Who Benefits from Targeted Property Tax Relief? Evidence from Virginia Elections[*]

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Abstract

This study examines the market impact of targeted property tax relief, which is critical for understanding who exactly benefits from this policy. Specifically, we investigate this in the context of two state-wide ballot measures in Virginia that provided property tax relief intended to aid seniors and disabled veterans respectively. Using residential MLS microdata from Virginia, results from a regression discontinuity analysis show that once the 2010 tax relief measures passed on Election Day, property values rose sharply in response to the sudden increase in demand of homeownership among the targeted groups. As part of our identification strategy, we find that "senior preferred" housing as well as areas with higher proportions of seniors and veterans experienced the highest price appreciation, while areas with fewer veterans or seniors saw little impact. Further, we explore numerous alternative hypotheses and specifications that might explain this discontinuity. The findings suggest that this policy provides an immediate benefit to current homeowners, thereby offsetting benefits for subsequent homeowners within the targeted groups. This represents a critical unintended consequence of targeted property tax relief as a (very popular) policy tool more generally, as an immediate capitalization into home prices subsequently increases the cost of housing for many individuals the relief was intended to help.

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1. Introduction

Property taxes are critical revenue sources for local and state governments,[1] and like any tax, the property tax is often used as a policy tool to achieve specific economic and social policy objectives. Most commonly, state and local governments have enacted property tax relief with the intention of aiding specific groups based on age, income, disability and/or veteran status. While these measures differ in both size and scope across states and municipalities, targeted tax relief measures were in place in all 50 states and the District of Columbia as of 2012.[2] The broader question of this study is whether or not property tax relief programs affect the incentives and behavior of the groups they target, unintentionally altering the market for real estate in ways that may, in part, offset the objectives of the policy. More specifically, if property tax relief increases the demand for homeownership among these targeted groups on the margin, then any potential price jump would benefit current homeowners (at the time of the policy change). Yet, the corresponding homeownership cost hikes may potentially offset some or all of the tax relief benefit among the targeted groups who would subsequently purchase homes at higher prices. Despite its popularity, the effects of this policy tool have largely gone unexamined, and the answer regarding who really benefits from this type of policy depends primarily on its effect on the local real estate market in which it is implemented.

In this study, we examine the effect of property tax relief on home prices using a recent permanent change in policy via amendments to Virginia's Constitution, selected directly by the voters. On Election Day 2010 (November 2), two ballot measures were proposed to Virginia voters statewide. The first ballot measure, Question 1, was an initiative to make it easier for localities to exempt the elderly and disabled from property taxes.[3] Senior citizens make up a

[1] According to the Lincoln Institute, property taxes accounted for on average 29% of local governments' total tax revenues, amounting to $1,390 per capita or nearly one-half-trillion dollars nationally in 2011. See https://www.lincolninst.edu/subcenters/significant-features-property-tax/census/

[2] For more information, see the Lincoln Institute of Land Policy's summary of tax relief programs by state here: https://www.lincolninst.edu/subcenters/significant-features-property-tax/Report_Residential_Property_Tax_Relief_Programs.aspx

[3] The exact text of the Question read: "*Shall Section 6 of Article X of the Constitution of Virginia be amended to authorize legislation that will permit localities to establish their own income or financial worth limitations for purposes of granting property tax relief for homeowners not less than 65 years of age or permanently and totally disabled?*". The *Washington Post*, a national newspaper that has a wide circulation in Virginia, endorsed Question 1

sizable portion of Virginia's population according to the U.S. Census Bureau, as 12.1% of Virginia's population is over the age of 65.[4] The second ballot measure in 2010, Question 2, would directly exempt disabled veterans from paying property taxes statewide.[5] While this group is much more limited in number than seniors, Virginia has traditionally been a state with a large military presence. According to the U.S. Department of Veteran Affairs, Virginia was home to 819,490 veterans in 2010, with 130,106 receiving disability compensation or pension payments.[6] A subset of the latter figure will be eligible for the property tax exemption, but the number may not be trivial, particularly in a number of Virginia communities that house larger veteran populations. Moreover, in light of Novy-Marx (2009) and Piazzesi and Schneider (2009), which show that small shocks to demand and small segments of the market can drive relatively large changes in home prices, it remains an empirical question as to whether targeted property tax relief can trigger significant price movements in a housing market.

Election Day is an event not unlike firms' earnings announcements or macroeconomic data press releases, where broad information is revealed and markets may swiftly respond. Methodologically, targeted tax relief offers a unique, quasi-experimental setting to examine a sudden demand shock initiated by a specific policy, where only subsections of the market are affected (i.e. "treated") and can be examined accordingly. In the case of 2010, the election outcome triggered an unambiguous shift in demand for homeownership, particularly among the groups that would now expect greater property tax relief in the future. Using microdata from a multiple listing service in Virginia, we take a regression discontinuity[7] approach to identify the effect of the passage of these Election Day constitutional measures on a particular real estate market. The empirical literature has not yet examined the immediate capitalization of such tax changes or specific, targeted tax relief initiatives, which is a key contribution of this paper.

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because it would "simplify the unwieldy current procedure" and allow localities greater flexibility to exempt seniors and disabled from local real estate taxes. The *Post* article is available online at: http://www.washingtonpost.com/wp-dyn/content/article/2010/10/29/AR2010102906950.html

[4] Seniors and homeowners generally vote at high rates, exercising substantial influence over fiscal incentives and tax policy at the local level (Rockoff 2010).

[5] The exact text of the Question read as follows: "*Shall the Constitution be amended to require the General Assembly to provide a real property tax exemption for the principal residence of a veteran, or his or her surviving spouse, if the veteran has a 100 percent service-connected, permanent, and total disability?*".

[6] For more details on the Virginia veterans snapshot, see the following: http://www.va.gov/opa/publications/factsheets/ss_virginia.pdf

[7] Since a measure of time will be used as our "running variable," we acknowledge that it may be more precise to refer to this approach as interrupted time series (ITS). However, we take license with the more general terminology of regression discontinuity for the remainder of this paper, acknowledging this semantic distinction at the outset.

From a public policy standpoint, evaluating the efficacy of property tax relief requires a more complete picture of the impact of tax relief initiatives, which are widely supported by voters and policy-makers as a policy tool to aid selected groups in need.[8] Empirically, our regression discontinuity design allows us to estimate the magnitude of the sudden shift in demand in this market (or sub-markets). We find that home prices jumped sharply after the election, particularly in areas with higher concentrations of senior citizens, veterans, and for housing types that are generally preferred by seniors. Given that the statewide initiative is arguably exogenous to particular locales, we use areas with few elderly, veterans, less preferred housing types for seniors, and other years' elections as counterfactuals to further identify the causal impact of this treatment. Our findings are robust to numerous alternative specifications and counterfactuals, providing strong evidence that the effect is properly identified. The results suggest that an unintended consequence of this policy tool is that property tax relief is immediately capitalized into home prices in markets specific to the targeted groups; and, the most direct beneficiary of such capitalization is current homeowners (whether or not they belong to the targeted groups) who receive a windfall price appreciation at the time of the policy change.

2. Property Tax Capitalization

Virginia voters passed both property tax relief measures on Election Day 2010, which would not only benefit seniors and disabled veterans, but would also change the incentives for these groups going forward. The most direct impact of these measures was that expansion of tax relief (or the expectation of this benefit) would immediately make homeownership relatively more attractive to these groups on the margin, effectively increasing the demand for homeownership among seniors and disabled veterans.[9] While examining a related property tax issue, Yinger et al. (1988) argued that estimating the corresponding effect on prices is critical, stating that "the impact of these changes on property owners, which is central to an evaluation of the policies, depends on capitalization" (p. 7). Empirically, the question remains whether home prices fully or partially capitalize the sudden change in demand, as the answer reveals the more immediate beneficiaries of the policy change.

[8] We are also responding to the call by Shan (2010) for further research into property tax relief, given that "virtually no evidence has been presented to rationalize the prevalence of property tax relief programs and the political popularity associated with expanding these programs" (p. 205).

[9] The Virginia property tax relief measures only apply to primary domiciles, which largely exclude vacation homes. For more on the effect of property tax changes on vacation homes, see Johnson and Walsh (2013).

Following the seminal work on property tax capitalization of Oates (1969), a number of empirical studies have examined the extent to which property taxes are capitalized into real estate prices. This literature has largely been motivated by exploring the Tiebout mechanism (see Tiebout 1956), as well as the theoretical ambiguity of housing supply's response to changes in demand. In terms of basic supply and demand, if the housing supply curve is upward sloping or even perfectly inelastic (fixed), then a tax change that results in a change in demand will be directly capitalized into the property prices. Empirically, after controlling for other factors that explain variation in price, prices are said to be "fully capitalized" when the expected tax liabilities account for the remaining price differences. Full capitalization implies that property owners at the time of the tax change would receive the full benefit of tax relief or would bear the full burden of tax hikes, depending on the direction of the policy change. On the other hand, partial (or zero) capitalization would imply that future owners would shoulder some (or all) of the benefits/costs. This case is likely the most commonly understood role of targeted property tax relief, where no capitalization would mean that the real benefit is simply the stream of lower tax payments for these groups as directly intended by the policy. The key task for empiricists in this literature is to estimate the extent to which taxes (or changes in taxes) are subsequently capitalized into home prices, determining whether there is full, partial, or no capitalization.

The empirical literature on property tax capitalization has mixed results regarding the extent to which property taxes more generally are capitalized into home prices. According to Sirmans et al. (2008), which reviews the literature on property taxes, the most common result is partial capitalization.[10] However, Sirmans et al. (2008) note that nine studies have found full capitalization of property taxes,[11] while seven have found no significant[12] property tax capitalization into home prices.[13] Although, to this point, the literature has largely focused on property tax differentials across localities (and subsequent changes to these differentials) to estimate the capitalization of property taxes into real estate prices. Yet, no study to date has

[10] For examples of partial capitalization, see Oates (1969), Gustely (1976), Steward (1978), Richardson & Thalheimer (1981), Goodman & Thibodeau (1998).

[11] For examples of (near) full capitalization, see Lewis & McNutt (1979), Reinhard (1981), Goodman (1983), Palmon & Smith (1998), McDonald & Yurova (2006).

[12] For examples of no capitalization, see Gronberg (1979), Johnson & Lea (1982), Haurin & Brasington (1996).

[13] See Sirmans et al (2008) for the full review of the property tax capitalization literature. For a complete review of the property tax initiative literature, see Sirmans and Sirmans (2012). Much of the literature to this point has focused on initiatives that apply more generally (like Massachusetts' Proposition 2 ½ and California's 13). For an example of the latter, see Anderson (2011).

examined statewide tax relief initiatives targeted at specific groups, which is a key gap in the literature filled by our study. This is an important question for targeted property tax relief initiatives because if much of the tax relief is capitalized into home prices at the onset of the initiative, then it is the current homeowners (on Election Day) that primarily gain from this policy, particularly owners of homes within areas that these groups prefer. While a subset of these homeowners that initially gain from tax relief may be from the targeted group, the higher cost of homeownership means that subsequent members of these groups will not realize much of the benefit from property tax relief if it is fully capitalized into home prices.

From an empirical standpoint, one benefit of the 2010 ballot measures is the fact that they are not driven by local fiscal concerns and that they apply to the entire state. By contrast, strictly local changes in property taxes may be more endogenous to the locality's fiscal situation, changing demographics, and a variety of other inter-related causal factors. For example, struggling, underfunded school systems may be a primary driver of a particular local property tax hike, but examining the corresponding impact on real estate prices becomes more complicated as one would have to disentangle the school effect from the pure tax effect. Alternatively, the statewide ballot measures create an exogenous expectation of tax relief for the affected groups, largely independent of local fiscal concerns, as seniors and disabled veterans are only a smaller subset of the population of taxpayers.[14]

3. Methodology – A Regression Discontinuity Approach

3.1. Why Regression Discontinuity?

Like many elections, the 2010 election was an off-year election that garnered extensive media attention during the months that preceded it. However, there was very little coverage of the Virginia property tax relief ballot initiatives until the weeks around the election itself. Figure 1 shows the number of newspaper articles referencing the property tax relief amendment (or related keywords) during 2010. It is clear that there was little reporting of these issues in the

[14] In some cases, localities offset tax relief for targeted groups with corresponding increases in taxes for non-targeted groups, such that the overall budget for public services does not fall very much (if at all). However, in other cases, where this tax revenue is not offset and public expenditures (e.g. local schools) fall, then the overall expected impact on demand is more ambiguous. In either case, these may attenuate the estimated effect in the opposite direction as the primary predicted increase in demand, which further motivates an empirical estimation of the overall effect of tax relief policies on home prices.

press until the week of (and prior to) the election, and virtually no polling data available on this issue in advance of the election. In fact, there were 20 articles statewide focusing on the property tax amendments in the week preceding the election and 25 during the election week (week 44), with only seven total articles published in the first 40 weeks of the year. Given the lack of media coverage, it is likely that the broader public had limited knowledge of the amendments prior to the election. On Election Day 2010, newspaper articles in conjunction with news broadcasts (and the act of voting itself) increased the awareness of the amendments and a clear discontinuity in information regarding the property tax amendment.[15] For this reason we use the week of the election as the point when demand suddenly shifts in this market and motivates our use of a regression discontinuity design.[16]

[Figure 1 about here]

Regression discontinuity (RD) design is a useful estimation strategy when estimating the impact of a treatment that is assigned in a discontinuous manner. In general, regression discontinuity operates by modeling the trends of the outcome variable across different values of a variable that determines treatment (referred to as the "running variable" in the regression discontinuity literature). The size of the treatment effect is calculated as the difference between the intercepts of the two trends measured at the value of the running variable where the treatment begins. Given that a discontinuous amount of information about specific property tax relief policies do not become known until Election Day (or the week surrounding it), a home's sale date is the running variable in this setting. The election week serves as the discontinuity at which we examine the treatment effect of the corresponding shift in demand. In other words, the key difference between the real estate market just before the election and just after the election is the

[15] The widespread newspaper endorsements near the election largely emphasized their support; and, it was widely understood that this measure would help reduce the property tax burden for the elderly and disabled. The *Loudoun Times* endorsement was representative of media outlets around the state: "anytime we can find a nexus between providing targeted tax relief that supports the most vulnerable in society, the elderly, the disabled, and infirm veterans…you'll find us in a position of advocacy." Their endorsement is available at the following link: http://www.loudountimes.com/index.php/news/editorial/editorial_constitutional_amendments_two_and_a_half_thumbs-up468/

[16] The passage of the property tax relief amendment was not unforeseeable, given that both measures in 2010 passed overwhelmingly. However, it is likely that real estate agents and other market experts knew this in advance, which would have some impact on the market just prior to the election. However, it is important to note that the degree to which these ballot measures were anticipated only attenuates a discontinuous effect, making the estimate of any effect at the discontinuity more conservative.

shift in demand for homeownership for the treated groups, which is the discontinuous effect we aim to estimate empirically in the proceeding sections.

3.2. Baseline RD Approach

We begin by investigating the impact of the election by using a standard two-trend regression discontinuity research design within a hedonic sale price regression, seen below in equation (1).

$$ln(SalePrice_h) = \alpha + \beta_1(SaleWeek_h - C) + \beta_2\mathbf{1}(SaleWeek_h \geq C)(SaleWeek_h - C) + \beta_3\mathbf{1}(SaleWeek_h \geq C) + \beta' X_h + \varepsilon_h \quad (1)$$

Specifically, we use the logged sales price of house *h* as the outcome. The sales week trend has been re-centered around the appropriate cutoff (C) for the election (week 44). The coefficient on this trend (β_1) captures the market trend in sale price over time. We have also included this same trend interacted with an indicator variable equal to one when the sales week was at or past the election week cutoff. The coefficient (β_2) on this interaction term represents the difference in the market trend pre- and post-election. The aforementioned indicator variable equals one when the sales week is after the election, and is the estimated difference in the sale price due to the election. The discontinuity coefficient (β_3) estimates the difference in the intercept at the discontinuity and can be interpreted as the treatment effect of the election.

Finally, X_h represents the following controls common to hedonic price regressions that account for observable characteristics of heterogeneous properties: age of structure, square footage, number of bedrooms and bathrooms, size of garage (in number of cars), logged acreage, logged time on market, indicators for whether the structure is a condo/townhome, had a basement, no acreage, garage, was vacant, and census block group fixed effects.[17] Because our variation is at the sale week level, we cluster the standard errors on the sale week, but estimates are very similar when clustering by census block group. We estimate the impact of each election separately and include sixteen weeks of sales prior to and after each election in our analysis, which corresponds to week 28 of the election year through week 8 of the following year.[18]

[17] The results are not particularly sensitive to the choice of spatial fixed effects. Using zip codes, for example, yields similar results.
[18] See section 6 for additional discussion of the choice of this window or "bandwidth."

3.3. Identification Strategy and Sample Stratification

The methodological approach outlined above yields an estimate of the treatment effect of the election on the overall sample of homes within a particular timeframe. However, property tax relief primarily affects the overall market insofar as it affects specific, targeted groups. Methodologically, this policy setting affords us a quasi-experimental opportunity to isolate the treatment effect by estimating discontinuities (or treatment effects) in "more treated markets" in contrast with "less treated markets." Specifically, if the effect is properly identified, then we expect the increase in demand for homeownership to be most pronounced (and lead to the most price appreciation) in the areas with high concentrations of the tax relief's targeted groups. In other words, we should see a larger treatment effect of the election on areas with the most treated individuals. Relatedly, we also expect to see a larger increase in demand for the *types* of homes preferred by the impacted groups. Conversely, if the property tax relief measures are driving price appreciation, then we should expect little price appreciation for areas with low concentrations of these groups (i.e. the "least treated" markets) if the effect is properly identified. Therefore, we stratify our sample based on the proportions of elderly or veterans within each block group to isolate the effect of the election in these areas.[19] In later tests, we stratify by what we later describe as "senior preferred housing," so that we may identify non-geographically based sub-markets in which a treated group prefers.

When reading this study, it is critical to consider the primary findings in the context of the totality of all results presented. No single test stands alone in identifying the causal effect; however, taken together, both the main tests and the placebo tests provide considerable evidence that the primary findings are properly identified. For the majority of our analysis we use a standard two-trend design, but also provide estimates using a single trend, a quadratic trend, dual quadratics, or dual cubic trends. If the effect is robust, then the results should also not be particularly sensitive to the choice of bandwidth, or number of weeks on either side of the discontinuity. We return to this point in section 6.

[19] See the next section for additional discussion on how we define "high elderly" and "high veteran" areas.

3.4. Broader Election (Season) Effect?

The empirical literature has already established that markets react to changes in the political landscape, particularly elections. Exploiting exogenous changes in prediction markets on Election Day, Snowberg et al (2007) found that markets across the board reacted strongly to both anticipated and revealed election information. In a close election in 2004, Snowberg et al (2007) found that equity prices, interest rates, oil prices, and the value of the dollar changed sharply in response to election news. More generally, going back to 1880, they find that Republican presidents raise stock valuations by 2-3 percent. Since the 1980s, Snowberg et al (2007) also find that electing Republican presidents is associated with rising bond yields. Relatedly, Knight (2006) found that the party platforms of Bush and Gore during the 2000 presidential election were capitalized into equity prices, in that markets prices anticipated that certain industries benefit from a particular candidate.[20]

In light of the literature above, it is possible that the estimated 2010 effect reflects a broader election effect on home prices, rather than the narrower effect associated with particular real estate-related policy changes. Hence, we estimate the same regression discontinuity (equation (1) above), but for different election years to test whether a more general election effect emerges. Exploring whether there is an effect during other elections addresses a few potential issues. First, as implied by the previous literature above, the overall policy and macroeconomic environment may change in ways that affect a particular real estate market and could be an alternative explanation for our main results. Second, and relatedly, the 2010 election was an election where Congress switched parties and the Republicans took control of the House of Representatives. Third, the election season itself could represent a seasonal aspect of home prices that explains the jump in prices around this time of year.

While estimating the regression discontinuity for multiple years addresses these issues, two recent elections stand out as important counterfactuals. For example, the prior year's election (2009) provides a good counterfactual in contrast to 2010, because in Virginia, 2009 was a post-crisis off-year election where there was no real estate-related measure on the ballot. While off-year election turnout is generally low, both the 2009 and the 2010 elections in Virginia

[20] For additional studies of politics/elections on markets, see Santa-Clara and Rossen (2003), Herron (2000), Lang and Shackleford (2000), Slemrod and Greimel (1999), and Sinai and Gyourko (1997).

had very similar statewide turnout rates of approximately 35%, most likely because the primary statewide race was a close gubernatorial election in 2009. We also examine 2006 as an important counterfactual because it represents the opposite political shift in Congress (where the House of Representatives flipped from Republican to Democrat controlled), where we would expect to see the opposite effect as compared to 2010 if the Congressional switch is driving the 2010 result.[21] More broadly, if the results are properly identified, then testing whether there is a similar effect in other "off year" elections provides important counterfactuals, allowing us to distinguish the 2010 effect from another, possibly more general election season effect. The estimates from alternate elections and the estimates for the low elderly and low veteran stratifications provide "placebo tests" for our methodology.[22]

4. Data

We use residential real estate data from a large multiple listing service (MLS) located in central Virginia, including Richmond and other surrounding areas. The coverage of our MLS data represents a typical housing market that includes urban, suburban, and rural sales. Richmond is a medium-sized city located in the eastern part of central Virginia and the MLS covers much of the "Greater Richmond" area (or Richmond MSA). Among others, Levitt and Syverson (2008) point out that MLS data are entered by real estate agents and can be incorrect or incomplete. As a result, we gave careful consideration to variables used with missing data and extreme values. We retain homes that sold less than 580 days after being listed (top 1 percent culled), and sold for between $50,000 and $500,000 (approximately the top and bottom 5 percent culled).[23] Our final sample consists of 78,214 properties sold within this residential real estate market between 2006 and 2012.

[Table 1 about here]

[21] If the Republicans taking the House of Representatives in 2010 somehow drastically affected the expectations of future policy and its subsequent effects on the real estate market, then reversing this should also have the opposite effect (i.e. a sudden drop in real estate prices).

[22] An obvious exception would be the 2008 election, where the election season also coincided with a severe financial crisis and housing crash.

[23] We also keep homes that are smaller than 5,400 square feet, have 1 to 5 bedrooms, 1 to 6.5 bathrooms, fewer than 40 acres, and 5 or fewer stories. Acreage is an optional field in this MLS, and includes many homes with zeroes and null values entered within this field. Rather than culling a substantial number from this sample, we simply include a dummy variable if an observation had either a zero or null value for the acreage entry. Our overall findings are not sensitive to the choice of including these observations.

The first two columns of Table 1 report the summary statistics for all properties in our overall sample sold between 2006 and 2012, as well as the election year of interest, 2010. The 2010 summary statistics only include our analysis sample from the 28th week of 2010 to the 8th week of 2011. The average sales price for the full sample and 2010 fall within approximately 10% of each other ($229,505 for 2006 – 2012 and $211,930 for 2010), whereas 2010 resides near the national trough in home prices following the U.S. housing crisis. Relatedly, the average time on market or liquidity is quite different (63 days compared to 79). These two differences can be largely attributed to differences in the housing market pre- and post-crisis.[24] Otherwise, the housing characteristics are quite consistent across the sample.

In the next set of columns, we stratify our summary statistics by "high/low elderly" (columns 3 and 4) or "high/low veteran," (columns 5 and 6) but it is useful here to define these geographical areas and discuss relevant summary statistics across these sub-samples. The smallest geographical level in which the U.S. Census (publically) reports figures on the proportion of elderly or veterans is the census block group level. Block groups are broadly defined "neighborhoods" that are subsections of census tracts, which are designed to be "small, relatively permanent statistical subdivisions of a county...designed to be homogenous with respect to population characteristics, economic status, and living conditions,"[25] and contain between 600 and 3,000 people (usually around 1,500). The block group level data on age and veteran status used to stratify the sample are from the U.S. Census 2006-2010 ACS Five Year Estimates. In Virginia, the median proportion of elderly (65 years of age or over) within a block group is 22.2 percent, and the median veteran proportion is 23.2 percent. Therefore, within our sample, we classify all block groups with a proportion of elderly over the median as "high elderly," and all block groups below this threshold as "low elderly."[26] Likewise, block groups as

[24] Concerns about overall market trends motivate our use of a regression discontinuity design, which allows us to control for trends that occur over this time period. We discuss the time trend controls (and variants of the time trend's functional form) in later sections.

[25] See www.census.gov for more detail, specifically: https://www.census.gov/geo/reference/gtc/gtc_bg.html

[26] The results in this study are not particularly sensitive to how we define "high elderly" or "high veteran" as the results are qualitatively similar (albeit more pronounced) when we associate high elderly/veteran areas as above the upper quartile threshold, and other thresholds. See the results in Figure 8 and section 6 for further discussion.

"high veteran" if their proportion of veterans is above the median, or "low veteran" if it falls below this threshold.[27]

[Figure 2 about here]

To illustrate where the most affected areas are likely to be located, Figure 2 includes a map of all the census block groups in Virginia at the top, with the block groups available in the dataset color-coded dark gray. The bottom two maps show only the block groups analyzed and depict the low elderly or low veteran block groups in light gray and high elderly or high veteran block groups in dark gray. Based on the home structure characteristics for which we have data, the summary statistics show that there are some differences in housing characteristics across areas,[28] as we might expect, indicating that these groups may have different housing preferences. Given that the 2010 initiative would most immediately impact seniors and disabled veterans (i.e. "treated" groups), our hypothesis is that the increase in demand for homeownership would be the most pronounced in these areas (i.e. more "treated" areas), which we test in the next section.

5. Results

5.1. Baseline Results and Stratifications

The 2010 property tax relief ballot measures had a significant effect on the real estate market just after the election. Table 2 reports the coefficient estimates from estimating Equation 1 for the windows surrounding the 2010 election, stratifying the sample by proportion of elderly and veterans in each census block group. The first column reports the estimated impact of the election in 2010 on the entire sample, resulting in a 5.2 percent increase in home prices. This represents the overall effect of the tax relief measures on the real estate market, implying that the targeted groups, while relatively small, still represent a sizable enough contingent to affect market prices substantially. The tax relief measures may also have a broader effect on the market

[27] We are using data on veterans to proxy for the areas that are likely more affected by this ballot measure, given the limited data availability on disabled veterans that qualify for this tax exemption. This measure is far from perfect, but we expect an area's number of veterans to be correlated with number of disabled veterans.

[28] The high elderly sub-sample generally has slightly lower priced homes than the low elderly areas ($207,605 compared to $214,899), and have slightly older structures, are slightly smaller, and sit on larger lots. Also, the average home within high veteran areas tends to be more modestly priced when compared to that of low veteran areas ($199,893 vs. $221,495), whereas age, square footage, bedrooms, and bathrooms are all nearly identical across these sub-samples. Nonetheless, given that homes have heterogeneous characteristics, we control for these observable characteristics to account for such differences within a hedonic regression framework.

because of the long term nature of a home as an asset, where tax relief later in life may bring down the expected lifetime cost of a home for all homeowners in the long run. The broader market effect is consistent with Novy-Marx (2009) and Piazzesi and Schneider (2009), showing that relatively small shocks to demand and relatively small segments of the market can drive significant changes in real estate prices.

[Table 2 about here]

All real estate markets are local; and, it is commonly said that the most important factor to consider in real estate is (as any agent will repeat three times): location. If the estimation approach is identifying a causal relationship between these tax relief measures and increased demand, then we should find the effect to be larger in locations where the overall change in demand for homeownership has increased the most, that is, greater proportions of veterans and elderly. Given the role location plays in real estate, this expectation should not be surprising, as certain neighborhoods possess characteristics and amenities preferable and convenient to seniors and veterans.[29] Indeed, Table 2 also shows that the impact is much larger when restricting the sample to block groups with high proportions of elderly individuals, as seen in column 2 (a 7.2 percent increase in home prices). Alternatively, block groups with lower proportions of seniors report a much smaller, but significant increase in sale price of 4 percent as seen in column 3.[30] Columns 4 and 5 include the estimates stratifying by proportion of veterans in each block group. Areas with high proportions of veterans report an 8.1 percent increase in property prices while low veteran block groups show no statistically significant effect.[31]

[Figure 3 about here]

The estimated effects can also be seen graphically in Figure 3, cleanly illustrating the discontinuity in 2010 for the areas with high proportions of the tax relief's targeted groups. These figures are created using the coefficients from Table 2 and are stratified in the same

[29] Rather than speculate which neighborhood characteristics and amenities seniors and veterans desire, we depend on the revealed preferences of where they actually live by using census information. We will discuss property-specific preferences in the next section.
[30] We discuss this smaller, but still significant result in greater detail in section 6.A..
[31] See Appendix Table 1 for the coefficients on all the included covariates in Table 2.

manner.[32] It is clear from the trends that the market was still bottoming out in the wake of the housing bubble. However, note the acute price jumps in the top panels of Figure 3, representing a clear shift in home prices just after the election. None of the counterfactual stratifications in Figure 3 reveal large discontinuities or drastic changes in the post-election trend, and appear to the naked eye to follow (approximately) the pre-election trend. Taken together, the graphs show clear evidence of the upward shift in home prices in the areas expected to be impacted the most by property tax relief, providing additional evidence that the increase in home prices was due to the property tax amendments specifically.

[Table 3 about here]

If this is merely an election season effect, we should see similar trends in the data for other years around the same timeframe. Table 3 provides the estimates of the discontinuity for all elections from 2006 through 2012. Within our data, there appears to be no clear, consistent election effect on real estate. These null results provide additional evidence that the 2010 results are properly identified, showing that there is no clear "November effect" or broader macro policy effect from temporary uncertainty associated from elections in this market. It is also revealing that the 2009 and 2006 elections, specifically, show no discontinuity effect. Again, the 2009 election was the off-year gubernatorial election that also coincided in the wake of the U.S. financial crisis, showing no statistically significant impact on the market overall, nor in elderly/veteran areas. Further, the 2006 results should serve as refuting evidence against the alternative hypothesis, that the 2010 election result is somehow a response to Republicans taking the House of Representatives, by showing that there appears to be no market impact when Republicans lose the House.

[Figure 4 about here]

It is also clear that the 2010 trend break is not merely a "November effect" or some recurring election season phenomenon, as the visual evidence from Figures 4 and 5 further confirms no impact during the alternative elections for either group. Indeed, when the 2010 panels stand in contrast to the alternative elections (placebo tests), our primary results stand out

[32] The figure displays predicted weekly averages for a home with fixed characteristics, and are created using coefficients from the hedonic regression of logged sales price on closing week indicators and the controls included in our RD model.

like the proverbial sore thumb. Overall, the evidence for any general impact of elections or election seasons on local real estate prices is thin. While there may be a more nuanced effect of elections on real estate (e.g. different impacts in certain geographical areas), we leave further exploration of this for future research.

[Figure 5 about here]

5.2. Property-specific Stratifications

The stratifications above focus on geography as determining which markets the increase in demand may be most pronounced. However, housing market idiosyncrasies are not exclusively location-specific. Housing markets may also be property-specific,[33] in that buyers may be shopping for a particular kind of house, possessing a certain set of characteristics specific to the buyer. In Table 4, we similarly estimate the 2010 discontinuity, but we stratify across homes that we define as "senior preferred," or simply homes that possess characteristics that may be reasonably attributed to senior preferences in this market. Specifically, a home is defined as "senior preferred" if it possesses one or more of the following characteristics: 1) it has a first-floor master bedroom or 2) it is single story or 3) it is handicap accessible or 4) it is an older home (i.e. older than 30 years) or 5) it is within a 55 and over community.[34] Intuitively, regardless of the tax incentive, it is difficult to imagine any 65 year old purchasing a new three story home with the master bedroom on the top floor. Hence, if the effect is properly identified, we should find no increase in demand for homes as described in the latter example.

[Table 4 about here]

This property-specific stratification produces similar results as the geographic stratifications, in that price appreciation occurs for the types of homes we most expect seniors to demand. Overall, Table 4 shows that the property tax initiatives in 2010 resulted in a 7.4 percent increase in "senior preferred" housing in the full sample, with no effect on non-preferred homes.

[33] For example, when examining property tax capitalization and public schools, Gallagher et al. (2013) used the size of homes (namely, "small" homes) as part of their identification strategy.

[34] The first and fourth characteristics come from fields within the MLS, but the second, third, and fifth come from the "comments" field written by the listing agent when the property is marketed. The latter may not be exhaustive, given that some agents may choose not to advertise these features, depending on their marketing strategy. While the data is imperfect, it is not obvious that this introduces bias in the direction of the result. Moreover, the results are also consistent when senior preferred is simply defined as 1 or 4 (i.e. not using the comments).

In fact, "senior preferred" homes in elderly areas appreciated by 9.9 percent just after the 2010 election, while non-preferred homes in those areas saw no appreciation. Even "senior preferred" homes in low elderly areas appreciated just after the 2010 election, albeit by a smaller amount (5.3 percent). Figure 6 provides the graphical estimates from Table 4, showing a clear shift in the prices for "senior preferred" housing and very little impact on non-preferred homes. Taken together, the mounting evidence suggests that, while the real estate market is idiosyncratic, the 2010 property tax relief initiatives had an immediate, predictable (directional) impact on the overall real estate market and its sub-markets.

[Figure 6 about here]

6. Further Identification and Robustness

6.1. Alternative RD Specifications

The primary results from the previous section are not very sensitive to choice of bandwidth (i.e. number of weeks used in the analysis). In Table 5, we include estimates that modify the bandwidth and the pre- and post-election trends for the elderly stratification and the veteran stratification respectively. The top panels in Table 5 include estimates for elderly stratifications in 2010, while the bottom panels show the estimates for the veteran stratifications. Overall, Table 5 shows that the primary results are robust to alternative specifications of the trends. In the first column (which corresponds to regressions (1), (6), (11), and (16)), the regressions use a single trend through the entire sample. The next column utilizes a quadratic trend. Both columns produce very similar coefficient estimates to the two-trend discontinuity effect. In the final three columns, we increase the bandwidth to a full year (or six months in the pre- and post-election time periods), which correspond to the 18th week of the election year through the 17th week of following year. A two-trend, linear specification with a longer bandwidth yields a 5.4 percent increase for the high elderly areas and 5.6 percent increase for the high veteran areas. These are qualitatively similar, albeit a bit smaller, than the estimates using the shorter bandwidth. The full-year results are more in line with the results from our default bandwidth when the trends are non-linear (two quadratics or two cubics). Generally, as the bandwidth is extended, more caution is needed in modeling the trends correctly as there is greater worry about other attenuating factors, such as seasonality of housing markets.

[Table 5 about here]

Figure 7 displays a number of additional robustness checks that are conventional for studies that employ the regression discontinuity design. First, panels A and B display varying bandwidths for both the high elderly and high veteran block group specifications, including the coefficient and 95 percent confidence interval for a dual quadratic model.[35] The default bandwidth at four months is shown with a dashed line. The coefficients are similar for all bandwidths; and, all but one of the confidence intervals does not overlap with zero, indicating that choice of bandwidth is not driving our results. Second, panels C and D use our default dual linear trend model with a four month bandwidth, but estimates the model for 12 placebo discontinuities, varying monthly from six months below to six above the true discontinuity (dashed line at 0). No placebo discontinuity is as large or statistically significant as the true discontinuity and almost all are statistically insignificant. Lastly, panels E and F estimate a density test (McCrary, 2008) to determine if there is any manipulation of the running variable (sale week) by estimating a dual linear trend model with the number of sales as the outcome. There is no statistically significant discontinuity for either group (or the low elderly or low veteran groups that are not included in the figure) providing initial evidence that there is no manipulation of the running variable, or change in the number of sales around the election week, which we will investigate further in section 6.2.[36]

[Figure 7 about here]

To this point we have classified "high elderly" and "high veteran" areas as block groups with above median proportions of these groups. However, while the 50th percentile threshold is entirely arbitrary, our results are not qualitatively sensitive to this choice. The top two panels of Figure 8 illustrates the distribution of the effect across different levels of treatment by plotting the discontinuity coefficient and 95 percent confidence interval (using equation 1) when varying the sample analyzed from the 5th to 95th percentile of the proportion elderly or veteran in the block group (window of 10 percent on either side of the proportion). The coefficient is not

[35] Using the dual linear model results in large significant effects for bandwidths below 8 months and becomes smaller for larger bandwidths, but this is expected as seasonality and the impact of the recovery from the Great Recession result in a non-linear home price trend. Using the dual cubic model results in large coefficients that are significant for bandwidths above four months to at least 18 months.

[36] There is also no discontinuity when modeling the trends using dual quadratics, cubics, or local linear regression for the density test.

significant and small in the lower end of the treatment distribution and grows with size and statistical significance to the higher end.

[Figure 8 about here]

While it is clear that the treatment effect is most pronounced in the areas we expect, it is not immediately apparent why there is a small, significant effect in low elderly areas (in previous tables' low elderly stratifications). Given that homes are long-term assets, some capitalization of property tax relief into home prices in younger areas may be rational. But, a more obvious explanation stands out. Geographically, Figure 2 shows that not all high veteran and high elderly areas overlap. Some of the effect in lower elderly areas may be "contaminated" treatment effects from high veteran areas, and vice versa. To address this possibility, we include the figure in the bottom panel of Figure 8 that repeats the exercise of the two figures above, but uses the addition of the proportion elderly and veteran in each block group to capture both treated groups. The results here are even starker than the distribution of the effect for the two groups separately. We extend this exercise in Table 6 that reports the results for the sample stratified into block groups that have both high elderly (above median) and high veteran populations, set in contrast with areas with low elderly and low veteran populations. Effectively, this removes the possibility of the other treatment effect "contaminating" the untreated group, so to speak. The results in Table 6 show that the election's effect on home prices are stronger for the high sample of both elderly and veteran, with a 9.2 percent increase in home prices in those areas. Alternatively, Table 6 also shows no change in home prices after the election for the low sample.[37]

[Table 6 about here]

6.2. Supply?

Finally, we examine whether there is a discontinuity in the supply of homes on the market, or a significant decrease in homes listed on the market that could be driving this price change around the time of the 2010 election. Figure 9 shows the average number of new listings per week that went on the multiple listing service during the months surrounding the election. While the number of new listings is not exactly flat, it is clear that there is no substantial

[37] See Appendix Figure A1 for graphical estimates for the combined high elderly and high veteran block groups for all years.

decrease in listings that can explain the price jump around this time period. In fact, no discontinuity for 2010 is statistically significant at any conventional level. However, the data on new listings appears a bit noisy around this time of year because of the natural discontinuity at the beginning of the year, with a surge in new listings in January (as compared to December). Figure A2 in the appendix shows that this is a common break at the beginning of every year, which follows a predictable dip in new listings around the holidays each year. That is, if 2010 were merely a holiday effect, we should see the effect on prices every year. Therefore, if the supply of new homes is not explaining the price jump, then this provides additional evidence that our methodological approach properly identifies the demand shock as the primary driver of the discontinuity in real estate prices.

[Figure 9 about here]

7. Conclusion

While property tax relief measures are often intended to aid specific groups, an unintended consequence of this kind of tax relief is that, on the margin, it increases demand for homeownership among these groups. In our study, we examined two property tax relief measures in Virginia that were specific to the elderly and disabled veterans, finding that they had an immediate effect on home prices after the voters approved them on Election Day. Specifically, we found that overall home prices rose by approximately 5.2 percent in response to the increase in demand for homeownership. Indeed, the tax relief policies provide unique, quasi-experimental methodological setting where the "treatment" is assigned to specific groups. We find that the effect was as much as a 7.2 percent price appreciation in areas with high concentrations of seniors, 8.1 percent in high veteran areas, and 7.4 percent for "senior preferred" homes in all areas. Conversely, the tax relief measures had little if any effect in areas with fewer "treated" groups (seniors or veterans), which one would expect if the effect of the tax relief measures was properly identified. Moreover, we find limited evidence that elections (without property tax relief initiatives) or the election seasons themselves have a significant, general impact on the real estate market.

To put these results in perspective, consider the average Virginia homeowner, who in 2014 paid $1,862 in property taxes.[38] A back-of-the-envelope calculation shows that exempting a 65-year-old disabled veteran homeowner from paying property taxes (assuming a life expectancy of 19 additional years[39] and a discount rate of 5 percent) is approximately $22,500 in net present value. This is likely to be an overestimate of the present value for some, as some elderly homebuyers were older than 65, but an underestimate for younger disabled veterans who would have longer periods of receiving the tax relief benefit. Compare this with the estimated impact (measured in levels) for the dual treated block groups (both above median elderly and veteran), which can be calculated by taking the relative impact (9.2 percent) and multiplying it by the pre-election average home price in 2010 ($203,196), amounting to an effect of roughly $18,700. The effects are smaller when restricting the above median high elderly and high veteran separately, with the high elderly and high veteran dollar effects equal to $15,000 and $16,250 respectively. Although simple, this calculation does imply that much of the benefits of the 2010 tax relief amendment initially went to current homeowners, or were fully capitalized into home prices in the highest treated areas (i.e. combination high elderly and high veteran and senior preferred homes in elderly areas) and significantly capitalized into our high veteran and high elderly areas. This immediate capitalization is roughly analogous to other, familiar demand shocks that are dependent on expectations of the future (like the stock price of a tech company that showcases an exciting new product, generating distant profits). While we leave the examination of longer-run effects to future research,[40] it is important to highlight that the short-run (and possibly longer-term) consequence of this policy is an arbitrary benefit to homeowners at the time of the policy change, whether or not they fall within the targeted groups.

Insofar as tax relief pushes up home prices, the next generation of elderly or disabled veterans may not internalize the full benefits of tax relief, given that much of it would be paid up

[38] For more details on tax rates, see http://www.tax-rates.org/virginia/property-tax

[39] The average 65-year-old Virginian has a life expectancy of 18.9 additional years - http://www.cdc.gov/mmwr/preview/mmwrhtml/mm6228a1.htm

[40] A long-run analysis would require additional data and a different methodological approach. Methodologically, there are a number of factors that become more difficult to disentangle over time (e.g. the long-run supply response, other demand shocks, etc.). While the exemption to disabled veterans was direct and went into effect in short order, the elderly ballot measure simply allowed localities greater flexibility to extend or change property tax relief policies in the future. According to a 2012 Cooper Center report (via the University of Virginia), it was clear that, "it is still too early to see the full ramifications of this new local power. It will become apparent in the next few years whether localities find the state's limitations too restrictive or sufficient for local conditions" (p. 41).
http://www.coopercenter.org/sites/default/files/econ/TaxRates/taxrates2012/trbook2012.pdf

front when they purchase the home. More intuitively, Yinger et al. (1988) explained that, "no one can buy a house with relatively low taxes unless he or she pays a relatively high price for the house; that is, people with relatively low taxes must pay for the privilege" (p. 7). Of course, in order to avoid paying for the capitalized tax benefit, seniors and disabled veterans may also choose otherwise less-preferred housing (e.g. a three-story townhome in a young neighborhood with a lot of children nearby), which may represent an additional non-monetary unintended consequence of this type of policy. As a matter of public policy, these unintended consequences should be considered as policy-makers and voters determine the efficacy of targeted tax relief programs.

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Table 1: Summary Statistics (Means and Standard Deviations)

	2006 - 2012	*2010*	*Elderly (2010)*		*Veteran (2010)*	
			High	*Low*	*High*	*Low*
	(1)	(2)	(3)	(4)	(5)	(6)
Sale Price (Mean)	$223,848	$211,930	$207,605	$214,899	$199,893	$221,495
	(94,636)	(97,494)	(96,887)	(97,813)	(89,248)	(102,592)
Sale Price (Median)	$207,500	$194,000	$190,000	$196,120	$182,675	$203,000
Age of Structure	27.39	26.44	28.24	25.21	26.02	26.77
	(25.12)	(24.45)	(23.93)	(24.72)	(22.10)	(26.16)
Square Footage	1,904	2,016	1,986	2,037	1,979	2,046
	(714)	(776)	(755)	(789)	(734)	(806)
Number of Bedrooms	3.40	3.45	3.38	3.50	3.44	3.46
	(0.76)	(0.79)	(0.75)	(0.81)	(0.75)	(0.81)
Number of Bathrooms	2.23	2.34	2.29	2.37	2.30	2.37
	(0.75)	(0.84)	(0.83)	(0.84)	(0.79)	(0.87)
Acreage (no 0s)	1.15	1.17	1.60	0.89	1.29	1.08
	(2.90)	(2.80)	(3.04)	(2.60)	(2.68)	(2.88)
No Acreage	0.35	0.24	0.27	0.22	0.28	0.22
	(0.48)	(0.43)	(0.45)	(0.42)	(0.45)	(0.41)
Vacant	0.43	0.53	0.55	0.51	0.56	0.50
	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Basement	0.14	0.15	0.16	0.15	0.15	0.15
	(0.35)	(0.36)	(0.37)	(0.35)	(0.36)	(0.36)
Condo or Townhouse	0.10	0.11	0.10	0.11	0.08	0.13
	(0.30)	(0.31)	(0.30)	(0.32)	(0.27)	(0.33)
Garage Size (# Cars)	0.88	1.00	1.01	1.00	1.01	1.00
	(0.98)	(1.00)	(1.01)	(0.99)	(1.00)	(1.00)
Time on Market	63.10	79.02	83.35	76.05	81.71	76.88
	(67.91)	(73.90)	(78.60)	(70.35)	(75.75)	(72.33)
Prop. Veteran in C.B.G.	0.22	0.22	0.27	0.19	0.30	0.16
	(0.09)	(0.09)	(0.09)	(0.08)	(0.06)	(0.05)
Prop. Over 65 in C.B.G.	0.21	0.21	0.31	0.14	0.26	0.18
	(0.10)	(0.10)	(0.07)	(0.05)	(0.11)	(0.08)
Senior Preferred Housing	0.58	0.57	0.68	0.50	0.62	0.53
	(0.49)	(0.50)	(0.47)	(0.50)	(0.49)	(0.50)
N	78,214	5,086	2,070	3,016	2,252	2,834

Source: Central Virginia Regional MLS

Notes: Column 1 includes means (standard deviations) for homes sold from the 1st week of 2006 through the 52nd week of 2012. All other columns include the analysis weeks from the 28th week of 2010 to the 8th week of 2011. Summary statistics for 2010 have been stratified by whether the proportion elderly and veteran in each block group is above or below the state median.

Table 2: Main Results – Regression Discontinuity around the 2010 Election

	Full Sample	***High Elderly***	***Low Elderly***	***High Veteran***	***Low Veteran***
	(1)	(2)	(3)	(4)	(5)
Discontinuity	0.052***	0.072***	0.040***	0.081***	0.029
	(0.012)	(0.017)	(0.015)	(0.015)	(0.020)
Trend	-0.004***	-0.006***	-0.003**	-0.006***	-0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Post-Trend	-0.003**	-0.000	-0.005**	-0.002	-0.004*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	11.657***	11.619***	11.645***	11.641***	11.313***
	(0.084)	(0.088)	(0.063)	(0.084)	(0.096)
N	5,086	2,070	3,016	2,252	2,834

Source: Central Virginia Regional MLS

Notes: Estimates are from a two-trend regression discontinuity using a cutoff at week 44 and a bandwidth from week 28 in 2010 through week 8 in 2011 (i.e. 16 week windows). Estimates are stratified by whether the block group contains above or below the state median proportion of elderly (high elderly = treated) and veterans (high veteran = treated). Untabulated controls (for all regressions) include age of structure, square footage, logged acreage, logged time on market, number of bedrooms and bathrooms, garage (number of cars), indicators for whether the structure is a condo or townhouse, has a basement, no acreage, is vacant, and block group fixed effects. Standard errors clustered on sale week are in parenthesis. * 10%, ** 5%, *** 1%.

Table 3: Regression Discontinuity Results by Year

	Treatment	**Placebo Test - Other Elections**					
		Full Sample					
	2010	*2006*	*2007*	*2008*	*2009*	*2011*	*2012*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discontinuity	0.052***	-0.004	0.008	-0.009	0.010	0.008	-0.016
	(0.012)	(0.005)	(0.007)	(0.010)	(0.007)	(0.014)	(0.011)
N	5,086	8,944	7,012	5,296	6,039	5,819	6,522
				High Elderly			
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Discontinuity	0.072***	-0.008	0.021**	-0.005	0.019	-0.017	-0.012
	(0.017)	(0.007)	(0.009)	(0.013)	(0.012)	(0.023)	(0.022)
N	2,070	3,513	2,744	2,158	2,395	2,430	2,681
				Low Elderly			
	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Discontinuity	0.040***	-0.001	0.001	-0.011	0.006	0.027*	-0.019
	(0.015)	(0.006)	(0.009)	(0.014)	(0.009)	(0.014)	(0.012)
N	3,016	5,431	4,268	3,138	3,644	3,389	3,841
				High Veteran			
	(22)	(23)	(24)	(25)	(26)	(27)	(28)
Discontinuity	0.081***	0.001	0.006	-0.015	0.000	-0.021	-0.017
	(0.015)	(0.008)	(0.011)	(0.012)	(0.014)	(0.016)	(0.021)
N	2,252	3,842	2,920	2,300	2,599	2,446	2,724
				Low Veteran			
	(29)	(30)	(31)	(32)	(33)	(34)	(35)
Discontinuity	0.029	-0.008	0.010	-0.005	0.017*	0.031	-0.012
	(0.020)	(0.006)	(0.010)	(0.014)	(0.009)	(0.019)	(0.012)
N	2,834	5,102	4,092	2,996	3,440	3,373	3,798

Source: Central Virginia Regional MLS

Notes: Estimates are from a two-trend regression discontinuity using a cutoff at week 44 and a bandwidth of week 28 in the stated year through week 8 of the following year. Estimates for each year are provided for the full sample and stratified by elderly (high elderly = treated) and veteran (high veteran = treated) census block groups. Estimates are included for the "treatment year" (2010) and several other "placebo years" to estimate the effect of elections in general. Untabulated controls include age of structure, square footage, logged acreage, logged time on market, number of bedrooms and bathrooms, garage (number of cars), indicators for whether the structure is a condo or townhouse, has a basement, no acreage, is vacant, and block group fixed effects. Standard errors clustered on sale week in parenthesis. * 10%, ** 5%, *** 1%.

Table 4: Senior Preferred Real Estate Stratifications

	Full Sample		***High Elderly***		***Low Elderly***	
	Senior Preferred	Not Senior Preferred	Senior Preferred	Not Senior Preferred	Senior Preferred	Not Senior Preferred
	(1)	(2)	(3)	(4)	(5)	(6)
Discontinuity	0.074***	0.011	0.099***	0.007	0.053*	0.013
	(0.020)	(0.014)	(0.029)	(0.030)	(0.028)	(0.016)
Trend	-0.006***	-0.003***	-0.009***	-0.002	-0.003	-0.004***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Post-Trend	-0.004**	0.002	0.001	0.002	-0.009***	0.001
	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)
Constant	11.570***	11.913***	11.528***	12.005***	11.498***	11.882***
	(0.085)	(0.092)	(0.091)	(0.117)	(0.135)	(0.105)
N	2,894	2,192	1,402	668	1,492	1,524

Source: Central Virginia Regional MLS

Notes: Estimates are from a two-trend regression discontinuity using a cutoff at week 44 and a bandwidth of week 28 in 2010 through week 8 in 2011. Estimates are stratified by whether the characteristics of the home are preferred by seniors (home is at least 30 years old, single story, first floor master, handicap, or in a 55+ community) and estimates are included for the full sample and stratified by proportion elderly. Untabulated controls include age of structure, square footage, logged acreage, logged time on market, number of bedrooms and bathrooms, garage (number of cars), indicators for whether the structure is a condo or townhouse, has a basement, no acreage, is vacant, and block group fixed effects. Standard errors clustered on sale week in parenthesis. * 10%, ** 5%, *** 1%.

Table 5: Alternative Specifications

	Default Window (2010)		**Full Year Window (2010**		
	Single Trend	Quadratic	2 Trend	2 Quadratic	2 Cubic
			High Elderly		
	(1)	(2)	(3)	(4)	(5)
Discontinuity	0.072***	0.072***	0.054***	0.073***	0.078***
	(0.017)	(0.017)	(0.014)	(0.017)	(0.023)
N	2,070	2,070	3,782	3,782	3,782
			Low Elderly		
	(6)	(7)	(8)	(9)	(10)
Discontinuity	0.044**	0.042***	0.011	0.050***	0.052**
	(0.018)	(0.014)	(0.016)	(0.017)	(0.021)
N	3,016	3,016	5,444	5,444	5,444
			High Veteran		
	(11)	(12)	(13)	(14)	(15)
Discontinuity	0.082***	0.082***	0.056***	0.077***	0.093***
	(0.017)	(0.016)	(0.015)	(0.016)	(0.020)
N	2,252	2,252	3,984	3,984	3,984
			Low Veteran		
	(16)	(17)	(18)	(19)	(20)
Discontinuity	0.032	0.030	0.005	0.043*	0.034
	(0.020)	(0.019)	(0.016)	(0.024)	(0.027)
N	2,834	2,834	5,242	5,242	5,242

Source: Central Virginia Regional MLS

Notes: Estimates are from a regression discontinuity with the trends modeled using the specification at the top of each column. All specifications use a cutoff at week 44. The *Default Window* uses a bandwidth of week 28 in 2010 through week 8 in 2011 and the *Full Year Window* uses weeks 18 in 2010 through week 17 in 2011. Estimates are stratified by elderly (high elderly = treated) and veteran (high veteran = treated) block groups. Untabulated controls include age of structure, square footage, logged acreage, logged time on market, number of bedrooms and bathrooms, garage (number of cars), indicators for whether the structure is a condo or townhouse, has a basement, no acreage, is vacant, and block group fixed effects. Standard errors clustered on sale week in parenthesis. * 10%, ** 5%, *** 1%.

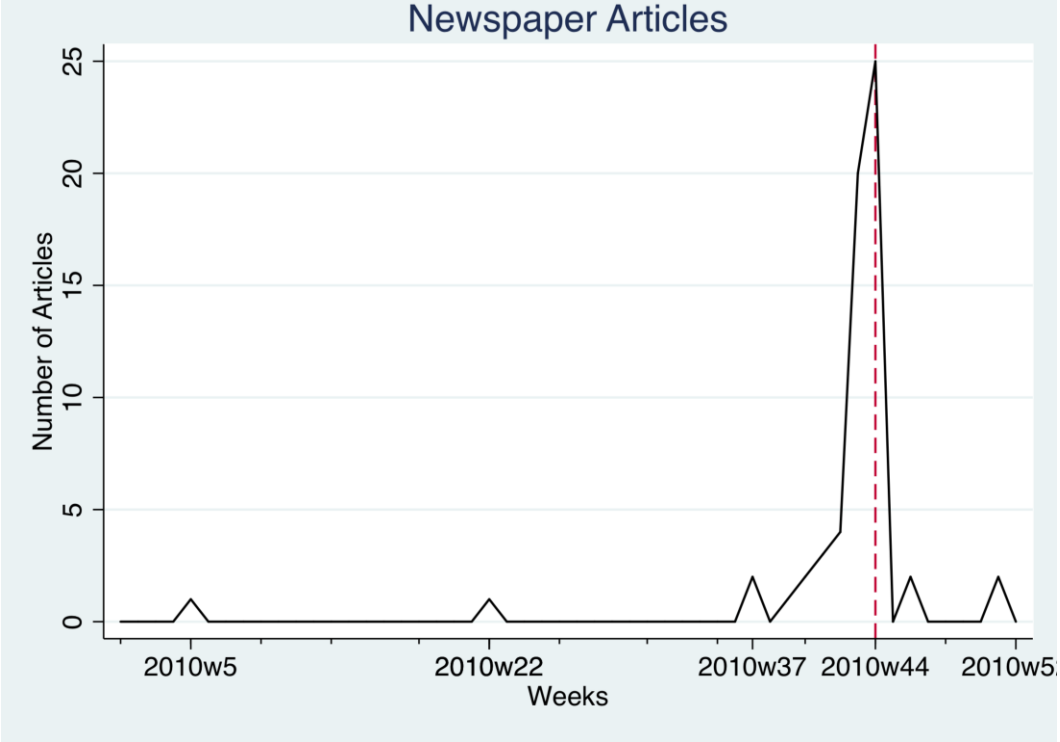
Table 6: Stratified by Combined High and Low Treatment

	High Elderly High Veteran (1)	***Low Elderly Low Veteran*** (2)
Discontinuity	0.092***	0.027
	(0.020)	(0.018)
N	1,335	2,103

Source: Central Virginia MLS

Notes: Estimates are from a two-trend regression discontinuity using a cutoff at week 44 and a bandwidth of week 28 in 2010 to week 8 in 2011. Estimates are stratified for the combination of high elderly (above median) and high veteran (column 1) and low elderly and low veteran (column 2). Untabulated controls include age of structure, square footage, logged acreage, logged time on market, number of bedrooms and bathrooms, garage (number of cars), indicators for whether the structure is a condo or townhouse, has a basement, no acreage, is vacant, and block group fixed effects. Standard errors clustered on sale week in parenthesis. * 10%, ** 5%, *** 1%.

Figure 1: Property Tax Newspaper Articles in Virginia



Source: Articles from newsbank.com

Notes: Figure shows the number of articles per week in Virginia discussing the property tax ballot amendments during 2010. Search terms used: property tax ballot, property tax amendment, disabled veteran tax, elderly tax.

Figure 2: Data Universe and Geographical Stratifications

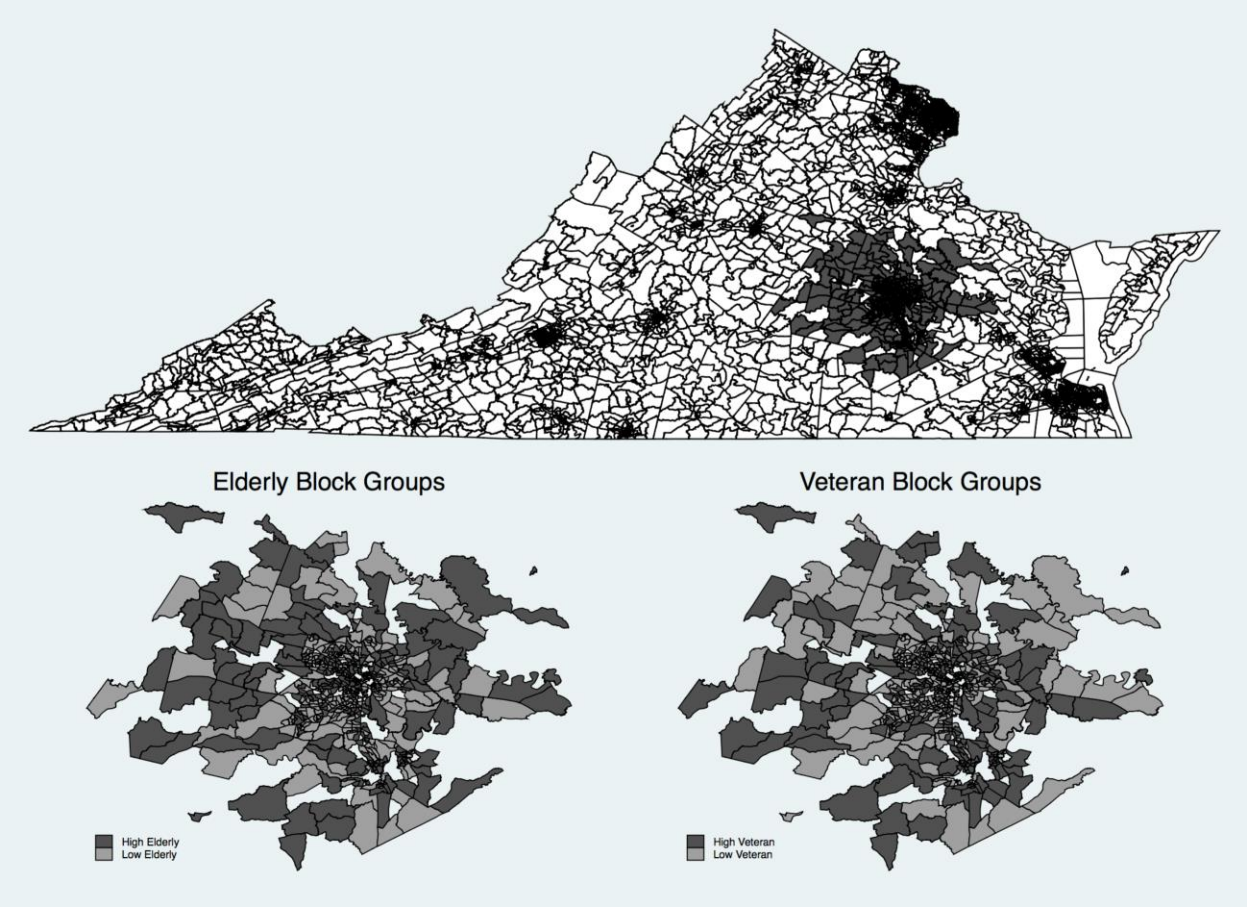
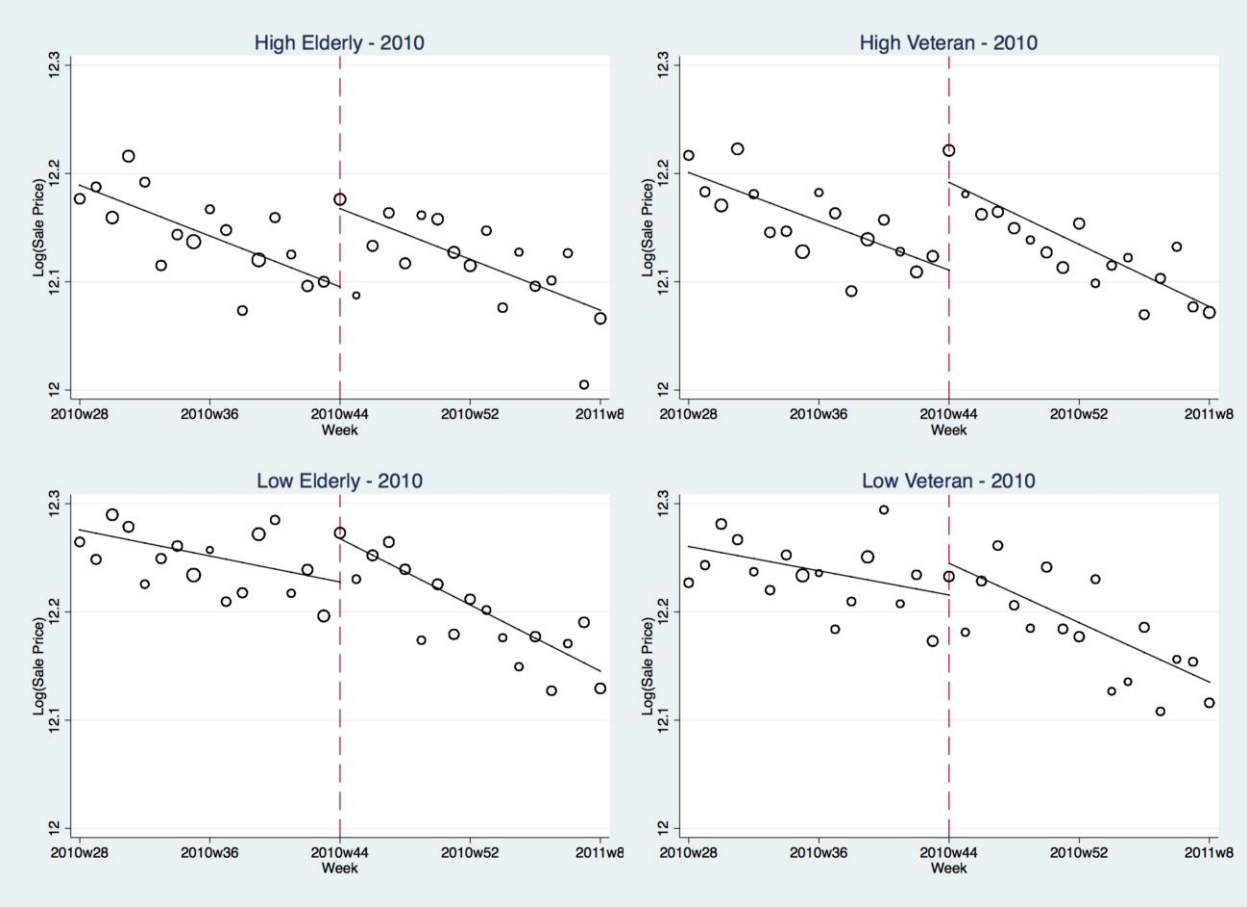
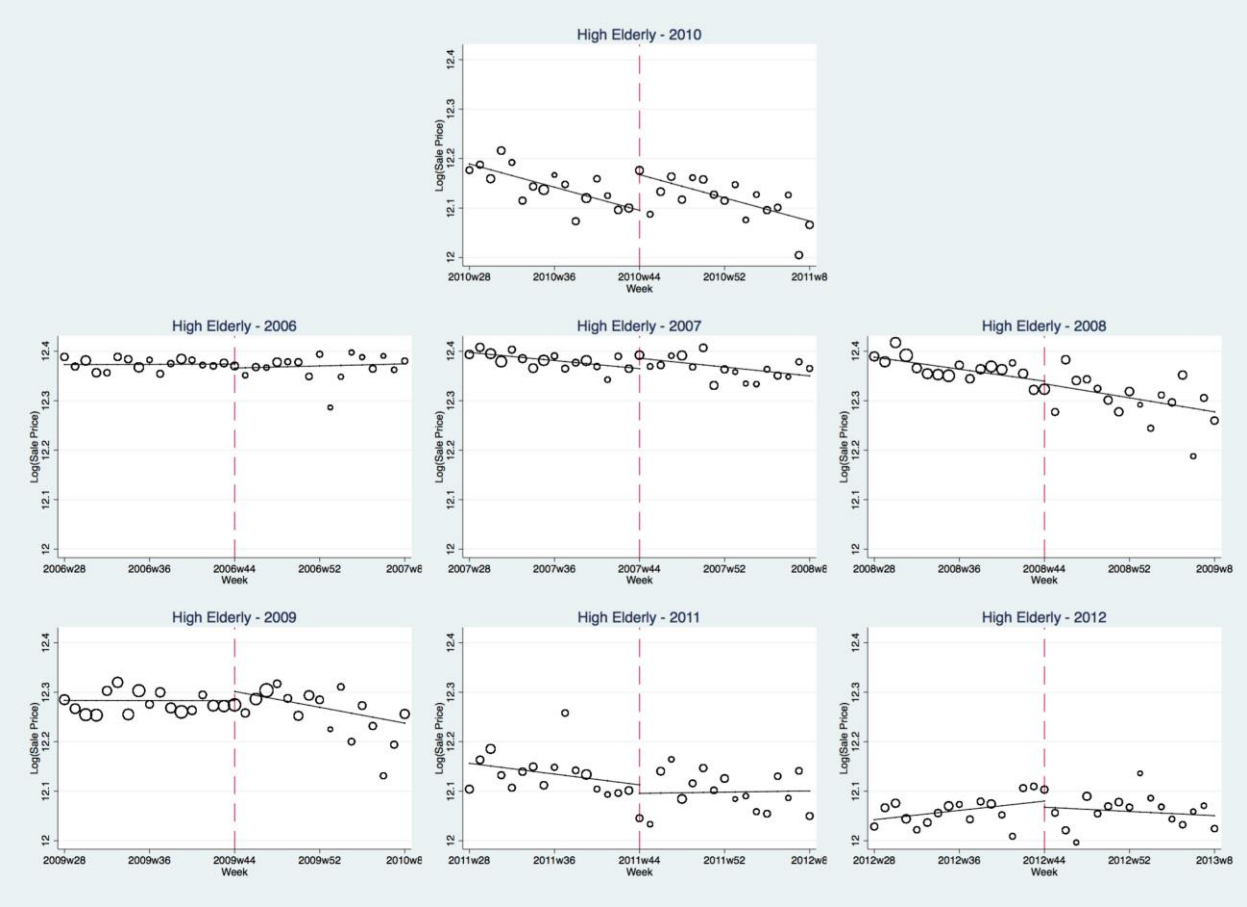


Figure 3: Main Results – Prices and Trends by Week



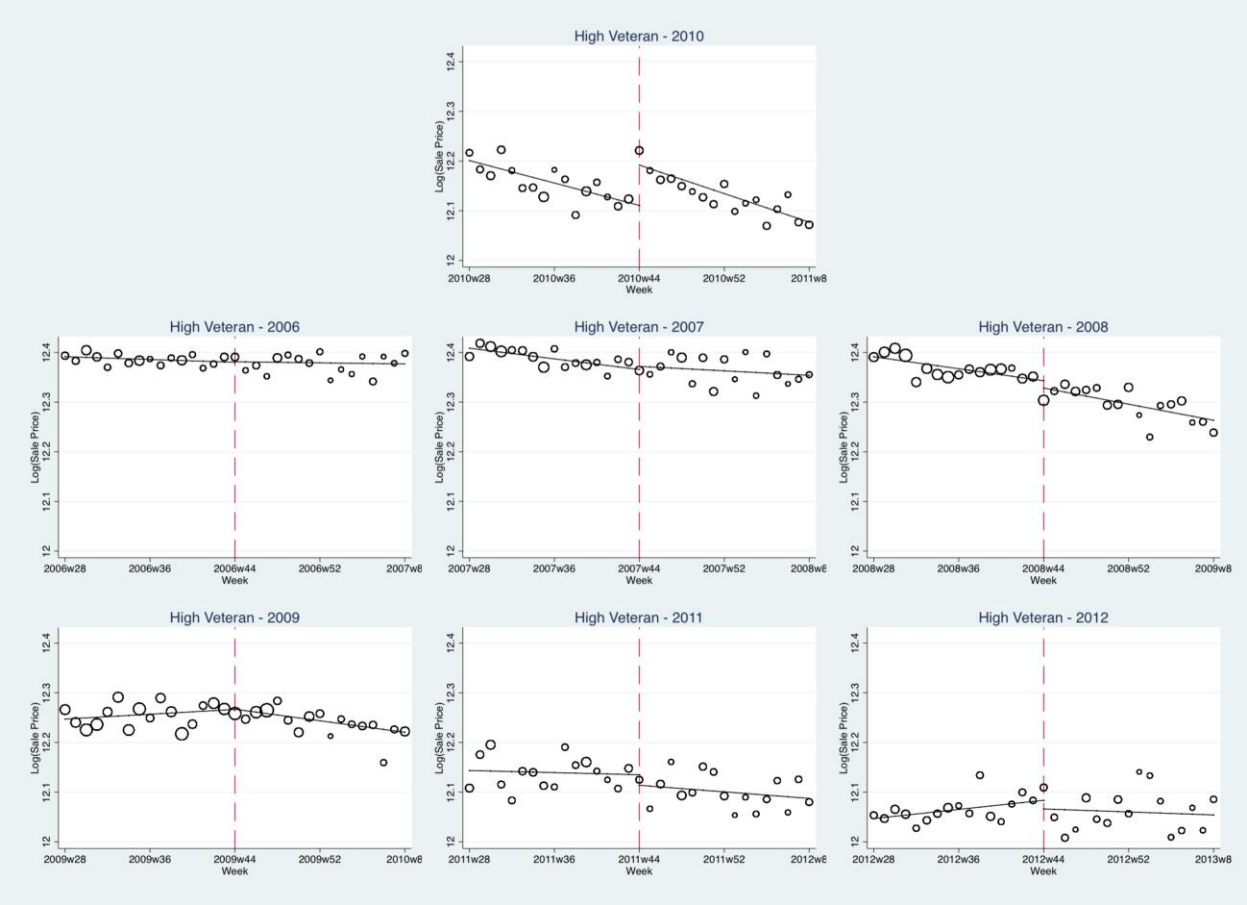
Source: Central Virginia Regional MLS.
Notes: Figure created using estimates in Table 2. Circle sizes correspond to number of homes sold or volume within the specified week.

Figure 4: All Years – Elderly



Source: Central Virginia Regional MLS.
Notes: Figure created using the high elderly estimates in Table 3. 2010 is the treatment year, with the other years serving as placebo tests for other election effects. Circle sizes correspond to number of homes sold or volume within the specified week.

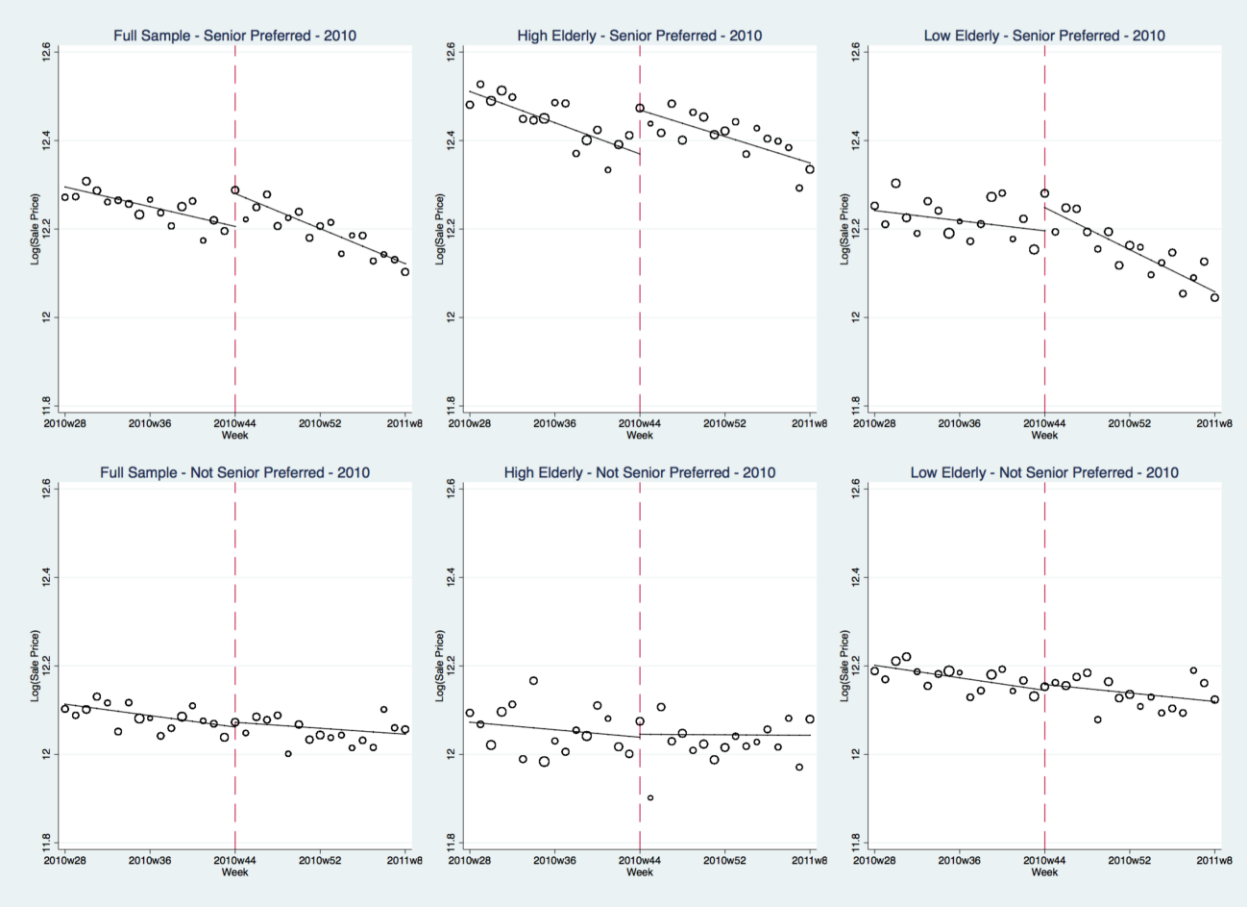
Figure 5: All Years – Veteran



Source: Central Virginia Regional MLS.

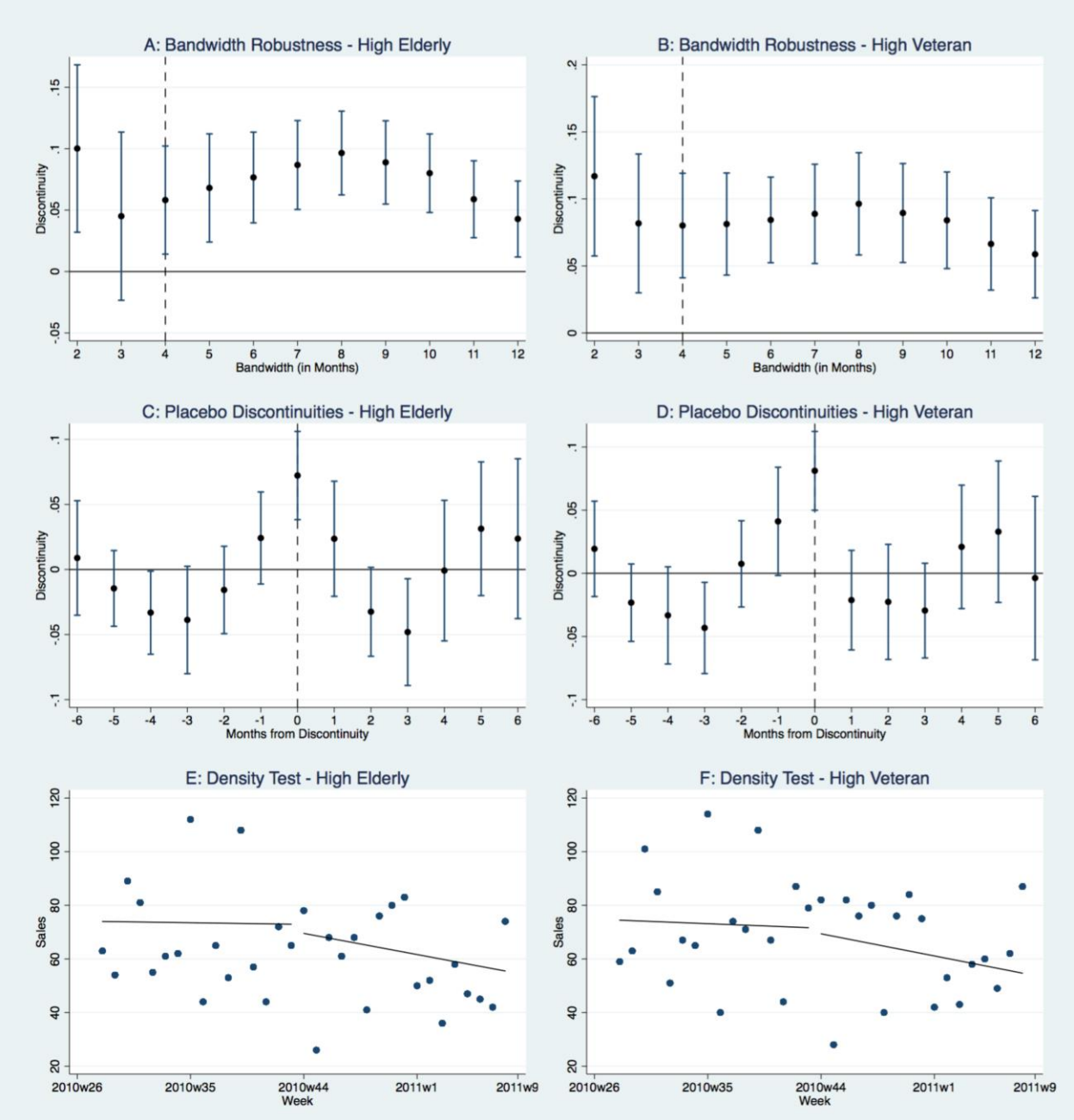
Notes: Figure created using the high veteran estimates in Table 3. 2010 is the treatment year, with the other years serving as placebo tests for other election effects. Circle sizes correspond to number of homes sold or volume within the specified week.

Figure 6: Senior Preferred Real Estate



Source: Central Virginia Regional MLS.
Notes: Figure created using estimates in Table 4. Circle sizes correspond to number of homes sold or volume within the specified week.

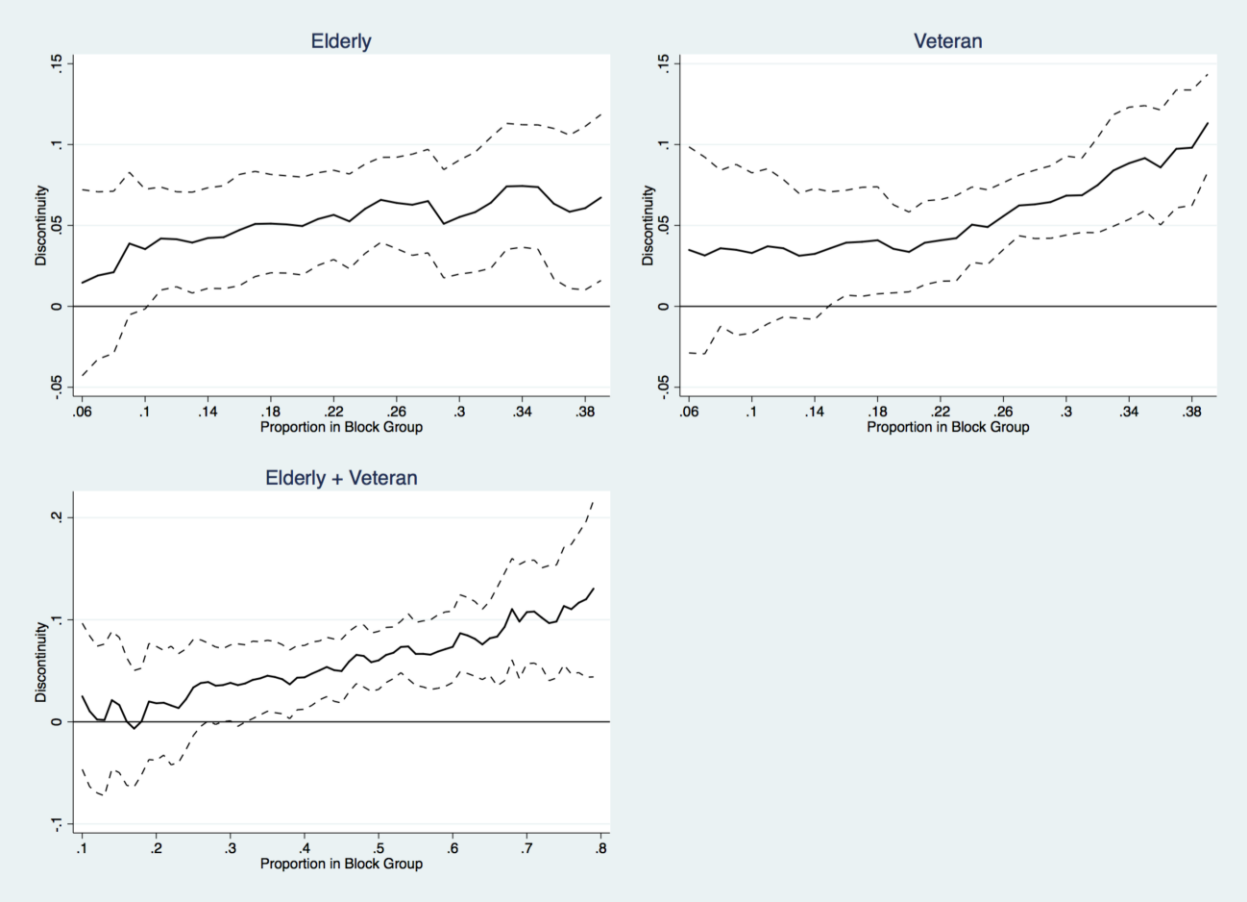
Figure 7: Robustness Checks



Source: Central Virginia Regional MLS.

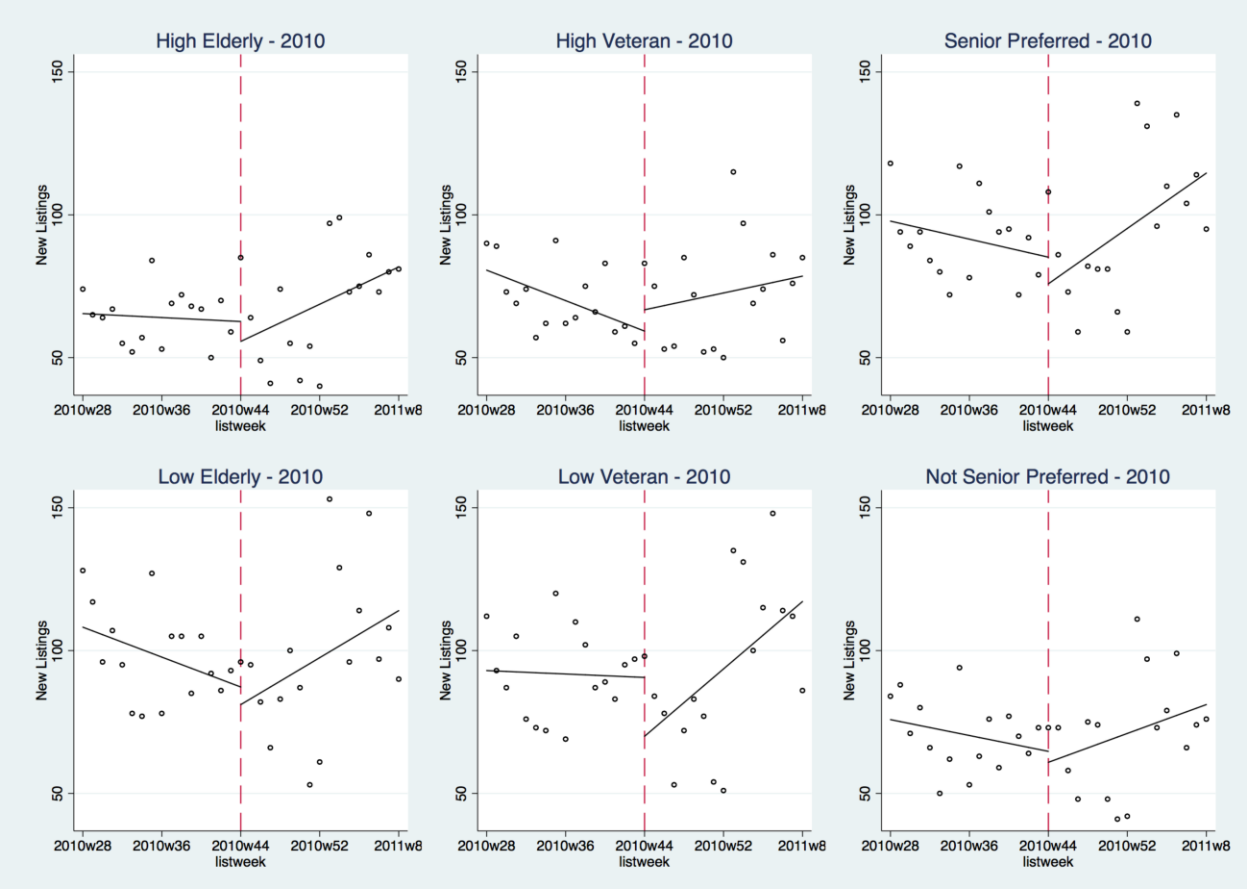
Notes: Panels A-D display the coefficient and 95 percent confidence interval for the discontinuity variable. Panels A-B are estimated using a dual quadratic model varying the size of the bandwidth (number of months included per side of the discontinuity). Panels C-D are estimated using a dual linear trend model with placebo discontinuities varying by x months on either side of the true discontinuity at 0. Panels E-F are from a dual linear trend RD model with the number of sales per sale week as the outcome to check for discontinuities in the running variable (McCrary, 2008).

Figure 8: Heterogeneous Treatment Effects



Source: Central Virginia Regional MLS.
Notes: Figures display the discontinuity coefficient and 95 confidence interval for varying samples that include a window (10% on each side) around the proportion of eldery, veteran, or elderly + veteran in the block group. Higher values on the x-axis indicate higher proportions of treated individuals in the block group.

Figure 9: Supply Dynamics and New Listings



Source: Central Virginia Regional MLS.
Notes: Regression discontinuity of new listings, stratified by elderly and veteran block groups and senior preferred homes.

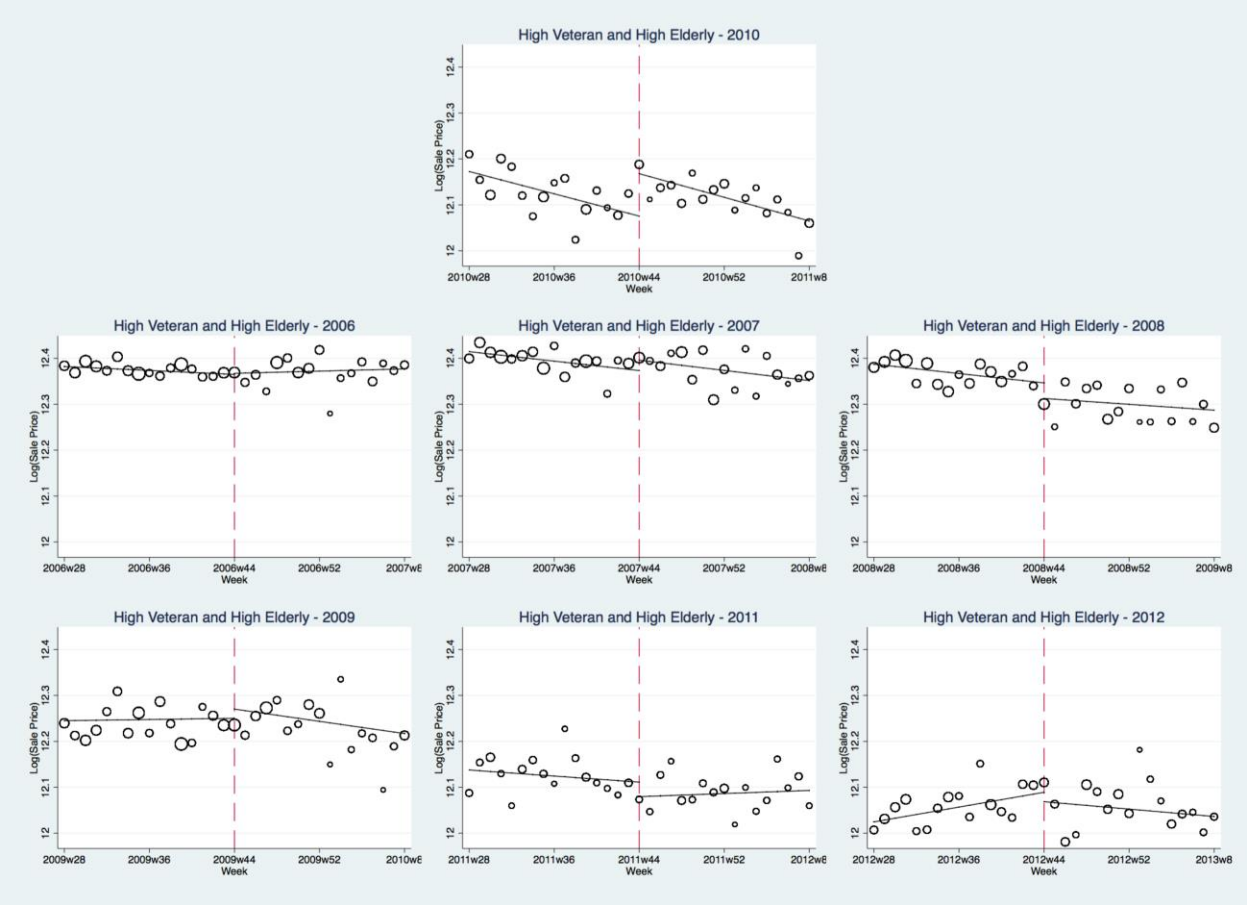
Table A1: Main Results – Showing Controls

	Full Sample	***High Elderly***	***Low Elderly***	***High Veteran***	***Low Veteran***
	(1)	(2)	(3)	(4)	(5)
Discontinuity	0.052***	0.072***	0.040***	0.081***	0.029
	(0.012)	(0.017)	(0.015)	(0.015)	(0.020)
Trend	-0.004***	-0.006***	-0.003**	-0.006***	-0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Post-Trend	-0.003**	-0.000	-0.005**	-0.002	-0.004*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Age	-0.004***	-0.004***	-0.004***	-0.004***	-0.004***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Square Footage	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of Bedrooms	-0.001	-0.006	0.003	-0.012	0.006
	(0.006)	(0.013)	(0.006)	(0.013)	(0.008)
Number of Bathrooms	0.036***	0.042***	0.032***	0.041***	0.034***
	(0.005)	(0.009)	(0.007)	(0.007)	(0.006)
Log (Acreage)	0.016***	0.008	0.022***	0.018**	0.012
	(0.005)	(0.008)	(0.006)	(0.007)	(0.008)
No Acreage	-0.028***	-0.008	-0.045***	-0.023*	-0.029**
	(0.009)	(0.014)	(0.013)	(0.012)	(0.012)
Vacant	-0.152***	-0.149***	-0.155***	-0.154***	-0.151***
	(0.007)	(0.011)	(0.008)	(0.009)	(0.010)
Basement	-0.002	0.020	-0.020	-0.012	0.009
	(0.015)	(0.019)	(0.020)	(0.020)	(0.018)
Condo or Townhouse	-0.021	-0.018	-0.020	0.009	-0.046***
	(0.014)	(0.023)	(0.018)	(0.023)	(0.015)
Garage Size (#Cars)	0.065***	0.066***	0.064***	0.064***	0.067***
	(0.004)	(0.007)	(0.006)	(0.006)	(0.006)
Log (Time on Market)	-0.006***	-0.003	-0.008***	-0.006	-0.006*
	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)
Constant	11.657***	11.619***	11.645***	11.641***	11.313***
	(0.084)	(0.088)	(0.063)	(0.084)	(0.096)
N	5,086	2,070	3,016	2,252	2,834

Source: Central Virginia Regional MLS

Notes: Estimates are from a two-trend regression discontinuity using a cutoff at week 44 and a bandwidth of week 28 in 2010 through week 8 of the 2011. Estimates are stratified by block groups containing different proportions of elderly (high elderly = treated) and veterans (high veteran = treated). This table includes the full range of coefficients for Table 2, aside from the block group fixed effects that are not shown. Standard errors clustered on sale week are in parenthesis. * 10%, ** 5%, *** 1%.

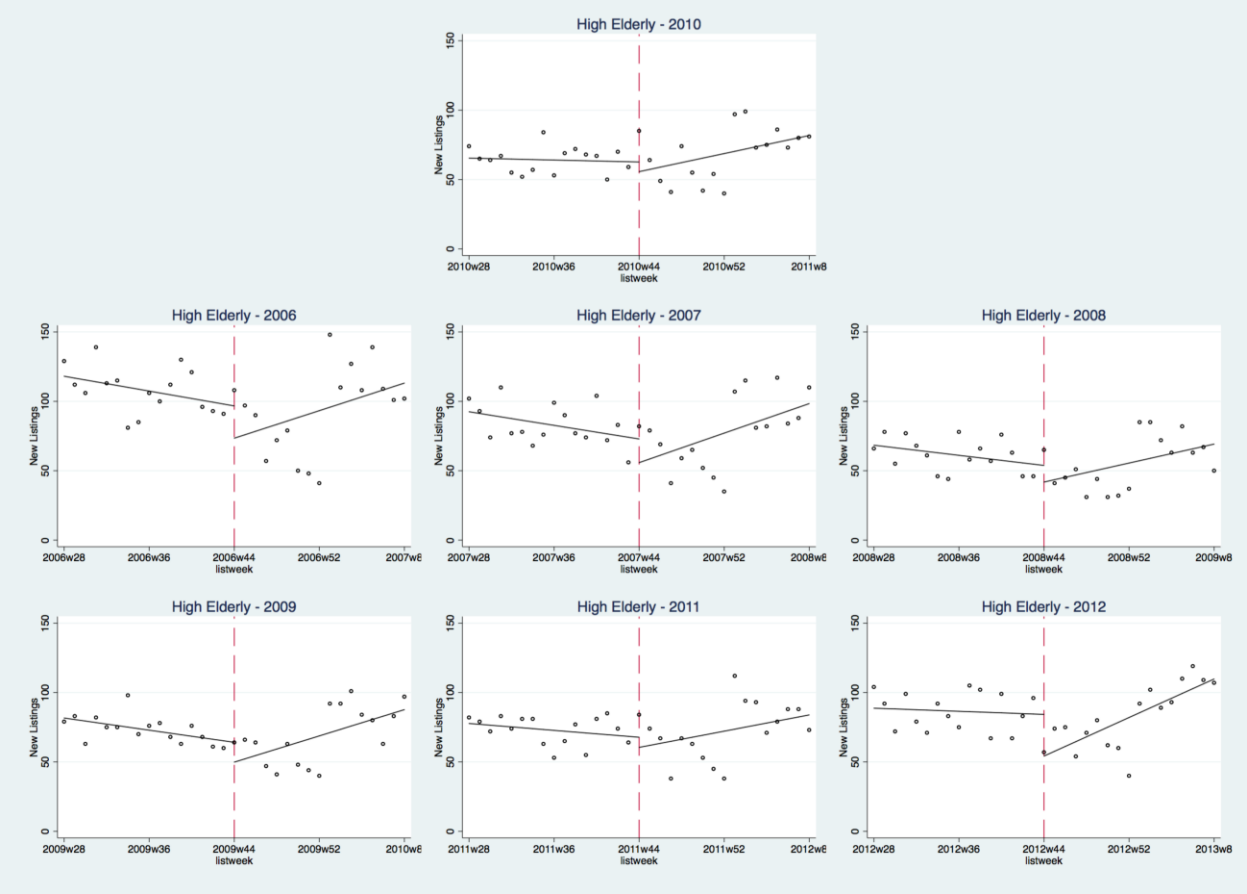
Figure A1: All Years – High Elderly & High Veteran



Source: Central Virginia Regional MLS.

Notes: Figure created using the combination of high elderly and high veteran stratification. Circle sizes correspond to number of homes sold or volume within the specified week.

Figure A2: New Listings in High Elderly – All Years



Source: Central Virginia Regional MLS.
Notes: Regression discontinuity of new listings in high elderly areas stratified by year to show that there is a common increase in the number of listings at the beginning of year.