.168	0.198
House Characteristics	✓	✓	✓	✓
Financing Controls	✓	✓	✓	✓
Investor Control	✓	✓	✓	✓
Zip-QY FE	✓	✓	✓	✓
Subsequent Defaults	✓		✓	
Data Sample	Panel C	Panel C	Panel D	Panel D

*p<0.1; **p<0.05; ***p<0.01

Note: Panel A of Table 9 compares the time to close (in days) for properties purchased in bidding wars with those purchased in non-bidding war transactions. Time to close is measured as the number of days from the listing's contract date to its sale date. Panel B compares the resale time on market (in days) for properties originally bought through bidding wars with those purchased in non-bidding war transactions. Resale time on market variable is measured as the number of days from the property's listing date to its contract date when it is resold. Panel C compares buyers' holding periods, measured as the number of days between the initial purchase and the subsequent resale. Column 1 uses the returns transaction sample in Panel C of Table 2. Column 3 uses the financed returns transaction sample in Panel D of Table 2. Columns 2 and 4 further restrict the returns sample in the preceding columns to only include transactions that did not subsequently default. The zip code-by-quarter-by-year fixed effects are for the time of purchase in Panels A and C and the time of resale in Panel B. The observation counts in Panel A (B) are slightly lower than those in Panel C because we drop observations whose time to close (resale time on market) is missing, negative, or greater than 180 (730) days. Standard errors are clustered by zip code.

Table 10. Listing Agent Instrument

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.013^{***}		-0.066^{***}		0.037^{***}	
	(0.0004)		(0.003)		(0.001)	
Bidding war (fitted)		-0.026^{***}		-0.126^{***}		0.020^{***}
		(0.002)		(0.018)		(0.003)
Observations	4,745,684	4,745,684	4,745,684	4,745,684	4,745,684	4,745,684
Adjusted R^2	0.247	0.246	0.171	0.170	0.236	0.235
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Each column in Table 10 is estimated using the subsample of transactions involving listing agents with at least ten sales in the returns sample in Panel C of Table 2. The odd columns display reduced form estimates examining the subsequent performance of bidding war transactions relative to non-bidding war transactions. The even columns display 2SLS estimates where we instrument for a bidding war using listing agent fixed effects. Columns 1 and 2 examine unlevered returns, Columns 3 and 4 examine levered returns, and Columns 5 and 6 examine the probability of subsequent default of bidding war transactions relative to non-bidding war transactions. Standard errors are clustered by zip code.

Table 11. Probability of a Bidding War by Neighborhood Demographic Quartiles

	Fraction Black	Median Family Income	Fraction College Degree
	(1)	(2)	(3)
Quartile 2	-0.005^{***}	0.020^{***}	-0.003^{***}
	(0.00004)	(0.0001)	(0.0001)
Quartile 3	0.029^{***}	0.030^{***}	-0.016^{***}
	(0.0001)	(0.0002)	(0.0001)
Quartile 4	0.025^{***}	0.023^{***}	-0.039^{***}
	(0.0001)	(0.0004)	(0.0001)
Observations	13,681,109	13,681,109	13,681,109
Adjusted R^2	0.120	0.120	0.120
House Characteristics	✓	✓	✓
Financing Controls	✓	✓	✓
Investor Control	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓
Subsequent Defaults	✓	✓	✓
Data Sample	Panel A	Panel A	Panel A

*p<0.1; **p<0.05; ***p<0.01

Note: Table 11 examines whether the probability of a bidding war transaction varies with zip code-level demographics from the 2011 American Community Survey. Each column displays coefficient estimates from a modified version of Equation 4.2, where the dependent variable equals 1 if the transaction is a bidding war and 0 otherwise. The regressions compare quartiles 2 to 4 of the given neighborhood demographic relative to quartile 1. Standard errors are clustered by zip code.

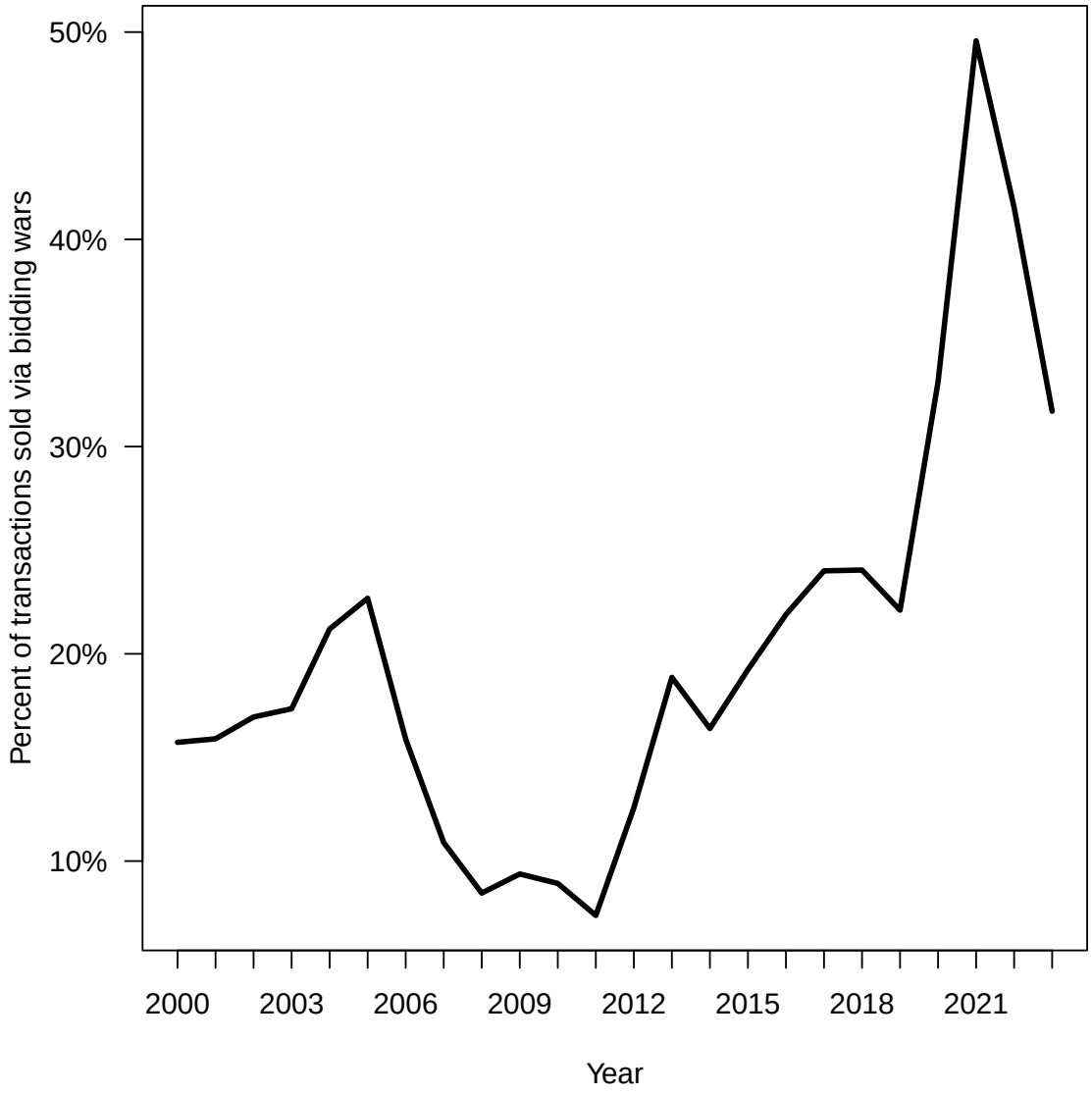
Table 12. Heterogeneity by Zip Code level Demographics

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
	(1)	(2)	(3)	(4)
Panel A: Unlevered Annualized Returns				
Fraction Black	-0.008^{***}	-0.009^{***}	-0.012^{***}	$-0.020^{***,\dagger}$
Median Family Income	-0.024^{***}	-0.012^{***}	-0.009^{***}	$-0.005^{***,\dagger}$
Fraction College Degree	-0.021^{***}	-0.013^{***}	-0.008^{***}	$-0.006^{***,\dagger}$
Panel B: Levered Annualized Returns				
Fraction Black	-0.041^{***}	-0.041^{***}	-0.060^{***}	$-0.105^{***,\dagger}$
Median Family Income	-0.128^{***}	-0.057^{***}	-0.040^{***}	$-0.023^{***,\dagger}$
Fraction College Degree	-0.109^{***}	-0.061^{***}	-0.039^{***}	$-0.030^{***,\dagger}$
Panel C: Subsequent Default				
Fraction Black	0.016^{***}	0.017^{***}	0.020^{***}	$0.025^{***,\dagger}$
Median Family Income	0.025^{***}	0.021^{***}	0.019^{***}	$0.014^{***,\dagger}$
Fraction College Degree	0.026^{***}	0.023^{***}	0.019^{***}	$0.008^{***,\dagger}$

*p<0.1; **p<0.05; ***p<0.01

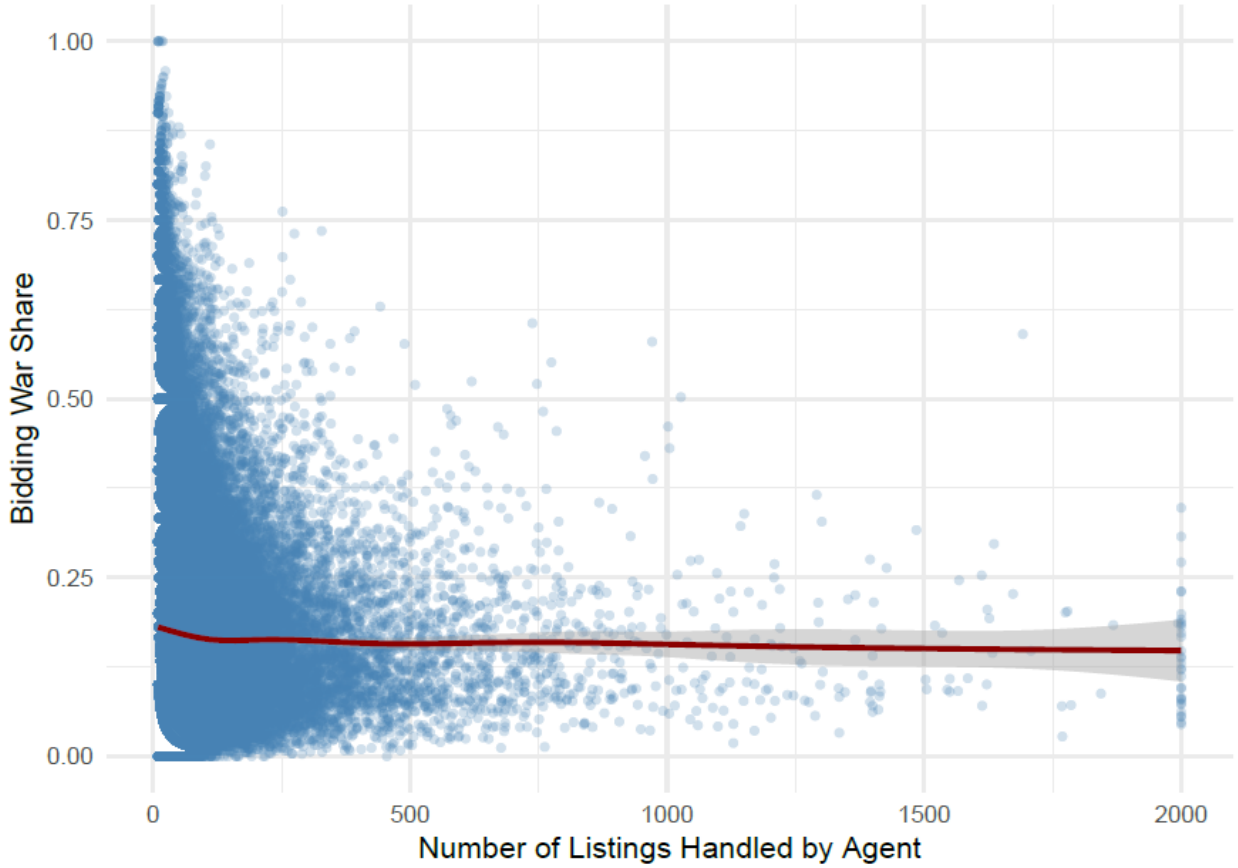
Note: Table 12 displays bidding war coefficient estimates from subsample analyses across quartiles of various zip-level demographic characteristics from the 2011 American Community Survey. Panel A displays bidding war coefficient estimates from Equation 4.1 examining the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions. Panel B displays bidding war coefficient estimates from Equation 4.1 examining the annualized levered returns of bidding war transactions relative to non-bidding war transactions. Panel C displays bidding war coefficient estimates from Equation 4.2 examining the probability that a homebuyer in a bidding war transaction subsequently defaults relative to a non-bidding war transaction. † indicates the Quartile 4 bidding war coefficient estimate is statistically different than the Quartile 1 bidding war coefficient estimate.

Figure 1. Bidding War Share



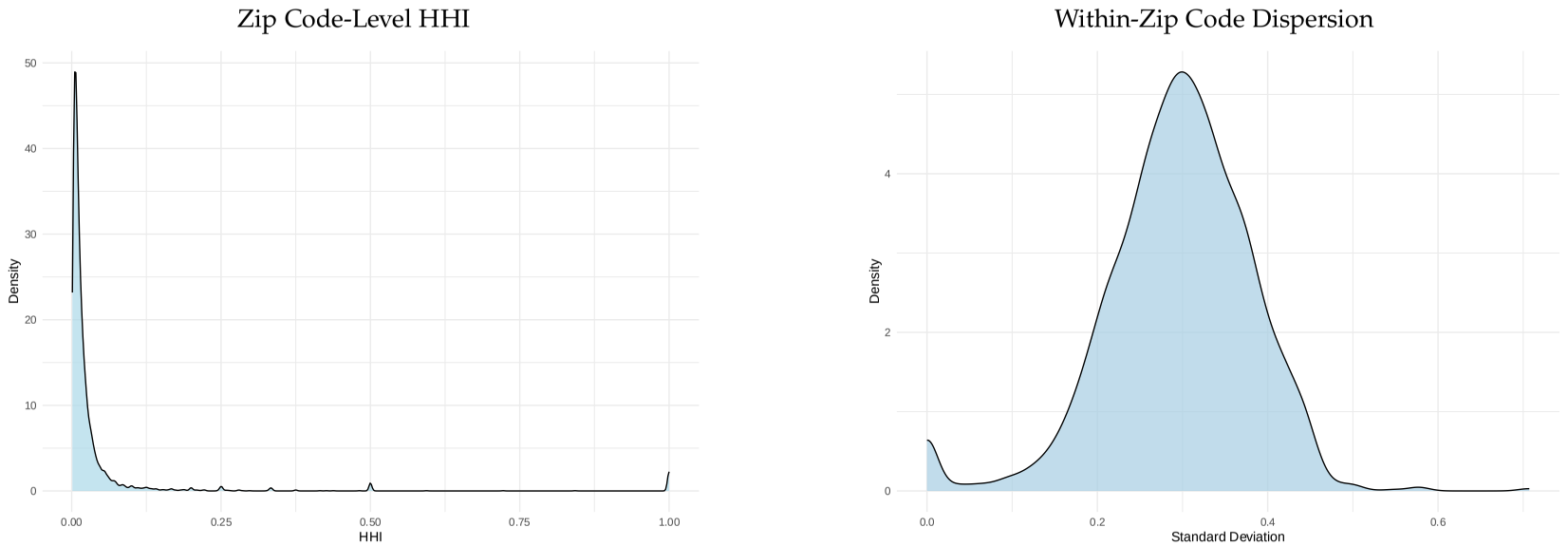
Note: Figure 1 plots the percent of transactions that sold via bidding wars each year from January 2000 through June 2023.

Figure 2. Agent Listing Volume and Bidding War Probability



Note: Figure 2 plots the relationship between an agent's listing volume and their propensity to generate bidding wars. Each point represents an individual listing agent. The horizontal axis shows the total number of listings handled (winsorized at 2,000 listings), and the vertical axis reports the agent's mean bidding war share. The fitted red line captures the average relationship between listing volume and bidding war incidence across listing agents.

Figure 3. Listing Agent Concentration and Bidding War Dispersion



Note: Figure 3 examines zip code-level listing agent concentration and bidding war dispersion. Panel A plots the distribution of the zip code-level Herfindahl-Hirschman Index (HHI) based on each agent's share of listings in that zip code. Panel B plots the distribution of withinâzip code dispersion in agents' biddingâwar propensities, measured as the standard deviation of agent-level bidding war rates within each zip code.

Online Appendix to

The Winner's Curse in Housing Markets

Soon Hyeok Choi & Adam Nowak & Patrick Smith & Alexei Tchistyi

A Bilateral Bargaining (Internet Appendix)

In Section 2, we considered a scenario where two or more potential buyers compete to buy a house, and the house is sold to the highest bidder, provided the winning bid exceeds the reservation value $\underline{\theta}$. We now turn our attention to the case where there is only one potential buyer. Let P^A be the list price, set by the seller. Unlike the reservation value, a list price is not binding in the sense that the seller can always accept a lower offer. For this reason, list price was not part of our analysis of the auction equilibria. However, the seller cannot demand that a buyer pays above the list price, which can be a factor in bilateral negotiations.[1]

In the setting with one buyer, the buyer can either pay the list price P^A, or negotiate a new price with the seller. We assume that the buyer and the seller have equal bargaining power, and the negotiation results in the Nash bargaining solution $\frac{\underline{\theta}+\theta_1}{2}$, where θ_1 is the buyers' signal. Thus, the price p_1^B paid for the house equal to

$$p_1^B = \min\left\{P^A, \frac{\underline{\theta}+\theta_1}{2}\right\}. \tag{A.1}$$

The highest price the seller can possibly negotiate is $\frac{\underline{\theta}+\bar{\theta}}{2}$, when $\theta_1 = \bar{\theta}$. Thus, it is optimal for the seller to set the list price equal to the expected value of the house, $\mu = \frac{\underline{\theta}+\bar{\theta}}{2}$. Hence, when there is only one buyer i, the seller always negotiates with the buyer, resulting in the transaction price of

$$p_1^B = \frac{\underline{\theta}+\theta_1}{2}. \tag{A.2}$$

[1] Refusing to accept the full asking price can expose a seller to lawsuits and penalties. If a buyer offers the full asking price and meets all other conditions set by the seller, but the seller refuses to sell, the listing agent may still be entitled to their commission. This is because the agent has fulfilled their part of the agreement by securing a buyer who meets the seller's terms.

Since $E[\theta_1] = \frac{\underline{\theta}+\bar{\theta}}{2}$, the expected price P^B paid for the house is given by

$$P^B = \frac{\underline{\theta} + \frac{\underline{\theta}+\bar{\theta}}{2}}{2} = \mu - \frac{\bar{\theta} - \underline{\theta}}{4}. \tag{A.3}$$

Comparing (A.3) to (2.6) and (2.11), we note that bilateral bargaining with one buyer results in a lower selling price on average than that in bidding wars with multiple buyers.

We summarize the above observations in Proposition 4.

Proposition 4. *To strengthen their bargaining position, the seller optimally set the list price to match the expected intrinsic value of the home:*

$$P^A = \mu. \tag{A.4}$$

In the event that only one buyer shows up, the expected sale price is given by

$$P^B = \mu - \frac{\bar{\theta} - \underline{\theta}}{4}. \tag{A.5}$$

Proof. *See Appendix A.*

Our baseline model takes the list price as given. In the Internet Appendix, we relax this assumption by allowing sellers to set list prices strategically. The extension shows that underpricing can arise in equilibrium and can amplify winner's curse effects through endogenous entry and buyer sorting, thereby complementing the baseline mechanism.[2] This extension does not impact the empirical strategy. We quantify the winner's curse effects based on subsequent house price appreciation and mortgage defaults.

B Underquoting (Internet Appendix)

We extend the baseline model by allowing the seller to choose the list price strategically. Poisson arrival of entrants with intensity depends on the list price. Other primitives,

[2] Gargano and Giacoletti (2026) provide evidence that strategic underpricing can raise seller revenue in housing auctions: when Australian underquoting laws forced list prices upward, sale prices and sale probabilities fell. Our extension rationalizes the same underpricing force through limited-rationality bidding and heterogeneous bidder entry.

including signals, the property value, and the bid functions, carry over from Section 2. We show that the seller optimally underprices, so bidding wars arise endogenously. Our extension model shows that strategic underpricing and the winner's curse can operate as complementary mechanisms consistent with the empirical results.

B.1 Strategic Underpricing

B.1.1 Rational Equilibrium

The primitives are as in Section 2. A single indivisible house has reservation value $\underline{\theta}$. Each potential buyer i observes a private signal θ_i drawn iid from $U[\underline{\theta}, \bar{\theta}]$, and the realized value, conditional on n entrants, is

$$v = \frac{1}{n}\sum_{i=1}^{n}\theta_i, \qquad E[v] = \mu = \frac{\underline{\theta}+\bar{\theta}}{2}.$$

The seller chooses a list price $P^A \geq \underline{\theta}$ before buyers arrive. The number of entrants n is drawn from a Poisson distribution with intensity $\lambda(P^A)$ (Levin and Smith, 1994):

$$n \sim \text{Poisson}\big(\lambda(P^A)\big),$$

where λ is twice continuously differentiable, strictly positive, and strictly decreasing in P^A.[3] A lower list price thus attracts more entry.

After observing P^A and entering, each buyer draws a signal and learns n. The transaction proceeds as in the baseline. If $n = 0$, no buyer arrives and the seller retains the house at reservation value $\underline{\theta}$. If $n = 1$, the buyer and seller bargain bilaterally and the price is

$$p_1^B(P^A, \theta_1) = \min\left\{P^A, \frac{\underline{\theta}+\theta_1}{2}\right\}, \tag{B.1}$$

as in equation (A.1). If $n \geq 2$, the house is sold through a first-price sealed-bid auction. In this subsection all bidders are sophisticated (i.e., experienced), as in the

[3] Our Poisson entry process is related to the stochastic-entry framework of Levin and Smith (1994). They model mixed-strategy bidder entry, which implies a binomial number of entrants; our Poisson specification can be viewed as the large-market analogue in which the seller's strategic list price shifts the entry intensity $\lambda(P^A)$.

rational equilibrium of Section 2.1; they bid according to Proposition 1 and the auction revenue is P_n^R from equation (2.6).

Let $R_n(P^A)$ denote the seller's gross payoff in the n-bidder state. Then

$$R_0 = \underline{\theta}, \qquad R_1(P^A) = E\left[\min\left\{P^A, \frac{\underline{\theta}+\theta_1}{2}\right\}\right], \qquad R_n = P_n^R \text{ for } n \geq 2. \tag{B.2}$$

The seller chooses $P^A \geq \underline{\theta}$ to maximize expected payoff net of the reservation value:

$$\max_{P^A \geq \underline{\theta}} \Pi^R(P^A) = \sum_{n=0}^{\infty} p_n\big(\lambda(P^A)\big)\,[R_n(P^A) - \underline{\theta}], \tag{B.3}$$

where $p_n(\lambda) = \lambda^n e^{-\lambda}/n!$. For $n \geq 2$ the auction outcome does not depend on P^A, since the list price does not enter the bid functions. The list price affects expected revenue through two channels: it shapes the Poisson distribution of n via the entry intensity λ, and it caps the bilateral price when $n = 1$.

Proposition 5. *In the rational equilibrium, the seller's optimal list price satisfies*

$$\hat{P}^R < \mu.$$

Proof. See Appendix C.5.1 for the proof.

The intuition underlying the proposition is that a strategically lower list price attracts more bidders, which in turn raises the expected transaction price.

B.1.2 Limited Rationality Equilibrium

We now turn to the seller's problem in the limited rationality environment of Section 2.2. The primitives, the Poisson entry process, and the bilateral payoff carry over unchanged. The only adjustment is the auction revenue P_n^L for $n \geq 2$.

The seller's gross payoff in the n-entrant state under limited rationality is

$$R_n^L(P^A) = \begin{cases} \underline{\theta} & \text{if } n = 0, \\ E\left[\min\left\{P^A, \dfrac{\underline{\theta}+\theta_1}{2}\right\}\right] & \text{if } n = 1, \\ P_n^L & \text{if } n \geq 2. \end{cases}$$

Let $\widetilde{R}_n^L(P^A) \equiv R_n^L(P^A) - \underline{\theta}$. The seller's expected payoff net of the reservation value is

$$\Pi^L(P^A) = \sum_{n=0}^{\infty} p_n\big(\lambda(P^A)\big)\, \widetilde{R}_n^L(P^A), \qquad p_n(\lambda) = \frac{\lambda^n e^{-\lambda}}{n!}.$$

Because the bilateral payoff is unchanged,

$$\left.\frac{d\widetilde{R}_1}{dP^A}\right|_{P^A=\mu} = 0, \qquad \widetilde{R}_1 \text{ is constant on } (\mu, \infty).$$

The next proposition characterizes the seller's optimal list price. It shows that the seller optimally lists below fundamental value and, moreover, that the optimal list price is strictly lower under the limited rationality equilibrium than under the rational equilibrium.

Proposition 6. *In the limited rationality equilibrium, the seller's optimal list price satisfies $\hat{P}^L < \mu$. If Π^R and Π^L are strictly quasiconcave in P^A, then $\hat{P}^L < \hat{P}^R$.*

Proof. See Appendix C.5.2 for the proof.

The intuition is that limited rationality strengthens the seller's incentive to underprice. In both the rational and limited rationality equilibria, a lower list price attracts more bidders, increasing expected competition and raising the expected transaction price. Limited rationality adds a second force: because less experienced buyers bid more aggressively than rational buyers, the marginal value of attracting additional entrants is larger under the limited rationality equilibrium.

The strategic underpricing analysis so far treats all bidders as a single type. The entrant pool can be readily generalized to a mixture of more and less experienced buyers, with type-specific Poisson arrival rates that respond differently to the list price. One can show that, when less experienced buyers are more responsive to the list price, strategic underpricing tilts the entrant pool toward less experienced types. In addition, in an auction with more than two entrants, less experienced types are overrepresented among winners relative to their entrant share, because they bid more aggressively at every signal. The two channels reinforce each other: underpricing routes the marginal winner of a bidding war disproportionately to a less experienced buyer.

C Proofs (Internet Appendix)

C.1 Proof of Proposition 1

Proof. We start by conjecturing that each buyer i bids according the following linear function of the private signal θ_i:

$$b_i = \underline{\theta} + \alpha(\theta_i - \underline{\theta}), \tag{C.1}$$

For convenience, we define

$$\phi_i \equiv \frac{\theta_i - \underline{\theta}}{\bar{\theta} - \underline{\theta}}, \tag{C.2}$$

$$\beta_i \equiv \frac{b_i - \underline{\theta}}{\bar{\theta} - \underline{\theta}}. \tag{C.3}$$

We note that ϕ_i is uniformly distributed on $[0, 1]$, and

$$\beta_i = \alpha\phi_i. \tag{C.4}$$

Let's assume that bidder j decides to bid x, and define

$$\chi \equiv \frac{x - \underline{\theta}}{\bar{\theta} - \underline{\theta}}. \tag{C.5}$$

Probability j wins the auction is given by

$$\Pr(x > b_{-i}) = \Pr(\chi > \beta_{-i}) = \Pr(\chi > \alpha\phi_{-i}) = \Pr\left(\phi_{-i} < \frac{\chi}{\alpha}\right) = \left(\frac{\chi}{\alpha}\right)^{n-1}. \tag{C.6}$$

Conditional on winning the auction, bidder j payoff is given by

$$v - x = \frac{1}{n}\sum_{i=1}^{n}(\theta_i - x) = \frac{\bar{\theta} - \underline{\theta}}{n}\sum_{i=1}^{n}(\phi_i - \chi).$$

Thus, the expected payoff of bidder j can be written as follows

$$\begin{aligned}
& \left(\frac{\chi}{\alpha}\right)^{n-1} \frac{\bar{\theta}-\underline{\theta}}{n} \sum_{i=1}^{n} \left(E\left[\phi_i \left| \phi_j, \chi > \beta_{-j}\right.\right] - \chi\right) \\
= & \frac{\bar{\theta}-\underline{\theta}}{n} \left(\frac{\chi}{\alpha}\right)^{n-1} \sum_{i=1}^{n} \left(E\left[\phi_i \left| \phi_j, \chi > \beta_{-j}\right.\right] - \chi\right) \\
= & \frac{\bar{\theta}-\underline{\theta}}{n} \left(\frac{\chi}{\alpha}\right)^{n-1} \sum_{i=1}^{n} \left(E\left[\phi_i \left| \phi_j, \phi_{-j} < \frac{\chi}{\alpha}\right.\right] - \chi\right) \\
= & \frac{\bar{\theta}-\underline{\theta}}{n} \left(\frac{\chi}{\alpha}\right)^{n-1} \left(\phi_j + (n-1)\frac{\chi}{2\alpha} - n\chi\right).
\end{aligned}$$

The optimal bid χ must satisfy the first order condition:

$$\frac{n-1}{\alpha} \left(\frac{\chi}{\alpha}\right)^{n-2} \left(\phi_j + (n-1)\frac{\chi}{2\alpha} - n\chi\right) + \left(\frac{\chi}{\alpha}\right)^{n-1} \left(\frac{n-1}{2\alpha} - n\right) = 0,$$

which can be simplified as follows

$$\begin{aligned}
(n-1)\left(\phi_j + (n-1)\frac{\chi}{2\alpha} - n\chi\right) + \chi\left(\frac{n-1}{2\alpha} - n\right) &= 0, \\
(n-1)\phi_j + n\left(\frac{n-1}{2\alpha} - n\right)\chi &= 0.
\end{aligned}$$

Thus, the optimal bid take the following form.

$$\chi = \frac{2\alpha(n-1)}{n\left(1+2\alpha n - n\right)}\phi_j.$$

Given our conjecture (C.4), the equilibrium requires that

$$\alpha = \frac{2\alpha(n-1)}{n\left(1+2\alpha n - n\right)}.$$

Thus,

$$\alpha = \frac{n^2+n-2}{2n^2},$$

which is equivalent to (2.5).

The expected price P_n^R is equal to the expected highest bid. We note, that

$$b_i = \underline{\theta} + (\bar{\theta} - \underline{\theta})\beta_i = \underline{\theta} + (\bar{\theta} - \underline{\theta})\alpha\phi_i.$$

As a result,

$$P_n^R = \underline{\theta} + (\bar{\theta} - \underline{\theta})\alpha E\left[\phi_i \,|\phi_i > \phi_{-i}\right]. \tag{C.7}$$

The mean of the maximum order statistic of n uniformly distributed random variable ϕ_i is given by

$$E\left[\phi_i \,|\phi_i > \phi_{-i}\right] = \frac{n}{n+1}. \tag{C.8}$$

Thus,

$$\begin{aligned} P_n^R &= \underline{\theta} + (\bar{\theta} - \underline{\theta})\alpha\frac{n}{n+1} \\ &= \underline{\theta} + (\bar{\theta} - \underline{\theta})\frac{n^2+n-2}{2n^2}\frac{n}{n+1} \\ &= \frac{\bar{\theta}+\underline{\theta}}{2} - \frac{\bar{\theta}+\underline{\theta}}{2} + \underline{\theta} + (\bar{\theta} - \underline{\theta})\frac{n^2+n-2}{2n(n+1)} \\ &= \mu - \frac{\bar{\theta}-\underline{\theta}}{2} + (\bar{\theta} - \underline{\theta})\frac{n^2+n-2}{2n(n+1)} \\ &= \mu - \frac{(\bar{\theta}-\underline{\theta})}{n(n+1)}. \end{aligned} \tag{C.9}$$

□

C.2 Proof of Proposition 2

Proof. The proof of Proposition 2 is similar to that of Proposition 1, with equations (C.1) - (C.6) applying to both proofs. However, in the setting with limited rationality, bidder j, who bids x, believes that their payoff is going to be $\theta_i - x$, if he wins the auction.

Bidder j computes their expected payoff π_j^L as follows:

$$\begin{aligned} \pi_j^L &= \Pr\left(x > b_{-j}\right)(\theta_i - x) \\ &= \left(\frac{\chi}{\alpha}\right)^{n-1}(\bar{\theta} - \underline{\theta})\,(\phi_j - \chi). \end{aligned}$$

The bid χ that maximizes the perceived payoff must satisfy the first order condition:

$$\frac{n-1}{\alpha}\left(\frac{\chi}{\alpha}\right)^{n-2}(\phi_j-\chi)-\left(\frac{\chi}{\alpha}\right)^{n-1}=0,$$

which yields

$$\chi=\frac{n-1}{n}\phi_j.$$

Thus,

$$\alpha^L=\frac{n-1}{n}.$$

Analogous to equations (C.7)-(C.9), the expected price P_n^L can be written as follows

$$\begin{aligned}
P_n^L &= \underline{\theta}+(\bar{\theta}-\underline{\theta})\alpha^L E\left[\phi_i\,|\phi_i>\phi_{-i}\right] \\
&= \underline{\theta}+(\bar{\theta}-\underline{\theta})\alpha^L\frac{n}{n+1} \\
&= \underline{\theta}+(\bar{\theta}-\underline{\theta})\frac{n-1}{n}\frac{n}{n+1} \\
&= \frac{\bar{\theta}+\underline{\theta}}{2}-\frac{\bar{\theta}+\underline{\theta}}{2}+\underline{\theta}+(\bar{\theta}-\underline{\theta})\frac{n-1}{n+1} \\
&= \mu-\frac{\bar{\theta}-\underline{\theta}}{2}+(\bar{\theta}-\underline{\theta})\frac{n-1}{n+1} \\
&= \mu-\frac{(3-n)(\bar{\theta}-\underline{\theta})}{2(n+1)}.
\end{aligned}$$

□

C.3 Proof of Proposition 3

Proof. The proof follows directly from equations (2.12) and (2.13).

When $n=2$, $\Delta b_i=0$ and $\Delta P_n=0$. When $n>2$, $\Delta b_i>0$ and $\Delta P_n>0$.

We can rewrite (2.12) and (2.13) as follows

$$\begin{aligned}
\Delta b_i &= (1-\frac{1}{n})(1-\frac{2}{n})\frac{(\theta_i-\underline{\theta})}{2}, \\
\Delta P_n &= (1-\frac{2}{n+1})(1-\frac{2}{n})\frac{(\bar{\theta}-\underline{\theta})}{2}.
\end{aligned}$$

When n is increasing, $(1-\frac{1}{n})$, $(1-\frac{2}{n})$ and $(1-\frac{2}{n+1})$ are increasing. Thus, Δb_i and ΔP_n are increasing in n.

According to equation (2.11), the magnitude of a strong winner's curse is given by

$$P_n^L - \mu = -\frac{(3-n)(\bar{\theta}-\underline{\theta})}{2(n+1)}.$$

It is positive if and only if $n > 3$.

When $n = 3$, there is a weak winner's curse with magnitude

$$\Delta P_3 = \frac{(\bar{\theta}-\underline{\theta})}{12}.$$

□

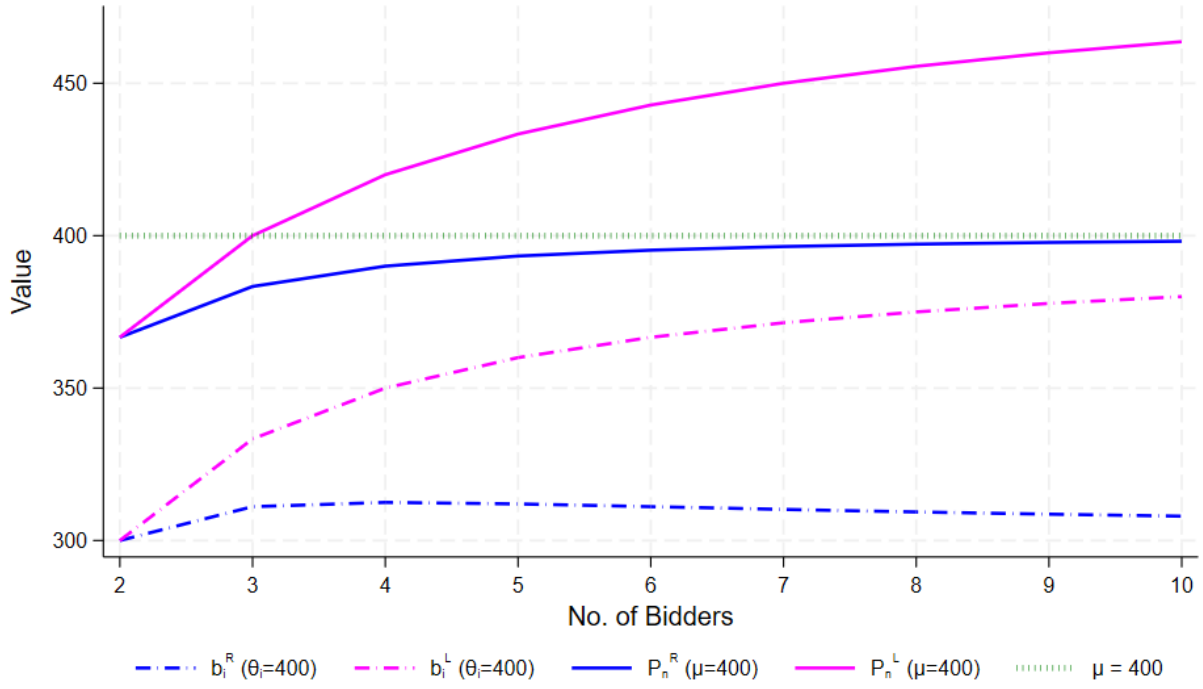
C.4 Numerical Example

Consider a baseline case where a house value (μ) is 400. Suppose bidder i has a noisy private signal (θ_i) of 400, which is drawn from the uniform distribution on the interval $[200, 600]$. By Proposition 1 and 2, we obtain the following bidding strategies and expected prices under two equilibria:

$$b_i^R = 200 + \frac{(n+2)(n-1)}{2n^2}\,200 \qquad P_n^R = 400 - \frac{200}{n(n+1)} \tag{C.10}$$

$$b_i^L = 200 + \left(\frac{n-1}{n}\right)\,200 \qquad P_n^L = 400 - \frac{(3-n)\,200}{2(n+1)} \tag{C.11}$$

Figure C.1. The Comparison of the Two Equilibria



Note: Figure C.1 plots the optimal bidding strategy and expected transaction prices under the rational (blue) and limited rationality (pink) equilibria. It assumes home value (μ) is 400 indicated with the green dotted line. The dash dotted lines represent the two equilibria's bidding strategies when the bidder receives a signal $\theta_i = 400$, which is drawn from the uniform distribution on the interval $[200, 600]$. The expected prices are demonstrated by the solid lines.

Figure C.1 demonstrates an example of the winner's curse in Proposition 3. When there are two bidders, the bidding strategies and expected prices are identical in the two equilibria. However, when there are 3 or more bidders, the bids and expected price in the limited rationality equilibrium are higher than those in the rational equilibrium. In addition, while the expected price in the rational equilibrium always remains below the average home value, the expected price under the limited rationality equilibrium exceeds the average house value when there are 4 or more bidders, illustrating the strong winner's curse.

C.5 Extension Proofs (Internet Appendix)

C.5.1 Proof of Proposition 5

Proof. Define $\widetilde{R}_n(P^A) \equiv R_n(P^A) - \underline{\theta}$. From (B.3) and (B.2),

$$\begin{aligned}
\Pi^R(P^A) &= \sum_{n=0}^{\infty} p_n\big(\lambda(P^A)\big)\, \widetilde{R}_n(P^A), \\
\widetilde{R}_0 &= 0, \\
\widetilde{R}_1(P^A) &= E\Big[\min\Big\{P^A, \tfrac{\theta+\theta_1}{2}\Big\}\Big] - \underline{\theta}, \\
\widetilde{R}_n &= P_n^R - \underline{\theta} \quad (n \geq 2).
\end{aligned}$$

Step 1: $d\widetilde{R}_1/dP^A$. For $P^A \in [\underline{\theta}, \mu]$, the cap $(\underline{\theta}+\theta_1)/2 \leq P^A$ binds iff $\theta_1 \leq \theta^\star \equiv 2P^A - \underline{\theta}$. With $\theta_1 \sim U[\underline{\theta}, \bar{\theta}]$,

$$\widetilde{R}_1(P^A) = \frac{1}{\bar{\theta}-\underline{\theta}} \int_{\underline{\theta}}^{\theta^\star} \frac{\theta_1 - \underline{\theta}}{2}\, d\theta_1 + \frac{1}{\bar{\theta}-\underline{\theta}} \int_{\theta^\star}^{\bar{\theta}} (P^A - \underline{\theta})\, d\theta_1.$$

Applying Leibniz's rule to each term ($d\theta^\star/dP^A = 2$),

$$\begin{aligned}
\frac{d\widetilde{R}_1}{dP^A} &= \frac{1}{\bar{\theta}-\underline{\theta}}\left[\frac{\theta^\star - \underline{\theta}}{2} \cdot 2\right] + \frac{1}{\bar{\theta}-\underline{\theta}}\Big[-(P^A - \underline{\theta}) \cdot 2 + (\bar{\theta} - \theta^\star)\Big] \\
&= \frac{(\theta^\star - \underline{\theta}) - 2(P^A - \underline{\theta}) + (\bar{\theta} - \theta^\star)}{\bar{\theta}-\underline{\theta}} \\
&= \frac{\bar{\theta} + \underline{\theta} - 2P^A}{\bar{\theta}-\underline{\theta}} \\
&= \frac{2(\mu - P^A)}{\bar{\theta}-\underline{\theta}}.
\end{aligned}$$

Hence $d\widetilde{R}_1/dP^A|_{P^A=\mu} = 0$. For $P^A > \mu$ the cap never binds, so $\widetilde{R}_1$ is constant on $[\mu, \infty)$.

Step 2: Poisson identity. For $p_n(\lambda) = \lambda^n e^{-\lambda}/n!$,

$$\begin{aligned}
\frac{dp_n}{d\lambda} &= \frac{n\lambda^{n-1}e^{-\lambda} - \lambda^n e^{-\lambda}}{n!} \\
&= \frac{\lambda^{n-1}e^{-\lambda}}{(n-1)!} - \frac{\lambda^n e^{-\lambda}}{n!}
\end{aligned}$$

$$= \quad p_{n-1}(\lambda) - p_n(\lambda),$$

with $p_{-1} \equiv 0$.

Step 3: Differentiate Π^R. Since $\widetilde{R}_0 = 0$ and $\widetilde{R}_n$ is independent of P^A for $n \geq 2$,

$$\frac{d\Pi^R}{dP^A} \quad = \quad \lambda'(P^A) \sum_{n=0}^{\infty} \big(p_{n-1}(\lambda) - p_n(\lambda)\big)\widetilde{R}_n + p_1\big(\lambda(P^A)\big)\frac{d\widetilde{R}_1}{dP^A}.$$

Reindexing $\sum_{n=0}^{\infty} p_{n-1}(\lambda)\widetilde{R}_n = \sum_{m=0}^{\infty} p_m(\lambda)\widetilde{R}_{m+1}$ converts the bracket to an increment form:

$$\frac{d\Pi^R}{dP^A} \quad = \quad \lambda'(P^A) \sum_{n=0}^{\infty} p_n\big(\lambda(P^A)\big)\big(\widetilde{R}_{n+1} - \widetilde{R}_n\big) + p_1\big(\lambda(P^A)\big)\frac{d\widetilde{R}_1}{dP^A}.$$

Step 4: $\widetilde{R}_{n+1} - \widetilde{R}_n > 0$ *for all* $n \geq 0$. From equation (A.3), $R_1(\mu) = \mu - (\bar{\theta} - \underline{\theta})/4$, so

$$\begin{aligned} \widetilde{R}_1(\mu) - \widetilde{R}_0 \quad &= \quad \mu - \frac{\bar{\theta} - \underline{\theta}}{4} - \underline{\theta} \\ &= \quad \frac{\underline{\theta} + \bar{\theta}}{2} - \underline{\theta} - \frac{\bar{\theta} - \underline{\theta}}{4} \\ &= \quad \frac{\bar{\theta} - \underline{\theta}}{4} > 0. \end{aligned}$$

With $\widetilde{R}_2 = P_2^R - \underline{\theta} = \mu - (\bar{\theta} - \underline{\theta})/6 - \underline{\theta}$,

$$\widetilde{R}_2 - \widetilde{R}_1(\mu) \quad = \quad -\frac{\bar{\theta} - \underline{\theta}}{6} + \frac{\bar{\theta} - \underline{\theta}}{4} = \frac{\bar{\theta} - \underline{\theta}}{12} > 0.$$

For $n \geq 2$, using $P_n^R = \mu - (\bar{\theta} - \underline{\theta})/[n(n+1)]$ from equation (2.6),

$$\begin{aligned} \widetilde{R}_{n+1} - \widetilde{R}_n \quad &= \quad P_{n+1}^R - P_n^R \\ &= \quad (\bar{\theta} - \underline{\theta})\left[\frac{1}{n(n+1)} - \frac{1}{(n+1)(n+2)}\right] \\ &= \quad (\bar{\theta} - \underline{\theta}) \cdot \frac{(n+2) - n}{n(n+1)(n+2)} \\ &= \quad \frac{2(\bar{\theta} - \underline{\theta})}{n(n+1)(n+2)} > 0. \end{aligned}$$

Step 5: Sign at $P^A = \mu$. Since $\lambda'(\mu) < 0$, $d\widetilde{R}_1/dP^A|_\mu = 0$, and the increments are positive,

$$\left.\frac{d\Pi^R}{dP^A}\right|_{P^A=\mu} = \lambda'(\mu)\sum_{n=0}^{\infty} p_n\big(\lambda(\mu)\big)\big(\widetilde{R}_{n+1} - \widetilde{R}_n\big) < 0.$$

Because $\widetilde{R}_1$ is constant on $[\mu, \infty)$, no maximizer lies in $[\mu, \infty)$, and $\hat{P}^R < \mu$. □

C.5.2 Proof of Proposition 6

Proof. The argument mirrors the proof of Proposition 5 with $\widetilde{R}^L_n \equiv R^L_n(P^A) - \underline{\theta}$. The bilateral term $\widetilde{R}_1$ is unchanged, so $d\widetilde{R}_1/dP^A|_\mu = 0$ and

$$\frac{d\Pi^L}{dP^A} = \lambda'(P^A)\sum_{n=0}^{\infty} p_n\big(\lambda(P^A)\big)\big(\widetilde{R}^L_{n+1} - \widetilde{R}^L_n\big) + p_1\big(\lambda(P^A)\big)\frac{d\widetilde{R}_1}{dP^A}.$$

Step 1: $\widetilde{R}^L_{n+1} - \widetilde{R}^L_n > 0$ for all $n \geq 0$.

For $n \geq 2$, using $P^L_n = \mu - (3-n)(\bar{\theta} - \underline{\theta})/[2(n+1)]$ from Proposition 2,

$$\begin{aligned}
\widetilde{R}^L_{n+1} - \widetilde{R}^L_n &= P^L_{n+1} - P^L_n \\
&= \left[\mu - \frac{(2-n)(\bar{\theta} - \underline{\theta})}{2(n+2)}\right] - \left[\mu - \frac{(3-n)(\bar{\theta} - \underline{\theta})}{2(n+1)}\right] \\
&= \frac{\bar{\theta} - \underline{\theta}}{2}\left[\frac{3-n}{n+1} - \frac{2-n}{n+2}\right] \\
&= \frac{\bar{\theta} - \underline{\theta}}{2} \cdot \frac{(3-n)(n+2) - (2-n)(n+1)}{(n+1)(n+2)} \\
&= \frac{\bar{\theta} - \underline{\theta}}{2} \cdot \frac{4}{(n+1)(n+2)} \\
&= \frac{2(\bar{\theta} - \underline{\theta})}{(n+1)(n+2)} > 0.
\end{aligned}$$

For $n = 1$, Proposition 3 at $n = 2$ gives $P^L_2 = P^R_2 = \mu - (\bar{\theta} - \underline{\theta})/6$, so $\widetilde{R}^L_2 = (\bar{\theta} - \underline{\theta})/3$, and

$$\widetilde{R}^L_2 - \widetilde{R}_1(\mu) = \frac{\bar{\theta} - \underline{\theta}}{3} - \frac{\bar{\theta} - \underline{\theta}}{4} = \frac{\bar{\theta} - \underline{\theta}}{12} > 0.$$

For $n = 0$, the rational proof gives $\widetilde{R}_1(\mu) - \widetilde{R}_0 = (\bar{\theta} - \underline{\theta})/4 > 0$.

Hence $d\Pi^L/dP^A|_\mu < 0$, and since $\widetilde{R}_1$ is constant on $[\mu, \infty)$, $\hat{P}^L < \mu$.

Step 2: $\hat{P}^L < \hat{P}^R$.

Since $\hat{P}^R$ is an interior maximizer of Π^R, the first-order condition gives $d\Pi^R/dP^A(\hat{P}^R) = 0$. Subtracting the two first-order derivatives, the common bilateral term cancels and

$$\frac{d\Pi^L}{dP^A} - \frac{d\Pi^R}{dP^A} = \lambda'(P^A) \sum_{n=0}^{\infty} p_n\big(\lambda(P^A)\big)\Big[\big(\widetilde{R}^L_{n+1} - \widetilde{R}^L_n\big) - \big(\widetilde{R}_{n+1} - \widetilde{R}_n\big)\Big].$$

The bracket vanishes for $n = 0, 1$, where the bilateral and two-bidder payoffs coincide, and is strictly positive for $n \geq 2$, since $\widetilde{R}^L_{n+1} - \widetilde{R}^L_n = 2(\bar{\theta} - \underline{\theta})/[(n+1)(n+2)]$ exceeds $\widetilde{R}_{n+1} - \widetilde{R}_n = 2(\bar{\theta} - \underline{\theta})/[n(n+1)(n+2)]$. With $\lambda'(P^A) < 0$ the gap is strictly negative, so $d\Pi^L/dP^A(\hat{P}^R) < 0$. Strict quasiconcavity of Π^L then places its maximizer strictly to the left of $\hat{P}^R$, hence $\hat{P}^L < \hat{P}^R$. □

D Data Overview (Internet Appendix)

D.1 Data Availability Statement

This study uses data from several sources. Our agreements with the data providers do not allow us to post or otherwise distribute the data. However, anyone interested in replicating our results can obtain the data. We provide details on how to acquire the data below.

Cotality Data: We use three Cotality datasets: multiple listing service (MLS), owner transfer (i.e., deed), and mortgage data. Researchers can purchase or request additional information about the data from Cotality via their website.

HMDA Data: The Federal Financial Institutions Examination Council (FFIEC) provides publicly available data gathered under the Home Mortgage Disclosure Act (HMDA) that Congress enacted in 1975. The HMDA data includes transaction-level information regarding home mortgage lending activity. Researchers can learn more about the data and download national datasets free of charge from the Federal Financial Institutions Examination Council website.

D.2 Data Filters

Prior to running the empirical analysis, we apply several filters to the single-family detached residential transaction data. We list the filters below and provide a detailed overview of the number of transactions that are dropped by state in Table D.1. Records are dropped that do not meet the following criteria:

1. filter 1: listing successfully sold
2. filter 2: 2000 $\leq$ sale year $\leq$ 2023
3. filter 3: \$20,000 $\leq$ sale price $\leq$ \$2,000,000
4. filter 4: \$20,000 $\leq$ list price $\leq$ \$2,000,000
5. filter 5: trim the top and bottom 1% of sale-to-list price ratio[4]
6. filter 6: age $\geq$ 1
7. filter 7: year built $\geq$ 1900
8. filter 8: keep one unique listing when posted to multiple MLS platforms
9. filter 9: house has a single listing per month
10. filter 10: number of sales in zip code each year $\geq$ 50
11. filter 11: number of sales in county in 2003 $\geq$ 2,000
12. filter 12: non-distressed transaction[5]
13. filter 13: sale year $\leq$ 2018

[4]We trim the data for two reasons. First, this filter removes obvious data entry errors. Second, the filter ensures outliers - both on the low and high end - do not drive our results. We found similar results in an earlier draft of this paper when we did not apply this filter.

[5]Before removing distressed transactions, we identify whether non-distressed transactions subsequently default (i.e., result in a distressed transaction). Accordingly, our analyses include non-distressed transactions that subsequently default but do not include the subsequent performance of distressed transactions.

D.3 HMDA Merge

We identify the race and household income of buyers using fields in the Home Mortgage Disclosure Act (HMDA) data. We merge the Cotality housing transaction and HMDA datasets using the following fields: year, census tract, loan amount, and lender name. Accordingly, cash transactions and transactions where the financing information (i.e., loan amount and lender name) is unavailable are not included in the merged dataset. The final row of Table D.1 reports the fraction of non-cash transactions with financing information available that we successfully merge with HMDA data. The match rate is quite high, ranging from 83% in Kentucky and Texas to over 91% in Massachusetts and New Hampshire.

D.4 HMDA Race and Ethnicity

After merging the datasets, we create five mutually exclusive race and ethnicity groupings based on the race and ethnicity categories available in HMDA: American Indian or Alaska Native (AIAN), Asian or Pacific Islander (API), Black, Hispanic, and White. We use information about the primary mortgage applicant as well as the co-applicant, where applicable.

We begin by classifying homebuyers as Hispanic if the ethnicity of one of the applicants is listed as “Hispanic or Latino" in the HMDA data. Once assigned to one of the five groupings, the transaction is removed from the process. Next, we classify homebuyers as Black if the race of one of the applicants is listed as “Black or African American" in the HMDA data. Then, we classify homebuyers as American Indian or Alaska Native (AIAN) if the race of one of the applicants is listed as “American Indian or Alaska Native" in the HMDA data. Then, we classify homebuyers as Asian or Pacific Islander (API) if the race of one of the applicants is listed as either “Asian" or “Native Hawaiian or Other Pacific Islander" in the HMDA data. Finally, we classify homebuyers as White if the race of one of the applicants is listed as “White" in the HMDA data. All remaining transactions whose buyers are not classified are not included in the empirical analysis.

Table D.1. Filtered transaction data by state (AL-IL)

	State									
filter	AL	AR	AZ	CA	CO	FL	HI	IA	ID	IL
no filter	1,516,326	434,726	3,240,998	18,471,501	2,700,268	5,336,599	211,848	169,934	456,941	2,827,970
filter 1	955,425	264,621	2,177,829	12,930,588	1,964,086	3,512,282	131,076	121,972	353,450	1,530,488
filter 2	924,921	232,936	2,113,998	12,078,515	1,926,771	3,450,268	110,662	110,846	353,443	1,529,918
filter 3	924,045	232,647	2,104,415	11,308,261	1,912,412	3,406,345	105,762	110,843	352,228	1,523,044
filter 4	922,536	231,897	2,099,830	11,264,167	1,909,291	3,391,978	104,896	110,712	351,804	1,520,026
filter 5	902,410	227,822	2,046,654	10,520,455	1,852,925	3,292,543	102,578	107,908	347,360	1,477,266
filter 6	780,371	182,399	1,962,108	9,838,179	1,754,379	3,077,820	97,536	101,358	288,500	1,326,136
filter 7	779,235	181,815	1,961,962	9,819,605	1,730,738	3,077,048	97,526	98,066	288,130	1,297,173
filter 8	546,533	179,984	1,944,635	5,274,823	942,006	2,862,377	97,038	97,873	287,596	1,282,998
filter 9	545,814	179,441	1,943,164	5,205,988	937,972	2,849,662	96,984	97,835	287,471	1,282,582
filter 10	544,398	178,968	1,941,166	5,200,043	936,557	2,848,344	96,888	97,154	287,025	1,280,694
filter 11	275,247	83,404	1,851,478	4,584,420	272,825	1,295,900	78,084	68,152	227,910	110,688
filter 12	242,707	74,172	1,529,678	3,707,929	246,600	1,058,160	70,664	64,513	193,428	96,757
filter 13	178,498	51,461	1,135,786	2,878,793	187,998	727,159	56,519	51,312	138,528	68,968
match rate	0.8702	0.8653	0.8523	0.8554	0.8892	0.8593	0.8455	0.8741	0.8808	0.8855

Note: Table D.1 tabulates the number of records that are dropped for each filter across the thirty states examined in this study. The second-to-last row for each column identifies the number of transactions that are included in the state-level analysis. The final row reports the HMDA match rate for each state.

Table A1: Filtered transaction data by state (KS-NY)

	State									
filter	KS	KY	MA	MI	MN	MO	NC	NH	NV	NY
no filter	407,412	1,000,306	2,095,808	5,130,393	2,553,957	1,693,601	2,422,057	539,360	1,028,630	5,009,949
filter 1	334,888	635,434	1,386,267	2,741,576	1,657,898	1,193,135	1,624,031	354,314	768,734	3,041,397
filter 2	301,811	590,168	1,131,196	2,439,331	1,360,287	1,103,025	1,561,005	327,491	758,741	2,643,448
filter 3	297,773	588,858	1,114,614	2,278,686	1,358,538	1,085,985	1,553,936	326,377	754,197	2,612,475
filter 4	297,267	587,247	1,111,369	2,270,715	1,356,226	1,083,563	1,551,462	325,859	752,291	2,603,024
filter 5	286,042	575,742	1,070,489	2,182,404	1,317,025	1,045,606	1,515,678	314,756	725,108	2,506,996
filter 6	240,690	316,385	1,036,273	2,092,398	1,246,067	894,312	1,363,034	298,445	700,018	2,403,250
filter 7	239,416	313,740	969,265	2,059,811	1,221,867	880,780	1,360,005	278,222	699,958	2,315,827
filter 8	239,337	310,359	924,924	1,699,763	1,214,696	877,523	1,321,924	264,822	697,777	1,551,022
filter 9	239,306	309,875	923,977	1,689,935	1,214,135	877,182	1,320,396	264,606	697,530	1,550,305
filter 10	239,206	309,318	914,294	1,688,344	1,210,500	875,677	1,318,235	258,715	697,196	1,545,949
filter 11	174,340	83,310	861,153	1,019,757	813,341	663,515	567,391	134,939	122,127	1,120,959
filter 12	160,430	78,857	793,832	834,707	720,037	577,481	500,422	121,893	98,252	1,047,765
filter 13	124,519	62,711	632,813	624,056	554,534	439,149	364,464	94,664	73,213	816,950
match rate	0.9052	0.8327	0.9102	0.8833	0.8915	0.8881	0.8828	0.9195	0.8805	0.9077

Note: Table D.1 tabulates the number of records that are dropped for each filter across the thirty states examined in this study. The final row for each column identifies the number of transactions that are included in the state-level analysis.

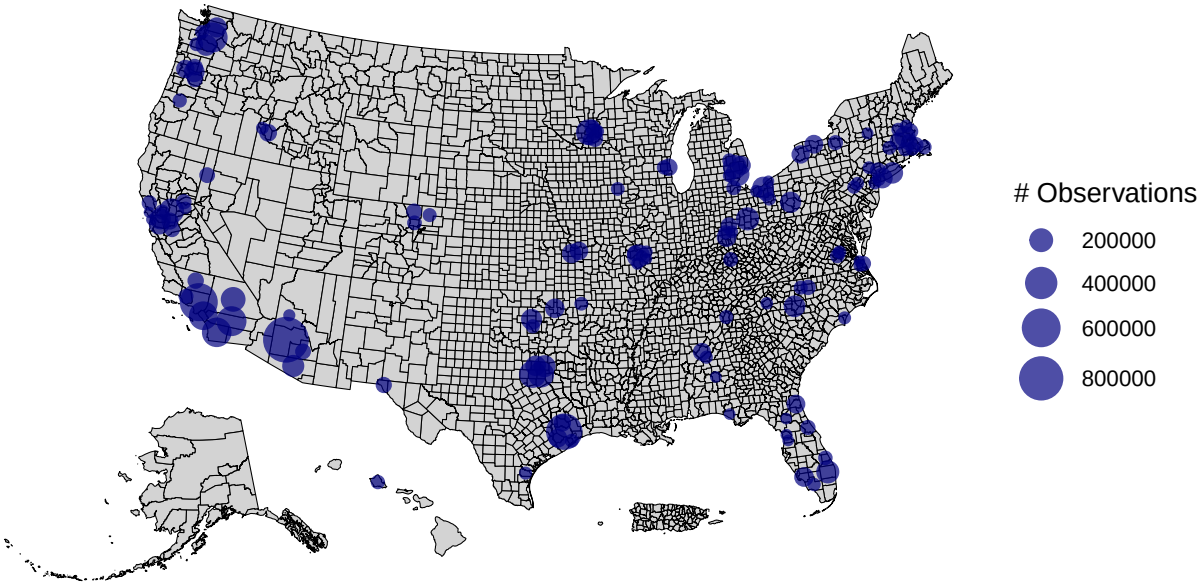
Table A1: Filtered transaction data by state (OH-WI)

	State									
filter	OH	OK	OR	PA	RI	TN	TX	VA	WA	WI
no filter	4,767,109	1,336,365	1,350,174	974,249	396,934	310,175	5,139,773	1,337,326	2,803,647	1,516,354
filter 1	2,957,353	990,289	868,193	726,373	242,009	155,169	3,852,038	875,351	1,920,193	987,544
filter 2	2,478,862	912,131	808,211	652,233	209,396	140,527	3,722,613	792,969	1,888,230	902,798
filter 3	2,474,501	785,612	805,472	651,850	208,291	140,335	3,697,677	792,426	1,853,707	901,558
filter 4	2,459,422	783,641	804,319	650,229	207,879	139,892	3,691,979	791,418	1,850,299	900,083
filter 5	2,386,732	770,208	778,498	633,282	201,831	136,455	3,619,468	766,024	1,769,760	873,595
filter 6	2,284,416	708,425	700,691	527,182	188,586	124,820	3,258,931	712,710	1,597,399	767,483
filter 7	2,237,832	708,309	696,233	511,072	188,181	124,618	3,256,501	709,816	1,593,498	745,063
filter 8	2,137,564	690,439	693,684	510,315	179,556	113,507	3,238,902	675,713	1,558,663	698,514
filter 9	2,134,663	689,596	693,404	510,269	179,471	112,519	3,237,958	675,173	1,557,817	680,643
filter 10	2,131,073	687,026	691,725	506,224	178,036	112,333	3,235,369	674,920	1,554,864	678,475
filter 11	1,407,573	479,004	530,088	339,263	87,386	85,828	2,711,904	417,319	1,190,453	222,528
filter 12	1,218,932	434,041	485,104	316,577	74,739	74,243	2,366,408	371,015	1,060,509	200,069
filter 13	914,107	327,429	375,805	242,029	56,644	53,527	1,721,026	276,288	815,728	150,656
match rate	0.8943	0.8545	0.8949	0.8981	0.8860	0.8957	0.8328	0.8742	0.8772	0.8877

Note: Table D.1 tabulates the number of records that are dropped for each filter across the thirty states examined in this study. The final row for each column identifies the number of transactions that are included in the state-level analysis.

D.5 Geographic Coverage

Figure D.1. Observations by County



Note: Figure D.1 displays the geographic coverage of the filtered housing transaction data used in the empirical analysis. The figure plots the number of observations at the county level.

E Miscellaneous (Internet Appendix)

E.1 Contemporaneous Overpayment

This section provides complementary analyses testing whether bidding war winners overpay at the time of purchase. Using a hedonic model that includes a rich set of housing characteristics and zip-by-quarter-by-year fixed effects, we construct a contemporaneous predicted value for the house at the time of sale. We use this predicted value to calculate each buyer's degree of overpayment as $dop_{ib} = \frac{P^*_{ib}}{P_{ib}} - 1$, where P^*_{ib} is the predicted value and P_{ib} is the actual transaction price for house i at time b. This measure identifies whether buyers pay above fundamentals, independent of subsequent maintenance, renovation, or resale decisions.

Table E.1 presents estimates from a modified version of Equation 4.2 that replaces the default indicator with the buyer's degree of overpayment as the dependent variable. In all four columns, we find that buyers in bidding war transactions significantly overpay relative to buyers in non-bidding war transactions. This pattern offers additional evidence that bidding war winners pay more than buyers of comparable houses in the same neighborhood and market conditions.

As noted in the main body of the paper, this contemporaneous overpayment result is not, by itself, sufficient to establish a winner's curse. A buyer may pay above fundamentals at purchase yet avoid a winner's curse if the transaction price becomes a salient reference point (Andersen et al., 2022; Giacoletti and Parsons, 2023) in future negotiations or market assessments. In such cases, the buyer's realized return need not suffer even if they initially overpaid. For this reason, we focus on bidding war winners' annualized returns and default outcomes to evaluate whether their initial overpayment results in a winner's curse.

Table E.1. Degree of Overpayment

	(1)	(2)	(3)	(4)
Bidding war	0.025***	0.018***	0.019***	0.013***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	14,195,334	8,840,857	6,369,394	3,970,369
Adjusted R^2	0.023	0.162	0.028	0.125
House Characteristics	✓	✓	✓	✓
Financing Controls	✓	✓	✓	✓
Investor Control	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓
Data Sample	Panel A	Panel B	Panel C	Panel D

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.1 examines the degree of overpayment of buyers who purchase houses in bidding war transactions relative to those in non-bidding war transactions using the four distinct nationwide housing transaction samples in Table 2. Each column displays bidding war coefficient estimates from a modified version of Equation 4.2 that replaces the default indicator with the buyer's degree of overpayment as the dependent variable. Every column includes the full set of housing characteristics, investor, and financing controls, as well as zip-by-quarter-by-year fixed effects. The data sample row identifies the corresponding panel of summary statistics in Table 2. Standard errors are clustered by zip code.

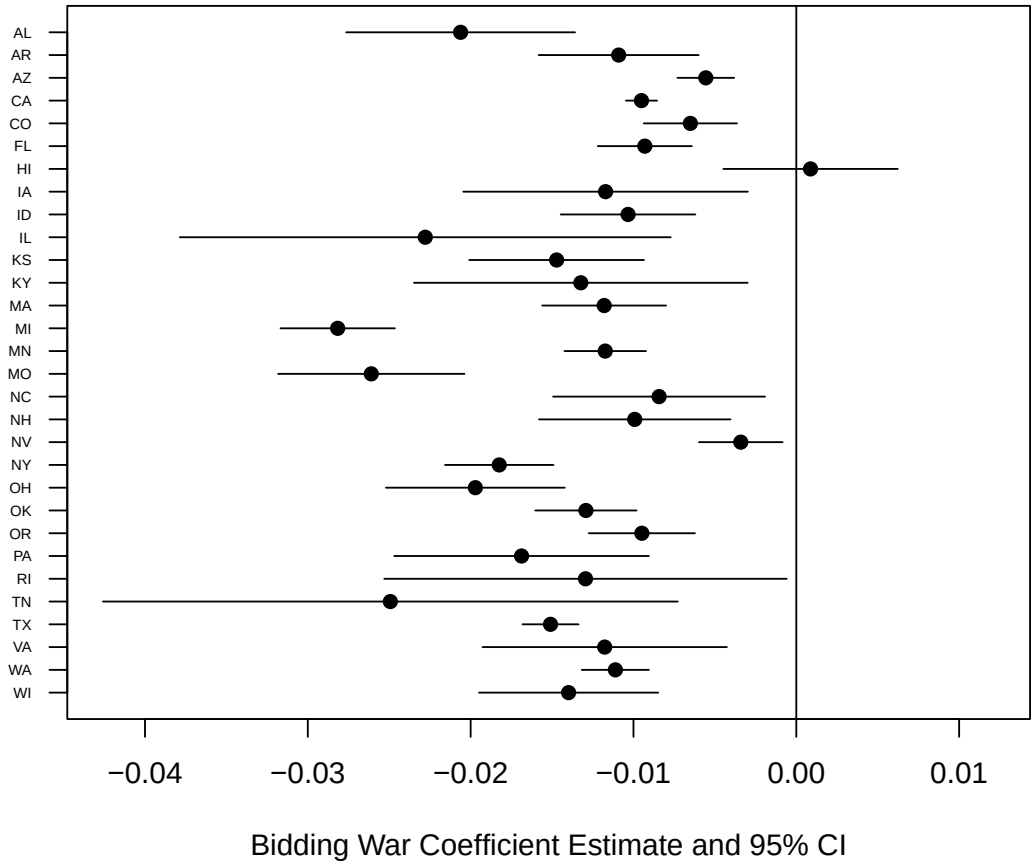
E.2 State-level Analyses

Figure E.1 plots the corresponding state-level bidding war coefficient estimates from Equation 4.1, examining homebuyers' unlevered returns by transaction type. The state-level analysis uses the full returns sample in Panel C of Table 2. The coefficient estimate for bidding war transactions is negative and statistically significant in 29 of the 30 states, indicating homebuyers who purchase their house in a bidding war experience lower annualized unlevered returns than homebuyers who do not purchase their house in a bidding war. The bidding war coefficient estimate is economically significant in those 29 statistically significant states: ranging from -0.3pp in Nevada, where the average annualized unlevered return is 3.8pp in the state sample, to -2.8pp in Michigan, where the average annualized unlevered return is 2.9pp in the state sample.

Figure E.2 plots the corresponding state-level bidding war coefficient estimates from Equation 4.1, examining homebuyers' levered returns by transaction type. The coefficient estimate for bidding war transactions is negative and statistically significant in 24 of the 30 states, indicating homebuyers who purchase their house in a bidding war experience lower annualized levered returns than homebuyers who do not purchase their house in a bidding war. The bidding war coefficient is economically significant in those 24 statistically significant states: ranging from -1.9pp in Arizona, where the average annualized levered return is 23.3pp in the state sample, to -15.7pp in Missouri, where the average annualized levered return is 33.5pp in the state sample.

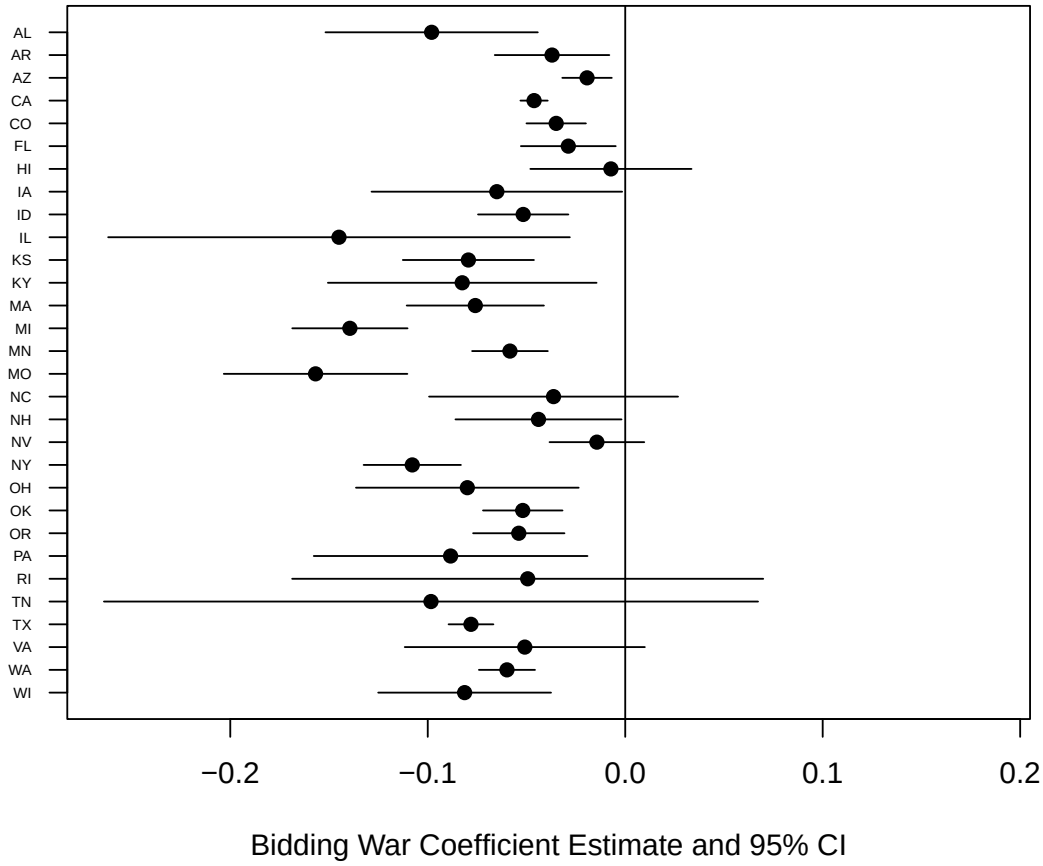
Figure E.3 plots the corresponding state-level bidding war coefficient estimates from Equation 4.2. The state-level analysis uses the full subsequent default sample in Panel A of Table 2. The coefficient estimate for bidding war transactions is positive and statistically significant in 28 of the 30 states in the data. The bidding war coefficient is economically significant in those 28 statistically significant states: ranging from 0.4pp in Massachusetts, where the probability of default is 4.5pp in the state sample, to 4.5pp in Tennessee, where the probability of default is 6.7pp in the state sample.

Figure E.1. Unlevered Returns by State



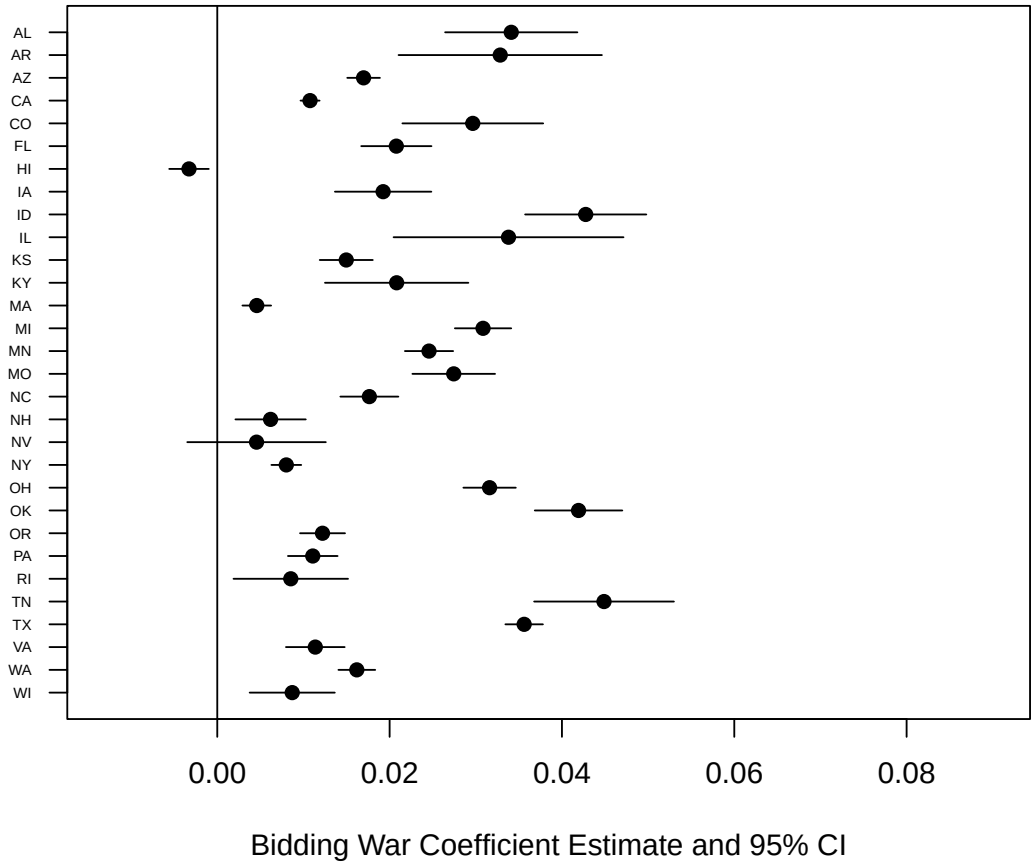
Note: Figure E.1 examines the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions at the state level. The figure plots the state-level bidding war coefficient estimates and 95% confidence intervals from Equation 4.1 using the returns sample in Panel C of Table 2.

Figure E.2. Levered Returns by State



Note: Figure E.2 examines the annualized levered returns of bidding war transactions relative to non-bidding war transactions at the state level. The figure plots the state-level bidding war coefficient estimates and 95% confidence intervals from Equation 4.1 using the returns sample in Panel C of Table 2.

Figure E.3. Subsequent Default by State



Note: Figure E.3 examines the probability of default at the state level for buyers who purchase their house in bidding war transactions relative to those who purchase their house in non-bidding war transactions. The figure plots the state-level bidding war coefficient estimates and 95% confidence intervals from Equation 4.2 using the subsequent default sample in Panel A of Table 2.

E.3 Omitted Variable Bias

Our main specifications (i.e., Equations 4.1 and 4.2) control for a rich set of observable factors. A natural concern, however, is that part of the estimated return and default differentials between bidding war and non-bidding war transactions could still reflect unobservable differences not captured by these controls. For unobservables to bias our estimates, they would need to vary systematically between buyers who win bidding wars and those who do not, and these differences would need to be correlated with both purchase outcomes and subsequent resale performance. This section assesses the potential impact of omitted variable bias by applying the sensitivity frameworks of Oster (2019) and Cinelli and Hazlett (2020).

Oster (2019)

The methodology in Oster (2019) quantifies how large the magnitude of the unobservables would need to be in order to drive the estimated treatment effects to zero. The sensitivity framework relies on the intuition that bias from unobservables is proportional to three components: (i) the change in the treatment coefficient when moving from a "short" specification with minimal controls to a "long" specification with the complete set of covariates; (ii) the relative movement in R^2 across these specifications compared to the maximum feasible R^2; and (iii) the proportional selection parameter δ, which captures the relative importance of unobservables versus observables.

We implement this framework using two approaches. The first approach uses the baseline regressions for default risk, unlevered returns, and levered returns in Equations 4.1 and 4.2. The second approach expands the baseline regressions to also include the quality-related tokens as additional controls. When examining annualized returns and default, the short specification includes only the bidding war indicator and the location-by-time fixed effects in Equations 4.1 and 4.2. When examining annualized returns, the long specification adds the holding period and investor controls in the first approach, as well as the tokens in the second approach. Whereas, when examining default, the long specification adds the investor, buyer financing, and house characteristic controls in the first approach, as well as the tokens in the second approach.

Table E.2 summarizes the magnitude of selection on unobservables that would be required to eliminate the estimated effects. Across all outcomes and specifications, the

implied values of δ are large, indicating that unobserved factors would need to be substantially more influential than the rich set of observed controls to overturn our conclusions. In Panel A (without tokens), the required δ values for unlevered and levered returns exceed 470 and 650, respectively, implying that unobservables would need to be hundreds of times more predictive of returns than observables (i.e., holding period and investor controls). The magnitude is extremely large due to the fact that both the short and long specifications use a repeat-sale approach that already controls for time-invariant unobservables. Accordingly, the results in columns 1 and 2 should be interpreted as the magnitude of selection on time-varying unobservables that would be required to eliminate the estimated effects. Even for subsequent default in column 3 of Panel A, where the required δ is smaller (11.30), unobservables would still need to be more than an order of magnitude stronger than observables to drive the coefficient to zero. This means that to bring the bidding war coefficient estimate to zero, unobservable variation would need to have an effect on default that is over 11 times as large as the one for observable variation, which is unlikely, since observable variation includes key information about the house (e.g., number of bedrooms, bathrooms, and size) and buyer (e.g., investor, financing method, and financing amount).

Panels B and C of Table E.2 show that incorporating qualityârelated tokens increases explanatory power and reduces the gap between short and long specifications, which mechanically lowers the implied δ values. Yet even with this extensive set of additional controls, the required selection on unobservables remains large for unlevered and levered returns, where δ ranges from roughly 6 to 18, and for default, where δ ranges from roughly 1 to 2. This means that even after conditioning on hundreds of qualityârelated text controls, unobservable factors would still need to be several times more predictive of annualized returns and subsequent default than the extensive set of time-invariant and time-varying controls included in Panels B and C in order to reduce the bidding war coefficient to zero. The necessary reduction is implausible given that agents have an incentive to include key information in their remarks about the property. Taken together, the results across all panels indicate that omitted variable bias of a plausible magnitude cannot account for the systematically lower returns and higher default rates observed among bidding war winners.

Cinelli and Hazlett (2020)

The methodology in Cinelli and Hazlett (2020) extends the omitted variable bias framework by expressing sensitivity to unobservables in terms of partial R^2 values and places these partial R^2 values in the context of a covariate used in the estimation. For example, can a confounder similar to CLTV explain away the bidding war estimate for default? The framework introduces two key measures of sensitivity to unobservables: the *robustness value*, which describes the minimum strength of association that unobserved confounding would need to have, both with the treatment and with the outcome, to change the research conclusions, and the partial R^2 of the treatment with the outcome, which shows how strongly confounders explaining all the residual outcome variation would have to be associated with the treatment to eliminate the estimated effect. These measures can be computed from standard regression output and provide a transparent benchmark for assessing how large unobserved confounding would need to be in order to overturn the estimated treatment effects.

We implement this framework using two approaches. The first approach uses the baseline regressions for unlevered returns, levered returns, and subsequent default in Equations 4.1 and 4.2. The second approach expands the baseline regressions to also include the quality-related tokens as additional controls. In the contour plots that follow, the horizontal axis plots the partial R^2 between the treatment (bidding war indicator) and a hypothetical unobserved confounder, Z, conditional on observed controls ($R^2_{D\sim Z|X}$). The vertical axis plots the partial R^2 between the outcome (returns or default) and the same confounder, conditional on the treatment and controls ($R^2_{Y\sim Z|X,D}$). The “Baseline" point corresponds to the estimated bidding war coefficient in the observed data. The additional points (e.g., 1Ã, 2Ã, 3Ã Benchmark Covariate) show the implied treatment effect under hypothetical confounders whose predictive strength matches, doubles, or triples the partial R^2 of the benchmark covariate with respect to both outcome and treatment. The zero effect contour traces combinations of partial R^2 values at which an unobserved confounder would be strong enough to reduce the estimated effect to zero.

Figure E.4 displays the Cinelli-Hazlett sensitivity contours for the unlevered annualized return regressions, using investor purchases as the benchmark covariate. In the quality-unadjusted specification (Panel A), the baseline estimate (-0.013) lies well below the zero-effect contour, and even confounders calibrated at five or ten times the strength of the investor purchase indicator (with implied adjusted estimates of -0.009

and -0.004, respectively) do not nullify the coefficient. Only a confounder roughly fifteen times as predictive of treatment and outcome as the investor purchase indicator would fully attenuate the estimate. Adding quality-related tokens in Panel B increases the explanatory power of the regression and lowers the baseline estimate to -0.009, but the conclusion remains the same: the quality-adjusted effect would be eliminated only by a confounder with predictive strength on the order of fifteen times that of the investor purchase indicator. Given that investor purchases are a key predictor of unlevered returns, an unobserved confounder of that magnitude is unlikely.

Figure E.5 displays the Cinelli-Hazlett sensitivity contours for the levered annualized return regressions, using investor purchases as the benchmark covariate. In the quality-unadjusted specification (Panel A), the baseline estimate (-0.064) lies well below the zero-effect contour, and even confounders calibrated at four or eight times the strength of the investor purchase indicator (with implied adjusted estimates of -0.042 and -0.02, respectively) do not nullify the coefficient. Only a confounder roughly twelve times as predictive of treatment and outcome as the investor purchase indicator would fully attenuate the estimate. Adding quality-related tokens in Panel B increases the explanatory power of the regression and lowers the baseline estimate to -0.045, but the conclusion remains the same: the quality-adjusted effect would be eliminated only by a confounder with predictive strength approximately twelve times that of the investor purchase indicator. Given that investor purchases are a key predictor of levered returns, an unobserved confounder of that magnitude is unlikely.

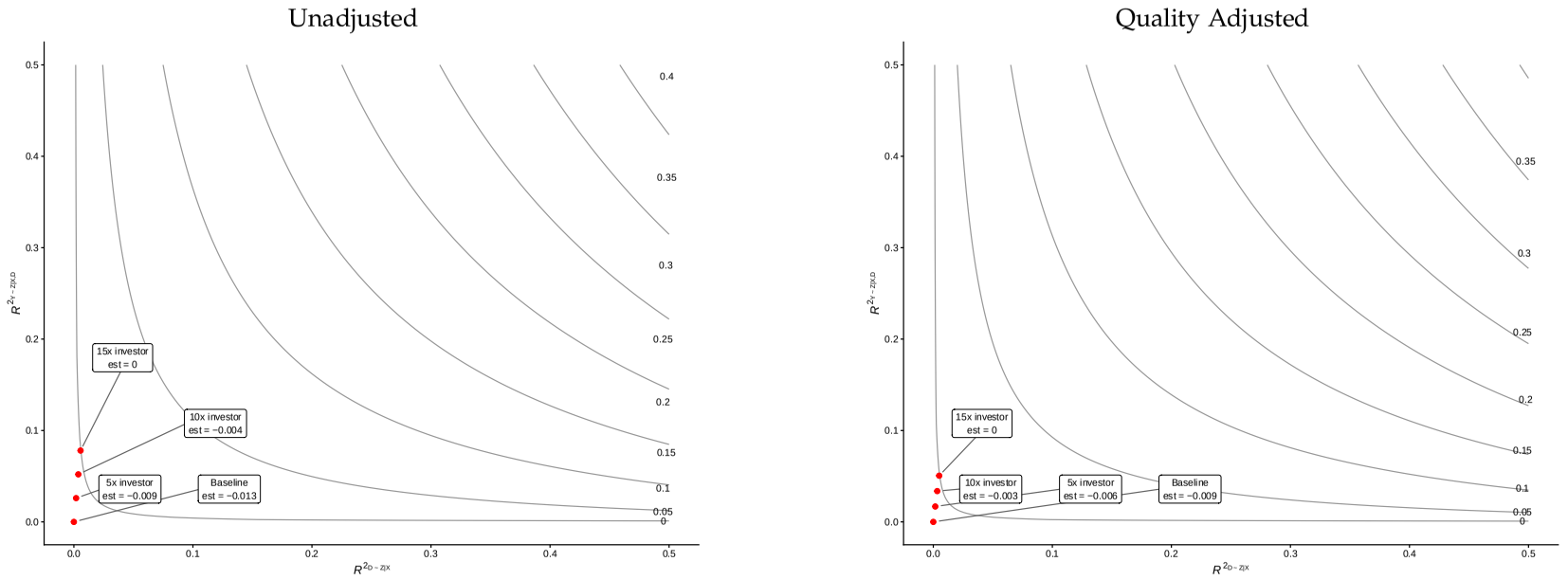
Figure E.6 displays the Cinelli-Hazlett sensitivity contours for the subsequent default regressions, using the CLTV indicators as the benchmark covariate. In the quality-unadjusted specification (Panel A), the baseline estimate (0.035) lies well below the zero-effect contour, and even confounders calibrated at one or two times the strength of the CLTV indicators (with implied adjusted estimates of 0.023 and 0.012, respectively) do not nullify the coefficient. Only a confounder roughly three times as predictive as the set of CLTV indicators would fully attenuate the estimate. Adding quality-related tokens in Panel B increases explanatory power and lowers the baseline estimate to 0.025, but the conclusion remains the same: the quality-adjusted effect would be eliminated only by a confounder with predictive strength on the order of three times that of the CLTV indicators. Given that CLTV is one of the strongest observable predictors of mortgage default, an unobserved confounder of that magnitude is highly implausible.

Table E.2. Statistical Diagnosis of Unobservable Characteristics

	Unlevered Returns (1)	Levered Returns (2)	Subsequent Default (3)
Panel A: Without Tokens			
δ	471.469	655.848	11.299
Coeff. Short	-0.014	-0.069	0.050
R^2 Short	0.344	0.275	0.277
Coeff. Long	-0.013	-0.064	0.035
R^2 Long	0.347	0.276	0.296
Panel B: Unigram Tokens			
δ	5.993	9.638	0.894
Coeff. Short	-0.014	-0.069	0.050
R^2 Short	0.344	0.275	0.277
Coeff. Long	-0.009	-0.045	0.025
R^2 Long	0.389	0.292	0.414
Panel C: Bigram Tokens			
δ	11.894	18.005	2.032
Coeff. Short	-0.014	-0.069	0.050
R^2 Short	0.344	0.275	0.277
Coeff. Long	-0.010	-0.050	0.030
R^2 Long	0.377	0.287	0.362

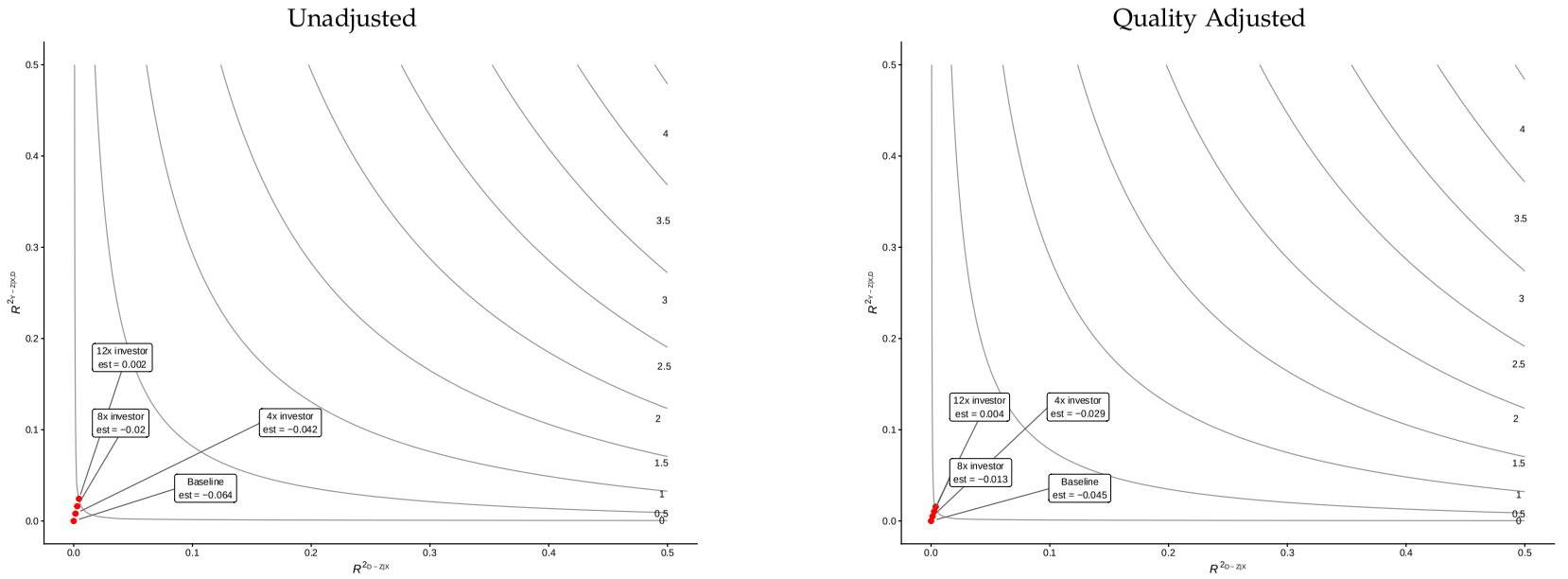
Note: Table E.2 reports the value of δ that would make the unbiased estimate of the treatment coefficient equal to zero (see Oster (2019) for more details) as well as estimates of the treatment coefficients and R^2s for the "short" and "long" regressions described in Section E.3. Panel A does not include the quality-related tokens from Section 5.1.1. Whereas Panel B includes the 695 unigram tokens and Panel C includes 679 bigram tokens from the post-double-selection estimations as additional controls controls.

Figure E.4. Unlevered Returns Sensitivity Contour Plots



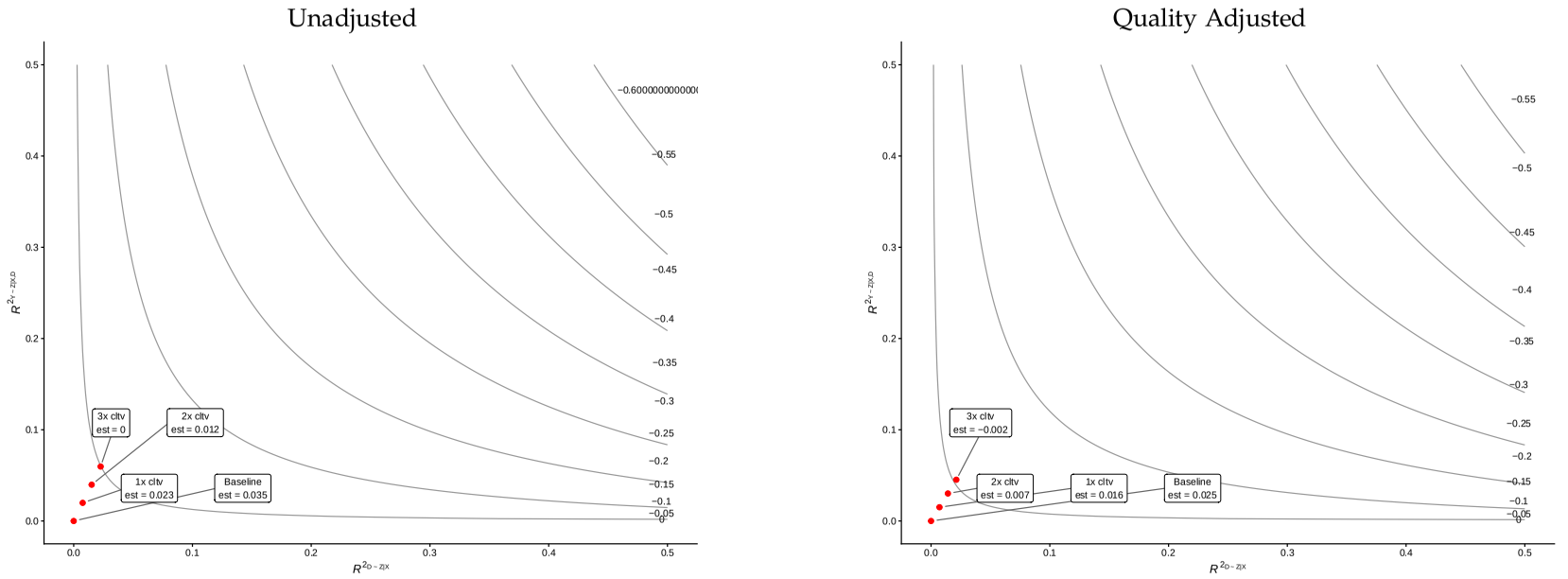
Note: Figure E.4 presents Cinelli-Hazlett sensitivity contour plots for the unlevered annualized return regression specified in Equation 4.1, using the indicator for investor purchases as the benchmark covariate. Panel A does not include quality-related tokens in the specification, while Panel B includes the additional text-based controls.

Figure E.5. Levered Returns Sensitivity Contour Plots



Note: Figure E.5 presents Cinelli-Hazlett sensitivity contour plots for the levered annualized return regression specified in Equation 4.1, using the indicator for investor purchases as the benchmark covariate. Panel A does not include quality-related tokens in the specification, while Panel B includes the additional text-based controls.

Figure E.6. Subsequent Default Sensitivity Contour Plots



Note: Figure E.6 presents Cinelli-Hazlett sensitivity contour plots for the subsequent default regression specified in Equation 4.2, using the set of CLTV indicators as the benchmark covariate. Panel A does not include quality-related tokens in the specification, and Panel B includes the additional text-based controls.

E.4 Listing Agent IV Estimation - Zip Code Level

This section provides a series of robustness checks showing that our IV estimates are largely unchanged when we exclude high-concentration zip codes in which an agent exceeds a 10 percent share in Table E.3, low-dispersion zip codes with a standard deviation less than 0.20 in Table E.4, and transactions by listing agents in the top 5% of listing volume within each state in Table E.5.

Table E.3. Listing Agent IV Using Low Agent Concentration Zip Codes ($\leq$ 10% share)

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.013^{***}		-0.065^{***}		0.036^{***}	
	(0.0004)		(0.003)		(0.001)	
Bidding war (fitted)		-0.032^{***}		-0.170^{***}		0.023^{***}
		(0.002)		(0.018)		(0.003)
Observations	4,463,373	4,463,373	4,463,373	4,463,373	4,463,373	4,463,373
Adjusted R^2	0.249	0.249	0.173	0.173	0.239	0.238
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.3 reruns the analyses in Table 10 using a subsample of low agent concentration zip codes in which an agent does not exceed a 10 percent share.

Table E.4. Listing Agent IV Using High Dispersion Zip Codes (SD $\geq$ .20)

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.013^{***}		-0.067^{***}		0.037^{***}	
	(0.0004)		(0.003)		(0.001)	
Bidding war (fitted)		-0.033^{***}		-0.177^{***}		0.025^{***}
		(0.002)		(0.018)		(0.003)
Observations	4,260,592	4,260,592	4,260,592	4,260,592	4,260,592	4,260,592
Adjusted R^2	0.252	0.252	0.179	0.179	0.240	0.239
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.4 reruns the analyses in Table 10 using a subsample of high-dispersion zip codes with a standard deviation greater than or equal to 0.20.

Table E.5. Listing Agent IV Without Top Listing Agents (Drop Top 5%)

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	−0.013***		−0.064***		0.036***	
	(0.0004)		(0.004)		(0.001)	
Bidding war (fitted)		−0.029***		−0.139***		0.024***
		(0.002)		(0.019)		(0.003)
Observations	3,299,696	3,299,696	3,299,696	3,299,696	3,299,696	3,299,696
Adjusted R^2	0.261	0.261	0.190	0.190	0.236	0.235
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

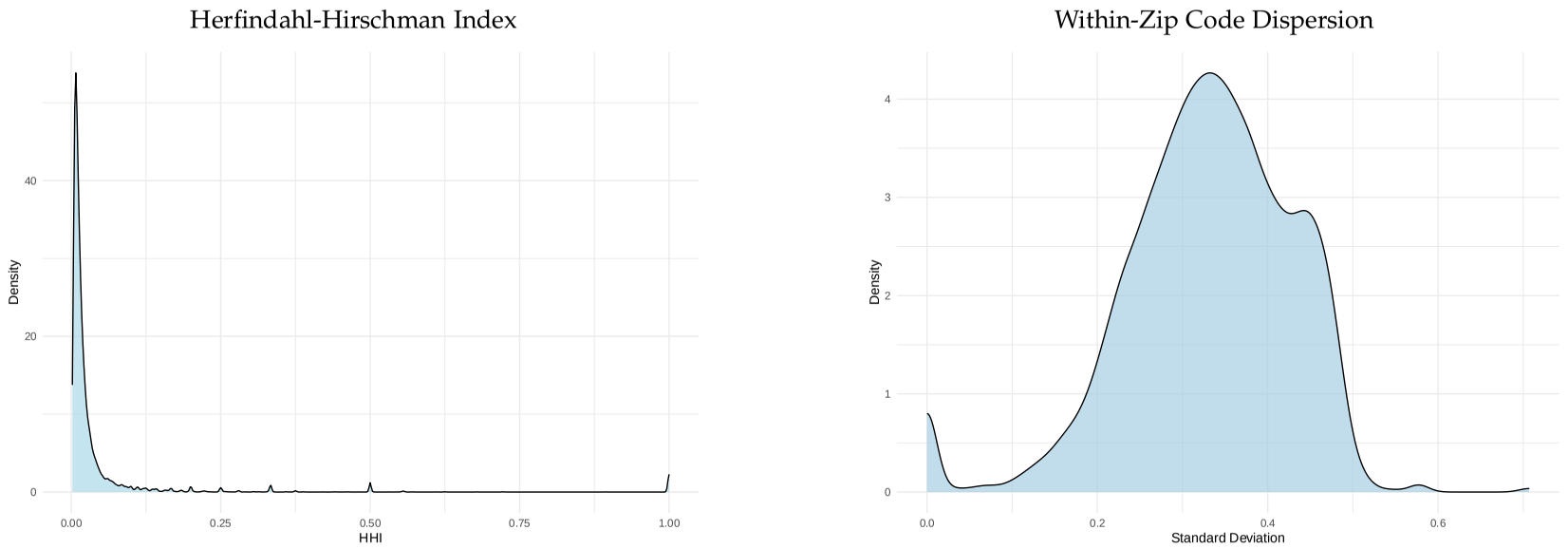
*p<0.1; **p<0.05; ***p<0.01

Note: Table E.5 reruns the analyses in Table 10 using a subsample of transactions that exclude agents in the top 5% listing volume within each state.

E.5 Listing Agent IV Estimation - Census Tract Level

This section provides a series of robustness checks showing that our IV estimates are largely unchanged when we run them at a more granular census tract level. Figure E.7 examines listing agent concentration and bidding war dispersion at the census tract level. Similar to the zip code-level results in the main paper, Panel A shows no single agent accounts for more than a small fraction of listings, and only a few census tracts contain an agent with more than 10 percent of the market. Similarly, Panel B shows agents within a census tract very rarely adopt a uniform bidding war listing strategy. The IV estimates are also similar when we exclude high-concentration census tracts in which an agent exceeds a 10 percent share in Table E.6, low-dispersion census tracts with a standard deviation less than 0.20 in Table E.7, and transactions by listing agents in the top 5% of listing volume within each state in Table E.8.

Figure E.7. Listing Agent Concentration and Bidding War Dispersion by Census Tract



Note: Figure E.7 examines census tract-level listing agent concentration and bidding war dispersion. Panel A plots the distribution of the census tract-level Herfindahl-Hirschman Index (HHI) based on each agent's share of listings in that census tract. Panel B plots the distribution of withinâcensus tract dispersion in agents' biddingâwar propensities, measured as the standard deviation of agent-level bidding war rates within each census tract.

Table E.6. Listing Agent IV Using Low Agent Concentration Census Tracts ($\leq$ 10% share)

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.011^{***}		-0.053^{***}		0.037^{***}	
	(0.0003)		(0.003)		(0.001)	
Bidding war (fitted)		-0.020^{***}		-0.095^{***}		0.019^{***}
		(0.002)		(0.021)		(0.002)
Observations	4,062,898	4,062,898	4,062,898	4,062,898	4,062,898	4,062,898
Adjusted R^2	0.463	0.463	0.430	0.430	0.234	0.233
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Tract-BuyQY FE	✓	✓	✓	✓	✓	✓
Tract-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.6 reruns the analyses in Table 10 at the census tract level using a subsample of low agent concentration census tracts in which an agent does not exceed a 10 percent share.

Table E.7. Listing Agent IV Using High Dispersion Census Tracts (SD $\geq$ .20)

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.011^{***}		-0.053^{***}		0.037^{***}	
	(0.0003)		(0.003)		(0.001)	
Bidding war (fitted)		-0.026^{***}		-0.134^{***}		0.019^{***}
		(0.002)		(0.019)		(0.002)
Observations	4,377,026	4,377,026	4,377,026	4,377,026	4,377,026	4,377,026
Adjusted R^2	0.477	0.477	0.461	0.461	0.233	0.232
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Tract-BuyQY FE	✓	✓	✓	✓	✓	✓
Tract-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.7 reruns the analyses in Table 10 using a subsample of high-dispersion census tracts with a standard deviation greater than or equal to 0.20.

Table E.8. Listing Agent IV Without Top Listing Agents (Drop Top 5%)

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	−0.010***		−0.044***		0.036***	
	(0.0004)		(0.004)		(0.001)	
Bidding war (fitted)		−0.022***		−0.088***		0.022***
		(0.002)		(0.024)		(0.003)
Observations	3,285,338	3,285,338	3,285,338	3,285,338	3,285,338	3,285,338
Adjusted R^2	0.474	0.474	0.399	0.399	0.228	0.227
Investor Control	✓	✓	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓	✓	✓
Tract-BuyQY FE	✓	✓	✓	✓	✓	✓
Tract-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
House Characteristics					✓	✓
Financing Controls					✓	✓
Data Sample	Panel C	Panel C	Panel C	Panel C	Panel C	Panel C

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.8 reruns the analyses in Table 10 using a subsample of transactions that exclude agents in the top 5% listing volume within each state.

E.6 Transaction-level Demographics

The empirical results in Section 6 suggest that socioeconomically vulnerable homebuyers are more susceptible to the winner's curse effects associated with bidding wars. This section examines the key predictions generated by Prediction 5 using transaction-level demographic and financial information from HMDA. We begin by examining the probability of winning a bidding war by homebuyer race and income.[6] Table E.9 displays coefficient estimates from a linear probability model that regresses the bidding war indicator variable on either the race or household income of the homebuyer as well as the full set of controls in Equation 4.2.

Column 1 of Table E.9 displays coefficient estimates for four mutually distinct race and ethnicity buyer groups (American Indian or Alaska Native, Asian or Pacific Islander, Black, or Hispanic) relative to White buyers who purchase housing in the same zip code during the same quarter.[7] The coefficient estimates in column 1 indicate that American Indian or Alaska Native (AIAN) and Asian or Pacific Islander (API) homebuyers are significantly less likely to purchase their house in a bidding war than White homebuyers. In contrast, Black and Hispanic homebuyers are significantly more likely to purchase their house in a bidding war than White homebuyers.

Column 2 of Table E.9 displays coefficient estimates for buyers in income quartiles 2 to 4 relative to buyers in income quartile 1. Given buyer incomes vary over space and time, we create the income quartiles within state and year. The coefficient estimates indicate that buyers in income quartile 2 are more likely to purchase their house in a bidding war than buyers in income quartile 1. However, higher-income buyers in income quartiles 3 and 4 are less likely to purchase their house in a bidding war than buyers in income quartile 1. Consistent with Prediction 5, the coefficient estimates in Table E.9 indicate that the probability of a bidding war is higher for low-income homebuyers and Black and Hispanic homebuyers. Although we do not observe whether the homebuyer has a college degree in the transaction-level data, education is likely correlated with household income.

[6] In unreported results, we also investigate the winner's curse effects by gender. Similar to Kim et al. (2019) and Goldsmith-Pinkham and Shue (2023), we find evidence of a gender gap in housing returns. However, we find no evidence that bidding wars meaningfully contribute to the gender gap.

[7] We determine the race and ethnicity of the homebuyer using demographic information about the mortgage applicant and co-applicant in HMDA. We successfully match over 80% of the financed subsequent default transaction sample in Panel B of Table 2 to HMDA. The internet appendix provides a description of our process and state-level match rates.

E.6.1 Homebuyer Race

This section tests the prediction generated by Prediction 5 that the winner's curse effects will be stronger for Black and Hispanic homebuyers. We test these predictions by modifying Equations 4.1 and 4.2 to include indicator variables for the race of the homebuyer and its interaction with the bidding war indicator variable. Table E.10 displays coefficient estimates for the four race and ethnicity buyer groupings relative to White buyers. Similar to Kermani and Wong (2026) and Kahn (2024), the coefficient estimates in columns 1 and 3 provide evidence consistent with the existence of a racial gap in realized housing returns. We further decompose this racial gap in returns by transaction type: bidding war vs. non-bidding war transactions.

For non-bidding war transactions in Table E.10, we find minority homebuyers earn lower unlevered and levered annualized returns than White homebuyers. In column 1 of Table E.10, American Indian or Alaska Native (AIAN) homebuyers experience 0.3pp lower unlevered annualized returns, Asian or Pacific Islander (API) homebuyers experience 0.5pp lower unlevered annualized returns, Hispanic homebuyers experience 0.7pp lower unlevered annualized returns, and Black homebuyers experience 1.2pp lower unlevered annualized returns than White homebuyers who purchase their house in non-bidding war transactions. Similar to Kermani and Wong (2026), we also find minority homebuyers are significantly more likely than White homebuyers to subsequently default during our sample period. For this reason, we find a large portion of the racial gap in housing returns can be accounted for by distressed sales (i.e., homebuyers who subsequently default). After removing transactions that subsequently default in column 2, the AIAN-White gap in annual returns drops from 0.3pp to 0.2pp lower unlevered annualized returns, the API-White gap drops from 0.5pp to 0.4pp lower unlevered annualized returns, the Black-White gap is more than cut in half from 1.2pp to 0.5pp lower unlevered annualized returns, and the Hispanic-White gap in annual returns is no longer statistically significant. A similar pattern is displayed for levered annualized returns in columns 3 and 4 of Table E.10.

For bidding war transactions in column 1 of Table E.10, we find buyers of every race who purchase their house in a bidding war experience lower annualized returns relative to same-race buyers in non-bidding war transactions. For example, White buyers who purchase their house in a bidding war experience 0.7pp lower unlevered annualized returns than White buyers who purchase their house in a non-bidding war. The bidding

war and race interaction coefficient estimates in column 1 also indicate that bidding wars meaningfully contribute to the racial gap in realized housing returns. Consistent with Prediction 5, we find Black and Hispanic homebuyers who purchase their house in a bidding war experience 0.2pp and 0.1pp lower annualized returns, respectively, than White homebuyers who purchase their house in a bidding war. Similarly, we find Black and Hispanic homebuyers who purchase their house in a bidding war are significantly more likely to default on their mortgage than White homebuyers who purchase their house in a bidding war.

E.6.2 Homebuyer Income

Next, we test the prediction generated by Prediction 5 that the winner's curse effects will be stronger for lower-income homebuyers. In Table E.11, the income quartile coefficient estimates compare higher-income buyers in quartiles 2 to 4 to lower-income buyers in quartile 1. For non-bidding war transactions, the coefficient estimates in column 1 indicate that higher-income buyers in income quartiles 2 to 4 earn lower annualized unlevered returns than lower-income buyers in income quartile 1. The coefficient estimates in columns 5 and 6 of Table E.11 indicate that higher-income buyers are less likely to default than lower-income buyers in non-bidding war transactions. Accordingly, after we remove transactions that subsequently default in column 2, we find even stronger evidence that higher-income buyers in income quartiles 2 to 4 earn lower annualized unlevered returns than lower-income buyers in income quartile 1 in non-bidding war transactions.

For bidding war transactions in column 1 of Table E.11, we find buyers in every income quartile who purchase their house in a bidding war experience lower annualized returns than buyers in the same income quartile who purchase their house in the same zip code at roughly the same time in non-bidding war transactions. For example, buyers in income quartile 1 who purchase their house in a bidding war experience 0.9pp lower annualized unlevered returns than buyers in income quartile 1 who bought their house in a non-bidding war. A review of the bidding war and income quartile interaction coefficient estimates in column 1 indicates that bidding wars adversely affect the realized unlevered returns of buyers in every quartile. That said, the results in Table E.9 indicate buyers in income quartiles 1 and 2 are more likely to purchase their house in a bidding war than buyers in income quartiles 3 and 4, implying they

may be more susceptible to the winner's curse we document. This conjecture is further supported in columns 3 and 4, where the bidding war and income quartile interaction coefficient estimates indicate that bidding wars meaningfully contribute to the income gap in realized levered housing returns.

Table E.9. Probability of Winning a Bidding War

Race		Income	
	(1)		(2)
AIAN	-0.005^{**}	Quartile 2	0.002^{***}
	(0.002)		(0.001)
API	-0.005^{***}	Quartile 3	-0.003^{***}
	(0.001)		(0.001)
Black	0.006^{***}	Quartile 4	-0.004^{***}
	(0.001)		(0.001)
Hispanic	0.017^{***}		
	(0.001)		
Observations	6,649,499		6,649,499
Adjusted R^2	0.131		0.131
House Characteristics	✓		✓
Investor Control	✓		✓
Financing Controls	✓		✓
Zip-BuyQY FE	✓		✓
Subsequent Defaults	✓		✓

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.9 examines the probability of winning a bidding war by the race and income of the homebuyer using a subsample of transactions where we can identify the homebuyer's demographic and financial information in HMDA. Each column displays coefficient estimates from a modified version of Equation 4.2, where the dependent variable equals 1 if the transaction is a bidding war and 0 otherwise. Column 1 displays coefficient estimates for the race and ethnicity of buyers relative to white buyers. Column 2 displays coefficient estimates for buyers with an income in quartiles 2 to 4 relative to buyers with a lower income in quartile 1. Standard errors are clustered by zip code.

Table E.10. Homebuyer Race

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	−0.007***	−0.005***	−0.029***	−0.022***	0.013***	0.021***
	(0.0002)	(0.0002)	(0.001)	(0.001)	(0.0004)	(0.001)
AIAN	−0.003***	−0.002**	−0.014***	−0.010*	0.006***	0.020***
	(0.001)	(0.001)	(0.005)	(0.006)	(0.002)	(0.003)
API	−0.005***	−0.004***	−0.016***	−0.015***	0.009***	0.034***
	(0.0002)	(0.0003)	(0.001)	(0.002)	(0.001)	(0.001)
Black	−0.012***	−0.005***	−0.036***	−0.017***	0.011***	0.080***
	(0.0004)	(0.0004)	(0.002)	(0.003)	(0.001)	(0.002)
Hispanic	−0.007***	0.0004	−0.014***	−0.001	0.018***	0.065***
	(0.0002)	(0.0002)	(0.001)	(0.002)	(0.001)	(0.001)
Bidding war × AIAN	−0.002	−0.0001	−0.003	−0.001	0.008**	0.014**
	(0.001)	(0.001)	(0.008)	(0.009)	(0.004)	(0.007)
Bidding war × API	0.002***	0.001*	0.008***	0.009**	−0.002*	−0.0003
	(0.0005)	(0.001)	(0.003)	(0.004)	(0.001)	(0.002)
Bidding war × Black	−0.002***	−0.002**	−0.003	−0.009	0.008***	0.006*
	(0.001)	(0.001)	(0.003)	(0.006)	(0.001)	(0.003)
Bidding war × Hispanic	−0.001***	−0.001	0.001	−0.004	0.006***	0.006***
	(0.0003)	(0.0004)	(0.002)	(0.003)	(0.001)	(0.002)
Observations	2,922,068	2,360,051	2,922,068	2,360,051	6,649,499	2,922,068
Adjusted R^2	0.594	0.439	0.318	0.296	0.219	0.298
Investor Control	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

*p<0.1; **p<0.05; ***p<0.01

Note: Table E.10 examines the subsequent performance of bidding war transactions by buyer race relative to white buyers using a subsample of transactions where we can identify the homebuyer's demographic and financial information in HMDA. Columns 1 to 4 display coefficient estimates from Equation 4.1 examining either the annualized unlevered or levered returns of bidding war transactions relative to non-bidding war transactions. Columns 5 and 6 display coefficient estimates from Equation 4.2 examining the probability that homebuyers who purchase their house in bidding war transactions subsequently default relative to those in non-bidding war transactions. Standard errors are clustered by zip code.

Table E.11. Homebuyer Income

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	−0.009***	−0.006***	−0.034***	−0.027***	0.016***	0.026***
	(0.0003)	(0.0003)	(0.002)	(0.002)	(0.001)	(0.001)
Quartile 2	−0.002***	−0.005***	−0.011***	−0.017***	0.001*	−0.009***
	(0.0002)	(0.0002)	(0.001)	(0.002)	(0.0004)	(0.001)
Quartile 3	−0.003***	−0.008***	−0.013***	−0.022***	−0.002***	−0.018***
	(0.0002)	(0.0002)	(0.002)	(0.002)	(0.0004)	(0.001)
Quartile 4	−0.004***	−0.011***	−0.014***	−0.025***	−0.003***	−0.024***
	(0.0003)	(0.0003)	(0.002)	(0.002)	(0.001)	(0.001)
Bidding war × Quartile 2	0.001***	0.0003	0.004**	0.004	−0.001	−0.002
	(0.0003)	(0.0004)	(0.002)	(0.003)	(0.001)	(0.002)
Bidding war × Quartile 3	0.002***	0.001*	0.005**	0.006**	−0.001*	−0.004**
	(0.0003)	(0.0004)	(0.002)	(0.003)	(0.001)	(0.002)
Bidding war × Quartile 4	0.004***	0.003***	0.014***	0.017***	−0.005***	−0.007***
	(0.0004)	(0.0005)	(0.003)	(0.004)	(0.001)	(0.002)
Observations	2,922,068	2,360,051	2,922,068	2,360,051	6,649,499	2,922,068
Adjusted R^2	0.594	0.440	0.318	0.296	0.218	0.295
Investor Control	✓	✓	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Hold Period	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

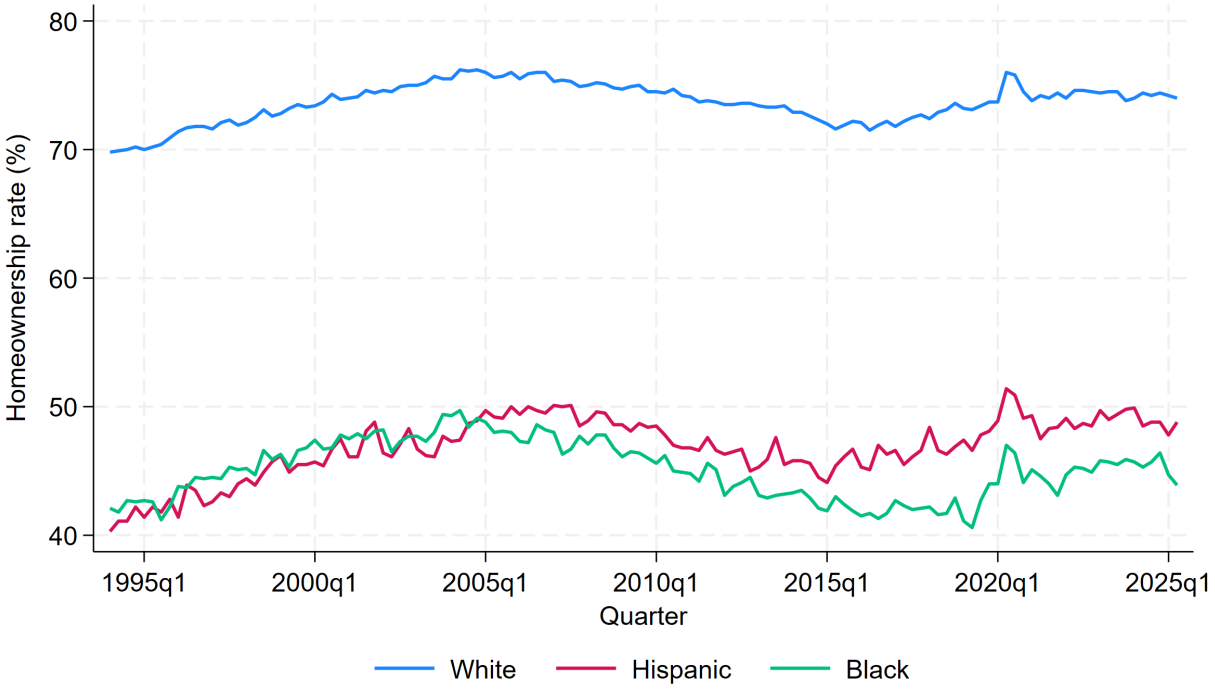
*p<0.1; **p<0.05; ***p<0.01

Note: Table E.11 examines the subsequent performance of bidding war transactions by income quartile relative to lower-income buyers in quartile 1 using a subsample of transactions in Table 2 where we can identify the gender of the homebuyer's demographic and financial information in HMDA. Columns 1 to 4 display coefficient estimates from Equation 4.1 examining either the unlevered or levered returns of bidding war transactions relative to non-bidding war transactions. Columns 5 and 6 display coefficient estimates from Equation 4.2 examining the probability that a bidding war transaction subsequently defaults relative to non-bidding war transactions. Standard errors are clustered by zip code.

E.7 Homeownership by Race

Figure E.8 plots the homeownership rates by race and ethnicity from 1994 through 2025. The figure highlights the relatively low levels of homeownership for black and Hispanic households relative to white households in the United States.

Figure E.8. Homeownership by Race



Note: Figure E.8 plots quarterly homeownership rates as defined by the U.S. Census Bureau's Housing Vacancy Survey as the share of households that are owner occupied by race and ethnicity from 1994 through 2025. Blue denotes the homeownership rate for Non-Hispanic White alone, red denotes the rate for Hispanic (of any race), and green denotes the rate for Black alone. Source: U.S. Census Bureau Housing Vacancy Survey via FRED.