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 predictions, we find homebuyers who purchase their house in a bidding war experience a 10.5pp lower average total unlevered return and are 1.9pp more likely to default. These winner's curse effects are more pronounced among socioeconomically vulnerable homebuyers.

Keywords: Winner's Curse, Bidding War, Housing Returns, Default

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1 Introduction

The winner's curse is a phenomenon in which the winner of a competitive bidding process overpays, often due to incomplete information or over-optimism about the asset's value. In the housing market, multiple homebuyers frequently compete for the same house resulting in a bidding war. This paper examines whether homebuyers who purchase their house in a bidding war overpay and suffer from a winner's curse. We test for a winner's curse by comparing the subsequent performance of bidding war transactions to 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. 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 homes. 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 Bucchianeri and Minson (2013) and Han and Strange (2014), 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 sale prices stay below list prices in rational equilibrium. In contrast, in limited rationality equilibrium, 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 those we do not.

We test for and find evidence of a winner's curse using over 14 million housing transactions across thirty 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 find 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.

The use of mortgage financing exposes homebuyers to default risk. We find strong evidence of a winner's curse when examining the effect of bidding wars on homebuyers' subsequent default behavior. Buyers who purchase their homes in bidding wars are 1.9 percentage points more likely to default on their mortgages than those who purchase in non-bidding war transactions. Consistent with our model, we show 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. Additionally, we demonstrate that our findings hold under several different definitions of bidding wars.

An alternative explanation for our findings is that bidding war winners' overpayment may be justified by rational decision-making, such as a strong personal preference for the house. We test and rule out this alternative explanation by comparing the holding period of buyers who purchase their house in bidding wars to those who purchase their house in non-bidding war transactions. If bidding war winners have a strong personal preference for the house they purchase, then we expect them to stay in their house longer. However, we find the opposite. Homebuyers who purchase their house in bidding wars have significantly shorter holding periods than those who purchase their house in non-bidding war transactions.

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, less-educated, and single-parent 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.

1.1 Related Literature

Our study builds on research examining the winner's curse in various settings. The phenomenon was first documented by Capen et al. (1971), who highlighted how aggressive bidding in oil lease auctions frequently led to overpayment. Subsequent theoretical advancements, such as those by Milgrom and Weber (1982), formalized the winner's curse in auction theory by incorporating the role of common values and bidder asymmetry. Empirical studies have tested for a winner's curse in 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).

We contribute to the literature by testing for the winner's curse in a new setting: the U.S. residential housing market. Our setting is ideal for several reasons. First, there is a common value component in a house from which buyers can extract distinct signals about its quality. Second, housing transaction data offers a rare opportunity to examine market participant behavior and its effect on market outcomes. Third, in contrast to previous research that tests for a winner's curse by measuring short-term returns (e.g., cumulative abnormal returns), we exploit the repeat sales of millions of housing transactions to measure homebuyers' long-term returns and identify future financial distress.

To the best of our knowledge, this is the first study to empirically test for a winner's curse in housing markets.[1] The home buying process is conventionally modeled as a sequential bargaining game where a seller considers an offer as it arrives (Yinger, 1981; Haurin et al., 2010; Carrillo, 2012). However, we show that bidding wars have become increasingly common, representing more than half of all housing transactions in recent years. This finding aligns with recent research examining the effects of bidding wars on housing market outcomes (Han and Strange, 2014; Bucchianeri and Minson, 2013; Liu and Smith, 2023) as well as news reports documenting the increasing frequency of bidding wars since the COVID-19 pandemic (Anderson, 2022).

To the extent that sellers can impose a deadline to collect offers and induce a bidding war, the bidding war process closely mirrors a first-price sealed bid auction (Williams, 1995; Quan, 2002). Since competitive bidding can lead the winning buyer to overpay relative to the house's intrinsic value (Lauermann and Speit, 2023), a winner's curse can arise. Our model of bidding wars in a first-price auction contributes to the existing research examining the winner's curse in common value auctions (Wilson, 1977; Engelbrecht-Wiggans, 1980; Milgrom and Weber, 1982; McAfee and McMillan, 1987), while complementing prior

[1] **[Mention the few studies that test for winner's curse in real estate auctions (cite those that explicitly test for winner's curse). Briefly explain why different than our setting.]**

studies that utilize the average of bidders' signals as the basis for the common value (Bikhchandani and Riley, 1991; Klemperer, 1998; Bulow et al., 1999; Goeree and Offerman, 2002).

In light of the current U.S. housing affordability crisis, there is considerable policy interest in evaluating the consequences of the rising frequency of bidding wars. Our finding that lower-income, Black, and Hispanic buyers are more susceptible to the winner's curse effects stemming from bidding wars aligns with recent research documenting racial disparities in housing returns (Kermani and Wong, 2021; Kahn, 2024). By showing these socioeconomically vulnerable homebuyers are particularly susceptible to the winner's curse effects, we provide a framework to better understand how bidding wars influence market inefficiencies and perpetuate economic inequities among homebuyers.

2 Model of Bidding Wars in the Housing Market

In this section, we construct a model of bidding wars in the housing market, in which multiple risk-neutral buyers can 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.[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[3].

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 bidder, who do not observe v directly. Instead, each bidder i obtains a noisy private signal θ_i, which is drawn 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:

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

[2] The limited rationality environment should not be interpreted as homebuyers necessarily behaving irrationally in this equilibrium. 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); Menzio (2023).

[3] The majority of homes in our data sample are sold below the asking price.

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, build 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

$$\pi_i^R = \Pr(b_i > b_{-i}) E\left[v - b_i | \theta_i, b_i > b_{-i}\right], \tag{2}$$

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 his or her private signal of the following form:

$$b_i^R = \underline{\theta} + a(\theta_i - \underline{\theta}), \tag{3}$$

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*

$$b_i^R = \underline{\theta} + a^R(\theta_i - \underline{\theta}), \tag{4}$$

where

$$\alpha^R = \frac{(n+2)(n-1)}{2n^2}. \tag{5}$$

The expected price P_n^R paid at the auctions with n bidders increases with the number of bidders n, but never exceeds μ, and is equal to

$$P_n^R = \mu - \frac{\bar{\theta} - \underline{\theta}}{n(n+1)}. \tag{6}$$

Proof. *See Appendix Section A.*

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

2.2 Equilibrium with Limited Rationality

In this subsection, we analyze an equilibrium, in which bidders fail to take into account all the available information. As in the rational equilibrium, bidders with limited rationality will make their offers conditional on their private signal. However, they will 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 his. 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 his expected payoff π_i^L as follows:

$$\pi_i^L = \Pr(b_i > b_{-i})\left(\theta_i - b_i\right). \tag{7}$$

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

$$b_i^L = \underline{\theta} + \alpha^L(\theta_i - \underline{\theta}), \tag{8}$$

for some $\beta > 0$. In the equilibrium with limited rationality, each bidder maximizes their perceived payoff as given by (7), considering that their competitors bid according to (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*

$$b_i^L = \underline{\theta} + \alpha^L(\theta_i - \underline{\theta}), \tag{9}$$

where

$$\alpha^L = \frac{n-1}{n}. \tag{10}$$

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

$$P_n^L = \mu - \frac{(3-n)(\bar{\theta} - \underline{\theta})}{2(n+1)}. \tag{11}$$

Proof. *See Appendix Section A.*

The rational and irrational equilibria share several common features. In both equilibria, bids (4) and (9) are increasing with private signals. In addition, the average prices (6) and (11) paid by the auction winners are increasing in the number of bidders n. This is not unexpected. 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. Figure 1 demonstrates these salient differences between the two equilibria with $\mu = 400$.

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 (4) - (6) and (9) - (11), we can write

$$\Delta b_i = \frac{(n-1)(n-2)}{2n^2}(\theta_i - \underline{\theta}), \tag{12}$$

$$\Delta P_n \;=\; \frac{(n-1)(n-2)}{2n(n+1)}(\bar{\theta}-\underline{\theta}). \tag{13}$$

Inspecting equations (11) and (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 (12) and (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 Section A.*

2.4 Bilateral Bargaining

We have so far 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. As a result, it 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.[4]

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

$$p_1^B = \min\left\{P^A, \frac{\underline{\theta}+\theta_1}{2}\right\}. \tag{14}$$

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

$$p_1^B = \frac{\underline{\theta}+\theta_1}{2}. \tag{15}$$

[4]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

$$P^B = \frac{\underline{\theta} + \frac{\underline{\theta}+\bar{\theta}}{2}}{2} = \mu - \frac{\bar{\theta} - \underline{\theta}}{4}. \tag{16}$$

Comparing (16) to (6) and (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 her bargaining position, the seller optimally set the list price to match the expected intrinsic value of the home:*

$$P^A = \mu. \tag{17}$$

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

$$P^B = \mu - \frac{\bar{\theta} - \underline{\theta}}{4}. \tag{18}$$

2.5 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 homes.

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 the repeat sale approach. The resale price should, on average, reflect the intrinsic value of the house. Hence, buyers who overpay for their homes will experience lower house price appreciation in the future. As a result, we have the first prediction of the model.

Hypothesis 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.

Hypothesis 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.

Hypothesis 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

Hypothesis 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.*

In addition, we use the seller agent's experience as a proxy for the likelihood of a bidding war. The intuition of the proxy is that experienced agents can attract more bidders and provoke a bidding war, which leads to the following prediction.

Hypothesis 5 *Bidding war transactions are more likely for homes listed by more experienced real estate agents.*

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. Similarly, although we do not observe the constraints homebuyers face in the housing market, we note that different groups of homebuyers have varying likelihoods of being exposed to these constraints (e.g., Clampet-Lundquist (2003), Harvey et al. (2020), and Bergman et al. (2024)). Accordingly, we expect to find stronger signs of the winner's curse for low-income, minority, and single-parent homebuyers. These points lead to the following prediction.

Hypothesis 6 *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 home buyers in the following homebuyer groups:*

a) Black and Hispanic

b) lower income

c) less educated

d) single parents

3 Data and Measurement

This section describes the data and the construction of several key measures, including our bidding war variable. We then provide summary statistics for the data we use in the empirical analysis.

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 nationwide multiple listing service (MLS) data with ownership transfer (i.e., deeds records) data from CoreLogic. 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. 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.[5] 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. Figure 2 displays the geographic coverage of the filtered dataset.

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 (as discussed in more detail below). 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 CoreLogic, 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.

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. In some cases, the number of bidders involved or bids received is observable in the data (Mateen et al., 2024). However, 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.[6] Our definition of bidding wars is similar to previous research on the topic (Bucchianeri and Minson, 2013; Han and Strange, 2014).

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 key-

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

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

words 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 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 they are only 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 the keyword appears in both bidding war and non-bidding war transactions at roughly the same rate.[7] A review of non-bidding war transactions shows that agents often mention a “spacious backyard that *offers* lots of privacy" or an “open floor design that *offers* room to grow." In other cases, the agent 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 above-mentioned difficulties. The additional context results in substantially 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 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 highlighting 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.[8] Taken together with the unigram and bigram bidding war keywords in Table 1, the location keywords provide evidence consistent with the underlying assumption of

[7]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.

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

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 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 Hypothesis 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 home-buyers 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 identify the remaining mortgage balance (i.e., outstanding principal) when the house is sold 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 unless the buyer refinances) 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 than buyers who purchase their houses via non-bidding war transactions (6.27 years versus 6.73 years).

4 Main Results

This section describes the regression methodology we use to test the empirical predictions in Section 2.5. Then we summarize our main results highlighting the difference in subsequent performance between bidding war and non-bidding war transactions.

4.1 Estimation Approach

We use a linear regression framework to examine 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:

$$r_{is} = BiddingWar_{ib}\beta_1 + Hold_{ib}\beta_2 + X'_{ib}\beta_X + \epsilon_{ib} \tag{19}$$

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 19 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:

$$Pr(Distress_{is}) = BiddingWar_{ib}\beta_1 + X_{ib}\beta_X + \epsilon_{ib} \tag{20}$$

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 20 include housing characteristics, financing, 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. Recognizing leverage is an important determinant of subsequent default, we also include indicator variables identifying the CLTV ratio for each transaction.[9] 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 19 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 Hypothesis 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 overpaying by 8.2pp for houses purchased in bidding wars.[10]

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 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 homebuyers who do not purchase their house in a bidding war. This coefficient is economically significant as the 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, these results provide evidence consistent with the empirical prediction in Hypothesis 1 that homebuyers who purchase their house in a bidding war overpay and suffer from a winner's curse.

Figure 3 plots the corresponding state-level bidding war coefficient estimates from

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

[10] 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 $(1.053/1.040)^{6.3} - 1 = 0.082$

Equation 19 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 -0.4pp in Nevada, where the average annualized unlevered return is 3.8pp in the state sample, to 2.8pp in Tennessee, where the average annualized unlevered return is 6.6pp in the state sample.

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 homebuyers who did not purchase their house in a bidding war. This coefficient is economically significant as the 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 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 6.5pp lower annualized levered returns than homebuyers who do not purchase their house in a bidding war. This coefficient is economically significant as the 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 empirical prediction in Hypothesis 2 that homebuyers who purchase their house in a bidding war overpay and suffer from a winner's curse.

Figure 4 plots the corresponding state-level bidding war coefficient estimates from Equation 19 examining homebuyers' levered returns by transaction type. The coefficient estimate for bidding war transactions is negative and statistically significant in 25 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 25 statistically significant states: ranging from -2.4pp in Arizona, where the average annualized levered return is 23.3pp in the state sample, to -17.2pp in Missouri, where the average annualized levered return is 33.5pp in the state sample.

4.4 Subsequent Default

The lower annualized returns for bidding war transactions provide evidence consistent with the predictions generated by Hypothesis 1 and 2 that bidding war winners overpay for their houses. This section tests Hypothesis 3, which predicts 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 even decrease in value, the buyers are more likely to be underwater, which increases their likelihood of default. Table 4 displays bidding war coefficient estimates from Equation 20 examining the probability that the buyer in a bidding war transaction subsequently defaults relative to a buyer in a non-bidding war transaction.

Column 1 of Table 4 uses the subsequent default sample of transactions 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 homebuyers who did not purchase their house in a bidding war. This coefficient is economically significant as the 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 later refinances) as well as transactions where financing information is unavailable. Column 2 re-estimates Equation 20 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 homebuyers who did not purchase their house via a bidding war. This coefficient is economically significant as the 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 homebuyers who did not purchase their house in a bidding war. This coefficient is economically significant as the probability of default in the sample is 18.3pp.

Figure 5 plots the corresponding state-level bidding war coefficient estimates from Equation 20. 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 0.5pp 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.

4.5 Alternative Bidding War Measures

We classify a transaction as a bidding war if its sales price exceeds its list price. In Section 3.2, we perform a systematic textual analysis of listing agents' public remarks to show that our bidding war classification strategy effectively captures bidding war transactions. In Table 5, we show that our main results 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, our key empirical results related to Hypothesis 1, 2, and 3 remain robust. 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 Winner's Curse?

A homebuyer may overbid for a house because they receive a bad signal, thereby suffering the winner's curse. Alternatively, the homebuyer may bid higher than other bidders because they have a higher valuation of the house. Perhaps, for consumption purposes. In this case, the homebuyer's overpayment is rational and they do not suffer a winner's curse. We examine this alternative explanation by comparing the holding period of buyers who purchase their houses in a bidding war to the holding period of buyers who do not purchase their houses in a bidding war. The underlying assumption of this test is that if homebuyers overbid because they value the house more, we should find that they have longer holding periods (i.e., they stay in their house longer and enjoy a higher aggregate utility flow).

Table 6 reports results from a modified version of Equation 20 where we regress the buyers' holding period (expressed in days) on the bidding war indicator variable and the full set of controls highlighted in Section 4.1. Column 1 of Table 6 uses the returns sample in Panel C of Table 2, where each buyer's purchase and subsequent sale dates are observable in the data. Column 3 of Table 6 uses the financed returns sample in Panel D of Table 2. Columns 2 and 4 further restrict the returns sample in the previous columns to only include transactions for homebuyers who did not subsequently default.

The coefficient estimate for bidding war transactions is negative and statistically significant in every column of Table 6. In Columns 1 and 3, the bidding war coefficient estimates indicate that homebuyers who purchase their house in a bidding war hold onto their house for 53 to 59 days less, on average, than homebuyers who purchase their house in non-bidding war transactions. After dropping homebuyers who subsequently default, the bidding war coefficient estimates indicate that homebuyers who purchase their house in bidding wars hold onto their house for 13 to 18 days less, on average, than homebuyers who purchase their house in non-bidding war transactions.

Overall, the results in Table 6 show that homebuyers who purchase their house through bidding wars have shorter holding periods compared to those who do not. This suggests that these buyers are not overbidding due to a strong preference for the house. If that were the case, we would expect bidding war transactions to have longer, rather than shorter, holding periods than non-bidding war transactions. These findings challenge the alternative explanation that homebuyers overbid because they place a higher valuation on the house. Instead, the results in Table 6 support the idea that homebuyers overbid due to receiving limited information signals, ultimately falling victim to the winner's curse.

5 Robustness Tests

5.1 Bidding War Concentration

Figure 6 highlights the cyclicality of bidding wars by plotting the percent of housing transactions sold via bidding wars over time. The figure uses the unfiltered transaction data set that includes transactions from January 2000 through the first half of 2023. From 2000 to 2007, bidding wars accounted for roughly 15 to 25% of all transactions. During the heart of the financial crisis (2008-2011), the share of bidding wars decreased to roughly 10% of all housing transactions before starting to rebound to pre-crisis levels from 2013 to 2019. Then in 2020, there was a sharp increase in the share of bidding wars to 30% of all transactions. In 2021, bidding wars accounted for just under 50% of all transactions before decreasing back to 35% in the first half of 2023.

The key prediction generated by Hypothesis 4 is that the winner's curse effects will be stronger in times and locations where bidding wars are more common. Table 7 re-examines the subsequent performance of bidding war transactions by running subperiod analyses based on the cyclicality highlighted above. Panel A of Table 7 uses transactions from the housing boom (2000-2007). The subperiod results in Panel A of Table 7 align with the full sample results in Tables 3 and 4, indicating that the subsequent performance of bidding war transactions is significantly worse than non-bidding war transactions. Specifically, the bidding war coefficient estimates in Columns 1 to 4 of Panel A indicate houses purchased via bidding wars have 0.8 to 1.3pp lower average annualized unlevered returns and 4.6 to 5.0pp lower average annualized levered returns than houses not purchased via bidding wars. Similarly, the bidding war coefficient estimates in Columns 5 and 6 of Panel A indicate that homebuyers who purchase their house in bidding wars are, on average, 3.7 to 4.7pp more likely to default than those who do not purchase their house in bidding wars.

The subperiod results in Panels B and C highlight the effect of market conditions on the subsequent performance of bidding war transactions. Panel B of Table 7 uses transactions from the housing bust (2008-2012), a period in time when bidding wars accounted for a relatively small fraction of all housing transactions. Similar to Panel A, though to a lesser extent, the bidding war coefficient estimates in Columns 1 and 3 of Panel B indicate that houses purchased via bidding wars experience significantly lower average annualized unlevered and levered returns than houses not purchased via bidding wars. The coefficient estimates in Columns 2 and 4 of Panel B indicate that conditional on not subsequently defaulting, homebuyers who purchase their house in a bidding war experience similar returns as homebuyers who did not purchase their house in a bidding war. That said, the bidding war coefficient estimates in Columns 5 and 6 of Panel B indicate that homebuyers who purchase their house in a bidding war are, on average, 1 to 2pp more likely to subsequently default than homebuyers who do not purchase their house in a bidding war.

Panel C of Table 7 uses transactions from the housing recovery (2013-2018), a period in time when both house prices and the share of bidding wars were increasing. Similar to Panel A, the bidding war coefficient estimates in Columns 1 to 4 of Panel C indicate that houses purchased in bidding wars have significantly lower annualized returns than houses purchased in non-bidding wars. The bidding war coefficient estimates in Columns 5 and 6 of Panel C indicate that homebuyers who purchase their house in a bidding war are not significantly more likely to default than homebuyers who do not purchase their house in a bidding war. On the surface, this finding conflicts with the results in Panels A and B. However, house prices were increasing during this subperiod, allowing distressed homeowners - who likely had positive equity - to sell their houses instead of defaulting.

Next, we create a bidding war concentration measure to further investigate whether the prediction generated in Hypothesis 4 is supported by empirical evidence. We measure bidding war concentration by calculating the fraction of transactions in a zip code that were purchased via a bidding war that year. We then assign each transaction to a bidding war concentration quartile where quartile 1 includes transactions in zip codes with the lowest levels of bidding war activity that year, and quartile 4 includes transactions in zip codes with the highest levels of bidding war activity that year. We modify Equations 19 and 20 to include the bidding war concentration quartiles and their interaction with the bidding war variable.

Table 8 examines the subsequent performance of bidding war transactions relative to non-bidding war transactions in zip codes with varying degrees of bidding war activity. Columns 1 and 2 examine the unlevered annualized returns and Columns 3 and 4 examine the levered annualized returns of bidding war transactions. In Column 1 of Table 8, the bidding war transaction and interaction coefficient estimates are all negative and statistically significant. This finding indicates that homebuyers who purchase their house in a bidding war experience lower unlevered annualized returns than homebuyers who do not purchase their house in a bidding war. We also find the magnitude of the winner's curse effect increases as the concentration of bidding war activity increases. The results are similar in Column 2, except that we only find evidence of lower unlevered annualized returns in zip codes with higher levels of bidding war activity (i.e., quartiles 2 to 4). The annualized levered return results in Columns 3 and 4 are consistent with the annualized unlevered return results in Column 2. Overall, these findings provide evidence consistent with Hypothesis 4 that the effects of the winner's curse are stronger in times and locations where bidding wars are more prevalent.

Columns 5 and 6 of Table 8 display bidding war coefficient estimates from Equation 20 examining the probability that a homebuyer in a bidding war transaction subsequently defaults relative to a non-bidding war transaction. The bidding war coefficient estimate is positive and statistically significant, indicating homebuyers who purchase their house via a bidding war are more likely to default on their mortgages than homebuyers who did not purchase their house in a bidding war. That said, the coefficients on the bidding war interaction coefficient estimates indicate the probability that a homeowner subsequently defaults initially increases (quartile 2) but then decreases (quartile 4) as the concentration of bidding war activity increases.

5.2 Listing Agent Experience

The prediction generated by Hypothesis 5 is that the winner's curse effects will be stronger if the house is listed by a more experienced real estate agent. We conjecture that more experienced agents - presumably with a broader network - will be able to attract more bidders and spark a bidding war.

[UPDATE THIS SECTION BASED ON RESULTS USING JUDGE IV RESEARCH DESIGN.]

6 The Winner's Curse and Housing Inequality

6.1 Neighborhood-level Demographics

Hypothesis 6 predicts a higher probability of a bidding war and stronger winner's curse effects for (a) minority, (b) lower income, (c) less educated, and (d) single-parent homebuyers. We initially test these predictions using neighborhood-level demographic data. Table 10 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 20, 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 10 show that the probability of a bidding war transaction increases with the median household income of the neighborhood and the fraction of Black, less educated (i.e., no college degree), and single-parent households in the neighborhood. These results provide evidence that is mostly consistent with Hypothesis 6, the only conflict being that the probability of a bidding war increases as the median household income of the neighborhood increases. We further explore this result using transaction-level household income from HMDA in the next section.

Table 11 examines how the subsequent performance of bidding war transactions varies with zip code-level demographics from the 2011 American Community Survey. Table 11 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 19, whereas the subsample analyses in Panel C (subsequent default) use Equation 20.

Panels A and B of Table 11 display (mostly) similar patterns for unlevered and levered returns across the zip code-level demographic quartiles. We find the magnitude of the lower average annualized returns for bidding war transactions decreases with income and education, but increases with the fraction of Black and single-parent 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 11, we find 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 and single-parent 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 Hypothesis 6 that the winner's curse effects we document are stronger for minority, lower-income, less educated, and single-parent homebuyers.

6.2 Transaction-level Demographics

The empirical results in the preceding section 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 Hypothesis 6 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.[11] Table 12 displays coefficient estimates from a linear probability model that regresses the bidding

[11] 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 in housing returns.

war indicator variable on either the race or household income of the homebuyer as well as the full set of housing characteristics, financing controls, and zip code-by-quarter-by-year fixed effects in Equation 20.

Column 1 of Table 12 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.[12] 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 12 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 Hypothesis 6, the coefficient estimates in Table 12 indicate that the probability of a bidding war is higher for low-income homebuyers and Black and Hispanic homebuyers. We do not observe whether the homebuyer has a college degree or is a single parent in the data at the transaction level, so we cannot directly test them here. That said, both education and single-parenting are likely correlated with household income.

6.3 Homebuyer Race

This section tests the prediction generated by Hypothesis 6 that the winner's curse effects will be stronger for Black and Hispanic homebuyers. We test these predictions by modifying Equations 19 and 20 to include indicator variables for the race of the homebuyer and its interaction with the bidding war indicator variable. Table 13 displays coefficient estimates for the four race and ethnicity buyer groupings relative to White buyers. Similar to Kermani and Wong (2021) 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.

[12]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.

non-bidding war transactions.

For non-bidding war transactions in Table 13, we find minority homebuyers earn lower unlevered and levered annualized returns than White homebuyers. In column 1 of Table 13, 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 (2021), 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 13.

For bidding war transactions in column 1 of Table 13, 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 buy their house in a bidding war experience 0.7pp (2.9pp) lower unlevered (levered) annualized returns than White buyers who bought their house in a non-bidding war transaction. 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 Hypothesis 6, 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.

6.4 Homebuyer Income

Next, we test the prediction generated by Hypothesis 6 that the winner's curse effects will be stronger for lower-income homebuyers. In Table 14, 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 14 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 **??**, 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 (3.4pp) lower annualized unlevered (levered) returns than buyers in income quartile 1 who bought their house in a non-bidding war transaction. 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 12 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.

7 Conclusion

This paper tests for a winner's curse in housing markets by comparing the subsequent performance of bidding war transactions to non-bidding war transactions. We develop a theoretical model that predicts that buyers who pay above the list price are more likely to overpay for their house, leading to lower price appreciation and higher mortgage default rates. Empirical evidence from over 14 million transactions confirms these predictions: bidding war winners experience significantly lower returns, both unlevered and levered, and are more likely to default on their mortgages. The effects are particularly pronounced during periods and in regions where bidding wars are more common. Alternative explanations, such as a strong personal preference for the house that justifies overpaying, are ruled out, as homebuyers who purchase their house in bidding wars tend to have shorter holding periods than those who purchase their house in non-bidding war transactions.

Additional analyses reveal that socioeconomically vulnerable homebuyers are disproportionately affected by the winner's curse. The disproportionate impact on socioeconomically vulnerable buyers raises concerns about equity and systemic inequality. Vulnerable buyers may lack the resources to navigate competitive markets, leading to financial distress and wealth erosion. Targeted assistance programs, such as down payment support, affordable housing initiatives, and pre-purchase counseling programs like those offered by the HUD, can help mitigate these effects. Additionally, addressing the root causes of bidding wars, such as limited housing supply, can alleviate demand pressures and create fairer, more sustainable housing markets.

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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
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
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
Log(Subsequent Sale Price)	12.4341	12.5114	12.4980
Cash Purchase	0.0553	0.0899	0.0839
Unknown Financing	0.2590	0.2999	0.2928
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
Log(Subsequent Sale Price)	12.4724	12.5506	12.5357
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

	Full Sample		Financed Subsample	
	(1)	(2)	(3)	(4)
Panel A: Unlevered Returns				
Bidding war	-0.013^{***}	-0.010^{***}	-0.008^{***}	-0.005^{***}
	(0.0004)	(0.0004)	(0.0002)	(0.0002)
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.264	0.230	0.475	0.342
Panel B: Levered Returns				
Bidding war	-0.069^{***}	-0.065^{***}	-0.031^{***}	-0.028^{***}
	(0.003)	(0.003)	(0.001)	(0.002)
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.176	0.193	0.240	0.232
Zip-BuyQY FE	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓
Subsequent Defaults	✓		✓	

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

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 19 examining the unlevered returns. Panel B displays coefficient estimates from Equation 19 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	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓

*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 20 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, financing controls, and zip-by-quarter-by-year fixed effects. 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	−0.015***	−0.013***	−0.013***	−0.013***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Observations	6,369,394	6,369,394	6,369,394	6,369,394
Adjusted R^2	0.264	0.264	0.264	0.264
Panel B: Levered Returns				
Bidding war	−0.074***	−0.061***	−0.066***	−0.063***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	6,369,394	6,369,394	6,369,394	6,369,394
Adjusted R^2	0.176	0.176	0.176	0.176
Panel C: Subsequent Default				
Bidding war	0.050***	0.051***	0.046***	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	0.234	0.234

*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. 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 in Panel C of Table 2. Panel A examines annualized unlevered returns, Panel B examines annualized levered returns, and Panel C examines subsequent default. Standard errors are clustered by zip code.

Table 6: Buyer Holding Period

	(1)	(2)	(3)	(4)
Bidding war	-59.623^{***}	-13.587^{***}	-53.072^{***}	-18.395^{***}
	(1.943)	(2.403)	(2.208)	(2.735)
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.165	0.192	0.168	0.198
House Characteristics	✓	✓	✓	✓
Financing Controls	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓
Subsequent Defaults	✓		✓	

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

Note: Table 6 examines the holding period of buyers in bidding war transactions relative to those in non-bidding war transactions. Each column displays coefficient estimates from a modified version of Equation 20, where the dependent variable is the buyers' holding period (expressed in days). 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 7: Subperiod Analyses

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Housing boom (2000-2007)						
Bidding war	-0.013^{***}	-0.008^{***}	-0.050^{***}	-0.046^{***}	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	0.442	0.363	0.281	0.300	0.138	0.212
Panel B: Housing bust (2008-2012)						
Bidding war	-0.005^{***}	-0.0002	-0.024^{***}	-0.005	0.010^{***}	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	0.496	0.553	0.591	0.650	0.057	0.106
Panel C: Housing recovery (2013-2018)						
Bidding war	-0.014^{***}	-0.014^{***}	-0.092^{***}	-0.091^{***}	-0.0005^{***}	-0.0002
	(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	0.337	0.338	0.302	0.303	0.008	0.028
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

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

Note: Table 7 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 19 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 19 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 20 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: Bidding War Concentration

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.006^{***}	-0.0005	-0.005	0.012	0.021^{***}	0.039^{***}
	(0.001)	(0.001)	(0.010)	(0.011)	(0.001)	(0.002)
Quartile 2	0.008	0.014	0.067	0.111	0.013	0.007
	(0.011)	(0.014)	(0.080)	(0.106)	(0.024)	(0.043)
Quartile 3	0.012	0.015	0.112	0.150	0.009	-0.002
	(0.012)	(0.015)	(0.086)	(0.112)	(0.027)	(0.049)
Quartile 4	-0.002	-0.001	0.021	0.027	0.00000	-0.015
	(0.015)	(0.018)	(0.108)	(0.138)	(0.028)	(0.051)
Bidding war × Quartile 2	-0.007^{***}	-0.007^{***}	-0.054^{***}	-0.059^{***}	0.004^{***}	0.007^{***}
	(0.001)	(0.001)	(0.011)	(0.013)	(0.001)	(0.002)
Bidding war × Quartile 3	-0.008^{***}	-0.010^{***}	-0.070^{***}	-0.080^{***}	0.001	-0.001
	(0.001)	(0.001)	(0.011)	(0.012)	(0.001)	(0.002)
Bidding war × Quartile 4	-0.009^{***}	-0.014^{***}	-0.081^{***}	-0.106^{***}	-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	0.264	0.230	0.176	0.193	0.179	0.251
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

*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 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 19 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 19 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 20 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: Listing Agent Instrument

	Unlevered Returns		Levered Returns		Subsequent Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Bidding war	-0.014^{***}		-0.071^{***}		0.037^{***}	
	(0.0004)		(0.003)		(0.001)	
Bidding war (fitted)		-0.029^{***}		-0.147^{***}		0.019^{***}
		(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	0.243	0.243	0.169	0.169	0.236	0.234
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Subsequent Defaults	✓	✓	✓	✓	✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

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

Note: Table 9 uses a subsample of listing agents with at least ten transactions in the data. 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 10: Probability of a Bidding War by Neighborhood Demographic Quartiles

	Fraction Black	Median Family Income	Fraction College Degree	Fraction Single Parents
	(1)	(2)	(3)	(4)
Quartile 2	-0.005^{***}	0.020^{***}	-0.003^{***}	0.005^{***}
	(0.00004)	(0.0001)	(0.0001)	(0.0001)
Quartile 3	0.028^{***}	0.030^{***}	-0.017^{***}	0.009^{***}
	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Quartile 4	0.025^{***}	0.020^{***}	-0.039^{***}	0.019^{***}
	(0.0001)	(0.0003)	(0.0001)	(0.0002)
Observations	13,681,109	13,681,109	13,681,109	13,681,109
Adjusted R^2	0.120	0.120	0.120	0.120
House Characteristics	✓	✓	✓	✓
Financing Controls	✓	✓	✓	✓
Zip-BuyQY FE	✓	✓	✓	✓
Subsequent Defaults	✓	✓	✓	✓

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

Note: Table 10 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 20, 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 11: 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	-0.009^{***}	-0.010^{***}	-0.013^{***}	$-0.022^{***,\dagger}$
Median Family Income	-0.026^{***}	-0.013^{***}	-0.009^{***}	$-0.005^{***,\dagger}$
Fraction College Degree	-0.022^{***}	-0.014^{***}	-0.009^{***}	$-0.007^{***,\dagger}$
Fraction Single Parent	-0.006^{***}	-0.009^{***}	-0.012^{***}	$-0.022^{***,\dagger}$
Panel B: Levered Annualized Returns				
Fraction Black	-0.045^{***}	-0.045^{***}	-0.065^{***}	$-0.114^{***,\dagger}$
Median Family Income	-0.140^{***}	-0.062^{***}	-0.044^{***}	$-0.024^{***,\dagger}$
Fraction College Degree	-0.118^{***}	-0.067^{***}	-0.042^{***}	$-0.032^{***,\dagger}$
Fraction Single Parent	-0.024^{***}	-0.047^{***}	-0.059^{***}	$-0.120^{***,\dagger}$
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	0.025^{***}	0.023^{***}	0.019^{***}	$0.008^{***,\dagger}$
Fraction Single Parent	0.011^{***}	0.020^{***}	0.022^{***}	$0.023^{***,\dagger}$

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

Note: Table 11 displays bidding war coefficient estimates across quartiles of various zip-level demographic characteristics from the 2011 American Community Survey. Panel A displays bidding war coefficient estimates from Equation 19 examining the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions. Panel B displays bidding war coefficient estimates from Equation 19 examining the annualized levered returns of bidding war transactions relative to non-bidding war transactions. Panel C displays bidding war coefficient estimates from Equation 20 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.

Table 12: Probability of Winning a Bidding War

Race	(1)	Income	(2)
AIAN	-0.005^{**}	Quartile 2	0.002^{***}
	(0.002)		(0.001)
API	-0.005^{***}	Quartile 3	-0.003^{***}
	(0.001)		(0.001)
Black	0.005^{***}	Quartile 4	-0.005^{***}
	(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	✓		✓
Financing Controls	✓		✓
Zip-BuyQY FE	✓		✓
Subsequent Defaults	✓		✓

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

Note: Table 12 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 20, 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 13: 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	0.592	0.436	0.316	0.294	0.219	0.298
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

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

Note: Table 13 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 19 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 20 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.

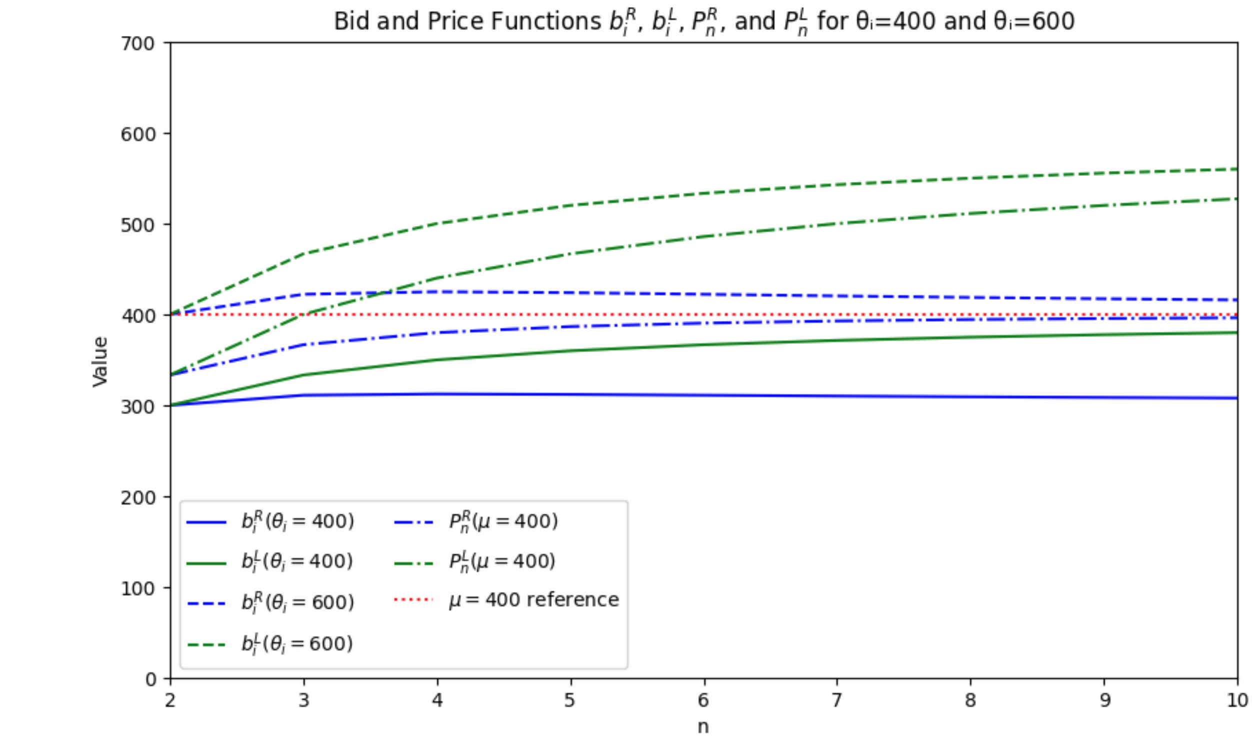
Table 14: Homebuyer Income

	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	0.592	0.437	0.316	0.294	0.218	0.295
Zip-BuyQY FE	✓	✓	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓		
Subsequent Defaults	✓		✓		✓	✓
House Characteristics					✓	✓
Financing Controls					✓	✓

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

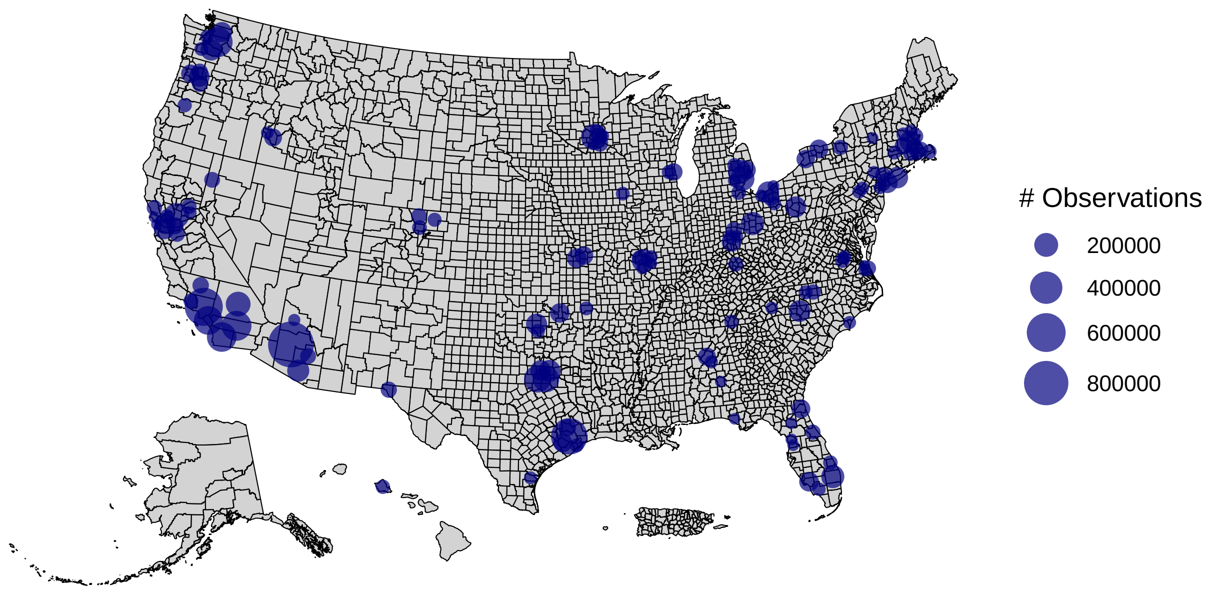
Note: Table 14 examines the subsequent performance of bidding war transactions by income quartile relative to lower-income buyers in quartile 1 using a subsample of transactions in Table 2 where we can identify the gender of the homebuyer's demographic and financial information in HMDA. Columns 1 to 4 display coefficient estimates from Equation 19 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 20 examining the probability that a bidding war transaction subsequently defaults relative to non-bidding war transactions. Standard errors are clustered by zip code.

Figure 1: The Comparison of The Two Equilibria



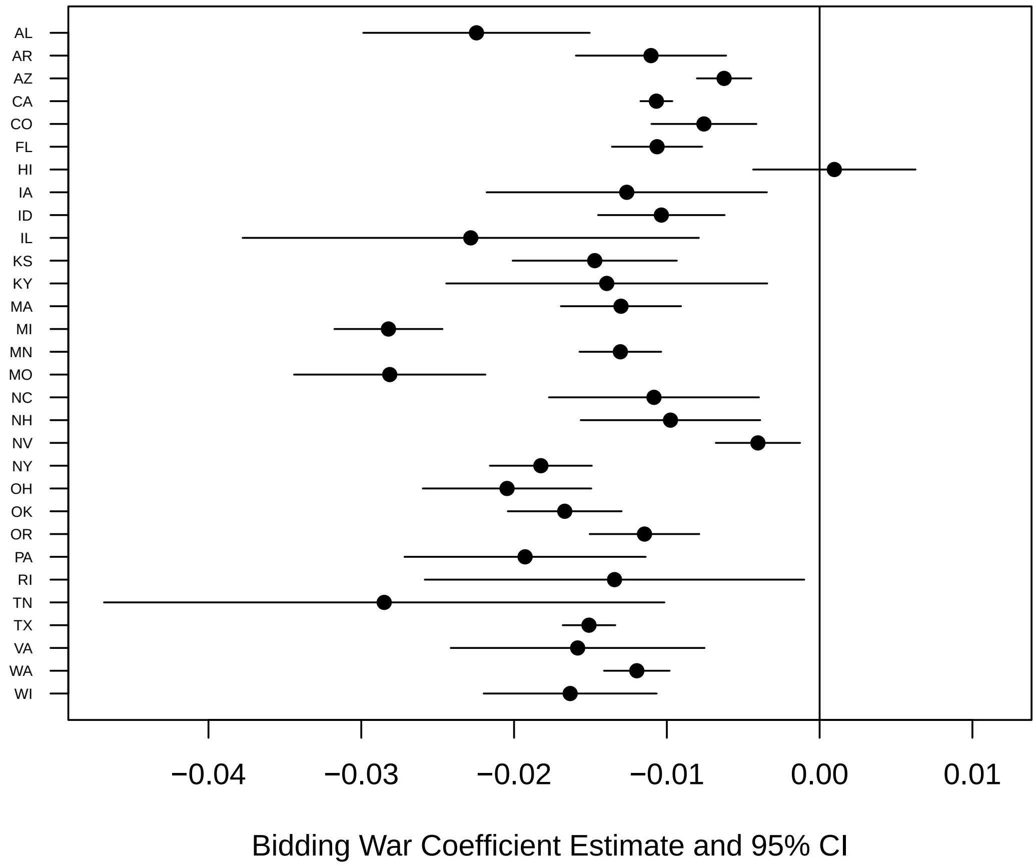
Note: Figure 1 plots the optimal bidding strategy and expected transaction prices under the rational (blue) and limited rationality (green) equilibria. In this example, we assume the home value (μ) is 400. The solid (dotted) line represents the two equilibria's bidding strategies when the bidder receives a signal $\theta_i = 400$ ($\theta_i = 600$). The expected selling prices are demonstrated by the dot-dashed lines.

Figure 2: Observations by County



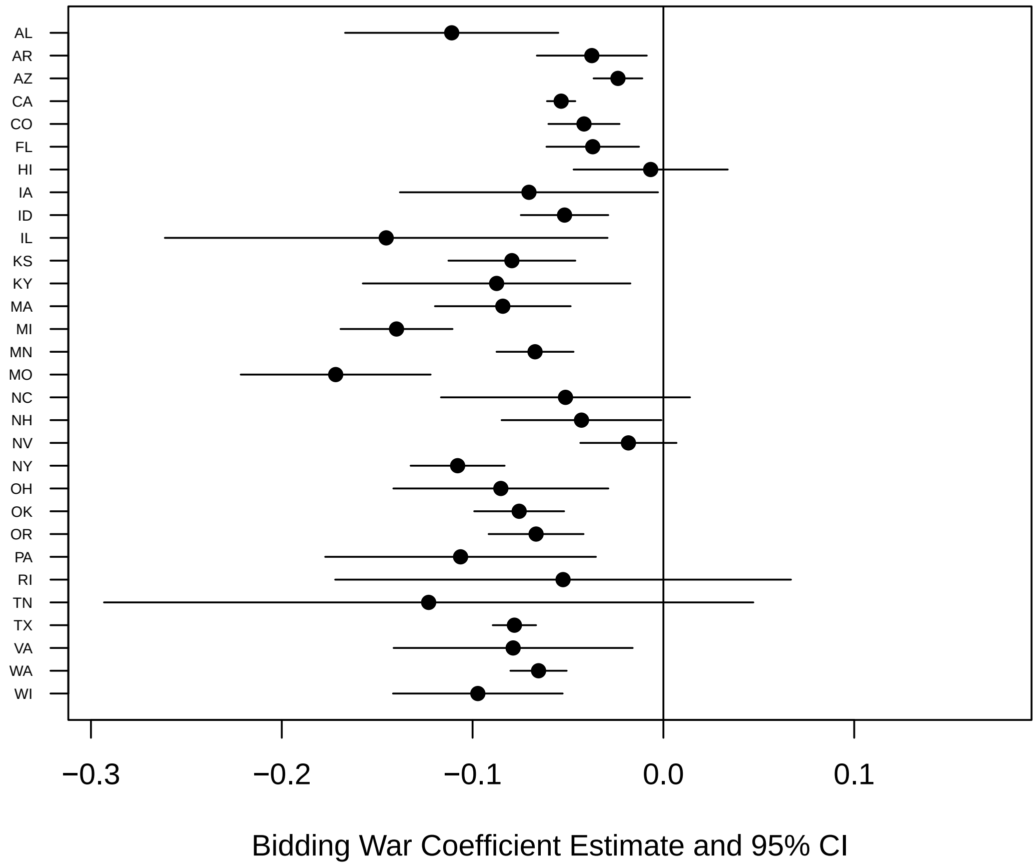
Note: Figure 2 displays the geographic coverage of the filtered housing transaction data used in the empirical analysis. The figure plots the number of observations at the county level.

Figure 3: Unlevered Returns by State



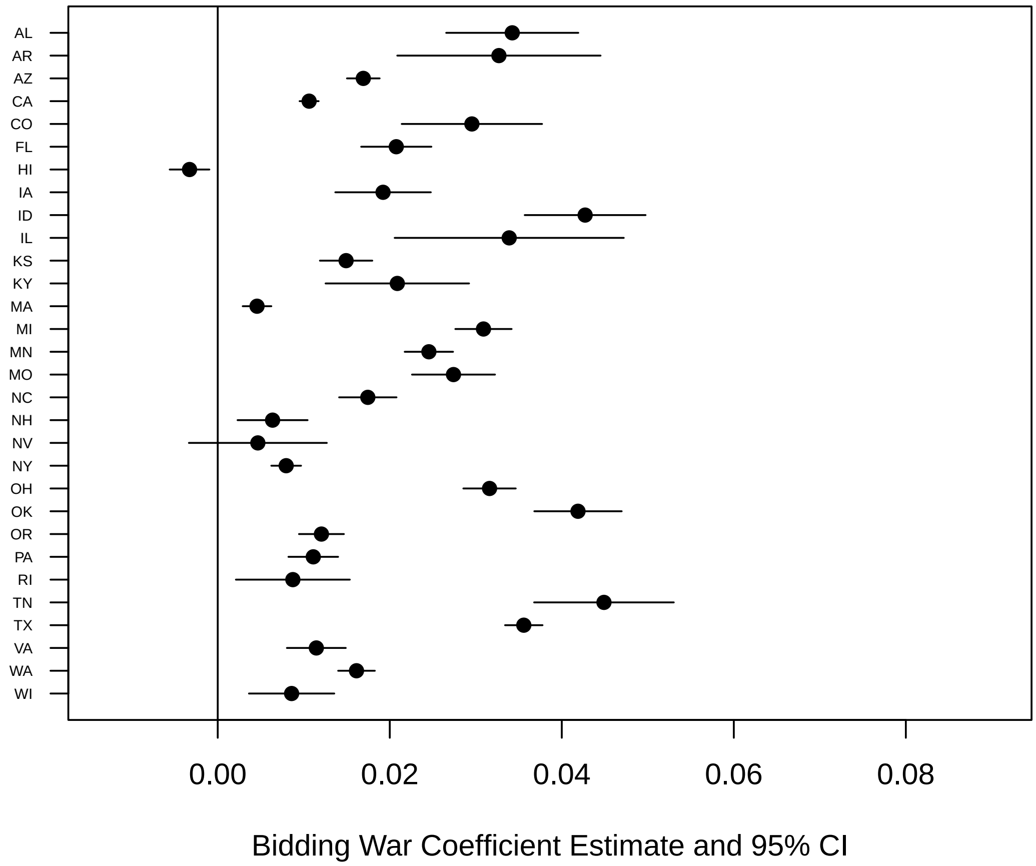
Note: Figure 3 examines the annualized unlevered returns of bidding war transactions relative to non-bidding war transactions at the state level. The figure plots the state-level bidding war coefficient estimates and 95% confidence intervals from Equation 19 using the returns sample in Panel C of Table 2.

Figure 4: Levered Returns by State



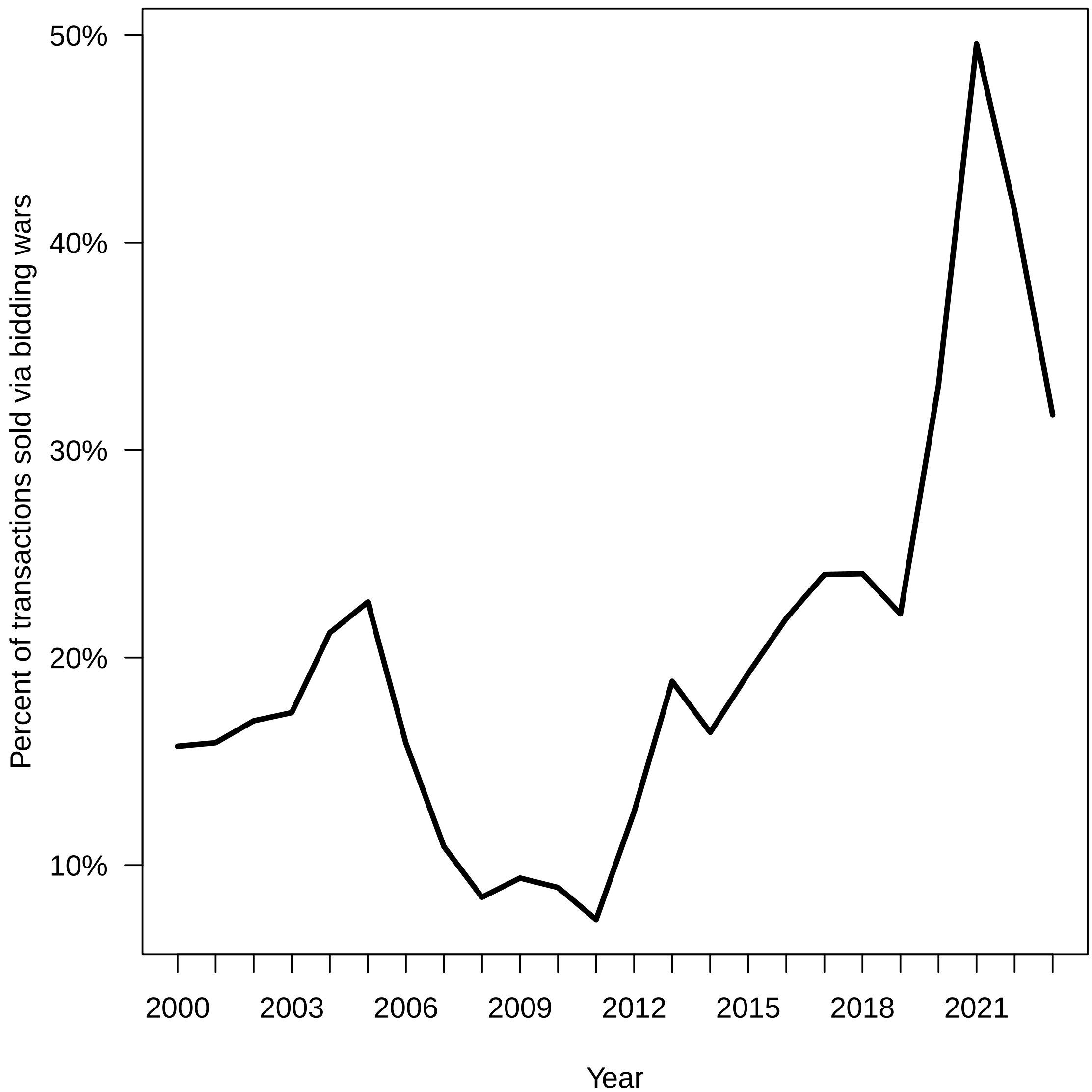
Note: Figure 4 examines the annualized levered returns of bidding war transactions relative to non-bidding war transactions at the state level. The figure plots the state-level bidding war coefficient estimates and 95% confidence intervals from Equation 19 using the returns sample in Panel C of Table 2.

Figure 5: Subsequent Default by State



Note: Figure 5 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 20 using the subsequent default sample in Panel A of Table 2.

Figure 6: Bidding War Share



Note: Figure 6 plots the percent of transactions that sold via bidding wars each year from January 2000 through June 2023.

Appendices

Contents

A Proofs

A.1 Proof of Proposition 1

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

$$b_i = \underline{\theta} + a(\theta_i - \underline{\theta}), \tag{21}$$

For convenience, we define

$$\phi_i \equiv \frac{\theta_i - \underline{\theta}}{\bar{\theta} - \underline{\theta}}, \tag{22}$$

$$\beta_i \equiv \frac{b_i - \underline{\theta}}{\bar{\theta} - \underline{\theta}}. \tag{23}$$

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

$$\beta_i = \alpha\phi_i. \tag{24}$$

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

$$\chi \equiv \frac{x - \underline{\theta}}{\bar{\theta} - \underline{\theta}}. \tag{25}$$

Probability j wins the auction is given by

$$\Pr(x > b_{-i}) = \Pr(\chi > \beta_{-i}) = \Pr(\chi > \alpha\phi_{-i}) = \Pr\left(\phi_{-i} < \frac{\chi}{\alpha}\right) = \left(\frac{\chi}{\alpha}\right)^{n-1}. \tag{26}$$

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

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

The optimal bid χ must satisfy the first order condition:

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

which can be simplified as follows

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

Thus, the optimal bid take the following form.

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

Given our conjecture (24), the equilibrium requires that

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

Thus,

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

which is equivalent to (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,

$$P_n^R = \underline{\theta} + (\bar{\theta} - \underline{\theta})\alpha E\left[\phi_i \left|\phi_i > \phi_{-i}\right.\right]. \tag{27}$$

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

$$E\left[\phi_i \left|\phi_i > \phi_{-i}\right.\right] = \frac{n}{n+1}. \tag{28}$$

Thus,

$$\begin{aligned} P_n^R &= \underline{\theta} + (\bar{\theta} - \underline{\theta})\alpha\frac{n}{n+1} \\ &= \underline{\theta} + (\bar{\theta} - \underline{\theta})\frac{n^2+n-2}{2n^2}\frac{n}{n+1} \\ &= \frac{\bar{\theta} + \underline{\theta}}{2} - \frac{\bar{\theta} + \underline{\theta}}{2} + \underline{\theta} + (\bar{\theta} - \underline{\theta})\frac{n^2+n-2}{2n(n+1)} \\ &= \mu - \frac{\bar{\theta} - \underline{\theta}}{2} + (\bar{\theta} - \underline{\theta})\frac{n^2+n-2}{2n(n+1)} \\ &= \mu - \frac{(\bar{\theta} - \underline{\theta})}{n(n+1)}. \end{aligned} \tag{29}$$

A.2 Proof of Proposition 2

The proof of Proposition 2 is similar to that of Proposition 1, with equations (21) - (26) applying to both proofs. However, in the setting with limited rationality, bidder j, who bids x, believes that his payoff is going to be $\theta_i - x$, if he wins the auction.

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

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

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 (27)-(29), the expected price P_n^L can be written as follows

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

A.3 Proof of Proposition 3

The proof follows directly from equations (12) and (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 (12) and (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 (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

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

B Data Overview

B.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.

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

B.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 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: winsorize top and bottom 1% of sale-to-list price ratio[13]
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 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[14]
13. filter 13: sale year $\leq$ 2018

[13]We winsorize 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.

[14]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.

Table 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 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 (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 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 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.

B.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 CoreLogic 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 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.

B.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.

C Additional Results

C.1 Financing Controls [[TEMP]]

In the body of the paper, we do not include financing controls when examining the annualized returns for bidding war transactions relative to non-bidding war transactions. We do not include financing controls when examining unlevered returns because unlevered returns, by definition, do not consider the financing used in the transaction. We do not include the financing controls in the levered results because (i) they are unavailable in some instances, and (ii) we follow the approach in Goldsmith-Pinkham and Shue (2023). Here, we modify Equation 19 to include the financing controls to check whether their inclusion materially impacts our results. Although we include the controls in the annualized levered returns regressions, we continue computing the hypothetical levered returns based on the most common mortgage type in the data: a 30-year fixed rate loan with an initial loan-to-value (LTV) ratio of 80%. In other words, our approach is the same except for including the financing control variables.

Table C1 presents the annualized unlevered and levered returns of bidding war transactions relative to non-bidding war transactions with the financing controls variables included in the regressions. Panel A displays coefficient estimates from Equation 19 examining the unlevered returns. Panel B displays coefficient estimates from Equation 19 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. The results in Table C1 show that including the additional financing controls does not materially impact the main results in our paper.

Table C1: Annualized Returns with Financing Controls

	Full Sample		Financed Subsample	
	(1)	(2)	(3)	(4)
Panel A: Unlevered Returns				
Bidding war	-0.012^{***}	-0.008^{***}	-0.007^{***}	-0.005^{***}
	(0.0003)	(0.0004)	(0.0002)	(0.0002)
Holding period	-0.006^{***}	-0.011^{***}	0.001^{*}	-0.004^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Cash purchase	0.056^{***}	0.057^{***}		
	(0.002)	(0.002)		
$0 < \text{CLTV} \leq 60$	-0.003^{***}	-0.007^{***}	-0.008^{***}	-0.012^{***}
	(0.0004)	(0.0005)	(0.0003)	(0.0003)
$60 < \text{CLTV} < 80$	-0.001^{***}	-0.003^{***}	-0.004^{***}	-0.006^{***}
	(0.0003)	(0.0004)	(0.0003)	(0.0003)
$80 < \text{CLTV} \leq 90$	-0.004^{***}	-0.003^{***}	-0.004^{***}	-0.003^{***}
	(0.0004)	(0.0005)	(0.0003)	(0.0004)
$90 < \text{CLTV} \leq 95$	-0.005^{***}	-0.004^{***}	-0.005^{***}	-0.003^{***}
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
$95 < \text{CLTV} \leq 100$	-0.015^{***}	-0.010^{***}	-0.010^{***}	-0.005^{***}
	(0.001)	(0.001)	(0.0004)	(0.0004)
$\text{CLTV} > 100$	-0.009^{***}	0.003^{***}	-0.007^{***}	0.005^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
CLTV is NA	0.031^{***}	0.036^{***}		
	(0.001)	(0.001)		
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.270	0.236	0.475	0.343
Zip-BuyQY FE	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓
Subsequent Defaults	✓		✓	

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

Table C1: Annualized returns with financing controls (cont.)

	Full Sample		Financed Subsample	
	(1)	(2)	(3)	(4)
Panel B: Levered Returns				
Bidding war	-0.050^{***}	-0.048^{***}	-0.029^{***}	-0.029^{***}
	(0.003)	(0.003)	(0.001)	(0.002)
Holding period	-0.076^{***}	-0.101^{***}	-0.028^{***}	-0.049^{***}
	(0.009)	(0.011)	(0.005)	(0.006)
Cash purchase	0.367^{***}	0.381^{***}		
	(0.012)	(0.013)		
$0 < \text{CLTV} \leq 60$	-0.012^{***}	-0.023^{***}	-0.045^{***}	-0.058^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
$60 < \text{CLTV} < 80$	-0.003	-0.007^{**}	-0.024^{***}	-0.031^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
$80 < \text{CLTV} \leq 90$	-0.017^{***}	-0.017^{***}	-0.017^{***}	-0.016^{***}
	(0.003)	(0.004)	(0.002)	(0.003)
$90 < \text{CLTV} \leq 95$	-0.028^{***}	-0.026^{***}	-0.024^{***}	-0.018^{***}
	(0.003)	(0.003)	(0.002)	(0.002)
$95 < \text{CLTV} \leq 100$	-0.092^{***}	-0.081^{***}	-0.057^{***}	-0.044^{***}
	(0.004)	(0.004)	(0.002)	(0.003)
$\text{CLTV} > 100$	-0.021^{***}	0.015^{**}	-0.010^{***}	0.036^{***}
	(0.006)	(0.007)	(0.003)	(0.005)
CLTV is NA	0.226^{***}	0.254^{***}		
	(0.009)	(0.010)		
Observations	6,369,394	5,204,348	3,970,369	3,185,005
Adjusted R^2	0.179	0.195	0.241	0.232
Zip-BuyQY FE	✓	✓	✓	✓
Zip-SaleQY FE	✓	✓	✓	✓
Subsequent Defaults	✓		✓	

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