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Racial and Ethnic Representation in Local Government†

By Brian Beach, Daniel B. Jones, Tate Twinam, and Randall Walsh*

Does the presence of underrepresented racial/ethnic groups in a legislative body differentially impact outcomes for members of those groups? We study close elections between White and non-White candidates for California city council and the corresponding impact on housing values, a summary statistic for neighborhood investment. We find electing non-White rather than White candidates generates differential home value gains in majority non-White 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 council member pivotality. (*JEL* D72, J15, R23)

> 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

The Voting Rights Act (VRA) of 1965 is one of the most important pieces of legislation in US history. Its passage returned the franchise to millions of Black Southerners, helped reduce racial disparities in public spending and the provision of public goods (Cascio and Washington 2013), and reduced the Black-White wage gap (Aneja and Avenancio-Leon, 2019).

Later amendments to the VRA and related court decisions have pushed not only for greater ballot access but also for greater presence of underrepresented groups in elected office (Grofman, Handley, and Niemi 1992). Implicit in these efforts was the

* Beach: Vanderbilt University, Department of Economics (email: brian.beach@vanderbilt.edu); Jones: University of Pittsburgh, Graduate School of Public and International Affairs (email: dbj10@pitt.edu); Twinam: William & Mary, Department of Economics (email: mail: tatwinam@wm.edu); Walsh: University of Pittsburgh, Department of Economics (email: mail: walshr@pitt.edu). Lucas Davis was coeditor for this article. We are grateful for helpful comments from Pat Bayer, Justin de Benedictis-Kessner, Morgane Laouenan, William Mathews, Rachel Meltzer, Chris Mothorpe, Allison Shertzer, and Carly Urban. Thanks also to seminar participants at Clemson University, Florida State University, George Washington University, Montana State University, Oberlin College, Oklahoma State University, the University of Hawaii, the University of Tennessee, and the University of South Carolina as well as conference participants at the Association for Public Policy Analysis and Management Fall Research conference, the American Real Estate and Urban Economics Annual Meeting, the Public Choice Annual Meeting, the Urban Economics Association Meeting, and the Local Political Economy Conference at George Washington University.

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notion that representation at the electoral *and* legislative stages of the political process is necessary to adequately serve the needs of historically marginalized groups. The link between racial/ethnic composition of elected officials ("descriptive representation") and the degree to which distinct racial/ethnic groups' policy preferences are acted upon by elected officials ("substantive representation") remains salient today, as an overrepresentation of White elected officials in local governments is pointed to as a driver of racial disparities in outcomes as varied as housing,[1] economic development,[2] and policing.[3]

Theory offers mixed predictions for whether the racial/ethnic identity of elected officials impacts substantive representation for members of the same group. Spatial competition/median voter models (Hotelling 1929; Downs 1957) and models that focus on appeals to swing groups (e.g., Dixit and Londregan 1996) suggest that the election of a group member per se should not affect policy outcomes. Conversely, citizen-candidate models, where politicians are motivated to implement their preferred policies (Osborne and Slivinski 1996; Besley and Coate 1997), as well as models where candidates are incentivized to induce core constituencies to vote (Glaeser et al. 2005), suggest that electing representatives from different race/ethnic groups could lead to different policy outcomes.

Given the overrepresentation of White elected officials in local government in the United States (Ricca and Trebbi 2022), this paper provides an empirical assessment of whether increased non-White representation differentially affects non-White—relative to White—constituents.[4] We study close elections between White and non-White candidates running for city council in California between the years 2005 and 2011. We adopt a regression discontinuity (RD) approach that exploits narrow victories as a source of identifying variation. We pair this election data with comprehensive housing transaction microdata. This allows us to identify the extent to which the election of a non-White city council member generates a differential change in housing prices in majority non-White neighborhoods.

Our focus on housing prices solves an important methodological challenge that arises when evaluating the importance of representation in local government. City councils make a number of important decisions, including—but not limited to—setting spending priorities, adopting rules and regulations that impact business and land development, appointing other government officials, and interacting with private contractors. These decisions have clear implications for the local economy, helping shape zoning, policing, pothole repair, trash pickup, and other local infrastructure investments. This broad scope of influence, paired with the fact that neighborhoods and politicians likely face a unique set of challenges, means that it would be easy to miss the importance of representation by considering distinct policy areas, as what may be a priority in some share of cities might not be a priority in the remainder.

[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.

[4] There is, of course, a large literature in economics and political science examining whether personal characteristics of elected officials (race, gender, partisan affiliation, etc.) affect policymaking, but there is less evidence on the differential impact that those candidates have for different groups. Section I reviews this relevant work.

Moreover, there is a lack of data that would allow a researcher to assess, across a large number of cities, sub-city distributional impacts.

Housing values, in contrast, offer a "summary statistic," 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 or for initiatives that are difficult to observe in data. For example, Albouy, Christensen, and Sarmiento-Barbieri (2020) show that proximity to a park increases house prices when the park is perceived as safe but decreases prices when the park is seen as unsafe. Thus, overall impacts may not be identifiable through the analysis of a set of unidimensional policy changes. Housing prices are also unique in that they reflect expectations about the future stream of amenities (Bishop and Murphy 2011, 2018).[5] Our focus on housing markets follows the long tradition of using house prices as a sufficient statistic for valuing public and private investments; see Oates 1969 (tax policy); Black 1999 (school quality); Linden and Rockoff 2008 (crime); Chay and Greenstone 2005 (environmental quality); and Turner et al. 2014 (land use regulation).

We find that, relative to the election of a White candidate, the election of a non-White candidate reduces preexisting gaps in housing prices across majority White and non-White neighborhoods. Robustness checks alleviate concerns that this result is driven by correlations between candidate race/ethnicity and political affiliation or between racial composition and neighborhood income. Price impacts are particularly pronounced when the election pushes the council closer to majority non-White, and are consistent with the assumption that our results are driven by a spatial reallocation of services to non-White neighborhoods. These effects are stronger in more heavily segregated cities, where there is more scope for such reallocation.

Our findings complement work on the VRA by Cascio and Washington (2013) and Aneja and 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 descriptive representation can be another important tool for addressing racial disparities. In this sense, our work is also closely related to work by Logan (2020), 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, more than a century since Reconstruction and the adoption of the Fourteenth Amendment and five decades after the passage of the VRA, descriptive representation has important implications for the well-being of underrepresented racial/ethnic groups.

I. Related Work

There is a large literature in economics and political science examining whether personal characteristics of elected officials (race, gender, partisan affiliation, etc.)

[5] We find little evidence of mean reversion (see Figure 4), suggesting that if expectations were a major driver, then those initial expectations likely came to fruition.

affect policymaking.[6] Most relevant for our work is the literature examining the impacts of electing politicians from particular ethnic or racial groups. Hopkins and 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, [there are] few differences between Black mayors and their White counterparts" (McCabe 2012, 691), a finding that is broadly consistent with the prior work that they review.

We highlight several papers from this literature that are especially relevant in that they do document distributional impacts of descriptive representation. Logan (2020) finds that Black political leaders in the Reconstruction era affected tax and land policy as well as the Black-White literacy gap. Pande (2003) studies mandated representation of scheduled castes and tribes in India and finds that newly represented groups benefit from transfers from the government. Sances and You (2017) document a relationship between the share of a city's population that is Black and the use of fines as revenue, a relationship that 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. Hinds and 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, highlighting a potential mechanism 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, Rainer, and Stratmann (2014) find that the Black mayors are 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.[7] Both issues are essential for understanding whether a causal link between racial/ethnic representation and differential outcomes by race/ethnicity exists. In our data, we directly test and reject both possibilities.

Our work is also related to a series of recent papers on school board representation, which collectively highlight that *who* is elected to local office can have important impacts on policy outcomes. Kogan, Lavertu, and Peskowitz (2021) and Fischer (2023) document differential impacts of non-White school board representation on non-White student outcomes in California schools. Shi and Singleton (forthcoming) document that an additional educator on a school board impacts teacher salaries, while Macartney and Singleton (2018) use an RD approach to document that an additional Republican on the board increases school segregation.

Finally, our paper relates to Beach and Jones (2017), but with two important points of differentiation. First, Beach and Jones (2017) study the impact of council *diversity* on overall levels of public good provision. In California in particular—the setting for that study and this one—an increase in council diversity is not necessarily

[6] Outside of race, previous empirical research has considered the impacts of partisan affiliation (e.g., Ferreira and Gyourko 2009; de Benedictis-Kessner and Warshaw 2016, 2020), gender (e.g., Ferreira and Gyourko 2014), and professional experience (e.g., Beach and Jones 2016; Kirkland 2021) on policy outcomes, yielding mixed results.

[7] Piliawsky (1985) offers an interesting narrative account of how Ernest Morial, the first Black mayor elected in New Orleans, affected Black communities.

equivalent to an increase in non-White representation.[8] They are therefore studying a different, though related, explanatory variable. Second, and more importantly, Beach and Jones (2017) study the impact of council diversity on citywide levels of expenditures. It is therefore inherently not a study of *distributional* outcomes. The present paper, on the other hand, studies the impacts of non-White representation on differences in outcomes across White and non-White neighborhoods. The focus is explicitly distributional. Namely, this paper speaks to the broader question of how and whether descriptive representation of otherwise underrepresented race/ethnic groups in elected office may lead to *differential impacts* for members of their groups. Beach and Jones (2017), in exploring citywide spending, could not speak to that issue. The papers are linked in that we anticipate that shifts in representation on the council could lead to shifts across neighborhoods in spending on public goods, but we cannot observe that and, instead, use housing prices as a summary statistic to capture such changes.

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 and Trounstine (2014b) 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. Using survey data, Hajnal and Trounstine (2014a) document large racial disparities in satisfaction with local public good provision, with Black residents reporting lower satisfaction than White respondents, while Marschall and Ruhil (2007) document that Black respondents' satisfaction is higher in cities with a Black mayor. 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, Donovan, and Karp (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, in turn helping address gaps in public good and service provision.

[8] Beach and Jones (2017) measure diversity using a fractionalization measure, taking in seven distinct racial/ethnic categories. Thus, for example, in their paper, an all-Black council that gains one White member would be coded as increasing in diversity, whereas in our paper this would be coded as a *decrease* in non-White representation.

II. 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). Given the breadth of a council's influence, housing prices offer a unique proxy for how different groups value the public goods provided by local government that is both widely available and spatially disaggregated. 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 council member in city G who is interested in directing benefits toward a particular subgroup of her electorate ($subgroup = k \in K$).[9] One approach would be a "group-targeting" strategy that directs resources to policies that 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, $\left\{G^k | k \in K\right\}$. A second "neighborhood-targeting" approach would direct city resources to specific neighborhoods ($neighborhood = j \in J$) where individuals of subgroup k are congregated. This policy will give rise to neighborhood-specific public good levels $\left\{G^j | j \in J\right\}$. 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 council member's ability to use a neighborhood targeting approach would be expected to increase with segregation levels.

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

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

where Y_i^k represents the marginal buyer's income, ξ^{jk} 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.[10]

[9] The channels delineated in our model will operate regardless of whether the council direct public goods toward a subgroup by reallocating resources from a fixed budget or by increasing total expenditures. We abstract from budgeting issues, as Proposition 13 constrains the ability of California cities to generate new revenue.

[10] For further exposition on this basic modeling approach see Polinsky and 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.

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 $dP_h/dG^k = V_{G^k}/V_Y$. Thus, at the margin, the change in transaction prices for houses purchased by individuals of group k will exactly measure individual willingness to pay for 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.[11] While nonmarginal changes do not allow for as simple an interpretation, the general implications are similar. This basic analysis underpins the large extant literature that uses 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. If, as is often the case, neighborhoods are segregated by group, 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 $dP_h/dG^j = V_{G^j}/V_Y$. 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- and place-based policies, changes in housing prices, and changes in welfare. It is natural to wonder whether it is appropriate to associate increased housing prices with increased welfare in cases where neighborhoods are comprised mainly of renters. For renter households, at least some portion of the benefit from increases in public goods will accrue to the owner in the form of higher rents. Along similar lines, one might worry that if a council member were to use 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 politicians to abandon policies that differentially benefit some residents) 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 non-White council member (see Table 6). Our results also hold after imposing sample restrictions on

[11] This result follows directly from the open-city assumption; see Polinsky and 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.

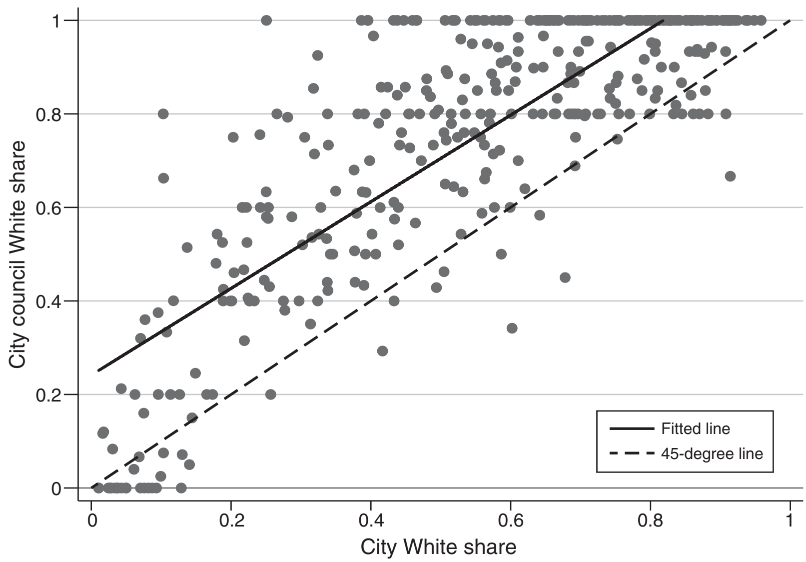

Figure 1. Assessing Non-Hispanic White Representation on California City Councils

Notes: Sample restricted to city councils where the ethnicity of all members is known. These councils typically served between 2005 and 2012 (see Section IVA for more details). City White share is from the 2000 census.

the share of rental units in the neighborhood. These findings provide support for our interpretation that increases in house prices reflect welfare improvements for the residents of those neighborhoods.

III. Empirical Context

We adopt an empirical approach that leverages narrowly decided elections between White and non-White candidates to obtain plausibly exogenous variation in non-White representation on a city council. In our core model, we examine whether changes in non-White representation generate differential housing market responses across White and non-White 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 non-White candidates and substantial variation in neighborhood composition. Despite this, underrepresentation is an issue in California cities. In Figure 1 we plot the city council's non-Hispanic White share against the city's non-Hispanic White share. We include a 45-degree line, which would correspond to situations where the council and the city have the same non-Hispanic White share, as well as a fitted line obtained from regressing council White share on city White share. The vast majority of our observations fall above the 45-degree line, indicating that non-Hispanic Whites are, on average, overrepresented on California's city councils. The fitted line lies entirely above the 45-degree line, which is again consistent with overrepresentation. The *y*-intercept is about 0.25, meaning that even in a city with a 0 percent non-Hispanic

White share, we would expect at least 1 of the 5 councilors to be non-Hispanic White. This may sound like an extreme characterization, but 49 percent of majority non-White cities are governed by majority non-Hispanic White councils.

An additional benefit of this context is that California state law provides a number of guidelines for the structure of municipal governments, which limits institutional variation when making cross-council comparisons. A city that adopts these default guidelines would have a council with five council members; each council member would serve staggered four-year terms, with elections filling multiple seats every two years, and the council members would be elected "at large" during a general municipal election. For instance, an election in 2004 might fill three of the five seats. If four candidates ran for office, then the candidates with the three highest vote shares would be elected, and they would serve a four-year term and face reelection in 2008. There would then be an election in 2006 to decide on the remaining two seats. A similar approach is used in most cities with more than five members.

Many municipalities conform to these guidelines. For instance, 88 percent of city councils contain exactly five council members, the legislated minimum, and 92 percent of cities elect council members through "at-large" elections. Moreover, 93 percent of cities use a "council manager" governance structure, meaning that the council dictates the policy and the mayor—who for 98 percent of cities is simply selected by the council from among its own members—oversees carrying out said policy. Larger cities tend to deviate from these guidelines either by having more members or by tying council seats 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 California city councils have considerable discretion in providing and reallocating public goods, there are also some important limitations. In California, elected school boards control local school policy, and Proposition 13 restricts property tax growth, often requiring new spending to be offset by reductions elsewhere. Data from the California State Controller's Office provide an overview of the many local goods and services that councils oversee. The vast majority of cities in our sample (89 percent) provide their own community development planning and manage their own parks and recreation services (88 percent). Over 70 percent 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 Bay Area Rapid Transit). Even if a city does not directly provide certain services, council members can influence 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 jurisdictions' policies regarding "the location of housing, business, industry, roads, parks, and other land uses, protection of the public from noise and other environmental hazards, and conservation of natural resources" (GOPR 2005, 1). California city councils are responsible for approving and modifying zoning ordinances, which have considerable power to affect patterns of local economic development at the neighborhood level. By controlling the distribution of land uses, these ordinances can strongly influence patterns of exposure to industrial activity and pollution, traffic congestion, employment opportunities, commercial amenities, and even street crime. They can also affect the location of new housing development, at both the small scale (backyard accessory dwelling units) and the larger scale (high-rise multifamily housing), which can directly and indirectly affect housing values.

A few examples from recent city council elections in California further illustrate these linkages. A major focus of Juan Carillo's campaign for council in Palmdale, California, 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. Once elected, Carillo introduced legislation to give individual council members responsibility for appointing planning commissioners.[12] The policing and treatment of immigrants was also a focus in many campaigns. For example, in 2008, Olga Diaz became the first self-identified Latina council member 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). The city 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 toward economic development, local revitalization, and quality-of-life issues (Florido 2009). As a final example, Sacramento NAACP President Betty Williams ran for council on a platform focusing on strengthening the city's Community Police Review Commission and targeting newly available tax revenues to job training and minority business start-ups.[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 non-White and White groups and neighborhoods. In the analysis that follows, we pursue a more systematic assessment of these linkages.

IV. 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.

[12] See Constante (2018).
[13] See Clift (2019).

A. *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 requires city council elections to be nonpartisan, 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 race/ethnicity, we rely on Beach and Jones (2017), who construct a dataset identifying the race/ethnicity for 4,226 of the 5,177 council members 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 candidates' 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 percent of the candidates in our sample. As a result, we can identify the partisan affiliation of two competing candidates in 61 percent of our sample elections.

B. *Neighborhood Characteristics*

We use census block group–level data from the 2000 decennial census to measure within-city neighborhood characteristics.[15] Thus, when we refer to "neighborhoods," we are referring to census block groups. We use 2000 census data, as opposed to, for instance, 2010 American Community Survey data, to ensure that our neighborhood controls are not endogenous to election outcomes.

[14] Sumner, Farris, and Holman (2020) assess the accuracy of data collected through Mechanical Turk using similar data collection methods and conclude that data collected through Mechanical Turk is "highly accurate." Beach and Jones (2017) drew on additional sources to validate their own data collection and reached a similar conclusion.

[15] All census data used in this paper (at block group, tract, zip code, and city levels) are obtained from IPUMS-NHGIS (Manson et al. 2022).

These data provide, for every block group, 100 percent counts of population, population in urban areas, population in rural areas, males, females, people over the age 18, people over the age 65, 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 tell us the number of individuals belonging to each of the following 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 the ethnic fractionalization index as a measure of neighborhood diversity.[16] We classify block groups as *majority White* if the non-Hispanic White population share is greater than 0.5 and *majority non-White* otherwise.

C. *Housing Prices and Characteristics*

We obtain transaction-level housing data provided by DataQuick Information Systems under a license agreement. This dataset includes the universe of single-family home sales in California between 2005 and 2011. Transaction records are matched with assessor records to identify bedrooms, bathrooms, stories, square footage, and year built. We trim the top and bottom 1 percent 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 (CZs) 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.

D. *Summary Statistics and Baseline Correlations*

Our main analysis employs an RD design, and so we only use a subset of the data gathered. The goal of the RD approach is to generate quasi-random assignment to treatment (election of a non-White council member) or counterfactual (election of a

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

Table 1—Summary Statistics

	All cities	Cities with known Wht. versus non-Wht. elections	Cities with known close Wht. versus non-Wht. elections
Panel A. City-level characteristics			
Total population	58,218	86,670	95,402
	(8,972)	(17,349)	(23,128)
Asian/Pac. Isl. share	0.090	0.124	0.134
	(0.005)	(0.009)	(0.011)
Black share	0.041	0.057	0.057
	(0.003)	(0.005)	(0.005)
Hispanic share	0.296	0.353	0.353
	(0.012)	(0.015)	(0.018)
White share	0.564	0.459	0.448
	(0.012)	(0.015)	(0.017)
Other share	0.002	0.002	0.002
	(0.000)	(0.000)	(0.000)
Ethnic fractionalization	0.440	0.522	0.534
	(0.008)	(0.010)	(0.011)
Council size	5.294	5.475	5.470
	(0.044)	(0.077)	(0.091)
District-based elections	0.161	0.226	0.183
	(0.017)	(0.028)	(0.030)
Observations	442	221	164
	All	Known Wht. versus non-Wht.	Known close Wht. versus non-Wht.
Panel B. Election-level characteristics			
Num. open seats	1.897	1.712	2.145
	(0.016)	(0.034)	(0.048)
Num. candidates	4.638	4.490	5.525
	(0.048)	(0.103)	(0.146)
Margin of victory	0.117	0.125	0.025
	(0.003)	(0.006)	(0.001)
Observations	2,749	549	276

Notes: Standard deviations in parentheses. Population and ethnicity shares come from the 2000 census. Council size and election information come from the California Elections Data Archive.

White council member). To achieve this, we restrict our sample to housing transactions in cities associated with an election that met the following conditions: (i) of the two marginal candidates (the last-place winner and first-place loser), one is White and the other is non-White, and (ii) the election was within an optimally selected bandwidth. Again, these elections fill multiple seats, and so the optimal bandwidth corresponds to the difference between the two marginal candidates. Our optimal bandwidth (6.44 percentage points) was chosen following Calonico, Cattaneo, and Titiunik (2014), which we discuss further in Section VB.

Table 1 provides basic summary statistics comparing cities and elections in our estimation sample. Panel A examines city-level characteristics for all cities (column 1), cities that ever experience an election where one of the marginal candidates is White and the other is non-White (column 2), and the subset of those cities where that ethnically diverse election was decided by no more than 6.44 percentage points

Table 2—Baseline Correlations

	DV is ln(Sale price), inflation and market adjusted			
	(1)	(2)	(3)	(4)
Maj. non-White block	−0.422	−0.151	−0.138	−0.224
	(0.037)	(0.029)	(0.026)	(0.079)
NW block × NW councilor				0.092
				(0.082)
Observations	2,043,282	2,043,282	2,043,282	2,043,282
Num. cities	380	380	380	380
Neigh. income		Y	Y	Y
House chars.			Y	Y

Notes: Robust standard errors (clustered at the city level) reported in parentheses. All regressions include county fixed effects and time fixed effects (monthly interval). Neighborhood income controls are median household income, percent of households receiving public assistance, and percent of households below the poverty line (all measured at the census block group level). House characteristics include square footage, age of the house, and fixed effects for number of bedrooms, bathrooms, and stories. Sale price adjustment described in Section IVC.

(column 3). Cities where we observe an ethnically diverse election occurring tend to be larger and have more underlying ethnic diversity, as measured by both their non-Hispanic White share and the ethnic fractionalization index, but these features are not more pronounced for the cities with close, ethnically diverse elections. Panel B examines election-level characteristics for all elections (column 1), elections where one of the marginal candidates is non-Hispanic White and the other is not (column 2), and the subset of ethnically diverse elections decided by no more than 6.44 percentage points (column 3). Comparing columns 1 and 2, we see that ethnically diverse contests are similar to the typical electoral contest in terms of the number of open seats, the number of candidates, and the margin of victory. When we restrict to close elections, the margin of victory mechanically becomes much smaller (2.5 percentage points on average instead of 12). We also see that these close elections have about one more candidate on average, which may reflect that there are slightly more total seats being decided.

In Table 2 we examine the naïve relationship between representation and house prices, which helps fix ideas for the remainder of our analysis. We restrict attention to transactions that occur in cities and years where we observe the ethnicity of all city council members. In column 1 we regress ln(sale price), inflation and market adjusted, on an indicator for whether that transaction occurred in a majority non-White neighborhood and see that homes in these neighborhoods sell for about 42 percent less than homes in majority White neighborhoods. In column 2 we control for various measures of neighborhood income (median household income, percent of households receiving public assistance, and percent of households below the poverty line), and in column 3 we control for a variety of housing characteristics (square footage, age of the house, bedrooms, bathroom, and number of stories). The baseline deficit falls from about 42 percent to about 14 percent (columns 2 and 3). Finally, we interact this neighborhood composition measure with an indicator for whether the council has a non-White council member. There we see that homes in majority non-White neighborhoods with no representation sell for about 22 percent less than similar homes located in majority non-Hispanic White neighborhoods. The interaction with our measure of ethnic representation (council contains a non-White counselor) is positive and about half of the original magnitude but statistically insignificant, providing suggestive evidence that housing disparities might be lower in areas with greater representation.

V. Main Analysis

Our main empirical approach is a panel-based RD design, similar to Cellini, Ferreira, and Rothstein (2010). We begin by expositing our analytical design—building up from a basic cross-sectional RD model.

A. *Empirical Approach*

We identify the causal impact of electing a non-White council member using local linear regressions estimated on a sample of close elections between White and non-White candidates. The closeness of an election is based on the difference between the non-White candidate's vote share and the White candidate's vote share—henceforth, non-White margin of victory. Thus, a positive margin of victory indicates that the non-White 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. As noted above, a single election often fills multiple seats, with the top K candidates in vote share filling the K available seats. Throughout all of our analyses, the RD specification focuses on the two marginal candidates; that is, we focus on elections where exactly one of the Kth and $(K+1)$th candidates is White and exactly one is non-White. The margin of victory employed in analyses is the margin between these two candidates.

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

$$\ln(p)_{hct} = \propto + \beta_1 \mathbf{1}\{NonWhiteWins_{ct}\} + \beta_2 marginofvictory_{ct} + \beta_3 \mathbf{1}\{NonWhiteWins_{ct}\} \times marginofvictory_{ct} + \varepsilon_{hct}, \tag{2}$$

where $\ln(p)_{hct}$ is $\ln(adjusted\ price)$ of house h in city c during council term t. $\mathbf{1}\{NonWhiteWins_{ct}\}$ is an indicator variable equal to one if the non-White candidate wins, which we fully interact with non-White margin of victory. The coefficient β_1 identifies the effect of a non-White candidate winning conditional on the margin of victory being zero. 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 non-White candidate.

While equation (2) can identify the impact of increased non-White representation on housing values overall, our main interest is in understanding whether an increase in representation differentially affects housing values in majority non-White neighborhoods. To address this question, we modify equation (2) by fully interacting all of the relevant variables (non-White win, non-White margin of victory, and the interaction of the two) with an indicator variable set equal to one if the house is located in a majority non-White neighborhood. The modified specification is then

$$(3) \ln(p)_{hct} = \propto + \beta_1 \mathbf{1}\{NWwins_{ct}\} + \beta_2 margin_{ct} + \beta_3 \mathbf{1}\{NWwins_{ct}\} \times margin_{ct} + \beta_4 \mathbf{1}\{NWwins_{ct}\} \times \mathbf{1}\{NWNeigh_h\} + \beta_5 margin_{ct} \times \mathbf{1}\{NWNeigh_h\} + \beta_6 \mathbf{1}\{NWwins_{ct}\} \times margin_{ct} \times \mathbf{1}\{NWNeigh_h\} + \beta_7 \mathbf{1}\{NWNeigh_h\} + \varepsilon_{hct},$$

where $\mathbf{1}\{NWNeigh_h\}$ is an indicator for whether the neighborhood (census block group) is majority non-White. Now, β_1 identifies the causal impact of a non-White victory on house prices in majority White neighborhoods, and β_4 (our primary coefficient of interest) identifies the *differential* effect of a non-White victory on non-White neighborhoods.

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. To reflect the level of treatment, our main specifications include election fixed effects. For cities with only one relevant election during the sample period, election fixed effects are equivalent to city fixed effects. For a city with more than one relevant election, each election is treated as a separate panel with a different fixed effect. In other words, our data in this approach are configured as a set of four-year panels centered around specific elections, with two years of preelection observations and two years of postelection observations. The presence of pre- and post- observations, as well as the inclusion of election-level fixed effects, allow us to evaluate the change in house prices in cities where the non-White candidate won relative to changes in house prices in cities where the non-White candidate lost. This contrasts with the cross-sectional approach, which simply compares postelection transactions in cities that elected a non-White candidate to postelection transactions in cities that elected a White candidate.

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

$$
\begin{aligned}
(4)\ln(p)_{hect} = {} & \propto + \beta_1 \mathbf{1}\{NonWhiteWins_{ec}\} + \beta_2 margin_{ec} \\
& + \beta_3 \mathbf{1}\{NonWhiteWins_{ec}\} \times margin_{ec} + \beta_4 \mathbf{1}\{NonWhiteWins_{ec}\} \\
& \times \mathbf{1}\{Post_{ect}\} + \beta_5 margin_{ec} \times \mathbf{1}\{Post_{ect}\} + \beta_6 \mathbf{1}\{NonWhiteWins_{ec}\} \\
& \times margin_{ec} \times \mathbf{1}\{Post_{ect}\} + \beta_7 \mathbf{1}\{Post_{ect}\} + \gamma_{ec} + \varepsilon_{hect}.
\end{aligned}
$$

Equation (4) is similar to equation (2) in that it does not yet allow for differential effects by neighborhood type. We take the ln(*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 non-White wins, margin of victory, and interaction variables, but these are now defined with respect to the election e. We then fully interact each of those variables with a new indicator variable, $\mathbf{1}\{Post_{ect}\}$, which equals one if the transaction occurs in the two years *after* election e and zero otherwise. We also include election fixed effects, γ_{ec}.[17]

Given that our primary focus is testing whether candidate race/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 (non-White winner, margin, post, and all interactions of these) with an indicator variable that equals one if the transaction occurred in a neighborhood is majority non-White and zero otherwise. Of primary interest are the coefficients on "*NonWhite wins* × *Post*," which identifies the effect of a non-White winner on housing values in White neighborhoods, and "*NonWhite wins* × *Post* × *NonWhite neighborhood*," which identifies the *differential* effect of a non-White winner on housing values in non-White 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.[18]

B. *Bandwidth Selection*

Several authors have proposed methods to identify the optimal bandwidth in a local linear RD approach (e.g., Calonico et al. 2014; Imbens and Kalyanaraman 2012). These methods balance the benefits of a narrower bandwidth (estimates drawn from observations that are close to the cutoff, increasing confidence in identifying a casual effect) with the benefits of a wider bandwidth (more observations, increasing

[17] In practice, $\mathbf{1}\{NonWhiteWins_{ec}\}$, $margin_{ec}$, and $\mathbf{1}\{NonWhiteWins_{ec}\} \times margin_{ec}$ are absorbed by the election fixed effects and are therefore not identified. We present them as part of equation (4) for illustrative purposes only.

[18] Housing characteristics are: square footage, age of the house, and fixed effects for number of bedrooms, bathrooms, and stories. Neighborhood controls are: population density, share pop. urban, race/ethnic 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 (all measured at the block-group level). Results are generally robust to the exclusion of these controls.

power). These methods would be well suited to identifying a bandwidth if one outcome was associated with each election. However, our setting involves a large number of housing transactions, occurring in various neighborhoods within a city, and where we expect effects to vary by neighborhood type. 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 election. We identify an appropriate bandwidth by collapsing our observations to the election level. For each ethnically diverse election, we take the average of ln(*adjusted housing prices*) in the two years following the election. This yields a single observation per election. We then use the Calonico, Cattaneo, and Titiunik (2014) bandwidth selection procedure, which suggests that the optimal bandwidth in our setting is 6.44 percentage points. Thus, in our main specification we include all marginal elections between a White and non-White candidate, conditional on the election being decided by 6.44 percentage points or fewer. Later, we demonstrate the robustness of our results to alternative bandwidths.

C. *Assessing the Validity of our RD Design*

The key assumption underlying basic RD designs is the continuity of both correlate densities and outcome probabilities across the treatment threshold. One concern is that non-White 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.[19] In online Appendix Figure A1, we follow McCrary (2008) and plot a discontinuous density function around the cutoff (non-White 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/disadvantage for non-White candidates in close elections. Turning to continuity of correlate densities, in online Appendix Figures A2, A3, and A4, we assess the identifying assumption that other observable characteristics behave smoothly around the cutoff. Here we see that for a wide variety of city, candidate, and housing characteristics, there are no discontinuities across the threshold. There is one important exception (panel A of online Appendix Figure A3): consistent with correlations between partisan affiliation and ethnicity in the general population, we find that non-White candidates are more likely to be a registered Democrat. While this finding is not surprising, it raises the possibility that our results are driven by partisan differences. We address this concern directly in two ways: first, by showing that our results continue to hold when we restrict the sample to close elections where both marginal candidates are of the same party (Figure 2), and in online Appendix Table A2, where we rerun our analysis on a sample of close Democrat versus Republican elections (regardless of race) and showing that the election of a Democrat does not differentially affect home values in majority non-White neighborhoods.

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

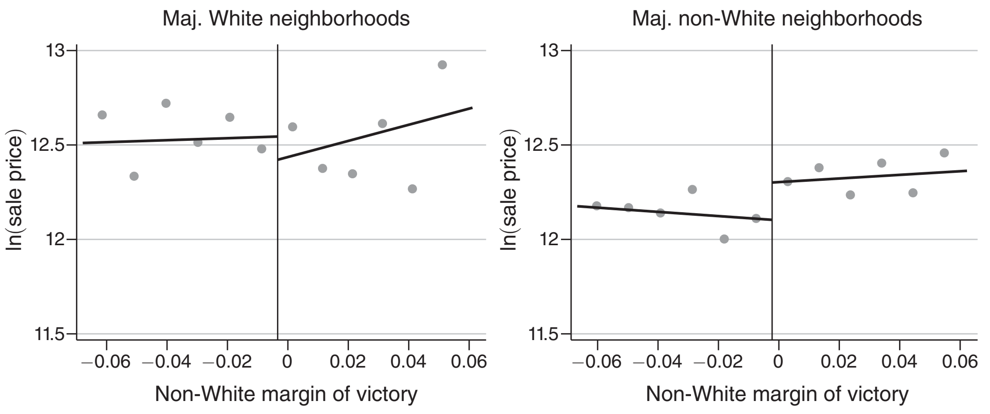

Figure 2. Cross-Sectional Assessment of a Non-White Candidate's Victory on Housing Prices by Neighborhood type

Notes: Sample restricted to transactions occurring in the two years after a close election between a White and a non-White candidate. Sale price is adjusted for inflation and market conditions, as described in Section IVC.

Because our elections involve multiple seats, it is important to consider any changes in the composition of nonmarginal winning seats. 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. Online Appendix Table A1 assesses this concern by taking the number of non-White city council members on the board as the outcome variable but otherwise running an RD specification that matches our main estimating equation. Column 1 presents cross-sectional results, while columns 2–5 present results in a panel framework that compares composition in each of the four years of the council term to the composition in the year just before the relevant election. The coefficients of interest are close to 1 and statistically indistinguishable from 1, suggesting that the narrow election of a non-White candidate is associated with a roughly one-person increase in the number of non-White members on the council. In column 6 we examine composition in year 5 (i.e., the year after the term is expected to end). This coefficient speaks to the long-run effects on council composition, which might operate through a successful or unsuccessful reelection bid or changes in the composition of other council seats. We find a point estimate that is close to zero and slightly negative, but the estimates are too imprecise to be informative (the confidence interval spans −1.3 to 1.1),

D. *Preliminary Results: Cross-Sectional RD approach*

Figure 2 offers a preliminary assessment on the differential impact of non-White city council members on majority non-White neighborhoods. The figure presents two RD plots, applying the Calonico, Cattaneo, and Titiunik (2015) procedure to our outcome of interest, ln(*adjusted sale price*). The left-hand panel examines transactions in majority White neighborhoods in the two years following the relevant election. The transactions are organized based on the non-White margin of

Table 3—Cross-Sectional RD Estimates of Council Member Ethnicity on Housing Values

DV is ln(sale price), inflation and market adjusted	(1)	(2)
Non-White winner	0.062	−0.024
	(0.035)	(0.041)
NW win × NW neighborhood		0.118
		(0.045)
Linear combo to recover effect in NW neigh.		
NW winner + (NW winner × NW neigh.)		0.094
		(0.036)
Observations	332,656	332,656
Num. cities	143	143

Notes: Robust standard errors (clustered at city level) in parentheses Sale price adjustment described in Section IVC. All regressions include city and time fixed effects, city time trends and controls for housing and neighborhood characteristics. 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. House characteristics include: square footage, age of the house, and fixed effects for number of bedrooms, bathrooms, and stories. Sample restricted to cities that experience an election between a White and non-White candidate that was decided within a 6.44 percentage point margin. Observations correspond to housing transactions occurring up to two years after the relevant election takes place. Non-White neighborhood indicator equals one if at least 50 percent non-White.

victory. The right-hand panel is similar, except that it corresponds to transactions in majority non-White neighborhoods. These figures indicate that the narrow election is associated with a slight decrease in prices in majority White neighborhoods and an increase in prices in majority non-White neighborhoods. Note that the mean sale price in majority White neighborhoods is higher, and so the election seems to be helping close that underlying disparity.

Table 3 presents formal cross-sectional RD estimates. For parsimony, we only report the coefficients that identify the causal impact of a non-White victory on housing prices. Column 1 reveals that housing prices increase by about 6 percent in cities where the non-White candidate was elected; however, column 2 shows that this effect appears to be driven by appreciation in majority non-White neighborhoods. In column 2 we see that, relative to the election of a White candidate, sale prices in majority White neighborhoods fall by an imprecisely estimated 2.5 percent following the election of a non-White candidate. In majority non-White neighborhoods, however, we see a relative increase on the order of 12 percent. Moreover, there is a positive effect on houses in non-White neighborhoods overall: the linear combination of the two coefficients suggests that non-White neighborhood housing values are roughly 9 percent higher after a non-White candidate wins (significant at the 1 percent level).

Table 4—Panel RD Estimates of Council Member Ethnicity on Housing Values

DV is ln(sale price), inflation and market adjusted	(1)	(2)	(3)	(4)
Panel A. Overall effects				
NonWht. win × *Post*	0.034	0.028	0.024	0.037
	(0.042)	(0.038)	(0.032)	(0.033)
Panel B. Effects by neighborhood type				
NonWht. win × *Post*	−0.029	−0.034	−0.032	−0.019
	(0.045)	(0.039)	(0.034)	(0.032)
NonWht. win × *Post* × *NonWht. neighborhood*	0.093	0.092	0.084	0.085
	(0.063)	(0.054)	(0.046)	(0.047)
Linear combo to recover NW neigh. effect				
(*NW win* × *Post*) + (*NW win* × *Post* × *NW neigh.*)	0.063	0.058	0.052	0.066
	(0.052)	(0.047)	(0.039)	(0.042)
Observations	688,800	688,800	688,800	688,800
Num. cities	146	146	146	146
House controls		Y	Y	Y
Neighborhood controls			Y	Y
City-level time trends			Y	Y

Notes: Robust standard errors (clustered at city level) in parentheses. All regressions include election fixed effects and time fixed effects. House and neighborhood controls follow from Table 3 (see those notes), except that they are fully interacted with the post period. Sample restricted to cities that experience an election between a White and non-White candidate that was decided within a 6.44 percentage point margin. Observations correspond to housing transactions occurring in the two years before and after the relevant election takes place. "Non-White neighborhood" equals 1 if the neighborhood is at least 50 percent non-White.

E. *Main Results: Panel-Based RD approach*

We now turn to our main results, employing a panel-based RD approach. Panel A of Table 4 identifies the causal impact of electing a non-White city council member on citywide 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 (6.44 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 ln(*sale price*), after adjusting for inflation and market conditions, 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.

Table 4 indicates that houses in majority non-White neighborhoods experience a relative appreciation following the election of a non-White council member. In panel A, average housing values increase by 3 percent in cities following the election of a non-White councilor, but the effect is imprecisely estimated. In panel B, we see that this imprecision stems from distributional effects. Across all specifications, we find that housing values in majority White neighborhoods fall by an imprecise 2–3 percent, while housing values in majority non-White neighborhoods differentially

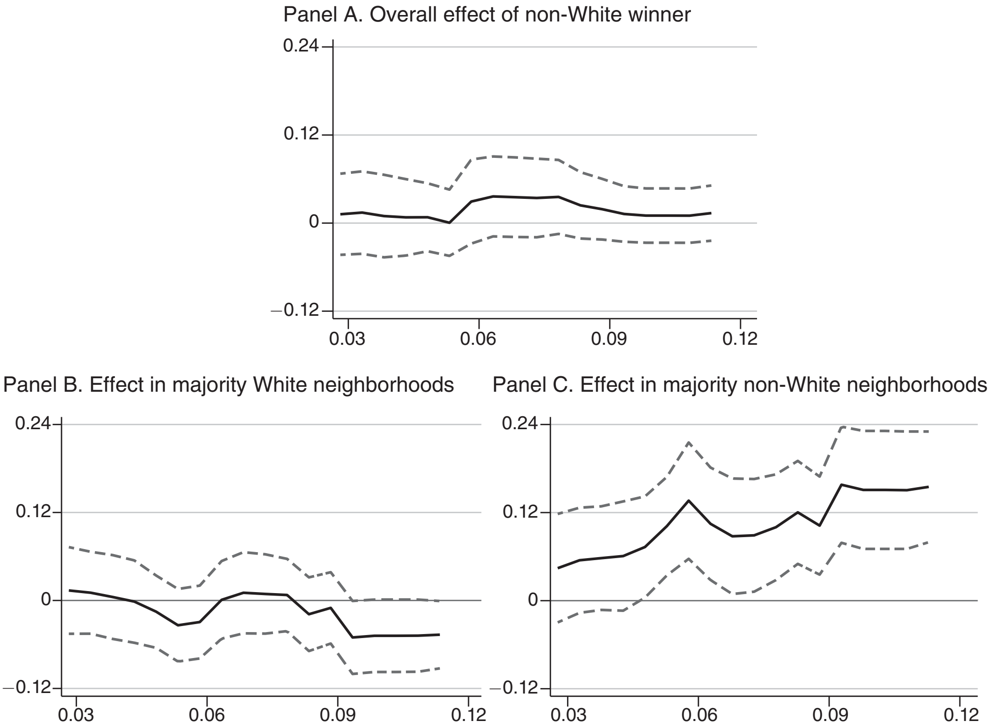

Figure 3. Assessing Robustness of Main Result to Alternative Bandwidths

Notes: These specifications mirror column 4 of Table 3. The dotted lines represent the 90 percent confidence intervals.

increase by about 9 percent. At the bottom of panel B, we recover the total effect on non-White neighborhoods by taking the linear combination of the two effects. Here we find a net price increase of about 6 percent when a non-White (rather than White) candidate is elected, although this absolute effect is less precisely estimated (the p-value is 0.12 column 4, our preferred specification). Note that these differential effects could be driven by the impact of non-White candidates, the impact of counterfactual White candidates, or some combination of the two.

F. *Robustness of Main Results*

Figure 3 examines the sensitivity of our main results with respect to bandwidth choice. The figure presents estimates that mirror column 4 of Table 4 but with varying margin of victory cutoffs. Panel A reports the primary coefficient of interest from the simpler specification that does not allow for differential effects across neighborhood types (i.e., panel A of Table 4). Panels B and C report the two main coefficients from the specification that allows for differential effects (panel B of Table 4). We reestimate these models for bandwidths ranging from 3 percentage points to 12 percentage points, in 0.5 percentage point increments. The bandwidth being used is

Table 5—Panel RD Estimates with Alternative Functional Form and Bandwidth Selection

	DV is ln(sale price), inflation and market adjusted				
	CCT			CV	IK
	(1)	(2)	(3)	(4)	(5)
Panel A. Overall effects					
NonWht. win × *Post*	0.019	0.037	0.022	0.032	0.008
	(0.020)	(0.033)	(0.035)	(0.031)	(0.027)
Panel B. Effects by neighborhood type					
NonWht. win × *Post*	−0.023	−0.019	−0.029	−0.016	−0.037
	(0.023)	(0.032)	(0.040)	(0.031)	(0.028)
NonWht. win × *Post* × *NonWht. Neigh.*	0.070	0.085	0.063	0.073	0.068
	(0.032)	(0.047)	(0.056)	(0.044)	(0.039)
Observations	500,499	688,800	910,867	737,032	590,564
Num. cities	122	146	162	151	131
Bandwidth	|4.35|	|6.44|	|9.26|	−6.67 to 8.33	|5.17|
Polynomial	0	1	2	1	1

Notes: Robust standard errors (clustered at city level) in parentheses. All regressions include election fixed effects, time fixed effects, city time trends, and the set of neighborhood and housing controls used in column 4 of Table 4 (see that table for full description). Note that the polynomial refers to our modeling of the margin-of-victory running variable. Observations correspond to housing transactions occurring in the two years before and after the relevant election takes place. "Non-White Neighborhood" equals 1 if the neighborhood is at least 50 percent non-White.

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 (dashed gray lines). We consistently find that the election of a non-White candidate helps reduce the preexisting gap in house prices between majority White and majority non-White neighborhoods (panel C), albeit with varying precision.

Table 5 shows that our main results are also robust to alternative functional forms and bandwidth selection procedures. Our main results model our running variable, non-White win margin, linearly. In column 1 of Table 5 we present results without this linear trend, bringing us closer to a difference-in-differences specification. Column 2 is our main result, and column 3 models the running variable with a quadratic. Note that the number of observations changes across columns 1 to 3 because the bandwidth procedure recommends a different bandwidth depending on polynomial choice. In column 4 we present results using cross validation (which allows the bandwidth to vary on either side of the threshold), and in column 5 we present results with the Imbens and Kalyanaraman (2012) optimal bandwidth. The results continue to point to a price decline in majority White neighborhoods on the order of 1.5 to 3.5 percent and a relative increase in majority non-White neighborhoods on the order of 7 to 8.5 percent.

Next, we present results from a wider set of years, which allows us to examine the dynamics of our effect. We do this within an "event study" framework, where we allow the treatment variables of interest ("*NonWht. Win*" and "*NonWht. Win* × *NonWht. Neigh.*) to interact with several period indicators. These results appear in Figure 4. There we see little evidence that house prices in majority non-White neighborhoods

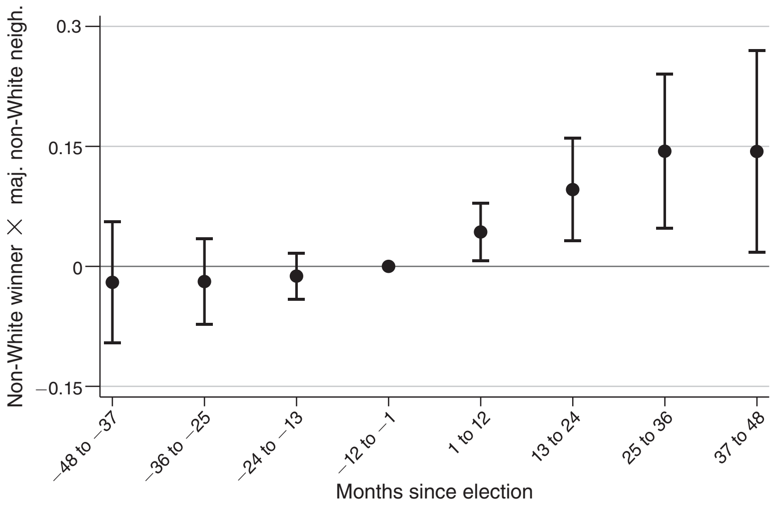

Figure 4. Event Study Estimates of Our Treatment Effect

Notes: Point estimates obtained by modifying column 4 in panel B of Table 4 to interact "NonWht. Win" and "NonWht. Win × NonWht. Neigh." with period a series of period indicators. The sample is expanded to include the four years preceding and following the relevant election. Ninety percent confidence intervals with standard errors clustered at the city level are depicted in the figure.

were trending up prior to the election of a non-White city councilor. Following the election, we see relative appreciation in the first year that is smaller (but statistically indistinguishable) than the second year. Council composition is subject to change in years 3 and 4, as that is when the remaining council seats are up for election. Despite this, we see little evidence of mean reversion, suggesting that our estimated effects were not quickly reversed.

Having established robustness to alternative functional forms, we now examine the sensitivity of our results by imposing various sample restrictions. These results are presented graphically in Figure 5. For the sake of comparison, we begin by displaying our main coefficient estimates, corresponding to the estimates reported in column 4 in panel B of Table 4; the white bar represents the impact of a non-White candidate victory in majority White neighborhoods and the shaded bar represents the differential impact in majority non-White neighborhoods.

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 non-White candidates being more likely to be registered Democrats. If our main result were driven by the fact that White versus non-White elections often imply Republican versus 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. In online Appendix Table A2 we analyze close elections that lead to the addition of a Democrat council member (relative to a Republican) and show that this

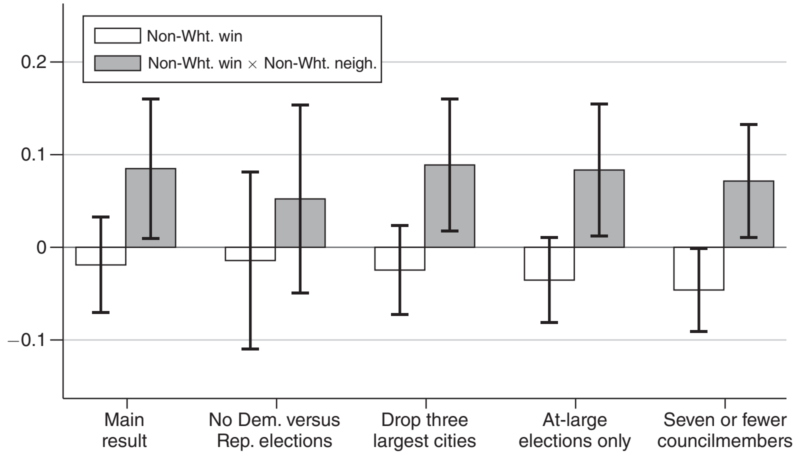

Figure 5. Assessing Sensitivity of Panel RD Results

Notes: Each pair of bars presents the relevant coefficient estimates from specifications that follow Table 4, panel B, column 4 (see the notes of Table 4 for a full list of controls). The figure depicts coefficients (and 90 percent confidence intervals) from the "Post × NonWht. win" and "Post × NonWht. win × NonWht. BG" coefficients.

does not lead to a differential appreciation in majority non-White neighborhoods, casting doubt on the idea that our results are driven by partisan preferences.

The remainder of Figure 5 reports three additional sensitivity tests: dropping the three largest cities in our sample; dropping cities with district-based elections; and dropping 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 results. District-based elections are more likely to produce descriptive representation in local elections (Abott and Magazinnik 2020), so our results could simply reflect the fact that council members generate benefits for their own districts, which happen to match their race/ethnicity. When we restrict to at-large elections in Figure 5 we see little movement in our estimates, consistent with the small number of district-based elections in our sample.

As late as 2010, fewer than 10 percent of cities held district-based city council elections. Driven by the California Voting Rights act there was a movement toward district-based elections[20] and by 2019 more than 25 percent of cities used district-based election procedures. In online Appendix Table A3 we present results that extend our sample of elections through 2018. This analysis requires the use of an aggregated measure of housing values and a coarser neighborhood definition, which is why we only present this analysis in the online Appendix. Nevertheless, Table A3 indicates that our effects, particularly in the post 2011 period, are coming from non-White candidates that were elected through at-large elections.

[20] See Appendix Figure A-1 from Hankinson and Magazinnik (2020) for evidence of the dramatic increase in district-based elections in California in the past ten years.

VI. Mechanisms

In this section we examine the mechanisms underpinning our main effects. We begin by benchmarking our findings to the broader hedonic literature, which helps establish that our magnitudes are consistent with a range of plausible policy changes. Then we show that our results are operating through realistic channels, namely that the results are more pronounced when the non-White candidate is a pivotal voter and in more ethnically segregated cities, where the scope for spatial distribution is highest. We then rule out gentrification as a potential channel, which boosts our confidence in interpreting our results as a relative welfare improvement for residents of non-White neighborhoods. Finally, we examine some specific policies and outcomes that may partially drive our housing market results; with some exceptions, the results are generally imprecisely estimated, consistent with the notion that different cities face different challenges and will therefore prioritize different policy outcomes.

The hedonic literature suggests that a number of policy changes would be consistent with the effects documented thus far. Turner, Haughwout, and Van Der Klaauw (2014), for instance, show that a 1 standard deviation increase in land-use regulation intensity (e.g., permit waiting times, the number of entities needed to approve a new project or a zoning change, and perceived political pressure) lowers land values by about 38 percent. On the role of policing, Albouy, Christensen, and Sarmiento-Barbieri (2020) show that houses located near parks that are perceived to be safe sell for a 5 percent premium, but that premium declines as crime increases. Chay and Greenstone (2005) show that a 12 percent decrease in TSP increased house prices by about 2.5 percent. More recently, Davis (2011) shows that proximity to power plants decreases housing values by 4–7 percent, while Gamper-Rabindran and Timmins (2013) find that, depending on initial exposure, house prices appreciate by 18–25 percent following the cleanup of hazardous waste sites.

One mechanism that is not plausible in our setting is changes in schooling. California city councils do not oversee local educational decisions. Nevertheless, for the purpose of bounding our effects, the evidence suggests that small changes in school amenities also generate large responses from housing markets. Black (1999) compares house prices on each side of school attendance boundaries and finds that a 5 percent increase in average test scores generates a 2.5 percent increase in house prices. Another experiment is court-ordered desegregation, which manipulated peer composition and resources. Boustan (2012) finds that housing values in cities that were placed under court-ordered desegregation fell by 12 percent relative to cities that did not face similar court orders.

The above results suggest that there are a range of policies that a city councilor might affect that could explain our findings. We find that the impact of the narrow election of a non-White candidate (relative to a White candidate) on White neighborhoods ranges from -3.5 to -1.5 percent, while the impact on non-White neighborhoods ranges from about 3 percent to 6.5 percent.[21] These effects would be

[21] The differential effect, which speaks to relative improvements, mechanically reads higher (6.3 to 8.5 percent).

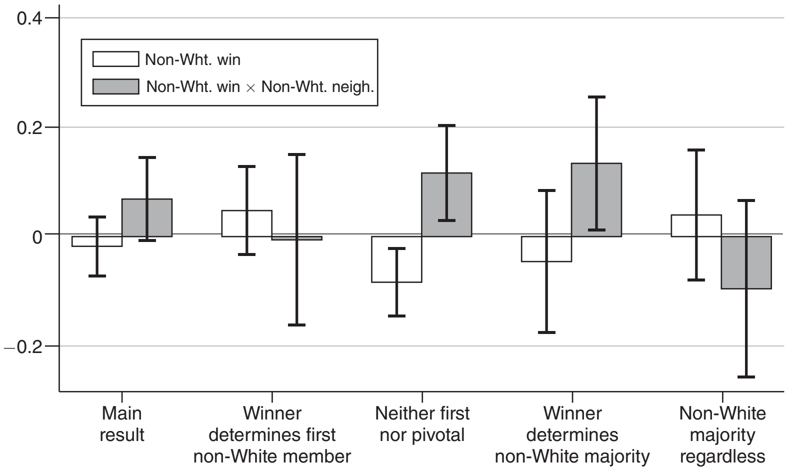

Figure 6. Heterogeneity in Impact of Non-White Victory by Composition of Rest of Council

Notes: Relative to earlier results, this sample is further restricted to cities where we know the ethnicity of every council member. Each pair of bars presents the relevant coefficient estimates from specifications that follow Table 4, panel B, column 4 (see the notes of Table 4 for a full list of controls). The figure depicts coefficients (and 90 percent confidence intervals) from the "Post × NonWht. win" and "Post × NonWht. win × NonWht. BG" coefficients.

consistent with a councilor helping lower the costs associated with starting a new business or housing development, reallocating policing resources, or helping regulate environmental disamenities. To the extent that council members are responding to unique challenges and circumstances, it is reassuring that there are many hedonic estimates that are consistent with our findings.

A. *Pivotality*

Figure 6 examines how the election of a non-White candidate interacts with the ethnic composition of the other council members. To explore this issue, we reestimate our main specification on four mutually exclusive subsamples based on the preexisting composition of the rest of the council: (i) councils where the non-White candidate would become the first non-White member on the council, (ii) councils where the non-White candidate would not be the first non-White member but the council would remain majority White even with the election of the non-White candidate, (iii) councils where the non-White candidate is "pivotal"—his or her election would shift the council from majority White to majority non-White, and (iv) councils where there would be a non-White majority regardless of whether the non-White candidate is elected. We observe strong impacts of non-White wins in cases where the non-White candidate is pivotal and when the non-White candidate is nonpivotal but is also not the first non-White member of council—suggesting that the impact of descriptive representation may hinge on the presence of a certain critical mass. Indeed, this is consistent with a substantial theoretical and empirical

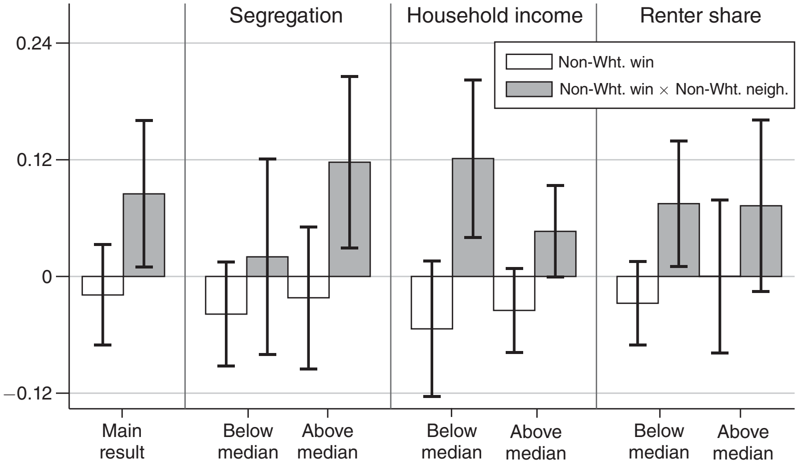

Figure 7. Heterogeneity by City and Neighborhood Characteristics

Notes: Each pair of bars presents the relevant coefficient estimates from specifications that follow Table 4, panel B, column 4 (see the notes of Table 4 for a full list of controls). The figure depicts coefficients (and 90 percent confidence intervals) from the "Post × NonWht. win" and "Post × NonWht. win × NonWht. BG" coefficients.

literature in political science, primarily studying women's representation (see, e.g., Funk, Paul, and Philips 2022).[22]

B. *Segregation*, *Income*, *and Home Ownership*

An important mechanism for explaining our results is the possibility that a non-White candidate wins and directs resources and services toward non-White neighborhoods. This channel is likely most effective in segregated cities where there are obvious non-White neighborhoods to direct resources toward. Similarly, as discussed in Section II, the impact of policies directed toward non-White individuals should have the largest measurable impacts in more segregated neighborhood.[23] Figure 7 presents results consistent with these conjectures: when we split the sample based on whether the election occurs in a city with above-median ethnic segregation, we see that our results are largely driven by more segregated cities.[24]

[22] That literature posits that an underrepresented group must meet some threshold before they are able to substantively represent their group due to being "tokenized" and ignored when in very small numbers. The threshold that is theorized, and confirmed empirically, where the underrepresented legislators begin to have an impact is, as in our setting, below 50 percent, as the legislators can begin to have an impact on debate and agenda setting even if their group is not pivotal in votes. Separately, note that race representation has been found to have an impact in other group decision-making contexts—juries—even when that group is not in a majority in the decision-making body (Anwar et al. 2012). The effect in that work operates through preventing the inclusion of a group member most likely to act *against* the underrepresented group's actions, which could potentially be at play in our setting.

[23] Ananat and Washington (2009) lay out why segregation has theoretically ambiguous effects on political efficacy and provide empirical evidence to increased racial segregation lowers Black political efficacy.

[24] We use the typical two-group dissimilarity index as our measure of diversity, with the two groups in question being White and non-White. Online Appendix Table A4 provides summary statistics for above- and below-median segregation cities. More ethnically segregated cities are more populous and have larger Hispanic shares but overall

Table 6—Assessing Neighborhood Turnover

Dependent variable:	ln(Predicted Price) (1)	ln(Transactions) (2)	ln(Evictions) (3)	ln(Eviction filings) (4)
Panel A. Overall effects				
NonWht. win × Post	0.001	−0.083	−0.028	0.015
	(0.003)	(0.085)	(0.062)	(0.063)
Panel B. Effects by neighborhood type				
NonWht. win × Post	0.003	−0.109	−0.005	0.047
	(0.008)	(0.134)	(0.080)	(0.077)
NonWht. win × Post	−0.004	0.044	−0.037	−0.048
× NonWht. Neigh.	(0.011)	(0.118)	(0.076)	(0.074)
Observations	45,927	45,931	24,866	25,440
Num. cities	146	146	125	125

Notes: Robust standard errors (clustered at the city level) in parentheses. Observations are at the block-group-by-year level. All regressions include election and time fixed effects. Columns 1 and 2 also include our neighborhood controls, as defined in Table 4. Predicted prices account for inflation, market conditions, and housing characteristics. Price and transaction data are from DataQuick. Eviction data are from the Eviction Lab (Desmond et al. 2018).

Given correlations between neighborhood racial/ethnic composition and income, is it 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 non-White share and lower non-White 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-council-member ethnicity and these characteristics. The remaining panels of Figure 7 show results when we split the sample to focus on neighborhoods with above/below-median income (third panel) and above/below-median renter shares (fourth panel). There we see results that are qualitatively similar to our main results, suggesting that our main results are not driven by distributional shifts toward lower-income neighborhoods or high-renter-share neighborhoods rather than neighborhoods with higher non-White shares.

In Table 6 we assess whether the increased housing prices we observe are driven by gentrification, which would impact the interpretation of our results as evidence of a relative welfare improvement. Here we ask whether there is a change in the type of house that is being sold, the volume of transactions that are occurring, the number of evictions completed, and the number of eviction filings. To maintain comparability across columns, observations are at the block-group-by-year level, which is the finest level at which we can obtain eviction data. Note that predicted prices are obtained from a regression that accounts for market conditions and inflationary pressure, and so a change here would reflect a change in the type of housing that is being sold. We see little evidence to support a narrative in which residents of majority non-White neighborhoods are being pushed out following the election of a non-White council member. There is no meaningful change in the type of housing

have similar levels of ethnic diversity. The elections occurring in segregated cities are more likely to be district based, although we hesitate to read too much into this institutional feature since the robustness checks in Figure 5 showed that our results continue hold when we discard district-based elections.

Table 7—Effects of Council Member Ethnicity on Local Economic Development

Dependent variable:	ln(Estab.) (1)	ln(Emp.) (2)	Estab. pc (3)	Emp. pc (4)
NonWht. win × Post	−0.043	−0.075	−0.001	−0.021
	(0.029)	(0.038)	(0.001)	(0.011)
NonWht. win × Post × NonWht. zip	0.164	0.298	0.004	0.062
	(0.101)	(0.129)	(0.002)	(0.032)
Mean	6.432	8.991	0.023	0.331
Num. cities	140	140	140	140
Observations	1,589	1,589	1,589	1,589

Notes: Robust standard errors (clustered at city level) in parentheses. All specifications are restricted to elections between White and non-White candidates decided by a margin of 6.44 percentage points or less. Regressions include election fixed effects, year fixed effects, and zip code-level equivalent of the neighborhood controls described in Table 4. Table displays coefficients capturing the causal impact of non-White candidate victory and suppresses other coefficients (e.g., non-White margin of victory).

being sold (column 1), and, if anything, transactions and evictions are falling after the relevant election (columns 2–4).

C. *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 White and non-White individuals for two reasons. First, from a theoretical perspective, they offer an "index" that allow 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 are systematically available and disaggregated to the neighborhood level limits what we can measure directly. There are, however, some noted exceptions to this second point that 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, 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 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 (ZBP) data. These data report the number of business establishments and employees by zip code on an annual basis. We estimate a panel RD model similar to our main specification, taking ZBP data as our outcomes. Results are reported in Table 7. We find that the election of a non-White council member differentially increases the logged number of establishments (column 1) and employees (column 2) in majority non-White zip codes. However, we highlight several reasons for caution in interpreting these results. First, the magnitudes are quite large, with the estimates suggesting an 16 percent increase in employment in non-White neighborhoods. Second, taking outcomes in per capita terms rather than logs (columns 3 and 4) suggests a similar but much less precise pattern of results. Finally, we have separately analyzed the impacts of non-White representation on

Table 8—Effects of Council Member Ethnicity on Policing and Crime

	DV is per capita				
	Police spending (1)	Total arrests (2)	Reported offenses (3)	Clearance rate (4)	NW arrest share (5)
NonWht. winner × Post	−3.501	0.001	0.001	0.010	−0.021
	(11.013)	(0.002)	(0.001)	(0.016)	(0.012)
Mean	312.251	0.045	0.043	0.196	0.688
Num. cities	118	118	118	118	118
Observations	619	620	620	620	620

Notes: Robust standard errors (clustered at the city level) in parentheses. All specifications are restricted to elections between White and non-White candidates decided by a margin of 6.44 percentage points or less. Regressions include election fixed effects and year fixed effects. The table displays coefficients capturing the causal impact of non-White candidate victory and suppresses other coefficients (e.g., non-White margin of victory).

employment at the tract-by-year level using LEHD Origin-Destination Employment Statistics data (Urban Institute 2022). Those results (online Appendix Table A5) also point to a differential increase in employment, but the effect is substantially smaller and statistically insignificant. As such, we conclude that there is suggestive, but not definitive, evidence of an increase in business activity in majority non-White neighborhoods.

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 are 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 take as our outcome variable an indicator for whether a TRI facility is operating within each tract-year pairing or whether a new TRI facility opened. Results are in online Appendix Table A6. We observe no significant effects either on average or by neighborhood type, suggesting that any increase in business activity was not associated with an increase in environmental threats faced by local residents. If anything, there is some evidence of a decrease in likelihood of a new TRI facility in non-White tracts, though that estimate—while economically meaningful—is not statistically significant.

Policing was also a common focus of non-White 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 race/ethnic group. These results appear in Table 8.

Columns 1–4 test for changes in general levels of policing and/or crime. We observe no significant changes in police spending per capita (column 1), total arrests (column 2), reported offenses (column 3), or the clearance rate (column 4), which is the number of "resolved" reported offenses (e.g., by arrest) divided by total offenses. In short, paralleling our results elsewhere in the paper, there is no impact of an additional non-White member on council on citywide levels of policing activity. However, in column 5 we do see that the *distribution* of policing shifts: the non-White share of arrests falls by 2.1 percentage points, significant at the 10 percent level.

We also have access to data on a broad range of citywide 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, race/ethnic group). Nonetheless, in online Appendix Table A7 we present results across a range of fiscal outcomes: expenditures, revenues, spending on public goods, safety, transportation, etc. Across each of the eight categories considered, the estimated effect of electing a non-White 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 non-White council members is not correlated with some other broad realignment 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 toward non-White neighborhoods/residents.

In addition to aggregate revenue and spending categories, online Appendix Table A8 explores the impact of electing a non-White council member on propensity to adopt revisions to city planning documents, and online Appendix Table A9 examines impacts on aggregate building permit activity. In both cases, we find no evidence of an impact on these city-level measures, which is perhaps expected, as our central finding points to a *distributional* shift in local amenities.

VII. Conclusion

In this paper, we use data from California to study the impact of racial/ethnic representation in local government on outcomes for otherwise underrepresented individuals and the neighborhoods in which they live. Our empirical strategy is twofold. First, we use fine-scale spatial variation in the evolution of housing prices across White and non-White 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 White and non-White candidates as a source of quasi-random variation in treatment. We find that, relative to the election of a White candidate, the election of a non-White candidate serves to offset preexisting gaps between non-White 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 correlations between the racial and income composition of neighborhoods.

Additional analysis points to how these distributional shifts occur. We find that the impact of an additional non-White candidate depends on the preexisting composition of the council that he or she enters. We observe the strongest effects when the non-White candidate helps form a voting block. In contrast, the first non-White candidate on council has no observable impact on housing prices, nor does a non-White candidate entering a council that is already majority non-White. This result suggests that in the absence of a majority, a submajority critical mass of non-White council members can have a marked impact on outcomes. In terms of specific channels of impact, we find that the election of a non-White candidate increases business activity in majority non-White neighborhoods and leads to shifts in arrest patterns away from non-White residents.

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