German Economic Review 20(4): e545–e570 doi: 10.1111/geer.12176

A Synthetic Control Assessment of the Green Paradox: The Role of Climate Action Plans

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Abstract. *This paper extends the green paradox literature by providing empirical insights into its existence. To check the green paradox theory, I analyse the production of coal in four major coal-producing US states that announced a greenhouse gas action plan. To form a statement on whether there is a treatment effect caused by the green policy, I employ Synthetic Control Methods to calculate counterfactual coal production trajectories. I find that Montana experienced a high and statistically significant increase in coal production that is supported by a set of validity indicators. However, there is no evidence for a paradox in Kentucky, Pennsylvania and Virginia.*

JEL classification: *O51, Q38, Q54.*

Keywords: Synthetic control method; green paradox; coal; climate action plans.

1. INTRODUCTION

This paper contributes to the existing literature on the green paradox by providing evidence for its existence. The well-known climate change scenario and its economic consequences have spurred politicians to pass several climate change action programs. However, some of these programs are said to suffer from the green paradox phenomenon, since they do not take reactions on the supply-side into account. According to Sinn (2008), a reduction in demand for fossil fuels on the part of a subset of countries has no positive effect on the evolution of climate change if supply remains constant. Therefore, the efforts of environmentally conscious countries are offset by lower world prices and, thus, increasing demand on the part of environmental sinners. Moreover, green policies that threaten future prices provide incentives for resource owners to extract their fossil fuels more rapidly. Following Allen *et al.* (2009) and Hoel and Jensen (2012), these policies not only accelerate global warming but also impose higher economic costs from climate change.

Although much effort has been spent in the theoretical strand of the green paradox literature (see, e.g., Eichner and Pethig, 2011; Van der Ploeg and Withagen, 2012; Van der Werf and Di Maria, 2012),[1] little is known about its empirical existence. To the best of my knowledge, only a few published studies analyse

1. For a review of several adverse effects of climate policies, see Van der Ploeg and Withagen (2015). A literature survey of the green paradox literature is provided by Jensen *et al.* (2015).

whether the green paradox is a relevant phenomenon one need to care about. Grafton *et al.* (2014) examine the effect of biofuel subsidies on fossil fuel production and carbon dioxide (CO_2) emissions in the US. They find that resource owners increase fossil fuel extraction due to a subsidy on biofuels, giving rise to a weak green paradox. Moreover, since first-generation biofuels come with a low potential for cradle-to-grave CO_2 emission savings compared to conventional fossil fuels, the emergence of a strong paradox – meaning an increase in total CO_2 emissions – is most likely (the underlying theoretical model is provided by Grafton *et al.*, 2012). Di Maria *et al.* (2014) use the announcement and implementation of the US Acid Rain Program as a case study to examine the effect on coal prices, heat input on the part of power plants and the sulphur content of combusted coal. Whereas they find that prices decreased after the announcement of the Acid Rain Program, which is evidence in favour of the green paradox, there is no indication of a demand-side switch towards high-sulphur coal. Therefore, they argue that the green paradox does exist, but its relevance is limited due to market conditions and technical restrictions on the demand-side. Another approach is taken by Lemoine (2017). He analyses the reaction of coal and gas futures when the carbon cap and trade legislation in the US unexpectedly collapsed in 2010. His results point to a strong green paradox because prices rose immediately after the bill collapsed in the Senate. Based on his calculation, the legislative process caused additional CO_2 emissions of approximately 12 million tons.

I aim to extend the scarce empirical strand of the literature by analysing the effect of climate change action plans on the coal production of four major coal-producing states in the US. The US serves as a good example because several states introduced climate change action plans, whereas others did not. These plans contain detailed steps to reduce a state's greenhouse gas emissions. Specifically, they consider a state's resource base, political structure and economy to find cost-effective greenhouse gas reduction opportunities. According to the green paradox theory, these programs threaten future fossil fuel prices and even the property rights of resource owners in the considered states, similar to the planned but collapsed federal carbon cap and trade legislation in 2010.

As mentioned by Lemoine (2017) coal markets are linked over time via extraction, storage and the number of coal-burning plants. Taking into account that the climate action plans are likely to reduce the marginal benefit of delaying extraction as well as the marginal benefit of storage, it is reasonable to assume that resource owners accelerate extraction to maximise their profit under the new conditions. Besides, it is unlikely that the action plans affect the number of currently running coal plants because they are long-lived investments. Therefore, I hypothesise that the adoption of a climate action plan increases state level coal production. This assumption is in line with a theoretical model published by Konrad *et al.* (1994). They show that the threat of resource expropriation has the effect of imposing an additional interest rate on capital. Thus, resource owners accelerate extraction if they gain access to property right protected bank accounts. Specifically, they aim to convert insecure resources *in situ* into secure capital on bank accounts.

To test this hypothesis empirically, I use the synthetic control method (SCM). SCM allows to analyse the net effect of a climate change program on coal production by providing a baseline projection for the treated states (treated means a program was passed), assuming there has been no treatment. I perform placebo tests in space to make a statement on inference and in time to control for the reaction of resource owners to public debates over greenhouse gas programs.

Based on this approach, I find that there is no increase in coal production in Kentucky, Pennsylvania and Virginia. However, Montana experienced a strong and statistically significant increase in coal production due to the climate change action plan.

2. METHOD AND DATA

2.1. Synthetic control method

I use SCM, as introduced by Abadie and Gardeazabal (2003), Abadie *et al.* (2010, 2015), to determine the treatment effect of greenhouse gas programs on the extraction rates of coal at the state level. For each treated state, SCM provides a synthetic counterfactual as a convex combination of control states. The optimal weights – which are assigned to the control states from the donor pool – are determined by a data-driven approach based on pre-intervention characteristics. The net treatment effect can be obtained from the difference between the actual outcomes and its counterfactual.

This procedure is advantageous over other identification strategies, such as OLS-based difference-in-differences estimations, for three reasons (for a more detailed description of the advantages, see Abadie and Gardeazabal, 2003; Abadie *et al.*, 2015; Buchmueller *et al.*, 2011; Munasib and Rickman, 2015). First, the data-driven weighting approach allows prescind from the subjective choice of comparison units. Therefore, I do not have to select several 'no-greenhouse-gas-program' states as individual counterfactuals on the basis of seemingly objective criteria and similarities. It helps to avoid concerns about the validity of my results. This property becomes even more pronounced if one considers that the calculation of the synthetic control unit works without the inclusion of post-intervention outcome data. Consequently, SCM allows usto decide on the study design while remaining blind to how each decision affects the final results. According to Rubin (2001), a procedure like this increases the credibility of the final conclusions. Furthermore, it can be assumed that a linear combination of control units from the donor pool provides a better counterfactual than any single unit could.

Second, the restriction of the weights to be non-negative and sum to one makes the counterfactual more realistic. This feature arises from preventing extrapolation of the data outside the support of the predictor variables of the donor pool. Regression-based counterfactuals, however, do not restrict the weights for their linear combination to be between zero and one. In addition, regression weights are not reported in standard software packages, so researchers typically refuse to provide details on extrapolation biases. This issue distinguishes the SCM counterfactual from the traditional difference-in-differences estimate.

The third advantage regards the uncertainty and inference of the obtained treatment effects. The usual standard error approach may result in overstated significance levels in aggregated data models and does not reflect the ability of the control group to reproduce a trajectory that would have been followed by the treated units if there were no treatment at all. In contrast, SCM offers a new method to test inference. The so-called placebo test in space estimates a placebo treatment effect for each state of the donor pool and ranks the outcomes to obtain a distribution function of treatment effects. By means of this function, I can calculate a p-value that represents the chance to find a treatment effect that is at least of the same magnitude as that of the state of interest if treatment were distributed randomly across all states.

Moreover, SCM has proven its usefulness in several applications. Munasib and Rickman (2015) use SCM to examine the effect of oil and gas production from shale formations on several economic factors, such as employment, personal income, poverty rate or population at the county level. By applying several Monte Carlo simulation strategies, Gobillon and Magnac (2016) provide additional evidence regarding the suitability of SCM to evaluate regional policy effects. Furthermore, they analyse the impact of a French enterprise zone program on unemployment entries and exit status at the municipality level. Bohn *et al.* (2014) apply synthetic controls to assess the effect of a state-level act in the US. In detail, they explore the impact of the Legal Arizona Workers Act on the demographic composition of Arizona's resident population and, thereby, introduce a synthetic control-based difference-in-differences estimate. SCM is also used to overcome endogeneity problems. Pinotti (2015) estimates the economic costs of organised crime in southern Italy. To identify the causal effect running exclusively from crime on economic activity, he uses synthetic controls that mimic the regions Apulia and Basilicata. Because these two regions were exposed to an increase in criminal activity in the 1970s, it is appropriate to assume that a matching procedure based on pre-1970s data will yield a counterfactual that gives a trajectory of economic development in the absence of criminal activity. Consequently, the treatment effects only account for the effect of criminal activity on economic outcome and not vice versa. Hence, I am confident that using SCM is the best way to examine regional policy effects.

2.2. Synthetic control unit

The following presentation of the generation of synthetic control units is based on Abadie and Gardeazabal (2003), Abadie *et al.* (2010, 2015). Suppose that there is a sample composed of $i = 1, \ldots, J + 1$ units of which only the first unit is exposed to the treatment. This sample is observed at dates $t = 1, \ldots, T$, where T_0 denotes the number of pre-treatment periods so that $T_0 \in [1, T[$. $Y^N_{i,t}$ is the outcome of unit i at time t that would be observed in the absence of any treatment. In contrast, $Y^I_{i,t}$ is the outcome that would be observed for unit i at time t if unit i is exposed to a treatment in $t \in [T_0 + 1, T]$. The actual observed outcome $Y_{i,t}$ can be disaggregated in the following way:

$$Y_{i,t} = Y^N_{i,t} + TR_{i,t} \times D_{i,t}, \tag{1}$$

where $TR_{i,t} = Y^I_{i,t} - Y^N_{i,t}$ denotes the treatment effect and $D_{i,t}$ is a dummy variable that indicates whether unit i is exposed to the intervention at time t. Since one assumes that only unit 1 is subject to treatment and that the intervention on unit 1 has no radiating effect on the outcome of the other N units, one finds that $D_{i,t}$ takes the value 1 if $i = 1$ and $t > T_0$. Consequently, one can determine the treatment effect at times $T_0 < t \leq T$ by estimating $Y^N_{1,t}$, because $Y_{i,t}$ can be observed directly. Therefore, it is reasonable to assume that $Y^N_{i,t}$ is given by the following factor model:

$$Y^N_{i,t} = \delta_t + \Theta_t Z_i + \lambda_t \mu_i + \varepsilon_{i,t}, \tag{2}$$

where δ_t is an unknown common factor that is constant across all units, Θ_t is a $(1 \times r)$ vector of unknown parameters, Z_i denotes a $(r \times 1)$ vector of observed and unaffected predictors, λ_t is a $(1 \times F)$ vector of unobserved common factors that may vary over time,[2] μ_i is a $(F \times 1)$ vector of unknown factor loadings, and $\varepsilon_{i,t}$ are zero mean unobserved transitory shocks at the region level. Let us consider a $(J \times 1)$ vector of non-negative weights $W = (w_2, \ldots, w_{J+1})$, whose elements sum up to one. Each possible value of W gives a weighted average of donor units that represents a potential synthetic control. The resulting synthetic control units come with the following value of their outcome variable:

$$\sum_{j=2}^{J+1} w_j Y_{j,t} = \delta_t + \Theta_t \sum_{j=2}^{J+1} w_j Z_j + \lambda_t \sum_{J=2}^{J+1} \mu_j + \sum_{j=2}^{J+1} \varepsilon_{j,t} \tag{3}$$

However, one needs to find the optimal set of weights W^* such that pre-intervention matching:

$$\sum_{j=2}^{J+1} w_j Y_{j,t} = Y_{1,t} \quad \forall t = 1, \ldots, T_0 \tag{4}$$

$$\sum_{j=2}^{J+1} w_j Z_j = Z_1 \tag{5}$$

is achieved at least approximately. Such a W^* does exist if $\left(Y_{1,1}, \ldots, Y_{1,T_0}, Z'_1\right)$ is not too far away from the convex hull of $\left\{\left(Y_{2,1}, \ldots, Y_{2,T_0}, Z'_2\right), \ldots, \left(Y_{J+1,1}, \ldots, Y_{J+1,T_0}, Z'_{J+1}\right)\right\}$. In this case, and under standard conditions, $\hat{\alpha}_{1,t} = Y_{1,t} - \sum_{j=2}^{J+1} w^*_j Y_{j,t}$ can be used as an estimator of $\alpha_{1,t}$ for $T_0 < t \leq T$.

A suitable procedure to obtain W^* is described as follows. Define X_1 as a $(K \times 1)$ vector of pre-intervention values of predictor and outcome variables for the treated unit 1. Let X_0 denote a $(K \times J)$ matrix of the same variables for the J units from the donor pool. The optimal weights W^* are chosen to minimise the weighted distance between X_1 and X_0:

$$W^* = argmin_W (X_1 - X_0 W)' V (X_1 - X_0 W), \tag{6}$$

where V is a non-negative semidefinite $(K \times K)$ matrix whose diagonal elements reflect the importance of each considered predictor variable. At this step, the optimal $W^*(V)$ depends on the choice of the relative predictor importance. Among all possible matrices V, V^* is chosen to minimise the residual mean

2. This is another advantage of the traditional difference-in-differences approach because there it is assumed that λ_t is constant over time so that it can be eliminated by taking time differences.

squared prediction error (RMSPE) of the outcome variable during the pre-intervention period:

$$V^* = argmin_V (Y_1 - Y_0 W^*(V))'(Y_1 - Y_0 W^*(V)), \quad (7)$$

where Y_1 is a $(T_0 \times 1)$ vector of pre-treatment outcomes for unit 1 and Y_0 is a $(T_0 \times J)$ matrix that contains the pre-treatment outcomes for the donor units. Since there are infinitely many collinear solutions of V^*, the Euclidean norm of V^* is normalised to one. The optimal weights are given by $W^*(V^*)$. This synthetic control unit, which comes as a weighted average of units from the donor pool, is the best to reproduce unit 1′s trajectory in the absence of treatment.

2.3. Inference

Although I use aggregate data in the empirical analysis and do not need to worry about aggregation biases of micro data, the ability of my synthetic control unit to reproduce a counterfactual trajectory that would have been followed by the treated unit in the absence of treatment remains questionable (Abadie *et al.*, 2010). Consequently, I do not know whether the calculated treatment effect is just a consequence of a poor evolution of the counterfactual. The general procedure of SCM, however, allows to perform falsification exercises, termed ‘randomization tests’ or ‘placebo studies’, to test the significance of the treatment effect (Abadie *et al.*, 2010, 2015; Munasib and Rickman, 2015). This kind of inference is established even beyond the SCM approach (see, e.g., Auld and Grootendorst, 2004; DiNardo and Pischke, 1997; Li *et al.*, 2013). The in-space placebo study produces qualitative inferences by computing a distribution of a test statistic in the following manner. It assumes that treatment is assigned to actual untreated control units at random. Thus, a synthetic counterfactual is calculated for each unit in the donor pool to receive a distribution of placebo treatment effects. By means of this distribution function, one can state whether the magnitude of the actual intervention effect, as estimated by the synthetic counterfactual trajectory, is large relative to the distribution of placebo treatment effects. More specifically, if one denotes the actual treatment effect by TR_1 and the cumulative density function of placebo effects by $F(TR)$, the p-value of a one-tailed test of a null-hypothesis that $TR_1 \leq 0$ can easily be calculated by $p = 1 - F(TR_1)$(Munasib and Rickman, 2015).

Since my synthetic control states do not perfectly replicate the pre-treatment path of coal production, I apply the difference-in-differences treatment estimator as suggested by Bohn *et al.* (2014):

$$TR_i = \left(\overline{Y_{i,post}^{actual}} - \overline{Y_{i,post}^{synth}}\right) - \left(\overline{Y_{i,pre}^{actual}} - \overline{Y_{i,pre}^{synth}}\right), \quad (8)$$

where $\overline{Y_{i,post}^{actual}}$ is the average actual post-intervention coal production of state i, $\overline{Y_{i,post}^{synth}}$ is the average production of the counterfactual in the post-treatment period, $\overline{Y_{i,pre}^{actual}}$ denotes the average actual pre-treatment production and $\overline{Y_{i,pre}^{synth}}$ is the average pre-intervention coal production of the synthetic counterfactual.

To assert that the synthetic control units serve as suitable counterfactuals, I also provide a set of validity measures (Abadie *et al.*, 2015; Bohn *et al.*, 2014; Jardón *et al.*, 2018). Specifically, I report the relative average pre- and post-treatment differences between the unit exposed to treatment and its synthetic counterfactual, I calculate the post- to pre- root mean square prediction error ratio defined as:

$$RMSPE_{ratio} = \frac{\sqrt{\frac{1}{T-T_0}\sum_{T_0+1}^{T}\left(Y_{1,t} - Y_{synth,t}\right)^2}}{\sqrt{\frac{1}{T_0}\sum_{1}^{T_0}\left(Y_{1,t} - Y_{synth,t}\right)^2}}, \tag{9}$$

as well as the pre-treatment R^2:

$$R^2 = 1 - \frac{\sum_{1}^{T_0}\left(Y_{1,t} - Y_{synth,t}\right)^2}{\sum_{1}^{T_0}\left(Y_{1,t} - \overline{Y}_1\right)^2}. \tag{10}$$

The R^2 measures the fraction of variance of the observed data that is explained by its synthetic counterfactual. It ranges between $-\infty$ to 1, where 1 denotes a perfect replication and $-\infty$ yields if and only if that there is no variance in the observed data. Thus, a high (close to 1) pre-treatment R^2 indicates a good synthetic counterfactual. The $RMSPE_{ratio}$, however, measures the impact of treatment on the explained fraction of variance of the observed outcome. It ranges from 0 to ∞. If treatment had an effect on the outcome, the fraction of explained variance decreases after treatment occurs. Consequently, a high $RMSPE_{ratio}$ denotes a structural break in the outcome variable.

Although the SCM approach focuses on a single unit as the treatment group, I do not collapse the treatment states to construct an average treatment unit prior to applying the SCM. Instead, I follow Gobillon and Magnac (2016) and repeat the SCM procedure for each of them and provide separate inferences.

2.4. Data

In terms of climate change, what matters is not who emits the carbon dioxide but how much carbon dioxide is emitted. Therefore, I do not focus on state-level emissions or coal prices but on the amount of coal produced. My outcome variable *total short tons of coal produced* is provided by the US Energy Information Administration (EIA) and covers a period from 1983 to 2014. To represent the coal production well, I consider a set of energy-based predictor variables containing the *total state-level energy production [trillion Btu]*, the *total state-level energy consumption [trillion Btu]* and the *renewable energy production [trillion Btu]*. These variables are taken from the EIA State Energy Data System.[3] Additionally, I include the *population density [people per 1,000 km²]* of a state based on US census bureau data. To control for the capability to extract coal, I use the amount of *recoverable reserves [billion short tons]* as a predictor. These data are processed and published by Höök and Aleklett (2009). Furthermore, I include the *Gross Domestic Product [$-US]* (GDP) as provided by the Bureau of Economic Analysis as a proxy of demand for energy. The GDP data are available from 1986 onwards. At this point, I have to mention that there is a discontinuity in the GDP data in 1997 due to a change of the calculation method. However, since all states are affected by this discontinuity, I do not need to worry about biased estimates in the SCM.

3. Data are available from 1960 onwards.

Unfortunately, I cannot include transportation costs of coal to control for state level import and export options, because they are withheld by EIA to avoid disclosure of individual company data. To include the trade openness of a state as an exogenous predictor variable, nevertheless, I apply two proxies. First, I include the *Foreign Distribution [short tons]* of state coal production as reported in the EIA Annual Coal distribution Report. Second I use data from the Seamless Digital Chart of the World (SDCW) as well as boundary data from the Global Administrative Areas Project (GADM) to calculate the average *distance to coast [pixel in ArcGIS]* of each state. As mentioned by Yi and Feiock (2015) the level of per capita emissions is an important determinant for the adoption of a climate action plan. Therefore, I include the *average per capita emissions during the 1990s [Metric Tons CO_2 per capita]* as provided by the Environmental Protection Agency (EPA) in the set of predictor variables.

My donor pool consists of 16 states (Alabama, Georgia, Idaho, Indiana, Kansas, Louisiana, Mississippi, Nebraska, North Dakota, Ohio, Oklahoma, South Dakota, Tennessee, Texas, West Virginia and Wyoming) that neither passed a climate action plan nor officially announced greenhouse gas emission targets. At this stage I need to mention that – even in my donor pool states – some cities adopted local action plans. Tang *et al.* (2010) analyse these local plans and figure out that they are very limited in their 'action approaches'. Thus, I am confident that these local plans have no effect at the state level

I am aware of the trade-off between inner and outer optimisation in the calculation of the synthetic control unit and its consequences. As theoretically and empirically shown by Kaul *et al.* (2015), the inclusion of all pre-treatment outcome variables as separate predictors makes all other predictor variables irrelevant. Hence, instead of including all of the pre-treatment coal production data, I opt for the inclusion of coal production data at two points in time, namely, 1990 and the pre-treatment year, to yield a smooth pre-treatment calibration as well as high residual predictor weights.

Another issue regards the synthetic control unit's predictor and outcome values. As shown by Abadie *et al.* (2010, 2015), estimators are asymptotically unbiased only if the counterfactual pre-treatment outcome variable as well as predictors correspond to the characteristics of the treated unit. Although I aim to include all predictor variables in each analysis at the same time, I have to drop several variables in cases in which the treated unit's characteristics fall outside the support of the convex hull of the control units.[4] For the same reason and because my donor pool cannot reproduce trajectories of states that are poor in coal production, I restrict my empirical analysis to four major coal-producing US states.

3. ANALYSIS

The application of SCM to the green paradox requires that the implementation of the climate action plans represent actual exogenous shocks to the coal

4. Due to word count and space limitations, I only report the final results. However, the first calculation results do not differ much.

Table 1 Determinants of climate action plan adoption:

	Kentucky	Pennsylvania	Virginia	Montana
Per capita emissions (million	$3, 4 \times 10^{-}5$	$2, 2 \times 10^{-}5$	$1, 6 \times 10^{-}5$	$3, 5 \times 10^{-}5$
metric tons CO_2 – above 3. quartile)	Yes	No	No	Yes
Last election won by republican	No	No	No	No
Number of surrounding states that adopt a plan	3	1	1	0
Unexpected shock	No	No	No	Yes

Source: C2ES (2016) and EPA (2017).

producers. Unfortunately, there is no way to prove that the adaption has been an entirely exogenous shock. However, to verify this assumption at least partly, I refer to Yi and Feiock (2015) who analyse the causes of climate action plan adoptions in the US. According to their estimation strategy, three factors significantly affect the politician's decision whether or not to adopt an action plan. Specifically, citizen ideology (green or democratic), low per capita carbon emissions and high policy diffusions (in terms of number of surrounding states that already adopt a plan) increase the probability that the state adopts a climate action plan. I, thus, consider the announcement of a climate action plan as an unexpected shock to coal producers, if at least two of the three following conditions hold. First, the average per capita emissions in the 1990s are above the third quartile in the entire US distribution.[5] Second, the last pre-treatment gubernatorial elections were won by a republican. Third, at most one of the surrounding states adopted an action plan before. The features of the four states are depicted in Table 1.

Based on this short summary, I expect to find the strongest effect in Montana because it is most likely that the announcement of a climate action plan represented an unexpected shock on coal producers. This assumption can be augmented by looking at Figure 1 in which I plot states of the US – coloured according to whether they passed an action plan (green) or not (red). It becomes obvious that of all treated states, Montana shares the longest border with sates that did not passed an action plan by far. Thus, Montana's coal producers might be able to export their coal to non-treated states. In the other states, coal producers may have anticipated an impending legislation. Moreover, they might not be able to export their production. Thus, the SCM may not find any effect of the adoption of a climate action plan on coal production in Kentucky, Pennsylvania and Virginia.

Since I follow Gobillon and Magnac (2016) and repeat the SCM procedure for each state, I discuss the action plans and results in separate subsections.

5. I focus on the 1990s because several states announced a plan in the late 1990s and early 1900s. Thus, considering a more recent period may yield an endogenous comparison if these plans have any impact on greenhouse gas emissions.

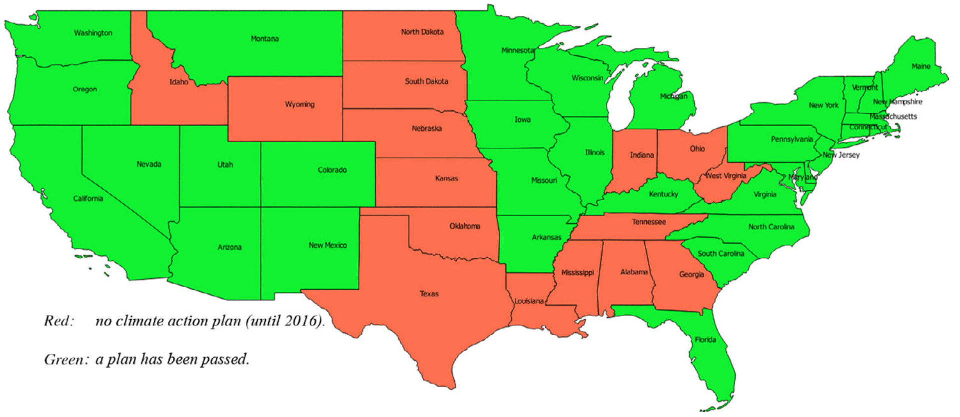


Figure 1 Map of action plans

3.1. Kentucky

With an average output of 1.54×10^8 short tons per year, Kentucky was the number one coal-producing state in the US during the 1980s.[6] In 2008, Kentucky announced a '7-Point Strategy for Energy Independence'. Among others, this plan contains strategies on how to increase renewable energy production and energy efficiency. Two years later, the Kentucky Climate Change Action Plan Council (KCAPC 2011) convened to further the 7-Point Strategy. The final action plan was published in 2011 and recommended the following strategies to achieve a 20% reduction in greenhouse gases from 1990 levels within the next 19 years:

Strategy 1: Improvements in energy efficiency in residential, commercial, industrial and transportation sectors shall contribute in an offset of at least 18% of the projected 2025 energy demand.
Strategy 2: Increasing the renewable energy generation to provide 1,000 MW by 2025.
Strategy 3: Increasing the production of biofuels such as ethanol and biodiesel to satisfy 20% of the current total motor fuel demand.
Strategy 4: Developing a coal-to-liquid industry that will produce four billion gallons of fuel per year.
Strategy 5: Augmentation of natural gas production with the implementation of a major coal-gasification industry to produce the equivalent of 100% of annual gas demand.
Strategy 6: Application of Carbon Capture and Storage (CCS) technology to 50% of all coal based energy applications.
Strategy 7: Examining the use of nuclear power.

Due to the high importance of the coal industry for Kentucky's economy (more than 17,000 jobs), the 7-Point-Strategy explicitly aims at diversifying the coal industry's product line by developing a coal to liquid and coal to gas industry. Although these goals per se reduce the chance of the green paradox to occur

6. I consider the 1980s as an example because there had been no climate act at that time.

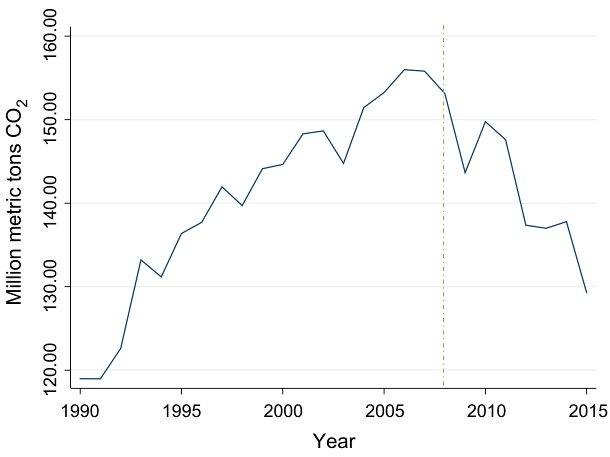


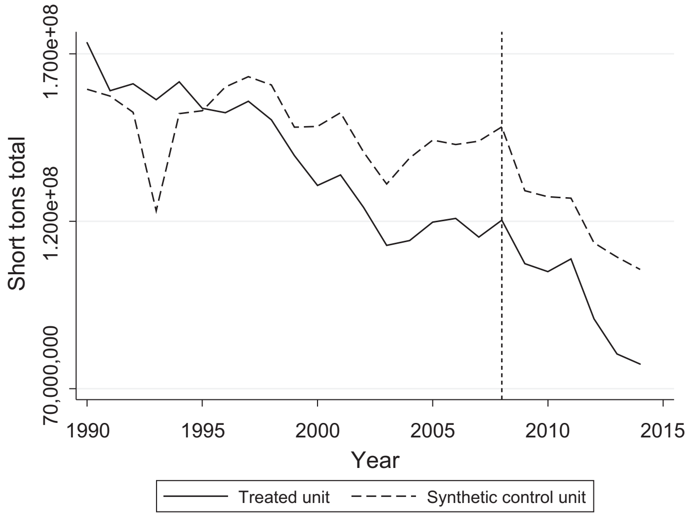
Figure 2 Carbon dioxide emissions in Kentucky

as they increase demand and, thus, stabilize the value of coal for the next decade (s), their effect is offset by strategy 6. According to Steinkraus (2016a) the cradle-to-grave costs of CCS amount to 60$ per ton of stored CO_2. Consequently, the application of CCS significantly reduces the future net value of coal and gives rise to the green paradox.

To make a statement on how successful the achievement of target is so far, I plot the total state level emissions from 1990 onwards in Figure 2. It becomes obvious, that the announcement of the 7-Point Strategy marks the beginning of a downward slope in the CO_2 emissions. To be more specific, the emission level decreased by 17% during the post treatment period. As the green paradox requires increasingly strict regulations, I am confident that the climate action plan serves as a good policy treatment in order to analyse the green paradox.[7] However, one need to keep in mind that announcement did not necessarily represente an unexpected shock to coal producers.

Based on the timetable of green policies, I consider the year 2008 as the treatment date in a first step. Here, I exclude *renewable energy production* as well as *the foreign coal distribution* from the set of predictor variables because these characteristics are far beyond the support of the convex hull of control variables. A graphical and tabular illustration of the results can be seen in Figure 3. It is quite obvious that there is no distinct effect in the graphical illustration. However, the difference-in-differences treatment estimator (Bohn *et al.*, 2014) reveals a negative treatment effect of −16,655,766 short tons of coal, which is contrary to my expectation and the green paradox hypothesis. To make a statement on the significance of this effect, I perform placebo tests in space and rank the outcomes. Kentucky's treatment effect is at rank 1 of 17, corresponding to a *p*-value of 0.0588.

7. I do not find any important coinciding events that hinder the causal interpretation of the SCM results.



Donors (Weights): Ohio (0.073), West Virginia (0.927). RMSPE: $2.11 \cdot 10^7$

Predictor	Treated Unit	Synthetic Unit	Relative Weights
Short Tons Total (1990)	$1.73 \cdot 10^8$	$1.59 \cdot 10^8$	0.322
Short Tons Total(2007)	$1.15 \cdot 10^8$	$1.44 \cdot 10^8$	0.177
Tot. Energy Production	3,424,230	3,581,084	0.282
Tot. Energy Consumption	1,398,468	966,958	0.038
Distance to Coast	6.69	3.62	0.004
Recoverable Reserves	22.05	30.51	0.096
GDP	29,416	25,817	0.000
Population Density	337.7	314	0.028
CO_2 per Capita	34.4	55.1	0.052

Validity Indicator	
Post- to Pre- RMSPE ratio	1.402
pre-treatment *R*	0.149
Ø pre-treatment difference (rel.)	-0.068
Ø post-treatment difference (rel.)	-0.256

Figure 3 Kentucky, Treatment in 2008

One could argue that resource owners already anticipate the start of debates on green policies. Hence, I repeat the analysis, but consider the year 2006 as the date of treatment to perform a so-called placebo test in time. The results are shown in Figure A1 in the appendix. Again, I do not find eye-catching evidence for the green paradox. Nevertheless, the treatment effect is still negative and amounts to −16,137,915 short tons of coal. Its rank in the placebo tests in space remains at 1 of 17. Despite the significance of the treatment effect, the weak performing validity indicators show the low capability of the counterfactual to mimic Kentucky's pre-treatment coal production trajectory. Thus, the case of Kentucky gives no entirely convincing evidence against the existence of the green paradox.

3.2. Pennsylvania

Pennsylvania's Climate Change Advisory Committee (CCAC) was established in 2008 and completed their final Climate Change Action Report by December 2009. The action plan is framed by a narrow time scale (2000–20) and a high carbon reduction goal (30% below 2000 levels). In detail, it contains 52 specific recommendations that are likely to yield a greenhouse gas reduction of approximately 36%. A revised and updated version of the plan was published by the Department of Environmental Protection (DEP) in 2013 and 2015. Besides the recommendation of nine legislative actions, it notes a shale-gas-caused shift from coal to natural gas use that is accompanied by the deactivation of coal-fired power plants. Moreover, the work plan also recommends the application of CCS, an improvement of coal-fired power plant efficiency by 5% as well as a reduction of sulphur hexafluoride emissions. The focus on coal arises from the fact that approx. 93% of Pennsylvania's greenhouse gas emissions from electricity production were caused by the combustion of coal. All actions may cause a future decrease in the net value of coal and, thus, provide incentives to mine owners to accelerate extraction. On the contrary, the DEP passed new and stricter safety laws for underground mines in 2008. This regulation as well as the fact that the announcement did not represent an unexpected shock may offset the green paradox effect.

However, similar to the case of Kentucky, the establishment of the CCAC denotes a structural break in the emission data as it can be seen in Figure 4. During the post treatment period, the level of CO_2 decreased by almost 16%. Based on this development, I consider the establishment of the CCAC as a reasonable treatment date.

In the SCM analysis it turns out that I have to exclude the *population density* as well as the *recoverable reserves* from the set of predictor variables because the synthetic control unit is unable to reproduce Pennsylvania's characteristics

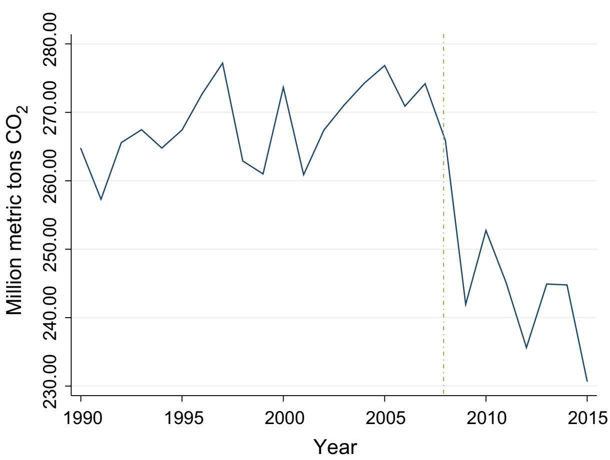
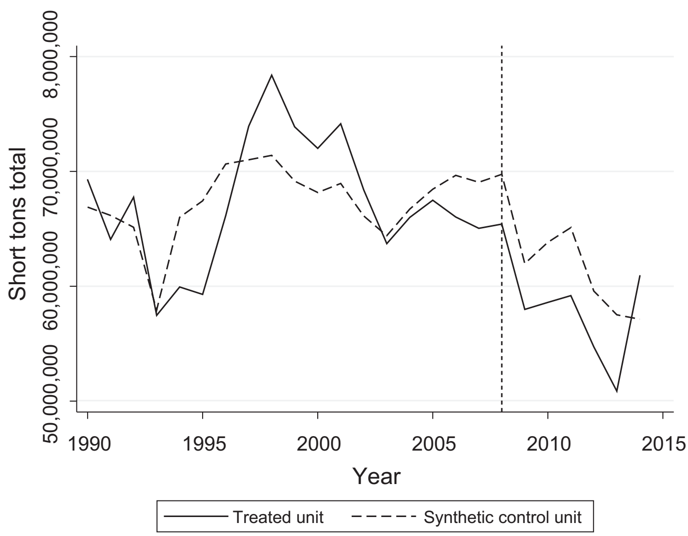


Figure 4 Carbon dioxide emissions in Pennsylvania

adequately. Nevertheless, this approach yields a good fit in the trade-off between inner- and outer-optimisation, as shown in Figure 5.

Similar to the case of Kentucky, the graphical illustration reveals no eye-catching evidence of a green paradox. Moreover, the estimated treatment effect amounts to a negative value of −3,491,202 short tons of coal. This effect corresponds to a rank of 2 of 17 in the placebo test and is thus far beyond being positive indeed. This result is supported even by the validity indicators because they reveal the ability of the synthetic control unit to mimic the pre-treatment coal production of Pennsylvania. The same holds if I perform a placebo test in time



Donors (Weights): Georgia (0.391), Ohio (0.005), Texas (0.318), West Virginia (0.247), Wyoming (0.039).

RMSPE: $4.07 \cdot 10^6$

Predictor	Treated Unit	Synthetic Unit	Relative Weights
Short Tons Total (1990)	$6.93 \cdot 10^7$	$6.69 \cdot 10^7$	0.034
Short Tons Total (2007)	$6.50 \cdot 10^7$	$6.91 \cdot 10^7$	0.032
Tot. Energy Production	2,598,749	5,113,7,1	0.000
Tot. Energy Consumption	3,798,517	3,855,943	0.011
Renewable Energy Prod.	97,247	100,784	0.007
Distance to Coast	2.48	3.75	0.002
GDP	33,765	33,304	0.015
Foreign Distribution	6005	5995.6	0.900
CO_2 per Capita	22.1	38.2	0.001

Validity Indicator	
Post- to Pre- RMSPE ratio	1.244
pre-treatment *R*	0.440
Ø pre-treatment difference (rel.)	-0.004
Ø post-treatment difference (rel.)	-0.069

Figure 5 Pennsylvania, Treatment in 2008

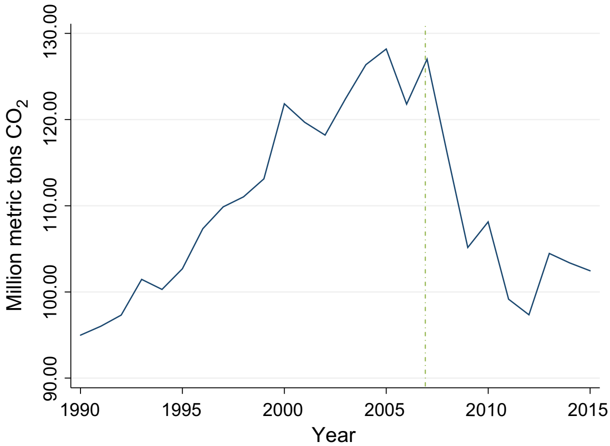


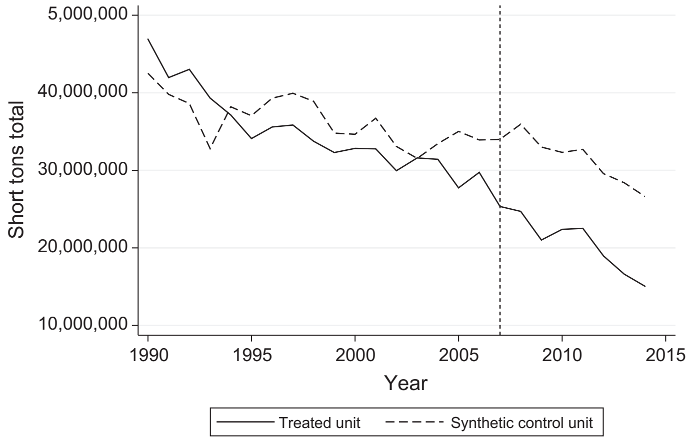
Figure 6 Carbon dioxide emissions in Virginia

and consider an earlier treatment date (2006), as shown in Figure A2 in the appendix. Specifically, the treatment effect increases to −4,543,385 and remains at rank 2 of 17. However, due to the contemporaneous adoption of new safety laws, I cannot conclude that the announcement of green policies did not cause a green paradox phenomenon in Pennsylvania. At least, I can conclude that there has been now acceleration in coal extraction in absolute terms.

3.3. Virginia

Before Virginia's Climate Action Plan was released by the Governor's Commission on Climate Change (GCCC) in 2008, the Department of Mines, Minerals and Energy (DMME) completed the Virginia Energy Plan based on information from the Virginia Energy Plan Advisory Group (VAG) in 2007. This plan contains several energy-based goals for the year 2017, including reducing the growth rate of energy use by 40% and capping their greenhouse gas emissions at 2000 levels. These goals were included in the Climate Action Plan and partially extended. Among others, the plan recommends upgrades to coal-fired plants that allow for the combustion of a fuel-mix containing up to 20% biomass. However, in contrast to Kentucky and Pennsylvania, Virginia's plan does not contain detailed aspects of CCS applications but focuses on investments into research in order to make CCS less cost-intensive. Taking into account that a proposal to ban new coal-fired plants until CCS is proven feasible was rejected, a coal industry friendly view becomes apparent. As the plan's key aspects address transportation and natural sequestration of carbon it seems that Virginias Plan has little potential to cause a green paradox in the coal industry. This expectation is supported by the fact that the plan did not represent an unexpected shock to coal producers.

Though, the success of the plan including the outperformance of goals becomes obvious when looking at Figure 6. Until now, emissions were not only capped at 2,000 levels but actually reduced by 16%.



Donors (Weights): Nebraska (0.081), Ohio (0.377), South Dakota (0.035), Tennessee (0.347), West Virginia (0.160)

RMSPE: $4.19 \cdot 10^6$

Predictor	Treated Unit	Synthetic Unit	Relative Weights
Short Tons Total (2000)	$3.28 \cdot 10^7$	$3.46 \cdot 10^7$	0.005
Tot. Energy Production	1,225,858	1,187,867	0.002
Tot. Energy Consumption	1,696,008	2,195,303	0.098
Renewable Energy Prod.	89,826	90,027	0.187
Recoverable Reserves	180.6	180.6	0.454
GDP	37,442	31,325	0.001
Population Density	572.8	562.4	0.247
CO_2 per Capita	15.7	27.8	0.005

Validity Indicator	
Post- to Pre- RMSPE ratio	2.748
pre-treatment *R*	0.388
Ø pre-treatment difference (rel.)	-0.053
Ø post-treatment difference (rel.)	-0.537

Figure 7 Virginia, Treatment in 2007

In the empirical analysis, I thus consider the year 2007 as a suitable treatment date.[8] In this regard, I have to include the pre-treatment coal production in the year 2000 as a predictor variable because the inclusion of other outcome variables either comes with a relative predictor weight of approximately 90%, or it yields a poor outer-optimisation. Additionally, I have to exclude the *distance to coast* as well as the *foreign distribution of coal* (see Figure 7). Still, I do not find evidence for the existence of a green paradox, due to a treatment effect of −9,313,035 short tons of coal (rank 4 of 17 in the placebo test). The negative treatment effect is even more pronounced if I take the year 2005 as a treatment date into account (see Figure A3 in the appendix). Specifically, the average difference-in-differences between Virginia and its counterfactual amounts to -1×10^7

8. I do not find evidence of significant changes in mining or safety regulation during the period of investigation that may harm the causal interpretation of final results.

Figure 8 Carbon dioxide emissions in Montana

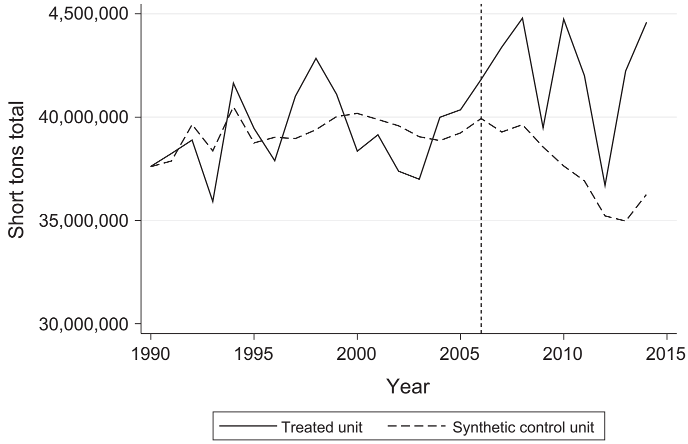
short tons of coal (rank 4 of 17). Based on the treatment effects and the well performing validity indicator, I can conclude that there is no green paradox in Virginia.

3.4. Montana

Montana's Climate Change Advisory Committee (MCCAC 2007) was established by the Montana Department of Environmental Quality (MDEQ) in December 2005. Two years later, the committee published their Final Climate Change Action Plan. It contains a total of 54 policy recommendations that aim to reduce the total amount of greenhouse gas emissions to 1990 emission levels within the next 13 years. Among others, the plan recommends to pursue research and development projects on CCS financed for example, by coal severance taxes. Since Montana has a prospering Coal-to-Liquid industry, which is very CO_2 intense, energy efficiency and process options – such as enhanced combined heat and power or CCS – shall be applied to reduce energy and electricity based emissions. At the same time, the Montana Legislature endorsed the Governor's recommendation to increase the share of renewable energy production in total energy production to 25% by the year 2025. All of these features put pressure on future demand of coal and, thus, may cause a green paradox. This effect may have been boosted due to the fact that the plan represented an unexpected shock to coal producers.

To make a statement on the effectiveness of the plan, Figure 8 shows Montana's annual greenhouse gas emissions. It becomes obvious, that the maximum turning point in CO_2 emissions was reached in 2007. Considering the year 2006 as reasonable treatment date, I find that emissions decreased by almost 10% during the post treatment period. Based on this result, I am confident that coal producers may have assumed that regulations become stricter over time.

In the computation of Montana's counterfactual, I have to exclude *Population Density, Foreign Distribution of Coal* as well as *Renewable Energy Production* from the



Donors (Weights): Louisiana (0.007), North Dakota (0.931), Texas (0.002), West Virginia (0.052), Wyoming (0.008). RMSPE: $2.06{\cdot}10^6$

Predictor	**Treated Unit**	**Synthetic Unit**	**Relative Weights**
Short Tons Total (1990)	$3.76{\cdot}10^7$	$3.76{\cdot}10^7$	0.238
Tot. Energy Production	769,833	754,676	0.263
Tot. Energy Consumption	340,568	333,252	0.418
Distance to Coast	12.9	12.8	0.045
Recoverable Reserves	89.8	106.4	0.037
GDP	25,344	28,341	0.001
CO_2 per Capita	34.9	67.7	0.000

Validity Indicator	
Post- to Pre- RMSPE ratio	3.224
pre-treatment *R*	0.199
Ø pre-treatment difference (rel.)	-0.002
Ø post-treatment difference (rel.)	0.106

Figure 9 Montana, Treatment in 2006

set of predictor variables. In addition, the total coal production in 2005 has a relative predictor weight of approximately 75%. Thus, I opt for the inclusion of the coal production in 1990 as the only pre-treatment outcome variable in the set of predictors. The case of Montana reveals eye-catching evidence for the existence of a green paradox, as can been seen in Figure 9. Importantly, this effect can be interpreted causally because I do not find any coinciding updates of coal mining regulations. The treatment trajectory is also illustrated in Figure 10 and shows the divergence of Montana and its counterfactual after the treatment in 2006 in comparison to the placebo treatment trajectories. In detail, the treatment effect amounts to 6,100,087 short tons of coal. This result comes with a rank of 1 of 17 in the placebo test, corresponding to the best-possible *p*-value of 0.0589. Moreover, these findings are strongly supported by the validity indicators because they highlight the structural break in 2006 as well as the suitability of the synthetic counterfactual to mimic Montana's coal trajectory.

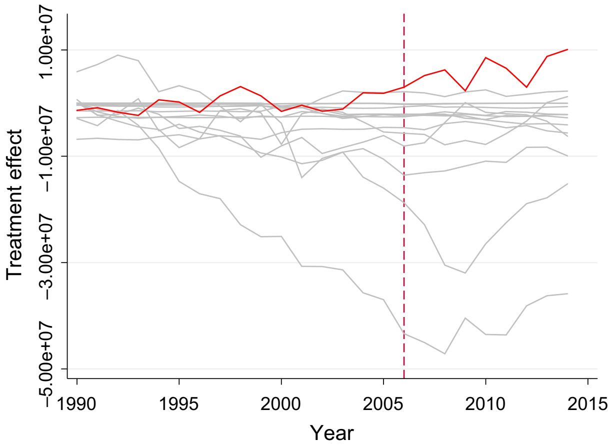


Figure 10 Treatment trajectories of Montana (red) and the donors (grey)

However, Montana's coal production is far above its counterfactual's coal production even in 2004. Therefore, I consider the year 2004 as date of treatment in a placebo test in time (see Figure A4). It turns out that the treatment effect amounts to an additional 5,407,501 short tons of coal annually and is still at rank 1 of 17. Hence, I am confident that the green paradox hypothesis holds for Montana.

4. CONCLUSION AND POLICY IMPLICATIONS

The green paradox theory states that policy interventions that aim to reduce the future level of greenhouse gas emissions may cause an increase in today's emissions due to the failure to include the supply-side. Although this paradox is an important issue in the debate on climate change, there has been little effort in the investigation of its existence. To the best of my knowledge, only three published papers provide evidence in favour of or against its existence. Thus, in this paper, I sought to enlarge the empirical strand of the literature on the green paradox. To do so, I used the state-level announcement of Climate Change Action Plans or the establishment of Climate Action Committees as a suitable policy treatment. I ran several SCM models for four major coal-producing states in the US to estimate the treatment effect, which is defined as the average difference-in-differences between the considered state and its synthetic counterfactual.

However, as the empirical literature is mixed, so are my results: On the one hand, I find distinct negative treatment effects for Kentucky, Pennsylvania and Virginia. However, these results do not necessarily imply evidence against the existence of a green paradox: They are either not supported by the validity indicators and/or the placebo tests in space or they do not reveal a causal climate action plan effect due to contemporary safety regulations. On the other hand, Montana's treatment effect is positive, statistically significant and supported by the set of validity indicators. One could argue that these results are biased due to small donor pool biases, a poor fit of the counterfactual to reproduce the treated unit's trajectory or unsuitable predictors. However, since the residual mean squared prediction errors are comparatively small and due to the good fit of predictors I am confident that the results are unbiased.

As a consequence, I conclude that reactions on the side of resource owners are very case dependent. Hence, green policies do need to consider the supply-side because – based on the data and econometric strategy – I cannot rule out that the green paradox does not indeed exist. Among others, long term contracts, restrictions on the demand-side, limited access to the world market, diverse extraction costs as well as different coal qualities may explain the occurrence or absence of the paradox. Specifically, policies must consider the incentives they provide to resource owners. The only way to ensure that green policies come with lower extraction rates is to assert that the net present value of the resources *in situ* remains constant regardless of the type of policy. As shown by Steinkraus (2016b), subsidizing human capital is a suitable and politically legitimate way to fulfil this restriction because investments in education increase the future productivity of fossil fuels and, thus, cause higher future price levels. The so-produced positive net present value effect can be consumed by the introduction of strong green policies. Consequently, a slower speed of resource extraction might be achieved.

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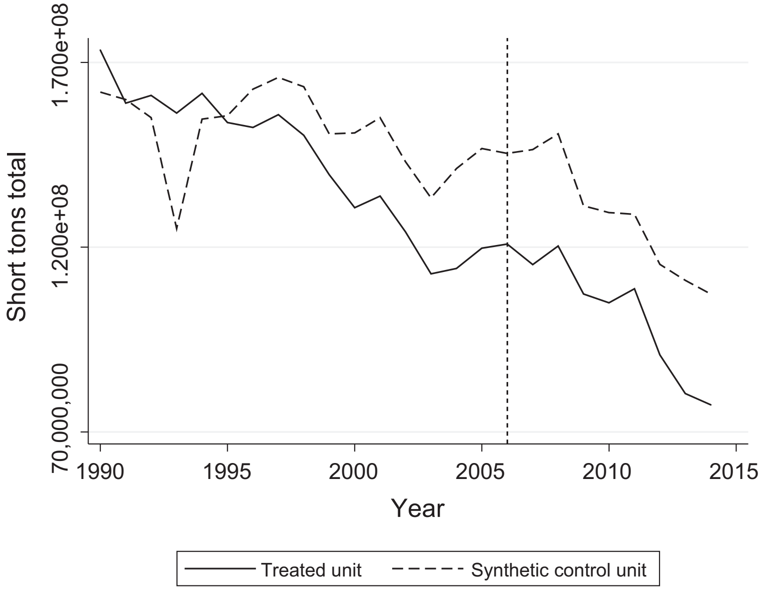
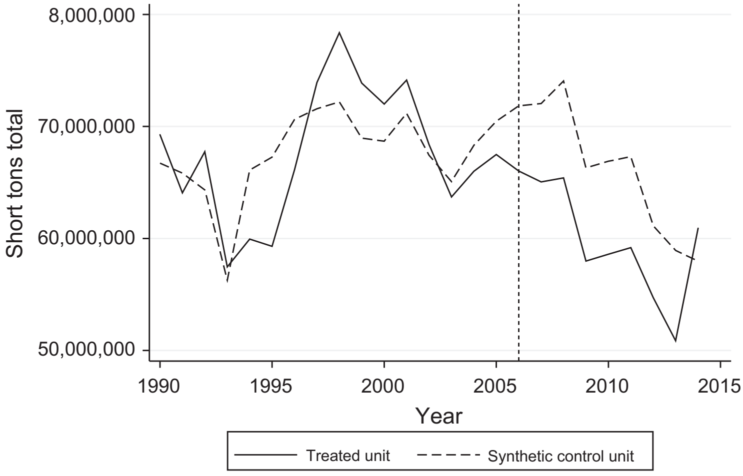
APPENDIX



Donors (Weights): Ohio (0.054), West Virginia (0.946). RMSPE: $2.05 \cdot 10^7$

Predictor	Treated Unit	Synthetic Unit	Relative Weights
ShortTonsTotal(1990)	$1.73 \cdot 10^8$	$1.62 \cdot 10^8$	0.193
ShortTonsTotal(2005)	$1.20 \cdot 10^8$	$1.47 \cdot 10^8$	0.022
TotEnergyProduction	3,437,845	3,617,187	0.503
TotEnergyConsumption	1,371,907	906,811	0.041
DistanceToCoast	6.69	3.57	0.005
RecoverableReserves	24.9	35.8	0.200
GDP	28,453	24,730	0.001
PopulationDensity	337.7	301	0.031
CO_2 per Capita	34.4	55.8	0.004

Figure A1 Kentucky, Treatment in 2006

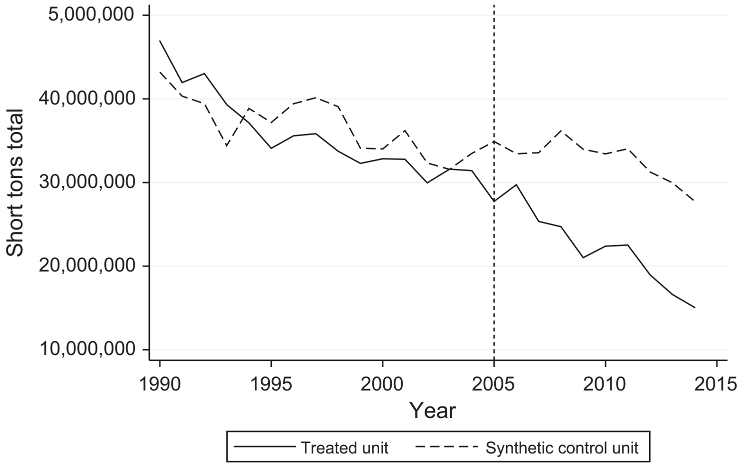


Donors (Weights): Georgia (0.429), Ohio (0.132) Texas (0.114), West Virginia (0.277), Wyoming (0.048).

RMSPE: $3.94 \cdot 10^6$

Predictor	**Treated Unit**	**Synthetic Unit**	**Relative Weights**
Short Tons Total (1990)	$6.93 \cdot 10^7$	$6.67 \cdot 10^7$	0.003
Short Tons Total (2007)	$6.75 \cdot 10^7$	$7.05 \cdot 10^7$	0.001
Tot. Energy Production	2,595,305	2,909,697	0.000
Tot. Energy Consumption	3,793,050	2,551,905	0.000
Renewable Energy Prod.	96,854	96,877	0.994
Distance to Coast	2.48	3.89	0.000
GDP	32,515	31,666	0.000
Foreign Distribution	6005	6755	0.000
CO_2 per Capita	22.1	37.7	0.000

Figure A2 Pennsylvania, Treatment in 2006

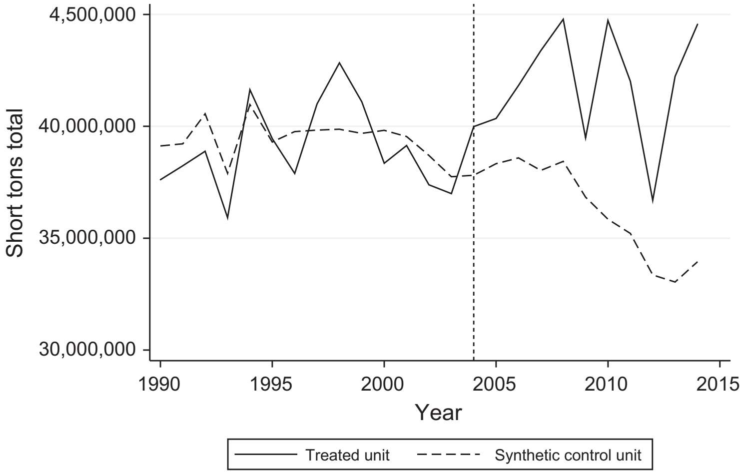


Donors (Weights): Alabama (0.10), Idaho (0.247), Ohio (0.524), West Virginia (0.129)

RMSPE: $3.43 \cdot 10^6$

Predictor	**Treated Unit**	**Synthetic Unit**	**Relative Weights**
Short Tons Total (2000)	$3.28 \cdot 10^7$	$3.40 \cdot 10^7$	0.000
Tot. Energy Production	1,224,587	1,222,804	0.002
Tot. Energy Consumption	1,656,028	2,337,837	0.000
Renewable Energy Prod.	88,358	88,334	0.761
Recoverable Reserves	3.5	14	0.000
GDP	35,770	28,924	0.000
Population Density	572.8	572.7	0.237
CO_2 per Capita	15.7	25	0.000

Figure A3 Virginia, Treatment in 2005



Donors (Weights): Idaho (0.021) North Dakota (0.818), South Dakota (0.071), West Virginia (0.09).

RMSPE: $1.63 \cdot 10^6$

Predictor	**Treated Unit**	**Synthetic Unit**	**Relative Weights**
Short Tons Total (1990)	$3.76 \cdot 10^7$	$3.91 \cdot 10^7$	0.002
Tot. Energy Production	754,557	751,396	0.955
Tot. Energy Consumption	337,443	303,130	0.028
Distance To Coast	12.9	12.5	0.011
Recoverable Reserves	89.8	141.4	0.001
GDP	24,167	26,646	0.002
CO_2 per Capita	34,9	62,3	0.000

Figure A4 Montana, Treatment in 2004

