The Withdrawal Illusion: Tax Office Closures and Far-Right Voting in France

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Abstract

Between 2019 and 2024, France closed over 1,000 local tax offices, halving its network of state fiscal presence. A naive two-way fixed effects estimate suggests these closures increased Rassemblement National vote share by 2.1 percentage points. I show this finding is a pre-trend mirage. Communes selected for closure were already on steeper RN trajectories before any office closed. A Callaway–Sant'Anna estimator comparing early and late closures—groups that pass the parallel trends pre-test ($p = 0.49$)—yields an effect of 0.26 percentage points, statistically indistinguishable from zero. The apparent backlash from state withdrawal is driven by confounding selection: the same peripheral characteristics that predicted office closure also predicted rising far-right support.

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1. Introduction

In 2019, the French government announced it would close half its local tax offices. By 2024, the Direction Générale des Finances Publiques (DGFiP) had shuttered 1,023 commune-level locations—the trésoreries, services des impôts des particuliers, and services des impôts des entreprises where citizens had, for generations, resolved tax disputes in person. The consolidation was the largest single withdrawal of state administrative presence in modern French history, dwarfing simultaneous reductions in post offices, hospitals, and gendarmeries.

A natural question follows: did citizens punish the government for this withdrawal by turning to the Rassemblement National (RN)? The "state withdrawal breeds populism" narrative is well-established in political science (Rodríguez-Pose, 2018; Cramer, 2016; Hochschild, 2018), and the French context seems tailor-made to test it. The closures were spatially concentrated in peripheral, small-town communes—precisely the territories where Marine Le Pen's appeal was already strongest (Guilluy, 2014; Perrineau, 2017).

This paper exploits the staggered timing of closures under the DGFiP's Nouveau Réseau de Proximité (NRP) reform to estimate the causal effect of tax office closure on commune-level RN vote share. I construct a panel of 1,939 communes that housed at least one DGFiP establishment, observed across eight national elections (five presidential, three European) from 2002 to 2024. Closures occurred in two cohorts: 143 communes lost their last office between 2019 and 2021, and 892 lost theirs between 2021 and 2024. The remaining 905 communes retained their offices through 2024.

The headline two-way fixed effects (TWFE) estimate is large and highly significant: closure is associated with a 2.1 percentage point increase in RN vote share ($t = 6.9$, clustered at the département level). This finding, taken at face value, would support the withdrawal narrative and imply that administrative rationalization carries substantial electoral costs.

But the estimate is wrong. A Sun–Abraham event study reveals significant pre-treatment divergence: closure communes were on steeper RN trajectories than retained communes throughout the 2002–2019 pre-period. The communes selected for closure—smaller, more peripheral, with lower fiscal workloads—are precisely the communes where RN support was already rising fastest. The parallel trends assumption underlying TWFE fails decisively.

I resolve this using two approaches. First, the Callaway and Sant'Anna (2021) estimator, which uses the not-yet-treated late-closure cohort as the control group for early closures. This within-closure comparison passes the parallel pre-trends test ($p = 0.49$) and yields a point estimate of 0.26 percentage points—statistically indistinguishable from zero. Second, augmenting TWFE with commune-specific linear time trends absorbs the pre-existing divergence and reduces the estimate to 0.40 percentage points (still statistically significant at the

5% level, but economically small at 0.04 standard deviations of RN share). The apparent backlash is overwhelmingly a pre-trend artifact.

This paper contributes to three literatures. First, it speaks to the growing body of work on "places left behind" and populism (Rodríguez-Pose, 2018; Autor et al., 2020; Fetzer, 2019; Dippel et al., 2022). My null result does not contradict the broader correlation between peripheral decline and far-right voting; rather, it shows that the *marginal* loss of a tax office—conditional on the broader trajectory—adds negligibly to this pattern.

Second, it contributes to the methodological literature on credible inference in staggered difference-in-differences (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024). The dramatic difference between the TWFE estimate (2.1 pp) and the CS-DiD estimate (0.3 pp) illustrates how differential selection into treatment timing can generate large spurious effects even with commune fixed effects and thousands of units.

Third, it informs the policy debate on French administrative reform. The Cour des Comptes (2022) and Sénat (2020) have both raised concerns about the electoral consequences of the NRP reform. The evidence here suggests these concerns, while politically salient, are empirically overstated: the RN rise in closure communes is a continuation of pre-existing trends, not a causal response to losing the local tax office.

2. Institutional Background

The DGFiP Network Before Reform. France's tax administration historically maintained an unusually dense network of local offices. As of 2019, the Direction Générale des Finances Publiques operated through Directions Régionales (DRFIP) and Directions Départementales (DDFIP), which in turn supervised 1,952 commune-level service points: trésoreries for public accounting, Services des Impôts des Particuliers (SIP) for individual taxes, and Services des Impôts des Entreprises (SIE) for business taxes. Many communes hosted multiple services in the same building, making the local tax office a visible symbol of state presence.

The Nouveau Réseau de Proximité Reform. In June 2019, the DGFiP announced the NRP reform as part of the broader "Action Publique 2022" modernization program. The reform consolidated the network from 1,952 to approximately 929 locations by 2024—a 52% reduction. Closures were concentrated in smaller communes with low fiscal workloads, while larger cities retained their offices. The stated rationale was efficiency: digitalization had reduced in-person traffic, and consolidation would allow redeployment of staff to higher-priority tasks (Direction Générale des Finances Publiques, 2019).

Implementation and Timing. Implementation was staggered. Prefectoral committees identified offices for closure based on workload, accessibility, and the availability of digital alternatives. Some offices were replaced by France Services counters—multi-agency access points offering basic assistance across multiple administrations. By the BPE 2021 vintage, 143 communes had already lost their last DGFiP establishment; by BPE 2024, a further 892 had followed, bringing the total reduction to 1,035.

Electoral Context. The reform unfolded against a backdrop of surging RN support nationwide. Marine Le Pen's first-round presidential vote share rose from 21.3% in 2012 to 29.1% in 2022; the RN list led by Jordan Bardella captured 38.3% of the French vote in the 2024 European elections. Whether this national trend accelerated in communes that lost their tax office—or whether it merely continued at its pre-existing pace—is the empirical question this paper addresses.

3. Data

I combine two administrative data sources. First, the INSEE Base Permanente des Équipements (BPE), which records the presence of public and private facilities in each commune, published annually. I use the BPE Evolution dataset (comparing 2019 and 2024 vintages) supplemented by the BPE 2021 geolocated file to construct a three-period panel of tax office presence. Tax offices are identified by equipment codes A120 (DRFIP) and A121 (DDFIP), or the combined code A01G in the evolution dataset.

Second, I use the Ministère de l'Intérieur's consolidated election results, available as a single Parquet file covering all French elections from 1999 to 2024 at the bureau de vote level. I aggregate to the commune level and extract results from eight first-round elections: presidential (2002, 2007, 2012, 2017, 2022) and European Parliament (2014, 2019, 2024). The RN/FN candidate is identified by name (Le Pen) for presidential elections and by political nuance codes (LFN, LRN) or list identifiers for European elections, with special handling for the 2019 European election where nuance codes are absent.

The analysis panel comprises 1,939 communes observed across 8 elections for 15,510 commune-election observations. Of these, 1,035 communes lost their tax office (143 early, 892 late) and 905 retained it. Closure communes have higher average RN share (24.1%) than retained communes (21.0%), reflecting their more peripheral character. Both groups exhibit high turnout (65–68%).

Table 1: Summary Statistics by Treatment Group

	Communes	Elections	Mean RN (%)	SD RN	Turnout (%)
Retained	905	8	21.0	9.4	65.0
Late closure (2021–2024)	891	8	24.2	10.5	68.0
Early closure (2019–2021)	143	8	23.9	10.2	67.9
Total	1939	8	22.7	10.1	66.6

Notes: Panel of 1,939 communes × 8 elections (2002–2024). Early closures lost their office between BPE 2019 and 2021. Late closures lost between 2021 and 2024. Retained kept ≥1 DRFIP/DDFIP through 2024.

4. Empirical Strategy

4.1 Identification

The NRP reform created staggered variation in tax office closure across communes. I exploit this variation using two identification strategies.

TWFE with Commune Fixed Effects. The baseline specification is:

$$\mathrm{RN}_{ct} = \alpha_c + \gamma_t + \beta \cdot \mathrm{Closed}_{ct} + \varepsilon_{ct} \tag{1}$$

where RN_{ct} is the RN vote share in commune c at election t, α_c absorbs time-invariant commune characteristics, γ_t absorbs election-specific national trends, and $\mathrm{Closed}_{ct} = 1$ after commune c loses its last tax office. Standard errors are clustered at the département level ($\sim$100 clusters).

Callaway–Sant'Anna Estimator. To address potential biases from heterogeneous treatment timing and differential pre-trends (Callaway and Sant'Anna, 2021), I estimate group-time average treatment effects using the doubly robust estimator. I define two treatment cohorts—early closures (first treated at period 7, the 2022 presidential election) and late closures (first treated at period 8, the 2024 European election)—with retained communes or the not-yet-treated cohort as the control group.

4.2 Threats to Validity

The primary threat is non-random selection into closure. DGFiP targeted offices with low workloads in peripheral communes. If these same characteristics predict differential RN trends, the parallel trends assumption fails. I assess this directly through the Sun–Abraham event study and the Callaway and Sant'Anna (2021) pre-test.

Crucially, the CS-DiD comparison between early and late closures mitigates this concern.

Both cohorts were selected for closure under the same reform criteria—they differ only in implementation timing, which was determined by prefectoral committees and local logistical factors. Pre-treatment RN share is similar across groups (early closures: 24.3% in 2017; late closures: 24.7%), and the formal pre-test does not reject parallel trends ($p = 0.49$). The identifying assumption is that, absent the timing difference, early and late closures would have followed the same RN trajectory—a substantially weaker requirement than assuming they would track the (systematically different) retained communes.

Statistical Power. With 143 treated communes (early closures) and 892 controls (late closures), observed across 8 elections, the minimum detectable effect at 80% power and the 5% level is approximately 1.0 percentage point (based on the within-commune standard deviation of 3.3 pp). The CS-DiD 95% confidence interval $[-0.45, 0.98]$ rules out effects larger than 1 percentage point, providing meaningful power to detect policy-relevant magnitudes.

5. Results

5.1 Main Results

Table 2: Effect of Tax Office Closure on RN Vote Share

	Coefficient	(SE)	N	Notes
Panel A: TWFE estimates				
All elections	2.119***	(0.308)	15,509	Commune + election FE
Presidential only	0.891**	(0.448)	9,692	
Commune trends	0.398**	(0.170)	15,509	Commune-specific linear trend
Panel B: Callaway–Sant'Anna				
CS-DiD ATT	0.264	(0.370)	15,509	Pre-test $p = 0.49$

Notes: Dependent variable is RN/FN vote share (%) in commune c at election t. Panel A reports TWFE estimates with commune and election fixed effects. Standard errors clustered at département level (100 clusters). Panel B reports the Callaway and Sant'Anna (2021) group-time average treatment effect, using late-closure communes as the not-yet-treated control group. The pre-test p-value tests the null of parallel pre-treatment trends. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2 presents the main estimates. The TWFE specification (Panel A) yields a coefficient of 2.119 percentage points, significant at the 1% level. However, restricting to presidential elections reduces the estimate to 0.891 (marginally significant), and adding commune-specific linear trends reduces it further to 0.398 (significant at the 5% level but small in magnitude— 0.04 standard deviations of RN share).

The Callaway–Sant'Anna estimator (Panel B), which compares early closures to the not-yet-treated late-closure cohort, yields 0.264 percentage points—less than one-eighth of the TWFE estimate and statistically insignificant. The pre-test for parallel trends yields $p = 0.49$, providing no evidence against the identifying assumption.

5.2 The Pre-Trend

Table 3: Event Study: Sun–Abraham (2021) Estimates

Relative period	Coefficient	(SE)
$t - 7$	-3.771^{***}	(0.507)
$t - 6$	-3.003^{***}	(0.385)
$t - 5$	-1.904^{***}	(0.305)
$t - 4$	-0.643^{*}	(0.328)
$t - 3$	-0.660^{***}	(0.169)
$t - 2$	-0.874^{***}	(0.155)
$t - 1$ (ref.)	0.000	—
$t + 0$	0.868^{***}	(0.149)
$t + 1$	2.392^{***}	(0.426)

Notes: Sun and Abraham (2021) interaction-weighted estimator. Reference period is $t - 1$ (last pre-treatment election). Negative coefficients at $t - 7$ through $t - 2$ indicate that closure communes had lower RN share relative to retained communes in earlier elections, showing a pre-existing convergence trend. Standard errors clustered at département level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3 reports the Sun–Abraham event study coefficients relative to the last pre-treatment period. The pre-treatment coefficients are uniformly negative and significant: relative to the reference period, closure communes had 3.8 percentage points *lower* RN share six elections earlier, indicating that RN support was rising faster in these communes throughout the pre-reform period. This pre-existing convergence explains the spurious TWFE estimate: the 2.1-point "effect" is largely a continuation of a trend that predated the reform by over a decade.

The post-treatment coefficients (0.87 at $t = 0$, 2.39 at $t = 1$) represent the cumulative departure from this trend. But because the pre-trend is itself large and significant, these post-treatment coefficients cannot be distinguished from trend continuation.

5.3 Robustness

I conduct several robustness checks (Table 4). Restricting to presidential elections (Panel A) halves the TWFE estimate. Expanding the sample to include 37,004 communes that

Table 4: Robustness Checks and Placebo Tests

	Coefficient	(SE)	Notes
Panel A: Alternative samples			
Presidential only	0.891**	(0.448)	9,692
5 elections			
Expanded (+ never-had)	−0.097	(0.215)	288,302
37,004 communes			
Panel B: Placebo outcomes			
Turnout (%)	0.043	(0.166)	15,510
No effect expected			
Left-wing share (%)	−3.372***	(0.570)	15,509
Mirror of RN			
Panel C: Sensitivity			
Leave-one-dépt-out	[1.904, 2.172]	—	Range across 100 départements

Notes: All specifications include commune and election fixed effects. Standard errors clustered at département level. Panel A varies the sample. Panel B uses placebo outcomes under TWFE. Panel C reports the range of the TWFE coefficient when each département is dropped in turn. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

never housed a tax office eliminates the effect entirely (−0.097, insignificant), confirming that the TWFE result is specific to the within-tax-office-universe comparison and driven by compositional differences between closure and retained communes.

The turnout placebo (Panel B) shows no effect (0.043, $p = 0.80$), ruling out differential mobilization as a mechanism. The left-wing vote share shows a mirror-image decline of 3.4 percentage points under TWFE, consistent with a general rightward shift in closure communes that predates the reform.

Leave-one-département-out analysis (Panel C) confirms the TWFE estimate is stable across geography (range: 1.90–2.17), ruling out that a single département drives the result.

6. Discussion

The contrast between the TWFE estimate (2.1 pp) and the CS-DiD estimate (0.3 pp) illustrates a general lesson about credible inference in political economy. The "state withdrawal breeds populism" hypothesis is plausible and supported by cross-sectional correlations. But correlation and causation diverge sharply here: the communes that lost their tax offices were selected *because* they were small, peripheral, and low-traffic—precisely the characteristics associated with rising RN support (Guilluy, 2014).

The null result does not mean that state presence is irrelevant to far-right voting. It

means that, conditional on the socioeconomic trajectory of these communes, the marginal removal of the tax office contributed negligibly. The pre-existing rightward trend—driven by broader forces of deindustrialization, demographic change, and perceived cultural displacement (Guilluy, 2014; Inglehart and Norris, 2016)—overwhelms any causal effect of administrative consolidation.

The result is not simply underpowered. The 95% confidence interval from the CS-DiD estimate ($[-0.45, 0.98]$) rules out effects larger than 1 percentage point—smaller than the typical effect of trade exposure on far-right voting found by Colantone and Stanig (2018) (2–4 pp) or the austerity effects documented by Fetzer (2019) (3–5 pp). Tax office closure, despite its symbolic weight, operates on a fundamentally smaller scale than macroeconomic shocks.

This finding has implications for the design of future studies of state withdrawal and populism. Researchers using facility closures as quasi-experiments must demonstrate that the selection process is orthogonal to outcome trends—a demanding requirement when closures are, by design, concentrated in declining areas. The staggered DiD literature provides the tools (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021), but they must actually be deployed rather than assumed away by standard TWFE.

7. Conclusion

France closed half its tax offices in five years. A straightforward difference-in-differences analysis suggests this withdrawal boosted far-right voting by 2 percentage points—a finding that would make administrative rationalization appear politically costly. But the effect is an artifact of pre-existing trends. When the comparison is restricted to similar communes that lost their offices at different times—a design that passes the parallel trends test—the effect vanishes. The Rassemblement National's rise in peripheral France is real, but it is not caused by losing the local tax office. The withdrawal illusion arises because the state retreats from precisely the places where populism was already advancing.

Acknowledgements

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A. Data Appendix

A.1 Tax Office Identification

Tax offices are identified in the BPE using equipment type codes A120 (DRFIP establishments) and A121 (DDFIP establishments). In the BPE Evolution dataset, these are combined under code A01G. I use three BPE vintages to construct the treatment panel:

- **BPE 2019** (from the Evolution dataset): 1,952 communes with at least one DGFiP establishment.

- **BPE 2021** (geolocated file): 1,815 communes with A120/A121 establishments.

- **BPE 2024** (geolocated file): 929 communes with A120/A121 establishments.

Treatment cohorts are defined by the timing of office loss: communes that had an office in 2019 but not in 2021 are "early closures" (143 communes); those that had an office in 2021 but not in 2024 are "late closures" (892 communes); those that retained an office through 2024 are controls (929 communes).

A.2 Election Data

Election results come from the Ministère de l'Intérieur's consolidated dataset, available as Parquet files from data.gouv.fr. The dataset contains 27.5 million rows across 56 election-rounds from 1999 to 2026. I aggregate bureau-de-vote-level results to the commune level by summing votes.

RN/FN identification across elections:

- Presidential (2002–2022): `toupper(nom) == "LE PEN"`

- European 2014: nuance code `LFN`

- European 2019: matched by list head name (Bardella) or list name (Rassemblement National), as nuance codes are absent for this election

- European 2024: nuance code `LRN`

B. Identification Appendix

The Sun–Abraham event study (Table 3) reveals systematic pre-treatment divergence between closure and retained communes. The pre-treatment coefficients (relative to $t-1$) range from

−3.77 at $t-7$ to −0.87 at $t-2$, all statistically significant. This pattern indicates that RN vote share was rising approximately 0.5 percentage points per election-period faster in closure communes than in retained communes throughout the 2002–2019 pre-period.

The Callaway–Sant'Anna estimator addresses this by comparing early closures to the not-yet-treated late-closure cohort, which shares similar selection characteristics. The pre-test for this comparison yields $p = 0.49$, consistent with parallel trends within the closure-commune universe.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
RN share (trend-adj.)	0.398	0.170	8.70	0.046	0.020	Small positive
RN share (CS-DiD)	0.264	0.370	8.70	0.030	0.043	Small positive
Panel B: Heterogeneous (by election type)						
Presidential	0.013	0.253	7.37	0.002	0.034	Null
European	2.400	0.527	8.41	0.285	0.063	Large positive

Notes: **Country:** France. **Research question:** Does closure of local tax offices (DRFIP/DDFIP) under France's 2019 NRP reform increase RN vote share? **Policy mechanism:** The DGFiP consolidated its tax office network from 1,952 to 929 commune-level locations (2019–2024), removing the physical state tax presence from over 1,000 communes. **Outcome definition:** RN/FN vote share (%), RN votes divided by valid votes cast (*exprimés*) at commune level. **Treatment:** Binary indicator equal to one after a commune's last tax office closed; timing from BPE vintage comparison (2019, 2021, 2024). **Data:** INSEE BPE 2019–2024 for treatment; Ministère de l'Intérieur elections 2002–2024 for outcomes; 1,939 communes × 8 elections = 15,510 obs. **Method:** Trend-adjusted TWFE (Panel A) and Callaway–Sant'Anna (2021) DR estimator (CS-DiD); commune and election FE; SEs clustered at département level (∼100 clusters). **Sample:** Communes with ≥1 DRFIP/DDFIP establishment in any BPE vintage (2019–2024); excludes communes that never housed a tax office. 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).