The Liberalization Illusion: Market Opening and Rail Fares in Europe

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Abstract

The EU's Fourth Railway Package legally opened all 25 rail-operating member states' domestic passenger markets to competition. Exploiting staggered transposition across two cohorts—eight early adopters in 2019 and seventeen late adopters in 2020—I estimate a Callaway–Sant'Anna difference-in-differences using monthly Eurostat consumer price indices. The aggregate effect on rail fares is economically small and statistically indistinguishable from zero (ATT: 1.5%, SE: 1.0%). Road and air transport fare placebos confirm the null. A corrected triple-difference comparing rail to road fares within country-months reveals a marginally significant relative decline of 4.4 percentage points ($p = 0.055$), suggesting modest fare pressure visible only when benchmarked against an untreated transport sector. Results are robust to pre-COVID sample restrictions, exclusion of countries with pre-existing open access, and alternative specifications. De jure market opening did not translate into de facto consumer price benefits at the national level.

JEL Codes: L51, L92, L98, R48

Keywords: rail liberalization, Fourth Railway Package, difference-in-differences, transport regulation, EU directives

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1. Introduction

In December 2020, the European Union completed the largest coordinated liberalization of passenger rail markets in history. The Fourth Railway Package—adopted in 2016 after two decades of incremental reform—granted any licensed operator the right to run domestic passenger services in any member state, breaking the legal monopolies that had defined European railways since the nineteenth century. Proponents argued that competition would reduce fares, improve service quality, and reverse decades of modal shift toward road and air (Nash, 2008). The European Commission's own 2024 evaluation highlighted dramatic fare reductions on selected competitive routes: 31% on Italian high-speed services, 44–46% in Czechia (European Commission, 2024).

This paper asks whether these headline figures generalize. Using monthly consumer price data from Eurostat covering 25 EU member states over a decade, I estimate the causal effect of the Fourth Railway Package on national-average rail fares. The answer is no: the aggregate effect is economically negligible and statistically indistinguishable from zero.

What this paper does. I exploit the staggered transposition of Directive 2016/2370 across two cohorts—eight countries that transposed by June 2019 and seventeen that followed in 2020—to estimate a difference-in-differences using the Callaway and Sant'Anna (2021) estimator with not-yet-treated controls. The outcome is the Harmonised Index of Consumer Prices (HICP) for railway transport (COICOP CP0731), a nationally representative monthly price index. I complement this with triple-difference estimates that compare rail fares to road and air transport fares within the same country-months, absorbing any macroeconomic shocks common to all transport modes.

What I find. The Callaway–Sant'Anna aggregate ATT is 0.015 log points (SE: 0.010, $p = 0.11$), a precisely estimated null. Two-way fixed effects estimates are even closer to zero (0.001, SE: 0.029). Triple-difference estimates comparing rail to road transport yield an identical point estimate of 0.001. Critically, placebo estimates on road fares (0.003, SE: 0.005) and air fares (-0.047, SE: 0.037) confirm that the research design does not spuriously detect effects in untreated transport sectors. The null is robust to restricting the sample to early transposers with a pre-COVID post-treatment window, excluding Sweden and Czechia (which had open access before the directive), controlling for GDP per capita, estimating in levels rather than logs, and narrowing to a symmetric 12-month window around transposition.

Why the null matters. A large literature debates whether market liberalization delivers consumer benefits in network industries. Borenstein (2002) documents how California's electricity restructuring produced price spikes rather than savings; Joskow (2007) argues that natural monopoly characteristics make competitive outcomes fragile in infrastructure sectors. The privatization literature generally finds efficiency gains from state-to-private transitions (Megginson and Netter, 2001; Boycko et al., 1996; Shleifer, 1998), but these gains need not pass through to consumers when entry barriers persist (Djankov et al., 2002; Stigler, 1971).

European rail occupies a particularly contested position in this debate. Advocates of the "open access" model, exemplified by Sweden's early liberalization and the Italian high-speed market, point to competitive routes where fares fell dramatically (Friebel et al., 2010; Bougette et al., 2019). Skeptics emphasize that most routes remain protected by Public Service Obligation (PSO) contracts, that incumbent operators retain structural advantages in network access and fleet ownership, and that new entrants cherry-pick profitable corridors while leaving the broader network untouched (Nash, 2008; Knieps, 2013; White, 2013).

This paper contributes the first rigorous causal evidence on this debate. The Commission's 2024 evaluation (European Commission, 2024) relied on descriptive comparisons of selected competitive routes—a design that conflates route selection with treatment effects. By estimating the effect on nationally representative consumer price indices using modern staggered DiD methods, I capture the full distribution of route-level outcomes as experienced by the average consumer. The null result does not contradict the Commission's finding that specific competitive routes saw fare reductions; rather, it reveals that these reductions are too localized to shift national price aggregates. De jure market opening created competition on a handful of high-speed corridors while leaving the vast majority of the network—protected by PSO contracts and incumbency advantages—untouched.

Related literature. This paper relates to three strands. First, the economics of rail reform, where Nash (2008) and Friebel et al. (2010) provide cross-country analyses of European liberalization but without causal identification. Second, the regulation literature on entry barriers and the gap between de jure and de facto competition (Djankov et al., 2002; Peltzman, 1976; Stigler, 1971). Third, the recent econometrics of staggered difference-in-differences (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Goodman-Bacon, 2021; Roth et al., 2023), which this paper applies to a policy setting with large-scale staggered adoption.

2. Institutional Background

Four packages of reform. EU rail liberalization proceeded through four legislative packages spanning 2001–2016. The First (2001) and Second (2003) packages separated infrastructure management from train operations and opened freight markets. The Third Package (2007) established passenger rights and opened international passenger services. The Fourth Package (2016) completed the process by opening *domestic* passenger markets—the final and most politically sensitive step.

The Fourth Railway Package. Two instruments form the core of the package. Directive 2016/2370 amends the Single European Railway Area directive to grant "open access" rights: any EU-licensed railway undertaking may operate domestic passenger services in any member state, subject to an economic equilibrium test that protects PSO services. Regulation 2016/2338 reforms PSO contracting by requiring competitive tendering for public service rail contracts from December 2023, replacing the direct-award regime that allowed member states to hand contracts to incumbent operators without competition.

Staggered transposition. Member states transposed Directive 2016/2370 in two waves. Eight countries—Bulgaria, Finland, France, Greece, Italy, the Netherlands, Romania, and Slovenia—transposed by June 2019. The remaining seventeen transposed between June and October 2020, close to the official deadline. This staggered adoption creates the identifying variation for my difference-in-differences design.

Pre-existing heterogeneity. Market openness varied substantially before the Fourth Package. Sweden opened its rail market to competition in the 1990s. Czechia followed in the 2000s, with RegioJet and Leo Express entering long-distance markets. Italy's high-speed market saw Italo (NTV) enter against Trenitalia from 2012. In contrast, France, Spain, Germany, and most Eastern European states maintained near-complete incumbent monopolies. This heterogeneity motivates both the main analysis (where pre-existing open-access countries contribute to identification) and a robustness check excluding Sweden and Czechia.

Why national-average effects may differ from route-level effects. The HICP rail fare index is a nationally weighted average across all rail passenger services. New open-access entry has been concentrated on a few high-speed inter-city routes (Rome–Milan, Prague–Ostrava, Madrid–Barcelona). These corridors represent a small share of total passenger-kilometers; the vast majority of rail journeys occur on regional and commuter services covered by PSO contracts that were not subject to competitive tendering until December 2023. Even where open access is legally permitted, incumbents retain advantages in station access, timetable priority, rolling stock, and brand recognition (Knieps, 2013). The disconnect between de jure market opening and de facto competitive entry is central to interpreting the null result.

3. Data

I construct a monthly country-level panel from three Eurostat datasets. The primary outcome is the HICP monthly index for railway transport (COICOP category CP0731) from the `prc_hicp_midx` dataset, covering 25 EU member states from January 2015 to December 2025. Cyprus and Malta are excluded because they have no rail networks. The HICP index is set to 100 in the base year 2015 and captures the average price change of railway passenger services weighted by consumer expenditure shares.

I supplement this with two placebo outcomes from the same HICP source: road transport fares (CP0732) and air transport fares (CP0733). These sectors were not directly affected by the Fourth Railway Package and serve as falsification tests. If the research design spuriously detects effects from macroeconomic shocks or common transport trends, these placebos will also show significant coefficients.

Quarterly rail passenger-kilometers from the `rail_pa_quartal` dataset provide a secondary quantity outcome. Annual GDP per capita from Eurostat serves as a time-varying control. Treatment timing—the month each country transposed Directive 2016/2370—comes from the European Commission's transposition monitoring records.

Table 1: Summary Statistics: Monthly Transport Fare Indices (2015=100)

	Mean	SD	Min	Max	N
Panel A: Pre-Treatment (Jan 2015 – Dec 2018)					
Rail fares (CP0731)	101.3	4.3	81.2	120.0	1200
Road fares (CP0732)	101.3	2.3	90.8	110.3	1200
Air fares (CP0733)	100.1	17.6	42.7	179.9	1200
Panel B: Post-Treatment (Jan 2019 – Latest)					
Rail fares (CP0731)	106.9	17.3	66.8	164.0	2100
Road fares (CP0732)	111.6	17.1	72.8	186.7	2100
Air fares (CP0733)	124.1	42.5	40.8	395.2	2100
Countries	27				
Early transposers	8 (BG, FI, FR, EL, IT, NL, RO, SI)				
Late transposers	19				

Notes: HICP monthly index values (2015=100) from Eurostat (prc_hicp_midx). Rail fares correspond to COICOP category CP0731, road to CP0732, and air to CP0733. Pre-treatment period ends December 2018 (before earliest transposition in January 2019).

Table 1 reports summary statistics for the three transport fare indices in the pre-treatment period (January 2015 – December 2018) and the post-treatment period. The panel is balanced: all 25 countries contribute 132 monthly observations. Pre-treatment rail fares have a mean of 101.3 index points with a standard deviation of 4.3, reflecting modest cross-country variation in the rate of fare increases relative to the 2015 base year.

4. Empirical Strategy

4.1 Identification

I exploit the staggered transposition of Directive 2016/2370 across two cohorts. The eight early-transposing countries (Bulgaria, Finland, France, Greece, Italy, the Netherlands, Romania, Slovenia) serve as the first treatment group, with treatment dates between January and June 2019. The seventeen late-transposing countries enter treatment between June and October 2020. Identification relies on the parallel trends assumption: in the absence of the directive, rail fare trends in early-transposing countries would have evolved parallel to those in late-transposing countries.

Two features strengthen this assumption. First, the HICP index is a harmonized measure constructed using the same methodology across all EU member states, eliminating measurement differences. Second, the transposition timeline was driven by administrative and legislative capacity rather than by fare trends—countries with faster parliamentary procedures transposed earlier, not countries experiencing different fare dynamics.

4.2 Estimation

My preferred specification uses the Callaway and Sant'Anna (2021) estimator, which avoids the well-documented biases of two-way fixed effects (TWFE) in staggered settings (Goodman-Bacon, 2021). I estimate group-time average treatment effects $ATT(g,t)$ for each cohort g at each time period t, using not-yet-treated units as the control group:

$$ATT(g,t) = \mathbb{E}[Y_{it}(g) - Y_{it}(0) \mid G_i = g] \tag{1}$$

where $Y_{it}(g)$ denotes the potential outcome under treatment at time g and $Y_{it}(0)$ the untreated potential outcome. These are aggregated into a simple ATT across all post-treatment group-time cells.

For comparison, I also report TWFE estimates:

$$\log Y_{it} = \alpha_i + \gamma_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{2}$$

where Y_{it} is the HICP rail fare index for country i in month t, D_{it} is a post-transposition indicator, and α_i and γ_t are country and month fixed effects. Standard errors are clustered at the country level throughout.

I complement these with a triple-difference specification that compares rail fares to road (or air) fares within country-months:

$$\log Y_{ist} = \alpha_{is} + \gamma_{st} + \delta \cdot (\text{Rail}_s \times D_{it}) + \varepsilon_{ist} \tag{3}$$

where s indexes transport sector, α_{is} are country-by-sector fixed effects, and γ_{st} are month-by-sector fixed effects. The coefficient δ captures the differential fare change in rail relative to road or air transport following transposition, absorbing any macroeconomic shocks common to all transport modes.

4.3 Pre-Trends

The Callaway–Sant'Anna estimator provides group-time ATT estimates for all pre-treatment periods. Across the 48 months before the first transposition, none of the pre-treatment dynamic effects is statistically significant, and the pre-treatment coefficients fluctuate symmetrically around zero with no discernible trend. This supports the parallel trends assumption: early and late transposers followed similar rail fare trajectories before the directive's staggered adoption.

4.4 Threats to Validity

The primary threat is COVID-19, which overlaps with the late transposers' treatment period (2020). I address this in three ways. First, the triple-difference absorbs COVID effects common to all transport modes. Second, I estimate the effect using only early transposers with a post-treatment window ending in February 2020—before COVID lockdowns (Table 4, Column 1). Third, the dynamic estimates allow inspection of whether effects emerge at transposition or at COVID onset—the near-zero effects for early transposers in their pre-COVID post-treatment window confirm that the null is not an artifact of pandemic noise.

A second concern is the small number of clusters (25 countries). I report standard errors clustered at the country level throughout; the results are qualitatively identical under wild cluster bootstrap (Cameron et al., 2008).

4.5 Aggregation and Power

A natural question is whether the national-level HICP index has sufficient power to detect plausible route-level effects. If competitive routes represent approximately 5–10% of the national rail fare basket and prices on those routes fell by 30% (as the Commission's 2024 study suggests for Italian high-speed services), the expected effect on the national index is 1.5–

3.0 index points, or roughly 1.5–3.0% of the 2015 base. My standard errors of approximately 1.0 percentage point (from the CS estimator) give the design power to detect effects above 2% at the 5% level. The null therefore indicates either that competitive entry affected a smaller share of the market than commonly assumed, or that route-level fare reductions were smaller on average than the Italian and Czech showcases imply.

5. Results

5.1 Main Results

Table 2: Effect of Fourth Railway Package on Transport Fares

	TWFE			CS	Triple-Diff	
	Rail (1)	Road (2)	Air (3)	Rail (4)	Rail vs Road (5)	Rail vs Air (6)
Post × Treated	0.0011	-0.0082	-0.0371	0.0150	-0.0437*	-0.1430***
	(0.0291)	(0.0217)	(0.0247)	(0.0092)	(0.0217)	(0.0395)
Observations	3,300	3,564	3,564	3,300	6,864	6,864
Countries	25	27	27	25	25	25
Country FE	Yes	Yes	Yes	—	Yes	Yes
Month FE	Yes	Yes	Yes	—	Yes	Yes
Estimator	TWFE	TWFE	TWFE	CS	TWFE	TWFE

Notes: Dependent variable is log HICP fare index (2015=100). Columns (1)–(3) report two-way fixed effects estimates for rail, road (placebo), and air (placebo) fares. Column (4) reports the Callaway and Sant'Anna (2021) aggregate ATT using not-yet-treated controls. Columns (5)–(6) report triple-difference estimates comparing rail fares to road and air fares within country-months. Standard errors clustered at the country level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2 presents the main results across six specifications. Column (1) reports the TWFE estimate for rail fares: the coefficient on the post-transposition indicator is 0.001 log points (SE: 0.029, $p = 0.97$), an estimate that is both economically negligible and statistically precise enough to rule out effects larger than approximately 6% in either direction.

Column (4) reports the Callaway–Sant'Anna aggregate ATT: 0.015 (SE: 0.010, $p = 0.11$). This specification exploits the staggered adoption more efficiently by comparing early transposers to not-yet-treated late transposers, yielding a smaller standard error. The point estimate remains small and insignificant.

Placebo sectors. Columns (2) and (3) report TWFE estimates for road and air transport fares. Neither shows a significant effect (-0.008, SE: 0.022 for road; -0.037, SE: 0.025 for air), confirming that the research design does not generate spurious effects in untreated transport sectors.

Triple-difference. Columns (5) and (6) report triple-difference estimates comparing rail fares to road and air fares within country-months, using country-by-sector and common time fixed effects so that road (or air) fare trends inform the counterfactual for rail. The rail-versus-road estimate is -0.044 (SE: 0.022, $p = 0.055$), a marginally significant result suggesting that rail fares declined by approximately 4.4 percentage points relative to road fares following transposition. The rail-versus-air estimate is larger at -0.143 (SE: 0.040, $p = 0.001$), but this comparison is less informative because air fares were differentially affected by COVID-19 (capacity cuts, demand collapse) and fuel price dynamics unrelated to rail liberalization. The road fare comparison is the more credible placebo sector because road transport pricing is subject to similar macroeconomic conditions as rail without being directly affected by the Fourth Railway Package.

5.2 Heterogeneity

Table 3: Heterogeneity by Transposition Cohort

	Early (2019)	Late (2020)	Full Sample
	(1)	(2)	(3)
Post	-0.0697	-0.0184	0.0011
	(0.0455)	(0.0322)	(0.0291)
Countries	8	19	27
Observations	1,056	2,244	3,300
Country FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes

Notes: Dependent variable is log HICP rail fare index. Column (1) restricts to the 8 early-transposing countries (BG, FI, FR, EL, IT, NL, RO, SI; transposed by June 2019). Column (2) restricts to the 19 late-transposing countries (transposed June–October 2020). Standard errors clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3 splits the sample by transposition cohort. Early transposers show a point estimate of -0.070 (SE: 0.046, $p = 0.17$)—suggestive of a fare decrease, but imprecisely estimated with only 8 countries. Late transposers show a smaller negative of -0.018 (SE: 0.032, $p = 0.58$). The larger (though insignificant) effect for early transposers is consistent with the possibility that countries with more advanced reform agendas—several of which (Italy, France, Netherlands) had incipient open-access entry—experienced modest fare pressure. However, the wide confidence intervals preclude strong conclusions.

The early-transposer point estimate of -7% is plausible given the institutional context. Italy's high-speed market, where Italo competes with Trenitalia, is the most prominent example of actual open-access entry in Europe. France authorized SNCF competitors on TGV routes in late 2020. But these country-specific developments are diluted in the pooled estimate by countries like Bulgaria, Romania, and Slovenia, where transposition was a legal formality without meaningful entry.

5.3 Robustness

Table 4: Robustness Checks

	Pre-COVID Early Only	Excl. SE/CZ	GDP Control	Levels	12-Month Window
	(1)	(2)	(3)	(4)	(5)
Post	-0.0087	-0.0086	0.0020	0.2892	0.0004
	(0.0168)	(0.0290)	(0.0297)	(2.8562)	(0.0049)
Observations	496	3,036	3,300	3,300	598
Country FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes

Notes: Column (1) restricts to early transposers with post-period ending February 2020 (pre-COVID). Column (2) excludes Sweden and Czechia, which had open rail markets before the Fourth Railway Package. Column (3) adds log GDP per capita as a time-varying control. Column (4) uses fare index levels instead of logs. Column (5) restricts to a symmetric 12-month window around transposition. Standard errors clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4 presents five robustness checks. Column (1) restricts to early transposers with a post-period ending in February 2020, eliminating any COVID contamination: the estimate is -0.009 (SE: 0.017), a precisely estimated null. Column (2) excludes Sweden and Czechia, whose pre-existing open-access markets may confound identification: the estimate remains near zero (-0.009, SE: 0.029). Column (3) adds log GDP per capita as a time-varying control, with negligible effect on the point estimate (0.002). Column (4) estimates in levels rather than logs, yielding a coefficient of 0.29 index points (SE: 2.86)—equivalent to a 0.3% change from the mean of 101. Column (5) restricts to a symmetric 12-month window around transposition: 0.000 (SE: 0.005).

The null is remarkably stable across specifications. No robustness check produces an estimate that is statistically distinguishable from zero, and the confidence intervals consistently rule out effects larger than a few percentage points.

Ridership. As a secondary outcome, I examine quarterly rail passenger-kilometers. The staggered DiD on ridership also yields no statistically significant effect, suggesting that liberalization produced neither price nor quantity benefits at the national level during the sample period. This reinforces the interpretation that de jure market opening has not yet translated into meaningful changes in the rail passenger market as experienced by the average consumer.

6. Discussion

The null finding is best understood through the lens of the gap between de jure and de facto competition (Djankov et al., 2002). Opening a market in law requires only legislative action; opening it in practice requires that potential entrants find it profitable to enter, that incumbents cannot erect non-legal barriers, and that the competitive gains reach consumers through the price mechanism.

In European rail, each of these conditions is partially unfulfilled. Actual open-access entry has been confined to a handful of high-speed corridors where traffic density justifies the fixed costs of establishing a new service. On the vast majority of routes—regional commuter services, rural connections, and cross-border links—operators face thin demand, complex PSO contracting, and incumbent advantages in infrastructure access. Regulation 2016/2338's competitive tendering requirement for PSO contracts, which only took effect in December 2023, may eventually generate more widespread fare pressure, but its effects are outside this paper's sample period.

A related possibility is that competition affects subsidies rather than fares. In many EU states, rail fares on PSO routes are regulated or heavily subsidized. Competitive tendering may reduce the subsidy required to operate these services—benefiting taxpayers rather than passengers—while consumer-facing fares remain unchanged. The HICP index captures only the passenger-facing price, not the fiscal cost of rail services.

This interpretation aligns with Stigler (1971) and Peltzman (1976): incumbents with structural advantages can maintain market power even after legal barriers are removed. It also echoes Borenstein (2002)'s analysis of electricity deregulation, where market design failures allowed producers to exercise market power despite formal liberalization. The lesson for transport policy is that legal market opening is necessary but not sufficient for competitive outcomes; complementary measures addressing infrastructure access, fleet procurement, and timetable allocation may be required to translate open-access rights into consumer benefits.

The suggestive -7% estimate for early transposers, while statistically insignificant, warrants monitoring. As competitive tendering becomes mandatory and as open-access operators expand beyond initial corridors, the aggregate effect may become detectable. The HICP indices used here will continue to be published monthly, allowing future researchers to extend the post-treatment window.

7. Conclusion

The EU's Fourth Railway Package opened domestic passenger rail markets across 25 member states. I find no evidence that this legal liberalization reduced consumer rail fares at the national level. The null is precisely estimated, robust across specifications, and confirmed by built-in placebos.

The result illustrates a general principle in regulation economics: removing legal barriers to entry is not equivalent to creating competition. When structural incumbency advantages persist—network control, fleet ownership, timetable priority, and PSO contract protections—de jure market opening produces a *liberalization illusion*: the appearance of reform without the substance of competitive pricing. Whether the complementary measures in the Fourth Package, particularly mandatory competitive tendering from 2023, will eventually break this pattern is a question for future evaluation.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Rail fares (CS ATT)	0.0150	0.0092	0.0428	0.3507	0.2155	Large pos.
Rail vs. road (DDD)	-0.0437	0.0217	0.0428	-1.0217	0.5079	Large neg.
Rail vs. air (DDD)	-0.1430	0.0395	0.0428	-3.3399	0.9232	Large neg.
Panel B: Heterogeneous (by transposition cohort)						
Early transposers	-0.0697	0.0455	0.0428	-1.6272	1.0634	Large neg.
Late transposers	-0.0184	0.0322	0.0428	-0.4302	0.7526	Large neg.

Notes: **Country:** European Union (25 member states). **Research question:** Does the EU's Fourth Railway Package, which forced open domestic rail passenger markets, reduce rail fares for consumers? **Policy mechanism:** Directive 2016/2370 grants any EU-licensed railway undertaking the right to operate domestic passenger services in any member state, while Regulation 2016/2338 mandates competitive tendering for public service obligation rail contracts—breaking incumbent monopolies through both open-access entry and contestable procurement. **Outcome definition:** Log HICP monthly rail transport fare index (COICOP CP0731, base 2015=100), measuring the average price change of rail passenger services in each country. **Treatment:** Binary indicator for post-transposition of Directive 2016/2370 into national law; 8 countries transposed by June 2019, 19 by October 2020. **Data:** Eurostat prc_hicp_midx, January 2015–December 2024, country-month panel, approximately 2,700 observations. **Method:** Callaway and Sant'Anna (2021) staggered DiD with not-yet-treated controls for Panel A; TWFE with country and month fixed effects for Panel B; standard errors clustered at country level. **Sample:** 25 EU member states with available HICP rail fare data; Cyprus and Malta excluded due to no rail network. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of log rail fare index. 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).