Anchoring on Historical Reference Points: How and When Round-Number Price Thresholds Distort the Relationship between Current and Previous Home Prices

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ABSTRACT

Buyers often strive to negotiate low prices for durable goods, such as vehicles, homes, appliances, or art. Psychologically-salient round number reference points (*e.g.,* $10,000) influence these purchasing decisions. However, existing research does not capture how these round-number reference points from the past influence the anchoring effect of previous sales prices on future resale valuations. We argue that the anchoring effect of prior sales prices on subsequent prices of durable goods is discontinuous at round numbers, such that it matters disproportionately whether a previous sales price reached a round-number threshold. Buyers paying a price just *below* a round number may sacrifice money because they receive disproportionately less when reselling the good. We use data on over 13,000 repeat residential real estate transactions and an approach similar to a regression discontinuity design to find that real estate buyers who previously paid an amount just under a $10,000 reference point subsequently list and sell their homes for about 1.3 percent (over $2000) less on average than do buyers selling comparable homes who previously paid at or above this threshold. This drop is in addition to the expected price based on home characteristics and the general relationship between previous and current sales prices. A laboratory experiment with 1010 participants increases confidence in causality. We also find that market mechanisms and the negotiation process do not correct for these discontinuities: Lower *initial listing* prices carry through to *final sales* prices. However, we find strong evidence suggesting that using a highly-experienced agent attenuates these effects.

Keywords: Anchoring, Real Estate, Reference Points, Experience, Negotiations

1. INTRODUCTION

People buying durable goods such as real estate typically aim to pay prices that fall below psychologically significant maximum prices. These amounts are often multiples of "round" numbers, such as $10,000, which serve as cognitively-accessible reference points against which people subjectively judge the quality of their outcomes (e.g., Baillon et al. 2020, Wallace and Etkin 2018). Knowing this, sellers may attempt to attract buyer interest by engaging in psychological "charm-pricing"—listing properties at prices like $399,000 rather than at round prices like $400,000 (Allen & Dare 2004, Basu 1997, Cardella and Seiler 2016, Gendall et al. 1997). This pricing strategy, which relies on people paying heightened attention to the left digits of price and neglecting digits to the right (e.g., Englmaier et al. 2018, Lacetera, Pope, and Sydnor 2012, Sokolova et al. 2020, Thomas and Morwitz 2005), can be so effective that the mere presence of the digit 9 in an ending position of a list price can raise demand (Anderson and Simester 2003) and even final sales prices (Repetto and Solís 2019).

This paper examines whether this outsized influence of round numbers on people's judgment and behaviors (e.g., Allen et al. 2017, Markle et al. 2018) may work in concert with the anchoring-and-adjustment heuristic (e.g., Galinsky & Mussweiler 2001, Jung et al. 2016, Mason et al. 2013, Rader et al. 2015, Tversky and Kahneman 1974) to affect negotiators' behaviors and outcomes in subsequent sales of the same durable good. We argue that the anchoring effect of prior sales prices on subsequent prices of durable goods is discontinuous at round numbers, such that it matters disproportionately whether a previous sales price reached a round number. Buyers who pay amounts that fall just under round numbers may consequently create disproportionately large sacrifices in future resale prices, such that buyers who eventually resell their goods might be better off paying *more* when doing so elevates the initial sales price to or just above a round number. This bias remains uncorrected by the market or negotiation process and creates economically meaningful market inefficiencies. Finally, we argue that market experience, which exposes the individual to many potential anchors, will mitigate this bias, in the same way that experience can attenuate price precision effects (e.g., Loschelder et al. 2016).

We test our arguments in the residential housing market, examining specifically whether home sellers are differentially anchored by the price they previously paid for their property depending upon whether the previous price fell short of or met a round number reference-point (i.e. a number evenly divisible by $10,000). We use an approach similar to a regression-discontinuity design and draw on archival data from repeat home sales in Utah between 1996 and 2014 to identify large discontinuous increases in list prices at $10,000 thresholds in prior sales prices. The magnitude of the average discontinuity is large, and ranges from $2050 to $3586 in our main results. Market mechanisms do not fully correct this price anchoring—discontinuities are observable in initial list price as well as in final negotiated sales prices. However, we find that agent experience attenuates the pricing bias. The list and final sales prices of listings with more experienced agents reflect substantially smaller anchoring to thresholds in prior sales prices compared to listings with less experienced agents.

We support this archival field analysis with a pre-registered experiment involving 1,010 participants from Amazon's MTurk.com. We use a cross-nested experimental design in which participants each predict sales prices for five houses, and find that randomly assigned prior sales prices more strongly influence anticipated listing prices when they span a $10,000 threshold than when they do not. This provides causal support to our argument of intergenerational reference point anchoring in durable goods pricing.

Our work contributes to theory in a few important ways. First, we add to previous work on the influence of round number reference points (e.g., Dai, Milkman, Riis 2014, Englmaier et al. 2018, Lacetera, Pope, and Sydnor 2012) to show how round number reference points from the past disproportionately influence future anchoring and pricing in negotiations. Second, we show that the combination of the anchoring-and-adjustment heuristic and the disproportionate influence of reference points on judgment can lead negotiators to make suboptimal decisions. This adds to cognitive negotiation theory (Neale and Bazerman 1991) by demonstrating how motivations to achieve goals within negotiations can affect outcomes in subsequent negotiations. Third, we show the limits of the market and negotiation process in correcting for pricing biases from the anchoring and adjustment heuristic across transactions. This highlights a challenge for professional service firms whose role is to act as an intermediary in valuing and transacting durable goods (e.g., Beggs and Graddy 2009). Finally, we contribute to the literatures in human capital and experience by showing the role experience plays in avoiding biases. This highlights an underexplored but important benefit of human capital in knowledge-intensive industries.

The findings of this paper also have important implications for decision-making and management. They suggest that cognitive heuristics and biases can influence financially important decisions in ways that persist across time and are not corrected by the negotiation process or the market. Sellers, buyers, and intermediaries may not consider the impact that selling or buying just below a reference point might exert on future subsequent valuations. However, given the prevalence of psychological charm pricing (e.g., Basu 1997) buyers often buy durable goods just below round number price thresholds. This work exposes the financial cost and benefit of falling short or exceeding such thresholds on future valuations. For managers, our findings suggest that agent experience is an important resource that can be used to reduce the effect of heuristics and biases in transactions. Organizations may capitalize on this knowledge embedded within experienced employees by instituting mentorship or other training programs. Such programs may allow managers to scale the debiasing effects of human capital or use them strategically to improve performance.

2. CONTEXT PREVIEW: RESIDENTIAL REAL ESTATE

The setting for this paper is residential real estate. The vast majority of homes listed for sale in the United States are entered by listing agents into area-specific multiple listing service (MLS) databases. The MLS records detailed data on home and transaction characteristics, home prices including original and final list prices and final sales price, concessions given, showing instructions, time on market, agent and brokerage identities, and public comments regarding the unique features and condition of homes. Full access to the MLS is limited to licensed real estate agents and brokers in a given area. Basic home information is often also available to consumers via the MLS and third-party websites. While consumers have access to information through these websites, final sales prices and some historic data are not always public information, and consequently not accessible through government records. MLS databases are the primary databases used by agents to access historic and current home sales data, which are used in valuing homes based on nearby comparable homes.

Agents function as expert intermediaries in real estate transactions, and differences in their motivation (e.g., Rutherford et. al. 2005, Levitt and Syverson 2008, Gubler, 2019) and human capital (e.g., Gubler 2019, Gubler and Cooper 2019) predict their ability to capture value for their clients. Listing agents play an important role in determining original list prices and advise sellers regarding potential price changes depending on buyer reactions. Listing agents also play a key negotiating role, as they seek to close the sale at the highest possible price. Buyer agents play a critical role in determining offer prices. They likewise play a key negotiating role, as they seek to close the sale at the lowest possible price. While agents should act with fiduciary responsibility to their respective client, previous work has found this does not always happen (e.g., Levitt and Syverson, 2008). Moreover, agents have been found to be affected by anchoring bias (Northcraft and Neale 1987), and the bias is strong enough that overpricing properties by setting list prices that exceed likely sales prices seems to be an effective strategy (Bokhari and Geltner 2011, Bucchianeri and Minson 2013).

Home sellers have limited information and experience in determining listing prices and consequently rely on the expertise of listing agents. After setting the original list price home sellers and listing agents may adjust the price upward or downward, depending on consumer interest in the home. The final sales price reflects the agreed-on final sales price, although additional concessions can be made through closing costs to homebuyers by sellers, which are not reflected in the final sales price.

3. THEORY

3.1 The Anchoring-and-Adjustment Heuristic

Research on the anchoring-and-adjustment heuristic has shown that first offers and other anchors can significantly influence negotiation outcomes and final prices paid (see Orr and Gurthrie 2005 and Furnham and Boo 2011 for reviews). Previous studies in real estate have likewise established a relationship between prior and future sales prices for properties, both due to the underlying common value of the property and due to the anchoring-and-adjustment heuristic (e.g., Baucelles et al. 2011, Bucchianeri and Minson 2013, Genesove and Mayer 2001, Haurin et al. 2010, Kristensen, and Gärling 2000). While previous work on anchoring has largely assumed a linear relationship between previous price paid and subsequent price,[1] we argue that this relationship is not linear because round numbers serve as reference points that play an outsized role in the decision-making of buyers, sellers, and their agents.

3.2 Role of Round Numbers in Decision Making

Round numbers affect the decisions people make (e.g., Dai et al. 2014). Marathon runners strive to post times that fall just under them (Allen et al. 2016, Markle et al. 2018); used car values plummet when odometers hit multiples of 10,000 (Englmaier et al. 2018, Lacetera, Pope, and Sydnor 2012); and people retake standardized tests when their scores fall just below round numbers (Pope and Simonsohn 2011). Round numbers exert this influence and create discontinuous valuations of outcomes because people use them as reference points to simplify decision-making (Kahneman 1992).

The discontinuities created by round numbers can stem from people paying limited attention to non-leftmost digits. For example, car valuations are influenced disproportionately more by 10,000 odometer changes or by car production year, instead of by smaller odometer changes or car production dates within a production year (e.g., DellaVigna 2009, Lacetera et al. 2012). This attention to left-side digits largely explains why properties are often listed at prices like $199,000 instead of $200,000 (e.g., Allen and Dare 2004).

3.3 Influence of Reference Points on Intergenerational Anchoring

Because cognitive reference points can be discontinuous in the valuation of outcomes, the perceived change in value in going from below a reference point to that reference point or above can be much greater than what other similar numerical changes produce (Heath et al. 1999). People may therefore view properties that previously sold at or just above a reference point to be qualitatively more valuable than properties that sold just below the round number. Because people are susceptible to the heuristic of paying attention to leftmost digits, a house that *previously* sold for $260,000 may be seen as more valuable than a house that

[1] The exception is research on loss aversion showing that people are hesitant to sell properties for less than they originally paid for those properties (Diekmann et al., 1996; Einiö et al., 2008; Graddy et al. 2014).

previously sold for $259,000. However, smaller differences in value may be perceived between a house that previously sold for $260,000 and one that previously sold for $261,000.

Real estate sellers and their agents may reflect these discontinuities in the original listing prices for relisted homes, even though they may also be influenced by other potential anchors like the prices of recently sold comparable homes and algorithm-generated estimates (e.g., Zillow's Zestimate). If so, a $1,000 gap between two properties' previous sales prices should predict a larger difference in the two properties' subsequent listing prices when that $1,000 gap changes the ten-thousands digit of the property than when it does not. We expect that the relationship between previous sales price and subsequent list price is discontinuous at round numbers (i.e., multiples of $10,000). Specifically, we expect that the subsequent sales price differences between properties with previous sales prices that straddle round numbers exceed, ceteris paribus, the sales price differences between properties with previous sales prices not straddling round numbers.

3.4 Do Market Forces and the Negotiation Process Reduce Round Number Discontinuities?

Round number discontinuities in the relationship between previous sales prices and subsequent listing prices may carry through to affect final resell prices, as first offers robustly anchor final prices in many contexts (Gunia et al. 2013). While the "marvel of the market" rests in its ability to aggregate and communicate information among dispersed actors and to set prices efficiently (Hayek 1945: 526), we do not expect market forces and the negotiation process, in which negotiators search out and use information to persuade their counterparts to get more favorable terms, to correct for discontinuities in previous sales prices in our setting for a few reasons. First, the heterogeneity in houses, market cycles, and geographic areas can make it difficult for housing sellers and buyers to know which information is relevant for their properties. Second, while agents have access to pricing information during the time period we study, price data were not always readily available to consumers. Third, a lack of consumer expertise and experience makes it difficult for consumers to evaluate accessible information (Dulleck and Kerschbamer 2006, Gubler 2019, Gubler and Cooper 2019, Teece 2003). Finally, consumers and agents may be unaware of the bias and its influence on home pricing. We therefore expect pricing discontinuities at round numbers for real estate listing prices to manifest in final sales prices.

This prediction may appear inconsistent with research showing that properties actually sell for *more* when their list prices are just below round number reference points (Repetto and Solís 2019). However, such listing prices have this effect because they attract more potential buyers. No such process should occur with previous sale prices because buyers rarely screen on previous sales prices.

3.5 Does Agent Experience Reduce Discontinuities around Round Numbers?

Real estate agents and experts in other fields are not immune to bias from the anchoring and adjustment heuristic (Beggs and Graddy 2009, Northcraft and Neale 1987, Orr and Guthrie 2006). They may engage in systematic processing about decisions within their field of expertise and still be affected by the anchoring-and-adjustment heuristic (e.g., Chen and Chaiken 2009, Englich 2005, Englich et al., 2006). Anchoring is strong enough that overpricing real estate properties by setting list prices that exceed likely sales prices can generate higher sales prices (Bokhari and Geltner, 2011, Bucchianeri and Minson 2013). Agents may consequently not protect clients against discontinuities created by anchoring on previous sale prices. We argue, however, that the experience level of agents may influence the strength of the bias. We reason that experienced agents' exposure to many properties and anchors could diminish the influence of any one particular anchor on judgments of value. Experienced agents may therefore be less affected by any particular anchor (such as that from round-number thresholds in previous sales prices) even if their processing is as heuristic as that of inexperienced agents. Moreover, experienced agents' heightened negotiation experience may mitigate the anchoring effect (cf. Cardella and Seiler, 2016). We therefore expect agent experience to moderate pricing discontinuities, such that they are smaller for agents with high experience than for agents with low experience.

4. ARCHIVAL STUDY

4.1 Data and Restrictions

We first use archival field data from repeat home sales to identify whether and to what extent real estate listing prices increase discontinuously at $10,000 thresholds in prior sales prices. We then examine whether market mechanisms, the negotiation process, or agent experience correct for discontinuities. Our archival data are drawn from a major county in Utah for 1996-2014. The dataset contains detailed information on home and transaction characteristics, home listing prices including original and final listing prices, final sales prices, closing concessions given, showing instructions, time on market, agent and brokerage identities, and public comments regarding the unique features and condition of homes. We used agent identifiers to measure agent experience levels.

Our data for this study include MLS-listed homes in Utah County that were sold between 1996 and early 2014 that relisted again for sale. We drop from our sample the top one-half percent of homes by number of relistings, as these homes appear to be matched erroneously (i.e., sold multiple times yearly for multiple years). We impose additional sample restrictions in order to reduce noise in our data that would hurt estimate precision. First, because of data demands we limit our sample to price thresholds for which we have significant data support. Similar to Lacetera et al. (2012), we restrict our analysis to the home price range (i.e., $90,000 and $260,000, which is 82% of all property sales) containing the majority of home sales in our geographic location. The choice of these cutoffs is inevitably ad hoc, so we present the consistency of our model with different cutoffs in robustness tests. Second, we drop the top and bottom one percent of homes by price appreciation (454 homes), which likely reflects unobservable significant events, such as fire, flood, mold, murders, suicides, significant renovations, or large location-specific changes to the desirability of an area or home. Finally, we omit homes that are either short sales or bank owned (3,105 homes), as these homes undergo a different sales process, including auctions, and thus receive extra scrutiny on listing and final sales prices.

4.2 Identification Strategy and Model

The residential real estate setting allows us to observe home transactions across time and to control for house and transaction characteristics in each listing that might influence price. This permits controlled estimation of price discontinuities at multiple round number (i.e., $10,000) price thresholds. These thresholds are quasi-exogenous to home characteristics in the small price windows examined. It also allows us to test whether the negotiation process, market forces, or agent experience correct for bias from the anchoring and adjustment heuristic.

We empirically model price anchoring in home sales using the approach implemented in Lacetera et al. (2012), which showed how discrete 10,000-mile thresholds on used car odometers influenced auction sale prices. The model follows the basic logic of a regression discontinuity model and has been used in prior work testing for discontinuities at round number thresholds (e.g., Lee and Lemieux 2010, Pope et al. 2015 & Englmaier et al 2017).

Our model estimates whether gaps in subsequent listing prices are larger for homes that previously fell just above or just below a ten-thousands digit threshold (e.g. \$359,000 vs \$361,000) than they are for homes that did not (e.g. \$357,000 vs \$359,000). In other words, we model how discrete \$10,000 thresholds in the *prior* home sales price predict increases in the *subsequent* home sales price while controlling for all observable home and market characteristics in both periods, as well as the underlying continuous empirical relationship between prior and subsequent sales prices.

We model the price of a home listing as the underlying hedonic value of the home based on its observable characteristics (e.g., bedrooms, bathrooms, square footage, age) and market factors such as year and month of sale as well as location (zip code). We control for unobservable factors that might influence the previous/current price relationship by including a fifth-order polynomial based on previous home sale prices, as well as 81 dummy variables created from text analysis of each home's posted public comments that highlight unique aspects of the home. Finally, we control for observable upgrades since the prior sale. Our OLS model is:

$$price_{it} = f(price_{it-1}) + \sum_{k=10}^{25} \beta_k D[price_{it-1} \geq k * (10{,}000)] + \gamma X_{it} + \delta X_{it-1} + \eta T_t + \varepsilon_{it}$$

where $price_{it}$ is the price from the current sale of home *i* at time *t*. In our models, we examine three primary prices as dependent variables: the original list price set by the seller and listing agent, the eventual sales price negotiated in the market, and the final sales price less concessions (e.g., money given towards closing costs), which we label as the *net price*. These three price measures not only provide robustness for our findings, but also inform whether a potentially biased listing price initiated by the listing agent is fully corrected by market forces or the negotiation process.

The function $f(price_{it-1})$ is a flexible fifth-order polynomial function of the publicly-available prior sales price that captures the underlying smooth relationship between prior and current home prices.[2] The Ds represent indicator variables for the previous sale price being above a $10,000 threshold, such that the β_k coefficients estimate separate discontinuities at each threshold. Our key statistical test is if these β_k coefficients are jointly statistically different from zero. If so, we should observe a significant average discontinuity across these thresholds.

The vector X_{it} represents observable characteristics of the current home in time t, while X_{it-1} controls for any changes in these characteristics since the previous sale. The vector T_t controls for all observable time trends, including dummies for year and month of the current sale as well as the logged number of days between sales. All models conservatively estimate robust standard errors clustered by real estate agency to account for commonalities among real estate brokerages.

Table 1 provides summary statistics for the main variables in our sample, which includes 16,111 unique home relistings. Of these, 13,673 (85%) were sold. Figure 1 presents the distribution of prior sales prices for all listings in our data. There is obvious non-random assignment of homes around $10,000 thresholds, which is expected given charm pricing and the propensity for price rounding with large numbers. Since our empirical model is designed to compare similar homes around small price windows, such as $189,000 and $190,000, we include house, transaction, and renovation controls in our analyses (see appendix Table A1 for the list of control variables) that help account for differences between homes, including since the last sale. Such controls are warranted in our analysis, as there appear to be small differences on observables between homes around each threshold, as shown in our balance tests in Table A2 (appendix). Identifying round-number anchoring in our model relies on the assumption that any

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[2] Our results are robust to 7th order polynomials as well (see appendix Table A5). There is no optimal polynomial choice, since higher-order polynomials over-fit the model while lower-order ones fail to account for non-linearity in the underlying relationship between subsequent sales prices.

differences in houses just on either side of the threshold are observable in our control variables. This assumption should be reasonable given our extensive set of controls and our very high r-squared values (r-squared > 0.9 for the fully-controlled models). The 5th order polynomials also account for unobservable factors that might influence price across a broader range of prices. Our extensive list of controls, the consistency of our estimates across different levels of controls, and the passage of time and consequent changes in housing, market, and location characteristics between the first and second sale raises confidence that any unobservable characteristic would not eliminate the average estimated discontinuity in our models.

5. RESULTS

5.1 Main Model Results

Table 2 presents the main results for our pooled sample. For each of the three dependent variables, we present two models with increasing control variables to show robustness. The first model controls only for time trends T_t, while the second model includes controls for zip codes, home characteristics, transaction characteristics, and renovation indicators.

Each column lists the average of the sixteen discontinuity coefficients presented below it, along with an F-statistic for the test that these sixteen coefficients are jointly different from zero. Column (2), the fully-specified list price model, finds a large and precise average discontinuity of $2,358. This suggests that the difference in current list prices for homes that previously sold just below a $10,000 threshold compared to homes that sold just above that threshold is on average $2,358 greater than is the difference for homes with previous sales prices that do not straddle a $10,000 threshold but have equivalently-sized differences in previous sale prices. We emphasize that this coefficient is large and economically meaningful, because it implies a future average return of over 1000% if a buyer crosses a $10,000 threshold by paying $236 more. The estimated effect is robust across the two models. As we predicted, sellers and agents anchor on round numbers in previous sale prices when setting prices for newly listed homes. It also suggests homebuyers are heavily penalized in future sales by paying just below a $10,000 threshold.

Column (4) presents the fully-specified model for final home sales prices, which finds an average discontinuity of $2,149. This effect is statistically indistinguishable in magnitude from the list-price effect

(Wald test of equality $p = 0.68$).[3] Regressions using the net price yield similar results and provide support for our hypothesis that the discontinuity would manifest in final sales prices. Collectively, the base models provide strong evidence that real estate agents on average anchor on prior sales prices when listing a home, and that this anchoring effect is not eliminated by the market through subsequent negotiations. In other words, market forces do not efficiently overcome behavioral bias.

5.2 Results on Agent Experience

We next examine whether agent experience influences the magnitude of the discontinuities in subsequent sales. To do so we reran our main model on listing agent experience subsamples, with agent experience cut at the median.[4] The fully controlled models are found in columns 1 and 2 of Table 3. These subsample results support our prediction that the original list price discontinuity would be significantly larger (about $4000 larger) for agents with low experience compared to high experienced agents ($p = 0.002$). This difference again remains uncorrected by the market, as shown in the sales price ($p = .003$) and net price ($p = 0.004$) models. Thus, the bias is much larger for inexperienced agents compared to experienced agents.

The buyer agent experience results are found in Table 4 and suggest a similar story. For these analyses we present the net price models only, as buyer agents have little influence over listing prices. The discontinuities for the fully controlled models, shown in columns 3 and 4, are approximately $2000 larger for less-experienced buyer agents than for highly experienced buyer agents, although the results are not significantly different from each other ($p = 0.179$). While the difference is not statistically significant, the pattern is consistent with the listing agent experience results and suggests the bias reduces with agent experience. The less-precise results for buyer agent experience compared to listing agent experience is perhaps unsurprising, as listing agents have direct influence over the original price and buyer agents only influence price through the negotiation process.

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[3] All subsequent effect size comparisons use Wald tests.

[4] Subsample analyses are used instead of interaction effects because multiple interactions would impose heavy burdens on the data and make the models difficult to interpret A fully-interacted model would require interacting agent experience with each of the five polynomial terms as well as each of the sixteen discontinuities. We are insufficiently powered for such a model.

5.3 Exploratory Analyses on the Role of Organizational Support

We additionally exploit organizational variation in real estate agencies to investigate whether the level of organizational support provided to agents influences the anchoring and adjustment heuristic. Agents must be licensed to sell real estate and are required to work under a licensed broker. Brokers can either work as self-employed individuals or form a brokerage. Forty-three percent of listings in our sample are by nationally franchised brokerages, such as RE/MAX, Caldwell Banker, Keller Williams, or Prudential. The average brokerage employs 25 unique agents in a year. While self-employed brokers retain the entire commission on a sale (typically three percent of the final home sales price), they do not enjoy benefits typical of a larger brokerage, including support staff, real estate leads, training, brand image, and other transaction support. Conversely, while agents at larger brokerages enjoy such brokerage benefits, they are required to split higher commission amounts with their employing brokerage and/or pay a fixed monthly desk fee. Brokerages vary in the amount of support given to agents, with some offering more support at a larger commission split and others providing more limited support but allowing agents to keep a greater amount of their commission.

While we cannot observe all the specific support services for each agency, we were able to gather data on 1) brokerage size (number of unique agents employed by the brokerage each year), and 2) national franchise affiliation. Table A3 presents the first of these results. We first split the sample by the median of brokerage size. Larger brokerages typically have more support services available to agents, improved training, and benefit from a larger number of colleagues. These benefits should aid agents in pricing homes to the market while avoiding biases. Our results, shown in Table A3, suggest that there is only weak evidence suggesting a difference between large and small firms. Column 1 shows a discontinuity that is almost twice as large for small firms compared to large firms, but this difference is not statistically significant ($p = 0.333$) and the discontinuity difference decreases for sale and net price.

Table A4 presents results splitting the sample by whether or not the brokerage belongs to a national franchise, such as Coldwell-Banker or ReMAX. Agents in franchised brokerages potentially have increased access to support services, tools, training, and codified knowledge that should allow them to better avoid bias and price homes to the market, compared to non-franchise agents. Our results in Table A4 show an average discontinuity for agents in non-franchise brokerages that is twice that of agents in franchise brokerages, although the difference is again not statistically significant (p = 0.167). The pattern suggests that the market decreases the discontinuity for agents in non-franchise brokerages, although again the difference is not statistically significant. While the pattern of results from the above analyses are suggestive, these organizational results do not provide strong evidence suggesting that organizational support given to real estate agents significantly attenuates intergenerational bias from the anchoring and adjustment heuristic.

5.4 Robustness Tests

To rule out alternative explanations, and provide further confidence in our results, we ran multiple robustness checks. First, we tested for robustness of our main result, which use a 5th order polynomial around the pricing discontinuities, to a 7th order polynomial. One of the challenges of our approach is choosing the right order of polynomial to control for unobservables around the pricing discontinuities, while not overfitting the data. The 7th order polynomial results are presented in Table A5. They show qualitatively similar results to the main results presented in the paper.

Second, we conducted placebo tests to ensure that our estimated discontinuities were not simply artifacts of our data structure and model. To do so, we repeated our model 100 times, adjusting the discontinuity by \$100 each time. This exercise answers two questions. First, do statistically significant discontinuities appear more often than we should expect, and in places inconsistent with our theory? Second, do the estimated discontinuities consistently decrease the further away they are set from the original \$10,000 threshold? We present the point estimates and 95% confidence intervals in Figure A1, where two patterns are observable. First, discontinuities are evident at not only \$10,000 intervals, but also at \$5,000 thresholds. Second, the estimated discontinuities change as we would expect if our effect is real. Point estimates decrease as the threshold decreases and drop dramatically when the threshold is set higher than the \$10,000 mark.

Third, we investigated whether homes that previously fell just on either side of the round number reference point perform differently on dimensions other than price, such that the price discontinuity may be explained by differences in time on market. Columns 1-3 of Table A6 show no such difference for time of market. Despite homes above the round number reference points having higher list and sales prices, there is no significant difference in time on market for these homes compared to homes that previously fell just below the round number reference points.

Fourth, we tested our results for robustness using an expanded subset of the data. We repeat our fully-specified models five times, symmetrically expanding our sample by $10,000 in both directions for each model. Our largest sample therefore includes homes between $50,000 and $310,000. Figure A2 in the Appendix show the estimated average discontinuity effect sizes for each sample, with the average discontinuity effect size remaining positive and significantly different from zero in all models. The difference between these effect sizes and our primary model is small and statistically insignificant.

Finally, we note that our results on agent experience suggest the discontinuities are driven by the anchoring and adjustment heuristic rather than by agents' strategic behaviors. If agents were strategically pricing based on heuristics in prior sales prices, we should observe more experienced agents having *larger* discontinuity estimates and improved performance outcomes, rather than the smaller ones that we identify. Given that experience is associated with smaller biases, it is unlikely that the identified discontinuities are intentional.

6. EXPERIMENTAL STUDY

The archival results demonstrate that differences in previous sales prices that cross round number reference points correlate with disproportionately large changes in subsequent listing and sales prices, compared to commensurate sales price differences that do not cross round number reference points. However, the number of observations did not afford us enough power to estimate agent fixed effects, and it is possible that homes that fall just below the thresholds could be different in some unobservable way from homes that sell just above the thresholds. Our models may consequently not be controlling entirely for quality differences or for sorting by buyers or agents. While this seems doubtful given our extensive controls, the explanatory power of our models (r-squared over 0.9), and the robustness of our models to multiple specifications, we cannot definitively rule this out.

To test for causality more directly, we conducted a pre-test and a pre-registered experiment on Amazon's Mturk.com. The designs were similar, but the pre-test had less than a third-of-the participants and included only three previous sale price conditions: far-above the round-number reference point, just above it, and just below it. Results were consistent with our predictions (d = 2.16), but the study was significantly underpowered. The Appendix displays the pre-test results and provides additional detail.

6.1 Participants and Method

A total of 1,010 participants (44.0% female; Age: M =35.43, SD = 11.31) recruited from Amazon's Mturk.com participated in the study. The sample size is large because capturing discontinuities requires comparing commensurate differences in house prices above and below the reference point to commensurate differences in house prices for homes that straddle the reference point. This demands many participants (see Schochet 2019). The pre-registration of the study is available at: http://aspredicted.org/blind.php?x=hz2388. We excluded submissions from duplicate IPs (n=73). We also pre-registered that we would exclude submissions completed in less than 220 seconds (n=22). After examining the data and reading press articles about the presence of a "bot panic" on MTurk during the week of our study (see Dreyfuss 2018, Stokel-Walker 2018), we determined that 65 additional responses had been generated by bots and excluded them.[5] Finally, we added a Captcha screen to our survey and ran 164 more participants to reach our pre-registered sample size. We excluded 13 of these participants for the criteria listed above. Data appear at:

https://osf.io/6yvws/?view_only=8370ae7a89d14f6db58badcf21a36056.

[5] These responses were identifiable because bots consistently estimated list prices that were less than $100,000, which was $96,000 lower than the lowest previous sale price of the median-priced house and $47,000 lower than the lowest previous sale price of the lowest-priced house. Moreover, the frequency of estimates spiked between $80,000 and $100,000. Approximately 22% of all estimates occurred in this range, while only 8% of estimates occurred in the $100,001 - $150,000 range. We thus excluded submissions in which the house value was estimated to be less than $100,000. For comparison, the mean number of such estimates/participant in the sample as a whole was 0.37.

Participants examined information about five houses for sale in San Antonio, Texas in order to estimate the sales price for each property. We compiled housing profiles using pictures and addresses from Zillow.com. For each house participants saw the square footage, the numbers of bedrooms and bathrooms, the walkability score, the quietness score, the rating of the local elementary school, and a map of the surrounding area. They also saw five pictures of the home and the ostensible previous sales price, which we manipulated. For each house participants also viewed pictures of three comparable homes that had recently sold and information about those houses' square footage, the number of bedrooms, the number of bathrooms, the previous sales date, and the previous sales price.

Participants used a scale ranging from 0 (Extremely Bad) to 100 (Extremely Good) to rate home and location quality. They then estimated the most appropriate listing price for each house. Once finished, participants indicated how much the previous sale price and the price of comparable homes affected their listing price estimates. Participants then provided their age, gender, and zip code. We used zip code information and Zillow.com to look up median house prices for each zip code in order to control for any anchoring effect that participants' home real estate market might have on list price estimates (see Simonsohn and Loewenstein 2006). Participants finally indicated whether or not they had previously purchased real estate.

We manipulated whether the previous sales price of each listed house was slightly below a round number reference point, further below a reference point, just above a reference point, or further above a reference point. Table 6 displays home prices for each of the conditions. Participants viewed houses from a range of conditions.

6.2 Results

We excluded housing value estimates more than three standard deviations away from the mean estimate for each house. These values appear to result from participants omitting a zero. Fourteen participants did not provide zip codes and so were dropped. To account for between-property variance in housing prices, we standardized the values of the estimated list prices for each house. We pre-registered that the standardized value of the list price estimates would be our primary dependent variable, but we also present results using non-standardized values in Table 7. We analyzed the data at the level of the participant-house coupling. We used cross-nested mixed-models (Gelman and Hill 2006, Kenny et al. 2006) to account for interdependence of data around both participants and houses.

Table 5 reports correlations and descriptive statistics. Table 6 reports mean estimated list prices by house and previous sale price condition, and Table 7 reports mixed model results. We use Wald tests to identify whether the difference in estimated listing prices between the just-above and just-below conditions is larger than equivalent price differences that do not cross the $10,000 thresholds. Altogether we perform three Wald tests comparing estimates from our mixed model. Two of these tests examine whether the difference in parameter estimates that span the round-number reference point (just above – just below) are equivalent to differences in the parameter estimates that do not. The first two tests compare the spanning difference (just above – just below) with two non-spanning differences separately (Tests 2 and 3). Our primary test (Test 1) examines whether the spanning difference is jointly statistically different from Test 2 and Test 3.

Column 1 of Table 7 shows that the estimated subsequent price differences between homes in the just-above and just-below conditions exceeded the price differences between the far-above and just-above conditions and the differences between the just-below and far-below conditions ($p_{\text{Test 1}} = .036$). The comparison with the above-threshold condition was significant ($p_{\text{Test 2}} =. 022$), but the comparison with the below-threshold condition did not reach significance ($p_{\text{Test 3}} = .127$). The condition did not affect estimates of housing or location quality (*p*s > .35).

Participants reported that the previous sale price of the home (*M* = 5.36, *SD* = 1.24) and the prices of comparable homes (*M* = 5.67, *SD* = 1.16) strongly influenced their estimates. Neither variable significantly interacted with the dummy variable to predict subsequent list price.

The experimental results suggest that the position of previous sales prices relative to round number reference points causally affected current listing price. Participants set disproportionately lower current listing prices when the previous sales price was just below a round-number reference point than they did when the previous sales prices was just above a round-number reference point.

7. DISCUSSION AND CONCLUSION

This paper has demonstrated the important yet previously undocumented role round numbers from prior sales prices play in future determinations of value. Using archival real estate data, we found the anchoring effect of previous sales prices on subsequent listing and sales prices to be discontinuous at numbers cleanly divisible by $10,000. Neither market forces nor the negotiation process significantly decreased the size of this pricing bias. The results therefore suggest that buyers who pay prices that fall just below round numbers may receive reduced their profits, compared to those who pay prices just above round numbers, if they decide to resell the property. Importantly, however, we found that the experience level of agents did significantly attenuate the bias. Transactions with inexperienced agents exhibited significantly larger discontinuities than transactions with highly experienced real estate agents. This suggests knowledge gained from experience can attenuate the anchoring and adjustment heuristic, and consequently reduce intergenerational pricing bias. A pre-registered experiment provided evidence of causality.

This paper contributes to the collective knowledge of cognitive heuristics and biases by demonstrating that round number reference points play an outsized role in intergenerational pricing and anchoring. The findings illustrate that people's susceptibility to reference points and the anchoring and adjustment heuristic can, in combination, lead them to make suboptimal decisions. The paper therefore adds to cognitive negotiation theory (Neale and Bazerman 1991) by demonstrating how motivations to achieve goals within negotiations can affect outcomes in subsequent negotiations. It similarly complements existing research on history dependence in negotiations (e.g., Bokhari and Geltner 2011, Einiö et al. 2008, Beggs and Graddy 2009). Standard economic models account for path dependency in market prices but do not predict that numerical reference points, such as round numbers, play any role in price correlations across multiple sales.

The paper also adds to our understanding of how experience or expertise affect individual susceptibility to cognitive biases. Research has generally found that expertise does not entirely insulate people from cognitive biases, such as anchoring (Englich et al. 2006, Northcraft and Neale 1987, Orr and Guthrie 2005). Our results provide a more nuanced understanding of the impact of expertise by showing that gradations in experience correspond to gradations in susceptibility to some cognitive heuristics in valuations. They also add to previous work on how human capital correlates with performance (e.g., Rosen 1983; Hitt et al. 2001; Crook et al. 2011; Gubler 2019) by highlighting an additional avenue through which human capital may lead to higher individual and organizational performance.

More generally, the results of our paper show that cognitive heuristics and biases can influence financially important decisions in ways that persist across time. People looking to resell durable goods may thus be well-advised to think carefully about the prices they offer to obtain these goods, as these decisions may have ramifications for future resale prices that will not be corrected by the negotiation process. Our work suggests that there may be a benefit to reaching and exceeding such thresholds in transactions, particularly if the buyer is considering selling the good again in the future. The magnitude of our estimated effect sizes suggests the payoff could be substantial, with gains or losses of between $2,000 to $5,000 in the final resale price in real estate transactions. Buyers intending to eventually resell would consequently need to "make up" more than the average discontinuity drop (i.e., $2,150) if the final sales price drops below a relevant round number reference point. The results also show that the benefit of hiring an experienced agent, particularly if the home previously fell below the reference point, is large as inexperienced agents show greater susceptibility to previous listing price placement.

Finally, the findings in this paper have important managerial implications. Managers should be aware of this potential bias and know that markets and the negotiation process will not correct for mispricing. Because individual experience and expertise may reduce the effect of heuristics and biases in transactions, organizations that employ such expert intermediaries may consequently capitalize on this knowledge by instituting mentorship programs to scale the debiasing effects of human capital. However, our organizational support results suggest that currently used training programs, observation of colleagues, and other forms of organizational support may have only limited value in helping experts avoid bias. Organizations may therefore need to devote resources to different types of support that can leveraging the debiasing effects of human capital embedded in experienced employees to increase organizational performance and improve the decision making of less experienced employees.

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FIGURES AND TABLES

Figure 1: Histogram of Prior Sales Price at $1000 Buckets

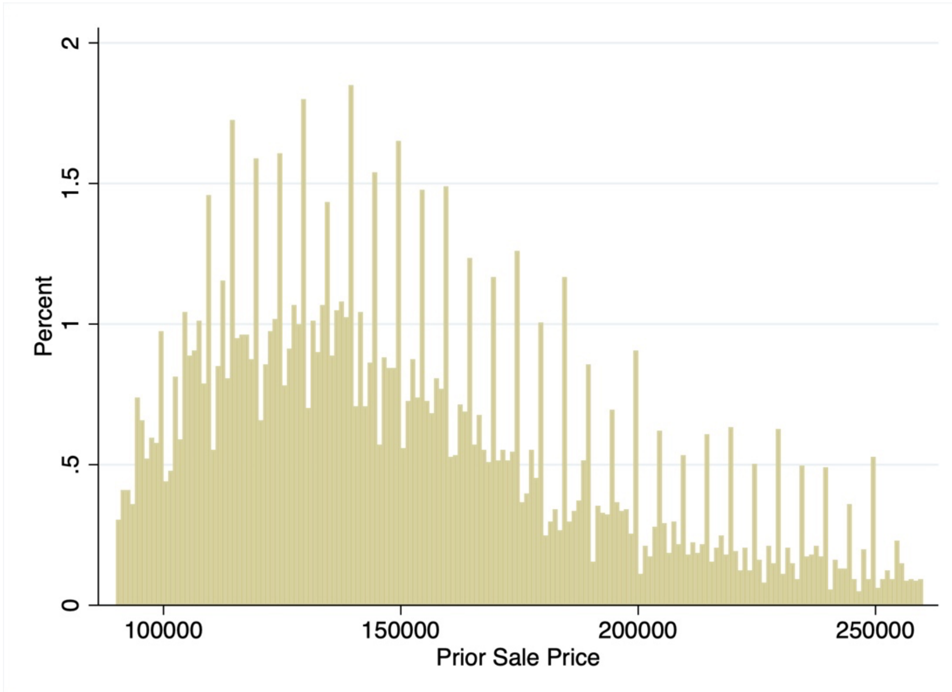
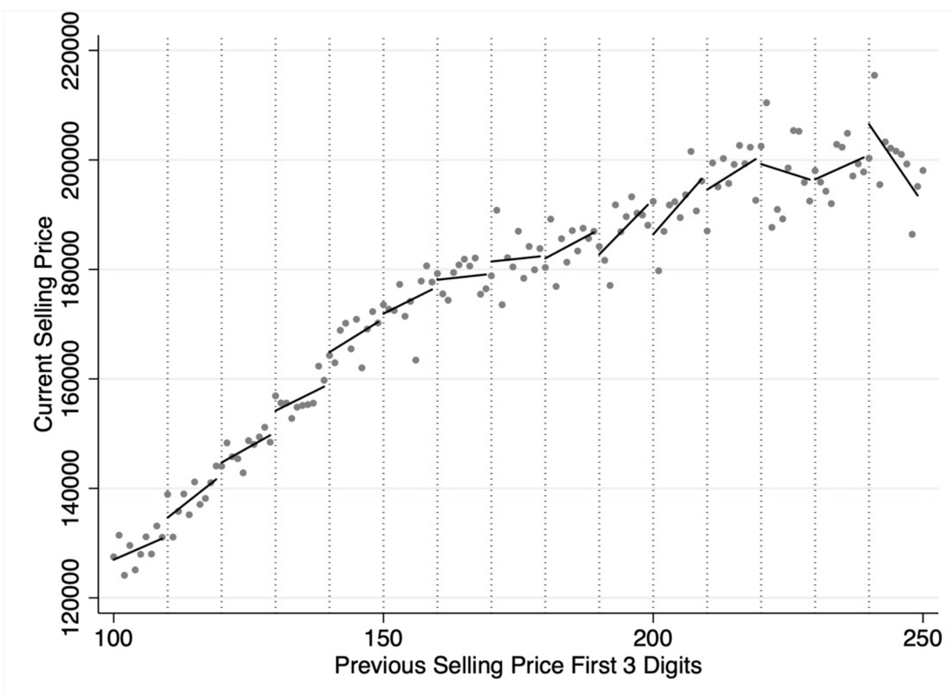


Figure 2: Raw Data Discontinuities at $10,000 Cutoffs



Note: This figure presents raw price data bucketed by $1000 previous price intervals. Solid lines represent linear fits for each $10,000 price interval.

Table 1: Descriptive Statistics, Archival Data

Archival Data Variable	N	Mean	Std Dev	Min	Max
Original list price	16,111	185305	54422	44000	529900
Sale price	13,673	175887	49701	52000	375000
Net price	13,667	173427	49427	52000	375000
Previous sale price	16,111	152127	39461	90005	259990
Agent experience	16,111	127.35	183.12	1	1317
Brokerage size	16,111	24.65	26.71	1	178
Franchise brokerage	16,111	0.45	0.50	0	1
Days on market	16,102	96.07	72.15	0	727
Fail	16,111	0.15	0.36	0	1

Table 2: Main Discontinuity Results

Dependent Variable:	(1) List Price	(2) List Price	(3) Sales Price	(4) Sales Price	(5) Net Price	(6) Net Price
Avg. Discontinuity	**3586.25**	**2358.44**	**3142.66**	**2149.81**	**3087.40**	**2050.45**
F-stat	**9.49**	**15.07**	**8.64**	**11.02**	**8.44**	**9.64**
Prob > F	**0.0021**	**0.0001**	**0.0034**	**0.0009**	**0.0038**	**0.002**
$100k	4601.2*	744.9	3476.0	573.5	3392.3	510.5
	(2410.8)	(1494.9)	(2243.0)	(1424.1)	(2205.6)	(1454.6)
$110k	3944.9**	684.3	2563.7	-107.5	2403.0	-145.6
	(1997.0)	(875.4)	(1564.5)	(764.6)	(1550.8)	(766.2)
$120k	4477.7***	1597.2**	3733.7***	900.7	3686.1***	959.5
	(1423.8)	(673.9)	(1293.8)	(738.8)	(1293.4)	(730.9)
$130k	4401.4***	1224.5*	4086.9***	1046.4	4156.3***	951.5
	(1563.6)	(654.0)	(1277.5)	(679.1)	(1296.7)	(697.7)
$140k	2231.7	495.2	3527.5***	1371.8**	3705.0***	1460.9**
	(1515.6)	(735.2)	(1238.7)	(681.0)	(1235.8)	(681.7)
$150k	745.4	242.9	1050.3	758.8	1079.5	679.6
	(1729.3)	(715.8)	(1583.7)	(786.3)	(1562.2)	(801.3)
$160k	576.6	857.7	2218.6	2104.9**	2195.3	2028.2**
	(1900.3)	(1085.2)	(1436.2)	(917.6)	(1407.7)	(920.1)
$170k	-1420.3	259.4	-1466.0	65.4	-1524.8	-48.2
	(2190.8)	(1051.5)	(1979.7)	(1131.5)	(1962.4)	(1135.5)
$180k	-381.9	169.9	2919.1	2746.1**	2548.7	2419.2**
	(2590.5)	(1191.1)	(2359.6)	(1219.4)	(2348.8)	(1224.5)
$190k	-2242.4	2018.7	327.5	2865.8**	327.6	2820.2**
	(2832.3)	(1414.8)	(2747.2)	(1411.2)	(2706.1)	(1428.1)
$200k	3097.0	1852.2	2097.7	2320.4	1872.6	2067.5
	(3363.2)	(1865.2)	(2876.9)	(1579.1)	(2832.7)	(1565.5)
$210k	3827.6	6521.8***	3384.9	3825.0**	3276.7	3747.4**
	(3665.9)	(1857.0)	(3600.6)	(1905.0)	(3591.3)	(1870.9)
$220k	4843.1	3676.9	5532.2	3853.1*	5556.1	3719.4
	(4905.7)	(2754.6)	(4069.3)	(2269.5)	(4049.9)	(2305.1)
$230k	3698.3	6473.0***	506.4	4589.0**	577.4	4581.2**
	(4748.5)	(2393.6)	(4580.7)	(2292.4)	(4534.2)	(2278.3)
$240k	8688.5*	3555.9	7084.5	4201.1	6663.1	3778.2
	(4488.3)	(2715.1)	(4577.0)	(2951.0)	(4514.1)	(2951.6)
$250k	16291.4**	7360.7	9239.6	3282.4	9483.5	3277.7
	(6875.1)	(4552.4)	(6546.5)	(4397.0)	(6568.8)	(4376.5)
5th-Order Poly	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Zip Code Dummies		YES		YES		YES
House Controls		YES		YES		YES
Transaction Controls		YES		YES		YES
Renovation Controls		YES		YES		YES
Observations	16111	13584	13673	13584	13667	13584
R-squared	0.693	0.929	0.711	0.917	0.713	0.915

Note: Models are estimated using OLS with errors clustered by agency. The bolded lines present a joint significance test for whether all estimated discontinuities are jointly statistically different from zero. Time controls include dummies for year and month of the current sale as well as the logged number of days between sales. House, transaction, and renovation controls listed in appendix Table A1. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Discontinuity Results by Listing Agent Experience

Dependent Variable:	(1) Low LA XP List Price	(2) High LA XP List Price	(3) Low LA XP Sales Price	(4) High LA XP Sales Price	(5) Low LA XP Net Price	(6) High LA XP Net Price
Avg. Discontinuity	**4925.45**	**1056.22**	**4172.16**	**1055.35**	**4017.40**	**961.74**
F-stat	**24.23**	**1.79**	**15.48**	**1.92**	**14.27**	**1.55**
Prob > F	**0.0000**	**0.182**	**0.0001**	**0.1662**	**0.0002**	**0.2147**
$100k	-3378.4*	3158.8*	-1419.4	2097.5	-1472.2	2062.7
	(1930.9)	(1913.1)	(1840.6)	(1894.7)	(1854.5)	(1922.3)
$110k	49.0	965.7	239.2	-332.2	197.9	-345.8
	(1169.0)	(1114.8)	(1271.8)	(940.4)	(1275.5)	(947.3)
$120k	2817.3**	756.3	2369.6*	-146.3	2198.9*	13.6
	(1254.1)	(884.9)	(1236.2)	(1004.9)	(1216.3)	(1011.6)
$130k	2375.6**	611.6	1425.9	878.5	1435.5	741.6
	(1117.0)	(787.0)	(1055.6)	(894.8)	(1073.7)	(897.8)
$140k	678.4	247.2	1861.0	1129.9	2099.0*	1123.0
	(1301.1)	(892.9)	(1190.1)	(824.9)	(1198.6)	(825.1)
$150k	512.5	-82.6	670.5	854.4	524.7	836.1
	(1303.2)	(936.2)	(1322.2)	(986.4)	(1329.5)	(991.1)
$160k	2251.9	52.9	2696.5*	2053.2*	2883.4*	1852.6*
	(1568.0)	(1384.0)	(1623.3)	(1105.8)	(1629.6)	(1114.4)
$170k	1404.4	-545.4	579.7	-355.9	450.3	-450.3
	(1790.0)	(1246.0)	(1729.4)	(1339.3)	(1724.5)	(1332.6)
$180k	648.6	72.2	5140.4**	1551.0	4731.9**	1238.1
	(1760.0)	(1542.7)	(2071.3)	(1511.3)	(2078.8)	(1514.8)
$190k	4194.0**	687.2	4346.0**	1828.7	4204.6*	1814.1
	(2106.1)	(1706.8)	(2198.2)	(1544.3)	(2208.4)	(1559.4)
$200k	1401.0	2169.1	2994.4	1636.1	2500.1	1508.8
	(2821.0)	(2491.4)	(2771.1)	(1945.6)	(2802.2)	(1914.8)
$210k	11355.4***	3744.3	9819.2***	77.9	9231.6***	200.1
	(3009.4)	(2326.6)	(2889.7)	(2389.6)	(2977.3)	(2315.4)
$220k	9065.1**	1161.1	7560.1*	1429.2	7239.3*	1365.8
	(4384.9)	(3249.7)	(4005.9)	(2736.2)	(4058.0)	(2761.6)
$230k	10290.3**	4766.4	10760.9***	1196.1	10507.9***	1237.5
	(4102.9)	(2950.3)	(3837.9)	(2574.5)	(3843.1)	(2568.1)
$240k	13389.8***	-1332.5	11360.7**	-54.2	10587.3**	-350.4
	(4645.5)	(3384.1)	(5304.2)	(3582.8)	(5362.4)	(3573.2)
$250k	21752.2***	467.4	6349.9	3041.7	6958.3	2540.6
	(6972.4)	(6269.0)	(6699.6)	(5268.9)	(6689.4)	(5309.3)
5th-Order Poly	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Zip Code Dummies	YES	YES	YES	YES	YES	YES
House Controls	YES	YES	YES	YES	YES	YES
Transaction Controls	YES	YES	YES	YES	YES	YES
Renovation Controls	YES	YES	YES	YES	YES	YES
Observations	4799	8785	4799	8785	4799	8785
R-squared	0.927	0.931	0.918	0.918	0.915	0.916

Note: Models are estimated using OLS with errors clustered by agency. The bolded lines present a joint significance test for whether all estimated discontinuities are jointly statistically different from zero. Time controls include dummies for year and month of the current sale as well as the logged number of days between sales. House, transaction, and renovation controls listed in appendix Table A1. Low and high listing agent experience measured as being below or above the agent experience median. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Discontinuity Results by Buyer Agent Experience

	(1)	(2)	(3)	(4)
	Low BA XP	High BA XP	Low BA XP	High BA XP
Dependent Variable:	Sales Price	Sales Price	Net Price	Net Price
Avg. Discontinuity	**2932.27**	**1356.91**	**2927.57**	**1198.57**
F-stat	**8.60**	**2.61**	**8.27**	**2.01**
Prob > F	**0.0035**	**0.1064**	**0.0042**	**0.1563**
$100k	-166.6	932.2	-206.4	839.6
	(1907.2)	(1754.9)	(1939.0)	(1767.8)
$110k	403.8	-806.2	484.1	-971.3
	(1173.0)	(993.7)	(1181.3)	(975.6)
$120k	1255.3	506.2	1709.8	245.2
	(1218.3)	(991.6)	(1210.3)	(974.1)
$130k	459.4	1622.1	567.3	1378.3
	(1056.9)	(1027.3)	(1067.2)	(1042.1)
$140k	2096.0*	903.0	2416.7**	829.8
	(1082.1)	(899.9)	(1084.0)	(905.9)
$150k	517.9	970.4	448.6	875.4
	(1271.3)	(1050.3)	(1301.4)	(1088.9)
$160k	3498.7**	1176.1	3343.8**	1166.2
	(1416.4)	(1114.3)	(1455.7)	(1129.2)
$170k	448.3	-98.8	229.3	-138.0
	(1466.3)	(1307.7)	(1498.3)	(1286.5)
$180k	3135.3*	2170.1	2691.8	2010.6
	(1890.6)	(1518.0)	(1911.3)	(1545.2)
$190k	5114.2**	1240.4	4864.4**	1383.2
	(2044.1)	(1813.7)	(2075.3)	(1835.9)
$200k	4950.9**	-22.2	4645.1*	-219.4
	(2431.5)	(2324.2)	(2482.4)	(2327.7)
$210k	2474.0	4889.3**	2711.2	4590.1**
	(3387.4)	(2366.5)	(3456.0)	(2331.9)
$220k	7270.1**	226.5	7207.4**	31.1
	(3460.1)	(2831.2)	(3495.0)	(2840.9)
$230k	4536.7	3742.4	5272.6	3268.4
	(3695.2)	(3266.0)	(3622.4)	(3298.1)
$240k	6917.7	1620.7	6338.6	1289.8
	(4299.7)	(3828.9)	(4280.5)	(3875.5)
$250k	4004.7	2638.3	4116.8	2597.9
	(7045.2)	(5195.2)	(6986.1)	(5330.9)
5th-Order Poly	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES
Zip Code Dummies	YES	YES	YES	YES
House Controls	YES	YES	YES	YES
Transaction Controls	YES	YES	YES	YES
Renovation Controls	YES	YES	YES	YES
Observations	5958	7626	5958	7626
R-squared	0.923	0.919	0.921	0.918

Note: Models are estimated using OLS with errors clustered by agency. The bolded lines present a joint significance test for whether all estimated discontinuities are jointly statistically different from zero. Time controls include dummies for year and month of the current sale as well as the logged number of days between sales. House, transaction, and renovation controls listed in appendix Table A1. Low and high buyer agent experience measured as being below or above the agent experience median. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Study 2 Correlations and Means

	1	2	3	4	5	6	7
1. Est. Listing Price	1.00						
2. Age	0.02	1.00					
3. Female	0.01	0.07 **	1.00				
4. Purchased Real Estate	0.01	0.29 **	0.00	1.00			
5. Median Home Price in Zip Code	0.00	-0.13 **	-0.21 **	0.02	1.00		
6. Estimated Quality	0.23 **	0.02	-0.10 **	-0.01	0.06 **	1.00	
7. Estimated Location	0.29 **	0.01	-0.07 **	0.02	0.08 **	0.58 **	1.00
Mean	317,928	36	0.45	1.57	368,205	75	70
Median	293,000	32	0.00	2.00	231,200	80	75

** Correlation is significant at the 0.01 level (2-tailed).
Note: N= 4,959 Person-House Estimates. Purchased Real Estate denoted by a dummy variable.

Table 6: Study 2 Estimated List Price by Condition and House

Previous Sale Price Position Relative to Round Number	House 1 Previous Sale Price	House 1 Estimated Listing Price *M*	*SD*	*N*	House 2 Previous Price	House 2 Estimated Listing Price *M*	*SD*	*N*
Well Above	153,000	159,594	15,160	226	203,900	208,547	26,130	243
Slightly Above	151,000	161,673	17,275	240	201,300	207,818	20,591	248
Slightly Below	149,000	158,034	17,477	231	198,700	202,932	20,302	245
Well Below	147,000	155,728	16,705	243	196,100	202,772	21,947	242
Total		158,742	16,811	940		205,527	22,477	978

Previous Sale Price Position Relative to Round Number	House 3 Previous Price	House 3 Estimated Listing Price *M*	*SD*	*N*	House 4 Previous Sale Price	House 4 Estimated Listing Price *M*	*SD*	*N*
Well Above	164,500	309,117	35,716	244	293,000	301,115	39,295	247
Slightly Above	161,500	309,879	36,769	249	291,000	299,283	47,388	253
Slightly Below	158,500	305,190	39,113	237	289,000	296,487	40,655	245
Well Below	155,500	300,995	39,307	254	287,000	293,780	38,457	249
Total		306,267	37,870	984		297,671	41,661	994

Previous Sale Price Position Relative to Round Number	House 5 Previous Price	House 5 Estimated Listing Price *M*	*SD*	*N*
Well Above	604,500	620,176	51,089	237
Slightly Above	601,500	618,222	47,057	244
Slightly Below	598,500	614,390	55,132	249
Well Below	595,500	618,552	44,964	241
Total		617,798	49,716	971

Note: Table displays participants' estimated listing prices for each of the four conditions for each of the five houses in Study 2. Well above represents a previous sales prices that are more than $1,500 greater than the round number, just above represents previous sale prices that are up to $1,500 above the round number, just below represents previous sale prices that are as low as $1,500 below the round number, and well below represents previous sale prices that are more than $1,500 below the round number.

Table 7: Linear Cross-Nested Mixed Models

	All Participants	Participants Who Have Purchased Property	All Participants	Participants Who Have Purchased Property
Dependent Variable:	(1) Z-Score of Estimated Price	(2) Z-Score of Estimated Price	(3) Estimated Price	(4) Estimated Price
	Estimate	Estimate	Estimate	Estimate
Intercept	-0.0109 (0.036)	-0.043 (0.040)	315,697*** (71,992.2)	318,943*** (72,107.4)
Well Above (a)	0.087*** (0.019)	0.045 (0.024)	5,311.9*** (1,272.9)	2,790.1 (1,638.9)
Just Above (b)	0.091*** (0.019)	0.076** (0.024)	5,164.5*** (1,266.7)	4,632.4** (1,639.9)
Just Below (c)	0.020 (0.019)	0.006 (0.024)	871.3 (1,281.3)	370.2 (1,667.9)
Median House Price in Home Zip	-1.64e-07*** (3.10e-.08)	-2.23e-07*** (3.81e-.08)	-.0088*** (.0019)	-0.0124 (0.0023)
Discontinuity Tests				
1. Wald [(b-c)>mean(a-b,c-0)]	X^2 = 4.39 (p=0.036)	X^2 = 4.87 (p=0.027)	X^2 = 3.54 (p=0.060)	X^2 = 3.63 (p=0.057)
2. Wald (b-c>a-b)	X^2 = 5.28 (p=0.022)	X^2 = 6.19 (p=0.013)	X^2 = 3.57 (p=0.060)	X^2 = 4.60 (p=0.032)
3. Wald (b-c>c-0)	X^2 = 2.34 (p=0.127)	X^2 = 2.42 (p=0.120)	X^2 = 2.38 (p=0.123)	X^2 = 1.81 (p=0.179)
Log Likelihood	-3,583	-1,982.3	-57,911	-33,313
Likelihood Ratio Test (vs. OLS)	X^2 = 1,316.6 (p=0.000)	X^2 = 914.2 (p=0.000)	X^2 = 15,512.8 (p=0.000)	X^2 = 9006.0 (p=0.000)
# Participants	996	574	996	574
# Houses	5	5	5	5
Observations	4,889	2,815	4,889	2,815

Note: This table presents mixed model (HLM) results from Study 2, with five houses cross-nested with 996 participants. *Well above* represents a previous sales prices that are more than $1,500 greater than the round number, *just above* represents previous sale prices that are up to $1,500 above the round number, *just below* represents previous sale prices that are as low as $1,500 below the round number, and *well below* represents previous sale prices that are more than $1,500 below the round number. The omitted category is well below. The formal tests of the discontinuity are Wald tests, which are consistent with a large discontinuity but under-powered.

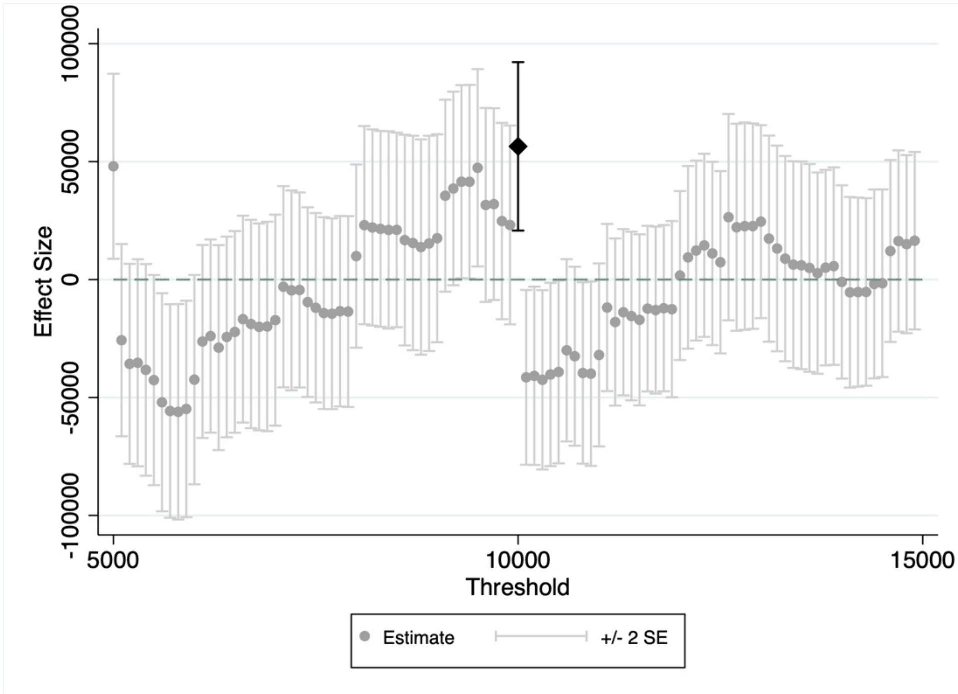
APPENDIX

Appendix 1: Experimental Pretest

The design of the pretest was similar to the design of Study 2 but included only three previous sale price conditions: far-above the round-number reference point, just above it, and just below it. We provided 300 participants recruited from Amazon's Mturk.com website with a host of information and pictures about seven properties to make participants' experiences comparable to what home buyers see when viewing properties on real estate websites. For each house, participants viewed one of three versions of the previous sale price: a price above the reference point (e.g., \$261,000), a price just below the reference point (e.g., \$259,000), or a price significantly below the reference point (e.g., \$257,000). The hypothesis test for this design is that the difference in estimated price between the just-above and just-below conditions would be much larger than the difference between the just-below and far-below conditions.

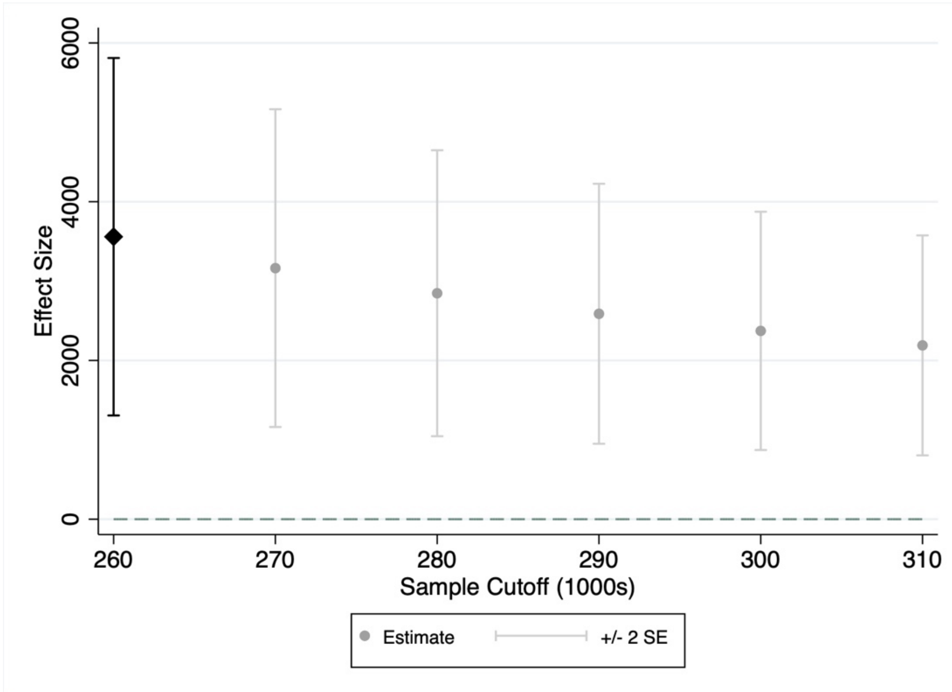
Table A7 shows correlations, Table A8 shows the mean estimated listing price by condition by house, and Table A9 shows the results of the mixed-model analyses. When we used standardized values of the housing estimate as the dependent variable, we found that the average difference (M = \$6,258, SD = \$3,025) in estimated list prices between the just-above and just-below conditions was much larger than the average difference (M = -\$332, SD = \$3,065) between the just-below and far-below conditions (X^2 = 2.03, p=0.154, d = 2.16). While the estimated discontinuity is large, the variance is also large. The pretest revealed a need for significantly more power in order to detect discontinuities. We therefore pre-registered a new study using a much larger sample and added the control variable of participants' home zip codes.

Figure A1: Discontinuity Placebo Tests



Note: This figure presents the average estimated listing price discontinuities for our fully controlled model with 100 placebo simulations. For each simulation we assigned the discontinuity to a $100 interval within $5,000 above and below the true $10,000 round number discontinuity. The results suggest that our main results using the $10,000 round number discontinuities is not driven by a spurious correlation in the data or our model.

Figure A2: Robustness to Sample Cutoff Points



Note: This table presents the average estimated discontinuity for the fully controlled model with variations in the sample cutoff. The main models presented in the paper use a sample cutoff of $250,000. This table shows robustness to a sample cutoff of $310,000.

Table A1: Control Variables

Archival Data Variable	N	Mean	Std Dev	Min	Max
House Controls					
Total # of bedrooms	16,111	3.71	1.08	1	9
Total # of bathrooms	16,111	2.36	0.79	1	7
Total # of kitchens	16,111	1.06	0.24	1	3
Total # of fireplaces	16,111	0.41	0.63	0	5
Total # of laundry rooms	16,111	0.99	0.30	0	3
Total # of dining rooms	16,111	0.09	0.28	0	2
Total # of family rooms	16,111	1.18	0.68	0	4
% of basement finished	16,111	46.68	46.19	0	100
Garage capacity	16,111	1.27	0.97	0	10
Pool	16,111	0.07	0.25	0	1
log(square feet)	16,111	7.55	0.37	6.26	8.63
log(acres)	16,111	0.16	0.13	0	2.57
Year built	16,111	1982	25.25	1848	2013
Property type	16,111	3.87	1.04	1	6
log(HOA fee)	16,111	1.34	2.02	0	7.19
Quality control dummies (*See list below)					
Transaction Controls					
Immediate possession	16,111	0.25	0.43	0	1
Dual agent	16,111	0.14	0.35	0	1
Dual office	16,111	0.17	0.38	0	1
Time and Geographic Controls					
Days since last sale	16,111	1484.07	1082.88	1	6530
Year	16,111	2008	3.74	1996	2014
Month	16,111	6.10	3.23	1	12
Zip	16,111	84316	295.10	84003	84664

Quality control dummies: TLC, needs updating, estate sale, foreclosure, handyman, as is, rehabber, bank owned, priced to sell, motivated, potential, close, exclamation, new, spacious, elegance, beautiful, remodeled, historic, maintained, wonderful, fantastic, charming, stunning, amazing, granite, immaculate, breathtaking, neighborhood, spectacular, landscaped, stained glass, built in, tasteful, must see, fabulous, leaded, delightful, move in, gourmet, Corian, custom, unique, maple, newer, hurry, pride, clean, quiet, dream, block, huge, deck, mint, hardwood, views, new roof, upgraded, vaulted, floor plan, award, hot tub, tile, cul-de-sac, jacuzzi, park, brick, value, windows, mother in law, stainless, theater, surround sound, pickiest, rare, starter, master, cute, warranty, temple, fenced

Renovation controls: Change in house, transaction, and quality controls between periods

Table A2: Balance T-Tests for Each $10,000 Round Number Threshold

Previous Sales Price +/- 500 from $100,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	79	82	1318.671	1337.683	-19.012	62.653	0.762
Acreage	79	82	0.125	0.116	0.009	0.021	0.663
# Bedrooms	79	82	2.785	2.732	0.053	0.131	0.685
# Bathrooms	79	82	1.608	1.646	-0.039	0.095	0.684
% Finished Basement	79	82	11.772	18.512	-6.740	5.000	0.180
Year Built	79	82	1967.544	1970.805	-3.261	4.783	0.496

Previous Sales Price +/- 500 from $110,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	94	139	1383.457	1559.986	-176.528	60.550	0.004
Acreage	94	139	0.141	0.133	0.009	0.021	0.677
# Bedrooms	94	139	2.840	3.108	-0.267	0.111	0.017
# Bathrooms	94	139	1.702	1.734	-0.032	0.087	0.717
% Finished Basement	94	139	17.713	31.403	-13.690	4.897	0.006
Year Built	94	139	1971.383	1970.727	0.656	3.896	0.866

Previous Sales Price +/- 500 from $120,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	112	148	1594.473	1718.885	-124.412	63.374	0.051
Acreage	112	148	0.146	0.165	-0.019	0.017	0.248
# Bedrooms	112	148	3.152	3.378	-0.227	0.121	0.061
# Bathrooms	112	148	1.750	1.912	-0.162	0.086	0.062
% Finished Basement	112	148	22.795	30.169	-7.374	4.889	0.133
Year Built	112	148	1972.902	1969.216	3.686	3.693	0.319

Previous Sales Price +/- 500 from $130,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	136	158	1756.801	1878.076	-121.275	53.248	0.023
Acreage	136	158	0.158	0.176	-0.018	0.014	0.197
# Bedrooms	136	158	3.346	3.506	-0.161	0.095	0.093
# Bathrooms	136	158	1.926	2.101	-0.175	0.075	0.020
% Finished Basement	136	158	35.919	35.190	0.729	5.026	0.885
Year Built	136	158	1978.110	1977.722	0.389	3.081	0.900

Previous Sales Price +/- 500 from $140,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	127	173	1983.559	2031.139	-47.580	69.631	0.495
Acreage	127	174	0.178	0.289	-0.111	0.076	0.146
# Bedrooms	126	174	3.571	3.563	0.008	0.096	0.932
# Bathrooms	127	174	2.173	2.184	-0.011	0.073	0.884
% Finished Basement	127	174	39.134	44.609	-5.475	5.199	0.293
Year Built	127	174	1982.890	1981.638	1.252	2.437	0.608

Previous Sales Price +/- 500 from $150,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	121	142	2025.372	2158.331	-132.959	75.594	0.080
Acreage	122	142	0.177	0.185	-0.008	0.013	0.534
# Bedrooms	122	141	3.492	3.752	-0.260	0.107	0.016
# Bathrooms	122	142	2.344	2.465	-0.121	0.078	0.124
% Finished Basement	122	142	34.107	47.845	-13.739	5.509	0.013
Year Built	122	142	1987.320	1982.113	5.207	2.507	0.039

Previous Sales Price +/- 500 from $160,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	110	134	2224.573	2122.336	102.237	70.987	0.151
Acreage	110	134	0.202	0.323	-0.121	0.156	0.439
# Bedrooms	110	134	3.955	3.627	0.328	0.129	0.012
# Bathrooms	110	134	2.536	2.396	0.141	0.082	0.089
% Finished Basement	110	134	50.564	45.187	5.377	5.842	0.358
Year Built	110	134	1987.164	1983.903	3.261	3.024	0.282

Previous Sales Price +/- 500 from $170,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	102	92	2188.529	2225.587	-37.058	90.131	0.681
Acreage	102	92	0.169	0.203	-0.034	0.022	0.129
# Bedrooms	102	92	3.422	3.902	-0.481	0.133	0.000
# Bathrooms	102	92	2.422	2.511	-0.089	0.096	0.354
% Finished Basement	102	92	34.167	42.500	-8.333	6.402	0.195
Year Built	102	92	1990.618	1989.424	1.194	2.520	0.636

Previous Sales Price +/- 500 from $180,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	59	99	2534.322	2503.222	31.100	104.455	0.766
Acreage	59	99	0.190	0.231	-0.041	0.028	0.145
# Bedrooms	59	99	3.966	3.949	0.017	0.183	0.928
# Bathrooms	59	99	2.746	2.556	0.190	0.115	0.099
% Finished Basement	59	99	56.102	43.566	12.536	7.593	0.101
Year Built	59	99	1990.576	1985.192	5.384	3.277	0.102

Previous Sales Price +/- 500 from $190,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	64	70	2465.938	2375.271	90.666	108.031	0.403
Acreage	64	70	0.203	0.254	-0.051	0.074	0.496
# Bedrooms	64	70	3.859	4.186	-0.326	0.173	0.062
# Bathrooms	64	70	2.703	2.743	-0.040	0.111	0.722
% Finished Basement	64	70	44.359	57.829	-13.469	7.932	0.092
Year Built	64	70	1995.094	1990.857	4.237	2.767	0.128

Previous Sales Price +/- 500 from $200,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	57	85	2437.333	2619.576	-182.243	122.202	0.138
Acreage	57	85	0.249	0.284	-0.036	0.045	0.427
# Bedrooms	57	85	4.105	4.200	-0.095	0.187	0.613
# Bathrooms	57	85	2.684	2.800	-0.116	0.123	0.347
% Finished Basement	57	85	52.877	52.471	0.407	7.874	0.959
Year Built	57	85	1988.509	1991.341	-2.832	3.501	0.420

Previous Sales Price +/- 500 from $210,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	30	59	2781.067	2735.186	45.880	140.530	0.745
Acreage	30	59	0.457	0.232	0.224	0.165	0.176
# Bedrooms	30	59	4.000	4.102	-0.102	0.263	0.700
# Bathrooms	30	59	2.567	2.898	-0.332	0.161	0.042
% Finished Basement	30	59	48.167	45.034	3.133	10.368	0.763
Year Built	30	59	1988.100	1989.746	-1.646	4.926	0.739

Previous Sales Price +/- 500 from $220,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	32	69	2535.875	2886.884	-351.009	144.392	0.017
Acreage	32	69	0.212	0.375	-0.163	0.116	0.162
# Bedrooms	32	69	4.188	4.261	-0.073	0.243	0.764
# Bathrooms	32	69	2.844	2.884	-0.040	0.151	0.790
% Finished Basement	32	69	53.406	44.362	9.044	10.153	0.375
Year Built	32	69	1994.688	1989.203	5.485	4.243	0.199

Previous Sales Price +/- 500 from $230,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	38	56	2930.447	2890.232	40.215	148.503	0.787
Acreage	38	56	0.231	0.219	0.013	0.023	0.584
# Bedrooms	38	56	4.500	4.089	0.411	0.213	0.056
# Bathrooms	38	56	3.053	2.911	0.142	0.149	0.344
% Finished Basement	38	56	53.947	53.321	0.626	9.875	0.950
Year Built	38	56	1998.184	1994.875	3.309	2.264	0.147

Previous Sales Price +/- 500 from $240,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	25	51	3013.600	3107.647	-94.047	158.652	0.555
Acreage	25	51	0.218	0.242	-0.024	0.031	0.438
# Bedrooms	25	51	4.240	4.078	0.162	0.278	0.563
# Bathrooms	25	51	2.920	3.137	-0.217	0.139	0.122
% Finished Basement	25	51	45.320	50.882	-5.562	11.508	0.630
Year Built	25	51	1999.600	1993.059	6.541	2.979	0.031

Previous Sales Price +/- 500 from $250,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	21	64	2868.238	3266.219	-397.981	179.035	0.029
Acreage	21	64	0.243	0.428	-0.185	0.162	0.257
# Bedrooms	21	64	4.190	4.172	0.019	0.247	0.940
# Bathrooms	21	64	2.952	3.078	-0.126	0.190	0.511
% Finished Basement	21	64	27.143	43.672	-16.529	11.416	0.151
Year Built	21	65	1997.143	1996.062	1.081	3.119	0.730

Note: Balance tests compare homes within $500 (above and below) of a $10,000 round number reference point threshold. Data are presented for variables cited in the literature as being key determinants of home value. P-values for Wald tests testing differences in variable means are presented in the final column.

Table A3: Discontinuity Results by Organizational Size

Dependent Variable:	(1) Large Firm List Price	(2) Small Firm List Price	(3) Large Firm Sales Price	(4) Small Firm Sales Price	(5) Large Firm Net Price	(6) Small Firm Net Price
Avg. Discontinuity	**1845.47**	**3046.01**	**2315.30**	**1934.38**	**2233.88**	**1827.24**
F-stat	**5.91**	**9.40**	**8.43**	**2.96**	**7.42**	**2.59**
Prob > F	**0.0162**	**0.0022**	**0.0042**	**0.0855**	**0.0072**	**0.1079**
$100k	745.1	481.3	1788.7	-2484.5	1585.4	-2372.1
	(1778.6)	(2610.8)	(1722.3)	(2515.6)	(1754.6)	(2553.0)
$110k	148.9	1564.0	-428.5	499.4	-520.0	530.4
	(941.7)	(1686.4)	(869.1)	(1489.5)	(888.4)	(1483.5)
$120k	1341.3	1972.3	449.0	1811.2	487.6	1828.9
	(881.2)	(1362.2)	(951.4)	(1327.1)	(938.0)	(1319.5)
$130k	1648.1**	314.2	1037.9	1325.0	1044.6	1076.6
	(753.0)	(1198.4)	(816.9)	(1200.0)	(839.3)	(1205.0)
$140k	263.3	173.0	1380.0*	809.4	1548.2**	763.5
	(927.9)	(1378.2)	(767.2)	(1275.6)	(779.1)	(1283.5)
$150k	156.4	-42.4	867.9	293.3	874.3	148.2
	(805.5)	(1356.0)	(892.5)	(1466.9)	(932.2)	(1487.0)
$160k	1616.4	-728.0	2833.5**	472.1	2928.5**	164.4
	(1544.6)	(1452.8)	(1253.6)	(1517.6)	(1261.0)	(1512.7)
$170k	798.4	-737.9	638.1	-1124.4	490.5	-1164.3
	(1381.9)	(1510.9)	(1538.2)	(1508.6)	(1539.7)	(1498.5)
$180k	635.2	84.5	4248.2***	1104.4	4019.8***	648.6
	(1602.3)	(1700.8)	(1550.7)	(1898.1)	(1534.3)	(1891.2)
$190k	2061.7	1931.9	1463.7	4398.3**	1527.3	4265.3**
	(1950.7)	(1890.7)	(1840.6)	(2010.7)	(1903.0)	(2002.8)
$200k	562.7	4673.0*	1096.8	5622.5**	847.9	5248.8**
	(2510.1)	(2741.2)	(2061.1)	(2520.2)	(2060.7)	(2491.0)
$210k	5809.1**	7359.9***	2173.8	6677.2**	2331.3	6326.3**
	(2407.3)	(2755.0)	(2497.0)	(2628.2)	(2441.6)	(2652.0)
$220k	2002.0	6624.3	1717.8	7736.3*	1365.9	7835.6*
	(3447.5)	(4684.0)	(2652.3)	(3995.3)	(2707.3)	(4007.1)
$230k	7205.7**	6812.4*	5550.2*	5097.6	5755.5*	4927.0
	(3244.4)	(3639.4)	(3059.4)	(3737.7)	(3027.3)	(3801.3)
$240k	970.1	6986.9	5967.3*	1542.4	5228.0	1632.4
	(3138.4)	(4610.9)	(3561.6)	(5002.3)	(3579.4)	(4969.8)
$250k	3563.1	11266.7*	6260.2	-2830.3	6227.1	-2623.6
	(6275.4)	(6596.1)	(4842.1)	(7664.6)	(4783.2)	(7780.3)
5th-Order Poly	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Zip Code Dummies	YES	YES	YES	YES	YES	YES
House Controls	YES	YES	YES	YES	YES	YES
Transaction Controls	YES	YES	YES	YES	YES	YES
Renovation Controls	YES	YES	YES	YES	YES	YES
Observations	8412	5172	8412	5172	8412	5172
R-squared	0.930	0.929	0.917	0.919	0.915	0.917

Table A4: Discontinuity Results by Franchise Non-Franchise Brokerages

Dependent Variable:	(1) Non-Franchise List Price	(2) Franchise List Price	(3) Non-Franchise Sales Price	(4) Franchise Sales Price	(5) Non-Franchise Net Price	(6) Franchise Net Price
Avg. Discontinuity	**3094.08**	**1514.80**	**2355.42**	**1794.49**	**2398.32**	**1534.62**
F-stat	**16.02**	**2.99**	**6.09**	**4.13**	**6.08**	**2.87**
Prob > F	**0.0001**	**0.0854**	**0.0138**	**0.0437**	**0.0139**	**0.0922**
$100k	-370.1	2180.7	-1666.2	3108.7	-1649.6	2919.0
	(1864.3)	(2374.7)	(1809.3)	(2221.0)	(1869.2)	(2255.9)
$110k	-732.3	2182.6	-1374.7	1282.0	-1388.2	1186.9
	(963.8)	(1379.2)	(956.0)	(1165.3)	(966.3)	(1179.8)
$120k	807.8	2578.5***	653.2	1233.8	605.3	1378.2
	(943.5)	(937.4)	(891.7)	(1208.4)	(882.6)	(1182.9)
$130k	532.1	1707.2	794.5	907.8	443.6	1105.7
	(829.3)	(1042.7)	(943.0)	(1069.7)	(979.5)	(1049.1)
$140k	415.1	346.1	1570.8	850.5	1733.3*	828.8
	(978.6)	(1103.8)	(963.0)	(1028.8)	(983.4)	(1023.8)
$150k	783.5	-362.8	948.7	222.7	930.0	60.4
	(1068.9)	(931.4)	(1066.0)	(1198.8)	(1117.7)	(1185.6)
$160k	1049.9	592.7	1918.4	2003.9	1990.5	1730.8
	(1447.3)	(1601.2)	(1316.2)	(1270.1)	(1324.5)	(1289.0)
$170k	1650.3	-1373.1	857.5	-1089.0	1042.7	-1641.0
	(1369.1)	(1590.7)	(1304.9)	(1848.8)	(1312.3)	(1821.1)
$180k	1150.2	-345.5	2893.3*	2737.0	2887.9*	1993.9
	(1688.0)	(1551.6)	(1689.7)	(1710.3)	(1698.3)	(1704.6)
$190k	4787.4***	-711.5	6019.3***	-765.1	6157.2***	-976.1
	(1602.6)	(2112.2)	(1757.7)	(2035.0)	(1763.0)	(2098.9)
$200k	4915.3*	-1241.6	2083.7	2865.7	2022.6	2400.9
	(2846.7)	(2012.3)	(2137.6)	(2271.0)	(2117.9)	(2223.6)
$210k	7967.6***	5442.2**	5721.0**	1693.0	5661.9**	1668.2
	(2443.8)	(2727.5)	(2674.9)	(2490.8)	(2585.0)	(2494.0)
$220k	7545.6*	-658.5	7318.9**	308.5	7455.3**	-139.1
	(3878.1)	(3443.4)	(3358.7)	(2851.0)	(3395.7)	(2924.6)
$230k	9122.2***	2704.3	4317.1	4518.1	4459.5	4478.4
	(3246.8)	(3497.1)	(3435.1)	(3110.9)	(3416.5)	(3114.9)
$240k	4278.9	3201.0	4441.2	4236.3	4254.9	3568.4
	(3796.2)	(3391.7)	(4162.2)	(4036.9)	(4185.7)	(3989.8)
$250k	5601.7	7994.4	1190.0	4598.0	1766.4	3990.5
	(6647.1)	(5348.6)	(7134.7)	(4943.0)	(7118.8)	(4877.1)
5th-Order Poly	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Zip Code Dummies	YES	YES	YES	YES	YES	YES
House Controls	YES	YES	YES	YES	YES	YES
Transaction Controls	YES	YES	YES	YES	YES	YES
Renovation Controls	YES	YES	YES	YES	YES	YES
Observations	7396	6188	7396	6188	7396	6188
R-squared	0.929	0.931	0.916	0.920	0.914	0.918

Table A5: Main Discontinuity Results, 7^{th} Order Polynomial

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	List Price	List Price	Sales Price	Sales Price	Net Price	Net Price
Avg. Discontinuity	**3492.65**	**2332.59**	**3128.72**	**2144.74**	**3069.98**	**2045.18**
F-stat	**8.95**	**14.70**	**8.51**	**10.97**	**8.28**	**9.57**
Prob > F	**0.0029**	**0.0001**	**0.0036**	**0.0010**	**0.0041**	**0.0020**
$100k	4167.9*	592.6	3487.1	543.7	3398.2	479.4
	(2522.7)	(1470.1)	(2298.0)	(1395.3)	(2260.9)	(1427.4)
$110k	3545.0	1256.0	1812.4	4.52	1564.4	-28.9
	(2343.5)	(923.0)	(1786.7)	(887.1)	(1746.2)	(887.0)
$120k	4806.7***	2041.4***	3375.4**	987.7	3294.8**	1050.1
	(1385.9)	(620.8)	(1369.9)	(797.1)	(1361.6)	(786.2)
$130k	5027.7***	1182.7*	4344.8***	1038.2	4451.9***	943.0
	(1649.8)	(662.6)	(1348.4)	(684.7)	(1333.6)	(704.4)
$140k	2515.7	73.7	4066.3***	1289.2	4305.8***	1374.9*
	(1747.4)	(853.5)	(1392.1)	(815.7)	(1372.4)	(823.5)
$150k	381.1	-293.7	1485.7	653.7	1555.1	570.1
	(1987.3)	(899.9)	(1789.7)	(883.7)	(1773.8)	(909.2)
$160k	-252.5	548.4	2247.8	2044.3**	2213.6	1965.1**
	(2019.3)	(1057.5)	(1594.7)	(944.4)	(1575.1)	(954.7)
$170k	-2193.8	362.0	-1852.3	85.5	-1965.5	-27.2
	(2328.9)	(1074.3)	(2129.5)	(1157.2)	(2103.4)	(1159.7)
$180k	-343.7	759.2	2243.4	2861.6**	1798.5	2539.5*
	(2671.9)	(1392.2)	(2422.1)	(1369.3)	(2405.6)	(1367.6)
$190k	-1457.2	2860.7*	-254.8	3030.8*	-302.4	2992.0*
	(3184.3)	(1673.3)	(3118.4)	(1554.8)	(3078.9)	(1574.1)
$200k	4743.7	2611.6	1847.5	2469.2	1622.3	2222.5
	(4270.3)	(2084.4)	(3652.4)	(1673.1)	(3589.4)	(1660.3)
$210k	5584.7	6851.8***	3625.0	3889.6**	3570.4	3814.7**
	(4118.5)	(1823.2)	(3826.3)	(1862.0)	(3823.3)	(1834.6)
$220k	5399.0	3239.4	6156.5	3767.4	6254.7	3630.2
	(5171.7)	(2811.3)	(4191.4)	(2334.2)	(4181.5)	(2371.8)
$230k	2244.1	5317.2**	1216.7	4362.6	1338.8	4345.4
	(5117.3)	(2670.4)	(5282.3)	(2719.2)	(5244.3)	(2716.2)
$240k	5883.5	2307.2	7397.6	3956.4	6960.8	3523.4
	(5445.1)	(2977.3)	(5284.5)	(3214.3)	(5244.6)	(3235.2)
$250k	15830.3**	7611.2*	8860.4	3331.5	9058.3	3328.8
	(7031.9)	(4566.2)	(6650.4)	(4455.3)	(6651.4)	(4428.0)
7th-Order Poly	YES	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES	YES
Zip Code Dummies		YES		YES		YES
House Controls		YES		YES		YES
Transaction Controls		YES		YES		YES
Renovation Controls		YES		YES		YES
Observations	16112	13584	13673	13584	13667	13584
R-squared	0.693	0.929	0.711	0.917	0.713	0.915

Note: Models are estimated using OLS with errors clustered by agency. The bolded lines present a joint significance test for whether all estimated discontinuities are jointly statistically different from zero. Time controls include dummies for year and month of the current sale as well as the logged number of days between sales. House, transaction, and renovation controls listed in appendix Table A1. * p<0.1, ** p<0.05, *** p<0.01.

Table A6: Performance Results

	(1)	(2)	(3)
Dependent Variable:	Days on Mkt	Days on Mkt	Days on Mkt
Avg. Discontinuity	**1.147**	**0.635**	**2.909**
F-stat	**0.25**	**0.08**	**1.76**
Prob > F	**0.6189**	**0.7771**	**0.1852**
$100k	-1.62	-2.88	-2.87
	(5.70)	(5.56)	(5.73)
$110k	-2.93	-2.89	-0.27
	(3.97)	(3.92)	(4.02)
$120k	1.40	1.07	1.42
	(4.32)	(4.43)	(4.27)
$130k	-2.64	-2.20	-0.29
	(3.34)	(3.28)	(3.32)
$140k	1.17	0.83	1.53
	(3.50)	(3.34)	(3.24)
$150k	1.73	1.27	4.89
	(4.02)	(3.83)	(3.87)
$160k	-1.32	-2.48	0.26
	(3.84)	(3.77)	(3.83)
$170k	7.24*	6.75	11.7***
	(4.32)	(4.35)	(4.23)
$180k	-0.18	0.46	4.34
	(5.41)	(5.34)	(5.00)
$190k	-3.44	-4.53	7.38
	(5.30)	(5.34)	(5.05)
$200k	-2.82	-3.00	0.73
	(5.12)	(5.05)	(5.25)
$210k	-5.37	-4.92	8.71
	(6.55)	(6.44)	(7.09)
$220k	-0.36	-1.17	1.54
	(7.25)	(7.09)	(7.66)
$230k	-4.05	-5.24	-5.78
	(6.98)	(6.89)	(7.17)
$240k	7.03	5.80	0.071
	(9.14)	(8.74)	(8.19)
$250k	24.5**	23.3**	13.2
	(10.7)	(10.3)	(11.0)
5th-Order Poly	YES	YES	YES
Time Controls	YES	YES	YES
Zip Code Dummies		YES	YES
House Controls		YES	YES
Transaction Controls		YES	YES
Renovation Controls			YES
Observations	16101	16101	13577
R-squared	0.076	0.094	0.102

Note: Models are estimated using OLS with errors clustered by agency. The bolded lines present a joint significance test for whether all estimated discontinuities are jointly statistically different from zero. Time controls include dummies for year and month of the current sale as well as the logged number of days between sales. House, transaction, and renovation controls listed in appendix Table A1. Days on market is the difference between original listing and close date. Failure is defined as a home being listed but not selling within the original listing contract timeline. * p<0.1, ** p<0.05, *** p<0.01.

Table A7: Correlations and Means in Pretest

	1	2	3	4	5	6
1. Est. Listing Price	1.00					
2. Age	0.00	1.00				
3. Female	0.00	0.10 **	1.00			
4. Purchased Real Estate	0.01	0.32 **	0.07 *	1.00		
6. Estimated Quality	0.33 **	-0.03	-0.16 **	-0.01	-0.07	
7. Estimated Location	0.24 **	0.06 *	-0.13 **	0.02	0.06 **	1.00
Mean	282,089	34.9	0.41	1.57	75	70
Median	205,000	31.0	0.00	2.00	80	75

** Correlation is significant at the 0.01 level (2-tailed).

Note: N= 1,422 Person-House Estimates. Purchased Real Estate denoted by a dummy variable.

Table A8: Pretest Estimated List Price by Condition and House

Previous Sale Price Position Relative to Round Number	House 1 Previous Sale Price	House 1 Estimated Listing Price *M*	*SD*	*N*	House 2 Previous Price	House 2 Estimated Listing Price *M*	*SD*	*N*
Slightly Above	151,000	184,564	95,912	98	201,300	222,474	60,938	95
Slightly Below	149,000	166,849	70,238	97	198,700	204,597	28,549	89
Well Below	147,000	168,088	71,419	90	196,100	206,821	47,700	96
Total		173,332	80,465	285		211,425	48,442	280

Previous Sale Price Position Relative to Round Number	House 3 Previous Price	House 3 Estimated Listing Price *M*	*SD*	*N*	House 4 Previous Sale Price	House 4 Estimated Listing Price *M*	*SD*	*N*
Slightly Above	301,500	318,914	55,921	95	291,000	173,686	63,764	96
Slightly Below	298,500	311,101	48,784	97	289,000	177,603	73,340	93
Well Below	295,500	311,033	41,860	90	287,000	173,582	64,093	94
Total		313,711	49,274	282		174,939	66,953	283

Previous Sale Price Position Relative to Round Number	House 5 Previous Price	House 5 Estimated Listing Price *M*	*SD*	*N*
Slightly Above	601,600	542,579	66,426	95
Slightly Below	598,400	541,282	61,956	91
Well Below	595,200	535,858	66,959	94
Total		539,901	65,023	280

Note: Table displays 289 participants' estimated listing prices for each of the three conditions for each of the five houses in the pretest. Just above represents previous sale prices that are up to $1,600 above the round number, just below represents previous sale prices that are as low as $1,600 below the round number, and well below represents previous sale prices that are more than $1,600 below the round number.

Table A9: Linear Cross-Nested Mixed Models in Pretest

Dependent Variable:	All Participants (1) Z-Score of Estimated Price	All Participants (2) Estimated Price
	Coefficient Estimate	Coefficient Estimate
Intercept	-0.0826 (0.0543)	283364.7*** (62,246.0)
Just Above (a)	0.00583* (0.0264)	6,527.6* (3,024.6)
Just Below (b)	-0.0039 (0.0267)	-332.3 (3,065.2)
Discontinuity Test		
Wald (a-b>b-0)	X^2 = 2.03 (p=0.154)	X^2 = 1.83 (p=0.176)
Log Likelihood	-905.5	-17,354
Likelihood Ratio Test (vs. OLS)	X^2 = 1517.4 (p=0.000)	X^2 = 2,936.4 (p=0.000)
# Participants	289	289
# Houses	5	5
Observations	1,410	1,410

Note: This table presents mixed model (HLM) results from the pilot study, with five houses cross-nested with 289 participants. *Just above* represents previous sale prices that are up to $1,600 above the round number, *just below* represents previous sale prices that are as low as $1,600 below the round number, and *well below* represents previous sale prices that are more than $1,600 below the round number. The omitted category is well below. The formal tests of the discontinuity are Wald tests, which are consistent with a large discontinuity but under-powered.