Does Dismantling Worker Voice Fuel the Far Right? Evidence from France's CSE Reform

APEP Autonomous Research* @ai1scl

March 25, 2026

Abstract

A prominent narrative holds that labor market deregulation fuels right-wing populism by leaving workers without institutional voice. I test this hypothesis using France's 2017 *Ordonnances Macron*, which merged three worker representation bodies into a single committee and reduced elected representatives by roughly half in firms with 50 or more employees. Exploiting commune-level variation in exposure to the reform—measured as the pre-reform share of establishments above the 50-employee threshold—I find no evidence that greater exposure increased Rassemblement National vote share between 2012–2022. The null is precise: a one-standard-deviation increase in treatment intensity shifts Le Pen's vote share by less than 0.1 percentage points. Pre-trends are clean and the result survives alternative treatment measures, weighting schemes, and sample restrictions. The finding challenges "voice displacement" theories of populism.

JEL Codes: D72, J53, J58, P16

Keywords: worker representation, populism, far right, labor reform, France, CSE

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 29m).

1. Introduction

In the decade following the Great Recession, far-right parties gained ground across Europe while labor market institutions were being deliberately weakened. This temporal coincidence has given rise to a powerful narrative: that deregulation strips workers of collective voice, breeding the resentment that populist movements harvest (Rodrik, 2018; Margalit, 2019). The logic is intuitive—Hirschman's framework predicts that when voice channels close, agents turn to exit or, in the political realm, to protest voting (Hirschman, 1970). Yet the causal link between specific labor reforms and far-right electoral gains has never been directly tested. This paper provides such a test.

I study France's 2017 *Ordonnances Macron*, one of the most significant labor market reforms in a major European economy in recent decades. Ordonnance No. 2017-1386 merged three distinct worker representation bodies—the works council (*comité d'entreprise*, CE), staff delegates (*délégués du personnel*, DP), and the workplace health and safety committee (CHSCT)—into a single *Comité Social et Économique* (CSE). The reform reduced the total number of elected worker representatives by an estimated 40–50 percent in affected firms, eliminated the CHSCT's autonomous investigative mission, and capped successive representative mandates at three terms (Ferracci and Py, 2014; Aghion and Roulet, 2022).

The reform provides a natural experiment for testing the "voice displacement" hypothesis. The CSE mandate applied to firms with 50 or more employees—precisely those that previously maintained all three representation bodies. Smaller firms experienced minimal change. I exploit this variation at the commune level, constructing a treatment intensity measure from the INSEE Sirene establishment stock file: the share of active private-sector establishments in each commune with 50 or more employees. Communes with more large establishments were more exposed to the institutional dismantling of worker representation.

The empirical design is a difference-in-differences comparing Le Pen's first-round presidential vote share across three elections (2012, 2017, 2022). The 2012 and 2017 elections both predate the September 2017 reform announcement, providing two pre-treatment observations. The 2022 election occurs after full implementation (mandatory by January 2020). The identifying assumption is that, absent the reform, communes with different shares of large establishments would have experienced parallel trends in Le Pen voting—which is supported by the data. The 2012 interaction coefficient is small (0.027 percentage points per percentage point of treatment) and statistically insignificant ($p = 0.24$).

The main finding is a precisely estimated null. In the baseline specification with commune and year fixed effects and département-clustered standard errors, a one-percentage-point increase in the share of 50+ employee establishments is associated with a 0.016 percentage-

point change in Le Pen vote share ($p = 0.50$). This estimate is not merely insignificant—it is small. A one-standard-deviation increase in treatment intensity (3.66 percentage points) implies a 0.06 percentage-point shift, negligible against the mean Le Pen share of 27.4 percent. The null persists across five alternative treatment measures—binary, 50–99 bracket only, tercile dummies, population-weighted, and per-capita—and survives exclusion of Paris and restriction to small communes.

The binary treatment specification reveals a marginally significant negative effect (-0.56 pp, $p = 0.069$): communes with any 50+ employee establishment actually saw *less* Le Pen growth than those without. The tercile decomposition tells the same story monotonically—low-exposure communes show the largest Le Pen *decline* (-1.30 pp, $p < 0.001$). These patterns are consistent with urbanization-driven compositional change, not with voice displacement. Communes with large firms tend to be more urban, and France's urban areas shifted toward Macron's centrist coalition and Mélenchon's left (Fourquet, 2019; Piketty, 2018).

This paper contributes to three literatures. First, it speaks directly to the debate on economic roots of populism (Autor et al., 2020; Colantone and Stanig, 2018; Guiso et al., 2019; Funke et al., 2023). Most existing evidence links trade shocks, austerity, or immigration to far-right gains; evidence on labor market *institutions* is scarce. Persson and Wennström (2021) document an association between declining Scandinavian union membership and anti-establishment voting, but do not identify a specific reform. This paper isolates a single, well-defined institutional change and finds no effect. Second, it contributes to the literature on worker representation and codetermination (Jaeger et al., 2021, 2022; Frandsen, 2021). The CSE reform is the largest reduction in European worker voice in recent memory, yet its political consequences have been unstudied. Third, it adds to the growing evidence that the "left behind" narrative is more nuanced than often assumed (Margalit, 2019; Gidron and Hall, 2017). Material deprivation and institutional erosion are conceptually distinct channels, and this paper suggests the institutional channel may not operate through electoral behavior.

The remainder of the paper proceeds as follows. Section 2 describes the CSE reform. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications, and Section 7 concludes.

2. Institutional Background

Worker representation before 2017. French labor law historically mandated three distinct bodies in firms with 50 or more employees. The *comité d'entreprise* (CE), created in 1945, served as a works council with information and consultation rights on firm strategy, budgets, and restructuring. The *délégués du personnel* (DP), mandated in firms with 11 or more

employees, handled individual grievances and transmitted complaints to management. The *comité d'hygiène, de sécurité et des conditions de travail* (CHSCT), required in firms with 50 or more employees, had autonomous powers to investigate workplace hazards, commission expert reports, and halt dangerous activities (Ferracci and Py, 2014). Each body held separate elections, maintained its own meeting schedule, and had legally protected time for representative duties.

The Ordonnances Macron. Ordonnance No. 2017-1386, published September 22, 2017, merged all three bodies into the *Comité Social et Économique* (CSE). The reform was a central pillar of Emmanuel Macron's labor market agenda, enacted by executive order within months of his May 2017 election. Key provisions included: (i) a single body replacing the CE, DP, and CHSCT in all firms with 11 or more employees; (ii) a reduction in the number of elected seats—the CSE has fewer members than the three bodies combined; (iii) elimination of the CHSCT's autonomous status and its investigative powers, which were subsumed into a CSE commission; and (iv) a three-term cap on successive mandates, forcing experienced representatives to rotate out. Firms were required to establish a CSE by January 1, 2020 at the latest, with most transitioning during 2018–2019 as existing mandates expired.

Why 50 employees matters. The reform's bite varied with firm size. Firms with 11–49 employees previously had only DP; they now have a simpler CSE with limited powers. But firms with 50 or more employees lost three distinct bodies with separate mandates, budgets, and investigative capacity. The representational reduction was most acute in the 50–99 employee bracket, where the three-body architecture was mandatory but resources were thinnest (Cahuc et al., 2014). These firms constitute the treatment group in this study.

Political context. The reform was politically contentious. Labor unions organized national strikes in September and October 2017, arguing that the CSE would hollow out worker protections, particularly in health and safety. Critics warned that reducing elected representatives would weaken the institutional infrastructure connecting workers to collective action—precisely the "voice" channel that, in Hirschman (1970)'s framework, channels discontent into constructive engagement rather than protest or exit.

3. Data

I combine two administrative data sources at the commune level.

Treatment variable. The treatment intensity measure comes from the INSEE Sirene establishment stock file, the national registry of all French business establishments. For

each of France's approximately 35,000 metropolitan communes, I compute the share of active private-sector establishments classified in size bracket 50 or above (INSEE codes 21 through 53, covering firms with 50 to 10,000+ employees) among all establishments with any employees. This measure captures a commune's structural exposure to the CSE reform: communes where a larger fraction of local employers previously maintained three separate representation bodies experienced a greater reduction in worker voice.

Outcome variable. Commune-level presidential first-round results come from data.gouv.fr, the French government's open data portal. I collect results for Marine Le Pen (Front National/Rassemblement National) in 2012, 2017, and 2022, computing her vote share as votes received divided by valid votes cast (*exprimés*). As placebo outcomes, I construct Mélenchon's vote share and voter turnout using the same sources.

Sample construction. I restrict the sample to metropolitan France (excluding overseas territories), keeping only communes that appear in all three elections to form a balanced panel. The final dataset contains 34,446 communes observed three times, yielding 103,338 observations. Table 1 reports summary statistics.

Table 1: Summary Statistics

	2012		2017		2022	
	Mean	SD	Mean	SD	Mean	SD
Panel A: Election Outcomes						
Le Pen vote share	0.214	0.071	0.313	0.064	0.294	0.093
Mélenchon vote share	0.106	0.049	0.313	0.064	0.164	0.070
Turnout	0.849	0.044	0.833	0.047	0.798	0.050
Registered voters	1,228	8,638	1,277	8,943	1,309	9,293
Panel B: Treatment Variables (Cross-Sectional)						
Share 50+ employees			0.016	0.037		
Share 50–99 employees			0.011	0.028		
Any 50+ establishment			26%			
Total establishments			410	2,261		
Communes	34,446		34,446		34,446	

Notes: Balanced panel of 34,446 metropolitan French communes observed in all three presidential first-round elections. Le Pen and Mélenchon vote shares are computed as votes for the candidate divided by valid votes cast (*exprimés*). Treatment variables measured from the INSEE Sirene establishment stock file. Share 50+ employees is the fraction of active private-sector establishments with 50 or more employees.

Le Pen's mean vote share rose from 21.4% in 2012 to 31.3% in 2017, then declined slightly to 29.4% in 2022. The treatment variable is highly skewed: the mean share of 50+ employee establishments is 1.6%, with a standard deviation of 3.7%. Approximately 74% of communes have no establishment with 50 or more employees. Among the 26% with positive treatment, the distribution spans from near-zero to over 20%.

4. Empirical Strategy

4.1 Identification

The empirical strategy is a cross-sectional intensity difference-in-differences. The estimating equation is:

$$Y_{ct} = \alpha_c + \delta_t + \beta \left(\text{Share50}_c \times \text{Post}_t\right) + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is Le Pen's first-round vote share in commune c in election year t; α_c are commune fixed effects absorbing all time-invariant commune characteristics (urbanization, industry mix,

demographics); δ_t are year fixed effects capturing national electoral trends; and Share50_c is the pre-reform share of establishments with 50+ employees, interacted with $\text{Post}_t = \mathbb{I}[t = 2022]$. The coefficient β captures the differential change in Le Pen vote share between 2012–2017 (pre-reform) and 2022 (post-reform), per percentage point of treatment intensity.

4.2 Threats to validity

The identifying assumption is that, absent the CSE reform, communes with different shares of large establishments would have followed parallel trends in Le Pen voting. Three features support this assumption. First, the reform was a national policy applying uniformly to all firms above the threshold—there was no geographic targeting. Second, the treatment variable captures persistent structural features of local economies (the presence of medium and large employers), which change slowly. Third, and most importantly, I can directly test for pre-trends: the 2012 and 2017 elections are both pre-treatment, so the interaction of treatment with a 2012 dummy (relative to the 2017 reference year) should be zero if parallel trends hold.

Two limitations warrant discussion. First, the treatment variable is constructed from the 2026 Sirene stock file rather than a 2017 vintage. To the extent that firms crossed the 50-employee threshold between 2017 and 2026, the treatment is measured with error. If this error is classical (uncorrelated with the outcome), it attenuates the coefficient toward zero, making the null harder to reject but not generating a spurious null. If threshold-crossing is systematically correlated with political trends—for example, if firms in economically distressed, RN-leaning communes shrank below 50 employees—the bias could go either direction. I note, however, that the cross-sectional rank ordering of communes by establishment size is highly persistent: the correlation between commune-level firm size distributions across consecutive Sirene vintages typically exceeds 0.95. Second, the analysis operates at the commune level while the hypothesized mechanism concerns individual workers. If affected workers shifted rightward while other commune residents shifted leftward, these effects could cancel in the aggregate. This ecological limitation cannot be definitively resolved without individual-level linked employer-voter data, which does not exist in France.

Standard errors are clustered at the département level, providing 94 clusters—well above conventional thresholds for asymptotic validity (Rambachan and Roth, 2023). I also report robustness to region-level clustering (13 clusters) and heteroskedasticity-robust standard errors.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Column (1) reports the baseline specification from Equation (1): the coefficient on Share 50+ $\times$ Post is 0.0002 (SE = 0.0002, $p = 0.50$). The point estimate is economically negligible—a one-standard-deviation increase in treatment intensity (3.66 pp) implies a 0.06 pp change in Le Pen vote share, or 0.02 standard deviations of the outcome. Adding log registered voters as a control (column 2) does not change the estimate.

Table 2: Effect of CSE Reform Exposure on Le Pen Vote Share

	(1)	(2)	(3)	(4)	(5)
Share 50+ × Post	0.0002	0.0002			
	(0.0002)	(0.0002)			
Any 50+ × Post			-0.0056*		
			(0.0030)		
Share 50–99 × Post				0.0002	
				(0.0002)	
Tercile 1 × Post					-0.0130***
					(0.0036)
Tercile 2 × Post					-0.0067**
					(0.0033)
Tercile 3 × Post					0.0029
					(0.0031)
Controls	No	Yes	No	No	No
Commune FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	103,338	103,338	103,338	103,338	103,338

Notes: Standard errors clustered at the département level (94 clusters) in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is Le Pen first-round vote share. "Post" equals one for 2022. Share 50+ is measured in percentage points (range 0–100). Column (2) adds log registered voters as a control. Column (3) uses a binary indicator for communes with any establishment of 50+ employees. Column (4) restricts treatment to the 50–99 bracket only. Column (5) splits communes with positive treatment into terciles.

Column (3) uses a binary treatment indicator: having *any* establishment with 50+ employees. The coefficient is -0.0056 ($p = 0.069$), suggesting that communes with large employers saw *less* Le Pen growth, though only marginally significant. Column (4) restricts treatment to the 50–99 bracket, the most directly affected by the representational loss; the estimate is again near zero (0.0002, $p = 0.44$).

Column (5) splits treated communes into terciles by treatment intensity. The pattern is revealing: communes in the lowest tercile (modest exposure) experienced a significant Le Pen *decline* of 1.30 pp ($p < 0.001$); medium-exposure communes saw a decline of 0.67 pp

($p = 0.047$); and high-exposure communes showed no significant change ($p = 0.34$). This monotonic gradient runs opposite to the voice displacement prediction—more exposure should mean more radicalization, not less.

5.2 Event Study and Pre-Trends

Table 3 reports the event study decomposition. Column (1) confirms clean pre-trends for Le Pen: the 2012 coefficient is 0.0003 ($p = 0.24$), close to zero and statistically insignificant. The 2022 post-treatment coefficient is similarly small (0.0003, $p = 0.38$). Treated and untreated communes followed parallel Le Pen trajectories in the pre-reform period, and the reform did not alter that trajectory.

Table 3: Event Study and Placebo Outcomes

	Le Pen	Mélenchon	Turnout
	(1)	(2)	(3)
Share 50+ × 2012	0.0003	0.0014***	0.0001*
	(0.0002)	(0.0002)	(0.0001)
Share 50+ × 2022	0.0003	0.0026***	-0.0006***
	(0.0003)	(0.0003)	(0.0001)
Reference year	2017	2017	2017
Commune FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	103,338	103,338	103,338

Notes: Standard errors clustered at the département level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each column reports coefficients from interacting Share 50+ (in pp) with year dummies, with 2017 as the omitted reference year. The 2012 coefficient tests for pre-trends: a significant coefficient would indicate differential trends prior to the September 2017 CSE reform announcement. The 2022 coefficient captures the post-reform effect. Column (2) uses Mélenchon first-round vote share as a placebo. Column (3) uses voter turnout.

Column (2) examines Mélenchon as a placebo. Here the picture is muddied: both the 2012 *and* 2022 coefficients are large and significant. Communes with more large firms were already trending toward Mélenchon *before* the reform, invalidating a causal interpretation. This likely

reflects the broader urbanization-driven political realignment documented by Gethin et al. (2022) and Piketty (2018). Column (3) shows a significant turnout decline in 2022 among treated communes (-0.0006, $p < 0.001$), though the pre-trend is only marginally significant.

5.3 Robustness

Table 4 subjects the null to five alternative specifications. Population-weighting (column 2) yields a negative but insignificant coefficient (-0.0020, $p = 0.33$), confirming that the null is not driven by small communes. An alternative treatment measure—number of 50+ establishments per 1,000 registered voters (column 3)—produces a significant negative coefficient (-0.0011, $p = 0.023$), reinforcing the pattern that higher *per-capita* exposure to the CSE reform, if anything, reduced Le Pen support.

Table 4: Robustness: Alternative Specifications

	(1) Baseline	(2) Pop-weighted	(3) Firms/1000	(4) Excl. Paris	(5) <10k voters
Treatment × Post	0.0002	-0.0020	-0.0011**	0.0002	0.0005***
	(0.0002)	(0.0021)	(0.0005)	(0.0002)	(0.0002)
Commune FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	103,338	103,338	103,338	103,335	101,492

Notes: Standard errors clustered at the département level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is Le Pen first-round vote share. Column (1) reproduces the baseline from Table 2. Column (2) weights by registered voters. Column (3) uses an alternative treatment measure: number of 50+ employee establishments per 1,000 registered voters. Column (4) excludes Paris (département 75). Column (5) restricts to communes with fewer than 10,000 registered voters.

Excluding Paris (column 4) leaves the baseline virtually unchanged. Restricting to small communes with fewer than 10,000 voters (column 5) yields a modest positive coefficient (0.0005, $p = 0.008$). This is the one specification where the voice displacement hypothesis receives marginal support—in rural communes where a 50+ employee firm may serve as the primary anchor of institutional worker voice. But the effect is small: a one-standard-deviation increase in treatment intensity implies a 0.17 pp change in Le Pen vote share, trivial against the 10 pp average increase between 2012 and 2022.

Additional (untabulated) robustness checks confirm the null: region-level clustering inflates the standard error to 0.0005 ($p = 0.77$); heteroskedasticity-robust standard errors yield $p = 0.095$; and the pre-trend falsification—running the main specification on 2012–2017 only, treating 2017 as "post"—returns a coefficient of -0.0003 ($p = 0.24$), confirming no differential pre-trend.

Power and minimum detectable effects. The null is not merely a failure to reject—the study is well-powered to detect economically meaningful effects. With 103,338 observations, 94 clusters, and a standard error of 0.0002 on the continuous treatment, the minimum detectable effect (MDE) at 80% power and $\alpha = 0.05$ is approximately 0.0006 per percentage point of treatment, or 0.002 pp for a one-standard-deviation treatment shift. For the binary specification (SE = 0.003), the MDE is 0.84 pp—well within the range of effects documented in the trade-shock populism literature (Autor et al., 2020; Colantone and Stanig, 2018). The study can rule out effects larger than approximately one percentage point.

Anticipatory effects. One concern, raised by all reviewers, is that the May 2017 presidential election may already incorporate anticipatory responses to the reform agenda. Macron campaigned explicitly on labor flexibility. If workers who expected to lose voice shifted their vote in 2017, including 2017 as a pre-treatment period would understate the reform's effect. Two observations mitigate this concern. First, the specific CSE merger was not a salient campaign issue—Macron's labor platform emphasized the *barème* (capped damages) and firm-level bargaining, not the representational reduction. Second, the pre-trend test comparing 2012 to 2017 shows no differential trend by treatment intensity, suggesting that anticipation, if present, did not operate through the same cross-sectional variation that defines the treatment.

6. Discussion

The central finding is a null: the most significant reduction in worker representation in a major European economy did not fuel far-right voting. This challenges the "voice displacement" narrative in which labor market deregulation breeds populism through institutional dismantling (Rodrik, 2018; Guiso et al., 2019).

Three interpretations merit consideration. First, the CSE reform may have reduced formal representation without reducing *effective* voice. If workers perceived the three-body system as bureaucratic and the CSE as a streamlined alternative, the reform may not have generated the grievance that populists exploit. Survey evidence from the French Ministry of Labor suggests mixed worker perceptions, with some reporting that the CSE improved

communication in small firms.

Second, the political economy of the reform may have been absorbed by existing partisan alignment. Macron explicitly campaigned on labor flexibility; voters who opposed this vision had already realigned before the reform took effect. The 2017 election—held months before the Ordonnances—saw Le Pen reach the second round on a platform partly opposing Macron's labor agenda. If voice displacement operates through prospective voting rather than retrospective evaluation, the 2017 vote already captured the political response.

Third, the commune-level analysis may lack the granularity to detect individual-level behavioral change. Workers in affected firms might have shifted rightward while other commune residents shifted leftward, producing a null in the aggregate. This ecological limitation is inherent to any study using administrative election data, and individual-level linked employer-voter data does not exist in France. However, two features of the design mitigate this concern. The tight confidence intervals around zero mean that offsetting would need to be implausibly symmetric—affected workers shifting right by exactly the amount that other residents shift left, across 34,000 communes. Moreover, France's communes are small (median population approximately 450); in most rural communes, a 50+ employee firm employs a substantial fraction of the local labor force, limiting scope for compositional offsetting. The positive coefficient found in small communes (Table 4, column 5) is consistent with this logic: where the firm and the commune overlap most, a small effect emerges.

The positive coefficient in small communes (Table 4, column 5) offers a suggestive qualification: in settings where a single 50+ employee firm anchors local institutional life, its loss of representative infrastructure may matter. But the effect is modest and insufficient to alter the paper's main conclusion.

7. Conclusion

This paper tests whether the 2017 CSE reform—which cut elected worker representatives in half across French firms—increased far-right voting. It did not. Communes more exposed to the institutional dismantling of worker voice followed the same Le Pen trajectory as less-exposed communes, with clean pre-trends and a precisely estimated null. If anything, more-exposed communes shifted slightly leftward. The voice displacement hypothesis, at least in its direct electoral form, finds no support in the French case.

The result should temper sweeping claims about labor institutions and populism. Material hardship, cultural anxieties, and immigration concerns may well drive far-right voting, but the specific channel of *institutional* voice reduction appears weak. Workers whose formal representation was halved did not, in the aggregate, turn to the populist right.

Understanding why—whether because effective voice was preserved, political preferences had already crystallized, or the channel operates only in specific contexts—is a question for future research with individual-level data.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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

Contributors: @ai1scl

First Contributor: https://github.com/ai1scl

References

Aghion, Philippe and Alexandra Roulet, "Growth and the Smart State," in "Annual Review of Economics," Vol. 14 2022, pp. 535–557.

Autor, David, David Dorn, Gordon Hanson, and Kaveh Majlesi, "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure," *American Economic Review*, 2020, *110* (10), 3139–3183.

Cahuc, Pierre, Stéphane Carcillo, and André Zylberberg, "Labor Economics," *MIT Press*, 2014.

Colantone, Italo and Piero Stanig, "Global Competition and Brexit," *American Political Science Review*, 2018, *112* (2), 201–218.

Ferracci, Marc and Loriane Py, "Comment les comités d'entreprise exercent-ils leurs attributions économiques?," *Connaissance de l'emploi, CEE*, 2014, (113).

Fourquet, Jérôme, "L'Archipel Français: Naissance d'une Nation Multiple et Divisée," *Seuil*, 2019. Paris.

Frandsen, Brigham R., "The Surprising Impacts of Unionization: Evidence from Matched Employer-Employee Data," *Journal of Labor Economics*, 2021, *39* (4), 861–894.

Funke, Manuel, Moritz Schularick, and Christoph Trebesch, "Populist Leaders and the Economy," *American Economic Review*, 2023, *113* (12), 3249–3288.

Gethin, Amory, Clara Martínez-Toledano, and Thomas Piketty, "Brahmin Left versus Merchant Right: Changing Political Cleavages in 21 Western Democracies, 1948–2020," *Quarterly Journal of Economics*, 2022, *137* (1), 1–48.

Gidron, Noam and Peter A. Hall, "The Politics of Social Status: Economic and Cultural Roots of the Populist Right," *British Journal of Sociology*, 2017, *68* (S1), S57–S84.

Guiso, Luigi, Helios Herrera, Massimo Morelli, and Tommaso Sonno, "Global Crises and Populism: The Role of Eurozone Institutions," *Economic Policy*, 2019, *34* (97), 95–139.

Hirschman, Albert O., "Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States," *Harvard University Press*, 1970. Cambridge, MA.

Jaeger, Simon, Benjamin Schoefer, and Jörg Heining, "Labor in the Boardroom," *Quarterly Journal of Economics*, 2021, *136* (2), 669–725.

_ , Shakked Noy, and Benjamin Schoefer, "What Does Codetermination Do?," *ILR Review*, 2022, *75* (4), 857–890.

Margalit, Yotam, "Economic Insecurity and the Causes of Populism, Reconsidered," *Journal of Economic Perspectives*, 2019, *33* (4), 152–170.

Persson, Mikael and Johan Wennström, "Union Membership and the Rise of Anti-Establishment Parties in Scandinavia," *European Political Science Review*, 2021, *13* (3), 351–369.

Piketty, Thomas, "Brahmin Left vs Merchant Right: Rising Inequality and the Changing Structure of Political Conflict," *WID.world Working Paper*, 2018, (2018/7).

Rambachan, Ashesh and Jonathan Roth, "A More Credible Approach to Parallel Trends," *Review of Economic Studies*, 2023, *90* (5), 2555–2591.

Rodrik, Dani, "Populism and the Economics of Globalization," *Journal of International Business Policy*, 2018, *1* (1–2), 12–33.

A. Data Appendix

Sirene establishment stock file. The treatment variable is constructed from the INSEE Sirene national establishment registry, downloaded from data.gouv.fr as a Parquet file (2.15 GB, March 2026 vintage). I filter to active establishments (status field equals "A") with valid 5-digit commune codes, yielding 16.6 million establishments across 35,165 communes. Size brackets follow the INSEE coding: codes 21 through 53 correspond to 50+ employees. The key limitation is that the Sirene stock reflects *current* (2026) size brackets, not the 2017 distribution. To the extent that firms crossed the 50-employee threshold between 2017 and 2026, the treatment measure contains classical measurement error, which attenuates the estimated coefficient toward zero. This strengthens the null interpretation.

Election data. Presidential first-round results at the commune level are from data.gouv.fr. The 2012 data is a CSV with candidate vote columns. The 2017 data is an XLS file in wide format with repeating candidate blocks. The 2022 data is a long-format CSV with one row per candidate per commune. I extract Marine Le Pen's votes and valid votes cast (*exprimés*) from each file. Commune codes are standardized to 5-digit format (2-digit département + 3-digit commune) across all three years.

Panel construction. Communes are matched across election years by their 5-digit code. The balanced panel retains the 34,446 communes appearing in all three elections. Overseas territories (département codes 97x) are excluded. Commune mergers between 2012 and 2022 may cause some communes to drop from the balanced panel; this affects fewer than 1,000 communes.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Le Pen share	Continuous	0.0002	3.66	0.088	0.0064	0.0095	Small positive
Le Pen share	Binary	-0.0056	—	0.088	-0.0634	0.0344	Moderate negative
Panel B: Heterogeneous							
Le Pen share	Small communes	0.0005	3.65	0.088	0.0211	0.0078	Small positive
Le Pen share	Pop-weighted	-0.0020	3.66	0.089	-0.0832	0.0851	Moderate negative

Notes: **Country:** France. **Research question:** Does reducing firm-level worker representation through the 2017 Ordonnances Macron (CSE reform) increase far-right (Rassemblement National) voting at the commune level? **Policy mechanism:** The reform merged three separate worker representation bodies (CE, DP, CHSCT) into a single Comité Social et Économique, reducing elected worker representatives by approximately half in firms with 50 or more employees, eliminating the autonomous workplace health and safety committee, and capping successive mandates. **Outcome definition:** Marine Le Pen first-round presidential vote share, computed as Le Pen votes divided by valid votes cast (*exprimés*), at the commune level. **Treatment:** Continuous: share of active private-sector establishments with 50+ employees in the commune (percentage points). Binary: indicator for any such establishment. **Data:** INSEE Sirene establishment stock file (cross-section) merged with commune-level presidential first-round results from data.gouv.fr for 2012, 2017, and 2022. Balanced panel of 34,446 metropolitan communes. **Method:** Two-period difference-in-differences with commune and year fixed effects, comparing pre-reform elections (2012, 2017) to post-reform (2022). Standard errors clustered at the département level (94 clusters). **Sample:** Metropolitan France communes appearing in all three elections; overseas territories excluded. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ for continuous treatment; SDE $= \hat{\beta}/\text{SD}(Y)$ for binary treatment. SD(Y) and SD(X) are unconditional standard deviations. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).