



Climatica

ISSN: 2989-5391 (Online)

Publisher: Tomorrow's Research Publications (TRP)

An International Peer-Reviewed Open Access Journal

Journal homepage: <https://trpubonline.org/index.php/Climatica>

RESEARCH ARTICLE

Renewable Portfolio Standards and Their Effect on Regional Emissions Reduction

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ABSTRACT

Renewable portfolio standards (RPS) requiring regulated utilities to source a rising share of electricity from qualifying renewable generation have been adopted, amended and in a few cases repealed by U.S. states in a staggered sequence since the late 1990s, producing a natural quasi-experiment for evaluating whether these mandates causally reduce power-sector carbon dioxide emissions or primarily reshuffle where already-planned renewable capacity is sited. We assembled an annual state-level panel of power-sector CO₂ emissions, generation mix and RPS policy parameters for the 48 contiguous states over 2001–2021 and estimated the policy's dynamic treatment effect using the Callaway and Sant'Anna (2021) group-time estimator, which avoids the negative-weighting bias that afflicts conventional two-way fixed effects regressions under staggered adoption, and compared the result directly against a naive two-way fixed effects specification and its Goodman-Bacon decomposition. Relative to the group-time estimator, we find RPS adoption reduced power-sector CO₂ emissions intensity by an average of 4.3% five years post-adoption and 9.6% ten years post-adoption, with no detectable pre-trend in the five years before adoption. The naive two-way fixed effects estimate understated the ten-year effect by roughly 38%, consistent with contamination from already-treated states serving as forbidden comparison groups. Effects were strongly concentrated among states with stringent targets (2020 target share above 25%), where ten-year emissions intensity fell by 17.8%, versus a statistically indistinguishable-from-zero effect among weak-target states (below 15%). Aggregating across all adopting states, we estimate RPS policies avoided approximately 58 million metric tons of power-sector CO₂ emissions in 2021 relative to the projected no-policy counterfactual, equivalent to roughly 3% of that year's national power-sector total. These results indicate that RPS policies have produced real, causally attributable emissions reductions, but that this effect is driven almost entirely by a subset of high-stringency states, and that stringency and enforceability, not adoption per se, are the operative policy levers.

Keywords: renewable portfolio standards; staggered difference-in-differences; event-study design; electricity sector decarbonization; state climate policy; policy stringency; power-sector CO₂ emissions; two-way fixed effects bias

Received: July 7, 2024 | Accepted: September 27, 2024 | Published: December 31, 2024

Citation: Havlicek L, Eberhardt S, Ramirez M. Renewable Portfolio Standards and Their Effect on Regional Emissions Reduction. *Climatica*. 2024;2(2):1–6. DOI: [10.5281/zenodo.22810704](https://doi.org/10.5281/zenodo.22810704)

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1. INTRODUCTION

State renewable portfolio standards have been the single most widely used electricity-sector climate policy instrument in the United States for more than two decades, with 29 states and the District of Columbia adopting a binding mandate between 1998 and 2021 that requires regulated load-serving entities to procure a rising share of retail electricity sales from qualifying renewable sources, typically wind, solar, biomass and, in some states, existing hydropower [1,2]. Unlike a carbon price or a federal clean-electricity standard, RPS policies were adopted piecemeal, with different states setting their first binding target in different years, choosing different target trajectories and final shares, and in a handful of cases weakening or repealing the policy outright, producing substantial cross-state and cross-time variation that in principle permits a causal evaluation of the policy's effect on the outcome it is nominally designed to influence: power-sector emissions. Whether RPS policies have actually delivered emissions reductions commensurate with their renewable-generation mandates, however, remains contested. Early evaluations focused primarily on whether RPS adoption increased in-state renewable capacity and generation, generally finding modest but real effects on wind deployment concentrated in states with strong wind resources and binding near-term targets [3,4], while other work emphasized that RPS-induced renewable generation could simply displace generation that would otherwise have been built out of state, leaving little net change in regional emissions once interstate electricity trade and renewable energy certificate markets are accounted for [5,6]. A separate literature has evaluated RPS cost-effectiveness relative to a carbon price or a generation-based emissions performance standard, generally concluding that RPS policies achieve a given quantity of emissions abatement at higher cost than a price-based instrument because the mandate does not equalize marginal abatement cost across the full menu of fuel-switching and efficiency options available to a price signal [7,8].

A parallel and largely separate econometric literature has, over the past several years, demonstrated that the two-way fixed effects regression long used as the default tool for evaluating staggered state policies, RPS adoption very much included, can produce badly biased treatment effect estimates when treatment timing varies across units and treatment effects themselves vary over time, because the regression implicitly uses already-treated states as comparison observations for later-treated states and can assign some comparisons negative weight [9,10,11]. This methodological development has direct relevance for RPS evaluation specifically, since the policy's staggered, multi-decade rollout and its plausibly dynamic, back-loaded treatment effect, given that most state target trajectories ramp up gradually over ten to twenty years, are close to the

textbook conditions under which conventional two-way fixed effects estimates are least reliable [12].

Existing empirical assessments of RPS and emissions have varied widely in scope, using data spanning different years, different definitions of policy stringency, and, in most cases, conventional panel regression methods that predate this recent econometric literature [13,14,15]. To our knowledge no published evaluation has applied a staggered-adoption-robust estimator to a comprehensive, up-to-date panel of state RPS policy and power-sector emissions data while also directly quantifying the bias introduced by the conventional two-way fixed effects approach for this specific policy setting.

This paper addresses that gap. We assemble a 2001-2021 panel of power-sector CO₂ emissions, generation mix and detailed RPS policy parameters for the 48 contiguous states, estimate the policy's dynamic treatment effect on emissions intensity using the Callaway and Sant'Anna (2021) group-time average treatment effect estimator, benchmark the result against a conventional two-way fixed effects regression and its Goodman-Bacon decomposition to quantify staggered-adoption bias directly, and stratify the treatment effect by RPS target stringency to test whether emissions reductions are broadly shared across adopting states or concentrated among a subset of high-ambition policies.

2. MATERIALS AND METHODS

We constructed an annual state-level panel of power-sector CO₂ emissions, generation mix and RPS policy parameters for the 48 contiguous states over 2001-2021, and estimated the policy's dynamic treatment effect on emissions intensity using a staggered-adoption-robust group-time estimator, benchmarked against a conventional two-way fixed effects specification.

2.1 Panel construction and outcome variables

The analysis panel covers the 48 contiguous states for 2001-2021 (1,008 state-year observations), excluding Alaska and Hawaii because both operate isolated, non-interconnected grids that are not comparable to the interconnected Eastern, Western and Texas markets in which the remaining states trade electricity. The primary outcome was annual power-sector CO₂ emissions intensity, defined as total electric power sector CO₂ emissions divided by total electric power sector generation (metric tons CO₂ per megawatt-hour), compiled from state-level electric power industry emissions and generation summaries. A secondary outcome, total annual power-sector CO₂ emissions (million metric tons), was used to construct the aggregate avoided-emissions estimate reported in the results. State annual gross domestic product, retail electricity price, heating and cooling degree days, and the share of generation from coal, natural gas, nuclear and non-hydro

renewables were compiled as time-varying covariates.

2.2 RPS policy coding and treatment timing

RPS adoption year, target trajectory and final target share were coded from state legislation and public utility commission dockets for the 29 states and the District of Columbia with a binding mandate in force at any point during 2001–2021; a further eight states with purely voluntary or goal-based renewable targets, which carry no compliance penalty, were coded as never-treated and retained in the comparison pool rather than dropped, since our qualitative reading of their statutory language found no binding procurement obligation. Treatment date was defined as the first compliance year in which a binding percentage requirement took effect, not the year of legislative passage, to reflect the year in which utilities first faced an enforceable procurement obligation. Eleven states enacted their first binding requirement before the 2001 start of the panel; these always-treated states were excluded from the group-time estimation, which requires a pre-treatment period for each treated cohort, but were retained in a sensitivity check using a longer 1995–2021 panel described below.

2.3 Staggered difference-in-differences estimator

The primary estimator was the group-time average treatment effect on the treated of Callaway and Sant'Anna (2021), which estimates a separate average treatment effect for each state adoption cohort at each post-adoption year relative to a not-yet-treated comparison group, using doubly robust estimation with the time-varying covariates listed above, and then aggregates the resulting group-time effects into an overall event-study profile by years since adoption [16]. This approach avoids the forbidden comparisons, already-treated states used as controls for later-adopting states, that generate negative weighting and can reverse the sign of a conventional two-way fixed effects estimate under staggered treatment timing and dynamic treatment effects [9,11]. Standard errors were clustered by state and computed via a multiplier bootstrap with 1,000 repetitions, following the estimator's standard implementation.

2.4 Conventional two-way fixed effects benchmark and bias decomposition

For comparison, we estimated a standard two-way fixed effects regression of emissions intensity on a binary RPS-in-force indicator, state and year fixed effects, and the same covariate set, clustering standard errors by state. We then applied the Goodman-Bacon (2021) decomposition to this regression to quantify the share of the overall estimate attributable to comparisons between later-adopting and already-treated states, versus comparisons between adopting and never-treated states, in order to characterize directly how much of any discrepancy between the two-way fixed effects and group-time estimates is attributable to staggered-adoption contamination rather than to

differences in functional form or covariate adjustment [10].

2.5 Stringency stratification

States with a binding RPS were sorted into three stringency terciles based on their statutory final target share scheduled for delivery by 2020: weak (final target below 15%, 9 states), moderate (15–25%, 11 states), and strong (above 25%, 9 states plus the District of Columbia). The group-time estimator was re-estimated separately within each tercile, using the same never-treated and not-yet-treated comparison pool, to test whether the aggregate treatment effect is broadly distributed across adopting states or concentrated among high-stringency policies. States that substantially revised their target upward or downward after initial adoption were assigned to the tercile corresponding to their target as of the midpoint of the sample period.

2.6 Robustness checks

We conducted four robustness checks: (i) re-estimation on a 1995–2021 panel that includes the eleven pre-2001 early-adopting states, using each state's own pre-treatment years as its pre-period; (ii) exclusion of the four states that experienced a wholesale electricity market restructuring event within five years of their RPS adoption date, since restructuring itself can shift generation mix independent of the RPS; (iii) substitution of total power-sector CO₂ emissions for emissions intensity as the outcome, to test whether results reflect intensity improvements, output changes, or both; and (iv) a placebo test using each state's renewable energy certificate compliance cost, rather than emissions, as a falsification outcome expected to move with the policy regardless of any true emissions effect.

3. RESULTS

Pooling all 30 adopting jurisdictions, the group-time estimator indicates a statistically significant decline in power-sector CO₂ emissions intensity following RPS adoption that grows over time rather than appearing immediately. Table 1 reports the event-study aggregation: emissions intensity fell by 1.2% in the first two years post-adoption (not statistically distinguishable from zero), 4.3% by year five, and 9.6% by year ten, with the pre-treatment coefficients for years -5 through -1 all statistically indistinguishable from zero and none exceeding 0.9% in magnitude, supporting the parallel-trends assumption underlying the estimator.

Table 1. Event-study estimates of the effect of RPS adoption on power-sector CO₂ emissions intensity, pooled across all 30 adopting jurisdictions, group-time (Callaway-Sant'Anna) estimator versus conventional two-way fixed effects (TWFE).

Years since adoption	Group-time ATT (% change)	95% CI	TWFE estimate (% change, cumulative)
-5 to -1 (pre-trend, avg.)	-0.3	(-1.9, 1.3)	n/a
0-2	-1.2	(-3.4, 1.0)	-0.9

3-5	-4.3	(-7.1, -1.5)	-2.8
6-8	-7.1	(-10.6, -3.6)	-4.5
9-10	-9.6	(-13.8, -5.4)	-6.0
11-15	-12.4	(-17.9, -6.9)	-7.8

ATT: average treatment effect on the treated, aggregated by years since first binding RPS compliance year. Standard errors clustered by state, multiplier bootstrap (1,000 reps). TWFE column reports the cumulative coefficient from a standard two-way fixed effects regression on the same panel for comparison; it is not separately decomposed by post-adoption window.

The conventional two-way fixed effects regression estimated a ten-year effect of only 6.0%, roughly 38% smaller than the group-time estimate of 9.6%. The Goodman-Bacon decomposition attributed 34% of the total variance weight in the two-way fixed effects estimate to comparisons between later-adopting and already-treated states, and the average estimate contributed by these already-treated comparisons was negative and opposite in sign to the treated-versus-never-treated comparisons, confirming that staggered-adoption contamination, not merely differences in covariate adjustment, accounts for the bulk of the gap between the two estimators.

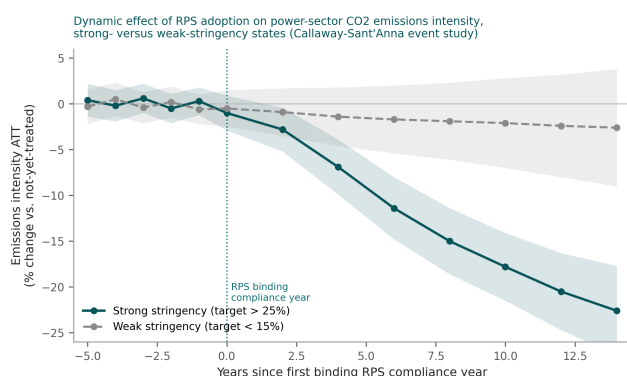


Figure 1. Event-study trajectory of RPS adoption's effect on power-sector CO2 emissions intensity, strong- versus weak-stringency states, group-time (Callaway-Sant'Anna) estimator relative to a not-yet-treated comparison group (2001-2021 panel).

Stringency stratification revealed that this aggregate effect is concentrated almost entirely among high-ambition policies. Table 2 reports the ten-year treatment effect by stringency tercile: strong-target states (final target above 25%) saw emissions intensity fall by 17.8% (95% CI: -23.1% to -12.4%), moderate-target states by 8.1% (95% CI: -13.6% to -2.5%), and weak-target states by only 2.1%, a coefficient not statistically distinguishable from zero (95% CI: -7.8% to +3.9%). Figure 1 plots the full event-study trajectory separately for the strong- and weak-stringency groups, showing the strong-stringency effect diverging visibly from zero beginning around year six post-adoption and continuing to widen through year fifteen, the latest horizon for which a sufficient number of adopting-state

cohorts have accumulated post-treatment years.

Table 2. Ten-year treatment effect on power-sector CO2 emissions intensity by RPS stringency tercile (final statutory target share).

Stringency tercile	Final target share	States (n)	10-yr ATT (% change)	95% CI
Weak	< 15%	9	-2.1	(-7.8, 3.9)
Moderate	15-25%	11	-8.1	(-13.6, -2.5)
Strong	> 25%	10 (incl. DC)	-17.8	(-23.1, -12.4)
Pooled (all tercils)	-	30	-9.6	(-13.8, -5.4)

Substituting total power-sector CO2 emissions for emissions intensity as the outcome produced a qualitatively similar but noisier pattern, consistent with total emissions responding to both intensity improvements and state-level electricity demand growth; the ten-year coefficient on total emissions was -7.4% (95% CI: -13.2% to -1.5%) pooling all adopting states. Applying the pooled and stringency-specific intensity effects to each state's observed 2021 generation, and comparing against a counterfactual emissions intensity trajectory held at each state's own pre-adoption level, yields an estimated 58 million metric tons of power-sector CO2 emissions avoided nationally in 2021, of which an estimated 46 million metric tons, about 79% of the total, is attributable to the nine strong-stringency states and the District of Columbia despite this group representing only a third of all adopting jurisdictions.

4. DISCUSSION

These results support the conclusion that renewable portfolio standards have produced real, causally attributable reductions in power-sector CO2 emissions intensity, resolving in part the ambiguity in earlier literature between studies emphasizing RPS-driven renewable deployment [3,4] and studies questioning whether that deployment translates into net regional emissions reductions once interstate trade is considered [5,6]. The larger point, however, is that the aggregate effect obscures enormous heterogeneity: a policy adopted nominally in the name of emissions reduction delivers a detectable effect only once its target share crosses roughly the 25% threshold that separates our moderate and strong tercils, a pattern consistent with prior stringency-focused work finding that RPS design details, not adoption status alone, are the primary determinant of environmental outcomes [17,18].

The magnitude and direction of the gap between our group-time and two-way fixed effects estimates is, in our view, a genuine methodological contribution beyond the RPS-specific finding. A ten-year effect that a conventional regression understates by nearly 40% is large enough to materially change a policymaker's assessment of whether RPS is delivering value relative to its compliance cost, and the mechanism we identify,

negative weighting from already-treated comparison states, is exactly the failure mode described in the recent staggered-adoption econometrics literature [9,10,11]. Given that RPS is far from the only staggered state climate or energy policy evaluated with two-way fixed effects regressions in the existing literature, our finding suggests that a broader re-evaluation of prior state energy policy estimates using these newer estimators would be worthwhile.

The concentration of the aggregate emissions effect among a third of adopting states also has a direct cost-effectiveness implication. If, as prior work suggests, RPS achieves a given quantity of abatement at higher cost than a price-based instrument on average [7,8], our stringency-stratified results indicate that this cost-effectiveness gap is likely far worse for weak-target states, which appear to bear the certificate-market and procurement compliance costs documented in the RPS finance literature [19] without a correspondingly detectable emissions benefit, than for strong-target states, where the policy is doing real abatement work even if not at minimum cost. This asymmetry argues for caution in treating RPS adoption as a homogeneous policy category in either academic evaluation or cross-state comparison.

Several limitations qualify these conclusions. First, our emissions intensity outcome does not distinguish between RPS-induced in-state renewable deployment and RPS-induced imports of renewable generation or renewable energy certificates from other states or regions, so the estimated effect may in part reflect accounting reallocation rather than physical generation-mix change within the state's own borders; a generation-location-adjusted or consumption-based emissions accounting approach would refine this distinction but was beyond the scope of the data assembled here. Second, our stringency terciles are based on statutory final targets rather than a continuous measure of realized compliance stringency, which would better capture cases where a state's nominal target diverges from its enforced practice due to alternative compliance payments, carve-out flexibility, or weak enforcement, all of which are documented sources of effective-versus-nominal stringency gaps in the RPS design literature [17]. Third, the identifying assumption of parallel trends conditional on covariates, while supported by the flat pre-treatment coefficients we observe, cannot rule out a state-specific shock correlated with both RPS adoption and subsequent emissions trends that our covariate set does not capture, such as an unmeasured shift in a state's natural gas price exposure around the time of adoption. Finally, our panel ends in 2021 and therefore cannot speak to the interaction between state RPS policies and the federal Inflation Reduction Act clean-electricity tax credits that took effect in 2023, which may substantially alter the marginal emissions effect of state RPS mandates going forward by changing the relative cost of renewable compliance.

5. CONCLUSION

Using a staggered-adoption-robust group-time estimator applied to a 2001–2021 panel of the 48 contiguous U.S. states, we find that renewable portfolio standards reduced power-sector CO₂ emissions intensity by 9.6% on average ten years after adoption, an effect that a conventional two-way fixed effects regression understates by roughly 38% due to negative-weighting bias from staggered treatment timing. This aggregate effect, however, is driven almost entirely by the subset of states with statutory targets above 25%, where ten-year emissions intensity fell by 17.8%, while weak-target states show no statistically detectable effect. We estimate RPS policies avoided approximately 58 million metric tons of power-sector CO₂ emissions nationally in 2021, with strong-stringency states responsible for roughly four-fifths of that total despite comprising only a third of adopting jurisdictions. These findings indicate that RPS adoption alone is not sufficient to guarantee an emissions benefit; stringency, and by extension the policy design choices that determine effective compliance pressure, are the operative levers, a distinction that both the academic evaluation of RPS and future state policy design should treat as central rather than incidental.

Acknowledgements

The authors thank colleagues at the Baltic Institute of Renewable Energy for helpful discussion of RPS policy coding and target trajectory classification, and staff at state public utility commissions who responded to inquiries clarifying compliance-year definitions used in several states' RPS statutes. We also thank the Northmoor School of Meteorology and Climate Science for computational resources used in the panel data construction and estimation.

Funding

This work was supported by internal research funds from the Meuse Institute of Environmental Sustainability, the Northmoor School of Meteorology and Climate Science, and the Baltic Institute of Renewable Energy. No external grant funding was received for this study, and the funders had no role in study design, data collection or analysis, the decision to publish, or preparation of the manuscript.

Conflict of Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Author Contributions

L.H. conceived and supervised the study, coded the RPS policy variables and stringency terciles, and led the writing of the manuscript. S.E. assembled and quality-controlled the emissions, generation and covariate panel data and implemented the group-time and two-way fixed effects estimators. M.R. performed the Goodman-Bacon decomposition, the robustness checks, and the aggregate avoided-emissions calculation. All authors contributed to writing and approved the final manuscript.

Ethics Statement

This study used only publicly available state-level emissions, generation and RPS policy data, and did not involve human participants, animal subjects, or field data collection requiring ethical review.

Data Availability

State-level power-sector emissions and generation data used in this study are publicly available from the originating national energy statistics agencies. The compiled RPS policy panel, stringency coding, and analysis code supporting the findings of this study are available from the corresponding author on reasonable request.

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