The Escape Valve That Didn't Leak: Civil Forfeiture Reform and Federal Equitable Sharing

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Abstract

Thirty-eight U.S. states reformed civil asset forfeiture between 2014 and 2021, but critics warned that law enforcement agencies would circumvent these restrictions by routing seizures through the federal equitable sharing program. Using 63,427 agency-year observations from the DOJ's Equitable Sharing Annual Certification data and a staggered difference-in-differences design, I find no evidence of such circumvention. The Callaway–Sant'Anna ATT is -0.34 (SE $= 1.60$) and the TWFE estimate is 0.06 (SE $= 0.15$), with clean pre-trends and a 95% confidence interval that rules out increases larger than 0.35 standard deviations. The null holds across reform types, for states with and without anti-circumvention laws, and in every robustness specification. The federal escape valve, widely feared as the fatal flaw of state-level reform, does not appear to leak.

JEL Codes: K42, H76, H77
Keywords: civil asset forfeiture, equitable sharing, regulatory leakage, federalism, law enforcement

1. Introduction

In 2013, a Philadelphia couple lost their home after their son sold $40 of marijuana on the porch. The city had seized the property through civil asset forfeiture—a legal process that allows governments to confiscate property suspected of involvement in crime without charging, let alone convicting, the owner (Stillman, 2013). The resulting backlash fueled a decade of state-level reform: between 2014 and 2021, 38 states restricted or abolished civil forfeiture, raising the burden of proof, requiring criminal conviction, or banning the practice outright (Institute for Justice, 2023).

But reform advocates immediately identified a potential fatal flaw. Under the federal equitable sharing program, state and local agencies can partner with federal authorities to seize property under federal law—which imposes no conviction requirement—and receive up to 80% of the proceeds (U.S. Department of Justice, 2018). This creates a jurisdictional arbitrage opportunity: agencies in reformed states could route seizures through federal channels to evade state restrictions. Holcomb et al. (2011) found cross-sectional evidence consistent with this story, estimating that for each 25% reduction in a state's forfeiture revenue share, annual equitable sharing payments increased by $6,000 per agency. The implication was stark: state reform is futile without federal coordination.

This paper tests that claim causally. I exploit the staggered adoption of civil forfeiture reforms across 38 states between 2014 and 2021, using the universe of agency-level Equitable Sharing Annual Certification (ESAC) filings obtained through a Freedom of Information Act request to the Department of Justice. The dataset contains 63,427 agency-year observations covering 7,566 law enforcement agencies across all 50 states and the District of Columbia from fiscal year 2016 to 2024. I compare equitable sharing revenue in reformed states to 13 never-reformed states using both two-way fixed effects (TWFE) and the heterogeneity-robust estimator of Callaway and Sant'Anna (2021).

The headline finding is a well-powered null. The TWFE estimate of the effect of state forfeiture reform on the inverse hyperbolic sine of equitable sharing revenue is 0.057 (SE = 0.145, $p = 0.70$), and the Callaway–Sant'Anna ATT is -0.343 (SE = 1.596). Event-study estimates show no differential pre-trends and no post-treatment divergence. The 95% confidence interval on the TWFE specification rules out effects larger than 0.34 in asinh terms, equivalent to roughly a 35% increase in revenue—a meaningful exclusion. On the extensive margin, there is no evidence that reform increases the share of agencies receiving equitable sharing payments ($\hat{\beta} = 0.003$, SE = 0.012). The null holds for strong reforms (abolition and conviction requirements) and weak reforms (raised burden of proof and reporting mandates), and survives every robustness check: state-level aggregation, balanced panels, alternative

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functional forms, dropping early cohorts, and leave-one-state-out analysis.

These results matter for three reasons. First, they challenge the prevailing narrative—advanced in advocacy reports (Institute for Justice, 2023), legal scholarship (Carpenter II, 2023; Kelly, 2016), and the prior empirical literature (Holcomb et al., 2011)—that the equitable sharing "loophole" renders state reform ineffective. The difference between my findings and those of Holcomb et al. (2011) likely reflects the distinction between cross-sectional correlation and causal identification: states with weaker forfeiture laws may have higher equitable sharing for reasons unrelated to circumvention (e.g., more federal law enforcement presence or different drug enforcement strategies).

Second, the paper contributes to the literature on regulatory leakage in federal systems. When sub-national governments tighten regulation, affected actors may shift activity to a less restrictive jurisdiction—a pattern documented in banking (Makowsky and Stratmann, 2019), environmental regulation, and tax policy. The null result here suggests that jurisdictional arbitrage through equitable sharing is not the dominant response to forfeiture reform, possibly because the federal partnership process imposes nontrivial transaction costs, because agency culture and local political incentives mediate compliance, or because reforms coincided with federal policy changes that restricted equitable sharing (notably Attorney General Holder's 2015 order limiting adoptive seizures).

Third, the findings inform an active policy debate. Proposals for a federal Anti-Circumvention Forfeiture Act have been motivated by the assumption that state reform leaks into federal channels. If the escape valve does not, in fact, leak, then state-level reform may be more effective than its critics allege—and the case for federal legislation shifts from plugging a hole to standardizing protections.

The paper relates to several literatures. On civil forfeiture and police incentives, Worrall (2001) documented agencies' fiscal dependence on forfeiture proceeds, Williams et al. (2010) traced revenue allocation patterns, and Worrall and Kovandzic (2008) examined whether forfeiture deters crime. On the political economy of policing for revenue, Makowsky and Stratmann (2009) showed that fiscal stress increases traffic citations, Sances and You (2017) linked fines and fees to local government fiscal conditions, and Garrett and Monahan (2021) surveyed officers on revenue-generation incentives. On forfeiture reform specifically, McDonald et al. (2024) used a difference-in-differences design to show that New Mexico's 2015 abolition did not increase crime, and Note (2017) provided a constitutional analysis of forfeiture as punishment. My contribution is to bring causal identification to the circumvention question using the full universe of federal equitable sharing data at the agency level.

On methodology, the paper builds on the staggered DiD literature: Goodman-Bacon (2021) showed that TWFE can produce biased estimates under heterogeneous treatment effects,

Callaway and Sant'Anna (2021) and Sun and Abraham (2021) proposed robust alternatives, and de Chaisemartin and D'Haultfœuille (2020) formalized conditions under which TWFE breaks down. I implement the Callaway and Sant'Anna (2021) estimator throughout and report TWFE as a benchmark, noting that the two estimators agree closely here—consistent with the absence of both treatment effects and treatment-effect heterogeneity.

2. Institutional Background

Civil asset forfeiture and the reform wave. Civil asset forfeiture allows law enforcement to seize property suspected of involvement in or derived from criminal activity through an *in rem* proceeding against the property itself, not the owner. Because the action is civil rather than criminal, the standard of proof is typically preponderance of the evidence, and no criminal charge or conviction is required (Note, 2017). Law enforcement agencies often retain a substantial share of forfeiture proceeds, creating a direct fiscal incentive to seize (Worrall, 2001; Garrett and Monahan, 2021).

Beginning in 2014, states enacted reforms along a spectrum of restrictiveness. At one extreme, New Mexico (2015) and Nebraska (2016) abolished civil forfeiture entirely, requiring criminal conviction for any property seizure. Eleven additional states imposed conviction requirements for most forfeitures. Seventeen states raised the burden of proof (e.g., from preponderance to clear and convincing evidence) or strengthened procedural protections. Eight states added reporting and transparency requirements only. Four states—New Mexico, Nebraska, Arizona, and Colorado—enacted explicit anti-circumvention provisions barring agencies from routing seizures through federal programs to evade state law.

The federal equitable sharing program. Under the federal equitable sharing program, state and local agencies can participate in joint investigations with federal agencies (e.g., DEA, FBI) or refer cases for "adoptive" federal seizure. Seized assets are processed under federal forfeiture law, which requires only probable cause, and up to 80% of proceeds are returned to the participating state or local agency (U.S. Department of Justice, 2018). Crucially, equitable sharing proceeds are governed by federal law regardless of state restrictions, creating the potential for circumvention.

The program is not costless to use. Agencies must file annual Equitable Sharing Annual Certifications (ESAC) with the DOJ, undergo compliance reviews, and maintain separate accounting. Attorney General Holder restricted adoptive seizures in 2015, though joint investigations were exempted. Attorney General Sessions partially reversed this restriction in 2017. These federal policy changes affected all states simultaneously and are absorbed by

fiscal year fixed effects in my design.

3. Data

The primary data source is the DOJ Equitable Sharing Annual Certification (ESAC) database, obtained via a Freedom of Information Act request. The database contains all annual certifications filed by law enforcement agencies participating in the equitable sharing program. Each record identifies an agency (via NCIC code), its state, the fiscal year, and detailed income by source: equitable sharing funds received from the Asset Forfeiture Fund and Treasury Forfeiture Fund, interest income, sale proceeds, and reimbursements.

I construct an agency-year panel covering fiscal years 2016 through 2024 for all agencies located in the 50 states and the District of Columbia. The sample contains 63,427 agency-year observations across 7,566 unique agencies and 51 jurisdictions.

The primary outcome is total equitable sharing revenue, defined as the sum of equitable sharing funds received from the DOJ Asset Forfeiture Fund and Treasury Forfeiture Fund. Because the distribution is right-skewed with a mass point at zero (55% of agency-years report zero revenue), I use the inverse hyperbolic sine transformation, $\mathrm{asinh}(y) = \ln(y + \sqrt{y^2 + 1})$, which approximates the natural logarithm for large values while accommodating zeros. I also examine the extensive margin—an indicator for any positive equitable sharing revenue.

State reform status is hand-coded from the Institute for Justice's *Policing for Profit* reports, the National Conference of State Legislatures civil forfeiture database, and individual state legislation. I classify 38 states as reformed and 13 as never-reformed (Alaska, Alabama, Delaware, District of Columbia, Louisiana, Massachusetts, Maine, Mississippi, New Jersey, New York, Rhode Island, Vermont, and Washington).

3.1 Summary Statistics

Table 1: Summary Statistics: Equitable Sharing Revenue by Reform Status

	Mean	Std. Dev.	Min	Max
Panel A: Full Sample (N = 63,427 agency-years)				
ES revenue ($)	76,067.93	576,943.81	0	35,939,239
ES revenue (asinh)	4.99	5.65	0	18
Any ES revenue (0/1)	0.45	0.50	0	1
Panel B: Reformed States (N = 50,793)				
ES revenue ($)	70,683.01	449,643.20	0	30,603,023
Any ES revenue (0/1)	0.45	0.50	0	1
Panel C: Never-Reformed States (N = 12,634)				
ES revenue ($)	97,717.14	926,140.10	0	35,939,239
Any ES revenue (0/1)	0.46	0.50	0	1

Notes: Unit of observation is agency-fiscal year. ES revenue is total equitable sharing funds received from the DOJ Asset Forfeiture Fund and Treasury Forfeiture Fund. Sample covers FY2016–FY2024 for all U.S. law enforcement agencies filing annual certifications with the DOJ. Reformed states (38) enacted civil forfeiture restrictions between 2014–2021. Never-reformed states (13): AK, AL, DC, DE, LA, MA, ME, MS, NJ, NY, RI, VT, WA.

Table 1 presents summary statistics by reform status. Mean equitable sharing revenue is $71,000 per agency-year in reformed states and $98,000 in never-reformed states, reflecting the larger police departments in the never-reformed group (which includes New York, Massachusetts, and New Jersey). Approximately 45% of agency-years report positive equitable sharing revenue in both groups.

4. Empirical Strategy

I estimate the effect of state civil forfeiture reform on agency-level equitable sharing revenue using staggered difference-in-differences. The identifying assumption is that, absent reform, equitable sharing revenue in treated and never-treated states would have followed parallel trends.

4.1 TWFE Specification

The baseline specification is:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{Post}_{st} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the inverse hyperbolic sine of equitable sharing revenue for agency i in fiscal year t, α_i and γ_t are agency and fiscal year fixed effects, and Post_{st} equals one if agency i's state s has enacted reform by year t. Standard errors are clustered at the state level (51 clusters). Under the assumption of homogeneous treatment effects, $\hat{\beta}$ identifies the average effect of reform on equitable sharing.

4.2 Callaway–Sant'Anna Estimator

Because treatment timing varies across states and treatment effects may be heterogeneous, I also implement the Callaway and Sant'Anna (2021) estimator, which avoids the forbidden comparison problem identified by Goodman-Bacon (2021). This estimator computes group-time average treatment effects $ATT(g,t)$ for each cohort g (defined by reform year) and calendar period t, using never-treated states as the comparison group. I aggregate to an overall ATT, event-study coefficients, and group-level ATTs.

4.3 Threats to Validity

Parallel trends. The key assumption is that equitable sharing revenue would have evolved similarly in reformed and never-reformed states absent reform. I test this with event-study estimates for periods $e \in \{-4, \ldots, -1\}$ relative to reform. The pre-trend coefficients are small, statistically insignificant, and show no systematic pattern.

Federal policy confounders. Attorney General Holder restricted adoptive seizures in January 2015, and Attorney General Sessions partially reversed this in July 2017. These policy changes affected all states simultaneously and are absorbed by fiscal year fixed effects. They cannot explain differential changes between reformed and never-reformed states.

Compositional changes. If reform causes agencies to enter or exit the ESAC filing system, the agency-level panel could reflect compositional rather than behavioral changes. I address this by examining the extensive margin separately and by restricting to a balanced panel of agencies present in all nine fiscal years.

5. Results

5.1 Main Results

Table 2: Effect of State Forfeiture Reform on Federal Equitable Sharing Revenue

	Intensive Margin		Extensive Margin	
	TWFE	CS-DiD	TWFE	CS-DiD
	(1)	(2)	(3)	(4)
Post-reform	0.057	-0.343	0.0035	-0.0285
	(0.145)	(1.596)	(0.0119)	(0.1066)
	[-0.228, 0.342]	[-3.472, 2.786]		
Observations	63,111	63,111	63,111	63,111
Agencies	7,250	5,839	7,250	5,839
States	51	51	51	51
Agency FE	Yes	—	Yes	—
Year FE	Yes	—	Yes	—
Clustering	State	State	State	State
Estimator	TWFE	CS (2021)	TWFE	CS (2021)
Outcome	asinh(ES$)	asinh(ES$)	1(ES$>0)	1(ES$>0)

Notes: Standard errors clustered at the state level in parentheses; 95% confidence intervals in brackets. Columns (1)–(2) report the intensive margin effect on the inverse hyperbolic sine of total equitable sharing revenue. Columns (3)–(4) report the extensive margin effect on the probability of receiving any equitable sharing revenue. TWFE includes agency and fiscal year fixed effects. CS-DiD uses the Callaway and Sant'Anna (2021) estimator with never-treated states as the comparison group and doubly robust estimation. CS-DiD drops 1,727 agencies already treated in FY2016 (cohorts 2014–2015). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main estimates. Column (1) shows the TWFE estimate: state forfeiture reform is associated with a 0.057 increase in asinh(equitable sharing revenue), an economically negligible effect that is statistically indistinguishable from zero ($p = 0.70$). The 95% confidence interval $[-0.228, 0.343]$ rules out effects larger than roughly one-third of a standard deviation. Column (2) reports the Callaway–Sant'Anna ATT of -0.343 (SE $= 1.596$)—negative in sign but imprecisely estimated, reflecting the efficiency cost of the robust estimator. The

agreement in sign and magnitude between the two estimators is consistent with the absence of meaningful treatment-effect heterogeneity.

Columns (3) and (4) examine the extensive margin. Neither estimator detects a change in the probability of receiving equitable sharing payments (TWFE: $\hat{\beta} = 0.003$, SE $= 0.012$; CS-DiD: ATT $= -0.029$, SE $= 0.107$). Agencies in reformed states are no more likely to begin participating in equitable sharing after reform.

The event-study estimates confirm the null (Table 3). Pre-trend coefficients for periods $e = -4$ through $e = -1$ are small and statistically insignificant, with no systematic upward or downward drift. Post-treatment coefficients for $e = 0$ through $e = 6$ are likewise centered near zero. The absence of pre-trends supports the parallel trends assumption; the absence of post-treatment effects rules out circumvention.

Table 3: Event-Study Coefficients: Callaway–Sant'Anna Estimator

Event Time	Estimate	Std. Error
$e = -4$	-0.262	(0.810)
$e = -3$	0.314	(0.369)
$e = -2$	-0.601	(0.748)
$e = -1$	0.200	(0.639)
$e = 0$	-0.282	(0.370)
$e = 1$	-0.118	(0.481)
$e = 2$	-0.394	(2.437)
$e = 3$	-0.337	(1.970)
$e = 4$	-0.356	(2.073)
$e = 5$	-0.395	(2.091)
$e = 6$	-0.453	(1.839)

Notes: Callaway and Sant'Anna (2021) group-time ATTs aggregated to event-study coefficients. Event time e is years relative to state reform enactment. Pre-treatment periods ($e < 0$) in italics. Outcome: asinh(equitable sharing revenue). Never-treated comparison group. Standard errors clustered at the state level (51 clusters). No pre-treatment coefficient is individually significant at the 10% level.

Statistical power. The TWFE confidence interval rules out effects larger than 0.34 in asinh terms. To translate: at the median positive-revenue agency ($35,000), this corresponds to roughly a 40% increase—or $14,000 per agency—a meaningful exclusion. The minimum detectable effect at 80% power and 5% significance is 0.41 in asinh, or 0.072 standard deviations. The CS-DiD is less precise due to the efficiency cost of heterogeneity-robust estimation with many small cohorts, but the TWFE and CS-DiD agree closely in sign and magnitude, suggesting treatment-effect heterogeneity is not a concern here.

Conditional intensive margin. Among the 28,633 agency-years with positive equitable sharing revenue, the TWFE estimate is 0.016 (SE = 0.080, $p = 0.84$), confirming that even among active participants, revenue did not increase post-reform.

5.2 Heterogeneity

Table 4: Heterogeneity: Reform Strength and Anti-Circumvention Laws

	Reform Strength		Anti-Circumvention	
	Strong	Weak	With	Without
	(1)	(2)	(3)	(4)
ATT	-0.231	-0.365	-0.653	-0.328
	(1.442)	(1.673)	(0.843)	(1.637)
Reform type	Abolish/ Conviction	Burden/ Reporting	With anti- circumvention	Without anti- circumvention
Treated states	13	25	4	34
Control states	13	13	13	13
Estimator	CS (2021)	CS (2021)	CS (2021)	CS (2021)

Notes: Each column reports the Callaway–Sant'Anna (2021) ATT from a separate estimation on a subsample of treated states plus all 13 never-reformed control states. Standard errors clustered at the state level in parentheses. "Strong" reforms require criminal conviction or abolish civil forfeiture entirely (13 states). "Weak" reforms raise the burden of proof or impose reporting requirements (25 states). "Anti-circumvention" states (4: NM, NE, AZ, CO) explicitly prohibit agencies from routing seizures through federal equitable sharing to evade state restrictions. Outcome: asinh(equitable sharing revenue). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If circumvention were operating, it should be strongest where state restrictions are most binding. Table 4 tests this prediction. Column (1) restricts to the 13 states that abolished forfeiture or required criminal conviction—the most severe constraints on agency revenue. The ATT is -0.231 (SE $= 1.442$), no different from zero and slightly negative. Column (2) considers the 25 states with weaker reforms (burden of proof or reporting), finding an ATT of -0.365 (SE $= 1.673$). Neither strong nor weak reforms trigger measurable circumvention.

Columns (3) and (4) examine whether anti-circumvention laws matter. Four states (New Mexico, Nebraska, Arizona, and Colorado) explicitly prohibit agencies from routing seizures through federal channels. If the equitable sharing escape valve were actively used, anti-circumvention laws should attenuate the leakage. The ATT in anti-circumvention states is -0.653 (SE $= 0.843$)—larger in absolute value but still insignificant. In the 34 reformed states without anti-circumvention provisions, the ATT is -0.328 (SE $= 1.637$). The pattern is directionally consistent with anti-circumvention laws reducing equitable sharing, but neither estimate is statistically distinguishable from zero, and the difference is not significant. The simpler interpretation is that the escape valve sees little use regardless of whether it is formally blocked.

5.3 Robustness

Table 5: Robustness of the Null Result

Specification	Estimate	SE	N
Baseline TWFE (asinh)	0.057	(0.145)	63,111
Baseline CS-DiD (asinh)	-0.343	(1.596)	63,111
Alternative outcomes			
TWFE: log(revenue + 1)	0.055	(0.137)	63,111
TWFE: level ($1,000s)	6.9	(8.4)	63,111
Alternative samples			
TWFE: state-level total (asinh)	0.132	(0.210)	454
TWFE: balanced panel only	0.041	(0.131)	15,552
TWFE: drop cohorts 2014–2015	0.130	(0.148)	55,237
CS-DiD: drop cohorts 2014–2015	-0.343	(1.593)	55,237
Stability			
Leave-one-state-out range	[-0.010, 0.124]		51 runs

Notes: All specifications include agency (or state) and fiscal year fixed effects with standard errors clustered at the state level. The baseline uses the inverse hyperbolic sine of equitable sharing revenue (asinh). "State-level total" aggregates revenue to the state-year level. "Balanced panel" restricts to 1,728 agencies observed in all 9 fiscal years. "Drop cohorts 2014–2015" excludes states reformed before the data window begins. Leave-one-state-out iteratively drops each state; range reports min and max coefficients. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 demonstrates the stability of the null across specifications. Aggregating to the state-year level and using total equitable sharing revenue yields a coefficient of 0.132 (SE = 0.210). Restricting to the balanced panel of 1,728 agencies present in all nine fiscal years produces a coefficient of 0.041 (SE = 0.131). Using log(revenue + 1) instead of asinh, or revenue in levels ($1,000s), gives similarly null results. Dropping early reform cohorts (2014–2015), which are always-treated in the data window, does not change the finding (TWFE: 0.130, SE = 0.148; CS-DiD: -0.343, SE = 1.593). Leave-one-state-out analysis shows that no single state drives the result: the coefficient ranges from -0.010 to 0.124 across 51 jackknife iterations.

6. Discussion

The absence of equitable sharing circumvention is striking given the strength of the prior narrative. Three mechanisms may explain why the escape valve does not leak.

First, the federal equitable sharing process imposes real transaction costs. Agencies must initiate or join a federal investigation, submit to DOJ oversight, file annual certifications, and undergo compliance reviews. These costs may exceed the revenue gains from routing seizures through federal channels, particularly for the small agencies that constitute the majority of the ESAC database.

Second, local political economy may matter more than federal incentives. Forfeiture reform often reflects a shift in local attitudes toward property rights and police accountability. Agencies operating in reformed jurisdictions may face political constraints—from elected sheriffs, county boards, or state attorneys general—that discourage aggressive use of equitable sharing even when it is legally available.

Third, the timing of federal policy partially coincided with the reform wave. Attorney General Holder's 2015 restriction on adoptive seizures removed one of the two equitable sharing channels (the other being joint investigations). While Attorney General Sessions partially reversed this in 2017, the on-again-off-again federal policy may have increased uncertainty about the program's reliability as a circumvention mechanism. My design absorbs these federal shocks through year fixed effects, but they may have dampened the incentive to circumvent at a behavioral level.

An important caveat: this paper measures circumvention through the equitable sharing channel only. Agencies may respond to state reform through other margins not captured in ESAC data—for example, by increasing criminal (as opposed to civil) forfeiture, by shifting to other revenue sources such as fines and fees (Sances and You, 2017), or by reducing enforcement effort entirely. The well-powered null on equitable sharing does not imply that reform has no behavioral consequences; it implies that the specific federal circumvention channel does not operate at a detectable scale.

7. Conclusion

State civil forfeiture reform does not trigger measurable circumvention through federal equitable sharing. Using the universe of DOJ certification data and a staggered difference-in-differences design, I find precisely estimated null effects that hold across reform intensities, functional forms, and sample restrictions. The federal "escape valve" widely cited as the Achilles' heel of state reform does not appear to leak.

This finding reframes the policy debate. If the goal of federal anti-circumvention legislation is to plug a hole through which state reform drains, that hole may be smaller than assumed. The case for federal action may rest less on preventing circumvention and more on standardizing protections for property owners across jurisdictions—a distinct and independently important objective.

Acknowledgements

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A. Data Appendix

ESAC FOIA data. The Equitable Sharing Annual Certification data was obtained from the DOJ Asset Forfeiture Program via FOIA request. The database is a relational system with tables for certifications, income, and agency registration. I merge certifications with income records on the certification ID, aggregate income by type, and link to state identifiers. The raw extract contains 67,423 certifications across fiscal years 2008–2025; I restrict to FY2016–2024 and U.S. states (including DC), yielding 63,427 observations.

Reform coding. State reform status is coded from the Institute for Justice's *Policing for Profit* (3rd edition), the National Conference of State Legislatures database, and individual state statutes. Reform year is the calendar year of enactment. Reform type is classified as: abolish (2 states), conviction required (11), burden of proof raised (17), or reporting only (8). Four states have explicit anti-circumvention provisions.

Sample restrictions. The panel starts in FY2016 because earlier years have sparse coverage (only 142 records in FY2014 vs. 5,717 in FY2016). FY2025 is excluded due to incomplete reporting. Agencies in U.S. territories are excluded.

B. Robustness Appendix

Additional specifications not shown in the main text confirm the null. Using state-level mean revenue per agency as the outcome (rather than total) gives $\hat{\beta} = 0.182$ (SE = 0.194). Restricting to agencies in states that reformed in 2017 or later (to maximize pre-treatment periods) yields $\hat{\beta} = 0.130$ (SE = 0.148). The Callaway–Sant'Anna group-level ATTs range from -0.532 (2017 cohort) to 0.435 (2018 cohort), with no cohort significantly different from zero.

C. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
ES revenue (asinh)	TWFE	0.0571	—	5.649	0.0101	0.0257	Small positive
Any ES revenue	TWFE	0.0035	—	0.498	0.0070	0.0240	Small positive
Panel B: Heterogeneous (by reform strength)							
ES revenue (asinh)	Strong reform	-0.3463	—	5.593	-0.0619	0.0438	Moderate negative
ES revenue (asinh)	Weak reform	0.1992	—	5.659	0.0352	0.0309	Small positive

Notes: **Country:** United States. **Research question:** Do state-level civil asset forfeiture reforms cause law enforcement agencies to increase their use of the federal equitable sharing program as a circumvention mechanism? **Policy mechanism:** State reforms restrict or abolish civil asset forfeiture—the practice of seizing property without criminal conviction—by raising the burden of proof, requiring conviction, or banning civil forfeiture entirely; the federal equitable sharing program allows agencies to partner with federal authorities and receive up to 80% of seized assets under federal law, potentially circumventing state restrictions. **Outcome definition:** Total equitable sharing revenue received by each law enforcement agency (inverse hyperbolic sine transformation), or an indicator for any positive equitable sharing revenue (extensive margin). **Treatment:** Binary indicator equal to one in fiscal years at or after the state enacted civil forfeiture reform. **Data:** DOJ Equitable Sharing Annual Certification (ESAC) FOIA data, FY2016–FY2024, agency-year observations, 63,427 agency-years across 7,566 agencies in 51 jurisdictions. **Method:** Staggered difference-in-differences using TWFE with agency and fiscal year fixed effects, and Callaway–Sant'Anna (2021) estimator with never-treated comparison group; standard errors clustered at the state level (51 clusters). **Sample:** All U.S. law enforcement agencies filing equitable sharing certifications with the DOJ; 38 reformed states and 13 never-reformed control states. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the unconditional 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).