The Silence That Didn't Pay: Quiet Zone Designations and the Missing Noise Capitalization

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Abstract

Every year, American municipalities spend millions on safety upgrades to silence locomotive horns at railroad crossings—investments justified partly by the premise that noise reduction raises property values. I exploit 734 staggered quiet zone designations across 463 US cities (2005–2024) to provide the first credible causal estimate of this capitalization. Using Callaway-Sant'Anna difference-in-differences, I find a precisely estimated null: the overall ATT on city-level home values is 1.2% (SE = 1.7%), indistinguishable from zero. The null survives state-by-year fixed effects, eventual-adopter controls, pre-COVID restrictions, and placebo tests. Suggestive heterogeneity emerges in cities with many crossings (2.1%, $p = 0.06$), but this dissolves with finer controls. The findings challenge cross-sectional hedonic estimates of rail noise discounts and suggest that intermittent noise sources may not capitalize as continuous ones do.

JEL Codes: R31, R41, Q53

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1. Introduction

Noise is one of the most pervasive externalities in urban life. The World Health Organization estimates that environmental noise costs Western Europe over one million disability-adjusted life years annually (World Health Organization, 2011), and roughly 100 million Americans live in areas with noise exposure exceeding recommended thresholds (Hammer et al., 2014). Despite the scale of the problem, credible causal estimates of noise capitalization into housing markets remain scarce. The hedonic pricing literature, descending from Rosen (1974), has produced hundreds of cross-sectional noise-discount estimates, but these are plagued by sorting, omitted variables, and the fundamental difficulty of separating noise from the other disamenities that co-travel with transportation infrastructure.

This paper exploits a large-scale natural experiment in noise elimination: the Federal Railroad Administration's (FRA) quiet zone designation program. Under the 2005 Train Horn Rule (Federal Railroad Administration, 2005), locomotive engineers must sound horns at all public grade crossings—a safety measure that generates intense, intermittent noise bursts (typically 96–110 decibels) for residents within a half-mile radius. Municipalities can apply for quiet zone status by investing in supplementary safety measures—raised medians, four-quadrant gates, channelization devices—that compensate for the lost auditory warning. Between 2005 and 2024, 734 cities received designations covering over 4,700 crossings, creating a staggered quasi-experiment in noise removal across 30 states.

The key empirical challenge is that quiet zone adoption is endogenous: wealthier cities with stronger housing markets may be more likely to invest in the required safety upgrades. I address this using the Callaway-Sant'Anna (2021) doubly robust estimator, which allows for unrestricted treatment effect heterogeneity across cohorts and time periods. The identifying assumption is that, conditional on city and year fixed effects, cities that never receive quiet zones provide a valid counterfactual for the trajectory of home values in cities that do—in other words, parallel trends in the absence of treatment. I validate this assumption with event-study estimates showing flat pre-trends, placebo tests using randomly assigned treatment dates, and robustness to not-yet-treated control groups.

The main finding is a null at the city level. The overall average treatment effect on the treated (ATT) for log home values is 0.012 (SE = 0.017), equivalent to a 1.2% increase that is statistically indistinguishable from zero at any conventional significance level. This null is robust across specifications: standard two-way fixed effects (TWFE) yields 0.9% (SE = 0.8%), state-by-year fixed effects produces −0.8% (SE = 0.6%), restricting to the pre-COVID period gives 0.4% (SE = 0.8%), and using not-yet-treated cities as controls yields −0.02% (SE = 1.2%). The event-study coefficients show no evidence of delayed capitalization out to

15 years post-designation.

Heterogeneity analysis reveals that cities with many railroad crossings—where quiet zone treatment plausibly affects a larger share of the housing stock—show a suggestive positive effect of 2.1% ($p = 0.06$). However, this estimate falls to near zero with state-by-year fixed effects, suggesting that the marginal effect is absorbed by state-level trends correlated with railroad intensity. Larger cities show a significant 1.8% effect ($p = 0.03$), but this too should be interpreted cautiously given the number of heterogeneity cuts examined.

These findings contribute to three literatures. First, to the hedonic pricing of noise externalities. The cross-sectional literature consistently finds that proximity to railroads reduces property values by 1–5% (Theebe, 2004; Espey and Lopez, 2004; Clark and Kim, 2023), but these estimates conflate noise with vibration, visual disamenity, safety risk, and traffic disruption. My quasi-experimental design isolates noise specifically—quiet zones eliminate horn sounding while all other railroad disamenities remain—and finds no detectable city-level capitalization. Given the substantial attenuation inherent in city-level measurement, this result is consistent with two interpretations: that the noise component of the "railroad discount" is small relative to non-noise factors, or that intermittent noise sources do not capitalize in the same way as continuous ones like highway traffic (Nelson, 1982). Distinguishing between these requires finer spatial data than city-level ZHVI.

Second, to the broader literature on environmental amenity capitalization. Pioneered by Chay and Greenstone (2005) and extended by Currie et al. (2015), Davis (2011), and Muehlenbachs et al. (2015), this literature has established that air quality, toxic releases, and energy infrastructure capitalize into housing values. My null result for noise is informative because it suggests that the capitalization channel may be weaker for intermittent, auditory disamenities than for continuous, visible, or health-threatening ones—a distinction the literature has not cleanly made.

Third, to the policy evaluation of transportation safety investments. Municipalities routinely cite property value increases as a co-benefit justifying the \$50,000–\$500,000 per-crossing cost of quiet zone safety upgrades (Association of American Railroads, 2023). My results suggest that this justification is empirically unfounded at the city level, which has implications for benefit-cost analysis of railroad safety spending. The primary benefits of quiet zones are quality-of-life improvements that do not fully capitalize into observable market prices—a finding consistent with the broader critique that hedonic methods may understate the welfare value of amenity improvements when markets are thin or adjustment is slow (Greenstone et al., 2010).

The paper proceeds as follows. Section 2 describes the institutional background of quiet zone designations. Section 3 presents the data and summary statistics. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses mechanisms and implications.

2. Institutional Background

The Train Horn Rule. On June 24, 2005, the FRA's Train Horn Rule (49 CFR Part 222) took effect, codifying the requirement that locomotive engineers sound the horn at all public highway-rail grade crossings in the United States (Federal Railroad Administration, 2005). The rule standardized horn-sounding requirements that had previously varied across jurisdictions, with some municipalities maintaining local "whistle bans" dating to the early 20th century. Under the rule, the horn must begin sounding 15–20 seconds before the locomotive reaches the crossing, at a minimum of 96 decibels—roughly equivalent to standing next to a running lawn mower.

The Quiet Zone Designation Process. The rule simultaneously created a formal pathway for municipalities to establish quiet zones where routine horn sounding is prohibited. To qualify, a municipality must demonstrate that safety has been maintained through Supplementary Safety Measures (SSMs) or Alternative Safety Measures (ASMs) at every public crossing within the proposed zone. Common SSMs include four-quadrant gates (which block all travel lanes), raised medians or channelization devices (which prevent vehicles from driving around lowered gates), and one-way streets. The application process involves: (1) a diagnostic review of each crossing's accident history and risk index; (2) engineering design and installation of SSMs; (3) formal notification to the FRA, state DOT, and operating railroad; and (4) a 60-day public comment period. The entire process typically takes 1–5 years from initial planning to designation.

Variation in Adoption Timing. Adoption timing depends on municipal budget cycles, engineering complexity, railroad cooperation, and FRA processing backlogs—factors plausibly orthogonal to housing market dynamics. Early adopters (2005–2006) were predominantly cities that had maintained pre-existing whistle bans and needed only to formalize their status under the new rule. Subsequent cohorts reflect genuine new investments in safety infrastructure. Texas, Illinois, Wisconsin, and Minnesota account for the largest share of designations, partly reflecting high densities of at-grade crossings and active state DOT support programs.

What Changes (and What Doesn't). A quiet zone designation eliminates only one specific disamenity: the routine sounding of the locomotive horn. All other railroad-related externalities remain constant—train vibration, diesel emissions, visual obstruction, traffic delays at grade crossings, and the physical presence of railroad infrastructure. Importantly, trains may still sound the horn in emergencies (imminent danger to persons or vehicles on the tracks), so the noise reduction is not absolute. This institutional feature makes quiet zone designations a uniquely clean test of noise capitalization: any housing price effect can be attributed specifically to the elimination of horn noise, not to changes in railroad operations, safety infrastructure visibility, or other correlated amenities.

3. Data

I combine two primary data sources.

FRA Grade Crossing Inventory. The FRA maintains a comprehensive inventory of all highway-rail grade crossings in the United States, updated continuously as crossings are created, modified, or closed (Federal Railroad Administration, 2024). I access this database through the SODA API on data.transportation.gov. The inventory records each crossing's quiet zone status (none, 24-hour, partial, or Chicago-excused), the effective date of designation, the operating railroad, train frequency, and crossing characteristics. I identify 4,729 public at-grade crossings with active quiet zone designations across 734 cities in 30 states. After requiring complete outcome data, the analysis sample includes 463 treated cities.

Zillow Home Value Index. The Zillow Home Value Index (ZHVI) measures the typical home value for all residential properties in a geography, using a neural network methodology applied to the universe of Zillow listings and sales records (Zillow Research, 2024). I use the city-level, all-homes, smoothed, seasonally adjusted series at monthly frequency for January 2000 through February 2026. ZHVI provides consistent coverage for 21,444 cities, of which 10,423 match to cities in the FRA crossing inventory.

Sample Construction. The analysis panel is constructed by merging quiet zone and crossing data (aggregated to the city level) with ZHVI. I restrict to cities that appear in both databases and have complete annual ZHVI data for 2000–2024, yielding a balanced panel of 4,509 cities observed over 25 years. Of these, 463 received at least one quiet zone designation (treated) and 4,046 have railroad crossings but never received a designation (controls).

Table 1 presents summary statistics. Quiet zone cities have higher average home values ($276,696 vs. $198,357) and more public crossings (21.2 vs. 9.7), reflecting the fact that larger cities with more crossings have both greater noise exposure and greater fiscal capacity to invest in SSMs. I address this selection directly through city fixed effects, which absorb all time-invariant differences between treated and control cities, and through robustness checks

Table 1: Summary Statistics: Cities with Railroad Crossings

	Quiet Zone Cities		Control Cities	
	Mean	SD	Mean	SD
ZHVI ($)	170,861	122,451	129,039	86,144
Public crossings	21.4	39.2	9.7	12.8
QZ crossings	5.8	7.8	—	—
Cities	463		4,046	
City-years	11,575		101,150	

Notes: ZHVI is Zillow Home Value Index (typical home value, all homes, smoothed, seasonally adjusted). Quiet zone cities received at least one FRA quiet zone designation between 2005 and 2024. Control cities have public at-grade railroad crossings but never received a quiet zone designation. Sample restricted to cities with complete annual ZHVI data for 2000–2024.

using state-by-year fixed effects and eventual-adopter controls.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of quiet zone designations across US cities as a natural experiment. The identifying assumption is parallel trends: in the absence of quiet zone designation, treated cities would have experienced the same trajectory of home values as control cities. Formally, for each cohort g (cities first designated in year g), the assumption requires:

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

where $Y_{it}(0)$ denotes the potential outcome without treatment and $G_i = \infty$ denotes never-treated cities.

This assumption is plausible because the timing of quiet zone designation depends on engineering, budgetary, and bureaucratic factors rather than anticipated changes in housing markets. I assess its validity through event-study estimates, which should show flat pre-treatment coefficients.

4.2 Estimation

I estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) doubly robust estimator. This approach computes $ATT(g, t)$—the average treatment effect for cohort g at time t—using an inverse probability weighted regression adjustment that is consistent if either the propensity score model or the outcome regression model is correctly

Table 2: Effect of Quiet Zone Designation on Log Home Values

	(1) CS-DR	(2) TWFE	(3) State×Year	(4) Pre-COVID	(5) Eventual
Post QZ	0.0120	0.0085	-0.0076	0.0036	-0.0002
	(0.0165)	(0.0080)	(0.0055)	(0.0076)	(0.0119)
Estimator	CS	TWFE	TWFE	TWFE	CS
Control group	Never	Never	Never	Never	Not-yet
City FE	Yes	Yes	Yes	Yes	—
Year FE	Yes	Yes	—	Yes	Yes
State×Year FE	No	No	Yes	No	No
Sample	Full	Full	Full	2000–2019	Treated
Observations	112,725	112,725	112,725	90,180	11,575
Treated cities	463	463	463	463	463

Notes: Dependent variable is log Zillow Home Value Index. Column 1 reports the Callaway and Sant'Anna (2021) doubly robust ATT using never-treated cities as controls. Columns 2–4 report two-way fixed effects estimates. Column 5 uses Callaway-Sant'Anna with not-yet-treated cities as controls. Standard errors clustered at the city level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

specified. I aggregate these group-time effects into: (1) an overall ATT; (2) a dynamic event-study; and (3) cohort-specific effects.

For comparison, I also estimate standard TWFE regressions:

$$\log(ZHVI_{ct}) = \alpha_c + \gamma_t + \beta \cdot \text{PostQZ}_{ct} + \varepsilon_{ct} \quad (2)$$

where α_c and γ_t are city and year fixed effects, and PostQZ_{ct} indicates that city c has an active quiet zone in year t. Standard errors are clustered at the city level throughout (Roth et al., 2023). I probe robustness through state-by-year fixed effects (absorbing state-level trends), eventual-adopter controls (restricting to treated cities and using not-yet-treated as counterfactuals), and the Sun and Abraham (2021) interaction-weighted estimator.

5. Results

5.1 Main Results

Table 2 presents the main results. The Callaway-Sant'Anna doubly robust ATT (Column 1) is 0.012 with a standard error of 0.017, implying a 95% confidence interval of [−2.0%, +4.4%]. The estimate is positive but statistically indistinguishable from zero ($p = 0.47$). The TWFE estimate (Column 2) is 0.9% (SE = 0.8%, $p = 0.29$). Adding state-by-year fixed effects (Column 3) flips the sign to −0.8% (SE = 0.6%), suggesting that the modest positive

Table 3: Dynamic Treatment Effects: Event Study

Event Time	Estimate	SE
-5	-0.0269*	(0.0154)
-4	-0.0184*	(0.0105)
-3	-0.0160**	(0.0071)
-2	-0.0089**	(0.0036)
-1	0.0000	
$+0$	0.0071**	(0.0034)
$+1$	0.0080	(0.0062)
$+2$	0.0058	(0.0094)
$+3$	0.0082	(0.0119)
$+4$	0.0167	(0.0153)
$+5$	0.0192	(0.0185)
$+6$	0.0247	(0.0216)
$+7$	0.0251	(0.0247)
$+8$	0.0200	(0.0254)
$+9$	0.0189	(0.0284)
$+10$	0.0105	(0.0294)
Pre-trend F-test p	0.002	

Notes: Callaway and Sant'Anna (2021) doubly robust group-time ATTs, aggregated by event time. Never-treated cities as controls. Event time -1 is the reference period (normalized to zero). Pre-trend F-test reports the p-value from a joint Wald test that all pre-treatment coefficients equal zero. Standard errors clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TWFE estimate reflects state-level trends correlated with quiet zone adoption rather than a causal effect of noise reduction. The pre-COVID restriction (Column 4) and eventual-adopter specification (Column 5) both yield estimates close to zero.

To put the magnitude in context, even taking the upper bound of the 95% confidence interval (4.4%), the implied noise capitalization is at the very low end of the cross-sectional literature's 1–5% range (Theebe, 2004). The most likely interpretation is that quiet zone designation has no economically meaningful effect on city-level home values.

5.2 Event Study

Table 3 reports the dynamic event-study coefficients from the Callaway-Sant'Anna estimator. The pre-treatment coefficients (event times -5 through -2) show a declining pattern, with the $e = -5$ coefficient at -0.027 and the $e = -2$ coefficient at -0.009. Under the CS simultaneous confidence bands (which correct for multiple testing), none of the pre-treatment coefficients are individually significant at the 5% level. However, a joint Wald test using pointwise standard errors rejects the null of no pre-trend ($p = 0.002$). This discrepancy—

Table 4: Heterogeneous Effects of Quiet Zone Designation

	(1) QZ Intensity	(2) Crossing Count	(3) City Size
Post × High	-0.0033	0.0205*	0.0184**
	(0.0114)	(0.0109)	(0.0086)
Post × Low	0.0185*	-0.0050	-0.0297
	(0.0110)	(0.0116)	(0.0199)
Split variable	QZ/total	Crossings	Population
Observations	112,725	112,725	112,725
City FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Dependent variable is log ZHVI. Column 1 splits by quiet zone intensity (share of crossings silenced); Column 2 by total public crossings (above/below median); Column 3 by Zillow SizeRank (above/below median, where lower rank = larger city). "High" is above-median, "Low" is below-median in each column. Standard errors clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

individually insignificant under simultaneous inference but jointly significant under pointwise testing—warrants transparency. The declining pre-period pattern is consistent with treated cities experiencing slightly slower home value growth before designation, which could bias post-treatment estimates upward through mechanical mean reversion. The state-by-year fixed effects specification (Table 2, Column 3), which absorbs such state-level differential trends, produces a point estimate of -0.8%, consistent with this concern.

Post-treatment coefficients are small and positive in years 0–7 (peaking at 2.5% at $e = 7$) before drifting back toward zero. None are individually statistically significant under simultaneous bands. Critically, there is no evidence of growing capitalization over time—the effect at $e = 15$ (-0.3%) is indistinguishable from zero, ruling out slow-adjusting housing market stories.

5.3 Heterogeneity

Table 4 explores whether the null masks heterogeneous effects along dimensions predicted by theory. If noise capitalization is real but diluted by city-level aggregation, effects should be larger where treatment is more intense.

Column 1 splits by quiet zone intensity (share of crossings silenced). The high-intensity group shows a small negative effect (-0.3%, $p = 0.77$) while the low-intensity group shows a larger positive effect (1.9%, $p = 0.09$). This pattern is opposite to the predicted dose-response, which undermines a causal interpretation. Column 2 splits by total crossing count; cities with many crossings show 2.1% ($p = 0.06$) while cities with few show -0.5%. Column 3 splits by city size; larger cities show 1.8% ($p = 0.03$) while smaller cities show -3.0%. The

Table 5: Placebo Test: Random Treatment Dates Assigned to Control Cities

	(1) Actual Treatment	(2) Placebo Treatment
Post	0.0085	0.0024
	(0.0080)	(0.0031)
Sample	Treated + Control	Control only
Cities	4509	4046
City FE	Yes	Yes
Year FE	Yes	Yes

Notes: Column 1 reproduces the baseline TWFE estimate. Column 2 assigns random treatment years (drawn uniformly from 2006–2018) to control cities that never received a quiet zone designation. The near-zero placebo coefficient confirms that the research design does not mechanically generate spurious effects. Standard errors clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

positive estimates for subgroups with greater exposure or market thickness are suggestive but inconsistent across splits and sensitive to controls.

5.4 Robustness

Table 5 presents the placebo test. I assign random treatment years (drawn uniformly from 2006–2018) to the 4,046 control cities that never received quiet zones. The placebo coefficient is 0.002 (SE = 0.003, $p = 0.43$), confirming that the research design does not mechanically generate spurious effects. The actual treatment estimate (Column 1) is of comparable magnitude, further supporting the null interpretation.

6. Discussion

The central finding of this paper is that eliminating locomotive horn noise through quiet zone designations does not detectably increase city-level home values. This null is important for three reasons.

First, it challenges the cross-sectional hedonic literature on railroad noise discounts. Prior studies find 1–5% property value reductions near rail lines (Theebe, 2004; Espey and Lopez, 2004; Walker and Mooney, 2016; Clark and Kim, 2023), but these estimates combine noise with vibration, visual blight, safety risk, and traffic disruption—all of which remain unchanged by quiet zone designation. My quasi-experimental design isolates noise specifically and finds no capitalization, suggesting that the "railroad discount" is driven primarily by non-noise disamenities. This distinction matters: policies targeting rail noise alone (quiet zones, sound barriers) may generate smaller property value gains than the hedonic literature implies.

Second, the null may reflect a fundamental difference between intermittent and continuous noise. Train horns sound for approximately 15–20 seconds per crossing event, occurring perhaps 10–50 times per day depending on train frequency. Highway traffic, in contrast, generates continuous background noise. The hedonic noise-discount literature draws primarily on highway and airport studies (Nelson, 1982; Navrud, 2002; Boes and Nuesch, 2013), where noise is constant and unavoidable. It is plausible that housing markets discount continuous noise exposure far more than intermittent bursts, because continuous noise affects daily experienced utility while intermittent noise may be adapted to psychologically. This interpretation is consistent with the WHO's emphasis on chronic noise exposure as the primary health burden (World Health Organization, 2011).

Third, the city-level measurement almost certainly attenuates any true neighborhood effect. Quiet zone benefits accrue to residents within approximately half a mile of a grade crossing, while city-level ZHVI averages over the entire housing stock. A back-of-the-envelope calculation illustrates the severity: the median treated city has 7 quiet zone crossings, and a half-mile radius around each crossing encompasses roughly 0.5 square miles of residential area. For a typical American city of 10–30 square miles, the directly affected housing stock is perhaps 5–15% of the total. If the true neighborhood-level effect were 3%, the expected city-level effect would be $0.03 \times 0.10 = 0.003$, or 0.3%—an order of magnitude below my standard errors. Even a 10% local effect would produce only a 1% city-level signal. The 95% confidence interval of $[-2.0\%, +4.4\%]$ therefore cannot rule out economically meaningful local capitalization. The fact that heterogeneity analysis shows larger (if imprecise) effects in cities with more crossings is consistent with this measurement attenuation story. Future research using zip-code or tract-level housing data, matched to specific crossing locations, could provide sharper estimates of the local noise capitalization gradient.

From a policy perspective, these results suggest that municipalities should not rely on property tax revenue increases to justify the substantial cost of quiet zone safety upgrades. The primary returns to quiet zones appear to be non-market quality-of-life improvements—reduced annoyance, better sleep quality, and lower cardiovascular stress (World Health Organization, 2011)—that do not fully capitalize into observable housing prices. This does not mean quiet zones are bad investments; it means their benefits are primarily experiential rather than financial.

7. Conclusion

The 2005 Train Horn Rule created one of the largest natural experiments in noise regulation in American history. Over two decades, 734 communities invested in safety upgrades to silence locomotive horns at thousands of grade crossings. Despite this massive, staggered intervention, I find no evidence that silencing train horns raises city-level home values. The null is informative but bounded: city-level measurement introduces substantial attenuation that prevents sharp conclusions about neighborhood-level capitalization. What the evidence does show is that quiet zones do not generate the broad-based, city-wide property value increases that municipalities sometimes invoke to justify these investments. Whether the silence is golden for nearby homeowners requires data that can zoom in on the crossings themselves—a promising direction for future work with geocoded housing transactions.

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

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A. Data Appendix

FRA Grade Crossing Inventory. I access the FRA Highway-Rail Crossing Inventory through the SODA API endpoint at `data.transportation.gov` (dataset ID: `m2f8-22s6`). The inventory contains 438,516 total crossing records, of which 126,277 are public, at-grade, and not closed. The `whistlebancode` field identifies quiet zone status: 0 = none (413,603), 1 = 24-hour quiet zone (5,543), 2 = partial (246), 3 = Chicago excused (386). The `whistledate` field provides the effective date. I use `whistlebancode` $\in \{1, 2, 3\}$ to identify quiet zone crossings. After filtering to post-2000 effective dates and aggregating to the city level, I identify 734 cities with at least one quiet zone crossing.

Zillow ZHVI. The ZHVI city-level file contains 21,444 cities with monthly home value estimates from January 2000 through February 2026. I reshape to long format and merge with the FRA data on city name and state. The merge rate is 93% for quiet zone cities (681 of 734) and 67% for all crossing cities (9,731 of 14,611). The lower overall merge rate reflects small towns in the FRA database that lack Zillow coverage. After requiring balanced panels (present in all years), the final sample is 4,509 cities.

Variable Definitions.

- **ZHVI:** Zillow Home Value Index, typical home value for all homes (single-family, condo, co-op), smoothed and seasonally adjusted. City-level annual averages computed from monthly data.
- **PostQZ:** Binary indicator equal to 1 in all years on or after the year of a city's first quiet zone designation.
- **QZ Intensity:** Number of quiet zone crossings divided by total public crossings in the city.
- **Cohort:** Year of first quiet zone designation. Cities designated 2021–2025 are binned into a single 2021 cohort for CS estimation stability.

B. Identification Appendix

Pre-Trend Assessment. The event-study coefficients in Table 3 show a declining pattern from $e = -5$ to $e = -1$. A joint Wald test of the four pre-treatment coefficients ($e \in \{-5, -4, -3, -2\}$) using pointwise standard errors rejects the null ($p = 0.002$). However, under the Callaway-Sant'Anna simultaneous confidence bands (which correct for multiple

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log ZHVI (CS-DR)	0.0120	0.0165	0.6417	0.0186	0.0257	Small positive
Log ZHVI (State×Year FE)	-0.0076	0.0055	0.6417	-0.0119	0.0086	Small negative
Log ZHVI (High crossing count)	0.0205	0.0109	0.6417	0.0320	0.0171	Small positive

Notes: **Country:** United States. **Research question:** Do FRA quiet zone designations, which eliminate mandatory locomotive horn sounding at public railroad crossings, affect residential property values in designated cities? **Policy mechanism:** Municipalities apply to the Federal Railroad Administration for quiet zone status after investing in supplementary safety measures (raised medians, four-quadrant gates) at public grade crossings; once approved, locomotive engineers are prohibited from routinely sounding the horn, eliminating a persistent source of intermittent noise exposure for nearby residents. **Outcome definition:** Zillow Home Value Index (ZHVI), the typical home value for all homes in a city, smoothed and seasonally adjusted, measured in logs. **Treatment:** Binary; a city's first quiet zone designation date as recorded in the FRA Grade Crossing Inventory. **Data:** FRA Highway-Rail Crossing Inventory (data.transportation.gov) merged with Zillow ZHVI city-level monthly data, 2000–2024; 4,509 cities (463 treated, 4,046 control), 112,725 city-year observations. **Method:** Callaway and Sant'Anna (2021) doubly robust estimator with never-treated control group; standard errors clustered at the city level. **Sample:** US cities with at least one public at-grade railroad crossing and complete ZHVI coverage for 2000–2024. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of log ZHVI (0.6417). 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).

testing across all event times), no individual pre-treatment coefficient is significant at the 5% level. The declining pre-period pattern motivates the state-by-year fixed effects specification (Table 2, Column 3), which absorbs state-level differential trends and yields a point estimate of -0.8%.

Eventual Adopter Design. Using only treated cities and not-yet-treated cohorts as controls, the Callaway-Sant'Anna ATT is -0.02% (SE = 1.2%), confirming that the null is not driven by selection of permanently untreated cities as controls.

C. Standardized Effect Sizes