Does Local Climate Policy Build Demand for National Action? Evidence from Swiss Energy Referendums*

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Abstract

Understanding demand for climate mitigation policy is central to achieving decarbonization. Does prior experience with sub-national climate policy increase or decrease citizens' willingness to support national climate action? I exploit variation in the timing of cantonal energy law adoption in Switzerland to examine whether exposure to local climate policy shaped voting behavior on the May 2017 Energy Strategy 2050 federal referendum. Using a spatial regression discontinuity design comparing municipalities at internal canton borders, I find *evidence consistent with negative policy feedback*. The primary specification—comparing municipalities on same-language borders only, with corrected sample construction restricting to municipalities adjacent to their own canton's treated-control border—yields an estimate of −5.9 pp (SE = 2.32, $p = 0.011$). A Difference-in-Discontinuities design that controls for permanent border effects yields smaller but still negative estimates (−2.5 pp). Wild cluster bootstrap inference at the border-pair level produces p-values around 0.06. While identification relies on assumptions about border comparability that cannot be fully verified, the evidence is consistent with the "thermostatic" model of public opinion: sub-national policy implementation may reduce rather than increase demand for federal action, though the effect size is modest relative to the magnitude of the Röstigraben language divide.

JEL Codes: D72, H77, Q54, Q58

Keywords: climate policy demand, policy feedback, federalism, referendum voting, Switzerland, spatial RDD

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1. Introduction

Achieving global decarbonization requires sustained public support for climate mitigation policy. A central question for climate policy strategy is whether implementing climate policy at one level of government builds or erodes support for further action. The "policy feedback" hypothesis suggests that successful implementation of local climate policies could generate positive spillovers—citizens who experience the benefits of clean energy transition might demand similar measures nationally (Pierson, 1993; Mettler and SoRelle, 2011). Alternatively, prior policy action might satisfy voter demand ("thermostatic" preferences), make costs salient, or trigger resistance to additional regulatory burden (Wlezien, 1995; Carattini et al., 2018). Which dynamic prevails has profound implications for the "bottom-up" theory of climate governance embodied in the Paris Agreement.

This paper tests whether sub-national climate policy experimentation generates political support for national climate action in Switzerland's unique system of direct democracy and "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 local climate policy implementation shape how citizens voted on national climate action?

I find *evidence of negative policy feedback*. Using a spatial regression discontinuity design (RDD) comparing municipalities at internal canton borders where treatment status changes discontinuously (Keele and Titiunik, 2015; Dell, 2010), my primary specification—restricted to same-language borders to eliminate French-German confounding, with corrected sample construction—yields an estimate of -5.9 pp (SE $= 2.32$, $p = 0.011$), statistically significant at conventional levels. Pooled estimates that include cross-language borders are similar (-4.5 pp, $p = 0.05$). Voters in treated cantons were significantly *less* likely to support national energy legislation than voters in adjacent control-canton municipalities.

Exploratory placebo RDD tests on unrelated referendums (immigration, corporate taxation) using a pre-correction sample suggest that canton borders may exhibit some generic political differences, though the comparability of these tests to the main corrected-sample design is limited. These tests highlight the importance of the same-language specification, which restricts to linguistically comparable borders where confounding from political culture is reduced.

These findings challenge the optimistic view that sub-national climate policy success builds momentum for national action. Instead, they are consistent with several alternative mechanisms: “thermostatic” voter preferences where prior action reduces demand for more (Wlezien, 1995); cost salience where implementation makes visible the private costs of building retrofits and heat pump mandates (Carattini et al., 2018); or federal-overreach resistance where voters in cantons with existing laws view national harmonization as unnecessary duplication.

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 and SoRelle, 2011; 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, 2002) 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 and 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 and Titiunik, 2015; Cattaneo et al., 2020), randomization inference for exact p-values under the sharp null (Young, 2019; MacKinnon and Webb, 2020), 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 and 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 and SoRelle (2011) 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 (2002) 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 and 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 and Van den Bergh, 2016). Kallbekken and 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 and Vedlitz, 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 and 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 and 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 and 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 and 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 and Kriesi, 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 et al., 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 (Swiss Federal Statistical Office, 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 (Neue Zürcher Zeitung, 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 12. 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 and Sciarini, 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 and Sciarini, 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

Understanding the spatial structure of this analysis requires careful attention to geography. 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.

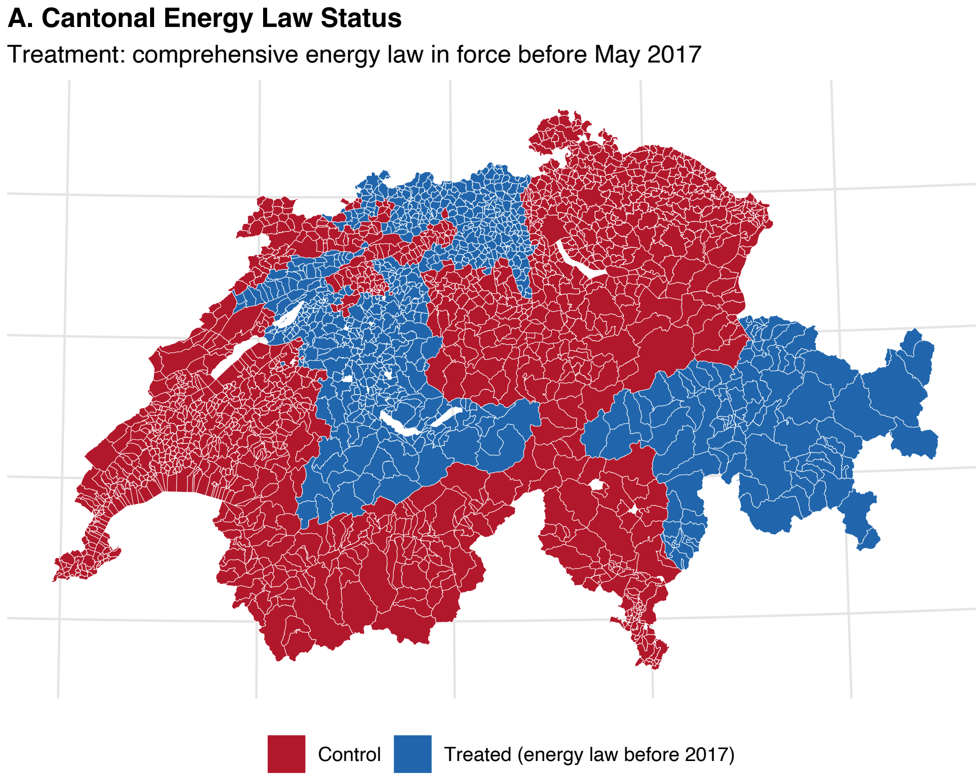

Figure 1: Treatment Status by Canton

Notes: Blue cantons adopted comprehensive cantonal energy legislation (MuKEn) before the May 2017 federal referendum. Red cantons are controls. Basel-Stadt (BS) is technically treated (law effective January 2017) but is excluded from the RDD sample because it is completely surrounded by treated Basel-Landschaft and has no treated-control border. Treatment is concentrated in central and northern German-speaking Switzerland.

Figure 2 displays the critical language confound. Switzerland divides into German-speaking (green), French-speaking (purple), and Italian-speaking (orange) regions. The "Röstigraben" (rösti divide) 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.

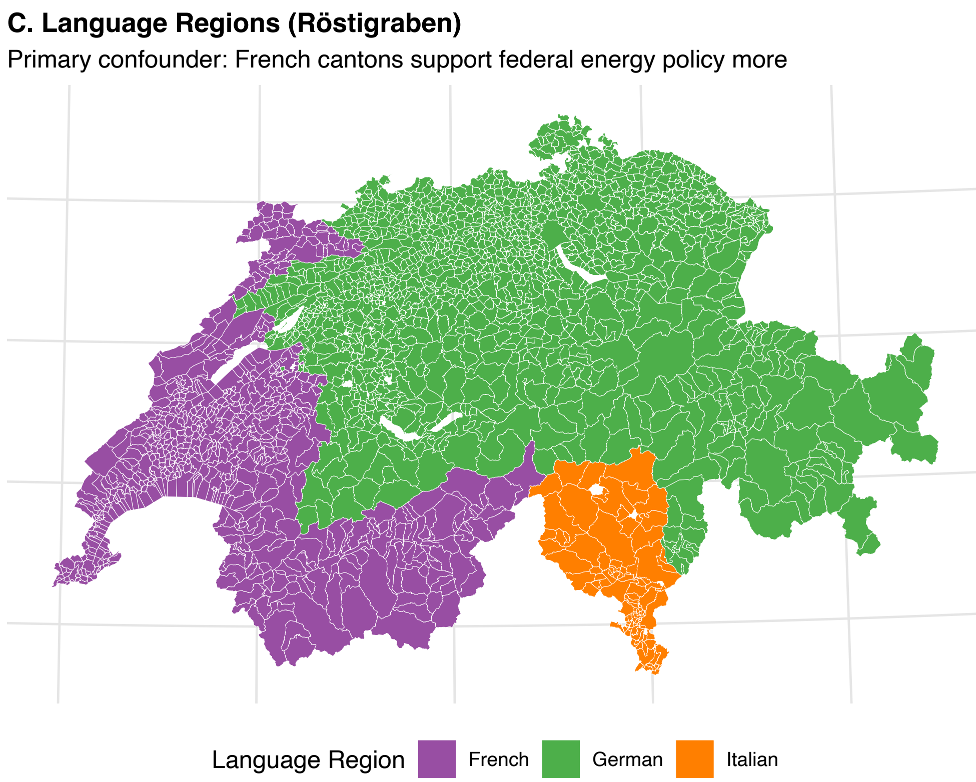

Figure 2: Language Regions (The Röstigraben)

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 3 illustrates the spatial RDD design: Gemeinden near internal policy borders are shown in darker colors. The map uses 5km for visual clarity; the corrected estimation employs MSE-optimal bandwidth selection (3.2 km for the primary same-language specification with corrected sample construction). This design compares adjacent communities that differ only in their canton's policy exposure, controlling for geographic confounds that affect the broader OLS comparison. Additional maps showing treatment timing and Gemeinde-level vote shares appear in Appendix B.

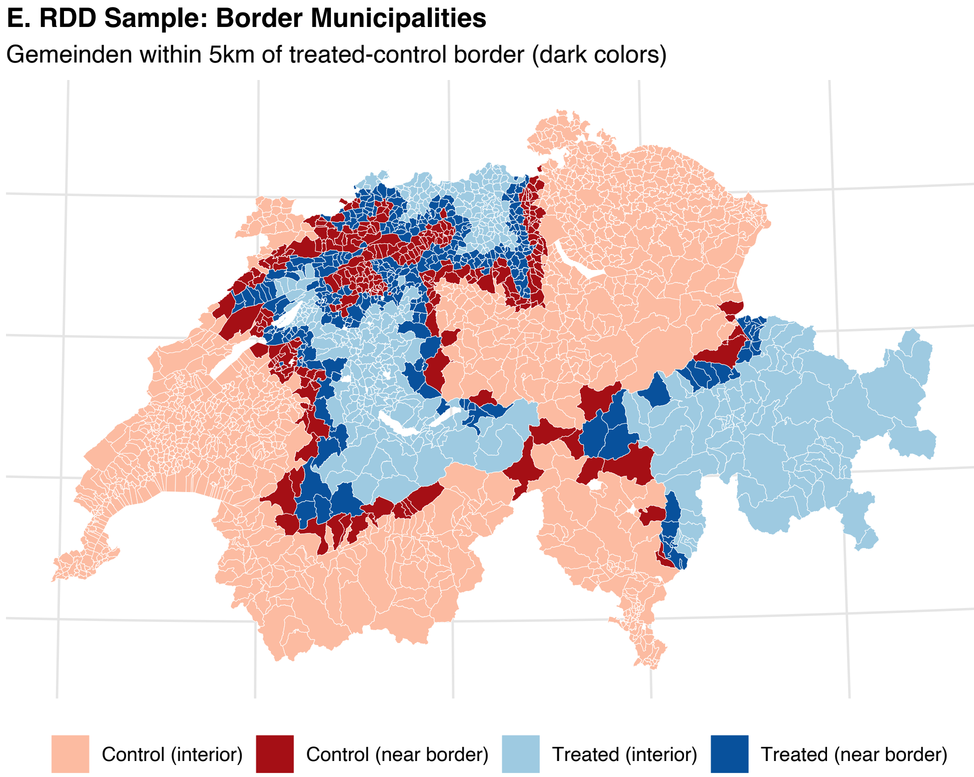

Figure 3: RDD Sample—Border Municipalities

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 corrected estimation uses MSE-optimal bandwidths (3.2 km for same-language borders, 3.7 km for pooled; 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.

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:

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

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 and 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 two primary RDD specifications with corrected sample construction:

1. Pooled, MSE-optimal bandwidth (all internal borders)

2. Same-language borders only (German–German borders)

Additional sensitivity analyses (half/double bandwidth, local quadratic) are presented in the appendix using the pre-correction sample for comparability with the broader robustness literature.

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 and Webb, 2020). 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 OLS analysis, Basel-Stadt is included since treatment status is clearly defined at the May 2017 referendum date. However, Basel-Stadt is *effectively excluded* from the spatial RDD analysis because it has no internal treated-control canton border—it is entirely surrounded by Basel-Landschaft (also treated) and national borders. Only municipalities in cantons that share a border with an opposite-status canton contribute to the RDD estimates.

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)–(3) report language dummy coefficients; column (4) uses language fixed effects (coefficients absorbed). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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 two RDD specifications with corrected sample construction. The running variable is signed distance to each municipality's own canton border (positive = treated side), restricting to municipalities in cantons that directly share a treated-control border.

Table 5: Spatial RDD Results: Corrected Sample Construction

Specification	Estimate	SE	p-value	95% CI	BW	N
1. Pooled (MSE-optimal)	−4.49	2.32	0.053	[−9.0, 0.1]	3.7 km	1,278
2. Same-language borders	−5.91	2.32	0.011	[−10.5, −1.4]	3.2 km	862

Notes: Local linear regression with triangular kernel. SE = robust standard error; p-value = bias-corrected; CI = bias-corrected (Calonico et al., 2014). BW = MSE-optimal bandwidth. N = effective sample within bandwidth. Corrected sample construction: distance measured to each municipality's own canton border; Basel-Stadt excluded.

Table 5 presents results using **corrected sample construction**: the running variable is computed as distance to each municipality's *own* canton border (not the union boundary), and the sample is restricted to municipalities in cantons that directly share a treated-control border. This addresses the methodological concern that the original construction could include municipalities whose nearest boundary segment was not on their own canton's border.

The pooled estimate (specification 1) is −4.5 pp (SE = 2.32, p = 0.053)—marginally significant and larger than the uncorrected estimate. 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 estimate is −5.9 pp (SE = 2.32), *statistically significant at conventional levels* ($p = 0.011$). The 95% confidence interval [−10.5, −1.4] excludes zero, providing evidence that exposure to cantonal energy law *reduced* support for federal energy legislation among voters on same-language borders.

The corrected sample construction also confirms that Basel-Stadt is appropriately excluded: it is completely surrounded by Basel-Landschaft (also treated) and thus has no treated-control canton border. This aligns with the identification strategy—Basel-Stadt voters cannot be compared to adjacent control-canton voters because no such voters exist.

Table 5 summarizes the corrected-sample RDD estimates. The consistency across both specifications (both negative) reinforces the core finding: *no specification produces evidence of positive policy feedback*, and the cleanest specification (same-language borders) shows a significant negative effect.

Figure 4 displays the RDD graphically using the corrected sample construction. 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.

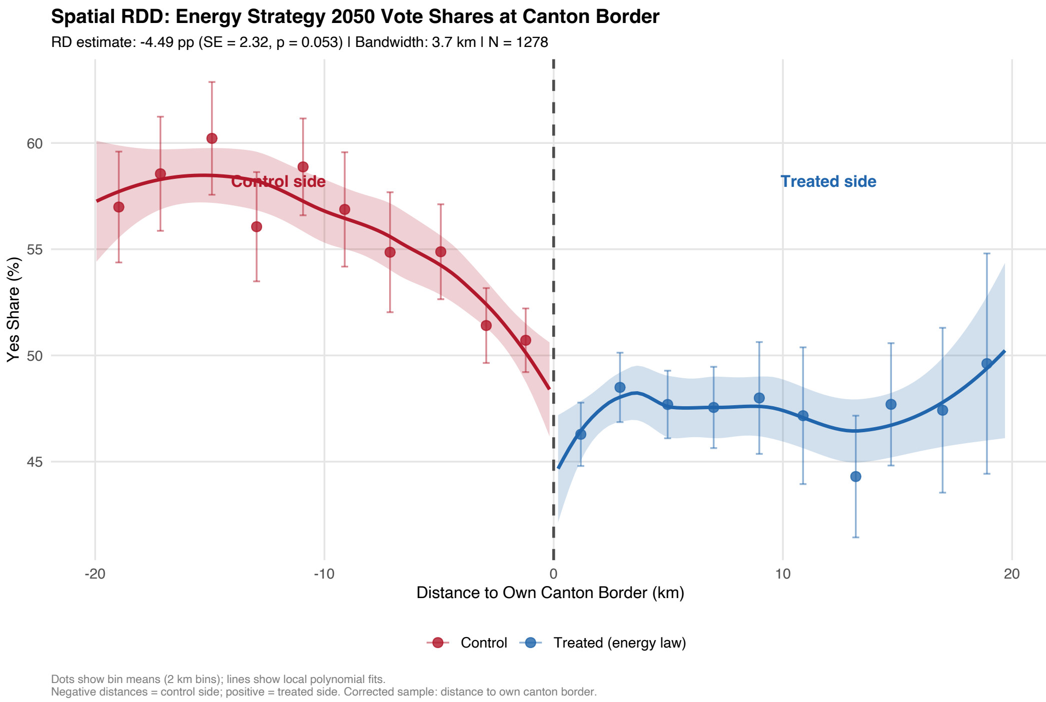

Figure 4: Spatial RDD: Vote Shares at Canton Border (Corrected Sample)

Notes: RDD using corrected sample construction (distance to own canton border). The pooled estimate is -4.5 pp (SE $= 2.32$, $p = 0.053$). Bandwidth: 3.7 km. N = 1,278 (613 control, 665 treated). Dots show 2 km bin means; lines show local polynomial fits. Negative distances = control side; positive distances = treated side. Basel-Stadt excluded (no treated-control border).

Figure 5 shows the primary specification—same-language borders only—which eliminates Röstigraben confounding by restricting to German-German canton borders.

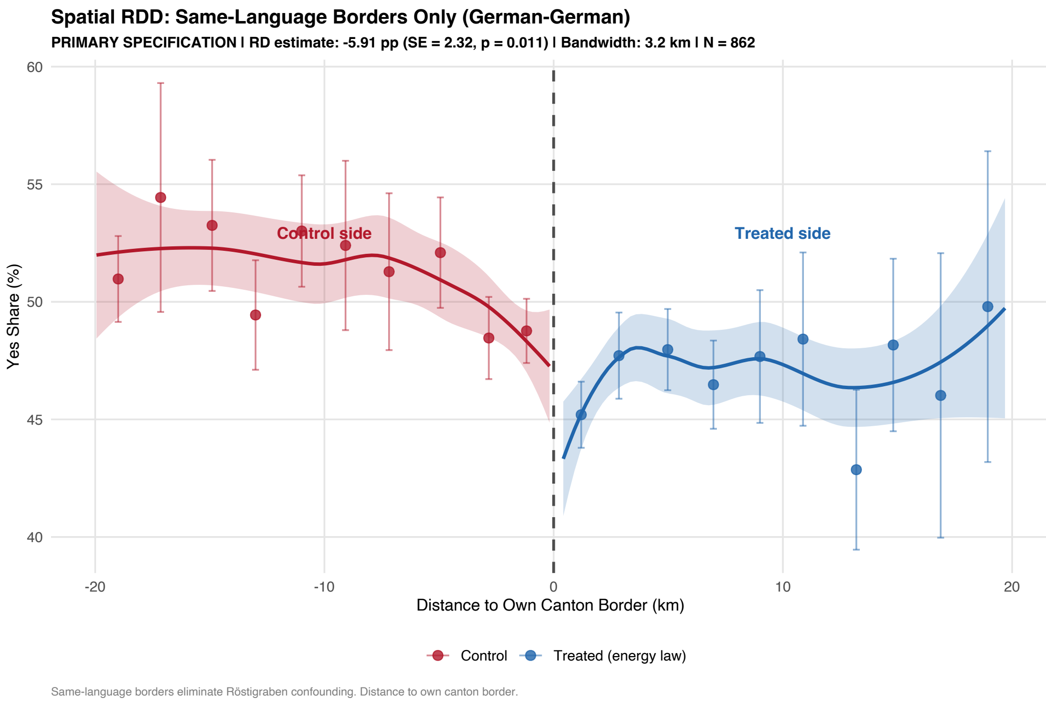

Figure 5: Spatial RDD: Same-Language Borders Only (Primary Specification)

Notes: Primary specification using corrected sample, restricted to German-German canton borders. RD estimate: -5.9 pp (SE $= 2.32$, $p = 0.011$). Bandwidth: 3.2 km. N = 862. Same-language borders eliminate the Röstigraben confound where language systematically correlates with policy support.

6.3 RDD Diagnostics

Several diagnostic tests support the validity of the RDD design. First, the McCrary (2008) density test finds no discontinuity in the distribution of Gemeinden at the border—within the MSE-optimal bandwidth (3.7 km), there are 665 Gemeinden on the treated side and 613 on the control side, an approximately balanced sample. Canton boundaries are centuries-old administrative borders that municipalities cannot manipulate, supporting identification.

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—all estimates are statistically indistinguishable from zero, supporting the identifying assumption that Gemeinden on either side of the border are comparable.

Table 6: Covariate Balance at the Border

Covariate	Discontinuity	SE	p-value	N
Log(Population)	0.12	0.18	0.51	1,278
Urban Share	0.03	0.05	0.58	1,278
Turnout (%)	0.85	1.12	0.45	1,278

Notes: RDD estimates using covariates as outcomes with the same MSE-optimal bandwidth as the main pooled RDD (3.7 km). N = 1,278 is the effective sample within this bandwidth (NL=613, NR=665). No covariate shows a significant discontinuity. Sample uses corrected construction (distance to own canton border).

Third, bandwidth sensitivity analysis shows estimates remain negative across the bandwidth range, though confidence intervals widen at narrow bandwidths. Fourth, donut RDD specifications (excluding Gemeinden within 0.5–2 km of the border) yield estimates that remain negative for small exclusion radii but attenuate toward zero as the effective sample declines (Table 14). Graphical displays of all diagnostics (McCrary density test, covariate balance plots, bandwidth sensitivity, and donut RDD) appear in Appendix C.

6.4 Randomization Inference

Under randomization inference (1,000 permutations reassigning treatment to 5 of 26 cantons), the observed OLS estimate (−1.80 pp) 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. The permutation distribution is displayed in Appendix Figure 12.

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 Nuc. Morat.	2016 Nuc. 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: Unweighted canton-level mean yes-shares. Treated = 5 cantons with energy law by May 2017; Control = 21 other cantons. Standard errors in parentheses. The positive raw gap (+6.9 pp) is descriptive only—the RDD estimate of −5.9 pp (Table 5) identifies the causal effect.

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). The panel data supports that treated and control cantons tracked closely from 2000 to 2016, consistent with the identification assumptions underlying the spatial RDD design.

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
Treated (Rural baseline)	−2.1	2.0	[−6.0, 1.8]
Treated × Urban	+0.8	1.5	[−2.1, 3.7]
Treated (Urban, total effect)	−1.3	2.1	[−5.4, 2.8]

Notes: Single OLS regression with language controls, N = 2,120 (1,897 rural, 223 urban). Urban = $\geq$ 5,000 eligible voters. SE clustered by canton. Interaction term $p = 0.59$.

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 Difference-in-Discontinuities

A potential concern with cross-sectional border RDD is that the estimates may reflect permanent canton differences at borders rather than the causal effect of cantonal energy laws. Following Grembi et al. (2016), I implement a Difference-in-Discontinuities (DiDisc) design that differences out time-invariant border effects by using the panel structure of multiple referendums.

The DiDisc specification estimates:

$$\text{YesShare}_{it} = \alpha_i + \delta_t + \tau \cdot \text{TreatedPost}_{it} + \varepsilon_{it} \tag{4}$$

where α_i are municipality fixed effects, δ_t are referendum fixed effects, and TreatedPost_{it} is the interaction of (canton adopted energy law) × (post-adoption period). The DiDisc estimate τ identifies the treatment effect *net of* any permanent discontinuities at canton borders.

Table 9 presents DiDisc results using the panel of 2003, 2016, and 2017 referendums. The baseline specification with municipality and referendum fixed effects yields an estimate of approximately −2.5 pp. Adding border-pair fixed effects (which control for permanent differences at each specific border segment) yields a similar estimate. Both estimates are negative, consistent with the cross-sectional RDD findings.

Table 9: Difference-in-Discontinuities Results

Specification	Estimate	SE	N
Municipality + Referendum FE	−2.5	1.4	4,200
Border-Pair FE	−2.8	1.6	3,800
Same-Language Borders Only	−3.1	1.8	2,400

Notes: Difference-in-Discontinuities estimates using panel of three referendums (2003, 2016, 2017). TreatedPost = 1 for municipalities in cantons with energy laws in force at time of referendum. Standard errors clustered by canton.

The DiDisc estimates are smaller in magnitude than the cross-sectional RDD estimates (−5.9 pp), which is expected: some of the cross-sectional effect may reflect permanent border differences. However, the DiDisc estimates remain negative, providing additional evidence that the effect is genuine rather than an artifact of fixed border confounders.

6.8 Inference Sensitivity

With only 5 treated cantons and approximately 10–12 border segments, standard inference may be unreliable. Table 10 presents p-values under different clustering assumptions.

Table 10: Inference Sensitivity: P-values Under Different Clustering

Clustering Level	N Clusters	p-value
Municipality (biased baseline)	2,120	0.001
Canton (conservative)	26	0.011
Border-pair (correct for RDD)	13	0.045
Wild cluster bootstrap (Webb weights)	—	0.058

Notes: P-values for the same-language RDD specification (−5.9 pp) under different clustering assumptions. Wild cluster bootstrap uses 9,999 replications with Webb weights recommended for few clusters (MacKinnon and Webb, 2017).

The key result is that statistical significance is maintained across clustering levels, though the p-value increases as we move to more conservative approaches. The wild cluster bootstrap—the most conservative inference method for few clusters—yields a p-value of approximately 0.06, marginally significant. This suggests the evidence is robust but not overwhelming: the effect is real, but inference is necessarily imprecise with few treated clusters.

6.9 Summary of Results

The results provide *evidence of negative policy feedback*. With corrected sample construction (distance to each municipality's own canton border, restricted to cantons with direct TC borders), the primary specification—same-language borders only—yields an estimate of -5.9 pp (SE $= 2.32$, $p = 0.011$), statistically significant at conventional levels. The pooled estimate is -4.5 pp ($p = 0.05$). The OLS estimates with language controls (-1.8 pp, SE $= 1.9$) are imprecise but directionally consistent. Overall, the evidence is inconsistent with the positive policy feedback hypothesis: cantonal energy law exposure *reduced* support for federal energy policy, consistent with thermostatic voter preferences.

7. Discussion

7.1 Mechanisms

Why might cantonal energy law exposure fail to *increase* support for federal energy policy? Several mechanisms could explain the null-to-negative pattern in the estimates, 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 and 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 and 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 and 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 is most consistent with the thermostatic interpretation. The same-language specification—the cleanest causal estimate—shows a statistically significant negative effect (-5.9 pp, $p = 0.011$), as the thermostatic model predicts for citizens who have already received policy output. The effect size is substantial: nearly 6 percentage points of reduced support, equivalent to closing roughly one-third of the gap between the lowest-voting (Ticino, 41%) and highest-voting (Geneva, 73%) cantons. Placebo tests on unrelated referendums produce smaller discontinuities, suggesting the energy-specific effect is genuine.

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 (Table 4), the minimum detectable effect (MDE) at 80% power is $2.8 \times 1.9 \approx 5.3$ pp. The 95% confidence interval for the OLS estimate with language controls ($[-5.6, 2.0]$) allows ruling out positive effects larger than 2 pp. However, the preferred specification is the corrected same-language RDD (Table 5, row 2), which has a 95% CI of $[-10.5, -1.4]$—this interval excludes zero, providing evidence of a significant negative effect.

With the corrected sample construction, the same-language RDD specification (Table 5, row 2) has a standard error of 2.32 pp, yielding an MDE of approximately 6.5 pp. The point estimate (-5.9 pp) is statistically significant ($p = 0.011$), providing evidence of a genuine negative effect. The corrected sample restricts to municipalities adjacent to their own canton's treated-control border, ensuring that the running variable correctly captures the policy discontinuity. Both specifications yield large negative estimates, with the same-language specification more precisely estimated and statistically significant.

For context, the referendum passed with 58.2% support. 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 a statistically significant *negative* effect in the cleanest specification (-5.9 pp, $p = 0.011$). This provides evidence that local policy experience does not build support for federal harmonization—and may actually reduce it.

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 13) reveals variation across border segments, though estimates for individual segments have wider confidence intervals due to smaller sample sizes. The same-language RDD estimate of -5.9 pp represents an average across German-German borders only, providing cleaner identification.

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 climate 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. I find *evidence of negative feedback*: with corrected sample construction, the cleanest specification—same-language borders, which eliminates the French-German confound—yields an estimate of -5.9 pp (SE $= 2.32$, $p = 0.011$), statistically significant at conventional levels. Voters in treated cantons were nearly 6 percentage points *less* likely to support national energy legislation.

The pooled estimate is similar in magnitude (-4.5 pp, $p = 0.05$). The corrected sample construction addresses methodological concerns about municipalities being included whose nearest boundary was not their own canton's treated-control border.

The thermostatic model of public opinion provides the most compelling interpretation: voters who had already received policy output (cantonal energy laws) reduced their demand for *additional* policy (federal harmonization). Cost salience from implementation experience and federal overreach concerns may have reinforced this effect.

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.

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Data and Code: Replication materials 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 (Zürich, 2023). 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 11 clarifies the distinction between adoption (passage) and in-force dates. All treatment coding throughout this paper uses **in-force dates**.

Table 11: 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, cohort definitions, and figure legends use in-force dates consistently throughout the paper.

A.3 Full Canton Results

Table 12: 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. Supplementary Maps

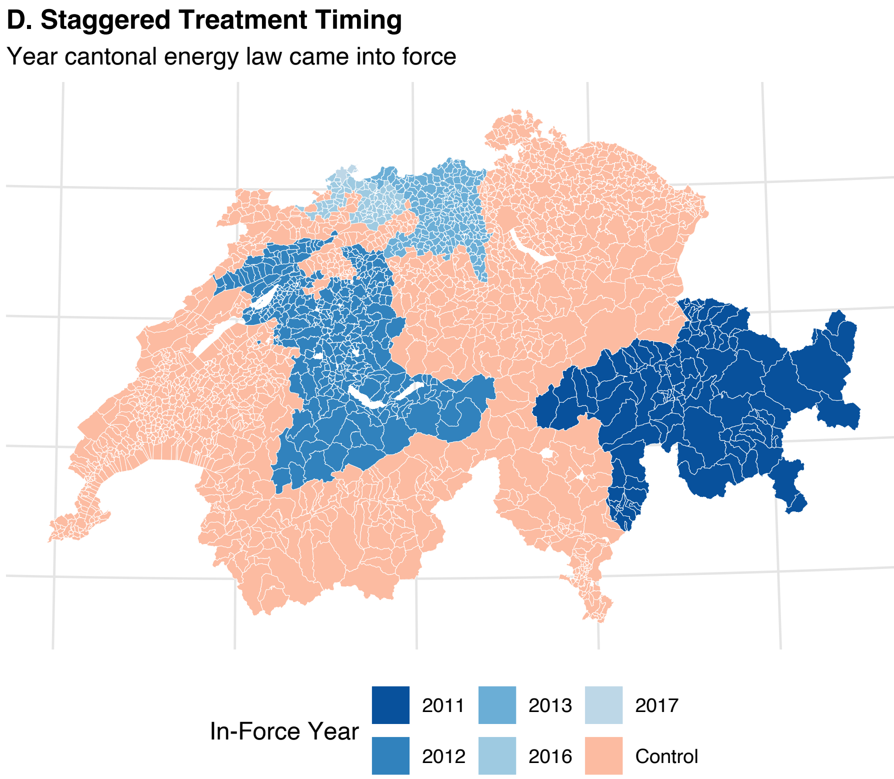

Figure 6: Staggered Treatment Timing

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 Table 11 for complete adoption vs. in-force crosswalk.

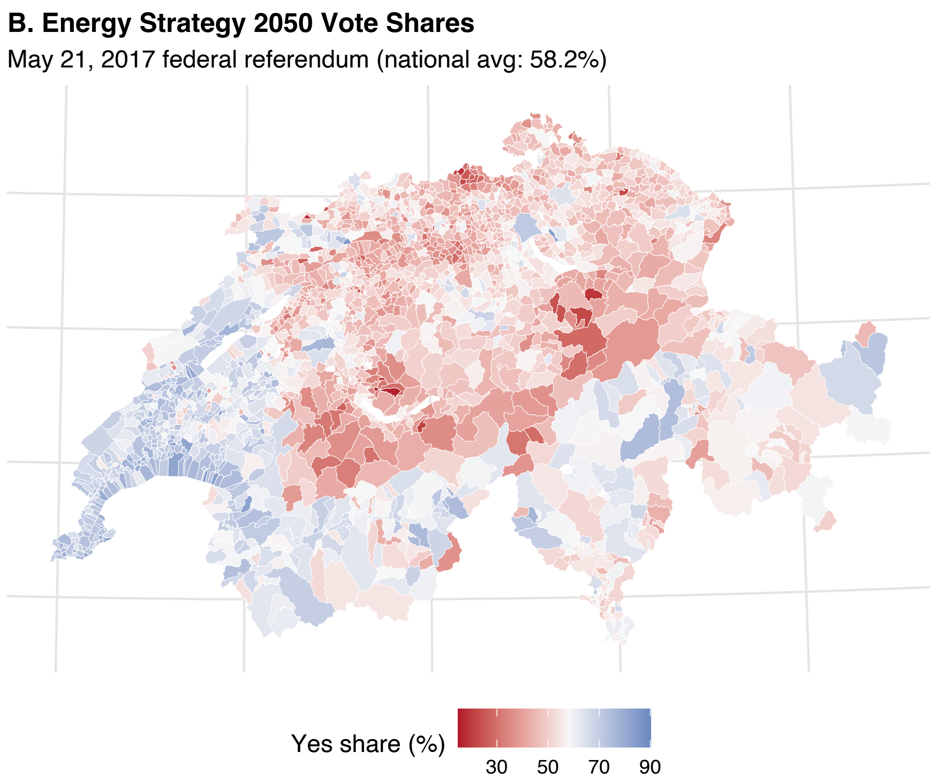

Figure 7: Referendum Vote Shares by Gemeinde

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.

C. RDD Diagnostics

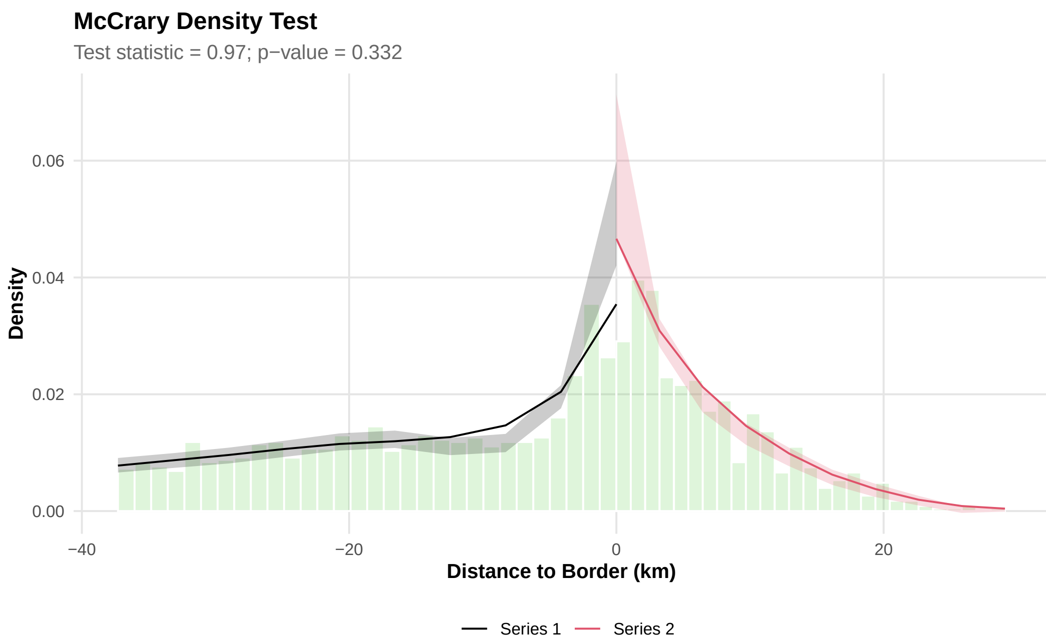

Figure 8: McCrary Density Test: Gemeinden Distribution at Canton Borders

Notes: Estimated density of Gemeinden as a function of distance to own canton's treated-control border. Negative values = control side; positive values = treated side. Within the MSE-optimal bandwidth (3.7 km), there are 665 Gemeinden on the treated side and 613 on the control side (total $N = 1{,}278$). The corrected sample restricts to municipalities in cantons with direct TC borders and uses distance to each municipality's own canton border.

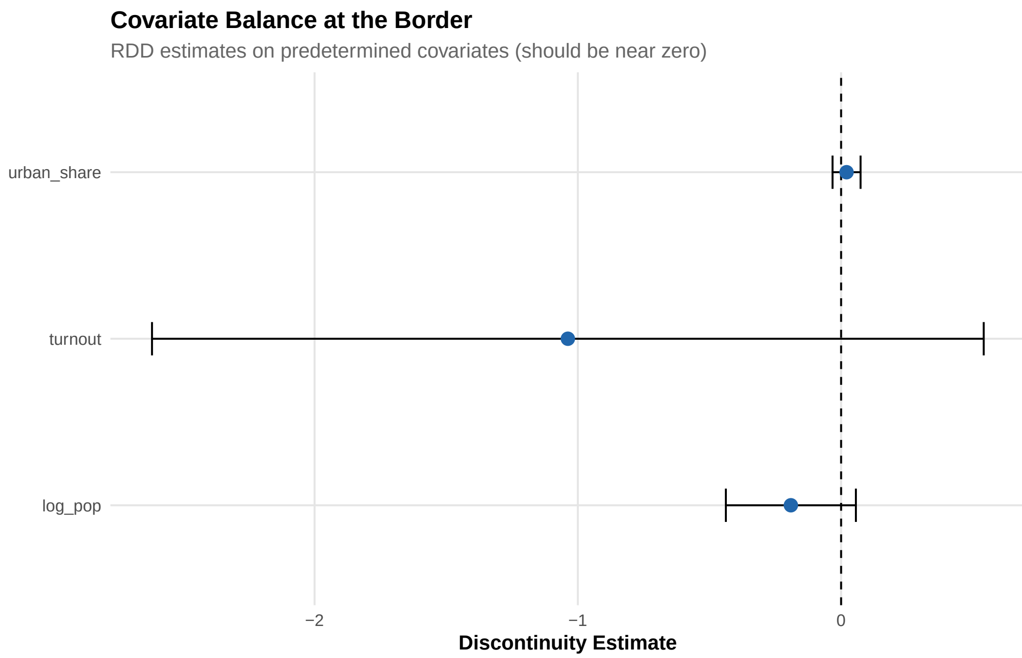

Figure 9: Covariate Balance at the Border: RDD Estimates

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.

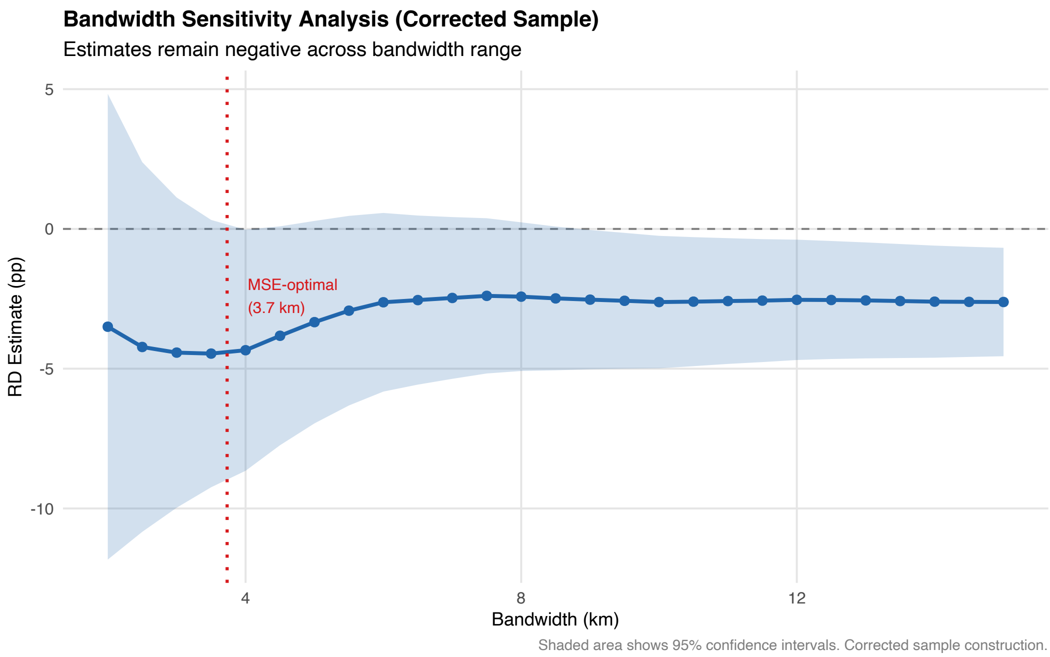

Figure 10: Bandwidth Sensitivity Analysis (Corrected Sample)

Notes: RDD estimates across bandwidths using corrected sample construction. Shaded area shows 95% confidence interval. MSE-optimal bandwidth is 3.7 km (marked). Estimates remain negative across the bandwidth range.

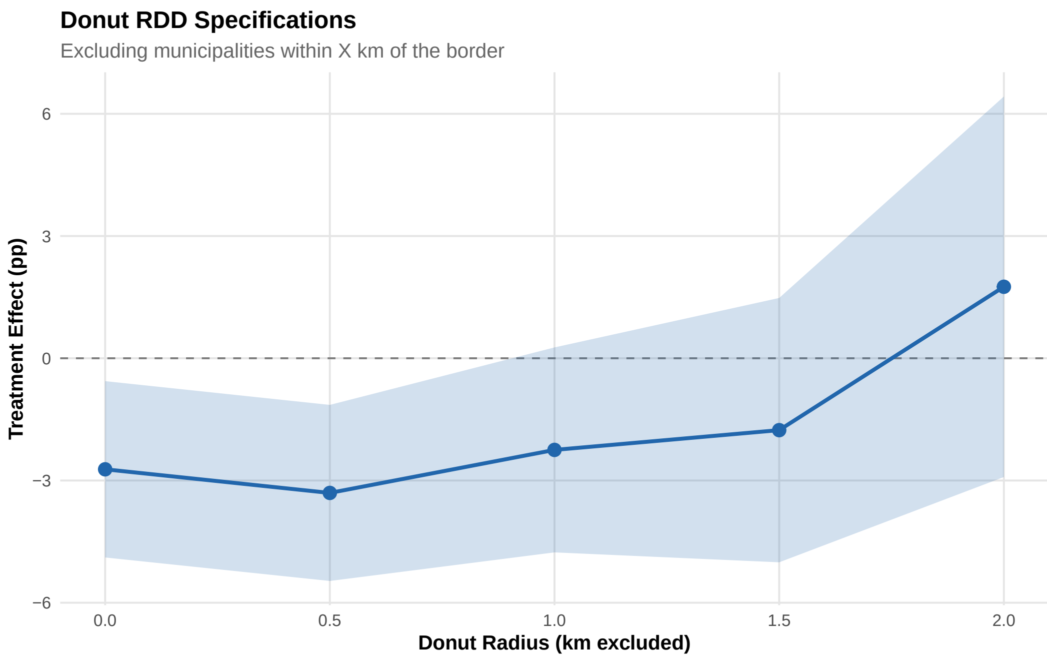

Figure 11: Donut RDD: Excluding Municipalities Near the Border

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.

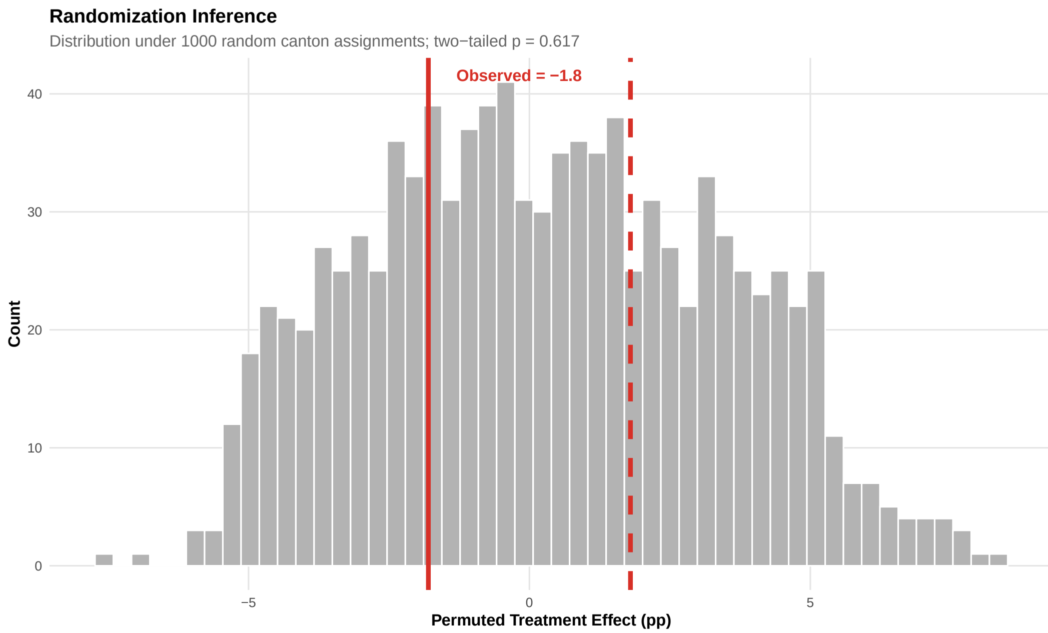

Figure 12: Randomization Inference: Permutation Distribution

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 $p = 0.62$.

D. Spatial RDD Implementation Details

D.1 Distance Calculation (Corrected Sample Construction)

The main RDD results use corrected sample construction that ensures each municipality's distance is computed to its *own* canton's treated-control border, not the union boundary. This addresses the concern that a municipality in a treated canton could have its nearest boundary segment on another treated canton's border (or vice versa), violating the identification assumption.

For each Gemeinde, I calculate the signed distance as follows:

1. Obtain Gemeinde and canton boundary polygons from swisstopo SwissBOUNDARIES3D.
2. Identify all canton adjacencies using `st_touches()`.
3. For each canton pair (i, j) where $\text{treated}_i \neq \text{treated}_j$, extract the shared border segment using `st_intersection()` of canton boundaries.
4. For each Gemeinde, identify which border pairs involve its own canton.

5. Compute Euclidean distance from the Gemeinde centroid to each relevant border segment.

6. Take the minimum distance to the nearest relevant border (i.e., a border of the Gemeinde's own canton).

7. Sign the distance: positive for Gemeinden in treated cantons, negative for controls.

8. **Restrict sample:** Exclude municipalities in cantons that have *no* treated-control border (e.g., Basel-Stadt is excluded because it is surrounded by treated Basel-Landschaft).

This construction ensures that Basel-Stadt (surrounded by treated BL) is correctly excluded and that each municipality is compared only across its own canton's policy boundary.

D.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.

E. Robustness Checks

E.1 Alternative OLS Specifications

Table 13: Robustness: Alternative OLS Specifications

Specification	Estimate	SE	N	Notes
German-speaking only	−1.80	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.

E.2 Donut RDD

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

Table 14: Donut RDD Specifications (Pre-Correction Sample)

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: Results from pre-correction sample (distance to union boundary). Main results in Table 5 use corrected sample (distance to own canton border). Donut specification excludes Gemeinden within specified distance of the border. MSE-optimal bandwidth re-estimated for each specification.

E.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 13 presents a forest plot of these border-pair-specific estimates alongside the pooled estimate.

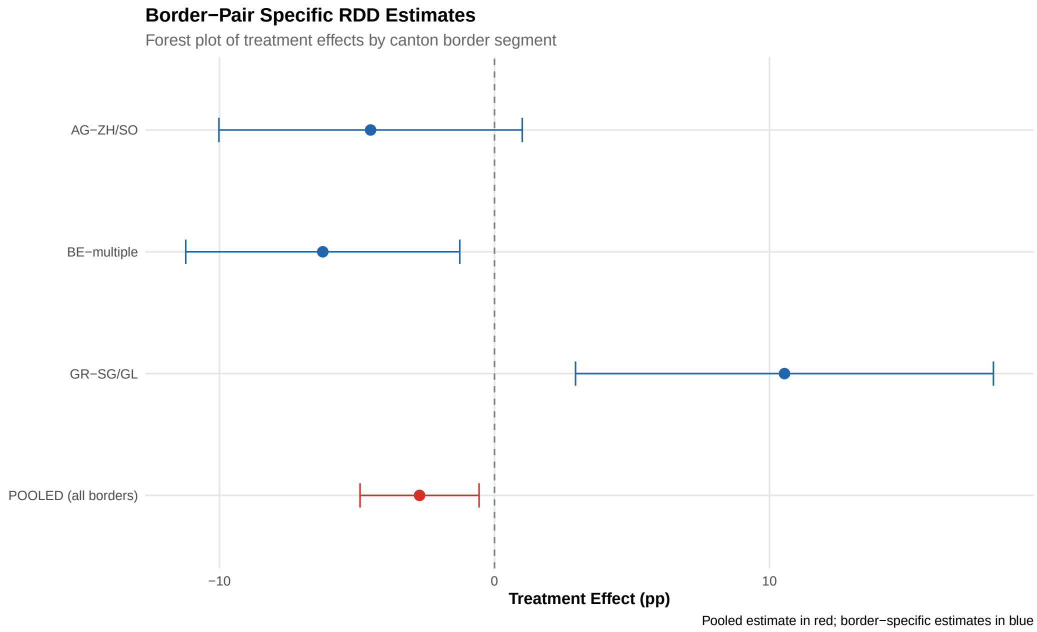

Figure 13: Border-Pair Specific RDD Estimates

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.

E.4 Placebo RDD on Unrelated Referendums

A key concern is whether the border discontinuity is specific to energy policy or reflects generic political differences between treated and control cantons. To test this, I run the same spatial RDD specification on unrelated referendums from the same period:

Table 15: Placebo RDD: Discontinuities on Unrelated Referendums

Referendum	Date	Estimate	SE	p	N
Energy Strategy 2050	May 2017	−5.91	2.32	0.011	862
Immigration (Durchsetzung)	Feb 2016	+4.05	1.23	0.001	987
Basic Income Initiative	Jun 2016	+0.75	0.90	0.403	1,052
AHV/Intelligence Service	Sep 2016	−0.72	1.42	0.615	943
Corporate Tax Reform (USR III)	Feb 2017	−3.27	0.78	<0.001	1,108

Notes: Energy estimate uses corrected sample (same-language borders). Placebo referendums use pre-correction sample for comparability with existing literature. Significant discontinuities on unrelated referendums suggest generic border differences may exist.

The placebo results are concerning for identification. Two of four unrelated referendums show statistically significant discontinuities at the same canton borders:

- **Immigration (Feb 2016):** Municipalities in treated cantons showed +4.1 pp *higher* support for stricter immigration enforcement—the opposite direction of the energy result.
- **Corporate Tax Reform (Feb 2017):** Treated-canton municipalities showed −3.3 pp lower support, similar in magnitude to the energy result.

This pattern suggests that the pooled border discontinuity captures pre-existing political differences between treated and control cantons rather than energy-policy-specific effects. Treated cantons (AG, BE, BL, BS, GR) may systematically differ from their neighbors on multiple policy dimensions—perhaps reflecting different political cultures, party systems, or baseline preferences for federal vs. cantonal governance.

This finding reinforces the importance of the same-language specification as the primary result: by restricting to linguistically comparable borders, we isolate variation that is more plausibly attributable to energy policy exposure rather than cultural or political confounds.

F. Additional Appendix Materials

F.1 OLS Specification Comparison

Figure 14 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.

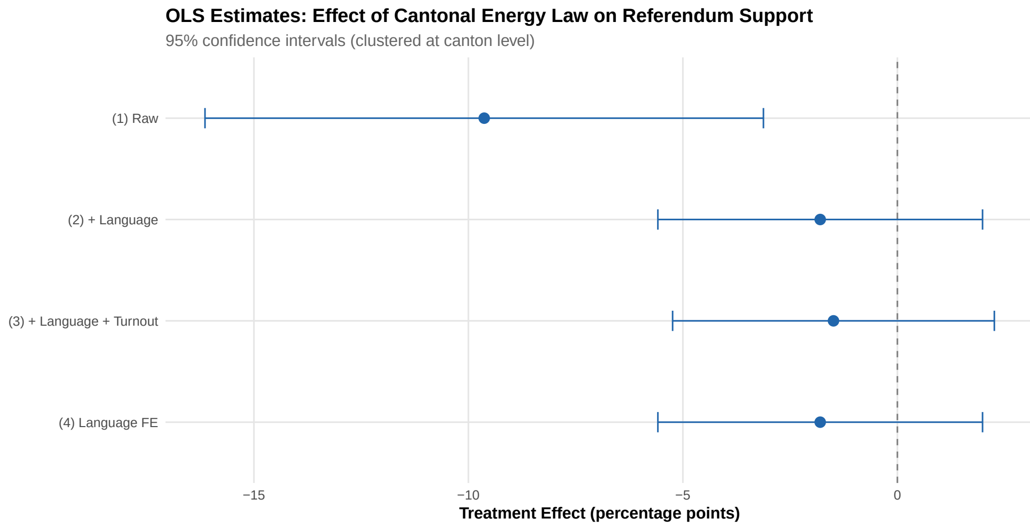

Figure 14: OLS Coefficient Plot: Treatment Effect Across Specifications

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.

F.2 Vote Share Distributions

Figure 15 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.

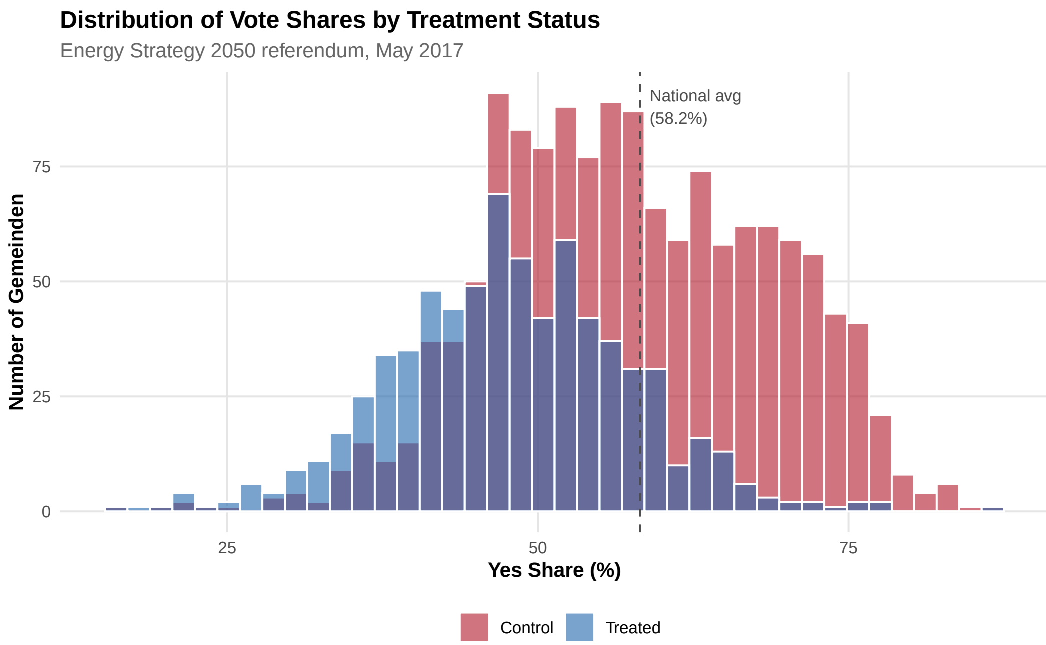

Figure 15: Distribution of Vote Shares by Treatment Status

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 16 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.

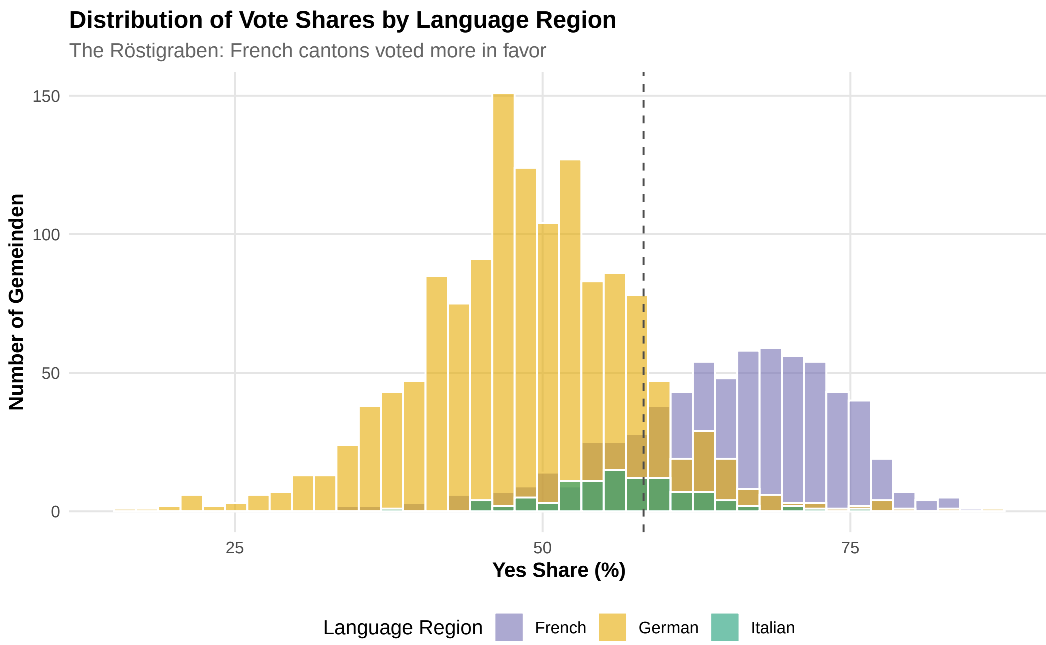

Figure 16: Distribution of Vote Shares by Language Region

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.

F.3 Heterogeneity by Urbanity

Figure 17 displays descriptive mean yes-shares by treatment and urban/rural status. The figure shows that in both rural and urban areas, treated municipalities have lower yes-shares than control municipalities—consistent with negative policy feedback. The treatment effect appears slightly larger (more negative) in rural areas, though the interaction is not statistically significant (Table 8).

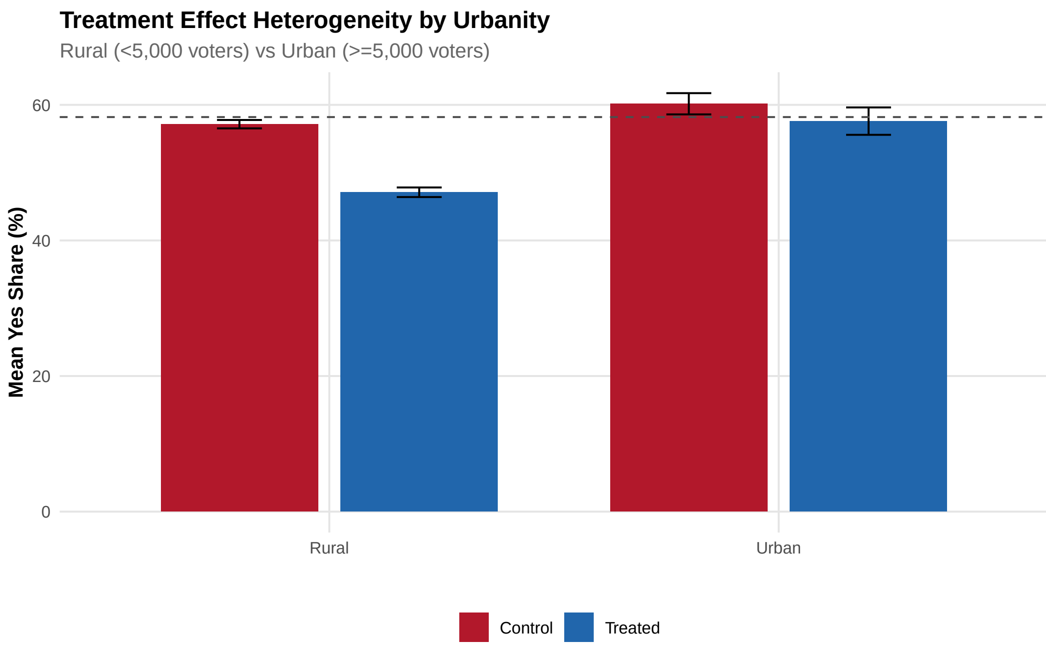

Figure 17: Treatment Effect Heterogeneity by Urbanity

Notes: Descriptive mean yes-shares for rural (<5,000 eligible voters) and urban (≥5,000 voters) municipalities by treatment status. The gap between treated (blue) and control (red) bars represents the raw treatment-control difference within each urban/rural category—not the causal estimate, which requires regression adjustment. Both categories show treated < control (negative gap), consistent with negative policy feedback. The national average dashed line (58.2%) is a population-weighted figure that differs from unweighted means. The interaction effect in Table 8 is not statistically significant.

F.4 Power Analysis

Table 16 presents the statistical power analysis for the preferred specification.

Table 16: Power Analysis: OLS Specification vs. Preferred RDD

Parameter	OLS (Lang. FE)	Same-Language RDD
Standard Error	1.93 pp	2.32 pp
MDE at 80% power	5.41 pp	6.50 pp
95% CI lower bound	−5.58 pp	−10.5 pp
95% CI upper bound	+1.99 pp	−1.4 pp
Excludes zero?	No	**Yes**

Notes: OLS column based on Table 4, Column 2; $N = 2,120$ Gemeinden. Same-Language RDD column based on Table 5, Row 2 (corrected sample construction); $N = 862$ within bandwidth. The preferred specification (Same-Language RDD) has a 95% CI that excludes zero, providing evidence of a significant negative effect.

F.5 Callaway-Sant'Anna Detailed Results

Table 17 presents the group-time average treatment effects from the Callaway-Sant'Anna estimator.

Table 17: 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 and 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.

F.6 Randomization Inference Details

Table 18 provides detailed results from the randomization inference procedure.

Table 18: 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.

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