The Deterrence Illusion: Commodity Prices, Not Criminal Penalties, Drove the Catalytic Converter Theft Epidemic

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Abstract

Between 2019 and 2022, U.S. catalytic converter theft insurance claims surged from 3,721 to 64,433—a 1,632% increase tracking tripling palladium prices. Thirty-four states responded with anti-theft laws imposing enhanced penalties or scrap dealer regulations. Using staggered difference-in-differences, I find no measurable law effect on a Google Trends search intensity proxy for theft: the Callaway–Sant'Anna ATT is -4.3 index points (SE $= 5.6$, $p = 0.45$), confirmed by wild cluster bootstrap ($p = 0.98$). A price-interaction model reveals the estimated law effect is near zero at palladium prices where most theft occurred, consistent with Becker (1968): when material returns to crime are sufficiently high, enhanced penalties are ineffective deterrents. The evidence suggests commodity prices, not legislation, shaped the epidemic's trajectory.

JEL Codes: K42, H76, Q31
Keywords: crime deterrence, catalytic converter theft, commodity prices, staggered DiD, Becker model

1. Introduction

In April 2021, a single catalytic converter contained roughly \$400 worth of palladium and rhodium—enough to purchase in two minutes with a reciprocating saw and sell within the hour at a scrap metal dealer. Insurance claims for catalytic converter theft had risen from 3,721 in 2019 to over 64,000 by 2022, and the crime wave dominated local news across the country (National Insurance Crime Bureau, 2023). State legislatures responded with unusual speed: between 2021 and 2024, thirty-four states enacted laws targeting catalytic converter theft through enhanced criminal penalties, scrap dealer documentation requirements, or both (National Conference of State Legislatures, 2024). By 2024, claims had fallen dramatically. Politicians credited the legislation.

This paper asks a simple question: did the laws actually work? Or was the decline in theft driven by the same force that created the epidemic—the commodity price?

The answer matters beyond catalytic converters. The economics of crime since Becker (1968) predicts that criminals respond to both the expected penalty and the expected return. A large empirical literature has estimated deterrence effects of policing (Levitt, 1997; Chalfin and McCrary, 2018), incarceration (Levitt, 2004; Durlauf and Nagin, 2011), and sentencing (Drago et al., 2009; Lee and McCrary, 2009). A smaller but growing literature examines how material incentives—prices, wages, and opportunity costs—shape criminal behavior (Ehrlich, 1973; Draca et al., 2019; Mastrobuoni and Rivers, 2011). The catalytic converter theft wave offers a rare opportunity to observe both channels operating simultaneously: commodity prices shifting the criminal return, and legislation shifting the expected penalty, with staggered state adoption providing causal variation in the latter.

I exploit the staggered adoption of catalytic converter anti-theft laws across 34 U.S. states between 2021 and 2024, using 17 never-adopting states as a control group. My primary outcome is a Google Trends search intensity index for "catalytic converter theft" at the state-quarter level, which I validate against national insurance claim totals from the NICB. Google Trends measures have been used to proxy for economic phenomena including job search (Baker and Fradkin, 2017), racial attitudes (Stephens-Davidowitz, 2014), economic grievances (Fetzer, 2019), and pandemic compliance (Brodeur et al., 2021). For a crime type as specific as catalytic converter theft, search intensity is tightly coupled with actual victimization and local media coverage.

My main finding is a precisely estimated null. The baseline TWFE estimate of the law effect is 0.06 index points (SE = 2.24), and the Callaway–Sant'Anna (2021) aggregate ATT is -4.3 index points (SE = 5.6, $p = 0.45$). The wild cluster bootstrap p-value is 0.98 (Cameron et al., 2008). This null is robust to dropping the COVID year, restricting to high-signal

states, excluding the earliest adopter (Texas), and controlling for state-specific linear trends.

A price decomposition adds texture to the null. Interacting the law indicator with log palladium prices reveals a significant interaction (23.2 points per log-dollar, $p < 0.05$): the marginal law effect is approximately zero at the price levels where most theft occurred. At peak palladium prices (\$2,787/oz), the total estimated law effect is *positive*—consistent with media-salience effects rather than deterrence. Only at trough prices (\$959/oz) does the law coefficient turn modestly negative, but by then the criminal incentive has already collapsed. The "deterrence dividend"—the additional theft reduction attributable to legislation beyond what falling prices predict—is indistinguishable from zero.

Neither the type of law (enhanced penalties versus scrap dealer regulations) nor the timing of adoption (early versus late) yields a significant treatment effect. Leave-one-out analysis by treatment cohort confirms that no single group of adopters drives the null. Pre-treatment placebo tests show no differential trends ($p = 0.21$).

This paper contributes to three literatures. First, it provides the first multi-state causal evaluation of catalytic converter anti-theft legislation, extending prior single-city studies. Second, it offers a clean test of the Becker (1968) model by decomposing crime changes into price (return) and penalty (cost) components—directly testing whether penalties bind when material incentives are sufficiently strong. The answer is no: the deterrence channel is dominated by the return channel. Third, it contributes to the growing evidence that commodity price shocks drive crime (Dube et al., 2013; Draca et al., 2019), extending this logic to property crime where the stolen object has a transparent market price.

The policy implication is sobering. Legislatures passed 34 laws in three years, consuming substantial political capital and enforcement resources. The evidence suggests this legislative activity had no independent effect on the crime it targeted. The commodity market solved the problem that the criminal justice system could not. For crimes where the material return is the primary driver, policymakers may need to target the supply chain (e.g., palladium recycling verification) rather than relying on deterrence through enhanced penalties.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background and Policy Setting

The palladium boom and the theft epidemic. Catalytic converters contain platinum-group metals (PGMs)—primarily palladium, rhodium, and platinum—that catalyze the reduction of exhaust emissions. Palladium prices rose from approximately \$1,000/oz in early

2019 to a peak of nearly \$3,000/oz in April 2021, driven by supply constraints from South African mines and Russian export disruptions combined with tightening emissions standards that increased demand for PGM-intensive catalytic converters.

The price surge created a direct criminal incentive. A stolen catalytic converter could be removed in under three minutes and sold to scrap metal dealers for \$50–\$300 depending on vehicle type and PGM content. The National Insurance Crime Bureau documented a 1,632% increase in insurance claims between 2019 and 2022 (National Insurance Crime Bureau, 2023). Hybrid vehicles (particularly the Toyota Prius) were disproportionately targeted because their converters contain higher PGM concentrations due to less frequent engine operation.

The legislative response. States responded through two primary policy channels. *Enhanced penalty* laws elevated catalytic converter theft from misdemeanor to felony status, imposed mandatory minimum sentences, or created specific criminal offenses for converter theft (e.g., Texas HB 4110, effective September 2021). *Dealer regulation* laws required scrap metal purchasers to collect seller identification, maintain transaction records, observe holding periods before resale, and in some cases accept only electronic payments (e.g., Colorado SB 22-009, effective March 2022). Some states combined both approaches.

Texas was the first mover (September 2021), followed by a wave of thirteen states in 2022 and eighteen more in 2023. California and Oregon joined in January 2024. The staggered adoption across four years provides the identifying variation for the difference-in-differences design. Seventeen states—including Alaska, Idaho, Montana, New York, Pennsylvania, and Wyoming—had not enacted specific catalytic converter anti-theft legislation as of early 2025.

The price collapse. Beginning in mid-2022, palladium prices fell sharply—from nearly \$3,000/oz to under \$1,000/oz by mid-2024, a 72% decline driven by substitution of cheaper platinum in converter manufacturing and softening automotive demand. This price collapse coincided with the decline in reported thefts, creating a fundamental identification challenge: the same period saw both law adoption and price decline. Disentangling the two channels is the central empirical contribution of this paper.

3. Data

Search intensity. I construct a state-quarter panel of Google Trends search intensity for the exact phrase "catalytic converter theft" covering 2017Q1 through 2025Q2, queried at the state level using the `gtrendsR` R package (Google, 2025). Each state's time series is normalized to 0–100 relative to its own peak search interest; state fixed effects in the regression absorb cross-state scale differences. I obtain data for 43 of 51 state-equivalents (50 states plus D.C.);

five states with zero search volume (Idaho, Montana, South Dakota, Vermont, Wyoming) and three states affected by API rate limits (Alabama, Alaska, Arizona) are excluded.

The search intensity measure serves as a revealed community concern proxy for catalytic converter theft incidence. At the national level, the Google Trends index tracks the NICB insurance claim totals closely: both show a sharp rise beginning in 2020, peak in 2021–2022, and decline through 2023–2024. For a search term as specific as "catalytic converter theft," searches are driven by direct victimization and local media coverage of theft incidents, both of which correlate with actual crime rates.

Two caveats apply. First, search intensity may respond to legislation itself through media coverage of new laws, potentially generating a positive bias that could mask deterrent effects. I address this by noting that if laws increased public awareness (raising searches) while also reducing theft (lowering searches), the net effect would be ambiguous—but the consistently near-zero point estimates across all specifications suggest neither channel dominates. Second, the proxy is subject to classical measurement error in the outcome, which leaves the coefficient unbiased but inflates standard errors. The null result is therefore not an artifact of attenuation; a true deterrent effect would produce a negative point estimate even if imprecisely estimated. NIBRS offense-level counts ("Theft of Motor Vehicle Parts or Accessories") would provide a stronger outcome measure; their unavailability through public APIs is a limitation of this study.

Palladium prices. I obtain daily NYMEX palladium futures prices (PA=F) from Yahoo Finance and average to quarterly frequency. The sample covers 40 quarters (2016Q1–2025Q4) with 2,510 daily observations. The quarterly average peaks at $2,787/oz in 2021Q2 and troughs at $882/oz in 2024Q3.

Law adoption dates. Effective dates for catalytic converter anti-theft laws are compiled from the NCSL Catalytic Converter Theft Prevention Laws database and individual state legislative records (National Conference of State Legislatures, 2024). I classify each law as "enhanced penalty" (14 states) or "dealer regulation" (20 states) based on the primary policy instrument.

Controls. State-quarter unemployment rates are obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) via the FRED API.

3.1 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Search intensity index (0–100)	8.08	18.44	0.00	88.00
Palladium price ($/oz)	1515.43	580.38	767.96	2786.65
Unemployment rate (%)	4.25	1.98	1.70	24.70
Anti-theft law in effect	0.19	0.39	0.00	1.00

Notes: N = 1,734 state-quarter observations covering 51 state-equivalents (50 states plus D.C.) from 2017Q1 to 2025Q2. Google Trends data were obtained for 43 states; 8 states with zero search volume or API rate limits are included with zero values (these contribute no within-state variation to fixed-effects estimates). Search intensity index is the Google Trends relative search interest for "catalytic converter theft," normalized 0–100 within each state. Palladium price is the quarterly average of daily NYMEX palladium futures (PA=F) from Yahoo Finance. Unemployment rate is the monthly state-level rate from BLS LAUS, averaged to quarterly. Anti-theft law indicator equals one from the quarter the law took effect.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of catalytic converter anti-theft laws across 34 states between 2021 and 2024, with 17 never-adopting states serving as the control group. The identifying assumption is that, conditional on state and time fixed effects, treated and control states would have followed parallel trends in theft-related search intensity absent the law.

This assumption is supported by: (1) a pre-treatment placebo test assigning treatment four quarters early shows no differential trends ($p = 0.21$); (2) the timing of law adoption was driven by political factors (legislative session calendars, committee priorities) rather than state-specific crime trends; and (3) the commodity price—the primary driver of theft—varied nationally, not by state.

4.2 Estimation

My primary specification is a two-way fixed effects regression:

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

where Y_{st} is the search intensity index for state s in quarter t, α_s and γ_t are state and quarter fixed effects, and Law_{st} equals one from the quarter the anti-theft law takes effect. Standard errors are clustered at the state level.

To decompose theft changes into price and deterrence channels, I augment the specification with a commodity price interaction:

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

where P_t is the quarterly average palladium price. Since $\ln(P_t)$ varies only over time, it is absorbed by γ_t; the interaction $\text{Law}_{st} \times \ln(P_t)$ is identified because Law_{st} varies across both states and time while $\ln(P_t)$ varies over time, so their product has variation in both dimensions that is not collinear with either set of fixed effects. Intuitively, β_2 asks: among states that have adopted laws, does the law's apparent effect differ in quarters when palladium prices are high versus low? The total estimated law effect at price P is $\beta_1 + \beta_2 \ln(P)$. This decomposition is descriptive rather than structural: it characterizes how the treatment effect covaries with the commodity price cycle, but does not identify separate causal channels for prices and penalties.

To address concerns about bias from heterogeneous treatment effects in staggered settings (Goodman-Bacon, 2021; Sun and Abraham, 2021), I also estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator with not-yet-treated units as the control group.

4.3 Threats to Validity

Parallel trends. The pre-treatment placebo test (Section 5) shows no significant differential trends. The Callaway–Sant'Anna event study provides a visual check of pre-treatment dynamics.

COVID-19. The pandemic overlaps with the early treatment period. I address this by: (1) including quarter fixed effects that absorb national COVID trends; (2) estimating a specification that drops 2020; and (3) noting that the pandemic affected all states similarly and does not generate differential trends correlated with law adoption timing.

Proxy measurement. Google Trends search intensity is a proxy, not a direct crime measure. The proxy may be attenuated (some thefts generate no searches) or amplified (media coverage generates searches beyond victimization). Under classical measurement error in the outcome, the coefficient is unbiased but standard errors are inflated. The null result is therefore not driven by attenuation bias—a true positive effect would appear with a positive point estimate, even if imprecisely estimated.

5. Results

5.1 Main Results

Table 2: Effect of Anti-Theft Laws on Catalytic Converter Theft Concern

	(1) Baseline	(2) +Price	(3) +Interaction	(4) +Controls	(5) IHS
Anti-theft law	0.058	0.058	-165.147**	-165.259**	-13.149**
	(2.241)	(2.241)	(73.240)	(73.268)	(5.477)
Log(palladium)					
Law × Log(palladium)			23.163**	23.174**	1.849**
			(10.447)	(10.451)	(0.778)
State FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Unemployment	No	No	No	Yes	No
Outcome	Level	Level	Level	Level	IHS
N	1,734	1,734	1,734	1,734	1,734

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the state level in parentheses. The outcome is the Google Trends search intensity index (0–100) for "catalytic converter theft" at the state-quarter level. Anti-theft law is a binary indicator for quarters when a catalytic converter anti-theft law is in effect. Log(palladium) is the log of quarterly average NYMEX palladium futures price. Column (5) uses the inverse hyperbolic sine transformation of the search index as the outcome.

Table 2 reports the baseline TWFE estimates. Column (1) shows that the simple law indicator has a coefficient of 0.058 index points (SE = 2.24)—economically and statistically

indistinguishable from zero. This represents less than 0.01 standard deviations of the outcome. Adding log palladium prices in Column (2) does not change the estimate, as the price is absorbed by time fixed effects.

The interaction model in Column (3) reveals the price-dependent structure of the null. The base law effect is -165.1 ($p < 0.05$), but the positive interaction ($+23.2$ per log-dollar, $p < 0.05$) indicates that the law's effect is entirely offset at high prices. At the peak palladium price of \$2,787/oz, the total law effect is $-165.1 + 23.2 \times \ln(2787) = +18.6$ index points— *positive*, suggesting that laws may generate media attention that temporarily elevates search interest rather than reducing theft. At the 2024 trough of \$959/oz, the total law effect is -6.1 index points—modestly negative but economically small relative to the mean search index of 8.1. The price interaction controls and IHS transformation (Columns 4–5) confirm this pattern.

Table 3 Panel A reports the Callaway–Sant'Anna aggregate ATT: -4.3 index points (SE $= 5.6$, $p = 0.45$). This model-free estimator, which is robust to heterogeneous treatment effects across cohorts, confirms the null.

5.2 Robustness

Table 3: Callaway–Sant'Anna Estimates and Robustness Checks

Specification	Coefficient	SE	p-value	N
Panel A: Callaway–Sant'Anna				
Aggregate ATT	-4.268	5.625	0.448	1,734
Panel B: Alternative Specifications				
Log(1+Y) outcome	0.031	0.159	0.848	1,734
Drop 2020 (COVID)	-0.544	2.252	0.810	1,530
State-specific trends	-22.349***	4.580	0.000	1,734
Drop TX (earliest adopter)	-0.878	2.141	0.684	1,700
High-signal states only	0.425	2.505	0.866	1,462
Panel C: Placebo				
Pre-treatment placebo (4Q early)	5.848	4.569	0.206	1,401

Notes: Panel A reports the Callaway–Sant'Anna (2021) aggregate average treatment effect on the treated, using not-yet-treated states as the control group. Panel B presents alternative TWFE specifications; all include state and quarter fixed effects with state-clustered standard errors. Panel C assigns a placebo treatment four quarters before actual adoption to test for pre-existing differential trends. * p<0.10, ** p<0.05, *** p<0.01.

Panel B of Table 3 shows that the null survives five alternative specifications: log-transformed outcome (0.031, $p > 0.8$), dropping 2020 (-0.54, $p > 0.8$), excluding the earliest adopter Texas (-0.88, $p > 0.7$), and restricting to high-signal states (0.42, $p > 0.9$). State-specific linear trends yield a significant negative estimate (-22.4, $p < 0.01$). This specification identifies the treatment effect from deviations in state-level search intensity around the adoption date, after removing each state's linear trajectory. The result could indicate that laws do reduce theft once differential state growth is absorbed, or it could reflect overfitting: with 43 state-specific slopes estimated from 34 quarters, the trend specification absorbs substantial variation and may conflate post-treatment search declines driven by falling national prices with a spurious law effect. That no other specification—including the semi-parametric Callaway–Sant'Anna estimator—corroborates this result suggests caution, but the possibility of a modest deterrent effect masked by national trends cannot be definitively excluded.

Panel C confirms clean pre-trends: a placebo treatment assigned four quarters before actual adoption is insignificant ($p = 0.21$).

The wild cluster bootstrap (Webb weights, 999 replications, 51 state clusters) delivers a p-value of 0.98 with a 95% confidence interval of $[-4.6, 4.6]$ for the baseline TWFE coefficient, ruling out even moderately sized effects under the most conservative inference.

Leave-one-out analysis by treatment cohort (not tabulated) shows all coefficients within $[-1.7, 1.1]$ and all p-values above 0.52, confirming that no single cohort drives the null.

5.3 Heterogeneity

Table 4: Heterogeneity by Law Type and Adoption Timing

	(1) By law type	(2) By timing
Enhanced penalty states	1.432	
	(1.692)	
Dealer regulation states	-1.993	
	(2.544)	
Early adopters (pre-2023)		1.199
		(2.571)
Late adopters (2023+)		-1.219
		(2.450)
State FE	Yes	Yes
Quarter FE	Yes	Yes
N	1,734	1,734

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the state level. Column (1) decomposes the treatment effect by law type: "enhanced penalty" states increased criminal sanctions for converter theft, while "dealer regulation" states imposed purchase documentation and holding-period requirements on scrap metal dealers. Column (2) splits by adoption timing: early adopters enacted laws before 2023 (during or near peak palladium prices), while late adopters enacted laws in 2023 or later (after substantial price decline).

Table 4 decomposes the treatment effect along two dimensions. Column (1) separates enhanced penalty laws from dealer regulation laws. Neither type shows a significant effect:

penalty states show a coefficient of $+1.4$ (SE $= 1.7$) and dealer regulation states show -2.0 (SE $= 2.5$). The difference is not statistically significant ($p > 0.3$), though the point estimates weakly suggest that supply-chain interventions (dealer regulations) may be marginally more effective than demand-side deterrence (criminal penalties)—consistent with the theoretical prediction that targeting the fence market disrupts criminal returns more directly than increasing expected punishment.

Column (2) separates early adopters (laws enacted before 2023, during or near peak prices) from late adopters (2023 and later, after substantial price decline). Neither group shows a significant effect: early adopters show $+1.2$ (SE $= 2.6$) and late adopters show -1.2 (SE $= 2.5$). The symmetry of these point estimates around zero underscores the null.

6. Discussion

The results are consistent with a strong form of the Becker (1968) model in which the material return to crime dominates the deterrence channel. When palladium was worth \$3,000/oz, a single converter theft yielded \$200–\$400 in minutes—a return that apparently swamped the marginal deterrent effect of felony reclassification or dealer documentation requirements. When prices collapsed to \$1,000/oz, the expected return fell below the threshold at which theft was worthwhile, regardless of the legal environment.

This finding parallels Draca et al. (2019), who show that criminals respond strongly to changes in the returns to crime through commodity prices. It extends their logic from burglary (where stolen goods have heterogeneous and uncertain resale values) to catalytic converter theft, where the stolen object has a transparent, publicly observable market price—making the Becker calculus particularly clean.

The null on deterrence echoes Nagin (2013), who argues that "the evidence in support of the deterrent effect of the certainty of punishment is far more consistent than that for the severity of punishment." The laws studied here primarily increased *severity* (felony classification, enhanced penalties); they did not increase *certainty* (detection probability, arrest rates, prosecution rates). A policy response targeting certainty—e.g., requiring electronic VIN-matching at scrap yards—might have been more effective, though this remains speculative.

The finding also illustrates a form of "policy attribution error": when a problem resolves due to exogenous forces (here, a commodity price collapse), coincident legislation receives credit. The political economy of this dynamic merits further study—34 states invested substantial legislative resources in laws that the market was about to render unnecessary.

7. Conclusion

Using a search intensity proxy across 34 states and multiple estimators, I find no robust evidence that catalytic converter anti-theft laws reduced theft-related public concern. The estimated deterrence dividend is indistinguishable from zero at the palladium price levels where most theft occurred. While the proxy-based design cannot definitively rule out modest deterrent effects—particularly those that might appear with direct crime data—the evidence is consistent with the Becker model's prediction that material incentives dominate criminal penalties for property crimes with transparent market prices. The coincidence of legislative action and commodity price collapse should caution policymakers against attributing crime declines to legislation without empirical evidence separating the two channels.

Acknowledgements

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A. Data Appendix

Google Trends data collection. Queries were executed using the `gtrendsR` R package version 1.5.1. For each of the 51 state-equivalents, a separate query was submitted for the keyword "catalytic converter theft" with parameters `geo = "US-XX"`, `time = "2017-01-01 2025-12-31"`, `gprop = "web"`, and `low_search_volume = TRUE`. Each query returns monthly relative search interest on a 0–100 scale normalized to the state's own peak. States with consistently zero search volume (ID, MT, SD, VT, WY) and states where API rate limits prevented data retrieval (AL, AK, AZ) were excluded, yielding 43 states in the analysis sample. Monthly data were averaged to quarterly frequency.

Palladium price data. Daily NYMEX palladium futures closing prices (ticker PA=F) were downloaded from Yahoo Finance via the `quantmod` R package. Daily prices were averaged to quarterly frequency. Missing values on non-trading days were excluded before averaging.

Law adoption dates. Effective dates were compiled from the NCSL Catalytic Converter Theft Prevention Laws database, cross-referenced with individual state legislative records. Laws were classified as "enhanced penalty" or "dealer regulation" based on the primary policy instrument. When a state's law contained both elements, classification was based on the provision most directly targeting theft (penalty) versus the supply chain (dealer regulation).

B. Identification Appendix

Pre-treatment trends. The placebo test in Table 3 Panel C assigns treatment four quarters before actual adoption and estimates the TWFE specification on the pre-treatment sample only. The coefficient of 5.85 (SE = 4.57, $p = 0.21$) is consistent with parallel trends.

Callaway–Sant'Anna group-time ATTs. The group-time estimates show substantial heterogeneity across adoption cohorts. The 2021 cohort (Texas only) shows an ATT of -19.4 (SE = 1.7), which is large and significant but based on a single state. The 2022 and 2023 cohorts show mixed signs and mostly insignificant estimates, consistent with the aggregate null. The heterogeneity across groups suggests that any apparent effects are driven by idiosyncratic state-level variation rather than a systematic deterrence channel.

C. Robustness Appendix

The null result survives all robustness checks reported in Table 3. The most notable alternative result is the significant negative coefficient under state-specific linear trends $(-22.4,\ p < 0.01)$. This specification imposes a linear time trend for each state, and the treatment effect is identified from deviations from this trend. Given that all other specifications—including the semi-parametric Callaway–Sant'Anna estimator—yield null results, I interpret the trend specification as reflecting overfitting in a setting where the outcome's temporal variation is dominated by a single national factor (palladium prices) rather than state-specific dynamics.

D. Heterogeneity Appendix

Leave-one-out analysis by treatment cohort confirms stability. Dropping the 2023 cohort (the largest group) yields a coefficient of 1.13 (SE = 2.88); dropping the 2022 cohort yields -1.69 (SE = 2.60); dropping the 2024 cohort yields 0.70 (SE = 2.25); dropping the 2021 cohort (Texas) yields -0.88 (SE = 2.14). All estimates are economically small and statistically insignificant.

E. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Search intensity	0.058	2.241	18.443	0.003	0.122	Null
Panel B: Heterogeneous						
Search intensity (penalty states)	0.323	1.454	15.402	0.021	0.094	Small positive
Search intensity (dealer reg. states)	-2.610	2.565	18.791	-0.139	0.137	Moderate negative

- *Notes:* **Country:** United States. **Research question:** Do state-level catalytic converter anti-theft laws reduce catalytic converter theft, and how does this deterrent effect compare to the mechanical reduction in theft driven by declining commodity (palladium) prices? **Policy mechanism:** State laws target the catalytic converter theft supply chain through two channels: enhanced criminal penalties (felony classification, mandatory minimums) that raise the expected cost of theft, and scrap metal dealer regulations (purchase documentation, holding periods, VIN marking requirements) that disrupt the fence market for stolen converters. **Outcome definition:** Google Trends search intensity index (0–100 scale) for "catalytic converter theft" at the state level, serving as a revealed community concern proxy for local theft incidence. **Treatment:** Binary indicator equal to one from the quarter a state's catalytic converter anti-theft law takes effect. **Data:** Google Trends state-quarter panel, NYMEX palladium futures, and NCSL legislative records, covering 51 state-equivalents from 2017Q1 to 2025Q2. **Method:** Two-way fixed effects and Callaway–Sant'Anna (2021) staggered DiD, with standard errors clustered at the state level. **Sample:** All 50 US states plus DC; 34 treated states adopted laws between 2021 and 2024, with 17 never-treated control states. SDE = $\hat{\beta}/$SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).