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MINORITY REPRESENTATION IN LOCAL GOVERNMENT

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ABSTRACT

Does minority representation in a legislative body differentially impact outcomes for minorities? To examine this question, we assemble a novel dataset identifying the ethnicity of over 3,500 California city council candidates and study close elections between white and nonwhite candidates. We find that narrowly elected nonwhite candidates generate differential gains in housing prices in majority nonwhite neighborhoods. This result, which is not explained by correlations between candidate race and political affiliation or neighborhood racial composition and income, suggests that increased representation may help reduce racial disparities. Consistent with a causal interpretation, results strengthen with increased city-level segregation and council-member pivotality. Observed changes in business patterns and policing underpin our results.

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"The principal difficulty lies, and the greatest care should be employed in constituting this Representative Assembly. It should be in miniature, an exact portrait of the people at large. It should think, feel, reason, and act like them." — John Adams, 1776

1. Introduction

The Voting Rights Act (VRA) of 1965 is one of the most important pieces of legislation in U.S. history. Its passage returned the franchise to millions of southern blacks (Grofman et al., 1992) and helped reduce racial disparities in public spending and the provision of public goods (Cascio & Washington, 2013). Later amendments to the VRA and related court decisions have pushed not only for greater ballot access but also for greater representation of minorities in elected office.[1] Implicit in these efforts is the notion that representation at *both* the electoral *and* legislative stages of the political process is necessary to adequately serve the needs of minority citizens. The debate over this issue remains salient today, as a lack of minority representation in local governments is pointed to as a driver of racial disparities in outcomes as varied as housing[2], economic development[3], and policing.[4] With increased efforts to reduce partisan gerrymandering, the issue will likely stay policy relevant, as efforts to limit partisan gerrymandering will necessarily reduce the number of majority-minority districts and may, in turn, reduce the number of black members of Congress.

Despite its central importance in policy debates, whether the election of minority candidates will convey more effective representation for minority groups – beyond what was achieved through the grant of the franchise – remains an open question. Economic theory is split on the matter. Spatial competition/median voter models (Hotelling, 1929; Downs, 1957) and models that focus on appeals to swing groups (e.g., Dixit & Londregan, 1996) suggest that the election of a group member *per se* should not affect policy outcomes. Conversely, "citizen-candidate" models, where politicians are motivated to implement their preferred policies (Osborne & Slivinski, 1996; Besley & Coate, 1997), as well as models where candidates are incentivized to induce core constituencies to vote (Glaeser et al., 2005), suggest that electing minority representatives could lead to different policy outcomes.

[1] We provide further background information on these efforts in Section 2.
[2] https://www.citylab.com/equity/2018/07/when-blacks-joined-city-government-zoning-decisions-changed/564056/
[3] https://nextcity.org/daily/entry/anaheim-city-council-vote-latino-district-at-large-california
[4] https://www.demos.org/publication/problem-african-american-underrepresentation-city-councils

We are thus left with an empirical question. Unfortunately, there is limited systematic evidence on whether minority representation differentially affects minority outcomes.[5] We endeavor to close this gap by studying close elections between white and nonwhite candidates running for city council in California between the years 2005 and 2011. We adopt a regression discontinuity approach that exploits the narrowness of the victory as a source of identifying variation. We pair this election data with comprehensive housing transaction microdata, which allow us to identify the extent to which the election of a nonwhite city council member generates a differential change in housing prices in majority nonwhite neighborhoods.

Our focus on housing markets follows the long tradition of using house prices as a sufficient statistic for valuing public and private investments at the neighborhood level.[6] Housing prices are unique in their ability to offer a deep measure of welfare that can be tabulated by neighborhood racial composition for a broad set of cities. Neighborhood level data is important since many of the goods and services provided by city governments are localized and impact only certain areas of the city. Examples include zoning policy, road improvements, public transit, parks, crime, and economic development projects.[7] Relative to examining these policies themselves, however, housing prices allow us to assess net changes in well-being. This distinction is particularly important given the potential for interactions between different types of

[5] There is, of course, a large and related literature examining whether candidate characteristics affect overall policymaking. See, for instance, Grose (2005) on black members of congress, Ferreira & Gyourko (2009, 2014) on the partisan affiliation and gender of mayors (respectively), Beach & Jones (2016) on business experience for city council candidates, or Hopkins & McCabe's (2012) summary of the extant literature on black mayors. These studies rarely ask whether minority representation differentially impacts minority resident outcomes, perhaps (as we discuss below) because local race-tabulated measures of welfare are difficult to find. Three notable exceptions are Logan's (2018) analysis of black political leaders in the reconstruction era, which considers tax and land policy as well as the black-white literacy gap, Nye et al.'s (2014) analysis of black mayors elected in large cities, and Pande's (2003) analysis of the impacts of quotas for members of underrepresented castes in Indian state legislatures. Nye et al. (2014) is perhaps most relevant to us in that they consider modern and local elections in the US, although they focus only on large cities. They find that the election of a black mayor is associated with improved black labor market outcomes, but their study cannot distinguish the impact of candidate race from candidate party or effects based on race from those based on income. Both of these issues are essential for understanding whether a causal link between minority representation and minority outcomes exists, which is why we attempt to rule out both of these possibilities empirically. Two other related papers are Beach & Jones (2017) and Sances & You (2017). Beach & Jones (2017) study the impact of council *diversity* on overall levels of public good provision; however, they (as well as the broader literature on the impact of diversity on policymaking) are generally silent on the effect of minority representatives on outcomes for minority groups. Sances & You (2017) document a relationship between the share of a city's population that is black and the use of fines as revenue, but also find that this relationship diminishes with the increased black representation on the city council. However, their findings are largely descriptive, and the authors themselves caution against interpreting them as causal.

[6] Seminal examples include: Oates, 1969 (tax policy); Black, 1999 (school quality); Linden & Rockoff, 2008 (crime); and, Chay & Greenstone, 2005 (environmental quality), and Turner et al., 2014 (land use regulation).

[7] Note that elected school boards (rather than city councils) determine local school policy and spending.

policies. For example, Albouy et al. (2018) show that proximity to a park increases house prices when the park is safe but decreases prices when the park is unsafe. Thus, examining just one policy dimension may offer an incomplete picture of how well-being is changing. Albouy et al.'s findings also highlight one mechanism through which minority representation may impact housing prices. Even if the election of a nonwhite councilmember does not immediately result in the construction of a new park (or similar project), it may lead to increased maintenance or policing of existing public assets in majority nonwhite neighborhoods – and therefore affect housing markets. Housing prices are also unique in that they reflect expectations about the future stream of amenities (Bishop & Murphy 2011, 2018).[8] In short, there are a host of channels through which minority representation may differentially affect minority neighborhoods and housing prices offer a unique proxy for assessing these changes.

Thus, we begin our analysis by considering housing markets. We find that the election of a nonwhite candidate is associated with higher housing prices in majority nonwhite neighborhoods and lower housing prices in majority white neighborhoods; this result is relative to the election of a white candidate. As we document in a later section, there is a pre-existing gap in housing prices across these two neighborhood types, even controlling for a variety of housing and neighborhood characteristics. This gap may reflect differential access or proximity to local amenities. In this context, our main housing market result can alternatively be stated as follows: the election of a nonwhite candidate reduces the gap in housing prices across minority and non-minority neighborhoods. This result is quite robust. It is not driven by correlations between candidate race and political affiliation or between racial composition and neighborhood income. Price impacts are particularly pronounced when the election causes the council to flip from majority white to majority nonwhite but dissipate once the majority councilmembers are nonwhite. Consistent with the assumption that our results are driven by a spatial reallocation of services to minority neighborhoods, these effects are stronger in more heavily segregated cities, where there is more scope for such reallocation.

We next turn to an analysis of potential mechanisms. Here, data limitations make the analysis more challenging. Nonetheless, we are able to provide evidence that both increased

[8] Bishop & Murphy (2011, 2018) provide theoretical and empirical support for the importance of incorporating homebuyers' forward-looking *expectations* of amenities. This allows for the possibility that our results are driven by changes in expectations about the spatial allocation of local goods and services. Consistent with this idea, we do find evidence that the effects occur relatively quickly. However, we also see little evidence of mean reversion, which suggests that those initial expectations were likely correct.

business activity in minority neighborhoods and changes in police behavior relative to minority citizens are associated with the election of minority council members.

These results complement Washington (2012) and Cascio and Washington (2013), who find that that an increase in minority voters' power at the ballot box generates differences in the behavior of the politicians they elect and results in an increase in transfers to counties with more black residents. We consider increases in representation, independent from enfranchisement, and find that increasing minority representation within the legislative process provides another avenue for addressing racial disparities. In this sense, our work is also closely related to concurrent work by Logan (2018), which shows that the election of black politicians during the Reconstruction era affected overall tax and land policy while also helping to decrease the black-white literacy gap. Our results suggest that the historical patterns documented by Logan (2018) are likely to still hold today.

2. An Overview of Under-representation Amongst U.S. Elected Officials

Minority groups are dramatically underrepresented in politics at all levels of government in the United States. In 2017, roughly 90 percent of all U.S. elected officials (county, state, and federal) were non-Hispanic whites – despite the fact that only 60 percent of Americans identify as non-Hispanic white.[9] This difference is also pronounced within city governments. The 1992 Census of Governments reveals that 92% of city council members (amongst respondents to the Census survey) were white. Although the Census no longer collects such information, a 2011 survey of 654 city governments in the western United States reveals that – like at higher levels of government – roughly 90 percent of city council members are non-Hispanic whites.[10]

Concerns about whether this type of underrepresentation generates a differential impact on residents of different ethnic groups have become increasingly salient in recent years. For example, following the shooting of Michael Brown in Ferguson, MO, the popular press[11] and

[9] This figure is drawn from the New Organizing Institute (NOI) Reflective Democracy dataset, which identifies the ethnicity of over 40,000 elected officials from the year 2017. See http://wholeads.us/electedofficials/ for additional details.

[10] This figure is drawn from the International County/City Management Association (ICMA) 2011 "Municipal Form of Government" survey, which is conducted roughly every five years. ICMA collects the data by sending a survey to city officials. Of 654 cities responding, 80% responded with information about race. Given the focus on western states, this imbalance is even greater: only 57 percent of the adult population identifies as non-Hispanic white in those states.

[11] See, for instance, http://www.latimes.com/opinion/op-ed/la-oe-hajnal-minority-voters-elections-20140827-story.html

think tanks[12] argued that the lack of minority representation in Ferguson's city government might contribute to racial gaps in outcomes. At the time, 66 percent of Ferguson's population was black, while five out of six of its councilmembers were white. Indeed, Hajnal and Trounstine (2014) document that racial and ethnic minorities are less likely to express satisfaction with the services provided by their local governments; they also document that this relationship is strongest in cities where the share of black public employees is the smallest, suggesting a link between minority representation in government and local government responsiveness to minority groups' preferences.

Increasing minority representation in local government as a solution for gaps in policy outcomes is not a new idea. While the Voting Rights Act of 1965 was largely aimed at ensuring equal access to the ballot, later amendments and related court cases focused on ensuring that minorities experienced equal opportunities to be elected to political office. For instance, in a 1980 Supreme Court case, *Mobile v. Bolden*, plaintiffs argued that at-large elections for city councils dilute minority votes, and therefore present a barrier to minority representation. The court ruled against the plaintiffs in that case, arguing that evidence of discriminatory intent is required. In response to the decision in *Mobile v. Bolden,* in 1982 Congress amended the Voting Rights Act, shifting the standard so that a discriminatory electoral *outcome* – rather than actual *intent* – constituted a violation of the Act. This amendment led a number of local governments to switch from at-large elections for city council to district-based elections. These changes resulted in increased minority representation at the local level (Sass & Mehay, 1995; Shah et al., 2013). Beyond the immediate effect on representation in city councils, the 1982 amendment had its first significant impact on the Court's stance towards the ability of minority citizens to elect their preferred candidate in a 1986 case, *Thornburg v. Gingles.* Here, arguments surrounded whether North Carolina's recent redistricting plan diluted the minority vote and violated the Voting Rights Act in doing so. The court ruled that it did. A central point of contention in the case was the evidence collected by the District Court that black voters tended to vote for black candidates and white voters tend to vote for white candidates. Thus, spreading the minority vote across multiple districts when it would have been possible to form a majority-minority district prevented minority citizens from electing their preferred candidate.

[12] See, for instance, http://www.demos.org/publication/problem-african-american-underrepresentation-city-councils

Across all of these developments, there was an implicit (and sometimes explicit) assumption that the election of minority officials is a necessary condition for substantive minority representation in policymaking.[13] This is the claim that we analyze here.

3. City Councils in California

California state law provides a number of guidelines for the structure of municipal governments. City councils must contain at least five councilmembers who are elected "at-large" during a general municipal election. Councilmembers serve staggered four-year terms, with elections filling multiple seats every two years.[14] Elections are nonpartisan, so neither the voters nor we observe a candidate's political party.[15] California state law defines the mayor as simply another member of the city council and does not provide for any additional powers. The mayor is typically selected by the city council from amongst its own members. In these "council-manager" cities, the council (including the mayor) dictates policy for the city, which is in turn carried out by the city manager.

There are two ways that a city can deviate from the above guidelines. If the city is "general law" – the default form of government for incorporated cities – then it can submit a ballot measure to be approved by the electorate. For "chartered" cities, any deviation must be specified in the city's charter. A 2006 survey conducted by the International City/County Management Association (ICMA) provides a number of statistics illustrating that most cities conform to the state's guidelines.[16] Specifically, 93 percent of cities are council-manager cities and the mayor serves on the city council for 98 percent of the cities – because of this, we do not treat mayoral elections differently from any other councilmember election. 88 percent of cities

[13] The question of whether a "minority-preferred candidate" must be a minority was debated by Supreme Court justices during Thornburg v. Gingles case. While the Court was in agreement that North Carolina's redistricting plan prevented minority voters from electing their preferred candidate, there was disagreement over whether a "minority-preferred candidate" must be (and can only be) a minority candidate. Justice Brennan argued "it is the status of the candidate as the chosen representative of a particular racial group, not the race of the candidate, that is important" and that "only the race of the voter, not the race of the candidate, is relevant to vote dilution analysis." Justice White, on the other hand, implies that only a minority can be a minority-preferred candidate, and Justice O'Connor acknowledges that candidate race is at least one of several factors to use in defining the "minority-preferred" candidate (Yut, 1995).

[14] For instance, there may an election in 2004 to fill 3 of the 5 seats followed by an election in 2006 to fill the remaining two seats.

[15] In additional analyses, we link our candidate records to voting registration records. This allows us to restrict to elections where both candidates are from the same party, which produces identical results.

[16] The survey in question is ICMA's 2006 *Municipal Form of Government* survey, which is the source of all of the statistics in this section.

only have five councilmembers, and they are elected at-large for 92 percent of cities. A city's institutional structure also tends to be relatively stable over time. In the five years preceding the survey fewer than seven percent of cities attempted to alter their form of government. When cities do attempt to alter their form of government, it is typically to switch from at-large to district based elections (or vice versa). Many of these attempts, however, are ultimately unsuccessful.

4. Housing Markets and Elections Data

4.1 California Election Data Archive (CEDA)

Our empirical approach draws on narrowly won elections between two candidates of different ethnicities in order to obtain plausibly exogenous variation in minority representation on a city council. Implementing this approach requires information on the closeness of the election as well as the ethnicity of both the winning and losing candidates. The *California Election Data Archive* (CEDA) reports the number of votes each candidate received for every local government election between 1994 and 2014. CEDA also lists the number of council seats that were available, which makes it possible to identify the candidates that narrowly won and narrowly lost the election. In addition to reporting relevant outcome variables, CEDA also lists the candidate's full name. CEDA does not collect information on candidate ethnicity and so we build upon earlier efforts by Beach & Jones (2017) to construct a dataset identifying the race/ethnicity of California city council candidates and members.

4.2 Councilmember and candidate ethnicity data

This subsection describes the data on councilmember and candidate ethnicities constructed here and in Beach & Jones (2017). We focused our data collection efforts for ethnicity on the 5,177 individuals who either (1) served on the city council between 2005 and 2011 or (2) ran for city council but lost, with particular attention paid to "marginal candidates" (the highest vote getter amongst the losing candidates) . Our ethnicity data come from three sources. First, we contacted 450 cities to inquire about the availability of ethnicity information for city councilmembers and

candidates. 230 cities responded to this request but only 96 were able to provide us with any information. Those 96 cities, however, provided ethnicity information for 714 councilmembers and candidates.

To fill the gaps, we collected pictures of councilmembers and candidates from candidate websites, newspaper articles, voting pamphlets, and other sources. We successfully located pictures for 3,615 councilmembers and candidates. After collecting these pictures, we conducted a survey on Amazon's "Mechanical Turk" website where we asked workers to report the candidate's ethnicity based on the candidate's name and picture.[17] The worker could choose from the following options: White, Black, Native American, East Asian or Pacific Islander, Indian, Middle Eastern or North African, or Hispanic. We also asked the worker to identify the sex of the candidate.

We collected ten unique responses for each candidate. There was no limit to the number of photographs a worker could code, but they never observed a candidate more than once.[18] We use these responses to code ethnicity only if a majority of workers agreed on a single response. This restriction removes 31 individuals from our sample. The average rate of agreement for the remaining 3,584 individuals was 94 percent, which implies that on average 9.4 of the ten workers chose the same ethnicity. The average rate of agreement for reported sex was 99 percent.

Because our contact with cities and our collection of pictures occurred simultaneously, there is some overlap between the two samples. For 263 individuals, we have ethnicity information obtained from both the city and from our Mechanical Turk methods. This overlap provides an opportunity to assess the accuracy of our Mechanical Turk method. In general, the Mechanical Turk responses matched the response from cities 95 percent of the time. The correlation between whether the city identified a candidate as White and whether the Mechanical Turk workers identified the candidate as White is 0.89. The correlations for Black, Asian, and Hispanic response are 1, 0.84, and 0.88, respectively.[19]

These correlations suggest that mismatches between city and "Mechanical Turk" responses likely result from the Mechanical Turk choosing "White" instead of "Asian" or

[17] Workers were specifically asked to indicate the race/ethnicity/ancestral background that provides the best description of the individual based on their name and photograph.

[18] For the sake of incentive compatibility, workers were told that the responses from other workers would be used to judge the accuracy of their work. Specifically, workers were not paid unless a majority of their responses matched the modal response for each candidate.

[19] We are unable to compute correlations for Native American, Middle Eastern, and Indian because they are a much smaller share of the population and do not appear in our "overlap" sample.

"Hispanic". To remedy this issue, we obtained ethnicity information on Asian and Hispanic politicians from the *National Association of Latino Elected and Appointed Officials* and the *Asian Pacific American Institute for Congressional Studies*, respectively. These organizations maintain lists of government officials that are of Hispanic or Asian origin, and together they provided us with the ethnicity of 571 Asian and Hispanic candidates (191 of which were not listed in either our "city" or "Mechanical Turk" sources). Because we have ethnicity information from three sources (city, Mechanical Turk, and ethnic lists), we assign candidates an ethnicity in the following way: we use the information provided by cities as the true ethnicity whenever possible. We then rely on the lists obtained from ethnic organizations to identify any remaining Hispanic or Asian candidates. Mechanical Turk responses are used to fill in any gaps. This approach further increases the accuracy of the Mechanical Turk responses because we are only relying on their ability to determine whether a candidate is White, Black, Native American, Indian, or Middle Eastern. Our final sample includes ethnicities for 4,226 of the 5,177 councilmembers and candidates who either served on the city council between 2005 and 2011 or ran for city council but just lost.

4.3 Census Data

We draw on Census block group-level data from the 2000 Decennial Census to measure within-city neighborhood characteristics. Thus, when we refer to "neighborhoods" throughout the paper, we are in fact referring to Census block groups. We use 2000 Census data, as opposed to – for instance – 2010 American Community Survey data, so that neighborhood characteristics are not potentially influenced by our treatment variables.

Our data provide, for every block group, 100% counts of: population, population in urban areas, population in rural areas, males, females, people over the age 18, people over the age 65, households, households with various family structures (single male, single female, married with children, etc.), total housing units, vacant housing units, renter-occupied housing units, and owner-occupied housing units. The data also provide 100% counts by block group for the following race groups: non-Hispanic white, non-Hispanic black, non-Hispanic Asian/Pacific Islander, non-Hispanic Native American, Hispanic, and other race. For all of these variables, we convert these counts into shares. Population density is constructed by dividing the population of

the block group by its land area. We also construct a measure of diversity of the block group, which we use as a control; specifically, we use the race shares and construct the standard fractionalization index for each block group.[20]

Importantly, we use the block group race counts (and, in turn, the constructed race group shares) to identify block groups as *majority white* (block group white population share > 0.5) or *majority nonwhite* (block group white population share < 0.5). In assessing how nonwhite candidates differentially impact neighborhoods with a larger share of nonwhite residents, this is our main means of splitting the data, though we show later in the paper that the results are robust to other methods of identifying "high nonwhite" neighborhoods.[21]

4.4 Housing transactions data and the construction of the adjusted housing price

Our main outcome variable is housing values, which we obtain from transactions data provided by DataQuick Information Systems under a license agreement with the vendor. This dataset includes the universe of single-family home sales in California between 2005 and 2011. The transaction records are matched with assessor data recording the number of bedrooms/bathrooms in each home as well as the number of stories, square footage of the home, and the age of the home at the sale date. We trimmed the top and bottom 1% of observations to eliminate homes transferred for the nominal amount of $1 and homes valued in excess of $2.8 million.

To account for variation in overall price levels across local housing markets and over time, we estimate year-by-quarter price indices for each of the 18 distinct commuting zones (CZ's) in our data and the adjust observed sales prices using these indices.[22] Specifically, to estimate price indices, we regress the log of the transaction price on year-by-quarter-by-commuting zone (CZ) fixed effects, as well as a vector of housing characteristics (e.g., number of bedrooms, and others noted above) and neighborhood characteristics (all of the block group-level shares described in the previous subsection, plus population density and ethnic

[20] Fractionalization is a standard index for measuring diversity and is calculated as: $Fractionalization_{bg,2000} = 1 - \sum_e \left(share_{bg,2000,e}\right)^2$ where $share_{bg,2000,e}$ is the share of the population in block group *bg* during the year 2000 that is of ethnicity *e*.

[21] We also note that, in identifying majority white and majority nonwhite neighborhoods, we exclude the small share of the population that identifies as multiracial in the 2000 Census. To be more precise, a majority nonwhite neighborhood is a block group where [(sum of single-race nonwhite population)/(sum of single-race population)]>0.5. Results are extremely similar if we include multi-racial counts.

[22] See Sieg et al. (2002) for background on estimating spatially delineated price indices.

fractionalization). The year-quarter-CZ fixed effects are taken as the log of the price index for the local housing market at a given point in time. To normalize prices, we then divide nominal prices by the appropriate year-by-quarter CZ-level price index to construct what we refer to throughout as the *adjusted housing price*. We use the log of this adjusted price as our main outcome variable.

4.5 Partisan affiliation of candidates

Finally, in supplemental analyses we test whether our results can be explained by partisan preferences rather than ethnicity, noting that the two are correlated. California city council elections are nonpartisan, in the sense that partisan affiliation is not listed on the ballot. Thus, partisan affiliation is not provided in the California Election Data Archive (CEDA). Instead, we draw on California voter registration data files, which contain the universe of registered voters in California and identify each voter's partisan affiliation (if registered with a party). We match individuals in the voter registration files to the city council candidates listed in CEDA to identify candidates' partisan preferences. We do so in an iterative series of matches based on last name, first name (or first initial), and city (or county), as well as some manual matching. Our matching is quite conservative and is intended to favor missing observations over false matches. Ultimately, we identify 81% of candidates in our sample. We identify the partisan affiliation of both candidates in a relevant election (a narrow election with a white and nonwhite candidate) in 61% of elections.

4.6 Summarizing data and sample restrictions

Our initial dataset (before making sample restrictions) consists of housing transactions in all California cities between 2005 and 2011. A housing transaction is the unit of analysis in our main empirical specifications. To assess the impact of increased minority representation, which – in this setting – is interpreted as the addition of a nonwhite member to a city council, we employ a regression discontinuity (RD) approach. The goal of the RD approach is to generate quasi-random assignment to a treatment (election of a nonwhite councilmember) or counterfactual (election of a white councilmember). To accomplish this goal, we restrict our sample to housing

transactions in cities associated with an election that met the following conditions: (1) of the two marginal candidates (the last-place winner and first-place loser[23]), one is white and the other is nonwhite, and (2) the election was reasonably "close". In our main analysis, "close" elections are those decided by 5.88 percentage points or less.[24]

Table 1 provides basic summary statistics for the cities in our estimation sample, and shows how they compare to California cities as a whole. Column 1 reports average demographics for all California cities. Column 2 reports demographic information for cities with a marginal election between a white and a nonwhite candidate, while Column 3 narrows further to the elections that form our estimation sample – narrowly decided white vs. nonwhite elections. Notably, cities with a nonwhite vs. white election are larger and more ethnically/racially diverse than California cities as a whole (Column 2 vs. Column 1). Cities in the main estimation sample (Column 3) are closer in size to the average California city, but remain more diverse.

Table 2 provides summary statistics on election characteristics, again throughout California (Column 1), for all white vs. nonwhite elections (Column 2), and for the estimation sample elections (Column 3). Worth noting in Column 1 is that elections where both marginal candidates are white are most common, but elections between one white and one nonwhite candidate are not rare; 34% of elections fall into this category. As noted in Columns 2 and 3 the marginal nonwhite candidate is often Hispanic, but, as we discuss later, our results are not solely driven by Hispanic candidates. Relative to all California elections, elections between a white and a nonwhite candidate are more likely to be district-based and are also more likely to be an election to a larger council (Column 2 vs. Column 1). Both of these differences are likely driven by the fact that cities that make up the Column 2 sample are larger, and larger cities are more likely to hold district-based elections and have larger councils. In Column 3, we see that – in our main estimation sample – the councils that candidates are running for are more like the average California council.

Finally, given our focus on differences in prices between majority white and majority nonwhite neighborhoods and how the election of a nonwhite councilmember affects this difference, it is useful to establish the baseline differences in prices across these two types of

[23] Because most city council elections are "at-large", elections typically fill multiple seats. For instance, a city council may have three seats to fill, with five candidates competing. In such an election, the relevant candidates from our perspective are the third place and fourth place candidates, or the last-place winner and the first-place loser.

[24] This optimal bandwidth was chosen following Calonico et al. (2014). See section 5.2 for more details.

neighborhoods. Figure 1 plots the distributions of adjusted log prices in majority nonwhite neighborhoods (gray bars) and majority white neighborhoods (clear bars with black outline). The distribution of prices in white neighborhoods is clearly shifted more to the right than the distribution of prices in nonwhite neighborhoods, indicating that houses in majority white neighborhoods are more expensive.

In Table 3 we explore how much of this gap can be explained by observable house and neighborhood characteristics. In the table, we report a series of simple regressions from the full sample, before making any election-related restrictions. Column 1 reports a bivariate regression, simply regressing adjusted log price on an indicator for whether the house is in a majority nonwhite block group. These results confirm what is seen in Figure 1; house prices are lower, on average, in majority nonwhite neighborhoods. Moving from Column 1 to Column 5, we gradually incorporate additional controls (including city fixed effects, as many of our main specifications will include fixed effects at that level). We see that much (but not all) of the gap across neighborhoods can be explained by observable house and neighborhood characteristics. In Column 5, our richest specification, we still observe a significant point gap in prices between houses in nonwhite neighborhoods and white neighborhoods; houses in nonwhite neighborhoods sell for roughly 5.5 percentage points less than houses in majority white neighborhoods, even after controlling for a rich set of house and neighborhood controls. Thus, in assessing the differential impact of candidate ethnicity on house prices in nonwhite neighborhoods, we are essentially testing whether minority representation reduces or increases the pre-existing gap in house prices across neighborhood types that we have documented here.

5 Basic cross-sectional regression discontinuity approach

Our main empirical approach is a panel-based regression discontinuity design, similar to Cellini et al. (2010). Before turning to those results, however, this section describes the basic form of our empirical approach and previews our main results using a simpler cross-sectional regression discontinuity model.

5.1 Empirical approach (Cross-sectional RD)

Broadly, our main empirical specifications are local linear regressions estimated within a narrow bandwidth in "nonwhite margin of victory" around the cutoff value (Nonwhite margin of victory = 0). We define a "nonwhite margin of victory" as the difference between the nonwhite candidate's vote share and the white candidate's vote share. A positive margin of victory indicates that the nonwhite candidate is successfully elected and a nonwhite margin of victory close to zero indicates an extremely close election.

Taking housing transactions as our unit of observation we begin by restricting the sample to transactions in cities that had a narrow election between a white and nonwhite candidate. Since councilmembers serve staggered four-year terms, the composition of the council is only stable for two years. Accordingly, we further restrict our sample to transactions occurring during the two-year "council term" following the relevant election.[25]

The simplest version of our empirical specification is thus:

$$\begin{aligned} \ln(p)_{hct} = {} & \propto + \beta_1 \mathbf{1}[Nonwhite\ wins_{ct}] + \beta_2 margin\ of\ victory_{ct} \\ & + \beta_3 \mathbf{1}[Nonwhite\ wins_{ct}] * margin\ of\ victory_{ct} \\ & + \varepsilon_{hct} \end{aligned} \tag{1}$$

where $\ln(p)_{hct}$ is the adjusted log price of house h in city c during council term t. $\mathbf{1}[Nonwhite\ wins_{ct}]$ is an indicator variable equal to one if, in the narrow election between a white candidate and nonwhite candidate, the nonwhite candidate wins and is elected to office. This indicator variable is fully interacted with the nonwhite candidate's "margin of victory". The coefficient on the main effect of $\mathbf{1}[Nonwhite\ wins_{ct}]$, β_1, therefore identifies the effect of a nonwhite candidate winning conditional on the margin of victory being zero, which therefore implies a very close election. Thus, under the assumption that winners of very close elections are

[25] We restrict our analysis to the first two years for two reasons. First, it is possible that the councilmembers elected in the first election after a nonwhite candidate's victory are somehow a reaction to the nonwhite candidate and/or the policies adopted during the nonwhite councilmember's first two years in the council. Second, failing to restrict the time window of housing transactions would generate results that overweight early elections relative to late elections. This is especially relevant in this setting given the time period: early elections occurred prior to the housing crisis and recession, while later elections did not. For similar reasons, in our panel specifications (discussed more in a later section) we restrict our analysis to one council term before and after a relevant election. We should note, however, that results are very similar without the two-year restriction. In the panel approach, we also include housing transactions that occur in the two years prior to the relevant election.

essentially random (an assumption that is particularly likely to hold in low-information and low-turnout elections such as city council races), β_1 identifies the *causal* impact of electing a nonwhite candidate (rather than a white candidate).[26]

The above equation identifies the impact of increased minority representation on housing values overall. Our main interest, however, is the differential impact of minority representation on housing values in minority neighborhoods. To address this question, we modify equation (1), fully interacting all of the relevant variables (nonwhite win, margin of victory, the interaction of nonwhite win and margin) with an indicator variable set equal to one if the house is located in a majority nonwhite block group. The modified specification is then:

$$\begin{aligned} \ln(p)_{hct} = {} & \propto + \beta_1 \mathbf{1}[Nonwhite\ wins_{ct}] + \beta_2 margin_{ct} + \beta_3 \mathbf{1}[Nonwhite\ wins_{ct}] * margin_{ct} \\ & + \beta_4 \mathbf{1}[Nonwhite\ wins_{ct}] * \mathbf{1}[Nonwht.BG_h] + \beta_5 margin_{ct} * \mathbf{1}[Nonwht.BG_h] \\ & + \beta_6 \mathbf{1}[Nonwhite\ wins_{ct}] * margin_{ct} * \mathbf{1}[Nonwht.BG_h] + \beta_7 \mathbf{1}[Nonwht.BG_h] + \varepsilon_{hct} \end{aligned} \quad (2)$$

where "Nonwht. BG" is an indicator for whether the block group is majority nonwhite. Now, β_1 identifies the causal impact of a nonwhite victory on house prices in majority white neighborhoods. β_4 (the coefficient on the interaction between "Nonwhite wins" and "Nonwht. BG") identifies the *differential* effect of a nonwhite victory on nonwhite neighborhoods, or the impact of a nonwhite victory on the gap between housing values in nonwhite and white neighborhoods. β_4 is therefore the coefficient in which we are most interested.

Finally, the empirical specifications reported in this section control for housing characteristics (number of bedrooms, bathrooms, stories, square footage), neighborhood characteristics (as described in the data section), year-month dummies, and city-specific linear time trends. Because the final estimation sample is highly selected (narrow elections between white and nonwhite candidates, with only two years of observations after the election, etc.), we "control" for these characteristics by estimating a general hedonic regression of adjusted log house price on these controls in the universe of housing transactions, and then taking the residual. In practice, our outcome variable is the residual of a hedonic regression, with housing, neighborhood, and time characteristics differenced out. In the next section (the panel RD

[26] Note that, while the regression is estimated within a range of margin of victory that is already reasonably narrow, identification of a causal effect occurs *at the cutoff*. Observations within our narrow bandwidth, but farther from the cutoff, are simply used to fit a linear relationship between housing prices and nonwhite margin of victory on both sides of the cutoff. Mechanically, β_1 is the difference between the two fitted lines at a margin of victory of zero.

approach), we present a fuller set of results where we exclude some or all of these controls to assess their importance.

5.2 Bandwidth selection

Several authors have proposed methods to identify the optimal bandwidth in a local linear RD approach (Calonico et al., 2014; Imbens & Kalyanaraman, 2012). These attempt to balance the benefits of a narrower bandwidth (estimates drawn from observations that are very close to the cutoff, increasing confidence in identifying a casual effect) with the benefits of a wider bandwidth (more observations lending more power). These methods are well-suited to identifying a bandwidth when one outcome is associated with each election, as would be the case – for instance – when testing whether a narrow victory for politician *i* in election *t* impacts that politician's vote share in election *t+1*. However, our setting involves a large number of housing transaction observations associated with each narrow victory. Using typical bandwidth selection procedures on our full sample would yield an artificially small bandwidth, as there are many observations very close to the cutoff, but many of them belong to the same small set of cities/elections.

Instead, to identify a bandwidth, we collapse our observations to the election level. That is, for each relevant election, we take the average of log housing prices, adjusted as described in Section 4.4, in the two years following the election. This yields a single observation per election. We then ultimately use the Calonico et al. (2014) bandwidth selection procedure, which suggests that the optimal bandwidth in our setting is 5.88 percentage points. Our main specifications include all marginal elections between a white and nonwhite candidate, conditional on the election being decided by 5.88 percentage points or less (in either direction). Ideally, the Calonico et al. bandwidth selection procedure is used for each specification, but given our data and approach, that is impractical. We instead take a bandwidth of 5.88 percentage points as our main bandwidth, and – at several points in our analysis – demonstrate the robustness of our results to a variety of alternative bandwidths.

5.3 Assessing the validity of our regression discontinuity design

Before proceeding to the results, we address a common concern in regression discontinuity designs: the running variable (in this case, nonwhite margin of victory) should be balanced around the cutoff (the point where one candidate barely wins). This characteristic implies that there should be roughly the same number of observations just to the left of *nonwhite margin=0* as there are just to the right of *nonwhite margin=0*. This issue is especially important to our research design. Not only have some questioned the "randomness" near the cutoff when applying regression discontinuity designs to electoral outcomes (Caughey & Sekhon, 2011; Grimmer et al., 2011), but Vogl (2014) documents concerns specifically in the context of race and city politics (primarily only in southern states in the US). In Figure 2, we follow McCrary (2008) and plot a discontinuous density function around the cutoff (nonwhite margin=0). The figure demonstrates that the density just to the left of the cutoff is statistically indistinguishable from the density just to the right of the cutoff. Thus alleviating, in our dataset, the concerns raised by Vogl.[27]

It is also critical to the regression discontinuity design that other observable characteristics be smooth around the cutoff. We assess this assumption in a series of figures reported in the appendix (Appendix Figures 1, 2, and 3). There we see that for a wide variety of city, candidate, and housing characteristics, there is no clear discontinuity at the cutoff. There is one important exception (seen in Appendix Figure 2): we find that the likelihood that the winning candidate is a Democrat jumps dramatically at the cutoff. In other words, consistent with correlations between partisan affiliation and ethnicity in the general population, we find that a nonwhite candidate is more likely to be a Democrat (and less likely to be a Republican) than a white candidate. While this finding is not surprising, it may mean that our results are in fact driven by partisan differences rather than differences between white and nonwhite candidates. We can, however, directly test for this concern, and we do so in a later section. Ultimately, we find no evidence that our results are driven by partisan differences.

[27] Code for this procedure is available from McCrary's website.

5.4 Cross-sectional regression discontinuity results

We now turn to the results of our cross-sectional regression discontinuity approach. Columns 1 and 2 of Table 4 report the results of estimating equations (1) and (2) respectively within a 5.88 percentage point bandwidth around the cutoff. For parsimony, we only report the coefficients that identify the causal impact of a nonwhite victory on housing prices and suppress additional coefficients (e.g., margin of victory). Column 1 of Table 4 reveals that there is no overall impact of a nonwhite victory on overall housing prices. Housing prices increase by 0.9 percentage points, but the estimate is not statistically significant. However, turning to Column 2 – where we allow for differential effects by neighborhood type – we see that the ethnic identity of the winning candidate has a substantial impact on outcomes. Relative to cities where a white candidate narrowly wins, the election of a nonwhite candidate is associated with a 5.9 percentage point reduction in housing values in majority white block groups (based on the "Nonwht. winner" coefficient). Conversely, nonwhite winners have a significantly positive *differential* effect on housing values in majority nonwhite block groups (based on the "Nonwht. winner X Maj. nonwht. BG" coefficient). Moreover, this is a positive effect on houses in nonwhite neighborhoods overall: the linear combination of the two coefficients (reported in the bottom panel of the table) suggests that nonwhite housing values are roughly 5.5 percentage points higher after a nonwhite candidate wins.

We emphasize that these results, like most of the results reported in this paper, are relative to the election of a white candidate. The relative effect could be driven either by nonwhite councilmembers causing higher prices in nonwhite neighborhoods or lower prices in white neighborhoods. Alternatively, the relative effect could be driven by white councilmembers causing either lower prices in nonwhite neighborhoods or higher prices in white neighborhoods. Later analyses suggest that our data are more consistent with the latter explanation.

Figure 3 presents the same pattern of results graphically. These figures are binned scatterplots: we take the nonwhite candidate's margin of victory on the x-axis, and take the average of housing values (within bins in nonwhite margin) on the y-axis. A line is fit through the resulting averages. Figure 3(a) includes all data in our estimation sample and, as with the regression analysis, reveals no clear change in overall housing values as we move from narrow nonwhite losses (just to the left of a margin of victory of zero) to narrow nonwhite wins (just to

the right). Figure 3(b) restricts the estimation sample to majority white block groups. Here, average housing values are lower just to the right of the cutoff, providing additional evidence of the negative effect of a nonwhite candidate on housing values in white neighborhoods. Similarly, Figure 3(c), which restricts the estimation sample to majority nonwhite block groups, suggests a positive effect of nonwhite victory on housing values.[28]

Appendix Figure 5 documents that results are similar across a wide range of bandwidths, ranging from 0.03 to 0.12 (roughly one-half to double the main bandwidth).[29] Appendix Table 1 reports alternative RD specifications. There, the same pattern of results emerges when we collapse the data to block-group level averages and also when we employ a global polynomial RD approach. A placebo test is also reported in Appendix Table 1. There are no effects of the election of a nonwhite candidate in the two years preceding the election, minimizing concerns about trends preceding the election.

6 Panel regression discontinuity approach

In this section we employ a panel-based regression discontinuity approach similar to that used by Cellini et al. (2010). Broadly, the goal in shifting to a panel-based strategy is to increase our confidence in recovering a causal effect by taking advantage of both across-city and within-city variation in identifying the effect of electing a nonwhite candidate. Our approach here also has the advantage of allowing us to separately identify the effects of white and nonwhite winners, which we discuss in more detail below.

6.1 Methodology

Like Cellini et al. (2010), our level of treatment under this approach is not the city, but is instead the election (or, the election-by-city pairing).[30] For each relevant election, we restrict the sample

[28] Note that Figure 3(c) is not directly comparable to the "Nonwht. Win X Maj. Nonwht. BG" coefficient from Table 3, Column 2. That coefficient captures the differential effect of a nonwhite win on nonwhite block groups (relative to the effect on white block groups). The Figure captures the overall effect of a nonwhite win on nonwhite block groups. Figure 3(c) is therefore comparable to the linear combination "Nonwht. Win" + "Nonwht. Win X Maj. Nonwht. BG" reported at the bottom of Table 3.

[29] Appendix Figure 4 documents the numbers of unique housing transactions and elections contained within each bandwidth.

[30] Cellini et al. (2010) study the impacts of school bond elections on local outcomes. Other aspects of their methodology, not employed here, are aimed at dealing with the fact that a failure to pass a municipal bond in one

to the council terms immediately preceding and following a relevant election.[31] To reflect the level of treatment, our main specifications include election fixed effects, not city fixed effects. For cities with only one relevant election during the sample period, election fixed effects are equivalent to city fixed effects. For a city with more than one relevant election, each relevant election is treated as an entirely separate panel, with a different fixed effect. In other words, our data in this approach is set up as a set of four-year panels centered around elections, with two years of pre-election observations and two years of post-election observations. The presence of pre- and post- observations, and the inclusion of election-level fixed effects, will ultimately allow us to identify the impact of electing a nonwhite (or white) candidate relative to a baseline level of housing prices in the same city in the two years prior to the election.

More precisely, we estimate variations on the following specification:

$$\begin{aligned} \ln(p)_{hect} = \propto &+ \beta_1 \mathbf{1}[Nonwhite\ wins_{ec}] + \beta_2 margin_{ec} + \beta_3 \mathbf{1}[Nonwhite\ wins_{ec}] * margin_{ec} \\ &+ \beta_4 \mathbf{1}[Nonwhite\ wins_{ec}] * \mathbf{1}[Post_{ect}] + \beta_5 margin_{ec} * \mathbf{1}[Post_{ect}] \\ &+ \beta_6 \mathbf{1}[Nonwhite\ wins_{ec}] * margin_{ec} * \mathbf{1}[Post_{ect}] + \beta_7 \mathbf{1}[Post_{ect}] + \gamma_{ec} + \varepsilon_{hect} \end{aligned} \quad (3)$$

This equation is similar to equation (1) in that it is aimed at identifying the overall effects of nonwhite victory on housing prices, and does not yet allow for differential effects by neighborhood type. We take the adjusted house price for house *h* in city *c*, sold within two years (before or after) of election *e*, as our outcome. On the right hand side, we include the same "Nonwhite wins", "margin of victory", and interaction variables, but note that these are now defined with respect to the election *e* (and city *c*). We then fully interact each of those variables with a new indicator variable, $\mathbf{1}[Post_{ect}]$, which is equal to one if the housing transaction occurs in the two years *after* election *e* and zero otherwise. We also include election fixed effects, γ_{ec}.

Note that, in reference to a particular election, the winning candidate and his/her margin is constant throughout the four-year panel. Therefore, the election fixed effects absorb all of the main effects of the RD-related variables (nonwhite wins, margin, and their interaction). The RD variables interacted with "Post" are not absorbed by the fixed effects, and allow us to identify the effect of a nonwhite candidate's victory. In particular, of primary interest is β_6, the coefficient on

year impacts the likelihood that another bond election is held again soon. With regularly scheduled city council elections every two years, this is not a problem in our setting.

[31] As before, "council term" refers to the stable two year period in which there is no changes in the composition of the city council.

"Nonwhite wins" X "Post". This coefficient identifies the differential effect of being in a city where a nonwhite candidate has very narrowly won (again, because this coefficient identifies the effect of "Nonwhite wins" *conditional* on Nonwhite margin of victory equaling zero) in the two years after that election occurred (rather than the two years before), relative to the counterfactual where a white candidate wins.

As in the cross-sectional estimations, our primary focus is testing whether candidate ethnicity has different effects on different types of neighborhoods. For our main specifications, we therefore estimate a modified version of equation (3), where we interact all "treatment" variables (nonwhite winner, margin, post, etc.) with an indicator variable equal to one if a block group is majority nonwhite and zero otherwise. Of primary interest are the coefficients on "Nonwhite wins" X "Post", which identifies the effect of a nonwhite winner on housing values in white neighborhoods, and "Nonwhite wins" X "Post" X "Nonwhite block group", which identifies the *differential* effect of a nonwhite winner on housing values in nonwhite neighborhoods.

6.2 Main results

Our main results are reported in Table 5. Panel A reports specifications where we do not allow for heterogeneous effects of councilmember race by neighborhood type (Equation 3). Panel B reports modified specifications where we interact all relevant variables with an indicator for whether a house is in a majority nonwhite block group in order to test for differential treatment effects. All of these specifications restrict the sample to the optimal bandwidth (5.88 percentage points) and include election-level fixed effects.

As we move from Column 1 to Column 4, we include a larger set of controls. Column 1 simply takes the adjusted log house price as the outcome with no controls for house or neighborhood characteristics. Column 2 includes minimal controls for housing characteristics. Column 3 adds controls for neighborhood characteristics, and Column 4 controls for city-specific time trends. Column 4 is our richest specification, and represents our main approach.[32]

[32] To clarify: as we move from Column 1 to Column 4, controls are added to the regression used to construct the residual which we take as our outcome variable.

In Panel A, we find that the election of a nonwhite councilmember has no statistically significant effect on average housing values at a citywide level. This is true regardless of which specification is used. In our preferred specification (Column 4), a nonwhite candidate's victory is estimated to increase housing values by 0.3 percent.

Panel B shows that, despite the absence of a clear citywide effect, the election of a nonwhite candidate does have clear distributional effects. Across all specifications, we find a significant positive coefficient on "Nonwhite win X Post X Majority nonwhite block group", which indicates that the election of a nonwhite candidate (rather than a white candidate) leads to differential increases in housing values in nonwhite neighborhoods. This result can be interpreted as a reduction in the pre-existing gap in housing values across white and nonwhite neighborhoods.

In our preferred specification (Column 4), we find that the election of a nonwhite winner leads to housing values in majority white neighborhoods that are 5.6 percentage points lower than if a white candidate had won. We observe a large differential positive effect in majority nonwhite neighborhoods. The effect on nonwhite neighborhoods is 8.8 percentage points more positive than the effect on white neighborhoods.

Towards the bottom of the table we calculate the combined effect. That is, we consider the linear combination of "Nonwht win X Post" + "Nonwht win X Post X Maj nonwht BG". This statistic allows us to assess whether houses in majority nonwhite block groups experienced an absolute increase in value when a non-white candidate won. Here we find a net price increase of 3.2 percentage points when a nonwhite (rather than white) candidate is elected, although this absolute effect is less precisely estimated. Again, as in the cross-sectional RD results, we emphasize that these results reflect the differential effect of a nonwhite candidate winning relative to the effect of a white candidate winning. These differential changes could either be driven by the nonwhite candidate or the counter-factual white candidate or some combination of both. We explore that issue more directly in a later section – either way, this differential price effect serves to reduce the pre-existing gap between white and non-white neighborhoods.

6.3 Robustness of Main Results

Figure 4 documents that the estimates from our main specifications are robust to a variety of alternative bandwidths. Panel A reports the estimated coefficient on "Nonwht. win (X Post)" from a specification where we do not allow for differential treatment effects by neighborhood type (similar to Panel A of the previous table). For bandwidths from 0.03 to 0.12, there is no evidence of a change in average citywide housing prices.[33] Panels B and C report estimates from a specification where we do allow for differential treatment effects (similar to Panel B of the previous table). Panel B reports the estimated "Nonwht. win (X Post)" coefficients from a variety of bandwidths, while Panel C reports the estimated "Nonwht. win X Nonwhit. BG (X Post)" coefficients. These figures demonstrate that our results are robust to a wide range of bandwidths.

Appendix Table 2 reports a series of sensitivity and placebo tests. The main results are robust to including city fixed effects or block group-by-election fixed effects, in lieu of the election fixed effects used elsewhere. Our results are also robust to alternative methods of controlling for whether a block group has a high share of nonwhite residents.[34] We also report the results of a placebo test, where – rather than taking the two years before ("pre") and two years after ("post") a relevant election – we take four years before the election as our sample period and set the placebo-post dummy equal to one in the two years before the election. The test reveals no effect. As in the cross-sectional analysis, these results help mitigate concerns around trends in the housing market prior to the election. Next, we test to see if our results could be explained by systematic changes in the composition of houses that sell following the election of a white (non-white) candidate.[35] We find no evidence of composition effects. In a somewhat related set of tests, Appendix Table 3 reports results of specifications aimed at assessing whether there is any change in sales volume when a nonwhite candidate narrowly wins; we find no such evidence overall or when allowing for differential effects in majority nonwhite block groups.

[33] As noted before, Appendix Figure 4 reports the numbers of housing transactions and elections contained within each bandwidth.

[34] Specifically, we show that results are the same if: the definition of "Majority nonwhite" accounts for multiracial residents (Column 3), the "Majority nonwhite" dummy is replaced with a dummy indicating that the share of nonwhite residents in a block group exceeds the local median (Column 4), and "Majority nonwhite" dummy is replaced with a continuous measure of the share of nonwhite residents (Column 5).

[35] Here, we construct an index of housing characteristics based on the coefficients of a single hedonic regression and test for systematic changes in this quantity index.

Finally, in Appendix B, we discuss and report analysis where we separately consider the impacts of Black, Asian, and Hispanic candidates. Our main analysis focuses on the impacts of a nonwhite candidate (broadly defined) winning when running against a White candidate. We do so mainly for the sake of statistical power; focusing on specific groups dramatically reduces the number of close elections we can leverage for identification. Similarly, our main analysis assesses the differential impact of a nonwhite candidate's victory on housing prices in majority nonwhite neighborhoods. While nearly 50% of block groups in California are majority nonwhite, the numbers of block groups that are majority Hispanic, Black, or Asian/Pacific Islander are much smaller, which limited our ability to consider the impacts of a nonwhite candidate's victory on more specific neighborhood types. Appendix B nonetheless aims to provide a sense of how results vary for specific groups, both with respect to winning candidates and neighborhood composition. Overall, though less precise, the results broadly reveal that group-specific effects are consistent with our main results. They also reveal that – especially for Black and Hispanic candidates – specific types of nonwhite candidates tend to have an impact on all types of nonwhite block groups, which helps to justify our more parsimonious focus on just nonwhite vs. white block groups in the main analysis.

6.4 Heterogeneity: pivotality, timing, income, ownership, segregation and party

Having documented our main result in section 6.2 and the robustness of that result in section 6.3, we now evaluate heterogeneity in how and where the observed patterns occur.

We first consider *when* additional minority representation matters. That is, does an additional nonwhite councilmember only impact outcomes when they shift the council away from majority white? To explore this issue, we re-estimate our main specification on four mutually exclusive subsamples, based on the composition of the rest of the council: (1) councils where the nonwhite candidate would become the first nonwhite member on the council; (2) councils where the nonwhite candidate would not be the first nonwhite member, but the council would remain majority white even with the election of the nonwhite candidate; (3) councils where the nonwhite candidate is "pivotal" – his or her election would shift the council from majority white to majority nonwhite; and (4) councils where there would be a nonwhite majority regardless of whether the nonwhite candidate is elected or not. These results are presented

graphically in Panel B of Figure 5. For the sake of comparison, Panel A of the figure depicts our main coefficient estimates; the white bar represents the impact of a nonwhite candidate victory in majority white neighborhoods (the "Post X Nonwht. win" coefficient); the gray bar represents the differential impact of a nonwhite victory in majority nonwhite neighborhoods (the "Post X Nonwht. win X Nonwht. BG" coefficient). The results show that the election of a nonwhite candidate has little impact when the nonwhite candidate is the first nonwhite member on the council or when there is already a nonwhite majority on the council. However, the results also show that the election of a nonwhite candidate can have an impact even when the nonwhite candidate is not "pivotal"; we observe strong impacts of nonwhite wins both in cases where the nonwhite candidate is pivotal, but also when the nonwhite candidate is non-pivotal (but not the first nonwhite member on the council).

Next, we consider how our estimates evolve over time, both within the two-year council period and beyond. To do so, we estimated an extended version of our model wherein we extend the model to include up to four years (rather than two years) after the election date.[36] We also replace the "Post" indicator with a series of dummy variables indicating that a housing transaction is taking place within: each six month interval during the first two years following the election and one more indicator for transactions occurring during the final two years of the councilmembers elected term. These results are depicted graphically in Figure 6. Panel A reports the "Nonwhite wins (X [time period])" coefficients (indicating the impact of a nonwhite winner on housing values in white neighborhoods) while Panel B reports the "Nonwhite wins X Nonwhite block group (X [time period])" coefficients (indicating the differential impact of a nonwhite winner on nonwhite block groups).

We see that, during the two years that a council is stable, the pattern observed in our main results occur immediately and remain persistent during the entire two-year period. Looking beyond the two years following the election, the effect of a nonwhite winner is somewhat attenuated and imprecisely measured. That is, we cannot reject that there is no effect of a nonwhite candidate beyond the initial two years that he or she is in office. We caution however that, because of our relatively short panel, there is substantial variance in the number of years – and therefore in the number of housing transactions – available in the data after the initial two-

[36] Given our relatively short panel, it is often the case that there are not four full years' worth of data after an election present in our sample, so we extend the sample to either include four years after the election or whatever smaller number of years possible.

year period. In particular, the "> 2 year" estimates are implicitly weighted towards cities with relevant elections early in the sample period.[37] Furthermore, the transactions occurring beyond years 2 and 4 also coincide with the next election cycle, which has the potential to introduce noise into our estimates. It therefore remains unclear whether there is a return to the baseline after two years, a compositional shift in the sample used to generate the estimate, or simply a loss of precision in the estimates driven by a smaller sample.

In Figure 7, we consider several additional dimensions of heterogeneity in our treatment effects; both to rule out alternative explanations for our results and also to shed light on the mechanisms driving our results. As with Figure 5, we present results graphically, with estimates from our main specification in Panel A for comparison. First, given correlations between neighborhood income levels and minority share, it is possible that our results are the result of distributional shifts in policy attention to or away from wealthy or less wealthy neighborhoods, rather than shifts to or from higher minority share and lower minority share neighborhoods. While we control for block group-level income characteristics (median income, percent below the poverty line, and percent on public assistance) in all of our main specifications, we did not allow for interactions between winning councilmember ethnicity and these characteristics. To test whether income correlations explain our results, we split our sample into "high median income" block groups and "low median income" block groups. We define a block group as "high median income" if the median income in the block group is above the sample median of median income across all block groups; when this is not true, a block group is defined as "low median income". We then repeat our main specification on these subsamples. Figure 7 Panel B reports the impact of a nonwhite victory (for white neighborhoods and the differential effect on nonwhite neighborhoods) separately for "Low income" and "High income" block groups. Notably, in both subsamples, the pattern of results are qualitatively similar to our main results. This suggests that our main results are not entirely explained by distributional shifts towards lower income neighborhoods rather than distributional shifts towards neighborhoods with a higher share of nonwhite residents. In Panel C, we test whether results differ in areas with a high

[37] This could be especially relevant as the housing crisis and Great Recession occur in the middle of our sample. The ">2 year" period for cities with earlier "treating elections" would have fallen in the middle of these events. Of course, our identification strategy compares the effect of a nonwhite victory relative to a white victory in the same time period, so that alone is not enough to explain the return of prices to baseline. It is possible however that there is an interactive effect of increased minority representation and the Great Recession.

versus low share of renters, again defining "high" and "low" relative to the sample median of block group renter share, and again find that results are similar across the two subsamples.[38]

Panels D and E consider a different dimension. Here, we assess how a city's level of segregation interacts with our results. As discussed above, an important potential mechanism for explaining our results is the possibility that a nonwhite candidate wins and directs resources and services towards nonwhite neighborhoods. In order for this channel to be effective, there must be clear nonwhite neighborhoods to direct resources towards, which would generally occur to a greater degree in more segregated cities. Therefore, if this mechanism explains our results, we would expect stronger results in more segregated cities. This is exactly what we see in Panel D. Splitting the sample into cities that are high or low on a city-level dissimilarity index (which measures segregation),[39] we see that our results are almost entirely driven by more segregated cities. Panel E aims to measure segregation at a more local level; specifically, for every block group, we measure the nonwhite share of surrounding block groups. We then split the sample into block groups with a high or low nonwhite share in neighboring block groups, with "high" defined as above sample median. A nonwhite block group surrounded by mostly white neighboring block groups is more isolated than a nonwhite block group surrounded by other nonwhite block groups. Consistent with Panel D, we find that the impact of a nonwhite victory on nonwhite block groups is larger for the more isolated block groups, though the difference between the two estimates is not statistically significant.

We next address another important alternative explanation for our results. As noted earlier, there is a correlation between a candidate's ethnicity and a candidate's partisan preferences. Recall that we have matched our data on city council candidates to voter registration files to identify candidates' registered partisan affiliations. In our data, of the nonwhite candidates involved in white versus nonwhite elections, 70% are registered as Democrats, 20% are registered as Republicans, and 10% are registered as some other party (or indicated no party

[38] Related to the two tests discussed here: one concern is that increased housing prices in majority nonwhite neighborhoods are evidence of gentrification, which may represent a negative outcome for nonwhite residents. While we cannot fully rule this out, we test whether the election of a nonwhite candidate is associated with any change in the count or rate of evictions at the block group level. We find no evidence of a change in evictions, either overall or in nonwhite block groups in particular. (These results are available upon request.) This, paired with the similar effects observed across high and low income neighborhoods, point away from a gentrification explanation for our results.

[39] We use the typical two-group dissimilarity index as our measure of diversity, with the two groups in question being white and nonwhite.

preference). Of white candidates, 38% are Democrats, 48% are Republicans, and 14% are other/no preference.[40] This led to the one source of covariate imbalance around the cutoff in Appendix Figure 2: a narrowly elected nonwhite candidate is significantly more likely to be a Democrat.

To address this issue, In Panel B of Figure 8, we re-estimate our main specification on a sample that excludes all elections where one of the two marginal candidates is a Republican and the other is a Democrat. If our main result were driven by the fact that white vs. nonwhite elections often imply Republican vs. Democrat elections, then when excluding such elections we should expect something closer to a null result. Instead, results are very similar to our main results, if somewhat less precise due to the reduction in sample size.

The remainder of Figure 8 reports additional sensitivity tests. We drop the three largest cities in our sample in Panel C, drop cities with district-based elections in Panel D, and drop the small number of cities with large (>7 members) councils in Panel E. Across all three panels, results are very similar to the main result. The exclusion of district-based elections is perhaps the most noteworthy of these three results; a plausible alternative explanation for our results might have been that nonwhite neighborhoods benefit from the election of a nonwhite councilmember, but only because some elections are district-based, so this in fact may simply reflect a councilmember generating benefits for his or her own district which happens to match his or her ethnicity. Panel D shows that cannot be the full explanation for our results.

6.5 Are these results driven by white or nonwhite winners?

The results thus far indicate that, relative to electing a white candidate, the election of a nonwhite candidate leads to lower housing values in white neighborhoods and a more positive differential effect on nonwhite neighborhoods. These results are consistent with two possible narratives: (1) the election of a nonwhite candidate directly leads to lower housing values in white neighborhoods and higher housing values in nonwhite neighborhoods or (2) the election of a nonwhite candidate has relatively little effect, while the election of a *white* candidate leads to

[40] It is worth noting that the strength of the nonwhite-Democrat correlation weakens in cities where there was a *close* election between a white and nonwhite candidate. The white-Republican correlation is essentially the same in these cities as in the full sample. Restricting the sample to *close* elections between a white and nonwhite candidate: 63% of nonwhite candidates are Democrats, 28% of nonwhite candidates are Republicans; 38% of white candidates are Democrats, 50% of white candidates are Republicans.

higher housing values in white neighborhoods and lower housing values in nonwhite neighborhoods. Our panel-based RD approach allows us to speak to which of these two narratives is more consistent with the data. Unlike in a cross-sectional RD approach, where identification stems from comparing cities where nonwhite candidates narrowly won to cities where they narrowly lost, here our identification of the effect of increased minority representation stems from comparing housing prices in a city where a nonwhite (or white) candidate was narrowly elected to housing prices *in the same city* in the years just before that election occurred. This allows us to speak to the distinct effects of electing white or nonwhite councilmembers.

Recall that in our main panel-based RD estimating equation (Equation 3), we interact the typical RD variables (nonwhite wins, nonwhite margin) with a "Post" dummy equal to one in the two years after the relevant election and equal to zero in the two years prior. Throughout the paper, we have focused our attention on the "Post X nonwhite wins" and "Post X nonwhite wins X nonwhite block group" coefficients, which identify the effect of a narrowly elected nonwhite winner on majority white block groups and the differential effect of a nonwhite winner on majority nonwhite block groups *relative* to any counterfactual effects of narrowly elected white winners. We note, however, that unless the "Post" dummy itself – which is also included in the specification but not reported in the tables in this paper – captures a generic post-close election effect, then it can be interpreted as the effect of a narrowly elected *white* winner on white block groups.[41] Likewise, the "Post X Nonwhite block group" coefficient can be interpreted as the differential effect of a *white* winner on nonwhite block groups. Similarly, the linear combination of the "Post" and "Post X Nonwhite winner" coefficients can be interpreted as the overall effect of a nonwhite winner, rather than simply the effect of a nonwhite winner *relative* to a white winner, with similar interpretations for coefficients interacted with "Nonwhite block group".

As noted, this interpretation comes with the caveat that there must not be a general post-election effect on all cities (regardless of the ethnicity of the winner). Appendix Table 4 shows that there is little or no evidence of such a general post-election effect.[42] We therefore feel

[41] Recall that there are also "Post X Nonwhite margin" interactions. This explains why "Post", by itself, captures the effect of a white winner when "Nonwhite margin" is zero, or very close to it.

[42] Columns 1 and 2 use a sample that includes both "close" (within the 5.88 percentage point bandwidth) elections and elections that are not close (wins or losses of up to 50 percentage points). In our four-year election panels, we regress the log of adjusted house price on the "Post" dummy. We control for the same set of house, neighborhood, and time characteristics as in the cross-sectional specifications as well as election fixed effects. That specification

confident interpreting the "Post" (and relevant interacted variables) as the effect of a white winner, and the linear combination of "Post" and "Post X Nonwhite winner" (and relevant interactions) as the overall effect of a nonwhite winner. Appendix Table 5 replicates our main results (mirroring Table 5), but reports the additional "Post" coefficients and linear combinations of interest. Focusing on our richest specification in Column 4, we find that the models actually indicate that our results may be driven by the election of white councilmembers rather than nonwhite councilmembers. The estimated effect of a white councilmember on majority white block groups is 0.052, significant at the 5% level. The estimated differential effect of a white councilmember on majority nonwhite block groups is -0.093, significant at the 10% level. The same estimates for nonwhite councilmembers are -0.004 and -0.02, respectively, with neither estimate statistically significant. In short, as we have documented throughout the paper, nonwhite councilmembers have an effect *relative* to what would happen if a white councilmember had been elected; This last set of results suggest that this relative effect may in fact be driven by white councilmembers' victories. However, given that this interpretation hinges on the ability to interpret the "Post" coefficient solely as the effect of a white councilmember winning a narrow election, these results should be read with caution.

7 Evidence on mechanism from outside of the housing market

To better understand the mechanisms underlying our main housing price results, we consider the impact of increased nonwhite representation on a variety of non-housing outcomes. We first consider a broader set of non-housing outcomes that could reasonably be expected to reflect underlying council activities/policies. We then turn to direct measures of city government activity and policy. The second of these two categories can only be measured at the city level. Thus, while we can examine whether these activities and policies change with the election of a nonwhite candidate, we cannot test whether those changes disproportionately target nonwhite

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reveals no *general* impact on house prices in the two years after an election relative to the two years before. Of greater interest is whether there is a post-election effect specifically for close elections. Column 2 runs a specification similar to Column 1, but interacts "Post" with a dummy indicating that the election was close, defined here to mean that the election fell within our main bandwidth of 5.88 percentage points. Again, there is no evidence of a generic post-election effect, whether in close elections or landslide elections. Finally, even if there is not a post-election effect as a whole, or a differential post-election effect in the narrow bandwidth (relative to the rest of the sample), our identification occurs at the cutoff (margin of victory of zero). Thus, Column 3 tests whether there is a generic post-election effect at the cutoff by restricting the sample to the main bandwidth and interacting "Post" with the margin of victory. The noninteracted "Post" identifies the effect of being in the post-period conditional on the election being very close (specifically, conditional on a margin of victory of zero). Again, there is no effect.

neighborhoods. Outcomes in the first category, on the other hand, are often available at a sub-city level.

All results reported in this section employ the panel-based regression discontinuity design used elsewhere in the paper, with all specifications estimated with a bandwidth of 5.88 for consistency with our main results. Unless otherwise noted, the outcomes in this section are measured on an annual basis.

7.1 Evidence on other city and neighborhood outcomes

We begin our analysis of broader outcomes by considering the spatial distribution of business activity. City council members can play an important role mediating between constituent business owners and the city's various regulatory and permitting agencies. Thus, one way that council members may differentially effect neighborhood level outcomes is through their impact on the spatial patterns of business activity. To assess this channel, we draw on the Census Bureau's ZIP Code Business Patterns data. These data report the number of business establishments by ZIP code on an annual basis. Panel A of Table 6, Column 1 reports the effect of a nonwhite candidate's victory on the log of business establishments throughout a city, which is not significantly different than zero. However, turning to Panel B, where we allow for a differential effect in majority nonwhite ZIP codes, we observe a pattern consistent with our housing price results: relative to the election of a white candidate, the election of a nonwhite candidate is associated with a differential increase in business establishments in majority nonwhite areas, and a small decrease in majority white areas.[43]

Next, we draw on data from the Environmental Protection Agency's Toxic Release Inventory (TRI) program, which records the presence of business facilities that release toxic chemicals into the environment. We collapse the data to a count of TRI facilities at the Census tract-by-year level and test whether the election of a nonwhite candidate impacts the overall presence or local distribution of TRI facilities. Specifically, we take as our outcome variable an indicator for whether a TRI facility is operating within each tract-year pairing.[44] Column 2 of Table 6 reports these results. Again, Panel A reports the citywide impact of a nonwhite candidate

[43] We have tested whether prices of non-residential properties change, either citywide or by neighborhood type, and find no such evidence.
[44] 96.38% of tract-year pairings have either 0 or 1 TRI facilities.

while Panel B allows for a differential effect in majority nonwhite Census tracts. We observe no significant effects in either panel. This is perhaps noteworthy in light of our finding from Column 1. Column 1 documents that more business establishments are found in nonwhite areas when a nonwhite candidate is elected and Column 2 suggests that the business establishments moving in do not pose an environmental threat to the local residents, at least as far as that can be measured by the TRI data.

We also test whether there is any change in the presence of developments that qualify for a Low-Income Housing Tax Credit (LIHTC). LIHTC is a Federal tax credit provided to developers to encourage the development and construction of affordable housing. Although city governments do not administer the tax credit, cities must still provide building permits for new developments and could play a role in encouraging LIHTC-eligible development. Column 3 of Table 6 tests for any effect of a nonwhite candidate on the presence of LIHTC development, and fails to detect a significant impact.[45]

Next, we ask whether nonwhite councilmembers affect policing outcomes. We consider police outcomes for two reasons. First, as noted in Section 2, both the popular press and think tanks have recently argued that a lack of minority representation within city government might contribute to the poor treatment of blacks by city police officers. Second, a large literature has documented a strong link between crime and property values (Linden & Rockoff, 2008; Pop & Pope, 2012).

We draw on data from the Federal Bureau of Investigation's Uniform Crime Report. The data report arrests at the city-by-year level, so we are unable to test for differential effects on different neighborhoods. The data do, however, report arrests separately by race group, which we take advantage of to test for differential effects. These results appear in Table 7. In Column 1, we take the log of arrests per capita as our outcome variable, and find no evidence of a change in overall arrest rates.[46] Columns 2 and 3 take nonwhite and white arrest rates as outcomes, respectively; the results are imprecisely estimated, but the direction of the coefficients suggest a decrease in nonwhite arrests and an increase in white arrests. This combination of results is

[45] The LIHTC program provides incentives to locate in neighborhoods based on the income of residents; as LIHTC development is already directed towards particular neighborhoods on the basis of something other than race, it may be unsurprising that we do not observe a differential effect here.

[46] In additional analyses, not reported here, we test whether the election of a nonwhite candidate is associated with a change in reported crime. We find no evidence that this the case. This impacts the interpretation of our result in Table 7; for instance, if reported crime was decreasing but arrests remained constant, that could suggest an increase in the intensity of policing. Instead, our results may suggest no change in the intensity of policing.

captured differently in Column 4, where we take as an outcome the share of total arrests where the arrested individual is nonwhite. Despite the lack of change in overall arrest rates, the results in column 4 suggest that election of a non-white council member leads the composition of arrests to shift away from nonwhite residents.

To summarize, our first set of non-housing analysis produces three main results: 1) the election of a nonwhite candidate is associated with a differential increase in business establishments in nonwhite areas, 2) we see no change in exposure to TRI facilities amongst majority nonwhite neighborhoods, and 3) we observe evidence of a shift in arrests, away from nonwhite individuals. These set of results reinforce our main results and point towards nonwhite councilmembers generating improvements in welfare for residents of nonwhite neighborhoods.

7.2 Evidence on city policy

We next report results on outcomes that directly reveal actions taken by the city government. These data are measured only at a city-wide level, and so we are unable to test for differential targeting of policies towards particular groups or areas within the city. Nevertheless, the results do offer some further insight on how minority councilmembers may affect policy.

Table 8 assesses whether the election of a nonwhite candidate impacts a city's expenditures or revenues. We draw on data from California Cities' Annual Financial Transaction Reports. All outcomes are real dollar amounts (taking 2010 as a base year), measured on a per capita basis, and logged. The table shows that the election of a nonwhite council member has no identifiable impact on overall expenditure or revenue, nor on specific categories of expenditures (general government administration, salaries, public goods[47], public safety, transportation, or community development).

These null results are important for two reasons. First, they are consistent with the larger literature on candidate identity and policymaking at the local level, which has largely shown that candidate identity does not influence policymaking. Again, what is missing from this literature is an analysis of the distribution of funds across neighborhoods and/or groups. Our analysis of direct policy changes suffers from the same data limitations as this extant literature. However,

[47] "Public goods" is measured as the combination of spending on public safety, transportation, community development, health, culture, and leisure. The measure therefore includes a large bulk of cities' total expenditures, excluding debt servicing and internal costs (government salaries, etc.).

our analysis of housing prices, business activity and policing outcomes suggests that who we elect can have important differential impacts across neighborhoods/groups. Viewed through this lens, the lack of city-wide budget impacts documented in Table 8 points to a mechanism whereby nonwhite candidates may be shifting spending away from white neighborhoods/residents and towards nonwhite neighborhoods/residents.

In the Appendix, we present two additional sets of results. First, we test whether the election of a nonwhite candidate leads a city to adopt new ordinances through updates to their General Plans, which dictate cities' policies on issues like land use, housing, noise, etc. These results, which draw on data from California's Annual Planning Survey, are reported in Appendix Table 6. There, we observe no clear indication that the election of a nonwhite councilperson generates a change on this front. Next, in Appendix Table 7 we draw on building permits data to tests whether the election of a nonwhite candidate impacts the number (or valuation) of building permits for residential properties and find no clear change.[48]

Overall, we find no evidence of any impact on the measures which more directly reflect changes in city policy; however, none of these measures can be disaggregated to the neighborhood level. Thus, the null results here are consistent with the absence of an effect of a nonwhite candidate on *city-wide* outcomes. Indeed, the combination of these results with our housing results highlight the advantage of taking housing prices as our main outcome, as they reveal in a way that the policy outcomes cannot that the impact of a nonwhite candidate may only occur at sub-city level.

8 Conclusion

Our paper examines the impacts of minority representation on the distribution of public and private investments in cities, specifically testing whether the election of a nonwhite city councilmember benefits majority nonwhite neighborhoods. In doing so, we explore how democratic representation confronts a long-standing problem in American cities: ethnic and racial segregation. Numerous studies have documented the negative impacts of segregation, especially for minority group members. Segregation exacerbates black-white disparities in

[48] We have also tested whether the election of a nonwhite candidate impacts the number of building permits approved for non-residential buildings and find no effect there either. In both cases, we are limited to city-level counts of permits. Thus, as is suggested by our business patterns data, we may well be missing changes in the spatial distributions of permit activity.

income (Ananat, 2011), political efficacy (Ananat and Washington, 2009) and schooling (La Ferrara and Mele, 2006), and dampens the overall level of local public good provision (Trounstine, 2016). It is easy to see how local governments may play a role here; the goods and services they provide are often highly localized in nature, so a large degree of segregation within a city can imply that some race groups have less access to these goods and services. Our study, then, poses the question: can increased minority representation in local government help reduce disparities in the face of segregation?

We draw on a unique dataset, which allows us to identify the ethnicity of a large number of California city council members and candidates. We then focus on narrow elections between white and nonwhite candidates and employ regression discontinuity to generate quasi-random assignment of the winning candidate, which – in turn – influences whether a city experiences increased minority representation. We then assess the extent to which the election of a nonwhite candidate affects nonwhite neighborhoods.

Results indicate that increased nonwhite representation has heterogeneous effects on house prices. In particular, we find that the election of a nonwhite candidate is associated with higher housing values in nonwhite neighborhoods and lower values in white neighborhoods (relative to the election of a white candidate). This result cannot be explained by the correlation between partisan affiliation and race, nor can it be explained by the correlation between the racial composition of neighborhoods and the income level of those neighborhoods. Instead, the results appear to be genuinely driven by the race of the candidate and the race of the neighborhood. This conclusion is reinforced by our finding that the impact of electing a minority candidate is increasing in both pivotality and city-level segregation. Finally, our findings vis-à-vis business patterns and policing outcomes provide some initial insights into mechanism.

These findings are particularly relevant given ongoing efforts to increase the representation of minorities in elected office. Since the 1965 VRA, legislators and courts have tried to ensure equal representation of minorities in both the electoral and legislative stages of the political process. Existing work has shown that increasing representation at the electoral stage generated large benefits to minority populations. However, much less is known about the impact of increasing representation at the legislative stage – a margin upon which there is still much work to be done if the goal is to achieve proportionate representation. Our results show that

electing minority council members can have marked impacts on outcomes in minority neighborhoods.

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TABLES

Table 1: Summary statistics: Characteristics of cities in estimation sample

VARIABLES	(1) All cities	(2) Cities with wht. vs. nonwht. elections	(3) Cities with wht. vs. nonwht. elections, optimal bandwidth (Main estimation sample)
Total population	56,939.67	144,402.31	72,665.14
	(185,534.5)	(432,722.3)	(125,242.2)
Pop. share:	0.09	0.14	0.15
Asian/Pac. Isl.	(0.11)	(0.14)	(0.15)
Pop. share:	0.04	0.06	0.05
Black	(0.06)	(0.08)	(0.06)
Pop. share:	0.30	0.37	0.37
Hispanic	(0.25)	(0.22)	(0.23)
Pop. share:	0.56	0.42	0.42
White	(0.26)	(0.21)	(0.21)
Pop. share:	0.00	0.00	0.00
Other	(0.00)	(0.00)	(0.00)
Ethnic fractionalization	0.44	0.54	0.54
(a measure of diversity)	(0.17)	(0.13)	(0.13)
Number of cities	458		
Number of elections		362	201

Standard deviations in parentheses. Population and ethnicity shares come from the 2000 census.

Table 2: Summary statistics: Characteristics of councils and candidates

VARIABLES	(1) All Elections	(2) Wht. Vs. Nonwht. Elections	(3) Wht. Vs. Nonwht. Elections, optimal bandwidth (Main estimation sample)
Council / Election Characteristics			
Council size	5.32	5.68	5.38
	(0.97)	(1.43)	(0.91)
District-based elect.	0.09	0.19	0.07
	(0.29)	(0.39)	(0.26)
Rest-of-council		0.68	0.66
white share		(0.28)	(0.28)
Rest-of-council		0.28	0.25
is all white		(0.44)	(0.42)
Candidate Characteristics			
Share of elections:	0.34		
Wht. vs. Nonwht.	(0.47)		
Share of elections:	0.45		
Both wht.	(0.50)		
Share of elections:	0.21		
Both nonwht.	(0.41)		
Candidate Characteristics (Conditional on Wht. vs. Nonwht.)			
Nonwht. candidate:		0.17	0.19
Asian / Pac. Isl.		(0.38)	(0.40)
Nonwht. candidate:		0.16	0.16
Black		(0.37)	(0.37)
Nonwht. candidate:		0.61	0.58
Hispanic		(0.49)	(0.50)
Nonwht. candidate:		0.06	0.07
Other		(0.24)	(0.26)
Number of elections	1,986	362	201

Standard deviations in parentheses. Note that the rest of council ethnicity shares are only able to be calculated for a subset of the elections where we can identify the ethnicity of all other councilmembers.

Table 3: Full sample, baseline difference in prices between houses in maj. nonwhite block groups and houses in maj. white block groups

	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price	(5) Adjusted Log Price
Maj. nonwhite BG	-0.286***	-0.385***	-0.277***	-0.170***	-0.055***
	(0.040)	(0.041)	(0.030)	(0.017)	(0.012)
City FEs		X	X	X	X
House chars.			X	X	X
BG income chars.				X	X
Other BG chars.					X
Observations	2,876,011	2,876,011	2,876,011	2,875,933	2,875,933
R-squared	0.040	0.575	0.670	0.683	0.696

All specifications include the full sample of housing transactions. No controls are included other than those noted in the table.

House characteristics: # bathrooms, # bedrooms, # stories, sq. footage, age at sale; *Block group income characteristics*: Median household income, % below pov. level, % on public assistance; *Other block group characteristics*: pop. density, share pop. urban, share pop. male, share pop. over 18, share pop. over 65, total pop., share households by household structure (married with children, married without children, etc.), share vacant housing, share renter occupied housing, share owner occupied housing.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Cross-sectional RD approach: Effects of councilmember ethnicity on housing values overall and by neighborhood type

	Main cross-sectional specifications: Transactions in 2 years after relevant election	
	(1)	(2)
Nonwht. winner	0.009	-0.059**
	(0.010)	(0.024)
Nonwht. win X Maj. nonwht. BG		0.114***
		(0.039)
Linear combination: Full effect of nonwht. win on nonwht. BG		
Nonwht. winner + (Nonwht. win X Maj. nonwht. BG)	-	*0.055*** (0.021)
Observations	308,360	308,360

The outcome variable is an adjusted log house price, with house characteristics, block group characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections between white and nonwhite candidates. Table displays coefficient capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

The specifications include a sample of housing transactions that take place up to two years after the relevant election takes place. "Maj. nonwht. BG" is a dummy equal to 1 if the block group is at least 50% nonwhite.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Panel RD approach: Effects of councilmember ethnicity on housing values overall and by neighborhood type

	(1)	(2)	(3)	(4)
Panel A: Overall effects				
Nonwht. win X Post	0.038	0.036	0.033	0.003
	(0.044)	(0.040)	(0.040)	(0.020)
Panel B: Differential effects by neighborhood type				
Nonwht. win X Post	-0.058	-0.063	-0.063	-0.056**
	(0.046)	(0.046)	(0.045)	(0.028)
Nonwht. win X Post	0.147**	0.152***	0.152**	0.088**
X Maj. nonwht. BG	(0.061)	(0.057)	(0.059)	(0.041)
Linear combinations to measure full effect of nonwht. winner on nonwht. block groups				
(Nonwht. win X Post) +	*0.089**	*0.090***	*0.089**	*0.032*
(Nonwht. win X Post X	(0.050)	(0.045)	(0.047)	(0.027)
Maj. Nonwht. BG)				
Level of FEs	Election	Election	Election	Election
House controls		X	X	X
Block group controls			X	X
Linear city time trends				X
Observations	603,036	603,036	602,977	602,977

The outcome variable is an adjusted log house price, with house characteristics, block group characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections between white and nonwhite candidates, decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Effects of councilmember ethnicity on local economic development

	(1)	(2)	(3)
Outcome variable:	Log of number of establishments in ZIP	Any TRI facility in tract? (=1, 0 otherwise)	LIHTC
Panel A: Overall effects			
Nonwht. win X Post	0.006	0.001	-0.016
	(0.009)	(0.009)	(0.011)
Panel B: Differential effects by neighborhood type			
Nonwht. win X Post	-0.038*	0.002	-0.015
	(0.021)	(0.018)	(0.012)
Nonwht. win X Post	0.078**	0.000	0.000
X Maj. nonwht. area	(0.032)	(0.024)	(0.021)
Geographic area	ZIP	Census tract	Census tract
Level of FEs	Election	Election	Election
ZIP controls	X		
Tract controls		X	X
Observations	1,541	10,857	10,935

All specifications are restricted to elections between white and nonwhite candidates, decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Effects of councilmember ethnicity on policing and crime

	(1)	(2)	(3)	(4)
Outcome variable:	Log of Arrests per Capita	Log of Nonwhite Arrest Rate (Nonwhite arrests / Nonwhite pop.)	Log of White Arrest Rate (White arrests / White pop.)	Nonwhite share of arrests (Nonwhite arrests / All arrests)
Nonwht. win X Post	-0.010	-0.042	0.054	-0.016*
	(0.048)	(0.052)	(0.054)	(0.008)
Observations	589	589	589	589

All specifications are restricted to elections between white and nonwhite candidates, decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Effect of councilmember ethnicity on city financial activity

	(1)	(2)	(3)	(4)
Outcome variable:	Total Expenditures	Total Revenue	General Gov't Expenditures	Salaries & Wages
Nonwhite winner	0.003	-0.069	-0.170	-0.032
	(0.043)	(0.045)	(0.105)	(0.037)
	(5)	(6)	(7)	(8)
Outcome variable:	Public goods Expenditures	Public safety Expenditures	Transportation Expenditures	Community Development Expenditures
Nonwhite winner	0.007	-0.018	-0.042	-0.036
	(0.049)	(0.029)	(0.087)	(0.118)
Observations	636			

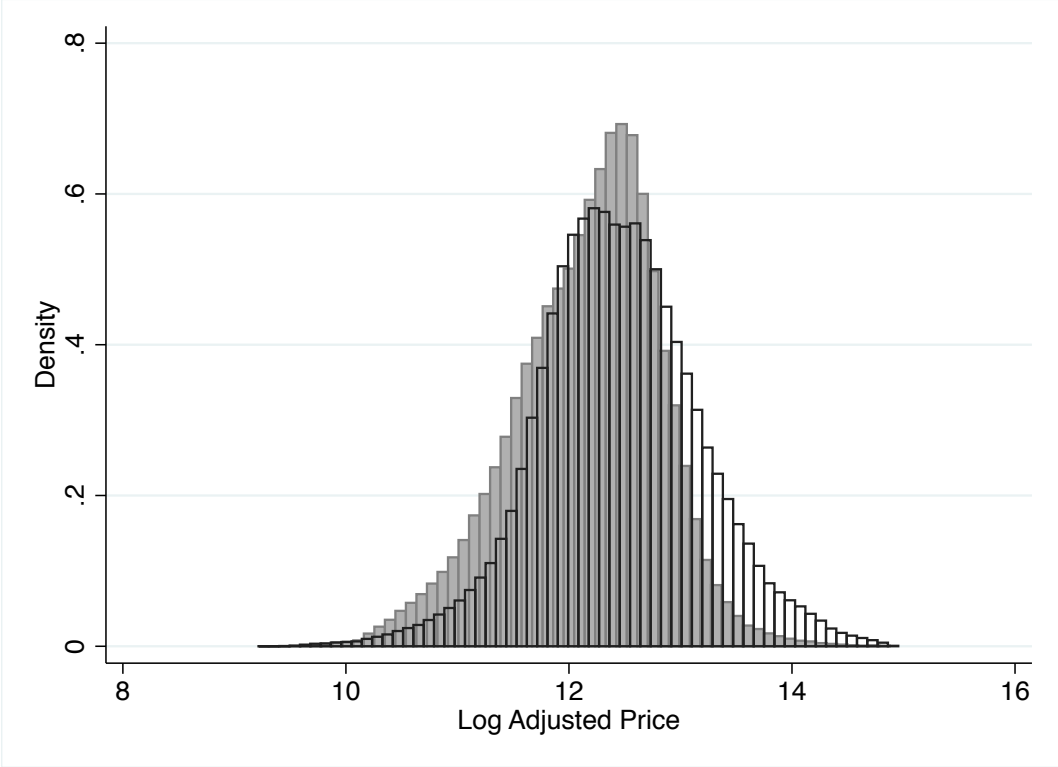
"Nonwhite winner" in the table refers to the interaction between the nonwhite winner indicator and the "post-election" indicator. That is, the reported coefficients identify the differential effect of a nonwhite winner after the election (relative to outcomes in the same city before the election). All specifications use the optimally selected bandwidth.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

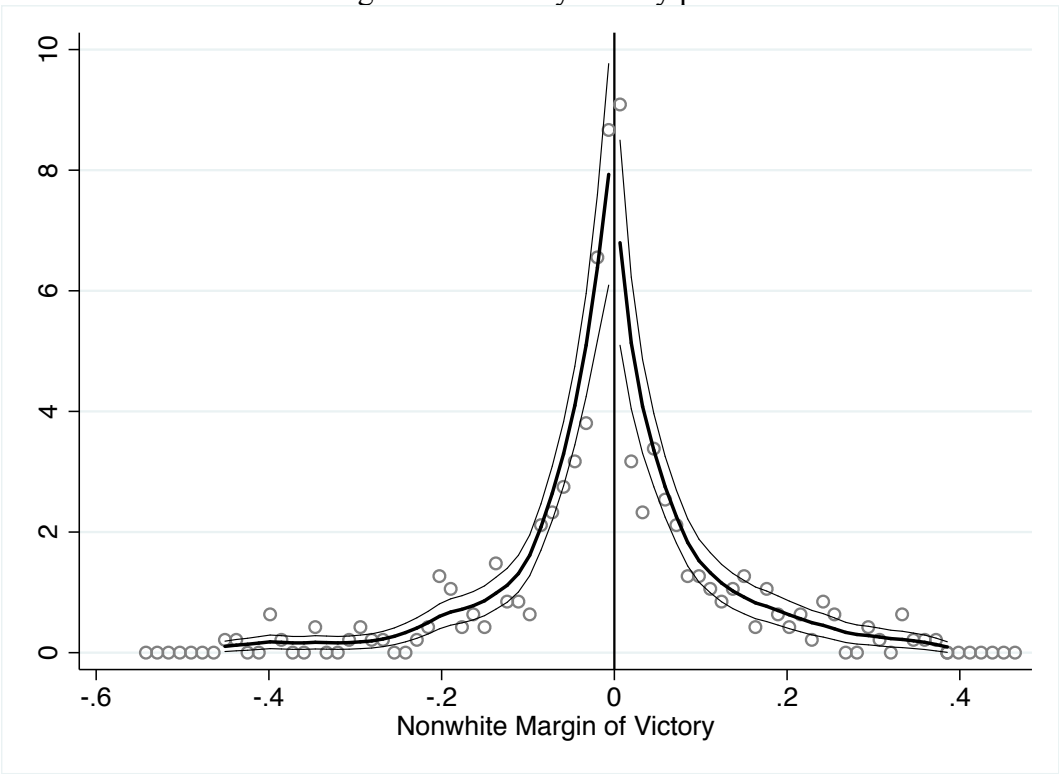
FIGURES

Figure 1: Distribution of log adjusted house prices by neighborhood type



Note: Grey bars indicate distribution of log adjusted prices in majority nonwhite neighborhoods. Clear bars with black outlines indicate distribution of log adjusted prices in majority white neighborhoods.

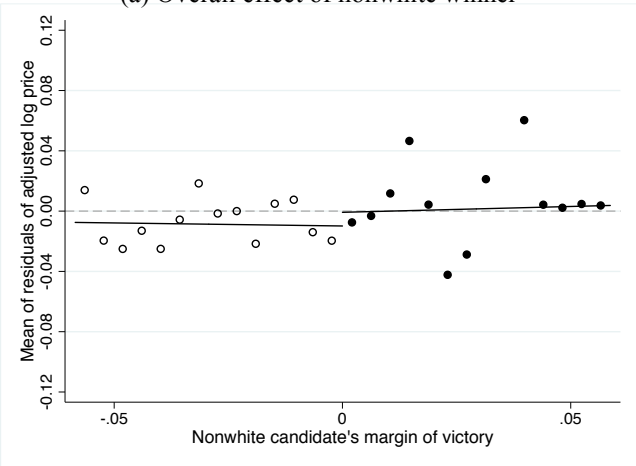
Figure 2: McCrary density plot



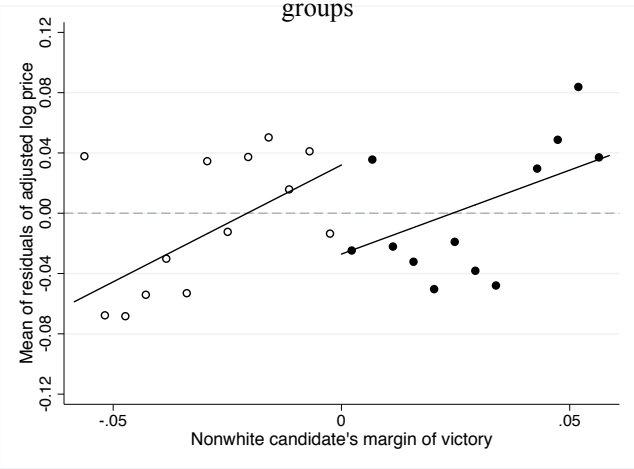
Note: The figure depicts the distribution of relevant elections around the cutoff that determines whether a white or nonwhite candidate wins. The x-axis measures the nonwhite candidate's margin of victory (nonwhite candidate vote share minus white candidate vote share).

Figure 3: Effect of a nonwhite candidate victory on housing prices, cross-sectional comparison

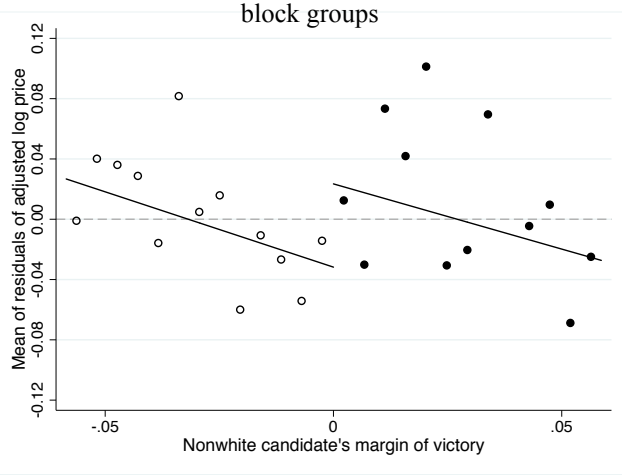
(a) Overall effect of nonwhite winner



(b) Effect of nonwhite winner on majority white block groups



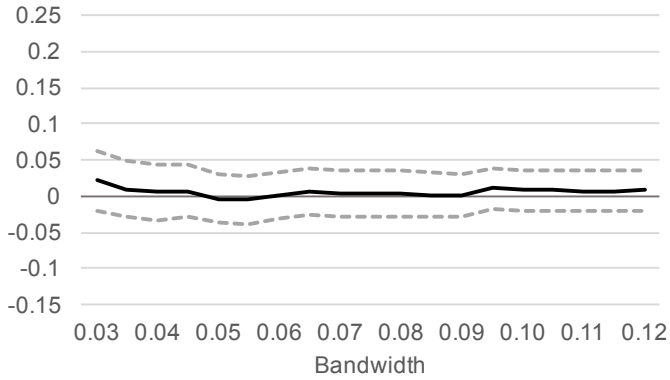
(c) Effect of nonwhite winner on majority nonwhite block groups



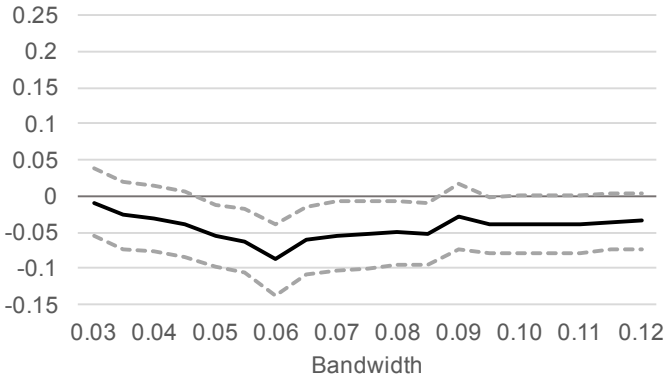
Note: As in our cross-sectional regression analysis, the samples in these figures are restricted to cities where there was an election between a white candidate and nonwhite candidate, and to housing transactions that occur within two years following the relevant election. Panel (b) further restricts the sample to majority white block groups, while Panel (c) restricts the sample to majority nonwhite block groups. The y-axis variable is the residual variation remaining in the log adjusted housing price after controlling for housing characteristics, block group characteristics, and city-specific time trends, as described in text. Each point on the figure captures the average outcome within a small range of the x-axis variable. When nonwhite candidate's margin of victory > 0, the nonwhite candidate has won.

Figure 4: Panel RD: Effect of a nonwhite candidate victory, across multiple bandwidths

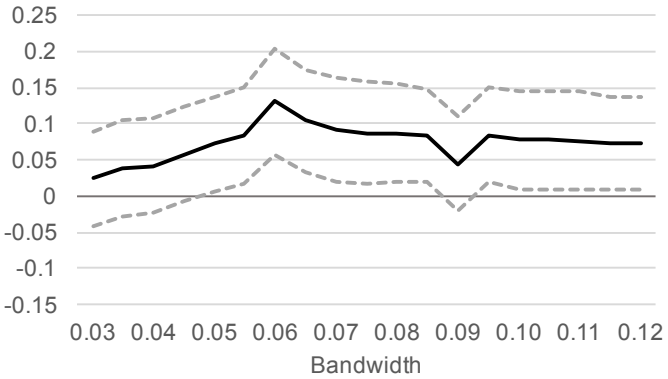
(a) Overall effect of nonwhite winner



(b) Effect of nonwhite winner on majority white block groups

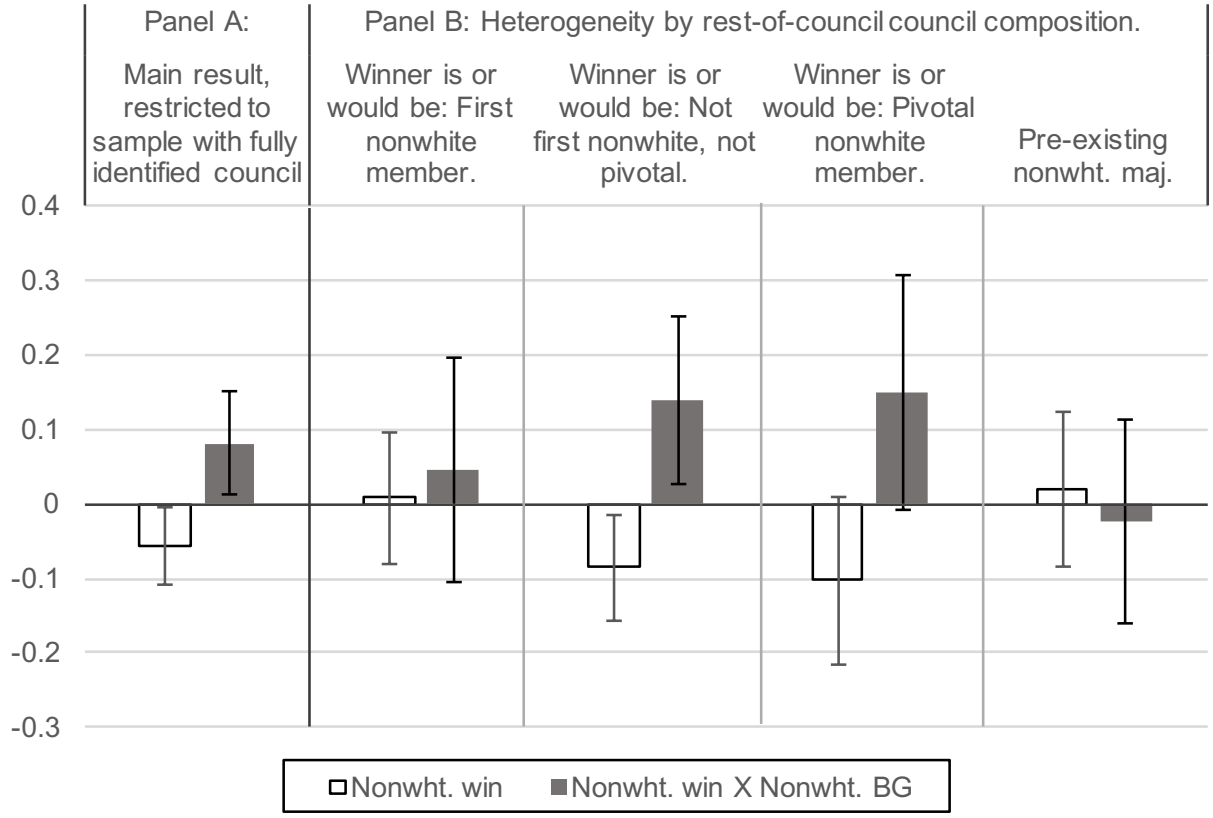


(c) Differential effect of nonwhite winner on majority nonwhite block groups



Note: Panel (a) of the figure depicts the result of the panel RD approach, which does not allow for heterogeneity in the treatment effect across neighborhood types. The specification matches the one used in Column 4, Panel A, Table 3, except that it is estimated repeatedly for a variety of bandwidths ranging from 0.03 to 0.12. The figure reports the "Nonwhite winner X Post" coefficient for each bandwidth, with the bandwidth noted along the x-axis.
Panels (b) and (c) of the figure depicts the result of the panel RD approach which does allow for heterogeneity in the treatment effect across neighborhood types. The specification matches the one used in Column 4, Panel B, Table 3, except that it is estimated repeatedly for a variety of bandwidths ranging from 0.03 to 0.12. Panel B reports the "Nonwhite winner X Post" coefficient for each bandwidth, with the bandwidth noted along the x-axis, while Panel C reports the "Nonwhite winner X Post X Nonwhite block group" coefficient.
In all panels, the dotted lines represent the 90% confidence intervals.

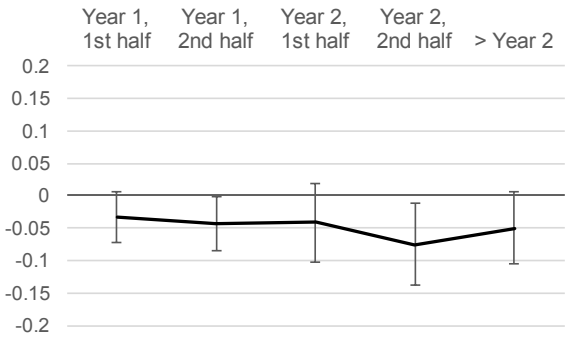
Figure 5: Panel RD: Heterogeneity in impact of nonwhite victory by composition of rest of council



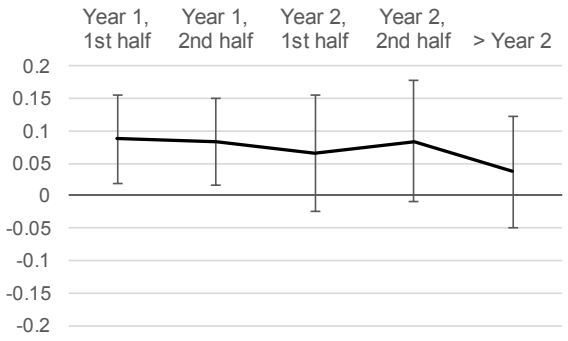
Note: This figure depicts results from five distinct regressions, with each pair of bars representing the relevant coefficient estimates from a single regression. The regressions are aimed at assessing how the treatment effect varies with the impact that a nonwhite win would have on the composition of the council. Doing so requires that we restrict the sample to the set of councils where we have identified the race/ethnicity of *every* council member, and not just the winning candidate (and narrowly losing candidate). Panel A simply replicates our main result on this smaller sample. Panel B reports the results of splitting the sample into four mutually exclusive and exhaustive categories based on the composition of the rest of the council. The first specification in Panel B is restricted to observations where a nonwhite winner "is or would be: First nonwhite member". These are observations where all other members of the council are white. The second specification in Panel B is restricted to observations where a nonwhite winner "is or would be: Not first nonwhite, not pivotal". These are observations where there is already at least one other nonwhite member on the council, but – even with the narrow election of an additional nonwhite member – the council would still be majority white. The third specification in Panel B is restricted to observations where a nonwhite winner "is or would be: Pivotal nonwhite member". These are observations where the narrow election of the nonwhite candidate would flip the council from being majority white to majority nonwhite. Finally, the fourth specification in Panel B is restricted to observations where a council has a "Pre-existing nonwht. maj.". These are observations where regardless of whether the nonwhite candidate in question wins or loses, the council would already have a nonwhite majority. Note that the figure depicts coefficients from the "Post X Nonwht. win" and "Post X Nonwht. win X Nonwht. BG" coefficients. That is, in contrast to the previous figure, this figure has reverted back to reporting the differential effect of a nonwhite victory. 90% confidence intervals around estimates are also depicted in the figure.

Figure 6: Panel RD: Dynamics of effects of nonwhite win

(a) Effect of nonwhite winner on majority white block groups

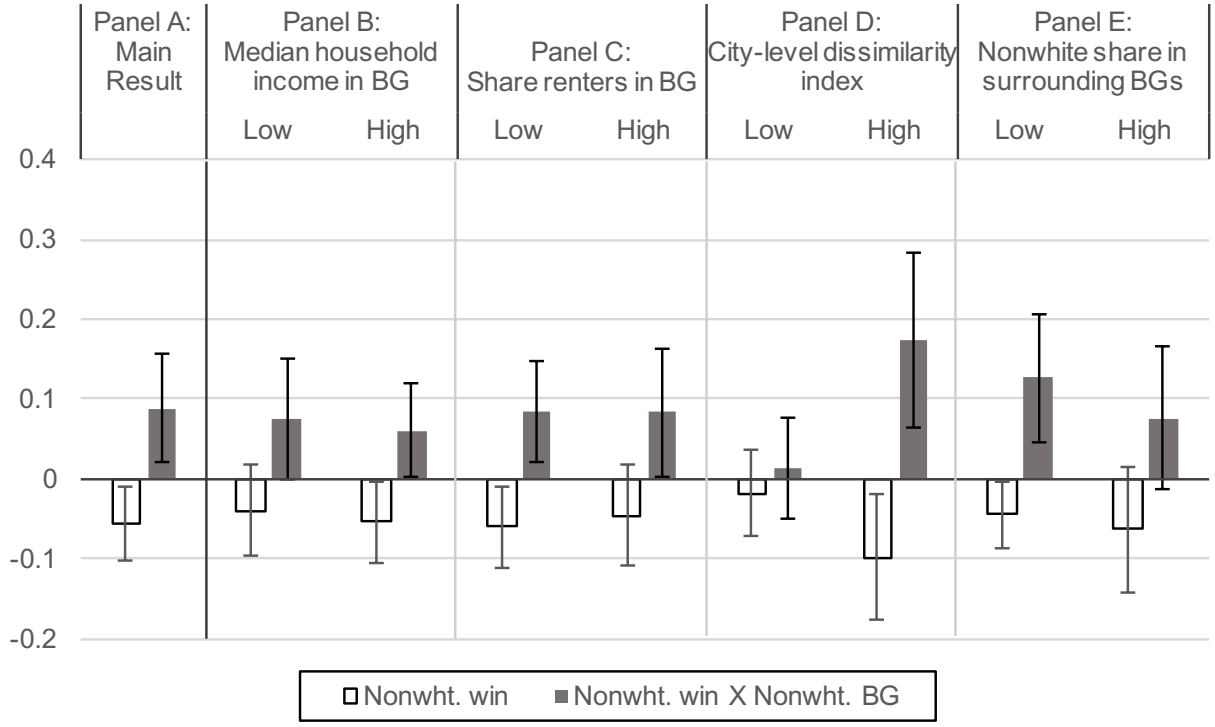


(b) Differential effect of nonwhite winner on majority nonwhite block groups



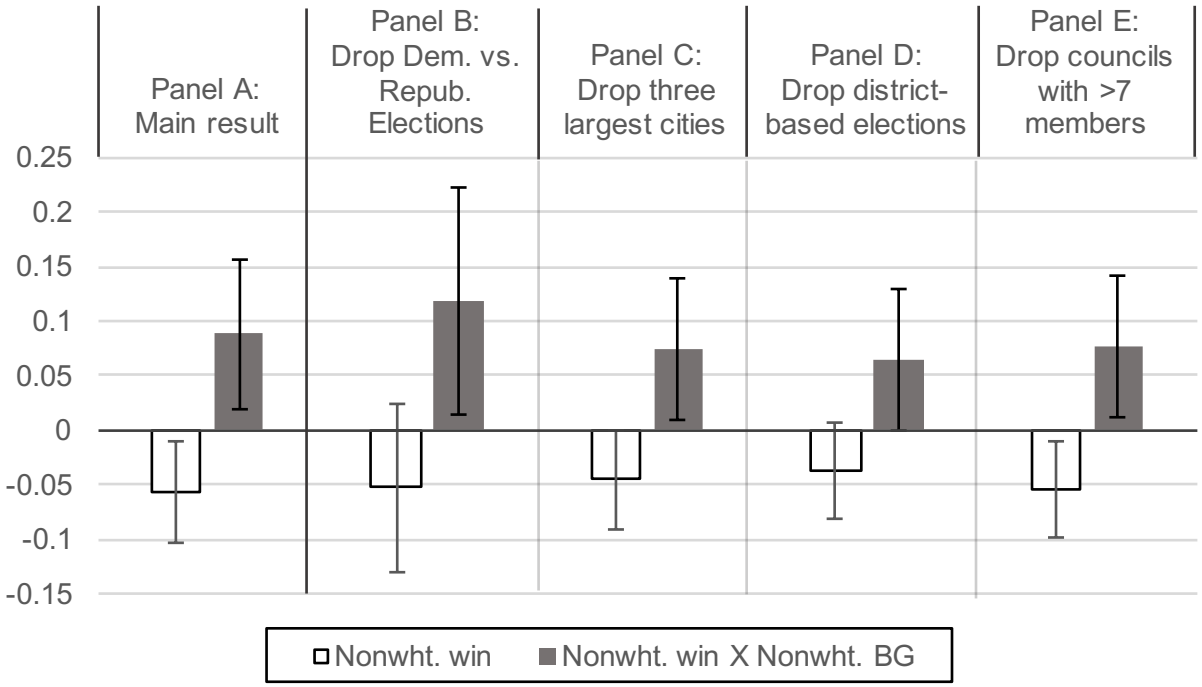
Note: This figure plots coefficients from a single regression, with each point depicting a particular coefficient and with bars around each point depicting 90% confidence intervals. The specification uses the same outcome variable, the same bandwidth, and includes the same set of controls as our main specification (Table 3, Panel B, Column 4). The specification differs in two ways. First, the sample includes housing transactions two years before a relevant election and housing transactions up to *four* year after a relevant election. Given the relatively short timespan of our panel, many elections do not include a full four years after the election, so in those cases we include as many years as possible. Second, rather than the simple "post" indicator to capture effects after an election has occurred, we include a vector of indicator variables equal to one if the housing transaction is in the first half year, second half year, third half year, fourth half year, or more than two years after the election. We fully interact these variables with all of the variables we would typically interact with "post." Panel (a) represents coefficients from the interaction of these time variables with "Nonwht. Wins", thereby measuring the effect of a nonwhite winner (relative to a white winner) on majority white block groups. Panel (b) represents coefficients from the interaction of these time variables with "Nonwht. Wins X Nonwht. BG", thereby measuring the differential effect of a nonwhite winner (relative to a white winner) on majority nonwhite block groups.

Figure 7: Panel RD: Heterogeneity in impact of nonwhite victory by block group or city characteristics



Note: This figure depicts results from nine distinct regressions, with each pair of bars representing the relevant coefficient estimates from a single regression. Panel A simply reports our main result (from Table 3, Panel B, Column 4) for the sake of comparison with the rest of the figure. The remaining panels split the sample in various ways as noted in the panel headings, splitting the sample based on whether a housing transaction takes place in a block group (or city) that is above median ("High") or below median ("Low") in the noted characteristic. For example, in Panel B, we identify the median "Median Household Income" across all block groups, and run separate regressions for housing transactions that take place in block groups with above- and below-median "Median Household Income". Panel D splits the sample into cities that are above- or below- median on the dissimilarity index between white and nonwhite residents, which measures segregation. Panel E uses a measure of the nonwhite share in block groups surrounding each block group. A "High" block group in that panel is one that is surrounded by block groups with a large nonwhite share. Note that the figure depicts coefficients from the "Post X Nonwht. win" and "Post X Nonwht. win X Nonwht. BG" coefficients. 90% confidence intervals around estimates are also depicted in the figure.

Figure 8: Panel RD: Additional robustness checks



Note: This figure depicts results from five distinct regressions, with each pair of bars representing the relevant coefficient estimates from a single regression. Panel A simply reports our main result (from Table 3, Panel B, Column 4) for the sake of comparison with the rest of the figure. Note that the figure depicts coefficients from the "Post X Nonwht. win" and "Post X Nonwht. win X Nonwht. BG" coefficients. 90% confidence intervals around estimates are also depicted in the figure. Panel B drops observations from the sample where the relevant white vs. nonwhite election was contested between a Democrat and a Republican. The only remaining elections in the sample either feature two candidates from the same party, or – in a smaller set of cases – at least one independent candidate. Note that most city council elections are nonpartisan and so partisan affiliation is not reported in our city council election returns data. We instead identify partisan affiliation of candidates using the California Voter Registration Files. Panel C drops observations from the three largest cities in our main estimation sample, which is already restricted to housing transactions in cities with an election between a white and nonwhite candidate that was decided by 5.88 percentage points or less. The cities dropped in this specification are Long Beach, San Diego, and Santa Ana. Panel D drops observations where the relevant election is district-based rather than at-large. Panel E drops observations from the minority of cities with councils with more than seven members.

Appendix A: Additional Tables and Figures

Appendix Table 1: Cross-sectional RD approach: Placebo & Alternative RD approaches

	Cross-sectional analysis: Placebo: Transactions in 2 years prior to relevant election	Cross-sectional analysis: Block-group-level averages	Cross-sectional analysis: Block-group-level averages (weighted by # of transactions)	Cross-sectional analysis: Global polynomial approach
	(1)	(2)	(3)	(4)
Panel A: Overall effects				
Nonwht. winner	0.002	0.004	0.013	0.018**
	(0.010)	(0.014)	(0.011)	(0.009)
Panel B: Differential effects by neighborhood type				
Nonwht. winner	-0.007	-0.048	-0.057**	-0.028
	(0.017)	(0.031)	(0.025)	(0.017)
Nonwht. win X Maj. nonwht. BG	0.020	0.094**	0.117***	0.075**
	(0.026)	(0.048)	(0.041)	(0.029)
Observations	290,908	9,081	9,081	887,591

The outcome variable is an adjusted log house price, with house characteristics, block group characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections between white and nonwhite candidates. Table displays coefficient capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

All specifications are restricted to elections between white and nonwhite candidates. All specifications are cross-sectional and are restricted to housing transactions that take place within two years after a relevant election, except for Column 1 which restricts to housing transactions two years prior to the election. Columns 2-3 reduce the sample to block-group level averages in the outcome variable during this time period, so the unit of observation is a block group. Like main specifications, Columns 2-3 are local linear regressions and restrict the sample to elections decided by a margin of 5.88 percentage points or less. Specifications reported in Column 3 are weighted by the number of transactions that make up the average. Column 4 maintains the housing transaction as the unit of observation (like in the main specifications), but does not restrict to elections decided by 5.88 percentage points or less. Instead, in those columns, we take a global polynomial regression discontinuity approach, and allow "nonwhite margin of victory" to enter (and interactions with margin) as a cubic function.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2: Panel RD approach: Sensitivity and placebo tests

	Alternate Fixed Effects		**Alternate definitions of "High nonwhite" block groups**			**Placebo tests**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome variable:	Log adjusted price	Log adjusted price	Log adjusted price	Log adjusted price	Log adjusted price	Log adjusted price	Log *predicted* price
Time period:	Two years before and after relevant election					*Four years before relevant election*	Two years before and after relevant election
Nonwht. win X Post	-0.053**	-0.038	-0.058*	-0.029	-0.119**	0.008	0.004
	(0.026)	(0.025)	(0.030)	(0.022)	(0.052)	(0.029)	(0.022)
Nonwht. win X Post	0.085**	0.060*				0.003	-0.002
X Maj. nonwht. BG	(0.040)	(0.033)				(0.039)	(0.038)
Nonwht. win X Post			0.088**				
X Maj. nonwht. BG (alt)			(0.043)				
Nonwht. win X Post				0.066*			
X High nonwht. BG				(0.039)			
Nonwht. win X Post					0.198**		
X BG nonwht. share					(0.090)		
Level of FEs	City	Block group X Election	Election	Election	Election	Election	Election
House controls	X	X	X	X	X	X	X
Block group controls	X	X	X	X	X	X	X
Linear city time trends	X	X	X	X	X	X	X
Observations	602,977	602,977	602,977	602,977	602,977	484,234	602,977

The outcome variable is an adjusted log house price, with house characteristics, block group characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections between white and nonwhite candidates, decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

In Column 3, nonwhite share used to identify majority nonwhite block groups is calculated as "Single race nonwhite and multiracial count / Total count", rather than "Nonwhite count / Total single-race count". "High nonwht. BG" (Column 4) is a dummy equal to 1 if the block group is higher than the within-city median of block group nonwhite share. "BG nonwht. share" (Column 5) is a continuous variable equal to nonwhite share within the block group.

In the placebo test reported in Column 6, the sample period is four years before a relevant election. "Post" is equal to one in the two years preceding the relevant election.

In the placebo test reported in Column 7, the outcome variable is the log of the "predicted price" – which is predicted based on observable characteristics of the house (bedrooms, bathrooms, etc.), block group demographic characteristics, and city fixed effects.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3: Impact of nonwhite candidate win on residential property transaction sales volume

	(1)	(2)	(3)
	Sales volume	Sales volume per 1,000 pop.	Any sales (=1 if yes, 0 otherwise)
Mean of dep. var.:	*1.68*	*3.40*	*0.59*
Panel A: Overall effects			
Nonwht. win X Post	-0.055	0.042	-0.005
	(0.192)	(0.386)	(0.025)
Panel B: Differential effects by neighborhood type			
Nonwht. win X Post	-0.054	-0.071	-0.022
	(0.201)	(0.388)	(0.024)
Nonwht. win X Post	0.028	0.258	0.032
X Maj. nonwht. BG	(0.226)	(0.417)	(0.027)
Level of FEs	Election	Election	Election
Block group controls	X	X	X
Observations	370,259	370,130	370,259

The sample consists of block group-by-month level counts of residential property transactions. The outcome variable in Column 1 is the number of transaction. In Column 2, the outcome variable is the number of transactions per 1,000 in the block group population. In Column 3, the outcome variable is an indicator variable equal to one if any transactions occurred in that block group-month combination. All specifications are restricted to elections between white and nonwhite candidates, decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4: Testing for a generic post-close election effect

	(1)	(2)	(3)
Post-election	-0.001	0.003	-0.007
	(0.003)	(0.005)	(0.004)
Post-election X Close		-0.010	
		(0.007)	
Restrict to optimal bandwidth?	No	No	Yes
Includes "post-election" by margin interaction?	No	No	Yes
Observations	1,681,867	1,681,867	602,977

All specifications are restricted to elections between white and nonwhite candidates, and include two years before and after the election. All specifications also include "election" fixed effects and take the adjust log housing price (with controls residualized out) as the outcome varaible. Columns 1 and 2 includes a sample of all elections between a margin of -50 percentage points and 50 percentage points. Column 3 restricts the sample to elections within the optimal bandwidth (-5.88 percentage points to 5.88 percentage points). Though not reported, the specification reported in Column 3 (unlike Columns 1 and 2) interacts the "Post-election" dummy with the white candidate's margin of victory. Thus, the "Post-election" dummy in this specification identifies the effect of being post-election in a city where the election was very narrowly decided (an election close to the cutoff of 0 percentage point margin of victory).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 5: Panel RD approach: Reporting additional coefficients to allow for interpretation of impact of white and nonwhite candidates

	(1)	(2)	(3)	(4)
Panel A: Overall effects				
Post	-0.062**	-0.035	-0.039	-0.009
	(0.031)	(0.030)	(0.031)	(0.013)
Nonwht. win X Post	0.038	0.036	0.033	0.003
	(0.044)	(0.040)	(0.040)	(0.020)
Linear combinations: Measuring the overall – rather than differential – effect of a nonwhite win				
Post + (Nonwht. win X Post)	-0.024	0.002	-0.005	-0.006
	(0.032)	(0.030)	(0.029)	(0.014)
Panel B: Differential effects by neighborhood type				
Post	0.051*	0.069**	0.042	0.052**
	(0.029)	(0.034)	(0.031)	(0.020)
Post X Maj. nonwht. BG	-0.167***	-0.154***	-0.123***	-0.093***
	(0.042)	(0.041)	(0.035)	(0.027)
Nonwht. win X Post	-0.058	-0.063	-0.063	-0.056**
	(0.046)	(0.046)	(0.045)	(0.028)
Nonwht. win X Post	0.147**	0.152***	0.152**	0.088**
X Maj. nonwht. BG	(0.061)	(0.057)	(0.059)	(0.041)
Linear combinations: Measuring the overall – rather than differential – effect of a nonwhite win				
Post + (Nonwht. win X Post)	-0.007	-0.002	0.028	-0.004
	(0.042)	(0.040)	(0.044)	(0.029)
(Post X Maj. nonwht. BG) +	-0.020	-0.020	-0.020	-0.020
(Nonwht. win X Post X Maj. nonwht. BG)	(0.044)	(0.044)	(0.046)	(0.044)
Level of FEs	Election	Election	Election	Election
House controls		X	X	X
Block group controls			X	X
Linear city time trends				X
Observations	603,036	603,036	602,977	602,977

The outcome variable is an adjusted log house price, with house characteristics, block group characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections between white and nonwhite candidates, decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 6: Likelihood of adopting a revision to City Plans

	(1)	(2)	(3)	(4)
Outcome variable:	Planning change: Any (=1 if so, 0 otherwise)	Planning change: Land use	Planning change: Circulation	Planning change: Housing
Nonwhite winner	-0.041	0.004	-0.024	-0.047
	(0.119)	(0.082)	(0.083)	(0.111)
	(5)	(6)	(7)	(8)
Outcome variable:	Planning change: Open space	Planning change: Conservation	Planning change: Safety	Planning change: Noise
Nonwhite winner	0.040	0.024	0.025	0.032
	(0.071)	(0.069)	(0.067)	(0.074)
Observations	741			

"Nonwhite winner" in the table refers to the interaction between the nonwhite winner indicator and the "post-election" indicator. That is, the reported coefficients identify the differential effect of a nonwhite winner after the election (relative to outcomes in the same city before the election). All specifications use the optimally selected bandwidth.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 7: Effect of councilmember ethnicity on residential building permit activity

	(1)	(2)	(3)	(4)
Outcome variable:	Single Family Unit Building Permits (per 10k in pop)	Multi Family Unit Building Permits (per 10k in pop)	Single Family Permit Valuation per capita	Multi Family Permit Valuation per capita
Nonwhite winner	7.524	5.326	211.154	90.682
	(12.828)	(6.667)	(229.426)	(124.428)
Observations	634			

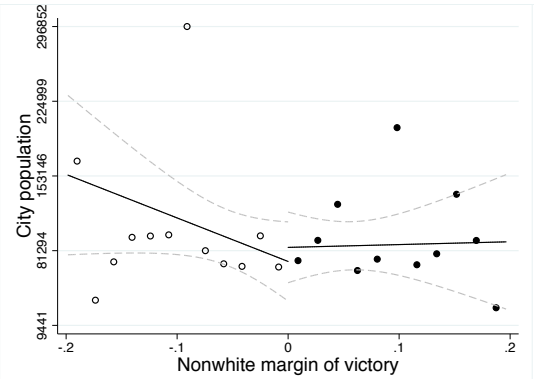
"Nonwhite winner" in the table refers to the interaction between the nonwhite winner indicator and the "post-election" indicator. That is, the reported coefficients identify the differential effect of a nonwhite winner after the election (relative to outcomes in the same city before the election). All specifications use the optimally selected bandwidth.

Robust standard errors (clustered at city-level) in parentheses

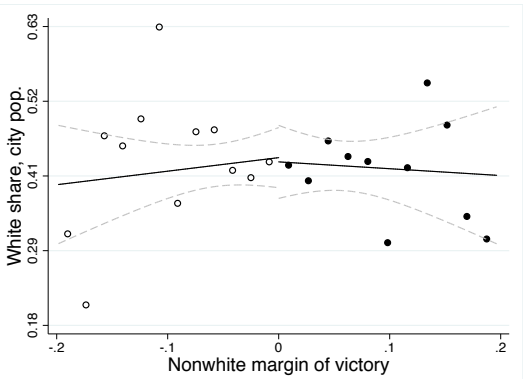
*** p<0.01, ** p<0.05, * p<0.1

Appendix Figure 1: Covariate balance tests: City characteristics

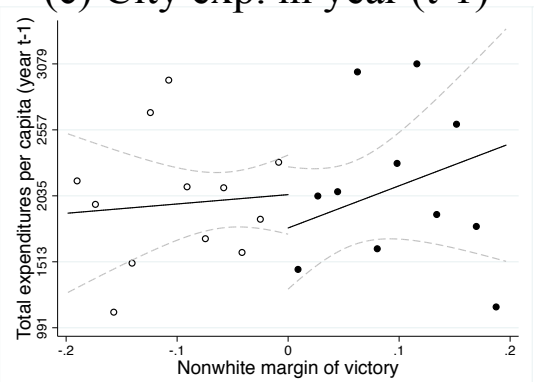
(a) City population



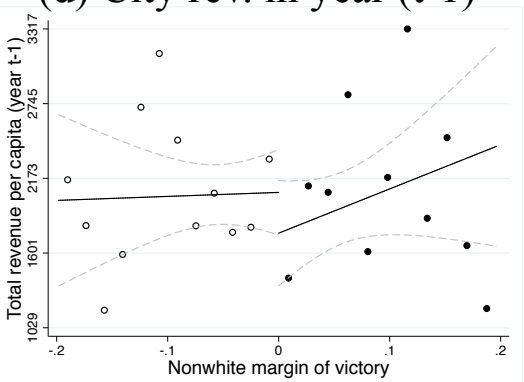
(b) White share of city pop.



(c) City exp. in year (t-1)

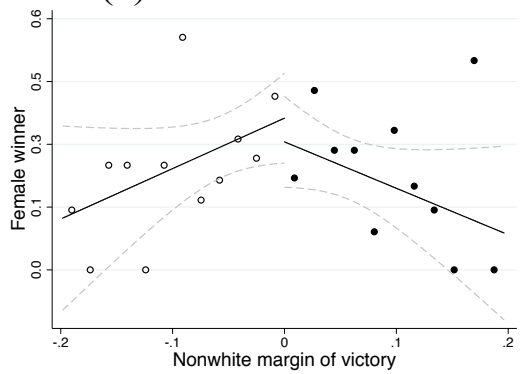


(d) City rev. in year (t-1)

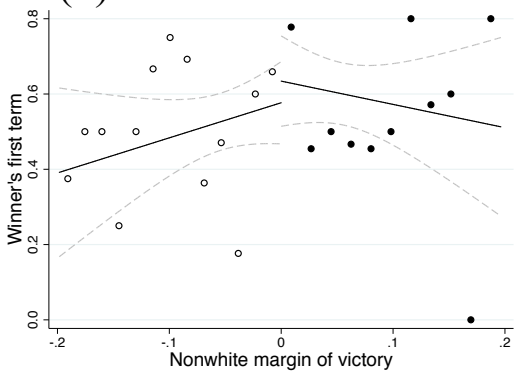


Appendix Figure 2: Covariate balance tests: Candidate characteristics

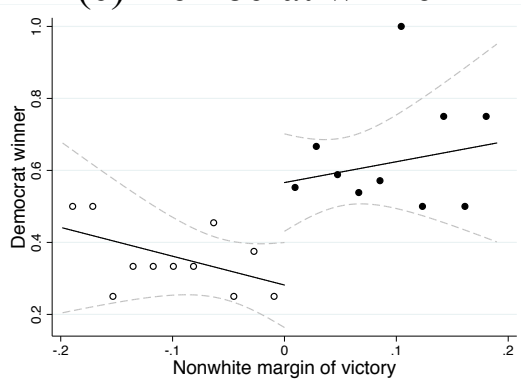
(a) Female winner



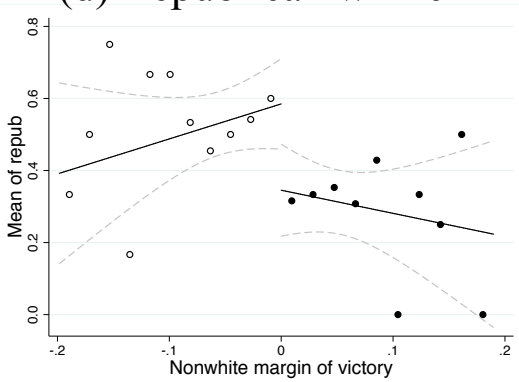
(b) Winner's first term



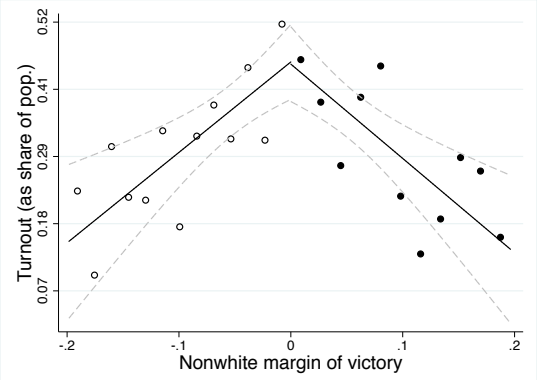
(c) Democrat winner



(d) Republican winner

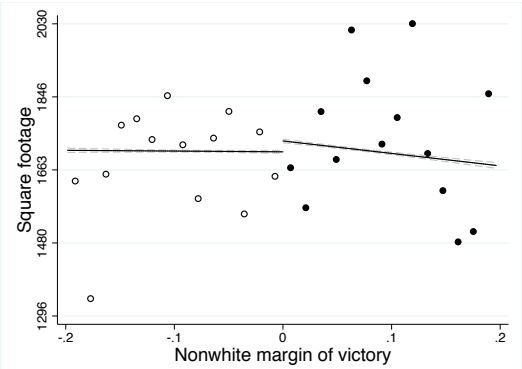


(e) Turnout

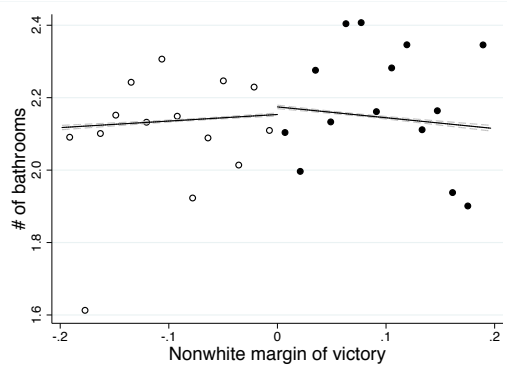


Appendix Figure 3: Covariate balance tests: Housing characteristics

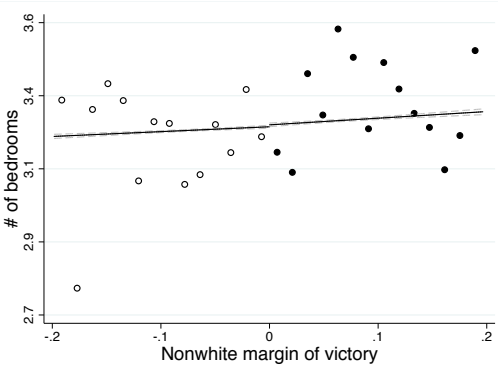
(a) Square footage



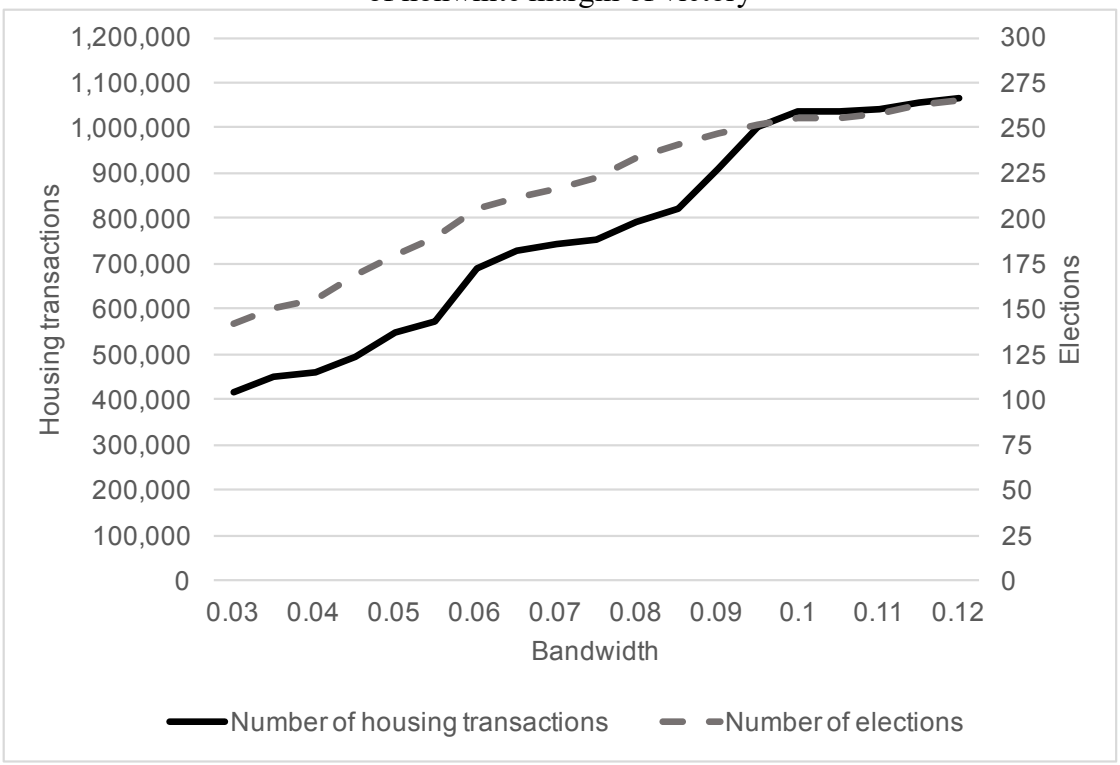
(b) # of bathrooms



(c) # of bedrooms



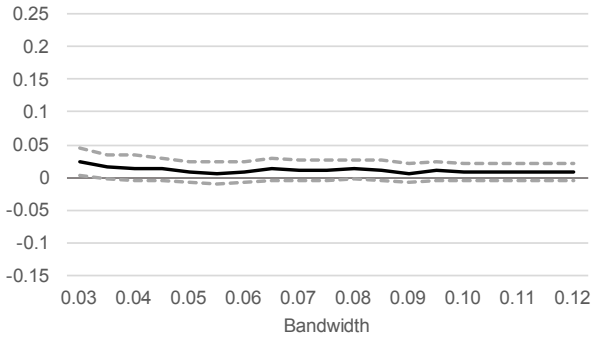
Appendix Figure 4: Number of housing transactions and elections within a variety of bandwidths of nonwhite margin of victory



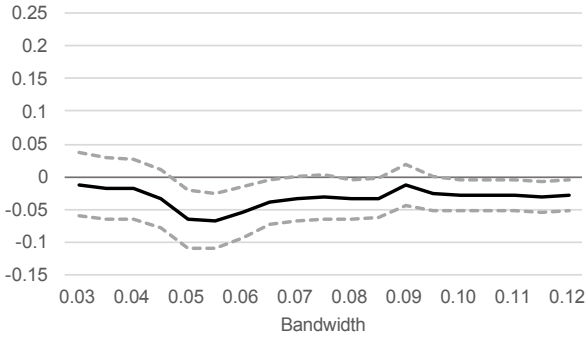
Note: The figure plots the number of housing transactions (solid black line, left vertical axis) and the number of unique elections (dotted gray line, right vertical axis) within each bandwidth between 0.03 and 0.12.

Appendix Figure 5: Cross-sectional RD approach: Effect of a nonwhite candidate victory, across multiple bandwidths

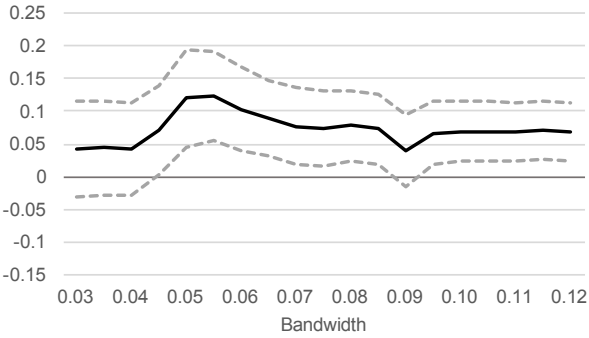
(a) Overall effect of nonwhite winner



(b) Effect of nonwhite winner on majority white block groups



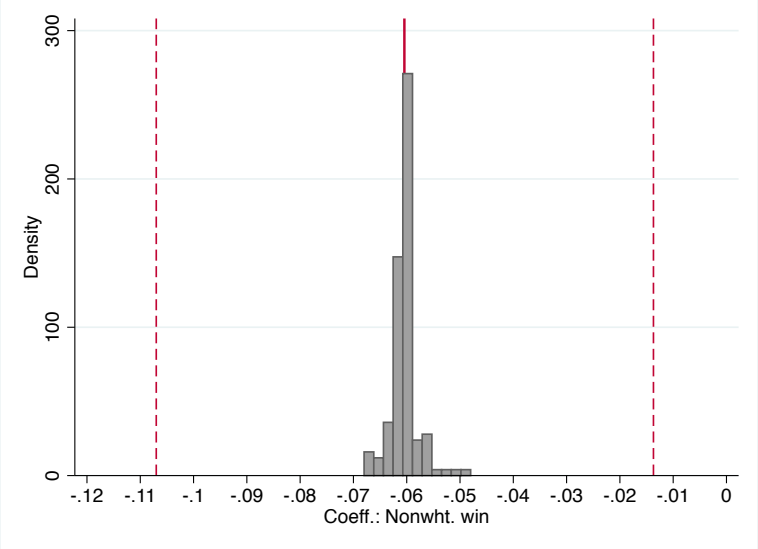
(c) Differential effect of nonwhite winner on majority nonwhite block groups



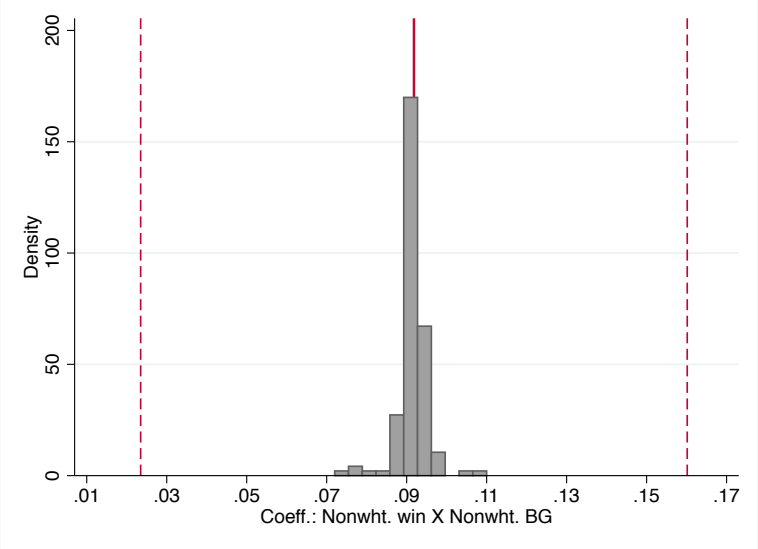
Note: Panel (a) of the figure depicts the result of the cross-sectional RD approach which does not allow for heterogeneity in the treatment effect across neighborhood types. The specification matches the one used in Column 1 of Table 2, except that it is estimated repeatedly for a variety of bandwidths ranging from 0.03 to 0.12. The figure reports the "Nonwhite winner" coefficient for each bandwidth, with the bandwidth noted along the x-axis.
Panels (b) and (c) of the figure depicts the result of the cross-sectional RD approach which does allow for heterogeneity in the treatment effect across neighborhood types. The specification matches the one used in Column 2 of Table 2, except that it is estimated repeatedly for a variety of bandwidths ranging from 0.03 to 0.12. Panel B reports the "Nonwhite winner" coefficient for each bandwidth, with the bandwidth noted along the x-axis, while Panel C reports the "Nonwhite winner X Nonwhite block group" coefficient.
In all panels, the dotted lines represent the 90% confidence intervals.

Appendix Figure 6: Distribution of coefficients from 138 regressions, dropping one city in each regression

(a) Distribution of "Nonwhite wins" coefficient



(b) Distribution of "Nonwhite wins X Nonwhite block group" coefficient



Notes: Solid red lines indicate the estimated coefficient from our main specification. Dashed red lines indicate the confidence interval of that estimate, again from the main specification. The main specification includes 138 unique cities. In these figures, we plot the distribution of coefficients resulting from 138 regressions that are similar to the main specification, except that each specification excludes exactly one city.

APPENDIX B: Race/ethnicity specific effects

This appendix reports race/ethnic-group specific effects. Our main analysis focuses on the impacts of a nonwhite candidate (broadly defined) winning rather than a White candidate. We do so mainly for the sake of statistical power; focusing on specific groups dramatically reduces the number of close elections we can leverage for identification. Similarly, our main analysis assess the differential impact of a nonwhite candidate's victory on housing prices in majority nonwhite neighborhoods. While nearly 50% of block groups in California are majority nonwhite, the numbers of block groups that are majority Hispanic, Black, or Asian/Pacific Islander are much smaller, which limited our ability to consider the impacts of a nonwhite candidate's victory on more specific neighborhood types. This section nonetheless aims to provide a sense of how results vary for specific groups, both with respect to winning candidates and neighborhood composition.

Figure B.1 documents the simplest departure from our main analysis. There, we assess how candidates from more specific race/ethnic groups (Black, Hispanic, East Asian/Pac. Islander) differentially impact majority nonwhite block groups. The far left panel of the figure reproduces our main result for the sake of comparison; there, we find that electing a nonwhite candidate leads to lower housing values in majority White neighborhoods than electing a White candidate, but that electing a nonwhite candidate also has a clear (positive) differential effect on majority nonwhite block groups. In the remaining panels of the figure, we report coefficients from a slightly modified specification. Specifically, while our main specification interacts "NonWhite winner" with the margin of victory and an indicator for whether the housing transaction takes place in a nonwhite block group, our modified specification replaces the "NonWhite winner" indicator with a vector of dummy variables indicating that a Black candidate won, a Hispanic candidate won, or an East Asian/Pac. Islander candidate won. Otherwise, the specification is identical. In particular, the sample is still restricted to narrow elections between White and NonWhite candidates, so each of the resulting coefficients can be interpreted as the effect of a Black candidate (or Hispanic, or Asian) winning *relative* to a White candidate. As in our main analysis, we plot the impacts of each of these types of candidates on housing transactions in majority White block groups ("[group] win", represented by the White bar) and the differential impacts of each type of candidate on majority nonwhite block groups ("[group] win X Nonwht. BG", gray bar). Notably, the impact of Hispanic candidate victories are very similar to our main result; as we documented in our summary statistics table, Hispanic candidates represent the largest share of nonwhite candidates in our sample. We also find, however, that Black candidates have a clear positive differential impact on nonwhite block groups, despite the much smaller number of Black candidates in our sample. Unlike Hispanic candidates (and unlike our main analysis), there is less clear evidence on the impact of Black candidates on majority White block groups. Finally, the estimated impacts of Asian/Pacific Islander candidates are sufficiently imprecise that we hesitate to attempt to interpret them. This is not too surprising; the share of East Asian/Pacific Islander candidates is also quite small. In short, this figure suggests that there is clear evidence that our main result – nonwhite candidates have a differentially positive impact on nonwhite block groups – is driven both by Black and Hispanic candidates, despite the fact that Hispanic candidates represent the largest share of nonwhite candidates in our main analysis.

Of course, while understanding how and whether different types of candidates differentially impact nonwhite block groups, it is also worth considering how different types of candidates differentially impact block groups heavily populated by residents from their own

race/ethnic group. Tables B.1-B3. aim to do just that. In all three tables, we conduct analysis which departs from the main analysis. Table B.1 restricts the sample to elections between White candidates and Hispanic candidates. Tables B.2 and B.3 do the same for Black vs. White elections and Asian vs. White elections, respectively. In the specifications reported in these tables, rather than interacting "[group] winner" with a dummy indicating that a housing transaction takes place in a majority nonwhite neighborhood, we interact the "[group] winner" dummy with various measures that capture more specific race/ethnic group composition within the block group. For instance, in Column 1 of Table B.1, we interact "Hispanic winner" with a continuous measure of the share of the population in the relevant block group that is Hispanic. In Column 2 of Table B.1, we interact "Hispanic winner" with a dummy indicating that Hispanics are the modal group within the relevant block group.[1] We do the same for Black and Asian in Columns 1 and 2 of Tables B.2 and B.3. For all three tables, Columns 3 and 4 test how candidates impact block groups that represent their own group, but also how they impact other block groups where some other nonwhite group has a large presence. Specifically, Column 3 extends the specification from Column 1. Rather than *only* interacting "[group] winner" with "[group] share of population in the block group", we also include interact "[group] winner" with "other nonwhite groups' share of population in the block group". For example, in Table B.1, "Hispanic winner" is interacted with "Hispanic share" and "share of the population that is nonwhite and Non-Hispanic". Column 4 extends the specification from Column 2, including dummies indicating whether the modal group in the block group is Hispanic, Asian, or Black, regardless of the race of the candidate in question.

Results in all three tables are imprecisely estimated, which – for the reasons described at the beginning of this appendix – is not surprising. For instance, as noted in Table B.2, once we focus only on elections between Black and White candidates, we are identifying effects from just 32 unique elections. Similarly, there are simply not many block groups where the modal group is Hispanic, Black, or Asian (especially for the latter two groups), which motivates the increased emphasis on the continuous population share in these tables.

Overall, though less precise, the results of the three tables broadly reveal that group-specific effects are consistent with our main results. They also reveal that – especially for Black and Hispanic candidates – specific types of nonwhite candidates tend to have an impact on *all* types of nonwhite block groups, which helps to justify our simpler focus on just nonwhite vs. white block groups in the main analysis.

Columns 1 and 2 of Table B.1 seem to reveal limited impacts of Hispanic candidates, though still in the same direction of the main results. However, keep in mind that those columns only allow for the possibility that a Hispanic candidate has a differential effect on largely Hispanic neighborhoods. If a Hispanic candidate has an impact on other types of largely nonwhite

[1] Note that we use a dummy indicating which group is modal, rather than which group is in the majority. This is for two reasons: (1) when we based our analysis around whether a neighborhood was majority nonwhite, by definition a majority had to either be majority white or nonwhite, so the comparison group was clear. A neighborhood that is not majority Hispanic does not necessarily imply that some other group represents more than 50% of the population. Defining neighborhoods based on the modal group on the other hand allows for mutually exclusive categories; a neighborhood where Hispanic is not the modal group *is* modal in some other group. (2) The share of block groups that are majority Hispanic, Black, or Asian is extremely small. For instance, less than 1% of observations in our sample are in majority Black block groups. The less restrictive "modal" requirement increases the number of block groups that we can identify differential effects from.

neighborhoods that is more similar to the effect that they have on Hispanic neighborhoods, than implicitly grouping other nonwhite neighborhoods with largely white neighborhoods would make it appear that there is a limited impact of electing a nonwhite candidate. This appears to explain the Columns 1-2 findings. In Columns 3 and 4, where we allow for differential impacts on Hispanic, Black, and Asian neighborhoods, we see that the election of a Hispanic candidate actually appears to have a *larger* positive differential impact on non-Hispanic nonwhite neighborhoods than on Hispanic neighborhoods.

A similar phenomenon is observed in Table B.2. There is a positive impact of electing a Black candidate in largely Black block groups (Columns 1 and 2), but also a clear positive impact on Hispanic and (to a lesser extent) Asian block groups (Columns 3 and 4).

Finally, the patterns in Table B.3 are slightly different. While Asian candidates have a clear positive impact on largely Asian neighborhoods, there is less evidence of an impact on other types of nonwhite neighborhoods. We again caution though that these results are based on a very small number of elections, 38 in this case.

Figure B.1: Estimated impacts of winners from specific race/ethnic groups on White and nonwhite block groups

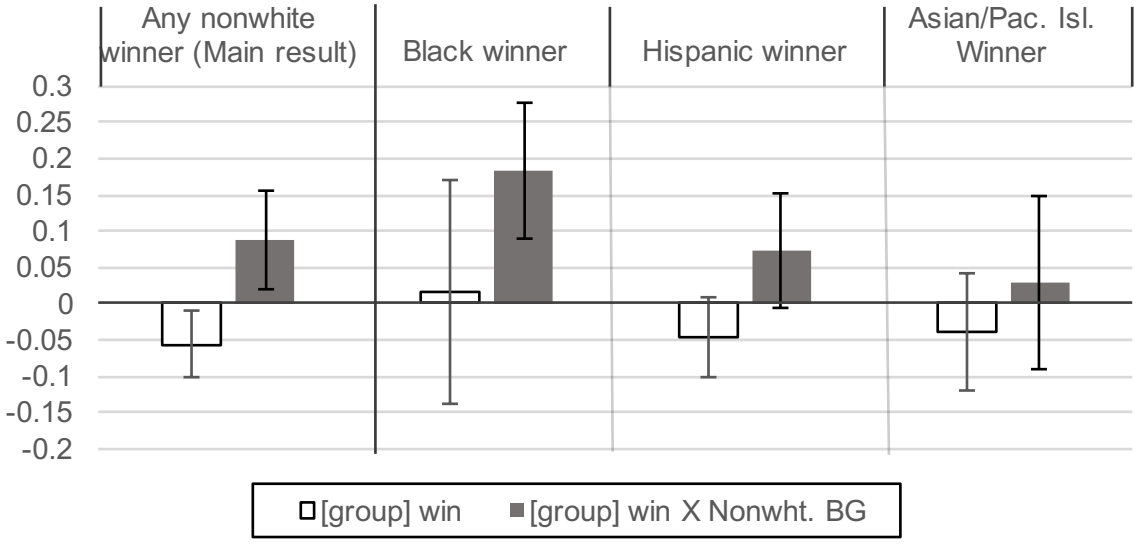


Figure notes: This figure reports coefficients from two regressions. The far left panel ("Any nonwhite winner") reproduces our main result for the sake of comparison. The remaining three panels ("Black winner", etc.) report results from a specification that modifies our main specification. We replace the "Nonwhite win" indicator with a vector of indicator variables equal to one if a Black candidate, Hispanic candidate, or Asian/Pac. Islander candidate won, respectively. As in the main specification, we are restricted to a sample of elections between White and nonwhite candidates, so the omitted comparison category are the cases where the White candidate won. 90% CIs depicted.

Table B.1: Hispanic winner

VARIABLES	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price
Hisp. Win X Post	-0.026	-0.019	-0.101	-0.031
	(0.049)	(0.033)	(0.069)	(0.036)
Hisp. Win X Post X Share Hisp.	0.038		0.097	
	(0.096)		(0.106)	
Hisp. Win X Post		0.018		0.031
X (Modal group in BG = Hisp.)		(0.046)		(0.049)
Hisp. Win X Post			0.298**	
X Share other nonwht.			(0.141)	
Hisp. Win X Post				0.128**
X (Modal group in BG = Asian)				(0.064)
Hisp. Win X Post				0.567**
X (Modal group in BG = Black)				(0.246)
Observations		364,700		
Unique elections		117		

General notes: The outcome variable is an adjusted log house price, with house characteristics, block group characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections decided by a margin of 5.88 percentage points or less. Table displays coefficients capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory).

Table-specific notes: All specification restricted to elections between White and Hispanic candidates. “Share Hisp.” captures the share of the local block group population that is Hispanic for a given a housing transaction. “Share other nonwht.” captures the share of the local block group population that is nonwhite and non-Hispanic. “Modal group in BG = X” equals one if the largest group in the local block group is X.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table B.2: Black winner

VARIABLES	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price
Black Win X Post	0.011	0.042	-0.150	0.001
	(0.109)	(0.098)	(0.136)	(0.100)
Black Win X Post X Share Black	0.886		0.999	
	(1.028)		(1.118)	
Black Win X Post		-0.012		0.215
X (Modal group in BG = Black)		(0.020)		(0.172)
Black Win X Post			0.354	
X Share other nonwht.			(0.237)	
Black Win X Post				0.349***
X (Modal group in BG = Hisp.)				(0.084)
Black Win X Post				0.096
X (Modal group in BG = Asian)				(0.103)
Observations		140,341		
Unique elections		32		

General notes: See Table B.1 notes.

Table-specific notes: All specification restricted to elections between Black and Hispanic candidates. “Share Black” captures the share of the local block group population that is Black for a given a housing transaction. “Share other nonwht.” captures the share of the local block group population that is nonwhite and non-Black. “Modal group in BG = X” equals one if the largest group in the local block group is X.

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table B.3: East Asian / Pacific Islander winner

VARIABLES	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price
Asian Win X Post	-0.059	-0.067	-0.047	-0.061
	(0.059)	(0.058)	(0.145)	(0.071)
Asian Win X Post X Share Asian	0.084		0.076	
	(0.148)		(0.187)	
Asian Win X Post		0.077*		0.088
X (Modal group in BG = Asian)		(0.042)		(0.088)
Asian Win X Post			-0.048	
X Share other nonwht.			(0.274)	
Asian Win X Post				-0.017
X (Modal group in BG = Hisp.)				(0.104)
Asian Win X Post				*omitted*
X (Modal group in BG = Black)				(-)
Observations		66,645		
Unique elections		38		

General notes: See Table B.1 notes.

Table-specific notes: All specification restricted to elections between White and East Asian/Pacific Islander candidates. “Share Asian” captures the share of the local block group population that is Asian for a given a housing transaction. “Share other nonwht.” captures the share of the local block group population that is nonwhite and non-Asian. “Modal group in BG = X” equals one if the largest group in the local block group is X. Note that in Column 4 “Asian Win X Post X Modal group in BG=Black” is omitted. There are no observations of block groups where the modal group is Black within the set of observations the restricted to (narrow elections, etc.).

Robust standard errors (clustered at city-level) in parentheses

*** p<0.01, ** p<0.05, * p<0.1