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Research article

The effect of cap-and-trade on sectoral emissions: Evidence from California

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ABSTRACT

We study the impact of California's cap-and-trade system on carbon emissions in the electricity and industrial sectors. We use US state-level panel data covering the period 2005–2019 and apply the synthetic control method to construct an optimal counterfactual for per capita emissions in each sector. In our experiment, emissions in the power sector fall below counterfactual emissions by 48%. In the industrial sector, the state's emissions are 6% higher than those of the synthetic control unit by the end of the observation period. Thus, cap-and-trade failed to deliver decarbonization across both sectors. While the abatement in the power sector was facilitated by complementary policies and driven by a switch from natural gas to renewables, California's policy mix has disincentivized emission reductions in the industrial sector.

1. Introduction

While global emissions are still rising and the chances of not surpassing 1.5 degrees Celsius of global warming are shrinking, the debate about the appropriate measures to tackle climate change remains unresolved. Most economists argue that carbon pricing is a highly effective instrument to reduce emissions.[1] Critics, however, question market-based approaches, and emission trading in particular, for being politically unfeasible and, if implemented, too weak to deliver deep decarbonization alone (see, e.g., Cullenward and Victor (2020), Lilliestam et al. (2021) and Tvinnereim and Mehling (2018)). As the empirical literature on the effectiveness of carbon pricing provides ambiguous evidence (Green, 2021), there is a strong need for more research, particularly on cases that, so far, remain understudied.

Our study contributes to this literature by estimating the abatement effects of introducing cap-and-trade in California. We use the synthetic control method (SCM) introduced by Abadie and Gardeazabal (2003) and Abadie et al. (2010) to construct a counterfactual assessing the causal effect on CO2 emissions in the electricity and industrial sectors. In our experiment, power sector emissions fall by 6% annually compared to the counterfactual over the treatment period. Emissions in the industrial sector increased on average by less than 1% annually relative to the synthetic control unit. Our results add to existing evidence suggesting that carbon trading can achieve limited emission abatement, particularly in the power sector, but has so far been less effective across multiple sectors (see, e.g., Cullenward and Victor (2020), Green (2021), Haites et al. (2023) and Tvinnereim and Mehling (2018)).

Evaluating industrial and power sector emissions allows us to make inferences about the cross-sectoral effectiveness of the measure. The general idea behind carbon trading is that emissions are mitigated where it is cheapest, minimizing the overall costs of regulation (Schmalensee and Stavins, 2017a). Regulated firms can either cut emissions or buy permits, while regulators limit emissions by capping the number of allowances. For trading schemes covering multiple sectors, this means that mitigation efforts could concentrate on one sector while emission levels prevail in others. This can be intended (e.g., when free allowances are allocated to firms in a sector particularly prone to carbon leakage) or unintended (e.g., when complementary policies cause emissions to shift from one sector to another). Our research design permits us to analyze these dynamics. Hence, we understand cross-sectoral effectiveness as the measure's capability to induce emission reductions across multiple sectors or, at least, not (intentionally or unintentionally) harm abatement in other sectors. This metric, however, must be distinguished from overall effectiveness, which refers to the net abatement effect. Thus, a policy that is not

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[1] In 2019, The Wall Street Journal published a letter signed by more than 3000 US economists proposing the introduction of a carbon tax, arguing that this is "the most cost-effective lever to reduce carbon emissions" and the only remedy in line with "sound economic principles" (Climate Leadership Council, 2019).

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effective across sectors is still effective in overall terms if its positive effect in one sector is not outweighed by its negative effect in another sector.

Assessing the relevant mechanisms, we investigate (1) whether the estimated emission reductions in the power sector were compensated by carbon leakage, (2) whether free allowances incentivized rising emissions in the industrial sector, and (3) how the cap-and-trade system was affected by complementary environmental policies. Estimations based on the SCM imply that emissions from natural gas decreased significantly in the power sector while the share of renewables increased. Furthermore, our study suggests that the combination of cap-and-trade with complementary policies in the power sector has disincentivized mitigation efforts in the industrial sector.

The paper is organized as follows. Section 2 briefly summarizes the existing literature on the impacts of carbon trading on emissions. Section 3 presents background information on California's cap-and-trade program and Section 4 our methodological approach and sample. The empirical evidence is presented in Section 5, Section 6 discusses mechanisms, and Section 7 concludes the paper.

2. Related literature

While a global attempt at carbon pricing waits to be seen, multiple regional, national, and supranational pricing schemes have been implemented worldwide. In 2023, 73 jurisdictions, representing 23% of global GHG emissions, enacted carbon pricing (World Bank, 2023). Half of them rely on emission trading. However, it is difficult to empirically estimate the effectiveness of these measures as they are often implemented together with other environmental policies, and their effects on emissions tend to be hard to distinguish from exogenous factors such as energy prices or economic shocks. Still, several studies have conducted ex-post analyses of carbon trading programs.

The European Emission Trading Scheme (EU ETS) was implemented as one of the earliest carbon markets and received most of the scholarly attention until now (e.g., Bayer and Aklin (2020), Cheze et al. (2020), Colmer et al. (2022), Dechezleprêtre et al. (2023), Jaraite and Di Maria (2016) and Schaefer (2019)). The majority of studies show that the EU ETS only had a small abatement effect on covered firms in the first trading phase but registered increasing effectiveness over time (Martin et al., 2016). Most recent estimates suggest the average treatment effect of the EU ETS on regulated emissions to range between 8 and 12% (Colmer et al., 2022; Dechezleprêtre et al., 2023). The Regional Greenhouse Gas Initiative (RGGI), a cap-and-trade system introduced in 2009 by nine northeastern US states, was the first carbon market to address CO2 emissions in the US. However, it covers only the power sector and, thus, represents a less comprehensive approach than California's cap-and-trade program. While ex-post analysis suggests that the instrument has reduced electricity emissions by approximately 20% (Chan and Morrow, 2019; Murray and Maniloff, 2015) there is also evidence for significant carbon leakage effects[2] (Fell and Maniloff, 2018; Lee and Melstrom, 2018; Yan, 2021). Ex-post evaluations of the regional carbon trading pilot projects in China have concluded that the instrument has effectively reduced emissions across multiple sectors, presenting estimates ranging from 15 to 25% (e.g., Chen and Lin (2021), Hu et al. (2020), Zhang et al. (2019, 2020a) and Zhang et al. (2020b)).

In addition to these case studies, some scholars have used cross-country data to estimate the environmental effects of pricing carbon. Best et al. (2020) estimated that the annual CO2 emissions growth rate for fuel combustion is around two percentage points lower in countries with some form of carbon tax than those without. Similar results obtained by Rafaty et al. (2020) find that introducing a carbon price reduces the annual growth rate of CO2 emissions by roughly 1 to 2.5%.

While the evidence on the effectiveness of carbon trading, in general, is biased by a large body of literature on the EU ETS, California's cap-and-trade system is particularly under-evaluated (Green, 2021). Hence, this study seeks to diversify the evidence on the effectiveness of emission trading.

So far, only a few studies directly compare the effects of carbon trading in different sectors. For some programs, a comparable analysis is unfeasible, as they only apply to the power sector (e.g., RGGI and the Chinese national trading scheme). Other carbon markets with broader sectoral coverage are less studied (e.g., New Zealand's or Korea's emission trading scheme). The only major exemption is the EU ETS. It covers, until now, electricity, some particular industries, and aviation (ICAP, 2022). Bayer and Aklin (2020) use the SCM to estimate the abatement between 2008 and 2016 in four sectors covered by the EU ETS: energy, metals, minerals, and chemicals. They find that the instrument has caused emissions to fall in all four industries by 20 to 30% without large differences in scale. Most studies, however, only study the impacts of the EU ETS on specific sectors (e.g., Löschel et al. (2019) and Schaefer (2019)) or on regulated firms (e.g., Colmer et al. (2022), Dechezleprêtre et al. (2023) and Jaraite and Di Maria (2016)), not on aggregate emissions in different sectors.

3. California's cap-and-trade program

Even though California does not sit at the table when international climate policies are discussed, it represents the 5th largest economy in the world by scale (CBS News, 2018). Therefore, it is an important player in global climate policy. The state is not just home to many of the world's largest enterprises; it is also highly affected by the consequences of global warming. In recent years, it experienced an unseen wave of devastating wildfires, floods, and extreme droughts. In response to this, its parliament passed the Global Warming Solutions Act in 2006, also known as Assembly Bill 32 (AB-32), which introduced several environmental policy measures, such as energy efficiency standards for electricity producers, a low carbon fuel standard for motor vehicle fuels, and most significantly a cap-and-trade program (Schmalensee and Stavins, 2017b). The Bill aimed to reduce GHG emissions to their 1990 level by 2020. This means a reduction of approximately 15% according to official estimates of the California Air Resource Board (CARB), which is responsible for monitoring the carbon market (CARB, 2008). While most other measures came into effect earlier, the cap-and-trade system began its operation six years later, in 2012. In its first compliance period from 2013 to 2014, only electricity generation, electricity import, and large industrial facilities[3] were covered. For its second compliance period starting in 2015, the trading scheme was extended to also cover suppliers of fuels.[4] In the same year, the CARB's chair, which is responsible for monitoring the system, already called it an official success (Environmental Defense Fund, 2015). The state reached its goal for 2020 four years ahead of schedule in 2016 (CARB, 2021a). Subsequently, the emission reduction target for 2030 was set to 40 percent below the 1990 levels in Senate Bill 32 (SB-32). Carbon neutrality is to be achieved by 2045.

Due to the comparably high sectoral coverage, California's carbon market is considered one of the most comprehensive in the world (ICAP, 2022). With its ambitious climate policies, California is often seen as a model case for environmental action (see, e.g., Mazmanian

[2] If firms face strict environmental regulations in one jurisdiction, they are incentivized to relocate to another where regulation is less stringent. This can cause emissions to shift from one place to another, undermining the effectiveness of the measure.

[3] The covered production facilities included cement, glass, hydrogen, iron and steel, lead, lime manufacturing, nitric acid, petroleum, and natural gas systems, petroleum refining, and pulp and paper manufacturing (ICAP, 2022).

[4] The covered fuels include natural gas, petroleum gas, and reformulated blendstock for oxygenate blending and distillate fuel oil (ICAP, 2022).

et al., 2020; Meckling et al., 2017; Pahle et al., 2018). However, empirical evidence on its effectiveness remains scarce (Green, 2021). It is still unclear what share of the state's emission reduction can be attributed to carbon trading.

To our knowledge, no study has so far attempted to estimate the sectoral emission abatement induced by the cap-and-trade program. Mascia and Onali (2023) apply a Difference-in Difference (DiD) design to assess the impact on aggregate emissions but fail to identify an effect. Using a differential trend-break model, the impact on regulated firms was quantified by Hernandez-Cortes and Meng (2023). They find that CO2 emissions have fallen by 9% annually between 2012 and 2017 compared to unregulated firms. Making several restrictions, they exclude all firms which are also subject to other environmental regulations. This largely rules out interaction effects; however, the firms considered in their analysis are only responsible for 5% of total emissions, questioning the external validity of the results. Mastrandrea et al. (2020) use index decomposition methods assessing to what degree economic activity, environmental policies, and market forces have contributed to emission reductions. Their findings suggest that the financial crisis has played a major role in reaching emission targets early, while climate policies have become increasingly important over time. They do not, however, estimate how single policies have contributed to emission abatement. Therefore, the controversy about the instrument's effectiveness remains unresolved. Our study builds on the existing literature to develop empirically grounded evidence on the effects of California's cap-and-trade program on sectoral emissions.

4. Method & data

In recent years, California was not the only state able to reduce its carbon emissions. In fact, between 2012 and 2019, more than half of all US states registered falling emissions.[5] Consequently, we should not simply attribute the decrease in emissions observed in California to any environmental policy, let alone to a particular one. Hence, the methodological challenge is to build a reliable counterfactual. Methods relying on a business-as-usual scenario do not consider general trends in emissions and are therefore not viable to assess the effect of single policies. Measuring the instrument's performance by looking at emission reduction targets the state has set is also highly problematic. The AB-32 defined the aim of reducing GHG emissions to their 1990 level by 2020. While California peaked in 2004, the referred-to threshold was passed in 2016 (CARB, 2018). This early achievement might have also been due to recession effects (Mastrandrea et al., 2020).

Consequently, the impact of the cap-and-trade system seems to be at least questionable and needs to be further scrutinized. Comparing California to the average of all other US states is problematic as well since most states have a very different industrial structure and rely on a different energy mix. Even before the introduction of cap-and-trade, California registered much lower emission levels than the average US state in both sectors under examination (see Fig. 1). For instance, emissions in the electricity sector fell considerably for the sample mean comprising all US states between 2005 and 2012, while California's emissions only fell moderately. However, we are interested in comparing California to states that reveal a similar level of emissions and similar pre-treatment trends. Therefore, we apply the SCM.

The method provides multiple advantages in our case. First, the counterfactual is constructed to minimize the distance function between the pre-treatment outcomes of the treated and the synthetic control unit. This reduces the selection bias, as the comparison unit is not selected by the researcher but is estimated algorithmically. Second, the method is highly transparent, as both the weights for the control units and the weights for the predictors can be revisited. A third advantage is the possibility of integrating covariates, which are then considered in calculating the weights. We use five covariates: (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 the financial industry per capita.[6]

We use US state-level panel data from 2005 to 2019. California's cap-and-trade system was enacted in 2012.[7] Hence, the pre-treatment period spans over seven years. The sample ends in 2019. Although newer data is already available, we did not include it in the analysis as we intended to exclude possibly distorting effects of the COVID-19 pandemic. However, this means that long-term effects can neither be analyzed nor predicted with this sample. Our data includes all 50 US states (excluding the District of Columbia), although not all of them are considered in the donor pool. First, we excluded Hawaii to prevent far-fetched comparisons.[8] In the analysis of the power sector emissions, we also excluded all states participating in the RGGI program, as they have implemented emission trading in the sector as well (Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Rhode Island, and Vermont).[9] The remaining 38 states represent the donor pool for the evaluation of electricity emissions. As RGGI does not regulate the industrial sector, we include the full sample of 48 states in the analysis of industrial emissions.[10] In general, we used the largest possible sample and only included essential restrictions as California is incomparable with most states in terms of CO2 emissions (see Fig. 1). A larger sample, thus, allows us to find a better pre-treatment fit.

We estimate the effect on per capita CO2 emissions measured in metric tons per person for the power and industrial sectors. In 2013, when the first compliance period of the program started, the two sectors made up 33% of the state's total emissions.[11] All variables, except those measuring energy and carbon intensity, are expressed in per capita terms, making the treated unit more comparable.[12] We use four years of lagged outcome variables as additional predictors to facilitate finding a well-fitted counterfactual. All other predictors are averaged over the entire pre-treatment period.

To check the results' validity, we conduct placebo tests in time and space as proposed by Abadie et al. (2010): For the in-time placebo test, we assign an alternative treatment year to check whether the results are bound to the timing of the instrument or whether the effect also appears when a fake treatment year is set. In such a case, we cannot identify a causal link between the measure's implementation and the effect. For the placebo test in space, we construct synthetic controls for all (untreated) units in the donor pool and compare their performance with

[5] Calculations based on own dataset.

[6] We choose covariates (1), and (2) to find a counterfactual that best resembles California in terms of reliance on fossil fuels. The covariates (3) and (4) serve to prevent the algorithm from creating a counterfactual that does not match California's economic structure. Covariate (5) controls for disturbing effects of the financial crisis.

[7] The first compliance period started in 2013. The first auctions and the monitoring, however, already started in 2012. Thus, we assume that firms were already adjusting to the measure, making 2012 the more suitable treatment year.

[8] Despite its geographical disparities, we did not exclude Alaska. Similarly to California, it relies strongly on natural gas for electricity generation.

[9] Virginia is included even though it is part of the RGGI since it only joined in 2021.

[10] The results do not strongly depend on RGGI states as control units. We receive similar results when RGGI states are excluded from the donor pool. Still, we include them in the main specification to achieve a better pre-treatment fit.

[11] Own calculation, based on data from US Energy Information Administration.

[12] California's economy is the largest in the country in absolute terms. So, the values would exceed those of most comparison units if the variables were not in per capita terms. Thus, the transformation of data helps us to create a suitable counterfactual. Additionally, per capita values allow us to control for different population trends, which potentially impact overall emissions.

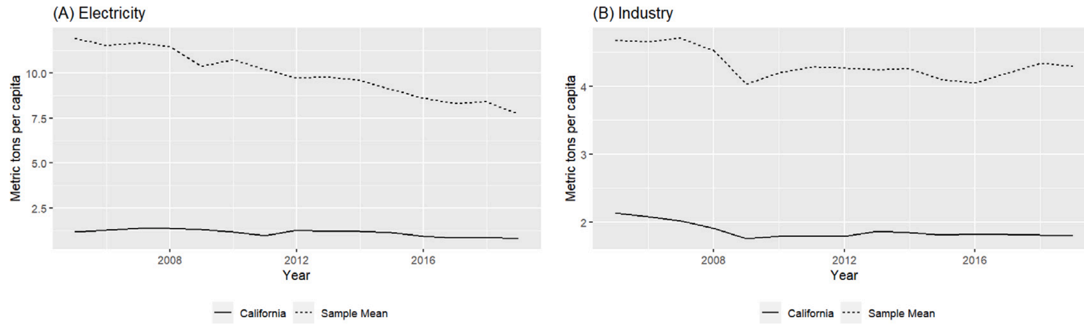

Fig. 1. CO2 emissions for electricity and industrial sector 2005–2019: California versus sample mean.

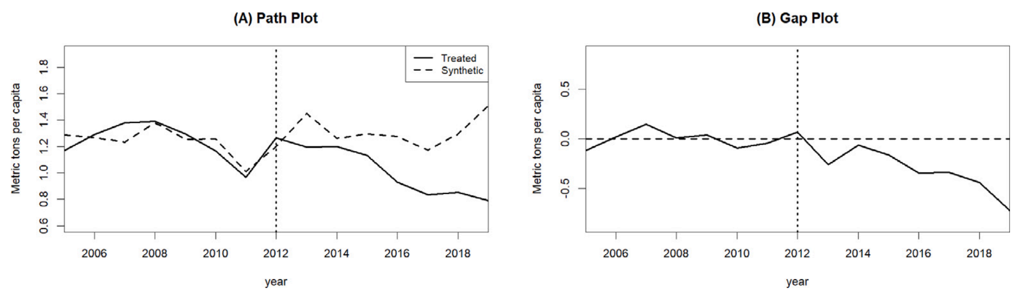

Fig. 2. CO2 emissions in the electricity sector 2005–2019: California versus synthetic California.

the effect for the (actually) treated unit. Then, we calculate the mean squared prediction error (MSPE) for the pre- and post-treatment period and plot the post/pre-MSPE ratios of all individual placebo tests. A low pre-treatment MSPE indicates a good fit of the counterfactual, while a high post-treatment MSPE serves as an indicator of a large treatment effect. Thus, the MSPE of the treated unit should be particularly low for the pre-treatment period and high for the period after the intervention. The robustness of the results can only be verified if the treated unit's post/pre-MSPE ratio is one of the highest in the sample.

5. Results

5.1. Electricity sector

The power sector was in 2013, when the first compliance period started, responsible for emitting 13% of California's total CO2 emissions. Power plants were covered by the cap-and-trade program from the beginning. We estimate how the emissions for electricity production have evolved after the introduction of the measure in comparison to a synthetic counterfactual and find a strong divergence between the two trends starting in 2012 when carbon trading was enacted (see Fig. 2). In the first compliance period (until 2015) the effect is small. Afterwards, however, the gap between California's emissions and its synthetic counterpart widens strongly. From panel (A), we can also see how emissions for both units fell after 2008, which can most certainly be attributed to the recession caused by the financial crisis. While the electricity emission trend bounced back for California as well as for the comparison unit after the crisis, our results suggest that cap-and-trade has hindered emissions in California from reclaiming pre-crisis levels. However, large emission reductions in the sector were only achieved in the second compliance period after 2015. On average, we estimate that emissions fall below the counterfactual by 48% in the seven-year period, thus by 7% annually. Although one could assume that firms might have anticipated the regulation, as five years lie between the adoption of AB-32 and the start of cap-and-trade, we cannot find evidence for a substantial anticipation effect.

The synthetic control unit mainly consists of Idaho (79%), South Dakota (17%), and Alaska (4%) (see Table 3 in the appendix). Hence, the result strongly relies on one state. We conduct a leave-one-out test, as proposed by Abadie et al. (2010), where we iteratively exclude those states that have received a positive weight>0.01 from the donor pool and compare the results to those of the main specification. We can show that comparable estimations are attained when South Dakota or Alaska are eliminated, but no proper pre-treatment fit can be found if Idaho is excluded (see Fig. 10 in the appendix). This is not problematic as such since the composition of the counterfactual is based on an algorithmic decision considering the criteria we defined to be relevant in advance. Due to the low carbon intensity of Idaho's energy supply, its per capita emissions are comparable to those in California (see Table 2 in the appendix). The state generates about two-thirds of its electricity from renewable energies (EIA, 2023a).[13] The covariates are only sparsely considered as the weights are almost exclusively distributed between the lagged emission values (see Table 4 in the appendix). This is a common problem with synthetic controls. The trade-off between finding a well-suited pre-treatment fit and taking into consideration unobserved confounders is often resolved in favor of the former (Kaul et al., 2022), also because the algorithm used[14] has a tendency to do so. Abadie (2021) argues that all weights should, in practice, be chosen to best resemble the pre-treatment trajectory of the treated unit, meaning that only under this condition, should covariates be considered. Hence, disregard for the covariates does not discredit the results.

We test the robustness of the results by conducting placebo tests in time and space. First, we run the analysis for an alternative treatment year to see whether the effect is actually bound to the year of the introduction of cap-and-trade. We chose 2011 as the alternative treatment year since this was the point when emissions recovered from the recession. Running the synthetic control analysis for 2011, thus, allows us to ensure that the observed divergence is not simply caused by different recovery trajectories. The analysis shows that the results do

[13] Although renewable energy sources only supply about half of California's in-state electricity generation, a large share is generated through natural gas which is much less carbon-intensive than coal (see Fig. 8). Therefore emissions in the power sector are comparable to those in Idaho.

[14] We use the Synth package in R proposed by Abadie et al. (2011).

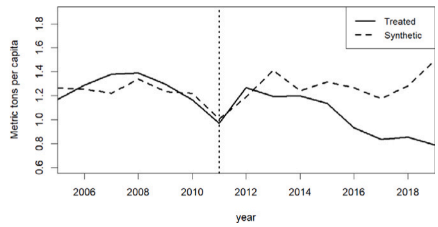

Fig. 3. In-time placebo test for electricity emissions: California versus synthetic California.

Table 1
Per capital emission ranks in full sample for states with highest post/pre-MSPE ratio.

State	Electricity	Coal
Pennsylvania	18	17
South Carolina	23	23
Alabama	8	11
Oklahoma	10	16
California	47	47

not change, and the effect still kicks in after 2012 (see Fig. 3). Thus, we can identify a link between the emission reduction and the timing of the intervention.

Second, we conduct placebo tests for all units in the donor pool and plot the results and the corresponding ranked post/pre-MSPE ratios in Fig. 4. We run the synthetic control analysis for all states for the year 2012 as if they were treated. To make reasonable inferences about the robustness of the results it is insufficient to compare the post-treatment trajectories of the treated unit with those that received fake treatment. For most units, the pre-treatment fit is much worse, causing a higher volatility in the post-treatment stage. Hence, the post/pre-MSPE ratio is crucial. It evaluates the divergence in the post-treatment period in relation to the divergence in the pre-treatment period. The results show that California ranks among those states with the highest value, having the 5th highest post/pre-MSPE ratio out of a sample of 39 states. Hence, the estimated effect is more significant than the fake treatment effect of 86% of states in the sample. This implies that the effect is not random. It is only exceeded by some states in the sample when they are assigned random treatment.

These states, however, all register much higher per capita emissions in the power sector at the time of treatment. Moreover, they relied more heavily on coal for power generation than California. This makes emission reductions easier as it allows for fuel switching from coal to a less carbon-intensive energy source such as natural gas instead of having to switch from fossil fuels to renewables. Table 1 illustrates how the five states performing best in the in-space placebo test rank for per capita electricity sector emissions and per capita emissions from coal in the full sample comprising all US states. Thus, the fact that California is outperformed by these states in the placebo test can be explained by large differences in the power mix and does not undermine the robustness of the results. It rather shows that no state with a comparable level of per capita power sector emissions exhibits a similar abatement effect when assigned random treatment.

5.2. Industrial sector

Industrial facilities emitting at least 25,000 metric tons of CO2/year are also subject to California's cap-and-trade program. Comparing our data for industrial emissions with firm-level data from CARB's Mandatory Reporting of Greenhouse Gas Emissions (MRR), we estimate that 85%–95% of emissions in the sector are covered by the regulation.[15] Consequently, we would expect total industrial emissions to decline after the introduction of cap-and-trade. In 2013, when the first compliance period began, industrial facilities were responsible for 20% of the state's total emissions. Our estimates show that emissions in the sector have not decreased relative to the counterfactual after cap-and-trade was introduced (see Fig. 5). We even find that emissions in the sector exceeded counterfactual emissions in reaction to the intervention by 6% in 2019. After 2012, emission estimates for California's industry outpace counterfactual emissions and retain a higher level until approximately 2016. Although the positive effect is small and shrinks toward the end of the observation period, it persists.

For this model, synthetic California primarily consists of Massachusetts (58%), Washington (18%), Rhode Island (16%), Connecticut (3%), and Alaska (3%) (see Table 6 in the appendix). Thus, the counterfactual, in this case, relies less on one state than in the experiment before. Conducting a leave-one-out test, we can show that similar estimations are obtained if the states forming the synthetic control unit are iteratively excluded from the donor pool (see Fig. 11 in the appendix). The control variables are also better represented in the predictor weights (see Table 7 in the appendix). The lagged outcome variables, however, still primarily determine the composition of the synthetic control unit, otherwise, it would be impossible to find a good pre-treatment fit.

The robustness of the results is, again, tested by applying placebo tests. First, we conducted an in-time placebo test (see Fig. 6). Analogous to the analysis of electricity emissions, we define 2011 as an alternative treatment year. The results show that moving the treatment year does not significantly affect the timing of the emission reduction. The divergence between the two emission trajectories is still only observable from 2012 onwards. This suggests that the effect is specific to the actual treatment year. Thus, we can reasonably assume that the outcome is an effect of the introduction of emission trading.

Second, we conduct a placebo test in space (see Fig. 7). The results reveal that the effect of the intervention on industrial emissions is small in comparison to the effect of fake treatment on other states. Evaluating the post/pre-MSPE ratios, however, shows that California ranks 4th out of 49 states in the sample. Thus, California shows a more significant effect than 92% of states receiving fake treatment allocation. Again, this implies that the effect is not random. It is even quite surprising that California ranks among the top four in this test considering that the results show a relative increase in emissions, contrary to what would have been suspected evaluating the effects of environmental regulation.

6. Mechanisms

We have shown that the introduction of California's cap-and-trade program is associated with a decline in electricity emissions, particularly in the second compliance period (starting in 2015), and a slight increase in industrial emissions compared to counterfactual outcomes. Methodologically, the results strongly rely on the construction of a particular control unit and the definition of particular covariates. Especially in the case of power sector emissions, the options for finding a well-fitting counterfactual are limited. The analysis is dominated by one state and lagged emission values for constructing the synthetic control unit. In other cases, the SCM has generated higher estimates than, for instance, DiD when assessing the abatement effects of carbon pricing (Green, 2021).

Nevertheless, the results are puzzling. Carbon trading in California has been to a certain degree effective in reducing emissions from power plants. However, considering that decarbonization needs to take place across all sectors of the economy, the fact that it was associated with a rise in emissions from industrial facilities questions its cross-sectoral effectiveness. To understand the dynamics at work and to derive policy implications we study three mechanisms that might explain our findings: we investigate (1) whether the estimated emission reductions in the power sector were compensated by carbon leakage, (2) whether free allowances incentivized rising emissions in the industrial sector, and (3) how the cap-and-trade system was affected by complementary environmental policies.

[15] The exact estimate depends on the definition of industrial activities. Since CARB and EIA do not have a common methodology, we express the outcome as a range.

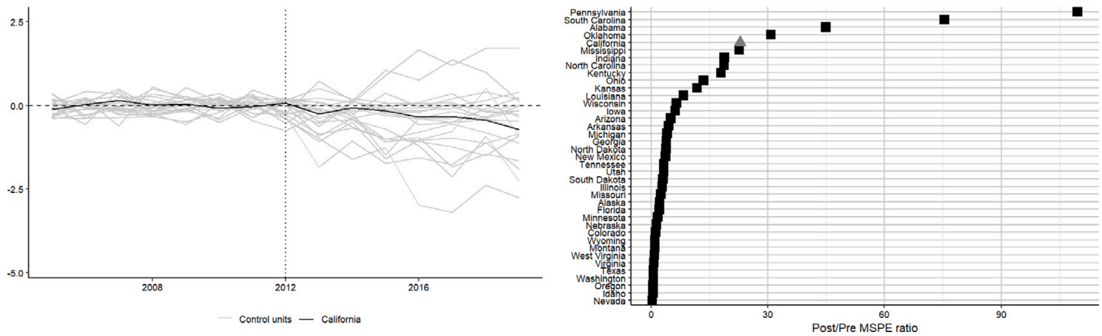

Fig. 4. In-space placebo test and post/pre-MSPE ranking for CO2 emissions in the electricity sector.

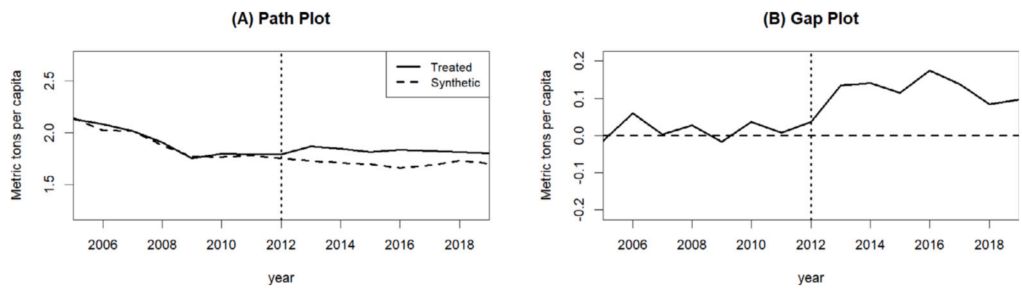

Fig. 5. CO2 emissions in the industrial sector 2005–2019: California versus synthetic California.

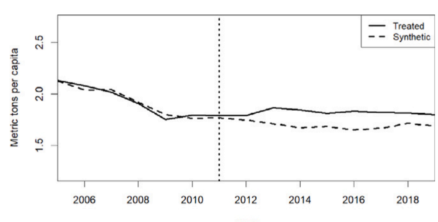

Fig. 6. In-time placebo test for industrial emissions: California versus synthetic California.

6.1. Carbon leakage

One problem concerning the effect size estimated in our analysis lies in the carbon leakage in the power sector. Regional trading schemes are particularly prone to carbon leakage as firms are usually able to easily relocate to neighboring regions (Fell and Maniloff, 2018). Power plants could have been more prone to leakage than industrial facilities due to resource shuffling, which implies that utilities rearrange contracts to import energy from less carbon-intensive energy sources (e.g. renewables or natural gas), while high carbon-intensive energy (e.g. from coal plants) is then served to other states with less strict environmental regulation. Although California's cap-and-trade program covers electricity importers and, in principle, bans resource shuffling the prohibition was relaxed in 2014, potentially allowing for carbon leakage in the power sector (Cullenward, 2014). Although researchers have modeled the potential leakage effect (see, e.g., Caron et al. (2015)) empirical estimations are difficult due to overlapping policies and market developments.

These uncertainties demand taking a closer look at how cap-and-trade affected the state's electricity mix. While carbon pricing is often associated with a fuel switch from coal to natural gas (Lilliestam et al., 2021), California has relied strongly on natural gas for electricity generation, even before cap-and-trade was enacted. Coal has only played a marginal role. Therefore, we expect mitigation to be driven by a decline in natural gas production. Data on in-state electricity generation reveals a reduction in natural gas production and, simultaneously, an uptake of renewables after 2014, when power sector emissions fell most rapidly and cap-and-trade was extended to cover transportation fuels and natural gas (see Fig. 8). While in 2012, natural gas was still used for 61% of total in-state electricity generation, this figure declined to 43% by 2019.

By conducting a synthetic control analysis for CO2 emissions from natural gas production we can show that this reduction was directly linked to the introduction of cap-and-trade. We identify a strong decline in emissions from natural gas in the treatment period (see Fig. 9). This outcome is robust to changes in the treatment year (in-time placebo test) and performs well compared to randomly assigned treatment in the control group (in-space placebo test).[16]

The decline in emissions from natural gas production could, of course, still be offset by resource shuffling. However, as this trend was accompanied by increasing in-state electricity generation from renewables, large-scale carbon leakage is unlikely. Consequently, the estimated abatement effect in the sector is, at least to an important degree, driven by an uptake of renewables and not solely by carbon leakage. Yet, we cannot rule out leakage effects entirely and need to assume that we moderately overestimate the real treatment effect.

Carbon leakage could still serve as an explanation for some of the differences in effectiveness if limited to the power sector. Bartram et al. (2022) estimate the leakage effect in the industrial sector using plant-level data and a Difference-in-Difference design. They find that regulated firms who have plants in multiple states reduce their emissions in California by 33% relative to plants in other states, but compensate for this by increasing emissions in other states by 29% compared to those firms who have no plants in California. As industrial emissions did not fall below counterfactual outcomes in our analysis, we assume that the estimated effect did not significantly impact overall emissions in the sector.

In general, we conclude that although some part of the estimated emission abatement might be compensated by increasing emissions

[16] We provide the results of the robustness tests in the appendix (see Figs. 12 and 13).

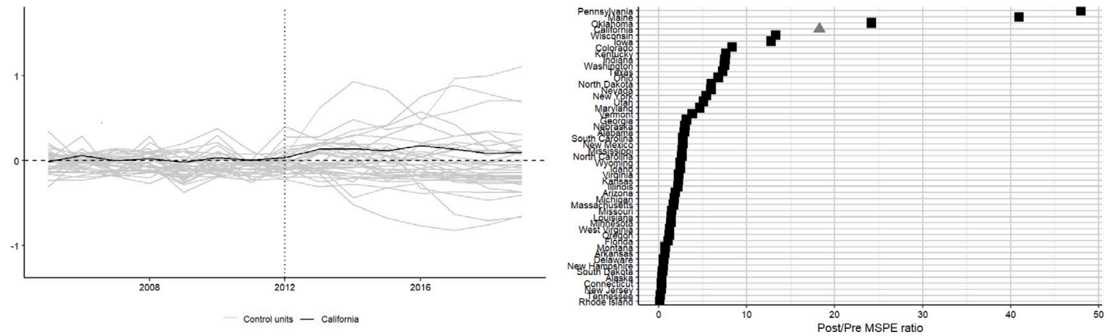

Fig. 7. In-space placebo test and post/pre-MSPE ranking for CO2 emissions in the industrial sector.

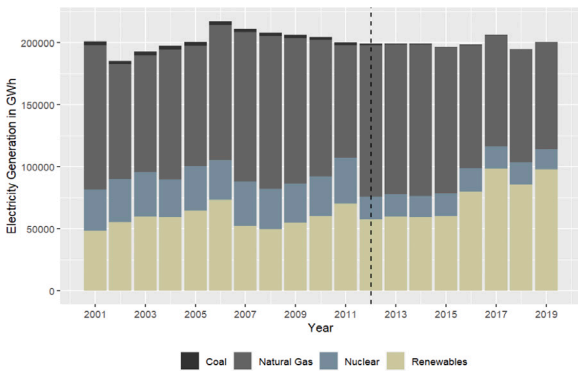

Fig. 8. In-state electricity generation by energy source in California from 2001 to 2019.

elsewhere, a significant part is induced by the increasing use of renewables in the power sector. Moreover, leakage affects not only emissions from the power sector but also industrial emissions. It can, therefore, not serve as an explanation for the one-sided effectiveness of the measure.

6.2. Free allowances

The generous allocation of free allowances might explain the positive effect on emissions in the industrial sector. Regulated entities have, in fact, received free permits to prevent carbon leakage and provide transition assistance. The amount of free allowances a firm receives is based on the estimated relocation risk, the carbon intensity of the product produced, and the level of production (CARB, 2023). Policymakers in California, thus, rely on output-based allocation (Fowlie, 2011). The average number of free allowances provided declines annually. CARB estimates that approximately 90% of all certificates were allocated during the first years of the program (CARB, 2015). Hence, firms had only a limited incentive to reduce emissions as raising production levels even guarantees a larger number of free allowances under output-based allocation. Consequently, the generous allocation of free permits might have caused low prices and thereby hindered more incremental emission reductions (Cullenward and Coghlan, 2016; Cullenward et al., 2019). However, industrial firms have not received more free allowances than electricity providers. In fact, industrial facilities even receive a smaller share of the allocated allowances even though industrial emissions make up a larger share of the state's total CO2 emissions than emissions from the power sector (CARB, 2021b). Therefore, we cannot attribute the ineffectiveness of cap-and-trade in the industrial sector to an unequal allocation of free allowances.

6.3. Complementary policies

Complementary policies might have contributed to abatement in the power sector and to a shift of emissions toward the industrial sector. California's introduction of carbon trading was linked to AB-32, which enacted multiple climate policies aimed at inducing emission abatement.[17] Out of these, only the renewable portfolio standard (RPS) directly affects emissions from power plants by requiring plants to procure 33% of their electricity from renewable sources by 2020. While the target has been met, it remains unclear to what degree the RPS and cap-and-trade, respectively, have been responsible for emission reductions. None of the additional measures have directly targeted industrial emissions. The low carbon fuel standard for gasoline (LCFS) targets life-cycle emissions in the transport sector but might have still impacted emissions in other sectors indirectly. It demands transport fuel producers and distributors to reduce carbon intensity by 10% until 2022.

Methodologically, the effectiveness of the RPS and the LCFS cannot be pinned down to a certain intervention date, as the defined quota must only be fulfilled within a larger time period. Thus, its effectiveness can solely be analyzed in terms of target fulfillment, while emission trading serves as an intervention with a clearly defined starting date. As our results show that the estimated mitigation effect is specific to the time of the introduction of cap-and-trade, we assume that the measured emission abatement was primarily induced by cap-and-trade. Even though we have not controlled for complementary policies, the states composing the synthetic control unit for electricity emissions have adopted several renewable energy policies. While Idaho does not have an RPS, it offers low-interest loans and tax deductions for energy efficiency and renewable energy projects (EIA, 2023a). South Dakota relies on a voluntary RPS and has installed other utility policies and financial incentives to promote renewable energies (EIA, 2023b). As California's complementary policies are more comprehensive, we still need to assume that the estimated effect is not solely attributable to cap-and-trade.

The RPS and other renewable energy policies related or unrelated to AB-32[18] have incentivized the uptake of renewables in the power sector and thereby reduced the costs of mitigation. Although the introduction of cap-and-trade resulted in an increase of renewables in the electricity mix, renewable energy was already abundant before 2012 (see Fig. 8). Thus, the RPS and other complementary policies can, most likely, be accredited for making renewable energy available even before carbon trading was enacted, creating favorable conditions for emission abatement under cap-and-trade. This policy sequence of implementing measures that foster the availability of low-carbon alternatives first and

[17] Four so-called "complementary policies" next to cap-and-trade are considered as most significant for reaching emission reduction targets in California : (1) tailpipe emission standards for cars and trucks; (2) Low carbon fuel standards for gasoline; (3) Energy efficiency standards for new buildings; and (4) renewable portfolio standards for electricity utilities (Mazmanian et al., 2020; Wara, 2014).

[18] California, for instance, also started the "California Solar Initiative" in 2006, introducing rebates and tax credits on solar modules (Van Benthem et al., 2008).

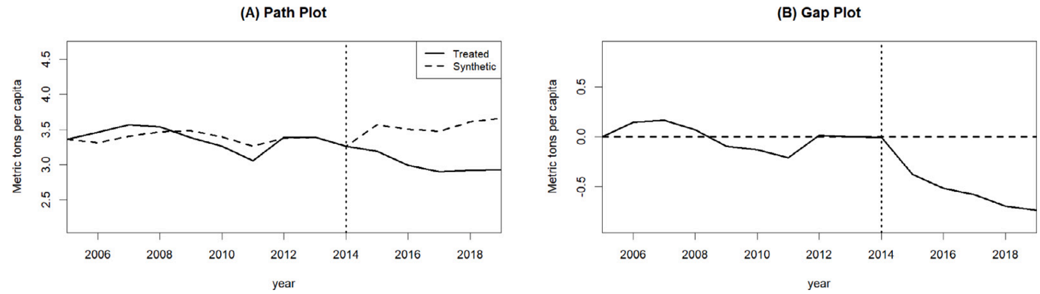

Fig. 9. CO2 emissions from natural gas production 2005–2019: California versus synthetic California.

carbon pricing second delivers deeper emission cuts (Meckling et al., 2017; Pahle et al., 2018). Thus, we presume that the estimated emission abatement in the electricity sector was induced by cap-and-trade but facilitated by complementary policies, most significantly the RPS.

From a cross-sectoral perspective, however, the combination of cap-and-trade and RPS might have caused a shift of emissions from the power to the industrial sector. This phenomenon is known as a waterbed effect (van den Bergh et al., 2021; Perino, 2018). Emission reduction induced by an RPS frees or reduces the demand for permits and, thus, lowers the market's carbon price, disincentivizing mitigation for firms in other covered sectors (Van den Bergh et al., 2013; Delarue and Van den Bergh, 2016). Hence, part of the emission abated in the electricity sector might have solely been shifted to industry. This could explain why cap-and-trade has not worked across both sectors.

7. Conclusion and policy implications

We evaluate the effectiveness of California's cap-and-trade program across the two initially covered sectors in the first seven years of its operation and are able to identify an annual abatement effect of 6% in the electricity sector. Industrial emissions increase relative to counterfactual outcomes despite carbon trading. Although the results for power sector emissions are less robust than those for industrial emissions, we provide evidence underscoring their validity. Consequently, cap-and-trade was successful in reducing emissions from electricity generation but unable to reduce emissions across both covered sectors, implying that the broad coverage of the program does not enhance its sectoral effectiveness. While complementary policies in the power sector have facilitated abatement by developing low-carbon alternatives for electricity generation, negative synergies between cap-and-trade and California's RPS might have caused a waterbed effect disincentivizing abatement for industrial facilities.

These results have important policy implications. We show that broad sectoral coverage does not necessarily cause more mitigation when adopting carbon trading. If sectors face different abatement costs mitigation efforts will, most likely, be distributed unequally. This becomes a problem when particular sectors are not reducing emissions at all. Similarly, more climate policies do not necessarily lead to more emission reduction. Complementing carbon trading with other policies targeting only a specific sector of multiple covered under cap-and-trade can cause negative synergies and harm decarbonization efforts. The effectiveness of carbon trading does not only depend on stringency but also on design. Policymakers should consider sequencing and synergy effects when developing a climate policy mix. As more and more countries and constituencies around the world develop plans to decarbonize their economy this becomes increasingly relevant.

CRediT authorship contribution statement

Christian Lessmann: Conceptualization, Investigation, Methodology, Supervision, Writing – original draft, Writing – review & editing. **Niklas Kramer:** Conceptualization, Data curation, Formal analysis, Investigation, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.enpol.2024.114066.

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