Anchoring on Historical Round Number Reference Points: Evidence from Durable Goods Resale Prices

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Scott S. Wiltermuth*
University of Southern California

Timothy Gubler*
Brigham Young University

Lamar Pierce
Washington University in St. Louis

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*Denotes authors contributed equally.

ABSTRACT

This paper examines how people price the resale of durable goods in systematically biased ways. We show across four studies that the anchoring effect of durable goods' prior sales prices on subsequent valuations is discontinuous at psychologically-salient round number reference points (e.g., $10,000 increments) because these numbers create qualitative differences in how people perceive values below them vs. values at/above them. Resellers set disproportionately larger subsequent prices when previous prices move from just below round-number thresholds (e.g., $349,000) to those at or just above these thresholds (e.g., $351,000). The findings show that buyers who pay a price just *below* a round number therefore may sacrifice money because they receive disproportionately less when reselling the good. Market forces only partially attenuate this pricing bias, but valuator experience seems to play moderating role. Archival data shows that home buyers who previously paid just under a $10,000 reference point subsequently listed their homes for about 1.8 percent (over $3700) less on average than did buyers selling comparable homes who previously paid at or above a round number threshold. This drop is observable controlling for home characteristics and the general relationship between previous and current prices. Three experimental studies looking at housing and used car markets replicate these findings, highlight the mechanism, and increase confidence in causality. Market mechanisms and the negotiation process attenuate discontinuities by about 30%, but lower initial *listing* prices persist to final *sales* prices. We find additional weak evidence suggesting valuator experience may attenuate intergenerational pricing bias.

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

1. INTRODUCTION

People often buy durable goods that they will end up selling later. People resell real estate, vehicles, heavy machinery, electronics, luxury clothing, art, and more, creating large and growing resale markets with dedicated platforms, firms, and other organizations (e.g., Bain and Company 2018, U.S. Department of Housing and Urban Development, 2021). In many of these markets, people know (or can reliably estimate) the original sale price of the good. People buying and selling real estate can access previous prices through their agents or through the Multiple Listing System. Similarly, people looking to buy or sell used vehicles can make reasonable estimations by looking to the original manufacturers' suggested retail prices (MSRP) for those vehicles.

Researchers have established that these previous prices can influence the prices at which people subsequently list those goods (e.g., Bucchianeri and Minson 2013, Diekmann et al., 1996; Einiö et al., 2008; Janiszewski & Uy 2008). In this paper we argue that our collective understanding of how people use information about past sales to set prices is incomplete in an important way. We propose specifically that people can better understand and predict the relationship between past prices and current prices if they account for the fact that round numbers, such as $10,000, often serve as cognitively-accessible reference points that create qualitative differences between how people perceive outcomes below the reference point and how they perceive outcomes at or above the reference point. In other words, we suggest that it matters disproportionately whether a previous sales price reached a round-number threshold. We expect jumps in subsequent sale prices when previous prices move from just below round-number thresholds (e.g., $349,000) to just above those thresholds (e.g., $351,000), and we expect those jumps to be bigger than those produced when prices move the same amount but do not cross thresholds (e.g., $351,000 to $353,000). We further argue that characteristics of the agents and principals involved in resales can attenuate the size of this discontinuity.

Understanding the dynamics introduced by round numbers is important in large part because 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, that 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 $349,000 rather than at round prices like $350,000 (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 and 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 examine whether buyers who pay amounts that fall just under round numbers may consequently create disproportionately large sacrifices in future resale prices, such that those who eventually resell their goods might be better off paying *more* when doing so elevates the initial negotiated sales price to just above a round number. We test whether this bias will remain uncorrected by the market or negotiation process and consequently create economically meaningful market inefficiencies. Finally, we test whether that market experience, which exposes an 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). Such a result would be consistent with List's (2003) arguments that market experience can attenuate biases. Knowing the effect of valuator experience on the strength of the bias would be important, as even relatively inexperienced sellers and buyers could access this sort of market experience by employing expert intermediaries (e.g., Teece 2003) if they lack the time to acquire experience themselves.

This bias is likely to operate in a variety of contexts in which previous prices are known (e.g., auctioned items, durable goods), but not in markets in which previous prices are unknown. We first test our arguments using data from the housing market. We examine 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, ranging from \$2898 to \$7678 in our main results (approximately 30-75% of the \$10,000 discontinuity or 1.8-3.8% of the mean original home listing price). A buyer can thus enjoy a future average return of over 289% if they cross a \$10,000 threshold by paying \$1000 more.

Market mechanisms attenuate but do not fully correct this price anchoring—discontinuities are observable in initial list price as well as in final negotiated sales prices. However, we see some signs that listing agent experience may attenuate the pricing bias. This likely stems from experienced listing agents being exposed to a larger set of anchors than less experienced agents are, which would attenuate the impact of previous sale price as an anchor on initial listing prices. Finally, we explore the potential role of organizational support given to real estate agents in reducing bias using exploratory post hoc analyses. These results provide only weak evidence suggesting that larger or franchise brokerages avoid intergenerational pricing bias more than smaller or non-franchise brokerages.

One obvious concern with our identification is the endogenous selection in prior sales prices based on some unobservable personal or property characteristic. We address this in two ways. First, we explain that the stability of our coefficient estimates across models, combined with our very high R^2 (0.941), suggest that any bias from omitted variables would be very small (Oster 2019). Second, we support the 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. We also conduct an experiment showing that the position of new vehicles' manufacturers' suggested retail prices relative to round number reference points can disproportionately influence the valuation of the cars after they have been used for several years. These experiments and the logic from Oster (2019) collectively suggest that our main effects are highly unlikely to be driven entirely by unobservable characteristics of the property, buyer, or seller on either side of the threshold.

Our work contributes to theory in a few important ways. We add to previous work on pricing in resales of goods and to the 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) by showing how round number reference points from the past disproportionately influence future anchoring and pricing in negotiations when past prices are known. The combination of the anchoring-and-adjustment heuristic and the disproportionate influence of these reference points on judgment can lead negotiators to make suboptimal decisions. This likewise adds to cognitive negotiation theory (Neale and Bazerman 1991) by demonstrating how motivations to achieve goals within negotiations can affect outcomes in subsequent negotiations.

The paper also shows 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, Teece 2003). However, our results contribute to the growing literature on the role of human capital and experience in avoiding biases (e.g., Choudhury, Starr, Agarwal 2020; List 2003; Sutcliffe and McNamara 2001), and more generally to literatures on human capital investments and learning through individual or peer experience (e.g., Becker 1962; Chan, Li, Pierce 2014; Greenwood et al. 2019; Simon 1991), by showing the role agent experience plays in avoiding intergenerational pricing biases. This highlights an underexplored but important benefit of human capital in knowledge-intensive industries. It also contributes to the literatures around learning in organizations from experience (e.g., Simon 1991, Huckman and Pisano, 2006; Lawrence 2018; Thornton & Thompson, 2001), by showing the potential benefit experienced employees might play in reducing organization-level biases.

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 (Simon, 1991) 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

Residential real estate provides the setting for Studies 1, 2, and 3 in this paper. Most 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, listing and buyer agents, and brokerage identities, and public comments regarding the unique features and condition of homes. In the state of Utah, full access to the MLS is limited to licensed real estate agents and brokers in each 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 using comparable homes 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 ostensibly seek to close the sale at the lowest possible price. While agents should act with fiduciary responsibility to their respective clients, previous work has found this does not always happen (e.g., Levitt and Syverson, 2008). Agents may be motivated, for example, to minimize transaction times to generate sales volume. 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 and analysis 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 by home buyers and sellers, although additional concessions can be made through closing costs to homebuyers by sellers, which are not reflected in the final sales price. In our sample, the final sales price is typically lower than the original list price by about five percent, although this varies over time depending on market conditions.

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). Anchors exert this influence on negotiators' behavior, in part, by increasing the cognitive accessibility of information consistent with the anchor (e.g., Mussweiler, 2002; Mussweiler & Strack, 1999). An anchor therefore leads negotiators to consider information selectively and to make offers closer to that anchor than they otherwise would have.

Previous studies in real estate have also established a relationship between prior and future sales prices for properties, both due to the underlying common value of the property and 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). Previous work on anchoring has largely assumed a linear relationship between previous price paid and subsequent price, with two exceptions. First, research on loss aversion has shown that people are reluctant to sell properties for less than they originally paid for them (Diekmann et al., 1996; Einiö et al., 2008; Graddy et al. 2014). Second, research has shown that precise number anchors exert stronger anchoring effects than round number anchors (Janiszewski & Uy, 2008), which can be helpful for sellers but can also lead potential buyers to refrain from entering bids because they see the precise prices as less flexible (Lee, Loschelder, Schweinsburg, Mason, & Galinsky, 2018).

Our paper examines whether there is an unidentified source of discontinuity in the anchoring effect that previous prices exert on subsequent prices. Instead of comparing round prices with precise prices, we examine whether discontinuities can arise because round numbers influence perceptions of value in meaningful ways, such that previous prices that fall just below round number reference points yield disproportionately lower subsequent valuations than numbers that are at or above the threshold.

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). In other words, they can feel like milestones or goals (Heath, Larrick, & Wu, 1999), such that people who pay prices that fall below them may consider their purchases more successful than people who pay above them. They may also consider them to be qualitatively different prices, even when the differences between the numbers are small.

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 than they are by odometer changes that do not change the 10,000 digit (e.g., DellaVigna 2009, Lacetera et al. 2012). This attention to left-side digits largely explains why properties are often originally listed at prices like $189,000 instead of $190,000 (e.g., Allen and Dare 2004). We note that the scale of the prices determines which digit will likely serve as an important round number reference point. Because real estate buyers and sellers are largely negotiating on the scale of thousands or tens of thousands of dollars, $10,000 might be the most salient threshold, with potential secondary effects larger intervals such as $100,000. In contexts in which prices are lower, increments of $1,000, $100, or even $10 may be viewed as round benchmark figures.

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, people may see a house that *previously* sold for $260,000 as disproportionately more valuable than a house that previously sold for $259,000. That is, they may see smaller differences in value between a house that previously sold for $260,000 and one that previously sold for $261,000 than they would when the price difference is between $259,000 and $260,000. People's focus on the (second) left-most digit in such a case should lead both the seller and the potential buyers to perceive the house that previously sold for $259,000 as having lower value than if that house had previously sold for $260,000, even if they cannot identify or infer differences in the two houses.

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 therefore expect that the relationship between previous sales price and subsequent list price is discontinuous at round numbers (i.e., multiples of $10,000). Our first hypothesis follows:

Hypothesis 1: 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 fully 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. This leads to our second hypothesis:

Hypothesis 2: Pricing discontinuities at round numbers for property listing prices will also manifest in final sales prices.

This prediction may appear inconsistent with research showing that properties actually sell for *more* when their current 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 sales prices because buyers rarely screen on previous prices, as it is not possible using the filters of any of the top five real estate websites.

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. This is similar to previous arguments about the role of domain expertise and experience in reducing bias through drawing on relevant outside information (Choudhury, Starr, Agarwal 2020). 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. Experienced agents' heightened negotiation experience may likewise mitigate the anchoring effect (cf. Cardella and Seiler 2016). Finally, experienced agents may have learned over time how sellers, buyers, and agents typically approach listing and buying homes. We posit specifically that listing agent experience is likely to be helpful in setting the initial listing price.

We therefore believe that experienced listing agents may have at least two possible sources of resistance to this pricing bias. First, their exposure to a greater number of other possible anchors, such as the prices of other similar homes, will diminish the influence of the previous sales price as an anchor. Second, they may have developed processes that allow them to minimize the influence of the previous sale price on the listing prices they suggest to potential sellers. We also believe that exposure to more anchors and having more developed pricing processes may lead experienced listing and buyer agents to move more off of anchors established by counterparts' offers within the negotiation. Our last hypothesis follows:

Hypothesis 3a: Listing agent experience will moderate pricing discontinuities, such that discontinuities are smaller for agents with high experience than for agents with low experience.

Hypothesis 3b Buyer agent experience will moderate pricing discontinuities, such that discontinuities are smaller for agents with high experience than for agents with low experience.

Based on our conversations with several experienced real estate agents, we do not believe that agents are aware of this source of bias. None of the agents with whom we spoke indicated awareness of it.

4. OVERVIEW OF STUDIES

We propose that round numbers introduce discontinuities in the anchoring effect of prior sales prices on subsequent list and sale prices of durable goods. We test our ideas through four studies, three of which are set in the context of residential real estate. Study 1 examines whether the position of previous sale prices relative to round numbers disproportionately affects subsequent home list and sale prices. The study also tests whether agent experience and organizational support influences the size of observed discontinuities. In Study 2 we tested causality by manipulating the previous sale price of five houses profiled in materials provided to participants, and asking participants to provide the listing price that they would ask for each property. Study 3 tests whether changes in previous sale price that span round numbers generate larger changes in the perceived likelihood that the house would have luxury amenities than equivalently large changes in previous sale prices that do not span round numbers. This study aims to provide evidence of how round number benchmarks influence anchoring by leading individuals to generate selectively different information about houses below vs. at or above the round number reference point. (cf. Mussweiler & Strack, 1999). Study 4 examines the generalizability of the effects, by testing whether round number benchmarks also influence subsequent estimated list prices of used cars.[1] It also provides the most precise and causally valid test in a design where participants are incentivized to estimate future sales prices accurately.

[1] We note that we conditioned one additional experimental pilot that tested whether the discontinuities we observed in Studies 1 – 4 also exist in the relationship between wholesale hardware prices and retail hardware prices. For a

5. STUDY 1: ARCHIVAL STUDY

5.1 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 and the negotiation process or agent experience attenuate these discontinuities. Our archival data are drawn from two major counties in Utah for 1996-2014. These were the two counties for which we were granted data access and we know of no reason why we would be more likely to find effects in these counties than in any others. 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 these counties 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 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 $310,000, 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 results with different cutoffs in robustness tests in the appendix (see Figure A4). Second, we drop the top and bottom one percent of homes by price appreciation (2,185 homes), which likely reflects unobservable significant events, such as fire, flood, mold, murders, suicides, significant renovations, or

variety of reasons involving the variance of responses, our use of drop down menus, and the tendency of participants to provide round prices, the study did not work. We nonetheless provide the registration for this study in order to be transparent: https://aspredicted.org/DFH_2YT.

large location-specific changes to the desirability of an area or home. Finally, we omit homes that are either short sales or bank owned (6,875 homes), as these homes undergo a different sales process, including auctions, and thus receive extra scrutiny on listing and final sales prices. Our final sample includes 83,164 unique home relistings. Of these, 69,078 (83%) were eventually resold.

5.2 Identification Strategy and Model

The ideal identification strategy to test our hypotheses would randomly assign final sales prices to comparable homes around $10,000 round number price thresholds. We would then estimate the causal effect of these thresholds on future listing and sales prices. While our setting has many strengths, the sales price data presented in Figure 1 show non-random assignment of homes around $10,000 thresholds. This is expected given charm pricing and the propensity for price rounding with large numbers. However, this would be most problematic to identification and lead to selection concerns if certain types of houses systematically fall just above or below a round number threshold, and the model does not account for these differences. Figure 2 provides data on this possibility for our sample. It shows that predicted sales prices are largely similar for homes that fall just above and just below the $10,000 price thresholds (labeled as 00 on the X axis). Figures A1 and A2 (appendix) show similar results. Homes that fall just above and below the thresholds are similar in square footage and in the concessions provided by a seller to a buyer at closing.

More granular balance tests are presented in Table A1 (Appendix). These tests compare homes that fall within $500 of each round number threshold (9,735 total homes within $500 above and 6,134 total homes within $500 below). These again suggest that homes on either side of the threshold are similar, but also show small differences on observables between homes around some thresholds. We explicitly control for these differences in our empirical model and supplement with two experiments to reduce selection concerns. Over 26 percent of homes in our sample are within $1000 of a $10,000 round number threshold.

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 (RD) 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). Like Pope et al. (2015) and Englmaier et al. (2017), we do not employ a formal regression discontinuity model. Since random assignment near the threshold is a crucial identifying assumption, and we cannot claim random assignment but must instead control for it, any causal claims from an RD model would be inappropriate.

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., \$259,000 vs \$261,000) than they are for homes that did not (e.g., 257,000 vs \$259,000). The model fundamentally estimates whether there is a jump in future listing and sales prices at each \$10K threshold by comparing homes within \$10,000 above each price threshold to those \$10,000 below the threshold, while netting out the "typical" influence of previous sales prices on future sales prices (using a flexible polynomial function for previous sales price) and controlling for observable home and market characteristics in both periods. The main test is whether we see disproportionate jumps at each \$10,000 price interval jointly.

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 (e.g., fixer-upper, great views, vaulted ceilings, etc. See Table A2 for the variable list). Finally, we control for observable upgrades since the prior sale. Our 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. We include house, transaction, and renovation controls in our analyses (see appendix Table A2 for the list of control variables) to help account for differences between homes, including since last 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 are estimated using OLS and conservatively estimate robust standard errors clustered by real estate agency to account for commonalities within real estate brokerages. We use extensive time controls for listing year and month to control for time effects, including the 2008 housing crisis.

As noted above, identifying round-number anchoring in our model relies on the assumption that any 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 model r-squared values (r-squared > 0.9 for the fully-controlled models). As Oster (2019) notes, parameter estimates that do not change as observable controls are added, combined with a high r-squared, suggest that omitted variables are unlikely to significantly bias estimates. 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

[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 (Englmaier et al. 2017).

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. However, to reduce selection concerns, we also present experimental results in future sections.

5.3 RESULTS

5.3.1 Main Model Results

Table 1 presents descriptive statistics for the primary variables in our analyses. Figure 3 presents the main result from the raw data at each \$10,000 price threshold, in line with our estimation strategy. While these figures provide evidence suggestive of Hypothesis 1, they do not include any controls. Table 2 presents the main results for the models specified above. 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 twenty-one discontinuity coefficients presented below it, along with an F-statistic for the Wald test that these twenty-one coefficients are jointly different from zero. Column (2), the fully-specified list price model, finds a large and precise average discontinuity of \$3,764. 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 \$3,764 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. The estimated effect is robust across the two models, providing support for Hypothesis 1. Sellers and their agents anchor on round numbers in previous sale prices when setting prices for newly listed homes. These results imply that homebuyers are heavily penalized in future sales by paying just below a \$10,000 threshold.[3]

[3] We note that the pricing discontinuities appear larger in the partially controlled model (i.e., column 1) for prices near the tails of the distribution, and they appear larger for the fully controlled model (i.e., column 2) for prices at the top end of the price distribution. This could be driven by the relative thinness of the data at the high and low end of the price distribution as well as by lower levels of price sensitivity for higher net worth sellers.

Column (4) presents the fully-specified model for final home sales prices, which finds an average discontinuity of $2,897. We emphasize that this coefficient is large and economically meaningful. It implies a future average return of 289% if a buyer crosses a $10,000 threshold by paying $1000 more. This estimated sales price discontinuity is about 30% less than the list-price effect (Wald test of equality p = 0.02), suggesting that the market attenuates but does not fully correct for pricing discontinuities.[4] Regressions using the net price yield similar results (Columns 5 and 6). Collectively, these base models provide strong evidence that real estate agents and their clients anchor on prior sales prices when listing a home, and that this anchoring effect is not fully eliminated by the market through subsequent negotiations. In other words, market forces do not overcome behavioral bias. This supports Hypothesis 2.

5.3.2 Results on Agent Experience

We next examine whether listing 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 listing agent experience cut at the median.[5] The fully controlled models are found in columns 1 and 2 of Table 3. We find that the original list price discontinuity is about 46% larger for listing agents with low experience compared to high experienced listing agents. However, the p-value on this difference did not reach significance at conventional levels (p = 0.1099). This difference again is not fully corrected by the market, as shown in the sale price and net price models. Thus, the bias appears larger for inexperienced listing agents compared to experienced agents, but our confidence in the results is limited due to the statistical power requirements of jointly testing forty-two coefficients from two separate models.

The buyer agent experience results are found in Table 4. For these analyses we present the sales and net price models only, as buyer agents have little influence over listing prices. The discontinuities for the fully controlled net price models, shown in columns 3 and 4, are approximately $763 larger for more

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

[5] 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 twenty-one discontinuities. We are insufficiently powered for such a model.

experienced buyer agents than for less experienced buyer agents, but the results are not identifiably different from each other ($p = 0.37$). 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 pricing in a limited way through the negotiation process. Altogether, we find only weak support for Hypothesis 3a, and no support for Hypothesis 3b.

5.3.3 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, Coldwell 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 to help cover the supported services. 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. Smaller firms have fewer support services, but potentially stronger incentives for each agent to get the pricing right because the smaller firm often leaves more of the commission to agents. Our results, shown in Table A3, show only weak evidence suggesting a difference between large and small firms. Column 1 shows a discontinuity that is 45% larger for large brokerages compared to small brokerages, but this difference is not statistically significant (p = 0.186) and the discontinuity difference between large and small brokerages decreases for sales and net prices. Interestingly, we find that the initially larger discontinuities are attenuated more by the market for large brokerages than for small brokerages. The market reduces pricing discontinuities by $1,023 for small brokerages (p = .0157) and by only $395 for small brokerages (p = 0.506).

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 similar discontinuities between franchised and non-franchised brokerages (p-values > 0.74 for all price differences). This pattern of results suggests that organizational support does not significantly attenuate intergenerational pricing bias from the anchoring-and-adjustment heuristic. These results are consistent with prior research (e.g., Levine & Prietula 2014, Sutcliff & McNamara 2001), which document that efforts to combat biased decision-making across organizations often fail to produce lasting effects.

5.3.4 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 used 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 over the $10,000 price interval. 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 A3. Two patterns are observable. First, discontinuities are evident at not only $10,000 intervals, but also potentially 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. Columns 1-3 of Table A6 suggest that homes above round number thresholds may stay on the market slightly longer (~1.5 days on average), although the difference is only marginally significant (p-values range from .085 to .125) and the magnitude is unlikely to explain our estimated effect size. Columns 4 and 5 show that the probability of a failed sale does not seem to change for homes just above or below the thresholds. Together these results suggest that agents and sellers may be slightly more patient in selling homes that previously sold just above a round number threshold. This is consistent with the anchoring-and--adjustment heuristic explanation for the main results in the paper, as agents and sellers see homes that previously sold just above a round number threshold as relatively more valuable.

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 $40,000 and $360,000. Figure A4 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.

Fifth, we tested for the presence of additional pricing discontinuities at the $100,000, $50,000, and $5,000 round number price thresholds. Using our fully controlled model we found similar discontinuities to the $10,000 thresholds for the $50,000 and $100,000 thresholds (p-values = 0.344 and 0.382 respectively for original list price and p-values = 0.795 and 0.730 respectively for sale price). At the $100,000 price threshold, we found an average discontinuity of \$2894.45 for original list price (p-value = 0.014) and \$3215.02 for sale price (p-value = 0.004). At the \$50,000 price threshold, we found an average discontinuity of \$3218.61 for original list price (p-value = 0.000) and \$3033.37 for sale price (p-value = 0.000). For the \$5,000 thresholds, we found an average discontinuity of \$3308.66 for original list price (p-value = 0.000) and \$2797.02 for sale price (p-value = 0.000). Interestingly, the \$5,000 threshold discontinuities are distinguishable from the \$10,000 thresholds (p-values = 0.004 for original list price and 0.114 for sale price), with smaller effects at the \$5,000 thresholds (-\$1475 for original list price and -\$977 for sale price).

Sixth, we investigated how time since last sale affected our estimated price discontinuities. To do so we reran our main analyses on subsamples of the data, split by time since last sale. In the results presented in Table A7, which splits the data into thirds based on time since last sale, we find no strong evidence that the price discontinuities attenuate with time. The discontinuities persist and are quantitatively similar for each of the time since last sale subsamples. We speculate that this may be driven by the limited price inflation in the Utah real estate market during our sample time period, which muted potentially attenuating effects from time, as well as the relatively short time to resell in our data.

Finally, we note that our results on listing agent experience suggest the discontinuities are driven by the anchoring-and-adjustment heuristic rather than by agents' strategic behaviors. If listing agents were strategically pricing based on heuristics in prior sales prices, we should observe more experienced listing 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. Our discussions with real estate agents likewise suggest that they are unaware of this anchoring-and-adjustment bias.

6. STUDY 2: HOUSE PRICE EXPERIMENT

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 listing or buyer 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 and consequent selection issues, 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), the passage of time between sales, and the robustness of our models across 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. Pre-test 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. The pre-registration document, which includes our names, for the pre-test is available upon request. We will provide the link within the document if/when the paper is accepted for publication.

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.[6] Finally, we added a Captcha screen to our survey and ran 164

[6] 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

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.

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. Participants viewed houses from a range of conditions.

6.2 Results

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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.

Per our preregistration, 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 (i.e., created z-scores) 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-effects models (Gelman and Hill 2006, Kenny et al. 2006) to account for interdependence of data around both participants and houses. These models, also referred to as hierarchical linear models or multilevel models, estimate independently distributed random effects for both the participants and the questions.

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).

Results were similar for those participants who have purchased property. As Column 2 shows, 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 ($p_{\text{Test 1}} = .027$). The comparison with the above-threshold condition was significant ($p_{\text{Test 2}}$ =. 023), but the comparison with the below-threshold condition did not reach significance ($p_{\text{Test 3}} = .120$). The condition did not affect estimates of housing or location quality (*p*s > .35). Effects similar to those in Columns 1 and 2 are shown in Columns 3 and 4 of Table 7, each of which uses unstandardized estimated prices as the dependent variable.

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. STUDY 3: SELECTIVE ACCESSIBILITY

Mussweiler and Strack's (1999) selective accessibility model of anchoring holds that people anchor in part because anchor values lead people to selectively generate knowledge consistent with the anchors provided. Their model suggests that potential home buyers are likely to generate more knowledge consistent with the house being of high quality and therefore expensive if they see previous house sale prices that are at or above round number reference points than if they see previous sale prices that are below those round number reference points.

We therefore examine in Study 3 whether previous sale prices that fall just above round number reference points lead people to selectively generate more knowledge that is consistent with the idea that the house is more valuable than homes with previous sale prices that fall just below round number reference points. We also examine whether these differences exceed differences produced by equivalently large differences in sale prices that do not involve crossing a round number threshold. Finding such patterns would be consistent with round number reference points affecting the anchoring process of potential home sellers and buyers.

7.1 Participants and Method

A total of 576 undergraduates (44.1% female; Age: *M* =20.40, *SD* = 2.07) at a large university on the West Coast of the United States participated in the study in exchange for course credit. We designed the study to test whether $2,000 changes that crossed the $400,000 benchmark produced larger changes in the likelihood of participants selecting high-quality characteristics of houses than $2,000 changes that did not cross a benchmark. The pre-registration of the study is available at: https://aspredicted.org/NXG_TJ3. We pre-registered that we would exclude submissions completed in less than 80 seconds, a criterion that was based on a longer version of the survey we designed but did not administer.[7] Participants completed the study in a median time of 84 seconds. As this rule eliminated nearly half the participants, we report results with no exclusions as well as those excluding all those who took less than 80 seconds. We note that the patterns and levels of significance remained the same regardless of these exclusions. Data appear at: https://osf.io/6yvws/files/.

We showed participants a picture of a house and instructed them to imagine that a 1,700 square-foot, three-bedroom, two-bath house in San Antonio had previously sold for a given price. We instructed participants that, “Houses at this price point in this neighborhood in San Antonio tend to have certain features. Below we show how likely it is that houses at this specific price point have the features listed below.” The graphic indicated that homes at that price point had a 76% chance of fresh paint, a 24% chance of having a wine refrigerator, a 64% chance of having new energy-efficient appliances, a 64% chance of having a landscaped yard, a 77% chance of having smart-home features, a 42% chance of having a solid-wood front door, and a 49% chance of having crown moldings.

[7] We set the minimum time in our preregistration before eventually reducing the number of tasks participants were required to complete.

On the next screen we instructed participants to imagine that they saw another 1,700 square feet, three-bedroom, two-bathroom house in the same neighborhood of San Antonio for a price that was either \$2,000 higher or lower than the house they just viewed. We asked them to indicate using sliding scales (0: Extremely Unlikely to 100: Extremely Likely) how likely they thought it was that each of the characteristics would be present at houses at the new price point. We instructed participants that we had included the likelihood that such houses have these features at the price point of the first house they saw (i.e., \$399,000) and that they should adjust likelihoods as appropriate from these base value.

Price Manipulation. There were six price conditions. A sixth of participants saw the original house priced at \$397,000 and estimated the likelihood of characteristics for a house of \$399,000. The price changed from \$399,000 to \$397,000 for a different sixth of participants. A sixth saw the price change from \$399,000 to \$401,000, and a sixth saw the price change from \$401,000 to \$399,000. A sixth saw the price change from \$401,000 to \$403,000, while the final sixth saw the price from \$403,000 to \$401,000. As such, the design of the experiment was a 2 (direction of change: up vs. down) x House estimate (change: \$399K to/from \$397 vs. \$399K to/from \$401K vs. \$401K to/from \$403,000).

Dependent Variable. Per our preregistration, we standardized the changes in likelihood for each attribute, reversed the sign for changes in conditions in which the price was decreased, and created an index across all seven characteristics.

Participants concluded the study by indicating their gender.

7.2 Results and Discussion

Table 8 reports standardized values of mean changes in likelihood of attributes, indexed across all seven attributes. Per our preregistration we reversed the sign on changes in likelihoods associated with price decreases so that we could directly compare them with changes in likelihoods associated with price increases. A 2 (Change: Increase vs. Decrease) x 2 (Across Round Number Threshold: Across vs. Not Across) ANOVA revealed a marginally significant main effect of moving across the round-number threshold, $F(1, 572) = 3.841$, $p = .051$. This main effect indicates that price changes that crossed the \$400,000 round number benchmark induced larger changes in perceived likelihood of high-quality attributes in housing than did equally large changes in price that did not cross this threshold. Neither the main effect of increase ($F(1,572) = .234$, $p = .629$), nor the Across Threshold x Increase interaction ($F(1,572) = .374$, $p = .541$) were significant. When we re-ran the ANOVA excluding all participants who took less than 80 seconds to take the survey, the main effect of moving across the round-number threshold remained marginally significant, $F(1, 298) = 3.696$, $p = .056$. We did not ask participants to estimate the worth of the house and therefore could not test for mediation.

8. STUDY 4: DEPRECIATING ASSETS

The previous studies examine how round number reference points affect intergenerational pricing in housing, which is an asset that often appreciates in value. In Study 4 we examine if this same phenomenon occurs within the context of assets that typically depreciate over time. It is possible that previous sale prices exert a stronger influence on subsequent prices when the assets are depreciating than appreciating, as people are typically more averse to losses than they are seeking of gains (e.g., Tversky and Kahnemann 1991).

In this experiment, we investigate specifically whether round number reference points produce discontinuities in the relationship between vehicles' original Manufacturer's Suggested Retail Prices (MSRPs) and their resale value. We also examine whether round number reference points that are multiples of $5,000 produce the discontinuities observed in the other studies.

8.1 Participants and Method

A total of 1,405 participants (49.0% female; Age: $M = 34.43$, $SD = 12.34$) recruited from Prolific participated in the study. The pre-registration of the study is available at: https://aspredicted.org/TH9_6RD. We also pre-registered that we would exclude submissions completed in less than 180 seconds (n=134). Data appear at: https://osf.io/6yvws/?view_only=8370ae7a89d14f6db58badcf21a36056.

Participants viewed information for used cars in order to estimate the price they would ask for the cars. There were seven cars total, but each participant was randomly assigned to view only six of them. For each car they viewed the make, model, a picture of the car, the mileage, a description of the car from Edmonds.com and the Manufacturer's Suggested Retail Price when the car was new. We told each participant that they would be estimating the value for the car in Chicago, Illinois, as location can affect values for used cars. We also told participants that, for three of the cars, if their estimate was within 5% of the averaged value estimated by two automotive pricing websites they would receive a $1 bonus. Participants therefore had the opportunity to earn additional dollars in bonuses. We used an objective, externally-generated measure of value in lieu of a profit-maximizing number based on a demand-curve in order to reduce the uncertainty participants would face in the study. However, we acknowledge that motivating participants to think about how to maximize their expected profit would have mirrored more closely the psychology of people selling cars. For each vehicle, participants answered the question “What price would you ask for this vehicle?” After seeing the last vehicle, participants answered three questions assessing their familiarity with used car prices. The questions read “How familiar with used car prices before today’s survey?”, “How many cars have you purchased?”, and “How many cars have you sold”. Participants then provided their age and gender. We manipulated the MSRP of each vehicle using a Gaussian/normal distribution. For four vehicles the mean value was $30,000, for one vehicle it was $20,000, and for two vehicles it was $25,000. The standard deviation was $700 for all cars. We rounded car prices to the nearest $100. Thus, some vehicles had MSRPs that were below the round number reference point and some vehicles had prices somewhat above it, with many of them very close to the reference point. This allowed us to examine whether discontinuities appear at the $10,000 and $5,000 reference points.

8.2 Results

Table 9 provides means and correlations for the main variable by vehicle. Per our preregistration, we excluded vehicle value estimates more than three standard deviations away from the mean estimate for each vehicle. To account for between-vehicle variance in vehicles prices, we standardized the values (i.e., created z-scores) of the estimated list prices for each vehicle. We pre-registered that the standardized value of the list price estimates would be our primary dependent variable, with the unit of analysis at the participant-vehicle coupling. Table 10 reports descriptive statistics for the main variable by vehicle.

As in Study 1, we analyzed the experimental data using a regression discontinuity design with local linear models that allow for separate underlying linear relationships between MSRP and participants’ estimations of resale values on either side of the round number threshold. We implemented these models through cross-nested mixed-effects (i.e., multilevel) models (Gelman and Hill 2006, Kenny et al. 2006) to account for participant- and vehicle- random effects (intercepts).[8] The variable of interest is the discontinuous jump at the round number on which the distribution of randomly-assigned MSRPs are centered.

Figure 4 visually presents the raw data for all observations within $1,000 of the original MSRP reference points collapsed by mean estimated resale value into $100 bins. The discontinuity is clearly evident in Figure 4, shown by the large jump at round number MSRP reference points (shown as 0 in the figure on the X-axis). Table 11 presents the main controlled results. We find that participant estimates of value jump approximately 0.14 - .16 in the z-score of estimated price (depending on the data bandwidth used) for vehicles above the MSRP round number threshold compared to vehicles below. This translates to an average increase of $632 to $723 per vehicle across all vehicles included. This effect is robust to different data bandwidths ($1000, $500, and $200). It is also robust with an alternate dependent variable (shown in column 8). These results again support Hypothesis 1 and provide evidence that intergenerational pricing discontinuities around round number thresholds may apply in different contexts.

Appendix Figure A5 visually presents the pricing discontinuities depending on whether the vehicle's original MSRP round number threshold is at a $10,000 (e.g., $30,000) or $5,000 (e.g., $25,000) threshold. Table 11 columns 4 and 5 present the mixed model results. These results suggest that the average discontinuity appears to be driven by the vehicles around the $10,000 MSRP threshold, with no strong or precisely estimated effect at the $5,000 thresholds.

Finally, Table 11 shows mixed model results split by the median of reported participant familiarity with used car prices. These results are shown in columns 6 and 7. They show that the pricing bias appears larger for participants with low familiarity. The estimated effect for low familiarity participants is 0.17

[8] Individual vehicles in the experiment by design will have different average estimated resale price levels, and individual respondents are likely to have idiosyncratic estimation of depreciation levels. The random effects account for these in a more statistically efficient way, and are appropriate because of the random assignment of the key independent MSRP.

while the effect for high familiarity participants is 0.083. The estimated effect for low familiarity participants is 0.17 while the effect for high familiarity participants is 0.083. Given the standard errors of 0.03 for each estimate, a T-test indicates that these are statistically different at $p < 0.01$. While the discontinuity manifests for both the low and high experienced participants, this suggests that experience attenuates the pricing bias, consistent with the logic driving Hypotheses 3a and 3b. We note that we did not find that the number of cars that were purchased or sold previously by participants meaningfully moderated the effects.

Consistent with Hypothesis 1, Study 4 provides evidence that the position of MSRPs relative to round number reference points can causally affect used car pricing in the future. This suggests that the effect from our real estate study generalizes to the sequential sales of other durable goods or physical assets and represents a behavioral bias that exists across multiple contexts.

We expected that people who either purchased or sold cars somewhat frequently would be less susceptible to the anchoring effect than would people who did not. We also expected high correlations between familiarity with prices and the number of times people bought or sold cars. The data did not show strong correlations between experience and knowledge of prices. Familiarity with car prices correlated only at 0.14 with number of cars sold and at 0.25 with number of cars purchased. The results therefore seem to suggest that those broadly knowledgeable about car prices are not necessarily the same people who buy or sell cars frequently. We speculate that selling or buying a car allows for knowledge of one vehicle's pricing but perhaps not a broader knowledge that people interested in cars possess. We note that we did not predict *a priori* these differences in effects for familiarity and experience.

9. 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 and a series of experiments, 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. This finding suggests that round number discontinuities may distort the previously identified anchoring relationship between previous and future sales prices. Market forces and the negotiation process attenuated but did not fully correct for most of this pricing bias. The results consequently imply that buyers who pay prices that fall just below round numbers may receive lower future prices for their property when reselling, compared to those who pay prices just above round numbers.

We found some weak evidence that experience, both for professional intermediaries as well as for individuals valuing the durable goods, attenuates this pricing bias. Transactions with inexperienced listing agents exhibited significantly larger discontinuities than transactions with highly experienced agents. In vehicle valuations, pricing discontinuities were significantly larger for participants with limited experience with used car pricing, compared to those with more experience. This suggests that knowledge gained from experience can potentially attenuate the anchoring-and-adjustment heuristic, and consequently reduce intergenerational pricing bias. Exploratory post hoc analyses found no evidence for a moderating role of organizational support in reducing pricing bias. Our pre-registered experiments provided causal evidence for our main effect, for anchoring as the mechanism, and helped reduce empirical concerns around selection. They also showed that the discontinuities introduced by round number reference points can affect sales prices of depreciating assets.

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 (Bendersky and Curhan 2009, Neale and Bazerman 1991) by demonstrating how motivations to achieve goals within negotiations can affect outcomes in subsequent negotiations.

It also complements existing research on history dependence in negotiations (e.g., Beggs and Graddy 2009, Bokhari and Geltner 2011, Einiö et al. 2008). Standard economic models account for path dependency in market prices and largely assume linearity. Scholars have refined those models by showing that round numbers can exert smaller anchoring effects than round numbers exert (e.g., Janiszewski & Uy

2008; Lee, Loschelder, Schweinsburg, Mason, & Galinsky, 2018). However, neither these models nor existing theory predict that numerical reference points, such as round numbers can affect price correlations across multiple sales by psychologically demarcating sets of prices. Future work should examine these issues in additional settings and contexts. It may be particularly interesting to investigate if these effects hold in more polychromic cultures in which people view time as less linear (see Adair and Brett 2005). Likewise, it would be interesting to observe whether the effects persist in more complex cross-cultural business contexts (Weiss 1993).

Future research should also examine in what context the time value of money effectively offsets the benefits of paying just over round number benchmarks for durable goods. Buyers may not benefit from paying prices at or above round number benchmarks when they are uncertain about how long they will hold the asset or when the opportunity cost of using that money is high, as is the case when interest rates are high. Buyers might also be reticent to take the risk involved with paying a small premium to reach the round number benchmark even when the likely return is worth the risk. Research may consequently be needed to determine how buyers can be convinced to pay extra today in hopes of selling more tomorrow.

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. This implies benefits for organizations as they leverage individual-level experience to avoid bias (Choudhury, Starr, Agarwal 2020, Simon 1991), and suggests an important role for organizations in organizing individual knowledge and skills (Kogut and Zander, 1996) from experience. They also add to previous work on human capital and performance (e.g., Coff 1997; Crook et al. 2011, Gubler 2019, Hitt et al. 2001, Rosen 1983) 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 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 fully corrected by the market or 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 $2,898 and $6684 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., $2,898) if the final sales price drops below a relevant round number reference point. The results also show that the benefit of hiring an experienced listing agent, particularly if a home previously fell below a relevant round number reference point, may be large as inexperienced listing agents may have greater susceptibility to previous listing price placements. Understanding the extent to which certain types of sellers or buyers might select higher or lower experienced listing or buyer agents presents a potential avenue for future work.

Practical and Managerial Implications

Finally, the findings in this paper have important practical and managerial implications for those involved in negotiations. First, they suggest to buyers that negotiating a price just below a round number in an initial negotiation could be costly if they intend to resell the good later under conditions where the previous price is available to future buyers. The findings may also prove useful to sellers in the initial sale of a good, as sellers may be able to use our findings to justify to buyers rounding up the price if they are planning to resell the good. The findings also suggest that negotiators may need to set more aggressive anchors when they are trying to sell a good that previously sold for a price just below a round number. These insights could be included more directly in curriculum and trainings to managers and negotiators.

The findings also imply that people attempting to buy properties or goods that previously sold at or just above a round number may benefit from looking at more properties, as the prices of those properties may serve as countervailing anchors that diminish the anchoring effect that the property's previous sale price may produce. In states like Utah that only allow agents to access previous sale prices for real estate, they may need to ask their agents to broaden their search of resales data to include similar properties that sold just below round numbers. Conducting these broader searches may diminish the effect that the previous sale price's relationship to the round number benchmark has on the subsequent price. Of course, if sellers and other potential buyers do not try to gather information themselves to combat this bias, those potential buyers who adjust their bids downward may lose out on purchases. Intermediaries advising clients would be well-advised to consider this tradeoff when consulting with their clients. Moreover, future research should therefore investigate empirically whether such adjustments prove helpful to potential buyers.

Finally, there are important implications for organizations that employ professional intermediaries. 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 not help 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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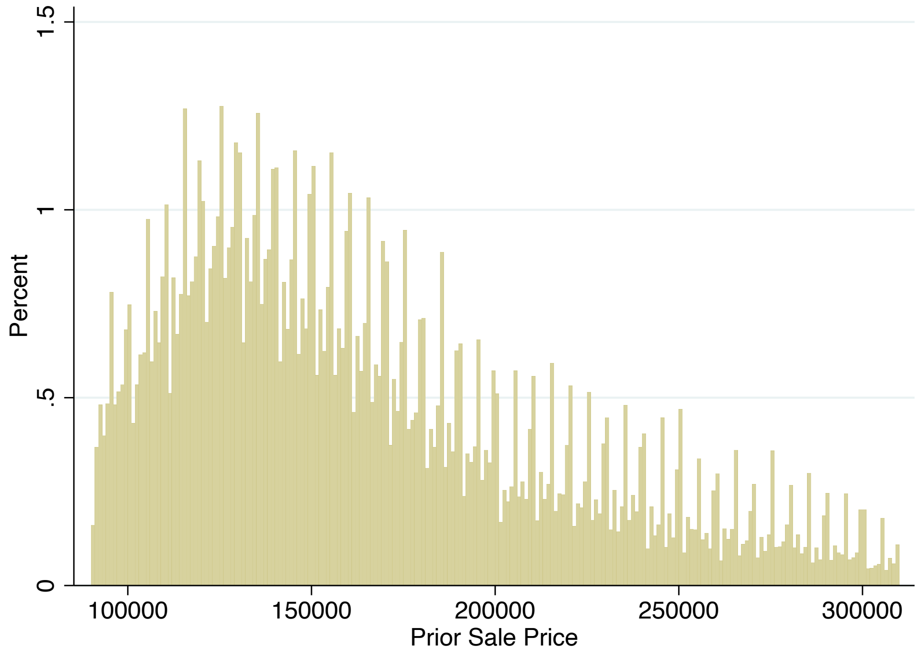
**Endnotes: Real estate listing data were provided courtesy of the Wasatch Front Regional Multiple Listing Service (WFRMLS). WFRMLS has not reviewed or verified any of the numbers or statistics in this paper.

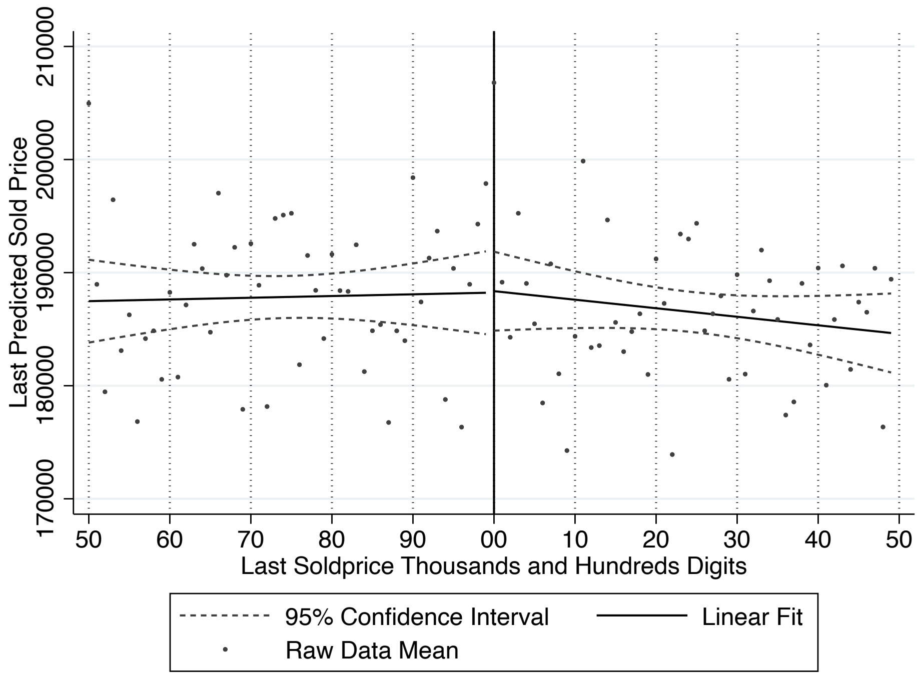
FIGURES AND TABLES

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



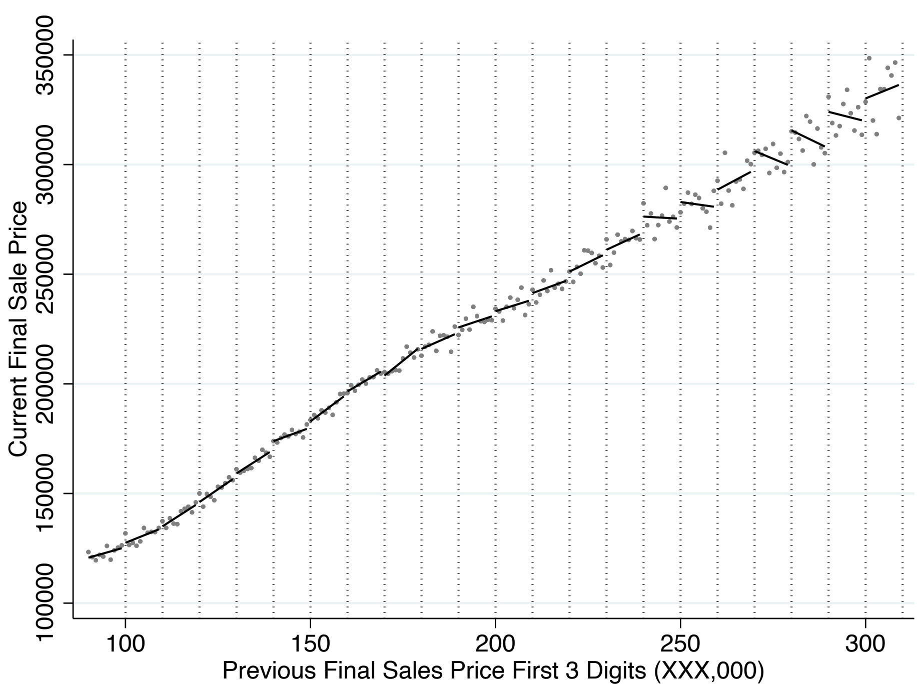
Note: This figure presents the histogram of all initial final sales prices broken out at $1000 price intervals. It suggests potential non-random assignment of homes around $10,000 thresholds, likely driven by rounding or charm pricing.

Figure 2: Study 1 Identification Test—Last Predicted Sale Price Similar for Homes Around $10,000 Cutoffs



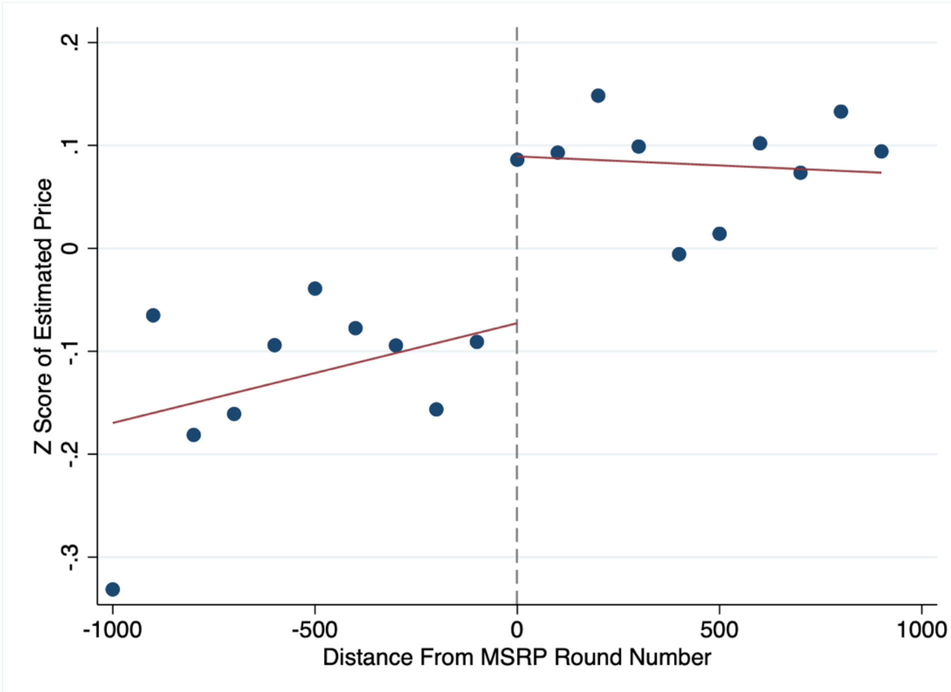
Note: This figure presents the estimated predicted home sales prices for all previous sales bucketed by the thousands and hundreds digits in the final sales prices. "00" refers to $10,000. It shows that predicted home sales prices are largely similar for homes that fall just above and just below the $10,000 price thresholds.

Figure 3: Study 1 Raw Data Discontinuities Shown 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. There are fewer data points for intervals on the right side of the figure, resulting in less precise estimates of linear fit.

Figure 4: Study 4 Binned Scatter Plot Using All Vehicles from Car Pricing Experiment



Note: This figure presents a raw data binned scatter plot at $100 increments around original MSRP round number price thresholds using all vehicle estimates from the car pricing experiment. Solid lines represent linear fits above or below the round number price threshold in original MSRP, which is represented as "0" on the x-axis.

Table 1: Study 1 Descriptive Statistics, Archival Data

Archival Data Variable	N	Mean	Std Dev	Min	Max
Original list price	83164	203915	74022	25740	679900
Sale price	69078	193113	68178	33000	487000
Net price	69051	190456	68032	33000	487000
Previous sale price	83164	164614	51267	90001	309995
Agent experience	69084	198.46	363.15	1	3398
Brokerage size	83164	52.13	70.71	1	490
Franchise brokerage	83164	0.49	0.50	0	1
Days on market	83122	89.36	70.94	0	727
Fail	83164	0.17	0.38	0	1

Table 2: Study 1 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	**7678.02**	**3763.67**	**6683.86**	**2897.82**	**6694.55**	**2923.91**
F-stat	**74.82**	**76.55**	**52.14**	**38.21**	**34.35**	**38.37**
Prob > F	**0.0000**	**0.0000**	**0.0000**	**0.0000**	**0.0000**	**0.0000**
$100k	7331.2***	918.8	6027.6***	638.2	6137.5***	757.6
	(1437.4)	(976.1)	(1451.2)	(846.0)	(1434.3)	(848.0)
$110k	5573.7***	18.2	4527.8***	-353.6	4464.7***	-357.2
	(955.8)	(564.3)	(978.1)	(511.8)	(966.5)	(507.6)
$120k	5796.9***	521.5	5126.3***	387.3	5022.4***	334.9
	(892.0)	(407.0)	(832.2)	(435.3)	(822.5)	(433.0)
$130k	5314.0***	1033.6***	4813.4***	1132.4***	4847.5***	1080.5***
	(859.8)	(389.1)	(856.2)	(404.5)	(856.9)	(401.9)
$140k	4129.8***	922.1***	4279.0***	1103.7***	4249.2***	1075.3***
	(920.4)	(322.7)	(858.9)	(383.2)	(850.2)	(381.9)
$150k	2892.1***	1608.5***	2996.9***	1881.3***	3075.2***	1924.0***
	(789.5)	(353.2)	(795.8)	(393.2)	(776.4)	(386.0)
$160k	2309.6**	1673.2***	2826.8***	1599.9***	2802.2***	1610.8***
	(908.2)	(398.5)	(862.5)	(411.4)	(854.4)	(418.6)
$170k	2708.8**	1205.4**	2401.4**	1079.2**	2376.0**	1070.0**
	(1260.8)	(484.6)	(1141.8)	(506.9)	(1142.2)	(513.4)
$180k	1794.8	1389.0***	1959.6	1393.6**	1907.8	1376.4**
	(1295.4)	(512.8)	(1286.5)	(596.1)	(1281.9)	(597.5)
$190k	2345.2	1780.5***	2329.1*	1287.0**	2331.8*	1266.2**
	(1444.0)	(683.3)	(1276.6)	(607.6)	(1247.0)	(609.5)
$200k	2501.4	1453.8**	2828.5*	1380.6*	2718.9*	1359.0*
	(1640.1)	(704.0)	(1634.9)	(761.6)	(1616.2)	(747.1)
$210k	6260.6***	2216.1***	4383.7**	695.5	4430.5**	820.6
	(1857.3)	(720.9)	(1764.6)	(732.8)	(1768.2)	(735.2)
$220k	9433.2***	3657.8***	7367.2***	1976.6**	7367.0***	1892.8**
	(1985.6)	(789.7)	(1835.2)	(849.1)	(1828.6)	(842.0)
$230k	9111.0***	6133.2***	8428.6***	4309.6***	8455.8***	4354.3***
	(2048.7)	(922.0)	(2007.4)	(1107.5)	(2002.1)	(1110.8)
$240k	13321.4***	6688.8***	9357.5***	2842.1**	9474.9***	3059.7**
	(2325.7)	(1300.5)	(2599.7)	(1390.9)	(2611.4)	(1405.7)
$250k	8399.4***	5801.2***	7052.1***	3640.5**	7139.8***	3739.0**
	(2789.1)	(1396.9)	(2621.4)	(1437.5)	(2642.8)	(1458.9)
$260k	16108.7***	11400.3***	12844.2***	6806.7***	12933.9***	6879.2***
	(2922.4)	(1688.8)	(3018.6)	(1684.7)	(3054.2)	(1715.6)
$270k	13383.1***	7039.7***	12537.2***	6568.7***	12562.1***	6684.8***
	(3175.3)	(1905.5)	(3129.5)	(1827.6)	(3128.2)	(1834.0)
$280k	14956.8***	9726.3***	11697.7***	7121.6***	11909.0***	7309.2***
	(3216.2)	(1936.3)	(3503.1)	(1975.0)	(3531.4)	(1997.7)
$290k	13437.4***	7538.4***	14339.0***	7737.1***	14270.7***	7599.1***
	(4448.2)	(2745.1)	(4308.4)	(2724.7)	(4324.4)	(2741.0)
$300k	14129.5***	6310.6**	12237.5**	7626.2**	12108.9**	7566.0**
	(4964.9)	(3198.3)	(4951.4)	(3030.2)	(4990.8)	(3061.5)
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	83164	68416	69078	68416	69051	68416
R-squared	0.711	0.941	0.715	0.926	0.717	0.925

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 A2. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Study 1 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	**4900.92**	**3353.40**	**3966.23**	**2531.41**	**4008.21**	**2554.71**
F-stat	**38.62**	**41.69**	**22.88**	**24.74**	**22.97**	**24.92**
Prob > F	**0.0000**	**0.0000**	**0.0000**	**0.0000**	**0.0000**	**0.0000**
$100k	-14.8	1291.1	-289.5	1030.7	-131.6	1140.0
	(1175.9)	(1286.0)	(1072.7)	(1156.0)	(1064.3)	(1158.4)
$110k	-1078.5	432.6	-990.3	-98.6	-958.9	-116.6
	(842.8)	(711.5)	(863.0)	(677.0)	(874.5)	(666.9)
$120k	210.6	579.6	179.7	464.7	86.8	425.4
	(687.1)	(502.3)	(691.9)	(539.2)	(693.3)	(532.7)
$130k	749.6	1136.5**	596.8	1396.5***	623.0	1314.3***
	(624.5)	(513.6)	(673.2)	(506.8)	(675.4)	(496.8)
$140k	391.0	1063.5***	-112.9	1529.5***	-20.2	1439.3***
	(645.9)	(411.2)	(727.9)	(467.2)	(730.7)	(468.3)
$150k	2153.5***	1310.9***	2229.5***	1669.6***	2268.5***	1717.6***
	(635.7)	(426.2)	(757.6)	(463.0)	(771.0)	(456.3)
$160k	2136.9***	1497.9***	1222.4	1758.1***	1381.5*	1700.4***
	(772.4)	(485.6)	(781.2)	(500.6)	(791.8)	(508.9)
$170k	744.3	1401.6**	984.4	1145.7**	962.4	1138.5**
	(891.1)	(557.9)	(943.5)	(566.9)	(955.5)	(570.5)
$180k	2535.0**	858.4	2029.2*	1038.7	1931.9*	1063.6
	(1085.2)	(594.0)	(1143.2)	(696.8)	(1150.2)	(701.3)
$190k	3446.6***	1214.3	2318.0*	987.3	2144.6*	1018.2
	(1226.2)	(796.4)	(1211.2)	(711.6)	(1221.2)	(718.8)
$200k	2561.2**	1060.7	1663.8	1246.1	1457.9	1296.1
	(1274.9)	(810.9)	(1435.5)	(888.0)	(1421.4)	(869.2)
$210k	3736.7**	1708.0*	2178.1	156.0	2197.1	326.8
	(1478.2)	(913.1)	(1492.8)	(921.9)	(1496.8)	(936.0)
$220k	5548.6***	2906.7***	3346.5*	1457.1	3145.4*	1405.4
	(1688.0)	(933.2)	(1815.0)	(943.1)	(1823.3)	(937.4)
$230k	7736.1***	5719.2***	5759.5***	3893.4***	5747.0***	3964.9***
	(1919.7)	(1071.2)	(1921.9)	(1281.1)	(1935.8)	(1275.5)
$240k	8683.6***	6085.4***	6020.5**	1854.7	5779.4**	2232.5
	(2376.9)	(1546.1)	(2604.7)	(1536.6)	(2643.9)	(1548.3)
$250k	6947.8**	5450.7***	4651.7*	3391.3**	4733.2*	3497.0**
	(2821.4)	(1549.3)	(2821.7)	(1571.5)	(2825.6)	(1603.2)
$260k	14713.0***	10437.2***	10147.2***	5796.7***	9948.3***	5944.3***
	(3176.6)	(1939.6)	(2997.7)	(1913.3)	(3048.2)	(1950.2)
$270k	6408.1*	6940.7***	8466.1**	5772.8***	8683.7**	5880.2***
	(3459.8)	(2236.8)	(3439.9)	(1987.5)	(3453.5)	(2001.9)
$280k	11645.6***	9147.3***	6449.3**	7305.0***	6713.0**	7467.1***
	(3256.5)	(2328.1)	(3281.3)	(2322.8)	(3312.9)	(2349.7)
$290k	12685.5***	5479.4	14449.1***	5196.6*	14341.5***	5029.8
	(4543.5)	(3342.3)	(4378.4)	(3086.6)	(4463.0)	(3104.7)
$300k	10978.7*	4699.6	11991.7*	6167.8**	13137.9*	5764.2*
	(6356.8)	(3624.0)	(6735.0)	(3075.4)	(6770.5)	(3117.2)
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	20576	47840	20576	47840	20576	47840
R-squared	0.942	0.941	0.926	0.926	0.926	0.925

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 A2. Low and high listing agent experience are measured as being below or above the agent experience median. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Study 1 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	**2374.12**	**3141.73**	**2408.40**	**3171.52**
F-stat	**10.18**	**34.47**	**10.58**	**34.13**
Prob > F	**0.0014**	**0.0000**	**0.0012**	**0.0000**
$100k	645.5	603.0	787.4	697.7
	(1083.7)	(1052.5)	(1085.7)	(1063.4)
$110k	74.9	-770.7	81.1	-788.0
	(701.9)	(675.1)	(686.0)	(670.7)
$120k	291.0	405.6	241.0	361.6
	(678.3)	(564.9)	(682.4)	(558.3)
$130k	1052.6*	1208.5**	965.1*	1188.6**
	(549.2)	(606.7)	(553.5)	(603.3)
$140k	1218.5**	932.3*	1184.3*	916.4*
	(618.0)	(518.5)	(623.3)	(524.2)
$150k	2641.3***	1416.4**	2663.2***	1481.3***
	(625.6)	(564.2)	(621.3)	(564.0)
$160k	1348.6**	1850.2***	1335.2*	1884.2***
	(679.8)	(551.8)	(687.0)	(566.9)
$170k	1542.3**	865.2	1603.3**	816.8
	(784.8)	(717.8)	(790.8)	(729.3)
$180k	1492.5	1402.7*	1473.5	1380.9*
	(949.8)	(822.8)	(955.3)	(829.4)
$190k	1490.1	1218.6	1426.1	1225.4
	(980.6)	(815.1)	(996.6)	(816.7)
$200k	-11.0	2174.0**	53.6	2108.5**
	(1424.8)	(1017.7)	(1398.8)	(1007.3)
$210k	437.6	847.0	646.5	914.3
	(1240.7)	(903.6)	(1247.5)	(906.1)
$220k	2738.5*	1518.8	2525.4	1508.3
	(1539.4)	(1051.8)	(1537.0)	(1043.4)
$230k	2186.0	5312.2***	2287.6	5347.3***
	(1711.3)	(1362.5)	(1727.0)	(1373.2)
$240k	2553.5	2926.4*	2839.8	3097.7*
	(2043.0)	(1768.0)	(2051.7)	(1782.8)
$250k	2259.1	4400.4***	2384.0	4500.6***
	(2546.4)	(1619.5)	(2574.0)	(1639.8)
$260k	3531.9	8231.2***	3712.8	8253.3***
	(2996.8)	(2032.4)	(3060.8)	(2056.9)
$270k	3446.9	8105.6***	3418.3	8337.1***
	(3368.6)	(2203.1)	(3386.6)	(2229.5)
$280k	5936.8*	7699.1***	6090.6*	7923.6***
	(3274.6)	(2247.1)	(3277.6)	(2277.7)
$290k	6528.3*	8533.1***	6501.2*	8367.7***
	(3895.7)	(3214.3)	(3915.8)	(3232.9)
$300k	8452.0	7096.6*	8356.6	7078.8*
	(5187.5)	(3702.3)	(5179.3)	(3758.2)
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	27677	40739	27677	40739
R-squared	0.925	0.926	0.924	0.925

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 A2. Low and high buyer agent experience are 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: Study 2 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.

Table 8: Study 3 Standardized Value of Change in Likelihoods of Likely Attributes

Price Changes				
Type	Valence	Mean	Std. Dev.	N
Not Across Threshold	Decrease	-0.13	0.37	178
	Increase	0.12	0.49	186
Across Threshold	Decrease	-0.18	0.44	106
	Increase	0.22	0.51	106

Table 9: Study 4 Correlations and Means

	1	2	3	4	5	6	7
1. Est. Listing Price	1						
2. MSRP Provided	0.569 **	1					
3. Over Benchmark	0.095 **	0.155 **	1				
4. Female	0.008	0.007	0.019	1			
5. Familiarity with Cars	0.066 **	-0.002	0.017	-0.171 **	1		
6. Cars Sold	-0.035 **	0.003	-0.002	-0.108 **	0.218 **	1	
7. Cars Purchased	-0.033 **	0	0.002	-0.073 **	0.238 **	0.711 **	1
Mean	17,699	27,131	0.52	1.52	5.59	1.30	2.72
Median	18,000	29,200	1.00	2.00	5.00	0.00	2.00

** Correlation is significant at the 0.01 level (2-tailed test).
Note: 7,671 person-car estimates.

Table 10: Study 4 Descriptive Statistics by Vehicle

Vehicle	N	MSRP Mean	MSRP SD	Estimated Price Mean	Estimated Price SD
2015 Toyota Highlander	1,193	29,976	716	19,742	4,957
2014 Honda Civic	1,234	19,980	688	11,760	3,382
2016 Ford F-150	1,227	30,003	685	20,816	5,102
2016 Chevrolet Impala LT	1,195	24,993	701	16,314	4,214
2016 Acura ILX	1,218	29,999	676	20,103	4,893
2016 Subaru Outback 2i	1,223	30,034	711	19,855	4,932
2015 Hyundai Sonata Sport	1,197	25,009	710	15,686	4,158

Table 11: Study 4 Car Pricing Mixed Model Results

Dependent Variable:	(1) Z-Score of Estimated Price	(2) Z-Score of Estimated Price	(3) Z-Score of Estimated Price	(4) Z-Score of Estimated Price	(5) Z-Score of Estimated Price	(6) Z-Score of Estimated Price	(7) Z-Score of Estimated Price	(8) Residual Percent
	MSRP within 1K	MSRP within $500	MSRP within $200	MSRP at 5K thresholds	MSRP at 10K thresholds	Low familiarity	High familiarity	MSRP within 1K
MSRP over round # threshold	0.14***	0.19***	0.16*	0.019	0.14***	0.17***	0.083**	0.023***
	(0.028)	(0.042)	(0.091)	(0.052)	(0.029)	(0.030)	(0.032)	(0.005)
Distance from MSRP threshold	0.00010**	-0.00001	0.00039	0.00016***	0.00010***	0.00009***	0.0001***	0.00002**
	(0.00004)	(0.00011)	(0.00053)	(0.00005)	(0.00003)	(0.00003)	(0.00004)	(0.00001)
Interaction	-0.00004	-0.00007	-0.00035	-0.00005	0.00003	-0.000005	0.00004	-0.00001
	(0.00006)	(0.00015)	(0.00076)	(0.00007)	(0.00004)	(0.00004)	(0.00005)	(0.00001)
Observations	6520	4010	1746	2166	5505	4190	3481	6520

Note: Standard errors in parentheses. This table presents mixed models results for the Study 4 car valuation experiment. “MSRP over round # threshold” is a dummy for if the original MSRP presented was over or under a relevant round number threshold. The residual percent dependent variable is calculated as the estimated price divided by the original MSRP threshold. The interaction variable is the interaction between the first and second variables in the table.
* p<0.1, ** p<0.05, *** p<0.01.

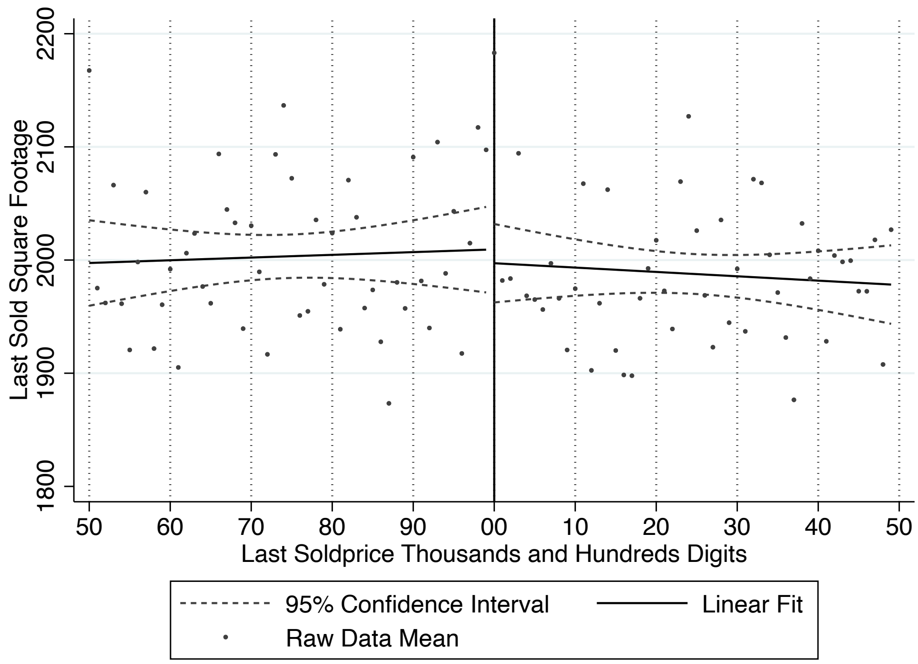
APPENDIX

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.

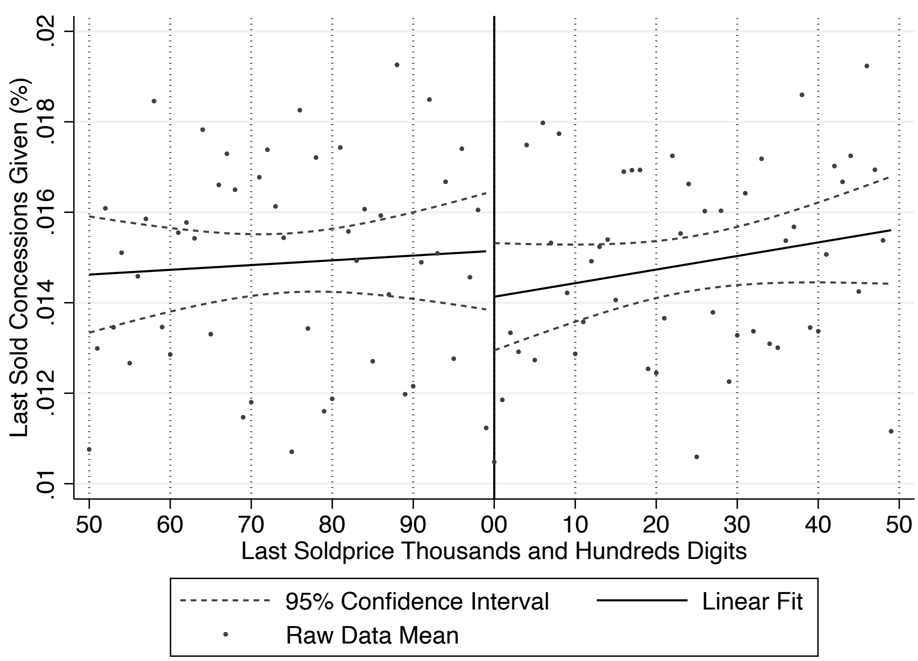
Table A8 shows correlations, Table A9 shows the mean estimated listing price by condition by house, and Table A10 shows the results of the mixed-model analyses. When we used standardized values of the housing estimate as the dependent variable, we found that the average difference (M = \$6,258, SD = \$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: Study 1 Identification Test—Home Sq. Footage Similar Around $10,000 Discontinuities



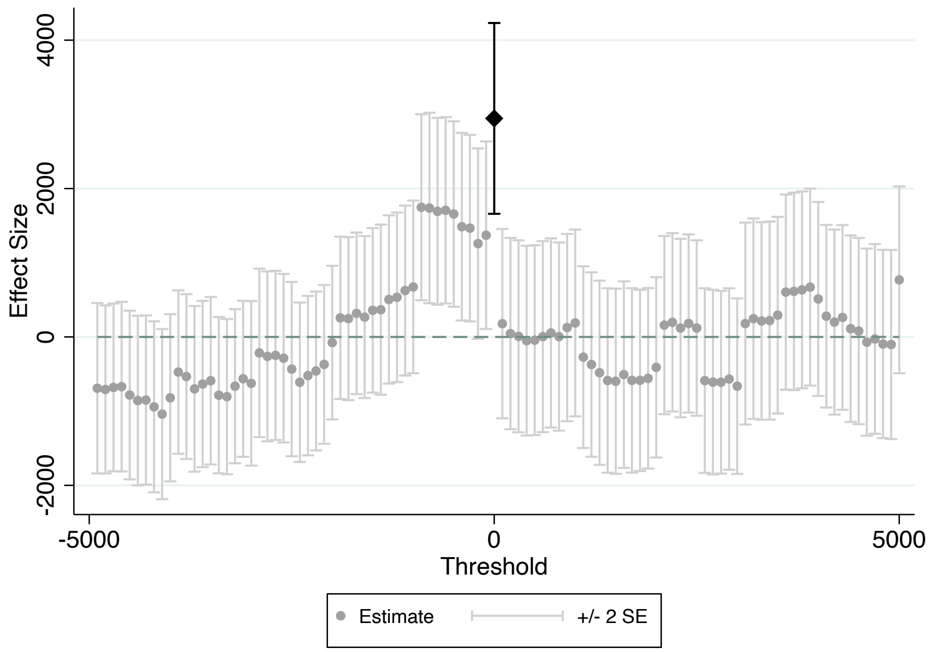
Note: This figure presents raw data for home square footage for all previous sales bucketed by the thousands and hundreds digits in that sales final sales prices. "00" refers to $10,000. It shows homes are similar in total square footage around the $10,000 price thresholds.

Figure A2: Study 1 Identification Test—Percentage of Seller Concessions Provided Around $10,000 Discontinuities are Similar



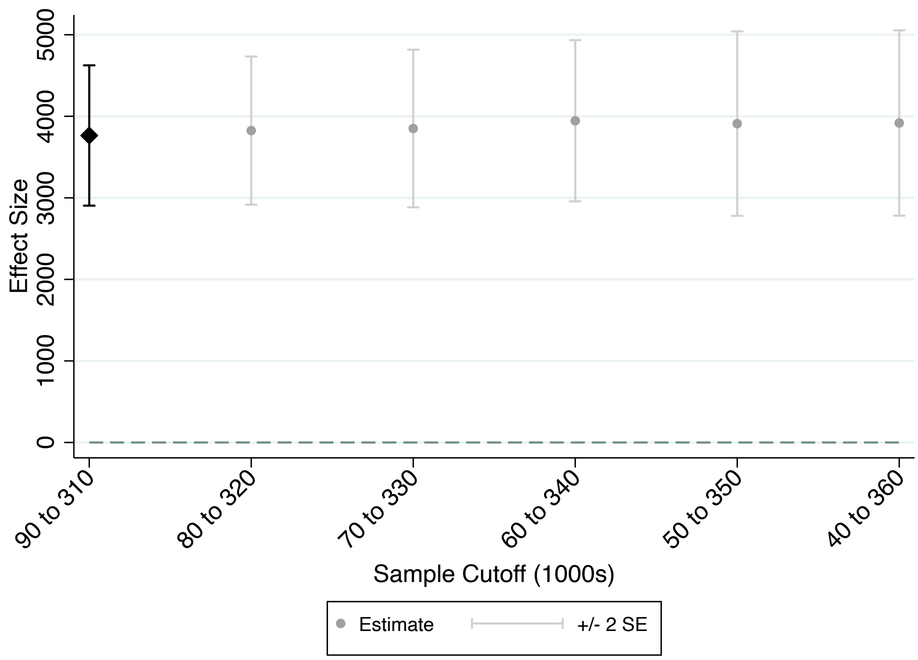
Note: This figure presents raw data for seller concessions given, calculated as a percentage of the home sales price, bucketed by the thousands and hundreds digits in that sales final sales prices. "00" refers to $10,000. A higher percentage of seller concessions reduces the net price for buyers. This figure suggests similar seller concessions for houses above and below the threshold.

Figure A3: Study 1 Discontinuity Placebo Tests



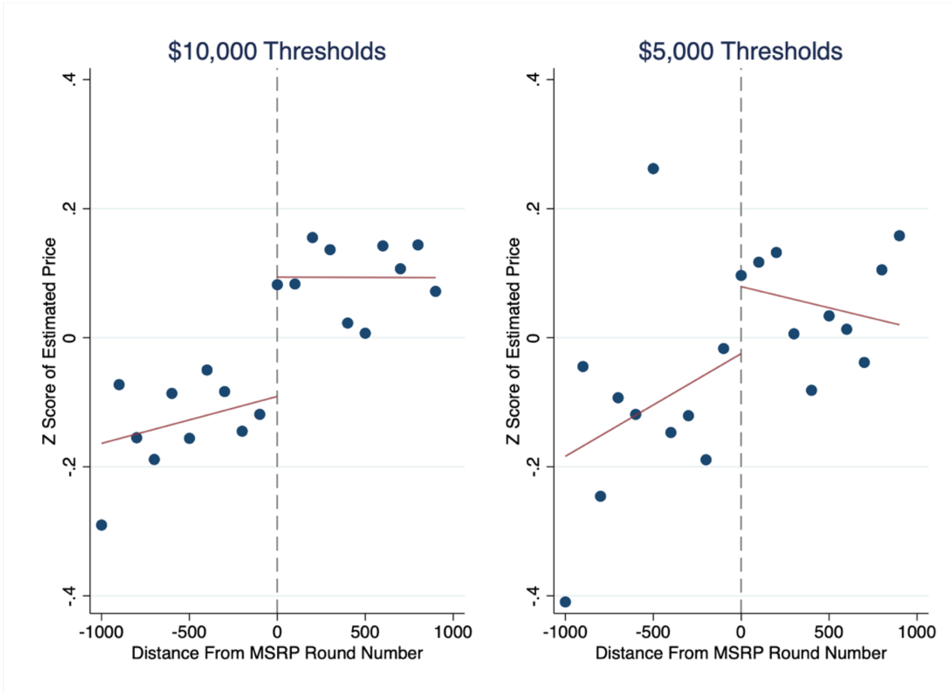
Note: This figure presents the average estimated sales price discontinuities for our fully controlled model with 100 placebo simulations. For each simulation we assigned the discontinuity to a $100 interval within $5,000 above and below the true $10,000 round number discontinuity. The results suggest 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 A4: Study 1 Robustness to Sample Cutoff Points



Note: This table presents the average estimated sales price discontinuity for the fully controlled model with variations in the sample cutoff at both the bottom and top ends of the distribution. The main models presented in the paper use a sample cutoff of $90,000-$310,000 (bolded estimate shown as the diamond on the far left of the figure). This table presents robustness of our main model estimates to a maximum sample cutoff of $360,000 on the top end, with the bottom end of the distribution extended to $40,000.

Figure A5: Study 4 Binned Scatter Plot of Car Price by $10K and $5K Thresholds



Note: This figure presents a raw data binned scatter plot using all vehicle estimates from the car pricing experiment. The panel on the left shows the discontinuity for the five vehicles whose MSRP spanned a $10K price threshold. The panel on the right shows the discontinuity for the two vehicles whose MSRP spanned a $5K price threshold. Solid lines represent linear fits above or below the round number price threshold.

Table A1: Study1 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	337	519	1416.602	1452.921	-36.31863	29.66191	.2211315
Acreage	337	520	.1317211	.1376154	-.0058943	.0072711	.4177903
# Bedrooms	337	520	2.928783	2.996154	-.0673705	.0633792	.2880938
# Bathrooms	337	520	1.543027	1.567308	-.024281	.0413735	.5574443
% Finished Basement	337	520	27.37389	29.61538	-2.241497	2.743824	.4141991
Year Built	338	520	1961.207	1961.531	-.3236686	1.922089	.866313

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	374	683	1477.449	1565.378	-87.92855	27.49113	.0014229
Acreage	374	683	.1409893	.1419327	-.0009433	.0074846	.8997257
# Bedrooms	374	683	3.026738	3.122987	-.0962489	.0575152	.0945348
# Bathrooms	374	683	1.671123	1.698389	-.0272665	.0392318	.487203
% Finished Basement	374	683	31.57487	36.79649	-5.22162	2.643491	.0484973
Year Built	374	683	1968.086	1964.772	3.313966	1.704262	.0520987

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	537	665	1630.108	1717.517	-87.40929	25.68914	.0006895
Acreage	537	665	.1524767	.1550226	-.0025458	.0057997	.6607678
# Bedrooms	537	665	3.180633	3.374436	-.1938029	.0537829	.0003269
# Bathrooms	537	665	1.780261	1.864662	-.0844009	.0372202	.0235302
% Finished Basement	537	665	40.81192	42.33534	-1.52342	2.492751	.5412226
Year Built	538	665	1968.697	1968.713	-.0157559	1.467697	.9914365

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	597	780	1713.626	1745.512	-31.88507	23.58949	.176705
Acreage	597	780	.158526	.1620897	-.0035638	.0048048	.4583869
# Bedrooms	597	780	3.301508	3.357692	-.0561848	.0477216	.239262
# Bathrooms	597	780	1.859296	1.901282	-.0419856	.0343166	.2213598
% Finished Basement	597	780	46.66332	44.81154	1.851778	2.369578	.4346547
Year Built	598	781	1972.05	1969.498	2.552088	1.397968	.0681317

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	537	765	1838.741	1877.336	-38.59479	26.97271	.1527041
Acreage	537	766	.1634078	.1967363	-.0333285	.0181767	.0669448
# Bedrooms	536	766	3.43097	3.460836	-.0298654	.0506039	.5551735
# Bathrooms	537	766	2.046555	2.010444	.0361111	.0375711	.33666
% Finished Basement	537	766	49.00186	49.58616	-.5842997	2.456397	.8120205
Year Built	539	766	1975.382	1972.304	3.078012	1.349745	.0227425

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	555	764	1928.953	2016.463	-87.5102	27.84909	.0017136
Acreage	556	764	.1661331	.177055	-.0109219	.0050887	.0320296
# Bedrooms	556	761	3.48741	3.646518	-.1591077	.0499204	.0014703
# Bathrooms	556	764	2.133094	2.217277	-.084184	.0378017	.0261164
% Finished Basement	556	764	47.32374	53.62173	-6.297987	2.456176	.0104531
Year Built	556	764	1976.038	1974.692	1.345361	1.347396	.3182252

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	468	705	2071.429	2017.555	53.87488	30.36568	.0762894
Acreage	468	705	.1790812	.201305	-.0222238	.0332751	.5043417
# Bedrooms	468	705	3.666667	3.558865	.1078014	.059032	.0680819
# Bathrooms	468	705	2.320513	2.190071	.1304419	.0414701	.0017001
% Finished Basement	468	705	56.67521	50.60993	6.065285	2.595542	.019617
Year Built	468	705	1975.325	1974.577	.7474814	1.539964	.6274913

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	461	622	2138.523	2128.111	10.41184	34.73618	.7644327
Acreage	461	622	.1768764	.1858039	-.0089275	.0067545	.1865455
# Bedrooms	461	622	3.574837	3.596463	-.0216257	.0594741	.716216
# Bathrooms	461	622	2.314534	2.278135	.0363986	.0485372	.4534712
% Finished Basement	461	622	46.20174	50.7926	-4.590869	2.685647	.0876622
Year Built	461	622	1980.098	1973.683	6.414334	1.602822	.0000671

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	341	516	2213.66	2267.684	-54.02428	41.54018	.1937696
Acreage	341	516	.198563	.2116473	-.0130842	.0140609	.352354
# Bedrooms	341	516	3.624633	3.70155	-.076917	.0679275	.2578094
# Bathrooms	341	516	2.404692	2.352713	.0519789	.0500624	.2994317
% Finished Basement	341	516	52.19648	55.26744	-3.070961	3.067341	.317023
Year Built	341	516	1975.833	1973.634	2.199124	1.875145	.2412126

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	299	477	2362.201	2291.208	70.99312	47.32126	.1339608
Acreage	299	477	.1992308	.2131027	-.013872	.0212859	.5147898
# Bedrooms	299	477	3.732441	3.737945	-.005504	.0717744	.938894
# Bathrooms	299	477	2.545151	2.471698	.0734524	.0549159	.1814387
% Finished Basement	299	477	50.71906	55.75262	-5.033557	3.277737	.1250254
Year Built	299	477	1983.09	1975.138	7.951936	1.91876	.0000378

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	283	395	2340.668	2390.073	-49.40557	52.45333	.3465824
Acreage	283	395	.2005654	.2031646	-.0025992	.0121663	.8308935
# Bedrooms	283	395	3.759717	3.832911	-.0731941	.0819566	.3721306
# Bathrooms	283	395	2.572438	2.518987	.0534508	.0616691	.3863941
% Finished Basement	283	395	54.30742	57.4481	-3.140681	3.455174	.3636851
Year Built	283	395	1981.813	1975.711	6.101328	2.064652	.003234

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	176	414	2474.682	2407.37	67.31225	60.08651	.2630622
Acreage	176	414	.2373295	.2174879	.0198416	.0348102	.5688987
# Bedrooms	176	414	3.721591	3.785024	-.0634332	.0915946	.4888693
# Bathrooms	176	414	2.5	2.521739	-.0217391	.0697518	.7554063
% Finished Basement	176	414	48.8125	54.14976	-5.337258	4.077032	.1910105
Year Built	176	414	1980.273	1973.529	6.743742	2.52227	.0077109

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	165	397	2488.6	2544.139	-55.53854	68.74514	.4194967
Acreage	165	398	.2455152	.2346985	.0108167	.0322203	.7372152
# Bedrooms	165	398	4.036364	3.801508	.2348561	.0983616	.0172851
# Bathrooms	165	398	2.672727	2.610553	.0621745	.0678181	.3596503
% Finished Basement	165	398	59.80606	53.13317	6.672895	4.148751	.1083079
Year Built	165	398	1979.436	1977.49	1.946414	2.458631	.4288901

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	184	339	2541.31	2646.198	-104.8879	66.46036	.1151262
Acreage	184	339	.2010326	.2175811	-.0165485	.013994	.2375298
# Bedrooms	184	338	3.896739	3.828402	.0683368	.0937991	.4666086
# Bathrooms	184	339	2.673913	2.666667	.0072464	.074596	.9226513
% Finished Basement	184	339	54.14674	55.9351	-1.788364	4.059224	.6597085
Year Built	184	339	1979.554	1976.681	2.872932	2.691244	.2862357

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	181	309	2684.066	2836.725	-152.6586	69.23359	.0279213
Acreage	181	309	.2028177	.2331392	-.0303215	.0178068	.0892418
# Bedrooms	181	308	3.966851	3.996753	-.0299024	.0975933	.7594325
# Bathrooms	181	309	2.674033	2.834951	-.1609183	.067575	.0176328
% Finished Basement	181	309	56.97238	54.6343	2.338071	4.190598	.5771462
Year Built	181	309	1978.343	1979.961	-1.618624	2.635803	.5394411

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	166	378	2779.669	2812.926	-33.25725	71.83693	.6435826
Acreage	166	378	.235241	.2538624	-.0186215	.0323311	.5648807
# Bedrooms	166	378	3.993976	3.957672	.0363039	.1015195	.7207777
# Bathrooms	166	378	2.837349	2.820106	.0172436	.0704602	.8067597
% Finished Basement	166	378	52.13855	56.00265	-3.864091	4.167002	.3541798
Year Built	167	378	1981.443	1978.034	3.408722	2.584999	.1878403

Previous Sales Price +/- 500 from $260,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	116	237	2853.19	2886.211	-33.02132	97.38227	.734745
Acreage	116	237	.2456897	.2525738	-.0068842	.0221992	.7566613
# Bedrooms	116	237	4.017241	3.953586	.0636549	.1188995	.5927351
# Bathrooms	116	237	2.758621	2.746835	.0117852	.0884891	.894125
% Finished Basement	116	237	56.08621	55.4346	.6516077	4.958629	.8955272
Year Built	116	237	1982.233	1975.321	6.912084	3.113007	.0270305

Previous Sales Price +/- 500 from $270,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	89	215	2977.202	3056.056	-78.85357	109.4157	.4716645
Acreage	89	215	.3230337	.2884651	.0345686	.0693026	.6182787
# Bedrooms	89	215	3.921348	4.204651	-.2833028	.1263453	.0256681
# Bathrooms	89	215	2.707865	2.865116	-.1572511	.0966295	.1047031
% Finished Basement	89	215	53.83146	63.51163	-9.680167	5.374095	.0726586
Year Built	89	215	1980.36	1977.135	3.224667	3.179387	.3112792

Previous Sales Price +/- 500 from $280,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	80	201	2864.863	3071.791	-206.9285	115.0095	.0730627
Acreage	80	201	.18475	.2429851	-.0582351	.021229	.0064787
# Bedrooms	80	200	3.8375	4.275	-.4375	.1494395	.0036984
# Bathrooms	80	201	2.8	2.870647	-.0706468	.1042266	.4984483
% Finished Basement	80	201	57.825	62.18905	-4.364055	5.679655	.4429195
Year Built	80	201	1977.875	1975.597	2.277985	3.825435	.5520027

Previous Sales Price +/- 500 from $290,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	93	197	3043.462	3183.102	-139.6392	112.0905	.2138606
Acreage	93	197	.2849462	.2973604	-.0124142	.0542294	.819094
# Bedrooms	92	197	4.163043	4.116751	.0462922	.1376178	.7368284
# Bathrooms	93	197	2.935484	2.956853	-.0213689	.0982114	.8279098
% Finished Basement	93	197	46.80645	58.16244	-11.35598	5.755631	.0494491
Year Built	93	197	1988.538	1973.132	15.40565	3.58408	.0000236

Previous Sales Price +/- 500 from $300,000 Threshold

Variable	N Below	N Above	Mean Below	Mean Above	Diff.	Std. Dev. Diff.	P-Value
Square Feet	92	155	3033.772	3141.329	-107.5573	125.0384	.390523
Acreage	92	155	.2502174	.2450968	.0051206	.0278638	.8543433
# Bedrooms	92	155	4.097826	4.154839	-.0570126	.1486153	.7015887
# Bathrooms	92	155	2.793478	2.909677	-.1161992	.1121207	.3010486
% Finished Basement	92	155	59.86957	62.71613	-2.846564	5.73275	.6199557
Year Built	92	155	1982.739	1979.09	3.648808	3.582117	.3093893

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 A2: Study 1 Control Variables

Archival Data Variable	N	Mean	Std Dev	Min	Max
House Controls					
Total # of bedrooms	83164	3.70	1.06	1	15
Total # of bathrooms	83163	2.30	0.81	1	7
Total # of kitchens	83164	1.05	0.22	1	5
Total # of fireplaces	83164	0.62	0.70	0	11
Total # of laundry rooms	83160	0.98	0.28	0	3
Total # of dining rooms	83164	0.15	0.36	0	2
Total # of family rooms	83161	1.18	0.68	0	4
% of basement finished	83163	57.27	44.39	0	100
Garage capacity	83158	1.42	0.95	0	16
Pool	83164	0.07	0.26	0	1
log(square feet)	83164	7.57	0.35	6.21	8.92
log(acres)	83164	0.16	0.10	0	2.94
Year built	83164	1974	26.23	1848	2013
Property type	83152	3.92	0.80	1	6
Quality control dummies (*See list below)					
Transaction Controls					
Immediate possession	83164	0.27	0.44	0	1
Dual agent	83164	0.13	0.33	0	1
Dual office	83164	0.10	0.30	0	1
Time and Geographic Controls					
Days since last sale	83164	1514.16	1103.70	1	6530
Year	83164	2007	3.70	1996	2014
Month	83164	6.12	3.23	1	12
Zip	83164	84142	162.14	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 A3: Study 1 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	**4127.44**	**2844.59**	**3063.72**	**2476.54**	**3103.69**	**2449.98**
F-stat	**66.30**	**12.09**	**30.08**	**10.43**	**30.13**	**10.15**
Prob > F	**0.0000**	**0.0005**	**0.0000**	**0.0013**	**0.0000**	**0.0015**
$100k	2426.3**	-2771.7**	1336.2	-1046.2	1461.8	-989.6
	(1154.6)	(1397.5)	(1031.9)	(1333.3)	(1033.4)	(1340.4)
$110k	451.2	-1264.6	22.1	-1346.8	-15.2	-1307.1
	(651.0)	(883.9)	(605.7)	(843.3)	(604.3)	(839.7)
$120k	613.7	426.4	251.2	882.6	183.2	859.1
	(490.5)	(791.4)	(533.4)	(807.4)	(532.0)	(800.1)
$130k	870.3*	1385.0*	687.9	2183.0***	668.1	2053.4***
	(446.8)	(726.6)	(468.3)	(747.9)	(465.6)	(748.6)
$140k	933.1**	899.3	914.7**	1569.2**	885.6*	1541.4**
	(372.1)	(652.6)	(462.6)	(681.8)	(462.5)	(676.5)
$150k	1713.3***	1432.5**	2018.6***	1700.0**	2059.7***	1770.5**
	(415.2)	(691.2)	(463.1)	(751.5)	(453.9)	(757.3)
$160k	1687.3***	1725.3**	1349.6***	2112.7**	1352.5***	2157.3***
	(446.1)	(849.9)	(479.0)	(830.5)	(493.0)	(826.3)
$170k	2001.1***	-442.2	1465.5**	277.3	1420.9**	388.8
	(583.5)	(906.0)	(586.2)	(971.8)	(602.9)	(967.6)
$180k	1413.6**	1519.6	1917.3***	270.5	1853.1**	368.0
	(616.3)	(985.2)	(721.7)	(1081.3)	(724.1)	(1087.1)
$190k	2741.0***	-461.2	1090.6	1574.5	1000.7	1722.7
	(845.2)	(1150.1)	(707.8)	(1271.7)	(715.2)	(1263.5)
$200k	2352.3***	-462.8	2519.4***	-1131.3	2416.6***	-953.0
	(825.2)	(1292.1)	(885.6)	(1454.3)	(871.6)	(1444.7)
$210k	2505.1***	1664.3	561.4	1056.4	787.6	933.4
	(887.1)	(1305.0)	(817.6)	(1588.6)	(821.4)	(1595.0)
$220k	3606.2***	3546.5**	1423.2	3350.2*	1349.8	3234.7*
	(871.9)	(1778.0)	(949.3)	(1746.8)	(939.4)	(1748.3)
$230k	6026.9***	6585.5***	4567.1***	3955.4*	4706.2***	3733.9*
	(1038.2)	(2018.2)	(1293.5)	(2043.5)	(1295.4)	(2056.9)
$240k	6833.1***	6546.9**	2289.5	4451.7*	2486.0	4692.1*
	(1481.2)	(2583.3)	(1624.5)	(2422.8)	(1639.9)	(2445.3)
$250k	4908.8***	7792.7**	2984.5*	5140.3*	3180.4*	5005.4*
	(1519.3)	(3059.8)	(1628.7)	(2877.0)	(1659.8)	(2900.7)
$260k	11285.2***	11029.2***	6285.2***	7600.7**	6459.2***	7341.1**
	(1926.6)	(3546.1)	(2010.7)	(3118.5)	(2064.9)	(3134.5)
$270k	6980.6***	7403.0**	6311.8***	7826.8**	6498.9***	7738.6**
	(2252.8)	(3582.8)	(2145.5)	(3239.8)	(2152.3)	(3259.2)
$280k	9466.5***	10353.0**	7243.6***	6706.3*	7525.2***	6579.9*
	(2231.2)	(4038.6)	(2337.0)	(3657.0)	(2362.3)	(3718.2)
$290k	10645.8***	-1046.8	9856.1***	1782.3	9905.9***	1089.7
	(3235.1)	(4818.0)	(3265.2)	(4554.9)	(3281.3)	(4588.4)
$300k	7215.0*	3876.4	9242.8**	3091.7	8991.2**	3489.4
	(4017.5)	(4965.1)	(3635.4)	(5158.7)	(3691.0)	(5211.7)
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	48237	20179	48237	20179	48237	20179
R-squared	0.943	0.936	0.928	0.922	0.927	0.921

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 A2. Large and small firms are measured as being above or below the median of brokerage size, calculated using the number of agents employed at the brokerage in a given year. * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Study 1 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	**3705.88**	**3727.24**	**3020.66**	**2670.73**	**3018.82**	**2720.09**
F-stat	**38.93**	**37.63**	**24.35**	**15.90**	**24.12**	**16.19**
Prob > F	**0.0000**	**0.0000**	**0.0000**	**0.0001**	**0.0000**	**0.0001**
$100k	-917.8	2568.0*	-399.7	1472.9	-282.4	1584.4
	(1048.4)	(1387.5)	(910.5)	(1277.9)	(918.8)	(1276.1)
$110k	-1081.5	1185.0	-820.7	165.4	-811.5	152.8
	(713.6)	(765.7)	(695.5)	(700.9)	(702.0)	(688.8)
$120k	461.4	677.9	663.9	90.0	627.3	26.4
	(570.0)	(605.8)	(565.1)	(672.0)	(559.2)	(673.7)
$130k	1065.7*	974.5*	1484.1**	621.5	1377.4**	650.8
	(579.3)	(510.2)	(617.2)	(500.9)	(614.8)	(495.3)
$140k	944.5*	921.7**	1360.1***	830.1	1353.2***	789.2
	(493.0)	(419.5)	(509.0)	(562.6)	(507.6)	(561.6)
$150k	1568.7***	1665.4***	2105.0***	1684.5***	2064.0***	1843.8***
	(516.1)	(479.0)	(573.7)	(539.7)	(590.7)	(503.2)
$160k	1799.5***	1489.7***	2205.5***	831.9	2286.0***	765.1
	(644.4)	(480.1)	(603.9)	(567.8)	(601.1)	(590.0)
$170k	1160.7*	1184.5*	1147.5	825.4	1178.2	795.7
	(702.8)	(654.7)	(762.1)	(677.9)	(765.5)	(694.1)
$180k	1295.4*	1482.0**	946.6	1691.3*	957.1	1646.4*
	(753.8)	(699.4)	(781.7)	(905.9)	(787.1)	(904.1)
$190k	2019.3**	1417.8	2180.4**	296.6	2159.4**	292.9
	(990.9)	(946.1)	(931.7)	(764.5)	(934.4)	(763.7)
$200k	639.7	2282.6**	-224.1	2810.1***	-59.1	2590.6**
	(1035.4)	(887.9)	(1028.1)	(1045.1)	(1018.6)	(1024.9)
$210k	2896.2***	1430.2	1068.3	179.9	1015.8	482.1
	(1092.4)	(934.3)	(1183.1)	(857.6)	(1181.7)	(870.8)
$220k	4074.6***	3152.1***	2899.9**	960.7	2933.0**	762.9
	(1231.7)	(969.6)	(1275.7)	(1124.8)	(1267.7)	(1107.3)
$230k	6809.0***	5371.4***	3271.6**	5104.5***	3212.4**	5231.7***
	(1400.9)	(1215.0)	(1491.6)	(1584.4)	(1481.5)	(1605.3)
$240k	6654.3***	6464.1***	2926.6*	2523.2	3125.3*	2746.1
	(1818.2)	(1829.8)	(1753.9)	(2075.3)	(1778.8)	(2104.4)
$250k	7718.9***	3947.2**	4749.9**	2673.5	4773.4**	2848.6
	(2096.7)	(1850.4)	(1940.4)	(2077.7)	(1950.7)	(2121.6)
$260k	11617.7***	10956.5***	7877.6***	5488.4**	7680.6***	5753.5**
	(2638.9)	(2093.6)	(2474.9)	(2330.6)	(2495.0)	(2398.0)
$270k	8132.3***	6098.4**	6878.7***	6395.2**	6844.0***	6652.5**
	(2562.5)	(2823.2)	(2509.0)	(2625.0)	(2522.2)	(2641.6)
$280k	12471.4***	6870.0**	8044.9***	6137.1**	8187.9***	6372.8**
	(2559.1)	(2796.6)	(2454.7)	(3014.7)	(2488.1)	(3033.7)
$290k	2624.0	11327.6***	9229.5***	5610.1	8806.5**	5710.7
	(4059.2)	(3712.0)	(3470.4)	(4016.8)	(3515.6)	(4034.5)
$300k	5869.5	6805.4	5838.2	9693.0**	5966.9	9422.8**
	(4714.8)	(4268.2)	(4377.2)	(4109.2)	(4447.9)	(4136.7)
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	34531	33885	34531	33885	34531	33885
R-squared	0.938	0.944	0.922	0.930	0.921	0.929

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 A2. Franchise brokerages are brokerages that belong to national franchised chains (i.e., Keller Williams, RE/Max, Coldwell Banker, etc.). * p<0.1, ** p<0.05, *** p<0.01.

Table A5: Study 1 Main Discontinuity Results, 7th Order Polynomial

Dependent Variable:	(1) List Price	(2) List Price	(3) Sales Price	(4) Sales Price	(5) Net Price	(6) Net Price
Avg. Discontinuity	**6296.26**	**3000.77**	**5291.53**	**2068.77**	**5307.29**	**2098.06**
F-stat	**72.10**	**80.79**	**52.95**	**32.31**	**53.50**	**33.00**
Prob > F	**0.0000**	**0.0000**	**0.0000**	**0.0000**	**0.0000**	**0.0000**
$100k	7069.0***	608.2	5783.2***	450.0	5889.0***	560.7
	(1408.5)	(985.8)	(1445.9)	(845.1)	(1426.3)	(847.5)
$110k	6445.3***	438.4	4960.6***	64.1	4909.1***	67.9
	(1150.8)	(596.7)	(1053.6)	(530.2)	(1096.5)	(524.1)
$120k	6350.8***	1021.0**	5507.0***	781.3*	5409.1***	739.8
	(960.8)	(422.5)	(891.2)	(458.4)	(898.5)	(458.1)
$130k	5171.4***	1308.7***	4837.8***	1250.6***	4869.3***	1207.6***
	(872.6)	(361.7)	(876.4)	(390.0)	(884.6)	(385.7)
$140k	3505.7***	885.2***	3968.3***	931.0**	3931.1***	904.4**
	(1053.5)	(333.0)	(976.6)	(386.2)	(996.2)	(384.5)
$150k	2201.0**	1259.9***	2527.0***	1481.0***	2599.5***	1517.7***
	(954.2)	(383.7)	(867.7)	(379.5)	(872.1)	(376.9)
$160k	1860.2*	1165.1**	2374.7**	1162.7**	2346.7**	1161.5**
	(1037.4)	(459.1)	(1027.7)	(482.1)	(1013.4)	(487.3)
$170k	2705.3**	720.6	2165.6*	797.3	2139.7*	771.5
	(1357.0)	(469.2)	(1205.5)	(495.4)	(1212.3)	(501.9)
$180k	2214.4	1089.7**	2067.9	1374.0**	2015.3	1342.1**
	(1387.8)	(514.5)	(1352.3)	(597.7)	(1366.6)	(596.6)
$190k	2973.2*	1805.3**	2781.7**	1639.4**	2780.5**	1609.2**
	(1568.8)	(723.4)	(1329.9)	(649.2)	(1334.7)	(651.3)
$200k	3066.2*	1829.9**	3494.5**	2011.4**	3375.5*	1989.8**
	(1776.0)	(823.1)	(1748.6)	(878.1)	(1735.7)	(864.1)
$210k	6559.7***	2946.9***	5104.1**	1501.4*	5139.0**	1637.0*
	(2168.4)	(859.2)	(2048.7)	(858.7)	(2059.7)	(864.9)
$220k	9349.9***	4634.3***	7903.0***	2741.1***	7894.6***	2679.5***
	(2349.7)	(948.2)	(2263.1)	(952.5)	(2276.1)	(946.4)
$230k	8728.9***	7064.8***	8553.8***	4680.8***	8585.9***	4754.0***
	(2485.3)	(969.2)	(2379.8)	(1073.9)	(2394.5)	(1076.0)
$240k	12943.3***	7363.8***	8905.8***	2680.8*	9052.2***	2925.7*
	(2492.0)	(1360.3)	(2875.1)	(1488.9)	(2893.8)	(1502.7)
$250k	8223.7***	5858.5***	6041.7**	2777.2*	6177.9**	2895.0*
	(3097.5)	(1529.9)	(2870.6)	(1538.3)	(2897.8)	(1561.7)
$260k	16293.2***	10657.1***	11327.0***	5323.1***	11469.2***	5387.2***
	(3700.3)	(1951.5)	(3406.1)	(1829.4)	(3465.8)	(1871.3)
$270k	13442.2***	5371.5**	11159.6***	5001.2**	11176.7***	5059.9**
	(3951.8)	(2116.6)	(3744.7)	(2129.4)	(3801.1)	(2141.0)
$280k	13118.1***	6987.1***	11658.6***	6795.7***	11693.0***	6848.8***
	(4154.8)	(2549.8)	(4233.6)	(2531.7)	(4253.0)	(2543.9)
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	81305	67014	67668	67014	67641	67014
R-squared	0.693	0.939	0.698	0.923	0.701	0.922

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 A2. * p<0.1, ** p<0.05, *** p<0.01.

Table A6: Study 1 Performance Results

Dependent Variable:	(1) Days on Mkt	(2) Days on Mkt	(3) Days on Mkt	(4) Prob.(Fail)	(5) Prob.(Fail)
Avg. Discontinuity	**1.6786**	**1.7753**	**1.5627**	**-0.0024**	**-0.0063**
F-stat	**2.56**	**2.97**	**2.36**	**0.16**	**1.87**
Prob > F	**0.1101**	**0.0851**	**0.1250**	**0.6890**	**0.1721**
$100k	-5.57**	-4.24*	-2.89	-0.017	-0.0062
	(2.42)	(2.41)	(2.52)	(0.014)	(0.011)
$110k	-2.33	-1.91	-2.24	-0.0093	-0.012
	(1.88)	(1.84)	(1.92)	(0.010)	(0.0090)
$120k	-0.23	0.98	0.23	-0.0075	-0.0092
	(1.62)	(1.60)	(1.63)	(0.010)	(0.0081)
$130k	-0.95	-0.35	-1.31	0.00039	-0.00042
	(1.65)	(1.63)	(1.63)	(0.010)	(0.0080)
$140k	0.40	0.56	1.20	-0.0036	-0.0091
	(1.45)	(1.47)	(1.43)	(0.0089)	(0.0075)
$150k	-0.23	-0.54	-0.11	0.0042	0.00019
	(1.76)	(1.76)	(1.73)	(0.010)	(0.0080)
$160k	1.18	1.47	2.21	0.019*	0.014*
	(1.57)	(1.56)	(1.55)	(0.010)	(0.0083)
$170k	1.63	1.36	2.04	-0.0051	-0.0042
	(1.92)	(1.89)	(1.74)	(0.012)	(0.0087)
$180k	0.59	0.90	2.46	0.020	0.014
	(2.00)	(2.00)	(2.05)	(0.012)	(0.010)
$190k	4.16**	4.63**	6.11***	0.012	0.011
	(2.04)	(2.03)	(2.00)	(0.013)	(0.010)
$200k	3.15	2.85	3.71	-0.011	-0.012
	(2.41)	(2.40)	(2.44)	(0.016)	(0.013)
$210k	1.78	2.60	4.15	-0.015	-0.0038
	(2.50)	(2.49)	(2.52)	(0.015)	(0.012)
$220k	3.56	3.76	3.63	-0.011	-0.019*
	(2.70)	(2.71)	(2.74)	(0.015)	(0.011)
$230k	4.56	4.30	4.33	0.0065	0.0030
	(2.93)	(2.91)	(3.04)	(0.018)	(0.014)
$240k	7.11**	7.84***	5.92**	-0.035**	-0.034**
	(2.97)	(2.92)	(2.81)	(0.018)	(0.014)
$250k	13.5***	12.9***	11.8***	-0.027	-0.032**
	(3.59)	(3.52)	(3.75)	(0.021)	(0.016)
$260k	5.82	4.51	0.89	-0.032	-0.037**
	(3.82)	(3.88)	(4.39)	(0.024)	(0.017)
$270k	7.58*	6.51	3.27	0.024	-0.00096
	(4.25)	(4.13)	(4.19)	(0.024)	(0.019)
$280k	0.33	-0.35	-2.59	-0.023	-0.028
	(4.38)	(4.37)	(4.61)	(0.025)	(0.020)
$290k	-0.31	-0.047	0.0049	0.062*	0.037
	(4.46)	(4.34)	(4.13)	(0.032)	(0.023)
$300k	-10.5*	-10.4*	-9.99*	-0.00096	-0.0054
	(6.16)	(6.04)	(6.00)	(0.037)	(0.026)
5th-Order Poly	YES	YES	YES	YES	YES
Time Controls	YES	YES	YES	YES	YES
Zip Code Dummies		YES	YES		YES
House Controls		YES	YES		YES
Transaction Controls		YES	YES		YES
Renovation Controls			YES		
Observations	83122	83079	68382	83164	83121
R-squared	0.092	0.118	0.113	0.094	0.366

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 A2. 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: Study 1 Subsample Results by Time Since Last Sale

		Average discontinuity	P-value	F-stat
Original List Price	First third	3730.65	0.0000	37.53
	Second third	2437.8	0.0008	11.31
	Final third	4296.57	0.0000	26.38
Sold Price	First third	2774.36	0.0000	17.36
	Second third	2784.48	0.0001	16.16
	Final third	2463.79	0.0007	11.45
Net Price	First third	2804.76	0.0000	17.91
	Second third	2734.01	0.0001	15.27
	Final third	2550.45	0.0006	11.82

Note: Our main model results were repeated above for subsamples based on the variable "time since last sale", split into thirds. "time since last sale" was calculated as the difference between the sale date and the next original listing date for a home. Models were estimated using the same approach and control variables as in the fully-controlled models presented in Table 2.

Table A8: Study 2 Pre-Test 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 A9: Study 2 Pretest Estimated List Price by Condition and House

Previous Sale Price Position Relative to Round Number	House 1				House 2			
	Previous Sale Price	Estimated Listing Price			Previous Price	Estimated Listing Price		
		M	*SD*	*N*		*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				House 4			
	Previous Price	Estimated Listing Price			Previous Sale Price	Estimated Listing Price		
		M	*SD*	*N*		*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	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 A10: Study 2 Pretest Linear Cross-Nested Mixed Models

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