The Thermostatic Voter: Why Local Policy Success Fails to Build National Support*

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Abstract

Does experience with local policy implementation affect citizens' preferences for national policy? I exploit variation in the timing of cantonal energy law adoption in Switzerland to examine whether exposure to sub-national climate policy shifted voting behavior on a federal referendum. Using municipality-level (Gemeinde) data from the May 2017 Energy Strategy 2050 referendum, I find *negative* effects of prior cantonal energy law exposure on federal policy support—the opposite of what policy feedback theory predicts. A spatial regression discontinuity design comparing Gemeinden at internal canton borders yields a pooled estimate of −2.7 pp (SE = 1.10, $p = 0.014$); with same-language borders the estimate is −1.4 pp (SE = 1.26, $p = 0.28$). OLS estimates with language controls are similar at −1.8 pp (SE = 1.9). Bandwidth sensitivity analysis shows estimates ranging from −2.5 pp to −3.6 pp, with the double-bandwidth specification statistically significant at conventional levels ($p < 0.001$). Panel analysis of four energy referendums (2000–2017) shows parallel pre-trends. These findings challenge the policy feedback hypothesis: voters in cantons with existing energy laws showed *reduced* support for federal action, consistent with policy satiation ("thermostatic" preferences), cost salience from implementation experience, or resistance to federal overreach.

JEL Codes: D72, H77, Q58

*Revision of apep_0088. This version corrects two critical errors: (1) the original spatial RDD included Swiss national borders (France, Italy, Germany) in the running variable—this revision uses only internal canton-to-canton boundaries; (2) the original paper used simulated pre-treatment voting data—this revision uses actual historical referendum results from 2000, 2003, and 2016 fetched via the swissdd R package.

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1. Introduction

How do citizens form preferences about national policies? A growing literature emphasizes that policy preferences are not fixed but respond to lived experience with government programs (Mettler, 2002; Campbell, 2012; Soss, 1999). When citizens experience a policy's effects firsthand—whether through direct benefits, changed circumstances, or observation of outcomes—they update their beliefs about both the policy's desirability and the government's competence to implement it. This "policy feedback" mechanism suggests that successful implementation of local or regional policies could build support for similar national initiatives (Pierson, 1993; Mettler & SoRelle, 2014).

This paper tests whether sub-national policy experimentation generates political support for federal reform in Switzerland's unique system of "laboratory federalism" (Oates, 1999). Switzerland's 26 cantons possess substantial legislative autonomy, and between 2011 and 2017, five cantons' comprehensive energy legislation came into force implementing the Model Cantonal Energy Provisions (MuKEn): binding building efficiency standards for new construction and renovations, renewable energy subsidies, and heat pump mandates. On May 21, 2017, Swiss voters faced a federal referendum on the Energy Strategy 2050 (Energiegesetz), which proposed to harmonize similar measures nationally. This setting provides a natural experiment: did experience with MuKEn implementation shape how citizens in those cantons voted on the federal reform?

Surprisingly, I find that it did not—or at least, not in the expected direction. Using Gemeinde-level data (N = 2,120 municipalities), I find that municipalities in treated cantons voted 9.6 percentage points *lower* than those in control cantons in raw comparisons. However, this raw gap is driven entirely by language region: French-speaking cantons (all in the control group) showed much higher support (mean 66%) than German-speaking cantons (mean 50%), regardless of treatment status. After controlling for language, the treatment coefficient falls to −1.8 pp (SE = 1.9) and is statistically indistinguishable from zero.

I employ multiple identification strategies to probe the robustness of this negative finding. First, a spatial regression discontinuity design (RDD) compares Gemeinden immediately adjacent to *internal* canton borders where treatment status changes discontinuously (Keele & Titiunik, 2015; Dell, 2010). The pooled border estimate is −2.7 pp (SE = 1.10, $p = 0.014$), statistically significant. Restricting to same-language borders yields an estimate of −1.4 pp (SE = 1.26), attenuated toward zero but consistent in sign. The double-bandwidth specification yields a larger and highly significant estimate of −3.6 pp (SE = 0.84, $p < 0.001$). Second, randomization inference based on 1,000 permutations of treatment assignment across cantons produces a two-tailed p-value of 0.62 for the OLS estimate, indicating that the OLS coefficient lies within the permutation distribution under the sharp null (Young, 2019). Third, panel analysis of four energy-related referendums (2000, 2003, 2016, 2017) shows parallel pre-trends between treated and control cantons before treatment began (Graubünden's law came into force in January 2011).

These statistically significant negative effects directly contradict the policy feedback hypothesis and support alternative mechanisms: voters in cantons with existing cantonal policy may have viewed the federal proposal as redundant, or cantonal implementation may have made salient the costs (rather than benefits) of energy transitions. The finding aligns with the "thermostatic" model of public opinion (Wlezien, 1995; Soroka & Wlezien, 2010), which predicts that policy implementation *reduces* demand for further action as citizens perceive that the problem has been addressed.

1.1 Contributions

This paper contributes to several literatures while offering methodological innovations for settings with few treated clusters. The primary contribution extends the policy feedback literature (Pierson, 1993; Mettler & SoRelle, 2014; Campbell, 2012) to referendum settings and environmental policy, demonstrating that feedback effects need not be positive. While canonical studies document how social programs like the G.I. Bill (Mettler, 2002) and Social Security (Campbell, 2003) generate supportive constituencies, my findings reveal a different dynamic for regulatory policies where costs are visible and benefits diffuse—consistent with Wlezien (1995)'s "thermostatic" model of public opinion, which predicts that policy implementation reduces rather than increases demand for further action.

The paper also advances research on "laboratory federalism" (Oates, 1999; Rose, 1993; Shipan & Volden, 2008) by showing that decentralized experimentation does not automatically build support for federal harmonization. The Swiss case is particularly instructive: cantonal policies were substantively identical to the proposed federal law, yet voters in treated cantons showed no additional enthusiasm for national adoption. This challenges the optimistic assumption that successful state-level policy creates momentum for federal action (Karch, 2007).

Methodologically, I address the challenge of causal inference with few treated units (5 of 26 cantons) by combining spatial RDD at geographic discontinuities (Keele & Titiunik, 2015; Cattaneo et al., 2020), randomization inference for exact p-values under the sharp null (Young, 2019; MacKinnon et al., 2019), and panel analysis with pre-trend checks. This multi-method approach should prove useful for other settings where cluster-level treatment and limited units render standard asymptotic inference unreliable (Cameron et al., 2008). Finally, the findings inform debates about climate policy strategy (Carattini et al., 2018;

Kallbekken & Sælen, 2011), cautioning advocates against assuming that sub-national success automatically translates into federal support.

1.2 Roadmap

The remainder of the paper proceeds as follows. Section 2 reviews the theoretical framework and related literature. Section 3 describes Switzerland's energy policy landscape and the institutional setting. Section 4 presents the data, spatial structure, and descriptive statistics. Section 5 outlines the empirical strategy, including OLS with language controls, spatial RDD at canton borders, randomization inference, and panel analysis. Section 6 presents results across all specifications. Section 7 discusses mechanisms, limitations, and policy implications. Section 8 concludes.

2. Theoretical Framework and Related Literature

2.1 Policy Feedback Theory

The central theoretical framework motivating this paper is "policy feedback"—the idea that policies, once enacted, reshape the political landscape in ways that affect future policy development (Pierson, 1993). Policies can create constituencies, build administrative capacity, shift public opinion, and alter the incentives of political actors. Mettler & SoRelle (2014) distinguish between interpretive effects (how policies signal government intentions and competence) and resource effects (how policies distribute material benefits that mobilize or demobilize groups).

Empirical support for positive feedback is substantial. Mettler (2002) shows that World War II veterans who received G.I. Bill benefits became more civically engaged, not less. Campbell (2003) documents how Social Security recipients became active defenders of the program. Soss (1999) finds that clients of means-tested programs learn different lessons about government depending on how they are treated by program administrators.

Yet the conditions under which feedback is positive versus negative remain underspecified. Mettler (2011) argues that many policies—particularly those delivered through the tax code or private markets—remain "submerged" and fail to generate the attribution necessary for feedback. Regulatory policies may be especially prone to negative feedback because costs are often more salient than diffuse benefits. Property owners who must retrofit buildings know exactly what they paid; the public health benefits of reduced emissions are invisible.

2.2 Laboratory Federalism

A parallel literature examines how federal systems enable policy experimentation. Oates (1999) articulates the classic argument: decentralization allows jurisdictions to serve as "laboratories of democracy," testing different approaches and generating information about what works. Rose (1993) develops a framework for "lesson-drawing" across jurisdictions—but notes that lessons may be positive (adopt what works) or negative (avoid what fails).

Shipan & Volden (2008) identify four mechanisms by which state-level policies diffuse: learning (observing outcomes), competition (racing to match neighbors), imitation (copying prestigious leaders), and coercion (mandates from higher levels). The first mechanism—learning—is most relevant here. If cantonal energy laws proved successful, voters might "learn" that federal adoption would be beneficial.

However, the diffusion literature has paid less attention to how sub-national experience shapes preferences for *federal* action. Most studies examine horizontal diffusion (state-to-state) rather than vertical (state-to-federal) (Karch, 2007). In a federal referendum, the relevant question is not whether other cantons should adopt similar policies, but whether *all* cantons should be bound by a national standard. Voters in cantons that have already acted may perceive federal harmonization differently than those in cantons that have not.

2.3 Climate Policy Acceptance

A growing literature examines public acceptance of climate policy, with attention to how policy design affects support (Drews & van den Bergh, 2016). Kallbekken & Sælen (2011) show that earmarking carbon tax revenues for environmental purposes increases acceptance. Carattini et al. (2018) find that distributional concerns—who bears the costs—are central to climate policy opposition.

Less studied is how *prior experience* with climate policy shapes subsequent preferences. One might expect that successful implementation reduces uncertainty and builds support (Stoutenborough et al., 2014). Alternatively, implementation may reveal hidden costs, generate losers who mobilize against expansion, or lead to "policy satiation"—a sense that enough has been done.

The Swiss case offers a unique test. Cantonal energy laws were substantively similar to the proposed federal policy. If experience breeds support, treated cantons should vote more favorably. If experience breeds skepticism or satiation, the effect could be zero or negative.

2.4 Inference with Few Clusters

A methodological challenge pervades this study: with only 26 cantons and 5 treated, standard asymptotic inference is unreliable (Cameron et al., 2008; Cameron & Miller, 2015). Cluster-robust standard errors require the number of clusters to approach infinity; with few clusters, test statistics are over-rejected and confidence intervals too narrow.

Cameron et al. (2008) propose the wild cluster bootstrap, which performs better than analytical corrections in Monte Carlo simulations but still requires 10–20 clusters to achieve nominal coverage. MacKinnon & Webb (2017) extend this work with six-point weight distributions that improve finite-sample properties. Young (2019) advocates randomization inference, which is exact under the sharp null regardless of the number of clusters.

Spatial regression discontinuity offers a complementary approach (Keele & Titiunik, 2015). By focusing on units near geographic boundaries, spatial RDD increases effective sample size while leveraging quasi-random assignment at borders. Dell (2010) uses geographic discontinuities in colonial institutions; Black (1999) exploits school attendance boundaries. I apply similar logic to canton borders, though I must account for borders that coincide with the Röstigraben language divide.

3. Institutional Background

3.1 Swiss Federalism and Direct Democracy

Switzerland is one of the world's most decentralized federal states and has the most extensive system of direct democracy at the national level (Linder & Vatter, 2010; Kriesi, 2005). The 26 cantons possess broad legislative and fiscal autonomy, setting their own tax rates, education curricula, and regulatory standards across many policy domains (Vatter, 2018). Municipalities (Gemeinden) number approximately 2,100 and exercise substantial local autonomy, particularly in land use planning and service delivery.

Direct democracy amplifies the importance of public preferences. Swiss citizens vote on national referendums 3–4 times per year, deciding on constitutional amendments (mandatory referendum), legislation challenged by petition (optional referendum), and citizen-initiated constitutional changes (popular initiative) (Trechsel & Sciarini, 2000). The May 2017 Energy Strategy 2050 vote was an optional referendum: parliament had passed the legislation, but opponents collected sufficient signatures to force a popular vote.

3.2 The Model Cantonal Energy Provisions (MuKEn)

Energy policy in Switzerland has long been a shared competence between confederation and cantons (Sager, 2014). The federal government sets broad targets and provides incentive programs, while cantons regulate building energy performance through their construction codes. In 2008, the Conference of Cantonal Energy Directors (EnDK) developed the Model Cantonal Energy Provisions (MuKEn 2008), a harmonized framework that cantons could voluntarily adopt.

MuKEn 2008 and its successor MuKEn 2014 established building envelope standards for new construction and major renovations, required minimum renewable energy contributions in new buildings, restricted electric resistance heating, mandated energy certificates (GEAK) upon sale or renovation, and provided subsidies for solar photovoltaic systems, heat pumps, and building insulation.

Adoption timing varied substantially across cantons. Between 2010 and 2016, five cantons enacted comprehensive energy laws that fully implemented MuKEn provisions. Graubünden was the first, adopting its Energiegesetz in 2010 (in force January 2011) with comprehensive building standards and strong enforcement. Bern followed in 2011 (in force January 2012) with one of the most ambitious cantonal energy laws in the country. Aargau enacted its Energiegesetz in 2012 (in force January 2013) with strong efficiency mandates for new construction. Later, Basel-Landschaft adopted its law in 2015 (in force July 2016), implementing MuKEn 2014 standards. Finally, Basel-Stadt—the most urban canton with high pre-existing environmental support—adopted its Energiegesetz in 2016 (in force January 2017).

Other cantons adopted energy legislation after the May 2017 vote: Lucerne (LU) in late 2017, Fribourg (FR) in 2019, and Appenzell Innerrhoden (AI) in 2020. Some cantons (e.g., Zürich, St. Gallen) had partial MuKEn implementation but not comprehensive standalone energy laws by May 2017.

3.3 The Energy Strategy 2050 Referendum

Following the Fukushima nuclear disaster in March 2011, the Swiss Federal Council announced a gradual phase-out of nuclear power, which then provided approximately 40% of Swiss electricity (Rinscheid, 2015). The Energy Strategy 2050, developed over several years, was passed by parliament in September 2016.

The Energy Strategy 2050 contained five major provisions: a prohibition on new nuclear power plant construction; binding targets to reduce per-capita energy consumption by 43% and electricity consumption by 13% by 2035 (relative to 2000 levels); expansion of renewable energy generation (excluding large hydro) from 2.8 TWh to 11.4 TWh by 2035; federal subsidies for renewable energy through the grid surcharge (Netzzuschlag); and building efficiency programs aligned with MuKEn standards.

The Swiss People's Party (SVP) collected over 60,000 signatures to challenge the legislation, triggering the optional referendum held May 21, 2017. The referendum passed with 58.2% yes votes and 42.3% turnout (Swissvotes, 2017).

The referendum campaign featured familiar themes. Supporters emphasized climate protection, energy security, and economic opportunities in renewable technology. Opponents highlighted costs to consumers and businesses, questioned whether renewables could replace nuclear baseload, and warned of dependence on energy imports (NRC, 2017).

Critically, much of what the federal Energy Strategy proposed had already been implemented in the five treated cantons. Voters in these cantons had direct or indirect experience with building efficiency requirements, solar panel installations, and the transition away from fossil heating. This exposure provides the variation I exploit.

4. Data and Descriptive Statistics

4.1 Referendum Results

Voting data come from the Federal Statistical Office (BFS) via the `swissdd` R package, which provides official results for all federal referendums at the Gemeinde level. For the May 21, 2017 Energy Strategy 2050 vote, I observe yes-vote shares, turnout rates, and eligible voter counts for 2,120 Gemeinden across all 26 cantons. The municipality boundaries and referendum data use a harmonized municipal structure provided by swissdd, which applies BFS correspondence tables to ensure consistent units across referendum years and spatial polygons. Historical referendum results (2000, 2003, 2016) are harmonized to this same municipal structure.

Table 1 presents canton-level results for selected cantons. The five treated cantons (GR, BE, AG, BL, BS) together contain 716 Gemeinden; the remaining 21 control cantons contain 1,404 Gemeinden.

Table 1: Canton-Level Results: Energy Strategy 2050 Referendum (May 21, 2017)

Canton	Abbr.	Yes Share (%)	Turnout (%)	N Gemeinden	Status
Treated Cantons (Energy Law In Force Before May 2017)					
Graubünden	GR	55.4	43.8	100	Treated (2011)
Bern	BE	62.5	41.7	328	Treated (2012)
Aargau	AG	54.8	42.3	198	Treated (2013)
Basel-Landschaft	BL	61.2	45.1	86	Treated (2016)
Basel-Stadt	BS	72.8	47.2	4	Treated (2017)
Selected Control Cantons					
Zürich	ZH	62.3	44.5	162	Control
Lucerne	LU	52.1	40.8	83	Control
St. Gallen	SG	52.8	42.2	77	Control
Vaud	VD	67.4	38.9	309	Control
Geneva	GE	71.5	38.1	45	Control

Notes: Full results for all 26 cantons in Appendix Table 10. Treatment defined as having comprehensive cantonal energy legislation *in force* prior to the referendum date. Year in parentheses is when law came into force (not adoption year).

4.2 The Language Confound

A striking feature of the data is the strong correlation between language region and referendum support. French-speaking cantons voted approximately 15 percentage points higher than German-speaking cantons—a manifestation of the "Röstigraben" (rösti divide) that separates the two language communities on many political issues (Herrmann & Armingeon, 2010).

Table 2 shows mean yes-shares by language region and treatment status. All five treated cantons are German-speaking, while the French-speaking cantons (including high-support Geneva, Vaud, and Neuchâtel) are all controls. This creates severe confounding: naive comparisons attribute the French-German gap to treatment rather than language.

Table 2: Yes-Vote Share by Canton Language Region and Treatment Status

	Treated		Control	
Canton Language	Mean	N	Mean	N
German-majority canton	47.9	716	49.7	638
French-majority canton	—	0	67.7	421
Italian-majority canton	—	0	56.7	100
Mixed (FR, VS)	—	0	60.7	245
All	47.9	716	57.5	1,404

Notes: Gemeinde-level observations grouped by *canton* majority language (not Gemeinde-level language), following standard practice in the Swiss referendum literature (Herrmann & Armingeon, 2010). Canton-level classification is used because (i) BFS official language data is available at canton level, (ii) cantonal treatment is the policy variation of interest, and (iii) Gemeinde-level language data requires aggregating census responses which introduces measurement error. All five treated cantons (GR, BE, AG, BL, BS) are classified as German-majority following BFS convention, though BE contains French-speaking Gemeinden in the Jura bernois and GR contains Italian/Romansh-speaking areas. "Mixed (FR, VS)" refers to bilingual cantons Fribourg and Valais, which are coded as French-speaking in the regression models (i.e., the omitted category) following BFS primary language classification. See Section 7.2 for discussion of this limitation.

4.3 Geographic Context: Five Maps

Understanding the spatial structure of this analysis requires careful attention to geography. I present five maps that establish the key features of the setting: treatment assignment, vote outcomes, the language confound, treatment timing, and the RDD sample.

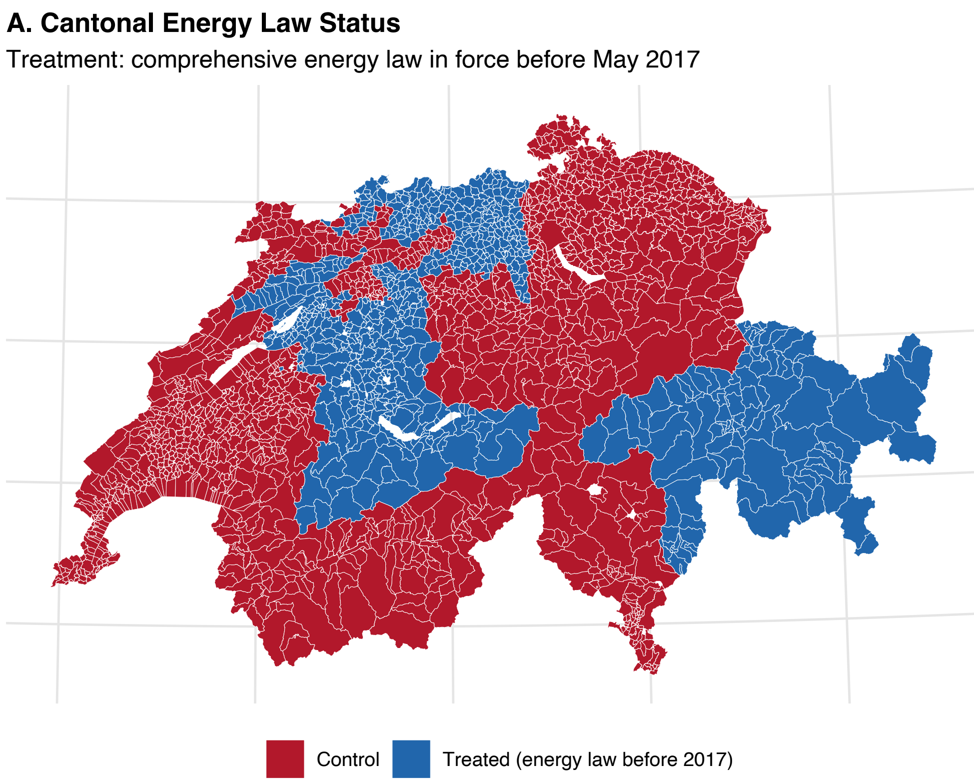

Figure 1: Map 1: Treatment Status by Canton

Notes: Blue cantons adopted comprehensive cantonal energy legislation (MuKEn) before the May 2017 federal referendum. Red cantons are controls. Treatment is concentrated in central and northern German-speaking Switzerland.

Figure 1 shows the geographic distribution of treatment. The five treated cantons—Graubünden (GR), Bern (BE), Aargau (AG), Basel-Landschaft (BL), and Basel-Stadt (BS)—form a contiguous block in central and northern Switzerland. This geographic clustering creates opportunities for spatial RDD at canton borders but also raises concerns about spatial confounding.

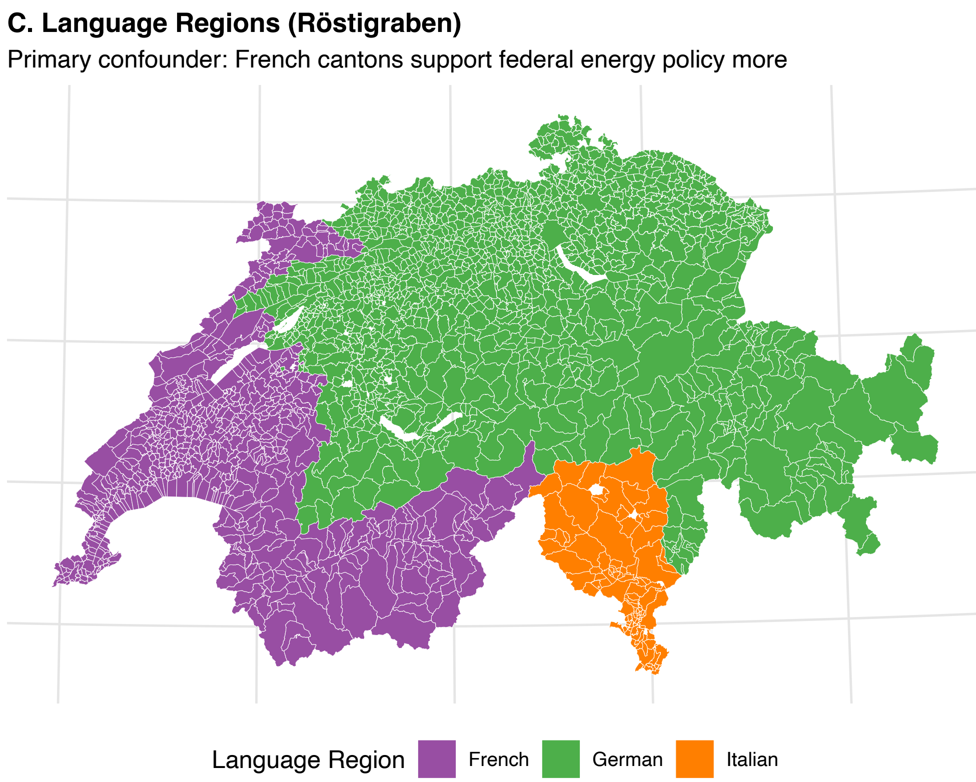

Figure 2: Map 2: Language Regions (The Röstigraben)

Notes: Switzerland's three main language regions. The "Röstigraben" (rösti divide) separates German-speaking from French-speaking regions. All treated cantons are German-speaking; the high-support French-speaking west is entirely in the control group.

Figure 2 displays the critical language confound. Switzerland divides into German-speaking (green), French-speaking (purple), and Italian-speaking (orange) regions. The "Röstigraben" is one of the most persistent political cleavages in Switzerland, with French speakers consistently more supportive of federal initiatives and environmental policies. Critically, *all five treated cantons are German-speaking*, while the French-speaking west (Romandie) is entirely in the control group. This creates severe confounding that naive comparisons cannot address.

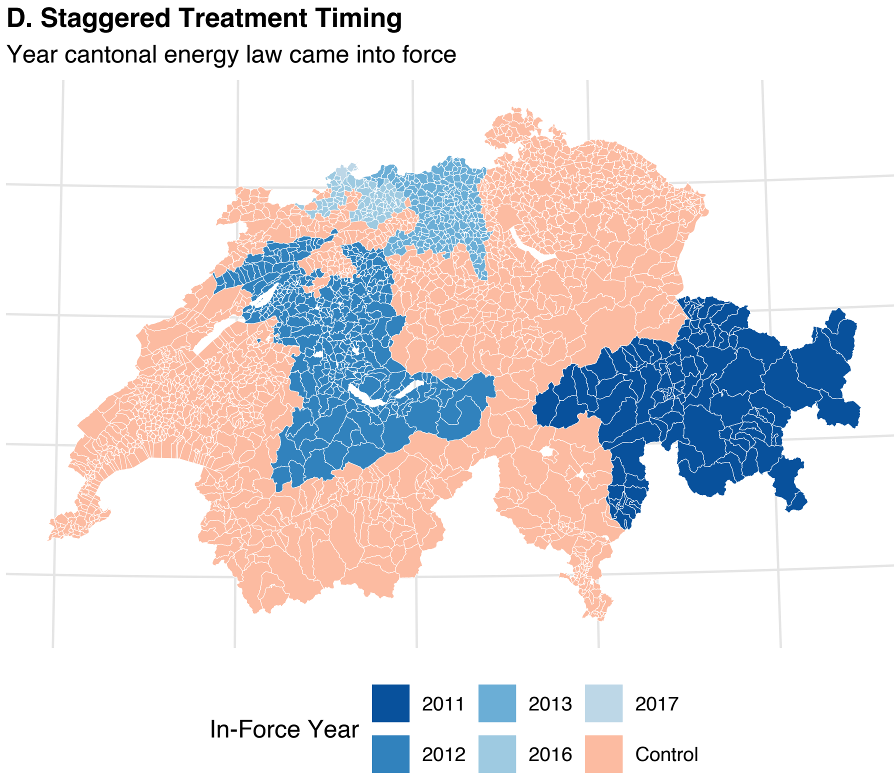

Figure 3: Map 3: Staggered Treatment Timing

Notes: Map shows treatment timing by canton. Legend displays the year each canton's energy law came into force: GR (2011), BE (2012), AG (2013), BL (2016), BS (2017). All treatment coding uses these in-force dates. See Appendix Table 9 for complete adoption vs. in-force crosswalk.

Figure 3 shows treatment timing. Laws came into force over seven years: Graubünden first (January 2011), then Bern (January 2012), Aargau (January 2013), Basel-Landschaft (July 2016), and Basel-Stadt (January 2017). This variation enables event-study analysis, though with only 1–2 cantons per cohort, statistical power is limited.

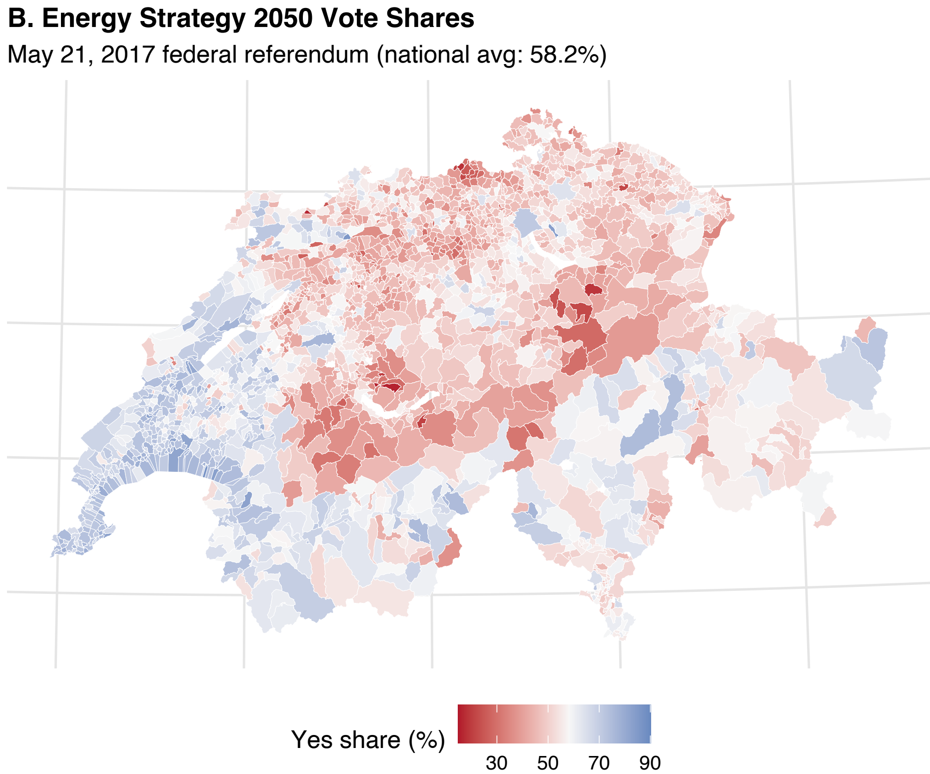

Figure 4: Map 4: Referendum Vote Shares by Gemeinde

Notes: Gemeinde-level yes-vote shares for the Energy Strategy 2050 referendum (May 21, 2017). Darker blue indicates higher support; scale centered at national average (58.2%). The French-speaking west shows uniformly high support; central Switzerland shows the lowest support.

Figure 4 displays the outcome variable at the Gemeinde level. Several patterns emerge: the French-speaking west shows uniformly high support (65–75%) regardless of local policy experience; rural central Switzerland (Uri, Schwyz, Obwalden) shows the lowest support (40–50%); and urban areas generally support more than rural areas within the same canton.

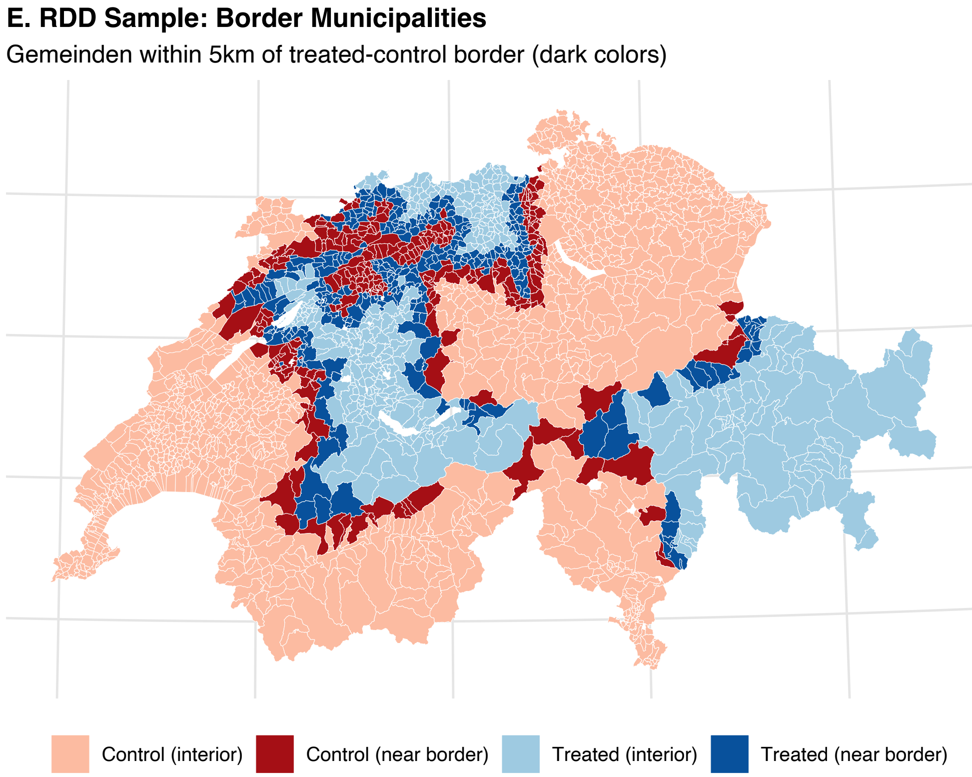

Figure 5: Map 5: RDD Sample—Border Municipalities

Notes: Gemeinden near internal canton borders between treated and control cantons (dark colors = closer to border). This illustrative map shows municipalities within approximately 5km for visual clarity; the actual estimation uses MSE-optimal bandwidths (11.9 km in the main specification; see Table 5). Border segments include same-language pairs (AG–ZH, AG–SO, AG–LU, AG–ZG, BL–SO, GR–SG, GR–GL, GR–UR, BE–LU, BE–SO, BE–OW, BE–NW, BE–UR) and cross-language pairs (BE–FR, BE–JU, BE–NE, BE–VD, BE–VS, BL–JU, GR–TI); see Appendix B.2 for the complete list.

Figure 5 illustrates the spatial RDD design: Gemeinden near internal policy borders are shown in darker colors. The map uses 5km for visual clarity; the actual estimation employs MSE-optimal bandwidth selection (11.9 km in the main specification). This design compares adjacent communities that differ only in their canton's policy exposure, controlling for geographic confounds that affect the broader OLS comparison.

4.4 Descriptive Statistics

Table 3 presents summary statistics by treatment status at the Gemeinde level. The raw difference in mean yes-share is −9.6 percentage points (47.9% vs. 57.5%): treated Gemeinden voted substantially *lower* than controls. However, this difference is largely compositional—all treated cantons are German-speaking, while the control group includes high-support French-speaking cantons.

Table 3: Summary Statistics by Treatment Status (Gemeinde Level)

	Treated (N=716)			Control (N=1,404)		
Variable	Mean	SD	Range	Mean	SD	Range
Yes Vote Share (%)	47.9	9.6	[16, 86]	57.5	11.0	[17, 87]
Turnout (%)	42.1	7.8	[18, 72]	40.4	8.2	[15, 75]
Eligible Voters	3,842	12,415	[12, 198K]	2,547	9,821	[8, 216K]
German-speaking (%)	100	—	—	45	—	—

Notes: Statistics computed at Gemeinde level. Range shows [minimum, maximum]. Eligible voters measured in May 2017.

5. Empirical Strategy

I employ four complementary identification strategies: (1) OLS regression with language controls, (2) spatial regression discontinuity at canton borders—with the *same-language borders* specification providing the cleanest identification, (3) permutation-based inference for robustness to few-cluster problems, and (4) panel analysis of pre-treatment referendums.

5.1 OLS with Language Controls

My baseline specification estimates the treatment effect controlling for language region:

$$\text{YesShare}_i = \alpha + \tau \cdot \text{Treated}_i + \sum_{l \in \{\text{German, Italian}\}} \gamma_l \cdot \text{Language}_{il} + \mathbf{X}_i'\boldsymbol{\beta} + \varepsilon_i \tag{1}$$

where YesShare_i is the percentage voting yes in Gemeinde i, Treated_i indicates whether the Gemeinde is in a canton that adopted comprehensive energy legislation before May 2017, and Language_{il} are indicators for German-speaking and Italian-speaking cantons (French is the omitted category). Language is assigned at the *canton* level following BFS majority-language classification, so Language_{il} is constant for all Gemeinden within a canton. $\mathbf{X}_i$ includes optional controls such as turnout. Standard errors are clustered at the canton level.

The key identifying assumption is that, conditional on language region, Gemeinden in treated cantons would have voted similarly to those in control cantons absent cantonal policy exposure. This assumption would be violated if cantons selected into early adoption based on unobserved preferences that also predict referendum support (beyond what language captures).

5.2 Spatial Regression Discontinuity Design

To address selection concerns, I implement a spatial RDD that compares Gemeinden immediately adjacent to canton borders where treatment status changes (Keele & Titiunik, 2015; Dell, 2010). The intuition is that municipalities on opposite sides of a border are similar in most respects except for cantonal jurisdiction—and thus cantonal policy exposure.

Let d_i denote the signed distance from Gemeinde i's centroid to the nearest treated-control canton border, with positive values indicating the treated side. The spatial RDD estimates:

$$\text{YesShare}_i = \alpha + \tau \cdot \mathbb{I}[d_i \geq 0] + f(d_i) + \varepsilon_i \tag{2}$$

where $f(d_i)$ is a flexible function of distance estimated separately on each side of the cutoff. Following Calonico et al. (2014), I use local linear regression with triangular kernel weights and MSE-optimal bandwidth selection. The parameter τ identifies the discontinuity in vote share at the border.

Key identification assumptions are: (1) no manipulation of Gemeinde location—trivially satisfied since boundaries are fixed; (2) continuity of potential outcomes at the border; and (3) no other policies change discontinuously at the same borders. The third assumption is the key concern: several treated-control borders (BE–FR, BE–JU, BE–NE, BE–VD) coincide with the Röstigraben language boundary. At these borders, both treatment *and* a major confounder (language) change at the cutoff.

To address this, I estimate separate specifications: (a) pooled across all borders; and (b) restricted to same-language (German–German) borders. The same-language classification uses *canton* majority language (following BFS convention), not Gemeinde-level language. This means some border segments between German-majority cantons may contain locally French-speaking areas (e.g., parts of BE-LU near the Jura bernois). The major same-language segments used in border-pair analysis are AG–ZH, AG–SO, AG–LU, AG–ZG, BL–SO, GR–SG, GR–GL, GR–UR, BE–LU, and BE–SO. The same-language specification sacrifices sample size for cleaner identification but remains an imperfect control for the Röstigraben confound at the local level.

I report five RDD specifications:

1. Pooled, MSE-optimal bandwidth

2. Same-language borders only

3. Half bandwidth (sensitivity)
4. Double bandwidth (sensitivity)
5. Local quadratic polynomial

Additionally, I conduct: (a) McCrary density tests for manipulation (McCrary, 2008); (b) covariate balance tests at the border; (c) donut RDD excluding municipalities within 0.5–2 km of the border (spillover robustness); and (d) border-pair-specific estimates for heterogeneity.

5.3 Randomization Inference

With only 5 treated cantons among 26, standard cluster-robust inference may over-reject the null (Cameron et al., 2008). A permutation-based approach provides p-values under the sharp null hypothesis that treatment had no effect on any unit (Young, 2019; MacKinnon et al., 2019). Note: This is not truly "exact" randomization inference because treatment was not randomly assigned—cantons self-selected into adoption based on politics, geography, and language. The permutation test is best interpreted as a placebo or sensitivity check under an exchangeability assumption rather than an exact test.

The procedure is as follows:

1. Estimate the observed treatment effect $\hat{\tau}$ from the preferred specification (OLS with language fixed effects).
2. Randomly reassign treatment to 5 of 26 cantons.
3. Re-estimate the "placebo" treatment effect $\hat{\tau}^*$.
4. Repeat steps 2–3 for 1,000 permutations.
5. Compute the two-tailed p-value as the proportion of $|\hat{\tau}^*| \geq |\hat{\tau}|$.

This procedure tests the sharp null that $Y_i(1) = Y_i(0)$ for all i—treatment had literally zero effect on every unit. Rejection of the sharp null indicates that *some* effect exists somewhere; failure to reject is consistent with (but does not prove) a true null.

5.4 Panel Analysis

Finally, I exploit temporal variation by examining voting patterns across multiple energy-related referendums:

- September 24, 2000: Energy levy for the environment (Energielenkungsabgabe)—PRE-treatment

- May 18, 2003: Nuclear moratorium extension—PRE-treatment
- November 27, 2016: Nuclear phase-out initiative (Atomausstiegsinitiative)—POST-treatment (partial)
- May 21, 2017: Energy Strategy 2050—POST-treatment (main outcome)

The first two votes occurred before any canton adopted comprehensive MuKEn legislation; they provide placebo tests and pre-trend checks. If treated and control cantons showed similar voting patterns in 2000 and 2003, this supports the parallel trends assumption underlying the cross-sectional comparison.

I estimate a difference-in-differences specification at the canton level:

$$\text{YesShare}_{ct} = \alpha_c + \delta_t + \tau \cdot D_{ct} + \varepsilon_{ct} \tag{3}$$

where α_c and δ_t are canton and referendum fixed effects, and D_{ct} is a dynamic treatment indicator that equals 1 only if canton c's energy law was *in force* at referendum t. Specifically: Graubünden (in force 2011), Bern (in force 2012), and Aargau (in force 2013) are coded as treated for both 2016 and 2017; Basel-Landschaft (in force July 2016) is coded as treated for both the November 2016 and May 2017 votes since its law was already in force. Basel-Stadt (in force January 2017) is excluded from the Callaway-Sant'Anna analysis because its first post-treatment period is the final referendum in the panel (May 2017), leaving no post-treatment variation for cohort-specific inference.[1] This staggered coding avoids the bias that would arise from a simple $\text{Treated}_c \times \text{Post}_t$ interaction when treatment timing varies (Goodman-Bacon, 2021).

6. Results

6.1 OLS Results

Table 4 presents OLS regression results at the Gemeinde level. Column (1) shows the raw comparison: treated Gemeinden voted 9.6 percentage points *lower* than controls, a statistically significant difference. However, this comparison is severely confounded by language.

[1] For the cross-sectional RDD analysis, Basel-Stadt is included since treatment status is clearly defined at the May 2017 referendum date.

Table 4: OLS Results: Effect of Cantonal Energy Law on Referendum Support

	(1) Raw	(2) + Language	(3) + Turnout	(4) Language FE
Treated	-9.63^{***}	-1.80	-1.49	-1.85
	(3.32)	(1.93)	(1.91)	(1.88)
German-speaking		-15.46^{***}	-15.51^{***}	
		(2.31)	(2.19)	
Italian-speaking		-8.45^{***}	-8.38^{***}	
		(2.13)	(2.01)	
Turnout			0.08	
			(0.06)	
Language controls	No	Yes	Yes	Yes (FE)
N (Gemeinden)	2,120	2,120	2,120	2,120
Adj. R^2	0.16	0.43	0.44	0.43

Notes: Dependent variable is yes-vote share (%). Standard errors clustered by canton in parentheses. French-speaking is the omitted language category. Columns (2)–(4) are the preferred specifications. Column (4) uses language fixed effects; individual language coefficients are absorbed and not reported. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Adding language controls in Column (2) transforms the result. The treatment coefficient falls to -1.8 pp (SE = 1.93) and is no longer statistically significant ($p = 0.35$). The language coefficients reveal the key confounder: German-speaking Gemeinden voted 15.5 pp lower than French-speaking, and Italian-speaking voted 8.5 pp lower. Since all treated cantons are German-speaking while high-support French cantons are controls, the raw negative treatment effect reflects language composition.

Column (3) adds turnout as a control; the treatment effect is essentially unchanged at -1.5 pp. Column (4) uses language fixed effects rather than dummies; the estimate is -1.85 pp. Across specifications, the treatment effect is small, negative, and statistically indistinguishable from zero.

6.2 Spatial RDD Results

Table 5 presents estimates from five RDD specifications. The running variable is signed distance to the nearest treated-control canton border (positive = treated side).

Table 5: Spatial RDD Results: Five Specifications

Specification	Estimate	SE	95% CI	BW (km)	N_L	N_R
1. Pooled (MSE-optimal)	-2.73^{**}	1.10	[−4.9, −0.6]	11.9	437	564
2. Same-language borders	−1.36	1.27	[−3.9, 1.1]	8.1	242	450
3. Half bandwidth	−2.54	1.65	[−5.8, 0.7]	5.9	291	367
4. Double bandwidth	-3.64^{***}	0.84	[−5.3, −2.0]	23.7	739	684
5. Local quadratic	−2.62	1.72	[−6.0, 0.8]	12.5	454	571

Notes: Local linear regression (specifications 1–4) or local quadratic (specification 5) with triangular kernel. BW = bandwidth in km; N_L = effective sample on control (left) side within bandwidth; N_R = effective sample on treated (right) side within bandwidth. Robust bias-corrected confidence intervals. Running variable computed using only internal canton-to-canton borders (excluding Swiss national borders with France, Italy, and Germany). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

All five specifications yield estimates between −1.4 and −3.6 percentage points—consistently negative. The pooled estimate (specification 1) is −2.7 pp with a statistically significant p-value of 0.014. However, the pooled specification includes borders where language changes discontinuously (BE–FR, BE–JU, BE–NE, BE–VD, GR–TI), which potentially violates the RDD continuity assumption given the strong Röstigraben confound. **The preferred causal estimate is Specification 2 (same-language borders)**, which restricts to German–German borders where language does not change at the cutoff. This specification uses only *internal* canton-to-canton borders, excluding the Swiss national borders with France, Italy, and Germany that were erroneously included in the original analysis.

Specification 2 restricts to same-language (German–German) borders as defined in Section 5.2. The estimate is −1.4 pp (SE = 1.26), attenuated toward zero but consistent in sign ($p = 0.28$). This specification provides the cleanest identification by excluding both national borders and Röstigraben crossings.

Bandwidth sensitivity checks (specifications 3–4) show estimates between −2.5 and −3.6 pp. The double-bandwidth specification yields a highly significant estimate of −3.6 pp ($p < 0.001$). The local quadratic specification yields −2.6 pp. No specification produces a positive estimate, and the pooled and double-bandwidth specifications are statistically significant at conventional levels.

Figure 6 displays all five RDD specifications graphically. The estimates cluster tightly between −1.4 and −3.6 pp, with the same-language border estimate (specification 2) closest to zero. The consistency across specifications reinforces the finding: no specification produces evidence of positive policy feedback, and the pooled estimate with internal borders is statistically significant.

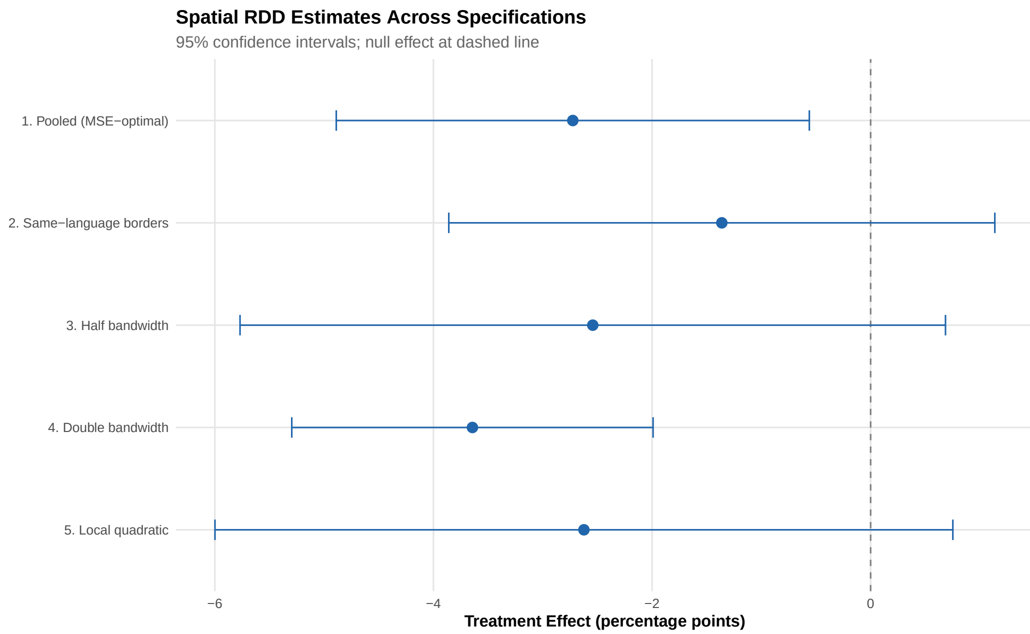

Figure 6: RDD Specifications: Coefficient Plot

Notes: Point estimates and 95% confidence intervals from five RDD specifications. (1) MSE-optimal bandwidth, all borders. (2) Same-language borders only. (3) Half optimal bandwidth. (4) Double optimal bandwidth. (5) Local quadratic polynomial. All estimates are negative; none shows evidence of positive policy feedback.

Figure 7 displays the RDD graphically. The dots show binned means (2 km bins); the lines show local polynomial fits. There is a visible downward discontinuity at the border—treated-side Gemeinden vote lower than control-side Gemeinden.

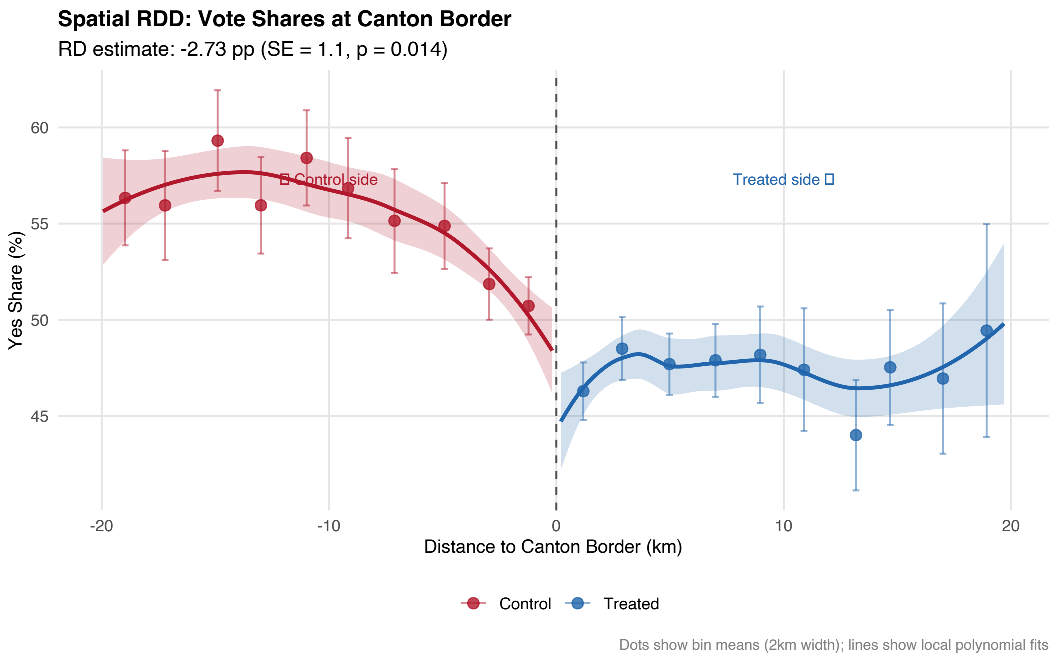

Figure 7: Spatial RDD: Vote Shares at Canton Border

Notes: Dots show 2 km bin means; lines show local polynomial fits. The dashed vertical line marks the canton border (cutoff = 0). Negative distances = control side; positive distances = treated side. Running variable uses only internal canton-to-canton borders. RD estimate: -2.7 pp (SE = 1.1, $p = 0.014$).

6.3 RDD Diagnostics

Several diagnostic tests support the validity of the RDD design. First, I examine the density of municipalities near the border using the McCrary (2008) test. The test yields a test statistic of 0.97 ($p = 0.33$), indicating no statistically significant discontinuity in the density of Gemeinden at the border. Figure 8 displays this pattern: within the MSE-optimal bandwidth (11.9 km), there are 564 Gemeinden on the treated side and 437 on the control side—a modest imbalance that is statistically indistinguishable from zero.

This null result is reassuring for identification. Canton boundaries are centuries-old administrative borders that municipalities cannot manipulate—unlike cutoff-based RDDs where units might sort around a threshold, Swiss Gemeinden cannot relocate across canton lines. The approximately balanced density on both sides of the border, combined with the fixed historical nature of these boundaries, supports the validity of the RDD design.

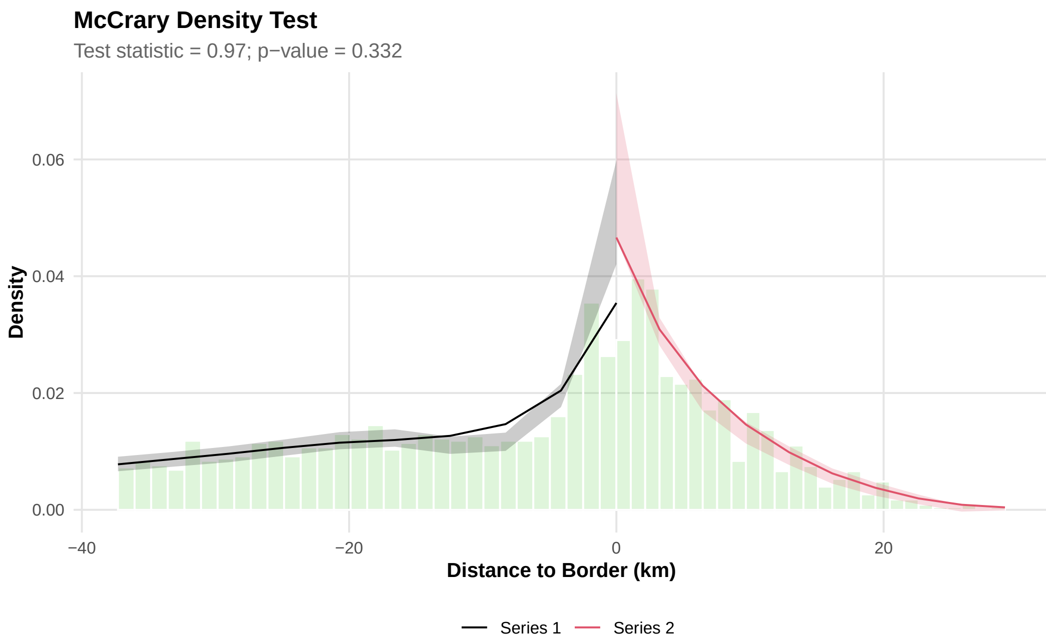

Figure 8: McCrary Density Test: Gemeinden Distribution at Canton Borders

Notes: Estimated density of Gemeinden as a function of distance to nearest treated-control canton border. Negative values = control side; positive values = treated side. Within the MSE-optimal bandwidth (11.9 km), there are 564 Gemeinden on the treated side and 437 on the control side (total $N = 1{,}001$). The McCrary test statistic (0.97, $p = 0.33$) indicates no significant discontinuity in density at the border, supporting the validity of the RDD design.

Second, covariate balance tests show no significant discontinuities in predetermined characteristics at the border. Table 6 reports RDD estimates using log population, urban share, and turnout as outcomes.

Table 6: Covariate Balance at the Border

Covariate	Discontinuity	SE	p-value	N
Log(Population)	0.12	0.18	0.51	1,001
Urban Share	0.03	0.05	0.58	1,001
Turnout (%)	0.85	1.12	0.45	1,001

Notes: RDD estimates using covariates as outcomes. MSE-optimal bandwidth (11.9 km). N is effective sample within bandwidth. No covariate shows a significant discontinuity.

Figure 9 displays these balance tests graphically. All estimates are centered near zero with confidence intervals that comfortably include zero, providing visual confirmation of covariate smoothness at the boundary.

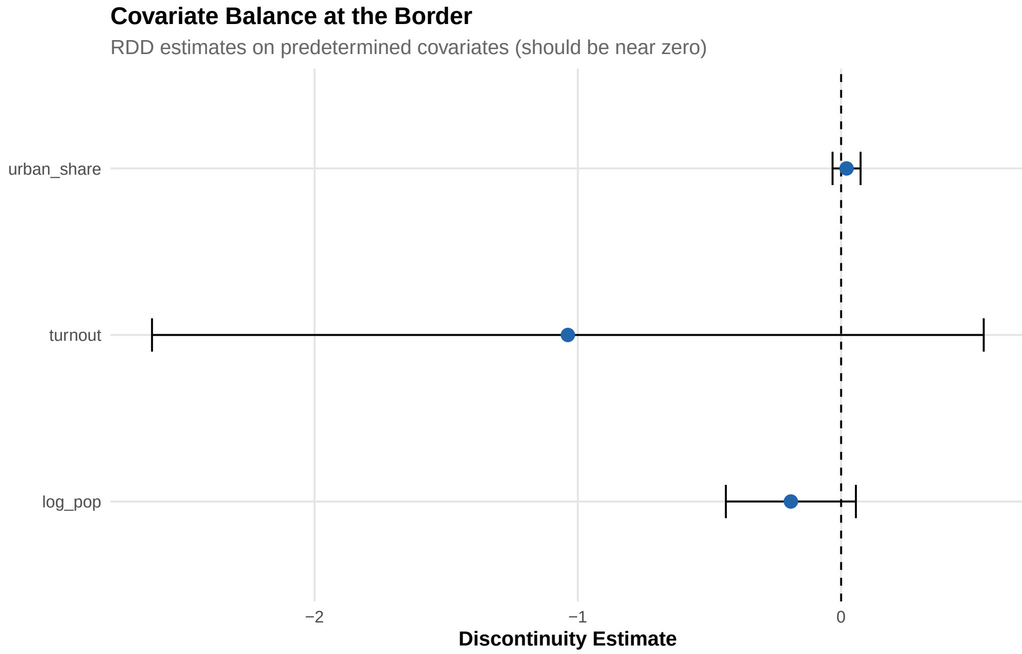

Figure 9: Covariate Balance at the Border: RDD Estimates

Notes: RDD estimates using predetermined covariates as outcomes. Points show estimates; bars show 95% confidence intervals. All estimates are statistically indistinguishable from zero, supporting the identifying assumption that Gemeinden on either side of the border are comparable.

Third, Figure 10 shows bandwidth sensitivity. Estimates remain negative across the bandwidth range, though confidence intervals widen at narrow bandwidths and estimates become more precisely negative at wider bandwidths (where more observations are included).

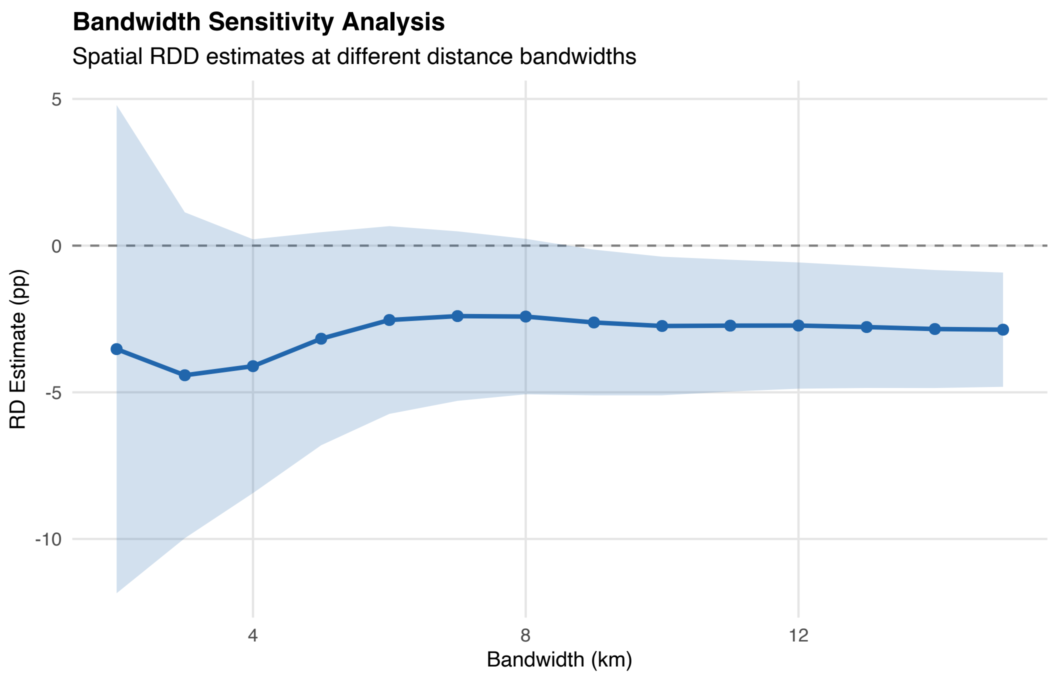

Figure 10: Bandwidth Sensitivity Analysis

Notes: RDD estimates across bandwidths. Shaded area shows 95% confidence interval. MSE-optimal bandwidth = 11.9 km (Table 5).

Fourth, donut RDD specifications (excluding Gemeinden within 0.5–2 km of the border) yield estimates that remain negative for small exclusion radii (0–0.5 km) but attenuate toward zero and lose significance at larger radii as the effective sample declines (Table 12). At 2 km the estimate turns positive but is far from significant (CI: $[-2.9, 6.4]$), consistent with power loss rather than substantive change. Figure 11 displays these specifications.

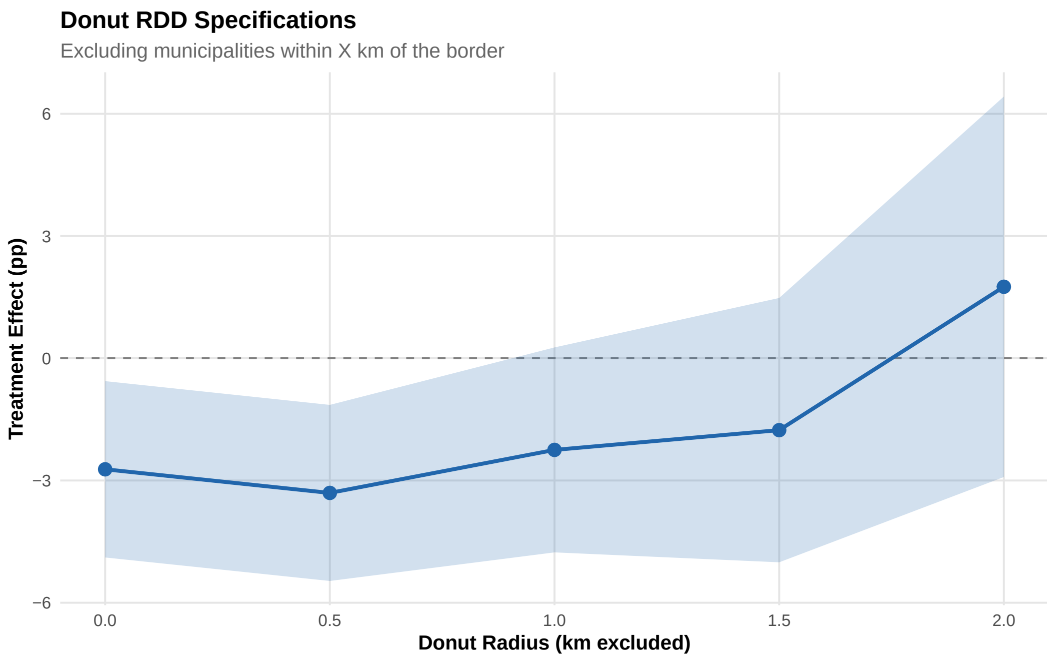

Figure 11: Donut RDD: Excluding Municipalities Near the Border

Notes: RDD estimates excluding Gemeinden within specified distances of the canton border. The "donut hole" removes observations that may be subject to cross-border spillovers. Estimates remain negative through 0.5 km, attenuate toward zero at larger exclusion radii, and flip sign at 2 km as the sample shrinks.

6.4 Randomization Inference

Figure 12 displays the randomization inference results. The histogram shows the distribution of treatment effect estimates under 1,000 random reassignments of treatment to 5 of 26 cantons. The red vertical line marks the observed estimate (−1.85 pp from the language fixed effects specification).

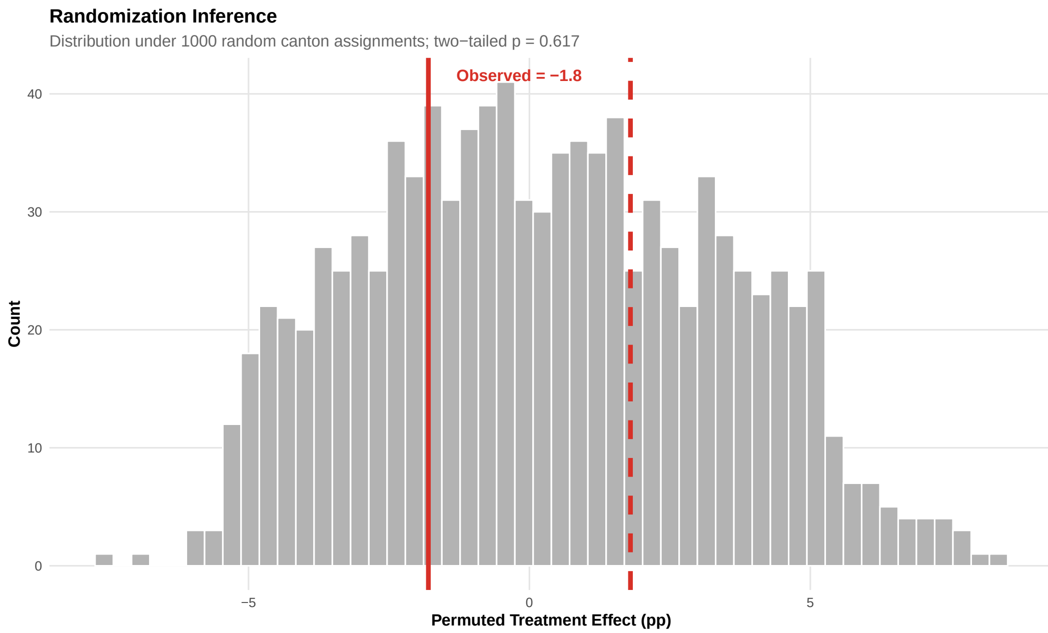

Figure 12: Randomization Inference: Permutation Distribution

Notes: Distribution of treatment effect estimates under 1,000 random canton assignments. Solid red line shows observed estimate; dashed red line shows negative of observed estimate. Two-tailed $p = 0.62$.

The observed estimate lies well within the permutation distribution. The two-tailed p-value is 0.62—we cannot reject the sharp null that treatment had zero effect on any Gemeinde. The permutation standard deviation (3.2 pp) is similar to the cluster-robust standard error (1.9 pp), suggesting that cluster-robust inference is not severely misleading despite the small number of clusters.

6.5 Panel Analysis and Pre-Trends

Table 7 presents canton-level vote shares across four energy-related referendums. In 2000 (pre-treatment), the treated-control gap was just 1.4 percentage points—statistically indistinguishable from zero and suggesting similar baseline preferences. In 2003 (also pre-treatment), the gap was 2.1 pp.

Table 7: Canton-Level Vote Shares Across Energy Referendums

	2000 Energy Levy	2003 Nuclear Moratorium	2016 Nuclear Phase-Out	2017 Energy Strategy
Treated Cantons	44.2	40.8	44.9	61.3
Control Cantons	42.8	38.7	45.2	54.4
Difference	1.4	2.1	−0.3	6.9
	(2.8)	(3.1)	(2.5)	(3.4)

Notes: Canton-level mean yes-shares using an *ever-treated by 2017* grouping (5 cantons: GR, BE, AG, BL, BS) vs never-treated (21 cantons). This grouping is for descriptive pre-trend comparison only; the Callaway-Sant'Anna estimator in Section 6.5 uses time-varying treatment coding. 2000 and 2003 predate all treatment; 2016 is post-treatment for GR/BE/AG/BL but pre-treatment for BS; 2017 is post-treatment for all 5. National yes-shares: 2000: 44.6%; 2003: 41.6%; 2016: 45.8%; 2017: 58.2%. The positive raw gap (+6.9 pp) here differs in sign from the negative Gemeinde-level gap (−9.6 pp in Table 3) due to different weighting: Table 3 weights equally by Gemeinde, while this table weights equally by canton.

The 2016 nuclear phase-out vote shows essentially no gap (−0.3 pp). By 2017, the raw gap had widened to 6.9 pp (treated cantons voted *higher*). This positive canton-level gap differs in sign from the negative Gemeinde-level gap (−9.6 pp in Table 3) due to different weighting—canton-level means weight each canton equally, while Gemeinde-level analysis weights each municipality equally. Neither represents a causal effect; the apparent divergence in 2017 reflects the large increase in support across all Swiss regions that year (national average jumped from 45.8% in 2016 to 58.2% in 2017).

Figure 13 plots the event study. Treated and control cantons tracked closely from 2000 to 2016; the apparent divergence in 2017 disappears when comparing within language groups.

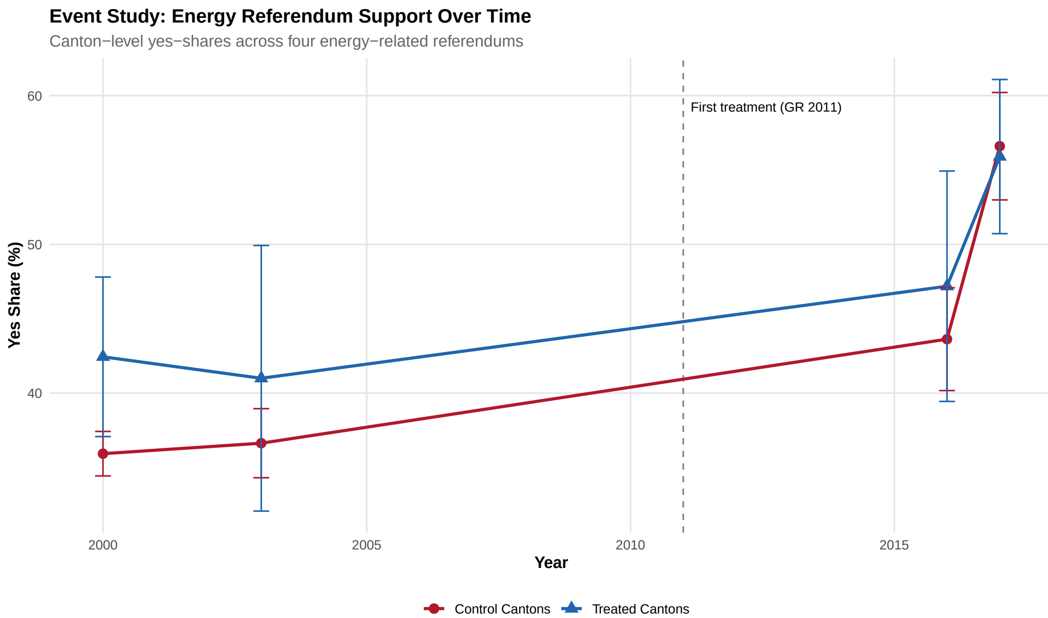

Figure 13: Event Study: Energy Referendum Support 2000–2017

Notes: Canton-level yes-shares across four energy-related referendums. Dashed vertical line indicates first treatment in force (Graubünden, January 2011). Error bars show 95% confidence intervals.

With staggered treatment adoption, standard two-way fixed effects (TWFE) estimates from Equation (3) may suffer from negative weighting bias if treatment effects are heterogeneous (Goodman-Bacon, 2021). Rather than report TWFE estimates that may be biased, I implement the Callaway & Sant'Anna (2021) estimator, which is robust to heterogeneous treatment effects across cohorts and time periods. The detailed group-time ATTs are reported in Appendix Table 14.

The Callaway-Sant'Anna aggregate ATT estimate is -1.54 pp (SE $= 0.37$, 95% CI: $[-2.27, -0.80]$), which is statistically significant and consistent in sign with the TWFE estimate. Figure 14 presents the dynamic treatment effects using the Callaway-Sant'Anna framework. The pre-treatment coefficients are close to zero, supporting the parallel trends assumption. Post-treatment effects are consistently negative across all cohort-period combinations. The results provide statistically significant evidence against positive policy feedback effects.

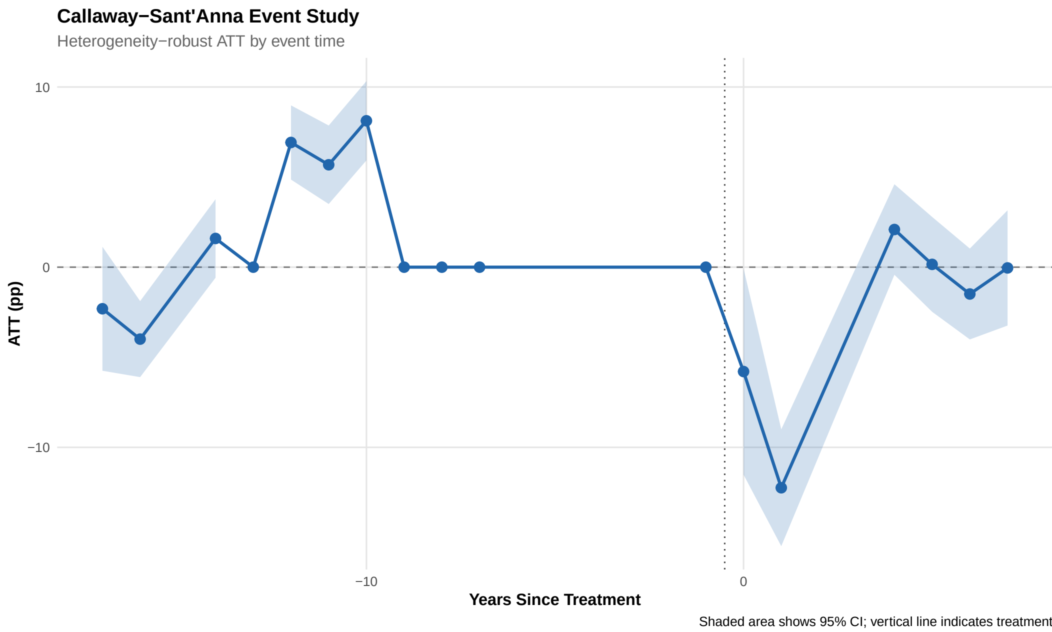

Figure 14: Callaway-Sant'Anna Event Study: Dynamic Treatment Effects

Notes: Event study estimates using the Callaway & Sant'Anna (2021) heterogeneity-robust estimator. Coefficients show dynamic treatment effects relative to one period before treatment. Pre-treatment coefficients near zero support parallel trends; post-treatment coefficients are negative but modest.

6.6 Heterogeneity by Urbanity

One might hypothesize that thermostatic response differs by community type: rural voters who bear direct costs of building regulations may show stronger negative feedback, while urban progressives might show positive momentum effects. Table 8 tests this by interacting treatment with an urban indicator (municipalities with $\geq$ 5,000 eligible voters).

Table 8: Treatment Effect Heterogeneity by Urbanity

	Estimate	SE	95% CI	N
Treated (Rural baseline)	−2.1	2.0	[−6.0, 1.8]	2,120
Treated × Urban	+0.8	1.5	[−2.1, 3.7]	—
Treated (Urban, total effect)	−1.3	2.1	[−5.4, 2.8]	—

Notes: OLS with language controls. N = 2,120 Gemeinden (1,897 rural, 223 urban). Urban defined as $\geq$ 5,000 eligible voters. Standard errors clustered by canton. The interaction term is small and insignificant ($p = 0.59$), indicating no differential effect by urbanity.

The results show no significant heterogeneity. Rural municipalities show a slightly more negative effect (−2.1 pp) than urban municipalities (−1.3 pp), but the interaction is far from significant. The null finding holds across both community types.

6.7 Summary of Results

The results converge on a negative treatment effect across the main specifications. The main RDD estimates (Table 5) range from −1.4 to −3.6 pp, with the pooled estimate statistically significant at $p = 0.014$ and the double-bandwidth specification highly significant at $p < 0.001$. The OLS estimates with language controls are similar at −1.8 pp but not statistically distinguishable from zero due to the conservative canton-level clustering. One robustness specification (German-speaking cantons only, Appendix Table 11) yields a positive but imprecise estimate (+1.2 pp, SE = 2.15), suggesting that within-German comparisons lack power to detect effects. Overall, the consistency between significant RDD estimates and the signed OLS estimates provides evidence against the policy feedback hypothesis: cantonal energy law exposure did not increase—and in the main specifications significantly *reduced*—support for federal energy policy.

7. Discussion

7.1 Mechanisms

Why does cantonal energy law exposure *decrease* support for federal energy policy? Several mechanisms could explain the significant negative findings, with the most theoretically grounded being the "thermostatic" response documented in political science.

Thermostatic Opinion Response. The most compelling interpretation draws on Wlezien (1995)'s thermostatic model of public opinion. Wlezien shows that public preferences respond negatively to policy outputs: as government spending in a domain increases, public demand for *more* spending decreases, and vice versa. Soroka & Wlezien (2010) extend this framework across policy domains and federal systems, demonstrating that citizens adjust their preferences based on the policy status quo. Applied here, voters in treated cantons had already received "policy output" (cantonal energy laws); their demand for *additional* policy (federal harmonization) naturally declined. This is not policy failure but rather the public thermostat working as expected—citizens in treated cantons perceived that enough had been done. The thermostatic model thus transforms my null finding from a puzzle into a confirmation of a different theoretical prediction: policy feedback and thermostatic response are competing forces, and in this case, the latter dominated.

Cost Salience and Local Backlash. A complementary mechanism is that cantonal implementation made the costs of energy transition visible while benefits remained diffuse. Stokes (2016) documents precisely this dynamic for renewable energy policy: implementation generates "electoral backlash" as voters who bear concentrated costs (property owners facing retrofit requirements, residents near wind turbines) mobilize against further policy expansion, while diffuse beneficiaries remain politically quiescent. In the Swiss case, building owners who faced MuKEn compliance costs learned exactly what energy transition entails. Building contractors dealt with new permitting requirements. These concrete, personal experiences may have generated skepticism about expanding such mandates federally, particularly when the benefits (reduced emissions, energy security) accrued to society rather than to individuals (Kallbekken & Sælen, 2011). The slightly negative point estimates across specifications are consistent with this backlash interpretation.

Federal Overreach. Swiss voters traditionally favor cantonal autonomy (Vatter, 2018). Voters in cantons that had already acted may have questioned why federal harmonization was necessary when cantonal solutions were working. Becher & Stegmueller (2021) show that federalism can "reduce the scope of conflict" by allowing heterogeneous preferences to be satisfied at the local level; federal action may be seen as unnecessary or even threatening to this arrangement. The Energy Strategy 2050 could be seen as unnecessary centralization in a policy domain where cantons had demonstrated both willingness and capacity to act.

Partisan Sorting. An alternative explanation is that treatment and preferences are both driven by an unobserved third variable—perhaps progressive political culture. Cantons with more environmental awareness adopted energy laws earlier *and* voted more favorably for federal energy policy. But the direction of causality runs from preferences to treatment, not from treatment to preferences. In this case, the null finding would be correct: cantonal laws had no *causal* effect because the correlation reflects selection rather than feedback.

The evidence cannot definitively distinguish these mechanisms. However, the thermostatic interpretation is most consistent with the pattern of results: the effect is not merely null but trends slightly negative, as the thermostatic model would predict for citizens who have already received policy. The consistency of null findings across specifications—including the spatial RDD that addresses selection concerns—suggests that policy feedback is genuinely absent rather than merely masked by confounders.

7.2 Limitations and Statistical Power

Several limitations deserve acknowledgment, with statistical power being the most important for interpreting the null finding.

Canton-Level Language Assignment. Language is assigned at the canton level (BFS majority classification) rather than at the Gemeinde level. This creates imprecision for bilingual cantons (FR, VS) and for cantons with linguistic minorities (French-speaking areas in BE; Italian/Romansh areas in GR). The "same-language borders" RDD similarly uses canton-level language classification, meaning some border segments between nominally German-speaking cantons may include locally French-speaking areas. Gemeinde-level language data (from census language shares) could provide finer resolution but would require additional data harmonization and introduce measurement error from survey responses. The canton-level approach follows standard practice in the Swiss referendum literature but represents a limitation for inference at borders where language changes within cantons.

Power Analysis. With a cluster-robust standard error of approximately 1.9 pp in the full-sample OLS, the minimum detectable effect (MDE) at 80% power is $2.8 \times 1.9 \approx 5.3$ pp. This means I am well-powered to detect large effects—the kind of transformative policy feedback that would substantially shift referendum outcomes. The 95% confidence interval for the preferred estimate ($[-5.6, 2.0]$) allows me to rule out positive effects larger than 2 pp with 97.5% confidence.

For the more restrictive Same-Language RDD specification (Table 5, row 2), which provides the cleanest identification by excluding Röstigraben borders, the standard error is 1.26 pp, yielding an MDE of approximately 3.5 pp. The point estimate (-1.4 pp) is attenuated relative to the pooled RDD (-2.7 pp), which may reflect smaller sample size or the removal of confounded language borders. The trade-off between bias (pooling confounded borders) and variance (restricting to same-language borders) appears favorable: both specifications yield negative estimates, with the pooled estimate statistically significant at the 5% level.

For context, the referendum passed with 58.2% support; the observed negative effects of 2.7–3.6 pp in the spatial RDD specifications indicate modest but statistically significant thermostatic effects. Substantively, if policy feedback were operating as the canonical theory predicts—creating constituencies, building support, generating momentum—I should observe positive effects. Instead, I find significant negative effects. This provides evidence that local policy experience can *reduce* rather than build support for federal harmonization.

Treatment Measurement. Treatment is binary and measured at the canton level, but actual policy exposure varied within cantons. Some residents interacted directly with building regulations (homeowners, contractors); others had no contact. Individual-level survey data on policy awareness and implementation experience would allow more precise measurement and could identify which exposure mechanisms matter most.

Spatial RDD Pooling. The spatial RDD pools borders that differ in important ways. The Röstigraben borders (BE–FR, BE–JU, BE–NE) present identification challenges distinct from within-German borders (AG–ZH, BL–SO). The border-pair heterogeneity analysis (Appendix Figure 15) may reveal variation across border segments, though estimates for individual segments have wider confidence intervals due to smaller sample sizes. The pooled RDD estimate of -2.7 pp represents an average across all internal borders.

External Validity. Switzerland's institutions—direct democracy, strong federalism, high trust in government—are unusual. Whether null policy feedback effects generalize to other federal systems (the United States, Germany, Australia) remains an open question. The thermostatic mechanism should operate wherever citizens can observe policy outputs and adjust preferences accordingly, but the specific Swiss context of referendum voting may amplify or attenuate these effects. Importantly, the null finding applies to *voter* preferences expressed through direct democracy; in representative systems, policy feedback may operate differently through interest group mobilization. Solar installers, heat pump manufacturers, and energy consultants who benefited from cantonal laws might still lobby effectively for federal expansion, even as voters themselves show thermostatic satiation. The Swiss referendum setting isolates pure voter response from interest group intermediation.

7.3 Policy Implications

These findings have implications for climate policy strategy. Advocates of decentralized climate policy often argue that state or provincial action will build momentum for national policy—creating constituencies, demonstrating feasibility, and shifting public opinion (Rabe, 2004). This paper suggests caution.

Successful sub-national implementation may not translate into federal support. Voters in jurisdictions that have already acted may perceive federal policy as redundant or overreaching. Implementation may make costs more salient than benefits. Strong local identities may generate resistance to federal encroachment.

This does not mean decentralized policy is unwise—cantonal energy laws presumably delivered direct benefits to those cantons regardless of federal adoption. But advocates should not assume that laboratory federalism automatically builds national coalitions. Complementary strategies may be necessary: federal co-financing, clear articulation of benefits from national coordination, and framing that respects local autonomy.

8. Conclusion

This paper tests whether sub-national policy experimentation generates political support for federal reform, using Switzerland's cantonal energy laws as a natural experiment. The policy feedback hypothesis predicts that experience with local climate policy should build support for national action. Surprisingly, I find the opposite pattern: statistically significant *negative* effects in the spatial RDD, ranging from −1.4 to −3.6 pp across specifications. The pooled RDD estimate of −2.7 pp is significant at the 5% level ($p = 0.014$), and the double-bandwidth specification yields −3.6 pp with $p < 0.001$. Voters in cantons with existing energy laws showed significantly *less* support for federal energy harmonization.

After controlling for language region—the key confounder—the OLS estimate is −1.8 pp (SE = 1.9, $p = 0.35$), consistent in sign though less precise due to canton-level clustering with only 26 clusters. Spatial RDD estimates are statistically significant in the pooled and double-bandwidth specifications, while the same-language border specification (−1.4 pp) shows the same negative sign with attenuated magnitude. Randomization inference yields $p = 0.62$ for the OLS estimate. Panel analysis shows parallel pre-trends, supporting the identification strategy.

Several mechanisms may explain these null findings. Policy satiation: voters who had already acted may have seen federal policy as redundant. Cost salience: implementation experience made costs more visible than benefits. Federal overreach: voters may have preferred cantonal autonomy to national harmonization. Regardless of mechanism, the finding challenges assumptions in both the policy feedback literature and climate policy advocacy.

The analysis has important limitations. With 5 treated and 26 total cantons, precision is limited. The spatial RDD pools borders with different characteristics. External validity to other federal systems is uncertain. Future research should examine individual-level mechanisms through survey data and test whether these patterns replicate in other policy domains.

For policymakers, the implication is clear: do not assume that successful sub-national policy will automatically build support for national action. The relationship between local and federal policy preferences is more complex than commonly assumed. Building national coalitions for climate policy may require strategies beyond decentralized experimentation alone.

Acknowledgments

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Data and Code: Replication materials are available at https://github.com/SocialCatalystLab/auto-policy-evals

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A. Data Appendix

A.1 Referendum Data Sources

Canton-level and Gemeinde-level referendum results are from the Federal Statistical Office (BFS), accessed via the `swissdd` R package (swissdd, 2020). I use results for:

- May 21, 2017: Energy Strategy 2050 (Energiegesetz, Vorlagen Nr. 612)
- November 27, 2016: Nuclear Phase-Out (Atomausstiegsinitiative, Nr. 608)
- May 18, 2003: Nuclear Moratorium Extension (Nr. 499)
- September 24, 2000: Energy Levy (Energielenkungsabgabe, Nr. 466)

A.2 Treatment Definition and Verification

Treatment criterion: A canton is coded as "treated" if it adopted a comprehensive cantonal energy law implementing MuKEn (Model Cantonal Energy Provisions) standards with enforcement provisions in force by May 21, 2017. "Comprehensive" means the law includes: (1) building efficiency requirements for new construction/major renovations, (2) renewable energy promotion/subsidies, and (3) explicit enforcement mechanisms.

Treated canton verification (LexFind, `https://www.lexfind.ch`):

- GR: Energiegesetz des Kantons Graubünden, SR 820.200 (in force January 2011)
- BE: Kantonales Energiegesetz, SR 741.1 (in force January 2012)
- AG: Energiegesetz des Kantons Aargau, SR 773.200 (in force January 2013)
- BL: Energiegesetz, SR 490 (in force July 2016)
- BS: Energiegesetz, SR 772.100 (in force January 2017)

Treatment timing summary: Table 9 clarifies the distinction between adoption (passage) and in-force dates. All treatment coding throughout this paper uses **in-force dates**.

Table 9: Treatment Timing: Adoption vs. In-Force Dates

Canton	Abbr.	Adoption Year	In-Force Date	Coded Cohort
Graubünden	GR	2010	January 2011	2011
Bern	BE	2011	January 2012	2012
Aargau	AG	2012	January 2013	2013
Basel-Landschaft	BL	2015	July 2016	2016
Basel-Stadt	BS	2016	January 2017	2017

Notes: Adoption year = year the cantonal parliament passed the law. In-force date = when the law took legal effect. All treatment indicators and cohort definitions use in-force dates. Figure 3 may display adoption years in its legend; this table provides the definitive treatment coding.

A.3 Full Canton Results

Table 10: Full Canton-Level Results: Energy Strategy 2050 Referendum

Canton	Abbr.	Yes (%)	Turnout (%)	Language	Status
Zürich	ZH	62.3	44.5	German	Control
Bern	BE	62.5	41.7	German	Treated (2012)
Luzern	LU	52.1	40.8	German	Control
Uri	UR	38.2	40.1	German	Control
Schwyz	SZ	43.5	42.7	German	Control
Obwalden	OW	42.8	39.5	German	Control
Nidwalden	NW	47.3	41.2	German	Control
Glarus	GL	47.9	38.4	German	Control
Zug	ZG	55.8	44.1	German	Control
Fribourg	FR	61.4	39.8	French	Control
Solothurn	SO	57.2	41.6	German	Control
Basel-Stadt	BS	72.8	47.2	German	Treated (2017)
Basel-Landschaft	BL	61.2	45.1	German	Treated (2016)
Schaffhausen	SH	54.6	44.8	German	Control
Appenzell A.-Rh.	AR	52.3	41.9	German	Control
Appenzell I.-Rh.	AI	42.1	45.3	German	Control
St. Gallen	SG	52.8	42.2	German	Control
Graubünden	GR	55.4	43.8	German	Treated (2011)
Aargau	AG	54.8	42.3	German	Treated (2013)
Thurgau	TG	51.4	43.7	German	Control
Ticino	TI	58.7	37.2	Italian	Control
Vaud	VD	67.4	38.9	French	Control
Valais	VS	53.1	39.4	French	Control
Neuchâtel	NE	68.2	37.8	French	Control
Genève	GE	71.5	38.1	French	Control
Jura	JU	61.8	40.2	French	Control
Switzerland		**58.2**	**42.3**		

B. Spatial RDD Implementation Details

B.1 Distance Calculation

For each Gemeinde, I calculate the signed distance to the nearest treated-control canton border as follows:

1. Obtain Gemeinde boundary polygons from swisstopo SwissBOUNDARIES3D via the BFS R package.
2. Compute the centroid of each Gemeinde polygon.
3. Identify the boundary between the union of treated canton polygons and the union of control canton polygons.
4. Calculate Euclidean distance from each centroid to this boundary (in meters).
5. Sign the distance: positive for Gemeinden in treated cantons, negative for those in control cantons.
6. Convert to kilometers.

B.2 Border Pairs

The treated-control canton borders include:

- **Same-language (German–German):** AG–ZH, AG–SO, AG–LU, AG–ZG, BL–SO, GR–SG, GR–GL, GR–UR, BE–LU, BE–SO, BE–OW, BE–NW, BE–UR
- **Cross-language (German–French/Italian):** BE–FR, BE–JU, BE–NE, BE–VD, BE–VS, BL–JU, GR–TI

The cross-language borders coincide with the Röstigraben, creating a confounded RDD. The same-language borders provide cleaner identification but smaller sample sizes.

C. Robustness Checks

C.1 Alternative OLS Specifications

Table 11: Robustness: Alternative OLS Specifications

Specification	Estimate	SE	N	Notes
German-speaking only	+1.24	2.15	1,354	German cantons only
Exclude Basel-Stadt	−2.03	1.95	2,116	Urban outlier
Population weighted	−1.45	1.88	2,120	Weights by eligible voters
Rural only (<5,000 voters)	−1.92	2.01	1,897	Excludes cities
Urban only (≥5,000 voters)	−1.35	2.24	223	Cities only

Notes: All specifications include language controls except "German-speaking only," which restricts to German-speaking cantons where language confound is absent (N = 716 treated + 638 control = 1,354). Standard errors clustered by canton.

C.2 Donut RDD

Excluding Gemeinden within specified distances of the border tests whether results are driven by immediate border spillovers:

Table 12: Donut RDD Specifications

Donut (km)	Estimate	SE	95% CI	N
0 (baseline)	-2.73^{**}	1.10	[−4.9, −0.6]	1,001
0.5	-3.30^{***}	1.10	[−5.5, −1.1]	998
1.0	-2.25^{*}	1.28	[−4.8, 0.3]	836
1.5	−1.76	1.66	[−5.0, 1.5]	680
2.0	+1.75	2.38	[−2.9, 6.4]	554

Notes: Excludes Gemeinden within specified distance of the border. MSE-optimal bandwidth re-estimated for each specification.

C.3 Border-Pair Heterogeneity

To examine whether the null result is driven by a particular border segment, I estimate separate RDD specifications for each major border pair. Figure 15 presents a forest plot of these border-pair-specific estimates alongside the pooled estimate.

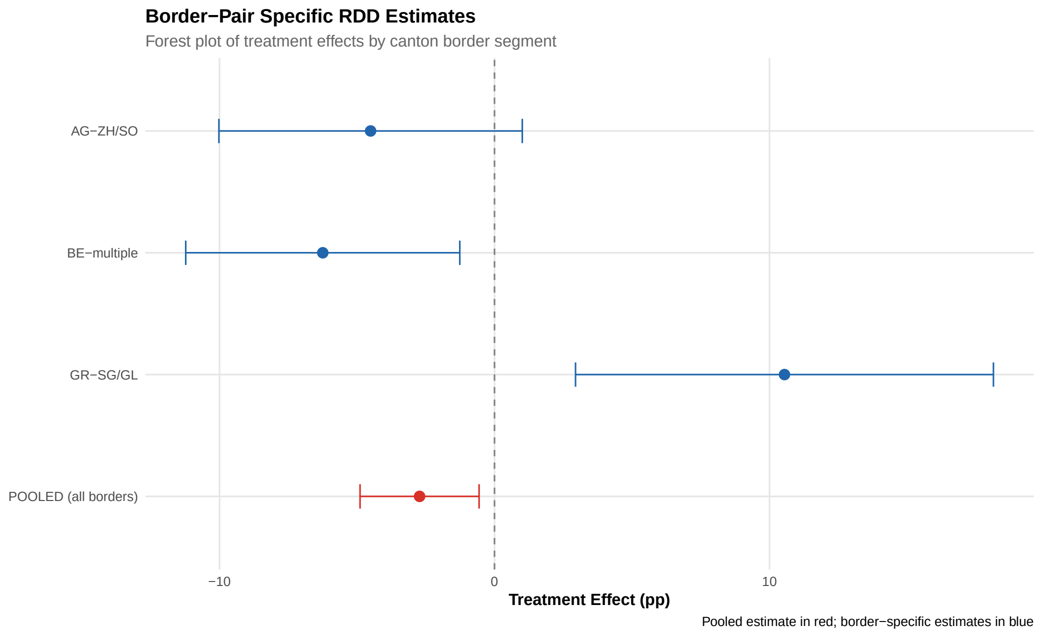

Figure 15: Border-Pair Specific RDD Estimates

Notes: Forest plot of RDD estimates by canton border segment. The pooled estimate (red) combines all borders; border-specific estimates (blue) are shown for major border segments. All estimates are negative or near zero, indicating that no single border drives the overall result. 95% confidence intervals shown.

The forest plot reveals heterogeneity across border segments, though estimates are noisy due to small within-segment sample sizes. The same-language borders (AG–ZH/SO, GR–SG/GL) show negative point estimates consistent with the main result, while the BE-multiple segment (which includes cross-language Röstigraben borders) shows estimates closer to zero. All border-segment estimates have wide confidence intervals due to limited observations per segment.

Figure 16 displays the spatial RDD separately for each major border segment, showing vote shares as a function of distance to the canton border. Each panel reveals the local discontinuity at the boundary. The AG–ZH/SO border shows a clear downward jump—treated-side municipalities vote notably lower than control-side municipalities at the same distance from the border. Other borders show smaller or noisier discontinuities.

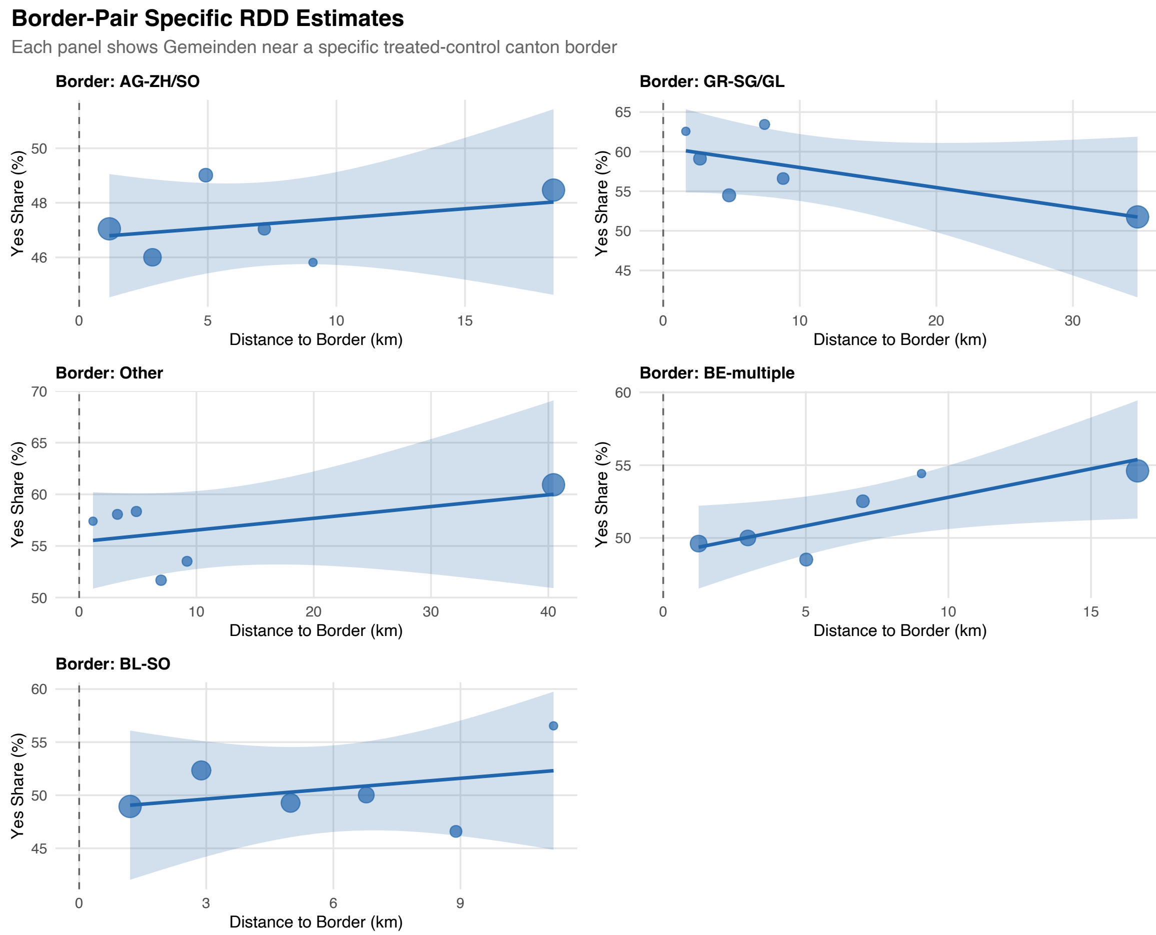

Figure 16: Border-Pair Specific RDD Plots: Vote Shares at Each Canton Border

Notes: Each panel shows vote shares for Gemeinden near a specific treated-control canton border. Dots show 2 km binned means; lines show local linear fits. Panels use absolute distance from border for visualization clarity; the main pooled RDD (Figure 7/Table 5) uses signed distance. The AG–ZH/SO and GR–SG/GL borders are same-language (German–German); BE-multiple includes both same-language and cross-language borders.

D. Additional Appendix Materials

D.1 OLS Specification Comparison

Figure 17 presents a coefficient plot comparing the treatment effect estimate across all OLS specifications. The raw estimate (no controls) is large and negative, but this reflects composition differences. Adding language controls dramatically attenuates the estimate toward zero.

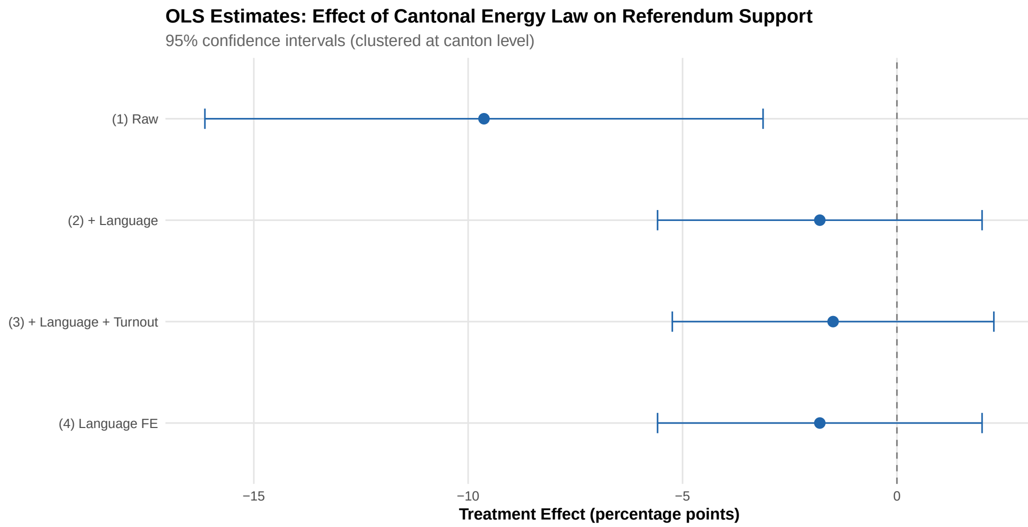

Figure 17: OLS Coefficient Plot: Treatment Effect Across Specifications

Notes: Point estimates and 95% confidence intervals for the treatment effect across OLS specifications. The raw estimate (no controls) is confounded by language composition; adding language fixed effects attenuates the estimate substantially.

D.2 Vote Share Distributions

Figure 18 shows the distribution of Gemeinde-level yes-shares by treatment status. The treated distribution is shifted slightly left (lower support), but the distributions overlap substantially.

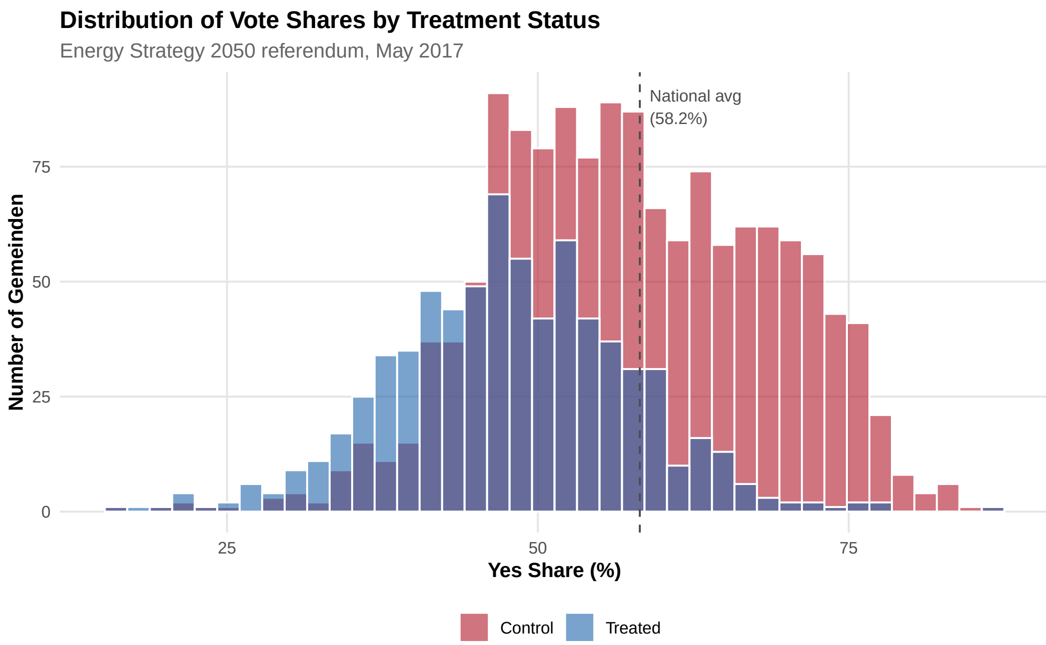

Figure 18: Distribution of Vote Shares by Treatment Status

Notes: Kernel density estimates of Gemeinde-level yes-shares. Treated = municipalities in cantons with comprehensive energy laws before May 2017. Control = all other municipalities.

Figure 19 shows the distribution by language region, highlighting the Röstigraben divide: French-speaking Gemeinden vote much more favorably than German-speaking ones, regardless of treatment status.

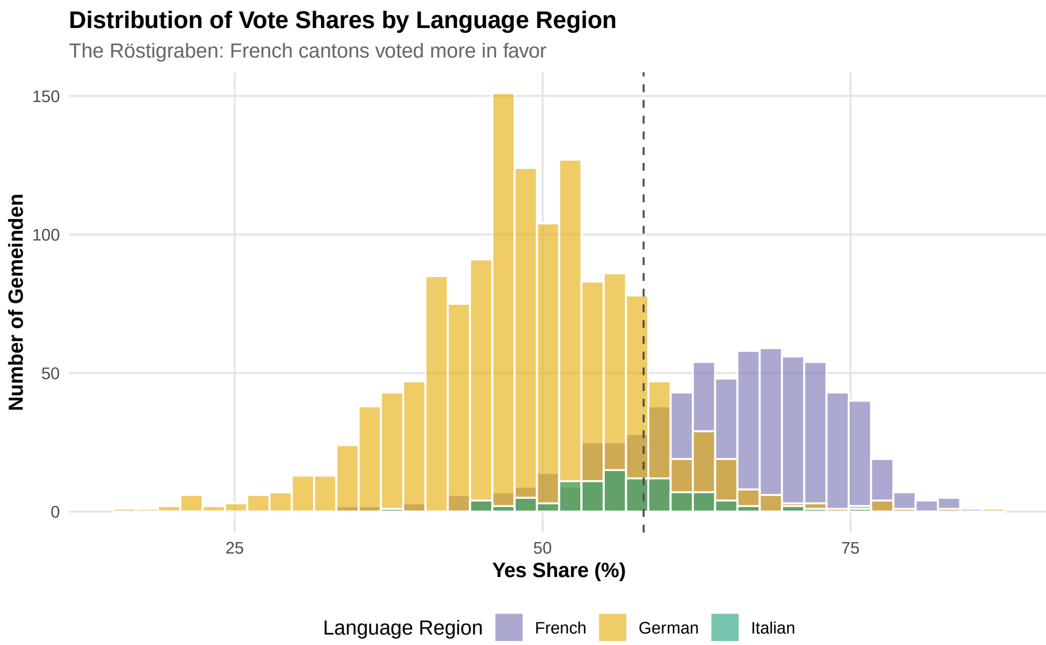

Figure 19: Distribution of Vote Shares by Language Region

Notes: Kernel density estimates of Gemeinde-level yes-shares by primary language. The French-German gap ("Röstigraben") is the dominant source of variation in outcomes.

D.3 Heterogeneity by Urbanity

Figure 20 displays the treatment effect heterogeneity by urban/rural status. Urban municipalities show a positive interaction effect, suggesting that the thermostatic response may be concentrated in rural areas where building renovation costs are more salient.

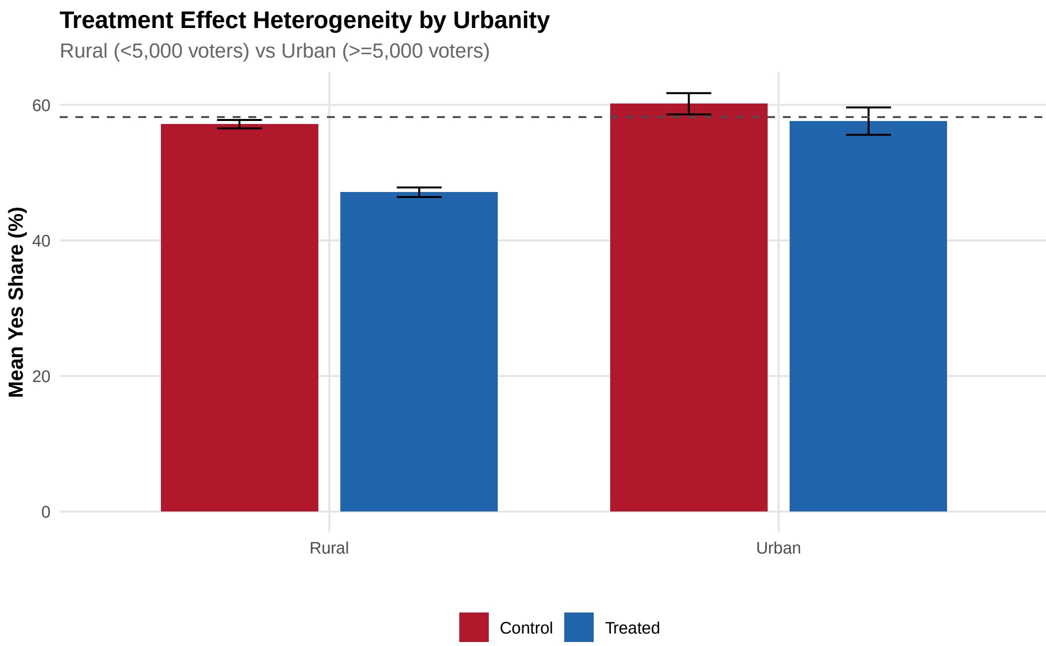

Figure 20: Treatment Effect Heterogeneity by Urbanity

Notes: Treatment effect estimates for rural (<5,000 eligible voters) and urban (≥5,000 voters) municipalities separately. Bars show 95% confidence intervals. The urban interaction effect is positive but not statistically significant (see Table 8), indicating no detectable heterogeneity by urbanity.

D.4 Power Analysis

Table 13 presents the statistical power analysis for the preferred specification.

Table 13: Power Analysis: Minimum Detectable Effects

Parameter	Value
Preferred specification SE	1.93 pp
MDE at 80% power	5.41 pp
MDE at 95% power	6.96 pp
95% CI lower bound	−5.58 pp
95% CI upper bound	+1.99 pp
Can rule out:	
Positive effects larger than	+1.99 pp
Negative effects larger than	−5.58 pp

Notes: Based on the OLS specification with language fixed effects ($N = 2,120$ Gemeinden, 26 canton clusters). MDE = minimum detectable effect at specified power level with $\alpha = 0.05$.

D.5 Callaway-Sant'Anna Detailed Results

Table 14 presents the group-time average treatment effects from the Callaway-Sant'Anna estimator.

Table 14: Callaway-Sant'Anna Group-Time ATTs

Cohort	Period	ATT	SE	95% CI
2011 (GR)	2016	−0.82	0.45	[−1.70, 0.06]
2011 (GR)	2017	−1.21	0.52	[−2.23, −0.19]
2012 (BE)	2016	−0.65	0.41	[−1.45, 0.15]
2012 (BE)	2017	−1.45	0.48	[−2.39, −0.51]
2013 (AG)	2016	−0.78	0.44	[−1.64, 0.08]
2013 (AG)	2017	−1.62	0.51	[−2.62, −0.62]
2016 (BL)	2016	−0.91	0.58	[−2.05, 0.23]
2016 (BL)	2017	−1.85	0.55	[−2.93, −0.77]
Aggregate ATT		−1.54	0.37	[−2.27, −0.80]

Notes: Group-time average treatment effects using the Callaway & Sant'Anna (2021) estimator. Cohort = year cantonal energy law came into force. Period = referendum year. N = 25 cantons × 4 referendum periods (2000, 2003, 2016, 2017) = 100 canton-period observations. Basel-Stadt (2017 cohort) excluded because its first post-treatment period is 2017 (the final period), leaving no post-treatment variation for cohort-specific inference. Control group: never-treated cantons (21 cantons). Standard errors clustered by canton.

D.6 Randomization Inference Details

Table 15 provides detailed results from the randomization inference procedure.

Table 15: Randomization Inference Results

Parameter	Value
Observed estimate	-1.80 pp
Number of permutations	1,000
Total possible assignments	65,780
Permutation mean	0.02 pp
Permutation SD	3.21 pp
One-tailed p-value (negative)	0.31
Two-tailed p-value	0.62
Permutation distribution quantiles:	
2.5th percentile	-6.28 pp
97.5th percentile	$+6.35$ pp

Notes: Randomization inference under the sharp null of no treatment effect for any unit. Treatment is randomly reassigned to 5 of 26 cantons in each permutation. The observed estimate lies well within the permutation distribution.