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Home is Where the Bias Is

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Home Is Where the Bias Is

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An abstract of
A dissertation submitted to the Faculty of the
James T. Laney School of Graduate Studies of Emory University
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Doctor of Philosophy
in Business
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Abstract

Home Is Where the Bias Is
By Stephen Brincks

My dissertation consists of two essays, ("Home is Where the Bias Is: The Disposition Effect in the Housing Market and its Impact on the Financial Crisis," and "How Does Political Uncertainty Impact Bank Lending? Evidence from Close US Gubernatorial Elections"). In my first essay, I study the disposition effect (i.e., the tendency for individuals to be reluctant to realize their losses) in the housing market. I find strong evidence for the disposition effect in the housing market. Homeowners are 25 times more likely to sell homes for zero-profit (break-even) than at any other price point, leave money on the table when selling homes with gains, and avoid selling homes with unrealized losses. I find a strong inverse relationship between the percentage of homes with unrealized losses and future housing sales and price appreciation at the zip code level. During the recent housing boom and bust, zip codes with the strongest disposition effect peaked a year later than zip codes with the weakest disposition effect, had a 12% smaller price run-up, and a 10% smaller crash. The evidence is consistent with the hypothesis that the disposition effect mitigated the financial crisis by reducing the housing price run-up and subsequent crash. In my second essay, I examine how political uncertainty impacts US bank lending. I find that banks reduce new lending by about 60% in the two quarters prior to close gubernatorial elections. Bank stock volatility spikes by 11% and bank stock returns are 5.7% lower in the quarter before close elections. Lending only partially recovers after close elections, suggesting that political risk persists after elections are over. My results hold for subsamples of contiguous border counties and MSAs (including matched samples) and are stronger for small banks, highly leveraged banks, and geographically concentrated banks. Using a variety of tests, I show that my lending supply-side channel is distinct from a corporate demand-side channel. Overall, the evidence suggests that political risk has a negative impact on bank lending.

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Essay 1: Home Is Where the Bias Is: The Disposition Effect in the Housing Market and its

Impact on the Financial Crisis

1   Hypothesis and Introduction

Do individuals behave rationally or are they influenced by behavioral biases? If individuals are biased, what impact do their biases have on markets? One prominent way researchers have attempted to answer these questions is by studying the disposition effect (i.e., the tendency for individuals to be reluctant to realize their losses) and its influence on markets. Despite a large literature on the disposition effect, there is still no consensus on whether individuals are reluctant to realize losses.[1] Although many researchers have examined the disposition effect in the stock market, little research has examined the disposition effect in the housing market. I help fill this gap in the literature by examining how the disposition effect impacts the US housing market.

The housing market is likely to be a better setting than the stock market for testing whether individuals are reluctant to realize losses for several reasons. First, individuals are more likely to remember a home's purchase price than a stock's purchase price because home purchases are rare, time-consuming transactions that involve significantly more money than the average stock transaction. Second, a larger proportion of the US population owns a home (63.7%) than directly owns stocks (13.9%), especially less sophisticated individuals that are more likely to be biased.[2] Lastly, individuals are more likely to have an emotional attachment to their homes that may make them more reluctant to realize losses.

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[1] While some papers have found support for investor reluctance to realize losses (Shefrin and Staman (1985); Odean (1998)), others have found evidence against it (Ben-David and Hirshleifer (2012)) or find the evidence inconclusive (Chordia, Goyal, and Jegadeesh (2016)).
[2] See Dhar and Zhar (2006). Data comes from the 2016 Fed Survey of Consumer Finances.

Understanding the impact of the disposition effect on the housing market is important for at least four reasons. First, the housing market is the largest asset market in the United States, with a net worth of $31.8 trillion, slightly exceeding the nearly $30 trillion valuation of the US stock market.[3] Second, housing wealth constitutes a greater portion of total household net wealth than stock wealth ($185,000 vs $25,000).[4] Third, the housing market is essential for the real economy: the marginal propensity to consume from housing wealth is greater than stock market wealth (Mian, Rao, and Sufi (2013)). Lastly, as the recent financial crisis has shown, housing booms and busts have a significant impact on banks and the economy. For all these reasons, understanding how the disposition effect impacts the housing market is an important economic question.

Identifying whether individuals are reluctant to realize losses is challenging. Evidence must be inferred from observing homeowner selling decisions. However, selling decisions are likely to be determined by many variables, including unobserved variables such as the need to relocate for job-related reasons. If unobserved variables affect the decision to sell and vary with the magnitude of the gain or loss, then empirical tests of the disposition effect may be confounded.

My identification strategy relies on using a discontinuity at the *zero-profit point* (i.e., the nominal purchase price and selling price are the same) implied by theoretical models of prospect theory and realization utility. Prospect theory postulates that the shape of the utility function changes from a convex function for losses to a concave function for gains at the zero-profit point. Similarly, realization utility implies that a discontinuity in utility occurs at the zero-profit point because realizing losses, even very small losses, causes a sharp drop in utility. By using a

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[3] As of 2017. See Zillow for aggregate housing market value; Wilshire 5000 is used for equity market value.
[4] Data comes from the 2016 Fed Survey of Consumer Finances.

regression discontinuity design (RDD) to compare differences in selling behavior for very small gains to very small losses, I can account for unobserved variables.[5] The identifying assumption is that unobservable variables such as the need to relocate for a job are unlikely to discontinuously vary for small gains compared to small losses.

I find strong evidence of a reluctance to realize losses in the housing market. I show that homeowners are significantly more likely to sell houses for small gains than small losses, with a kink point occurring at the zero-profit point. The histogram of realized nominal profit or loss shows an enormous discontinuity at the zero-profit point: the probability that a homeowner sells their house for a zero profit is 25 times greater than the probability of realizing either a small gain or loss. Using web-scraped multiple listing service (MLS) data that covers a house's entire listing history, I show that the desire to break-even is the reason for the clustering of transactions at the zero-profit point. Houses listed at a break-even price (zero-profit point) sit on the market for 100 additional days before being sold, are 50% more likely to be dropped from the market, and their prices are reduced by 4-5% more than non-break-even houses. Homeowner reluctance to realize a loss, even a very tiny loss, strongly impacts selling decisions.

Next, I examine whether predictions made by several prominent behavioral explanations for the disposition effect are consistent with my findings. Realization utility (Barberis and Xiong (2012)), prospect theory (Kahneman and Tversky (1979)), and combinations of both (Ingersoll and Jin (2012); Henderson (2012)) have been used to explain the disposition effect. Realization utility predicts that an individual's probability of selling an asset increases with the magnitude of the profit, along with a jump in the probability of selling an asset for a small gain instead of a

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[5] Small gains are defined as houses sold for a 0-5% price gain, while small losses are defined as a 0-5% price loss. Different breakpoints are used for robustness. As the magnitude of the gain or loss moves further away from the zero point, identification weakens because other determinants of selling behavior may vary with the magnitude of the gain or loss.

small loss. My findings are consistent with these predictions. I find strong evidence of a jump in the probability of realizing a small gain or zero profit compared to a small loss, and I find that a homeowner's probability of selling increases with the magnitude of the gain. Having found support for realization utility, I next examine predictions made by prospect theory.

Prospect theory postulates that an individual's utility function changes from a concave function for gains to a convex function for losses at the purchase price (i.e., the reference point). If homeowners have prospect theory preferences, then their mark-up behavior (i.e., whether homeowners list their houses for a price greater than or less than fair market value) around the zero-profit point should differ for houses with gains versus losses. This is what I find. Homeowners mark up the prices of houses with losses to try to reduce the magnitude of the loss. Every negative 10% difference between a house's fair market value (FMV) and its purchase price is associated with a 3.4% higher selling price. [6] In contrast, homeowners leave money on the table when selling houses with gains. Every positive 10% difference between a house's FMV and its purchase price is associated with a 0.5-0.7% lower selling price. Houses with unrealized losses take longer to sell, while houses with small gains sell the quickest, suggesting that homeowners want to lock in small, certain gains to avoid the chance of realizing a loss. Since I find support for both realization utility and prospect theory, my results are most consistent with models that use *both* (Ingersoll and Jin (2012); Henderson (2012)) to explain the disposition effect.

Next, I examine whether the disposition effect impacts zip code level housing sales and price appreciation. For zip code level tests, I examine the relationship between the percentage of houses in a zip code with unrealized losses and the zip code's next quarter or year housing sales

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[6] I use county house tax assessments as my primary measure of a house's FMV. The results are robust to using hedonic pricing models, Zillow estimates, or only counties with highly accurate tax assessments.

and price change. As an additional identification strategy, I construct a variable, *kink ratio* (i.e., the ratio of the number of small gains and zero-profit point transactions to the number of small losses at the zip code), that measures the strength of the disposition effect around the zero-profit point. In zip codes without a disposition effect, the kink ratio should be close to 1. I then examine differences between zip codes with the strongest and weakest disposition effect during the recent housing boom and bust.

I find that a two standard deviation increase in the percentage of houses below purchase price is associated with a 34% reduction in the total number of houses sold in the next quarter and a 36% reduction in price appreciation. These effects persist over the next year. After sorting zip codes into quintiles based on the strength of their disposition effect around the zero-profit point (kink ratio), I rerun the analysis. There is no relationship between the percentage of houses with unrealized losses and future housing sales or price appreciation for zip codes with the weakest disposition effect around the zero profit point. This finding further supports the hypothesis that behavioral biases cause the disposition effect.

Finally, I investigate whether or not the disposition effect may have impacted the recent financial crisis. Grinblatt and Han (2005) show that the disposition effect can slow price adjustment by causing individuals to sell assets with gains too soon and to hold assets with losses too long. In the context of the housing market, the disposition effect should have reduced the price run-up during the boom and lessened the price drop during the bust. This is what I find. Zip codes with the strongest disposition effect take nearly one year longer to peak, have a 12% smaller aggregate price run-up, and have a 10% smaller crash than zip codes with the weakest disposition effect. Typically, individual biases are perceived to have a negative impact on

markets, but my results imply that the disposition effect created a positive spillover effect for the housing market by partially mitigating the recent housing crash.

Although the findings are strong and highly significant, unobserved variables may bias zip code level tests if my proxy for the strength of the disposition effect, kink ratio, is correlated with unobserved determinants of housing prices. I use several strategies to mitigate this concern. First, I use zip code and year-quarter fixed effects to control for time-invariant characteristics. Next, I match zip codes with the strongest and weakest disposition effect on income and education. Coefficient estimates from matching are very similar to the previous results. Since education and income are likely to be correlated with potential omitted variables, these tests lessen the concern that omitted variable bias is a problem. Lastly, I match zip codes with the strongest and weakest disposition effect on price run-up (or time of run-up) during the housing boom and examine how prices and sales evolve during the bust.[7] I find that zip codes with the strongest disposition effect have smaller price drops and slower recoveries than matched zip codes. Although I can not make a causal claim that the disposition effect mitigated the financial crisis, the combination of all of my tests provide evidence consistent with this hypothesis.

This paper contributes to the vast literature on the disposition effect. I find strong evidence that homeowners are reluctant to realize losses, even very tiny losses. My findings are most consistent with models by Ingersoll and Jin (2012) and Henderson (2012) that use prospect theory and realization utility to explain the disposition effect. In the stock market, Ben-David and Hirshleifer (2012) fail to find evidence for realization utility and conclude that investors are not reluctant to realize losses.[8]

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[7] Griffin and Maturana (2016) use the same methodology to control for omitted variables in regressions that examine whether dubious origination practices contributed to the housing bust. By matching zip codes on price/time run-up during the boom, I reduce the likelihood that unobservable variables may bias my findings during the bust.

[8] There are several potential explanations for why I find strong evidence for realization utility in the housing market while others find inconclusive evidence in the stock market, including the saliency of house purchase prices (Barberis and Xiong (2012)).

The closest paper to mine is Genesove and Mayer (2001)'s paper that finds evidence of loss aversion (i.e., condominium owners subject to losses attain higher selling prices) in a sample of 5,000 Boston condominiums in the 1990s. My paper differs from theirs in the following ways: 1) I find a clustering of transactions at the zero-profit point, which is strong evidence for a reluctance to realize losses, 2) I test for realization utility by looking at the probability of selling as a function of gain or loss, 3) I quantify the strength of the disposition effect across zip codes and find that the disposition effect is associated with fewer sales and lower price appreciation, and 4) I find evidence consistent with the hypothesis that the disposition effect partly mitigated the financial crisis.

My paper also contributes to the household finance literature (Campbell (2006)) that examines the consequences of suboptimal individual decisions.[9] I find that homeowners leave an average of $770-$1,080 on the table if their home is above its purchase price. One important implication of homeowner reluctance to sell houses with losses is reduced labor market mobility.[10]

This paper also has implications for the literature that examines the recent housing crisis (e.g., Mian and Sufi (2009); Chinco and Mayer (2015); Adelino et al. (2016); Burnside et al. (2016)). The evidence is consistent with the hypothesis that the disposition effect partially mitigated the housing run-up and bust. My results imply that in the absence of the disposition effect (which is stronger for lower income and less well-educated zip codes), the negative consequences of credit expansion on subprime borrowers would have been worse. One broader implication of my findings is that the disposition effect may have reduced the negative

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[9] Households take out excessively expensive loans (Agarwal et al. (2009)), fail to maximize employer-matched 401(k) contributions (Choi et al. (2011)), and fail to refinance early or wait too long (Agarwal et al. (2013); Andersen et al. (2015)).
[10] See Chan (2001), Engelhardt (2003), Brown and Matsa (2017), and Gopalan et al. (2018).

externalities associated with foreclosures, such as price declines (Campbell, Giglio, and Pathak (2011)) and reduced labor market productivity (Bernstein, McQuade, and Townsend (2016)).

2   Data and Variable Construction

The primary data sources for my paper are online county tax assessor websites that contain information about house characteristics, sales, and yearly tax assessments. House listing data (MLS) is collected from several online websites and purchased from various data brokers. Demographic controls at the zip code level come from the 2011 American Community Survey. I have also downloaded monthly zip code sale and price information from Zillow. In the first part of the paper, houses are the primary level of analysis. Zip codes are the primary level of analysis in the second half.

County tax websites typically contain information about house characteristics, past sales, and yearly assessed tax value. For nearly all of the houses in my sample, basic information such as the number of bedrooms and baths, square footage, and lot size is available. For most of the houses in the sample, additional property information is available, including the year that the house was built, whether the house has a basement or garage, and whether any major house improvements have been made. Table 1 shows basic summary statistics for all houses in my sample. The median house in my sample is a 1656 square foot house built in 1979 with 3 bedrooms and 2 bathrooms. The vast majority of houses in my sample are single-family homes: the number of bedrooms ranges from 2 (10th percentile) to 4 (90th percentile) while the number of baths ranges from 1 (10th percentile) to 3 (90th percentile). House square footage ranges from 1244 (10th percentile) to 3052 (90th percentile) while the house's construction year ranges from 1957–2004.

County tax websites typically contain information about a house's yearly assessed tax value and transaction history. Some counties do not provide housing transaction information and/or tax assessment information. For counties without transaction information, I try to collect housing transaction data from online listing services or other websites. Table 1 shows that the average selling price of a house is $282,305 while the median selling price is $204,000. The average assessed tax value is $234,314 while the median assessed tax value is $169,000. This disparity shows that tax assessments are on average below a home's FMV, which may bias some of my tests. Later in this paper, I discuss several strategies that I use to reduce this concern. The average (median) home is sold for a nominal gain of 46.7% (28.6%), and roughly 75% of homes are sold for a nominal gain or for zero profit. The distribution is heavily right-skewed, with over 20% of homes sold for a nominal gain of 100% or more.

For some of my tests, I need more detailed data than the previous purchase price and final selling price of a home. Knowing the full sequence of initial listing price, any price changes, and time until final sale is useful for testing whether homeowners have realization utility or prospect theory preferences. In total, I have listing data on nearly 1.2 million homes. Since my sources are online websites, almost all listing observations occur after 2005. The average (median) listing price for all houses is roughly $365,972 ($239,900), with the 10th–90th percentile ranging from $79,000 to $675,000. A significant percentage of houses (37.7%) listed for sale are dropped from the market before a sale occurs. The final selling price is lower for roughly three-quarters of listed homes with an average (median) price reduction of 10.7% (5.26%). The average home remains on the market for roughly 3-4 months before a sale occurs, though a significant subset remain on the market for a longer period of time (at the 90th percentile, homes remain on the market for nearly 2 years). Houses dropped from the market and relisted within a short period of

time are treated as one listing, though the results are unchanged if each dropped listing is treated as a separate observation.

Figure 1 shows the time series of total yearly sales in my sample. In total, slightly more than 4.3 million housing transactions occur over the roughly 25-year sample period. By 1998, most counties (those that report housing transactions) provide housing transaction information. Annual housing sales reach a peak of 830,000 around 2005 before falling to a trough of 300,000 in 2011.

Panels A and B of Figure 2 compare the geographic distribution of houses in my sample to the geographic distribution of the overall US population. The main takeaway is that my sample is broadly similar to the overall US population, which partially alleviates the concern that my sample is not representative of the overall US. Houses from all fifty states are included in my sample, though data is sparse for Western and Midwestern states with low population densities. California is the state with the most houses in my sample while Alaska has the fewest houses.

For some of the tests in my paper, I need to estimate a house's FMV. Since the FMV of a house is unobserved, a proxy must be used in its place. One estimation option is using a hedonic price equation with variables such as the number of bedrooms or baths, square footage, and location to predict a house's value. Genesove and Mayer (2001) use this approach to calculate a house's FMV for a small sample of Boston condominiums. My dataset covers a large panel of houses spanning thousands of zip codes over the entire United States. Similar houses (with the same number of baths and bedrooms, or comparable square footage, etc.) may be priced differently depending on location, especially in large cities. For this reason, I use an alternative approach to estimate a house's FMV. As a robustness check, I estimate a basic hedonic price model and find my results unchanged.

In lieu of a hedonic price equation, I use county tax assessment data to estimate the FMV of a house. Tax assessments are extremely detailed and capture many characteristics that affect a house's value. Crucially, tax assessments account for location and may account for unobserved quality differences among houses. One disadvantage of using tax assessment data is the loss of roughly 1/3 of the sample size because some counties do not have tax assessment data. Although a larger sample size is always desirable, the benefits of using accurate house valuations are likely to outweigh the costs. One potential concern about using tax assessments as a proxy for house valuation is their systematic underestimation of a house's FMV. I find that tax assessments are often less than the selling prices of houses in the sample. Some counties set assessed tax value as a percentage of FMV while other counties do not update tax prices every year. As a robustness check, I rerun my results using only zip codes where the average yearly assessed tax value is within (5%) 10% of the final selling price. None of my results materially change.

Two additional controls, LTV (loan-to-value ratio) and Months_Bet (i.e., the number of months between the day the home is purchased and day the home is sold), are included in most regressions. LTV, or loan-to-value ratio, is the ratio of the remaining loan amount divided by the FMV of a house. Genesove and Mayer (1997) show that the loan-to-value ratio is an important determinant of both the decision to sell a home and the final selling price. Since I do not have loan information for each house, I calculate the remaining loan amount by assuming that homeowners use a standard 30-year mortgage at prevailing interest rates with a 20% down payment. The remaining loan amount is divided by the FMV of the house to get the LTV. For ease of interpretation, I subtract .8 from the LTV and truncate the LTV from below at 0. Months_Bet is the number of months between a house's selling date and purchase date. Additional zip-level demographic control variables include the percentage of the population that

is white, the percentage with a high school diploma, the percentage below the poverty line,

median income, the unemployment rate in 2011, and the labor force participation rate.

3   Are Homeowners Reluctant to Realize Losses?

The histogram of realized gains and losses shows graphical evidence that homeowners

are reluctant to realize losses. In Figure 3a, there is a sharp jump in the number of transactions at

the zero-profit point. The highest probability of selling occurs exactly at the zero-profit point,

which is the opposite of what Ben-David and Hirshleifer (2012) found in the stock market. In

their sample, the lowest probability of selling occurs at zero with a distinct V-shape occurring at

the origin. Figure 3b shows how unique the zero point is: it dominates all other transaction points

by a large magnitude. I interpret this striking finding as evidence for the disposition effect in the

housing market.

I use a regression discontinuity design to formally test whether the probability of selling

jumps at zero. The dependent variable is transaction, a binary variable that equals one if a

transaction occurs at a given price. Each quarter, I compute the fair market value (FMV) of each

house and calculate the log gain or loss (log difference) for the current homeowner. If a housing

transaction occurs during the quarter, transaction takes a value of one; otherwise transaction

takes a value of zero. If a sale does occur, log difference is calculated as the sale price minus the

purchase price of the house. Equations 1 and 2 show the setup.

The key variable of interest is the non-negative dummy, which takes a value of one if the

housing return is non-negative and zero if the housing return is negative. I use a variety of

regression specifications (squared, cubed, to the fourth power) and cutpoints (2.5%, 5%, 7.5%,

10%) to ensure robustness. Two control variables (months between and loan-to-value ratio) are

included in the regression. Columns 1–3 of Table 2 show that the non-negative dummy is highly significant across all model specifications. The RDD results strongly suggest that homeowners are reluctant to realize losses.

I use three additional tests to understand why transactions are clustering at the zero-profit point. I test whether homes initially listed at the zero-profit point, or *zero-listed* homes (i.e., the initial listing price is the same as the previous purchase price), have larger price reductions, take longer to sell, or are more likely to be dropped from the market. I hypothesize the following: 1) the probability of a homeowner dropping a zero-listed home is greater than the probability of a homeowner dropping a non-zero listed home, 2) the average price reduction of a zero-listed home will be greater, and 3) zero-listed homes will sit on the market for a longer period of time before a sale occurs.

Homeowners can avoid realizing losses by dropping homes from the market and waiting for price appreciation. Housing prices may rise due to business cycle fluctuations or inflation. In the case of inflation, the real value of the home remains unchanged, but the *nominal* value will increase. Eventually, the home can be sold for a price equal to or greater than the purchase price. Business cycle fluctuations may temporarily depress the value of a home, especially if it was purchased at the top of a business cycle, and waiting may provide an opportunity to sell the home without a loss at a date in the future. To test whether homeowners are more likely to drop zero-listed homes, I examine the relationship between Drop, a binary variable that takes a value of one if the listing is dropped, and the zero-profit binary variable (Equation 3). I use zip code-year fixed effects and cluster errors at the zip code level (Probit results are nearly identical). Columns 3 and 4 of Table 3 provide evidence consistent with the hypothesis that homeowners are more likely to drop zero-listed homes from the market compared to other homes. Univariate

regressions show that the probability of a homeowner dropping a listing is 38.3% for all homes and 56.2% for zero-listed homes – an increase of 46.7% (t-statistic = 29.36). Multivariate regression results are similar, with a probability of 37.6% for non-zero listed homes and a 56.1% probability for zero-listed homes, a 49.2% increase (t-statistic = 30.35). The fact that a majority of zero-listed homes are dropped from the market before a sale occurs suggests that they are initially overpriced.

To further strengthen this conclusion, I examine the relationship between price reduction, the final selling price of each home minus the initial listing price, and the zero-profit point dummy. If homeowners have marked up zero-listed homes more than other homes, then I expect to find a negative sign. Equation 4 shows the regression (zip code-quarter fixed effects are used with errors clustered at the zip code level). The results show that the final selling prices of zero-listed homes are reduced by roughly 4-5% more than non-zero listed homes. Row 1 of Table 3 shows that zero-listed homes are reduced by 4.9% (t-statistic = -4.3333) more than non-zero listed homes in the univariate regression, while row 2 shows that non-zero listed homes are reduced by 3.95% (t-statistic = -3.47) more than other homes. The average home's list price is reduced by 10-11%, while the average zero-listed home price is reduced by 14-16%.

Finally, I investigate whether zero-listed homes take longer to sell than non-zero listed homes. Previous research by Genesove and Mayer (1997) suggests that homeowners face a tradeoff between price and selling time. Homes listed at full market value or above market value take longer to sell and may sit on the market for months, while underpriced homes sell quickly. If homeowners want to avoid realizing a loss, then zero-listed homes may sit on the market for a long period of time. To test this prediction, I regress the zero-profit dummy and controls on time between the initial listing date and the final sale date (equation 5). Zero-listed homes take a

significantly longer period of time to sell than non-zero listed houses. While the average home takes roughly 272.5 days to sell, zero-listed homes take an average of 383.6 days, an additional 111.1 days, or 41% longer (row 5 of Table 3, t-statistic = 12.15). Multivariate regressions show similar results, with an additional 116.4 days for zero-listed houses, a 43.6% increase over the average time of 266.9 days (t-statistic = 12.73). I interpret these results as evidence that homeowners seek to avoid realizing losses by letting overvalued homes sit on the market while waiting for better offers.

Overall, the empirical results provide strong evidence that homeowners are reluctant to realize losses. Homeowners that initially list their homes at the zero-profit point are more likely to drop the listing, reduce the price, or let their homes sit on the market compared to non-zero listed homes. The tendency for homeowners to drop zero listed homes from the market suggests that homeowners would rather forego sales than take nominal losses. Zero-listed homes take longer to sell than non-zero listed homes by roughly 3.5 months, and their final selling price is 4-6% lower.

4 Realization Utility and Prospect Theory Tests

In this section, I examine whether homeowners have realization utility preferences. First proposed by Barberis and Xiong (2009), realization utility postulates that investors receive a burst of utility when a mental account or investing episode is closed. Selling an asset at a gain creates a positive investing episode, while selling an asset at a loss creates a negative investing episode. Investors think about their investing history as a series of disjointed investing episodes defined by the name of the investment, the purchase price, and the sales price. In contrast with realization utility, standard economic theory postulates that investors receive pleasure or pain at

the point of consumption. Only a change in the absolute level of wealth impacts utility, not the act of realizing a gain or loss.

My previous findings suggest that homeowners have sign realization utility preferences. Sign realization preferences imply that utility jumps sharply upward at zero because incurring even a very small loss is painful. If homeowners care about the sign of a transaction, then a sharp jump in transactions should occur at zero. That is exactly what I find, and my MLS listing data analysis also supports this conclusion.

Next, I test for evidence of magnitude realization preferences among homeowners. Figure 4 plots the distribution of realized gains and losses with the zero-profit point removed (Figure 3 minus the zero point). Figure 4 shows that the probability of selling has a distinct kink at the origin. While the probability of selling a house is downward sloping to the left of the origin, the probability sharply increases to the right of the origin. Homeowners appear to exhibit a combination of sign and magnitude realization preferences. The decline in the probability of selling for small negative values is consistent with sign realization utility. Because realizing any loss is painful, regardless of magnitude, homeowners are least likely to realize small losses. The strong, positive slope to the right of the origin is consistent with magnitude realization preferences. Homeowners appear to enjoy realizing gains, especially compared with realizing comparably sized losses.

I formally test for evidence of magnitude realization utility by running a regression kink to quantify the relative change in the probability of selling around zero. Column 4 of Table 2 is the main column that uses a linear function for gains and losses with a +/- 5% cut point. A variety of regression specifications and cut points are used to ensure robustness. The null hypothesis (that homeowners are rational and do not care about small losses versus small gains)

implies that the slope to the right and to the left of the zero-profit point are equal. Realization utility predicts a negative slope on the left side of the origin (because homeowners do not want to realize small nominal losses) and a positive slope on the right side of the line (because homeowners want to realize gains). The results support this prediction. The slope of the line to the left of the origin is strongly negative (.182) and highly significant (-19.62). The probability of selling a home drops by .91% relative to an unconditional probability of selling a home of 2.86%. The slope of the line to the right of the origin is positive (.352) and highly significant (t-statistic = 22.20). The probability of selling a home increases by 1.52% when the magnitude of the gain increases to 5% above the purchase price. Given that the unconditional probability of selling a home is 2.86%, this represents a 53% jump in the likelihood of selling a house. Having found support for realization utility, I now turn to prospect theory.

Prospect theory postulates that an individual's utility function is steeper for losses compared to equivalently sized gains relative to a reference point. In the context of the housing market, the purchase price of a home is the natural reference point for a homeowner. Since losses are painful, homeowners may list homes with losses at a price above FMV. However, these homes are unlikely to sell quickly. At this point, the homeowner will have three options: drop the price and realize a painful loss, let the house sit on the market, or drop the listing and forgo a sale. Since the majority of moves are local and discretionary, homeowners with prospect theory preferences are likely to pursue the second or third options.

Prospect theory makes the opposite prediction for homes with an FMV above the reference price. Since the gain is now in the concave region of the utility function, homeowners favor locking in a certain gain instead of hoping for further price appreciation. Given the choice between selling a home for a certain gain or allowing the house to remain on the market,

homeowners with prospect theory preferences will opt to quickly sell the home to minimize risk. A housing market shock could drop the FMV of the home below the purchase price, causing the transaction to fall into the convex region of the homeowner's utility function.

Equations 6-10 show the regression specifications for testing prospect theory preferences.[11] The final log selling price, S_{it}, is a function of house i's fair market value (FMV) at time t, $LOSS_{it}$ (the difference between the previous purchase price of house i at time t and the house's FMV at time s, truncated from below at zero), and $GAIN_{it}$ (the difference between house i's FMV at time t and the house's previous purchase price at time s, truncated from below at zero). The LOSS variable measures the impact of prospect theory preferences in the convex region of the homeowner's utility function. If a home has a gain, then LOSS takes a value of 0. If the home has a loss, then LOSS is a positive value that represents the dollar value loss on the house. GAIN takes a value of 0 if the home has an unrealized loss and a positive value if the home has a again. GAIN measures the impact of prospect theory preferences in the concave portion of the utility function. Prospect theory provides the theory-based motivation for modeling GAIN and LOSS as separate variables: the relationship between the FMV of the house and the past purchase price varies for gains versus losses.

Table 4 shows the main results for the prospect theory regressions. Since both the dependent and independent variables are in logs, the results are interpreted in log-log terms. Regressions use zip code-quarter fixed effects, and standard errors are clustered at the zip code level. In column 1, the coefficient on Log LOSS indicates that each 1% loss is associated with a .341% higher selling price over FMV (t-statistic = 33.54). If a house is 10% underwater, then the final selling price is roughly 3.46% higher than its FMV. This result supports the theory that

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[11] Log prices are used in all equations

homeowners are loss averse and mark up the value of underwater homes to reduce painful losses. The GAIN coefficient of -.0532 indicates that every 10% positive difference between a house's FMV and purchase price is associated with a .532% lower final selling price (t-statistic = -20.64). This result is consistent with homeowners leaving money on the table for homes with gains. The inclusion of a squared term reinforces this interpretation. The squared term is positive while the linear term is negative, indicating that homeowners leave more money on the table when the FMV is close to the purchase price. Prospect theory predicts that homeowners should be most eager to lock in small gains, which is what I find.

I use Genovese and Mayer's robustness framework, as shown in equations 9 and 10, to help alleviate concerns that unobserved house quality is confounding the results. Equation 9 includes the residual term from equation 10. Intuitively, the error term represents the unobserved quality of the unit: if the unit is more valuable than its predicted fair market value, then the residual will be positive. Genovese and Mayer show that including the residual from equation 10 establishes a lower bound on the coefficient, while omitting this term (as done in equation 6) establishes an upper bound. Because this model needs tax assessment data for the year in which the home was originally purchased, my sample size drops. To establish a baseline result, I rerun the model from column 1 on the smaller sample size. Despite the reduction in sample size, the coefficient estimates are nearly identical to the original sample, .386 versus .341 and -.0657 versus -.0532. In column 3, log model error is statistically insignificant and barely changes the coefficients of interest. Log LOSS drops from .386 to .382 while log GAIN drops from -.0657 to -.0616. The insignificance of log model error suggests that unobserved house quality isn't confounding the results.

As a further robustness check, I rerun the prospect theory regressions for zip codes with highly accurate yearly tax assessments. For each county, I calculate the average difference between a home's final selling price and the home's assessed tax value. Counties with an average difference of 10% or less between the assessed tax value and actual selling price are included in columns 4 and 5. The results are very close to the estimates produced in columns 1–3 and highly significant. Log LOSS ranges from .396 to .406 while LOG GAIN ranges from -.0384 to -.0502. Including log past model error does not alter the results. Also notice that log assessed tax value is very close to 1 (.967 to .969), indicating that log assessed tax value accurately captures the FMV of the house.

In conclusion, the empirical results strongly confirm prospect theory predictions. Homeowners mark up the value of homes with FMVs below their purchase price but leave money on the table for homes with FMVs above the purchase price. Homes with small gains are underpriced the most, consistent with the prediction that homeowners seek to lock in small gains to avoid the risk of a small gain turning into a loss. By underpricing a home, homeowners can generate a quick sale and ensure that prices do not fall into the convex region of the utility function.

Lastly, I test whether homeowners are more likely to realize large losses than small losses, a prediction made by models that combine realization utility and prospect theory (Ingersoll and Jin (2012; Henderson (2012)). Their models predict that homeowners with deep losses will sell to "reset" their homes' reference price. Figure 5 plots the probability of selling as a function of gain or loss within a +/- 25% window. The main takeaway is that the probability of selling increases linearly as the magnitude of the loss increases. Homeowners are roughly twice as likely to sell a home at a 25% loss than at a small loss of 5% (1.8% versus .85%, significant at

the 1% level). Homeowners are most likely to realize large gains, followed – in this order – by large losses, small gains, and finally small losses. Models that only use realization utility (Barberis, 2012) suggest that homeowners are more likely to realize small losses than large losses, while models with both predict the opposite. Overall, my empirical results are most consistent with behavioral models that combine realization utility and prospect theory preferences.

My identification strategy relies on using a discontinuity around zero to test whether homeowner behavior varies for small gains versus small losses. The discontinuity around zero controls for potential confounding factors because any other determinant of selling is unlikely to systematically vary for small gains compared to small losses. For this reason, tests of sign realization utility are well-identified. The sharp kink in the probability of selling at the origin is fairly striking evidence that homeowners prefer to realize gains instead of losses. However, identification weakens as the probability of selling moves farther away from zero. Confounding factors such as the need to move for a new job, changes in family size, or changes in property taxes are likely to vary with the magnitude of a house's gain or loss. The last prediction that I test, whether homeowners prefer to realize large losses instead of small losses, is the weakest-identified test. Since large losses are far away from zero, it is possible that other omitted factors may be impacting the results. For example, the higher probability of selling large losses may be driven by foreclosures stemming from the recent housing bust. I address this concern by removing observations from the period of the financial crisis (2007–2011) and for houses held for less than 36 months. Results are unchanged. Still, caution needs to be used for interpreting these results – I can only argue that the results are consistent with predictions made by realization utility and prospect theory models.

5 Does the Disposition Effect Impact Overall Housing Sales and Prices?

The results discussed so far show that the disposition effect impacts homeowner selling decisions. For the rest of the paper, I examine whether the disposition effect impacts zip code level housing sales and price appreciation. I find a strong relationship between the percentage of homes below purchase price in a zip code and fewer housing transactions and slower price appreciation over the next several quarters. For zip codes with the weakest disposition effect around the zero point, the percentage of homes with unrealized losses doesn't predict future housing transactions or price appreciation. Next, I investigate whether the disposition effect impacted the housing boom and bust during the financial crisis. I find that zip codes with the strongest disposition effect took longer to peak, had a smaller cumulative price run-up, and a smaller price crash than zip codes with the weakest disposition effect. My evidence is consistent with the hypothesis that the disposition effect partially mitigated the financial crisis by reducing the housing boom and causing the crash to be smaller than it might otherwise have been.

My main measure of the disposition effect uses the ratio of transactions with small gains or no gain (zero-profit point) to the number of transactions with small losses at the zip code level. I set a cutoff of 5% for the calculation of small gains and losses. After setting the cutoff, I calculate two ratios: kink1_ratio (small gains + zero profit) / (small losses) and kink1_percent_ratio (small gains + zero profit) / (small gains + zero profit + small losses). Alternative cutoffs of 2.5% and 10% are used for robustness. I include zero profit transactions in the numerator because realization utility and my empirical results suggest that a reluctance to realize losses is the driving force behind zero profit transactions. For some of the regression specifications, I use ranked versions of the above-mentioned variables. Zip codes are sorted and

ranked into quintiles based on each measure. Zip codes in the top quintile (Q5) have the strongest disposition effect while zip codes in the bottom quintile (Q1) have the weakest disposition effect. Because I need a sufficient sample size to rank zip codes by small gains and losses, I aggregate small gains and losses over all years before calculating the ratio.

Realization utility motivates the construction of my measure. In zip codes with realization utility preferences, I expect the number of transactions with small gains or zero gains to be greater than the number of transactions with small losses. In zip codes without realization utility, the number of transactions with small gains or zero gains should be nearly equal to the number of transactions with small losses.

As a robustness check, I construct a variable that measures the strength of a zip code's prospect theory preferences and rerun my analysis. Odd pricing is the number of transactions with a non-zero last digit. Odd pricing is ubiquitous throughout consumer pricing, including in the housing market. In the context of the housing market, a home with an FMV close to $200,000 may be priced at $199,999 or $199,995. In my sample, 85% of houses sales have a non-zero last digit, with the digit nine appearing most frequently.

Prospect theory provides an explanation for odd pricing. Boyles, Lynch, and Mounts (2007) propose that consumers evaluate the price of consumer goods relative to a reference point and then apply prospect utility to the price. For example, consumers evaluate an item priced at $19.95 relative to a rounded reference price of $20.00. Because $19.95 is less than the reference price, consumers feel slightly better off, and their utility falls in the concave portion of the utility function (small gain). In contrast, an item priced at $20.05 is also compared to a reference price of $20.00, but here consumers perceive the item as overpriced relative to the reference point, and their utility falls in the convex portion of the utility function (small loss). Since small losses

create negative utility, consumers are less likely to buy the item. Rational businesses understand that consumers possess prospect theory preferences, so these businesses choose to rationally price their goods slightly under rounded reference prices to maximize sells.

For robustness, I calculate the percentage of homes in a zip code with a selling price non-divisible by 100 and the percentage of houses in a zip code with a selling price non-divisible by 1000. This proxy is also calculated over the entire time period to ensure sufficient sample size. For some regression specifications, I rank these proxies into quintiles.

I rerun the realization utility and prospect theory tests across the kink ratio or odd pricing quintiles. Figure 6 shows that zip codes with the weakest disposition effect around the zero-profit point (Q1) have a relatively flat kink at the origin while zip codes with the strongest disposition effect (Q5) have a very sharp kink, with the right slope steeper than the left slope. For Q1 zip codes, the left side of the kink is slightly negative (-.036) while the right side of the kink is almost zero and statistically insignificant (Panel A of Table 5). In contrast, Q5 zip codes have a -.190 coefficient for the left side of the kink and a .481 coefficient for the right side.

I now test whether prospect theory preferences vary in strength across zip codes. The coefficients on LOSS in Panel B of Table 5 show significant variation across zip codes while the coefficients on GAIN do not. Zip codes with the strongest disposition effect (Q5) have a coefficient of .459 while zip codes with the weakest disposition effect (Q1) have a coefficient of .212. A home with a FMV that is 10% below its purchase price has a final selling price that is 4.59% above its FMV in Q5 zip codes, more than double the 2.12% coefficient for Q1 zip codes. For odd pricing, zip codes with the strongest prospect theory preferences have a coefficient of .458 while zip codes with the weakest prospect theory preferences have a coefficient of .282. I find that the log GAIN coefficient is nearly identical across all models and quintiles, indicating

that homeowners are relatively uniform in leaving money on the table. Coefficients range from -.0603 to -.0848 across all models in Panel B of Table 5. All coefficients are statistically significant. Overall, it appears that homeowners in all zip codes display prospect theory preferences, but loss aversion is stronger for more biased zip codes.

Lastly, I test whether listing behavior varies across zip codes. I find that homeowners in zip codes with the strongest disposition effect are more likely to drop listings, reduce listing prices (indicating that they overprice listings by a greater amount), and let houses sit on the market longer compared to homeowners in zip codes with the weakest disposition effect. Panel A of Table 6 presents the results for kink1 rank and kink2 rank. Kink1 rank is defined as (small gains + zeros) / (small losses) at the zip code level, then sorted into quintiles. Kink2 rank is defined the same, but the cutoff for gains and losses is 10% instead of 5%. Homeowners in Q5 zip codes drop 42.4% of home listings while homeowners in Q1 zip codes drop 31.68% of home listings, a highly significant difference of over 10% (t-statistic = 6.0). Results are slightly stronger for kink2 rank: Q5 homeowners drop 42.5% of home listings while Q1 homeowners drop 30.65% of listings, a difference of 11.8%. Homeowners in zip codes with the strongest disposition effect reduce initial listing prices by 4-6% more than homeowners in zip codes with the weakest disposition effect (t-statistic = -2.104 for kink1 rank, -3.378 for kink2 rank). Lastly, homeowners in Q5 zip codes take longer to sell their homes relative to Q1 zip codes, resulting in a net 80–90 day difference between Q1 and Q5 listings.

To sum up, homeowners in zip codes with the strongest disposition effect 1) are more likely to drop listings without a sale, 2) reduce house prices by a larger amount, and 3) take longer to sell their homes.

6   Impact of Disposition Effect on Transactions and Price Appreciation

For each zip code, I construct quarterly measures of total houses sold and price appreciation. For each zip code quarter, I construct the following variables: total number of houses, total number of houses sold (sum of all completed sales), number of homes with losses (established using the estimated value of each house for each quarter and represented either by a loss dummy [a binary variable whose value is 1 if the house's FMV is less than the purchase price] or 0 otherwise), percent of houses with losses (number of homes with losses divided by total number of homes), and next quarter (year) price change (using Zillow price indexes). In order to calculate whether the house has a loss, I use the homeowner's previous purchase price and Zillow zip code price indexes to calculate the FMV.

Instead of using tax assessment data (which is only available for a limited number of zip codes), I adjust the previous selling price of each home by the average Zillow zip code price change for each quarter. For example, assume that a house in zip code 30396 is sold for $300,000 in the fourth quarter of 2011. For the 1st quarter of 2012, Zillow data indicates that house prices rose by 1.3% in that zip code. I multiply the $300,000 by 101.3% and calculate the FMV of the house as $303,900. Since $303,900 is greater than the previous purchase price of $300,000, this house has a gain, and the Loss binary variable is coded as 0. I continue to calculate the FMV in this manner until the sample size ends or the house is sold. If a house is sold, I use the new purchase price as the FMV for that quarter and adjust the value of the house using the above description. Using this methodology instead of tax assessment data to calculate profit or loss greatly increases the number of zip codes available for analysis. Results are robust to using tax assessment data instead of Zillow data.

Equations 14–17 show the regressions in this section. The dependent variable is the percentage of houses sold in the next quarter (or year) in zip code i, while the independent variable of interest is the percentage of homes with a loss in zip code i at year-quarter t. Zip code and year-quarter fixed effects are used while errors are clustered at the zip code level.

Tables 7 and 8 show that the percentage of homes with losses for a given zip code year-quarter is strongly associated with lower future housing sales and reduced home price appreciation. If no homes are below purchase price in a zip code-quarter, then roughly 1.02% of all homes will be sold during that quarter. If every home is below purchase price (Loss = 1), then the total percentage of homes sold drops by nearly 58% to only .44% per quarter (t-statistic = -14.70). A two-standard deviation increase in the percentage of homes below purchase price (each standard deviation is about .30) is associated with a 34% reduction in the total number of homes sold each quarter. The impact persists for the next year. Roughly 4.777% of homes are sold every year if no homes are below purchase price, while only 2.31% of homes are sold every year if every home is below purchase price (t-statistic = -5.722). A two-standard deviation change in the percentage of homes below purchase price reduces the percentage of houses sold by nearly 30.6%.

An increase in the percentage of homes below purchase price is also associated with lower home price appreciation over the next quarter or next year (Columns 1 and 2 of Table 8). If no homes are below purchase price, the average next quarter zip code return is roughly 1.13%. In contrast, if all homes are below purchase price, then the average return drops to only .46% (a 59.29% reduction). A two-standard deviation increase in the number of homes below purchase price lowers the next quarter's return by nearly 36%. Yearly returns show a similar result. A two-

standard deviation increase in the percentage of homes below purchase price reduces future price appreciation by nearly 20%.

Having shown that the percentage of homes below purchase price is strongly associated with fewer houses sold in the next quarter or year and lower price appreciation, I next examine whether results vary across zip codes. I use the same regression specification discussed in the previous section but run separate regressions for each kink ratio quintile. Yearly results are shown for brevity, and variables are defined as before.

For zip codes with the weakest disposition effect, none of the coefficients are significant: the percentage of homes below purchase price is not associated with fewer housing transactions or with lower price appreciation. In contrast, zip codes with the strongest disposition effect have statistically significant coefficients for both variables. The empirical results suggest that the disposition effect impacts the aggregate housing market.

7   The Disposition Effect and the Financial Crisis

Having presented evidence suggesting that the disposition effect impacts zip code housing sales and price appreciation, I now investigate the relationship between the disposition effect and the recent financial crisis. Researchers have explored the financial crisis from several different perspectives, but no one has yet examined whether the disposition effect (i.e., the reluctance to realize losses) may have impacted the crisis.

Homeowners with prospect theory and realization utility preferences prefer to sell homes for moderate gains to lock in profits and avoid the possibility of a loss. Since gains are in the concave part of the utility function while losses are in the convex part, selling homes for small gains is preferable to waiting for large, risky gains. Homeowners may leave money on the table

during rising markets because they sell their homes for less than the FMV. Both of these mechanisms slow price adjustments and may dampen speculative-driven price increases during a housing boom. Zip codes with the strongest disposition effect should have had smaller and slower price run-ups. During the housing bust, these zip codes should have had smaller price declines and take longer to recover.

These predictions are similar to findings by Grinblatt and Han (2005) and Frazzini (2006) on stock market momentum. Grinblatt and Han argue that underreaction to news by investors with prospect theory preferences explains stock market momentum. When a large number of investors have unrealized capital losses, they are reluctant to sell their positions. As a result, stocks with a large number of investors with paper losses are overvalued and drift lower. In contrast, when a large number of investors have profitable positions, they are more likely to sell their positions and dampen price appreciation. As a result, these stocks are undervalued and drift higher.

I construct three variables at the zip code level to assess the impact that the disposition effect had on the housing boom and bust: PeakRet (cumulative price run-up during the boom), PeakTime (number of months the boom lasted), and BotDrop (maximum price decline from the peak to the trough). For each zip code, I calculate the total number of months between the peak month (the month with the highest Zillow price index from 2003 to 2010) and the starting month of December 2001. I filter out zip codes with peak months after December 2010. The cumulative price run-up is the percent difference between the peak month's price index and the starting month's price index. For the maximum price decline, I measure the percent difference between the peak month and the minimum month (the month with the lowest Zillow price index between the peak month and the end of the sample).

Equations 18-20 show the OLS regression specifications used to test whether the disposition effect impacted the housing boom and bust. The dependent variables are PeakRet, PeakTime, and BotDrop, the independent variable of interest is $BEHAVE_i$, and controls include the percentage of homeowners who are white, the percentage of the population below the poverty line, median income, unemployment rate in 2011, percentage of homeowners in the labor force in 2011, and percentage with a bachelor's degree. The main variable of interest ($BEHAVE_i$) is kink ratio rank. Kink ratio rank takes a value between 1 to 5, with 1 representing zip codes in the quintile of zip codes with the weakest disposition effect and 5 representing zip codes in the quintile of zip codes with the strongest disposition effect)

During the housing boom, zip codes with the strongest disposition effect (Q5) experienced smaller price run-up and took longer to reach their peak prices. Each increase in the kink ratio rank is associated with a 2.44% decrease in the total price run-up from starting date to peak date (t-statistic = -5.228). Q5 zip codes appreciated by roughly 12% less than Q1 zip codes. Results are very similar for other behavioral proxies. The duration of the price run-up is significantly longer for more biased zip codes: every quintile increase in kink ratio rank is associated with a .942 quarter increase (t-statistic = 10.58). The most biased zip codes peaked roughly one year after the least biased zip codes. These results support the hypothesis that the disposition effect delayed housing price adjustments and reduced the price run-up.

During the housing bust, the zip codes with the strongest disposition effect experienced smaller price drops. Each quintile increase in kink ratio rank is associated with a 2.34% smaller price decline (t-statistic = 11.06). The difference between the quintile of zip codes with the strongest and weakest disposition effect is 9.36%.

Although the previous results are strong and consistent for multiple proxies of the disposition effect, identification still remains an issue. Omitted variables may impact the results: perhaps zip codes with a stronger disposition effect are systematically different from zip codes with a weaker disposition effect. To help alleviate identification concerns, I match zip codes based on education or income and rerun the results. For each zip code in the highest quintile, I select a matching zip code in the lowest quintile. Matching variables include education and income because: a) both of these variables are correlated with my measures of bias at the zip code level (lower-income and less well-educated individuals display stronger biases) and b) both of these variables are likely to be correlated with other determinants of aggregate macroeconomic behavior, such as future employment opportunities. For each matching pair, I estimate coefficient differences and then test whether the average coefficient difference is statistically significant and similar in magnitude to the previous results. Overall, the matching results are highly significant and similar in magnitude to my previous findings.

8 Conclusion of First Essay

Using a unique web-scraped dataset of over several million housing transactions, I find strong evidence that homeowners are reluctant to realize losses. There is an enormous jump in the probability of selling a home at the zero-profit point compared to other price points. I interpret this jump as evidence that homeowners are reluctant to realize losses, even very tiny losses. Zero-listed homes are more likely to be dropped compared to non-zero listed homes. Zero-listed homes take longer to sell by 120 days, and initial listing prices are reduced by 4-6% more than non-zero listed homes. Homeowners want to break-even, so they markup the listing price of houses with unrealized losses. Since these homes are overpriced relative to their FMV,

they sit on the market. Faced with the choice of either reducing the price (and realizing a loss) or dropping the listing, the majority of homeowners decide to drop the listing. Houses that are not dropped remain on the market for an extended period of time and eventually have their listing prices significantly reduced.

My empirical findings are consistent with models that use both realization utility and prospect theory preferences to explain the disposition effect. Homeowners' selling behavior is consistent with both sign realization and magnitude realization preferences. The probability that a homeowner sells a house increases with the magnitude of the gain. A sharp kink in the probability of selling occurs at zero. The sharp kink around zero is unlikely to be attributable to non-behavioral explanations. Zip codes with the weakest disposition effect do not have a kink around zero, while zip codes with the strongest disposition effect have a pronounced kink around zero.

At the zip code level, I find a strong relationship between the percentage of homes below purchase price and future housing sales and price appreciation. Zip codes with the strongest disposition effect around the zero point have a stronger relationship between the percentage of homes with unrealized losses and future housing sales and price changes, while the relationship is non-existent for zip codes with the weakest disposition effect around zero. Lastly, I find evidence consist with the hypothesis that the disposition effect mitigated the financial crisis. Zip codes with the strongest disposition effect had a smaller total price run-up over a longer period of time compared to zip codes with the weakest disposition effect. They also had a smaller cumulative price drop and took longer to recover.

Essay 2: How Does Political Uncertainty Impact Bank Lending? Evidence from Close US

Gubernatorial Elections

9   Hypothesis and Introduction

Does political uncertainty impact the economy? Increasingly, the answer seems yes. Whether it's presidential tweets, political referendums such as Brexit, government shutdowns, or radical policy differences among political parties (such as reducing corporate tax rates versus Medicare for all), markets react to political risk in the short-run. However, it is less clear whether political risk impacts economic decisions (such as lending decisions) or resolves once an election is over. Some economists have argued that political risk creates an "uncertainty tax" on the economy that reduces economic growth while other economists have downplayed the importance of political risk for developed countries with strong institutions.[12] In this paper, I examine how political uncertainty impacts US bank lending.

Evaluating the impact of political uncertainty on bank lending is challenging for several reasons. First, as Kelly, Pastor, and Veronesi (2016) point out, measuring the effects of political uncertainty requires using an event like an election, global summit, or central bank meeting instead of an endogenous proxy for political uncertainty. The event must be significant enough to impact bank loan decisions and difficult to hedge. Second, the identification strategy must cleanly isolate the impact of political uncertainty while controlling for endogenous relationships among bank lending, economic risk, and political risk. This is challenging because political risk is closely intertwined with economic risk, as shown by Baker, Bloom, and Davis (2016).   Bank lending is likely to be correlated with economic variables that influence political outcomes. For

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[12] See Vanguard Chief Economist Joseph Davis, (Bloom 2009)

example, periods of high unemployment or weak GDP growth are associated with more competitive elections. If my identification strategy fails to control for economic uncertainty, then my estimates will be biased.

To overcome these challenges, I use close gubernatorial elections (elections with a 6% or less difference in vote percentage between the top two candidates) as an exogenous source of political uncertainty and gubernatorial term limits to cleanly isolate the impact of political uncertainty on bank lending. Close gubernatorial elections increase uncertainty regarding future state-level policy decisions. Governors are arguably the most powerful political actors in a state and often set the agenda for legislative decisions, which makes it likely that gubernatorial elections generate sufficient uncertainty to impact bank lending. Previous research has shown that gubernatorial elections matter for economic outcomes.[13] For example, banks may be affected by state laws regulating lending, foreclosures, or bank competition. Close elections may also affect the creditworthiness of potential borrowers, causing banks to delay lending decisions until political uncertainty is resolved.

Gubernatorial elections have several features that help mitigate endogeneity concerns. Gubernatorial election dates are set by state constitutions, which alleviates the concern that the timing of an election could be endogenous. Because gubernatorial elections are not synchronized (gubernatorial terms vary from 2- 6 years across states), there is significant cross-sectional and time-series heterogeneity in my sample that helps to control for unobserved factors that impact bank lending. Banks in my sample are exposed to multiple elections (treatments) over time. By restricting my sample to close elections, I ensure that banks can't predict election

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[13] Gubernatorial elections impact household equity participation (Agarwal, Aslan, Ren 2017), corporate investment (Jens 2017), IPO activity (Colak, Durnev and Qian, 2017), political connections (Do, Lee and Nguyen 2015), municipal bond markets (Gao and Qi, 2013), regulatory action (Leverty and Grace, 2018), and bank failure (Liu and Ngo 2014).

outcomes or prepare ahead of time for policy changes. Since the majority of banks in my sample are concentrated in one state, it is unlikely that banks can diversify away state-specific political risk.

To further reduce the concern of endogeneity, I use gubernatorial term limits (a binary variable equaling one if a state has term limits) as an instrumental variable for close elections. Gubernatorial term limits increase political uncertainty by preventing incumbent governors from seeking reelection. Incumbents are far more likely to win reelection compared to challengers (over 87% of incumbents win reelection in my sample), so barring incumbents from running for reelection increases political uncertainty by creating more competitive elections. Using an IV helps alleviate the concern that close elections are endogenous. Because close elections are more likely when the economy is weak, the trend in bank lending for the treated sample may differ from the control sample. This would violate the parallel trends assumption and bias my results. As long as the exclusion restriction holds (term limits only impact bank lending through close elections), then the IV will be unbiased. Term limits are fixed by state constitution or referendum (36 of 50 states have term limits) and arguably cannot be manipulated by politicians or banks.[14] Since gubernatorial term limits are a relatively narrow legal requirement that only impact the governor, it is reasonable to believe that the exclusion restriction holds. Nevertheless, I present evidence to rule out several alternative stories that challenge instrument validity.

I find that banks located in states with term limited close elections make 1.43% fewer total loans relative to control banks. Since the mean (median) quarterly change in total bank loans is 2.28% (1.5%) per quarter, close elections reduce the mean (median) growth in bank loans by 62.7% (95.3%). These results suggest that banks pause new lending until political

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[14] A few states adopt term limits during my sample. Excluding these states does not change my results.

uncertainty is resolved. Banks that are more exposed to state-specific political risk (small, highly leveraged, and geographically concentrated banks) reduce lending by a larger amount. The smallest two quintiles of banks (ranked by total assets) reduce new lending by 80% (1.95% fewer total loans) in the quarter before a gubernatorial election while the largest quintile does not reduce lending. The most levered banks reduce lending by roughly twice as much as the least levered banks, and banks concentrated in one state reduce lending by almost twice as much as banks that span multiple states. As an additional robustness check, I restrict my sample to banks located in contiguous border counties (border MSAs) and use county-pair year (msa-year) fixed effects to control for local economic shocks that may bias the results. The subsample results remain highly significant and similar in magnitude to the full sample results. Matching banks in border counties (or MSAs) by bank size also produces similar, significant results.

Publicly traded banks experience a spike in volatility and lower returns in the quarter before gubernatorial elections. Using the same difference-in-difference framework and IV, I find that bank stock returns are 5.72% lower and volatility 11% higher in the quarter before close elections. In a seven-day window around election day, bank stocks experience a 4.7% abnormal return (6.1% for small banks). The abnormal stock reaction around election dates strongly suggests that the outcome of the election (which candidate wins) matters for banks. Since publicly traded banks tend to be larger than the average bank in my sample (and I find a larger drop in lending for small banks compared to large banks), return and volatility results should be viewed as a lower bound for the impact of political uncertainty on bank valuation.

Having shown that banks reduce lending before close gubernatorial elections, I next examine whether bank lending recovers in the quarters following a close election. If political risk is transitory and resolves once the election outcome is known, then lending should fully recover in

the next several quarters following an election. On the other hand, if political risk is long-lasting and uncertainty remains after the governor is elected, then lending may not recover. I find that bank lending only partially recovers following close elections. Lending is .446% and .218% higher in the next two quarters after close elections. By the end of the next year, the difference in total loans between treated and non-treated banks is -1.34% (almost significant at the 10% level), roughly ½ the culminative drop during the 2nd and 3rd quarters.   For small banks, lending is statistically lower a year after close elections and volatility is statistically higher several quarters after close elections, suggesting that elections do not fully resolve political uncertainty.

I use a variety of tests to rule out alternative explanations. One possible explanation is demand-based: perhaps bank customers (especially corporations) reduce their demand for new loans in response to political uncertainty. Companies may respond to heightened political risk by foregoing investment and reducing requests for new loans, as documented by Jens (2017). I use several strategies to disentangle a bank-driven supply side channel from a corporate demand-driven channel. First, I examine several types of bank loan categories to ensure that commercial and industrial (C&I) loans are not driving the results. I find that banks reduce lending for several categories of loans, including personal loans and real estate loans. Next, I sort banks into quintiles based on the ratio of C&I loans to all loans and find that results are stronger for banks with the lowest percentage of C&I loans (where C&I loans account for less than 10% of all loans). Volatility spikes are stronger and event study abnormal returns larger for banks with few C&I loans. Banks that have never made C&I loans still reduce lending before close elections. Although these tests don't completely rule out a demand-side story, they suggest that my results are not a repackaging of previous findings that political uncertainty causes corporations to reduce investment.

This is the first study that establishes that political uncertainty impacts bank lending through a supply-side channel. Since banks are among the most important components of the economy, my findings show that political uncertainty impacts an important economic channel. By using a difference-in-difference framework combined with term limits as an IV, I utilize a strong identification framework to establish a link between political uncertainty and bank lending in the United States, a large, developed country with strong institutions. It is highly likely that political uncertainty has a stronger impact on other countries and markets outside the United States. I also find that election outcomes don't fully resolve political uncertainty.

I make several contributions to several strands of literature. First, I contribute to the growing literature that examines how political uncertainty impacts the economy and financial markets. Previous research has shown that gubernatorial elections impact household equity participation and financial decision (Agarwal, Aslan and Ren, 2017), corporate investment (Jens, 2017), IPO activity (Colak, Durnev, and Qian, 2017), political connections (Do, Lee, and Nguyen, 2015), municipal bond markets (Gao and Qi, 2013), regulatory action (Leverty and Grace, 2018), and bank failure (Liu and Ngo, 2014). In the international context, policy uncertainty impacts aggregate business cycles and economic recoveries (Baker et al., 2014; Baker, Bloom, and Davis, 2016; Julio and Yook, 2016) and risk premia (Boutchkova et al., 2012; Pastor and Veronesi, 2012, 2013; Brogaard and Detzel, 2016; Kelly, Pastor, and Veronesi, 2016). Political uncertainty impacts a variety of firm-level decisions including capital expenditures (Gulen and Ion, 2016), research and development (Atanassov, Julio, and Leng, 2016), equity issuance (Colak, Durnev, and Qian, 2016), and merger and acquisitions (Bonaime, Gulen, and Ion, 2017).

Second, I contribute to the theoretical literature that examines how uncertainty impacts investment and lending decisions. Many papers examine how firm investment behaves under

uncertainty, including seminal papers by Abel (1983) and Bernanke (1983). Although I study bank lending instead of corporate investment, my results suggest that banks delay loan decisions until political uncertainty is resolved. Lastly, I contribute to the literature that studies how elections and political risk impact financial institutions. Bank failure is less common in the year before gubernatorial elections (Liu and Ngo, 2014), gubernatorial elections cause regulators to delay insurance decisions (Leverty and Grace, 2018), government-owned banks in developing markets increase lending before elections (Dinc, 2005), and political factors determine the likelihood of bank bailout and moral hazard (Dam and Koetter, 2014; Kostovetsky, 2015). I contribute to this literature by showing that political uncertainty negatively impacts bank valuation and lending decisions.

10 Data and Variables

I collect political data (including election outcomes, vote share, and party identification) from Congressional Quarterly (CQ) Press online database from 1985-2017. Term limit data is hand-collected from various online and print sources. Election is a binary variable taking the value of 1 (a gubernatorial election occurred in the state during the quarter or year) or 0 (no election occurred in the state for the quarter or year). Similarly, presidential election is a binary variable that takes a value of 1 if a US presidential election occurred in the given year or quarter. Margin is the percentage difference between the top two vote getters in the election. My sample runs from 1985-2017 and includes all gubernatorial elections, including the special election in California in 2003, except for elections around the financial crises. I omit gubernatorial elections from 2007-2011 to prevent the financial crises from biasing the findings. Results are nearly

identical if observations from a shorter window of time around the crisis are excluded (2008-2010) or if all observations from the financial crisis are included.

Table 10 provides summary statistics for the 1600 state election-year observations. Close is a binary variable that equals one if the vote differential between the first and second place candidates is less than six percentage points, or roughly twice the 3% margin of error for most polls. About 27.6% of elections fall within the 6-point margin with an average (median) vote differential of 3.187% (3.32%). For non-close elections, the average (median) vote differential is roughly 23.67% (18.68). Results are robust to using different breakpoints for close elections.

Term limits, the instrumental variable that I use for identification, is a binary variable that equals one if the incumbent governor is term-limited for the current election. Nearly all states (35 of 36 states) with term limits have a binding two-term limit set by a state's constitution or state referendum. Eight of the 35 states impose a two-term lifetime limit on governors. Once a governor has served two terms, they are no longer allowed to run for the office of governor. The remaining 27 states with consecutive term limits allow previous governors to run again after a waiting period (either 4, 8 or 12 years), though it is extremely rare for governors to run again after serving two terms. Virginia does not allow governors to run for reelection, though governors may run again after waiting one term. Excluding Virginia from the sample does not change any results.

A total of 23.3% of elections are term-limited in my sample. Term-limited elections have a median margin of 8.85%, nearly half the 15.06% median margin of non term-limited elections. Close elections are more likely to be term-limited than non-close elections (31.9% versus 20.1%). Based on these preliminary statistics, it appears that term limits are associated with closer elections.

State-level economic variables are included in all regressions to control for time-varying economic conditions. Among the controls included in the regressions are quarterly state unemployment rate (unemply) from the Bureau of Labor Statistics (bls.gov), annual change (quarterly when available) in state gross domestic product (ΔGDP) from the Bureau of Economic Analysis (bea.gov), and a recession binary variable equal to one if a recession occurred during the quarter (defined by the National Bureau of Economic Research).

Table 11 presents bank quarter-level summary statistics for the sample from 1985-2017. All variables have at least 1,161,398 observations. Bank variables (size, leverage ratio, profit, return on equity, and loss) are downloaded from Bank Regulatory dataset while macroeconomic and political variables come from the previously stated sources. The main dependent variable of interest, ΔTotal Loans, is the quarterly percentage change in total loans and is winsorized from above (observations with a value greater than 100% are dropped) to remove obvious data errors. The mean (median) percentage change in loans is 2.28% (1.5%) per quarter. Roughly 24% of all observations occur during a presidential election year, 25% occur during a gubernatorial election year, 7% occur during a close gubernatorial election, and 5% occur during a term-limited election.

Daily returns from the Center for Research in Security Prices (CRSP) are used to calculate market returns and volatility in difference-in-difference and event-study regressions. I use a difference-in-difference framework to test whether political uncertainty impacts bank stock returns (average of all daily CRSP returns in a quarter) or volatility around close gubernatorial elections. Volatility is defined as the square root of a stock's mean squared daily return over the given quarter and is used to test whether or not political uncertainty creates market uncertainty. For event study analysis, I use the absolute value of abnormal bank stock returns in a 7-day

trading window around gubernatorial election dates to test whether investors react to the outcome of close elections. Bank Regulatory data is matched with CRSP/Compustat data using either the PERMCO-RSSD link from the Federal Reserve Bank of New York (https://www.newyorkfed.org/research/banking_research/datasets.html) or via Compustat Banking database.

11 Motivation and Channel

Elections can create political uncertainty for banks in two ways. First, elections may result in policy changes that directly impact bank profitability, capital structure, and risk. For example, changes in interstate banking laws allowed banks to open branches across state lines, generating new lending opportunities (positive) while simultaneously increasing bank competition (negative). Banks delay issuing loans until policy uncertainty is resolved. Secondly, elections may impact the general macroeconomic environment and the creditworthiness of borrowers in the state. Changes in the level of spending, tax rates, or firm-level policies may impact the riskiness of potential borrowers. Bernanke (1983) explains why firms delay investment during periods of greater political uncertainty. In Bernanke's model, firms can choose among different investment options with different payoffs. Because projects are costly to reverse (cost to switching between projects once started), firms delay investment until uncertainty is resolved. Extending the logic of Bernanke's model to banks suggests that banks will defer lending until after political uncertainty is resolved. If changes to state policies (such as minimum wage laws, regulation, subsidies, spending) affect loan payoffs, then banks will defer lending until after the election.   If political risk is transitory and quickly resolved, then lending should recover in the next several quarters after an election. If political uncertainty lingers long after an election, then

lending may only partially recover (perhaps a close gubernatorial election creates a new state of greater political uncertainty).

In addition to increasing political uncertainty, gubernatorial elections may also impact bank risk premia. Pastor and Veronesi (2012) and Kelly, Pastor and Veronesi (2016) show that political uncertainty impacts risk premia and causes a decrease in investment for some firms. Elections may cause a bank's risk premia to rise, which causes banks to reduce lending regardless of whether the riskiness or payoffs for potential loans change. Elections may also increase the risk premia of firms that banks may lend to, which could also contribute to a reduction in bank lending. Many of the banks in my sample are not publicly traded, so a risk premia explanation is not sufficient to explain my results. Nevertheless, I find evidence strong that gubernatorial elections impact bank stock returns and volatility.

Since I use gubernatorial elections as my source of political uncertainty, it's important to establish that governors have sufficient control over the political process to change state-level policies and banks cannot easily diversify away political risk. Governors, as representatives of the executive branch, share political power with the legislative and judicial branch. A large literature in political science has examined the relative power of governors, state legislatures, and courts and concluded that governors are the strongest single actor in the political process. Governors not only possess veto power over legislation, but also typically set the legislative agenda. A large, growing literature has demonstrated that gubernatorial elections impact many different markets and economic decisions. gubernatorial elections impact IPO activity (Colak, Durben and Qian, 2017), the value of political connections (Do, Lee and Nguyen, 2015), municipal bond markets (Gao and Qi, 2013), and corporate investment (Jens 2016). These

findings strongly suggest that governors have sufficient influence over state policies to impact bank lending.

The risk associated with gubernatorial elections cannot be easily diversified away. In the United States, there are four gubernatorial election cycles. Each year, at least two states hold gubernatorial elections while the average number of elections per year is five. The vast majority of banks in my sample are highly concentrated in one state, which greatly limits their ability to hedge political risk. Even if banks span multiple states, it is difficult to hedge political risk for a few close elections each year. Because of significant fixed costs, banks are unlikely to open or close bank branches to avoid transitory political risk. For these reasons, it is unlikely that political uncertainty can be hedged away.

There are several potential alternative explanations for the change in bank lending around gubernatorial elections. One possibility is that my findings are a simple repackaging of previous findings by (Jens 2016) that corporations reduce investment before gubernatorial elections. Under this hypothesis, the reduction in bank loans is due to corporations voluntarily reducing borrowing (demand-side driven) instead of banks reducing lending (supply-side driven). Later in this paper, I use three strategies to separate the demand-side story from a supply-side story and find that the supply-side story (banks restrict lending due to political risk) has stronger explanatory power for my sample.

An alternative explanation for the decline in bank lending is political manipulation designed to alter the probability of a candidate winning. Previous research by Cole (2009) and Dinc (2005) find that government-owned banks increase lending before elections. Betrand, Kramarz, Schoar and Thesmar (2006) find that firms with a politically connected CEO increase investment before municipal elections in France. A common strand through all these papers is that the

government stimulates the economy to increase the probability of reelection. Since I find that banks reduce lending before gubernatorial elections, this alternative explanation is unlikely. The fact that I do not find any evidence of increased bank lending before gubernatorial elections suggests that banks do not feel political pressure to increase loans before elections, or that any political-driven bank lending is offset by the negative effects of political uncertainty. Subsample analysis and end do not support

Another explanation for the drop in bank lending around close gubernatorial elections is bank manipulation. Banks may try to manipulate the probability that a governor from a certain party wins the election. Since banks reduce lending before close elections, the implication of this hypothesis is that banks prefer challengers to incumbents, which is inconsistent with previous findings. Banks contribute more money to incumbents than challengers, which strongly suggests that banks favor incumbents. My empirical findings also do not support this alternative explanation. The manipulation hypothesis suggests that banks with significant market power should reduce lending by a larger amount than smaller banks, since the net impact of a reduction in lending by a small bank on the election is minimal. However, consistent with a risk-based explanation, I find that small banks reduce their lending by a larger amount than big banks. Overall, my findings fit with a political uncertainty explanation for the reduction in bank lending more than any alternative explanation.

12 Model and Identification

Estimating the impact of political uncertainty on bank lending requires overcoming several significant challenges. First, an exogenous shock to political uncertainty is needed for identifying the response of bank lending to a change in political risk. Second, political risk must be

separated from business cycle risk. My difference-in-difference framework combined with term limits as an IV allows me to overcome each of these challenges.

Gubernatorial elections create an exogenous shock to political uncertainty. As (Kelly, Pastor and Veronesi, 2016) point out, measuring the effects of political uncertainty requires using an event like an election, global summit, or central bank meeting instead of a proxy for political uncertainty (such as an uncertainty index). Using gubernatorial elections as my source of political uncertainty has the advantage of fixed election deadlines. Because all elections in my sample are predetermined by state constitutions (with the exception of the California special election), the issue of selection bias is eliminated because governors cannot call an election when they consider their political fortunes to be favorable, such as in a parliamentary system.

The relatively large number of gubernatorial elections allows me to use banks in states without elections as a control group in a difference-in-difference framework. I estimate the following difference-in-difference model:

$$\Delta \text{TotLoans} = \beta_0 + \beta_1 \text{close\_election}*Q3 + \beta_2 \text{election} + \beta_3 \text{pres\_election}Q3 + \beta_4 Q3 +$$
$$\lambda_1 \text{Econ\_controls} + \lambda_2 \text{Bank\_controls} + \gamma_s + \zeta_t + \varepsilon \quad (1)$$

Banks in states with an election in the designated quarter/year are the treated group while banks in states without an election are the control group. I use quarterly dummies to control for potential cyclicality in bank lending. The dummy variable election captures any effects of the election cycle on bank lending as well as any systematic differences between banks in treated states versus banks in control states. The key coefficient is the interaction term between close election and the quarterly dummy, with close election*Q3 representing the impact of political

uncertainty on the quarter before a close gubernatorial election. Later in the paper, I examine whether gubernatorial elections impact bank lending several quarters before and after a gubernatorial election. For these regressions, the same framework is used and the key variable of interest is close election*Qtr, where Qtr may be Q1, Q2, Q3, Q4 (four quarters of a gubernatorial election year), Q5, Q6 (two quarters following a gubernatorial election year), or a combination of the quarters (such as Q2-Q8, the total change in bank loans from the second quarter of a gubernatorial election to the fourth quarter of the year after gubernatorial elections).

Presidential elections are a potentially confounding source of political uncertainty distinct from gubernatorial elections. I control for the potential effect of presidential elections by interacting the presidential election dummy with Q3 (pres_election*Q3) in the main regression specification. In all bank lending regression specifications, presidential election variables are statistically insignificant and have no impact on the main variable of interest. For robustness, I omit gubernatorial elections that occur in the same year as a presidential election and find that the results are unchanged. As an additional robustness test, I include a third treatment effect (presidential election*gubernatorial election*Q3) and find that the results do not change. This difference-in-difference-in-difference model uses close election, presidential election, and quarterly dummy as treatments to capture the effect of political uncertainty caused by gubernatorial elections after controlling for political uncertainty caused by presidential elections. The reduction in bank lending appears to be driven by political uncertainty from close gubernatorial elections.

Several assumptions need to hold for the above model to capture the casual effect of political uncertainty on bank lending. The treatment variable, close election, must be exogenous. In the context of my model, this assumption requires banks to avoid self-selection into a state

based whether the state does or does not have an election. Because of the fixed required to open a branch, banks are extremely unlikely to close and open branches to avoid political elections. In addition, results are unchanged if I restrict my sample to banks concentrated in one state, which completely eliminates the possibility of self-selection. The second assumption, that banks cannot influence the year in which an election is held, also holds. Election cycles are set by state constitutions and typically can only be changed by constitutional amendment. The last change to an election cycle occurred in 1984 in Arkansas, when the state changed its election cycle from 2 years to 4 years. As a robustness check, I omit Arkansas from the sample and find the results unchanged.

If my framework is valid, then there should be no significant differences between bank lending in treated versus nontreated states except around close gubernatorial elections (parallel trends assumption). The differences between treated and non-treated groups are statistically significant only in the two quarters before close elections. None of the coefficients are significant when all elections are included. Placebo tests that randomly assign gubernatorial elections to random quarters are not significant. These results strongly suggest that the parallel trends assumption holds.

A significant challenge to identifying the impact of political uncertainty is separating political risk from business cycle risk. Baker, Bloom, and David (2016) use a broader uncertainty index to measure economic uncertainty and find that political uncertainty is strongly correlated with presidential elections, wars (First and Second Gulf War), terrorist attacks (9/11), stock market crashes, and the financial crises. Since political and economic uncertainty are so closely intertwined, an instrumental variable is needed for a clean estimation of the importance of political risk. Weak economies often cause elections to be closer because voters blame the

incumbent party and/or governor for the weak economy (Atkeson and Partin, 1995). A weak economy often encourages stronger challengers to enter the race (Van Dunk, 1997) and allows challengers to raise more money. Bill Clinton's famous 1992 campaign slogan "It's the economy, stupid," illustrates the importance of economic conditions for election outcomes (political analysts cited the weak economy and 1991 as a key reason that George Bush lost reelection).

I use state term limits as an IV to resolve the problem of reverse causality for close gubernatorial elections. Term limits increase political uncertainty by preventing incumbent governors from seeking reelection. Incumbents have a number of structural advantages that increase their likelihood of reelection. Incumbents are overwhelmingly likely to win reelection (Nearly 83.9% of incumbents in my sample win reelection) compared to non-incumbents (34.5% of non-incumbents in my sample are elected). I use term limits instead of incumbency as my instrumental variable to avoid selection bias. Incumbent governors are less likely to run for reelection if their probability of winning is low (Atkeson and Partin, 1995; Van Dunk, 1997). Since the probability of winning is closely tied to the state of the economy, using incumbency could bias my results.

In order for term limits to be a valid IV, both the relevance and exclusion conditions must be satisfied (Roberts and Whited, 2012). I test the relevance condition by examining first-stage IV results with close_electionQ3 as the dependent variable and term_limitsQ3 and controls (pres_electionQ3, election, quarter, state-level economic controls, and bank-level controls) as explanatory variables. The regression's F-statistic of 18.98 and R^2 of 20.46% shows that term limits are strongly related to election closeness. The strong relationship between election closeness and term limits alleviate the concern that my instrument is weak. Alternative

regression specifications with different breakpoints for close elections are all strongly significant and reinforce the relevance of the instrument.

The exclusion condition, the requirement that the only effect term limits have on bank lending comes through election closeness, can't be directly tested. However, a careful examination of the institutional features of term limit laws strongly suggests that the exclusion restriction holds. Term limits are specified in state constitutions and rarely ever changed. It is unlikely that any actor, such as banks, firms, or governors could change the laws to serve their political interests. There are several states that adopted term limit laws during the early 1990s (Arizona, California, Colorado, Michigan, Wyoming) in response to public demand for greater electoral accountability and improved governance. There is no evidence that any political or economic actors supported term limits to economically advantage themselves. Many politicians opposed the imposition of term limits, and there is no reason to believe that the adoption of term limits biased my sample. For robustness, I omit states that adopted term limits during my sample period and find that the results are unchanged.

Alternative explanations for bank lending could challenge instrument validity. If banks or politicians try to manipulate bank lending to influence election outcomes, then the IV is called into question. Political manipulation has been documented in countries outside the United States (Dinc, 2005; Cole, 2009; Bertran, Kramarz, Schoar and Thesmar, 2006) for both bank and firm lending. However, evidence for political manipulation is limited in the United States. Drazen (2001) finds no evidence of political manipulation during US elections or any other OCED countries.

The goal of political manipulation is to increase the probability that the ruling political party or incumbent governor wins the next election. Any political manipulation that tries to

stimulate the economy should cause an increase in bank lending, the opposite of my empirical findings. This suggests that either political intervention does not matter for bank lending or that my coefficients would have been larger in the absence of political manipulation.

Even if limited political intervention does occur, the exclusion restriction is still likely to hold if the goal of political manipulation is to affect the outcome of a close election. It is difficult to build a narrative that decouples manipulation from election closeness. If political manipulation is the result of close elections, then term limits remains a valid instrument for measuring the effects of political uncertainty. Based on above given reasons, it is reasonable to conclude that term limits satisfy both the relevance and exclusion conditions and is a valid instrument for election closeness.

13 Results

Table 12 presents the main difference-in-difference results that test whether gubernatorial elections reduce bank lending. All regressions, except column 3, use a broad number of control variables. Economic control variables include quarterly state unemployment rate (unemp) from the Bureau of Labor Statistics (bls.gov), annual change in state gross domestic product (ΔGDP) from the Bureau of Economic Analysis (bea.gov), and a recession binary variable equal to one if the quarter occurs during a National Bureau of Economic Research defined recession. Q3 is dummy variable taking a value of 1 if the quarter is the third quarter of the year. Political control variables include election (a binary variable taking a value of 1 if a gubernatorial election occurs during the quarter, 0 otherwise) and pres_election*Q3 (a dummy variable taking a value of 1 if a presidential election occurs in the third quarter of the year, 0 otherwise). Bank-level controls

include size (log total bank assets), leverage ratio, profitability, return on equity, and loss. State and year fixed effects are used while standard errors are double clustered by state and year.

In column 1, I use electionQ3 as the treatment variable and examine whether the coefficient is statistically significant. This coefficient is nearly zero, indicating that gubernatorial elections have no impact on bank lending. In column 2, I restrict the sample to only close elections (elections where the difference between the top two candidates is 6% or less). In this case, the coefficient is negative and highly significant, (-.00429 with a t-stat of -2.589), indicating that banks in states with gubernatorial elections reduce lending by.429% compared to banks in states without elections. These results suggest that only close elections matter for bank lending and help mitigate the concern that systematic differences between states holding elections and states not holding elections are biasing the results.

In columns (3) and (4), I use the closeness measure fitted from a first-stage estimation of close election*Q3 on the IV (term limits*Q3) and controls. The interpretation of the coefficient of interest, close electionQ3, remains the same: it measures whether close elections reduce bank loans in states with elections compared to bank loans in states without elections. I find that gubernatorial elections reduce aggregate bank loans by 1.43% in states with elections relative to banks in states without elections. IV estimation yields a coefficient nearly three times as large as OLS estimation (column 4 versus column 2). Column 3 excludes macroeconomic and bank control variables. Including these control variables cause the estimated coefficient to slightly drop, from 1.68% to 1.43% while remaining highly significant.

The stronger IV estimates suggest that OLS regressions significantly understate the impact of political uncertainty on bank lending. This understatement is likely due to misattributing political uncertainty as economic weakness. Since weak economics tend to create closer political

elections and political and economic risk are closely intertwined, using an IV to tease out the impact of political uncertainty from economic uncertainty is essential.

Political uncertainty can be confounded by economic uncertainty if weak economic conditions increase the probability of a close election[15]. If banks have already reduced lending due to a weak economy, then the change in bank lending due to a political shock is likely to be smaller because banks have already reduced lending. The change in bank loans from Q1 to Q2 (or Q2 to Q3) will be smaller for the treated group than if the economy was strong. As a result, the coefficient on close election*Q3 will be biased downward and underestimate the true effect of political uncertainty on bank lending. IV estimates will be larger than the OLS estimates because IV regressions use term limits as an unbiased source of political uncertainty. My findings of a large difference between IV and OLS regression coefficients suggest that reverse causality is a significant problem that requires an IV.

In Table 13, I test whether or not gubernatorial elections impact bank stock returns and volatility. I use the same difference-in-difference framework, except the dependent variable is either mean daily stock returns (columns 1-3) or volatility (columns 4-6). Political and quarter control variables are used, along with state and year fixed effects. Standard errors are double clustered as before. Overall, the results reinforce the hypothesis that gubernatorial elections impact banks: stock returns are lower and volatility is higher in the quarter before gubernatorial elections. IV estimates are larger than the OLS regressions, likely due to the reverse causality between economic weakness and close elections. Stock returns are 5.72% lower (t-stat=-2.44) in the third quarter before gubernatorial elections while daily volatility is .0068 higher (t-

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[15] A large literature in political science strongly suggests that economic weakness causes closer elections. Structural models that try to predict election outcomes use economic variables such as the unemployment rate and GDP growth to predict the outcome of elections. Thus, it is highly likely plausible that economic weakness confounds political uncertainty.

stat=2.469). As an additional check on whether or not gubernatorial elections directly impact bank valuation, I examine abnormal returns from a market model in a seven-day window around election outcomes.

The CARS (Table 14) strongly suggest that gubernatorial elections impact bank valuation. Bank stocks have abnormal CARs (BHARs) of 4.689% with t-stats of 30.957 (4.740% with t-stat of 30.32) in the seven-day window around gubernatorial elections. Since some gubernatorial elections occur on the day as presidential elections (for presidential election cycle years), presidential elections may confound my event study results. In columns 3 and 4, I omit presidential election years from my event study and report the results. Market reactions to gubernatorial elections are slightly stronger in non-presidential years than presidential election years: the average bank stock has a seven-day CAR of 4.885% and a BHAR of 4.943%. As a final check, I omit all gubernatorial elections that occur in either a presidential election year or a Congressional election year (midterm elections) and report the results in columns 5 and 6. Bank stocks have an average CAR of 3.822% and an average BHAR of 3.824% for non-federal election years. Both coefficients are significant at the 1% level and confirm that the gubernatorial elections move bank stock prices.

Having shown that banks reduce lending before gubernatorial elections, the next question to ask is whether bank lending recovers in the quarters following a gubernatorial election. Whether or not bank lending recovers depends on how quickly political risk resolves. If political risk is transitory and resolves once the election outcome is known, then lending should fully recover in the next several quarters following an election. On the other hand, if political risk is long-lasting and uncertainty remains after the governor is elected, then lending may only partially recover following gubernatorial elections. Either story or a combination of both are plausible.

Following a governor's election, banks will have additional information that allows them to make more informed lending decisions. Banks may delay making loans for a short period of time until the election is over, then make loan decisions based on new information gleaned from the outcome. A full resolution of political uncertainty is unlikely to occur until months (years) after the outcome. An example from the 2016 presidential election illustrates this point: although the Republican Party ran on repealing the Affordable Care Act and won control of both Congress and the Presidency, they were unable to repeal the Affordable Care Act because three Republican Senators voted against the repeal. The final outcome of the bill was unknown until the votes were tallied months after the election. Even after legislation is passed, political uncertainty may remain because executives and bureaucratic agencies have significant leeway in implementing legislation. Ultimately, how quickly political uncertainty resolves is an empirical question.

I find that bank lending declines in the two quarters before gubernatorial elections and only partially recovers following elections. In the second quarter of gubernatorial election years, lending drops by 1.24% (t-stat = -2.141), which is only slightly less than the 1.43% drop in the 3rd quarter. Cumulatively, total loans decline by nearly 2.7% for banks in states impacted by gubernatorial elections compared to banks in non-impacted states. Lending is higher (though not significantly so) for banks in states with gubernatorial elections during the fourth and fifth quarter (the fifth quarter is the first quarter of the year after a gubernatorial election). Lending is .446% higher in the fourth quarter and .218% higher in the fifth quarter. By the end of the next year following gubernatorial elections (Q8), the culminative difference in total loans between treated and non-treated banks is -1.34% (almost significant at the 10% level), roughly ½ the

culminative drop during the 2nd and 3rd quarters. These results suggest that political uncertainty only partially resolves following gubernatorial elections.

In Panel B, I examine daily stock returns around gubernatorial elections and find that stock returns are slightly, but not significantly higher for treated banks in the fourth quarter (.00120). The results in Panel C present a similar story: volatility is not significantly higher for treated banks except for the third quarter, although treated banks have higher volatility in all quarters. Overall, the stock return and volatility data suggest that the market response to political uncertainty quickly resolves following the outcome of gubernatorial elections.

I now test whether the impact of political uncertainty varies with various bank characteristics, including bank size, bank leverage, and geographic concentration. A priori, I expect political uncertainty to have a stronger effect on small banks than large banks for several reasons. Smaller banks are likely to be less diversified than larger banks and have access to fewer funding sources. If gubernatorial elections create uncertainty about bank loan payoffs, then smaller banks may have less ability to hedge or diversify away political risk. If gubernatorial elections create political risk, then I expect highly leveraged banks to reduce bank lending more than low-leveraged banks. The reasoning is straightforward: highly leveraged banks are more exposed to shocks than low-leveraged banks and more likely to reduce bank lending in response to political risk. Lastly, I expect geographically concentrated banks to reduce bank lending more than multistate banks with branches in non-treated states.

I sort banks into size quintiles based on total assets each year and rerun the difference-in-difference regression for each quintile. In addition to checking whether the results are stronger for smaller banks than larger banks, I also confirm that large mega banks are not driving the results. Panel A of Table 16 shows that the reduction in bank lending is stronger for smaller

banks and significant for the bottom four quintiles. Banks in quintiles 1-4 reduce bank lending by an average of 1.57% in the third quarter before gubernatorial elections. The largest banks do not reduce bank lending, suggesting that they can either diversify away risk or are less exposed to political risk. The quintile results are consistent with a political uncertainty story and inconsistent with alternative explanations such as bank or political manipulation. It is unlikely that smaller banks would attempt to manipulate electoral outcomes while larger banks would refrain. Smaller banks are less likely to have political connections than larger banks and less able to affect the overall state economy. My results are also inconsistent with the hypothesis that politicians try to manipulate bank lending before elections. Any policies designed to help banks or stimulate the economy would focus on large, influential banks that have a stronger influence on the economy.

In Panel B of Table 17, I sort banks by leverage (defined as the ratio of total loans to total assets) and find that highly leveraged banks reduce lending by a larger amount than low leveraged banks. Banks in the highest quintile of leverage reduce lending by roughly twice as much as banks in the smallest quintile of leverage (1.57% versus .879%) while the top quintile of banks reduce lending by nearly 35% more than the bottom 80% of banks (1.57% versus 1.16%). I also examine stock returns and volatility across leverage quintiles and find that highly leveraged banks experience larger stock declines and larger increases in volatility than low leveraged banks. The stock returns of the most leveraged banks are -.00424 compared to -.00256 for the 80% least leveraged banks, nearly 60% lower. Highly leverage bank volatility is nearly five times greater than the rest of the sample, .0251 versus .00498. Investors clearly view highly leveraged banks as more exposed to political uncertainty than low leveraged banks. Taken

together, these results are consistent with the hypothesis that political uncertainty causes highly leveraged banks to reduce lending more than low leveraged banks.

Having shown that small banks reduce lending more than large banks, I now examine whether or not political uncertainty resolves more quickly for large banks than small banks. Since smaller banks appear more exposed to political risk, it is possible that lending rebounds more slowly. To test this hypothesis, I split banks into small banks (bottom 40% of banks ranked by total assets each quarter) and large banks (top 40% of banks) and run difference-in-difference regressions for quarters Q2-Q5. Results are shown in Panels A and B of Table 17. Small banks reduce bank lending in the 2nd and 3rd quarters by a culminative 3.5% compared to a 1.8% drop for large banks over the same period. Lending rebounds by roughly 1% for small banks in Q4 and Q5 and .8% for large banks. By the end of the next year following gubernatorial elections (Q2-Q8), lending has fully recovered for large banks while remaining significantly lower for small banks. Taken together, these results suggest that political uncertainty has a transitory impact on large banks that resolves within a year, while political uncertainty has a long-lasting impact on small banks that doesn't fully dissipate over time.

Volatility analysis (presented in Panels C and D of Table 17) reinforce the bank lending results: political uncertainty lingers for small banks while quickly resolving for large banks. For small banks, volatility is statistically higher in quarters Q3, Q4, and Q5 of gubernatorial elections. Coefficient estimates are larger for close electionQ4 and close electionQ5 than close electionQ3, which shows that investors remain concerned about political uncertainty several quarters after gubernatorial elections. The heightened market volatility for small bank stocks may contribute (or be a side effect) of reduced bank lending. In Panel D, large bank volatility is

confined to the third quarter of gubernatorial election years and insignificant afterwards, showing that political uncertainty resolves ions for large banks.

Lastly, I examine whether geographically concentrated banks (uni-state banks that only have branches in the treated state) experience a larger drop in bank lending than multistate banks that have branches in multiple states. In columns 3-4 of Table 20, I find that bank lending drops by 1.49% for uni-state banks during Q3 compared to.752% for multistate banks. The stronger results for uni-state banks compared to multistate banks provides additional evidence that political uncertainty causes banks to reduce lending.

The volatility and bank lending results are fully consistent with the hypothesis that political uncertainty causes banks to reduce lending and inconsistent with alternative hypotheses. Stories that allege manipulation by either banks or politicians imply that political uncertainty should immediately resolve after the election, yet neither bank lending nor volatility returns to prelection levels. In addition, either of these alternative stories predict that large banks (which are the more important political actors) should reduce lending. My evidence strongly suggests that political risk is the driver of the bank lending reductions.

14 Supply-Side or Demand-Side?

In this section, I try to rule out the possibility that my results are simply a repackaging of previous findings that corporations reduce bank lending (Jens 2016) due to political uncertainty (demand-side explanation). I use three strategies to test whether my results are a repackaging: first, I examine loan growth across different loan categories to ensure that a reduction in corporate loans (commercial and industry loans, C&I loans) are not driving the results. Second, I rank banks by the ratio of corporate loans to total loans and examine whether or not banks with

high ratios are driving the results. Lastly, I examine non-treated subsidiary banks that are part of treated bank holding companies and find that non-treated subsidiaries also reduce lending. All three tests suggest that a separate supply-side channel exists in addition to the previously documented demand-side channel.

In Table 18, I examine bank lending across four different loan categories: C&I loans (Commercial and Industrial Loans), personal loans, real estate loans, and mortgages. Banks reduce lending in three of the four categories, while banks increase mortgage loans. Personal loans decline by 1.23% (significant with a t-stat of -2.057), real estate loans decline by .597% (significant with t-stat of -1.708), and C&I loans decline by 1.01% (not significant with a t-stat of -1.296). Mortgage loans increase by 3.79% (significant with a t-stat of 1.961), perhaps because banks shift lending from risky loans such as real estate, personal loans, and C&I loans to safer mortgages. Overall, the results show changes in lending is broader than corporate loans.

As an additional check on whether the supply-side channel is separate from a demand-side channel, I rank banks by C&I quintiles and rerun the difference-in-difference regressions with close electionQ3 as the treatment of interest. Bank lending results are presented in Panel A, stock returns in Panel B, and volatility in Panel C. Overall, the results in all panels strongly support the supply-side explanation over the demand-side hypothesis. C&I loans comprise an average of 43.16% of total loans for banks in the highest quintile while C&I loans comprise only 4.73% of loans in the lowest quintile. Generally, the coefficients are stronger for banks with fewer C&I loans. Close_melectionQ3 is an insignificant .309% for banks in the top quintile and a highly significant .864% (t-stat=-8.077) for the lowest quintile. Results are even stronger for banks in the second quintile (-1.2%, t-stat=-3.385) and third quintile (-1.94%, t-stat=2.901).

Overall, the 80% of banks with the lowest ratio of C&I loans to total loans reduce lending by nearly four times banks with the highest ratio.

Stock returns are statistically negative across all quintiles, while the strongest increase in volatility occurs for banks in the lowest quintile of C&I loans. Volatility for banks in the lowest quintile of C&I loans is over three times as large as the highest quintile, while the bottom 80% of banks are roughly 2.5 times larger than the top quintile. gubernatorial election CARs sorted by C&I loan ratios show that market reactions are strong across all quintiles. Banks with zero C&I loans have the strongest market reaction (4.884%, t-stat=6.8365). Taken together, the bank lending and market reaction results demonstrate that the demand-side channel does not subsume the supply-side channel. My findings suggest that the supply-side channel is more important than the demand-side channel for my sample. Banks with few C&I loans reduce lending by a larger amount, have stronger market reactions, and larger increases volatility than banks with many C&I loans.

Lastly, I test whether political uncertainty impacts non-treated bank subsidiaries that are subsidiaries of treated bank holding company (BHC). For example, suppose that a close gubernatorial election is occurring in Georgia while Alabama does not have an election. I examine whether a BHC that is headquartered in Georgia (treated state) reduces lending to its subsidiaries with headquarters in other states without gubernatorial elections. For example, suppose that a BHC headquartered in Georgia also has a bank subsidiary in Alabama. If close elections create political uncertainty for the bank holding company, then it is possible that the BHC will reduce lending for the subsidiary due to the heightened risk. This is exactly what I find: bank subsidiaries reduce lending by 1.18% (column 2) when their BHC is in a treated state and at least 50% of total bank assets are in the treated state. This coefficient is only slightly less

than the roughly 1.4% drop in bank lending for banks in treated states, indicating that there are significant spillover effects from treated BHC to non-treated bank subsidiaries. In column 1, I use the same framework but use all elections instead of close elections. The coefficient is slightly smaller, .808%, but statistically significant.

Showing that non-treated banks are impacted by treated BHCs helps to rule out many alternative stories. First, this finding is only consistent with a supply-side story that political uncertainty causes banks to reduce lending. There is no reason for corporations to reduce borrowing in non-treated states (demand-side story). If a demand-side story was driving the results in the rest of the paper, then I would expect to find a difference between subsidiaries for treated versus treated BHC subsidiaries. In addition, my results also help rule out the political manipulation hypothesis. Politicians have no reason to manipulate bank lending in a state without a gubernatorial election. If political manipulation was driving the results, then I should find no impact for non-treated subsidiaries. Similarly, banks lack an incentive to manipulate lending in states without a gubernatorial election. Overall, my findings strongly support a supply-side political uncertainty channel.

15 Robustness

Although I control for the impact of presidential elections with a dummy variable (pres election*Q3), it is still possible for my results to be biased if the dummy variable doesn't fully control for the impact of presidential elections on bank lending. I run several additional robustness checks to ensure that my results are not confounded by presidential elections. Regardless of specification or controls used, my core results remain unchanged. In column A, I use a difference-in-difference-in-difference specification with pres election*election*Q3 and

pres election*election as additional control variables. In this regression, close election*Q3 is measures the impact of gubernatorial elections in non-presidential election years on bank lending. The coefficient of interest, close election*Q3 is nearly identical to the difference-in-difference estimation.

I also examine whether federal elections impact the results. In column 2, I use federal electionQ3 as a control variable in place of pres electionQ3. Federal election takes a value of 1 if a federal election (either congressional or presidential) occurs during the year and zero otherwise. The treatment effect slightly strengthens to -.0166 and remains highly significant. In columns (3) and (4), I break the sample into presidential and non-presidential elections and compare the coefficients. Close electionQ3 remains highly significant and is very similar to the full sample results (-.0134 with t-stat of -4.139). The drop in bank lending is stronger for gubernatorial elections during presidential election years (-.0163), though the coefficient is not statistically significant (likely due to sample size issues, the presidential election cycle contains only 1/5 as many observations). In column 5, I examine gubernatorial elections that only occur during congressional elections and find that the results are stronger for congressional elections (-.0215) than the full sample. Column 6 presents results for off-year elections (years without a midterm or presidential election). The drop in bank lending is similar to the full sample, -1.83%. The lack of significance for off-year elections reflects the relative scarcity of gubernatorial elections during off-years (less than 15% of gubernatorial elections occur during off-election years). Overall, the drop in bank lending is not confined to any electoral cycle (presidential, congressional, off-year).

As an additional robustness check, I define close election using several different breakpoints and rerun the main regression results. Using breakpoints of the closest one-third of elections, the

closest one-fourth of elections, and the closest one-fifth of elections, I find that the treatment effect is very similar to the breakpoint of 6% used throughout this paper and highly significant across all regression specifications. For comparison, roughly 27.6% of elections are considered close under the 6% margin criteria. The magnitude of the treatment effect seems to slightly increase as the breakpoint becomes more stringent, consistent with closer elections have a stronger impact on bank lending than less close elections.

16 Conclusion of Second Essay

In this paper, I provide the first empirical evidence that political uncertainty impacts bank lending, bank stock returns, and volatility. Using a difference-in-difference framework combined with term limits as an IV, I show that political uncertainty causes banks to reduce lending by roughly 3% in the two quarters before close gubernatorial elections. Using term limits as an IV for close elections significantly strengthens the results, suggesting that OLS estimates are biased by reverse causality between economic weakness and close gubernatorial elections. The stock returns of publicly-traded banks are statistically lower and volatility higher in the quarter before close gubernatorial elections. Bank stocks have an abnormal reaction of roughly 4.5% in a 7-day window around close gubernatorial elections, suggesting that close directly impact bank valuations and providing support for a risk-premia based explanation by Peronesi and Veronesi (2013). The drop in bank lending is larger for subsamples that are more likely to be exposed to political uncertainty: geographically concentrated banks, small banks, and highly leveraged banks.

Bank lending only partially recovers in the quarters following close gubernatorial elections. One year after close elections, roughly bank lending remains roughly 1.4% lower (almost

significant at the 10% level). Analyzing the lending recovery by bank size shows that small banks suffer larger drops and smaller recoveries than big banks. Big bank lending fully recovers within a year after close gubernatorial elections, while small bank lending remains significantly lower. Small bank stock volatility remains higher for several quarters after close gubernatorial elections while big bank volatility returns to normal within one quarter. Overall, these results suggest that political uncertainty only partially resolves following close gubernatorial elections.

My findings are consistent with political uncertainty causing a reduction in bank lending and inconsistent with alternative stories. Political manipulation is an unlikely explanation for my findings for several reasons. First, the sign of my results is wrong: if politicians seek to stimulate the economy to increases the probability of winning an election, then banks should increase bank lending before elections. In addition, any political manipulation should target large banks more than small banks since large banks have a greater ability to influence the economy. However, I find that the largest banks do not reduce lending by a statistically significant amount. Lastly, my findings that BHC reduce lending for subsidiaries in states without elections is inconsistent with political manipulation story. Another alternative explanation, that banks reduce lending in order to manipulate electoral outcomes, is inconsistent with evidence as well. If this story is true, then I should expect banks to increase lending (banks generally support incumbents over uncertain challengers), that big banks would increase lending by a larger amount than small banks, and no reduction in lending should occur for subsidiaries in states without elections. None of these predictions are consistent with my evidence. The likeliest explanation for my results is that political uncertainty causes banks to reduce lending.

To alleviate the concern that my results are a repackaging of previous findings that corporations reduce investment due to political uncertainty (Jens 2016), I use multiple tests to

separate a supply-side explanation (bank lending) from a demand-side explanation (corporations reduce investment). First, I show that the drop in bank lending occurs for multiple categories of loans, including personal loans, real estate loans, and C&I loans; mortgage loans increase. Next, I sort banks by exposure to corporate loans (C&I loans divided by total loans) and find that banks with smaller exposure to C&I loans have a larger reduction in bank lending, greater increases in volatility, and stronger market reactions than banks with smaller exposure to corporate loans. Lastly, I find that bank holding companies headquartered in states with close gubernatorial elections reduce lending to subsidiaries in states without elections, a result that is only consistent with a supply-side channel. Overall, my results show that a demand-side explanation does not subsume a supply-side explanation.

My findings have larger implications for the literature examining how political uncertainty impacts financial markets and the economy. Banks are among the most important economic actors, so any factor that causes banks to reduce lending will have a significant impact on the economy. In this paper, I examine how political uncertainty caused by close gubernatorial elections impacts bank lending. There are more additional sources of political uncertainty that likely impact bank lending and other economic actors, such as presidential elections, referendums such as Brexit, and governmental shutdowns. The United States is a developed country with strong institutions, so findings strong results for a developed country suggest that political uncertainty is an important risk source for banks. Since international banks often operate in more risky political environments where policy changes are likely to be severe than US banks, it is likely that political uncertainty should be more impact in the international context.

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Figures and Tables

Figure 1: Total Houses Sold by Year over Sample Period
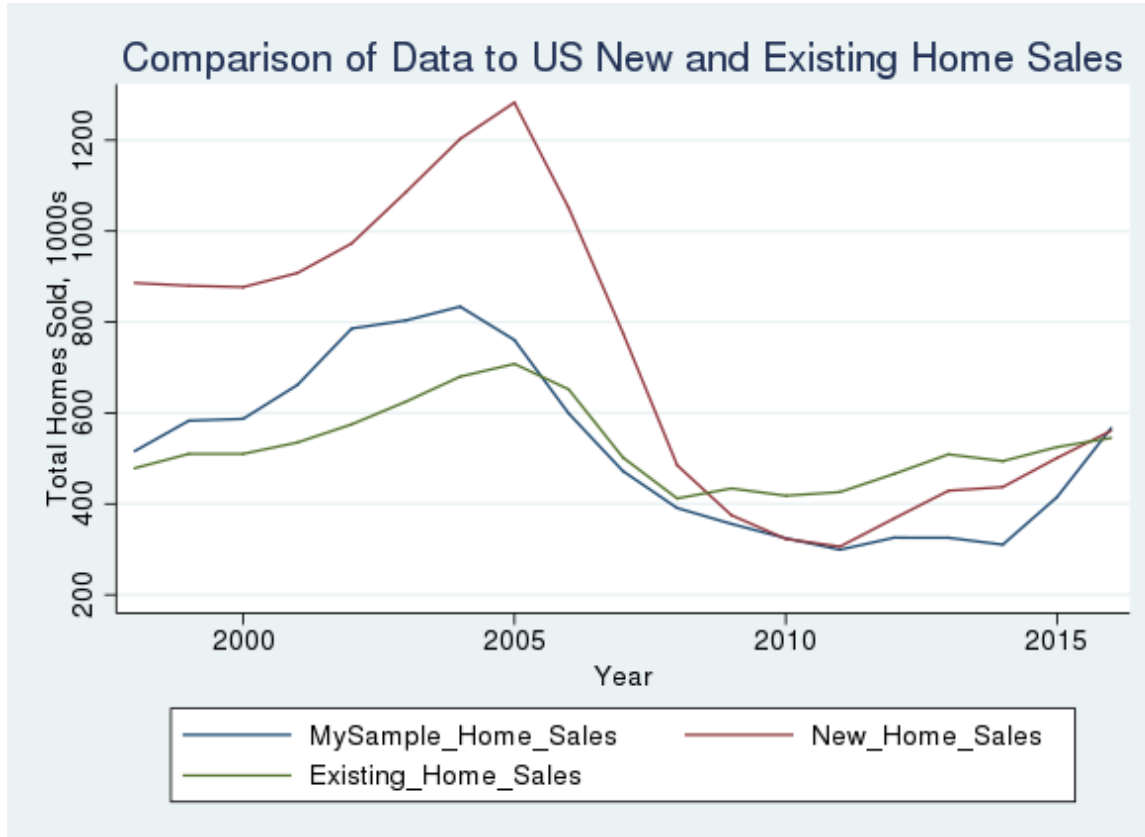
Figure 2: Comparison of My Sample to Overall US Population

Distribution of My Sample

0.0246 – 0.2732
0.0158 – 0.0246
0.0068 – 0.0158
0.0038 – 0.0068
0.0011 – 0.0038
0.0002 – 0.0011

Distribution of US Population

0.031 – 0.121
0.020 – 0.031
0.014 – 0.020
0.009 – 0.014
0.003 – 0.009
0.002 – 0.003
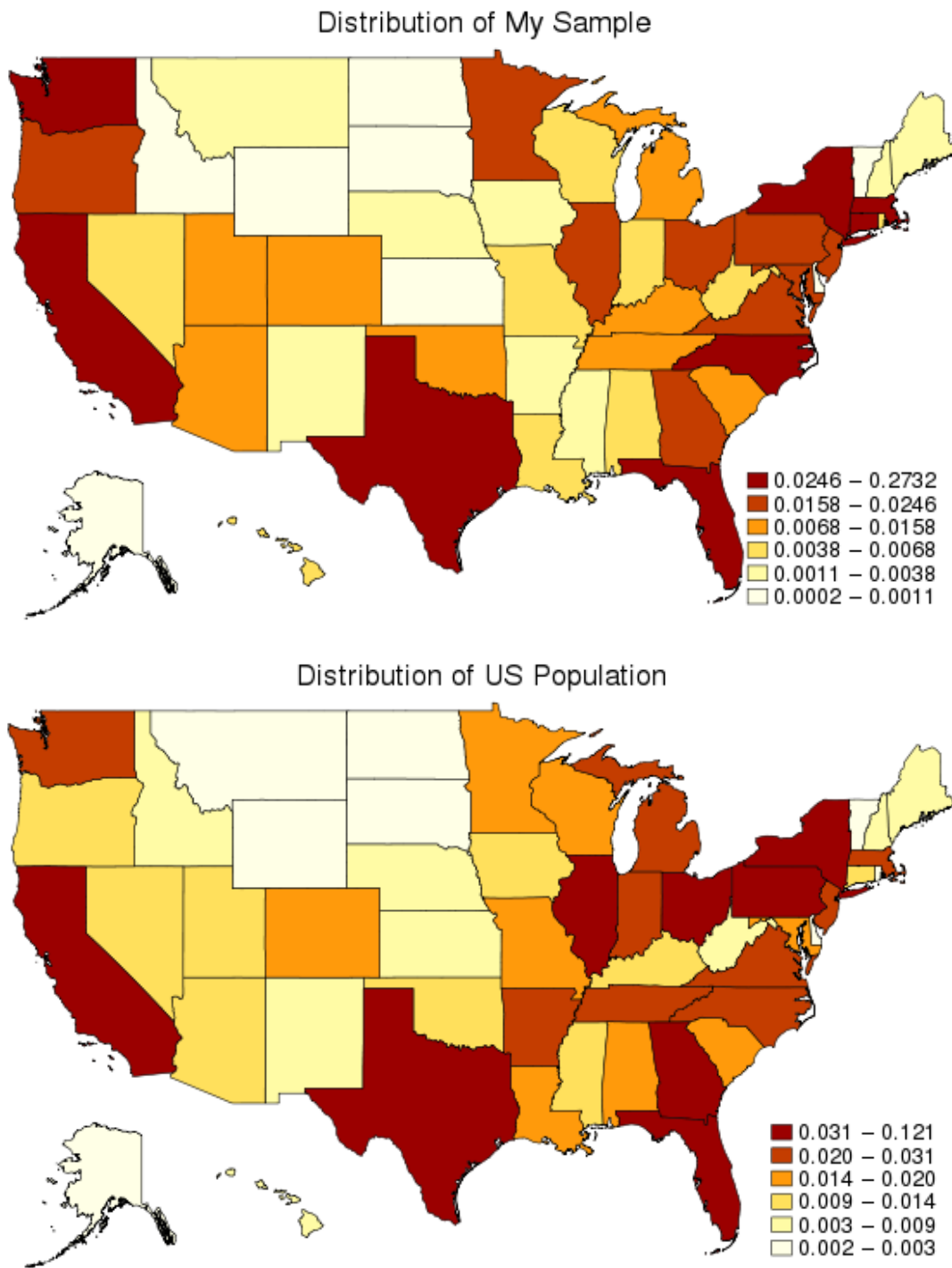
Figure 3: Number of Transactions +/- 5% Nominal House Profit and Zero Point

Histogram of Transactions around Zero Point (+/- 5%)

Histogram of Transactions around Zero Point
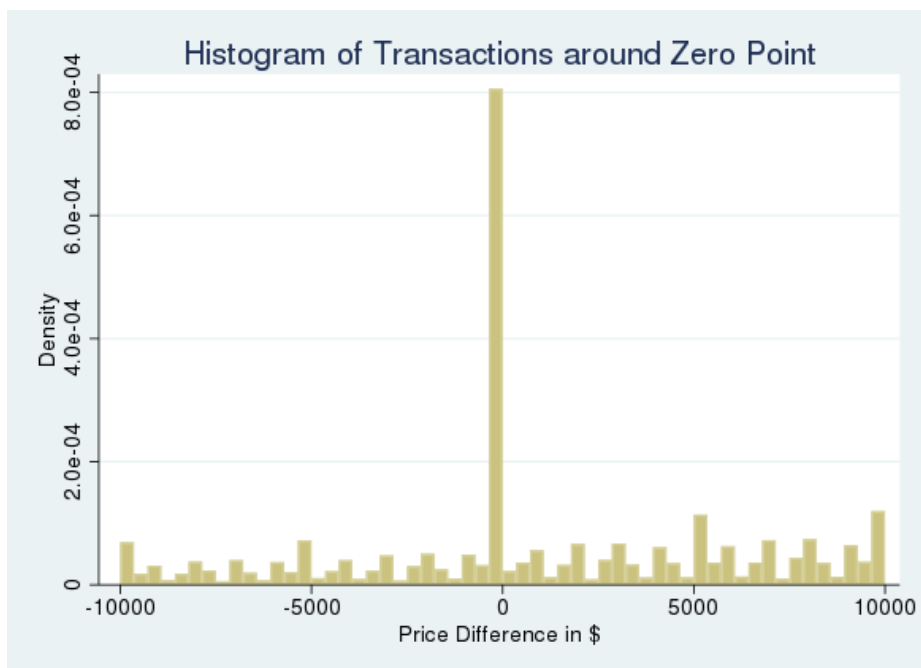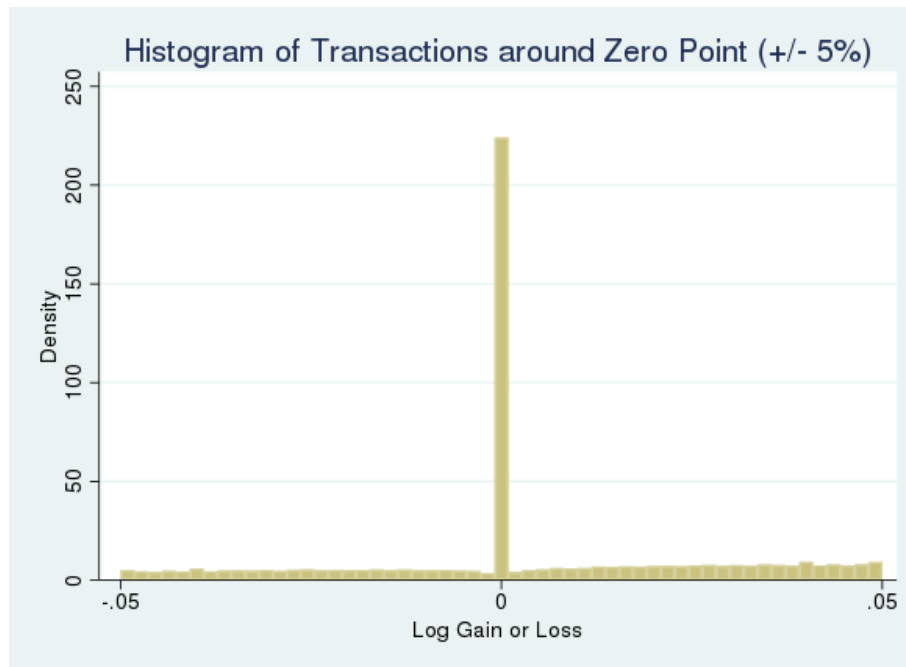
Figure 4: Probability of Selling as a Function of Price (+/- 10%)
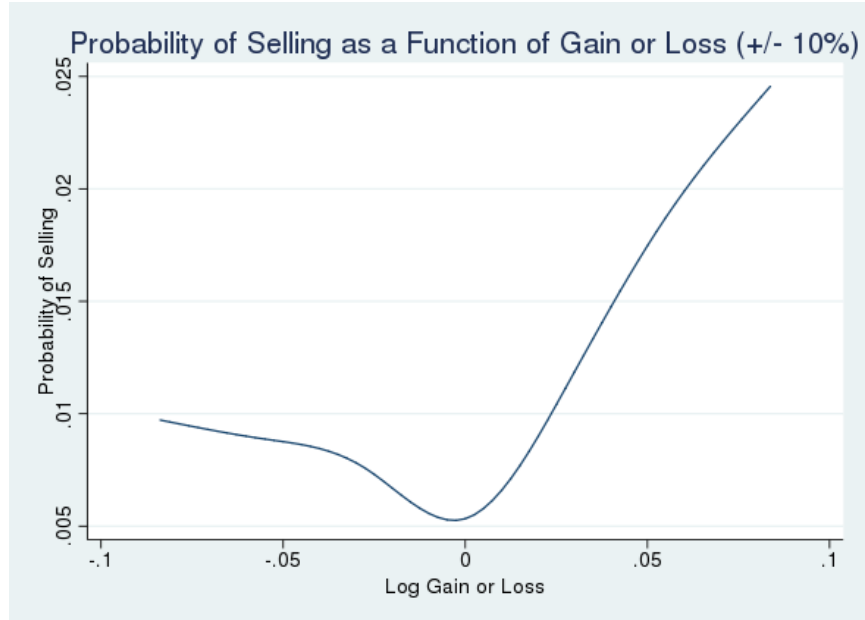
Figure 5: Probability of Selling as a Function of Price (+/- 25%)
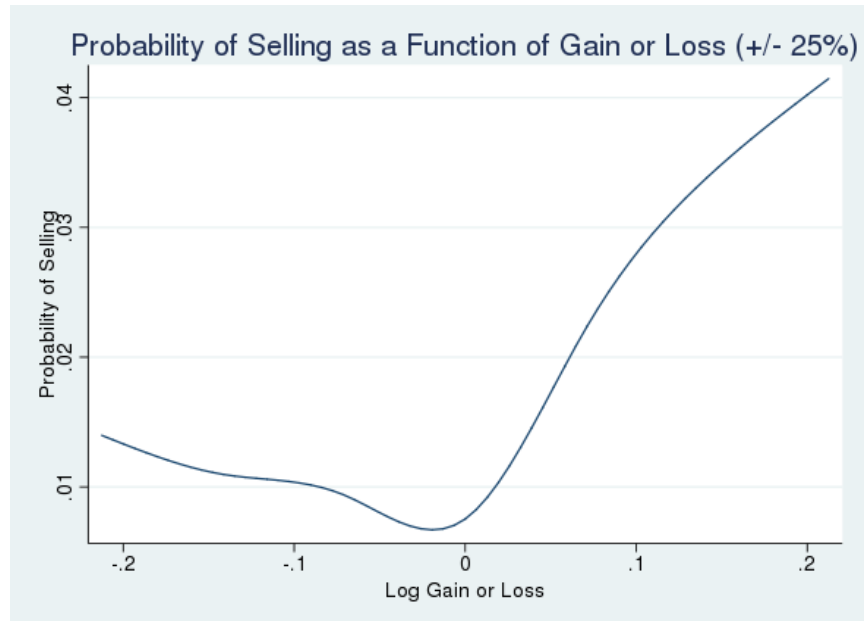
Figure 6: Realization Utility Kink for Most and Least Biased Zips
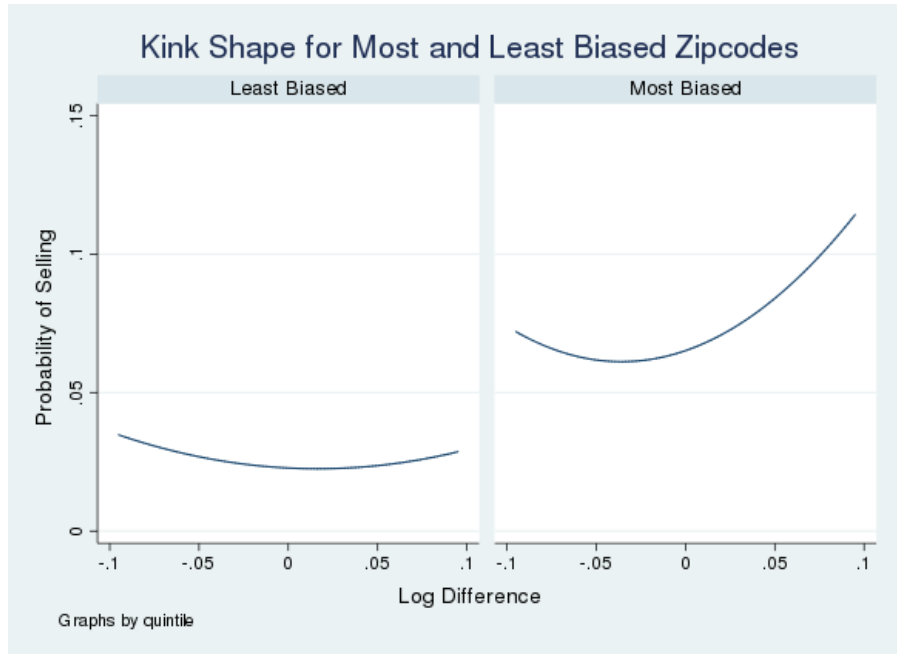
Figure 7: Days between Purchase and Selling Dates for +/- 5%
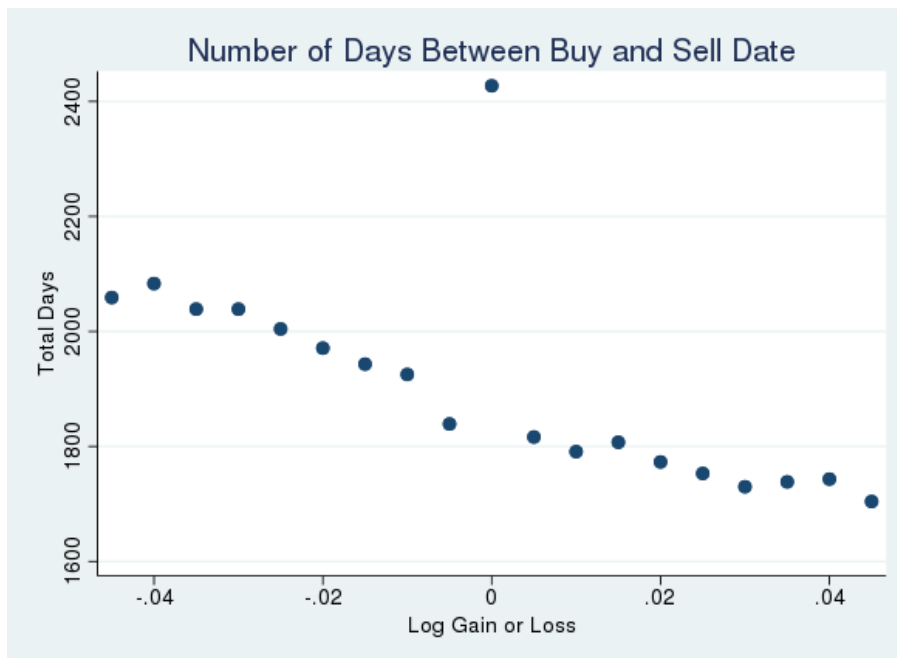
Figure 8: State Map Showing Gubernatorial Term Limits

Gubernatorial Term Limits across States

82
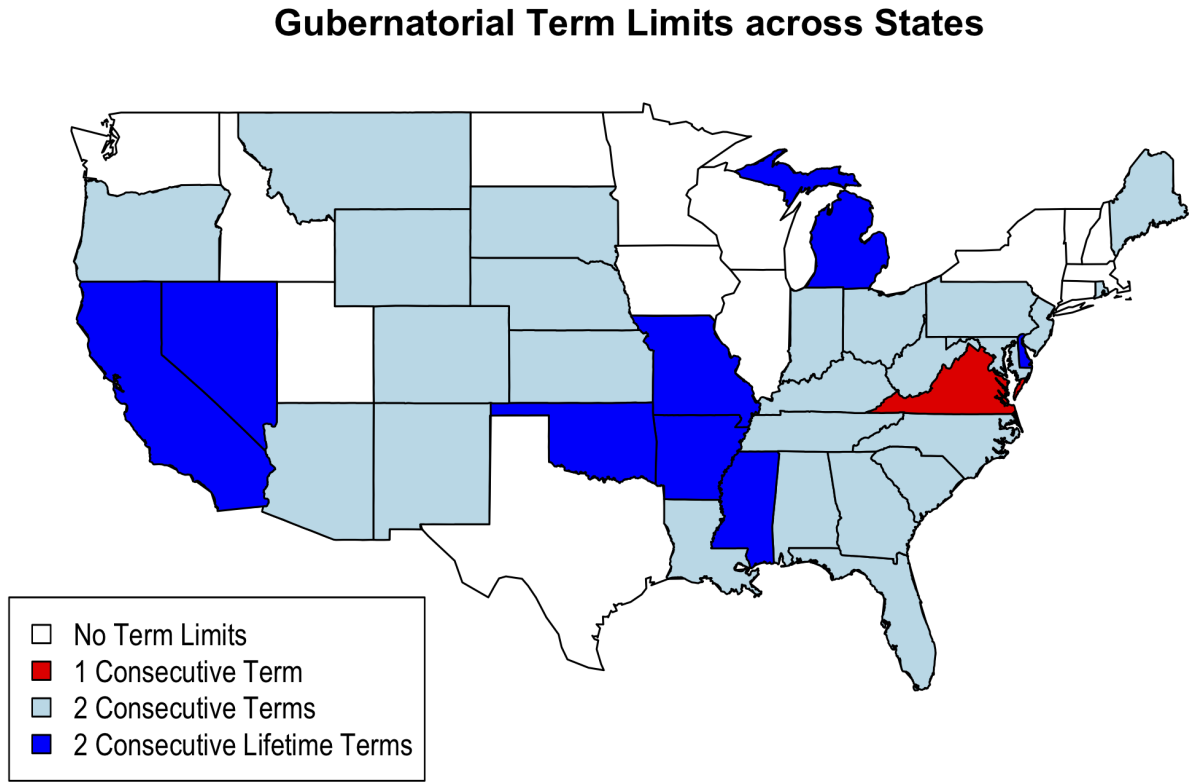
Table I: Sample Summary Statistics

This table presents summary statistics for the dataset used in this paper. Panel A presents information on home characteristics, while

Panel B presents information on homes listed for sale. Panels C and D present aggregate zip code statistics while panel E shows

aggregate home sales by year.

	Panel A: Sample House Characteristics							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	N	p50	mean	sd	p10	p25	p75	p90
Baths	19203761	2	2.189	.918	1	1.75	2.5	3
Beds	18244577	3	3.205	1.005	2	3	4	4
Square Footage	20295637	1656	1878	994.6	984	1244	2272	3052
Built Year	20068264	1979	1974	30.4	1936	1957	1979	2004
Sell Price	4,319,878	204,000	282,305	346949	62,000	120,000	343,000	542,000
Purchase Price	4,316,643	150,000	258,852	1078149	41,500	85,000	260,000	432,000
Gain or loss	4,106,604	.2864	.4674	.9281	-.4420	0	.8943	2.311
Average Tax Assessment	2,017,098	169,000	234,314	364,741	34,400	87,440	295,800	473,860
Months between Buy and Sell	4,323,853	51	70.28	67.19	10	25	93	154

	Panel B: Sample Listing Characteristics							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	N	p50	mean	sd	p10	p25	p75	p90
Average Listing Price	1.,228,295	239,900	365,972	854,282	79,000	139,900	399,900	675,000
Average Sales Price	764,737	217,300	304,370	460,253	72,000	130,000	355,000	580,000
Average Price Reduction	764,737	-0.0526	-0.107	0.723	-0.397	-0.170	0	0.0398
Time until Sale	764,737	114	273.1	442.9	20	51	276	685

Panel C: Aggregate Zip Code Statistics

VARIABLES	(1) N	(2) p50	(3) mean	(4) sd	(5) p10	(6) p25	(7) p75	(8) p90
Average Quarterly Return	430,760	0.00922	0.00868	0.0279	-0.023	-0.0064	0.0247	0.0410
Average Yearly Return	411,450	0.0358	0.0347	0.0916	-0.071	-0.016	0.0884	0.144
Houses Sold Year	417,765	0.176	0.249	0.328	0	0.0442	0.301	0.558
Percent Underwater	420,607	0.0856	0.252	0.306	0	0	0.463	0.737
Houses Sold Quarter	420,309	0.0308	0.0740	0.154	0	0	0.0750	0.167

Panel D: Aggregate Boom and Bust Statistics

VARIABLES	(1) N	(2) p50	(3) mean	(4) sd	(5) p10	(6) p25	(7) p75	(8) p90
Boom Length	9,968	17	18.18	6.511	12	14	21	30
Bust Length	9,968	20	15.44	12.66	-10	13	23	26
Peak Cumulative Ret	8,738	0.446	0.491	0.302	0.140	0.248	0.679	0.904
Bust Cumulative Drop	9,067	-0.253	-0.277	0.149	-0.501	-0.367	-0.160	-0.102

Panel E: Yearly Housing Transactions

Year	Total Sales	% Gain	Year	Total Sales	% Gain	Year	Total Sales	% Gain
1998	515945	0.747	2004	833995	0.909	2010	324345	0.507
1999	583016	0.809	2005	760505	0.920	2011	299063	0.484
2000	587018	0.856	2006	598942	0.905	2012	325672	0.526
2001	661744	0.878	2007	472126	0.838	2013	325401	0.621
2002	785798	0.886	2008	390876	0.601	2014	310247	0.664
2003	803586	0.883	2009	355985	0.484	2015	414864	0.684

Table 2: Regression Discontinuity and Kink around Zero-Profit Point

Table 2 presents regression discontinuity and regression kink results using a variety of cut points and polynomial specifications.

Dependent variable is a binary variable taking the value of 0 (no sell) or 1 (sell). Log diff is the log difference between the fair market

value of the house and the purchase price of the house. Robust t-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

| VARIABLES | Probability of Selling | | | | |
	Regression Discontinuity			Regression Kink	
Nonnegative Dummy	0.659***	0.397***		0.955***	0.971***
	(449.0)	(421.6)		(1,233)	(1,247)
Zero-Profit Point			0.504***		
			(457.2)		
Log Diff	1.690**	-0.838***	-0.419	-0.121***	-0.182***
	(2.346)	(-6.234)	(-1.613)	(-13.13)	(-19.62)
Log Diff*Nonnegative	-314.6***	-57.01***	-118.2***	0.264***	0.352***
	(-362.5)	(-335.7)	(-367.9)	(16.84)	(22.20)
Loan-to-Value Ratio	4.80e-09***	2.51e-09***	2.86e-09***	3.19e-09***	1.81e-09***
	(7.496)	(4.711)	(5.521)	(7.069)	(3.966)
Months_Bet	0.00125***	0.00132***	0.00125***	0.000997***	0.000923***
	(179.8)	(256.3)	(248.6)	(227.7)	(218.4)
Constant	-0.0146***	-0.0204***	-0.0169***	-0.00433***	0.0286***
	(-11.19)	(-26.31)	(-18.18)	(-16.28)	(126.7)
Polynomial Order	4	3	4	1	1
Cutoff Point	2.5%	5%	5%	2.5%	5.0%
Observations	1,291,687	2,488,896	2,488,896	2,488,896	2,488,896
R-squared	0.404	0.178	0.223	0.410	0.398

Table 3: Zero-Profit Point (Break-Even) Analysis

Table 3 examines whether homeowner listing behavior is different for homes initially listed at the zero-profit point (i.e., break-even) compared to homes not listed at the zero-profit point.

VARIABLES	Amount Price is Reduced		Drop Probability		Time until Sale	
Zero-Profit Point	-0.0490***	-0.0395***	0.179***	0.185***	111.1***	116.4***
	(-4.333)	(-3.470)	(29.36)	(30.35)	(12.15)	(12.73)
Log Diff		-0.254***		-0.00182**		11.78***
		(-99.55)		(-2.488)		(11.99)
Log Diff Square		0.0759***		0.00692***		2.986***
		(88.38)		(36.69)		(12.09)
Log Diff Cubed		-0.00188***		-7.19e-05***		-0.147***
		(-58.50)		(-6.787)		(-14.01)
Log Diff Fourth		-0.000227***		-4.32e-05***		-0.0185***
		(-32.39)		(-26.89)		(-10.18)
Constant	-0.107***	-0.115***	0.383***	0.376***	272.5***	266.9***
	(-1,905)	(-190.5)	(8,765)	(1,416)	(6,013)	(1,024)
Observations	764,737	764,737	1,228,107	1,228,107	764,737	764,737
R-squared	0.000	0.440	0.001	0.053	0.000	0.027
Zip Code Year FE	YES	YES	YES	YES	YES	YES
Number of FE	59,885	59,885	75,693	75,693	59,885	59,885
Clustering	Zip code	Zip code	Zip code	Zip code	Zip code	Zip code

Table 4: Prospect Theory Regressions

Table 4 examines whether homeowner decisions are consistent with prospect theory preferences. The dependent variable is the log selling price of the home while the main variables of interest are log Loss and log Gain.

VARIABLES	Log Selling Price				
	Full Sample	Full Model	Robust Model	Close Tax Assessments	
Log Assessed Tax Value	0.706***	0.734***	0.733***	0.967***	0.969***
	(150.6)	(120.3)	(120.5)	(61.93)	(63.85)
Log Past Model Error			0.00414		-0.0119*
			(1.383)		(-1.905)
Log Loss	0.341***	0.386***	0.382***	0.396***	0.406***
	(33.54)	(37.03)	(37.97)	(6.161)	(6.528)
Log Gain	-0.0532***	-0.0657***	-0.0616***	-0.0384***	-0.0502***
	(-15.05)	(-10.66)	(-9.282)	(-3.542)	(-3.837)
Loan-to-Value Ratio	6.85e-06***	4.23e-06***	4.24e-06***	5.36e-05***	5.29e-05***
	(3.516)	(2.986)	(2.977)	(10.70)	(10.89)
Months_bet	0.000970***	-0.00253***	-0.00255***	-0.00134***	-0.00131***
	(39.44)	(-37.72)	(-36.80)	(-6.572)	(-6.187)
Constant	3.729***	3.384***	3.392***	0.280	0.253
	(66.01)	(46.26)	(46.56)	(1.387)	(1.289)
Observations	2,016,599	686,953	686,953	70,812	70,812
R-squared	0.245	0.259	0.259	0.399	0.399
Zip Code Year FE	YES	YES	YES	YES	YES
Number of FE	69,260	52,746	52,746	7,908	7,908
Clustering	Zip code	Zip code	Zip code	Zip code	Zip code

Table 5: Zip Code Subset Regressions

Panel A presents regression kink results using a linear specification and +/- 5% cut point. Dependent variable is an indicator variable taking the value of 0 (no sell) or 1 (sell) for the quarter. Zip codes are independently sorted into quintiles based on the percentage of the zip code with a high school degree, median household income in 2011, or Odd_10 (the percentage of homes with the last digit ending in zero). Zip codes with fewer high school graduates (Q1), lower median incomes (Q1), and more odd pricing (Q5) are more biased.

	Panel A: Realization Utility Regressions (Kink Regressions)					
	(1) HighSch Q1	(2) HighSch Q5	(3) Income Q1	(4) Income Q5	(5) Odd_10 Q1	(6) Odd_10 Q5
VARIABLES						
Log Diff	-0.244***	-0.0981	-0.0940	-0.134***	-0.0360**	-0.190***
	(0.0310)	(0.0620)	(0.0608)	(0.0139)	(0.0184)	(0.0238)
Zero-Profit Point	0.950***	0.955***	0.965***	0.948***	0.959***	0.950***
	(0.00312)	(0.00522)	(0.00517)	(0.00108)	(0.00106)	(0.00211)
Log Diff*Nonnegative	0.430***	0.227**	0.461***	0.195***	0.00671	0.481***
	(0.0527)	(0.108)	(0.107)	(0.0235)	(0.0310)	(0.0410)
Loan-to-Value Ratio	6.35e-09***	5.02e-08	2.30e-07***	4.11e-09***	4.47e-08***	2.12e-09***
	(1.38e-09)	(3.97e-08)	(1.91e-08)	(5.95e-10)	(1.08e-08)	(5.78e-10)
Months_bet	0.00111***	0.000986***	0.000892***	0.00107***	0.000815***	0.00113***
	(1.47e-05)	(3.03e-05)	(3.05e-05)	(6.49e-06)	(8.73e-06)	(1.12e-05)
Constant	-0.00242***	-0.00407*	-0.00892***	-0.00222***	-0.00298***	-0.000324
	(0.000893)	(0.00217)	(0.00187)	(0.000399)	(0.000613)	(0.000695)
Observations	283,157	54,680	47,580	1,235,568	477,023	504,005
R-squared	0.269	0.409	0.450	0.421	0.681	0.311

Panel B tests whether realization utility and prospect theory preferences vary across zip codes based on Kink1 (small gains + zeros)/

(small losses) and Odd_10 (percentage of homes sold with a non-zero last digit). The dependent variable is the log selling price of the

home while the main variables of interest are log loss and log gain.

VARIABLES	Panel B: Prospect Theory Behavioral Regressions: Odd_10 and Kink1_Ratio				
	Odd_10 Q1	Odd_10 Q5	Kink1 Q1	Kink1 Q5
Log Assessed Tax Value	0.723***	0.711***	0.671***	0.750***
	(77.55)	(70.93)	(83.05)	(99.63)
Log LOSS	0.282***	0.458***	0.212***	0.459***
	(18.31)	(27.18)	(11.85)	(29.46)
Log GAIN	-0.0696***	-0.0610***	-0.0603***	-0.0848***
	(-5.075)	(-11.50)	(-3.968)	(-17.13)
Loan-to-Value Ratio	2.13e-05***	3.86e-06**	1.30e-05**	5.99e-06***
	(4.638)	(2.472)	(2.419)	(3.486)
Months_Bet	0.000656***	0.000872***	0.00117***	0.000296***
	(9.434)	(19.05)	(17.86)	(6.552)
Constant	3.485***	3.672***	4.091***	3.227***
	(30.94)	(31.58)	(41.62)	(36.13)
Observations	437,461	390,938	420,759	405,013
R-squared	0.280	0.240	0.226	0.300
Zip Code Year FE	YES	YES	YES	YES
Number of Zip code-year FE	6,696	28,391	16,811	14,694
Clustering	Zip code	Zip code	Zip code	Zip code

Table 6: Zero-Profit Point Analysis by Zip Code

Panel A examines whether homes sold at the zero-profit point differ from other homes for zip codes sorted by kink1_rank (small gains + zeros) / small losses. Q1 zip codes have the weakest disposition effect around the zero point while Q5 zip codes have the strongest disposition effect.

	Panel A: Zero-Profit Point Analysis					
VARIABLES	Drop Probability Q1	Drop Probability Q5	Amount Price is Reduced Q1	Amount Price is Reduced Q5	Time until Sale Q1	Time until Sale Q5
Zero-Profit Point	0.211***	0.191***	0.000108	-0.0954**	123.3***	129.8***
	(15.13)	(11.06)	(0.00419)	(-2.338)	(6.399)	(4.448)
Log Diff	0.0117***	-0.00964***	-0.250***	-0.247***	25.43***	7.275***
	(5.496)	(-4.385)	(-45.84)	(-34.29)	(10.97)	(2.626)
Log Diff Square	0.00852***	0.00661***	0.0780***	0.0825***	3.576***	2.799***
	(14.23)	(11.42)	(39.84)	(35.32)	(5.718)	(3.642)
Log Diff Cubed	-0.000133***	-0.000144**	-0.00182***	-0.00261***	-0.225***	-0.231***
	(-4.515)	(-2.497)	(-23.72)	(-14.30)	(-8.722)	(-5.165)
Log Diff Fourth	-5.33e-05***	-5.44e-05***	-0.000247***	-0.000325***	-0.0233***	-0.0277***
	(-10.95)	(-7.423)	(-15.50)	(-12.75)	(-5.131)	(-3.964)
Constant	0.291***	0.425***	-0.140***	-0.112***	231.0***	284.9***
	(475.5)	(462.6)	(-114.1)	(-50.77)	(481.2)	(330.1)
Observations	213,183	152,686	150,128	88,242	150,128	88,242
R-squared	0.006	0.005	0.522	0.349	0.004	0.001
Zip Code Year FE	YES	YES	YES	YES	YES	YES
Number of FE	7,127	7,076	6,875	6,669	6,875	6,669

Panel B examines whether homes sold at the zero-profit point differ from other homes for zip codes sorted by kink1_rank or kink2_rank.

Q1 zip codes have the weakest disposition effect around the zero point while Q5 zip codes have the strongest disposition effect.

	(1) Drop Probability	(2) Amount Price is Reduced	(3) Time until Sale	(4) Drop Probability	(5) Amount Price is Reduced	(6) Time until Sale
Panel B: Zero-Profit Point Analysis: Drop Prob, Amt Reduce, and Time Market (kink1_rank & kink2_rank)						
VARIABLES						
Kink1 Rank	0.0268***	-0.00866*	21.11***			
	(6.000)	(-2.104)	(6.100)			
Kink2 Rank				0.0295***	-0.0153***	22.53***
				(10.69)	(-3.378)	(6.839)
Zero-Profit Point	0.228***	-0.0528***	141.7***	0.229***	-0.0527***	141.5***
	(10.20)	(-3.618)	(4.919)	(10.15)	(-3.604)	(4.914)
Log Diff	-0.00161	-0.246***	13.88*	-0.00203	-0.245***	13.63*
	(-0.262)	(-20.99)	(2.051)	(-0.329)	(-21.17)	(2.009)
Log Diff Square	0.00862***	0.0719***	4.321***	0.00852***	0.0720***	4.236***
	(7.891)	(20.34)	(3.649)	(7.754)	(20.51)	(3.585)
Log Diff Cube	-6.89e-05	-0.00197***	-0.153*	-6.69e-05	-0.00197***	-0.152*
	(-0.964)	(-13.19)	(-1.929)	(-0.933)	(-13.25)	(-1.912)
Log Diff 4th	-5.40e-05***	-0.00020***	-0.0267***	-0.00053***	-0.00020***	-0.0262***
	(-6.813)	(-7.866)	(-3.571)	(-6.715)	(-7.939)	(-3.503)
Constant	0.290***	-0.187***	312.7***	0.277***	-0.172***	302.6***
	(3.927)	(-6.507)	(13.22)	(3.863)	(-5.801)	(11.99)
Observations	1,043,785	683,734	683,734	1,042,384	683,115	683,115
R-squared	0.017	0.429	0.015	0.018	0.429	0.015
Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Clustering	Year	Year	Year	Year	Year	Year

Table 7: Percentage of Homes Below Purchase Price and Future Housing Sales

Table 7 examines whether the percentage of homes below purchase price in a zip code predicts the percentage of homes sold in the next quarter or year.

	Relationship between % Homes Below Purchase Price and Future Housing Sales			
VARIABLES	Next Quarter % Houses Sold	Next Year % Houses Sold	Next Year % Houses Sold (Weakest DE)	Next Year % Houses Sold (Strongest DE)
% Below Purchase Price	-0.00580***	-0.0246***	0.00366	-0.0391***
	(-14.70)	(-5.722)	(0.570)	(-3.843)
Constant	0.0102***	0.0477***	0.0314	0.0475***
	(158.0)	(16.69)	(1.228)	(9.528)
Observations	413,984	396,203	40,767	68,974
R-squared	0.312	0.569	0.290	0.628
Controls	YES	YES	YES	YES
Zip code Fixed Effects	YES	YES	YES	YES
Year-Qtr Fixed Effects	YES	YES	YES	YES
Number of Clusters	6,286	6,253	718	999

In panel B, I match zip codes based on education and income. For each zip code in the quintile with the strongest disposition effect, I find a matching zip code in the quintile with the weakest disposition effect that is closest in terms of education or income. After running separate regressions for each treated and control zip code (regressions are the same as in Panel A), I subtract the coefficient estimate for the control zip code from the treated zip code. Finally, I get the average difference across all zip code combinations and report it.

Bootstrapped standard errors are used.

Matched Coefficient Difference

VARIABLES	Next Year % Houses Sold					
Coefficient Difference	-.03526***	-.02850***	-.03982***	-.04507***	-.03294***	-.03136***
	(.001)	(.001)	(.001)	(.001)	(.003)	(.001)
Matching Variable	Income	Income	Education	Education	Inc & Edu	Inc & Edu
Number of Matches	1	10	1	10	1	10
Observations	2,178	21,926	2,157	22,273	689	6,573

Table 8: Percentage of Homes Below Purchase and Future Price Changes

Table 8 examines whether the percentage of homes below purchase price in a zip code predicts price appreciation in the next quarter or year.

	Relationship between % Houses Below Purchase Price and Future Price Changes			
VARIABLES	Next Quarter Price Change	Next Year Price Change	Next Year Price Change Q1 (Weakest DE)	Next Year Price Change Q5 (Strongest DE)
% Below Purchase Price	-.670***	-1.117***	.07936	-1.543***
	(-24.78)	(-11.40)	(0.320)	(-6.269)
Constant	1.134***	3.716***	2.195***	3.921***
	(56.58)	(43.54)	(5.186)	(19.30)
Observations	414,631	396,502	40,848	68,998
R-squared	0.329	0.478	0.421	0.453
Controls	YES	YES	YES	YES
Zip Code Fixed Effects	YES	YES	YES	YES
Year-Qtr Fixed Effects	YES	YES	YES	YES
Number of Zip Code Clusters	6,292	6,259	719	999

In panel B, I match zip codes based on education and income. For each zip code in the quintile with the strongest disposition effect, I find a matching zip code in the quintile with the weakest disposition effect that is closest in terms of education or income. After running separate regressions for each treated and control zip code (regressions are the same as in Panel A), I subtract the coefficient estimate for the control zip code from the treated zip code. Finally, I get the average difference across all zip code combinations and report it.

Bootstrapped standard errors are used. Robust t-statistics in parentheses.

	Matched Coefficient Difference (Prices)					
	Next Year % Price Change					
VARIABLES						
Coefficient Difference	-2.231***	-2.459***	-3.518***	-2.714***	-3.518***	-2.714***
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Matching Variable	Income	Income	Education	Education	Inc & Edu	Inc & Edu
Number of Matches	1	10	1	10	1	10
Observations	582	5,638	550	5,739	199	1,879

Table 9: Housing Boom and Bust Results

Panel A examines whether zip codes with the strongest disposition effect performed differently than zip codes with the weakest disposition effect during the recent housing boom and bust. Kink1 Rank takes a value between {1,5} with 1 representing zip codes with the weakest disposition effect and 5 representing zip codes with the strongest disposition effect. PeakRet is the cumulative total price increase from the starting date of December 2001 to the peak price obtained during the housing boom. PeakTime is the number of quarters between the starting date of December 2001 to the peak price date. Similarity, BotDrop is the cumulative total price drop from the peak of the boom to the bottom of the bust. Heteroskedasticity robust standard errors are used. In panel B, I match zip codes based on education, income, or runup during housing boom. For each zip code in the top quintile, I find a matching zip code in the lowest quintile that is closest for the given variable. After subtracting the difference for each treated-control pair, I get the average differences and report it.

Panel A: Housing Boom and Bust Tests			
VARIABLES	PeakRet (%)	PeakTime (Qtrs)	BotDrop (%)
Kink1 Rank	-2.441***	0.942***	-2.347***
	(-5.228)	(10.58)	(11.06)
Constant	138.1***	20.49***	67.42***
	(16.29)	(15.18)	(-18.13)
Controls	YES	YES	YES
Observations	2,825	2,988	2,847
R-squared	0.260	0.132	0.405

Panel B: Matched Coefficient Difference

VARIABLES	PeakRet (%)		PeakTime (Qtrs)		BotDrop (%)		
Coefficient Difference	-10.44***	-10.1***	3.686***	3.342***	-7.983***	-7.342***	-9.221***
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Matching Variable	Income	Education	Income	Education	Income	Education	Runup
Observations	2,098	2,129	2,720	2,708	2,283	2,310	2,225

Table 10: State-Level Summary Statistics

Summary statistics for state-level variables for the sample of 1,600 state-year observations from 1985-2017. Quarterly observations (macroeconomic variables) are annualized. GDPΔ is from the Bureau of Economic Analysis (bea.gov), unemployment (Unemply) is from the Bureau of Labor Statistics (bls.gov), election data is from Congressional Quarterly Press (CQ Press) and term limit data is hand-collected.

Panel A: State-Level Summary Statistics for All Observations

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	median	mean	sd	min	max
Unemply	1,600	5.4	5.695	1.895	2.200	13.90
GDPΔ	1,600	4.9	4.926	3.531	-13.8	24.50
close	1,600	0	0.0725	0.259	0	1
election	1,600	0	0.263	0.440	0	1
recession	1600	0	0.089	0.284	0	1

Panel B: State-Level Summary Statistics for All Elections

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	median	mean	sd	min	max
Unemply	420	5.4	5.648	1.923	2.400	13.70
GDPΔ	420	5	4.975	3.676	-13.8	24.50
termlimit	420	0	0.233	0.423	0	1
margin	420	13.45	18.01	16.45	0	100
close	420	0	0.276	0.448	0	1
recession	420	0	0.081	0.273	0	1

Panel C: State-Level Summary Statistics for Close Elections

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	median	mean	sd	min	max
Unemply	116	5.65	5.809	1.870	2.400	11.10
GDPΔ	116	4.25	4.131	3.887	-13.8	11
termlimit	116	0	0.319	0.468	0	1
margin	116	3.32	3.187	1.744	0	5.980
recession	116	0	0.052	0.222	0	1

Panel D: State-Level Summary Statistics for Non-Close Elections						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	median	mean	sd	min	max
Unemply	304	5.4	5.587	1.943	2.400	13.70
GDPΔ	304	5.2	5.298	3.546	-12.80	24.50
termlimit	304	0	0.201	0.401	0	1
margin	304	18.68	23.67	16.03	6.060	100
recession	304	0	0.092	0.289	0	1

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Table 11: Bank-Level Summary Statistics

Quarterly summary statistics for bank-level variables from 1985-2017. Bank variables (size, lev_rat, profit, roe, loss) are constructed from Bank Regulatory data, GDPΔ is from the Bureau of Economic Analysis (bea.gov), unemployment (Unemply) is from the Bureau of Labor Statistics (bls.gov), election data is from Congressional Quarterly Press (CQ Press) and term limit data is hand-collected.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	median	mean	sd	min	max
ΔTotal Loans	1,188,275	0.0150	0.0228	0.106	-0.1992	0.4147
Unemply	1,293,810	5.5	5.811	1.821	2.200	14.60
GDPΔ	1,293,810	5.2	5.044	3.004	-26.60	24.50
recession	1,293,810	0	0.0782	0.269	0	1
size	1,213,022	11.186	11.35	1.484	0	21.41
lev_rat	1,166,899	0.0914	0.0840	9.966	-9,862	1
profit	1,161,671	0.0053	0.0169	10.39	-50.84	10,822
roe	1,161,634	0.0552	0.0443	4.344	-1,812	3,062
loss	1,161,398	0.0008	-0.00785	11.15	-12,018	20.59
Preselection	1,293,810	0	0.240	0.427	0	1
close	1,293,810	0	0.0711	0.257	0	1
election	1,293,810	0	0.254	0.435	0	1
termlimit	1,293,810	0	0.0515	0.227	0	1

Table 12: Change in Bank Lending in the Quarter Before Gubernatorial Elections

Regression results from a difference-in-difference estimation from 1985-2017 with either election*Q3 (column 1) or close election*Q3 (columns 2-6) as the treatment variable. The dependent variable is the quarterly percentage change in total bank loans and close election takes a value of 1 if the difference between the two top candidates is less than 6%. For IV regressions (columns 3-6), Fitted close*Q3 is the predicted values from a first-stage regression of close on termlimit and controls. Column 5 only uses border counties and column 6 only uses border MSAs.

VARIABLES	Quarterly Percent Change in Total Loans					
	All Elections	Close Elections	Close IV	Close IV	Border Counties	Border MSA
Election*Q3	-0.000905					
	(-0.687)					
Close Election*Q3		-0.00429**				
		(-2.589)				
Fitted Close*Q3			-0.016***	-0.0143***	-0.0115***	-0.0221**
			(-18.14)	(-4.896)	(-3.64)	(-2.13)
Election	-0.000397	-0.000322	0.000352	0.000378*	-.00062	-0.000568
	(-0.999)	(-0.835)	(0.472)	(1.720)	(-0.60)	(-0.43)
Q3	0.00383**	0.00389**	0.0047**	0.00462**	.00485***	0.00352**
	(2.139)	(2.159)	(2.577)	(2.465)	(3.96)	(2.33)
Pres Election*Q3	0.00214	0.00213	0.00137	0.00178	0. 0022	0.00163
	(1.368)	(1.358)	(0.873)	(1.130)	(1.08)	(0.65)
Size	-0.00109	-0.00109		-0.00109	-0.0006	-0.0037***
	(-1.462)	(-1.438)		(-1.463)	(-.82)	(-2.83)
Lev_ratio	0.126**	0.126**		0.126**	0.0491	-0.00468
	(2.471)	(2.471)		(2.471)	(1.06)	(-0.07)
Profit	-0.181*	-0.181*		-0.181*	-0.2174***	-0.204**
	(-1.998)	(-1.999)		(-2.000)	(-3.50)	(-2.39)
Roe	6.24e-05	6.23e-05		6.22e-05	0.00075	5.45e-05*
	(1.023)	(0.904)		(1.054)	(1.43)	(1.78)
Loss	-0.674***	-0.674***		-0.674***	-0.7165***	-0.298***
	(-3.583)	(-3.585)		(-3.587)	(-4.15)	(-3.56)
Recession	0.00686*	0.00713*		0.0081***	0.00417***	0.00680**

	(1)	(2)	(3)	(4)	(5)	(6)
	(1.954)	(2.004)		(2.794)	(1.24)	(2.183)
Unemply	-0.00634***	-0.00635***		-0.006***	-0.0047***	-0.003***
	(-6.107)	(-6.068)		(-6.230)	(-.93)	(-3.79)
ΔGDP	0.00071**	0.000699**		0.00068**	0.00206***	0.000128
	(2.512)	(2.487)		(2.510)	(6.00)	(0.253)
Observations	970,489	970,489	1,011,029	970,489	367,747	108,288
R-squared	0.032	0.032	0.011	0.032	0.033	0.0542
FE	State, Yr	State, Yr	State, Yr	State, Yr	CountyPair* Yr	MSA*Yr
IV	NO	NO	YES	YES	YES	YES

Panel B: Matching Results

	Border County		Border MSA	
VARIABLES	Quarterly Percent Change in Total Loans			
Difference	-.00987***	.-0089***	-.01093***	-.1478***
	(-3.061)	(-6.44)	(27.8158)	(-3.993)
Observations	1,984	9,836	455	2,058
Number Matches	1	5	1	5

Table 13: Bank Stock Returns and Volatility Before Gubernatorial Elections

Regression results from a difference-in-difference estimation from 1985-2017 with either election*Q3 (columns 1 and 4) or close election*Q3 (columns 2-3,5-6) as the treatment variable. The dependent variable is quarterly stock returns in columns (1-3) and quarterly average volatility in columns (3-6) Close election takes a value of 1 if the difference between the two top candidates is less than 6%. For IV regressions (columns 3 and 6), fitted close*Q3 is the predicted values from a first-stage regression of close on termlimit and controls.

| VARIABLES | Quarterly Stock Return | | | Quarterly Average Daily Volatility | | |
	All Elections	Close Elections	Close IV	All Elections	Close Elections	Close IV
Election*Q3	-0.00103			0.00150*		
	(-0.17)			(1.703)		
Close Election*Q3		-0.0125*			0.0015**	
		(-1.82)			(2.057)	
Fitted Close*Q3			-0.0572**			0.0068**
			(-2.44)			(2.469)
Election		-.00027	0.0037*		-0.00041	-0.0008**
		(-0.44)	(1.72)		(-1.632)	(-2.222)
Q3	-0.00048*	-0.0124*	-0.0086	-0.00108	-0.00078	-0.00121
	(-1.791)	(-1.88)	(-1.35)	(-1.091)	(-0.808)	(-1.152)
Pres Election*Q3	0.00129	0.0261	0.02485	0.000226	0.000107	0.000262
	(1.540)	(1.56)	(1.47)	(0.104)	(0.0493)	(0.118)
Observations	62,747	62,533	62,533	62,747	62,747	62,747
R-squared	0.081	0.1063	0.097	0.203	0.203	0.201
State, Year FE	YES	YES	YES	YES	YES	YES
IV	NO	NO	YES	NO	NO	YES

Table 14: Event Study Results from Close Gubernatorial Elections

Event study regression results from a seven-day window around close gubernatorial elections. In Panel A, columns 1-2 report full sample results, columns 3-4 report non-presidential year results, and columns 5-6 report off-year (non-federal election years, only odd years) results. In Panel B, event study CARS are reported by bank stock size (ranked by market cap each year). In panel C, event study CARS are reported by bank stock C&I loan ratio (commercial and industrial loans as a percentage of total loans).

Panel A: Full Sample Close Elections Event Study

VARIABLES	CAR	BHAR	CAR	BHAR	CAR	BHAR
Absolute Value of Abnormal Ret	.04689***	.04740***	.04885***	.04943***	.03822***	.038236***
(-3,+3 window)	(30.957)	(30.3203)	(27.8158)	(27.2401)	(13.1573)	(13.5721)
Observations	1,388	1,388	1,148	1,148	211	211
All Elections	YES	YES				
Non-Presidential Election			YES	YES		
Off-Year Election					YES	YES

Panel B: Close Elections Event Study by Size

VARIABLES	Size Quintile					
	Small	2	3	4	Large	1-4
CAR(-3,+3)	.06113***	.05172***	.04401***	.04073***	.03660***	.04955***
	(12.2629)	(14.4930)	(15.4318)	(15.9389)	(16.3611)	(26.9031)
Observations	281	268	266	268	258	1083

Panel C: Close Elections Event Study by C&I Loan Ratio

VARIABLES	C&I Quintile					
	Zero	Small	2	3	4	5

CAR(-3,+3)	.04884***	.04037***	.04679***	.04110***	.04531***	.04283***
	(6.8365)	(15.7426)	(18.8712)	(19.6911)	(16.1323)	(15.8304)
C&I Mean %	0	.0638242	.1392161	.1982668	.2645865	.4262267
Observations	51	279	271	272	271	266

Table 15: Quarterly Change in Loans, Stock Returns, and Volatility

Regression results from a difference-in-difference estimation from 1985-2017 with close election*Qtr as the treatment variable. Q1, Q2, Q3, and Q4 correspond to the first, second, third, and fourth quarter of a gubernatorial election year. Q5 is the quarter after a gubernatorial election (elections occur in the fourth quarter) while Q2-Q8 is the aggregate change in bank-loans from the second quarter of a gubernatorial election year to the fourth quarter of the year after a gubernatorial election. Close election takes a value of 1 if the difference between the two top candidates is less than 6% and is the predicted values from a first-stage regression of close on termlimit and controls. Panel A presents bank lending results, Panel B presents stock return results, and Panel C presents stock volatility results.

<div align="center">Panel A: Change in Bank Lending in Quarters Before Close Elections</div>

VARIABLES	Quarterly Percent Change in Total Loans					
	Q1	Q2	Q3	Q4	Q5	Q2-Q8
Close Election*Qtr	0.00459	-0.0124**	-0.0143***	0.00446	0.00218	-0.0134
	(0.653)	(-2.141)	(-4.896)	(0.526)	(0.240)	(-1.540)
Observations	970,489	970,489	970,489	970,489	960,020	945,318
R-squared	0.038	0.039	0.032	0.032	0.035	0.068
Controls	YES	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES	YES

Panel B: Quarterly Stock Returns Before Close Elections

VARIABLES	Quarterly Stock Return			
	Q2	Q3	Q4	Q5
Close Election*Qtr	.00082	-0.05716**	0.0172	-0.00705
	(0.06)	(-2.44)	(0.78)	(-0.61)
Observations	62,533	62,533	62,533	57,041
R-squared	0.1059	0.097	0.0966	0.1036
Controls	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES
IV	YES	YES	YES	YES

Panel C: Quarterly Average Stock Volatility Before Close Elections

VARIABLES	Quarterly Average Daily Volatility			
	Q2	Q3	Q4	Q5
Close Election*Qtr	0.00129	0.00685**	0.00275	0.00187
	(0.284)	(2.469)	(0.974)	(0.493)
Observations	62,747	62,747	62,747	59,276
R-squared	0.203	0.201	0.203	0.208
Controls	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES
IV	YES	YES	YES	YES

Table 16: Change in Bank Lending Before Gubernatorial Elections by Size and Leverage

Regression results from a difference-in-difference estimation from 1985-2017 with close election*Q3 as the treatment variable. Close election takes a value of 1 if the difference between the two top candidates is less than 6% and is the predicted values from a first-stage regression of close on termlimit and controls. The dependent variable is the quarterly percentage change in total bank loans. Panel A presents results from sorting on size (size defined by total assets) while Panel B presents results sorted by leverage ratio.

Panel A: Change in Bank Lending Before Close Elections by Size Quintile

VARIABLES	Quarterly Percent Change in Total Loans					
	Small	2	3	4	Large	1-4
Close Election*Q3	-0.0139***	-0.0251***	-0.00837*	-0.0148*	0.00249	-0.0157***
	(-3.656)	(-5.040)	(-1.712)	(-1.870)	(0.423)	(-6.652)
Observations	253,003	267,934	267,058	263,345	242,874	1,051,341
R-squared	0.078	0.069	0.050	0.042	0.029	0.052
Controls	YES	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES	YES

Panel B: Change in Bank Lending Before Close Elections by Leverage Quintile

VARIABLES	Quarterly Percent Change in Total Loans					
	Low	2	3	4	High	1-4
Close Election*Q3	-0.00879	-0.0150***	-0.00546	-0.0159***	-0.0157**	-0.0116***
	(-1.204)	(-2.932)	(-0.998)	(-2.985)	(-2.690)	(-3.417)
Observations	261,529	263,939	262,525	261,450	244,771	1,049,443
R-squared	0.074	0.037	0.034	0.036	0.078	0.041
Controls	YES	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES	YES

Table 17: Quarterly Change in Loans and Volatility for Small and Large Banks

Regression results from a difference-in-difference estimation from 1985-2017 with close election*Qtr as the treatment variable. Panel A presents bank lending results for small banks (smallest 40% of banks ranked by total assets each quarter) , Panel B presents bank lending results for large banks (largest 40% of banks ranked by total assets each quarter), Panel C presents stock volatility results for small banks, and Panel D presents stock volatility results for large banks.

Panel A: Quarterly Change in Bank Lending Before Close Elections for Small Banks

VARIABLES	Quarterly Percent Change in Total Loans				
	Q2	Q3	Q4	Q5	Q2-Q8
Close Election*Qtr	-0.0140	-0.0208***	0.00776	0.00327	-0.0233***
	(-1.523)	(-3.991)	(0.750)	(0.282)	(-2.788)
Observations	392,541	392,541	392,541	385,360	377,464
R-squared	0.067	0.055	0.057	0.040	0.055
Controls, IV, FE	YES	YES	YES	YES	YES

Panel B: Quarterly Change in Bank Lending Before Close Elections for Large Banks

VARIABLES	Quarterly Percent Change in Total Loans				
	Q2	Q3	Q4	Q5	Q2-Q8
Close Election*Qtr	-0.0106	-0.00787	0.00582	0.00249	0.00266
	(-1.526)	(-1.569)	(0.675)	(0.304)	(0.225)
Observations	376,716	376,716	376,716	374,480	369,932
R-squared	0.029	0.026	0.026	0.033	0.082
Controls, IV, FE	YES	YES	YES	YES	YES

Panel C: Quarterly Stock Volatility Before Close Elections for Small Banks

VARIABLES	Quarterly Average Daily Volatility				
	Q2	Q3	Q4	Q5	Q6
Close Election*Qtr	0.00290	0.00360***	0.00741***	0.00773***	-0.000182
	(0.497)	(2.763)	(9.739)	(2.801)	(-0.0672)
Observations	25,106	25,106	25,106	24,122	24,122
R-squared	0.232	0.232	0.232	0.237	0.237
Controls, IV, FE	YES	YES	YES	YES	YES

Panel D: Quarterly Stock Volatility Before Close Elections for Large Banks

VARIABLES	Quarterly Average Daily Volatility				
	Q2	Q3	Q4	Q5	Q6
Close Election*Qtr	-0.00254	0.0116***	-0.00118	-0.00636	-0.00114
	(-0.671)	(3.037)	(-0.237)	(-0.995)	(-0.320)
Observations	25,075	25,075	25,075	23,267	23,267
R-squared	0.231	0.226	0.231	0.234	0.229
Controls, IV, FE	YES	YES	YES	YES	YES

Table 18: Change in Bank Lending Before Gubernatorial Elections by Loan Category

Regression results from a difference-in-difference estimation from 1985-2017 with close election*Q3 as the treatment variable. Close election takes a value of 1 if the difference between the two top candidates is less than 6% and is the predicted values from a first-stage regression of close on termlimit and controls. The dependent variable is the quarterly percentage change in total bank loans (column A), CI Commercial and Industrial Loans (column B), personal loans (column C), real estate loans (column D), or mortgages (Column E).

VARIABLES	All Loans	CI Loans	Personal Loans	Real Estate	Mortgages
Close Election*Q3	-0.0143***	-0.0101	-0.0123*	-0.00597*	0.0379*
	(-4.896)	(-1.296)	(-2.057)	(-1.708)	(1.961)
Observations	970,489	719,463	953,400	960,673	71,689
R-squared	0.032	0.014	0.014	0.021	0.021
Controls	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES

Table 19: Change in Bank Lending Before Gubernatorial Elections by C&I Loan Ratio

Regression results from a difference-in-difference estimation from 1985-2017 with close election*Qtr as the treatment variable. Results are sorted by the ratio of CI loans (commercial and industrial loans) to total loans. Q1, Q2, Q3, and Q4 correspond to the first, second, third, and fourth quarter of a gubernatorial election year. Q5 is the quarter after a gubernatorial election (elections occur in the fourth quarter) while Q2-Q8 is the aggregate change in bank-loans from the second quarter of a gubernatorial election year to the fourth quarter of the year after a gubernatorial election. Close election takes a value of 1 if the difference between the two top candidates is less than 6% and is the predicted values from a first-stage regression of close on termlimit and controls. Panel A presents bank lending results, Panel B presents stock return results, and Panel C presents stock volatility results.

Panel A: Quarterly Change in Bank Lending Before Close Elections by CI Loan Quintile

VARIABLES	Quarterly Percent Change in Total Loans					
	Low	2	3	4	High	1-4
Close Election*Q3	-0.00864***	-0.0120***	-0.0194***	-0.00731	-0.00309	-0.0120***
	(-8.077)	(-3.385)	(-2.901)	(-1.124)	(-0.565)	(-8.239)
CI Loan Ratio	.0473	.1193	.1783	.2524	.4316	.1493
Observations	209,224	215,854	216,432	217,015	207,773	858,527
R-squared	0.034	0.056	0.063	0.080	0.125	0.042
Controls	YES	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES	YES

Panel B: Quarterly Stock Returns Before Close Elections by CI Quintile

VARIABLES	Quarterly Stock Returns					
	Low	2	3	4	High	1-4
Close Election*Q3	-0.0680**	-0.0355*	-0.0455*	-0.0491	-0.0783**	-0.0550**
	(-2.30)	(-1.75)	(-2.00)	(-1.70)	(-2.07)	(-2.50)
Observations	30,914	6,891	6,895	6,891	6,926	51,591
R-squared	0.086	0.119	0.108	0.116	0.115	0.0947
Controls	YES	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES	YES

Panel C: Quarterly Stock Volatility Before Close Elections by CI Quintile

VARIABLES	Quarterly Average Daily Volatility					
	Low	2	3	4	High	1-4
Close Election*Q3	0.00896	0.00128	0.00517*	0.00152	0.00267**	0.00676**
	(1.632)	(0.328)	(1.960)	(0.352)	(2.122)	(2.249)
Observations	30,994	6,926	6,926	6,926	6,959	51,772
R-squared	0.343	0.130	0.083	0.135	0.137	0.210
Controls	YES	YES	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES	YES	YES
IV	YES	YES	YES	YES	YES	YES

Table 20: Change in Bank Lending for Uni-State Banks and Non-treated Subsidiaries

Regression results from a difference-in-difference estimation from 1985-2017 with either election*Q3 (column 1) or close election*Q3 (columns 2-4) as the treatment variable. Columns 1 and 2 present results from regressions using non-treated subsidiaries of treated bank holding companies (all elections are used in column 1, while close elections are used in column 1), column 3 presents results for banks located in only one state, and column 4 presents results for banks located in multiple states. Close election takes a value of 1 if the difference between the two top candidates is less than 6% and is the predicted values from a first-stage regression of close on termlimit and controls. The dependent variable is the quarterly percentage change in total bank loans.

VARIABLES	Quarterly Percent Change in Total Loans			
Election*Q3	-0.00808*			
	(-1.906)			
Close Election*Q3		-0.0118	-0.0149***	-.00752
		(-1.211)	(-4.610)	(-1.09)
Observations	871,808	871,808	868,525	73,566
R-squared	0.031	0.041	0.044	0.0638
Controls	YES	YES	YES	YES
State, Year FE	YES	YES	YES	YES
IV	NO	YES	YES	YES
Non-treated State	YES	YES		
UniState Only			YES	
Multistate Only				YES