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ABSTRACT

Estimates of loss aversion in housing sales prices may be biased because expected losses correlate with housing and borrower unobservables. We provide new evidence of loss aversion in sales price by differencing loss aversion estimates between sellers who exhibit focal point bias in their initial mortgage amount and those who do not. Although focal point bias and loss aversion are associated with different families of behaviors, recent evidence suggests links between these biases. Revisiting experimental data, subjects with high levels of loss aversion were more likely to use round numbers. Using housing data, estimates of loss aversion are 10 percentage points higher for sellers with round number mortgage amounts as a share of expected loss. Differences in expected loss are balanced over both housing and mortgage attributes, are stable as additional of controls are added, and are robust to using a discontinuity style regression centered on mortgage amounts of round numbers. On the other hand, traditional estimates of loss aversion fall by as much as 73 percent as additional controls are added. This study provides unique evidence that loss aversion and focal point bias are found together, and presents new, more robust evidence that loss aversion influences housing sales prices.

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1. Introduction

Loss aversion is a key phenomenon documented within behavioral economics and occurs when individuals place more weight on nominal losses than on gains and/or may face a psychological cost associated with realizing these losses.[1] Loss aversion in the housing market may be especially important due to the size of the housing market, the magnitude of any individual transaction, and the complexity of housing price dynamics. The seminal study in this area, Genosove and Mayer (2001), found that a $10,000 expected loss in Condominium sales in Boston led to approximately a $2,500 higher listing price, but found small, insignificant effects when looking at sales prices, after controlling for unobserved housing quality using previous sales price. On the other hand, Anenberg (2011), Bokhari and Geltner (2011), and Bracke and Tenreyro (In Press) use more heterogeneous samples of housing or commercial buildings in the case of Bokahari and Geltner (2011) and find much larger effects on sales prices ranging from 20 to 40 percent using similar models to Genosove and Mayer (2001).[2] These studies document important effects of loss aversion on market outcomes, specifically prices, beyond the effects on individual behaviors or choices, such as listing price.[3]

While Anenberg (2011) argues that larger effects of loss aversion would be expected in a more heterogeneous market where individual sellers have more market power, Genosove and Mayer (2001) note that the variable of interest, expected loss, may include both unobserved housing attributes and any premium paid or discount received at the time of original purchase,

[1] In behavioral economics, loss aversion can also be thought of as anchoring based on the purchase price. Other studies document anchoring based on housing prices in a buyer's previous location (Lambson et al. 2004; Simonsohn 2006) and based on previous sales within the same government program (Arbel et al. 2014).
[2] In one rare exception, Anderson et al. (2020) estimates a structural model of loss aversion estimating the concavity of the relationship between sale prices and expected gains and losses. Some studies examine loss aversion in housing markets using a low stake, experimental framework, see for example Scott and Lizieri (2012) and Paraschiv and Chenavaz (2011).
[3] Also see studies of loss aversion on mobility by Engelhardt (2003) and mortgage default by Ong et al. (2007).

which may lead to bias if exposure to expected losses correlates with other factors that influence sales price. For example, houses that are of higher quality may experience smaller declines in their market price during an economic downturn. Also, households with strong preferences for specific attributes may have fewer outside options and so be more likely to pay a premium above the market price. These same sellers may also differ from other homeowners in their likelihood of changing jobs or facing financial distress during an economic downturn when housing prices are often depressed.[4] This potential bias might be substantially less important in the relatively homogeneous sample of Boston condominiums and their owners examined by Genesove and Mayer (2001).

Using more than 500,000 single-family housing transactions in Connecticut from 1994 to 2017, we replicate the large effects of expected losses on housing prices estimating that expected loss raises housing sales prices by 40 percent of the amount of the loss. However, these results are very sensitive to controls. We conduct balancing tests regressing housing, mortgage, and census tract attributes on expected loss plus the standard controls in the Genesove and Mayer (2001) model. The model fails balance badly using a sample of heterogeneous, single-family housing with expected loss correlating with virtually all housing, mortgage, and location attributes. Expected loss is higher for larger housing units, larger mortgages, higher loan to value ratio mortgages, higher share minority locations, but also in higher income and more educated locations. Our estimates of loss aversion fall by half to 20 percent when adding these controls, and to only 10 percent after adding controls for tract fixed effects and fixed effects for detailed bins based on the mortgage amount.[5] This erosion of loss aversion estimates by the addition of controls should be

[4] See Shen and Ross (2021) and Nowak and Smith (2020) for evidence that the composition of owner-occupied housing on the market changes over both traditionally observable and unobservable housing attributes as the economy recovers from a downturn. Bayer et al. (2016) show that the composition of borrowers changes over the housing cycle.
[5] Zhou et al. (2021) also document a potential upward bias in estimates of the effect of loss aversion on sales prices based on changes in the composition of repeat sales transactions as housing price levels fluctuate, see Nowak and Smith (2020) and Shen and Ross (2021).

especially concerning given Stango and Zinman's (2020a) conclusion that standard demographics, cognitive ability and even personality traits are typically poor predictors of behavioral biases.

To develop more robust and compelling evidence on the effect of loss aversion on housing sales prices, we attempt to identify a dimension over which individuals are likely to differ in their extent of loss aversion so that we can condition out any overall correlation between expected loss and housing unit unobservables and/or more traditional economic unobservables of sellers, like non-housing wealth or employment stability. Stango and Zinman (2020a) conclude that behavioral biases are very stable within person over time, but as noted above find that traditional individual controls are relatively poor predictors of behavioral biases. However, several studies (e.g., Stango et al., 2017a, b; Pagel, 2018; Dean and Ortoleva, 2019; Stango and Zinman, 2020b) document that loss aversion is observed in conjunction with other behavioral biases.[6] Our goal in this paper will be to both contribute to the literature on psychological biases by documenting heterogeneity in loss aversion, and to provide new, more robust evidence on the effect of loss aversion on housing sales prices.

We propose focal point or round number bias as a second behavioral bias that we can observe for home sellers by looking at their initial mortgage amount choice when they purchased their current home. Pope et al. (2015), Chava and Yao (2017), Backus et al. (2019), Wiltermuth et al. (2020), Meng (2020), and Repetto and Solís (2020) provide evidence that focal point bias

[6] The literature offers no conclusive correlation between loss aversion and other behavioral bias. Stango et al. (2017a, b) examine the relationship between various biases and characteristics. Stango et al. (2017a) find that loss aversion is positively correlated with executive attention and risk aversion (financial), and negatively correlated with patience. Stango et al. (2017b) find that loss aversion is negatively correlated with college degree, positively correlated with age, income, and financial literacy. However, Chapman et al. (2018) find that loss aversion is more prevalent in people with high cognitive ability. Pagel (2018) proposes that news-utility preferences can generate inattention which is consistent with myopic loss aversion. Dean and Ortoleva (2019) suggest that loss aversion is strongly related to both buy risk and the endowment effect. Stango and Zinman (2020b) show that risk aversion is positively correlated with loss aversion and and ambiguity aversion.

influences housing market outcomes. Notably, Chava and Yao (2017), Wiltermuth et al. (2020), and Meng (2020) document persistent behavioral differences between buyers who coordinate on a round number for a housing sales price and those who do not. Chava and Yao (2017) show that focal point buyers were less likely to refinance their mortgage when interest rates fall,[7] and Wiltermuth et al. (2020) and Meng (2020) show that focal point buyers later listed and sold properties at different prices than non-focal point buyers with nearly identical purchase prices. We focus on the initial mortgage amount because sellers (in the first sale) have far less stake in the mortgage amount than, for example, the sales price, and lenders are typically focused on loan to value and income ratios, rather than the specific mortgage amount.

While focal point bias and loss aversion are associated with different families of behaviors, bounded rationality (e.g., Lacetera et al., 2012) and prospect theory (Barberis, 2013) respectively, recent evidence suggests a link between the two biases. Both behavioral biases involve anchoring either to previous information for loss aversion or to impressions created by salient left digits.[8] Gabaix (2018) ties loss aversion to limited attention, a foundation of bounded rationality models.[9] Pagel (2018) proposes a model that she describes as news-utility preferences, which incorporate a

[7] Deng et al. (2003) document such heterogeneity in the context of mortgage prepayment and default a separation between those who "ruthlessly" pull the trigger on these options when they are "in the money" and those who fail to act rationally when making these decisions.

[8] Numerous studies on consumer behavior and marketing study the effects of odd-numbered prices, e.g., 99-ending prices caused by left-digit bias, a tendency that consumers perceive a $4.99 as much lower than $5.00 (Kashyap, 1995; Levy et al., 2011; Hall, 2015). Other examples of focal point bias effects include suggested donation amount in direct mail fundraising (Reiley and Samek, 2019), effects of list price or registration year on sales price of used cars (Lecetera et al., 2012; Busse, 2013; Englmaier et al., 2018), borrowers using round numbers when misreporting assets in mortgage applications (Garmaise, 2015), and stock trading at round numbers of share prices (Bhattacharya et al., 2012; Kuo et al., 2015;).

[9] In Section 4.3.2 Motivated attention, Gabaix states that "in loss aversion, people pay more attention to losses than gains, something prima facie opposite to a self-serving attention bias." (p.303). This claim is consistent with an "ostrich effect" when people pay more attention to things that please them and avoid things that hurt them (Karlsson et al., 2009; Sicherman et al., 2016; Olafsson and Pagel, 2017). Pagel (2018) further extends this idea to a news-utility in which bad news hurts people more than good news pleases them.

coefficient of loss aversion by generating inattention. Fraser-Mackenzie et al. (2015) find evidence that left digit bias is more pronounced in financial markets when stock trades involve losses.

To test this proposition directly, we examine data from a recent experiment on loss aversion by Karle et al. (2015). In this study, consumers experience a loss by first being asked to identify a preferred sandwich between two choices and then face randomized prices for these sandwiches. This study experimentally measures loss aversion by presenting the subjects with a series of lotteries, and also surveys the subjects concerning how much they spend for lunch, creating an opportunity for the subjects to be either precise about the cost of lunch or to round the cost of lunch to the nearest 5 Euros. Individuals that reported round numbers when asked about past spending were measured as having substantially higher loss aversion in the experiment. Specifically, borrowers who reported round numbers score a half a standard deviation higher on an experimental measure of Tversky and Kahneman's (1992) loss aversion parameter than those who did not. This analysis strongly suggests that loss aversion is higher among individuals who tend to coordinate on round numbers.

Turning to the housing market, we test for a relationship between the effect of expected losses on sales price and whether a seller exhibits focal point bias or a bias towards round numbers when selecting their purchase mortgage amount, i.e., amounts in multiples of $5,000. This test can both provide evidence on whether loss aversion is higher for those who exhibit focal point bias, and given the balancing test failures above provide more robust evidence that loss aversion influences housing sales prices. Economic factors that correlate with losses during a housing downturn, such as the increased risk of unemployment, contractions in credit markets, and changes in the return to unobserved housing attributes, will likely affect all sellers whether or not their decisions suffer from psychological bias. These effects will be differenced out, at least partially, when we difference the estimates of loss aversion across the two groups.

Credit constraints also can be an important source of confounding variation because housing price declines that result in losses can also erode housing equity, reducing mobility and changing the bargaining position of sellers (Genosove and Mayer 1997; Chan 2001; Engelhardt 2003; Bracke and Tenreyro In Press).[10] Therefore, we define our sample of individuals who selected round numbers in the purchase mortgage amount, as well as our non-round number control group, excluding buyers who appear credit-constrained because they selected one of the key loan-to-value (LTV) thresholds that affect access to and the price of mortgage credit, e.g., 0.80, 0.90, 0.95, 0.97, and 1.00 or utilized subordinate debt, i.e., a second mortgage at origination. We do not include these observations in either our psychologically biased or control group subsample since conditional on housing price the economic incentives to bring LTV down to a specific threshold may result in a rational buyer choosing a round mortgage amount or a psychologically biased buyer being forced into a non-round amount.

Using the same Connecticut housing transaction data discussed above, we first show that there are unusually high probabilities of "round number mortgages", with the mortgage amounts ending with 5,000 or 10,000. When non-credit constrained borrowers decide how much to borrow, they potentially either calculate the loan amount rationally and precisely based on a desired down payment, or just take a rough guess potentially selecting a round number. In the former case, round loan amounts should be no more likely than non-rounded numbers. Therefore, given the spikes in the distribution of mortgage amounts at round numbers, a substantial share of homeowners at these

[10] Specifically, Bracke and Tenreyro (In Press) examine the relative importance of credit constraints and loss aversion by examining a subsample of cash only purchases. They find that loss aversion is relatively more important in affecting prices, while credit constraints are relatively more important in affecting selling propensities.

spikes likely systematically sorted into these round number outcomes. Accordingly, homeowners at round number mortgages are more likely to suffer from focal point bias, and we empirically test whether sales outcomes consistent with loss aversion are more likely among this subsample.

Specifically, we follow the analytical framework of Genesove and Mayer (2001) except that we interact both a dummy for round number mortgage and a dummy for credit-constrained borrower individually with the expected loss and with all additional controls. We find that the estimated coefficient on the interaction of round number mortgage with expected loss implies that our focal point biased subsample sells their houses at a price that is higher by approximately 10 percent of the expected loss relative to the sales prices for our control sample. Unlike direct effects of expected loss, balancing tests suggest that the difference in the correlation between expected loss and housing, mortgage, and location attributes between the focal point biased and control samples is modest. The balance test estimates are statistically insignificant for all of the mortgage and housing attributes. Further, while the interaction of loss and round number mortgage fails balance on a few census tract attributes, the inclusion of the balancing test attributes leaves the estimate on expected loss interacted with round number mortgage unchanged, while for our control group the estimated effect falls by half.

Next, we follow Backus et al. (2019) and estimate a model similar to a regression discontinuity model. Specifically, like Backus et al. (2019), we exploit the assumption of smoothness over the running variable to detect behavioral differences between individuals concentrated at round numbers and those at nearby continuous numbers, rather than detecting the effect of a treatment that occurs at or above a threshold. For non-credit constrained borrowers, a thousand dollars smaller or larger mortgage should not have a discrete effect on economic circumstances and so departures from a smooth relationship between mortgage amounts and sales price can be interpreted as evidence that individuals exhibiting behavioral biases are disproportionately concentrated at round number mortgages. We compare the sales price of borrowers who experience a predicted loss and have a mortgage amount exactly at a $5,000 increment to borrowers who have nearly identical mortgage amounts, but whose mortgage amounts are not a round number. After conditioning on a mortgage amount running variable, the magnitude of the interaction between expected loss and the round number mortgage dummy is very similar to our baseline regression results reported above, but the inclusion of mortgage amount bin fixed effects reduces the estimate on expected loss for the control group by more than half relative to our baseline estimates (and to between 1/4 and 1/3rd of the original estimate after adding additional controls). Therefore, the across group difference in the estimates of loss aversion is relatively stable to the use of a discontinuity-based identification strategy, while the control group/cross-sectional estimate of loss aversion erodes substantially when comparisons are restricted to borrowers with relatively similar sized initial mortgages.

Our paper contributes to the extensive literature on anchoring and loss aversion first by providing evidence that loss aversion is higher for individuals who appear to coordinate on round numbers. These effects are large with expected losses having almost double the effect for the round number mortgage sample than for our control sample after controlling for mortgage amount and census tract. To our knowledge, Wiltermuth et al (2020), and Meng (2020) are the only studies that document differences in future housing sales prices between individuals who selected or experienced a round-number transaction price and those that did not. However, our study differs in several ways. First, the observed differences in these earlier studies may arise because either the focal point buyers are different or because they are responding to the salient left digit in the previous price. By focusing on the mortgage amount, we identify differences between individuals rather than potentially confounding these differences with the effects of the left digit of the previous sales price. Then, we utilize these differences to show that loss aversion in the housing market is larger for owners who selected a round number mortgage amount than for owners that did not.

Finally, these findings provide more robust evidence that loss aversion can affect prices in housing market transactions. The estimated differences in the effect of expected losses on sales prices are quite robust to the addition of controls. On the other hand, estimates of the effect of loss aversion on market outcomes, i.e., sales prices, in a broad sample of housing transactions can be very sensitive to the inclusion of additional controls. For example, homeowners typically face losses during economic downturns, and compositional changes in the housing market over the business cycle, as documented by Shen and Ross (2021) and Nowak and Smith (2020), may create a strong correlation between expected losses and unobserved housing and neighborhood quality.

2. Experimental Evidence on Loss Aversion and Round Numbers

To provide direct evidence of a connection between loss aversion and the selection of round numbers, we test the relationship between reporting round numbers on a survey question and an estimated measure of loss aversion using data from an earlier experiment. Karle et al. (2015) study the effect of loss aversion on consumption using an experiment where they first ask individuals to report their preferences between two sandwiches and then randomize the prices of the two sandwiches so that some buyers face a loss involving a higher than expected price for the sandwich that they prefer. This experiment was conducted with University of Mannheim students in the fall of 2010. As part of this study, they had individuals make choices across a series of lotteries and sure pay-offs intended to measure loss aversion to estimate the loss aversion parameter from Tversky and Kahneman's (1992) exponential utility function. To mitigate the effect of outliers, they also categorize the continuous measure by assigning individuals values between 1 to 4 capturing from “loss-seeking or neutral” to “strongly loss-averse.” In addition, individuals were asked to report how much they typically spend on lunch in Euros when they went out to buy lunch. We then create a variable that takes a value one if they report spending 5, 10, or 15 Euros on lunch, and zero if they reported a non-round number for lunch expenditures. Approximately 21% of the sample reported typically spending 5, 10, or 15 Euros on lunch with most of those individuals reporting 5 Euros. Following Karle et al. (2015), we drop individuals from the sample who gave inconsistent responses to the lottery questions that were used to assess loss aversion.

Table 1 presents the results of models that regress either the continuous measure of loss aversion (columns 1-3) or categorical measure (columns 4-6) on a dummy variable for whether the individual reported either 5, 10, or 15 Euros as what they typically spend on lunch. Columns (1) and (4) present the univariate regression, columns (2) and (5) present results adding standard controls for age, gender, income, and number of semesters of university completed. The last two columns control for the self-report of the number of times the individual typically eats lunch out each week. All coefficient estimates on the round number dummy variable are statistically significant and sizable. The univariate models imply 3.34 higher loss aversion for the round number subsample using the continuous measure or 0.49 standard deviations and 0.56 higher or 0.58 standard deviations for the categorical measure. The addition of controls tends to erode the magnitude of the estimates, but they are still sizable ranging between 0.44 and 0.50 standard deviations. The subset of people who rounded when responding to a specific question exhibited higher levels of loss aversion in an experiment that is entirely independent of our study. These results support our suggestion that focal point bias and loss aversion can be together.

3. Housing Sales Data and Sample Construction

Our housing data contains 548,568 single-family residential transactions between January 1994 and December 2017 in 6 labor market areas (LMAs) across 169 towns in Connecticut.[11] Data were collected by town halls monthly from 1994 onward. Our data contain property characteristics and mortgage information at the time of each sale. These timely records are important because many repeat sales studies (e.g., Anenberg, 2011) have property characteristics at the time of the second sale, but not the first sale.[12] To avoid a common bias in repeat sales that arises from ignoring home improvements, we mitigate the threat of large unobservable quality changes between sales by deleting observations with changes in interior size between sales greater than 5%, and directly control for any smaller changes in the housing unit by calculating predicted price based on the hedonic attributes at the time of the sale.

Our data also include the names of buyers and sellers, which allows us to use a fuzzy logic routine to ensure that the seller in the second transaction was the buyer in the first, as required to assign an expected loss and initial mortgage amount to an individual seller. When there is more than one buyer or seller recorded, we ensure that at least one of the sellers/buyers in the second transaction was the buyer/seller in the first.[13]

With a fixed starting point of the sample, the number of repeat sales tends to increase as one moves forward in time through the sample. As a result, we require that the second sale occurs after 1999, a point at which the ratio of repeat sales to all sales has stabilized within our sample.

[11] Our initial sample includes over 1.5 million transactions. Following Clapp and Salavei (2010), our sample is restricted to single-family residential properties with 1) warranty deeds, 2) sale price over $50,000, 3) interior footage over 300sf and lot size between 1,500 sf and 10 acres, 4) more than three rooms and at least one bathroom, 5) structures built between 1901 and 2013, and 6) records of assessed building and land value.
[12] The towns are well distributed throughout the state, with good representation beyond the I-95, I-91 and I-84 corridors. Most importantly, there are many towns far from New York City, an international financial hub that has grown rapidly over the 20 years covered by the study. Connecticut is often described as two states: the wealthy, growing southwestern towns closest to the New York City and the remainder of the state.
[13] We use Matchit in STATA to perform the fuzzy match.

Otherwise, the sample will substantially over-represent homes that sell rapidly. Our repeat sale sample includes 139,674 observations of second sales. The repeat subsample contains somewhat smaller, older houses than the single sale sample, as has been found in previous studies.

When analyzing sale probability, we assume that the relevant population of houses are those ever sold during our sample period, 1994-2017. We then construct a sample of 4,058,238 house-year observations based on 366,557 unique properties. This sample consists of 500,579 sale spells, which start from the year after the sale of a property and end in the year of the next sale or the end of our sample period to account for censoring. For example, if a property sells for the first time in 2003, it cannot be included in the sample until 2004 as the loss and gain variables before 2003 are unknown. Similar to our analysis using sale prices, we begin the analysis of sale probability using sales that occur in 2000 or later to make our repeat sample more representative.

We classify the buyers of the first sale (i.e., the sellers of the second sale) into three groups: the round number mortgage group, the LTV-focused group, and the control group. The LTV-focused group includes likely credit-constrained buyers who either select one of the key LTV thresholds (i.e., an LTV ratio equals one of the critical ratios, e.g., 0.80, 0.90, 0.95, 0.97, and 1.00, that suggest that the buyer targeted an important LTV ratio in the market) or took out a second mortgage at the time of purchase (subordinate debt). Appendix 2 summarizes details on identifying critical LTV ratios. The round number mortgage group includes buyers with the mortgage amounts ending with 5,000 or 10,000 excluding buyers in the LTV-focused group. The control group includes the rest of the sample after removing both the round number and the LTV-focused samples. By construction, the control group includes only buyers who had non-round mortgage amounts and did not appear to be financially constrained.

Figure 1 provides a visualization of the clumping of mortgage amounts at round numbers that are multiples of $5,000. We group mortgages into $1,000 bins (rounded down). There is an excess mass of round number mortgages, highlighted by the gray bars. In Table 2, we present the descriptive statistics separately for the expected loss and the expected gain subsamples. We find that sellers with an expected loss are more likely to be in our round number subsample. They also appear to be more financially constrained. The comparison of "months since purchase" reveals that loss is positively correlated with the holding period between sales, consistent with loss aversion. In terms of housing attributes, houses in the loss sample are larger and older. Sellers with an expected loss have lower LTV ratios and are less likely to have a second mortgage at purchase. In terms of neighborhood attributes (measured using the 1990 census prior to our sample period), sellers with expected loss are in census blocks with a higher percent of male, white, and a college degree. Sellers with an expected loss also reside in neighborhoods with higher household income, lower poverty, and lower unemployment rate. These differences suggest that the sample may not be balanced between home sellers with expected losses and those with expected gains. We will present formal balancing tests after presenting our empirical model specifications.

3. Empirical Models and Results

We first model the log of sale price for seller *i* of type *l* in the quarter of purchase *s*, the quarter of sale *t*, and labor market area *c*, in a repeat sale framework following Genesove and Mayer (2001) as:

$$P_{ilstc} = \beta Round_{ils} * Loss_{ilst} + \gamma LTVfocus_{ils} * Loss_{ilst} + \boldsymbol{\delta X}_{ils} + \theta_{ct} + \varepsilon_{ilstc} \quad (1)$$

where $Round_{ils}$ is the round number mortgage dummy which equals one for sellers whose first mortgage amount (taken from the time of purchase) ends with zero or five on thousands and without maximizing critical LTV thresholds, and zero otherwise; $LTVfocus_{ils}$ is a dummy variable which equals one if buyers select one of the key LTV thresholds (e.g., 0.80, 0.90, 0.95, 0.97, and 1.00) or use second mortgages (i.e., subordinate debt) to maximize mortgage credit. $Loss_{ilst}$ is expected loss and defined as the difference between the purchase price and the expected market value of the second sale truncated above zero. i.e., $Loss_{ilst} = \left(logPrice_{ils} - log\widehat{Price}_{ilt}\right)^{+}$. $\boldsymbol{X}_{ils}$ includes $lumpy_{ils}$, $LTVfocus_{ils}$, $Loss_{ilst}$, and standard controls in the GM model, including the expected market value of the second sale, residual from the first stage hedonic model, months since purchase, and equity position at the second sale, as well as the interaction of $lumpy_{ils}$ and $LTVfocus_{ils}$ with these additional controls. For equity position or current LTV based on expected sales price, we follow Anenberg (2011) and Abel (2018) and measure equity position using an estimated remaining mortgage balance amortized using the 30-year FHFA mortgage rate observed at purchase. θ_{ct} is the LMA-by-year-by-quarter fixed effects and absorbs time-varying local market conditions and seasonality.[14] We cluster standard errors at the same level as our LMA-quarter fixed effects.[15]

Bokhari and Gelter (2011) find that sellers with an expected gain are willing to accept a lower price. For completeness, we next include the expected gain in equation (1) and run the following regression:

$$P_{ilstc} = \beta^{l} Round_{ils} Loss_{ilst} + \gamma^{l} LTVfocus_{ils} Loss_{ilst} + \beta^{g} Lumpy_{ils} Gain_{ilst} + \gamma^{g} LTVfocus_{ils} Gain_{ilst} + \boldsymbol{\delta X}_{ils} + \theta_{ct} + \varepsilon_{ilstc} \quad (2)$$

[14] Results are very similar using models with town-by-year fixed effects (Clapp and Zhou, 2019; Clapp et al., 2020).
[15] As there are only six LMA's, we cluster at LMA-quarter level which is the same level as our fixed effects (Bertrand, Duflo, and Mullainathan, 2004). Results clustered at the LMA level are highly similar.

where $Gain_{ilst}$ is expected gain, measured using the difference between the purchase price and the expected market value of the second sale truncated below zero, i.e., $Gain_{ilst} = \left(logPrice_{ils} - \widehat{logPrice}_{ilt}\right)^{-}$.

Before our analysis, we perform a battery of balancing tests. The objective is to examine whether the sample is balanced in terms of hedonic, mortgage, and census characteristics overall, and then whether it is balanced in terms of the differences between the round number mortgage and the unconstrained control group samples. We replace the outcome variable in equation (2) with a rich set of house, mortgage, and census characteristics. Columns (1) and (2) of Table 3 show the coefficients and standard errors of expected loss. The results suggest that expected loss is correlated with most of the observed attributes. As a result, losses may also be correlated with unobservables which might bias both the loss effects estimated cross-sectionally for our control group and those results documented in the previous literature.

However, most of the coefficients for the interactions between expected loss and the round number mortgage dummy in columns (3) and (4) are smaller than the direct estimates on loss and statistically insignificant, suggesting at most modest evidence that attributes differ based on losses when we compare the round number mortgage sample to the control group. All the loss interaction coefficients for housing and mortgage attributes are statistically insignificant, and many of the interaction coefficients for census characteristics are statistically insignificant. However, we fail balance for few census characteristics such as racial composition, poverty, and housing vacancy. We partially mitigate this concern in later analyses when we include interactions between borrower-type dummies (i.e., round number mortgage and LTV focused) and all the hedonic, mortgage, and census characteristics. The purpose of this follow-up analysis is to examine the stability of the loss aversion estimates to these controls both for the control group and for the difference between the round number mortgage group and the control group. We will also examine models that include borrower-type-by-tract fixed effects to control for tract unobservables since all of our balance failures on the interaction terms arise on tract attributes.

3.1 Results for Expected Loss

Table 4 shows our baseline results. We start with a standard model used in Genesove and Mayer (2001) in column (1), i.e., equation (1) without the seller type interaction terms. The estimated coefficient for expected loss is large: a 10% expected loss is associated with 4% increase in sale price. In column (2), when we interact the round number mortgage dummy with expected loss and other controls, we find that those focal point biased individuals sell their houses at a price that is higher by about 10 percent of the loss relative to the control group. In column (3), we add expected gain to the model from column (1). In column (4), we further add gain interactions with seller types to the model from column (2). We find that the magnitudes of both the coefficients on *Loss* for the control group and on *Loss*Round Mortgage* are stable with the inclusion of controls for gains. The control variables have the expected sign and magnitude as in previous studies.

We show additional results in Table 5. Following the literature (e.g., Genesove and Mayer, 1997, 2001; Engelhardt, 2003; Anenberg, 2011), we use an alternative equity position calculated based only on current LTV when above the key threshold of 0.8 so that equity position is the minimum of zero and the estimated current LTV minus 0.8.[16] Panel A reproduces the main results from Table 4, and Panel B presents results with the alternative measure for equity position. The coefficient estimates for loss and loss interaction with round number mortgage are highly similar

[16] Genesove and Mayer (1997, 2001) use Stein (1995)'s theory as motivation to look for a reduced form relation between high LTV ratios, loss aversion, and prices. They find no statistically significant effect on selling prices for LTV values below 0.8, consistent with the theory of a threshold effect.

to the baseline results in Panel A and Table 4. The coefficients for truncated equity position (unreported) are positive, but statistically insignificant.

In Panel C of Table 5, we model sale hazard in a panel data framework. We write the hazard function for homeowner *i* of type *l* at the year of purchase *s*, calendar year *t*, and labor market area *c*, as:

$$Prob(Sale)_{ilstc} = Round_{ils} * Loss_{ilst} + \gamma LTVfocus_{ils} * Loss_{ilst} + \boldsymbol{\delta X}_{ils} + \theta_{ct} + \varepsilon_{ilstc} \quad (3)$$

The difference between equation (1) and (3) is that equation (1) models sale prices at the time of the second sale, while equation (3) models sale probability as a survival likelihood using panel data with every year following the initial purchase representing an observation until either a sale occurs or the end of the sample is reached. This means that the loss and gain variables vary over time *t* within a sale spell as the market price varies over time. In our baseline controls in equation (3), we also control for time-varying equity position, again calculated using the estimated remaining mortgage balance divided by the expected sales price at time *t.* Instead of months since purchase, we control for years since purchase to fit our house-year panel. θ_{ct} is the LMA-by-year fixed effects. We again cluster standard errors at the LMA-year level.

We estimate equation (3) using a linear probability model to allow for a more exhaustive set of controls while maintaining computational tractability. The results in Panel C suggest a negative relationship between loss and sale hazard consistent with loss-averse sellers leaving the property on the market longer to hold out for a higher sales price. Specifically, a 10% increase in loss is associated with a 0.7 to 1 basis point reduction in sale hazard among round number sellers, relative to the control group. The effect is economically significant because the yearly hazard is

3.66 basis point. In unreported results, there is no effect on the gain side as the gain coefficients and gain*round interactions are insignificant.

3.2 Stability of the Estimated Effects of Expected Loss

Given that our sample fails balance cross-sectionally over most housing mortgage and neighborhood attributes, we examine whether our estimation results are stable when we include all the balance controls for the hedonic, mortgage, and census characteristics (see Table 3) and further interact the round number mortgage dummy, as well as the LTV focused dummy, with these variables. Comparing our baseline results reproduced in Panel A of Table 6 with the results adding balance control interactions in Panel B, we find the sales price interaction coefficients for loss and round number mortgage are still statistically significant and virtually identical in magnitude. However, the expected loss coefficients for the control group fall by almost 50% from 0.4 to 0.2. The cross-sectionally estimated effects of expected loss are far more sensitive to controls for observables than the interactive effect arising from our round number mortgage subsample. However, comparing Panel C with Panel D for the probability of sale, we find the coefficients for *Loss*Round Mortgage* becomes substantially more negative when we include balance control interactions, suggesting a potential positive bias created by omitted variables.

We further include seller-type-by-tract fixed effects in Table 7. We still observe significantly positive (negative) coefficients for *Loss*Round Mortgage* in Panels A and B (C and D) using sale prices (sale probability) as the outcome variable. The magnitudes of the interaction coefficients reduce from 0.1 to 0.07 when we include tract by seller type fixed effects in our model sale prices. The use of tract fixed effects and their interactions controls for tract-level unobservables, but also may exacerbate measurement errors in the expected loss experienced by the borrowers due to the limited number of repeat sales transactions within each census tract. The estimates for the probability of sale remain stable at 0.01 with the inclusion of tract by seller type fixed effects.

We next follow Backus et al. (2019) and estimate a model similar to a regression discontinuity design (RDD) model where the discontinuity occurs at round numbers. Like an RDD, the model exploits the assumption of smoothness of unobservables over the running variable, in our case unobservables related to economic circumstances. However, instead of modeling the effect of a treatment that occurs at a discontinuity, this model captures unobservables differences between individuals associated with behavioral biases that are disproportionately associated with buyers/borrowers who coordinated on round numbers when selecting their mortgage amount. This design also differs from the traditional RDD design because the discontinuity test is estimated using only the observations exactly at the discontinuity, rather than those to the right of the discontinuity. The idea is to compare the sale prices of a seller who experiences an expected loss and has selected a mortgage amount exactly at a round number, with a seller who has a similar mortgage amount but on either side of this round number, under the argument that small changes in mortgage amount should result in only small changes in economic circumstances.

In Figure 2, we plot the stacked discontinuity sample by creating a \$2,500 bandwidth on either side of each round mortgage amount where the running variable takes a value of zero at the round number mortgage amount. Each \$2,500 bin is further divided on either side into 10 bins, and we have an 11^{th} bin containing the individuals with round number mortgage amounts. The observations of credit-constrained borrowers are dropped. The gray dots are unconditional correlations between expected loss and the difference between sale price and expected sale price. The gray lines are fitted lines for the 10 points below 0 and separately for the 10 points above 0. The pattern suggests that there is a discontinuity and with a substantially higher correlation at zero.

As a comparison, we also plot the correlation between expected loss and selected housing, mortgage, and census attributes. For ease of presentation, we show two attributes in each category in Figure 3. Unlike Figure 2, all correlations at the round number in Figure 3 are within the scatterplot of correlations for the non-round number mortgage bins.

For our parametric models, we take mortgage multiples of $5,000 to be the round number thresholds and define the operator $round(x, y)$ to be the value of x rounded to the nearest positive multiple of y. Essentially, we created the same stacked discontinuity sample from Figures 2 and 3 with a $2,500 bandwidth on either side by defining normalized mortgage amounts, *Run*, as

$$Run = mortgage\ amounts - round(mortgage\ amounts, thresholds). \quad (4)$$

We define an indicator variable *Above* for *Run* greater than zero, and our *Round number mortgage* dummy is one when *Run=0*. We include *Run*, *Above*, *loss*, *loss*Round mortgage*, *loss*Run*, and *loss*Above* in equations (1) and (2). The variable *Run* represents the running variable, and the interaction with *loss* allows the slope of the running variable to vary on either side of the round number. We also include similar interactions for the LTV focused dummy. As noted above, we use a bandwidth of $2,500 around each round number so that the stacked samples do not overlap. We also include fixed effects containing the round number and all the observations within the bandwidth of that number so that a unique fixed effect represents each stacked subsample of data.

These results are shown in Table 8. Panel A presents the baseline discontinuity results, Panel B adds balancing test controls and their interaction with seller type, notably for round number mortgage, and Panel C adds census tract by seller type fixed effects. Column (1) presents results for a model that only includes expected loss, and column (2) adds the controls for expected gain. In panel A, we find that the estimates on expected loss for the control group have fallen by more than half to between 0.15 and 0.18 with the inclusion of the mortgage amount bin fixed effects, but that the discontinuity estimate at 0.10 is basically the same as our baseline interaction estimate from the simple regression. As before, adding controls for all the balancing test variables and their interaction with seller type or adding census tract by seller type fixed effects has minimal impact on the discontinuity estimate with estimates ranging between 0.11 and 0.12. However, the addition of these controls further reduces the estimates for the control group to between 0.11 and 0.15, between 25 and 40 percent of the initial control group estimate in Table 4, and well within the much smaller range of estimated effects of 3-18 percent found by Genosove and Mayer (1997) in condominium study.[17] Focusing on the final panel of Table 8, the effect of expected loss on sales price for the round number sample is nearly double the effect for the control group.

As our discontinuity estimates are based on multiple thresholds, we follow Bertanha (2020) and run a discontinuity regression separately for each threshold (i.e., $5,000 mortgage window). Then we get an aggregate estimate by weighting the individual estimates and follow Brunner et al.'s (2019) application by bootstrapping the standard errors. This robustness test provides a more general approach to tests for effects at discontinuities because it allows the running variables to be different for every round number, consistent with a linear approximation of the running variable relationship within each bandwidth. There are 200 cutoffs in our sample, and we limit our sample to round number cut-offs with more than 50 observations within the bandwidth. The coefficient estimate of $Round_{ils}Loss_{ilst}$ following the model specification in Table 4, column (2) (column

[17] Genosove and Mayer (1997) obtain an insignificant 3 percent effect on housing sales price when including the house price residual from the original home purchase as a control for housing unit unobservables. They interpret those estimates as a lower bound and treat the 18 percent effect estimate that arises without the control for housing unit unobservables as an upper bound.

(4)) is 0.117 (0.100) with standard error of 0.004 (0.004), highly consistent with our baseline results.

3.3 Follow-up Analyses

Next, as flippers may be more likely to invest or renovate before reselling, improving the unobservables of the housing unit, one might be concerned that observed price appreciation may be likely due to such improvements rather than loss aversion in this subsample. Therefore, we examine whether our results are affected by the presence of flippers. We follow Bayer et al. (2020) and use the names of the buyer and seller to identify flippers as individuals engaged in buying and selling at least two different properties while holding them for less than two years. Bayer et al. (2020) also identify flippers using a second home method where the buyer is observed to hold the second property and the additional property is sold within two years. The method we used here is more conservative because the individual must conduct multiple flips in our sample period while the second home method does not require contemporaneous overlap in property holdings. We find a similar pattern of the percentage of flipper-involved transactions over our sample period (unreported) as in Bayer et al. (2020), although we observe an overall smaller proportion of flippers because we only focus on single-family housing and Connecticut is less subject to speculative activities compared to Los Angeles. In Table 9, we conclude that our results are robust to dropping sales where either the buyer, the seller, or a combination of buyer and seller were identified as flippers.

Further, Ben-David and Hirshleifer (2012) suggest that loss aversion is stronger for short holding periods when the purchase price is likely to be most salient as a reference point. We classify our sample into two sub-samples: months since purchase (year since purchase) greater or below median in Panels A and B (C and D) of Table 10. Similar to Ben-David and Hirshleifer (2012), we find that the expected loss effect for our round number mortgage sample relative to the control group is stronger for a short holding period with the magnitude of the effect doubling for our short holding period subsample. Similarly, we find larger effects of expected loss on the likelihood of sale for the short holding period subsample, again consistent with sellers leaving the property on the market longer to obtain a higher sales price. On the other hand, the cross-sectional effects of expected loss on sales price, as indicated by the estimates for the control group, do not vary with the holding period.

4. Conclusions

We provide evidence that loss aversion is stronger among buyers who exhibited focal point bias by reporting or selecting round numbers. First, we use data from an experiment to establish that individuals who rounded when responding to a survey question exhibited higher levels of loss aversion during the experiment. Second, we observe larger effects on expected loss in the housing market on sales price for sellers who selected a round mortgage amount during their initial purchase. Unlike earlier studies of focal point bias and housing sales prices, which rely on left digit effects arising in the previous purchase price, our study is able to isolate the effect of individual heterogeneity separate from the direct effects of the previous sales price left digit on the perceived value of the housing unit because we identify incidents of focal point bias from the purchase mortgage amount.

Further, our analysis of loss aversion in the housing market potentially provides much more robust evidence that loss aversion influences housing sales prices. All studies that have found a strong relationship between expected loss and sales prices were conducted with a very heterogeneous sample of real estate properties. We also use a heterogeneous sample of single-family homes, and similarly find a strong relationship between expected loss and sales price. However, we demonstrate that expected loss correlates strongly with housing, mortgage and location variables, and that the inclusion of controls erodes the estimated effect by between 50 and 75 percent. On the other hand, the differences in expected loss between the focal point biased sample and the control group are much more weakly related to observable controls, and the estimated differential effect of expected losses on sales price for focal point biased sellers is very stable as controls are added.

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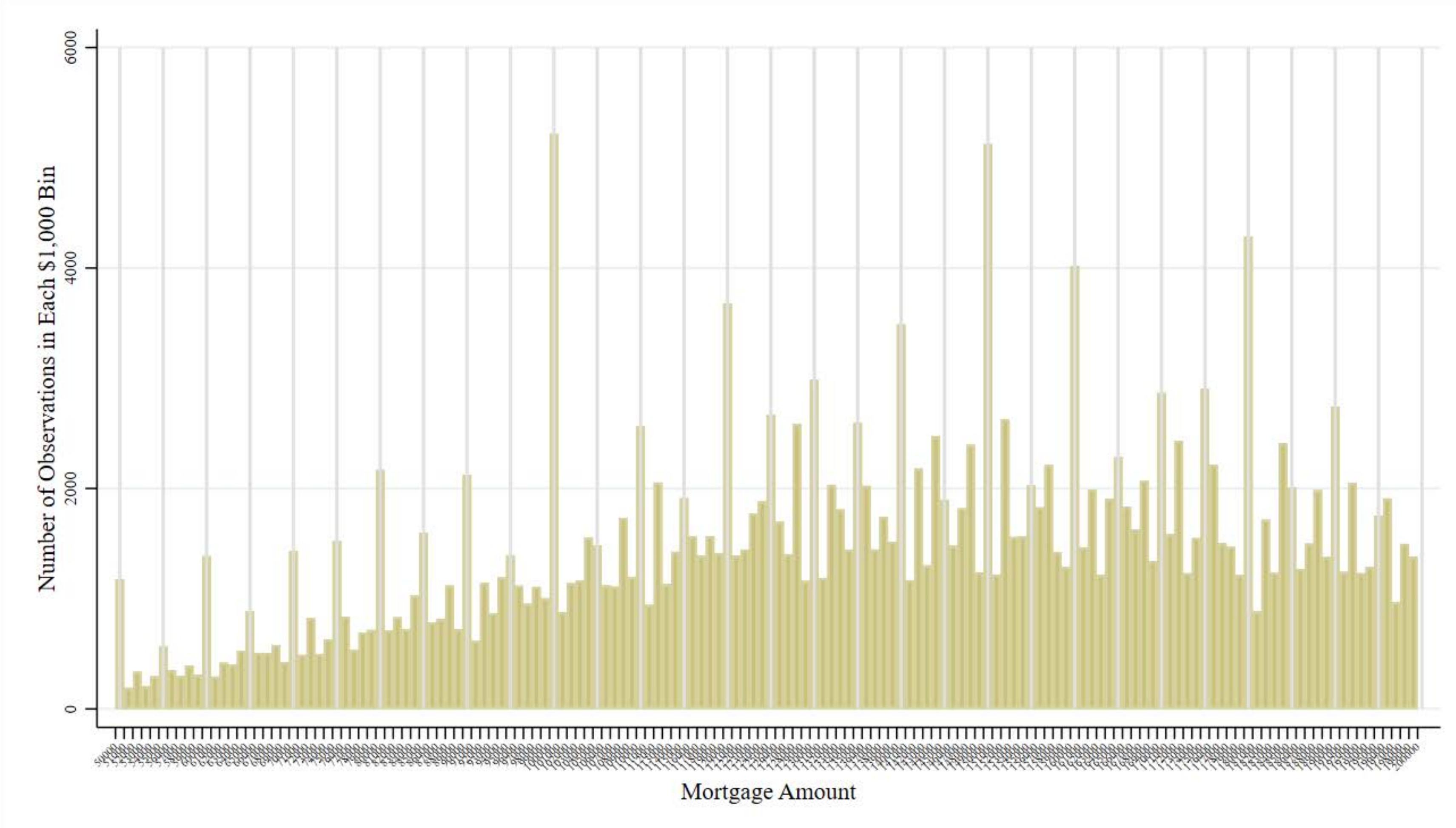
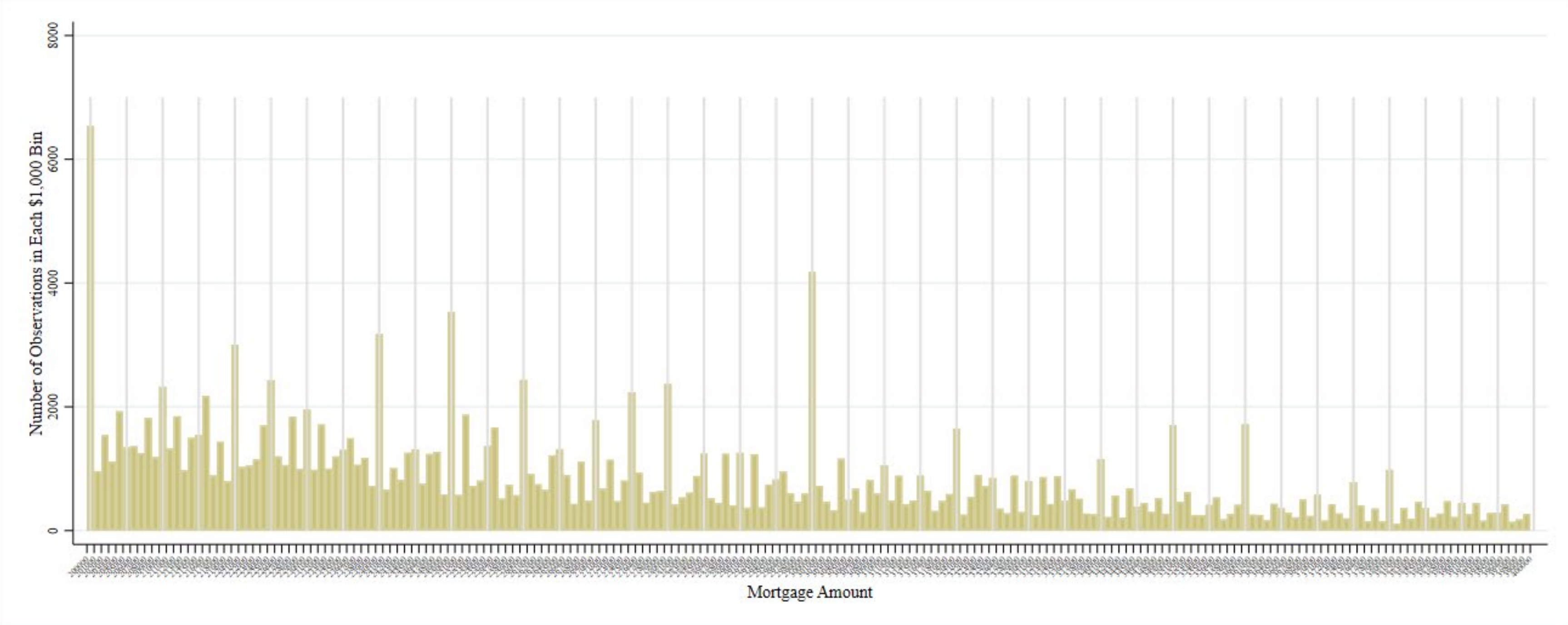
Figure 1: Mortgage Amount Histograms

Panel A: Mortgage Amount $50,000–$200,000

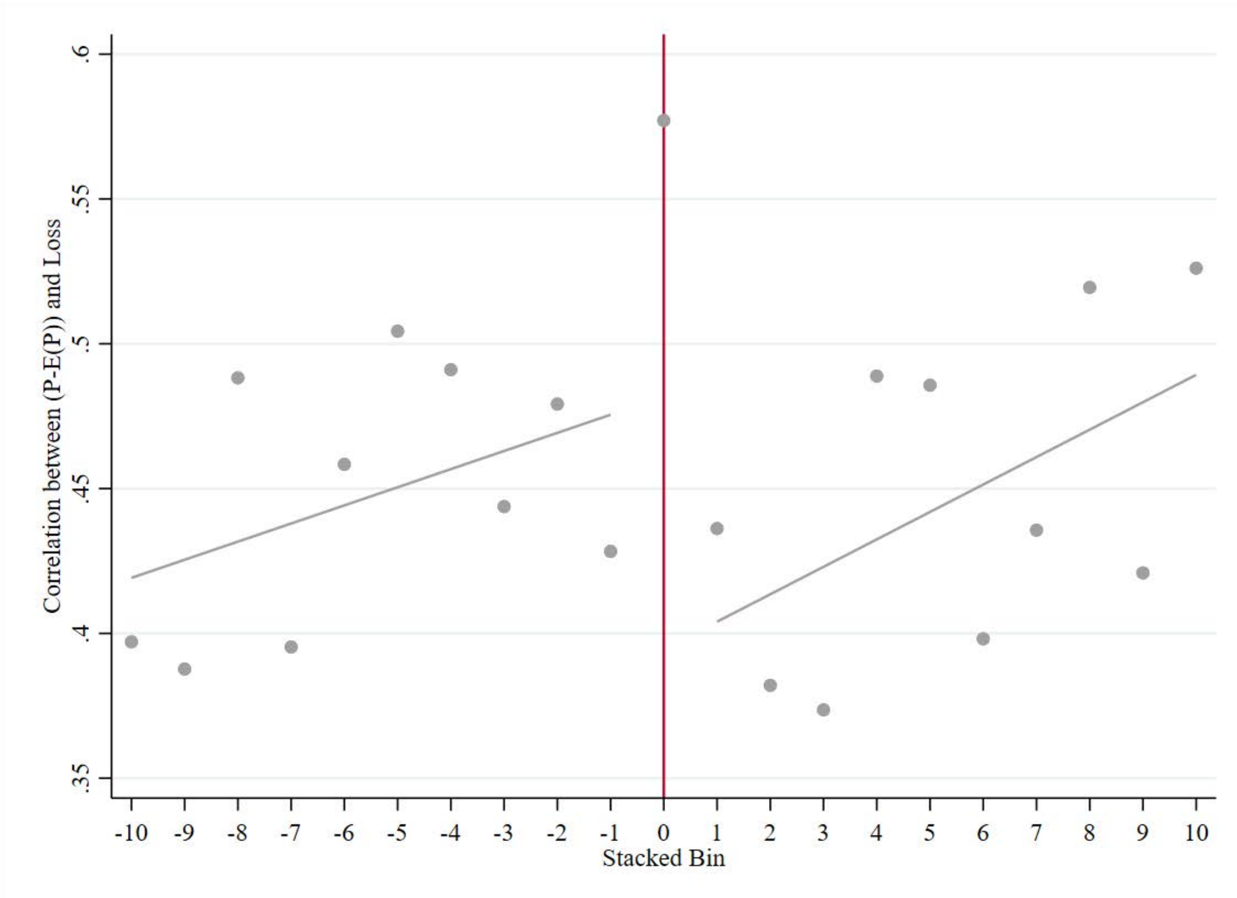


Panel B: Mortgage Amount $200,000–$400,000



Notes. This figure shows histograms for houses whose mortgage amounts were between $50,000 and $200,000 ($200,000 and $400,000) in Panel A (B). The histogram groups mortgages into $1,000 bins (rounded down). Gray bars indicate multiples of $5,000.

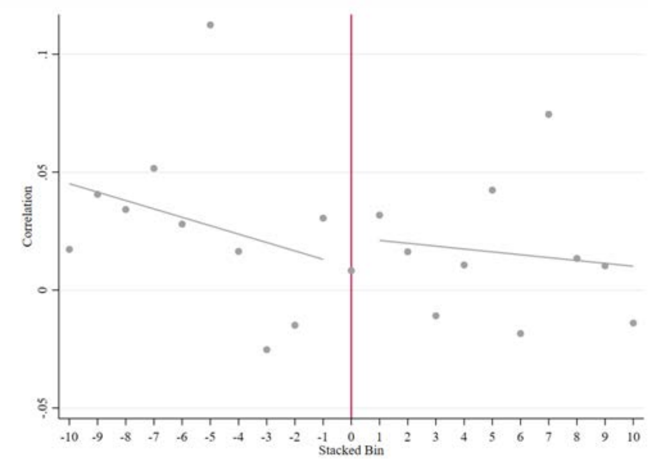
Figure 2: Discontinuities at Round Numbers



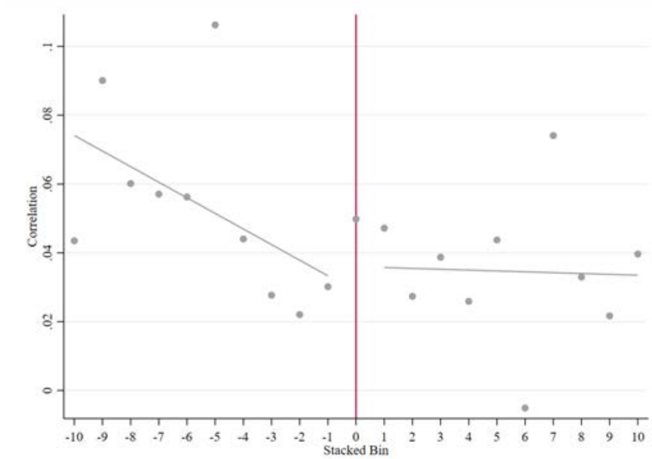
Notes. A stacked discontinuity sample is created with a $2,500 bandwidth on either side. Each $2,500 bin is further divided on either side into 10 bins, and has an 11^{th} bin for round numbers. The observations of credit-constrained borrowers are dropped. The gray dots are unconditional correlations between expected loss and the difference between sale price and expected sale price. The gray lines are fitted lines for the 10 points below 0 and separately for the 10 points above 0.

Figure 3: Covariates at Round Numbers

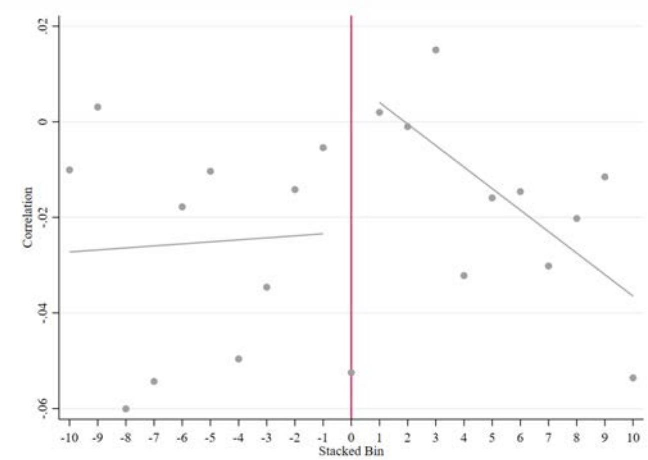
Panel A: House Attribute - Interior Size



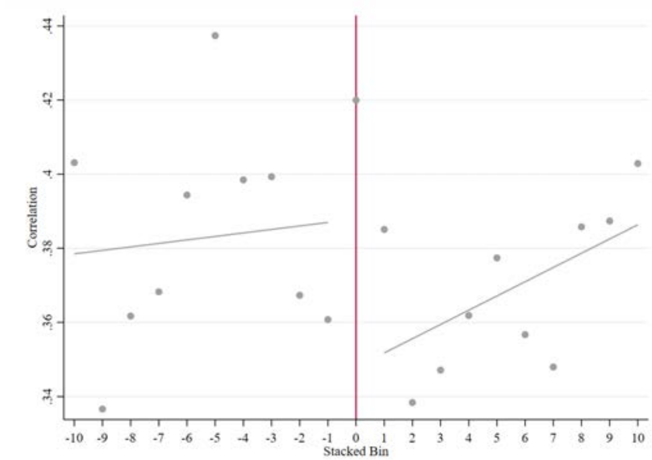
Panel B: House Attribute - Age



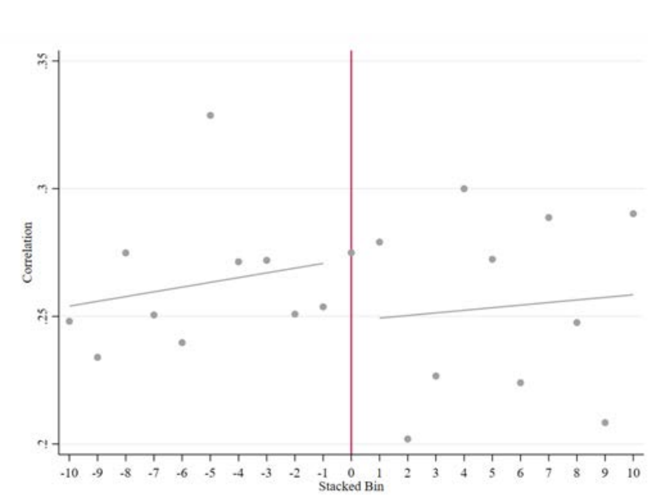
Panel C: Mortgage Attribute – LTV



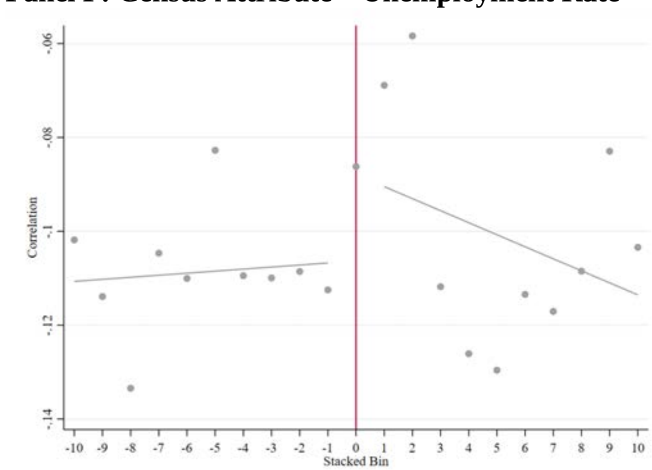
Panel D: Mortgage Attribute – Mortgage Amount



Panel E: Census Attribute - % College Education



Panel F: Census Attribute – Unemployment Rate



Notes. A stacked discontinuity sample is created with a \$2,500 bandwidth on either side. Each \$2,500 bin is further divided on either side into 10 bins, and has an 11^{th} bin for round numbers. The observations of credit-constrained borrowers are dropped. The gray dots are unconditional correlations between expected loss and house, mortgage, and census attributes. The gray lines are fitted lines for the 10 points below 0 and separately for the 10 points above 0.

Table 1: Loss Aversion and Self-Reported Round Numbers

	Loss Aversion (Continuous Measure)			Loss Aversion (Categorical Measure)		
Rounded Reporting	3.336**	3.433**	3.021*	0.564***	0.471**	0.427*
	(1.480)	(1.614)	(1.618)	(0.211)	(0.226)	(0.228)
Age		-0.092	-0.102		0.001	-0.000
		(0.159)	(0.158)		(0.022)	(0.022)
Gender (Male=1)		0.941	1.211		-0.226	-0.197
		(1.287)	(1.286)		(0.180)	(0.181)
Semester		-0.009	0.015		-0.011	-0.008
		(0.072)	(0.072)		(0.010)	(0.010)
Work Income (log)		-0.081	-0.093		0.025	0.024
		(0.234)	(0.233)		(0.033)	(0.033)
Number of Lunches Out per Week			-0.798*			-0.084
			(0.465)			(0.066)
Constant	3.242***	5.290	7.267*	1.590***	1.672***	1.881***
	(0.673)	(3.673)	(3.820)	(0.096)	(0.515)	(0.538)
R-squared	0.039	0.043	0.067	0.055	0.066	0.079
Observations	126	121	121	126	121	121

Notes. This table shows results from regressions of loss aversion on rounded reporting dummy and control variables using the experimental data in Karle et al. (2015). Loss aversion (continuous measure) in columns (1) through (3) is estimated from an experiment of choices between lotteries and sure payment. To mitigate the influence of outliers, Karle et al. (2015) categorize the continuous measure into four numeric categories ranging from “1 - loss-seeking or neutral” to “4 - strong loss-averse” that are used in columns (4) through (6).

Table 2: Summary Statistics

	Loss > 0		Loss < 0	
	Mean	Std Dev	Mean	Std Dev
	(1)	(2)	(3)	(4)
Log of Sale Price	12.77	0.81	12.43	0.67
Dummy Round Number Mortgage	0.21	0.41	0.15	0.36
Dummy LTV Focused	0.47	0.50	0.44	0.50
Loss	0.32	0.28	0.00	0.00
Gain	0.00	0.00	0.46	0.37
Market Price (log)	12.49	0.58	12.52	0.59
1st Residual	0.28	0.34	-0.22	0.41
Equity Position (LTV)	0.69	0.34	0.56	0.39
Equity Position (LTV Truncated)	0.07	0.14	0.05	0.21
Months since the Previous Sale	68.27	43.75	64.83	55.09
Hedonic Characteristics				
Interior Size (sf.)	1,922	1,005	1,863	990
Lot Size (sf.)	30,965	41,555	29,293	41,125
2-3 bathrooms	0.51	0.50	0.49	0.50
>3 bathrooms	0.10	0.30	0.09	0.29
Age	57.77	32.00	55.94	31.62
Mortgage Attributes				
Mortgage Amount (First Mortgage, log)	12.52	0.63	11.95	0.61
Combined Mortgage Amount (log)	12.52	0.63	11.96	0.61
Loan to Value Ratio	0.70	0.53	0.73	0.96
Combined Loan to Value Ratio	0.70	0.53	0.73	0.96
Presence of Second Mortgage	0.01	0.07	0.03	0.18
Census block characteristics (Census 1990)				
Percent Female	0.52	0.02	0.52	0.02
Percent white	0.96	0.07	0.92	0.15
Median Household income	77,744	32,108	65,353	26,143
Percent with college education	0.37	0.18	0.28	0.16
Percent households with kids	0.34	0.09	0.35	0.09
Average household size	2.76	4.35	2.75	2.60
Percent below poverty	0.02	0.03	0.04	0.07
Percent of owner-occupied housing with mortgage	0.69	0.13	0.68	0.15
Unemployment rate	0.04	0.03	0.05	0.04
Vacancy rate	0.06	0.07	0.05	0.05
Median value of owner-occupied housing	302,372	138,101	243,731	105,497
Percent of 65 and over	0.14	0.07	0.14	0.07

Notes. This table shows means and standard deviations for repeat-sale transactions with expected loss and expected gain.

Table 3: Balance Tests

	Loss		Loss*Round	
	Parameter	SE	Parameter	SE
	(1)	(2)	(3)	(4)
Hedonic Characteristics				
Interior Size	200.839*	(21.21)	-26.137	(19.648)
Lot Size	8332.194	(1460.822)	-2068.738	(1881.025)
Number of bathrooms	0.145***	(0.020)	0.019	(0.024)
Number of bedrooms	0.451**	(0.039)	-0.062	(0.028)
Age	0.557***	(0.028)	-0.043	(-0.58)
Mortgage Attributes				
Mortgage Amount (First Mortgage)	0.367***	(0.021)	0.010	(0.022)
Combined Mortgage Amount	0.371***	(0.021)	0.009	(0.022)
Loan to Value Ratio (First Mortgage)	-0.360***	(0.024)	0.131	(0.078)
Combined Loan to Value Ratio	-0.357***	(0.024)	0.130	(0.078)
Census block characteristics (Census 1990)				
Percent Female	0.003***	(0.001)	-0.001	(0.001)
Percent black	0.035**	(0.003)	-0.017***	(0.003)
Percent white	-0.057**	(0.005)	0.024***	(0.003)
Log Median Household income	0.039***	(0.015)	-0.015	(0.014)
Percent with college education	0.090***	(0.004)	-0.000	(0.006)
Average household size	-0.013***	(0.003)	-0.002	(0.004)
Percent below poverty	0.025***	(0.004)	-0.008***	(0.002)
Percent of owner-occupants w/ mortgage	-0.005	(0.004)	-0.003	(0.004)
Unemployment rate	0.007***	(0.001)	-0.002	(0.001)
Vacancy rate	0.044***	(0.004)	-0.014***	(0.003)
Median value of owner-occupied housing	0.103***	(0.017)	0.011	(0.018)
Percent of 65 and over	0.000	(0.002)	0.002	(0.002)

Notes. This table summarizes results from regressions of observable hedonic, mortgage, and census characteristics on loss, loss*round mortgage dummy, control variables, and a vector of labor-market-area-by-quarter fixed effects in equation (2). Models are estimated using OLS with errors clustered by labor market area-by-quarter.

Table 4: Baseline Results

	G&M	Round	w/ Gains	Round w/ Gains
	(1)	(2)	(3)	(4)
Loss	0.402***	0.388***	0.392***	0.398***
	(0.011)	(0.017)	(0.012)	(0.018)
Loss*Round Mortgage		0.100***		0.102***
		(0.020)		(0.020)
Loss*LTV Focused		0.071***		0.122***
		(0.021)		(0.022)
Gain			0.028**	0.006
			(0.012)	(0.013)
Gain*Round Mortgage				-0.035**
				(0.017)
Gain*LTV Focused				-0.172***
				(0.017)
Market Price	0.900***	0.901***	0.897***	0.901***
	(0.004)	(0.005)	(0.004)	(0.005)
Market Price*Round Mortgage		-0.010*		-0.006
		(0.005)		(0.005)
Market Price*LTV Focused		0.006		0.016***
		(0.005)		(0.006)
1st Residual	0.500***	0.423***	0.519***	0.423***
	(0.008)	(0.009)	(0.007)	(0.010)
1st Residual*Round Mortgage		0.039***		0.020
		(0.012)		(0.015)
1st Residual*LTV Focused		0.223***		0.104***
		(0.009)		(0.016)
Months	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Months*Round Mortgage		0.001***		0.001***
		(0.000)		(0.000)
Months*LTV Focused		-0.000		-0.000
		(0.000)		(0.000)
Equity Position	-0.008***	-0.024**	-0.008***	-0.024**
	(0.003)	(0.010)	(0.003)	(0.010)
Equity Position*Round Mortgage		0.036***		0.036***
		(0.011)		(0.011)
Equity Position*LTV Focused		-0.415***		-0.547***
		(0.021)		(0.029)
Round Mortgage	0.022***	0.085	0.023***	0.044
	(0.003)	(0.063)	(0.003)	(0.065)
LTV Focused	-0.089***	0.163**	-0.087***	0.181**
	(0.003)	(0.070)	(0.003)	(0.074)
Constant	1.369***	1.374***	1.400***	1.363***
	(0.054)	(0.060)	(0.050)	(0.061)
LMA-Year-Quarter FE	Yes	Yes	Yes	Yes
R-squared	0.810	0.819	0.810	0.820
Observations	139,674	139,674	139,674	139,674

Notes. This table summarizes results from regressions of sale price on loss, loss*round mortgage, and controls. Standard errors are clustered at the labor-market-area-by-quarter level. Column (1) follows a standard model used in Genesove and Mayer (2001). Column (2) adds interactions between loss and borrower types (round number mortgage and LTV focused). Column (3) adds expected gain to column (1). Column (4) adds gain interactions to column (2).

Table 5: Additional Results

	G&M	Round	w/ Gains	Round w/ Gains
	(1)	(2)	(3)	(4)
A. Baseline				
Loss	0.402***	0.388***	0.392***	0.398***
	(0.011)	(0.017)	(0.012)	(0.018)
Loss*Round Mortgage		0.100***		0.102***
		(0.020)		(0.020)
B. Alternative Equity Position				
Loss	0.401***	0.403***	0.391***	0.408***
	(0.011)	(0.017)	(0.012)	(0.018)
Loss*Round Mortgage		0.102***		0.103***
		(0.020)		(0.020)
C. Probability of Second Sale				
Loss	-0.005	-0.005*	-0.015**	-0.017**
	(0.003)	(0.003)	(0.006)	(0.007)
Loss*Round Mortgage		-0.010***		-0.007***
		(0.002)		(0.002)

Notes. Panels A and B (Panel C) show results from regressions of sale price (probability of the second sale) on loss, loss*round mortgage, and control variables in Table 4. Standard errors are clustered at the labor-market-area-by-quarter level. Panel A reproduces the baseline results from Table 4. Panel B replaces the equity position variable from Panel A with an alternative equity position measure, defined as current LTV truncated at above 0.8. Panel C estimate equation (3) using a linear probability model for sale spells which start from the year after the sale of a property and end in the year of next sale or the end of our sample period to account for censoring.

Table 6: Adding Balance Control Interactions

	G&M	Round	w/ Gains	Round w/ Gains
	(1)	(2)	(3)	(4)
A. Baseline				
Loss	0.402***	0.388***	0.392***	0.398***
	(0.011)	(0.017)	(0.012)	(0.018)
Loss*Round Mortgage		0.100***		0.102***
		(0.020)		(0.020)
B. Baseline + balance control interactions				
Loss	0.281***	0.221***	0.222***	0.204***
	(0.010)	(0.019)	(0.012)	(0.020)
Loss*Round Mortgage		0.100***		0.108***
		(0.022)		(0.022)
C. Probability of Sale				
Loss	-0.005	-0.005*	-0.015**	-0.017**
	(0.003)	(0.003)	(0.006)	(0.007)
Loss*Round Mortgage		-0.010***		-0.007***
		(0.002)		(0.002)
D. Probability of Sale + balance control interactions				
Loss	-0.003	0.003	-0.021**	-0.015
	(0.006)	(0.004)	(0.009)	(0.009)
Loss*Round Mortgage		-0.033***		-0.030***
		(0.005)		(0.006)

Notes. Panel A and B (Panels C and D) show results from regressions of sale price (probability of the second sale) on loss, loss*round mortgage and control variables in Table 4. Standard errors are clustered at the labor-market-area-by-quarter level. Panel A (Panel C) reproduces the baseline results from Table 4 (Table 5 Panel C). Panels B and D adds all the balance controls for the hedonic, mortgage, and census characteristics (see Table 3) and further interact the round mortgage dummy, as well as the LTV focused/subordinate debt dummy, with these variables.

Table 7: Adding Tract FEs

	G&M	Round	w/ Gains	Round w/ Gains
	(1)	(2)	(3)	(4)
A. Baseline				
Loss	0.402***	0.388***	0.392***	0.398***
	(0.011)	(0.017)	(0.012)	(0.018)
Loss*Round Mortgage		0.100***		0.102***
		(0.020)		(0.020)
B. Baseline + tract by type FEs				
Loss	0.275***	0.279***	0.265***	0.277***
	(0.009)	(0.012)	(0.009)	(0.013)
Loss*Round Mortgage		0.061***		0.066***
		(0.016)		(0.016)
C. Probability of Sale				
Loss	-0.005	-0.005*	-0.015**	-0.017**
	(0.003)	(0.003)	(0.006)	(0.007)
Loss*Round Mortgage		-0.010***		-0.007***
		(0.002)		(0.002)
D. Probability of Sale + tract by type FEs				
Loss	-0.007*	-0.007*	-0.018**	-0.020***
	(0.004)	(0.004)	(0.007)	(0.007)
Loss*Round Mortgage		-0.012***		-0.009***
		(0.002)		(0.002)

Panels A and B (Panels C and D) show results from regressions of sale price (probability of the second sale) on loss, loss*round mortgage and control variables in Table 4. Standard errors are clustered at the labor-market-area-by-quarter level. Panel A (Panel C) reproduces the baseline results from Table 4 (Table 5 Panel C). Panels B and D adds tract-by-borrower-type fixed effects.

Table 8: Discontinuity Analysis

	Round	Round w/ Gains
	(1)	(2)
A. Baseline		
Loss	0.176***	0.152***
	(0.035)	(0.036)
Loss*Round Mortgage	0.103***	0.095***
	(0.034)	(0.036)
Loss*Run	-0.008	-0.007
	(0.025)	(0.025)
Loss*Run*Above	0.007	0.010
	(0.044)	(0.045)
B. Baseline + balance control interactions		
Loss	0.131***	0.149***
	(0.032)	(0.034)
Loss*Round Mortgage	0.117***	0.113***
	(0.035)	(0.035)
Loss*Run	-0.019	-0.019
	(0.023)	(0.023)
Loss*Run*Above	0.029	0.032
	(0.039)	(0.039)
C. Baseline + tract by type FEs		
Loss	0.119***	0.109***
	(0.025)	(0.026)
Loss*Round Mortgage	0.102***	0.089***
	(0.026)	(0.027)
Loss*Run	-0.022	-0.021
	(0.019)	(0.019)
Loss*Run*Above	0.036	0.038
	(0.032)	(0.032)

This table summarizes results from regressions of sale price on loss, loss*round mortgage, loss*Run, loss*Run*Above and control variables in Table 4. Run is normalized mortgage amounts around each round number. Above is an indicator variable for Run greater than zero. Standard errors are clustered at the labor-market-area-by-quarter level. Panel A shows the baseline results. Panel B adds all the balance controls for the hedonic, mortgage, and census characteristics (see Table 3) and further interacts the round mortgage dummy, as well as the LTV focused/subordinate debt dummy, with these variables. Panel C adds tract-by-borrower-type fixed effects.

Table 9: Deleting Flippers

	G&M	Round	w/ Gains	Round w/ Gains
	(1)	(2)	(3)	(4)
A. Baseline (no flipper as buyer)				
Loss	0.403***	0.389***	0.394***	0.400***
	(0.011)	(0.017)	(0.012)	(0.018)
Loss*Round Mortgage		0.099***		0.101***
		(0.020)		(0.020)
B. Baseline (no flipper as seller)				
Loss	0.409***	0.400***	0.404***	0.414***
	(0.011)	(0.017)	(0.012)	(0.017)
Loss*Round Mortgage		0.090***		0.091***
		(0.020)		(0.020)
C. Baseline (no flipper as buyer or seller)				
Loss	0.410***	0.402***	0.405***	0.416***
	(0.011)	(0.016)	(0.012)	(0.017)
Loss*Round Mortgage		0.089***		0.090***
		(0.020)		(0.020)
D. Probability of Sale (no flipper as buyer)				
Loss	-0.005	-0.005*	-0.016**	-0.017***
	(0.003)	(0.003)	(0.006)	(0.007)
Loss*Round Mortgage		-0.010***		-0.008***
		(0.002)		(0.002)
E. Probability of Sale (no flipper as seller)				
Loss	-0.006**	-0.007***	-0.015**	-0.018***
	(0.003)	(0.003)	(0.006)	(0.007)
Loss*Round Mortgage		-0.008***		-0.006***
		(0.002)		(0.002)
F. Probability of Sale (no flipper as buyer or seller)				
Loss	-0.006**	-0.007***	-0.016**	-0.018***
	(0.003)	(0.003)	(0.006)	(0.007)
Loss*Round Mortgage		-0.009***		-0.006***
		(0.002)		(0.002)

This table summarizes results deleting flippers. Panels A, B and C (Panels C, D, and F) show results from regressions of sale price (probability of the second sale) on loss, loss*round mortgage and control variables in Table 4. Standard errors are clustered at the labor-market-area-by-quarter level. Panels A and D show results deleting flippers as the buyer in the first sale. Panels B and E show results deleting flippers as the seller in the second sale. Panels C and F show results deleting flippers as either the buyer or the seller.

Table 10: Holding period

	G&M	Round	w/ Gains	Round w/ Gains
	(1)	(2)	(3)	(4)
A. Baseline (below median)				
Loss	0.465***	0.399***	0.501***	0.450***
	(0.017)	(0.028)	(0.019)	(0.030)
Loss*Round Mortgage		0.210***		0.217***
		(0.035)		(0.037)
B. Baseline (above median)				
Loss	0.473***	0.453***	0.542***	0.535***
	(0.014)	(0.019)	(0.014)	(0.019)
Loss*Round Mortgage		0.021		-0.004
		(0.024)		(0.023)
C. Probability of Sale (below median)				
Loss	0.003	0.006	-0.049***	-0.047***
	(0.005)	(0.004)	(0.012)	(0.013)
Loss*Round Mortgage		-0.015***		-0.007*
		(0.004)		(0.004)
D. Probability of Sale (above median)				
Loss	-0.023***	-0.025***	-0.079***	-0.084***
	(0.004)	(0.004)	(0.019)	(0.019)
Loss*Round Mortgage		-0.006**		-0.004
		(0.003)		(0.003)

This table summarizes results deleting flippers. Panels A, B and C (Panels C, D, and F) show results from regressions of sale price (probability of the second sale) on loss, loss*round mortgage and control variables in Table 4. Standard errors are clustered at the labor-market-area-by-quarter level. Panels A (Panel C) shows results based on observations with months since purchase (year since purchase) below median. Panels B (Panel D) shows results based on observations with months since purchase (year since purchase) above med

Appendix 1: Variable Definitions

Variable	Definition
Sale price	Log of sale price of the second sale.
Sale probability	An indicator variable if house *i* was sold in year *t*.
Round mortgage	An indicator variable if 1st mortgage amount at purchase with 0 or 5 on 000' and LTV focused dummy equals zero.
LTV focused	An indicator variable if the LTV ratio at purchase equals one of the critical ratios, e.g., 0.80, 0.90, 0.95, 0.97, and 1.00, that suggest that the buyer targeted an important LTV ratio in the market) or took out a second mortgage at the time of purchase (subordinate debt). Appendix 2 summarizes details on identifying critical LTV ratios.
Loss	Difference between the first sale price and the expected price truncated above at zero. It is measured at the time of the second sale (year t within a sale spell) in repeat sale analysis (panel data analysis) for sale price (sale probability) as the outcome variable.
Gain	Difference between the first sale price and the expected price truncated below at zero. It is measured at the time of the second sale (year t within a sale spell) in repeat sale analysis (panel data analysis) for sale price (sale probability) as the outcome variable.
Equity Position	Equity position of the loan assuming a 30-year mortgage amortized using the 30-year mortgage interest rate at purchase. It is measured at the time of the second sale (year t within a sale spell) in repeat sale analysis (panel data analysis) for sale price (sale probability) as the outcome variable. An alternative equity position is measured as an equity position truncated at above 0.8.
Expected price	Predicted value estimated by the hedonic model
First residual	The residual from the hedonic regression for the first sale
Month	Number of months between the first and second sale used in repeat sale analysis for sale price as the outcome variable
Years since last sale	Number of years since purchase used in panel data analysis for sale probability as the outcome variable
Housing Characteristic	
Interior size	Interior size (sq. ft.) of the house
Lot size	Lot size (sq. ft.) of the house
2-3 bathrooms	An indicator variable if 2-3 bathrooms
> 3 bathrooms	An indicator variable if > 3 bathrooms
Age	Age of the house
Mortgage attributes	
Mortgage amount	Log of 1^{st} mortgage amount (taken from the first sale)
Combined mortgage amount	Log of combined mortgage amount (taken from the first sale)
LTV ratio	Loan-to-value ratio (taken from the first sale)
CLTV ratio	Combined loan-to-value ratio (taken from the first sale)
Presence of second mortgage	An indicator variable if there is a second mortgage in the first sale
Census block characteristics (Census 1990)	
Percent female	Percent of female population
Percent white	Percent of white population
Median income	Median Household Income (in 2000 Dollars)
Percent with college education	Percent of population with college degree
Percent of households with kids	Percent of married-couple families
Average household size	Average household size
Percent below poverty	Percent of households below poverty level
Percent of owner-occupied housing with mortgage	Percent of owner-occupied houses with mortgage
Unemployment rate	Unemployment rate
Vacancy rate	Percentage of vacant housing units
Median value of owner-occupied housing	Median value of owner-occupied housing (in 2000 Dollars)
Percent of 65 and over	Percent of age 65 and over

Appendix 2: Identifying Critical LTV Thresholds

Given the complexities of the mortgage market, we use a data-driven approach to establishing LTV ratios associated with borrowers attempting to hit critical thresholds within the mortgage market. We start with the standard critical LTVs, including 0.8, 0.9, 0.95, 0.97, 1.00. As one never gets exactly 0.8. The exact LTV is something like 0.800001. We follow Pope et al. (2015), round down LTVs into 3-digit bin, and define the critical LTV thresholds using these bins. For example, 0.7912 will be round to 0.791.

In addition to the standard LTVs, we run histograms of the number of loans at different LTV percentage points (e.g., 0.80<=LTV<0.81) to check actual spikes. For example, we observe huge spikes at 0.95 due to conforming loan limit with PMI and at 0.97 due to the FHA limit. Specifically, we perform checks for: (A) every 0.001 from 0.780 to 0.820, from 0.880 to 0.920 and 0.930 to 0.960 for the entire sample; (B) every 0.001 from 0.960 to 1.010 by splitting the sample into three parts: (1) start to Q32008, (2) Q42008 to Q42014, and (3) Q12015 to the end of the sample.

After checking the spikes in the histogram (unreported), we identify the following critical LTVs:

- 0.799, 0.800, 0.899, 0.900, 0.949, 0.950 for the entire sample,
- 0.969, 0.970, 0.983, 0.984, 0.991, 0.992, 0.999, 1.000 before 2009,
- 0.974, 0.981, 0.986, 1.000 from 2009 to 2014, and
- 0.970, 0.981, 1.000 from 2015 to the end of sample.

Although these spikes vary over the sample period and some do not fall right at integers, these critical LTVs can be justified. For example, Fannie Mae had a smaller Flex 97 program launched after 2008. The fact that post-2008 FHFA increased their loan requirements from 3 to 3.5 percent explains the mortgage spike at 0.974. The 0.986 might be some additional mortgages that were made at 0.97 – there were some exceptions to the 0.965. We justify spikes at 0.981 and 0.984 (just over 0.98) as the borrowers could roll the upfront mortgage insurance premium into the mortgage amount. Spikes at 0.991 are because prior to 2008 there were quite a few non-governmental mortgages right at 0.99.

We defined constrained borrower based on LTV thresholds, instead of CLTV thresholds. This is because having a second mortgage usually involves a credit constraint, we lump people who have a second mortgage together with people who hit a specific LTV threshold. Nevertheless, our results do not change if we use CLTV because there is only a small fraction of borrowers with a second mortgage.