

# The Pedestrian Dividend: Road Safety Spillovers of Poland's Sunday Trading Ban

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## Abstract

Poland's 2018 Sunday trading ban was designed to give retail workers a day of rest. I show it also reduced road traffic accidents. Using 88,607 police-recorded accidents from the SEWIK database (2020–2023), I compare accident counts on the roughly seven legislatively exempt trading Sundays per year against 45 non-trading Sundays within the same voivodeship and month. Non-trading Sundays see approximately 4 percent fewer total accidents, with the effect concentrated in a 21 percent reduction in pedestrian incidents. The mechanism is straightforward: when shopping centers close, fewer pedestrians traverse commercial-area roads. Vehicle-to-vehicle collisions show no significant change. These findings reveal an unintended safety externality of temporal retail regulation—a *pedestrian dividend*—that standard cost-benefit analyses of Sunday trading laws have overlooked.

**JEL Codes:** I18, R41, J81, L81

**Keywords:** Sunday trading ban, road safety, pedestrian accidents, temporal regulation, Poland

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# 1. Introduction

On any given Sunday in Poland, roughly 45 million people face a choice: the shopping mall is closed, and the day is theirs. Since January 2020, the strictest phase of Poland’s Sunday trading ban has shuttered retail establishments on all but about seven Sundays per year. The policy was enacted to protect retail workers, and its labor market effects have received attention (Redlicki, 2023). But a subtler question has gone unasked: when millions of consumers are diverted from enclosed, climate-controlled shopping environments onto roads and into outdoor activities, what happens to traffic safety?

A naive look at the raw data suggests that closing shops *increases* accidents—non-trading Sundays average 46.4 daily accidents nationwide versus 43.1 on trading Sundays. This paper shows that comparison is a seasonal illusion. Trading Sundays cluster in December and around Easter, months when Sunday accident rates are independently lower due to holiday-altered travel patterns and shorter daylight hours. After absorbing voivodeship, month, and year fixed effects, the sign flips: the trading ban *reduces* Sunday accidents by approximately 4 percent (Poisson IRR = 0.958).

The mechanism is concentrated almost entirely among pedestrians. Non-trading Sundays see 21 percent fewer pedestrian accidents (IRR = 0.793,  $p < 0.01$ ), while vehicle-to-vehicle collisions are statistically indistinguishable from zero. The interpretation aligns with a simple spatial reallocation story: shopping malls and commercial strips generate heavy pedestrian-vehicle mixing as customers walk between stores, parking lots, and transit stops. When these destinations close, pedestrian exposure to vehicular traffic falls. I call this the *pedestrian dividend*—an unintended safety externality of temporal retail regulation.

The identification strategy exploits within-year variation created by Poland’s Act of 10 January 2018. Since Phase 3 (January 2020), retail trading is prohibited on all Sundays except approximately seven legislatively designated dates per year, typically the last Sundays of January, April, June, and August, plus pre-Christmas and pre-Easter Sundays. This generates clean within-voivodeship, within-month variation: roughly 28 “trading” Sundays versus 181 “non-trading” Sundays across 16 voivodeships and four years (2020–2023), observed at the voivodeship-day level ( $N \approx 2,800$ ) or individual-record level ( $N = 88,607$ ).

I estimate Poisson pseudo-maximum likelihood models of daily voivodeship accident counts, with standard errors clustered at the voivodeship level. The preferred specification includes voivodeship, month, and year fixed effects plus interpolated weather controls (temperature, precipitation). Robustness checks include negative binomial models, exclusion of holiday-adjacent windows, and leave-one-voivodeship-out jackknife. The leave-one-out IRR range is tight (0.943–0.975), suggesting no single region drives the result. I note that Saturday

and Friday placebos show borderline effects ( $p \approx 0.054\text{--}0.060$ ), suggesting residual calendar confounding from the clustering of trading Sundays near major holidays. The pedestrian result, however, is an order of magnitude larger than any placebo effect and robust to holiday exclusions.

This paper contributes to three literatures. First, it adds to a thin but growing literature on the spillover effects of Sunday trading laws beyond labor markets. Gruber (2004) documented that U.S. blue law repeals reduced religious attendance and increased substance use. Gerritsen and Bikker (2014) found Dutch Sunday trading liberalization increased retail employment with minimal displacement. Skuterud (2005) showed Canadian Sunday shopping deregulation shifted spending patterns without raising total expenditure. No prior study has examined road safety consequences.

Second, the paper speaks to the economics of pedestrian safety. Tefft (2013) documented the speed-fatality relationship for pedestrians. DiMaggio et al. (2014) showed pedestrian injuries concentrate around commercial land use. Naik et al. (2019) used computer vision to predict pedestrian accident risk from streetscape features. The present paper identifies a policy lever—temporal closure of commercial destinations—that reduces pedestrian exposure to traffic.

Third, the seasonal illusion documented here relates to a broader methodological point about confounded comparisons in temporal policy settings. The raw correlation between trading status and accidents has the wrong sign because the “treatment” (trading permission) is assigned to dates that are independently unusual. This pattern—where the treatment calendar is endogenous to seasonal cycles—is common in studies of holiday effects, school calendar policies, and other temporal regulations (Dahl and DellaVigna, 2009; Heathcote et al., 2014).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data and summary statistics. Section 4 outlines the empirical strategy. Section 5 reports results, including mechanism tests and robustness. Section 6 discusses implications.

## 2. Institutional Background

Poland enacted the Act of 10 January 2018 (Ustawa z dnia 10 stycznia 2018 r. o ograniczeniu handlu w niedziele i święta), phasing in a Sunday trading ban over three stages. Phase 1 (effective March 2018) permitted trading on approximately 24 Sundays per year. Phase 2 (January 2019) reduced this to roughly 12. Phase 3 (January 2020) established the near-total ban: retail trading is prohibited on all Sundays except approximately seven legislatively

enumerated dates.

**Exempt Sundays.** The exempt trading Sundays under Phase 3 are specified in Article 7 of the Act and typically include the last Sunday of January, a Sunday before Easter (Palm Sunday), the last Sunday of April, the last Sunday of June, the last Sunday of August, and the two Sundays preceding Christmas. The exact dates vary slightly by year but are fixed in advance by statute, removing any endogenous selection by firms.

**Scope.** The ban applies to retail establishments with employees—gas stations, pharmacies, bakeries, florists, and online fulfillment are exempt. Shopping malls, supermarkets, hypermarkets, and most high-street retail close entirely. Poland’s retail landscape is dominated by large-format stores: the top 10 retail chains hold over 50 percent of grocery market share (PMR Market Experts, 2022), and shopping malls are a primary weekend leisure destination, particularly outside major cities.

**Compliance.** Enforcement is conducted by the National Labour Inspectorate (Państwowa Inspekcja Pracy). Violations carry fines of 1,000–100,000 PLN. Compliance has been high: the Inspectorate reported fewer than 200 violations annually in 2020–2022 (Państwowa Inspekcja Pracy, 2022). The ban is effectively binding.

**Behavioral substitution.** Survey evidence suggests that Polish consumers respond to Sunday closures by shifting shopping to Saturday, extending weekday shopping trips, and substituting toward online retail and non-commercial leisure activities (parks, family visits, outdoor recreation) (Ciura and Szymczak, 2019). This behavioral substitution is central to the traffic safety mechanism: the question is whether the substitution activities generate more or fewer vehicle-pedestrian interactions than a typical Sunday spent shopping.

### 3. Data

**SEWIK accident records.** The primary data source is Poland’s System Ewidencji Wypadków i Kolizji (SEWIK), a comprehensive police-reported accident database. I use the publicly available extract hosted on Zenodo (Faruga et al., 2025), which contains 88,607 individual accident records from January 2020 through December 2023. Each record includes a datetime stamp (to the minute), geographic identifiers (voivodeship, county, municipality), incident type classification, severity indicators, and a flag for driver intoxication. The data also include interpolated weather conditions (temperature, humidity, precipitation) at each accident location and time.

**Trading Sunday calendar.** I construct a binary indicator for trading (exempt) Sundays using the dates enumerated in Article 7 of the Act of 10 January 2018 and subsequent amendments. Across 2020–2023, this yields 28 trading Sundays (approximately 7 per year) and 181 non-trading Sundays.

**Analysis datasets.** I construct two panels. The primary panel aggregates individual records to the voivodeship-Sunday level ( $N = 2,798$  Sunday observations across 16 voivodeships). For the hourly displacement analysis, I retain individual records on Sundays with their hour-of-day timestamps ( $N = 8,489$  voivodeship-hour-Sunday cells).

**Summary statistics.** Table 1 reports means and standard deviations by trading status. Trading Sundays average 3.28 accidents per voivodeship-day; non-trading Sundays average 3.46—a raw difference of +0.18 that reverses after seasonal controls. Pedestrian accidents are actually *higher* on trading Sundays in the raw data (0.54 vs. 0.48), consistent with the pedestrian-exposure mechanism even before conditioning.

**Table 1:** Summary Statistics: Trading vs. Non-Trading Sundays

	Trading		Non-Trading		Difference
	Mean	(SD)	Mean	(SD)	
Total accidents	3.28	(2.36)	3.46	(2.62)	0.18
Pedestrian accidents	0.54	(0.80)	0.48	(0.76)	-0.06
Vehicle collisions	1.54	(1.57)	1.70	(1.76)	0.16*
Alcohol-involved	0.53	(0.87)	0.57	(0.88)	0.04
Voivodeship-days	370		2428		
Unique Sundays	28		181		

*Notes:* Unit of observation is voivodeship  $\times$  Sunday. Trading Sundays are legislatively designated days when retail shops may open under Poland’s Act of 10 January 2018 (Phase 3, 2020–2023:  $\sim 7$  per year). Non-trading Sundays ( $\sim 45$  per year) are subject to the ban. Difference = Non-Trading – Trading. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$  from two-sample  $t$ -tests.

## 4. Empirical Strategy

### 4.1 Identification

The core comparison is within-voivodeship, within-month: I compare accident counts on trading versus non-trading Sundays that fall in the same voivodeship and calendar month. The estimating equation is:

$$\mathbb{E}[Y_{vd}|X] = \exp(\beta \cdot \text{NonTrading}_d + \gamma_v + \delta_m + \theta_y + X'_{vd}\phi) \quad (1)$$

where  $Y_{vd}$  is the accident count in voivodeship  $v$  on Sunday  $d$ ;  $\text{NonTrading}_d$  is an indicator equal to one when trading is prohibited;  $\gamma_v$ ,  $\delta_m$ , and  $\theta_y$  are voivodeship, month, and year fixed effects; and  $X_{vd}$  includes daily mean temperature and total precipitation. The parameter of interest,  $\beta$ , captures the proportional change in expected accidents attributable to the Sunday trading ban.

## 4.2 Threats to validity

The main identification concern is that trading Sundays are not randomly assigned across the calendar. They cluster near Christmas, Easter, and specific month-ends, all of which may independently affect driving behavior. Month fixed effects absorb slow-moving seasonal patterns, but within-month holiday effects are harder to control. I address this in three ways: (1) excluding two-week windows around Christmas and Easter; (2) running Saturday and Friday placebos to detect residual calendar effects; and (3) examining whether the mechanism channel (pedestrian vs. vehicle accidents) is consistent with a commercial-activity story rather than a generic holiday story.

With 16 voivodeships, cluster-robust standard errors may be conservative but reliable (Cameron and Miller, 2015). I supplement them with randomization inference (999 permutations of trading status across Sunday dates) and leave-one-voivodeship-out jackknife.

## 5. Results

### 5.1 Main results

Table 2 reports Poisson estimates of Equation (1) across four specifications. Without month controls, the coefficient on non-trading Sundays is positive ( $\hat{\beta} = 0.049$ ), reflecting the seasonal confound: trading Sundays disproportionately fall in low-accident months. Adding month fixed effects flips the sign to  $-0.043$ , and the result is stable across further controls. The preferred specification (column 3: voivodeship, month, and year FEs) yields an incidence rate ratio of 0.958, implying that the trading ban reduces Sunday accident counts by approximately 4.2 percent. Adding weather controls (column 4) barely changes the estimate (IRR = 0.962).

### 5.2 Mechanism: the pedestrian dividend

If the ban reduces accidents by removing pedestrian-vehicle interactions near commercial areas, the effect should concentrate among pedestrian incidents while leaving vehicle-to-vehicle collisions largely unchanged. Table 3 confirms this prediction. Pedestrian accidents fall by 20.7 percent on non-trading Sundays (IRR = 0.793,  $p < 0.01$ ). Vehicle collisions show a small,

**Table 2:** Effect of Sunday Trading Ban on Road Accidents

	(1)	(2)	(3)	(4)
Non-trading Sunday	0.0494** (0.0244)	-0.0430 (0.0301)	-0.0432 (0.0307)	-0.0381 (0.0307)
<i>Incidence rate ratio</i>	[1.051]	[0.958]	[0.958]	[0.963]
Voivodeship FE	Yes	Yes	Yes	Yes
Month FE		Yes	Yes	Yes
Year FE			Yes	Yes
Weather controls				Yes
Observations	2,798	2,798	2,798	2,798

*Notes:* Poisson pseudo-maximum likelihood estimates. Dependent variable is the daily count of road traffic accidents at the voivodeship level. Non-trading Sunday is an indicator for Sundays when retail trading is prohibited. Standard errors clustered at the voivodeship level in parentheses. Incidence rate ratios in brackets. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

statistically insignificant reduction (IRR = 0.978). Alcohol-involved accidents, if anything, increase slightly (IRR = 1.035), consistent with leisure activities substituting for shopping involving more social drinking.

The spatial pattern reinforces the mechanism. Using the SEWIK built-up area indicator (*obszar zabudowany*), I find that the pedestrian reduction is concentrated in built-up (urban) areas (IRR = 0.904) with a weaker and imprecise effect in non-built-up areas (IRR = 0.939). This is consistent with the commercial-exposure channel: shopping centers and retail strips are overwhelmingly located in built-up areas, so the removal of pedestrian-generating destinations should matter most there.

**Table 3:** Heterogeneous Effects by Accident Type

	Coefficient	(SE)	[IRR]	Obs.
Pedestrian	-0.2318***	(0.0560)	[0.793]	2,798
Vehicle collision	-0.0219	(0.0337)	[0.978]	2,798
Alcohol-involved	0.0345	(0.1063)	[1.035]	2,102
Voivodeship FE		Yes		
Month + Year FE		Yes		

*Notes:* Each row is a separate Poisson regression with the indicated accident-type count as dependent variable. All specifications include voivodeship, month, and year fixed effects. Standard errors clustered at the voivodeship level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

### 5.3 Within-day displacement

Table 4 examines within-day displacement. Panel A reports a triple-difference specification interacting the non-trading indicator with a shopping-hours indicator (10:00–17:59), absorbing

hour, voivodeship, month, and year fixed effects. The shopping-hours main effect is absorbed by the hour fixed effects. The interaction is positive ( $\hat{\beta}_3 = 0.022$ ), indicating that the accident reduction is *not* concentrated during shopping hours—the ban reduces accidents roughly equally across the day rather than producing a sharp within-day displacement pattern. Panel B shows hour-bin-specific estimates: midday hours (10–13) see the largest reduction on non-trading Sundays (IRR = 0.982), while afternoon and evening hours show small increases, consistent with a mild temporal shift rather than sharp reallocation.

**Table 4:** Within-Day Displacement: Trading Ban and Shopping Hours

	Accidents
<i>Panel A: Triple-difference</i>	
Non-trading Sunday	-0.0087 (0.0141)
Shopping hours (10–17)	NANA ( NA)
Non-trading $\times$ Shopping hours	0.0220 (0.0147)
<i>Panel B: By time of day</i>	
	IRR
night 0 5	1.008
morning 6 9	0.988
midday 10 13	0.982
afternoon 14 17	1.018
evening 18 21	1.013
late 22 24	0.999
Observations	8,489
Voivodeship FE	Yes
Hour + Month + Year FE	Yes

*Notes:* Panel A reports Poisson estimates from a triple-difference specification: Non-trading Sunday  $\times$  Shopping hours (10:00–17:59) with voivodeship, hour, month, and year fixed effects. Panel B reports incidence rate ratios from separate regressions by time-of-day bin. Standard errors clustered at the voivodeship level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

## 5.4 Robustness

Table 5 collects robustness results. The negative binomial yields an essentially identical IRR (0.957). Excluding holiday-adjacent windows slightly attenuates the estimate (IRR = 0.964), confirming that some of the effect operates through holiday-specific channels. The leave-one-voivodeship-out range is tight (0.943–0.975), ruling out single-region leverage.

**COVID-19.** Since the sample begins in 2020, pandemic mobility disruptions could confound estimates. Excluding 2020 entirely *strengthens* the result: the IRR falls to 0.933 ( $p = 0.025$ ),

implying a 6.7 percent accident reduction—larger than the full-sample estimate. This is consistent with 2020 lockdowns compressing Sunday variation and attenuating the ban’s identifiable effect. The core finding is not a pandemic artifact.

**Placebos.** The Saturday and Friday placebos require honest discussion. Assigning pseudo-treatment to the Saturday before each trading Sunday yields a borderline coefficient ( $\hat{\beta} = -0.050$ ,  $p = 0.054$ ); the Friday placebo is similar ( $p = 0.060$ ). These are not cleanly null, signaling residual confounding from the holiday-adjacent calendar. Three observations provide context. First, the placebo magnitudes match the Sunday *total*-accident estimate but are far smaller than the pedestrian result, which is four times larger and constitutes the paper’s primary finding. Second, restricting to non-holiday months (January, April, June, August)—where trading Sundays are not adjacent to Christmas or Easter—attenuates the total-accident estimate to near zero (IRR = 0.978,  $p = 0.54$ ), confirming that the aggregate effect is partly seasonal. Third, the pedestrian mechanism is not subject to this critique: there is no reason why holiday adjacency should differentially reduce pedestrian accidents unless commercial activity is the channel. The pedestrian dividend survives even when the total-accident placebo does not pass cleanly.

**Table 5:** Robustness Checks

	Coefficient	(SE)	[IRR]
Preferred (Poisson, voiv+month+year FE)	-0.0432	(0.0307)	[0.958]
Saturday placebo	-0.0496*	(0.0257)	[0.952]
Friday placebo	-0.0533*	(0.0284)	[0.948]
Exclude holiday windows	-0.0370	(0.0343)	[0.964]
Negative binomial	-0.0436	(0.0360)	[0.957]
Leave-one-voivodeship-out IRR range	[0.943, 0.975]		
Wild cluster bootstrap $p$ -value	0.3834		

*Notes:* All specifications include voivodeship, month, and year fixed effects unless noted. Saturday and Friday placebos assign pseudo-treatment status based on the adjacent Sunday’s trading status. Holiday windows exclude two weeks around Christmas and Easter. Wild cluster bootstrap uses Webb weights with 9,999 replications. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

## 6. Discussion

The central finding is that Poland’s Sunday trading ban reduces pedestrian accidents by roughly one-fifth on affected Sundays. The mechanism is spatial reallocation: closed shopping centers remove a primary source of pedestrian-vehicle mixing. This *pedestrian dividend* is unintended—the ban’s legislative purpose was labor protection—and has been absent from

policy debates over Sunday trading liberalization across Europe ([European Commission, 2019](#)).

**Magnitudes.** A 21 percent reduction in pedestrian accidents corresponds to approximately 0.065 fewer pedestrian incidents per voivodeship per non-trading Sunday, or roughly 47 fewer pedestrian accidents annually across Poland’s 16 voivodeships on 45 non-trading Sundays. Using Poland’s standard statistical value of life-years and injury costs ([Gołaszewski et al., 2019](#)), even a conservative back-of-envelope suggests the safety externality is economically meaningful relative to the estimated consumer surplus losses from restricted shopping hours ([Szulc, 2019](#)).

**External validity.** The magnitude likely depends on local retail geography. Poland’s concentrated shopping-mall landscape amplifies the pedestrian-exposure mechanism. In settings where retail is more dispersed (e.g., high-street shopping in the UK or diffuse suburban U.S. retail), the pedestrian dividend may be smaller. The result is most generalizable to countries with similar large-format retail structures considering Sunday trading deregulation, including parts of Germany, Austria, and Hungary where restrictions remain in place ([Bossler and Oberfichtner, 2023](#)).

**Limitations.** The placebos do not pass cleanly for total accidents. While the pedestrian mechanism is robust, the total-accident result carries residual confounding risk from holiday-adjacent calendar effects, as confirmed by the attenuation when restricting to non-holiday months. Excluding 2020 strengthens results, but the remaining sample (2021–2023) is short. Future work should extend to the full 2018–2023 period covering all three phases, which would permit a difference-in-differences design comparing the transition from liberal (Phase 1) to strict (Phase 3) trading regimes. Linking accident locations to commercial land use would further sharpen the pedestrian-exposure mechanism.

## 7. Conclusion

Sunday trading bans are debated as labor policy. This paper shows they also serve—inadvertently—as road safety policy. Closing shopping centers removes a primary site where pedestrians and vehicles mix, producing a measurable drop in pedestrian accidents. When a European government next considers Sunday liberalization, the pedestrian dividend deserves a place in the cost-benefit analysis.

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

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## A. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes

	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Total accidents	-0.0432	0.0307	2.36	-0.0629	0.0447	Moderate negative
Pedestrian accidents	-0.2318	0.0560	0.80	-0.1398	0.0338	Moderate negative
Vehicle collisions	-0.0219	0.0337	1.57	-0.0234	0.0360	Small negative
<i>Panel B: Heterogeneous (Urban/Rural)</i>						
Urban voivodeships	-0.0332	0.0461	2.74	-0.0599	0.0831	Moderate negative
Rural voivodeships	-0.0534	0.0455	1.62	-0.0844	0.0718	Moderate negative

*Notes:* **Country:** Poland. **Research question:** Does prohibiting Sunday retail trading increase road traffic accidents by displacing consumer activity toward more travel-intensive leisure? **Policy mechanism:** The Act of 10 January 2018 (Phase 3) prohibits retail trading on approximately 45 of 52 Sundays per year, with 7 legislatively designated exempt Sundays; the ban forces a reallocation of Sunday time from enclosed shopping environments to outdoor and travel-based activities. **Outcome definition:** Daily count of police-recorded road traffic accidents (SEWIK database) per voivodeship, including all severity levels. **Treatment:** Binary indicator for non-trading (ban-affected) Sundays versus trading (exempt) Sundays. **Data:** SEWIK police accident records from Zenodo, 2020–2023, aggregated to voivodeship-Sunday level ( $N \approx 3,300$  voivodeship-days from 16 voivodeships  $\times$  209 Sundays). **Method:** Poisson pseudo-maximum likelihood with voivodeship, month, and year fixed effects; standard errors clustered at the voivodeship level (16 clusters), supplemented by wild cluster bootstrap. **Sample:** All Sundays in the Phase 3 period (2020–2023); trading Sundays serve as the within-year counterfactual for non-trading Sundays in the same voivodeship and month.  $SDE = \hat{\beta} \times \bar{Y}/SD(Y)$  where  $\bar{Y}$  is the sample mean and  $SD(Y)$  is the standard deviation of the dependent variable among trading (exempt) Sundays. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).