

At-Large Elections and Minority Representation in Local Government

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Abstract: *Despite a long history of legal challenges alleging that elections conducted at-large suppress minority representation, this remains the dominant electoral system in local governments throughout the United States. Moreover, a large empirical literature remains divided over the present-day impact of at-large elections on the political success of underrepresented groups. We reconcile the competing findings in this literature by providing contingent, causal estimates of the effect of conversion from at-large to ward elections on minority officeholding, using a novel identification strategy afforded by the California Voting Rights Act of 2001. We find a dramatic positive effect of conversion in districts where Latinos constitute a sufficiently large share of the voting population, and in large and residentially segregated districts. When these conditions are not satisfied, we consistently see null estimated effects.*

Verification Materials: The materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/0MXT0H>.

Free and fair elections are the very essence of modern democracy. The institutions that structure how this exercise of popular will is translated into political representation are no less important. In the United States, electoral rules governing who can vote and how votes are aggregated to legislative seats have been used to exclude women and ethnic minorities from full political participation. By the same token, the past half century has seen a number of attempts at reforming the rules of the game to correct these historical inequities.

One prominent example has been the push to eliminate at-large voting in local elections across the country. In at-large elections, voters across an entire constituency have the opportunity to select candidates for every available seat in its legislative body. This is in contrast to the classic majoritarian scheme that divides the constituency

into wards, each having its own seat in the legislature.¹ In at-large systems, its opponents claim, the minority vote is diluted by majority interests voting as a bloc. If minorities are highly concentrated in particular regions, as they have been historically, then switching to ward representation can at least guarantee them seats wherever they constitute a local majority, thereby increasing their voice in the political process.

If these claims are accurate, then the elimination of at-large voting would have profound, far-reaching consequences for political representation across the United States. At-large systems are still the prevailing institution in American local elections: As of 2012, approximately 64% of U.S. cities relied exclusively on at-large voting for their city council elections, with another 21% employing some combination of at-large and ward systems (Clark

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This work would not have been possible without assistance from the Spencer Foundation “via a Lyle Spencer Research Award (201600072)” that allowed us to collect these data for the Education Governance and Accountability Project (EGAP) at The Ohio State University. Special thanks go to Vlad Kogan, Stéphane Lavertu, and Zachary Peskowitz at EGAP, and to Nolan McCarty, Kosuke Imai, and members of the Imai Research Group at Princeton University for their invaluable feedback on this project. Additional thanks go to participants of the 2017 CSAP American Politics Conference at Yale University. We are also grateful to Robert Rubin for many informative conversations.

¹This representational scheme is known by many different names, including *district*, *by-trustee*, and *single-member*. For the sake of clarity, we will use the term *ward* throughout and reserve *district* to refer to the entire political unit—in our empirical case, the school district—whether it has at-large elections or is further subdivided into wards.

American Journal of Political Science, Vol. 00, No. 00, xxxx 2020, Pp. 1–17

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DOI: 10.1111/ajps.12512

and Krebs 2012).² Governing bodies elected at-large—city councils, school boards, and municipal boards—make decisions about how education is funded, where roads are built, and how water and sanitation services are delivered; they determine housing, economic development, transportation, and urban planning policies that shape their constituents' daily lives.

Recognizing the importance of local electoral institutions, coalitions of minority groups and civil rights advocates have mobilized to take legal action against at-large voting districts, resulting in several historical waves of conversion to wards (Browning, Marshall, and Tabb 1986). Most recently, the California Voting Rights Act (CVRA) of 2001 lowered the legal standard of victory for plaintiffs suing at-large districts for suppressing minority representation. The result was a flurry of successful litigation across the state to break up at-large school board and city council districts into wards, amassing millions of dollars in legal fees and settlements, and more than tripling the incidence of ward-based systems in California over the 15 years following the CVRA's passage (California Common Cause 2017). But these institutional reform efforts have also generated significant controversy—even among supporters of their general goals. Ward elections, critics argue, breed parochialism, fragmentation, and *less* responsiveness to the interests of the larger constituency; moreover, reforms have not been nearly as effective as expected at increasing the number of Latino representatives elected to office.³ Meanwhile, costly lawsuits deplete already overstretched municipal budgets, hurting the very constituents whom the law was meant to empower.

At least one of the focal points of this debate—the effectiveness of conversion to ward representation in propelling minority candidates to office—is an empirical question that a careful study of electoral institutions and outcomes ought to resolve. Unfortunately, a vast research agenda attempting to measure the relative merits of the two systems for minority officeholding has been unable to produce conclusive results. Although there is substantial evidence that ward elections for local office result in greater representation for Black and Latino voters (Davidson and Grofman 1994; Davidson and Korbel 1981; Engstrom and McDonald 1981; Karnig and Welch 1982; Leal, Martinez-Ebers, and Meier 2004; Marschall,

Ruhil, and Shah 2010; Meier et al. 2005; Molina Jr. and Meier 2018; Moncrief and Thompson 1992; Polinard 1994; Robinson and England 1981; Stewart, England, and Meier 1989; Trounstine and Valdini 2008), a number of additional studies have found no effect (Bullock and MacManus 1987; Cole 1974; Fraga 2009; MacManus 1978; Welch 1990), with still others that posit a *negative* association between ward elections and minority electoral success (Meier and Rutherford 2014; Welch and Karnig 1978).

Two factors explain the lack of scholarly consensus on this subject: the highly contingent nature of the effect of electoral reform, and the extreme difficulty of recovering plausibly causal estimates of this effect. Ward elections can only help minority candidates win office when the minority group is sufficiently large and geographically concentrated, such that it constitutes a local majority. Identifying the cases where these important prerequisites are in place is beyond the scope of most data sets that have been available to date, but it is essential for a proper test of the reform.⁴ Moreover, the vast majority of studies have attempted to identify the effects of at-large versus ward representation by comparing localities with each kind of system or, at best, those that have switched to ward systems to those that have not. But even after controlling for any number of covariates, crucial unobserved differences remain between areas with long histories under each rule, and those that choose to switch to wards are fundamentally different from those that do not. The result is a selection effect that can bias the estimated quantity of interest in either direction.

As the political debate in California intensifies—and additional states such as Texas appear poised to follow California's lead in undertaking reform⁵—questions about the relative merits of at-large and ward systems for the interests of historically underrepresented groups take on a new urgency. The contribution of the present study is to provide the first causal, contingent, and policy-relevant estimates of the effect of conversion from at-large to ward representation on minority officeholding. To estimate these effects, we apply a novel research design that takes advantage of exogenous variation in electoral reform induced by the CVRA, as well as an original panel data set that allows us to measure key predictors of the reform's success.

²These figures are estimates that come from surveys conducted by the International City/County Management Association. Though imperfect, and subject to survey measurement error, these estimates are the best measure we have of the frequency of local at-large elections.

³See, for instance, <https://www.latimes.com/politics/la-pol-ca-voting-rights-minorities-california-20170409-story.html>.

⁴Some notable exceptions that consider the contingent effects of institutional arrangements include Marschall, Ruhil, and Shah (2010) and Trounstine and Valdini (2008).

⁵See <https://www.texastribune.org/2018/12/11/richardson-isd-school-board-representation/>.

Our findings shed light on why the academic literature has for so long failed to arrive at a consensus—and on the conditions that advocates who hope to achieve real, lasting change need to target in their efforts. Consistent with theoretical expectations, we find a dramatic positive effect of being forced to convert to ward elections under the CVRA on Latinos' ability to get elected to California school boards among districts with a high level of residential segregation. Our analysis also uncovers a key moderator of the effect of reform that previous studies of at-large elections have ignored, though scholars have long recognized its importance for political mobilization: the size of districts, as measured by total school enrollment. In large and segregated districts, the effects of electoral reform are generally positive and steadily increasing in the size of the Latino community—a difference that rises above one additional Latino officeholder for every three available seats—but may also be negative when the minority community is sufficiently small. When these conditions are not met, we consistently see null estimated effects. Additionally, we examine districts that voluntarily chose to convert from at-large to ward elections. Through the use of an instrumental variables framework, we show that conversions spurred by district-adjacent legal threats have a large and unconditional positive impact on the political fortunes of the Latino community.

Theory

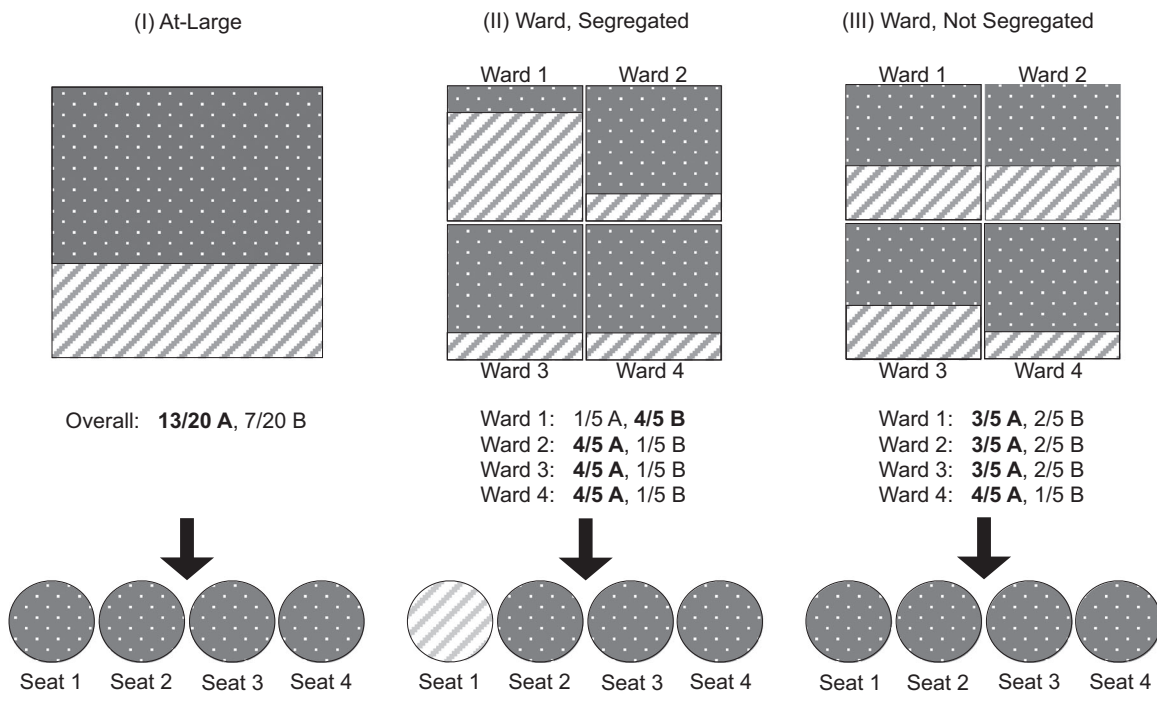
Scholars of local and urban politics have deservedly devoted significant attention to the differential effects of at-large and ward elections on officeholding among blacks, Latinos, and women.⁶ The justification for their focus on descriptive representation—broadly conceived as the resemblance between the representative and the represented along some politically salient dimension, such as sex or ethnicity (Pitkin 1967)—is twofold. First, descriptive representation is valuable in its own right, as it has been shown to increase underrepresented groups' sense of political efficacy, trust in government, and legitimacy of the governing regime, and to provide role models for those groups (Dovi 2002; Mansbridge 1999; Phillips 1991, 1995, 1998). Second, descriptive representation may lead to more tangible gains, such as an increase in the share of public resources allocated to a given group, or an improvement in the quality of life of its members (Browning, Marshall, and Tabb 2003; Fraga, Meier, and England 1986;

Haider-Markel, Joslyn, and Kniss 2000; Leal, Martinez-Ebers, and Meier 2004; Marschall and Ruhil 2007; Meier, Stewart, and England 1991; Wald, Button, and Rienzo 1996).

How might ward elections increase descriptive representation of minorities compared to at-large systems? A key condition for this relationship to hold is that the voting population be segregated enough for the minority group to constitute a local majority in at least one ward, and that the political boundaries be drawn accordingly (Marschall, Ruhil, and Shah 2010; Sass 2000; Trounstein and Valdin 2008). To understand why this is the case, consider the unit depicted in Figure 1. The diagram illustrates how votes in an electorate (above) are translated to seats in a governing body (below) under at-large and ward systems. In each of the three cases shown, the polity consists of two groups with opposing political interests: Majority Group A, which comprises 13/20 of the voting population, and Minority Group B, which comprises the remaining 7/20. In the first case, there is an at-large system: Every member of the electorate gets to vote on every seat in the legislature as it becomes available. As long as Group A can field a candidate for every race, it will be victorious every time, leaving the sizable minority without any political representation. Moving to a ward system, as Cases II and III show, may improve electoral outcomes for Group B—*conditional* on how the members of this group are distributed. In Case II, the electorate is divided into four wards, each with its own seat in the legislature. Because the boundaries are drawn such that the minority population constitutes a local majority in Ward 1, Group B is able to capture one seat and increase its voice in the legislature. But as Case III shows, geographic segregation is a necessary condition for ward representation to yield electoral gains for this group. Here, Group B is distributed approximately equally throughout the wards and remains a minority in each one. As a result, the problem present in the at-large system is replicated one level down, and wards yield no representational gains for the minority.

Another key determinant of whether ward systems can improve electoral outcomes for underrepresented groups is district size—though there are compelling reasons to believe the effect may swing in either direction. On the one hand, the likelihood of finding willing and qualified candidates to run for office increases with district size, and those candidates may be able to take advantage of greater resources and more sophisticated political organization. On the other hand, smaller districts may advantage political outsiders who have less money and experience, as they can garner support from their local communities through face-to-face contact rather than

⁶See Table 1 in Marschall, Ruhil, and Shah (2010) for a useful review of the vast empirical literature on black representation alone.

FIGURE 1 Conversion of Votes to Seats, Wards vs. At-Large Districts

Note: Group A is represented in dark gray, and Group B is represented in light gray. Squares represent the geographical distribution of the voting population; circles represent seats on the legislative council. Proportion of the electorate comprising Groups A and B is the same in each case.

large-scale campaigns. And while scholars have gone so far as to caution against extrapolating from findings on large units to smaller ones (Welch 1990), no further empirical work, to our knowledge, has focused on this important contingency.

For policy makers weighing the benefits of conversion from at-large districts to wards, process also matters. In the United States, reform has generally occurred in one of two ways: Localities have been forced to change their systems as the result of successful litigation against them, or they have chosen to do so voluntarily, either by popular vote or a unilateral decision of the governing body. In the following section, we discuss the historical context of American local electoral reform, culminating in the adoption of California's own Voting Rights Act. The CVRA led to a dramatic rise in both kinds of conversions: those resulting from legal action (or the threat thereof), which began to favor plaintiffs after the law's passage, and those undertaken voluntarily as the issue gained salience around the state. In both cases, the law's implementation and the way in which reform unfolded introduced some random variation in the likelihood that districts would change their systems, and we discuss how we exploit this in our empirical analysis.

Causal Identification through the CVRA

Historical Context

Since the passage of the federal Voting Rights Act of 1965, at-large districts around the country have come under legal attack on the grounds of minority vote dilution (see Browning, Marshall, and Tabb 1986), but not all plaintiffs have been successful in federal court. The Supreme Court ruling in *Thornburg v. Gingles* in 1986 clarified the standards that a claimant must meet in order to demonstrate that at-large elections are responsible for a failure of representation, setting a high bar for plaintiffs alleging vote dilution in at-large elections (Epstein and O'Halloran 1999; Trounstein and Valdin 2008). Many suits filed under the federal Voting Rights Act since then have been unsuccessful as a result. Then, in 2001, California passed its own Voting Rights Act, eliminating the *Gingles* requirements and imposing a much lower standard on plaintiffs: To win in court, they would only have to demonstrate the presence of "racially polarized voting" in the district. Moreover, unlike in cases filed under federal law, the CVRA required the district being sued to pay all legal fees, even if the two parties chose to settle out of court.

Civil rights groups across California quickly recognized a powerful tool in the CVRA. Under this new law, suing an at-large district for minority vote dilution had relatively little downside compared to federal cases: Most of the time, the prospect of paying exorbitant legal fees convinced districts to convert at the mere threat of legal action, and if a case went all the way to trial, the low “racially polarized voting” standard almost ensured victory for the plaintiff. The result was a mobilization of efforts around the state to initiate legal action in as many at-large districts as possible where there was a mismatch between the size of the Latino population and its representation in local government, a movement described as no less than “a quiet revolution” with the potential to transform “the literal face of California politics” (Fleming 2013). In 2002, the vast majority of California’s school districts had at-large elections (906 of 978 districts in our sample); by 2017, 138 of these districts had switched to ward systems, either voluntarily or under direct threat of legal action.

For a complete picture of how the CVRA transformed California’s electoral landscape, we estimate the effects of two distinct treatments on Latino officeholding across the state: first, the direct effect of conversion to ward representation on districts threatened with litigation, and second, the indirect effect on districts that were incentivized to convert of their own accord by the new legal standard. Taken together, these estimands do more than measure the differences between at-large and ward systems, which is where the vast majority of the academic literature stops; rather, they illuminate the *change* in minority representation we can expect from precisely the sort of mechanism that policy makers would use to induce institutional reform.

The Effect of Conversion by Legal Threat: A Fixed Effects Approach

The constraint upon the number of CVRA cases that civil rights groups could file was not the availability of at-large districts where a case could succeed, but rather the willingness of potential plaintiffs to engage in legal action. Civil rights groups operating at the state or national level could not initiate suits themselves, but had to do so on behalf of residents of the jurisdiction that they were taking to court. Through a series of interviews with the lawyers centrally involved in the cases tried under the CVRA, we learned about the process driving the conversion of school districts to ward elections in California. First, they identified all of the at-large districts across the state where there was a sizable Latino population (as a general rule

of thumb, at least 15%) and a misalignment between the size of the Latino population and representation. Next, they arranged meetings with local organizations within these districts to educate citizens about at-large voting. They would ask whether community members felt that minority interests could be better represented in local government, and whether they thought their districts would benefit from ward representation; invariably, the answer was yes. They also hired statistical consultants to assess whether there was evidence of “racial polarization” in the selected districts, but given the low standard set by the CVRA and the demographic realities on the ground, this step did not eliminate many candidates. The most significant drop-off from initial identification to ultimate litigation occurred at the level of plaintiff recruitment: Although the lawyers had no difficulty demonstrating the value of legal action to the communities they approached, they struggled to convince specific individuals to shoulder the burden themselves.

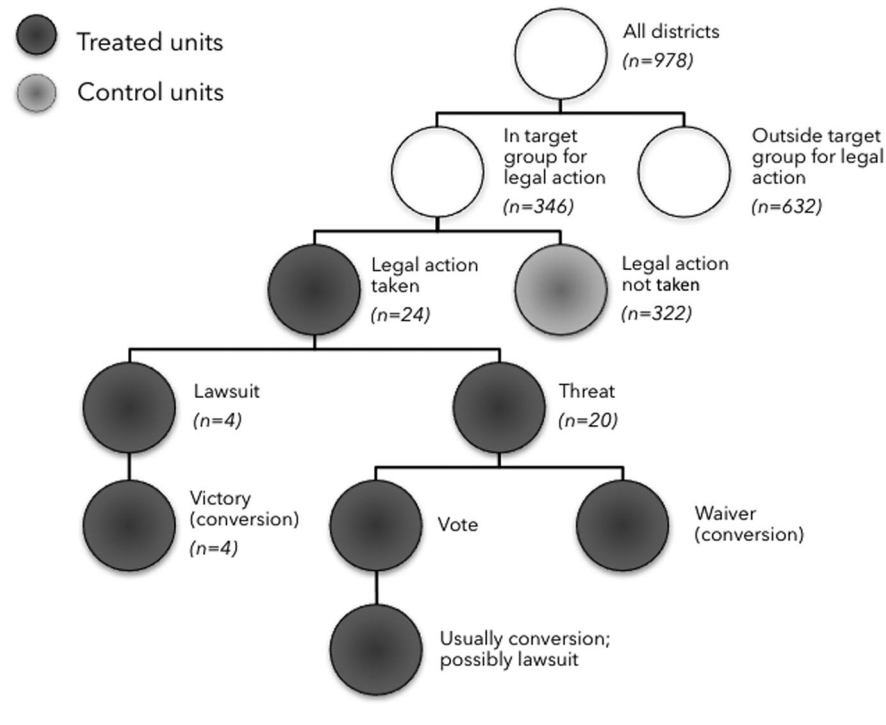
Importantly, the ability of civil rights lawyers to identify plaintiffs in targeted areas, with limited time and resources at their disposal, was not systematically related to the key political characteristics of those districts. To be sure, the *people* who stepped forward as plaintiffs do not represent a random sample of citizens. As Robert Rubin, a lawyer with the Lawyers’ Committee on Civil Rights (LCCR) who spearheaded CVRA litigation, stated, “you’re asking a member of a disenfranchised community to fight the establishment,” so the plaintiff would often be a retiree or someone not working for the city. However, conditional on being identified for legal action, the *communities* from which these plaintiffs were drawn closely resembled those where a plaintiff could not be found, and they varied widely in “political sophistication”: Whereas some were highly organized, others were unprepared for a legal victory and could not field a competitive candidate after winning their case.⁷

Figure 2 summarizes all of the pathways to conversion as a result of legal action, and it helps clarify how we define the treatment and control groups in our fixed effects analysis. A total of 346 at-large districts met the LCCR’s criteria for identifying potential litigants. The criteria were simple: The district’s population had to be at least 15% Latino, the voting-eligible population had to be less than 60% Latino,⁸ and the school board had to

⁷Conversation with Robert Rubin.

⁸If the voting-eligible population was greater than 60% Latino, Latinos would be the majority bloc and therefore hurt by conversion to ward districts. We follow the LCCR’s protocol in choosing a 60% threshold rather than a bare 50% majority in order to adjust for low turnout within this group. This does not dramatically change the sample, and the results are not sensitive to this choice.

FIGURE 2 Process of Legally Mandated Conversion from At-Large Districts to Wards under the CVRA



be composed of proportionally fewer Latinos than the district's population. The remaining 632 districts in the state were outside of their target group for one of three reasons: They already had ward elections by the time of the CVRA's passage, they did not satisfy the demographic and political selection criteria described above, or they voluntarily converted without the lawyers' intervention. Within the target group, four suits were successfully filed, with one going to court and three others settling, all in favor of the plaintiff. In an additional 20 cases, districts received a threat letter stating that they would soon be sued if they did not promptly change their systems. The threat was credible: Organizations only sent such a letter if they had in fact identified claimants from the community who were prepared to move forward if necessary (see Appendix A.1 in the supporting information [SI] for an example of such a letter).

Regardless of whether a school board was merely threatened with legal action or taken all the way to court, the final outcome was the same: The district converted to ward elections, either by vote or by obtaining a waiver from the state permitting them to convert by fiat. These cases, shown in dark gray in Figure 2 (and listed in full SI in Appendix A.4), constitute the treatment group in our analysis. The control group, shown in light gray, includes all of the districts that would have followed the same

deterministic trajectory if not for the short-term inability to find a plaintiff. Within this sample of potentially treated districts, we estimate the two-way fixed effects regression model:

$$Y_{it} = \beta_0 + \beta_1 \text{proportionLatino}_{it} + \beta_2 \text{ward}_{it} + \beta_3 (\text{proportionLatino} * \text{ward})_{it} + \mathbf{X}_{it} \gamma + \eta_i + \rho_t + \varepsilon_{it}, \quad (1)$$

where the outcome Y_{it} is the proportion of seats up for election in school district i and election year t won by Latino candidates; $\text{proportionLatino}_{it}$ is the proportion of the over-18 population of the district that is both Latino and eligible to vote (a native-born or naturalized U.S. citizen); ward_{it} is a binary indicator for having ward elections; \mathbf{X}_{it} is a vector of financial, demographic, and socioeconomic controls (described more fully in the Data and Measurement section); and η_i and ρ_t are district and year fixed effects, respectively.

The primary quantity of interest is the marginal effect of conversion estimated at a given level of Latino population, or $\beta_2 + \beta_3 * \text{proportionLatino}$, because if the reform did indeed improve minority electoral outcomes, then the Latino composition of school boards should rise with the size of the Latino population in the district. This is the modeling approach recommended by Engstrom

and McDonald (1981) and used in other recent empirical studies on the subject (e.g., Meier and Rutherford 2014; Trounstein and Valdini 2008).

Our estimates recover the causal effects of conversion from at-large to ward representation. The fixed effects account for any time-invariant differences between treated and control districts that could bias the results. The assumption for identification is that there are also no unobserved time-variant sources of selection into treatment. We are confident that this is the case. The two sets of districts have statistically indistinguishable pretreatment electoral outcomes *even before applying any controls*. However, to minimize the potential for confounding and to increase the precision of our estimates, we additionally control for a wide range of district-level demographic, financial, and socioeconomic characteristics, enumerated in the Data and Measurement section below.

The Effect of Voluntary Conversion: An Instrumental Variables Approach

Most of the CVRA's effect was not through lawsuits. Fearing repercussions, most places that converted did so of their own accord. One factor that made districts more likely to convert, particularly early on, was the presence of legal action in the same county. According to one media report, for instance, a lawsuit in nearby Madera convinced a school superintendent in Fresno County to mandate reform across all the districts under his jurisdiction. Moreover, he faced no political opposition. "I've had no chafing on the part of anybody," he said. "They said, 'It's the right thing to do. Let's do it'" (Landsberg (2009)). Litigation nearby, it seems, served as an exogenous shock to some districts that were amenable to minority incorporation but unaware of the potential problems with at-large representation or lacking in political will to enact reform; alternatively, they were fearful of the costs of litigation but previously unaware of the legal threat posed by the CVRA. In this analysis, we use legal action in another district in the same county as a binary instrument for the treatment of voluntary conversion to ward representation. We prefer a binary instrument of same-county membership to a continuous distance measure because CVRA cases are initiated in the county courts, and because the county is the next administrative unit above school districts in local educational governance; thus, we would expect information about legal action to diffuse over county-level networks rather than uniformly over geographical distance.

For the instrumental variables (IV) analysis to identify a causal effect of voluntary conversion, the exclusion restriction must be satisfied: Nearby legal action must

only affect Latino representation in a school district by increasing its propensity to convert to ward elections, and not by any other means. This rules out, for example, the possibility that nearby legal action mobilizes Latinos to push for greater representation under their current at-large system through candidate financing or voter turnout. We do not think this is likely to be the case. The rhetoric around the CVRA cast at-large elections as a first-order barrier to minority political access, and lawyers were actively seeking plaintiffs to take part in legal action; it is difficult to imagine these activities mobilizing community activism for something other than electoral reform. Nevertheless, any such spillover effects should bias our results downward, as they would reduce the contrast between electoral outcomes in at-large and ward districts.

Data and Measurement

Treatment

We constructed a comprehensive data set tracking the process of conversion from at-large to ward representation that was set in motion by the CVRA. To do so, we conducted an extensive search of state and local media accounts, school board minutes, and publicly available records of waiver requests from the California Department of Education. For every school district in California, we documented which electoral system was in place in every year from the CVRA's passage in 2001 through 2016, as well as how every switch occurred: by specific type of legal action (court ruling, settlement, or threat letter) or voluntarily. We also used data that we obtained from the Lawyers' Committee on Civil Rights (LCCR), the organization centrally involved in CVRA litigation, to reconstruct the sample it targeted for legal action according to its own stated criteria. This data set contains three key variables: the total number of seats on each district's school board, the number of those seats occupied by someone with a Latino last name, and the proportion of the district's population that was Latino. We used these variables to define a subgroup of districts where there was a sizable Latino population (over 15%) that exceeded the proportion of Latino representatives on the school board, just as the LCCR had done when they identified potential sites for legal action.

Outcomes

Through the Education Governance and Accountability Project at The Ohio State University, we have obtained the

names and vote counts of every candidate who ran for a school board position in California from 2001 to 2016. We aggregate these observations to construct our primary outcome as the number of school board candidates with Latino last names who won office as a proportion of the number of seats in the district up for election in that year.⁹

To unpack the mechanism by which more Latinos may have won office, we also construct two secondary outcome variables: one capturing the availability of Latino candidates, and another capturing their share of the vote. To measure the former, we compute the proportion of seats up for election in a given district-year that had at least one Latino candidate on the ballot. Constructing the outcome in this way tells us how many school board seats Latino candidates could have possibly won, given how many actually ran for office. For example, if two Latino candidates ran in an at-large election in which three seats were vacant, no more than two-thirds of the school district's seats could be filled by Latinos. If there were two different ward elections in a school district in a given year, and at least one Latino ran in one of them but not the other, no more than one-half of the district's school board seats could be filled by Latinos. Our other secondary outcome of interest is simply the vote share received by all candidates with Latino last names in each election, averaged across all races within a district-year.

Residential Segregation and Additional Controls

We measure residential segregation using the index of dissimilarity between whites and Latinos, computed at the school district level. The dissimilarity index captures how

⁹Our election data only give us names, not ethnicities, of candidates, so we identified Latinos using the `wru` package in R (Imai and Khanna 2017). This package employs a Bayesian prediction procedure that uses data from the U.S. Census to compute the probabilities that a person is of a given ethnicity, given his or her last name and geolocation at the county level. There is a valid concern that surname alone may fail to accurately reflect one's heritage, for instance, as a result of someone taking their partner's last name in mixed-ethnicity marriages, but for the present purposes, we do not believe this poses an issue. As Imai and Khanna (2016) point out, their identification method is biased only if the individual's surname is correlated with her location or personal attributes, including the rate of interracial marriage and the likelihood of changing her last name after marriage. So long as Latina women are no more or less likely to marry non-Latino men than non-Latina women are to marry Latino men, and so long as Latina women are no more or less likely to change their surname than non-Latina women after entering into a marriage with someone of a different ethnicity, the phenomenon of an individual changing their last name after marriage should only introduce random noise—but no bias—into our estimates.

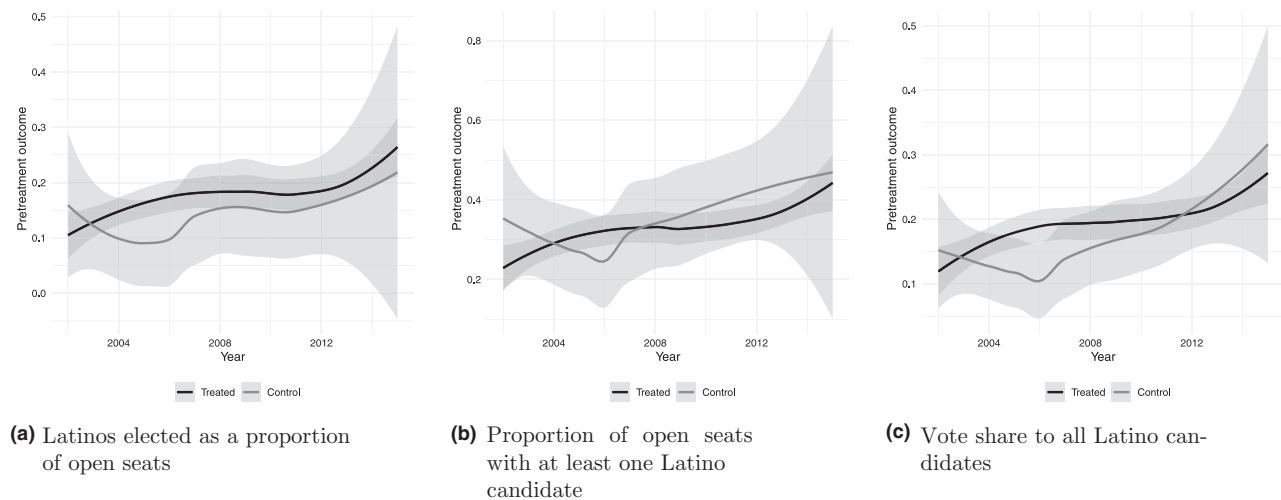
evenly whites and Latinos are distributed across schools within a district, and it is given by

$$\frac{1}{2} \sum_{i=1}^N \left| \frac{w_i}{W} - \frac{l_i}{L} \right|, \quad (2)$$

where w_i and l_i represent the number of whites and Latinos in school i , respectively; W and L represent the total number of whites and Latinos in the district, respectively; and N represents the total number of schools in the district. Another intuitive interpretation of this measure is the proportion of Latinos who would have to move to a different school in order for the composition of each school to be identical to the composition of the district as a whole (Ananat 2011). In general, a low dissimilarity index is considered to be below 0.3; 0.3 to 0.6 is considered moderate; and above 0.6 is considered high (Massey and Denton 1993). We choose to measure segregation with the dissimilarity index in keeping with a large literature that has favored its use (Ananat 2011; Collins and Margo 2000; Cutler and Glaeser 1997; Cutler, Glaeser, and Vigdor 1999; Massey and Denton 1993), but we supplement our analyses with the Theil index, an alternative measure of residential segregation, collected at the school district level by the Stanford Education Data Archive (SEDA).¹⁰

Finally, we use a vector of controls assembled from the U.S. Census and the California Department of Education. We select time-varying characteristics that are important correlates of Latino political participation and vote choice, including the proportion of students who are black, white, and Asian; financial characteristics, including property taxes collected, total current spending on instruction, and total educational revenues and expenditures, all scaled by enrollment, as well as enrollment itself; and socioeconomic factors, including median income in the district among all residents and specifically among Latinos, the proportion of students who receive free lunch and English-language learner (ELL) services, the proportion of the district living below the poverty line as well as between 100 and 149% of the poverty line, the proportion of Latinos who have less than a high school education, who have completed high school, and who have attended some college, the unemployment rate among Latinos, and the proportion of Spanish speakers who speak English “very well” as opposed to “less than very well.” Finally, we include a control for the total number of school board members since the representational consequences of winning a seat will vary with overall board size.

¹⁰Within our sample, the Theil index and the dissimilarity index are correlated at 0.79.

FIGURE 3 Comparing Pretreatment Electoral Outcomes within Sample Targeted for Legal Action

Note: Loess-smoothed lines are fitted through the data, and 95% confidence intervals are shown in gray.

Results

Validating the Selection of Control Units

We empirically verify our qualitative evidence that within the targeted subgroup, districts treated with legal action were analogous in prior Latino electoral performance to those where a plaintiff did not step forward. In other words, we do not see any evidence that lawyers strategically pursued litigation in districts where Latinos were more politically organized or successful or, conversely, where they were particularly underrepresented. Figure 3 shows that, for each of our three electoral outcomes, the treatment group—defined as any district that underwent conversion to ward elections through legal action at any point during the time series—is statistically indistinguishable from the control group—defined as all districts in the targeted sample that never underwent conversion over the same period. Here, we only include pretreatment data: As soon as a district converts to ward elections, it exits the sample.

Table 1 summarizes our sample of districts, contrasting the main variables of interest across the three categories of treatment status: those districts that remained at-large throughout our entire sample period and never converted to ward elections, those that converted after experiencing some sort of legal action, and those that converted voluntarily without any sort of legal coercion. For the latter two classes of school districts, the summary statistics were calculated using years before the treatment occurred.

Fixed Effects Analysis

There is no overall effect of conversion to ward districts on Latino officeholding (see column 1 of Table 2). But this result masks important heterogeneities: Namely, the reform leads to a closer alignment between the size of the Latino population and its descriptive representation in *large* and *segregated* districts. To construct these subgroups, we first compute the mean enrollment and dissimilarity index for each school district over the time series. We then define the threshold for inclusion in the “high” group on each condition based on the median value of each moderator among treated units.

Consistent with theoretical expectations, the degree of Latino–white segregation is a key moderator of the effect of conversion to ward districts on Latino officeholding. In Figure 4, we plot the marginal effects of conversion ($\beta_1 + \beta_2 * \text{proportionLatino}$ from Equation 1) against the proportion of the over-18 population of the district that is both Latino and eligible to vote (a native-born or naturalized U.S. citizen), henceforth Latino VEP. Panel (a) shows effects for the full targeted sample, and (b) and (c) disaggregate by low and high segregation subgroup, corresponding to the results reported in columns 1–3 of Table 2). The x-axis ranges over the observed distribution of Latino VEP in the data, and the histograms at top and bottom reflect these distributions in the treatment and control groups, respectively. In relatively integrated districts, there is no evidence that the reform increased minority representation. However, in segregated districts with large Latino populations, conversion had a large and

TABLE 1 Summary Statistics (Pretreatment)

	Never Converted			Legal Conversion			Voluntary Conversion		
	Min.	Max.	Mean (s.d.)	Min.	Max.	Mean (s.d.)	Min.	Max.	Mean (s.d.)
Latino Winners (Prop.)	0	1	0.159 (0.222)	0	0.417	0.122 (0.129)	0	0.625	0.153 (0.154)
Latino Candidates (Prop.)	0	1	0.296 (0.309)	0	0.708	0.346 (0.206)	0	1	0.379 (0.288)
Latino Vote Share (Prop.)	0	1	0.174 (0.212)	0	0.397	0.157 (0.104)	0	0.635	0.191 (0.151)
Dissimilarity Index	0	0.354	0.046 (0.066)	0	0.415	0.118 (0.108)	0	0.284	0.053 (0.064)
Enrollment	11	58,780	6,110 (8,724)	2,973	48,912	16,698 (10,993)	117	79,266	8,699 (12,524)
Latino VEP (Prop.)	0.062	0.580	0.250 (0.110)	0.139	0.401	0.273 (0.077)	0.042	0.739	0.274 (0.104)
Household Income (\$)	32,940	108,837	59,692 (15,396)	41,342	88,169	60,620 (11,689)	10,531	91,249	54,063 (11,867)
Size of School Board	3	10	5.124 (0.895)	4	7	5.750 (0.989)	3	9	5.336 (1.052)
N (School Districts)		322			24			112	

Note: The first two columns are constructed from a subset of school districts that were “eligible” to have legal action taken against them, as is reflected in the fixed effects analysis below. The third column is a subset of all, rather than strictly “eligible,” districts that voluntarily converted to ward elections and reflects the thrust of the instrumental variables analysis.

positive effect on the likelihood of Latinos winning elections. For instance, in a district with a Latino VEP of 40%, conversion to ward elections increased the proportion of seats that are won by Latinos by 31 percentage points.

The same pattern emerges when analyzing district size as a moderator of the effect of reform. Although conversion to ward elections in small school districts has no effect on the likelihood of Latinos winning office, there is a dramatic and precisely estimated positive effect in large districts that are composed of at least 30% Latinos (Figure 5). For example, when 40% of the VEP is Latino, the proportion of school board seats won by Latino candidates in large school districts increases by 47 percentage points after switching to ward elections.

How can we account for the absence of an effect of institutional change in small districts? One likely explanation is that sheer numbers are useful for propelling minority candidates to office. Larger constituencies are more likely to yield at least one high-quality candidate, and they allow candidates to build broader coalitions, mobilize voters more effectively, and take advantage of greater resources. According to a study of city council elections across the state by GrassrootsLab, conversions failed to translate into Latino representation in large part because of a shortage of candidates with the means to run (Willon 2017). Consistent with this claim, when we replicate our analysis on the intermediate outcome of running for, not winning, office (SI Figure B.2), we

see a pattern of results similar to our main findings: an increase in large districts commensurate with the size of the Latino population, and no change whatsoever in small districts.

In large and segregated districts with sufficiently small Latino populations, the reform actually had a *negative* effect on Latino officeholding. As the third panels of Figures 4 and 5 show, at Latino VEP of 0.20, conversions in both high-dissimilarity and high-enrollment districts *decreased* the proportion of seats won by Latinos by 20 percentage points ($p < .05$). The negative effects are not surprising: Not only do these districts lack a large enough Latino minority to constitute an influential voting bloc, but also, as critics of the reform have argued, introducing an ethnic gerrymander may amplify voters’ perceptions that political conflict falls along this particular dimension. The result—increasingly racially polarized voting, coupled with small numbers of Latino voters relative to other groups—may create new barriers to Latino electoral victories. As the histograms at the top of Figures 4 and 5 show, not many of the treated observations were both low on Latino VEP and high on segregation and/or enrollment, and therefore not many districts experienced negative treatment effects in practice; that said, reformers ought to carefully consider moving forward with conversion efforts under this set of adverse conditions.

Our data allow us to explore the pathways by which districts elected more Latino candidates to office when

TABLE 2 Effect of Ward Elections on Proportion of Elected Board Members Who Were Latino

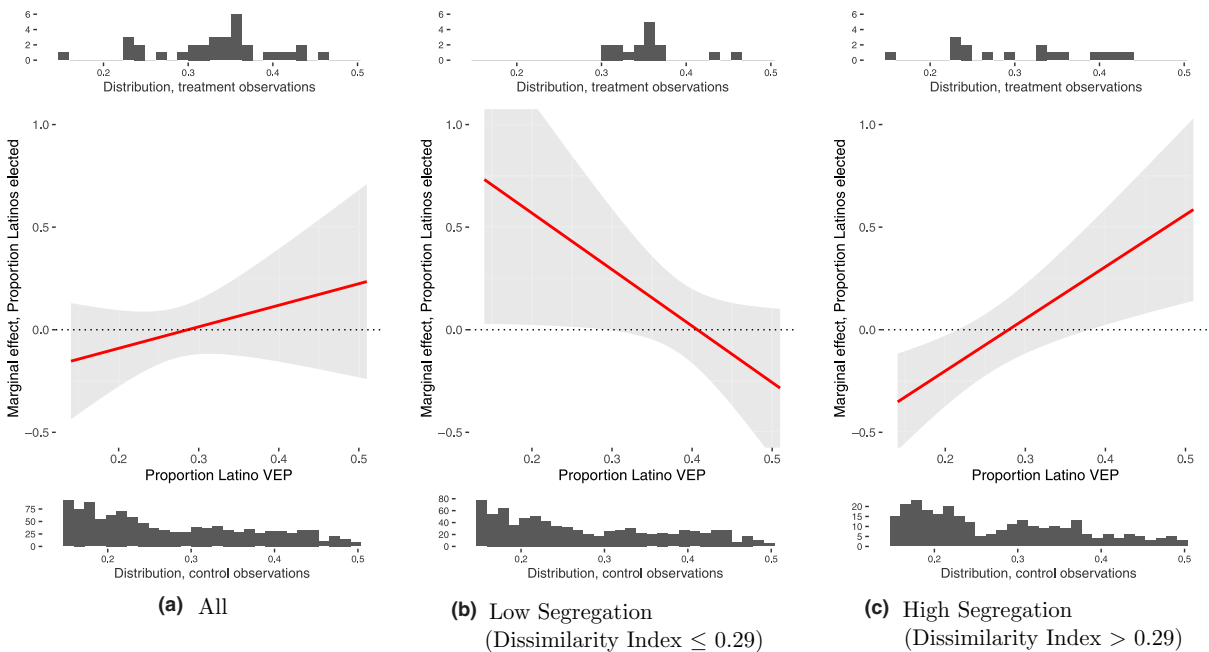
	Dependent Variable				
	All (1)	Segregation		District Size	
		Low (2)	High (3)	Low (4)	High (5)
Ward Elections	-0.301	1.119*	-0.708**	0.559	-0.873***
(Switch by Legal Threat)	(0.276)	(0.554)	(0.223)	(0.366)	(0.205)
Proportion Latino	-0.203	-0.159	-0.528	-0.183	0.880
(Voting Eligible)	(0.211)	(0.227)	(0.630)	(0.220)	(0.758)
Ward × Proportion Latino	1.050	-2.753	2.538**	-1.464	3.351***
	(0.986)	(1.415)	(0.829)	(1.009)	(0.596)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	1,477	1,117	360	1,158	319
R ²	0.620	0.643	0.587	0.649	0.607

Note: For complete results, see SI Table B.1.
 *p < .05, **p < .01, ***p < .001.

the right conditions were in place: Conversion to ward representation both encouraged Latino candidates to run for office in more school board races and increased the share of the vote that they collectively received. In SI

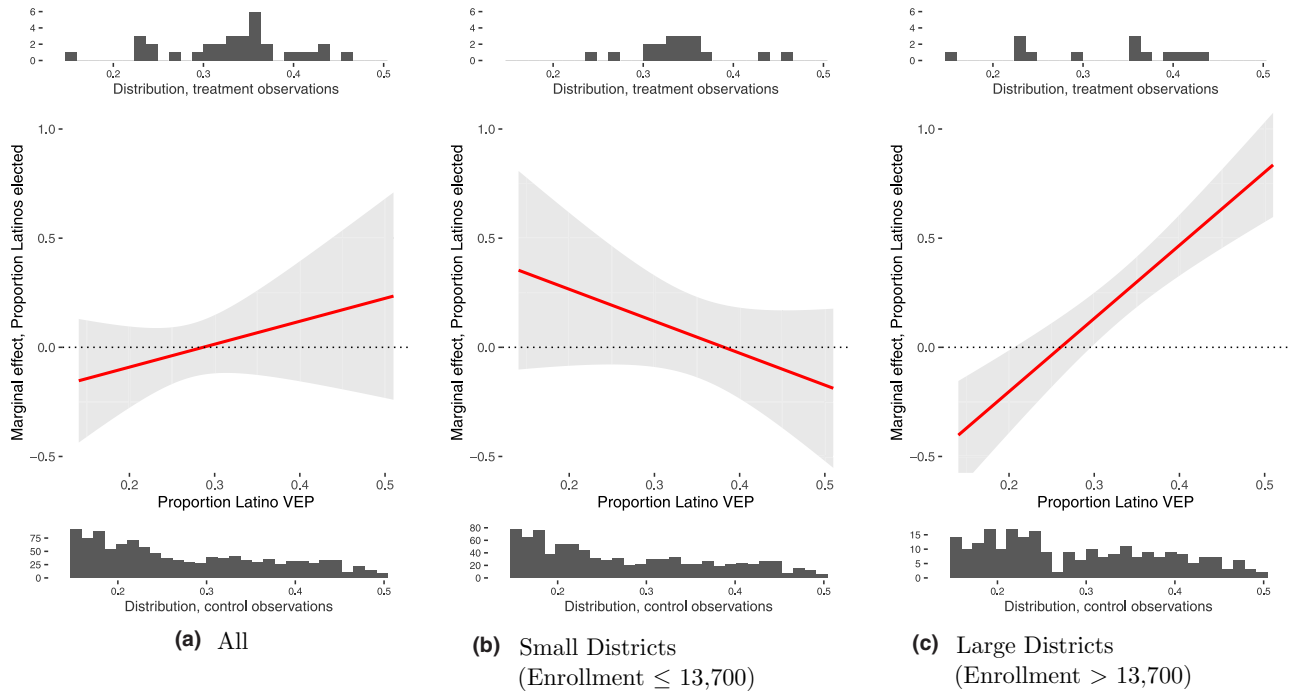
Figures B.1 and B.2, we show dramatic effects of conversion on Latino candidacy in only large and segregated districts. For instance, a large district with 40% Latino VEP saw a 48 percentage point increase in the number

FIGURE 4 Marginal Effects of Conversion to Ward Elections on Proportion of Elected Board Members Who Were Latino, by Geographic Segregation



Note: Results correspond to those reported in columns 1, 2 and 3 of Table 2; 95% confidence intervals are shown in gray.

FIGURE 5 Marginal Effects of Conversion to Ward Elections on Proportion of Elected Board Members Who Were Latino, by District Size



Note: Results correspond to those reported in columns 1, 4, and 5 of Table 2; 95% confidence intervals are shown in gray.

of elections with at least one Latino candidate in the race (as a proportion of all elections in the district that year). These candidates were also able to pull in a larger share of the vote: They received 27 more percentage points in segregated districts with a 40% Latino VEP (SI Figure B.3), and 38 more percentage points in large districts with a 40% Latino VEP (SI Figure B.4).

Finally, we compare districts that saw an increase in Latino candidacy after conversion to those where the reform had no immediate effect. Overall, 65% of legally treated districts had more races with at least one Latino candidate after the reform than prior. As SI Table B.2 shows, these successful districts were significantly larger than other treated districts where Latino candidacy did not increase. Unsurprisingly, they also reflected a higher level of wealth and social capital: Fewer students received free lunch and English-language services and lived below the poverty line, and more of the Latino population was employed and had some college education.

Robustness Checks

Our key findings are robust to alternative measures of residential segregation and definitions of the low and high

subgroups. In SI Figure B.5, we replicate Figure 4 using the Theil index instead of the dissimilarity index, recovering very similar estimates. Our findings, furthermore, do not hinge on the particular cutoffs that define subgroups on residential segregation and district size. For instance, when we define the high subgroups according to the top third rather than the top half of treated units, the pattern of results is unchanged, and the treatment effects increase in magnitude (see SI Figures B.6 and B.7).

Recent methodological work identifies two potential pitfalls of interpreting coefficients from a multiplicative interaction model such as the one we use for our two-way fixed effects specification (Hainmueller, Mummolo, and Xu 2018). First, the model relies on a linearity assumption: The interaction effect is assumed to change at a constant rate with the moderator (in this case, Latino VEP). In SI Figures B.8 and B.9, we relax this assumption, instead estimating a noninteracted model in each of three bins constructed based on the distribution of Latino VEP among treated units. We find that the effects of conversion are indeed generally increasing with Latino VEP and statistically significantly different from one another; thus, our conclusions do not crucially depend on the linearity assumption. A second potential danger is a lack

TABLE 3 Legal Action Taken Against Nearby School District as an Instrument for Voluntary Conversion from At-Large to Ward Representation

	First Stage (1)	Seats Won by Latinos (2)	Latino Candidates (3)	Latino Vote Share (4)
In-County Legal Threat	0.139*** (0.029)			
Voluntary Conversion		-1.219 (0.666)	-0.524 (0.650)	-0.983 (0.567)
Proportion Latino		0.364** (0.111)	0.548*** (0.123)	0.350*** (0.105)
Voluntary \times Proportion Latino		4.099* (1.883)	2.420 (1.804)	3.551* (1.650)
Controls	Yes	Yes	Yes	Yes
Observations	3,433	3,386	3,398	3,398
R ²	0.036	0.211	0.390	0.333
F-statistic on Instrument	130.6			

Note: Robust standard errors clustered by school district are in parentheses. Estimated intercept and controls are not reported.
*p < .05, **p < .01, ***p < .001.

of common support of the moderator across treatment and control groups, which can result in overextrapolation if the linearity assumption is not satisfied. To guard against this, we only report marginal effects in the region of Latino VEP where we can find both treated and control units—in practice, between the minimum and maximum values of Latino VEP in the treatment group.

IV Analysis

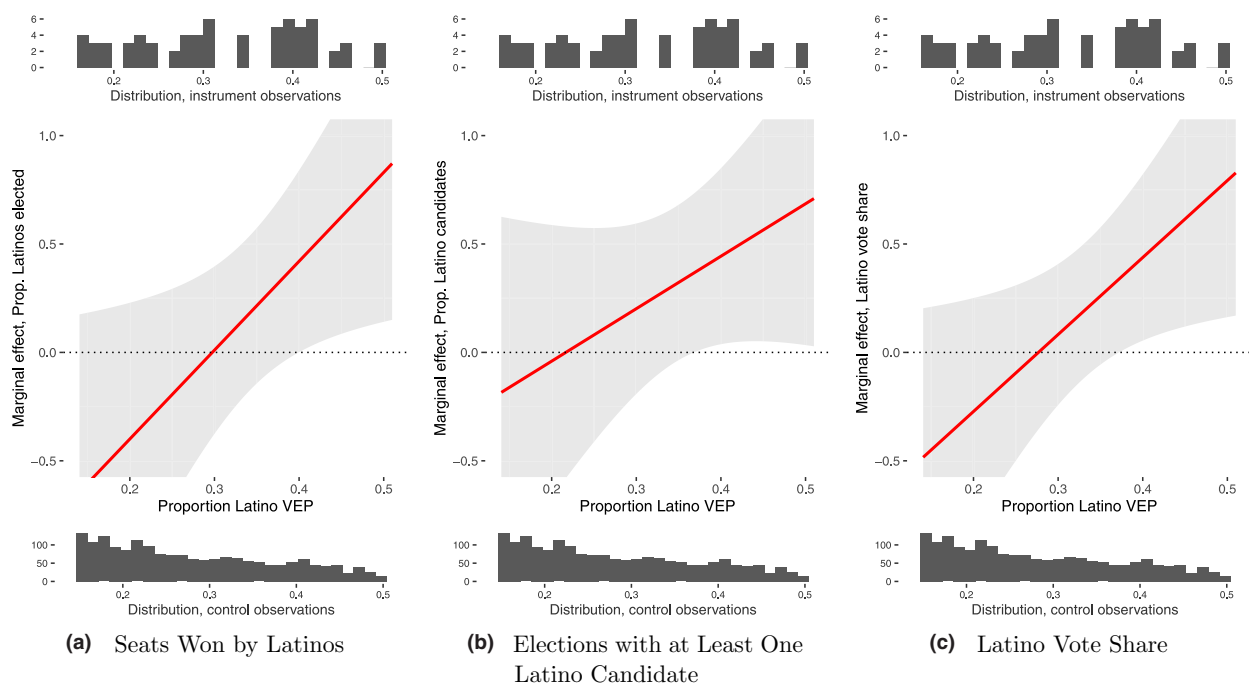
We isolate the causal relationship between voluntary conversions of school districts to ward voting and Latino political performance by utilizing an instrumental variables framework. We do this by treating the incidence of a legal action being initiated against a nearby school district $_{k_c}$ (within the same county c) as having no direct impact on the success of Latino candidates in district $_{i_c}$ (satisfying the exclusion restriction) but as being a strong predictor of district $_{i_c}$'s decision to pursue its own electoral reform (satisfying the strong first-stage requirement).

District $_{i_c}$ might choose to convert after observing a legal threat being made against a neighboring school district for a number of reasons, the most salient being the fear of becoming a subsequent target for legal action. The CVRA was written in a way such that (1) litigation was virtually guaranteed to be successful in forcibly converting school districts, and (2) the cost of pursuing litigation would be essentially zero for the plaintiff due to the districts' obligation to cover the plaintiff's legal bills. Thus,

for reform advocates, pursuing litigation was a win-win scenario: They were all but guaranteed success, their costs were covered, and the redistricting plan drawn up by the school district or county was subject to the courts' approval before being implemented. On the other hand, if districts could manage to get ahead of what they (rightly) perceived as an impending wave of litigation, they could avoid all of the costs associated with legal action and retain control over their redistricting plans, so long as they satisfied outside legal observers. For those districts that might be especially fearful of a protracted, costly legal battle that could stoke backlash from white constituents, the incentive to voluntarily and peaceably convert to ward elections designed to satisfy legal challengers would be particularly strong. As such, the effect of electoral reform on Latino representation in forcibly converted districts is likely to be considerably different from the effect in districts that voluntarily converted.

Table 3 presents the results of the first- and second-stage regressions using the full sample of school districts (after removing districts that were ward systems for the entirety of our sample and districts that were directly threatened by civil rights groups via legal action), and the treatment effects are depicted visually in Figure 6. The instrument is a binary indicator of whether the school district resided in a county in which there occurred at least one legal action taken against another school district the year before or anytime prior, and the treatment is voluntary conversion to ward voting by nontargeted districts. The dependent variables remain the same three political outcomes from the previous section.

FIGURE 6 Marginal Effect of Voluntary Conversion to Ward Elections: Instrumental Variables Analysis



The effect of voluntary conversion is striking. The first panel of Figure 6 shows that the number of school board seats won by Latinos increases 42 percentage points when the population is composed of 40% voting-eligible Latinos. On average, this amounts to Latino candidates winning 29% of school board seats up for election under an at-large system versus Latino candidates winning 71% of seats under a ward system. This gain is explained both by an increase in Latino vote share and by the percentage of Latinos who run for election. Both increase by about 45 percentage points, going from 26% to 70% as a share of the vote on average and from 38% of elections having at least one Latino candidate to 83% of elections having at least one Latino candidate, on average. This positive impact of electoral reform becomes statistically significant when at least 40% of the voting-eligible population is Latino.

Conclusion

The research presented in this article showed, for the first time, that there is a causal link between electoral institutions and Latino political success in local U.S. government. Specifically, the descriptive representation of Latinos is likely to improve significantly after moving from at-large to ward elections in school

districts that are large and residentially segregated, and where the Latino population is of sufficient size. When all of these conditions are met, the positive impact of reform is striking, exceeding one additional officeholder for every three available seats. This is in spite of the fact that (1) the CVRA set a relatively low bar for demonstrating racially polarized voting, so the treated districts need not have had the most egregious representation gaps to begin with; (2) the federal Voting Rights Act predated the CVRA in proactively pursuing instances of minority vote dilution, so it already eliminated the most egregious offenders; (3) Latinos had to be politically mobilized and to run effective candidates to take advantage of the electoral reforms; and (4) most instances of electoral reform under the CVRA took place as voluntary conversions in which the school board or county—and not the state—controlled the redistricting maps. Given these countervailing conditions, it is impressive to see the size and significance of these contingent effects of electoral reform under the CVRA.

On the other hand, when these important conditions are not in place, moving from at-large to ward elections can actually have null, or even negative, effects on Latino descriptive representation, with the additional downsides of imposing expensive legal fees, large transaction costs, and often divisive political conflict on already overburdened districts. The presence of these contingencies may

help explain why a large and active academic literature on the subject has produced so many conflicting findings, and it opens avenues for future work. Although our finding on the moderating effect of residential segregation is entirely consistent with theoretical predictions based on how the respective institutions aggregate votes, more research is needed to understand the mechanisms driving our novel finding on district size. Political networks, resources, and the sheer availability of viable candidates are all plausible explanations that should be investigated further.

Our findings also highlight that when undertaking electoral reform, process matters for outcomes. Whereas our analysis of legally mandated conversions uncovered positive *and* negative effects moderated by key conditions, districts that converted voluntarily saw unconditionally positive effects at every step of the electoral process, from candidacy to vote share to the ultimate outcome of Latino officeholding. While one might expect, a priori, that districts that select into electoral reform are already more concerned about minority representation and would therefore exhibit smaller treatment effects, it seems instead that buy-in from institutional actors contributed dramatically to the reform's effectiveness.

Taken together, we hope our findings can inform the best way forward for reformers who aim to increase the voice of minorities in the political process. A useful takeaway is that, at least on the dimension of segregation, the federal VRA seems to have gotten it right. Though the CVRA was written with the best intentions in mind, it caused a number of low-segregation school districts to convert to ward elections that would not have done so under the federal standard, and they may have done so needlessly, and at great cost to them. Additionally, both the CVRA and the federal VRA miss one of the crucial conditions necessary for improvements in representation, independent of segregation: the size of the district. Even in places that are not particularly segregated, greater enrollment strongly predicts the likelihood of successful electoral reform, an insight that may extend to other forms of local government such as city councils and municipal boards. Overall, our findings on these contingencies and on the success of reforms undertaken voluntarily suggest that, rather than a broad, sweeping legal remedy, states should consider offering information, demographic analysis, and technical assistance for conversion targeted to precisely those districts where reform is likely to have the largest impact.

Finally, our work has shed light on the limitations of institutional change, and on the persistent barriers to minority political access that require even greater investments than interventions such as the CVRA. Changing the

way that votes are aggregated into seats may lead to some easy representational gains under certain conditions, but it does not change more fundamental realities like the propensity of minority candidates to run for office, the resources at their disposal when they do, and the levels of voter turnout and mobilization. As we show, districts that were able to take full advantage of the CVRA had relatively low levels of poverty and unemployment and high levels of English-language proficiency and education, and reformers should target these crucial constraining factors alongside institutional change.

There is still much work to be done to understand the relationship between electoral institutions and substantive representation. We did not find any effect of conversion on a number of educational, achievement, or financial outcomes (not reported). At least part of this finding may be a result of the relatively short period of policy implementation that we can possibly study at this time. It is likely that the graduation and dropout rates of Latino students, for example, are slow-moving variables that benefit from policy changes only over the long run. In future years, we should be able to better gauge the long-term policy consequences of improved descriptive representation on local school boards.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix A: Additional Information about the CVRA

Appendix B: Additional Tables and Figures