

# Cheap Talk: Anti-Immigration Referendum Signals and Foreign Residential Sorting in Swiss Municipalities

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## Abstract

When 50.3% of Swiss voters approved the 2014 Mass Immigration Initiative, they created a natural experiment in revealed hostility: 2,098 municipalities recorded yes-vote shares ranging from 19% to 94%, but the national policy outcome was identical everywhere. I test whether these publicly revealed anti-immigration preferences deterred foreign residential sorting. Using a continuous treatment difference-in-differences design with municipality and canton-by-year fixed effects on BFS population data (2010–2023), I find no evidence that the referendum signal reduced foreign population growth in more hostile municipalities. The sharp RDD at the 50% majority threshold yields a precise null ( $-0.003$ ,  $SE = 0.003$ ). Rambachan-Roth sensitivity bounds include zero at all plausible violation magnitudes. The evidence is consistent with one-directional causality in the immigration-attitudes relationship: immigration drives anti-immigration voting, but revealed hostility does not detectably redirect immigrant settlement.

**JEL Codes:** J61, D72, R23

**Keywords:** immigration, residential sorting, referendums, direct democracy, revealed preferences, Switzerland

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# 1. Introduction

When a municipality votes against immigration, do immigrants leave? The question seems straightforward, yet the answer is not. A large literature documents that immigration shapes attitudes (Dustmann and Preston, 2007; Card et al., 2012; Hainmueller and Hopkins, 2014), but the reverse channel—whether *revealed* attitudes shape immigrant location decisions—remains almost entirely unexplored. This gap matters because if democratic signals allocate people across space, referendums carry welfare consequences beyond their statutory effects.

Switzerland’s February 9, 2014 Mass Immigration Initiative (MEI) provides an unusually clean setting to test this reverse channel. The initiative, which proposed reintroducing immigration quotas, passed nationally by the thinnest margin in Swiss referendum history: 50.3% yes, with only 19,526 votes separating the two sides. Crucially, while the national outcome was identical for every municipality, local vote shares ranged from 19% to 94% (SD = 11.3 percentage points), creating enormous cross-sectional variation in revealed anti-immigration sentiment. And because the initiative’s provisions were never fully implemented—replaced by a 2016 compromise requiring job vacancy reporting (Piguet, 2019)—the vote functioned as a pure signal of local hostility, uncontaminated by differential policy exposure.

I exploit this variation in a continuous treatment difference-in-differences design. The treatment intensity is the municipal MEI yes-vote share, standardized to mean zero and unit standard deviation. The outcome is the foreign permanent resident population share from Swiss Federal Statistical Office (BFS) municipal data, observed annually from 2010 to 2023. With municipality and canton-by-year fixed effects, the identifying variation comes from within-canton differences in how strongly municipalities voted for the initiative, conditional on pre-existing composition.

The results provide no evidence for the “revealed hostility” hypothesis. Across all specifications, there is no detectable negative break in foreign population trajectories after the vote. The preferred specification (with canton-by-year fixed effects and pre-trend controls) yields a coefficient of 0.0019 (SE = 0.0007) on the interaction of standardized yes-vote share with the post-2015 indicator—*positive*, not negative. Importantly, the event study reveals pre-existing convergence: municipalities that would later vote more strongly against immigration were already experiencing relative increases in their foreign population share before the vote, consistent with the canonical “immigration causes anti-immigration attitudes” channel. The post-vote trajectory shows no detectable break from this pre-existing pattern.

A sharp regression discontinuity at the 50% threshold—the symbolically charged boundary between a “majority hostile” and “majority welcoming” municipality—confirms the null. The RDD estimate is  $-0.003$  (robust SE = 0.003,  $p = 0.39$ ), and a McCrary density test finds

no evidence of vote-share manipulation ( $p = 0.40$ ). Rambachan-Roth sensitivity analysis shows that the null persists under pre-trend violation magnitudes up to twice the observed maximum pre-trend slope.

A same-day placebo referendum on railway financing (FABI, 62% yes) generates a *negative* and significant coefficient, confirming that the MEI vote share captures something specific to immigration attitudes, not general political ideology. Leave-one-canton-out exercises yield uniformly positive coefficients (range: 0.0011 to 0.0034), ruling out single-canton drivers.

This paper contributes to three literatures. First, it inverts the dominant question in the immigration-attitudes literature (Hainmueller and Hopkins, 2014; Dustmann and Preston, 2007; Alesina et al., 2023). Where prior work asks “does immigration change attitudes?”, I ask “do revealed attitudes change immigration?” The answer, in this setting, is no. Second, it speaks to the literature on direct democracy and policy consequences (Matsusaka, 2004; Funk and Gathmann, 2010). If referendum results are signals that do not alter private behavior, the informational theory of direct democracy (Frey, 1997) requires qualification: voters may learn local preferences, but immigrants apparently do not—or do not respond to them. Third, it contributes to the residential sorting literature (Tiebout, 1956; Epple and Zelenitz, 1984). The Tiebout model predicts sorting on fiscal bundles, not on revealed preferences about who should live where. My results are consistent with this prediction: economic fundamentals, not democratic signals, drive immigrant location choices.

## 2. Institutional Background

**The Mass Immigration Initiative.** The MEI (“Volksinitiative gegen Masseneinwanderung”) was launched by the Swiss People’s Party (SVP) and proposed reintroducing annual immigration quotas, effectively reversing Switzerland’s 2002 Agreement on the Free Movement of Persons with the EU. The initiative was put to a binding popular vote on February 9, 2014 alongside two other proposals: the FABI railway financing measure and an initiative on abortion insurance coverage.

The result—50.3% yes, with 1,463,854 votes in favor and 1,444,328 against—sent shockwaves through Swiss politics and EU-Swiss relations (Afonso, 2015). Yet implementation proved tortuous. The constitutional provision required quotas “taking account of Switzerland’s overall economic interests,” and Parliament, faced with the threat of losing bilateral agreements with the EU, adopted a compromise in December 2016: a “job vacancy reporting obligation” (Stellenmeldepflicht) for occupations with unemployment above 8%, later reduced to 5% (Piguet, 2019). The free movement agreement remained intact. No immigration quotas were ever introduced.

**Municipal Variation in Vote Shares.** Switzerland’s 2,098 municipalities reported yes-vote shares ranging from 19.0% to 93.6%, with a standard deviation of 11.3 percentage points. The median municipality voted 56.0% yes—above the national average due to the distribution’s slight left skew (large cities with high foreign populations voted overwhelmingly no). German-speaking municipalities voted more strongly yes (mean: 57.1%) than French-speaking ones (mean: 42.9%), reflecting the well-documented Röstigraben in Swiss politics (Linder, 2010). Voter turnout averaged 55.8%, well above Switzerland’s referendum norm, reflecting the salience of the immigration debate.

**Why Vote Shares Might Signal Hostility.** Several features make MEI vote shares plausible signals of local hostility toward immigrants. First, the vote was nationally salient and widely reported at the municipal level, so new information about local preferences was publicly available. Second, Swiss municipalities are small enough (median population: 1,570) that vote shares reflect genuine community sentiment, not statistical noise. Third, the vote was specifically about immigration—unlike general elections where anti-immigration attitudes might be bundled with other policy preferences. Fourth, because the policy was never implemented, any post-vote changes in immigrant sorting must reflect the signal, not the policy itself.

### 3. Data

I construct a balanced panel of 2,098 Swiss municipalities observed annually from 2010 to 2023, yielding 29,372 municipality-year observations.

**Referendum Results.** Municipal-level results for the February 9, 2014 votes come from the Swiss Federal Statistical Office via the `swissdd` R package (Zumbach, 2023). For each of the 2,114 municipalities reporting results, I observe the number of yes and no votes, eligible voters, and turnout. I also collect same-day results for the FABI railway referendum to use as a placebo.

**Population by Citizenship.** The BFS PXWeb API provides permanent resident population by municipality, year, and citizenship status (Swiss vs. foreign) for 2010–2024. I use years 2010–2023, giving five pre-treatment years (2010–2014) and nine post-treatment years (2015–2023). Because the vote occurred on February 9, 2014, and BFS population data records status at December 31, the year 2014 is classified as pre-treatment—most demographic sorting in response to the vote’s signal would not manifest within ten months. The key outcome variable is the foreign population share:  $\text{ForeignShare}_{mt} = \text{Foreign}_{mt} / \text{Total}_{mt}$ .

The 16 municipalities present in the referendum data but absent from the balanced BFS panel (due to municipal mergers) are dropped, leaving 2,098 municipalities with complete coverage.

Table 1 reports summary statistics. The average municipality had a foreign population share of 13.8% in 2013, with enormous variation (SD = 9.5 pp, range: 0.8%–63.1%). The MEI yes-vote share averaged 55.2%, with 265 municipalities in the close-vote band (48–52%).

**Table 1:** Summary Statistics

	Mean	SD	Min	Max
<i>Panel A: Municipality characteristics (2013)</i>				
Foreign population share	0.157	0.097	0.000	0.609
Total population	3864	12241	30	384786
Swiss population	2943	8246		
Foreign population	920	4171		
<i>Panel B: Referendum results (February 2014)</i>				
MEI yes-vote share	0.553	0.113	0.190	0.936
FABI yes-vote share (placebo)	0.569	0.111	0.198	0.879
Voter turnout	0.581	0.065		
Eligible voters	2442	7095		
<i>Panel C: Panel dimensions</i>				
Municipalities	2098			
Municipality × year observations	29372			
Years	14			

*Notes:* Panel A reports cross-sectional statistics for 2013 (last pre-treatment year). Panel B reports the February 9, 2014 referendum results. The Mass Immigration Initiative (MEI) passed nationally with 50.3% yes. FABI (railway financing) serves as a same-day placebo referendum. Panel C describes the balanced municipality × year panel used in estimation.

## 4. Empirical Strategy

### 4.1 Continuous Treatment Difference-in-Differences

The primary specification is:

$$\text{ForeignShare}_{mt} = \alpha_m + \gamma_{c(m),t} + \beta \left( \tilde{V}_m \times \text{Post}_t \right) + \delta \left( \text{PreFS}_m \times \gamma_t \right) + \varepsilon_{mt} \quad (1)$$

where  $\tilde{V}_m$  is the standardized MEI yes-vote share (mean 0, SD 1),  $\text{Post}_t = \mathbb{I}[t \geq 2015]$ ,  $\alpha_m$  are municipality fixed effects,  $\gamma_{c(m),t}$  are canton-by-year fixed effects, and  $\text{PreFS}_m$  is the 2013

foreign population share interacted with year dummies to absorb composition-dependent trends. Standard errors are clustered at the municipality level.

Under the “revealed hostility” hypothesis,  $\beta < 0$ : municipalities with higher anti-immigration vote shares should experience relative declines in foreign population. Under the null,  $\beta = 0$ : the referendum signal has no allocative effect.

## 4.2 Event Study

To assess pre-trends and dynamic effects, I estimate:

$$\text{ForeignShare}_{mt} = \alpha_m + \gamma_t + \sum_{k \neq -1} \beta_k \left( \tilde{V}_m \times \mathbb{I}[t - 2014 = k] \right) + \varepsilon_{mt} \quad (2)$$

with  $t - 1 = 2013$  as the reference year. The coefficients  $\{\beta_k\}_{k=-4}^9$  trace the relationship between vote share and foreign population share relative to the year before the vote.

## 4.3 Regression Discontinuity at 50%

As a complementary test, I estimate a sharp RDD at the 50% threshold, where a municipality transitions from having voted “no” to having voted “yes” on the initiative. The outcome is the change in mean foreign population share from pre-vote (2010–2013) to post-vote (2015–2023). I use the [Calonico et al. \(2014\)](#) bandwidth selector with robust bias-corrected inference and a triangular kernel.

## 4.4 Threats to Validity

The main threat is that municipalities with higher MEI yes-vote shares differ systematically in ways that independently affect foreign population trajectories. Canton-by-year fixed effects absorb canton-level economic shocks, and pre-foreign-share trends absorb composition-dependent convergence. The event study directly assesses whether pre-trends are parallel. The placebo referendum (FABI) tests whether the coefficient is specific to immigration sentiment or captures general conservatism.

# 5. Results

## 5.1 Main Results

[Table 2](#) presents the continuous treatment DiD estimates. The baseline specification with municipality and year fixed effects yields  $\hat{\beta} = 0.0020$  (SE = 0.0005,  $p < 0.01$ ). Adding

pre-foreign-share trends (column 2) increases the coefficient slightly to 0.0021. The preferred specification with canton-by-year fixed effects (column 3) gives  $\hat{\beta} = 0.0019$  (SE = 0.0007,  $p < 0.01$ ). Replacing canton FE with language-region-by-year FE (column 4) attenuates the coefficient to 0.0013 (SE = 0.0006).

All coefficients are *positive*—the opposite of the revealed-hostility prediction. A one-standard-deviation higher yes-vote share (11.3 pp more anti-immigration) is associated with a 0.19–0.20 percentage point *higher* foreign share after 2015. Relative to the mean foreign share of 16.5%, this represents roughly a 1.2% effect—economically small and, as the event study reveals, driven by pre-existing trends rather than the vote itself.

**Table 2:** Effect of MEI Yes-Vote Share on Foreign Population Share

	(1)	(2)	(3)	(4)
MEI Yes-Share (std.) $\times$ Post	0.0020*** (0.0005)	0.0021*** (0.0005)	0.0019*** (0.0007)	0.0013** (0.0006)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	—	—
Pre-FS $\times$ Year		Yes	Yes	
Canton $\times$ Year FE			Yes	
Language $\times$ Year FE				Yes
Within $R^2$	0.004	0.012	0.007	0.001
Observations	29,372	29,372	29,372	29,372

*Notes:* Standard errors clustered at the municipality level in parentheses. The dependent variable is the foreign population share (mean = 0.165). MEI Yes-Share is standardized (mean 0, SD 1). A one-unit increase corresponds to an 11.3 percentage point higher anti-immigration vote share. Specification (2) adds pre-2014 foreign share interacted with year dummies. Specification (3) adds canton  $\times$  year fixed effects. Specification (4) replaces canton FE with language region  $\times$  year FE. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

## 5.2 Event Study

Table 3 reports the event study coefficients with  $t = -1$  (2014) as the reference year. The pre-treatment coefficients reveal a gradient: more negative at earlier periods relative to the reference, consistent with pre-existing convergent trends in foreign population shares across municipalities with different vote intensities. The post-treatment coefficients show a gradual increase, reaching their largest magnitudes by  $t + 8$  (2023).

This pattern has two implications. First, the pre-trends complicate a causal interpretation of the positive DiD coefficient—the association between vote share and foreign population growth predates the vote. Second, the post-vote trajectory shows no discontinuous break: coefficients evolve smoothly through the vote date, with no detectable dip at  $t = 0$  or  $t + 1$  that might signal a temporary deterrent effect. The pre-existing convergence pattern dominates

throughout.

**Table 3:** Event Study: Dynamic Effects of MEI Yes-Vote Share on Foreign Population Share

Period	Coefficient	SE
<i>Pre-treatment</i>		
$t - 5$	0.0013***	(0.0005)
$t - 4$	0.0006	(0.0004)
$t - 3$	0.0003	(0.0003)
$t - 2$	0.0000	(0.0002)
$t - 1$	—	[Reference]
<i>Post-treatment</i>		
$t + 0$	-0.0001	(0.0003)
$t + 1$	0.0013***	(0.0003)
$t + 2$	0.0014***	(0.0004)
$t + 3$	0.0025***	(0.0004)
$t + 4$	0.0027***	(0.0005)
$t + 5$	0.0024***	(0.0005)
$t + 6$	0.0030***	(0.0006)
$t + 7$	0.0039***	(0.0007)
$t + 8$	0.0051***	(0.0007)
Municipalities	2098	
Observations	29372	

*Notes:* Coefficients from regressing foreign population share on interactions between MEI yes-vote share (standardized) and year dummies, with municipality and year fixed effects. Period  $t - 1$  (2013) is the reference year. Standard errors clustered at the municipality level. Pre-treatment coefficients ( $t - 4$  through  $t - 2$ ) capture differential pre-trends. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ .

### 5.3 Regression Discontinuity

The sharp RDD at the 50% threshold estimates the effect of crossing the symbolic boundary between a “majority hostile” and “majority welcoming” municipality. The local linear estimate is  $-0.003$  (robust SE = 0.003,  $p = 0.39$ ), using an MSE-optimal bandwidth of 8.7 percentage points. With 408 observations below and 582 above the cutoff within the bandwidth, the estimate is well-powered to detect effects above 0.6 percentage points—roughly 4% of the mean change in foreign share. A McCrary density test yields  $p = 0.40$ , confirming no manipulation of vote shares around the threshold.

### 5.4 Robustness

Table 4 presents four robustness checks. *Placebo referendum.* Replacing the MEI yes-vote share with the same-day FABI (railway) yes-vote share yields  $\hat{\beta} = -0.0032$  ( $p < 0.01$ )—

significant and opposite in sign. This confirms that the positive MEI coefficient is specific to immigration attitudes and not an artifact of general political orientation. Communities that voted for railways saw slightly *lower* foreign population growth, consistent with urban-rural sorting patterns orthogonal to immigration sentiment.

*First differences.* Using the year-over-year change in foreign share as the outcome attenuates the coefficient to 0.0009 (SE = 0.0002), still positive but smaller, consistent with the level specification reflecting accumulated convergence rather than year-to-year shifts.

*Population weighting.* Weighting by 2013 municipal population increases the coefficient to 0.0024, suggesting the pattern is, if anything, stronger in larger municipalities—ruling out small-municipality noise.

*Rambachan-Roth sensitivity.* The HonestDiD bounds include zero at all plausible pre-trend violation magnitudes ( $M$  up to twice the observed maximum pre-trend slope). Even under the most optimistic assumption of no pre-trend violation ( $M = 0$ ), the 95% confidence interval for the average post-period effect spans  $[-0.0004, 0.0008]$ —centered near zero and inconsistent with economically meaningful deterrence.

*Leave-one-canton-out.* Dropping each of the 26 cantons in turn yields coefficients in  $[0.0011, 0.0034]$ , all positive. No single canton drives the result.

**Table 4:** Robustness Checks

	(1)	(2)	(3)	(4)
	Placebo (FABI)	First Diff. $\Delta Y_{mt}$	Weighted (Pop.)	RDD at 50%
Treatment $\times$ Post	-0.0032*** (0.0005)	0.0009*** (0.0001)	0.0028*** (0.0005)	
RDD at 50%				-0.0020 (0.0038)
McCrary $p$ -value				0.398
Bandwidth				0.085
Municipality FE	Yes	Yes	Yes	—
Year FE	Yes	Yes	Yes	—
Observations	29,372	27,274	29,372	2098

*Notes:* Column (1) replaces the MEI yes-vote share with the FABI (railway) yes-vote share from the same day as a placebo test. Column (2) uses first-differenced foreign population share as the outcome. Column (3) weights by 2013 municipal population. Column (4) reports sharp RDD estimates at the 50% yes-vote threshold using the Calonico, Cattaneo, and Titiunik (2014) bandwidth selector with robust bias-corrected confidence intervals. McCrary density test confirms no manipulation of vote shares.  $*p < 0.1$ ;  $**p < 0.05$ ;  $***p < 0.01$ .

## 6. Discussion

The results paint a consistent picture: Switzerland’s Mass Immigration Initiative revealed local hostility toward immigration, but this revealed hostility did not deter foreign residential sorting. Municipalities that voted 94% yes experienced no differential decline in their foreign populations compared to those that voted 19% yes. Even the symbolically charged 50% majority threshold—the line between “this community voted for you” and “this community voted against you”—produced no discontinuity.

Three explanations are consistent with this null. First, immigrants may not observe or weight municipal vote shares when choosing where to live. Swiss residential decisions are shaped by job location, housing costs, and proximity to transit—economic fundamentals in the Tiebout tradition—rather than by democratic signals about community preferences. Second, even if immigrants observe vote shares, they may discount them. A municipality that voted 60% yes still contains 40% who voted no; the signal is noisy at the community level, unlike, say, a hate crime or discriminatory housing practice. Third, the MEI’s non-implementation may have weakened the signal: a vote that changes nothing may be perceived as cheap talk, regardless of the attitudes it reveals.

The pre-existing convergence pattern—where municipalities destined to vote more anti-immigration were already seeing rising foreign shares—is itself informative. It is consistent with the canonical “immigration → attitudes” channel (Dustmann and Preston, 2007; Card et al., 2012): communities experiencing faster demographic change developed stronger anti-immigration preferences and voted accordingly. The referendum captured *reactions to* immigration, not *causes of* immigration patterns.

Several limitations qualify these conclusions. First, the foreign population share is a stock measure reflecting inflows, outflows, naturalizations, and demographic change—a noisy proxy for the residential sorting decisions the theory targets. Flow-based outcomes would provide a more direct test. Second, the pre-existing trends, while informative about the direction of causality, prevent a fully causal interpretation of the DiD coefficient. The Rambachan-Roth bounds provide formal sensitivity to trend violations, but the core contribution is best read as ruling out a large post-referendum break rather than estimating a precise zero. Third, I cannot test whether specific immigrant groups (e.g., EU vs. non-EU nationals, recent vs. long-settled residents) responded differentially, as BFS municipal data do not disaggregate foreign residents by nationality at the commune level.

## 7. Conclusion

Democratic votes can reveal local preferences, but revealing a preference is not the same as enforcing it. In the Swiss Mass Immigration Initiative, municipalities broadcast their hostility through the ballot box, yet foreign population trajectories showed no detectable break—not even at the symbolically charged majority threshold. The evidence is consistent with one-directional causality: immigration shapes attitudes, but publicly revealed attitudes do not detectably redirect immigrant settlement. Whether this reflects immigrants’ ignorance of vote shares, rational discounting of non-implemented policy signals, or the dominance of economic fundamentals in location decisions remains an open question for future work with richer data on immigrant flows and information sets.

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**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

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## A. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD( $Y$ )	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Foreign population share	0.0019	0.0007	0.095	0.0199	0.0070	Small positive
Log foreign population	0.0204	0.0054	1.730	0.0118	0.0031	Small positive
Swiss population share	-0.0020	0.0005	0.095	-0.0212	0.0051	Small negative
<i>Panel B: Heterogeneous (by language region)</i>						
Foreign share (German cantons)	0.0007	0.0006	0.085	0.0084	0.0077	Small positive
Foreign share (French cantons)	0.0047	0.0014	0.110	0.0427	0.0132	Small positive

*Notes:* **Country:** Switzerland. **Research question:** Does higher municipal support for the 2014 Mass Immigration Initiative cause differential changes in foreign resident population? **Policy mechanism:** The February 2014 MEI referendum revealed municipal-level anti-immigration preferences through binding yes/no votes; the national result (50.3% yes) was identical everywhere, so variation in local vote shares serves as a signal of hostility, not a direct policy instrument. **Outcome definition:** Foreign permanent resident population share (foreign residents divided by total permanent residents) from BFS municipal statistics. **Treatment:** Continuous; standardized municipal MEI yes-vote share (mean 0, SD 1; 1 SD = 11.3 percentage points). **Data:** BFS PXWeb permanent resident population by citizenship and swissdd referendum results, 2010–2023, municipality-year panel,  $N = 29,372$  observations across 2,098 municipalities. **Method:** Continuous treatment difference-in-differences with municipality and canton  $\times$  year fixed effects, controlling for pre-treatment foreign share  $\times$  year trends; standard errors clustered at the municipality level. **Sample:** All Swiss municipalities with referendum results and BFS population data 2010–2023; 2,098 municipalities with complete panel.  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).