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Trading Carbon for Change: A Causal Analysis of Cap-and-Trade in the Northeastern United States

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Master's Thesis

April 8, 2025

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Title: Trading Carbon for Change: A Causal Analysis of Cap-and-Trade in the Northeastern United States

Level: Master's thesis

Month and year: 4/2025

Number of pages: 57 (41+7+9)

Supervisor: Matti Mitrunen

Keywords: carbon leakage, emission trading schemes, fuel switching, placebo test, synthetic control method

Abstract:

The world is under increasing pressure to develop mechanisms that reduce emissions while maintaining a robust economy required by modern societies. Different approaches have been proposed, yet consensus on the most effective one remains elusive. This thesis examines the effectiveness of a cap-and-trade program as an environmental regulatory approach. Specifically, I analyze the impact of Regional Greenhouse Gas Initiative on carbon emissions in the electric power sector. I use state-level panel data covering the period 1997–2018 and apply the synthetic control method to construct an optimal counterfactual for the treatment region. To account for cross-regional leakage and fuel switching in participating and non-participating states, I extend the method in secondary analyses. Based on my results, the program reduces emissions in the electric power sector by 31.82% relative to the counterfactual, while having no measurable impact on the economy. I find evidence of more pronounced reductions in carbon-intensive fuels, while emissions from lower-carbon alternatives increase, particularly in non-participating neighboring states. I conclude that local environmental regulation can be successful without hindering economic growth, and I contribute to the literature by providing causal inference on the diverse effects of a cap-and-trade program.

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1 Introduction

Balancing economic growth with ecological limits is a widely recognized challenge in economics. This challenge is intensified by the persistent reliance on fossil fuels and polluting energy systems, alongside the growing scarcity of natural resources as global demand continues to grow (Acemoglu, Akcigit, Hanley, & Kerr, 2016; Azomahou, Laisney, & Van, 2006; W. D. Nordhaus, 1977; Stott, Stone, & Allen, 2004). Large-scale, international climate agreements often face challenges in enforcement and measurable impact (Barrett, 2003; Bénabou & Tirole, 2010; Pizer, 2006), while financial theory indicates that sustainable investing is suboptimal as restricting investment options imposes diversification constraints (Markowitz, 1952). Yet, the proposed solutions rely heavily on different economic approaches.[1] Regardless of the chosen approach, the costs of climate change are evident if no action is taken (Bilal & Känzig, 2024; Stern, 2006; Weitzman, 2009). This thesis focuses on regulation as a proposed solution, examining the implementation of a cap-and-trade program in the Northeastern U.S.

W. Nordhaus (2019) presents a theoretical visualization (Figure 1) of the highly recognized Dynamic Integrated Model of Climate and the Economy (aka the DICE model)[2] which this thesis complements by providing empirical evidence on the effects of carbon trading. This is particularly relevant for understanding the impacts of real-world climate change policies addressed in the figure. I assume that the arrow from ecological and economic impacts to climate change policies hold, and aim to empirically demonstrate that policies have a positive impact on emission reductions.

[1] These include climate policies and regulation (W. Nordhaus, 2019; Pizer, 2002; Stern, 2006), technological innovation (Acemoglu, Aghion, Bursztyn, & Hemous, 2012; Popp, 2002), corporate action and green finance (Krueger, Sautner, & Starks, 2020; Matsumura, Prakash, & Vera-Muñoz, 2014), geoengineering (Barrett, 2008; W. Nordhaus, 2015), and behavioral change (Allcott & Kessler, 2019; Dietz, Gardner, Gilligan, Stern, & Vandenbergh, 2009).

[2] DICE is a type of Integrated Asset Model (IAM), designed to calculate the optimal path of emissions reductions by weighing the costs of mitigation against the benefits of avoided climate damages. IAMs are tools that combine knowledge from climate science, economics, and energy systems to evaluate the interactions between the economy and the climate (W. D. Nordhaus, 1991; Pearce, 2003).

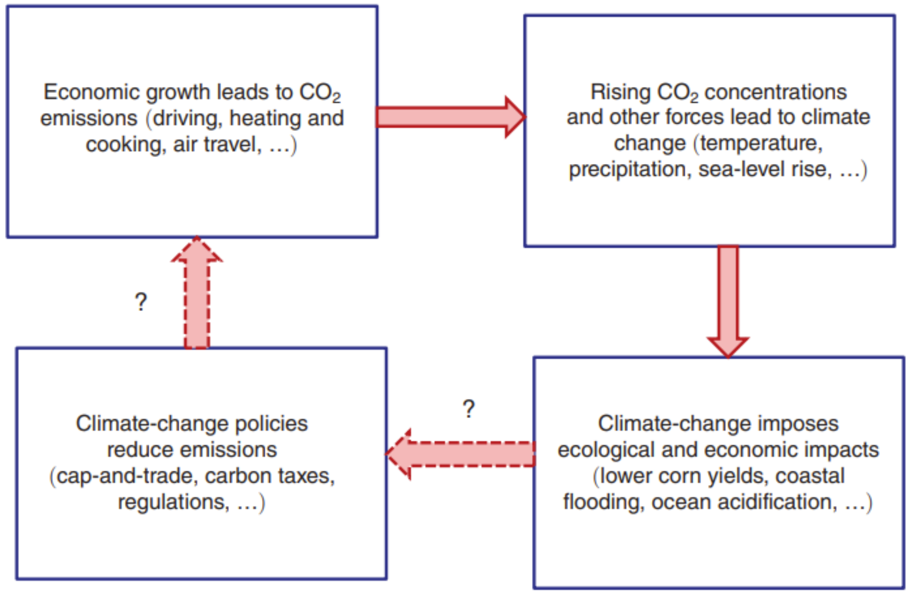

Figure 1: The Circular Flow of Global Warming Science, Impacts, and Policy (W. Nordhaus, 2019, p. 5)

Previous research suggest that carbon pricing, such as carbon taxes or Emission Trading Schemes (ETSs)[3], represent core instruments in climate policy aimed at internalizing the external costs of greenhouse gas (GHG) emissions (W. Nordhaus, 2019; Pearce, 2003). Despite their theoretical appeal, the practical effectiveness of ETSs is debated. Empirical research indicates that while such systems can achieve emission reductions, outcomes often depend on complementary policies as well as leakage effects and over-allocation of permits (Bartram, Hou, & Kim, 2022; Bayer & Aklin, 2020; Bushnell & Chen, 2009; Bushnell, Peterman, & Wolfram, 2007; Caron, Rausch, & Winchester, 2015; Chen, Liu, & Hobbs, 2011; Ellerman & Buchner, 2008; Kim & Kim, 2016; Lessmann & Kramer, 2024; Yan, 2021). Hassler, Krusell, and Olovsson (2024) argue that future research should prioritize understanding how different policies affect the economy in practice, rather than focusing solely on theoretically optimal solutions.

Previous studies on ETSs often face challenges in establishing credible causal research ap-

[3]ETS is a broader term referring to any market-based mechanism that enables the trading of emission allowances, whereas a cap-and-trade system specifically refers to a system with a fixed cap on total emissions and permitting firms to trade allowances within that limit. Nonetheless, these terms are often used interchangeably (EPA, 2025).

proaches. Empirical results are frequently constrained by reliance on correlational methods, limited time frames, difficulties in identifying and measuring spillover effects, and concerns about the robustness and significance of estimated impacts. Ex-post evaluations remain relatively scarce, and much of the existing research has concentrated on the EU ETS, potentially introducing bias to empirical findings. While approximately half of the studies acknowledge leakage, few explicitly incorporate it into their empirical models ("Does carbon pricing reduce emissions? A review of ex-post analyses, author=Green, Jessica F", 2021).

Similar to the EU ETS, the Northeastern states have adopted a cooperative approach by jointly establishing a cap-and-trade program, known as the Regional Greenhouse Gas Initiative (RGGI, pronounced 'Reggie'). Using data spanning the longest time period and including the most recent observations available, I analyze the RGGI and assess whether this environmental policy has had measurable effects on the RGGI region's emission reductions. I do this by applying the synthetic control method, and my approach combines the structures outlined by Abadie (2021); Abadie, Diamond, and Hainmueller (2010); Cunningham and Shah (2018), and Mideksa (2013). The synthetic control method constructs a weighted combination of control units to create a synthetic version of the treated unit, enabling a more accurate comparison of outcomes before and after the observed event. Overall, my thesis addresses the following research questions: (1) Did the RGGI lead to emission reductions? (2) Is there any evidence of cross-regional emission leakage or fuel switching? (3) Does the synthetic control accurately model the emission reduction effect?

I find that the program led to a reduction in emissions from the electric power sector. This finding is referred to as the primary result, as it evaluates whether the cap-and-trade program achieved its goal of reducing emissions from its target sector as a whole. Spillover effects, including cross-regional leakage and fuel switching, are referred to as secondary results. To validate carbon trading as a feasible regulatory approach, I show that the program had no measurable negative impact on economic growth. To strengthen the empirical analysis, I examine heterogeneous effects within participating states and test the robustness of the synthetic control by removing the states with the largest weights. I provide a causal interpretation of the spillover effects by applying the synthetic control method not only to electric power emissions, but also to three fuel types (coal, natural gas, and petroleum) both within the RGGI region and in neighboring states sharing the same energy transmission grid. The analysis reveals the strongest reductions in coal, while natural gas emissions increased, particularly in neighboring states. To strengthen the validity of the estimates, I conduct detailed inference tests for the primary results and broader statistical tests for all results, including confidence intervals, average treatment effects, and p-values. In the absence of randomization, the p-value represents the probability of obtaining an estimate equal to or larger than the observed one when the intervention is randomly reassigned in the dataset (Abadie et al., 2010).

This thesis makes a contribution to the existing research on ETSs by examining a relatively less researched intervention within a comprehensive time frame suitable for applying a quasi-experimental approach. Given that much of the existing research focuses on the EU ETS, focusing on the U.S. helps to broaden the empirical understanding of cap-and-trade programs, particularly in the context of informing the design of a functional global climate policy framework for effective emission reductions (Bushnell et al., 2007). Additionally, by analyzing a program within the U.S., I am able to estimate spillover effects under the assumption of relatively uniform cultural, institutional, and market structures across states. Contrary to earlier studies on RGGI, this thesis also considers the heterogeneity of effects within individual RGGI states, providing a more nuanced understanding of how cap-and-trade impacts may vary across participating entities. Consistent with the DICE model's emphasis on balancing climate and economic impacts, the program's neutral effect on GDP contributes to the literature on potential challenges and trade-offs between environmental and economic objectives (Hickel & Kallis, 2020; Stern, 2008). Furthermore, I examine the dynamics of different fuel types both within the RGGI region and across multiple non-RGGI states. This broader scope extends beyond the typical focus on Pennsylvania and Ohio (Fell & Maniloff, 2018; Yan, 2021). To address the statistical significance of the estimates, I report confidence intervals for my results, thereby strengthening the basis for causal inference. Additionally, I construct two different distributions of placebo estimates of the primary results that account for the divergence of the intervention effects. These inference tests address limitations in previous empirical synthetic control studies on cap-and-trade systems, observed in research of both U.S. (Kim & Kim, 2016; Lessmann & Kramer, 2024) and European ETSs (Bayer & Aklin, 2020).[4]

The rest of the thesis is structured as follows. Section 2 outlines the institutional background of environmental policies, beginning with U.S. climate policy and then focusing on

[4] For synthetic control applications on carbon taxes, see Leroutier (2022); Mideksa (2024).

the RGGI. Section 3 reviews related literature, covering environmental regulation, carbon pricing, and empirical and theoretical studies on ETSs. This section includes two of the most prominent programs, the EU ETS and China's ETS (World Bank Group, 2024), as well as U.S.-specific studies on programs in California and Northeastern states. Section 4 presents the data and describes the synthetic control method, including model formulation and underlying assumptions. Section 5 reports the empirical findings on electric power emissions, economy effects, heterogeneity across RGGI states, leakage, and fuel switching. It also includes inference tests using in-time and in-space placebo tests, followed by a discussion of limitations and suggestions for further research. Finally, Section 6 concludes by summarizing the results, relating them to existing research, and suggesting directions for future work.

2 Institutional Background of Environmental Policies

2.1 Overview on the United States

From the 1970s to the mid-1980s, the U.S. was a global leader in international environmental law, driving the adoption of major multilateral environmental agreements. Since the 1990s, however, the EU has emerged as the primary advocate for international environmental policies, while the U.S. has largely retreated from this role (Kelemen & Knievel, 2015). GHG emissions per capita and GDP per capita remain among the highest in the OECD, driven by the continued reliance on fossil fuels in the energy mix. Environmentally related taxes accounted for only 0.7% of GDP in 2020, the lowest share among G7 countries and well below the OECD average of 1.4% (OECD, 2023). State-level actions combined with moderate federal efforts could help achieve near-term emission targets, but long-term goals such as a reduction of 80% by 2050 are unattainable without new federal legislation. Power plants, responsible for 40% of fossil-fuel combustion emissions and one-third of the U.S. GHG inventory, offer the greatest reduction potential. Ambitious power plant standards could reduce emissions by up to 74% by 2035 (Bianco, Litz, Meek, & Gasper, 2013). Successful infrastructure investments require coordination across agencies and levels of government, but the U.S. lacks long-term vision and efficient procurement processes, and instead relies on fragmented, subsidy-based policies (Hassler et al., 2024; OECD, 2023).

As the world's largest economy, the U.S. plays a critical role in global climate efforts. While its environmental regulations are relatively less stringent, the country presents important opportunities to contribute to the assessment and design of effective climate policies through economic research (OECD, 2023). Between 2005 and 2016, energy-related carbon dioxide (CO_2) emissions declined in 41 U.S. states and increased in 9 states (excluding the District of Colombia). Maryland achieved the largest percentage decrease at 30% (24 million tons[5]), and Ohio recorded the greatest absolute decline of 64 million tons (24%). In contrast, Idaho experienced the largest percentage increase at 16% (3 million tons), while Texas had the largest absolute increase of 52 million tons (9%). In Ohio, coal-related CO_2 emissions from the electric power sector dropped by approximately 62 million tons during this period, while the state's economy grew modestly by less than 8%. At the same time, Texas saw a 23 million tons reduction in coal-related emissions from the power sector but experienced nearly 42% economic growth, largely driven by energy-intensive industries such as energy production and petroleum refining (EIA, 2019). While these state-level variations are complex, they signal the need for further empirical research to better understand the underlying economic and climate dynamics.

Historically, nuclear and hydroelectric power have been the primary sources of non-carbon-producing energy in the U.S. In recent years, neither has seen significant capacity growth, whereas non-hydropower renewables, like wind and solar, have grown rapidly. This growth has been driven by supportive policies at both the state and federal levels. State policies like Renewables Portfolio Standard (RPS) and federal tax credits have subsidized investments and made renewable energy more competitive with traditional power sources (EIA, 2019).

While many countries pursue national ETS frameworks, the U.S. has taken a more decentralized approach, with carbon markets operating primarily at the state level. California's cap-and-trade program and the RGGI are the most established examples. California's system covers multiple sectors, including power generation, industry, and transportation fuels, while the RGGI is a cooperative program among several Northeastern states that targets only power sector emissions. Additionally, new initiatives are emerging, such as Colorado's planned ETS expansion in 2024, while New York is advancing cap-and-invest programs. Despite these developments, some states, such as North Carolina and Virginia, have opted out of regional programs, reflecting the fragmented approach to carbon pricing in the U.S.

[5]'tons' refers to 'metric tons', as used in the original sources.

(World Bank Group, 2024). This fragmentation limits the potential efficiency gains of international emissions trading, as Paltsev et al. (2007) argues that such gains require U.S. climate policies to be substantially more stringent than those in other regions. I go through relevant literature of the cap-and-trade program in California and the RGGI in a later section.

2.2 The Regional Greenhouse Gas Initiative

The RGGI is a market-based program that involves 11 states in the Northeastern U.S. and aims to cap and reduce CO_2 emissions from the power sector. The program's first compliance period began on January 1, 2009, but its development began in 2003 and culminated in a Memorandum of Understanding (MOU) signed on December 20, 2005, by seven states: Connecticut, Delaware, Maine, New Hampshire, New Jersey, New York, and Vermont. By 2007, Massachusetts, Maryland, and Rhode Island joined the initiative, increasing participation to ten states. The first compliance period began in 2009, but auctions started in 2008 (C2ES, n.d.). While New Jersey withdrew in 2011, it rejoined in 2020. Virginia joined in 2021 but ceased participation in 2023. In line with its 2005 agreement, the RGGI states conducted a program review in 2012 to assess its effectiveness. Recognizing a disparity between the emission cap and actual emissions, the participating states agreed to reduce the cap by 45% to better align it with real-world emission levels. Over the years, the RGGI has undergone other program reviews to refine its emissions cap and improve its design (Ramseur, 2019; RGGI, n.d.).

The RGGI operates exclusively in the power sector, focusing on fossil fuel-fired power plants with a capacity of 25 megawatts or greater. These facilities are required to hold allowances equivalent to their CO_2 emissions over a three-year control period. Allowances are primarily distributed through quarterly auctions, with proceeds invested in energy efficiency, renewable energy, and other consumer benefit programs. The program's emission cap is designed to decline over time, reducing total emissions and encouraging a transition to cleaner energy sources (Ramseur, 2019; RGGI, n.d.). Even during periods of reduced emission demand, RGGI's price floor has maintained steady market conditions by preventing allowance prices from dropping to zero (B. C. Murray & Maniloff, 2015).

Between 2008 and 2019, the RGGI states collectively sold approximately 80% of their budgeted emission allowances through quarterly auctions, generating \$3.2 billion in proceeds. While the emission cap itself likely had a limited direct impact on power plant emissions in the region, the investment of auction proceeds in cleaner energy sources has likely contributed to reducing emissions (Ramseur, 2019).

3 Related literature

3.1 Environmental Regulation and Carbon Pricing

Climate policies can be assessed through social cost of carbon (SCC), which represents the estimated economic damage caused by emitting one additional ton of CO_2 into the atmosphere. SCC reflects the present value of future harm from climate change, including impacts on agriculture, health, and ecosystems (W. D. Nordhaus, 2017). Conceptually, it serves as a tool for guiding optimal climate policy by setting a price on CO_2 emissions that balances the costs of mitigation with the benefits of avoided damages (Pearce, 2003).[6] In other words, an effective climate policy must correct the market failure of underpriced emissions by ensuring their cost reflects their true environmental impact. As discussed earlier, this can be achieved through an ETS (quantity-based instrument), which sets a price on emissions and allows trading. Another approach is a carbon tax (price-based instrument), which directly imposes a levy on emissions (W. Nordhaus, 2019; Weber & Neuhoff, 2010). Compared to emission taxes, tradable permits allocate pollution rights to industries for free, whereas taxes impose a financial burden on polluters. Unlike uniform emission standards, which apply equally to all firms regardless of cost differences, tradable permits incentivize cost-effective reductions, directing investment toward the lowest-cost abatement opportunities (Aidt & Dutta, 2004).

Carbon price achieves four objectives. First, it signals consumers to reduce consumption of carbon-intensive goods and services. Second, it incentivizes producers to shift towards low-carbon energy sources and technologies. Third, it fosters innovation by providing financial incentives for the development and commercialization of low-carbon solutions. Finally, a carbon price reduces the informational burden associated with coordinating emissions reduc-

[6] SCC is commonly applied in IAMs, such as the DICE model (W. D. Nordhaus, 2017; Pearce, 2003).

tions across sectors (W. Nordhaus, 2019). As of 2024, 75 carbon pricing mechanisms were in operation globally, comprising 39 carbon taxes and 36 ETSs (World Bank Group, 2024). Carbon pricing dominates climate policy discussions, yet its ex-post performance remains poorly understood. It is also hard to estimate the effectiveness of these measures because of the overlap with other policies and external factors that also influence CO_2 emissions ("Does carbon pricing reduce emissions? A review of ex-post analyses, author=Green, Jessica F", 2021; Haites, 2018).

In general, carbon taxes outperform ETSs in reducing emissions. For instance, the EU ETS has achieved limited annual reductions of 0% to 1.5% on average ("Does carbon pricing reduce emissions? A review of ex-post analyses, author=Green, Jessica F", 2021), while literature on carbon tax report more substantial effects, including reductions of 5% to 15% in British Columbia between 2008 and 2015 (B. Murray & Rivers, 2015), and annual reductions of 6.3% in Sweden between 1990 and 2006 (Andersson, 2019). In Finland, emissions were 16% lower in 1995, 25% lower in 2000, and 30% lower in 2004 compared to the counterfactual, aligned with a twenty-fold increase in the value of carbon tax between 1990 and 2005 (Mideksa, 2024). Metcalf and Stock (2023) found no significant negative impacts of carbon taxes on GDP or employment in Europe, but rather a modest but not always significant effect on economic growth. Emission reductions were observed but underestimated due to the exclusion of low-cost abatement sectors, such as electricity. Shapiro and Metcalf (2023) analyzed a U.S. carbon tax targeting a 35% emissions reduction, finding mild long-term gains in output and consumption, slight increases in unemployment, and greater green technology adoption. Short-term effects suggested gradual output gains and minimal unemployment changes.[7]

While literature indicates that carbon taxes have generally dominated in emission reductions, their broader impacts can be suboptimal. Economic considerations, such as trade effects and innovation dynamics, influence the relative effectiveness of a carbon pricing approach. Strand (2013) compared a carbon tax and a cap-and-trade for a "policy bloc", and found that the policy bloc prefers a tax because it lowers fuel export prices, especially when the bloc is larger. Relying solely on carbon taxes to address both current emissions and the future path of innovation would require excessively high tax rates. This could lead to economic

[7]For more results on carbon tax effects, refer to Table 2 in "Does carbon pricing reduce emissions? A review of ex-post analyses, author=Green, Jessica F" (2021).

distortions in the short term, such as reduced production and consumption (Acemoglu et al., 2012). Weber and Neuhoff (2010) found that firm-level innovation in carbon-abatement technologies enhances the effectiveness of cap-and-trade systems by lowering emissions caps, which can in turn lead to higher expected carbon prices. Innovation can shift the preference toward a cap-and-trade system over carbon taxes.

3.2 Empirical Evidence of Emission Trading Schemes

3.2.1 General Overview

Bayer and Aklin (2020) used the synthetic control method to estimate the emission reduction effects of the EU ETS, finding a 1.2 billion ton CO_2 reduction (3.8%) from 2008 to 2016, with 8.1%–11.5% cuts in ETS-covered sectors. The study did not account for emission leakage to non-EU regions, making the results an upper-bound estimate of the global effect. Emission leakage can occur in two forms: Emissions from emission-intensive entities may shift to less regulated or lower-cost sectors that are in the same region but operate outside the regulated carbon market. This is referred to as cross-sectoral leakage (Lessmann & Kramer, 2024). Alternatively, emissions can relocate from one state to another, causing cross-regional leakage. Leroutier (2022) also apply the synthetic control method to estimate the impact of the UK's Carbon Price Support in 2013, finding a 20–26% annual reduction in power sector emissions between 2013 and 2017. Analysis of donor countries indicated minimal changes in trade and electricity imports, suggesting low leakage. However, the short pre-treatment period (2005–2012) may weaken the model specification.[8]

Dechezleprêtre, Nachtigall, and Venmans (2023) applied a matching methodology by combining installation-level inclusion criteria with difference-in-differences (DD). The analysis covered France, the Netherlands, Norway, and the UK, and it compared emissions and economic performance of ETS-regulated and non-ETS-regulated installations to identify causal effects. While accounting for firm size, region, and sector heterogeneity, the study was based on a small sample (240 ETS installations), leading to limited external validity, as the results

[8]This limitation is addressed by applying the same method to less precise aggregate data available since 1990. Although the composition of the synthetic UK differs, the estimated impact remains closely aligned with the original findings.

may not generalize to all EU ETS countries. Dechezleprêtre et al. (2023) found a 10% CO_2 reduction between 2005 and 2012 with no significant effects on profits or employment but an increase in revenues and fixed assets for regulated firms. Calel (2020) employed a similar matched DD approach, but differed in its emphasis on innovation outcomes, specifically low-carbon patenting and R&D spending, rather than emissions reductions or broader economic impacts.

Skepticism surrounding the EU ETS often stems from persistently low permit prices relative to the social cost of carbon, as high carbon prices are generally considered essential for effective emission reductions. However, Bayer and Aklin (2020) suggest that a carbon market can still be effective if it is a credible institution with the potential for future stringency. In such cases, firms may cut emissions even at low prices, because low prices can be a signal for weakening demand for permits. Additionally, Calel (2020) argues that the market can incentivize low-carbon innovation. Analyzing a panel of British firms under the EU ETS, they found that firms subject to the EU ETS increased their low-carbon patenting activity and R&D investments by 20–30%, contributing an estimated £350–£450 million in additional R&D spending and 100–150 new patents between 2005 and 2012. Although these effects do not necessarily translate into immediate reductions in the carbon intensity of output (Calel, 2020), evidence from Ellerman and Buchner (2008) suggests that firms engaged in abatement efforts during the first two years of EU ETS (2005–2006), as CO_2 emissions were approximately 3% below allocated allowances. This may suggest over-allocation, but the authors propose that it could also reflect firms reducing emissions to sell excess allowances.[9]

Overall, research on the EU ETS has yielded somewhat contradictory results. The divergence in findings can be attributed to differences in measurement approaches and data, country-specific variations, and historical comparisons with other cap-and-trade programs. Early research suggests that emissions reductions were initially driven by fuel switching in the power sector, reflecting adoption rather than innovation. In contrast, later evidence indicates that firms regulated under the EU ETS increased investment in low-carbon innovation, including R&D and patenting (Calel, 2020).

As carbon markets have expanded beyond Europe, China's ETS represents a significant step in global climate policy, demonstrating a distinct approach to emission trading within a

[9] Over-allocation and abatement are hard to disentangle, as firms might have reduced emissions to bank allowances rather than due to policy incentives (Ellerman & Buchner, 2008).

rapidly industrializing economy. Additionally, it offers an opportunity to broaden the empirical understanding of carbon markets. Zhang, Li, Li, and Guo (2020) employed a DD model to evaluate the impact of China's ETS pilots on economic growth and carbon emissions in the industrial sector. It compared seven pilot provinces with 23 control provinces before and after the ETS implementation. They also conducted a data envelopment analysis[10] to assess the efficiency of carbon trading markets. They found that the ETS reduced industrial CO_2 emissions by 24.2% in pilot regions while increasing industrial output by 13.6%. Hu, Ren, Wang, and Chen (2020) also implemented a DD model and analyze the impact of China's ETS on energy conservation and emission reduction. They found a 22.8% decrease in energy consumption and a 15.5% reduction in CO_2 emissions relative to non-pilot areas. Despite the ETS, coal remained the dominant energy source in China, as low coal prices and weak carbon price signals discouraged a shift to cleaner energy. However, the ETS led to industrial restructuring, with some high-emission industries shrinking or exiting. Consequently, emissions reductions were primarily driven by lower industrial output and improved technical efficiency, rather than fuel switching. Since firms that remained operational continued to rely on coal, the ETS had a limited impact on the energy structure. Additionally, the ETS was most effective in regions with strong environmental enforcement (greater regulatory oversight and penalties) and higher marketization levels (Hu et al., 2020).

As different countries and regions develop ETSs, their possible contribution to an international carbon market becomes increasingly relevant. Arvaniti and Habla (2021) develop a game-theoretic model to analyze negotiations over international carbon markets. The model applies a Nash bargaining solution to determine equilibrium emission levels and permit allocations between countries. The findings indicate that an international carbon market leads to a Pareto improvement if marginal damages differ significantly across countries, and if marginal damages and abatement costs are sufficiently aligned across countries. For example, bilateral carbon markets among major emitters (China, the EU, and the U.S.) would not achieve Pareto efficiency. The model assumes strong delegation, meaning governments do not override their appointed negotiators even if negotiations fail.[11] This assumption sim-

[10]Data envelopment analysis (DEA) is a non-parametric method used in operations research and economics to evaluate the efficiency of decision-making units (DMUs), such as firms or organizations, by comparing their input-output ratios relative to a best-practice frontier. For further details, refer to Zhou, Ang, and Poh (2008).

[11]For a comprehensive discussion of the theoretical framework and underlying assumptions, see Arvaniti and Habla (2021); Segendorff (1998).

plifies negotiations but may not fully capture real-world political dynamics, where leadership changes or shifts in policy priorities often lead to renegotiation or withdrawal from agreements. Consequently, the model may underestimate the risks of enforcement challenges or policy reversals in long-term carbon market cooperation.[12]

3.2.2 California and Northeastern States

California is widely recognized as a subnational leader in environmental and climate change policy (Chen et al., 2011), and its leadership is explained by four factors: preferences for action, legal authority to implement policies, robust institutional capacity, and effective governance (Mazmanian, Jurewitz, & Nelson, 2020).

Lessmann and Kramer (2024) used the synthetic control method to estimate the causal effect of California's cap-and-trade program on per capita CO_2 emissions in electricity and industrial sectors. They found that electricity sector emissions fell by 48% relative to the counterfactual, primarily due to a shift from natural gas to renewables. However, the counterfactual relied heavily on a small number of states. In the electricity sector, Idaho alone accounted for 79% of the total weight, raising concerns about the external validity of the results. Moreover, external factors, such as complementary policies and energy market shifts, complicate the interpretation of results. Bushnell et al. (2007) argues that the RPS is expected to have a larger short-term impact on emissions than cap-and-trade, as it requires additional renewable capacity to be built, rather than just shifting existing contracts.

Lessmann and Kramer (2024) found that carbon leakage was minimal, as most reductions were achieved through increased renewable energy generation rather than emissions shifting to other states. However, this claim is not supported by accompanying empirical evidence. In contrast, Bartram et al. (2022) used a DD approach on a U.S. plant-level data and presented contradictory findings, showing that financially constrained firms shift emissions and output from California to other states where they operate underutilized plants. Conversely, unconstrained firms did not exhibit such adjustments. Overall, while unconstrained firms maintained their total emission levels, constrained firms increased their total emissions following the implementation of the cap-and-trade program. Bushnell et al. (2007) found that

[12] For an overview of general-level research on ETSs, see Table 1 in "Does carbon pricing reduce emissions? A review of ex-post analyses, author=Green, Jessica F" (2021).

instead of achieving genuine emissions reductions, the program primarily resulted in contractual reshuffling[13], as utilities could purchase existing low-carbon power from other Western states while high-carbon power was redirected to non-regulated states. While the study did not establish causality, it addresses potential unintended consequences of California's cap-and-trade program.

Chen et al. (2011) states that California must consider the broader effects of emission regulation. If the goal is to control GHG emissions regionally in the West, a geographically restricted emissions trading program may have limited effects. However, if the intent is to pave the way for a federal CO_2 cap-and-trade system, the design must carefully balance the need to avoid market disruption and limit additional electricity price increases for consumers.

While California's cap-and-trade system is the broadest in the U.S. (ICAP, 2024), the Northeastern states have taken a different approach through a multi-state collaboration focused only on the power sector. B. C. Murray and Maniloff (2015) utilized state-level panel data from 1991 to 2012 to differentiate the responses of RGGI states from those of the rest of the U.S. Emission reductions attributed to RGGI were estimated using a theoretical econometric model with emission simulations, which provides limited support for causal inference. The study found that the overall decline in emissions was approximately 24%, and it was driven by multiple factors, with RGGI having the most significant impact. Other factors included falling natural gas prices, economic recession, and state renewable energy policies. The "announcement effect"[14] suggests firms began adjusting before the program formally started. Ruth et al. (2008) employed a combination of three models[15] to assess the economic and energy impacts of Maryland joining the RGGI. According to their estimates, net CO_2 reductions in the RGGI region amounted to approximately 6.35 million tons cumulatively over the period 2010-2025, accompanied by a decline in in-state coal and natural gas generation. While coal still remained a dominant energy source, its generation declines as Maryland increasingly relies on power imports. The policy was also projected to reduce CO_2 emissions

[13] This reshuffling is different from conventional carbon leakage because it does not require physical changes in power production, but rather a change in who buys power from whom (Bushnell et al., 2007).

[14] This is captured by introducing an indicator variable that takes a value of 1 for states after they announced their intention to join RGGI and 0 otherwise.

[15] (1) The Haiku model simulates electricity market equilibrium, incorporating investment decisions, emissions compliance, and electricity demand. (2) JHU-OUTEC allows for the examination of strategic pricing behavior in the power sector under transmission and emissions constraints. (3) IMPLAN is used to assess the economic impacts of Maryland's participation in RGGI, estimating effects on employment, gross state product, and tax revenues. For more details, refer to Ruth et al. (2008).

from electricity generators in Maryland, with only minor leakage effects.

Chan and Morrow (2019) contributed to causal effect estimation by conducting a DD model to compare emission changes before and after cap-and-trade implementation in RGGI states versus non-RGGI states. Their findings indicate that the program contributed to a reduction of 20% in CO_2 emissions (regression coefficient: –0.22*) among RGGI facilities compared to non-RGGI facilities over the period 2009–2016. Similarly, Yan (2021) employed a DD approach to assess the impact of RGGI on coal and natural gas consumption in the electric power industry across participating and non-participating states. Their analysis revealed a substantial reduction in coal consumption (73%) and a moderate decrease in natural gas consumption (30%) within participating states. Overall, CO_2 emissions declined by approximately 10% between 2009 and 2018. Potential unobserved confounders, such as changes in electricity demand or technological changes, may have introduced bias into the estimates. However, Ruth et al. (2008) found that Maryland's participation in RGGI led to lower electricity demand[16] due to investments in energy efficiency.

Fell and Maniloff (2018) also implemented a DD approach, but they defined two separate treatment effects: Power plants located within RGGI states, which are directly regulated, and power plants located in Pennsylvania and Ohio, which are outside the program but have strong transmission connections with the RGGI region and are expected to experience emission leakage. The control group consisted of generators in non-RGGI and non-leaker states that are unaffected by the cap-and-trade. Due to the complexity of electrical transmission, they caution that regression coefficients cannot be given a causal interpretation, and that any estimated effect should be regarded as merely correlative and supportive. Nonetheless, they found that RGGI led to a reduction in coal-fired power generation within participating states. Specifically, the RGGI-induced decrease in CO_2 emissions within the region amounted to 8.8 million tons annually, while the net CO_2 reduction attributable to RGGI (after accounting for leakage) was 4.3 million tons annually.

Figure 2 illustrates a decline in per capita electric emissions over time in the RGGI states. While these trends do not establish a causal link to the cap-and-trade program, they are consistent with findings from previous studies (Chan & Morrow, 2019; Fell & Maniloff, 2018; B. C. Murray & Maniloff, 2015; Ruth et al., 2008; Yan, 2021). It is important to note that

[16]Net electricity demand was estimated to decrease 1.5% in 2010 and nearly 3% in 2025.

the electric power sector emissions include coal, natural gas, and petroleum, but exclude emissions from imported electricity.

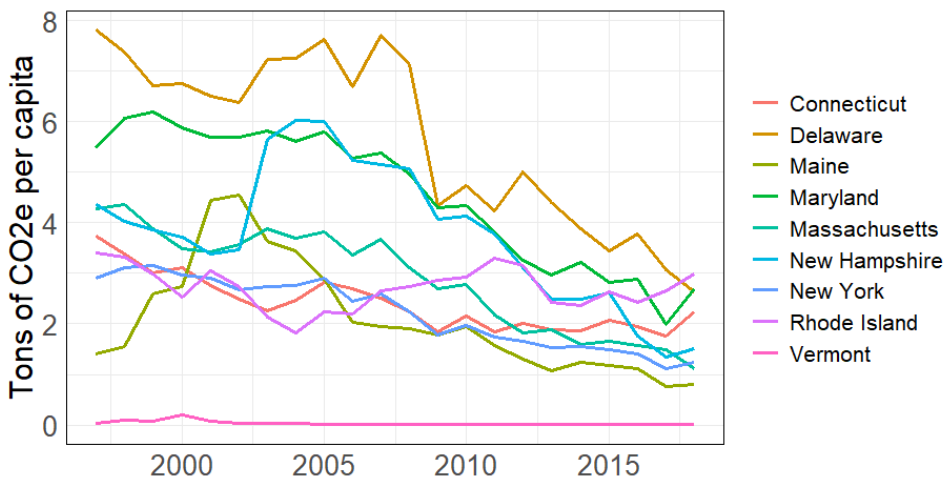

Figure 2: Per Capita Electric Power Emission Trends in RGGI States

Although literature suggests that the RGGI has achieved measurable emissions reductions, concerns remain about whether these reductions are partially negated by emission leakage and fuel switching.

There are many factors at play when it comes to leakage. If one sector reduces its emissions due to overlapping climate policies or higher carbon prices, emissions might increase in another sector.[17] This could result in a situation where one entity reduces emissions and requires fewer allowances, making those allowances available to others, and potentially leading to no net reduction in emissions. This is known as the waterbed effect, where emission reductions in one area displace, rather than eliminate, emissions throughout the system.[18] The waterbed effect applies to fixed-cap carbon markets (like the EU ETS or RGGI) where the total number of allowances is capped (Perino et al., 2019a; Perino, Ritz, & Van Benthem,

[17]Perino, Ritz, and Van Benthem (2019a) define this as 'internal carbon leakage'.

[18]Note how this differs from cross-sectoral/-regional leakage: The waterbed effect occurs within a cap-and-trade system when emission reductions in one sector lower carbon prices, leading to increased emissions elsewhere under the same cap. In contrast, leakage shifts emissions outside the regulated system, either across sectors or to other regions.

2019b). In California, the combination of cap-and-trade with RPS may have contributed to a waterbed effect: As RPS mandated utilities to procure a specific share of electricity from renewable sources, it simultaneously reduced permit demand, lowering the carbon price in the cap-and-trade market. This, in turn, weakened mitigation incentives for firms in other covered sectors (Lessmann & Kramer, 2024).[19]

Several studies have examined how the RGGI influenced fuel switching and leakage, with mixed results. As for fuel switching, Ruth et al. (2008) found that Maryland's participation in program led to a decrease in natural gas generation in the state, and Yan (2021) reported similar reductions in natural gas consumption for electricity generation across all participating states. In contrast, Kim and Kim (2016) estimated leakage effects within the RGGI states using the synthetic control method, and found that RGGI increased the share of natural gas in electricity generation by 10–15 percentage points, suggesting that the program encouraged fuel switching from coal to gas through a price signal favoring lower-carbon energy. However, their analysis did not explore variation in effects across individual states. Chan and Morrow (2019) also did not examine individual state shifts in natural gas emissions.

Regarding leakage, Yan (2021) estimated a 237% increase in natural gas use and a 7% decrease in coal use in Pennsylvania and Ohio. This cross-regional leakage effect was partly inevitable because the RGGI states participate in the regional electricity market, PJM Interconnection. Fell and Maniloff (2018) supported this finding, identifying Pennsylvania and Ohio as primary transmission interfaces into the RGGI region. Chan and Morrow (2019) argued that leakage is unlikely to significantly undermine overall effectiveness of the RGGI, as the displaced generation primarily originates from cleaner energy sources. Fell and Maniloff (2018) found that the program reduced emissions-intensive generation in regulated regions while encouraging cleaner generation in neighboring states, which led to an overall decrease in emissions across both regulated and unregulated regions.

The findings of increased cross-regional natural gas emissions (Fell & Maniloff, 2018; Yan, 2021) are supported by Figure 3, which presents slight upward trends for per capita natural

[19] In Europe, the waterbed occurs because the Carbon Price Support in the UK reduces the demand for emission permits from UK power installations. Since the supply of permits in the EU ETS is fixed and perfectly inelastic, the weaker demand in the UK leads to a decrease in the price of permits. As a result, other installations in the EU ETS, which are not subject to the price support, can buy more allowances at a lower price and increase their emissions, leaving aggregate emissions unchanged Leroutier (2022).

gas emissions for the non-RGGI states that are part of the PJM Interconnection.

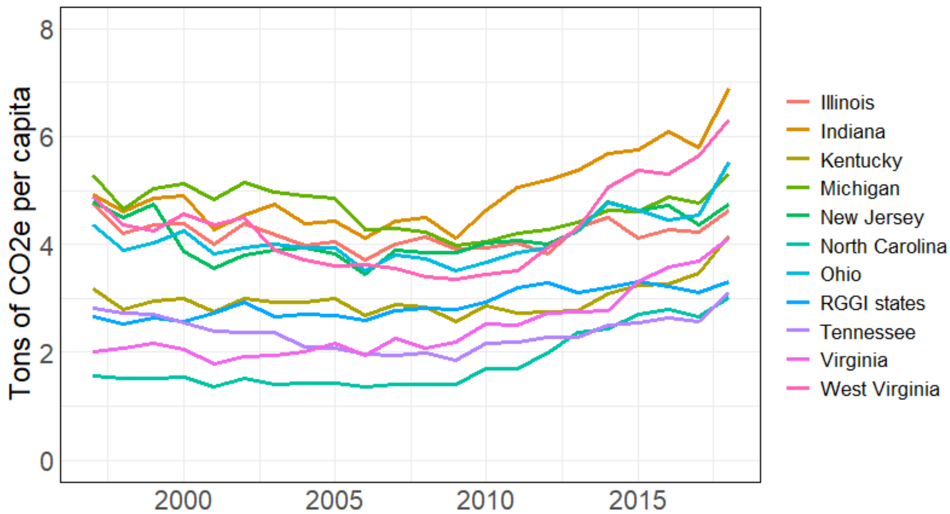

Figure 3: Per Capita Natural Gas Emission Trends in non-RGGI PJM States

In conclusion, attention should be paid to the relatively short time frames used in the empirical papers, especially when estimating the long-term-effects of the program. Second, the findings from Ruth et al. (2008) indicate that there might be some heterogeneity in the effects of the RGGI across participating states, though further analysis is needed to fully assess the scale and variation of these effects. Third, it is important to recognize that cross-regional leakage effects might extend beyond Pennsylvania and Ohio, and include other PJM states. Finally, the literature provides contradictory results on leakage and fuel switching (Calel, 2020; Hu et al., 2020; Kim & Kim, 2016; Ruth et al., 2008; Yan, 2021). Nevertheless, these results suggest that the role of natural gas as a transition fuel toward a lower-carbon future deserves further research (Kim & Kim, 2016; B. C. Murray & Maniloff, 2015).

4 Data and methods

4.1 Synthetic Control Method

The synthetic control method builds on the concept that a combination of control units often offers a better comparison for a treated unit than any individual control unit alone. The method constructs a synthetic version of the treated unit by creating a weighted combination of control units that approximates the path that the treated unit would have followed in the absence of an intervention. For example, Funke, Schularick, and Trebesch (2023) used the synthetic control method to construct a synthetic counterfactual for each populist leader episode, allowing them to estimate the causal effect of populist leadership on economic performance by comparing actual outcomes under populism to a weighted combination of non-populist countries that did not experience populist leadership. Similarly, Mideksa (2013) utilized synthetic control to estimate the impact of petroleum endowment on Norway's GDP per capita by constructing a synthetic control from non-oil-producing OECD countries to model Norway's pre-petroleum economy. Cunningham and Shah (2018) used the method to study the effects of decriminalizing indoor prostitution in Rhode Island. They compare post-legalization outcomes in Rhode Island to those in a synthetic version of the state, constructed from a combination of other states. Abadie and Gardeazabal (2003) were the first to apply the synthetic control method, examining the economic impacts of conflict through a case study of the terrorist conflict in the Basque Country in Spain. By comparing the actual outcomes to those of the synthetic control, these studies represent the method's ability to provide empirical counterfactuals for evaluating a policy intervention or an event by using panel data.

Compared to traditional regression methods, the synthetic control approach offers few advantages. By explicitly constructing a synthetic control as a weighted average of available control units, the method reveals both the contribution of each control unit to the counterfactual and the extent of similarity between the treated unit and the synthetic control in pre- and post-intervention outcomes. The method also restricts weights to be non-negative and sum to one, which mitigates the risk of extrapolation inherent in standard regression techniques. Another advantage of the synthetic control method is that it allows researchers to finalize study design without access to post-intervention outcomes, thereby reducing the risk of bias in study conclusions (Abadie et al., 2010).

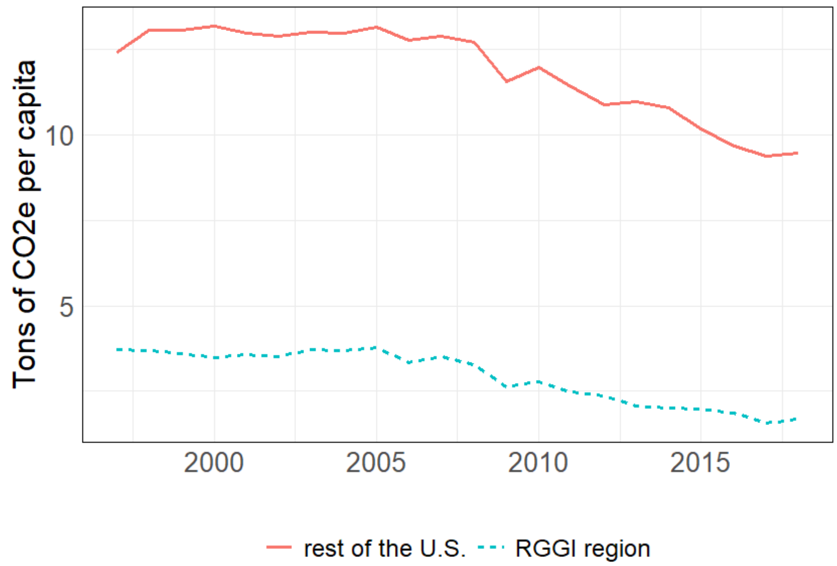

Figure 4: Per Capita Emissions for Electricity Sector: RGGI Region versus U.S. Sample Mean

The comparison of the RGGI region's per capita electric emission data with the average emissions of the rest of the states motivates the relevance of a synthetic control method in this thesis. Figure 4 displays this data from 1997 to 2018 and indicates that the average per capita emissions of other states are 3-4 times higher than those of the RGGI region. The gap between the two is so pronounced that fluctuations within the RGGI region's data are effectively obscured by the large y-axis scale required to represent the higher values of the other states.

4.1.1 Model Setting and Estimation

The synthetic control model developed by Abadie and Gardeazabal (2003) aims to create a synthetic counterpart to the treatment group. In our case, the treatment group for the primary model is the Northeastern states participating in the RGGI in our defined time frame, whereas the synthetic control is referred to as the synthetic (RGGI) region to estimate the treatment effect. For some of the secondary models, the non-RGGI PJM states are used as the treatment group. The synthetic controls are constructed as a weighted average of the untreated states, collectively known as the donor pool. The intervention refers to the launch of the cap-and-trade program, and throughout the empirical part, the terms intervention and cap-and-trade are used interchangeably. I will briefly go through the model derivation outlined by Abadie (2021) in one of their most recent works, as the paper also introduces assumptions that are applied in this section.[20]

Consider a dataset comprising $J+1$ units, indexed as $j = 1, 2, ..., J+1$. Let the first unit ($j = 1$) represent treated unit, which is affected by the intervention. The remaining J units ($j = 2, ..., J+1$) form the donor pool, consisting of untreated units unaffected by the intervention. The data spans T time periods, with the first T_0 periods occurring prior to the implementation of the intervention. For each unit j and time t, the outcome of interest Y_{jt} is observed. For each unit j, a set of k predictors of the outcome, denoted $X_{1j}, ..., X_{kj}$, is observed before and after the intervention.

The predictors for units $j = 1, ..., J+1$ are represented by $k \times 1$ vectors $\mathbf{X}_1, ..., \mathbf{X}_{J+1}$. The values of the predictors for the J untreated units are organized into a $k \times J$ matrix, given by $\mathbf{X}_0 = \begin{bmatrix} \mathbf{X}_2 & \cdots & \mathbf{X}_{J+1} \end{bmatrix}$. For each unit j and time t, Y_{jt}^N denotes the potential outcome in the absence of the intervention. For the treated unit ($j = 1$) in a period after the intervention is implemented ($t > T_0$), Y_{1t}^I represents the potential outcome under the intervention. The effect of the intervention on the affected unit in period t is therefore defined as

$$\tau_{1t} = Y_{1t}^I - Y_{1t}^N. \tag{1}$$

A synthetic control is defined as a weighted average of the units in the donor pool. It is formally defined by a $J \times 1$ vector of weights, $\mathbf{W} = (w_2, \ldots, w_{J+1})'$. The weights w_j determine the contribution of each unit in the donor pool to the synthetic control. Using these weights, the synthetic control estimate of the untreated outcome for the treated unit, Y_{1t}^N is given by

$$\hat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt}. \tag{2}$$

[20] For a detailed derivation, see Abadie (2021), and for an alternative derivation, see Abadie et al. (2010).

The estimated treatment effect for the treated unit in periods t is then calculated as

$$\hat{\tau}_{1t} = Y_{1t} - \hat{Y}_{1t}^{N}. \tag{3}$$

The weights for the synthetic control are determined by minimizing the distance between the treated unit's characteristics and a weighted combination of the donor units' characteristics. This is expressed as:

$$\|\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}\| = \sqrt{\sum_{h=1}^{k} v_h \left(X_{h1} - \sum_{j=2}^{J+1} w_j X_{hj} \right)^2}, \tag{4}$$

$$\text{subject to } w_j \geq 0 \quad \text{and} \quad \sum_{j=2}^{J+1} w_j = 1,$$

where $v_h > 0$ are predictor-specific weights that capture the relative importance of each predictor.

The estimated treatment effect for the treated unit is calculated as the difference between the observed outcome of the treated unit, Y_{1t} and the synthetic control's estimated outcome for the same period, which is a weighted average of the outcomes of the donor pool. This is expressed as:

$$\hat{\tau}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}. \tag{5}$$

4.1.2 Model Assumptions

For the synthetic control model to accurately capture the causal effect of the intervention, the following assumptions proposed by Abadie (2021) should hold:

Size of the effect and volatility of the outcome. Small effects are difficult to distinguish from other shocks, especially if the outcome variable is highly volatile. Volatility from common factors can be accounted for using synthetic controls, but unit-specific random noise must be minimized through filtering before applying the method.

Availability of a comparison group. A suitable comparison group is essential, and thus the donor pool should exclude units that adopted similar interventions or experienced idiosyncratic shocks during the study period. The donor units should be similar to the treated unit to minimize interpolation bias.

No anticipation. Economic agents or policy effects should not act in anticipation of the intervention. If such anticipation is suspected, the intervention should be backdated in the dataset to a period before these effects could occur.

No interference. There should be no spillover effects between treated and control units. If interference exists, units potentially affected by it should be excluded from the donor pool. Alternatively, spillover effects should be carefully analyzed during the interpretation of results. Chan and Morrow (2019) discuss a similar phenomenon, referring to it as stable unit treatment value assumption.[21]

Convex hull condition. The pre-intervention characteristics of the treated unit should be closely approximated by a convex combination of the control units. If this condition is violated, alternative transformations (e.g., time differences or growth rates) can be applied to the outcome variable.

Time horizon. The effects of some interventions may take time to appear or become large enough to detect. One option to avoid this is to simply wait until the effects fully unfold, though this is not ideal. A better approach is to use surrogate outcomes or leading indicators of the outcome variable.

4.2 Data Structure

The data sources are provided in Appendix A, and necessary datasets were constructed accordingly in R. The analysis covers the period from 1997 to 2018, with 2008 marking the RGGI turning point to account for the anticipation effect.[22] While the RGGI includes 11 states as of 2025, this analysis focuses on the nine states originally participating throughout the entire study period. The treatment region is constructed by averaging all predictor

[21] Chan and Morrow (2019) refer to SUTVA, where including states affected by leakage in the control group could bias estimates of the true intervention effect.

[22] Although the first compliance period began in 2009, first auctions were held in 2008. This suggests firms likely started adjusting earlier, making 2008 the appropriate treatment year.

variables across the treatment states and forming a new treatment unit based on these average values. As a result, both the RGGI and PJM states are treated as a single entity. The treatment states are naturally excluded from the donor pool, along with California, which is omitted due to its independent cap-and-trade program introduced in 2013. For the PJM region models, both the PJM states and RGGI states, along with California, are excluded from the donor pool. These exclusions ensure that the assumptions of available comparison group and no interference hold.

The outcome variable represents per capita emissions for the fuel type in question in each model. For instance, when examining electric power emissions, it serves as the outcome variable. The four fuel types modeled with the synthetic control are (a) electric power, (b) natural gas, (c) petroleum, and (d) coal.[23] Predictor variables remain the same in all models, and they are (1) energy intensity, (2) carbon intensity of the energy supply, (3) real GDP per capita, (4) turnover in the private goods-producing industry per capita, and (5) turnover in finance, insurance, real estate, rental, and leasing per capita. In every model, I also add three years of lagged outcome variables as additional predictors to enhance the fit of the counterfactual. I select predictors (1) and (2) to construct a counterfactual that closely resembles the RGGI region's reliance on fossil fuels.[24] Predictors (3) and (4) ensure the counterfactual aligns with the RGGI region's economic structure, while predictor (5) accounts for the disruptive effects of the financial crisis (Lessmann & Kramer, 2024). Outcome variables and predictors (4)-(5) are normalized to per capita, whereas (3) is normalized to GDP growth to avoid violation of the convex hull condition. For the economic analysis, the outcome variable is GDP growth, whereas the predictor variables include (1)-(2) and (4)-(5).

[23]Note that fuel types (b)–(d) may originate from power plants (type (a) emissions), as they reflect potential fuel switching from electric power plants.

[24]These variables can also be traced back in the DICE model, which addresses energy intensity and carbon intensity of energy supply by incorporating assumptions about technological change and emission reductions (W. D. Nordhaus, 1992).

5 Results

In this section, I present the results of synthetic control models. First, I demonstrate the primary results, the effects of the cap-and-trade program on per capita electric power emissions, the primary sector regulated under the RGGI. Next, I address the secondary results, cross-regional leakage and fuel switching. Lastly, I conduct in-depth inference checks on the primary results, along with more standard statistical tests that also include the secondary results. I then discuss the main limitations and suggest directions for future research.

5.1 Electric Power Emissions

Figure 5 displays the overall fit of the synthetic control model, comparing the pre-treatment characteristics of the actual RGGI region with its synthetic counterpart, which is constructed from the weighted predictors (1)-(5)[25] and population-weighted average of the 41 donor pool states. The descriptive statistics for electric emissions are displayed in Appendix B.

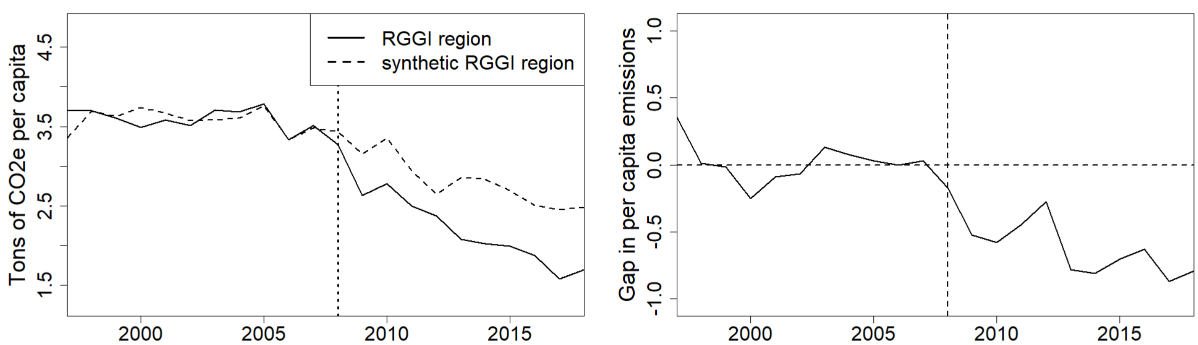

Figure 5: Trend Line and Gap Plot in Per Capita Electric Power Emissions

Notice that, unlike average CO_2 emissions in other states (shown in Figure 4), CO_2 emissions in the synthetic RGGI region closely follow the trajectory of the actual RGGI region throughout the entire pre-intervention period. Building on the DICE model, Figure 11 illustrates the impact of electric power regulation on economic growth. Based on the figure, the program appears to have no discernible effect on GDP in participating states.

[25]Including three years of lagged outcome variables (a)-(d) for the observed fuel type.

After the program's launch, per capita electric emissions in the RGGI region decline by 48.13%, compared to a decline of 27.70% in the synthetic region. Clearly, the decline in the treatment region is considerably greater. By comparing the emission reductions between the treated and synthetic units in 2018, emissions in the treatment region were 31.82% lower than in the counterfactual scenario, equivalent to a 0.79-ton reduction in CO_2 per capita. Based on the results, there is evidence that the RGGI led to emission reductions. This decline aligns with previous empirical findings of CO_2 emission reductions from electric power plants in the RGGI region (Chan & Morrow, 2019; Fell & Maniloff, 2018; Kim & Kim, 2016; B. C. Murray & Maniloff, 2015; Yan, 2021). However, the downward trends in both regions suggest that unobserved confounders could influence the estimates, making it difficult to fully isolate the intervention effect. This suggests that broader trends beyond the RGGI intervention, rather than anticipation of the program alone, likely contributed to the overall decline in emissions in the U.S. This indicates a potential violation of the no interference assumption.

Although B. C. Murray and Maniloff (2015) report statistically significant[26] evidence of an anticipation effect, my findings do not support the presence of such an effect exclusively within the treatment region. This is consistent with the findings of Lessmann and Kramer (2024), who found no evidence of an anticipation effect in California, despite a five-year gap between the program's adoption and implementation.

As for the synthetic region, it is difficult to identify specific drivers of the emission reduction trends. One possible factor is the Energy Policy Act of 2005, which provides loan guarantees for projects utilizing innovative technologies that reduce CO_2 emissions. The Act also mandates an increase in biofuel blending requirements for gasoline sold in the U.S. (EPA, 2024). RPSs in other states may have also played a role–by 2019, 30 states and the District of Columbia had adopted mandatory RPS policies, most of which had been implemented between 2004 and 2009 (Thombs & Jorgenson, 2020). These overlapping policies complicate the selection of an optimal donor pool, potentially violating the assumptions of availability of a comparison group and no interference. The findings also raise questions about whether regional cap-and-trade programs (RGGI and California's cap-and-trade) could have influenced emission reduction actions in other states.

[26] In their baseline regression model, they have announcement effect indicator, which equals 1 in years after a state's announcement to join RGGI but before implementation. Correspondingly, they provide program effect indicator, which equals 1 once the program is implemented.

5.1.1 Sensitivity Analysis

Based on the figures, the synthetic control seems to effectively replicate the selected predictors of the treatment region, therefore demonstrating a strong overall fit. However, a closer examination of the statistics of the model (Appendix B) reveals a few discrepancies.

Energy intensity of economy achieves a strong fit to the synthetic control, and it also carries the highest weight of 0.645 (see Table 4). Consequently, the model's inference may become overly dependent on this variable. Such reliance could undermine the robustness of the results if post-treatment dynamics are heavily influenced by this single predictor. Additionally, it is reasonable to question whether this weight is overly dominant, potentially superseding other predictors. This could indicate that other predictors fail to capture the pre-intervention characteristics of the treatment unit based on the given predictors.

There is also a substantial difference between the treatment and synthetic region for financial industry per capita (see Table 2), which suggests that the control states fail to accurately replicate the RGGI region's financial industry characteristics. This predictor was included to account for potential effects of the financial crisis, but the poor replication suggests that shocks related to the financial industry could influence the outcome. Both of these deviations imply a lack of control, and raise a concern about the assumptions of the availability of a comparison group and no interference, especially with regards to the predictor for financial industry, as it has a weight of 0 in the model. This is particularly concerning, as the financial crisis in 2008 likely had a significant impact on the model: Since emissions decreased in both the treated and synthetic units, it can be argued that the financial crisis was partly responsible for these trends, given that the recession impacted the entire U.S. The high mean value for financial sector in the RGGI region supports the notion that the RGGI region might have experienced the recession more intensely, which in turn could lead to more aggressive emission reductions. Again, we have evidence for the violation of no interference assumption.

The states with the largest weights (displayed in Table 3) are District of Columbia (0.341) and Washington (0.282), whereas the rest of the states have weights under 0.1. The prominence of these two states raises the question of whether they are overrepresented. To address this, I conduct sensitivity tests by sequentially removing the two states that contribute the most to the synthetic control. This helps to assess whether the estimated treatment effect remains robust or is highly sensitive to specific donor regions (Mideksa, 2013).

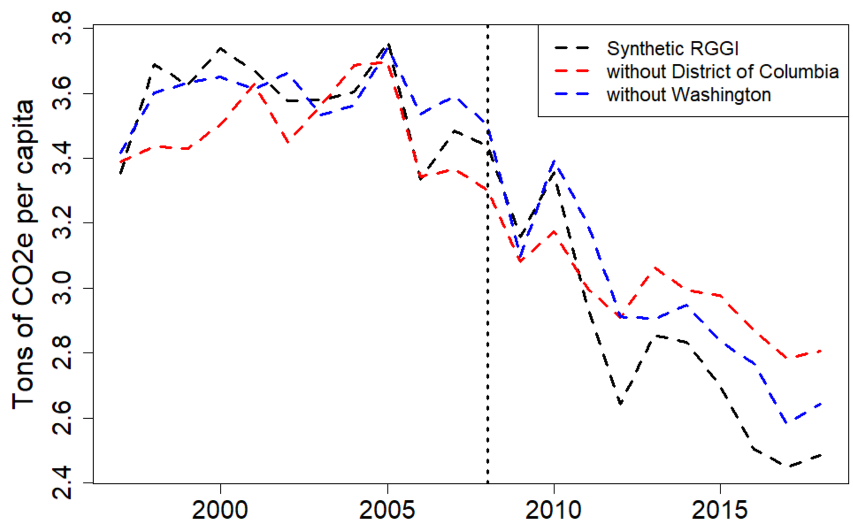

Figure 6: Sensitivity Test of Excluding States of High Weights in the Synthetic Control

Figure 6 illustrates the treatment effect of cap-and-trade when the states with the highest weights are excluded from the donor pool. By comparing the line plots, one can observe that the estimated synthetic control is robust to these state exclusions.[27]

5.1.2 Heterogeneity

Since the formation of the RGGI region does not follow a rigorous methodological approach, it is important to analyze the spread of the intervention effect by modeling effects separately for each RGGI state. Figure 12 displays each state alongside its synthetic counterpart. The degree of fit varies significantly across the treated states. The best matches are observed in Connecticut, Delaware, Maryland, Massachusetts, and New York. In contrast, Vermont is exceptionally poorly represented by the synthetic control, and the donor pool states seem to have substantially higher emissions levels than Vermont. These findings suggest that the

[27]The emission reductions from 2008 to 2018 are within a similar range, with values of -0.952, -0.498, and -0.863 (tons of CO_2 per capita) for the synthetic region, synthetic region without District of Columbia, and synthetic region without Washington, respectively.

CO_2 emission reductions may differ significantly across participating states.

In Figure 13, I present the heterogeneous effects of the program on natural gas emissions across the RGGI states. Based on my results, only Connecticut and Delaware show a clear increase in natural gas emissions, while the effect in other states appears to be relatively neutral, with only minor increases or decreases.

The varying quality of synthetic matches for heterogeneous effects calls for caution when interpreting state-level estimates, as poor pre-treatment fit may limit the validity of the estimated treatment effects. For example, Ruth et al. (2008) note that Maryland relies more heavily on coal[28] than other RGGI states and has invested considerably less in energy efficiency, indicating a critical structural difference. Maryland was also estimated to have excess allowances compared to other states, which resulted from substantial emission reductions that were partly driven by its initially high coal dependence.

5.2 Leakage and Fuel Switching

To assess spillover effects, this section evaluates whether estimated emission reductions in the electric power sector were counterbalanced by carbon leakage and fuel switching. I analyze differences across fuel types (coal, petroleum, and natural gas) and present results related to cross-regional leakage.

Figure 14 shows that in the RGGI region, petroleum and natural gas emissions have increased relative to the synthetic control, while emissions from coal have decreased. This indicates a fuel switching effect within the participating states. For leakage effects, I build on the findings of Fell and Maniloff (2018) and Chan and Morrow (2019) of cross-regional leakage. Both studies primarily focus on leakage into Pennsylvania and Ohio[29], although Chan and Morrow

[28] Main fuel sources for power generation consist of coal (56%), nuclear (28%), petroleum (7%), natural gas (4%), hydro (2%), and wood waste (2%) (Ruth et al., 2008).

[29] This restriction is justified by their roles as large electricity-producing states bordering the RGGI region, and their direct connection through shared transmission networks (Chan & Morrow, 2019; Fell & Maniloff, 2018).

(2019) also extend the analysis to other PJM states to capture broader regional leakage.[30] Instead of focusing solely on a limited set of states, I examine all PJM Interconnection states that are not part of the RGGI. While Chan and Morrow (2019) suggested that the observed increase in emissions may be concentrated among less pollution-intensive firms, my analysis considers all fuel types.

In Figure 15, I demonstrate that CO_2 emissions from electric power plants in PJM states have decreased. This is an interesting finding, as it suggests that states outside the RGGI experienced similar effects as those within the program. The decline is 14.92% (1.84 tons of CO_2 per capita), which is nearly half as much as the decline in the RGGI region. Natural gas emissions in PJM region increase by 19.23%. In the RGGI region, this increase is only 4.54%, indicating that the demand from neighbouring states increased. Simultaneously, coal emissions decreased and petroleum emissions remained largely unchanged. The reduction in coal emissions are 18.96% and 72.02% for the PJM region and RGGI, respectively.

While electric power and coal emissions have also decreased in PJM states, the reductions in the RGGI region are greater, further indicating that the cap-and-trade program contributed to these reductions. Yan (2021) found a decrease in natural gas emissions within the RGGI region, whereas I found contrary evidence indicating an increase in these emissions. Moreover, the increase in natural gas emissions is more pronounced in PJM states than in the RGGI region. This may appear counterintuitive, as one might expect both the reduction in carbon-intensive energy sources and the increase in cleaner alternatives to be more substantial within the regulated region. However, it is important to note that my analysis does not cover the wide range of cleaner energy sources (e.g., wind and solar). Therefore, the true effect of the increase in cleaner alternatives remains unclear. My results align with Fell and Maniloff (2018), who argued that the reduced coal generation within the RGGI states was not offset by an increase in local gas-fired generation but rather by generation shifts to surrounding non-RGGI states. Additionally, Kim and Kim (2016) argued that if RGGI had no additional effects, there would be no deviations in fuel switching trends between the RGGI region and other states. The overall increase in natural gas emissions also suggests that

[30] Empirically, leakage into Pennsylvania and Ohio was captured using a dummy variable PO, set to 1 if a facility is located in these states, and the interaction $PO \times Post$, which estimates the post-RGGI leakage effect, which is the main coefficient of interest. The same specification was repeated using a PJM dummy PJM for non-RGGI PJM states, with an interaction term $PJM \times Post$, to analyze leakage beyond Pennsylvania and Ohio.

broader U.S. trends, particularly falling natural gas prices (Kim & Kim, 2016; B. C. Murray & Maniloff, 2015), may have influenced fuel switching. These findings also reflect the view that natural gas acts as a "bridge" fuel to a lower-carbon future (B. C. Murray & Maniloff, 2015).

5.3 Inference Tests

The systematic approach of the synthetic control method allows the estimation of whether it fails to capture the true effect of the treatment. This is done using placebo tests, which assess whether untreated placebo units show effects similar to or larger than those observed for the treated unit. In other words, I check whether the estimated effects for the treatment region are of an unusually large magnitude. These placebo tests are categorized as in-time placebos, where the treatment is assigned to a different time for the treated unit, or in-space placebos, where the treatment is assigned to another unit from the donor pool (Abadie et al., 2010; Funke et al., 2023).

5.3.1 In-Time Placebo

I begin with an in-time placebo test. Since the MOU established the initial framework for the cap-and-trade program and was signed in 2005, it may have prompted preparatory actions among the RGGI states to begin reducing emissions, as noted by (B. C. Murray & Maniloff, 2015). To account for this, I implement a new synthetic control model with the intervention year set to 2005.

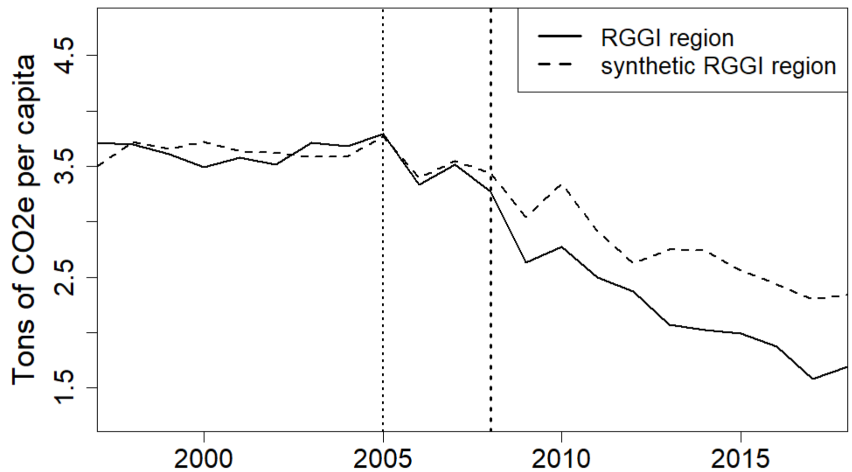

Figure 7: In-time Placebo Test for Per Capita Electricity Emission: RGGI vs. Synthetic RGGI

Figure 7 shows that from 1997 to 2005, the trend lines for both the RGGI region and its synthetic counterpart remain relatively stable with minimal fluctuations. As the first compliance period of the cap-and-trade program approaches, an interesting observation related to the assumption of no anticipation becomes evident. Between 2005 and 2007, emissions decline by 13.78% in the RGGI region and 8.68% in the synthetic region. These findings make it difficult to determine whether the MOU contributed to precautionary actions, as a decline is also observed in the synthetic region.

Lessmann and Kramer (2024) implement another in-time placebo test by setting the intervention year to 2011 to account for the economic recovery following the recession. Since the predictor for financial industry in my model is weak, my results do not inherently capture the effects of the recession. Therefore, adjusting for recession effects would likely provide no significance to my results.

5.3.2 In-Space Placebo

Next, I implement a comprehensive set of in-space placebo tests similar to that of Abadie et al. (2010) and Cunningham and Shah (2018). The proposed placebo test builds on the framework of permutation inference, where the distribution of a test statistic is determined by randomly assigning sample units to intervention and non-intervention groups. Similar to this, I apply the synthetic control method to each potential state within my sample. More specifically, I assign the method to each state in the donor pool, excluding those subject to the RGGI or California's cap-and-trade program. Synthetic controls are then constructed for these placebo-treated states as if they had experienced the intervention in 2008. As previously described, this approach allows for evaluating whether the intervention effect estimated for the treated unit is of greater magnitude than the effects estimated for randomly selected sample units.

The mean squared prediction error (MSPE) quantifies how well the synthetic control replicates the observed outcomes of the treated unit during a specified time period. It is calculated separately for the pre-treatment and post-treatment periods for all placebo units as

$$\text{MSPE}_{\text{pre}} = \frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{1t} - \hat{Y}_{1t}^N\right)^2 \quad \text{and} \quad \text{MSPE}_{\text{post}} = \frac{1}{T - T_0}\sum_{t=T_0+1}^{T}\left(Y_{1t} - \hat{Y}_{1t}^N\right)^2.$$

If the in-space placebo tests create pre-intervention line patterns that closely align to that of the actual treatment region, it suggests that the placebo units and the actual treatment unit are comparable based on their pre-intervention characteristics. In the post-intervention period, if the intervention had a similar effect on the placebo units, we would expect to see downward line patterns, or gaps[31], comparable to that of the actual treatment unit. Therefore, if the in-space placebo tests demonstrate that the post-intervention gap for the RGGI region is significantly greater than those for states without a cap-and-trade program, it provides strong evidence that the cap-and-trade program has contributed to a reduction in electric power emissions in the Northeastern states.

Figure 16 presents placebo estimates for all states, revealing one clear outlier, Wyoming, with pre-intervention MSPE value of 1541.68. Aside from this exception, the gaps for the RGGI region and the control states seem to hover relatively close to zero, both before and after the intervention, but it is difficult to draw further conclusions from this figure. The pre-intervention MSPE for RGGI is 0.02, whereas the median pre-intervention MSPE for

[31] A gap like this is demonstrated in the gap plot in Figure 5; for the corresponding confidence intervals, see Figure 19.

other states is 0.43. The average pre-intervention MSPE for other states is 39.15, which suggests it necessary to exclude the outlier from the figure to improve clarity and provide a more accurate graph.

Figure 17 displays all states except Wyoming. Based on this figure, the synthetic control method does not seem to construct an adequate representation of all placebo units. In other words, large and systematic deviations in the gaps for the placebo states in the pre-intervention period suggest that the observed trends in the placebo states are not well-captured by their synthetic counterparts. This suggests refining the focus to include only placebo units that are closely matched by their synthetic counterparts, allowing for a clearer comparison to determine if the emission reductions in the RGGI region are significant.

If the synthetic RGGI region failed to accurately replicate the per capita electric emissions of the real RGGI region before the cap-and-trade program, we would have attributed much of the post-cap-and-trade gap between the real and synthetic RGGI regions to the poor pre-treatment fit, rather than the impact of the cap-and-trade program itself. Similarly, placebo tests with poor pre-treatment fit cannot indicate how uncommon it is to observe a large post-treatment gap for a state that was well-fitted before the cap-and-trade. For this reason, I generated an additional version of a placebo gap plot, excluding states based on their pre-treatment MSPE levels (Abadie et al., 2010).

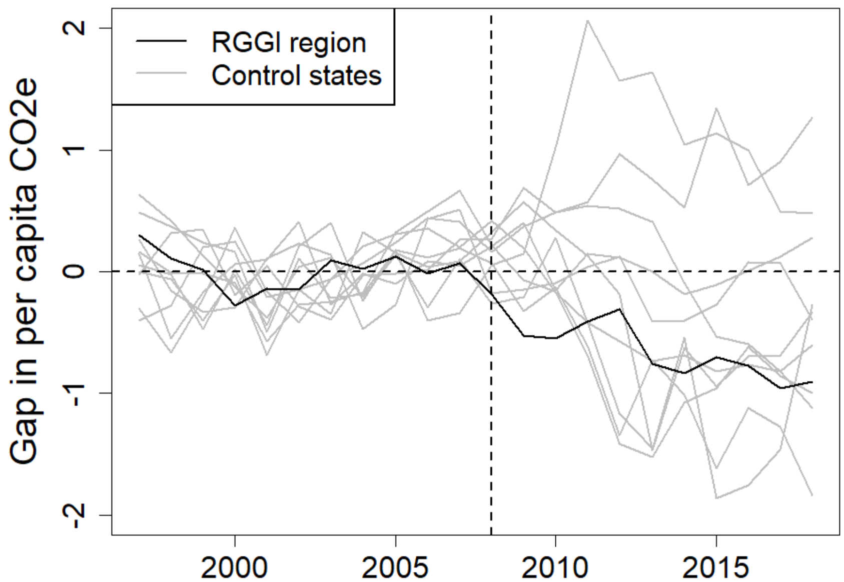

Figure 8: Per Capita Electricity Emission Gaps in RGGI and Placebo Gaps in 10 Control States (Discards States with Pre-Cap-and-Trade MSPE Five Times Higher than RGGI Region's)

In Figure 8, the comparison is limited to states whose synthetic units replicate the fit prior to the treatment. The y-axis is compressed compared to the earlier figures for better visualization. The pre-treatment MSPE for RGGI is 0.024, while the next smallest pre-intervention MSPEs are 0.053 for Michigan and 0.056 for Hawaii. Because there are no states that closely align with the RGGI region's pre-treatment MSPE value,[32] it is important to note that, prior to the intervention, the treated unit aligns more closely with its synthetic control characteristics than most of the placebo states.[33] Therefore, the synthetic control method fails to generate accurate counterfactuals for the placebo states with the given set of predictors.

When comparing the gap for the RGGI region to the gaps for the remaining 10 control states in Figure 8, the actual treatment gap does not appear highly unusual. To further assess the relative magnitude of the treatment effect, I examine the MSPE ratios, which provide the ratios of post- to pre-intervention MSPEs, thereby indicating the extent to

[32]Close alignment refers to a pre-treatment MSPE that is at most twice as high, as proposed by Abadie et al. (2010).

[33]Rest of the placebo states in the figure include South Carolina, Ohio, North Carolina, Louisiana, Pennsylvania, Colorado, Georgia, and Indiana.

which the intervention affects each unit. Figure 18 shows that Nebraska has the highest MSPE ratio of 58.01, followed by Arkansas with 30.47. The RGGI region has the third-highest MSPE ratio of 20.81, closely followed by Ohio (19.89) and Louisiana (15.36). To better understand the significance of these ratios, it is important to consider the difference in trends among states. Figure 8 indicates that while some states experience rising emissions after the intervention, others, such as the RGGI region, show declining emissions. However, this analysis does not reveal whether the decline in the RGGI region is substantial compared to other states experiencing similar downward trends. Consequently, states are ranked based on the frequencies of their post/pre-MSPE ratios and the directions of their gap lines.[34] This approach also ensures the validity of the findings by testing for statistical significance relative to random assignments.

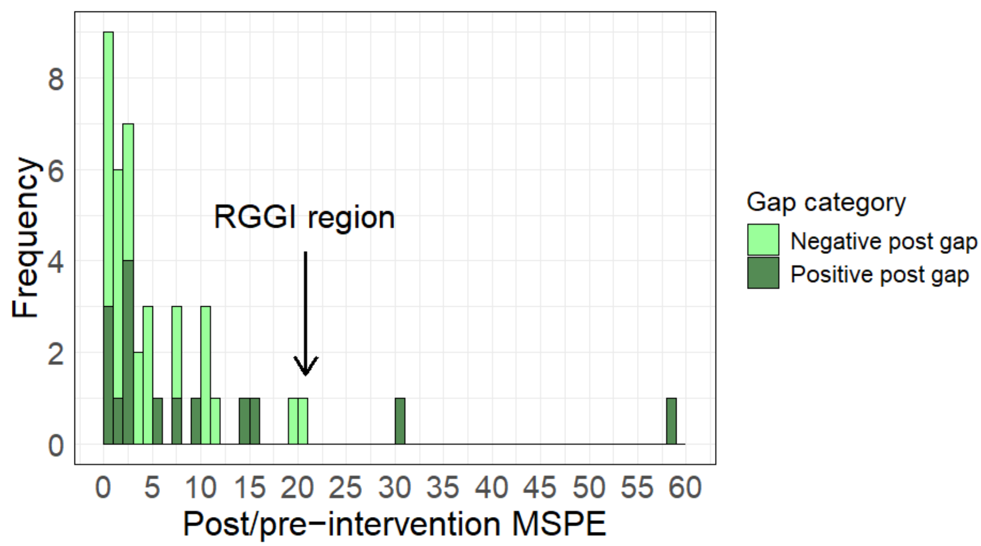

Figure 9: Ratio of Post-Intervention MSPE and Pre-Intervention MSPE: RGGI region and 41 Control States

Figure 9 illustrates the distribution of post/pre-intervention MSPE ratios. Although the ratio for the RGGI region is not uniquely distinct, it has the highest ratio among states with a negative post-intervention gap. If the intervention were assigned randomly within the data, the probability of observing a post/pre-intervention MSPE ratio at least as large as that of

[34] I take the average of all the gap values for each state over the post-intervention period. If the result is positive (negative), I interpret that there is a positive (negative) gap/direction.

the RGGI region, along with a negative gap line, would be 1/24, or approximately 0.04. This can be interpreted as the p-value for obtaining an MSPE ratio as large as that of the treatment unit, conditional on the sample including only units with declining emissions. This interpretation implies that the MSPE ratio for the treatment unit is statistically significant, indicating that the RGGI has a causal effect on emission reductions.

Unlike the MSPE ratio approach, which assesses the quality of the synthetic control model by comparing its fit before and after the intervention, this final method focuses solely on the post-intervention effects. It directly tests whether the treated unit's outcome is extreme relative to the distribution of placebo units, irrespective of the models' pre-intervention fits. This approach does not consider the fact that some placebo units with larger post-intervention effects may have poorer pre-intervention fits. As a result, their comparability to the treated unit may be overstated. I apply this inference test to all secondary synthetic control models discussed in this study.[35] A summary of all these secondary results is presented in Table 4.

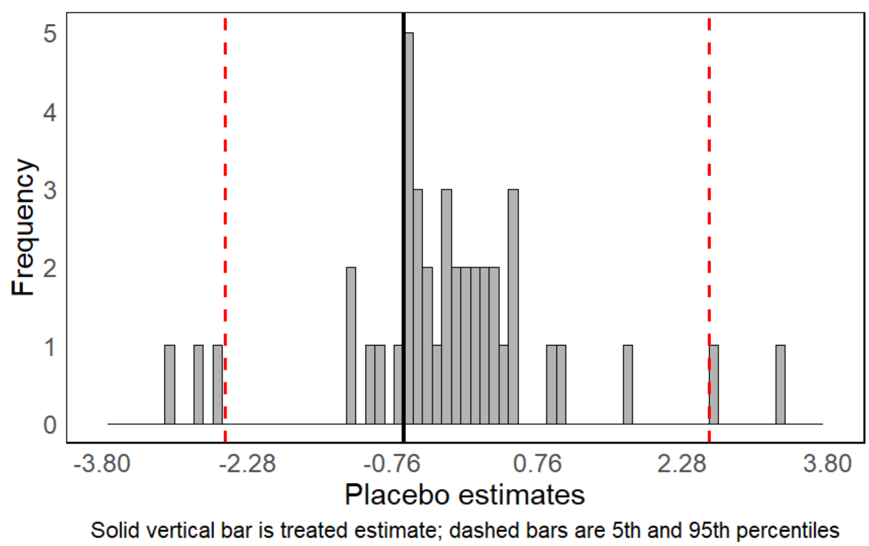

Figure 10: Ratio of Post-Intervention MSPE and Pre-Intervention MSPE: RGGI region and 41 Control States

Figure 10 displays the post-intervention gap distribution for primary results. The 5th and 95th percentile bounds serve as confidence intervals. Since the actual treatment effect falls within this range and the p-value is 0.26, the intervention effect is likely due to random

[35]Here again, I take the average of all the post-intervention gap values for each treated unit.

chance. Based on this inference approach, the intervention effects are not statistically significant for either the primary or secondary results.

5.4 Discussion

In this section, I briefly address research questions (1)–(3), outline the main limitations of the study, and provide suggestions for further research.

Although emissions decreased in both the RGGI region and its synthetic counterpart before the implementation of the cap-and-trade program, and both continued to follow a downward trend afterward, emissions in the RGGI region were still approximately 31.82% lower than in the synthetic region. Therefore, (1) the RGGI led to emission reductions. There was also (2) clear evidence of fuel switching and leakage, although these findings were not examined as thoroughly as the primary results.

As mentioned before, my primary analysis revealed a very limited representation of the financial industry. A more comprehensive identification of predictors related to the financial sector could improve the model. This would also allow for the in-time placebo test to be conducted for the financial crisis, potentially yielding more robust evidence of its effect. In general, the uneven weight distribution of my primary results could be investigated through a robustness test, assessing the sensitivity of the reported results to the inclusion of additional predictors for electric power emissions (Mideksa, 2013).

The heterogeneity analysis suggested that more detailed research approaches are needed to account for variations in industrial composition, energy portfolios, and baseline emission levels when evaluating the causes of variation in outcomes and the overall effectiveness of a cap-and-trade program.

I evaluated the inference of the models with two approaches. The MSPE ratio approach, which was only utilized for the primary results, evaluated the actual treatment effect relative to placebo-treated effects while accounting for model fit before the intervention, thereby strengthening causal interpretation when the pre-intervention fit is good. The MSPE ratio for the RGGI region was the largest among states with declining emissions and had a significant p-value, which indicates that the RGGI had a causal effect in reducing emissions. However, the model failed to accurately replicate the pre-intervention characteristics of the placebo-treated states. Further research should explore this issues by combining empirical work and theory that incorporate both environmental and economic constraints (such as DICE) to improve the identification of predictors for an accurate model fit. The second approach, which was implemented on both the primary and secondary results, tested whether the post-intervention outcome is extreme without considering pre-intervention model fit. This approach did not yield strong causal evidence for any of the models. Due to the fragmented approach and reliance on average values, further research should structure the model more carefully to better enable the use of standard statistical testing tools, as demonstrated by Cunningham and Shah (2018).

All the violations regarding the assumptions and inference tests raise concerns about the accuracy of the model. Thus, (3) it is easy to argue that the synthetic control method does not accurately model the emission reduction effects. However, the skepticism should primary focus on the fragmented research design or the application of the method in this particular context, rather than the method being inherently flawed. This again emphasizes the need for further empirical research on this topic.

The interaction between carbon pricing and complementary policies warrants further research, as broad sectoral coverage alone does not guarantee policy success without proper coordination (Lessmann & Kramer, 2024). Moreover, the lack of existing studies comparing ETSs to other mitigation instruments limits policymakers' ability to evaluate the relative effectiveness of different policy tools ("Does carbon pricing reduce emissions? A review of ex-post analyses, author=Green, Jessica F", 2021). These interactions also give rise to the waterbed effect, which is important to understand in order to assess the overall efficiency and unintended consequences of overlapping climate policies (Lessmann & Kramer, 2024; Perino et al., 2019b). On a larger scale, further research should explore scenarios with complementary economic approaches, such as climate clubs. These initiatives can help prevent possible free-riding, as nations face limited incentives to act alone W. Nordhaus (2019); Paltsev et al. (2007). Additionally, it is important to assess whether regional programs can contribute to nationwide or even global emissions reductions (Bayer & Aklin, 2020). This could be done by expanding access to global plant-level data to enable a more precise assessment of global leakage effects.[36] Finally, the lack of detailed data on renewable energy poses a significant

[36] My current estimates rely on indirect inference rather than direct plant-level emission tracking.

limitation, restricting the ability to further analyze fuel switching dynamics, renewable energy profitability, and other uncertainties associated with green transition in both the short and long term. Renewable energy policies are typically more effective in fostering green innovation in countries with liberalized energy markets (Nesta, Vona, & Nicolli, 2014), which suggests a potential research approach for assessing this relationship by comparing markets with different levels of liberalization.

Overall, my results suggest that more empirical research is needed to assess the economic and climate impacts of cap-and-trade programs, especially since the existing research on the topic has limitations and even contradictory results. In addition, more attention should be given to economic implications, such as labor market effects and sector-specific outcomes, as environmental regulations can impose uneven costs on energy-intensive industries (Curtis, 2018). This is particularly important given that a central goal of cap-and-trade initiatives is to guide the transition to cleaner energy markets (Paltsev et al., 2007).

6 Conclusions

This thesis examined how environmental regulation can be aligned with economic stability, particularly focusing on a cap-and-trade program in the Northeastern U.S. The results of the synthetic control model show that while electric power emissions generally decreased, natural gas emissions increased both within the participating states and in neighboring non-participating states. This indicates the presence of fuel switching. The increase in natural gas emissions was more pronounced in non-participating states, providing evidence of cross-regional leakage. However, the similarity in emission trends between participating and non-participating states raises questions about the extent to which these effects can be attributed solely to the policy intervention. There are also differences in the effects both across states and in comparison to previous literature. No radical effects on GDP are observed, supporting the notion that well-designed regulation and carbon pricing can reduce emissions without harming economic growth. These findings align with a broader argument that free markets and economic structures can help in shaping environmental outcomes.

Previous research suggests that GHG emissions pose a significant threat to life on earth, and a substantial body of literature emphasizes various economic approaches as a solution.

Under these conditions, investment in clean and renewable energy appears to be the optimal long-term strategy in all events, contributing not only to emissions reductions but also to sustained economic growth. The notion of comparative advantage further supports the idea that countries investing early in green technologies can benefit from leadership in these industries.

While local and regional initiatives can effectively reduce emissions in the targeted sectors without damaging the economy, their broader effectiveness remains questionable. Even though these policies can be a key part of the solution, these singular initiatives alone may not achieve the global scale of change needed to tackle climate change. Effective climate action and structural changes in energy markets will ultimately require multinational cooperation. As individual nations often have limited incentives to act independently, harmonized international policies, including coordinated carbon pricing and complementary instruments, will be essential to prevent issues like emission leakage and ensure global impact.

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**ChatGPT was used in this thesis to assist with writing, as well as LaTeX and R code refinement.*

Appendix A

All emission and energy related data was obtained from the U.S. Energy Information Administration (EIA) available at https://www.eia.gov/environment/emissions/state/

The data describing the economy and finances is available at the Bureau of Economic Analysis (BEA) under https://apps.bea.gov/regional/downloadzip.htm

Resident population data was provided by the Federal Reserve Economic Data (FRED) base at https://fred.stlouisfed.org/release/tables?rid=118eid=259194

Appendix B

Table 1: Predictor Weights in the Synthetic RGGI Region

	Weights			
Predictor	(a) Electric power	(b) Natural gas	(c) Petroleum	(d) Coal
GDP per capita	0.012	0.032	0	0
Carbon intensity of energy	0.054	0	0.001	0.142
Energy intensity of economy	0.645	0.001	0.009	0.006
Private industries per capita	0.097	0.002	0	0.047
Financial industry per capita	0	0.001	0.04	0.057
(a)-(d) emissions per capita 2000	0.030	0.604	0.494	0.485
(a)-(d) emissions per capita 2005	0.042	0.233	0.3	0.111
(a)-(d) emissions per capita 2008	0.121	0.128	0.156	0.152

Table 2: Per Capita Electric Emission Predictor Means for RGGI Region

	RGGI region	Synthetic	Average of 41 control states
GDP per capita	1.025	1.025	1.026
Carbon intensity of energy	54.283	54.173	61.992
Energy intensity of economy	3.625	3.626	6.418
Private industries per capita	0.008	0.008	0.010
Financial industry per capita	53.580	0.014	0.009
Electric emissions per capita 2000	3.488	3.739	13.456
Electric emissions per capita 2005	3.789	3.760	13.451
Electric emissions per capita 2008	3.267	3.437	12.984

Table 3: State Weights in the Synthetic RGGI Region

State	Weight	State	Weight
Alabama	0.003	Nebraska	0.005
Alaska	0.015	Nevada	0.010
Arizona	0.005	New Jersey	0.072
Arkansas	0.004	New Mexico	0.004
Colorado	0.009	North Carolina	0.012
District of Columbia	0.341	North Dakota	0.000
Florida	0.007	Ohio	0.008
Georgia	0.007	Oklahoma	0.003
Hawaii	0.009	Oregon	0.002
Idaho	0.005	Pennsylvania	0.007
Illinois	0.009	South Carolina	0.004
Indiana	0.005	South Dakota	0.006
Iowa	0.006	Tennessee	0.006
Kansas	0.005	Texas	0.004
Kentucky	0.003	Utah	0.005
Louisiana	0.001	Virginia	0.010
Michigan	0.086	Washington	0.282
Minnesota	0.010	West Virginia	0.001
Mississippi	0.003	Wisconsin	0.014
Missouri	0.007	Wyoming	0.000
Montana	0.003		

Table 4: Confidence Intervals for Outcome Variable Gaps

	Outcome variable				
	(a) Electric power	(b) Natural gas	(c) Petroleum	(d) Coal	GDP
RGGI region					
Average treatment gap	-0.641	0.308	0.942	-0.795	-0.005
5th percentile	-2.589	-0.883	-1.265	-2.970	-0.009
95th percentile	2.598	2.115	1.386	3.134	0.015
p-value	0.262	0.375	0.125	0.282	0.344
PJM region					
Average treatment gap	-1.478	0.565	-0.252	-1.357	-
5th percentile	-1.526	-0.914	-1.929	-2.781	-
95th percentile	2.700	2.512	1.746	2.385	-
p-value	0.125	0.313	0.250	0.219	-

Note: 'Average treatment gap' refers to the average difference between the treated unit and its synthetic counterfactual during the post-treatment period. '5th percentile' and '95th percentile' represent the bounds of a 90% confidence interval, constructed from the distribution of average post-treatment gaps obtained by applying the same synthetic control method to placebo-treated units. 'p-value' denotes the proportion of placebo estimates on the same side of the distribution as the treated estimate, indicating a stronger post-intervention effect; It represents the probability of obtaining an estimate at least as extreme as the placebo-treated unit.

Appendix C

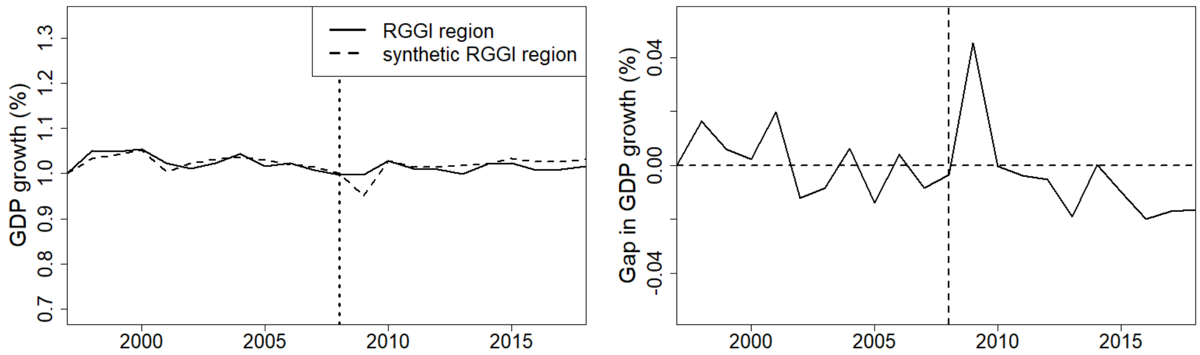

Figure 11: Trend Line and Gap Plot in GDP Growth

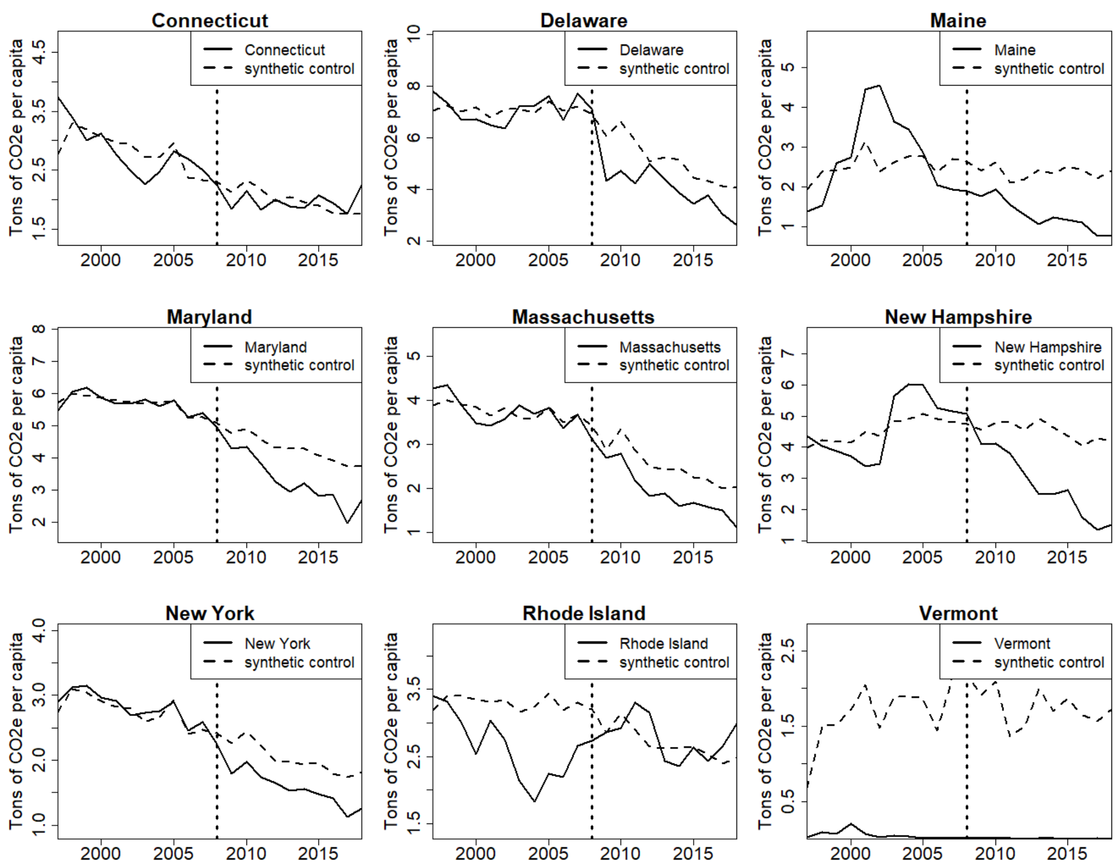

Figure 12: Heterogeneity: Electric Power Emissions Across the RGGI Region

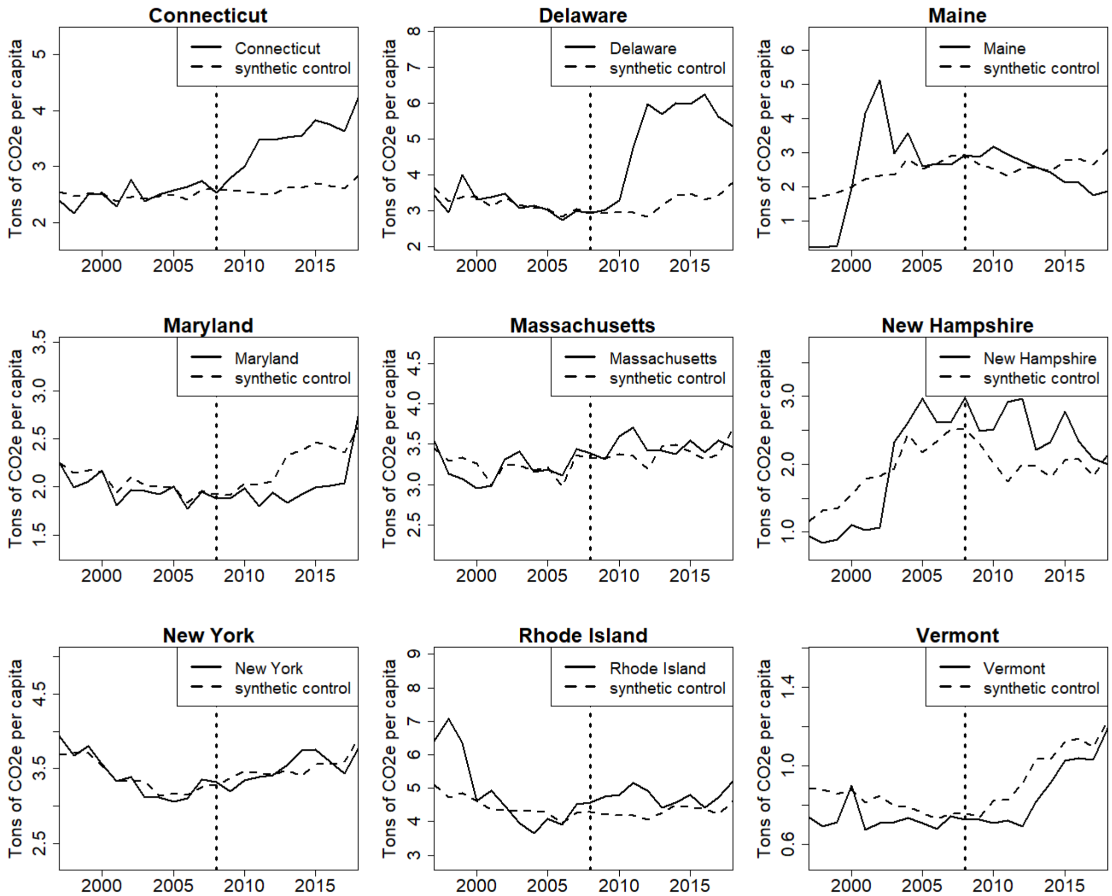

Figure 13: Heterogeneity: Natural Gas Emissions Across the RGGI Region

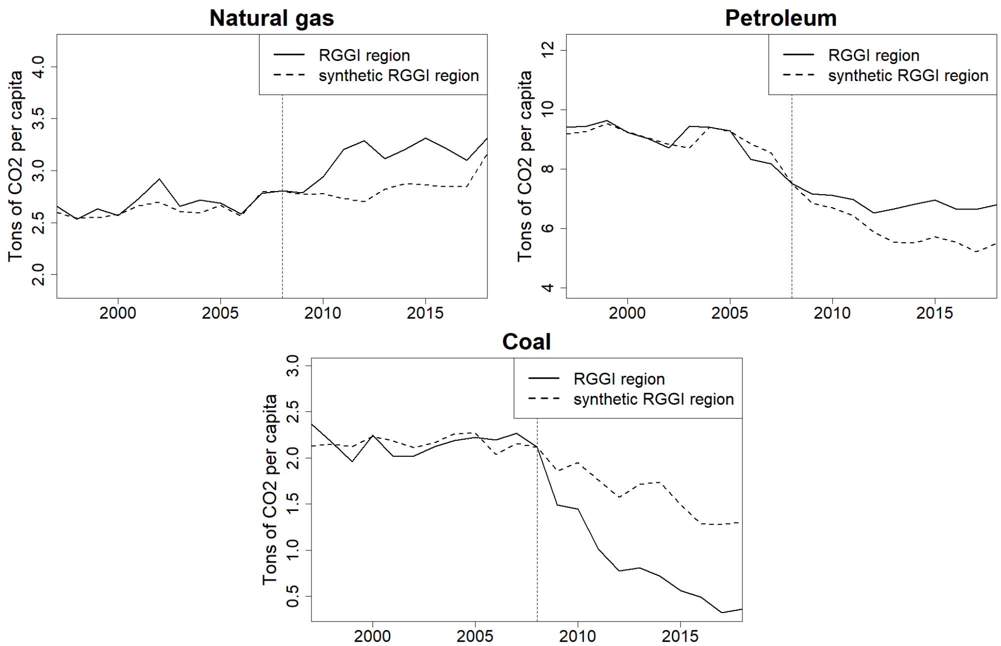

Figure 14: Trends in Per Capita Fuel Switching: RGGI vs. Synthetic RGGI

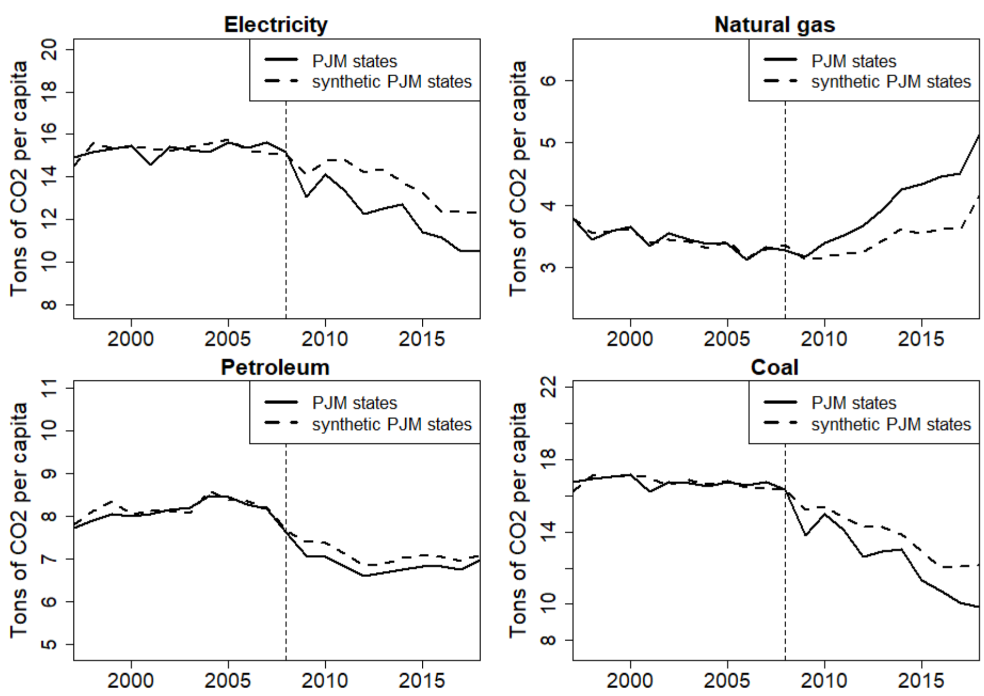

Figure 15: Trend Lines in Per Capita (a)-(b) Emissions: PJM States vs. Synthetic PJM States

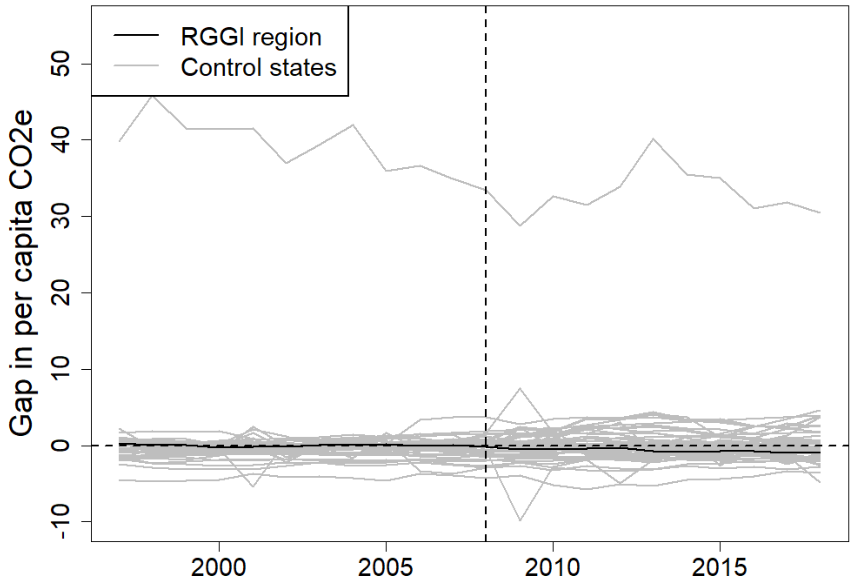

Figure 16: Per Capita Electricity Emission Gaps in RGGI and Placebo Gaps in 41 Control States

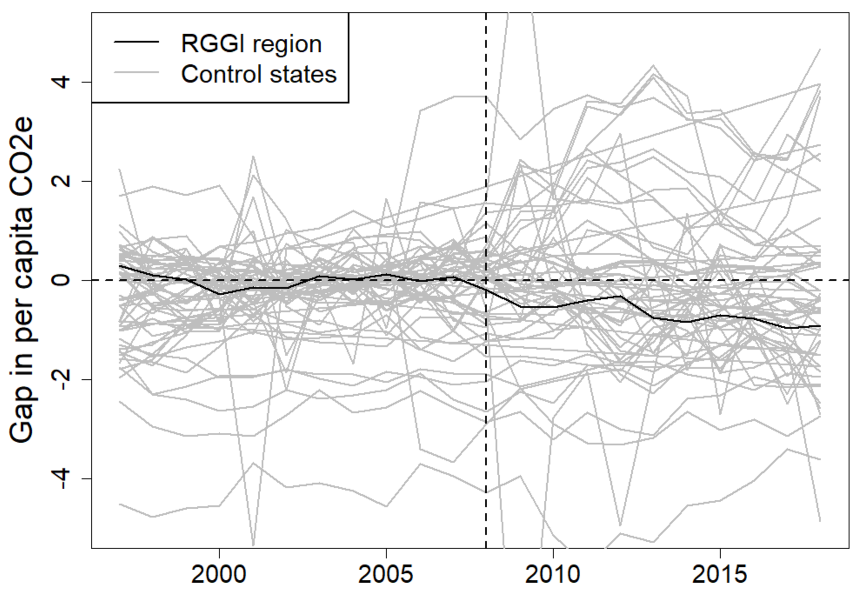

Figure 17: Per Capita Electricity Emission Gaps in RGGI and Placebo Gaps in 40 Control States (Discards Wyoming)

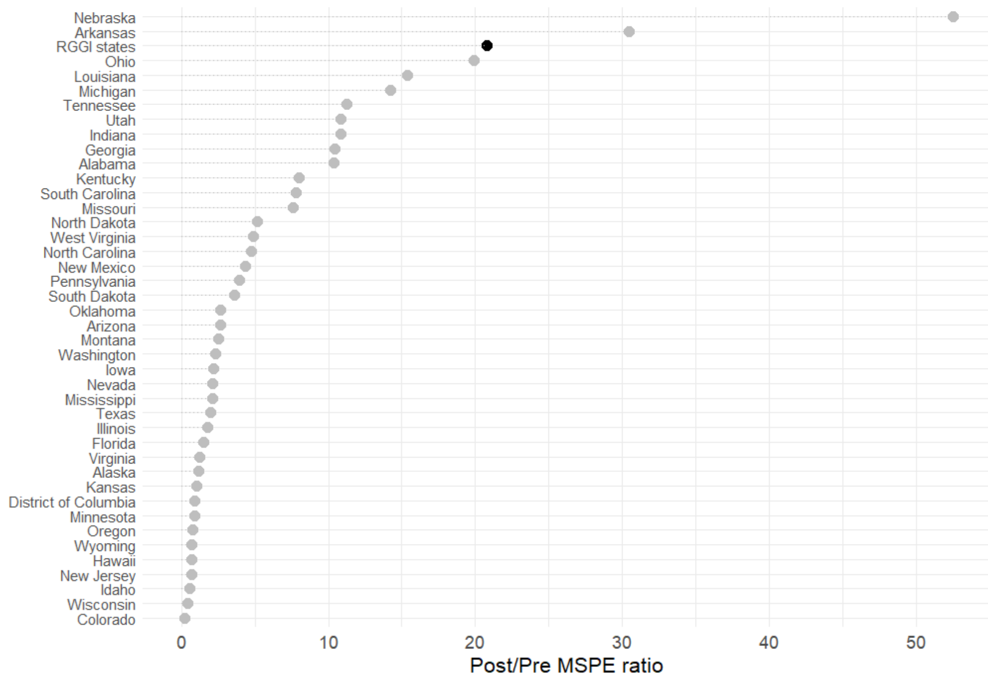

Figure 18: Ratio of Post-Cap-and-Trade RGGI MSPE and Pre-Proposition RGGI MSPE

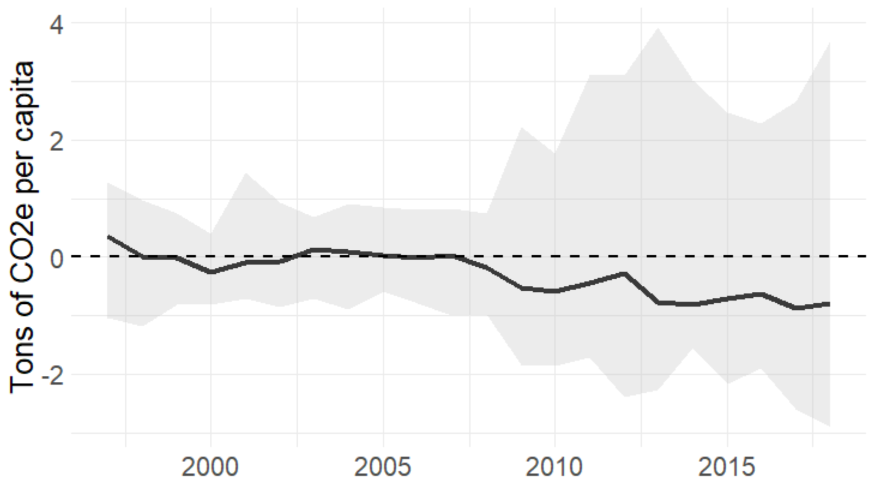

Figure 19: 5th and 95th Percentile Confidence Interval Bounds for Per Capita Electric Power Emission Gap in RGGI Region