The Supply-Equity Trade-off: The Effect of Spatial Representation on the Local Housing Supply

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Abstract

A central concern of governance is how the costs and benefits of collective goods are distributed over the population. While the institutions that structure spatial representation vary widely across U.S. municipalities, the distributive consequences of local electoral rules have not been adequately studied through a spatial lens. We leverage the California Voting Rights Act of 2001, which compelled over one hundred cities to switch from at-large to single-member district elections for city council, to causally identify how equalizing spatial representation changes the permitting of multifamily housing—the type most vehemently opposed by current residents, yet most essential to an affordable housing supply. District elections decrease the supply of new housing, but also end the disproportionate channeling of new housing into minority neighborhoods. Taken together, our findings speak to a trade-off inherent to spatial representation: the supply of collective goods and the equitable distribution of the associated costs.

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A central concern of governance is how the benefits and costs of collective goods are distributed over the population. But, many collective goods—e.g. public parks, transit hubs, or affordable housing—are bound to a physical location, meaning their benefits or costs are unavoidably spatially concentrated. The inherent difficulties of resolving conflict over the provision of spatial goods suggests a role for the democratic process (Valentini, 2013). And for this process to produce equitable outcomes, representation must be accessible across competing geographic constituencies. The stakes of geographic representation are particularly high in the American political context, where entrenched racial and economic disparities in political power have been constructed by, and in turn reconstructed, legacies of segregation (Trounstine, 2018; Soja, 2010). Thus, beyond being important in its own right, the distribution of spatial representation has the potential to reinforce or remedy existing disadvantage.

A particular instance of this spatial allocation problem concerns land uses that society needs, but few people want nearby. Known as locally unwanted land uses, 'LULUs' can range from new housing (Hankinson, 2018), to energy facilities (Stokes, 2016), to drug addiction treatment clinics (de Benedictis-Kessner and Hankinson, 2019). Because LULUs are perceived to threaten the property values, safety, or general quality of life of nearby residents, they have historically been channeled into the politically weakest areas. In response, efforts to increase equity often involve amplifying the spatial representation of these areas, increasing their ability to block the siting of the LULU. But repeated obstruction can lead to an undersupply over time. For LULUs with spatially diffuse benefits but significant value, such as an affordable housing supply, this undersupply can exacerbate inequality in the long run.

The importance of spatial representation in this *supply-equity trade-off* is potentially most salient in local politics. Municipal governments typically control the siting of LULUs, with political conflict over these decisions operating more along spatial rather than ideological dimensions (Marble and Nall, Forthcoming). Moreover, the institutions that structure spatial

representation differ across U.S. municipalities, allowing us to causally identify their effects. In this paper, we focus on a key feature of electoral institutions affecting the relative influence of geographic constituencies: how votes are aggregated into city council seats. Voters may be pooled into one large, multi-member district, with each citizen voting for several candidates ('at-large elections'). Or, they may be assigned to smaller, single-member districts, with each citizen voting for only one candidate ('district elections').[1] While both institutional forms aggregate the preferences of an identical voting population, they produce different constituencies for elected officials, with the former beholden to the population as a whole and the latter primarily to the voters in their district.

Of course, in reality, the constituency of the at-large legislator is not always the city as a whole. Elected officials are most responsive to those who participate, generally meaning wealthier, more highly educated, white voters, and this participation gap is exacerbated in local elections (Hajnal and Trounstine, 2005). Moreover, voters of similar socioeconomic status are spatially clustered. Consequently, at-large councils tend to overrepresent specific geographic areas, effectively redefining a council member's citywide constituency into one consisting of several high-turnout neighborhoods. When it comes to siting LULUs, council members are more likely to cater to these neighborhoods' preferences, blocking LULUs in politically powerful areas and channeling them into weaker ones.[2]

Our analysis highlights how moving the spatial scale of representation from at-large to districts tilts the balance away from supplying necessary LULUs and toward equitable cost-bearing. In district-elected legislatures, representatives are empowered to keep unwanted LULUs out of their districts by the institution of "aldermanic privilege," or simply universal logrolls. Because local governments generally lack partisan competition and the attendant organization that comes with competitive parties (Schaffner, Streb, and Wright, 2001), legislators struggle to resolve intertemporal bargaining breakdowns and prisoner's dilemma-style

[1]District representation is known by many names, including "by-trustee" and "ward." For clarity, we use the term "district" throughout.

[2]More likely, LULUs are not even proposed in the powerful neighborhoods, preventing researcher observation.

problems (e.g., Cox and McCubbins, 2005, 2007; Kiewiet and McCubbins, 1991; Weingast, 1979). What has emerged instead is the prevailing norm that legislators exercise outsized control over issues specific to their own jurisdictions (Burnett and Kogan, 2014; Schleicher, 2013). With this norm in place, the host legislator's opposition is all it takes to prevent LULUs from being sited in their district, even if the rest of the council favors it. Consequently, logrolls may simultaneously enhance distributive equity via greater neighborhood control while also hampering the long run supply of collective goods.

Of all possible LULUs, the permitting of new housing is perhaps ideal for measuring the effect of voter aggregation on the supply-equity trade-off. First, new housing is often viewed as a LULU by those living nearby due to the noise, traffic, loss of open space, change of aesthetics, and fear of new residents that the housing will bring (Einstein, Palmer, and Glick, 2019). As a result, housing is frequently blocked by politically powerful neighborhoods and channeled towards politically weak ones, creating an inequity in the distribution of housing's spatially concentrated costs. But new housing is necessary to preserve the affordability of a city, as constricting supply causes housing prices to rise even faster (see Been, Ellen, and O'Regan, 2019, for review). Thus, the siting of new housing acutely faces the supply-equity trade-off. Second, housing permitting is controlled almost exclusively by municipal governments, which are numerous and divided between at-large and district elections for city council. Third, housing permits are issued continually, providing far more observable outcomes than the extremely rare siting of new landfills or other LULUs.

In this paper, we show that voter aggregation affects the housing supply in two ways. First, the switch to district elections causes a 48 percent decrease in the permitting of multifamily housing—the type of housing most vehemently opposed by current residents but also most essential to an affordable housing supply. This decrease in supply supports our theory that district elections foster logrolls. Because at-large elections do not guarantee each neighborhood representation in city council, some neighborhoods are left out of the LULU-blocking voting coalition. Thus, at-large city councils are more able to channel unwanted

housing into unrepresented, politically weak neighborhoods. In contrast, district elections by definition distribute representation more evenly. With no spatially unrepresented neighborhoods, district-elected city councils face political pushback to housing throughout the city, causing fewer new units to be permitted citywide.

Second, we find that the switch to district elections ends the disproportionate channeling of new housing into minority neighborhoods, causing cities to more equally distribute new housing between their white and minority constituencies. Because at-large systems are more likely to underrepresent minority voters (Abott and Magazinnik, 2020; Collingwood and Long, 2019; Meier et al., 2005; Trounstine and Valdini, 2008), unwanted housing is more likely to be concentrated in minority neighborhoods, all else equal. When district elections empower neighborhood-level interests, they primarily amplify the voice of minority neighborhoods, as white neighborhoods are already represented by at-large coalitions. No longer able to channel housing into politically weak, minority neighborhoods, district-elected councils more evenly distribute new housing across neighborhoods—and consequently demographic groups.

Our findings are rare causal estimates of the effect of electoral institutions, as they so rarely change over time, and are subject to extreme selection bias when they do. We leverage the California Voting Rights Act of 2001 (CVRA), which spurred city councils to switch from at-large to district elections but introduced some conditionally random variation in the timing of these reforms. First, we use city-level panel data to measure the effect of conversion to districts on the amount and structural composition of new housing units permitted annually. Second, we use an original, 8-year panel dataset of geocoded housing approvals across six cities to capture the effect of district elections on the spatial distribution of new housing.

Framed as a supply-equity trade-off, a decrease in the supply of new housing threatens equity on the local and global scale. Limiting new housing not only raises the rents of existing residents, but also prices out those seeking to move to cities with high upward income mobility (Chetty et al., 2014). Supply restrictions thereby exacerbate income inequality (Ganong and

Shoag, 2017) and entrench existing patterns of racial segregation (Trounstine, 2018). Absent the large-scale production of subsidized housing, rising prices from a further constrained supply will disproportionately harm low-income communities, a constituency that district elections were meant to empower. We close with how to balance descriptive representation, distributive equity, and the necessary supply of housing with lessons for other policies with concentrated costs and diffuse benefits.

The Spatial Scale of Representation

Inherent in the politics of spatial goods are the incentives of elected officials. In pursuit of reelection, representatives strive to meet the needs of their constituencies. Even if legislating on the same policy questions for the same population, elected officials are expected to behave differently should their constituency within that population change. Possibly the most extreme change in constituency occurs when legislative bodies switch from multi-member, at-large elections to single member, district elections. While single and multi-member districts exist in assorted forms internationally, variation at the same level of government is perhaps most prevalent in the structure of American city councils. As of 2012, approximately 64 percent of American municipalities relied on at-large voting for their city council elections, whereas 14 percent used district elections, with the remaining 22 percent utilizing some form of hybrid systems (Clark and Krebs, 2012).[3]

This city-level variation largely stems from the early 20th century, when municipal reformers sought to counter the influence of machine-style politics via at-large electoral systems (Trounstine, 2009). Theoretically, at-large elections would produce city council members interested in the outcomes of the city as a whole, rather than the parochialism and patronage politics of their district. In practice, by expanding the scope of conflict to the city level, at-large elections allowed citywide coalitions to dominate. So long as the city maintained a majority white population, this white coalition could prevent the descriptive representation

[3]We use the term "city" throughout to describe any incorporated municipality.

of its minority citizens.

This use of institutional design for disenfranchisement did not go unnoticed. Section 2 of the Voting Rights Act of 1965 (VRA) specifically prohibits any "voting qualification or prerequisite to voting or standard, practice or procedure" meant to discriminate on the basis of race. After challenging direct impediments to black voter registration, civil rights advocates began using Section 2 to target Southern cities with at-large elections (Issacharoff, 1991). Though successful litigation was limited by a high standard of proof, Southern cities that converted to district elections in the wake of the VRA did experience increased minority representation (Sass and Mehay, 1995).

The post-VRA shift to district elections also affected policy outcomes. Southern cities that switched to district elections in the wake of the VRA generated higher pension benefits while simultaneously lowering funding for those benefits and decreasing investment in city infrastructure (Boylan and Stevenson, 2017). In other words, district-elected leaders were more likely to 'time-shift' expenditures, burdening future citizens with policy costs.

Other work has found weaker institutional effects. Looking at how voter preferences affect municipal policy outcomes, Tausanovitch and Warshaw (2014) find little evidence of a moderating effect of at-large versus district elections. However, they do not investigate outcomes or preferences linked to land use or distributional policies, an omission motivating our research in two ways.[4] First, land use is widely considered the primary policy domain of local politics, one almost exclusively controlled by the municipal government (Peterson, 1981). Second, whereas Tausanovitch and Warshaw (2014) compare the ideology of citizens to the ideological placement of policy outcomes, local housing policy has been found to lack a strong ideological dimension (Marble and Nall, Forthcoming). Together, the central importance of land use in local politics and its orthogonality to ideology call for a direct examination of the effect of institutions on these policy outcomes.

Similar to Boylan and Stevenson (2017)'s 'time-shifting' of public expenditures, district-

[4] "Finally, research in this area could benefit from examining a broader range of city policy outcomes, such as distributional or land development policies" (Tausanovitch and Warshaw, 2014, 621).

elected representatives are incentivized to 'spatially shift' burdensome LULUs out of their own district. Theoretically, were a LULU in the city's collective interest, the remaining $n-1$ council members would vote in favor of the siting proposal. However, this collective outcome of more housing is threatened by the iterated nature of city council voting. A form of legislative logrolling, city council members often defer to the preferences of the council member representing the host neighborhood. This local deference is repaid to each member in future siting decisions, allowing them to survive the political threat of a LULU when it is proposed for their own district (Burnett and Kogan, 2014; Schleicher, 2013).

With each neighborhood able to block new development, district-elected cities struggle to permit new housing compared to their at-large peers. Cross-sectional studies of local institutions support this theory, finding district elections associated with decreased permitting of single family homes (Lubell, Feiock, and De La Cruz, 2009), increased use of growth management regulation (Feiock, Tavares, and Lubell, 2008), greater restrictions on the siting of group homes (Clingermayer, 1994), and a weakened influence of the construction industry on permit approval times (Deslatte, Tavares, and Feiock, 2018). Most closely related to our own work, Mast (N.p.) finds that cities that adopted district elections between 1980 and 2018 experienced a 24% decline in housing units permitted annually. But while Mast (N.p.) uses a nationwide sample of cities who chose to adopt district elections, we examine cities that converted to district elections due to exogenous legal pressures. As an additional advance, we also measure changes in the spatial distribution of new housing, directly capturing the equity implications of the reform.

Importantly, our theoretical mechanism of interest—equalizing spatial representation—need not rely on changes in descriptive representation, which has occupied much of the recent scholarship on district elections.[5] Even without replacement, LULU politics incentivize all council members to block concentrated costs from their newly formed districts, which means districts can empower minority neighborhoods regardless of the race or socioeconomic

[5]See Marschall, Ruhil, and Shah (2010) for a review of the extensive literature on black representation alone.

background of their representatives. Our work therefore identifies an understudied set of distributional considerations that are distinct from the important, but better understood, descriptive consequences of district elections. That said, the interaction between race and LULU politics is a fruitful avenue for future work.

The Political Economy of Zoning

To show how electoral institutions activate housing's supply-equity trade-off, we detail the political process of housing approvals as well as public attitudes towards different types of housing. In the U.S., proposals for new development travel through one of two paths: 'by right' and discretionary review. By right proposals are allowed under existing regulations. For example, if a developer wants to build a 6-unit apartment building in an area zoned for up to 6 units of multifamily housing, that developer's application simply needs to meet the required building standards and codes. As a result, the 6-unit project is largely insulated from political pressure that could either downsize or even block the proposal.

However, if the developer wants to exceed the allowable capacity of the lot by building a 12-unit apartment building on that same parcel, her application will be subject to discretionary review by the city's planning commission and, occasionally, the city council. Review begins with a public hearing where any resident is allowed to speak for or against the proposal. After deliberation, members of the legislative body vote whether to approve the project by granting a zoning amendment. This discretionary review opens the permitting process to political demands, with voters directly pressuring members of city council.[6]

Like any regulatory regime, the discretionary review of housing proposals generates its own political economy. But unlike the distributive boon of pork barrel politics, new housing is usually seen as a distributive burden to nearby residents. Development brings noise and congestion, harming quality of life. New residents often consume more in public services than

[6]In California, members of the planning commission are also vulnerable. Not only are they appointed by city council, but their zoning decisions may be appealed to city council, effectively keeping their verdicts in line with council preferences.

they provide in tax revenue, raising the tax burden of existing property owners (Hamilton, 1976). Biases against social or racial outgroups may cause current residents to be wary of new neighbors, especially if those neighbors are of lower economic standing (Charles, 2006). These threats to property values lead homeowners in particular to oppose new housing in favor of the status quo (Fischel, 2001).

Still, housing preferences are not uniform, but vary based on the unit's structure. Among homeowners seeking to protect their home values, new single family homes are seen as the most tolerable form of housing (Marble and Nall, Forthcoming). For one, a single family home is far more expensive than a unit within a multifamily apartment building. Thus, future residents are more likely to be wealthy, white, and contribute more in tax revenue than they use in public services, mitigating some of the above concerns. Labeled "cumulative zoning," single family housing is typically permitted by right anywhere that is residentially zoned, whereas multifamily housing is restricted to specific areas or requires discretionary review.

The quantity and structure of new housing are consequences of both institutional design and political behavior. Low-turnout local elections and the discretionary review process reward the preferences of organized, wealthier homeowners who want either no new housing, single family housing, or housing channeled outside of their neighborhoods (Einstein, Palmer, and Glick, 2019; Einstein, Glick, and Palmer, 2020). At-large elections increase the likelihood of these outcomes by effectively disenfranchising minority voters. Less wealthy and more likely to be renters, these minority voters are also more vulnerable to rising rents via gentrification. Counterintuitively, minority voters may not only oppose new housing because it harms their quality of life, but also because they believe it will attract demand to their neighborhoods, causing rents in their neighborhoods to *increase* (Hankinson, 2018). Thus, we expect CVRA-spurred district elections to amplify the local opposition of these previously underrepresented minority communities, affecting the quantity, structure, and even spatial distribution of new housing.

Identifying the Causal Effect of District Elections on Policy Outcomes

Existing research has struggled to identify the causal effect of district elections on political and policy outcomes. Even after controlling for any number of covariates, crucial unobserved differences remain between cities with histories under each institutional form. Those that switch from one system to another are also likely to have unusual features that confound estimates of the effect of conversion.

We advance our understanding of the causal effect of voter aggregation by leveraging the CVRA. In the pursuit of equal representation, the CVRA lowered the legal standard for plaintiffs to win cases alleging minority vote dilution under at-large electoral systems. To prove discrimination under the VRA, plaintiffs have to meet a three part test of minority size and geographic compactness, minority political cohesion, and a bloc voting majority (Kousser, 1992). Under the CVRA, plaintiffs no longer have to demonstrate a specific geographic district where a minority is concentrated enough to establish a majority. Additionally, city governments are responsible for all associated legal and court fees, even in the case of an out-of-court settlement. These changes have spurred a wave of enforcement litigation against cities with at-large elections.

The gradual roll-out of electoral reform induced by the CVRA lends itself to the use of a difference-in-differences framework with staggered treatments to estimate the effect of converting to district elections. The assumption under which this approach identifies a *causal* effect is that, absent the reform, the permitting of new housing would have moved in parallel among cities that switched and those that maintained at-large systems. The case for our identification strategy is strengthened by the fact that, to this day, not one of the more than one hundred cities that have been threatened with legal action has successfully avoided eventual conversion to districts; it has only been a matter of the time and money spent fighting, and ultimately losing, the battle. This eliminates the possibility of any

selection into treatment caused by cities' differential resources, racial attitudes, or any other latent characteristics driving their ability to resist reform and, simultaneously, their housing politics.

Of course, the selection of cities for legal action in the first place was not a random process: as Appendix Table B-1 shows, cities that converted to districts ('switchers') are larger, less wealthy, and more residentially segregated on average than those that remained at-large ('nonswitchers'). They also tend to have a weaker track record of Latino electoral success, suggesting that lawyers and plaintiffs were strategic in targeting cities where they could make the most convincing case for reform. What matters, however, is not any baseline differences between treated and control cities, but whether there are any unobserved confounding variables that would have driven switchers' housing outcomes to evolve differently from nonswitchers' outcomes had the CVRA never been adopted. This is unlikely to be the case. In numerous conversations with city council members and lawyers litigating CVRA cases, and in our review of several hundred local media articles and nearly a decade of legislative meeting minutes over six cities, we have repeatedly heard that the key factors that made cities vulnerable to legal action were having a large and residentially segregated Latino population, coupled with a poor track record of descriptive representation on city council—all of which we can measure and control for in our analysis. In all these conversations, we never encountered any discussion of housing politics driving CVRA litigation.[7]

To empirically validate the parallel trends assumption, we examine changes in single and multifamily housing over the pre-treatment period. As Appendix Figure G-4 shows, once we control for the set of covariates used in our fixed effects analysis, switchers and nonswitchers generally exhibit the same patterns of growth over the one-year to six-year periods preceding 2016, which is a pre-treatment year for the majority (77%) of eventually treated cities in our sample.[8] The plotted estimates are the coefficients on a binary indicator for being in

[7]Moreover, when we posed an open-ended question about how one might expect housing outcomes to change when moving from at-large to district elections, even experts such as city managers, planning commissioners, and affordable housing advocates did not consistently see a connection between the two.

[8]We omit cities that switched earlier from this analysis.

the eventually treated group from bivariate regressions (circles) and regressions with full controls (triangles), where the outcome is the change in housing units over the period on the x-axis. While we do observe that, looking over a pre-treatment period of three or more years, switchers were experiencing faster growth in single-family housing than nonswitchers, these differences disappear when we apply the relevant set of controls. Crucially, we do not see systematic differences in multifamily housing trends—our primary theoretical outcome of interest—between groups, even before any covariate adjustments.

Eventually Treated Subgroup Analysis

The CVRA's low standard for plaintiffs alleging minority vote dilution under at-large elections meant that numerous cities across California could in principle face successful litigation and be required to convert to districts. However, conversions happened at a slow trickle at first, accelerating only in 2016.[9] Given the large number of equally appropriate candidates for legal action, what determined the timing of conversion among cities that eventually switched to districts? Our data and interviews both suggest that, conditional on being above a certain threshold of Latino population and total size, eventually treated cities were not being selected for legal action by any substantively meaningful criteria. As far as a city's housing or ethnic politics, demographic characteristics, or potential responsiveness to district elections were concerned, the timing of treatment was as good as random.

What, then, drove the process of conversion for treated cities? In general, plaintiffs came from one of three sources. First, they could emerge from political networks internal to the city: in Anaheim, for instance, the suit was brought by Jose Moreno, a school board representative and local education policy leader who was frustrated by the inequitable distribution of resources in the city and saw CVRA litigation as a potential remedy.[10] Alternatively, plaintiffs could be recruited by one of the centrally organized statewide activist networks: the Mexican American Legal Defense Fund (MALDEF) or the Southwest Voter Registra-

[9]See Appendix Figure A-1 for the proportion of cities that had district elections from 2010 to 2018.
[10]Conversation with Jose Moreno, 01/13/20.

tion Education Project (SVREP). Using in-house demographers, these groups identified and recruited for legal action at-large cities with histories of Latino underrepresentation; where Latinos constituted at least 20% of the population such that majority-Latino districts could be drawn; and where the total population was over 50,000 people, as MALDEF leadership believed that smaller cities stood less to gain from district elections.[11] But due to internal capacity constraints and competing priorities—both SVREP and MALDEF have missions that extend beyond voting rights and serve geographic regions beyond California—these groups did not ramp up their litigation efforts until 2018, filing only a limited number of suits before then. Finally, private law firms that stood to profit from CVRA cases entered the fray, since victory for the plaintiff was nearly assured, while the defendant shouldered all legal fees. These lawyers were less discriminating in their case selection, targeting cities of all sizes and with more tenuous prospects of gaining Latino council seats. However, these actors have only emerged in recent years, as it took several test cases to reveal the full extent of the law's favorability to plaintiffs.

We empirically verify what determines a city's propensity to switch to districts using a standard survival model. In Figure 1, we plot the exponentiated coefficients from survival analyses on two samples: all California cities except those that were always by-district (panel a), and eventually treated cities that satisfied MALDEF's more stringent criteria (panel b).[12] Looking across all cities, the predictors of switching to districts are consistent with what we know qualitatively about the selection process. Having a larger Latino population, greater residential segregation, a larger total population, and weaker Latino electoral performance all substantially increase the annual hazard rate of conversion; on the whole, cities that made weaker cases for conversion generally switched later on. However, focusing on the cities that eventually switched and that satisfied MALDEF's criteria, none of these predictors have any detectable effect; in other words, there were far more cities that were well-suited for

[11] Conversation with Thomas Saenz, President and General Counsel of MALDEF, 01/13/20.

[12] Segregation is measured by the Theil's H index (2011). Density is population per square mile of land area (2010). "Past Latino success" is the proportion of seats up for election over the prior two election cycles that were won by Latino candidates.

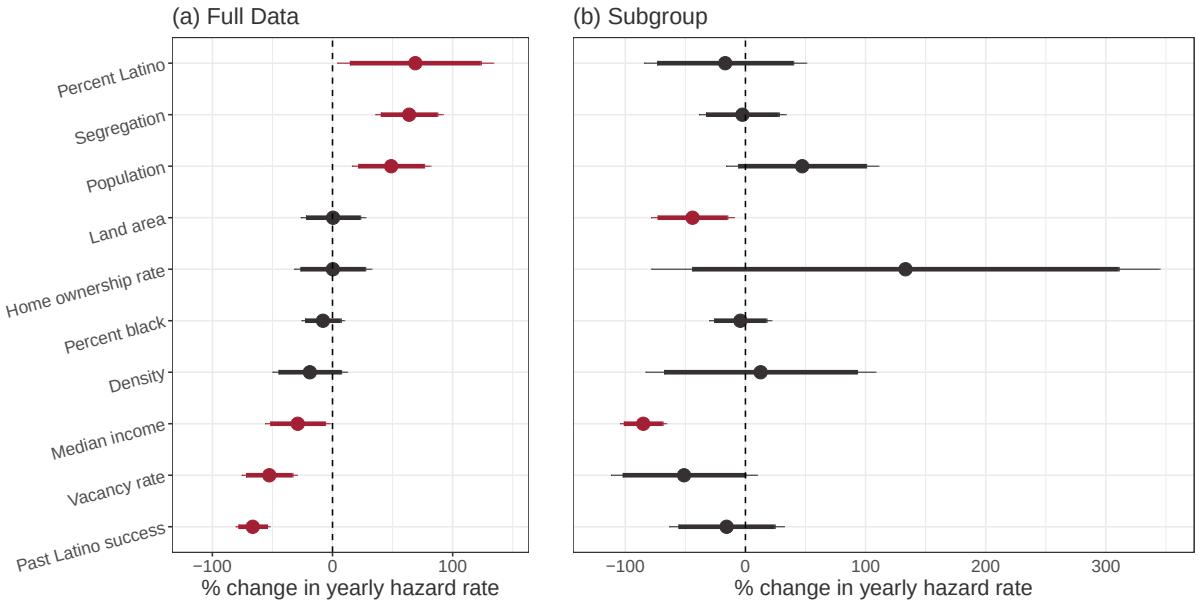

Figure 1: Predictors of selection into treatment (coefficients from survival model transformed into percent change). Variables are standardized to mean 0, standard deviation 1 based on the full data. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

conversion than there was initial capacity, and within that group there were not substantively meaningful predictors of the timing of conversion.[13]

The as-if-random timing of treatment for this high-propensity subgroup makes a strong case for a second identification strategy: a fixed effects analysis restricted to the subgroup in panel b of Figure 1. Focusing on this group has two distinct advantages. First, we get strong causal identification, as later-treated cities within this group serve as the best counterfactuals for early-treated cities when we exploit timing of treatment in this way. Second, we get a substantively meaningful, policy-relevant estimate that is separate from our main analysis: a local average treatment effect for the group of cities that can most directly benefit from district elections. Our analyses of aggregate housing outcomes will use the full statewide sample and this high-propensity subgroup.

[13] We do see that income and land area played a role in the timing of conversion, but have seen no qualitative evidence that lawyers thought about these criteria.

Hypotheses

We expect that cities that switch to district elections under the CVRA will experience changes in the amount, structure, and spatial distribution of new housing units permitted. Primarily, the switch to district elections will decrease the number of new units permitted annually. However, we predict heterogeneous effects across the structure of those units as well as the city's level of segregation and racial diversity. Finally, we believe district elections will affect the spatial distribution of new housing *within* cities, specifically the relationship between new buildable capacity and a neighborhood's racial composition.

First, we expect the decrease in units permitted to be driven almost exclusively by a decrease in the permitting of multifamily housing for three reasons. As stated, single family homes are viewed as more benign, meaning there is little neighborhood opposition to be amplified by district constituencies. Also, because they require a large amount of space per unit, single family homes are rarely proposed in already developed parts of a city. Instead, they are built on the outskirts, where there are few neighbors to provoke. Finally, because of cumulative zoning, single family homes rarely require discretionary review. Thus, NIMBY opposition lacks the venue to publicly pressure legislators to veto or scale back single family proposals. In contrast, multifamily housing is almost uniformly less desirable to neighbors, more likely planned in densely populated areas, and more often requires discretionary review vulnerable to district-based pressures.

Second, we expect the effect of district elections to vary across cities according to levels of segregation and racial diversity. For district elections to make a meaningful difference in representation, minority racial groups must be segregated enough to form majority-minority districts (Abott and Magazinnik, 2020; Trounstine and Valdini, 2008). Once formed, these districts can more easily elect a minority candidate, changing the ethnic composition of a city council. Segregated cities are also more likely to create the initial conditions for an unequal distribution of housing. If white, politically engaged voters are evenly distributed throughout the city, no neighborhood can serve as a 'dumping ground' for undesirable housing, and

district elections have no imbalance to correct. Though we are unable to discern which of the two pathways plays the bigger role, we expect higher levels of segregation to be associated with a larger decrease in units permitted annually.

Variation is also likely to stem from the size of the racial minority in a city, since minority-dominated neighborhoods are the ones most likely to lack representation under at-large systems, and that necessarily gain spatial, if not descriptive, representation in the conversion to districts. The moderating effect of minority size is of great policy relevance, as reformers have clashed over how large a Latino population makes a city worth targeting for CVRA litigation,[14] but its expected direction is unclear. On the one hand, the larger the Latino population, the greater its potential voting power, and the better its ability to coalesce as a counterweight to a white voting bloc. Thus, we might expect inequities in the spatial distribution of housing to be *less pronounced* among at-large cities with substantial Latino populations. On the other hand, given the lower levels of turnout in this group, it is extremely unlikely that even a city with a Latino majority will dominate the politics of the city under at-large elections. By the same token, the more Latino-dominated districts that can be drawn, the more we might expect the composition of a council to change under district elections, which would *amplify* our theorized treatment effect.

Along with changes in the housing supply across cities, we also expect a change in the spatial distribution of new housing within cities. District elections mean representation has been evenly divided across the city. In turn, council members will find it harder to channel unwanted housing into underrepresented communities. Because previously underrepresented neighborhoods are likely to be minority communities, we expect that any positive relationship between minority communities and new housing permitted will weaken under district elections. In other words, race will become less predictive of a neighborhood's housing burden, all else equal.

[14]Conversation with Thomas Saenz, 01/13/20.

Research Design and Data

To test our hypotheses, we constructed a comprehensive database of all 482 municipalities in California. We recorded each city's council structure (district or at-large) and, for the 135 cities that switched to district elections, the year of its first district election, which we use as the date of treatment throughout this study.[15]

Aggregate Outcomes

We first test the effect of district elections on the number of housing units permitted each year at the city level. To do so, we use a panel of housing permit data from 469 municipalities from 2010 to 2018 collected by the U.S. Census Building Permits Survey.[16] These data include the number of total units permitted as well as the distribution of new units between single family and multifamily housing.[17] For each analysis, we use a generalized difference-in-differences framework with the city as the unit of analysis, the switch to district elections as the treatment, and logged units permitted as our dependent variable (e.g., Kahn, 2011). Specifically, we include city and year fixed effects as well as city-specific linear time trends. Huber-White standard errors are clustered at the city level. To account for time-varying city attributes, we include 5-year estimates from the American Community Survey from 2010 to 2018 of population, percent non-Hispanic white, percent black, percent Hispanic, median income, homeownership rate, home vacancy rates, and median home value.[18] We first present results using the full statewide sample, then show findings from the high-propensity subgroup of eventually treated cities.

To test for heterogeneous effects across cities, we use the same model but compare cities

[15] We investigate the treatment timing using a Granger causality test. As expected, Appendix Figure D-2 shows a treatment effect concentrated in the year of the first district election.

[16] 13 municipalities did not report annual housing permit data to the U.S. Census.

[17] The Census definition of single-family housing includes rowhouses and townhouses. Because these structures also evoke NIMBY opposition (Whittemore and BenDor, 2019), our analysis may underestimate the differential effects by housing type.

[18] We impute missing data for control variables throughout using Amelia (Honaker, King, and Blackwell, 2011). The medians and ranges of control variables are substantively the same before and after imputation.

in the top tercile of our variable of interest to those in the bottom tercile. We do so by including an interaction for being in the top tercile and dropping the middle tercile of data, thus directly comparing the treatment effect of district elections across cities with high and low values of segregation and racial diversity. This estimation approach guards against the pitfalls of interpreting coefficients from multiplicative interaction models that lean heavily on assumptions of linearity and common support (Hainmueller, Mummolo, and Xu, 2019). Tercile cutpoints are based on the average values among switcher cities pre-treatment. We measure citywide segregation using the Theil's H index as calculated in Trounstine (2016).[19]

Spatial Outcomes

Having measured the effect of district representation on the aggregate supply of housing, we apply our theory to the spatial distribution of the housing supply. To measure these geographic effects, we constructed a dataset of zoning changes emerging from the discretionary review process. Within our 6 sampled cities, we reviewed every meeting of the planning commission and city council from 2011 through 2018, totaling over 2,000 meetings. We coded details of each housing proposal and zoning change approved for development, including the number of units, the composition of units, the proposal's address, and year of approval.[20] Importantly, this coding reflects any increase in the by right 'buildable capacity' of the city, giving us the universe of legislative decisions allowing new housing to be built.[21] We geocoded these decisions to the Census block group level and merged them with time-varying socioeconomic variables drawn from the American Community Survey. These block group-level controls include median income, percent non-Hispanic white, percent black, percent Hispanic, homeownership rate, residential vacancy rate, and median home value.

The intensity of this data collection required sampling cities. First, we selected cities with

[19] Due to collinearity with our interaction variables, time-varying measures of racial demographics are excluded from these heterogeneous effects models.

[20] Coding decisions are discussed in Appendix C.

[21] While housing proposals count are immediately permitted units ready for construction, neighborhood-wide zoning changes may not be developed for years and may even be modified in future meetings.

multiple years of post-treatment data. Second, we chose cities that had a non-Hispanic white population large enough to potentially dilute minority representation via bloc majority voting. Third, we chose cities large enough to generate enough new permits that an effect would be detectable. These decision rules winnowed treated cities to Santa Barbara, Escondido, and Anaheim. We match these treated cities to similarly sized and racially composed cities with at-large elections as controls: Santa Cruz, San Buenaventura (Ventura), and Glendale, respectively.[22] Although these cities are larger and more diverse than the average California city, we believe our spatial findings capture a generalizable mechanism behind how district elections affect the spatial distribution of new housing development.

We examine the distributive equity of the housing supply by estimating the intervening effect of a neighborhood's racial composition on its annual change in buildable capacity. We replicate our first model—the generalized difference-in-differences design—using the block group as our unit of analysis. Our dependent variable is log housing units approved annually via discretionary review. To capture the role of race, we define white and minority block groups using cutpoints defined by the top and bottom tercile of percent non-Hispanic white in treated cities prior to treatment.[23] 'White' block groups are more than 54% white, and 'minority' block groups are less than 34% white. We use these cutpoints to classify the block groups of all six treated and control cities as either minority or white, dropping the middle tercile of block groups from the analysis.

To measure the effect of district elections within cities, we interact our independent variable, the switch to district elections, with an indicator for being a minority block group. This interaction signifies whether district elections affect the housing supply differently within minority blocks groups compared to non-minority block groups. We use this interaction to measure the causal effect of district elections on the equity of the distribution of housing

[22] Table B shows demographic data of these matched pairs. Of note, Ventura held their first district election in 2018, which is accounted for in our difference-in-differences model.

[23] We define cutpoints based on treated cities because they have significantly larger nonwhite populations than untreated cities. Defining cutpoints based on all cities would create major imbalances in the assignment of treated units to groups, and therefore pose challenges to estimation.

between white and minority neighborhoods. We include city fixed effects so our model only compares minority and white block groups within the same city. Year fixed effects and city-specific linear time trends are also used, with Huber-White standard errors clustered at the city level. We estimate our standard errors using a wild bootstrap (Cameron, Gelbach, and Miller, 2008). For time-varying controls, we use the block group-level covariates for median income, homeownership rate, residential vacancy rate, and median home value.

Results

We present our results in the same order as our hypotheses, moving from the effect of district elections on the citywide supply to the effect on the distribution of housing within cities.

Aggregate Outcomes

Figure 2 shows the effect of district elections on the number of housing units permitted annually for the full sample of California cities (circles) and the subgroup of high-propensity eventually treated cities (triangles). For interpretability, we present coefficients transformed from log housing to the percent change in housing units permitted, with total housing units to the left and single family and multifamily housing to the right.[24] Conversion to districts decreases permitting of all housing units by 21 percent, which is not statistically significant by conventional standards ($p = .10$).[25] Disaggregating by the structure of new housing, we see that this total effect averages over a significant 48% decrease in the permitting of multifamily units ($p = .01$), and a precisely estimated null effect on single family homes. The pattern of results is consistent with our hypothesis, as multifamily housing is both less desirable

[24] Results in tabular form are presented in Appendix F.

[25] The confidence intervals in Figure 2 are computed by transforming the standard errors from Table F-3 using the delta method. This transformation tips the estimate on total units in the eventually treated subgroup from just shy of the 5% significance threshold to just over it (see Feng et al. (2014) for a discussion of inference on log-transformed variables). Because this cutoff is arbitrary, we cautiously conclude that there is suggestive but not definitive evidence that districts decrease the total housing supply in this subgroup; regardless, our basic pattern of significant changes in multifamily housing and no change in single family housing is unchanged.

and more vulnerable to NIMBY pressure via discretionary review compared to single family housing. Furthermore, our estimates are nearly identical to Mast (N.p.)'s estimate of a 24 percent decrease in total housing units and a 47 percent decrease in multifamily housing found using a national sample of cities from 1980 to 2018.

These treatment effects grow when examining our subset of high-propensity eventually treated cities. Among these cities, district elections decrease the permitting of all new housing units by 34 percent ($p = .07$), with zero effect on the permitting of single family housing and a 67 percent decrease in the permitting of multifamily housing ($p = .01$). Reformers targeted these large cities with substantial Latino populations because that is where they thought they could most dramatically increase Latino political influence. Given that this is one of our primary mechanisms, larger effects for the high-propensity subgroup are consistent with theoretical expectations.

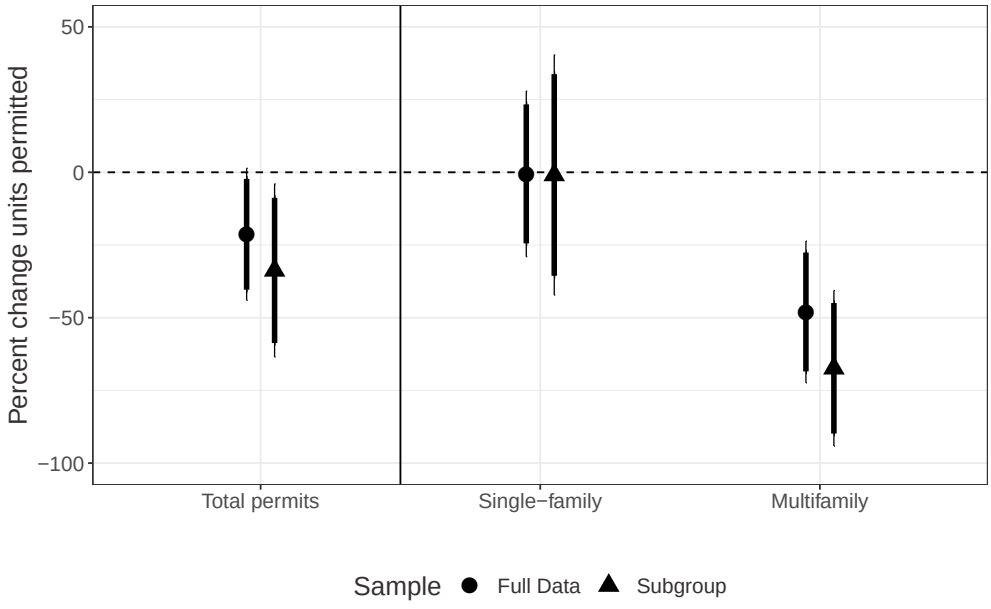

Figure 2: Treatment effects and confidence intervals for the full sample of California cities (circles) and the high-propensity subgroup (triangles). Points indicate the percent change in units permitted within cities following conversion to single member districts. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines). Full results are shown in Appendix Table F-3.

Robustness Checks

One concern for identification is whether cities that switched to district elections were already becoming more likely to permit fewer housing units prior to the change in electoral system. As noted above, we do not see systematic differences in housing trends between switchers and nonswitchers prior to 2016 (see Appendix Figure G-4). Additionally, we use a Granger causality test to visualize how the housing trends of treated cities differed from those of control cities before and after switching to district elections. Appendix Figure D-2 shows that the estimates are close to (and statistically no different from) zero prior to the year of the first district election, t. In contrast, the estimates on multifamily housing are uniformly negative and greater than 50 percent in absolute value following the year of the first district election.

More broadly, a growing recent literature in economics and political science has been concerned with issues around the identification and interpretation of treatment effects in panel data when treatments occur at different times across units, and under heterogeneous treatment effects (e.g., Abraham and Sun, 2019; Goodman-Bacon, 2018; de Chaisemartin and D'Houltfœuille, 2018; Callaway and Sant'Anna, 2018). This rich literature has produced a large number of alternative approaches with advantages over the standard two-way fixed effects model. We use two of these approaches to validate our main results. First, we address the potential bias in standard errors induced by serially correlated outcomes identified by Bertrand, Duflo, and Mullainathan (2004). We follow the authors' recommendation to collapse the data to a two-period panel and ignore the time-series variation altogether, and the resulting estimates, reported in Appendix Figure G-5, are substantively similar to the pattern of results reported in our main Figure 2.

Second, we address any potential bias in our two-way fixed effects estimates that may be induced by units switching in and out of treatment at different times, discussed by Imai and Kim (2019). We use their method (Imai, Kim, and Wang, 2019) to match each treated unit to a set of control cities based on their permitting of multifamily housing over three pre-

treatment periods, along with our usual battery of time-varying controls, and then estimate a difference-in-differences for each post-reform year, relative to the year before the reform was adopted.[26] The method achieves good balance on both the time-varying controls and the pre-treatment trends in the outcome, as illustrated by the null effects at $t-3$ and $t-2$ in Appendix Figure G-6. The same figure shows a statistically significant effect in the first post-treatment year $(t+1)$ that is very similar to our main effect—a 52 percent decrease in new multifamily housing.

Heterogeneous Effects

Next, we test for variation in the effect of district elections on multifamily housing across cities, both by a city's segregation and racial diversity. Our findings support the hypothesis that equalizing spatial representation was the causal mechanism behind the observed change in the local housing supply: the biggest changes occurred in cities with large and geographically segregated Latino populations, which stood the most to gain from district elections.

We visualize these results in Figure 3, with the percent change in multifamily units permitted annually as our dependent variable. Results are shared both for the full sample of California cities (circles) and for the subgroup of high-propensity eventually treated cities (triangles). Treatment effects across levels of segregation are on the left, with effects by percent non-Hispanic white presented on the right.[27]

On the left, the effect of district elections appears driven by cities with high levels of segregation. The difference between low and high segregation cities is not statistically significant due to imprecision in the estimate of the former, but the results affirm our expectations that high segregation cities are the ones that produce the initial conditions for large differen-

[26] We use the PanelMatch package in R (Imai and Kim, 2019) to implement Mahalanobis distance matching, though our results are not sensitive to this choice. We use three pre-treatment periods and two post-treatment periods because those are the choices that maximize the number of periods included in our analysis while preserving a large enough sample of treated cities.

[27] For histograms of subgroup variables and cutpoints, see Appendix E.

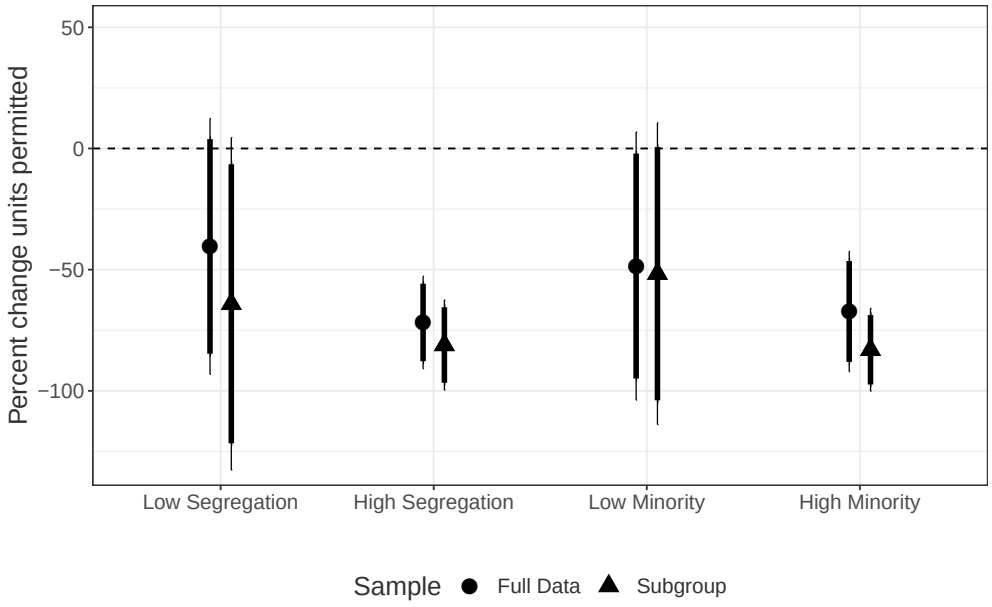

Figure 3: Treatment effects and confidence intervals for the full sample of California cities (circles) and the high-propensity subgroup (triangles) comparing cities in the top and bottom terciles of segregation (left) and percent non-Hispanic white (right). Points indicate the percent change in multifamily units permitted within cities following conversion to single member districts. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines). Full results are shown in Appendix Table F-4.

tial policy outcomes. On the right, we see that cities with the largest minority populations are consistently driving the overall effect, while low minority cities seem to have heterogeneous effects that our model does not account for. This suggests that the ability to draw more Latino-majority districts increased this group's political power in the local permitting process, though tracing the effect through this causal pathway is an avenue for future research.

Spatial Outcomes

Having measured the effect of district elections at the city level, we next look at the effect of district elections on log housing units approved at the block group level. Our dependent variable includes both single family and multifamily housing, as all units in this dataset were vulnerable to NIMBY political pressure via discretionary review. Appendix Table H-5 shows

the results in tabular form as well as heterogeneous effects for single family and multifamily housing.

We find that moving to district elections significantly decreases the disparity in permitting between white and minority neighborhoods. Under at-large representation, minority block groups see 44 percent more housing units approved annually compared to their white block group counterparts within the same city, even after controlling for demographic and housing market covariates (Figure 4). This baseline difference falls just shy of the 5% statistical significance threshold in the original model (see Appendix H). After switching to district elections, the effect of being in a minority block group falls to a statistically insignificant *negative* 16 percent.[28] The difference between these estimates represents the causal effect of districts on the racial disparity in permitting, and is negative and statistically significant ($p < .001$) in all specifications. In other words, districts reduce differential responsiveness to the NIMBY interests of white as opposed to minority neighborhoods.

Discussion

Faced with racially polarized voting and neighborhood segregation, civil rights advocates have viewed district elections as a pathway to descriptive and—hopefully—substantive representation. With carefully drawn districts, previously underrepresented neighborhoods can be almost guaranteed a voice in the legislative body. But efforts like the CVRA change both the racial and spatial composition of constituencies. District-elected council members are not merely accountable to the local interests of their constituents; with logrolls in place, they also have the ability to advance local interests with little interference. Thus, district-elected officials may alter the balance between equity and supply for policies with concentrated costs, potentially to the long-term detriment of the citizens they are meant to empower.

For example, we show that district elections spatially disperse the concentration of new

[28] District elections do not change the number of housing units permitted in white block groups (see Appendix Table H-5).

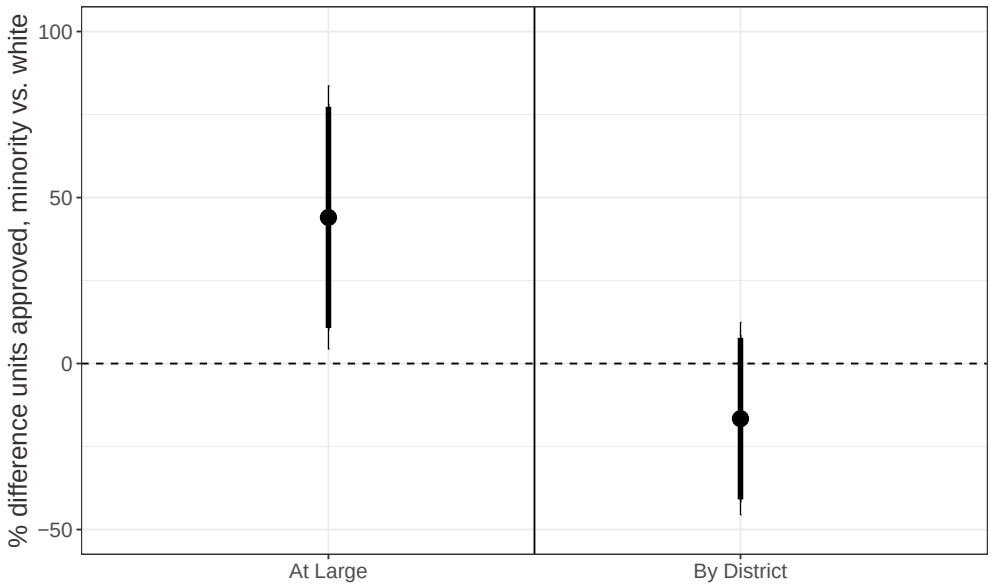

Figure 4: Points indicate the percent difference in housing units permitted between minority block groups and white block groups. On the left are all block groups in at-large systems, including treated units pre-treatment. On the right are block groups in treated cities, post-treatment. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines). See Appendix H for results in tabular form and details on the computation of these confidence intervals.

housing, breaking the correlation between minority block groups and unwanted development. While this may be in the local, short-term interest of newly empowered minority voters, district elections also decrease the citywide permitting of multifamily housing by 48 percent. This restriction of supply is likely to drive citywide housing costs even higher, disproportionately burdening the lower-income minority community the reform was meant to assist. Put simply, the decentralized neighborhood control of district elections may spur long-term collective action challenges, trading spatially concentrated inequalities (new housing units) for a spatially diffuse burden (citywide housing costs).

Other policies with concentrated costs and diffuse benefits also risk undersupply when subjected to local logrolling institutions. Because city councils and county commissions govern the vast majority of land use decisions in the United States, we expect this supply-equity trade-off to stymie the siting of most LULUs, from clean energy infrastructure, to opioid addiction treatment facilities, to COVID-19 test facilities (e.g., Gray, 2020). The pernicious effects of logrolling have even been documented outside of land use. Hills Jr and Schleicher (2011) argue that the closing of military bases and the easing of trade tariffs present concentrated costs for nearby communities and affected industries, respectively. Within Congress, both policies saw inefficient, logroll-type outcomes until institutional reform bundled the individual policy decisions and removed substantial discretion from the legislature. We suggest a similar reform for LULU siting.

State governments have an interest in each city permitting their fair share of new housing to maintain statewide housing affordability and economic growth. To counter this decrease in the multifamily supply, district elections can be paired with top-down pressure from the state government via withholding intergovernmental transfers and grants (e.g., Elmendorf, Forthcoming). Under at-large elections, this top-down pressure would likely channel housing into underrepresented, minority neighborhoods, exacerbating spatial inequalities of distributive policy. But under district elections, with more equal representation secured, the push for supply would be more evenly spread across neighborhoods. This pressure would simul-

taneously generate new housing to avoid rising prices while equitably distributing its spatial burden.

Conclusion

This study provides evidence for the causal effect of district elections on the supply and equity of one type of distributive policy: the local housing supply. The exogenous treatment of CVRA-spurred district elections caused a substantial decrease in the permitting of new multifamily housing, demonstrating how local control magnifies the ability of NIMBY voices to block new housing through discretionary review. However, district elections have also reshaped the distribution of housing, supressing the disproportionate channeling of this locally unwanted land use into minority neighborhoods. Equity has been gained at the expense of supply.

Policies with concentrated costs and diffuse benefits are rarely popular (Wilson, 1980). But LULUs present a uniquely challenging concentrated burden, one subject to the spatial aggregation of voters. We have identified how the spatial scale of representation affects the trade-off between local interests and collective outcomes—between distributive equity and aggregate supply. Moving from this trade-off to the pursuit of *both* goals requires designing institutions that actively harness voter behavior and electoral incentives to overcome these spatial collective action problems.

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Supporting Information/Supplementary Appendix for "The Supply-Equity Trade-off: The Effect of Spatial Representation on the Local Housing Supply "

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A District Elections, California

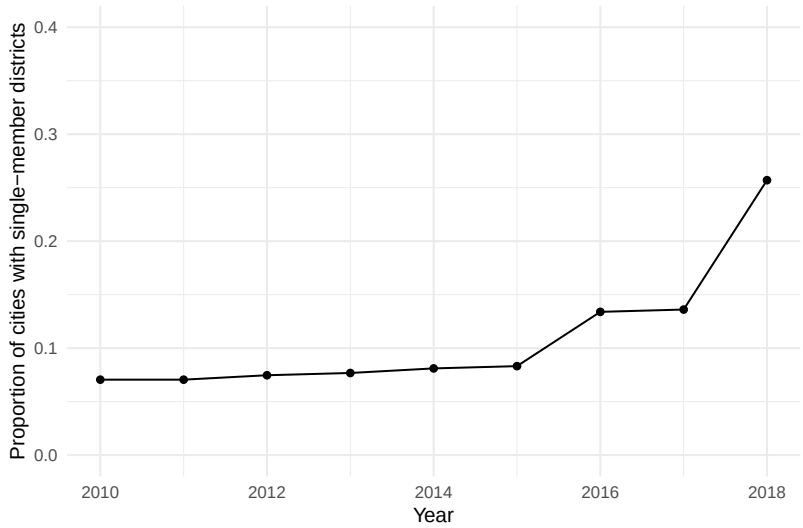


Figure A-1: Proportion of California cities with district elections over time.

B Summary Statistics

Table B-1: Characteristics of Cities in Fixed Effects Analysis, by Treatment Status

	Mean (Untreated)	Mean (Switchers)	Mean (Switchers, $\geq$ 20% Latino, $\geq 50,000$ pop)	p-value of difference, all switchers vs. untreated	p-value of difference, subgroup vs. untreated
Population					
Number of people	35,174	77,703	107,063	0.00	0.00
Percent non-Hispanic	49	43	35	0.01	0.00
Percent Black	3	5	6	0.01	0.00
Percent Asian	10	11	11	0.25	0.36
Percent Latino	28	29	35	0.76	0.00
Past Latino electoral success					
Prop. of seats w/Latino elected	0.20	0.11	0.14	0.00	0.06
Prop. of seats w/Latino candidate	0.30	0.36	0.47	0.07	0.00
Latino vote share	0.20	0.14	0.18	0.01	0.49
Income and land use					
Median household income ($)	71,182	66,669	61,522	0.10	0.00
Median home value ($)	496,403	410,650	366,846	0.00	0.00
Home vacancy rate	0.10	0.07	0.07	0.00	0.00
Home ownership rate	0.59	0.59	0.58	0.81	0.25
Density (population per sq. mile)	4,165	4,102	4,640	0.83	0.22
Residential segregation (Theil index)	0.03	0.07	0.09	0.00	0.00
Housing outcomes					
Units permitted annually, single-family	43	82	96	0.00	0.00
Units permitted annually, multifamily	32	61	77	0.01	0.00
N	301	135	62		

Table B-2: Characteristics of Cities in Spatial Analysis, by Treatment Status

	Mean (Treatment)	Mean (Control)	p-value of difference
Median income	64192	57470	0.00
Median home value	446913	501240	0.00
Home ownership rate	0.45	0.40	0.01
Home vacancy rate	0.07	0.06	0.18
Proportion Black	0.03	0.02	0.00
Proportion non-Hispanic white	0.47	0.68	0.00
Proportion Hispanic	0.37	0.17	0.00

C Coding Zoning Amendments

To geocode increases in buildable capacity within cities, we reviewed the meeting minutes of the two bodies which control the discretionary review of new housing proposals: the planning commission and city council. We begin with minutes from 2011, as Census block group boundaries will be stable post-2010. This allows enough time to establish pre-trends within our treated cities. For each proposal, we recorded the street address, total units, and the divide of units between single family and multifamily housing.

As political outcomes, our goal was to identify the year the proposal emerged from the discretionary process. This year may be different from the year of construction and even different from the year of the final permit, as the final permit may rely on a back and forth the discretionary body about design details even after the number of units has been approved. To identify this year of final discretionary review, we first check if the city council voted on the project. Any lower board decisions can be appealed to city council, meaning the voice of the city council is the most important discretionary hurdle. If city council does vote on the project, we use the year of the city council vote. If city council does not vote on the project, we used the year of the last density-based discretionary approval by the planning commission.

Occasionally, a city will make a change to their overall zoning code by amending the General Plan. Such changes affect a swath of the city, potentially many neighborhoods and thousands of individual parcels. While these zoning changes (or 'rezonings') may not become reality until a decade into the future, they are politically meaningful increase in the capacity to build by right. As a result, we code each rezoning by its increase in buildable capacity. Because the overlap between block groups and upzoned neighborhoods is not perfect, this process involves discretion in allocating upzoned units across multiple block groups. Still, we believe we have generated the most accurate multi-city representation of changes in allowable density over the past 8 years.

There are several types of residential proposals we do not include. First, we do not collect data on renovations nor conversions of apartments to condominiums. The legalization of existing illegal units is coded, as legalization is similar enough to building a new unit. Additionally, we include proposals by commercial enterprises seeking to designate part of their existing structure as residential. Finally, we do not collect data on permits approved by the staff of the city's planning division. These projects are less vulnerable to discretionary approval and often are only reviewed for conformance with existing code.

Ultimately the data we collect represent the corpus of permits that were approved by passing through political gauntlet of discretionary review. These data capture the output of permits that should be most directly affected by the change in representation from district elections.

D Granger Test

The switch to district elections is a treatment with imprecise timing. First, cities stagger their council elections, with only half of a city's council seats contested every two years. As a result, the first district election only changes the constituencies of half of the city council members. Second, members of city council elected in the fall do not take office until January of the following calendar year. Thus, district elections should not directly affect permitting until the year after the first election. However, indirect effects likely occur earlier. Upon deciding to switch, council members may alter their behavior to secure re-election via a district-based campaign. Controversial housing proposals may have trouble winning approval as council members seek to gain a new identity as a 'neighborhood defender' (Einstein, Glick, and Palmer, 2020).

We investigate the imprecision of treatment timing using a Granger causality test. To conduct this test, we create indicator variables for each year pre- and post-treatment among switchers, treating four years before treatment as the baseline year. We then regress logged multifamily units permitted on these indicator variables as well as city and year fixed effects, city-specific time trends, and time-varying controls.

We plot the coefficients on the indicator variables from three years prior to two years or more after treatment in Figure D-2. The figure shows a treatment effect that begins in the year of the first district election, t; consequently, we use t as the time of treatment throughout this study. Prior to the year of the first district election, the estimates are close to (and statistically no different from) zero. In contrast, the estimates are uniformly negative and greater than 50 percent in absolute value following year t, dropping to as low as a statistically significant 66-percent decrease in multifamily units permitted during the first year post-treatment. In short, the null estimates pre-treatment suggest that the observed effect of elections is not driven by pre-treatment differences in trends between switching and nonswitching cities. Thus, the specification in Figure 2 likely captures the causal effect of district elections on the local housing supply.

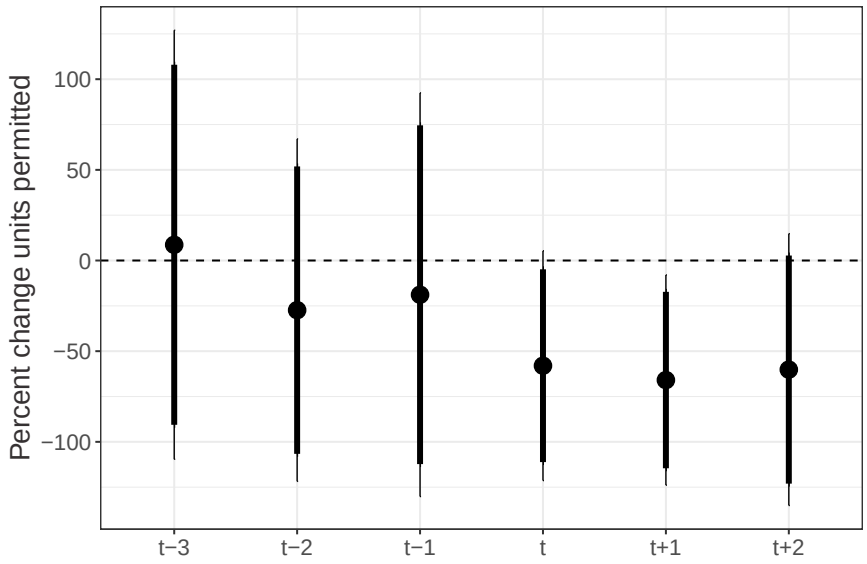
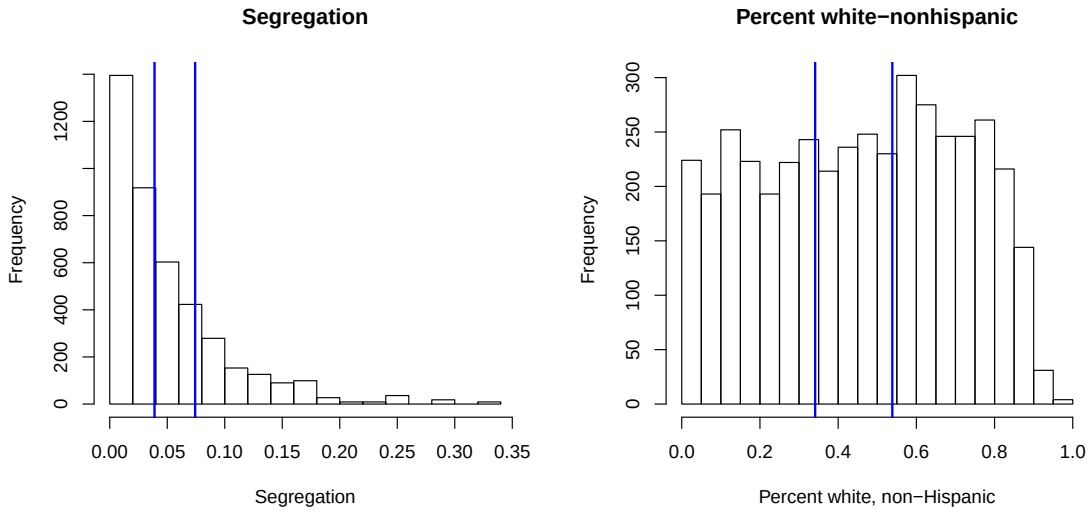


Figure D-2: Effect of district elections on multifamily units permitted, checking for pre-treatment differences in outcomes. This figure plots coefficient estimates, with lines indicating 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines), from three years prior to the switch to two years or more after.

E Distributions of Heterogenous Effects Variables

Figure E-3: Distributions of variables used to assess heterogeneous effects across cities. Tercile cutpoints marked in blue.



F Tabular Results for Aggregate Outcomes

Table F-3: Effect of Conversion to Single-Member Districts on Housing Permits

	Full Data			*Subgroup*		
	Total	Single	Multi	Total	Single	Multi
	(1)	(2)	(3)	(4)	(5)	(6)
Single-member districts	−0.240	−0.006	−0.654**	−0.412	−0.009	−1.121**
	(0.147)	(0.146)	(0.239)	(0.229)	(0.213)	(0.418)
Population (thousands)	−0.004	0.001	−0.007*	0.003	0.004	0.001
	(0.003)	(0.001)	(0.003)	(0.004)	(0.003)	(0.007)
Percent non-Hispanic white	0.272	0.577	0.601	5.231	−0.509	12.276
	(1.536)	(1.294)	(1.381)	(9.223)	(8.509)	(15.621)
Percent Black	−0.022	−0.012	−0.004	−0.115	0.058	−0.165
	(0.029)	(0.025)	(0.025)	(0.138)	(0.142)	(0.269)
Percent Hispanic	−0.003	−0.011	0.021	−0.025	−0.009	0.011
	(0.016)	(0.013)	(0.017)	(0.081)	(0.078)	(0.128)
Vacancy rate	0.347	0.328	1.032	−2.905	7.125	−2.649
	(1.497)	(1.283)	(1.411)	(9.597)	(8.299)	(19.329)
Home ownership rate	0.420	−0.863	1.755	16.738**	6.275	11.499
	(0.883)	(0.737)	(1.070)	(5.247)	(5.226)	(8.347)
Median home value (thousands)	−0.0005	−0.0004	−0.0001	−0.005	0.002	−0.013
	(0.0003)	(0.0002)	(0.0003)	(0.005)	(0.004)	(0.008)
Median income (thousands)	0.005	0.003	0.001	−0.007	−0.033	0.037
	(0.003)	(0.003)	(0.003)	(0.045)	(0.036)	(0.068)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City-specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,164	4,164	4,164	558	558	558
R^2	0.825	0.852	0.672	0.731	0.787	0.594

Note: *p<0.05; **p<0.01; ***p<0.001

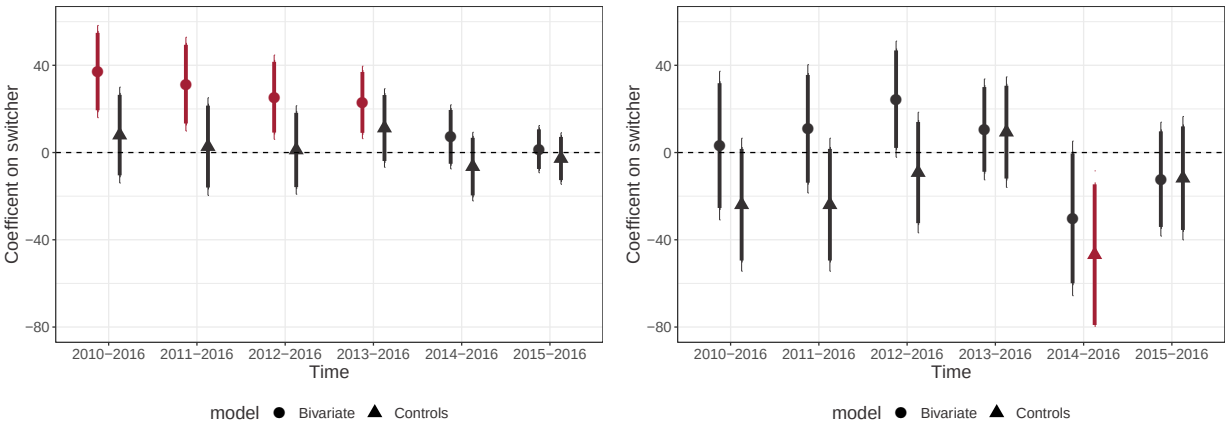
Table F-4: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with City Characteristics

	Full Data		*Subgroup*	
	(1)	(2)	(3)	(4)
Single-member districts	-1.116^{**}	-0.518	-1.776^{***}	-1.024
	(0.386)	(0.452)	(0.514)	(0.975)
SMD:High white group	0.453		1.049	
	(0.667)		(0.687)	
SMD:High segregation group		-0.748		-0.641
		(0.560)		(0.981)
Population (thousands)	-0.009^{*}	-0.007	-0.002	0.011^{*}
	(0.004)	(0.004)	(0.005)	(0.005)
Vacancy rate	2.377	1.357	54.452^{*}	-2.465
	(1.493)	(1.367)	(24.073)	(26.265)
Home ownership rate	1.172	1.491	0.496	21.477
	(1.041)	(0.951)	(10.220)	(12.360)
Median home value (thousands)	-0.0001	-0.0002	-0.005	-0.017
	(0.0003)	(0.0002)	(0.010)	(0.016)
Median income (thousands)	0.001	0.001	0.003	-0.044
	(0.003)	(0.003)	(0.102)	(0.088)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
City-specific Trends	Yes	Yes	Yes	Yes
Observations	3,237	3,141	324	360
R^2	0.666	0.687	0.649	0.604

Note: The main effects of "high white group" and "high segregation group" are omitted, as we include city fixed effects and a city's assignment to groups is time-invariant.
$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

G Robustness Checks for Aggregate Outcomes

Figure G-4: Treatment effects and confidence intervals from regressing a binary indicator for being in the group of cities that switched to districts on changes in housing units permitted over a given period, without controls (circles) and with the same vector of controls used throughout our main analyses (triangles). We focus here on cities that switched after 2016, which is 76% of switchers.



(a) Single-family housing units

(b) Multifamily housing units

Figure G-5: Treatment effects and confidence intervals computed using the two-period difference-in-differences approach recommended by Bertrand, Duflo, and Mullainathan (2004). Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

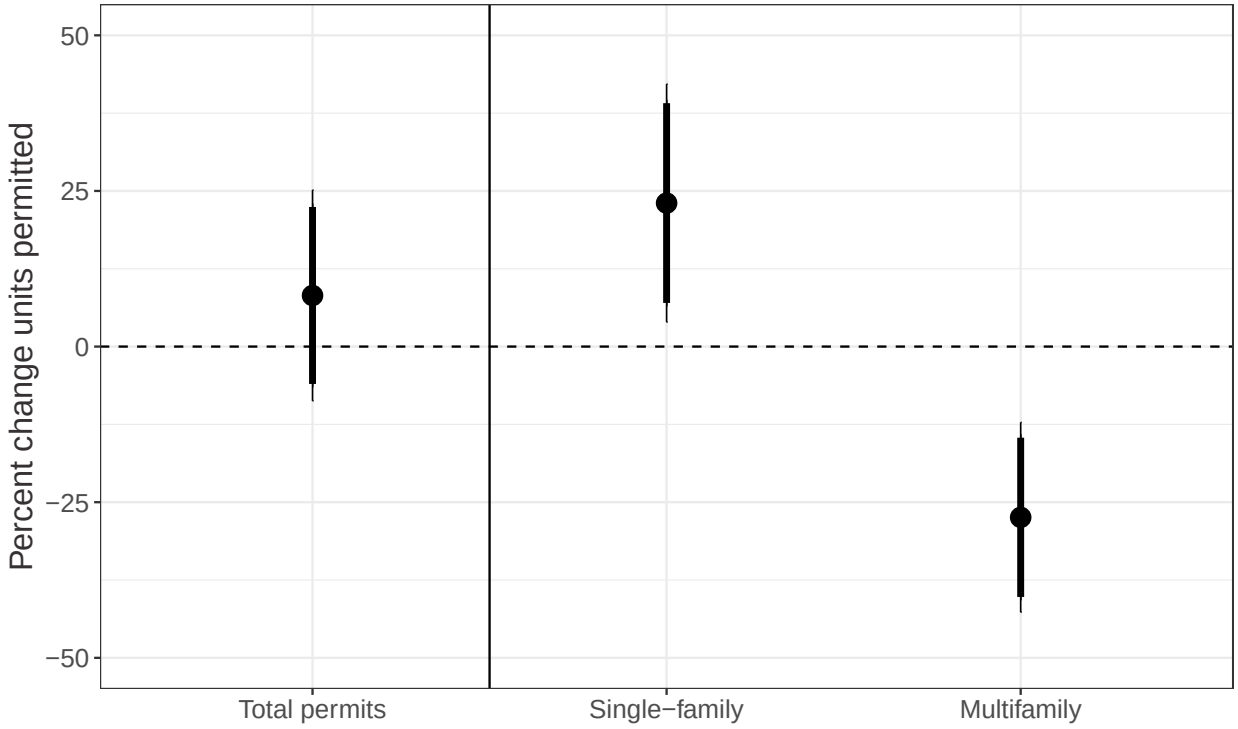
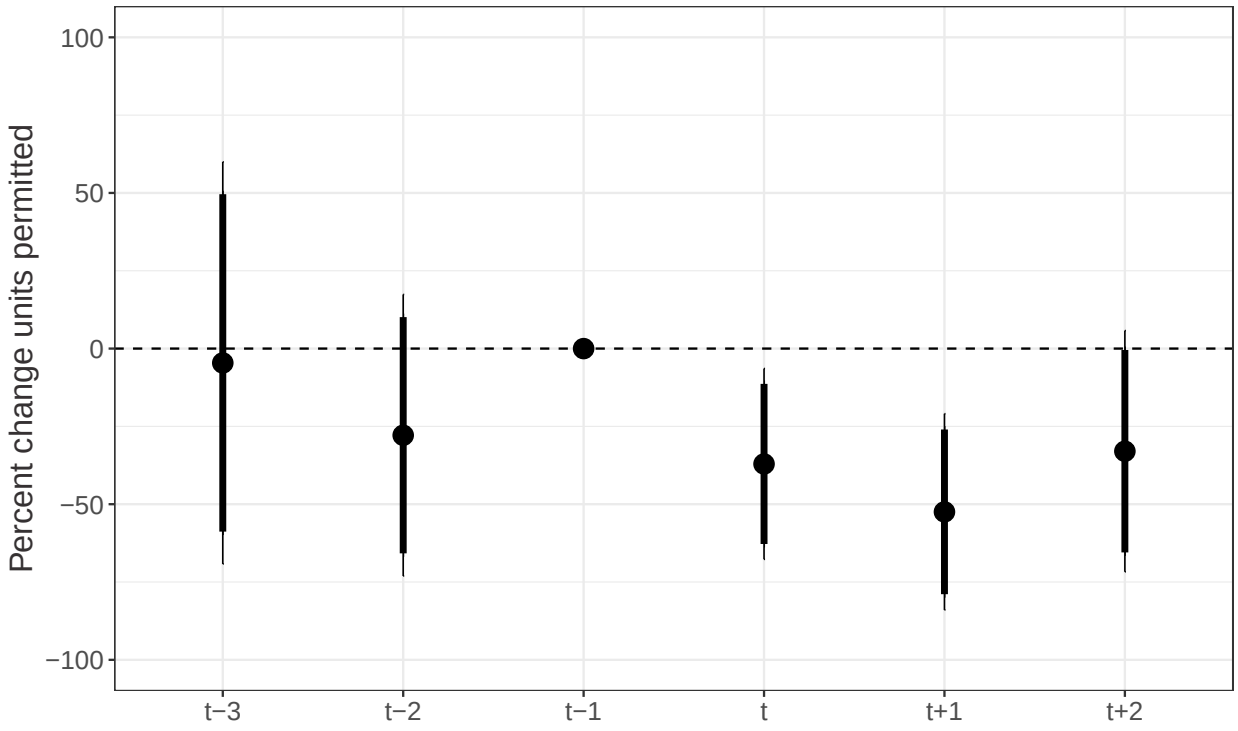


Figure G-6: Treatment effects computed using the nonparametric generalization of difference-in-differences recommended by Imai, Kim, and Wang (2018). All treatment effects are computed with respect to the first pre-treatment year (t-1). Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).



H Tabular Results and Standard Errors for Spatial Outcomes

Table H-5: Effect of Conversion to Single-Member Districts on Units Permitted, Logged

	Total Units	Multifamily Units	Single-family units
	(1)	(2)	(3)
Single-member districts	0.392	0.254	0.117
	$p = 0.176$	$p = 0.316$	$p = 0.623$
Minority block groups	0.365	0.393	0.048
	$p = 0.097$	$p = 0.134$	$p = 0.761$
SMD:Minority block groups	−0.546	−0.491	−0.120
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.496$
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes
Observations	1,136	1,136	1,136
Note:			*p<0.05; **p<0.01; ***p<0.001

We compute the p-values in Table H-5 using a wild bootstrap with city-level clusters, as cities are the unit of analysis at which treatment assignment occurs, and within which we expect the most meaningful correlation among unobserved components of outcomes (Abadie et al., 2017). We report only p-values because using the standard errors from this procedure (computed as the standard deviation of the bootstrap distribution of $\hat{\beta}$) relies heavily on the asymptotic normality of $\hat{\beta}$ in a context where large-sample theory may not apply (Roodman et al., 2019).

To compute the confidence intervals in Figure 4, we transform the coefficients within each bootstrap replicate in the same way that we transform the coefficients from our model: for the effect of high-minority block groups under at-large,

$$g(\beta_{minority}) = (e^{\beta_{minority}} - 1) * 100 \tag{1}$$

and for the effect of high-minority block groups under districts,

$$g(\beta_{minority}, \beta_{districts*minority}) = (e^{\beta_{minority}+\beta_{districts*minority}} - 1) * 100 \tag{2}$$

We then compute a normal bootstrap confidence interval, using as the standard error the standard deviation of the bootstrap distribution of the $\hat{\beta}$s.

To ensure that our inference is still valid without this normality assumption, we report

confidence intervals from two alternative approaches in Figure H-7. First, we repeat the procedure above with a basic bootstrap, which yields slightly asymmetric but otherwise similar confidence intervals. A second approach that is asymptotically valid is to follow the wild bootstrap procedure described in Cameron, Gelbach, and Miller (2008) and implemented in R by Esarey and Menger (2019), substituting into the Wald statistic the transformed estimates and standard errors (computed using the delta method). Thus, the Wald statistic under the original approach,

$$W_i = \frac{(\hat{\beta}_i - \hat{\beta}_{mod})^2}{\hat{\sigma}^2_{\beta_i}}$$

becomes

$$W_i' = \frac{(g(\hat{\beta}_i) - g(\hat{\beta}_{mod}))^2}{\hat{\sigma}^2_{g(\beta_i)}}$$

where $g(\cdot)$ is one of the transformations in equations 1 and 2 above, $\hat{\beta}_i$ is a regression coefficient computed in bootstrap replication i, $\hat{\beta}_{mod}$ is the coefficient from the original model, $\hat{\sigma}^2_{\beta_i}$ is the estimated variance of $\hat{\beta}_i$, and $\hat{\sigma}^2_{g(\beta_i)}$ is the estimated variance of $g(\hat{\beta}_i)$ computed via delta method. This process again yields very similar confidence intervals.

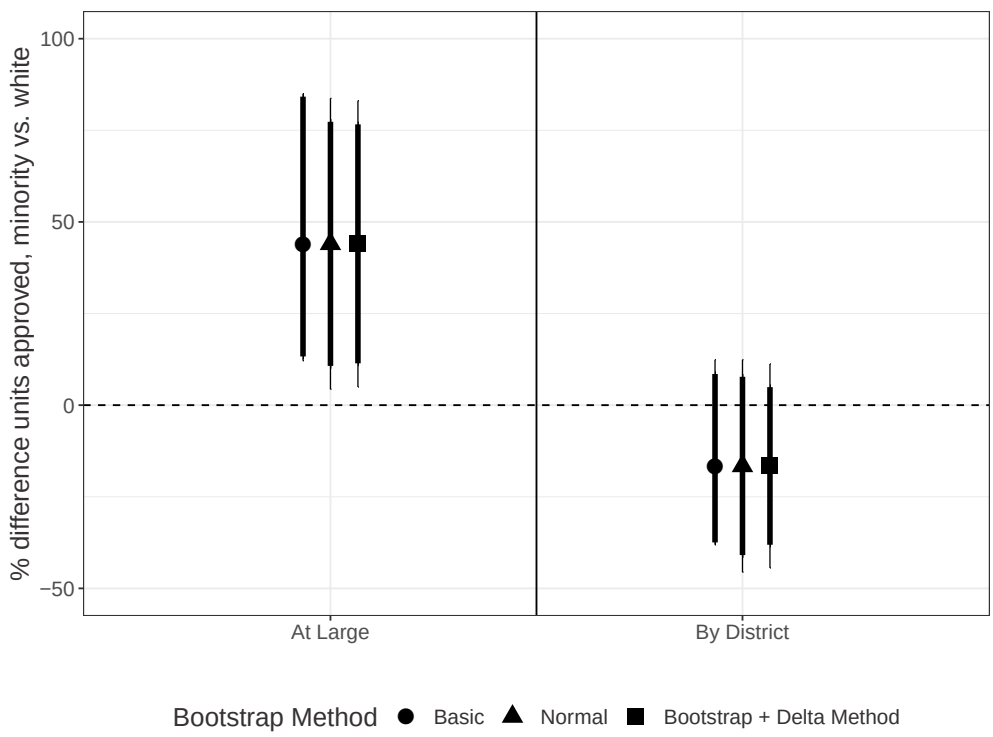


Figure H-7: Points indicate the percent difference in housing units permitted between minority block groups and white block groups. On the left are all block groups in at-large systems, including treated units pre-treatment. On the right are block groups in treated cities, post-treatment. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

Finally, we note that the main effect of minority block groups on total units, which falls shy of the 5% statistical significance threshold in Table H-5 ($p = 0.097$), becomes statistically significant when transformed from log points into percent change (left panel of Figure H-7). This inconsistency is a pitfall of using the log transformation (Feng et al., 2014), but the coefficient on minority block groups is not itself central to our argument. What matters is the consistently large and statistically significant effect (whether in log points or percent change) of the interaction of district elections and minority block groups, which means we can conclude with confidence that district elections reduce differential responsiveness to white as compared to minority neighborhoods' NIMBY interests.