# How Does Renewable Portfolio Standards Impact Carbon Emissions at the State Level, by Sector?

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# Abstract

In this paper, we examine the efficacy of Renewable Portfolio Standards in reducing carbon emissions. In addition, we set out to answer whether the composition of carbon emissions by sector changes in response to a higher RPS. Using a panel of state-level data from 1990 to 2017, we use regression analysis to estimate the effect of an higher RPS on carbon emissions by sector. The analysis uses fixed effects to control for time and state specific effects while including explanatory variables that control for meteorological, demographic, and economic factors. Results offer evidence that renewable portfolio standards significantly reduce state-level carbon emissions as well as sector-specific carbon emissions, albeit, with the exception of industrial carbon emissions.

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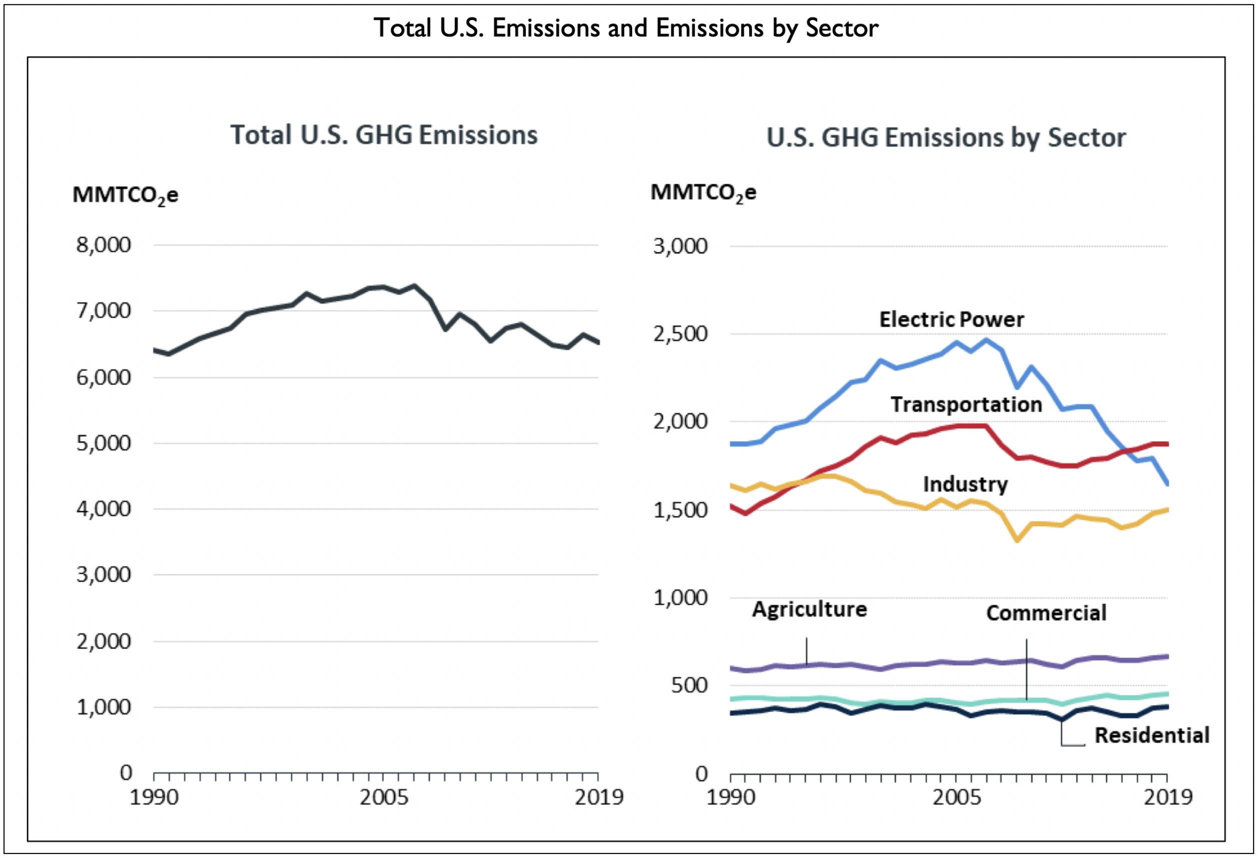
# 1. Introduction

Today we are facing a rapidly warming climate. Global temperatures are increasing, as evidence by the last seven years being the warmest in recent history, and 2016 and 2020 being tied for the warmest years on record (NASA, 2009). The planet has been warming at an unprecedented rate since the early 1900s, mainly due to increased human population and productivity (NASA, 2009). Extreme weather events, decreased snow cover, and rising oceans are just a few ways climate change is impacting our planet (NASA, 2009). Such changes have severe adverse ripple effects through environmental, social and economic systems. Nations and organizations worldwide are working to address climate change; however, methods for managing climate change vary widely in technological and political approaches. What can be certain is that countries need to increase the speed of the ongoing energy transition to cleaner energy sources. For the world to avoid the most severe consequences of climate change, effective policies that transition energy production away from fossil fuels is paramount. One type of policy instrument adopted by many U.S. states is a renewable portfolio standard (RPS), which is a state-level regulation that mandates a minimum amount for which energy on the grid has to come from renewable energy sources (NCSL, 2021). Managing how the U.S. produces power is a natural focus for climate change mitigation efforts, given that electric power production is the largest producer of greenhouse gas (GHG) emissions in the U.S. (CRS, 2021); see figure 1 on on page 5. My research aims to answer the following basic efficacy question: Are RPSs effective in reducing carbon emissions? To provide evidence for this question, we collect data for U.S. states for years 1990 to 2017 to estimate the impact that an RPS has on emissions. The analysis considers aggregate emissions as well as sector-specific emissions, while also taking advantage of the panel nature of the data to control for unobserved heterogeneity. Results indicate that an RPS is effective in reducing state-level emissions, specifically an one percentage point increase in RPS is associated with a 47 ton reduction in total US carbon emissions.

Figure 1: GHG Emissions




# 2. Background

## 2.1. Background: RPS Overview & History

In the United States, power plants are responsible for approximately 40 percent of the nation's carbon dioxide emissions (Yin and Powers, 2010). Coal has provided reliable base load power for more than a century, yet U.S. policy is making it clear that a transition away from coal-fired power plants is a top priority. Thus, renewables are beginning to take the place of traditional power sources and subsequently comprising a larger share of the national grid portfolio. Renewable energy development, however, is not without its complications, and policy solutions such as RPS can both help and hinder the energy transition.

As a policy, an RPS can take different forms at the state level, which causes a high degree of variation in the details such as deadlines, targets, trading mechanisms and compliance measures. Yet, this flexibility has in part been responsible for the policies popularity (Eastin, 2014). A RPS requirement is typically defined in terms of what proportion of the state's utilities power delivered must come from renewable sources (i.e. 100% of the energy sold in from Colorado utilities companies must be from renewable energy by 2050). There are a few states, such as Texas and Iowa, who have defined the RPS minimum to be the proportion of power generating capacity (NCSL, 2021). As per the EIA's definition of generation capacity, "electricity generation capacity is the maximum electric output an electricity generator can produce under specific conditions" (EIA, 2020). While "Electricity generation is the amount of electricity a generator produces during a specific period of time" (EIA, 2020). Moreover, generation is just one measure of production, hence, production and generation will be used interchangeably throughout this paper. Additional ways in which state RPS policies vary include: whether or not the RPS is mandatory, the date at which the RPS goal is to be achieved, whether the RPS focuses on power generation versus power generation capacity, and whether or not targets can be met by trading RPS credits (known as Renewable Energy Credits (RECs) across state lines).

In addition to policy variability, there is a high degree of variability in the implementation of RPS. Some states impose additional restrictions, such as requirement of certain types of renewables (such as solar), permitting the allowance of additional credits for select industries, and separate standards for select utility companies (such as Xcel Energy in Minnesota) (NCSL, 2021).Though, without a doubt the most controversial of RPS restrictions are in-state generation requirements, which occur in 5 different forms, including Distributed Generation Carve-Outs, Solar Carve-Outs, In-state minimums, In-state credit multipliers and In-state by defaults; see table 1 below.

Table 1: In State Generation requirements (NCCETC, 2014)

| Requirement | States |
|---|---|
| Distributed Generation Carve-Outs (5) | AZ, CO, IL, MN, NM |
| Solar Carve-Outs (6) | DE, MA, MD, NH, NJ, NY |
| In-State Minimums (4) | IA, NY, MI, TX |
| In-State Credit Multipliers (7) | AZ, CO, DE, KS, MI, MO, NV |
| By Default (1) | HI |
| In-State-Generation Restrictions of any kind (17) | AZ, CO, DE, IA, IL, KS, MA, MD, MI, MN, MO, NH, NJ, NM, NV, NY, TX |

All state-level RPS policies can be classified by the timeframe for which the RPS is to be achieved. State RPSs are classified as "Active", "Expired", "Goal" oriented, or none of the above. An Active RPS means that the state in question is still in pursuit of reaching a standard by a set date, in addition to being subject to fines if the standard is not met by the set target date. Alternatively, an Expired RPS means a state has satisfied a previous standard and has not set a new standard. While a Renewable Portfolio Goal (RPG) means that the state has laid out voluntary targets that are not enforced by fines in the event of non-completion. There are 12 states that have never had a renewable portfolio policy of any kind, see the table below. Throughout this paper, we will exclude Hawaii, D.C. and Alaska and use the period from 1990 to 2017. Virginia will also be classified as "no RPS" as its policy only passed in 2020 and is outside of the range of our data. Thus, as of 2017, there are 25 states that have active RPS's, 7 states with expired RPS, three states that have RPG's and 12 states that have never had any RPS or RPG; see table 2 below.

Table 2: RPS Classification *(excluding AK and HI)* (NCSL, 2021)

| Policy Condition | States |
|---|---|
| Active RPS (25) | AZ, CA, CO, CT, DE, IL, MA, MD, ME, MI, MN, MO, NH, NJ, NM, NV, NY, OH, OR, PA, RI, TX, VA, VT, WA |
| Expired RPS (7) | IA, KS, MT, ND, OK, SD, WI |
| RPG (3) | IN, SC, UT |
| Never Had a Renewable Portfolio Policy of Any Kind (12) | AL, AR, FL, GA, ID, KY, LA, MS, NE, TN, WV, WY |

## 2.2. Background: REC Overview

A Renewable Energy Certificate (REC) is equal to one MWh of electricity regardless of where it was produced (with the only exceptions in Arizona and Nevada where it is defined in kWh) (Hamrin, 2014). RECs serve a function of accounting and tracking renewable energy as it flows onto the grid, additionally RECs serve to distinguish renewable from nonrenewable energy (Hamrin, 2014).The definition of RECs (like RPS) differs slightly by state, though Hamrin (2014) reports that these differences are "small enough that interstate renewable energy markets have been able to operate smoothly and seamlessly" (Hamrin, 2014).The certification of RECs to generators of renewable power is a necessary part of the RPS implementation process (NCSL, 2021). As mentioned, RPS is a quantity-based policy instrument, using RECs as a "currency" for trade. More specifically, RECs are a market-based instrument of currency that certifies the bearer is in ownership of one MWh of certified renewable energy (Hamrin, 2014).The RECs are created when the power provider feeds some amount of certified renewable energy onto the grid, whereby the associated REC can then be sold. For example, the REC can be sold to a polluting firm as a carbon tax credit to offset their emissions or can be used as a credit against a utility's own power usage (Hamrin, 2014). Additionally, while REC can be traded in most states, some states have rules against REC trade. These states view in-state renewable energy generation differently than owning an REC that represents generation in other state, and these states often require in-state renewable energy generation to meet RPS requirements. The rational for restricting REC trade and creating in-state renewable generation requirements, is that nascent renewable industries may be better supported. On the other hand, restricting REC trade (i.e., requiring in-state generation) spurs greater in-state development in green energy sector. However, for renewable energy generation, the in-state requirement may increase the marginal cost of renewable energy generation for states not well positioned to generate renewable energy. Therefore, the potential co-benefits of economic development likely come at a cost. We explore this issue more in the literature review.

An additional RPS design issue is that some RECs are bundled while others are unbundled. For a REC to be unbundled means that the physical electricity can be sold separately from it. REC like a currency, serves the purpose of being fungible, bundling an REC, however, restricts that same function of fungibility. Hence, for the REC to be unbundled it does not impose the additional hurdle of delivering the same physical unit of energy purchased to the energy system of the utility company from which it was bought (Smith, 2010).

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Table 3: Bundled and Unbundled RPS States (Smith, 2010)

| Unbundled | States |
|---|---|
| Unbundled RECs Not Permitted | AZ, CA, NV, WI |
| Unbundled RECs Permitted | CO, CT, DC, DE, HI, MA, MD, ME, MI, MN, MO, MT, NH, NJ, NM, OH, PA, RI, TX, VT, WA, IA, IL, NY |

There are only 4 states with an RPS where RECs are strictly bundled, while 24 states allow RECs to be unbundled; see table 3 above (Smith, 2010). Unbundled RECs significantly ease the transmission of electricity being bought and sold across grids as well as minimizing further complexity in the electricity market (Smith, 2010) Unbundled RECs also reduce wastage by reducing the transmission distance. Thus, RECs reduce the line losses over very long distances as we can't yet efficiently store large amounts of energy (Smith, 2010).The EIA (Energy Information Administration) estimates that between 2016 and 2020, 5% of total electricity transmitted and distributed was lost throughout the United States (EIA, 2017). To summarize, unbundled RECs give the renewable energy generators the flexibility of being able to feed power into the local grid and sell the REC independently to utilities.

# 3. Lit Review

## 3.1. Lit Review: Criticisms Of RPS

Since the 1990's Renewable Portfolio Standards (RPSs) have become a mainstay policy for US states to bolster their production of renewable electricity. Despite this, economic theory shows that RPS are mired by inefficiencies and may not be the best suited policy for mitigating carbon emission (Lyo, 2016). In this vein, scholars have questioned the efficiency and effectiveness of RPS's in reducing carbon emissions at the state level (Yin and Powers, 2010). In one previous attempt to measure the effectiveness of RPS in reducing emissions, Bushnell et al. remarked “RPS may be one of the least efficient means of achieving greenhouse gas emission reductions, it does not reward generation from non-renewable sources of low carbon power, and rewards energy conservation only weakly” (Bushnell et al., 2007, p. 3). While numerous simulated models suggest that RPS’s underperform in reducing carbon emissions relative to other available polices, RPS have only expanded in reach (Fischer and Newell, 2008; Palmer and Burtraw, 2005). And while RPS is prevalent throughout the US, with many state RPSs sharing core regulations, they vary dramatically in design across states.

Design differences have been carefully detailed by Berry and Jaccard (2001); Wiser et al., (2005); Wiser et al., (2007); and Wiser and Barbose (2008) (Berry and Jaccard, 2001; Wiser et al., 2005, 2007; Wiser and Barbose, 2008). One critical difference between states is the degree of state-specific regulations regarding inter-state REC trading. The consequences include small state specific markets which can make large scale renewable energy development difficult with regional markets tending to lack price transparency. The results are inefficient energy markets at the cost of the utility ratepayers (Mack et al., 2011).

Not only do in-state generation requirements hinder the efficiency of regional or national REC markets, but in-state requirements hinder US renewable energy developers as well (Mack et al., 2011). Still, RECs can provide energy developers with a supplemental revenue stream and increase the ability to finance renewable energy projects (Mack et al., 2011). Some states have attempted to limit RECs such that they can only be produced in-state, however, this practice has been challenged legally (Elefant and Holt, 2011). While the lawsuit was settled outside of court, we can expect future resistance from utility companies who undoubtedly will continue with legal challenges.

## 3.2. Lit Review: Renewable Energy Capacity, Generation  Electricity Prices

Shrimali and Kniefel (2012) find that total retail electricity sales was negatively correlated with renewable energy capacity at the state level (Shrimali and Kniefel, 2011). Their finding supports RPS research concerning electricity prices, given that a greater amount of renewable as a percent of total energy generation has been shown to increase average electricity prices and subsequently drop demand for electricity. While some outliers (under very specific conditions) support the opposite conclusion (Fischer and Newell, 2008), most papers find RPS to increase electricity prices and decrease total carbon emissions (Eastin, 2014; Greenstone et al., 2019; Upton Jr and Snyder, 2017).

Yin and Powers (2010) report evidence that a RPS schedule for renewable generation is positively correlated with the percentage renewable energy capacity (Yin and Powers, 2010). In addition, they find that allowing for the free trade of REC's significantly weakened the impact of RPS's on the development of renewable energy capacity (Yin and Powers, 2010). It should be noted, the "capacity" is not synonymous with "generation"; capacity is the amount of generation possible, while generation refers to energy actually produced. In a similar study, Shrimali and Kniefel (2011) conducted an analysis focusing on renewable generation (Shrimali and Kniefel, 2011). They found that a RPS requirement had a significant positive effect on the proportion of renewable energy generation (Shrimali and Kniefel, 2011). Similarly, Carley (2011) finds RPS to have a significant and positive effect on the share of renewable energy-based electricity generated (Carley, 2011). Carley (2011) also reported that states with RPS have a higher renewable energy share compared with non-RPS states (Carley, 2011) - a result further supported by findings of Delmas and Monetes-Sacho (2011) (Carley, 2011; Delmas and Montes-Sancho, 2011). The result of Carley (2011), Delmas and Monetes-Sacho (2011) may seem intuitive, but it is important in forming consensus around the efficacy of RPS.

## 3.3. Lit Review: Reasons For Adopting

Many scholars have examined why states choose to adopt a RPS, with determinants ranging from economic development (Matisoff, 2008), regional diffusion (Berry, 1994; Chandler, 2009) and ideology (Carley and Miller, 2012; Huang et al., 2007; Lyo, 2016). Despite the frequency of which RPS's have been studied, a lack of data and subsequent noisy variables continue to be cited as being problematic in the search for accurate estimates when studying RPS's. Thus, for methodological convenience, many scholars have treated RPS policies as identical or have characterized the differences among them in a more simplistic manner, again subject to constraints of what data is available (Yin and Powers, 2010). Meanwhile, some scholars have focused on policy design features often with the determination that policy specifications are best predictors for RPS's success (Carley, 2011; Yin and Powers, 2010). Seeking to incorporate policy heterogeneity in RPS state policy is a noble goal, yet, implementing the classification of RPS polices into a workable framework presents problems and opportunities for criticism. One such criticism is the potential inability to compare or generalize findings due to the likelihood of classifications differing from scholar to scholar. More accurate measurement of RPS effectiveness may not come until more data is available (Eastin, 2014).

## 3.4. Lit Review: Politics & RPS Efficacy

From a political standpoint, the RPS has been particularly popular with Iowa implementing the first RPS in 1982. Following Iowa, there was a growing surge in RPS policies across several states from the 1990s into the 2000's (NCSL, 2021). During this time, the carbon intensity of US has decreased by about 50 percent according to the US Energy Information Administration (EIA, 2016). A question therefore looms on a connection between decreasing carbon emissions and the introduction of RPS. Theory suggests that an increase in renewable energy requirements (i.e., RPS schedules) will decrease the level of carbon emission and increase the share of energy generated from renewable energy resources (Sekar and Sohngen, 2014). Due to the higher cost of clean energy alternates, more stringent RPS schedules should increase retail prices and subsequently reduce the demand for energy consumption and corresponding carbon emissions. In accordance with the theory, Greenstone et al.(2019) find that the introduction of a RPS leads to a moderate reduction of carbon emissions, in addition to significantly increasing average energy prices (Greenstone et al., 2019). This finding is corroborated by Upton and Snyder (2017) via their use a synthetic difference-in-difference model with synthetic controls whereby they find that a RPS substantially increases electricity prices and modestly reduces emissions (Upton Jr and Snyder, 2017).

### 3.5. Lit Review: RPS Efficacy For Reducing Carbon Emissions

Two similar studies have focused on the efficacy of of RPSs to reduce carbon emissions. Sekar and Sohngen (2014) employ a panel model to examine how the composition of a state's economy explains carbon emissions (Sekar and Sohngen, 2014). They find that states that are more economically dependent of mining and healthcare are more carbon intensive than states with a large information sector (Sekar and Sohngen, 2014). Additionally their models seem to suggest somewhat paradoxically, that more population dense areas are less carbon intensive than states with low population densities, which may primarily be a result of an areas GDP, not their carbon pollution activity (Sekar and Sohngen, 2014). In agreement with the literature, they also find that carbon intensity rises with summer temperatures and falls with winter temperatures, though the generic measure for carbon emissions, not divided by GDP, may look differently (Sekar and Sohngen, 2014). A further distinction from Sekar Sohngen (2014) relative to our paper, is the measure for temperature related energy demand (Sekar and Sohngen, 2014). From Sekar Sohngen's 2014 paper, they measure temperature with a non-time-varying monthly 29-year average unsuitable for modeling with panel data (Sekar and Sohngen, 2014). A better measure for this purpose would be Heating Degree Days (HDD) and Cooling Degree Days (CDD). Still, with the inclusion of regional, demographic and economic controls, Sekar and Sohngen (2014) find the states that have implemented a RPS consistently have lower carbon intensity (Sekar and Sohngen, 2014). In a related paper, Greenstone et al. (2019) estimate a difference-in-difference model and find CO2 emissions decline by 71-250 million metric tons across 29 states over the 7 years after passing an RPS (Greenstone et al., 2019). Other studies however illustrate the complication of RPS's varying effectiveness at the state level (Wiser et al., 2022).

Sekar and Sohngen (2014) examines the compositions of GDP by sector and estimate the impact of these subsections of carbon intensity with the inclusion of RPS yearly percentage values (Sekar and Sohngen, 2014). In addition to the intuitive result that RPS reducing carbon intensity, they found that the introduction of RPS could increase emissions for certain states by changing the relative industry composition of GDP, say from being heavily service sector dominant to commercial (Sekar and Sohngen, 2014).While Sekar and Sohngen (2014) consider GDP by sector in explaining total carbon emissions in combination with RPS, there has been no study to my knowledge that has looked at sector-specific carbon emissions as the dependent variables in a series of models. Hence, for our contribution to the literature we use total carbon emissions as our dependent variable with separate models for each subset of carbon emissions by sector, inducing commercial, residential, transportation, and industrial carbon emissions. Also, as opposed to Sekar and Sohngen (2014), we are using total carbon emissions not carbon intensity. This choice of variable offers new insights to the existing literature as total carbon emissions is an appropriate metric for RPS efficacy. Moreover, as the World Resources Institute reports, carbon intensity is a less transparent measure and can lead to increased environmental uncertainty when used in the place of total carbon emission for regulatory targets (Baumert et al., 2005). Thus, for this paper, we are most interested in a RPS's impact on total carbon emissions, as well as sector-specific carbon emissions.

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# 4. Methodology

## 4.1. Methodology: Data

In the absence of comprehensive data on RPS research, we constructed a new data set for this study. The panel of annual data covered 48 U.S. states over 27 years (1970-2017). Hawaii and Alaska are excluded due to data limitations and non-conformity. Much of the data was collected from United States Energy information administration (EIA), but economic data was pulled from the Bureau of Economic Analysis (BEA) and meteorological data was collected from the National Centers for environmental Information (NOAA); see table 4 below for definitions and basic summary statistics.

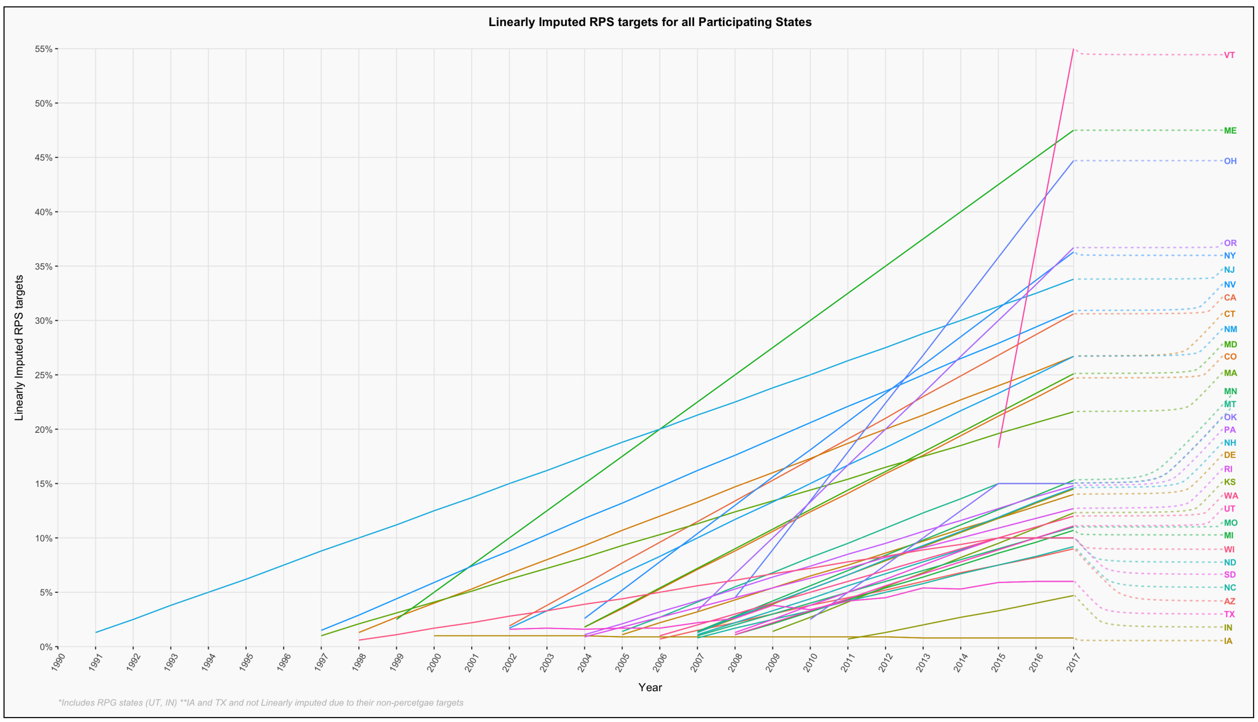
Table 4: Variable Definitions and Summary Statistics

| Variable | Definition | Mean | Min | Max |
|---|---|---|---|---|
| $Total_{CO_2}$ | Total carbon emissions (tons) | 111.12 | 2.60 | 683.24 |
| $RES_{CO_2}$ | Residential carbon emission (tons) | 7.21 | 0.8 | 39.3 |
| $COMM_{CO_2}$ | Commercial carbon emissions (tons) | 4.68 | 0.60 | 34.90 |
| $IND_{CO_2}$ | Industrial Carbon emissions (tons) | 20.55 | 0.40 | 264.60 |
| $TRAN_{CO_2}$ | Transportation Carbon Emissions (tons) | 37.09 | 3.00 | 235.20 |
| $Ret.Fin.Gov$ | Retail, Financial and government added % of total GDP | 0.39 | 0.26 | 0.60 |
| $VMT$ | Vehicle miles traveled (miles × $10^{-3}$) | 5716668.58 | 5716668.58 | 5716668.58 |
| $HDD$ | Heating degree days | 5214.07 | 430.00 | 10810.00 |
| $CDD$ | Cooling degree days | 1081.29 | 42.00 | 4156.00 |
| $Pop$ | Population (per 100,000 people) | 5970713.54 | 453401.00 | 39337785.00 |
| $GDP.per.cap$ | GDP per capita (millions of dollars) | 0.11420 | 0.00036 | 3.34424 |
| $Imputed.RPS$ | Linearly imputed RPS % values | 0.04 | 0.00 | 0.55 |

EIA provided data on energy consumption (EIA, 2022c), energy generation (EIA, 2022b), and state average retail electricity prices (EIA, 2022a). BEA provided annual GDP by state with the BEA code SAGDP. The BEA, however, changed there measure for GDP and subsequently their codes in the year 1997, with the code SAGDPN measuring GDP pre-1997 and SAGDPS measuring GDP post-1997. Thus for the purposes of our paper we combined both SAGDPN and SAGDPS classifications of GDP to get our 1990-2017 range of values (BEA, 2022). NOAA provided data on Heating Degree Days and Cooling Degree

Days (NOAA, 2022). U.S. state population data was retrieved state by state from Federal Reserve Economic Data (FRED) (FRED, 2022), whereby population density was manually calculated using state square mile measures from U.S. Census Bureau (Bureau, 2010). For the RPS schedules I retrieved data from Lawrence Berkeley National Laboratory—i.e., Berkeley Lab (Lab, 2021). For my RPS dummy variables I used the information available at the National Conference of State Legislatures (NCSL, 2021). For vehicle miles driven per capita, I retrieved data from the Eno Canter for Transportation (Eno, 2019a,b).

Figure 2: Imputed RPS percentage targets




For our control variable for service sectors of the economy we have constructed the variable $Ret.Fin.Gov$ which is the percentage aggregate of Retail as proportion of total GDP, Finance as a proportion of total GDP and Government administration as a proportion of total GDP. The underlying data for which we combined to create $Ret.Fin.Gov$ we are sourced from the BEA (BEA, 2022). Finally, for the most central variable in our paper, we linearly imputed RPS targets using the start and end dates provided from NCSL, see figure 2 above for a visual representation (NCSL, 2021).

## 4.2. Methodology: Models

Data collection yielded a balanced panel of annual state-level data over the 27 year period. We take advantage of the panel nature of the data to control for possible unobserved heterogeneity. In the decision to use fixed effects and random effects, we are assuming the presence of unobserved determinants, which can be controlled with fixed or random effects specifications. In deciding whether to use a random effect or fixed effects model, we performed Hausman tests for each model. The null hypothesis for the Hausman test is that the random effects estimator is consistent, while the alternative hypothesis is that Hausman test is not consistent. From the Hausman test, we find that we reject the null hypothesis that the random effects estimator is consistent, which directs us to use the fixed effects estimator (see appendix for more details). In other words, we are concluding that not all unobservable heterogeneity is accounted for, which subsequently results in a significant difference in the fixed effect and random effect estimators. Meanwhile, the fixed effects estimator controls for time-invariant heterogeneity by time-demeaning the variables, yet, in doing so we also lose degrees of freedom. One possible explanation for the presence of unobservable heterogeneity could be that we lack a variable for measuring the renewable energy potential of states which may not be reflected in our current models and would thereby biases our estimates. In the event the estimates from fixed effects and random effects were not significantly different, we have chosen to use random effects for the benefits of producing lower standard errors. We have 5 such fixed effects model each only changing with respect to the dependent variable. From our five models the independent variables are denoted by double dots to signify the time demeaned variables of our fixed effects models; see table 5 on page 19 below.

Table 5: Fixed Effect Models for Subsection Carbon Pollution

| | |
|---|---|
| Model 1 | $Total_{CO_2} = \beta_1 \ddot{Ret.Fin.Gov} + \beta_2 \ddot{VMT} + \beta_3 \ddot{HDD} + \beta_4 \ddot{CDD} + \beta_5 \ddot{POP} + \beta_6 \ddot{GDP.per.cap} + \beta_7 \ddot{Imputed.RPS} + \ddot{\epsilon}.$ |
| Model 2 | $RES_{CO_2} = \beta_1 \ddot{Ret.Fin.Gov} + \beta_2 \ddot{VMT} + \beta_3 \ddot{HDD} + \beta_4 \ddot{CDD} + \beta_5 \ddot{POP} + \beta_6 \ddot{GDP.per.cap} + \beta_7 \ddot{Imputed.RPS} + \ddot{\epsilon}.$ |
| Model 3 | $COMM_{CO_2} = \beta_1 \ddot{Ret.Fin.Gov} + \beta_2 \ddot{VMT} + \beta_3 \ddot{HDD} + \beta_4 \ddot{CDD} + \beta_5 \ddot{POP} + \beta_6 \ddot{GDP.per.cap} + \beta_7 \ddot{Imputed.RPS} + \ddot{\epsilon}.$ |
| Model 4 | $IND_{CO_2} = \beta_1 \ddot{Ret.Fin.Gov} + \beta_2 \ddot{VMT} + \beta_3 \ddot{HDD} + \beta_4 \ddot{CDD} + \beta_5 \ddot{POP} + \beta_6 \ddot{GDP.per.cap} + \beta_7 \ddot{Imputed.RPS} + \ddot{\epsilon}.$ |
| Model 5 | $TRAN_{CO_2} = \beta_1 \ddot{Ret.Fin.Gov} + \beta_2 \ddot{VMT} + \beta_3 \ddot{HDD} + \beta_4 \ddot{CDD} + \beta_5 \ddot{POP} + \beta_6 \ddot{GDP.per.cap} + \beta_7 \ddot{Imputed.RPS} + \ddot{\epsilon}.$ |

We also retrieved the between and within variation to get insights on our panel model. Looking at table 6 on page 19 and table 7 on page 20, we observe how variation differs from between states and variation within state for each of our numerical variables.

Table 6: Between Variation

| Variable | Definition | sd | Min | Max |
|---|---|---|---|---|
| $Total_{CO_2}$ | Total carbon emissions (tons) | 109.84 | 3.71 | 636.82 |
| $RES_{CO_2}$ | Residential carbon emission (tons) | 7.97 | 0.86 | 34.56 |
| $COMM_{CO_2}$ | Commercial carbon emissions (tons) | 4.98 | 0.71 | 27.02 |
| $IND_{CO_2}$ | Industrial Carbon emissions (tons) | 34.64 | 0.52 | 214.60 |
| $TRAN_{CO_2}$ | Transportation Carbon Emissions (tons) | 39.55 | 3.48 | 206.01 |
| $Ret.Fin.Gov$ | Retail, Financial and government added % of total GDP | 0.01 | 0.38 | 0.40 |
| $VMT$ | Vehicle miles traveled (miles $\times 10^{-3}$) | 5,670,235 | 702,205 | 30,732,344 |
| $HDD$ | Heating degree days | 2071.89 | 649.54 | 9140.25 |
| $CDD$ | Cooling degree days | 805.30 | 184.11 | 3,545.39 |
| $Pop$ | Population (per 100,000 people) | 64.42 | 5.19 | 349.28 |
| $GDP.per.cap$ | GDP per capita (millions of dollars) | 0.14 | 0.004 | 0.68 |
| $Imputed.RPS$ | Linearly imputed RPS % values | 0.04 | 0.00 | 0.17 |

Note: This data has 1344 observations from the year 1990 to 2017.

For example, by comparing the standard deviation from $Total_{CO_2}$ with both between variation and within variation, we see that there is significantly more variation between states, than variation within states across our 27 year panel. This pattern of a larger between variation is consistent for most of our variables. It is therefore, not surprising that that we reject our Hausman test and opt to used fixed effect estimation as opposed to random effects. If our within variation had been consistently higher than between variation it would be more likely we would have failed to reject the Hausman test that random effects is consistent and ultimately used random effects estimation resulting in smaller standard errors.

Table 7: Within Variation

| Variable | Definition | sd | Min | Max |
|---|---|---|---|---|
| $Total_{CO_2}$ | Total carbon emissions (tons) | 10.68 | -62.67 | 46.42 |
| $RES_{CO_2}$ | Residential carbon emission (tons) | 1.04 | -5.55 | 5.27 |
| $COMM_{CO_2}$ | Commercial carbon emissions (tons) | 0.76 | -6.02 | 7.88 |
| $IND_{CO_2}$ | Industrial Carbon emissions (tons) | 4.48 | -58.80 | 50.00 |
| $TRAN_{CO_2}$ | Transportation Carbon Emissions (tons) | 4.94 | -33.59 | 31.51 |
| $Ret.Fin.Gov$ | Retail, Financial and government added % of total GDP | 0.05 | -0.13 | 0.21 |
| $VMT$ | Vehicle miles traveled (miles × $10^{-3}$) | 1,018,802 | -6,043,873 | 5,575,968 |
| $HDD$ | Heating degree days | 381.76 | -1,122.25 | 1,669.75 |
| $CDD$ | Cooling degree days | 150.61 | -483.82 | 610.61 |
| $Pop$ | Population (per 100,000 people) | 8.59 | -53.38 | 59.08 |
| $GDP.per.cap$ | GDP per capita (millions of dollars) | 0.21 | -0.63 | 2.66 |
| $Imputed.RPS$ | Linearly imputed RPS % values | 0.06 | -0.17 | 0.51 |

Note: This data has 1344 observations from the year 1990 to 2017.

Given that our Hausman tests for all five of our models were rejected, we used fixed effect estimation for all our models. Additionally, HAC standard errors are used to account for heteroskedasticity and autocorrelation. In the Total model, results indicate that the coefficents for VMT, HDD, Population, VMT and Imputed RPS are highly statically significant. Further, we find that CDD weakly explains the variation in Total carbon emissions. As expected, estimates indicate that higher RPS targets lead to large reductions of total carbon emissions within our sample. Specifically, for every one percentage point increase in RPS, we see a 47-ton reduction in carbon emissions on average. The other three significant variables show only a small impact of total carbon emissions. Overall, our results from Model 1 -

$Total_{CO_2}$ - correspond to findings in the literature with the addition of VMT, which to our knowledge, hasn't been used in panel data models for explaining RPS impact on carbon emissions.

Table 8: Fixed Effect Models, and Robustness Checks

| Variable | Total | RES | TRAN | COMM | IND |
|---|---|---|---|---|---|
| $Ret.Fin.Gov$ | -0.5990622<br>(5.4465229) | -0.1092735<br>(0.5402854) | 1.8924361<br>(2.0345625) | 0.2639376<br>(0.4513703) | 2.2096947<br>(2.4720008) |
| $VMT$ | 0.0000102***<br>(0.0000006) | 0.0000000<br>(0.0000001) | 0.0000042***<br>(0.0000002) | 0.0000001*<br>(0.0000000) | 0.0000014<br>(0.0000003) |
| $HDD$ | 0.0028795***<br>(0.0007148) | 0.0010440***<br>(0.0000709) | -0.0000262<br>(0.0002670) | 0.0003846***<br>(0.0000592) | 0.0001583<br>(0.0003244) |
| $CDD$ | 0.0037845*<br>(0.0018725) | -0.0004415*<br>(0.0001858) | -0.0000653<br>(0.0006995) | -0.0000579<br>(0.0001552) | -0.0007991<br>(0.0008499) |
| $Pop$ | -0.69***<br>(0.07) | -0.01<br>(0.01) | -0.06*<br>(0.03) | -0.01<br>(0.01) | -0.36***<br>(0.x03) |
| $GDP.per.cap$ | -0.8810841<br>(1.2045528) | -0.0708213<br>(0.1194895) | 0.1866535<br>(0.4499638) | 0.0013336<br>(0.0998250) | 0.0124631<br>(0.5467076) |
| $Imputed.RPS$ | -47.4156432***<br>(4.2535987) | -3.9477022***<br>(0.4219494) | -9.0382183***<br>(1.5889426) | -1.5691230***<br>(0.3525090) | 0.1038182<br>(1.9305711) |
| $Adjusted\ R-squared$ | 0.30804 | 0.25324 | 0.52898 | 0.25324 | 0.15568 |
| $Observations$ | 1344 | 1344 | 1344 | 1344 | 1344 |
| $Hausman_{(df=7)}$ | $\chi$: 498.48*** | $\chi$: 1590.30*** | $\chi$: 498.48*** | $\chi$: 387.66*** | $\chi$: 1248*** |
| $Breusch-Godfrey_{(df=1)}$ | $LM$: 1176*** | $LM$: 1125*** | $LM$: 1154.6*** | $LM$: 1133.3*** | $LM$: 1214.7*** |
| $Breusch-Pagan_{(df=7)}$ | $BP$: 394.63*** | $BP$: 442.99*** | $BP$: 522.96*** | $BP$: 527.53*** | $BP$: 447.30*** |

Note: ***p<0.001; **p<0.01; *p<0.05

The overall story as seen from inspecting table 8 on page 21, is that *RPS* has the largest effect size in comparison to our other independent variables followed by *Heating Degree Days*, *Cooling Degree Days*, *Vehicle Miles Travelled*, and then *Population*. The general order of magnitude in our coefficients for our independent variables is unsurprising, except possibly in the instance of *Vehicles Miles Travelled*. When inspecting the coefficients of *Vehicles Miles Travelled*, we find that its units are comparatively small in reference to our other independent variables. All the while, *Transportation* makes up the second largest contributor in greenhouse gases in the Unites States, as seen from figure 1 on page 5.

For RPS – our independent variable of interest – we intuitively see highly statistically significant and negative coefficients for three of our four carbon subsection models ($RES$, $TRAN$, $COMM$), with *Transportation* having the largest effect size, followed by *Residential* and *Commercial*. For example, for every one percentage point increase in RPS, we see a 9 ton reduction in transportation related carbon emissions at the state level. An interesting result from our models is the lack of statistical significance for $RPS$ in our model for Industrial carbon; possibly suggesting that there may something structurally different about industrial carbon emissions. Conversely, it is also possible we are not controlling for all the appropriate effects to be able to a see significant coefficient for Industrial carbon, thus, potentially running into omitted variable bias.

For our next independent variable of interest, we see that our coefficients for *Heating Degree Days* is strongly statistically significant and positive for *Total*, *Residential* and *Commercial*, in that order of magnitude. Yet, there arises the question of why the coefficients for *Transportation* and *Industrial* are not significant. Referring to Map 1 and Map 2 (see appendix), you will see the northeast region featured more prominently in *Residential* and *Commercial* carbon emissions; indicating that higher carbon can be attributed to the underlying characteristic of those states. While we notice statistically significant coefficients for *Heating degree days*, in juxtaposition, *Cooling Degree Days* has only mildly significant coefficients and only for Residential Carbon. The relative findings of both *Heating Degree Days* and *Cooling Degree Days*, appear to suggest that Heating demand is the more dominant contributor of carbon emissions, with both larger coefficients and larger statistical significance.

For completeness, *Population* and *GDP per capita* acted as control variables across our five models. In each of our five model outputs, *Population* and *GDP per capita* effect sizes were either very small or statistically insignificant. We can say *Population* and *GDP per capita* presented as control variables, given that with stripped down models we find *Population* and *GDP per capita* to give larger and highly statistically significant coefficients, which is expected. For more interpretations model by model of our fixed effects estimation, see appendix.

## 5. Conclusion

If more data were available, we might be better able to control for unobserved effects to better test all compositions of carbon emissions. Had we already done so, we might have been able to verify as Sekar  Sohngen (2014) hypothesized, that RPS might decrease total carbon emissions with mixed directional effects across compositions of carbon by sector. Instead, we found that RPS reduced carbon emission in four out of our five models where Imputed RPS was significant. In addition, heating demand appears to the dominant player in carbon emissions as opposed to cooling demand. One possible limitation of our data is the necessary approximation of using linear imputation for the RPS schedules in order to build a parsimonious variable.

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# Appendix

Figure 3: Region 1

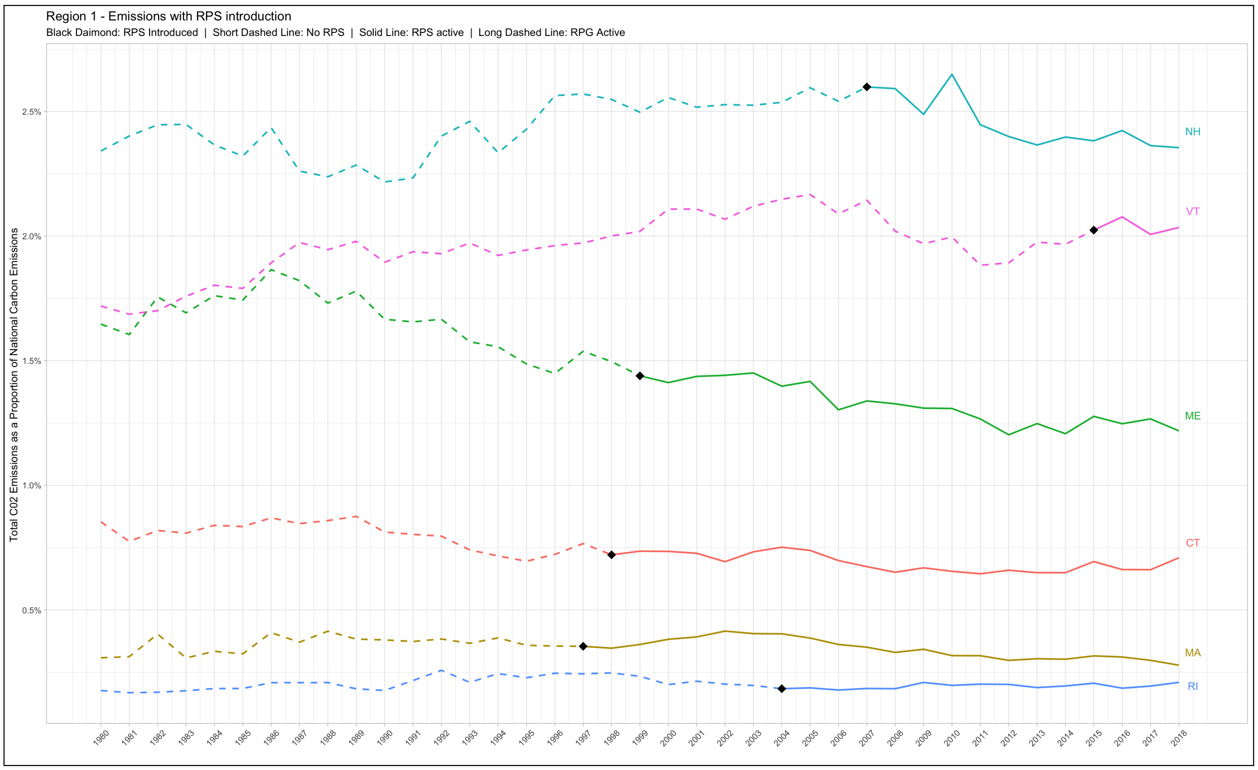

Figure 4: Region 2

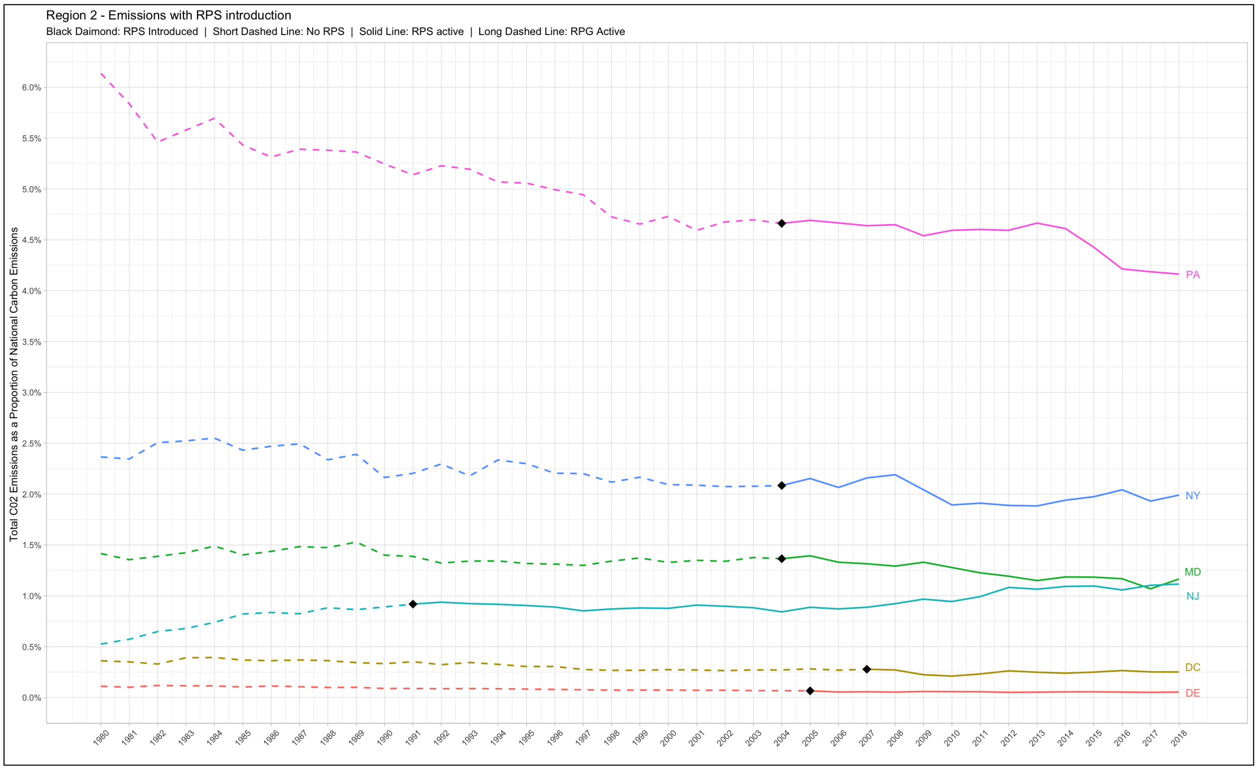


Figure 5: Region 3

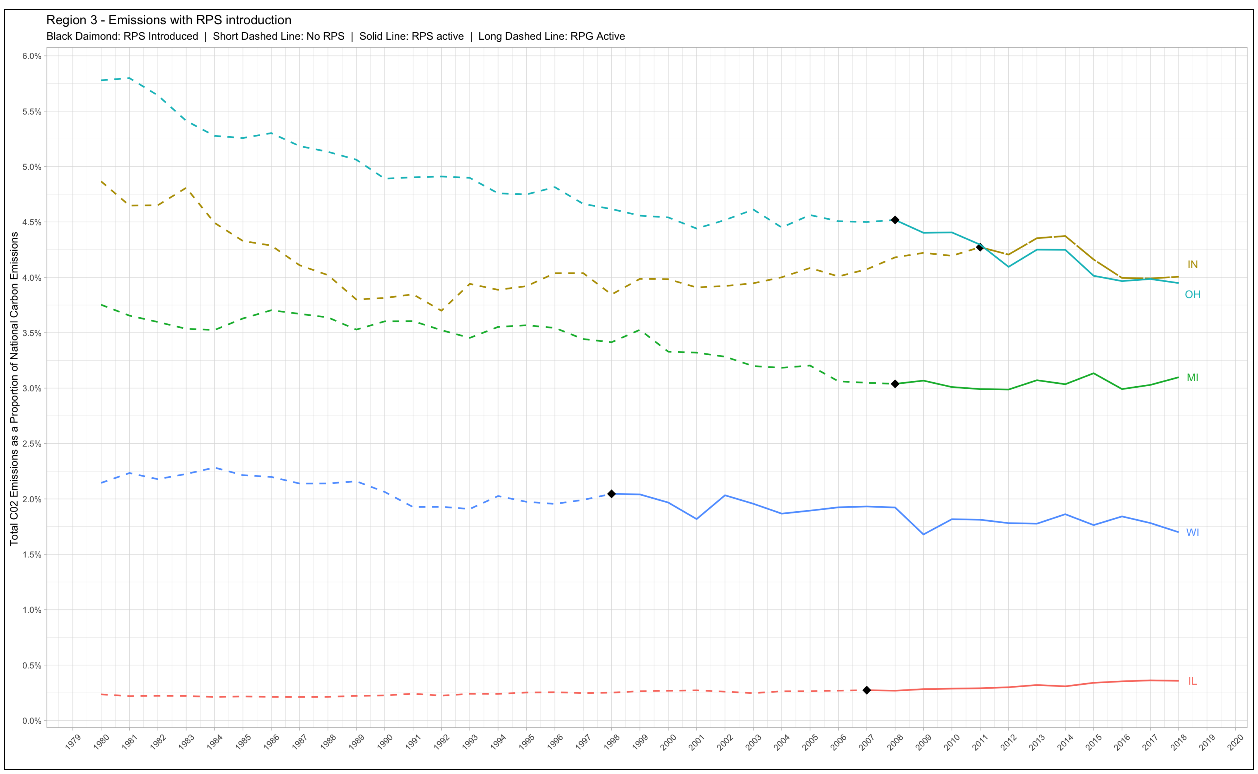

Figure 6: Region 4

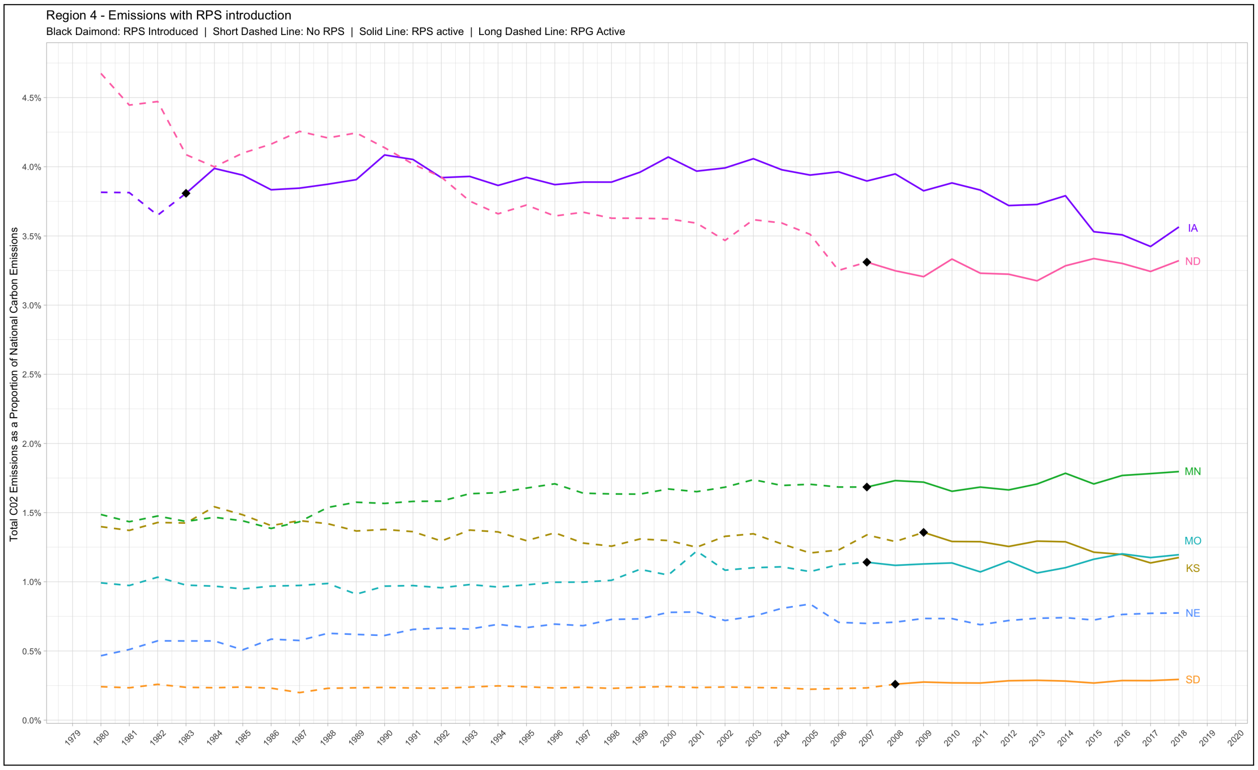


Figure 7: Region 5

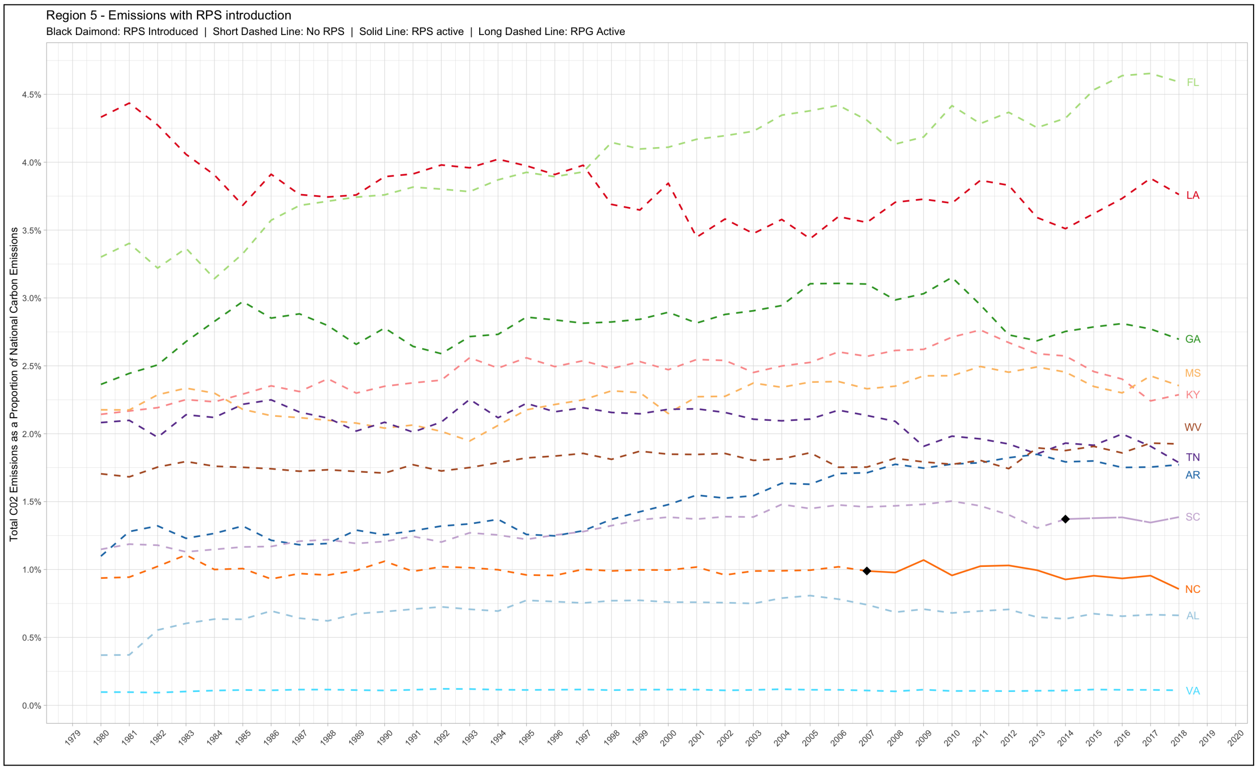

Figure 8: Region 6

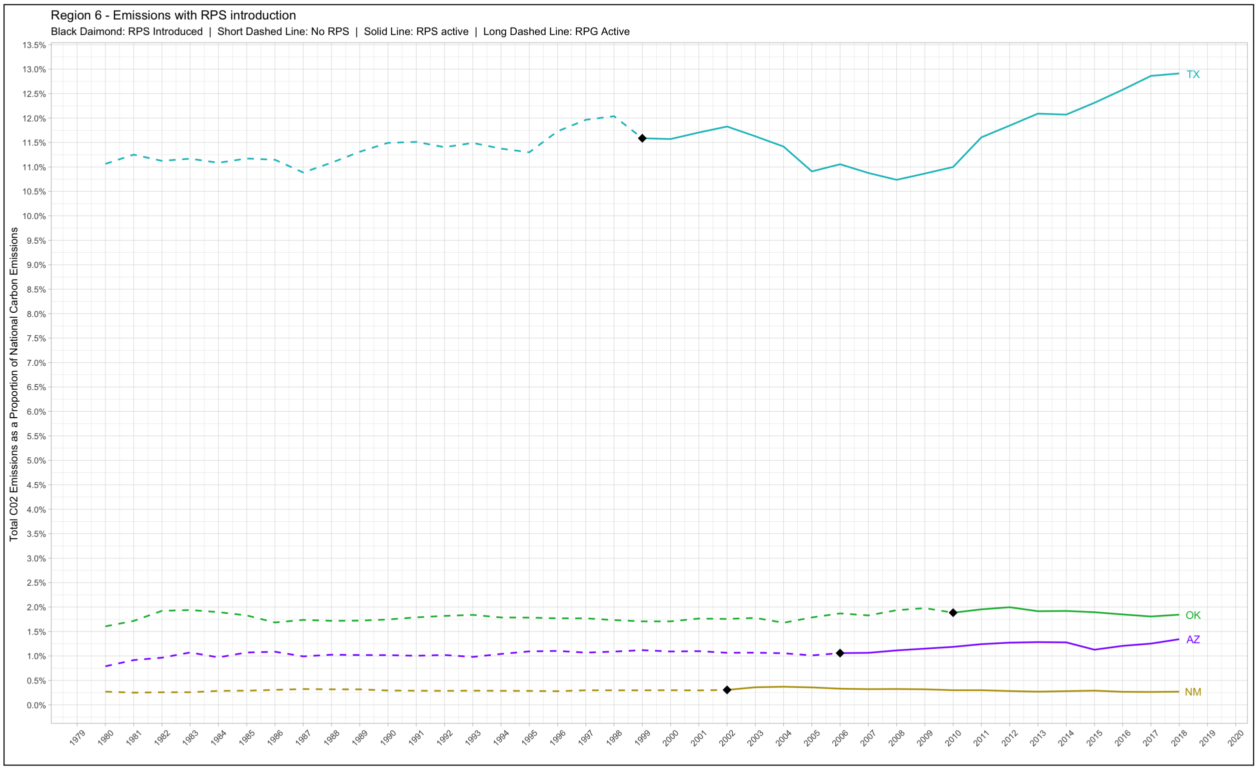


Figure 9: Region 7

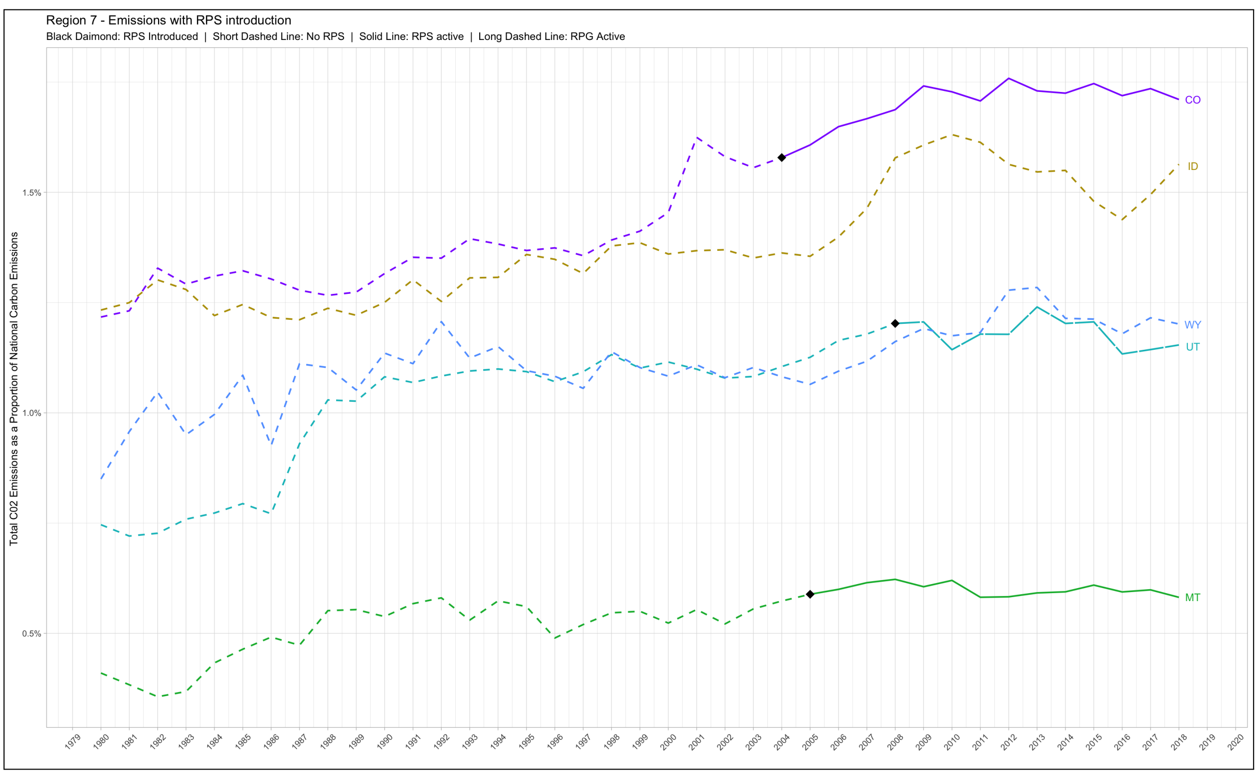

Figure 10: Region 8

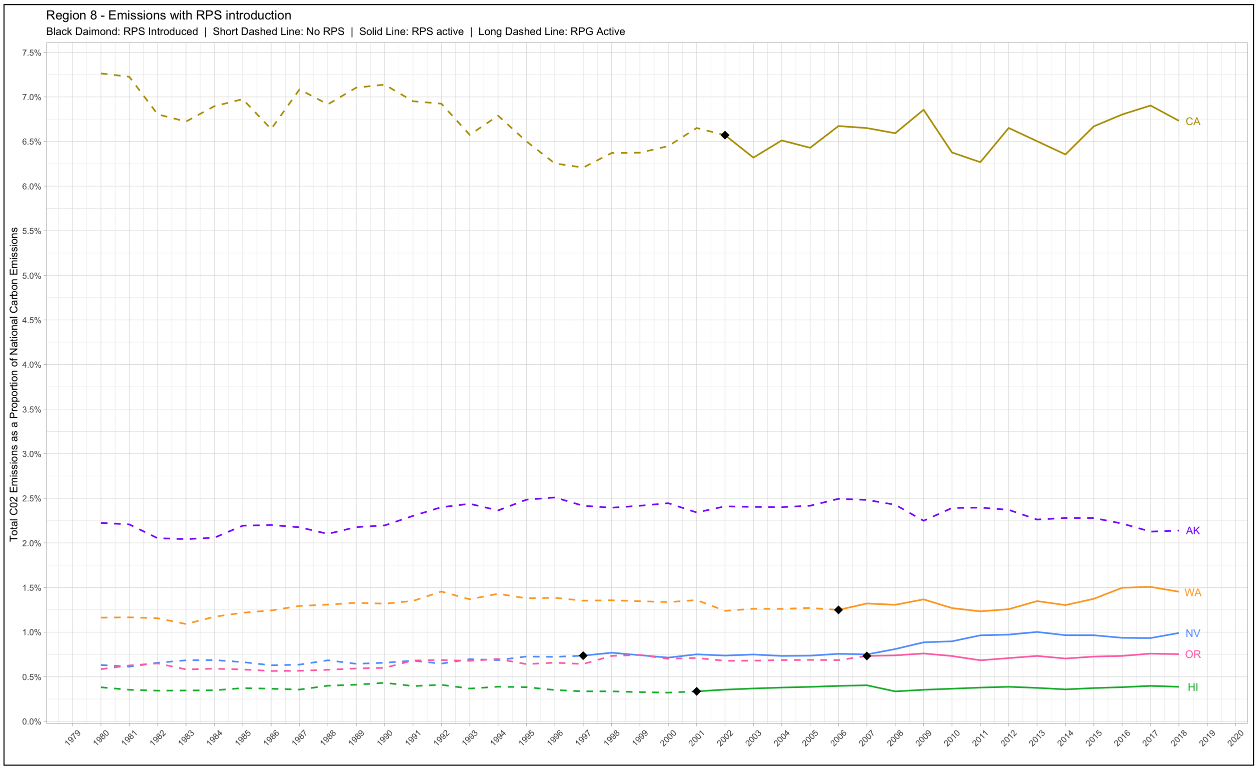

Table 9: correlation matrix

| | Total | COMM | IND | RES | TRAN | Ret.Fin.Gov | VMT | HDD | CDD | POP | GDP.pc | IMP.RPS |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Total | 1.00 | 0.41 | 0.82 | 0.34 | 0.76 | 0.00 | 0.68 | -0.27 | 0.31 | 0.65 | -0.12 | -0.07 |
| Comm | 0.41 | 1.00 | 0.36 | 0.94 | 0.66 | 0.00 | 0.70 | -0.05 | -0.05 | 0.79 | -0.25 | 0.17 |
| IND | 0.82 | 0.36 | 1.00 | 0.30 | 0.71 | -0.02 | 0.58 | -0.36 | 0.38 | 0.55 | -0.15 | -0.08 |
| RES | 0.34 | 0.94 | 0.30 | 1.00 | 0.60 | 0.00 | 0.64 | 0.05 | -0.18 | 0.73 | -0.24 | 0.16 |
| TRAN | 0.76 | 0.66 | 0.71 | 0.60 | 1.00 | 0.00 | 0.97 | -0.46 | 0.35 | 0.96 | -0.26 | 0.06 |
| Fin.Gov | 0.00 | 0.00 | -0.02 | 0.00 | 0.00 | 1.00 | 0.00 | 0.01 | 0.00 | 0.00 | -0.02 | -0.02 |
| VMT | 0.68 | 0.70 | 0.58 | 0.64 | 0.97 | 0.00 | 1.00 | -0.45 | 0.33 | 0.98 | -0.28 | 0.09 |
| CDD | 0.31 | -0.05 | 0.38 | -0.18 | 0.35 | 0.00 | 0.33 | -0.87 | 1.00 | 0.25 | -0.22 | -0.14 |
| Pop | 0.65 | 0.79 | 0.55 | 0.73 | 0.96 | 0.00 | 0.98 | -0.38 | 0.25 | 1.00 | -0.27 | 0.13 |
| GDP.p.c | -0.12 | -0.25 | -0.15 | -0.24 | -0.26 | -0.02 | -0.28 | 0.30 | -0.22 | -0.27 | 1.00 | -0.03 |
| Imp.RPS | -0.07 | 0.17 | -0.08 | 0.16 | 0.06 | -0.02 | 0.09 | 0.08 | -0.14 | 0.13 | -0.03 | 1.00 |

Figure 11: Individual spefic effect maps

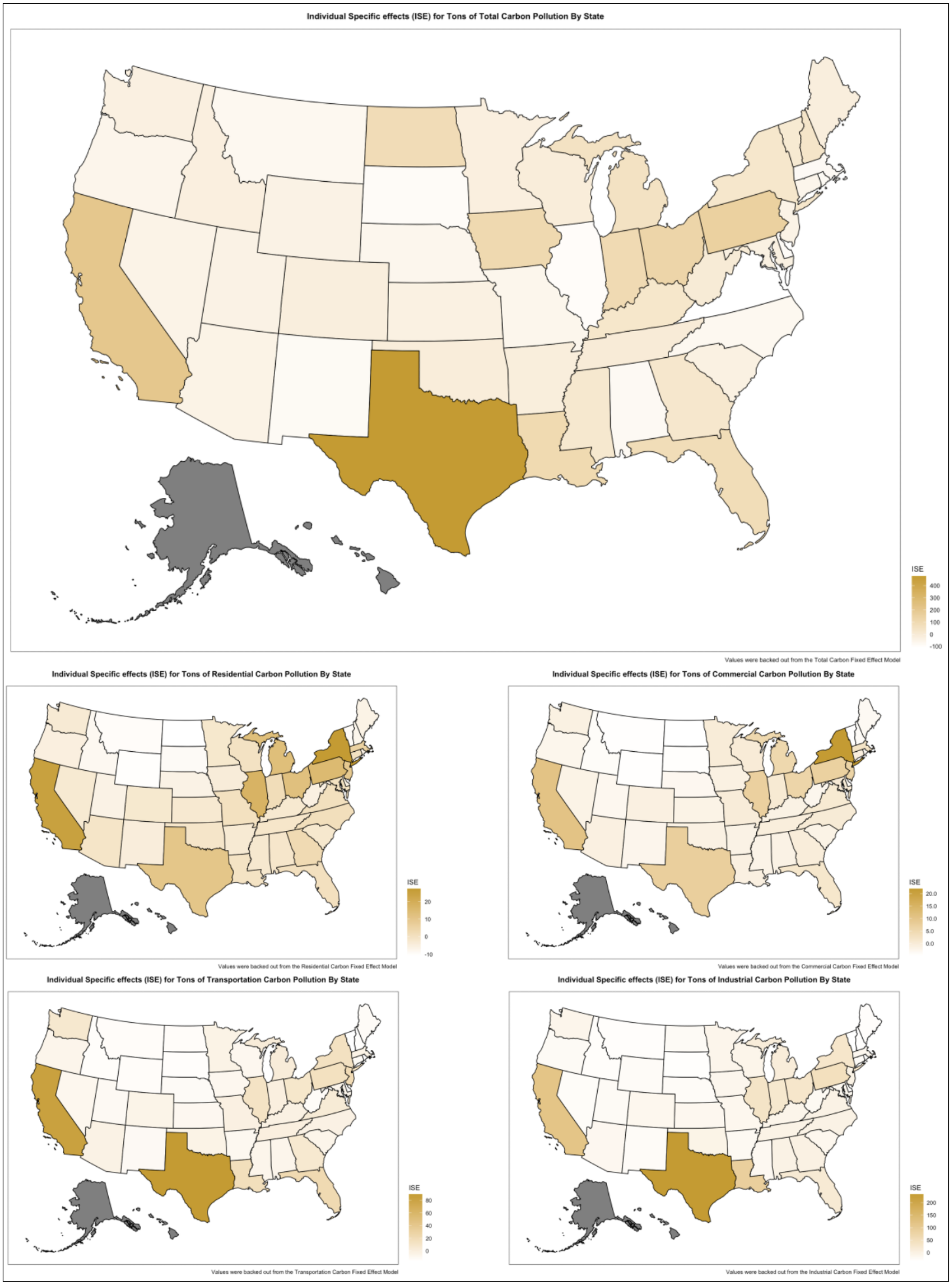

Table 10: Hausman Test for $Total_{CO_2}$ Model

| $\chi^2$ | *df* | *p-value* |
|---|---|---|
| 498.48 | 7 | ¡ 0.01 |

The null hypothesis for the Hausman test is that the random effects estimator is consistent. Meanwhile, the alternative hypothesis is that the fixed effect estimator and the random effects estimator are statistically different. For the $Total_{CO_2}$ model, we reject the hypothesis and subsequently use fixed effect estimation as opposed to random effects. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $Total_{CO_2}$ as our dependent variable.

Table 11: Hausman Test for $RES_{CO_2}$ Model

| $\chi^2$ | *df* | *p-value* |
|---|---|---|
| 1590.3 | 7 | ¡ 0.01 |

For $RES_{CO_2}$ model, we reject the hypothesis and subsequently use fixed effect estimation as opposed to random effects. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $RES_{CO_2}$ as our dependent variable.

Table 12: Hausman Test for $TRAN_{CO_2}$ Model

| $\chi^2$ | *df* | *p-value* |
|---|---|---|
| 387.66 | 7 | ¡ 0.01 |

For $RES_{CO_2}$ model, we reject the hypothesis and subsequently use fixed effect estimation as opposed to random effects. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $TRAN_{CO_2}$ as our dependent variable.

Table 13: Hausman Test for $COMM_{CO_2}$ Model

| $\chi^2$ | *df* | *p-value* |
|---|---|---|
| 303.86 | 7 | ¡ 0.01 |

For $COMM_{CO_2}$ model, we reject the hypothesis and subsequently use fixed effect estimation as opposed to random effects. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $TRAN_{CO_2}$ as our dependent variable.

Table 14: Hausman Test for $IND_{CO_2}$ Model

| $\chi^2$ | *df* | *p-value* |
|---|---|---|
| 1248 | 7 | ¡ 0.01 |

For $IND_{CO_2}$ model, we reject the hypothesis and subsequently use fixed effect estimation as opposed to random effects. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $IND_{CO_2}$ as our dependent variable.

Table 15: Studentized Breusch-Pagan Test for $Total_{CO_2}$ Model

| *BP* | *df* | *p-value* |
|---|---|---|
| 394.63 | 7 | ¡ 0.01 |

The null hypothesis for the Breusch-Pagan test is that there is no heteroskedasticity. Meanwhile, the alternative hypothesis is that there is heteroskedasticity. We reject the null hypothesis, and therefore conclude we have heteroskedasticity. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $Total_{CO_2}$ as our dependent variable.

Table 16: Studentized Breusch-Pagan Test for $RES_{CO_2}$ Model

| *BP* | *df* | *p-value* |
|---|---|---|
| 442.99 | 7 | ¡ 0.01 |

For the $RES_{CO_2}$ model, we reject the hypothesis and subsequently conclude the presence of heteroskedasticity in our model. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $RES_{CO_2}$ as our dependent variable.

Table 17: Studentized Breusch-Pagan Test for $COMM_{CO_2}$ Model

| *BP* | *df* | *p-value* |
|---|---|---|
| 527.53 | 7 | ¡ 0.01 |

For the $COMM_{CO_2}$ model, we reject the hypothesis and subsequently conclude the presence of heteroskedasticity in our model. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $COMM_{CO_2}$ as our dependent variable.

Table 18: Studentized Breusch-Pagan Test for $TRAN_{CO_2}$ Model

| *BP* | *df* | *p-value* |
|---|---|---|
| 522.96 | 7 | ¡ 0.01 |

For the $TRAN_{CO_2}$ model, we reject the hypothesis and subsequently conclude the presence of heteroskedasticity in our model. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $TRAN_{CO_2}$ as our dependent variable.

For the $IND_{CO_2}$ model, we reject the hypothesis and subsequently conclude the presence of heteroskedasticity in our model. The regressors in our test are $VMT$, $HDD$, $CDD$, *Pop*, *GDP.per.cap*, and *ImputedRPS*, with $IND_{CO_2}$ as our dependent variable.

Table 19: Studentized Breusch-Pagan Test for $IND_{CO_2}$ Model

| *BP* | *df* | *p-value* |
|---|---|---|
| 447 | 7 | ¡ 0.01 |

Table 20: Breusch-Godfrey Test for $Total_{CO_2}$ Model

| *LM test* | *df* | *p-value* |
|---|---|---|
| 1176 | 1 | ¡ 0.01 |

The null hypothesis of the Breusch-Godfrey test is that there is no autocorrelation. Meanwhile, the alternative hypothesis is there is autocorrelation. We reject the null hypothesis therefore indicating we have autocorrelation. The regressors in our test are $VMT$, $HDD$, $CDD$, $Pop$, $GDP.per.cap$, and $ImputedRPS$, with $Total_{CO_2}$ as our dependent variable.

Table 21: Breusch-Godfrey Test for $RES_{CO_2}$ Model

| *LM test* | *df* | *p-value* |
|---|---|---|
| 1125 | 1 | ¡ 0.01 |

The null hypothesis of the Breusch-Godfrey test is that there is no autocorrelation. Meanwhile, the alternative hypothesis is there is autocorrelation. We reject the null hypothesis therefore indicating we have autocorrelation. The regressors in our test are $VMT$, $HDD$, $CDD$, $Pop$, $GDP.per.cap$, and $ImputedRPS$, with $RES_{CO_2}$ as our dependent variable.

Table 22: Breusch-Godfrey Test for $TRAN_{CO_2}$ Model

| *LM test* | *df* | *p-value* |
|---|---|---|
| 1133.3 | 1 | ¡ 0.01 |

The null hypothesis of the Breusch-Godfrey test is that there is no autocorrelation. Meanwhile, the alternative hypothesis is there is autocorrelation. We reject the null hypothesis therefore indicating we have autocorrelation. The regressors in our test are $VMT$, $HDD$, $CDD$, $Pop$, $GDP.per.cap$, and $ImputedRPS$, with $TRAN_{CO_2}$ as our dependent variable.

Table 23: Breusch-Godfrey Test for $COMM_{CO_2}$ Model

| *LM test* | *df* | *p-value* |
|---|---|---|
| 1154.6 | 1 | ¡ 0.01 |

The null hypothesis of the Breusch-Godfrey test is that there is no autocorrelation. Meanwhile, the alternative hypothesis is there is autocorrelation. We reject the null hypothesis therefore indicating we have autocorrelation. The regressors in our test are $VMT$, $HDD$, $CDD$, $Pop$, $GDP.per.cap$, and $ImputedRPS$, with $COMM_{CO_2}$ as our dependent variable.

Table 24: Breusch-Godfrey Test for $IND_{CO_2}$ Model

| *LM test* | *df* | *p-value* |
|---|---|---|
| 1214.7 | 1 | ¡ 0.01 |

The null hypothesis of the Breusch-Godfrey test is that there is no autocorrelation. Meanwhile, the alternative hypothesis is there is autocorrelation. We reject the null hypothesis therefore indicating we have autocorrelation. The regressors in our test are $VMT$, $HDD$, $CDD$, $Pop$, $GDP.per.cap$, and $ImputedRPS$, with $IND_{CO_2}$ as our dependent variable.