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

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Abstract

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 district elections for city council, to causally identify how equalizing spatial representation changes the permitting of new housing. District elections decrease the supply of new multifamily housing, particularly in segregated cities with sizable and systematically underrepresented minority groups. But we also find evidence that district elections end the disproportionate channeling of new housing into minority neighborhoods. Our findings highlight a trade-off: at-large representation may facilitate the production of goods with diffuse benefits and concentrated costs, but it does so by forcing less politically powerful constituencies to bear the brunt of those 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 — public parks, transit hubs, or affordable housing — are bound to a physical location, meaning their benefits or costs are unavoidably spatially concentrated. While resolving conflict over the provision of these spatial goods calls for the democratic process (Valentini, 2013), equitable outcomes can only be expected if all geographic constituencies — each neighborhood within a city — have equal access to representation. The stakes of geographic representation are particularly high in the American 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, the distribution of spatial representation has the potential to reinforce or remedy existing disadvantage.

One 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 voice of these areas, strengthening 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 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 municipalities, allowing us to causally identify their effects. We focus on a key feature of electoral institutions affecting the relative influence of geographic constituen-

cies: 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.

In this paper, we estimate the causal effect of district elections on the supply–equity trade-off of new housing, a municipally-controlled land use with strong local opposition (Einstein, Palmer, and Glick, 2019). To do so, 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 switching 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.

Additionally, we contribute a framework for analyzing minority representation and electoral reform. Using election panel data, we measure city council control by race and the descriptive representation of each racial group relative to their population share within the municipality. Our approach reveals that control of California city councils is not exclusive to white majorities, nor is underrepresentation on city council always greatest among Latinos. Rather than relying on these heuristics, we identify the unique balance of power within each city, allowing for cleaner measurement of both preexisting representation gaps and of the effect of district elections.

Our findings are twofold. First, the switch to district elections decreases the permitting of multifamily housing — the type of housing most vehemently opposed by current residents

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[1]Also known as "by-trustee" or "ward" representation.

but also most essential to an affordable housing supply — primarily in cities where minorities are best positioned to benefit from the electoral reform. These are cities that are highly segregated and that had relatively small but vastly overrepresented racial majorities before undertaking reform. These conditional findings match existing research showing that district elections increase minority representation — the theoretical mechanism that drives our results — contingent on either the size of the minority population or its spatial segregation (Abott and Magazinnik, 2020; Meier et al., 2005; Trounstine and Valdini, 2008). Second, we present suggestive evidence 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 majority and minority constituencies.

Together, these findings support our theoretical contribution linking spatial representation to collective goods. Because at-large systems are more likely to underrepresent minority voters, 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 majority neighborhoods are already represented by at-large coalitions. No longer able to channel housing into politically weak minority neighborhoods, district-elected councils are forced to more evenly distribute new housing across neighborhoods — and consequently demographic groups.

But this decrease in the supply of new housing threatens equity both locally and nationally. Limiting new housing not only raises rents (see Been, Ellen, and O'Regan, 2019, for review), but also prices out those seeking to move to cities with high upward income mobility, exacerbating income inequality (Ganong and Shoag, 2017) and entrenching existing patterns of racial segregation (Trounstine, 2018). Absent the large-scale production of housing, rising prices from a further constrained supply will disproportionately harm low-income communities, a constituency that district elections are meant to empower. We close with how to balance descriptive representation, distributive equity, and the necessary supply of housing, including lessons for other policies with concentrated costs and diffuse benefits.

The Spatial Scale of Representation

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. 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).

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). They thought at-large elections would produce city council members interested in the outcomes of the city as a whole, not in the parochialism and patronage politics of their district. 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; low turnout in local elections exacerbates this participation gap (Hajnal and Trounstine, 2005). So long as an at-large city maintains a majority white turnout with racially polarized voting, a white coalition can secure an all-white city council. By contrast, cities that can draw districts where the underrepresented minority constitutes a local majority assure a minimal standard of descriptive representation.

The connection between institutional design and minority disenfranchisement has not gone 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. Though successful litigation was limited by a high standard of proof, Southern cities that were compelled to switch to district elections under the VRA experienced

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increased minority descriptive representation (Sass and Mehay, 1995).

The effect of district elections on policy outcomes is less clear. Tausanovitch and Warshaw (2014) find little evidence that policy responsiveness varies between the two institutional forms. However, they do not investigate land use or distributional policies, an omission motivating our research in two ways.[2] First, land use is widely considered the primary domain of local politics, one almost exclusively controlled by the municipal government (Peterson, 1981). Second, whereas Tausanovitch and Warshaw (2014) compare citizens' ideology to the ideological placement of policy outcomes, local housing policy has been found to lack a strong ideological dimension (Marble and Nall, Forthcoming).

In fact, city residents and local politicians alike are keenly aware that the spatial scale of representation matters for how resources are distributed across neighborhoods. Quoting a white constituent who spoke out in favor of moving to district elections, Anaheim city council member Jose Moreno recounts:

> She was saying, 'the one thing I noticed in my neighborhood is, the more Latinos moved in, the worse services we were getting — I don't see our streets getting taken care of, I see divestment happening from our neighborhoods. And what I've come to understand is, it's not that Latinos diminish the neighborhood; it's that politicians diminish Latinos, and when they move into a neighborhood that neighborhood is not invested in.'[3]

It appears this voter's diversifying neighborhood is losing its electoral importance for the white at-large coalition. Supporting district elections, this voter believes she can get the most out of city government when her elected representative is tied to her neighborhood, establishing a direct accountability mechanism for how land is used and resources allocated *in that space*.

Just as district-elected representatives are rewarded for bringing desirable resources into their districts, they are incentivized to shift burdensome LULUs out of their districts. In theory, were a LULU in the city's collective interest, every other council member would vote

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[2] "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).

[3] Conversation with Jose Moreno, 01/13/20.

in favor of the siting proposal, and it would pass. But city councils often operate according to a norm of legislative logrolling, wherein the council defers to the preferences of the member representing the host neighborhood. This local deference is repaid in future siting decisions, allowing everyone to survive the political threat of a LULU when it is proposed for their 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), and greater restrictions on the siting of group homes (Clingermayer, 1994). Most closely related to our own work, Mast (N.p.) finds that a nationwide sample of cities that switched to district elections between 1980 and 2018 experienced a decline in housing units permitted annually. But while Mast (N.p.) uses a sample of cities that includes those who chose to switch to district elections, we examine cities that switched to district elections due to conditionally exogenous legal pressures. Furthermore, we measure changes in the spatial distribution of new housing, directly capturing the equity implications of the reform.

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 local government, proposals for new development travel through one of two paths: "by-right" and discretionary review. By-right proposals are allowed under existing regulations. 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.[4]

Like any regulatory regime, the discretionary review of housing proposals generates its own political economy. But unlike the distributive boon of pork barrel spending, new housing is usually seen as a distributive burden to nearby residents. Development brings noise and congestion, harming quality of life. New residents may consume more in public services than they provide in tax revenue, raising the tax burden of existing property owners (Hamilton, 1976). Biases against 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). Counterintuitively, renters may not only oppose new market-rate 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).

Still, housing preferences 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 and white, and to contribute more in tax revenue than they use in public services, mitigating some of the above concerns. Labeled "cumulative zoning,"

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[4]In California, planning commissioners are not only appointed by city council, but their decisions may be appealed to city council, keeping their verdicts in line with council preferences.

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 resulting 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 only, or housing channeled outside of their neighborhoods (Einstein, Glick, and Palmer, 2020). By overrepresenting the majority, at-large elections increase the likelihood of new housing being channeled into effectively disenfranchised minority neighborhoods. In contrast, district elections have the potential to empower underrepresented minority neighborhoods to participate in city council land use decisions, lowering the overall quantity while equalizing the spatial distribution of new housing.

Hypotheses

Still, we do not expect all types of housing or all places to be affected equally — even within the group of cities deemed appropriate for conversion to districts under the CVRA. In this section, we discuss the conditions under which we expect conversion from at-large to district elections to decrease a city's permitting of new housing. First, we expect that *district elections will primarily decrease the permitting of multifamily rather than single-family housing (H1)*, for two reasons. First, not only does single-family housing tend to generate less neighborhood opposition (Marble and Nall, Forthcoming), but it requires more space per unit, and thus is usually built on the outskirts of a city, where there are fewer neighbors to provoke. Second, multifamily housing is most likely to require discretionary review, which is vulnerable to NIMBY ("Not in my backyard") pressure. Because of cumulative zoning, single-family homes are rarely subject to the same political process.

Three further conditions are related to the types of cities where we expect districts to most dramatically reshape the political process that generates new housing. The conditions

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under which an at-large system may be held legally responsible for minority vote dilution are succinctly stated by the *Gingles* test, the standard that plaintiffs must meet in order to win cases against at-large voting districts under the federal VRA. To prove that district elections are likely to increase minority representation, plaintiffs must show that the relevant racial or language minority group is "sufficiently large and geographically compact to constitute a majority in a single-member district"; that this group is "politically cohesive"; and that the majority usually votes as a bloc to defeat the minority's preferred candidates (*Thornburg v. Gingles*, 478 U.S. 30, 53 n. 21 (1986)). But the CVRA lowered the bar set by the *Gingles* test, requiring only that plaintiffs show evidence of "racially polarized voting," and thereby creating variation in the levels of segregation, demographic composition, and majority political power among treated cities. In keeping with recent studies that have identified conditional effects of district elections (Abott and Magazinnik, 2020; Meier et al., 2005; Trounstine and Valdini, 2008), our next set of hypotheses focuses on these city-level moderators.

First, district elections are more likely to improve descriptive representation when minorities are 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 racial composition of a city council. Cities with high levels of segregation are also likeliest to create the initial conditions for an unequal distribution of housing. If majority voters were evenly distributed throughout the city, no neighborhood could serve as a "dumping ground" for unwanted housing and district elections would have no imbalance to correct. Thus, our second hypothesis (*H2*) is that *district elections will decrease the permitting of multifamily housing in residentially segregated cities.*

Next, existing research has found the effect of district elections on descriptive representation to be greatest in cities with large shares of minority residents, where majority-minority districts can be more easily drawn (Abott and Magazinnik, 2020; Meier et al., 2005; Trounstine and Valdini, 2008). Most studies operationalize the minority population as one racial

group. While this approach may be appropriate for studying the federal VRA, which focused on at-large districts with underrepresented Black minorities in the American South, it is inadequate for California cities, which often include substantial populations of multiple racial groups that may or may not act as a unified political bloc for the purposes of voting rights claims (Sette, 2020). We therefore shift our focus to the population share of the dominant racial majority, defined as the group that systematically wins the most council seats.[5] District elections have the ability to dramatically change the council composition — and thus policy outcomes — in cities where the dominant group on council composes a relatively small share of the city's population. We therefore predict that *district elections will decrease the permitting of multifamily housing in cities with low majority populations (H3)*.

For a city to produce the initial inequality in unwanted housing for districts to correct, minority neighborhoods must lack council representation to champion their interests under an at-large system. We predict that the most dramatic policy changes will occur in cities where the racial majority on council is most overrepresented relative to its share of the city's population. Thus, *H4* states that *district elections should decrease the permitting of multifamily housing in cities where the council majority is significantly overrepresentative of that racial group's population share.*

Finally, 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, making it harder for city council members to channel unwanted housing into any given community. Because previously underrepresented areas are likely to be minority neighborhoods, we expect that any positive relationship between minority neighborhoods and new housing permitted will weaken under district elections. In other words, *race will become less predictive of a neighborhood's housing burden under district elections than at-large, all else equal (H5)*. Together, these predicted effects illustrate the

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[5]This technically includes dominant groups that capture a plurality, not a majority, of the council; however, in practice the dominant group captures a majority of the council approximately 90% of the time. For ease of interpretation, we use the term "majority" throughout.

connection between spatial representation and the supply–equity trade-off of collective goods.

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. Comparing cities that switch to district elections to those that remain at-large is no less prone to unobserved confounding, as cities that undertake successful reform are likely to already have stronger political representation of groups that stand to gain from district elections.

We advance our understanding of the causal effect of voter aggregation by leveraging the staggered timing of switching to districts within a group of comparable cities in the wake of the California Voting Rights Act. Rather than making potentially biased comparisons between cities that switched to districts and those that remained at-large, as most previous studies have done, we compare a set of *early switchers* to a similar set of *later switchers* in a generalized difference-in-differences framework. This analysis yields valid causal inferences as long as we account for any characteristics that both drive a city to switch to districts early on and simultaneously affect housing outcomes.

In this section, we briefly describe the context of the CVRA and explain how it motivates the construction of our analysis subgroup. Based on a set of interviews with key participants in CVRA litigation that we conducted in January 2020, we argue that, for a specific and readily identifiable type of city, there was a great deal of random chance in the timing of treatment. Simply focusing on this set of cities greatly reduces the threat of unobserved confounding; however, we additionally control for time-varying measures of these cities' housing markets and minority political strength, as well as city-specific time trends. The combination of these quasi-experimental and model-based approaches makes us confident

that our estimates represent the causal effect of district elections on housing outcomes. We provide empirical validation of our identification assumptions in the Results section below.

The CVRA's lowered standard for minority vote dilution meant that numerous cities across California could in principle face successful litigation and be required to switch to district elections. Furthermore, the CVRA incentivized litigation by making defendants — budget-constrained municipal governments — responsible for all associated legal and court fees, even in the case of an out-of-court settlement. However, switches happened slowly at first, accelerating only in 2016.[6] Given the large number of equally appropriate candidates for legal action, what determined the timing of treatment among cities that eventually switched to districts?

Direct legal pressure to switch to districts requires the identification of a plaintiff, a city resident who could claim harm from at-large elections. In general, plaintiffs came from one of three sources. First, they could emerge from internal political networks: in Santa Barbara, for instance, the suit was brought by a group of local activists who had been engaged in civil rights work in the city for decades.

Alternatively, plaintiffs could be recruited by one of the national or regional activist networks that became involved in CVRA litigation: the Mexican American Legal Defense and Educational Fund (MALDEF) or the Southwest Voter Registration Education Project (SVREP). Although these groups were no longer operating under the strict *Gingles* test, they nonetheless wanted to focus on cities that clearly stood to gain from district elections. Using in-house demographers, they identified and recruited for legal action at-large cities with histories of minority underrepresentation; where the minority group constituted at least 20% of the population such that majority-minority districts could be drawn; and where the total population was over 50,000 people, as MALDEF leadership believed that smaller cities would not benefit as much from district elections.[7] But due to internal capacity constraints and

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[6] See Appendix Figure A-1 for the proportion of cities that had district elections from 2010 to 2019.

[7] While these groups initially focused on Latino minorities, cities with sizable underrepresented Black or Asian minorities are no less targetable under the CVRA, and no different in their expected response to treatment under our theory. We therefore apply these standards for any racial minority in the construction

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competing priorities — both SVREP and MALDEF have missions that extend beyond voting rights and serve areas beyond California — these groups did not ramp up their litigation efforts until 2018, when SVREP decided to prioritize legal action in the still at-large cities that they considered overdue for reform. Appendix D.3 includes more details on this history from our interviews with MALDEF's leadership.

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 various sizes and with more tenuous prospects of gaining minority council seats upon switching to district elections.

Thus, on the whole, switching to districts under the CVRA was not a random process. The earliest switchers tended to be larger cities with significant disparities between their minority populations and minority council representation, which would hand reformers a meaningful and high-probability victory under the as-yet untested law. By contrast, cities targeted with litigation more recently have been, on average, smaller and less carefully chosen, as the CVRA's legal standard has been well-tested and the plaintiff's likelihood of success understood to be high. Nonetheless, *conditional* on being one of the numerous cities that MALDEF initially deemed appropriate for legal action, there was a considerable element of random chance in the timing of switching. With much on their agendas, MALDEF and SVREP had neither the resources nor the consistent institutional focus on voting rights cases to target all of these cities at once, and there was no coordinated strategy on the part of either organization to target the most unequal or vulnerable cities first.[8]

Motivated by the CVRA's unique context, we conduct a generalized difference-in-differences analysis using a subgroup of 60 cities that have, at any point between the CVRA's initial passage and the present day, switched or committed to switching to district elections, *and*

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of our sample.

[8]Source: Conversation with Thomas Saenz, President and General Counsel of MALDEF, 01/13/20, and Lydia Camarillo, President, SVREP, 02/06/20.

who satisfy MALDEF's more stringent criteria. Henceforth referred to as the "subgroup," this sample yields causal estimates under the assumption that the timing of switching is conditionally exogenous to our housing outcomes of interest, after applying statistical controls. Most critically, our time-varying controls include measures of minorities' past political success to account for cities with stronger internal political organization selecting into districts earlier (or, conversely, for cities with particularly low minority representation presenting themselves as most targetable to outside groups). Although we see no empirical evidence that cities selected into districts on the basis of their past housing outcomes (see Figure 1) — and, after reviewing hundreds of city council meeting minutes, no evidence that housing entered cities' deliberations about switching to districts — we nonetheless include several housing market indicators, such as vacancy rate, home ownership rate, and median home value.

Beyond causal identification, our subgroup yields a substantively meaningful, policy-relevant estimate, interpretable as a local average treatment effect for the kind of city that meets a minimum standard for benefiting from district elections. Thus, while our estimates are not generalizable to all cities, they are generalizable to precisely those cities that would realistically entertain the choice between the two systems.

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 136 cities that switched to district elections, the year of its first district election, which we use as the date of treatment throughout this study.

Subgroup Definition and Racial Representation

For the reasons discussed in the previous section, we define our analysis subgroup as California cities that would ultimately switch to districts, that have more than 50,000 residents, and where there is at least one underrepresented minority that comprises more than 20% of the population. We measure total population and minority population shares using U.S. Census data, and identify underrepresented minorities using the California Elections Data Archive (CEDA). CEDA's data contains the names and vote counts of every candidate who ran for city council in California from 1998 to 2019, allowing us to compute the number of Asian, Black, Latino, and non-Hispanic white city council candidates who won office in every city-year.[9] For each group, we define "past electoral success" in year t as the number of seats won by its members divided by the total number of council seats up for election in the city over the prior twelve years ($t - 12$ through $t - 1$).[10] Finally, we compare each racial group's past electoral success to its population share at the time of the city's first district election. A group is "underrepresented" if its past electoral success is less than 85% of its population share. This eliminates cities with minority populations that have been relatively successful in winning elections — cities that would not have been priority candidates for CVRA litigation in the eyes of reformers.

Our measurement of past electoral success also supplies a framework for the study of representation and the CVRA. Because multiple racial groups may be underrepresented within the same city, we should not always expect Latinos to benefit the most from district elections. To identify which racial group is most underrepresented, we select the one with the largest gap between its population share and past electoral success, out of all the underrepresented groups that comprise more than 20% of the city's population.[11] If there is not an

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[9]CEDA's data only includes names, not ethnicities, of candidates, so we coded ethnicity using the `wru` package in `R` (Imai and Khanna, N.d.).

[10]Twelve years is the longest fixed time period we can use, given that our housing panel begins in 2010 and CEDA's election data goes back to 1998.

[11]We follow the heuristic of 20% used by reformers, as it is a rough lower bound on the size of a group that could reasonably benefit from district elections: given 5-7 districts and generous assumptions about the group's compactness and voter turnout, 20% is approximately what is needed for a citywide minority to

underrepresented minority group with more 20% of the city's population, we code the most underrepresented group as "None." Table 1 shows the distribution of most underrepresented minority groups among all 136 cities that have agreed to switch to district elections, as well as the 60 cities that constitute our subgroup. While Latinos are the most underrepresented minority in 84% of subgroup cities, Asians constitute the remainder — a sizable 16%. Moreover, Table 1 shows that over one-third of all switchers fall short of reformers' conditions, having no clear underrepresented minority of sufficient size.

Second, our data allow us to identify which racial group has had the greatest electoral success in city council races. We define this "council-dominant majority" as the one with the highest past electoral success as of when they switch to districts. Unsurprisingly, whites dominate the council the vast majority of the time; still, 8% of all switchers and of subgroup cities have nonwhite council-dominant majorities, suggesting that the heuristic of white-dominated councils and Latino underrepresented minorities is not perfectly reliable. In fact, several CVRA lawsuits have been launched by Asian plaintiffs in cities with white or Latino-dominated city councils. We incorporate this nuance in all subsequent analyses throughout the paper.

Table 1: Council Representation by Racial Group

	Asian	Black	Latino	White	None
All switchers (136 cities)					
Council-dominant majority	0.02	0.01	0.05	0.92	0.00
Most underrepresented minority	0.09	0.00	0.56	0.00	0.35
Subgroup (60 cities)					
Council-dominant majority	0.03	0.03	0.02	0.92	0.00
Most underrepresented minority	0.16	0.00	0.84	0.00	0.00

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

constitute a district's electoral majority.

from 2010 to 2019 collected by the U.S. Census Building Permits Survey. These data include the number of total units permitted as well as the distribution of new units between single-family and multifamily housing.

The model specification used to test *H1* is given by Equation 1:

$$Y_{it} = \beta_0 + \beta_1 district_{it} + \mathbf{X}_{it}\gamma + \rho_i + \eta_t + \zeta_i(i*t) + \varepsilon_{it} \tag{1}$$

where Y is logged units permitted in city i and year t; *district* is a binary indicator for having district elections in place; ρ is a city fixed effect; η is a year fixed effect; and ζ is a city-specific linear time trend. We additionally account for time-varying city attributes: $\mathbf{X}$ includes percent non-Hispanic white, percent Black, percent Hispanic, median income, homeownership rate, home vacancy rate, and median home value (drawn from 5-year ACS estimates from 2010 through 2019) as well as past electoral success for the city's most underrepresented minority, as constructed for Table 1.[12] Huber-White standard errors are clustered at the city level.

To test our next three hypotheses — the conditional effects of district elections on seg-regated cities (*H2*), cities with relatively small majority populations (*H3*), and cities with significant majority overrepresentation (*H4*) — we first define data-driven thresholds for low and high values of each variable. We measure citywide segregation using the Theil's H index as calculated in Trounstine (2016). We define majority population share based on 5-year estimates from the American Community Survey (ACS) for the group identified as the council-dominant majority in Table 1. To compute majority control of council, we scale the majority group's past electoral success (defined in "Subgroup Definition and Racial Representation") by its population share; thus, values greater than one reflect descriptive overrepresentation, and values less than one reflect underrepresentation. Finally, we assign all subgroup cities to terciles according to their pretreatment segregation, majority popu-lation share, and majority council control. Distributions of these variables as well as the

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[12]We impute missing data for ACS control variables using Amelia (Honaker, King, and Blackwell, 2011).

cutpoints that determine assignment into terciles are shown in Appendix Figure A-2.

To assess conditional effects, we interact the treatment indicator with an indicator for being in the top or bottom tercile on segregation, majority population share, and majority overrepresentation, leaving out the middle tercile of data. This modeling strategy directly compares the treatment effect of district elections across cities with high and low values of these moderators; thus, it 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). For ease of interpretation, we define the baseline category in each model as the condition where we expect to see conditional effects: *high* segregation, *low* majority population share, and *high* past majority overrepresentation on council. Our full specifications are given in Equation 2:

$$Y_{it} = \beta_0 + \beta_1 district_{it} + \beta_2 (district * low\ segregation)_{it} + \mathbf{X}_{it}\gamma + \rho_i + \eta_t + \zeta_i(i * t) + \varepsilon_{it}$$

$$Y_{it} = \beta_0 + \beta_1 district_{it} + \beta_2 (district * high\ majority\ population)_{it} + \mathbf{X}_{it}\gamma + \rho_i + \eta_t + \zeta_i(i * t) + \varepsilon_{it}$$

$$Y_{it} = \beta_0 + \beta_1 district_{it} + \beta_2 (district * low\ majority\ control)_{it} + \mathbf{X}_{it}\gamma + \rho_i + \eta_t + \zeta_i(i * t) + \varepsilon_{it}$$

$$(2)$$

with racial composition variables omitted from $\mathbf{X}$, as they are highly correlated with the tercile indicators.

Spatial Outcomes

Next, we apply our theory to the spatial distribution of the housing supply. To test our fifth hypothesis, we constructed a dataset of zoning changes emerging from the discretionary review process by coding the minutes of every planning commission and city council meeting from 2011 through 2018.

The intensity of this data collection required sampling cities. We selected cities that would maximize our ability to detect a treatment effect should one exist. First, we selected

cities with multiple years of post-treatment data. Second, we chose cities that had a white majority large enough to potentially dilute the representation of a Latino minority via bloc majority voting. Third, we chose cities large enough to generate enough new permits across an array of neighborhoods that an effect on spatial distribution 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.[13] Although these cities are larger and more diverse than the average California city, we believe our spatial findings capture a mechanism generalizable to other medium to large, segregated cities.

Within our six 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.[14] 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. We geocoded these decisions to the Census block group level and merged them with time-varying socioeconomic variables drawn from the ACS. 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.

We examine the distributive equity of the housing supply by estimating the moderating effect of a neighborhood's racial composition on its annual change in buildable capacity. Our dependent variable is log housing units approved annually via discretionary review. We classify every block group in the six treated and control cities as "white" or "minority" using cutpoints defined by the top and bottom tercile of percent non-Hispanic white in each city prior to treatment. As before, we remove the middle tercile of data.[15]

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[13]Table A-2 shows demographic data for treated and control cities. Of note, Ventura held their first district election in 2018, which is accounted for in our difference-in-differences model.

[14]Coding decisions are discussed in Appendix Section D.4.

[15]Appendix Figure C-8 visualizes the raw permit data over time by block groups within cities.

To measure the effect of district elections within cities, we interact the treatment with an indicator for being a minority block group. This interaction signifies whether district elections affect the housing supply differently within minority block groups compared to white block groups. We use this interaction to measure the causal effect of district elections on the equity of the distribution of housing between white and minority neighborhoods. Our estimating equation is:

$$Y_{bit} = \beta_0 + \beta_1 district_{it} + \beta_2 (district * minority)_{bit} + \mathbf{X}_{bit}\gamma + \rho_i + \eta_t + \zeta_i(i * t) + \varepsilon_{bit} \quad (3)$$

where Y is log housing units approved via discretionary review in block group b in city i and year t, $minority$ is an indicator for being a minority block group, $\mathbf{X}$ is a vector of time-variant, block group-level controls (enumerated above), ρ is a city fixed effect, η is a year fixed effect, and ζ is a city-specific time trend. We estimate standard errors using a wild bootstrap (Cameron, Gelbach, and Miller, 2008) clustered at the city level, as that is the unit of analysis at which treatment assignment occurs, and within which we expect the most meaningful correlation among unobserved components of outcomes.

Results

First, we present a visual assessment of parallel pretreatment trends between treated and as-yet untreated cities in our subgroup. This check rules out two major threats to causal inference in this setting: selection into district elections on the basis of past permitting behavior, and preemptive changes to housing outcomes in anticipation of electoral reform. We then present our results in the same order as our hypotheses.

We generate Figure 1 by coding each subgroup city's time to treatment from $t - 3$ to $t + 3$, where t is the year of the first district election. We construct each city's associated "control" set out of all other cities in the subgroup that would not be treated over the same calendar years (though they would be treated at time $t + 4$ or later). Then, we plot the

raw outcomes for treated units from $t - 3$ to $t + 3$ in red, and the outcomes for all their associated controls in gray, fitting a linear trend on either side of t. Because we focus on similar, eventually treated cities, it is unsurprising that these trend lines are very close to one another in both levels and slopes over the pretreatment period.[16]

Figure 1: Pretrends, Logged Multifamily Permits (Subgroup)
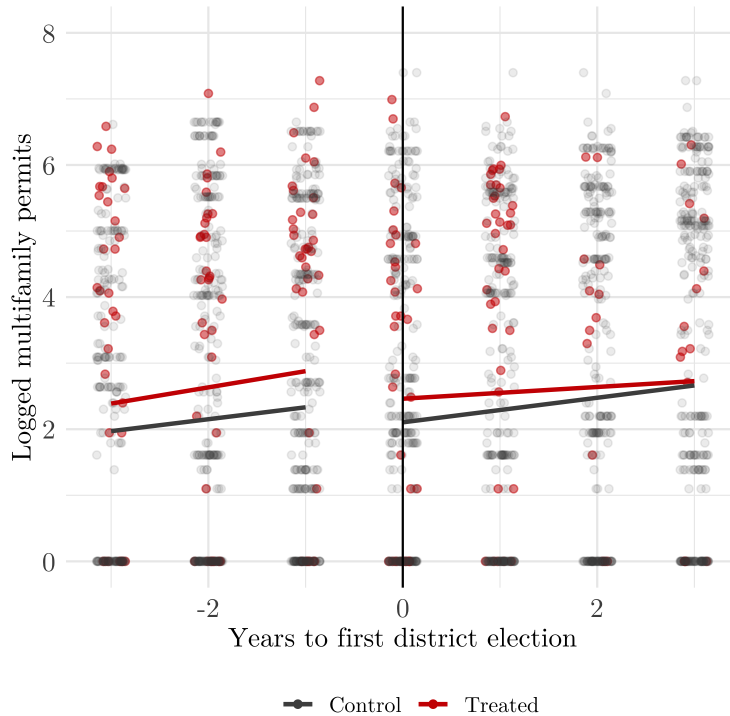
Effects on the Aggregate Supply of Housing

We first look at the overall effect of district elections on the number of housing units permitted annually for our subgroup cities. Column 1 of Table 2 shows that switching to districts decreases the permitting of multifamily housing units by 0.81 log points or 56 percent ($p = .08$). By contrast, Appendix Table B-3 shows that the effect on single-family housing is substantially smaller and too noisy to be meaningful. This pattern of results is consistent with multifamily housing being both less desirable and more vulnerable to NIMBY pressure

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[16]In Appendix Figure B-3, we verify that parallel trends also hold among the cities in the top and bottom terciles on segregation, majority population size, and majority council control.

via discretionary review compared to single-family housing.

Testing *H2*, within cities with high levels of segregation, district elections cause a 1.23 log point or 71 percent decrease in the permitting of multifamily housing ($p < .05$). The interaction term is positive but noisy, suggesting that cities with lower levels of segregation may experience less change from district elections. We next look at the size (*H3*) and overrepresentation (*H4*) of the racial majority group compared to the combined minority populations. In cities where the electorally dominant racial group composes a relatively small share of the population, district elections cause a 1.36 log point or 74 percent decrease in multifamily housing permitting ($p < .01$). Likewise, in cities with high levels of majority overrepresentation, district elections cause a 1.41 log point or 75 percent decrease in multifamily housing permitting ($p < .05$). The positive interaction term in both models suggests that the effect of district elections is smaller and less predictable in cities with larger and less overrepresented majority populations.

Robustness Checks

We report robustness checks for the analyses where we find the most significant effects on the aggregate housing supply: Columns 2-4 of Table 2 (*H2–H4*). To assess model dependence, Appendix Tables B-5 to B-7 decompose the specification in Equation 2 into a bivariate model without fixed effects as well as models with city and year fixed effects, time-varying controls, and city-specific time trends. The effect of district elections is consistently negative before the addition of time-varying covariates or city-specific time trends.

One concern for identification is whether cities that switched to district elections were already permitting fewer housing units prior to the change in electoral system. As shown in Figure 1, there is no reason to suspect this was the case; however, as an additional check, we use Granger causality tests to detect any potential "treatment effects" that may have emerged prior to cities' switching to district elections. Appendix Figures B-4 to B-6 show that our conditional estimates are close to (and statistically no different from) zero prior to

Table 2: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with City Characteristics

	H1	H2	H3	H4
	(1)	(2)	(3)	(4)
Single-member districts	−0.805	−1.226*	−1.355**	−1.405*
	(0.459)	(0.612)	(0.493)	(0.610)
SMD*Low segregation		0.388		
		(0.872)		
SMD*High majority population			0.159	
			(0.585)	
SMD*Low majority control				0.244
				(0.864)
Percent non-Hispanic white	0.080			
	(0.162)			
Percent Black	−0.379			
	(0.299)			
Percent Hispanic	0.051			
	(0.171)			
Population (thousands)	−0.055	−0.119	0.034	−0.073
	(0.103)	(0.129)	(0.079)	(0.113)
Vacancy rate	18.206	41.479	39.184	21.877
	(20.706)	(27.165)	(21.331)	(26.500)
Home ownership rate	10.872	15.738	9.366	8.472
	(8.841)	(11.774)	(8.364)	(10.846)
Median home value (thousands)	−0.010	−0.006	−0.005	−0.013
	(0.008)	(0.010)	(0.011)	(0.009)
Median income (thousands)	0.024	−0.002	−0.012	0.025
	(0.074)	(0.093)	(0.082)	(0.084)
Past minority representation	1.549	−1.096	2.379	−0.115
	(2.732)	(3.086)	(2.706)	(2.491)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
City-specific Trends	Yes	Yes	Yes	Yes
Observations	597	399	397	397
R^2	0.573	0.563	0.595	0.608

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

the year of the first district election, t. In contrast, the estimates on multifamily housing are uniformly negative and stable 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 (e.g., Bertrand, Duflo, and Mullainathan, 2004; de Chaisemartin and D'Houltfœuille, 2018; Imai and Kim, 2019). In particular, Goodman-Bacon (2018) has shown that out of all the two-by-two difference-in-differences estimators into which one can decompose the treatment effect in a two-way fixed effects model, only estimators that compare treated and never-treated units are unbiased for their group ATTs. Although our analysis focuses on eventually treated units, several of the latest switchers in the subgroup committed to holding their first district elections after 2019 — the last year of our panel. Thus, we conduct a Goodman-Bacon decomposition treating these late switchers as "never-treated" units, since we do not actually observe them under treatment in our data. In Appendix Figure B-7, we plot the distribution of these unbiased comparisons against all other comparisons that constitute the reported treatment effect, focusing on the terciles where we detect conditional effects in Table 2. Although there are not enough distinct comparisons to properly assess whether the two distributions differ, the average treatment effects among only treated-"never treated" comparisons remain negative, if somewhat reduced in magnitude.

Effects on the Spatial Distribution of Housing

We now turn to 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.

We first assess pretrends on our variable of interest: the difference between the logged number of units approved in block groups with high and low concentrations of minority

residents within the same city. Taking the same approach that we used to produce Figure 1, we show in Figure 2 that treated and control units follow similar pretreatment trajectories.

Figure 2: Pretrends, Difference in Logged Units Approved (High Minority Block Groups Minus Low Minority Block Groups)
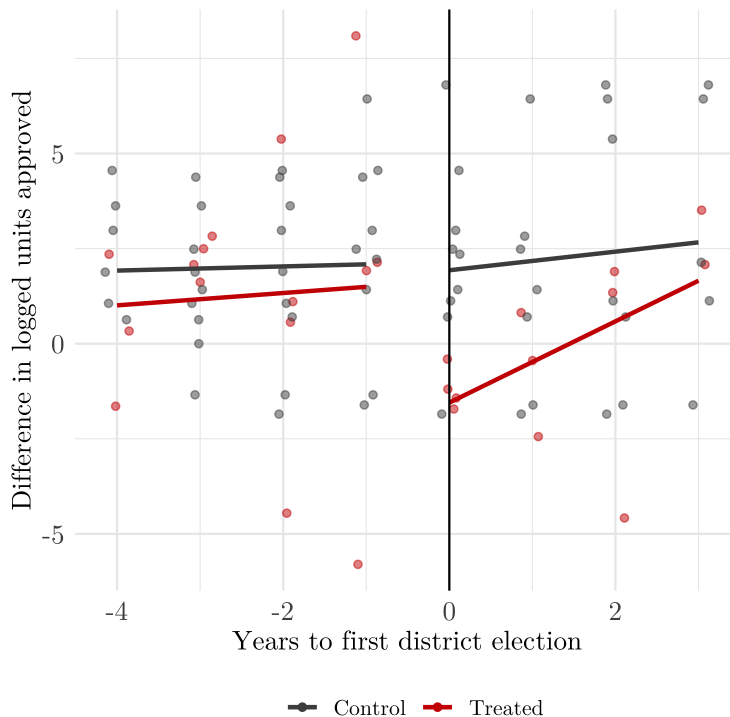

Table 3 shows the results of our spatial analysis in tabular form.[17] We find that moving to district elections significantly decreases the disparity in permitting between white and minority neighborhoods. Under at-large representation, minority block groups see 0.31 log points or 36 percent more housing units approved annually compared to their white block group counterparts, even after controlling for demographic and housing market covariates ($p < .01$). And while the effect of district elections for white block groups is not statistically different than zero, it is large and negative for minority block groups. Switching to district elections decreases the permitting of housing in minority block groups compared to white block groups by 0.42 log points or 35 percent ($p < .01$).

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[17]We report only p-values because using the standard errors from the wild bootstrap (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).

Table 3: Effect of Conversion to Single-Member Districts on Units Permitted, Logged

	Total Units	Multifamily Units	Single-family units
	(1)	(2)	(3)
Single-member districts	0.210	0.124	0.083
	$p = 0.126$	$p = 0.161$	$p = 0.444$
Minority block groups	0.311	0.370	−0.033
	$p = 0.000^{***}$	$p = 0.040^{*}$	$p = 0.521$
SMD*Minority block groups	−0.424	−0.358	−0.097
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.292$
Controls	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
City Trends	Yes	Yes	Yes
Observations	1,184	1,184	1,184

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

Figure 3 directly compares the racial disparity in permitting in at-large and district-based systems. On the left, we see the differential between white and minority neighborhoods under at-large elections, wherein minority neighborhoods take on 0.31 log points (36 percent) more units than white neighborhoods. On the right, under treatment, this differential falls to a statistically insignificant *negative* 0.11 log points (11 percent). The difference between these estimates represents the causal effect of districts on the racial equity in permitting. In other words, supporting *H5*, we find that districts reduce differential responsiveness to the NIMBY interests of white as opposed to minority neighborhoods.

Robustness Checks

Appendix Table C-8 decomposes the spatial outcomes model to test for sensitivity to different specifications. Further, to allay concerns that our results are being driven by one city, we sequentially drop each city from the sample in Tables C-9 and C-10. In every alternative sample and model specification, the effects are stable and statistically significant.

Our decision to define white and minority block groups with respect to each city's own

Figure 3: Difference in Housing Units Permitted between Minority Block Groups and White Block Groups, At-Large vs. District
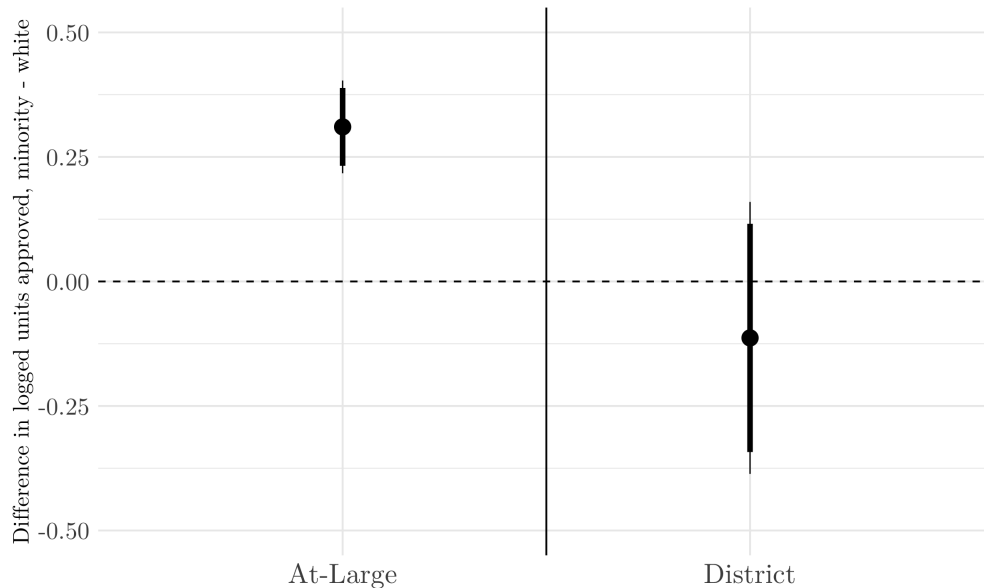
Notes: Left panel reflects all block groups in at-large systems, including treated units pre-treatment. Right panel reflects block groups in treated cities, post-treatment. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

distribution is driven by both theoretical and empirical concerns. We believe that the block groups with the highest *relative* minority concentrations in each city are likely to have the weakest political representation; moreover, our approach ensures balance in the number of white and minority block groups. However, applying uniform cutpoints across all cities — and thus ensuring that all "minority" block groups are indeed majority-minority — also has merits. We do so in Appendix Table C-11, and the interaction term grows even larger — unsurprisingly given the sharpened contrast between white and minority block groups.[18]

Finally, we use a Granger causality test to visualize how the housing trends of treated cities differ from those of control cities before and after switching to district elections. Appendix Figure C-9 shows that our coefficient of interest, the differential between white and minority block groups under district elections, is close to and statistically no different from zero prior to the year of the first district election (t). Upon treatment, the coefficient is

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[18]However, two cities are left without any minority block groups under this approach, so we favor the one taken in Table 3.

uniformly negative, although the greatest equity gains are concentrated immediately post-treatment.

Conclusion

Faced with racially polarized voting and neighborhood segregation, civil rights advocates have viewed district elections as a pathway to descriptive and, even more importantly, substantive representation for racial minorities. With carefully drawn districts, previously underrepresented neighborhoods can be nearly guaranteed a voice in local government. Our research contributes to a broad assessment of the consequences of this reform in two ways.

First, we find that district elections constrain the ability of city councils to permit new housing. However, segregated cities with sizable and systematically underrepresented minority groups — where reformers can most easily draw majority-minority districts — experienced the strongest effects. Our conditional results affirm findings from the growing literature on this reform: district elections interact with the underlying political landscape. Researchers studying this reform should test for conditional effects, and cities that do not meet these criteria may wish to pursue other aspatial reforms.

Second, we present evidence that district elections spatially disperse the concentration of new housing, breaking the correlation between minority block groups and unwanted development. While this may be in the hyperlocal, short-term interest of newly empowered minority voters, the restriction of the multifamily housing 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 trade spatially concentrated inequalities (new housing units) for a spatially diffuse burden (citywide housing costs).

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 testing and vaccination facilities (e.g., Gray, 2020). 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 reform bundled the individual policy decisions and removed substantial discretion from the legislature. We suggest a similar reform for housing permitting.

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 permitting, district elections can be paired with top-down pressure from the state government via withholding intergovernmental transfers (e.g., Elmendorf, 2019). Under at-large elections, this top-down pressure would channel housing into underrepresented minority neighborhoods, exacerbating 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 simultaneously generate new housing to counter rising prices while equitably distributing its spatial burden.

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. Institutional design to overcome the problem of allocating concentrated costs should move beyond this trade-off to the pursuit of *both* goals.

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Supplemental Information for "The Supply–Equity Trade-off: The Effect of Spatial Representation on the Local Housing Supply"

Contents

A Descriptive Statistics

Figure A-1: Proportion of California Cities with District Elections over Time
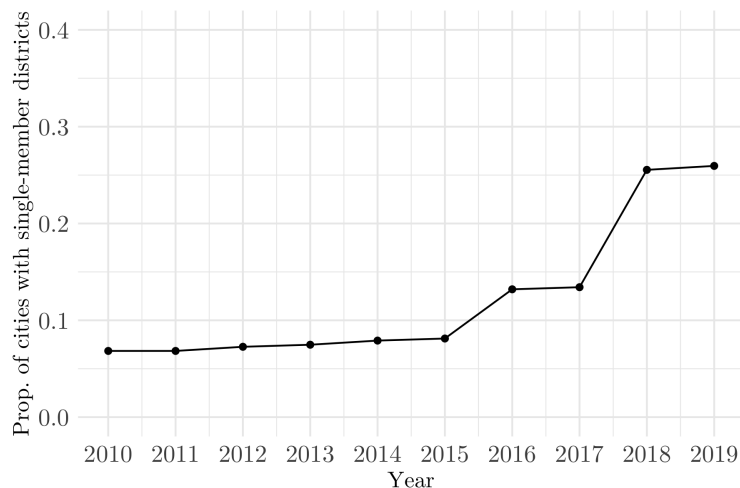
Table A-1: Characteristics of Cities by Type

	Mean (Untreated)	Mean (All switchers)	Mean (Subgroup)	p-value of difference, all switchers vs. untreated	p-value of difference, subgroup vs. untreated
Population					
Number of people	30,258	78,404	102,951	0.00	0.00
Percent non-Hispanic	48	43	36	0.02	0.00
Percent Black	3	5	6	0.01	0.01
Percent Asian	10	11	14	0.25	0.05
Percent Latino	29	29	33	0.89	0.11
Past electoral success					
Prop. of seats w/Latino candidate elected	0.18	0.11	0.09	0.00	0.00
Prop. of seats w/Black candidate elected	0.03	0.03	0.05	0.73	0.32
Prop. of seats w/Asian candidate elected	0.03	0.04	0.04	0.59	0.45
Prop. of seats w/white candidate elected	0.74	0.80	0.77	0.02	0.34
Income and land use					
Median household income ($)	71,310	66,856	63,859	0.11	0.02
Median home value ($)	499,112	412,141	395,692	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	1.00	0.42
Density (population per sq. mile)	4,132	4,102	4,599	0.92	0.20
Residential segregation (Theil index)	0.03	0.07	0.08	0.00	0.00
Housing outcomes					
Units permitted annually, single-family	44	83	93	0.00	0.00
Units permitted annually, multifamily	31	63	83	0.00	0.00
N	306	136	60		

Figure A-2: Distributions of Variables Used to Assess Conditional Effects

(a) Segregation

(b) Majority Population

(c) Majority Council Control
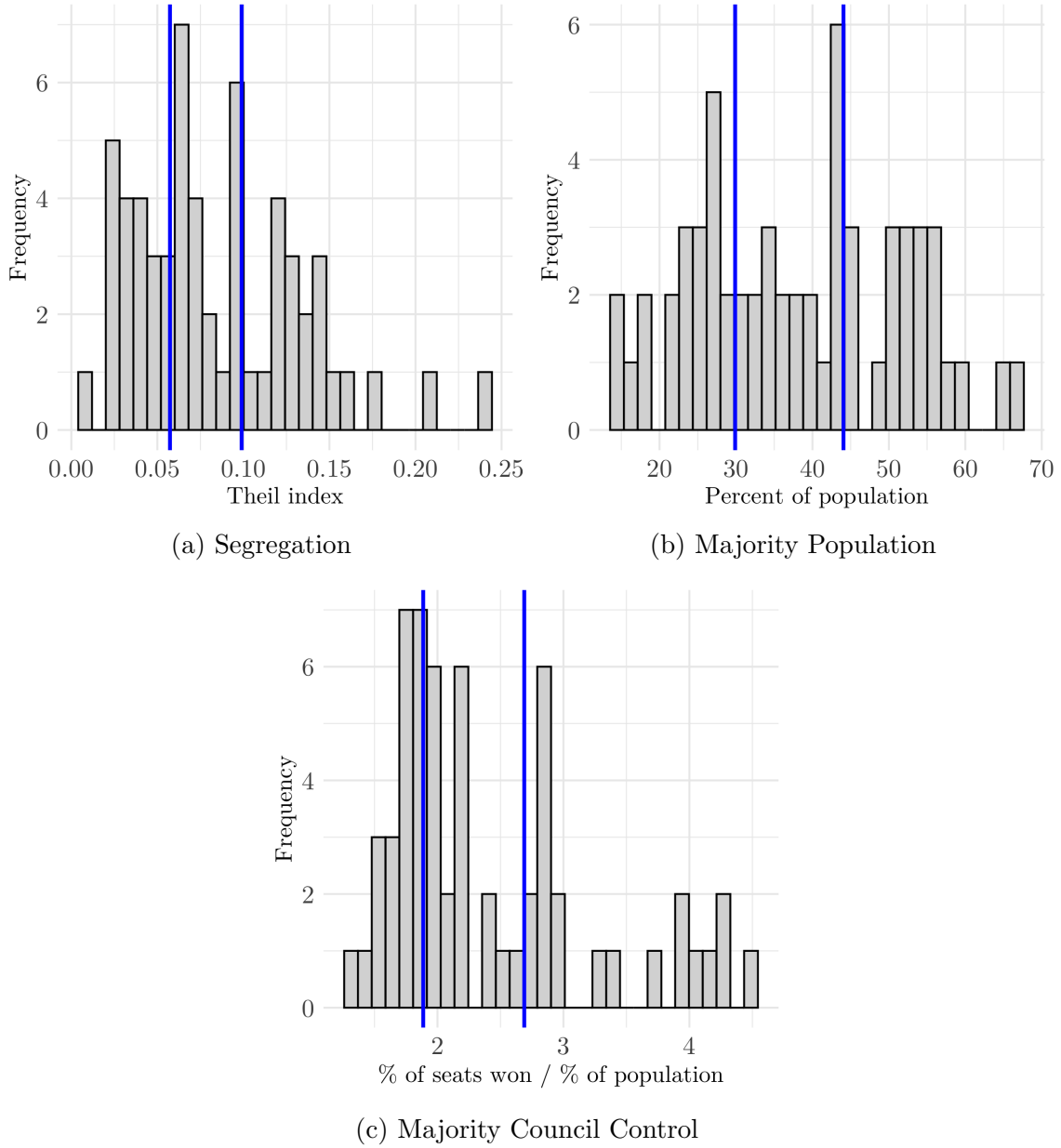
Notes: Tercile cutpoints are marked in blue. Distributions are defined over the pretreatment values of each variable for cities in the analysis subgroup. Assignment to terciles is determined at the city rather than observation level: our measure of segregation is time-invariant and observed pretreatment for all cities; for majority population size, we assign cities to terciles based on average values over their pretreatment panels; and for majority council control, we take each city's value from the year before their first district election, as this already incorporates a twelve-year pretreatment history.

Table A-2: Characteristics of Cities in Spatial Analysis by Type

	Mean (Treatment)	Mean (Control)	p-value of difference
Median income	63836	56294	0.00
Median home value	442599	530896	0.00
Home ownership rate	0.45	0.38	0.00
Home vacancy rate	0.07	0.07	0.78
Proportion Black	0.02	0.02	0.11
Proportion non-Hispanic white	0.49	0.69	0.00
Proportion Hispanic	0.35	0.14	0.00

B   Aggregate Outcomes

Figure B-3: Pretrends, Logged Multifamily Permits (Subgroup)
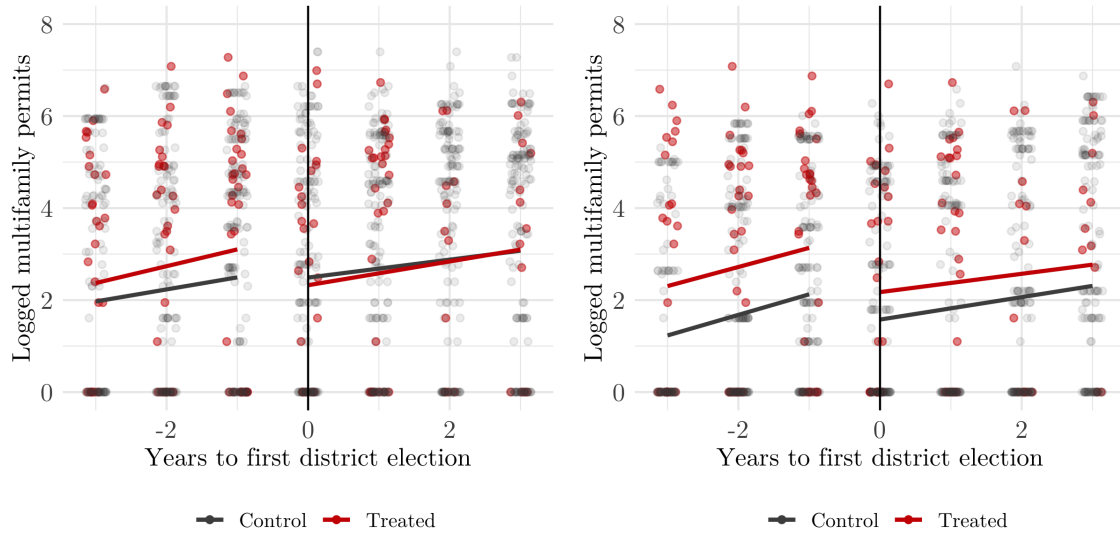
(a) Top & Bottom Terciles,
Segregation

(b) Top & Bottom Terciles,
Majority Population
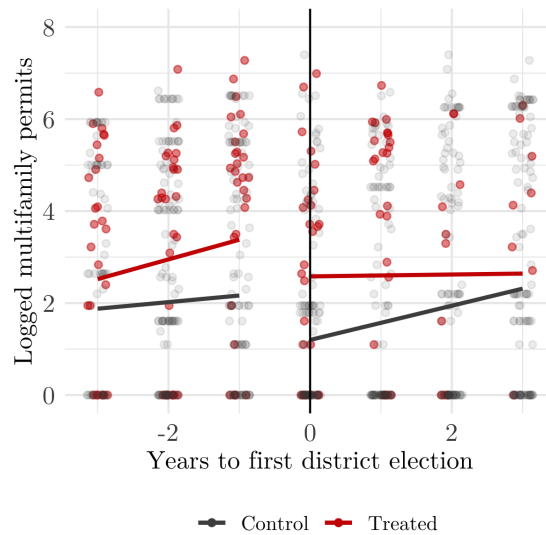
(c) Top & Bottom Terciles,
Majority Council Control

Table B-3: Effect of Conversion to Single-Member Districts on Housing Permits, By Housing Type (Subgroup)

	Total	Single-Family	Multifamily
	(1)	(2)	(3)
Single-member districts	−0.470	−0.227	−0.805
	(0.255)	(0.236)	(0.459)
Population (thousands)	−0.012	−0.025	−0.055
	(0.078)	(0.080)	(0.103)
Percent non-Hispanic white	0.016	−0.012	0.080
	(0.096)	(0.092)	(0.162)
Percent Black	−0.092	0.110	−0.379
	(0.132)	(0.144)	(0.299)
Percent Hispanic	0.023	0.025	0.051
	(0.080)	(0.086)	(0.171)
Vacancy rate	5.200	6.155	18.206
	(10.607)	(10.666)	(20.706)
Home ownership rate	18.395**	9.107	10.872
	(6.314)	(6.286)	(8.841)
Median home value (thousands)	0.004	0.007	−0.010
	(0.006)	(0.004)	(0.008)
Median income (thousands)	−0.014	−0.032	0.024
	(0.055)	(0.038)	(0.074)
Past minority representation	0.333	0.601	1.549
	(1.485)	(1.302)	(2.732)
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
City-specific Trends	Yes	Yes	Yes
Observations	597	597	597
R^2	0.679	0.751	0.573

Note: $^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Table B-4: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with City Characteristics (Subgroup)

	Total			Single-Family		
	(1)	(2)	(3)	(4)	(5)	(6)
Single-member districts	−0.349	−0.797	−0.713*	0.063	−0.412	−0.215
	(0.351)	(0.417)	(0.323)	(0.342)	(0.416)	(0.343)
SMD∗Low segregation	−0.408			−0.682		
	(0.395)			(0.458)		
SMD∗High majority population		0.169			0.043	
		(0.562)			(0.578)	
SMD∗Low majority control			0.911			0.750
			(0.490)			(0.503)
Population (thousands)	0.031	−0.006	0.0003	0.021	−0.065	0.008
	(0.080)	(0.092)	(0.084)	(0.077)	(0.097)	(0.088)
Vacancy rate	9.425	10.984	8.966	−0.456	6.748	11.167
	(11.199)	(13.171)	(14.097)	(10.679)	(13.617)	(14.718)
Home ownership rate	24.305**	16.338	17.932**	13.710	9.805	9.276
	(7.659)	(8.397)	(6.876)	(7.877)	(8.504)	(6.906)
Median home value (thousands)	0.009	0.009	0.002	0.012*	0.009	0.006
	(0.007)	(0.008)	(0.006)	(0.005)	(0.006)	(0.005)
Median income (thousands)	−0.053	−0.015	0.003	−0.061	−0.053	−0.022
	(0.064)	(0.062)	(0.057)	(0.056)	(0.045)	(0.043)
Past minority representation	−0.449	0.264	1.040	1.265	−0.422	2.492
	(1.824)	(1.925)	(1.769)	(1.640)	(1.853)	(1.568)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	399	397	397	399	397	397
R^2	0.688	0.673	0.700	0.784	0.739	0.747

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

Table B-5: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with Segregation (Subgroup), Robustness to Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
Single-member districts	0.105	−0.816	−1.183**	−1.036*	−1.226*
	(0.456)	(0.448)	(0.448)	(0.426)	(0.612)
SMD∗Low segregation	−0.119	0.302	0.406	0.541	0.388
	(0.713)	(0.481)	(0.481)	(0.572)	(0.872)
Population (thousands)				0.101	−0.119
				(0.064)	(0.129)
Vacancy rate				27.175	41.479
				(15.428)	(27.165)
Home ownership rate				14.567	15.738
				(9.525)	(11.774)
Median home value (thousands)				−0.007	−0.006
				(0.007)	(0.010)
Median income (thousands)				0.009	−0.002
				(0.078)	(0.093)
Past minority representation				−0.683	−1.096
				(2.371)	(3.086)
City FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
City-specific Trends	No	No	Yes	No	Yes
Observations	399	399	399	399	399
R^2	0.0003	0.450	0.549	0.471	0.563

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

Table B-6: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with Majority Population (Subgroup), Robustness to Alternative Model Specifications

	(1)	(2)	(3)	(4)	(5)
Single-member districts	−0.022	−0.951*	−1.488***	−1.178***	−1.355**
	(0.514)	(0.439)	(0.439)	(0.347)	(0.493)
SMD∗High majority population	0.379	0.855*	0.571	0.879*	0.159
	(0.716)	(0.427)	(0.427)	(0.442)	(0.585)
Population (thousands)				0.096	0.034
				(0.061)	(0.079)
Vacancy rate				25.343	39.184
				(13.674)	(21.331)
Home ownership rate				7.831	9.366
				(7.858)	(8.364)
Median home value (thousands)				−0.008	−0.005
				(0.007)	(0.011)
Median income (thousands)				−0.026	−0.012
				(0.070)	(0.082)
Past minority representation				−0.453	2.379
				(1.970)	(2.706)
City FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
City-specific Trends	No	No	Yes	No	Yes
Observations	397	397	397	397	397
R^2	0.003	0.490	0.587	0.507	0.595

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

Table B-7: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with Majority Control (Subgroup), Robustness to Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
Single-member districts	0.206	−0.761	−1.445**	−0.829	−1.405*
	(0.528)	(0.483)	(0.483)	(0.450)	(0.610)
SMD∗Low majority control	0.183	0.423	0.416	0.403	0.244
	(0.750)	(0.494)	(0.494)	(0.530)	(0.864)
Population (thousands)				0.092	−0.073
				(0.057)	(0.113)
Vacancy rate				30.534	21.877
				(16.124)	(26.500)
Home ownership rate				3.294	8.472
				(7.295)	(10.846)
Median home value (thousands)				−0.011	−0.013
				(0.006)	(0.009)
Median income (thousands)				0.038	0.025
				(0.060)	(0.084)
Past minority representation				−0.968	−0.115
				(1.721)	(2.491)
City FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
City-specific Trends	No	No	Yes	No	Yes
Observations	397	397	397	397	397
R^2	0.003	0.516	0.602	0.531	0.608

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

Figure B-4: Event Study Plot of Treatment Effects and Confidence Intervals, Top Segregation Tercile
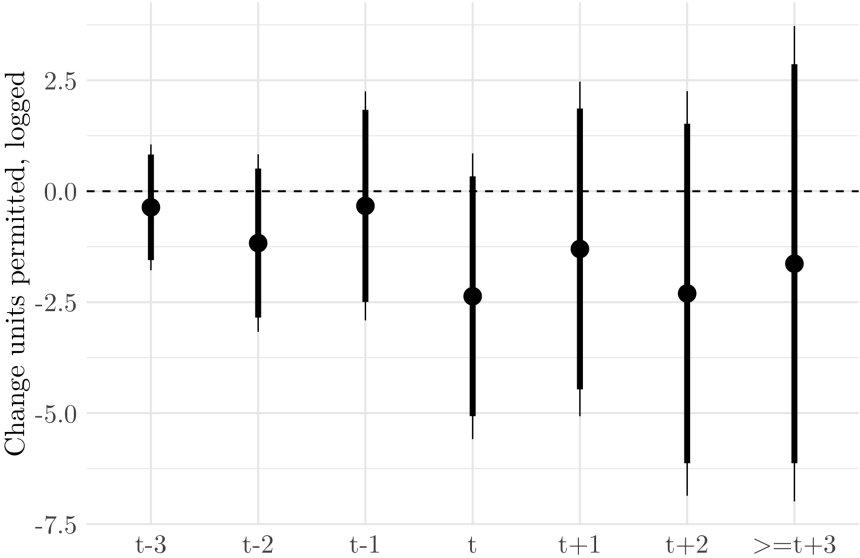

Figure B-5: Event Study Plot of Treatment Effects and Confidence Intervals, Bottom Majority Population Tercile
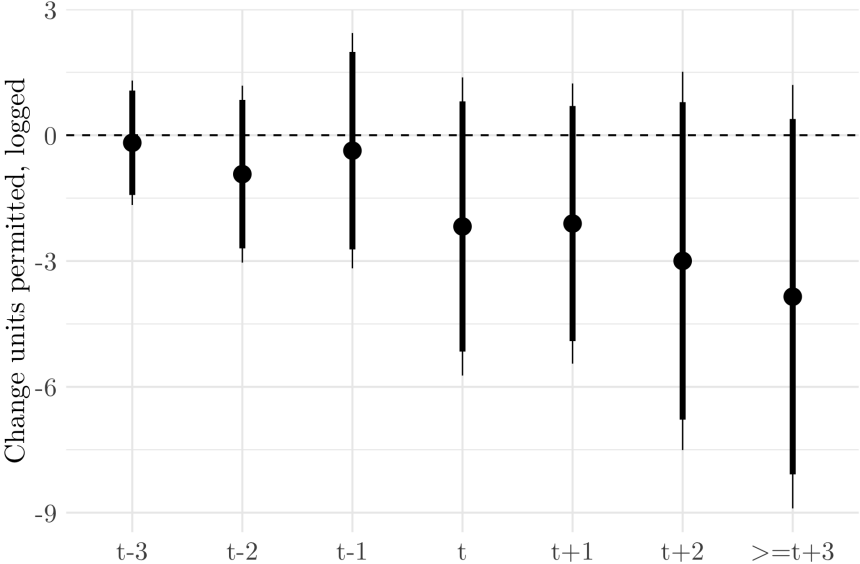
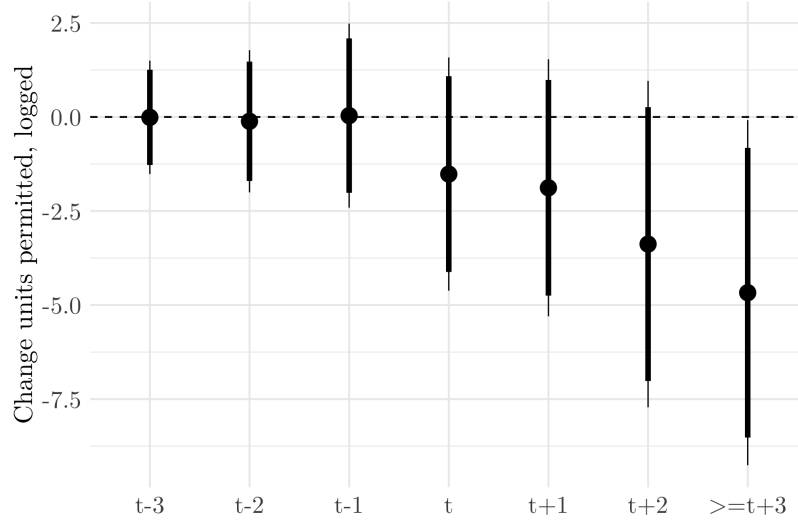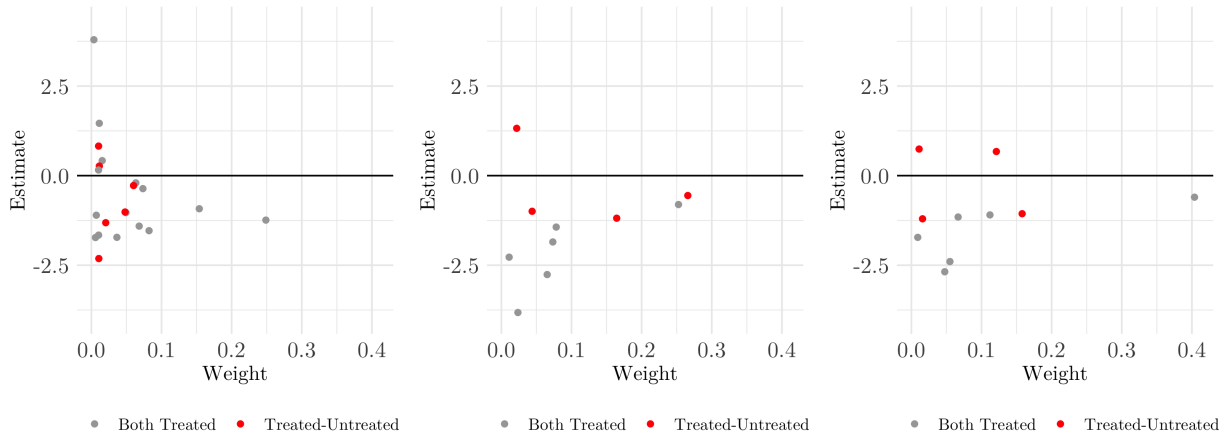Figure B-6: Event Study Plot of Treatment Effects and Confidence Intervals, Top Majority Control Tercile

Figure B-7: Goodman-Bacon Decomposition of the Effect of Single-Member Districts on Multifamily Permits (Subgroup)

(a) Subgroup, Top Segregation Tercile

(b) Subgroup, Bottom Majority Population Tercile

(c) Subgroup, Top Majority Control Tercile

Notes: Models in each panel are equivalent to a fully interacted version of Table 2, where the treatment effect on which we conduct the Goodman-Bacon decomposition corresponds to the effect reported under "Single-member districts." Each point represents one of the difference-in-differences comparisons that constitute the overall two-way fixed effects estimate, with the weight assigned to that estimate on the x-axis. "Untreated" units are eventual switchers that do not implement district elections over the course of this panel — that is, those that commit to holding their first district elections in 2020 or later.

C   Spatial Outcomes

Table C-8: Effect of Conversion to Single-Member Districts on Units Permitted, Logged

	(1)	(2)	(3)	(4)	(5)
Single-member districts	0.040	0.160	0.059	0.179	0.210
	$p = 0.749$	$p = 0.300$	$p = 0.761$	$p = 0.334$	$p = 0.126$
Minority block groups	0.387	0.387	0.387	0.312	0.311
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$
SMD*Minority block groups	-0.377	-0.377	-0.377	-0.425	-0.424
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$
Controls	No	No	No	Yes	Yes
City FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
City Trends	No	No	Yes	No	Yes
Observations	1,184	1,184	1,184	1,184	1,184

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

Table C-9: Effect of Conversion to Single-Member Districts on Units Permitted, Logged

|	Full	No Anaheim	No Escondido	No Glendale
	(1)	(2)	(3)	(4)
Single-member districts	0.210	0.115	0.065	0.222
	$p = 0.126$	$p = 0.132$	$p = 0.494$	$p = 0.205$
Minority block groups	0.311	0.326	0.318	0.352
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$
SMD∗Minority block groups	−0.424	−0.500	−0.403	−0.334
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes
Observations	1,184	832	1,040	1,008

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

Table C-10: Effect of Conversion to Single-Member Districts on Units Permitted, Logged

	Full	No Santa Barbara	No Santa Cruz	No Ventura
	(1)	(2)	(3)	(4)
Single-member districts	0.210	0.234	0.281	0.252
	$p = 0.126$	$p = 0.583$	$p = 0.126$	$p = 0.189$
Minority block groups	0.311	0.301	0.338	0.268
	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$	$p = 0.000^{***}$
SMD∗Minority block groups	−0.424	−0.426	−0.432	−0.431
	$p = 0.000^{***}$	$p = 0.249$	$p = 0.000^{***}$	$p = 0.000^{***}$
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes
Observations	1,184	928	1,072	1,040

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

Figure C-8: Raw Data, Total Units Permitted by Block Group/Year
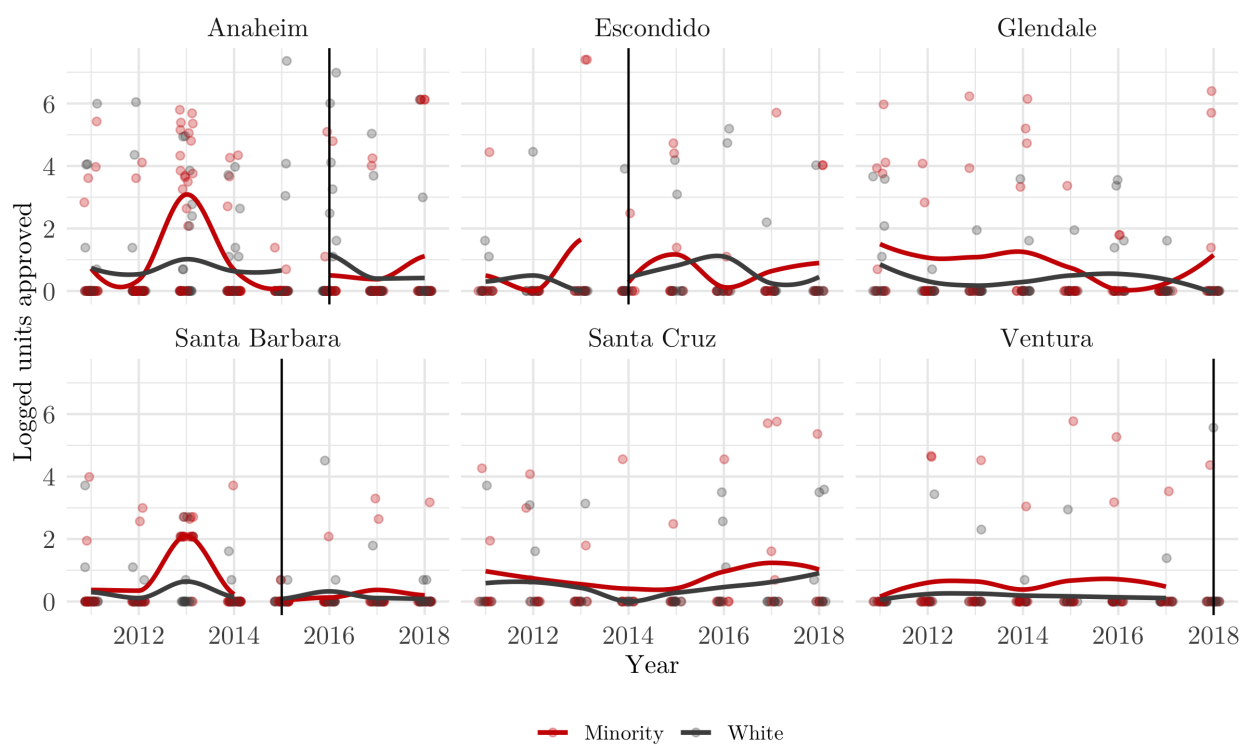
Notes: Vertical lines represent year of first district elections for treated cities. Terciles are defined according to the population distribution within each city.

Table C-11: Effect of Conversion to Single-Member Districts on Units Permitted, Logged Terciles Defined Over All Treated Cities (Minority block groups: less than 38 percent white, white block groups: more than 67 percent white)

	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
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
City Trends	Yes	Yes	Yes
Observations	1,136	1,136	1,136

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

Figure C-9: Event Study Plot of Spatial Diff-in-Diff Interaction
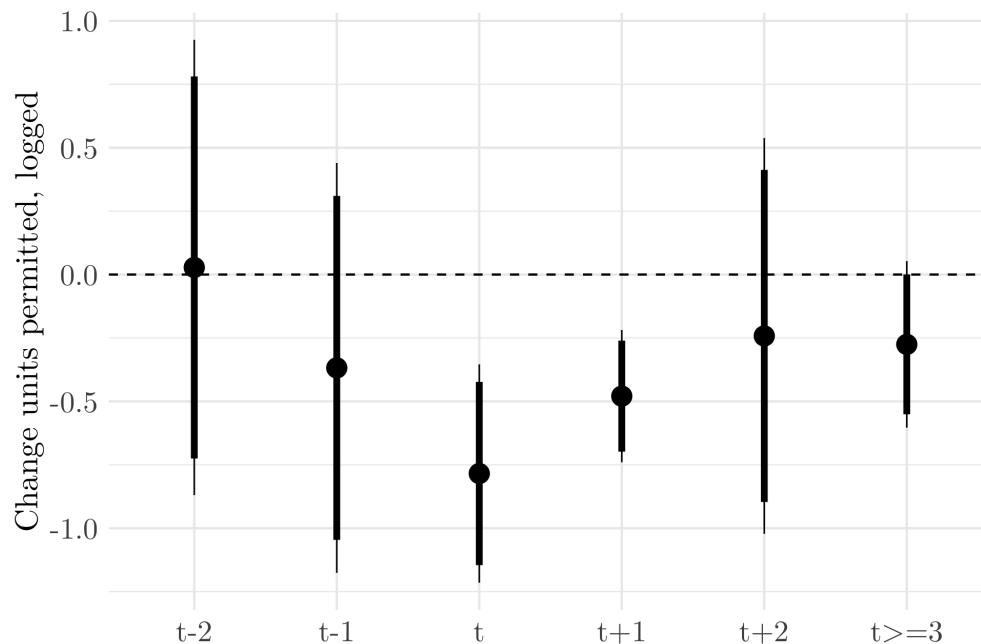
Notes: Baseline year is set to $t-3$ so that every treated city has at least one pretreatment year.

D Data Collection

D.1 Aggregate Permits

The Census Bureau's Building Permits Survey is the leading source of cross-municipality data on housing permits, surveying the over 20,000 local governments which permit 98% of US housing production. On average, 94% of units permitted are eventually completed, with the decrease in units stemming from design changes or permits abandoned (*Data Relationships between Permits, Starts, and Completions*, 2020). Our dependent variable is units permitted because permitting is a political decision, whereas building completions are affected by exogenous factors such as internal financing. Of note, the number of observations in our panel models falls below 600 and 400 because two of the cities in our subgroup were incorporated early in the panel. Eastvale was incorporated in 2010 and entered our panel in 2011. Jurupa Valley was incorporated in 2011 and entered our panel in 2012.

D.2 Electoral Institutions

We assembled an original panel dataset of city council structures from 2010 through the present for the 482 Census-designated places in California. We began by coding all of these cities as at-large, except for the 59 cities identified by California Common Cause to be by-district as of 2016 (`https://www.commoncause.org/california/wp-content/uploads/sites/29/2018/03/california-municipal.pdf`). For each of these cities, we used internet searches to learn the year of their first district election. To find all subsequent conversions to districts under the CVRA, we used a combination of internet searches, city council websites, local media reports, and interviews (see section D.3 below). For each city that converted, we collected the following information:

- Year of decision to convert

- Year of first district election

- Reason for conversion (lawsuit, threat letter)

- Method of conversion (court order, council resolution, or ballot initiative)

- Plaintiff/source of threat letter

D.3 Interviews with Key CVRA Stakeholders

We conducted a site visit to Southern California in January 2020 to talk to key stakeholders in CVRA litigation, local government, and housing politics. Their names, locations, and titles are given in Table D-12.

Table D-12: Stakeholders Interviewed During Site Visit to Southern California, January 2020

Name	City	Position
City Council		
Jose Moreno	Anaheim	City council member
Denise Barnes	Anaheim	City council member
Danny Fierro	Anaheim	Policy aide to city council member Jordan Brandman
Grant Henninger	Anaheim	Candidate for city council
Paul McNamara	Escondido	City council member and current mayor
Consuelo Martinez	Escondido	City council member
Olga Diaz	Escondido	City council member
Ardy Kassakhian	Glendale	City council member
Ara Najarian	Glendale	City council member and current mayor
Mike van Gorder	Glendale	Candidate for city council
Maegan Harmon	Santa Barbara	City council member
Oscar Gutierrez	Santa Barbara	City council member
Kristen Sneddon	Santa Barbara	City council member
Eric Friedman	Santa Barbara	City council member
Jeanette Sanchez-Palacio	Ventura	Candidate for city council
Planning Commissioners and Urban Planners		
Steve White	Anaheim	Planning Commission member
John Armstrong	Anaheim	Planning Commission member
Mike Strong	Escondido	Planning Commission member
Jeffrey Lambert	Ventura	Planning Commission member
Alex McIntyre	Ventura	City Manager
Sandy Smith	Ventura	Former Mayor and Land Use Consultant, Sespe Consulting
John Hecht	Ventura	Land Use Consultant, Sespe Consulting
Shine Ling	Los Angeles*	Urban Planner
Plaintiffs and Lawyers Involved in CVRA Litigation		
Thomas Saenz	Los Angeles	President and General Counsel, MALDEF
Lydia Camarillo	San Antonio, TX*	President, SVREP
Kevin Shenkman	Malibu*	Attorney for several CVRA plaintiffs & threat letters
Sebastian Aldana, Jr.	Santa Barbara	Plaintiff, CVRA lawsuit against City of Santa Barbara
Frank Banales	Santa Barbara	Plaintiff, CVRA lawsuit against City of Santa Barbara
Barry Capello	Santa Barbara	Attorney for plaintiffs, CVRA lawsuit against City of Santa Barbara

Conversation conducted by phone.

Name	City	Position

Community Organizers, Activists, and Interest Groups

Name	City	Position
Ada Briceño	Anaheim	Labor leader/Chair, Democratic Party of Orange County
Catherine Jurca	Glendale	Member, Glendale Historical Society Board of Directors
Lee Moldaver	Santa Barbara	Board Member, Citizens Planning Association of Santa Barbara County
Vijaya Jammalamadaka	Santa Barbara	President, League of Women Voters of Santa Barbara
Pedro Paz	Santa Barbara	Board Member, The Fund for Santa Barbara
Anna Marie Gott	Santa Barbara	Local Activist
Lucas Zucker	Ventura	Policy and Communications Director, CAUSE

Writers and Journalists

Name	City	Position
Spencer Custodio	Anaheim	Reporter, Voice of OC
Bill Fulton	Ventura	Urban planner and former mayor of Ventura, CA

Excerpts from Conversation with Thomas Saenz, President and General Counsel of MALDEF (January 13, 2010)

What informed your selection of cities in which to pursue legal action under the CVRA?
"There's no hard and fast rule, but we had to use some general criteria that include size of the jurisdiction and our ability to draw a majority Latino district. We have generally not challenged anyone under 25,000 in population, and our goal has been to focus on those that are over 50,000 in population. I think there are circumstances that apply in smaller jurisdictions that don't necessarily apply in larger jurisdictions. In small jurisdictions — and this is my personal view — there is a greater justification for an at-large system. If a city's so small that you don't see the distinction between neighborhoods that you see in larger jurisdictions, where the wealthier neighborhood ends up, wholly apart from race, having all the city council or governing body coming from one neighborhood — that's a little bit less likely to occur when it's a much smaller jurisdiction. We have also insisted on the ability to draw a Latino majority CVAP (Citizen Voting Age Population) district — a compact district, we're not going to pursue something where you can only draw a Latino district with spindles in different directions...We also look at electoral history. If there have been Latinos consistently elected, we won't even do an RPV (racially polarized voting) analysis and we will forego that jurisdiction for the moment."

Why did it take a couple years since the passage of the CVRA to see litigation take off?
"I can only speak for MALDEF: things were going on that kept us very busy in the early

years. Then I left, and litigation was more or less consciously downplayed by the leadership at the time, first for philosophical reasons, and ultimately for a mix of philosophical and financial reasons. I came back in 2009 and it took a little time to get a system up and running, but now we have a very good, comprehensive system to identify jurisdictions and move forward in systematically challenging at-large systems at the local level."

D.4   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 the 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.