

The Escape Valve Illusion: State Forfeiture Reform and the Federal Equitable Sharing Program

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Abstract

Between 2014 and 2022, 36 US states reformed their civil asset forfeiture laws. A persistent concern is that these reforms are undermined by an “escape valve”: law enforcement agencies routing seizures through the federal equitable sharing program, which operates under federal law regardless of state restrictions. Using the universe of DOJ equitable sharing certifications—51,868 agency-year observations covering 6,562 agencies across 50 states from FY2009 to FY2024—I find no evidence of circumvention. TWFE and Callaway-Sant’Anna estimates show that post-reform agencies did not increase their equitable sharing receipts (TWFE: -0.031 , $SE = 0.114$; CS-DiD: -0.470 , $SE = 0.899$). Stronger reforms (conviction requirements and abolition) show a marginally significant *decline* in equitable sharing (-0.235 , $SE = 0.141$). The data can rule out large-scale substitution into federal channels, suggesting that state reform retains bite despite overlapping federal jurisdiction.

JEL Codes: K42, H77, H11

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1. Introduction

In 2015, the Institute for Justice reported that the federal government returned \$6.8 billion in forfeited assets to state and local law enforcement agencies over the preceding decade through “equitable sharing”—a program that allows agencies to bypass state forfeiture restrictions by partnering with federal authorities (Carpenter et al., 2020). Reform advocates warned that no matter what states did to restrict civil forfeiture, this federal escape valve would render their efforts futile (Holcomb et al., 2018; Pimentel, 2017). By 2022, 36 states had enacted some form of forfeiture reform. The question remained: did the escape valve open?

This paper provides the first causal evidence on whether state forfeiture reform triggers circumvention through the federal equitable sharing program. The answer, using the universe of DOJ equitable sharing certifications, is no. Agencies in reformed states did not increase their equitable sharing receipts relative to agencies in states that did not reform.

The concern about regulatory leakage through equitable sharing is intuitive and well-documented descriptively. Under the DOJ’s Asset Forfeiture Program, state and local agencies can “adopt” a seizure—transferring it to federal jurisdiction in exchange for up to 80% of the proceeds (Worrall, 2004; Baicker and Jacobson, 2004). Because federal civil forfeiture law requires only a preponderance of evidence and imposes no conviction requirement, adoptive seizures effectively nullify state reforms that raise these thresholds. Several legal scholars have argued that this jurisdictional arbitrage makes state reform “largely symbolic” (Carpenter, 2019; Nelson, 2016; Holcomb et al., 2018).

Yet the empirical evidence on circumvention has been limited to cross-sectional correlations and case studies. Carpenter et al. (2020) documented a positive correlation between state reforms and equitable sharing activity, but without controlling for secular trends or the staggered timing of reform adoption. Holcomb et al. (2018) examined individual states but could not distinguish circumvention from broader trends in federal forfeiture policy.

I exploit the staggered adoption of forfeiture reforms across 36 states between 2014 and 2022 in a difference-in-differences framework. The key outcome is agency-level equitable sharing funds received, drawn from a FOIA release of the DOJ Equitable Sharing Agreement and Certification (ESAC) database. This dataset covers every state and local law enforcement agency that participates in equitable sharing, yielding 51,868 agency-year observations across 6,562 unique agencies.

The main finding is a null: TWFE estimates show a coefficient of -0.031 ($SE = 0.114$) on log equitable sharing funds, and the heterogeneity-robust Callaway-Sant’Anna estimator yields an ATT of -0.470 ($SE = 0.899$). At the extensive margin (participation), the effect is similarly null (-0.003 , $SE = 0.011$). The TWFE minimum detectable effect at 80% power is

0.32 log points—equivalent to a 38% increase in equitable sharing funds—meaning the data can rule out large-scale substitution into federal channels, though more modest circumvention remains possible.

Heterogeneity analysis provides suggestive evidence of a *compliance spillover*: agencies in states with the strongest reforms (abolition of civil forfeiture or criminal conviction requirements) show a marginally significant *decline* in equitable sharing of -0.235 log points (SE = 0.141, $p = 0.10$), while agencies in states with weaker reforms (burden-of-proof changes or reporting requirements) show no effect (-0.015 , SE = 0.142). This pattern is inconsistent with circumvention—which predicts that stronger reforms would trigger *more* leakage—and instead suggests that reform may shift institutional norms beyond its statutory scope.

This paper contributes to several literatures. First, it addresses the growing body of work on civil asset forfeiture and policing incentives (Baicker and Jacobson, 2004; Worrall, 2004; Holcomb et al., 2018; Kelly, 2022). Existing work has documented that equitable sharing revenues respond to fiscal incentives (Baicker and Jacobson, 2004) and that forfeiture powers may distort policing priorities (Worrall, 2004), but the causal effect of reform on circumvention has not been estimated. Second, it speaks to the broader question of regulatory leakage in federal systems (Sigman, 2014; Auffhammer and Kellogg, 2011). The finding that sub-national regulation *does not* trigger systematic jurisdictional arbitrage contrasts with settings like environmental regulation, where cross-border leakage is well-documented (Fowlie et al., 2014). Third, it provides the first systematic analysis of the DOJ ESAC database as a research resource, demonstrating its utility for studying law enforcement behavior at the agency level.

The paper proceeds as follows. Section 2 describes the institutional background of civil asset forfeiture and equitable sharing. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 presents results. Section 6 discusses mechanisms and implications.

2. Institutional Background

Civil asset forfeiture. Civil asset forfeiture allows law enforcement agencies to seize property suspected of being connected to criminal activity without charging the owner with a crime. The legal proceeding is *in rem*—against the property itself—and proceeds under civil rather than criminal standards of evidence. Historically, most states required only a preponderance of evidence, and the burden of proof often fell on the property owner to demonstrate innocence (Worrall, 2004).

The reform wave. Beginning in 2014, a bipartisan reform movement swept across US states. By 2022, 36 states had enacted some form of reform, varying in stringency: four states (New Mexico, Nebraska, North Carolina, and Maine) abolished civil forfeiture entirely, requiring criminal conviction and routing all proceeds through the general fund; eighteen states enacted conviction requirements; and twelve states raised the burden of proof to “clear and convincing evidence” or higher without requiring conviction. Four states enacted transparency and reporting requirements without changing substantive standards.

The equitable sharing program. The DOJ’s equitable sharing program, established under the Comprehensive Crime Control Act of 1984, allows state and local agencies to share in the proceeds of federal forfeitures. There are two pathways. In *joint investigations*, agencies collaborate with federal partners and share proceeds by agreement. In *adoptive seizures*, agencies make seizures under state law and then transfer them to a federal agency for processing under federal law, receiving up to 80% of the net proceeds. Adoptive seizures are the primary mechanism of concern, because they allow agencies to circumvent state restrictions: federal law governs the evidentiary standard, not state law.

The anti-circumvention response. Recognizing this vulnerability, three states—New Mexico (2015), Nebraska (2016), and Colorado (2017)—enacted explicit anti-circumvention statutes prohibiting agencies from transferring seizures to federal jurisdiction solely to avoid state restrictions. These laws provide a natural test: if circumvention is the primary channel, anti-circumvention laws should attenuate any post-reform increase in equitable sharing.

3. Data

The primary dataset is the DOJ Equitable Sharing Agreement and Certification (ESAC) database, obtained through FOIA. Every state and local law enforcement agency that participates in equitable sharing must file an annual ESAC certification reporting income received and expenditures made from the equitable sharing fund. The FOIA release covers the universe of certifications from FY2009 through FY2024.

Key variables. The main outcome variable is equitable sharing funds received (ESAC income type 1: “Equitable Sharing Funds Received”), which captures direct federal disbursements to the agency. I construct three outcome measures: (1) log of equitable sharing funds received plus one, to accommodate zeros; (2) an extensive margin indicator for whether the agency received any equitable sharing funds; and (3) the level of funds in dollars.

Sample construction. I match agency records to states using the NCIC state code field and restrict to US states and DC. I drop North Carolina, which abolished civil forfeiture before the panel begins (2000). I keep agencies with at least three years of certification data. The final sample contains 51,868 agency-year observations across 6,562 unique agencies in 50 states, with 36 states classified as reformed and 14 as never-reformed.

Treatment coding. I code the reform year for each state based on the effective date of the relevant statute, drawn from Institute for Justice reports and NCSL legislative databases. Reform types are classified as: abolition (2 states), conviction required (21 states), burden of proof raised (12 states), or reporting only (4 states). Anti-circumvention statutes are coded for NM, NE, and CO. [Table 1](#) presents summary statistics.

Table 1: Summary Statistics: Equitable Sharing Activity, Pre-Reform Period

	Full Sample	Reform States	Never-Reform
Agency-years	20,024	8,805	11,219
Unique agencies	4,945	3,521	1,424
Mean ES funds (\$)	65,960	43,286	83,756
SD ES funds (\$)	603,971	189,368	788,818
Mean log(ES funds + 1)	4.0	4.0	4.1
SD log(ES funds + 1)	5.1	5.1	5.2
Share receiving ES funds (%)	39.6	38.8	40.1
Mean agency budget (\$M)	39.9	23.0	53.2

Notes: Pre-reform period observations only (fiscal years before each state’s reform; all years for never-reform states). ES funds = Equitable Sharing Funds Received (DOJ ESAC Type 1 income). Sample restricted to US law enforcement agencies filing accepted ESAC certifications, FY2009–2024, excluding North Carolina (abolished pre-panel). 36 reform states, 14 never-reform states, 50 states total.

4. Empirical Strategy

The baseline specification is a two-way fixed effects (TWFE) regression:

$$Y_{a,t} = \alpha_a + \gamma_t + \beta \cdot \text{PostReform}_{s(a),t} + \varepsilon_{a,t} \quad (1)$$

where $Y_{a,t}$ is the outcome for agency a in fiscal year t , α_a are agency fixed effects, γ_t are fiscal year fixed effects, and $\text{PostReform}_{s(a),t}$ equals one if agency a ’s state s has enacted forfeiture reform by year t . Standard errors are clustered at the state level, yielding 50 clusters. The coefficient β captures the average change in equitable sharing activity for agencies in reformed

states relative to agencies in never-reformed states, after accounting for agency-specific levels and common year shocks.

Addressing staggered adoption bias. With treatment rollout spanning nine years (2014–2022), the TWFE estimator may be biased by heterogeneous treatment effects across cohorts (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeulle, 2020). I implement the Callaway and Sant’Anna (2021) estimator, which constructs group-time average treatment effects using only never-treated units as the comparison group and a varying base period. I aggregate to an overall ATT and to event-time coefficients for event study plots.

Identification. The identifying assumption is parallel trends: absent reform, agencies in states that eventually reformed would have followed the same trajectory as agencies in states that did not. I assess this using event-study estimates of pre-treatment coefficients from the Callaway-Sant’Anna framework. Year fixed effects absorb federal-level policy changes, including Attorney General Holder’s 2015 restrictions on adoptive seizures and Attorney General Sessions’ 2017 reversal, as these applied uniformly to all states.

5. Results

Main estimates. Table 2 reports the main results. Across all specifications and both estimators, the effect of state forfeiture reform on equitable sharing is economically small and statistically insignificant. The TWFE estimate on log equitable sharing funds is -0.031 ($SE = 0.114$), essentially zero. The Callaway-Sant’Anna overall ATT is -0.470 ($SE = 0.899$), negative but imprecise. Neither the extensive margin (participation) nor the level specification yields significant effects.

Power and minimum detectable effects. The TWFE standard error of 0.114 implies a minimum detectable effect (at 80% power, 5% size) of 0.32 log points, corresponding to a 38% change in equitable sharing funds. The data can therefore rule out circumvention of economically meaningful magnitude. A 38% increase would represent approximately \$24,000 per agency per year, or \$144 million in additional equitable sharing disbursements to agencies in reformed states—well below the \$6.8 billion cumulative total reported by the Institute for Justice but large enough to represent a meaningful policy response.

Pre-trends. The Callaway-Sant’Anna event study shows pre-treatment coefficients centered on zero: the estimates for event times -5 through -1 are 0.346, -0.208 , 0.371, -0.783 , and 0.960, with standard errors between 0.65 and 0.94. While individually noisy, they show no systematic upward or downward trend. Post-reform estimates decline monotonically from

−0.165 at event time 0 to −0.520 at event time 5, consistent with a gradual compliance spillover rather than circumvention.

Robustness. Table 3 presents robustness checks. The inverse hyperbolic sine transformation yields nearly identical results (−0.033, SE = 0.121). The placebo test—randomly assigning reform years to agencies in never-reformed states—produces a null coefficient (0.066, SE = 0.073). Leave-one-state-out analysis shows the TWFE coefficient ranges from −0.090 to +0.023 across 50 jackknife iterations, confirming that no single state drives the result.

At the state level, I find a positive but statistically insignificant effect on total equitable sharing funds (+0.760, SE = 0.477, $p = 0.12$) and on the number of agencies filing certifications (+12.3, SE = 13.6). These point estimates suggest some extensive-margin entry—additional agencies filing ESAC certifications post-reform—but the effects are imprecise.

Table 3: Robustness Checks

	Coefficient	SE	Obs.
<i>Panel A: Agency-Level Specifications</i>			
TWFE: log(ES+1) [baseline]	-0.031	(0.114)	51,868
TWFE: IHS(ES)	-0.033	(0.121)	51,868
CS-DiD: log(ES+1)	-0.470	(0.899)	51,868
Placebo (never-reformed states)	0.066	(0.073)	11,219
<i>Panel B: State-Level Aggregates</i>			
log(Total ES Funds)	0.760	(0.477)	604
Share of Agencies Participating	0.0459	(0.0405)	604
Number of Agencies Filing	12.3	(13.6)	604

Notes: All specifications include unit and fiscal year fixed effects. Standard errors clustered at state level. Panel A: Agency-level regressions. Placebo randomly assigns reform years to agencies in never-reformed states. Panel B: State-year aggregates (50 states \times 12–16 years). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Heterogeneity. Table 4 explores heterogeneity by reform stringency and anti-circumvention legislation. Agencies in states with strong reforms (abolition or conviction requirements, 20 treated states) show a marginally significant decline of −0.235 log points (SE = 0.141, $p = 0.10$), while agencies in states with weak reforms (burden of proof or reporting, 16 treated states) show essentially no effect (−0.015, SE = 0.142). This gradient is inconsistent with circumvention, which predicts that stronger reforms—imposing greater restrictions on state-level seizures—would trigger *more* substitution toward federal channels. Instead, the pattern suggests that strong reforms shift institutional norms, reducing equitable sharing

alongside state-level forfeiture.

The anti-circumvention subsample is too small (3 treated states) for precise estimation but shows a positive point estimate (+0.395, SE = 0.278), consistent with these states being selected for anti-circumvention legislation precisely because their agencies were more actively using equitable sharing.

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

	Strong Reform (1)	Weak Reform (2)	No Anti-Circ. (3)	Anti-Circ. (4)
Post Reform	-0.235 (0.141)	-0.015 (0.142)	-0.042 (0.116)	0.395 (0.278)
Dep. var.	log(Equitable Sharing Funds + 1)			
Agency & Year FE	Yes	Yes	Yes	Yes
Treated states	20	16	33	3
Observations	24,229	38,858	50,334	12,753

Notes: TWFE with agency and fiscal year fixed effects, standard errors clustered at state level. “Strong Reform” = states that abolished forfeiture or required criminal conviction (stringency ≥ 3). “Weak Reform” = states that raised burden of proof or enacted reporting requirements only (stringency ≤ 2). “Anti-Circ.” = states that also enacted anti-circumvention statutes barring federal equitable sharing adoption (NM, NE, CO). Each column includes agencies in the relevant reform-type states plus all never-reform state agencies as controls. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6. Discussion

The central finding is that state forfeiture reforms do not produce large average increases in agencies’ reported equitable sharing receipts. This result pushes back against the widely held belief that equitable sharing renders state reform “largely symbolic” (Carpenter, 2019), though the data cannot fully separate adoptive seizures (the mechanism of concern) from joint investigations in the ESAC totals. Subject to this caveat, the evidence suggests that state-level action retains more bite than commonly assumed.

Why doesn’t the escape valve open?. Three mechanisms may explain this result. First, administrative friction: adoptive seizures require coordination with federal agencies, paperwork, and processing delays that may make them unattractive relative to state-level forfeiture, even when state standards are tightened (Worrall, 2004). Second, institutional signaling: state reform may shift the political environment surrounding forfeiture, making agencies cautious about *all* forms of seizure activity, including federal channels. The stronger

effect of stringent reforms is consistent with this channel. Third, attention effects: reform may draw scrutiny from the public and media, making agencies reluctant to engage in any activity that could be perceived as circumvention.

Limitations. Several caveats apply. The ESAC data captures only agencies that file certifications; agencies that participate in equitable sharing without filing—or that exit the program entirely—are not observed. The data does not distinguish adoptive seizures (the circumvention channel) from joint investigations (which are less subject to circumvention concerns). With 50 state clusters, inference relies on cluster-robust standard errors, and the Callaway-Sant’Anna estimates are imprecise. Finally, the reform coding treats each state’s reform date as a single event, abstracting from implementation lags and enforcement variation.

Broader implications. The absence of regulatory leakage in this setting is noteworthy. In environmental regulation, cross-border leakage is a well-documented response to sub-national policy (Fowlie et al., 2014; Sigman, 2014). In forfeiture, the federal “escape valve” was structurally designed to enable circumvention, yet agencies did not exploit it. This suggests that institutional frictions and cultural norms may be more important constraints on regulatory arbitrage than the formal legal framework. For policymakers, the implication is that state forfeiture reform is not rendered futile by federal jurisdiction—though federal reform of the equitable sharing program would further strengthen the policy.

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Appendix: Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
log(ES Funds + 1)	-0.031	0.114	5.139	-0.0059	0.0221	Small negative
Participation	-0.0032	0.0107	0.489	-0.0065	0.0219	Small negative
ES Funds (\$)	5,348	9,042	603,971	0.0089	0.0150	Small positive
<i>Panel B: Heterogeneous (by Reform Stringency)</i>						
log(ES+1): Strong reform	-0.235	0.141	5.139	-0.0457	0.0274	Small negative
log(ES+1): Weak reform	-0.015	0.142	5.139	-0.0030	0.0276	Null

Notes: **Country:** United States. **Research question:** Do state civil asset forfeiture reforms cause law enforcement agencies to circumvent restrictions by increasing their use of the federal equitable sharing program? **Policy mechanism:** State reforms restrict or abolish civil asset forfeiture, raising the burden of proof for seizures or requiring criminal conviction; the federal equitable sharing program allows agencies to partner with federal authorities and retain up to 80% of seized assets under federal law, potentially bypassing state restrictions. **Outcome definition:** Log of equitable sharing funds received plus one (DOJ ESAC Type 1 income: direct federal equitable sharing disbursements to state/local agencies) and an indicator for any positive equitable sharing receipts. **Treatment:** Binary; state enactment of civil asset forfeiture reform (conviction requirement, burden-of-proof elevation, or abolition). **Data:** DOJ Asset Forfeiture Program ESAC FOIA, FY2009–2024, agency-year level, 51,868 observations across 6,562 agencies in 50 states. **Method:** TWFE with agency and fiscal year fixed effects, standard errors clustered at the state level (50 clusters). Callaway-Sant’Anna as robustness. **Sample:** US state and local law enforcement agencies filing accepted ESAC certifications; excludes North Carolina (abolished forfeiture before panel period). Panel restricted to agencies with ≥ 3 years of data. $SDE = \hat{\beta}/SD(Y)$ 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).