How Electoral Institutions Shape the Efficiency and Equity of Distributive Policy

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Abstract

How does the aggregation of voters affect the trade-off between the efficient production of collective goods and the equitable distribution of costs? We find that district elections amplify the local interests of previously underrepresented groups, but also threaten the collective provision of goods with concentrated costs and diffuse benefits. To do so, we leverage the California Voting Rights Act of 2001 as a conditionally exogenous institutional reform, compelling over one hundred cities in California to switch from multi-member ('at-large') to single-member ('district') elections for city council. Using panel data, we find that district representation causes a substantial decrease in the permitting of multifamily housing, the type of housing residents are most likely to oppose in their neighborhood. However, the reform also causes the housing that is permitted to be more affordable and future development to be more equitably spread throughout the city.

Keywords: institutions, representation, panel data, local political economy

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Two normative criteria generally guide collective decisions. One is efficiency in producing collective goods to enhance aggregate welfare. The other is equity in the distribution of the associated costs. These criteria are not only at the core of philosophical theories of justice, but are often framed as being in direct conflict (Hochschild, 1981; Le Grand, 1990; Okun, 1975; Weingast, Shepsle, and Johnsen, 1981). Whether our public policies favor efficiency or equity is a function of politics and the decisionmaking process. In this paper, we ask how our electoral institutions affect collective policy decisions along these key normative dimensions. Specifically, we measure how different aggregations of the same voting population directly change the balance between the efficient production of collective goods and the equitable distribution of costs.

In a representative democracy, elections aggregate voter preferences to produce elected officials. Voters can be aggregated into one large, multi-member district, with each citizen voting for several candidates ('at-large elections'). Or, voters can be disaggregated into several smaller, single-member districts, with each citizen voting for only one candidate to represent their district ('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 beholden only to the voters in their district.

In the United States, how we aggregate voters activates the efficiency-equity trade-off, especially at the local level. Local governments generally lack partisan competition (Schaffner, Streb, and Wright, 2001; Schleicher, 2007) and the attendant organization that comes with competitive parties. Thus, local legislators struggle to resolve intertemporal bargaining breakdown and prisoner's dilemma-style problems (Cox and McCubbins, 2005, 2007; Kiewiet and McCubbins, 1991; Key Jr, 1949; Weingast, 1979). Instead, district-elected legislatures have developed universal logrolls, whereby legislators exercise outsized control over issues specific to their own districts (Burnett and Kogan, 2014; Trounstine, 2009).

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

These local logrolls magnify the efficiency-equity dilemma in the siting of land uses that society needs but few citizens want nearby. These locally unwanted land uses ('LULUs') range from new housing (Hankinson, 2018), to energy facilities (Stokes, 2016), to drug addiction treatment clinics (de Benedictis-Kessner and Hankinson, Forthcoming). Because LULUs are perceived to threaten the property values, safety, or general quality of life of nearby residents, legislators representing the proposed host district have a strong electoral incentive to block their siting. Logrolls enable them to do so. With each council member able to block the siting, LULUs may become harder to build within district-elected cities (Schleicher, 2013). But LULUs are often needed within a city to achieve collective goals, such as an affordable housing supply or a robust energy infrastructure. Consequently, logrolls may simultaneously enhance distributive equity via greater neighborhood control while also hampering the aggregate production of collective goods.

Of all possible LULUs, the permitting of new housing is perhaps ideal for measuring the effect of voter aggregation on the efficiency-equity trade-off. First, new housing is often opposed most 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). Second, 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.

Finally, understanding the siting challenges of new housing has a strong normative motivation. Since 1970, real housing prices in the United States have dramatically increased, with growth led by the top quintile of high demand cities (Glaeser, Gyourko, and Saks, 2005). This rise in prices stems from an inability of new supply to meet demand due to political restrictions that limit the quantity of new homes and apartments (Ihlanfeldt, 2007; Jackson, 2016; Saks, 2008). While these restrictions are local, their consequences are global. Limits on new housing not only raise the rents of existing residents, but also price 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; Lens and Monkkonen, 2016) and entrench existing patterns of racial segregation (Rothwell and Massey, 2009; Sahn, 2019; Trounstine, 2018). More broadly, these regulations limit migration and residential density, slowing both regional and national economic growth (Glaeser and Gyourko, 2018; Hsieh and Moretti, 2019). Instead of fostering dense urban communities, the restrictions spur low density, carbon-intensive suburban sprawl, contributing to global climate change (Estiri, 2016; Ewing and Rong, 2008; Jones and Kammen, 2014).[2]

We show that voter aggregation affects the housing supply in two ways, illustrating the efficiency-equity trade-off. First, cities with district elections permit less new housing annually compared to cities with at-large elections. At-large representation does not guarantee each neighborhood representation in city council, meaning some neighborhoods are likely left out of the logroll. As a result, at-large city councils are more able to channel unwanted housing into these unrepresented, politically weak neighborhoods. In contrast, district elections by definition distribute representation evenly. With ostensibly no unrepresented, weak neighborhoods, district-elected city councils face political pushback to housing throughout the city, causing fewer new units to be permitted citywide. This decrease in new housing in district-elected cities represents a decline in the efficient production of a collective outcome, an affordable housing supply.

Second, cities with district elections permit housing *more equitably* across neighborhoods compared to their at-large peers. At-large representation has generally been found to suppress the descriptive representation of racial minorities (Engstrom and McDonald, 1981; Leal, Martinez-Ebers, and Meier, 2004; Marschall, Ruhil, and Shah, 2010; Meier et al., 2005; Molina Jr and Meier, 2018), albeit more so under specific conditions (Abott and Magazinnik, Forthcoming; Trounstine and Valdini, 2008; Welch, 1990). Because at-large systems are more likely to underrepresent minorities, unwanted housing is more likely to be concentrated

[2]See Been, Ellen, and O'Regan (2019) for a comprehensive review of the evidence on how adding new homes moderates price increases, making housing more affordable to low- and moderate-income families.

in minority neighborhoods. Therefore, when district elections empower local 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 city councils will more evenly distribute new housing across neighborhoods—and consequently demographic groups.

Together, these two processes capture the importance of voter aggregation on the efficiency-equity trade-off and democratic outcomes writ large. District representation may lead to a more equitable distribution of housing, but at the cost of building less housing overall. Because of limited new supply, local housing costs will likely rise even higher, disproportionately harming the well-being of low-income and minority communities. In other words, each district's local interest in blocking unwanted housing conflicts with a collective outcome of protecting affordability by permitting enough new supply to meet demand.

To causally identify the effect of voter aggregation on the housing supply, we leverage the California Voting Rights Act of 2001 (CVRA) as a conditionally exogenous treatment, spurring city councils to switch from at-large to district elections. First, we use city-level panel data to measure the effect of this conversion on the amount, structural composition, and affordability 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 within each city.

We find that the switch to district elections causes a 46 percent decrease in the permitting of multifamily housing. However, housing that is permitted is more likely to be subsidized and affordable to low-income residents. Additionally, district elections decrease the spatial concentration of new housing by breaking the correlation between a neighborhood's racial composition and its buildable capacity. Not only is new housing more equitably distributed across the city, but it is less likely to be concentrated in minority neighborhoods. Both of these policy outcomes reflect the immediate goals of groups believed to be empowered by the CVRA-spurred switch to district elections. We close by discussing the policy implications of these findings—how to balance descriptive representation and distributive equity with efficient collective outcomes both in responding to the housing affordability crisis and other policies with concentrated costs and diffuse benefits.

Theoretical Expectations

Inherent in the politics of efficiency and equity are the incentives of elected officials. In pursuit of reelection, representatives strive to meet the needs of their constituencies (Mayhew, 1974). Even if legislating on the same policy questions for the same population, an elected official is 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 at-large and district systems (Clark and Krebs, 2012).[3]

This city-level variation stems largely from the early 20th century, when municipal reformers sought to counter the influence of machine-style politics via at-large electoral systems (Davidson and Korbel, 1981; 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 (Banfield and Wilson, 1963). 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 of its minority citizens (Trebbi, Aghion, and Alesina, 2008). To Progressive Era reformers, minority groups were largely Catholic and

[3]We use the term "city" throughout the paper to stand in for any incorporated municipality. Regarding our data analysis, while California municipalities may self-describe as cities or towns, there is no legal distinction.

southern European immigrants (Bridges, 1999). To whites of the post-Reconstruction South, minority groups meant African-Americans (Kousser, 2000). Above all, the at-large aggregation of voters was a conscious decision in pursuit of exclusionary representation and policy outcomes.

The 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). Beyond representation, the post-VRA shift to district elections also affected policy outcomes, albeit with potentially unintended consequences. Southern cities that switched to district elections in the wake of the VRA were found to generate higher pension benefits while simultaneously lowering funding for those benefits and decreasing investment in infrastructure (Boylan and Stevenson, 2017). In other words, district-elected leaders were more likely to 'time-shift' expenditures, delaying unpopular policy costs to future voters.

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, that study does not investigate outcomes or preferences linked to land use or distributional policies, an omission which motivates 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

[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).

found to lack a strong ideological dimension (Marble and Nall, 2018). 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 the 'time-shifting' of expenditures, we believe that district-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).[5]

With each neighborhood able to block new development, district-elected cities will struggle to permit new housing compared to their at-large peers. Cross-sectional studies of local institutions support this theory, finding district elections to be associated with increased use of growth management regulation (Feiock, Tavares, and Lubell, 2008), greater restrictions on the siting of group homes (Clingermayer, 1994), decreased permitting of single family homes (Lubell, Feiock, and De La Cruz, 2009), and a weakened influence of the construction industry on permit approval times (Deslatte, Tavares, and Feiock, 2018). We build upon this theory by first measuring the causal effect of district elections on the volume and composition of the local housing supply. Second, we look within cities to observe the mechanism behind district-elected cities' decrease in new housing—the ability of previously underrepresented minority neighborhoods to block new LULUs.

Of note, district representation may affect the efficiency-equity trade-off through two

[5]Bridges (1999) describes a meeting between a developer and neighbors of a proposed development who wanted a smaller project. The meeting was mediated by their city council representative, Vincent Griego. "The developer, adamantly opposed to downsizing, threatened to go to the city council. At this point in the meeting, Griego stood up and explained that in this instance *he* was the council, and the developer would have to negotiate" (203).

pathways: council member replacement and accountability. First, district elections may increase the descriptive representation of minorities, changing the ideological composition of city council. Focused on inequality, new council members will protect minority neighborhoods from an unequal distribution of housing, causing a decline in new housing production overall. Second, district elections affect the incentives of existing council members. Even without replacement, LULU politics incentivize legislators to block concentrated costs from their districts, ducking any backlash from a citywide electorate unable to hold them accountable. While we believe both mechanisms operate in constraining the housing supply, our data are unable to disentangle the two. Either way, district elections would achieve more equitable outcomes for minority neighborhoods.

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. Even studies leveraging the reform of the Voting Rights Act of 1965 suffer from endogenous enforcement, relying on assumptions of unobservable covariates (e.g., Boylan and Stevenson, 2017).

We advance our understanding of the causal effect of voter aggregation by leveraging the California Voting Rights Act of 2001 (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, California city governments are now responsible for all legal and court fees, even in the case of an out of court settlement (Ingram, 2012). These changes have spurred a wave of enforcement litigation against cities with at-large elections.

Thus, the CVRA presents an opportunity for causal identification. In principle, the CVRA has enabled law firms to threaten litigation against the vast majority of at-large local governments in California. In practice, firms have only pursued legal action in a subset of these eligible jurisdictions due to limited resources, with litigation threats rolling out gradually over time. Importantly, of the more than 100 municipalities targeted, none of them have successfully resisted the switch from at-large to district elections.

We leverage this slow rollout over time to measure the effect of district elections on housing. Specifically, we use a generalized difference-in-differences framework, including city and year fixed effects as detailed below. This baseline two-way fixed effects estimate rules out confounding from any time-invariant factors, but also assumes that changes in electoral system are unrelated to any other changes that might also influence housing permitting. Still, other drivers of permitting may also change when a city switches to district elections. For example, the switch to district elections may correlate with changes in voter preferences for housing. In this case, district elections are not amplifying local NIMBY ('Not In My Backyard') interests, but are capturing newly emergent preferences, just as at-large elections would. We believe this to be unlikely. Through conversations with city council members and lawyers litigating CVRA cases, as well as reviewing several hundred local media articles and coding nearly a decade of legislative meeting minutes over six cities, we have not encountered any discussion of housing politics driving CVRA litigation.

Figure G-9 supports this theory, comparing pre-treatment housing trends between those that remained at-large and those that converted to district elections (once a city converted, we removed it from the sample that produced this figure). While cities that switched to districts permitted more housing than their at-large peers overall, they did not exhibit a statistically significant difference in their rate of permitting growth before they experienced electoral reform.

Furthermore, while an annual, city-level measure of NIMBY preferences does not exist, we proxy for changing preferences using time-varying measures of demographic variables, including population, median income, the percentage of white, black, and Latino residents, as well as the percentage of residents who are homeowners. Finally, we account for changes in the local housing market which may affect permitting activity by controlling for the city's residential vacancy rate and the median home value. We include these time-varying controls in all of our models.

We use several additional robustness checks to account for endogeneity. First, we repeat each city-level analysis using a subset of cities which either switch to district elections during our panel ('treated units') or have agreed to switch in an upcoming election ('to be treated'). As targets of CVRA litigation, these 'eventually treated' cities are likely to be more similar to each other than cities which neither have been nor may ever be targets of the CVRA. While this subset limits external validity to similar cities, it helps to control for unobservable confounders. The results of these models are substantively identical to those conducted on the full sample of California cities and are presented in Appendix I.

Additionally, 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 (Imai and Kim, 2019; Abraham and Sun, 2019; Goodman-Bacon, 2018; Słoczyński, 2018; de Chaisemartin and D'Houltfœuille, 2018; Athey and Imbens, 2018; Callaway and Sant'Anna, 2018; Borusyak and Jaravel, 2017; Chernozhukov et al., 2013; Wooldridge, 2005; Bertrand, Duflo, and Mullainathan, 2004). This rich literature has produced a large number of alternative approaches with advantages over the standard two-way fixed effects model. In our Results section, we detail two of these approaches to verify our main results.

In short, the distribution of legal threats across cities and over years generates a panel of conversions from at-large to district elections that is conditionally exogenous to our outcome of housing permitting. To our knowledge, this reform provides the most compelling opportunity to measure the causal effect of voter aggregation on policy outcomes, revealing the shift in political power from collective to local interests—from efficiency to equity. We measure this shift through changes in the permitting of new housing units via the process of discretionary review.

The Political Economy of Zoning

In the United States, proposals for new development travel through one of two paths: 'by right' and discretionary review (Schleicher, 2013). By right proposals are those that are allowed under the existing regulations, known as the zoning code. 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 necessary building standards and codes. As a result, the 6-unit project is 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.[6] Consequently, discretionary review opens the permitting process to political demands, with voters directly pressuring members of city council via the electoral connection. In California, members of the planning commission are also vulnerable. Not only are they appointed by city council, but their zoning decisions may

[6] The same discretionary review process applies to zoning variances and conditional use permits, which, while legally distinct, are effectively similar requests for exemption from the current zoning code.

be appealed to city council, effectively keeping their verdicts in line with council preferences. In other words, both legislative bodies are subject to the electoral connection and therefore the political effects of the CVRA.

Like any regulatory regime, the discretionary review of housing proposals generates its own political economy (Denzau and Weingast, 1982). But unlike the distributive boon of pork barrel politics, new housing is often 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 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; Mummolo and Nall, 2017). These threats to property values lead risk averse homeowners to oppose new housing in favor of the status quo (Fischel, 2001).[7] In contrast, renters generally prefer more housing. However, in cities with high housing prices, renters too may be risk averse, associating new housing with rising prices. Consequently, experimental evidence has found renters in high rent areas to support housing citywide, but also resist new market-rate housing when it is proposed for their neighborhood (Hankinson, 2018). Together, NIMBY homeowners and renters often present a united front, even if their respective goals of higher and lower housing prices are in conflict.

Magnifying the influence of NIMBY opposition are differentials in who participates in local government (Einstein, Palmer, and Glick, 2019; Einstein, Glick, and Palmer, 2020). The concentrated costs of housing felt by existing neighbors are more likely to mobilize a small Olsonian interest group of opponents rather than bringing out supporters of housing's diffuse benefits (Hills Jr and Schleicher, 2011; Olson, 1965). Thus, local meetings to discuss new housing disproportionately attract and amplify the voices of opponents. Not only are meeting attendees more likely to be opponents to new housing, but they are also more likely to be voters. Coupled with the incredibly low turnout of local elections, these

[7] Even in majority renter cities, neighborhoods with higher homeownership rates are more likely to be 'downzoned,' decreasing by right buildable capacity (Been, Madar, and McDonnell, 2014).

intense, mobilized preferences are believed to be weighed more highly by local officials in their permitting decisions (Einstein, Palmer, and Glick, 2019). These biases mean that even were a new development favored by the majority of residents, those voices of support are far less likely to be heard through the conduits of local democracy. In short, NIMBY opposition combines intense risk averse attitudes with disproportionate political participation to block local supply, increasing prices within both municipalities and entire metropolitan areas.

Still, housing preferences are not uniform, but vary based on the unit's structure and affordability. Housing may range from low density, detached single family homes to high density apartment buildings. New single family homes are seen as the most tolerable form of housing (Marble and Nall, 2018). 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 white and contribute more in tax revenue than they use in public services, mitigating some of the above concerns. Labeled "cumulative zoning," this preference for single family homes has existed since the inception of zoning in early 20th century (Hills Jr and Schleicher, 2010). As a formal hierarchy, single family housing is often permitted by right anywhere that is residentially zoned, whereas multifamily housing is restricted to specific areas or requires discretionary review.

Preferences also vary by affordability. The Department of Housing and Urban Development classifies a unit as "affordable" if its annual costs amount to no more than 30 percent of a household's annual income (Charette et al., 2015). Units affordable to those making above 120% of the Area Median Income (AMI) are considered 'above moderate-income' housing, whereas units affordable to a household making less than 80% AMI are 'low-income' housing. Generally speaking, residents concerned with maintaining property values will prefer above moderate-income housing, while residents concerned about rising prices and gentrification will prefer housing affordable to low-income residents (Hankinson, 2018).

The amount, structure, and affordability of new housing are all consequences of the political economy of zoning. The electoral connection gives control of these policy outcomes to voters through the discretionary review process. Spurred by voter pressure, legislative bodies may stifle new housing in well-represented neighborhoods, while permitting it in politically weak ones. Others only allow new housing if it includes units affordable to their constituents, be they wealthy homeowners or low-income renters.[8] If district elections enhance the political influence of minority neighborhoods and local interests, we should see those shifts reflected in the amount, type, and location of new housing permitted through discretionary review.

Hypotheses

We expect that cities that switch to district elections under the CVRA will experience changes in the amount, structure, affordability, 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. Additionally, we expect district aggregation to increase the share of new housing that is affordable to low-income residents. 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. First, as stated, single family homes are viewed as more benign, meaning there is little neighborhood opposition to be amplified by district constituencies. Second, 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, were there are few neighbors to provoke. Finally,

[8] A study of Atlanta, GA spotlights the importance of descriptive representation, with new housing disproportionately channeled into black neighborhoods until the election of black city council members (Hinds and Ordway, 1986). See Sances and You (2017) for recent advances on the connection between descriptive representation and distributive burdens.

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, 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 (Abott and Magazinnik, Forthcoming; Marschall, Ruhil, and Shah, 2010; Trounstine and Valdini, 2008; Vedlitz and Johnson, 1982). More segregated cities foster majority-minority districts. Once formed, these districts can more easily elect a minority candidate, changing the descriptive representation of city council. Segregated cities may also be more likely to create the initial conditions for an unequal distribution of housing. If white voters are evenly distributed, there will likely be fewer politically weak neighborhoods in which to concentrate housing. If no neighborhood serves as a 'dumping ground,' district elections will not have an imbalance to correct. Though we are unable to discern which of the two pathways is more responsible, 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 racial diversity of the city. To dilute minority influence, at-large elections require a voting population which is over 50% white. Below this threshold, the white population may struggle to control city council. Higher turnout among white residents means that a white population below 50% could still comprise over 50% of voters. The abysmally low turnout rates in local elections heighten this imbalance between white residents' share of the electorate and share of the overall city population (Hajnal and Trounstine, 2005). Therefore, we expect district elections to have the greatest effect on permitting in cities where non-Hispanic whites compose a larger share of the population. In contrast, cities with fewer whites are less likely to experience majority bloc voting under at-large elections, meaning the switch to districts will have less of an effect on their policy outcomes.

Third, we expect the effect of district elections to vary across levels of housing affordability. Given at-large elections dilute the power of minority voters, newly empowered neighborhoods are more likely to be composed of lower-income residents. Threatened by rising prices, these voters are likely to demand less market-rate housing commonly associated with gentrification and more housing affordable to low-income residents. Consequently, we predict that district elections will produce a distributional change in new supply, with a larger proportion of the housing that is permitted being affordable to low-income residents.

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. In turn, council members will find it harder to channel unwanted housing into underrepresented communities. Our hypotheses regarding spatial allocation are twofold. First, upon conversion to district elections, new housing approved through discretionary review will be more evenly distributed throughout the city. Second, 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 with the advent of district elections. In other words, race will become less predictive of a neighborhood's housing burden, all else equal.

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 166 cities that switched to district elections, the year of its first district election.

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 protector. We investigate the imprecision of treatment timing using a Granger causality test (e.g., Autor, 2003), adding indicator variables for each year pre- and post-treatment. Figure 2, discussed below, shows a treatment effect concentrated in the year of the first district election. Consequently, we use the year of first district election as the date of treatment throughout this study.

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.[9] These data include the number of total units permitted as well as the distribution of new units between single family and multifamily housing.[10] 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., Glaeser and Ward, 2009; Kahn, 2011). Specifically, we include city and year fixed effects as well as city-specific linear time trends (e.g., Dynarski, Jacob, and Kreisman, 2018; Wolfers, 2006), with Huber-White standard errors 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

[9] 13 of the 482 municipalities in California did not report annual housing permit data to the U.S. Census.

[10] The Census definition of single-family housing includes both detached and attached homes so long as the attached units are separated by a ground-to-roof wall and do not share heating/air-conditioning systems or utilities. Because rowhouses and townhouses also evoke NIMBY opposition (Whittemore and BenDor, 2019), our analysis may understate the differential effects by housing type.

income, homeownership rate, home vacancy rates, and median home value, with estimates interpolated for 2017 and 2018 observations.[11]

To test for heterogeneous effects across cities, we use the same model but compare cities 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. We measure citywide segregation using the Theil's H index (Thiel, 1972) as calculated in Trounstine (2016).[12]

Next, we provide direct evidence of how district elections affect the affordability of new housing. Each year, California cities report of the number of units they permit within each band of affordability: very low-income (0-50% AMI), low-income (50-80% AMI), moderate-income (80-120% AMI), and above-moderate income (above 120% AMI). To measure the effect of district elections on affordability, we repeat our first difference-in-differences model using the same specifications, but with the proportion of housing units permitted that are affordable to low and very-low income residents as our dependent variable. Furthermore, we capture the composition of this shift in affordability by repeating the difference-in-differences model for each income band of affordability, using log units permitted as our dependent variable.

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 com-

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

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

mission 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.[13] 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.[14] 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 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.[15] 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 housing supply.

To measure the spatial concentration of new housing, we calculated the Moran's I of housing units permitted each year. Moran's I is a measure of spatial clustering which compares the spatial distribution of a variable to an as-if random distribution (Moran, 1948).[16] Positive values show that the distribution is more concentrated than would otherwise be expected by randomness. While we have little doubt that new housing capacity will be

[13] Coding decisions are discussed in more detail in Appendix C.

[14] 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 revoked in future meetings.

[15] Table B shows demographic data of these matched pairs.

[16] Equation for Moran's I presented in Appendix D.

spatially concentrated, we expect this concentration to decrease after a city switches to district elections. To measure this change, we calculate the Moran's I using a queen contiguity matrix within each city for each year from 2011 to 2018. We then average the Moran's I within each treated city during the pre- and post-treatment periods, and present the difference. We repeat the process for our treated cities' matched pairs, defining pseudo-pre- and post-treatment periods to test for similar decreases in the concentration of new housing.

Finally, we examine the distributive equity of the housing supply by estimating the correlation between a neighborhood's racial composition and its increase 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 minority and non-minority block groups using cutpoints from the top and bottom tercile of percent non-Hispanic white in treated cities. 'White' block groups are more than 64% white, and 'minority' block groups are less than 35% 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 how the equity of the distribution of housing between white and minority neighborhoods changes with the switch to district elections. 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 by block group. For time-varying controls, we use the same covariates as in our aggregate supply models, now measured at the block group level.

Results

We present our results in the same order as our hypotheses, beginning with aggregate outcomes across cities. We start with the effect of district elections on single family and multifamily housing units permitted. We then examine our identification assumptions using a series of robustness checks. Next, we test for heterogeneous effects across cities and affordability levels. Finally, we present effects on the spatial distribution of housing within cities, measuring changes in both the concentration of new housing and the role of race in a neighborhood's housing burden.

Aggregate Outcomes

Figure 1 shows the effect of district elections on the number of housing units permitted annually. 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.[17] Conversion to district elections decreases permitting of all housing units by 21 percent, an effect that is just shy of the $p < .05$ threshold of statistical significance. Disaggregating this effect by the structural composition of new housing, conversion has no effect on the permitting of single family homes, but causes a 46 percent decrease in the number of multifamily units permitted annually. These results are consistent with our hypothesis, with multifamily housing being both less desirable and more likely to be vulnerable to local NIMBY pressure via discretionary review compared to single family housing. Results using cities 'eventually treated' are substantively identical to those conducted on the full sample of California cities and are presented in Appendix I.

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

[17] Results in table form are presented in Appendix F.

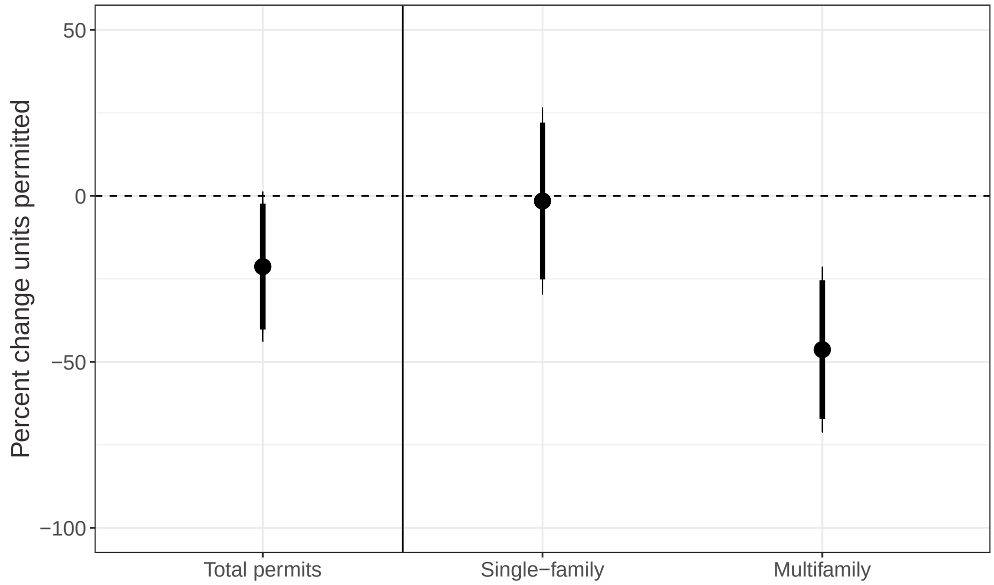

Figure 1: Treatment effects and confidence intervals among all cities. Points are regression coefficients and indicate the difference in number of units permitted within cities following the switch to district elections. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

system. As shown in Figure G-9, cities that switch to district elections do not express a differential in pre-treatment housing trends. We also use a Granger causality test to explore how the housing trends of treated cities differed from those of control cities before and after switching to district elections. To conduct this test, we plot the difference in outcomes between switcher and non-switcher cities, before and after the switch occurs. In this regression, the plotted treatment coefficients represent the difference in outcomes—net of city and year fixed effects, city-specific time trends, and time-varying controls—between switcher and non-switcher cities for each year relative to a baseline of four years before treatment. If, for example, β_{-3} were negative and significantly different from zero, cities that changed to district elections were already seeing lower housing permitting, suggesting that housing trends may cause cities to select into treatment.

We plot these coefficients—from three years prior to two years or more after treatment—in Figure 2, with the horizontal axis representing the number of years before or after the switch to district elections occurred. The figure 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 are uniformly negative and greater than 50 percent in absolute value following the year of the first district election, 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 non-switching cities. Thus, the specification in Figure 1 likely captures the causal effect of district elections on the local housing supply.

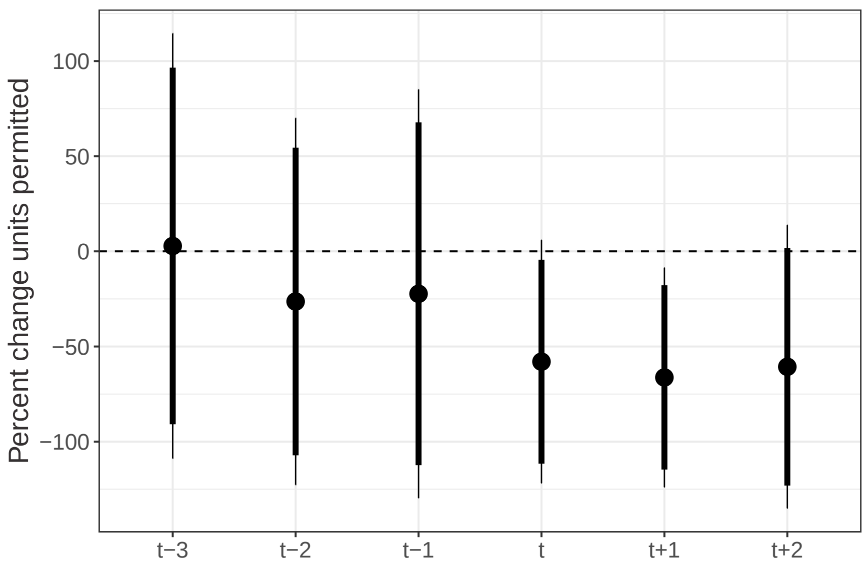

Figure 2: Effect of district elections on multifamily units permitted, checking for pre-treatment differences in outcomes. This figure plots coefficient estimates, with lines indicating 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines), from three years prior to the switch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

Next, as noted, we use two alternative approaches with advantages over the standard two-way fixed effects model. 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-8, are substantively similar to the pattern of results reported in our main Figure 1.

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 outcome histories for 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.[18] 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-9. The same figure shows a statistically significant effect in the first post-treatment year that is very similar to our main effect—a decrease of around 43 percentage points.

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. We visualize these results in Figure 3, with the percent change in multifamily units permitted annually as our dependent variable. Treatment effects across levels of segregation are on the left, with effects according to percent non-Hispanic white presented on the right.[19]

On the left, the effect of district elections appears driven by cities with high levels of segregation. The difference between high and low segregation cities is not statistically significant due to an imprecise effect of district elections in low segregation cities. These results suggest that high segregation may produce the conditions for large differential policy outcomes, whereas cities with low segregation have heterogenous effects our model does not account for.

Figure 3 also presents variation in the treatment effect across cities by racial composi-

[18] We use the PanelMatch package in R (Kim, Wang, and Imai, N.d.) 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.

[19] For histograms of subgroup variables and cleavages, see Appendix E.

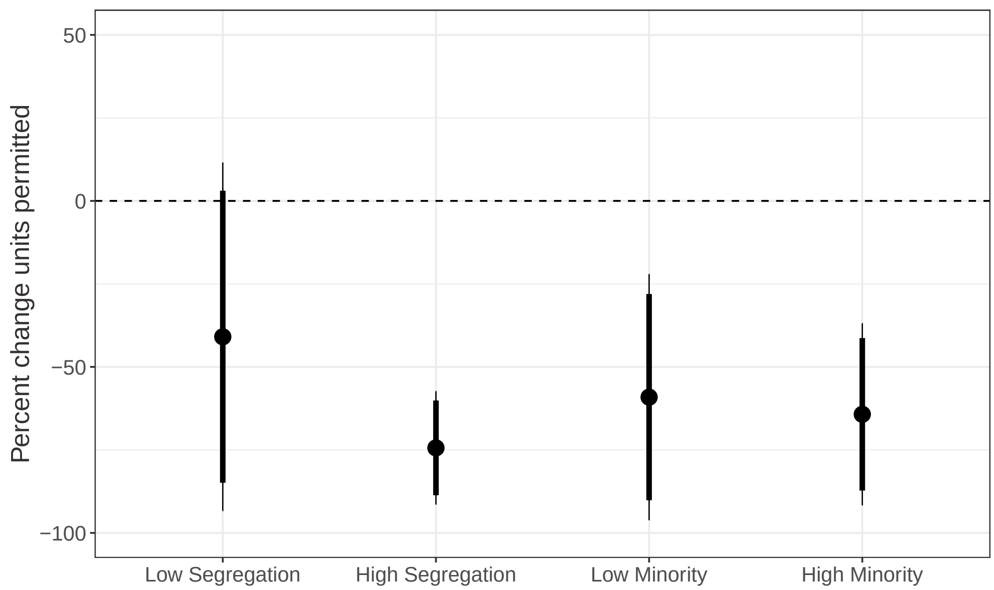

Figure 3: Treatment effects and confidence intervals comparing cities in the top and bottom terciles of segregation (left) and percent non-Hispanic white (right). Points are regression coefficients and indicate the difference in number of units permitted within cities following conversion to single member districts. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

tion. We expected the treatment effect to be concentrated in whiter cities, where district elections would make the largest difference in minority representation. However, treatment is comparably strong across levels of racial diversity. One reason for this lack of variation may be that given how low turnout is in local elections, the voting population in a high minority city may still be white enough to exclude representation. As a result, high minority cities with a fragile white majority voting bloc may be primed to respond with treatment effects as large as those found in whiter cities. Still, the precise mechanisms of segregation, race, and representation require additional analysis, which we outline in our Discussion.

Affordability

Next, Figure 4 presents the effect of district elections on the affordability of new housing, with the proportion of housing that is low-income on the left and changes in the units permitted by affordability band on the right. Switching to district elections causes an 11 percentage point increase in the share of housing that is affordable to low- and very low-income residents ($p = .08$). This 0.44 standard deviation increase appears driven by an increase in the permitting of low-income units, whereas moderate-income and above moderate-income ('High') housing is not affected by the switch in institutions. Unlike our Census permits data, affordability data from the CA Department of Housing and Community Development comes from a shorter panel, generally from 2015 to 2018, limiting our sample size and statistical power.[20]

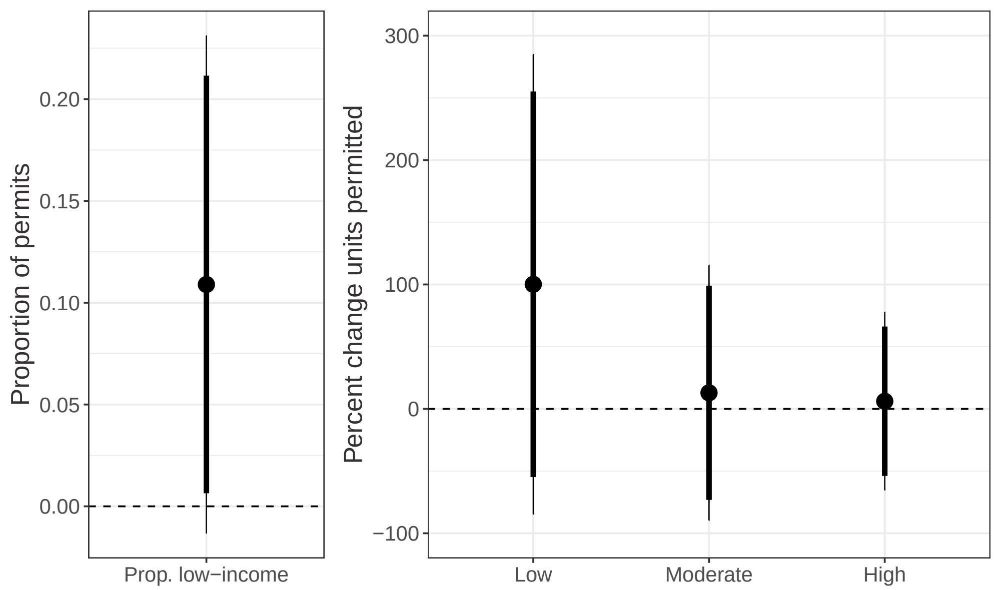

Figure 4: Treatment effects and confidence intervals among all cities. Points are regression coefficients and indicate the difference in the proportion of housing affordable to low-income residents (left) and the number of units permitted by affordability level (right) within cities following conversion to single member districts. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

Combining these aggregate outcomes, district representation reduces the permitting of multifamily housing, but housing which is permitted is more likely to be affordable to low-income residents. Unfortunately, the affordability data do not report whether the units are part of multifamily or single family housing. However, given low-income housing is almost universally multifamily due to construction costs, we can assume that the decrease in multifamily housing presented in Figure 1 is concentrated within market-rate housing.

[20] Variation in length of each city's panel data stems from administrative differences in when the State of California defines each city's planning period for meeting state-defined housing goals.

These policy outcomes reflect the CVRA's aim, to increase the representation of minority—and therefore low-income—voters.

Spatial Outcomes

Having measured the effect of district elections at the city level, we next look at policy outcomes within both treated and control cities. Previously, we focused on multifamily housing as the best proxy for units subject to discretionary review. If district elections increase NIMBY influence, we would expect to observe a change in multifamily permitting. For our within city data, we have built a corpus of all housing decisions subject to discretionary review across six cities. Be they single family or multifamily, these units are directly vulnerable to the political economy of zoning. No longer needing to proxy for units within reach of political pushback, we present the effects of district elections on total approved buildable capacity.

First, we measure the spatial concentration of new housing units by calculating the average Moran's I pre- and post-treatment for our three treated cities: Anaheim, Escondido, and Santa Barbara. As shown in Table 1, new housing is spatially concentrated more than would otherwise be expected under a random distribution in each city prior to switching to district elections.[21] This concentration is not normatively problematic, as cities are by definition spatial clusters of population. However, what is important is whether this concentration is a result of at-large voter aggregation, resulting in politically underrepresented neighborhoods. As evidence, after converting to district elections, housing not only decreases in spatial concentration, but decreases to the point of resembling an as-if random distribution in Santa Barbara and Escondido.[22]

Next, we use a generalized difference-in-differences framework to measure the effect of district elections on log housing units approved at the block group level. Our sample now

[21] Maps of pre- and post-treatment included in Appendix H.

[22] We replicate this process for our three control cities in Table D-4 and find varying trends of increasing and decreasing concentration, suggesting that district elections may be a sufficient, but not a necessary factor for dispersing housing permits.

Table 1: Moran's I, Pre- and Post-Treatment

City	Before Treatment	After Treatment
Santa Barbara	0.22***	0.06
Escondido	0.12**	0.03
Anaheim	0.14***	0.10**

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

includes both our three treated cities and three control cities, generating an 8-year panel of 223 block groups. We interact treatment with an indicator for minority block group, allowing us to compare treatment effects across white and minority block groups, dropping the middle tercile. Table 2 presents the treatment effects on log units approved, as well as on units disaggregated into single family and multifamily housing. Given all units recorded were subject to discretionary review, we discuss the results for total units. However, results examining only multifamily units are substantively the same.

Table 2: Effect of Conversion to Single-Member Districts on Units Permitted, Logged

	Total Units	Multifamily Units	Single-family units
	(1)	(2)	(3)
Single-member districts	0.892***	0.743***	0.157
	(0.244)	(0.203)	(0.150)
Minority block groups	0.487*	0.520**	−0.013
	(0.193)	(0.199)	(0.067)
SMD:Minority block groups	−0.567*	−0.554*	−0.041
	(0.258)	(0.238)	(0.092)
Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Linear Trends	Yes	Yes	Yes
Observations	736	736	736
R^2	0.162	0.193	0.063

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

Under at-large representation, minority block groups saw 63 percent (0.49 log points) 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. This racial imbalance represents the channeling of new housing into the communities most likely to be politically underrepresented in at-large elections. After switching to district elections, this racial relationship with housing breaks down. White block groups experienced a 144 percent (0.89 log points) increase in housing units approved annually. In contrast, because of the negative and statistically significant interaction between treatment and race, minority block groups saw a 9-percent *decrease* in new housing units under district elections. This change is presented in Figure 5, where we see the at-large imbalance in housing associated with race disappear after cities switch to district elections.

That white block groups experienced an increase in buildable capacity under district elections conflicts with our average treatment effect of less new housing under district elections. Importantly, our outcome variable is not identical to that in Aggregate Outcomes, but instead represents potential development which may be actionable over the next several years. Accounting for idiosyncrasies of these aggregate changes is limited by our sample of six cities. However, the distributive consequences of the district elections are clear and statistically significant. The combined increase in units approved in white block groups and decrease in minority block groups closes the gap in annual housing approvals between the two groups. In other words, conversion to district elections has—at least temporarily—broken the spatial relationship between a neighborhood's racial composition and new housing burden.

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

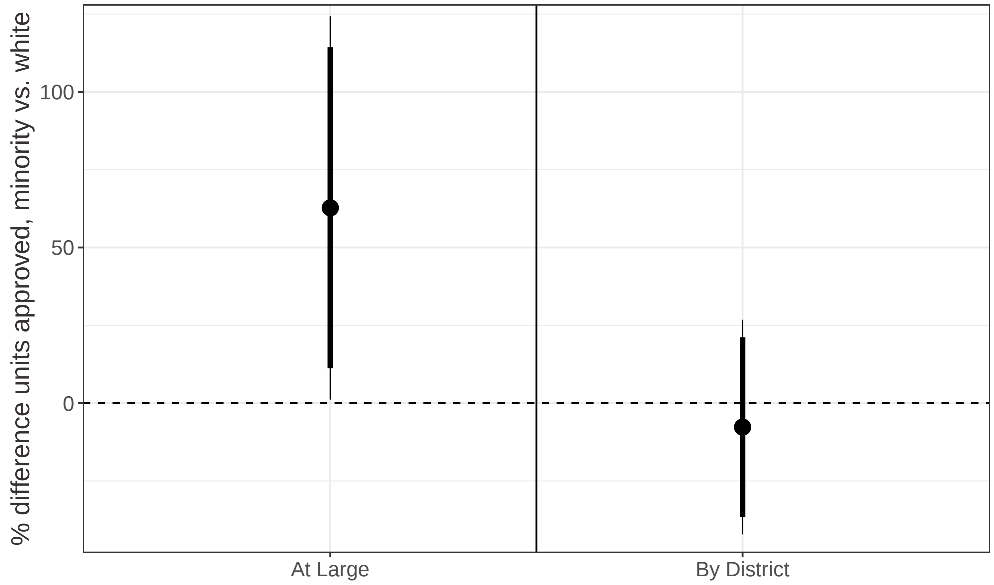

Figure 5: Points are regression coefficients from Table 2 and indicate the difference in housing units permitted between minority block groups and white block groups. On the left are all block groups in at-large systems, including treated units pre-treatment. On the right, block groups in treated cities, post-treatment. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

the racial and spatial composition of constituencies. District-elected council members are not only accountable to the local interests of their constituents, but also have the ability to advance those interests thanks to logrolling. Thus, district elections alter the balance between equity and efficiency for policies with concentrated costs, potentially to the detriment of the citizens they are meant to empower.

In this paper, we show the effect of district representation on one type of concentrated costs: the locally unwanted land use. District elections decrease the permitting of multi-family housing by 46 percent. However, the housing that is permitted is more likely to be subsidized and affordable, reflecting the preferences of previously excluded low-income and minority voters. District elections also spatially disperse the concentration of new housing, breaking the correlation between minority block groups and unwanted development. Simply by changing how cities aggregate voters, we observe this direct trade-off of efficiency in the production of housing and the equitable distribution of its costs.

These findings confront a larger efficiency-equity trade-off at the core of housing policy. By district cities permit fewer new multifamily units, but units permitted are more likely to be affordable to low-income residents. This is fortunate for those lucky enough to win a housing lottery and gain access to one of the price-restricted, subsidized units. But waitlists for these lotteries are measured in years and the majority of those eligible for housing assistance are unlikely to ever receive it (Collinson, Ellen, and Ludwig, 2015). Consequently, most low- and moderate-income residents rely on market-rate housing, which can only maintain affordability if new supply keeps up with demand.[23] Restricting the influx of multifamily housing is likely to drive their housing costs even higher (Mast, 2019). In other words, while manifesting the policy demands of low-income renters for less nearby market-rate housing and more affordable housing, district elections may be a hollow victory in the long run.

Beyond housing, other policies with concentrated costs and diffuse benefits risk undersupply when decided by logrolling institutions. Because city councils and county commissions govern the vast majority of land use decisions in the United States, we expect this efficiency-equity trade-off to plague the siting of nearly all LULUs, from the clean energy infrastructure necessary to combat climate change to the addiction treatment facilities needed to stem the opioid epidemic. Our findings also pair with lessons from policies outside of land use. As discussed by Hills Jr and Schleicher (2011), 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 policies and removed substantial discretion from the legislature. We suggest a similar reform.

To counter this decrease in multifamily supply, district elections can be paired with top-down pressure from the state government via withholding intergovernmental transfers and grants (e.g., Dillon, 2019; Elmendorf, Forthcoming). The state government has an interest in each city permitting its share of new housing to maintain statewide affordability and

[23] Affordability for low-income residents cannot be fully achieved with new supply and requires direct subsidy or regulations requiring its production with new market-rate housing (Been, Ellen, and O'Regan, 2019).

economic growth. As we demonstrate, with at-large elections, such top-down pressure would likely channel market-rate housing into underrepresented, minority neighborhoods. But under district elections, with more equal representation secured, the push for supply will be more equitably spread across neighborhoods. This pressure would generate new housing to secure market-rate affordability while also improving minority representation within cities, fulfilling the mission of the original Voting Rights Act of 1965.

Broadly defined, policies with concentrated costs and diffuse benefits have never been popular (Wilson, 1980). The NIMBY politics of locally unwanted land uses present a uniquely challenging concentrated burden, one subject to the spatial aggregation of voters. We have identified how this aggregation presents a 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 a better understanding of how institutions shape behavior, both among voters and the decision makers themselves.

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Supplementary Appendix for "How Electoral Institutions Shape the Efficiency and Equity of Distributive Policy"

A District Elections, California

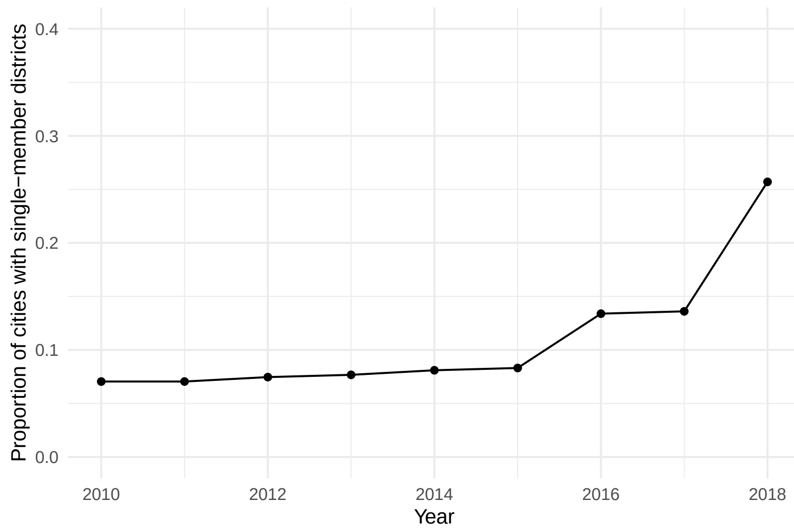


Figure A-6: Proportion of California cities with district elections over time.

B Matched Pairs Comparison

Table B-3: Comparison of Treated and Control Units, Pretreatment Covariates

	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 activity 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 and likely adds researcher measurement error. 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 a corpus of permits that were approved by passing through discretionary review. These data capture the output of permits that should be most directly affected by the change in representation from district elections.

D Moran's I

Consider a region divided in n areas and let y_i be a random variable measured in area i, with $i = 1, \ldots, n$. Moran's I is given by

$$I = \frac{n}{\sum_{ij} w_{ij}} \frac{\sum_{ij} w_{ij}(y_i - \overline{y})(y_j - \overline{y})}{\sum_i (y_i - \overline{y})^2} \tag{1}$$

where the value w_{ij} is the weight assigned to areas i and j, and $\overline{y} = \sum_i y_i/m$. Usually, w_{ij} will reflect the geographical distance between areas i and j, being defined, for example, as $w_{ij} = 1$ if the areas are adjacent and $i \neq j$, and by $w_{ij} = 0$, otherwise. However, weights can be defined depending on functions of distances between the areas. Moran's I ranges between -1 and 1 with large positive values indicating neighborhood similarity of the rates and values close to zero indicating absence of spatial autocorrelation.

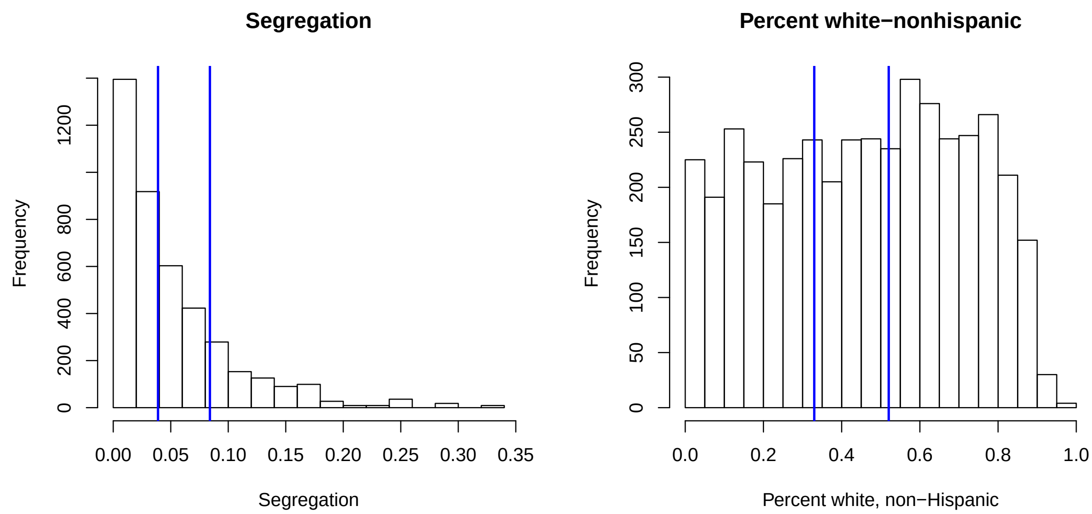
Table D-4: Moran's I, Pre- and Post-Treatment (Control Cities)

City	Before Treatment	After Treatment
Santa Cruz (Control for Santa Barbara)	0.05	0.09**
Ventura (Control for Escondido)	0.07**	0.04
Glendale (Control for Anaheim)	0.24***	0.12***

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

E Distributions of Heterogenous Effects Variables

Figure E-7: Distributions of variables used to assess heterogeneous effects across cities. Tercile cutpoints marked in blue.



F Table Results

Table F-5: Effect of Conversion to Single-Member Districts on Housing Permits

	All cities			Eventually treated subset		
	Total	Single	Multi	Total	Single	Multi
	(1)	(2)	(3)	(4)	(5)	(6)
Single-member districts	−0.239	−0.015	−0.622**	−0.123	0.131	−0.780**
	(0.146)	(0.146)	(0.237)	(0.167)	(0.158)	(0.284)
Population (thousands)	−0.001	−0.002	−0.0003	−0.004	−0.005	0.00003
	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)	(0.004)
Percent non-Hispanic white	−0.331	−0.089	−0.084	−0.397	0.814	−1.984
	(1.529)	(1.262)	(1.542)	(5.473)	(4.973)	(9.168)
Percent Black	−0.025	−0.015	−0.014	−0.095	0.005	−0.257
	(0.029)	(0.025)	(0.027)	(0.118)	(0.116)	(0.180)
Percent Hispanic	−0.006	−0.014	0.010	−0.010	−0.049	0.049
	(0.018)	(0.014)	(0.021)	(0.072)	(0.053)	(0.119)
Vacancy rate	0.961	0.423	1.642	5.220	2.046	10.793
	(1.618)	(1.388)	(1.546)	(6.610)	(5.902)	(10.857)
Home ownership rate	0.602	−0.601	1.723	7.882*	3.357	8.499
	(0.964)	(0.781)	(1.109)	(3.594)	(3.086)	(5.217)
Median home value (thousands)	−0.0004	−0.0003	−0.00000	0.002	0.001	0.001
	(0.0003)	(0.0003)	(0.0003)	(0.002)	(0.002)	(0.004)
Median income (thousands)	0.004	0.002	0.001	0.020	0.018	−0.004
	(0.004)	(0.003)	(0.004)	(0.028)	(0.025)	(0.047)
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	4,162	4,162	4,162	1,042	1,042	1,042
R^2	0.824	0.852	0.671	0.789	0.809	0.643

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

Table F-6: Effect of Conversion to Single-Member Districts on Housing Permits, by Affordability Status

	Prop low income	All permits	Low income	Moderate	Above moderate
	(1)	(2)	(3)	(4)	(5)
Single-member districts	0.109	0.256	0.694	0.122	0.060
	(0.062)	(0.289)	(0.471)	(0.463)	(0.344)
Population (thousands)	0.0001	0.003	−0.00004	0.006	−0.002
	(0.0005)	(0.003)	(0.003)	(0.004)	(0.004)
Percent non-Hispanic white	−1.569	0.430	−7.257	6.976	3.269
	(1.306)	(4.697)	(6.611)	(5.693)	(5.686)
Percent Black	−0.003	0.146	0.109	0.097	0.146
	(0.034)	(0.142)	(0.181)	(0.168)	(0.186)
Percent Hispanic	−0.008	0.028	−0.021	0.015	0.049
	(0.014)	(0.062)	(0.079)	(0.072)	(0.069)
Vacancy rate	−0.011	1.875	−2.934	0.129	2.987
	(1.535)	(5.313)	(8.313)	(6.810)	(6.548)
Home ownership rate	−0.627	2.999	−2.795	−2.327	1.495
	(0.820)	(2.874)	(4.165)	(3.383)	(3.515)
Median home value (thousands)	0.00004	−0.0002	0.00003	0.0003	−0.0003
	(0.0001)	(0.0005)	(0.0004)	(0.001)	(0.001)
Median income (thousands)	−0.001	0.001	−0.002	−0.010	0.003
	(0.001)	(0.007)	(0.009)	(0.008)	(0.008)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
City-specific Trends	Yes	Yes	Yes	Yes	Yes
Observations	1,550	1,550	1,550	1,550	1,550
R^2	0.670	0.885	0.711	0.792	0.888

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

Table F-7: Effect of Conversion to Single-Member Districts on Housing Permits, Interacted with City Characteristics

	All cities		Eventually treated	
	(1)	(2)	(3)	(4)
Single-member districts	-1.029^{**}	-0.526	-1.208^{**}	-0.526
	(0.391)	(0.453)	(0.387)	(0.490)
SMD:High white group	0.135		0.256	
	(0.602)		(0.622)	
SMD:High segregation group		-0.836		-0.822
		(0.557)		(0.568)
Population (thousands)	0.0004	-0.001	0.001	0.002
	(0.004)	(0.004)	(0.005)	(0.005)
Vacancy rate	2.448	1.900	22.205	11.269
	(1.665)	(1.507)	(13.204)	(12.718)
Home ownership rate	1.067	1.372	6.113	7.042
	(1.013)	(0.972)	(5.519)	(5.880)
Median home value (thousands)	-0.00001	0.00002	0.003	0.003
	(0.0003)	(0.0003)	(0.003)	(0.003)
Median income (thousands)	0.0004	0.0005	0.029	-0.012
	(0.004)	(0.003)	(0.049)	(0.053)
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	3,280	3,069	700	684
R^2	0.661	0.695	0.640	0.666

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

G Robustness Checks

Figure G-8: 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).

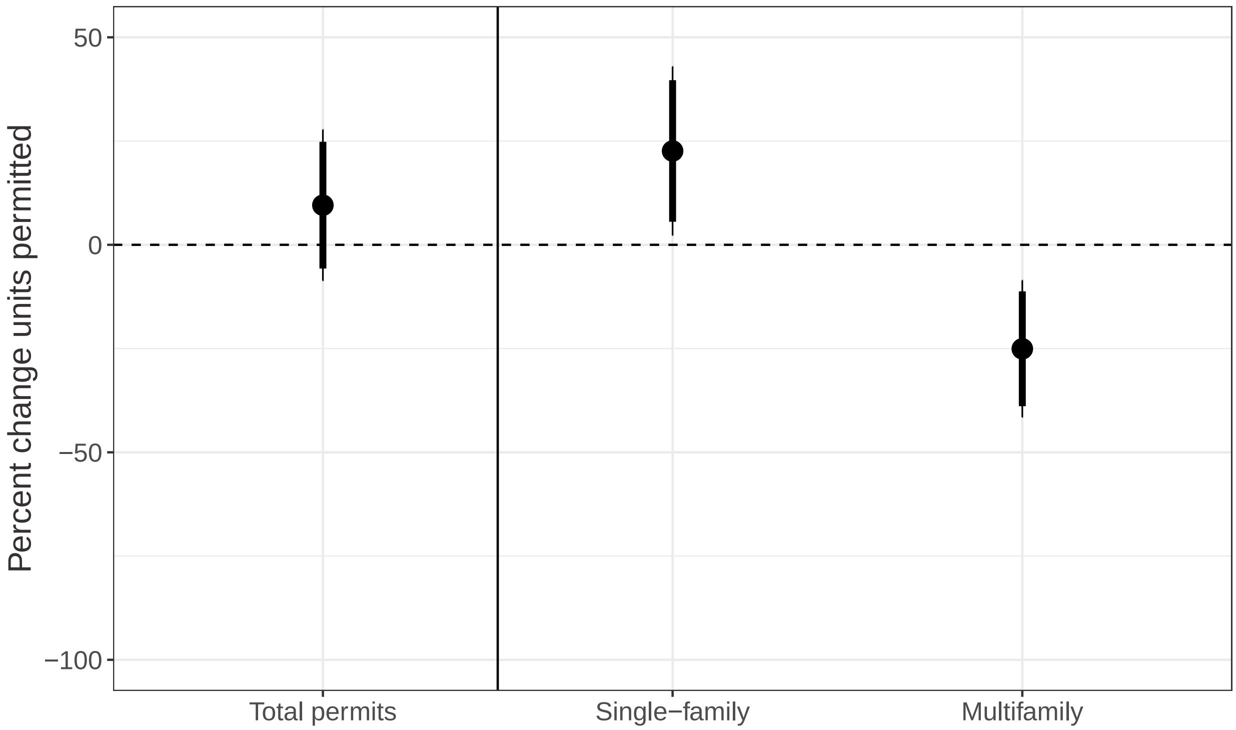


Figure G-9: Treatment effects computed using the nonparametric generalization of difference-in-differences recommended by Imai, Kim, and Wang (2018). All treatment effects are computed with respect to the first pre-treatment year (t-1). Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

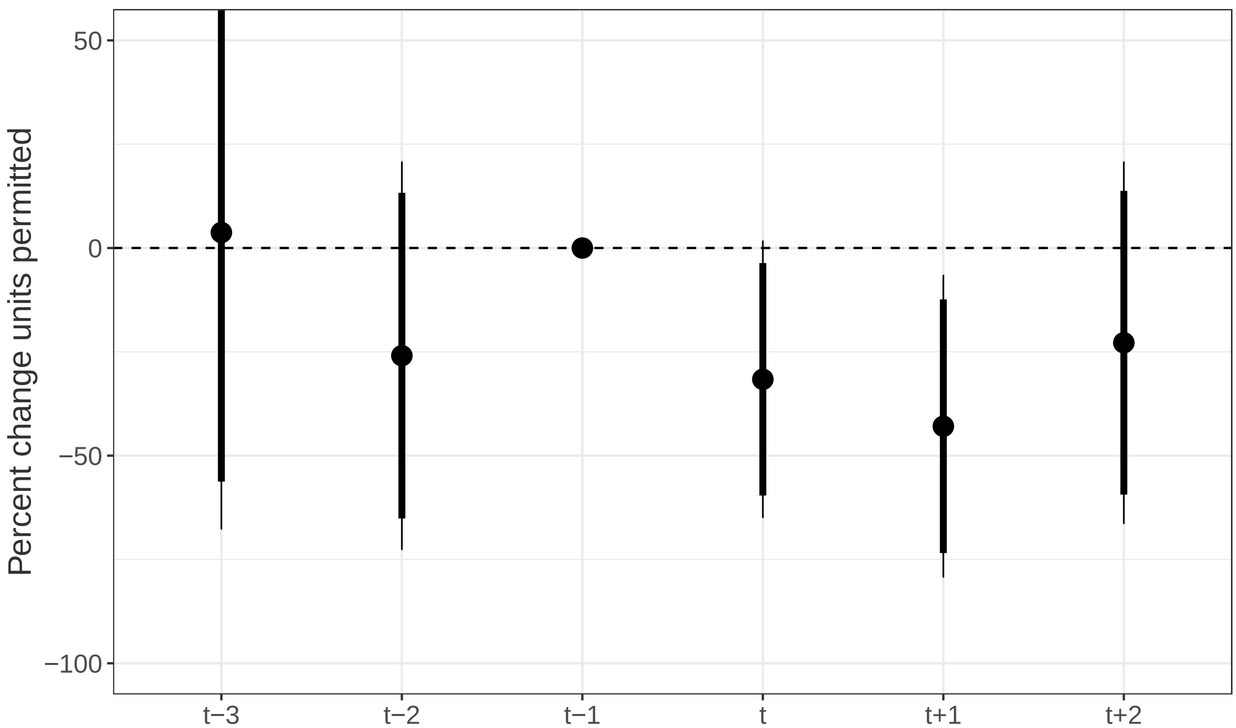
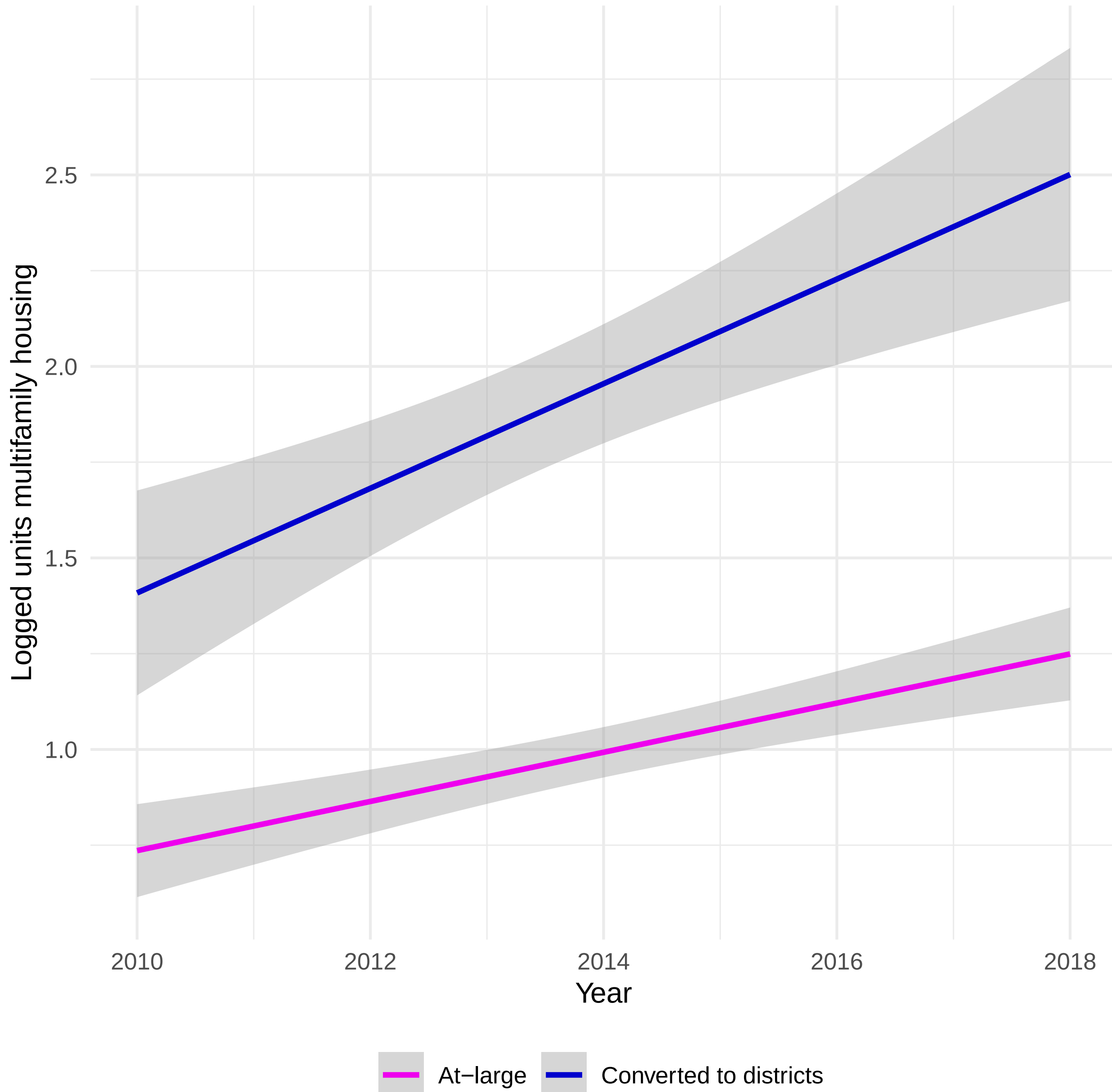
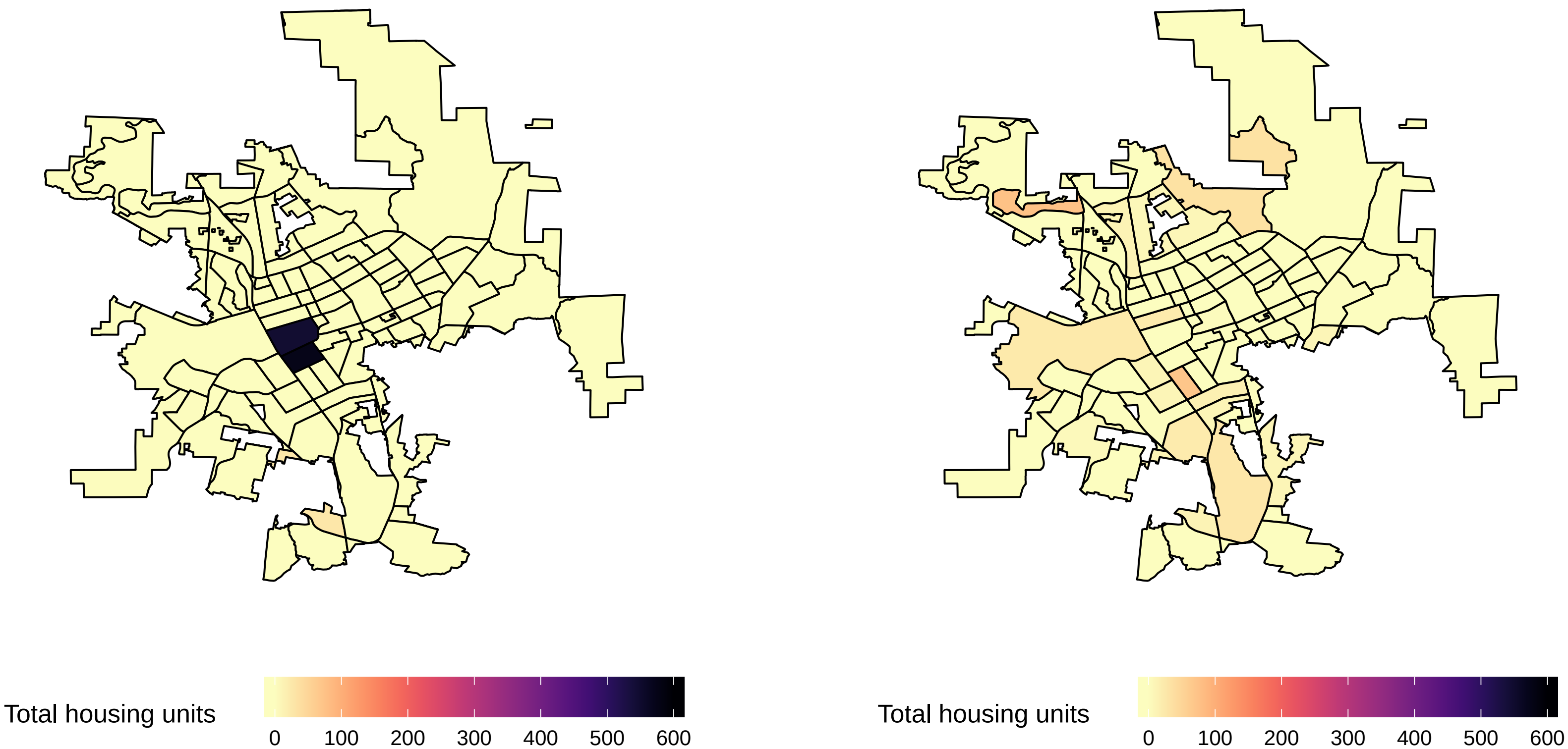


Figure G-10: Pre-treatment trends in multifamily housing. 95%-confidence intervals are shown in grey.



H Maps of Treated Cities, Housing Pre- and Post-Treatment

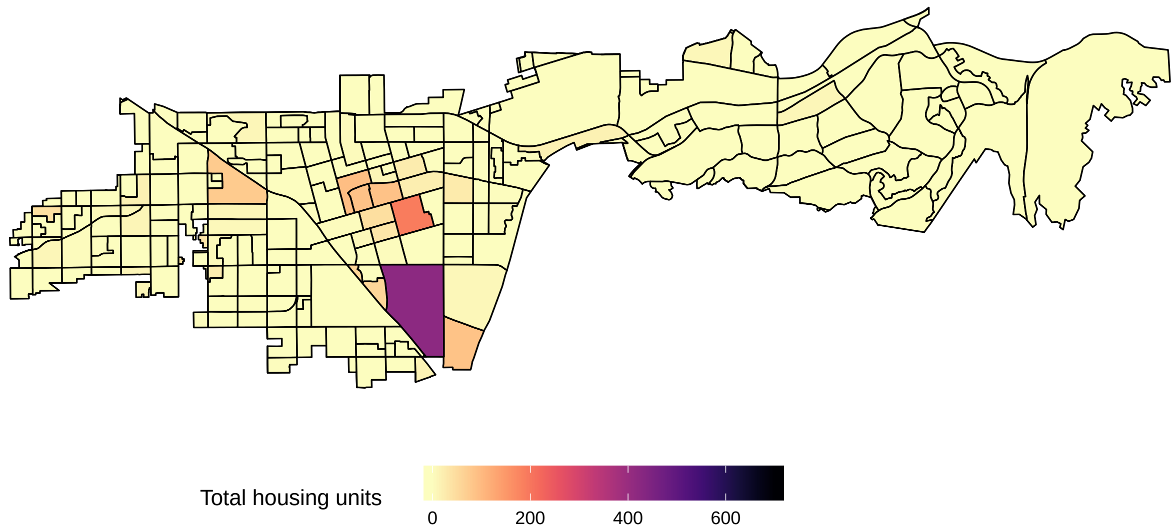
Figure H-11: Distribution of Total Housing, Escondido



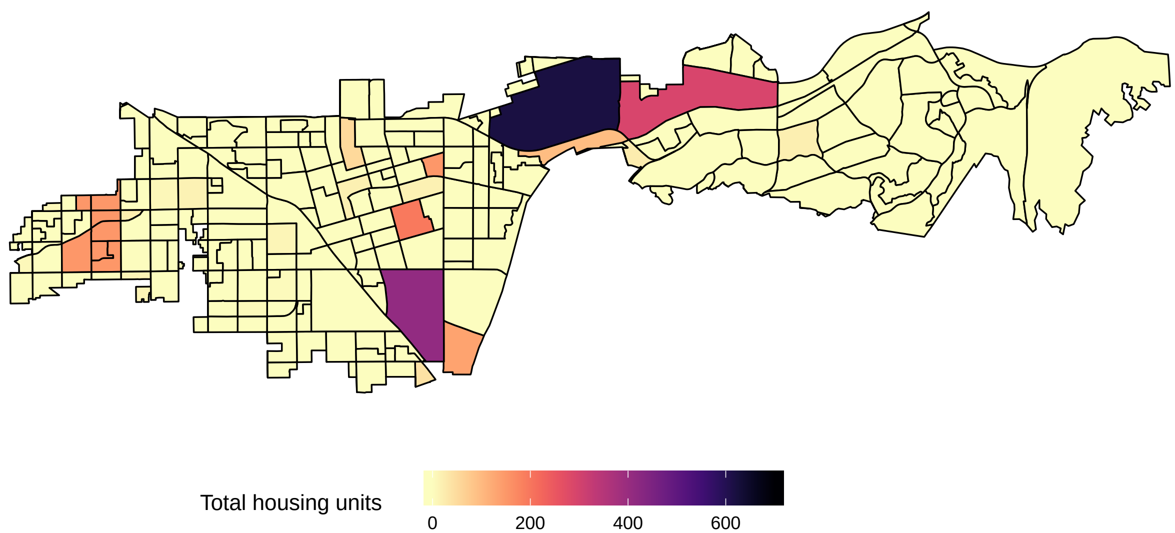
(a) Pre-treatment

(b) Post-treatment

Figure H-12: Distribution of Total Housing, Anaheim

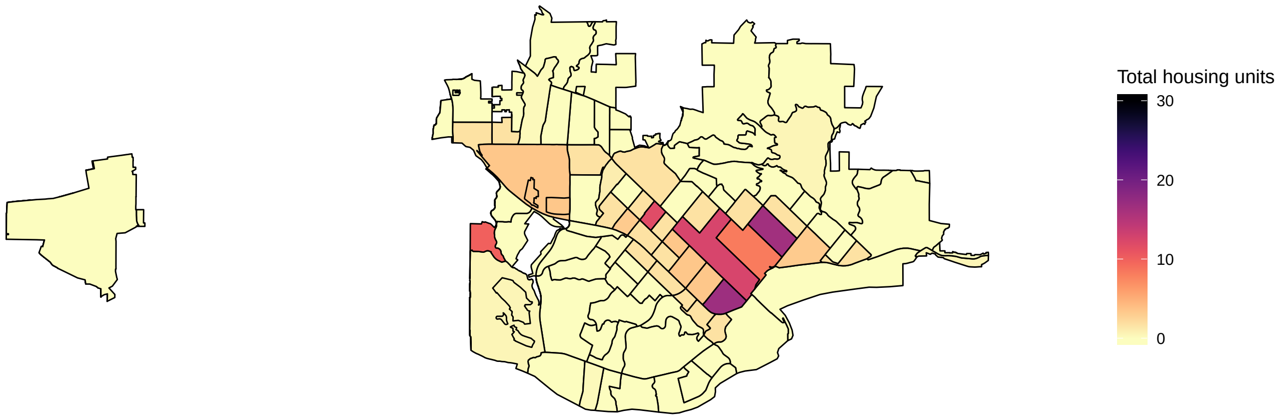


(a) Pre-treatment

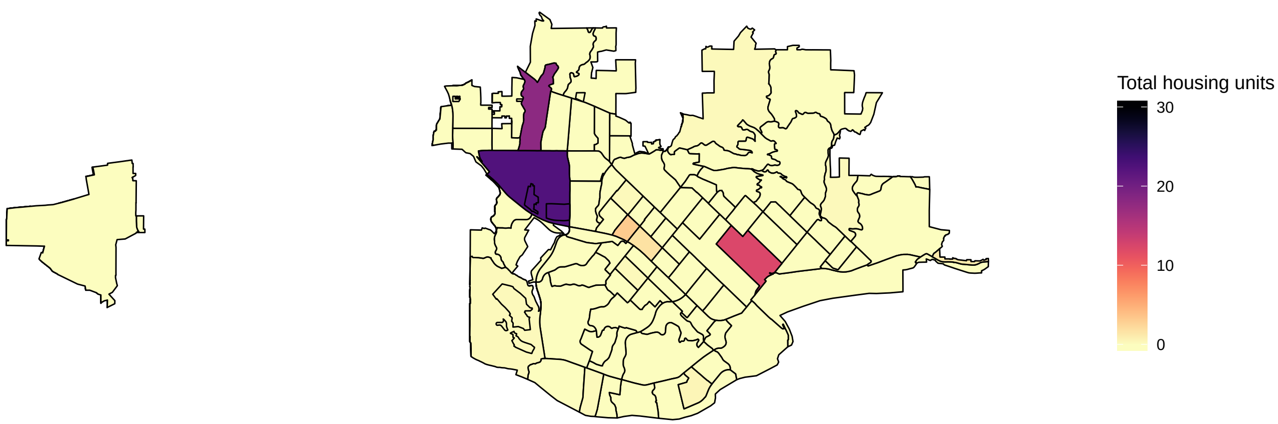


(b) Post-treatment

Figure H-13: Distribution of Total Housing, Santa Barbara



(a) Pre-treatment



(b) Post-treatment

I Results with Subset of 'Eventually Treated' Cities

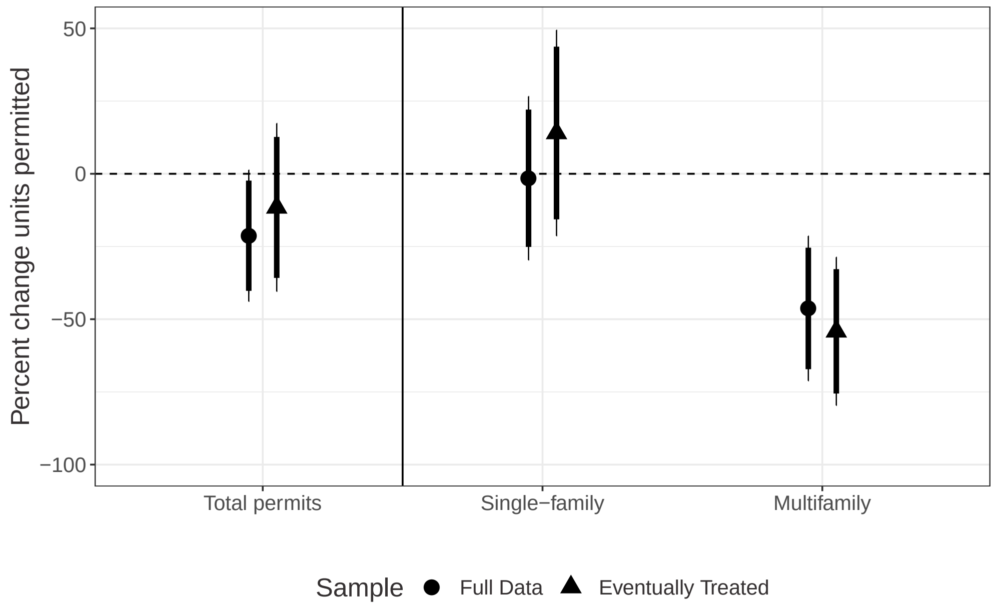


Figure I-14: Effect of district elections on multifamily units permitted, checking for pre-treatment differences in outcomes. This figure plots coefficient estimates, with lines indicating 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines), from three years prior to the switch to two years after.

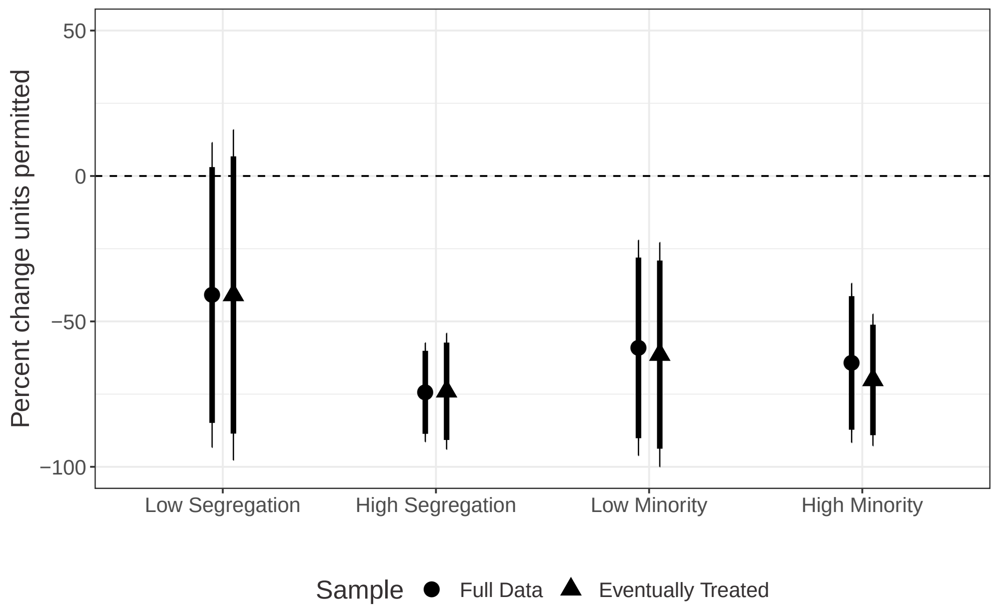


Figure I-15: Treatment effects and confidence intervals comparing cities in the top and bottom terciles of segregation (left) and percent non-Hispanic white (right). Points are regression coefficients and indicate the difference in number of units permitted within cities following conversion to single member districts. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).

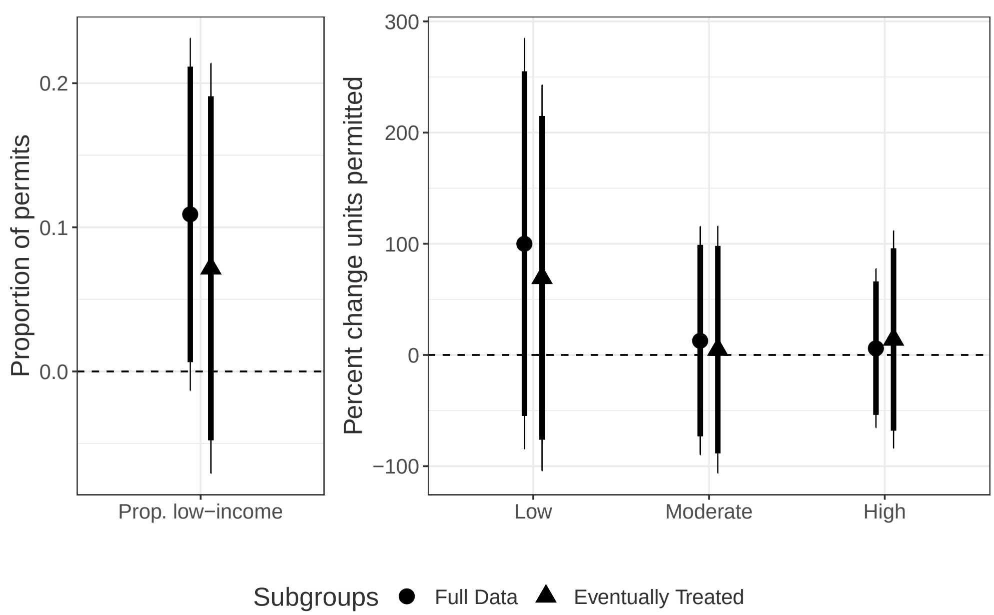


Figure I-16: Treatment effects and confidence intervals among all cities. Points are regression coefficients and indicate the difference in the proportion of housing affordable to low-income residents (left) and the number of units permitted by affordability level (right) within cities following conversion to single member districts. Lines indicate 95%-confidence intervals (thin lines) and 90%-confidence intervals (thick lines).