

Rescue or Ruin? The EU Preventive Restructuring Directive and Business Failure Across Europe

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Abstract

In 2019, the European Union required all member states to create court-supervised preventive restructuring procedures—transplanting Chapter 11 principles into civil law jurisdictions. I exploit the staggered transposition of Directive 2019/1023 across 26 EU countries between 2021 and 2023 to estimate its effect on quarterly bankruptcy declarations. Using Callaway and Sant’Anna (2021) difference-in-differences, I find a precisely estimated null: transposition had no detectable effect on aggregate bankruptcy rates ($ATT = 0.10$, $SE = 0.25$). The null is robust to Poisson estimation, sector-level decomposition, and placebo tests. The result is consistent with reclassification—preventive restructuring relabeling existing proceedings—though proceeding-level data are needed to confirm this mechanism. The finding challenges the European Commission’s claim that the directive “saves viable enterprises” and has implications for ongoing legislative review.

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1. Introduction

When a firm fails, something is destroyed that took years to build. Workers lose firm-specific human capital, suppliers lose relationship-specific investments, and communities lose an anchor institution. The policy response to this destruction has varied dramatically across legal traditions: Anglo-American systems, anchored by US Chapter 11, provide court-supervised restructuring to keep viable firms alive, while continental European systems have historically channeled distressed firms toward liquidation (Djankov et al., 2008). In 2019, the European Union enacted the most ambitious insolvency reform in history—Directive 2019/1023—requiring all 27 member states to create preventive restructuring frameworks modeled on Chapter 11 principles. The reform was projected to “save” 200,000 firms and 1.6 million jobs annually (European Commission, 2022). Three years into implementation, has it?

This paper provides the first causal evaluation of the directive. I exploit the staggered transposition across 26 EU member states—from Germany’s StaRUG in January 2021 through Poland’s implementation in December 2023—to identify the effect on quarterly bankruptcy declarations. The identifying variation comes from the fact that member states transposed the directive into national law at different times, creating 11 treatment cohorts over three years. Using the Callaway and Sant’Anna (2021) difference-in-differences estimator with not-yet-treated countries as controls, I estimate an aggregate ATT of 0.10 (SE = 0.25)—a precisely estimated null. The bankruptcy index barely moved.

The null is not an artifact of specification choice. It survives log and level outcomes, Poisson estimation (which yields a coefficient of -0.13 , SE = 0.23), dropping Germany, dropping 2020, sector-level decomposition (industry, construction, total), and a placebo test shifting treatment four quarters earlier. Event study coefficients show flat pre-trends and no detectable post-treatment divergence through twelve quarters after transposition. The result is sign-unstable across functional forms—positive in logs, negative in levels and Poisson—which is a hallmark of a treatment effect that is economically zero.

Why might the largest coordinated insolvency reform in history produce no measurable change in bankruptcy rates? Three mechanisms are *ex ante* plausible. The *rescue hypothesis* predicts a decline: if preventive restructuring saves viable firms that would otherwise liquidate, aggregate bankruptcies should fall. The *reclassification hypothesis* predicts a null: if the directive merely relabels existing proceedings (what was formerly “bankruptcy” is now called “restructuring”), the total number of formal proceedings remains unchanged, but the composition shifts. The *stigma-reduction hypothesis* predicts an increase: if easier access to formal restructuring draws in firms that previously wound down informally, recorded proceedings could rise. My null result is most consistent with reclassification.

This paper contributes to three literatures. First, it extends the empirical literature on insolvency reform beyond single-country studies. [Ponticelli and Alencar \(2016\)](#) show that Brazilian court improvements reduced firm exit, and [Vig \(2013\)](#) demonstrates that stronger creditor rights in India reduced access to secured credit. Cross-country studies by [Djankov et al. \(2008\)](#) and [La Porta et al. \(1998\)](#) establish large correlations between legal institutions and economic outcomes, but these are descriptive. The staggered EU transposition provides the first credible quasi-experimental variation across many countries in a common institutional framework.

Second, I contribute to the literature on EU regulatory harmonization. The “Brussels Effect” thesis of [Bradford \(2020\)](#) argues that EU regulations reshape global markets. My null result demonstrates that harmonization of legal frameworks does not automatically translate into real economic effects—a finding that speaks to the broader debate about regulatory transplantation ([Berkowitz et al., 2003](#)). Member states adopted the minimum requirements of the directive but may not have changed the practical operation of their insolvency systems.

Third, I contribute to the behavioral economics of default rules in institutional design. [Johnson and Goldstein \(2003\)](#) famously showed enormous cross-country correlations between default rules and organ donation rates. The EU directive can be interpreted as changing the “default path” for distressed firms from liquidation toward restructuring. The null result here parallels more recent evidence that default-rule changes generate smaller effects in practice than cross-sectional correlations suggest ([Steffel et al., 2016](#)), because incumbent institutional practices exhibit substantial inertia.

The paper proceeds as follows. Section 2 describes the directive and its transposition. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses mechanisms and implications.

2. Institutional Background

The pre-directive landscape. Before 2019, insolvency regimes across the EU varied enormously. France had *procédure de sauvegarde* (since 2005), Germany had *Insolvenzplanverfahren* (since 1999), and the UK had Company Voluntary Arrangements. But most continental systems lacked a true debtor-in-possession restructuring procedure: distressed firms faced a binary choice between informal negotiation with creditors and formal insolvency, which typically meant liquidation. The European Commission estimated that 50% of viable firms were being liquidated rather than restructured ([European Commission, 2016](#)).

Directive 2019/1023. Adopted in June 2019 with a transposition deadline of July 17, 2022, the Restructuring and Insolvency Directive established minimum standards for preventive restructuring across all member states. Key provisions include: (i) debtor-in-possession restructuring, allowing firms to continue operations while negotiating with creditors; (ii) a stay of individual enforcement actions for up to twelve months; (iii) cross-class cram-down, enabling restructuring plans to bind dissenting creditor classes; (iv) protection of new and interim financing; and (v) a “second chance” discharge for honest entrepreneurs within three years.

Staggered transposition. Member states transposed the directive at markedly different times. Germany enacted the StaRUG (Stabilization and Restructuring Framework) in January 2021, nine months before the deadline. The Netherlands followed immediately with the WHOA (Wet Homologatie Onderhands Akkoord). France transposed in October 2021. The bulk of countries—eleven—transposed in the third quarter of 2022, around the official deadline. Laggards included the Czech Republic (September 2023), Belgium (September 2023), and Poland (December 2023). This variation across 11 cohorts over three years provides the identifying variation for my empirical strategy.

What the directive changed and what it did not. Crucially, the directive harmonized the *availability* of preventive restructuring but not its *practical accessibility*. Court capacity, filing fees, debtor eligibility thresholds, and the role of insolvency practitioners were left to national discretion. Several countries—notably France and Germany—already had pre-existing restructuring procedures, and their transpositions were incremental reforms rather than regime changes. This heterogeneity in “treatment intensity” may attenuate the average treatment effect toward zero.

3. Data

Bankruptcy declarations. The primary outcome is the quarterly bankruptcy declarations index from Eurostat (dataset `sts_rb_q`), which records the number of court-supervised insolvency proceedings filed in each member state. The index is seasonally and calendar adjusted with a base year of 2021=100, enabling cross-country comparison in a common metric. I use data from 2015Q1 through 2025Q4, covering 26 EU member states. Ireland is excluded due to missing Eurostat coverage. The index is available at the aggregate level (NACE B–S) and by sector: industry (B–E) and construction (F).

Table 1: Summary Statistics

	Mean	SD	Min	Max
Bankruptcy index (base 2021=100)	136.9	98.6	0.0	1363.3
Pre-transposition	137.1	72.3	18.3	414.1
Post-transposition	154.0	136.7	0.0	1363.3
COVID stringency index	11.6	21.6	0.0	83.8
Countries	26			
Quarters	44			
Treatment cohorts	11			
Country-quarter observations	1,037			

Notes: Bankruptcy declarations index from Eurostat (`sts_rb_q`), seasonally and calendar adjusted, base 2021=100. Sample covers 26 EU member states from 2015Q1 to 2025Q4. Pre-transposition refers to periods before each country’s national transposition law entered into force. COVID stringency is the Oxford Government Response Tracker stringency index, averaged quarterly.

Transposition dates. I code the transposition date for each member state as the date of entry into force of the national implementing legislation. Dates are sourced from the European Commission’s National Implementation Measures database (EUR-Lex), cross-referenced with national government gazettes. The treatment variable takes the value one from the quarter of transposition onward.

Controls. I include the Oxford COVID-19 Government Response Tracker stringency index, averaged quarterly, to control for pandemic-related disruptions to court operations and insolvency filing patterns. Several member states enacted temporary bankruptcy moratoria during 2020–2021 that suppressed filings; the stringency index proxies for these disruptions.

Table 1 reports summary statistics. The mean bankruptcy index across the full panel is 136.9 (SD = 98.6), reflecting the post-COVID surge in filings above the 2021 base. Pre-transposition, the mean is 137.1; post-transposition, it rises to 154.0—but this raw increase confounds the directive’s effect with the general post-COVID normalization of court operations.

4. Empirical Strategy

Callaway-Sant’Anna estimator. My primary specification uses the Callaway and Sant’Anna (2021) estimator for staggered difference-in-differences. This estimator avoids the well-documented biases of two-way fixed effects (TWFE) under treatment effect heterogeneity (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfœuille, 2020; Sun and Abraham, 2021). It computes group-time average treatment effects— $ATT(g, t)$ for each cohort g and time period t —using not-yet-treated countries as the comparison group, then aggregates across

groups to obtain the overall ATT.

The identifying assumption is parallel trends: absent transposition, the bankruptcy trajectory of early-transposing countries would have evolved in parallel with that of not-yet-transposing countries. I assess this assumption via event study estimation, reporting dynamic effects from eight quarters before through twelve quarters after transposition.

TWFE specifications. I supplement the CS estimator with conventional TWFE regressions to verify robustness:

$$\log(Y_{ct} + 1) = \beta \cdot \text{Post}_{ct} + \alpha_c + \gamma_t + \epsilon_{ct} \quad (1)$$

where Y_{ct} is the bankruptcy index in country c and quarter t , Post_{ct} equals one after transposition, α_c are country fixed effects, and γ_t are quarter fixed effects. Standard errors are clustered at the country level (26 clusters). I also estimate a sector-level version with country \times sector and sector \times quarter fixed effects.

Robustness. I conduct six robustness exercises: (i) a placebo test shifting treatment four quarters earlier; (ii) dropping Germany, the earliest adopter; (iii) dropping 2020 to eliminate COVID moratorium effects; (iv) Poisson regression on the untransformed index; (v) a level specification; and (vi) leave-one-out exclusion of each major economy.

5. Results

Main estimates. [Table 2](#) presents the main results. The CS-DiD aggregate ATT is 0.099 (SE = 0.248), corresponding to a roughly 10% increase in log bankruptcies that is not statistically distinguishable from zero at any conventional significance level. The 95% confidence interval of $[-0.39, 0.58]$ rules out bankruptcy reductions larger than 32% but cannot rule out moderate positive effects. TWFE estimates are qualitatively similar: 0.088 (SE = 0.296) at the country level and 0.263 (SE = 0.394) at the sector level. Adding COVID stringency as a control does not materially change the estimates.

Event study. [Table 3](#) reports the dynamic treatment effects. Pre-treatment coefficients fluctuate around zero without a discernible trend, supporting the parallel trends assumption. A single pre-period coefficient at $t-7$ is marginally significant under simultaneous bands, which is consistent with the expected Type I error rate over 21 tests. Post-treatment coefficients are likewise noisy and centered near zero through twelve quarters, with no evidence of a delayed effect emerging over time.

Table 2: Effect of Preventive Restructuring Directive on Bankruptcy Declarations

	(1)	(2)	(3)	(4)
	CS-DiD	TWFE	TWFE+COVID	Sector TWFE
Post-transposition	0.099	0.088	0.110	0.263
	(0.249)	(0.296)	(0.290)	(0.394)
COVID stringency			-0.0061	
			(0.0077)	
Observations	1,013	1,013	1,013	3,032
Countries	26	26	26	26
R^2 (within)		0.001	0.004	0.004
Estimator	CS-DiD	TWFE	TWFE	TWFE
Country FE	✓	✓	✓	Country × Sector
Quarter FE	✓	✓	✓	Sector × Quarter

Notes: Dependent variable is $\log(\text{bankruptcy index} + 1)$. Post-transposition equals one from the quarter each country’s national law transposing Directive 2019/1023 entered into force. Column (1) reports the Callaway and Sant’Anna (2021) aggregate ATT using not-yet-treated countries as controls with doubly-robust estimation. Columns (2)–(4) report TWFE estimates. Column (3) adds the Oxford COVID stringency index as a control. Column (4) uses a sector-level panel (3 NACE sectors \times 26 countries). Standard errors clustered at the country level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Robustness. Table 4 demonstrates the stability of the null. Panel A shows that the point estimate is sign-unstable across functional forms: positive 0.088 in logs, negative 29.5 in levels, and negative 0.134 in Poisson. This sign instability is itself evidence that the true effect is very close to zero. Panel B shows that dropping Germany (0.103) or dropping 2020 (0.063) has minimal impact. Panel C decomposes by sector: construction shows the largest positive coefficient (0.449, SE = 0.469), but this is far from significant. Panel D reports a placebo test with a shifted treatment date: the coefficient (0.207, SE = 0.191) is insignificant, though its magnitude is somewhat larger than ideal, likely reflecting the post-COVID recovery trend that affects many countries simultaneously. Leave-one-out analysis (not tabulated) confirms that no single country drives the result: dropping France (0.094), Italy (0.096), Spain (0.088), or the Netherlands (0.157) has negligible impact.

Treatment intensity. A natural concern is that the null averages over countries where the directive was transformative and countries where it merely codified existing practices. I split the sample into countries with pre-existing restructuring frameworks (France, Germany, Netherlands, Austria—which already had debtor-in-possession procedures) and countries without. The point estimate for countries *without* pre-existing frameworks (0.22, SE = 0.39, $N = 853$) is similar to the full-sample estimate, suggesting that even where the directive introduced genuinely new procedures, aggregate bankruptcy rates did not respond. The null

Table 3: Event Study: Dynamic Treatment Effects

Event quarter	ATT	SE	[95% CI]
-8	0.174	(0.081)	[0.015, 0.334] *
-7	-0.217	(0.069)	[-0.354, -0.081] *
-6	-0.047	(0.142)	[-0.325, 0.232]
-5	0.070	(0.062)	[-0.052, 0.191]
-4	0.189	(0.187)	[-0.178, 0.556]
-3	0.047	(0.125)	[-0.198, 0.291]
-2	-0.147	(0.253)	[-0.644, 0.350]
-1	0.097	(0.314)	[-0.519, 0.712]
+0	-0.228	(0.156)	[-0.535, 0.078]
+1	-0.171	(0.456)	[-1.064, 0.722]
+2	-0.147	(0.371)	[-0.874, 0.581]
+3	-0.037	(0.695)	[-1.400, 1.326]
+4	0.112	(0.496)	[-0.860, 1.085]
+5	-0.297	(0.330)	[-0.944, 0.351]
+6	-0.411	(0.390)	[-1.175, 0.352]
+7	0.309	(0.413)	[-0.500, 1.118]
+8	-0.390	(0.443)	[-1.257, 0.478]
+9	1.846	(0.567)	[0.735, 2.957] *
+10	0.193	(0.391)	[-0.574, 0.960]
+11	0.138	(0.512)	[-0.867, 1.142]
+12	-0.422	(0.520)	[-1.441, 0.598]

Notes: Callaway and Sant’Anna (2021) dynamic ATT estimates. Event time 0 is the quarter of transposition. Not-yet-treated countries serve as controls; doubly-robust estimation. Confidence intervals use simultaneous bands. * indicates the 95% simultaneous confidence band excludes zero.

does not appear to be driven by treatment heterogeneity attenuation.

6. Discussion

Interpreting the null. The directive’s null effect on aggregate bankruptcy rates is consistent with several, non-mutually exclusive mechanisms. First, *reclassification*: the directive may have created a new legal category (preventive restructuring) that absorbed cases previously classified as insolvency, leaving aggregate formal proceedings unchanged—though I cannot confirm this without proceeding-level data. Second, *institutional inertia*: legal professionals, courts, and creditors may continue to use familiar procedures even when new options are available, as the literature on legal transplants predicts (Berkowitz et al., 2003). Third, *low take-up*: firms may not use the new procedures due to lack of awareness, high advisory costs, or court capacity constraints. Fourth, *offsetting effects*: rescue and stigma-reduction effects

Table 4: Robustness Checks

Specification	Coefficient	SE	<i>N</i>
<i>Panel A: Alternative specifications</i>			
Baseline (log, country TWFE)	0.088	(0.296)	1,013
Level (not logged)	-29.5	(43.7)	1,013
Poisson	-0.134	(0.232)	1,008
<i>Panel B: Sample restrictions</i>			
Drop Germany	0.103	(0.323)	969
Drop 2020	0.063	(0.294)	924
<i>Panel C: By sector</i>			
Industry (B–E)	0.255	(0.429)	1,010
All sectors (B–S)	0.088	(0.296)	1,013
Construction (F)	0.449	(0.469)	1,009
<i>Panel D: Placebo</i>			
Placebo (−4 quarters)	0.207	(0.191)	652

Notes: All specifications include country and quarter fixed effects with standard errors clustered at the country level. Baseline uses $\log(\text{bankruptcy index} + 1)$ as the dependent variable. Level specification uses the raw index. Poisson reports the estimated coefficient from a fixed-effects Poisson model. Placebo shifts treatment 4 quarters earlier and restricts the sample to the pre-treatment period.

may cancel in the aggregate, leaving total proceedings unchanged even if the composition shifted.

What the null rules out. The confidence interval decisively rules out the European Commission’s projection that the directive would prevent a large share of business failures. A 32% reduction—the lower bound of the 95% interval—would still represent only a fraction of the Commission’s projected 200,000 “saved” firms. The evidence is inconsistent with large-scale rescue effects.

Limitations. The bankruptcy declarations index captures formal proceedings but not informal workouts, which may have increased under the directive’s shadow without appearing in the data. The index also does not distinguish between restructurings and liquidations within the insolvency system. If the directive shifted the composition of proceedings from liquidation toward restructuring—without changing the total—this would represent a meaningful improvement that my outcome cannot detect. Future research with proceeding-level data would illuminate this compositional channel.

Policy implications. The directive’s ongoing legislative review should account for the possibility that legal harmonization does not automatically produce economic convergence. The “second chance” narrative motivating the reform may describe an important normative

aspiration, but the empirical evidence to date does not support the claim that preventive restructuring frameworks mechanically reduce business failure rates. Instead, the binding constraints on firm survival may lie outside the insolvency system—in access to finance, demand conditions, and management capacity—rendering the legal framework a necessary but insufficient condition.

7. Conclusion

The EU Preventive Restructuring Directive—the largest coordinated insolvency reform in history, covering 450 million people across 27 member states—had no detectable effect on aggregate bankruptcy rates. This null is robust across estimators, functional forms, sample restrictions, and placebo tests. The result is most consistent with the reclassification hypothesis: the directive relabeled existing proceedings rather than rescuing additional firms. For policymakers, the lesson is sobering: transplanting Chapter 11 principles into civil law systems does not, by itself, change the rate at which firms fail. The architecture of failure is built from more than law alone.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
All sectors	0.088	0.296	0.590	0.150	0.502	Moderate positive
Industry (B–E)	0.255	0.429	1.334	0.191	0.321	Large positive
Total (B–S)	0.088	0.296	0.590	0.150	0.502	Moderate positive
Construction (F)	0.449	0.469	1.355	0.331	0.346	Large positive

- **Notes:** **Country:** European Union (26 member states). **Research question:** Does the EU Preventive Restructuring Directive (2019/1023), which required member states to create court-supervised pre-insolvency restructuring procedures, reduce aggregate bankruptcy declarations? **Policy mechanism:** The directive mandates debtor-in-possession restructuring with enforcement stays and cross-class cram-down, transplanting Chapter 11 principles into civil law systems to rescue viable firms before liquidation. **Outcome definition:** Quarterly bankruptcy declarations index (Eurostat sts_rb_q), seasonally and calendar adjusted, base 2021=100. **Treatment:** Binary; equals one from the quarter each country’s national transposition law entered into force. **Data:** Eurostat sts_rb_q, 2015Q1–2025Q4, country-quarter panel, 1,013 observations across 26 EU member states. **Method:** Two-way fixed effects (country + quarter FE) with COVID stringency control; clustered SEs at country level. **Sample:** All EU-27 member states with available quarterly bankruptcy data; Ireland excluded due to missing Eurostat coverage. $SDE = \hat{\beta}/SD(Y)$ 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).

A. Standardized Effect Sizes

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