CAN STATES PROMOTE MINORITY REPRESENTATION? ASSESSING THE EFFECTS OF THE CALIFORNIA VOTING RIGHTS ACT (FORTHCOMING *Urban Affairs Review*)

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ABSTRACT

One goal of the California Voting Rights Act (CVRA) is to broaden representation in largely White-controlled city councils state-wide by incentivizing cities to shift council electoral jurisdictions from at-large to single-member districts. However, little research has investigated whether the CVRA helped contribute to increased minority representation at the city level. This paper employs matching and difference-in-difference methods to determine if cities that switched to district elections as a result of the CVRA enhanced city council diversification. By comparing matched treatment and control group's racial composition of city councils before and after fully switching from at-large to district election jurisdictions, we estimate the average treatment effect (ATE) of city switching on minority city council representation at 10-12%. Further analysis reveals treatment effects are larger among cities with larger shares of Latinos (21%). Thus, states seeking to increase local-level minority representation should consider policies similar to those found in the California Voting Rights Act.

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Keywords Voting Rights · Minority Representation · Local Politics · State Politics · Redistricting

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Introduction

California legislators passed the California Voting Rights Act (CVRA) in 2001; on July 9, 2002, then-governor Gray Davis signed the bill into law. This act makes it easier for minority groups to seek racial representation at the local level. It does this by eliminating the federal Voting Rights Act (VRA) requirement that plaintiffs establish that minority voters are concentrated enough to form a majority within a single district. Under the CVRA, to force a city to shift to a single member district scheme, plaintiffs only have to establish the presence of racially polarized voting. At the time the CVRA was signed into law, 449 of California's 476 cities employed at-large districts to elect candidates to the city council.

Even in a state as ostensibly progressive as California, laws like the CVRA may be needed to expedite racial/ethnic representation at the local level. Large geographic regions of the state continue to lean politically conservative, particularly in the state's "Inland Empire" (Riverside and San Bernardino Counties), Central Valley, and remote interior north (colloquially coined Jefferson).[1] Compared to their coastal counterparts, Whites in these areas tend to vote overwhelmingly Republican, whereas minorities (disproportionately Latino) favor Democratic candidates (Collingwood et al., 2014; Barreto, 2010). These observations square with the notion that local politics can be conceptualized as a racial competition over resources (Kaufmann, 2004; Bishin et al., 2012; Kaufmann and Rodriguez, 2011) leading to majoritarian manipulation of electoral rules (Trebbi et al., 2008). Furthermore, significant evidence indicates that Whites perceive minority growth as a threat to their interests, whether that be self-esteem maintenance, perceptions of financial security, or outright racism (Newman, 2013; Newman et al., 2018). And even though the Latino population in many of these areas remains large, Whites still control local and city offices. For example, Latinos comprised 54% of Kern County's (Bakersfield) population as of 2018,[2] but just one Latina sat on the County Board of Supervisors.[3]

While we know that the Voting Rights Act of 1965 eventually led to a rise in Black political power across the South both in terms of voting and representation (Lublin, 1999a,b; Lublin and Voss, 2000; Grofman et al., 1992, 2000), and that at-large systems are generally more likely to elect majority-White councils than are district-based systems (Trounstine and Valdini, 2008; Bledsoe, 1986; Berry and Dye, 1979; Welch, 1990; Davidson and Korbel, 1981), no research has examined whether state laws – like the *California Voters Rights Act* – can influence the shape of local level racial representation. Furthermore, studies showing that at-large districts are less racially representative than are single-member districts do not take selection-effects into account; but rather employ model-based analyses to estimate the effects of system-type on racial representation (Welch, 1990, e.g.). A tighter design is to examine whether minority representation changes in cities that switch from at-large to single-member district.

This paper therefore asks the following question: Has the *California Voting Right Act* enhanced minority representation on city councils around the state? Because the CVRA reduced the standard of proof needed to force cities to shift from at large to district election jurisdictions, lawyers and activists initiated a campaign to sue cities for violating the law that almost certainly would not have happened had the CVRA not been in place. The CVRA clearly plays a causal – although not statistically identifiable – role in affecting city council racial representation. Given this, do cities that move from at-large to district-based elections actually elect greater shares of minorities to their respective city councils? More precisely, among cities with otherwise similar demographic profiles, what is the average effect of switching from at-large to single member district on minority city council representation?

To answer these interrelated sets of questions, however, given demographic and political changes present throughout California, we cannot simply examine minority council representation in cities that switched compared to cities that did not switch. The mechanism leading cities to shift from at-large to single member districts is likely not random, so city switchers' underlying covariate structure may produce the observed treatment effect (or lack thereof) as opposed to the treatment itself. For instance, cities with large and growing Latino populations might be more likely to shift from at-large to single member districts, and so the observed treatment effect is not a function of the treatment but rather of the racial/ethnic make-up of the city. Or, Whites in cities that switch to single member districts might be more progressive, and so the result of the observed treatment is a reflection of more liberal Whites expressing support for minority candidates.

At the same time, general trends in the state's political climate and demographic composition may co-occur as cities shift from at-large to district. It is possible, for instance, that all cities in California have elected more diverse city councils in recent years simply because the state as a whole and cities within it have become more diverse and politically Democratic. Specifically, it is possible that a city that changed from at-large to single-member district due to the CVRA may have elected more minority representatives across the same time period anyway due to changing demographic and political indicators.

Or, cities may be switching to districts not because of the CVRA, but as Trebbi et al. (2008) argue, the White citizenry may want to forestall the risk of losing all seats when the racial minority becomes the majority. We show this is not the case with respect to the CVRA. First, a number of cities that switched from at-large to district elections post CVRA did so under threat of lawsuit inspired by the passage of the CVRA not necessarily on account of demographic patterns. Second, empirically, the correlation between city switching and minority population size is just 0.108, indicating that city demographics played only a limited role in inducing city switching.

Still, to address these selection concerns, we employ a causal inference matching design and difference in difference approach to assess the racial representative effects of switching from at large to single member districts. Using a nearest neighbor matching algorithm, we pair each city that has moved from at-large to district-based elections (treatment) with a politically and demographically similarly situated city that did not make a shift during the same period (control). To take added precaution, we further specify a difference in difference design by comparing minority council representation before/after in cities that switched relative to similarly situated cities that did not switch. For treatment (switchers) and control (similarly situated non-switchers), we can therefore compare the share of city council members that are White prior to the first district-based election against city council racial composition after cities fully switched to single-member districts. Our estimation procedure guards against concerns about selection effects and endogeneity enabling the production of causal treatment effect estimates on minority council representation (Rubin, 2006; Morgan and Winship, 2015).

Regardless of our estimation technique, we find that shifting from at-large to election districts leads to, on average, between 10-11% increase in minority representation on a city council. This equates to roughly 1/2 of a council seat. However, as one might expect, we find that this representational effect is concentrated in cities with larger shares of Latinos. Specifically, high-density Latino cities obtain, on average, roughly 1 full council seat, whereas low-density Latino cities that switch tend not to see an increase in minority city council representation. Overall, our analysis suggests that laws like the CVRA can produce more racially representative, and therefore more democratic, local governments. Below, we outline relevant literature on voting rights and minority representation. We then review how the CVRA induced some cities to switch from electing city council members on an at-large basis to a single member district scheme. This review generates two hypotheses. Next we discuss our data and methods, followed by our results. We conclude with some final thoughts and suggestions for future research.

Descriptive Representation, Voting Rights, and District Type

Extensive research reveals the democratically positive effects of descriptive and minority representation. For instance, Mansbridge (1999) finds that descriptive representation produces substantive benefits for minority populations. This conclusion has been replicated in numerous studies. Both Brown and Banks (2014) and Brown (2014) find that Black female legislators are more likely to sponsor legislation beneficial to minority or otherwise disadvantaged groups, and Hero (2013) goes further in linking legislators' racial identities directly to the quality of support they provide to racially similar constituents.

Descriptive representation is also associated with increased minority political participation. For instance, Barreto et al. (2004) and Gay (2001) find that descriptively similar candidates lead to increased turnout by the represented population. Moreover, Gay (2010) finds that black and Latino residents are more likely to vote when there are more black or Latino legislators. The benefits of descriptive representation have even been connected explicitly to transitions towards by-district elections, such as in Bledsoe (1986).

Research demonstrates that at-large election districts systematically minimize minority representation. Berry and Dye (1979) showed that at-large elections systematically bias against minority city-council representation across the nation. Berry and Dye (1979) argue that at-large districts are so effective at blocking minority representation because 1) whites hold majority status within cities, and 2) city-wide contests require greater campaign resources. Other studies across the decades find similar results (Lublin, 1999a,b; Lublin and Voss, 2000; Grofman et al., 1992, 2000). For instance, in testing a variety of elements related to the VRA's "totality-of-the-circumstances" test, Bullock and MacManus (1993) find that city councils in the South tend to have greater shares of Black elected officials with single-member districts as opposed to at-large districts. Further, Welch (1990) finds that minorities continue to struggle in non-minority districts and attributes political representation in the South to VRA-inspired transitions. Additional research, namely Kousser (2008), has specifically looked at the impact of vote dilution on Latino populations in California. That paper found that

Latinos struggle to win elections in White majority districts. Latino candidates had a difficult time winning office when less than fifty percent of a district population is Latino.

This is not to say that the literature has been without nuance. For example, Trounstine and Valdini (2008) observed that by-district elections are only successful when districts are highly concentrated and where minorities make up substantial portions of the population. This suggests that shifts from at-large to single-member district may not immediately produce normatively desirous results – and that observable shifts in minority representation may be more likely to occur in cities with relatively large minority populations. We concur with this logic, which serves as the theoretical motivation behind our hypothesis that the effects of city-switching on minority council representation will be notably pronounced in largely Latino cities.

However, extant literature points to the success of legislation, as well as litigation, in enforcing the VRA and making cities transition to by-district elections. Davidson and Grofman (1994) observed that cities forced to switch from at-large to district throughout the South resulting in increased African American representation. The rest of the literature has similarly linked these transitions to the enforcement of the VRA (Trounstine and Valdini, 2008; Bledsoe, 1986; Berry and Dye, 1979; Welch, 1990; Davidson and Korbel, 1981, e.g.) Grofman et al. (1992) traced the increasing role of social science testimony in establishing racial polarization and vote dilution, while Davidson and Grofman (1994) pointed to the role of litigation in a majority of studied transitions in the South. This trend is not absolute, however, as Kousser (1999) found that assertions of colorblindness in terms of district creation have systematically excluded minority votes and been used to chip away at the protections put in place by the VRA.

While compelling, the work thus far raises questions that have been insufficiently explored due to the scholarships' methodological approaches. For instance, it is unclear whether demographic features of cities themselves predict whether that city is likely to transition from at-large to district or whether there is a confounding role for differing political climates (i.e., ideological; racial variation). This question generally hampers the efforts of analyses that compare cities that have already switched with cities that remain at-large and contrast representation in each.

The Case: The California Voting Rights Act

In 2001, Democratic state senator Richard Polanco introduced the *California Voting Rights Act (CVRA)*. At the time, California was only just emerging as a Democratic majority. The year later, for instance, Democrat Governor Gray Davis defeated Republican Bill Simon 47-42, winning just 18 of the state's 58 counties. In the 1990s, the state passed the now-infamous anti-immigrant Proposition 187, with polls showing Whites supporting the measure and Latinos opposing it (Lee et al., 2001).[4] However, since 2010, California has moved in a strongly Democratic direction, with formerly dominant Republican areas, such as Orange County, voting Democrat in the 2016 general election, [5] and White Democrats shifting in a strong pro-immigrant direction particularly with respect to sanctuary policy (Collingwood et al., 2018).

The CVRA sought to prohibit cities from organizing council elections at-large, which tends to have the effect of preventing protected classes, such as people of color, from electing desired candidates. In at-large cities, each member of the city council is selected by a majority of voters in the entire city, as opposed to voters selecting candidates in single-member districts. The CVRA's provisions essentially focused on cities where substantial Latino populations tended to support the same candidate, but because such Latino populations still remained a minority of total voters, at-large elections resulted in their candidates losing consistently.

Quickly, groups like the American Civil Liberties Union (ACLU) and the Mexican American Legal Defense and Education Fund (MALDEF) endorsed the bill. More than 60% of both houses supported the measure, and, on July 9, 2002, Governor Gray Davis signed the bill into law. Despite relatively broad support, numerous court challenges were also levelled at the law, delaying implementation. By 2006, however, in *Sanchez v. City of Modesto*, the Fifth District Court of Appeals ruled the law constitutional.

Nevertheless, the enactment of the CVRA has not resulted in the immediate mass transition away from at-large voting districts. Instead, the CVRA empowered citizens who were discriminated against to initiate legal action in order to compel their city to switch away from at-large districts. This places the burden of work on an already under-represented population, and requires the hiring of lawyers and possibly expert witnesses. Usually, these transitions lead to the creation of by-district elections, where each city is subdivided into districts and the voters within each district vote for a council member specifically tied to that district. Pursuant to the CVRA such districts would be tailored towards creating districts that would represent Latino populations, or those of other protected classes who have filed suit.

Initially, when challenged, many cities responded with sustained legal defenses. For example, a group of Latino citizens sued the city of Modesto eventually leading to a switch to by-district elections and a $3 million lawsuit. Palmdale also served as a high-profile case of city resistance to the CVRA. After being sued for non-compliance, Palmdale waged a three-year legal battle against the switch to by-district elections, eventually agreeing to the switch and to a settlement of $4.5 million. In the wake of such high-profile lawsuits, a wave of transitions to by-district elections continued, encouraged by widespread threats of lawsuits by legal firms. Given the high-profile losses of Modesto and Palmdale, upon being informed that a city was eligible for a lawsuit, many cities proceeded to change voluntarily but often delaying and staggering the switch to by-district elections.

City switching tends to occur for two reasons: 1) local activism and 2) legal pressure. While gathering data on local activism is nearly impossible to do in a systematic and comprehensive way, we attempt to account for this by incorporating party registration, racial demographics, demographic change and Latino voter turnout into our research design. Many of the cities that have switched to by-district elections did so due to the legal pressure of lawyers, such as the Malibu-based Kevin Shenkman. Knowing that the CVRA makes it legally easier to win voting rights cases, the CVRA incentivized lawyers and activists to push for election reform which they did not have prior to CVRA's passage. Shenkman, for instance, sent letters to eight of the treated cities threatening legal action if the cities did not switch from at-large to single-member districts and is generally considered to be central in the state-wide push to enforce the CVRA. While he only targeted a minority of treated cities, the impact of the Palmdale case, which Shenkman litigated, may have had a broader shift in encouraging other cities to transition. In an email to the paper's authors, Shenkman explained that he chose cities to contact based on a variety of factors, including demographic features, existing council demographics, and racial voting patterns,[6] but that his method was not systematic (i.e., ranking cities on some sort of multi-dimensional scale of least likely to most likely to increase minority representation).

While Shenkman's case selection was certainly not random, we argue that it was haphazard – as many demographically similarly situated cities as those targeted for CVRA challenge were not ultimately targeted.[7] This is underscored by the fact that Shenkman's office was not the only one targeting cities, further reducing the credibility of a systematic treatment. Furthermore, in our analysis, the presence of a lawsuit is no determination that cities would transition in a timely manner, or that they would transition quickly enough to be classified as treatment cities in this analysis. For instance, of the 30 cities in our control, Shenkman's office targeted five, and at least Santa Clarita received pressure from other sources. Therefore, just because a city was targeted by a lawsuit during the period of study does not mean they are substantively different than other cities which would receive notices too late to be included in our treatment. Ultimately, this observation strengthens our analysis, which is based on the potential outcomes notion that our "control" group would respond the same as our "treatment" group, had the treatment of city switching been applied to our "control" group.

One example is the city of Orange which only voted to change to by-district for the 2020 election and was excluded from the treatment. Orange received a letter from Shenkman's office urging compliance with the CVRA in April 2017. After the city refused to transition, Shenkman initiated a lawsuit, and the Orange city council voted to transition to by-district elections in April of 2019 for their 2020 elections. Orange did not see any additional diversity on their city council during the study period.

Another is the city of Apple Valley, which Shenkman contacted in January, 2019. In February 2019, Apple Valley voted to begin shifting to a by-district system, but will not hold its first elections until 2020. Just as in the case of Orange, the Apple Valley city council showed no increased diversity during the study period.

While our empirical method that follows is designed to examine the causal effects of CVRA-induced election system switching in California, a note on statistical inference and generalization is of value. California is one of the most diverse states in the nation, and while Whites are relatively Democratic in cities like Los Angeles, San Francisco, San Jose, and Santa Cruz, Whites in many areas of the state (i.e., Orange County, Inland Empire, Central Valley, interior North) reflect similar outlooks and voting patterns as Whites in other states across the U.S. In local elections though, even in cities like Los Angeles, racially polarized voting is apparent (Barreto et al., 2019). In assessing racially polarized voting in Los Angeles, for instance, Abosch et al. (2007) show that percentage Hispanic at the precinct level correlates at over 0.7 with Latino/non-Latino support for Latino candidates. Given this, we expect the minority council representation effects induced by laws like the CVRA to transport to other states that have relatively diverse populations, like New York, Illinois, Arizona, Texas, and Florida. Less diverse states, like Washington State and Idaho, however, still have regions with large Latino populations, and so laws like the CVRA are likely to expand minority representation in diverse cities within these less diverse states.

Hypotheses

Based on the extant literature, we think the CVRA will enhance minority representation in the cities that shift their election districts from at-large to district. We therefore test the following hypothesis:

- $H0_1$ Relative to similarly situated California cities, cities that shifted from at-large to district-based elections will experience no more or no less city council minority representation.
- Ha_1 Relative to similarly situated California cities, cities that shifted from at-large to district-based elections will experience greater city council minority representation.

However, building upon the work of Trounstine and Valdini (2008), we expect most of CVRA's effects to occur in high-density Latino cities that switched from at-large to single member districts. The reasoning is simple. First, even if a 25% Hispanic city switches from at-large to by-district, the citizen voting age population will reduce the city's share of eligible Latino voters. Second, among eligible voters, Latinos tend to vote at lower rates than Whites or Blacks.[8] Third, upon redistricting, White interests may continue to control the creation of district maps, which can be used to crack the Latino and minority population across districts reducing the likelihood of a minority winning a seat on the council. Finally, White interests may choose to hold city elections in off-years and down months like February, March, or June. This may have the effect of further depressing minority turnout. Thus, cities need to have relatively large shares of Latinos specifically and minorities generally to realize the effects of the CVRA.

Therefore, we further hypothesize that minority representation gains will be realized more in locations where Latinos compose a higher share of the population versus in locations where Latinos compose a smaller share of a city's population.

- $H0_2$ Relative to cities with lower shares of Latinos, cities with higher shares of Latinos that shifted from at-large to district-based elections will experience no more or no less city council minority representation.
- Ha_2 Relative to cities with lower shares of Latinos, cities with higher shares of Latinos that shifted from at-large to district-based elections will experience greater city council minority representation.[9]

Data and Methods

To test our hypotheses, we gathered a list of all 476 cities in California from the California Secretary of State's office. Next, we added on city-level demographic and political variables that are incorporated into our matching design. We include variables into our match that might theoretically explain why cities switch or why they had hesitated to switch earlier. Because all of our "treated" cities transitioned from at-large to by-district after 2010, our demographic data are taken from the 2010 Census.[10] This includes: percent Black, percent Asian, percent Hispanic,[11] percent change in Latino population from 2000 to 2010, percent 4-year college education or higher, median household income, median age, and city population.[12] From the Secretary of State, we gathered city-level voter registration by party data for

2010.[13] From this, we generated two variables: percent Democrat and percent Republican. Thus, our unit of analysis is the city.

In addition, we sought to balance our treatment and control groups by adjusting our match for White public opinion towards racial minorities and immigrants in the cities in question. While city-level data of that sort is not available, we used county-level data from pooled Cooperative Congressional Election Studies (CCES). The CCES asked the racial resentment battery of questions in 2018, 2012, and 2010, and it asked a battery of questions measuring tolerance towards immigrants during 2018, 2016, 2014, 2012, 2010, and 2006. We combined each scale across years to ensure a suitable county-level sample size, thereby treating our measure as a latent dimension. The CCES sample size remained under n=50 for two cities located in the sparsely populated Madera and Siskiyou counties (i.e., the towns of Madera and Tulelake, respectively), so an average was found with each adjacent county included. This gave a reliably robust estimate of racial and immigration-related attitudes for each county which included a city in question.[14]

Next, we developed our treatment universe by determining which cities practice by-district city council elections, and importantly, when said cities adopted those practices. Cities that switched before the passage of the CVRA are excluded from our analysis because temporally we cannot attribute their switch to the CVRA, and obviously they cannot be included as a similarly situated city to match our treatment group against. Thirty (30) cities fit this criterion. In addition, cities that had switched to district after 2001 but had only partially completed the transition (i.e., each city council seat had to have gone up for election at the time of this analysis) are excluded from the analysis. This includes a total of 82 cities which have announced their intention to transition but have only done so partially by the 2018 election. Thus, our total of potential "control" units is n=336 cities.

Table 1 presents covariate balance statistics between the "treatment" group and all potential "control' cities, revealing particular differences between Percent Hispanic, Percent Republican, and Percent B.A. or higher. A match will therefore balance the two comparison groups enabling more precision in the estimation of the effect size of a city switch on minority council representation.

[INSERT TABLE 1 ABOUT HERE]

With our list of cities that practice either by-district or at-large voting, we determined when each by-district city transitioned from at-large. However, because some cities take more than one election cycle to complete the "treatment", the transition was only determined to have been completed when each member of the city council had been voted into office based on by-district election rules. Thus, we measure our dependent variable – percentage of White officials on the city council – prior to announcement that the city would make the shift, and then again once the city had elected all city council members under new district rules. For example, in July 2014, MALDEF came to an agreement with the city of Merced to include an initiative to transition to by-district on the 2014 ballot. After this passed, Merced drew a district map and held elections for half of the city council on a by-district basis in 2016 before holding elections for the second half in 2018. In this case, 2018 was deemed the year that the transition had fully taken place, while 2013 was considered the pre-treatment year (Merced also switched to even-number years in 2016). City council member demographics were then collected for 2013, before the election, and late 2018, after the 2018 election.

To generate our outcome variable, we compiled records of the members of each city council during the year when the switch was announced and immediately post-transition. This was done by contacting the city clerks of each city and acquiring lists of city council members, either in electronic form or read out over the phone. For each city council member, we gathered photos and names to determine race or ethnicity. To further validate council members' race, we compared each surname against the Census surname database.[15] However, one complication with this approach is that most cities elect the mayor from the city council or see the same individuals rotating regularly through the mayor's office and the council. Thus, we generally include mayors in our city council calculations. However, in some by-district city councils, the mayor remains elected at-large. In these cases, the mayor was excluded from our city council calculations, as there is not reason to expect a change for this at-large position. Finally, because cities have a different number of elected council members, our dependent variable is measured in percent as opposed to raw counts (e.g., percent of city council in 2010 that is White; versus percent of city council in 2016 that is White).

With the treated cities selected, we conducted a nearest neighbor match against all other cities in California fitting our outlined criteria, resulting in a dataset of n=60 (30 treated cities, 30 control cities) (Ho et al., 2007; Iacus et al., 2012). After the match, the same process was repeated for each of the at-large (control group) cities, documenting the racial characteristics of each matched city's council members pre and post their treated comparison city's switch. That is, we collected demographic data for each city council during the same years as their by-district match.

Table 2 lists our treatment and control cities. For each treated city, we present its most similar non-treated city based on the following matched criteria: percent Black (2010 Census), percent Asian (2010 Census), percent Hispanic (2010 Census), percent White non-Hispanic (2010 Census), percent Hispanic growth (2000 Census to 2010 Census), percent registered Democratic 2010, percent registered Republican 2010, percent decline to state/independent 2010, percent 4-year college or higher (aged 25, 2010 Census), median household income (2010 Census), median age (2010 Census), city population broken up into five size categories, White racial attitudes (CCES), and White attitudes towards immigration (CCES).[16] For example, Anaheim (treated) is matched with Ontario (control), and Tulare (treatment) is matched with Lancaster (control).

Table 2 reveals the percent of a city's population that is White non-Hispanic, the percentage of the city council that was White before the shift to district elections, and the percentage of the council that is White after the shift election. Finally, we include a change White column, which reports the change in the White share of representatives calculated based off of our council racial determination above. This variable serves as our dependent variable in our matched difference of means analysis.

[INSERT TABLE 2 ABOUT HERE]

Figure 1 maps out the cities included in our analysis. Black circles represent treated cities, whereas white squares with an x denote control cities. The map reveals that most of the treated and control cities are located in or around the Central Valley, or the broader Los Angeles region (Los Angeles, Orange, San Bernardino, and Riverside Counties).

[INSERT FIGURE 1 ABOUT HERE]

Figure 2 plots a time line of the number of treated cities by year switch completed. In 2011, Modesto became the first city to switch from at-large to district-based elections as a result of a CVRA-inspired switch. In 2014, Menifee, Madera, and Sanger also switched to single-member district elections. However, despite California enacting the law in 2001, most of the activity in city shifts occurred in 2018, with 20 cities electing city council members via district when previously they had elected members at-large. It is hard to know exactly why the effects of the CVRA have taken so long to come into effect, but clearly the bulk of the activity has occurred only in recent years.

[INSERT FIGURE 2 ABOUT HERE]

Finally, we employ two separate analytic techniques to estimate the average treatment effect of switching from at-large to district. These methods include, first, a post-match difference of means t-test. This method is a direct test of the null versus alternative hypotheses, where μ_1 and μ_2 (below) signify the mean percent White city council in the control and treatment groups, respectively:

$$Null : \mu_1 - \mu_2 = 0 \tag{1}$$

$$Alternative : \mu_1 - \mu_2 \neq 0 \tag{2}$$

This difference represents the average treatment effect for a city switching from at-large to single-member district under our matched design.[17]

Second, we conduct a post-match difference in difference (DiD) OLS regression (Henderson, 2018; Donald and Lang, 2007; Obermeyer et al., 2014). The DiD model maintains a variety of assumptions that must be met/assuaged to ensure proper identification of a treatment effect. We review several of these post-analysis to demonstrate the robustness of our findings. In the DiD setup, we stack our matched data into a panel such that each city appears in the data twice, estimate the following equation, and cluster standard errors by city:

$$Y = \beta_0 + \beta_1 \times [Time] + \beta_2 \times [Treatment] + \beta_3 \times [Time \times Treatment] + \epsilon, \tag{3}$$

where Y = the percent of a city council that is White; Time is a vector of 0s and 1s (0 indicating a city or its control before the switch, and 1 indicating a city or its control after the switch); and Treatment is also a vector of 0s and 1s (0 indicating control and 1 indicating treatment). For robustness, we also include a difference in difference model with covariate adjustments, which takes on a similar form as above but includes a matrix of X covariates:

$$Y = \beta_0 + \beta_1 \times [Time] + \beta_2 \times [Treatment] + \beta_3 \times [Time \times Treatment] + \beta_k \times X_k + \epsilon, \tag{4}$$

This design accounts for city-level demographic variation and unobserved variables by comparing each city against itself. We interact our time variable by our treatment indicator to estimate the causal effect of switching from at-large to single member districts on minority city council representation (Imbens and Wooldridge, 2009). The group coefficient (β_2) captures the combined effects of all unmeasured city-level covariates that systematically differ between the two time periods and that do not change between the pre and post time periods. Similarly, the coefficient on (β_1) contains the effects of unmeasured covariates that change between the pre and post time periods but affect the outcome variable in the same manner for both groups. This design relies on several assumptions, which we discuss in our results section.

To test hypothesis 2, we further subdivide our treated data and each city's corresponding "control" city into two subsets above and below the mean percent Hispanic in the treated data (mean = 41.65%). Unfortunately, subdividing the data further produces exceedingly small datasets, thus, we focus on above/below the mean. We place treated cities that have an Hispanic population below 41.65% into the "low Latino" subset, and cities with Latino population at 41.65% or higher into the "high Latino" subset.[18] We then replicate the analysis outlined above.

Results

Main Analysis

A cursory examination of Table 2 certainly suggests that treated cities affect representation to a greater degree than do control cities. However, to begin our analysis, we conduct a difference of means t-test. The mean percent change White city council in the control condition is -0.083, whereas the treatment percent change White is -0.191. The difference between the two is statistically significant ($\mu = 0.102, t = 2.25, df = 29, p - value = 0.032$).[19] Our initial treatment effect estimate is 10.2%, which provides support for hypothesis 1. We estimate, on average, that cities that underwent a shift from at-large to district based elections during our time period increased their minority representation by 10.2 percentage points. To translate this to treated city seat-share, we multiply .102 by the mean and inter-quartile city council seat range (mean = 4.93; 1st Q = 4; 3rd Q = 5). Thus, we estimate the average council seat share to change from 0.504 seats, with a IQR range of 0.409 seats to 0.511 seats. This finding is supportive of our alternative hypothesis.

However, with such a design, it is possible that control and treatment cities are not perfectly matched on appropriate demographics. Table 3 presents relevant match covariates with t-statistics and p-values for each covariate. The right-most column "P_Value" clearly reveals no statistically significant differences across treatment condition.[20] Nonetheless, there are some minor differences between the two groups. For example, Pct. Hispanic is a bit larger in the treatment group (41.65) than in the control group (39.54). Due to these qualitative imbalances, we also estimate the CVRA effects via multiple regression, which are presented in Tables A1- A3 in Appendix A. We include several alternative specifications, including controls for treatment election and number of city council seats. The models produce very similar treatment effect estimates as our difference of means estimator.

[INSERT TABLE 3 ABOUT HERE]

Our main analysis relies on a post-match OLS difference in difference regression. The treatment is indicative of a city that made the switch from at-large to district elections in response to the CVRA, and time becomes a pre (0)/post (1) variable. Here, we shift the outcome variable from a percent White difference measure to a stacked time 1 and time 2 arrangement such that each city appears in the data twice. To account for this stacking, we include a time variable where 0 = pre-shift time period, 1 = post-shift time period, and adjust for robust clustered standard errors. To ensure robustness we also estimate the model with additional time 1 controls. Table 4, Column 1, presents the main results. Consistent with our initial analysis, we estimate the CVRA-induced at-large to district election district shift effect at 10.2%. This effect is robust across a covariate adjustment model (Column 2).

[INSERT TABLE 4 ABOUT HERE]

Figure 3 plots out our difference in difference effects to aid interpretation. The points on the left (Pre Shift) reveal the percentage of the treated/controlled city councils that were White just before the treated cities changed to district-based elections. Seventy-nine percent (79%) of council representatives in pre-treated cities (black square with cross) were White, whereas 75% of control cities' representatives were White. After the switch, however, the very next election (or next two elections if the city rotates council seat election years) produced city councils where 60% of the representatives were White, a change of 19%. However, during the same time span, the non-treated cities' White representation dropped to 66.5%, an 8.8 point difference from the earlier period. Thus, assuming the same slope as the control if the treated cities had not undergone a shift in how they elect council members, we estimate their percent White on the city council would have been 70.3% (the gray square with a cross, Post Shift). We take the difference between this counter-factual outcome and our actual result to estimate CVRA's causal effect. Thus, with this method, we estimate the causal effect of CVRA-induced moves from at-large to district elections on minority representation gain at 10.2%.

[INSERT FIGURE 3 ABOUT HERE]

Difference in difference designs are tailored for non-random or potentially non-random treatment assignment (Angrist and Pischke, 2008; Morgan and Winship, 2015; Wing et al., 2018; Donald and Lang, 2007), but rely on the common trends assumption in its identification of a treatment effect. The primary assumption with the DiD design is the parallel trends assumption. That is, before entering the treatment time period, the trends in the outcome variable in the two groups should move up or down at a similar rate – in short, trends should be parallel going into the treatment period. To establish this, for each treatment city and its match, we gathered the racial characteristics of council members in the two elections preceding the pre-shift time period, which we call T-1 and T-2. Figure 4 presents our findings. From observation, we can easily see that the parallel trends assumption holds heading into the treatment period.

[INSERT FIGURE 4 ABOUT HERE]

Table C1 in Appendix C further validates the parallel trends assumption, by presenting results of two additional analyses. We test whether a positive and statistically significant interaction between time and "treatment" exist in the time-2 to time-1 period and the time -1 to pre-treatment period, respectively. We find no evidence of a statistical pre-treatment trend, as evidenced by small and statistically insignificant Treatment X Time coefficients.

Another concern with the common trends assumption is the possibility that group characteristics are not invariant with time or do not trend together (time varying but NOT group invariant). For instance, it might be possible that more racial minorities moved into treatment cities immediately after the switch to single member districts because they might now get more local-level representation. This is an unlikely possibility for two reasons: First, only two years separated our measurement of pre/post White council representation in 16 of our treated cities, with three at three years, and 11 at four years. The mean length of time between pre/post measurement is 2.5 years, which is not very long thereby we should not expect a lot of demographic movement across this time period.

Second, to provide added assurance to the group invariance assumption, we assessed whether the pre/post switch for Percent Hispanic and Latino Growth are group invariant (i.e. don't vary by group across the treatment time period). We took our city-level 2010 measures for Hispanic and Latino growth, respectively, and conducted a difference of means t-test between treatment/control as we showed in the balance table. For simplicity, we measure post switch shifts in 2018 for all cities. However, because we could not obtain city-level 5-year 2018 estimates from the American Community Survey (ACS), we gathered ACS county-level data and attached that to the city. We find no differences between the mean Latino growth or percent Hispanic, respectively, between treatment and control in either 2010 or 2018. This suggests group invariance on the demographics most likely to bias treatment effect measurement. [21]

Next, we collected party registration data for the 2018 election and conducted the same test. We find similar results. For instance, in 2010, Republican registration in treated cities was 37.65 and 39.44 for control cities – a difference of 1.76 percentage points. In 2018, Republican registration in treatment cities fell to 30.81%, but also in control cities to 32.97 for a difference of 2.16 percentage points. In both years, the relationship between party registration and treatment assignment is not statistically significant (2010: $t = 0.82674, df = 29, p - value = 0.4151$; 2018: $t = 0.86364, df = 29, p - value = 0.3949$). Thus, the two variables arguably most likely to drive changes in minority representation do not threaten the validity of our DiD methodology.

DiD designs strive to ensure strict exogeneity, which, in practice is difficult to fully establish (Wing et al., 2018). In the present case, we might observe a violation of strict exogeneity if our treatment variable is predicted by covariates one might plausibly think induces cities to switch from at-large to single-member districts. For instance, it seems plausible that cities within our treatment and control dataset with large Hispanic populations and high shares of Democrats might be more likely to switch to single member districts upon CVRA passage. We test this by regressing treatment on a host of covariates. Table C2 in Appendix C presents logistic regression results testing the strict exogeneity assumption. We find no evidence that covariates predict treatment, which strengthens the strict exogeneity requirement.

Finally, DiD analyses often include placebo tests to ensure the identification of the treatment. In effect, researchers aim to swap out the treatment group with a "fake" treatment group – a group that one knows was not affected by the treatment. In this case, one might, for instance, collect racial characteristics of non-treated California cities that did not enter into the matched control group. We opt for a more efficient and harder test. We randomly assigned our 30 treatment and 30 control cities to either the treatment or control condition before estimating a DiD OLS regression. We then simulated this process 5,000 times and stored the t-statistic on our Treatment X Time interaction term. Figure 5 shows that 93% of these simulations produced statistically insignificant Treatment X Time coefficients. Thus, when we simulate "fake" treatment groups comprised of a random combination of cities from our treatment and control, more than 90% of the time we do not estimate a statistically significant treatment effect. This is about what we would expect based on a random simulation process, and further strengthens our confidence in our design.

[INSERT FIGURE 5 ABOUT HERE]

Latino Subset Analysis

To evaluate hypothesis 2, we further subdivided our data into below/above the mean percent Hispanic among treated cities. There is good theoretical reason to anticipate heterogeneous treatment effects by percentage Latino. First, over the past two decades many cities in California have begun to approach 40%-50% more Latino population, but still lack commensurate city council representation, in part due to at-large districts. Cities like this that do switch may have large enough Latino populations – and hence electorates – to command at least one city council seat. Cities with smaller Latino populations may not have the voting base to ensure a city council seat, thus switching to at-large does not initially result in added minority representation.

To generate conditional average treatment effects for percent Hispanic, we subset the full dataset into two discrete datasets, where one dataset includes treatment cities above the mean percent Hispanic value and the other below the mean percent Hispanic value. For each subset, we include each city's respective "control" city.[22] We then conduct separate analyses on each dataset. To find evidence for our hypothesis we expect to observe minimal racial representation differences between treatment and control in the below mean percent Latino subset, but large effects in the above mean percent Latino subset.

We calculate a difference of means between treatment and control in the below mean percent Latino subset at 0.037 ($t = 0.568, df = 18, p = 0.576$). However, among the high percent Latino subset, we calculate a difference of means of 0.214 ($t = 3.099, df = 10, p < 0.05$). To place this into seat share context, we estimate racial minorities gaining 1.05 seats in cities that have on average 4.93 total seats. These results provide strong support for hypothesis 2.

Our difference in difference analysis reveals similar results. In Table 5, Column 1, the "Treatment X Time" variable shows no statistical and a small substantive treatment effect (3.8%, ns) among cities that are below the percent Hispanic mean. Table 5, Column 2, reveals clear support for a conditional average treatment effect among higher Latino-density cities. These results are consistent with our difference of means analysis – the "Treatment X Time" coefficient value is 0.214. This suggests that among high density Latino cities, a shift from at-large to single member district should increase the diversity of the city council by, on average 21.4%. Again, these findings provide strong support for hypothesis two.

[INSERT TABLE 5 ABOUT HERE]

Taken in total, these heterogeneous treatment effects make sense and are consistent with the extant literature (Lublin and Voss, 2000; Lublin et al., 2019). On the one hand, cities with fewer Latinos may have a hard time electing Latino representatives even after the city shifts to single-member districts. Given variation in Latino voter turnout (Barreto, 2010) and citizenship relative to the overall California population, and the fact that many cities hold local elections in off-years and not during general elections, even a 35% Hispanic city may not be able to elect a Latino to the city council after a switch from at-large to single member district. On the other hand, cities with more Latinos – say above 40% Hispanic – are more likely to have the minority population base to be able to contain a majority of voters in at least one or two (say, out of five) districts. Thus, these results suggest the real-world effects of laws like the California Voting Rights Act will most clearly manifest in high-minority cities that continue to elect city councils at-large.

Discussion

In this paper, we investigated whether the California Voting Rights Act (CVRA) has enhanced minority representation. This is an important question to investigate because other states, such as Oregon, are presently considering implementing similar policies, and Washington passed one in 2018. If such policies fail to produce desired outcomes, then states may shy away from potentially contentious debates about minority representation and continue status quo local politics. While a law like the CVRA makes it easier for minority groups to challenge the discriminatory practices of at-large election districts, the passage of a law does not necessarily mean that desired results will result.

However, our results suggest that states interested in equal racial representation might consider implementing a state voting rights law in a fashion similar to California. While policies designed to shift cities from at-large to district may not be enough to promote fully equitable racial representation, our findings suggest that these policies can shift representation in that direction to the tune of between 10-11 percentage points, and that this effect is primarily manifested in cities with larger shares of Latino residents (upwards of 20 percentage points). That is, in general, minorities gain seats when cities switch from at-large to district-based elections. Further, representation gains are more likely to be realized in cities with larger shares of Latinos and minorities, which is consistent with prior literature.

Our difference in difference analysis suggests that CVRA-induced at-large to district switches improves minority representation by 10%, or roughly half a city council seat. Among high-density Latino cities, we estimate 21.4% change in minority/White representation, equating to just over 1 full city council seat. While a democratic improvement, treated city councils were still 60% White post-switch. Whites are still over-represented in these locations by about 15 percentage points – based on their share of the population (which is 46% non-Hispanic White according to the 2010

Census). Thus, to achieve fairer representation still, one could make an argument that more institutional mechanisms are needed to bring about greater racial parity on city councils throughout California.

To our knowledge, just one other state, Washington State, has passed a similar state voting right act. Future research should investigate whether this state law has produced similar effects as those estimated here. However, Washington's law was only recently passed so we must wait a few election cycles before all cities will have had a chance to elect all city council members from districts.

Our study is not without its limitations. The CVRA was enacted in 2001, but, as Figure 2 demonstrates, it has taken a long time for cities to make the shift from at-large to district. Indeed, only recently (2018) have a sizable number of cities actually made the shift. At least thirty (30) cities are poised to make the shift in 2020. With more data, our estimates of minority representation effects may change. So scholars should continue to chart the impacts of state voting rights legislation employing similar methodologies. Indeed, while we took great pains to rule this out, it may be that those interested in electing more minorities to local government targeted the "easiest" cities first and so we may be over-estimating the average treatment effect here. Still, we maintain that our methodological approach effectively removes this argument, but future studies can answer this with even more data.

Further, it's possible that some the cities that switched to single member districts post-CVRA did so for orthogonal reasons. While we cannot rule this out completely because we do not know each city's individual decision-making, we find this argument unlikely because: 1) The CVRA gave activists and lawyers an easier path to winning legal challenges in the courts; 2) Many cities that demographically and politically look like city-switchers did not or have not yet switched; and 3) Many cities switched only after observing the high-profile CVRA court losses by the cities of Modesto and Palmdale.

While our research finds that the CVRA has led – on average – to greater minority representation among cities that have switched to district-based elections, we do not know whether this changed representation has led to qualitative policy changes. For instance, say a city council moves from five White, to three White, one Latino, and one percent of Asian descent. If policy revolves around majority vote outcomes, Whites can still control city-level policy output. Thus, future research could investigate whether these CVRA-induced reforms actually leads to long-run redistribution of power and resources within any given city.

Notes

[1]https://www.nytimes.com/elections/2012/results/states/california.html

[2]https://www.census.gov/quickfacts/kerncountycalifornia

[3]https://www.kerncounty.com/bos/

[4]https://migration.ucdavis.edu/mn/more.php?id=492

[5]https://www.nytimes.com/elections/2016/results/california

[6]While we cannot directly measure CVRA efforts in all cities, given that we know racially polarized voting (RPV) and council demographics are selection criteria, we conduct further analysis in Appendix D. Our concern is that because RPV is a selection criterion, we may see qualitatively higher RPV in treatment vs. control cities. We find clear evidence of RPV in both treatment and control.

[7]Shenkman's targeting did not ultimately produce a dataset where treated cities are geographically closer to Malibu than control cities. We tested this, finding that treated cities are a bit closer to Malibu (153.4 miles) than control cities (170.5 miles) but that the difference is not statistically significant (t=0.54, p=0.59).

[8]https://www.pewresearch.org/fact-tank/2019/05/01/historic-highs-in-2018-voter-turnout-extended-across-racial-and-ethnic-groups/

[9]We find the same effect for cities with higher shares of minorities overall.

[10]While we are unable to measure minority activism directly, we managed to secure the 2010 California voter file and estimate a percent Latino turnout measure by city. We then conducted a t-test between treatment and control post match, finding no statistically significant turnout differences by group: Control = 48.7% turnout, Treatment = 46.05% turnout, $t = 1.4403, df = 29, p - value = 0.1605$.

[11]Percent non-Hispanic White is the comparison.

[12]See Appendix B for a discussion of how we incorporate segregation into our analysis.

[13]https://elections.cdn.sos.ca.gov/ror/ror-pages/60day-gen-10/political-sub.pdf

[14]We could have employed ideology measures derived by Tausanovitch on the American Ideology Project, but these correlated heavily with our own measures, such as a -0.93 correlation with the percent Republican party registration and a 0.90 correlation with the percent Democratic party registration.

[15]https://www.census.gov/topics/population/genealogy/data/2010_surnames.html; https://www.census.gov/data/developers/data-sets/surnames.html

[16]We do not include a measure of segregation in our match due to the number of cities below 40,000 people in both the treatment and potential control cities. We do, however, include segregation as a control variable in our post-match regression analysis, and our difference in difference analysis. Our substantive results remain unchanged. See Appendix B for details about how we generated our segregation measure and conducted our analysis.

[17]$ATE = E[y_1(i) - y_0(i)]$, where 1 signifies the treatment group outcome variable and 0 the control group outcome variable.

[18]We conducted the same analysis for percent minority/non-White, with the median percent minority cut-off at 54.75%. The results are very similar as those found with the heterogeneous treatment effect among Latinos.

[19]A one-tailed test produces the same result, but with more statistical confidence: $t = 2.25, df = 29, p - value = 0.016$.

[20]We further conducted a joint hypothesis coefficient test with the null hypothesis that all balance test coefficients are equal to 0 and alternative hypothesis that NOT all coefficients are equal to 0. We find support for the null, $F = 0.497, p = 0.871$, which corroborates our balance test findings.

[21]Latino change between 2010-2018 $\mu = 0.0054, p = 0.802$. Percent Hispanic between 2010-2018: $\mu = -0.007, p = 0.3772$

[22]For both subsets, joint hypothesis tests reveal covariate balance across the treatment and control groups. Below the mean: $F = 0.682, p = 0.732$. Above the mean: $F = 0.482, p = 0.869$.

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Figures

Figure 1: CA Treatment (black circle) and control (white square with x).

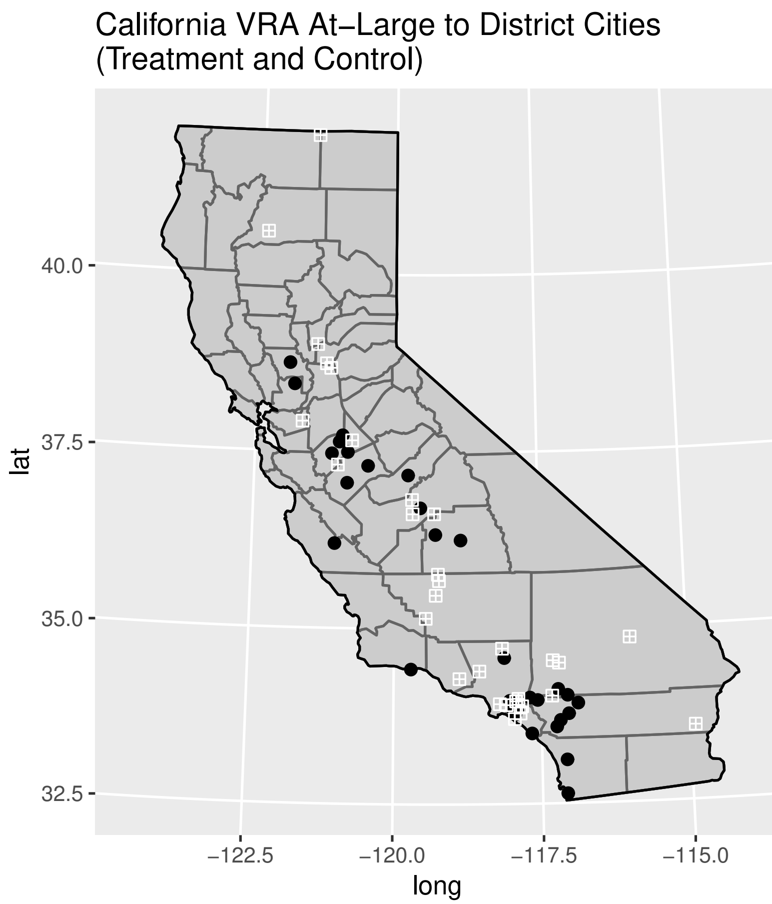


Figure 2: CA Treated cities by year. The number of cities that shifted from at-large to district jumped dramatically in 2018.

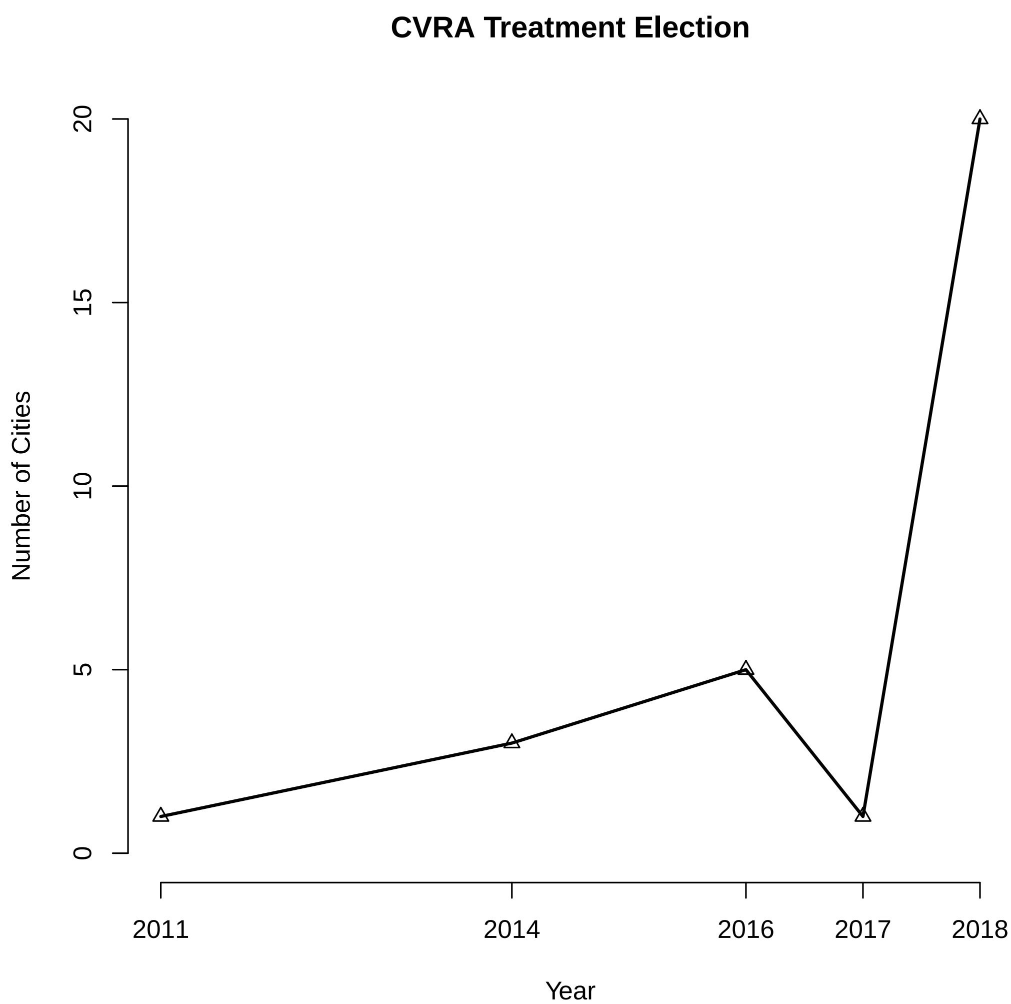


Figure 3: Difference in Difference coefficient plot revealing pre-post difference in percentage White on the city council.

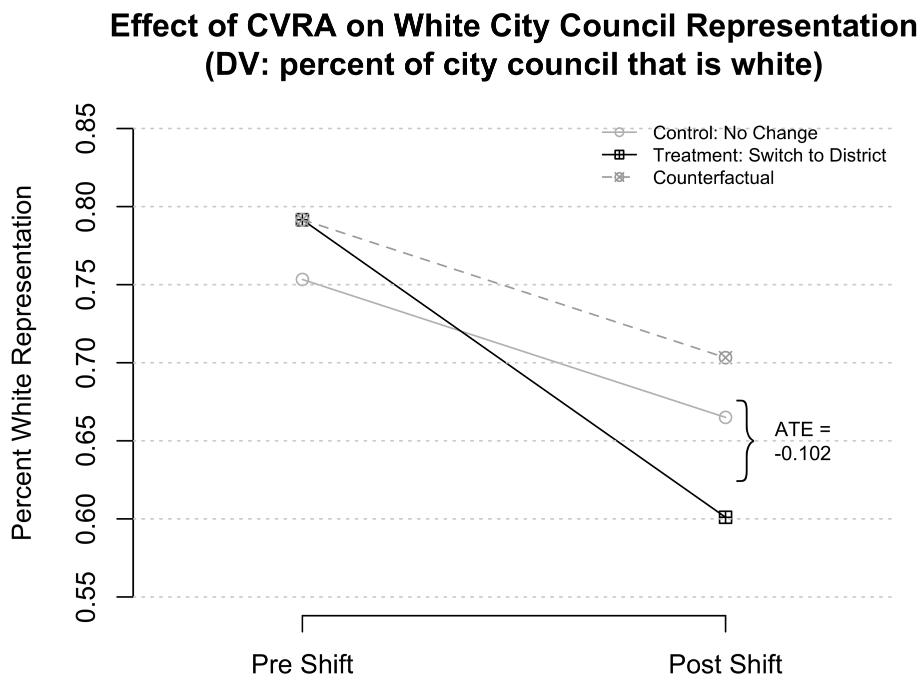


Figure 4: Parallel trends assumption test. Points T-2 and T-1 represent city council racial representation in the preceding two elections prior to each city's pre-treatment election.

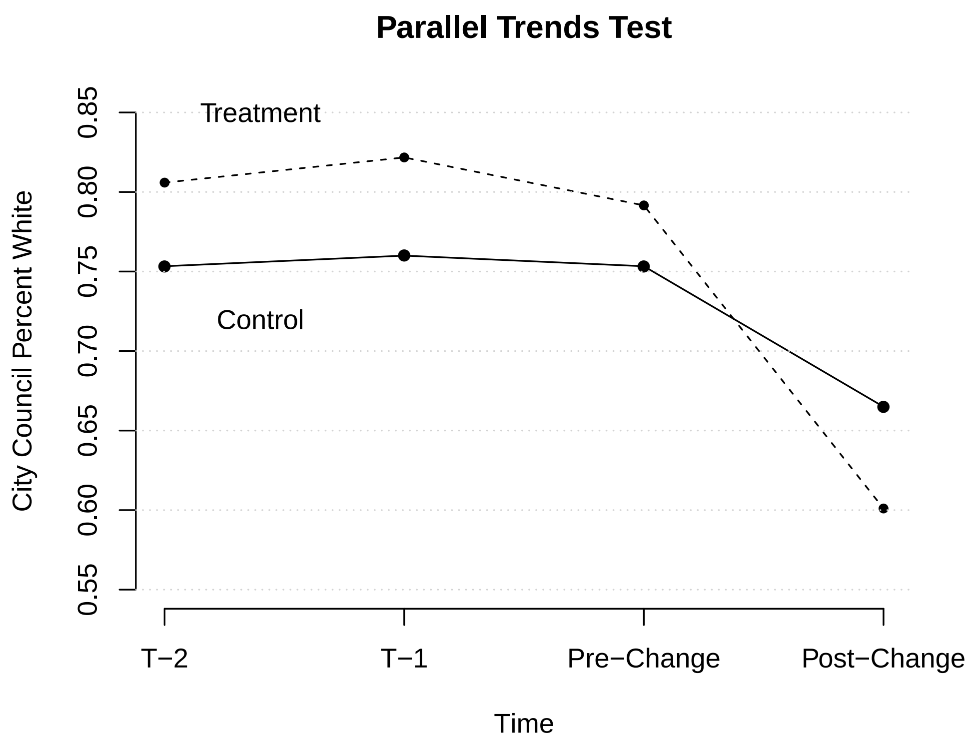
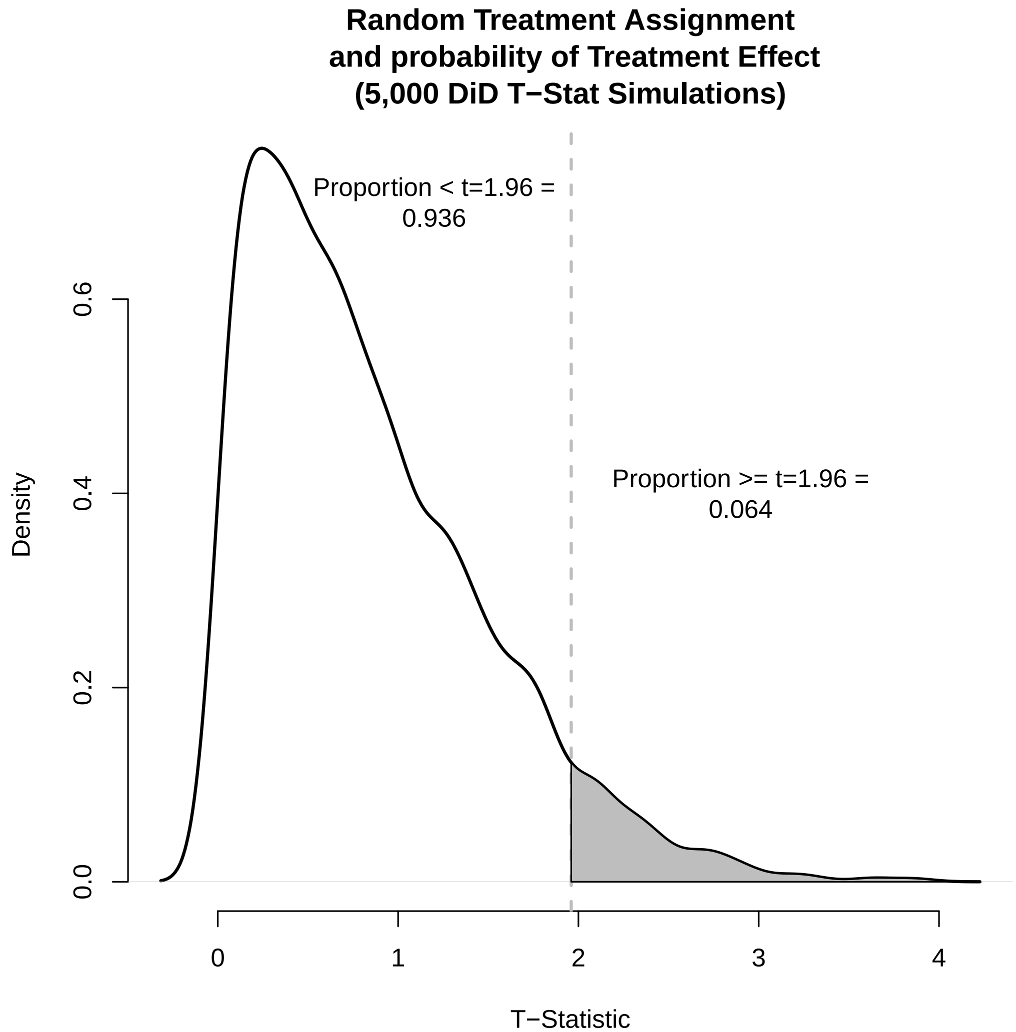


Figure 5: T-Statistic Random Assignment Simulations



Tables

Table 1: Balance table pre match, city-level covariates

	Means Treated	Means Control	SD Control	Mean Diff
Distance	0.34	0.06	0.10	0.29
% Black	3.89	3.38	5.31	0.51
% Asian	6.37	8.51	11.31	-2.14
% Hispanic	41.65	28.86	26.67	12.78
% Non-Hispanic White	45.25	55.96	27.56	-10.71
% Democratic	40.62	44.41	12.02	-3.79
% Republican	37.66	30.96	12.31	6.70
% Independent	21.72	24.64	4.50	-2.91
Pro-Immigrant Opinion	0.48	0.52	0.06	-0.04
Racial Resentment	0.39	0.42	0.07	-0.03
Percent Latino Change	0.22	0.23	0.13	-0.01
% BA or Higher	19.72	31.89	21.63	-12.16
Median Income	56352.37	69655.24	38477.40	-13302.88
Median Age	34.26	39.13	7.91	-4.87
City Size	2.53	1.65	0.91	0.88

Table 2: Treatment and Control cities, demographies, and representation pre and post at-large to district shift

	City, St	Treatment	Treatment Election	White Non-Hisp	Pre-White %	Post White %	Change White	Match
1	Anaheim, CA	1	2018	35.90	1.00	0.67	-0.33	Ontario
2	Banning, CA	1	2018	52.40	1.00	0.60	-0.40	Moorpark
3	Buena Park, CA	1	2018	38.20	1.00	0.80	-0.20	Blythe
4	Chino, CA	1	2018	37.60	1.00	0.50	-0.50	Tulelake
5	Chula Vista, CA	1	2018	31.70	0.40	0.40	0.00	Wheatland
6	Dixon, CA	1	2018	57.90	0.60	0.40	-0.20	Newman
7	EastVale, CA	1	2018	19.30	1.00	0.80	-0.20	Clovis
8	Escondido, CA	1	2016	51.90	0.40	0.50	0.10	Industry
9	Garden Grove, CA	1	2018	32.50	0.40	0.50	0.10	Santa Clarita
10	Hemet, CA	1	2018	70.30	0.80	1.00	0.20	Roseville
11	Highland, CA	1	2016	41.70	0.80	0.60	-0.20	Fowler
12	King City, CA	1	2018	17.10	0.80	0.60	-0.20	Delano
13	Los Banos, CA	1	2018	39.80	0.40	0.25	-0.15	Folsom
14	Madera, CA	1	2014	25.10	1.00	0.33	-0.67	Apple Valley
15	Menifee, CA	1	2014	48.70	0.80	1.00	0.20	Redding
16	Merced, CA	1	2018	37.80	0.83	0.50	-0.33	LaHabra
17	Modesto, CA	1	2011	59.60	0.86	0.71	-0.14	Shafter
18	Palmdale, CA	1	2016	41.00	1.00	0.50	-0.50	Norwalk
19	Patterson, CA	1	2018	36.10	0.60	0.50	-0.10	Grand Terrace
20	Riverbank, CA	1	2018	48.10	0.80	0.50	-0.30	Orange Cove
21	Sanger, CA	1	2014	15.90	0.00	0.00	0.00	McFarland
22	San Juan Capistrano, CA	1	2018	62.30	1.00	0.80	-0.20	Lynwood
23	Santa Barbara, CA	1	2017	58.30	0.86	0.67	-0.19	Brentwood
24	Tulare, CA	1	2016	43.80	1.00	0.40	-0.60	Lancaster
25	Turlock, CA	1	2016	60.40	0.80	0.80	0.00	Victorville
26	Visalia, CA	1	2018	54.90	1.00	1.00	0.00	Orange
27	Whittier, CA	1	2018	37.60	0.80	0.50	-0.30	Taft
28	Wildomar, CA	1	2018	71.90	1.00	1.00	0.00	Fountain Valley
29	Woodland, CA	1	2018	53.00	1.00	0.40	-0.60	Waterford
30	Yucaipa, CA	1	2018	76.70	0.80	0.80	0.00	Yorba Linda
31	Apple Valley, CA	0	2014	67.70	0.80	0.80	0.00	
32	Blythe, CA	0	2018	42.00	0.80	0.60	-0.20	
33	Brentwood, CA	0	2017	63.10	1.00	0.80	-0.20	
34	Clovis, CA	0	2018	67.50	0.80	0.60	-0.20	
35	Delano, CA	0	2018	9.20	0.00	0.00	0.00	
36	Folsom, CA	0	2018	74.20	1.00	1.00	0.00	
37	Fountain Valley, CA	0	2018	58.50	0.80	0.80	0.00	
38	Fowler, CA	0	2016	23.80	0.60	0.20	-0.40	
39	Grand Terrace, CA	0	2018	60.80	0.80	0.80	0.00	
40	Industry, CA	0	2016	26.90	1.00	0.80	-0.20	
41	LaHabra, CA	0	2018	41.40	0.60	0.40	-0.20	
42	Lancaster, CA	0	2016	52.40	0.80	0.60	-0.20	
43	Lynwood, CA	0	2018	2.90	0.00	0.00	0.00	
44	McFarland, CA	0	2014	10.20	0.80	0.40	-0.40	
45	Moorpark, CA	0	2018	62.40	1.00	1.00	0.00	
46	Newman, CA	0	2018	42.10	1.00	0.80	-0.20	
47	Norwalk, CA	0	2016	18.90	0.40	0.40	0.00	
48	Ontario, CA	0	2018	26.60	1.00	0.80	-0.20	
49	Orange, CA	0	2018	54.60	0.80	0.75	-0.05	
50	Orange Cove, CA	0	2018	6.80	0.00	0.00	0.00	
51	Redding, CA	0	2014	85.70	1.00	1.00	0.00	
52	Roseville, CA	0	2018	79.80	0.80	1.00	0.20	
53	Santa Clarita, CA	0	2018	69.30	1.00	0.80	-0.20	
54	Shafter, CA	0	2011	29.00	0.80	0.80	0.00	
55	Taft, CA	0	2018	79.10	1.00	1.00	0.00	
56	Tulelake, CA	0	2018	51.60	1.00	1.00	0.00	
57	Victorville, CA	0	2016	47.50	0.60	0.60	0.00	
58	Waterford, CA	0	2018	60.00	0.80	0.80	0.00	
59	Wheatland, CA	0	2018	66.10	1.00	1.00	0.00	
60	Yorba Linda, CA	0	2018	74.80	0.60	0.40	-0.20	

Table 3: Balance Table, city-level covariates

	Control	Treatment	Abs_Diff	T_Stat	P_Value
% Black	3.97	3.89	0.08	0.08	0.94
% Asian	5.23	6.37	1.14	-0.68	0.50
% Hispanic	39.57	41.65	2.08	-0.39	0.70
% Non-Hispanic White	48.50	45.25	3.25	0.62	0.54
% Democratic	39.47	40.62	1.15	-0.41	0.68
% Republican	39.42	37.66	1.77	0.68	0.50
% Independent	21.11	21.72	0.61	-0.77	0.44
% BA or Higher	20.58	19.72	0.86	0.31	0.76
Median Income	59312.53	56352.37	2960.17	0.61	0.54
Median Age	34.58	34.26	0.32	0.29	0.77
City Size	2.23	2.53	0.30	-1.00	0.32
Immigration Attitudes	0.48	0.48	0.00	0.17	0.87
Racial Attitudes	0.38	0.39	0.01	-0.79	0.43
% Latino Change	0.23	0.22	0.01	0.29	0.77

Table 4: Difference in difference regression estimating causal relationship between CVRA cities (treatment) and Minority city council representation. (Robust clustered standard errors)

	% White Change	
	Base Model	**Covariate Model**
	Model 1	**Model 2**
Treatment	0.038	0.065
	(0.070)	(0.053)
Time	-0.088^{***}	-0.088^{***}
	(0.024)	(0.024)
Treatment X Time	-0.102^{**}	-0.102^{**}
	(0.048)	(0.048)
% Black		-0.001
		(0.006)
% Asian		-0.004
		(0.005)
% Hispanic		-0.003
		(0.003)
% Democratic		-0.007
		(0.012)
% Republican		0.005
		(0.011)
Immigration Public Opinion		0.369
		(0.733)
Racial Resentment Public Opinion		0.828
		(0.876)
Percent Latino Change		0.834^{***}
		(0.300)
% BA or Higher		0.001
		(0.003)
Median Income		0.00000
		(0.00000)
Median Age		-0.011
		(0.009)
City Size		0.010
		(0.031)
Constant	0.753^{***}	0.545
	(0.054)	(1.065)
N	120	120
R-squared	0.069	0.529
Adj. R-squared	0.045	0.461
Chi-square	8.639^{**} (df = 3)	90.319^{***} (df = 15)

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table 5: Difference in difference regression estimating causal relationship between CVRA cities (treatment) and Minority city council representation. Below/Above Mean Percent Hispanic Subset. (Robust clustered standard errors)

	Pct. White Change	
	Below Mean Hispanic	Above Mean Hispanic
	Model 1	**Model 2**
Treatment	0.081	-0.036
	(0.071)	(0.144)
Time	-0.097^{***}	-0.073^{*}
	(0.031)	(0.039)
Treatment X Time	-0.038	-0.214^{***}
	(0.058)	(0.076)
Constant	0.758^{***}	0.745^{***}
	(0.057)	(0.109)
N	76	44
R-squared	0.079	0.141
Adj. R-squared	0.040	0.076
Chi-square (df = 3)	6.240	6.670^{*}

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Appendix A

Due to slight imbalances in the matched treatment and control groups, we conduct a post-match ordinary least squares regression analysis with the following form:

$$Y = \beta_0 + \beta_1 \times [Treatment] + \beta_k \times X_k + \epsilon, \quad (5)$$

where Y = the change in percent of the city council that is White, Treatment is a vector of 0s and 1s (0 indicating control and 1 indicating treatment), and X_k a matrix of control variables.

Table A1 estimates the treatment effect – shifting from at-large to district-based elections – on minority city council representation, with covariate controls. Tables A2 estimates a model with "year switched" fixed-effects. Table A3 estimates a model with a covariate for number of city council seats.

Table A1: Effect of CVRA on Percent White Change in City Council

	% White Change
Treatment	-0.117^{**}
	(0.049)
% Black	-0.013
	(0.008)
% Asian	0.006
	(0.005)
% Hispanic	0.0004
	(0.003)
% Democratic	-0.005
	(0.011)
% Republican	-0.003
	(0.011)
Immigration Public Opinion	1.668
	(1.024)
Racial Resentment Public Opinion	-0.668
	(1.100)
Percent Latino Change	0.487
	(0.330)
% BA or Higher	-0.004
	(0.005)
Median Income	-0.00000
	(0.00000)
Median Age	0.010
	(0.011)
City Size	0.043
	(0.030)
Constant	-0.670
	(1.065)
N	60
R-squared	0.328
Adj. R-squared	0.139

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table A2: Effect of CVRA on Percent White Change in City Council (Treatment Election Fixed Effect)

	% White Change
Treatment	-0.117^{**}
	(0.051)
% Black	-0.013
	(0.009)
% Asian	0.006
	(0.005)
% Hispanic	0.0002
	(0.003)
% Democratic	-0.006
	(0.012)
% Republican	-0.005
	(0.012)
Immigration Public Opinion	1.608
	(1.143)
Racial Resentment Public Opinion	-0.588
	(1.200)
Percent Latino Change	0.486
	(0.367)
% BA or Higher	-0.003
	(0.006)
Median Income	-0.00000
	(0.00000)
Median Age	0.010
	(0.012)
City Size	0.040
	(0.034)
Treatment Election = 2014	-0.023
	(0.170)
Treatment Election = 2016	-0.032
	(0.167)
Treatment Election = 2017	-0.085
	(0.242)
Treatment Election = 2018	-0.024
	(0.156)
Constant	-0.562
	(1.212)
N	60
R-squared	0.331
Adj. R-squared	0.060

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table A3: Effect of CVRA on Percent White Change in City Council (Number of Seats on Council)

	% White Change
Treatment	-0.126^{**}
	(0.049)
% Black	-0.015^{*}
	(0.008)
% Asian	0.008^{*}
	(0.005)
% Hispanic	-0.001
	(0.003)
% Democratic	-0.002
	(0.011)
% Republican	-0.002
	(0.010)
Immigration Public Opinion	1.288
	(1.041)
Racial Resentment Public Opinion	-0.415
	(1.098)
Percent Latino Change	0.528
	(0.326)
% BA or Higher	-0.002
	(0.005)
Median Income	-0.00000
	(0.00000)
Median Age	0.009
	(0.011)
City Size	0.046
	(0.030)
Number of Seats	-0.075
	(0.049)
Constant	-0.270
	(1.083)
N	60
R-squared	0.361
Adj. R-squared	0.162

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Appendix B

H-Index Scale Measure of Segregation

We did not include a segregation measure in our initial match because city-level segregation proves less reliable with smaller cities. For instance, fully eight of our treated cities had fewer than five Census tracts. Further, 209 of of the 476 cities in our city-level dataset have fewer than five Census tracts. By excluding all of these cities from the initial match we would be biasing our match and therefore interpretation of results in favor of larger cities.

Nonetheless, to begin to assess whether segregation effects our outcome measure, we calculated a measure of segregation for each city in our treatment and control that had five or more Census tracts, post-match. The treated cities excluded from this analysis are: Yucaipa, Wildomar, Sanger, Patterson, Los Banos, King City, East Vale, and Dixon. The control cities excluded from this analysis are: Wheatland, Waterford, Tulelake, Taft, Shafter, Orange Cove, Newman, McFarland, Grand Terrace, and Fowler.

The measure of segregation used is called the H-index, originally developed by Henri Theil. This is an important measure to take into account because highly segregated cities may result in greater minority representation post switch due to an ability to generate majority-minority districts.

The H-index measures how much racial diversity in each city neighborhood varies relative to the total city. For each city in California with five or more Census tracts (roughly 40-45,000 people), we collected tract-level data on racial characteristics. These data were then fed into the following algorithm using the HTheil function in R (Tivadar, 2014). The resulting output is a city-level diversity/segregation index for White/non-White measure of segregation (to match our outcome variable), which we then add on to our original city-level data. We also include White (Anglo)/Hispanic. The equation below represents the H-Index:

$$H = \sum_{n=1}^{N} \frac{P_n}{P_c} \left(\frac{E_c - E_n}{E_c} \right), \tag{6}$$

where P is the population of neighborhood n or city c, and E is the entropy of said neighborhood or city. Theil's entropy score is thus defined:

$$E = \sum_{r=1}^{R} (\pi_r) ln \frac{1}{\pi_r}, \tag{7}$$

where π_r is the population proportion of each racial group r within a geographical unit.

The H-index ranges from 0-1 where 0 equals very diverse (neighborhoods look like the city as a whole) and 1 very segregated (neighborhoods are completely racially homogeneous). Our matched treatment and control White/non-White H-index ranges from 0.0094 - 0.1616, $\mu = 0.072$, $sd = 0.0396$, whereas the Anglo/Hispanic H-index ranges from 0.0132 - 0.2149 ($mu = 0.092$, $sd = 0.05$)

Analysis including H-Index

With these data, we regressed all original independent variables, plus the new segregation measure onto change in city council treatment.[23] We employ both White/non-White and Anglo/Hispanic in separate models. In these segregation-augmented models (Columns 1 and 3 in Table B1), we find that our treatment covariate is negative and statistically significant, whereas our segregation measure (H-Theil) is not statistically significant. These results further confirm the original presented results that the CVRA produced a statistically and substantively significant increase in minority representation on local city councils – even when taking segregation into account.

Further, we specified two models – presented in Columns 2 and 4 – that interact segregation by treatment, to test the hypothesis that city council minority representation is more likely to occur in highly segregated cities that switch from at-large to single member district. The lack of statistical significance on the product terms in both models suggest that city-level segregation on its own may play a limited role in the redistricting process. While this is worthy of further research, it may be that even when more segregated cities change, those who are charged with drawing districts are able to do so in a way that potentially cracks highly segregated areas.

Table B1: Effect of CVRA on Percent White Change in City Council (Segregation Index)

	% White Change			
	H-Index Base	**H-Index Int.**	**H-Index Hispanic Base**	**H-Index Hispanic Int.**
	Model 1	**Model 2**	**Model 3**	**Model 4**
Treatment	-0.193^{***}	-0.126	-0.193^{***}	-0.105
	(0.069)	(0.137)	(0.068)	(0.134)
% Black	-0.003	-0.002	-0.003	-0.0001
	(0.010)	(0.010)	(0.010)	(0.011)
% Asian	0.008	0.008	0.008	0.008
	(0.006)	(0.006)	(0.006)	(0.006)
% Hispanic	0.004	0.004	0.004	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
% Democratic	-0.027	-0.027	-0.027	-0.028
	(0.017)	(0.018)	(0.017)	(0.017)
% Republican	-0.020	-0.020	-0.020	-0.021
	(0.016)	(0.016)	(0.016)	(0.016)
Immigration Public Opinion	3.141^{**}	2.883^{*}	3.124^{**}	2.861^{*}
	(1.433)	(1.521)	(1.426)	(1.478)
Racial Resentment Public Opinion	-2.107	-2.043	-2.062	-2.095
	(1.598)	(1.622)	(1.599)	(1.612)
Percent Latino Change	0.578	0.549	0.592	0.561
	(0.465)	(0.474)	(0.467)	(0.473)
% BA or Higher	0.003	0.003	0.003	0.004
	(0.007)	(0.007)	(0.007)	(0.007)
Median Income	-0.00001	-0.00001	-0.00001	-0.00001^{*}
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Median Age	0.015	0.015	0.014	0.016
	(0.013)	(0.014)	(0.013)	(0.013)
City Size	0.057	0.060	0.056	0.062
	(0.039)	(0.040)	(0.039)	(0.040)
H-Theil White-Non-White Index	0.290	0.899		
	(0.829)	(1.360)		
Treatment X H-Theil White-Non-White Index		-1.010		
		(1.773)		
H-Theil White/Anglo-Hispanic Index			0.240	0.725
			(0.622)	(0.892)
Treatment X H-Theil White/Anglo-Hispanic Index				-0.983
				(1.288)
Constant	0.354	0.397	0.344	0.443
	(1.595)	(1.617)	(1.594)	(1.612)
N	42	42	42	42
R-squared	0.517	0.523	0.518	0.528
Adj. R-squared	0.267	0.248	0.268	0.256

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Appendix C

Table C1: Difference in difference parallel trends assumption testing changes in percent white on city council in two pre-treatment time periods. (Robust clustered standard errors).

	Pct. White Change T-1 to Pre	**Pct. White Change T-2 to T-1**
	Model 1	**Model 2**
Treatment	0.062	0.053
	(0.065)	(0.068)
Time	−0.007	0.007
	(0.015)	(0.026)
Treatment X Time	−0.023	0.009
	(0.025)	(0.036)
Constant	0.760***	0.753***
	(0.048)	(0.055)
N	119	118
R-squared	0.011	0.013
Adj. R-squared	−0.015	−0.013
Chi-square (df = 3)	1.278	1.520

***p < .01; **p < .05; *p < .1

Table C2: Predicting treatment from observable covariates.

	% White Change
% Black	−0.073
	(0.092)
% Asian	0.028
	(0.055)
% Hispanic	0.001
	(0.033)
% Democratic	−0.064
	(0.128)
% Republican	−0.075
	(0.123)
Immigration Public Opinion	−11.820
	(12.107)
Racial Resentment Public Opinion	17.954
	(12.973)
Percent Latino Change	2.242
	(3.778)
% BA or Higher	−0.0002
	(0.061)
Median Income	−0.00002
	(0.00003)
Median Age	−0.019
	(0.121)
City Size	0.284
	(0.349)
Constant	5.044
	(12.317)
N	60
Log Likelihood	−38.687
AIC	103.374

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Appendix D

In deciding which cities to target with letters, our research found that lawyers weigh the presence of racially polarized voting (RPV) in deciding whether to threaten a city with a lawsuit. Given the data constraints regarding the statistical analysis of RPV, it is not possible to estimate racially polarized voting for every single treatment and control city in our data – thus we did not include estimtaes of city RPV in our initial match.

However, to begin to test this potential confounder, we subset our data to cities that, along with their match, have more than n=40 precincts,[24] and estimate RPV using ecological inference techniques available in the R package, eiCompare (Barreto et al., 2019; Collingwood et al., 2016). From the California statewide database,[25] we gathered precinct-level candidate vote data, and cross-walked this with racial demographic data at the block level from the 2010 U.S. Census.

To estimate the presence of RPV, we focus on the 2012 presidential election, which featured a Black versus White candidate – a Black candidate with broad minority support at the national level.[26] Focusing on the Obama-Romney 2012 election is practical and relatively comparable across cities. The alternative is gathering city-level election data that may feature non-comparable candidates across cities, making ecological inference cross-treatment group more challenging to interpret. Due to statistical limitations, and the fact that Latinos are the largest minority group in each of our cities, we focus on Latino vs. non-Latino in estimating the presence of RPV, where non-Latino is primarily comprised of White voters.

Our goal is to assess whether RPV exists in both treatment and control cities. If, for instance, RPV exists in treatment cities but not control, our treatment effect estimates may be a result of Latinos in control cities simply continuing to prefer White candidates (or White-backed). Here, we seek to rule out this possible confounder.

Table D1 presents mean RPV estimates across the treatment and control cities analyzed. The table shows the presence of RPV in both city groups. Cities in the treatment group reveal clear racially polarized voting, with Latinos favoring Obama 78% to 22%, a difference of 56%. Non-Latinos (White) in the treatment favor Romney 76% - 24% – a difference of 52%. In the control, the numbers are similar, with Latinos favoring Obama 77%-23%, a difference of 54%. Non-Latinos (Whites) favor Romney 65% - 35%, a difference of 30%. While RPV appears a bit higher in treatment cities, analysts would conclude clear RPV in both treatment and control cities.

Other facts, such as demographic considerations, that lawyers used to determine city selection were either controlled for in the match or addressed elsewhere. One fact mentioned by Kevin Shenkman's office was the diversity of the city council at the time. We performed a t-test to compare the percent of White members on the city council during the treatment year. This proved statistically insignificant (p=0.595), with the control having an average of 0.75 percent and the treatment an average of 0.79.

Table D1: Racially Polarized Voting (RPV) means between treatment and control cities with cities (and their match) over population size n=40 precincts

	Treatment		Control	
	Non Latino	Latino	Non Latino	Latino
Obama	23.63	78.12	34.93	76.93
Romney	76.35	21.88	65.04	23.07