Political Polarization Affects Households' Financial Decisions: Evidence from Home Sales*

W. Ben McCartney, John Orellana-Li, and Calvin Zhang†

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Abstract

Political identity and partisanship are salient features of today's society. Using deeds records and voter rolls, we show that current residents are more likely to sell their homes when opposite-party neighbors move in nearby than when unaffiliated or same-party neighbors do. This is especially true when the new neighbors are politically active, consistent with an animosity between parties mechanism. We conclude that affective polarization is not limited to purely political settings and affects one of the household's most important financial decisions, their home transactions.

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†McCartney: McIntire School of Commerce, University of Virginia, ben.mccartney@virginia.edu. Orellana-Li: The Graduate Center, City University of New York, jorellana@gradcenter.cuny.edu. Zhang: Federal Reserve Bank of Philadelphia, calvin.zhang@phil.frb.org.

1 Introduction

The importance of party affiliation for voter choice was discussed as early as Campbell et al. (1960), but its role in nonpolitical arenas was not seriously considered until Green et al. (2004) argued that "party identification is a genuine form of social identification." Today, households' political identities correlate with everything from their coffee preferences to their moral worldviews (DellaPosta et al., 2015). In consumer finance, for example, political affiliation shapes investor beliefs (Cookson et al., 2020), retirement portfolios (Meeuwis et al., 2018), and business formation decisions (Engelberg et al., 2022). Growth in the salience of political identity and partisan bias has changed the social fabric and led to an America where people, at least in surveys, loathe those of the other political party (Gentzkow, 2016; Iyengar et al., 2019; Iyengar and Westwood, 2015; Kalmoe and Mason, 2019; Mason, 2015, 2018).

A natural question is if this declared resentment between parties, a phenomenon called affective polarization, also influences real economic decisions. While extant work finds that affective polarization, distinct from partisan bias and partisan perception, matters for partners' marriage decisions and employers' hiring decisions (Colonnelli et al., 2020; Gift and Gift, 2015; Huber and Malhotra, 2017; Iyengar et al., 2018; McConnell et al., 2018), its effects in other financial arenas have remained relatively unexplored. We fill this gap with evidence that incumbent residents are more likely to sell their homes and move when new neighbors of the opposite political affiliation move in nearby, compared to new neighbors of the same or no affiliation.

Residential real estate, and home sales in particular, is an ideal setting to investigate the economic effects of affective polarization. First, nearly two-thirds of U.S. households are homeowners, and, for most of them, their home is their largest sellable asset (Campbell, 2006; Goldsmith-Pinkham and Shue, 2020; Gomes et al., 2021). Second, selling a home is costly, requiring homeowners to pay commissions (typically paid entirely by the seller), the costs of showing the home, and a number of fees and taxes (Haurin and Gill, 2002),[1] plus the additional search, emotional, and other transactions costs associated with finding a new place to live and relocating there. Third, the claim that animosity between parties drives Democrats and Republicans to move away from each other has been contentious since first hypothesized, and evidence either supporting or refuting the claim would be

[1]Average closing costs for sellers currently range from $17,000 to $22,000: https://www.zillow.com/sellers-guide/costs-to-sell-a-house/

of interest to researchers and policymakers alike (Ansolabehere et al., 2006; Bishop, 2009; Glaeser and Ward, 2006; McGhee and Krimm, 2009).

To identify whether or not political polarization affects house sales, we must overcome two empirical challenges. A naive analysis might observe political segregation in the cross section (Brown and Enos, 2021), and then conclude that households *must* prefer to live near co-partisans. However, households who value similar amenities choose to co-locate (Martin and Webster, 2020; Mummolo and Nall, 2017). We may, therefore, observe residents moving away from neighbors of the opposite party not because of a distaste for the neighbors themselves, as in Schelling (1969, 1971), but because neighbors with different political affiliations have different preferences for neighborhood amenities and the provision of public goods (Tiebout, 1956). The second empirical challenge is that nearest neighbors might not be random if households have already moved away from neighbors they dislike.

To address these challenges, we turn first to the CoreLogic Solutions Real Estate data, which contains information on real estate transactions and property characteristics. We then design an algorithm that uses complete addresses and precise geolocations to identify properties that are exactly next door to each other. Using homeowner names and addresses, we merge in individual-level political affiliation, voter history, and demographic characteristics from the North Carolina voter registration data (we discuss implications for generalizability in Section 4.3). Our final data set is a 2005 to 2019 quarterly panel of single family homes that details the home sale decisions and political affiliations of current residents and the political affiliations of their nearest neighbors.

We solve the problem of endogenous current neighbors by restricting our estimation sample to those incumbent-by-quarter observations that received a *new* neighbor either next door or two doors down (hereafter nearby). Our empirical strategy compares the home sale likelihoods of incumbents whose new nearby neighbor was affiliated with the *opposite* political party (the treatment group) to other current residents in the same neighborhood who *also* got new nearby neighbors, but whose new neighbor was *not* affiliated with the opposite political party (the control group). Our preferred specification includes a number of control variables – homeowner party, race, age, birth state, and tenure; an interaction between homeowner and neighbor race; and property size and age – and block group-by-quarter-by-party-by-race fixed effects. The fixed effects absorb the neighborhood-level reasons that current residents might choose to sell, leaving just the effect of the new neighbors. This test yields our headline result: Among current residents who got new nearby neighbors, those whose new neighbors are affiliated with the opposite political party are .31 percentage points, or 4 percent, more likely to sell their homes within two years than otherwise similar incumbents whose new neighbors are not affiliated with the opposite party.

The identifying assumption of our research design is that, conditional on the control variables and within the fixed effects cells, new neighbors are randomly assigned to current residents. To test the validity of this assumption, we compare treated and control residents along a number of observable dimensions. With respect to homeowner characteristics (year of birth, birth state, tenure at the residence, political activity, and down payment) and property characteristics (square feet, year built, and assessed value), we find that treated and control current residents and their homes are indistinguishable. Next, we show that average house prices on the blocks where treated households live move in parallel with those on the blocks where control households live. Finally, we show our results are robust to using just the most homogeneous block groups, where our identifying assumption is most likely to be valid.

Two potential threats to validity cannot be addressed directly with our main research design and require different approaches. The first such threat arises if the *hyperlocal* neighborhoods in which treated and control incumbents live are changing in a value-neutral way toward something that one party prefers. For example, consider a neighborhood where a new public transit stop is added. We might observe Republican incumbents who live near it being more likely to get new Democratic neighbors *and* themselves being more likely to leave. They would not be leaving because of the arrival of the new opposite-party neighbor, *per se*, but because they do not like living nearby a new transit stop. To address this and related concerns, we compare current residents on the same *block* who did *not* get a new nearby neighbor. By looking within blocks, we force both treated and control incumbents to be similarly exposed to the same new amenity or disamenity. Using this second strategy, we find that current residents who live either next door or two doors down from the new arrival are significantly more likely to sell than similar residents who live just slightly farther away on the same block.

A second concern is that incumbents might differ in ways that are unobservable and also correlated with their treatment assignment. For example, if especially unfriendly incumbents are both more likely to move away and less likely to have been welcoming to a same-party household shopping for a new home, then the results of our main research design would be biased. Our second research design can also suffer from this problem if an incumbent only has a house next-door go on the market in the first place because their unfriendliness drove away the prior neighbor. We address concerns of this nature with a third research design that includes an arriver-event fixed effect. Specifically, we define a quasi-experiment as the arrival of a new neighbor and the relevant sample as the eight nearest incumbents (two on either side and four across the street). We find that, within those eight nearest neighbors, the move out rates of opposite-party incumbents are significantly higher than those of unaffiliated incumbents or incumbents who share a party affiliation with the new arrival. Another advantage of this test is that, like our second strategy, it controls for very local trends and neighborhood characteristics. Three different research designs, along with a battery of other robustness tests, consistently point to incumbents moving away from new neighbors of the opposite party because of the new neighbors themselves, and not what their arrival might be correlated with.

Next, we explore how the estimated effect varies over time, both relative to the arrival of the new neighbor and in the time series. We begin by analyzing the timing of move-outs and document that not until more than a year has passed do the move-out rates of treated and control residents begin to diverge significantly. This delayed effect is consistent with our observed response being caused by something about the new neighbor, in particular, since it takes time for current residents to learn they dislike their new neighbors and then sell their homes. Next, we show that the estimated effect is relatively stable over the time series. A noteable exception is during the recession when rates of home selling fell significantly. Motivated by this time series evidence, we investigate how sensitive the main effect is to local house market liquidity. We find much larger effect sizes in hot markets than cool ones, both over time and across neighborhoods. Since round-trip transaction costs are likely lower in hot markets due to smaller bid-ask spreads and faster resolutions of uncertainty, we interpret this finding as evidence that, while households prefer to not live near opposite-party neighbors, the amount they will pay to realize this preference is not unlimited. Importantly, when we look at a longer time horizon, we find that cool markets just *delay* the move-outs of treated households.

In the final section of the paper, we discuss potential drivers of our main result. We present evidence consistent with an affective polarization mechanism, that is, with households loathing those of the opposite party and not wanting to live near them. While we cannot be certain that affective polarization is the only mechanism at play – incumbents who get new opposite-party neighbors might become more pessimistic about their neighborhood's trajectory or learn from them and experience a shifting of preferences – we present several pieces of evidence especially consistent with animosity between parties driving our result. We begin by pointing to recent surveys which show, for example, that 28% of Americans would be somewhat or very upset if their child were to marry someone from the opposite party[2] and 15% of respondents answered "yes" when asked, "Do you think we'd be better off as a country if large numbers of [opposing party] in the public today just died?" (Kalmoe and Mason, 2019). Next, we use the voter history file – a publicly available data set detailing all the elections each registered voter participated in – to classify how politically involved households are. We use participation as a proxy for how partisan households are since those who are the most likely to participate in elections are also those who hold the most antagonistic views of the other party.[3] Consistent with an affective polarization mechanism, we find that households are especially likely to sell when the opposite-party neighbor who moves in nearby is also highly partisan.

An important limitation of all questions about the importance of affective polarization, those asked by journalists and academics alike, is that political party, views on immigration, expectations about economic growth under different governments, and vehicle preferences are strikingly correlated (DellaPosta et al., 2015). Our research design means we *can* rule out those differences between parties that influence public good provision and local private amenities, but we are nevertheless limited in how precisely we can pinpoint which aspects of the neighbor's partisan identity affect current residents. Our central contribution *takes as given* that Americans increasingly loathe those affiliated with the opposite party, setting aside why exactly this is the case, and asks if this has real economic consequences. Future work will need to explore more deeply what it is about being members of the other party that makes parents not want them as in-laws, bosses not want them as employees, and households not want them as neighbors.

Related Literature: Our paper contributes to a rapidly growing literature in finance and politics. One part of this literature investigates how political identity and partisanship affect households' financial decisions via a partisan bias channel. Partisan bias is distinct from affective polarization in that partisan bias refers to how economic agents' interpretations and perceptions are influenced by

[2] Source: https://docs.cdn.yougov.com/t0hi1tcqs5/econTabReport.pdf

[3] Source: https://www.pewresearch.org/politics/2014/06/12/section-5-political-engagement-and-activism/

their personal politics, while affective polarization is best thought of as a consequence of increased partisan bias (Iyengar et al., 2019). This literature finds that partisan bias does indeed affect households' stock market participation decisions, portfolio choices, expectations, and returns (Bonaparte et al., 2017; Cookson et al., 2020; Kaustia and Torstila, 2011; Ke, 2020; Kempf and Tsoutsoura, 2021; Meeuwis et al., 2018). Gerber and Huber (2009) document that partisan bias affects consumption. Mian et al. (2021), on the other hand, find no effect on spending, but do show that partisan bias affects expectations and optimism, which have been shown to affect economic choices (Puri and Robinson, 2007). Not surprising, then, is new evidence that other major choices households make like starting businesses (Engelberg et al., 2022) and families (Dahl et al., 2021) are also a function of their political beliefs. We contribute evidence that affective polarization, distinct from partisan bias, affects the trading of assets. In so doing, we illustrate a new way that politics enters into households' financial decision making, via affective polarization. Furthermore, we expand the research agenda into a new domain, residential real estate.

Most related to this project is a recent paper, Bernstein et al. (2022), that investigates how partisan differences in views on climate change shape households' residential location decisions. Their careful analysis shows that Republicans, who in general are more skeptical than Democrats of climate change, are more likely to own homes threatened by rising sea levels. Our work complements theirs in three ways. First, instead of looking at location decisions in equilibrium, we directly examine the households' decision to move. Second, our work considers more explicitly the importance of affective polarization, a different phenomenon than the partisan bias explored by Bernstein et al. (2022). And finally, by focusing especially on partisan bias, Bernstein et al. (2022) show how politics might operate through a Tiebout sorting mechanism – where sea level rise can be thought of as a disamenity – while ours investigates the possibility that affective polarization might induce moves in line with Schelling preferences for nearest neighbors.

We also contribute to the large emotional trading literature that has documented many non-cash flow reasons households buy and sell assets (Shefrin and Statman, 1985). These reasons include, for example, regret (Frydman and Camerer, 2016; Strahilevitz et al., 2011), loyalty (Cohen, 2009), attachment (Bhutta et al., 2017), stigma (Guiso et al., 2013), and anxiety (Eisenbach and Schmalz, 2016). Our evidence suggests that antipathy towards outsiders is another non-cash flow motivator for the trading of financial assets. This is important for two reasons. First, correctly designing social insurance programs and financial regulations with an eye to household welfare requires a deep understanding of why households make the trading decisions that they do (Beshears et al., 2018).

And, second, households' real estate decisions have consequences for their own consumption, savings, and labor market choices and also for the broader economy (Adelino et al., 2015; Bernstein et al., 2021; Brown and Matsa, 2020; Mian et al., 2013; Piazzesi and Schneider, 2016). In particular, **?** show that households increase their spending, especially on home-related durables and home improvement, in the months around home purchases. Dutch data and data from the Panel Study of Income Dynamics analyzed by Bernstein and Koudijs (2020) suggest this excess spending is not out of home equity since households do not re-lever significantly upon moving. Taken together with our results, we can conclude that affective polarization likely affects households' consumption and savings decisions via induced home sales.

Ours is not the first paper to question the role that preferences for neighbors' political affiliations and partisan identities might have on home buying and selling. Motivated by high levels of observed political segregation (Bishop, 2009; Brown and Enos, 2021) and evidence that affective polarization affects who people choose to work for, hire, and marry (Colonnelli et al., 2020; Gift and Gift, 2015; Huber and Malhotra, 2017; Iyengar et al., 2018; McConnell et al., 2018), a number of studies in political science, geography, and sociology have investigated the possibility that affective polarization affects residential choice. Survey evidence finds that households do indeed voice a preference for living near co-partisans (Gimpel and Hui, 2015, 2017; Mummolo and Nall, 2017). However, evidence from surveys and even low-stakes field experiments might just reflect "expressive voting" or "political cheerleading" (Bullock et al., 2015; Prior et al., 2015), and, ultimately, an empirical investigation is required.

Two papers use ZIP code level data to conduct such analyses. Mummolo and Nall (2017) find that surveyed households do voice a preference for communities with a higher share of co-partisan neighbors but that this preference is of secondary importance. Consistently, their empirical analysis finds that partisans who move across ZIP codes do not choose destination ZIP codes with higher shares of co-partisans than their origin ZIP codes. Using a different approach, Martin and Webster (2020) compare Democrats and Republicans arriving in the same ZIP code and ask if Democrats choose census tracts within those ZIP codes that have higher shares of registered Democrats. They find that this is the case, but at magnitudes too small to explain current levels of political segregation. A strength of both Mummolo and Nall (2017) and Martin and Webster (2020) is that households can be followed as they move, necessary for estimating the importance of household mobility on political segregation. But the large size of ZIP codes and census tracts makes it challenging to disentangle preferences for nearby neighbors' politics with preferences for neighborhood amenities, like commute times, school quality, and affordability, that may be correlated with neighbors' politics.[4] Our data set, while limited in its ability to track households, precisely geolocates households and their neighbors allowing us to implement a research design that fixes neighborhood amenities, while leaving variation in neighbors' politics. We can therefore speak to the importance of neighbors' partisan identities, all else equal, in a way that extant work can not.

A full understanding of what explains political segregation requires not only tracking voter migration, but also demographic changes (especially older voters leaving the electorate and younger voters joining), and voters changing parties. The data set and research design used in this paper are therefore not well-suited for explaining levels of political segregation. Another related paper, Brown et al. (2022), uses national data to estimate that residential mobility explains roughly one sixth of political segregation, but their methodology, like that in Mummolo and Nall (2017) and Martin and Webster (2020) cannot disentangle moves motivated by neighbors' identities and moves motivated by preferences over amenity bundles. What we contribute to this ambitious literature on political segregation and its causes is new reduced-form evidence that nearby neighbors and their political identities affect residential choice via home sales.

2 Data Description

2.1 The North Carolina Voter Data

We use voter registration data from the state of North Carolina to classify households' political affiliations. The data set is free, available to the public and, unlike the voter data in many other states which describes just those *currently* registered to vote, available in snapshot-form going back to 2005.[5] In other words, we can observe everybody registered to vote at many specific points in time,

[4]The average ZIP code in North Carolina has a population of roughly 12,500 people and the average Census tract approximately 4,500.

[5]These data can be found online at https://dl.ncsbe.gov/index.html.

typically before major elections. The North Carolina State Board of Elections (NCSBE) includes in the voter registration data not only the full name of each person registered to vote, but their complete mailing address, age, race, sex, and state of birth. Furthermore, as these data are the official record of people eligible to vote, data entry errors are rare.

We classify voters as members of a party as follows. If a voter is registered with a particular party we say she is a member of that party. If a voter is officially unaffiliated with any party, but we see her vote in one party's primary elections and only the primaries of that party, we say she is a member of that party. Voters we never see affiliated with a party and who never vote in any primary are classified as unaffiliated. Finally, voters who are affiliated with more than one party over the time series are classified as multi-party.

2.2 The North Carolina Deeds Data

We supplement the North Carolina voter registration data with publicly available assessor and deeds data obtained from the CoreLogic Solutions Real Estate data set. This data set contains information on both transactions and home characteristics for all houses in the most populous North Carolina counties (covering more than 90% of the state's population). Key variables we observe include precise site address; latitude and longitude coordinates of the property; transaction date and type; names of owners; year built; and building square footage. We merge the deeds data with the North Carolina voter registration data by owner name and address in order to create a quarterly panel at the parcel level. We make the assumption that the owner of each *matched* home is a resident of the home. This is a largely innocuous assumption since voters are registered at only one location in North Carolina at a time. Non-person owners such as investment companies, banks, and trusts will never merge with the voter registration data so they will never be in our estimation sample.

2.3 Describing the the Final Sample

With this merged data set, we observe the owners of every property at the beginning of each quarter. We use the party affiliation from the North Carolina voter registration data to assign a political party to each home. For two-person households, we make some adjustments. Households where one owner is unaffiliated but the other owner is affiliated with a party are assigned to that party. We also classify homes as multi-party if the two owners are registered in opposite parties or if one of the owners is multi-party. For the purpose of this study, we drop multi-party homes from our sample, as we cannot unambiguously assign them to one of the two main parties, nor deem them unaffiliated.

We then identify the nearby neighbors for every parcel in North Carolina. Each household can have up to two next-door neighbors and up to two two-doors down neighbors, but may have zero or one of each. We start by using address conventions in the state of North Carolina. If two households are on the same street and have consecutive even or consecutive odd house numbers then we conclude that they are next door. The algorithm allows, for example, for 4100 and 4104 to be next door if no 4102 exists. We further require that two homes be within 0.10 miles to qualify as nearby.

The full merged sample includes over 44 million property-by-quarter observations between 2005 and 2019 in North Carolina.[6] The full sample covers nearly 5,000 unique census block groups, each with an average of just over 100 households.

To conduct our new nearby neighbor analysis, we restrict the sample to current resident-by-quarter observations where the current resident got a new nearby neighbor (either next door or two-doors down). The typical current resident is in the sample just one time, at the precise quarter when she got a new nearby neighbor. We also require that the new neighbor be in the merged sample so we can observe their political affiliation and race. In our main new-neighbor-experiment sample, we omit households who moved away in the same quarter or quarter immediately following the arrival of the new neighbor. Omitting these households does not meaningfully alter the results (indeed, we investigate these "immediate movers" separately in their own test), but the exclusion of current residents who moved out in the same or next quarter is more consistent with examining the effects of the new neighbors themselves, since the selling process often takes months from start to finish.

We also drop blocks with registered voter populations under twenty (since random neighbor assignment is less likely),[7] current residents who have not yet lived in their homes for a year (since move-outs from this group are more likely to be the result of some unforeseen emergency), and current residents who have not yet lived through an election (since neighbor partisanship is usually more salient around elections).[8] None of these sample restrictions drops many households and we

[6]When we say quarter we mean year-quarter. For example, 2009Q1 corresponds to the days between January 1, 2009 and March 31, 2009.

[7]Our Census block level covariates are constructed using all registered voters found in the NCSBE voter files, which includes renters, while the "households" we refer to are homeowners from the CoreLogic Solutions Real Estate who we successfully match to the North Carolina voter registration data set. Households frequently contain more than one registered voter.

[8]To isolate the effect of a particular new neighbor, we also drop observations where a current resident gets more than

confirm in robustness tests that they do not meaningfully affect our results. Finally, we drop the last two years in the data because our outcome variable requires that we observe current residents for at least two years following the arrival of the new neighbor. Table 1 summarizes this sample.

[TABLE 1 HERE]

Our main sample is composed of 405,731 current resident-by-quarter observations where the current resident got a new nearby neighbor. On average, 7.79% of these current residents will have moved before two years has passed. We define opposite party neighbor as a dummy equal to one if the current resident is Democratic and the new nearby neighbor is Republican or vice versa. If either the current resident or the new neighbor is unaffiliated with either major party, we say that the two households are not opposite party. As defined, 25% of our sample got new neighbors affiliated with the opposite party.

Our data set has two advantages. First, identifying the effects of home sale decisions made because of an aversion to nearby neighbor, as in Schelling (1969), separately from home sale decisions made because of preferences for neighborhood characteristics, as in (Tiebout, 1956), almost by definition requires parcel-level data.[9] That is, in order to control for the effects of amenities, and thus absorb relocation decisions made for Tiebout-style reasons, we need to include very fine geography-by-time fixed effects.[10] This consequently necessitates treatment assignment to occur at a level *more* granular than local neighborhood, regardless of what specific geography is being used to define neighborhood. Second, our long time series means we can observe new households arrive and current residents leave. This is important since households with strong preferences for nearby neighbors have likely already moved away and any purely cross-sectional test will therefore be difficult to interpret.

one new nearby neighbor in the same quarter. This drops just over 1% of the pre-estimation sample.

[9] An influential body of work has investigated the role of large-neighborhood neighbors, where households might be said to have thousands of neighbors, on relocation (Bayer et al., 2014, 2007; Bayer and McMillan, 2012; Boustan, 2010; Card and DiNardo, 2000; Card et al., 2008; Cutler et al., 1999; Wong, 2013). And while the importance of the very nearest neighbors for home sales has long been assumed (Schelling, 1969), it has, to the best of our knowledge, lacked the evidence necessary to disentangle its effects from those of neighborhood characteristics and amenities (Tiebout, 1956). An exception is contemporaneous work that uses a strategy similar to ours to investigate how neighbor race influences home sale decisions (Bayer et al., 2022).

[10] A number of neighborhood-level amenities affects households' relocation decisions including school quality (Black, 1999), criminal activity (Linden and Rockoff, 2008), pollution (Banzhaf and Walsh, 2008; Bayer et al., 2009), and access to public transportation (Glaeser et al., 2008).

3 The Identification Strategy

When trying to understand the importance of neighbors for households' home sale decisions, a natural starting point is to look at the characteristics of households' current neighbors. If households tend to live near people who look or behave like them, then we might conclude that households actively prefer to live near similar people. But this naive analysis is plagued by two serious endogeneity concerns. One concern is that current residents with very strong preferences for neighbor-type may have already moved away such that, at any point in time, a households' nearest neighbors may not be randomly assigned, even conditional on very local geographies. We solve this problem by using the arrival of new neighbors as a quasi-experiment. A second concern is that the demographics of a neighborhood's residents are highly correlated with the bundle of local amenities. Households may choose to live near people who look and behave like them, not because of those shared characteristics or beliefs, per se, but because they have common preferences for public goods, like public school quality or police spending. We solve this problem by zooming in, past the level of local amenities, all the way down to the parcel level. In this way, we compare current residents who share the *same* bundle of local amenities, but whose nearest neighbors have *different* characteristics.

To solve the two key problems, we design a novel, new neighbor approach. This strategy compares two current residents living in the same census block group who both got new nearby (next-door or two-doors-down) neighbors in the same quarter. The new neighbor component solves the endogenous current neighbors problem and the shared neighborhood component solves the problem of endogeneity between neighbors' characteristics and the provided amenity bundle. Our hypothesis is as follows: If affective polarization affects real financial decisions, then current residents whose new nearby neighbors are affiliated with the opposite party will be more likely to sell their homes than current residents whose new neighbors are not. Specifically, we estimate the following equation:

$$\begin{aligned}\text{Sell Next Two Years}_i = \beta \times \text{New Neighbor Opposite Party}_i \\ + \text{Controls}_i \times \Theta + \eta_{group \times quarter \times party \times race} + \epsilon_i, \quad (1)\end{aligned}$$

where *Sell Next Two Years*$_i$ is an indicator variable (= 100) if household i sells their home in the two years following the arrival of the new nearby neighbor. *New Neighbor Opposite Party*$_i$ is a dummy (= 1) indicating that household i's new nearby neighbor is affiliated with the opposite party. We control for the homeowner's age, birth state, race, party, and residential tenure; the size and age of their residence; and whether the new neighbor's race is different than theirs. In some specifications we further control for the current resident's down payment, a variable which is missing for more than half of the sample (the half who moved in prior to when coverage in the deeds data begins).

Since move-out rates vary across time and space, we must include geography and time fixed effects in our model. Furthermore, some amenities might have different effects on people affiliated with different political parties. For example, access to public transportation might be particularly appealing to people affiliated with the Democratic party. This introduces a potential source of bias. If current residents' move-out rates vary by political party in ways that are correlated with the arrival of new nearby neighbors of differing politics, then using only a geography-by-time fixed effect is insufficient. A similar concern arises if our model uses white residents as controls for Black residents. Fortunately, the richness of our data allows for a simple solution: We include in all specifications a group-by-quarter-by-party-by-race fixed effect.

The inclusion of these fixed effect cells allow us to rule out neighborhood-amenity explanations for move-out rates. Households living in the same census block group are exposed to similar amounts of pollution and have similar access to public transportation. Neighborhood gentrification, or other neighborhood changes that have a common effect on the move-out rates of people living in the neighborhood, will therefore be absorbed. All together, our effect of interest is estimated using only variation in the move-out rates of current residents in the same neighborhood, at the same time, affiliated with the same party, and of the same race, but whose new, quasi-randomly assigned neighbors have different political affiliations, all while conditioning on a battery of control variables.

We thus disentangle the effect of neighbors, per se, from the effects of neighborhood-level amenities and demographics. A trade-off of this strategy is that some neighborhoods never conduct one of the quasi-experiments implied by our model. The test requires, first, a mix of Democrats and Republicans to be currently residing and moving into the neighborhood and, second, that these residents be owner-occupants, not renters. Therefore, neighborhoods that are either already highly politically segregated or predominately inhabited by renters are not included in the final sample.[11] This limi-

[11] Figure A1 shows which block groups in North Carolina ever had two current residents of the same party and race who

tation means our test cannot estimate the effect of an opposite-party neighbor in perfectly politically segregated neighborhoods since, mechanically, we observe no "experiments" there.

4 Main Results

4.1 The Main Result

[TABLE 2 HERE]

We present the main result of this paper in Table 2. We start in column (1) with the full sample and simplified research design. Among the full sample of current resident-by-quarter observations, we see that the average current resident has a 6.99% likelihood of selling their home in the next two years. We then regress the home sale decision on a dummy equal to one if the current resident has at least one nearby neighbor affiliated with the opposite party. We find a huge difference in the likelihoods of selling between current residents that have an opposite-party neighbor living nearby (in one of the two homes on either side) and those that do not. Specifically, these residents are 1.018 percentage points, or 14.6%, more likely to sell within the next two years. But as discussed in previous sections, this estimate is biased in two ways.

In column (2), we address the first source of bias. Current residents might be moving away from opposite party neighbors, not because of a distaste for the neighbors themselves, but because having opposite party neighbors is correlated with an imperfect local basket of provided amenities. To absorb moves for these reasons, we include a group-by-quarter-by-party-by-race fixed effect. Column (2) thus compares two similar current residents living in the same neighborhood at the same time. Again, we find that current residents with opposite party neighbors are significantly more likely to sell within two years. However, current nearest neighbors are potentially *not* random if households with strong preferences for who their nearest neighbors are have already moved in order to try and realize their preferences.

In column (3), we limit the sample to just those incumbents that got new neighbors and remove the fixed effects. Estimating this model produces an estimate that is likely biased upwards for the same reasons as the estimate in column (1). In columns (4) and (5), we add the fixed effects back in.

both get new nearby neighbors, one of whom is the opposite party as the current resident and one of whom is not, at the same time.

In column (5), we see that current residents treated with a new opposite-party neighbor are 0.251 percentage points, or 3.2%, more likely to sell their homes than otherwise similar current residents who, at the same time, also got a new neighbor but one not of the opposite political party. Our identifying assumption is that, by conditioning on granular fixed effects, assignment to the treatment is random. If this were the case, and given a large enough sample, the inclusion of control variables would not matter. This is exactly what we find. Controlling for a number of variables hardly moves the estimate at all, as shown in column (6) (estimates are presented in detail in Table A1). In this case, current residents who get a new nearby neighbor of the opposite party are 0.314 percentage points or 4.0% more likely to sell their homes within two years.

In our final specification, presented in column (7), we consider the importance of negative equity and housing lock (**?**). While we do not observe current residents' month-to-month equity positions, we do observe, for a subsample, their down payments. When we include four different buckets of CLTV at time of purchase, we document an effect statistically similar in magnitude to that of our preferred estimate in column (6). We show, however, in column (8) that the difference between the preferred specification and the one that includes CLTV is dirven entirely by changes to the sample. Only incumbents who have moved in since coverage began have non-missing CLTVs at the time of purchase in the data. Our interpretation of the difference between (6) and (7), therefore, is that incumbents who have lived in their current homes for a long time (since at least 2004) are less likely to leave, for any reason.

4.2 Implications and Economic Importance

An effect size of 0.314 percentage points is significant since selling and then moving is extraordinarily costly. Selling alone costs an average of $20,000 and moving costs, while variable, are also expensive. Posting on social media and responding to surveys is cheap. But finding an effect in our high-stakes context is striking evidence that, on average, residents derive strong disutility from living near people affiliated with the opposite party.

In this section, we estimate the total move costs paid by incumbents selling their homes and moving away from nearby neighbors affiliated with the opposite party. We start by looking at column (6) of Table 2. Between 2005 and 2017, 405,731 current residents are involved in a quasi-experiment. That is, 405,731 incumbents live in block groups heterogeneous enough with respect to political affiliation that they and another incumbent of the same political party and the same race both received new nearby neighbors, at least one of whom was affiliated with the opposite party. Of this sample, 24.63%, or 99,932 incumbents, were treated. The baseline move-out rate of these 405,731 incumbents was 7.79%, and treated incumbents were .314 percentage points more likely to move-out than control incumbents. Our most conservative estimates imply, therefore, that receipt of opposite-party neighbors caused 341 incumbents to sell their homes.[12] Estimating closing costs of $20,000 per sale this translates to nearly $7,000,000 in financial transactions costs, not including emotional and mental costs, paid by incumbents in order to move away from opposite-party neighbors. We can also frame the aggregate effect with respect to total in-sample move-outs. We observe 31,606 moves of which 341 were treatment-induced and therefore estimate that 1.1% of all moves are a causal reaction to the arrival of opposite-party neighbors.[13]

There are at least two reasons this estimated aggregate effect likely underestimates the magnitude of affective polarization's effect on residential choice. First, our experiments occur in relatively purple neighborhoods. It could be, for example, that the arrival of a very outspoken Republican to a predominantly Democratic neighborhood might induce many incumbent move-outs. But if that arrival never occurs in the data, because the most intense partisans avoid purple neighborhoods in the first place, then we cannot estimate its impact. Second, using same-block group incumbents' move-out rates as a baseline means we cannot estimate the effect that opposite-party neighbors might have on all incumbents of the neighborhood. In other words, it may well be that treated and control residents *both* have higher move-out likelihoods than they would have in a counterfactual world where the opposite-party neighbor never arrived, but we cannot observe this.

The second problem is insurmountable given our research design, but we can partially solve the first by looking to the second column of Table 2. Doing so allows us to use the full sample of homeowners in North Carolina, not just those living in neighborhoods where experiments occurred. While this specification is not as well-identified as our preferred specification, it still includes the control variables and fixed effects that together absorb many of the neighborhood-level reasons that incumbents leave.[14] The results of this test imply that between 2005 and 2017, 19,863 move-outs

[12]Calculation: 405,731 × .2463 × .00341.

[13]Calculation: 341 / (405,731 × .0779).

[14]A large literature uses this design, without the novelty of new arrivals, to estimate causal effects of neighbors. These papers rely on the thinness of housing markets to argue that even households *current* nearest neighbors are quasi-randomly assigned (Agarwal et al., 2020; Bayer et al., 2021, 2008; Bollinger and Gillingham, 2012; Grinblatt et al., 2008;

(0.64% of all moves) and nearly $400,000,000 in seller-paid closing costs are the result of households moving because of the presence of opposite-party neighbors very nearby.[15]

4.3 Generalizability

Our paper uses data from North Carolina for the reasons discussed in Section 2.1. We view the benefits of detailed, accurate data that covers a long time series (and several million households) to be worth the cost of national coverage. Furthermore, North Carolina's population is representative of the population of the United States along a number of demographic characteristics (**?**). An important exception is that, since North Carolina is a swing state, politics might be especially salient. And, because of this, we hesitate to take our back-of-the-envelope calculations out-of-sample to the rest of the United States. Furthermore, the decomposition exercise in Brown et al. (2022) shows that the factors contributing to political segregation vary across the country.

[TABLE 3 HERE]

To speak to the role affective polarization might have in other states, we split current residents of North Carolina by birth region (the NCSBE collects state of birth from voters when they register). In Table 3, we show that our results are similar for all incumbents, regardless of birth region, with the exception of those current residents born in the South (outside of North Carolina). While suggestive that an aversion to out-party neighbors may affect incumbents across the country, we urge caution for two reasons. First, the sample sizes are relatively small. Second, residents born out-of-state are not randomly in our sample. For example, Californians who stay in California are likely systematically different than Californians who move to North Carolina. Future work will need to explore more deeply how neighbors' partisan identities affect residential choice decisions in other states or for specific subgroups not well-represented in North Carolina.

5 Robustness of the Main Result

In the subsections that follow, we conduct a number of tests to demonstrate the validity of our research design and the robustness of our main result. We begin in Section 5.1 by showing that treated

Gupta, 2019; McCartney and Shah, 2022; Towe and Lawley, 2013).

[15] Calculation: 44,447,261 × .1266 × .00353; 19,863 / (44,447,261 × .0700).

and control residents are largely indistinguishable on observables, and use this as our first piece of evidence that underlying differences between which current residents are treated and which are not is unlikely to drive the main result. In Section 5.2, we address the key concern that the amenity bundle or expectations about changes to the amenity bundle may differ between treated incumbents and control incumbents. We address a variety of other potential concerns in Section 5.3. Finally, acknowledging that our research design still ultimately relies on unproveable assumptions, we test our prediction using alternative designs that rely on different identifying assumptions. We describe these tests and their results in Section 5.4.

5.1 Treated and Control Groups are Economically Indistinguishable

For our model to produce an unbiased estimate of the effect of interest, we assume that, when comparing current residents in the same census block group of the same party and race who got new nearby neighbors at the same time, the political affiliations of their new nearby neighbors are as-if random. We start by referencing the large body of work on neighbor peer effects that has used multiple strategies to illustrate the conditionally random assignment of nearest neighbors (Agarwal et al., 2020; Bayer et al., 2021, 2008; Bollinger and Gillingham, 2012; Grinblatt et al., 2008; Gupta, 2019; McCartney and Shah, 2022; Towe and Lawley, 2013). A strength of our design compared to previous designs is that our assumption of random *new* neighbors is relatively *weaker*. However, we can use our detailed data to go a step further.

[TABLE 4 HERE]

In Table 4, we present the results from a series of tests that compare the personal characteristics of treated and control current residents. To create the table, we use the same strategy detailed in Equation 1, but replace the outcome variable with a variety of characteristics of current residents and their properties. In short, we find few economic or statistical differences between the two groups. For example, the estimate in column (1) shows that current residents who get new nearby neighbors of the opposite political party have lived in their homes .052 quarters, or 4.8 days, longer than similar current residents whose new nearby neighbors were not affiliated with the opposite party. Moving along the table, we reach a similar conclusion when investigating homeowner age, birth state, and partisanship, and, when we can observe it, whether they purchased their home with cash and, if they used a mortgage, whether it was a very high-combined-loan-to-value (CLTV) one.

We continue to use similar logic in columns (7) through (9) where we test for differences between treated and control current residents with respect to their property's characteristics (square feet, year built, and assessed value). As before, we find that treated and control current residents are economically indistinguishable. One statistically significant difference between the treatment and control groups is in the average age of their homes. While statistically different, we view the difference, 0.158 years or just under 2 months, as economically small and unlikely to explain our main effect.

The analysis conducted in this table is important because it directly addresses the natural concern that current residents treated with opposite party neighbors might be different than current residents whose new neighbors are not affiliated with the opposite party. For example, one may worry that young households are more likely to be treated or that opposite party arrivals will choose older, cheaper houses. We address concerns of this nature by, at least when the outcome is observable, explicitly ruling them out in the data. Our finding that observable characteristics are uncorrelated with treatment assignment is compelling evidence in support of our identifying assumption.

5.2 Ruling out Within-Neighborhood Variation

A key assumption of our research design is that, within a census block group, new neighbors could have picked any vacant house with equal probability. However, while census block groups are very small (a block group has an average of just over 100 households in our sample), variation in hyperlocal areas within block groups is still possible. For example, we might be concerned about neighborhoods with one block that is either nicer than the surrounding blocks or becoming so. If this block is also more likely to attract out-party arrivals, than the validity of our research design is violated. To rule out this alternative explanation we (i) compare the trend in average assessed home values around treated homes to the trend in average assessed home values around control homes, (ii) restrict our sample to just the most homogeneous neighborhoods where variation in hyperlocal neighborhoods is least likely, and (iii) conduct a heterogeneity test that takes advantage of distances between new neighbors and current residents.

5.2.1 Pre-Trends in House Prices

A standard test to rule out concerns related to differences in unobservables is to compare the move-out rates of the treatment and control groups in the periods before and after an event. But the way our empirical strategy is designed means we cannot conduct a standard pre-trends analysis. Our sample, by construction, is composed of only *current* residents who get new nearby neighbors. Households that moved out prior to the arrival of the new neighbor's arrival are, therefore, not in the sample. Put another way, the "pre-trend" of our outcome of interest, were we to graph it, would simply be a flat line on the x-axis since by construction none of the households in our sample sold their homes before the new neighbor's arrival.

[FIGURE 1 HERE]

Another option is to compare the house prices of treated homes to control homes. However, conducting this analysis requires a regularly updating estimate of parcel-level house prices, estimates that are notoriously noisy. Instead, we plot the trends in *block-level* assessed house prices. In some cases, treated and control incumbents reside on the same block, in which case the lines are identical, but in many cases they do not. We calculate the history of average assessed home values on blocks where treated incumbents live and blocks where control incumbents lives. We then regress the block's mean assessed home value on a dummy for the treatment interacted with an event-time dummy and a block group-by-year fixed effect.[16] We present the results of this test in Figure 1. There is an observable drop in local house prices on treated blocks in the year before the new neighbor arrives, but this drop is not statistically significant. Overall, our finding of relatively parallel trends in assessed house prices is inconsistent with the concern that arrivers choose blocks diverging from the neighborhood average.

5.2.2 Neighborhood Price and Political Homogeneity

[FIGURE 2 HERE]

Our next strategy for ruling out a variation-within-neighborhood explanation is to conduct our tests just in especially homogeneous neighborhoods where our identifying assumption is most likely

[16] We cannot conduct this analysis at the quarter level since assessments update only yearly.

to be valid. To do this, we use not just the average assessed home value, but the entire distribution of assessed home values. Specifically, we calculate the interquartile range of assessed house prices in each block group. When this difference is large, some homes in the block group may be much nicer than others. By focusing on those block groups where the difference is the smallest, therefore, we can limit the sample to just those neighborhoods where we are most confident that new arrivals are indifferent between hyperlocal neighborhoods within the block group. In Figure 2, we find consistent results across subsamples. We interpret the similar effect sizes as evidence that our main results are not driven by hyperlocal variation, at least insofar as hyperlocal variation is manifested in hyperlocal house price dispersion.

[FIGURE 3 HERE]

In Figure 3, using a similar logic, we calculate the difference in the share of current residents affiliated with the Democratic Party between the most and least Democratic blocks in the block group. As before, we say that the smaller this difference, the more homogeneous the block group. If it was the case that nonrandom treatment assignment was more prevalent in very heterogeneous neighborhoods, then we would expect to see stronger (but spurious) results in those areas. However, this is not what we see in the data.

5.2.3 Heterogeneity by Distance Between Immediate Neighbors

[TABLE 5 HERE]

Finally, we re-estimate our main model, but allow the effect of getting a new opposite-party nearby neighbor to vary based on how close, precisely, the new neighbor lives to the current resident. To create Table 5, we calculate the distance between current residents and their neighbors and discretize that difference into three categories. We find that it is especially at parcels where new opposite-party neighbors live very, very close by (less than .015 miles or 80 feet) that current residents are more likely to move away after the new neighbors arrive. In column (4), we estimate our model on the full sample and interact the main effect with the distance categories and reach the same conclusion as in columns (1) through (3). If changes in hyperlocal amenities were systematically pushing away some types of residents, we would expect all current residents who get opposite party nearby neighbors to be similarly (and spuriously) affected by the arrival of the new neighbor, regardless of precisely how nearby the new neighbor lives. This is not what we find. Instead, the result in Table 5 is more consistent with current residents preferring not to live very nearby to neighbors affiliated with the opposite party, but a buffer of 80 feet or more being enough to limit the negative effects.

5.3 Addressing Other Concerns with the Main Research Design

In this section we address a number of other potential concerns with our research design, sample construction, and definition of key variables.

[TABLE 6 HERE]

The first concern we tackle is one of uncommon support between the treatment and control group that invalidates our inference. Our solution to this problem is to use a coarsened exact matching methodology (Iacus et al., 2011, 2012) that helps reduce imbalances between treatment and control current residents. In our version of this methodology, we require that every treated household – those who got new opposite-party nearby neighbors – match to at least one control household – those with new neighbors not affiliated with the opposite party. To be a valid match, treated and control households must share the same census block group, party, and race and must both have gotten a new nearby neighbor in the same calendar quarter. Group-by-quarter-by-party-by-race fixed effect cells that do not contain both a treated household and a control household are thus omitted from the sample. This methodology ensures that our results in Table 2 are not driven by differences in the support of the treatment and control groups. While this sample is smaller, reflecting the strict requirements of the match, it is one where we are particularly confident that the control group provides a good counterfactual. The results, presented in column (1) of Table 6 are similar in both economic and statistical significance to our main results.

Furthermore, to be a treated household requires getting a new nearby neighbor of the *opposite* political party. That is, we say that a pair of next-door neighbors are opposite-party neighbors if, and only if, one is Democrat and the other is Republican. Note that this means there are seven pairwise combinations of Democrat, Republican, and unaffiliated households defined as not-opposite and used in Table 2 for estimating the main effect. In this matching methodology, though, all current residents are, by construction, either Democrats or Republicans. Therefore, our result from column (1) of Table 6 shows that our main results are robust to omitting unaffiliated households from the analysis.

Extant work shows that homophily occurs along many dimensions. All of our tests control for incumbent race and whether it differs from new neighbor race, but other characteristics may matter as well. Many of these, like religion or the presence of children, we do not observe. Our next test, presented in column (2), includes a control for the age of the new neighbor (we use the age of the first listed homeowner on the deed that matches to the NC voter data). We also include a dummy for different age that we set equal to 1 if the incumbent's age and new arrival's age differ by twenty years or more. The main result is largely unchanged suggesting that, while households may have preferences over neighbors' age (and a likely correlated presence of children), this seems orthogonal to party conditional on the other controls and fixed effects.

In column (3), we adjust the definition of nearest neighbor to also include the four closest homes on the other side of the street. As in our main tests, incumbent residents are more likely to sell their homes when either a next-door or across-the-street neighbor affiliated with the opposite party moves in. However, the estimate using this definition result is somewhat smaller than that in the main test, consistent with neighbors across the street playing a smaller role in incumbents' move-out decisions. This result lines up with the growing literature on social interactions between neighbors that show effects decay rapidly as distance increases (Bayer et al., 2021; McCartney and Shah, 2022).

Our next test, presented in column (4), addresses the concern that new neighbors might avoid particularly partisan current residents. Specifically, households searching for a new home might be able to infer the political affiliation and partisan intensity of a potential neighbor by observing their yard signs in the months leading up to elections. We limit this concern by dropping all observations where new neighbors moved in during election years (even numbered years). Despite the large drop in sample size, the estimated effect size is similar and statistically significant. This suggests that new arrivals to the neighborhood are not choosing parcels based on inferences about the political affiliations of their potential new next-door neighbors.

Another potential concern is that elementary schools districts, which can be very small, might not encompass entire block groups (Caetano and Macartney, 2021). Previous work has shown that households' location decisions are motivated by local schools (Bayer et al., 2007; Black, 1999), so the possibility that differences in elementary schools drive our main result requires investigation. We acquire data on elementary school district boundaries (school attendance areas for first grade) for the year 2009-2010 from the School Attendance Boundary Information System (SABINS). To investigate the concern that variation in elementary schools within block groups biases our main result, we focus on the sample of block groups that are served by precisely one elementary school.[17] In column (5) of Table 6, we show that the effect of a new opposite-party nearby neighbor is statistically indistinguishable when estimated using our full sample and when estimated using the subsample of block groups served by precisely one elementary school.

In the last test presented in Table 6, we acknowledge that by looking just at sell decisions we ignore residents who would prefer to leave but might not be able to sell. In column (7), we estimate the same model as before, but replace the outcome variable with sell or list. The sample mean of this variable is higher, at nearly 10% as opposed to 7.5%, reflecting people who list their homes but do not sell them. That said, the effect of getting a new nearby neighbor of the opposite party has a similar effect on listing and selling.

Experiments in our design occur at the group-by-quarter level and we therefore have, in essence, a cross section of group-by-quarter observations. We acknowledge, however, that there may be slightly wider spatial and temporal correlation than this, so we cluster at the more conservative tract-by-year level. To account for additional spatial and temporal correlations in the error terms, we cluster our standard errors in a variety of ways in Online Appendix Table A2. Under all clustering regimes, our main conclusion is unchanged.

Finally, we acknowledge that by choosing just one preferred specification in Table 2, we are making decisions that are "defensible, arbitrary, and motivated" (Simonsohn et al., 2020). To address this concern, we follow the direction of Simonsohn et al. (2020) and conduct a specification curve analysis, presented in Figure 4.

[FIGURE 4 HERE]

The key modelling assumptions we make in our main test are how to limit the sample, which control variables to include, and what geography and time fixed effects to use. The main sample is that used in Table 2, the full sample is the same as the main sample but without the sample

[17] Approximately 13% of our sample live in block groups with missing school district boundary data.

restrictions,[18] and the matched sample restricts to just treated and control households with common support. In Figure 4, we re-estimate our effect of interest using 128 different specifications. The consistently positive and significant estimates illustrate the robustness of our main result to these modeling choices.

5.4 Alternative Research Designs

Our preferred research design relies on the following identifying assumption: Assignment of new neighbors to incumbents is conditionally random. One threat is that unobserved differences in the neighborhoods immediately around the two incumbents is correlated both with treatment assignment and incumbents' plans to move away. A second threat is that arrivers might choose new neighbors in part because of the identity of the incumbents. Neither this concern nor the first can be directly addressed with our main test. In this section, we take a different approach and design two new tests with *different* identifying assumptions.

5.4.1 Comparing Residents on the Same Block (Strategy 2)

Our paper's second empirical strategy zooms down to the census block level and compares incumbents who got new nearby neighbors of the opposite party to other incumbents of the same race and party and on the same census *block* (as opposed to block group) who did *not* get new nearby neighbors. In other words, we compare otherwise identical households that vary only in how far away they live from the *same* new opposite-party neighbor. The identifying assumption of this test is that both of the incumbents were equally likely to have a house nearby to them go up for sale. We show in Table A3 that incumbents who live nearby the new opposite-party arrival (the treatment group) are relatively similar, at least on observables, to other same-race, same-party incumbents who live slightly farther away (the control group).

The advantages of our second design are twofold. First, by removing the strict requirement that control incumbents *also* get new neighbors at the same time, we can use a sample much larger than our main sample. Second, we can include a block-level fixed effect that helps rule out the possibility that changes in hyperlocal neighborhood attributes that push away or attract similar households can

[18] The sample restrictions drop small blocks, residents who do not live through an election, residents who have not lived in their homes for at least a year, and residents who move out in the quarter immediately following the new nearby neighbor's arrival.

explain our main result. That is, the small size of blocks makes it challenging to think of amenities that attract out-party neighbors to one part of the block more than another.

[TABLE 7 HERE]

We present the results of this test in Table 7. In column (1), we show that treated current residents are 0.388 percentage points, or 5.4%, more likely to move out within two years than other residents on the same block who live slightly farther away from the new neighbor. In column (2), we restrict to a one-to-one match. That is, for each treated incumbent, we find the most similar same-block control incumbent. The results, after this sample restriction, are similar to those estimated in column (1).

5.4.2 Comparing Residents Immediately Nearby an Arriver (Strategy 3)

Our third strategy zooms in even farther. To conduct this test we start by looking at incumbents immediately around new arrivals. For each arrival, we then compare those nearby incumbents who share a party affiliation with the new arrival to the other immediately nearby incumbents who do not. We define "immediately nearby" as the up-to eight nearest homes: The homes next-door and two-doors down on either side and the four closest homes on the other side of the same street. There is a trade-off of this test relative to our preferred one. On the one hand, this test compares incumbents of *different* parties, and Republican incumbents may be inappropriate counterfactuals for even very nearby Democratic incumbents. On the other hand, we show in Table A4 that despite being affiliated with different parties, the treated and control incumbents around the arriver appear very similar on observables. Furthermore, this test helps rule out the possibility that variation in hyperlocal amenities can explain the positive effect of the treatment we estimate using our preferred research design. A final benefit is that, by using just one common arriver to affect all the households, concerns about residents endogenously affecting whether they are treated or not are diminished.

[TABLE 8 HERE]

We present the results of this test in Table 8. From column (1), we see that 1,066,754 incumbent-by-quarter observations in the panel get a new neighbor either next-door, two-doors down, or in one of the four closest houses across the street. Of these, 11.01% get a new neighbor affiliated with the opposite party. That this share is lower than the treated share in our main tests is not surprising. Our main test's sample, somewhat by design, is restricted to relatively "purple" neighborhoods. This sample, however, includes all instances of new neighbors who eventually register to vote.[19] The key result from column (1) is that those incumbents in the immediate neighborhood of the new arrival who are affiliated with the opposite party of the arriver are .436 percentage points, or 6.0 percent, more likely to have moved away within 2 years of the new neighbor's arrival than other incumbents who are either unaffiliated or share party affiliation with the new neighbor. The inclusion of controls in column (2) does not meaningfully alter the estimate; not surprising given the similarities in treated and control residents documented in Table A4.

6 Heterogeneity Over Time

In this section we investigate two different aspects of timing. In Section 6.1, we conduct an event-study to analyze how the effect changes in the quarters following the arrival of the new neighbor. We then explore changes in the effect over the time series in Section 6.2

6.1 Quarters Since the New Neighbor's Arrival

[FIGURE 5 HERE]

To conduct our event study, we repeatedly estimate Equation 1 but vary the dependent variable. That is, instead of estimating the effect on sell within two years, we investigate the effect on sell within one quarter, within two quarters, and so on.[20] We then plot these estimated coefficients in Figure 5. We find that the estimates increase in magnitude as time passes since the new neighbor's arrival.

We propose three likely explanations. First, current residents need time to learn about their new neighbor's type. They might not know immediately if their new neighbors believe wearing masks during a pandemic infringes on First Amendment rights or if anybody driving a non-electric car

[19] The sample size is not the same as eight times the number of fixed effect cells since not all eight houses either exist (if the house is at the end of a block, or if there are not four houses across the street on the same block) or if the houses are not owner occupied by registered voters.

[20] Note, by construction, this test no longer drops households that move out in the quarter immediately after getting a new nearby neighbor so the coefficient estimate when the dependent variable is move-out within eight quarters (or two years) will not precisely match the main result, column (6), of Table 2.

should be treated with contempt. Second, even if current residents have learned enough to know they want to move, it takes time to realize that preference. Third, labor market frictions, school market frictions, and the consequent thinness of the house market during the winter months mean that even if a homeowner knows in November that they want to leave, it is far easier to find a new home to move to in the spring and summer months. Our data is not detailed enough to pull apart these three channels. However, the lag is also helpful in what explanations it rules out. If getting a new nearby neighbor affiliated with the opposite party was correlated with some hyperlocal amenity change that was attracting households of one party while pushing away those of the other, then we might expect to see a (spurious) immediate "effect" of the new neighbor's arrival. We see no such effect, further suggesting that a a differences in amenities story is driving our main result.

Next, we ask if the lag depends on when in the election cycle the new neighbor arrived. Arrivals that occur during or immediately before times when politics is especially salient, like in the summer and fall before an election, might lead to faster incumbent departures. To test this hypothesis, we conduct a subsample analysis that splits the experiments into those that occur in election years and those that do not. For each of these samples, we look at differences between treated and control incumbents on sell within two years and sell within one year. We present the results of these tests in Table 9.

[TABLE 9 HERE]

In the first two columns, we see that incumbents whose treatments occur during election years do have a relatively higher estimated likelihood of having left within one year, 3.4% higher versus just 1.4% higher, but neither of these estimates is significantly different from 0. The relatively small difference between these two estimates could be because political differences are not any more salient even during election years. Incumbents may learn about their neighbors' politics not through their yard signs, but from other social interactions and visual inferences that do not vary over the election cycle. It could also be that they *do* learn about their neighbors' politics sooner during election years, but that it takes more than a year for incumbents to decide to leave and then sell their homes at significant levels. After two years have passed, incumbents treated in election and non-election years both show likelihoods of having moved away that are similar to our main result and similar to each other. Our takeaways from this analysis are twofold. First, arrivals during election years do not seem to lead to especially faster incumbent sales. Second, disentangling how much of the lag between new neighbor arrival and incumbent departure in our main result is due to delayed learning about neighbor type versus home sale transaction costs will require finer data than we have available.

6.2 Changes Over the Time Series

[FIGURE 6 HERE]

In this section, we investigate how our results vary over the the time series. One possibility is that, as political affiliation has become more salient, the effect sizes have increased over time. To investigate, for each of our three strategies, we plot the estimated treatment effect year over year. Figure 6 plots these estimates and shows three things. First, the results are broadly similar between the three strategies over the time series. Second, and perhaps surprisingly, the overall pattern is one of a relatively consistent effect size over the whole time period, including election years and non-election years. Future work will need to investigate more deeply whether this reflects the population or is an artefact of the peculiarities of our research designs. Finally, the third finding is the noticeable dip in the effect of an opposite-party new neighbor on incumbent moves during the Great Recession. Motivated by this finding, we explore how our main effect varies over the housing cycles.

We create a measure of market liquidity using the multiple listing service data from CoreLogic Solutions. We take all listings with a close date and calculate the difference between the close date and the list date. We then take the median value of this variable for each county-by-quarter. We say that a county-by-quarter housing market is "hot" if that median value is less than 100 days and "cool" otherwise.

[TABLE 10 HERE]

In Table 10, we estimate how the main effect differs as a function of the hotness of the local house market. In columns (1) and (2), we compare within house markets across time. The results are more than three times as large in hot housing markets. We show a similar difference in columns (3) and (4) when we instead fix time and compare across the state. Finally, in column (5), we include our canonical fixed effect, and find an interaction effect that is large and statistically significant. Overall, the results of this table suggest that our main effect is driven by incumbents moving away from opposite-party neighbors in hot markets.

The main result coming mostly in sellers' markets suggests two things. First, unsurprisingly, when it is easier for incumbents to sell, those who want to move away are more likely to do so. Second, and more subtly, Table 10 illustrates that while incumbents might prefer to move away from opposite-party neighbors, there is a limit on how much they are willing to pay to realize this preference. In other words, they are not willing to sell their homes at depressed-market prices in order to move.

[FIGURE 7 HERE]

That said, it is challenging to measure the "discount" at which an incumbent might be selling their house since this requires us to assume something about the "true" value of the home. To investigate our hypothesis that sellers want to leave, but not at the expense of how much they can sell for the house, we look at a longer time horizon. We see that when opposite-party neighbors arrive in cool markets, incumbents *do* prefer to leave. Indeed, the treatment effects in hot and cold markets are indistinguishable after five years. But incumbents treated in cold markets wait until conditions improve before selling, as evidenced by the long delay in their relative move-out likelihoods.

Figure 7 also shows that, in hot markets, all of the out-sized effect on treated households occurs in the first two years. Incumbents either move away from their new neighbors within two years or not at all. This result also helps rule out a differentially changing amenities story since we would then expect to see the "treatment effect" continue to widen if that was the mechanism driving the main result.

7 An Affective Polarization Mechanism

The evidence presented so far shows that neighbors themselves, over and above any correlation their attributes have with neighborhood amenities, matter for incumbents' home sale decisions. Specifically, current residents move out when new neighbors affiliated with the opposite party move in nearby. As discussed in the introduction, animosity between partisans is a natural potential mechanism. 28% of Americans respond that they would be somewhat upset or very upset if their child were to marry someone from the opposite party.[21] 15% of respondents in a different survey answered

[21] Source: https://docs.cdn.yougov.com/t0hi1tcqs5/econTabReport.pdf.

"yes" when asked, "Do you think we'd be better off as a country if large numbers of [opposing party] in the public today just died?" (Kalmoe and Mason, 2019). In this section, we discuss the possibility that this distaste for members of the opposite party is the mechanism behind our main results.

7.1 Degree of Partisanship

Our samples have so far included all residents who got new neighbors, regardless of how politically engaged they are. Among the sample of registered voters, though, some people identify with their registered political party more than others and also dislike members of the opposite party more. In other words, if our hypothesis of partisan-fueled selling is correct, we would expect to see that more partisan neighbors have stronger effects on surrounding households. In this section, we investigate heterogeneity of our effects over variation in the partisanship of incumbents and their new nearby neighbors. To measure how intensely people affiliate with their party, we turn to the voter history files. We observe every election each voter participated in and use that to create a time-invariant measure of partisanship. We classify voters as partisan if they participated in more than 75% of the November federal elections in which they were eligible to vote. Participation is affected by many factors, but our motivation for this proxy can be summarized as follows: Registered voters who vote frequently are more likely to be especially partisan.[22] We then re-estimate our main test on different subgroups of current resident and new nearby neighbors. The results of this analysis are presented in Table 11.

[TABLE 11 HERE]

In columns (1) and (4), we compare current residents who got new nearby neighbors we tag as non-partisan to current residents who got new neighbors we tag as partisan. In column (1) we find that current residents, on average, are unaffected by new neighbors if those new neighbors are non-partisan. Note that these new neighbors are, as before, affiliated with the opposite party. Our interpretation of this null result is entirely consistent with neighbors themselves mattering. In this case, the characteristic of interest – political affiliation – is unlikely to be especially salient to the new neighbor's surrounding residents, so, naturally, the current residents are unaffected by it. In

[22] This claim is supported by survey data from the Pew Research Center: https://www.pewresearch.org/politics/2014/06/12/section-5-political-engagement-and-activism/.

contrast, in column (4) we look at just new neighbors who *are* partisan. Here we document a large and significant effect. The average resident is .456 percentage points or 6.0% more likely to move after getting a partisan new neighbor of the opposite party compared to a resident who got a partisan new neighbor of the same party (unaffiliated voters are very rarely classified as intense).

In columns (2), (3), (5), and (6) we also take subsamples of current residents. From this analysis, we see that the effects of new partisan neighbors tend to be larger on non-partisan current residents. One interpretation is that households who would rather ignore politics entirely especially dislike living nearby people of the opposite political affiliation when those households are also especially likely to make their feelings known. In all four of these columns, we note the large standard errors and urge caution in over-interpretation. When we move on to a fully interacted model in column (7), we again find that living nearby to partisan households makes all residents more likely to relocate.

[TABLE 12 HERE]

Our first proxy for partisanship has two important limitations. First, prior work has shown that residents' political decisions are endogenous to their neighbors' political decisions. Specifically, residents are more likely to be politically active when surrounded by same-party neighbors (Perez-Truglia, 2018; Perez-Truglia and Cruces, 2017). Second, our proxy assumes that partisanship does not change over time. To provide further evidence on the importance of partisanship, we adjust our measure to be a function of only those participation decisions made immediately prior to the new nearby neighbor's arrival. Specifically, we classify current residents as either "voting", if they voted in the most recent federal election before the arrival of the new neighbor, or non-voting, if they did not. Note, since we cannot observe the prior participation decisions of arrivals, this new measure only applies to current residents. We conduct this analysis in Table 12. The takeaway from this analysis is the same as before: Current residents are affected most by new neighbors who are highly partisan.

7.2 Discussion of Potential Alternative Mechanisms

We presented two alternative possible mechanisms in the introduction. First, incumbents who get new opposite-party neighbors might become more pessimistic about their neighborhood's trajectory. Second, incumbents may learn from their neighbors and consequently experience an increased likelihood of moving out. More work will be needed to tease apart these different channels and the varied ways that households may respond to opposite-party neighbors.

A final note is that we do not observe how, specifically, households feel about their new neighbors. That is, the likelihood of a current resident moving away is not observed. We only observe realized move-outs. On average, we find that treated household are more likely to have moved away and conclude that, on average, getting a new opposite-party neighbor makes households more likely to move away. But it may well be that some households, when treated, became *less* likely to move away and we would not be able to infer this using our data and research design.

These caveats and limitations aside, our body of results show that political affiliation is an important and economically meaningful neighbor attribute. We present evidence consistent with current residents disliking living near those affiliated with the opposite party. The results of this section also help us rule out the unobserved hyperlocal amenity explanation for our main result. If incumbents were moving away solely because of expected changes to local neighborhoods that were correlated with the party affiliation of new arrivals, then such neighborhood effects would have to channel only through new residents who frequently vote. Therefore, we interpret these results as evidence most consistent with an opposite-party neighbor effect that is driven, at least in part, by political polarization and the hostility with which some party affiliates treat those associated with the opposite party. And the driver of the average effect is incumbents who find themselves living nearby to others who (i) have different views, preferences, and behaviors and (ii) are especially likely to care very deeply about that and make it known that they do.

8 Conclusion

Until now, the extant literature has shown little evidence that the political affiliations of households' social connections – their neighbors, co-workers, and friends and family – affect their financial decisions. In this paper, we contribute new evidence that households' home sale decisions are affected by the political affiliations of their very nearest neighbors. Cleanly identifying this real effect of political identity moves the literature forward in an important way. Politics as identity and hostility to those of the opposite party are increasingly important features of our society, and it is crucial we understand if these have real effects on household finance.

An important limitation of this exercise is that political affiliation may be a stand-in for other characteristics. This concern, while valid, is also hard, or perhaps impossible, to address. A growing body of work in political science and sociology argues that political identity *is* the social identity of many people (DellaPosta et al., 2015; Iyengar et al., 2019; Mason and Wronski, 2018). Said another way, it is difficult to think about the counterfactual where everything about the new neighbor – the music they listen to, the people they have over, the car they drive, their gun ownership status, the dogs they own, the interactions they have with their neighbors when getting the mail, if their neighbors would feel comfortable having them watch their children – is the same, but their political affiliation is different. It is exactly this bundling of preferences, choices, and partisan identity that has given rise to affective polarization. But whether this affective polarization influenced home sale decisions, or indeed decisions in most economic domains, has proven difficult to show. An illustration of affective polarization's real effects is what this paper contributes.

In this paper, we follow the large literature in political science and treat political affiliation as a bundled package, leaving the underlying factors that drive partisan bias and affective polarization inside a black box. That said, there are two potential mechanisms we can rule out. First, we can rule out an amenity-preferences story since what the arrival of the new neighbor means for changes to both public good provision and private amenities is shared by treated and control incumbents. Second, by explicitly controlling for it, we can rule out the possibility that current residents are not moving away from people of the opposite party, but rather people of a different race (see Bayer et al. (2022) for more on the importance of neighbor race). Consequently, we can conclude that our results are *not* driven by the amenity preferences component or racial component of partisan identity. Understanding the root causes of affective polarization is an immense challenge that future work will be wrestling with for years. In this paper, we take as given that people *claim* to not like those affiliated with the opposite party. What we contribute is evidence that this reported distaste for members of the opposite party – whatever its precise underlying causes – affects a real and important household financial decision, house sales.

In summary, we document that an aversion to living near members of the opposite party is an important factor in households' home sale decisions. Our causal test shows that households are willing to sell their homes and move – an enormously costly activity – when presented with opposite-party neighbors. Our paper contributes to a number of research agendas by (i) identifying an economically important real effect of politics on household financial decision making, (ii) showing that affective polarization is an important non-cash flow reason that households trade assets, and (iii) shedding new light on a potential cause of political segregation, partisan motivated home sales.

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Figure 1: Block-Level House Prices: The Blocks of Treated versus Control Incumbents

To create this figure, we calculate average assessed home values on blocks where the treated incumbent lives and blocks where control incumbents live in the two years before the arrival of the new neighbor to two years after their arrival. We then plot the block's mean assessed home value on a dummy for the treatment interacted with an event-time dummy after demeaning at the block group-by-year level. To ease interpretation we then calculate fitted values using the results of the regression. We plot 95% confidence bands in dashed lines.

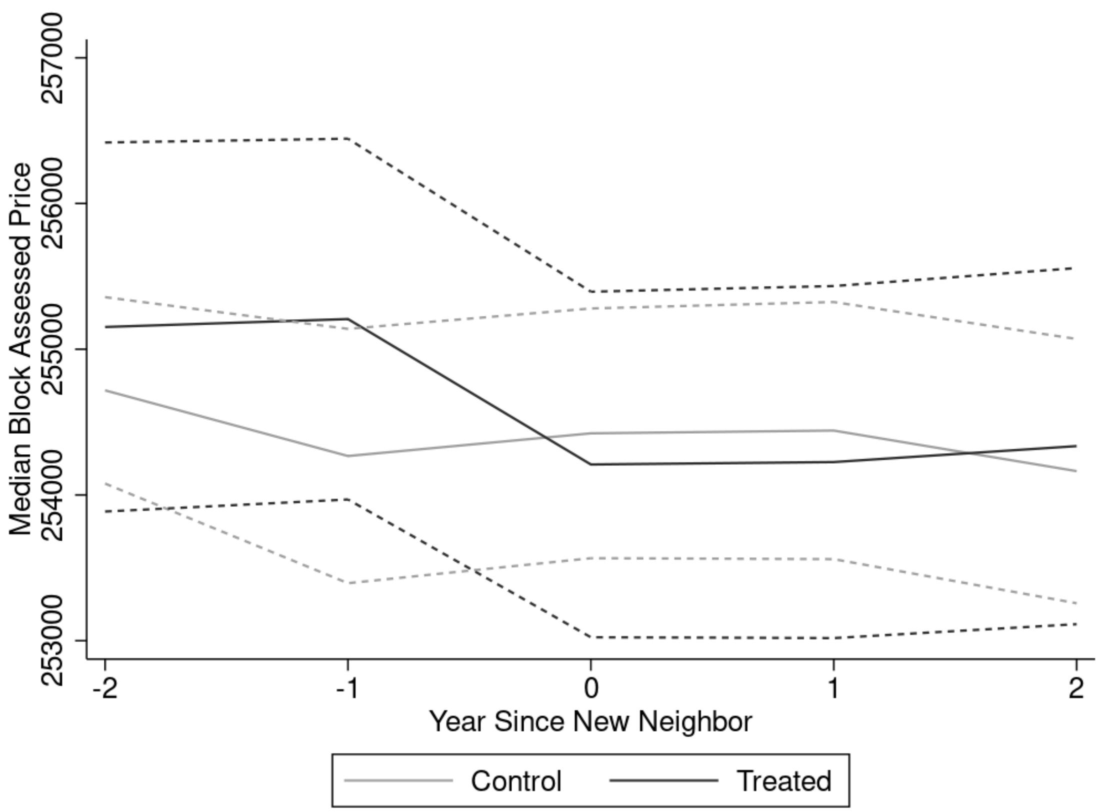


Figure 2: Block Group Homogeneity - Price

To create this figure we estimate our main model on a number of subsamples. The sample and models used are identical to Table 2 column (6) except for the broader fixed effect cells. For each census block group, we calculate the interquartile range in assessed house price in each year. We then take subsamples of the main sample with increasingly small interquartile ranges.

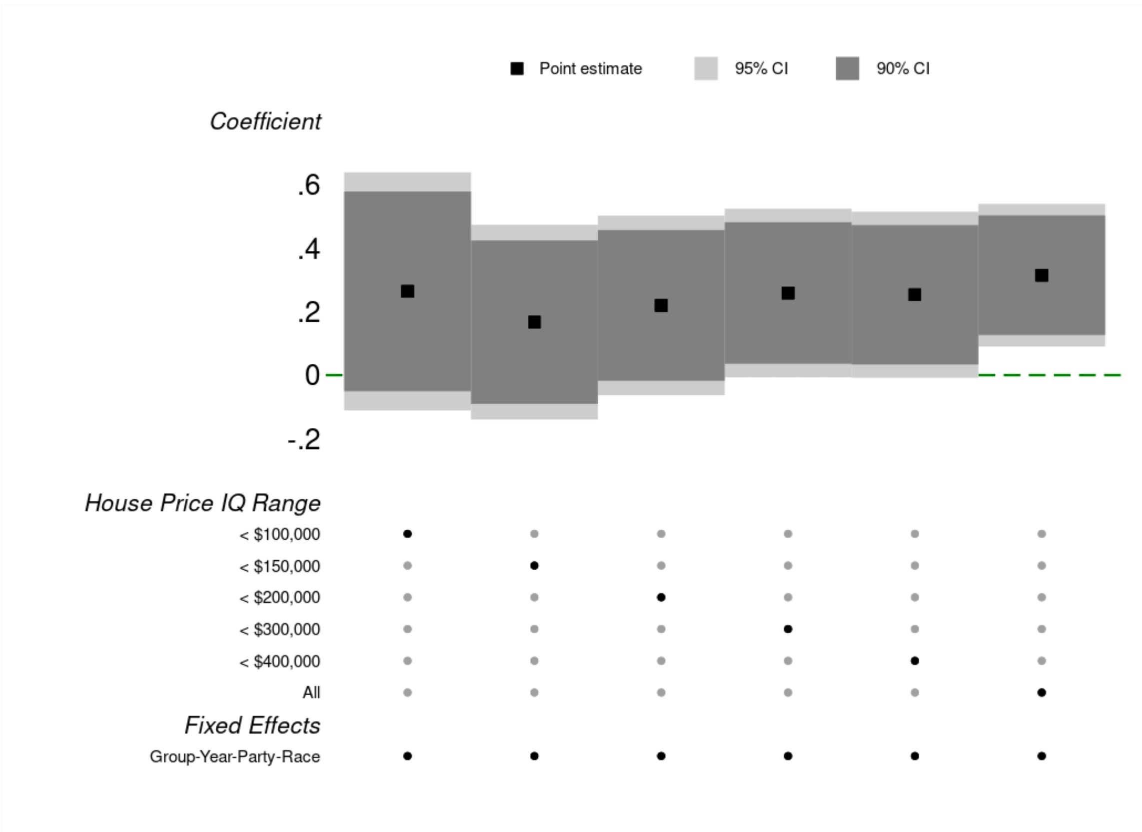


Figure 3: Block Group Homogeneity - Politics

To create this figure we estimate our main model on a number of subsamples. The sample and models used are identical to Table 2 column (6) except for the broader fixed effect cells. For each census block group by quarter, we calculate the difference between the Democratic block share of the most Democratic block in the block group and the least Democratic block in the block group. We then take subsamples of the main sample with increasingly small differences.

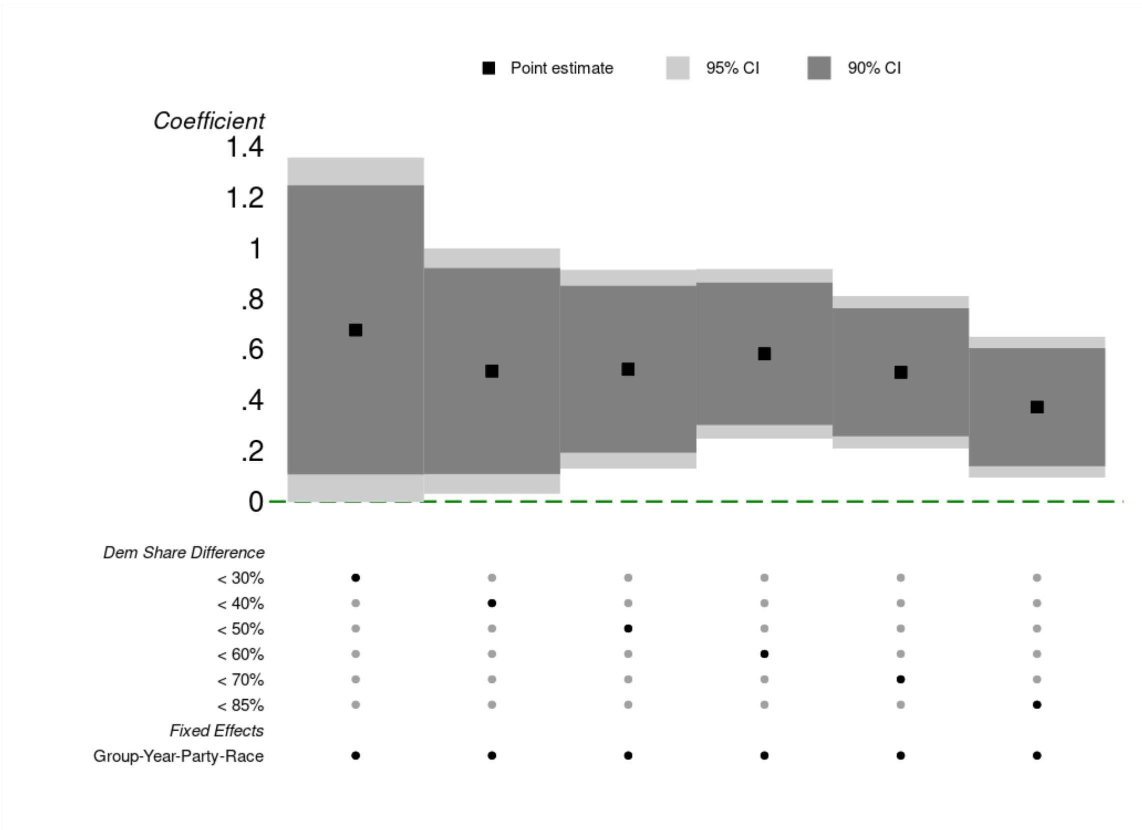


Figure 4: Specification Curve for Strategy 1

To create this figure we estimate 128 alternative specifications as suggested by Simonsohn et al. (2020). We use three samples. The main sample is that used in Table 2, the full sample is the same as the main sample but does not drop small blocks, residents who do not live through an election, residents who have not lived in their homes for at least a year, or residents who move out in the quarter immediately following the neighbor's arrival, and the matched sample is that used in column (1) of Table 6. Current resident control variables include homeowner party, race, age, birth state, and tenure, along with an interaction between homeowner and neighbor race. Property controls include property size and age. Finally, we vary the fixed effects we use. Our primary approach is detailed more specifically in Table 2 and denoted by the black point estimates.

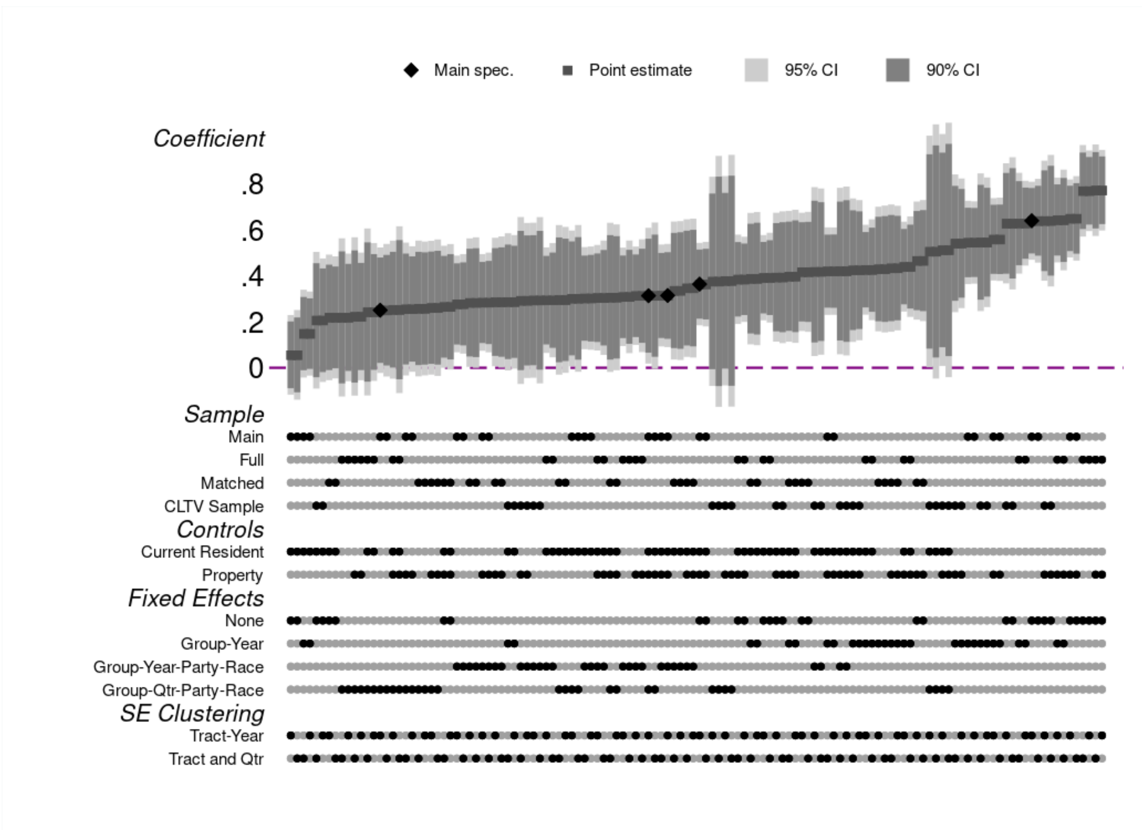


Figure 5: The Effect Over Time of a New Opposite-Party Nearby Neighbor

This event study presents our analysis from column (6) of Table 2, which estimates the relative likelihood of moving out after getting a new neighbor of the opposite political party compared to a current resident in the same block group, of the same race and party affiliation, who at the same time got a new neighbor not affiliated with the opposite party. To create this figure we vary the dependent variable across different time horizons, i.e., what is the relative likelihood that the treated resident moved out after some number of quarters. The data set contains only the quarter when the current resident got a new nearby neighbor conditional on both the current resident and the new neighbor existing in the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. The sample used to create this figure is different from the sample used in Table 2 in that this sample includes current residents who moved out in the quarter immediately following the new neighbor's arrival. For this reason, the estimated coefficient when looking at 8 quarters since new neighbor arrival is not identical to that estimated in Table 2. 95% confidence intervals are plotted with dashed lines.

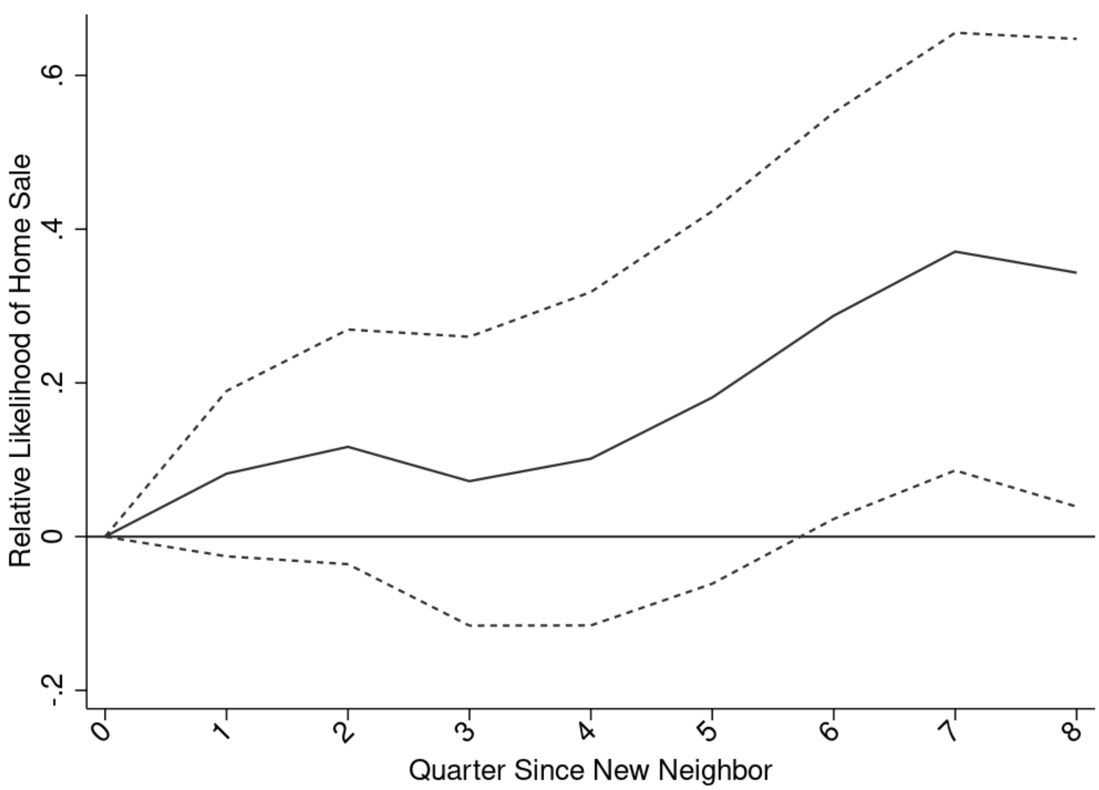


Figure 6: Heterogeneity Over Time

In this figure, we plot the estimate of the main effect year by year. We plot the main effect of each of the three main strategies. Strategy 1 corresponds to column (6) of Table 2. Strategy 2 corresponds to column (1) of Table 7. Strategy 3 corresponds to column (2) of Table 8.

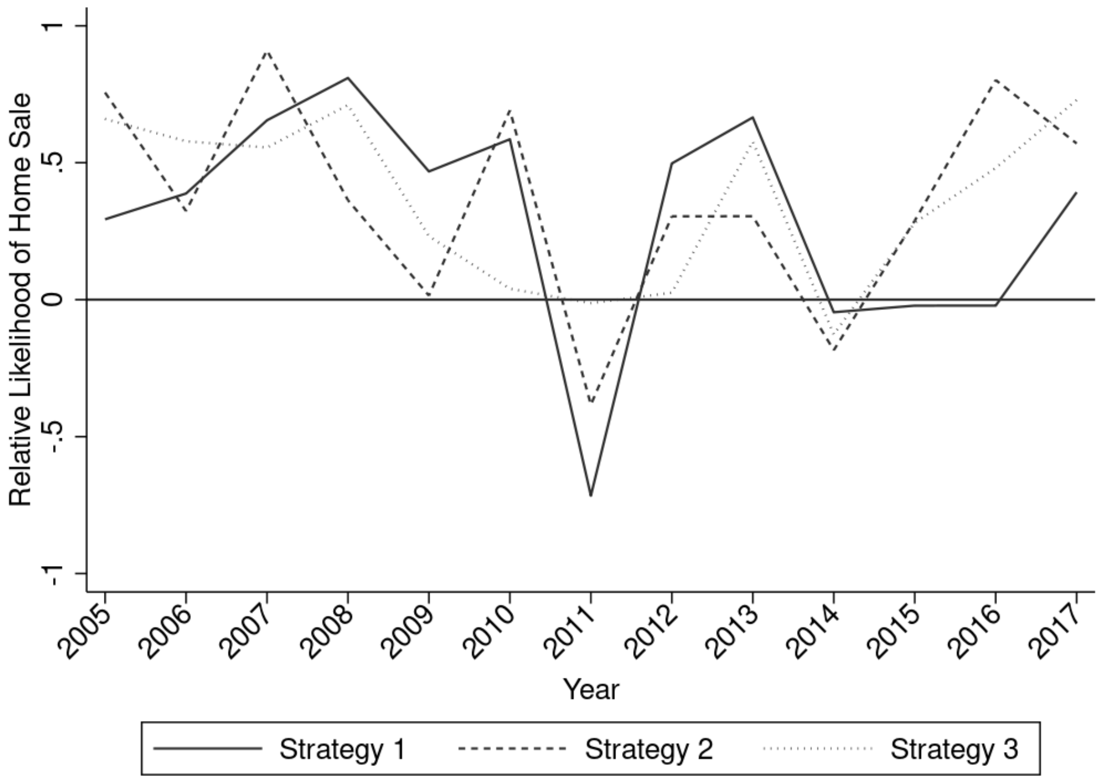


Figure 7: Hot or Not

To create this figure we construct a measure of market "hotness" by calculating the difference between MLS listing close dates and list dates for each county-by-quarter. We denote a county-by-quarter market as "hot" if that median difference is less than 100 days and as "cool" otherwise. We then estimate the same model used in column (6) of Table 2 on subsamples split by "hot" and "cold" markets and plot the probability of selling after receiving a new opposite party nearby neighbor along varying time horizons (i.e., from 1 to 20 quarters).

Table 1: Describing the Sample of Households Who Got New Nearby Neighbors

This table describes the sample of current resident-by-quarter observations where the current resident got a new nearby neighbor (nearby properties are those up to two houses down on either side) and both the current resident and the new neighbor exist in the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. The sample is restricted to blocks with at least twenty registered voters and to owners who have lived at least one year in their home, have lived through an election, and who did not move away in the quarter immediately following the new neighbor's arrival. The home sale variable comes from the CoreLogic Solutions Real Estate deeds data. The political party and demographics information come from the voter registration data. Partisan equals 1 if the individual voted in at least 75% of the November federal elections over their registration period. This information comes from the voter history file. The property characteristics come from the CoreLogic Solutions Real Estate assessor data. Opposite party equals 1 if the current resident is a Democrat and the new neighbor a Republican or vice versa and 0 otherwise. Different race equals 1 if the current resident is non-white and the new neighbor is white or vice versa and 0 otherwise.

	Count	Mean	Std Dev
Dependent Variable			
Sell within 2 Years (0-100)	405,731	7.79	26.80
Current Resident Politics			
Democrat	405,731	0.38	0.49
Republican	405,731	0.46	0.50
Unaffiliated	405,731	0.16	0.37
Partisan	405,731	0.66	0.47
Current Resident Demographics			
Race: White	405,731	0.85	0.36
Race: Black	405,731	0.15	0.35
Homeowner Age (Years)	405,731	51.41	14.37
Born in NC	405,731	0.30	0.46
Tenure (Quarters)	405,731	20.86	14.83
Property Characteristics			
Year Built	405,731	1987	19
Building Sq Ft	405,731	2,505	978
New Nearby Neighbor Politics			
Opposite Party	405,731	0.25	0.43
Democrat	405,731	0.34	0.48
Republican	405,731	0.39	0.49
New Nearby Neighbor Demographics			
Different Race	405,731	0.18	0.39

Table 2: The Effect of Opposite-Party Nearby Neighbors on Current Residents' Home Sales

This table presents the coefficient estimates of the effect of new neighbors' political affiliations on current residents' home sale decisions. In columns (1) and (2), the sample consists of all current residents and the independent variable is a dummy equal to 1 if any of the households' nearby neighbors are affiliated with the opposite party. In columns (3)-(6), the sample consists of all current resident-by-quarter observations where current residents get a new nearby neighbor and the independent variable is a dummy equal to 1 if the new nearby neighbor is affiliated with the opposite party. Columns (7) and (8) further restrict this sample by dropping those with missing combined loan-to-value (CLTV) ratios at purchase. Nearby neighbors are those living either one or two houses down on the same side of the street and within 0.1 miles. All current residents have between 0 and 4 nearby neighbors. We require that both the current resident and the nearby neighbor exist in the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. The sample is restricted to blocks with at least twenty registered voters and to owners who have lived at least one year in their home and have lived through an election. Control variables include homeowner party, race, age, birth state, and tenure; an interaction between homeowner and neighbor race; and property size and age. All coefficient estimates correspond to a percentage point (0-100) change in the probability of selling at some point in the next two years. In column (2), the count of fixed effect cells refers to the number of distinct block group-by-quarter-by-party-by-race observations present in the full sample. In column (5), it refers to the number of distinct group-by-year-by-party-by-race observations. In columns (5)-(8), it refers to the number of distinct block group-by-quarter-by-party-by-race observations. See Table A1 for the complete list of control variables and their estimated coefficients. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (=100)							
Sample:	*All Current Residents*		*Current Residents Who Got New Nearby Neighbors*				*& Nonmissing CLTV*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Has Nbr Opp Party	1.019***	0.353***						
	(0.034)	(0.034)						
New Nbr Opp Party			0.640***	0.278***	0.251*	0.314**	0.517**	0.513**
			(0.087)	(0.107)	(0.139)	(0.151)	(0.257)	(0.257)
Control Variables		X				X	X	X
CLTV Bucket							X	
Fixed Effects								
Group × Year × Party × Race				X				
Group × Qtr × Party × Race		X			X	X	X	X
Counts								
N	44,566,950	44,439,968	617,539	565,171	457,057	405,731	168,059	168,059
Fixed Effect Cells		1,274,229		95,600	135,139	122,196	57,990	57,990
Sample Means								
Dependent Variable	6.99	7.00	7.46	7.64	7.79	7.79	9.09	9.09
Has Nbr Opp Party	0.1266	0.1266						
New Nbr Opp Party			0.2401	0.2407	0.2418	0.2463	0.2303	0.2303

Table 3: Split by Birth State

This table conducts the same analysis as column (6) of Table 2 with broader fixed effect cells on strict subsets of the sample used in that estimation. For each incumbent, we use the state of birth field from the North Carolina State Board of Elections, to classify them as being from one of the four Census Regions: Northeast, Midwest, South, and West. We exclude from the South group those incumbents born in North Carolina. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)				
Sample:	*Current Residents Who Got New Nearby Neighbors*				
Subsample, Current Resident Birth Region	Northeast (1)	Midwest (2)	South (3)	West (4)	North Carolina (5)
New Nbr Opp Party	0.808**	0.570	0.078	1.366	0.466**
	(0.349)	(0.542)	(0.350)	(1.405)	(0.221)
Controls	X	X	X	X	X
Fixed Effects					
Group × Year × Party × Race	X	X	X	X	X
Counts					
N	70,627	36,318	70,704	6,522	131,380
Fixed Effect Cells	21,377	12,845	22,771	2,841	37,619
Sample Means					
Dependent Variable	8.47	9.47	8.83	9.78	6.83
New Nbr Opp Party	0.2340	0.2323	0.2479	0.2352	0.2644

Table 4: Balance Test – Comparing the Current Resident Characteristics of the Treatment and Control Groups

This table presents the coefficient estimates of the "effect" of getting a new opposite-party nearby neighbor on a number of outcomes related to characteristics of the current residents and their properties. The sample is identical to the one used in our main tests. Control variables include all those included in the main test reported in column (6) of Table 2, except for the variable that is being analyzed on the left hand side. Variables are as defined in the text and Table 1. Cash purchase is observed only for those current residents' whose purchase loan we observe. CLTV > 95% equals 1 if the current resident's purchase mortgage had a combined loan-to-value ratio above 95%. This variable is defined only for those current residents whose mortgage amount was strictly greater than 0. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Sample:	*Current Residents Who Got New Nearby Neighbors*								
Category	Homeowner Characteristics						Property Characteristics		
Dependent Variable:	Tenure (Qtrs) (1)	Age (Yrs) (2)	Born in NC (3)	Partisan (4)	Cash Purchase (5)	CLTV > 95% (6)	Log Bldg Sqft (7)	Home Age (Yrs) (8)	Log Assd Value (9)
New Nbr Opp Party	0.052	0.125^{*}	0.000	-0.002	-0.003	-0.005	0.001	0.158^{**}	-0.001
	(0.053)	(0.073)	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.063)	(0.002)
Controls	X	X	X	X	X	X	X	X	X
Fixed Effects									
Group × Qtr × Party × Race	X	X	X	X	X	X	X	X	X
Counts									
N	405,731	405,731	405,731	405,731	168,059	168,059	405,731	405,731	296,844
Fixed Effect Cells	122,196	122,196	122,196	122,196	57,990	57,990	122,196	122,196	90,403
Sample Means									
Dependent Variable	20.86	51.41	0.30	0.66	0.10	0.33	7.75	23.87	12.25
New Nbr Opp Party	24.63	24.63	24.63	24.63	23.03	23.03	24.63	24.63	23.91

Table 5: Heterogeneity over Distance to the New Nearby Neighbor

This table estimates the same model as column (6) of Table 2 on strict subsets of the sample used in Table 2. Specifically, we split current residents who got new neighbors into three groups based on the distance between their house and the house the new neighbor moved into. The distance thresholds for these three groups are: (1) 0 to 0.015 miles from the new neighbor, (2) greater than 0.015 but less than or equal to 0.03 miles from the new neighbor, and (3) greater than 0.03 but less than 0.1 miles from the new neighbor. Control variables include all those included in the main test reported in column (6) of Table 2. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)			
Sample:	*Current Residents Who Got New Nearby Neighbors*			
Subsample, Distance to Neighbor (Miles):	0 – 0.015 (1)	0.015 – 0.030 (2)	0.030 – 0.100 (3)	All (4)
New Nbr Opp Party	0.706 (0.594)	0.322 (0.264)	0.258 (0.296)	0.683** (0.341)
Distance: 0.015 – 0.030 Miles				-0.439** (0.173)
Distance: 0.030 – 0.100 Miles				-0.759*** (0.196)
New Nbr Opp Party × 0.015 – 0.030 Miles				-0.615* (0.364)
New Nbr Opp Party × 0.030 – 0.100 Miles				-0.206 (0.386)
Control Variables	X	X	X	X
Fixed Effects				
Group × Qtr × Party × Race	X	X	X	X
Counts				
N	38,685	140,151	109,565	405,731
Fixed Effect Cells	15,522	51,828	41,893	122,196
Sample Means				
Dependent Variable	9.19	7.99	7.02	7.79
New Nbr Opp Party	0.2210	0.2463	0.2547	0.2463

Table 6: Robustness of the Main Result – Assorted Tests

This table presents models that slightly vary either the sample or specification used in column (6) of Table 2. In column (1) we use a strict subset of the data that requires the group-by-quarter-by-party-by-race cell to contain both a treated current resident and a control current resident to be included in the sample. Column (2) includes a categorical variable for the age of the new neighbor and a dummy variable for if the current resident and the new neighbor have very different ages (absolute value of age difference greater than 20). In column (3), we increase the sample to include all current residents who got a new neighbor either two doors down or in one of the four closest houses across the street and less than 0.1 miles away. We then say that a current resident is treated if a new opposite party neighbor moved into to one of those (up to) 8 homes. In column (4), we drop from the main sample any current residents whose new neighbor arrived during an election year (even numbered years). In column (5), we restrict to the subsample of census block groups served by exactly one elementary school. In column (6) we use as the outcome variable a dummy equal to 100 if the current resident sold or listed their home within 2 years of the new neighbor's arrival. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (=100)					or Listed (=100)
Sample:	*Current Residents Who Got New Nearby Neighbors*					
Subsample:	Matched	All	+ X St Nbr	Non-Elec Yrs	1-School Groups	All
	(1)	(2)	(3)	(4)	(5)	(6)
New Nbr Opp Party	0.328**	0.295*		0.354*	0.339	0.489**
	(0.163)	(0.159)		(0.203)	(0.251)	(0.204)
New Nbhd-Nbr Opp Party			0.214**			
			(0.096)			
Control Variables	X	X	X	X	X	X
New Nbr Age		X				
New Nbr Diff Age (=1)		X				
Fixed Effects						
Group × Qtr × Party × Race	X	X	X	X	X	X
Counts						
N	182,524	371,424	910,083	218,318	145,966	256,473
Fixed Effect Cells	43,244	114,219	207,276	65,565	43,261	78,248
Sample Means						
Dependent Variable	7.87	7.79	7.35	7.61	7.98	9.01
New Nbr Opp Party	0.4032	0.2473	0.2470	0.2450	0.2529	0.2546

Table 7: Within Census Block Analysis

This table estimates the effect of a new opposite-party neighbor on current residents who live very nearby (one or two doors down) compared to current residents who (i) did not get new nearby neighbors and (ii) live on the same block. The sample used in the first column includes all current resident-by-quarter observations where a current resident got a new nearby neighbor of the opposite party. We then add to the sample current residents on the same census block, in the same quarter, affiliated with the same party, and of the same race who did not receive a new nearby neighbor. The control variables and other sample restrictions are otherwise identical to column (6) of Table 2. In column (2), we further restrict the control group by using age group, ethnicity, born in NC, residential tenure, year built, house square feet, and land square feet as tiebreakers such that we are left with a one-to-one-match between treated current residents and same-block, same-quarter, same-race, and same-party control current residents. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)	
Sample:	*Current Residents Who Got New Block Nbrs & All Matches*	*One-to-One Matched Subsample*
	(1)	(2)
New Nbr Opp Party	0.388***	0.423**
	(0.102)	(0.173)
Control Variables	X	X
Fixed Effects		
Block × Qtr × Party × Race	X	X
Counts		
N	1,456,627	221,157
Fixed Effect Cells	106,517	94,150
Sample Means		
Dependent Variable	7.25	7.35
New Nbr Opp Party	0.0861	0.4961

Table 8: Analysis with an Arriver Fixed Effect

This table estimates the effect of a new arrival on incumbents who live in the immediate neighborhood of the arriver (one or two houses down and four houses across the street). The test compares incumbents who have the opposite party affiliation of the new arrival to other incumbents who do not. Each arrival happens in just one quarter, so the new arrival fixed effect compares incumbents in the same immediate neighborhood at the same time. The control variables are the same as those used in column (6) of Table 2. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)	
Sample:	*Current Residents Living in Immediate Neighborhood of a New Arrival*	
	(1)	(2)
New Nbhd-Nbr Opp Party	0.436***	0.395***
	(0.121)	(0.128)
Control Variables		X
Fixed Effects		
New Arrival	X	X
Counts		
N	1,066,754	947,865
Fixed Effect Cells	305,340	277,013
Sample Means		
Dependent Variable	7.23	7.27
New Nbr Opp Party	0.1101	0.1124

Table 9: Learning the New Neighbors' Type or Time Transaction Cost

Column (1) conducts a similar analysis to column (6) of Table 2 except (i) on the subset of experiments that occurred during non-election (odd) years and (ii) using sell within 1 year as the outcome instead of sell within 2 years. Column (2) uses the same outcome variable but on the subset of experiments that occurred during election (even) years. Columns (3) and (4) are the same except with the outcome as sell within 2 years as in our main tests. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Sample:	*Current Residents Who Got New Nearby Neighbors*			
Dependent Variable:	Current Resident Sold Within 1 Year (0-100)		Current Resident Sold Within 2 Years (0-100)	
Subsample, Year of New Nbr:	Nonelection Years (1)	Election Years (2)	Nonelection Years (3)	Election Years (4)
New Nbr Opp Party	0.042	0.121	0.354*	0.267
	(0.133)	(0.142)	(0.203)	(0.225)
Controls	X	X	X	X
Fixed Effects:				
Group × Qtr × Party × Race	X	X	X	X
Counts				
N	218,318	221,935	218,318	187,413
Fixed Effect Cells	65,565	67,150	65,565	56,631
Sample Means				
Dependent Variable	3.09	3.52	7.61	8.00
New Nbr Opp Party	0.2450	0.2445	0.2450	0.2477

Table 10: Heterogeneity in the Main Effect over Market "Hotness"

To create this table, we look at all listings with a non-missing close date and calculate the difference between the close date and the list date. We calculate the median value of this variable for each county-by-quarter and say that a county-by-quarter housing market is "hot" if the median value is less than 100 days and "cool" otherwise. Columns (1) and (2) split the sample into hot and cool markets and include a group-by-party-by-race fixed effect. Columns (3) and (4) use the same split, and include, instead, a quarter-by-party-by-race fixed effect. Column (5) conducts the same analysis as column (6) of Table 2 but interacting the main effect with a dummy for if the county-by-quarter market is hot or not. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)				
Sample:	*Current Residents Who Got New Nearby Neighbors*				
Subsample, Housing Market	Hot (1)	Cool (2)	Hot (3)	Cool (4)	All (5)
New Nbr Opp Party	0.508***	0.133	0.634***	0.215*	0.014
	(0.185)	(0.121)	(0.170)	(0.112)	(0.186)
New Nbr Opp Party × Hot					0.703**
					(0.291)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Party × Race	X	X			
Qtr × Party × Race			X	X	
Group × Qtr × Party × Race					X
Counts					
N	175,081	349,379	177,830	352,440	388,568
Fixed Effect Cells	9,928	15,930	279	306	117,139
Sample Means					
Dependent Variable	8.42	7.09	8.39	7.08	7.82
New Nbr Opp Party	0.2395	0.2479	0.2394	0.2478	0.2461

Table 11: Heterogeneity Over Partisanship

This table refits the model from column (6) of Table 2. For each column, we limit the sample by the partisanship of current residents and new neighbors. We define households as partisan if they participated in more than 75% of the November federal elections in which they were eligible to vote between 2004 and 2018. More specifically, we include eight elections: The 2004, 2006, 2008, 2010, 2012, 2014, 2016, and 2018 November elections. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)						
Sample:	*Current Residents Who Got New Next-Door Neighbors*						
Subsample, Current Resident:	*All*	*Nonpartisan*	*Partisan*	*All*	*Nonpartisan*	*Partisan*	*All*
Subsample, New Neighbor:	*Nonpartisan*	*Nonpartisan*	*Nonpartisan*	*Partisan*	*Partisan*	*Partisan*	*All*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New Nbr Opp Party	0.204	0.127	0.226	0.456*	0.739	0.196	0.398
	(0.278)	(0.759)	(0.351)	(0.247)	(0.800)	(0.285)	(0.351)
Curr Res Partisan							-4.760***
							(0.195)
New Nbr Partisan							0.339
							(0.236)
Curr Res Part × New Nbr Part							-0.684***
							(0.264)
New Nbr Opp Party × Curr Res Part							-0.122
							(0.386)
New Nbr Opp Party × New Nbr Part							0.127
							(0.480)
New Nbr Opp Party × Curr Res Part × New Nbr Part							-0.217
							(0.539)
Control Variables	X	X	X	X	X	X	X
Fixed Effects							
Group × Qtr × Party × Race	X	X	X	X	X	X	X
Counts							
N	157,032	34,237	80,991	184,196	32,528	107,551	405,731
Fixed Effect Cells	56,680	14,663	31,763	64,272	14,129	40,140	122,196
Sample Means							
Dependent Variable	7.97	11.43	6.03	7.65	11.82	5.86	7.79
New Nbr Opp Party	0.2349	0.2143	0.2446	0.2549	0.2395	0.2607	0.2463

Table 12: Defining Current Resident's Partisanship Using Just Election Participation Prior to the New Neighbor's Arrival

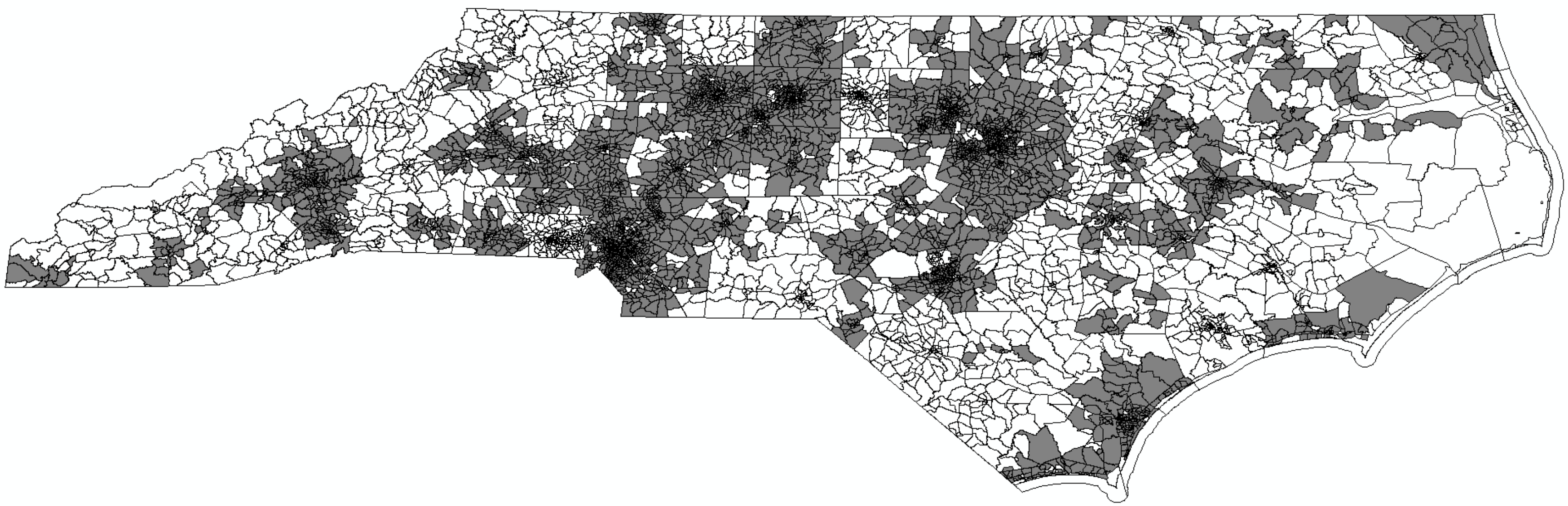
This table is identical to Table 11 except current resident partisanship is defined by whether or not they voted in the most recent federal election. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (0-100)						
Sample:	*Current Residents Who Got New Next-Door Neighbors*						
Subsample, Current Resident:	*All*	*Nonvoting*	*Voting*	*All*	*Nonvoting*	*Voting*	*All*
Subsample, New Neighbor:	*Nonpartisan*	*Nonpartisan*	*Nonpartisan*	*Partisan*	*Partisan*	*Partisan*	*All*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New Nbr Opp Party	0.063	0.478	0.048	0.654**	1.145	0.433	0.287
	(0.316)	(0.838)	(0.400)	(0.271)	(0.883)	(0.312)	(0.399)
Curr Res Voting							-2.437***
							(0.227)
New Nbr Partisan							0.318
							(0.265)
Curr Res Voting × New Nbr Part							-0.653**
							(0.299)
New Nbr Opp Party × Curr Res Voting							-0.210
							(0.442)
New Nbr Opp Party × New Nbr Part							-0.051
							(0.603)
New Nbr Opp Party × Curr Res Voting × New Nbr Part							0.300
							(0.539)
Control Variables	X	X	X	X	X	X	X
Fixed Effects							
Group × Qtr × Party × Race	X	X	X	X	X	X	X
Counts							
N	130,838	22,911	77,610	160,745	22,774	104,629	347,096
Fixed Effect Cells	47,885	9,740	30,130	56,270	9,802	38,639	105,714
Sample Means							
Dependent Variable	8.24	9.21	7.55	7.89	9.84	7.08	8.04
New Nbr Opp Party	0.2305	0.2144	0.2357	0.2516	0.2371	0.2567	0.2428

A Internet Appendix for "Political Polarization Affects Households' Financial Decisions: Evidence from Home Sales"[1]

Figure A1: Map of Coverage of FE Cells

This figure shades in dark blue the census block groups in North Carolina where at least one "experiment" occurs at some point in the sample.



[1] McCartney, William, John Orellana-Li, and Calvin Zhang, Internet Appendix to "Political Polarization Affects Households' Financial Decisions: Evidence from Home Sales," Journal of Finance. Please note: Wiley is not responsible for the content or functionality of any supporting information supplied by the authors. Any queries (other than missing material) should be directed to the authors of the article.

Table A1: Main Results – All Coefficients

This table is identical to Table 2 but also includes the estimates of all the control variables. Data are from the merged CoreLogic Solutions Real Estate and North Carolina voter registration data set. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (=100)							
Sample:	*All Current Residents*		*Current Residents Who Got New Nearby Neighbors*				*& Nonmissing CLTV*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Has Nbr Opp Party	1.019***	0.353***						
	(0.034)	(0.034)						
New Nbr Opp Party			0.640***	0.278***	0.251*	0.314**	0.517**	0.513**
			(0.087)	(0.107)	(0.139)	(0.151)	(0.257)	(0.257)
Neighbor Politics								
Nbr: Unaffiliated (omitted)								
Nbr: Democrat	0.744***	0.181***						
	(0.023)	(0.020)						
Nbr: Republican	1.474***	0.407***						
	(0.021)	(0.017)						
New Nbr: Democrat			-0.694***	-0.223**	-0.235	-0.190	-0.081	-0.077
			(0.091)	(0.111)	(0.145)	(0.158)	(0.265)	(0.265)
New Nbr: Republican			-0.674***	-0.257**	-0.200	-0.112	-0.271	-0.278
			(0.092)	(0.109)	(0.142)	(0.154)	(0.258)	(0.258)

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Dependent Variable:	Current Resident Sold within 2 Years (=100)							
Sample:	*All Current Residents*		*Current Residents Who Got New Nearby Neighbors*				*& Nonmissing CLTV*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
...table continued from previous page								
Current Resident Demographics								
Born in NC		-1.079***				-0.583***	-0.675***	-0.639***
		(0.017)				(0.116)	(0.204)	(0.204)
Age: Under 40 (omitted)								
Age: 41 to 60		-4.371***				-4.718***	-4.302***	-4.468***
		(0.029)				(0.153)	(0.225)	(0.223)
Age: 61 and over		-4.148***				-4.895***	-5.346***	-5.763***
		(0.031)				(0.175)	(0.305)	(0.289)
Tenure: 2 Years or Less (omitted)								
Tenure: 3 to 4 Years		3.390***				4.055***	4.492***	4.519***
		(0.031)				(0.203)	(0.276)	(0.276)
Tenure: 5 to 6 Years		4.182***				5.082***	5.945***	5.973***
		(0.039)				(0.247)	(0.343)	(0.343)
Tenure: 7 to 8 Years		4.070***				5.587***	6.939***	6.969***
		(0.045)				(0.276)	(0.389)	(0.389)
Tenure: 9 years or more		2.970***				3.937***	5.026***	5.063***
		(0.037)				(0.218)	(0.328)	(0.328)
New Nbr Different Race (=1)						0.082	0.010	0.006
						(0.167)	(0.271)	(0.271)
CLTV: 0% (omitted)								
CLTV: (0%, 80%]							-0.247	
							(0.342)	
CLTV: (80%, 95%]							0.290	
							(0.365)	
CLTV: (95%, 125%]							1.147***	
							(0.366)	
table continued on next page...								

Dependent Variable:	Current Resident Sold within 2 Years (=100)							
Sample:	*All Current Residents*		*Current Residents Who Got New Nearby Neighbors*				*& Nonmissing CLTV*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
...table continued from previous page								
Property Characteristics								
Bldg Sq Ft: Less than 1,249 (omitted)								
Bldg Sq Ft: 1,250 to 1,999		0.094***				0.733***	-0.109	-0.196
		(0.026)				(0.249)	(0.515)	(0.516)
Bldg Sq Ft: 2,000 to 2,999		-0.163***				0.190	-0.889*	-1.096**
		(0.029)				(0.262)	(0.529)	(0.529)
Bldg Sq Ft: More than 3,000		-0.644***				-0.065	-1.254**	-1.567***
		(0.032)				(0.283)	(0.559)	(0.559)
Year Built: Before 1960 (omitted)								
Year Built: 1960 to 1979		-0.224***				0.021	0.220	0.240
		(0.025)				(0.310)	(0.679)	(0.680)
Year Built: 1980 to 1999		0.905***				1.254***	1.771***	1.756***
		(0.028)				(0.314)	(0.662)	(0.662)
Year Built: After 1999		2.230***				2.385***	2.494***	2.461***
		(0.035)				(0.336)	(0.679)	(0.679)
Fixed Effects								
Group × Year × Party × Race				X				
Group × Qtr × Party × Race		X			X	X	X	X
Counts								
N	44,566,950	44,439,968	617,539	565,171	457,057	405,731	168,059	168,059
Fixed Effect Cells		1,274,229		95,600	135,139	122,196	57,990	57,990
Sample Means								
Dependent Variable	6.99	7.00	7.46	7.64	7.79	7.79	9.09	9.09
Has Nbr Opp Party	0.1266	0.1266						
New Nbr Opp Party			0.2401	0.2407	0.2418	0.2463	0.2303	0.2303

Table A2: Clustering

This table presents the results of estimating column (6) of Table 2 under different clustering regimes. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable:	Current Resident Sold within 2 Years (=100)							
Sample:	*Current Residents Who Got New Nearby Neighbors*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Nbr Opp Party	0.314**	0.314**	0.314**	0.314**	0.314**	0.314**	0.314**	0.314*
	(0.151)	(0.150)	(0.153)	(0.156)	(0.134)	(0.155)	(0.137)	(0.161)
Controls	X	X	X	X	X	X	X	X
Fixed Effects:								
Group × Qtr × Party × Race	X	X	X	X	X	X	X	X
Cluster Level	Single	Single	Single	Single	Single	Single	Double	Double
Cluster Detail								
Tract × Year	X							
Group × Quarter		X						
Tract			X				X	
Group				X				X
Year					X		X	
Quarter						X		X
Counts								
N	405,731	405,731	405,731	405,731	405,731	405,731	405,731	405,731
Fixed Effect Cells	122,196	122,196	122,196	122,196	122,196	122,196	122,196	122,196
Sample Means								
Dependent Variable	7.79	7.79	7.79	7.79	7.79	7.79	7.79	7.79
New Nbr Opp Party	0.2463	0.2463	0.2463	0.2463	0.2463	0.2463	0.2463	0.2463

Table A3: Falsification Tests for Research Design Two

This table presents the coefficient estimates of the "effect" of getting a new opposite-party nearby neighbor on a number of outcomes related to characteristics of the current residents and their properties. The sample is identical to the one used in Table 7. Control variables include all those included in the main test reported in column (1) of Table 7, except for the variable that is being analyzed on the left hand side. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. Variables are as defined in the text and Table 1. Cash purchase is observed only for those current residents' whose purchase loan we observe. CLTV > 95% equals 1 if the current resident's purchase mortgage had a combined loan-to-value ratio above 95%. This variable is defined only for those current residents whose mortgage amount was strictly greater than 0. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Sample:	*Current Residents Who Got New Nearby Neighbors*								
Category	Homeowner Characteristics						Property Characteristics		
Dependent Variable:	Tenure (Qtrs)	Age (Yrs)	Born in NC	Partisan	Cash Purchase	CLTV > 95%	Log Bldg Sqft	Home Age (Yrs)	Log Assd Value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
New Nbr Opp Party	0.087**	-0.095*	-0.008***	0.002	-0.003	-0.000	-0.006***	-0.812***	-0.003**
	(0.034)	(0.050)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.042)	(0.001)
Controls	X	X	X	X	X	X	X	X	X
Fixed Effects									
Group × Qtr × Party × Race	X	X	X	X	X	X	X	X	X
Counts									
N	1,456,627	1,456,627	1,456,627	1,456,627	678,077	678,077	1,456,627	1,456,627	1,087,657
Fixed Effect Cells	106,517	106,517	106,517	106,517	79,581	79,581	106,517	106,517	78,076
Sample Means									
Dependent Variable	21.22	52.38	0.30	0.67	0.11	0.31	7.80	22.01	12.29
New Nbr Opp Party	0.0861	0.0861	0.0861	0.0861	0.0804	0.0804	0.0861	0.0861	0.0830

Table A4: Falsification Tests for Research Design Three

This table presents the coefficient estimates of the "effect" of getting a new opposite-party nearby neighbor on a number of outcomes related to characteristics of the current residents and their properties. The sample is identical to the one used in Table 8. Control variables include all those included in the main test reported in column (2) of Table 8, except for the variable that is being analyzed on the left hand side. Standard errors, adjusted for clustering at the tract-by-year level, are reported in parentheses. Variables are as defined in the text and Table 1. Cash purchase is observed only for those current residents' whose purchase loan we observe. CLTV > 95% equals 1 if the current resident's purchase mortgage had a combined loan-to-value ratio above 95%. This variable is defined only for those current residents whose mortgage amount was strictly greater than 0. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Sample:	*Current Residents Who Got New Nearby Neighbors*								
Category	Homeowner Characteristics						Property Characteristics		
Dependent Variable:	Tenure (Qtrs)	Age (Yrs)	Born in NC	Partisan	Cash Purchase	CLTV > 95%	Log Bldg Sqft	Home Age (Yrs)	Log Assd Value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
New Nbr Opp Party	0.000	0.068	-0.003	-0.008***	0.001	-0.002	-0.002**	0.022	-0.002***
	(0.012)	(0.061)	(0.002)	(0.002)	(0.002)	(0.004)	(0.001)	(0.031)	(0.001)
Controls	X	X	X	X	X	X	X	X	X
Fixed Effects									
Group × Qtr × Party × Race	X	X	X	X	X	X	X	X	X
Counts									
N	947,865	947,865	947,865	947,865	392,246	392,246	947,865	947,865	709,369
Fixed Effect Cells	277,013	277,013	277,013	277,013	139,201	139,201	277,013	277,013	205,270
Sample Means									
Dependent Variable	21.41	51.52	0.30	0.65	0.10	0.34	7.74	25.02	12.22
New Nbr Opp Party	0.1124	0.1124	0.1124	0.1124	0.1043	0.1043	0.1124	0.1124	0.1097