Cleaner bills, cleaner air: Stringent Renewable Portfolio Standards policy design leads to significant environmental co-benefits

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Abstract

Renewable Portfolio Standards (RPSs) are the most popular form of state-level renewable energy policy in the United States. Past studies found that the RPS policy stringency could substantially affect policy outcomes. This study dives deeper into the RPS policy design, exploring the impact of four types of discrepancies that result in non-binding renewable commitments for the state. The policy impacts are examined by estimating a difference-in-differences model on a state-level panel data set from 1990 to 2018. We find that while stringent RPS policies generate sizeable environmental co-benefits, discrepancies within RPS policies attenuate those co-benefits. The environmental performance gaps are most significant in non-carbon pollutants: 5-9 years after enacting an RPS policy, the average state under clean RPS policies sees SO_2 emission decrease by 57.6% and NO_x emission decrease by 33.7%. In contrast, the average state under discrepant RPS policies sees a 25.1% decrease in SO_2 emissions and zero effects on NO_x emissions. These environmental performance gaps can be explained by discrepancies' impacts on the power generation fuel mix — smaller cuts in coal and oil, slower growth in natural gas, and less generation from hydropower and nuclear. Using the EPA's CO-Benefits Risk Assessment (COBRA) model, our analysis indicates that RPS design discrepancies caused forgone health co-benefits of between \$12-22 billion from SO_2 and NO_x emissions in 2016.

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Keywords: Renewable portfolio standards; Air pollution; Policy stringency; Difference-in-Differences

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1. Introduction

With a lack of federal-level renewable energy mandates before the Inflation Reduction Act of 2022, Renewable Portfolio Standards (RPSs) have been one of the most important state-level policies in driving power sector decarbonization and fostering renewable energy adoptions in the United States for the past 20 years. RPSs typically mandate a specific percentage of a state's retail electricity sales to come from renewable sources. As of 2021, 36 states and the District of Columbia have implemented RPSs or Clean Energy Standards (CESs), making these policies critical tools at the state level.[1]

Despite its pivotal role in the clean energy transition, the effectiveness of RPSs in carbon mitigation, renewable energy uptake, and their impact on electricity prices remains a subject of ongoing debate. Recent causal evidence suggests that while RPSs decrease carbon emissions by 10-25%, they also increase electricity prices by 2-11% and have limited effects in encouraging renewable energy deployment (Feldman & Levinson, 2023; Fullerton & Ta, 2024; Greenstone & Nath, 2021; Upton & Snyder, 2017; Wolverton et al., 2022). This debate is further complicated by the diverse RPS designs adopted by different states, resulting in variations in policy stringency from percentage requirements to design features like credit multipliers, exemptions, permitted technologies, and voluntary targets (Bernstein & Hoffmann, 2018; Carley et al., 2018; Carley & Miller, 2012; Fischlein & Smith, 2013). Studies have found that stringency of RPS policies can affect renewable energy adoption (e.g., Carley et al. 2018), but their effect on other outcomes remains unclear.

Beyond the central aim of decarbonization, increasing renewable adoption potentially offers significant air pollution co-benefits by transitioning the power generation from dirtier to cleaner energy sources (Fell et al., 2021; Millstein et al., 2017; Rivera et al., 2024; Sergi et al., 2020). However, empirical evidence is scarce regarding how RPS policies allocate these co-benefits heterogeneously across states and rely heavily on projection and simulation models (Barbose et al., 2016; Johnson & Novacheck, 2015; Wiser et al., 2017). One notable exception is

[1] Amongst other differences, CES mainly differs from RPS in that they include nuclear generation as part of the portfolio. For the purpose of this study, we treat CES and RPS as the same policy, and will use RPS to represent both whenever appropriate.

Hollingsworth & Rudik (2019), who empirically estimated the effects of REC demands on air quality and health benefits through changing plant-level power generation portfolios. Their results show that a 1% increase in renewable generation target creates air pollution co-benefits from $100,000 to over $100 million depending on the size of the state. Yet while previous literature demonstrates the influence of RPS policy stringency on renewable energy adoption (Carley et al., 2018), the broader impacts of these design variations, particularly on environmental outcomes and the corresponding co-benefits, remain less understood.

This paper aims to fill these gaps by empirically estimating the air pollution co-benefits of RPS policies and their heterogeneity across varied RPS policy designs. Following definitions in previous literature (Carley et al., 2018; Carley & Miller, 2012), we examine four RPS policy characteristics that cause discrepancy and reduce stringency: qualified exemptions, credit multipliers, permitted carbon-emitting technologies, and voluntary targets. We constructed a unique database covering all RPS and CES laws from 1990 to 2018, meticulously documenting the statutory language in state legislations concerning the assessed policy designs. We quantified the degree of RPS discrepancy caused by a policy design in terms of the difference it creates between nominal and binding minimum renewable generation targets. A larger difference signifies a less stringent policy. According to this measure, we defined a dummy variable *Discrepancy* that categorized RPS policies into two groups[2]: "clean" RPS, which lack these discrepancies, and “discrepant” RPS, which contain at least one discrepancy.

Utilizing this database, we empirically examine the dynamic effect of RPS policies and their design stringency on economic and environmental outcomes. Identification leverages the staggered timing of RPS adoption across states over time, resulting in a staggered adoption difference-in-differences design. Estimation relies on both fixed-effect estimators and imputation-based robust DID estimators (Sun & Abraham, 2021), and our results remain consistent across estimator choices. To estimate the heterogeneous policy effects between clean and discrepant RPS policies, we modify the event-study specification by intersecting the *Discrepancy* dummy variable with the timing of the RPS treatment. This gives us a measure of

[2] The value of the *Discrepancy* dummy variable varies by state and year, indicating the discrepant feature of the legislation which can be revised when a new RPS policy was passed. Its value is set to zero before the state passed its first RPS policy bill.

the average performance gaps caused by having discrepant RPS policies, i.e., the difference between clean and discrepant RPS policies, across the timing of RPS implementation. Since most RPS policies have gradual phase-in features, we summarize the average treatment effects into the short-run (0-4 years post RPS), medium-run (5-9 years post RPS), and long-run (>=10 years post RPS) effects following the practice in Greenstone and Nath (2021) and Deschenes et al. (2023).

The validity of our study design relies on the parallel trend assumption conditional on covariates such as political control, weather, and other state and regional environmental and clean energy policies (e.g. net metering, NOx trading). While a straightforward check for pre-trend is difficult because a number of states switched between clean and discrepant law designs over our study period, we provide suggestive checks by excluding those switcher states and examining the parallel trend assumption using Roth (2022)'s procedure. With the exception of ambient $PM_{2.5}$ concentration, the Roth (2022) procedure suggests that observed performance gap between clean and discrepant RPS are more likely to occur under parallel trend rather than under hypothetical pretrends.

We find that when evaluated as an aggregate, on average, RPS adoption reduces carbon dioxide and sulfur dioxide emissions by 14.1% and 37.3%, aligning with previous studies (Greenstone & Nath, 2021; Upton & Snyder, 2017). The novelty of our results lies in revealing the differences in environmental outcomes between "clean" and "discrepant" RPS policies, which are most pronounced in non-carbon emissions. In the medium run, the average state under clean RPS policies sees SO_2 emission decrease by 57.6% and NO_x emission decrease by 33.7%. In contrast, the average state under discrepant RPS policies sees a 25.1% decrease in SO_2 emissions and almost zero effects on NO_x emissions. Clean and discrepant RPS do not differ much in their impacts on carbon emission reduction or electricity prices. Between the four sources of design discrepancy, the observed environmental performance gap is mostly driven by exemptions within RPS policies, aligning with evidence suggesting that exempted municipal producers see less change in their generation portfolios or financial health compared to non-exempt producers (Hong et al., 2023).

Taking our estimates into EPA's CO-Benefits Risk Assessment (COBRA) model, our analysis indicates that RPS design discrepancy caused forgone health co-benefits of between $12 billion and \$22 billion from SO_2 and NO_x emissions in 2016. This is around 38% of the total health co-benefits of RPS policies if all states had adopted clean RPS, or 40%-70% of the additional electricity costs according to Greenstone and Nath (2021)'s estimate. Our estimates are close in magnitude to that of results from simulation and computational equilibrium models, e.g. in Wiser et al. (2017) and Dimanchev (2019).

We then explore mechanisms through which clean and discrepant RPS policies differ in their environmental performance. We find that the wedge is not driven by differential adoption of renewable energy. In fact, states under discrepant RPS generate 2.9 percentage points more power from wind and solar energy in the medium run, while states under clean RPS do not see changes in wind and solar generation shares. Rather, the gap in health co-benefits is mainly attributed to the faster transition in generations from coal and oil to natural gas under clean RPS policies. In 5-9 years after RPS passed into law, states under clean RPS see an 11.4 percentage point reduction in coal and oil generation and a 7.4 percentage point increase in natural gas generation. States under discrepant RPS, on the other hand, see only a 4.9 percentage point reduction in coal and gas and a 1.24 percentage point increase in natural gas generation.

Our study makes several contributions to the literature. First, we contribute to a growing literature on causally identifying the effect of RPS policies on economic and environmental outcomes (Barbose et al., 2016; Deschenes et al., 2023; Feldman & Levinson, 2023; Fullerton & Ta, 2024; Greenstone & Nath, 2021; Upton & Snyder, 2017; Wiser et al., 2017; Wolverton et al., 2022), as well as a broader literature on the environmental effects of renewables in the electricity sector (Fell et al., 2021; Holland et al., 2020; Novan, 2015). While prior literature mainly focuses on the effect of decarbonization, adoption of renewable energy, and electricity prices, we document economically sizeable health co-benefits that are heterogeneous across RPS policy design. Our study complements Hollingsworth & Rudik (2019), which quantifies air quality co-benefits through the channel of Renewable Energy Credits (RECs) markets.

Second, we contribute to the literature on the effect of policy stringency across jurisdictions. Prior studies have documented performance gaps in RPS design and implementation on decarbonization and renewable energy adoption (Carley et al., 2018; Carley & Miller, 2012; Fullerton & Ta, 2024; Lyon & Yin, 2010) as well as the financial health of exempted vs. non-exempted power producers (Hong et al., 2023). We extend this literature by providing the first quantification of RPS stringency affected by law design, i.e., legal conditions that can reduce the binding renewable share required by a state's RPS policy. Moreover, we systematically analyze the environmental effects of these non-binding components in RPS policies. We also complement the broader literature on the heterogeneous environmental and economic impact of federalism, for example on electric vehicles (Holland et al., 2016), hazardous wastes (Blundell et al., 2021), wetlands (Aronoff & Rafey, 2023), or pollution monitoring and enforcement (He et al., 2020).

Third, we contribute to the literature on the co-benefits and unintended consequences of environmental policy. We empirically document large non-climate-change co-benefits generated by a renewable energy policy and how variations in the co-benefits are driven by policy design. This complements studies on the co-benefits of the clean energy transition (Burney, 2020; Fell et al., 2021; Rivera et al., 2024; Sergi et al., 2020), as well as other environmental policies such as the SO_2 and NO_x program (Deschênes et al., 2017; Muller et al., 2011; Schmalensee & Stavins, 2013), agro-climate policy (Zuidema et al., 2023), and water pollution (Weng et al., 2023).

This paper proceeds as follows: Section 2 provides an overview of the existing research examining the effect of RPS adoption and policy stringency. Section 3 describes the method of quantifying RPS policy stringency and details other data sources for the empirical analysis. Section 4 describes our empirical strategy to identify the overall and heterogeneous RPS effects across the policy design. Section 5 presents the results. Section 6 concludes.

2. Renewable Portfolio Standards: Policy Design and Stringency

2.1. RPS policies and economic outcomes

RPS policies generally require a certain percentage of a state's retail electricity load to be generated from qualified renewable energy sources. This study utilizes a data set containing 34 states that have adopted a Renewable Portfolio Standard (RPS) or Clean Energy Standards (CES) policy by year 2018 and 12 "never treated" control states that never adopted RPS or CES during our study period.[3] Table 1 provides a list of states' first RPS legislative bills and their enactment

[3] Four states are excluded from our analysis: Iowa, Texas (capacity goals rather than sales targets), Kansas (peak electricity demand goals) and West Virginia (RPS repealed in 2015) are excluded from the analysis.

date. For the past decades, RPS has been one of the most important instruments in achieving energy sector decarbonization in the United States with the long absence of federal-level renewable energy policies. [4]With the federal government's 100% clean energy goal by 2035, RPSs become an increasingly important policy and scholarly topic. The key question is whether RPS policies can effectively contribute to this ambitious goal.

A growing body of literature examined the effects of RPS policy on observable outcomes, with most studies focusing on the efficacy and cost-effectiveness of RPS to achieve its policy intent: reduce carbon emissions and foster renewable energy adoption. However, these studies presented a mixed, sometimes conflicting, message about the efficacy of RPS policies. Studies have found that RPS policies are associated with moderate reductions in carbon emissions, but the size of the estimated effect varies by study design, ranging from 3.5% to 10-25% (Feldman & Levinson, 2023; Greenstone & Nath, 2021; Upton & Snyder, 2017).[5] At the same time, studies also documented that RPS policies increase electricity prices and suppress electricity demand, with effect sizes also varying by study design. Upton and Snyder (2017) and Greenstone and Nath (2021) both find that electricity prices increase by 11% after states adopt RPS, while Wolverton, Shadbegian, and Gray (2022) found a 2% increase in electricity prices in a utility-level analysis. Greenstone and Nath (2021) concluded that the equivalent abatement cost of greenhouse gas emission ranges between $60-300 per ton of CO_2, putting it at the higher end of options to mitigate greenhouse gas emissions.

In theory, encouraging renewable energy generation creates both climate benefits through reducing carbon emissions and environmental co-benefits through reducing air pollutants such as sulfur dioxide, nitrous oxides, and particulate matter. Yet studies that document the air pollution co-benefits are relatively scant compared to those studying the effects of carbon emission or renewable energy adoption, and those studies rely heavily on projection and simulations rather

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[4] Federal level policies are available from the 2022 Inflation Reduction Act (P.R. 117-169), which provides production and investment tax credits for renewable energy production and development. Our study (ending 2018) predates the IRA.

[5] Upton and Snyder (2017) found a statistically insignificant reduction of 3.7% using a standard difference-in-differences design. Greenstone and Nath (2021) found a medium-run reduction of 10%-25% after taking into consideration the dynamic progression of RPS requirements and thus its impacts. Feldman and Levinson (2023) expands the analysis to include the impact of cross-state REC markets and found a 30-40% elasticity of REC demand on carbon emission reduction.

than statistical evidence. For example, Johnson and Novacheck (2015) integrated an economic dispatch model with a renewable project selection model and found that CO_2, SO_2, and NO_x emissions will decrease by 13%, 13%, and 12% respectively if the state of Michigan increases renewable penetration by 10%. Wiser et al. (2017) simulated the effect of carbon and air pollutant abatements generated from RPS policies across the nation using ReEDS, an electric generation capacity expansion model. They find that existing RPS policies will yield $97 billion in air pollution benefits and $161 billion in climate change benefits over 2015-2050 by linking emission reduction with an air pollution dispersion model (Muller, 2014; Muller et al., 2011).

On the contrary, existing statistical analysis suggests that on average, there is weak evidence that RPS policies actually create significant air pollution co-benefits. For example, Eastin (2014) found weak evidence that RPS adoption decreases particulate matter. Using a difference-in-differences framework, Nath and Greenstone (2021) found that RPS adoption has no effect on NO_x emission and $PM_{2.5}$ concentration, and weak-to-no-effects on SO_2 depending on modeling specifications. An exception is Hollingsworth and Rudik (2019), who empirically estimated the effects of REC demands on plant-level power generation portfolios. They then translated the marginal change in power mix to emission reductions and health benefits by using plant-specific emission factors and an air pollutant dispersion model. Their results show that a 1% increase in renewable generation target creates air pollution co-benefits from $100,000 to over $100 million depending on the size of the state.

One potential reason why previous studies found no average effect of air pollutant co-benefits could be that the direct benefits and co-benefits of RPS depend on their policy design, such that the heterogeneity is masked when analyzing the average treatment effect of RPS adoption. In Section 2.2 below, we provide an overview of existing studies regarding RPS policy stringency, which leads to our creation of a unified metric for the presence of discrepant policy design in RPS policies.

2.2. Design stringency of RPS policies

One major difficulty in understanding the efficacy and cost-effectiveness of RPS policies is that they can differ in multiple ways between states and over time. RPS policies typically have a mandated *nominal goal* of percentage renewable sales to be achieved by a target year, with interim annual goals that gradually increase over time.[6] The timelines and magnitudes of these interim RPS goals vary between states. Moreover, RPS policies can differ in multiple other aspects of their designs, such as qualified energy sources, exemptions, or multipliers for certain technologies, thus creating variations in the binding renewable energy share required in the RPS policies (Fischlein & Smith, 2013). These systematic differences in law design could potentially explain the mixed evidence on RPS's policy impacts. As noted by Bernstein and Hoffman (2018), RPSs yield significant value towards decarbonizing the power sector in some cases while incentivizing more efficient fossil-fired generation and yielding unintentional carbon lock-in in other cases. This variability stems from the individual characteristics of the policy and the area to which it is applied (Bernstein & Hoffmann, 2018).

A number of previous studies have attempted to develop a score for the 'stringency' of RPS policies, usually measured as the difference between pre-existing and target levels of renewable energy divided by the number of years between the initial and target years, with some studies accounting for the percentage of the total load covered by the policy (Barbose, 2021; Carley & Miller, 2012; Feldman & Levinson, 2023; Fullerton & Ta, 2024; Yin & Powers, 2010; Zhou & Solomon, 2020). While such stringency measures account for time constraints and the total coverage of RPS, many of them do not factor in many of the nuanced differences in RPS design features that affect law enforcement, such as the existence of credit multiplier[7], eligible technology, or exemptions and carve-outs, that also quantitatively affects the actual binding target of an RPS policy.[8] A notable exception is Carley et al. (2018), which uses a stringency metric identical to the one described above but also includes categorical variables capturing the policy design heterogeneity in RPS policies, including credit multipliers, qualified energy sources, exemptions, and voluntary targets. Using these variables, Carley et al. (2018) found that both the stringency level and the level of planning of an RPS have significant effects on

[6] The exceptions are Iowa and Texas which have capacity goals, and Kansas which has goals for peak electricity demand. We exclude these three states from our analysis.

[7] Fullerton and Ta (2024) constructed a continuous policy stringency variable to measure electricity sales covered by RPS, which does not account for enforcement stringency. Dummy variables indicating the presence of credit multipliers and carve-out requirements in the state's RPS policy are included as control variables in their empirical analysis.

[8] In addition, as Feldman and Levinson (2023) noted, the difference between pre-existing and target levels of renewable energy is in itself endogenous to electricity price: cheaper electricity price automatically increases stringency through demand effect.

renewable energy adoption. Yet Carley et al. (2018) still lacks a measure that integrates all relevant policy aspects, making cross-discrepancy comparisons difficult. In this paper, we extend these previous attempts by taking a purely quantitative approach, compiling a comprehensive database of current and historical RPS legislation bills in the US before 2018. Included in this database is a unified metric accounting for four distinct policy stringency designs affecting the enforcement of the state's RPS requirement, where we describe our method of quantifying different types of RPS laws and constructing a unified stringency metric for our empirical analysis in the next section.

3. Data

3.1. Constructing a unified RPS stringency metric

Our identification of the policy effect of clean against discrepant RPS policies depends on a novel measure of policy discrepancy we are presenting. We put together a database to quantitatively document, for each US state, both the *nominal* commitments that are presented in RPS and CES statutes and the *binding* commitments, i.e., the minimum percentage of renewable energy sales needed to comply with the nominal commitment.[9] The gap between the nominal and the binding commitment is what we refer to as the *nonbinding* commitment, or *discrepancy*. Table 1 presents a list of nominal versus binding commitments in the states' first RPS legislation. For each RPS legislative bill, we identified the voluntary, expired, carbon-emitting, and otherwise misrepresented contributions permitted within the statute. These characteristics potentially lead to less actual renewable energy deployment than the law's nominal targets. We computed a state's binding commitment by excluding these misrepresented contributions from its nominal commitment. Part of our proposed design entails the standardized mapping of all technology types mentioned in standards to specific RPS and CES commitment classes. We propose an examination of the historical evolution of binding commitments to reveal long-term trends in the relative aggressiveness of targets made. We also compare binding portions to observed levels of renewable development across states to assess the empirical effects that various standards have had. The database also allows us to track differences between nominal

[9] A database documenting the RPS stringency metric will be published with the paper as part of the replication package.

and binding commitments, thereby highlighting likely areas where states' commitments may be misinterpreted or misrepresented.

The process of documenting RPS policy discrepancy started from acquiring the universe of RPS statutes ever passed from North Carolina State University's DSIRE database and the National Conference of State Legislatures (NCSL) database and historic commitments from state databases.[10, 11] Once the initial list was confirmed, the respective bills and laws representing the commitments were found and listed individually. Only commitments that entered state legislature and changed renewable target percentages or dates were tracked. For each legislative bill, RPS and CES commitments were recorded along with the respective target dates. The majority of commitments are made up of sub-targets based on generation technology. In order to reflect this, commitments were split into several classes, each with a unique set of eligible technologies. The goal of this database was to calculate the minimum amount of renewable and clean energy required to meet the commitment as a percentage of total state sales, i.e., the *"binding commitment"*. We separately coded statutes with generation capacity goals and excluded them from our main analysis.[12] We also coded statutes that require different targets for different utility types (e.g., North Carolina's 2008 RPS), separately documented each type of targeted utility and aggregated them back into one state-level commitment.

We then record policy characteristics, i.e., source of discrepancies, that could potentially allow the nominal commitment to be met without renewable or carbon-neutral electricity sales. We identify four such characteristics and quantify their effects on each class. The effects are then integrated to the class-level, and further aggregated to utility and state levels as weighted averages. The four types of discrepancies are:

1) **Voluntary shares**: Many commitments contained language that explicitly rendered some (or all) classes completely or partially voluntary. Voluntary shares of the commitment do not count towards the binding minimum as utilities are under no legal obligation to procure it.

2) **Carbon-Emitting Shares:** RPS commitments vary widely in the set of technologies that are eligible to fulfill the commitment target. Here we standardize what qualifies as an RPS/CES

[10] DSIRE is available at https://www.dsireusa.org/.

[11] NCSL is available at https://www.ncsl.org/technology-and-communication/ncsl-50-state-searchable-bill-tracking-databases.

[12] Texas Utilities Code Ann. §39.904 (1999) and Iowa Code 476.41 et seq (1983).

technology based on a system of classification based on emissions at the point of generation (which we term tailpipe emissions). We split technologies into four mutually exclusive and exhaustive categories:

A. No Tailpipe Emissions, Non-Fossil-Fuel, Non-Mineral-Based: Technologies that do not emit carbon at the point of generation and do not derive energy from fossil or mineral fuels fall under this category. Some examples include solar and wind energy. Technologies qualifying under this category are considered RPS technologies.

B. Tailpipe Emissions, Non-Fossil-Fuel, Non-Mineral-Based: This category includes technologies that release carbon at the point of generation but do not derive energy from fossil or mineral fuels. This category is made up of biomass, biogas, and liquid biofuels derived from plant growth. Technologies qualifying under this category are considered RPS technologies.

C. No Tailpipe Emissions, Fossil-Fuel or Mineral-Based: This category includes technologies that do not emit carbon into the atmosphere but are not renewable as they are based on fossil fuel or mineral sources of energy. A common example of this category is nuclear energy. Technologies qualifying under this category are considered CES technologies.

D. Tailpipe Emissions, Fossil-Fuel or Mineral-based: This category includes traditional energy sources that release carbon in the atmosphere and are based on fossil fuel or mineral sources. Fossil fuel generation that does not include carbon capture technology falls under this category. Technologies qualifying under this category were considered CO_2 technologies.

3) **Double-Counted Shares:** Many states provide credit bonuses to specific generation technologies or facilities. A credit bonus means that the generation facility gets more than one Renewable Energy Credit (REC) for each megawatt-hour of electricity generated. When computing the effect of a credit multiplier on the binding commitment, the maximum potential double counting was calculated and subtracted from the nominal commitment subsequently.

4) **Adjustment for Excluded Consumption:** Some RPSs only target a subset of electricity companies and exempt others (for example Investor-Owned Utilities or Municipal Utilities), such that the percentage target reflects only a percentage of utility sales rather than total state sales. To adjust these commitments to percentages of total state sales, the sales made by targeted entities are divided by total sales made in the state.

After organizing the categories, we calculated the total energy generation capacity for each state. This allowed us to convert commitments and classes from megawatts (MW) into percentages. Each filter applied to the nominal commitment produces a numerical factor, adjusting the original commitment to reflect the binding commitment. These filters, which can be calculated at the same time, are applied simultaneously. After that, commitment classes expressed as percentages are adjusted in the order of voluntary contributions, expired commitments, carbon-emitting shares, double counting, and excluded consumption. Classes in MW term are adjusted by the same filters except that the adjustment for excluded consumption is replaced by a conversion step. After filtering all classes, they are combined to determine the total binding percentage of Clean Energy Standard (CES) and Renewable Portfolio Standard (RPS) commitments for each original commitment, i.e., the minimum amount of actual renewable (as define above) generation required to fulfill the goals. Additionally, the impact of each filter is aggregated to measure their overall effect on the binding minimum commitment.

Figure 1 depicts the final unified metric of RPS stringency. All RPS policies are listed by their nominal commitment (in hollow, black-bordered bars) and nonbinding shares of commitments separated by their respective sources of discrepancies. Note that for some laws, the total amount of discrepancy can exceed the total amount of nominal commitment when multiple discrepancy categories are built into the law since each category is calculated separately. Effectively, this means that the binding commitment is zero since utilities can, in theory, utilize a combination of these discrepancies to comply with the commitment.

Table 2, Panel C summarizes the policy design variables affecting law stringency for time periods post RPS implementation.

3.2. Other data sources for empirical analysis

In this section, we describe the additional sets of data compiled to enable the empirical analysis of the policy effect of RPS and heterogeneity associated with law stringency. To facilitate our analysis, we organize the data set to form state-by-year panel spanning 1990-2018, although many of the underlying data could be disaggregated into finer spatial and/or temporal scales, for example generation by source (down to the facility level) or ambient $PM_{2.5}$ concentration (down to 1km x 1km grids).

Table 2 presents summary statistics for states, categorized by whether they have ever adopted RPS before 2018.[13]

Data on electricity generation and emissions are sourced from the Energy Information Administration (EIA) Form 860 and Form 923. These forms provide information on generation and capacity by fuel type, as well as the associated CO_2, SO_2, and NO_x emissions for power plants with a nameplate capacity above 1 Megawatt (MW). Data on electricity retail price is obtained from EIA's Form 861 (previously Form 826). $PM_{2.5}$ concentration annual average is derived from Van Donkelaar (2019) reanalysis product, available at the 1km resolution. Air Quality Index (AQI) data is obtained from the Environmental Protection Agency, available at the county-year level. The $PM_{2.5}$ and AQI measures are aggregated to the state level using population-weighted averaging to approximate the average exposure across each state.

[13] West Virginia repealed its RPS policy in 2015. Montana repealed its RPS policy in 2021. Our analyses excluded West Virginia but included Montana in the panel data span years 1990-2018.

Table 2, Panel B summarizes the time-varying covariates used to control for confounders that might bias the treatment effect of RPS. Gross state product is acquired from the US Bureau of Economic Analysis (BEA). Political data on the control of state gubernatorial and legislative parties are obtained from Klarner politics.[14] Availability of net metering program is collected from North Carolina State University's DSIRE program.[15] Participation in the NO_x trading program is compiled from the EPA public information website.[16] Percentage of energy exported is obtained from the EIA. Heating degree days (above 65°F) and cooling degree days (below 65°F) are acquired via National Weather Service's Climate Prediction Center, which provide population-weighted degree day metrics.[17]

4. Empirical Strategy

4.1. Generalized difference-in-differences

To identify the policy effect of Renewable Portfolio Standards (RPS) ignoring diversity in the policy design stringency, we use variation in the enact time of RPS policies, suggesting a difference-in-differences (DID) empirical strategy. RPS policies are considered as an absorbing treatment with staggered timing, which is taken as the first year a state passed its RPS. We focus on the dynamic effects of RPS and estimate the event-study specification in equation (111):

$$Y_{s,t} = \sum_{\tau \neq -1} \beta_\tau \mathbf{1}\{t - RPS_s = \tau\} + X_{s,t} + \alpha_s + \delta_t + \varepsilon_{st}, \tag{1}$$

where $Y_{s,t}$ is an environmental outcome of interest for state s in year t (e.g. natural logarithm of CO_2 emission), and RPS_s is the year when state s passed its first RPS legislation. Hence, τ is the number of years post RPS treatment, which is set to infinity for "never treated" states. α_s and δ_t are the state and year fixed effects added to control for unobserved state attributes and for any national trends in the outcome variable. $X_{s,t}$ is a set of time-varying covariates including state-level political indicators, gross state product per capita, natural gas price, population, share of exported energy, annual heating degree-days (HDDs), annual cooling

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[14] Available via https://www.klarnerpolitics.org/datasets-1.

[15] Available via https://www.dsireusa.org/

[16] Available via https://www.epa.gov/power-sector/nox-budget-trading-program

[17] Available via https://www.cpc.ncep.noaa.gov/products/analysis_monitoring/cdus/degree_days/

degree-days (CDDs), and binary indicators for net metering programs and NOx trading programs. We estimate equation (1) using a panel data set of 46 states over 29 years (1990-2018), including 34 treated states that adopted RPS during our study period, and 12 "never treated" control states.[18] We cluster standard errors at the state level to allow for correlation of errors within states over time.

We omit period $\tau = -1$ so that β_τ's are the dynamic treatment effects of RPS on the outcome after τ years relative to the year immediately prior to RPS passage. The event-study estimates are useful in two important ways. First, they provide estimates for the longer-term patterns of RPS policy effects after passage. Second, the β_τ's reveal the temporal profile of impacts to the RPS states in the pre-treatment periods, allowing us to validate the parallel trend assumption to ensure internal validity of the difference-in-differences (DID) model. With the presence of a "never treated" group in our data, i.e., states never implemented RPS by 2018, identification in our setting relies on the weakest parallel trend assumption that states that have adopted RPS would have behaved the same as states that never adopted RPS if not for RPS adoption (Marcus & Sant'Anna, 2021). We present the event-study plots and perform pretend tests in Section 4.3 to show that there are no insignificant differences found in the pre-existing trends of the RPS states and control states.

4.2. Identify the impact of discrepant policy designs

We construct a *Discrepancy* dummy variable, such that a non-stringent RPS legislation is indicated by the presence of a positive nonbinding portion of the state's nominal RPS commitment in that year. We explore the differentiated dynamic effects of RPS policy stringency on the environmental outcomes by intersecting the post-treatment indicators in equation (1) with the $Discrepancy$ dummy. The modified event-study specification is defined in equation (2):

$$Y_{s,t} = \sum_{\tau \neq -1} (\beta_\tau + \gamma_\tau \times Discrepancy_{st}) \cdot \mathbf{1}\{t - RPS_s = \tau\} + X_{s,t} + \alpha_s + \delta_t + \varepsilon_{st}. \tag{2}$$

[18] The state of West Virginia repealed its Alternative and Renewable Energy Portfolio Standard in 2015 after its establishment in 2009. West Virginia is excluded from our empirical analysis. We also exclude three other RPS states due to unique designs in their RPS policies — Iowa and Texas set RPS targets in terms of renewable capacity levels rather than percentages of retail electricity sales, and Kansas requires a percentage of peak electricity demand.

$Discrepancy_{st} = 1$ if state s has a discrepant RPS policy design in year t.[19] The γ_τ coefficients assess variations in the dynamic treatment effects of RPS resulting from discrepant policy designs, while β_τ's are the effects of a discrepancy-free policy. Hence, opposite signs of β and γ imply that a discrepant policy design could diminish the RPS treatment effect on emission and air quality outcomes.

The effects of RPS policies tend to ramp up slowly over time. For example, Deschenes et al. (2023) found a 5-year lag in renewable deployment after RPS implementation. We summarize the event-study estimates in equation (2) with three structural breaks in the post-RPS period. The 7-year pre-RPS period ($-7 \leq \tau \leq -1$) is used as reference.

$$\begin{aligned} Y_{s,t} &= \beta_1 \mathbf{1}\{0 \leq \tau \leq 4\} + \beta_2 \mathbf{1}\{5 \leq \tau \leq 9\} + \beta_3 \mathbf{1}\{\tau \geq 10\} + \\ &\gamma_1 \mathbf{1}\{0 \leq \tau \leq 4\} \times Discrepancy_{st} + \\ &\gamma_2 \mathbf{1}\{5 \leq \tau \leq 9\} \times Discrepancy_{st} + \\ &\gamma_3 \mathbf{1}\{\tau \geq 10\} \times Discrepancy_{st} + \theta \mathbf{1}\{\tau < -7\} + X_{s,t} + \alpha_s + \delta_t + \varepsilon_{st}. \end{aligned} \tag{3}$$

For discrepant-free RPS policies, β_1 summarizes the short-run average treatment effect of the treated (ATT) in the first 5 years, i.e., $0 \leq \tau \leq 4$, which is the period covered by all RPS states in our balanced panel. β_2 is the medium-run ATT (5-9 years). And β_3 is the long-run ATT, i.e., 10 years or more post RPS implementation. The γ coefficients identify the average differences in RPS treatment effects induced by discrepant policy designs in the short-, medium-, and long-run. The β_2 and γ_2 estimates are used to derive the environmental co-benefit counterfactuals between clean versus discrepant RPS policies.

To further investigate the differentiated effects across RPS policy characteristics, we decompose the effect of discrepancy to four policy designs[20] in equation (4).

$$Y_{s,t} = \beta_1 \mathbf{1}\{0 \leq \tau \leq 4\} + \beta_2 \mathbf{1}\{5 \leq \tau \leq 9\} + \beta_3 \mathbf{1}\{\tau \geq 10\} + \theta \mathbf{1}\{\tau < -7\} + \tag{4}$$

[19] We allow the discrepancy dummy to vary by year, as states may amend the RPS/CES legislation, which lead to changes in the nominal targets and/or the degree of policy discrepancy. $Discrepancy = 0$ for control/"never-treated" states and the pre-RPS time periods of RPS states.

[20] Definitions of the discrepant policy designs are described in Section 3.1. The policy designs are not mutually exclusive, i.e., an RPS policy can have one or multiple discrepant characteristics that lead to less stringent policy enforcement.

$$\mathbf{1}\{5 \leq \tau \leq 9\} \times \left(\gamma_{e,2} E_{st} + \gamma_{v,2} V_{st} + \gamma_{m,2} M_{st} + \gamma_{c,2} C_{st}\right) +$$
$$\mathbf{1}\{\tau \geq 10\} \times \left(\gamma_{e,3} E_{st} + \gamma_{v,3} V_{st} + \gamma_{m,3} M_{st} + \gamma_{c,3} C_{st}\right) + X_{s,t} + \alpha_s + \delta_t + \varepsilon_{st}.$$

The γ coefficients report the decomposed effects of four discrepant policy designs: $E = 1$ if the RPS statute includes adjusted or excluded electricity sales; (2) $V = 1$ if a positive share of the RPS commitment is voluntary; (3) $M = 1$ if the RPS commitment double counts certain technologies with credit multipliers; (4) $C = 1$ if the RPS commitment includes carbon-emitting technologies as eligible generation sources.

4.3. Examine parallel trends

Before we dive into results, we first examine the assumptions underscoring our generalized DID estimates. We find limited evidence of violations in two conditional parallel trend assumptions. First states with RPS would have behaved similarly to states without RPS if not for the passage of RPS. Second, states under discrepant RPS would have behaved the same as states under clean RPS should they passed clean laws. Testing the first parallel trend assumption is straightforward since it pertains to the standard DID setting with staggered treatment adoption. The event-study plots in Figure 2 provide an intuitive visual validation of the first parallel trend assumption for identifying the dynamic effects of RPS on environmental outcomes regardless of any law discrepancy.[21] However, recent literature suggests that the pretrend tests may have low power and could introduce statistical bias to estimates conditioned on passing the pretrend tests (Roth 2022). We assess the power of the pretrend test, utilizing the dynamic ATT estimates from equation (1), examining how likely a hypothesized linear trend is detected to violate the parallel trend assumption. Test results are presented in Appendix Table A.1, showing that only ambient $PM_{2.5}$ and electricity price failing the pretrend tests, both outcomes have insignificant treatment effects in the break model (which we will present in the next section and in Table 4).

The second parallel trend assumption, which ensures the validity of our estimated environmental performance gap due to discrepant policy designs, requires more attention. Because some states switched from a clean RPS to a discrepant one, or vice versa, we are unable

[21] Appendix Figure A.1 presents the event-study plots for the power sector outcomes.

to define a time-invariant discrepancy status associated with each state. As such, standard parallel trend tests in quasi triple-difference settings do not readily apply here. We provide a suggestive test by removing those "switcher" states and only keep states that have "always" implemented clean RPS policies and those that never passed discrepancy-free RPS policies in the sample.[22] By doing so, we can construct consistent clean and discrepant groups of states, enabling the estimation of event-study coefficients before RPS adoption mimicking a triple difference design. The event study estimate is given by equation (5):

$$Y_{s,t} = \sum_{\tau \neq -1} (\beta_\tau + \gamma_\tau \times D_s) \cdot \mathbf{1}\{t - RPS_s = \tau\} + X_{s,t} + \alpha_s + \delta_t + \varepsilon_{st}. \tag{5}$$

where the dummy variable $D_s = 1$ for state s if $Discrepancy_{st} = 1$ for all post-RPS periods. The pre-treatment coefficients $\gamma_{-6 \leq \tau < \leq -2}$ and post-treatment coefficients $\gamma_{0 \leq \tau \leq 9}$ are utilized to perform the pretrend test. The event study estimands are estimated using two-way fixed effects (TWFE), which ensures unbiased examination of the parallel trend assumption.[23]

We present a variety of metrics for pretrends beyond the traditional "eyeballing" procedure, though a visualization of the event-study results. Results are presented in Table 3.. We start by estimating the level and slope of the pre-trends, presented in columns (2) and (3). While models on most outcome variables having pre-trend coefficients statistically different from zero in joint significance tests, only three of them have a statistically significant linear trend pre-treatment. This is reassuring since significance in trends, rather than differences in raw levels (which can be demeaned), indicate non-parallel pretrend.

In columns 4-7 of Table 3, we present power analysis following Roth (2022)'s procedure, which calculates linear pretrends using event study coefficients -6 to -2, and project hypothetical post-treatment trends under non-parallel pretrend. The test generates two Bayesian test statistics: a Bayes factor, with smaller BF indicating the model is more likely to pass the pre-test; and a likelihood ratio of the observed coefficients under the hypothesized linear trend relative to under

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[22] Clean RPS states are states with discrepancies identified in none or at most 2 post-RPS years. Discrepant RPS states are states that have never passed a clean RPS. States that had their RPS discrepancy status switched overtime are removed in estimating equation (5).

[23] Imputation-based methods, for example in Sun and Abraham (2021) or Callaway and Sant'Anna (2021), are not preferred to be used to check for parallel trends because the pre-trend coefficients are potentially biased.

parallel trends. Smaller likelihood ratio (less than 1) means observed coefficients are more likely to be observed under parallel trend. We note here that the standard power analysis will be excessively sensitive since most RPS policies exhibit a "phase-in" periods where initial commitment levels are low but gradually increase over time. This means that 1) the phase-in years will be used to estimate treatment effects, rending the treatment effects closer to zero; 2) any pre-trends will be amplified when we are interested in the medium-run treatment effects rather than treatment effects right after the treatment.[24] We find that with the exception of log($PM_{2.5}$), all other outcome variables have a likelihood ratio smaller than 1, meaning the observed treatment effects are more likely to be observed under parallel trend rather than the hypothesized pretrend. Overall, these tests suggest that the parallel trend is unlikely to drive the observed environmental performance gap between clean and discrepant RPS policies.

5. Results

5.1. RPS policy in general improves environmental outcomes

We start by presenting results estimating the dynamic treatment effects of RPS adoption. Figure 2 plots the event-study estimates of the generalized difference-in-differences (DID) model, i.e., the β_τ coefficients in equation (1), along with pointwise 95% confidence intervals. We find that, without distinguishing variation in RSP policy designs, states after adopting RPS see increasing reductions in their power-sector CO_2 and SO_2 emissions. Figure 2 shows a slow effect in emission reductions, with more significant reductions starting around 5 years after the state implemented RPS. This result about lagged effects aligns with the findings in Deschenes et al. (2023). For annual NOx emission, PM2.5 concentration, and the 90th percentile of AQI, we see almost no changes, even though the event-study coefficients seem to suggest declining trends post RPS implementation. The event-study estimates illustrate consistent patterns in the dynamic treatment effects between two staggered adoption design estimators — the two-way fixed effects

[24] The average pre-trend will be twice as large for treatment years 5-9 compared to years 0-4.

(TWFE) estimator and the robust DID estimator proposed by Sun and Abraham (2021).[25, 26] This confirms the robustness of our results against potential bias due to heterogeneous treatment effects across adoption cohorts.

We summarize the dynamic effects of RPS policy to 5-year time intervals in Table 4. Consistent with the findings from the event study, we see an overall decrease in emissions and air quality outcomes, suggested by negative coefficients in Panel A of Table 4. In order to examine factors driving the emission declines, we investigate the break-model ATT on the power sector outcomes in Panel B of Table 4.[27] We see a decrease in the share of coal and oil generation after the state adopted RPS, while the natural gas generation mildly grows. We also find an increased share of generation from clean energy sources. Examining the absolute values of the ATT estimates across different generation fuel groups, our results suggest that coal and oil generation reductions after states adopted RPS/CES policies are likely compensated by increased generations from clean energy sources, i.e., wind, solar, hydro and nuclear, as well as from the natural gas generation units. We do not see strong empirical evidence on the cost of RPS in terms of electricity price hikes. This, as explained by Lee (2023), is potentially due to the short-run decrease in the marginal cost of "brown" electricity and the long-run decrease in the average cost as less "brown" electricity is produced, which counteract the increases in REC costs.

5.2. Law stringency affects RPS's effect on emissions

We first examine the heterogeneity in RPS's dynamic effects associated with presence of the characterized discrepancies in the RPS policies. We find that, compared to discrepancy-free/ "clean" RPS, discrepant policy designs significantly weaken the impact of RPS on emission reductions and air quality improvements (Figure 3). These weakened effects are most pronounced for non-CO_2 emission, i.e., SO_2 and NO_x. We also see less improvement in air

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[25] Recent literature suggests using traditional fixed-effect estimators (TWFE) could lead to biases in the presence of treatment effect heterogeneity under a staggered adoption setting because of issues related to negative unit weights (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). That said, we also recognize that estimating the differential effects from RPS policy discrepancy requires us to rely on a fixed-effect estimator since imputation-based methods have not yet been extended to even quasi-triple-difference settings, let alone our setting with potential switches into and out of discrepancy.

[26] We checked for other robust DID estimators and the dynamic effects are similar to that presented here.

[27] Event studies of the power sector outcomes are presented in Appendix **Error! Reference source not found.**.

quality, measured by the 90th percentile of AQI, when states' nominal commitments in RPS policies are not stringently enforced.

The 5-year average ATT estimates in Table 5 corroborate our findings from the event-study plot. In the medium term (5-9 years post RPS implementation), power-sector emissions decrease by 16.9% for CO_2, 57.6% for SO_2, and 33.7% for NO_x under clean RPS. However, discrepancies in RPS nearly negate the policy's impact on NO_x (to 0.1%) and reduce the impact on SO2 by almost half (to -25.1%). Correspondingly, we observe a significant gap in RPS's effect on the AQI 90th percentile, with a 12.1% drop under clean RPS versus a 1.7% drop under discrepant RPS. Although the empirical evidence for $PM_{2.5}$ is not significant (Table 5, Panel A column 4), the opposite signs of the "5-9 year" coefficients imply the same finding — discrepant policy designs prevent the full realization of RPS's potential to mitigate pollution. [28] Our results highlight the critical role that policy enforcement stringency plays in determining the efficacy of RPS measures. The minimal impact of policy discrepancies on CO_2 reduction after RPS implementation suggests that the resulting welfare losses will be concentrated on foregone health co-benefits rather than climate benefits.

Estimates in Table 6 explain the potential mechanisms that lead to heterogeneous environmental outcomes under clean against discrepant RPS policies. We focus on changes in the generation fuel mix and the differentiations associated with policy discrepancies.[29] Less stringent RPS policies hinder the shift from coal and oil to less polluted natural gas. We see a diminished RPS treatment effect by approximately 6 percentage points in the reduction of coal and oil generation share (Table 6, Panel A column 1) as well as in the increase of natural gas generation share (Table 6, column 2) in the medium term. Additionally, we find that clean RPS policies tend to introduce less wind and solar but more clean generations from hydropower and nuclear. This may be due to the interplay between different types of policy discrepancies, with the combined effect depending on the significance of counteracting forces.

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[28] The ATT differences in emission are primarily driven by "excluded sales" type of discrepancy (Table 5 Panel B). The complete estimates are detailed in Appendix Table A.2.

[29] Event studies of the power sector outcomes with discrepancy intersections are presented in Appendix Figure A.2. Appendix Table A.3 summarizes the event studies to break model ATT estimates.

Panel B of Table 6 presents the estimates of equation (4) and highlights the variation of RPS effects associated to four sources of policy design discrepancy, focusing on the medium-term effects (5-9 years post RPS implementation). The complete estimates of equation (4) are detailed in Appendix Table A.3. We find the aggregated effect is primarily driven by the "Excluded Sales" condition, where some states exempt a subset of electric utilities, such as municipal producers, from the RPS requirements. When this type of exemption is present, the estimates in Table 6 show about a 7.8 percentage point smaller reduction in coal and oil generation and an 8.5 percentage point less increase in natural gas generation.

Mixed effects are observed for the wind and solar generation percentage. The "Multiplier Credit" condition generally allows utilities to receive double or more credit toward meeting the RPS requirement by wind and solar energy. This results in 4.7 percent less generation from these clean energy sources compared to scenarios without the multiplier credit. Our finding implies that utilities can comply with the standard with lower levels of renewable deployment and reduced integration costs, as evidenced by the negative estimate for electricity price (Table 6, Panel B column 6). The "Excluded Sales" and "Carbon emitting" conditions, which exempt certain utilities or non-renewable technologies from RPS regulations, tend to increase the wind and solar generation share. Examining the laws, we find the "Excluded Sales" conditions usually exempt municipal producers from meeting the RPS targets of the state. Hong et al., (2023) found that municipally-owned utilities are significantly less affected by RPS than investor-owned utilities (IOUs). Different from IOUs, municipal producers have smaller generation capacities and a larger share of their capacities from flexible generation technologies, such as natural gas combustion turbine, which can ramp quickly to respond to load changes.[30] The exemptions of municipal producers and/or carbon-emitting technologies, while allowing more flexible fossil-fuel generation (or less reduction in it), could help reduce the costs of integrating intermittent wind and solar energy into the grid.

5.3. Environmental co-benefits are diminished due to non-stringent RPS policies

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[30] We compare generation capacities from different fuel and technologies between the investor-owned utilities (IOUs) and the municipally-owned utilities (Muni). Appendix Figure A.4 shows that, compared to IOUs, a larger percentage of generation capacities of Muni is from flexible sources, such as pumped storage hydropower and natural gas generators.

One key debate in the efficacy of RPS is whether the monetized benefits of such policies exceed its costs. Previous studies have focused largely on decarbonization-related benefits from transiting to renewable sources of generation (Barbose et al., 2016; Greenstone & Nath, 2021; Wiser et al., 2017), with estimates ranging from $5-50 billion per year depending on scenarios projected and choices of the social cost of carbon. At the same time, studies have documented large monetary costs associated with implementing RPS policies, specifically related to the elevated energy prices that consumers face. Estimates suggest that consumers face cost hikes ranging from 2% to 11% (Barbose et al., 2016; Greenstone & Nath, 2021; Wiser et al., 2017; Wolverton et al., 2022). As argued by Greenstone and Nath (2021), this would put the economic cost of using RPS as a decarbonization tool between $60-300, exceeding the cost of many other decarbonization policies and technologies.

One potential reason that previous studies found RPS to be cost-ineffective in achieving decarbonization is that important air quality co-benefits are excluded from the analysis, especially when the air pollution benefits are masked when pooling clean and discrepant RPS policies together. Here we attempt to fill this gap by directly monetizing the estimated environmental gap between clean and discrepant RPS from observation-driven, reduced-form causal estimates to shed light on the potential economic impact of RPS policy designs.

Two challenges remain. First, while emission reductions occur from point sources within a state's jurisdictions, air pollutants like SO_2 and NO_x mixed nonuniformly in the atmosphere and can form secondary air pollutants. Furthermore, emitted pollutants can be transported across space and over state boundaries secondary through atmospheric processes, leading to non-local exposures. Second, pollution exposures have spatially heterogeneous impacts on avoided health damages because depending on population density, the number of people affected by the exposure will be heterogenous across space. This prevents us from getting a monetized value of avoided damages by simply multiplying the reduced-form coefficients with the social cost of pollution (e.g., estimates from Deschênes et al. (2017)), as is in the case of monetizing damages from reduced carbon emissions.

To address these two challenges, an integrated assessment model (IAM) is needed to link reduced-form estimates on state-level percentage reduction of pollutants to power-sector emissions to a pollutant transport model, then to population-level exposure, and finally to marginal damages for each pollutant. Prior literature in monetizing pollution damages (Abman et al., 2024; Colmer et al., 2020; Hollingsworth & Rudik, 2019; Tessum et al., 2019) have used different models, including the APEEP model (Mendelsohn & Muller, 2013), the HYSPLIT model (Draxler & Hess, 1998), the CMAQ model (Binkowski & Roselle, 2003), or the InMAP model (Tessum et al., 2017).

Here we rely on EPA's Co-Benefits Risk Assessment Health Impacts Screening and Mapping Tool (COBRA), an integrated screening model to estimate the health and economic impacts of changes in air pollutant emissions (USEPA, 2024). COBRA starts with emission inventories by sector and user-input scenario of emissions. Changes in pollution emissions are translated into changes in ambient air pollutant concentrations using a simplified source-receptor (S-R) matrix (Baker et al., 2023).[31] Specifically, SO_2 and NO_x emissions are converted into fine particulate matters ($PM_{2.5}$) by multiplying emissions with the transfer coefficients associated with PM sulfate ion and PM nitrate ion. NOx is converted into O_3 via a transfer coefficient.[32] $PM_{2.5}$ and O_3 exposure is then linked to population health impacts identified by the epidemiology literature, including impacts on mortality and morbidity outcomes.[33] Since mortality outcomes capture the majority of monetized $PM_{2.5}$ damages, COBRA provides two sets of monetized estimates, one based on Wu et al. (2020)'s lower estimate, and the other based on Pope et al. (2019)'s higher estimate to reflect the scientific uncertainty related to the $PM_{2.5}$ – mortality relationship.

We monetize the foregone benefits from RPS design discrepancy by injecting the medium-run reduced-form estimates of emissions between clean and discrepant RPSs in Table 5, 55.4% for SO_2 and 33.2% for NO_x, into COBRA version 5.0.[34] We use COBRA's 2016 baseline emission scenario for the power sector, with the list of RPS policies taken from 2010. 31 states

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[31] As Baker et al. (2023) documented, the S-R approach performs similarly to more complicated pollutant transport models like CMAQ.

[32] NO_x and VOC (volatile organic compounds) form O_3 via photochemical reactions. SO_2 does not form O_3.

[33] The full sets of health and occupational impacts considered in COBRA include health impacts related to mortality (general population and infant), nonfatal heart attacks, asthma, emergency room visits, hospitalization, stroke, lung cancer, and work and school losses (USEPA 2024).

[34] The reduced-form estimates from Table 5 are in a log-linear form. Converting them back into percentage terms yield $1-e^{-0.590} = 0.445$, and $1-e^{0.403} = 0.332$.

had active RPS policies in place in 2010, with 21 discrepant and 10 clean RPSs.[35] As mortality outcomes involves both short-term and long-term death after exposure, we use a 2% discount rate following Office of Management and Budget (OMB)'s guidance laid out in Circular A-4.[36]

We find that the foregone benefit caused by RPS design discrepancies ranges from $12 billion to $22 billion annually (in 2016 dollars). Elevated levels of $PM_{2.5}$ causes 650-1500 foregone (statistical) lives saved, which monetize to $7.2-16 billion. Elevated levels of O_3 caused foregone 400 foregone lives saved, which monetized to $4.3 billion. Other estimated foregone damages account for less than 10% of the total monetized value. A detailed breakdown of monetized value by category is presented in Appendix Table A.4.

We find that only 43% of the foregone benefits accrue in states with a discrepant RPS. 57% of the foregone benefits come from states that did not implement a discrepant RPS.[37] This highlights recent debates in the transboundary effects of point sources pollution, for example in Ohio v. EPA (2024), where the Supreme Court struck down EPA's "Good Neighbor Rule", requiring upwind power plants and other point sources to consider effects for downwind states. We also find that the largest foregone benefits occur in the Great Lakes and the Northeast region (see geographical distribution of foregone benefits in Figure 4), similar to that of Hollingsworth and Rudik (2019). Of the top 20 counties with the largest foregone benefits, 5 are in Pennsylvania, 5 are in New York, 3 are in Maryland, 3 are in Michigan, and 2 are in Ohio.[38] Of which, Maryland, New York, and Ohio have clean RPS policies in 2010, again highlighting the transboundary nature of point source pollution reduction.

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[35] The 22 states with discrepant RPSs are Arizona, California, Colorado, Connecticut, Maine, Michigan, Minnesota, Missouri, Montana, Nevada, New Mexico, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, South Dakota, Utah, Vermont, Virginia, and Washington. The 10 states with clean RPSs are Delaware, Illinois, Massachusetts, Maryland, New Hampshire, New Jersey, New York, Ohio, Rhode Island, and Wisconsin. Hawaii is excluded from the analysis because COBRA only covers the continental US.

[36] Available at https://www.whitehouse.gov/wp-content/uploads/2023/11/CircularA-4.pdf.

[37] Our calculation is conservative in that states with discrepant RPSs also would have received benefits from other states that has discrepant RPSs.

[38] The top 20 counties ranked by the largest foregone benefits are: Cook County, Illinois; Wayne County, Michigan; Allegheny County, Pennsylvania; St Louis County, Missouri; Cuyahoga County, Ohio; Philadelphia County, Pennsylvania; Oakland County, Michigan; Macomb County, Michigan; Erie County, New York; Kings County, New York; Baltimore County, Maryland; Westmoreland County, Pennsylvania; Franklin County, Ohio; Montgomery County, Pennsylvania; Queens County, New York; Nassau County, New York; Suffolk County, New York; Baltimore City County, Maryland; Lancaster County, Pennsylvania; and Montgomery County, Maryland.

We also calculated the actual benefits (avoided damages) generated by RPS policies using the reduced-form estimates from Table 5, by separately calculating the counterfactual environmental outcomes for clean and discrepant RPS policies had they not been adopted by 2010.[39] The total health benefits from RPS policies range from \$17 billion to \$34 billion, with $PM_{2.5}$ causing \$13-30 billion and O_3 causing \$4 billion.[40] The impact from ozone is much smaller than the impact from particulate matters because we find zero impacts of discrepant RPS on NO_x emission reductions, which in turn leads to ozone. We again find that the benefits are concentrated in the Great Lakes and the Northeast region, similar to that of Hollingsworth and Rudik (2019).[41, 42]

Our findings suggest that RPSs generate economically significant co-benefits from air pollution reductions, comparable to their compliance costs and benefits from decarbonization. Compliance cost of RPS estimated to range from \$4 billion annually with a 2% electricity price increase in Barbose et al. (2019) and Wolverton et al. (2023), to \$30 billion annually with a 11% electricity price increase in Greenstone and Nath (2021). Estimated benefits from decarbonization ranges from \$28 billion (Wiser et al. 2017) to \$33 billion (Greenstone and Nath, 2021) under a social cost of carbon \$185-190 per ton of CO_2 suggested by recent literature and regulatory determination (Rennert et al., 2022; USEPA, 2023).[43]

Estimation of the air pollution co-benefits is rare in previous literature, with the closest study being Wiser et al. (2017), who find that RPSs yield an air pollution benefit of \$4.6 billion.[44] Their high renewable scenario, for which states adopt aggressive RPS policies, lead to an annual

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[39] States with clean RPS would see their SO_2 and NO_x emissions to be 56.65% and 33.16% higher in 2016 if not for the passage of a clean RPS on or before 2010. States with discrepant RPS would see their SO_2 to be 21.81% higher, and NO_x to remain the same in 2016, if not for the passage of a discrepant RPS on or before 2010.

[40] A detailed breakdown of monetized value by category is presented in Appendix Table A.5.

[41] The top 20 counties ranked by actual avoided damages are: Cook County, Illinois; Allegheny County, Pennsylvania; Cuyahoga County, Ohio; Wayne County, Michigan; Franklin County, Ohio; Hamilton County, Ohio; Philadelphia County, Pennsylvania; St Louis County, Missouri; Oakland County, Michigan; Baltimore County, Maryland; Kings County, New York; Baltimore City, Maryland; Summit County, Ohio; Macomb County, Michigan; Erie County, New York; Montgomery County, Ohio; Queens County, New York; Marion County, Indiana; Nassau County, New York; and Prince Georges County, Maryland.

[42] Readers are referred to Appendix Figure A.5 for the full spatial distribution of actual avoided damages.

[43] The original estimate from Wiser et al. (2017) was \$161 billion over the period of 2015-2050, which annualizes to \$7.5 billion per year under a social cost of carbon of \$50 per ton of CO_2.

[44] Hollingsworth and Rudik (2019) estimated the avoided damages of a 1% increase in REC demand, though they do not provide an aggregate number across the nation.

benefit of $26 billion. Our findings are larger in magnitude for a number of reasons. First, our analysis takes into account new advances in the literature on the air pollution – mortality relationship, leading to higher instances of mortality in the case of particulate matters exposure and added mortality effects of Ozone exposure. Second, our analysis uses a lower discount rate of 2%, which increases the present value benefits from long-term exposure-related mortality outcomes.

6. Conclusions

This study provides new empirical evidence on the environmental impacts of Renewable Portfolio Standards (RPS) and highlights the critical role of policy design in determining their effectiveness. Using a difference-in-differences approach with staggered adoption timing, we find that RPS policies generally lead to reductions in power sector emissions and improvements in air quality. However, our analysis reveals substantial heterogeneity in these effects based on the stringency of RPS policy enforcement.

Our key contribution is to demonstrate that discrepancies in RPS policy design - such as exemptions, credit multipliers, and inclusion of carbon-emitting technologies - significantly attenuate the environmental benefits of these policies. This attenuation is particularly pronounced for non-CO2 pollutants like SO2 and NOx. We estimate that in the medium term (5-9 years post-implementation), clean RPSs reduce SO2 emissions by 57.6% and NOx emissions by 33.7%, while discrepant RPSs lead to only a 25.1% reduction in SO2 and negligible effects on NOx. These differences translate into substantial foregone health co-benefits, which we estimate to be between $12 billion and $22 billion annually. Most of these foregone benefits would have been accrued outside of the immediate state boundary that implemented the discrepant RPS policies.

The stark contrast in outcomes between clean and discrepant RPS underscores the critical importance of policy design stringency and their unintended consequences. Our results demonstrate that seemingly minor differences in RPS policy design can lead to substantial variations in environmental co-benefits, highlighting potential tradeoffs between consumer welfare, public finance, and policy efficacy. This emphasizes the need for policymakers to carefully consider the long-term and wide-ranging impacts of their policy choices, as well as potential interactions with other existing regulations and market forces.

Our study also reveals important interstate externalities in renewable energy policy. While the carve-outs and electricity price savings usually benefit constituents within state borders or within the same electricity market, major proportions of the foregone health benefits from discrepant RPS designs would be accrued to other states. This situation exemplifies the classic problem of under-provision of public goods, where individual states may not have sufficient incentives to implement stringent policies due to spillover benefits. Our findings echo previous studies and have implications for environmental federalism (Holland et al., 2016; Blundell et al., 2021; Aronoff & Rafey, 2023; He et al., 2020), indicating that purely state-level decision-making may not fully account for the broader regional or national impacts of local policies. It underscores the potential value of regional cooperation or federal coordination in maximizing the benefits of renewable energy policies.

Finally, our research highlights the substantial co-benefits of stringent RPS policies, particularly in terms of improved air quality and associated health outcomes. The magnitude of these co-benefits suggests that evaluations of RPS policies focusing solely on carbon reductions or renewable energy deployment may significantly underestimate their total societal value. This finding calls for a more comprehensive approach to policy assessment that fully accounts for these additional benefits. It implies that the true cost-effectiveness of RPS policies may be considerably higher than previously estimated when these co-benefits are factored in. Policymakers should therefore consider these broader impacts when designing and evaluating renewable energy policies, potentially leading to more ambitious and stringent clean energy targets that can be justified by the full range of benefits they provide.

As the United States and other nations accelerate their clean energy transition, important lessons can be drawn from the implementation of state-wide renewable energy policies. Our findings suggest that the path to effective decarbonization is not merely about setting ambitious targets, but also about crafting policies with careful attention to enforcement mechanisms, cross-border impacts, and comprehensive benefit assessments. Although this study does not focus on it, policy designs affecting legal enforceability and transparency can influence the equitable distribution of policy effects. For example, if exempted energy producers are located near disadvantaged communities, the foregone benefits will disproportionally burden these areas. Future research could build on these findings by further exploring the downstream impacts of environmental performance, downscaling the analysis into plant-level analyses to reveal more insights, and investigating the distributional consequences of various RPS designs.

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Figures and Tables

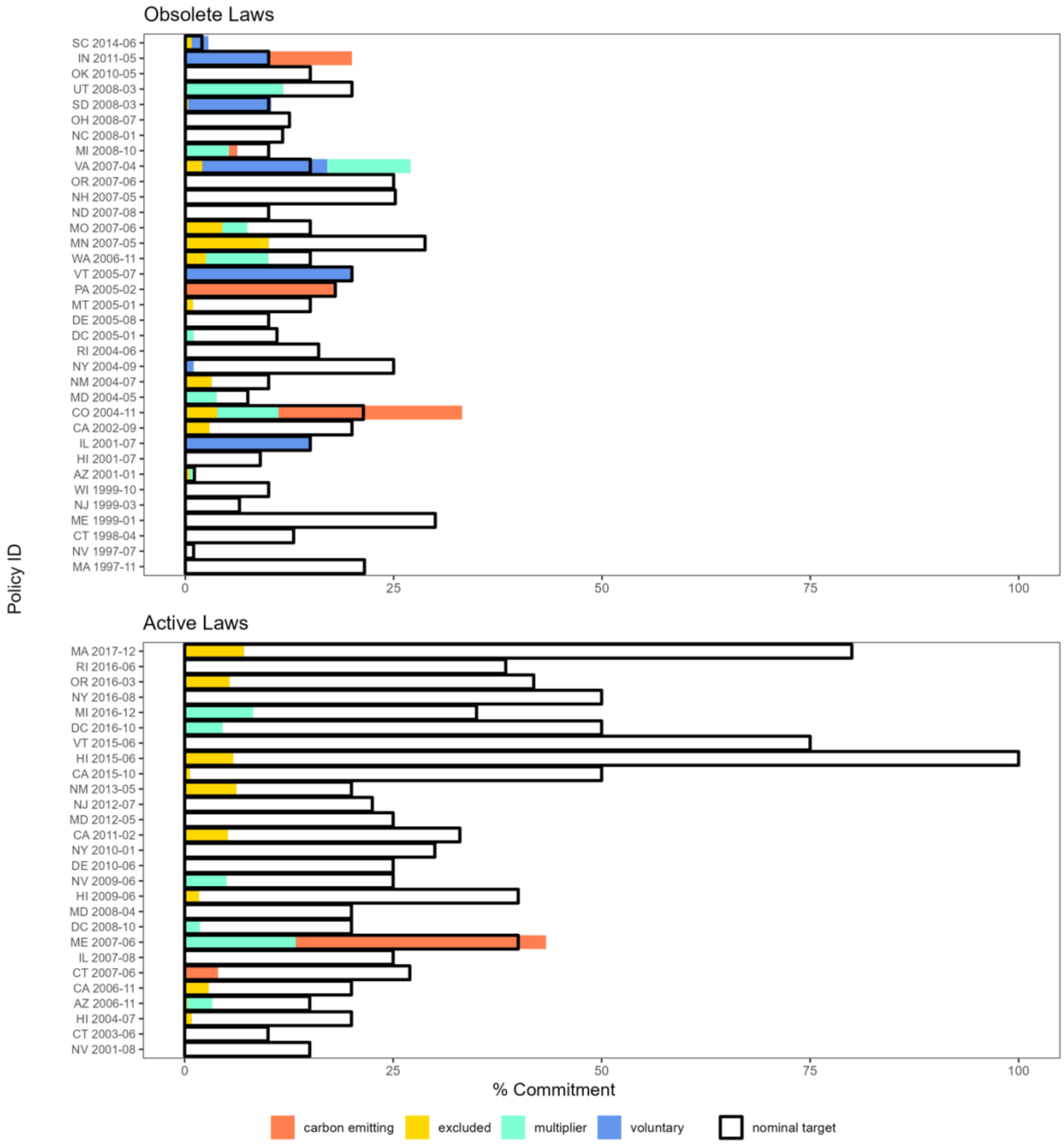

Figure 1 RPS policies, nominal commitments, and characterized discrepancy. The Y-axis labels denote each RPS policy bill by their respective state and time (year-month) of implementation uniquely identifying each law. Hollow bar with black border denotes the nominal commitment in percentage. Orange, yellow, turquoise, and blue bars denote carbon-emitting, excluded, multiplier, and voluntary shares of commitment respectively.

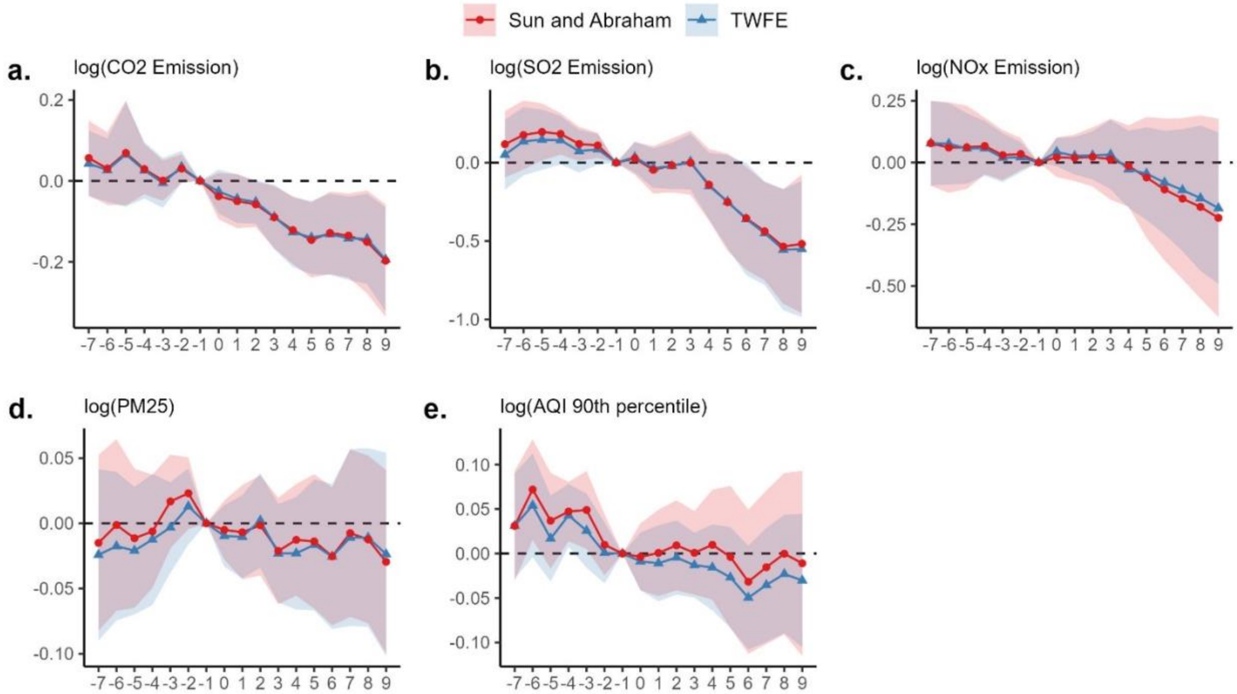

Figure 2 Event-study plots show estimates of the β_τ coefficients in equation (1). Dynamic effects of RPS policy without distinguishing between the clean and discrepant RPS policies on the emission outcomes are shown for the natural logarithms of (a) CO2 emission, (b) SO2 emission, and (c) NOx emission. Dynamic effects on the air quality outcomes are shown for the natural logarithms of (d) PM2.5 annual average and (e) AQI 90^{th}-percentile. Blue line with triangles depicts the DID estimators of two-way fixed effect (TWFE). Red line with circle markers depicts the DID estimators of Sun and Abraham (2021). The ribbon areas are the 95% confidence intervals of the estimators.

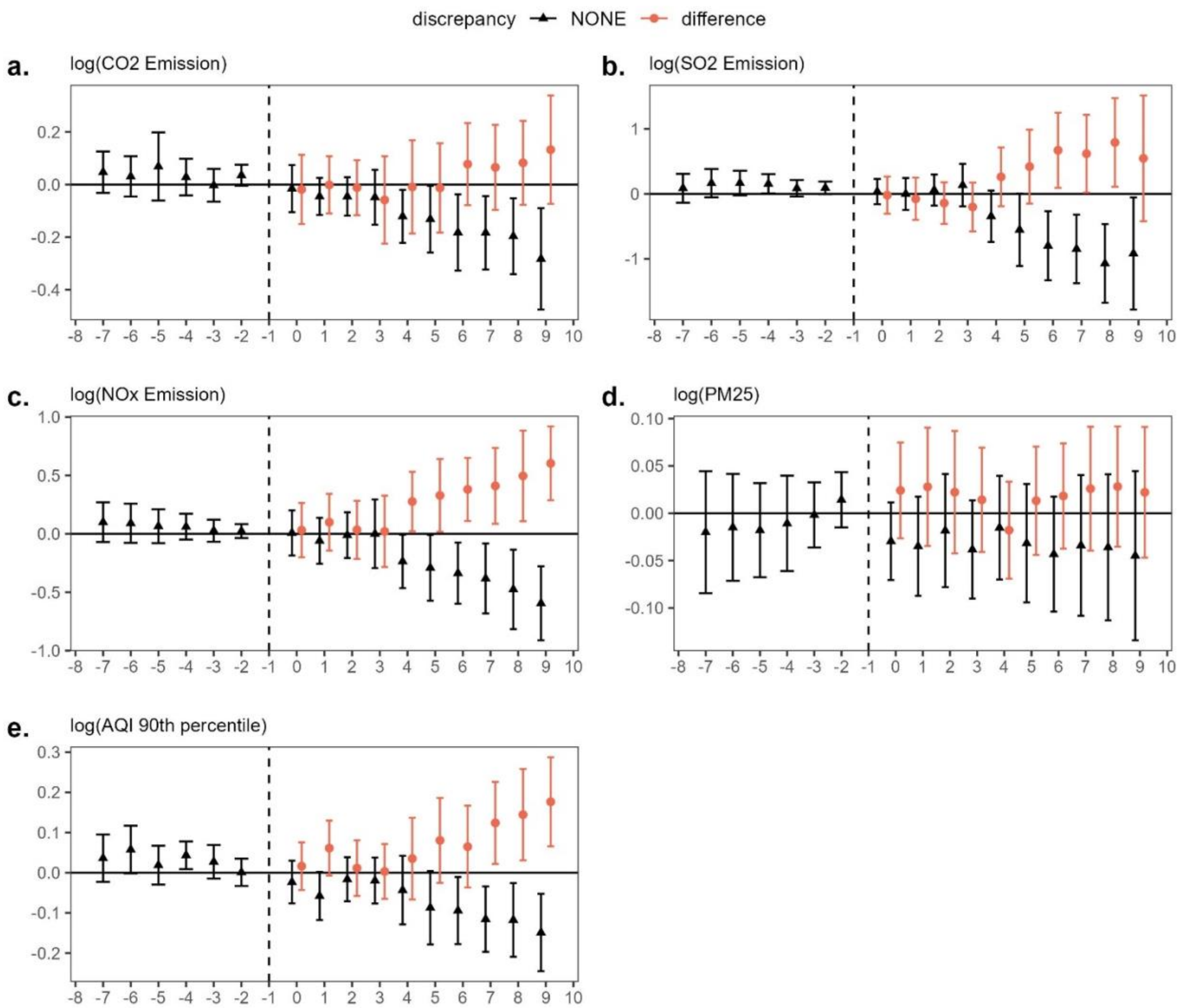

Figure 3 This figure presents estimates of the β_τ and γ_τ coefficients in equation (2). Discrepancy implies the existence of non-binding targets in the active RPS legislation, varying by state and by year. The environmental outcomes of interest in panel (a)-(e) are the natural logarithms of CO2 emission, SO2 emission, NOx emission, PM2.5 annual average, and AQI 90th-percentile. The points and error bars are the estimates and 95% confidence intervals of the baseline discrepant-free RPS treatment effects (black triangles) and of the differences due to the presence of discrepant policy designs (orange circles).

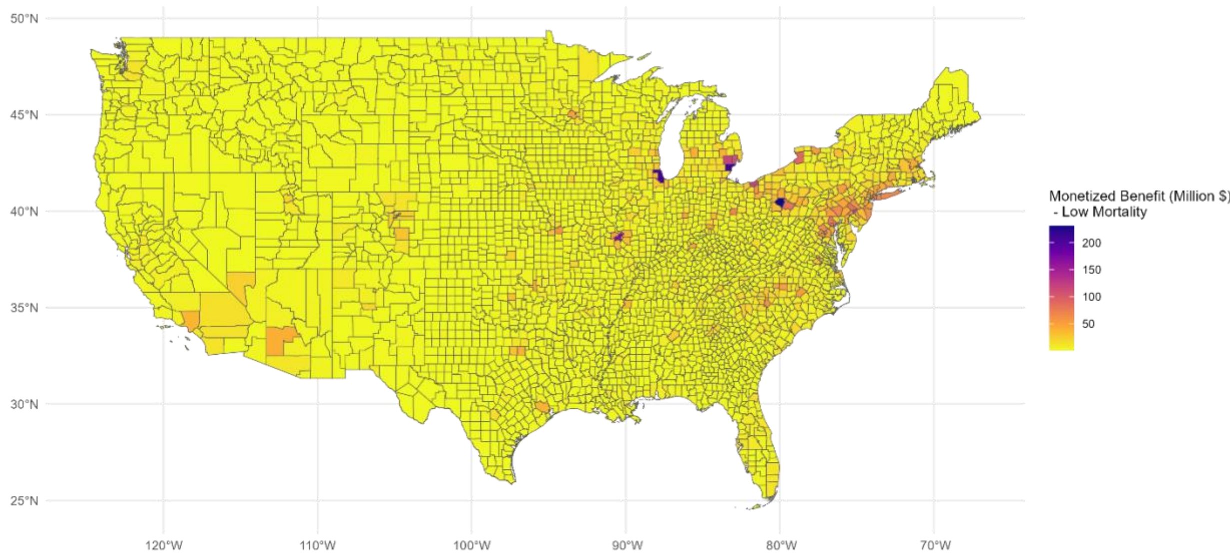

Figure 4 Foregone Benefits Caused by RPS Discrepancy. Map depicts the monetized foregone benefits in 2016 (in million 2016 dollars), as a result of discrepant RPS policies active in 2010, for each county in the continental US. Lighter yellow – darker purple gradients denote lower to higher foregone benefits. Estimates are generated using COBRA's low PM-mortality instance estimates based on Wu et al. (2019) under a 2% discount rate.

Table 1 State Renewable Portfolio Standards and targets defined in the first RPS legislation. The legislative bill identifier, enact date, nominal and binding minimum targets are listed for the 34 RPS states in our sample. Iowa, Texas, Kansas, and West Virginia are excluded.

State	Legislative Bill Identifier	Enact Date	Nominal Target (percent)	Binding Minimum (percent)
AZ	AAC R14-2-1618	Jan 01, 2001	1.1	0.1
CA	CA SB 1078	Sep 12, 2002	20.0	17.1
CO	CRS 40-2-124	Nov 01, 2004	21.4	0.0
CT	CT HB 5005	Apr 29, 1998	13.0	13.0
DE	DE SB 74	Aug 08, 2005	10.0	10.0
HI	HI HB 173	Jul 12, 2001	9.0	9.0
IL	20 ILCS 688	Jul 01, 2001	15.0	0.0
IN	IN SB 251	May 19, 2011	10.0	0.0
MA	MA General Statutes C. 25A S. 11F	Nov 25, 1997	21.5	21.5
MD	MD SB 869	May 20, 2004	7.5	3.8
ME	ME PL C. 316	Jan 01, 1999	30.0	30.0
MI	MI SB 213	Oct 06, 2008	10.0	4.3
MN	MN SF 146	May 04, 2007	28.8	18.7
MO	MO R.S. 393.1020 et seq.	Jun 27, 2007	15.0	7.6
MT	MT MCA 69-3-2001 et seq.	Jan 04, 2005	15.0	14.1
NC	NC SB 3	Jan 01, 2008	11.7	8.6
ND	ND HB 1506	Aug 24, 2007	10.0	9.8
NH	NH Title XXXIV Section 362F	May 14, 2007	25.2	25.2
NJ	NJ PL 1999 Ch. 23	Mar 01, 1999	6.5	6.5
NM	NM SB 43	Jul 01, 2004	10.0	6.8
NV	NV AB 226	Jul 01, 1997	1.0	1.0
NY	NY Public Service Commission Order approving RPS	Sep 24, 2004	25.0	24.0
OH	OH SB 221	Jul 31, 2008	12.5	12.5
OK	OK HB 3028	May 27, 2010	15.0	14.8
OR	OR SB 838	Jun 06, 2007	25.0	25.0
PA	PA SB 1030	Feb 28, 2005	18.0	0.0
RI	RI S 2082	Jun 29, 2004	16.0	16.0
SC	SC SB 1189	Jun 02, 2014	2.0	0.0
SD	SD HB 1123	Mar 21, 2008	10.0	0.0
UT	UT SB 202	Mar 27, 2008	20.0	8.3
VA	VA. Code § 56-585.2	Apr 04, 2007	15.0	0.0
VT	VT Statutes 30-8001	Jul 01, 2005	20.0	0.0
WA	WA Initiative 936	Nov 15, 2006	15.0	5.0
WI	WI State Legislature CH 196	Oct 27, 1999	10.0	10.0

Table 2 Descriptive statistics of RPS and control states for difference-in-differences (DID) estimation. Panel A summarizes the outcome variables in the DID regression models. Panel B summarizes the covariates included in the DID regression models. Panel C summarizes the policy design variables including characterized discrepancies in years post states' RPS implementations.

Panel A: Outcome Variables						
	RPS states			**Control states**		
	N	**Mean (std dev)**	**Min-Max**	**N**	**Mean (std dev)**	**Min-Max**
CO_2 (1000 metric tons)	986	37339.63 (32408.56)	6.58-133416.55	348	47592.93 (33431.82)	438.13-131543.37
SO_2 (1000 metric tons)	986	163.75 (279.94)	0.01-2045.98	348	205.47 (232.05)	2.56-892.35
NO_x (1000 metric tons)	986	78.13 (91.58)	0.15-535.62	348	97.94 (89.76)	1.93-353.55
AQI 90th percentile	986	74.71 (21.20)	29.73-164.65	319	69.47 (17.11)	36.97-131.84
PM2.5 (µg/m^3)	693	9.20 (2.74)	4.40-16.80	231	9.97 (2.92)	4.80-17.20
% of Generation: Coal and Oil	986	45.80 (30.24)	0.03-96.96	348	48.56 (28.01)	0.11-97.56
% of Generation: Natural Gas	986	18.50 (22.36)	0.00-98.94	348	20.76 (22.29)	0.03-79.67
% of Generation: Wind & Solar	986	2.09 (4.77)	0.00-32.23	348	0.92 (2.99)	0.00-18.48
% of Generation: Hydro & Nuclear	986	30.35 (25.02)	0.00-96.46	348	27.33 (20.21)	1.32-94.28
% of Generation: Other Renewable	986	2.84 (4.76)	0.00-37.14	348	2.06 (1.67)	0.00-8.54
Electricity Price (cents per kWh, 2015 USD)	986	10.44 (3.53)	5.10-34.04	348	8.46 (2.37)	5.10-17.73
Panel B: State-level Time-varying Covariates						
	RPS states			**Control states**		
	N	**Mean (std dev)**	**Min-Max**	**N**	**Mean (std dev)**	**Min-Max**
Democrat-controlled Legislature	986	0.41 (0.49)	0-1	348	0.35 (0.48)	0-1
Republican-controlled Legislature	986	0.34 (0.48)	0-1	348	0.44 (0.50)	0-1
Democratic-controlled Governorship and Legislature	986	0.22 (0.42)	0-1	348	0.19 (0.39)	0-1
Democratic-controlled Governorship and Legislature	986	0.24 (0.43)	0-1	348	0.32 (0.47)	0-1
Governor's Party (0=GOP; 1=DEM)	986	0.48 (0.49)	0-1	348	0.40 (0.48)	0-1
Log(Gross State Product)	986	11.85 (1.12)	9.35-14.82	348	11.53 (0.95)	9.45-13.74

Natural Gas Price ($ per 1000 ft^3)	986	6.13 (3.00)	2.00-32.39	348	5.38 (2.11)	0.48-11.37
Log(Population)	986	15.14 (1.02)	13.24-17.49	348	14.90 (0.98)	13.03-16.87
Net Metering Program (0=No, 1=Yes)	986	0.63 (0.48)	0-1	348	0.34 (0.48)	0-1
NOx Trading Program (0=No, 1=Yes)	986	0.09 (0.29)	0-1	348	0.05 (0.22)	0-1
% of Energy Exported	986	0.09 (0.44)	-0.84-2.74	348	0.17 (0.60)	-0.64-2.61
Heating Degree Days (HDD)	986	5553.19 (2055.92)	0.00-10810.00	348	4461.66 (2821.51)	430.00-11702.00
Cooling Degree Days (CDD)	986	964.86 (900.16)	42.00-5213.00	348	1518.92 (994.76)	0.00-4156.00
Panel C: Policy Design Variables post RPS Implementation						
	RPS states			**Control states**		
	N	**Mean (std dev)**	**Min-Max**	**N**	**Mean (std dev)**	**Min-Max**
Discrepant Law in Effect (binary)	489	0.66 (0.47)	0-1	--	--	--
Nominal Target (% of total sales)	489	20.54 (13.87)	1.00-100.00	--	--	--
Binding Minimum (% of total sales)	489	5.28 (7.55)	0.00-33.33	--	--	--
Excluded Sales (% of total sales)	489	1.03 (2.10)	0.00-10.02	--	--	--
Voluntary Sales (% of total sales)	489	1.38 (4.30)	0.00-20.00	--	--	--
Multiplier Credit (% of total sales)	489	1.69 (3.49)	0.00-13.33	--	--	--
Carbon Emitting (% of total sales)	489	2.20 (6.60)	0.00-30.00	--	--	--

Table 3 Diagnostic Tests for Parallel Trend for the Discrepancy Model

Outcome	Event Study Coefficients: Control vs. Clean RPS					
	Pre-trend Test		Power Analysis (Roth 2022)			
			Positive Pretrend		Negative Pretrend	
	Pretrend Level	Pretrend Slope	Bayes Factor	Likelihood Ratio	Bayes Factor	Likelihood Ratio
log(CO2 Emission)	-0.039***	-0.017	0.119	0	0.119	0
log(SO2 Emission)	-0.064***	0.011	0.116	0.001	0.116	0.001
log(NOx Emission)	-0.01***	0.008	0.119	0.253	0.119	0
log(PM25)	-0.022***	0.018**	0.118	171,678	0.118	0
log(AQI 90th percentile)	-0.035***	0.002	0.121	0.057	0.121	0.002
% of Generation: Coal & Oil	-3.85***	0.567	0.12	0.794	0.12	0
% of Generation: Natural Gas	3.779***	-1.553***	0.119	0	0.119	0.228
% of Generation: Wind and Solar	-0.993**	0.252**	0.116	0.759	0.116	0
% of Generation: Hydro and Nuclear	0.996***	0.861	0.118	0	0.118	0
% of Generation: Other Renewable	0.002	-0.005	0.113	0	0.113	0.002
Electricity Price	0.443***	-0.07	0.121	0	0.121	0.211

Note: Diagnostic tests based on event study regressions that estimates differences in outcomes between clean and discrepant RPS states over time. Sample consists of states with consistently clean and discrepant RPS policies as well as non-RPS states. States with both clean and discrepant RPS policies (i.e., switchers) are omitted. Pre-trend test in column 2 presents the average of pre-trend coefficients for five pre-treatment years, β_{-6} to β_{-2}, as well as hypothesis tests on those coefficients being jointly zero (indicated by the trailing stars). Column 3 presents the linear trend coefficient for the five pre-treatment years plus the reference period, β_{-6} to β_{-1}, as well as hypothesis tests on the trend coefficient equal to zero (indicated by the trailing stars). Pre-trend power analysis follows Roth (2022) by selecting a pre-trend slope for 5 pre-treatment periods (periods -6 to -2) and test the likelihood of the observed 10 post-treatment effects (periods 0 to 9) under parallel trend versus the hypothesized linear pretrend. Slope of the linear trend is selected such that the power of the pretest is close to to 0.9. The sign of the linear slope is positive for columns 4-5, and negative for columns 6-7. The Bayes factor (column 4 and 6) is the ratio of the probability of "passing" the pre-test under the hypothesized linear trend relative to under parallel trends: smaller BF means more likely to pass the pre-test. The likelihood ratio (column 5 and 7) corresponds to the ratio of the likelihood of the observed coefficients under the hypothesized linear trend relative to under parallel trends: smaller likelihood ratio means observed coefficients are more likely to be observed under parallel trend. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table 4 Break model ATT estimates. The estimates represent the time-varying effects of an RPS policy in general, without distinguishing across different policy designs.

Panel A: Estimates for the emission and air quality outcomes						
	(1)	(2)	(3)	(4)	(5)	
	log(CO_2)	log(SO_2)	log(NO_x)	log(PM2.5)	log(AQI_{90})	
0-4 years post RPS	-0.083**	-0.102	-0.011	-0.011	-0.033*	
	(0.041)	(0.089)	(0.056)	(0.019)	(0.019)	
5-9 years post RPS	-0.152***	-0.467***	-0.128	-0.019	-0.054*	
	(0.051)	(0.172)	(0.123)	(0.030)	(0.032)	
≥ 10 years post RPS	-0.243***	-0.802***	-0.213	0.014	-0.065	
	(0.079)	(0.236)	(0.190)	(0.045)	(0.049)	
Obs.	1334	1334	1334	924	1305	
R^2	0.987	0.945	0.944	0.933	0.831	
Panel B: Estimates for the power sector outcomes						
	(6)	(7)	(8)	(9)	(10)	(11)
	Coal and Oil	Natural Gas	Wind and Solar	Hydro and Nuclear	Other Renewables	Electricity Price
0-4 years post RPS	-2.154*	0.155	1.393**	0.274	-0.020	0.035
	(1.194)	(1.683)	(0.689)	(1.288)	(0.229)	(0.240)
5-9 years post RPS	-6.965***	3.279	2.024*	1.541	-0.264	0.226
	(2.448)	(2.756)	(1.126)	(2.001)	(0.298)	(0.302)
≥ 10 years post RPS	-9.408**	5.275	1.668	1.762	-0.010	0.417
	(4.335)	(4.224)	(1.329)	(3.222)	(0.495)	(0.516)
Obs.	1334	1334	1334	1334	1334	1334
R^2	0.945	0.902	0.658	0.948	0.925	0.924

Notes: In Panel A, the dependent variables are the natural logarithm of CO2 emission (1), natural logarithm of SO2 emission (2), natural logarithm of NOx emission (3), natural logarithm of AQI 90th-percentile (4), and natural logarithm of PM2.5 annual average (5). In Panel B, the dependent variables are the percentage of power generation by coal and oil (6), natural gas (7), wind and solar (8), hydro and nuclear (9), other renewables (10), and the electricity price (11). All regression models are estimated controlling the covariates in

Table 2 and including state and year fixed effects. Standard errors are clustered at the state level. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table 5 Break model ATT estimates for the emission and pollution outcomes, distinguishing between "clean" RPS and the differences due to discrepant policy designs.

	(1)	(2)	(3)	(4)	(5)
	log(CO_2)	log(SO_2)	log(NO_x)	log(PM2.5)	log(AQI_{90})
Panel A: Heterogeneous Effects Associated to Policy Discrepancy					
Baseline ATT of Discrepancy-free RPS					
0-4 years post RPS	-0.053	-0.065	-0.074	-0.030	-0.054**
	(0.045)	(0.119)	(0.081)	(0.024)	(0.023)
5-9 years post RPS	-0.185***	-0.859***	-0.411***	-0.044	-0.129***
	(0.068)	(0.259)	(0.140)	(0.031)	(0.041)
≥ 10 years post RPS	-0.263***	-1.187***	-0.632***	-0.053	-0.176***
	(0.097)	(0.312)	(0.195)	(0.046)	(0.057)
ATT Differences due to RPS Policy Discrepancy					
(0-4 years) × Discrepancy	-0.048	-0.089	0.064	0.020	0.025
	(0.065)	(0.146)	(0.099)	(0.022)	(0.025)
(5-9 years) × Discrepancy	0.046	0.570**	0.412***	0.029	0.112**
	(0.081)	(0.276)	(0.144)	(0.021)	(0.045)
(≥ 10 years) × Discrepancy	0.030	0.578	0.631***	0.092***	0.172***
	(0.095)	(0.380)	(0.154)	(0.034)	(0.058)
Obs.	1334	1334	1334	924	1305
R^2	0.987	0.948	0.949	0.935	0.839
Summary of policy effect (using estimates of 5-9 years)	*CO_2 Emission*	*SO_2 Emission*	*NO_x Emission*	*PM2.5 Concentration*	*AQI 90^{th}-percentile*
Clean RPS	-16.9%	-57.6%	-33.7%	-4.3%	-12.1%
Discrepant RPS	-13.0%	-25.1%	0.1%	-1.5%	-1.7%
Panel B: ATT Differences by Source of Discrepancy					
(5-9 years) × exclude	0.176**	0.514	0.387**	0.030	0.170***
	(0.074)	(0.310)	(0.158)	(0.032)	(0.057)
(5-9 years) × voluntary	-0.189*	-0.009	0.018	-0.020	-0.008
	(0.101)	(0.355)	(0.306)	(0.055)	(0.084)
(5-9 years) × multiplier	-0.009	0.307	-0.045	0.024	-0.052
	(0.074)	(0.239)	(0.142)	(0.042)	(0.056)
(5-9 years) × carbon	0.106	0.158	0.185	0.035	0.062
	(0.088)	(0.222)	(0.138)	(0.026)	(0.056)
Obs.	1334	1334	1334	924	1305
R^2	0.988	0.949	0.949	0.937	0.844

Note: In Panel A, Discrepancy = 1 if the state has an active RPS legislation with non-binding target due to any of the four categories of discrepancy. In Panel B, the RPS policy discrepancy effects are examined by four types of discrepant policy designs: excluded sales (exclude = 1), voluntary sales (voluntary = 1), multiplier credit (multiplier = 1), and carbon emitting (carbon = 1). The four categories are non-exclusive, i.e., a state's RPS legislation can have multiple discrepant features. Dependent variables are the natural logarithm of CO_2 emission (1), natural logarithm of SO2 emission (2), natural logarithm of NOx emission (3), natural logarithm of PM2.5 annual average (4), and natural logarithm of AQI 90th-percentile (5). All regression models are estimated controlling the covariates in

Table 2 and including state and year fixed effects. Standard errors are clustered at the state level. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively. The summary of policy effect, in percentage change, is computed using coefficient estimates of the "5-9 years" interval.

Table 6 Break model ATT estimates for the power sector outcomes, distinguishing between "clean" RPS and the differences due to discrepant policy designs.

	(1)	(2)	(3)	(4)	(5)	(6)
	Coal and Oil	Natural Gas	Wind and Solar	Hydro and Nuclear	Other Renewable	Electricity Price
Panel A: Heterogeneous Effects Associated to Policy Discrepancy						
Baseline ATT of Discrepancy-free RPS						
0-4 years post RPS	-2.028	1.202	0.924	-0.733	0.009	-0.001
	(1.558)	(2.741)	(0.732)	(2.270)	(0.442)	(0.488)
5-9 years post RPS	-11.359***	7.358*	-0.011	3.884*	-0.185	0.037
	(3.537)	(3.982)	(0.968)	(2.312)	(0.389)	(0.407)
≥ 10 years post RPS	-13.365**	8.253	-1.465	6.072*	-0.082	0.405
	(5.711)	(5.591)	(1.329)	(3.058)	(0.456)	(0.570)
ATT Differences due to RPS Policy Discrepancy						
(0-4 years) × Discrepancy	-0.548	-1.283	0.477	1.820	-0.044	0.048
	(2.028)	(2.827)	(0.769)	(2.048)	(0.398)	(0.510)
(5-9 years) × Discrepancy	6.450*	-6.121	2.953**	-3.237*	-0.127	0.289
	(3.824)	(3.926)	(1.206)	(1.826)	(0.309)	(0.443)
(≥ 10 years) × Discrepancy	5.913	-4.395	4.720***	-6.548**	0.115	0.010
	(6.123)	(5.793)	(1.501)	(2.865)	(0.863)	(0.658)
Obs.	1334	1334	1334	1334	1334	1334
R^2	0.946	0.903	0.682	0.949	0.925	0.925
Summary of policy effect (using estimates of 5-9 years)	*Coal and Oil*	*Natural Gas*	*Wind and Solar*	*Hydro and Nuclear*	*Other Renewable*	*Electricity Price*
Clean RPS	-11.36%	7.36%	-0.01%	3.88%	-0.19%	0.04 ¢/kWh
Discrepant RPS	-4.91%	1.24%	2.94%	0.65%	-0.31%	0.33 ¢/kWh
Panel B: ATT Differences by Source of Discrepancy						
(5-9 years) × exclude	7.770*	-8.533**	6.619***	-5.927**	0.155	0.537
	(3.909)	(3.770)	(1.877)	(2.475)	(0.598)	(0.533)
(5-9 years) × voluntary	0.889	-1.765	1.108	0.083	-0.187	0.099
	(4.761)	(5.130)	(3.435)	(2.390)	(0.397)	(0.293)
(5-9 years) × multiplier	2.097	0.631	-4.678**	1.142	0.628	-0.830*
	(3.625)	(3.546)	(1.880)	(2.449)	(0.407)	(0.491)
(5-9 years) × carbon	-2.165	-0.289	3.927**	-1.227	-0.394	0.643
	(3.748)	(3.991)	(1.732)	(1.680)	(0.318)	(0.631)
Obs.	1334	1334	1334	1334	1334	1334
R^2	0.949	0.908	0.719	0.950	0.928	0.930

Note: In Panel A, Discrepancy = 1 if the state has an active RPS legislation with non-binding target due to any of the four categories of discrepancy. In Panel B, the RPS policy discrepancy effects are examined by four types of discrepant policy designs: excluded sales (exclude = 1), voluntary sales (voluntary = 1), multiplier credit (multiplier = 1), and carbon emitting (carbon = 1). The four categories are non-exclusive, i.e., a state's RPS legislation can have multiple discrepant features. Dependent variables are the percentage of power generation by coal and oil (1), natural gas (2), wind and solar (3), hydro and nuclear (4), other renewable (5), and the electricity price (6). All regression models are estimated controlling the covariates in

Table 2 and including state and year fixed effects. Standard errors are clustered at the state level. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively. The summary of policy effect, in percentage change and cent per kWh, is computed using coefficient estimates of the "5-9 years" interval.

Appendix Figures and Tables

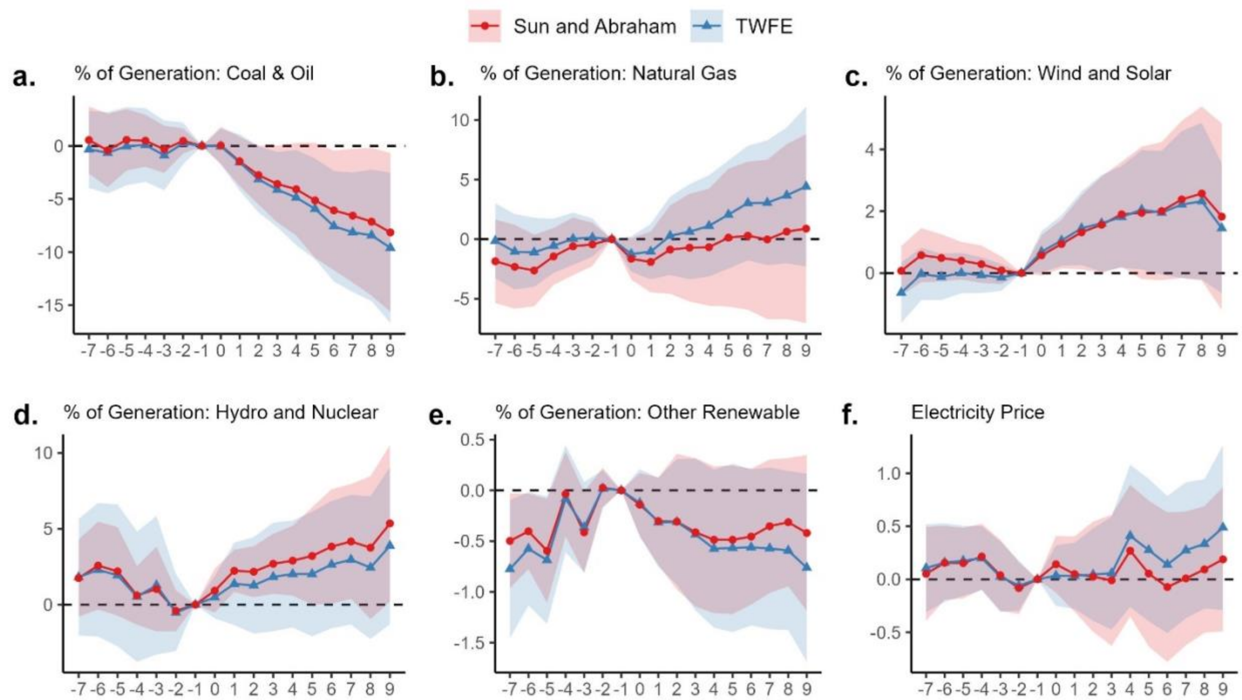

Figure A.1 Event-study plots show estimates of the β_τ coefficients in equation (1). Outcomes of interest in panels (a)-(f) are electricity generation share by fuel type including coal and oil, natural gas, wind and solar, hydro and nuclear, other renewable such as biomass and geothermal, and the electricity price. Blue line with triangles depicts the DID estimators of two-way fixed effect (TWFE). Red line with circle markers depicts the DID estimators of Sun and Abraham (2021). The ribbon areas are the 95% confidence intervals of the estimators.

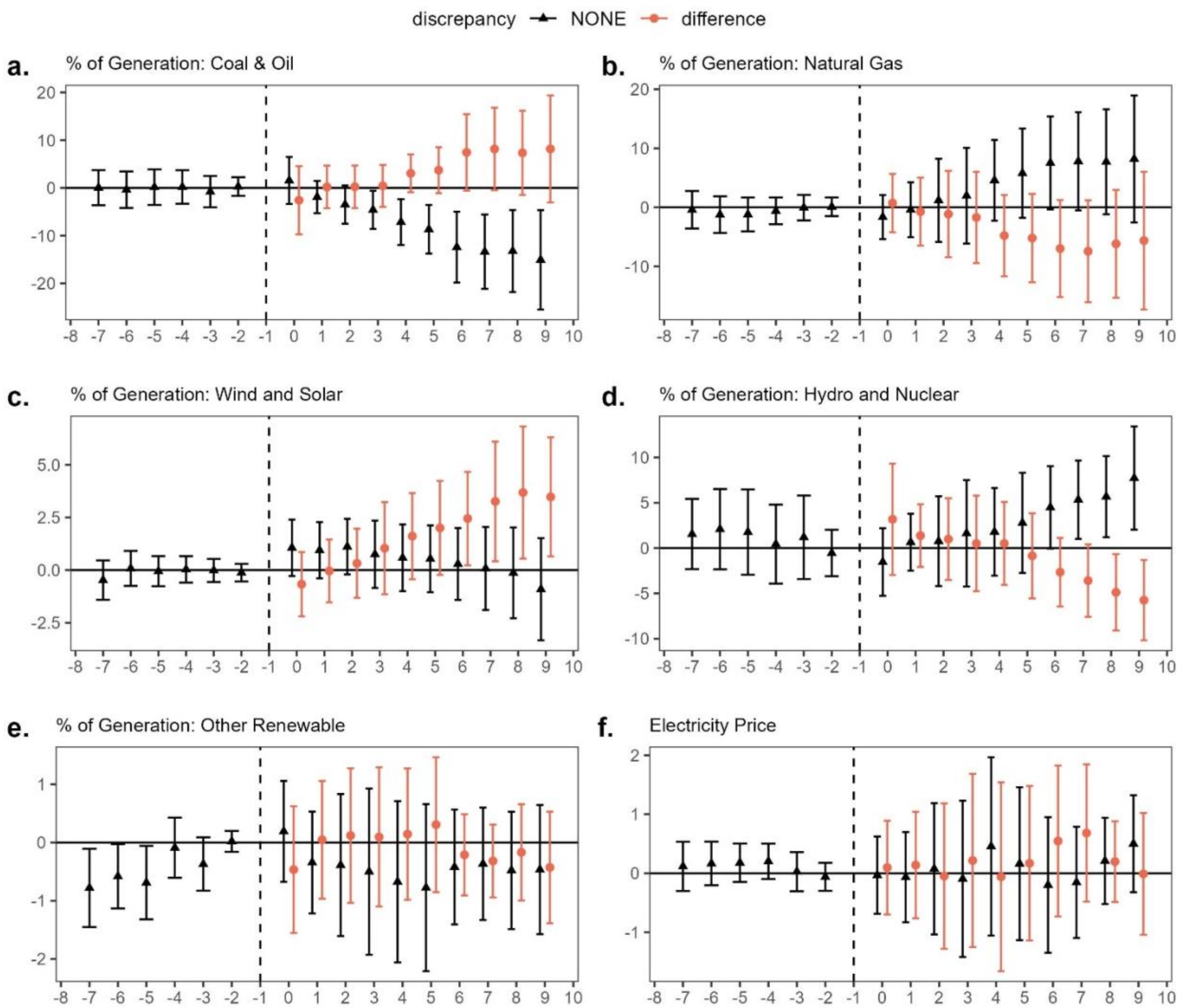

Figure A.2 This figure presents estimates of the β_τ and γ_τ coefficients in equation (2). Discrepancy implies the existence of non-binding targets in the active RPS legislation, varying by state and by year. Panels (a)-(f) depict the dynamic effects on power sector outcomes: electricity generation share by fuel type include (a) coal and oil, (b) natural gas, (c) wind and solar, (d) hydro and nuclear, (e) other renewable energy sources such as biomass and geothermal, and (f) the electricity retail price in cents per kWh. The points and error bars are the estimates and 95% confidence intervals of the baseline discrepant-free RPS treatment effects (black triangles) and of the differences due to the presence of discrepant policy designs (orange circles).

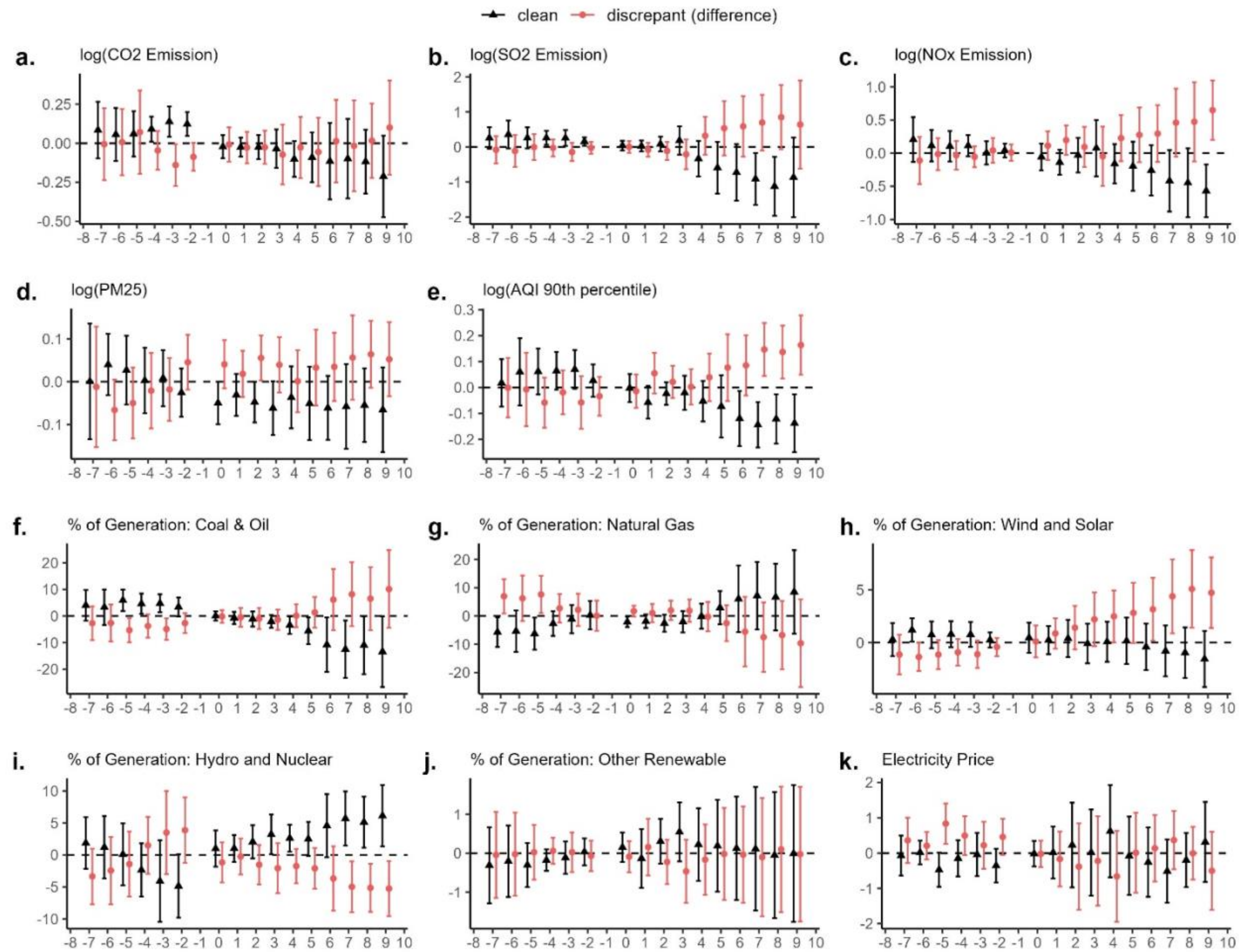

Figure A.3 Dynamic effects of RPS policy and the differences between clean and discrepant RPS states. The "difference" values depicted in the figure are coefficients of the intersections between the "discrepant RPS state" dummy and the time dummy indicating number of years post RPS passage. Clean RPS states are states with discrepancies identified in none or at most 2 post-RPS years. Discrepant RPS states are states that have never passed a clean RPS. States that had their RPS discrepancy status switch overtime are removed in this analysis. The pollution outcomes from power generation are (a) the natural logarithms of CO_2 emission, (b) SO_2 emission, (c) NO_x emission. The air quality outcomes are measured by (d) the natural logarithm of PM2.5 annual average and (e) the natural logarithm of AQI 90^{th}-percentile. Outcomes in terms of electricity generation share by fuel type include (f) coal and oil, (g) natural gas, (h) wind and solar, (i) hydro and nuclear, and (j) other renewable. The electricity price outcome (k) is in cents per kWh in 2015 dollars. The points and error bars are the coefficient estimates and 95% confidence intervals of the RPS treatment effects in a clean RPS state (black triangles) and of the differences in treatment effects if in a discrepant RPS state (orange circles).

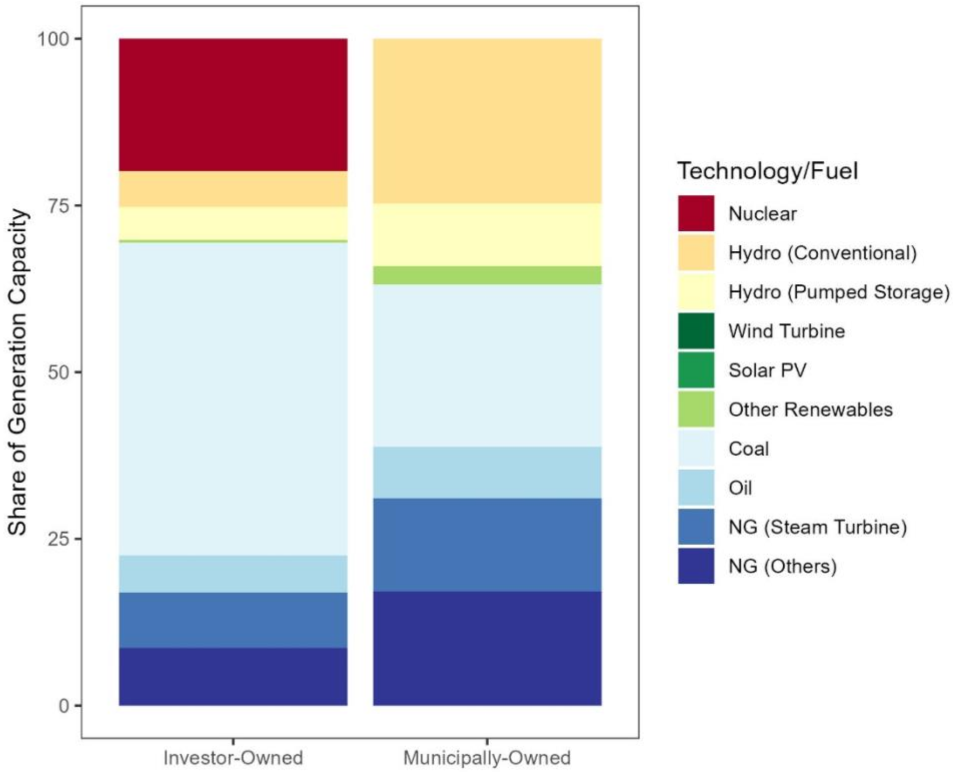

Figure A.4 Compare municipal producers and investor-owned utilities (IOUs). For the 34 RPS states in our data sample, the nameplate capacities of generators owned by investors and by municipalities are aggregated. Generator ownership data is obtained from EIA Form 860, extracting all operational generators between 1997-2014. Primary fuel of the generator and technologies are grouped to compute the share of generation capacities in each "Technology/Fuel" group. Generation technologies in the "NG (Others)" category include natural gas internal combustion engine, natural gas fired combustion turbine, natural gas fired combined cycle, and other gases.

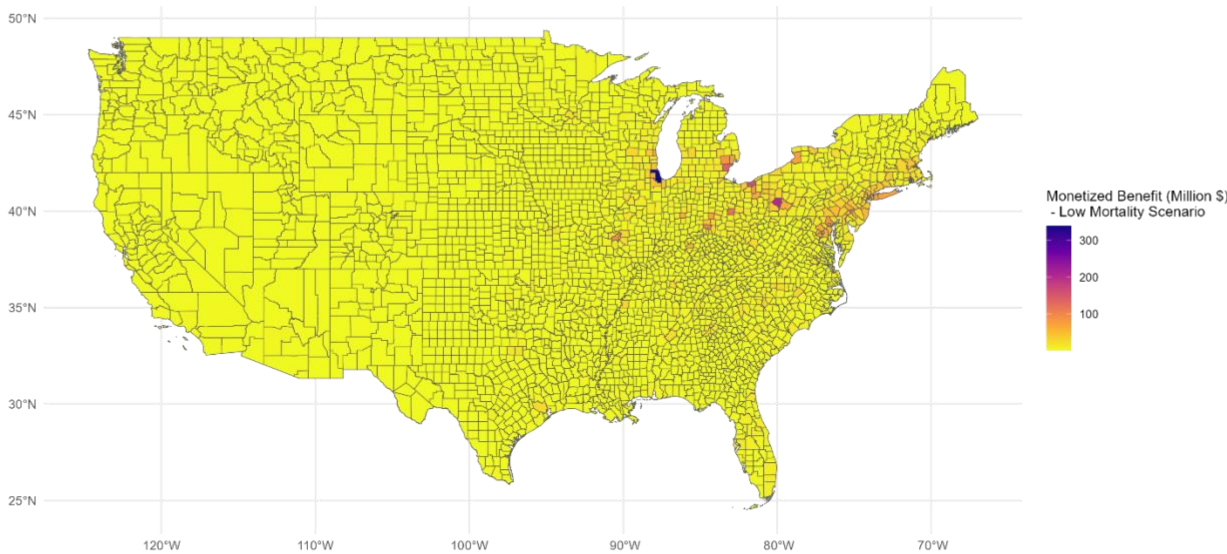

Figure A.5 Monetized Benefits for Actual RPS policy. Map depicts the monetized benefits (avoided damages) in 2016 (in million 2016 dollars), as a result of every RPS policies active in 2010 (clean and discrepant), for each county in the continental US. Darker blue color denotes higher foregone benefits. Lighter yellow color denotes lower foregone benefits. Estimates are generated using COBRA's low PM-mortality instance estimates based on Wu et al. (2019) under a 2% discount rate.

Table A.1 Diagnostic Tests for Parallel Trends for the Baseline Model

Outcome	Event Study Coefficients: Control vs. Clean RPS					
	Pre-trend Test		Power Analysis (Roth 2022)			
			Positive Pretrend		Negative Pretrend	
	Pretrend Level	Pretrend Slope	Bayes Factor	Likelihood Ratio	Bayes Factor	Likelihood Ratio
log(CO2 Emission)	0.03***	-0.007	0.116	0.001	0.116	0
log(SO2 Emission)	0.118***	-0.027**	0.114	0	0.114	0.227
log(NOx Emission)	0.046**	-0.015***	0.115	0	0.115	0.272
log(PM25)	-0.008	0.006**	0.118	0	0.118	0
log(AQI 90th percentile)	0.028***	-0.01**	0.121	0	0.121	13.322
% of Generation: Coal & Oil	-0.247***	0.088	0.112	0	0.112	0.016
% of Generation: Natural Gas	-0.509***	0.273**	0.116	0.083	0.116	0
% of Generation: Wind and Solar	-0.07	0	0.114	0.004	0.114	0.004
% of Generation: Hydro and Nuclear	1.104***	-0.517**	0.112	0	0.112	0.001
% of Generation: Other Renewable	-0.338	0.135**	0.119	0.54	0.118	0
Electricity Price	0.1***	-0.048**	0.118	0	0.118	1.112

Note: Diagnostic tests based on event study regressions that estimates differences in outcomes between RPS and non-RPS states. Pre-trend test in column 2 presents the average of pre-trend coefficients for five pre-treatment years, β_{-6} to β_{-2}, as well as hypothesis tests on those coefficients being jointly zero (indicated by the trailing stars). Column 3 presents the linear trend coefficient for the five pre-treatment years plus the reference period, β_{-6} to β_{-1}, as well as hypothesis tests on the trend coefficient equal to zero (indicated by the trailing stars). Pre-trend power analysis follows Roth (2022) by selecting a pre-trend slope for 5 pre-treatment periods (periods -6 to -2) and test the likelihood of the observed 10 post-treatment effects (periods 0 to 9) under parallel trend versus the hypothesized linear pretrend. Slope of the linear trend is selected such that the power of the pretest is close to to 0.9. The sign of the linear slope is positive for columns 4-5, and negative for columns 6-7. The Bayes factor (column 4 and 6) is the ratio of the probability of "passing" the pre-test under the hypothesized linear trend relative to under parallel trends: smaller BF means more likely to pass the pre-test. The likelihood ratio (column 5 and 7) corresponds to the ratio of the likelihood of the observed coefficients under the hypothesized linear trend relative to under parallel trends: smaller likelihood ratio means observed coefficients are more likely to be observed under parallel trend. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table A.2 Break model ATT estimates for the emission and pollution outcomes by four categories of discrepancy

	(1) $\log(CO_2)$	(2) $\log(SO_2)$	(3) $\log(NO_x)$	(4) log(PM2.5)	(5) $\log(AQI_{90})$
0-4 years post RPS	-0.101*	-0.126	-0.076	-0.024	-0.059**
	(0.051)	(0.096)	(0.074)	(0.024)	(0.026)
5-9 years post RPS	-0.231***	-0.831***	-0.353***	-0.047	-0.139***
	(0.062)	(0.245)	(0.123)	(0.032)	(0.041)
≥ 10 years post RPS	-0.319***	-1.248***	-0.588***	-0.059	-0.189***
	(0.089)	(0.290)	(0.182)	(0.046)	(0.055)
ATT Differences due to RPS Policy Discrepancy: 0-4 years post RPS					
(0-4 years) × exclude	0.119	0.107	0.164	0.009	0.060*
	(0.082)	(0.124)	(0.105)	(0.028)	(0.034)
(0-4 years) × voluntary	-0.208	-0.299**	-0.069	-0.013	-0.021
	(0.141)	(0.125)	(0.116)	(0.038)	(0.043)
(0-4 years) × multiplier	-0.022	0.034	-0.061	0.013	-0.029
	(0.048)	(0.184)	(0.115)	(0.032)	(0.037)
(0-4 years) × carbon	0.078	0.087	-0.021	0.035	0.021
	(0.106)	(0.164)	(0.104)	(0.023)	(0.041)
ATT Differences due to RPS Policy Discrepancy: 5-9 years post RPS					
(5-9 years) × exclude	0.176**	0.514	0.387**	0.030	0.170***
	(0.074)	(0.310)	(0.158)	(0.032)	(0.057)
(5-9 years) × voluntary	-0.189*	-0.009	0.018	-0.020	-0.008
	(0.101)	(0.355)	(0.306)	(0.055)	(0.084)
(5-9 years) × multiplier	-0.009	0.307	-0.045	0.024	-0.052
	(0.074)	(0.239)	(0.142)	(0.042)	(0.056)
(5-9 years) × carbon	0.106	0.158	0.185	0.035	0.062
	(0.088)	(0.222)	(0.138)	(0.026)	(0.056)
ATT Differences due to RPS Policy Discrepancy: ≥ 10 years post RPS					
(≥ 10 years) × exclude	0.180**	0.652	0.673***	0.097*	0.218***
	(0.087)	(0.432)	(0.188)	(0.049)	(0.079)
(≥ 10 years) × voluntary	-0.064	-0.798***	-0.482	-0.167	-0.148
	(0.089)	(0.205)	(0.406)	(0.147)	(0.169)
(≥ 10 years) × multiplier	0.027	0.578*	-0.020	0.069	0.028
	(0.088)	(0.316)	(0.175)	(0.054)	(0.055)
(≥ 10 years) × carbon	-0.101	-0.074	0.303*	0.018	0.069*
	(0.089)	(0.278)	(0.160)	(0.027)	(0.039)
Obs.	1334	1334	1334	924	1305
R^2	0.988	0.949	0.949	0.937	0.844

Note: The RPS policy discrepancy effects are examined by four categories of discrepant policy designs: excluded sales (exclude = 1), voluntary sales (voluntary = 1), multiplier credit (multiplier = 1), and carbon emitting (carbon = 1). The four categories are non-exclusive, i.e., a state's RPS legislation can have multiple discrepant features. Columns of the table present results for various dependent variables: natural logarithm of CO2 emission (1), natural logarithm of SO2 emission (2), natural logarithm of NOx emission (3), natural logarithm of PM2.5 annual average (4), and natural logarithm of AQI 90th-percentile (5). All regression models are estimated controlling the covariates in

Table 2 and including state and year fixed effects. Standard errors are clustered at the state level. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table A.3 Break model ATT estimates for the power sector outcomes by four categories of discrepancy

	(1) Coal and Oil	(2) Natural Gas	(3) Wind and Solar	(4) Hydro and Nuclear	(5) Other Renewable	(6) Electricity Price
0-4 years post RPS	-3.256**	1.999	0.564	0.307	-0.165	-0.038
	(1.366)	(2.525)	(0.668)	(1.897)	(0.373)	(0.400)
5-9 years post RPS	-11.527***	7.942*	-1.129	4.755**	-0.418	0.046
	(3.456)	(4.012)	(1.000)	(2.282)	(0.444)	(0.400)
≥ 10 years post RPS	-14.124**	9.898*	-2.337*	6.139*	-0.269	0.493
	(5.395)	(5.461)	(1.258)	(3.058)	(0.456)	(0.552)
ATT Differences due to RPS Policy Discrepancy: 0-4 years post RPS						
(0-4 years) × exclude	2.667	-3.681	2.185**	-1.132	0.252	0.305
	(1.948)	(2.474)	(0.963)	(1.999)	(0.354)	(0.483)
(0-4 years) × voluntary	0.008	-0.811	-1.412	2.582	-0.159	0.211
	(1.926)	(2.387)	(1.322)	(1.772)	(0.315)	(0.410)
(0-4 years) × multiplier	-0.935	1.563	-1.327*	0.218	0.378*	-0.659*
	(1.742)	(2.211)	(0.751)	(1.427)	(0.225)	(0.345)
(0-4 years) × carbon	-3.119	0.736	1.317**	1.770	-0.388	0.100
	(2.793)	(2.878)	(0.644)	(1.341)	(0.326)	(0.566)
ATT Differences due to RPS Policy Discrepancy: 5-9 years post RPS						
(5-9 years) × exclude	7.770*	-8.533**	6.619***	-5.927**	0.155	0.537
	(3.909)	(3.770)	(1.877)	(2.475)	(0.598)	(0.533)
(5-9 years) × voluntary	0.889	-1.765	1.108	0.083	-0.187	0.099
	(4.761)	(5.130)	(3.435)	(2.390)	(0.397)	(0.293)
(5-9 years) × multiplier	2.097	0.631	-4.678**	1.142	0.628	-0.830*
	(3.625)	(3.546)	(1.880)	(2.449)	(0.407)	(0.491)
(5-9 years) × carbon	-2.165	-0.289	3.927**	-1.227	-0.394	0.643
	(3.748)	(3.991)	(1.732)	(1.680)	(0.318)	(0.631)
ATT Differences due to RPS Policy Discrepancy: ≥ 10 years post RPS						
(≥ 10 years) × exclude	9.780*	-9.738*	6.546***	-7.255	0.833	0.736
	(5.725)	(5.168)	(1.890)	(4.354)	(1.419)	(0.658)
(≥ 10 years) × voluntary	-9.040	13.144	-2.217	-1.735	-0.093	0.808
	(7.917)	(11.121)	(4.989)	(2.104)	(0.592)	(0.598)
(≥ 10 years) × multiplier	5.007	-3.618	-0.608	-1.303	0.475	-2.182***
	(5.252)	(3.764)	(2.189)	(3.830)	(0.938)	(0.762)
(≥ 10 years) × carbon	-6.882	4.715	4.586*	-1.445	-1.402**	0.769
	(4.789)	(4.751)	(2.373)	(2.915)	(0.591)	(0.934)
Obs.	1334	1334	1334	1334	1334	1334
R^2	0.949	0.908	0.719	0.950	0.928	0.930

Note: The RPS policy discrepancy effects are examined by four categories of discrepant policy designs: excluded sales (exclude = 1), voluntary sales (voluntary = 1), multiplier credit (multiplier = 1), and carbon emitting (carbon = 1). The four categories are non-exclusive, i.e., a state's RPS legislation can have multiple discrepant features. Columns of the table present results for various dependent variables: the percentage of power generation by coal and oil (1), natural gas (2), wind and solar (3), hydro and nuclear (4), other renewable (5), and the electricity price (6). All regression models are estimated controlling the covariates in

	(1) Coal and Oil	(2) Natural Gas	(3) Wind and Solar	(4) Hydro and Nuclear	(5) Other Renewable	(6) Electricity Price

Table 2 and including state and year fixed effects. Standard errors are clustered at the state level. ***, **, * indicate statistical significance at 1%, 5% and 10%, respectively.

Table A.4 Monetized Foregone Benefits for Discrepant RPS Policies Active in 2010, Itemized by Health Endpoint

Health Endpoint	Pollutant	Change in Incidence (cases, annual)		Monetary Value (in 2016 dollar) (dollars, annual)	
		Low	High	Low	High
Mortality	PM2.5 + O3	1,100	1,900	$11,000,000,000	$21,000,000,000
Mortality, All Cause	PM2.5	650	1,500	$7,200,000,000	$16,000,000,000
Mortality, O3 Short-term Exposure	O3	17	17	$190,000,000	$190,000,000
Mortality, O3 Long-term Exposure	O3	380	380	$4,300,000,000	$4,300,000,000
Nonfatal Heart Attacks	PM2.5	410	410	$26,000,000	$26,000,000
Infant Mortality	PM2.5	6.5	6.5	$77,000,000	$77,000,000
Hospital Admits, All Respiratory	PM2.5 + O3	110	110	$1,900,000	$1,900,000
ER Visits, Respiratory	PM2.5 + O3	1,600	1,600	$1,900,000	$1,900,000
Asthma Onset	PM2.5 + O3	4,300	4,300	$250,000,000	$250,000,000
Asthma Symptoms	PM2.5 + O3	740,000	740,000	$120,000,000	$120,000,000
ER Vists, Asthma	O3	6.2	6.2	$3,900	$3,900
Lung Cancer Incidence	PM2.5	45	45	$1,500,000	$1,500,000
Hospital Admits, Cardiovascular	PM2.5	82	82	$1,800,000	$1,800,000
Hospital Admits, Alzheimers	PM2.5	300	300	$5,000,000	$5,000,000
Hospital Admits, Parkinsons	PM2.5	40	40	$710,000	$710,000
Stroke Incidence	PM2.5	35	35	$1,600,000	$1,600,000
Hay Fever/Rhinitis Incidence	PM2.5 + O3	29,000	29,000	$24,000,000	$24,000,000
Cardiac Arrest, Out of Hospital	PM2.5	9.2	9.2	$430,000	$430,000
ER Visits, All Cardiac	PM2.5	200	200	$320,000	$320,000
Minor Restricted Activity Days	PM2.5	530,000	530,000	$50,000,000	$50,000,000
School Loss Days	O3	260,000	260,000	$340,000,000	$340,000,000
Work Loss Days	PM2.5	89,000	89,000	$21,000,000	$21,000,000
Total PM Health Effects				$7,500,000,000	$17,000,000,000
Total O3 Health Effects				$5,100,000,000	$5,100,000,000
Total Health Effects				$12,000,000,000	$22,000,000,000

Note: Scenario calculated using COBRA version 5.0. Foregone benefits for states with discrepant RPSs in 2010, including AZ, CA, CO, CT, HI, ME, MI, MN, MO, MT, NC, ND, NM, NV, OK, OR, PA, SD, UT, VA, VT, WA. Generation, emission, and population scenarios are based in 2016. Foregone emission gaps are 43.4% for SO2 and 33.7% for NOx. Discount rate is set at 2%.

Table A.5 Monetized Actual Benefits (Avoided Damages) for RPS Policies Active in 2010, Itemized by Health Endpoint

Health Endpoint	Pollutant	Change in Incidence (cases, annual)		Monetary Value (in 2016 dollar) (dollars, annual)	
		Low	High	Low	High
Mortality	PM2.5 + O3	-1,530	-3,000	-$17,000,000,000	-$34,000,000,000
Mortality, All Cause	PM2.5	-1,150	-2,600	-$13,000,000,000	-$29,000,000,000
Mortality, O3 Short-term Exposure	O3	-16	-16	-$170,000,000	-$170,000,000
Mortality, O3 Long-term Exposure	O3	-360	-360	-$4,000,000,000	-$4,000,000,000
Nonfatal Heart Attacks	PM2.5	-740	-740	-$47,000,000	-$47,000,000
Infant Mortality	PM2.5	-11.5	-11.5	-$140,000,000	-$140,000,000
Hospital Admits, All Respiratory	PM2.5 + O3	-159	-159	-$3,000,000	-$3,000,000
ER Visits, Respiratory	PM2.5 + O3	-1,820	-1,820	-$2,300,000	-$2,300,000
Asthma Onset	PM2.5 + O3	-5,300	-5,300	-$300,000,000	-$300,000,000
Asthma Symptoms	PM2.5 + O3	-940,000	-940,000	-$110,000,000	-$110,000,000
ER Vists, Asthma	O3	-5.8	-5.8	-$3,600	-$3,600
Lung Cancer Incidence	PM2.5	-80	-80	-$2,700,000	-$2,700,000
Hospital Admits, Cardiovascular	PM2.5	-146	-146	-$3,100,000	-$3,100,000
Hospital Admits, Alzheimers	PM2.5	-520	-520	-$8,900,000	-$8,900,000
Hospital Admits, Parkinsons	PM2.5	-71	-71	-$1,300,000	-$1,300,000
Stroke Incidence	PM2.5	-62	-62	-$2,900,000	-$2,900,000
Hay Fever/Rhinitis Incidence	PM2.5 + O3	-35,000	-35,000	-$29,000,000	-$29,000,000
Cardiac Arrest, Out of Hospital	PM2.5	-16.3	-16.3	-$750,000	-$750,000
ER Visits, All Cardiac	PM2.5	-350	-350	-$570,000	-$570,000
Minor Restricted Activity Days	PM2.5	-930,000	-930,000	-$89,000,000	-$89,000,000
School Loss Days	O3	-240,000	-240,000	-$290,000,000	-$290,000,000
Work Loss Days	PM2.5	-157,000	-157,000	-$37,000,000	-$37,000,000
Total PM Health Effects				-$13,000,000,000	-$30,000,000,000
Total O3 Health Effects				-$4,700,000,000	-$4,700,000,000
Total Health Effects				-$18,000,000,000	-$35,000,000,000

Note: Scenario calculated using COBRA version 5.0. Actual benefits (avoided damages) for states with active RPS policies in 2010. Effects of clean and discrepant RPS are separately calculated. States with discrepant RPSs include AZ, CA, CO, CT, HI, ME, MI, MN, MO, MT, NC, ND, NM, NV, OK, OR, PA, SD, UT, VA, VT, and WA. States with clean RPS include DE, IL, MA, MD, NH, NJ, NY, OH, RI, and WI. Generation, emission, and population scenarios are based in 2016. Counterfactual emission increases are 57.6% for SO_2 and 33.7% for NO_x for states with a clean RPS. Counterfactual emission increases are 25.1% for SO_2 and 0% for NO_x for states with a discrepant RPS. Discount rate is set at 2%.