

Minority Representation in Local Government*

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Abstract: Does minority representation in a legislative body differentially impact outcomes for minorities? To examine this question, we study close elections for California city council seats 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 can reduce racial disparities. Our results strengthen with increased city-level segregation and councilmember pivotality. Regarding mechanisms, we observe changes in business patterns and policing behavior, which may help explain our results.

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

1. Introduction

The Voting Rights Act (VRA) of 1965 is one of the most important pieces of legislation in U.S. history. Its passage returned the franchise to millions of southern blacks, 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 housing¹, economic development², and policing.³ The issue of descriptive representation is also intrinsically tied to current debates around partisan gerrymandering, as reductions in partisan gerrymandering are likely to reduce the number of majority-minority districts.

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 franchise – remains an open question. 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

¹ <https://www.citylab.com/equity/2018/07/when-blacks-joined-city-government-zoning-decisions-changed/564056/>

² <https://nextcity.org/daily/entry/anaheim-city-council-vote-latino-district-at-large-california>

³ <https://www.demos.org/publication/problem-african-american-underrepresentation-city-councils>

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.

We are thus left with an empirical question. Unfortunately, there is limited systematic evidence on whether minority representation differentially affects minority outcomes.⁴ 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, city services, and/or economic development.

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); Chay & Greenstone, 2005 (environmental quality); and Turner et al., 2014 (land use regulation). Housing prices are also 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

⁴ Needless to say, there is highly relevant work, which we review in Section 2.

impact only certain areas of the city. Examples include zoning policy, road improvements, public transit, parks, crime, and economic development projects.⁵

Relative to examining the spatial dimensions of these policies directly, which in many cases is simply not possible, housing prices serve as an “index number” allowing us to assess 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).⁶ 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.

⁵ Note that elected school boards (rather than city councils) determine local school policy and spending.

⁶ 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.

Our findings complement work on the Voting Rights Act by Cascio & 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 increasing minority representation in elected office can be another important tool for addressing racial disparities. In this sense, our work is also closely related to 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. In total, our results suggest that today, well more than a century since Reconstruction and the adoption of the 14th Amendment and five decades after the passage of the Voting Rights Act, descriptive representation has important implications for the wellbeing of minority citizens.

2. Related Work

There is a large literature in economics and political science examining whether personal characteristics of elected officials (race, gender, partisan affiliation, etc.) affect policymaking.⁷ Most relevant for our work is the literature examining the impacts of electing ethnic or racial minority-group politicians. Hopkins & McCabe (2012) thoroughly review research examining the impacts of electing a black mayor and also provide new causal estimates on the matter. They conclude that “across a range of measures of taxing, spending, and hiring, we find few differences between black mayors and their white counterparts,” a finding that is broadly consistent with the prior work that they review.

⁷ Outside of race, previous empirical research has considered the impacts of partisan affiliation (e.g., Ferreira & Gyourko, 2009; de Benedictis-Kessner & Warshaw, 2016; de Benedictis-Kessner & Warshaw, 2018), gender (e.g., Ferreira & Gyourko, 2014), and professional experience (e.g., Beach & Jones, 2016) on policy outcomes at the local level, yielding mixed results.

A major limitation of this early work is its inability to speak to intra-city variation in policy – either by citizen minority status or neighborhood. 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, Hinds & Ordway’s (1986) study of representation and zoning decisions in Atlanta, 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.

Hinds & Ordway (1986) and Nye et al. (2014) are perhaps the most relevant to this paper. Hinds & Orway (1986) document a baseline inequality in zoning decisions pertaining to black and white neighborhoods, and then show that the inequality goes away when black representatives are elected. This work sheds light on the potential mechanisms underpinning our results. We view our work as complementary, in that by using house prices as the outcome variable we are able to document a similar relationship but for a much broader set of cities. Nye et al. (2014) is similar 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 issues are essential for understanding whether a causal link between minority representation and minority outcomes exists. In our data, we directly test and reject both possibilities.

Finally, 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 caution against interpreting them as causal.

A larger body of work highlights the central role that race and ethnicity play in local politics and public good provision more generally, and reinforces support for the mechanisms that we argue drive our results. For instance, Hajnal & Trounstein (2014a) show that in local elections voters are substantially divided in their candidate choices along racial lines, more so than along other dimensions (e.g., class). The division they document is particularly strong when candidates are from different racial or ethnic groups, suggesting demand for descriptive representation. Two other papers strengthen the potential link between local service provision and representation. In survey data, Hajnal & Trounstein (2014b) document large racial disparities in satisfaction with local public good provision, with black residents reporting lower satisfaction than white respondents, while Marschall & Ruhil (2007) document that black respondents' satisfaction is higher in cities with a black mayor, even controlling – to the extent possible – for objective measures of service provision. This last result reinforces that part of the distributional effect of minority representation may come through micro-level changes, e.g., more attention to street cleaning in certain neighborhoods, that are difficult to detect even with rich data on cities' spending and other activities. These micro-level changes may come about through improved channels of communication between minority residents and their representatives in the council (Mansbridge, 1999). Indeed, in the context of the US House of Representatives, Banducci et al. (2004) document that black survey respondents are more likely to report having contacted their representative recently when their representative is black. Thus, at the local level, one may posit a chain of

causality wherein minority representation leads to improved communication between minority communities and local officials, and this, in turn, helps address gaps in public good and service provision in those communities.

3. Conceptual Framework

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 by characterizing the link between policies that differentially affect members of one group and 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 ($subgroup = k \in K$).⁸ One approach would be a “group-targeting” strategy that directs resources to policies which differentially benefit individuals 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

⁸ In the simple model presented here, we abstract from budgeting issues. The council could direct public goods towards a subgroup either by reallocating resources from a fixed budget or by increasing total expenditures. The channels delineated in our model will operate in either case. However, it is worth noting that California cities are relatively constrained in their ability to generate new revenue because of Proposition 13.

“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 $\{G^j | j \in J\}$. 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:

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

where Y_i^k represents the marginal buyer’s income, ξ^{jk} is 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.⁹

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

⁹ For further exposition on this basic modeling approach see Polinsky & Shavel (1976), Rosen (1974) and Sieg et.al. (2002). We ignore property taxes for simplicity, though it would be straightforward to incorporate taxes into the model.

purchased by individuals of group k will exactly measure individual willingness to pay for increases in the group-specific public good. And, to reiterate, when the marginal buyer is not a member of the targeted group and therefore does not value the increased public good level we would expect no change in price.¹⁰ While non-marginal changes do not allow for as simple an interpretation, the general implications are similar. This basic analysis 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 do not observe the group membership of individual home purchasers. However, if as is often the case, neighborhoods are segregated by group-type then neighborhood-level price changes will capture the benefits associated with group-specific policies.

The comparative statics for neighborhood targeted policies are similar, except now the marginal household is characterized by 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 style 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. It is natural to wonder whether it is appropriate to

¹⁰ This result follows directly from the open city assumption, see Polinsky & Shavel (1976). If 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.

associate increased housing prices with increased welfare in cases where neighborhoods are comprised mainly of renters. Indeed, the majority non-white neighborhoods in our sample have an average renter share of 0.48 compared to 0.31 in majority white neighborhoods. 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 a 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 policies that differentially benefit minority citizens) or in terms of actual benefits (i.e. leading to the complete leakage of potential benefits). Further, we observe no change in the volume of housing transactions or rate of evictions following the election of a nonwhite councilmember, as discussed below. Our results also hold after imposing sample restrictions on the share of rental units 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.

4. Empirical Context

For our analysis of descriptive representation, we adopt an empirical approach that leverages narrowly decided elections between white and nonwhite candidates 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 many 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 composition. An additional benefit of this context is that California state law provides a number of guidelines for the structure of municipal governments, which limits variation in government structure, and in turn facilitates comparisons across councils without substantial variation in institutional features. A city that adopts these default guidelines would have a council with five councilmembers, each councilmember would serve staggered four-year terms, with elections filling multiple seats every two years, and the councilmembers would be elected “at large” during a general municipal election. For instance, there may an election in 2004 to fill 3 of the 5 seats. If 4 candidates ran for office, then the candidates with the 3 highest vote shares would be elected, they would serve a 4 year term and face reelection in 2008. There would then be an election in 2006 to decide on the remaining two seats.

Many municipalities conform to these guidelines. 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. Larger cities tend to deviate from these guidelines, either by having more councilmembers or by tying a council seat to a district within the city. Our analysis is robust to the inclusion/exclusion of large cities, cities with many council seats, and cities with district-based elections.

While local governments have considerable discretion in providing and reallocating public goods, it is perhaps useful to be specific about what that process looks like in California.¹¹ In California, elected school boards, rather than city councils, control local school policy. Proposition 13 also imposes substantial restrictions on property tax growth, often requiring new spending to be offset by reductions elsewhere. Nevertheless, councils supervise the administration of many local goods and services, and 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 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 jurisdiction's 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

¹¹ Piliawsky (1985) offers an interesting narrative account of how Ernest Morial – the first black mayor elected in New Orleans, affected black communities.

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 difference 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.¹² Policing and treatment of immigrants was also a focus in many campaigns. For 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, and the police department 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

¹² See Constante (2018).

platform focuses on strengthening the city's Community Police Review Commission and targeting newly available tax revenues to job training and minority business startups.¹³

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

5. 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 in turn.

5.1 Election Outcomes and Candidate Characteristics

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. Since these elections fill multiple seats on the council, the narrow winner is the candidate with the lowest number of votes that was successfully elected to the council while the narrow loser is the candidate with the next highest number of votes.

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

¹³ See Kumamoto and Smith (2019).

requires city council elections to be non-partisan, 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.

For minority status, we rely on Beach & Jones (2017), who construct 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 during this time period and lost narrowly. 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.¹⁴

We identify individual candidate's partisan affiliations by linking 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.

5.2 Neighborhood Characteristics

We use 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

¹⁴ 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”. Beach & Jones (2017) drew on additional sources to validate their data collection as well.

block groups. We use 2000 Census data, as opposed to – for instance – 2010 American 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.¹⁵

We classify block groups as *majority white* if the white population share is greater than 0.5. Conversely, *majority nonwhite* block groups are those with a white population share less than 0.5.¹⁶ 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.

5.3 Housing Prices and Characteristics

We obtain information on housing prices from transaction-level data provided by DataQuick Information Systems under a license agreement. This dataset includes the universe of single-family

¹⁵ Fractionalization is a standard index for measuring diversity and is calculated as: $Fractionalization_{bg,2000} = 1 - \sum_e (share_{bg,2000,e})^2$ where $share_{bg,2000,e}$ is the share of the population in block group bg during the year 2000 that is of ethnicity e .

¹⁶ 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 $[(\text{sum of single-race nonwhite population})/(\text{sum of single-race population})] > 0.5$. Results are essentially unchanged if we instead include multi-racial counts.

home sales in California between 2005 and 2011. The transaction records are matched with assessor records to identify: 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.

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.

5.4 Summary Statistics

Our main analysis employs a regression discontinuity (RD) design, and so we only use a subset of the data that we have gathered. The goal of the RD approach is to generate quasi-random assignment to treatment (election of a nonwhite councilmember) or counterfactual (election of a white councilmember). To achieve this, 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), 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 decided by 5.88 percentage points or less. Again, these elections fill multiple seats, and so this 5.88 percentage point difference corresponds to the difference between the two marginal candidates.¹⁷ This optimal bandwidth was chosen following Calonico et al. (2014). See section 6.2 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 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 present additional data 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.

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.

¹⁷ The marginal winner is the candidate who among those that were elected had the lowest vote share, while the marginal loser is the candidate who among those that were not elected had the highest vote share. If there were 3 open seats and 5 candidates, the marginal winner would be the candidate with the third highest vote share and the marginal loser would have the fourth highest vote share. The election would only be relevant if the difference between those vote shares was less than 5.88 percentage points and if one of those candidates was white while the other candidate was nonwhite.

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 rule out the possibility that our results are solely driven by Hispanic candidates.

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 (185,534.5)	72,665.14 (125,242.2)
Asian/Pac. Isl. share	0.09 (0.11)	0.15 (0.15)
Black share	0.04 (0.06)	0.05 (0.06)
Hispanic share:	0.30 (0.25)	0.37 (0.23)
White share:	0.56 (0.26)	0.42 (0.21)
Other share:	0.00 (0.00)	0.00 (0.00)
Ethnic fractionalization	0.44 (0.17)	0.54 (0.13)
Panel B: Council Characteristics		
Council size	5.32 (0.97)	5.38 (0.91)
District-based elect.	0.09 (0.29)	0.07 (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.

Since our goal is to examine differential changes in housing prices, it is useful to establish the existence of baseline differences in prices across majority white and majority nonwhite neighborhoods. Appendix Table A10 reports regression results building up from a bivariate regression of adjusted log price on an indicator for whether the house is in a majority nonwhite neighborhood. There we see that house prices are lower, on average, in majority nonwhite neighborhoods. The difference persists with the full set of controls and fixed effects included in our main analysis: in Column 5, we observe that houses in nonwhite neighborhoods sell for roughly 5.5 percent less than houses in majority white neighborhoods, potentially reflecting local government underinvestment in neighborhoods of color. Thus, in assessing the differential impact of candidate ethnicity on house prices in nonwhite neighborhoods, we are essentially testing whether minority representation reduces this pre-existing gap in house prices across neighborhood types.

6 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.

6.1 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:

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

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 of the relevant variables (nonwhite win, margin of victory, the interaction of nonwhite win and margin) with an indicator variable set equal to one if the house is located in a majority nonwhite neighborhood. The modified specification is then:

$$\ln(p)_{hct} = \alpha + \beta_1 \mathbf{1}[NW\ wins_{ct}] + \beta_2 margin_{ct} + \beta_3 \mathbf{1}[NW\ wins_{ct}] * margin_{ct} + \beta_4 \mathbf{1}[NW\ wins_{ct}] * \mathbf{1}[NW\ Neigh_h] + \beta_5 margin_{ct} * \mathbf{1}[NW\ Neigh_h] + \beta_6 \mathbf{1}[NW\ wins_{ct}] * margin_{ct} * \mathbf{1}[NW\ Neigh_h] + \beta_7 \mathbf{1}[NW\ Neigh_h] + \varepsilon_{hct} \quad (3)$$

where $\mathbf{1}[NW\ Neigh_h]$ 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.

While equation (3) adequately identifies 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, our main analysis uses a panel-based parallel to Equations 2 and 3, which we describe next. We do, however, report estimates from the simpler cross-sectional in the next section. Both models yield similar results.

In the panel model, we restrict the sample to the two-year council terms immediately preceding and following a relevant election.¹⁸ To reflect the level of treatment, our main specifications include election fixed effects, as in Cellini et al. (2010). 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 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 specific elections, with two years of pre-election observations and two years of post-election observations. The presence of pre- and post- observations, as well as the

¹⁸ Again, “council term” refers to the two-year period between elections in which there are no changes in the composition of the council.

inclusion of election-level fixed effects, allow us to evaluate the change in house prices in cities where the nonwhite candidate won relative 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:

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

Equation (4) 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} .¹⁹

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 (3), interacts all “treatment” variables (nonwhite 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

¹⁹ 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.

identifies the *differential* effect of a nonwhite winner on housing values in nonwhite neighborhoods. As in the discussion above, the latter will be of primary interest. Finally, we include controls for housing characteristics, neighborhood characteristics, year-month dummies, and city-specific linear time trends.²⁰

6.2 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 an appropriate bandwidth, we collapse our observations to the election level. 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

²⁰ 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.

Calonico et al. (2014) bandwidth selection procedure, which suggests that the optimal bandwidth in our setting is 5.88 percentage points. Thus, in our main specification we include all marginal elections between a white and nonwhite candidate, conditional on the election being decided by 5.88 percentage points or less. 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 for our analysis and demonstrate the robustness of our results to a variety of alternative bandwidths.²¹

6.3 Assessing the Validity of our Regression Discontinuity Design

The key assumption underlying basic RD designs is the continuity of both correlate densities and outcome probabilities across the treatment threshold. As we discuss at length in Appendix 1 and summarize here, our data conform to these requirements. Turning first to continuity of outcome probabilities, a particular concern in our close election context is that nonwhite candidates may have a distinct electoral advantage (or disadvantage), which would undermine our assumption that the outcome of a close election is as good as random.²² 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, turning to continuity of correlate densities, in Appendix Figures A2, A3, and A4, we assess the identifying assumption that other

²¹ Appendix Figure A6 documents the number of unique elections and the number of unique housing transactions that occur within each bandwidth.

²² 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.

observable characteristics behave smoothly around the cutoff. Here we see that for a wide variety of city, candidate, and housing characteristics, there are no discontinuities across the threshold. There is one important exception (seen in Appendix Figure A3 panels c and d): 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. We address this concern directly below by restricting the sample to close elections where both marginal candidates are of the same party and demonstrating that this restriction does not meaningfully change our results.

Because our elections involve multiple seats, it is important to consider any changes in the composition of non-marginal winning seats. Recent work by Folke (2014) raises the issue that the election of a single candidate to a legislative body, even in a quasi-random/narrow election, may be related to broader changes in the composition of the rest of the legislative body. In Appendix Table A11 we assess this concern directly by taking the number of non-white city councilmembers on the board as the outcome variable, but otherwise running a regression discontinuity specification that matches our main estimating equation. In the resulting estimates, the coefficient attached to “non-white winner” is statistically indistinguishable from 1, suggesting that close elections are not systematically correlated with broader changes in the legislative body. With our preferred panel RD approach, the coefficient is 0.989 (p-val. < 0.01). In other words, the narrow election of a nonwhite candidate is associated with a roughly one person increase in the number of nonwhite members on the council.

6.4 Preliminary Results: Cross-sectional regression discontinuity approach

In this subsection, we present results from our simpler cross-sectional regression discontinuity approach. The next subsection will report our main results, employing a panel-based regression discontinuity approach.

Table 2: Cross-sectional RD estimates of councilmember ethnicity on housing values

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

The outcome variable is the adjusted log house price. Both specifications include controls for house characteristics, neighborhood characteristics, and city trends. 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

Columns 1 and 2 of Table 2 report the results of estimating cross-sectional equations (2) and (3) 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.

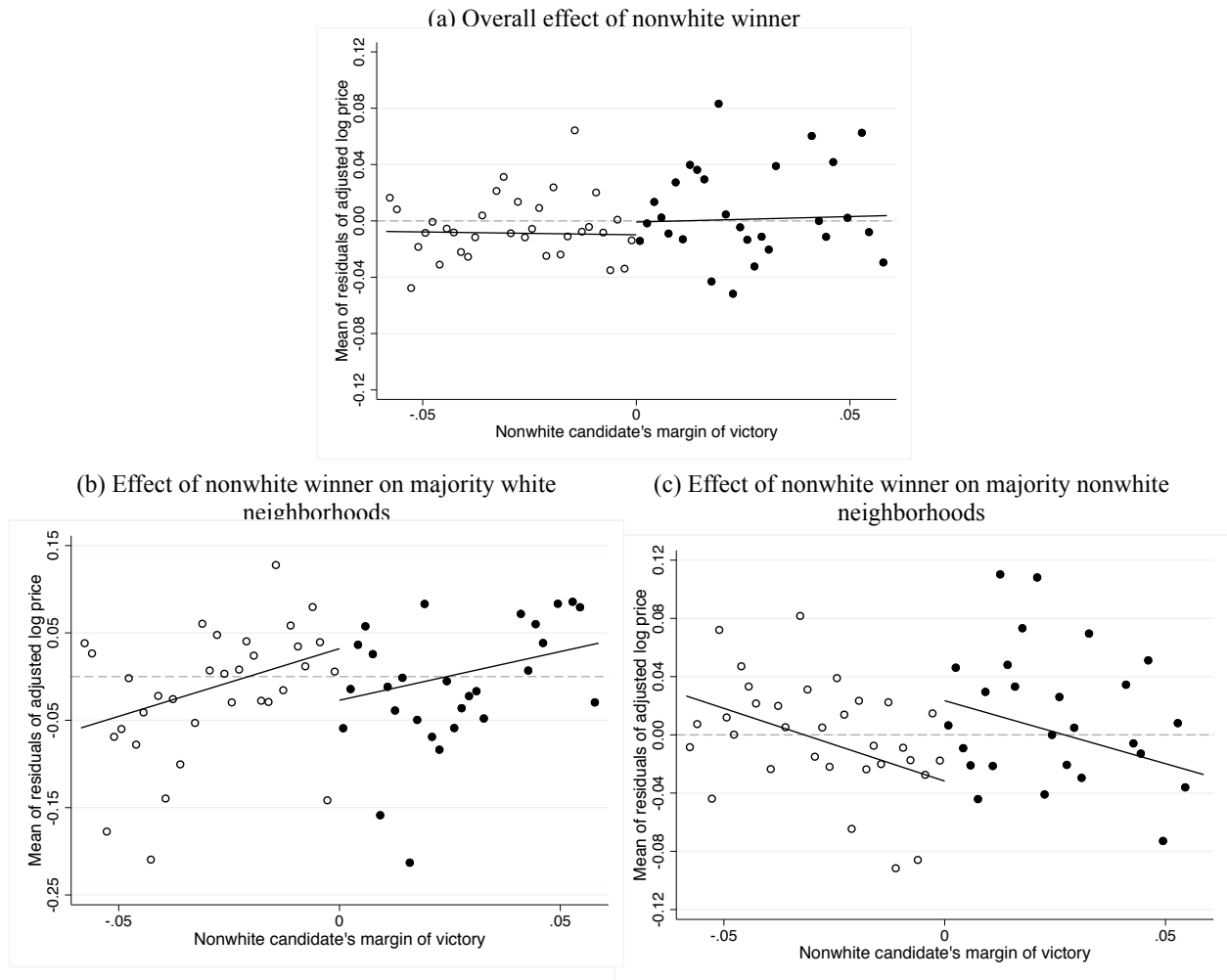
winner” coefficient). Conversely, nonwhite winners have a significantly positive *differential* effect on housing values in majority nonwhite neighborhoods (based on the “Nonwht. winner X Maj. Nonwht. Neigh.” coefficient). Moreover, this is a positive effect on houses in nonwhite neighborhoods overall: the linear combination of the two coefficients suggests that nonwhite neighborhood housing values are roughly 6.7 percent higher after a nonwhite candidate wins.

Figure 1 presents the same pattern of results graphically. Each panel is a binned scatterplot, where we plot average housing values 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). For this graphical analysis only, we take as our outcome variable the residual of a regression differencing out basic observable characteristics of the house and neighborhood. In the regression analysis, we simply control for such characteristics. 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, while Panel (c), which restricts the estimation sample to majority nonwhite neighborhoods. Results from these panels indicate a positive effect of nonwhite victory on housing values in non-white neighborhoods and a negative effect in white neighborhoods.²³ Note that Panel (c) does not perfectly map into our primary coefficient of interest, Nonwhite Winner X Nonwhite Neighborhood, but instead depicts the *overall* effect in majority nonwhite neighborhoods and is therefore instead a parallel to the linear combination in the bottom panel of Table 2.

²³ Note that Panel (c) is not directly comparable to the “Nonwhite winner X Nonwhite. Neighborhood” coefficient from Table 2, 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 4(c) is therefore comparable to the linear combination “Nonwhite winner” + “Nonwhite winner X Nonwhite Neighborhood” reported at the bottom of Table 2.

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.

Figure 1: Effect of a nonwhite candidate victory on housing prices, cross-sectional comparison



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.

6.5 Main Results: Panel-based regression discontinuity approach

We now turn to our main results, employing a panel-based regression discontinuity approach. 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 both our richest and most 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.

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 those that would

obtain if a white candidate had won. We observe a corresponding differentially positive effect in majority nonwhite neighborhoods. Overall, the effect on nonwhite neighborhoods is 11 percent more positive than the effect on white neighborhoods.

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 (0.044)	0.035 (0.042)	0.035 (0.041)	0.007 (0.027)
Panel B: Differential effects by neighborhood type				
Nonwht. win X Post	-0.058 (0.046)	-0.066 (0.046)	-0.068 (0.045)	-0.064** (0.030)
Nonwht. win X Post X Nonwht. Neigh.	0.147** (0.061)	0.153*** (0.056)	0.156*** (0.056)	0.111** (0.045)
<u>Linear combination to recover full effect on Nonwhite Neighborhoods</u>				
<i>(NW win X Post) +</i>	<i>0.089*</i>	<i>0.087*</i>	<i>0.088*</i>	<i>0.046</i>
<i>(NW win X Post X</i> <i>NW. Neighborhood)</i>	<i>(0.050)</i>	<i>(0.047)</i>	<i>(0.048)</i>	<i>(0.035)</i>
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

This finding 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.

Near the bottom of Table 3 we compute 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.²⁴ 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 be driven by the impact of nonwhite candidates, the impact of counterfactual white candidates, or some combination of the two.

6.6 Robustness of Main Results

In Figure 2 we present a series of robustness checks surrounding our main result. For the sake of comparison, Panel A of the figure depicts our main coefficient estimates, corresponding to the 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 shaded 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, with nonwhite candidates being 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

²⁴ 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).

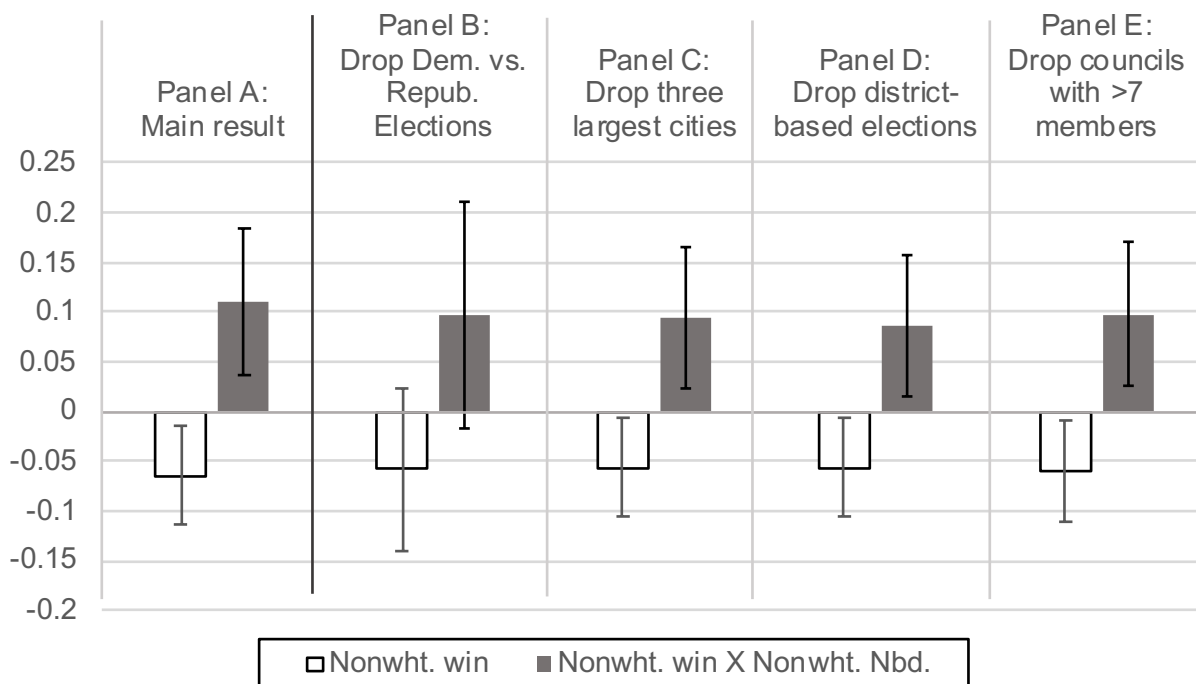
main result is driven by partisan preferences, Panel B presents results excluding all elections where one of the two marginal candidates is a Republican and the other is a Democrat. If our main result were driven by the fact that white vs. nonwhite elections often imply Republican vs. Democrat elections, 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 reduced sample size.²⁵

The remainder of Figure 2 reports additional sensitivity tests. Panel C drops the three largest cities in our sample;²⁶ 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 our main results are not driven by district elections. Dropping larger cities or cities with larger councils accounts for the fact that large cities tend to have larger councils. The other tests in Figure 2 ensure that our results are not being driven by those cities.

²⁵ In a separate set of regressions, we have tested whether the narrow election of a Democrat (rather than a Republican) has any effect on housing prices, city-wide or differentially for majority nonwhite neighborhoods. Those regressions reveal no evidence of an impact of partisan affiliation on housing prices, city-wide or separately by neighborhood. In a specification matching the set of controls and bandwidth of Table 3, Panel B, Column 4, we find coefficients of 0.004 and -0.015 on the main effect (Dem Win) and interaction term (Dem Win X Maj. Nonwh. N'Hood), respectively, with neither coefficient significantly different than zero.

²⁶ 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.

Figure 2: Robustness of panel RD results

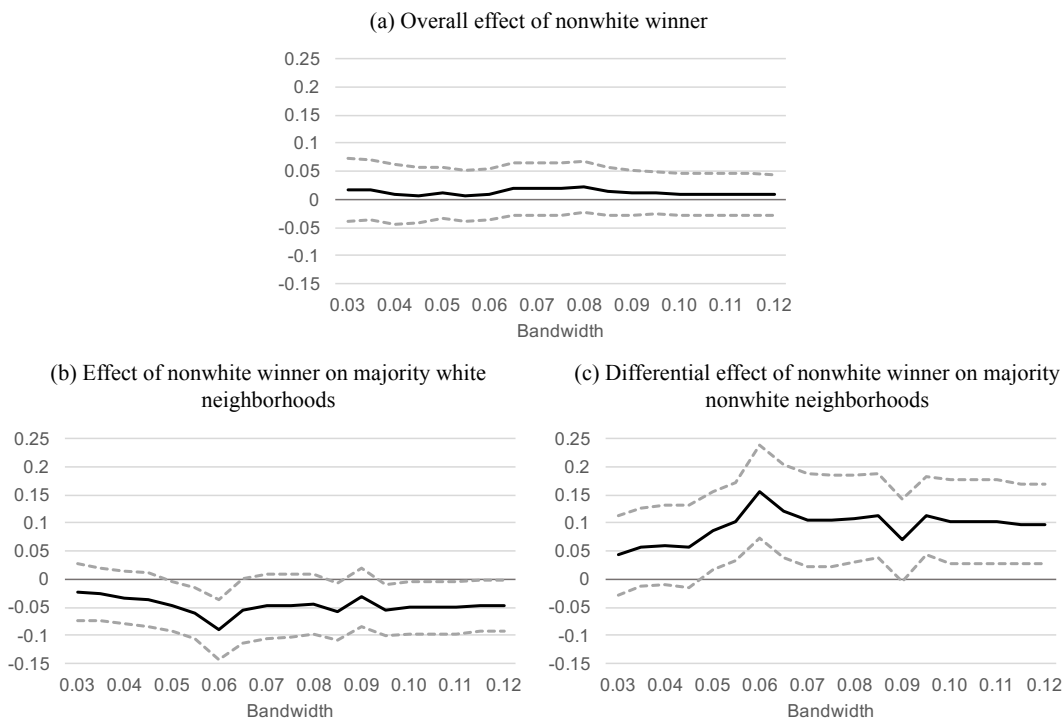


Note: This figure depicts results from five distinct regressions, with each pair of bars representing the relevant coefficient estimates from a single regression. Panel A simply reports our main result (from Table 3, Panel B, Column 4) for the sake of comparison with the rest of the figure. Note that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

Next, we evaluate the impact of bandwidth selection. Figure 3 shows that our general conclusions are not sensitive to our choice of bandwidth. The figure 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. We re-estimate these models for bandwidths ranging from 3 percentage points to 12 percentage points (roughly half and double our preferred 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. 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).

Figure 3: 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.

Table 4 reports additional robustness and placebo tests. Here 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 a neighborhood as

“high nonwhite” if it is 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 models yield results consistent with our main result.

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					<i>Four years before relevant election</i>	Two years before and after relevant election
Nonwht. win X Post	-0.062** (0.030)	-0.036 (0.027)	-0.025 (0.026)	-0.166*** (0.059)	-0.007 (0.020)	-0.010 (0.034)	0.004 (0.027)
Nonwht. win X Post X Maj. Nonwht. Neigh.	0.113*** (0.043)	0.074** (0.034)			0.057** (0.024)	0.011 (0.042)	-0.009 (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

Column 5 reports results from 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 model 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 finding 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.

Columns 6 and 7 present two placebo tests. In Column 6 we back our panel up by two years, 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.²⁷

²⁷ In a related set of tests, Appendix Table A2 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.

In Section 3 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 driven by electing residents of one specific neighborhood rather than the race of the candidate.

7 Mechanisms

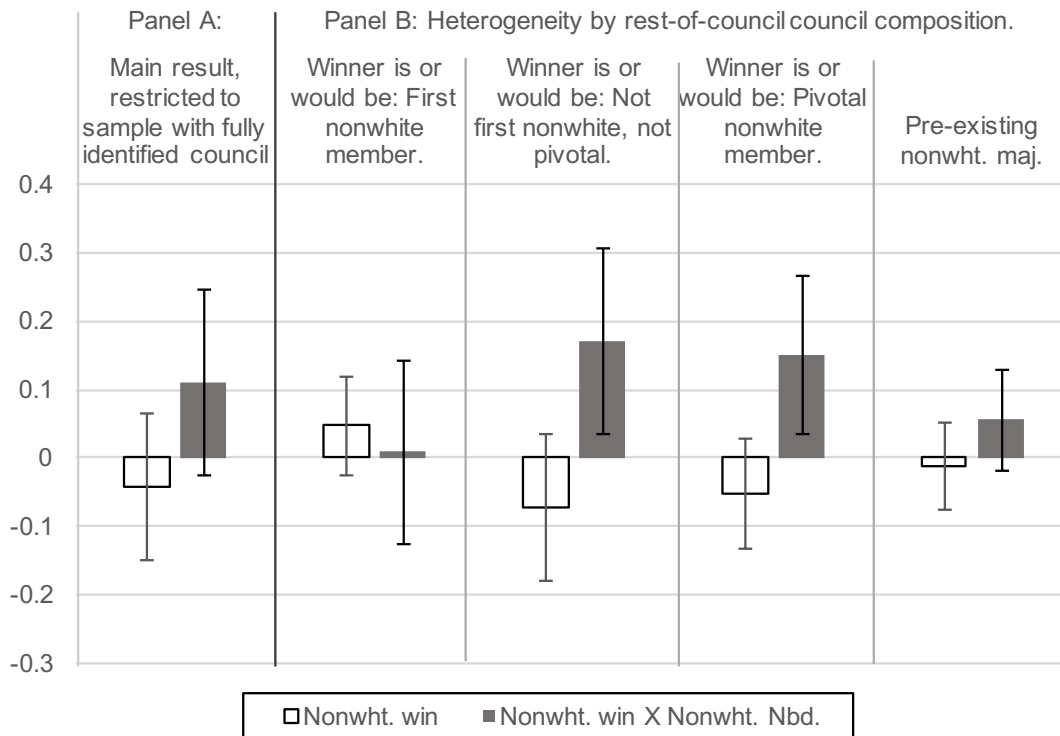
We next present analysis that explores the mechanisms underlying our main results. We begin by unpacking the basic housing price results in terms of pivotality, income, ownership and segregation levels. We then consider specific policies and outcomes that may drive our housing market results.

7.1 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.

Figure 4: 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 4. Again, Panel A of the figure depicts our main coefficient estimates; the white bar represents the impact of a nonwhite candidate

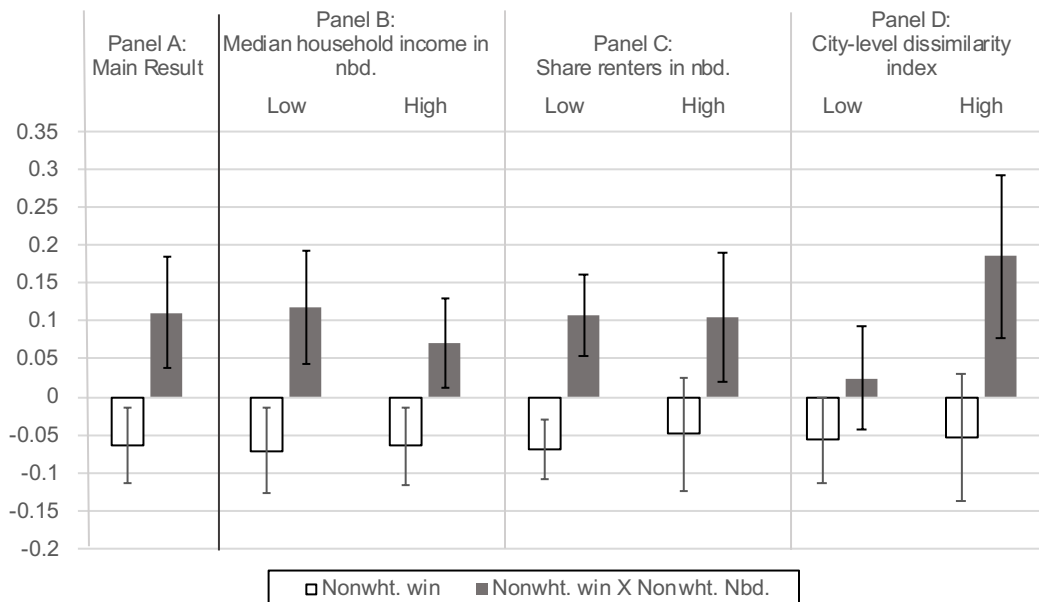
victory in majority white neighborhoods (the “Post X Nonwht. win” coefficient); the shaded bar represents the differential impact of a nonwhite victory in majority nonwhite neighborhoods (the “Post X Nonwht. win X Nonwht. Neigh.” coefficient). The results show that the election of a nonwhite candidate 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. Conversely, we observe strong impacts of nonwhite wins in cases where the nonwhite candidate is pivotal. There is also a strong impact of increased nonwhite representation when the nonwhite candidate is non-pivotal, but is also not the first nonwhite member of council – suggesting the impact of descriptive representation may hinge on the presence of a certain critical mass.

7.2 Income, ownership and segregation

We consider several additional dimensions of heterogeneity in our treatment effects, to both shed light on mechanisms and to rule out alternative explanations for 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 5). This suggests that our main results are not driven 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.²⁸

Figure 5: 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 D 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.

²⁸ A related 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 out this possibility, 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 (See Appendix Table A9). This finding, paired with the similar effects observed across high and low income neighborhoods, point away from a gentrification explanation for our results.

Panel D considers 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),²⁹ we see that our results are largely driven by more segregated cities.

7.3 Changes in policies/outcomes that underpin our main results

As noted above, we use housing prices as a proxy for changes in policy and spending patterns that differentially affect minority and non-minority individuals for two reasons. First, from a theoretical perspective they are an appropriate “index” number for aggregating across the broad range of policies/outcomes that can be influenced by city councils. Second, the paucity of data relating to these policies/outcomes that is systematically available and disaggregated to the neighborhood level limits what we can measure directly. There are, however, some noted exceptions to this second point which we now consider.

First, in our discussion of specific candidate examples, concern about inequities in 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, both directly and via city policy, there is

²⁹ We use the typical two-group dissimilarity index as our measure of diversity, with the two groups in question being white and nonwhite.

scope for council members to affect 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 differentially increases the number of business establishments in majority nonwhite areas and decreases the number in majority white areas.

Of course, the concerns raised by candidates about economic development 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 and, in general, can be a good proxy for locally undesirable land uses (see Shertzer et al., 2018). 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, suggesting that the increase in business activity observed in Column 1 was not associated with an increase in environmental threats faced by local residents.

Table 5: Effects of councilmember ethnicity on local economic development

	(1)	(2)
<i>Outcome variable:</i>	Log of number of establishments in ZIP	Any TRI facility in tract? (=1, 0 otherwise)
Panel A: Overall effects		
Nonwht. win X Post	0.006 (0.009)	0.001 (0.009)
Panel B: Differential effects by neighborhood (tract) type		
Nonwht. win X Post	-0.038* (0.021)	0.002 (0.018)
Nonwht. win X Post X Maj. nonwht. area	0.078** (0.032)	0.000 (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

Policing was also a common focus of minority candidates. Because each municipality typically has its own police force, there is 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 aggregated 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.

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 (p-value = .054) 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.

Table 6: Effects of councilmember ethnicity on policing and crime

	(1)	(2)	(3)	(4)
<i>Outcome variable:</i>	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

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 A6 we present results across a range of fiscal outcomes: expenditures, revenues, spending on public goods, safety, transportation, etc. Across each of the eight 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 placebo test, suggesting that the election of minority council members is not correlated with some other broad re-alignment in local government. Finally, they suggest 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.³⁰

8 Conclusion

In this paper, we use data from California to study the impact of minority representation in local government on outcomes for minority individuals and the neighborhoods in which they live. Our basic empirical strategy is two-fold. First, we utilize fine-scale spatial variation in the evolution of housing prices across minority and non-minority neighborhoods as a sufficient statistic for the value of government policies to the residents of said neighborhoods. Second, we leverage the outcomes of close elections between minority and non-minority candidates as a source of quasi-random variation in treatment. We find that, relative to the election of a white candidate, election of a non-white candidate serves to offset pre-existing gaps between minority and white neighborhoods. Consistent with the assumptions underlying our basic hedonic approach, we find that the largest effects occur in more segregated cities. Further, and in contrast to previous work in this area, we can rule out important alternative explanations for our main conclusion including correlations between the race of candidates and their partisan affiliations, and the racial and income composition of neighborhoods.

³⁰ In addition to aggregate revenue and spending categories, In Table A7 we explore the impact of electing a minority council member on propensity to adopt revisions to city planning documents and in Table A8 we examine impacts on aggregate building permit activity. In both cases, we find no evidence of any impact on these city-level measures.

Additional analysis points to how these distributional shifts occur. We find that the impact of an additional nonwhite candidate depends on the pre-existing composition of the council that he or she enters. We observe the strongest effects when the nonwhite candidate is “pivotal,” flipping the council to majority nonwhite; but, notably, we also find strong results for candidates who are not the first nonwhite council-member but are also not “pivotal.” In contrast, the first nonwhite candidate on council has no observable impact on housing prices, nor does a minority candidate entering a council that is already majority nonwhite. This result suggests that in the absence of a majority, a sub-majority critical mass of minority council-members can have a marked impact on outcomes. In terms of specific channels of impact, we find that the election of a nonwhite candidate increases business activity in majority nonwhite neighborhoods and leads to shifts in arrest patterns away from nonwhite residents.

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