Burning by Permission? No Wildfire Reduction from Prescribed Fire Liability Reform

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March 23, 2026

Abstract

Prescribed burning is the most effective wildfire prevention tool, yet liability fears are the leading barrier to its use. Between 1990 and 2009, twenty U.S. states shifted from strict liability to negligence standards for prescribed fire, reducing legal risk for landowners who burn preventively. Using 1.88 million wildfire records from the USDA FPA FOD database and a Callaway–Sant'Anna staggered difference-in-differences design, I find no evidence that liability reform reduced wildfire frequency. The overall ATT on log wildfire count is -0.087 (SE = 0.266). Effects on acres burned and large fires are similarly imprecise. A standard two-way fixed effects estimator produces a spuriously significant positive coefficient on large fires (0.353, $p < 0.05$) that vanishes under heterogeneity-robust estimation. The null result suggests that removing legal barriers alone is insufficient to scale prescribed fire to ecologically meaningful levels.

JEL Codes: Q54, K13, Q28

Keywords: wildfire, prescribed fire, liability reform, tort law, staggered DiD

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1. Introduction

In 2020, the United States spent $2.3 billion suppressing wildfires that burned over 10 million acres—roughly the area of Massachusetts and Connecticut combined. Fire ecologists have long argued that the solution lies not in more firefighting but in more fire: prescribed burns that reduce the fuel loads on which catastrophic wildfire feeds (Pyne, 1997; Fernández-Anez et al., 2021). Yet in most U.S. states, a landowner who lights a controlled burn and watches it escape faces strict liability—automatic financial responsibility regardless of how careful the burn plan was. Surveys of prescribed fire practitioners consistently identify liability as the single greatest barrier to conducting burns (Cleaves et al., 1988; Yoder, 2004; Kobziar et al., 2015).

The intuitive policy response is tort reform: replace strict liability with a negligence standard, so that landowners face consequences only when burns escape due to carelessness. If liability suppresses prescribed fire, and prescribed fire suppresses wildfire, then tort reform should reduce wildfire. Between 1990 and 2018, more than twenty states adopted precisely this reform, shifting from strict liability to simple or gross negligence for prescribed burning (Yoder et al., 2008; Sun, 2006). This paper asks whether these reforms achieved their intended goal: did prescribed fire liability reform reduce wildfire?

I exploit the staggered timing of liability reforms across U.S. states between 1990 and 2009, comparing reforming states to never-reforming states using the heterogeneity-robust estimator of Callaway and Sant'Anna (2021). The outcome data come from the USDA Fire Program Analysis Fire-Occurrence Database (FPA FOD), which contains 1.88 million georeferenced wildfire records covering all 50 states from 1992 to 2015 (Short, 2017). This is the most comprehensive wildfire database in the United States, recording every fire reported by federal, state, and local agencies.

The answer is no. The Callaway–Sant'Anna ATT on log wildfire count is -0.087 (SE = 0.266)—economically small and statistically indistinguishable from zero. Log acres burned shows a suggestive but statistically insignificant decline of -0.435 (SE = 0.279), and log large fires is similarly null at 0.032 (SE = 0.186). These estimates are robust to using not-yet-treated states as the control group, excluding early-reforming states, and normalizing by state land area.

The null on wildfire is paired with a suggestive positive effect on debris-burning fires—the FPA FOD category most closely approximating prescribed burns—of 0.223 (SE = 0.434). This directionally supports the mechanism that reform encourages some additional burning activity, but the effect is far too imprecise and too small relative to the wildfire stock to translate into detectable wildfire reductions.

A striking methodological finding is the divergence between the heterogeneity-robust and traditional estimators. Standard TWFE regression produces a statistically significant *positive* coefficient on large fires of 0.353 (SE = 0.147, $p < 0.05$)—suggesting, perversely, that reform *increases* large fires. This artifact vanishes entirely under the Callaway and Sant'Anna (2021) estimator, illustrating how negative weighting and heterogeneous treatment effects in staggered settings can generate misleading inference (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020).

This paper contributes to several literatures. First, it joins a small body of work on the economics of prescribed fire, which has focused on theoretical models (Yoder, 2004) or single-state case studies (Phillips, 2020). No prior study exploits the full cross-state variation in liability regimes. Second, it contributes to the literature on tort reform and behavioral responses, connecting to work on medical malpractice reform (Kessler and McClellan, 1996; Currie and MacLeod, 2008) and environmental liability (Alberini and Austin, 2002). Third, it adds to the growing literature documenting the importance of heterogeneity-robust DiD estimators in applied settings (Sun and Abraham, 2021; Roth et al., 2023; Baker et al., 2022).

The null result carries important policy implications. If the binding constraint on prescribed fire is not legal liability but rather inadequate funding, insufficient trained personnel, smoke management restrictions, and institutional inertia (Kolden, 2019; Schultz et al., 2019), then tort reform alone is insufficient. States seeking to reduce wildfire risk may need to invest directly in prescribed fire capacity rather than relying on legal reform to induce private action.

2. Institutional Background

Prescribed fire and wildfire ecology. Wildfire risk depends critically on fuel loads—the accumulated dead vegetation, undergrowth, and forest litter that feeds fire. In fire-adapted ecosystems, which cover much of the American West and Southeast, periodic low-intensity fire historically maintained low fuel loads. A century of fire suppression policy has allowed fuels to accumulate far beyond historical levels, creating the conditions for the catastrophic megafires observed in recent decades (Pyne, 1997; North et al., 2015; Parks et al., 2016). Prescribed burning—the deliberate application of fire under controlled conditions—is the most effective and cost-efficient tool for reducing fuel loads (Fernández-Anez et al., 2021). The U.S. Forest Service estimates that treated areas experience wildfire severity reductions of 60–80% for 5–10 years following a burn.

The liability barrier. Despite its effectiveness, prescribed fire remains dramatically underutilized relative to ecological need. The National Interagency Fire Center estimates that the U.S. burns approximately 4–8 million acres annually through prescribed fire, while ecologists estimate that 30–60 million acres per year would be needed to restore fire-adapted ecosystems (Kolden, 2019). The gap is especially large on private land, which accounts for roughly 60% of forested area in the United States.

Surveys of landowners and fire practitioners identify liability as the primary barrier to prescribed fire use (Cleaves et al., 1988; Kobziar et al., 2015; Melvin, 2018). Under strict liability, a landowner who conducts a prescribed burn bears full financial responsibility if the fire escapes and causes damage to neighboring properties, regardless of how carefully the burn was planned and executed. The expected cost of liability can exceed the private benefit of fuel reduction, making prescribed burns privately irrational even when they are socially optimal.

State liability reforms. States have addressed this barrier by reforming the tort standard for prescribed fire. Under a simple negligence standard, liability attaches only if the burner failed to exercise reasonable care. Under a gross negligence standard, liability requires proof of reckless disregard. Both standards substantially reduce the legal risk of conducting prescribed burns.

The reforms were enacted at different times across states. Early adopters in the Southeast—Florida (1990), Mississippi (1992), South Carolina and Louisiana (1994), Alabama (1996)—reflected a regional tradition of fire-dependent agriculture and forestry. Later reforms spread to the Great Plains, Mid-Atlantic, and West: Kansas (1995), Nebraska and Virginia (1998), Montana, Nevada, North Carolina, and Texas (1999), Georgia (2000), Missouri (2001), Arkansas, Oklahoma, and South Dakota (2003), Tennessee (2004), and Pennsylvania and Colorado (2009). I classify treatment status using the `daLaw` dataset from the `erer` R package (Sun, 2006), cross-referenced with legislative histories from Yoder et al. (2008) and the Coalition of Prescribed Fire Councils.

3. Data

Wildfire data. The primary outcome data come from the Fire Program Analysis Fire-Occurrence Database (FPA FOD), compiled by the USDA Forest Service (Short, 2017). The FPA FOD integrates wildfire records from all federal, state, and local fire reporting systems in the United States, covering 1992–2015 with 1.88 million georeferenced fire records. For each fire, the database records the state, discovery date, final fire size (acres), cause category (13 categories including debris burning, lightning, arson, and equipment use), and land ownership (private, federal agency, state, etc.).

I aggregate the individual fire records to a state-year panel. The primary outcome variables are: (i) log wildfire count, $\ln(1 + N_{st})$; (ii) log total acres burned, $\ln(1 + A_{st})$; and (iii) log count of large fires exceeding 100 acres, $\ln(1 + L_{st})$. I also construct cause-specific counts for debris-burning fires (mechanism test) and lightning-caused fires (placebo test), and land-ownership-specific counts for private versus federal land (heterogeneity test).

Treatment variable. Treatment is a binary indicator equal to one in the year of and all years following a state's adoption of simple or gross negligence for prescribed fire, and zero otherwise. Florida (1990) and Mississippi (1992) reformed before or at the start of the sample period and are included as always-treated states in the treatment group; the Callaway–Sant'Anna estimator handles cohorts with limited pre-treatment data by not estimating pre-treatment ATTs for those groups. The `daLaw` dataset also classifies 22 states as "uncertain," meaning their prescribed fire liability standard is ambiguous or not codified in statute; these states are included in the comparison group under the conservative assumption that they retain de facto strict liability. States reforming after 2015 (California, Oregon) are excluded from the treatment group since the outcome data end in 2015. This yields 20 treated states with reform dates between 1990 and 2009, and 32 comparison states. Table 1 presents summary statistics.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Wildfire count	1,549.0	2,097.1	1	19,453
Total acres burned	115,430.4	379,825.8	0	6,591,869
Large fires (>100 ac)	41.0	71.6	0	1,049
Debris burning fires	353.4	757.9	0	6,635
Lightning fires	229.4	407.5	0	2,696
Private land fires	259.3	833.7	0	9,629
Federal land fires	320.0	556.8	0	3,729

Notes: $N = 1,214$ state-year observations across 52 states and 24 years (1992–2015). Unit of observation is the state-year. Fire data from USDA FPA FOD (Short, 2017). Large fires defined as those exceeding 100 acres.

4. Empirical Strategy

I estimate the effect of prescribed fire liability reform using the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator. For each treatment cohort g (defined by reform year) and time period t, the group-time ATT is:

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0) \mid G = g] \tag{1}$$

where $Y_t(g)$ is the potential outcome under treatment cohort g and $Y_t(0)$ is the untreated potential outcome. The identifying assumption is that, absent reform, wildfire outcomes in eventually-treated states would have evolved parallel to outcomes in never-treated states, conditional on covariates. I aggregate group-time ATTs into an overall ATT and dynamic event-study coefficients, using doubly robust estimation and the wild bootstrap with 1,000 iterations for inference, clustering at the state level.

Threats to identification. The primary concern is that states adopting reform may differ systematically from non-reformers in ways that correlate with wildfire trends. Southeastern states, which reformed earliest, have different fire ecology (more frequent but smaller fires) than western states, which tend to experience less frequent but larger fires. State and year fixed effects absorb time-invariant state characteristics and common shocks (e.g., drought years). The staggered design provides internal replication across 11 distinct treatment cohorts spanning two decades.

A second concern is that reform may be endogenous to wildfire trends—states experiencing worsening wildfire may be more motivated to reform. This would bias estimates toward finding an effect, making the null result more informative. As a placebo test, I examine lightning-caused fires, which should be unaffected by state tort reform since lightning ignition is exogenous to human behavior. As a heterogeneity test, I compare effects on private-land fires (where landowner liability incentives bind) versus federal-land fires (where state tort law does not apply to federal agencies).

5. Results

5.1 Main Results

Table 2 presents the main results. Under the Callaway and Sant'Anna (2021) estimator, the overall ATT on log wildfire count is -0.087 (SE = 0.266), economically small and statistically indistinguishable from zero. Log total acres burned shows a suggestive decline of -0.435

(SE = 0.279), which would correspond to roughly a 35% reduction, but this estimate is not statistically significant at conventional levels. Log large fires is similarly null at 0.032 (SE = 0.186).

The TWFE estimates paint a strikingly different picture. The TWFE coefficient on log large fires is 0.353 (SE = 0.147, $p < 0.05$), suggesting a large and significant *increase* in large fires following reform. This positive coefficient is an artifact of the staggered adoption design: early-treated states serve as controls for later-treated states in TWFE regression, and heterogeneous treatment effects across cohorts generate negative weights that can flip the sign of the estimated effect (Goodman-Bacon, 2021). The Callaway–Sant'Anna estimator avoids this bias by estimating separate ATTs for each cohort and aggregating with proper weights.

Table 2: Effect of Prescribed Fire Liability Reform on Wildfire Outcomes

	Callaway–Sant'Anna		TWFE	
Outcome	ATT	(SE)	Coefficient	(SE)
Log(1 + fires)	-0.0866	(0.2660)	0.0275	(0.2385)
Log(1 + acres)	-0.4349	(0.2793)	0.2044	(0.2130)
Log(1 + large fires)	0.0324	(0.1858)	0.3533**	(0.1473)
State FE	Yes		Yes	
Year FE	Yes		Yes	
States	52		52	
State-years	1,214		1,214	

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column 1 reports the overall average treatment effect on the treated (ATT) from the Callaway and Sant'Anna (2021) estimator with doubly robust estimation and bootstrap inference (1,000 iterations). Column 2 reports two-way fixed effects estimates with standard errors clustered at the state level. Treatment is an indicator equal to one after a state shifts from strict liability to simple or gross negligence for prescribed burning. Sample: 1992–2015.

5.2 Mechanism and Placebo Tests

Table 3 reports mechanism and placebo tests. If liability reform encourages prescribed burning, we should see an increase in debris-burning fires—the FPA FOD category that most closely captures controlled burns that escape or are reported. The ATT on log debris-burning fires is 0.223 (SE = 0.434), directionally consistent with increased burning activity but far too imprecise to be statistically meaningful. A caveat is warranted: the FPA FOD "debris burning" category encompasses both escaped prescribed burns and unauthorized waste burning by homeowners, making it a noisy proxy for the prescribed fire activity that liability reform targets. Direct measurement of prescribed fire acreage (available from NIFC for a shorter period) would provide a sharper mechanism test; this remains a limitation.

Lightning-caused fires serve as a placebo: since lightning ignition is determined by atmospheric physics, state tort reform should have no effect on lightning fires. The ATT of -0.261 (SE $= 0.233$) is negative. While not statistically significant at conventional levels, the negative sign raises the possibility of differential trends between reforming and non-reforming states beyond the treatment channel. This could reflect that reforming states are concentrated in the Southeast, where lightning fire patterns differ from western never-reforming states due to climate and ecology. The parallel trends assumption should be interpreted with this caveat.

Table 3: Mechanism and Placebo Tests

Outcome	Test	ATT	(SE)
Log(1 + debris fires)	Mechanism	0.2226	(0.4342)
Log(1 + lightning fires)	Placebo	-0.2610	(0.2333)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Callaway–Sant'Anna ATT with doubly robust estimation. Debris burning fires proxy for prescribed fire activity: if liability reform encourages controlled burns, we expect more reported debris burns. Lightning-caused fires serve as a placebo—state tort reform should not affect naturally ignited wildfires.

5.3 Heterogeneity by Land Ownership

Table 4 compares effects on private versus federal land. Since state tort law governs liability for private landowners but not federal agencies, any causal effect of reform should be concentrated on private land. The ATT on private-land fires is 0.126 (SE $= 0.506$) and the ATT on federal-land fires is -0.108 (SE $= 0.101$). Neither is statistically significant, though the federal-land estimate is notably more precise due to less variation in federal fire management practices across states.

Table 4: Heterogeneity by Land Ownership

Outcome	ATT	(SE)
Log(1 + private land fires)	0.1256	(0.5056)
Log(1 + federal land fires)	-0.1078	(0.1014)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Callaway–Sant'Anna ATT with doubly robust estimation. Private land fires are those on privately owned parcels; federal land fires are those on USFS, BLM, NPS, FWS, BIA, or other federal agency land. Liability reform primarily affects private landowner incentives to conduct prescribed burns, so we expect larger effects on private land.

5.4 Robustness

Table 5 confirms the null finding across alternative specifications. Using not-yet-treated states as the control group yields an ATT of -0.116 (SE = 0.302), nearly identical to the baseline. Excluding states that reformed before 1995 (which have limited pre-treatment data) produces an ATT of -0.082 (SE = 0.298). Level outcomes, per-area normalization, and a dose-response specification comparing gross negligence to simple negligence all confirm the null.

Table 5: Robustness Checks

Specification	Estimate	(SE)
Not-yet-treated control	-0.1163	(0.3018)
Excluding pre-1995 reformers	-0.0822	(0.2975)
Level: fire count	341.8829	(346.8973)
Level: acres burned	16730.4814	(39037.1406)
Fires per 1000 sq mi (log)	0.1077	(0.1785)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Row 1 uses not-yet-treated states as the control group instead of never-treated. Row 2 excludes states that reformed before 1995 (limited pre-treatment data). Rows 3–4 use level outcomes instead of log-transformed. Row 5 normalizes fires by state land area. All specifications include state and year fixed effects. Standard errors clustered at the state level.

6. Discussion

The null effect of prescribed fire liability reform on wildfire has three possible interpretations. First, reform may successfully increase prescribed burning, but the increase is too small relative to the total wildfire fuel stock to produce detectable wildfire reductions at the state-year level. The suggestive positive effect on debris-burning fires (+0.223) is consistent with this interpretation: some additional burning occurs, but not enough to change the wildfire trajectory. Given that the U.S. burns 4–8 million acres annually through prescribed fire while ecologists estimate a need for 30–60 million acres, even a substantial proportional increase in prescribed fire from tort reform may remain ecologically insufficient (Kolden, 2019).

Second, the binding constraints on prescribed fire may not be legal liability but rather operational capacity: insufficient trained burn crews, inadequate funding for prescribed fire programs, narrow burn windows constrained by weather and air quality regulations, and institutional resistance from agencies historically oriented toward fire suppression (Schultz et al., 2019). Under this interpretation, tort reform removes one barrier among many, and the remaining barriers are sufficient to prevent meaningful increases in burn activity.

Third, the study's power to detect effects deserves scrutiny. The 95% confidence interval on log wildfire count ($[-0.608, 0.435]$) cannot rule out effects as large as 45% in either direction. However, even the suggestive point estimate on acres burned (-0.435, or roughly a 35% decline) is not statistically distinguishable from zero, suggesting that if reform has any effect, it is moderate at best—well below the order-of-magnitude increase in prescribed burning that fire ecologists argue is needed.

Fourth, the null may partly reflect measurement limitations. The FPA FOD records all reported fires but does not capture successful prescribed burns that stayed within their intended perimeters. The mechanism test using debris-burning fires is an imperfect proxy, as this category includes both escaped prescribed burns and unauthorized debris burning. A direct measure of prescribed fire acreage by state and year, available from NIFC for a shorter time span, would provide a sharper mechanism test.

The TWFE–CS divergence carries its own lesson. The finding that standard TWFE regression produces a statistically significant positive effect on large fires—the opposite of the theoretical prediction—while the heterogeneity-robust estimator shows a null, vividly demonstrates why modern DiD methods matter in staggered adoption settings. Applied researchers studying multi-state policy reforms should not rely on traditional TWFE (Roth et al., 2023).

7. Conclusion

Prescribed fire is the most potent weapon in the wildfire arsenal, and liability fear is its most-cited obstacle. Yet this paper finds no evidence that removing the liability obstacle through tort reform actually reduces wildfire. The result suggests that the wildfire crisis may require direct investment in prescribed fire capacity—more burn crews, more funding, more permissive smoke management rules—rather than legal reform alone. The prescribed fire liability trap is not that tort law prevents burning. It is that even when the law allows burning, the system is not built to burn.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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

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A. Data Appendix

Fire Program Analysis Fire-Occurrence Database (FPA FOD). The FPA FOD is compiled by the USDA Forest Service from 38 federal, state, and local fire reporting systems (Short, 2017). The 5th edition contains 1,880,465 georeferenced wildfire records spanning 1992–2015. Each record includes the fire discovery date, final fire size (acres), statistical cause (13 categories), reporting agency, land ownership classification, and geographic coordinates. I aggregate individual fire records to the state-year level, computing total fire count, total acres burned, count of large fires (> 100 acres), and cause- and ownership-specific subtotals.

Treatment classification. State prescribed fire liability regimes are classified using the `daLaw` dataset from the `erer` R package (Sun, 2006), which records each state's current liability standard as strict (0), uncertain (1), simple negligence (2), or gross negligence (3). I supplement this cross-sectional classification with reform dates from legislative histories, law review articles (Yoder, 2004; Yoder et al., 2008; Brenner and Franklin, 2017), and reports from the Coalition of Prescribed Fire Councils. The final dataset identifies 22 states that adopted reform between 1990 and 2021, of which 20 have reform dates within the FPA FOD sample period (1992–2015).

Sample construction. The analysis panel consists of 1,214 state-year observations ($\approx$52 states $\times$ 24 years minus gaps). States with territories included in the FPA FOD (e.g., Puerto Rico) are retained. States reforming before 1993 (Florida, Mississippi) are coded as never-treated since they lack pre-treatment data. States reforming after 2015 (California, Oregon) are excluded from the treatment group. Washington (2018) is included as a treated state but contributes no post-treatment observations in the current data.

B. Identification Appendix

Pre-trends. The Callaway and Sant'Anna (2021) estimator produces event-study coefficients that serve as a pre-trend test. Pre-treatment coefficients are generally small and statistically insignificant, though the singular covariance matrix in some cohort-time cells prevents computation of a formal joint pre-test Wald statistic. This is a known consequence of small group sizes in staggered settings with many cohorts.

Alternative control group. Using not-yet-treated states as the control group instead of never-treated states yields nearly identical estimates (ATT $= -0.116$ vs. -0.087 for log fires), indicating that the choice of control group does not drive the null result.

C. Robustness Appendix

Gross vs. simple negligence. A dose-response specification interacting the treatment indicator with a gross negligence indicator yields a negligence-standard coefficient of 0.019 (SE = 0.244) and a gross negligence increment of 0.045 (SE = 0.735). Neither is significant, and the gross negligence increment is imprecise due to few states (Georgia, Nevada, Pennsylvania) adopting this more protective standard within the sample period.

Level outcomes. TWFE regressions on fire count in levels yield a coefficient of 342 (SE = 347), and on total acres of 16,730 (SE = 39,037). Both are null.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log(1+fires)	-0.0866	0.2660	2.095	-0.0413	0.1270	Small negative
Log(1+acres)	-0.4349	0.2793	3.250	-0.1338	0.0859	Moderate negative
Log(1+large)	0.0324	0.1858	1.739	0.0186	0.1068	Small positive

Notes: **Country:** United States. **Research question:** Does shifting from strict liability to simple or gross negligence for prescribed burning reduce wildfire frequency and severity at the state level? **Policy mechanism:** State tort reform reduces the legal risk faced by private landowners and land managers who conduct prescribed burns—the most effective wildfire prevention tool—by replacing automatic liability for fire escape with a negligence standard requiring proof of carelessness or recklessness. **Outcome definition:** Log-transformed annual state-level wildfire count, total acres burned, and count of large fires exceeding 100 acres, from the USDA FPA FOD. **Treatment:** Binary indicator equal to one after a state enacts prescribed fire liability reform. **Data:** USDA FPA FOD (Short 2017), 1992–2015, state-year panel, 1,214 observations across 52 states. **Method:** Staggered DiD with Callaway–Sant'Anna (2021) estimator, doubly robust estimation, bootstrap inference (1,000 iterations). **Sample:** All 50 U.S. states plus DC over 24 years; treatment group comprises 20 states enacting reform between 1990 and 2009. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).