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Carrie Gill

Corey Lang

Shanna Pearson-Merkowitz

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THE AFTERMATH OF BALLOT BOX SUCCESS AND FAILURE: EVIDENCE FROM LAND PRESERVATION REFERENDUMS

Carrie Gill
Administrator, Rhode Island Office of Energy Resources
cannegill@gmail.com

Corey Lang
Professor, Environmental and Natural Resource Economics
University of Rhode Island
clang@uri.edu

Shanna Pearson-Merkowitz[1]
Professor, School of Public Policy
University of Maryland, College Park
spearson@umd.edu

Abstract:
State and local governments put hundreds of referendums on the ballot each year. Often, they pass but sometimes they fail. What happens after a successful or failed attempt at the ballot box? Do advocates go back to voters with another request? And if they do, do they tend to succeed? We employ a regression discontinuity empirical framework to causally estimate referendum dynamics in the arena of land conservation. Our results suggest municipalities where a referendum just barely fails hold about 0.5 more referendums and pass about 0.28 more referendums than municipalities that just barely pass, meaning initial defeat is often reversed. We also investigate whether strategic changes are made in election approaches for those that try again. We find no evidence of systematic patterns in strategic revisions for municipalities that fail their first referendum. However, when revisions are made, our evidence suggests that voters appear to respond positively.

Keywords: government bonds, regression discontinuity, open space, voter referendum, direct democracy, land preservation

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[1] Corresponding author. Authors are listed in alphabetical order. Earlier versions of this research were presented at the 2015 annual meeting of the Northeast Agricultural and Resource Economics Association and at the University of Rhode Island and the 2016 State Politics and Policy Conference, May 19-21, 2016. Dallas, TX. This work was supported by the USDA National Institute of Food and Agriculture, Agricultural and Food Research Initiative Competitive Program, Agriculture Economics and Rural Communities, grant number: 2015-67024-22937. The authors declare no competing interests.

INTRODUCTION

Local governments in the United States are often constitutionally bound to ask voters for approval before issuing government debt or raising taxes. These additional resources pay for critical infrastructure for schools, land conservation, watershed cleanup, roads, and even municipal waste treatment. However, voters may not always approve of the projects or funding priorities their elected or appointed leaders have deemed worthy enough to place on the ballot. For example, in 2018 alone state governments placed bond projects on statewide ballots worth $21.1 billion, but voters only approved $12.25 billion in bonds to be purchased. In 2022, voters in 37 states voted on 132 statewide ballot measures that included both bond projects, taxes, and other ballot questions: Voters approved 90 and rejected 42.

What happens after a referendum fails? Do those who advocated for the project or policy simply give up on the project, or do they return to the voters and ask for approval again? Likewise, what happens after a successful referendum? Do advocates and legislators take the success as a cue that they can return to the voters for more? Particularly in the area of referendums for funding (bonds and dedicated taxes), are advocates revenue maximizers who will continue to return to voters for as much as they are willing to approve and ask them again in the face of defeat? While a literature exists looking at the import of the wording of referendum titles and descriptions, and the length and ordering of questions on a ballot (e.g. Augenblick and Nicholson 2016; Brunner, Robbins, and Simonsen 2021; Matsusaka 2018), investigating the real-world consequences of failed referendums (Kogan et al. 2017), as well as a large literature on ballot referendums writ large (Dyck and Lascher 2019; Matsusaka 2008; Smith and Tolbert 2009), no quantitative causal studies have investigated the actions of advocates and policy makers after a referendum failure or passage and the outcomes of these second attempts.

We employ a regression discontinuity empirical framework to estimate causal effects of referendum outcomes. We use a comprehensive dataset of U.S. municipal-level land preservation referendums from 1988-2012, which allows us to perform a large-N analysis that captures the presence of multiple proposals in a single venue. We find that municipalities that fail an initial referendum tend to put the issue back on the ballot soon after, and that second attempts are successful about half the time. We also explore revisions to referendum characteristics for the subset of municipalities that hold multiple referendums and test the efficacy of these revisions. While we find no statistical evidence of systematic strategic revisions to subsequent referendums for municipalities that fail at their first attempt, when revisions are made, voters appear to respond positively.

POLICY ENTREPRENEURS AND THE LOCAL BALLOT BOX AND VOTER RESPONSE

Political science and public policy literatures refer to advocates for policy change who make strategic decisions about how to pursue their policy goals as "policy entrepreneurs" (PEs). While there is a rich literature on PE decisions about venue choice (e.g. the courts, the legislature, or the ballot box) and level of government they pursue (e.g. Congress, the state, or a locale) (Ley 2016; Ley and Weber 2015; Mintrom and Norman 2009, 2009; Petridou and Mintrom 2021; Roberts and King 1991; Weissert 1991), little work investigates how PEs react to succeeding or failing in their quests at the ballot box. Understanding these dynamics provides politically relevant insight about how to proceed after a successful or defeated attempt at policy change.

Kingdon (1994) notes that PEs can be "in or out of government, in elected or appointed positions, in interest groups or research organizations. But their defining characteristic… is their

willingness to invest their resources—time, energy, reputation, and sometimes money—in the hope of a future return" (p. 122). PEs may take the form of groups of people or of individuals either inside or outside a community (Mintrom and Norman 2009).

PEs also choose a venue in which to seek their proposed policy change, a process that involves weighing the benefits and drawbacks of different venue options (Baumgartner and Jones 2010), including which venue gives the greatest likelihood of success and associated costs of pursuing policy change. PEs may also seek to make changes in multiple venues at a time (Burns 1994; Gray and Lowery 2000) or use public access venues (like the courts or the voter initiative) when legislatures fail to act on issues entrepreneurs desire (Bowler and Donovan 2004; Gerber et al. 1998).

In this paper we investigate PE behavior using referendums because referendums only appear on the ballot as a result of efforts made by actors who wish the local government to use financial resources to preserve or change the status quo.[1] As a result, the policy actors involved in getting referendums onto the ballot and working to gain enough community support to gain passage are precisely the type of PEs that Kingdon and Mintrom and Norman describe. In order for a referendum to be placed on the ballot, a community member, elected or appointed official, or group must successfully push the local legislature to place it on the ballot.

As a form of direct democracy, local referendums present an important way in which residents can inform policy and signal their opinions about the importance of an issue, opinions that may differ from local developers or planners on policies that are close to home (Bowler and Donovan 2004). Seeking policy change at the local level also has some advantages for PEs as there may be a more manageable number of hurdles and actors involved in the process. Even in terms of voter support, there are fewer people in a smaller geographic area than if the policy

change were made at the state level. Changes at the local government level, if successful in several locales, can also increase the rate at which a state is likely to adopt a policy through a "trickle up" or "vertical diffusion" process (Karch and Rosenthal 2016). However, local governments also have important constraints: they may not have the authority to regulate all policy areas and they have very few options and considerable limitations on their ability to fund new programs.

Do Policy Entrepreneurs Reintroduce Referendums on Future Ballots after a Ballot Success or Failure?

In any given venue, PEs can decide to try again either when they have succeeded or when they have failed. For example, bills in legislatures rarely pass the first year they are introduced. Bills are regularly reintroduced until a policy window opens (e.g. Kathlene, Clarke, and Fox 1991). Furthermore, PEs may change their proposals over time, so they better fit into a particular policy window or problem definition (Kingdon 1994).

Banaszak (1996) documents the process of the Swiss and US fights for women's suffrage and finds that even after a failure at the ballot box, PEs worked to get the issue back on the ballot in an attempt to gain eventual passage of full women's suffrage. Further, PEs in Sweden made tactical decisions about when to hold a second referendum and were strategic regarding the wording of the second referendum following an initial failure at the ballot box (Banaszak 1996). Recent research has found that communities that reject referendums often vote on them again and this dynamic must be accounted for when assessing the impacts of public spending on housing prices and other outcomes (Cellini et al. 2010, Kogan et al. 2017, Lang 2018). Following this logic, we hypothesize that PEs will continue their agenda of policy change following either

an initial defeat or success in the same venue.

Indeed, referendum elections are quite different than candidate elections or a normal process of budgeting where tradeoffs must be made. In referendums, voters are asked simply to "up or down" a single spending item. As a result, they cannot weigh tradeoffs or compare two different scenarios (or candidates) and hold out for their preferred distribution of goods and taxes (e.g. Romer and Rosenthal 1978). Advocates are likely "revenue-maximizers" for their cause—they wish to have the maximum resources that voters and legislators will devote to their preferred spending area. Policy entrepreneurs with access to the ballot box, should push to have the referendum appear again following a failure or following an overwhelming success. Thus, the likelihood of a subsequent referendum could be a continuous function of the vote margin (the proportion of yes votes at the ballot box minus the proportion required for passage), meaning the outcome of the referendum does not have an effect on holding subsequent referendums (null hypothesis). Alternatively, proponents of referendums that just barely failed may be discouraged from holding subsequent referendums in the face of failure and either move venues or discontinue their efforts. However, the reverse could also be true; proponents that just barely failed may be more likely to try again. PEs may choose to continue within a chosen venue either because it is the only venue that is able to make their desired change, other venues are not available for political or financial reasons, or because they have already invested resources in the chosen venue and continuing on is less costly than changing venues (Buffardi, Pekkanen, and Smith 2015). Thus, we hypothesize that proponents will continue their agenda of policy change following an initial defeat by holding a second referendum soon after their focal election when the election is close.

However, when focal referendums either succeed or fail by large margins, the logic

changes. When a focal referendum fails by a large margin, it is unlikely that a subsequent referendum would be held in the near future because proponents will likely perceive their cause as lost or decide a different venue may be more fruitful. Similarly, those that pass by a large margin may be very likely to hold a subsequent referendum, as proponents see there is widespread support for the issue.

Voter Response to Repeated Referendums

How do voters respond to the presence of a referendum topic reappearing on a future ballot? One the one hand, voter knowledge is quite low in general, and local elections have low salience and low voter turnout in general. Voters may not understand what they are voting on, particularly when it comes to referendums with costs associated. Recent evidence suggests that voters have little understanding of the relationship between tax increases presented on the ballot and their actual tax bills (Cozza, Elkins, and Hudson 2021; Lang et al. 2022) and that aggregate bond costs have very little impact on voter support (Bechard, Lang, and Pearson-Merkowitz Forthcoming). Furthermore, several studies have documented status quo bias in referendum voting (Bowler and Donovan 2000; Donovan and Bowler 1998; Morisi 2018) and valence voting (e.g. a gut "yes") (Cozza, Elkins, and Hudson 2021; Dyck and Pearson-Merkowitz 2019). While previous literature has found that the information environment is critical to referendum passage (Bowler and Donovan 2000; Lupia 1994), most of these studies focus on state level ballot questions in which there is considerable press coverage. Given the low level of information and salience of local referendums, a failed referendum could be taken as a sign of low voter knowledge or a function of who turned out that day, possibly driven by something else on the ballot or other random or non-random factors. Returning to the ballot may allow

advocates to engage in community outreach and build support for the project. Thus, we hypothesize that on average voter support will increase in each subsequent election compared to the focal referendum.

Do PEs modify referendums after a focal election?

We also perform three exploratory analyses to investigate potential ways PEs may change the content of a referendum: the dollar amount proposed, the funding structure, and the election date. Each of these is exploratory and we do not have directional hypotheses that drive expectations.

First, proponents who just barely fail in their focal referendums could reduce proposed funding in a second election, as a smaller price tag may be thought to be more acceptable to voters. On the other hand, those who experience success in a focal referendum may increase the proposed amount of a subsequent referendum depending on how much surplus voter support existed in the first referendum.[2] Of course, in many cases costs are fixed. For example, in the area of land preservation, the cost of purchasing a farm to conserve cannot be adjusted—the jurisdiction must either purchase it at the cost the owners are willing to sell, or not purchase it. As a result, we are agnostic about the likelihood that PEs will (or can) go this route.

Second, PEs may seek to alter the date of the referendum to either get more voters by moving the referendum to the date of a well-attended major election or increase the proportion of special interest voters and advocates by moving the referendum to the date of a less attended special election (e.g. Anzia 2011; Berry and Gersen 2010; but see Kogan, Lavertu, and Peskowitz 2018). Meredith (2009) finds that agenda-setters with power over the timing of a referendum will schedule referendums when other races on the ballot will attract voters that will

support the measure. However, given that different election cycles exist in different locales, and that if an off-cycle or on-cycle election is beneficial to the policy entrepreneur's cause is likely context and issue specific, we are hesitant to expect uniform results. Some issues may benefit from a larger population voting whereas others may benefit from a smaller population voting depending on the nature of the ideology of the area, and the ideological distance between the PEs, the active opponents, and the median voter (e.g. Dynes, Hartney, and Hayes 2021).

Third, advocates could attempt an alternative financing mechanism that is more favorable to voters after a failure. In particular, given the unpopularity of tax increases, one might expect proposals that initially asked for a tax but failed, to be more likely to change to proposing a bond to increase support. But for those that have an initial success with a bond referendum, they may be motivated to attempt to secure a permanent fund through the passage of a tax. However, again, this strategic change may not be possible in all instances. If the policy entrepreneur is seeking the purchase of a large farm, bond financing is more appropriate than tax funding as a large sum is needed at one point in time, but no money is needed thereafter. As a result, we do not have preconceived hypotheses regarding systemic changes in this regard.

How do voters respond to changes to the referendum?

Lastly, we explore what effect these changes, when they do appear, have on voter support. We propose PEs generally understand how voters respond to referendum characteristics in their area and leverage their experience to modify subsequent referendums accordingly. Also, we suspect issues that appear on the ballot in repeat elections have a natural upward slope of support as voters get used to voting yes. Thus, we expect any changes to positively affect the election outcome and the vote margin.

DATA AND METHODS

In order to test our hypotheses, we use data from municipal referendums for open space preservation following a large literature on open space preservation proposals on state and local ballots (Altonji, Lang, and Puggioni 2016; Banzhaf, Oates, and Sanchirico 2010; Heintzelman, Walsh, and Grzeskowiak 2013, 2013; Kotchen and Powers 2006; Lang and Pearson-Merkowitz 2022; Lang, Prendergast, and Pearson-Merkowitz 2018; Lowry 2018; Lowry and Scott Krummenacher 2017; Nelson, Uwasu, and Polasky 2007; Pearson-Merkowitz and Lang 2020; Prendergast, Pearson-Merkowitz, and Lang 2019; Sundberg 2006). We use data from the Trust for Public Land's *LandVote* database (TPL). The database is a comprehensive listing of state, county, special district, and municipal-level referendums to acquire and/or preserve land since 1988. For each referendum, TPL reports a description of the referendum, jurisdiction, total proposed funding of the ballot measure, total approved funding, proposed conservation-specific funding for land acquisition and protection, approved conservation funding, financing mechanism, date of referendum, vote proportions for and against, as well as descriptive notes. This dataset has two major benefits. First it is the only comprehensive dataset of bond referendums currently in existence. Second, a large literature exists on the determinants of voting outcomes for open space preservation proposals on state and local ballots that helps inform our control variables and expectations (e.g., Prendergast et al. 2019; Pearson-Merkowitz and Lang 2020; Altonji et al. 2016; Heintzelman et al. 2013; Banzhaf et al. 2010; Kotchen and Powers 2006). In the coterminous United States, from 1988 to 2012, 1,031 municipalities held 1,640 referendums to acquire and preserve open spaces. Of the municipalities in our dataset, 351 held more than one referendum. See Tables A1 and A2 for a breakdown of sample referendums by year and by state.

We test our first hypotheses using a regression discontinuity (RD) design, which allows for causal inference (Imbens and Lemieux 2008). Generally, treatment effects are estimated by comparing outcomes for treated units to control units. The problem with this simplistic method is that there are unobserved determinants of outcomes that can bias estimated treatment effects. RD improves inference by effectively comparing observations in a range close to the treatment threshold, that is, observations that are just barely treated or just barely not treated. By comparing observations that are close to the threshold, treatment status can be assumed as good as random. RD exploits this quasi-random treatment assignment to identify the casual effect.

In our application, we compare municipalities that have barely passed versus barely failed a referendum and test for differences in future referendum behavior. We model the number of subsequent referendums following a first referendum as a function the first referendum's vote margin, which is the running variable. We test for a discontinuity in number of subsequent referendums around a threshold of a zero-vote margin,[3] representing a referendum that just barely passes or fails.

[Table 1 about here]

The first critical assumption of RD is that the potential outcomes of observations within this neighborhood are continuous. Therefore, any discontinuity in outcome across the threshold can be attributed directly to the outcome of the first referendum, instead of to other confounding factors that may exist over the entire range of vote margins as would be the case if we employed OLS. We cannot test the assumption of continuous potential outcomes directly because we only observe one voting outcome for each referendum, but we can indirectly assess the assumption by comparing observable characteristics for municipalities that pass or fail. If there is balance between these groups, then our assumption is plausible. We matched municipalities to

socioeconomic data in the 2000 U.S. Decennial Census and present summary statistics for all municipalities in our dataset (Table 1, Column 1), municipalities that pass at least one referendum (Column 2) and that fail at least one referendum (Column 3), and municipalities that pass and fail with a vote margin within five percent (Columns 5 and 6, respectively), as well as t-statistics for the difference of means (Columns 4 and 7). Municipalities in our dataset have similar socioeconomic characteristics regardless of success or failure at the ballot box, especially those that have vote margins within five percent of passing. These similarities support the use of our regression discontinuity framework; municipalities that hold referendums with vote margins near the cutoff point are statistically similar in observable characteristics and are likely to be similar in unobserved characteristics as well. In addition, we compare socioeconomic and referendum characteristics using an RD framework, both graphically and using regression (See Figure A1 and Table A4 in the Online Appendix). These analyses showed that socioeconomic and referendum characteristics are generally continuous in vote margin, supporting our assumption that any discontinuity in number of future referendums at the threshold is due to success or failure of a first referendum rather than another factor.

The second critical assumption of RD designs is that there is no manipulation of the running variable. The fact that many thousands of people vote on each of these referendums suggests that manipulation of the outcome is quite difficult. That being said, we still investigate this possibility rigorously. Figure A2 in the Online Appendix presents a density plot of every sample municipality's first referendum, which forms the running variable. The data are noisy, but do not indicate a clear discontinuity at the threshold. As a second and more formal test, we employ the Cattaneo et al. (2018) test of smoothness of the running variable's density around the threshold. The p-value for the hypothesis of no discontinuity at the threshold is 0.571, indicating

no statistical difference. Thus, this evidence suggests that there is no manipulation of the running variable.

Using a regression discontinuity framework, Cellini et al. (2010) find that districts that fail an initial school bond measure are more likely to pass a subsequent measure. If we were to simply model the number of subsequent referendums as a function of success or failure, then our estimates would compare municipalities that fail by a large margin to those that pass with a large margin, which are arguably very different in both observable and unobservable characteristics (likely introducing selection bias). The benefit of RD is that we control for vote margin and estimate only the *local* differential effect for ostensibly similar municipalities that barely fail or barely pass referendums. In fact, our empirical analysis compares our RD approach to a more traditional cross-sectional approach and finds results differ by an order of magnitude.

We illustrate our null and alternative hypotheses in the context of an RD framework in Figure 1, which shows patterns that may be observed in the data under the various hypotheses. Figure 1 plots the number of future referendums held as a function of vote margin in the first referendum, with the threshold for passage being a vote margin of zero. Our null hypothesis is represented by the graph on the left. We have given the relationship between vote margin and future referendums an intuitive positive slope (though this is not necessary for our research design), which would suggest that voters self-select into municipalities that match their preferences for open space and future referendums reflect these preferences but are not impacted by the outcome of the first referendum. The middle graph represents H1, where municipalities that just barely fail a first referendum are less likely to hold a future referendum relative to those that pass the first referendum. The discontinuous jump in likelihood of holding a subsequent referendum is apparent at the threshold. The right graph represents H2, where municipalities that

just barely fail a first referendum are more likely to try again by holding future referendums, while those that pass are less likely to hold future referendums.[4]

[Figure 1 about here]

We generate two dependent variables for estimating temporal dynamics, the number of future referendums held and the number of future referendums passed in each municipality in our dataset. The number of future referendums held following a first referendum i by municipality m within τ years of a first referendum is defined by

$$held_{im}^{\tau} = \begin{cases} \sum_{t}^{t+\tau} referendum\_held_{mt} \\ missing\ if\ i\ occured\ within\ \tau\ of\ the\ end\ of\ the\ dataset \end{cases} \quad (1)$$

where $referendum\_held_{mt}$ equals one for each referendum held in municipality m between t and $t + \tau$. If the entire timeframe τ does not fall within the span of the dataset, we treat $held_{im}^{\tau}$ as missing. Because our analysis relies on a count of referendums, we discard observations where the first referendum occurs too close to the end of our dataset to have complete data for the number of future referendums. We likewise generate a dependent variable counting the number of referendums that pass within a timeframe of the first referendum, $passed_{im}^{\tau}$.

We create these variables for timeframes τ of one, three, and five years. We include these timeframes to account for collective local memory of past referendums assuming recent referendums are the more salient than older referendums. Additionally, each timeframe may capture towns with different constraints. While some towns may be able to schedule a repeat election within one year, others may be financially or constitutionally constrained to the next normal election.[5]

For illustration purposes, consider the example of Park Ridge Borough, NJ. Park Ridge held referendums in 2003, 2009, 2010, and 2012. We consider each of these referendums as the first referendum, in turn, and calculate the number of subsequent referendums held. When the

2003 referendum is considered the first referendum, the number of referendums held within one, three, and five years are all zero since the next referendum held was in 2009, six years past the date of the first observed referendum. When 2009 is considered the first referendum, the number of referendums held within one year equals one because of the 2010 referendum ($held^{1}_{2009,Park\ Ridge} = 1$), the number of referendums held within three years equals two because of the 2010 and 2012 referendums ($held^{3}_{2009,Park\ Ridge} = 2$), and the number of referendums held within five years is not included in model because the timeframe extends past the end of our dataset ($held^{5}_{2009,Park\ Ridge} = missing$). In our regression model, each referendum is considered a first referendum and represents one observation.

Using our RD framework, we define the treatment variable to be failing a first referendum and model the number of referendums held (or passed) within τ years as a function of treatment and control flexibly for vote margin of first referendum i at time t:

$$held^{\tau}_{imt} = \beta^{\tau} \cdot fail_{imt} + \gamma^{\tau}_{1} \cdot fail_{imt} \cdot votemargin_{imt} + \gamma^{\tau}_{2} \cdot fail_{imt} \cdot votemargin_{imt}{}^{2} + \gamma^{\tau}_{3} \cdot fail_{imt} \cdot votemargin_{imt}{}^{3} + \gamma^{\tau}_{4} \cdot votemargin_{imt} + \gamma^{\tau}_{5} \cdot votemargin_{imt}{}^{2} + \gamma^{\tau}_{6} \cdot votemargin_{imt}{}^{3} + \boldsymbol{\delta^{\tau}} \cdot \boldsymbol{X_{mt}} + \varepsilon^{\tau}_{imt} \quad (2)$$

where $held^{\tau}_{im}$ is defined by equation (1), $fail_{im}$ is a binary variable equal to 1 if the first referendum i in municipality m failed, and β^{τ} is our variable of interest. We interpret β^{τ} as the differential number of referendums held within timeframe τ following a fail outcome, *ceteris paribus*. A negative significant value of β^{τ} suggests that a municipality that fails a first referendum is likely to hold fewer referendums in the future than a municipality that passes a first referendum. A positive significant value of β^{τ} suggests that a municipality that fails a first referendum is likely to hold more votes in the future than a municipality that passes. An insignificant value of β^{τ} indicates that binary voting outcome has no discernible effect on

number of future referendums. In our preferred model (described in equation 2), we control for vote margin of the first referendum using a cubic polynomial applied to either side of the threshold for passage. We also report the results from using a quadratic functional form and a local linear specification. We include a vector $\boldsymbol{X_m}$ of municipality socioeconomic characteristics, state fixed effects, and year fixed effects.[6] Last, ε_{imt}^{τ} is the error term. We use ordinary least squares estimation with robust standard errors clustered at the municipality level. Equally important, we also estimate Equation (2) using $passed_{imt}^{\tau}$ as the dependent variable.

Revisions

Our second analysis focuses on exploring revisions made to referendum characteristics and their effectiveness. We examine changes made to referendums in municipalities that hold more than one referendum. Our exploration begins with estimating how referendum characteristics change as a function of voting outcomes. Specifically, we look at revisions[7] between a first referendum and the next subsequent referendum in total proposed funding (logged subsequent total proposed funding per capita minus logged first referendum total proposed funding per capita), percentage point change in proportion of conservation funding to total funding, whether the measure uses a bond as the financing mechanism, and whether the referendum was held in November of a presidential election year (e.g., Guber 2003; Bechtel, Hangartner, and Schmid 2016):

$$revision_{im}^{\tau} = characteristic_{i+1,m} - characteristic_{im} \quad (3)$$

As with our dependent variables in the referendum dynamics analysis, we use a timeframe of five years following the first observed referendum. In addition to our reasoning in the temporal dynamics framework, we include this timeframe to account for collective local

memory and political relevance of past referendums. Only pairs of first and subsequent referendums are included in our sample for this analysis. One argument against this method is that selection bias is likely to exist for those municipalities that decide to hold a subsequent referendum. While this is true, we argue that because we restrict our sample to include only municipalities that held more than one referendum, selection bias is present across all municipalities in this sample, and our coefficient of interest will still have a valuable interpretation with internal validity for municipalities that hold multiple referendums. We use OLS to estimate the model

$$revision_{imt}^{\tau} = \beta^{\tau} \cdot fail_{imt} + \boldsymbol{\delta^{\tau}} \cdot \boldsymbol{Z_{mt}} + \varepsilon_{imt}^{\tau} \quad (4)$$

where $revision_{im}^{\tau}$ is defined in equation (3) and $\boldsymbol{Z_{mt}}$ is a vector including socioeconomic variables, a binary variable equal to one if the municipality is in Massachusetts or New Jersey,[8] and a binary variable equal to one if the subsequent referendum was held during the recession 2008-2012.[9] Here, β^{τ} is again our variable of interest. A significant β^{τ} indicates a significant difference in each revised characteristic between municipalities that fail versus pass their first referendum, which would suggest that municipalities strategically revise referendums.

Finally, we test whether these revisions are correlated with voter support of subsequent referendums to see if these changes are correlated with a larger vote margin. We model change in vote margin, $votemarginchange_{im}^{\tau}$, of the subsequent referendum (within a five-year timeframe) as a function of revisions to referendums characteristics interacted with vote outcome of a first referendum:

$$votemarginchange_{imt}^{5} = \alpha^{5} \cdot fail_{imt} + \boldsymbol{\gamma^{5}} \cdot \boldsymbol{revisions_{imt}^{5}} + \boldsymbol{\beta^{5}} \cdot fail_{imt} \cdot \boldsymbol{revisions_{imt}^{5}} + \boldsymbol{\delta^{5}} \cdot \boldsymbol{Z_{mt}} + \varepsilon_{imt}^{5} \quad (5)$$

Here, $\boldsymbol{revisions_{imt}^{5}}$ is a vector of all four possible revisions as defined above. The interaction of

the revisions with $fail_{imt}$ provides information about how the revisions differ by initial referendum outcome. $\boldsymbol{Z_{mt}}$ is defined as in equation (4).[10]

In both Equations (4) and (5), we have moved away from the RD framework. The equations also simplify the set of control variables. These are necessary steps given the substantial reduction in sample size caused by focusing on municipalities that hold more than one referendum. However, changing from RD to standard OLS limits the causal interpretation of these results, and instead we encourage a more descriptive interpretation. In addition to estimating both models with the full sample, we also estimate them with a reduced sample of initial referendums within 10 percentage points of the passing threshold to incorporate the RD logic and improve the counterfactual.

RESULTS

Referendum Dynamics

We begin with Figure 2, which shows the number of future referendums held (top row) and passed (bottom row) within one year (left column), three years (middle column), and five years (right column) versus vote margin of a first referendum, similar to the setup of Figure 1. The data is binned at two-percent intervals in vote margin and data are fitted flexibly using a locally weighted (lowess) regression to illustrate the magnitude of the discontinuity around a vote margin of zero. The plots in the top row are most similar to the right-most plot in Figure 1, providing suggestive support for the alternative hypothesis that municipalities that fail a referendum try again while those that pass a referendum do not. Intuitively, the magnitude of the effect grows as the timeframe increases; a longer timeframe allows more opportunity to hold additional referendums. The bottom row of plots shows that municipalities that just barely fail a

first referendum are also more likely to pass more future referendums than those that just barely pass. This observation lends additional support for the "try again" hypothesis.[11]

[Figure 2 about here]

Table 2 shows the results of the model described in equation (2) where panel (A) uses the number of referendums held within five years of an initial referendum as the dependent variable and panel (B) uses the number of referendums passed.[12] Column 1 uses a cubic polynomial to control for vote margin on either side of the threshold. Column 2 instead uses a quadratic polynomial. Columns 1 and 2 also include municipality-level socioeconomic demographics and state and year fixed effects. Column 3 uses a local linear functional form, applying the Calonico et al. (2014) optimal bandwidth. The model in column 1 is our preferred model and the coefficient of interest is interpreted as a change in the number of referendums. Specifically, for panel (A), a failed outcome on a first referendum leads to that municipality holding 0.472 more referendums within five years compared to a municipality that was successful in their first referendum, all else equal.[13]

[Table 2 about here]

Table 3 compares the effect of vote outcome on number of referendums held within a timeframe ranging from one to five years following the initial referendum: column *n* corresponds to the number of referendums held (panel A) and passed (panel B) within *n* years. The estimated effect on number of referendums held remains highly significant through all timeframes and grows in magnitude from 0.3 more referendums held within one year to 0.5 more referendums held within five years following a failed first referendum.

[Table 3 about here]

Repeating this analysis using the number of referendums *passed* within a timeframe as

the dependent variable (panel B of Tables 2 and 3) shows a similar pattern with a smaller effect size. In Table 2, panel (B), a municipality that fails a first referendum passes 0.284 more referendums within the next five years than a municipality with a successful first referendum. Table 3 shows that the magnitude of the effect ranges from 0.1-0.3 referendums passed over the various timeframes.[14]

The magnitude of these results warrants discussion. In our opinion, they are large in the sense that because they imply that nearly half of municipalities try again after failure and over 25% of those that fail actually succeed in passing a subsequent referendum. Some intuition may suggest that communities would just give up after defeat by popular vote. That being said, it is important to note that, on average, funding for land conservation for communities that initially fail never reaches parity with communities that initially pass. This result follows from the 25% future passage rate and, as we will show in the next section, proposed funding declines for subsequent referendums.

Strategic Behavior

Do PEs strategically revise characteristics such as proposed funding and financing mechanism in subsequent referendums? Our analysis here restricts our sample to include only those municipalities that hold more than one referendum. We present means for municipalities by initial outcome and difference in means along with results from our estimation of equation (4) in Table 4. Our models suggest that there is a significant relationship between the first outcome and the future proposed funding amount. Specifically, municipalities that fail a first referendum tend to decrease total proposed funding per capita by about 40% relative to those that pass.[15] The magnitude and significance of the relationship between the first outcome and revised funding

degrades when only municipalities with marginal first outcomes are included, suggesting this strategic revision is driven by municipalities with a strong preference for open space rather than by municipalities attempting to garner additional support through strategic revisions following a marginal outcome.[16] We find no evidence of revising the proportion of conservation-specific funding.

[Table 4 about here]

There are no significant revisions to financing mechanism or the date of referendum. The majority of municipalities considered in the five-year timeframe do not revise the financing mechanism (86%), and of those municipalities that fail a first referendum, an equal number switch to taxes or to bonds (N=5). The majority of our sample does not revise when they hold the subsequent referendum (68%). Nearly a fifth of municipalities move their subsequent referendums to November of an election year (18.9% of those who pass a first referendum and 20.0% of those who fail), while a relatively small proportion of municipalities that fail an initial referendum move the subsequent referendum to a date other than November of an election year (9.6% versus 14.3%).

[Table 5 about here]

Table 5 presents results of a regression of change in vote margin between subsequent referendums on the outcome of a first referendum and interactions with revisions to proposed total funding per capita and proportion of conservation-specific funding, finance mechanism, and date of the election. We control for socioeconomic characteristics, whether the municipality is in Massachusetts or New Jersey, and whether the subsequent referendum was held during the recession. Column 1 shows results for the entire sample and Column 2 restricts the dataset to all first referendums within 10% of the cutoff point. Both columns present results for the five-year

timeframe. First consider the model with all observations. Prior vote outcome is highly significant and positive, indicating that a failed referendum is likely to garner greater support in the subsequent election (an additional 11.3%) and a passed referendum is likely to garner less support (a decrease of 2.9%). We cannot conclusively infer the mechanism for this effect, but we offer two possible explanations. First, simple mean reversion: there is an average preference for open space and vote margin oscillates about this mean. A simple regression of change in vote margin on initial vote margin shows that this oscillation occurs around the 10% vote margin mark, consistent with mean voter support. However, a second explanation is that repeated exposure to referendums of a single topic increases the likelihood of voter support, until reaching a ceiling of support around 10% vote margin. Further research is necessary to confirm if either of these mechanisms is at play. For the sample restricted to marginal first outcomes, simply holding a subsequent referendum garners more support for all referendums. Municipalities that fail a first referendum by less than 10% increase voter support by 10.7% in the subsequent referendum. Municipalities that only marginally pass their first referendum are likely to garner an additional 4.4% on the subsequent referendum.

There is no significant effect of changes in proposed funding per capita for municipalities that pass an initial referendum, but this insignificance is suggestive of greater voter acceptance, given that these municipalities tend to increase funding amounts. For municipalities that marginally fail a first referendum, we find no correlation with a change in total funding per capita, but, for those whose referendums contain multiple topics (e.g. affordable housing and historic preservation as well), we do find a significant increase in voter support for increasing the proportion of funding specific to conservation. A one-tenth percentage point change in proportion of conservation funding to total funding is positively correlated with a 2.2 percent

change in vote margin.

In the full sample (column 1), switching from tax financing to bond financing significantly increases voter support for municipalities that pass their first referendum. This effect appears to be driven by a few municipalities that passed their first referendums using bond financing with vote margins greater than 25% (N=44). Of this subsample, 35 municipalities proposed bond financing in the subsequent referendum, one switched from tax financing to bond financing, and eight switched from bond financing to tax financing. The average change in vote margin for those eight municipalities was -25.2%, but only three of these municipalities sacrificed enough support to fail the subsequent referendum. The coefficient on the interaction with fail is insignificant, indicating no difference in slope between outcomes. However, we cannot glean much insight because there are few observations for municipalities that fail an initial referendum and then revised the financing mechanism (N=10) and all of these municipalities passed their subsequent referendums with margins ranging from 4% to 26%, regardless of whether they revise to tax or bond financing.

Lastly, we find a significant correlation between election timing and change in vote margin. In the full sample (Column 1), moving the referendum to coincide with a presidential election correlates with an increase in vote margin for the set of municipalities that fail their first referendum. In the restricted sample, all municipalities with a marginal first outcome increase their vote margin when revising the timing of the subsequent referendum to November of an election year (or vice versa). Off-year elections such as midterms typically have reduced participation (McDonald 2015), which may allow advocates or special interest groups, who are more likely to participate, to hold more influence in referendum outcomes (e.g. Meredith 2009). Moving the subsequent referendum to coincide with a main election such as a presidential

election with higher participation tends to increase voter support. This effect suggests that the general public, including those who do not necessarily identify as open space advocates, does indeed show a preference for open space, and when the public is less likely to vote (as in a month that does not coincide with a presidential election), there is less support for open space referendums.[17]

DISCUSSION

We research what happens following a failed or successful referendum, using referendums to preserve open space in U.S. municipalities as our case study. Our results suggest that municipalities that fail a first referendum tend to hold another referendum in the near future, and that holding a repeat referendum increases the vote return enough that a second referendum is likely to pass. This suggests that advocates for policy changes benefit from a "try and try again" approach at the ballot box. Our evidence also suggests that places that have successful referendums tend to be smart about garnering that support for additional projects; towns that pass a first referendum and hold a subsequent referendum tend to ask for more money, and although it may decrease their vote share, the referendums still pass.

From these findings, we infer several important implications for political scientists, policy analysts, land planning scholars, and land economists. First, advocates do not seem to be put off by an initial failure if they come close to success. We find significant evidence that there is a "try and try again" approach and that it pays off. Second, these findings also indicate that policies can benefit from appearing on the ballot multiple times; vote margins increase in successive elections. Defeat is not the end of the road for a ballot proposition and reintroduction can gain support.[18]

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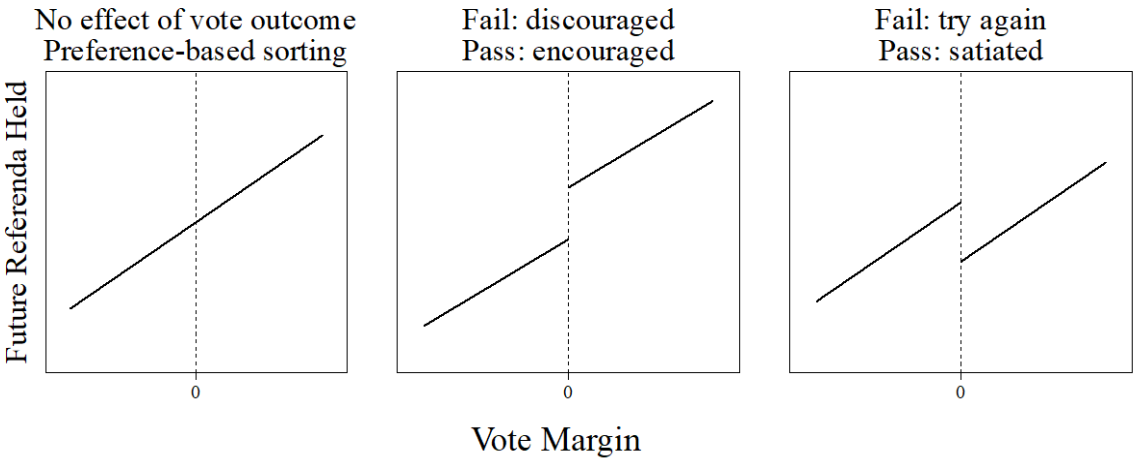
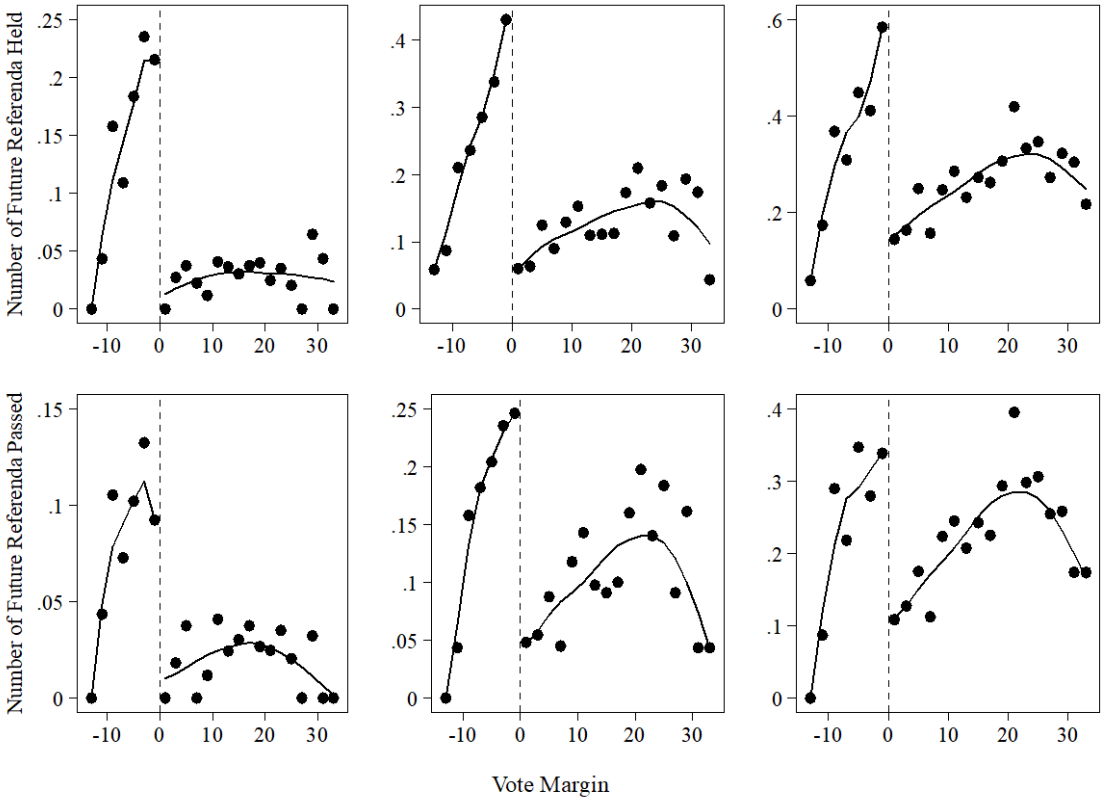
Figure 1: Intuition behind regression discontinuity design



Notes: Each graph hypothesizes the number of future referendums held (vertical axis) as a function of vote margin for an initial referendum (horizontal axis). Points to the left of the vertical dashed line represent a failed initial referendum while those to the right represent a passed first referendum. The left graph represents a scenario with no discontinuity around a zero vote margin and the upward slope could be interpreted as preference-based sorting. The middle graph represents a scenario where municipalities that pass a referendum are encouraged to hold more in the future while those that fail a referendum are discouraged from holding future referendums. The right graph represents a scenario where municipalities that just barely fail a referendum are more likely to hold future referendums to try again, while those that just barely pass a referendum are satiated and less likely to hold future referendums.

Figure 2: Number of future referendums held and passed following an initial referendum



Notes: Each graph displays the number of future referendums held (top row) and passed (bottom row) one year (left column), three years (middle column), and five years (right column) following an initial referendum. The vertical axis is number of referendums (note that y-axes vary in scale) and the horizontal axis is vote margin; points to the left of the dashed line represent a failed first referendum and points to the right of the dashed line represent a passed first referendum. Referendum vote margins are binned in 2% intervals and only vote margins between [-14, 34] are displayed, representing 95% of the dataset. Data are fitted flexibly using a locally weighted regression to illustrate the magnitude of the discontinuity around a vote margin of zero. Data are for municipality-level referendums in the coterminous United States 1988-2012 from the Trust for Public Land's *LandVote* database.

Table 1: Socioeconomic and demographic descriptive statistics

		All municipalities			Vote margin within 5%		
	All	Pass	Fail	t-stat	Pass	Fail	t-stat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Population	42,012	44,195	33,862	-1.08	55,354	34,819	-0.92
	(153,235)	(167,259)	(66,581)		(260,382)	(65,903)	
Population density	1,650	1,595	1,703	0.75	1,602	1,636	0.17
	(2,270)	(2,244)	(2,045)		(1,954)	(1,647)	
Under 18 (%)	25.0	24.9	25.2	1.09	25.1	25.5	0.91
	(4.6)	(4.7)	(4.5)		(4.6)	(4.0)	
Over 65 (%)	12.6	12.5	12.2	-0.86	12.7	11.8	-1.58
	(5.6)	(5.8)	(5.0)		(6.2)	(4.6)	
High school (%)	89.3	89.5	89.2	-0.67	89.4	89.6	0.22
	(6.2)	(6.2)	(5.7)		(5.4)	(5.0)	
College (%)	38.1	39.1	36.4	-2.72	37.1	36.9	-0.11
	(15.2)	(15.2)	(14.8)		(14.1)	(14.1)	
Income ($)	64,075	64,942	63,786	-0.84	63,518	64,848	0.61
	(21,179)	(21,668)	(19,569)		(20,428)	(19,887)	
Recent Construction (%)	19.1	19.4	18.2	-1.34	19.5	18.8	-0.45
	(13.9)	(14.3)	(12.2)		(13.5)	(12.9)	
Owner Occupied (%)	70.2	70.3	71.5	1.02	71.2	72.4	0.70
	(16.9)	(17.3)	(15.5)		(15.5)	(14.6)	
Democratic (%)	54.2	54.1	54.8	0.94	54.6	55.1	0.44
	(10.8)	(10.7)	(10.7)		(10.3)	(9.3)	
Unemployment (%)	3.9	3.9	3.9	0.12	3.8	3.9	0.86
	(2.6)	(2.7)	(1.9)		(1.7)	(2.1)	
Municipalities	1031	841	324		225	141	

Notes: Socioeconomic and demographic data are from the 2000 U.S. Decennial Census for municipalities that held at least one open space referendum in the coterminous United States 1988-2012 according to the Trust for Public Land's *LandVote* database. High school and college are educational attainment variables representing the percent of population that completed those degrees. Income is median household income. Recent construction represents the percent of houses constructed 1990-2000. Standard deviations are shown in parentheses.

Table 2: The effect of a marginal vote for an initial referendum on referendums within five years

	(1)	(2)	(3)
A. Number of referenda held			
Fail	0.472***	0.492***	0.485***
	(0.0989)	(0.0802)	(0.091)
R-squared	0.196	0.195	-
B. Number of referenda passed			
Fail	0.284***	0.321***	0.273***
	(0.0824)	(0.0684)	(0.081)
R-squared	0.203	0.202	-
Cubic polynomial in vote margin	Y	N	N
Quadratic polynomial in vote margin	N	Y	N
Local linear	N	N	Y
Socioeconomic variables	Y	Y	N
State fixed effects	Y	Y	N
Year fixed effects	Y	Y	N
Observations	1,387	1,387	547

Notes: The dependent variable is number of referendums held (Panel A) and number of referendums passed (Panel B) within five years of a first referendum. The coefficient of interest is on Fail, a binary variable equal to one if the first referendum failed and zero otherwise. Column 1 controls for vote margin on either side of the threshold for passage using a cubic polynomial. Column 2 instead uses a quadratic polynomial. Columns 1-2 include socioeconomic variables and state and year fixed effects. Column 3 applies the 'rdrobust' local linear estimator developed by Calonico et al. (2014). The number of observations reported in Column 3 are the effective number of observations used after the optimal bandwidth is estimated. The effective number of observations is 567 is Panel A and 547 in Panel B. The implied bandwidth for Panel A is 9.105 and 8.783 for Panel B. Data are for municipality-level referendums held in the coterminous United States 1988-2012 from the Trust for Public Land's *LandVote* database. Robust standard errors are in parentheses and clustered at the municipality level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 3: The effect of a marginal vote for an initial referendum on future referendums

	within 1 year	2 years	3 years	4 years	5 years
	(1)	(2)	(3)	(4)	(5)
A. Number of future referendums held					
Fail	0.263***	0.357***	0.408***	0.399***	0.472***
	(0.052)	(0.064)	(0.070)	(0.086)	(0.099)
B. Number of future referendums passed					
Fail	0.136***	0.190***	0.245***	0.242***	0.284***
	(0.041)	(0.055)	(0.063)	(0.075)	(0.082)
Cubic polynomial in vote margin	Y	Y	Y	Y	Y
Socioeconomic variables	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Observations	1,560	1,542	1,510	1,477	1,387

Notes: The dependent variable is number of referendums held (Panel A) and number of referendums passed (Panel B) within y years of a first referendum. The coefficient of interest is on Fail, a binary variable equal to one if the first referendum failed and zero otherwise. Each column represents a longer timeframe of subsequent referendums, from within one subsequent year (column 1) to five years (column 5). The cubic polynomial in vote margin controls for vote margin on either side of the threshold for passage. Data are for municipality-level referendums held in the coterminous United States 1988-2012 from the Trust for Public Land's *LandVote* database. Robust standard errors are in parentheses and clustered at the municipality level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 4. Revisions to subsequent referendum characteristics

		Observations	First pass (std. dev.)	First fail (std. dev.)	Difference (t-stat.)	Controlling for covariates (std. err.)
		(1)	(2)	(3)	(4)	(5)
Change in Total Proposed Funding per Capita	Overall	359	0.267	-0.039	-0.307	-0.353
			(0.953)	(0.952)	(-2.846)***	(0.113)***
	Within 10%	172	0.133	-0.071	-0.204	-0.250
			(0.974)	(0.930)	(-1.369)	(0.166)
Change in Conservation Proportion	Overall	359	.002	-0.013	-0.014	-0.008
			(0.264)	(0.171)	(-0.532)	(0.026)
	Within 10%	172	0.036	-0.005	-0.041	-0.056
			(0.334)	(0.156)	(-1.101)	(0.040)
Bond Financing	Overall	359	-0.041	0.000	0.041	0.042
			(0.404)	(0.296)	(0.972)	(0.049)
	Within 10%	172	0.000	-0.009	-0.009	0.025
			(0.395)	(0.291)	(-0.178)	(0.065)
November of Election Year	Overall	359	0.045	0.104	0.059	-0.010
			(0.576)	(0.536)	(0.930)	(0.074)
	Within 10%	172	0.031	0.084	0.053	0.045
			(0.612)	(0.534)	(0.601)	(0.124)

Notes: Table presents changes to referendum characteristics within a five-year timeframe by first referendum outcome. Change in total proposed funding per capita is the difference between log(subsequent total proposed funding per capita) and log(first referendum total proposed funding per capita). Conservation proportion is a ratio of conservation-specific funding to total proposed funding and change in conservation proportion is the subsequent proportion minus the first referendum proportion. Change in bond financing equals 1 if the municipality switched from a different financing mechanism to a bond, -1 if vice versa, and 0 if there is no change. Similarly, Change November election year ballot is equal to 1 if the municipality switched the subsequent ballot to coincide with a presidential election, -1 if vice versa, and 0 if there is no change. Column 5 controls for socioeconomic characteristics, whether the municipality is in MA or NJ, and whether the referendum occurred during the recession (2008-2012), with standard errors clustered at the municipality level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Effects of revisions on vote margin of subsequent referendums

	All observations (1)	Within 10% of cutoff (2)
Fail	14.14***	6.256***
	(1.611)	(1.945)
Change in total proposed funding per capita	0.48	0.67
	(1.075)	(1.824)
Fail x Change in total proposed funding per capita	0.682	0.728
	(1.383)	(2.087)
Change in conservation proportion	3.689	-5.758
	(3.430)	(3.929)
Fail x Change in conservation proportion	3.985	22.02**
	(8.848)	(8.765)
Change bond financing	6.397**	1.124
	(2.858)	(3.093)
Fail x Change bond financing	-6.569	-3.489
	(4.323)	(6.034)
Change November election year ballot	0.916	5.554**
	(1.300)	(2.466)
Fail x Change November election year ballot	3.812*	-2.041
	(2.283)	(2.917)
Constant	-2.859***	4.424**
	(0.916)	(1.744)
Socioeconomic variables	Y	Y
D(MA or NJ)	Y	Y
D(Recession)	Y	Y
Observations	359	172
R-squared	0.285	0.260

Notes: The dependent variable is change in vote margin (subsequent vote margin minus first vote margin) for a timeframe of five years between referendums. Column 1 uses all observations and column 2 restricts the sample to only observations with the first referendum vote margin within 10% of the cutoff. Change in total proposed funding per capita is the difference between log(subsequent total proposed funding per capita) and log(first referendum total proposed funding per capita). Conservation proportion is a ratio of conservation-specific funding to total proposed funding and change in conservation proportion is the subsequent proportion minus the first referendum proportion. Change in bond financing equals 1 if the municipality switched from a different financing mechanism to a bond, -1 if vice versa, and 0 if there is no change. Similarly, Change November election year ballot is equal to 1 if the municipality switched the subsequent ballot to coincide with a presidential election, -1 if vice versa, and 0 if there is no change. All columns include socioeconomic variables, a binary variable equal to one if the municipality is in Massachusetts or New Jersey, and a binary variable equal to one if the referendum is held during the recession 2008-2012. Data are for municipality-level referendums held in the coterminous United States 1988-2012 from the Trust for Public Land's LandVote database. Robust standard errors are in parentheses and clustered at the municipality level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

[1] There are two means by which local referenda appear on the ballot: legislative referral and citizen ballot collection. Which one is employed is largely determined by the rules of the local jurisdiction. In addition, for many locales, the jurisdiction is required to have a vote if they wish to take on debt or raise taxes, in which case, even if the legislature is unanimous in its support, it must go to the electorate for ratification. We do not have data on the local jurisdictions' rules regarding ballot access and mandatory votes. However, all of these would require a policy entrepreneur advocating for the policy change. In the case of legislative referenda, members of the legislature may be the policy entrepreneurs or they may be the intermediary between the entrepreneurs and the ballot, whereas when citizens gather signatures to place the issue on the ballot, a local citizen or group of citizens are the policy entrepreneurs.

[2] For any given referendum, there is roughly a 75 percent chance of passage given how rarely referendums fail in the US. PEs may be aware of the statistical chance of passage and propose subsequent referendums until they succeed. On the other hand, PEs may attribute success or failure to referendum characteristics and revise accordingly.

[3] We define vote margin as the proportion of votes in favor of the referendum minus the proportion of votes needed for passage. Some referendums require a larger than 50% vote in favor to pass (e.g. 2/3rds support or 60% support). In our data, we treat these cutoffs as the votes needed for passage. So, each observation has its own cutoff for passage, but the vote margin is the difference between what is needed for passage and the actual vote.

[4] It is important to note that one limitation of our modeling strategy is that we can only distinguish differences between municipalities that just barely fail or pass a referendum; we cannot know whether this discontinuity is caused by one group holding more referendums or by the other group holding fewer, we only know the relative difference in number of subsequent referendums held.

[5] Results using timeframes of 2, 4, and 6 years are similar. Sample size decreases with longer timeframes.

[6] We include state and year fixed effects to account for statewide policies related to open space, governmental and public prioritization of open space needs among other macroeconomic factors, state-level differences in ease of policy-making using ballot measures (e.g., Bowler and Donovan 2004), and yearly government budget changes. For example, Massachusetts and New Jersey enacted notable policies facilitating ballot measures and provision of open space at the municipal level (see Heintzelman et al. 2013 for an overview of New Jersey's Green Acres Program and Kotchen and Powers 2006 for an overview of Massachusetts's Community Preservation Act).

[7] Twenty-one municipalities voted on more than one ballot measure on the same day (N=55). Results of one measure cannot affect the appearance of a measure or strategically impact the characteristics of a measure that appears simultaneously. Therefore, we aggregated these measures using the sums of total proposed funding, proposed conservation funding, total approved funding, and approved conservation funding. Of municipalities that held multiple referendums on the same day, eight municipalities had measures that proposed different financing mechanisms and two municipalities passed one measure but failed the other.

[8] We control for Massachusetts and New Jersey because both states have unique legislation that encourages local governments to adopt land preservation policies, as discussed in footnote 6. These two states have had the most referendums by a fair margin, see Table A2 in the Online Appendix.

[9] We use binary control variables rather than fixed effects and omit the RD framework to free up degrees of freedom given the smaller sample size of municipalities that held at least two referendums.

[10] One question, of course is if the municipality is holding a second referendum on the exact same land conservation question or if the future election regards a different plot of land or project. Unfortunately, our data does not allow us to tell if it is the exact same land conservation request (e.g. same land) or there is a different project put forward to voters. A good example here is Glasbury, Connecticut's recent (2022) bond which asked voters for $3 million for land preservation. The Landvote database simply lists this bond's subject as "Bond for the preservation of open space and purchase of development rights" and the actual wording of the ballot was no more specific. As a result, what we can tell is if they return to the electorate for a second attempt at the general purpose of land conservation, but not for the same project. We could infer from the change in amount requested which towns change plots, but given land prices change, we cannot be sure this inference would be correct. Land could get less or more expensive in the interim year.

[11] Table A3 in the Online Appendix is a transition matrix detailing the vote margin of each focal referendum and the results of the next subsequent referendum and provides additional evidence for this hypothesis.

[12] Table A5 in the Online Appendix presents results building up to the model described by equation (2), including a simple OLS estimate without controlling for vote margin. Table A6 in the Online Appendix presents coefficients on socioeconomic variables for one-year and five-year timeframes.

[13] Because we are comparing municipalities that initially fail to those that initially pass, it is just as valid to interpret the results as communities that barely pass a referendum hold on average 0.472 fewer referendums in the future than communities that barely fail a referendum.
[14] The RD results are robust to a falsification test that moves the passing vote margin threshold to +10 (see Table A7 in the Online Appendix).
[15] We define proportional change in total proposed funding as log (subsequent total proposed funding) – log (first referendum total proposed funding). Therefore, percent change is calculated by exponentiating the difference (column 4) and the regression coefficient (column 5).
[16] An alternative explanation is that significance decreases because sample size decreases.
[17] Table A8 in the Online Appendix presents an alternative version of Table 5 that changes the definition of November to include both presidential and midterm elections. The change to November is no longer associated with increased approval. This set of findings suggests that municipalities that pass a referendum and try again only get a bump in support if they hold the follow up referendum in November of presidential year and not November of a midterm year.
[18] Data availability statement. Replication materials are available in the Journal of Public Policy Dataverse at https://doi.org/10.7910/DVN/ISIXLR