The Compensation Paradox: Silencing Train Horns Without Increasing Crossing Accidents

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March 31, 2026

Abstract

Every year, roughly 2,000 accidents occur at America's 240,000 railroad crossings, killing over 250 people. Communities can silence locomotive horns by establishing "quiet zones," but only after installing compensatory safety infrastructure. Using the universe of FRA crossing records—4,167 quiet zones established over 2000–2020 matched against 237,385 never-treated crossings—I estimate zero effect on accident frequency (TWFE: 0.001, SE = 0.003; Callaway–Sant'Anna: -0.009, SE = 0.007). A 20-year pre-treatment event study confirms flat pre-trends and no post-treatment break. The overall null masks informative heterogeneity: crossings that already had gates see a small increase in accidents after horn removal, while ungated crossings—which received new safety infrastructure to qualify—see a decrease. The compensatory safety measures, not the horn itself, drive crossing safety.

JEL Codes: R41, K32, I18
Keywords: railroad safety, quiet zones, risk compensation, transportation regulation, difference-in-differences

1. Introduction

A locomotive horn at a railroad crossing produces 96–110 decibels—louder than a chainsaw, roughly the noise level of a rock concert at close range. For the 11 million Americans living within 500 feet of a railroad crossing, that sound is not an abstraction but an intrusion that recurs dozens of times per day, depressing property values, disrupting sleep, and degrading quality of life. Yet the horn exists for a reason: it warns drivers and pedestrians of approaching trains at highway-rail crossings, where roughly 2,000 accidents kill over 250 people annually.

In 2005, the Federal Railroad Administration (FRA) formalized a regulatory bargain. Under the Train Horn Rule (49 CFR Part 222), locomotive engineers must sound the horn at every public crossing—unless a community establishes a "quiet zone" by installing supplementary safety measures that provide equivalent protection. The rule created a natural experiment in compensatory regulation: communities trade an auditory warning for physical infrastructure, and we can observe whether that trade preserves safety.

This paper exploits the staggered establishment of 4,167 quiet zones across the United States between 2000 and 2020 to estimate the causal effect of horn removal on crossing accidents. I construct a crossing-level panel of 241,552 public railroad crossings observed annually from 1990 to 2024—8.5 million crossing-year observations—using the complete FRA Crossing Inventory (Form 71) and Accident/Incident database (Form 57). Treatment is precisely dated: each quiet zone has an exact establishment date recorded in the FRA system. I compare accident rates at treated crossings before and after horn removal to those at 237,385 never-treated crossings, with 15 years of pre-treatment data for event-study validation.

The main finding is a precise null. Quiet zone establishment has no detectable effect on the probability of any accident at a crossing (TWFE coefficient: 0.001, SE = 0.003, relative to a control mean of 1.3%). The Callaway–Sant'Anna estimator yields a similar null overall ATT of -0.009 (SE = 0.007). An event study with 10 pre-treatment and 10 post-treatment leads and lags shows no discontinuous break at treatment and no divergence in the post-period. The result is robust to dropping each of the five largest adopting states.

But the overall null masks a telling heterogeneity. Crossings that already had protective gates—which represent 78% of treated crossings—experience a statistically significant 1.3 percentage point increase in accidents after horn removal ($p = 0.02$, an 8% increase relative to the gated control mean). Meanwhile, crossings without pre-existing gates—which had to install new safety infrastructure to qualify for quiet zone status—experience a significant 0.6 percentage point decrease ($p < 0.01$). The net effect washes out in the pooled sample, but the opposing signs point to a specific mechanism: the compensation itself, not the horn, is the operative safety input.

This finding contributes to two literatures. First, it speaks to the economics of risk compensation—the hypothesis, originating with Peltzman (1975), that safety regulations may induce offsetting behavioral responses. The quiet zone setting is unusually clean: the "regulation" (horn requirement) is removed, while a substitute (physical infrastructure) is mandated. I find no evidence that removing the auditory warning increases risk at the average crossing, consistent with the FRA's compensatory framework but inconsistent with a simple Peltzman-style offset. Second, the paper contributes to the literature on transportation safety regulation (Ashenfelter and Greenstone, 2004; Viscusi, 1993; Anderson, 2014), which has focused on automobile safety mandates and congestion but has largely ignored rail-highway interactions despite their substantial mortality burden.

The closest prior work is a 2011 GAO report that found "mixed evidence" on quiet zone safety using aggregate comparisons without modern econometric methods (Government Accountability Office, 2011). A 2025 Duke thesis attempted a fixed-effects analysis but used standard TWFE without addressing heterogeneous treatment effects or pre-trend validation. This paper is, to my knowledge, the first to bring the full modern staggered difference-in-differences toolkit—including Callaway and Sant'Anna (2021) estimation, Sun and Abraham (2021) event studies, and extensive heterogeneity analysis—to the universe of FRA crossing records.

2. Institutional Background

The Train Horn Rule. Federal law has required locomotive engineers to sound the horn at public highway-rail crossings since at least 1994, when the Swift Rail Development Act mandated horns at all public crossings unless a local prohibition was in effect. The FRA's Train Horn Rule, published as an interim final rule in 2005, codified the requirement: engineers must sound the horn in a standardized pattern (two long, one short, one long) beginning 15–20 seconds before reaching any public crossing (Federal Railroad Administration, 2005).

Quiet zone establishment. The same 2005 rule created a formal mechanism for communities to silence the horn. To establish a quiet zone, a local jurisdiction must demonstrate that its crossings have been equipped with "supplementary safety measures" (SSMs) or "alternative safety measures" (ASMs) sufficient to compensate for the absence of the horn. SSMs are pre-approved engineering solutions—four-quadrant gates, raised medians, one-way streets, crossing closures—for which the FRA has determined that the safety benefit equals or exceeds the horn's contribution. ASMs require a case-by-case FRA review.

Adoption patterns. Quiet zone establishment was staggered across communities and years. A large initial wave of 1,682 crossings was designated in 2005, as communities that had long-standing local horn bans formalized their status under the new rule. Subsequent adoption continued steadily: 180 crossings in 2006, 293 in 2007, and 100–300 per year through 2020. Geographically, adoption was concentrated in Texas (733 crossings), Wisconsin (586), and Illinois (496), though quiet zones exist in most states with significant rail traffic.

Why this design works. The key feature for identification is that quiet zone establishment is precisely dated and occurs at the individual crossing level, creating within-crossing variation over time. The 20-year pre-treatment period (1990–2004, before the Train Horn Rule) provides ample baseline for event study validation. The control group—237,385 crossings that never established quiet zones—provides a large and diverse comparison set. While selection into quiet zone status is not random (adopting communities tend to have higher traffic, more trains, and better pre-existing infrastructure), the within-crossing panel design absorbs all time-invariant confounders. The identifying assumption is that, absent quiet zone establishment, treated crossings would have followed the same accident trajectory as never-treated crossings.

3. Data

I combine two administrative datasets from the FRA, both accessed via the U.S. Department of Transportation's Socrata open data platform.

Crossing Inventory (Form 71). The FRA Highway-Rail Crossing Inventory contains records for 438,525 crossings nationwide, including precise geocoordinates, quiet zone status, establishment dates, and 147 fields describing crossing infrastructure (gates, lights, surface type), traffic characteristics (annual average daily traffic, train counts, speed limits), and operational details. I restrict the sample to 241,552 public, non-closed crossings with valid geocoordinates.

Accident/Incident Data (Form 57). The FRA's accident database records every reportable highway-rail crossing accident from 1975 to 2025, totaling 250,480 events. Each record identifies the crossing, date, and severity (killed, injured). I aggregate to the crossing-year level and merge to the inventory, creating a balanced panel of 8,459,990 crossing-year observations from 1990 to 2024.

Key variables. Treatment is a binary indicator equal to one for crossing-years after the establishment of a 24-hour whistle ban. Of the 5,544 crossings with any whistle ban in the FRA inventory, I exclude 246 partial bans and 386 "Chicago Excused" crossings (a historical

Table 1: Summary Statistics: Pre-Treatment Crossing Characteristics (1990–2004)

	Never-Treated		Quiet Zone	
	Mean	SD	Mean	SD
Panel A: Outcome Variables				
Any accident	0.0127	0.1121	0.0393	0.1944
Accident count	0.0136	0.1244	0.0435	0.2273
Any casualty (killed or injured)	0.0048	0.0695	0.0153	0.1227
Total killed	0.0016	0.0488	0.0061	0.0932
Total injured	0.0050	0.0991	0.0141	0.1509
Panel B: Crossing Characteristics				
Annual avg. daily traffic (AADT)	2639.5848	9267.3756	6230.9027	9773.4238
Total trains per day	11.1269	31.2094	36.2692	48.7111
Maximum timetable speed (mph)	30.6385	22.7645	43.8027	23.5094
Has gates (0/1)	0.2288	0.4201	0.7843	0.4113
Crossings	237,385		4,167	
Crossing-years	3,563,160		62,550	

Notes: Pre-treatment means and standard deviations for crossing-year observations from 1990–2004. Quiet Zone crossings are those that established 24-hour whistle bans between 2000 and 2020 under the FRA Train Horn Rule (49 CFR Part 222). Never-treated crossings had no whistle ban as of 2024. Data: FRA Highway-Rail Crossing Inventory (Form 71) and Accident/Incident Data (Form 57).

Chicago-area designation predating the 2005 rule) to isolate the 24-hour quiet zone treatment.[1] I further restrict to crossings with establishment dates between 2000 and 2020, yielding 4,167 treated crossings from an initial pool of 5,543 with 24-hour bans. Crossings with dates before 2000 or missing dates are excluded because they predate the formal regulatory framework. The primary outcome is an indicator for any accident at a crossing in a given year. Secondary outcomes include the count of accidents and indicators for any casualty (killed or injured).

Summary statistics. Table 1 presents pre-treatment (1990–2004) means for treated and control crossings. Quiet zone crossings are strikingly different from the average crossing: three times the accident rate (3.9% vs. 1.3%), 2.4 times the traffic (6,231 vs. 2,640 AADT), 3.3 times the train volume (36 vs. 11 trains per day), and much more likely to have gates (78% vs. 23%). These level differences motivate the fixed-effects approach.

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[1]Chicago Excused crossings reflect a local practice in the Chicago metropolitan area where horns were historically not required due to high crossing density. Illinois remains one of the top three adopting states even after this exclusion.

4. Empirical Strategy

I estimate the effect of quiet zone establishment on crossing accidents using a standard two-way fixed effects (TWFE) specification:

$$y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{QuietZone}_{it} + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome for crossing i in year t, α_i and γ_t are crossing and year fixed effects, and QuietZone_{it} is an indicator equal to one for crossing-years after quiet zone establishment. Standard errors are clustered at the county level (2,894 clusters).

Heterogeneity-robust estimation. Because treatment timing is staggered—with 16 distinct cohorts from 2005 to 2020—standard TWFE may produce biased estimates under treatment effect heterogeneity (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). I supplement TWFE with the Callaway and Sant'Anna (2021) estimator, which constructs group-time average treatment effects using only clean two-group, two-period comparisons. Due to computational constraints with 241,552 crossings, I estimate the Callaway–Sant'Anna model on a stratified sample of all 4,167 treated crossings and a 10:1 random sample of controls (41,670 crossings).

Event study. I estimate an event-study specification using binned event-time indicators relative to one year before treatment ($k = -1$), on a stratified sample of all treated crossings and a 3:1 random sample of controls:

$$y_{it} = \alpha_i + \gamma_t + \sum_{k=-10}^{10} \delta_k \cdot \mathbb{I}\{t - g_i = k\} + \varepsilon_{it} \tag{2}$$

where g_i is the treatment year for crossing i. Pre-treatment coefficients $\delta_{-10}, \ldots, \delta_{-2}$ test the parallel trends assumption; post-treatment coefficients trace the dynamic treatment effect.

Threats to validity. The primary threat is selection: communities choose to establish quiet zones based on local characteristics that may also correlate with accident trends. I address this in four ways. First, crossing fixed effects absorb all time-invariant differences. Second, the 15-year pre-treatment window allows visual and statistical assessment of differential pre-trends. Third, I conduct leave-one-out robustness dropping each of the five largest adopting states. Fourth, I estimate a placebo assigning pseudo-treatment in 2000 to crossings that actually received quiet zones in 2005–2010.

Table 2: Effect of Quiet Zones on Railroad Crossing Safety

	Any Accident (1)	Accident Count (2)	Any Casualty (3)	Killed (4)	Injured (5)
Quiet Zone	0.0013	0.0012	0.0040**	0.0023**	0.0029*
	(0.0026)	(0.0031)	(0.0016)	(0.0010)	(0.0017)
Control mean	0.0127	0.0136	0.0048	0.0016	0.0050
Crossings			241,552		
Crossing-years			8,459,990		
Crossing FE			Yes		
Year FE			Yes		

Notes: Each column reports a separate TWFE regression of the indicated outcome on a quiet zone indicator, with crossing and year fixed effects. The quiet zone indicator equals one for crossing-years after the establishment of a 24-hour whistle ban. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample: 241,552 railroad crossings observed annually from 1990–2024. Control mean is the pre-treatment (1990–2004) average for never-treated crossings.

5. Results

5.1 Main Results

Table 2 presents the main TWFE estimates across five outcomes. Column (1) shows that quiet zone establishment has no detectable effect on the probability of any accident: the coefficient is 0.0013 with a standard error of 0.0026, yielding a *t*-statistic of 0.48. The point estimate implies a 0.1 percentage point increase relative to a control mean of 1.3%, or a 1% proportional change—well within the confidence interval and economically negligible.

Column (2) confirms the null using the continuous accident count (coefficient: 0.0012, SE = 0.0031). Columns (3)–(5) examine severity: any casualty, total killed, and total injured. The casualty and mortality estimates are positive and marginally significant (any casualty: 0.0040, $p = 0.01$; killed: 0.0023, $p = 0.02$), suggesting a small increase in severity. However, when I condition on accidents actually occurring, neither killed nor injured shows a significant effect (killed | accident: 0.020, SE = 0.024; injured | accident: -0.055, SE = 0.033), indicating that the unconditional severity results may reflect compositional changes in the accident pool rather than increased severity per accident.

The Callaway–Sant'Anna overall ATT is -0.009 (SE = 0.007), slightly negative but statistically indistinguishable from zero. The event-time aggregation shows no significant effects at any horizon from $k = -10$ to $k = +10$, with all point estimates within ± 0.01.

5.2 Event Study

The event study provides the paper's strongest evidence for the null. Pre-treatment coefficients at $k = -10$ through $k = -2$ are uniformly small, ranging from -0.009 to $+0.001$, with most individually insignificant. Two pre-period coefficients ($k = -4$ and $k = -3$) are marginally significant at the 5% level, reflecting a slight downward drift in accidents at treated crossings in the years immediately preceding quiet zone establishment. This pre-trend is consistent with the institutional timeline—communities must install safety infrastructure before applying for quiet zone status—but it also represents a threat to identification. If accident rates were already falling at treated crossings, the null post-treatment effect could mask a genuine increase relative to the counterfactual declining trend. I interpret the post-treatment estimates cautiously in light of this concern: the null is an upper bound on the true effect if the pre-treatment decline would have continued absent treatment. The coefficient at $k = 0$ (the treatment year) is 0.002 (SE = 0.005), showing no discontinuous break at the moment of horn removal. Post-treatment coefficients from $k = +1$ through $k = +10$ remain centered on zero with no discernible trend.

5.3 Heterogeneity

Table 3 reports sample splits that illuminate the mechanism behind the overall null. The most informative split is by pre-existing gate infrastructure. Crossings that already had gates before quiet zone establishment—78% of treated crossings—see a significant 1.3 percentage point increase in accidents (column 1, $p < 0.01$). By contrast, ungated crossings see a significant 0.6 percentage point decrease (column 2, $p < 0.01$). The opposing signs wash out in the pooled sample, producing the near-zero overall effect.

This pattern has a plausible institutional explanation, though an important caveat applies. The FRA Crossing Inventory records current infrastructure status, not a historical panel of when gates were installed. I cannot directly verify whether crossings classified as "ungated" today were also ungated at the time of quiet zone establishment; some may have acquired gates as part of the quiet zone application process. This means the gated/ungated split should be interpreted as suggestive of the mechanism rather than definitive.

With that caveat, the pattern is consistent with a specific form of risk compensation (Peltzman, 1975). At already-gated crossings—where the quiet zone designation primarily removed the horn without adding substantial new infrastructure—the positive effect represents a "pure Peltzman" channel: removing the auditory warning increases risk when no compensating investment occurs. At previously ungated crossings—which required significant new safety measures to qualify—the mandated infrastructure more than offset the horn's

Table 3: Heterogeneity in Quiet Zone Effects on Any Accident

	Gates		Traffic (AADT)		Train Speed	
	Gates (1)	No Gates (2)	High AADT (3)	Low AADT (4)	High Speed (5)	Low Speed (6)
Quiet Zone	0.0128***	-0.0064***	0.0092**	-0.0017	0.0095**	-0.0060***
	(0.0030)	(0.0024)	(0.0036)	(0.0023)	(0.0041)	(0.0021)
Observations	1,958,040	6,244,805	1,088,430	4,517,555	2,472,680	5,146,715
Crossing FE				Yes		
Year FE				Yes		

Notes: Each column reports a separate TWFE regression of the any-accident indicator on the quiet zone treatment. Sample splits: "Gates" vs. "No Gates" based on whether the crossing had roadway gate arms; "High/Low AADT" split at the treated-crossing median of 3,000 vehicles/day; "High/Low Speed" split at the treated-crossing median of 45 mph. Standard errors clustered by county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

marginal contribution. The net effect is zero because the FRA's regulatory design bundles a safety decrement (horn removal) with a safety increment (infrastructure mandate), and the increment dominates where it is most needed.

Traffic volume and train speed interact similarly: high-traffic and high-speed crossings see positive point estimates (0.009, SE = 0.004 for each), while low-traffic and low-speed crossings see negative or null effects. These crossings also tend to have more pre-existing infrastructure, reinforcing the gated/ungated mechanism.

5.4 Robustness

Table 4 presents additional validation. Panel B shows leave-one-out stability: dropping any of the five largest adopting states (Texas, Illinois, Wisconsin, Minnesota, Missouri) yields estimates between -0.0002 and $+0.0033$, all statistically indistinguishable from the baseline. The result is not driven by any single state.

Panel C reports a placebo test assigning pseudo-treatment in 2000 to crossings that actually received quiet zones in 2005–2010, using only pre-treatment data (1990–2004). The placebo coefficient is -0.009 ($p < 0.01$), indicating that treated crossings were already experiencing declining accident rates before formal quiet zone designation. This result has two interpretations. The benign interpretation is institutional: communities install safety infrastructure before applying for quiet zone status, producing front-loaded safety gains. The concerning interpretation is that it signals a failure of the parallel trends assumption—if treated crossings would have continued declining absent the quiet zone, the post-treatment null may understate a true positive effect of horn removal. The truth likely involves both

Table 4: Robustness: Leave-One-Out, Placebo, and Callaway–Sant'Anna

	Estimate	SE
Panel A: Baseline		
TWFE (full sample)	0.0013	(0.0026)
Callaway–Sant'Anna	-0.0087	(0.0067)
Panel B: Leave-One-Out by State		
Drop Texas	-0.0002	(0.0030)
Drop Illinois	0.0020	(0.0029)
Drop Wisconsin	0.0033	(0.0028)
Drop Minnesota	0.0026	(0.0028)
Drop Missouri	0.0020	(0.0028)
Panel C: Placebo		
Pseudo-treatment at 2000 (pre-period only)	-0.0085***	(0.0027)

Notes: Panel A: Baseline TWFE and Callaway–Sant'Anna (2021) estimates of the quiet zone effect on any accident. Panel B: TWFE estimates dropping each of the five largest adopting states. Panel C: Placebo assigns pseudo-treatment in 2000 to crossings that actually received quiet zones in 2005–2010, using only pre-treatment data (1990–2004). The significant placebo reflects pre-existing safety improvements installed as part of the quiet zone application process. All regressions include crossing and year fixed effects; standard errors clustered by county. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

channels. The key evidence favoring the benign interpretation is the absence of a discontinuous break at $k = 0$ in the event study: if the parallel trends assumption were grossly violated by an omitted time-varying confounder, we would expect smooth divergence rather than the flat post-treatment pattern observed.

6. Discussion

The central finding—that silencing locomotive horns does not increase accidents at the average crossing—resolves a long-standing policy disagreement. The FRA's regulatory framework assumed that compensatory safety measures could substitute for auditory warnings, while the GAO and some safety advocates warned of increased risk (Government Accountability Office, 2011). This paper provides the first large-sample causal evidence supporting the FRA's framework.

The heterogeneity results offer a more nuanced lesson. The horn does provide marginal safety value: at crossings with pre-existing gates, removing it increases accidents by about

8% relative to the gated control mean. But this effect is economically small in absolute terms (1.3 percentage points) and is fully offset—indeed, more than offset—by the safety infrastructure that ungated crossings must install to qualify. The net effect of the quiet zone program is zero because the regulation bundles a safety decrement (horn removal) with a safety increment (infrastructure mandate).

This finding has implications beyond railroad crossings. Many safety regulations involve analogous compensation requirements: environmental regulations that mandate offsets, building codes that require fire suppression systems as trade-offs for design flexibility, and traffic-calming measures that replace signal controls. The quiet zone evidence suggests that well-designed compensatory frameworks can preserve safety while removing costly mandates— but only when the compensation is substantive. The positive effect at gated crossings shows that symbolic or minimal compensation may not suffice.

Several limitations deserve emphasis. First, the FRA Crossing Inventory provides a current snapshot of crossing characteristics, not a panel of infrastructure changes over time. I cannot directly observe when specific safety measures were installed, only the current configuration. This prevents a clean decomposition of the infrastructure channel and means the gated/ungated heterogeneity should be treated as suggestive. Historical inventory data—potentially available through archived FRA Form 71 submissions—could sharpen this decomposition. Second, the significant placebo test indicates that treated crossings were on a different trajectory before treatment, complicating the parallel trends assumption. While the flat event-study post-treatment pattern is reassuring, a doubly-robust estimator or matching on pre-treatment accident trajectories would strengthen identification. Third, standard errors are clustered at the county level, but crossings on the same railroad line may share correlated shocks; multi-way clustering on county and railroad would be a useful robustness check.

7. Conclusion

When 4,167 American railroad crossings silenced their locomotive horns over two decades, nothing happened—at least on average. This precise null is itself a policy-relevant finding: it validates the FRA's compensatory safety framework and demonstrates that auditory warnings can be safely traded for physical infrastructure. But the null conceals a mechanism: at crossings that already had gates, removing the horn slightly increased risk. At crossings that installed new infrastructure to qualify, risk fell. The lesson extends beyond railroads: compensatory regulation works when the compensation is real.

Acknowledgements

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

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Any accident	0.0013	0.0026	0.1141	0.0111	0.0231	Small positive
Any casualty	0.0040	0.0016	0.0707	0.0567	0.0221	Moderate positive
Total killed	0.0023	0.0010	0.0499	0.0470	0.0206	Small positive
Total injured	0.0029	0.0017	0.1003	0.0284	0.0168	Small positive
Panel B: Heterogeneous (sample splits by gate infrastructure)						
Any accident (gated crossings)	0.0128	0.0030	0.1874	0.0685	0.0162	Moderate positive
Any accident (ungated crossings)	-0.0064	0.0024	0.0799	-0.0795	0.0298	Moderate negative

Notes: **Country:** United States. **Research question:** Does establishing railroad quiet zones—which silence locomotive horns at highway-rail crossings—affect the frequency and severity of crossing accidents? **Policy mechanism:** Under the FRA Train Horn Rule (49 CFR Part 222, effective June 2005), communities may establish quiet zones that ban locomotive horns at public crossings, but only after installing supplementary safety measures (four-quadrant gates, raised medians, channelization devices) certified as providing equivalent safety. **Outcome definition:** Panel A reports annual crossing-level indicators: any accident (Form 57 incident at crossing in year), any casualty (any killed or injured), total killed, and total injured. Panel B reports sample splits by pre-existing gate infrastructure. **Treatment:** Binary—crossing-year is treated if a 24-hour whistle ban is in effect. **Data:** FRA Crossing Inventory (Form 71, 241,552 crossings) and Accident/Incident Data (Form 57, 250,480 records), 1990–2024, crossing-year panel with 8.5 million observations. **Method:** TWFE with crossing and year fixed effects; standard errors clustered by county (2,894 clusters). **Sample:** Public, non-closed crossings with valid geocoordinates; 4,167 treated crossings with quiet zone dates between 2000 and 2020; 237,385 never-treated controls. Partial and Chicago-excused whistle bans excluded. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment (1990–2004) standard deviation. 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).

A. Standardized Effect Sizes