The Consolidation Null: Forced Intercommunal Mergers and Far-Right Voting in France

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Abstract

A leading narrative in European political economy holds that institutional consolidation — removing governance from local hands — fuels populist backlash. I test this claim using the largest forced municipal-structure reform in recent European history: France's 2015 Loi NOTRe, which eliminated 39% of intercommunal bodies (EPCIs) on January 1, 2017, reassigning approximately 17,000 communes to larger, more distant structures. Exploiting the universal treatment date with commune and election fixed effects, I find a precisely estimated null effect on Rassemblement National vote share across four presidential elections. The point estimate is 0.20 percentage points (SE = 0.17, 96 département clusters), with clean pre-trends and stability across specifications. The null survives continuous treatment-intensity measures, département×year fixed effects, and balanced-panel restrictions. Forced intercommunal consolidation did not measurably increase far-right support.

JEL Codes: D72, H70, H77
Keywords: intercommunal mergers, populism, far-right voting, Loi NOTRe, difference-in-differences

1. Introduction

On January 1, 2017, roughly 17,000 French communes woke up inside new intercommunal structures they had not chosen. The Loi NOTRe — passed in August 2015 and implemented through prefectoral decrees — had raised the minimum population threshold for *intercommunalités* (EPCIs) from 5,000 to 15,000, forcing the consolidation of 2,064 EPCIs into 1,268. Competences over water, waste, transport planning, and economic development were transferred from familiar commune-level institutions to larger, more distant bodies. The reform was unprecedented in scale: 39% of France's intercommunal structures vanished overnight.

This paper asks whether this forced institutional consolidation fueled support for the Rassemblement National (RN), France's far-right populist party. The question matters because a growing literature links governance distance to populist backlash. Rodrik (2021) argues that the democratic deficit in supranational governance breeds anti-globalization populism. Dustmann et al. (2019) document how refugee allocation erodes trust in local institutions. Autor et al. (2020) show that economic shocks feed support for ideologically extreme candidates. If even domestic institutional consolidation — moving decision-making from the commune to the EPCI — can activate the same backlash channel, the implications for European governance reform are profound.

The identification strategy exploits the sharp, universal treatment date. On January 1, 2017, approximately 17,000 communes were forced into new EPCI structures, while roughly 18,000 remained in their pre-reform EPCI. I construct a commune×election panel spanning four presidential elections (2007, 2012, 2017, 2022) using official commune-level results from data.gouv.fr, merged with historical EPCI composition tables from the DGCL. The treatment variable is a binary indicator for whether a commune's EPCI changed, with a continuous alternative measuring merger intensity as the log ratio of post- to pre-reform EPCI population.

The result is a precisely estimated null. The binary difference-in-differences estimate is 0.20 percentage points (SE = 0.17), less than 3% of the pre-reform FN mean of 17%. With département×year fixed effects absorbing regional trends, the estimate shrinks to -0.01 (SE = 0.12). An event study confirms clean pre-trends: the 2007 coefficient is -0.02 (SE = 0.14), trivially close to zero. The post-reform coefficients are 2017: -0.09 (SE = 0.30) and 2022: $+0.48$ (SE = 0.30); neither is statistically significant at the 5% level. The 95% confidence interval for the pooled estimate rules out effects larger than 0.54 percentage points, or roughly 3% of the pre-reform mean. The null is stable across EPCI-clustered standard errors (SE = 0.14), balanced-panel restrictions, and the continuous intensity measure.

This null is informative, not empty. The design has features that should produce a

detectable effect if one exists: 17,000 treated communes provide high statistical power; 96 département clusters support well-calibrated inference; two pre-treatment elections confirm parallel trends; and the treatment — forced reassignment to a new EPCI — is a real change in governance structure, not a marginal policy tweak.

The paper contributes to three literatures. First, it provides the first causal estimate of forced intercommunal merger effects on populist voting. Prior work on municipal mergers in Scandinavia focuses on turnout (Lassen and Serritzlew, 2011; Koch and Rochat, 2017; Lapointe et al., 2023), not populist party support. Second, it contributes to the political economy of governance structure, where the "democratic distance" hypothesis (Dahl and Tufte, 1973) predicts that larger jurisdictions reduce political engagement and trust. The null here suggests this channel — if it operates at all — is too weak to move far-right vote share in the French context. Third, it adds to the growing body of well-identified null results in political economy (Cantoni et al., 2019; Ferrara and Testa, 2024), where the absence of an effect constrains theory at least as much as a positive finding.

Two features of the setting help interpret the null. First, the Loi NOTRe reform was administrative, not fiscal. Communes retained their tax-setting power, their mayors, and their identities. Citizens may not have perceived the EPCI reassignment as a meaningful loss of democratic control. Second, the reform was implemented by prefects following a national mandate, not by local politicians. The absence of a visible local "villain" may have muted the potential for backlash.

The paper proceeds as follows. Section 2 describes the institutional setting and the Loi NOTRe reform. Section 3 details the data construction. Section 4 presents the empirical strategy. Section 5 reports results, and Section 6 discusses implications.

2. Institutional Background

French intercommunal structures. France has approximately 35,000 communes — far more local governments per capita than any comparable European country. To achieve economies of scale in public service delivery, French law provides for *intercommunalités* or EPCIs (*Établissements Publics de Coopération Intercommunale*), which are groupings of communes that jointly manage specific competences. EPCIs are governed by councils composed of municipal delegates, not directly elected, creating what scholars term an "indirect democratic" layer (Kerrouche, 2008).

Before 2017, the intercommunal map of France had evolved organically. A series of laws — the Joxe Law (1992), the Chevènement Law (1999), and the RCT Law (2010) — encouraged voluntary intercommunal cooperation, but the resulting map was fragmented. Many EPCIs

had fewer than 5,000 inhabitants and lacked the critical mass for effective service delivery.

The Loi NOTRe. The Loi portant nouvelle organisation territoriale de la République (Loi NOTRe), enacted August 7, 2015, raised the minimum EPCI population threshold from 5,000 to 15,000, with derogations for mountain zones (down to 5,000) and low-density areas. Prefects were tasked with preparing revised Schémas Départementaux de Coopération Intercommunale (SDCI) by March 2016, with implementation by January 1, 2017.

The reform was massive. The number of EPCIs with own-source taxation (*à fiscalité propre*) fell from 2,064 on January 1, 2016 to 1,268 on January 1, 2017 — a 39% reduction. Approximately 17,000 communes were assigned to a different EPCI. Competences transferred to the new, larger EPCIs included water management, waste collection, transport planning, and economic development zones.

Crucially, the reform was *not* a municipal merger. Communes retained their legal identity, their mayors, their budgets, and their tax-setting authority. The reform changed the *intercommunal* layer, not the communal layer. This distinction matters for interpretation: the treatment is a shift in which joint-governance body a commune belongs to, not a loss of communal sovereignty. It also distinguishes this setting from the Scandinavian municipal mergers studied by Lassen and Serritzlew (2011) and Lapointe et al. (2023), where communes themselves were dissolved.

Treatment variation. The reform created a natural control group: communes whose EPCI SIREN code was identical in 2016 and 2017. Approximately 18,000 communes remained in their pre-reform EPCI because their EPCI already met the 15,000 threshold or received a derogation. A measurement concern is that some "control" communes belong to EPCIs that absorbed smaller neighbors; these communes experienced a change in EPCI composition even though their EPCI code did not change. This would bias estimates toward zero if absorbing EPCIs also generates backlash. The preferred specification with département×year fixed effects addresses this by absorbing regional-level trends common to absorbing and non-absorbing EPCIs within a département. Among treated communes, treatment intensity varied: some were absorbed into a neighboring EPCI of similar size (modest change), while others were folded into much larger metropolitan structures (large change).

3. Data

EPCI composition. I construct the commune-to-EPCI mapping using the DGCL's annual composition tables for 2016 and 2017, archived at data.cquest.org.[1] For each commune, I observe the EPCI SIREN code in both years. The treatment indicator equals one if the codes differ. The continuous treatment intensity is $\log(\text{EPCI pop}_{2017}/\text{EPCI pop}_{2016})$, measuring the size change experienced by each commune. For untreated communes (no EPCI change), intensity is zero.

Election results. I use commune-level results from the first round of French presidential elections in 2007, 2012, 2017, and 2022, obtained from data.gouv.fr.[2] The primary outcome is the FN/RN vote share — Le Pen's votes as a percentage of expressed (valid) votes. I also construct turnout as expressed votes divided by registered voters. The commune code is standardized as the concatenation of the two-digit département code and three-digit commune code.

Panel construction. I match election results to the EPCI composition data using the commune code. The resulting panel contains approximately 35,000 communes observed across four elections, yielding roughly 139,000 commune×election observations. Table 1 presents summary statistics.

Table 1: Summary Statistics

	Mean FN/RN Share (%)	SD FN/RN Share	Mean Turnout (%)	SD Turnout	Communes	Obs
Treated, Pre-reform	16.91	7.76	84.59	4.45	17,099	34,184
Control, Pre-reform	17.07	7.68	84.51	4.53	17,793	35,579
Treated, Post-reform	30.33	7.96	79.44	5.29	17,281	34,135
Control, Post-reform	30.29	8.16	79.31	5.34	17,972	35,539
Full Sample	23.65	10.32	81.96	5.56	35,253	139,437

Notes: FN/RN vote share is the percentage of expressed votes for the Front National (2007–2012) or Rassemblement National (2017–2022) in the first round of presidential elections. Treated communes are those whose EPCI changed on January 1, 2017 due to the Loi NOTRe reform. Control communes are those whose EPCI was unchanged.

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[1] Original source: Direction Générale des Collectivités Locales, composition communale des EPCI à fiscalité propre. The 2016 file identifies 2,064 unique SIREN-coded EPCIs; the 2017 file identifies 1,268.

[2] Ministère de l'Intérieur, résultats des élections. The 2002 presidential election lacks commune-level data on the platform and is therefore excluded.

Pre-reform FN/RN vote share is nearly identical across treated and control communes (16.9% vs. 17.1%), consistent with the parallel trends assumption. Both groups experienced the same dramatic rise to approximately 30% by 2017–2022, driven by the broader secular trend toward the RN.

4. Empirical Strategy

4.1 Identification

I estimate a difference-in-differences model exploiting the sharp January 1, 2017 treatment date:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot (\text{Treated}_c \times \text{Post}_t) + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the FN/RN vote share in commune c at election t, α_c and γ_t are commune and election fixed effects, Treated_c indicates whether commune c's EPCI changed, and Post_t indicates post-2017 elections. Standard errors are clustered at the département level (96 clusters), reflecting both the administrative level at which prefects prepared the SDCI and the spatial correlation of political shocks within départements (Bertrand et al., 2004). Results are robust to EPCI-level clustering (1,268 clusters, SE = 0.14).

The identifying assumption is that, absent the reform, FN/RN vote share would have evolved in parallel across treated and control communes. I test this with an event study replacing the single treatment indicator with election-specific interactions (omitting 2012 as the reference):

$$Y_{ct} = \alpha_c + \gamma_t + \sum_{s \neq 2012} \delta_s \cdot (\text{Treated}_c \times \mathbf{1}[t = s]) + \varepsilon_{ct} \tag{2}$$

A natural concern is that EPCI assignment correlates with pre-reform commune characteristics that also predict RN trends. I address this in three ways. First, the near-zero 2007 coefficient in the event study directly tests for differential pre-trends. Second, I augment Equation 1 with département×election fixed effects, which absorb all regional-level confounds including local economic shocks and political dynamics. Third, I restrict to a balanced panel of communes present in all four elections to eliminate compositional effects.

Since all treated communes received treatment simultaneously, the Callaway–Sant'Anna estimator collapses to the standard two-way fixed effects estimator. There is no staggered adoption to correct for.

4.2 Threats to Validity

Selection into treatment. EPCI mergers were not random: smaller, rural EPCIs below the 15,000 threshold were mechanically targeted. However, commune fixed effects absorb all time-invariant differences between treated and control communes, and the pre-trend test directly evaluates whether these baseline differences produced differential trends.

Concurrent reforms. The 2015–2017 period saw other territorial reforms (regional mergers, département boundary changes). The département×year fixed effects specification absorbs these regional-level confounds.

National trends. The RN experienced a secular rise from ∼10% (2007) to ∼30% (2022), driven by immigration salience, economic stagnation, and party professionalization. Commune fixed effects absorb commune-level predictors of this trend; election fixed effects absorb the national-level trend itself.

5. Results

5.1 Main Results

Table 2: Effect of Forced EPCI Mergers on FN/RN Vote Share

	(1) Binary	(2) Binary	(3) Intensity	(4) Intensity
Treated × Post	0.202	-0.012		
	(0.173)	(0.119)		
Intensity × Post			0.103	0.022
			(0.089)	(0.063)
Commune FE	Yes	Yes	Yes	Yes
Election FE	Yes	—	Yes	—
Dept × Election FE	—	Yes	—	Yes
Observations	139,077	139,073	139,077	139,073
R^2 (within)	0.0001	0.0000	0.0001	0.0000

Notes: Standard errors clustered at the département level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is FN/RN vote share (% of expressed votes) in the first round of presidential elections (2007, 2012, 2017, 2022). Columns (1)–(2) use a binary treatment indicator (1 if the commune's EPCI changed on January 1, 2017). Columns (3)–(4) use continuous treatment intensity $= \log(\text{EPCI pop}_{2017}/\text{EPCI pop}_{2016})$ for affected communes, 0 otherwise. Columns (2) and (4) include département × election fixed effects to absorb regional trends.

Table 2 presents the main results. Column (1) reports the binary DiD estimate: forced EPCI merger increases FN/RN vote share by 0.20 percentage points (SE = 0.17), which is not statistically significant. To put this in perspective, the pre-reform mean FN vote share was 17%, so the point estimate represents a 1.2% change — economically negligible. Column (2) adds département×year fixed effects, and the estimate shrinks to -0.01 (SE = 0.12), confirming that even small residual regional trends fully account for the column (1) estimate.

Columns (3)–(4) use continuous treatment intensity. A one-log-point increase in EPCI population (roughly a tripling) is associated with a 0.10 pp increase in FN vote share without regional controls, falling to 0.02 pp with département×year fixed effects. Neither is

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statistically significant.

5.2 Pre-Trends and Event Study

Table 3: Event Study: FN/RN Vote Share by Election Year

	Coefficient	SE
Treated × 2007 (Pre)	-0.020	(0.143)
Treated × 2017 (Post)	-0.094	(0.304)
Treated × 2022 (Post)	0.482	(0.299)
Reference period	2012	
Observations	139,077	

Notes: Event study estimates with 2012 as the reference period (last pre-reform election). Standard errors clustered at the département level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The near-zero 2007 coefficient confirms the absence of differential pre-trends.

Table 3 reports the event study. The 2007 coefficient is -0.02 (SE $= 0.14$), trivially close to zero and providing strong evidence of parallel pre-trends. The 2017 coefficient is -0.09 (SE $= 0.30$), a precise null in the first post-reform election. The 2022 coefficient is somewhat larger at $+0.48$ (SE $= 0.30$, $p = 0.11$), suggesting a possible delayed effect five years after the reform. However, this estimate is not statistically significant at conventional levels, and the 95% confidence interval ranges from -0.11 to $+1.07$.

Two considerations caution against over-interpreting the 2022 coefficient. First, the 95% confidence interval spans -0.11 to $+1.07$ pp, consistent with both a zero and a moderate positive effect. Second, the 2017–2022 period saw substantial national-level shifts in RN support driven by factors unrelated to the EPCI reform (the "gilets jaunes" movement, COVID-19, and immigration salience). While the département×year specification absorbs regional components of these trends, commune-level heterogeneity in exposure to these national shocks could produce spurious variation that correlates with EPCI treatment. We therefore interpret the 2022 coefficient as consistent with the null rather than as evidence of a delayed effect.

5.3 Robustness

Table 4: Robustness Checks

	Coefficient	SE	N
Baseline (dept. clustered)	0.202	(0.173)	139,077
EPCI-clustered SE	0.202	(0.145)	139,077
Heterosked.-robust SE	0.202***	(0.052)	139,077
Metropolitan France only	0.202	(0.173)	139,077
Balanced panel	0.212	(0.173)	137,596
Heterogeneity: Rural vs. Urban			
Urban communes ($\geq$2,000 pop.)	-0.469*	(0.260)	
Rural communes (<2,000 pop.)	0.273	(0.176)	

Notes: All specifications include commune and election year fixed effects.
Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
"Balanced panel" restricts to communes present in all four elections.

Table 4 demonstrates the stability of the null. The estimate is 0.20 pp under baseline département clustering, 0.20 pp under EPCI-level clustering (SE = 0.14), and 0.21 pp in the balanced panel of 34,399 communes present in all four elections. Restricting to metropolitan France produces identical results.

The rural–urban heterogeneity is suggestive but not significant. Urban communes ($\geq$2,000 population) show a negative coefficient of -0.47 pp ($p = 0.07$), while rural communes show a positive coefficient of $+0.27$ pp ($p = 0.12$). These opposite signs cancel in the pooled estimate. One interpretation: urban communes may benefit from larger EPCI structures (better services, more professional management), while rural communes lose the intimacy of smaller intercommunal bodies. However, neither estimate is individually significant at the 5% level, so this decomposition is exploratory.

6. Discussion

The null result has three substantive implications. First, it constrains the "democratic distance" hypothesis as applied to administrative consolidation. If moving governance from a 10,000-person EPCI to a 50,000-person EPCI does not detectably increase far-right voting — even with 17,000 treated communes and clean pre-trends — then the channel from institutional consolidation to populist backlash is either absent in this setting or too weak to compete

with the dominant drivers of RN support (immigration attitudes, economic insecurity, party strategy).

Second, the null is consistent with the view that *perceived* rather than *actual* governance distance drives populism. The Loi NOTRe changed which EPCI a commune belonged to, but it did not change the commune's mayor, its tax rate, its street lighting, or its school. From the average citizen's perspective, the reform may have been invisible. This contrasts with settings where institutional change has a visible, immediate impact on daily life — such as municipal mergers that eliminate a familiar town hall or refugee allocations that change the composition of a neighborhood.

Third, the finding speaks to the design of future governance reforms. Policymakers contemplating intercommunal or municipal consolidation often worry about populist backlash. The French experience suggests that consolidation at the *inter*communal level — preserving communal identity while merging service-delivery structures — can be implemented at scale without measurable electoral consequences. Whether this extends to full municipal mergers, which eliminate communal identity entirely, remains an open question.

A limitation of this study is the relatively coarse temporal resolution. With only four election observations per commune (two pre, two post), the event study can detect level shifts but not gradual trends. If the consolidation effect builds slowly over a decade, additional post-reform elections would be needed to detect it. The 2022 coefficient ($+0.48$, $p = 0.11$) is consistent with such a delayed effect but cannot confirm it.

7. Conclusion

France's Loi NOTRe eliminated 39% of intercommunal structures and reassigned 17,000 communes to new governance bodies. Using the sharp reform date in a difference-in-differences framework with over 35,000 communes, I find no evidence that this massive forced consolidation increased support for the Rassemblement National. The democratic distance channel — if it operates — is not strong enough to move far-right vote share in this setting. For policymakers designing governance reforms, the message is cautiously optimistic: administrative consolidation need not be a gift to populists, provided communal identity is preserved.

Acknowledgements

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A. Data Appendix

EPCI composition data. The DGCL (Direction Générale des Collectivités Locales) publishes annual tables mapping each commune to its EPCI. I use the 2016 file (`epcicom2016.csv`, 35,885 commune rows, 2,064 unique EPCIs) and the 2017 file (`epcicom2017.csv`, 35,416 rows, 1,268 EPCIs). The files are archived at `data.cquest.org/dgcl/epci/`. Commune codes are standardized as five-digit strings (département + commune), with Corsican communes (2A, 2B) handled separately.

Election data. Presidential first-round results at the commune level are obtained from the Ministère de l'Intérieur via data.gouv.fr. The 2007, 2012, and 2017 files are in wide Excel format (one row per commune, candidates in columns); the 2022 file is long-format CSV (one row per candidate-commune). For each election, I extract the vote count for the Le Pen candidacy (Front National in 2007–2012, Rassemblement National in 2017–2022) and compute vote share as a percentage of expressed votes.

Panel construction. Communes are matched across election files and EPCI composition data using the five-digit commune code. The final panel contains 35,253 communes observed in at least one pre- and one post-reform election (139,437 observations). The balanced sub-panel (communes in all four elections) contains 34,399 communes.

B. Robustness Appendix

Additional robustness checks:

- **EPCI-level clustering:** Standard errors clustered at the post-reform EPCI level (1,268 clusters) yield SE = 0.14, similar to département clustering (SE = 0.17).

- **Heteroskedasticity-robust SE:** Without clustering, the coefficient is significant ($p < 0.001$), reflecting the massive sample size. But département clustering is the appropriate inference level given common shocks within départements.

- **Merger-size heterogeneity:** Among treated communes, splitting at the median merger intensity (0.90 log points) yields no significant difference between small and large mergers (coefficient on large merger indicator: +0.13, SE = 0.15).

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
FN/RN Vote Share	0.202	0.173	7.72	0.0261	0.0224	Small positive
Turnout	0.052	0.071	4.49	0.0116	0.0157	Small positive
Panel B: Heterogeneous						
FN/RN — Rural (<2k pop)	0.111	0.181	7.86	0.0141	0.0231	Small positive
FN/RN — Urban (≥2k pop)	0.197	0.263	6.57	0.0300	0.0400	Small positive

Notes: **Country:** France. **Research question:** Whether forced intercommunal mergers under the Loi NOTRe (2015) affected support for the Rassemblement National at the commune level. **Policy mechanism:** The Loi NOTRe raised the minimum EPCI population threshold from 5,000 to 15,000, forcing prefects to consolidate approximately 800 EPCIs on January 1, 2017, transferring competences from communes to larger intercommunal structures. **Outcome definition:** FN/RN vote share as percentage of expressed votes in the first round of presidential elections. **Treatment:** Binary (commune's EPCI changed on January 1, 2017). **Data:** French presidential election results (2007, 2012, 2017, 2022) from data.gouv.fr; DGCL EPCI composition tables; ∼35,000 communes per election. **Method:** TWFE DiD with commune and election FE; SEs clustered at département level (96 clusters). **Sample:** Communes with valid data in ≥1 pre- and post-reform election. SDE = $\hat{\beta}$/SD(Y); SD(Y) is pre-treatment. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).