

Running Towards Rankings: Ranked Choice Voting's Impact on Candidate Entry and Descriptive Representation*

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Abstract Does the implementation of a Ranked Choice Voting System increase the number, diversity, and quality of candidates competing in local elections? Using original data from over 250 cities across three decades and employing a pre-registered difference-in-differences approach with matching, I find a significant increase in the size of the candidate pool following implementation. However, this effect dissipates in later election cycles, indicating RCV has no long-term effect on the candidate pool size. Furthermore, the increase in the size of the candidate pool observed in the short-term is largely caused by low quality candidates with little chance of winning office. Finally, I find RCV has no effect on the diversity of the candidate pool in the short or long-term. These findings call into question several benefits claimed by proponents of RCV, and indicate that RCV may not be powerful enough to influence candidate entry decisions at the local level.

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Though perhaps a trite expression, local governments as "laboratories of democracy" continues to be an apt description of how new institutional structures spread across the United States. The 1888 presidential election scandal, involving the treasurer of the RNC instructing county party chairmen to purchase votes, led Louisville, Kentucky to adopt the secret ballot. Only four years later, citizens in 38 states were following Louisville's example (Ackerman 1998). Similarly, the potential for at-large elections to break party machines in the early 1900s led to widespread adoption before the determination that it disenfranchised minority voters led to a shift back to district elections (Judd and Swanstrom 2012). Both examples demonstrate the power of local electoral reform to cause nationwide changes, with one serving as a success story and the other as a cautionary tale.

Ranked Choice Voting (RCV) is yet another electoral reform sweeping across the country. Interest in RCV has rapidly grown following New York City's implementation, and beyond the two states using RCV statewide (Alaska and Maine), at least 29 others are considering adopting RCV in some form. Often presented as a cure-all for various electoral ailments, its list of potential benefits is long. Among the most important of these purported benefits is RCV's potential to increase candidate entry, quality, and diversity.

These benefits address some of the most serious problems facing local politics. Increasing the size and quality of candidate pools in local elections could mitigate issues such as low levels of interest in municipal elections and its knock-on effects on representation and accountability (see Warshaw 2019 for a review). Similarly, the lack of diversity in local politics is an enduring problem, and a large body of research focuses on understanding how institutional structures such as partisanship (e.g. Trounstein and Valdini 2008) and at-large elections (e.g. Crowder-Meyer, Gadarian, and Trounstein 2015), can attenuate the diversity gap. Claims that RCV increases diversity put it in the middle of this scholarly debate, and RCV may offer a new solution to what has been an intractable problem.

In this article, I assess whether RCV increases the size, diversity, and quality of the candidate

pool in local elections. First, I walk through the arguments for why RCV may impact the calculus for potential candidates and encourage higher quality and more diverse candidates to run for office. Then, using original data and a difference-in-differences quasi-experimental design paired with matching as a pre-processing step, I empirically test those claims. I find there is an increase in the number of candidates running for office following RCV adoption. Beyond the short-term effects, I also assess RCV's long-term effects on candidate pool size. I find positive effects in the short-term dissipate, and are likely caused by political uncertainty rather than systematic change in the factors influencing candidate entry.

Next, I investigate whether RCV affects the quality of candidates running for office. Using two measures of viability and one measure of experience, I find that any increase in the candidate pool is among candidates with low levels of support and little government experience. Finally, I find that RCV does not encourage more female or non-White candidates to run.

Having established these results, I explain why RCV does not have its intended impact on candidate entry. I empirically test several mechanisms through which RCV might increase candidate pool size and discuss the lack of any significant effect. Instead, I argue RCV's effect on candidate entry decisions occurs too late in the candidate recruitment and entry process to influence candidate pool composition. I conclude by discussing the potential causes of the temporary upswing in the candidate pool size. Rather than the panacea for our democracy's various political ailments, RCV fits the more pessimistic pattern among electoral innovations of high expectations and dashed hopes.

1 RCV in the United States

In RCV elections, voters rank candidates in order of preference. To select a winner, the candidate with the fewest first place votes is eliminated and the voters who selected that candidate first have their votes reallocated to their second choice. This process occurs until one candidate

receives over 50% of the votes.

RCV's rapid spread across the United States has been due to the attractive suite of benefits it claims to offer. One benefit of RCV is it discourages negative campaigning, an attribute confirmed by the empirical literature (Donovan, Tolbert, and Gracey 2016; Kropf 2021). Candidates avoid speaking too harshly of their opponents in an attempt to receive rival candidate's supporters' second and third place rankings. Sometimes, this leads to candidates endorsing direct competitors, urging their supporters to pick that candidate second.

Another purported advantage is more representative electoral outcomes. Voters may feel enfranchised by RCV because it allows a more complete expression of preferences. Furthermore, RCV may lead to more representative outcomes because it stops candidates with a medium-sized group of supporters overcoming a large field of alternatives. In this scenario, a plurality system may elect a candidate unpopular with most voters because that majority did not land on a consensus candidate. In RCV, this scenario is avoided. Finally, RCV's effect on representation depends on whether it has a positive or negative effect on ballot errors and turnout. Some have found RCV does not have any effect on turnout or increase ballot exhaustion, though other studies have found the opposite (Coll 2021; Neely and Cook 2008; Curtice and Marsh 2014; Zvulun 2012; Endersby and Towle 2014; Neely and McDaniel 2015).

A third benefit is RCV minimizes strategic voting (Simmons, Gutierrez, and Transue 2022). In plurality systems, supporters of fringe candidates may fear they are wasting their vote. This can lead them to strategically pick a more mainstream candidate who would not be their first choice. In RCV, this shouldn't occur. Voting for a fringe candidate first followed by the mainstream candidate later allows for an expression of a voter's true preferred candidate while also ensuring the voter's ballot is not exhausted before the final round.

Perhaps most important is how RCV changes the decision-making calculus for potential candidates. Specifically, proponents of RCV argue it encourages more candidates to run for office (Copper and Greenwood 2018). Reducing worries of a spoiler effect encourages candidates

from the same voting bloc to run concurrently and diminishes the incentives for political gatekeepers to restrict entry (Bordignon, Nannicini, and Tabellini 2016). The more civil nature of RCV elections should also encourage candidates to run even when their chances of winning are low. Single-issue candidates can run in a quasi-coalition with more high-profile candidates to boost their profile. Finally, RCV elections are less expensive because they eliminate runoff campaigns. Literature on spending limits confirms lowering campaign costs stimulates candidate entry (Fouirnaies 2021).

Beyond increasing the number of candidates running for office, RCV may also increase the diversity of those candidates (Copper and Greenwood 2018). Women and minority candidates typically receive less encouragement from party elites (Fox and Lawless 2004; Krook 2010; Niven 1998). Removing incentives for political gatekeepers to restrict entry especially benefits women and minority candidates, encouraging a more diverse candidate pool. The more civil campaign environment may also benefit women due to the conflict and competition gap between men and women considering running for office (Kanthak and Woon 2015; Schneider et al. 2016). Finally, lowering election costs may also increase diversity, as women considering running for office are more cost averse (Werner and Mayer 2007).

The increased number of candidates in RCV may also lead to more competitive campaigns with more qualified candidates. Higher quality candidates are more strategic when considering running for office (Maestas et al. 2006). Though RCV does not impact the number of seats up for election, it can affect whether a potential candidate thinks the political environment is right for them to run. Strategically, RCV offers more avenues to victory. For example, a candidate can outflank a copartisan then secure their votes in a subsequent round. These additional paths to victory may encourage more high-quality candidates to run for office, leading to more competitive elections.

Research on RCV's effect on candidate entry has mostly focused on only a few, large cities (McCannon 2022; John, Smith, and Zack 2018). This narrow focus limits these studies' external

validity. Smaller cities are experimenting with RCV, and it is unclear whether these studies apply to cities with very different political contexts. Work looking across all RCV cities is descriptive rather than causal (Terrell, Lamendola, and Reilly 2021). This research has also focused on only one aspect of candidate entry, whether that be the diversity or the raw number. To date, no existing study investigates the full range of potential effects RCV could have on candidate entry.

This paper fills this gap in the literature by evaluating all RCV cities in the United States using a causal strategy to test how RCV affects candidate entry. I apply this causal strategy to outcomes looking at the overall number of candidates, the quality of candidates, and the diversity of candidates. Thus, these results provide the first comprehensive analysis of the various effects RCV may have on the candidate pool.

2 Empirical Strategy

Forty-seven cities in thirteen states across the country have used RCV since 2000. While the most high-profile RCV cities are more liberal, RCV is not a strictly liberal policy. In Utah the Republican party supports RCV, and several Republican-majority states use RCV for overseas and military voters. Support for RCV is also not uniform on the left, with Democratic leaders such as California governor Gavin Newsom taking anti-RCV stances. While most cities use RCV for mayoral and city council elections, several have only implemented RCV for one or the other. Appendix A shows a list of all cities using RCV, which elections they use it for, as well as RCV's implementation date.

I use five different outcome variables to assess claims about the effects of RCV on the size, diversity and quality of the candidate pool. My first outcome variable is the raw number of candidates running for office. Without an increase in the number of candidates running, knock-on benefits such as more diverse or higher quality candidates are unlikely. None of the arguments for why RCV would increase the diversity or quality of candidates running are based on replace-

ment; without an increase in the overall size of the candidate pool, there is little reason to expect an increase in diversity or quality.

While standard practice is to measure candidate quality using past government experience, there are several obstacles to using it here. Municipal offices are often the first place candidates gain experience. City council candidates are less likely to have held elected office, as there are few offices below the municipal level. Second, collecting information about the prior experience of candidates running for local offices is difficult. Voter guides in certain cities may include a candidate's background, however many candidates for local office fail to submit an entry or address their policy views rather than experience.

To get around this, I use competitive campaigns as a proxy for higher quality candidates. High-quality candidates are more likely to run high-quality campaigns that garner more votes, leading to more candidates who are viable and, overall, more competitive elections. To measure the viability of a campaign, I use the number of votes the candidate received. While I use viability as a proxy for quality, more candidates receiving large vote shares is positive on its own. More competitive elections means voters have more realistic options, and can enhance governmental legitimacy and responsiveness.

To identify changes to the quality of the candidate pool, I measure the number of viable candidates using two assignment rules. The first marks every candidate receiving at least 5% of the vote as viable. A low bar, over 5% of the vote indicates a candidate has some real voter base beyond friends and family. The stricter of the two is the 70% LW rule (Niemi and Hsieh 2002). This measure identifies all candidates who received at least 70% of the vote obtained by the winning candidate with the fewest votes as viable. While researchers often use Laakso and Taagepera's effective number of parties measure to estimate the effective number of candidates as well, the measure cannot account for multi-member districts, a common occurrence in local elections with at-large city councilmembers (Laakso and Taagepera 1979). The LW rule is com-

patible with both single-member and multi-member districts, making it ideal for this study ¹.

Finally, to identify changes to the diversity of the candidate pool, I use the Gender and REthnicity packages in R to estimate the gender and ethnicity of each candidate² (Xie 2022; Blevins and Mullen 2015). Using predicted gender and ethnicity, I measure the proportion of female candidates and the proportion of non-White candidates in each race.

A longitudinal approach to estimating RCV's effect is possible with election data from RCV cities only. However, the results from such an approach would be plagued by bias. Choosing to implement RCV is typically a political battle between interest groups, and elections following implementation may systematically differ from those prior (Santucci 2017). If so, looking only across time may inflate the effect of RCV. Similarly, general temporal trends could also artificially inflate the estimated effect.

As an alternative, a cross-sectional approach faces its own challenges. Cities choosing to implement RCV likely differ from those without RCV in ways that are difficult to quantify. For example, choosing a new voting system may indicate a more involved civic environment that already encourages larger candidate pools. These differences would be difficult to account for, even with a regression model controlling for a wide variety of independent variables.

Concerns over period effects in a longitudinal study and omitted variable bias in a cross-sectional study point to difference-in-differences as the ideal method for identifying RCV's effect on the candidate pool. Difference-in-differences models account for unobserved variation across cities and time, eliminating a significant amount of bias. This feature makes difference-

¹In Appendix F I rerun my models using the effective number of candidates instead of the LW Rule, with similar results.

²To assuage potential concerns over the accuracy of these autocoding procedures, I use alternative prediction methods for each procedure and rerun the analyses. These results are in Appendix H.

in-differences models an appealing strategy for electoral reform studies (Becher and González 2019; Burden et al. 2014). Without the potential for random assignment of voting systems in real-world settings, difference-in-differences offers one of the more rigorous ways to identify the causal effect of an intervention such as RCV.

A difference-in-differences approach, while better able to account for unobserved variation across time and units, still poses a challenge in identifying a reasonable control group. Collecting election data on the entire population of non-RCV cities is infeasible, as there are over 18 thousand cities/towns across the United States. City election data is difficult to collect, as there is no centralized data source. Within states and counties there is variation in who carries out elections and maintains records, with some cities doing it themselves and others outsourcing to county governments. Furthermore, regulations vary for how long governments must keep election records. Finally, many smaller cities do not house election results online, meaning data collection would require traveling to thousands of cities across the country.

To overcome this, I use matching as a pre-processing step to identify cities that could serve as the control group to the treated RCV cities³. To identify this sample of non-RCV cities, I use two datasets: the American Community Survey (ACS) 5-year estimates and the ICMA Municipal Form of Government Survey. Though the best dataset on municipal institutions in the United States, the survey has a response rate under 33 percent that is biased toward larger cities (*2018 Municipal Form of Government Survey Report* 2018). Thus, I match on the missingness of the data and collect missing institutional data on the matched cities during the election data collection process. The set of variables used for this matching step include demographic variables such as population, economic variables such as median income, and institutional variables such as their missingness from the ICMA database. Finally, I match on the year RCV implementation occurred.

³Appendix C discusses why the matching in this study does not pose concerns related to regression-to-the-mean (RTM) bias.

With these variables, I use genetic matching with four different specifications. Genetic matching maximizes the balance of covariates across the control and treatment groups, while the variation in matching specifications generates a larger sample of control cities (Diamond and Sekhon 2013). Each matching specification identified five control cities for each treated city. This ensured enough cities to collect data from even if some did not have data available. While this pre-processing step is critical, the goal is not to identify perfectly matched cities. A difference-in-differences approach can account for systematic differences between control and treated units, and this pre-processing step focused on identifying non-RCV cities that are broadly comparable to RCV cities.

The matching process resulted in 311 high-potential matches for 45 RCV cities, with each of the four strategies selecting a similar set of control cities for each treated city. Following my pre-processing step but prior to collecting election data, I submitted a pre-analysis plan with the list of cities to assuage concerns these cities were selected because of the results they produce⁴.

Having identified my control cities, I collect original election data for control and treatment group cities before and after RCV implementation. While the election immediately prior to and immediately after are the minimum needed, I collect data for elections within ten years of the implementation date. For example, a city implementing (or matched with a city implementing) RCV in 2010 would have data collected for all elections between 2000 and 2020. Collecting this additional data allows me to analyze pre-treatment and post-treatment trends.

For these cities, a research assistant and I hand-coded election data collected from municipal and county websites, direct contact with local officials, public records requests, newspaper archives, and city council meeting minutes. We collected data on over six thousand unique elections occurring between 1995 and 2022 across 273 different cities. Data collected included candidate names, incumbency status, how many seats were up for election, vote totals, write-in votes, runoff or primary election results, and election winners. For many of these cities, this is

⁴The preregistration pdf is in Appendix J.

the first time election data has been analyzed by scholars.

Though I attempted to collect election data within ten years of RCV implementation, in many cases this was not possible. Some cities implemented RCV recently, and thus have only held one or two RCV elections. For other cities, election records stop beyond a certain date. This results in some cities, like San Francisco, having data collected from an entire 20-year period. For cities like Goshen UT, which began using RCV in 2021, election data is only available from 2011 to present day.

I also dropped several cities during data collection. Some control cities do not popularly elect a mayor, and thus couldn't be used to compare with treated mayoral elections. Other control cities did not hold elections post RCV implementation due to different elections cycles. Finally, some cities did not have accessible historical election data. While some cities failed to respond to data requests, others only had data available in hard copy format or deleted their records after a certain amount of time⁵.

Table 1 below shows two balance tests. First, I show the balance between the original set of control and treated cities. Second, I show the balance between control and treated cities that I successfully collected data on. For each balance test I include the average values for each group, as well as the Kolmogorov-Smirnov Test p-value, which tests the null hypothesis that the two dataset values for each variable are from the same continuous distribution. Across all variables included, the KS Test p-values are above 0.05, indicating that the null hypothesis cannot be rejected.

To estimate the causal effect of RCV on each of the five outcome variables, I use a simple, two-period, difference-in-differences approach. I estimate my DiD model using the equation:

⁵For cities with primaries, I collapse different party primaries into one candidate field, using primary vote totals to calculate viability. This only effects 9 cities in the sample, and in Appendix K I show the results hold if they are dropped.

Variable	All Matched Cities			All Collected Cities		
	Control Mean	Treatment Mean	KS Test P-Value	Control Mean	Treatment Mean	KS Test P-Value
Year	2017	2017	0.736	2017	2017	0.838
Population	263,614	71,529	0.392	274,600	81,424	0.742
Proportion White	0.799	0.807	0.836	0.798	0.800	0.746
Proportion Black	0.062	0.059	0.906	0.062	0.059	0.706
Proportion Asian	0.052	0.047	0.738	0.053	0.051	0.886
Proportion 18 or Less	0.256	0.245	0.596	0.261	0.249	0.580
Proportion 18 to 34	0.254	0.251	0.888	0.250	0.250	0.898
Proportion 35 to 54	0.263	0.261	0.902	0.265	0.261	0.634
Proportion 55 and Up	0.227	0.242	0.150	0.224	0.239	0.154
Proportion No High School Degree	0.082	0.083	0.586	0.079	0.081	0.518
Proportion High School Degree	0.204	0.226	0.068	0.204	0.223	0.138
Proportion Some College	0.303	0.306	0.766	0.305	0.312	0.400
Proportion Bachelors Degree	0.261	0.248	0.644	0.263	0.248	0.784
Proportion Graduate Degree	0.149	0.138	0.142	0.150	0.136	0.074
Median Income	34,183	33,400	0.870	34,865	34,000	0.920
Proportion Homeowners	0.667	0.675	0.566	0.674	0.683	0.650
Employment Ratio	0.676	0.664	0.182	0.680	0.666	0.132
Proportion Female	0.503	0.505	0.850	0.501	0.504	0.766
Proportion Missing from ICMA	0.667	0.817	NA	0.651	0.828	NA
Cities from Middle Atlantic	0.022	0.019	NA	0.023	0.024	NA
Cities from Mountain	0.622	0.547	NA	0.651	0.568	NA
Cities from New England	0.067	0.090	NA	0.047	0.052	NA
Cities from Pacific	0.089	0.084	NA	0.093	0.104	NA
Cities from South Atlantic	0.067	0.087	NA	0.047	0.040	NA
Cities from West North Central	0.111	0.135	NA	0.116	0.168	NA

Table 1: The first three columns show the balance between the treatment and control groups identified by the matching algorithms, while the last three columns show the balance between the treatment and control groups that election data was successfully collected on. The KS Test p-value tests the null hypothesis that the two dataset values for each variable are from the same continuous distribution should be rejected.

$$y_{it} = \alpha_0 + \beta_1 \times \text{Time Period} + \beta_2 \times \text{RCV Cities} + \beta_3 \times \text{Time Period} \times \text{RCV Cities} + \sum \beta_i X_i + \sigma_c + \epsilon \quad (1)$$

The interaction term, *Time Period x RCV Cities*, represents the difference-in-differences estimator, while *Time Period* represents the temporal effect and *RCV Cities* the effect of being in the treatment group. I also include group fixed-effects to account for the matching discussed above. Additionally, there are several control variables that are included in the final model, represented by $\sum \beta_i X_i$. The unit of analysis is the city-election (*i*).

I estimate the DiD model above separately for city council and mayoral elections. As mentioned earlier, some cities implement RCV for mayoral elections, some for city council elections, and some for both. To simplify the estimation, I run one model for city council seats and one for mayoral seats.

Both models include a control for whether an incumbent was running for office. An incumbent typically depresses the number of other candidates who choose to run, but is information that's unavailable prior to collecting election data. For city council elections, I include two additional control variables. The first is an indicator for whether city council seats are district-based, at-large, or mixed. At-large elections, as city-wide races akin to a mayoral election, may attract more candidates due to their higher status. Second, I include the number of seats up for election. Some city council elections are multi-member, meaning that, even prior to RCV, citizens voted for two or more candidates in the same election. Similar to incumbency status, this information was unavailable prior to data collection but affects the number of candidates running for office. If candidates are competing for two or more seats rather than one, more candidates will enter the race.

Finally, I use a simple two-period difference-in-differences model because I standardize and

center the elections around the implementation date. In every figure, time period -1 is the period immediately prior to implementation while time period 0 is the first election following RCV implementation⁶.

3 Results

3.1 Parallel Trends

Before looking at the main results, I first examine pre-treatment trends. Collecting data up to ten years prior to implementing RCV allows for testing the parallel trends assumption critical to causally identifying a treatment effect using difference-in-differences. To do this, I add pre-treatment leads to my main model. Including pre-treatment leads in the model allows for an examination of the outcome dynamics pre-treatment. Lead coefficients with insignificant effects demonstrate that there is no difference in temporal trends for the treatment and control groups pre-treatment.

This model compares each pre-treatment time period to the period immediately preceding treatment. Since all time periods involved in the model are pre-treatment, the DiD estimator for each time period should be statistically insignificant. Besides including dummy variables for each period pre-treatment and dropping all post-treatment data, the model specification is identical to the main DiD model. I estimate this pre-treatment DiD model using the equation:

$$y_{it} = \sum_{T=-5}^{-1} \beta_1 x \text{Time Period} + \beta_2 x \text{RCV Cities} + \sum_{T=-5}^{-1} \beta_3 x \text{Time Period} x \text{RCV Cities} + \sum \beta_i X_i + \sigma_c + \epsilon \quad (2)$$

⁶I discuss why this specification avoids the issues of Callaway and SantAnna (2021) and Goodman-Bacon (2021) in Appendix D, and provide alternative specifications in Appendix I.

In Figure 1 I plot the coefficients of interest: the effect sizes of the treatment prior to the treatment occurring. Similar to the main DiD model, I estimate separate models for city council and mayoral elections and for each of the five dependent variables discussed above.

As RCV implementation occurs in time period zero, the pre-treatment trends stretch from negative five to negative one. Importantly, the coefficient and confidence interval for time period -1 are zero, as it is the baseline period. Across election type, time period, and dependent variable, there appears to be no pre-treatment treatment effect. Nearly all the plotted coefficients are statistically insignificant, meaning control and treatment group trends are parallel. While not proof of the parallel trends assumption, it is the most compelling evidence available. This confirms the main identification assumption for difference-in-differences is reasonable, and that RCV causes any effect seen post implementation.

3.2 Candidate Pool Size

Table 2 below shows the results of the two-period differences-in-differences model for the raw number of candidates. The first row in the table shows RCV leads to a substantively large and statistically significant increase in the number of candidates running in city council and mayoral elections. For mayoral elections, the increase in the candidate pool following RCV is 2.125, while for city council it is 1.215. Adding over two candidates to mayoral races and more than one candidate in city council races demonstrates RCV has an sizeable impact on how many people are choosing to run. This impact would likely be noticeable to voters, who could see over three more candidates on their ballot if voting for a mayor and city council member in the same cycle. The larger increase in mayoral races compared to city council races, which could be due to mayoral offices being more coveted positions or to higher levels of media attention, testifies to the necessity of modeling the two election types separately.

The standard two-period difference-in-differences model used in the table only measures

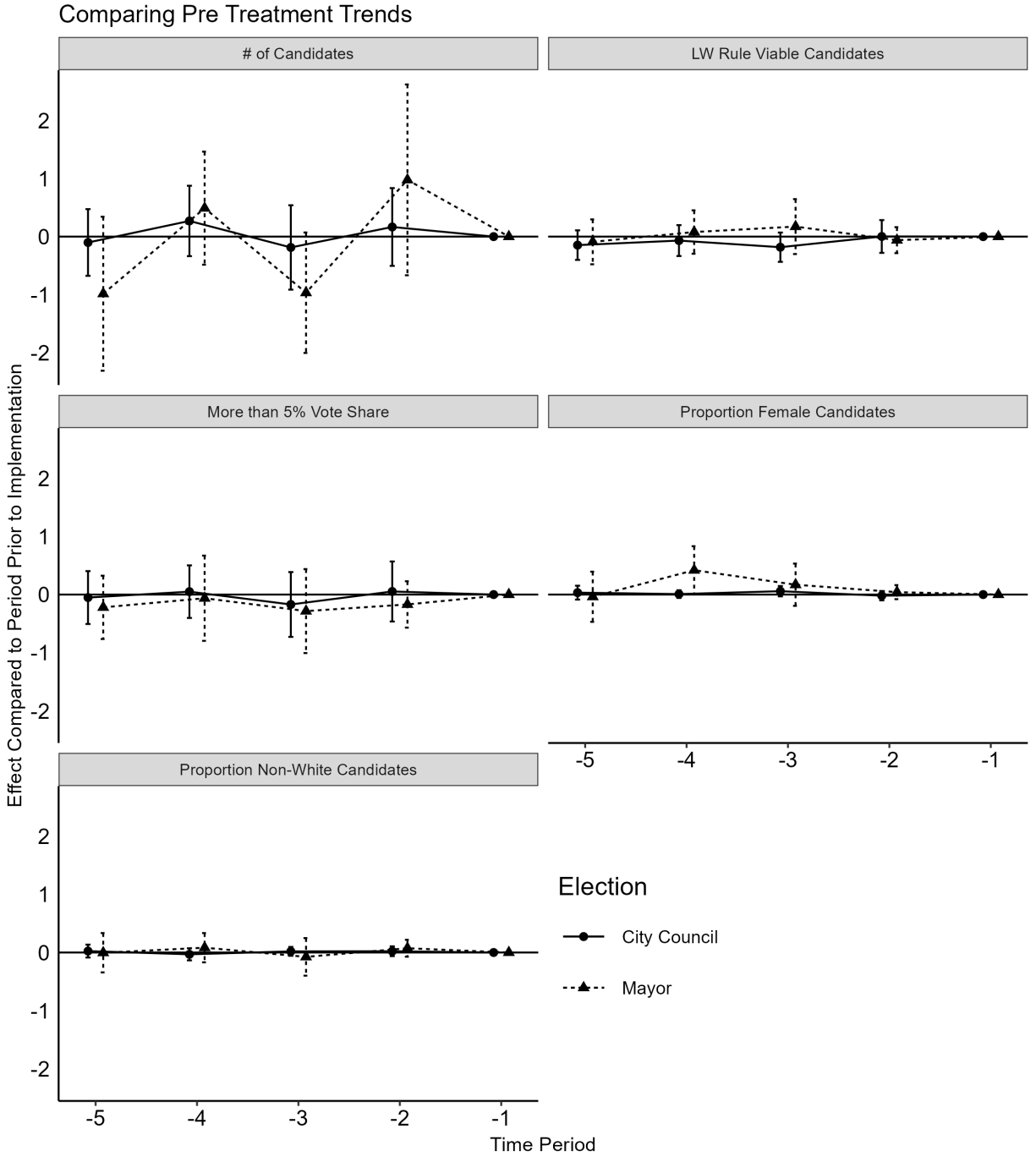


Figure 1: Shows the treatment coefficients for DiD model with leads across mayoral and city council elections for all five outcome variables of interest. Time period -1 (immediately prior to true RCV implementation) is used as the reference period that all other time periods are compared to.

	Mayor (1)	City Council (2)
RCV Implementation × RCV Cities	2.125*** (0.6647)	1.215** (0.4525)
RCV Implementation	-0.0417 (0.1187)	-0.0570 (0.0785)
RCV Cities	0.9228*** (0.3054)	0.3148 (0.3223)
Incumbent	-1.005*** (0.2431)	-0.3092 (0.2180)
At-Large and District		-0.4069 (0.3468)
District Only		-0.6887** (0.2820)
# of Seats Open		1.636*** (0.2439)
Observations	292	1,135
R ²	0.43253	0.49795
Within R ²	0.23951	0.36015
Matched Group fixed effects	✓	✓

Table 2: Number of Candidates by Election Type

the effect of RCV immediately following implementation. However, this effect may dissipate if excitement over RCV fades, or it may grow as political elites adapt to the new system. To measure how RCV continues to affect the candidate pool size following that first election, I modify the standard DiD model to include post-treatment lags. These post-treatment lags, much like the pre-treatment leads but looking forwards rather than backwards, will demonstrate the treatment effect of RCV in multiple periods post-treatment. I estimate this post-treatment DiD model with lags using the equation:

$$y_{it} = \sum_{T=-1}^5 \beta_1 x \text{Time Period} + \beta_2 x \text{RCV Cities} + \sum_{T=-1}^5 \beta_3 x \text{Time Period} x \text{RCV Cities} + \sum \beta_i X_i + \sigma_c + \epsilon \quad (3)$$

The only difference between equation 3 and equation 2 is the time periods included. Time period -1 is still the baseline, however the other time periods are all post rather than pre-implementation. In Figure 2, I plot the treatment effect for each time period following implementation for mayoral and city council races. If the treatment effect is constant, the figure would show consistent and significant coefficients across each time period.

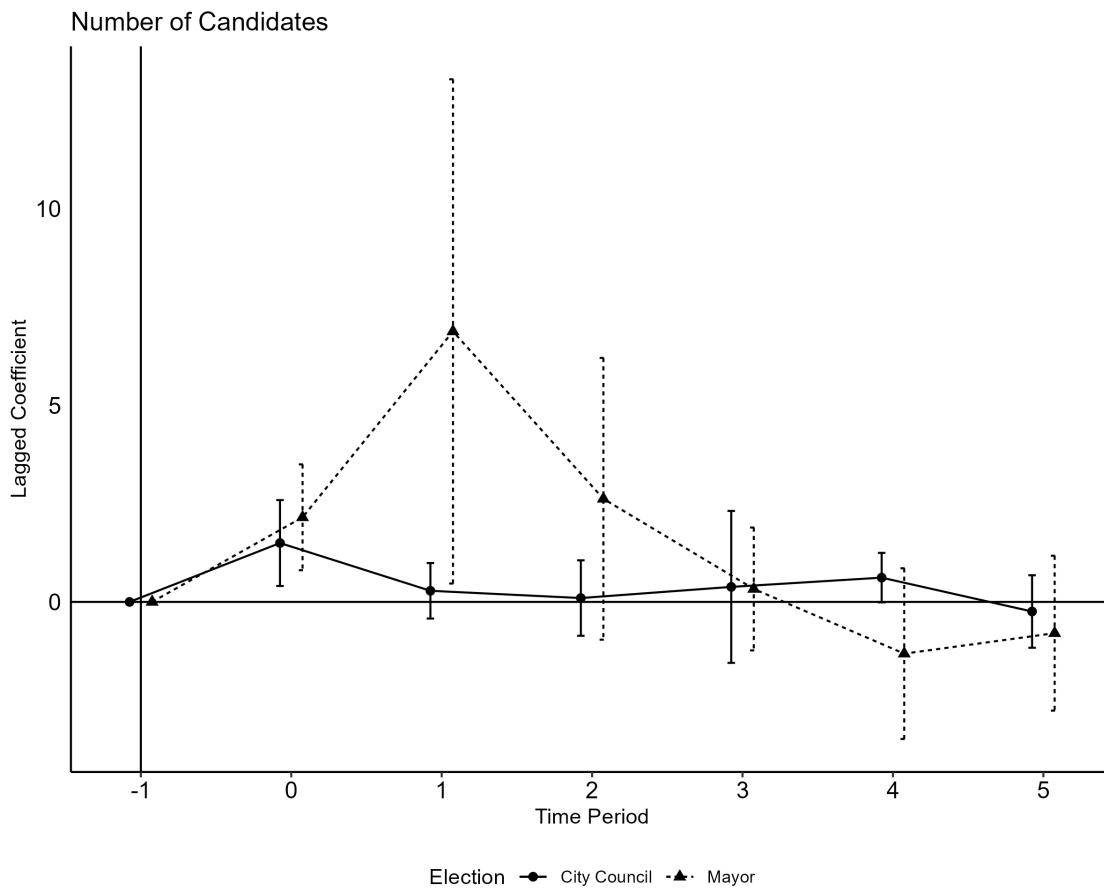


Figure 2: Shows the treatment coefficients for DiD model with lags across mayoral and city council elections. Time period -1 (immediately prior to true RCV implementation) is used as the reference period. City council elections are the solid lines with circles, while mayoral elections are the dotted lines with triangles.

In Figure 2, the positive and significant effects for mayoral and city council elections from

Table 2 appear in time period 0, the first time period following RCV implementation. Looking at city council races (the solid line), no time period besides the first has a significant effect, a trend caused by the decline in the effect of RCV on candidate number as time progresses. This result indicates that, while RCV implementation leads to an increase in the number of candidates, it only does so in the short-term. Mayoral elections follow a similar pattern, though the second time period post implementation also shows a positive effect. Following time period 1 however, there is again a decline in both the substantive effect and its significance, and by time period 4 the effect size is below zero. For both types of elections, RCV has only a transitory effect on the number of candidates.

3.3 Candidate Pool Composition: Quality

More candidates offer voters more choices, however, if the additional candidates running are low quality, voters gain little. Because RCV is a more complex voting system, adding low quality candidates to a race could increase confusion among voters. Thus, the type of candidate RCV encourages to run is just as important as whether RCV encourages more candidates overall.

Table 3 shows the two-period DiD results for the two measures of viability in city council and mayoral elections. Starting with RCV's effect on the number of candidates receiving at least 5% of the vote, there is a substantively large and statistically significant effect for both types of elections. For city council elections, there are approximately 0.73 more candidates who receive over 5% of the vote, while mayoral elections see a 0.54 increase. These effect sizes are substantively smaller than the increase in the raw number of candidates, which we would expect to see if some of the new candidates are low quality. While RCV has a positive and significant effect on the number of candidates receiving over 5% of the vote, it does not affect the number of candidates considered viable using the LW rule. For both city council and mayoral elections, the effect of RCV is substantively close to zero and statistically insignificant. The LW rule is a more stringent measure of candidate viability, indicating the increase in the number of candidates

caused by RCV is predominantly leading to bottom tier candidates joining the race rather than candidates who have realistic aims of winning the election. From the raw number, to the 5% rule, to the LW rule, we see less and less of an effect of RCV as the measure of viability becomes more stringent.

	Mayor		City Council	
	More 5% (1)	LW Rule (2)	More 5% (3)	LW Rule (4)
RCV Implementation × RCV Cities	0.5410*	0.0590	0.7315**	-0.0503
	(0.2951)	(0.1660)	(0.2699)	(0.1205)
RCV Implementation	0.0401	0.0134	-0.0295	0.0461
	(0.0662)	(0.0729)	(0.0514)	(0.0564)
RCV Cities	0.5467**	-0.0666	0.1533	-0.0493
	(0.2206)	(0.1352)	(0.1903)	(0.1066)
Incumbent	-0.6169***	-0.2954***	-0.1820	0.0191
	(0.1286)	(0.0863)	(0.1434)	(0.0846)
At-Large and District			-0.1587	-0.0643
			(0.2170)	(0.1425)
District Only			-0.5009**	-0.1305
			(0.1908)	(0.1425)
# of Seats Open			1.369***	1.194***
			(0.1795)	(0.1237)
Observations	292	292	1,135	1,135
R ²	0.35749	0.14467	0.54720	0.72253
Within R ²	0.19968	0.05559	0.44479	0.65233
Matched Group fixed effects	✓	✓	✓	✓

Table 3: Quality of Candidates by Election Type

Similar to candidate number, I can examine the long-term effects of RCV on candidate quality. While RCV may not affect the number of viable (LW Rule) candidates in the first period post implementation, it may have a delayed effect. Viable candidates are more strategic in their decision to run for office, and following an electoral rule change there may be some reticence to run. Instead, more strategic (and likely more viable) candidates could wait an election cycle to see how the electoral dynamics have shifted. If this is the case, RCV's effect on the number of

viable candidates may be delayed. To test this, I estimate the post treatment lags using equation 3 with the two quality measures as the outcome variables of interest. In Figure 3, I plot the treatment effect for each time period following implementation.

Starting with the first graph in Figure 3 looking at the more lenient 5% rule, the pattern for city council and mayoral elections matches Figure 2. While the election following RCV implementation shows a significant increase in the number of candidates receiving over 5% of the vote, it is the only time period where the result is significant. By the fifth period, the coefficients for both types of elections drop below zero, though not significantly. It is only in the immediate aftermath of the electoral rule change that RCV has any noticeable effect.

The second graph in the figure confirms RCV has no impact on the number of viable (LW Rule) candidates, either in the first period following implementation or any period thereafter. All the coefficients are close to zero, indicating there is no treatment effect regardless of the length of time examined. Using viability as a proxy measure of candidate quality, the above results show that RCV largely encourages lower quality candidates to run for office, and only does so in the short-term.

As mentioned above, standard practice for measuring candidate quality is to use past government experience (Berkman and Eisenstein 1999; Maestas and Rugeley 2008). This measure is unfortunately unavailable for all the cities in my sample, as most local candidates do not build campaign websites or submit information for voter guides. However, California's unique set of election rules allows me to test this standard measure of candidate quality on a subsample of my data.

Following Anzia and Bernhard, I leverage California's election rules that allow candidates to provide a ballot designation describing their current occupation within a narrow character constraint (Anzia and Bernhard 2022). Using their keyword dictionaries, I generate measures of experience based on the words chosen. From these measures of experience, I use an indicator for whether a candidate has prior experience in government as a new measure of candidate

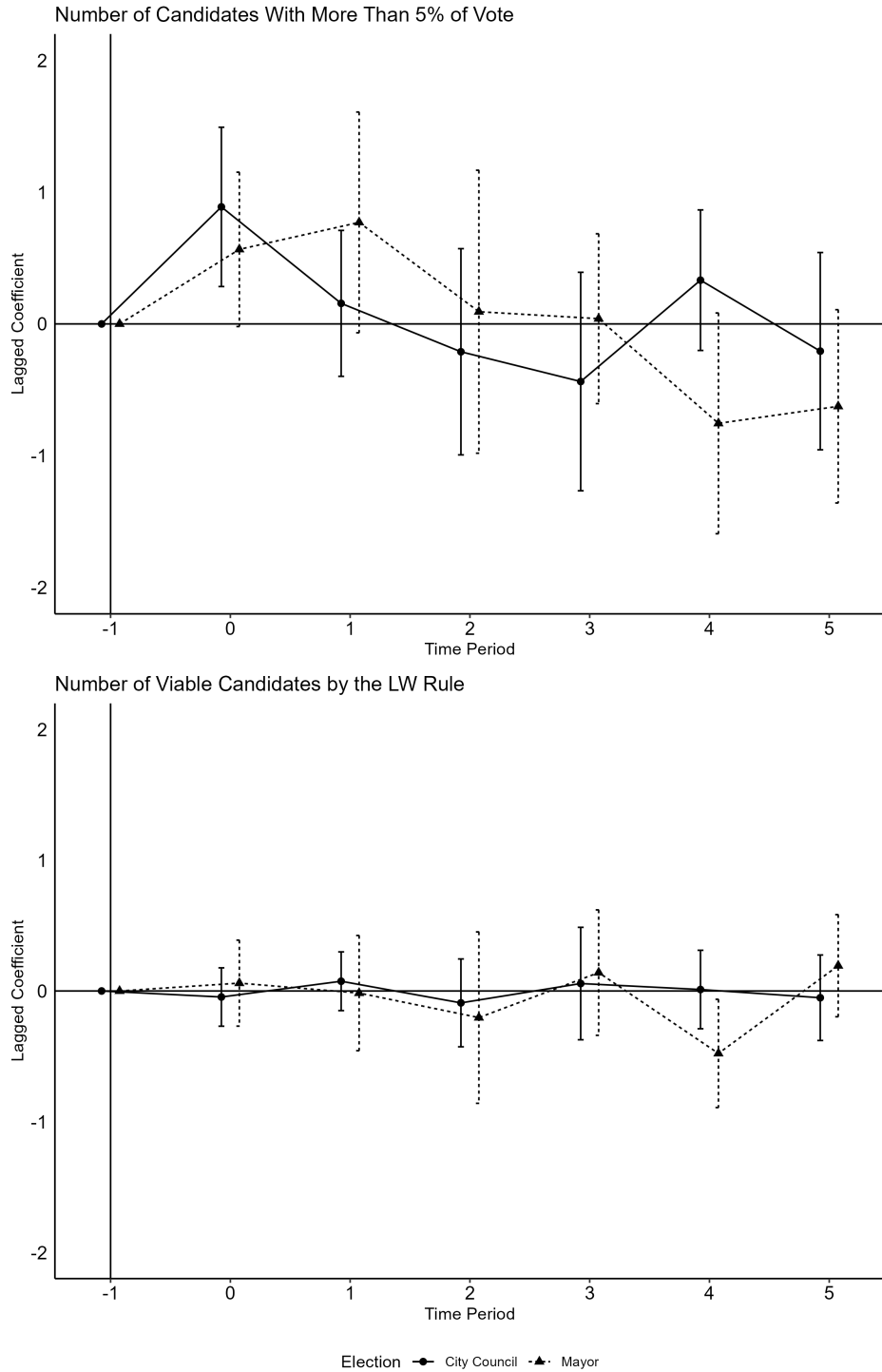


Figure 3: Shows the treatment coefficients for DiD model with lags across mayoral and city council elections. Time period -1 (immediately prior to true RCV implementation) is used as the reference period. City council elections are the solid lines with circles, while mayoral elections are the dotted lines with triangles.

quality.

Because only California has this ballot designation rule standardized across the state, the sample of RCV cities drops to the four located in California (Berkeley, Oakland, San Francisco, and San Leandro). While most of the control cases for these cities are also in California, Corvallis OR and Seattle WA are not. For these two cities I collected voter guides for the elections included in the sample and generated potential ballot designations based on those guides. I then code these potential ballot designations in the same way as the California data.

The data collected for Corvallis and Seattle only include elections immediately before and after implementation rather than for ten years on either side. This precludes an analysis of pre-treatment trends, and thus it is difficult to make a causal claim about the results of any DiD analysis. However, the knowledge that the control and treatment cities demonstrate parallel trends across the other outcome variables suggests these cities display similar electoral trends. Finally, using only four treatment cities and their respective matched control cities reduces the power to identify significant results. That said, a strong positive effect would indicate that the use of viability measures rather than experience measures was a poor choice and cast doubt on the interpretability of those findings. Once again I use a basic difference-in-differences model with two time periods and matched group fixed effects. I show the results of the estimations in Table 4.

For both mayoral and city council elections, there is no positive effect of RCV on the experience level of the candidates who choose to enter the race. Instead, both coefficients of interest are negative, though only the result for city council elections is statistically significant. Rather than suggesting the use of viability as a measure for quality was biasing results to zero, these findings suggest that the use of viability biases results upwards, as even low-quality candidates can be electorally successful.

Across all three different measures of quality, evidence suggests that the increase in candidates immediately following RCV implementation is made up of low quality candidates with lit-

	Mayor (1)	City Council (2)
RCV Implementation × RCV Cities	-0.6003 (0.5367)	-0.3030* (0.1007)
RCV Implementation	0.5875 (0.3590)	0.0199 (0.0638)
RCV Cities	0.9024 (0.4231)	0.1594 (0.2125)
Incumbent	-0.8143 (0.3585)	0.4361** (0.0843)
At-Large and District		0.0809 (0.3025)
District Only		0.0567 (0.1439)
# of Seats Open		0.4270** (0.0934)
Observations	38	150
R ²	0.26308	0.49230
Within R ²	0.26058	0.42232
Matched Group fixed effects	✓	✓

Table 4: Number of Candidates with Government Experience by Election Type

the hope of winning. Furthermore, the increase in low quality candidates is short-term, quickly dissipating after the first two election cycles.

3.4 Candidate Pool Composition: Diversity

Candidates do not need to win to alter the political environment, and can use their platform to bring important issues into the political spotlight. While RCV does not encourage more competitive candidates to run for office, there is still a benefit to encouraging more diverse candidates. Furthermore, the increase in diversity may be RCV supporters' most important claim when it comes to RCV's effect on the candidate pool. Increasing diversity in local elections is important both for the legitimacy of local government as well as for the upstream effects it could

have on descriptive representation at the state and federal level. Regardless of the overall size of the candidate pool, increasing the diversity of the pool would be a significant benefit of RCV.

Table 5 shows results for the two measures of diversity for both city council and mayoral races. Across all four models, the effect of RCV is insignificant both substantively and statistically. While for mayoral elections the coefficients of interest are negative and for city council they are positive, none reach statistical significance. Shown in Appendix G, the lagged treatment effects are also all insignificant, demonstrating there is no delayed effect of RCV on the diversity of the candidate pool.

	Mayor		City Council	
	Proportion Female (1)	Proportion Non-White (2)	Proportion Female (3)	Proportion Non-White (4)
RCV Implementation × RCV Cities	-0.0330 (0.0735)	-0.0482 (0.0777)	0.0340 (0.0358)	0.0565 (0.0458)
RCV Implementation	0.0111 (0.0297)	-0.0049 (0.0400)	-0.0074 (0.0194)	-0.0114 (0.0131)
RCV Cities	0.1145* (0.0593)	0.0512 (0.0690)	0.0469 (0.0305)	0.0657 (0.0429)
Incumbent	-0.0094 (0.0366)	-0.0197 (0.0440)	-0.0115 (0.0132)	-0.0212 (0.0207)
At-Large and District			0.0776** (0.0357)	0.0426 (0.0527)
District Only			0.0230 (0.0408)	0.0099 (0.0564)
# of Seats Open			0.0353*** (0.0109)	0.0167 (0.0296)
Observations	292	292	1,133	1,133
R ²	0.13396	0.13037	0.07350	0.11823
Within R ²	0.02762	0.00301	0.01503	0.01634
Matched Group fixed effects	✓	✓	✓	✓

Table 5: Proportion of Female or non-White Candidates by Election Type

There are several explanations for why these results may occur. First, elections in RCV cities

may experience a ceiling effect on the diversity of candidates. The third row of coefficients shows that cities choosing to implement RCV may already have a slightly more diverse candidate pool, and thus any effect of RCV could be smaller in those cities compared to if a random city switched to RCV. A ceiling effect would be more prominent in mayoral elections, as they directly mirror the electorate that implemented RCV. A second explanation is that the initial increase in the candidate pool is so large that it drowns out an increase in the diversity. Using a proportion instead of a raw count would be susceptible to this, and in Appendix E I show that this is the case. Whether more diverse candidates in a less diverse candidate pool is a positive is debatable, but RCV does not encourage non-White or female candidates any more than it encourages white male candidates.

Numerous articles and policy briefs published by proponents of RCV have found increases in diversity following RCV implementation. However, these studies typically focus on one city, and employ a simple before-and-after comparison. Using a more comprehensive and causally identified model, I find that these studies should not be taken as evidence that RCV causes a more diverse candidate pool. Instead, RCV appears to have no consistent effect on candidate pool diversity, either in the short-term or long-term.

4 Mechanisms

In the previous sections, I find that RCV's impact on the candidate pool in local elections is marginal and fleeting. While RCV leads to an immediate increase in the number of candidates, the effect dissipates. Furthermore, the temporary increase is among lower quality candidates; the candidates encouraged to run are not serious challengers. Finally, RCV does not induce a more diverse candidate pool in the short- or long-term. To examine further why the purported benefits of RCV for the candidate pool are not realized, I examine several of the mechanisms that could have caused the changes claimed by RCV proponents. Above, I point to lower cam-

campaign costs and fewer incentives for political elites to restrict entry as reasons for a larger candidate pool. Below, I focus on each in turn to determine if the conditions necessary for RCV to affect the candidate pool are present in only a subset of the cities.

4.1 Campaign Costs

While RCV makes elections cheaper by eliminating run-offs, it may only impact candidate entry decisions in cities with expensive elections. Small cities that implemented RCV may have elections mostly conducted door-to-door, reducing the cost considerations. Alternatively, it may be that smaller cities are where cost matters most, as larger cities have expensive campaigns regardless. To test if RCV only increases candidate entry in small or large cities, I separate my data in two groups based on RCV city population. If RCV only has an effect in small cities, then there may be a sustained positive effect in that sub-sample that was hidden when combined with larger cities. The alternative may also be true, and the effect may only appear in larger cities. In Figure 4 I separate the treated cities using a 100 thousand population cutoff and rerun the analysis on candidate number. This population cutoff is substantively arbitrary, but provides a balanced comparison of cities.

For city council and mayoral elections in small and large cities, there is no consistent effect beyond the first time period. Similar to the main results, any significant effect occurs in the first period of implementation, followed by a drop-off in subsequent periods. Though these results are noisier due to the smaller sample sizes, the interpretation is the same. In neither small nor large cities do we see any long-term effect of RCV on candidate entry. This means, using city size as a proxy for campaign costs, that in both high and low cost campaigns the effect of RCV is only in the short term. One potential explanation is that local elections may not vary when it comes to runoff elections. In smaller cities, runoff elections may be rare even prior to RCV. In larger cities, the significant cost of running a campaign, even if only once, may already restrict the candidate pool to a select few. If this is the case, then RCV's impact on the cost of the campaign

may be negligible, leading to the lack of any effect on candidate entry.

4.2 Constrained Coalitions

Alternatively, eliminating the spoiler effect may only be important in cities with coalitions that have constrained candidate entry. In these cities, political elites may use the opportunity provided by RCV to allow a backlog of interested candidates to run for office. Because the data collected here is not detailed enough for a comprehensive analysis of political coalitions, I use the number of Black and Latino candidates as a proxy. Racial divisions are one of the few durable factors identified in local elections, and many argue that local elections are largely a competition between racial groups over local resources (Hajnal and Trounstein 2014). It may be that only cities with coalitions strong enough to limit candidate entry will benefit from the elimination of the spoiler effect.

As a proxy, I limit the data to treated cities that had only one Black candidate and only one Latino candidate in the election prior to RCV. I use this rather than the size of the Black and Latino populations because the mechanism focuses on constrained coalitions, not large ones. A large minority population may not act as a single coalition, while a small minority population might not be politically organized. Instead, RCV should affect the cities with coalitions built around race that are both well-structured and self-constrained. While not the perfect measure of either coalitional strength or a backlog of candidates, using the number of Black and Latino candidates in the election prior to RCV narrows the cities to those we would expect have the most potential to experience an expansion in the candidate pool.

In Figure 5 I run separate models for the RCV cities that had one Black candidate and one Latino candidate prior to implementation. For constrained coalitions of Black voters, we see no long-term increase in the number of candidates following RCV. Similar to the main model, after a slight increase in the first period post-implementation, the effect dissipates, leading to a negative effect by the final period in the sample. For the constrained coalitions of Latino voters,

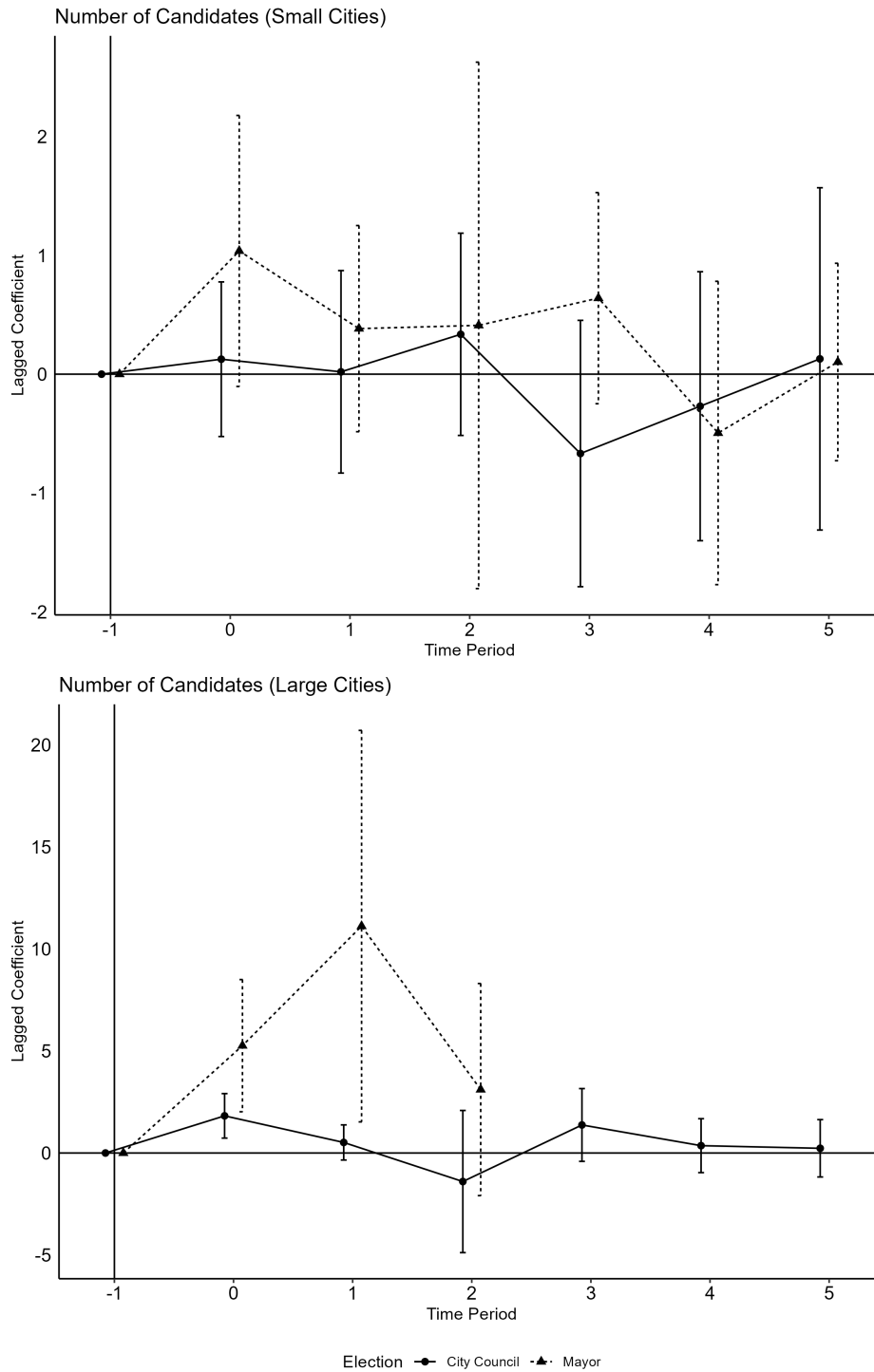


Figure 4: Shows the treatment coefficients for DiD model with lags across mayoral and city council elections. Time period -1 (immediately prior to true RCV implementation) is used as the reference period that all other time periods are compared to. City council elections are the solid lines with circles, while mayoral elections are the dotted lines with triangles.

there is similarly no increase. These results demonstrate that RCV does not increase the number of candidates by relaxing political elites' control over candidate entry.

4.3 Candidate Pipeline

RCV does not affect the size, quality, or diversity of the candidate pool in the long term. Looking beyond the causal mechanisms examined in this study, one explanation for this is that RCV's impact occurs too late in the candidate recruitment and entry pipeline. Research on municipal candidate emergence is still lacking, however we do know recruitment continues to play a prominent role, with descriptive surveys of mayoral candidates suggesting that most are recruited to some extent (Carroll and Sanbonmatsu 2010). Furthermore, many candidates who were not formally recruited acknowledge that informal recruitment networks encouraged them to run. Recruitment is even more important for increasing diversity, as women are more likely to need recruitment to consider running for office (Butler and Preece 2016; Crowder-Meyer 2013).

From the perspective of political recruiters, it is unlikely RCV impacts recruitment activity. Reducing the spoiler effect does not mean more candidates from the same coalition are preferable. Multiple recruited candidates requires splitting political resources and potentially sows confusion among supporters. While the correct ordering of candidates should eliminate the spoiler effect, whether voters use their rankings to do so is still unclear (Mcdaniel 2016; Burnett and Kogan 2015). Suggestive evidence points to political organizations maintaining their endorsement strategies following RCV implementation. In the 2021 New York City Mayoral Democratic primary (the first to use RCV), nearly every major union, including the Service Employees International Union, the Communications Workers of America Union, the Public Sector Employee Union, and the United Federation of Teachers Union, endorsed only one candidate out of the 13 running. For any candidate that needs to be recruited to run for office, RCV is unlikely to affect whether they run.

If RCV were to affect political gatekeepers, it should occur where there is a backlog of poten-

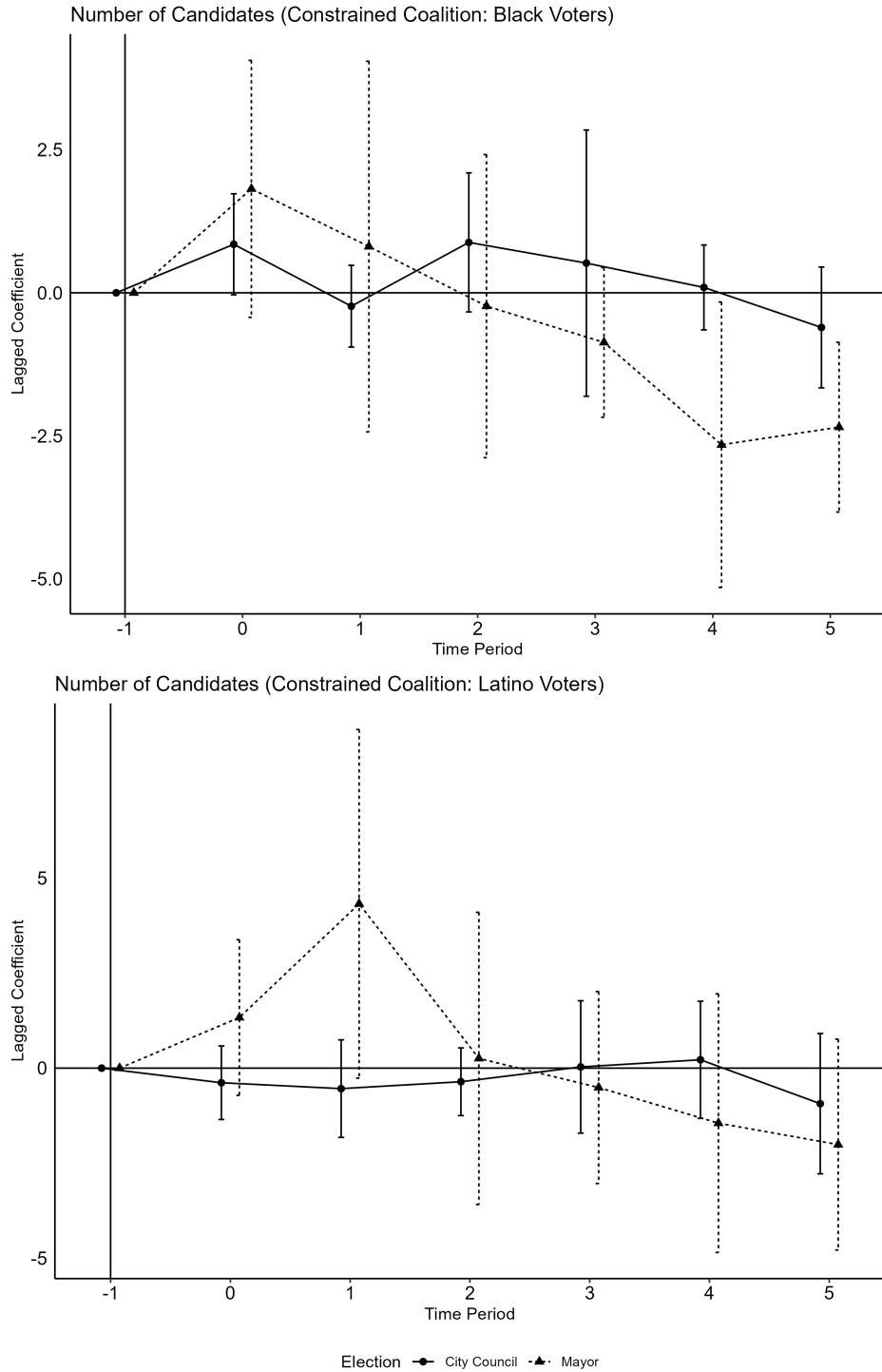


Figure 5: Shows the treatment coefficients for DiD model with lags across mayoral and city council elections. Time period -1 (immediately prior to true RCV implementation) is used as the reference period that all other time periods are compared to. City council elections are the solid lines with circles, while mayoral elections are the dotted lines with triangles.

tial candidates. While at higher levels of office this backlog may be common, at the municipal level it seems unlikely. In my dataset, 25% of mayoral elections and 30% of city council elections were uncontested, and among elections with no incumbents over 15% remained uncontested. Perhaps the most vivid example from my sample is Hoytville, Ohio, a town with fewer than 500 residents that struggles to convince anyone to run for office. Though Hoytville is not representative, it illustrates that rather than a backlog of candidates, cities frequently face a dearth.

For potential candidates encouraged to run by friends and family, it is also unlikely RCV would have an effect. Informal encouragement to run is likely focused on suitability for the job or likelihood of winning, neither of which is influenced by RCV. Though there is some argument that RCV increases the number of avenues for winning, it requires complex strategic planning that is beyond most politically inexperienced citizens. Furthermore, RCV does not change how many seats are up for election, and still requires winning candidates to receive 50% of the vote.

Instead, RCV may only affect candidate entry for those already considering running for office. When deciding whether to run, potential candidates may consider how much money it will cost, or how much negative scrutiny they will face. Once this point in the decision-making process is reached, most potential candidates have already dropped out of the candidate pipeline. While RCV may impact candidates at this late stage, it is unlikely the effect would be large enough to change the size and composition of the candidate pool in any one city.

If RCV has no effect on the candidate pipeline, why is there a surge in candidates immediately following implementation? RCV, as an exciting new voting system that has gained nationwide attention, likely generates media attention in cities that adopt it. Furthermore, these cities likely expect the benefits RCV proponents claim it offers. Both increased media attention and high expectations would increase interest in local elections and cause miscalculations among those considering running for office. Candidates may be unclear how RCV will change the political dynamics, or be overly optimistic that it increases their chances of winning. If RCV implementation acts primarily as a destabilizing factor, then the jump in the candidate pool

size should be temporary. The confusion caused by RCV would lead to an initial change in the political environment, before fading into an insignificant aspect of city politics.

Though I cannot evaluate this explanation of how RCV influences candidate pool composition, there is suggestive evidence supporting it. The city of Minneapolis first implemented RCV in 2009, and following the election conducted a survey among the candidates (Elections & Rules 2018). This same survey was also carried out following the 2013 and 2017 elections, offering a view into candidates' perceptions of RCV. The sample of each survey is small, but provides one of the few views into candidate perceptions beyond position-taking in the media. When asked if RCV had negatively impacted their campaign, candidates gave increasingly negative reviews. In 2009 only 19% of candidates believed it had negatively impacted their campaign, but by 2017 that number increased to 44%. This points to candidates having unreasonably high expectations in the first election cycle, followed by decreasing interest. It is this kind of excitement that could spark an increase in the candidate pool. The individuals most likely to make such a miscalculation are inexperienced candidates, the exact group who see a jump in the candidate pool following RCV implementation.

5 Conclusion

Using a difference-in-differences quasi-experimental design, I examine whether several purported benefits of RCV have been realized in local elections. Contrary to the wide-ranging benefits claimed by RCV supporters, I find little evidence that RCV changes the candidate pool, either in its size, quality, or diversity. Though there is an initial increase in the number of candidates following RCV implementation, the effect quickly dissipates. Furthermore, the candidates who make up this initial increase are of low quality. RCV also has no effect on the diversity of the candidate pool, either for the proportion of female or non-White candidates. Instead, RCV appears to disrupt the political environment immediately following implementation, before quickly be-

coming part of the status quo.

While these results may disappoint RCV's strongest proponents, the evidence presented here focuses only on RCV's effects on candidate entry. RCV has been proven to positively affect the tone of campaigns and potentially increase voter engagement. Furthermore, RCV may lead to diversity in ways that are unmeasured here. For example, RCV could encourage more centrist candidates or certain types of inexperienced candidates who can offer a new perspective on local issues.

For cities and states to serve as laboratories of democracy, "experiments" at the local level must be examined empirically. Though RCV is increasingly popular, this study indicates that our understanding of its costs and benefits is still lacking. To ensure RCV does not become the next innovation to spread quickly only to be slowly rolled back, further research is needed. Future work can build on this data by analyzing whether RCV encourages more centrist or extreme candidates to run for office. Additionally, the spread of RCV to other levels of government offers an opportunity to examine whether the results found here generalize to more visible elections. Finally, as RCV becomes more common and entrenched in local governments, the longer-term effects of RCV on the candidate pool will become easier to assess.

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