Decision-Makers as Decision-Shapers: Evidence on the Influence of Representatives on Direct Democratic Votes*

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July 18, 2026

Abstract

This paper estimates whether elected representatives causally shape citizens' decisions in direct democratic votes. We study Swiss referenda, where voters decide on the same policy proposals that legislators previously vote on in parliament, allowing a direct comparison between representatives' positions and citizens' binding policy choices. Using close elections between candidates with opposing positions on a given issue, we implement a regression discontinuity design that generates quasi-random variation in the issue positions of elected representatives. Having an issue supporter instead of an issue opponent as a representative increases the electorate's share of yes votes by about 8.7 percentage points. Changes in turnout explain about a quarter of the effect, at most one third. The effect tends to be larger where alternative information is scarce, consistent with representatives supplying cues on complex policies. The results imply that even when citizens can veto politicians, elected representatives remain important in shaping policy preferences and policy choice.

Keywords: direct democracy, political representation, referendum voting, voter behavior, legislative influence, political information, regression discontinuity design
JEL: D72, D78, D80, D91

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We thank Mark Schelker and the participants of the Australian Conference of Economists, the Workshop of the Swiss Network on Public Economics (SNoPE), the European Public Choice Society Meeting, the Economics Graduate Seminar of the University of Bayreuth (Germany), the 18th CESifo Workshop in Political Economy, and the Cookie Seminar at the University of Fribourg (Switzerland) for their comments.

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1 Introduction

Do elected representatives still shape policy when citizens themselves cast the decisive vote? This paper answers that question in the Swiss system of direct democracy, where voters decide in referenda on the same policy proposals that legislators previously voted on in parliament. We use close elections between candidates with opposing positions on a proposal to generate quasi-random variation in the issue positions of elected representatives. Using a regression discontinuity design, we find that electing an additional supporter of a proposal increases the share of yes votes in that person's constituency in the subsequent referendum by about 8.7 percentage points. The results suggest that direct democracy does not insulate political decisions from the influence of elected representatives.

That finding is not obvious. Direct democratic institutions are commonly understood as a device that limits the agency problems of representative democracy by allowing citizens to review and overturn legislative decisions (Gerber, 1996; Matsusaka, 2005). If referendum outcomes simply aggregate preexisting voter preferences, elected representatives should matter little once citizens have the final say. This assumes that voters enter direct democratic campaigns with stable preferences and sufficient information to evaluate often complex proposals on their own. In practice, it is costly for ordinary voters to understand policy issues, so they remain rationally ignorant (Downs, 1957). Under this assumption, referendum campaigns create an environment in which political actors can shape how voters interpret policy issues. Representatives may therefore matter not only through their parliamentary votes, but also through the cues they provide to citizens during referendum campaigns.

Identifying such influence is difficult because representatives are elected by the same constituencies whose later referendum choices we observe. Constituencies that elect supporters of an issue are also likely to be more supportive of that issue in the referendum, even in the absence of any causal influence from the elected representative. Simple correlations between legislators' positions and referendum outcomes, therefore, confound the influence of representatives with underlying voter preferences. A credible empirical design must isolate variation in representatives' issue positions that is unrelated to the electorate's latent support for the proposal.

We estimate the causal effect of legislators' issue positions on direct democratic decisions by focusing on close elections between candidates with opposing positions on a given pol-

icy proposal. For 263 Swiss referenda[1], each held in all 26 cantons (6,838 canton-referendum pairs), we identify the marginally elected and marginally non-elected candidates from the most recent election and keep observations in which those two candidates take opposing positions on the proposal later decided in the referendum. We refer to these cases as *opposing races*. When the policy-supporting candidate closely wins rather than closely loses such a race, the composition of parliamentary representation shifts for reasons that are plausibly unrelated to the electorate's subsequent referendum preferences. We use this quasi-random variation in a fuzzy regression discontinuity design (Calonico, Cattaneo, and Titiunik, 2014), with the supporting candidate's vote margin as the running variable and actual parliamentary entry as the treatment.

Switzerland provides an ideal institutional setting to study this question. Federal referenda allow citizens to vote on the same legislative proposals, with identical wording, that were previously decided in parliament (Carey and Hix, 2013; Stadelmann, Portmann, and Eichenberger, 2013). This makes it possible to compare representatives' positions and citizens' direct democratic votes on exactly the same issues rather than on broad ideological scales or survey questions. We construct a novel dataset linking referendum outcomes in all 26 cantons to the issue positions and electoral margins of elected and non-elected candidates for 263 federal referenda held between 1996 and 2026. We measure candidates' electoral closeness using the relative vote margin of Luechinger, Schelker, and Schmid (2024), which captures both party-level and within-party competition in proportional representation systems.

Our estimates indicate that an issue supporter who narrowly makes it into parliament, rather than an issue opponent, increases the share of yes votes in the subsequent referendum by roughly 8.7 percentage points. Note that, given our regression discontinuity design, this is a local average treatment effect. It identifies the effect of marginal changes in representatives' issue positions on a subset of votes, not the average effect of representatives' positions across all referenda or constituencies. The paper does not claim that representatives fully determine voter choices, nor that the estimated effect generalizes mechanically beyond the influence of closely elected legislators. It shows that, even where citizens formally have the final say, elected representatives can causally affect binding policy outcomes.

We contribute to the literature on political influence and public opinion (Broockman and Butler, 2017; Bullock, 2011; Slothuus and Bisgaard, 2021) by moving beyond survey attitudes

[1]For simplicity, we use the term *referenda* to refer to both federal initiatives and referenda.

and examining binding policy decisions. Existing work shows that party cues, elite messages, and campaign information can affect reported opinions. There is much less causal evidence on whether politicians shape actual policy choices with real consequences (Burnett and Parry, 2014; Colombo and Kriesi, 2017). Lenz (2012) shows in panel surveys that voters follow politicians they already support and adopt their policy positions. We test whether this cue-taking extends beyond stated attitudes and show that the position of a quasi-randomly elected legislator causally shifts the binding referendum outcome itself. The paper also contributes to the close-election literature in political economy (Marshall, 2022). Prior studies typically use close races to identify the effects of fixed politician characteristics such as party affiliation, incumbency, gender, or ideology. We extend that logic to referendum-specific issue positions, allowing the same empirical framework to identify how the election of a supporter rather than an opponent affects voters' decisions on the same proposal.

Direct democracy does not eliminate the political role of representatives beyond agenda-setting. While initiatives allow citizens to propose their own policies, legislators already shape what citizens vote on in referenda, as the proposals originate in parliament. Our findings show the influence extends further. Even after parliament has voted and the proposal is on the ballot, legislators continue to shape how citizens vote. Legislators determine the content of legislative and constitutional amendments, which may later be challenged by a referendum. The threat of a referendum has an indirect impact on politics by holding elected officials accountable beyond the regular elections (Matsusaka, 2014). Our findings show that, in turn, legislators also influence how voters evaluate proposals at the ballot box. If representatives shape voters' preferences rather than merely implement them, the distinction between representative democracy and direct democracy is less sharp than standard institutional distinctions imply. Direct democratic institutions are therefore less effective at holding politicians accountable than the standard view suggests.

The remainder of the paper proceeds as follows. Section 2 discusses the related literature and the conceptual channels through which representatives may influence referendum outcomes. Section 3 describes Switzerland's institutional setting and the construction of the dataset. Section 4 presents the empirical design. Section 5 reports the main results and validation exercises and decomposes the effect into vote switching and turnout. Section 6 examines the conditions under which the effect is stronger. Section 7 summarizes our results and discusses their broader implications for democratic representation.

2 Related literature and conceptual framework

This paper contributes to two main literatures. The first studies how political actors shape citizens' preferences. Party cues, elite messages, media exposure, and campaign communication can affect political attitudes and reported opinions (Broockman and Butler, 2017; Bullock, 2011; Campbell et al., 1960; Slothuus and Bisgaard, 2021). That literature establishes that voter preferences are responsive to information and signals from political elites rather than fully fixed in advance (Gerber and Jackson, 1993). However, much of the evidence comes from surveys or survey experiments, where the outcome is stated opinion rather than a binding policy choice (Hill and Huber, 2019; Zaller and Feldman, 1992).

A smaller literature examines whether politicians and parties affect views on concrete policy proposals. Survey experiments show that voters update their opinions on ballot measures and policy issues when exposed to party positions, endorsements, or policy arguments (Boudreau and MacKenzie, 2014; Colombo and Kriesi, 2017; Grewenig et al., 2020). These studies leave open whether the same responsiveness extends to actual votes with real consequences. Reported attitudes in a survey, however, do not necessarily reflect binding votes (Hill and Huber, 2019; Zaller and Feldman, 1992). This is especially true given that survey respondents do not necessarily have experience with direct democratic decision-making, nor are they subject to the political discourse that precedes a popular vote.

The paper also contributes to the literature that uses close elections to identify the causal effects of politicians on downstream outcomes (Marshall, 2022). Existing studies exploit close electoral races to examine the effects of party affiliation, incumbency, ideology, gender, age, religion, and other politician characteristics (Baskaran and Hessami, 2023; Bhalotra, Clots-Figueras, and Iyer, 2021; Carlino et al., 2023; Carlsson, Dahl, and Rooth, 2021; Marx, Pons, and Rollet, 2024; Matsubayashi, 2013). We apply that logic to referendum-specific issue positions rather than to fixed politician characteristics. This extension matters because it allows the same empirical framework to identify whether electing a supporter rather than an opponent of a specific proposal shifts voter support for that proposal, and it enables a direct comparison between representatives' and voters' choices on the same issues. To our knowledge, we are the first to apply this design to candidates' *issue positions*, rather than their personal characteristics or previous electoral performance.

The conceptual mechanism that allows politicians to affect the outcomes of direct democratic votes runs through voters' rational ignorance (Downs, 1957). Direct democratic proposals are often costly for voters to evaluate. Therefore, many citizens are unlikely to acquire detailed information about every issue on the ballot. In that environment, representatives can matter by providing visible and credible cues about the content or consequences of a proposal (Boudreau and MacKenzie, 2014; Bullock, 2011). Legislators vote publicly in parliament, participate in campaigns, appear in the media, and signal partisan or ideological interpretations of policy choices (Stadelmann and Torgler, 2013). If voters use those signals when forming judgments, then quasi-random variation in representatives' issue positions should translate into differences in referendum outcomes.

This mechanism is plausible, but it should not be overstated. The empirical design identifies whether representative positions affect referendum outcomes. It does not, by itself, identify whether that influence works through persuasion, information transmission, party mobilization, or some other channel. The decomposition in Section 5 shows through which behavioral margin the influence runs, and the moderator analysis in Section 6 provides suggestive evidence on when it is stronger, but not definitive proof of why it operates.

3 Institutional context and data

We use Switzerland's institutional setting to estimate the causal effect of legislators' policy decisions in parliament on the outcomes of direct democratic votes on the same policy proposals. Swiss citizens can challenge laws and constitutional amendments adopted by parliament through referenda or submit their own proposals through federal initiatives. These votes are binding and allow citizens to decide on the same policy proposals, with identical wording, that legislators previously voted on in parliament. This institutional feature enables a direct comparison between the policy choices of legislators and voters.

3.1 Swiss parliament and electoral system

The Swiss federal parliament consists of the National Council (lower house) with 200 seats and the Council of States (upper house) with 46 seats. Members of both chambers serve four-year terms and are elected simultaneously across all 26 cantons, which function as electoral districts. We focus on the National Council, where seats are allocated proportionally to each

canton's permanent resident population. As a result, cantons have between one (in the six smallest cantons) and 36 (in the largest canton, Zürich) National Councilors.[2] Elections follow an open ballot proportional representation (PR) system. In single-seat cantons, the elections de facto follow a first-past-the-post system. Parties and other electoral groups nominate candidates on lists. Voters can cast as many votes as their canton has seats. They can submit a party list unchanged, modify lists by crossing out names and writing down others a second time (pre-accumulating), combine names from different party lists (splitting), or fill out an empty list from scratch (for details, see, Federal Chancellery, 2025).

3.2 Swiss referenda

With Switzerland's direct democratic institutions, decisions made by parliament do not automatically become law. Citizens can challenge parliamentary decisions through referenda by collecting 50,000 signatures within 100 days. Constitutional amendments are automatically subject to a referendum. In addition, federal initiatives allow citizens to propose constitutional changes themselves by collecting 100,000 signatures within 18 months. Referenda enable citizens to vote on the same policy proposal with identical wording as politicians do in parliament. Referenda cover a wide range of policy areas, including foreign policy, public finances, transport and infrastructure, social policy, energy, and agriculture.[3] Referenda allow citizens to rank proposed legislation against the status quo, and the outcome of the referendum is decisive.

Citizens vote by mail or in person on up to four days per year, often deciding on multiple referenda. All eligible citizens receive the voting material by mail without registering. The materials include the complete text of the proposed changes to the law or the constitution, arguments from the campaigns of both supporters and opponents, and official recommendations from the Federal Council and Parliament.

Referendum campaigns in Switzerland are highly informative and follow clear regulations. Advertisements for referenda are prohibited on the radio and television, but they are permitted in newspapers. The media provide comprehensive and balanced coverage of referenda (Udris, 2023). Politicians publicly advocate for their positions in interviews and appear in dedicated

[2] The numbers refer to the 52nd legislative period (2023-2027). From 1963 to 2011, seats were redistributed across cantons periodically over multiple legislative terms. Since 2011, seats are redistributed every legislative term (Parliamentary Services, 2024).

[3] See Parliamentary Services (2025a) for more information on all referenda.

programs on television, radio, and in the press. All of their votes in parliament are publicly available through the official parliamentary website and the official bulletin of the Federal Assembly. For policy proposals that are subject to referenda, which are the focus of this study, media outlets often report on how individual politicians voted. Before referenda, both the Swiss Broadcasting Corporation (SRG) and the media group Tamedia conduct representative polls. The results appear on television, radio, and in newspapers, and receive wide national coverage (for further information on voting polls, see Bursztyn et al., 2024).

The continuity and intensity of the political discourse make Swiss referenda particularly credible for studying the influence of politicians on voter preferences. Popular votes always follow parliamentary votes, and the majority decision in Parliament serves as a voting recommendation for the voters (Stadelmann and Torgler, 2013). By design, parliament always approves policy proposals that are subject to a referendum after passing through parliament. For these votes, the recommendation is always to accept the proposed legislation. Policies proposed by citizens as federal initiatives, by contrast, are typically rejected by a majority in parliament.

3.3 Data

We use data on 29,562 candidates for the Swiss National Council from eight federal elections (1995-2023), provided by the Federal Statistical Office. The data cover the number of votes, election outcomes, party list affiliations, names, ages, genders, and places of residence for each candidate; the number of votes per party list; and information on alliances between party lists.

Using these data, we calculate the electoral closeness of each candidate in every election. Measuring closeness at the candidate level in PR systems is not straightforward because seats are first assigned to party lists and then to candidates within those lists. As a result, competition arises both between parties and within party lists. We calculate the *relative vote margin* proposed by Luechinger, Schelker, and Schmid (2024), which overcomes the limitations of previous approaches that focused only on either party-level or within-party competition (Clots-Figueras, 2011; Freier and Odendahl, 2015; Hyytinen et al., 2018). The method accounts for both sources of competition by calculating candidates' vote surplus to the loss of a seat or their vote shortfall to the gain of a seat. It identifies the candidate margin and the party margin. The candidate margin is the smallest vote change needed to surpass the last elected candidate

on the list. The party margin is the smallest vote change needed for the party to gain an additional seat, which would let the candidate win based on list position. For each seat-candidate combination, the binding margin is the smaller of these two values.[4] A candidate's overall vote margin to win (lose) a seat is then the maximum (minimum) of these binding margins across all seat-candidate combinations. To ensure comparability across districts and elections, we divide each candidate's vote margin by the number of voters who turned out in the election.[5]

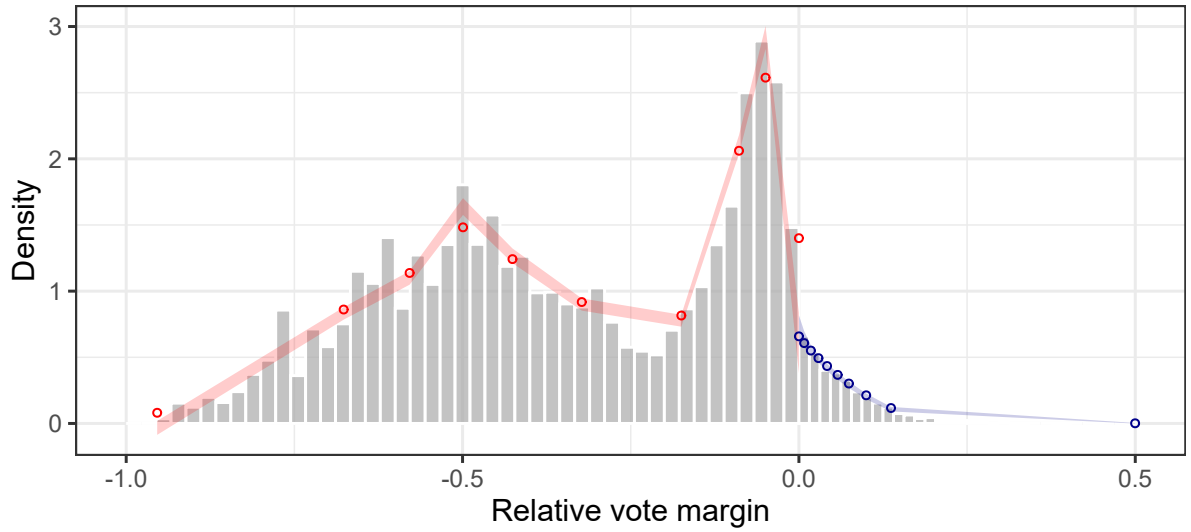
Figure 1 shows the histogram and local polynomial density estimate of the relative vote margin for elected candidates ($vote\,margin > 0$) and non-elected candidates ($vote\,margin < 0$) below and above the cutoff. The figure and the empirical manipulation testing results, following Cattaneo, Jansson, and Ma (2020), show that the relative vote margin is continuous at the cutoff of 0 ($T = 0.785,\ p = 0.432$). Note that the vote margin does not perfectly determine whether a candidate enters parliament. Some legislators are in parliament despite having negative vote margins, while others are not in parliament despite having positive vote margins. This occurs when elected candidates resign from office or do not take office in the first place, allowing the next best candidate from a party list to assume office. Figure B1 in the appendix shows candidates' probability of entering parliament conditional on their vote margin.

We match candidates to all referenda held during the legislative term following the election in which they participated. We match candidates to referenda using the referendum date, meaning that matched candidates are in office at the time the electorate votes at the ballot box. On average, the popular vote takes place 256 days after the corresponding parliamentary vote ($SD = 108$). The time gap ranges from 80 to 716 days. The final data set includes 919,989 candidate$\times$referendum observations, covering 263 referenda across eight legislative periods and 26 electoral districts.

We directly observe the issue positions of elected candidates based on their roll-call votes in parliament, which we obtain from the official web services of the Swiss Parliament (Parliamentary Services, 2025b). Candidates who do not enter parliament never cast a roll-call vote. We infer their positions from the party line. Thus, we measure the positions of elected and non-elected candidates differently. Although the Swiss Federal Constitution entitles par-

[4]For example, on party list P two candidates, C_1 and C_2, are elected. C_2 has 800 votes more than the third-ranked candidate C_3. The party would lose its second seat if it scored 1,000 votes less and would gain a seat if it scored 500 votes more. Thus, C_2's binding margin for the third seat is 800, and C_3's binding margin for the second seat is 500.

[5]Luechinger, Schelker, and Schmid (2024) propose the vote margin divided by the number of *eligible voters* as the relative vote margin.



Notes: The figure shows a histogram and local polynomial density estimate of the relative vote margin for candidates below and above the cutoff (Cattaneo, Jansson, and Ma, 2020).

Figure 1: Distribution of vote margins in the full candidate sample

liamentarians to vote without instructions, party lines remain highly influential. In our data, which cover legislators' votes on policy proposals with subsequent referenda, only 6.94 percent of legislators voted against their party's official voting recommendations, suggesting that party lines appropriately approximate actual voting behavior. We assess robustness by approximating positions uniformly with party lines for all candidates and by restricting the sample to referenda with high party discipline. Note that misclassifications do not bias our results as long as they are symmetric. That is, as long as misclassifications occur just as often among supporters as among opponents. Symmetric misclassifications may still attenuate the estimated effect. Our robustness checks, restricting the sample to referenda with high party discipline, where misclassification is less likely, yield similar estimates, suggesting that any such attenuation is small.

We collect additional referendum information from the Swissvotes dataset, which is published by Année Politique Suisse, an online platform hosted by the Institute of Political Science at the University of Bern. From this dataset, we obtain the referendum party lines, official results, issue topics, and measures that describe the campaign and media environment. In particular, we use media coverage (the number of newspaper articles published in the 12 weeks before the vote), media tonality (ranging from -100 for fully negative to $+100$ for fully positive coverage), and campaign balance, defined as the share of advertisements advocating a yes

vote among all referendum-related advertisements appearing in approximately 50 print media outlets, excluding neutral advertisements. For further details, see Swissvotes (2026).

We further draw on representative post-referendum surveys (VOX analyses), which cover all 263 referenda in our dataset. These surveys, conducted within ten days of each referendum, collect detailed information from 1,000 to 3,000 respondents on their socioeconomic characteristics, voting behavior, and knowledge of the proposals. The surveys are widely regarded as the standard data source for studying Swiss referenda; their results are frequently discussed in newspapers and have been used extensively in the literature (e.g. Ahlfeldt, Maennig, and Mueller, 2022; Stutzer, Baltensperger, and Meier, 2019). From these surveys, we construct referendum-level measures of perceived policy complexity and importance by aggregating respondents' evaluations. Complexity is measured using a binary indicator reported in the survey, while importance is measured on a 0–10 scale.[6]

Our main outcome variable is the electorate's share of yes votes in a referendum at the cantonal level. This yields 6,838 canton×referendum observations (263×26). For each cantonal-level referendum result, we observe the position and electoral closeness of all candidates who competed in the last election. The result is a unique dataset that links candidates' electoral closeness and issue positions to their voters' decisions on identical policy proposals. It enables us to implement a close-election RD design to estimate the causal effect of politicians' issue positions on decisions in direct democratic votes.

4 Identification strategy and empirical design

Our objective is to estimate whether electing a representative who supports a policy proposal changes voters' support for that same proposal in the subsequent referendum. The main empirical challenge is endogeneity. Representatives are elected by the very constituencies whose referendum choices we study, so constituencies that elect supporters of a proposal are also likely to be more favorable toward that proposal. A simple comparison between cantons represented by supportive and opposing legislators would therefore confound the influence of representatives with underlying voter preferences.

[6] Importance is assessed with the question: "On a scale from 0 (not important at all) to 10 (very important), how important was [referendum title] for you personally?" Complexity is measured with the question: "Did you find it rather easy (indicator = 1) or rather difficult (indicator = 2) to understand what [Referendum Title] was about?"

To address this problem, we exploit close elections between candidates with opposing positions on the same proposal. We construct an RD sample from the 6,838 canton-referendum pairs in our dataset. For each pair, we identify the marginally elected and marginally non-elected candidates from the most recent National Council election before the referendum. These are the elected candidate with the smallest vote margin and the non-elected candidate with the largest vote margin. We retain pairs in which those two candidates hold opposing positions on the proposal later decided in the referendum. We call these observations *opposing races*. In such cases, a close victory by the supportive candidate rather than the opposing candidate changes the issue position represented in parliament while holding constant the underlying electoral environment. Our RD sample is therefore referendum-specific. The two marginal candidates remain fixed throughout a legislative term, but whether they form an opposing race depends on the specific proposal under consideration.[7]

We observe opposing races in four scenarios. The majority of opposing races arises because the two marginal candidates belong to different parties with opposing party lines, and the elected candidate follows the party line (1,168 observations; 85.5%). When the elected candidate deviates from the party line, we observe an opposing race either when the candidates belong to different parties with identical party lines (73 observations; 5.34%), or when both candidates belong to the same party (93 observations; 6.81%). In all other cases, the candidates hold opposing positions, and the party of the elected candidate has not issued a voting recommendation (32 observations; 2.34%). Because we infer non-elected candidates' positions from party lines, some opposing races may be misclassified. As discussed in Section 3, symmetric misclassification does not bias the design.

Overall, we observe 1,366 opposing races out of 6,838 canton$\times$referendum pairs. The supporter enters parliament in 736 of these races, while the opponent does so in 630. For each opposing race, the electoral margin of the policy-supporting candidate serves as the running variable. Positive values indicate that the supporter wins the opposing race, whereas negative values indicate that the opponent wins. As we are interested in the causal effect of elected representatives *in parliament*, we define the treatment as parliamentary entry by the supporter, not merely electoral victory. Some candidates with negative margins enter parliament when an elected candidate resigns. These noncompliant observations appear on the left side of the running variable. We quantify this below (Section 4.1). Because victory in a close electoral race

[7] Exceptions arise only when a candidate resigns or dies during the term.

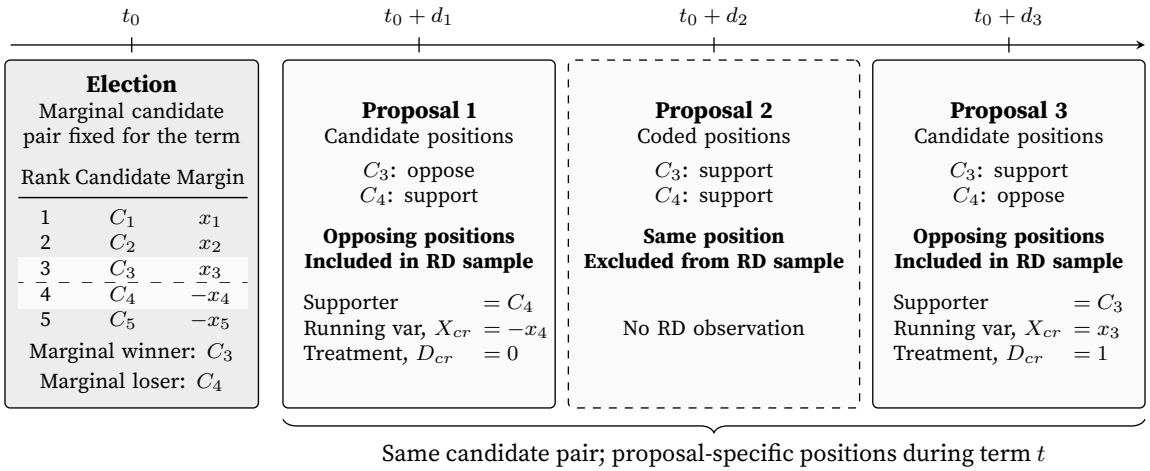


Figure 2: Referendum-specific sample construction and fuzzy RD design

Notes: The figure presents a stylized example for one canton and legislative term. The marginal electoral winner C_3 and marginal electoral loser C_4 are fixed within the term, but their positions vary across proposals. A canton×referendum pair enters the RD sample only when the candidates take opposing positions. The running variable X_{cr} is the normalized vote margin of the candidate who supports proposal r; $\mathbf{1}\{X_{cr} \geq 0\}$ is the electoral assignment; actual parliamentary entry by the supporter is the treatment. Positions are observed from roll-call votes for candidates in parliament and inferred from party recommendations for candidates outside parliament. Parliamentary replacements can make actual treatment differ from electoral assignment.

is quasi-random, the issue position of the marginally elected candidate is plausibly exogenous to voter policy preferences (Lee and Lemieux, 2010).

Figure 2 summarizes the referendum-specific construction of the RD sample. Within each canton and legislative term, the marginally elected and marginally non-elected candidates are fixed, while their issue positions may vary across referenda. A canton×referendum pair enters the RD sample only when these candidates hold opposing positions. The running variable is the vote margin of the issue supporter, so it is negative when the supporter is the marginally non-elected candidate and positive when the supporter is the marginally elected candidate. The treatment indicates whether the issue supporter is in parliament when the referendum is held, and the outcome is the canton's share of yes votes. Because parliamentary replacements can cause actual parliamentary entry to differ from the electoral outcome, we estimate a fuzzy RD design. Table A1 in the appendix reports summary statistics for the running variable and the number of opposing races by canton and election outcome. Figure B2 shows the distribution of opposing races across cantons and referenda.

We estimate the LATE of having an additional issue supporter as a representative on constituents' referendum support using the following RD model:

$$
\begin{aligned}
yes\ share\ voters_{cr} = \alpha + \beta_1 supporter\ in\ parliament_{cr} + \\
f(vote\ margin\ supporter_{cr}) + \\
supporter\ in\ parliament_{cr} \times g(vote\ margin\ supporter_{cr}) + \epsilon_{cr}
\end{aligned} \tag{1}
$$

The outcome $yes\ share\ voters_{cr}$ is the share of yes votes in canton c on referendum r. The running variable $vote\ margin\ supporter_{cr}$ is the relative vote margin of the policy-supporting marginal candidate. The treatment indicator $supporter\ in\ parliament$ equals one if the supportive candidate is in parliament when referendum r is held and zero if the opposing candidate is in parliament. The vote margin does not perfectly assign actual parliamentary entry because elected candidates may resign or decline to take office. The functions $f(\cdot)$ and $g(\cdot)$ allow for flexible, potentially asymmetric relationships between the running variable and the outcome. The coefficient of interest β_1 captures the local average treatment effect of electing an issue supporter on the constituency's referendum support.

Because the vote margin does not perfectly determine parliamentary entry, we implement a fuzzy RD design. We use the indicator that the supporter's vote margin is weakly positive, $\mathbf{1}\{vote\ margin\ supporter_{cr} \geq 0\}$, as an instrument for the supporter's actual parliamentary entry. We estimate the model using local polynomial regression following Calonico, Cattaneo, and Titiunik (2014). Figure B3 in the appendix shows the probability that an issue supporter enters parliament as a function of their vote margin. In our RD sample, 975 of the 1,366 observations have negative vote margins. Among these, 346 are non-compliers, meaning that 35.5 percent of observations below the cutoff come from legislators who nonetheless entered parliament.

4.1 Sample validity

The credibility of this design depends on whether observations just above and below the cutoff are comparable. We assess this in three ways. First, we test for discontinuities in the density of the running variable at the cutoff. Second, we test whether observations near the cutoff are similar in terms of observable characteristics. Third, since constituents' referendum support likely depends on the positions of all legislators in a canton, we test whether the election of an

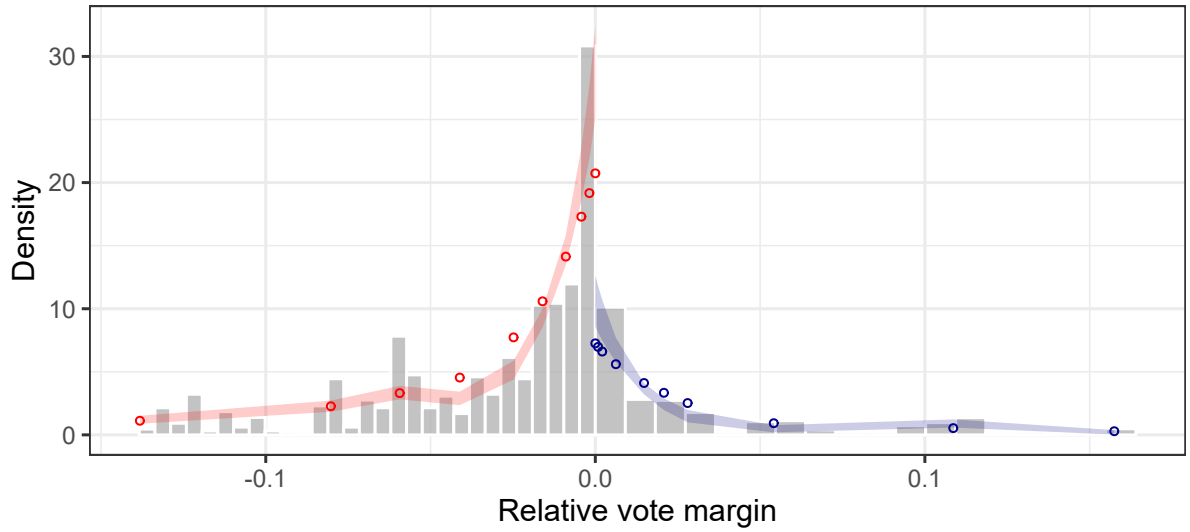
issue supporter in an opposing race significantly changes the share of issue supporters among the canton's legislators.

We test for discontinuities in the density of the vote margin at the cutoff following Cattaneo, Jansson, and Ma (2020). Figure 3 shows a discontinuity: there are more observations just below the cutoff, where the supporter barely lost, than just above it, where the supporter barely won. A jump in the density would usually imply endogenous sorting. In our design, this would mean that issue supporters strategically lost close elections with subsequent referenda on issues they support, which is highly unlikely. The full candidate sample confirms this interpretation. Figure 1 shows a continuous density at zero across all candidates regardless of their issue positions.

The jump is specific to the RD sample. The likely source is asymmetric noncompliance. In our fuzzy design, some candidates with negative margins enter parliament because the original winner resigned and was replaced by the next candidate on the party list. These noncompliant observations remain in the RD sample on the left side of the cutoff, inflating the number of observations with negative margins relative to positive ones. We confirm this directly. When we reconstruct the RD sample using the electoral outcome to define the marginal candidate rather than parliamentary presence, the 346 noncompliant observations shift from the left side of the cutoff to the right. The density test reverses direction: there are now significantly more observations above the cutoff than below ($T = 4.39, p < 0.001$). The noncompliant observations mechanically drive the imbalance.

Figure B4 in the appendix confirms that this does not threaten identification. Excluding observations within ± 0.0002, ± 0.0004, ± 0.0006, and ± 0.0008 of the cutoff yields stable, statistically significant estimates throughout. Strategic sorting would concentrate bias near the threshold and produce shrinking estimates as observations close to the cutoff are removed. We do not observe this.

Second, we examine whether observable covariates differ at the cutoff. We estimate sharp RD specifications using these covariates as outcomes, with optimal bandwidth selection for inference as proposed by Cattaneo, Idrobo, and Titiunik (2020). The results in Figure 4 show that observed covariates are balanced at the cutoff at the 99 percent confidence level. However, six covariates differ at the cutoff at the 95 percent level (Table A3). These are the Yes-voting recommendations by the Parliament and the Federal Council, the number of supportive and neutral advertisements, and the economy and infrastructure topics.



Notes: Local polynomial density estimation for issue supporters and opponents in opposing electoral races based on Cattaneo, Jansson, and Ma (2020).

Figure 3: RD validity—Density of referendum-supporter vote margin in the RD sample

The imbalances in Parliament and Federal Council yes-vote recommendations deserve particular attention, as these are important cues on which voters rely when deciding on referenda. Note that the coefficients on both variables are *negative*. Close supporter victories are systematically more likely on proposals where Parliament and the Federal Council recommended voting *no*. This means that, if anything, the imbalance works *against* our finding. The imbalance, therefore, biases our estimate downward rather than upward and cannot explain the positive effect we document. To confirm this formally, Table A4 reports the baseline RD estimate controlling directly for indicators equal to one if the Parliament or the Federal Council recommends voting yes as covariates. The coefficient on $supporter\ in\ parliament$ is larger than our baseline estimate of 0.087, consistent with the conservative direction of the imbalance. Furthermore, specifications with referendum fixed effects, which absorb all referendum-level variation, yield similar estimates (see Table A5 in the appendix).

The imbalances on the number of supportive and neutral advertisements per referendum are likely mechanical. Referenda where a supporter closely wins may feature slightly more pro-yes campaign activity. The economy and infrastructure topic imbalances are absorbed by the referendum fixed effects specifications, which yield similar estimates (Table A5).

Finally, because constituents' policy preferences potentially depend on the positions of all legislators in a canton, we assess whether a supporter's victory in an opposing race increases

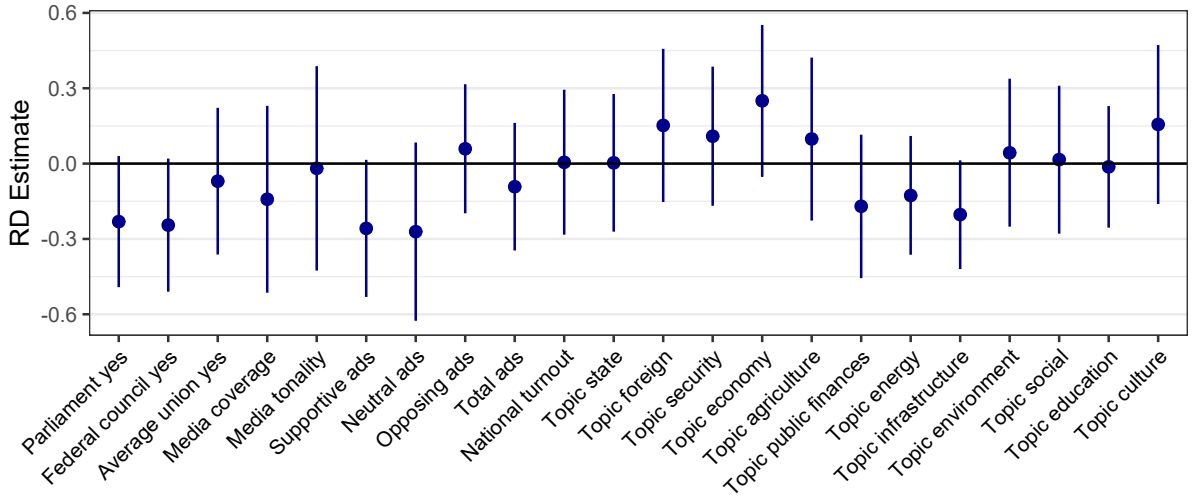


Figure 4: RD validity—Covariate balance test

Notes: The figure shows robust and bias-corrected ITT RD estimates and 99% confidence intervals for various covariates using local linear regression with a triangular kernel and coverage error rate optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Table A2 provides descriptions of the covariates. Table A3 in the Appendix provides formal results.

the share of issue supporters among the legislators. Table A6 in the appendix shows that issue supporter victories in close opposing races increase the share of issue supporters among the legislators of a canton.

5 Results

5.1 Baseline results

This section presents fuzzy RD results for the effect of having an issue supporter as a representative on the electorate's support for the same policy proposal. We use local linear regression with robust, bias-corrected estimates and optimal bandwidth selection following Calonico, Cattaneo, and Farrell (2019) and Calonico, Cattaneo, and Titiunik (2014). As cantons vary in their number of representatives (district magnitude), we weight observations by the inverse of district magnitude to account for the stronger influence that legislators from smaller cantons may exert on voter preferences. The weighted coefficient is therefore the effect of moving a canton's marginal representative from opponent to supporter, not the effect of its full delegation.

Figure 5 plots the canton yes-share against the issue supporter's vote margin. Referendum support jumps upward discontinuously at the electoral threshold, where an issue supporter narrowly enters parliament instead of an issue opponent.

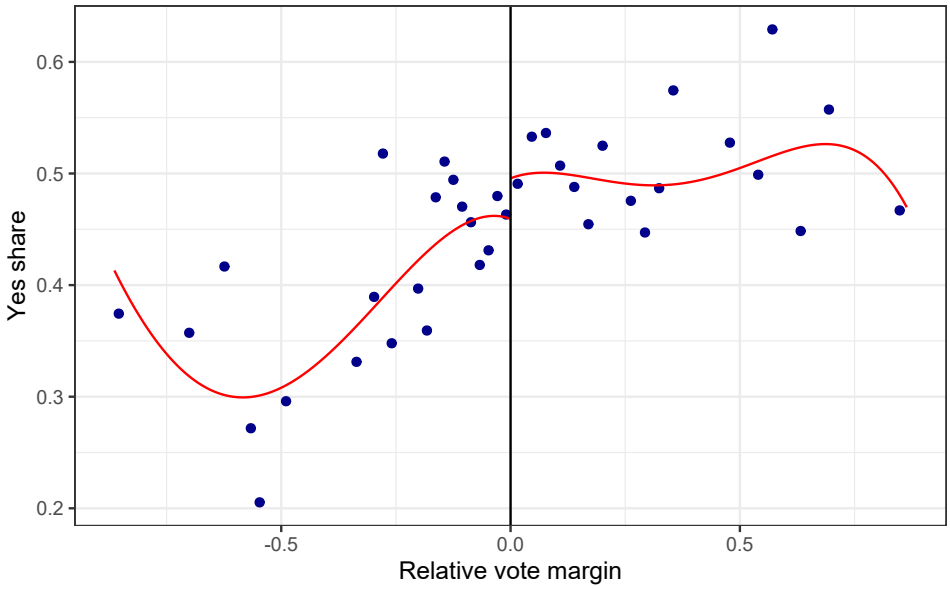


Figure 5: Referendum support and the issue supporter's vote margin

Notes: The figure shows binned means of the canton yes-share against the issue supporter's vote margin, with a fourth-order polynomial fit on each side of the cutoff, following Calonico, Cattaneo, and Titiunik (2015a,b). The vertical line marks the electoral threshold, where an issue supporter narrowly enters parliament instead of an issue opponent. The plot displays the reduced-form relationship. Table 1 reports the fuzzy LATE.

To quantify this shift, we use the Mean Square Error (MSE) optimal bandwidth proposed by Calonico, Cattaneo, and Farrell (2019) in our main specification (1) in Table 1. The estimated bandwidth is 0.037. The analysis, therefore, includes candidates who either needed votes from an additional 3.7 percent of voters to be elected or would not have been elected had they not received votes from 3.7 percent of the voters. Using the MSE-optimal bandwidth, the fuzzy RD coefficient of 0.087 implies that, among the opposing races, electing the issue supporter increases the canton yes-share in the subsequent referendum by 8.7 percentage points. Relative to the sample mean of 47.36 percent, this corresponds to an increase of about 18 percent, raising support to roughly 56.1 percent. Note, however, that the estimate is local. It is identified from the 748 canton-referendum observations that fall within the optimal bandwidth, drawn from the 1,366 opposing races, rather than from the full set of 6,838 canton-referendum outcomes. The coefficient should therefore be interpreted as the effect of marginal changes in parliamentary representation in close races, not as an average effect across all cantons and referenda.

As the number of observations differs on either side of the cutoff, Specification (2) uses two different MSE-optimal bandwidths for each side. The estimated coefficient remains sta-

Table 1: Effect of an issue supporter in parliament on referendum support

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.087***	0.101***	0.112***	0.127***
	(0.011)	(0.013)	(0.011)	(0.013)
First stage estimates	0.782***	0.770***	0.779***	0.762***
	(0.012)	(0.016)	(0.013)	(0.017)
N	1366	1366	1366	1366
$N_{effective}$	531;217	443;240	499;207	414;221
BW type	mserd	msetwo	cerrd	certwo
BW size	0.037	0.024;0.062	0.030	0.019;0.051
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.360	47.494	47.110	47.235

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

tistically significant, though slightly smaller. To assess interference, Specification (3) uses one common CER-optimal bandwidth, and Specification (4) uses two different CER-optimal bandwidths. Both specifications confirm that electing a supportive candidate in an opposing race has a positive and significant effect on voter policy support.

5.2 Robustness and validity

We first ensure that the design is adequately powered. Using the robust canton-clustered standard error, the minimum detectable effect at 80 percent power and the 5 percent level is 3.1 percentage points, which is below the estimated 8.7 percentage points. Table A7 in the appendix reports the statistical power to detect a range of effect sizes. We conclude that the estimate is precise enough to identify effects of the size we find.

We follow the RD validity test proposed by Cattaneo, Idrobo, and Titiunik (2020) to provide further evidence for the validity of our baseline results. Figure 6 shows that our results are robust across different bandwidths. Using various percentages of the MSE-optimal bandwidth yields statistically significant positive coefficients. The estimated LATE declines monotonically as the bandwidth widens. The decline may be partly mechanical, as wider bandwidths include electoral races farther from the cutoff, where electoral outcomes are less likely to be random.

Next, we re-estimate the RD at a series of placebo cutoffs on either side of the true threshold. We restrict the sample to observations on the side of the true cutoff where the placebo cutoff is located and re-center the running variable at each placebo value. We use one placebo

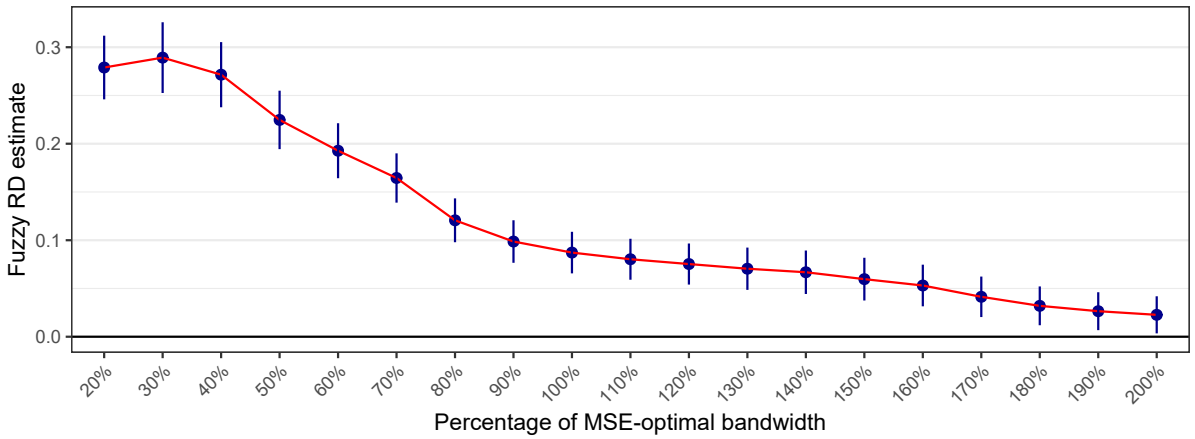


Figure 6: RD validity—Estimates across bandwidths

Notes: The figure shows robust and bias-corrected fuzzy RD estimates and 95% confidence intervals using local linear regression with a triangular kernel and MSE-optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b), at 20%-200% of the MSE-optimal bandwidth. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter being in parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Table A13 in the appendix provides formal results.

cutoff inside (± 0.03) and one outside (± 0.06) the MSE-optimal bandwidth (0.037) on both sides of the true cutoff. Estimating the ITT at artificially chosen cutoffs, shown in Figure B5 in the appendix, yields an effect only at the true cutoff. Figure B4 in the appendix presents results from "donut-hole" specifications, which assess how sensitive our results are to observations very close to the cutoff by excluding them. Estimations excluding observations within ± 0.0002, ± 0.0004, ± 0.0006, and ± 0.0008 all yield statistically significant coefficients.

To verify that the estimated effect is not driven by differences across referenda or persistent differences across cantons, we re-estimate the model with the corresponding fixed effects. Our results remain positive and statistically significant with referendum fixed effects, canton fixed effects, or both (see Table A5 in the appendix). Using epanechnikov or uniform kernel functions also does not alter our results (see Table A10 in the appendix). We also obtain similar results when using local quadratic or local cubic specifications (see Table A11 in the Appendix). As a placebo, we re-estimate the design using the national yes-share as the outcome, which a single canton's marginal seat should not affect (Table A12 in the appendix).

To test whether the estimated effects are sensitive to how electoral closeness is measured, we further estimate our model using (1) the vote margin divided by the number of *seats* a canton has in the National Council (district magnitude) and (2) the vote margin divided by the number of *eligible voters* in the canton. Estimations using both measures confirm our previous results,

as Tables A14 and A15 in the appendix show. In addition, Table A16 in the appendix shows that we obtain similar results without weighting observations.

Our fuzzy RD design identifies a local average treatment effect for compliers. As a robustness check, we also estimate intention-to-treat (ITT) effects, which capture the effect of a supporter narrowly winning the election regardless of whether the candidate ultimately enters parliament (Kaliski, Keane, and Neal, 2025). The ITT estimates in Table A17 in the appendix confirm our main findings and, as expected, are smaller. The ITT captures the effect of electoral assignment, while the fuzzy LATE scales it up by the first-stage compliance rate. The ratio of the ITT to the LATE recovers the share of compliers near the cutoff.

In our main analysis, we measure the positions of elected and non-elected candidates differently. We use roll-call votes in parliament to measure the positions of elected candidates and the party line to measure the positions of non-elected candidates. To avoid potential bias arising from different measurement methods, we also construct the RD sample using national and cantonal party lines as approximations for the issue positions of both elected and non-elected candidates. By estimating our main specification in this sample, we essentially estimate the LATE of a seat gain from supporting versus opposing parties. The results presented in Tables A18 and A19 in the appendix confirm our main findings. As an additional test of the position approximation for non-elected candidates, we restrict the main sample to votes with high party cohesion in parliament, excluding referenda where many legislators deviated from the party line (above the 90th percentile, corresponding to more than 30 out of 200 legislators). In these votes, party lines provide a particularly accurate issue position proxy. Re-estimating the main specification on this restricted sample yields very similar results (see Table A20 in the Appendix).

As a further robustness check, we replicate the analysis using individual-level voting data from the VOX post-referendum surveys. In the main RD design, we link the vote margin of the supportive candidate in each opposing race to the official cantonal referendum result. In this alternative sample, we instead link the same vote margin to the self-reported votes of VOX respondents in the corresponding canton and referendum. This produces multiple individual voting decisions for each opposing race. The approach provides a micro-level validation of the aggregate results and reduces the concern that they arise from cantonal aggregation rather than changes in individual voting behavior. Although the VOX surveys are not representative at the cantonal level, they provide useful variation for this purpose. We estimate the same

specifications as in the main analysis and apply the baseline bandwidth because the running variable is unchanged.

The results in Table A21 in the appendix confirm the main findings. The estimated effects remain statistically significant across specifications and are slightly larger than in the aggregate analysis. Table A22 further shows that the results are robust to alternative bandwidths defined as different percentages of the baseline bandwidth.

Finally, Table C1 in the Appendix reports IV[8] estimates instrumenting the total share of issue supporters among a district's legislators with the share of *closely elected* supporters (Baskaran and Hessami, 2023; Clots-Figueras, 2011). The IV coefficients are consistent with the baseline RD estimates. In the IV strategy, a closely elected supporter may replace either an issue opponent or another supporter. We treat the IV results as a secondary robustness check, not an independent identification strategy. Section C in the Appendix provides more details on the design and results.

5.3 Vote switching or turnout

Changes in the referendum support can arise through vote switching or mobilization. To measure how much of our baseline effect is driven by mobilization, we re-estimate the main specification with the difference between cantonal and national referendum turnout as the outcome variable.[9] We find that an issue supporter entering parliament raises relative turnout by 2.3 percentage points (Table A23 in the appendix). Mobilization thus occurs, but it does not explain the entire 8.7 percentage point rise in the share of yes votes: Just below the cutoff, where the supporter narrowly lost, the electorate votes at an average turnout of 49.0 percent and a share of yes votes of 46.9 percent. An increase in turnout by the estimated 2.3 percentage points, with every additional voter voting yes, would result in the share of yes votes increasing to 49.3 percent. Mobilization alone thus moves the share of yes votes by at most 2.4 percentage points, about a quarter of the baseline effect.[10]

The main effect is therefore also driven by changes in voting behavior. To see how votes change, we re-estimate the baseline design with the number of yes votes and the number of no

[8] For examples of IV strategies applied to similar research questions, see (Essig et al., 2021; Freier and Odendahl, 2015; Gabel and Scheve, 2007); for overviews, see (Mogstad and Torgovitsky, 2018; Sovey and Green, 2011).

[9] Since national turnout does not differ at the threshold (see Table A3 in the appendix), the difference accounts for nationwide variation in turnout across referenda.

[10] With the upper bound of the confidence interval of the turnout estimate, mobilization accounts for at most 3.0 percentage points, one third of the baseline effect.

votes per eligible voter as outcomes. Table A23 in the appendix shows that an issue supporter entering parliament increases yes votes per eligible voter by 4.2 percentage points but also reduces no votes per eligible voter by 2.0 percentage points.[11] The decline in no votes suggests that some citizens who would have voted no under an issue opponent instead vote yes or abstain under an issue supporter.[12] Table A25 in the appendix reports the turnout and deviation effects in full, as both intention-to-treat and fuzzy estimates.

Our results provide evidence that legislators' issue positions causally affect citizens' decisions in direct democratic votes. The 8.7 percentage-point increase in the share of yes votes when an issue supporter, rather than an issue opponent, is in parliament represents a substantial influence on the outcomes of popular votes. As voters decide on the same policy proposal in referenda that legislators previously decided on in parliament, our results are consistent with representative influence extending beyond agenda-setting to affect citizen preferences on specific policies.

6 Conditions for legislative influence

We established *that* legislators' issue positions causally influence voters' decisions on identical issues in direct democratic votes. In this section, we examine *when* such influence is most likely to arise. Based on the previous literature, we expect that if voters use legislators' positions as informational cues when deciding on referenda, their influence should be stronger when other policy information is scarce and when their positions are more salient. To test these predictions, we re-estimate the baseline fuzzy RD design on five prespecified moderators motivated by the cue mechanism. Three capture information scarcity (complexity, importance, and media coverage) and two capture legislator salience (campaign balance and media tonality). We split the sample at the median of each moderator and estimate each subgroup effect at its own MSE-optimal bandwidth (Calonico, Cattaneo, Farrell, et al., 2025). Because the subgroup analyses reduce sample size and involve multiple comparisons, we report Benjamini-Hochberg-adjusted p-values for the subgroup differences (Benjamini and Hochberg, 1995). As a robustness check, Table A27 in the appendix reestimates the subgroup estimates using the

[11] Consistent with this, re-estimating the baseline model with the log of yes votes per eligible voter as the outcome, a measure that does not net out turnout mechanically, yields a positive and statistically significant effect (Table A24 in the appendix).

[12] Simply having a close opposing electoral race before the referendum barely affects turnout. In the full sample of 6,765 canton-referendum pairs with available turnout information, turnout in cantons with a close opposing race exceeds national turnout by at most 0.7 percentage points ($p < 0.1$, Figure B6).

unified, theoretically grounded framework for analyzing RD heterogeneity of Calonico, Cattaneo, Farrell, et al. (2025). That estimator fits the subgroups jointly and adjusts the difference test for their shared clustering. We do not use this framework as our primary approach for analyzing heterogeneity because it is currently limited to sharp RDs, which only allows us to estimate ITT coefficients. We interpret the results as suggestive evidence on the informational-cue mechanism, not as independent causal estimates.

Motivated by the argument that "uncertainty gives rise to persuasion" (Downs, 1957), we first examine whether legislators' influence is stronger in low-information environments. Prior research shows that voters rely more heavily on heuristics, such as legislators' positions, when information is scarce, costly, or difficult to process (Cohen, 2003; Dhami, al-Nowaihi, and Sunstein, 2019; Grewenig et al., 2020). We therefore use several proxies for information scarcity. In particular, we consider referenda that receive limited media coverage, as well as referenda that survey respondents rate as complex or unimportant in post-referendum surveys. We aggregate these responses to the referendum level by calculating the average reported complexity (binary measure) and importance (0–10 scale) for each vote and then split referenda at the median of these measures.

Second, we examine whether legislators' influence is stronger when their positions are more salient (Chan et al., 2019; Murphy and Shleifer, 2004). Salience may increase when campaigns are intense or when media coverage favors the legislator's position.[13] We use campaign balance and media tonality as proxies for the salience of the legislator's position.

Figure 7 presents the results. In each panel, dot size scales with the number of observations. Red (blue) coefficients are (not) statistically significant at the 5% level. The Δp label reports the Benjamini-Hochberg-adjusted p-value of the difference between the two subgroup estimates.

Panel A of Figure 7 shows point estimates that mostly line up with the low-information mechanism, though the subgroup differences are imprecise. The effect is larger and statistically significant for complex referenda and near zero for less complex ones. The difference points as predicted but just misses significance after the multiple-testing correction (Benjamini-Hochberg $\Delta p = 0.051$). Media coverage points the same way, with a larger and significant effect where coverage is limited, though the two groups do not differ

[13] As campaign funding disclosure became mandatory only in 2024, we follow Sciarini and Tresch (2011) and Goldberg, Lanz, and Sciarini (2019) and use the share of advertisements promoting a yes vote as a proxy for supporter campaign intensity.

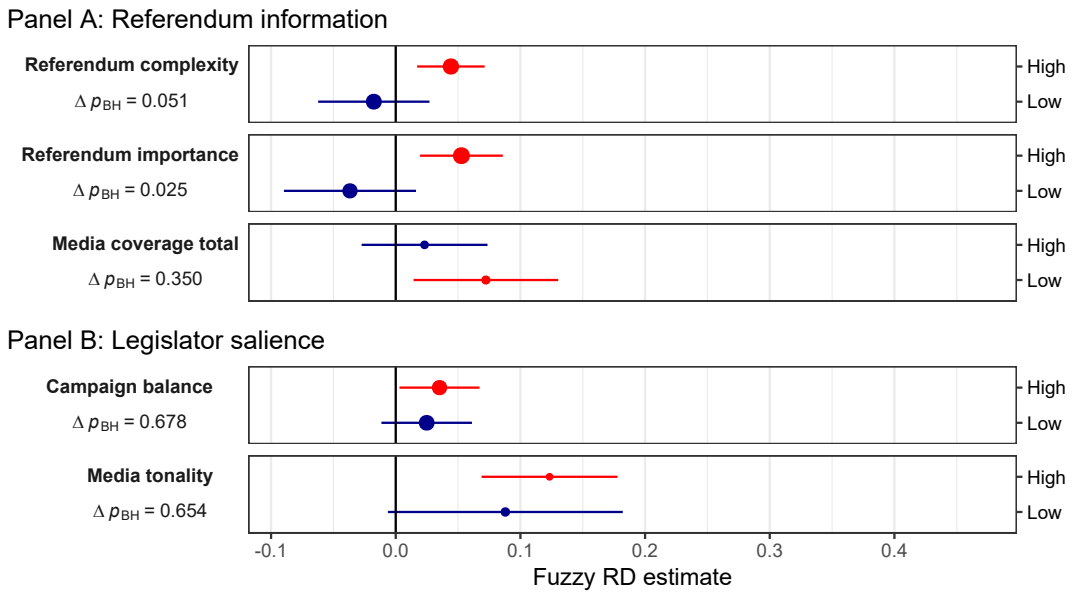


Figure 7: Effect of an issue supporter in parliament on referendum support—Moderator analyses

Notes: The figure shows bias-corrected fuzzy RD estimates with 95% confidence intervals from local linear regressions estimated separately within each subsample, each at its own MSE-optimal bandwidth, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Each moderator is split at its median. Dot size is proportional to the number of observations. Red (blue) coefficients are (not) statistically significantly different from zero at the 5% level. The Δp label reports the Benjamini-Hochberg-adjusted p-value from a z-test of the difference between subgroup estimates, using bias-corrected estimates and robust standard errors (Benjamini and Hochberg, 1995). Table A26 in the Appendix reports the full numerical results.

significantly. Importance is the exception. The effect is larger for important referenda, the only difference that survives the correction ($\Delta p = 0.025$). This runs against the prediction that cues matter less when voters are more motivated to inform themselves. One reconciliation is that perceived importance tracks how salient an issue is for legislators, so the split may capture cue salience rather than information scarcity.

Panel B examines the salience of legislator cues. The effect is larger where supportive campaigns dominate and where media coverage favors the proposal, both as predicted and significant in those subgroups. Neither difference is statistically significant, so the salience channel is at most suggestive.

Taken together, the directions of the moderator estimates are consistent with the cue mechanism. For every moderator except importance, the effect is larger in the subgroup where cues should matter more, and it is significant there. The differences between subgroups, however, are mostly indistinguishable from zero after the multiple-testing correction. The sharp intention-to-treat estimator of Calonico, Cattaneo, Farrell, et al. (2025), reported in Appendix

Table A27, likewise finds no significant subgroup difference. We therefore read the heterogeneity as suggestive support for the mechanism rather than proof.

7 Conclusion

This paper studies whether elected representatives shape policy even when citizens have the final say in direct democratic votes. We use Switzerland's system of direct and representative democracy, which allows us to compare legislators' decisions in parliament with their electorate's support on identical policy proposals in referenda. We observe the outcomes of 263 referenda held between 1996 and 2026 across Switzerland's 26 cantons and match them to the positions and electoral closeness of all lower house candidates from the same canton who ran in the election held before that referendum. To estimate causal effects, we exploit close elections between candidates with opposing positions on referendum proposals as a source of exogenous variation in legislators' issue positions in a regression discontinuity design. When the policy-supporting candidate closely wins rather than closely loses such a race, the composition of parliamentary representation shifts for reasons that are plausibly unrelated to the electorate's subsequent referendum preferences.

We find that an additional issue supporter among a canton's representatives increases the constituency's share of yes votes in the subsequent referendum by 8.7 percentage points. The finding holds across alternative bandwidths, fixed effects, kernels, polynomials, and an instrumental variable approach. Decomposing the effect into vote flows shows that it works through citizens who would otherwise vote no and either switch or abstain. Changes in turnout account for at most one third of the effect. The moderator analyses show that the effect is larger where cues should matter more, such as complex or low-coverage referenda. This pattern is consistent with voters leaning on legislators' cues when independent information is scarce. These subgroup differences are not statistically significant, so we treat the cue interpretation as consistent with the evidence, not as established by it. The result implies that even in institutions designed to let citizens overrule politicians, the composition of elected representation can causally shift binding policy outcomes.

Our findings carry normative implications for democratic institutions. Representative democracy rests on a principal-agent relationship in which voters delegate power to legislators (Bianco, 1994; Persson and Tabellini, 2000). Standard principal-agent models predict that

information asymmetries can generate moral hazard. Legislators with private information may pursue their own interests over those of constituents (Grossman and Helpman, 2001). Our results point to a complementary channel. Even when voters retain formal authority through direct democratic instruments, information asymmetry lets legislators shape how that authority is exercised.

What remains unclear is how this affects the quality of political decisions. Where legislators are accountable and disciplined by electoral incentives, their influence on the outcomes of direct democratic votes may improve their quality. Legislators with superior policy knowledge provide signals that reduce the cost of information acquisition (Downs, 1957). Where interests diverge, direct democracy may not fully override politicians' policy agendas. To assess the welfare implications of our findings, future research should examine whether legislative influence is stronger when the interests of constituents and legislators diverge. This may be the case for policies with concentrated benefits and diffuse costs, or for referendum decisions on parliamentary proposals rather than popular initiatives.

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Appendix for

Decision-Makers as Decision-Shapers: Evidence on the Influence of Representatives on Direct Democratic Votes[1]

Yves Kläy, Reiner Eichenberger, Marco Portmann, David Stadelmann

Appendix A: Additional tables

Table A1: RD sample summary statistics by canton and election outcome

	not in parliament						in parliament					
	observations		vote margin				observations		vote margin			
	total	distinct	mean	sd	min	max	total	distinct	mean	sd	min	max
AG	38	11	−0.012	0.011	−0.031	0.000	46	9	−0.041	0.029	−0.078	0.004
AI	16	3	−0.498	0.147	−0.561	−0.064	12	2	0.429	0.220	0.064	0.561
AR	55	7	−0.081	0.078	−0.208	−0.002	31	5	0.074	0.083	0.002	0.204
BE	9	5	−0.003	0.001	−0.005	−0.001	26	9	−0.022	0.015	−0.043	0.001
BL	21	5	−0.034	0.021	−0.059	−0.001	33	7	−0.118	0.073	−0.183	0.046
BS	23	6	−0.032	0.039	−0.100	−0.001	21	7	−0.066	0.072	−0.151	0.033
FR	13	5	−0.011	0.010	−0.043	0.000	17	5	0.005	0.012	−0.003	0.043
GE	14	6	−0.005	0.005	−0.013	0.002	25	9	−0.014	0.034	−0.079	0.015
GL	31	5	−0.226	0.119	−0.342	−0.063	26	3	0.226	0.120	0.063	0.342
GR	12	3	−0.034	0.016	−0.043	−0.006	9	5	0.015	0.010	0.002	0.026
JU	20	8	−0.049	0.061	−0.156	−0.003	22	6	0.041	0.058	0.001	0.156
LU	14	6	−0.010	0.004	−0.013	−0.001	31	9	−0.056	0.036	−0.133	0.011
NE	47	8	−0.027	0.008	−0.034	−0.006	36	8	−0.063	0.099	−0.283	0.034
NW	28	5	−0.405	0.251	−0.705	−0.054	36	3	0.441	0.272	0.054	0.705
OW	21	5	−0.186	0.306	−0.864	−0.004	30	5	0.317	0.307	0.004	0.864
SG	17	5	−0.025	0.012	−0.041	−0.009	22	7	−0.088	0.085	−0.178	0.036
SH	31	6	−0.114	0.057	−0.165	−0.004	54	3	0.027	0.084	−0.114	0.167
SO	16	4	−0.015	0.017	−0.035	0.000	36	5	−0.010	0.024	−0.073	0.022
SZ	33	7	−0.057	0.029	−0.080	−0.002	31	6	0.041	0.066	−0.138	0.109
TG	11	6	−0.034	0.023	−0.058	−0.012	11	4	−0.031	0.072	−0.108	0.071
TI	22	7	−0.016	0.009	−0.031	0.000	19	7	−0.051	0.045	−0.084	0.029
UR	29	5	−0.296	0.192	−0.492	−0.028	22	3	0.157	0.180	0.028	0.492
VD	31	6	−0.002	0.003	−0.013	0.000	47	8	−0.042	0.055	−0.184	−0.003
VS	26	7	−0.017	0.008	−0.029	−0.002	20	9	−0.029	0.078	−0.192	0.031
ZG	17	6	−0.048	0.019	−0.057	−0.002	15	5	0.058	0.049	0.002	0.113
ZH	35	13	−0.003	0.002	−0.009	0.000	58	10	−0.010	0.010	−0.049	0.001
Total	630	160	−0.090	0.157	−0.864	0.002	736	159	0.038	0.179	−0.283	0.864

Notes: The table presents summary statistics for the number of total and distinct MP observations and relative vote margins of issue supporters in close opposing electoral races in the RD sample. MPs in parliament may have negative vote margins because of resignations of initially elected candidates.

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Table A2: Variable descriptions

Variable	Description	Source
Yes share voters$_{cr}$	Share of yes votes (in percent) cast by voters in canton c on referendum r.	Swissvotes, 2026
Vote margin$_{ir}$	Electoral margin by which politician i was elected before referendum r based on Luechinger, Schelker, and Schmid, 2024.	FSO
In parliament$_{ir}$	Indicator equal to 1 if politician i was a member of the National council when referendum r was held, 0 otherwise.	Parliamentary Services, 2025b
Elected$_{ir}$	Indicator equal to 1 if politician i had a positive vote margin when referendum r was held, 0 otherwise.	FSO
Decision$_{ir}$	Indicator equal to 1 (0) if politician i voted yes (no) on the policy proposal behind referendum r.	Parliamentary Services, 2025b
Party position$_{ir}$	Indicator equal to 1 (0) if politician i's party recommended voting yes (no) on referendum r.	Swissvotes, 2026
District magnitude$_{ir}$	Number of seats of politician i's canton when referendum r was held.	Swissvotes, 2026
Parliament date$_r$	Date on which the policy proposal behind referendum r was held.	Parliamentary Services, 2025b
Referendum date$_r$	Date on which referendum r was held.	Swissvotes, 2026
Referendum type$_r$	Factor variable for the type of referendum r (mandatory, optional, or initiative).	Swissvotes, 2026
Parliament yes$_r$	Indicator equal to 1 if a majority of the National Council voted yes on the policy behind referendum r.	Swissvotes, 2026
Federal council yes$_r$	Indicator equal to 1 if the Federal Council recommended voting yes on referendum r, 0 otherwise.	Swissvotes, 2026
National turnout$_r$	National voter turnout (in percent) in referendum r.	Swissvotes, 2026
Cantonal turnout$_{cr}$	Voter turnout (in percent) in canton c in referendum r.	Swissvotes, 2026
Votes same day$_r$	Number of referenda held on the same day as referendum r.	Swissvotes, 2026
Complexity$_r$	Average complexity (1–2) of referendum r as reported in post-referendum surveys (VOX). Respondents answer: "Did you find it rather easy (indicator = 1) or rather difficult (indicator = 2) to understand what [Referendum Title] was about?"	gfs.bern
Importance$_r$	Average importance (0–10) of referendum r as reported in post-referendum surveys (VOX). Respondents answer: "On a scale from 0 (not at all important) to 10 (very important), how important was [referendum X] for you personally?"	gfs.bern
Average union yes$_r$	Average voting recommendation for referendum r by the six largest interest groups (Economiesuisse, Schweizerischer Gewerbeverband, Schweizer Bauernverband, Schweizerischer Gewerkschaftsbund, Travail.Suisse, Schweizerischer Arbeitgeberverband)	Swissvotes, 2026
Media coverage$_r$	Number of articles in major print and online outlets in the 12 weeks before referendum r, excluding the final week.	Swissvotes, 2026
Media tonality$_r$	Continuous measure of media tone (-100 = all negative coverage, +100 = all positive) in major outlets during weeks 2–12 before referendum r, excluding the final week.	Swissvotes, 2026
Supportive ads$_r$	Number of advertisements promoting a yes vote on referendum r that appeared in around 50 print media outlets.	Swissvotes, 2026
Neutral ads$_r$	Number of neutral advertisements on referendum r.	Swissvotes, 2026
Opposing ads$_r$	Number of advertisements promoting a no vote on referendum r.	Swissvotes, 2026
Total ads$_r$	Number of advertisements on referendum r (supportive, neutral, opposing).	Swissvotes, 2026
Supportive ads share$_r$	Percentage of supportive advertisements, excluding neutral advertisements.	Swissvotes, 2026
Topic area$_r$	Factor variable for the policy area of referendum r (State organisation; Foreign policy; Security; Economy; Agriculture; Public finance; Energy; Transport and infrastructure; Environment and living; Social; Education and research; Culture).	Swissvotes, 2026

Table A3: RD validity—Formal covariate balance test

Variable	RD est.	Robust p-val	BW	$N_{effective}$	CI lower	CI upper
Parliament yes	−0.231	0.022	0.054	585;233	−0.492	0.030
Federal council yes	−0.245	0.017	0.054	582;228	−0.510	0.020
Average union yes	−0.070	0.535	0.061	600;224	−0.362	0.222
Media coverage	−0.142	0.326	0.084	362;113	−0.514	0.230
Media tonality	−0.019	0.904	0.070	319;113	−0.426	0.388
Supportive ads	−0.258	0.015	0.059	575;214	−0.531	0.015
Neutral ads	−0.271	0.049	0.109	337;117	−0.626	0.084
Opposing ads	0.059	0.552	0.059	588;214	−0.198	0.316
Total ads	−0.092	0.352	0.067	632;231	−0.346	0.162
National turnout	0.005	0.962	0.064	670;250	−0.283	0.294
Topic state	0.003	0.979	0.073	693;252	−0.271	0.277
Topic foreign	0.152	0.199	0.072	693;252	−0.153	0.457
Topic security	0.109	0.311	0.081	726;252	−0.168	0.386
Topic economy	0.250	0.033	0.061	653;240	−0.053	0.552
Topic agriculture	0.098	0.439	0.068	675;250	−0.227	0.422
Topic public finances	−0.170	0.125	0.058	618;233	−0.456	0.115
Topic energy	−0.127	0.168	0.078	697;252	−0.363	0.110
Topic infrastructure	−0.203	0.016	0.057	588;233	−0.420	0.013
Topic environment	0.043	0.705	0.076	697;252	−0.251	0.338
Topic social	0.016	0.890	0.066	670;250	−0.279	0.310
Topic education	−0.013	0.890	0.085	741;252	−0.255	0.229
Topic culture	0.156	0.205	0.090	742;252	−0.161	0.472

Notes: The table reports robust and bias-corrected sharp RD estimates for various covariates using local linear regression with a triangular kernel and coverage error rate optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Table A2 provides descriptions of the covariates.

Table A4: Effect of an issue supporter in parliament on referendum support—Voting-recommendation controls

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.117***	0.161***	0.173***	0.165***
	(0.012)	(0.010)	(0.013)	(0.011)
First stage estimates	0.778***	0.743***	0.769***	0.746***
	(0.013)	(0.013)	(0.015)	(0.014)
N	1361	1361	1361	1361
$N_{effective}$	498;204	498;225	452;178	452;214
BW type	mserd	msetwo	cerrd	certwo
BW size	0.030	0.031;0.052	0.025	0.025;0.042
Recommendation controls	✓	✓	✓	✓
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.110	47.167	47.138	47.341

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Models include controls for Parliament and Federal Council referendum yes vote recommendations. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A5: Effect of an issue supporter in parliament on referendum support—Fixed effects

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Panel A: Referendum Fixed Effects				
Treatment ($supporter\ in\ parliament = 1$)	0.183***	0.146***	0.178***	0.133***
	(0.014)	(0.008)	(0.013)	(0.008)
First stage estimates	1.035***	0.785***	1.051***	0.852***
	(0.008)	(0.010)	(0.007)	(0.009)
N	1366	1366	1366	1366
$N_{effective}$	137;70	414;95	132;70	382;92
BW type	mserd	msetwo	cerrd	certwo
BW size	0.003	0.020;0.006	0.003	0.016;0.005
Referendum FE	✓	✓	✓	✓
p value	0.000	0.000	0.000	0.000
Mean DV in %	46.992	47.736	46.880	48.044
Panel B: Canton Fixed Effects				
Treatment ($supporter\ in\ parliament = 1$)	0.121***	0.142***	0.123***	0.153***
	(0.009)	(0.008)	(0.010)	(0.009)
First stage estimates	0.885***	0.899***	0.902***	0.906***
	(0.013)	(0.012)	(0.013)	(0.021)
N	1366	1366	1366	1366
$N_{effective}$	540;217	528;183	504;208	499;167
BW type	mserd	msetwo	cerrd	certwo
BW size	0.040	0.036;0.026	0.033	0.029;0.021
Canton FE	✓	✓	✓	✓
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.276	47.194	47.146	46.946
Panel C: Referendum and Canton Fixed Effects				
Treatment ($supporter\ in\ parliament = 1$)	0.354***	0.059***	0.395***	0.054***
	(0.009)	(0.003)	(0.036)	(0.002)
First stage estimates	0.955***	1.281***	0.912***	1.302***
	(0.013)	(0.021)	(0.038)	(0.020)
N	1366	1366	1366	1366
$N_{effective}$	63;53	460;55	46;49	426;50
BW type	mserd	msetwo	cerrd	certwo
BW size	0.002	0.026;0.002	0.001	0.022;0.001
Referendum FE	✓	✓	✓	✓
Canton FE	✓	✓	✓	✓
p value	0.000	0.000	0.000	0.000
Mean DV in %	48.690	47.359	48.857	47.511

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Estimations include referendum fixed effects in Panel A, canton fixed effects in Panel B, and referendum and canton fixed effects in Panel C. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.
The first-stage estimate exceeds one in some fixed-effects specifications. Fixed effects demean all variables by their group mean before estimation. After subtracting the group mean, the indicator for whether the issue supporter is in parliament is no longer bounded between zero and one, so the first-stage coefficient loses its probability interpretation. This does not imply a compliance rate above 100%. The second-stage LATE remains valid as the ratio of the reduced-form and first-stage coefficients within the fixed-effects transformation.

Table A6: Effect of a supporter winning an opposing race on the share of supporting legislators

Dependent variable	Yes share legislators			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.581***	0.663***	0.590***	0.655***
	(0.031)	(0.035)	(0.031)	(0.035)
First stage estimates	0.840***	0.775***	0.827***	0.768***
	(0.010)	(0.015)	(0.010)	(0.016)
N	1366	1366	1366	1366
$N_{effective}$	742;252	455;252	697;252	426;252
BW type	mserd	msetwo	cerrd	certwo
BW size	0.095	0.026;0.095	0.078	0.021;0.078
p value	0.000	0.000	0.000	0.000
Mean DV in %	49.419	53.295	49.694	54.077

Notes: Bias-corrected and robust point estimates and clustered standard errors at the canton level are shown in parentheses that relate the issue position of the winner of an opposing race to the share of yes votes among the legislators of canton c on the policy proposal with subsequent referendum r. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A7: Statistical power and minimum detectable effect

Effect size (pp)	Statistical power
2.0	0.444
4.0	0.953
6.0	1.000
8.0	1.000
8.7†	1.000

Notes: Power analysis for the main fuzzy RD specification (Specification (1) of Table 1), following the RD power framework of Cattaneo, Titiunik, and Vazquez-Bare (2019). Effect sizes are on the local average treatment effect scale, in percentage points of the canton yes-share. *Statistical power* is the probability of rejecting the null of no effect at the 5% level against an effect of the stated size, computed from the robust canton-clustered standard error of the estimated effect. † marks the estimated effect. The minimum detectable effect at 80% power is reported in the text.

Table A8: RD validity—Donut-hole

Donut-Hole radius	RD est.	Robust p-val	BW	N	$N_{effective}$	CI lower	CI upper
0	0.087	0.000	0.037	975;391	531;217	0.066	0.109
0.0002	0.086	0.000	0.036	965;384	518;209	0.063	0.108
0.0004	0.150	0.000	0.025	955;377	433;166	0.120	0.179
0.0006	0.147	0.000	0.025	942;376	420;165	0.117	0.177
0.0008	0.170	0.000	0.023	940;366	397;149	0.136	0.203

Notes: Formal results for Figure B4. The table reports robust and bias-corrected fuzzy RD estimates and 95% confidence intervals excluding certain observations near the cutoff using local linear regression with a triangular kernel and MSE-optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter being in parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out.

Table A9: RD validity—True and artificial cutoffs

Alternative cutoff	RD est.	Robust p-val	BW	$N_{effective}$	CI lower	CI upper
−0.06	−0.088	0.398	0.013	13;52	−0.355	0.180
−0.03	−0.111	0.389	0.011	32;65	−0.443	0.221
0	0.042	0.000	0.059	618;233	0.018	0.066
0.03	0.059	0.028	0.134	466;92	−0.010	0.129
0.06	−0.041	0.203	0.062	263;50	−0.125	0.042

Notes: Formal results for Figure B5. The table reports robust and bias-corrected ITT RD estimates and 95% confidence intervals for various artificial cutoffs using local linear regression with a triangular kernel and MSE-optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter being elected instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. The true cutoff (0) is included for comparison with artificial cutoffs.

Table A10: Effect of an issue supporter in parliament on referendum support—Alternative kernels

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Panel A: Epanechnikov Kernel				
Treatment ($supporter\ in\ parliament = 1$)	0.076***	0.074***	0.122***	0.087***
	(0.011)	(0.013)	(0.012)	(0.012)
First stage estimates	0.783***	0.781***	0.782***	0.780***
	(0.012)	(0.013)	(0.013)	(0.015)
N	1366	1366	1366	1366
$N_{effective}$	512;213	499;228	479;195	453;217
BW type	mserd	msetwo	cerrd	certwo
BW size	0.034	0.030;0.052	0.028	0.025;0.042
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.237	47.167	46.952	47.341
Panel B: Uniform Kernel				
Treatment ($supporter\ in\ parliament = 1$)	0.056***	0.066***	0.116***	0.073***
	(0.011)	(0.016)	(0.012)	(0.013)
First stage estimates	0.779***	0.790***	0.792***	0.788***
	(0.011)	(0.015)	(0.014)	(0.016)
N	1366	1366	1366	1366
$N_{effective}$	479;195	440;221	440;174	413;217
BW type	mserd	msetwo	cerrd	certwo
BW size	0.028	0.023;0.047	0.023	0.019;0.039
p value	0.000	0.000	0.000	0.000
Mean DV in %	46.952	47.103	46.845	47.273

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with an epanechnikov kernel in Panel A and a uniform kernel in Panel B, and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A11: Effect of an issue supporter in parliament on referendum support—Alternative local polynomial orders

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Panel A: Quadratic				
Treatment ($supporter\ in\ parliament = 1$)	0.131***	0.127***	0.197***	0.145***
	(0.013)	(0.014)	(0.016)	(0.015)
First stage estimates	0.768***	0.768***	0.763***	0.761***
	(0.015)	(0.015)	(0.017)	(0.019)
N	1366	1366	1366	1366
$N_{effective}$	650;237	585;252	565;221	556;250
BW type	mserd	msetwo	cerrd	certwo
BW size	0.059	0.055;0.084	0.047	0.044;0.067
p	2	2	2	2
q	3	3	3	3
p value	0.000	0.000	0.000	0.000
Mean DV in %	46.888	47.529	47.159	47.459
Panel B: Cubic				
Treatment ($supporter\ in\ parliament = 1$)	0.182***	0.145***	0.193***	0.187***
	(0.015)	(0.014)	(0.016)	(0.016)
First stage estimates	0.775***	0.776***	0.772***	0.770***
	(0.016)	(0.016)	(0.018)	(0.018)
N	1366	1366	1366	1366
$N_{effective}$	742;252	742;288	697;252	697;252
BW type	mserd	msetwo	cerrd	certwo
BW size	0.094	0.093;0.116	0.076	0.075;0.093
p	3	3	3	3
q	4	4	4	4
p value	0.000	0.000	0.000	0.000
Mean DV in %	46.745	46.922	46.798	46.798

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local quadratic (Panel A) and local cubic (Panel B) regression with a triangular kernel, and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The local-polynomial used to construct the bias-correction, q, is of one order higher than the one to construct the point estimator p. Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A12: RD validity—Placebo effect on national referendum support

Dependent variable	National yes share			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	-0.053^{***}	-0.025	-0.028^{*}	-0.001
	(0.012)	(0.014)	(0.012)	(0.014)
First stage estimates	0.781^{***}	0.773^{***}	0.778^{***}	0.765^{***}
	(0.012)	(0.016)	(0.013)	(0.017)
N	1366	1366	1366	1366
$N_{effective}$	531;217	453;220	499;207	420;217
BW type	mserd	msetwo	cerrd	certwo
BW size	0.037	0.025;0.046	0.030	0.021;0.038
p value	0.000	0.070	0.025	0.956
Mean DV in %	47.551	47.648	47.447	47.705

Notes: Placebo test. The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the national share of yes votes on referendum r, which a single canton's marginal seat should not affect. The treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A13: RD validity—Alternative bandwidths

MSE-optimal BW %	RD est.	Robust p-val	BW	$N_{effective}$	CI lower	CI upper
20	0.279	0.000	0.007	254;121	0.246	0.312
30	0.289	0.000	0.011	300;129	0.253	0.326
40	0.272	0.000	0.015	348;133	0.238	0.305
50	0.225	0.000	0.018	404;161	0.194	0.255
60	0.193	0.000	0.022	428;173	0.164	0.221
70	0.164	0.000	0.026	455;181	0.139	0.190
80	0.121	0.000	0.030	499;207	0.098	0.143
90	0.099	0.000	0.033	504;208	0.077	0.121
100	0.087	0.000	0.037	531;217	0.066	0.109
110	0.080	0.000	0.041	540;217	0.059	0.102
120	0.075	0.000	0.044	556;218	0.054	0.097
130	0.070	0.000	0.048	565;221	0.049	0.092
140	0.067	0.000	0.052	579;228	0.044	0.089
150	0.060	0.000	0.055	585;233	0.038	0.082
160	0.053	0.000	0.059	631;233	0.032	0.075
170	0.041	0.000	0.063	669;247	0.020	0.062
180	0.032	0.002	0.067	675;250	0.012	0.052
190	0.026	0.008	0.070	688;250	0.007	0.046
200	0.023	0.021	0.074	697;252	0.003	0.042

Notes: Formal results for Figure 6. The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel, varying bandwidths, and canton-level clustered standard errors, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out.

Table A14: Effect of an issue supporter in parliament on referendum support—District magnitude scaled vote margin

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.096***	0.036***	0.112***	0.058***
	(0.010)	(0.009)	(0.011)	(0.009)
First stage estimates	0.777***	0.788***	0.771***	0.784***
	(0.018)	(0.016)	(0.017)	(0.017)
N	1366	1366	1366	1366
$N_{effective}$	554;220	613;252	529;206	590;246
BW type	mserd	msetwo	cerrd	certwo
BW size	516.082	673.174;1011.178	423.975	553.030;830.709
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.362	47.211	47.166	47.055

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of the canton's representatives in the National Council. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A15: Effect of an issue supporter in parliament on referendum support—Eligible voters scaled vote margin

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.087***	0.072***	0.100***	0.089***
	(0.011)	(0.011)	(0.012)	(0.012)
First stage estimates	0.786***	0.781***	0.780***	0.775***
	(0.017)	(0.017)	(0.017)	(0.018)
N	1366	1366	1366	1366
$N_{effective}$	570;219	551;239	544;216	527;225
BW type	mserd	msetwo	cerrd	certwo
BW size	0.022	0.019;0.030	0.018	0.016;0.025
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.287	47.603	47.418	47.186

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of eligible voters. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A16: Effect of an issue supporter in parliament on referendum support—Unweighted

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.122***	0.119***	0.141***	0.137***
	(0.012)	(0.012)	(0.013)	(0.013)
First stage estimates	0.706***	0.702***	0.724***	0.723***
	(0.016)	(0.016)	(0.016)	(0.016)
N	1366	1366	1366	1366
$N_{effective}$	502;207	504;208	455;181	460;195
BW type	mserd	msetwo	cerrd	certwo
BW size	0.031	0.032;0.033	0.026	0.027;0.027
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.197	47.146	47.116	47.159

Notes: The table reports unweighted, robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A17: Effect of electing an issue supporter on referendum support—Intention-to-treat

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ vote\ margin > 0$)	0.042***	0.045***	0.056***	0.059***
	(0.009)	(0.009)	(0.009)	(0.009)
N	1366	1366	1366	1366
$N_{effective}$	618;233	585;240	565;221	556;221
BW type	mserd	msetwo	cerrd	certwo
BW size	0.059	0.055;0.061	0.048	0.045;0.050
p value	0.000	0.000	0.000	0.000
Mean DV in %	47.117	47.440	47.159	47.105

Notes: The table reports robust and bias-corrected ITT RD estimates using local linear regression with a triangular kernel, and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter being elected instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A18: Effect of an issue supporter in parliament on referendum support—Positions from national party lines

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.115***	0.107***	0.164***	0.127***
	(0.005)	(0.005)	(0.005)	(0.005)
First stage estimates	0.780***	0.748***	0.774***	0.750***
	(0.006)	(0.005)	(0.007)	(0.003)
N	1499	1499	1499	1499
$N_{effective}$	510;233	404;282	466;196	351;267
BW type	mserd	msetwo	cerrd	certwo
BW size	0.028	0.016;0.074	0.023	0.013;0.060
p value	0.000	0.000	0.000	0.000
Mean DV in %	45.684	46.411	45.638	46.038

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Positions for both elected and non-elected candidates are proxied by national party lines. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A19: Effect of an issue supporter in parliament on referendum support—Positions from cantonal party lines

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.051***	0.122***	0.065***	0.137***
	(0.004)	(0.005)	(0.005)	(0.005)
First stage estimates	0.801***	0.762***	0.797***	0.755***
	(0.006)	(0.007)	(0.006)	(0.004)
N	1475	1475	1475	1475
$N_{effective}$	604;233	420;264	576;229	361;246
BW type	mserd	msetwo	cerrd	certwo
BW size	0.048	0.016;0.068	0.040	0.014;0.056
p value	0.000	0.000	0.000	0.000
Mean DV in %	46.133	46.216	46.203	45.861

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Positions for both elected and non-elected candidates are proxied by cantonal party lines. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A20: Effect of an issue supporter in parliament on referendum support—High party-cohesion referenda

Dependent variable	Yes share voters			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.075***	0.111***	0.089***	0.128***
	(0.009)	(0.011)	(0.009)	(0.010)
First stage estimates	0.791***	0.769***	0.785***	0.760***
	(0.013)	(0.016)	(0.013)	(0.017)
N	1207	1207	1207	1207
$N_{effective}$	503;187	408;187	475;183	380;183
BW type	mserd	msetwo	cerrd	certwo
BW size	0.046	0.027;0.051	0.038	0.022;0.042
p value	0.000	0.000	0.000	0.000
Mean DV in %	46.731	46.776	46.943	46.809

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The sample further excludes referenda with low party cohesion, defined as votes in which deviations from the party line exceed the 90th percentile (more than 30 legislators). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A21: Effect of an issue supporter in parliament on individual vote choice (VOX)

Dependent variable	Yes vote VOX			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.101**	0.094**	0.106**	0.113**
	(0.038)	(0.036)	(0.039)	(0.037)
First stage estimates	0.677***	0.707***	0.696***	0.727***
	(0.007)	(0.007)	(0.007)	(0.007)
N	53834	53834	53834	53834
$N_{effective}$	31877;8850	28949;9060	30228;8584	26791;8924
BW type	mserd	msetwo	cerrd	certwo
BW size	0.037	0.024;0.062	0.030	0.019;0.051
p value	0.008	0.009	0.006	0.002
Mean DV in %	50.293	50.083	49.915	50.046

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and standard errors clustered at the canton×referendum level in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The sample is constructed by matching VOX respondents' votes to supporters in close opposing races. The outcome is an indicator equal to one if the respondent reports voting yes in the referendum. The running variable is the vote margin of the issue supporter, and treatment indicates whether the supporter won the election. The specifications use the bandwidths from the main RD. We cluster at the canton×referendum cell because the running variable and treatment are constant within a cell and the sample repeats each cell across its VOX respondents. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A22: Effect of an issue supporter in parliament on individual vote choice (VOX)—Alternative bandwidths

MSE-optimal BW %	RD est.	Robust p-val	BW	$N_{effective}$	CI
20	0.184	0.000	0.007	18,669;6,585	0.103;0.265
30	0.194	0.000	0.011	20,704;6,666	0.111;0.277
40	0.193	0.000	0.015	23,082;6,899	0.106;0.280
50	0.158	0.000	0.018	26,448;7,835	0.077;0.239
60	0.141	0.001	0.022	27,798;8,032	0.061;0.221
70	0.126	0.002	0.026	29,336;8,260	0.047;0.204
80	0.111	0.004	0.030	30,228;8,584	0.034;0.187
90	0.104	0.007	0.033	30,973;8,607	0.029;0.179
100	0.101	0.008	0.037	31,877;8,850	0.027;0.176
110	0.098	0.010	0.041	32,431;8,850	0.024;0.172
120	0.095	0.011	0.044	32,982;8,877	0.021;0.168
130	0.092	0.013	0.048	33,383;8,924	0.019;0.165
140	0.090	0.015	0.052	33,906;8,939	0.018;0.162
150	0.086	0.017	0.055	34,032;8,999	0.015;0.157
160	0.081	0.021	0.059	35,879;8,999	0.012;0.151
170	0.075	0.027	0.063	36,355;9,092	0.008;0.142
180	0.070	0.035	0.067	36,532;9,104	0.005;0.135
190	0.065	0.044	0.070	36,950;9,104	0.002;0.129
200	0.061	0.053	0.074	37,284;9,140	−0.001;0.124

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel, varying bandwidths, and standard errors clustered at the canton×referendum level, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The sample is constructed by matching VOX respondents' votes to supporters in close opposing races. The outcome is an indicator equal to one if the respondent reports voting yes in the referendum. The running variable is the vote margin of the issue supporter, and treatment indicates whether the supporter won the election. The specifications use the bandwidths from the main RD.

Table A23: Effect of an issue supporter in parliament on vote flows and turnout

	Yes votes per eligible	No votes per eligible	Turnout deviation
Treatment	0.042***	−0.020***	0.023***
	(0.008)	(0.005)	(0.003)
First stage estimates	0.794***	0.781***	0.790***
	(0.012)	(0.012)	(0.012)
N	1366	1366	1366
$N_{effective}$	581;228	528;216	585;233
BW size	0.053	0.035	0.056
p value	0.000	0.000	0.000
Mean DV in %	22.369	25.543	1.176

Notes: The table reports bias-corrected fuzzy RD estimates with robust standard errors, using local linear regression with a triangular kernel, MSE-optimal bandwidths, and canton-level clustering, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Yes (no) votes per eligible voter equal the cantonal yes (no) share multiplied by cantonal turnout. Turnout deviation equals cantonal turnout minus national turnout in the same referendum. National turnout is unaffected at the threshold (Table A3). Table A2 provides variable descriptions.

Table A24: Effect of an issue supporter in parliament on log yes votes per eligible voter

Dependent variable	$\log(yes\ votes \div eligible\ voters)$			
	(1)	(2)	(3)	(4)
Treatment ($supporter\ in\ parliament = 1$)	0.268***	0.303***	0.308***	0.363***
	(0.033)	(0.037)	(0.033)	(0.040)
First stage estimates	0.785***	0.774***	0.782***	0.768***
	(0.012)	(0.015)	(0.013)	(0.016)
N	1366	1366	1366	1366
$N_{effective}$	540;217	460;252	504;208	426;250
BW type	mserd	msetwo	cerrd	certwo
BW size	0.039	0.027;0.080	0.032	0.022;0.065
p value	0.000	0.000	0.000	0.000

Notes: The table reports robust and bias-corrected fuzzy RD estimates using local linear regression with a triangular kernel and canton-level clustered standard errors in parentheses, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the log of yes votes per eligible voter in canton c on referendum r, and the treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.

Table A25: Effect of an issue supporter in parliament on turnout and its deviation—ITT and fuzzy estimates

	Turnout ITT	Turnout fuzzy	Deviation ITT	Deviation fuzzy
Treatment	0.029***	0.034***	−0.006*	0.023***
	(0.006)	(0.007)	(0.002)	(0.003)
First stage estimates	−−	0.781***	−−	0.790***
		(0.012)		(0.012)
N	1366	1366	1366	1366
$N_{effective}$	618;233	531;217	455;181	585;233
BW size	0.059	0.037	0.026	0.056
p value	0.000	0.000	0.018	0.000
Mean DV in %	47.941	48.130	1.540	1.176

Notes: The table reports robust and bias-corrected RD estimates for cantonal turnout and for its deviation from national turnout, each as an intention-to-treat estimate (ITT, the effect of the supporter winning the election) and as a fuzzy estimate (the effect of a supporter sitting in parliament), using local linear regression with a triangular kernel, MSE-optimal bandwidths, and canton-level clustering, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. Turnout deviation equals cantonal turnout minus national turnout in the same referendum. The running variable is the issue supporter's vote margin divided by the number of voters who turned out.

Table A26: Effect of an issue supporter in parliament on referendum support—Moderator analyses

Variable	Median	Sample	RD est.	P-value	BW	N	CI	Diff.	Δp (BH)
Referendum complexity	1.602	Low	−0.018	0.440	0.124	652	−0.062;0.027	0.062*	0.051
		High	0.044	0.001	0.096	681	0.017;0.071		
Referendum importance	5.830	Low	−0.037	0.174	0.077	601	−0.09;0.016	0.089**	0.025
		High	0.053	0.002	0.105	732	0.02;0.086		
Media coverage total	245.500	Low	0.072	0.015	0.115	340	0.014;0.13	−0.049	0.350
		High	0.023	0.369	0.075	335	−0.027;0.074		
Campaign balance	0.469	Low	0.025	0.180	0.069	637	−0.011;0.061	0.01	0.678
		High	0.035	0.032	0.109	605	0.003;0.067		
Media tonality	−0.500	Low	0.088	0.067	0.097	345	−0.006;0.182	0.035	0.654
		High	0.123	0.000	0.049	330	0.069;0.178		

Notes: The table reports bias-corrected fuzzy RD estimates with robust standard errors for various subsamples, using local linear regression with a triangular kernel and canton-level clustering, following Calonico, Cattaneo, and Titiunik (2014, 2015b). Observations are weighted by the inverse number of representatives per canton. The dependent variable is the share of yes votes in canton c on referendum r. The treatment is an issue supporter entering parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. *Low* (*High*) denotes the subsample below (at or above) the median of the moderator, reported in the Median column. *Diff.* reports the difference in RD estimates between the two subgroups (High minus Low), with significance stars from a z-test using bias-corrected estimates and robust standard errors. Δp *(BH)* reports the Benjamini-Hochberg-adjusted p-value of that difference across the five moderators (Benjamini and Hochberg, 1995). Table A2 provides variable descriptions.

Table A27: Moderator analyses—rdhte intention-to-treat robustness

Variable	Sample	ITT est.	P-value	N	BW	Diff.	Δp (BH)
Referendum complexity	Low	0.067	0.118	501	0.088	0.051	0.915
	High	0.060	0.470	484	0.103		
Referendum importance	Low	−0.016	0.849	465	0.118	0.066	0.685
	High	0.074	0.094	574	0.120		
Media coverage total	Low	0.066	0.400	241	0.109	−0.053	0.915
	High	0.057	0.098	252	0.104		
Campaign balance	Low	0.021	0.724	492	0.128	0.022	0.485
	High	0.113	0.048	439	0.083		
Media tonality	Low	0.104	0.043	249	0.114	−0.055	0.915
	High	0.087	0.057	254	0.113		

Notes: The table reports the heterogeneity estimator of Calonico, Cattaneo, Farrell, et al. (2025) (`rdhte`), which fits the two subgroups jointly and forms the subgroup-difference test from robust bias-corrected estimates and their joint canton-clustered covariance. The design is sharp, so the estimand is the intention-to-treat effect of a supporter's electoral victory rather than the fuzzy effect of parliamentary entry used in the main analysis. The ITT is smaller by the first-stage compliance rate. Observations are weighted by the inverse number of representatives per canton, and each subgroup uses its own MSE-optimal bandwidth. *Diff.* is the High-minus-Low difference in ITT estimates, and Δp *(BH)* is its Benjamini-Hochberg-adjusted p-value across the five moderators. Table A2 provides variable descriptions.

Appendix B: Additional figures

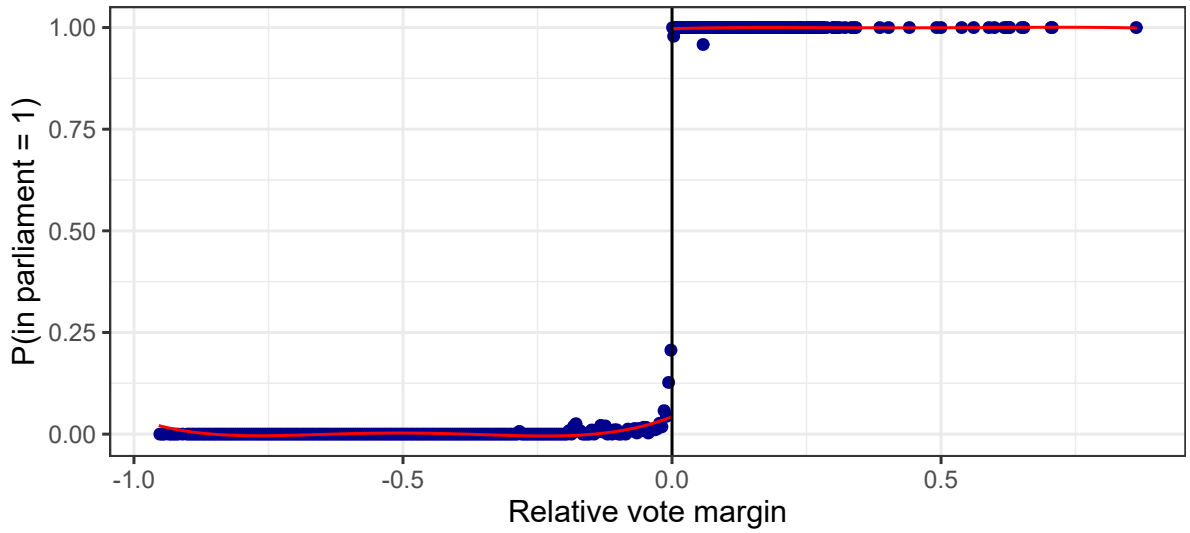

Figure B1: Probability of entering parliament conditional on the vote margin—Full sample

Notes: Binned probability of being in parliament against the running variable, the relative vote margin following Luechinger, Schelker, and Schmid (2024) (the vote margin divided by the number of voters who turned out), across all candidates and elections. Noncompliance occurs because of resignations.

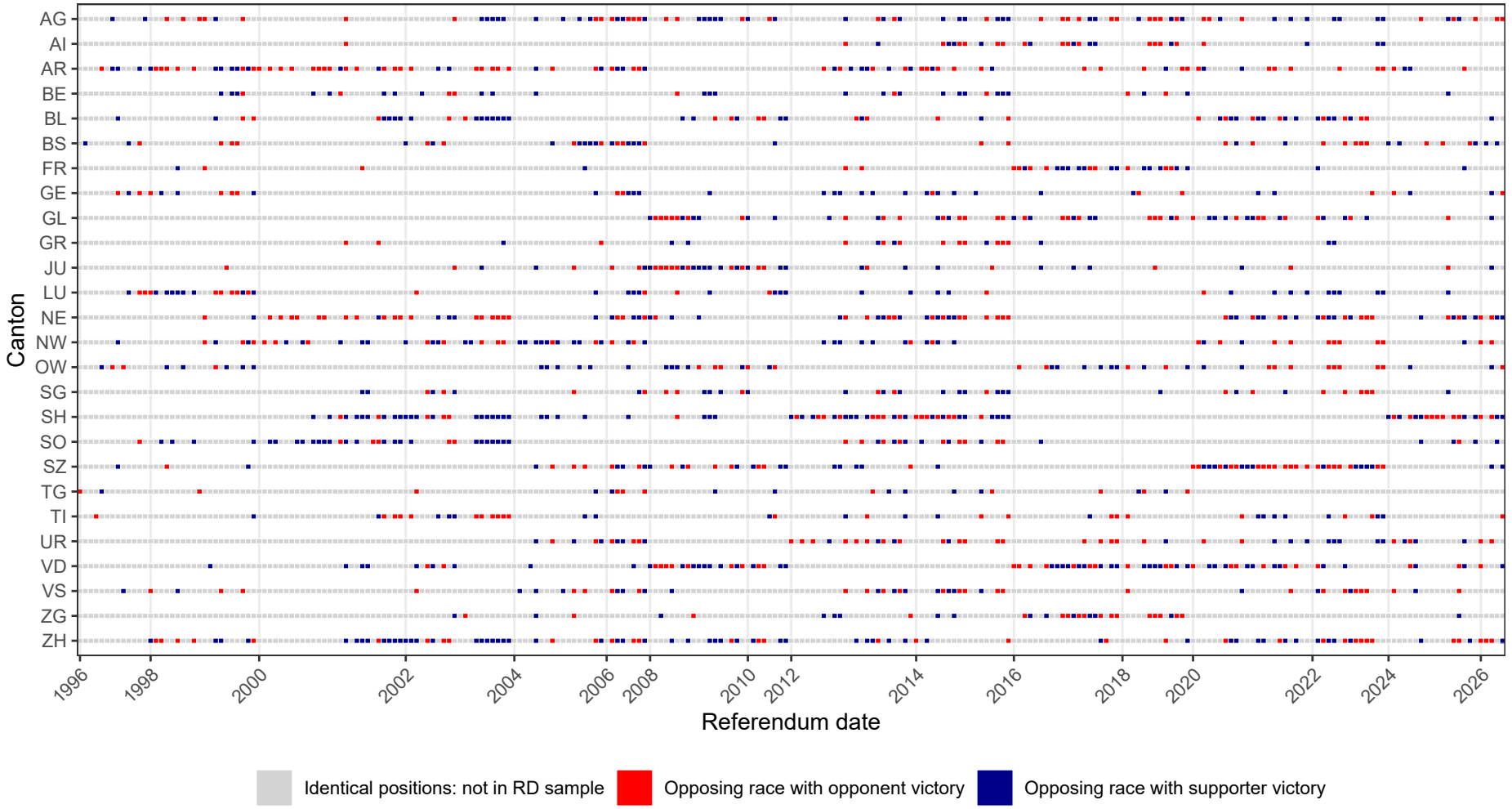

Figure B2: Occurrence and outcomes of opposing races by canton and referendum

Notes: Red (blue) tiles indicate canton×referendum pairs where the marginal candidates in the previous election held opposing positions and the opponent (supporter) won. For grey tiles, the marginal candidates held identical positions. Referenda on the x-axis are in chronological order.

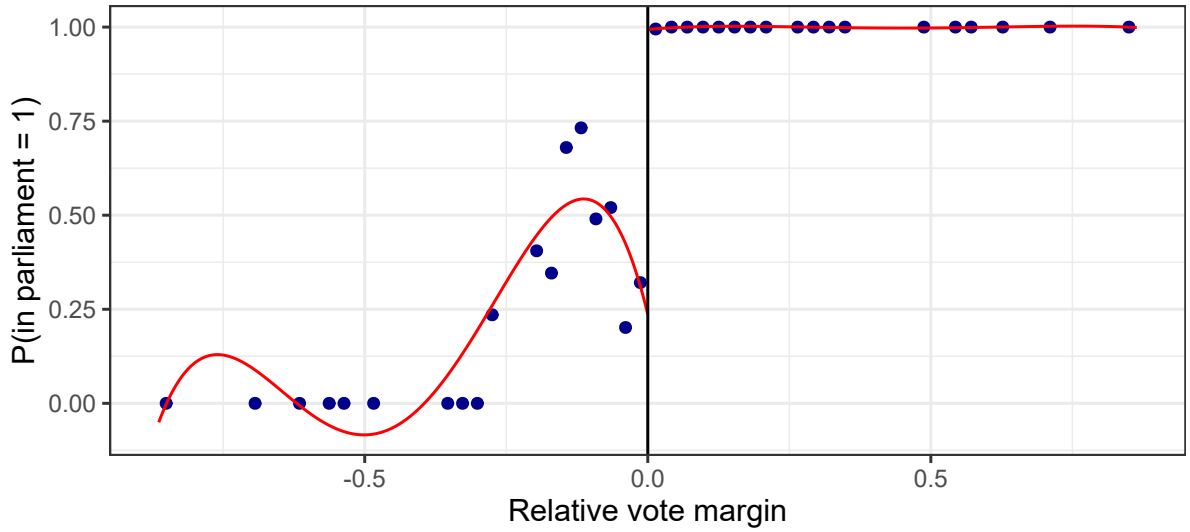

Figure B3: Probability of entering parliament conditional on the vote margin—RD sample

Notes: Binned probability of being in parliament against the running variable, the relative vote margin following Luechinger, Schelker, and Schmid (2024) (the vote margin divided by the number of voters who turned out), across candidates in opposing races (RD sample). Noncompliance occurs because of resignations.

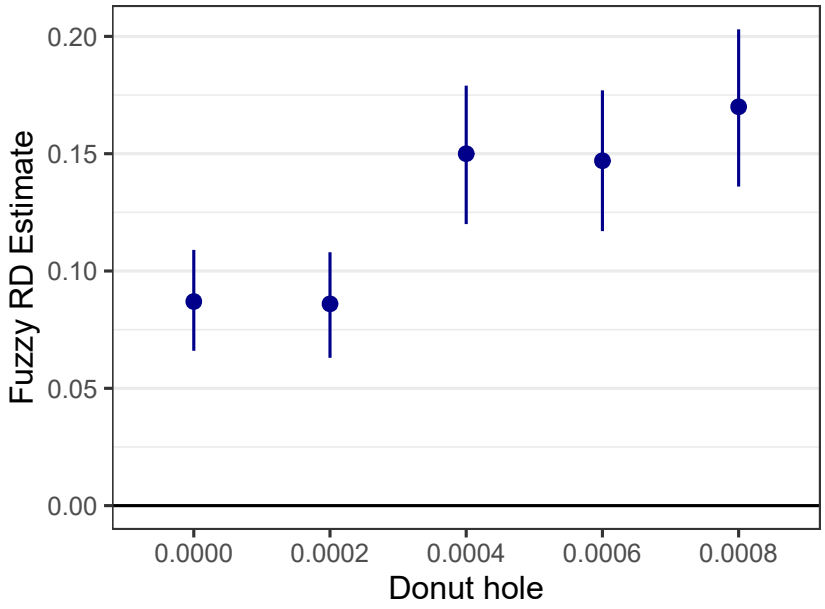

Figure B4: RD validity—Donut-hole exclusions

Notes: The figure shows robust and bias-corrected fuzzy RD estimates and 95% confidence intervals from donut-hole specifications that exclude observations near the cutoff, using local linear regression with a triangular kernel and MSE-optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter being in parliament instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Table A8 in the appendix provides formal results.

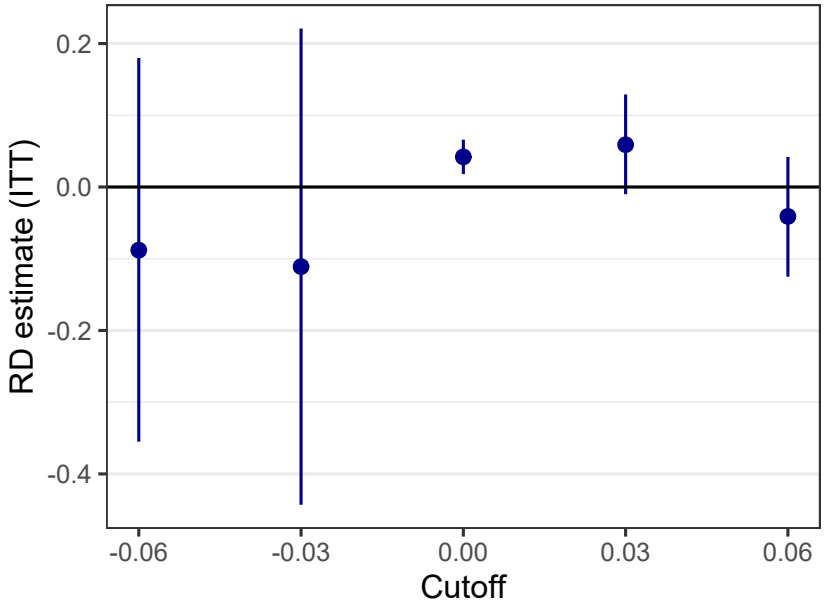

Figure B5: RD validity—True and artificial cutoffs

Notes: The figure shows robust and bias-corrected ITT RD estimates and 95% confidence intervals at the true cutoff (0) and at artificial cutoffs, using local linear regression with a triangular kernel and MSE-optimal bandwidth selection, following Calonico, Cattaneo, and Titiunik (2014, 2015b). The dependent variable is the share of yes votes in canton c on referendum r, and the treatment is an issue supporter being elected instead of an issue opponent. The running variable is the issue supporter's vote margin divided by the number of voters who turned out. Table A9 in the appendix provides formal results.

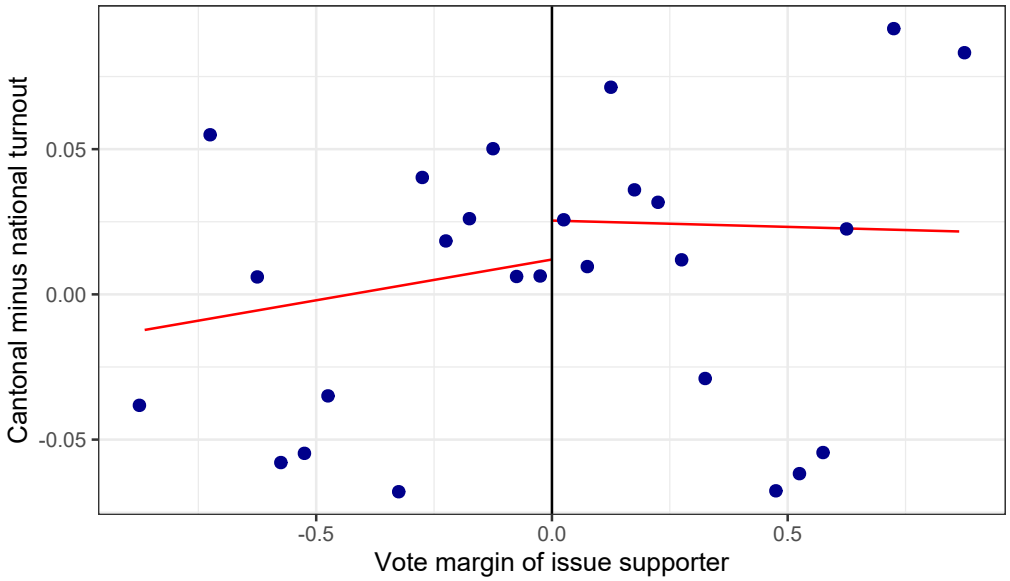

Figure B6: Turnout deviation and the vote margin of the issue supporter

Notes: The figure shows the deviation of cantonal turnout from national turnout against the issue supporter's vote margin across all opposing races. Dots are means within bins of width 0.05. Lines are linear fits estimated separately on each side of the threshold.

Appendix C: Instrumental variable approach

We address endogeneity between legislators' decisions and voters' preferences using an instrumental variables (IV) approach.[14] As in our RD design approach, our IV strategy uses the quasi-random variation in the issue positions of the legislators of a canton arising from close electoral races.

The outcome variable is again the share of yes votes of the voters from canton c on referendum r ($yes\ share\ voters_{cr}$). The endogenous explanatory variable of interest is the share of issue supporters among a canton's legislators ($yes\ share\ legislators_{cr}$). We calculate this as the number of yes-voting legislators of canton c divided by the canton's total number of legislators. We follow Clots-Figueras (2011) and Baskaran and Hessami (2023) and instrument the share of issue supporters among a canton's legislators by the share of *closely elected* issue supporters among a canton's legislators. This instrument captures the extent to which randomness in the outcome of close elections increases or decreases the share of a canton's issue supporters in parliament.

We calculate the share of closely elected legislators supporting a referendum as follows: we construct an indicator variable C_{icrt} for closely elected candidates. The indicator is equal to 1 if the candidate's relative vote margin is within a certain bandwidth. We vary the bandwidth to validate our results. We sum up the number of closely elected issue supporters for each referendum and canton. To account for the differential number of legislators per canton, we divide this number by district size (i.e., the number of legislators of the canton).

In formal terms:

$$Z_{cr} = \left(\sum_{i}^{N_{cr}} Close_{icr} Elected_{icr} Support_{icr} \right) / \sum_{i}^{N_{cr}} Elected_{icr} \tag{2}$$

where subscripts i denote candidates, c cantons and r referenda. $Close_{icr}$ is an indicator equal to one if a candidate was part of a close election. $Elected_{icr}$ is an indicator equal to one for elected candidates. $Support_{icr}$ is an indicator equal to one for candidates supporting referendum r. N_{cr} is the canton's number of legislators during the legislative period in which the parliamentary vote with subsequent referendum was held.

[14] For examples of IV strategies applied to similar research questions, see (Essig et al., 2021; Freier and Odendahl, 2015; Gabel and Scheve, 2007); for overviews, see (Mogstad and Torgovitsky, 2018; Sovey and Green, 2011).

We use a standard 2SLS approach where the first stage regression estimates the effect of the share of closely elected issue supporters on the share of a canton's issue supporters:

$$yes\ share\ legislators_{cr} = \pi_0 + \pi_1 close\ supporter\ share_{cr} + \nu_{cr} \tag{3}$$

The second-stage regression uses the predicted values from the first stage to estimate the effect of the share of a canton's issue supporters on voter preferences:

$$yes\ share\ voters_{cr} = \alpha + \beta_1 yes\ \widehat{share\ legislators}_{cr} + \epsilon_{cr} \tag{4}$$

Table C1: Instrumental variable estimates—Effect of legislators' support on voter support

Dependent variable	Yes share voters		
	(1)	(2)	(3)
Panel A: Second stage			
Yes share legislators	0.137***	0.155***	0.157***
	(0.029)	(0.027)	(0.037)
Panel B: First stage			
Instrument Z	0.404***	0.557***	0.392***
	(0.040)	(0.077)	(0.038)
Panel C: Reduced form			
Instrument Z	0.055***	0.086***	0.061***
	(0.015)	(0.022)	(0.016)
Close election definition	Margin $< h_{\text{MSE}}$	Margin $< 2 * h_{\text{MSE}}$	Margin $< \frac{1}{2} h_{\text{MSE}}$
Canton fixed effects	✓	✓	✓
Referendum fixed effects	✓	✓	✓
First-stage F-stat.	1,019.5	2,691.1	596.4
Observations	5,434	5,424	5,471
R^2	0.8802	0.8786	0.8771
Within R^2	0.1324	0.1215	0.1202

Notes: The table reports two-stage least squares (2SLS) estimates of the effect of the share of legislators supporting a referendum on voter preferences. The endogenous explanatory variable is the share of legislators in canton c who voted yes on the policy proposal. The instrument is the share of closely elected legislators from canton c who supported the proposal. Specifications (1) to (3) define candidates within one, two, and half the MSE-optimal vote margin bandwidth (h_{MSE}) as closely elected. The first-stage F-statistics are large because the instrument is a component of the endogenous variable. The share of closely elected supporters is part of the overall share of supporting legislators, so a strong first stage is mechanical. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$.