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Efficacy of Carbon Pricing: An Econometric Analysis of the Regional Greenhouse Gas Initiative

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Abstract

This study analyzes the effect of the Regional Greenhouse Gas initiative (RGGI), a carbon pricing system in the northeast of the United States, on CO_2 emissions. To estimate the effect, several econometric methods are used, including the *synthetic control method*. For the state of Maryland, the estimated causal effect is a reduction in CO2 emissions of 1.27 tons per capita in the year 2019 compared to a scenario in the absence of the RGGI. This result is robust to a number of placebo tests.

Zusammenfassung

Diese Studie analysiert die Auswirkungen der Regional Greenhouse Gas Initiative (RGGI), eines Kohlenstoffpreissystems im Nordosten der Vereinigten Staaten, auf CO_2 Emissionen. Um den Effekt abzuschätzen werden mehrere ökonometrische Methoden verwendet, einschließlich der *Synthetic Control Method*. Für den Bundesstaat Maryland ist der geschätzte kausale Effekt eine Reduzierung der CO2-Emissionen um 1,27 Tonnen pro Kopf im Jahr 2019 im Vergleich zu einem Szenario ohne RGGI. Dieses Ergebnis ist robust gegenüber einer Reihe von Placebo-Tests.

Contents

Chapter 1

Introduction

Climate emergency is a race we are losing, but it is a race we can win.[1]

António Guterres
UN Secretary-General

In order to win the race to halt global warming and prevent the most devastating damages, it is necessary to bring net carbon dioxide emissions of the world economy to zero (Pierrehumbert 2019). Countless policy instruments have been proposed to achieve this goal. Probably the most appealing instrument for economists is carbon pricing. This means introducing a price on CO_2 emissions to internalize the external costs of greenhouse gas (GHG) emissions. Increasing the price on activities that result in higher GHG emissions means creating an economic signal for polluters who then have an incentive to shift consumption or invest in technologies that avoid GHG emissions (The World Bank 2014). This would be an ideal tool to reduce GHG emissions sharply in a cost-effective way, based on the principle "polluter pays" (Bowen 2011).

Carbon pricing is an increasingly popular policy tool to combat climate change. The first country that introduced a CO2 tax was Finland in the 1990s (Vourc'h and Jimenez 2000). Today, there are more than 40 countries and several cities, states and provinces that use some kind of carbon pricing mechanism, and there are more which plan to implement them in the future (The World Bank 2014). Broadly speaking, there are three different approaches how to put a price on GHG emissions: Introducing a carbon tax like in Finland, implementing a cap-and-trade system (C&T) like e. g. the European Trading System, that was introduced in 2005 in the European Union, or a hybrid form (European Commission 2015). In a C&T system, emitters have to buy allowances for GHG emissions and therefore pay a higher price. The two systems have the same goals and could theoretically lead to the same effects. In this thesis, the term

[1] United Nations 2021.

carbon pricing is used to sum up different ways a jurisdiction could implement a price on GHG emissions.

Carbon pricing can internalize the externalities associated with CO2 emissions and subsequently promote cost-effective abatement (Aldy and Stavins 2012). It is clear that there is a negative effect from a carbon pricing policy on GHG emissions, but the magnitude of the effect can be difficult to measure. There are too many other factors that might influence GHG emissions in a given region to control for in a regression analysis. For a well-designed policy to combat climate change it is necessary to be able to predict the effect of a carbon price. In this thesis, the effect of a carbon pricing policy is analyzed empirically.

Regional Greenhouse Gas Initiative. The Regional Greenhouse Gas Initiative (RGGI) is a mandatory C&T program on GHG emissions in participating states in the United States. It was established in 2005 and started to auction emission allowances in 2008. The cap has been decreased each year, and the auction proceeds are reinvested in the states to improve energy efficiency and renewable energy. The participating states are Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Rhode Island, Vermont and Virginia which are all located in the northeast of the US (Regional Greenhouse Gas Initiative 2021b). California is the only other US state that has implemented a Carbon pricing system (Center for Climate and Energy Solutions 2021). The most populated US state has commenced a C&T program in 2012 (Woo et al. 2017).

Chapter 2 summarizes literature that has been published regarding the topic in recent years, chapter 3 discusses the methods that are used in this thesis, chapter 4 describes the data used for the empirical analysis, Chapter 5 presents the results, chapter 6 discusses the results and 7 concludes.

Chapter 2

Literature Review

In recent years, a lot of studies have investigated the efficacy of Carbon Pricing mechanisms. Broadly speaking, there are three different categories of Carbon pricing programs that were analyzed theoretically by Goulder and Schein (2013). A jurisdiction can implement a tax on carbon emissions, it can introduce a C&T system or it can adopt a hybrid form, which would be a C&T with a minimum price and/or a price ceiling. The main difference is that a carbon Tax fixes the price of carbon, but leaves the amount of emissions that are emitted eventually uncertain, and an C&T fixes the amount of emissions that are allowed and leaves the price of emissions uncertain.

Regarding the main goal of a carbon pricing system, which is creating incentives to reducing allocation, they are equivalent. Regarding distributional effects or international competitiveness concerns due to additional costs of domestic firms, these systems can also be used equivalently. The differences are in the details, for example: Uncertainty in the price dimension (C&T) might be harmful for businesses, while uncertainty in the amount of emissions (carbon tax) is problematic when the goal is to reach certain emission targets. This can be somewhat mitigated by a hybrid system.

One recent study that investigates the efficacy of carbon pricing was done by Best, Burke, and Jotzo (2020). The authors published a cross-country empirical study where they estimated the contribution of carbon pricing to reduce CO_2 emissions in 142 countries over a period of 20 years. In their sample, 43 countries had a carbon price in place at the end of their study period. They find that an additional Euro per ton of CO_2 is associated with a 0.3 percentage points lower emission growth in the subsequent period, while controlling for a lot of different policy variable that could play a role. However, they still compared very different jurisdictions, and a lot of relevant policy variables that could have an influence of the difficulty to implement carbon pricing policies (e. g. support for environmentally protecting policy in the population) were not taken into account due to the lack of data. Also, different abatement costs due to the availability of natural resources (wind or solar energy versus coal or gas deposits), or different economic structures (industrial sector versus service sector) were not taken into account. While their analysis is insightful, they could therefore only capture correlation, and not causation.

To compare different carbon pricing programs, they used the concept of an *effective carbon price*, which was published in OECD 2015 and described in detail in Dolphin, Pollitt, and Newbery 2019. Different pricing regimes have different prices in place in different sectors in the economy. For example, in a lot of jurisdictions that have a carbon price in place there are exemptions for exporting sectors to avoid competitive disadvantages. The effective carbon price is computed from the carbon price and the percentage of the economy that faces the price, which makes it possible to compare those over different jurisdictions.

One quasi experimental study that finds a causal effect of a carbon tax and a value-added tax on transport fuel in Sweden was done by Andersson (2019). The author used the synthetic control method that was introduced by Abadie, Diamond, and Hainmueller (2010) to estimate the causal effect of the Swedish carbon tax. He finds that CO_2 emissions declined almost 11 % per year after the implementation of both taxes, where 6 % is attributed to the carbon tax. According to this study, carbon pricing has a strong effect on CO_2 emissions.

Other studies that investigated different European countries could only find a very small or insignificant effect of carbon taxes on CO_2 emissions. Bruvoll and Larsen (2004) study the effect of the relatively high carbon tax in Norway that is in place since 1991. They find that the reduction in CO_2 emissions was largely due to other reasons and only to a small extent due to the carbon tax. Another study used Difference in Difference estimation to investigate the effect of carbon taxes in northern Europe (Lin and Li 2011). The authors found that the carbon tax in Finland had a negative and significant effect on CO_2 emissions, but the taxes in Denmark, Sweden and the Netherlands were insignificant.

In recent years, a few studies were published that investigate the RGGI program. Murray and Maniloff (2015) did an empirical estimation of the contribution of different factors to the decline in CO_2 emissions in the electricity sector in RGGI states. Their analysis shows that by 2012 about half of the emission reduction is due to the RGGI program. The rest of the effect is due to lower natural gas prices and other environmental policies. Hibbard et al. (2018) write that the RGGI program has not only reduced CO_2 emissions, but also yielded $ 5 billion in economic benefits and tens of thousands of jobs. Abt Associates (2017) analyze the health impacts of the RGGI program and conclude that hundreds of lives were saved and monetary value of $ 5.7 billion in form of health saving and other benefits was generated. Huber (2013) analyzed the auctioning process of the RGGI.

Chapter 3

Methods

3.1 Ordinary Least Squares

One simple way to estimate the effect of carbon pricing on GHG emissions is regression analysis. Using per capita CO_2 emissions as the dependent variable and the price of CO_2 emissions as an explanatory variable yields the following model:

$$Y = \beta_0 + X\beta + \varepsilon$$

where Y is a $n \times 1$ vector of the CO_2 emissions per capita, β_0 is an intercept, X is a $n \times 1$ vector of the price of emitting CO_2 emissions and ε is an error term vector. n is the sample size. Estimating this model yields

$$Y = \hat{\beta}_0 + X\hat{\beta} + e$$

where $\hat{\beta}_0$ and $\hat{\beta}$ are estimators for β_0 and β and e is the residual. $\hat{\beta}$ is estimated with Ordinary Least Squares (OLS), which minimizes the sum of squared differences between the observed and predicted variables.

The simplest regression to estimate is with only one explanatory variable. Including more variables might improve the fit of the model.

Under the Gauss-Markov conditions, the OLS estimator is the most efficient linear unbiased estimator. Those conditions include strict exogeneity. Exogeneity implies that the explanatory variables are uncorrelated with the error term. If this is violated, the estimator is biased. (Wooldridge 2012) p. 351f

3.2 Difference in Differences

A different method that is used in this thesis is *Difference in Differences*. This is a method for policy evaluation that estimates the treatment effect on the treated object(s). It allows to exclude other confunding factors that might affect the variable of interest.

Assume there are n observed units, with the variable of interest x_1 to x_n, over t time periods. There are m treated units and $(m-n)$ non-treated units $(m < n)$. The first m units are treated, so x_1 to x_t are exposed to a certain policy, and the x_{t+1} to x_m units are non exposed to it. The former is called the treatment group, the latter is called the control group. The treatment takes place at time t_0. The goal is to find the causal effect of the treatment, but the treated group can only be observed before the treatment without the treatment (x_1^N to x_m^N before t_0) of after the treatment with the treatment (x_1^T to x_m^T after t_0). The causal effect of the treatment on the first unit would be $x_1^T - x_1^N$ after t_0, but x_1^N is only observable before t_0, not after t_0.

The idea of Difference in Differences is to compare the treated units (x_1^T to x_m^T after t_0) with the non-treated units (x_{m+1}^N to x_n^N after t_0). Taking the Differences between the average treated units and the average non-treated units ($\frac{1}{m}\sum_{i=1}^{m} x_i - \frac{1}{n-m}\sum_{i=m+1}^{n} x_i$) yields an unbiased estimator for the average treatment effect on the treated units, if the parallel trend assumption holds.

The parallel trend assumption is the identifying assumption for the Difference in Differences estimation. It holds if the variable of interest would behave (increase, decrease or remain the same) in the treatment group and the control group after t_0 in the absence of the treatment. Unfortunately, the treatment group is not observable in the absence of the treatment, so this assumption is untestable. If the treatment is not randomly assigned, the treatment group and the control group might differ in some key characteristics and might face different shocks after t_0. If there is reason to believe that such differences are prevalent in the data, the control group might not be the best counterfactual to the control group (Angrist and Pischke 2008) p. 169ff.

3.3 Synthetic control method

The synthetic control method (SCM) was originally proposed by Abadie et al. (2010) and has been applied in many different areas of economic research since. Athey and Imbens (2017) wrote in a much cited sentence that "the synthetic control approach [...] is arguably the most important innovation in the policy evaluation literature in the last 15 years."

The SCM can be interpreted as an expansion of the Difference in Differences estimation method. It relaxes the parallel trend assumption which is the main drawback of the Difference in Differences method because it is violated in many empirical contexts. The main idea is that instead of comparing the treatment group with an average (or a single unit) of a control group, a counterfactual is constructed as a weighted average of the control group. The weights are chosen such that the synthetic counterfactual resembles the characteristics of the treated unit as closely as possible. These characteristics are defined by a set of predictors, which are suited for predicting the variable of interest in the sample. A more convincing counterfactual than the simple average can be constructed like that.

The time period before the treatment is used to decide on the weights and to test if the synthetic unit and the observed data match. When comparing those two, the credibility of the synthetic control method can be checked. If the variable of interest of the treated unit and the synthetic control do not match up reasonably well in the time span before the intervention, the method should not be used with the given data set.

This is also the main drawback of the synthetic control method. The predictor variables together must be able to predict the variable of interest of the treated unit in order for the synthetic control method to be viable. In order to construct a convincing counterfactual, much data is needed to get such predictors that can be used to closely match the treated unit from a weighted average from the control group. These data requirements cannot be met in all contexts. It might also be impossible if the treated unit is too different from the control group to construct a convincing counterfactual. However, since it is easy to check if the identifying assumption holds, this can be applied in many contexts if enough data can be collected.

The predictors should be chosen such that they are able to predict the outcome variable. They should also be unaffected by the treatment. In-time and in-space spillover effects create biases for the estimator.

Also, as Lu (2021) wrote, if the outcome variable follows a non-stationary autoregressive process, the synthetic control estimator would be biased. In this case, Lu suggests using first differences of the outcome variable.

3.3.1 Formalization

The presented model here follows what Abadie, Diamond, and Hainmueller (2010) published in their original paper and what Cunningham (2021) wrote in an article about the synthetic control method.

In the context of this work, we think of treated units as regions. There are $j+1$ observed regions. For simplification, the first of those regions is exposed to the treatment. Let Y_{it}^N be the variable of interest that is observed in region $i = 1, \ldots.., j$ at time $t = 1, \ldots...., T$. At time $T_0 + 1$ intervention is implemented. We assume that there are no in-time and in-space spillover effects, so the outcomes of the untreated regions and the outcomes of the treated unit before the treatment are not affected by the treatment. $Y_{11}, \ldots., Y_{1T_0}$ and any Y_{it} where $i > 1$ are unaffected by the treatment, and $Y_{1T_0+1}, \ldots., Y_{1T}$ are subject to the treatment. These assumptions are discussed in the context of this work in Section 6.

Let Y_{it}^I be the outcome that would be observed for unit i at time t if unit i would be exposed to the treatment (I for intervention). Let $\alpha_{it} = Y_{it}^I - Y_{it}^N$ be the treatment effect for unit i at time t, and let D_{it} be a dummy indicator that is 1 if unit i is exposed to the treatment at time t and 0 otherwise. It is therefore 1 only for unit $i = 1$ at time $t > T_0$ and 0 for all others. The observed outcome for unit i at time t is $Y_{it} = Y_{it}^N + \alpha_{it} D_{it}$.

The estimator for used α_{1t} is

$$\hat{\alpha}_1 t = Y_{1t} - \sum_{i=2}^{j+1} w_i^* Y_{it}$$

where w_i^* is a vector of weights that are optimally chosen to construct the best fitting counterfactual.

W contains the weights: $W = (w_2, ..., w_{j+1})$. W is chosen to minimize $||X_1 - X_0W||$, where X_1 and X_0 are the chosen predictors of the outcome variable after the intervention. All weights are restricted to be non-negative and they sum up to 1. $w_j \geq 0$ and $\sum_{i=2}^{j+1} w_i = 1$.

Abadie, Diamond, and Hainmueller (2010) use

$$||X_1 - X_0W|| = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)}$$

where V is some $(k \times k)$ symmetric and positive semidefinite matrix. They choose a V that minimizes the mean squared prediction error:

$$\sum_{t=1}^{T_0} (Y_{1t} - \sum_{i=2}^{j+1} w_i^*(V)Y_{it})^2$$

Chapter 4

Data

For the empirical estimation of the effect of the RGGI program, the author uses annual panel data from 1990–2019 on CO_2 emissions in the electricity sector for each US state. The data comes from the U.S. Energy Information Administration (2021a).

The data for the price of CO_2 emissions from the RGGI states comes from Regional Greenhouse Gas Initiative (2021a). For each auction, the quantity of emission allowances that were offered and sold and the clearing price is publicized. The auction takes place 4 times a year (March, June, September and December). The other data is only available annually, so the author adjusted the clearing price for the auction to annual data. For that, each December auction gets assigned to the following year and weighted the clearing price from each auction according to the amount of emissions that were sold to compute a properly weighted average annual price on CO_2 emissions. For the CO_2 price in California, data from California Air Resource Board (2021) is used, which is an agency in the government of California that is subordinate to the Californian Environmental Protection Agency. This dataset contains the auction results of the Californian Cap and Trade program. Auctions take place in February, May, August and November. Results that are reported are the total allowances offered, sold, and the clearing price. A weighted average annual price on CO_2 emissions from that data is used.

As one of the predictor variables for the synthetic control method the author uses net electricity imports. Annual data is from U.S. Energy Information Administration (2021c). The net interstate flow of electricity is combined with the electricity trade with Mexico and Canada. The data for annual GDP for each state is from Bureau of Economic Analysis (2021). Unfortunately, this data was only available from 1997 onward. The population data to compute the per capita values is from Federal Reserve Bank of St. Louis (2021). For the computation of the population density, data for the area of each US state is used from U.S. Department of Commerce (2010). The data for the total amount of electricity produced as well as for the contribution of each energy source is from U.S. Energy Information Administration (2021b).

Chapter 5

Results

5.1 Graphical Analysis

First of all, we will have a look at the time trend of the CO_2-emission from the electricity generation sector per capita in different states to see if there is indication that there is a correlation with the introduction of a price on CO_2 emissions.

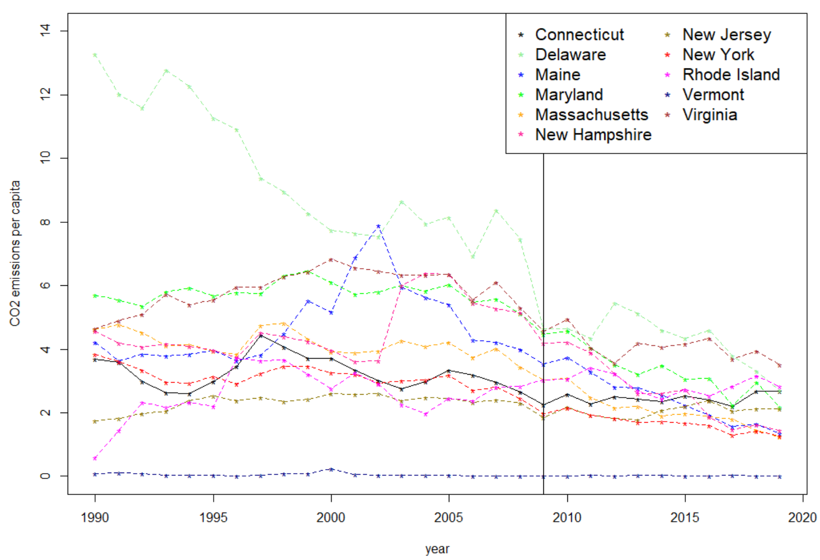


Figure 5.1: CO_2 per capita of RGGI states

Figure 5.1 shows the per capita CO_2 emissions from the electricity generating sector of all states that are part of the RGGI from 1990–2019. The vertical line marks the year 2009, the year in which the cap and therefore a price on CO_2 emissions took effect. For better visibility, Delaware is excluded in the next graph, which has much higher levels of emissions per capita than all the other RGGI states.

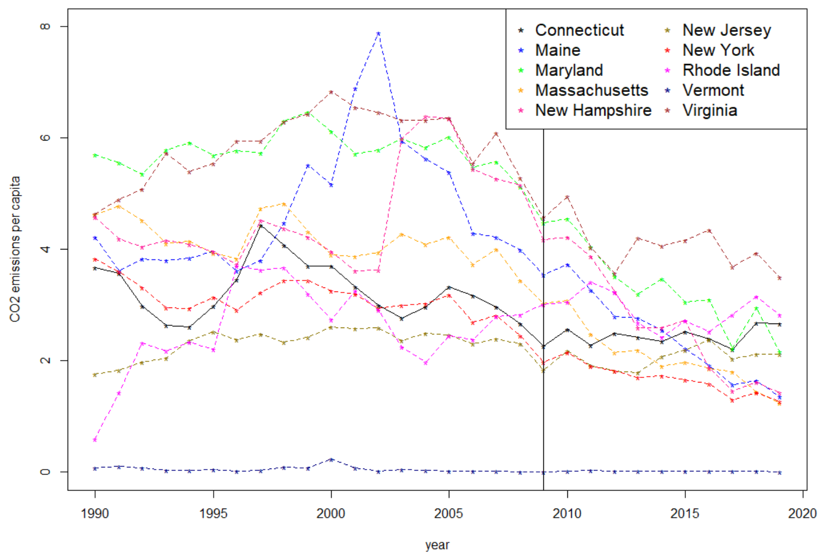


Figure 5.2: CO_2 per capita of RGGI states without Delaware

Figure 5.2 shows a clear downward trend in per capita emissions since the beginning of the century in most RGGI states. This trend does not begin with the introduction of the RGGI, but rather a few years before, and it is also not at the same time in all states. The discussion about implementing a regional C&T program already begun in 2003 (Regional Greenhousegas Initiative 2021), and at the end of 2005 Connecticut, Delaware, Maine, New Hampshire, New Jersey, New York and Vermont agreed on a Memorandum of Understanding to implement the RGGI. Therefore, it was commonly known that there will be a cap and subsequently a price on CO_2 emissions from the electricity sector several years before the actual implementation. It is therefore reasonable to believe that forward looking agents in the economies reacted to the C&T in advance to avoid paying higher prices once it is implemented since adjusting the electricity portfolio takes some time.

A different possibility is that there was some common shock that caused the CO_2 emissions from electricity production to decline across states.

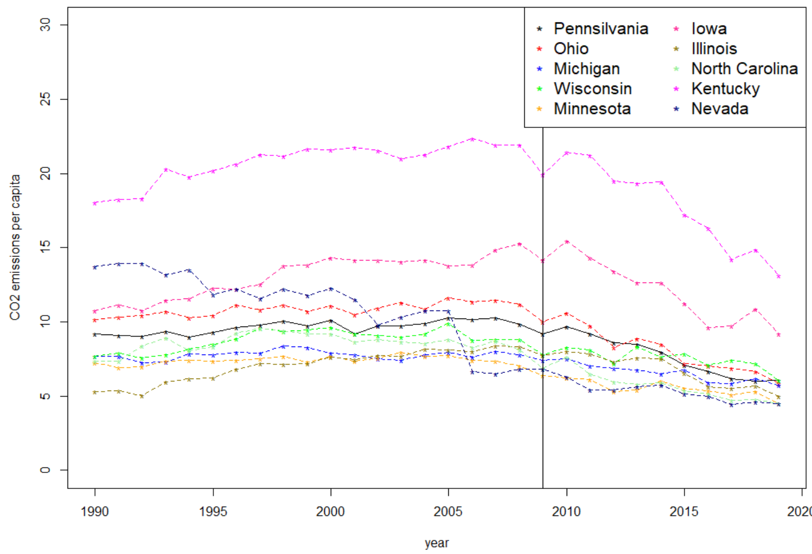


Figure 5.3: CO_2 per capita of some non-RGGI states

Figure 5.3 shows the same plot for a selection of other US states in the north and east that are not under and C&T for comparison. In this sample, we can also see a downward trend since the beginning of the 2000s, but it seems to be smaller than what we saw before from the RGGI states. This supports the idea that there is some downward effect from the carbon price on CO_2 emissions.

5.2 Empirical Estimation

In this section different empirical methods are used to formally analyze the effect from the RGGI C&T on CO_2 emissions.

5.2.1 Ordinary Least Squares

The OLS model

$$Y = \hat{\beta}_0 + X\hat{\beta} + e$$

is estimated, where the variable of interest Y is the CO_2 emissions per capita from the electricity sector and X is a vector of the control variables. Model 1 in table 5.1 uses only the price of CO_2 emissions in the energy sector as a control variable. Model 2 uses uses data on GDP per capita, the total electricity production and the net electricity imports as additional control variables.

In table 5.1 we can see the regression results. Model 1 associates a decrease of 1.571 tons of CO_2 emissions per capita with an additional dollar per ton of

	Dependent variable:	
	CO_2 emissions	
	(1)	(2)
CO_2 price	-1.571^{***}	-0.466^{**}
	(0.242)	(0.236)
GDP per capita		-0.00000^{*}
		(0.00000)
total electricity production		-0.000
		(0.000)
Net electricity import		-0.024^{***}
		(0.002)
Constant	11.443^{***}	11.872^{***}
	(0.358)	(0.576)
Observations	1,530	1,172
R^2	0.027	0.197
Adjusted R^2	0.026	0.195
Residual Std. Error	13.553 (df = 1528)	12.228 (df = 1167)
F Statistic	42.056^{***} (df = 1; 1528)	71.745^{***} (df = 4; 1167)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 5.1: OLS estimation

CO_2 emissions in the electricity sector. This is significant at the 1 % level. This would mean a decrease of 8.42 tons of CO_2 emissions per capita in the year 2019 in RGGI states. This corresponds to a reduction of more than 50 million tons of CO_2 in total in the state of Maryland alone, which would be very high since the electricity sector of Maryland in total emitted about 13 million tons of CO_2 in the year 2019. However, we can see that the R^2 is rather low, which means that only 2.6 % of the variation in the data can be explained by the model. This was to be expected however, because large variations of the CO_2 price come from other factors, as many states are not subject to a CO_2 price at all and they still have a very different electricity generating portfolio and therefore very different CO_2 emissions per capita.

The second model associates a decrease of -0.47 tons of CO_2 emissions per capita with an additional dollar per ton of CO_2 emissions in the electricity sector. This is significant at the 5 % level. The introduction of the control variables reduced the effect because some part of the reduction in the CO_2 price comes from the controlling variables. The R^2 is higher, model 2 can explain about almost 20 % of the total variation in the data.

5.2.2 Difference in Differences

The Difference in Differences method is used to estimate the effect of the RGGI on CO_2 emissions in the electricity sector. The treatment group are all RGGI states, and the control group are all other US states except California. California is excluded because it is part of a different C&T that was implemented at a different time and can therefore not be included in the same Difference in Differences estimation. The average of the treatment group is compared to the average of the control group. As the dependent variable, CO2 emissions per capita is used.

Table 5.2 shows in column 1 the results of the Difference in Difference estimation with all RGGI states as treatment group and all states that are not subject to a C&T as control, with 2009 as the treatment year. The Difference in Difference estimation does not yield a significant effect from the RGGI on CO_2 emissions. The effect estimated is actually positive, which goes against what was to be expected, since the RGGI put a cap on CO_2 emissions from the electricity sector which should have reduced CO_2 emissions. It might be the case that a part of the effect already started before 2009, as was discussed in the graphical analysis, because agents are forward-looking and may have adapted to the RGGI before its actual implementation. If the dummy-variable time, which indicates when the treatment came into effect, is moved to an earlier time-point, the interaction term becomes smaller and eventually becomes negative if the year 2003 is used. But this was even before the first commitment was signed, and the effect is still insignificant. Column 2 shows the results from the estimation when 2005 is used as an intervention year, which is the year in which the first agreement was signed to implement the RGGI.

Another Difference in Differences estimation can be done by using only one treated RGGI state and the states that are not subject to a C&T as controls.

Table 5.3 column 1 shows the results of the Difference in Differences estima-

	Dependent variable:	
	CO_2pc	
	(1)	(2)
treated	-9.351^{***}	-9.220^{***}
	(1.077)	(1.214)
time	-2.261^{***}	-1.520^{**}
	(0.796)	(0.768)
treated:time	0.434	0.057
	(1.779)	(1.717)
Constant	13.703^{***}	13.634^{***}
	(0.482)	(0.543)
Observations	1,500	1,500
R^2	0.077	0.074
Adjusted R^2	0.075	0.072
Residual Std. Error (df = 1496)	13.280	13.300
F Statistic (df = 3; 1496)	41.461^{***}	39.823^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 5.2: Difference in Difference estimation

	Dependent variable:	
	CO_2pc	
	(1)	(2)
treated	-8.200^{**}	-8.067^{**}
	(3.420)	(3.855)
time	-2.327^{***}	-1.566^{*}
	(0.893)	(0.862)
treated:time	-0.120	-0.355
	(5.648)	(5.452)
Constant	13.988^{***}	13.918^{***}
	(0.541)	(0.610)
Observations	1,200	1,200
R^2	0.013	0.010
Adjusted R^2	0.011	0.008
Residual Std. Error (df = 1196)	14.721	14.743
F Statistic (df = 3; 1196)	5.385^{***}	4.191^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 5.3: Difference in Difference estimation Maryland

tion with only Maryland as a treated state and all states that are not subject to an C&T as controls. The estimator is negative but insignificant. If the intervention is moved to an earlier time, the estimator becomes smaller, so the negative effect becomes stronger. However, it still stays insignificant. Column 2 shows the results for a intervention year of 2005.

	Dependent variable:	
	CO_2pc	
	(1)	(2)
treated	-8.074^{**}	-8.067^{**}
	(3.515)	(3.855)
time	-2.127^{**}	-1.566^{*}
	(0.879)	(0.862)
treated:time	0.406	-0.355
	(5.558)	(5.452)
Constant	13.986^{***}	13.918^{***}
	(0.556)	(0.610)
Observations	1,200	1,200
R^2	0.012	0.010
Adjusted R^2	0.009	0.008
Residual Std. Error (df = 1196)	14.726	14.743
F Statistic (df = 3; 1196)	4.800^{***}	4.191^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 5.4: Difference in Difference estimation Virginia

Table 5.4 column 1 shows the results of the Difference in Difference estimation with only Virginia as a treated state and all states that are not subject to a C&T as controls. The estimator is positive and insignificant. If the intervention is moved to an earlier time, the estimator becomes negative. However, it still stays insignificant. Column 2 shows the results for a intervention year of 2005.

5.2.3 Synthetic Control Method

The synthetic control method is used to construct a counterfactual that is more convincing than the unweighted average of all the control states. The constructed counterfacual is called the *synthetic state*. The method is performed on each state that is subject to a C&T with all other US states that are not subject to a C&T as potential donors. The variable of interest is CO_2 emissions per capita from the electricity sector. The predictors used are:

- CO_2 emissions from the electricity sector
- net electricity imports
- GDP per capita (only available from 1997–2019)
- total electricity production
- population density
- the percentage of electricity that is produced from each source:
  - coal
  - natural gas
  - petroleum
  - nuclear power
  - wind
  - solar
  - hydroelectric power
  - geothermal
  - biomass

For most RGGI states, the synthetic control method is unable to reproduce the pre-intervention values of the CO_2 emissions per capita. The states are so different from the control states that even the optimal weights (given the restriction from chapter 3) fail to construct a convincing counterfactual. The state with the best fit is Maryland. As an indicator for best fit the sum of the absolute values of the gaps between the synthetic Maryland and the observed data until the intervention is computed, which is 2.78.

Maryland

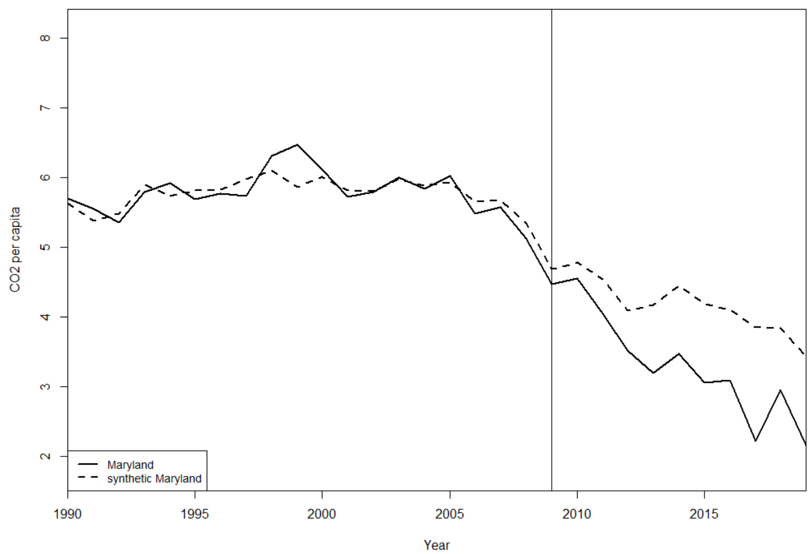


Figure 5.4: SCM Maryland

Figure 5.4 shows the time trend of CO_2 emissions from the electricity sector per capita from 1990–2019. The vertical line represents the observed data, and the dotted line shows the synthetic Maryland that is constructed by the weighted average of the donor states. The vertical line indicates the year of intervention, 2009. As we can see, the lines are reasonably close to each other most of the time until 2009, which is the time the RGGI came into effect. The only pre-intervention years with a larger divergence between the synthetic Maryland and the observed data is 1998 and 1999, where there was an unusually large spike in electricity production from petroleum power plants. The amount of electricity production and subsequently CO_2 emissions from those was doubled from 1997 to 1998 and increased even more in 1999, before it went down again. Still, the synthetic Maryland is able to track the path of CO_2 emissions from the observed data from Maryland reasonably well until the treatment, which indicates that it is a more convincing counterfactual than the average of the donor pool, which was used for the Difference in Differences method. It is also easy to see that the CO_2 emissions from the electricity sector would have decreased even in the absence of the RGGI program, but not as much as they actually did.

The effect of the introduction of the RGGI on CO_2 emissions from the electricity sector is the gap between the synthetic path and the actual path. Figure 5.5 plots those gaps.

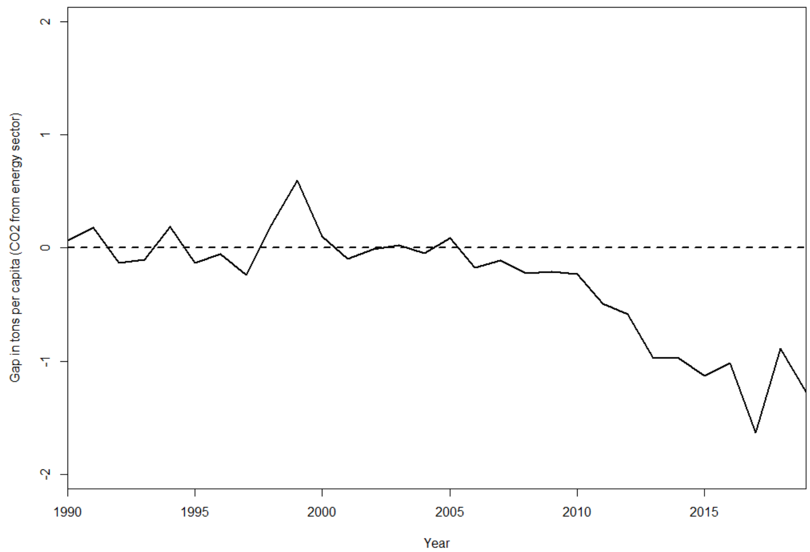


Figure 5.5: SCM Maryland gaps

There are only small divergences from the gap until 2009. Then, the gap increases over time until 2019, with a small outlier in 2018. In 2019 the gap was -1.27, which means that due to the introduction of the RGGI program, the emissions from the electricity sector were 1.27 tons of CO_2 per capita lower compared to the scenario without the RGGI.

The weights assigned to the different states from the donor pool are zero for most states, except for District of Columbia, Florida, Idaho, Minnesota, Mississippi, Nebraska, North Carolina and Tennessee. All weights add up to 1 due to a restriction imposed in chapter 3. They are reported in Table 5.5.

state	weights
District of Columbia	0.054
Florida	0.077
Idaho	0.248
Minnesota	0.474
Mississippi	0.005
Nebraska	0.001
North Carolina	0.026
Tennessee	0.115

Table 5.5: Weights synthetic Maryland

Virginia

For Virginia, the synthetic control method constructs a counterfactual that is somewhat close to the observed data of CO_2 emissions per capita. The sum of the absolute values of the gaps between the synthetic Virginia and the observed data until the intervention is 4.31.

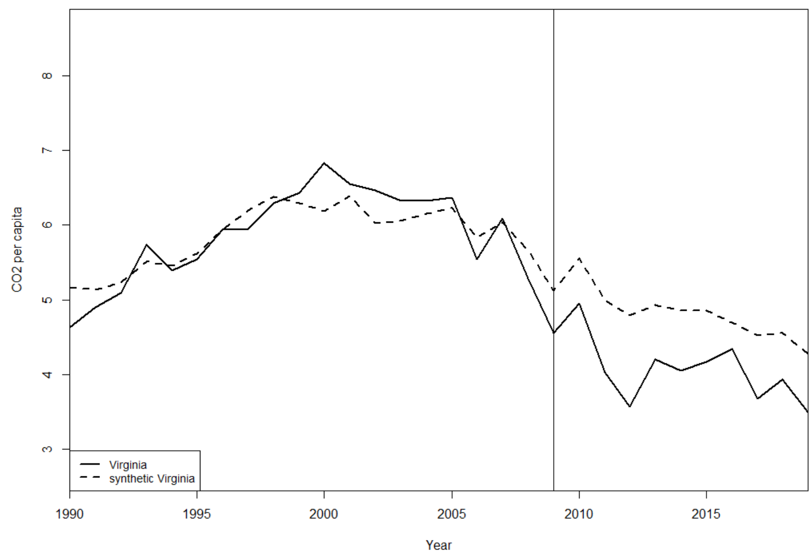


Figure 5.6: SCM Virginia

Figure 5.6 shows the time trend of CO_2 emissions from the electricity sector per capita from 1990–2019 for Virginia. The fit is not as good as with Maryland, but is is still close enough to be worth analyzing.

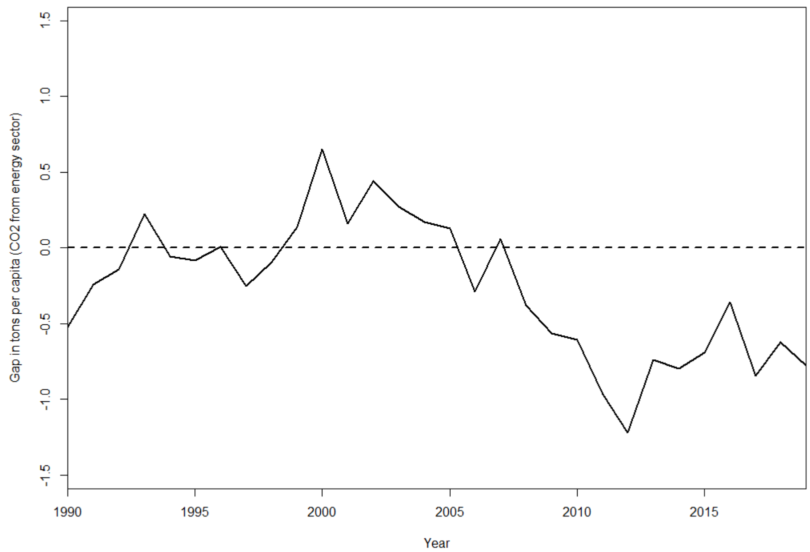


Figure 5.7: SCM Virginia gaps

Figure 5.7 shows the gaps between the synthetic Virginia and the observed data, which is the effect of the RGGI program on CO_2 emissions. The gap in 2019 was -0.78, which means that in 2019, the electricity sector of Virginia emitted 0.78 tons CO_2 per capita less than what would have been emitted in absence of the RGGI program.

The non-zero weights assigned to the different states to construct the synthetic Virginia are reported in table 5.6.

state	weights
Florida	0.255
Hawaii	0.049
Idaho	0.288
Mississippi	0.109
North Carolina	0.156
Wisconsin	0.142

Table 5.6: Weights synthetic Virginia

Chapter 6

Discussion

6.1 Carbon Leakage

The introduction of a tax on a good in a region might have unintended consequences. In the case of a price on CO_2 emissions in the electricity sector, these consequences could be a shift from producing electricity locally to importing it from another area, where there is no price on CO_2. If this is the case, we would need to interpret the results accordingly. The intended effect of a policy like the RGGI is a shift from CO_2 intensive energy production (e. g. fossil fuels like coal) to a more environmentally friendly production technology (renewable energies). Also, a shift from coal to gas could be expected and intended, since producing electricity from gas emits less CO_2 than from coal. An unintended effect would be if, instead of producing more environmentally friendly energy, electricity would be imported from other places where there is no (or a lower) price on CO_2 emissions to avoid the cost of the tax. This unintended effect from climate policy in general is called the *Carbon Leakage effect* and can be applied to different industries that are regulated to some extent (Babiker 2005). An analysis of this issue regarding the RGGI can be found in Chen (2009).

In case of electricity production, this should not be as big of an issue as in other industries, since transporting electricity over longer distances leads to losses, so it can be regarded as more costly than importing other goods. Transporting electricity over larger distances causes transmission costs in the form of necessary infrastructure and loss in electricity over distance. These transmission costs depend on the infrastructure and technology that is used (Csanyi 2014). The main concern in the context of this study regarding this carbon leakage effect is therefore electricity imports from neighboring states.

To test if this is an issue in the case of the empirical estimation of the effect of the RGGI program on CO_2 emissions, the author performs correlation tests between the price on CO_2 and the amount of electricity that is traded between states and with Canada and Mexico. The overall correlation coefficient over all states is 0.2669 and is significant at the 1 % level. The correlation of only the states that are subject to a CO_2 price is 0.1906 and is significant on the 1 % level. The correlation of only Maryland is 0.5974 and is significant on the 1 % level. The correlation of only Virginia is 0.1022 and is not significant. This means there is significant correlation between the price of CO_2 and the imports and should be further investigated.

When looking at Maryland's electricity production the data shows that it decreased electricity production from 2009–2019 from 43.7 million Megawatt-hours (MWh) to 39.3 million MWh. The net-imports of Maryland however stayed almost the same (+257.3 British Thermal units (Btu)[1] in 2009, +253.8 Btu in 2019). It is therefore unlikely that Maryland decreased electricity production and increased imports from states that are not subject to a price on CO_2 emissions. Maryland's neighbor state that it shares the largest border, Pennsylvania, also had stable net-imports (-658.8 Btu in 2009 and -669.1 Btu in 2019). The second state that shares a border that is not part of the RGGI is Washington D.C., which also had stable net-imports (128.7 and 114.1 Btu). The last state that Maryland shares a border that does not have a price on CO_2 emissions, West Virginia, even increased their net-imports (from -398.1 to -291.5 Btu). This does not support the carbon leakage hypothesis, the correlation reported seems to be caused by other factors. Nevertheless, this is not a thorough analysis and part of the measured effect from the RGGI might be due to carbon leakage.

6.2 OLS Estimation

The OLS estimation of Maryland reported a very strong association of a price of CO_2 on CO_2 emissions. While this is evidence that there is a negative effect of the RGGI program on CO_2 emissions, the magnitude of the effect reported by the OLS estimation should be interpreted with care.

One potential issue might be selection bias: The states chose to be part of the RGGI program or not. The states might have chosen to participate based on their electricity production portfolio. The more dependent their electricity production is on CO_2 intensive fossil fuels (like coal and petroleum) and the less renewable energy the states produce, the more costly it is for the state to participate in the RGGI. Also, the availability of natural resources that allows for using renewable energy (e. g. coast for efficient offshore wind power plants and tidal power, rivers for hydroelectric power plants) or for using of fossil fuels (natural gas or oil reserves) affects the costs associated with participating in the RGGI program. If it is cheap to produce electricity in a state from renewable sources, or from producing electricity from natural gas which emits less CO_2 than e. g. coal power plants, the costs of participating is lower and it is more likely that they participate in the RGGI. If that is the case, the effect of the RGGI program on other states would be lower because it would require a lower cap and subsequently a higher price on CO_2 emissions for firms to switch to

[1] A British thermal unit (Btu) is a measure of the heat content of fuels or energy sources (U.S. Energy Information Administration 2021d).

a less CO_2 intensive electricity production technology. Also, other additional policies that encourage the use of renewable energy production plants (e. g. subsidies for solar panels) makes it cheaper for a state's economy to participate in the RGGI program. The RGGI states have some common features that give reason to believe that such a selection bias is an issue here: All of them are located in the northeast of the USA, which gives them similar geographic prerequisites. All of them except Vermont have access to the Atlantic ocean. To analyze the effect of other environmental policies of the states, it is useful to compare the political landscape of the states. In all of the RGGI state's a majority of the population have voted for the Democratic candidate in the last 4 presidential elections, and all of them except Maryland, Massachusetts and New Hampshire have Democratic governors, which makes it more likely that there are additional environmental policies in place. All of these similarities makes it likely that there is some kind of selection bias which means that the OLS estimation results should not be interpreted as the true effect.

6.3 Difference in Difference Estimation

The Difference in Difference estimation did not yield significant results for all states and for the specification with only Maryland and only Virginia as the treated units. The identifying assumption is the parallel trend assumption, which means that the treated units would have behaved the same as the average of the control group in the absence of the treatment (World Bank 2013). To check if this assumption holds, we can have a look at the time trend of the control states and of Maryland and Virginia.

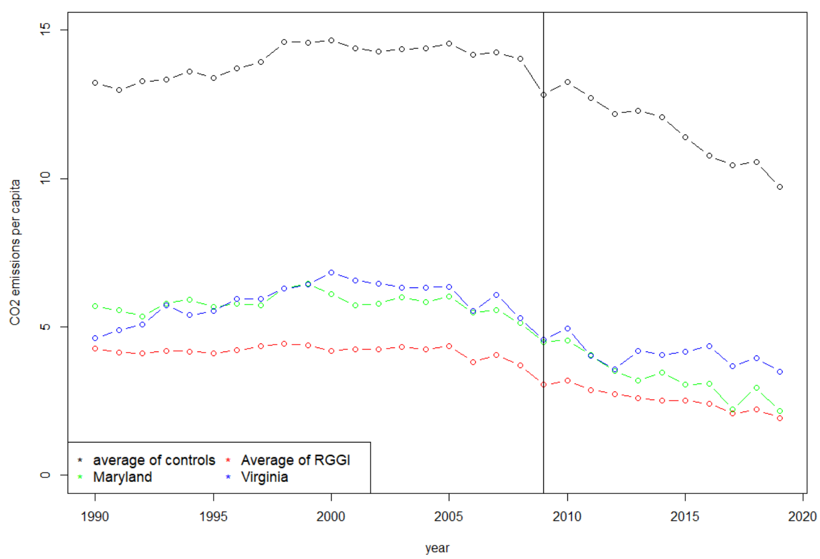


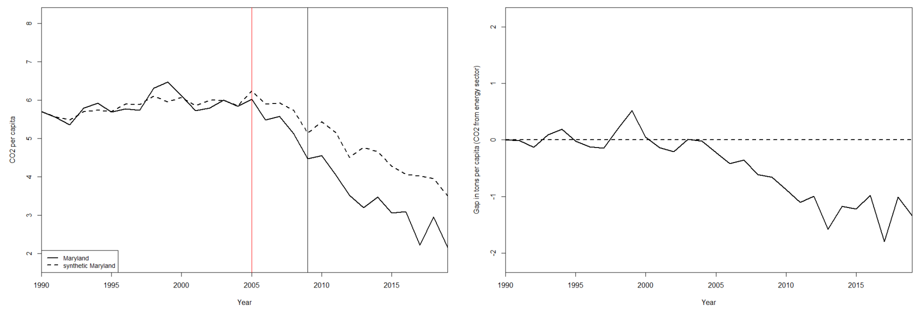
Figure 6.1: Difference in Difference parallel trend assumption

Figure 6.1 shows the trend of CO_2 emissions per capita from electricity production. It compares the average of all control states with the average of all RGGI states and Maryland and Virginia. It is easy to see that the average CO_2 emissions per capita of all control variables is a lot higher than the other plots. Also, the trends are not moving parallel. This is strong evidence that the parallel trend assumption is violated and that the Difference in Difference estimation results should not be interpreted as the true effect.

6.4 Synthetic Control Method

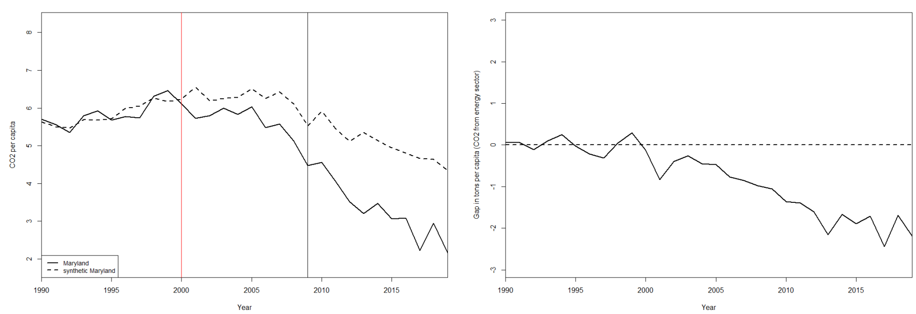
As mentioned in section 3.3, it is necessary that the variable of interest is stationary for the Synthetic Control Method to return an unbiased estimator. To test this, the author performed an augmented Dickey Fuller test (Cheung and Lai 1995). The p-value is below 0.01, so the Null of a Unit root can be rejected, which is evidence in favor of stationarity.

For the state of Maryland, there was evidence for a significant negative effect from the RGGI program on CO_2 emission per capita in the electricity sector. To test the validity of this estimation the author performed the following placebo test, following what Andersson (2019) did in his analysis of the Swedish carbon tax: "in-time", "in-space" and leave-one-out".



(a) time-trend (b) gaps

Figure 6.2: in-time shift 2005 Maryland



(a) time-trend (b) gaps

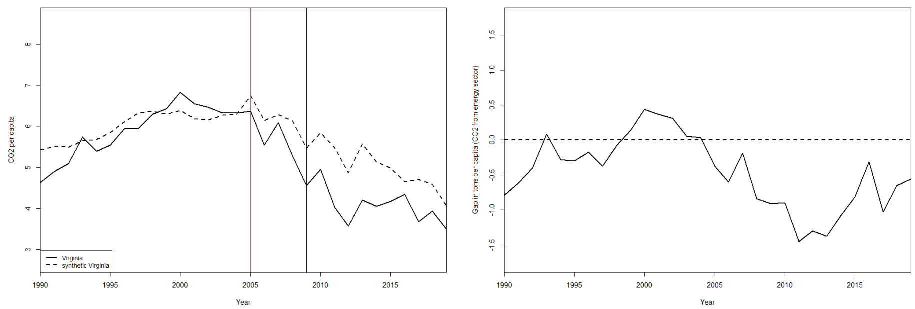
Figure 6.3: in-time shift 2000 Maryland

6.4.1 In-time Shift

For the in-time test, the author shifted the year of the treatment to check if there are some other factors from earlier periods that might have caused the estimated effect. Also, there might be some anticipation effects from before 2009 that can be accounted for in an in-time-shift test. In-time tests were performed for an intervention period in 2005 and in 2000.

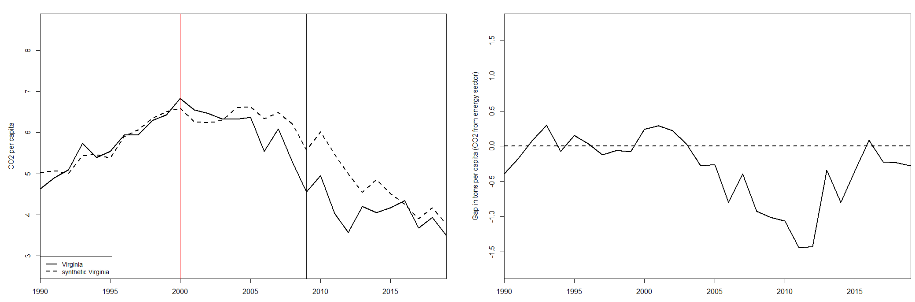
Figure 6.2 shows the time trend and the gaps of synthetic Maryland with a shifted intervention in 2005. The red vertical line depicts the time of the intervention that was chosen to construct the plots. Figure 6.3 shows the same for a shifted intervention in 2000. The effect of the RGGI increases with the in-time shift moving more into the past, which indicates that some of the effect of the RGGI was happening even before the intervention in 2009.

Figures 6.4 and 6.5 show the same graphs for Virginia. Here, the effect of the intervention decreased when moved further into the past, but the negative



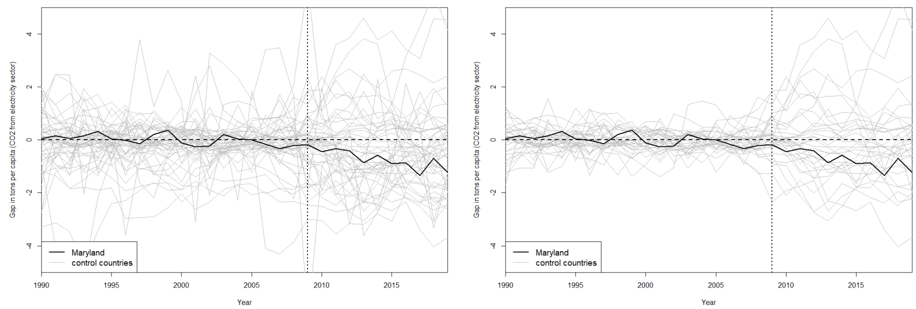
(a) time-trend (b) gaps

Figure 6.4: in-time shift 2005 Virginia



(a) time-trend (b) gaps

Figure 6.5: in-time shift 2000 Virginia



(a) all states (b) high mspn excluded

Figure 6.6: in-space placebo test gaps

effect of the RGGI is still captured. The magnitude of the effect changes quite a bit due to the time-shift, so the true causal effect of the RGGI program on CO_2 emissions in the electricity sector in Virginia might be lower than what the estimation from the synthetic control method indicates.

6.4.2 In-space Shift

The in-space placebo test is performed by iteratively treating every state in the control group with the treatment and use the synthetic control method to construct synthetic counterfactuals. With this test, the magnitude of the treatment effect on the treated state can be compared to the potential magnitude of the effect on other states. This is to check if the effect estimated on the treated unit is larger than on other states.

Figure 6.6 shows the results of the in-space placebo test. The black line represents the gaps of Maryland, the light gray lines are the gaps from the other states. Figure 6.6a shows the gaps of all states. Not all states' paths can be tracked well by a synthetic control. In Figure 6.6b the states with a pretreatment mspe (mean squared prediction error) that is larger than 20 times the mspe of Maryland are excluded. 17 of the 50 control states are excluded, which leaves 33.

We can see that the effect of the RGGI on CO_2 emissions in Maryland is larger than it would be in most states, but it is not the largest. By far the largest negative gap would have a synthetic Kansas with -3.67, followed by Tennessee and South Carolina with -2.23 and -2.22.

6.4.3 Leave-one-out

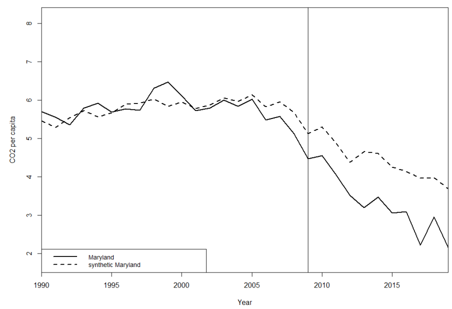
The leave-one-out placebo test is performed by iteratively leaving one state from the donor pool out to check if the results are driven by a single control unit. The donor states are District of Columbia, Florida, Idaho, Minnesota, Mississippi, Nebraska, North Carolina and Tennessee, so there are 8 sample variations where each sample left one donor state out of the potential donor pool.

Figures 6.7 and 6.8 show the time trends and the gaps of all sample variations of Maryland.

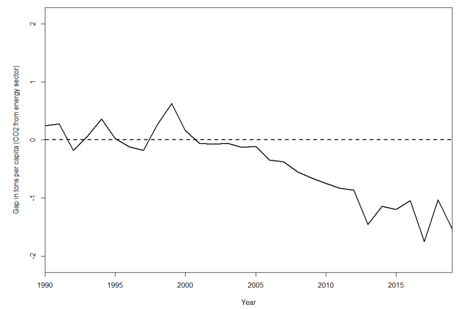
sample variation	gaps in 2019	cumulated gap
without DC	-1.53	4.21
without Florida	-1.25	3.24
without Idaho	-1.17	2.50
without Minnesota	-1.24	2.55
without Mississippi	-1.17	2.34
without Nebraska	-1.06	2.78
without North Carolina	-1.25	2.61
without Tennessee	-1.49	3.33

Table 6.1: leave-1-out sample variations gaps Maryland

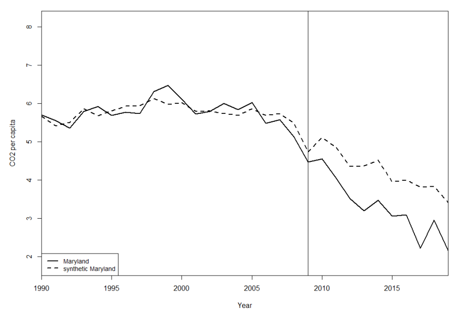
Table 6.1 reports in the third column the sum of the absolute values of the gaps between the synthetic Maryland without the respective state in the donor pool and the observed data until the intervention as an indicator of how well the synthetic Maryland fits the data. While some are better than others all are reasonably low. In the second column it reports the gap in the year 2019, which is the effect of the RGGI program on the CO_2 emissions per capita as an indicator of how much the sample variation changes the result compared with the result with the full potential donor pool which was -1.27. All of them are close to the result which indicates that no single donor state drives the results.



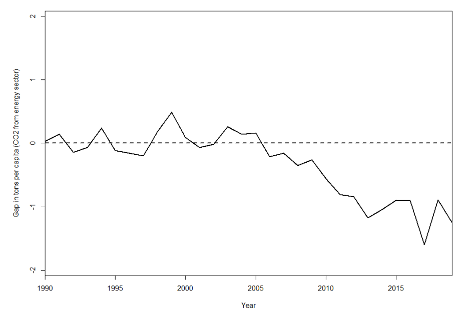
(a) without District of Columbia



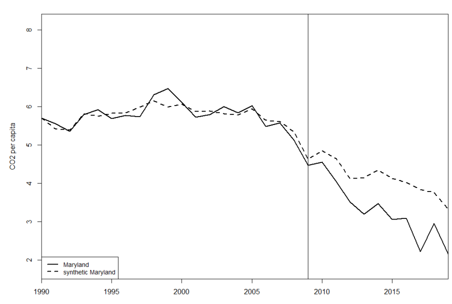
(b) without District of Columbia gaps



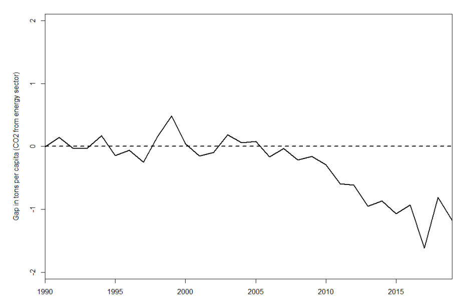
(c) without Florida



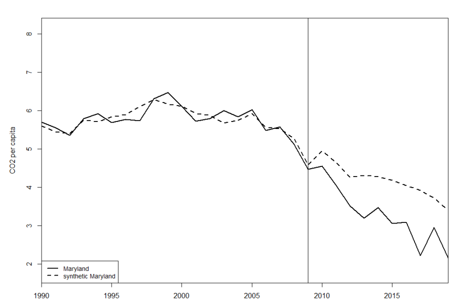
(d) without Florida gaps



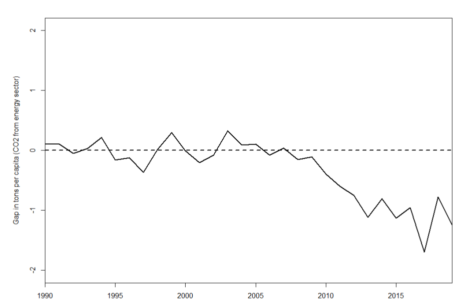
(e) without Idaho



(f) without Idaho gaps

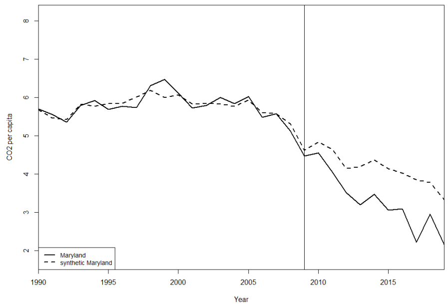


(g) without Minnesota

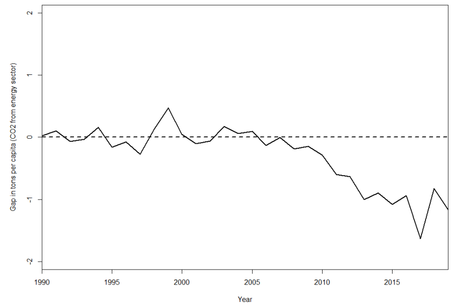


(h) without Minnesota gaps

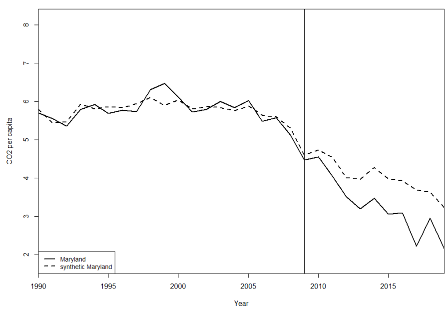
Figure 6.7: leave-1-out sample variations Maryland 1



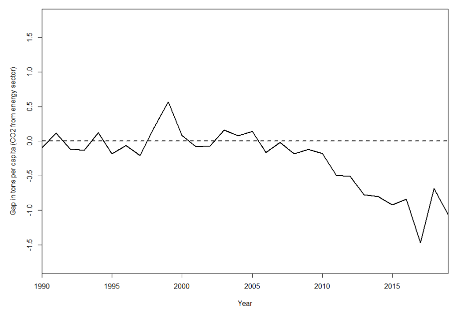
(a) without Mississippi



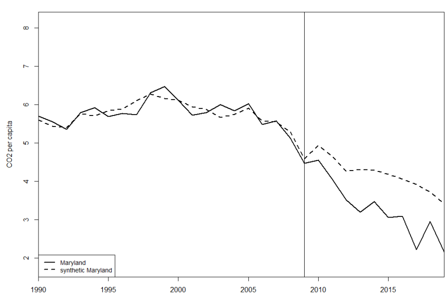
(b) without Mississippi gaps



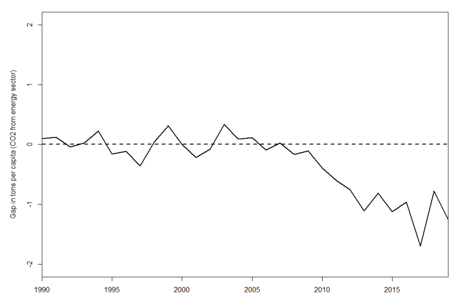
(c) without Nebraska



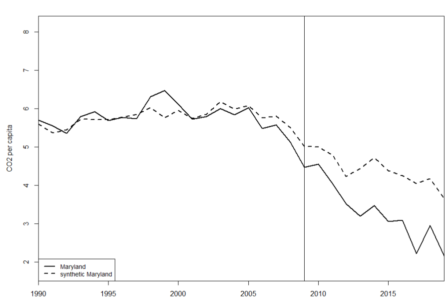
(d) without Nebraska gaps



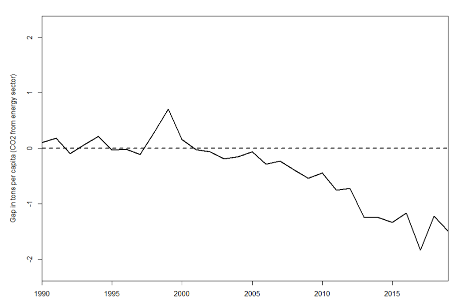
(e) without North Carolina



(f) without North Carolina gaps



(g) without Tennessee



(h) without Tennessee gaps

Figure 6.8: leave-1-out sample variations Maryland 2

Chapter 7

Conclusion

The Regional Greenhouse Gas Initiative is a Cap and Trade program that sets an upper limit on CO_2 emissions in the electricity sector by auctioning allowances and therefore makes emissions costly. This higher price gives an incentive for economic agents to reduce CO_2 emissions. The OLS regression analysis shows evidence for a negative effect, but should not be interpreted as causal due to the expected selection bias. The Difference in Differences estimation does not show a significant effect of the RGGI program on CO_2 emissions, but the parallel trend assumption is violated in this context. With the synthetic control method the author estimates a causal negative effect from the RGGI program on CO_2 emissions in the electricity sector in the state of Maryland, which is robust to a number of placebo tests. The results for the electricity sector in Virginia are similar to those in Maryland, but less robust. Carbon leakage might cause some of the estimated effect in both states.

Carbon pricing can reduce emissions, but the magnitude depends on the context and is difficult to measure. The synthetic control method makes it possible to analyse this issue in different contexts. Recent publications have introduced new modifications of the synthetic control method that might improve estimation results and reduce biases (Ben-Michael, Feller, and Rothstein 2021). To further investigate the effect of carbon pricing on emissions and improve results, these new methods can be applied to the RGGI and other carbon pricing regimes.

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