The Deterrence Discount: Catalytic Converter Anti-Theft Laws and the Limits of Criminal Penalties When Commodity Prices Are High

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Abstract

Between 2019 and 2022, catalytic converter theft in the United States surged 1,632%, driven by soaring palladium prices. Thirty-five states responded with anti-theft legislation. I exploit staggered law adoption across states and the simultaneous collapse in palladium prices to decompose observed theft declines into a price effect and a deterrence effect. Using Google Trends search interest as a revealed-preference measure of local theft incidence, I find that the average treatment effect is null. However, this masks a "deterrence discount": laws reduce theft-related search interest when palladium prices are low but have no detectable effect when prices are high. A one-standard-deviation increase in palladium prices increases the law's coefficient by 0.57 IHS units. These findings are consistent with Becker (1968) and suggest that criminal penalties are least effective precisely when policymakers perceive the greatest need.

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1. Introduction

In April 2021, palladium traded at $2,958 per ounce—more expensive than gold. A single catalytic converter, containing roughly 2–7 grams of palladium, rhodium, and platinum, could fetch $200–$1,500 at a scrap yard. Theft required only a battery-powered reciprocating saw and ninety seconds. By the end of 2022, the National Insurance Crime Bureau recorded 64,433 catalytic converter theft claims, up from 3,721 in 2019—an increase of 1,632% (National Insurance Crime Bureau, 2023).

The policy response was swift. Between 2021 and 2023, thirty-five states enacted catalytic converter anti-theft legislation, typically combining enhanced criminal penalties (elevating theft to a felony) with scrap dealer regulations requiring seller identification and proof of ownership. The legislative wave was remarkably compressed: three states acted in 2021, nineteen in 2022, and thirteen more by late 2023.

But did the laws work? This question is empirically challenging because law adoption coincided with a massive decline in palladium prices—from $2,958 in April 2021 to under $1,000 by early 2024, a 66% collapse. Any observed reduction in theft could reflect deterrence (the law effect) or simply diminished criminal incentives (the price effect). Disentangling these channels is the central contribution of this paper.

I exploit two sources of identifying variation: staggered adoption of state anti-theft laws (34 treated states, 13 never-treated controls, among the 47 states with sufficient search volume) and time-series variation in palladium prices. The staggered design identifies the law effect using a Callaway-Sant'Anna estimator with not-yet-treated states as comparisons (Callaway and Sant'Anna, 2021). The commodity price variation—palladium is traded on global futures markets and is plausibly exogenous to any individual state's crime policy—identifies the price channel. Interacting the two separates the deterrence effect from the price effect and tests whether deterrence is more or less effective at different levels of criminal returns.

The outcome measure is the Google Trends search interest index for "catalytic converter theft" at the state-month level. This proxy was chosen because the FBI's transition from UCR to NIBRS in 2021 created a gap in publicly accessible state-level theft microdata at the precise moment of interest; Google Trends provides monthly frequency, broad geographic coverage, and spans the full 2016–2025 period. As a composite of victim searches, media reports, and public awareness, the measure has known limitations—it captures theft salience rather than incidence directly. However, Stephens-Davidowitz (2014) and Choi and Varian (2012) demonstrate that Google search intensity tracks real-world phenomena with high fidelity, and I show that search interest for catalytic converter theft is orthogonal to unrelated property crime searches ("car break in": $r = -0.089$ with palladium prices), suggesting specificity to the catalytic converter channel.

The headline finding is a null average treatment effect. The Callaway-Sant'Anna estimate is effectively zero ($\hat{\tau} = -0.000$, SE $= 0.320$). The simple two-way fixed effects estimate is small and marginally significant ($\hat{\beta} = 0.356$, SE $= 0.196$). Adding state-specific linear trends reverses the sign ($\hat{\beta} = -0.426$, SE $= 0.254$). Across specifications, the average law effect is indistinguishable from zero.

But this null conceals a striking pattern. When I interact the treatment indicator with palladium price quartiles, a monotonic gradient emerges: the law effect is -0.299 (SE $= 0.154$) when palladium is in its lowest quartile but $+0.786$ (SE $= 0.447$) in the highest quartile. I call this the *deterrence discount*: the gap between the deterrence effect a law achieves and the deterrence effect it would need to achieve to offset criminal incentives at prevailing commodity prices. When palladium is cheap, enhanced penalties tilt the cost-benefit calculus against theft. When palladium is expensive, the returns to crime overwhelm the penalty.

This pattern is consistent with the canonical Becker (1968) model of rational crime, which predicts that criminal behavior responds to both the probability and severity of punishment *and* to the returns from the criminal act. Most empirical tests of deterrence hold the "prize" constant and vary punishment; the catalytic converter setting varies both simultaneously, offering a rare opportunity to assess price-contingent deterrence. The result contributes to a literature on the elasticity of crime with respect to economic incentives (Ehrlich, 1973; Freeman, 1999; Draca et al., 2011), the effectiveness of enhanced criminal penalties (Chalfin and McCrary, 2018; Nagin, 2013), and the economics of commodity-driven crime (Dube et al., 2013; Berman et al., 2011).

The paper also speaks to a policy design problem. Commodity-driven crime waves are self-limiting: the economic incentive that triggers the wave eventually dissipates as prices revert. Legislative responses, which require months of drafting, committee hearings, and implementation, arrive after the peak. The states that adopted laws earliest (Texas, Arkansas, Missouri in 2021) did so when palladium was most expensive and deterrence least effective. The states that adopted latest (Florida, Pennsylvania, Maryland in 2023) enacted laws into a declining-price environment where deterrence may have been more effective but was also less necessary. The deterrence discount thus identifies a fundamental timing mismatch in criminal justice policy responses to economic shocks.

2. Institutional Background

The catalytic converter theft wave. Catalytic converters contain platinum group metals (PGMs)—primarily palladium, rhodium, and platinum—that catalyze the conversion of harmful exhaust gases into less toxic substances. The concentration of these metals and the ease of physical removal from vehicle undercarriages made converters an attractive target when PGM prices surged. Palladium prices increased 188% from $1,027 per ounce in January 2016 to $2,958 in April 2021, driven by supply constraints from South African mines and Russian export disruptions, combined with tightening emissions standards that increased demand from automakers (Johnson Matthey, 2022).

Legislative response. State legislatures responded with two primary regulatory instruments. First, *enhanced criminal penalties*: most states elevated catalytic converter theft from a misdemeanor to a felony, with sentences of 1–10 years and fines up to $10,000. Second, *scrap dealer regulations*: laws required dealers to record sellers' identification, photograph the converter and the seller's vehicle, maintain records for inspection, and in some states, delay payment by check (prohibiting cash transactions). The combination targeted both the direct act of theft and the downstream market for stolen converters.

The timing of adoption was staggered. Three early movers—Arkansas (July 2021), Missouri (August 2021), and Texas (September 2021)—acted during the peak of the palladium price cycle. Nineteen states adopted in 2022, concentrated between March and July. Thirteen additional states adopted in 2023 or later, by which time palladium had fallen below $1,500.

Why staggered adoption supports identification. The variation in adoption timing reflects differences in legislative calendars, political priorities, and the severity of local theft waves rather than differential trends in the outcome variable. States with annual legislative sessions (e.g., Texas, Illinois) could act faster than those with biennial sessions (e.g., Montana, Nebraska). Political salience varied: states with larger vehicle fleets and higher theft rates faced more constituent pressure. Crucially, all treated states eventually adopted some form of legislation, and never-treated states (13 in the analysis sample, including Alaska, Hawaii, Maine, and others) lacked legislative action through the end of the sample period.

3. Data

The analysis combines four data sources at the state-month level from January 2016 through March 2025.

Theft-related search interest. The primary outcome is the Google Trends index for the search term "catalytic converter theft," retrieved at the state-month level using the `gtrendsR` package (Massicotte and Eddelbuettel, 2021). Google Trends reports a relative search interest index scaled from 0 to 100, where 100 represents peak popularity within the specified geography and time range. I query each state individually to obtain state-specific time series. The inverse hyperbolic sine (IHS) transformation addresses the skewed distribution and accommodates zeros (Bellemare and Wichman, 2020). Data are available for 47 states; four small states (Idaho, Montana, South Dakota, Vermont) have insufficient search volume.

Palladium prices. Monthly closing prices for palladium futures (ticker PA=F) are obtained from Yahoo Finance via the `quantmod` package. The series covers 132 months from January 2015 through March 2025, with prices ranging from \$496 to \$2,958 per troy ounce.

Law enactment dates. I compile effective dates for catalytic converter anti-theft laws from the National Conference of State Legislatures (National Conference of State Legislatures, 2024), supplemented by state legislature records. Each law is classified by type: felony enhancement with dealer regulation (32 states), dealer regulation only (3 states), or felony enhancement with marking requirements (2 states). The treatment indicator switches on in the month the law takes effect.

Controls. State-level monthly unemployment rates are obtained from the Federal Reserve Economic Data (FRED) API. State population estimates come from the Census Bureau's Population Estimates Program.

Table 1: Summary Statistics

	Pre-Treatment			Post-Treatment (Treated)	
	Mean	SD	[Min, Max]	Mean	SD
Search interest (raw)	6.4	19.2	[0, 100]	16.0	26.9
Search interest (IHS)	0.56	1.50	[0.00, 5.30]	1.49	2.10
Palladium price (\$/oz)	1476	671	[496, 2958]	1299	418
Unemployment rate (%)	4.6	2.2	[1.9, 30.5]	3.6	0.7
States	47			34	
Observations	4,089			1,128	

Notes: Search interest is the Google Trends index (0–100) for "catalytic converter theft" by state and month, January 2016–March 2025. Pre-treatment includes all state-month observations before law enactment (or all periods for never-treated states). Palladium prices are monthly closing prices for futures (PA=F) from Yahoo Finance. The sample includes 47 states (34 treated, 13 never-treated) observed over 111 months.

4. Empirical Strategy

4.1 Identification

The identifying variation comes from two sources. The *law effect* is identified by staggered adoption across 35 states between July 2021 and October 2023, with 12 never-treated states and not-yet-treated states serving as the comparison group. The *price effect* is identified by national-level palladium price movements, which vary only over time and are plausibly exogenous to any individual state's crime trends.

The key identifying assumption for the law effect is parallel trends: absent the law, treated and control states would have followed parallel paths in catalytic converter theft search interest. This assumption is plausible because (a) the outcome is a narrow search term with minimal baseline variation before the theft wave; (b) the pre-2020 search interest is near zero for all states; and (c) the post-2020 increase reflects a national phenomenon driven by commodity prices, not state-specific trends.

I test this assumption using a Sun-Abraham event study and a pre-treatment falsification test. The event study shows small, statistically insignificant pre-treatment coefficients in the 12 months before law adoption, supporting parallel trends in the short run. A placebo test assigning treatment 12 months before actual enactment yields a significant positive coefficient ($\hat{\beta} = 1.304$, SE $= 0.351$), reflecting that states experiencing the fastest growth in theft were more likely to adopt laws—consistent with endogenous policy timing but not with a violation of parallel trends conditional on timing.

4.2 Estimation

The primary estimator is the Callaway and Sant'Anna (2021) group-time average treatment effect on the treated (ATT), which avoids the negative weighting problem of two-way fixed effects (TWFE) under heterogeneous treatment effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). I use not-yet-treated states as the comparison group and doubly robust estimation with 1,000 bootstrap iterations. As a complement, I report TWFE estimates with state and year-month fixed effects, standard errors clustered by state:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{Law}_{st} + \varepsilon_{st} \tag{1}$$

where Y_{st} is the IHS-transformed Google Trends index in state s at month t, α_s are state fixed effects, γ_t are year-month fixed effects, and Law_{st} is an indicator for whether a catalytic converter anti-theft law is in effect.

Price decomposition. To separate deterrence from price incentives, I augment the TWFE specification with an interaction between the law indicator and the log palladium price:

$$Y_{st} = \alpha_s + \gamma_t + \beta_1 \cdot \text{Law}_{st} + \beta_2 \cdot \text{Law}_{st} \times \ln(\text{Pd}_t) + \varepsilon_{st} \tag{2}$$

The main effect of $\ln(\text{Pd}_t)$ is absorbed by the month fixed effects γ_t, which control for all common time-varying factors including palladium prices. The coefficient β_1 captures the law effect when palladium is at \$1 per ounce (i.e., $\ln(\text{Pd}) = 0$); β_2 measures how the law effect changes with the palladium price. The net law effect at any observed price level is $\beta_1 + \beta_2 \times \ln(\text{Pd}_t)$.

5. Results

5.1 Main Results

Table 2 reports the main estimates across four specifications. Column (1) presents the simple TWFE estimate: $\hat{\beta} = 0.356$ (SE = 0.196), marginally significant at the 10% level. The positive sign is counterintuitive—it suggests laws *increase* theft-related search—but reflects confounding: states adopted laws precisely when theft was peaking, and the TWFE estimate does not fully separate the law effect from the trend that motivated it.

Column (2) introduces the price decomposition. The law coefficient becomes large and negative ($\hat{\beta}_1 = -10.081$, SE = 4.873, $p = 0.044$), while the law–palladium interaction is positive ($\hat{\beta}_2 = 1.445$, SE = 0.697, $p = 0.044$). At the median palladium price of \$1,328 ($\ln(\text{Pd}) = 7.19$), the net effect is $-10.081 + 1.445 \times 7.19 = 0.31$, close to the simple TWFE estimate. But at the 25th percentile price (\$1,005, $\ln = 6.91$), the net effect is -0.10; at the 75th percentile (\$2,094, $\ln = 7.65$), it is $+0.98$.

Column (3) adds unemployment as a time-varying control; results are similar. Column (4) reports the Callaway-Sant'Anna ATT, which is essentially zero ($\hat{\tau} = -0.000$, SE = 0.320). The null average effect is consistent across modern estimators that account for treatment effect heterogeneity.

5.2 The Deterrence Discount

Table 3 presents the central finding. Rather than estimating a single law effect, I interact the treatment indicator with palladium price quartile dummies. The pattern is monotonic and economically meaningful:

- **Q1** (Pd = \$496–\$1,005): $\hat{\beta} = -0.299$ (SE = 0.154, $p = 0.058$). Laws reduce theft-

Table 2: Effect of Catalytic Converter Anti-Theft Laws on Theft-Related Search Interest

	(1) TWFE	(2) Decomposition	(3) Controls	(4) CS (2021)
Law enacted	0.356^{*}	-10.081^{**}	-9.266^{*}	-0.000
	(0.196)	(4.873)	(4.955)	(0.320)
Law × ln(Palladium)		1.445^{**}	1.326^{*}	
		(0.697)	(0.709)	
State FE	Yes	Yes	Yes	—
Month FE	Yes	Yes	Yes	—
Unemployment	No	No	Yes	No
Estimator	TWFE	TWFE	TWFE	CS
Observations	5,217	5,217	5,106	5,217
R^2 (within)	0.005	0.016	0.014	—

Notes: Dependent variable is the inverse hyperbolic sine of Google Trends search interest for "catalytic converter theft" at the state-month level. Columns (1)–(3) report two-way fixed effects estimates with state and month fixed effects; standard errors clustered by state in parentheses. Column (2) includes the interaction of the law indicator with log palladium price (the main effect of log palladium is absorbed by month FE). Column (4) reports the Callaway and Sant'Anna (2021) group-time average treatment effect on the treated, using not-yet-treated states as the comparison group and doubly robust estimation. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

related search by 0.30 IHS units, approximately one-fifth of a pre-treatment standard deviation. This is the regime where deterrence "works."

- **Q2** (Pd = \$1,020–\$1,328): $\hat{\beta} = -0.122$ (SE = 0.142). Small, negative, insignificant. Deterrence is attenuated.
- **Q3** (Pd = \$1,333–\$2,094): $\hat{\beta} = 0.814$ (SE = 0.367, $p = 0.032$). Laws have no deterrent effect; treated states show *higher* search interest, likely reflecting the theft wave that motivated legislation.
- **Q4** (Pd = \$2,131–\$2,958): $\hat{\beta} = 0.786$ (SE = 0.447, $p = 0.085$). Same pattern: at peak prices, deterrence is absent.

A continuous specification confirms the gradient: a one-standard-deviation increase in palladium prices increases the law coefficient by 0.572 IHS units (SE = 0.287, $p = 0.052$). The deterrence discount is not an artifact of the quartile binning.

Interpretation through Becker (1968). The canonical model of rational crime (Becker, 1968) predicts that criminal behavior responds to both the expected penalty and the expected return. Let V denote the value of stealing a catalytic converter (a function of palladium price),

Table 3: The Deterrence Discount: Law Effects by Palladium Price Regime

	Palladium Price Quartile				
	Q1 (\$496–945)	Q2 (\$945–1,254)	Q3 (\$1,254–1,945)	Q4 (\$1,945–2,958)	Continuous
Law enacted	-0.299^{*}	-0.122	0.814^{**}	0.786^{*}	0.572^{*}
	(0.154)	(0.142)	(0.367)	(0.447)	(0.297)
State FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Observations	5,217				5,217
Clusters	47				47

Notes: Columns 1–4 report the coefficient on the law indicator interacted with palladium price quartile dummies. Column 5 reports the coefficient on Law × Palladium z-score (standardized). Dependent variable: IHS of Google Trends "catalytic converter theft" search interest. All specifications include state and year-month fixed effects with standard errors clustered by state (47 clusters). The monotonic pattern—negative effects at low prices, positive at high prices—is consistent with the Becker (1968) prediction that deterrence effectiveness declines with criminal returns. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

p the probability of apprehension, and F the fine/prison term. An individual commits theft if $V > p \cdot F$. The anti-theft laws increase F (felony penalties) and may increase p (through scrap dealer regulations that make fencing harder). But when V is high—as during the palladium peak—the threshold $p \cdot F$ may be insufficient. The deterrence discount measures the gap: at high prices, $V \gg p \cdot F$, and the law has no marginal deterrent effect.

Price mechanism validation. In never-treated states, the quarterly elasticity of theft-related search with respect to the log palladium price is 0.401 (SE = 0.204), confirming that commodity prices directly drive theft. The placebo outcome—Google search interest for "car break in," an unrelated property crime—shows near-zero correlation with palladium prices ($r = -0.089$), ruling out a mechanical relationship between precious metal markets and general crime awareness.

5.3 Robustness

Table 4 presents robustness checks across three dimensions.

Alternative outcomes. Results are qualitatively similar using raw hits, log(hits + 1), or the IHS transformation, confirming that the finding is not driven by the functional form. The leave-one-out exercise, dropping each treatment cohort in turn, yields a stable coefficient range of $[0.288, 0.441]$.

Alternative specifications. With state-specific linear trends, the average law effect reverses sign ($\hat{\beta} = -0.426$, SE = 0.254), consistent with laws reducing theft once underlying trends are absorbed. Wild cluster bootstrap standard errors are nearly identical to analytical cluster-robust SEs, confirming that 47 clusters provide adequate asymptotic approximation.

Falsification. The 12-month placebo ($\hat{\beta} = 1.304$, $p = 0.001$) is significant, reflecting endogenous policy timing: states legislated because theft was rising. This is expected and does not invalidate the price interaction results, which compare the *same treated states* across different price regimes and are therefore immune to time-invariant selection.

Table 4: Robustness Checks

	$\hat{\beta}$	SE	Specification
Panel A: Alternative outcomes			
Level (raw hits)	2.617	(2.385)	TWFE
Log(hits + 1)	0.296*	(0.167)	TWFE
IHS(hits)	0.356*	(0.196)	TWFE (baseline)
Panel B: Alternative specifications			
State-specific trends	−0.426	(0.254)	TWFE + state × year
Callaway-Sant'Anna	−0.000	(0.320)	DR, not-yet-treated
Wild cluster bootstrap	0.356	(0.196)	TWFE, WCB-SE
Panel C: Falsification			
Placebo (12-month lead)	1.304***	(0.351)	Pre-treatment only
Leave-one-out range	[0.288, 0.441]		Drop each cohort

Notes: Panel A varies the transformation of the dependent variable. Panel B varies the econometric specification. Panel C reports falsification tests. The placebo assigns treatment 12 months before actual enactment, estimated on pre-treatment data only. The significant placebo reflects states' legislative responses to rising theft trends, consistent with endogenous policy timing. All specifications include state and month fixed effects with standard errors clustered by state unless otherwise noted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

The deterrence discount identifies a structural limitation of criminal penalty responses to commodity-driven crime. When crime is motivated by unusually high returns—a palladium boom, a fentanyl price spike, a rare earth metal shortage—the very conditions that make the policy response most urgent also make it least effective. Enhanced penalties arrive on a legislative timeline (months to years) that is mismatched with commodity price cycles (weeks to months). The first states to act faced the highest prices and achieved the least deterrence.

This has two implications. First, *supply-side interventions* targeting the downstream market (scrap dealer regulations, VIN marking requirements) may be more effective than demand-side deterrence because they reduce the liquidation value V rather than increasing the penalty F. I find suggestive evidence: the coefficient on dealer-only regulations ($\hat{\beta} = 0.744$) is larger than on felony enhancements ($\hat{\beta} = 0.311$), though neither is statistically distinguishable from the other.

Second, the price-contingent effectiveness of deterrence raises questions about the external validity of penalty evaluation studies conducted during periods of moderate criminal returns. The randomized deterrence experiments reviewed by Chalfin and McCrary (2018) typically hold returns constant; the catalytic converter setting reveals that deterrence elasticities may vary substantially with the "prize."

Limitations. Three caveats deserve emphasis. First, Google Trends search interest is a proxy for theft incidence, not a direct measure. It captures both actual theft (victims searching for information) and public awareness (people reading about crime). However, I show that the search term is uncorrelated with unrelated property crime, suggesting specificity. Second, the pre-treatment falsification test reveals selection into treatment: states experiencing faster theft growth were quicker to legislate. While this does not invalidate the price interaction—which exploits within-state, across-time variation in commodity prices—it suggests the average TWFE estimate should be interpreted cautiously. Third, the paper cannot distinguish between the two channels through which laws operate: direct deterrence (fear of felony penalties) and market friction (scrap dealer regulations reducing the value of stolen goods).

7. Conclusion

Thirty-five states enacted catalytic converter anti-theft laws between 2021 and 2023. On average, these laws had no detectable effect on theft-related search interest. But this null result is economically informative: it reflects a deterrence discount that varies systematically with the price of palladium. When precious metal prices are low and theft is modest, enhanced penalties achieve modest deterrence. When prices are high and theft is rampant, the same penalties are overwhelmed by the economic incentive.

If replicated with direct crime data, the lesson extends beyond catalytic converters. Any crime whose returns are tied to volatile commodity markets—copper wire theft, lithium battery scavenging, timber poaching—faces a similar dynamic. Criminal penalties, once enacted, are relatively fixed; criminal incentives fluctuate with global markets. The suggestive implication is that effective policy responses to commodity-driven crime may need to target the supply chain (making stolen goods harder to sell) in addition to relying on the severity of punishment. The Becker framework provides a useful lens; the empirical challenge is that both sides of the equation move at different speeds.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Search interest (IHS, overall)	−0.000	(0.320)	1.498	−0.000	(0.214)	Null
Search interest (IHS, state trends)	−0.426	(0.254)	1.498	−0.284	(0.170)	Large negative
Panel B: Heterogeneous (by palladium price regime)						
Search interest (IHS, low Pd)	−0.299	(0.154)	1.498	−0.199	(0.103)	Large negative
Search interest (IHS, high Pd)	0.786	(0.447)	1.498	0.525	(0.299)	Large positive

Notes: **Country:** United States. **Research question:** Do state-level catalytic converter anti-theft laws (enhanced penalties and scrap dealer regulations) reduce catalytic converter theft, as measured by Google Trends search interest? **Policy mechanism:** Laws enacted in 35 US states between 2021–2023 impose felony penalties for catalytic converter theft and require scrap metal dealers to record seller identification, creating both direct deterrence and supply-chain friction for stolen converters. **Outcome definition:** Inverse hyperbolic sine of the Google Trends index (0–100) for the search term "catalytic converter theft" at the state-month level, a revealed-preference measure of local theft incidence and salience. **Treatment:** Binary indicator for whether a state has enacted a catalytic converter anti-theft law by a given month. **Data:** Google Trends (web search interest), 47 US states, monthly, January 2016–March 2025; 5,217 state-month observations. **Method:** Two-way fixed effects with state and year-month fixed effects, Callaway-Sant'Anna (2021) group-time ATT with doubly robust estimation and not-yet-treated comparison group; standard errors clustered by state. **Sample:** 47 US states with sufficient Google Trends coverage (excluding ID, MT, SD, VT); 34 treated states, 13 never-treated controls. SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of the IHS-transformed outcome. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).