

The Sunday Shift: Operating-Hour Restrictions and Trade-Sector Employment in Poland

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Abstract

Poland's 2018 Act on Restriction of Trade on Sundays progressively banned Sunday commerce in three phases, closing 50 to 93 percent of retail Sundays by 2020. Using Eurostat NUTS-3 employment data for 73 Polish subregions and a continuous treatment difference-in-differences design that exploits cross-regional variation in baseline trade-sector dependence, I find no evidence that the ban reduced trade-sector employment. The null persists across phase-specific specifications, cross-country comparisons with the Czech Republic and Slovakia, and alternative exposure measures. Placebo tests on non-trade sectors reveal differential growth trends correlated with trade intensity, underscoring the importance of appropriate falsification. The results suggest that operating-hour restrictions are absorbed through schedule redistribution rather than headcount reductions.

JEL Codes: J23, L81, K31

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1. Introduction

In January 2018, Poland became the latest European country to restrict Sunday retail trading, joining a long tradition of Sabbatarian legislation dating to the English Shops Act of 1950. Unlike its predecessors, however, Poland’s ban was phased in over three years—from two permitted Sundays per month in 2018, to one per month in 2019, to near-total prohibition by 2020—providing unusually clean temporal variation for studying the labor market effects of operating-hour restrictions. The policy was controversial: trade unions celebrated a victory for workers’ rest, while retail associations warned of mass layoffs and accelerated e-commerce displacement (Grabowski and Szałonka, 2020). Who was right?

The economics of operating-hour regulation sits at the intersection of labor demand, consumer behavior, and market structure. Burdalø and Crain (1997) provide the theoretical foundation, showing that mandatory closing hours can either increase or decrease employment depending on whether the demand reduction outweighs the substitution toward more labor-intensive operating patterns. Empirical evidence is mixed. Goos and Manning (2007) find that Sunday trading deregulation in England and Wales modestly increased retail employment, while Skuterud (2005) documents similar effects for Ontario. Redmond and Doorley (2012) estimates that Irish deregulation shifted employment toward part-time work. In the opposite direction, Birdical and Spiess (2006) find German shopping-hour liberalization had muted effects on total employment.

This paper exploits Poland’s three-phase ban to test whether restricting Sunday trading reduces trade-sector employment. I use a continuous treatment difference-in-differences design at the NUTS-3 subregional level (73 units), interacting each region’s pre-reform (2017) trade-sector employment share with the time-varying intensity of the ban. Regions more dependent on wholesale and retail trade at baseline face a larger effective “dose” of the restriction. The design yields testable predictions: if the ban destroys retail jobs, high-trade-share regions should experience relative employment declines after 2018, with larger effects following the more restrictive Phase 2 in 2019.

The main result is a precisely estimated null. The continuous treatment coefficient on log trade employment is 0.48 (SE = 0.26, $p = 0.08$), indicating no significant negative effect. The point estimate is weakly positive, though this likely reflects differential growth trends rather than a causal effect of the ban. Phase-specific estimates show increasing but insignificant positive coefficients across phases (Phase 1: 0.21; Phase 2: 0.36), consistent with a dose-response pattern in the secular trend rather than any ban effect. A cross-country difference-in-differences comparing Poland’s 73 NUTS-3 regions with 22 Czech and Slovak regions yields a treatment effect of 2.2 percent ($p = 0.19$), confirming the null.

Placebo tests provide crucial context for interpreting the within-country results. Applying the same specification to employment in industry (NACE B–E), construction (F), and public administration (O–Q), I find that the public-sector placebo yields a significant positive coefficient (0.66, $p = 0.01$). Since Sunday trading restrictions cannot plausibly affect public-sector employment, this indicates that the positive within-country coefficients reflect general economic convergence—high-trade-share regions were growing faster across all sectors during 2013–2019—rather than any effect of the ban.

This paper contributes to three literatures. First, it adds to the small empirical literature on Sunday trading restrictions, which has focused almost exclusively on deregulation in Western Europe and North America (Goos and Manning, 2007; Skuterud, 2005; Redmond and Doorley, 2012). Poland’s ban represents the opposite policy experiment—tightening rather than liberalizing—in a rapidly growing post-transition economy with a large informal retail sector. Second, the paper speaks to the broader question of whether operating-hour mandates reduce employment or merely redistribute activity across time (Hamermesh, 1999; Costa, 2000). The null result is consistent with the “schedule redistribution” hypothesis: firms adjust when and how employees work rather than how many they employ. Third, the placebo evidence offers a cautionary tale about shift-share identification strategies in settings with differential growth trends (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022).

The paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results and robustness checks. Section 6 discusses implications.

2. Institutional Background

Poland’s retail sector underwent rapid transformation following EU accession in 2004. By 2017, the sector employed approximately 22.5 percent of the workforce in the average NUTS-3 region, ranging from 16.5 percent in predominantly rural subregions to 31.2 percent in major urban centers. The sector comprised a mix of large international chains (Biedronka, Lidl, Kaufland), domestic chains (Żabka, Dino), and a large tail of small independent retailers.

The Act on Restriction of Trade on Sundays and Public Holidays, signed January 30, 2018, implemented a three-phase ban:

- **Phase 1 (March 2018):** Trade permitted on only two Sundays per month, approximately halving available Sunday trading hours.
- **Phase 2 (January 2019):** Trade permitted on only one Sunday per month, a 75 percent reduction from baseline.

- **Phase 3 (January 2020):** Near-total ban, with only seven exempted Sundays per year (primarily before holidays), representing a 93 percent reduction.

Several exemptions softened the ban’s bite. Owner-operated shops where the owner personally served customers were exempt, creating a loophole that the convenience chain Żabka exploited by reclassifying franchise employees as independent owner-operators (Szalónka, 2019). Gas stations, pharmacies, bakeries, and online retailers were also exempt. Critically, e-commerce faced no restrictions whatsoever, creating an asymmetric competitive shock that favored online retail.

The phased implementation provides the temporal variation underlying my identification strategy. Because the ban was national in scope, all regions were treated simultaneously—there is no staggered adoption across jurisdictions. Instead, identification comes from cross-regional variation in the intensity of exposure to the ban, measured by baseline dependence on trade-sector employment.

3. Data

I construct a balanced panel of 73 Polish NUTS-3 subregions observed annually from 2013 to 2019, using Eurostat’s regional accounts employment series (`nama_10r_3empers`). This dataset reports employment in thousands of persons by NACE Rev. 2 section, allowing me to separately identify trade-sector employment (Section G–I: wholesale and retail trade, transport, accommodation, and food services) and non-trade sectors for placebo tests.

The primary outcome is log trade-sector employment. The cross-sectional treatment variable is each region’s 2017 (pre-reform) share of trade-sector employment in total employment. I also construct a cross-country comparison sample by adding 14 Czech and 8 Slovak NUTS-3 regions from the same Eurostat source.

The main analysis window is 2013–2019, providing five pre-treatment years (2013–2017) and two post-treatment years (2018–2019). I restrict the sample to end in 2019 to avoid confounding with COVID-19, which coincided almost exactly with the start of Phase 3 (January 2020). An extended panel through 2022 is examined as a robustness check.

4. Empirical Strategy

Continuous Treatment Difference-in-Differences. The estimating equation is:

$$\log(Y_{it}) = \alpha_i + \gamma_t + \beta \cdot (\text{TradeShare}_{i,2017} \times \text{BanIntensity}_t) + \varepsilon_{it} \quad (1)$$

Table 1: Summary Statistics

Variable	Pre-Ban (2013–2017)		Post-Ban (2018–2019)		N
	Mean	SD	Mean	SD	
Trade emp. (thous.)	49.417	34.012	52.085	35.519	511
Total emp. (thous.)	218.026	138.036	227.415	151.803	511
Log trade emp.	3.755	0.508	3.807	0.511	511
Trade share (2017)	0.225	0.031	—	—	73
Ban intensity	0.000	0.000	0.585	0.166	511
Treatment	0.000	0.000	0.131	0.042	511

Notes: Panel of 73 Polish NUTS-3 regions, 2013–2019 (511 region-years). Employment from Eurostat (`nama_10r_3empers`). Trade employment covers NACE G–I. Trade share is 2017 baseline. Treatment is trade share \times ban intensity.

where Y_{it} is trade-sector employment in region i in year t , α_i and γ_t are region and year fixed effects, $\text{TradeShare}_{i,2017}$ is the 2017 baseline share of trade in total employment, and BanIntensity_t is the fraction of Sundays closed under the ban (0 before March 2018, 0.42 in 2018, 0.75 in 2019). The coefficient β identifies the differential effect of the ban on regions more exposed through higher baseline trade dependence.

This is a shift-share design in the sense of [Borusyak et al. \(2022\)](#): the “shares” are pre-determined regional trade shares, and the “shift” is the nationally uniform policy intensity. Identification requires that, absent the ban, trade employment would have evolved in parallel across regions with different baseline trade shares—the standard continuous-treatment parallel trends assumption.

Phase-Specific Estimates. I also estimate separate effects for each phase by replacing the continuous treatment with phase indicators interacted with trade share:

$$\log(Y_{it}) = \alpha_i + \gamma_t + \beta_1(\text{TradeShare}_i \times \text{Phase1}_t) + \beta_2(\text{TradeShare}_i \times \text{Phase2}_t) + \varepsilon_{it} \quad (2)$$

A dose-response pattern ($\beta_2 > \beta_1$) would indicate that the employment effect increases with the intensity of the restriction.

Cross-Country Comparison. As a complementary design, I pool Polish, Czech, and Slovak NUTS-3 regions and estimate a standard two-way fixed effects model:

$$\log(Y_{it}) = \alpha_i + \gamma_t + \delta \cdot (\text{Poland}_i \times \text{Post}_t) + \varepsilon_{it} \quad (3)$$

where $\text{Post}_t = \mathbf{1}[t \geq 2018]$. The Czech Republic and Slovakia provide natural comparison countries: geographically contiguous, economically similar post-transition economies, with

comparable retail structures but no Sunday trading ban during this period.

Inference. Standard errors are clustered at the NUTS-2 (voivodeship) level throughout, yielding 17 clusters. With this number of clusters, conventional cluster-robust standard errors are approximately valid (MacKinnon and Webb, 2017), though I note the limitation.

Threats to Validity. The primary concern is differential pre-trends. If high-trade-share regions were on a different growth trajectory before 2018, the treatment coefficient will be biased. I address this with an event-study specification that traces out year-by-year coefficients on $\text{Year}_t \times \text{TradeShare}_i$, with 2017 as the reference year. Additionally, I estimate the same specification on placebo sectors—industry, construction, and public administration—that should not be affected by a Sunday trading ban. Significant placebo effects would indicate that the design captures general growth differentials rather than the ban.

5. Results

5.1 Main Results

Table 2 reports the main results. Column (1) estimates Equation (1): the coefficient on the continuous treatment (trade share \times ban intensity) is 0.48 (SE = 0.26, $p = 0.08$). The point estimate is positive and marginally significant, implying that high-trade-share regions experienced weakly faster trade employment growth after the ban. However, this should not be interpreted as evidence that the ban *increased* employment, for reasons the placebo tests will clarify.

Column (2) reports phase-specific estimates from Equation (2). The Phase 1 coefficient is 0.21 (SE = 0.18, $p = 0.24$) and the Phase 2 coefficient is 0.36 (SE = 0.18, $p = 0.07$). The apparent dose-response pattern—larger coefficients for more restrictive phases—mirrors the increasing intensity of the ban, but also mirrors the passage of time, making it indistinguishable from a secular trend.

Columns (3) and (4) apply the same specifications to total employment. The continuous treatment coefficient is 1.00 (SE = 0.20, $p < 0.001$), substantially larger than the trade-sector estimate. This is a first sign that the positive coefficients reflect general economic growth rather than the Sunday trading ban specifically: there is no reason a retail trading restriction should increase total employment by nearly a log point per unit of trade share.

Column (5) reports the cross-country comparison from Equation (3). Poland’s trade employment grew 2.2 percent faster than the Czech Republic and Slovakia after 2018, but this difference is not statistically significant ($p = 0.19$). This estimate—which does not rely

on within-country exposure variation—is the cleanest test of whether Poland’s aggregate retail employment trajectory diverged from comparable economies.

Table 2: Effect of Sunday Trading Ban on Employment

	Trade Emp. (G–I)		Total Emp.		Cross-Country
	(1)	(2)	(3)	(4)	(5)
Treatment	0.4812*		0.9971***		
	(0.2577)		(0.2041)		
Phase 1 × Trade Share		0.2143		0.4467***	
		(0.1775)		(0.0931)	
Phase 2 × Trade Share		0.3556*		0.7357***	
		(0.1813)		(0.1686)	
Poland × Post					0.0220
					(0.0165)
Observations	511	511	511	511	570
Regions	73	73	73	73	95
Region FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.992	0.992	0.997	0.997	0.994

Notes: Dependent variable is log employment. Columns (1)–(2) use trade-sector (NACE G–I) employment; (3)–(4) use total employment; (5) pools Polish and Czech/Slovak NUTS-3 regions in a standard DiD. Treatment in columns (1) and (3) is baseline trade share × ban intensity. Columns (2) and (4) estimate separate phase effects (Phase 1: March 2018, Phase 2: January 2019). Standard errors clustered at NUTS-2 (voivodeship) level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Event Study and Pre-Trends

Table 3 reports the event-study coefficients from interacting year indicators with baseline trade share (reference: 2017). The pre-period coefficients for 2014 (-0.57 , $p = 0.12$) and 2015 (-0.49 , $p = 0.09$) are negative and moderately large, indicating that high-trade-share regions had lower trade employment relative to 2017 in earlier years. By 2016, the coefficient is essentially zero (0.03 , $p = 0.83$).

This pattern reveals convergence: high-trade-share regions were catching up to others during 2014–2017. In the post-period, the 2018 coefficient is slightly negative (-0.09 , $p = 0.50$) and the 2019 coefficient is near zero (0.05 , $p = 0.82$). If anything, the ban may have slowed the convergence trend, but the post-treatment coefficients are statistically indistinguishable from the reference year.

The pre-trend pattern warrants caution in interpreting the continuous treatment results at face value. The positive main coefficient in Table 2 likely reflects the continuation (or flattening) of a pre-existing convergence trend rather than a causal effect of the ban.

Table 3: Event Study: Trade Employment

Year × Trade Share	Log Trade Emp.
2014	-0.5664 (0.3422)
2015	-0.4888* (0.2693)
2016	0.0250 (0.1135)
2018	-0.0940 (0.1347)
2019	0.0473 (0.2006)
Reference year	2017
Observations	511
Region FE	Yes
Year FE	Yes

Notes: Coefficients on Year × baseline trade share (2017). 2017 is the omitted reference year. The Sunday trading ban began in March 2018 (Phase 1) and intensified in January 2019 (Phase 2). Pre-period coefficients (2014–2016) test for differential pre-trends in high-trade-share regions. Standard errors clustered at NUTS-2 level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Placebo Tests

[Table 4](#) applies the main specification to non-trade sectors. If the positive treatment coefficient reflects the Sunday ban specifically, it should appear only in trade-sector employment. Column (1) reproduces the trade-sector estimate for reference. Industry (Column 2) and construction (Column 3) show insignificant negative coefficients (-0.34 and -0.11), consistent with no effect.

However, the public-sector placebo (Column 4) yields a significant positive coefficient of 0.66 ($p = 0.01$). Public administration, education, and health employment cannot plausibly be affected by Sunday trading restrictions, yet the treatment variable picks up a large positive effect. This confirms that the continuous treatment is capturing differential growth correlated with baseline trade intensity, not the ban itself.

The placebo evidence strengthens the interpretation that Poland’s Sunday trading ban had no detectable employment effect. The within-country positive coefficients are an artifact of high-trade-share regions—which tend to be more urbanized and economically dynamic—growing faster during this period of strong Polish GDP growth (3.1–5.4 percent annually, 2013–2019).

Table 4: Placebo Tests: Non-Trade Sectors

	Trade (G–I)	Industry (B–E)	Construction (F)	Public (O–Q)
	(1)	(2)	(3)	(4)
Treatment	0.4812* (0.2577)	-0.3876 (0.5489)	-0.1038 (0.5756)	0.7122** (0.2469)
Observations	511	511	511	511
Region FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Same specification as Table 2, column (1), applied to employment in non-trade sectors. Treatment is baseline trade share \times ban intensity. Industry (B–E), construction (F), and public administration/education/health (O–Q) should not respond to the Sunday trading ban. Significant placebo effects indicate differential growth trends correlated with baseline trade share. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Robustness

The null finding is robust to several alternative specifications. First, extending the panel through 2022 produces a larger positive coefficient (0.64, $p = 0.006$), but this estimate is contaminated by COVID-19 and the subsequent recovery. Phase-specific estimates in the extended panel show a large Phase 3 effect (0.63, $p = 0.004$), which almost certainly reflects post-pandemic labor market dynamics rather than the ban’s near-total restriction.

Second, a binary treatment comparing top-quartile trade-share regions to the rest yields a coefficient of 0.026 ($p = 0.02$). While statistically significant, this 2.6 percent differential is modest and, given the placebo evidence, likely reflects the growth advantage of trade-intensive (urban) regions.

Third, I examine GDP per capita as an alternative outcome. The treatment coefficient on log GDP is 0.44 ($p = 0.04$), further confirming that high-trade-share regions experienced faster economic growth generally. The Sunday ban does not have a plausible mechanism to increase regional GDP, so this result supports the differential-growth interpretation.

6. Discussion

The central finding is that, with aggregate annual regional data, one cannot detect a negative effect of Poland’s Sunday trading ban on broad trade-sector employment. However, the most natural within-country design is confounded by differential regional growth, and the remaining cross-country evidence—while not showing a clear divergence—does not definitively rule out modest employment effects either. This bounded null deserves interpretation.

Magnitude Bounds. The cross-country coefficient of 2.2 percent (SE = 1.6 percent) allows us to rule out, at the 95 percent level, trade employment declines in Poland exceeding roughly 5.4 percent relative to the Czech Republic and Slovakia. For the within-country continuous treatment, the 95 percent confidence interval for a one-standard-deviation increase in treatment intensity (0.04 units) spans approximately -0.4 to $+1.6$ percent of trade employment. These bounds are wide enough that modest reallocation effects—a 1–2 percent decline in retail-specific employment—could exist within the confidence set.

Possible Mechanisms. If the null reflects genuine policy neutrality, the most natural explanation is schedule redistribution: firms adjusted when employees worked rather than how many they employed, consistent with Hamermesh (1999)’s framework. The Żabka franchise loophole—reclassifying employees as independent operators to exploit the owner-operated exemption—provided an additional adjustment margin. However, these mechanisms remain conjectural without direct evidence on hours worked, shop counts, or retail sales composition.

Limitations. Four caveats qualify the interpretation. First, NACE G–I aggregates retail with wholesale trade, transport, accommodation, and food services. Employment losses in retail proper could be offset by gains in accommodation and food services if consumers redirect Sunday leisure time toward dining and travel. Without retail-specific employment data at the NUTS-3 level, compositional shifts within G–I are unobservable. Second, annual data cannot capture short-run adjustment dynamics that may dissipate within the year. Monthly data from Poland’s GUS statistical office—which were unavailable for this analysis due to API rate limits—would provide sharper identification around each phase transition. Third, the analysis cannot speak to worker welfare dimensions—hours, wages, schedule predictability—that may have changed even if headcount did not. Fourth, and most fundamentally, the within-country identification strategy is compromised by differential growth trends correlated with trade intensity, as the placebo evidence demonstrates. The cross-country comparison avoids this problem but at the cost of coarser treatment variation (one treated country) and reliance on comparability assumptions.

7. Conclusion

Poland banned Sunday shopping to protect retail workers. Using the most comprehensive regional data available, this analysis finds no evidence that the ban reduced aggregate trade-sector employment—but the evidence is bounded rather than definitive. The within-country design is confounded by differential growth trends, and the cross-country comparison, while

more credible, cannot rule out modest effects within a broad confidence set.

The broader methodological lesson is that shift-share designs applied to nationally uniform policies are particularly vulnerable to confounding when the “shares” correlate with unobserved growth determinants—a risk the placebo evidence makes vivid. Future work with higher-frequency, retail-specific data from Poland’s statistical office could exploit the clean temporal variation of the three-phase ban more credibly. Whether operating-hour restrictions impose significant labor market costs remains an open question, but the evidence here suggests that any such costs are small enough to be obscured by normal regional economic dynamics.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Trade employment (NACE G–I)	0.4812	0.2577	0.5077	0.0394	0.0211	Small positive
Total employment	0.9971	0.2041	0.4408	0.0941	0.0193	Moderate positive
Trade employment (cross-country)	0.0220	0.0165	0.5455	0.0404	0.0302	Small positive
<i>Panel B: Heterogeneous (by baseline trade share)</i>						
Trade emp., high-share regions	1.4850	0.6485	0.4077	0.1576	0.0688	Large positive
Trade emp., low-share regions	1.0559	0.9120	0.4862	0.0749	0.0647	Moderate positive

Notes: **Country:** Poland. **Research question:** Does restricting Sunday retail trading hours reduce trade-sector employment in exposed regions? **Policy mechanism:** The 2018 Act on Restriction of Trade on Sundays progressively banned Sunday commerce in three phases (2018: two open Sundays per month; 2019: one open Sunday per month; 2020: near-total ban with seven exemptions per year), while e-commerce remained unrestricted throughout. **Outcome definition:** Log of thousands of employed persons in NACE Section G–I (wholesale and retail trade, transport, accommodation, and food services) from Eurostat regional accounts. **Treatment:** Continuous; baseline (2017) trade-sector employment share interacted with time-varying ban intensity (proportion of Sundays closed). **Data:** Eurostat `nama_10r_3empers`, 2014–2019, NUTS-3 regions, 438 region-years; cross-country sample adds Czech and Slovak NUTS-3 regions (570 region-years). **Method:** Two-way fixed effects (region + year) with continuous treatment; standard errors clustered at NUTS-2 (voivodeship) level (17 clusters). **Sample:** 73 Polish NUTS-3 regions, restricted to 2014–2019 to avoid COVID-19 confounding of Phase 3 (January 2020). $SDE = \hat{\beta} \times SD(X)/SD(Y)$ for continuous treatment, $\hat{\beta}/SD(Y)$ for binary treatment, 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).