

Solar Renewable Energy Certificates and the Electricity Generation Mix: The Role of Initial Policy Stringency

Lulin Li, Shangyue Jiang, & Ling Huang

Abstract

Solar renewable energy certificate (SREC) programs provide dedicated incentives for solar generation. We use annual utility-level data from 2001–2023 and stacked event-study and difference-in-differences models to examine how SREC adoption changes the electricity generation mix. Our difference-in-differences estimates indicate a 19.2 percentage point increase in solar generation share and a 12.2 percentage point decline in non-clean generation share. Solar-share gains develop gradually and are concentrated in states with middle and high initial requirements, with estimated increases of 22.0 and 31.2 percentage points, respectively. Both groups exhibit similar declines in non-clean generation share, while the high-requirement group shows a larger decline in non-solar clean share in later years. Evidence of this latter pattern is less consistent across specifications. These findings connect initial policy requirements to the timing and composition of generation responses.

Keywords: Solar renewable energy certificates; renewable portfolio standards; electricity generation mix; policy stringency; stacked difference-in-differences.

JEL Codes: Q42 Q48 Q58 L94

1 Introduction

Since the early 2000s, state renewable energy mandates have become an important component of efforts to change the composition of electricity generation in the United States. These policies have developed alongside substantial growth in renewable generation, but the transition has been uneven across technologies. In 2023, solar accounted for approximately 3.9 percent of utility-scale electricity generation, compared with 10.2 percent for wind, while fossil fuels continued to supply approximately 60 percent of utility-scale generation (U.S. Energy Information Administration (2024)). Renewable portfolio standards seek to accelerate this transition by requiring electricity suppliers to obtain a specified amount of electricity from eligible renewable resources. However, a general renewable requirement need not provide comparable incentives for every eligible technology. When suppliers can satisfy an obligation using several resources, investment may concentrate in the least costly alternatives. Consequently, understanding the effectiveness of renewable mandates requires attention to both the amount of renewable generation they encourage and the technologies that expand or contract as the electricity mix changes (Wiser, Barbose, and Holt (2011)).

Several states have addressed this concern by establishing solar-specific requirements supported by markets for solar renewable energy certificates, commonly referred to as SRECs. An SREC generally represents the renewable attributes associated with one megawatt-hour of eligible solar generation. Electricity suppliers can meet their solar obligations by obtaining qualifying certificates, while certificate revenue provides an additional return to solar producers. These programs therefore combine a technology-specific requirement with flexibility in how suppliers comply. Nevertheless, their incentives depend on the requirement imposed and the rules governing the market. States differ in their initial targets, eligible sources, geographic trading restrictions, banking provisions, and alternatives to certificate surrender (Bird, Heeter, and Kreycik (2011)). Moreover, fixed compliance requirements can interact with uncertain supply to produce substantial variation in certificate prices, affecting the predictability of investment returns (Felder and Loxley (2012)). The existence of an SREC program therefore provides an incomplete description of the incentive facing producers. The requirement in place when a

state adopts the policy may help explain why otherwise similar policy adoptions are followed by different generation responses. A larger initial requirement could increase demand for eligible solar attributes and strengthen incentives to enter the market. Its effect on generation nevertheless depends on the availability of eligible supply and the opportunities to obtain certificates from other producers. This creates a distinction between the stated ambition of a policy and the adjustment it induces in the generation mix.

The purpose of this paper is to examine how SREC adoption changes the composition of utility generation and whether these changes differ with the initial policy requirement. We focus on three related outcomes: the share of generation supplied by solar, the share supplied by non-solar clean sources, and the share supplied by the remaining generation sources. This distinction allows us to examine whether increases in solar’s share are accompanied primarily by declines in non-solar clean generation or by declines elsewhere in the portfolio. Our principal contribution is to connect these patterns of generation reallocation to differences in the solar requirement at adoption. Rather than treating all adopting states as receiving an identical policy, we examine whether the size and timing of their responses vary with the initial requirement. This approach distinguishes the question of whether adoption is followed by solar expansion from the question of how the composition of that expansion differs across policy settings. In particular, a stronger initial requirement may be associated with more solar generation without producing an equally large reduction in the share supplied by the remaining sources. Examining solar alone would leave this distinction unresolved.

We use annual utility-level electricity data from the U.S. Energy Information Administration covering 2001–2023, which we combine with state solar requirements collected from government sources and measures of economic and weather conditions. The controlled sample includes 48 states, of which eight adopt SREC policies under our treatment definition between 2005 and 2010. We aggregate generation across plants associated with each utility and calculate the three generation shares using a common denominator. Non-solar clean generation includes hydroelectricity, wind, and waste. Following the classification used in the analysis, the remaining category, labeled non-clean generation, includes coal, natural gas, and oil. The data describe

electricity produced by the observed utilities; they do not measure all distributed solar installations, electricity consumption, or certificate purchases. Our analysis consequently concerns the composition of utility production rather than the full set of activities through which suppliers can meet their compliance obligations.

To examine the effects of SREC adoption, we estimate stacked event-study and difference-in-differences models that exploit variation in adoption timing across states. For each adoption cohort, we compare utilities in adopting states with utilities in states that have not adopted under our policy definition, including later-adopting states before their own treatment begins. Already-treated states do not serve as comparisons for subsequent cohorts. The models include state-by-cohort and year-by-cohort fixed effects, together with controls for population, real GDP per capita, solar irradiance, and heating and cooling degree days. This approach responds to concerns about comparisons across differently treated groups in settings with staggered adoption (Sun and Abraham (2021); Callaway and Sant’Anna (2021); Wing, Freedman, and Hollingsworth (2024a)). Within each adoption cohort, the design compares changes in generation shares between adopting and comparison states, conditional on economic and weather controls and cohort-specific state and year fixed effects. The estimates capture adjustments within existing utilities and changes in the composition of utilities observed in each state. We extend this framework to examine how adoption responses vary with the initial solar requirement. We measure each jurisdiction’s requirement in its adoption year and use this measure to assign adopting jurisdictions to low-, middle-, and high-requirement groups. Holding these assignments fixed, we estimate separate static and dynamic responses for each group. This approach allows us to examine how the magnitude, timing, and composition of estimated generation responses differ across initial policy settings.

Our baseline controlled difference-in-differences models estimate a 19.2 percentage point increase in solar generation share and a 12.2 percentage point decline in non-clean generation share following SREC adoption, relative to changes among comparison utilities. The estimated decline in non-solar clean share is 7.0 percentage points, although this estimate is not statistically distinguishable from zero at conventional levels. The event-study estimates indicate that

the solar response develops over time. Relative to the adoption-year treated–comparison contrast, the estimated increase in solar share is 15.9 percentage points four years after adoption and 19.7 percentage points in the category pooling five or more years after adoption. The corresponding declines in non-clean share are 12.6 and 12.1 percentage points. Because the adoption year is the omitted period, these dynamic estimates describe subsequent changes relative to that year and do not separately identify an immediate adoption-year effect. Likewise, the final event-time category combines multiple post-adoption years rather than representing an effect exactly five years after adoption.

The results differ across initial-requirement groups. In the controlled static models, the estimated solar-share increase is 22.0 percentage points in the middle group and 31.2 percentage points in the high group, while the low group has a small, statistically insignificant negative estimate. The middle and high groups also exhibit estimated declines in non-clean share of 17.4 and 18.1 percentage points, respectively. Thus, the ordering of the solar point estimates does not translate into a similarly large difference in the decline of non-clean generation. In the longer-run event-study category, the high group combines a 34.9 percentage point increase in solar share with declines of 18.0 percentage points in non-solar clean share and 16.9 percentage points in non-clean share, relative to the adoption-year contrast. These estimates illustrate the value of examining the full composition of generation: solar expansion can accompany changes between solar and other clean sources as well as between clean and remaining sources. Across the group-specific point estimates, the larger solar-share increase in the high-requirement group is accompanied by a larger decline in non-solar clean share, while the declines in non-clean share are similar in magnitude.

Allowing for differential pre-adoption trends and changes in those trends after adoption preserves the positive solar and negative non-clean level estimates, while changing their magnitudes and interpretation. In the aggregate trend specification, the estimated adoption-year level changes are a 10.8 percentage point increase in solar share and a 10.7 percentage point decline in non-clean share. These coefficients measure departures from an extrapolated pre-adoption trend at adoption, rather than the average post-adoption contrasts reported in the baseline

model. The middle and high groups retain positive solar and negative non-clean level estimates in the trend specifications. Taken together, the findings suggest that initial policy requirements are useful for understanding differences in generation responses, while the evidence for substitution away from non-solar clean sources is less uniform. Examining the three generation shares together characterizes two dimensions of portfolio adjustment: shifts between clean and non-clean generation and shifts between solar and other clean sources.

Our paper makes several contributions to the literature on renewable energy policy. First, we contribute to research examining how policy stringency shapes the effectiveness of renewable mandates. Yin and Powers (2010) show that accounting for differences in RPS requirements and design changes conclusions about their relationship with in-state renewable development. Carley et al. (2018) likewise examine the importance of policy stringency and institutional design. These studies establish that an adoption indicator alone can obscure meaningful differences among policies. We build on this insight by focusing on the requirement at the introduction of a solar-specific certificate program and tracing differences in the subsequent composition of utility generation. Initial-requirement groups allow us to examine whether heterogeneous responses are visible across broad policy categories. The contribution is evidence on how initial policy conditions relate to the path and composition of adjustment. This focus also connects the analysis of policy design to an outcome that includes competing generation sources, making the interpretation of solar expansion more informative.

Second, we contribute to the literature on SREC markets and solar-specific renewable policy. Earlier work documents the institutional development of these markets and the importance of implementation, including an analysis of New Jersey by Hart (2010) and a study of North Carolina by Gaul and Carley (2012). Subsequent research examines more specific market mechanisms. Coulon, Khazaei, and Powell (2015a) model interactions among certificate prices, investment, and supply, while Cohen, Elbakidze, and Jackson (2022) find that restrictions on outside SREC supply can increase in-state certificate prices and solar installations. More recently, Abito et al. (2025) show that relaxing geographic trading restrictions and interim targets can substantially reduce the modeled cost of achieving solar requirements. These

papers demonstrate that market rules matter for investment incentives and compliance costs. We complement this work by examining realized utility generation shares following adoption and the heterogeneity associated with the initial requirement. This extends attention from the operation of certificate markets and solar installation decisions to the generation portfolio in which solar expansion occurs.

Third, our paper is related to the broader literature evaluating market-based energy policies and the generation they displace. Evidence on general renewable portfolio standards remains mixed. Upton and Snyder (2017), using synthetic controls, find no relative increase in renewable generation, whereas Deschenes, Malloy, and McDonald (2023) find delayed increases in wind capacity and generation but no statistically significant solar response. These findings motivate distinguishing solar-specific requirements from broader mandates that can be satisfied with competing renewable technologies. Moreover, electricity-sector analyses show that the environmental and economic consequences of renewable support depend on substitution across generation sources and on interactions with electricity demand (Palmer and Burtraw (2005); Fischer and Newell (2008)). We contribute by separating solar, non-solar clean, and remaining generation shares within a common empirical framework. Interstate compliance trade also means that policy effects can extend beyond adopting states, as Hollingsworth and Rudik (2019) demonstrate for RPS policies. Our analysis complements this literature by examining changes in utility generation portfolios following SREC adoption and how these responses vary across initial policy settings.

Finally, we contribute to the literature distinguishing environmental certificates from the physical outcomes they are intended to encourage. Gillenwater (2008) explains why renewable electricity attributes should not be equated with emissions offsets. Gillenwater, Lu, and Fischlein (2014) examine investment additionality in voluntary green-power markets, while Langer et al. (2024) review evidence on the emissions consequences of voluntary renewable-certificate purchases. Although these voluntary markets differ from binding SREC requirements, they raise a common question about whether certificate incentives alter physical production decisions. Our analysis addresses that question through observed generation composition and dif-

ferences across initial mandatory requirements. By examining solar alongside non-solar clean and non-clean generation shares, we characterize the composition of the production response to certificate-based policy. This connects the literature on certificate incentives to observed changes in utility generation portfolios and the sources whose shares decline as solar’s share expands.

The paper proceeds as follows. Section 2 describes the institutional background of SREC programs and variation in their requirements. Section 3 presents the data and construction of the generation outcomes and initial policy measure. Section 4 outlines the empirical framework. Section 5 presents the adoption, initial-requirement, and sensitivity results. Section 6 concludes with a discussion of their implications for the design and evaluation of renewable energy policy.

2 Background

Renewable portfolio standards (RPS) establish demand for renewable electricity, typically by requiring covered electricity suppliers to meet a specified share of their sales with eligible resources. Renewable energy certificates (RECs) provide the accounting and trading mechanism through which many states implement these obligations. A REC represents the attributes of one megawatt-hour of renewable electricity generation. Suppliers can obtain certificates from their own eligible facilities or purchase them from other producers, allowing compliance procurement to extend beyond their generating portfolios. Certificates are recorded in tracking systems and retired when used, preventing their subsequent sale or reuse. REC markets also serve voluntary purchasers, while RPS compliance markets derive demand from state requirements (U.S. Energy Information Administration (2022); U.S. Environmental Protection Agency (2025)).

A broad renewable requirement allows eligible technologies to compete for compliance demand. When their costs differ, investment tends to favor technologies that can supply qualifying electricity at lower cost. Solar carve-outs reserve part of that demand for solar generation, providing support for a technology that historically faced higher costs than several competing renewable resources. Solar renewable energy certificates (SRECs) implement this requirement by identifying the attributes of qualifying solar production. Only certificates meeting the solar

program’s eligibility rules can satisfy its obligation. This creates a distinct source of revenue for eligible solar projects and changes their returns relative to other generation technologies (Wiser, Barbose, and Holt (2011)).

The SREC markets examined in this paper developed during the expansion of state solar requirements in the 2000s. New Jersey’s first solar compliance year was 2005, and the state increased its reliance on SREC markets as it moved away from rebates in 2007. Solar requirements also began in Pennsylvania in 2005, Maryland in 2008, and Massachusetts in 2010. These programs developed distinct arrangements for eligibility, compliance, and project finance (Bird, Heeter, and Kreycik (2011)).

The size of the solar requirement determines the volume of compliance demand. Under a sales-based standard, the required number of certificates depends on both the percentage target and covered electricity sales. Holding sales and eligibility rules fixed, a larger initial requirement creates greater demand for qualifying solar output. When existing supply is insufficient, this increases the incentive to develop new capacity. The initial requirement therefore matters through the demand it creates relative to the eligible supply already available. Subsequent target increases, anticipated supply growth, and the durability of the program also enter developers’ expectations about future certificate revenue (Coulon, Khazaei, and Powell (2015a)).

Geographic eligibility determines where that investment incentive operates. Allowing certificates from outside the adopting state expands the available supply and exposes local projects to competition from other locations. Restricting eligibility concentrates compliance demand among qualifying local facilities. Pennsylvania illustrates how these boundaries can change: Act 40 of 2017 introduced geographic connection requirements for solar eligibility while preserving protections for certain pre-existing contracts (Pennsylvania General Assembly (2017)). Consistent with this mechanism, Cohen, Elbakidze, and Jackson (2022) find that closing SREC markets to outside suppliers increased in-state certificate prices and solar installations. Thus, the geographic scope of eligible supply helps determine how much of the response to a state’s requirement occurs within that state.

Compliance provisions shape certificate prices and the timing of adjustment. Many programs allow suppliers to make a solar alternative compliance payment for a certificate shortfall. Where this option is available, its cost generally provides an upper bound on the price suppliers will pay for SRECs. Prices can fall substantially when eligible generation exceeds compliance demand (Bird, Heeter, and Kreycik (2011)). Banking provisions permit eligible unused certificates to meet future obligations, linking demand across compliance years. The resulting price path reflects current supply, accumulated certificates, and expectations about future requirements and investment (Coulon, Khazaei, and Powell (2015b)).

Because solar projects require substantial expenditure before they generate certificate revenue, the predictability of that revenue affects financing. Long-term contracts can provide developers with a more stable income stream, and several states introduced contracting or price-support arrangements (Bird, Heeter, and Kreycik (2011)). Massachusetts paired its solar carve-out with a clearinghouse auction for eligible unsold SRECs. Under the original design, the auction price was \$300 per certificate, with sellers receiving \$285 after administrative charges. The program also adjusted its annual requirement through a formula intended to balance supply and demand (Massachusetts Department of Energy Resources (2014)).

SREC revenue operates alongside other components of project returns. Electricity sales or savings from on-site generation provide value for the power produced, while federal tax incentives, state incentives, and net-metering arrangements influence investment costs and the value of output. These policies affect the amount of certificate revenue needed to make a solar project financially attractive. Consequently, a given solar requirement can generate different investment responses across markets with different production costs and complementary incentives (U.S. Energy Information Administration (2022)).

These institutions create two potential margins of adjustment in generation portfolios. Solar investment can expand renewable production relative to other sources. Where the solar requirement is nested within a broader renewable standard, as in Massachusetts, solar also

contributes toward the overall renewable obligation (Massachusetts Department of Energy Resources (2014)). This structure implies that a larger solar contribution can reduce the amount of other eligible renewable generation needed to meet a given overall target. Solar expansion can therefore accompany both a shift toward renewable generation and a change in its composition. These mechanisms motivate our examination of solar, non-solar clean, and remaining generation shares, together with differences associated with the requirement in place when the policy begins.

3 Data

We construct an annual utility-level panel that combines U.S. Energy Information Administration (EIA) electricity data with state SREC requirements and measures of economic and weather conditions over 2001-2023. The electricity records identify generators, their fuel classifications, generation, capacity, and associated plants and utilities. Generation is aggregated by fuel within each plant-year and then across plants associated with a utility. This produces annual utility portfolios while allowing the composition of generating plants to change over time.

The principal outcomes are the shares of electricity generation from solar and other clean sources. Let G_{it}^f denote generation by utility i in year t from fuel f , and let G_{it} denote total generation from coal, natural gas, hydroelectricity, oil, solar, waste, and wind. Generation reported in the residual “other” category is excluded from the denominator. The solar generation share is defined as $G_{it}^{\text{solar}}/G_{it}$, while the other clean generation share is defined as $(G_{it}^{\text{hydro}} + G_{it}^{\text{waste}} + G_{it}^{\text{wind}})/G_{it}$. To examine corresponding changes in the remainder of the generation portfolio, we also analyze the non-clean generation share, defined as $(G_{it}^{\text{coal}} + G_{it}^{\text{natural gas}} + G_{it}^{\text{oil}})/G_{it}$.

Annual policy requirements are collected from each state’s government website and matched to the utility panel by state and calendar year. The policy assignment uses the state recorded for each utility-year; utilities assigned to the same state receive the same annual policy value. Let A_s denote the first treatment year assigned to state s . The treatment definition includes New Jersey and Pennsylvania beginning in 2005, Delaware and the District of Columbia in 2007,

Maryland in 2008, Ohio in 2009, and Massachusetts, North Carolina, and New Hampshire in 2010.

We measure initial policy stringency as $D_s = 100 \times r_{s,A_s}$, where A_s denotes state s 's adoption year and r_{s,A_s} is its solar requirement in that year, expressed as a fraction. We rank treated states by D_s and assign them to low-, middle-, and high-requirement groups. This grouping captures differences in initial obligations that adoption timing alone does not distinguish, since states adopting in the same year may begin with different requirements. We hold these assignments fixed throughout the sample period. These fixed group assignments allow us to trace adoption responses across jurisdictions that entered the policy with different initial requirements.

We combine the electricity and policy panel with population estimates from the U.S. Census Bureau and real gross domestic product from the Bureau of Economic Analysis (BEA). Population is measured using July 1 estimates, combining the 2000–2010 intercensal series, the 2010–2020 intercensal series, and Vintage 2024 estimates for 2020–2023. Real GDP, expressed in chained 2017 dollars, is divided by the corresponding annual population to obtain real GDP per capita. Both population and real GDP per capita enter the regressions in natural logarithms. These variables account for changes in state population and economic conditions that may influence electricity demand and investment, consistent with the inclusion of demographic and economic characteristics in related renewable-energy policy studies (Zhou, Solomon, and Brown (2024); Dilek, Teixidó, and Serrano (2026)).

Weather controls are obtained from the National Oceanic and Atmospheric Administration (NOAA) and NASA's Prediction Of Worldwide Energy Resources (POWER) project. Annual heating and cooling degree days are constructed by summing NOAA's monthly state series, using a base temperature of 65 degrees Fahrenheit (Vose et al. (2014)). An annual value is retained only when all twelve monthly observations are available. These measures account for temperature-related demand and dispatch changes that can affect both solar generation and the denominator of the generation shares. Their inclusion is motivated by evidence on weather-

sensitive energy demand and their use in renewable-energy policy evaluations (Auffhammer and Mansur (2014); Dilek, Teixidó, and Serrano (2026)).

The solar-resource control uses NASA POWER’s monthly all-sky surface shortwave radiation data. Irradiance is a direct determinant of photovoltaic electricity production. Monthly daily-average radiation is aggregated to an annual daily average using calendar-day weights, including leap years. One-degree grid cells are combined into state averages using fixed geographic weights calculated from their intersections with Census state boundaries, with a latitude adjustment for cell area. The resulting measure is expressed in kilowatt-hours per square meter per day. We subtract each state’s 2001–2004 mean to construct an irradiance anomaly. Fixed geographic weights ensure that changes in this control reflect radiation conditions rather than the location of new solar capacity¹.

The sample excludes observations without utility identifiers, utility-years with nonpositive generation in the outcome denominator, and observations missing any of the five controls. The generation portfolio construction also excludes generator records with reported nonpositive generation. Missing degree-day values are not imputed; their absence excludes Alaska, Hawaii, and the District of Columbia from the controlled sample. The main estimation sample therefore covers the 48 contiguous states, including eight treated states, from 2001 through 2023. It contains 70,864 distinct utility-year observations from 7,703 utilities. Appending the five cohort-specific samples described below produces 305,612 utility-year-cohort observations. Table 1 reports summary statistics for the primary outcomes (top panel) and control variables (bottom panel). Columns 1 and 2 present statistics for utilities subject to and not subject to SREC policies, respectively. Columns 3–5 report the same statistics for utilities grouped by three levels of initial policy stringency. States with SREC policies tend to have higher solar irradiance, more heating degree days, and fewer cooling degree days than states without these policies.

¹Subtracting a fixed state mean is a normalization; with state-by-cohort fixed effects, it does not create additional identifying variation.

4 Methods

We estimate stacked event-study models to characterize changes in electricity-generation portfolios around SREC adoption and examine whether these responses differ across groups defined by initial policy stringency. We then estimate static stacked difference-in-differences models as robustness checks. The preferred specifications include the five state-year controls described above; corresponding specifications without these controls provide additional robustness checks.

For each adoption cohort $c \in \{2005, 2007, 2008, 2009, 2010\}$, we construct a separate sample. The treated group consists of utilities in states that first adopted an SREC policy in year c . The comparison group includes utilities in never-treated states and utilities in later-treated states observed before their own adoption. States treated before year c are excluded, and later-treated states leave the comparison group when their treatment begins. We append the five cohort-specific samples, applying the same control-data restrictions to each. For example, the 2005 stack includes utilities in New Jersey and Pennsylvania, utilities in never-treated states, and pretreatment observations from states adopting after 2005. The 2010 stack includes utilities in the 2010 adoption cohort and utilities in never-treated states.

Let A_s denote the adoption year in state s , and let $H_{sc} = \mathbf{1}\{A_s = c\}$ indicate membership in cohort c 's treated group. Event time is $k = t - c$. For $j = -4, \dots, 4$, define $E_{ct}^j = \mathbf{1}\{t - c = j\}$, with endpoint indicators $E_{ct}^{-5} = \mathbf{1}\{t - c \leq -5\}$ and $E_{ct}^5 = \mathbf{1}\{t - c \geq 5\}$. The treatment-by-event-time indicators are $T_{sct}^j = H_{sc}E_{ct}^j$. The baseline event-study model is

$$y_{isct} = \sum_{\substack{j=-5 \\ j \neq 0}}^5 \gamma_j T_{sct}^j + \mathbf{X}_{st}'\beta + \delta_{sc} + \lambda_{tc} + \varepsilon_{isct}. \quad (1)$$

Here, y_{isct} is one generation-share outcomes for utility i , state s , cohort c , and year t , and $\mathbf{X}_{st}$ contains the five state-year controls. The state index identifies the jurisdiction assigned to each utility-year observation. State-by-cohort fixed effects, δ_{sc} , absorb persistent differ-

ences across states within each stack, while calendar-year-by-cohort fixed effects, λ_{tc} , absorb shocks common to treated and comparison observations in that stack. The estimates summarize changes in generation shares among the utilities observed in each state-year, reflecting both within-utility adjustments and changes in the composition of observed utilities (Borusyak, Jaravel, and Spiess (2024); Sant’Anna and Xu (2026)).

The adoption year, $j = 0$, is the omitted category. Each coefficient γ_j measures the change in the treated-comparison outcome difference at event time j relative to the adoption-year difference. Observations five or more years before or after adoption are pooled into the respective endpoint coefficients.

Identification rests on parallel untreated trends conditional on the included controls and fixed effects, no anticipation, and no treatment spillovers to comparison observations. We report pre-adoption coefficients and examine sensitivity to specifications allowing differential linear trends before and after adoption. We estimate the models by pooled least squares without additional cohort or stack weights. The pooled coefficients therefore reflect the observations and residual variation available in the stacked sample (Wing, Freedman, and Hollingsworth (2024b)). Standard errors are three-way clustered by state-by-cohort, calendar-year-by-cohort, and utility. These dimensions allow for common disturbances within state-cohort and year-cohort cells and dependence among observations of the same utility across years and stacks.

To examine heterogeneity by initial policy stringency, we rank treated jurisdictions by D_s , the SREC requirement in the adoption year, and divide them into three equal-sized groups before applying the control-data restriction. The low-intensity group consists of Pennsylvania, Ohio, and the District of Columbia; the middle-intensity group consists of Maryland, New Jersey, and Delaware; and the high-intensity group consists of North Carolina, New Hampshire, and Massachusetts. Let B_s^q indicate membership in group $q \in \{1, 2, 3\}$. The group-specific event-study model is

$$y_{isct} = \sum_{q=1}^3 \sum_{\substack{j=-5 \\ j \neq 0}}^5 \gamma_{jq} T_{sct}^j B_s^q + \mathbf{X}'_{st} \beta + \delta_{sc} + \lambda_{tc} + \varepsilon_{isct}. \quad (2)$$

Equation (2) estimates a separate event-time path for each initial-intensity group. Each coefficient γ_{jq} measures the change in the treated-comparison outcome difference for group q at event time j , relative to that group's adoption-year difference. Group membership is fixed at adoption and remains unchanged when a state's SREC requirement subsequently changes.

As robustness checks, we estimate static stacked difference-in-differences models using the same stacked sample. Define $P_{sct} = H_{sc} \mathbf{1}\{t \geq c\}$. The baseline static model is

$$y_{isct} = \alpha P_{sct} + \mathbf{X}'_{st} \beta + \delta_{sc} + \lambda_{tc} + \varepsilon_{isct}. \quad (3)$$

The post-adoption period includes the adoption year and all subsequent observed years; the pretreatment period includes all available years before adoption. The coefficient α summarizes the pooled post-versus-pre change for treated observations relative to the corresponding change among eligible comparison observations, imposing a common coefficient across post-adoption years.

We also estimate a static model that allows the post-adoption coefficient to vary across the same initial-intensity groups:

$$y_{isct} = \sum_{q=1}^3 \alpha_q P_{sct} B_s^q + \mathbf{X}'_{st} \beta + \delta_{sc} + \lambda_{tc} + \varepsilon_{isct}. \quad (4)$$

Each coefficient α_q summarizes the post-versus-pre change for states in group q , relative to the corresponding change among eligible comparison observations.

5 Results

We begin our analysis by examining changes in utility generation portfolios following SREC adoption. Our controlled event-study estimates indicate a gradual shift toward solar and away from non-clean generation (Figure 1). We estimate a small increase in solar share during the first two years after adoption, followed by an increase of 15.9 percentage points in the fourth year and 19.7 percentage points in the pooled period five or more years after adoption. Non-clean generation share declines earlier and is about 12 percentage points lower in the fourth year and the pooled later period. For other clean generation share, we find an initial increase followed by a decline, although the estimated later-period decline of 7.6 percentage points is imprecise. We measure these changes relative to the adoption-year difference between utilities in adopting states and those in never-treated and not-yet-treated states, and report 90 percent confidence intervals. Our solar-share estimates are close to zero in the four years immediately preceding adoption. The estimates for other clean and non-clean generation shares also remain near zero over this period, with small movements across years.

We next examine whether these changes differ across states with different starting requirements. Our estimates place the solar expansion in the middle- and high-dose groups, with an earlier increase in the high-dose group (Figure 2). In this group, we estimate an increase in solar share beginning in the second year and reaching 34.9 percentage points in the pooled period five or more years after adoption. Our middle-dose estimates remain near zero through the third year before rising to 27.3 percentage points in the pooled later period. For the low-dose group, our point estimates remain near zero during the first four years and turn negative in the pooled later period.

We also find differences in the composition of the estimated portfolio shifts. In the pooled later period, the high-dose solar increase is accompanied by estimated declines of 18.0 percentage points in other clean generation share and 16.9 percentage points in non-clean generation

share. For the middle-dose group, we estimate corresponding declines of 7.6 and 19.7 percentage points. Our point estimates thus associate the larger solar expansion in the high-dose group with a larger reduction in other clean generation share, while the declines in non-clean generation share are similar in magnitude. In the low-dose group, we find higher other clean generation shares during the first four post-adoption years, but also in the four preceding years relative to the adoption-year reference. In the four years immediately preceding adoption, solar-share estimates are generally close to zero across all three groups. Other clean-share estimates tend to be positive in the low-requirement group and slightly negative in the high-requirement group. Non-clean-share estimates tend to be negative in the low-requirement group and positive in the middle- and high-requirement groups.

Our baseline difference-in-differences estimates also indicate higher solar and lower non-clean generation shares over the full post-adoption period (Table 2). Including the adoption year, we estimate a 19.2 percentage point increase in solar share relative to the pre-adoption period and the comparison group, accompanied by a 12.2 percentage point decline in non-clean generation share. Our estimated 7.0 percentage point decline in other clean generation share is imprecise. When we allow differential linear trends before and after adoption, we estimate adoption-year level changes of 10.8 percentage points for solar share and -10.7 percentage points for non-clean generation share, measured relative to the extrapolated pre-adoption trend. Our corresponding estimate for other clean generation share is near zero and imprecise. We estimate neither the pre-adoption slope differences nor the subsequent slope changes precisely.

Further, we examine whether the differences across starting-dose groups also appear in the DiD estimates (Table 3). Without trend adjustments, we estimate solar-share increases of 22.0 percentage points in the middle-dose group and 31.2 percentage points in the high-dose group, accompanied by declines in non-clean generation share of roughly 17–18 percentage points in both groups. Our low-dose solar estimate is negative and imprecise. We also estimate declines in other clean generation share in the middle- and high-dose groups, although these estimates are too imprecise to establish a decline in either group over the full post-adoption period.

When we condition on differential trends, our estimated adoption-year increases in solar share remain economically substantial at 17.5 percentage points in the middle-dose group and 28.0 percentage points in the high-dose group. In both groups, we estimate declines in non-clean generation share of about 18 percentage points relative to the extrapolated pre-adoption trend. Our low-dose level changes and adoption-year changes in other clean generation share remain imprecisely estimated. For other clean generation share, we estimate a downward change in the relative slope after adoption of about 0.9 percentage points per year, which we restrict to be common across dose groups. Our corresponding slope changes for solar and non-clean generation shares are imprecise.

Finally, we examine how the estimated effects of SREC adoption vary across state characteristics and initial market design. We estimate the models separately by subsample, retaining the controls, fixed effects, and clustering structure of the main analysis. The average post-adoption estimates in Panel A of the appendix tables generally preserve the main pattern of rising solar shares and declining non-clean shares.

Restricting the sample to states that had already enacted mandatory general RPS policies yields estimates close to the main results (Appendix Table A1). Solar share rises by approximately 17 percentage points and non-clean share falls by approximately 11 percentage points, while the change in non-solar clean share remains imprecisely estimated. The event study also shows the gradual solar response found in the main analysis (Appendix Figure A1). Within this restricted sample, states with middle and high initial requirements continue to show substantial solar gains and declining non-clean shares. The low-requirement group shows little sustained solar expansion (Appendix Table A2 and Figure A2).

Solar point estimates are larger in states with stronger solar resources, smaller generation sectors, and less preexisting non-solar clean generation (Appendix Tables A4–A6 and Figures A4–A6). The largest contrast is between the solar-resource groups; solar estimates differ relatively little across the fossil-dependence groups (Appendix Table A3 and Figure A3). Estimated non-clean shares decline on both sides of all four baseline splits, with precision varying

across subsamples. In states with stronger solar resources and less preexisting non-solar clean generation, the larger solar gains also coincide with declines in non-solar clean share. These results show that the composition of the generation-share adjustment varies across states, even as the decline in non-clean share remains a common pattern.

Local-only SREC markets, represented by Massachusetts and New Jersey, show larger solar point estimates and more sustained solar increases in the event studies than markets permitting outside supply (Appendix Table A7 and Figure A7). Estimated non-clean shares decline in both groups, consistent with the main results.

6 Conclusion

State renewable energy mandates have become an important instrument for changing the composition of electricity generation in the United States. Within these policies, solar renewable energy certificate (SREC) programs create a dedicated market for the attributes of solar generation, supplementing producers' electricity revenue. In this paper, we examine how SREC adoption changes utility generation portfolios and whether those changes differ with the solar requirement in place at adoption. We combine annual utility-level electricity data covering 2001–2023 with state policy requirements to examine changes in the shares of solar, non-solar clean, and non-clean generation.

Using stacked event-study and difference-in-differences models that exploit variation in adoption timing across states, we find substantial increases in solar generation share accompanied by declines in non-clean generation share. The event-study estimates show that the solar response develops over several years. Relative to adoption-year differences between treated and comparison utilities, the estimated solar-share increase reaches 15.9 percentage points in the fourth year and 19.7 percentage points in the pooled period five or more years after adoption. Non-clean generation share is approximately 12 percentage points lower in these later periods. The difference-in-differences estimates show the same overall pattern. When we allow differential linear trends before and after adoption, we estimate adoption-year level changes of 10.8 percentage points for solar share and -10.7 percentage points for non-clean share relative to

extrapolated pre-adoption trends. The corresponding change in non-solar clean share is close to zero and imprecisely estimated.

Finally, the estimated solar expansion is concentrated in states with middle and high initial requirements, with earlier growth in the high-requirement group. Both groups retain substantial positive solar and negative non-clean level estimates after accounting for differential trends. The group estimates also reveal differences in the composition of adjustment. In the later event-study period, the high-requirement group combines a larger estimated solar increase with a larger estimated decline in non-solar clean share, while declines in non-clean share are similar across the middle- and high-requirement groups. Evidence of reductions in non-solar clean share is less consistent across specifications. These findings identify the initial policy requirement as an important dimension for understanding the timing and composition of generation responses. They also highlight the value of evaluating solar policies through changes in the full generation portfolio: greater solar penetration can accompany reallocation among clean sources as well as a shift away from other generation.

References

- Abito, Jose Miguel et al. (2025). “Designing More Cost-effective Trading Markets for Renewable Energy”. In: *The Energy Journal* 46(4).
- Auffhammer, Maximilian and Erin T. Mansur (Nov. 2014). “Measuring climatic impacts on energy consumption: A review of the empirical literature”. In: *Energy Economics* 46, pp. 522–530.
- Bird, Lori, Jenny Heeter, and Claire Kreycik (2011). *Solar Renewable Energy Certificate (SREC) Markets: Status and Trends*. Tech. rep. NREL/TP-6A20-52868. National Renewable Energy Laboratory.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess (2024). “Revisiting Event-Study Designs: Robust and Efficient Estimation”. In: *The Review of Economic Studies* 91(6), pp. 3253–3285.
- Callaway, Brantly and Pedro H. C. Sant’Anna (2021). “Difference-in-Differences with multiple time periods”. In: *Journal of Econometrics* 225(2), pp. 200–230.
- Carley, Sanya et al. (2018). “Empirical evaluation of the stringency and design of renewable portfolio standards”. In: *Nature Energy* 3, pp. 754–763.
- Cohen, Jed J., Levan Elbakidze, and Randall Jackson (2022). “Interstate protectionism: the case of solar renewable energy credits”. In: *American Journal of Agricultural Economics* 104(2), pp. 717–738.
- Coulon, Michael, Javad Khazaei, and Warren B. Powell (Sept. 2015a). “SMART-SREC: A stochastic model of the New Jersey solar renewable energy certificate market”. In: *Journal of Environmental Economics and Management* 73, pp. 13–31.
- Coulon, Michael, Javad Khazaei, and Warren B. Powell (2015b). “SMART-SREC: A stochastic model of the New Jersey solar renewable energy certificate market”. In: *Journal of Environmental Economics and Management* 73, pp. 13–31.
- Deschenes, Olivier, Christopher Malloy, and Gavin McDonald (Oct. 2023). “Causal effects of Renewable Portfolio Standards on renewable investments and generation: The role of heterogeneity and dynamics”. In: *Resource and Energy Economics* 75, p. 101393.

- Dilek, Gökhan, Jordi J. Teixidó, and Mònica Serrano (Mar. 2026). “Carbon pricing effects on renewables: Evidence from California’s electricity market”. In: *Energy Economics* 155, p. 109174.
- Felder, Frank A. and Colin J. Loxley (2012). *The Implications of a Vertical Demand Curve in Solar Renewable Portfolio Standards*. Tech. rep. April. Rutgers Center for Energy, Economic and Environmental Policy.
- Fischer, Carolyn and Richard G. Newell (2008). “Environmental and technology policies for climate mitigation”. In: *Journal of Environmental Economics and Management* 55(2), pp. 142–162.
- Gaul, Chip and Sanya Carley (2012). “Solar set asides and renewable electricity certificates: Early lessons from North Carolina’s experience with its renewable portfolio standard”. In: *Energy Policy* 48, pp. 460–469.
- Gillenwater, Michael (2008). “Redefining RECs—Part 1: Untangling attributes and offsets”. In: *Energy Policy* 36(6), pp. 2109–2119.
- Gillenwater, Michael, Xi Lu, and Miriam Fischlein (2014). “Additionality of wind energy investments in the U.S. voluntary green power market”. In: *Renewable Energy* 63, pp. 452–457.
- Hart, David M. (2010). “Making, breaking, and (partially) remaking markets: State regulation and photovoltaic electricity in New Jersey”. In: *Energy Policy* 38(11), pp. 6662–6673.
- Hollingsworth, Alex and Ivan Rudik (2019). “External Impacts of Local Energy Policy: The Case of Renewable Portfolio Standards”. In: *Journal of the Association of Environmental and Resource Economists* 6(1), pp. 187–213.
- Langer, Lissy et al. (2024). “Does the purchase of voluntary renewable energy certificates lead to emission reductions? A review of studies quantifying the impact”. In: *Journal of Cleaner Production* 478, p. 143791.
- Massachusetts Department of Energy Resources (Apr. 2014). *RPS & APS Annual Compliance Report for 2012*. Tech. rep. Published April 22, 2014. Commonwealth of Massachusetts.
- Palmer, Karen and Dallas Burtraw (2005). “Cost-effectiveness of renewable electricity policies”. In: *Energy Economics* 27(6), pp. 873–894.

- Pennsylvania General Assembly (2017). *Act 40 of 2017*. Section 11.1, adding Section 2804, Alternative Energy Portfolio Standards, to the Administrative Code of 1929.
- Sant’Anna, Pedro H. C. and Qi Xu (2026). “Difference-in-Differences with Compositional Changes”. In: *Journal of Econometrics* 253, p. 106147.
- Sun, Liyang and Sarah Abraham (2021). “Estimating dynamic treatment effects in event studies with heterogeneous treatment effects”. In: *Journal of Econometrics* 225(2), pp. 175–199.
- U.S. Energy Information Administration (2022). *Renewable Energy Requirements and Incentives*. Energy Explained. Updated December 30, 2022. Accessed September 18, 2026.
- U.S. Energy Information Administration (2024). *What is U.S. electricity generation by energy source?* U.S. Energy Information Administration. 2023 preliminary generation figures from Electric Power Monthly, February 2024. Accessed September 17, 2026.
- U.S. Environmental Protection Agency (2025). *Energy Attribute Certificates (EACs)*. Updated October 20, 2025. Accessed September 18, 2026.
- Upton, Gregory B. and Brian F. Snyder (2017). “Funding renewable energy: An analysis of renewable portfolio standards”. In: *Energy Economics* 66, pp. 205–216.
- Vose, Russell S. et al. (2014). *NOAA Monthly U.S. Climate Divisional Database (NClimDiv)*.
- Wing, Coady, Seth M. Freedman, and Alex Hollingsworth (2024a). *Stacked Difference-in-Differences*. Tech. rep. 32054. National Bureau of Economic Research.
- Wing, Coady, Seth M. Freedman, and Alex Hollingsworth (Jan. 2024b). *Stacked Difference-in-Differences*.
- Wiser, Ryan, Galen Barbose, and Edward Holt (2011). “Supporting solar power in renewables portfolio standards: Experience from the United States”. In: *Energy Policy* 39(7), pp. 3894–3905.
- Yin, Haitao and Nicholas Powers (2010). “Do state renewable portfolio standards promote in-state renewable generation?” In: *Energy Policy* 38(2), pp. 1140–1149.
- Zhou, Shan, Barry D. Solomon, and Marilyn A. Brown (June 18, 2024). “The spillover effect of mandatory renewable portfolio standards”. In: *Proceedings of the National Academy of Sciences* 121(25), e2313193121.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)
	Treated	Untreated	High	Middle	Low
Generation Outcomes					
Solar share	0.364	0.142	0.511	0.355	0.099
Non-solar clean share	0.243	0.369	0.248	0.183	0.284
Non-clean share	0.393	0.489	0.242	0.462	0.617
Control Variables					
Log state population	15.892	15.921	15.752	15.681	16.329
Log real GDP per capita	11.017	10.956	11.048	11.086	10.903
Solar irradiance anomaly	0.062	0.026	0.062	0.056	0.068
Heating degree days (65 °F)	4958.668	4744.429	4736.480	4754.650	5542.820
Cooling degree days (65 °F)	968.684	1215.996	1029.325	1048.840	788.866

Notes: Table presents means for eligible utility–state–year observations during 2001–2023, with each observation counted once. Columns 1 and 2 report means for ever-treated and never-treated states, respectively. Columns 3–5 report means for the high, middle, and low terciles of treated states’ adoption-year solar requirements, using the analysis’s original state rankings and alphabetical tie-breaking rule. Generation shares are fractions. Non-solar clean generation includes hydro, waste, and wind; non-clean generation includes coal, gas, and oil, following the model definitions. Solar irradiance is an anomaly relative to the state’s 2001–2004 mean, measured in kWh/m²/day. Heating and cooling degree days are annual totals using a 65 °F base. The sample requires positive total generation, a non-missing utility identifier, and complete values for all five controls.

Table 2: Baseline Difference-in-Differences Estimates

Panel A: Difference-in-Differences						
	Solar		Non-solar Clean		Non-clean Energy	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	Baseline	With Controls	Baseline	With Controls
Post Treatment	0.1953** (0.0919)	0.1924** (0.0891)	-0.0756 (0.0489)	-0.0702 (0.0525)	-0.1196** (0.0590)	-0.1222** (0.0562)
Observations	313,265	305,612	313,265	305,612	313,265	305,612
R-squared	0.2630	0.2646	0.1144	0.1171	0.1915	0.1919

Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.1021** (0.0454)	0.1075** (0.0482)	-0.0000 (0.0275)	-0.0009 (0.0293)	-0.1021*** (0.0221)	-0.1066*** (0.0196)
Trend Treatment	0.0120 (0.0088)	0.0114 (0.0081)	-0.0055 (0.0052)	-0.0048 (0.0055)	-0.0064 (0.0068)	-0.0066 (0.0063)
Trend Post Treatment	-0.0069 (0.0136)	-0.0071 (0.0132)	-0.0006 (0.0055)	-0.0010 (0.0060)	0.0075 (0.0119)	0.0080 (0.0108)
Observations	313,265	305,612	313,265	305,612	313,265	305,612
R-squared	0.2632	0.2647	0.1145	0.1172	0.1915	0.1919

Notes: Table presents stacked DiD estimates of SREC adoption effects on generation shares during 2001–2023. Shares are fractions; 0.01 corresponds to one percentage point. Non-solar clean generation includes hydro, waste, and wind; non-clean generation includes coal, gas, and oil, following the model definitions. Panel A reports the average post-adoption effect. Post Treatment equals the treated-cohort indicator multiplied by an indicator for the adoption year or later. In Panel B, Post Treatment is the adoption-year level change. Trend Treatment interacts treatment status with years relative to adoption; Trend Post Treatment additionally interacts this term with the post indicator and measures the change in slope after adoption. Columns 1, 3, and 5 exclude controls. Columns 2, 4, and 6 include log state population, log real GDP per capita, solar irradiance anomaly, heating degree days, and cooling degree days. Missing controls reduce the controlled sample. All specifications include state-by-cohort and year-by-cohort fixed effects. Standard errors, clustered by state-by-cohort, year-by-cohort, and utility, appear in parentheses. Observations count stacked utility–state–year–cohort records. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Difference-in-Differences Estimates by Initial-Dose Tercile

Panel A: Difference-in-Differences						
	Solar		Non-solar Clean		Non-clean Energy	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	With Controls	Baseline	With Controls	Baseline	With Controls
Post Treatment $\times$ High	0.3223*** (0.1174)	0.3119*** (0.1164)	-0.1387* (0.0706)	-0.1309 (0.0808)	-0.1836** (0.0763)	-0.1810** (0.0751)
Post Treatment $\times$ Middle	0.2229*** (0.0573)	0.2200*** (0.0594)	-0.0422 (0.0358)	-0.0460 (0.0322)	-0.1807*** (0.0448)	-0.1740*** (0.0437)
Post Treatment $\times$ Low	-0.0405* (0.0214)	-0.0342 (0.0230)	0.0112 (0.0554)	0.0202 (0.0547)	0.0293 (0.0397)	0.0139 (0.0409)
Observations	313,265	305,612	313,265	305,612	313,265	305,612
R-squared	0.2641	0.2656	0.1145	0.1172	0.1917	0.1921

Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment $\times$ High	0.2860** (0.1235)	0.2799** (0.1244)	-0.0991 (0.0705)	-0.0963 (0.0791)	-0.1868** (0.0766)	-0.1836** (0.0727)
Post Treatment $\times$ Middle	0.1712** (0.0801)	0.1753** (0.0801)	0.0162 (0.0330)	0.0066 (0.0316)	-0.1874** (0.0770)	-0.1819** (0.0734)
Post Treatment $\times$ Low	-0.0864 (0.0734)	-0.0740 (0.0734)	0.0627 (0.0413)	0.0663 (0.0420)	0.0237 (0.0515)	0.0077 (0.0524)
Trend Treatment	-0.0015 (0.0021)	-0.0012 (0.0021)	0.0022* (0.0011)	0.0026 (0.0016)	-0.0007 (0.0016)	-0.0014 (0.0016)
Trend Post Treatment	0.0071 (0.0058)	0.0061 (0.0058)	-0.0087*** (0.0017)	-0.0087*** (0.0022)	0.0016 (0.0064)	0.0026 (0.0058)
Observations	313,265	305,612	313,265	305,612	313,265	305,612
R-squared	0.2643	0.2658	0.1146	0.1173	0.1917	0.1921

Notes: Table presents stacked DiD estimates by treated states' adoption-year solar-requirement terciles during 2001–2023. The original state rankings determine High, Middle, and Low, with dose ties broken alphabetically. Generation shares are fractions. Non-solar clean generation includes hydro, waste, and wind; non-clean generation includes coal, gas, and oil, following the model definitions. Post Treatment equals the treated-cohort indicator multiplied by an indicator for the adoption year or later. Panel A reports each tercile's average post-adoption effect. Panel B reports tercile-specific adoption-year level changes and two common trend coefficients. Trend Treatment interacts treatment status with years relative to adoption; Trend Post Treatment additionally interacts this term with the post indicator. These trends are common across terciles within each regression. Columns 1, 3, and 5 exclude controls. Columns 2, 4, and 6 include log state population, log real GDP per capita, solar irradiance anomaly, heating degree days, and cooling degree days. Missing controls reduce the controlled sample. All specifications include state-by-cohort and year-by-cohort fixed effects. Standard errors, clustered by state-by-cohort, year-by-cohort, and utility, appear in parentheses. Observations count stacked utility–state–year–cohort records. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 1
Event Study Estimates: Solar, Non-solar Clean, and Non-clean Energy

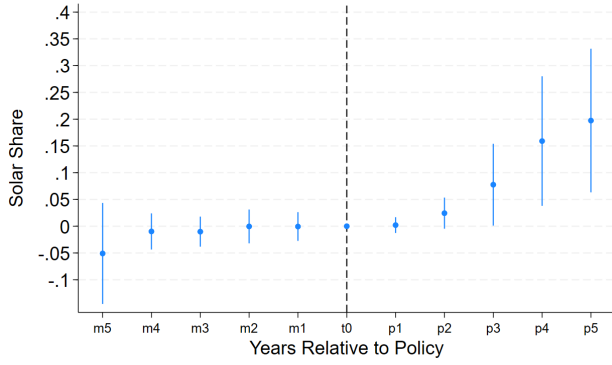


Figure 1a

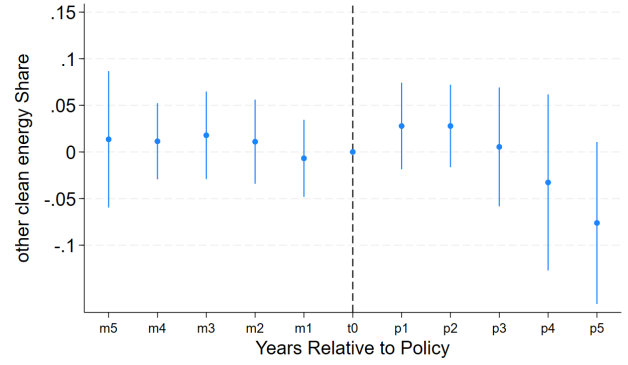


Figure 1b

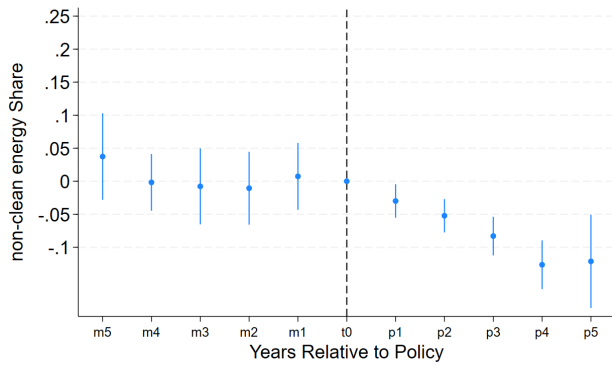


Figure 1c

Notes: Figures 1a–1c present controlled stacked event-study estimates for solar, non-solar clean, and non-clean generation shares, respectively, during 2001–2023. Shares are fractions. Non-solar clean generation includes hydro, waste, and wind; non-clean generation includes coal, gas, and oil, following the model definitions. The sample contains 305,612 stacked utility–state–year–cohort observations with complete controls. Comparison observations come from never-treated states and not-yet-treated states before their adoption. All models include state-by-cohort and year-by-cohort fixed effects, log state population, log real GDP per capita, solar irradiance anomaly, heating degree days, and cooling degree days. Event time is measured in years relative to SREC adoption; the adoption year (t_0) is omitted and plotted at zero. Labels m5–m1 and p1–p5 denote pre- and post-adoption years, with endpoint bins at five or more years before and after adoption. The dashed vertical line marks the adoption year. Vertical bars indicate 90% confidence intervals; standard errors are clustered by state-by-cohort, year-by-cohort, and utility.

Figure 2
Event Study Estimates by Initial Dose:
Solar, Non-solar Clean, and Non-clean Energy

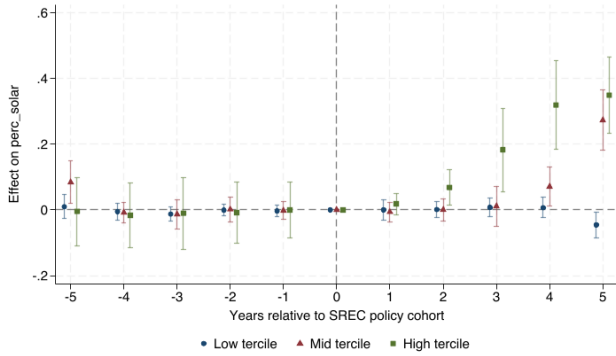


Figure 2a

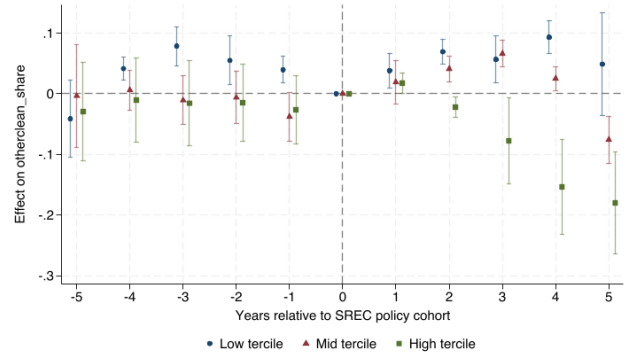


Figure 2b

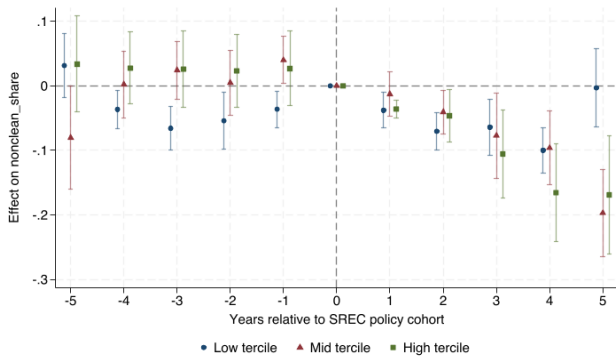


Figure 2c

Notes: Figures 2a–2c present controlled stacked event-study estimates by initial-dose tertile for solar, non-solar clean, and non-clean generation shares, respectively, during 2001–2023. Treated states are ranked by their adoption-year solar requirement and assigned to Low, Mid, and High tertiles, with ties broken alphabetically. The original state assignments are retained before control-data exclusions. Blue circles, maroon triangles, and green squares denote Low, Mid, and High, respectively; small horizontal offsets separate the series. Shares are fractions. Non-solar clean generation includes hydro, waste, and wind; non-clean generation includes coal, gas, and oil, following the model definitions. Each model uses 305,612 stacked observations, state-by-cohort and year-by-cohort fixed effects, and five controls: log state population, log real GDP per capita, solar irradiance anomaly, heating degree days, and cooling degree days. Comparison observations come from never-treated states and not-yet-treated states before adoption. Year zero is the omitted adoption year, plotted at zero for all tertiles; endpoint bins include five or more years before and after adoption. Dashed lines mark adoption and zero effect. Vertical bars indicate 90% confidence intervals; standard errors are clustered by state-by-cohort, year-by-cohort, and utility.

Appendix

Appendix Table A1
SREC Adoption Effects: Main Sample and RPS States

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Main sample	RPS states	Main sample	RPS states	Main sample	RPS states
Panel A: Difference-in-Differences						
Post Treatment	0.1924** (0.0891)	0.1693* (0.0923)	-0.0702 (0.0525)	-0.0607 (0.0652)	-0.1222** (0.0562)	-0.1085** (0.0545)
Observations	305,612	194,963	305,612	194,963	305,612	194,963
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.1075** (0.0482)	0.0982** (0.0488)	-0.0009 (0.0293)	0.0177 (0.0304)	-0.1066*** (0.0196)	-0.1159*** (0.0175)
Trend Treatment	0.0114 (0.0081)	0.0114 (0.0086)	-0.0048 (0.0055)	-0.0067 (0.0079)	-0.0066 (0.0063)	-0.0047 (0.0061)
Trend Post Treatment	-0.0071 (0.0132)	-0.0087 (0.0136)	-0.0010 (0.0060)	0.0007 (0.0084)	0.0080 (0.0108)	0.0080 (0.0090)
Observations	305,612	194,963	305,612	194,963	305,612	194,963

Notes: Main sample columns reproduce the controlled main estimates. RPS states columns retain treated and comparison states with mandatory general RPS legislation enacted and not repealed by the end of the year before the focal SREC cohort; eligibility is fixed within each stack. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Observations are stacked utility–state–year–cohort records. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A2
Effects by Initial Requirement: Main Sample and RPS States

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Main sample	RPS states	Main sample	RPS states	Main sample	RPS states
Panel A: Difference-in-Differences						
High initial requirement						
Post Treatment	0.3119*** (0.1164)	0.3001** (0.1202)	-0.1309 (0.0808)	-0.1402 (0.1015)	-0.1810** (0.0751)	-0.1599** (0.0788)
Middle initial requirement						
Post Treatment	0.2200*** (0.0594)	0.1778*** (0.0576)	-0.0460 (0.0322)	-0.0420 (0.0330)	-0.1740*** (0.0437)	-0.1358*** (0.0442)
Low initial requirement						
Post Treatment	-0.0342 (0.0230)	-0.0661** (0.0294)	0.0202 (0.0547)	0.0630 (0.0525)	0.0139 (0.0409)	0.0031 (0.0386)
Panel B: Difference-in-Differences with Trends in Event Timing						
High initial requirement						
Post Treatment	0.2799** (0.1244)	0.2023 (0.1397)	-0.0963 (0.0791)	-0.0643 (0.0807)	-0.1836** (0.0727)	-0.1380** (0.0607)
Trend Treatment	-0.0012 (0.0021)	-0.0003 (0.0036)	0.0026 (0.0016)	-0.0000 (0.0037)	-0.0014 (0.0016)	0.0003 (0.0036)
Trend Post Treatment	0.0061 (0.0058)	0.0131 (0.0099)	-0.0087*** (0.0022)	-0.0097** (0.0047)	0.0026 (0.0058)	-0.0034 (0.0060)
Middle initial requirement						
Post Treatment	0.1753** (0.0801)	0.0709* (0.0364)	0.0066 (0.0316)	0.1019*** (0.0307)	-0.1819** (0.0734)	-0.1728*** (0.0212)
Trend Treatment	-0.0012 (0.0021)	-0.0126*** (0.0040)	0.0026 (0.0016)	-0.0125 (0.0075)	-0.0014 (0.0016)	0.0251*** (0.0053)
Trend Post Treatment	0.0061 (0.0058)	0.0274*** (0.0071)	-0.0087*** (0.0022)	0.0017 (0.0088)	0.0026 (0.0058)	-0.0291*** (0.0065)
Low initial requirement						
Post Treatment	-0.0740 (0.0734)	0.0827*** (0.0115)	0.0663 (0.0420)	0.0097 (0.0094)	0.0077 (0.0524)	-0.0925*** (0.0196)
Trend Treatment	-0.0012 (0.0021)	-0.0060 (0.0043)	0.0026 (0.0016)	0.0160*** (0.0056)	-0.0014 (0.0016)	-0.0101*** (0.0035)
Trend Post Treatment	0.0061 (0.0058)	-0.0083 (0.0067)	-0.0087*** (0.0022)	-0.0164* (0.0097)	0.0026 (0.0058)	0.0247*** (0.0054)

Notes: Main sample columns reproduce the controlled pooled model with tercile-specific treatment effects and common trend slopes in Panel B. Its common slopes are repeated for comparison. RPS states columns estimate each treated tercile against the common eligible comparison pool, with separate fixed effects, control slopes, and trends. The restriction retains states with mandatory general RPS legislation enacted and not repealed before the focal cohort. Original tercile assignments are unchanged. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A3
SREC Adoption Effects by Baseline Fossil Dependence

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low	High	Low	High	Low	High
Panel A: Difference-in-Differences						
Post Treatment	0.2121 (0.1451)	0.2426** (0.1075)	-0.0973 (0.0687)	-0.1063** (0.0521)	-0.1149 (0.0854)	-0.1363** (0.0640)
Observations	167,648	137,964	167,648	137,964	167,648	137,964
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.1259* (0.0747)	0.0515* (0.0295)	-0.0080 (0.0519)	0.0367** (0.0175)	-0.1179*** (0.0407)	-0.0882*** (0.0296)
Trend Treatment	0.0169 (0.0176)	0.0156*** (0.0057)	-0.0101 (0.0081)	-0.0159*** (0.0047)	-0.0068 (0.0120)	0.0003 (0.0038)
Trend Post Treatment	-0.0143 (0.0224)	-0.0012 (0.0093)	0.0041 (0.0089)	0.0074 (0.0061)	0.0102 (0.0171)	-0.0062 (0.0054)
Observations	167,648	137,964	167,648	137,964	167,648	137,964

Notes: Both treated and comparison states are split by baseline fossil dependence (coal, gas, and oil divided by eight-fuel total generation), at 0.75000536. Nuclear is excluded from this baseline measure. State measures average the three pre-cohort years, requiring at least two valid observations. The median counts each treated state once using its own baseline. Low includes the median. Comparison states use the focal cohort's baseline years; membership is fixed within each stack. Each group is estimated separately. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Observations are stacked utility–state–year–cohort records. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A4
SREC Adoption Effects by Baseline Total State Generation

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low	High	Low	High	Low	High
Panel A: Difference-in-Differences						
Post Treatment	0.2653** (0.1068)	0.1824 (0.1549)	-0.1007** (0.0416)	-0.0992 (0.1089)	-0.1646* (0.0847)	-0.0832 (0.0552)
Observations	140,133	165,479	140,133	165,479	140,133	165,479
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.0649 (0.0557)	0.0887 (0.0929)	0.0292 (0.0179)	0.0348 (0.0588)	-0.0941* (0.0550)	-0.1235*** (0.0391)
Trend Treatment	0.0082 (0.0053)	0.0328* (0.0188)	-0.0135*** (0.0036)	-0.0191 (0.0150)	0.0053 (0.0047)	-0.0137** (0.0068)
Trend Post Treatment	0.0111*** (0.0032)	-0.0373* (0.0200)	0.0051 (0.0052)	0.0124 (0.0129)	-0.0162*** (0.0025)	0.0249** (0.0107)
Observations	140,133	165,479	140,133	165,479	140,133	165,479

Notes: Both treated and comparison states are split by baseline total state generation observed in the utility panel, at 69,334,974 MWh. State measures average the three pre-cohort years, requiring at least two valid observations. The median counts each treated state once using its own baseline. Low includes the median. Comparison states use the focal cohort's baseline years; membership is fixed within each stack. Each group is estimated separately. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Observations are stacked utility–state–year–cohort records. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A5
SREC Adoption Effects by Solar-Resource Potential

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low	High	Low	High	Low	High
Panel A: Difference-in-Differences						
Post Treatment	0.0953 (0.0992)	0.3840*** (0.0947)	0.0514 (0.0382)	-0.2147*** (0.0712)	-0.1467* (0.0854)	-0.1693*** (0.0328)
Observations	87,717	217,895	87,717	217,895	87,717	217,895
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.0969** (0.0449)	0.1557 (0.1304)	0.0023 (0.0240)	-0.0100 (0.0983)	-0.0991*** (0.0199)	-0.1458*** (0.0379)
Trend Treatment	0.0129** (0.0060)	0.0255*** (0.0059)	0.0016 (0.0063)	-0.0228*** (0.0085)	-0.0145* (0.0078)	-0.0028 (0.0050)
Trend Post Treatment	-0.0197*** (0.0054)	-0.0113* (0.0060)	0.0035 (0.0074)	0.0099 (0.0081)	0.0162 (0.0111)	0.0014 (0.0071)
Observations	87,717	217,895	87,717	217,895	87,717	217,895

Notes: Both treated and comparison states are split by mean state irradiance over 2001–2004, requiring all four years, at 3.8734751 kWh per square meter per day. The median counts each treated state once. Membership is fixed within each stack. Low includes the median. Each group is estimated separately. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Observations are stacked utility–state–year–cohort records. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A6
SREC Adoption Effects by Preexisting Non-solar Clean Generation

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low	High	Low	High	Low	High
Panel A: Difference-in-Differences						
Post Treatment	0.2570**	0.1221	-0.1778**	-0.0092	-0.0793*	-0.1130
	(0.1143)	(0.1036)	(0.0810)	(0.0266)	(0.0451)	(0.0926)
Observations	97,280	208,332	97,280	208,332	97,280	208,332
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.1704**	0.0437	-0.0219	0.0290*	-0.1485***	-0.0727**
	(0.0830)	(0.0526)	(0.0559)	(0.0148)	(0.0416)	(0.0344)
Trend Treatment	0.0074	0.0139**	-0.0111	-0.0025	0.0037	-0.0113
	(0.0120)	(0.0066)	(0.0094)	(0.0043)	(0.0050)	(0.0082)
Trend Post Treatment	-0.0008	-0.0115	-0.0018	-0.0007	0.0026	0.0122
	(0.0129)	(0.0133)	(0.0078)	(0.0055)	(0.0092)	(0.0132)
Observations	97,280	208,332	97,280	208,332	97,280	208,332

Notes: Both treated and comparison states are split by baseline non-solar clean generation (hydro, waste, and wind divided by eight-fuel total generation), at 0.027392596. State measures average the three pre-cohort years, requiring at least two valid observations. The median counts each treated state once using its own baseline. Low includes the median. Comparison states use the focal cohort's baseline years; membership is fixed within each stack. Each group is estimated separately. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Observations are stacked utility-state-year-cohort records. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A7
SREC Adoption Effects by Initial Market Openness

	Solar share		Non-solar clean share		Non-clean share	
	(1)	(2)	(3)	(4)	(5)	(6)
	Local-only	Outside supply	Local-only	Outside supply	Local-only	Outside supply
Panel A: Difference-in-Differences						
Post Treatment	0.3238*** (0.0257)	0.1273 (0.1249)	-0.0630** (0.0280)	-0.0756 (0.0755)	-0.2607*** (0.0232)	-0.0517 (0.0594)
Observations	296,681	300,782	296,681	300,782	296,681	300,782
Panel B: Difference-in-Differences with Trends in Event Timing						
Post Treatment	0.0968*** (0.0145)	0.1217 (0.0830)	0.0124* (0.0063)	-0.0074 (0.0409)	-0.1092*** (0.0149)	-0.1143*** (0.0308)
Trend Treatment	0.0058 (0.0050)	0.0133 (0.0094)	0.0033 (0.0043)	-0.0093 (0.0071)	-0.0091** (0.0044)	-0.0040 (0.0061)
Trend Post Treatment	0.0163* (0.0084)	-0.0192** (0.0096)	-0.0131** (0.0051)	0.0057 (0.0058)	-0.0032 (0.0069)	0.0135 (0.0090)
Observations	296,681	300,782	296,681	300,782	296,681	300,782

Notes: Each group is estimated separately against its eligible comparison pool. Initial market rules determine whether new solar generation must be in-state or may include outside supply. Local-only comprises Massachusetts and New Jersey; six treated states permit some outside supply. Classifications are fixed at initial compliance. Outcomes are shares: non-solar clean is hydro, waste, and wind; non-clean is coal, gas, nuclear, and oil; other generation is excluded. Models include the five controls and state-by-cohort and year-by-cohort fixed effects. Panel A Post Treatment is the average post-adoption contrast. In Panel B it is the adoption-year level change; Trend Treatment is the pre-adoption relative slope and Trend Post Treatment the additional post-adoption slope. Observations are stacked utility–state–year–cohort records. Standard errors clustered by state-by-cohort, year-by-cohort, and utility are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Figure A1

Event Study Estimates within States with Mandatory RPS Policies

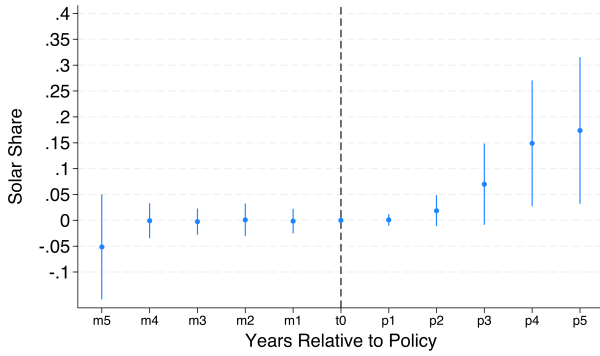


Figure A1a

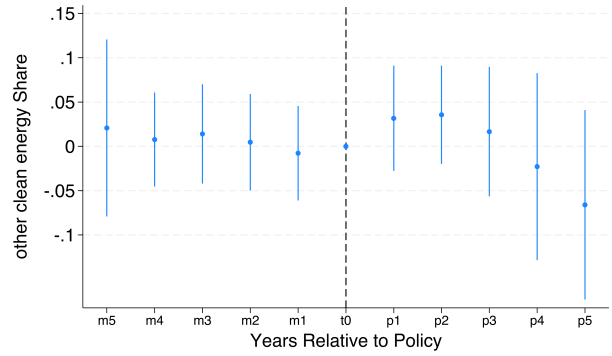


Figure A1b

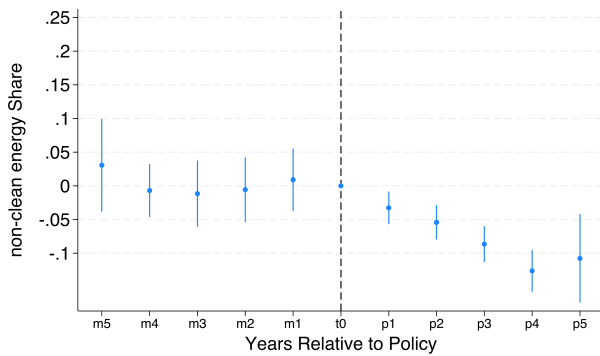


Figure A1c

Notes: Panels a–c show solar, non-solar clean, and non-clean shares. Treated and comparison states must have enacted a mandatory general RPS by the end of the year before the focal SREC cohort, without repeal by that date. Eligibility is fixed throughout each stack. Each path is a separate stacked event-study regression of utility generation shares. Non-solar clean comprises hydro, waste, and wind; non-clean comprises coal, gas, nuclear, and oil. Coefficients are relative to the omitted adoption-year treated–comparison contrast. Endpoints pool years ≤ -5 and ≥ 5 . Bars show 90% confidence intervals. Controls are log state population, log real GDP per capita, irradiance anomaly, and heating and cooling degree days. Models include state-by-cohort and year-by-cohort fixed effects, with standard errors clustered by state-by-cohort, year-by-cohort, and utility.

Appendix Figure A2

Event Study Estimates by Initial Dose within RPS States: Solar, Non-solar Clean, and Non-clean Energy

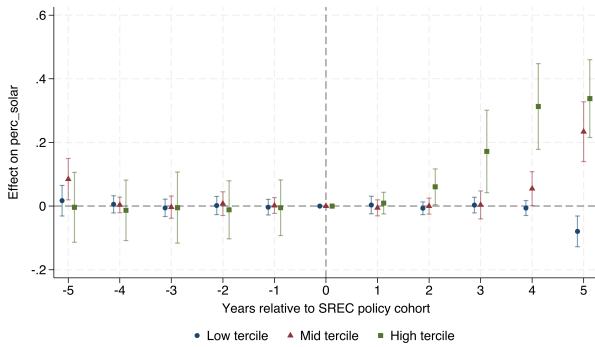


Figure A2a

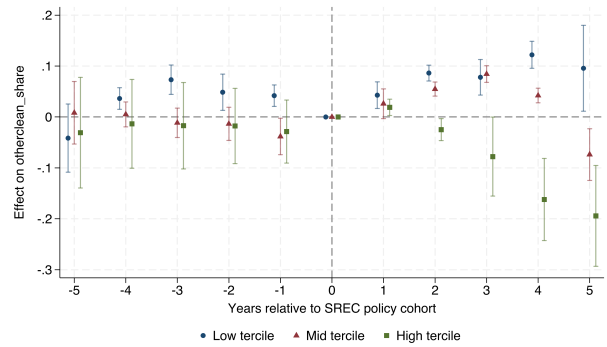


Figure A2b

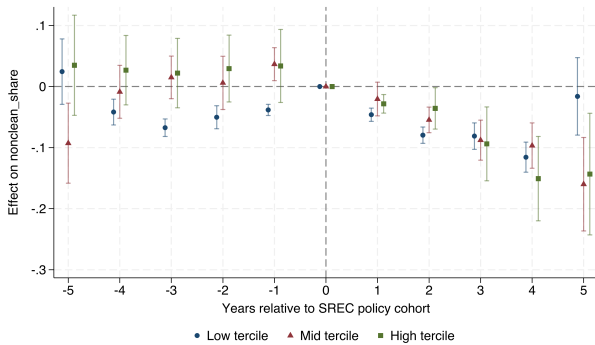


Figure A2c

Notes: Figures A2a–A2c present controlled stacked event-study estimates by initial-dose tercile for solar, non-solar clean, and non-clean generation shares, respectively, in the RPS-restricted sample. States must have enacted a mandatory general RPS before the focal SREC cohort, without repeal by that date. Original initial-requirement terciles are retained. Blue circles, maroon triangles, and green squares denote Low, Mid, and High, respectively; small horizontal offsets separate the series. Each path is a separate regression using the eligible RPS comparison pool; other focal treated terciles are excluded. Shares are fractions. Non-solar clean generation includes hydro, waste, and wind; non-clean generation includes coal, gas, nuclear, and oil. Models include state-by-cohort and year-by-cohort fixed effects and five controls: log state population, log real GDP per capita, solar irradiance anomaly, heating degree days, and cooling degree days. Year zero is the omitted adoption year, plotted at zero for all terciles; endpoint bins include five or more years before and after adoption. Coefficients are relative to the adoption-year treated–comparison contrast. Dashed lines mark adoption and zero effect. Vertical bars indicate 90% confidence intervals; standard errors are clustered by state-by-cohort, year-by-cohort, and utility.

Appendix Figure A3 Event Study Estimates by Baseline Fossil Dependence

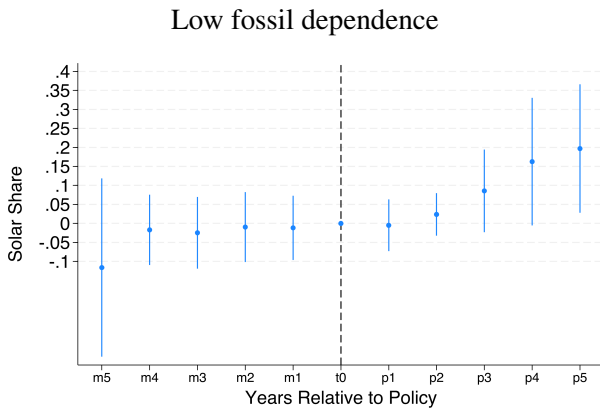


Figure A3a

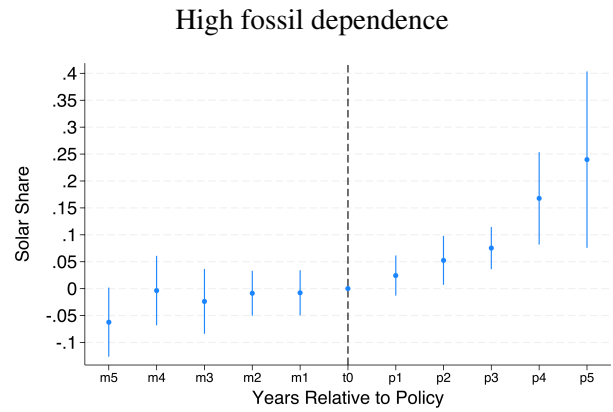


Figure A3b

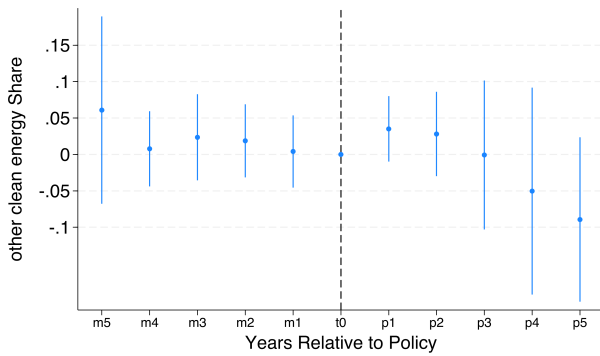


Figure A3c

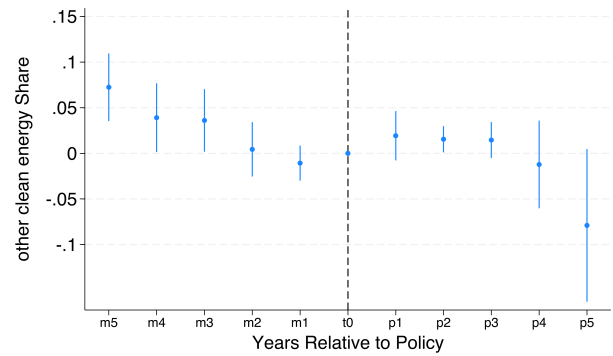


Figure A3d

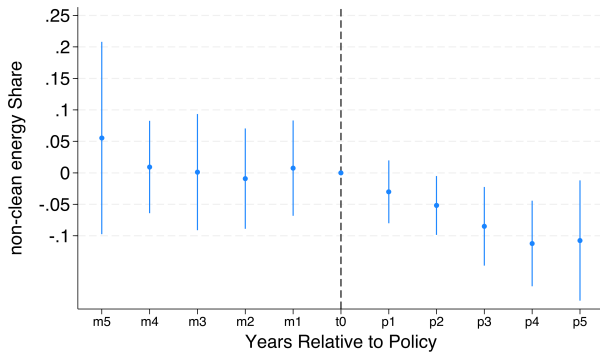


Figure A3e

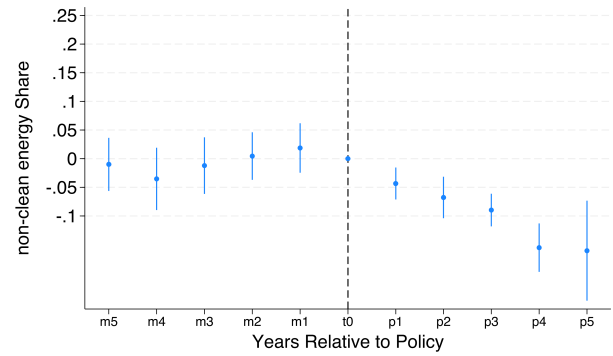


Figure A3f

Notes: Rows show solar, non-solar clean, and non-clean shares; columns show low and high fossil dependence. Fossil dependence is state coal, gas, and oil generation divided by the eight-fuel generation total, excluding other generation. Baseline averages use the focal cohort's preceding three years, requiring two valid years. Low/high groups use the common unweighted median across eight treated states, with ties assigned low. Both treated and comparison states are split; membership is fixed within each stack. Each path is a separate stacked event-study regression of utility generation shares. Non-solar clean comprises hydro, waste, and wind; non-clean comprises coal, gas, nuclear, and oil. Coefficients are relative to the omitted adoption-year treated-comparison contrast. Endpoints pool years ≤ -5 and ≥ 5 . Bars show 90% confidence intervals. Controls are log state population, log real GDP per capita, irradiance anomaly, and heating and cooling degree days. Models include state-by-cohort and year-by-cohort fixed effects, with standard errors clustered by state-by-cohort, year-by-cohort, and utility.

Appendix Figure A4 Event Study Estimates by Baseline Total State Generation

Low state generation

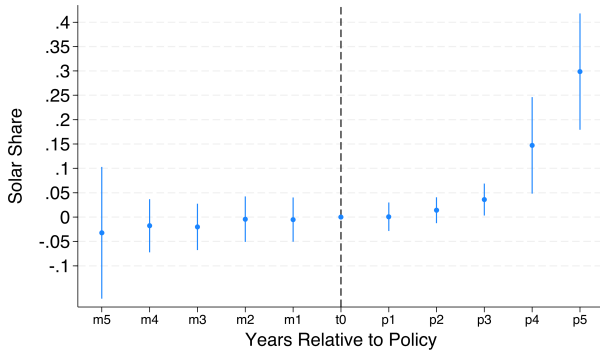


Figure A4a

High state generation

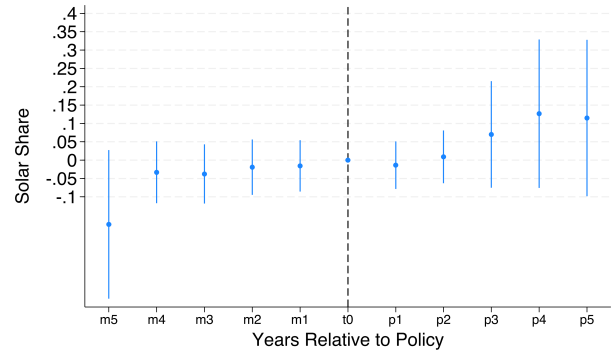


Figure A4b

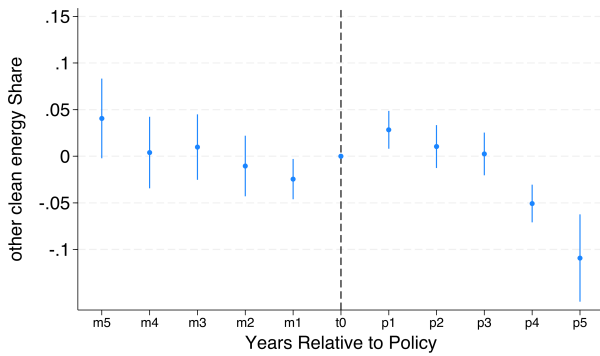


Figure A4c

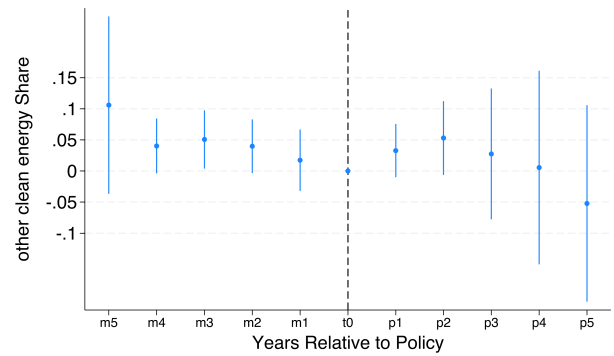


Figure A4d

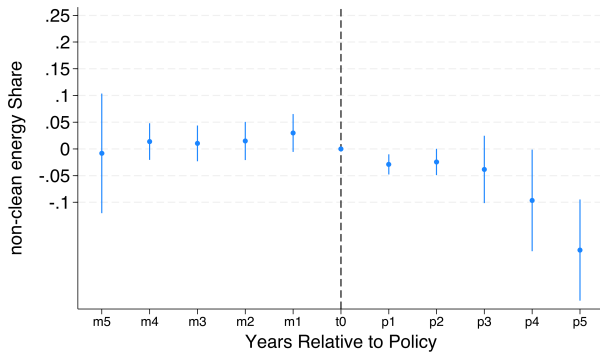


Figure A4e

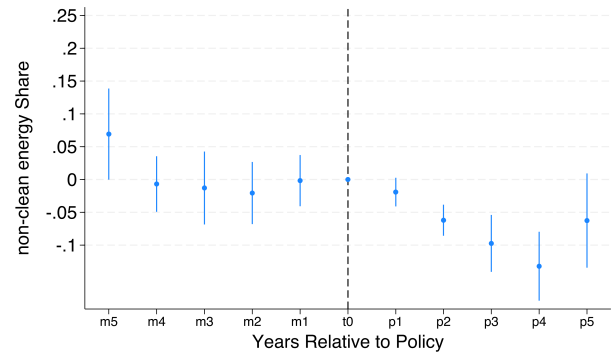


Figure A4f

Notes: Rows show solar, non-solar clean, and non-clean shares; columns show low and high state generation. Size is annual total state generation observed in the project utility panel, measured in MWh. Baseline averages use the focal cohort's preceding three years, requiring two valid years. Low/high groups use the common unweighted median across eight treated states, with ties assigned low. Both treated and comparison states are split; membership is fixed within each stack. Each path is a separate stacked event-study regression of utility generation shares. Non-solar clean comprises hydro, waste, and wind; non-clean comprises coal, gas, nuclear, and oil. Coefficients are relative to the omitted adoption-year treated-comparison contrast. Endpoints pool years ≤ -5 and ≥ 5 . Bars show 90% confidence intervals. Controls are log state population, log real GDP per capita, irradiance anomaly, and heating and cooling degree days. Models include state-by-cohort and year-by-cohort fixed effects, with standard errors clustered by state-by-cohort, year-by-cohort, and utility.

Appendix Figure A5 Event Study Estimates by Solar-Resource Potential

Low solar-resource potential

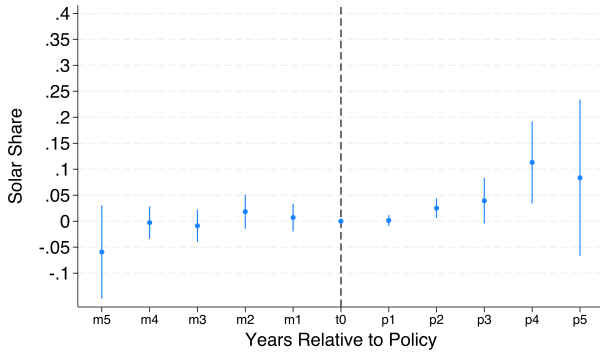


Figure A5a

High solar-resource potential

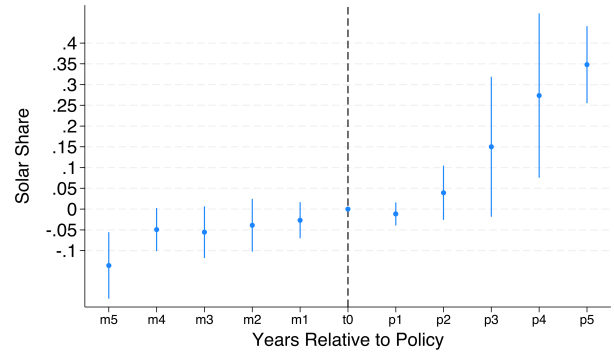


Figure A5b

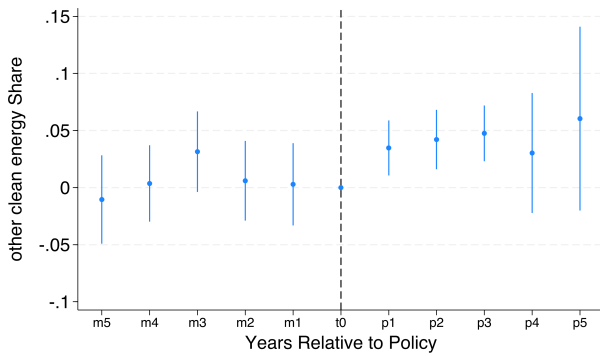


Figure A5c

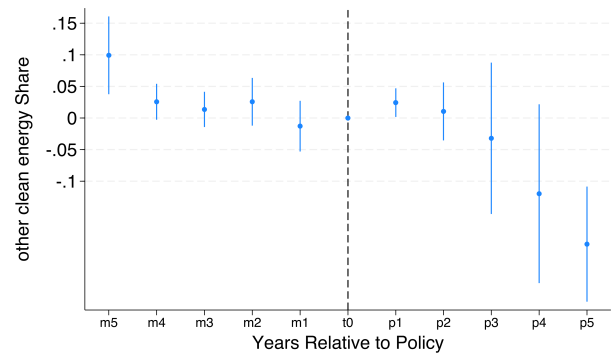


Figure A5d

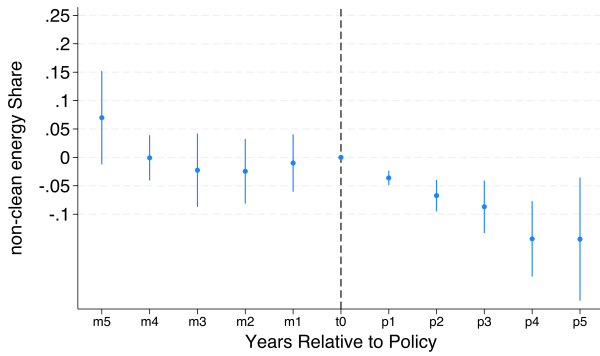


Figure A5e

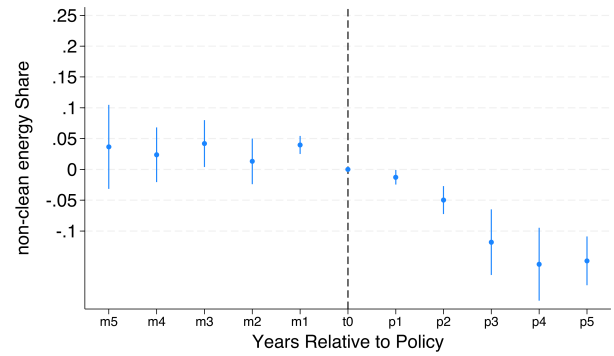


Figure A5f

Notes: Rows show solar, non-solar clean, and non-clean shares; columns show low and high solar-resource potential. Solar resources are average state irradiance over 2001–2004, requiring four valid annual observations. Low/high groups use the common unweighted median across eight treated states, with ties assigned low. Both treated and comparison states are split; membership is fixed within each stack. Each path is a separate stacked event-study regression of utility generation shares. Non-solar clean comprises hydro, waste, and wind; non-clean comprises coal, gas, nuclear, and oil. Coefficients are relative to the omitted adoption-year treated–comparison contrast. Endpoints pool years ≤ -5 and ≥ 5 . Bars show 90% confidence intervals. Controls are log state population, log real GDP per capita, irradiance anomaly, and heating and cooling degree days. Models include state-by-cohort and year-by-cohort fixed effects, with standard errors clustered by state-by-cohort, year-by-cohort, and utility.

Appendix Figure A6

Event Study Estimates by Preexisting Non-solar Clean Generation

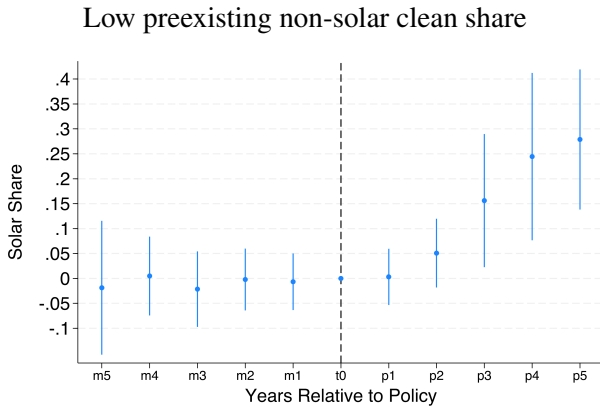


Figure A6a

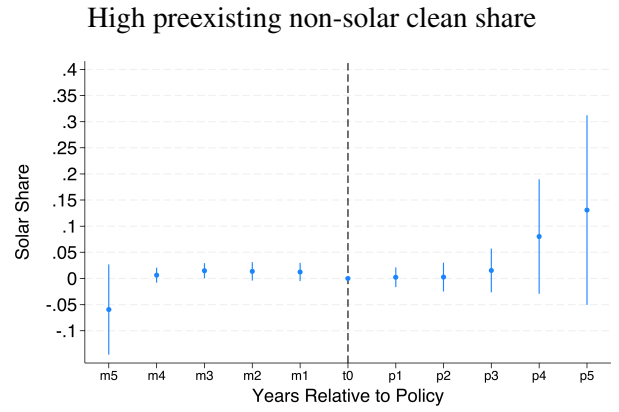


Figure A6b

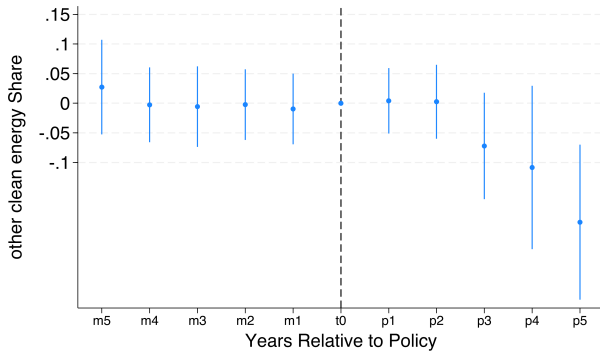


Figure A6c

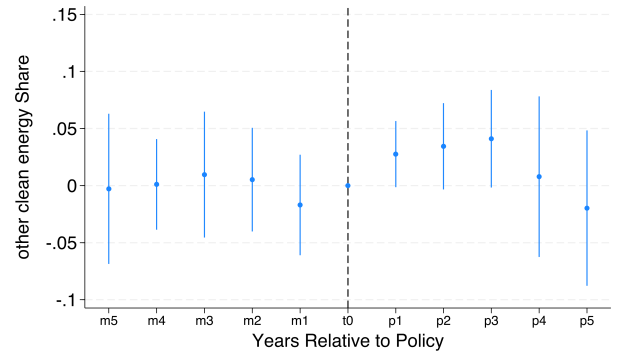


Figure A6d

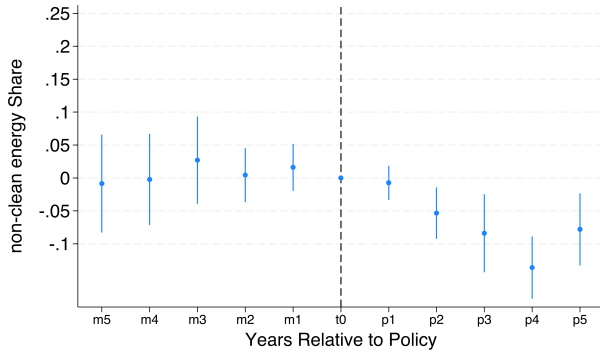


Figure A6e

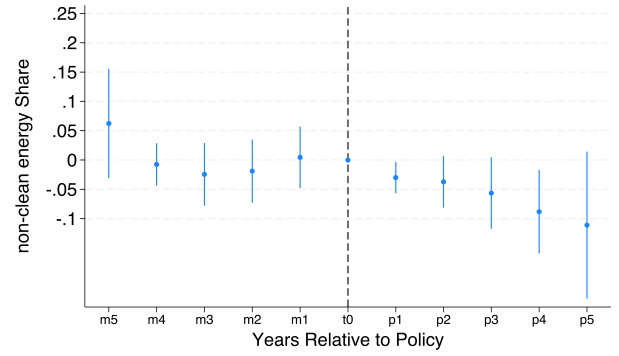


Figure A6f

Notes: Rows show solar, non-solar clean, and non-clean shares; columns show low and high preexisting non-solar clean shares. The baseline is state hydro, waste, and wind generation divided by the eight-fuel generation total, excluding other generation. Baseline averages use the focal cohort's preceding three years, requiring two valid years. Low/high groups use the common unweighted median across eight treated states, with ties assigned low. Both treated and comparison states are split; membership is fixed within each stack. Each path is a separate stacked event-study regression of utility generation shares. Non-solar clean comprises hydro, waste, and wind; non-clean comprises coal, gas, nuclear, and oil. Coefficients are relative to the omitted adoption-year treated–comparison contrast. Endpoints pool years ≤ -5 and ≥ 5 . Bars show 90% confidence intervals. Controls are log state population, log real GDP per capita, irradiance anomaly, and heating and cooling degree days. Models include state-by-cohort and year-by-cohort fixed effects, with standard errors clustered by state-by-cohort, year-by-cohort, and utility.

Appendix Figure A7 Event Study Estimates by Initial Market Openness

Local-only markets

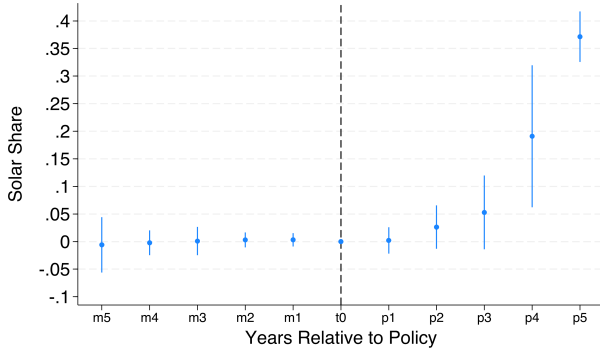


Figure A7a

Outside supply permitted

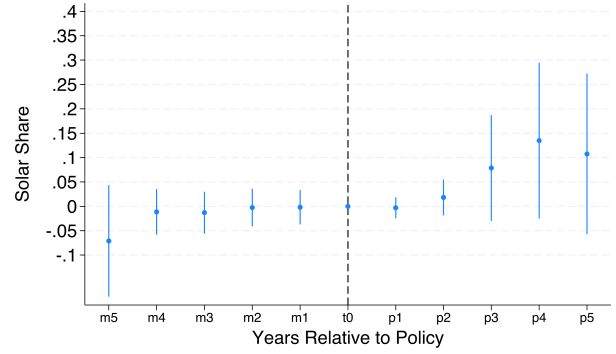


Figure A7b

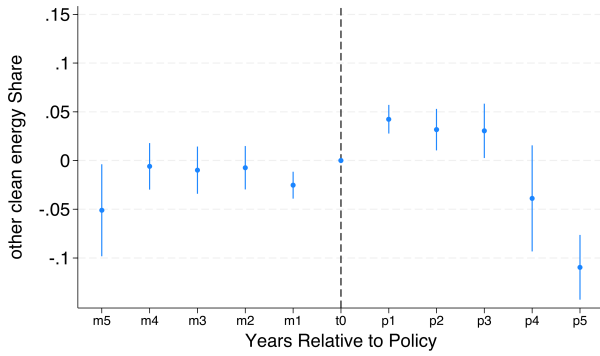


Figure A7c

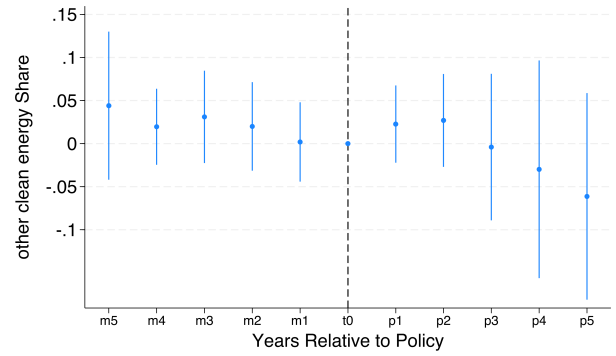


Figure A7d

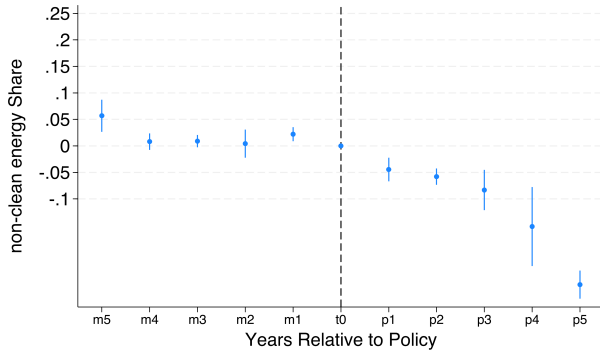


Figure A7e

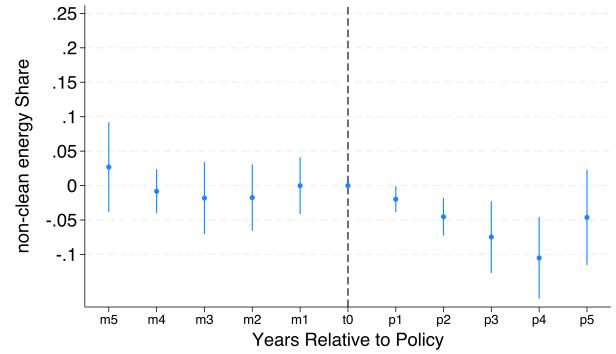


Figure A7f

Notes: Rows show solar, non-solar clean, and non-clean shares. Left: local-only markets (Massachusetts and New Jersey). Right: outside supply permitted. Initial compliance-year rules for new solar generation define fixed groups. Each group uses the usual eligible comparison pool; other focal treated groups are excluded. Estimates describe heterogeneity by initial market design. Each path is a separate stacked event-study regression of utility generation shares. Non-solar clean comprises hydro, waste, and wind; non-clean comprises coal, gas, nuclear, and oil. Coefficients are relative to the omitted adoption-year treated-comparison contrast. Endpoints pool years ≤ -5 and ≥ 5 . Bars show 90% confidence intervals. Controls are log state population, log real GDP per capita, irradiance anomaly, and heating and cooling degree days. Models include state-by-cohort and year-by-cohort fixed effects, with standard errors clustered by state-by-cohort, year-by-cohort, and utility.