Minority Representation in Local Government*

Brian Beach
Vanderbilt University and NBER

Daniel B. Jones
University of Pittsburgh

Tate Twinam
The College of William & Mary

Randall Walsh
University of Pittsburgh and NBER

This version: August 2019
[Manuscript in progress; prepared for conference circulation]

Abstract: Does minority representation in a legislative body differentially impact outcomes for minorities? To examine this question, we assemble a dataset identifying the ethnicity of over 3,500 California city council candidates and study close elections between white and nonwhite candidates. We find that 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 councilmember pivotality. Observed changes in business patterns and policing help to explain our results.

* Beach: Vanderbilt University, Department of Economics, VU Station B, Box #351819, 2310 Vanderbilt Pl, Nashville, TN37235. Email: brian.beach@vanderbilt.edu; Jones: University of Pittsburgh, Graduate School of Public and International Affairs, 230 S. Bouquet St., Pittsburgh, PA 15260. Email: dbj10@pitt.edu; Twinam: William & Mary, Department of Economics, 221 Jamestown Rd., Williamsburg, VA 23185. Email: twinam@uw.edu; Walsh: University of Pittsburgh, Department of Economics, 230 S. Bouquet St., Pittsburgh, PA 15260. Email: walshr@pitt.edu.

We are grateful for helpful comments from Pat Bayer, Morgane Laouenan, Rachel Meltzer, Chris Mothorpe, Allison Shertzer, and Carly Urban. Thanks also to seminar participants at Clemson University, Florida State University, George Washington University, Montana State University, Oberlin College, Oklahoma State University, the University of Hawaii, the University of Tennessee, and the University of South Carolina as well as conference participants at the Association for Public Policy Analysis and Management Fall Research conference, American Real Estate and Urban Economics Annual Meeting, Public Choice Annual Meeting, and the Urban Economics Association Meeting.

"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, helped reduce racial disparities in public spending and the provision of public goods (Cascio & Washington, 2013), and reduced the black-white wage gap (Aneja & Avenancio-Leon, 2019). 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 (Grofman et al., 1992). Implicit in these efforts was 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. This focus on the importance of descriptive representation 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 continued efforts to reduce partisan gerrymandering, the issue will likely stay policy relevant, as reductions in partisan gerrymandering would likely reduce the number of majority-minority districts and may, in turn, reduce the number of minority 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 that which 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.

---

[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 narrow victories as a source of identifying variation. We pair this election data with comprehensive housing transaction microdata. This approach allows us to identify the extent to which the election of a nonwhite city councilmember generates a differential change in housing prices in majority nonwhite neighborhoods, which we take as a proxy for investment in highly localized amenities.

Our focus on housing markets follows the long tradition of using house prices as a sufficient statistic for valuing public and private investments. Seminal examples of this approach 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). 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

[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, Hopkins & McCabe's (2012) summary of the extant literature on black mayors, or Mahadevan (2019) on political corruption in the Indian electricity market. 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.

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

themselves, which in many cases is simply not possible, housing prices serve as an "index number" which allows us to assess aggregate changes in well-being that arise from a broad mix of policies. This distinction is particularly important given the potential for interactions between different types of 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, overall impacts may not be identifiable through the analysis of a set of uni-dimensional policy changes. Finally, housing prices are also unique in that they reflect expectations about the future stream of amenities (Bishop & Murphy 2011, 2018).[8] In sum, 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.

Using this approach, we find that, relative to the election of a white candidate, the election of a nonwhite candidate reduces pre-existing gaps in housing prices across minority and non-minority neighborhoods. This result is not driven by correlations between candidate race and political affiliation or between racial composition and neighborhood income. Moreover, price impacts are particularly pronounced when the election causes the council to flip from majority white to majority 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.

Our findings complement work on the Voting Rights Act by Cascio and Washington (2013) and Aneja & Avenancio-Leon (2019) who find that expanding black voting rights changed the behavior of elected politicians in ways that benefited black residents. Our analysis highlights the fact that, beyond just extending the franchise, increasing minority representation within the legislative process may be an important tool for addressing racial disparities. This is particularly relevant given the work of Hajnal & Trounstine (2014a), which documents large racial disparities in satisfaction with local public good provision. Moreover, Hajnal & Trounstine (2014b) show that in local elections voters are substantially divided along racial dimensions, even more so than along other dimensions (e.g., class). This is particularly true when candidates are from different racial or

[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.

ethnic groups. This suggests that voters themselves are behaving under the assumption that improving descriptive representation in local government may help address underlying disparities. Our paper provides empirical support for this idea. 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. Thus, our results suggest that today, well more than a century since Reconstruction and the adoption of the 14th Amendment and five decades beyond the passage of the Voting Rights Act, descriptive representation remains important.

2. Conceptual Framework

Before proceeding to the empirical analysis, we first outline a simple conceptual framework to more precisely describe the link between minority representation, neighborhood- or group-specific investment, and housing prices.

In general, policies can differentially benefit one group relative to another either through direct impacts on individuals (e.g., policing, cultural events, differential hiring) or indirectly by targeting the neighborhoods in which group members are concentrated (e.g., business district development, infrastructure investment, or zoning). As we discuss below, while there are a few dimensions along which we can measure the differentiation embodied in such policies directly, it is generally not possible to acquire systematic data on city-level policies that are disaggregated along these two dimensions. Thus, to develop a broad measure of the impact of descriptive representation, we mainly focus on spatial differentiation in housing prices as proxies for how different groups value the public goods provided by local government. To provide context for our empirical analysis, we begin our analysis by characterizing the link between changes in policies that differentially affect members of one group and changes in housing prices, as measured at the neighborhood-level.

Consider a newly elected councilmember in city G who is interested in directing benefits towards a particular subgroup of her electorate ($subgroup = k \in K$).[10] One approach would be a "group-targeting" strategy that directs resources to policies which differentially benefit individuals

[10] Our aim here is to provide a simple characterization of the relevant housing market dynamics. One could imagine that the council directs public goods towards a subgroup either by reallocating resources from a fixed budget or by increasing totally expenditures. While the channels highlighted here will operate in either case, it is worth noting that California cities are relatively constrained in their ability to generate new revenue because of Proposition 13.

of subgroup k, regardless of the individual's neighborhood choice. This type of investment will give rise to a set of city-level group-specific public good levels, $\{G^k | k \in K\}$. A second "neighborhood-targeting" approach would direct city resources to specific neighborhoods ($neighborhood = j \in J$) where individuals of subgroup k are congregated. This policy will give rise to neighborhood-specific public good levels $\{G^j \middle| j \in J\}$. The potential for this strategy to differentially benefit members of a specific group is increasing in the proportion of neighborhood j that is comprised of group k. Thus, a councilmember's ability to use a neighborhood targeting approach would be expected to increase with segregation levels.

We posit a simple housing market model. First, given that the cities we evaluate are typically small relative to their housing markets, basic market dynamics are embedded in a small open city model. Abstracting from search frictions and assuming for simplicity that any surplus goes to the seller, the price level for house h in city neighborhood j will be determined so as to equate the marginal buyer's indirect utility in said house to that of a type-specific outside option, $\bar{V}^k$, whose level is exogenous to changes in the public goods provided in neighborhood j's city. Assuming that the marginal buyer is from group k, house price P_h is implicitly defined by:

$$V\left(Y_i^k - P_h, G^k, G^j, \xi^{jk}\right) + \varepsilon_{ih} = \bar{V}^k \tag{1}$$

where Y_i^k represents the marginal buyer's income, ξ^{jk} represents the value of non-public good related characteristics of neighborhood j to subgroup k, ε_{ih} is an idiosyncratic taste shock that buyer i has for house h and $\bar{V}^k$represents the value (in terms of indirect utility) of the outside option.[11]

Consider first the impact of policies that directly benefit members of group k, G^k. Given the open city assumption, impacts will be limited to homes where the marginal buyer is a member of the targeted group. Given equation (1), at these homes the marginal buyer's offer price will increase according to $\frac{dP_h}{dG^k} = -\frac{V_{G^k}}{V_Y}$. Thus, at the margin, the change in transaction prices for houses purchased by individuals of group k will exactly measure individual willingness to pay for

[11] For further exposition on this basic modeling approach see Polinsky and Shavel (1976), Rosen (1974) and Sieg et.al. (2002). For simplicity, we have ignored property taxes, though it would be straightforward to incorporate taxes into the model.

increases in the group-specific public good. And, to reiterate, when the marginal buyer is not a member of the targeted group and therefore doesn't value the increased public good level we would expect no change in price.[12] While non-marginal changes don't allow for as simple an interpretation general implications are similar, it is this basic analysis that underpins the large extant literature that utilizes housing prices as a proxy for valuing changes in public goods.

One key complication in our context is that we don't observe the group membership of individual home purchasers. However, if as is typically the case in our context, neighborhoods are segregated by group-type neighborhood-level price changes will capture the benefits associated with group-specific policies.

The price results for neighborhood targeted policies are similar, except now the marginal household is characterized neighborhood location instead of group type and equation (1) implies that $\frac{dP_h}{dG^j} = -\frac{V_{G^j}}{V_Y}$. Of course, this type of policy can only be effective if neighborhoods are segregated by type. Thus, in both cases we expect to more clearly identify the potential impact of descriptive representation in more segregated cities. The effectiveness of group-level policies is independent of segregation levels, but our ability to measure their impact relies on the presence of segregated neighborhoods. Conversely, we can measure the impact of neighborhood-level policies regardless of segregation levels, but their functionality in delivering group-specific benefits relies on the presence of segregated neighborhoods.

The above framework illustrates the link between group or place-based policies, changes in housing prices and changes in welfare. However, it is natural to wonder whether it is appropriate to associate increased housing prices with increased welfare in neighborhoods comprised mainly of renters. For renter households, at least some portion of the benefit from increases in public goods will accrue to the owner in the form of higher rents. Along similar lines, one might worry that, if the councilmember uses a neighborhood-targeting policy, she may spur a gentrification movement that displaces members of her subgroup. Both channels are likely operating to some degree in our study area and bear consideration. Nonetheless, it seems unlikely that they would be dispositive either in terms of politician behavior (i.e. leading minority politicians to abandon

[12] This result follows directly from the open city assumption, see Polinsky and Shavel (1976). to the extent that increasing public goods for one group requires decreasing public goods for another group, for instance due to budget constraints, we would expect to see a decline in prices for homes where the marginal buyer is outside of the targeted group.

policies that differentially benefit minority citizens) or in terms of actual benefits (i.e. leading to the complete leakage of potential benefits). Further to this point, we observe no change in the volume of housing transactions or rate of evictions following the election of a nonwhite councilmember. Our results are also not sensitive to the share of rentals in the neighborhood. These findings provide additional support for our interpretation that increases in house prices in nonwhite neighborhoods reflect increased well-being for the residents of those neighborhoods.

3. Empirical Context

For our analysis of descriptive representation, we adopt an empirical approach that leverages narrowly decided elections between candidates of different ethnicities in order to obtain plausibly exogenous variation in minority representation on a city council. In our core model, we examine whether changes in minority representation generate differential housing market responses across minority and non-minority neighborhoods.

We focus our analysis on city council elections in California. California is particularly apt for our study because it contains a large number of municipalities and is quite diverse – assuring that we observe both a large number of close elections between white and nonwhite candidates and substantial variation in neighborhood ethnic composition. An additional benefit of this context is that California state law serves to limit variation in the structure of municipal governments, thus providing for comparisons across councils without substantial accompanying variation in institutional features. For instance, 88% of city councils contain exactly five councilmembers, the legislated minimum. Councilmembers serve staggered four-year terms, with 92% of cities electing members through "at-large" elections. Moreover, 93% of cities use a "council-manager" governance structure meaning that the council dictates the policy and the mayor – who for 98% of cities is simply selected by the council from amongst its own members – oversees carrying out said policy.

So far, the discussion has been quite general about the nature of the public goods being provided and/or reallocated; however, it is worth considering what kinds of public goods are actually controlled by cities and subject to the influence of councilmembers. There are some limitations. As mentioned earlier, elected school boards, rather than city councils, control local school policy. Proposition 13 also imposes substantial restrictions on property tax growth. These tax restrictions will often require new spending to be offset by reductions elsewhere.

Councils supervise the administration of many local goods and services. The services directly provided by cities vary. Data from the California State Controller's Office provides an overview. The vast majority of cities in our sample (89%) provide their own community development planning and manage their own parks and recreation services (88%). Over 70% directly manage their own police forces. Around half manage firefighting, street lighting, and water and sewage provision, while around a quarter provide their own emergency medical services and libraries. Most do not directly provide solid waste disposal or public transit, either contracting out for these services or working with a larger municipality or special-purpose district (such as BART). Even if a city does not directly provide certain services, councilmembers may still have substantial influence over the behavior of the private companies with which they contract or the regional agencies with whom they have cooperative agreements.

Local governments also regulate and control land use. California state law requires that each jurisdiction adopt a comprehensive plan for its development; this plan encompasses a jurisdictions' policies regarding "the location of housing, business, industry, roads, parks, and other land uses, protection of the public from noise and other environmental hazards, and conservation of natural resources" (GOPR, 2001). In California, city councils are responsible for approving and modifying zoning ordinances, which have considerable power to affect patterns of local economic development at the neighborhood level. By controlling the distribution of land uses, these ordinances can strongly influence patterns of exposure to industrial activity and pollution, traffic congestion, employment opportunities, commercial amenities, and even street crime. They can also affect the location of new housing development, at both the small scale (backyard accessory dwelling units) and the larger scale (high-rise multifamily housing), which can directly and indirectly affect housing values.

A few examples from recent city council elections in California further illustrate these linkages. In 2016 Juan Carillo ran for council in Palmdale, California. A major focus of his campaign was the stark differences between his east side neighborhood where the vast majority of the city's Hispanic citizens live and the rest of the city. Carillo highlighted issues such as unhealthy chain restaurants and inferior parks. Upon winning election with 52% of the vote, Carillo introduced legislation to give individual council members responsibility for appointing planning commissioners.[13] Policing and treatment of immigrants was also a focus in many campaigns. For

[13] See Constante (2018)

example in 2008 Olga Diaz became the first self-identified Latino councilmember in Escondido, California. Despite the city's large Hispanic population, it had gained a reputation as a "city without pity" for undocumented immigrants (Jenkins, 2008). City Council had previously passed an ordinance targeting landlords who rented to undocumented immigrants, while the police department had established traffic checkpoints targeting unlicensed drivers (many of whom were undocumented). After Diaz's election, the previous 3-2 majority that generally favored anti-immigrant policies was broken, and the council shifted its focus towards economic development, local revitalization, and quality-of-life issues (Florido, 2009). As a final example, Sacramento NAACP president Betty Williams is currently running for the Sacramento City Council. Her platform focuses on strengthening the city's Community Police Review Commission and targeting newly available tax revenues to job training and minority business startups.[14]

These three examples provide anecdotal evidence regarding some of the channels through which candidates/council-members can pursue policies which differentially affect minority vs. non-minority groups and neighborhoods. In the analysis that follows, we pursue a more systematic assessment of these linkages.

4. Data

Our empirical analysis draws on four broad sources of data: election outcomes, candidate characteristics, house transactions, and neighborhood characteristics. This section describes each of these data sources.

4.1 Election Outcomes

Our source for election outcomes is the *California Election Data Archive* (CEDA). This archive reports the number of votes each candidate received for every local government election in California 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 the relevant outcome variables, CEDA also lists the candidate's full name and occupation. CEDA does not list the candidate's race or ethnicity. Further, California state law

[14] Kumamoto and Smith (2019)

requires city council elections to be non-partisan, and so political party does not appear on the ballot or in CEDA. Thus, we draw on this name and occupation information to supplement CEDA with data on candidate ethnicity and partisan affiliation.

Beach & Jones (2017) constructed a dataset identifying the race/ethnicity (and gender) for 4,226 of the 5,177 councilmembers and candidates who either served on a city council between 2005 and 2011 or ran for city council and lost narrowly, which we draw on in this paper. We refer readers to that paper for a detailed description of the data construction process. In short, the process entailed finding photographs of candidates online, then asking Amazon Mechanical Turk workers to code their assessment of the race the candidate based on the photo and name, with 10 workers coding each photo.[15] For the average candidate, 9.4/10 coders agreed on the race/ethnicity. Additional sources were used to validate the resulting dataset.[16]

In order to identify individual candidate's partisan affiliations, we link our candidate sample to California voter registration data files, which contain the universe of registered voters in California and their partisan affiliation (if registered with a party). We use 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 conservative in that we favor missing observations over false matches. Ultimately, we are able to match 81% of the candidates in our sample. As a result, we can identify the partisan affiliation of two competing candidates in 61% of our sample elections. We use these data to rule out that our results are driven by a change in the partisan composition of the council.

4.2 Neighborhood Characteristics

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", we are referring to Census block groups. We use 2000 Census data, as opposed to – for instance – 2010 American

[15] Recently, Sumner et al. (2019) assessed the accuracy of data collected through Mechanical Turk. They used similar methods to collect data on current local governments from throughout the US and compared the resulting data to an accepted benchmark dataset. They found that data collected through Mechanical Turk is "highly accurate".
[16] Specifically, Beach & Jones (2017) drew on rosters of Latino elected officials (from National Association of Latino Elected Officials) and Asian/Pacific Islander elected officials (from Asian Pacific American Institute for Congressional Studies), as well as direct information from a small number of cities that replied to emails. These sources were used in place of Mechanical Turk data where possible, but were also used to validate data resulting from Mechanical Turk.

Community Survey data, to ensure that our neighborhood controls are not endogenous to election outcomes.

These 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. 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.[19]

We identify block groups as *majority white* if the block group's white population share is greater than 0.5. Conversely, *majority nonwhite* block groups are those with a white population share less than 0.5.[20] While this distinction is the basis for our core analysis, we consider a set of alternative definitions for nonwhite neighborhoods in a series of robustness checks.

4.3 *Housing transactions data*

We obtain information on housing sales from transaction-level data provided by DataQuick Information Systems under a license agreement. This dataset includes the universe of single-family home sales in California between 2005 and 2011. The transaction records are also matched with assessor data to get the following housing characteristics: bedrooms, bathrooms, stories, square footage, and year built. We trim the top and bottom 1% of observations (in terms of price) to eliminate homes transferred for the nominal amount of $1 and homes valued in excess of $2.8 million.

---

[19] 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*.

[20] We exclude the small share of the population that identifies as multiracial from this calculation. More precisely, a majority nonwhite neighborhood is a block group where [(sum of single-race nonwhite population)/(sum of single-race population)]>0.5. Results are essentially unchanged if we instead include multi-racial counts.

To account for variation in price levels across local housing markets and over time, we follow Sieg et al. (2002) and estimate year-by-quarter price indices for each of the 18 commuting zones (CZ) in our dataset. We then use these estimated price indices to adjust the observed nominal prices for inflation. Specifically, we regress the log of the transaction price on year-by-quarter-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, population density, and ethnic 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. 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.

4.4 Data summary

To assess the impact of increased minority representation, which is measured as the addition of a nonwhite member to a city council, we employ a regression discontinuity (RD) design. 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). Specifically, 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[21]), one is white and the other is nonwhite, and (2) the election was within an optimally selected bandwidth. In our main analysis, we include elections are those decided by 5.88 percentage points or less. This optimal bandwidth was chosen following Calonico et al. (2014). See section 5.2 for more details and a discussion of sensitivity analysis.

Table 1 provides basic summary statistics comparing cities in our estimation sample (Column 2) to all cities in California (Column 1). Panel A focuses on city-level demographic information. Cities in our estimation sample are slightly larger (75,665 vs 56,939) and more diverse (white share of 42% vs. 56%) than cities in California as a whole. Panel B examines council

[21] City councils typically fill multiple seats through at large elections. 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.

characteristics and shows that the council sizes in our sample are roughly the same as the average California city council and seats on the council are determined in a similar way (through at large rather than district-based elections). The final two rows of Panel B draw on additional data from Beach & Jones (2017) on the ethnicity of the other councilmembers that would serve with the winner of the close election. Here we see that the average council in our estimation sample is 66% white and 25% are entirely white. Thus, we can generally interpret the election of a nonwhite candidate as leading to an increase in nonwhite representation on the council.

Table 1: Summary statistics

	(1) All cities	(2) Cities with close wht. vs. nonwht. elections
Panel A: City-level Demographics		
Total population	56,939.67	72,665.14
	(185,534.5)	(125,242.2)
Asian/Pac. Isl. share	0.09	0.15
	(0.11)	(0.15)
Black share	0.04	0.05
	(0.06)	(0.06)
Hispanic share:	0.30	0.37
	(0.25)	(0.23)
White share:	0.56	0.42
	(0.26)	(0.21)
Other share:	0.00	0.00
	(0.00)	(0.00)
Ethnic fractionalization	0.44	0.54
	(0.17)	(0.13)
Panel B: Council Characteristics		
Council size	5.32	5.38
	(0.97)	(0.91)
District-based elect.	0.09	0.07
	(0.29)	(0.26)
Rest-of-council white share		0.66
		(0.28)
Rest-of-council is all white		0.25
		(0.42)
Observations	458	201

Standard deviations in parentheses. Population and ethnicity shares come from the 2000 census. Observations in Column 1 correspond to cities while the observations in Column 2 correspond to the number of relevant elections that take place in our sample period.

A few additional summary statistics warrant attention. First, among the 1,986 elections in our sample where the ethnicity of both marginal candidates could be identified, both marginal candidates were white 45% of the time, both marginal candidates were nonwhite 21% of the time, and the remaining 34% of elections contained one white and one nonwhite marginal candidate. Thus, white vs. nonwhite elections are not particularly rare. Further, among all white vs. nonwhite elections, the nonwhite candidate is most often Hispanic (58% of all marginal white vs. nonwhite elections). The next largest shares are Asian/Pacific Islander (19%) and Black (16%). As we discuss later, we run race-specific regressions to help rule out the possibility that our results are solely driven by Hispanic candidates.

Table 2: Baseline price differences for houses in majority nonwhite neighborhoods

	DV is ln(Adjusted house price)				
	(1)	(2)	(3)	(4)	(5)
Maj. nonwhite neighborhood	-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
Neigh. income				X	X
Other neigh. 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. *House characteristics*: # bathrooms, # bedrooms, # stories, sq. footage, age at sale; *Neighborhood income*: Median household income, % below poverty level, % on public assistance; *Other neighborhood 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

Given our focus on differences in prices between majority white and majority nonwhite neighborhoods, we begin by establishing the existence of baseline differences in prices across these two types of neighborhoods. Column 1 of Table 2 reports a bivariate regression of adjusted log price on an indicator for whether the house is in a majority nonwhite neighborhood. These results indicate that 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 gap in prices between houses in nonwhite and white neighborhoods; houses in nonwhite neighborhoods sell for roughly 5.5 percent less than houses in majority white neighborhoods. Thus, in assessing the differential impact of candidate ethnicity on house prices in nonwhite neighborhoods, we are essentially testing whether minority representation affects this pre-existing gap in house prices across neighborhood types.

5 Main Analysis

Our main empirical approach is a panel-based regression discontinuity design, similar to Cellini et al. (2010). We begin by expositing our analytical design – building up from a basic cross-sectional RD model.

5.1 Empirical approach

We identify the causal impact of electing a minority council member using local linear regressions estimated on a sample of close elections between white and non-white candidates. More formally, we limit our sample to elections that fall within a narrow bandwidth of an equal vote share outcome. We define "nonwhite margin of victory" as the difference between the nonwhite candidate's vote share and the white candidate's vote share. Thus, A positive margin of victory indicates that the nonwhite candidate won the election and a negative margin of victory indicates that the white candidate was the winner; margins close to zero indicate a close election.

We take individual housing transactions as our unit of observation. Since councilmembers serve staggered four-year terms, the composition of the council is only stable for two years. Accordingly, in a simple cross-sectional model, analysis would be restricted to transactions occurring during the two-year "council term" following a relevant election – yielding the following empirical specification:

$$\begin{aligned} \ln{(p)_{hct}} \quad = \quad & \propto + \beta_1 \mathbf{1}[Nonwhite\ wins_{ct}] + \beta_2 margin\ of\ victory_{ct} \\ & + \beta_3 \mathbf{1}[Nonwhite\ wins_{ct}] * margin\ of\ victory_{ct} \\ & + \varepsilon_{hct} \end{aligned} \tag{2}$$

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 the nonwhite candidate wins, which we fully interact with the nonwhite candidate's "margin of victory." The coefficient β_1 therefore identifies the effect of a nonwhite candidate winning conditional on the margin of victory being zero. Thus, under the assumption that winners of close elections are 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.

While equation (2) can identify the impact of increased minority representation on housing values overall, our main interest is in understanding whether an increase in representation differentially affects housing values in minority neighborhoods. To address this question, we modify equation (2) by fully interacting all 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 neighborhood. The modified specification is then:

$$\begin{aligned} \ln(p)_{hct} = \; & \propto + \beta_1 \mathbf{1}[NW\ wins_{ct}] + \beta_2 margin_{ct} + \beta_3 \mathbf{1}[NW\ wins_{ct}] * margin_{ct} \\ & + \beta_4 \mathbf{1}[NW\ wins_{ct}] * \mathbf{1}[NW\ Neigh_h] + \beta_5 margin_{ct} * \mathbf{1}[NW\ Neigh._h] \\ & + \beta_6 \mathbf{1}[NW\ wins_{ct}] * margin_{ct} * \mathbf{1}[NW\ Neigh._h] + \beta_7 \mathbf{1}[NW\ Neigh._h] + \varepsilon_{hct} \end{aligned} \tag{3}$$

where "NW Neigh." is an indicator for whether the neighborhood (Census block group) is majority nonwhite. Now, β_1 identifies the causal impact of a nonwhite victory on house prices in majority white neighborhoods and β_4 identifies the *differential* effect of a nonwhite victory on nonwhite neighborhoods. As our main goal in this study is to test whether increased nonwhite representation generates *differential* benefits for majority nonwhite neighborhoods, β_4 will be the primary coefficient of interest as we discuss our results.

While equation (3) is adequate to identify the differential effect that is the main target of our analysis, stronger identification and more precise estimates can be gained by incorporating the basic logic of equation (3) into a panel data strategy. As such, the results presented throughout this paper uses a panel-based parallel to Equations 2 and 3, which we describe next. We do note, however, that estimates from the simpler cross-sectional approach are consistent with our main results. We report those estimates in Section 2 of our Appendix.

In the panel model, we restrict the sample to the two-year council terms immediately preceding and following a relevant election.[26] To reflect the level of treatment, our main specifications include election 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 a separate panel, with a different fixed effect. In other words, our data in this approach is configured 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, allow us to evaluate the change in house prices in cities where the nonwhite candidate won to changes in house prices in cities where the nonwhite candidate lost. This analysis contrasts with the cross-sectional approach, which simply compares post-election transactions in cities that elected a nonwhite candidate to post-election transactions in cities that elected a white candidate.

The panel analog to equation (2) is as follows:

$$\begin{aligned} \ln(p)_{hect} = {} & \propto + \beta_1 \mathbf{1}[Nonwhite\,wins_{ec}] + \beta_2 margin_{ec} + \beta_3 \mathbf{1}[Nonwhite\,wins_{ec}] * margin_{ec} \\ & + \beta_4 \mathbf{1}[Nonwhite\,wins_{ec}] * \mathbf{1}[Post_{ect}] + \beta_5 margin_{ec} * \mathbf{1}[Post_{ect}] \\ & + \beta_6 \mathbf{1}[Nonwhite\,wins_{ec}] * margin_{ec} * \mathbf{1}[Post_{ect}] + \beta_7 \mathbf{1}[Post_{ect}] + \gamma_{ec} + \varepsilon_{hect} \end{aligned} \qquad (4)$$

This equation is similar to equation (2) in that it 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 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}.[27]

Given that our primary focus is testing whether candidate ethnicity has different effects on different types of neighborhoods, we actually estimate a modified version of equation (4). The modified equation, which parallels equation (2), interacts all "treatment" variables (nonwhite

[26] Again, "council term" refers to the two-year period between elections in which there are no changes in the composition of the council.

[27] Note that in practice the "Nonwhite wins", "margin", and "Nonwhite wins * Margin" variables are absorbed by the election fixed effects, and are therefore not identified. We present them as part of equation (4) for illustrative purposes only.

winner, margin, post, and all interactions of these) with an indicator variable equal to one if a neighborhood 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 neighborhood", which identifies the *differential* effect of a nonwhite winner on housing values in nonwhite neighborhoods. As in the discussion above, the second of these will be of primary interest.

Finally, we incorporate into the empirical specifications reported in this section controls for housing characteristics, neighborhood characteristics, year-month dummies, and city-specific linear time trends.[28]

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 methods 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 would be well-suited to identifying a bandwidth if one outcome was 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 close to the cutoff, but many of them belong to the same 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 adjusted housing prices in the two years following the election. This yields a single observation per election. We then use the Calonico et al. (2014) bandwidth selection procedure, which suggests that the optimal bandwidth in our setting

[28] Specifically, we control for the following housing characteristics: num. of bedrooms, num. of bathrooms, num. of stories, square footage, age of the house; the following neighborhood characteristics, all at the block group level: population density, share pop. urban, race shares, gender shares, young and elderly population shares, shares of households by household composition (single, married, married with children, etc.), vacant housing share, renter occupied share, owner occupied share, and ethnic fractionalization, median household income, share below poverty line, and share on public assistance. As we will show, results are generally robust to the exclusion of these controls.

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. However, we demonstrate the robustness of our results to a variety of alternative bandwidths.[29]

5.3 Assessing the validity of our regression discontinuity design

In the appendix we present several tests aimed at illustrating the validity of our regression discontinuity design, which we summarize here. One concern is that in our close elections the nonwhite candidate may have a distinct advantage (or disadvantage), which would undermine our assumption that the outcome of a close election is as good as random.[30] In Appendix Figure A1, we follow McCrary (2008) and plot a discontinuous density function around the cutoff (nonwhite margin=0). That 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, which helps alleviate concerns about a systematic advantage for nonwhite candidate in close elections. Next, in Appendix Figures A2, A3, and A4, we assess the identifying assumption that other observable characteristics behave smoothly around the cutoff. Here 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 A3): 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 nonwhite candidates are more likely to be a registered Democrat (and less likely to be a registered Republican). While this finding is not surprising, it may mean that our results are in fact driven by partisan differences. In a later robustness check we address this concern by restricting the sample to close elections where both

[29] Appendix Figure A7 documents the number of unique elections and the number of unique housing transactions that occur within each bandwidth.

[30] While Caughey & Sekhon (2011) and Grimmer et al., (2011) have questioned the "randomness" near the cutoff when applying regression discontinuity designs to elections, Vogl (2014) documents concerns specifically in the context of race and city politics among southern US states.

marginal candidates share the same partisan affiliation. This restriction does not meaningfully change our results.

5.4 Main results

Panel A of Table 3 identifies the causal impact of electing a non-white city council member on city-wide property values – based on the specification presented in equation (4). Panel B of the table incorporates the full set of non-white neighborhood interactions. 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 increasingly larger sets of controls. Column 1 simply takes the adjusted log house price as the outcome with no controls for house or neighborhood characteristics. Column 2 adds controls for housing characteristics and Column 3 adds controls for neighborhood characteristics. Finally, Column 4 adds city-specific time trends. Column 4 is our richest specification, and is our preferred specification.

We find no evidence that the election of a nonwhite councilmember affects average housing values at a city-wide level (results reported in Panel A). 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 0.7 percent. The estimates in Columns 1-3 are larger in magnitude (roughly, a 3 percent increase in housing values), but are imprecisely estimated.

In contrast, our main results - presented in Panel B - show that this average effect masks clear distributional effects. Across all specifications, we find a significant positive coefficient on "Nonwhite win X Post X Majority nonwhite neighborhood", which indicates that the election of a nonwhite candidate (rather than a white candidate) leads to differential increases in housing values in nonwhite neighborhoods. Given the antecedent gap in housing prices across white and non-white neighborhoods, this result can be seen as helping to reverse these pre-existing differentials. In our preferred specification (Column 4), we find that the election of a nonwhite candidate leads to housing values in majority white neighborhoods that are 6.4 percent lower than if a white candidate had won. We observe a corresponding differential positive effect in majority nonwhite neighborhoods. The effect on nonwhite neighborhoods is 11 percent more positive than the effect on white neighborhoods. This is our main result: the election of a nonwhite candidate has a significantly different effect on outcomes in majority nonwhite neighborhoods than on white neighborhoods, indicating distributional consequences of increased descriptive representation.

Table 3: Panel RD estimates of councilmember ethnicity on housing values

	(1)	(2)	(3)	(4)
Panel A: Overall effects				
Nonwht. win X Post	0.038	0.035	0.035	0.007
	(0.044)	(0.042)	(0.041)	(0.027)
Panel B: Differential effects by neighborhood type				
Nonwht. win X Post	-0.058	-0.066	-0.068	-0.064**
	(0.046)	(0.046)	(0.045)	(0.030)
Nonwht. win X Post	0.147**	0.153***	0.156***	0.111**
X Nonwht. Neigh.	(0.061)	(0.056)	(0.056)	(0.045)
Linear combination to recover full effect on Nonwhite Neighborhoods				
(NW win X Post) +	*0.089**	*0.087**	*0.088**	*0.046*
(NW win X Post X NW. Neighborhood)	*(0.050)*	*(0.047)*	*(0.048)*	*(0.035)*
Level of FEs	Election	Election	Election	Election
House controls		X	X	X
Neighborhood controls			X	X
Linear city time trends				X
Observations	602,977	602,977	602,977	602,977

The outcome variable is an adjusted log house price. 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). House controls: num. of bedrooms, num. of bathrooms, num. of stories, square footage, age of the house. Neighborhood controls, all at the block group level: population density, share pop. urban, race shares, gender shares, young and elderly population shares, shares of households by household composition (single, married, married with children, etc.), vacant housing share, renter occupied share, owner occupied share, and ethnic fractionalization, median household income, share below poverty line, and share on public assistance.

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

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

Towards the bottom of the table we take the linear combination of "Nonwht win X Post" + "Nonwht win X Post X Nonwht. Neigh.". This statistic allows us to assess whether houses in majority nonwhite neighborhoods experienced a statistically significant overall increase in value when a nonwhite candidate won. Here we find a net price increase of 4.6 percent when a nonwhite (rather than white) candidate is elected, although this absolute effect is less precisely estimated.[36] We emphasize that these results reflect the differential effect of a nonwhite candidate winning relative to the effect of a white candidate winning. They could either be driven by the nonwhite candidate or the counterfactual white candidate or some combination of the two.

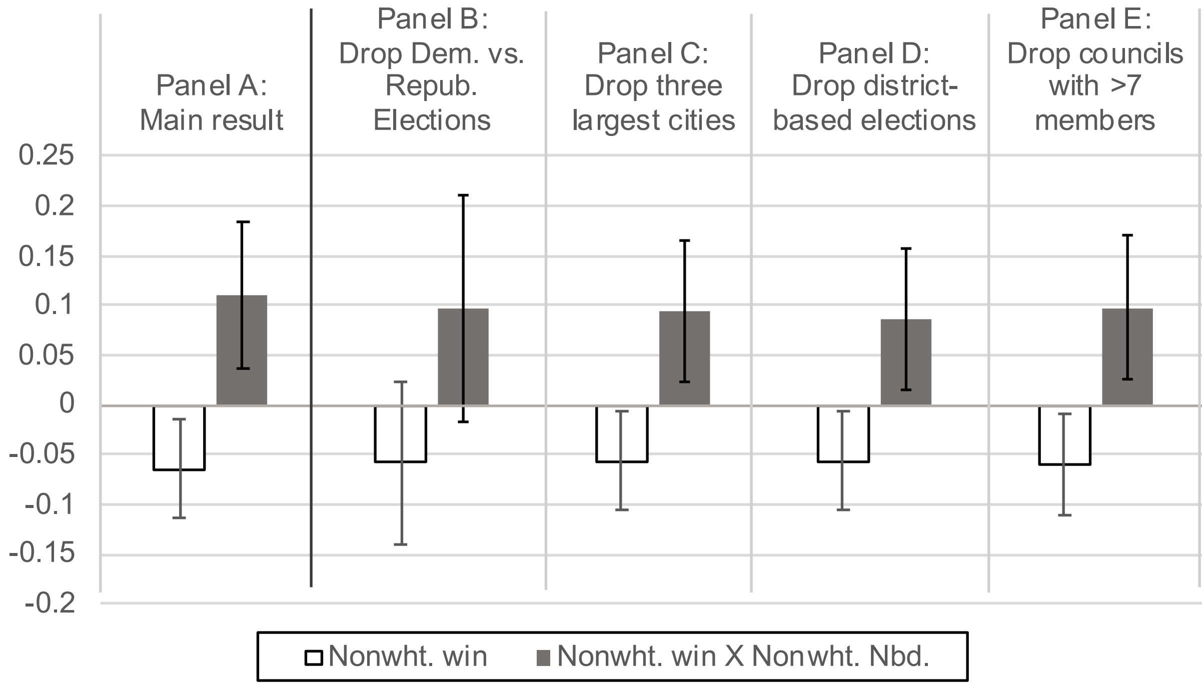
5.5 Robustness of Main Results

In Figure 1 we present a series of robustness checks for our main result. For the sake of comparison, Panel A of the figure depicts our main coefficient estimates, corresponding to estimates reported in Column 4 of Table 3; 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. Neigh." coefficient).

As our first robustness check we address the concern that our results may be driven by differences in partisan preferences. As noted earlier, there is a correlation between a candidate's ethnicity and a candidate's partisan preferences, where nonwhite candidates are more likely to be registered Democrats. 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 preference). Among the white candidates, 38% are Democrats, 48% are Republicans, and 14% are other/no preference. To determine if our main result is driven by partisan preferences, Panel B presents results excluding 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, though the standard errors are larger due to the reduction in sample size.

[36] We note that a small majority of neighborhoods in our estimation sample are majority nonwhite (54% vs. 46%). This helps explain why there is no average effect of nonwhite representation on housing prices (Panel A) even though the positive level effect on nonwhite neighborhoods (+0.046) is smaller in magnitude than the negative level effect in white neighborhoods (-0.064).

Figure 1: Robustness of panel RD results

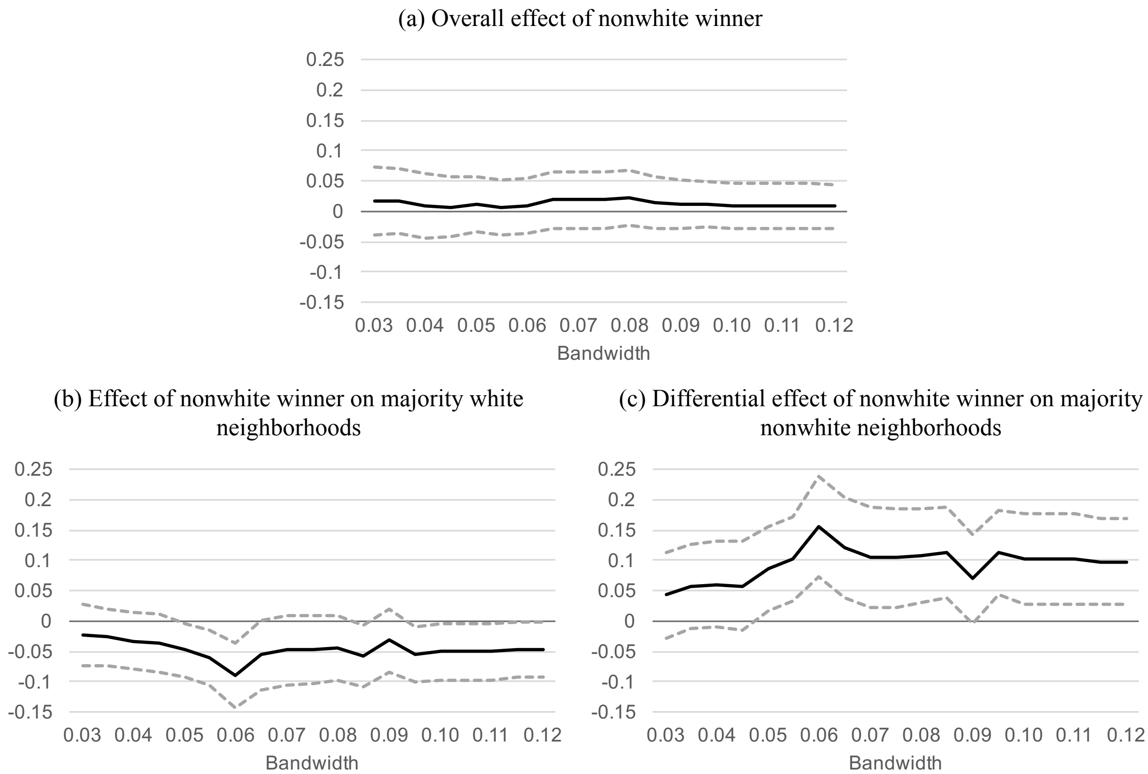


Note: This figure depicts results from five distinct regressions, with each pair of bars representing the relevant coefficient estimates from a single regression. Panel A simply reports our main result (from Table 3, Panel B, Column 4) for the sake of comparison with the rest of the figure. Note that 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. See notes of Table 3 for a full list of controls included in these regressions.

The remainder of Figure 1 reports additional sensitivity tests. In Panel C we drop the three largest cities in our sample; Panel D drops cities with district-based elections; Panel E drops the small number of cities with large (>7 members) councils. 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. One might be concerned that nonwhite candidates are more likely to be elected in district-based elections, and so our results could simply reflect the fact that councilmembers generate benefits for their own districts, which happen to match their ethnicity. Panel D shows that cannot be the full explanation for our results.

Panel C excludes larger cities. In a sense, Panel E does as well, as larger councils are most common in larger cities. Excluding larger cities is useful given that our unit of analysis is a housing transaction. As there are more transactions in larger cities, transactions in these cities make up a disproportionately large share of observations in our sample. [38] It is therefore useful to show that, despite that, those cities are not driving our results.

Figure 2: Effect of a nonwhite candidate victory, across multiple bandwidths



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 follows Column 4, Panel A, Table 3 but within a different bandwidth. 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 (Column 4, Panel B, Table 3). In all panels, the dotted lines represent the 90% confidence intervals.

Next, Figure 2 shows that our general conclusions are not sensitive to our choice of bandwidth. Figure 2 graphically depicts estimates from a series of specifications, re-estimating our main specifications with different bandwidths. Panel (a) reports the primary coefficient of interest from the simpler specification that does not allow for differential effects across neighborhood types, corresponding to Panel A in Table 3. Panels (b) and (c) report the two main coefficients from the specification that does allow for differential effects, corresponding to Panel B in Table 3.

[38] In a related test, we have also repeatedly estimated our main model, one time for each city in our model, where in each estimation, we exclude exactly one city. This is aimed at ensuring that our results are not largely driven by patterns in a single city, and indeed we get very similar results across all of these estimates. These results are available upon request.

We re-estimate these models for bandwidths ranging from 3 percentage points to 12 percentage points (roughly half and double the main bandwidth), in 0.5 percentage point increments. The bandwidth being used is reported along the horizontal axis of the figure. The corresponding y-axis value at each point (solid dark line) reports the coefficient estimate with confidence intervals (dotted grey lines).

We consistently find that the election of a nonwhite candidate helps reduce the pre-existing gap in house prices between majority white and majority nonwhite neighborhoods (Panel (c)), albeit with reduced precision for especially narrow bandwidths. Likewise, at all bandwidths housing values in majority white neighborhoods are lower than they would have been if a white candidate had been elected (Panel (b)).

Table 4 reports additional robustness and placebo tests. There we show that the main results are robust to including city fixed effects (Column 1) or neighborhood-by-election fixed effects (Column 2), instead of the election fixed effects used elsewhere. Columns 3 and 4 present results considering different definitions of nonwhite neighborhood. Column 3 defines high nonwhite as above the city's median nonwhite share, rather than simply above 50%, while Column 4 uses the continuous measure of nonwhite share in the neighborhood in lieu of the "majority nonwhite" dummy. Both yield results consistent with our main result.

Column 5 reports results of a specification that excludes controls for, or interactions with, the nonwhite candidate's margin of victory. This difference-in-differences approach identifies the impact of a nonwhite winner based on all observations 5.88 percentage points to the right and left of the cutoff. This contrasts with our main specification, which fits a line through margin of victory in order to identify the effect *at* the cutoff. Results are similar although the magnitude of the effects are attenuated: a nonwhite candidate has essentially no effect on majority white neighborhoods and a 5.7 percent differentially positive effect in majority nonwhite neighborhoods. This is noteworthy, in part because the magnitude from our main regression discontinuity estimates (Table 3) may be viewed as large. Those estimates, however, are drawn from the gap in housing prices at a margin of victory of zero, based on gaps between the endpoints of fitted lines on either side of the cutoff. Therefore, while the regression discontinuity approach provides clean identification on the direction of the results, the simpler difference-in-differences approach reported here may provide a more accurate sense of the magnitude of the effect.

Table 4: Sensitivity and placebo tests

	Alternative Fixed Effects		Alternative definitions of nonwhite neigh.		No Margin of Victory Interactions	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	-0.062**	-0.036	-0.025	-0.166***	-0.007	-0.010	0.004
	(0.030)	(0.027)	(0.026)	(0.059)	(0.020)	(0.034)	(0.027)
Nonwht. win X Post X Maj. Nonwht. Neigh.	0.113***	0.074**			0.057**	0.011	-0.009
	(0.043)	(0.034)			(0.024)	(0.042)	(0.047)
Nonwht. win X Post X High Nonwht. Neigh.			0.067*				
			(0.035)				
Nonwht. win X Post X Nonwht. share				0.288***			
				(0.101)			
Level of FEs	City	Neighborhood X Election	Election	Election	Election	Election	Election
House controls	X	X	X	X	X	X	X
Neighborhood 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

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). See notes of Table 3 for full set of controls. In Column 5, we omit interactions with and controls for the nonwhite candidate's margin of victory, but (like other specifications) restricts the sample to within the 5.88 percentage point bandwidth. "High Nonwht. Neigh." (Column 3) is a dummy equal to 1 if the neighborhood is higher than the within-city median of neighborhood nonwhite share. "Nonwht. share" (Column 4) is a continuous variable equal to nonwhite share within the neighborhood. In the placebo test reported in Column 6, the sample period is four years before a relevant election, and so "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.), neighborhood demographic characteristics, and city fixed effects.

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

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

Columns 6 and 7 present two placebo tests. In Column 6 we back our panel up by two years, thus taking the two-year period preceding the relevant election as the "treatment" period. The test reveals no effect, which alleviates concerns about pre-trends. Next, we ask whether our results could be explained by systematic changes in the composition of houses that sell following the election of a white (nonwhite) candidate. Here, we construct a "predicted" housing price, based only on observable characteristics of the house, and take that as an outcome variable. We find no evidence of composition effects. In a related set of tests, Appendix Table A3 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 neighborhoods.

Finally, in Section 4 of our appendix, we discuss and report an analysis where we separately consider the impacts of Black, Asian, and Hispanic candidates. For the sake of statistical power, our main analysis focuses on the impact of nonwhite candidates on white and nonwhite neighborhoods. While nearly 50% of neighborhoods in California are majority nonwhite, the numbers of neighborhoods that are majority Hispanic, Black, or Asian/Pacific Islander are much smaller, which limits our ability to consider the impacts of a nonwhite candidate's victory on more specific neighborhood types. Nevertheless, the appendix presents results for each ethnic group. 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 neighborhoods, which helps justify our more parsimonious focus on just nonwhite vs. white neighborhoods. These results also help alleviate concerns that our results are simply driven by electing residents of one specific neighborhood rather than the race of the candidate.

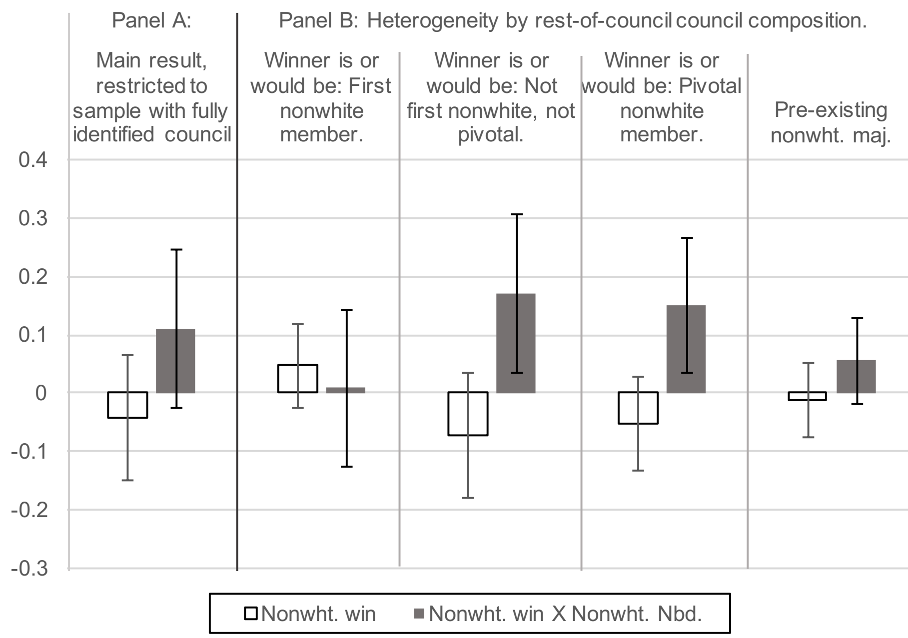
6 Mechanisms

In this section, we present results that help unpack the mechanisms at play. We begin by unpacking the basic housing price results in terms of pivotality, timing, income, ownership and segregation levels. We then work to disentangle the relative roles of non-white and white candidates that underlie our differential results. Finally, we identify shifts in policies/outcomes that may be driving our housing market results.

6.1 Pivotality

We begin with the role of pivotality and how the election of a nonwhite candidate interacts with the ethnic composition of the other councilmembers. To explore this issue, we re-estimate our main specification on four mutually exclusive subsamples, based on the pre-existing 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.

Figure 3: Heterogeneity in impact of nonwhite victory by composition of rest of council



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. Each pair of results in Panel B corresponds to a different sample restrictions. 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. See notes of Table 3 for a full list of controls included in these regressions.

These results are presented graphically in Panel B of Figure 3. 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. Neigh." 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 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).

6.2 Timing

In the appendix, we consider how our estimates evolve over time, both within the two-year period during which the council members are fixed and beyond. Results are depicted graphically in Figure A8. The model reported there extends the sample to include up to four years (rather than two years) after the election date; it also includes, in place of the “Post” dummy, 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 as well as an additional indicator for transactions occurring during the final two years of the councilmember’s elected term.

Changes in housing values occurs quickly and are 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. This could be because of a loss of power – relevant elections occurring towards the end of our study period won’t have a full four years of post-election data – but it could also be because 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.

6.3 Income, ownership and segregation

In Figure 4, 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. 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 neighborhood-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" neighborhoods and "low median income" neighborhoods. We define a neighborhood as "high median income" if the median income is above the sample median of median income across all neighborhoods; when this is not true, a neighborhood is defined as "low median income". Notably, in both subsamples, the pattern of results is qualitatively similar to our main results (see Panel B of Figure 4). 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 versus low share of renters, again defining "high" and "low" relative to the sample median of neighborhood renter share, and again find that results are similar across the two subsamples.[41]

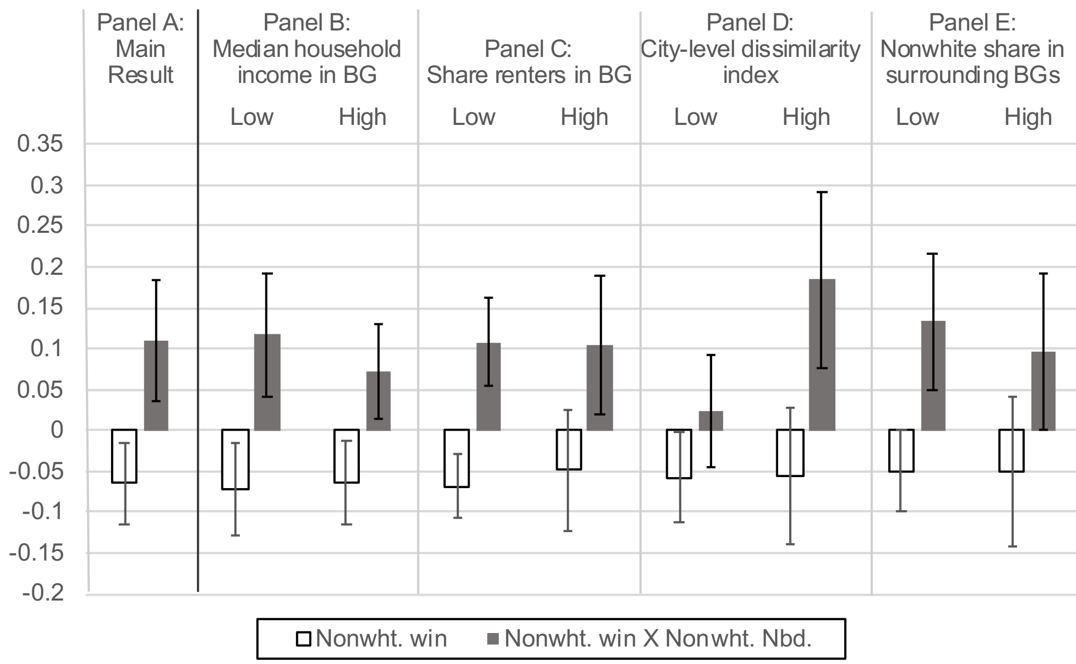
Panels D and E consider a different dimension. Here, we assess how a city's level of segregation interacts with our results. As discussed above, an important potential mechanism for explaining our results is the possibility that a nonwhite candidate wins and directs resources and services towards nonwhite neighborhoods. This channel is likely most effective in segregated cities where there are obvious nonwhite neighborhoods to direct resources towards. Similarly, the impact of policies directed towards nonwhite individuals should have the largest measurable impacts in more segregated neighborhood. The results in Panel D are consistent with these conjectures. Splitting the sample into cities that are high or low on a city-level dissimilarity index (which measures segregation),[42] we see that our results are largely driven by more segregated cities. Panel E aims to measure segregation at a more local level; specifically, for every neighborhood, we measure the nonwhite share of surrounding neighborhoods. We then split the sample into neighborhoods with a high or low nonwhite share in adjacent neighborhoods, with "high" defined as above sample median. A nonwhite neighborhood surrounded by mostly white neighborhoods

[41] 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 neighborhood level. We find no evidence of a change in evictions, either overall or in nonwhite neighborhoods in particular. (Results 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.

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

("low nonwhite share in surrounding block groups") is more isolated than a nonwhite neighborhood surrounded by other nonwhite neighborhoods ("high"). Consistent with Panel D, we find that the impact of a nonwhite victory is larger for the more isolated neighborhoods, though the difference between the two estimates is not statistically significant.

Figure 4: Heterogeneity in impact of nonwhite victory by neighborhood or city characteristics



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. Each pair of results in Panels B through E correspond to a different sample restriction. 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. See notes of Table 3 for a full list of controls included in these regressions.

6.4 Changes in policies/outcomes that underpin our main results

As noted earlier in the paper, we prefer to use housing prices as a proxy for changes in policy and spending patterns that differentially affect minority and non-minority neighborhoods for two reasons. First, from a theoretical perspective they are potentially an ideal "index" number for aggregating across the broad range of policies/outcomes that can be influenced by city councils. Second, because of the paucity of data relating to these policies/outcomes that is systematically available and dis-aggregated to the neighborhood level, we are very limited in what we can measure directly. There are however some noted exceptions.

In our discussion of specific candidate examples, concern about inequities in the neighborhood-level patterns of economic development was a recurring theme. Once elected, city council members play an important role mediating between constituent business owners and the city's various regulatory and permitting agencies. Thus, beyond campaign rhetoric, there is scope for council members to differentially affect neighborhood level outcomes 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. In Column 1 of Panel A in Table 5 we report 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 decrease in majority white areas.

Table 5: Effects of councilmember ethnicity on local economic development

	(1)	(2)
Outcome variable:	Log of number of establishments in ZIP	Any TRI facility in tract? (=1, 0 otherwise)
Nonwht. win X Post	0.006	0.001
	(0.009)	(0.009)
Nonwht. win X Post	-0.038*	0.002
	(0.021)	(0.018)
Nonwht. win X Post	0.078**	0.000
X Maj. nonwht. area	(0.032)	(0.024)
Geographic area	ZIP	Census tract
Level of FEs	Election	Election
ZIP controls	X	
Tract controls		X
Observations	1,541	10,857

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

Of course, the concerns raised by candidates about economic focused not only on levels of activity, but on types of activities as well. While data here is limited, we can test for exposure to polluting businesses using data from the Environmental Protection Agency's Toxic Release Inventory (TRI) program. The TRI 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. Column 2 of Table 5 reports these results. We observe no significant effects either on average or by neighborhood type. Thus suggesting that the increase in business activity documented in Column 1 wasn't associated with an increase in environmental threats faced by local residents, at least to the extent that these threats can be measured by the TRI data.

As discussed above, policing is also a common focus of minority candidates. Because each municipality typically has its own police force, here there is again scope for city council members to impact outcomes – both by setting formal policy and through informal oversight. To assess impact on this dimension, we draw on data from the Federal Bureau of Investigation's Uniform Crime Report. The data report arrests aggregate to the city-by-year level, so we are unable to test for differential effects by neighborhood. The data do, however, report arrests separately by race group, which we take advantage of to test for differential effects by minority status. These results appear in Table 6.

Table 6: 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 (0.048)	-0.042 (0.052)	0.054 (0.054)	-0.016* (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

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. 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. To allow for a direct test of differential (by minority status) effects, in Column 4, we take as an outcome the share of total arrests where the arrested individual is nonwhite. Here, we find that election of a non-white council member is associated with a statistically significant reduction in the minority share of overall arrests. Thus, despite the lack of change in overall arrest rates, it appears that the election of a nonwhite council member leads the composition of arrests to shift away from nonwhite residents.

We also have access to data on a broad range of city-wide budget statistics. While rich in terms of spending and revenue categories, these statistics cannot generally be disaggregated in a way that allows us to test for differential impacts by neighborhood (or as with policing, minority status). Nonetheless in Appendix Table A7 we present results across a range of fiscal outcomes: expenditures, revenues, spending on public goods, safety, transportation, etc. Across each of the 8 categories considered, the estimated effect of electing a nonwhite candidate is never statistically significant. These null results are of interest for several reasons. First, they demonstrate consistency with the larger literature on candidate identity and policymaking at the local level, which has largely shown that candidate identity does not observably influence jurisdiction-wide policy outcomes. Second, these results act as a type of placebo test that suggests that the election of minority council members isn't correlated with some other broad re-alignment in local government. Finally, they provide a lens which suggests that to the extent that changes in fiscal policy underlie our results it must be through non-white candidates shifting spending away from white neighborhoods/residents and towards nonwhite neighborhoods/residents.[45]

7 Conclusion

[45] In addition to aggregate revenue and spending categories, In Table A8 we explore the impact of electing a minority council member on propensity to adopt revisions to city planning documents and in Table A9 we examine impacts on aggregate building permit activity. In both cases, we find no evidence of any impact on these city-level measures.

Our paper examines the impact 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 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 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 councilmembers and candidates. We then focus on narrow elections between white and nonwhite candidates and employ a regression discontinuity design 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/ethnicity of the candidate and that 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 the mechanisms driving these results.

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 councilmembers can have marked impacts on outcomes in minority neighborhoods.

References

Albouy, D., Christensen, P., & Sarmiento-Barbieri, I. (2018). Unlocking amenities: Estimating public-good complementarity. *National Bureau of Economic Research*, (No. w25107).

Ananat, E. O. (2011). The wrong side (s) of the tracks: The causal effects of racial segregation on urban poverty and inequality. *American Economic Journal: Applied Economics*, 3(2), 34-66.

Ananat, E. O., & Washington, E. (2009). Segregation and Black political efficacy. *Journal of Public Economics*, 93(5-6), 807-822.

Aneja, A. P., & Avenancio-Leon, C. F. (2019). The effect of political power on labor market inequality: Evidence from the 1965 Voting Rights Act. *Working Paper.*

Beach, B., & Jones, D. B. (2016). Business as usual: Politicians with business experience, government finances, and policy outcomes. *Journal of Economic Behavior & Organization*, *131*, 292-307.

Beach, B., & Jones, D. B. (2017). Gridlock: Ethnic diversity in government and the provision of public goods. *American Economic Journal: Economic Policy*, 9(1), 112-136.

Besley, T. & Coate, S. (1997). An economic model of representative democracy. *Quarterly Journal of Economics,* 112(1):85-114

Bishop, K. C., & Murphy, A. D. (2011). Estimating the willingness to pay to avoid violent crime: A dynamic approach. *American Economic Review*, 101(3), 625-29.

Bishop, K. C., & Murphy, A. D. (2018). Valuing time-varying attributes using the hedonic model: When is a dynamic approach necessary? *Review of Economics and Statistics*.

Black, S. E. (1999). Do better schools matter? Parental valuation of elementary education. *Quarterly Journal of Economics*, 114(2), 577-599.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, *82*(6), 2295-2326.

Cascio, E. U., & Washington, E. (2013). Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965. *Quarterly Journal of Economics*, 129(1), 379-433.

Caughey, D., & Sekhon, J. S. (2011). Elections and the regression discontinuity design: Lessons from close US house races, 1942–2008. *Political Analysis*, 19(4), 385-408.

Cellini, S. R., Ferreira, F., & Rothstein, J. (2010). The value of school facility investments: Evidence from a dynamic regression discontinuity design. *Quarterly Journal of Economics*, 125(1), 215-261.

Chay, K. Y., & Greenstone, M. (2005). Does air quality matter? Evidence from the housing market. *Journal of Political Economy*, 113(2), 376-424.

Constante, A. (2018) *California's Latinos and Asian Americans target city councils with district elections*. NBC News https://www.nbcnews.com/news/asian-america/california-s-latinos-asian-americans-target-city-councils-district-elections-n879376 accessed 8/2/19.

Dixit, A., & Londregan, J. (1996). The determinants of success of special interests in redistributive politics. *The Journal of Politics*, 58(4), 1132-1155.

Downs, A. (1957). An economic theory of political action in a democracy. *Journal of Political Economy*, *65*(2), 135-150.

Ferreira, F., & Gyourko, J. (2009). Do political parties matter? Evidence from US cities. *Quarterly Journal of Economics*, 124(1), 399-422.

Ferreira, F., & Gyourko, J. (2014). Does gender matter for political leadership? The case of US mayors. *Journal of Public Economics*, 112, 24-39.

Florido, A. (2009). *How One Vote Changed Escondido's Immigration Equation*. Voice of San Diego.

Glaeser, E. L., Ponzetto, G. A., & Shapiro, J. M. (2005). Strategic extremism: Why Republicans and Democrats divide on religious values. *Quarterly Journal of Economics*, 120(4), 1283-1330.

Governor's Office of Planning and Research, State of California (GOPR). (2001). *A Citizen's Guide to Planning*

Grimmer, J., Hersh, E., Feinstein, B., & Carpenter, D. (2011). Are close elections random? *Unpublished manuscript*.

Grofman, B., Handley, L., & Niemi, R. G. (1992). Minority representation and the quest for voting equality. Cambridge University Press.

Grose, C. R. (2005). Disentangling constituency and legislator effects in legislative representation: Black legislators or black districts? *Social Science Quarterly*, 86(2), 427-443.

Hajnal, Z., & Trounstine, J. (2014a). Identifying and understanding perceived inequities in local politics. Political Research Quarterly, 67(1), 56-70.

Hajnal, Z., & Trounstine, J. (2014b). What underlies urban politics? Race, class, ideology, partisanship, and the urban vote. Urban Affairs Review, 50(1), 63-99.

Hopkins, D. J., & McCabe, K. T. (2012). After it's too late: estimating the policy impacts of black mayoralties in US Cities. *American Politics Research*, 40(4), 665-700.

Hotelling, H. (1929). Stability in competition, *The Economic Journal, 39*(153), 41-57.

Imbens, G., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *Review of Economic Studies*, *79*(3), 933-959.

Jenkins, L. (2008). *A Chance to Make History in Escondido.* San Diego Union-Tribune.

Kumamoto, A. & Smith, D. (2019) *Sacramento NAACP president running for City Council, says she wants promises kept to Meadowview.* The Sacramento Bee.

La Ferrara, E., & Mele, A. (2006). Racial segregation and public school expenditure. *Working paper.*

Lau, A. (2008). *Diaz Prevails to Become 1st Latino Councilmember.* San Diego Union-Tribune.

Linden, L., & Rockoff, J. E. (2008). Estimates of the impact of crime risk on property values from Megan's laws. *American Economic Review*, *98*(3), 1103.

Logan, T. D. (2018). Do black politicians matter? *National Bureau of Economic Research*, (No. w24190).

McCoy, M. (2004). 9 Council Candidates Debate Traffic, Housing. *The Press Democrat (Santa Rosa, CA)*.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, *142*(2), 698-714.

Mahadevan, M. (2019). The price of power: Costs of political corruption in Indian electricity. *Working paper*

Nye, J. V., Rainer, I., & Stratmann, T. (2014). Do black mayors improve black relative to white employment outcomes? Evidence from large US cities. *The Journal of Law, Economics, & Organization*, *31*(2), 383-430.

Oates, W. E. (1969). The effects of property taxes and local public spending on property values: An empirical study of tax capitalization and the Tiebout hypothesis. *Journal of Political Economy*, *77*(6), 957-971.

Osborne, M. J., & Slivinski, A. (1996). A model of political competition with citizen-candidates. *Quarterly Journal of Economics*, *111*(1), 65-96.

Pande, R. (2003). Can mandated political representation increase policy influence for disadvantaged minorities? Theory and evidence from India. *American Economic Review*, *93*(4), 1132-1151.

Pierce, L. (2018). Lee Pierce Candidate Statement District 2. *City of Santa Rosa*. https://srcity.org/160/Lee-Pierce-Candidate-Statement-District-. Accessed 12 July 2019.

Polinsky, A. Mitchell, and Steven Shavell. 1976. "Amenities and property values in a model of an urban area." *Journal of Public Economics*, 5.1-2: 119-129.

Rose, T. (2004). Challenger Unsettles Clayton Race – Council Incumbents Forced to Defend Spots Against Call for New Ideas. *Contra Costa Times (Walnut Creek, CA).*

Rosen, Sherwin. 1974. "Hedonic prices and implicit markets: product differentiation in pure competition." *Journal of Political Economy,* 82 (1): 34-55.

Sances, M. W., & You, H. Y. (2017). Who pays for government? Descriptive representation and exploitative revenue sources. *The Journal of Politics*, *79*(3), 1090-1094.

Sieg, Holger, V. Kerry Smith, H. Spencer Banzhaf, and Randy Walsh. (2002) "Interjurisdictional housing prices in locational equilibrium." *Journal of Urban Economics* 52(1): 131-153.

Sumner, J. L., Farris, E. M., & Holman, M. R.. (2019). “Crowdsourcing reliable local data.” *Political Analysis*, forthcoming.

Trounstine, J. (2016). Segregation and inequality in public goods. *American Journal of Political Science*, 60(3), 709-725.

Turner, M. A., Haughwout, A., & Van Der Klaauw, W. (2014). Land use regulation and welfare. *Econometrica*, *82*(4), 1341-1403.

Vogl, T. S. (2014). Race and the politics of close elections. *Journal of Public Economics*, *109*, 101-113.

Washington, E. (2012). Do majority-black districts limit blacks’ representation? The case of the 1990 redistricting. *Journal of Law and Economics*, *55*(2), 251-274.

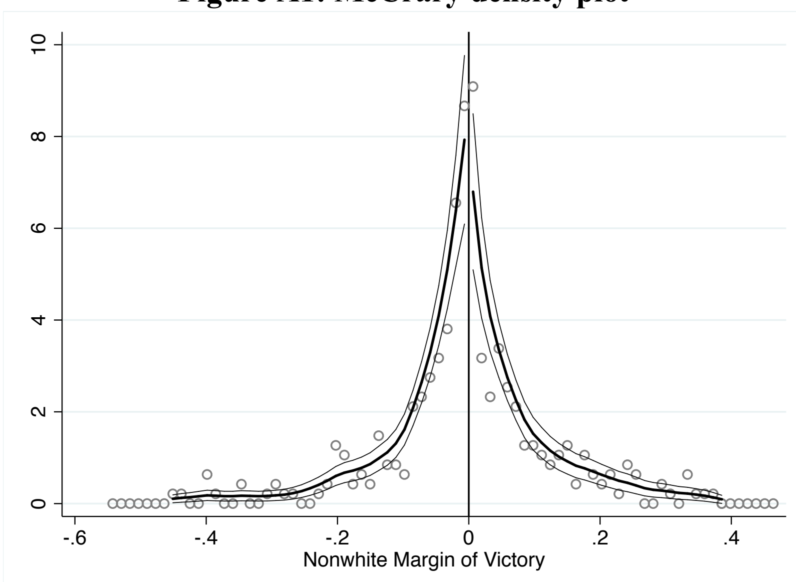
Yut, S. (1995). Using candidate race to define minority-preferred candidates under Section 2 of the Voting Rights Act. *University of Chicago Legal Forum,* (Vol. 1995, No. 1, p. 22).

Online Appendix for "Minority Representation in Local Government" by Brian Beach, Daniel Jones, Tate Twinam, and Randall Walsh

1. Assessing the validity of our regression discontinuity design

Figure A1 examines whether, in close elections, nonwhite candidates have a distinct advantage or disadvantage. If true, then we should be hesitant to interpret the outcome of a close election as random. The results of Figure A1 suggest that, near the cutoff value, the number of nonwhite winners is statistically indistinguishable from the number of white winners. This suggests that neither group has a distinct advantage/disadvantage.

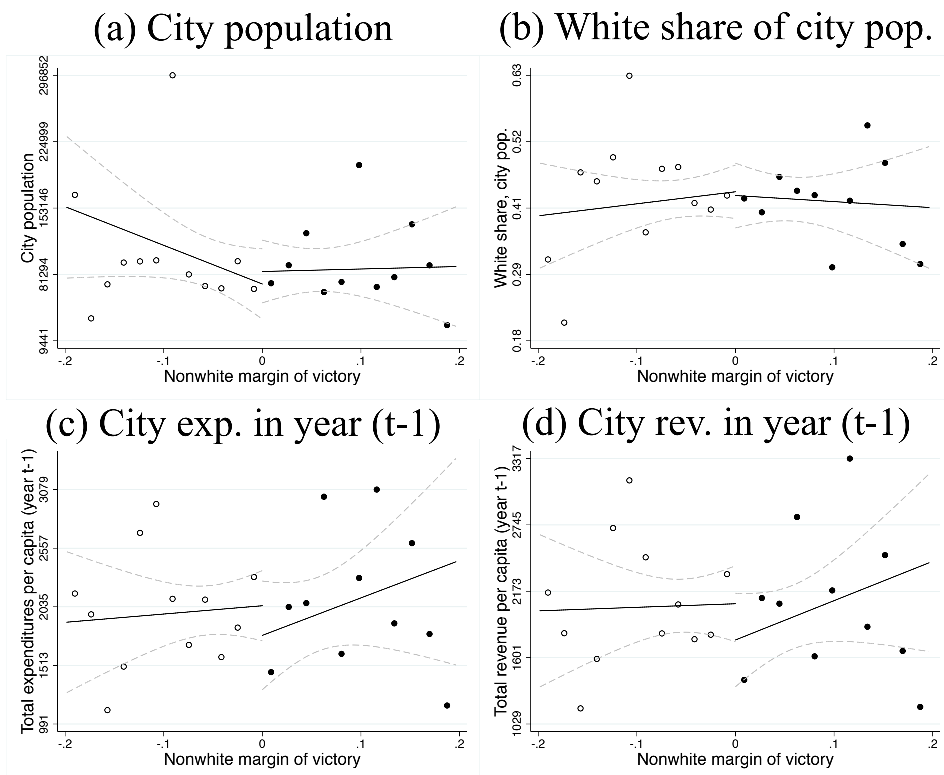
Figure A1: McCrary density plot



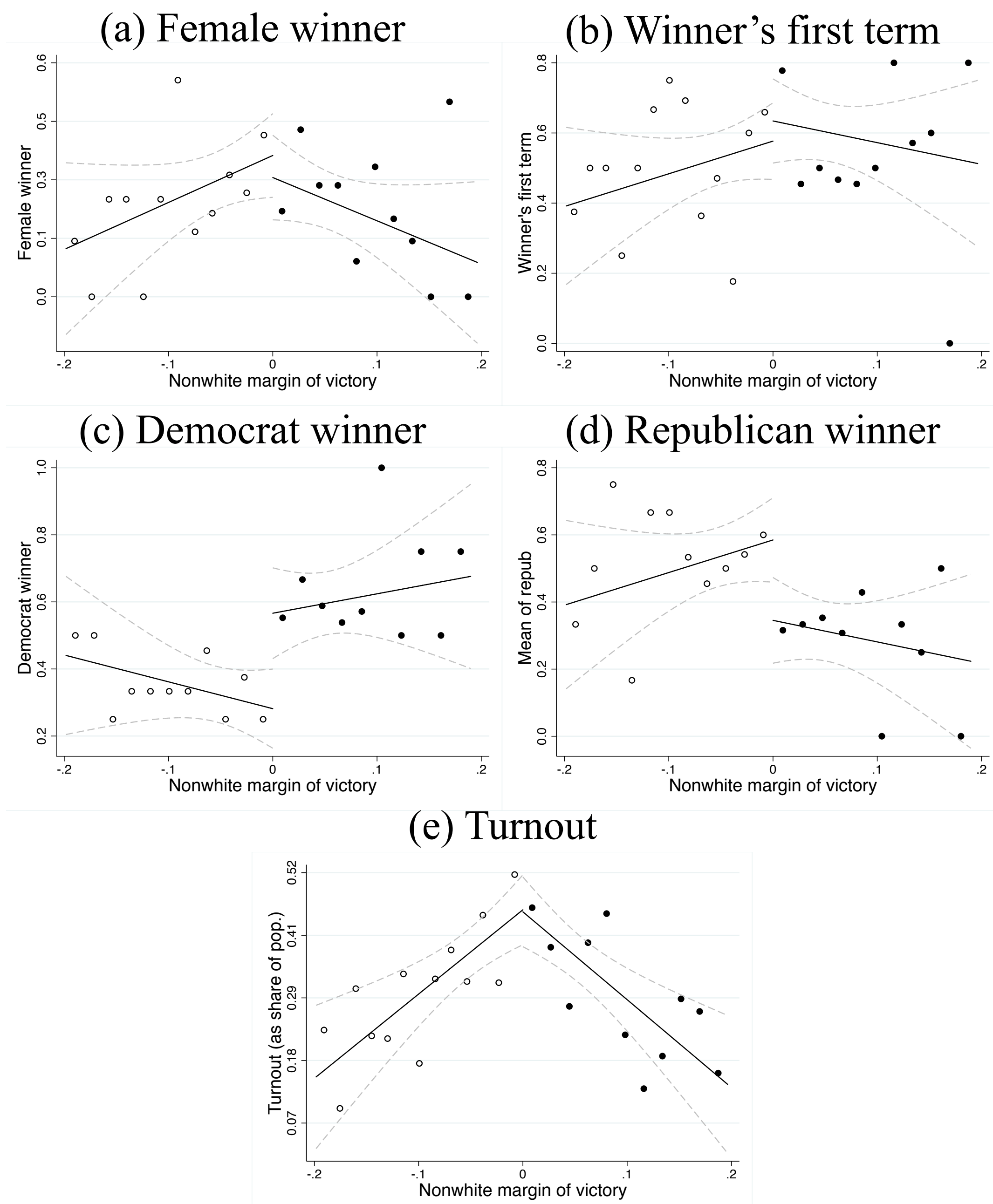
Note: The figure depicts the distribution of relevant elections around the cutoff that determines whether a white or nonwhite candidate wins. The x-axis measures the nonwhite candidate's margin of victory (nonwhite candidate vote share minus white candidate vote share).

Figures A2, A3, and A4 examine whether other city-level characteristics (A2), candidate characteristics (A3), and housing characteristics (A4) are balanced at the cutoff. The only variable where we can reject equality at the cutoff is partisan affiliation: nonwhite candidates are more likely to be registered Democrats (and less likely to be registered Republicans). This finding undermines about ability to disentangle whether our results are being driven by race or by partisan affiliation. However, in a robustness check, we can help shut down this channel by restricting attention to close elections where both marginal candidates belong to the same party. When we do that, we find very similar results, suggesting that our results are being driven by race rather than partisan affiliation.

Appendix Figure A2: Covariate balance tests: City characteristics

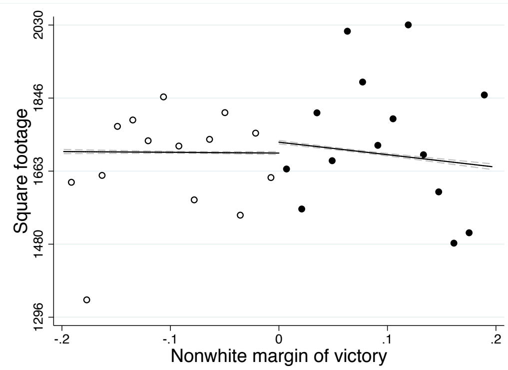


Appendix Figure A3: Covariate balance tests: Candidate characteristics

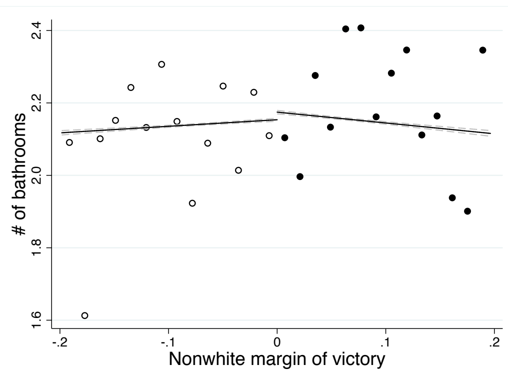


Appendix Figure A4: Covariate balance tests: Housing characteristics

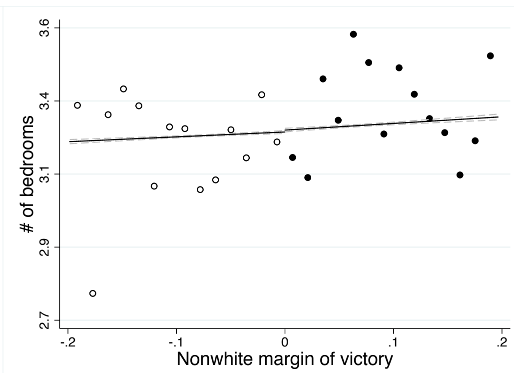
(a) Square footage



(b) # of bathrooms



(c) # of bedrooms



2. Cross-sectional RD results

2.1 Main results

Table A1 presents the results of our cross-sectional regression discontinuity approach, with Columns 1 and 2 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 reveals that housing prices increase by an imprecisely estimated 1.9 percent in cities where the nonwhite candidate was elected. However, this estimate masks substantial heterogeneity. Turning to Column 2 – where we allow for differential effects by neighborhood type – we see that, relative to cities where a white candidate narrowly wins, the election of a nonwhite candidate is associated with a 6.7 percentage point reduction in housing values in majority white neighborhoods (based on the "Nonwht. winner" coefficient). Conversely, nonwhite winners have a significantly positive *differential* effect on housing values in majority nonwhite neighborhoods (based on the "Nonwht. winner X Maj. Nonwht. Neigh." coefficient). Moreover, this is a positive effect on houses in nonwhite neighborhoods overall: the linear combination of the two coefficients suggests that nonwhite neighborhood housing values are roughly 6.7 percent higher after a nonwhite candidate wins.

Table A1: Cross-sectional RD estimates of councilmember ethnicity on housing values

	(1)	(2)
Nonwhite winner	0.019	-0.067*
	(0.023)	(0.037)
Nonwhite winner X Nonwhite Neighborhood		0.134***
		(0.048)
Linear combination to recover full effect on Nonwhite Neighborhood		
Nonwhite winner + (Nonwhite winner X Nonwhite Neighborhood)		*0.067***
		(0.027)
Observations	308,360	308,360

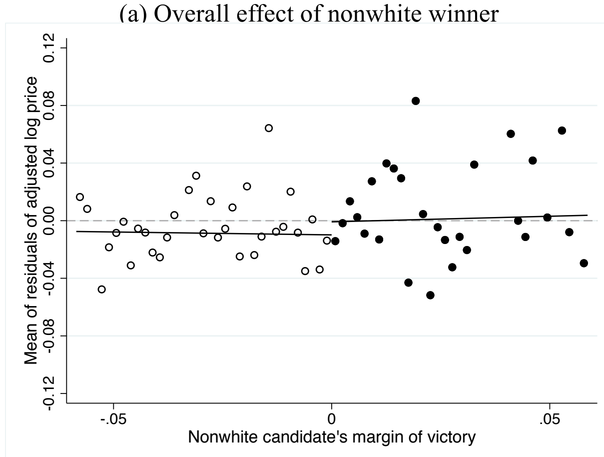
The outcome variable is an adjusted log house price, with house characteristics, neighborhood characteristics, and city trends controlled for by residualizing those characteristics out in the full sample, as described in text. All specifications are restricted to elections between white and nonwhite candidates that were decided within a 5.88 percentage point margin. Table displays coefficient capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory). Observations correspond to housing transactions that take place up to two years after the relevant election takes place. "Nonwhite Neighborhood" is a dummy equal to 1 if the neighborhood is at least 50% nonwhite.

Robust standard errors (clustered at city-level) in parentheses
*** p<0.01, ** p<0.05, * p<0.1

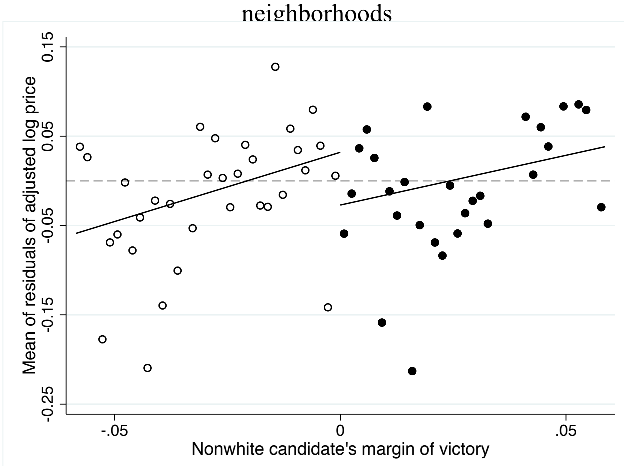
Figure A5 presents the same pattern of results graphically. Each panel is a binned scatterplot, where we plot average housing values (within bins in nonwhite margin) and fit two lines through the resulting averages (one for negative nonwhite margins, implying that the nonwhite candidate lost, and one for positive nonwhite margins, implying that the nonwhite candidate won). Panel (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 to narrow nonwhite wins. Panel (b) restricts the estimation sample to majority white neighborhoods. 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, Panel (c), which restricts the estimation sample to majority nonwhite neighborhoods, suggests a positive effect of nonwhite victory on housing values.[1]

Figure A5: Effect of a nonwhite candidate victory on housing prices, cross-sectional comparison (

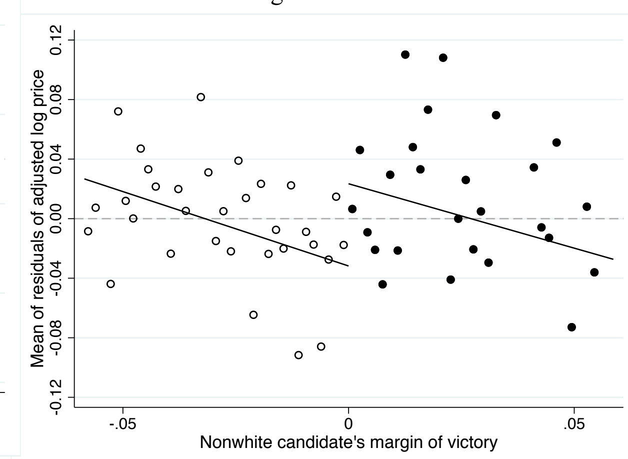
(a) Overall effect of nonwhite winner



(b) Effect of nonwhite winner on majority white neighborhoods



(c) Effect of nonwhite winner on majority nonwhite neighborhoods



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 neighborhoods, while Panel (c) restricts the sample to majority nonwhite neighborhoods. The y-axis variable is the residual variation remaining in the log adjusted housing price after controlling for housing characteristics, neighborhood characteristics, and city-specific time trends. 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.

[1] Note that Panel (c) is not directly comparable to the "Nonwhite winner X Nonwhite. Neighborhood" coefficient from Table A1, Column 2. That coefficient captures the differential effect of a nonwhite win on nonwhite neighborhoods (relative to the effect on white neighborhoods). The Figure captures the overall effect of a nonwhite win on nonwhite neighborhoods. Figure 3(c) is therefore comparable to the linear combination "Nonwhite winner" + "Nonwhite winner X Nonwhite Neighborhood" reported at the bottom of Table A1.

Like the panel-based regression discontinuity results that follow, the cross-sectional RD results are robust to adopting wider and narrower bandwidths. The basic pattern of results are also quite similar if we collapse the data down to block group-level averages, and assess the data at the block group-level, rather than the housing unit level. These results are available upon request.

3. Additional panel RD results

Table A2 collapses our data to the neighborhood (block group)-by-council term level. That is, rather than one observation per housing transaction, we have two observations per treated block group, one in the pre-election period and one in the post-election period. We do so to demonstrate that our results, and the precision of our estimates, are not artifacts of taking housing transactions as the unit of observation. Specifically, we take the averages of adjusted log prices and all of our controls at the neighborhood-by-pre/post period level. We then run our basic RD specification on the collapsed dataset, with average prices as the outcome and average housing and neighborhood characteristics as controls. The RD-relevant variables remain the same as in our main specifications, as those variables are constant within a city. Column 2 differs from Column 1 in that there we weight each observation by the number of transactions that occurred within that neighborhood-time period pairing. This is for comparison with the main results, which are implicitly weighted by the number of transactions. Finally, one key difference between these specifications and our main specification in the text, beyond the level of aggregation, is that – with only two periods – we cannot include city-specific trends in these specifications. These specifications are, therefore, most directly comparable to Column 3 of Table 3. In Columns 1 and 2 we confirm our main finding: there is a positive differential effect on housing prices in majority nonwhite neighborhoods. The magnitude of the results differ across the two specifications. The estimates in Column 2, not surprisingly, are very close to those reported in Column 3, Table 3 in the main text. Column 1 differs, as each block group receives equal weight. There, the positive differential effect on nonwhite neighborhoods is smaller, but there is *no* negative effect on white neighborhoods. Thus, the overall effect on nonwhite neighborhoods is similar to and in fact somewhat larger than in Column 2.

Table A3 takes number of transactions as the outcome variable to help rule out that our results are picking up some underlying neighborhood turnover.

Table A2: Panel-based RD, aggregated to block-level

	(1)	(2)
Nonwhite winner	0.004	-0.067
	(0.042)	(0.044)
Nonwhite winner X	0.093*	0.153***
Nonwhite Neighborhood	(0.049)	(0.056)
Linear combination to recover full effect on Nonwhite Neighborhood		
Nonwhite winner + (Nonwhite winner X Nonwhite Neighborhood)	0.097**	0.086*
Outcome	Nbd-by-time period level average of adjusted log house price	
Add'l controls	Neighborhood-averages of housing characteristics, neighborhood characteristics	
Weighting by # of transactions?	No	Yes
Observations	18,374	18,374

The outcome variable is an adjusted log house price, with house characteristics, neighborhood 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 that were decided within a 5.88 percentage point margin. Table displays coefficient capturing causal impact of nonwhite candidate victory and suppresses other coefficients (e.g., nonwhite margin of victory). Observations correspond to housing transactions that take place up to two years after the relevant election takes place. "Nonwhite Neighborhood" is a dummy equal to 1 if the neighborhood is at least 50% nonwhite.

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

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

Appendix Table A3: Impact 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 Nonwht. Neigh.	(0.226)	(0.417)	(0.027)
Level of FEs	Election	Election	Election
Neighborhood controls	X	X	X
Observations	370,259	370,130	370,259

The sample consists of neighborhood-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 neighborhood population. In Column 3, the outcome variable is an indicator variable equal to one if any transactions occurred in that neighborhood-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

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

4. Race/Ethnicity-Specific Results

This section reports race/ethnic-group specific effects. Figure A6 documents the simplest departure from our main analysis. There, we assess how Black, Hispanic, and East Asian/Pac. Islander differentially impact majority nonwhite neighborhoods. The far-left panel reproduces our main result for the sake of comparison. In the remaining panels, we report coefficients from a slightly modified specification. Specifically, we replace 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 neighborhoods ("[group] win", represented by the White bar) and the differential impacts of each type of candidate on majority nonwhite neighborhoods ("[group] win X Nonwht. BG", gray bar).

Figure A6: Estimated impacts of winners from specific race/ethnic groups on White and nonwhite neighborhoods

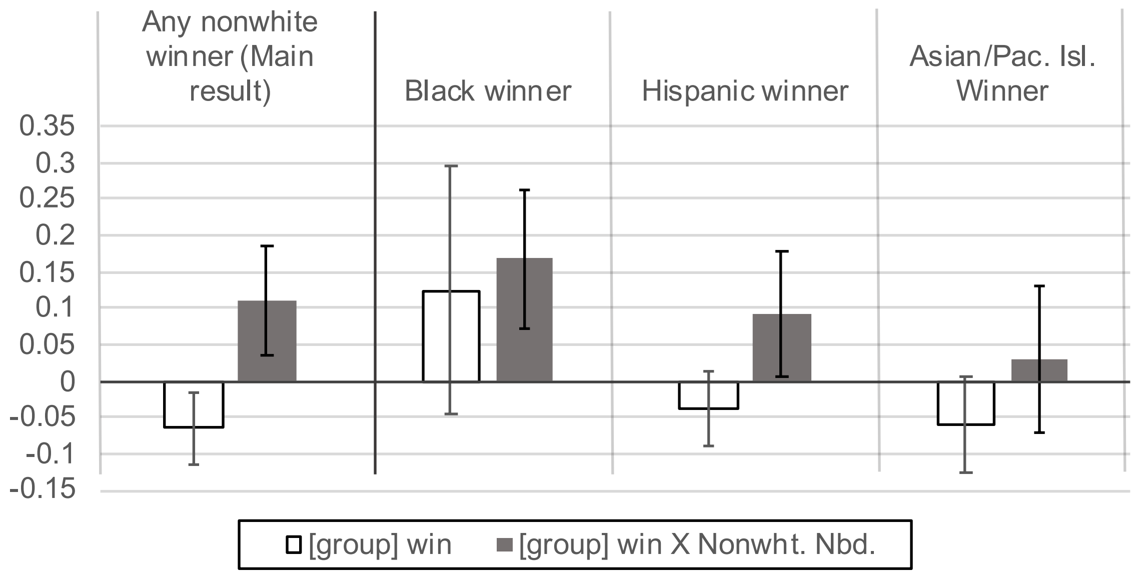


Figure notes: This figure reports coefficients from two regressions. The far-left panel ("Any nonwhite winner") reproduces our main result for the sake of comparison. The remaining three panels ("Black winner", etc.) report results from a specification that modifies our main specification. We replace 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.

Notably, the impact of Hispanic candidate victories are very similar to our main result. This makes sense since 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 neighborhoods, despite the much smaller number of Black candidates in our sample. The point estimate for the effect of black candidates on white neighborhoods is also positive, unlike our main result, but the confidence interval is extremely large. Finally, the estimated impacts of Asian/Pacific Islander candidates are sufficiently imprecise that we hesitate to attempt to interpret them. This too is not 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 neighborhoods – 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.

It is also worth considering how different types of candidates differentially impact neighborhoods heavily populated by residents from their own race/ethnic group. Tables A4-A6 aim to do just that. Table A4 restricts the sample to elections between White candidates and Hispanic candidates, while A5 and A6 make similar restrictions for White vs Black and White vs Asian elections, respectively. 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 neighborhood. For instance, in Column 1 of Table A4, we interact "Hispanic winner" with a continuous measure of the share of the population in the relevant neighborhood that is Hispanic. In Column 2 we interact "Hispanic winner" with a dummy indicating that Hispanics are the modal group within the relevant neighborhood.[2] We do the same for Black and Asian in Columns 1 and 2 of Tables A5 and A6. For all three tables, Columns 3 and 4 test how candidates impact neighborhoods that represent their own group, but also how they impact other neighborhoods 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 neighborhood", we also include interact "[group] winner" with "other nonwhite groups' share of population in the neighborhood". For example, in Table A4, "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 neighborhood is Hispanic, Asian, or Black, regardless of the race of the candidate.

---

[2] 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 neighborhoods that are majority Hispanic, Black, or Asian is extremely small. For instance, less than 1% of observations in our sample are in majority Black neighborhoods. The less restrictive "modal" requirement increases the number of neighborhoods that we can identify differential effects from.

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 A5, once we focus only on elections between Black and White candidates, we are identifying effects from just 32 unique elections. Similarly, there are not many neighborhoods 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. Despite the loss in precision, 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 – nonwhite candidates from particular groups tend to have an impact on *all* types of nonwhite neighborhoods. This helps to justify our simpler focus on just nonwhite vs. white neighborhoods in the main analysis.

Table A4: Restricting to White vs Hispanic elections

VARIABLES	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price
Hisp. Win X Post	-0.004	0.012	-0.094	0.003
	(0.055)	(0.035)	(0.077)	(0.040)
Hisp. Win X Post X Share Hisp.	0.076		0.136	
	(0.117)		(0.123)	
Hisp. Win X Post X (Modal Neigh. Group = Hisp.)		0.030		0.039
		(0.050)		(0.054)
Hisp. Win X Post X Share other nonwht.			0.363**	
			(0.141)	
Hisp. Win X Post X (Modal Neigh. Group = Asian)				0.061
				(0.057)
Hisp. Win X Post X (Modal Neigh. Group = Black)				0.752***
				(0.178)
Observations	364,700			
Unique elections	117			

General notes: The outcome variable is an adjusted log house price, with house characteristics, neighborhood 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 neighborhood population that is Hispanic for a given a housing transaction. "Share other nonwht." captures the share of the local neighborhood population that is nonwhite and non-Hispanic. "Modal Neigh. Group = X" equals one if the largest group in the local neighborhood is X.

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

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

Table A5: Restricting to White vs Black elections

VARIABLES	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price
Black Win X Post	0.036	0.087	-0.107	0.055
	(0.070)	(0.069)	(0.094)	(0.063)
Black Win X Post X Share Black	0.173		0.807	
	(0.547)		(0.743)	
Black Win X Post		0.049		0.058
X (Modal Neigh. Group = Black)		(0.033)		(0.281)
Black Win X Post			0.210	
X Share other nonwht.			(0.255)	
Black Win X Post				0.224***
X (Modal Neigh. Group = Hisp.)				(0.064)
Black Win X Post				0.028
X (Modal Neigh. Group = Asian)				(0.065)
Observations		140,341		
Unique elections		32		

General notes: See Table A4 notes. Table-specific notes: All specification restricted to elections between Black and Hispanic candidates. “Share Black” captures the share of the local neighborhood population that is Black for a given a housing transaction. “Share other nonwht.” captures the share of the local neighborhood population that is nonwhite and non-Black. “Modal Neigh. Group = X” equals one if the largest group in the local neighborhood is X.

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

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

Table A6: Restricting to White vs Asian elections

VARIABLES	(1) Adjusted Log Price	(2) Adjusted Log Price	(3) Adjusted Log Price	(4) Adjusted Log Price
Asian Win X Post	-0.080*	-0.043	0.036	-0.010
	(0.040)	(0.029)	(0.111)	(0.039)
Asian Win X Post X Share Asian	0.308**		0.225	
	(0.127)		(0.171)	
Asian Win X Post		0.170***		0.138**
X (Modal Neigh. Group = Asian)		(0.051)		(0.065)
Asian Win X Post			-0.311	
X Share other nonwht.			(0.229)	
Asian Win X Post				-0.095
X (Modal Neigh. Group = Hisp.)				(0.095)
Asian Win X Post				*omitted*
X (Modal Neigh. Group = Black)				(-)
Observations		66,645		
Unique elections		38		

General notes: See Table A4 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 neighborhood population that is Asian for a given a housing transaction. “Share other nonwht.” captures the share of the local neighborhood population that is nonwhite and non-Asian. “Modal Neigh. Group = X” equals one if the largest group in the local neighborhood is X. Note that in Column 4 “Asian Win X Post X Modal Neigh. Group =Black” is omitted. There are no observations of neighborhoods 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

5. Evidence from outside of the housing market

This section considers the impact of increased nonwhite representation on a variety of non-housing outcomes. The main text reports results that consider a broader set of non-housing outcomes that could reasonably be expected to reflect underlying council activities/policies. In this appendix, we turn to direct measures of city government activity and policy. 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.

5.1 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 A7 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[7], public safety, transportation, or community development).

Table A8 tests 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 give no clear indication that the election of a nonwhite councilperson generates a change on this front. Next, in Appendix Table A9 we draw on building permits data to tests whether the

[7] "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.).

election of a nonwhite candidate impacts the number (or valuation) of building permits for residential properties and find no clear change.[8]

Table A7: 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 (0.043)	-0.069 (0.045)	-0.170 (0.105)	-0.032 (0.037)
	(5)	(6)	(7)	(8)
Outcome variable:	Public goods Expenditures	Public safety Expenditures	Transportation Expenditures	Community Development Expenditures
Nonwhite winner	0.007 (0.049)	-0.018 (0.029)	-0.042 (0.087)	-0.036 (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

Appendix Table A8: 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 (0.119)	0.004 (0.082)	-0.024 (0.083)	-0.047 (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 (0.071)	0.024 (0.069)	0.025 (0.067)	0.032 (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

[8] 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.

Appendix Table A9: Impact 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		

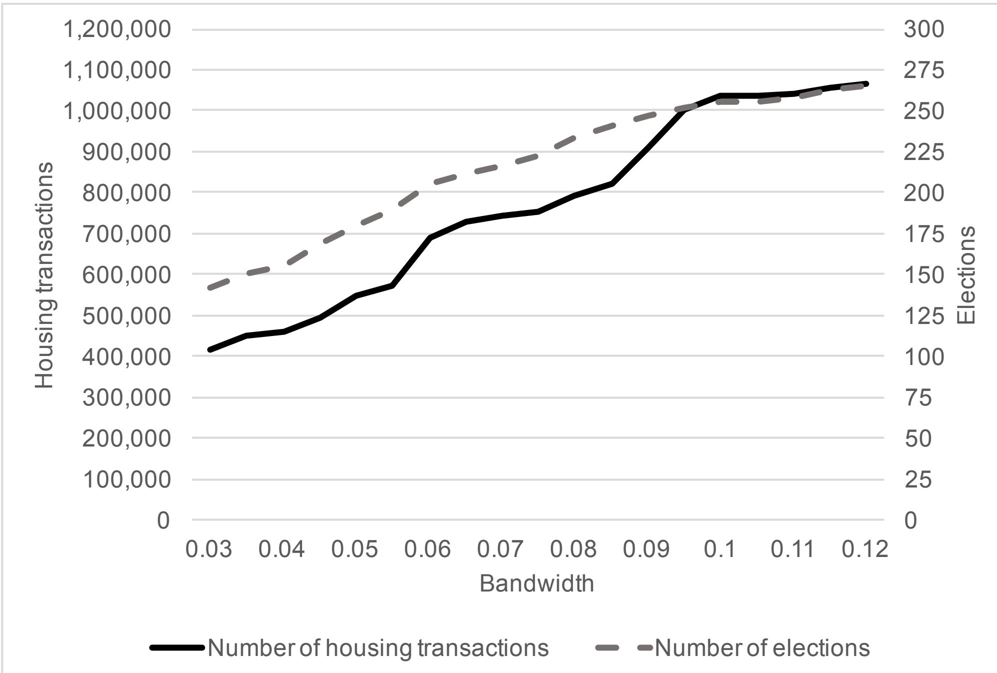
"Nonwhite winner" in the table refers to the interaction between the nonwhite winner indicator and the "post-election" indicator. That is, the reported coefficients identify the differential effect of a nonwhite winner after the election (relative to outcomes in the same city before the election). All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

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

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

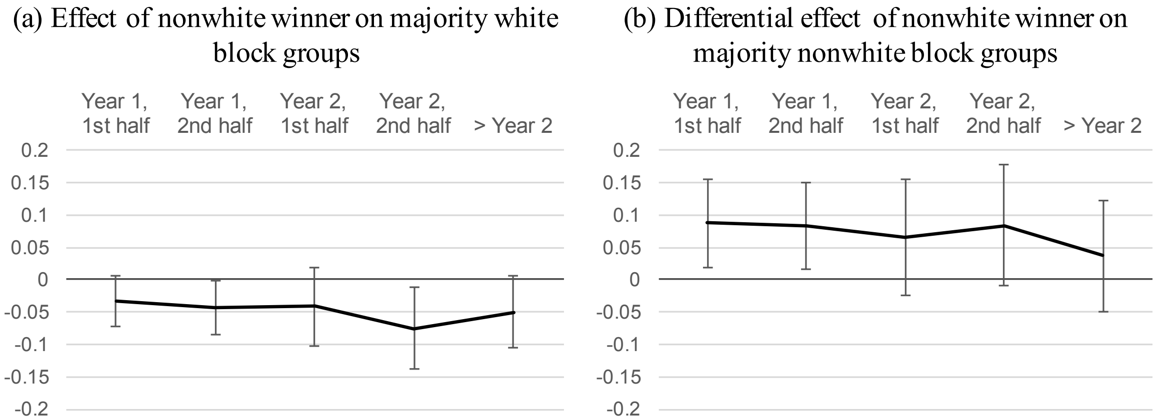
6. Other tables/figures

Figure A7: 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.

Figure A8: Panel RD: Dynamics of effects of nonwhite win



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 5). 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 time span 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, 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."