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Causal Effects of Renewable Portfolio Standards on Renewable Investments and Generation: The Role of Heterogeneity and Dynamics
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ABSTRACT

Despite a 30-year long history, Renewable Portfolio Standards (RPS) remain controversial and debates continue to surround their efficacy in leading the low-carbon transition in the electricity sector. Contributing to the ongoing debates is the lack of definitive causal evidence on their impact on investments in renewable capacity and generation. This paper provides the most detailed analysis to date of the impact of RPSs on renewable electricity capacity investments and on generation. We use state-level data from 1990-2019 and recent econometric methods designed to address dynamic and heterogeneous treatment effects in a staggered adoption panel data design. We find that, on average, RPS policies increase wind generation capacity by 600-1200 MW, a 44% increase, but have no significant effect on investments in solar capacity. Additionally, we demonstrate that RPSs have slow dynamic effects: most of the capacity additions occur 5 years after RPS implementation. Estimates for wind and solar electricity generation mimic those for capacity investments. We also find similar results using an alternate treatment definition that allows states to meet their RPS requirements with pre-existing renewable generation and renewable generation from nearby states.

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1 Introduction

Most industrialized countries now have commitments, or in a few cases laws, with targets to reach carbon neutrality status by 2050 or 2060. A central strategy to reach this target common across countries is the decarbonization of the electricity generation sector through expanding renewable resources. In the United States, the renewable portfolio standards (RPS), a state-level policy imposing standards for renewable electricity sales in a state, is one of the most prominent policies implemented to date with the goal of incentivizing decarbonization of the electricity sector. Beginning with Iowa in 1991, thirty states and Washington D.C have now enacted RPSs; these states represent more than 70 percent of the US population and 64% of total generation capacity in 2019.[1] As the U.S. federal government works toward its stated goal of 100% carbon-free electricity by 2035, many of the proposed federal policies mimic state-level RPS in how they displace fossil fuel use in electricity generation, reduce greenhouse gas emissions, and ensure reliable operation of the electrical grid.[2] Given the centrality of RPS to U.S. decarbonization goals, it is imperative to provide a better understanding how such policies affect the deployment of renewable electricity generation sources.

While RPSs have been designed and enacted to increase the share of renewable electricity supplied and sold in states adopting them, there is still limited consistent empirical evidence about their efficacy and whether RPS cause investments in renewable capacity. Two key issues complicate the identification of the causal effect of RPS on renewables. First, RPS policies are not randomly assigned across states, and previous studies suggest that political ideology, underlying renewable resource potential, labor market conditions, and interest group pressure are strong predictors of RPS adoption (Lyon (2016)). Second, due to significant differences in policy design and renewable resource endowments, RPS policies are likely to have dynamic and heterogeneous effects across states and have been adopted in a staggered manner since the mid 1990s (see Figure 1 below).

Causal identification in this setting is complicated by difficult to quantify state-specific characteristics such as political ideology and natural resource endowment which may correlate with both RPS implementation and the deployment of renewable energy generation. Further, national-level policies that are correlated with RPS implementation, such as the U.S. federal government Production Tax Credit also create challenges for causal identification. To address these identification concerns, virtually all of the prior empirical literature on the impact of RPSs uses panel data regressions with state and year fixed effects, often labelled as two-way fixed effects (TWFE) or sometimes difference-in-differences (DD) models. Recent econometric research has shown that in settings with heterogeneous treatment effects (like in the case of the RPS policy adoption), TWFE or DD estimators identify a weighted average of treatment effect parameters which may not correspond to the overall average treatment effect on the treated (ATT) (Sun and Abraham (2020), de Chaisemartin

[1] To date, more than 70 proposals for a national portfolio standard have been introduced but none has become law (Congressional Research Service, 2020).

[2] https://www.whitehouse.gov/briefing-room/statements-releases/2021/04/22/fact-sheet-president-biden-sets-2030-greenhouse-gas-pollution-reduction-target-aimed-at-creating-good-paying-union-jobs-and-securing-u-s-leadership-on-clean-energy-technologies/

and D'Haultfœuille (2020), Borusyak and Jaravel (2017), Goodman-Bacon (2021)).

We address these challenges by using the most comprehensive data available on RPS policies and renewable electricity capacity investments and generation in the U.S. and present new evidence on the causal effect of RPS policies on renewable electricity capacity investments and generation by renewable resource using state-level data for 1990-2019. We exploit the long time-series of data, combined with the staggered timing of RPS adoption across states to derive heterogeneity-robust estimates of the causal effect of RPS on renewables capacity and generation using the econometric methods recently developed in Callaway and Sant'Anna (2021). This approach, we believe, provides the first *source-specific* panel data evidence on the efficacy of RPS that is robust to treatment effect heterogeneity, a pervasive feature of such programs.

We find that, on average, RPS policies increase wind generation capacity by 600-1,200 MW but have no significant effect on investments in solar generation capacity. Relative to the average installed wind capacity in 2019 among ever-adopting states, the point estimates imply that wind generation capacity increased by 44% as the result of RPS, a sizeable increase. After modifying our empirical strategy to allow for treatment effect dynamics, we find that the impact of RPSs on wind capacity investments ramps up slowly: most of the capacity additions occur 5 years after RPS implementation. Overall our findings underscore the importance of accounting for dynamic responses to RPSs, of allowing for differential effects of RPSs on wind and solar investments, and of incorporating the most recent data available; since installed renewable capacity both increased by 40% (wind) and 164% (solar) between 2015 and 2019 alone.[3] As we explain below, these important considerations distinguish our paper from the previous literature.

Due to the now long history of RPS policies, dating back to 1991, and its significance for the electricity generation sector and for decarbonization goals, a sizable literature examines the impact of RPSs on renewable generation capacity investments, carbon emissions, and electricity prices. Overall, the evidence from the previous literature on the impact of RPS policies on the deployment of renewable electricity generation is mixed. Several studies (e.g., Shrimali et al. (2015) and Yin and Powers (2010)) find a positive relationship between RPS requirements (or compliance) and renewable electricity generation using a difference-in-differences type empirical strategy, and highlight the importance of controlling for state-specific features of both determinants and characteristics of policies across states. At the same time, other studies find little or no evidence of an effect of RPS policies and deployment of renewable generating capacity (e.g., Greenstone and Nath (2023), Fullerton and Ta (2022), Feldman and Levinson (2023), and Upton and Snyder (2017)). Notably most of this previous literature on RPS policies and the deployment of renewable electricity deployment implicitly relies on a staggered adoption empirical design with state and year fixed effects in an attempt to derive credible estimates.[4] As we argue below, the assumptions necessary

[3]Depending on the measure considered, the stringency of RPS requirements increased by 22% and 38% between 2015 and 2019.

[4]Some exceptions are Upton and Snyder (2017) who use the synthetic control method, Greenstone and Nath (2023) who use an estimator proposed by Sun and Abraham (2020) as a robustness check, and Feldman and Levinson (2023) who use an instrumental variables approach.

to lend a causal interpretation standard TWFE (DD) estimates from this previous literature are not valid in the setting RPS policy adoption.

Recent work by Hollingsworth and Rudik (2019) and Feldman and Levinson (2023) estimate the impact of RPSs on renewables deployment using an empirical framework that accounts for interstate sales of electricity via wholesale electricity markets. For each megawatt hour of electricity it generates, a renewable source creates one Renewable Energy Credit (REC). To achieve compliance with RPSs, utilities can purchase electricity from a renewable source (and its associated RECs) or directly purchase RECs which are sold separately from underlying electricity (commonly referred to as "unbundled" RECs). Because of interstate sales of RECs, RPSs may incentivize investments in renewables outside of the regulated state. Feldman and Levinson (2023) explicitly account for interstate trade by using states' net total in-state and out-of-state demand for RECs following the implementation of an RPS. Using an instrumental variables framework, Feldman and Levinson (2023) find that RPSs have an ambiguous impact on renewables investments.[5] As we discuss below, since renewables investments take time to occur, it is important to consider both interstate demand for RECs and dynamic effects when studying the impact of RPS policies.

Several recent papers also study the effects of RPS policies on electricity prices, emissions, and renewables deployment using analytical general equilibrium models (Bento et al. (2018), Fullerton and Ta (2022)). While these papers generally conclude that more stringent RPS policies unambiguously increase the price of electricity, they have ambiguous predictions for the effect of RPSs on renewables deployment.[6] Fullerton and Ta (2022) show that the effect on renewable capacity investments depends on state-specific transmission costs and natural resource endowments. For example, states with larger intermittent resource endowments (such as high wind class) may actually reduce renewable generation by increasing RPS stringency because the policy reduces demand for all electricity through higher retail prices. Thus a detailed empirical analysis is necessary to resolve the theoretical ambiguity.

We contribute to the empirical literature estimating the impacts RPS policies in four key ways. First, we bring in recent data on renewable capacity investments up to 2019 while other papers only consider the 2000s and the mid-2010s. The latter 2010s period (i.e., past 2015) is critical to properly measure the impact of RPS policies on renewables, since as Figure 1 shows, the net RPS requirement at the average state in the U.S. doubled between 2015 and 2019, reflecting dramatic increases in each state's standard. Thus, the post 2015 period meaningfully affects the impact of RPSs on renewable generation because it is a period where the intensity of each state's policy dramatically increases. Notably, the recent studies by Greenstone and Nath (2023) and Fullerton and Ta (2022) only consider data up to 2015 and so their estimates are likely biased downwards as a result.

[5] Finding an excludable instrument is difficult in this setting because it requires exogenous variation in the stringency of an RPS that is uncorrelated with state-specific, time-varying unobservables. Feldman and Levinson (2023) use a number of instruments in their analysis including (among other variables) out-of-state supply of RECs, sector-specific gross domestic product, and an indicator for which political party controls the state legislature.

[6] Using a coarsened exact matching algorithm, Wolverton et al. (2022) show that plants in RPS states faced electricity prices that were 2% higher than comparable plants in non-RPS states.

Second, unlike the recent literature, we analyze the impact of RPS policies on wind and solar separately. This distinction is important since pooling the analysis across solar and wind generation essentially confounds the marked differences in declining cost trends and innovation across solar and wind renewables in the U.S. (Wiser et al 2022; Bolinger, Seel, Warner, and Robson 2022). Our results clearly show the differential impacts of RPS policies on wind and solar investments.[7] Third, our analysis provides new insights by documenting the dynamic impacts of RPS policies in the longer-term, up to 11 years after policy implementation, which is made possible by our newly compiled data sets and longer time framework. Finally, our paper leverages the Callaway and Sant'Anna (2021) estimator that is robust to treatment effect heterogeneity in presence of staggered treatment adoption. Treatment effect heterogeneity is an innate feature of RPS programs due to differences in policy design and underlying renewable resource endowments in each state. An additional consideration that emerges from the recent econometric literature is that the standard TWFE/DD estimator may provide a biased estimate of the average treatment effect on the treated in presence of treatment heterogeneity. This is a critical concern in this setting since virtually all the previous literature uses DD methods, which calls into question the validity of the resulting empirical evidence.

The rest of this paper is organized as follows. Section 2 provides background on RPS policies and their implementation in the U.S. since 1991. Section 3 describes the data used in our analysis. Section 4 presents the empirical strategy and section 5 describes our results. Finally, section 6 concludes.

2 Details on RPS Programs in the United States

RPS requires retail electricity suppliers to provide a minimum percentage or amount of their retail load using eligible renewable electricity generation sources. Although RPS policies exist in 30 states and the District of Columbia as of 2021, their design differs significantly across states. Most significantly, minimum percentages or "targets" differ both in magnitudes and time frames across states. Furthermore, states differ in their eligibility requirements for existing renewable generation sources, exemptions for publicly owned utilities, enforcement mechanisms, incentives for specific renewable generation technologies, and compliance tracking systems. In the U.S., RPS policies apply to 58% of total retail electricity sales as of 2021 (Barbose (2021)).

Figure 1 shows the history of RPS adoption over time. While Iowa became the first state to adopt a mandatory RPS in 1991, most RPS states implemented their programs between 2000 and 2009. Typically, each state's annual percentage requirement increases gradually over time until it reaches its mandated goal. For example, California's RPS mandates that 60% of retail electricity sales come from renewable generation sources by 2030 and has interim targets of 44% by 2024 and 52% by 2027 (DSIRE (2021)). These time-varying targets within adopting states underscore the importance of examining the dynamic effects of the policy. Figure 2 plots the mean, 95th

[7] Feldman and Levinson (2023) estimate the effects of RPSs on wind and solar investments separately, but do not estimate dynamic effects which are important for understanding changes in investment behavior.

percentile, and 5th percentile of observed statutory RPS targets across all RPS states in the U.S. between 2000 and 2019. The statutory RPS targets have increased over time as more states adopt RPS policies and update existing legislation. While the average RPS target in 2000 was near 0 percent, the average statutory RPS target in the U.S. exceeded 20 percent in 2019. Although the RPS percentage requirement for each state may appear stringent, the effective standard may be much lower because some states allow existing renewable generation to qualify for compliance. For example, although California's standard was 20% of total retail electricity sales in 2010, it's effective standard was approximately 17% of sales after accounting for eligible existing generation. Such variation in how "constraining" RPS mandates are may introduce lags between the time a policy is first adopted and the time detectable impacts on renewable investments incentivized by the policy are made. Consequently, we focus on estimating the causal effect of implementing any RPS legislation on renewables deployment using a method that accounts for potential dynamic impacts, and also consider variation in standard stringency in a robustness analysis.

While all states with RPS policies mandate that a share of retail electricity sales come from renewable generation sources, they often differ in what sources are considered renewable. The list of designated technologies always includes wind and solar electricity generation, but often states differ in their classification of sources such as hydroelectric and nuclear generation as renewable. Furthermore, some states such as California exempt publicly owned utilities from the RPS standard, while others such as Colorado set separate, lower standards for publicly owned utilities.

States further differ in how the RPS policy encourages renewable development. Some states mandate that a certain percentage of the renewable generation used to comply with the RPS policy come from specific technologies. For example, Delaware's solar carve-out currently stipulates that solar generating sources comprise at least 2.25% of renewable generation used for RPS compliance. Additionally, some states such as Delaware enforce RPS policies by charging a fee (typically termed an 'Alternative Compliance Payment') for each unit of renewable generation that would be required to bring a utility into compliance with the standard. Other states such as California allow regulators to levy financial penalties on non-compliant utilities.

Most states monitor compliance with RPS policies using RECs which certify that a given unit of electricity qualifies to meet the standard. Typically, RECs are issued by regional authorities that encompass multiple states and issue a unique serial number for every megawatt-hour of generation produced by registered compliant generators. While some trading of RECs may occur across regions, most RECs used for RPS compliance occurs within a region.

As this brief overview highlights, RPS policies may appear straightforward, but in practice there is a large degree of heterogeneity across states in how they are implemented. This complexity requires sophisticated econometric methods in order to identify causal effects, as we demonstrate below.

3 Data and Preliminary Analysis

In order to estimate the impact RPS policies on the deployment of utility-scale renewable electricity generation installations, we compile a state-level panel data set on the relevant outcomes, policy variables, and predictors of renewable investments (Table A1). While many of the underlying data are recorded at the sub-state level (e.g., the county where wind turbines are located), we organize all the data at the state-level given that RPS policies are implemented by states. This section describes the data sources and presents summary statistics and preliminary analyses.

Figure 1 illustrates the timing of RPS policy adoption across states, focusing on the continental U.S. using data from Barbose (2021). This adoption will constitute the primary treatment indicator we consider in the empirical analysis. Each box represents a year, and are marked in black once a state adopts the policy. For example, Alabama has yet to adopt an RPS policy, while Arizona enacted it in 2002. By the end of 2019, 27 states had enacted RPS policies, with Iowa being the earliest adopter (1992) and Vermont being the latest (2015). Since no state has disadopted these policies during our sample period, there is a large degree of autocorrelation in the 'treatment status', which we address using cluster-robust inference in the empirical analysis.

Data on operating capacity by source is obtained from the Energy Information Administration (EIA) Form 860, which contains generator-level information at electric power plants with at least 1 MW of combined nameplate capacity. For this study, we use information on installed capacity in wind and solar, which we complement with the same information for coal or gas units (all recorded in MW). Importantly, Form 860 includes information on all operable generators in a given year, as well as the list of retired generators (along with their year of retirement). For operable and retired generators we observe the first year of operation, which allows to reconstruct a complete history of the total cumulative installed capacity (henceforth 'installed capacity') over time, by source (wind, solar, coal, and gas) from 1990-2019.

Figure 3 reports the national trends in installed utility-scale wind and solar electricity capacity. The deployment of capacity for both renewable resources follows a similar pattern, with wind installations beginning to emerge in the early 2000s, while utility-scale solar takes off around 2010. Growth in capacity appears roughly linear, reaching 100,000 MW for wind and 38,000 MW for solar by the end of the sample period in 2019. Many factors have contributed to the diffusion of these renewable technologies in addition to RPS policies, including reduction in levelized costs of operation, and federal and state-level production tax credits and other localized incentives (Hitaj 2013). The econometric methods detailed below are designed to control for the influence of those other factors.[8]

We also analyze the impact of RPS adoption on actual generation of electricity by source. Data on generation are obtained from EIA Form 906 which reports annual data on generation at the

[8]We note that utility investments in renewables reflects a large series of factors, including federal incentives (such as the Production Tax Credit), state incentives (such as RPS and tax credit programs), and local factors (such as generation potential, infrastructure, and economic conditions). Based on that, RPS policies can only explain a limited share of the variation in renewable capacity across states.

power plant level. Other auxiliary data sources are described in the Data Appendix. Table 1 presents baseline (1990) summary statistics tabulated for the 30 states that adopted an RPS policy during the period 1990-2019 and the 19 states that never adopted an RPS.[9] Columns (1) and (2) report sample averages while Column (3) reports the RPS state minus non-RPS state difference in means, with stars indicating statistical significance testing the null hypothesis of "no difference" based on an OLS linear regression with standard errors clustered by state. Panel A shows that on average, RPS states have marginally better infrastructure and wind speed endowments, with an additional 0.02 km of transmission per square km of state area, and average wind speed that is 0.2 meters per second higher. Solar irradiance, measured in kWh per square meter per year is a measure of total energy received from sun and a key determinant of solar electricity potential. The data in Table 1 indicates that solar irradiance is weakly smaller in RPS states. The small magnitude of the differences reported in Panel A and the lack of statistically significant differences indicate that natural resource endowments do no appear to be a key driver of RPS policy enactment.

Panel B shows (as expected) that RPS states had higher levels of wind and solar capacity installed in 1990, on average, than non-RPS states, although the differences are not statistically significant. On average in 1990, total installed wind capacity was 26 MW in states that ever-adopted an RPS, compared to 0 MW for states that never adopted the policy. At the same time, we note that coal capacity was lower in states that adopt RPS policies, while gas capacity was higher. These differences in capacity by source are mirrored in the average generation by source in Panel C. RPS states produced more renewable electricity, less coal-fueled electricity, and more gas-fueled electricity on average in 1990, but none of the differences are statistically significant.

Panel D reports sample averages for various potential predictors of investments in renewables in 1990, including state-level GDP per capita, state-level electricity price and consumption, and League of Conservation Voters (LCV) scores for each state's senator and house of representative members.[10] This correlational analysis reveals marked differences between states adopting RPSs and states never adopting them. GDP per capita was notably higher in RPS states in 1990. Electricity prices were also higher in RPS states, by $0.02 per kWh on average, as is total electricity consumption.[11]. Not surprisingly, RPSs are more likely to be adopted in states that scored higher in the LCV score index; the RPS - non-RPS difference is roughly 30 points and statistically significant. Since states that ever adopt RPS legislation differ from those that never adopt on a number of important observable margins, our preferred estimates will use not-yet-treated states in addition to never-treated states as a control group. We test the sensitivity of our estimates to this choice in the robustness analysis.

[9] Our sample of 49 units always excludes Alaska and Hawaii, but includes the District of Columbia which adopted an RPS in 2006.

[10] We obtain annual LCV scores between 1993 and 2013 for each state from Hollingsworth and Rudik (2019) and annual scores between 2014 and 2019 directly from the LCV website. The LCV describes its scoring methodology in the following way: "Annual scores are based on a scale of 0 to 100 and calculated by dividing the number of pro-environment votes cast by the total number of votes scored except for excused absences."

[11] Prices are in adjusted to 2019 dollars and represent the electricity price for all end use sectors

4 Empirical Approach

4.1 Estimating Impact of RPSs with Staggered Adoption and Treatment Effect Heterogeneity

The primary goal of this paper is to estimate the causal effect of RPS policies on the deployment of renewable electricity capacity investments and generation using a staggered adoption research design. In order to estimate the impact of RPS policies on the various outcomes of interest, the previous literature has typically used a difference-in-differences design with a two way fixed effects (TWFE) estimator with state and year fixed effects (Yin and Powers (2010), Shrimali et al. (2015), Hollingsworth and Rudik (2019), and Greenstone and Nath (2023)). The canonical regression equation for such models is:

$$y_{it} = \beta RPS_{it} + X_{it}'\theta + \gamma_i + \delta_t + \varepsilon_{it} \tag{1}$$

Where in the context our study, y_{it} denotes utility-scale wind or solar electric capacity installed (or generation) in state i at year t, RPS_{it} is a binary variable taking a value of one for all years following RPS implementation, and X_{it} is a vector of state-specific time varying control variables. The state fixed effects (γ_i) capture time-invariant characteristics of each state, such as underlying wind class, that determine renewable capacity installations and correlate with the probability that each state implements an RPS policy. Similarly, the year fixed effects (δ_t) control for annual shocks that are common to all states, and may be correlated with both renewable capacity installations and the probability of implementing an RPS policy. For example, the year fixed effects account for changes in the federal production tax credit, helping us to isolate the causal impact of RPS policies alone on the deployment of renewable electricity generation. The coefficient of interest, β is the average treatment effect on the treated (ATT) of an RPS policy on the outcomes (utility-scale wind and solar capacity and generation).

OLS estimation of equation 1 is straightforward. However, recent advances in econometric research show that, in the presence of treatment effect heterogeneity (i.e., where β can vary over time or across cross-sectional units), the standard TWFE estimator identifies a weighted average of group-time specific treatment effects which may not correspond to the overall ATT (Sun and Abraham (2020), de Chaisemartin and D'Haultfœuille (2020), Borusyak and Jaravel (2017), Goodman-Bacon (2021)). As explained earlier, due to important differences in RPS policy design across states and advancement in renewable generation technologies over time, it is reasonable to expect sizable treatment effect heterogeneity in this setting. For example, California's initial RPS target was 11.85% for all utilities while Missouri's was 2% and included a carve out for solar electricity generation.

The recent econometric literature shows that the ATT estimated by the standard TWFE/DD regression model includes already treated units in the control group. As a result, differences in outcome trends over time between the treatment and control group may be driven by dynamic treatment effects in already treated units. This issue could resolved by assuming homogenous treatment effects over time, this assumption is implausible in the context of RPSs. Since RPSs vary widely both across states and in how their intensity varies over time, there is reason to expect that the standard TWFE/DD regression is unlikely to identify the ATT and may even have a different sign than the true ATT.

In order to address the issues with the TWFE estimator, we use the estimator proposed by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) to estimate the impact of RPS policies. While several different estimators which are robust to treatment effect heterogeneity have been developed, the Callaway and Sant'Anna (2021) estimator is well suited to staggered adoption research designs with a binary treatment indicator as in our setting.[12] Furthermore, we employ the estimator proposed by Callaway and Sant'Anna (2021) because it provides a flexible framework for aggregating group-time specific treatment effect parameters into dynamic treatment effects.

In our setting, the estimator computes the treatment effect for each RPS adoption cohort, which we define as states that adopt an RPS in the same year, by differencing each cohort's outcomes in a post implementation year t with its outcome in the year prior to implementation (akin to the pre/post difference for the treatment group in standard DD estimation), and then computing the same difference for a control group that is not treated as of year t (akin to the pre/post difference for the control group in standard DD estimation). For example, $ATT_{g,t}$ denotes the average treatment effect on the treated for all states that implemented an RPS policy in year g at post-treatment time t relative to the year before treatment, $g-1$. The set of possible comparison groups for adoption cohort g could be all of the states that never adopt an RPS policy during the sample period or the set of states that never adopt an RPS *and* the states that have not yet adopted an RPS policy at year g. Our preferred estimates use the set of *not yet treated* and *never treated* states to construct the control groups because (as documented in Table 1), ever treated and never treated states differ on a number of relevant characteristics. However, estimates using only the never treated comparison group are similar to our main results, as shown in Table 4.

We implement this estimator using the outcome regression estimand proposed by Callaway and Sant'Anna (2021) because there is limited covariate overlap between RPS states and their not-yet-treated counterparts, which can lead to imprecise inference procedures when using inverse probability weighting and doubly robust estimators (Khan and Tamer (2010)). Our parameters of interest are the Average Treatment Effect on the Treated for RPS adoption cohort, g and event-time period t ($ATT_{g,t}$). Equation 2 presents the outcome regression estimand:

$$ATT_{g,t} = \mathbb{E}\left[\frac{G_g}{\mathbb{E}[G_g]}(Y_t - Y_{g-1} - \mathbb{E}[Y_t - Y_{g-1} \mid X, G_g = 0])\right] \tag{2}$$

Where G_g is an indicator variable equal to one if a state first implemented an RPS at period g, Y_t denotes the potential outcome at event-time period t, and Y_{g-1} denotes the potential outcome in period $g-1$, or the period directly preceding treatment. $ATT_{g,t}$ therefore compares the differential outcomes of states in adoption cohort g between period t and the period prior to

[12] See Sun and Abraham (2020), Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Strezhnev (2018), Ben-Michael et al. (2021), Imai et al. (2019), Borusyak and Jaravel (2017) for other proposed estimators.

RPS implementation to the same differential in states which are not yet treated by period g. In practice, the R did package estimates $\mathbb{E}[Y_t - Y_{g-1} \mid X, G_g = 0]$ for control states by regressing the difference in outcomes between period t and the period prior to policy implementation $(g-1)$ on time-invariant state specific characteristics and a constant separately for each year. Therefore our application of the Callaway and Sant'Anna (2021) estimator controls for unobserved time-invariant state specific factors (akin to a state fixed effect in the canonical TWFE model), and time effects common to all states (akin to year fixed effects in the canonical TWFE model).[13]

In our application, we allow the difference in outcomes described above to depend on controls for state-level endowment characteristics which previous research has suggested influence renewables deployment such as: wind potential (wind speed), solar irradiance, and total length of electricity transmission lines. Wind potential and solar irradiance capture a state's latent potential for renewable electricity generation, while the length of transmission lines measures potential grid access for renewable generation sources. Furthermore, we control for a set of baseline state level socioeconomic characteristics in 1990 (the first year in our sample) including gross domestic product (GDP) per capita, House and Senate League of Conservation Voting (LCV) scores, and electricity price per kilowatt hour of electricity. The House and Senate LCV scores rank representatives and senators based on their environmental voting record. We use these variables to capture the underlying degree of pro-environmental attitudes in each state.

To facilitate the interpretation of the empirical estimates, we summarize the $ATT_{g,t}$ parameters in 4 ways using the did R package from Callaway and Sant'Anna (2021). The parameter 'Overall ATT (cohort)' corresponds to the average effect of RPS policies experienced by all states that ever implement an RPS.[14] Similarly, we report the 'Overall ATT (year)' parameter, which corresponds to the average effect of implementing an RPS policy for states that have implemented an RPS for at least 11 years.[15] We chose 11 years because it allows us to examine the long run effects of RPSs for most of the treated states in our sample. This parameter first averages the heterogeneous effect of RPS policies across adoption-cohort groups within each time period for those states for which we observe at least 11 years of post-implementation data, before averaging these parameters across time periods. We also report the Overall ATT (year) parameter computed separately for post-implementation years 1-5 and 6-11. This provides a simple metric to gauge dynamic effects of RPS policies on renewable capacity investments and generation.

Estimation of the $ATT_{g,t}$ parameters in our setting relies on four assumptions. First, the data

[13] In particular, policies such as the federal production tax credit, which provides incentives for renewable generation investments at the national level, are accounted for as long as they affect the treated and control states at time t in the same way.

[14] Callaway and Sant'Anna (2021) recommend computing an overall ATT by first averaging the adoption-cohort, time specific treatment effects $\beta_{g,t}$ across post-implementation time periods for each cohort and then averaging the adoption cohort specific treatment effects.

[15] We chose to balance the panel of states when estimating the dynamic treatment effects because it prevents the estimate of the treatment effect from being driven by changes in the composition of the RPS ever-adoption group over time (Callaway and Sant'Anna (2021)). This sample restriction drops 2 of the 30 total RPS states (Iowa (1991) and Vermont (2015)) from the sample. These 2 RPS states represent a small fraction of treatment and outcome variation, accounting for 4% of total RECs demanded and 9% of total wind capacity investments during our sample period. We report estimates using an unbalanced panel of states (that includes Iowa and Vermont) in Table 4.

structure must be a panel or a repeated-cross section of states. Second, conditional common trends holds between the treated and not-yet-treated groups, conditional on covariates. Third, treatment follows a staggered adoption design (e.g., the treatment is binary, and never reverts back from "1" to "0"). Fourth, there is some overlap on baseline covariates between the treatment and control groups. Assumptions 1 and 3 are trivially satisfied in our setting since our sample consists of a balanced panel of states from 1990 to 2019 and we treat each RPS policy as irreversible.[16] While assumption 2 is impossible to formally test since it involves unobserved counterfactuals, we provide evidence that it is plausible by estimating pre-treatment period event study coefficients. Finally, to address assumption 4, we use the outcome regression estimand proposed by Callaway and Sant'Anna (2021) because there is limited covariate overlap between RPS states and their not-yet-treated counterparts, leading to imprecise inference procedures when using inverse probability weighting and doubly robust estimators (Khan and Tamer (2010)).

To conduct inference and compute standard errors, we use the multiplier bootstrap procedure described in Callaway and Sant'Anna (2021) which constructs simultaneous confidence intervals for the $ATT_{g,t}$ parameters. We cluster standard errors at the state level to allow for correlation of errors within each state over time.

4.2 Estimating the Impact of RPS Intensity

Since the binary treatment indicator ignores differences in RPS targets across states, we construct a continuous measure of treatment intensity measure following Feldman and Levinson (2023), Greenstone and Nath (2023), and Hollingsworth and Rudik (2019). Recall that utilities can comply with RPSs by generating renewable electricity themselves (creating RECs), purchasing renewable electricity (and associated RECs) from suppliers, or purchasing RECs that are unbundled from their underlying renewable electricity. Feldman and Levinson (2023) measure RPS intensity by calculating the total demand for RECs in each state. Since utilities can (in many cases) purchase RECs from out-of-state suppliers, total demand for RECs is composed of in-state and out-of-state demand.

Net in-state demand is the gross statutory RPS requirement less eligible renewable generation produced in the year before an RPS was passed.

$$Net\text{-}RPS_{it} = max(0, RPS_{it} - EligibleRenewables_{i,\tau i-1})$$

where the subscript i denotes a state, t a year, and τi is the year of RPS passage in state i. Since states cannot demand negative quantities of RECs, we assume that in-state demand for RECs is zero whenever states' eligible renewable generation in the year prior to policy enactment exceeds the RPS requirement.

[16] While an RPS could be reversible if states consistently meet their renewables generation targets, in practice states had zero net demand for RECs on 6 occasions (New Mexico in 2006 and Kansas in 2012, 2013, 2017, 2018, and 2019), supporting the irreversibility assumption.

Net out-of-state demand for RECs is the sum of the RPS goal in the states where state i can sell RECs to, less those states' contemporaneous renewables generation.

$$\textit{Net-Out-of-State-REC-Demand}_{it} = \sum_{j \in TP_i} max(0, RPS_{jt} - \textit{Renewables}_{jt})$$

where TP_i is the set of states to which state i is permitted to sell RECs. To identify TP_i for each state we use data on REC trading networks from Hollingsworth and Rudik (2019).[17] To calculate each state's total net REC demand, we then add its net in-state demand to its net out-of-state demand for RECs.

$$\textit{Total-Net-REC-Demand}_{it} = \textit{Net-RPS}_{it} + \textit{Net-Out-of-State-REC-Demand}_{it}$$

$\textit{Total-Net-REC-Demand}_{it}$ reflects state i's demand for RECs net of existing renewable generation from within its own borders and from states to which it can sell RECs. Since the estimator proposed by Callaway and Sant'Anna (2021) is specific to staggered adoption settings for a binary treatment, we create a discrete treatment indicator equal to one in all periods after $\textit{Total-Net-REC-Demand}_{it}$ exceeds its sample average level. Defining treatment in this way means that there could be anticipatory treatment effects if renewable generation capacity responds to below-average levels of $\textit{Total-Net-REC-Demand}_{it}$. We also consider an alternative treatment definition where we create a discrete treatment indicator equal to one in all periods after the first time that $\textit{Total-Net-REC-Demand}_{it}$ is positive.

Callaway et al. (2021) propose an alternative approach for non-binary treatments that could be used to estimate the relationship between RPS intensity and renewable generation investments while accounting for treatment effect heterogeneity. Since RPS intensity is a continuous treatment variable, Callaway et al. (2021) suggest estimating separate ATTs by cohort adoption group, event time period, and treatment dose, which is intractable in our setting: one would need to estimate separate ATTs for each RPS target changes by size and timing. Since most states revise their RPS targets at different dates and by different amounts during our sample period, the estimation of these separate ATTs is impractical and unlikely to be informative due to data demands that cannot be met in our setting. We therefore report estimates of the effect of RPS intensity using the binary treatment approach described above.

5 Results: Impact of RPS Policies

5.1 Wind Capacity and Generation

The empirical analysis begins by analyzing the impact of RPS implementation on wind electricity outcomes. Table 2 reports the results for installed wind capacity (Panel A) and wind generation (Panel B). The estimates in column (1) include no additional controls (besides the adoption

[17] Since Hollingsworth and Rudik (2019) collect data on REC trading networks between 1993 and 2016, we assume that these trading networks did not change from 1990 to 1993 and 2017 to 2019.

cohort and year fixed effects implicitly accounted for by the Callaway and Sant'Anna (2021) estimator). Column (2) adds the 'natural endowments' controls (wind potential, solar irradiance, and total length of transmission lines in the state, see Table 1 for details), and column (3) adds the 'socioeconomic' controls (GDP per capita, House and Senate League of Conservation Voting scores, and the price per kilowatt hour of electricity in 1990). Standard errors for all estimates are computed using a multiplier bootstrap method with clustering at the state level (Callaway and Sant'Anna (2021), Kline and Santos (2012), Belloni et al. (2017), Chernozhukov et al. (2018)).

The preferred estimates in column (3) indicate show that implementing an RPS policy increases installed wind capacity by 1,220 MW on average, across all states that ever adopted an RPS at any point during our sample period (Overall ATT (cohort)). This is a large effect, corresponding to 44% of the average installed wind capacity in 2019 among RPS states. Overall, across the estimates in columns (1) to (3), the size effect of the estimated ATT of RPS policy implementation ranges from 17% to 44%. Converting our preferred estimates to reflect the average percentage point change in the share of total capacity or generation from wind resulting from a 1 percentage point increase in the RPS target implies that the share of capacity increase by 0.41 percentage point.[18] The average impact of RPSs for the group of states for which we have at least 11 years of pre and post-implementation data (Overall ATT (year)) is of similar magnitude, implying that RPS policies lead to 713 MW in capacity additions. Decomposing the effect by post-implementation event time suggests that most of the increase in wind capacity investment occurs 6-11 years after RPS implementation (1,210 MW on average), as opposed to 241 MW in years 1-5. While all these estimates are positive, indicating that RPS policies were important contributor to the development of in-state wind electricity installation, it should be noted that the statistical significance is sensitive to the chosen specification, with the column (3) estimates generally being statistically different from 0 at the 5% significance level, while those in columns (1) and (2) typically are not.

The results for annual wind generation are shown in Panel B and generally mirror those for installed capacity. The Overall ATT (cohort) estimate is column (3) is 6,950 GWh while Overall ATT (year) is 3,720 GWh. The impact again is larger for years 6-11 after RPS implementation (compared to years 1-5). The ATT estimate for years 6-11 post-implement implies on average, RPS states increase annual wind generation by 6,490 GWh relative to their not-yet-treated counterparts. This effect corresponds to 176% of the mean wind generation and even 20% of mean coal generation among ever-adopting RPS states in 2019, again underscoring the importance of RPSs as drivers of renewables deployment. The statistical significance of the estimates of the impact of RPS policies on wind generation follows a similar pattern as those for wind capacity. The estimates in column (3), with the full set of natural endowments and socioeconomic controls are generally statistically significant the at the 5% level, while the column (2) estimates are qualitatively similar, but less precisely estimated.

Figures 4 and 5 present the unconditional dynamic treatment effects for wind capacity investments

[18] These results are consistent with estimates from Yin and Powers (2010) and Shrimali et al. (2015) who find that the share of electricity generated by renewables increases by 0.6 and 0.3 percentage points respectively.

and generation, respectively. Each point represents an event time-specific treatment effect which has been computed by averaging the group-time specific effects across adoption cohort groups, following the approach in Callaway and Sant'Anna (2021). Again, these are average effects for the subset of states for which we have 11 years of pre- and post-implementation data. We color-code the point estimates to reflect the pre-RPS adoption period (gray) and post RPS adoption period (orange). The corresponding 95% confidence intervals are represented by the length of the tickers. The pre-RPS adoption treatment effect estimates to the left of 0 on the horizontal axis are small and provide supporting evidence for the parallel trends assumption for wind capacity investments across the treated and control groups. In the case of wind generation (Figure 5), the pre-RPS adoption estimates also support the parallel trend assumption. The combined evidence in Figures 4 and 5 indicate that the estimates of the impact of RPS policies in Table 2 can be interpreted as credible estimates of the ATT of the policy.

The post-RPS adoption treatment effect estimates confirm the results in Table 2: RPS policies cause wind capacity investments and generation to increase in the post-policy adoption period.[19] Underscoring the dynamics in the impact of RPS adoption, the post-adoption point estimates are statistically significant at the conventional level only 5-7 years after RPS enactment for wind capacity investments and wind electricity generation. The estimates in the figure show the treatment effects appear to grow roughly linearly with post-adoption time. Importantly, through the 11 years of post-adoption data we have, the estimated impact of RPS on capacity investments and generation show no sign of reverting back to a null effect. This indicates that RPS policies created long-lasting change to the electricity sector of the states adopting them.

5.2 Solar Capacity and Generation

Next, we examine how RPS legislation has impacted solar capacity and generation. Table 3 is configured the same as Table 2 and presents the ATT estimates for solar capacity and generation in panels A and B respectively. While most of the estimates of the impact of RPS policies on solar capacity investments and generation are positive (as expected), they are smaller than their wind counterparts and vary in statistical precision across specifications. The preferred estimates in column (3) imply that, on average, implementing an RPS increases solar capacity by 155 MW in ever-adopting RPS states. The overall ATT (year) estimate similarly implies that wind capacity increases by 43 MW following RPS implementation. While statistically insignificant when all controls are included in the model, the estimated impact of RPS on solar electricity generation range between 676 and 902 GWh. As is the case with wind, much of the estimated RPS impacts on solar capacity additions and generation occur between 6 and 11 years after RPS implementation. The estimated standard errors for all estimates in column (3) are large relative to the ATT estimates such that the 95% confidence intervals for the RPS impact on solar energy all include zero.

Figures 6 and 7 display the estimated dynamic treatment effects for solar capacity and generation

[19] Note that the Overall ATT (year) estimates in Table 2 are just a weighted average of the event-time estimates from Figures 4 and 5.

respectively, computed in the same way as its counterpart in Figures 4 and 5. The pre-implementation estimates provide suggestive evidence that the parallel trends assumption holds between treated and not-yet-treated states. Furthermore, the post-implementation estimates (shown in orange) are small and indistinguishable from zero, confirming the results in Table 3.

Converting our preferred estimates to reflect the average percentage point change in the share of total capacity or generation attributable to solar resulting from a 1 percentage point increase in the RPS target implies that the share of capacity increases by 0.02 percentage point, although the lack of precision makes this conclusion tenuous. One likely explanation for the imprecise estimates of the effect on solar capacity and generation is that growth in solar capacity investment was limited prior to 2010. Figure 3 shows that while most of the wind capacity investment in the U.S. has occurred since 2000, similar increases in solar capacity investment did not meaningfully accumulate until 2010. This is consistent with evidence from Wiser et al. (2010) who suggest that wind generation proved more economically attractive and lower risk than solar in many regions of the U.S., leading to earlier investment in wind. For context, most solar electricity generation farms had installed capacity of 5 MW or less as of 2019 in the U.S.[20]

5.3 Robustness Analysis

We check the robustness of our estimates to a number of additional model specifications in Tables 4 and 5. Table 4 tests robustness to alternative control groups, sample selection, and treatment definitions, while Table 5 allows for anticipatory effects, alternative treatment cohort definitions, and presents the results from a TWFE estimator.

In Table 4, rows 3-6 focus on impacts on installed capacity (MW) and rows 7-10 focus on annual generation (GWh). All specifications control for both the natural endowment and socioeconomic covariates. Rows 1 and 2 replicate our preferred estimates of ATT (Year) from Tables 2 and 3, while all other rows depart from the baseline specification in one of four possible ways: (1) whether the control group is only the set of never treated states or the set of never and not yet treated states (as in the preferred estimates specification of Tables 2 and 3), (2) whether the panel of treated states is balanced for 11 pre- and post-treatment periods, (3) modelling the RPS policy as a binary indicator for net REC demand being above the sample average, and (4) modelling the RPS policy as a binary indicator for net REC demand being positive. Rows 3 and 7 replicate our preferred specification using the only control group of states that have never adopted an RPS policy with installed capacity and generation as outcomes, respectively. The results for wind using the never-treated group as the control are slightly larger than our preferred estimates, and remain statistically significant at the 95% confidence level. For solar energy, the estimates are virtually identical using the never-treated and not-yet-treated groups as control groups and the results remain statistically insignificant.

Rows 4 and 8 replicate the preferred estimates using an unbalanced panel of treated states rather than the subset of treated states for whom we observe 11 years of pre- and post-treatment

[20]Source: EIA (2019).

data. Using an unbalanced panel substantially increases the estimated treatment effect for both the capacity and generation outcomes. Consistent with the prior evidence, only the ATT estimates for wind are statistically significant at the 5% level. Callaway and Sant'Anna (2021) note that the estimates using the unbalanced panel of treatment units should be interpreted with caution since they may be driven by changes in the composition of treated units over time. The similarity of the estimates suggest this does not appear to be an issue in our setting.

Rows 5-6 and 9-10 use the treatment variable defined in section 4.2 which accounts for each state's net in-state and out-of-state demand for RECs rather than the policy implementation date. In general these estimates are qualitatively similar to the preferred estimates in rows 1 and 2. Rows 5 and 9 define treatment using a binary indicator equal to one for all years after a state's total net REC demand exceeds its sample average. Consistent with the dynamic effects shown in figure 4, we find effects that are larger in magnitude than those from our preferred specification. We find that after total net REC demand exceeds its average, wind capacity and generation increase by 1,120 MW and 5,620 GWh respectively. In rows 6 and 10 treatment is a binary indicator equal to one for all years after a state's total net REC demand is positive for the first time. The estimates imply that, on average, solar capacity increases by 114 MW and wind capacity by 671 MW after states face positive net REC demand. For generation, the results suggest once total net REC demand is positive, solar generation increases by 369 GWh and increases wind generation by 3,480 GWh. Similar to the results in Tables 2 and 3, the estimates for wind capacity and generation are typically more precise than those for solar capacity and generation.

Table 5 is organized similarly to Table 4, with rows 3-7 focusing on impacts on installed capacity (MW) and rows 8-12 focusing on annual generation (GWh). As in Table 4, all specifications control for both the natural endowment and socioeconomic covariates, and rows 1 and 2 replicate our preferred estimates of ATT (Year) from Tables 2 and 3. All other rows depart from the baseline specification in one of five possible ways: (1) allowing for 2 years of anticipatory effects, (2) dropping RPS states where net demand for RECs is 0 after RPS implementation, (3) grouping states into 3-year adoption cohorts rather than 1 year cohorts, (4) estimating a standard TWFE regression using OLS with net REC demand as the RHS variable of interest, and finally (5) estimating a TWFE regression using OLS with a binary indicator for RPS implementation as treatment.

Rows 3 and 8 modify our baseline empirical specification to allow for two years of anticipatory effects by computing the long difference in renewable capacity between year t and two years before RPS implementation. We find estimates that are slightly larger than in our baseline specification, suggesting that anticipatory effects do not meaningfully impact the baseline results.

Our baseline empirical specification relies on an assumption that treatment is irreversible, which could be violated if RPS targets are consistently nonbinding in the sample. To address this concern, rows 4 and 9 re-estimate the baseline specification after dropping any state that has net REC demand of 0 in any year following RPS implementation. For both capacity and generation, we find results that are similar to our baseline results, but lack precision.

Callaway and Sant'Anna (2021) suggest focusing on aggregated parameters (Overall ATT (year) and Overall ATT (cohort)) when cohort adoption groups include few units, pointing out that inference for the group-time ATT parameters may be unreliable. While this paper primarily focuses on these aggregate parameters, we also test the sensitivity of our results to grouping adoption cohorts into 3-year windows in rows 5 and 10. The estimates using these specifications are similar to our baseline results and increase slightly in precision.

Finally, rows 6-7 and 11-12 estimate the standard TWFE empirical specification using OLS with either net REC demand or a binary indicator for RPS implementation as treatment. Estimates using net REC demand lack precision and differ meaningfully from our baseline results, while those using the binary treatment indicator remain significant, but are much smaller that the baseline results. For example, the estimated relationship between RPS implementation and wind capacity investments from the TWFE model are 13% of the baseline result in this paper. This result highlights the importance of using panel data methods that are robust to treatment effect heterogeneity.

In summary, our results suggest that, on average, RPSs have played an important role in spurring wind energy investments (and generation) across the United States. The evidence for a causal effect on solar sources is weaker and generally statistically insignificant. It takes time for RPSs to impact installed wind capacity, with most additions occurring five or more years after policy implementation. As pointed out by Hollingsworth and Rudik (2019) and Feldman and Levinson (2023), RPSs can influence renewables investments in states that can sell RECs to RPS states.

6 Discussion and Conclusion

Renewable portfolio standards are the most prominent policy lever to stimulate investments in renewable electricity in the United States. Despite their more than 30-year long history, RPSs remain controversial and debates continue to surround their efficacy in leading the low-carbon transition in the electricity sector. This paper provides a careful evaluation of the impact of RPSs on renewable electricity capacity investments and generation, using recent panel data econometric methods suited for the analysis of staggered policy adoption with heterogeneous effects and the most up-to-date data available. These considerations are critical as they overturn results from the recent literature evaluating the impacts of RPS programs.

The results of this study point to 3 ways by which RPS legislation have changed the composition of electricity generation in the U.S. First, RPS legislation dramatically increased wind capacity investments and generation and this increase persists up to eleven years after policy implementation. Second, dynamic responses to the policy, which had not been fully analyzed in the previous literature are important: RPS policies take time to affect renewable capacity installations and generation, with much of our estimated effect occurring 6-11 years after the policy's initial implementation. Third, we find no evidence that RPS legislation had any effect on solar capacity investment or generation. One caveat on this last finding is that due to the timing of utility-scale solar deployment in the

U.S., our sample of data is not as well-suited to test the effect of RPSs on solar investments.

We can use our estimates to infer the contribution of RPS policies on total wind capacity installed (we ignore solar due to the small ATT estimates and the lack of statistically significant evidence). The estimated ATT of RPS on capacity 11 years post RPS implementation (relative to the year prior to implementation) is an increase of approximately 1000 MW (Table 2). Applying this estimate to the 29 states with RPS legislation as of 2019 implies that 29 GW, almost 30% of the current aggregate wind capacity is a result of RPS policies. While this is admittedly a simple and crude calculation, it nevertheless highlights the key role RPS played in developing the wind sector in the United States.

The empirical analysis also highlights the importance of explicitly accounting for the considerable heterogeneity in RPS legislation across states in empirical analyses. Amongst papers in the previous literature, our findings most closely resemble the results from Yin and Powers (2010) and Shrimali et al. (2015), both of which only find a positive effect on renewable generation after controlling for aspects that differ across states' RPS policies. This is a reassuring result that helps to reconcile the wide variety of prior estimates of RPS policies' impact on renewable generation. Our estimates also build on the prior literature by separately identifying RPS policies' effect on wind and solar generation. Despite evidence from Wiser et al. (2010) that wind generation was more economically feasible than solar in most regions of the U.S. prior to 2010, most prior research has grouped wind and solar generation together as an outcome.[21]

The U.S. and many other advanced economies are at a turning point where detailed and aggressive decarbonization plans are established. The Clean Energy Standard proposed by President Biden in 2021 shares many features with RPSs as they have been implemented by U.S. states since 1991. Taken together, the evidence presented in this paper indicates that a national Clean Energy Standard may promote investments in wind and solar production capacity and actual generation of renewable electricity. An important topic for future research is whether these investments will be sufficient for the energy sector to reach targets of zero emissions by 2035.

[21] Another working paper by Fullerton and Ta (2022), find no effect of RPS policies on generation from wind and solar power using the same estimator from Callaway and Sant'Anna (2021). One possible explanation for the discrepancy between our estimates and theirs is that they are not separately estimating the effect on wind and solar power.

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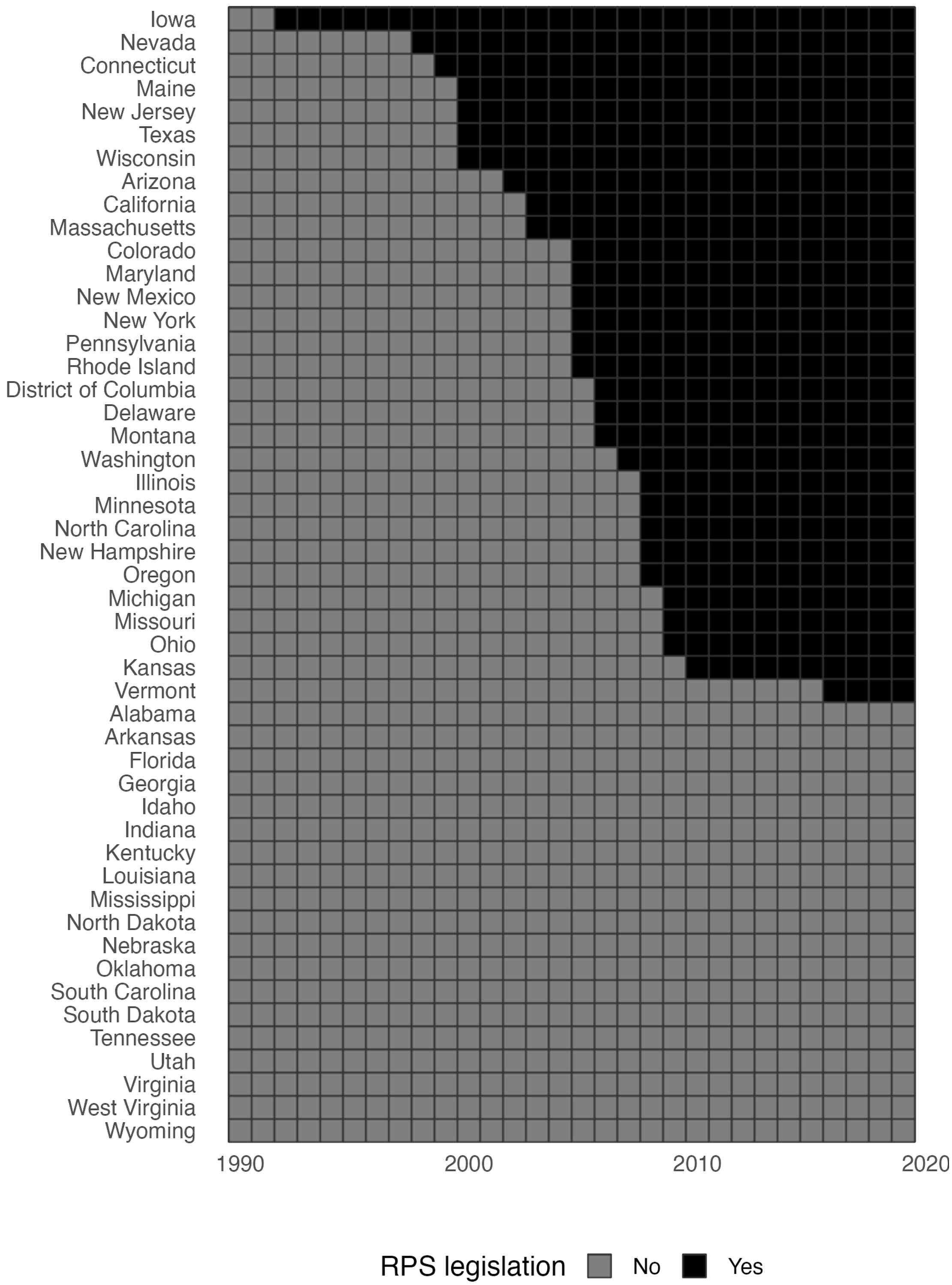
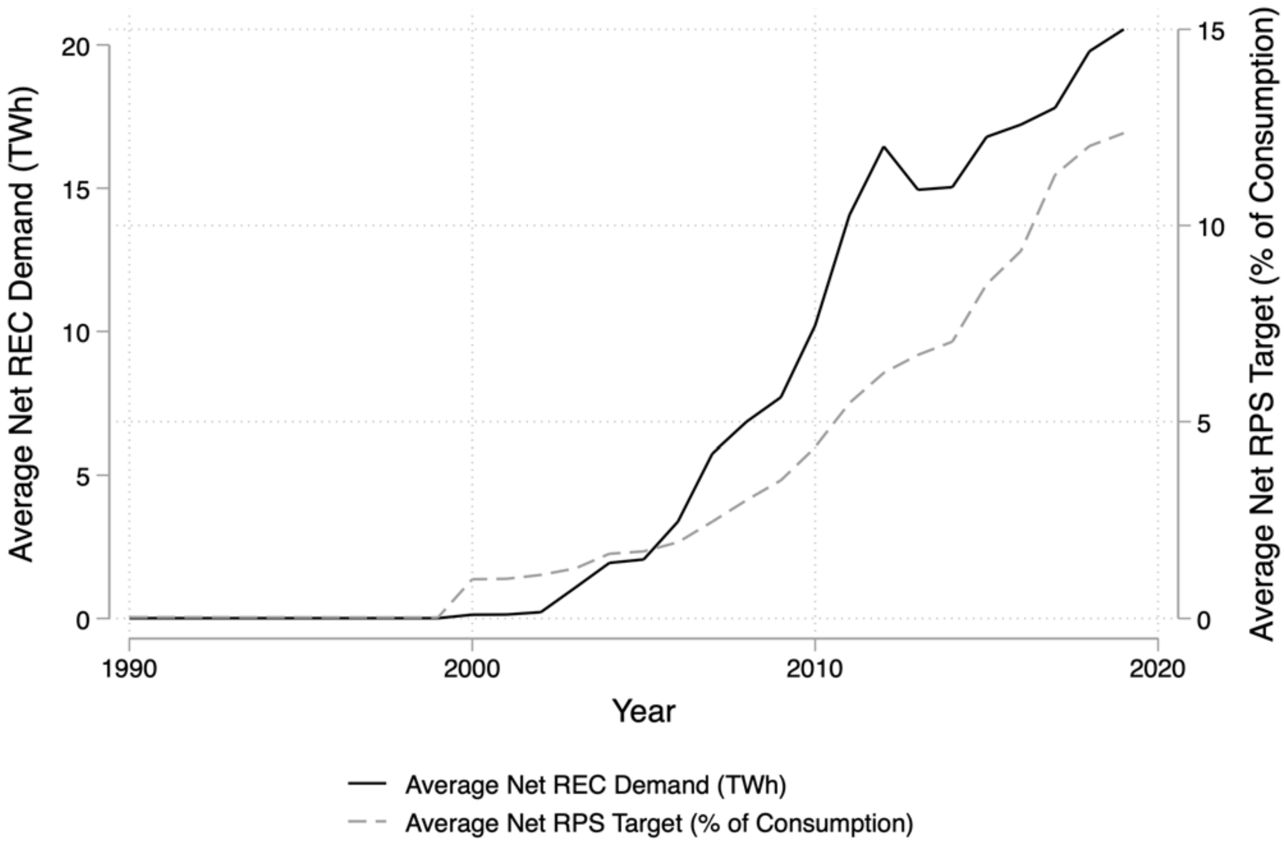
Figure 1: Renewables Portfolio Standard Status by State



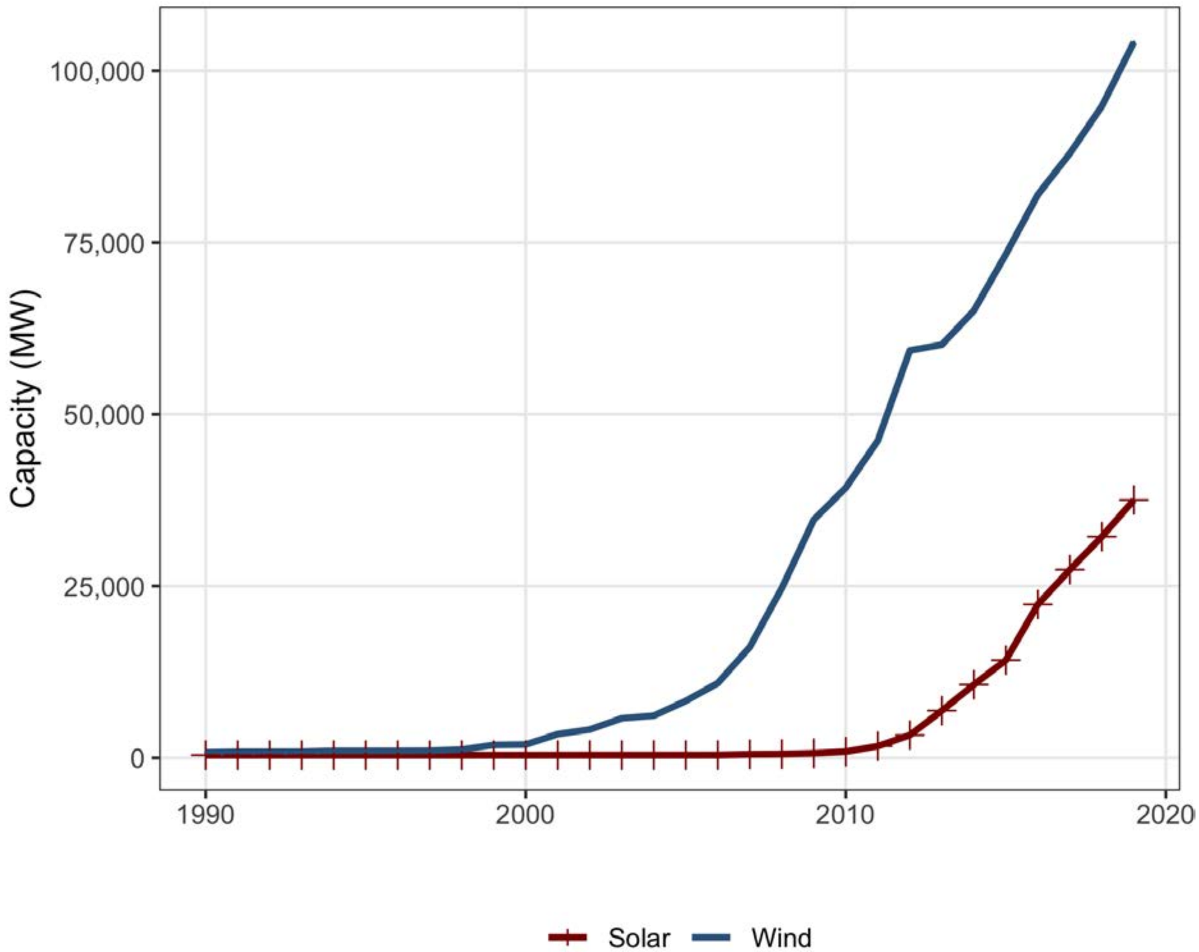
Notes: Each box is shaded black starting in the first year that a state adopts any RPS policy. Information on RPS adoption date was taken from Greenstone and Nath (2023).

Figure 2: Stringency of Renewables Portfolio Standard Over Time



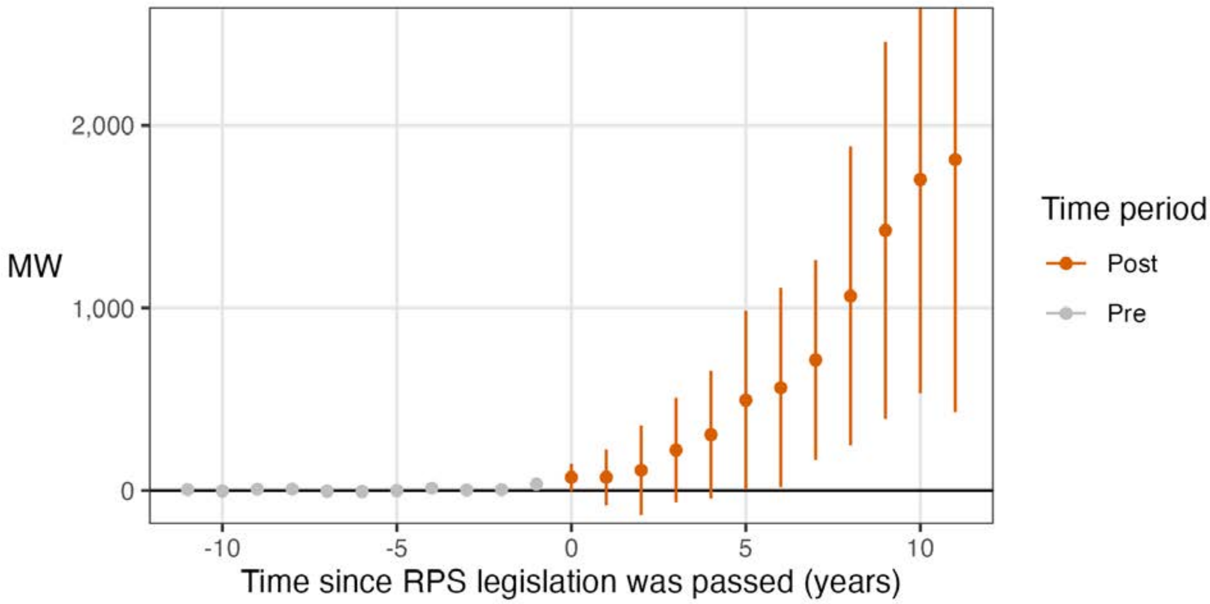
Notes: This figure shows the nominal RPS percentage targets over time based on data reported in Barbose (2021). Net RPS percentage targets measure the percent of applicable retail electricity sales required to be generated by renewable sources, net of preexisting renewable generation. Since electricity generators use Renewable Energy Credits (RECs) to satisfy their RPS requirement, RECs measure regulatory stringency. We construct net REC demand by taking total renewable capacity mandated under each state's RPS minus existing supply of RECs from existing in-state and out-of-state renewable generation sources. Since the definition of a renewable resource, type of regulated entity (e.g., public vs. privately owned utilities), and incentives for certain types of renewable generation differ considerably across states, comparison of targets across states is inadvisable.

Figure 3: Trends in Renewable Electricity Generation Capacity (MW)



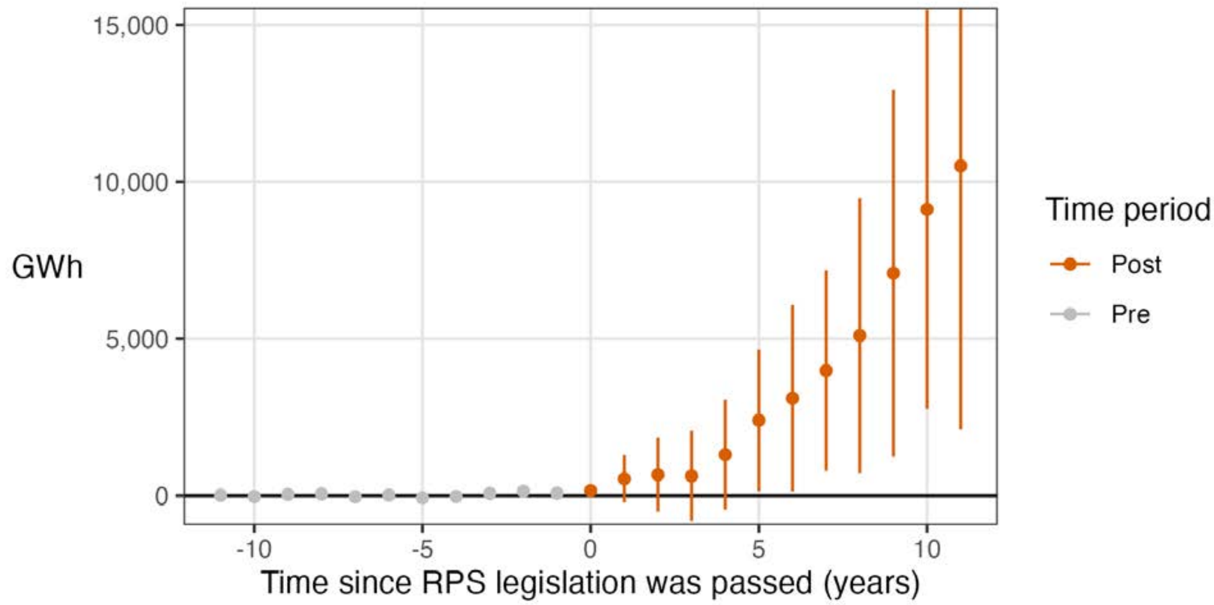
Notes: The blue (red with '+'s) lines plot the level of installed wind (solar) generation capacity in the continental U.S. annually between 1990 and 2019. Information on capacity installations by generation source was taken from the EIA Form 860 database.

Figure 4: Estimated Dynamic Treatment Effects of RPSs on Installed Wind Capacity (MW)



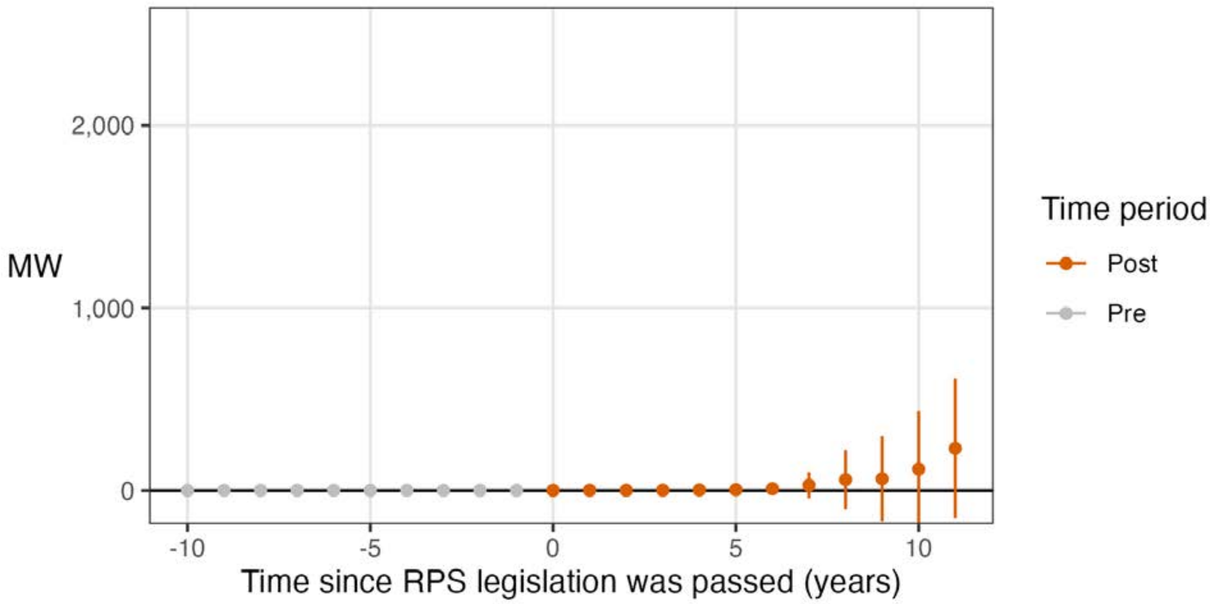
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. The model includes time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Figure 5: Estimated Dynamic Treatment Effects of RPSs on Wind Electricity Generation (GWh)



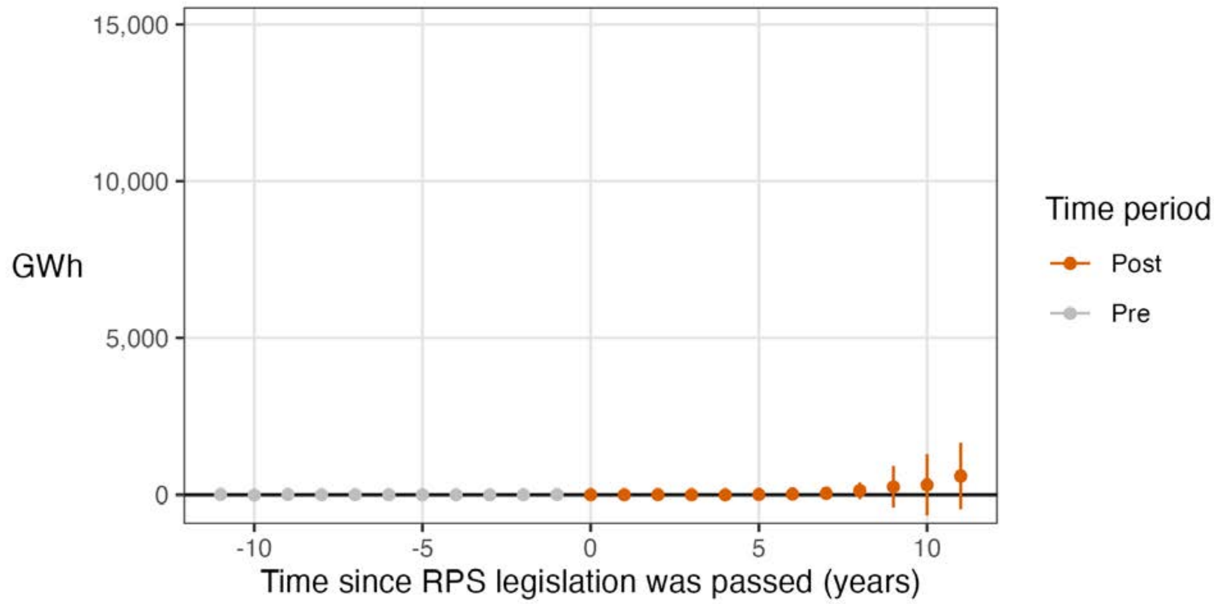
Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. The model includes time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Figure 6: Estimated Dynamic Treatment Effects of RPSs on Installed Solar Capacity (MW)



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. The model includes time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Figure 7: Estimated Dynamic Treatment Effects of RPSs on Solar Electricity Generation (GWh)



Notes: Each circle shows the estimated ATT averaged across treatment adoption cohorts and for each event-time period (t). The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the state level. The model includes time invariant controls for wind potential, solar irradiance, length of transmission lines per square kilometer of state area, 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price per kilowatt hour of electricity in 1990 at the state level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown in gray.

Table 1: Summary Statistics (1990)

	RPS states	Non RPS states	Difference
A. Infrastructure & Endowments			
Transmission lines (km per kmˆ2)	0.16	0.14	0.02
Wind speed (meter per second)	6.3	6.1	0.2
Solar irradiance (kWh / mˆ2 /year)	4.3	4.6	-0.2
B. Installed Capacity (MW)			
Wind	26.0	0.0	26.0
Solar	13.4	0.0	13.4
Coal	5,500.6	7,164.8	-1,664.2
Gas	4,182.7	2,873.30	1,309.4
Total	15,694.1	14,394.0	1,300.10
C. Generation (GWh)			
Wind	183	0	183
Solar	24	0	24
Coal	58,930	74,690	-15,761
Gas	18,468	9,714	8,754
Total	64,289	57,571	6,718
D. Other Predictors			
GDP per capita	42,278	34,145	8,132***
Electricity price (all end-use, $ / kWh)	0.12	0.10	0.02***
Electricity consumption (Bil. kWh)	59.6	46.3	13.4
House LCV score	62.1	41.7	20.4***
Senate LCV score	62.3	34.4	27.9***
Observations	30	19	

Notes: All summary statistics are for the year 1990. 'RPS states' are those that adopted any type of RPS legislation between 1990 and 2019 while 'Non-RPS' states are those that never adopted any type of RPS legislation. Only states in the continental U.S. are included in the sample. All dollar dominated variables are in 2019 constant dollars. Column 3 reports the mean difference between RPS and non-RPS states for each variable in 1990 and the stars indicate a significant difference across groups at the 0.05, 0.01, and 0.001 significance levels (*** p<0.001, ** p<0.01, * p<0.05).

Table 2: Estimated ATT of RPSs Impact on Installed Wind Capacity and Generation

	(1)	(2)	(3)
Panel A: Capacity (MW)			
Overall ATT (cohort)	652	475	1220**
	(405)	(389)	(410)
Overall ATT (year)	394	306	713*
	(219)	(222)	(278)
1-5 years post	241*	158	241
	(113)	(119)	(149)
6-11 years post	575	469	1210**
	(353)	(352)	(452)
Panel B: Generation (GWh)			
Overall ATT (cohort)	3260	2250	6950**
	(2240)	(2110)	(2270)
Overall ATT (year)	1740	1330	3720*
	(1140)	(1070)	(1500)
1-5 years post	1090*	680	1110
	(534)	(569)	(695)
6-11 years post	2550	2070	6490*
	(1850)	(1720)	(2580)
Controls			
Endowments		Yes	Yes
Sociopolitical			Yes
Observations	1380	1380	1380

Table 2: Notes: The 'Overall ATT (cohort)' parameters correspond to the average effect of RPS policies experienced by all states that ever implement an RPS. The 'Overall ATT (year)' parameters corresponds to the average effect of implementing an RPS policy for states that have implemented an RPS for at least 11 years. "1-5 years post" and "6-11 years post" are equivalent to Overall ATT (year), except that they are computed separately for post-implementation years 1-5 and 6-11 respectively. Standard errors are computed using a multiplier bootstrap procedure and clustered at the state level following Callaway and Sant'Anna (2021). Column 1 reports the unconditional estimates. Column 2 adds natural endowment controls for wind potential, solar irradiance, and length of transmission lines. Column 3 adds sociopolitical controls including 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price of electricity in 1990. Panel A reports estimates for megawatts of installed wind capacity as the outcome and panel B reports estimates for gigawatt-hours of wind electricity generation as the outcome. (*** p<0.001, ** p<0.01, * p<0.05).

Table 3: Estimated ATT of RPSs Impact on Installed Solar Capacity and Generation

	(1)	(2)	(3)
Panel A: Capacity (MW)			
Overall ATT (cohort)	201*	235**	155
	(92.8)	(85.9)	(106)
Overall ATT (year)	50.1	71.1	43.0
	(53.7)	(51.4)	(51.3)
1-5 years post	2.14	3.65*	1.51
	(1.92)	(1.68)	(1.93)
6-11 years post	98.3	139	84.7
	(101)	(100)	(99.2)
Panel B: Generation (GWh)			
Overall ATT (cohort)	762*	902*	676
	(375)	(354)	(409)
Overall ATT (year)	119	195	114
	(145)	(139)	(139)
1-5 years post	2.38	8.61*	1.78
	(5.40)	(4.13)	(5.57)
6-11 years post	236	382	227
	(275)	(273)	(273)
Controls			
Endowments		Yes	Yes
Sociopolitical			Yes
Observations	1380	1380	1380

Table 3: Notes: The 'Overall ATT (cohort)' parameters correspond to the average effect of RPS policies experienced by all states that ever implement an RPS. The 'Overall ATT (year)' parameters corresponds to the average effect of implementing an RPS policy for states that have implemented an RPS for at least 11 years. "1-5 years post" and "6-11 years post" are equivalent to Overall ATT (year), except that they are computed separately for post-implementation years 1-5 and 6-11 respectively. Standard errors are computed using a multiplier bootstrap procedure and clustered at the state level following Callaway and Sant'Anna (2021). Column 1 reports the unconditional estimates. Column 2 adds natural endowment controls for wind potential, solar irradiance, and length of transmission lines. Column 3 adds sociopolitical controls including 1990 per capita GDP, 1990 House and Senate League of Conservation Voting scores, and the retail price of electricity in 1990. Panel A reports estimates for megawatts of installed solar capacity as the outcome and panel B reports estimates for gigawatt-hours of solar electricity generation as the outcome. (*** p<0.001, ** p<0.01, * p<0.05).

Table 4: Robustness to Alternative Control Groups, Sample, and Treatment Definitions

Dependent variable	Balanced	Control	Independent variable	Solar	Wind
Capacity (MW)	**Yes**	**NYT**	**RPS Legislation**	**43** **(51.3)**	**713*** **(278)**
Generation (GWh)	**Yes**	**NYT**	**RPS Legislation**	**114** **(139)**	**3720*** **(1500)**
Capacity (MW)	Yes	NT	RPS Legislation	43 (51.3)	739** (278)
Capacity (MW)	No	NYT	RPS Legislation	284 (243)	1980* (869)
Capacity (MW)	Yes	NYT	Net REC demand (average)	353 (454)	1120* (543)
Capacity (MW)	Yes	NYT	Net REC demand (positive)	114 (97.8)	671* (323)
Generation (GWh)	Yes	NT	RPS Legislation	114 (139)	3830* (1510)
Generation (GWh)	No	NYT	RPS Legislation	1400 (1090)	11700* (5060)
Generation (GWh)	Yes	NYT	Net REC demand (average)	1410 (1800)	5620* (2830)
Generation (GWh)	Yes	NYT	Net REC demand (positive)	369 (351)	3480 (1800)

Notes: All specifications are estimated using the estimator developed by Callaway and Sant'Anna (2021) and report the 'Overall ATT (Year)' parameter which corresponds to the average effect of implementing an RPS policy among states that have implemented an RPS for at least 11 years. Entries in bold font are the preferred estimates from Tables 2 and 3. "Balanced" refers to whether the sample includes a balanced panel of states for 11 years before and after implementation of RPS legislation. The control group is either the set of Not Yet Treated *and* Never Treated (NYT) states or *just* the set of Never Treated states (NT). Specifications with "RPS Legislation" as the independent variable use a binary variable equal to one in all periods following implementation of an RPS policy as the treatment. The specifications with "Net REC Demand (average)" or "Net REC Demand (positive)" as the independent variable use the treatment definition explained in Section 4.2. "Net REC Demand (average)" is a binary variable equal to 1 for all years after a state's net REC demand exceeds the average in our sample. "Net REC Demand (positive)" is a binary variable equal to 1 for all years after a state's net REC demand is positive for the first time. Standard errors are computed using a multiplier bootstrap procedure and clustered at the state level following Callaway and Sant'Anna (2021) (*** p<0.001, ** p<0.01, * p<0.05).

Table 5: Robustness to Anticipation Effects, Sample Construction, Estimator, and Alternative Treatment Adoption Cohort Groups

Dependent variable	Anticipation	Drop states	Cohort Group	Method	Independent Variable	Solar	Wind
Capacity (MW)	**No**	**No**	**1-Year**	**C+S**	**RPS Legislation**	**43** **(51.3)**	**713*** **(278)**
Generation (GWh)	**No**	**No**	**1-Year**	**C+S**	**RPS Legislation**	**114** **(139)**	**3720*** **(1500)**
Capacity (MW)	Yes	No	1-Year	C+S	RPS Legislation	43 (51.3)	809** (301)
Capacity (MW)	No	Yes	1-Year	C+S	RPS Legislation	43 (94.2)	681 (479)
Capacity (MW)	No	No	3-Year	C+S	RPS Legislation	145 (122)	1220** (467)
Capacity (MW)	No	No	-	TWFE	Net REC demand (TWh)	2.4 (1.67)	14.1 (18.3)
Capacity (MW)	No	No	-	TWFE	RPS Legislation	-5.85 (3.31)	90.9* (39.1)
Generation (GWh)	Yes	No	1-Year	C+S	RPS Legislation	118 (137)	4190* (1660)
Generation (GWh)	No	Yes	1-Year	C+S	RPS Legislation	106 (247)	4860 (2910)
Generation (GWh)	No	No	3-Year	C+S	RPS Legislation	633 (500)	6900* (2690)
Generation (GWh)	No	No	-	TWFE	Net REC demand (TWh)	5.74 (3.37)	59.5 (99.5)
Generation (GWh)	No	No	-	TWFE	RPS Legislation	-14.4 (7.73)	536* (211)

Notes: All specifications include a balanced panel of states for 11 years before and after implementation of RPS legislation. Entries in bold font are the preferred estimates from Tables 2 and 3. "Anticipation" is "Yes" if the empirical model allows for 2 years of anticipatory effects prior to RPS implementation. "Drop states" is "Yes" if we exclude any state in our sample that may violate the irreversibility of treatment assumption. When "Cohort Group" is "3-Year", adoption cohort groups are defined in 3 year bins (1997-2000, 2001-2003, 2004-2006, and 2007-2009) rather than 1-year cohort bins. All specifications use the estimator proposed by Callaway and Sant'Anna (2021), except for the "TWFE" rows, which report standard two-way fixed effect estimates. Specifications with "RPS Legislation" as the independent variable use a binary variable equal to one in all periods following implementation of an RPS policy as the treatment. The specifications with "Net REC Demand (TWh)" as the independent variable use the treatment definition explained in section 4.2. Standard errors are computed using a multiplier bootstrap procedure and clustered at the state level following Callaway and Sant'Anna (2021) (*** $p<0.001$, ** $p<0.01$, * $p<0.05$).

Appendix A: Data Appendix

Table A1: Data Source and Description of Main Variables

Variable	Units	Source
Transmission lines	km per kmˆ2	Homeland Infrastructure Foundation-Level Data (HIFLD)
Wind speed	meters per second	NREL Wind Integration National Dataset (WIND)
Solar irradiance	kWh / mˆ2 /year	NREL Physical Solar Model version 3 Global Horizontal Irradiance Multi-year Annual Average
Installed capacity	MW	EIA Form EIA-860
Generation	GWh	EIA Form EIA-906
GDP per capita	$ per person	Bureau of Economic Analysis (BEA) dataset SAGDP2N
Electricity price	all end-use, $ / kWh	EIA State Energy Data System (SEDS)
Electricity consumption	Bil. kWh	EIA State Energy Data System (SEDS)
House LCV score	Scale $[0, 100]$	League of Conservation Voters (LCV) Scorecard
Senate LCV score	Scale $[0, 100]$	League of Conservation Voters (LCV) Scorecard
Fraction counties non-attainment	Share $[0, 1]$	Environmental Protection Agency (EPA) Greenbook