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# The Effect of Pricing Instruments on $CO_2$ Emissions

## Empirical Evidence from Australia

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## Abstract

This study investigates the emission reduction effects of a mix of market-based climate policies in Australia, where a dramatic ramp-up of incentives for renewable electricity generation was paired with a short-lived carbon tax. A synthetic control method is employed to estimate the joint effect of the policies. Contrary to the general perception in the existing literature, this study shows that the green electricity and carbon tax policies together caused a 7 percent reduction in emissions per capita from 2009 to 2018. The emission reduction impacts attenuated when the carbon price was repealed, and the renewable targets were softened. The study also finds that the policy mix did not reduce the production of Australian coal and may have expanded its export. The findings suggest that even imperfect climate change mitigation policies can have substantial and persistent effects on emissions as well as unintended consequences.

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# The Effect of Pricing Instruments on $CO_2$ Emissions: Empirical Evidence from Australia[*]

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## 1. Introduction

Economic theory suggests that carbon pricing is the most economically efficient policy instrument to mitigate climate change (Nordhaus, 1977; Poterba, 1991; Aldy et al., 2010). As of 2023, over 70 national or sub-national economies have introduced carbon pricing (World Bank, 2023). Hundreds of ex-ante studies have been carried out assessing the impacts of hypothetical carbon pricing schemes.[1] Yet a growing body of ex-post empirical evidence presents ambiguity, with some papers arguing that carbon pricing has produced only modest reductions in emissions (e.g. Green, 2021), others showing potentially substantial reductions in average emissions growth rates across jurisdictions (Best et al., 2020; Rafaty et al., 2020), and single-jurisdiction studies finding a mixture of large and small effects (e.g. Andersson, 2019; Pretis, 2022).

Emissions reductions from carbon pricing policies may have been modest - either on average or in specific jurisdictions - for a variety of reasons. It may be the case that prices levied on emissions have induced economically efficient, but small (and potentially costly) reductions in emissions. Alternatively, emissions prices may be inefficiently low due to economic concerns or political concessions, such as built-in exemptions that limit policy coverage (Rafaty et al., 2020). Moreover, carbon pricing policies may be complex, leading to delays in their implementation, or they may have uncertain futures due to political or economic reasons that lead firms to "wait-and-see" before changing input or output decisions. Finally, renewable energy policies also can take on a prominent role in promoting emissions reductions but may complicate the econometrics of estimating the effects of carbon prices. Many of these factors are relevant in the context we study.

This paper provides insights on the efficacy of real-world pricing instruments for greenhouse gas (GHG) reductions by focusing on the experience of Australia, which saw two major expansions (and subsequent adjustments to the expansions) of climate policy circa 2010. First, the ambition of Australia's Renewable Energy Target (RET) was substantially increased in 2009.[2] The RET requires entities that acquire electricity in wholesale markets to also acquire green certificates in

[1] Timilsina (2018) provides an introductory overview of different types of carbon pricing instruments, including carbon taxes, emissions trading system (ETS), and carbon offset mechanisms.

[2] A useful short overview of the relevant legislation is provided by St. John (2014). In our description here, we focus on the large-scale component of the RET which provides well-defined incentives in the wholesale electricity market. There is also a small-scale component to the RET which is captured in our estimates but focuses on small-scale technologies.

proportion to their market share or pay a financial penalty. The market for green certificates generated by accredited MegaWatt-hours (MWh) from renewable generation effectively creates a tax and transfer system, taxing load-serving entities to subsidize green generators. Initially passed at the start of the 2000s, the target aimed to grow renewable generation to 2 percent of the national total by 2010. Legislation passed in both 2009 and 2010 modified and increased the RET's goals to an ambitious 20 percent of generation from renewable sources by 2020, substantially increasing the implied compliance costs to liable entities and driving further investment in renewable energy projects (Cludius, Forrest, MacGill, 2014). In 2015, the large-scale renewable energy targets were adjusted downward from those set in 2009.[3]

Second, in 2011 Australia announced a scheme to put a price or tax on carbon emissions (the Carbon Pricing Mechanism or CPM), to take effect in 2012. However, the tax was short-lived: the scheme was only in place from 2012-2014.[4] The tax itself proposed to cover about 60% of Australia's total emissions by targeting the direct emissions (sometimes called Scope 1 emissions) of about 350 of the highest emitting firms in the country. Notably, this designation did not cover emissions from exported fossil fuels. The overarching plan of the tax included a preliminary phase from 2012-2014, in which a fixed price fee of 23 to 25 AUD would be charged per ton of $CO_2$ equivalent emitted by covered firms, followed by a secondary phase in which the fee would float, and Australian emissions allowances would integrate with world markets, specifically targeting trade with the EU ETS (O'Gorman and Jotzo, 2014). The plan was repealed in 2014.

Using a synthetic control approach, this study analyzes the joint effects of the "subsidy" and "tax",[5] exploiting the coincident timing of the policies' rollout in Australia to understand their combined effects. Our analysis sheds light on several interesting questions that arise in the context

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[3] We rely on targets to understand the timing of policy stringency for data availability reasons. Nelson et al. (2013) demonstrate a rapid rise in Renewable Energy Certificate creation from 2005-2010 in response to the RET, as well as fluctuations in certificate prices between $10 and $60 from 2003 to 2012. For a graphical depiction of the large-scale RET targets and further explanation of its scale, see Appendix A.

[4] According to Peel (2014), a series of referenda beginning in the Australian congress in 2008 strongly signaled the possibility of new climate policy. By 2010, the recently elected Prime Minister had formed a multi-party commission to formally consider the details of a new national climate policy.

[5] For simplicity and clarity, we refer to the expansion of the existing policy as "subsidy", and the new pricing policy either as the "tax" or "price" on carbon. In reality, the "subsidy" can only be described as a subsidy from the perspective of renewable producers, and in fact the RET imposes a demand-side cost on acquirers of wholesale electricity. However, due to the stated goals of the policy and the undifferentiated nature of that cost, we believe that "subsidy" is a reasonably accurate description of the policy. This terminology also serves to clearly delineate the RET from the "tax" on $CO_2$-producing activities, which imposes a differential fee on polluting activities.

of Australia's climate policies. These include: what were the combined and individual effects of the policies during the time of implementation? If emissions were reduced, were the reductions transitory, or did the policies shift emissions onto a long-run path below what would have occurred in the absence of the policy? Did firms sink costly investments that yielded future emissions reductions? Did they wait and see if uncertainty over the tax would be resolved? And finally, did the pricing policy lead to production leakage--in other words, was more Australian coal shipped abroad during and after the tax was in place?

Our synthetic control estimates examine the path of Australia's aggregate emissions per capita from 1980-2018 in the spirit of Andersson (2019). There are several challenges unique to this context, most importantly the selection of a donor pool of economies to replicate the trajectory of Australia's emissions during the 2000s. Among other adjustments to estimation, we limit the donor pool to economies with no national emissions policies during the sample period and focus on other high-emitting economies following the recommendations of Abadie (2021). A battery of robustness and sensitivity checks including the analysis of placebo policies at different times and leave-one-out analyses are carried out to test the validity of the research design and ensure the robustness of the results to changes in the donor pool. Both traditional permutation tests and uncertainty quantification based on prediction intervals are explored. To explain the mechanisms behind the causal effects, we explore patterns in data on emissions by sector and changes in generation capacity by fuel. Finally, a time series analysis of Australian coal production and exports is performed to explore the question of emission leakage.

We estimate that Australia's increase in renewal subsidies in tandem with its short-lived carbon tax caused a 7% reduction in average emissions per capita from 2009 to 2018. This is in sharp contrast with the general perception in the literature that Australian policies to combat climate change in the 2010s were largely ineffective (e.g. Anderson et al., 2023). The synthetic control analysis shows that Australian emissions per capita declined rapidly while the carbon tax scheme was in place and annual emissions did not rebound to a level consistent with their likely path had the policy changes never been implemented. Moreover, we show that contemporaneous with policy implementation, power sector emissions declined because of declining coal-fired generation, while wind and solar capacity rapidly increased. However, the rapid decline in emissions stalled in 2015, immediately following the repeal of the carbon price in 2014. Through a time-series analysis of coal production and exports, this study also presents evidence suggesting that coal production did not diminish and that coal exports may have accelerated due to the combined policies.

The paper is organized as follows. Section 2 presents a review of literature, followed by the description of data and methodology in Section 3. Results and sensitivity analyses are presented in Section 4. The mechanisms of the policies' impacts are explored in Section 5. Section 6 investigates if the carbon tax policy caused changes in coal production and exports. Section 7 concludes.

## 2. Literature Review

A number of studies investigate the effectiveness of carbon pricing and other pricing instruments to reduce GHG emissions. Timilsina (2022b) synthesizes results from existing empirical studies on the impacts of various pricing instruments on GHG emissions. Examples of such studies include Green (2021), Best et al. (2020), Dechezlepretre, et al. (2020), Andersson (2019), Jaraitė and Di Maria (2016), Murray and Maniloff (2015), Aydin and Ömer (2018) and Martin et al. (2014). With only the exceptions of Green (2021) and Best et al. (2020), these studies are either for a single country or a relatively small group of countries, mainly focusing on the European Union.

Green (2021) reviews empirical studies published since 1990 on the impacts of carbon pricing on $CO_2$ emissions. It concludes that carbon pricing policies have caused limited reduction in $CO_2$ emissions, between 0% and 2% per year on average. One difficulty is that the review compares many studies with heterogeneous identification strategies estimating the effect of many heterogeneous policies (including sectoral and fuel-level policies).

Best et al. (2020) estimate the effectiveness of carbon pricing at reducing national $CO_2$ emissions using panel data from 142 countries from 1997-2017. They find that the average annual growth rate of $CO_2$ emissions has been around two percentage points lower in countries with a carbon price system compared to countries without. They estimate that an additional euro per ton of $CO_2$ carbon price is associated with a reduction in the subsequent annual emissions growth rate of approximately 0.3 percentage points, all else equal. On methodology, Best et al. face two main identification challenges. First, a country panel with country and time fixed effects can only control for time-invariant country-specific characteristics and a global time trend that does not vary by country. Failure to capture unobserved time-varying country heterogeneity may lead to omitted variable bias. The second challenge is potential reverse causality that, all else equal, countries that actively seek a green economy have high acceptance of carbon pricing and are more likely to have related policies implemented. The endogeneity problem may bias the OLS estimates and generate misleading results. Rafaty, Dolphin and Pretis (2020) develop a new database of coverage-weighted carbon prices and employ a generalized panel synthetic control estimator to estimate the average effect of carbon pricing on emissions in 39 (mostly European) countries from 1990-2016. They conclude that the introduction of carbon pricing reduced the growth rate of emissions by 1-2.5 percentage points on average in their sample.

Studies focusing on a particular country use a variety of techniques to deal with identification challenges. Floros and Vlachou (2005) examine the response of manufacturing industries (two-digit level of international standards for industrial classification) to a carbon tax in Greece by employing the two-stage translog cost function to time series data during the 1982–1998 period. They find that the carbon tax reduces $CO_2$ emissions in manufacturing industries in Greece, in part by reducing demand and in part by causing substitution of petroleum fuels with electricity and substitution of energy with capital. Martin et al. (2014) finds a strong negative impact of a carbon tax on energy intensity and electricity use in manufacturing firms in UK. Aghion et al. (2016) investigate, using patent data from 3,412 automobile firms and individuals between 1965 and 2005 across 80 patent offices, how a carbon tax helps reduce $CO_2$ emissions in the long run. They find that clean innovation is stimulated by increases in fuel prices, whereas dirty innovation is depressed. They also show strong path dependency, which locks economies into high levels of carbon emissions, even after introducing a mild carbon tax or R&D subsidies for clean technologies.

Much empirical work on the effect of introducing a carbon tax relies on the assumption that consumers or businesses respond to a carbon tax in the same fashion as they would to an equivalent change in the price of the product due to other causes, such as fluctuations in the world oil price or local demand or supply shocks. Some recent studies have found that this need not be the case. For example, Rivers and Schaufele (2015) find that the demand for gasoline in British Columbia is responsive to the introduction of a carbon tax, and indeed the responsiveness to the tax is larger than that of an equivalent fluctuation in gasoline prices due to market forces. Employing a quasi-experimental technique in Sweden to investigate the impacts of a carbon tax on $CO_2$ emissions, Andersson (2019) finds a significant causal effect of carbon taxes on emissions. It finds that the carbon tax reduced transport sector $CO_2$ emissions by 11% percent relative to a synthetic control unit constructed from a comparable group of OECD countries, though some of the effect is attributable to a contemporaneous expansion of Sweden's VAT coverage. The study also shows that the carbon tax elasticity of demand for gasoline is three times larger than the price elasticity. Pretis (2022) employs several related causal inference techniques and finds that British Columbia's carbon tax produced comparably large emissions reductions in the transportation sector, but was unable to detect substantial or statistically significant reductions in BC's aggregate emissions. These heterogeneous findings seem consistent with those of Lin and Li (2011), who analyze $CO_2$ intensity in Scandinavian countries where the carbon tax has been introduced since the early 1990s using a difference-in-difference (DID) study design. While the carbon tax is playing a significant role in reducing $CO_2$ emissions in Finland, the impacts of carbon taxes are not significant in the remaining three countries (Denmark, Norway and Sweden).

A related literature examines policies in several European countries, where car taxes have been linked to the $CO_2$ emissions rate of the vehicle (in grams of $CO_2$/km) in hopes of encouraging drivers to purchase lower-emitting cars. Technically, this is not the same as imposing a carbon tax, yet consumers have responded to these car tax reforms (e.g., Klier and Linn, 2015; D'Halfoeuille et al., 2014; Alberini and Bareit, 2019; Alberini and Horvath, 2020), sometimes with an unintended consequence for the environment. Stitzing (2016), for example, reports that the large, emissions-linked registration tax that must be paid upon purchasing a car in Finland has induced many motorists to switch to diesel cars, which emit less $CO_2$ but potentially more NOx, a local and regional pollutant.

While we are unaware of published studies that specifically link the impact Australia's policies to any substantial or persistent reduction in Australia's carbon emissions, the available short-run evidence suggests such an effect may be plausible. Cludius, Forrest, and MacGill (2014), for instance, estimate that increased wind investment over the period 2011-2013 put downward pressure on wholesale electricity prices, while increased RET costs to acquiring entities (often electric utilities) were passed on to consumers in the form of retail electricity price premiums. O'Gorman and Jotzo (2014) provides a detailed study of the Carbon Pricing Mechanism (CPM) without explicitly estimating causal effects on emissions. Their results suggest that the CPM led to important changes in Australia's National Electricity Market (NEM). They report that in the first two years of the CPM regime relative to the two years prior, retail electricity prices spiked, demand declined, and the emissions intensity of generation declined leading to 8% lower emissions from the electricity sector. One important limitation of both studies is that neither could trace impacts of the CPM beyond the first two years of implementation.

Relatedly, recent evidence suggests that the CPM passed-through substantially and caused increases to wholesale electricity prices in Australia's NEM. Maryniak et al. (2019) estimate pass through to wholesale electricity futures prices between 67% and 150% while accounting for changing risk premia. Similarly, Nazifi et al. (2021) estimate pass-through of the CPM to spot electricity prices in excess of 100% across NEM regions even after attempting to account for varying plant dispatch costs. Cross (2024) uses a quasi-experimental technique that compares trends in US electricity prices to NEM prices and finds a pass-through rate of 88%, with the incidence of price increases largely borne by consumers. Thus the existing literature provides reason to believe that Australia's policy mix raised electricity prices, disadvantaged polluting generators, and drove emissions reductions. Ex-ante, it is unclear how large and persistent these effects were, a question which motivates our analysis.

## 3. Data and Empirical Strategy

Australia's political environment around 2010 was favorable to a major expansion of national climate policy. This circumstance led to an aggressive expansion of renewable energy support and a short-lived carbon pricing mechanism. We seek to jointly estimate the impact of these policies on the trajectory of Australia's emissions in the medium- to long-term (through 2018).

To estimate the impacts of Australia's climate policies causally and trace their dynamic effects over time, we adopt a quantitative case study approach (Abadie and Gardazebal, 2003; Abadie et al., 2015). Synthetic control and related estimation techniques for quantitative case studies have the advantage of minimal data requirements while providing a disciplined and transparent approach for the analysis of policies with aggregate data. Moreover, recent developments in synthetic control and related techniques have been shown to be more adaptable than the canonical form (Abadie, 2021) and capable of recovering causal effects where the assumptions of canonical synthetic control may not be met.

The analysis uses annual national economy-level panel data over the period 1980-2018, described further in the following sections. The key outcome variable is per capita emissions of $CO_2$. We form a control group capable of replicating Australia's pre-existing trend in emissions per capita prior to implementation of the new climate policies (as well as matching on other relevant characteristics). Candidate pre-period characteristics include emissions shares from different sectors, GDP per capita, and energy consumption of different emitting fuels (coal, natural gas, and petroleum products).

In estimating effects, our ideal thought experiment is a comparison between the true emissions trajectory of Australia and a counterfactual Australia which never experienced the rapid changes in environmental policy around 2010 that were truly observed (but otherwise equivalent in all respects). To form an appropriate donor pool for estimation of this counterfactual emissions trajectory by synthetic control, we exclude from the donor pool any economy subject to a national-level carbon price or ETS implemented from 1980-2017. This approach has some weaknesses: For example, we are unable to extract any information from variation in policy stringency or sectoral coverage, and we risk measurement error by omitting subnational policies or renewable generation incentives that may be present among donor economies. However, our approach also has some strengths: It is relatively transparent and simple, and appropriately ignores many policies that offer weak incentives for market participants and have little practical implication for national-level emissions (including sub-national policies that may suffer from significant within-border leakage). Additionally, for potentially omitted policies in the donor pool, our strategy is conservative in the sense that any policy effective in reducing emissions that shows up in the donor pool should bias our estimates toward finding a null effect (a 'control' economy that is 'treated').

### 3.1 Data

The outcome variable for the main analysis is the emissions of $CO_2$ per capita. Emissions data come from the *Emissions Database for Global Atmospheric Research* (EDGAR). The EDGAR dataset uses a bottom-up methodology to calculate $CO_2$ emissions in the power, industry, buildings, transport, and agriculture sectors. The methodology is applied consistently to all countries to facilitate comparison across economies.[6] We also use the EDGAR data to calculate

[6] Further information and downloads are available at https://edgar.jrc.ec.europa.eu/report_2022.

emissions shares by sectors. We focus our analysis on economies that can be matched to EDGAR data via country codes from the *World Development Indicators.*

Two variables generally considered important predictors of emissions, and that are indeed correlated with emissions across countries, are population size and economic output. We use the harmonized long run estimates of GDP per capita and population in the *Maddison Database*, which utilize a similar methodology and are published alongside the Penn World Tables (PWT).[7] All GDP figures are in real $2011 dollars.

Finally, to obtain a good fit and improve the credibility of the synthetic control estimates, we use fuel consumption of coal, natural gas, and petroleum products from the Energy Information Administration (EIA) *International Energy Statistics*.[8] We construct per-capita measures, scaling the EIA data by the population figures from the Maddison database. A full list of the relevant covariates appears in Table 1.

### 3.2 Methods: Synthetic Control and Extensions

In the basic setup for the case considered in the paper, we observe units $j = 1, ..., J + 1$ for time periods $t = 1, ..., T$. Without loss of generality let $j = 1$ be the treated unit, and consider an intervention that occurs at time $T_0 + 1 < T$. Denote observed outcomes (i.e. emissions per capita) by $Y_{jt}$ and potential outcomes $Y_{jt}(1), Y_{jt}(0)$ with and without treatment, respectively. The treatment effect for unit $j$ at time $t$ is defined as

$$\tau_{it} = Y_{jt}(1) - Y_{jt}(0). \tag{1}$$

Sometimes called the fundamental problem of causal inference, the object of interest $\tau_{1t}, t > T_0$ cannot be calculated because we only observe one of its two components. Synthetic control seeks to use a weighted sum of control units $2, ..., J + 1$ to impute the missing counterfactual outcome of the first unit (i.e. annual emissions per capita in Australia had the tax not been implemented). The weights are chosen subject to positivity and adding-up constraints $w_j^* \geq 0 \ \forall j$ and $\sum_{j=2}^{J+1} w_j = 1$.

[7] Available at https://www.rug.nl/ggdc/historicaldevelopment/maddison.
[8] Available at https://www.eia.gov/international/data/world.

Thus, the imputed counterfactual is a convex combination of control units, and the estimated treatment effect is given by

$$\widehat{\tau_{1t}} \;= Y_{1t}(1) - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{2}$$

for periods $T_0 + 1, \dots, T$. We use the estimator implemented by Abadie et al. (2011), which defines a ($k$ x 1) vector of characteristics of the treated unit $X_1$, and a ($k$ x $J$) matrix of the same characteristics for the control units $X_0$. The matrices $X_1, X_0$ are typically composed of pre-period values of the variable of interest (i.e. emissions per capita) or other variables with significant predictive power. Stacking the weights into a length $J$ vector $W = w_2, \dots, w_{J+1}$, the algorithm minimizes the objective function

$$sqrt\{\,(X_1 - X_0W)'V(X_1 - X_0W)\,\} \tag{3}$$

where V is defined as some ($k$ x $k$) symmetric and positive semidefinite matrix defining the relative importance of the $k$ characteristics in the minimization. In practice, we rely on the default proposed by Abadie et al. (2011) which performs a two-step optimization, choosing both $W$ and $V$ jointly to minimize the objective function (in other words, to maximize pre-treatment fit).

There are many extensions to synthetic control that have been proposed to make the method more versatile and improve pre-treatment fit. In particular, in the case of Australia, we implement the de-meaned estimator proposed both by Doudchenko and Imbens (2016) and Ferman and Pinto (2021), which we refer to as the DIFP estimator. We discuss reasons why this estimator is a more suitable choice below. Intuitively, the DIFP estimator imputes the counterfactual $Y_{1t}(0), t > T_0$ with an included intercept, replacing equation (2) with

$$\widehat{\tau_{1t}} \;= Y_{1t}(1) - (\,\mu + \sum_{j=2}^{J+1} w_j^* Y_{jt}\,) \tag{4}$$

where $\mu$ is also a parameter to be estimated. In practice, the DIFP estimator is implementable by demeaning observations for each economy from a pre-period value and then estimating the synthetic control as normal. This poses no additional complications for computation or inference, and just requires adjusting expression (3) so that $X_1, X_0$ are replaced by their time-demeaned analogues $\ddot{X}_1, \ddot{X}_0$.

To better understand the uncertainty in our estimates, in addition to the permutation test proposed by Abadie et al. (2010) we also consider the prediction interval procedure proposed in Cattaneo, Feng, and Titiunik (2021).

### 3.3 Estimation Details

The primary difficulty in applying synthetic control in our context is that Australia is an outlier in terms of emissions per capita. Australia's electricity generation is mainly coal-based, and it is also a major exporter of coal and natural gas. Coal-based electricity, coal mining and natural gas production are energy-intensive industries. In addition, many natural comparison economies that might help form a plausible counterfactual for Australia's emissions must be omitted from the sample because they implemented policies that would significantly affect emissions during the sample period. For instance, the implementation of the European Union's Emissions Trading System (EU ETS) in 2005 disqualifies all EU economies.

We form the donor pool for Australia in three main steps. First, we construct a global panel dataset of emissions, GDP per capita, and energy consumption using the data sources previously described. We only consider economies with no missing values in any year for any of the relevant indicators. Second, we use data provided by the World Bank (primarily from the Carbon Pricing Dashboard)[9] to identify every economy that implemented a national-level carbon price or ETS from 1980-2017 and disqualify them from the donor pool. Third, to address Australia's position as a large economy with relatively high emissions per capita, we limit our estimation sample to the top 35 largest emitters per capita with a population of at least 1 million (both measured on average from 1980-2000). The resulting donor pool is listed in Appendix B.[10] This third restriction speeds computation and reduces concerns of interpolation bias. Including all economies with complete data does not meaningfully change the chosen weights, and therefore does not affect the results.

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[9] https://carbonpricingdashboard.worldbank.org. The Australian carbon tax, incidentally, does not appear in the World Bank's carbon tracker data.

[10] We finalize the donor pool with one further exclusion - we drop the United States due to the adoption of hydraulic fracturing in the mid- to late-2000s, which displaced coal-fired generation and caused a drop in emissions around 2010.

Figure 1 motivates the use of the DIFP estimator. The solid line displays Australia's total emissions per capita from 1980-2018 while the dotted line displays the donor pool average over the same period. The grey lines show the total emissions per capita of each donor pool country. Both Australia and the donor pool average show a gradual upward trend in the 1980s as well as a gradual upward trend starting in the mid-1990s, while the donor pool average shows a drop around 1990. Australia's emissions show a drop in the mid- to late-2000s. Figure 1 suggests that it may be difficult to match Australia's *level* of emissions per capita, but it may be possible to match the *trend*, so this is the strategy we opt for. In practice, we implement the DIFP estimator by fitting a synthetic control model after "demeaning" the data by subtracting out country-specific average emissions per capita from 1980-2000, as suggested by Doudchenko and Imbens (2016) and Arkhangelsky et al. (2021).

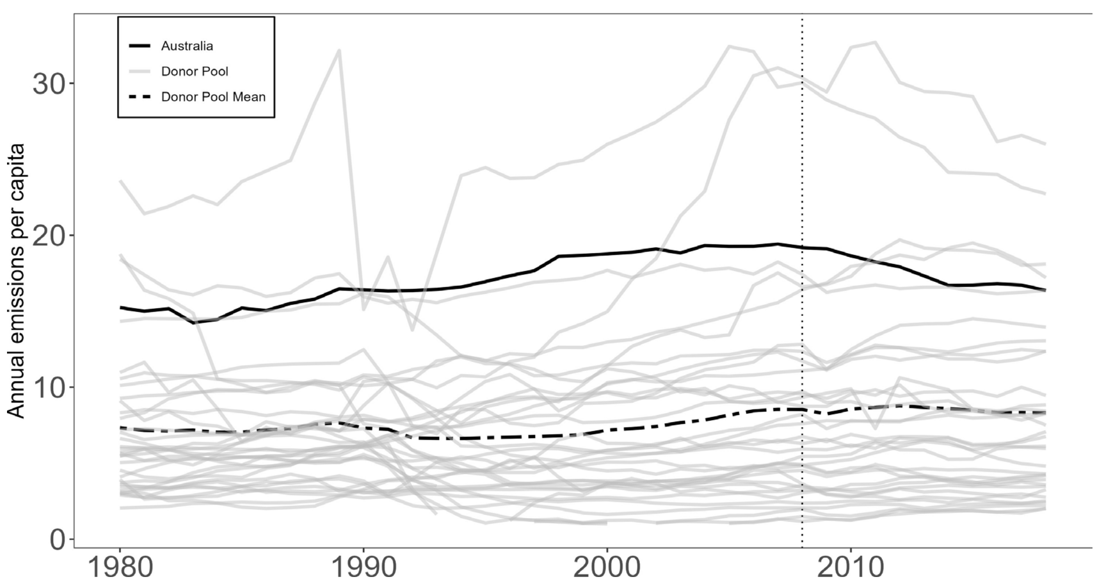



Figure 1: **Australian emissions per capita versus donor pool**

Given the potential for immediate market impacts in the year that the RET was updated (2009) and the ongoing controversy of a carbon pricing policy, we define $T_0$, the last year before treatment, to be 2008. This allows us to simultaneously estimate the impact of the two policies by estimating the counterfactual and examining the dynamic treatment effect as uncertainty over the tax unfolded. This approach has the additional benefit, for analyzing the tax, of conforming with the back-dating recommendation of Abadie (2021) to incorporate possible anticipation effects.

## 4. Results

Figure 2 illustrates the fit of our preferred synthetic control, where again the solid line represents Australia's true emissions profile over time. We use a mixture of lagged values of per capita emissions (prior to implementation of the tax) and covariates theoretically likely to be strong predictors of emissions. Covariates are displayed in Table 1 and discussed below. The fit of the synthetic control in the pre-period is very good: It is neither systematically high nor low, over-predicting in the mid-1990s and the mid-2000s, but under-predicting in the early 1990s and early 2000s. There is an under-prediction in the year 2009, but in 2010 the synthetic Australia and true Australia have almost exactly the same value. After 2010, Australia's emissions noticeably decline relative to its synthetic counterpart.

The estimated treatment effect is the gap between the synthetic Australia and true Australia after 2008 ($T_0$, represented by the vertical dashed line). The estimated treatment effect increases from 2011-2014 when the tax was announced and in place (indicated by the shaded vertical bars), before immediately flattening in 2015 with the repeal of the policy. The treatment effect is most easily read from the axis on the right side of the figure, which adds back Australia's pre-period intercept to both series. The estimated average emissions reduction over the period 2009-2018 is -1.37 metric tons of $CO_2$ per capita per year, or approximately 7.15% of emissions per capita in 2008. The pre-period average (absolute) deviation is approximately 0.23 metric tons, or about 1.3% of emissions per capita in 2008.[11] Relative to the standard deviation of Australia's emissions per capita in the pre-period, the estimated effect size is about 0.79 standard deviations while the pre-period error is approximately 0.13 standard deviations.

Table 1 displays further details on covariates in the pre-period for Australia, the synthetic unit, and the average of the donor pool sample. Unless otherwise indicated, each row displays the

[11] The ratio of post-to-pre period mean squared prediction error is more than 32, but this quantity is best understood in relative terms, which we address after presenting the main results.

covariate average over the period 1998-2008 (the 10 years prior to $T_0$). As mentioned above, Australia (the "Treated" unit) has a high share of emissions from the power sector, is relatively rich, and has a very high coal consumption per capita relative to the donor pool average. The synthetic unit does not balance perfectly on pre-period characteristics; however, it better matches Australia on several important dimensions. In particular, it closely matches the share of power sector emissions and income per capita, and is much closer in coal and petroleum consumption per capita than the donor pool average. On the other hand, it has worse fit in terms of natural gas consumption per capita.

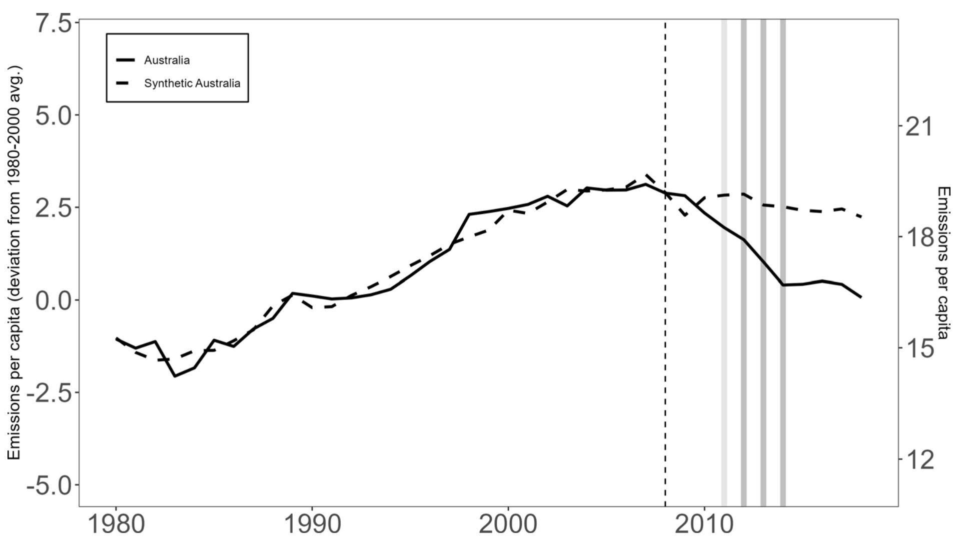



Figure 2: **Path plot of Australian emissions versus synthetic control**

Table 2 shows the composition of the synthetic unit, displaying non-zero weights from the donor pool. The chosen weights are importantly quite sparse, with the synthetic unit composed mostly of Taiwan, China, the combined economies of Israel, Gaza and West Bank, and Canada, in addition to Hong Kong SAR, China, which receives a weight of approximately 5%. We interpret Taiwan and Hong Kong as capturing regional (Pacific) economic and policy shocks. Canada is also a logical comparison country due to cultural similarities to Australia, particularly through British colonial history (as is Hong Kong). Canada and Australia are both significant producers, exporters, and consumers of coal and other fossil fuels, which helps to balance the synthetic unit with Australia in terms of power sector emissions share and coal consumption per capita. We hypothesize that the substantial weight on the economies of Israel, Gaza and West Bank may reflect similarities in climate and average income levels. We explore the sensitivity of the results to excluding certain weights in Figure 3.

Table 1: **Covariate balance**

| Variables | Treated | Synthetic | Donor Pool Avg. |
|---|---|---|---|
| Emissions share (power) | 0.52 | 0.47 | 0.40 |
| Emissions share (transportation) | 0.21 | 0.21 | 0.17 |
| Emissions share (other) | 0.28 | 0.33 | 0.43 |
| Log of gdp per capita (USD 000s) | 3.68 | 3.42 | 2.46 |
| Btus of coal per capita (Hundred MMs) | 1.04 | 0.46 | 0.14 |
| Btus of ng per capita (Hundred MMs) | 0.50 | 0.34 | 0.50 |
| Btus of petroleum per capita (Hundred MMs) | 0.94 | 0.97 | 0.65 |
| Emissions per capita in 2001 | 2.58 | 2.33 | 0.23 |
| Emissions per capita in 2007-2008 (avg.) | 3.00 | 3.14 | 1.49 |

Notes: Table reports pre-period values of covariates in Australia ('Treated' column, *N=1*), synthetic Australia ('Synthetic' column) and donor pool (Donor Pool Avg. column, *N=33*). Each row displays the covariate average over the period 1998-2008 unless otherwise indicated. See Section 3 for details.

Table 2: **Nonzero synthetic control weights**

| Weight | Country (Unit) |
|---|---|
| 0.39 | Taiwan |
| 0.30 | Israel, Gaza and West Bank |
| 0.26 | Canada |
| 0.05 | Hong Kong SAR |

Notes: Table reports nonzero weights in synthetic Australia. See Section 3 and Appendix B for details.

To build confidence in the empirical strategy, Figure 3 shows a leave-one-out sensitivity test of the result from Figure 2. To produce the figure, each of the four economies with nonzero weights in Table 2 was dropped from the donor pool and the synthetic control was re-estimated. The resulting counterfactuals are plotted in gray. The results show that the counterfactual path of Australian emissions does not qualitatively depend on the choice of the economies listed in Table 2. The estimated treatment effect would have been similar in each case. Overall, the figure shows that the qualitative conclusions and approximate magnitudes described above are not idiosyncratically dependent on the selection of the four economies in the optimal synthetic unit given our choices in forming the donor pool. This is a key sensitivity check due to potential measurement errors, such as the implementation of GHG reduction policies by several provinces in Canada around the time of Australia's policy. If effective in reducing Canada's aggregate emissions, these policies might induce downward bias in our estimated treatment effect; the analysis in Figure 3 suggests this bias is unlikely to be substantial.

To further build confidence in the empirical strategy, Figure 4 displays a placebo-in-time test of the methodology that produced Figure 2. To produce the figure, we back-dated the pre-treatment period $T_0$ by 10 years (as well as all covariates from Table 1), and evaluated a "fake" policy affecting emissions in Australia in the year 1998 during the period 1998-2008.[12] As before, the synthetic control was able to replicate the trend in emissions per capita reasonably well for (nearly) 20 years prior to the policy. In the case of Figure 4 however, there is only a small divergence between the synthetic unit and Australia's true emissions following the fake policy in 1998, as we should expect if the estimation strategy is valid. In fact, the ratio of MSPE between the post- and pre-periods of Figure 4 is less than 1. In Appendix C we shift the placebo treatment forward and back by two years (1996 and 2000) to show that the null placebo result is not dependent on the specific year chosen for "implementation" of the fake policy.

[12] Covariates are therefore averaged over the period 1988-1998, and lags of emissions per capita are from 1991 and 1997-1998.

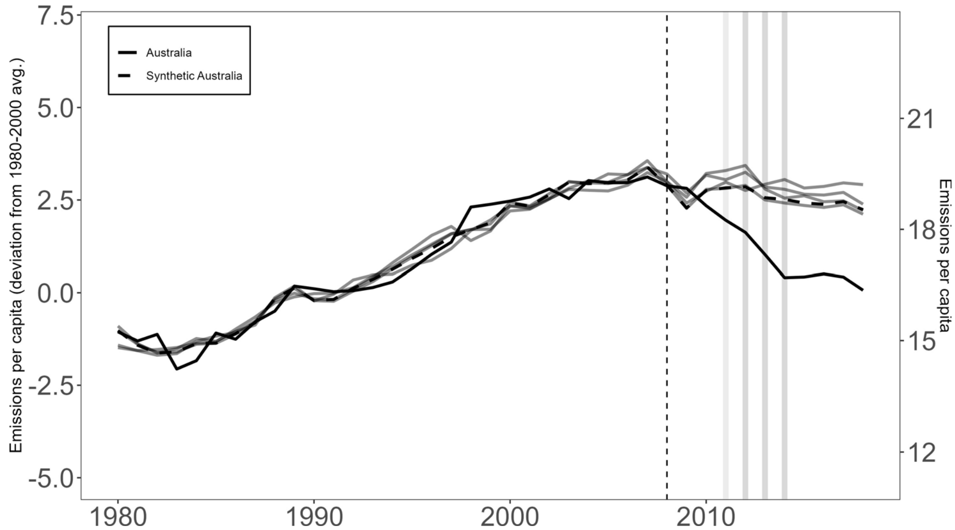



Figure 3: **Leave-one-out analysis**


Figure 4: **Placebo-in-time test**

Finally, in Figures 5 and 6 we show that the estimated treatment effect in Australia is extreme relative to other economies in the donor pool using the permutation test proposed in Abadie et al. (2010). The dark black line in Figure 5 shows the "gaps" between Australia's emissions per capita and its synthetic counterpart from 1980-2018. The light gray lines are the corresponding figure for each of the 33 donor pool economies, excluding those with much higher prediction error in the pre-period. Specifically, we exclude economies for which the mean squared prediction error was five or more times greater than Australia's pre-period MSPE.[13]

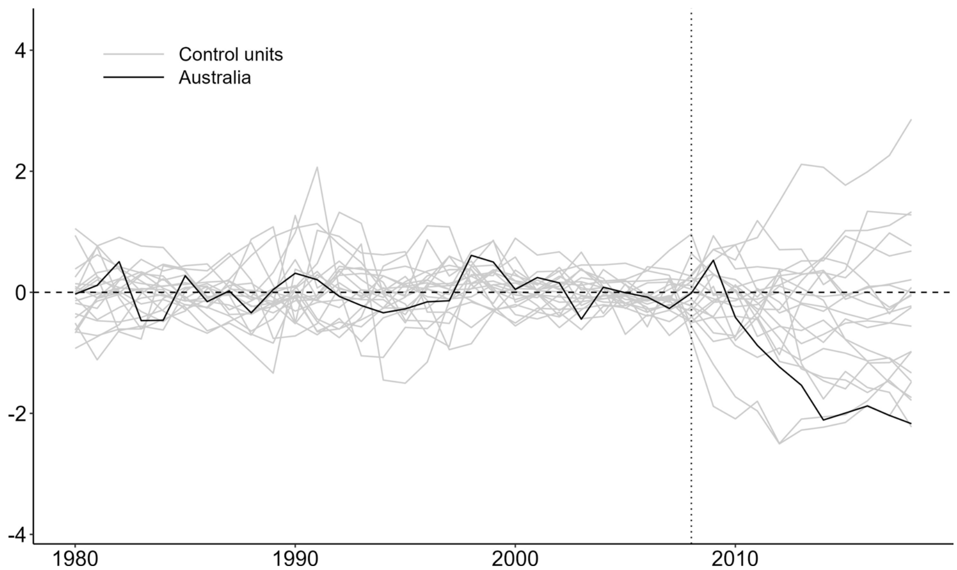



Figure 5: **Placebo-in-space (gaps plot)**

In Figure 6 we show the distribution of post-to-pre MSPE ratios for the same economies depicted in Figure 5. Australia's MSPE ratio between the post- and pre-periods was more than 30, and the 2nd most extreme of any of the other units included in the figure. Australia's estimated effect size is extreme regardless of the economies we include, as shown in Appendix E. The p-value implied by Figure 6 is approximately 0.091, the probability of randomly choosing an economy with an equal or larger Post/Pre MSPE ratio from among the countries with reasonably strong pre-period fit.

[13] Appendix D repeats this exercise with a less stringent MSPE threshold (20) and a more stringent MSPE threshold (2).

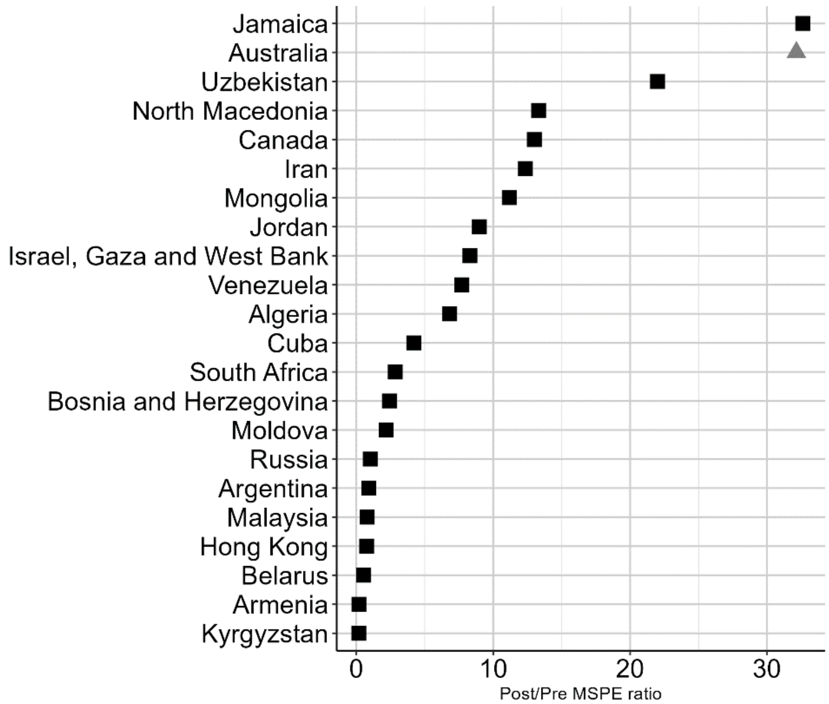



Figure 6: **Placebo-in-space (MSPE ratios)**

Because permutation p-values can be highly sensitive to the number of sample economies considered in the donor pool, we also quantify the uncertainty in our predicted counterfactuals using prediction intervals as proposed in Cattaneo et al. (2021). Appendix F shows prediction intervals for the estimated treatment effects. The two figures show prediction intervals with 90% coverage probability for $Y_{1t}(0)$, $t > T_0$, the counterfactual outcome in Australia after the policy changes according to the synthetic control estimation. The intervals account for both "in-sample" uncertainty due to imperfect weight selection in the pre-period as well as an estimate of "out-of-sample" uncertainty due to unpredictable shocks in the post-period.

In the first figure (F1), the uncertainty is estimated using the same model specification as in the main results. The second figure (F2) is constructed using only lags of emissions per capita and a constant. In both figures the true outcome consistently lies near the bottom of the 90% prediction interval for the estimated counterfactual by 2014. In the former case the prediction intervals are relatively large, indicating a marginally statistically significant effect only in 2018. In the latter case, the true value of Australian emissions lies outside of the prediction intervals in 2014, 2017, and 2018.[14]

## 5. Mechanisms

Some descriptive analysis gives insight into what drives the emissions reductions estimated in the previous section. Figure 7 shows which sectors account for the estimated reduction in emissions per capita. From Figure 7 it is evident that total emissions declines are generally dominated by the power sector, and further that there is little or no evidence of declining emissions from other sectors corresponding to the timing of the policy changes. In Appendix G, we confirm that coal was the main fuel whose consumption meaningfully declined after implementation of the policies.

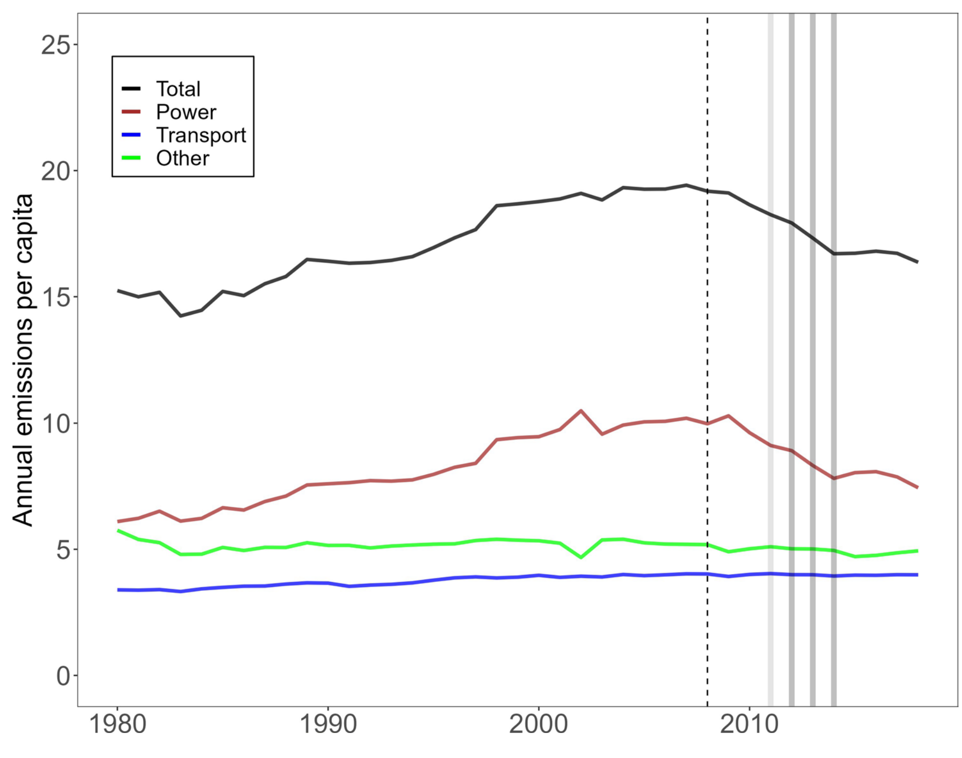



Figure 7: **Australia's emissions by sector**

Figure 8 digs deeper into mechanisms, plotting electricity capacity by resource type according to the EIA. A few interesting facts appear. From Figure 8 it appears that the implementation of the

[14] For more details on implementation, see the figure notes in Appendix F.

pricing policy may have slowed fossil fuel capacity investments.  Moreover, there appears to be a spike in fossil fuel capacity additions after its repeal. Additionally, in Figure 8 we see a substantial rise in renewable capacity, especially in solar generation. In Appendix G we also confirm that fossil fuel plants lost some market share (in terms of energy generated) to renewable, wind, solar, and hydro resources during this time. Rather than increasing with total electricity generation, fossil fuel generation declined from 2011-2014 and subsequently remained stable through 2018.

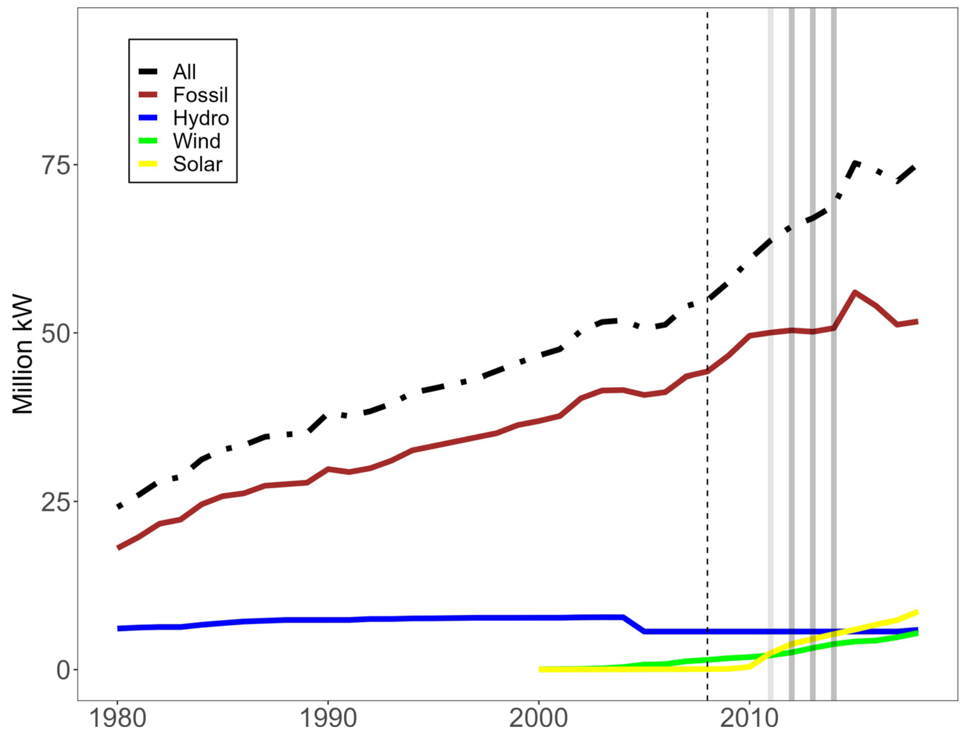
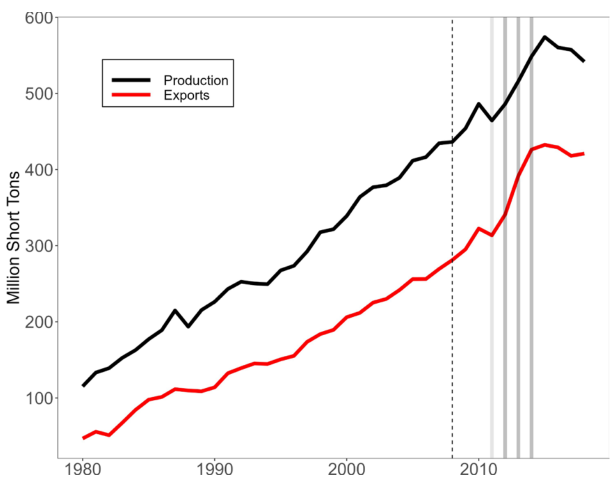



Figure 8: **Australia's electric generation capacity**

## 6. Exports

In this section we present descriptive and time series analysis that sheds light on the issue of leakage. From the raw data in Figure 9, it appears that coal production in Australia continued to increase during the policy period and may have even accelerated. Moreover, it seems likely that exports increased during the policy period given the apparent change in slope of the time series from 2011-2014. Alongside the figure, we display a table of the annual share of produced coal that was exported for select time periods. The table further contributes to the appearance that coal exports accelerated during the policy period. The share of coal exported increased throughout the sample, reaching its highest levels during the policy period.




| Period | Mean.Export.Share |
|---|---|
| 1981-1988 | 0.49 |
| 1989-1998 | 0.56 |
| 1999-2008 | 0.61 |
| 2009-2018 | 0.73 |
| 2009-2015 | 0.70 |
| 2011-2015 | 0.73 |

Figure 9: **Australia's coal production and exports**

To elaborate on the preliminary evidence of Figure 9, we implement a straightforward time series analysis of Australian coal production, exports, and export shares. While the time series analysis does not have the causal strength of the main analysis, it allows us to transparently explore whether Australia's climate policies passed up the supply chain and negatively affected the coal industry. We estimate the parameters of a model designed to test whether the upward trend in production and exports of coal throughout the sample accelerated during the policy period. We estimate three versions of the model, where the dependent variable $Y_t$ is coal production, coal exports, and the share of coal exported. In all cases we regress $Y_t$ against a constant, a time trend, and a trend break in 2008 (given by equation (5)). The coefficient $\beta_2$ tests whether a linear trend or a linear trend with a break during the policy period fits the data better. All standard errors are heteroskedasticity and autocorrelation consistent (HAC).

$$Y_t = \alpha + \beta_1 t + \beta_2 \mathbb{1}[t > 2008]t + \epsilon_t \tag{5}$$

Table 3 shows the results of the time series regressions. Column (1) of Table 3 shows results when the dependent variable is production, while column (2) shows results when the dependent variable is exports, both in millions of short tons (mmst) of coal. Column (3) has the share of coal exported annually as the dependent variable.

All three columns show a statistically significant upward trend throughout the sample period. In column (1) however, the trend break coefficient is insignificant, even though the point estimate is positive. Thus, the model fails to reject no change in coal production during the policy period. However, the coefficient on the trend break in both exports and the share of coal exported is significant, indicating an increase in exports during the policy period. In Appendix H we show a visual representation of the time series models for production and exports, estimated with equation (5). The figures show the data in Figure 9 with fitted values from the models in Table 3 overlaid. Consistent with the previous discussion, production follows a relatively steady upward trend throughout the entire sample (including the policy period). Exports show a more marked trend break in the policy period.

Table 3: **Production, Exports regressions**

| VARIABLES | (1) Production | (2) Exports | (3) Export Share |
|---|---|---|---|
| Trend break (2008) | 2.73 | 10.09*** | 0.007** |
| | (1.70) | (1.92) | (0.003) |
| Time | 11.62*** | 8.34*** | 0.007*** |
| | (0.25) | (0.24) | (0.001) |
| Constant | 100.34*** | 32.55*** | 0.445*** |
| | (5.18) | (4.85) | (0.025) |
| Observations | 39 | 39 | 39 |
| SE | HAC(4) | HAC(4) | HAC(4) |

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The time series estimates presented in this section suggest that the climate policies targeting domestic demand for emissions did not effectively pass up the supply chain to Australian coal producers: Production steadily increased and any demand shortage from the Australian electricity sector likely led to export leakage. If Australian coal was sold at a heavy discount and induced greater total coal consumption by importing countries, emissions leakage may have occurred.

## 7. Conclusions

This analysis explores the effect of real-world climate policies in Australia: a mixture of taxes and support for renewables in an environment of political uncertainty. Our analysis is a novel application of synthetic control and its variants, focusing on policies that were ultimately repealed or scaled back, and estimating dynamic medium- to long-run impacts. The analysis highlights that political constraints can predictably impact the outcomes of environmental policy, while showing that an imperfect patchwork of climate policies can nonetheless be effective in reducing emissions.

Specific to the experience of Australia, we conclude that Australia's renewable electricity policy expansion and short-lived carbon tax had a substantial and persistent impact on total emissions. To provide an illustrative calculation: an average annual emissions reduction of 1.37 tons of $CO_2$ per-capita, implicitly valued at $25 per ton by the Australian government, implies climate benefits of approximately $765 million dollars annually during the treatment period (2009-2018). When the carbon tax was repealed and the renewable energy subsidies were reduced, the effect of the policies attenuated, but Australia's emissions per capita did not rebound to a level consistent with our projection of expected emissions in the absence of the policy changes.

Since the policy mix was unilateral and aimed to reduce domestic emissions, it may have increased exports of Australian coal, potentially causing a form of emissions leakage. Additionally, this study cannot rule out a reversal and rebound of emissions past the final sample year of the analysis (2018). However, a total reversal of emission reductions seems unlikely given the long-lived nature of electric capacity investments and the clear and sustained upward trend in wind and solar power through the end of the study horizon. These facts together suggest that the policies' impacts persisted to the end of the sample and potentially beyond.

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# Appendix A. Details on RET Targets

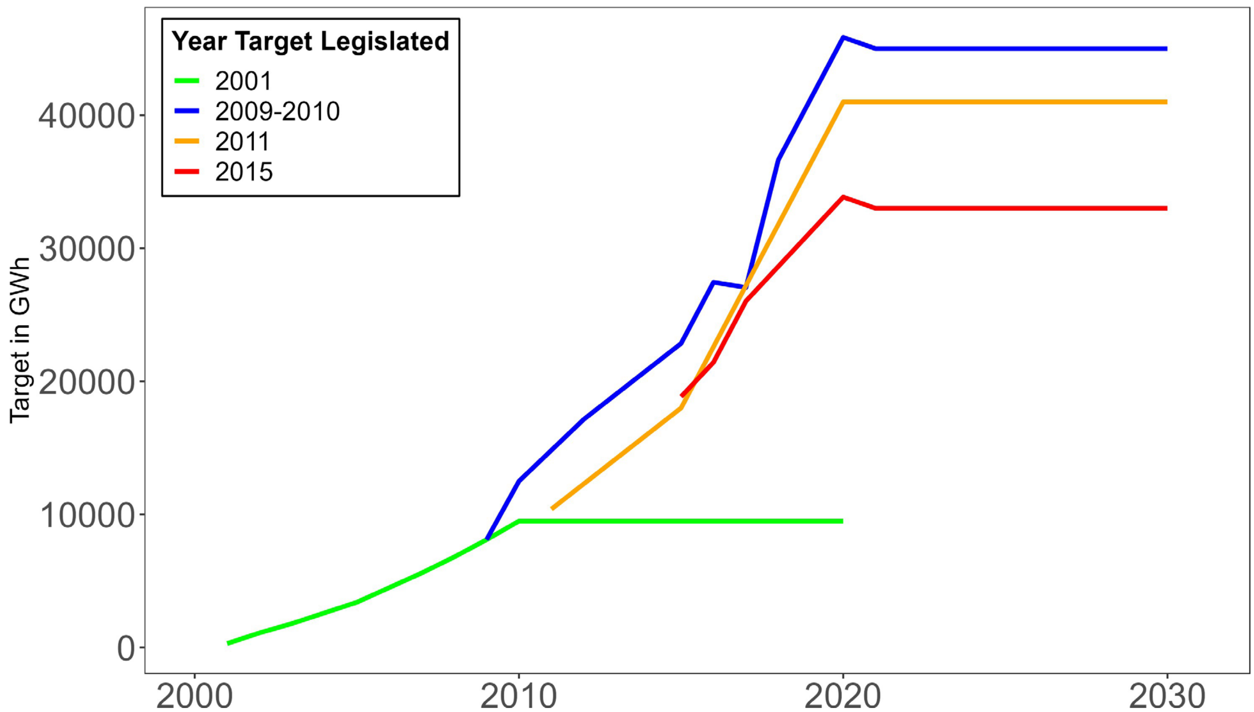


Figure A1: **Large-scale Renewable Energy Targets.** Figure displays LRETs by the year of legislation. See section 1 for details. Data for the plot were obtained from https://cer.gov.au.

**Further Detail:** Demand for "large generation certificates" is directly tied to the Large-scale Renewable Energy Targets, because the government increases the share of electricity purchases that retailers must cover with certificates to meet the legislated targets. For instance, this share was 9.87% in 2014. If liable entities don't meet the required share, they must pay $65 per megawatt-hour not met under the requirement, effectively capping the price (St. John, 2014) of the certificates.

# Appendix B. Donor Pool

Table B1: **Synthetic Control Donor Pool**[1]

| | | |
|---|---|---|
| Argentina | Armenia | Azerbaijan |
| Bosnia and Herzegovina | Belarus | Canada |
| Cuba | Algeria | Georgia |
| Hong Kong SAR, China[2] | Iran, Islamic Rep. | Iraq |
| Israel, Gaza and West Bank[3] | Jamaica | Jordan |
| Kyrgyzstan | Korea, Rep. | Kuwait |
| Libya | Moldova | North Macedonia |
| Mongolia | Malaysia | Oman |
| Russian Federation | Saudi Arabia | Singapore |
| Turkmenistan | Trinidad and Tobago | Taiwan, China |
| Uzbekistan | Venezuela, RB | South Africa |

Notes: Table lists non-treatment economies in the final estimation sample, also known as the donor pool. Australia is the treatment economy. See Section 3 for more details.

[1] We adopt EDGAR naming conventions for economies in the sample except for the footnoted economies.
[2] SAR: Special Administrative Region, SAR added for clarity.
[3] Some measurement error exists in this donor economy due to differences across data sources. The EDGAR data aggregates emissions from the economies of "Israel and the State of Palestine" while matching on ISO code ISR includes GDP and energy consumption measures only from Israel.

# Appendix C. Time Placebos

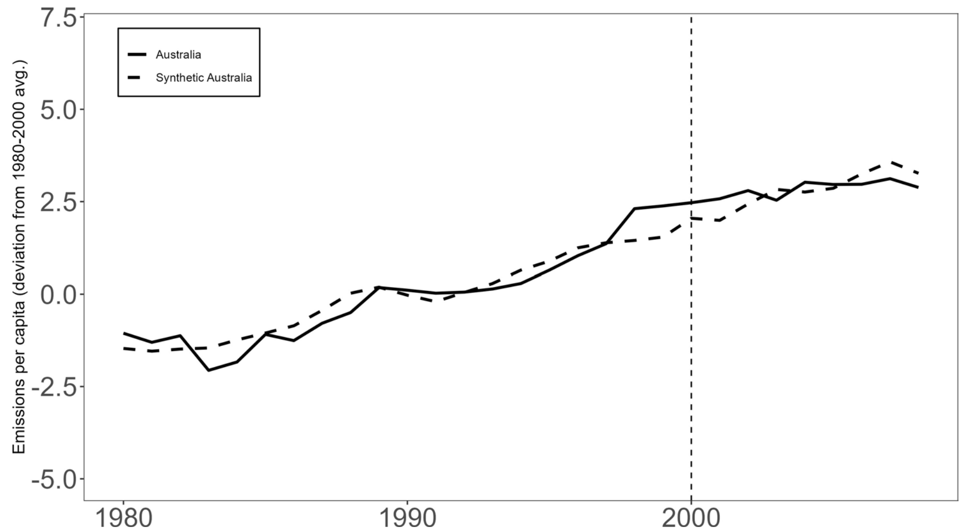


Figure C1: **Placebo-in-time (2000)**

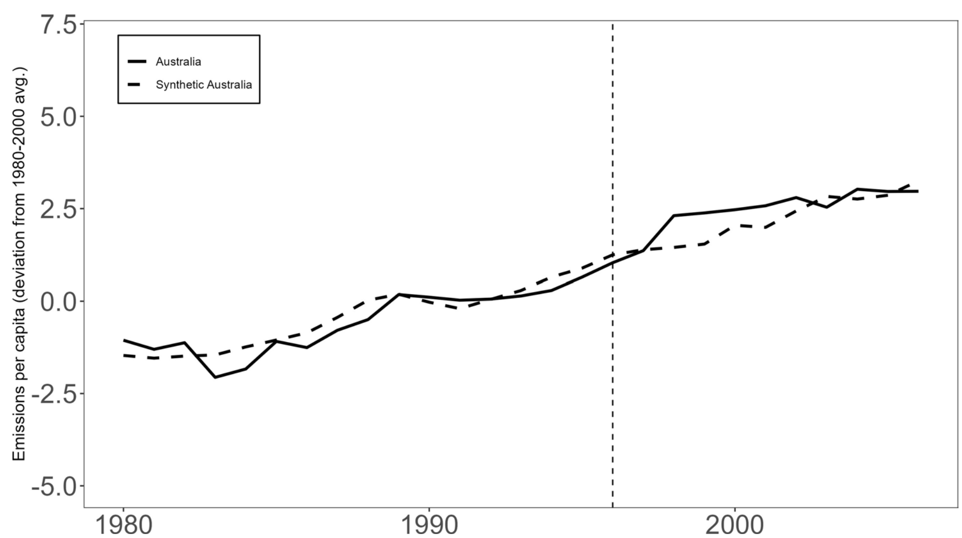


Figure C2: **Placebo-in-time (1996)**

# Appendix D. Placebo Path Plots

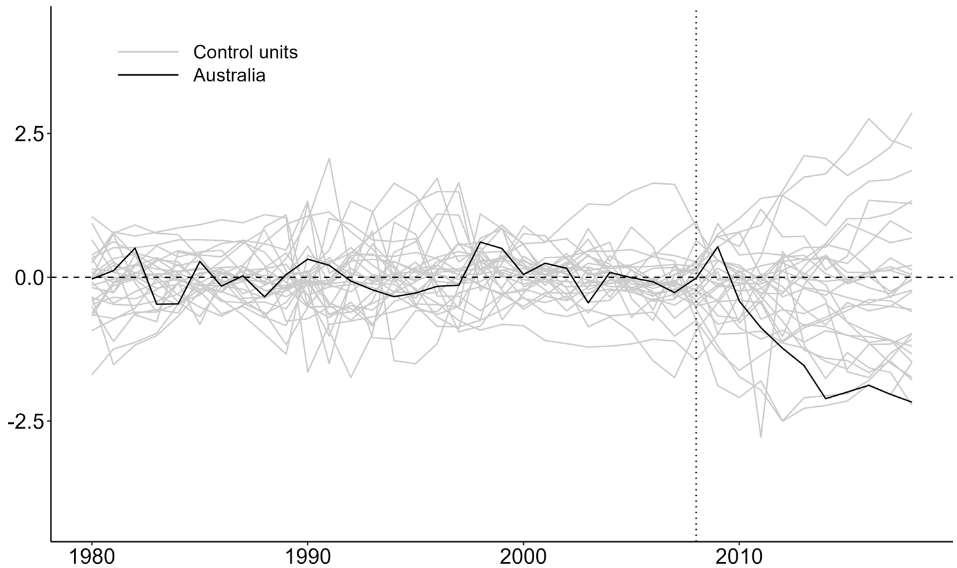


Figure D1: **Placebo-in-space (MSPE ratio threshold: 20)**

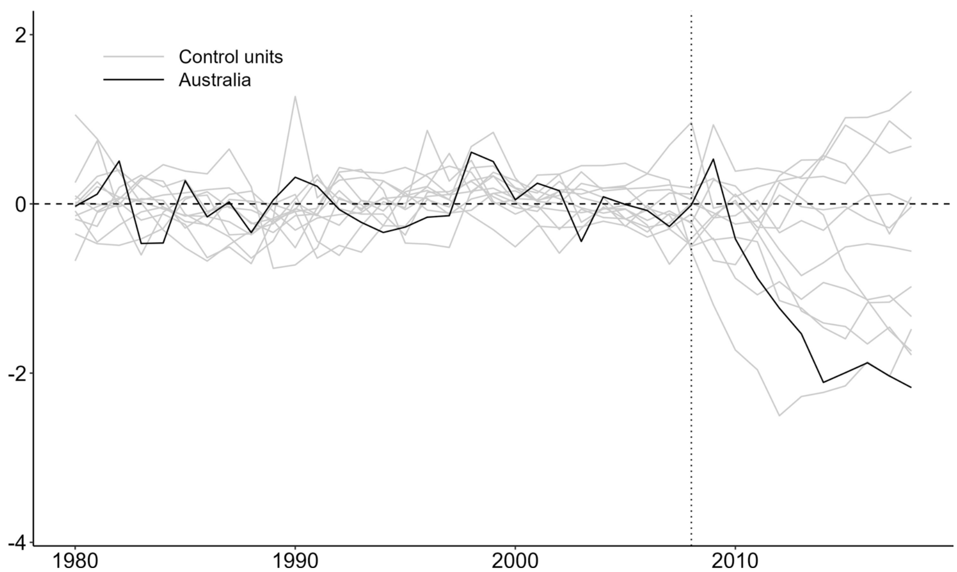


Figure D2: **Placebo-in-space (MSPE ratio threshold: 2)**

# Appendix E. Placebo Distribution Plots

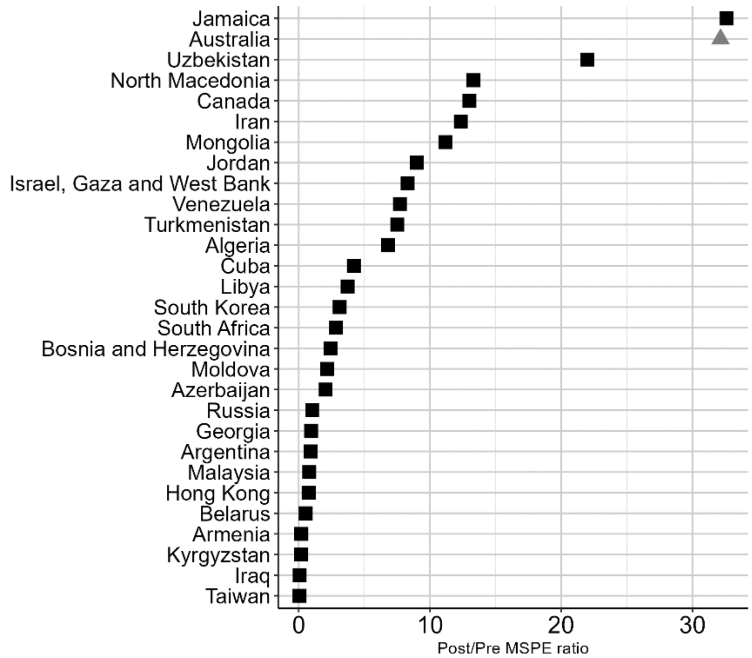


Figure E1: **Placebo-in-space (MSPE ratio threshold: 20)**

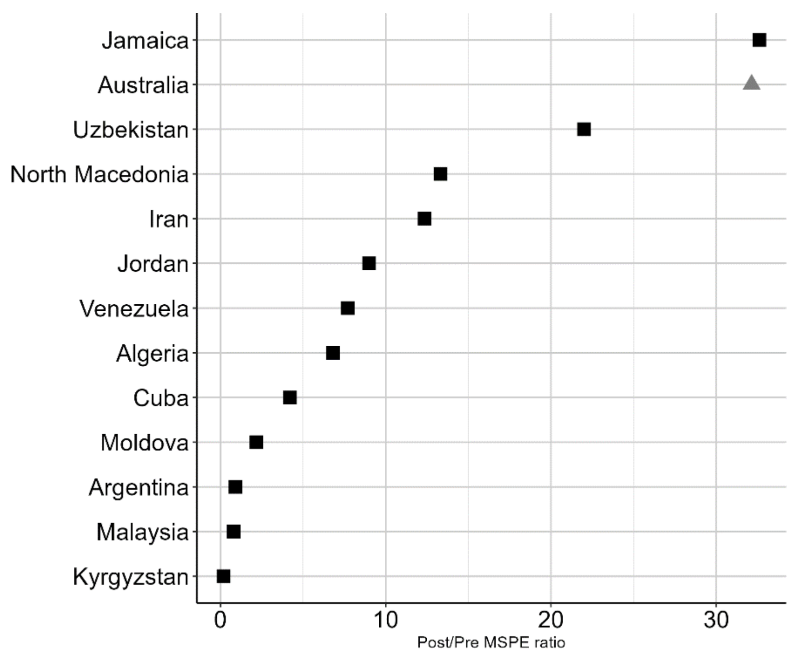


Figure E2: **Placebo-in-space (MSPE ratio threshold: 2)**

# Appendix F. Uncertainty Quantification

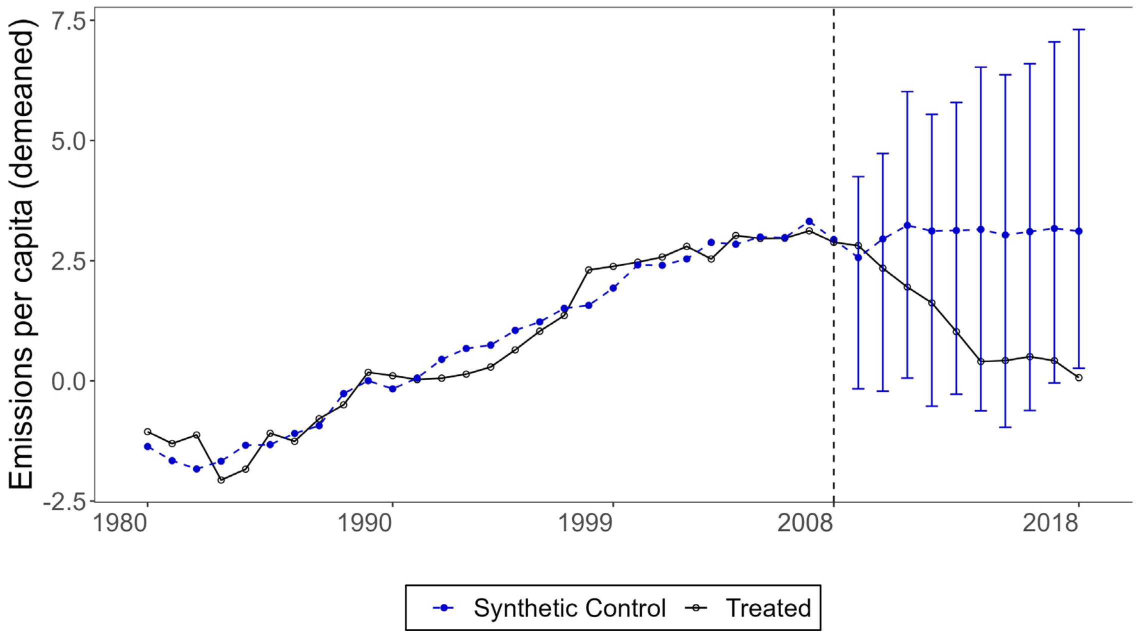


Figure F1: **Prediction intervals, with features.** Intervals formed using features in Table 1, methodology described in Cattaneo et al. (2021). The black line represents the level of the outcome for Australia (deviation from pre-period mean), while the blue line represents the synthetic control. Blue bars report 90% prediction intervals for $Y_{1t}(0)$. Intervals account for in-sample uncertainty using 1000 simulations of the pre-period error process and out-of-sample uncertainty assuming sub-Gaussian bounds.

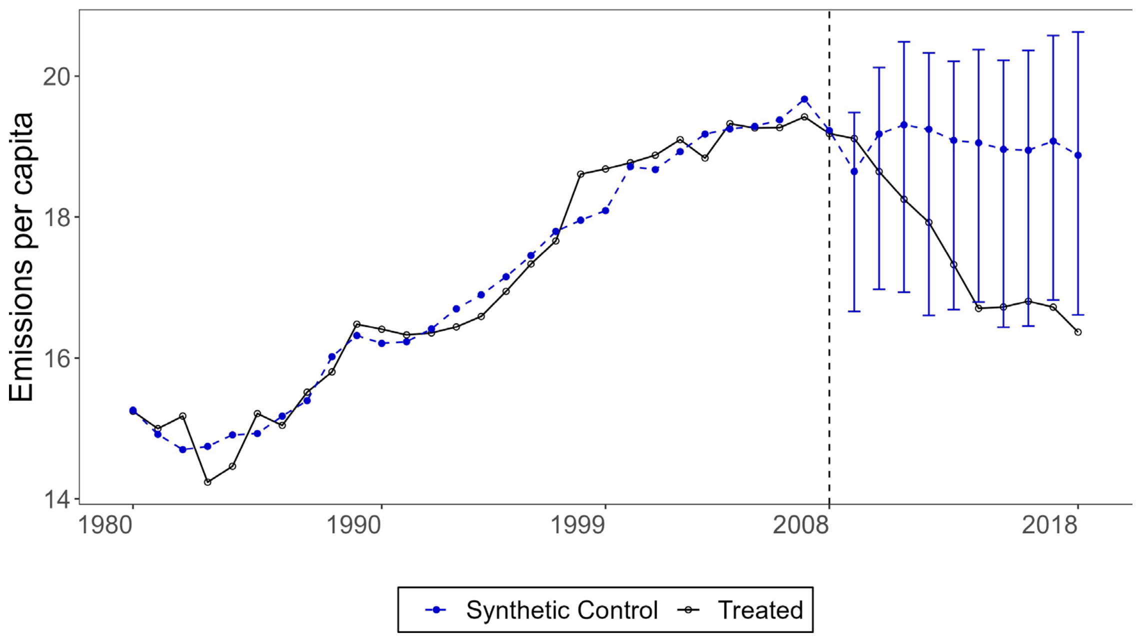


Figure F2: **Prediction intervals, only outcomes and intercept.** Intervals formed using methodology described in Cattaneo et al. (2021). The black line represents the level of the outcome for Australia, while the blue line represents the synthetic control. Blue bars report 90% prediction intervals for $Y_{1t}(0)$ Intervals account for in-sample uncertainty using 1000 simulations of the pre-period error process and out-of-sample uncertainty assuming sub-Gaussian bounds.

# Appendix G. More on Mechanisms

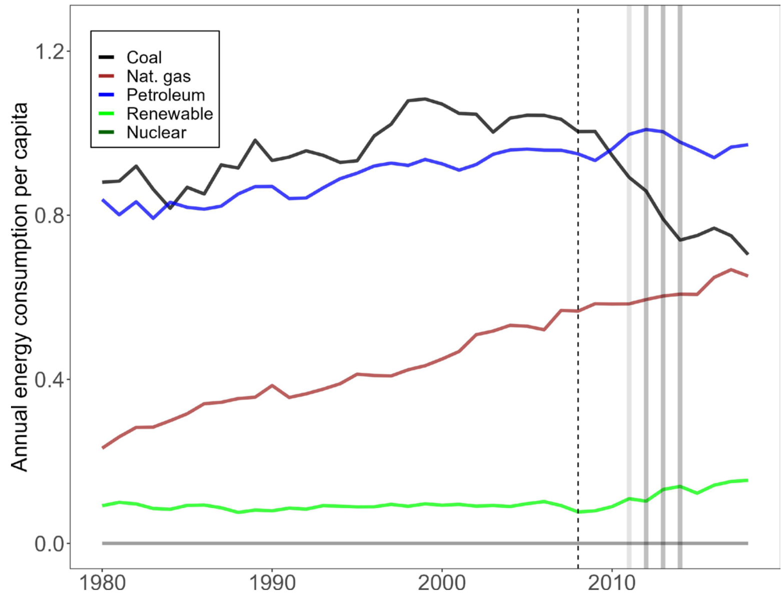


Figure G1: **Australia's energy consumption by source**

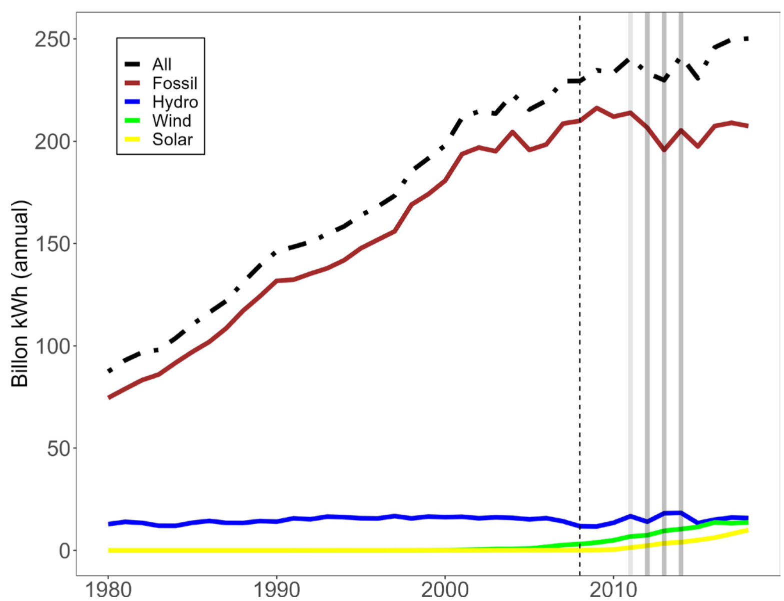


Figure G2: **Australia's electricity generation**

# Appendix H. More on Exports

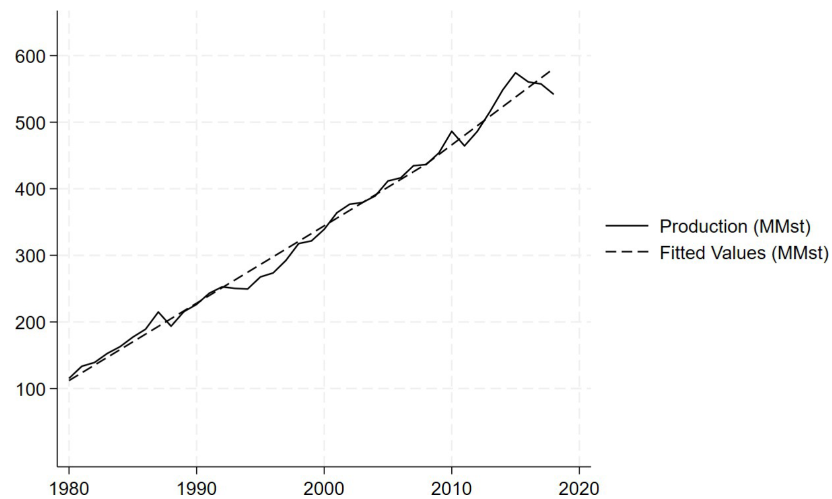


Figure H1: **Australia's coal production and fitted values from time series model**

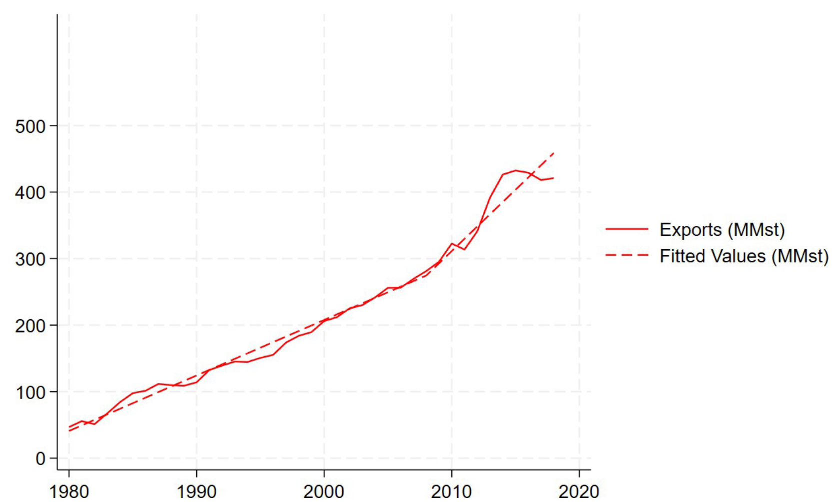


Figure H2: **Australia's coal exports and fitted values from time series model**