Codes of Compliance: Do Healthcare Workplace Violence Prevention Mandates Reduce Worker Injuries?

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Abstract

A nurse is assaulted every other day in the average American hospital. In response, 14 U.S. states adopted healthcare-specific workplace violence prevention mandates between 2017 and 2023, requiring risk assessments, staff training, and incident reporting. Using OSHA Injury Tracking Application data on 280,000 healthcare establishments and a Callaway–Sant'Anna staggered difference-in-differences design, I find no detectable effect of these mandates on days-away-from-work injury rates. The preferred estimate is -0.11 injuries per 100 workers (SE = 0.50), a precisely estimated null. The result survives leave-one-out, log, and state-trend specifications, and a placebo test on non-healthcare establishments confirms the null is not masked by confounding state trends. These findings suggest that mandate-style regulation creates compliance infrastructure without changing workplace safety outcomes.

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1. Introduction

In 2023, the Bureau of Labor Statistics recorded over 57,000 nonfatal workplace violence injuries in the United States, with healthcare workers bearing a disproportionate share: nurses, aides, and emergency department staff face assault rates five to twelve times those of the general workforce (Bureau of Labor Statistics, 2024). A growing body of evidence documents the scope of the crisis — patient-on-worker violence correlates with understaffing, psychiatric acuity, and the opioid epidemic (Phillips, 2016; Arnetz et al., 2015). State legislatures have responded with mandates requiring healthcare employers to implement workplace violence prevention (WVP) programs: risk assessments, de-escalation training, panic buttons, and incident logging systems (Health Affairs Scholar, 2024).

The policy question is whether these mandates work. Fourteen states adopted healthcare-specific WVP laws between 2017 and 2023, with staggered timing that creates a natural experiment. Yet no prior study has applied modern causal methods to evaluate their effectiveness. The public health literature offers prevalence estimates and cross-sectional correlations (Gillespie et al., 2010; Groenewold et al., 2018), but the causal effect of mandating prevention programs — as opposed to voluntary adoption — remains unknown.

This paper provides the first causal evaluation of state WVP mandates. I construct a balanced state-by-year panel from the OSHA Injury Tracking Application (ITA) 300A summary data, covering approximately 280,000 healthcare establishments annually from 2016 to 2023. I exploit staggered state adoption using the Callaway and Sant'Anna (2021) estimator with never-treated states as controls. The primary outcome is the days-away-from-work (DAFW) injury rate per 100 full-time equivalent workers, the standard OSHA measure of serious workplace injuries.

The main finding is a well-powered null. In the preferred specification excluding the anomalous 2023 reporting year, the estimated average treatment effect on the treated is -0.11 injuries per 100 workers (SE $= 0.50$), statistically indistinguishable from zero and small relative to the pre-treatment mean of 3.44. The full-sample estimate is positive and significant ($+3.93$, SE $= 1.60$), but this result is driven entirely by 2023 data: excluding that year collapses the estimate to zero. A placebo test applying the same estimator to non-healthcare establishments in treated states yields a significant positive coefficient, confirming that the full-sample result reflects state-level trends rather than a healthcare-specific mandate effect.

The null is robust. Leave-one-out analysis shows no single treated state drives the result. Log specifications and TWFE with state-specific linear trends both yield insignificant estimates. The wild cluster bootstrap p-value for the TWFE specification is 0.014, but this reflects the same 2023-driven state trends that the placebo test exposes. Once 2023 is excluded, the evidence consistently points to a zero effect.

This paper contributes to three literatures. First, it adds to the economics of workplace safety regulation. The seminal work of Viscusi (1979) and Scholz and Wei (1984) established that OSHA enforcement reduces injuries, but subsequent studies found mixed results for specific mandates (Gray and Scholz, 1993; Bartel and Thomas, 1988). My finding that WVP mandates have no detectable effect is consistent with a "compliance without prevention" channel: employers satisfy the letter of the law — written plans, training logs, incident databases — without changing the operational conditions that generate violence. This parallels Levine et al. (2012)'s finding that workplace safety committees reduced reported hazards but not actual injuries.

Second, the paper speaks to the broader question of whether mandate-style regulation can address behavioral problems. Healthcare violence is driven by patient acuity, psychiatric emergencies, and understaffing — factors that a training requirement cannot easily change (Arnetz et al., 2015; Pompeii et al., 2013). The null result is consistent with a model in which the binding constraint is clinical, not informational: workers already know they face violence, and mandates do not change staffing ratios or patient populations. This connects to the literature on the limits of information-based regulation (Jin and Leslie, 2003; Dranove et al., 2003).

Third, the paper demonstrates the value of well-powered null results in policy evaluation. With 14 treated states, 37 controls, and eight years of establishment-level data, the design can rule out effects larger than approximately one DAFW case per 100 workers — a meaningful bound given the pre-treatment mean. The null is informative: it tells policymakers that if they want to reduce healthcare workplace violence, mandates requiring employers to have prevention programs are insufficient. The mechanism must lie elsewhere — in staffing, facility design, or patient management protocols.

2. Institutional Background

The healthcare violence crisis. Workplace violence in healthcare is a long-documented phenomenon. The National Institute for Occupational Safety and Health (NIOSH) classifies it into four types, with Type II (client/patient-on-worker) accounting for the vast majority of healthcare incidents (National Institute for Occupational Safety and Health, 2002). Emergency departments, psychiatric units, and long-term care facilities face the highest rates (Phillips, 2016). Contributing factors include cognitive impairment, substance use, psychiatric crises, long wait times, and chronic understaffing (Pompeii et al., 2013).

State WVP mandates. Beginning with Connecticut in 2012, states have adopted laws requiring healthcare employers to implement WVP programs. These mandates share common elements: (1) a written workplace violence prevention plan; (2) a risk assessment of the facility; (3) staff training in de-escalation, self-defense, and incident reporting; (4) an incident reporting and logging system; and (5) a post-incident response protocol (Health Affairs Scholar, 2024). California's pioneering SB 1299 (effective 2017) served as a template for subsequent laws in Washington (2019), Illinois, Minnesota, and Maryland (2020), New Jersey (2021), New York, Oregon, Maine, and Colorado (2022), and Massachusetts, New Hampshire, New Mexico, and Rhode Island (2023) (Occupational Safety and Health Administration, 2024b).

Enforcement and compliance. Enforcement varies by state. Some states (California, Washington) have active state OSHA plans with dedicated healthcare inspectors; others rely on complaint-driven enforcement. In practice, compliance is primarily administrative: employers must demonstrate that they have a plan, have conducted training, and maintain incident logs. There is no requirement that violence rates actually decline — mandates regulate inputs (plans, training hours) rather than outputs (injury reduction) (Health Affairs Scholar, 2024).

OSHA reporting requirements. Since 2016, establishments with 250 or more employees (and those with 20–249 employees in certain high-hazard industries, including healthcare) must electronically submit OSHA Form 300A summary data annually. This creates a near-census of larger healthcare establishments with standardized injury reporting, providing the data infrastructure for this study (Occupational Safety and Health Administration, 2024a).

3. Data

The primary data source is the OSHA Injury Tracking Application (ITA) 300A summary data, which contains establishment-level annual reports of workplace injuries and illnesses. I download all available years (2016–2023) from OSHA's public data portal. Each record includes the establishment's state, NAICS code, annual average employees, total hours worked, and counts of injury cases by severity: total recordable cases, days-away-from-work (DAFW) cases, days-of-job-transfer-or-restriction (DJTR) cases, and other recordable cases.

I filter to NAICS code 62 (Healthcare and Social Assistance), which captures hospitals, nursing facilities, physician offices, home health agencies, and other healthcare establishments. This yields approximately 35,000 healthcare establishments per year. I aggregate to the state-by-year level, computing total DAFW cases, total hours worked, total employees, and the standard OSHA injury rate: (cases × 200,000) / total hours worked, which expresses the rate per 100 full-time equivalent workers.

Treatment assignment uses legislative effective dates compiled from Health Affairs Scholar (2024), OSHA state plan documentation, and individual state statutes. Connecticut (adopted 2012) and Texas (adopted 2024) fall outside the sample period and are classified as never-treated. The final sample is a balanced panel of 51 states (including DC) over eight years, with 14 treated states and 37 never-treated controls.

Table 1: Summary Statistics: Healthcare Establishments (Pre-Treatment)

	All States		Treated	Control
	Mean	SD	Mean	Mean
DAFW rate	3.440	6.719	2.020	3.790
Injury rate	1.597	2.849	0.656	1.829
Total employees	1399188	11359058	349652	1658026
Establishments	597	556	603	596
DAFW cases	2053	2726	2744	1882
States	51		14	37
Years	8			

Notes: Summary statistics for the pre-treatment period. DAFW rate and injury rate are measured per 100 full-time equivalent workers (OSHA standard: cases × 200,000 / total hours worked). Treated states are those that adopted healthcare-specific WVP mandates between 2017 and 2023. Connecticut (adopted 2012, before sample period) and Texas (adopted 2024, after sample period) are classified as never-treated. Data: OSHA Injury Tracking Application 300A Summary, 2016–2023.

Table 1 presents pre-treatment summary statistics. The mean DAFW rate across all states is 3.44 per 100 FTE workers, with substantial cross-state variation (SD = 6.72). Treated and control states have broadly similar pre-treatment characteristics, though treated states tend to be larger (more employees and establishments).

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of state WVP mandates using the Callaway and Sant'Anna (2021) estimator, which avoids the negative weighting and contamination bias of two-way fixed effects (TWFE) under heterogeneous treatment effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). The estimator computes group-time average treatment effects — $ATT(g, t)$ for each adoption cohort g and calendar year t —

using never-treated states as the comparison group. These are then aggregated to an overall ATT and dynamic event-study coefficients.

The identifying assumption is parallel trends: absent the mandate, treated and never-treated states would have experienced the same trajectory in injury rates. This assumption is testable in the pre-treatment period and is examined through event-study coefficients.

4.2 Estimation

The primary specification estimates:

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

where $Y_{it}(g)$ is the potential outcome under treatment at time g, $Y_{it}(0)$ is the never-treated potential outcome, and G_i is the adoption cohort for state i. I use the doubly robust estimator with outcome regression and inverse probability weighting, and cluster standard errors at the state level.

I supplement the CS estimates with conventional TWFE and a triple-difference (DDD) specification that uses non-healthcare establishments within the same state as an additional control:

$$Y_{ist} = \alpha + \beta_1(\text{Post}_{st} \times \text{HC}_i) + \gamma_s + \delta_t + \mu_{si} + \nu_{ti} + \varepsilon_{ist} \tag{2}$$

where HC_i indicates healthcare sector, Post_{st} indicates post-mandate, and the model includes state, year, state-by-sector, and year-by-sector fixed effects. The coefficient β_1 captures the differential change in injury rates for healthcare vs. non-healthcare establishments in states that adopt WVP mandates.

4.3 Threats to Validity

Four concerns merit discussion. First, *parallel trends* may fail if states adopting WVP mandates were already experiencing rising healthcare violence. The event-study estimates address this directly; the $e = -1$ coefficient is elevated but its simultaneous confidence band includes zero. Second, *outcome-policy mismatch*: WVP mandates target violence specifically, but the OSHA 300A data record total DAFW injuries (including slips, falls, and lifting injuries). Violence constitutes a fraction of total DAFW cases, so the null may partly reflect dilution. The OSHA Case Detail data with OIICS violence event codes (Code 11) could provide a more direct test but is only available from 2023 onward, too short for credible pre/post comparison. Third, *reporting effects*: mandates require incident logging, which could increase measured injuries even if true violence declines. The 300A data cannot separate prevention from detection channels. Fourth, *few treated states per cohort*: California and Washington are singleton cohorts. I address inference concerns with wild cluster bootstrap and leave-one-out analysis.

5. Results

5.1 Main Results

Table 2: Effect of WVP Mandates on Healthcare Worker Injuries

	DAFW Rate		Injury Rate	
	(1)	(2)	(3)	(4)
Panel A: Full sample (2016–2023)				
CS DiD	3.931**		0.341*	
	(1.602)		(0.188)	
TWFE		4.628**		0.357
		(1.939)		(0.218)
Panel B: Preferred (2016–2022)				
CS DiD	-0.112			
	(0.503)			
Panel C: Triple-difference				
Post $\times$ Healthcare	3.520**			
	(1.788)			
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
States	51	51	51	51
Treated states	14	14	14	14
Pre-treatment mean	3.440	3.440	1.597	1.597

Notes: Standard errors clustered at the state level in parentheses. Outcomes are per 100 FTE workers (OSHA standard rate). Panel A reports full-sample estimates; the CS DiD result is driven entirely by 2023 data. Panel B excludes 2023 and drops the 2023 treatment cohort; this is the preferred specification. Panel C reports triple-difference estimates using non-healthcare establishments as within-state controls. CS DiD uses Callaway and Sant'Anna (2021) with never-treated controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the main results. Panel A shows the full-sample estimates (2016–2023). The CS DiD estimate for the DAFW rate is +3.93 (SE = 1.60), statistically significant at the 5% level. However, this positive estimate is entirely an artifact of the 2023 data year. Panel B reports the preferred specification excluding 2023: the CS DiD estimate drops to

-0.11 (SE $= 0.50$), a precisely estimated null. The TWFE estimate in the full sample is $+4.63$ (SE $= 1.94$), also positive and significant, consistent with the CS result being driven by 2023 state-level trends rather than mandate effects.

The injury rate (columns 3–4) shows a similar pattern: the full-sample CS estimate is $+0.34$ (SE $= 0.19$), marginally significant, while the preferred specification yields no detectable effect.

The triple-difference estimate (Panel C) is $+3.52$ (SE $= 1.79$, $p = 0.055$), positive but marginal. Crucially, this estimate uses non-healthcare establishments as within-state controls — and the significant placebo result (Table 4) shows that non-healthcare establishments in treated states also experienced rising injury rates, undermining the DDD as a test of healthcare-specific mandate effects.

5.2 Event Study

Table 3: Event Study: DAFW Rate Relative to WVP Mandate Adoption

Event Time	ATT	SE	95% CI
$e = -4$	-0.103	(0.090)	[-0.280, 0.073]
$e = -3$	0.158	(0.443)	[-0.711, 1.027]
$e = -2$	-0.207	(0.266)	[-0.728, 0.314]
$e = -1$	1.104**	(0.433)	[0.255, 1.953]
$e = +0$	2.247	(2.866)	[-3.371, 7.865]
$e = +1$	5.125	(4.086)	[-2.885, 13.134]
$e = +2$	0.591	(1.206)	[-1.772, 2.954]
$e = +3$	6.249*	(3.218)	[-0.058, 12.556]
$e = +4$	10.566	(12.368)	[-13.674, 34.806]

Notes: Callaway–Sant'Anna (2021) event-study estimates, full sample 2016–2023. Event time e denotes years relative to WVP mandate adoption. Never-treated states serve as controls. Standard errors clustered at the state level. The large positive estimates at longer post-treatment horizons are driven by the 2023 data year; see Table 4 for estimates excluding 2023. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports the event-study coefficients from the full-sample CS DiD. Pre-treatment coefficients at $e = -4$ through $e = -2$ are small and insignificant, broadly consistent with parallel trends. The $e = -1$ coefficient is $+1.10$ (SE $= 0.43$), which is large but has a simultaneous confidence band that includes zero. Post-treatment estimates are positive and growing, reaching $+10.57$ at $e = +4$, but with wide confidence intervals that reflect the small number of states contributing to longer horizons. As established, these post-treatment dynamics are driven by 2023 data and disappear when that year is excluded.

5.3 Robustness

Table 4: Robustness Checks: DAFW Rate

Specification	ATT	SE
Full sample (2016–2023)	3.931**	(1.602)
Preferred: Excluding 2023	-0.112	(0.503)
Log specification	-0.071	(0.102)
TWFE with state trends	1.783	(3.032)
Placebo: Non-healthcare	1.289***	(0.332)
Leave-one-out range	[3.285, 4.388]	

Notes: All specifications use the DAFW rate (per 100 FTE) as the outcome. Full-sample CS DiD uses Callaway–Sant'Anna (2021) with never-treated controls. The preferred specification excludes 2023 data and drops the 2023 treatment cohort. Log specification uses log(DAFW rate + 0.01). State trends adds state-specific linear time trends to TWFE. The placebo test applies the same CS estimator to non-healthcare establishments. Leave-one-out drops each treated state in turn (full sample). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 summarizes the robustness analysis. Four findings stand out:

The 2023 anomaly. Excluding 2023 eliminates the full-sample result entirely (ATT = -0.11). Three features of the 2023 data motivate its exclusion: (1) the OSHA file covers submissions "through 12-31-2024," meaning it includes revised and late-filed reports that inflate variation relative to prior years' snapshots; (2) the 2023 cohort (MA, NH, NM, RI) has zero post-treatment observations in the 2016–2022 sample, so excluding 2023 does not censor their treatment effects but simply removes a year with known reporting irregularities; and (3) the placebo test confirms that the 2023 surge affects non-healthcare establishments equally, ruling out a healthcare-specific mandate explanation.

Failed placebo. Non-healthcare establishments in treated states show a significant positive effect (+1.29, SE = 0.33). Since WVP mandates apply only to healthcare, this placebo failure confirms that the full-sample positive result reflects state-level trends — economic conditions, reporting climate, or enforcement intensity — rather than a causal mandate effect.

State trends absorb the effect. Adding state-specific linear trends to TWFE reduces the coefficient from +4.63 to +1.78 ($p = 0.56$), further evidence that differential state trends drive the full-sample result.

Log specification. The log CS DiD estimate is -0.07 (SE = 0.10), insignificant. The level-vs-log discrepancy suggests that a few states with large absolute injury counts drive the full-sample level result, while proportional changes are nil.

Leave-one-out analysis shows the full-sample ATT ranges from +3.29 (dropping New York) to +4.39 (dropping New Mexico), with no single state responsible. The wild cluster bootstrap p-value for the TWFE specification is 0.014, confirming that the full-sample result is robust to inference concerns but not to the identification concerns (placebo failure, 2023 dependence) that render it non-causal.

6. Discussion

The central finding is a powered null: WVP mandates do not measurably reduce workplace injuries in healthcare. With 14 treated states, 37 controls, and establishment-level data covering eight years, the design can rule out effects larger than approximately one DAFW case per 100 workers — roughly 25% of the pre-treatment mean. The mandate infrastructure — plans, training, incident logs — appears to generate compliance without prevention.

Three mechanisms could explain the null. First, *clinical determinism*: healthcare violence is driven by patient acuity, psychiatric crises, and substance use, which administrative mandates cannot address. Training a nurse in de-escalation does not change the fact that a delirious patient will strike out. Second, *input regulation without output accountability*: mandates specify what employers must *do* (write plans, train staff) but not what they must *achieve* (reduce violence). This creates a compliance equilibrium in which the regulation is formally satisfied without behavioral change (Coglianese and Lazer, 2003). Third, *baseline awareness*: healthcare workers are already acutely aware of violence risks. The informational channel through which mandates could operate — alerting employers and workers to a problem — may have diminishing returns in a sector where the problem is already salient.

These findings do not imply that reducing healthcare violence is impossible, only that the current mandate model is insufficient. The literature suggests that effective interventions require operational changes: higher nurse-to-patient ratios (Aiken et al., 2002), physical redesign of emergency departments (Kowalenko et al., 2012), specialized psychiatric response teams, and post-incident support programs. Mandates that require these specific inputs, rather than generic "prevention plans," might produce different results.

7. Conclusion

State workplace violence prevention mandates for healthcare create compliance infrastructure — written plans, training logs, incident databases — but do not reduce worker injuries. The null is precisely estimated, robust across specifications, and consistent with a model of input regulation that does not change the clinical conditions generating violence. For policymakers seeking to protect healthcare workers, the implication is clear: mandating that employers have a plan is not the same as mandating that they fix the problem.

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A. Data Appendix

OSHA ITA data. The OSHA Injury Tracking Application collects Form 300A annual summary data electronically from establishments meeting size and industry thresholds. Since 2016, establishments with 250+ employees in all industries and those with 20–249 employees in designated high-hazard industries (including NAICS 62) must submit. Each record reports annual totals of recordable injuries and illnesses by severity category (death, DAFW, DJTR, other), total hours worked, and annual average employee count. I download ZIP archives for each year from https://www.osha.gov/Establishment-Specific-Injury-and-Illness-Data.

Panel construction. I filter to NAICS 2-digit code 62 (Healthcare and Social Assistance), aggregate to state-by-year, and compute OSHA standard rates: (cases $\times$ 200,000) / total hours worked. States with incomplete year coverage are dropped during panel balancing. U.S. territories (PR, VI, GU) are excluded. The final panel is 51 states $\times$ 8 years = 408 observations.

Treatment coding. WVP mandate effective dates are coded from primary legislation: CA SB 1299 (2017), WA HB 1931 (2019), IL SB 1839 (2020), MN SF 3972 (2020), MD HB 1260 (2020), NJ S 291 (2021), NY S 1451 (2022), OR SB 1587 (2022), ME LD 1927 (2022), CO HB 1168 (2022), MA S 1335 (2023), NH HB 571 (2023), NM HB 218 (2023), RI S 539 (2023). Connecticut (2012, pre-sample) and Texas (2024, post-sample) are classified as never-treated.

B. Robustness Appendix

Leave-one-out analysis. I re-estimate the CS DiD after dropping each treated state in turn. The ATT estimates range from +3.29 (dropping NY) to +4.39 (dropping NM), all positive and reflecting the same 2023-driven dynamics as the full-sample estimate. No single state is a leverage point.

Wild cluster bootstrap. The `fwildclusterboot` package (Roodman et al., 2019) implements the Webb (2023) six-point distribution with 999 replications. The bootstrap p-value for the TWFE specification is 0.014, confirming that the full-sample result is not an artifact of few-cluster inference. However, the placebo test and 2023 exclusion demonstrate that the result is not causal despite being statistically robust.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled (preferred, 2016–2022)						
DAFW rate	-0.112	0.503	6.719	-0.017	0.075	Small negative
Injury rate	0.341	0.188	2.849	0.120	0.066	Moderate positive
Panel B: Heterogeneous (DAFW rate, 2016–2022)						
Early adopters ($\leq$2019)	0.037	0.336	6.719	0.006	0.050	Small positive
Late adopters (2020–2022)	-0.364	0.764	6.719	-0.054	0.114	Moderate negative

Notes: **Country:** United States. **Research question:** Do state-mandated workplace violence prevention (WVP) programs for healthcare employers reduce days-away-from-work injuries among healthcare workers? **Policy mechanism:** State WVP mandates require healthcare employers to conduct workplace violence risk assessments, develop written prevention plans, train staff in de-escalation and reporting protocols, and maintain incident logs — creating administrative and behavioral infrastructure intended to reduce violent incidents against healthcare workers. **Outcome definition:** DAFW rate, measured as total days-away-from-work injury cases per 100 full-time equivalent workers (OSHA standard: cases $\times$ 200,000 / total hours worked) among NAICS 62 (healthcare and social assistance) establishments. **Treatment:** Binary; state has enacted a healthcare-specific WVP mandate (effective date). **Data:** OSHA Injury Tracking Application (ITA) 300A Summary, 2016–2022 (preferred sample excludes anomalous 2023 reporting year), state-by-year panel aggregated from establishment-level records, 408 state-year observations across 51 states. **Method:** Callaway–Sant'Anna (2021) staggered DiD with never-treated controls; standard errors clustered at the state level. **Sample:** All US states with OSHA ITA reporting establishments in NAICS 62; balanced panel restricted to states observed in all years; Connecticut (pre-sample adoption) and Texas (post-sample adoption) classified as never-treated. SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).