

Sen, Suphi; Sadikoglu, Serhan; Ji, Changjing; van der Werf, Edwin

Working Paper

The Effectiveness of Carbon Pricing: A Global Evaluation

CESifo Working Paper, No. 11291

Provided in Cooperation with:

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

Suggested Citation: Sen, Suphi; Sadikoglu, Serhan; Ji, Changjing; van der Werf, Edwin (2024) : The Effectiveness of Carbon Pricing: A Global Evaluation, CESifo Working Paper, No. 11291, CESifo GmbH, Munich

This Version is available at:

<https://hdl.handle.net/10419/305533>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

The Effectiveness of Carbon Pricing: A Global Evaluation

Suphi Sen, Serhan Sadikoglu, Changjing Ji, Edwin van der Werf

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

An electronic version of the paper may be downloaded

- from the SSRN website: www.SSRN.com
- from the RePEc website: www.RePEc.org
- from the CESifo website: <https://www.cesifo.org/en/wp>

The Effectiveness of Carbon Pricing: A Global Evaluation

Abstract

We estimate the effect of the staggered adoption of carbon pricing policies across the globe between 1990 and 2017 on per capita CO₂ emissions from fossil fuel combustion. Applying recent econometric techniques robust to treatment effect heterogeneity, we find reductions of 8 to 12 percent on average. Our dynamic treatment effect estimations indicate gradual adjustments after implementation, resulting in a 19 to 23 percent decrease after 10 years. These effects were primarily driven by resource substitution rather than improvements in energy efficiency, largely independent of the potential effects of renewable energy policies, and were not driven by short-term responses to carbon prices. These results highlight the role of carbon pricing policies in steering medium-term expectations and complementing the climate policy mix.

JEL-Codes: Q410, Q480, Q540, Q580.

Keywords: carbon pricing, cap and trade, emission trading, carbon tax, staggered design, dynamic treatment effects.

Suphi Sen

*Wageningen University, Environmental
Economics and Natural Resources Group
Wageningen / The Netherlands
suphi.sen@wur.nl*

Serhan Sadikoglu

*Tilburg University, Department of
Econometrics and Operations Research
Tilburg / The Netherlands
s.sadikoglu@tilburguniversity.edu*

Changjing Ji

*Shanghaitech University, Institute of Carbon
Neutrality, Shanghai / PR China
jichj@shanghaitech.edu.cn*

Edwin van der Werf

*PBL Netherlands Environmental Assessment
Agency, The Hague / The Netherlands
edwin.vanderwerf@pbl.nl*

October 25, 2024

We thank seminar participants at Kiel Institute for the World Economy, Utrecht School of Economics, the Free University of Amsterdam and the 2024 Annual Conference of the European Association of Environmental and Resource Economists for helpful comments and discussions. Ji acknowledges the support from the China Scholarship Council (Grant no: 202006030113).

1 Introduction

Despite the consensus in the Paris Agreement of 2015 on ambitious climate targets, global emissions are increasing rapidly (Liu et al., 2023), a trend sometimes described as “the greatest market failure the world has ever seen” (Stern, 2007). Economists generally consider carbon pricing policies key to achieving emission targets, as they are efficient by creating incentives to choose the cheapest abatement options. Despite its theoretical appeal, there are heated debates in scientific and policy circles on whether carbon pricing can be effective in addressing the climate crisis in practice.¹ Indeed, there is no consensus on the required level of carbon prices to achieve a certain reduction target, and the modeling predictions of the social cost of carbon, which is the basis for a first-best Pigouvian carbon tax, vary widely depending on modeling assumptions (Adler et al., 2017; Hepburn, 2017). The World Bank’s High-level Commission on Carbon Prices called for a global carbon price starting with at least \$40-\$80 per ton of CO₂ (Stiglitz et al., 2017). The commission suggests an “adaptive policy” which is updated depending on its effectiveness in the past.

The effectiveness of carbon pricing in practice can depart from theoretical predictions. Simulation models for evaluating the potential effects of carbon taxes have been calibrated using estimates of energy price elasticities. However, carbon pricing instruments and energy prices differ fundamentally in their signals for the future costs of carbon emissions. Energy prices embed temporary large variations, while the design of carbon pricing policies, even with low current prices, can imply higher future costs for emitting carbon. In fact, the coverage and price levels of existing carbon pricing schemes are incompatible with the objectives of the Paris Agreement (World Bank, 2023a). Still, a carbon pricing policy can induce emission reductions in the medium to long term by signaling a regulatory commitment to achieving climate targets (Bayer and Aklin, 2020). The strength of these signals and how they are perceived, in turn, can have an impact on the effectiveness of carbon pricing. There is evidence that carbon pricing is perceived as ineffective, and this perception reduces public support for these policies (Baranzini and Carattini, 2017; Carattini et al., 2017; Kallbekken and Sælen, 2011). Hence, its large-scale implementation faces political barriers (Douenne and Fabre, 2022; Meng and Rode, 2019). Understanding how and to what extent carbon pricing has reduced emissions can shed light on these debates.

In this paper, we investigate the effectiveness of carbon pricing policies in reducing CO₂ emissions. The initial implementation of carbon pricing policies took place between 1990 and 2000. Since then, these policies have been adopted by around 40 countries. We leverage this staggered adoption in our analysis. We compile a dataset on enactment years of carbon pricing policies (World Bank, 2023a) and annual CO₂ emissions (IEA,

¹See, for example, Rosenbloom et al. (2020) and van den Bergh and Botzen (2020).

2024a) covering 138 countries between 1981 and 2017. Using recent econometric advances in estimating average treatment effects in staggered designs, we assess the impact of implementing a carbon pricing policy on per capita CO₂ emissions from fossil fuel combustion. We perform thorough tests to assess whether the estimated parameters reflect causal impacts, and provide an in-depth analysis of the potential mechanisms through which these effects might have materialized.

We find that adopting a carbon price, in the form of a carbon tax and/or a cap-and-trade system, reduces per capita CO₂ emissions from fossil fuel combustion by 8 to 12 percent on average in the posttreatment period. The estimated dynamic treatment effects increase in absolute terms in the post-implementation period and stabilize after 10 years, indicating that these effects were realized gradually over the first 10 years following the implementation. The total estimated reduction in emissions per capita after 10 years is around 19-23 percent. We also show that carbon pricing policies achieved this reduction in emissions by triggering a substitution of CO₂-intensive fuels with clean alternatives, rather than improving the energy efficiency of productive activity. Furthermore, we find that the effect of carbon pricing policies was largely orthogonal to the potential effects of renewable energy policies. These results highlight the potential complementarities between carbon pricing, energy efficiency, and renewable energy policies in achieving the climate targets.

Our results imply that the carbon pricing policies enacted so far have achieved substantial emission reductions despite the fact that effective emission prices were generally low during our sample period (1981-2017). Indeed, we find that the observed carbon prices in our sample period do not have much explanatory power on the variation in emission levels. Hence, our results are likely to be driven by anticipatory responses due to a shift in expectations, induced by carbon pricing policies, toward higher carbon prices in the future (Bayer and Aklin, 2020). The enactment of a carbon pricing policy may signal a commitment of regulatory authorities to stricter climate policies in the future, which, in turn, can drive incentives toward timely diversification away from dirty investments. Investments in long-lasting dirty capital can result in lock-in effects leading to high compliance costs for decades. Given the limited carbon budget to keep global warming below a threshold level, timely diversification away from carbon-intensive capital can also reduce the risk of asset stranding.² This result has important implications for the potential role of carbon pricing in low-emitting developing countries, such as those proposed for Africa (UNFCCC, 2024).

The implications of our results for global emissions, and hence for the efficacy of carbon pricing, crucially depend on the potential spillover effects of implemented carbon pricing

²For recent reviews on the asset stranding risk, see, for example, Monasterolo (2020), Semieniuk et al. (2021), van der Ploeg (2020), and von Dulong et al. (2023). For evidence on how investors' expectations, climate policy uncertainty, and the risk of asset stranding interact, see Sen and von Schickfus (2020).

policies in unregulated jurisdictions. First, there could be positive spillovers, for example, due to cross-border technology spillovers following green innovations spurred by carbon pricing policies. In this case, our results would reflect a conservative estimate of the global efficacy of carbon pricing. Second, there could be negative spillovers due to carbon leakages. That is, polluting activities in regulated countries can relocate to countries with less stringent regulations, undermining the global efficacy of carbon pricing (Felder and Rutherford, 1993; Hoel, 1991). We provide evidence for small positive spillover effects that are not statistically significant at the conventional levels. These results indicate that, on average, the spillover effects are negligible. If anything, positive spillovers appear to be stronger than the leakage mechanism.

In our analysis we capitalize on two recent developments. First, a considerable number of countries have implemented carbon pricing policies in the last two decades. As a result, we now have a sufficient number of observations on the targeted outcomes to evaluate the effect of these policies. Our dataset includes 43 countries where a carbon tax and/or a cap-and-trade system have been implemented in our sample period. Second, we make use of recent econometric methods proposed in Callaway and Sant’Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D’Haultfœuille (2024) to properly estimate causal parameters of interest in settings with staggered adoption of treatment.³ In our application, countries implemented carbon pricing policies at different points in time. This design differs from the standard Difference-in-Differences (DiD) setup, where treatment occurs at a single point in time. Until recently, the common practice in the presence of staggered treatment has been to interpret standard two-way fixed effect estimates (TWFE) as causal treatment effects, which is based on the strong assumption of constant treatment effects over time and across cohorts receiving treatment at different points in time (Borusyak et al., 2022; de Chaisemartin and D’Haultfœuille, 2020; Goodman-Bacon, 2021). In this paper, we use estimation strategies that address these concerns by allowing for treatment effect heterogeneity and dynamic effects.

There is a large body of empirical literature analyzing the effects of environmental regulations on economic and environmental outcomes at the firm or more aggregate levels.⁴ Our work is related to the literature evaluating the effectiveness of carbon pricing instruments. We contribute to this literature by providing a global assessment that is not prone to external validity issues and, at the same time, by explicitly targeting the causal parameters of interest by applying recent econometric methods for staggered designs. Until recently, there have been a small number of studies on the effectiveness of carbon pricing, and the literature relied on indirect conclusions from estimates of energy

³See Baker et al. (2022) and de Chaisemartin and D’Haultfœuille (2023) for detailed surveys of this literature.

⁴See, for example, Aghion et al. (2016), Fowlie et al. (2012), He et al. (2020), Klier and Linn (2015), Ryan (2012), Sen (2015), and Walker (2013).

and fuel-price elasticities.⁵ Early literature also studied general fuel taxes on transport fuel consumption, which revealed that the effect of taxes and prices can differ (Davis and Kilian, 2011; Li et al., 2014).

As more countries have implemented carbon pricing policies, we now have more evidence on the effectiveness of carbon pricing instruments. It is not surprising that the previous literature provides ex post policy evaluations focusing on a single carbon pricing policy, or a small number of jurisdictions, or a specific sector such as transport (Anderson, 2019; Lin and Li, 2011; Pretis, 2022; Rivers and Schaufele, 2015). There is a growing body of literature evaluating the effects of the European Union Emissions Trading System (EU ETS). One strand of this literature evaluates the impacts of the EU ETS on manufacturing firms in specific European countries.⁶ At a more aggregate level, by using sectoral data across the EU member states, Bayer and Aklın (2020) analyze the effect of the EU ETS based on a comparison of participant and non-participant sectors. The EU ETS plays an important role in our estimations; however, we show that the weight of the other carbon pricing policies around the globe is higher than that of the EU ETS.

Some recent studies provide global assessments of carbon pricing policies. Best et al. (2020) analyze a dataset similar to ours and rely on standard cross-sectional or fixed-effect estimators. A crucial difference in our paper is that, in order to control for pretreatment differences across countries, we extend our dataset back to 1981, and follow recent advances in econometric theory on the estimation of causal parameters of interest in a staggered design. In a recent study, Rafaty et al. (2021) investigate the effects of carbon pricing policies in five sectors for a panel of 39 countries covering 1990–2016 using a synthetic control approach. Their outcome variable is the growth in emissions rather than the emission levels. Our paper differs in terms of sample coverage and outcome of interest, as well as methodology. Our approach explicitly accounts for complications due to "forbidden comparisons" (Borusyak et al., 2022), such as using previously treated groups as controls. We employ recent estimators robust to treatment effect heterogeneity and provide the associated placebo and/or parallel trends tests for the pretreatment differences. Finally, we provide an extensive analysis on the mechanisms that may drive the estimated emission reductions.

The remainder of this paper is organized as follows. In Section 2, we provide background information on carbon pricing and its applications around the world. Section 3 explains our methodology, and Section 4 describes our dataset. In Section 5, we present our main results. In Section 6, we provide detailed analyses on the potential mechanisms

⁵See, for example, Coglianesi et al. (2017), Reiss and White (2005), and Sen and Vollebergh (2018). For thorough reviews of the early literature, see Cuddington and Dagher (2011), Dahl (1993), and Maddala et al. (1997).

⁶For example, see Jaraite and Di Maria (2016) on Lithuania, Marin et al. (2018) and Colmer et al. (2024) on France, Löschel et al. (2019) on Germany, Klemetsen et al. (2020) on Norway, Cael (2020) on the UK, Dechezleprêtre et al. (2023) on the UK, France, the Netherlands, and Norway. Also see Cui et al. (2021) who analyze the effect of the Chinese pilot emission trading systems.

through which carbon pricing can reduce emissions. We analyze the implications of our results for global emissions in Section 7. Section 8 concludes.

2 Background information

In this section, we provide background information on the carbon pricing policies that have been implemented in the past and discuss the mechanisms through which they may have reduced emissions.

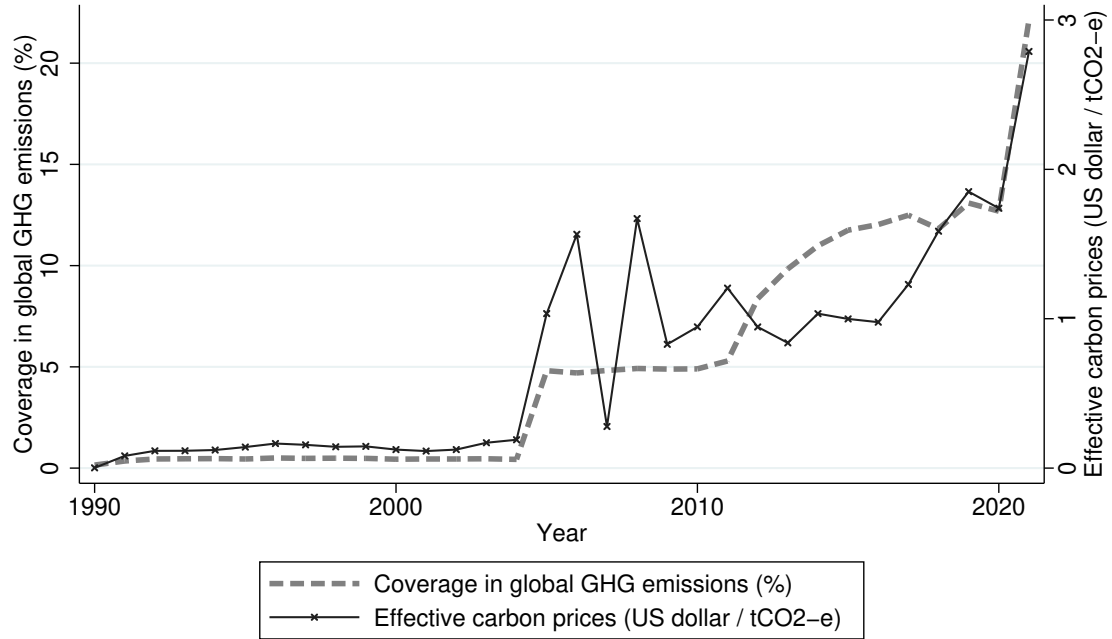
In the 2015 Paris Agreement, countries committed to limit global warming and, hence, to ambitious global emission reductions. Economists often regard carbon pricing, such as a carbon tax or a cap-and-trade system, as the central instrument of climate policy (Stiglitz et al., 2017). Carbon pricing can help achieve a climate goal at the lowest costs for society, follows the polluter pays principle, and can generate government revenues to support auxiliary measures such as subsidies for the adoption of low-emission technologies (Klenert et al., 2018; Nordhaus, 1991; Pearce, 1991; Sterner et al., 2019). In the scenarios of the Intergovernmental Panel on Climate Change (IPCC), the mitigation pathways that achieve the 2°C target with at least 66 percent probability suggest carbon prices ranging between US\$15 and US\$360 per ton of CO₂ equivalent in 2030 (IPCC, 2014). The World Bank High-Level Carbon Prices Commission concluded that a global carbon price of “at least” \$40 to \$80 per tCO₂-e by 2020, and \$50-\$100 by 2030 (Stiglitz et al., 2017) would be consistent with the 2°C target of the Paris Agreement. Rennert et al. (2022) estimate the social cost of carbon, which is the basis for a Pigouvian carbon tax, to be as high as \$185 per ton of CO₂. Hence, the required level of a global carbon price is unclear, and estimates of the social cost of carbon vary widely depending on modeling choices.

The implementation of carbon pricing policies can face political friction due to their distributional effects. Although the benefits of carbon pricing are global and realized over long time horizons, countries bear the costs individually in the short term, and it is up to individual countries whether to implement them. This local burden of climate mitigation is heterogeneous across different segments of society and across industries. Hence, the implementation of such policies at the scale to achieve the climate ambition of the Paris Agreement faces fierce political resistance from, for example, low-income households as in the yellow-vest movement in France (Douenne and Fabre, 2022), or emission-intensive sectors (Meng and Rode, 2019). Furthermore, the perceived ineffectiveness of carbon taxes is a major factor that explains the lack of public support.⁷

The political complexity of implementing carbon prices, let alone high ones, is reflected in the global coverage of greenhouse gas (GHG) emissions, as shown in Figure 1. Although the first carbon pricing instruments were introduced in the 1990s, for the first 15 years,

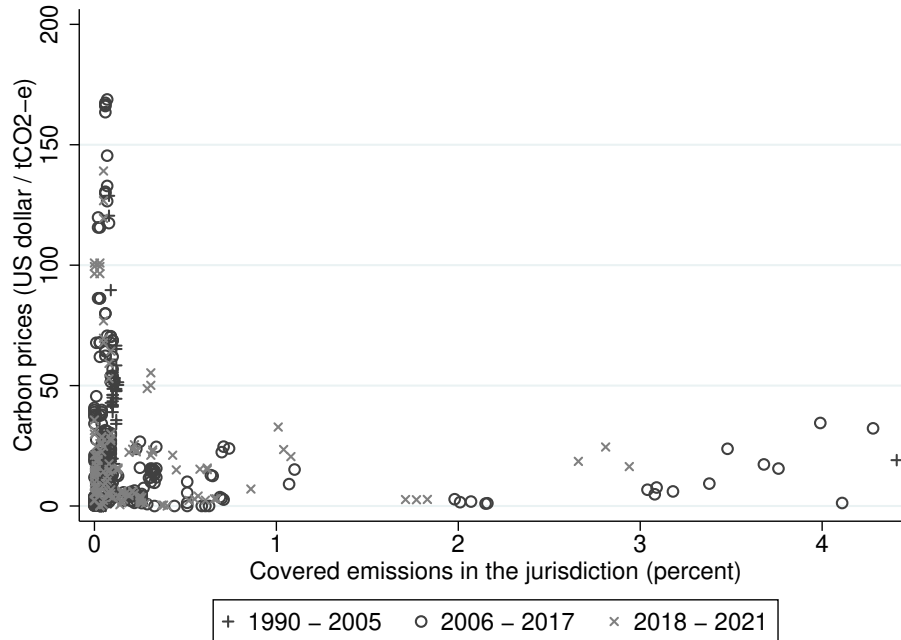
⁷See, for example, Kallbekken and Sælen (2011), Baranzini and Carattini (2017), Carattini et al. (2017) and Douenne and Fabre (2022).

Figure 1: Global effective carbon prices and coverage



Notes: This figure presents the percentage of global GHG emissions covered by carbon pricing schemes and the resulting global effective carbon prices as the weighted average of their coverage in global emissions (2018 US\$ per ton of CO₂-equivalent). Data: World Bank (2023a).

Figure 2: Carbon price levels and their coverage



Notes: This figure depicts the carbon prices (2018 US\$ per ton of CO₂-equivalent) and their coverage (percent of global GHG emissions covered) for all carbon pricing initiatives implemented between 1990 and 2021. Each point represents an observation identified by an initiative and year. Observations between 1990-2005, 2006-2017, and 2018-2021 are labeled with “+”, “O”, and “X”, respectively. Data: World Bank (2023a).

they covered less than 0.5 percent of global GHG emissions. The coverage increased to 5 percent with the introduction of the EU ETS in 2005, and remained around 10 percent until 2014. In 2017, which is the last year in our sample, the coverage was less than 15 percent. Figure 1 also shows that the global effective carbon price, which is an average weighted by covered emissions, has never been higher than \$1.90 per ton of CO₂ in our sample period. This situation may be the result of political frictions.

The design of policies may target these political frictions by, for example, gradually increasing the cost of emitting carbon over time to allow enough adaptation time for economic actors (van den Bergh et al., 2020). Indeed, existing carbon pricing schemes often include elements to ensure dynamic incentives while taking into account political complexity. Many carbon tax schemes start with relatively low rates and specify how the tax rate will increase over time. In Canada, for example, the federal government imposed a carbon price of CA\$20 per ton of CO₂-equivalent in 2019, which reaches CA\$65 in 2023, and CA\$170 in 2030. Emission trading systems, such as the EU ETS, are typically designed in multiple phases where coverage increases and the number of allocated emission allowances decreases over time. The Market Stability Reserve of the EU ETS was approved in October 2015 and began to operate in January 2019 to accommodate supply-demand imbalances that can lead to low prices. The intention is to create “stable expectations that encourage low carbon investments” (EC MEMO, 2014). In 2018, the European Commission approved that the Reserve would be extended with a cancellation procedure in 2023 that eventually led to the invalidation of 2.5 billion allowances (2.5Gt CO₂) in that year. Finally, the Linear Reduction Factor, embedded in the EU ETS, defines the rate of reduction for the annual number of allocated emission allowances. In 2018, the EU increased this rate from 1.74 percent to 2.2 percent.⁸

These design characteristics of carbon pricing instruments can induce firms and households to expect future increases in both coverage and stringency. These expectations may, in turn, shift investments in, for example, new machinery and vehicles to low-carbon alternatives, and thereby reduce current emissions despite low coverage and carbon prices. Figure 2 illustrates the covered share of emissions in global GHG emissions versus carbon prices for each initiative and also shows how this relationship has evolved over time. The observed pattern shows that typically either the coverage of these policies or the imposed price has been low, or both, and that this pattern did not change visibly over time. Therefore, it is unlikely that these policies substantially reduced emissions through short-term price responses. If past carbon pricing policies have been effective in reducing emissions, it is likely that the underlying mechanism is an induced shift in expectations about future carbon prices.

⁸For a detailed discussion of these reforms in the EU ETS, and related political interactions and dynamic incentives, see, for example, Perino (2018), Perino et al. (2022) and Sato et al. (2022).

3 Methodology

In this section, we describe our econometric model and estimation strategy. The empirical context is a panel dataset consisting of 138 countries observed over 37 years with variation in treatment timing.

Two-way fixed effects and staggered design. Our starting point to assess the effectiveness of carbon pricing is the following static two-way fixed effects specification:

$$e_{it} = \alpha_i + \lambda_t + \beta D_{it} + X'_{it}\delta + \varepsilon_{it}, \quad (1)$$

where e_{it} denotes the logarithm of per capita CO₂ emissions from fossil fuel combustion of country i in year t . We denote country and year fixed effects with α_i and λ_t , respectively. Here, D_{it} is the country-specific indicator for the post-treatment period, X_{it} is the vector of covariates, and ε_{it} is the error term. In this study, the treatment is the enforcement of a carbon pricing policy in the form of a carbon tax and/or a cap-and-trade policy. Hence, β is the main parameter of interest reflecting the efficacy of carbon pricing in reducing carbon emissions.

In a standard 2×2 Difference-in-Differences (DiD) design with two periods (before and after treatment) and two groups (treated and control), β would correspond to the average treatment effect on the treated (ATET) under the parallel trends assumption that the expectation of the outcome variable of both experimental groups would follow the same path without treatment. Our setup deviates from the standard DiD design as the treatment is not received at a single point in time. More specifically, we have a so-called staggered design in which cross-sectional units receive the treatment at different points in time and remain in the treatment group thereafter.

The causal interpretation of β in a staggered design, akin to the conventional DiD approach, has faced recent challenges.⁹ Goodman-Bacon (2021) shows that, under the parallel trends assumption, the static TWFE estimator for β is a weighted average of all possible 2×2 DiD estimators in the data. We apply this decomposition to our dataset in the next section. Goodman-Bacon (2021) further shows that the weights sum up to one, but some of them are possibly negative. Negative weights originate from comparisons using earlier treated groups, such as always treated units, as controls. Such comparisons, termed "forbidden comparisons" (Borusyak et al., 2022), can yield negative weights when the treatment effect is heterogeneous over time.¹⁰ Thus, assigning a causal interpretation to the estimand β requires the assumption that the treatment effect is constant over time,

⁹See Baker et al. (2022) and de Chaisemartin and D'Haultfœuille (2023) for detailed surveys of this literature.

¹⁰In this case, one could obtain a negative (positive) estimand β even though the treatment effects on all groups are positive (negative).

which is unlikely to hold in most applications (de Chaisemartin and D’Haultfœuille, 2023).

Event-study specification. Empirical studies with staggered design often use the event-study specification (i.e., a dynamic TWFE regression specification) to estimate dynamic treatment effects and provide a basis to assess the parallel trends assumption. The event study specification can be expressed as follows:

$$e_{it} = \alpha_i + \lambda_t + \sum_{l=-K}^L \beta_l D_{it}^l + X'_{it} \delta + \varepsilon_{it}, \quad (2)$$

where l measures the distance to the treatment date. Denote the date at which unit i starts receiving the treatment with T_i which defines a cohort of units that receive treatment on the same date. Then $D_{it}^l = 1\{t - T_i = l\}$ is an indicator for an individual i in cohort T_i which is l periods away from the treatment date. We follow the standard practice and exclude the period before treatment $l = -1$ as the base period, which we suppress in specification (2) for brevity.¹¹ It is common to bin or trim the periods outside the event window given by the set $[K, L]$.¹² Whether trimmed or binned, in our setup, β_l compares the difference between emissions of treated and untreated countries at l periods away from the enactment of a carbon pricing policy with the same difference in the base period.

In the event-study specification, the coefficients β_l capture dynamic treatment effects. For $l < 0$, β_l captures the DiD in the pretreatment period, which can be exploited as in-time placebo tests and provides a basis for evaluating the assumption of parallel trends. For $l \geq 0$, β_l captures the DiD in the post-treatment period and provides a basis to estimate the ATET. However, this interpretation requires the strong assumption of homogeneous treatment effects across cohorts (Borusyak et al., 2022; de Chaisemartin and D’Haultfœuille, 2020; Sun and Abraham, 2021), such that treatment effects follow the same path for every cohort in terms of the relative time index, which is a rather strong assumption. We therefore also apply recently developed estimators that allow for such heterogeneity in treatment effects.

Robust dynamic treatment effect estimators. We apply estimation strategies that are robust to heterogeneous treatment effects across groups and over time, recently introduced by Sun and Abraham (2021) (S&A), de Chaisemartin and D’Haultfœuille (2024) (dC&D), and Callaway and Sant’Anna (2021) (C&S). The differences between these es-

¹¹If there are no never-treated units, one needs to exclude two dynamic treatment indicators (Borusyak et al., 2022). We do have never-treated units in our sample.

¹²Trimming excludes these distant periods from the estimation sample, leading to a balanced panel in relative periods. The alternative is to keep these periods in the estimation sample by binning them into two groups $l < K$ and $l > L$, each represented with a single dummy variable in the event-study specification.

timators can be broadly classified by three main aspects, namely the construction of control group, the imposed parallel trends assumptions, and the use of covariates. Here, we discuss these differences and their implications for our estimations.

A staggered design generally includes alternative control groups. S&A propose a regression-based estimator following the event-study design and use either the never-treated group, or the last-treated group if there are no never-treated. In our case, we have a large never-treated group. dC&D and C&S allow using not-yet-treated units, which can be later-treated and/or never-treated units. In our baseline estimations, we use not-yet-treated units whenever possible. We also provide a robustness check by only using later-treated units as the control group.

Marcus and Sant’Anna (2021) investigate the differences in the parallel trends assumptions imposed by these three estimators. They all restrict posttreatment trends in the classical sense. That is, without treatment, the evolution of expected outcomes over time is the same for each group across all time periods. Marcus and Sant’Anna (2021) show that S&A and dC&D rely on a further assumption, namely the presence of parallel pretreatment trends, which C&S do not necessarily require. However, there is a trade-off between robustness to parallel pretrend violations and the efficiency of the estimators. We favor efficiency, as our dataset includes cohorts with few treated units, and apply the C&S estimator with a regression adjustment approach.¹³ For the same reason, we use bootstrapped standard errors with the dC&D and C&S estimators. We use asymptotic standard errors only with the regression-based estimators including the S&A estimator.¹⁴

The estimators introduced by dC&D and C&S allow the parallel trends assumption to hold conditional on covariates. dC&D further allow for time-varying covariates by imposing parametric restrictions. Time-varying covariates are incorporated through sample restrictions in the C&S approach. In our application, we use time-varying covariates. Following the common practice, We do so too when applying the S&A estimator, even though S&A derive their results under an unconditional parallel trends assumption.

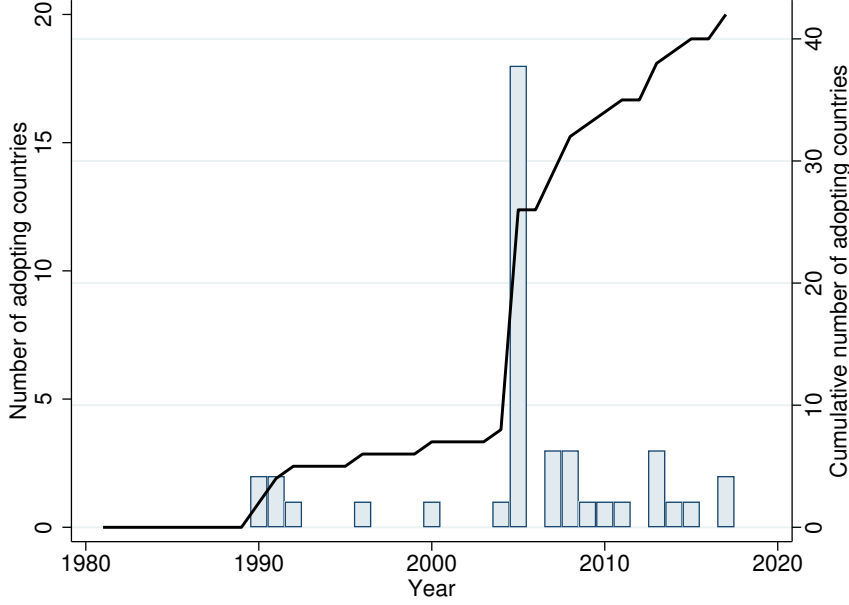
4 Data

This section describes our dataset. We start by describing our outcome and treatment variables. Next, we apply the Goodman-Bacon (2021) decomposition to provide some initial insights into our dataset. Finally, we describe our control variables.

¹³Using other alternatives, such as doubly robust regression and inverse probability weighting, does not change our conclusions. However, inverse probability weighting yields stronger effects compared to our baseline estimates.

¹⁴Using asymptotic or bootstrapped standard errors do not change our conclusions. The only exception is that the estimated asymptotic standard errors with the dC&D estimator are very large.

Figure 3: Adoption of carbon pricing policies



Notes: The bars show the number of countries that adopted a carbon pricing policy in a given year (cohort size; left axis); the line shows the cumulative number (right axis).

Outcome and treatment variables. Our treatment variable is a binary variable indicating whether a country had implemented a carbon pricing policy (a carbon tax and/or a cap-and-trade system) at the national and/or sub-national level based on the Carbon Pricing Dashboard provided by the World Bank (World Bank, 2023a).¹⁵ Figure 3 shows the historical development of the adoption of carbon pricing instruments. In our dataset, 43 countries have either a carbon tax or a cap-and-trade system, or both, in place by 2021. The number of countries adopting a carbon pricing policy in a given year, called cohort size, is generally three or less. The exception is the 2005 cohort, which consists of 18 EU Member States who newly adopted a carbon price with the introduction of the EU ETS. This cohort is by far the largest in our sample. However, its weight in our estimations is less than 0.5 as we will show later in this section. This observation highlights the importance of a global analysis.

Most of our estimation strategies assume that once a unit receives the treatment, it remains in the treatment group thereafter. However, in 2015, Australia abolished its emission trading scheme, which was introduced in 2012. This pattern violates the assumption of staggered design. Therefore, we simply drop Australia in our analyses. Including Australia has a negligible effect on our results.

In this paper, we exploit this staggered adoption of carbon pricing policies to estimate the causal effect of carbon pricing policies on per capita CO₂ emissions from fossil fuel

¹⁵See https://carbonpricingdashboard.worldbank.org/map_data. We provide the resulting list of countries in Appendix A.

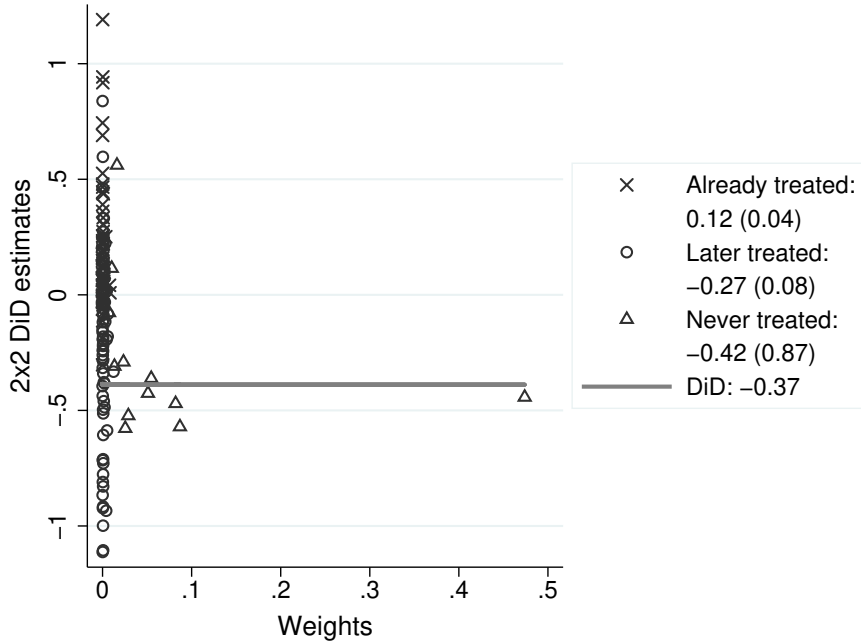
combustion, which we retrieve from the database of the International Energy Agency (IEA, 2024a). Fossil fuel combustion is the main source of CO₂ emissions and the main target of existing carbon pricing policies. Our baseline sample covers 138 countries from 1981 to 2017. Our choice of baseline sample period is mainly driven by two factors. First, as the first carbon pricing policies were introduced in the 1990s, we extend our dataset back to 1981 to better understand pretreatment differences in the evolution of per capita emissions across countries. The number of countries with available data on CO₂ emissions for 1980 and before is considerably lower. Second, as we described in Section 2, carbon prices and/or their coverage remained low until 2018 when the EU ETS price started to rise, coinciding with the implementation of the Market Stability Reserve within the EU ETS and the approval of the cancellation mechanism for a fraction of the reserved allowances. Furthermore, global energy markets experienced large shocks after 2018 such as the COVID-19 outbreak and the Russia-Ukraine war. Hence, we cap our baseline sample period at 2017 to avoid the potential influences of these large shocks on our results. We provide robustness checks on these choices.

Goodman-Bacon decomposition. Next, we apply the Goodman-Bacon decomposition (Goodman-Bacon, 2021) to our dataset to obtain some initial insights. The outcome variable is emissions per capita in logarithms, and the treatment variable describes whether a country has a carbon pricing policy in place in a given year. We apply the decomposition method without using any covariates, which eases interpretation and is sufficient to have an overview of the various comparisons that our dataset offers. However, the estimated DiD parameters should be interpreted with caution, as discussed in Section 3.

Figure 4 presents the results. It shows all possible 2×2 DiD estimates in our data and their weights. Each comparison is a combination of a treatment cohort and a potential control group. A treatment cohort is a group of countries that adopted a carbon pricing policy in the same year. Potential control groups are already-treated, never-treated, and later-treated groups. Throughout the paper, we refer to never- and later-treated groups together as the not-yet-treated group. The figure also reports the overall DiD estimate, which is simply the weighted average of all these DiD estimates. The estimated parameter indicates that having a carbon pricing policy in place is associated with 37 percent less per capita emissions on average, which is unrealistically high. This result is basically an artifact of not using any control variables and hints that the unconditional parallel trends assumption is not likely to hold. The main insights that can be derived from Figure 4 are more about the relative sizes of the group-specific DiD estimates and their weights, rather than their levels.

There are several takeaways from Figure 4 that will be helpful in interpreting our main results. First, using the already-treated units as a control group suggests a positive

Figure 4: Goodman-Bacon decomposition



Notes: This figure presents the results from the Goodman-Bacon decomposition (Goodman-Bacon, 2021) based on the estimation without any control variables. It shows all possible 2×2 DiD estimates in our data on the vertical axis and their respective weights on the horizontal axis. Each comparison is a combination of a treatment cohort and a potential control group. A treatment cohort is a group of countries implementing a carbon pricing policy in the same year. Potential control groups include already-treated, never-treated, and later-treated units. The legend of the figure also shows the average effects per comparison group together with their weights in parentheses.

effect of carbon pricing on CO₂ emissions, illustrating the problems related to forbidden comparisons discussed in Section 3. However, the weight of these comparisons in the overall estimated effect is only 4 percent. Therefore, the results are mainly driven by the valid comparisons with the never-treated and later-treated groups.

The overall average effect seems to be driven by the comparisons using the never-treated units as the control group, which leads to an estimate of -0.42 with a substantial weight of 87 percent. The comparisons based on the later-treated units yield a much smaller average effect, -0.27, with an 8 percent weight. We use estimation strategies that make use of these different groups.

Another important lesson from Figure 4 is about the comparison with a weight of 0.45, which is the weight of the comparison of the cohort of EU countries that adopted the EU ETS in 2005 but had not introduced a carbon price before, with never-treated units. The weight of the comparison of this cohort with the later-treated group is rather small. Overall, while EU ETS has considerable weight in our estimations, the rest of the cohorts still have a weight of more than 0.5, which highlights the importance of a global analysis.

Control variables. Our main control set includes logarithms of GDP per capita, population, and the share of urban population in total population. Throughout the paper, we refer to these variables as our *core* control set. GDP per capita series are from the Penn World Table (Feenstra et al., 2015).¹⁶ We obtained urban and total population data from the World Development Indicators (World Bank, 2023b).

We interpret our econometric model as a demand function for emissions in which emission is proportional to energy consumption. This approach is consistent with the fact that the IEA calculates emissions by multiplying the consumption levels of specific energy resources by their emission coefficients. From this perspective, GDP per capita controls for income effects. The strong correlation between GDP per capita and emissions per capita has been extensively analyzed in the Environmental Kuznets Curve literature where the main interest is a potential nonlinear relation between these two variables, captured by nonlinear terms of GDP per capita in a regression analysis.¹⁷ Following this literature, we also introduce its square in our core control set with an ad-hoc approach. Second, we control for the size of labor force with population, which accounts for any deviation from a constant returns-to-scale assumption. Our results are not sensitive to using employment instead of population data; however, the former restricts our sample size considerably. Third, we use urbanization rate to control for the feedback between spatial agglomeration of economic activity and resource efficiency (Martin and Ottaviano, 2001). We show that this *core* control set is sufficient to control for the differences in the pretreatment evolution of per capita emissions between the control and treatment groups. We provide further details on our dataset together with descriptive statistics in Appendix A.

In our baseline specification, we do not control for energy prices. We consider our treatment, the implementation of carbon pricing policies, and associated carbon prices as an exogenous source of variation in end-use energy prices, which is a common assumption in the literature.¹⁸ Using energy price information substantially reduces our sample size. However, we show that our results are robust in this subsample to controlling for end-use prices of energy and individual resources.

In addition to our *core* control set, we collect data on other control variables commonly used in the literature and provide extensive robustness checks. This *extended* control set includes a wide range of country characteristics in several dimensions: (i) *energy sector* characteristics such as energy intensity, (ii) other relevant *policy* measures such as the presence of feed-in tariffs for renewable energy, and (iii) *institutional factors* such as a democracy index. However, these variables either restrict our sample size considerably without adding much explanatory power (such as the democracy index), or they are

¹⁶We use the expenditure-side purchasing power parity adjusted real GDP series in 2017 US dollars.

¹⁷See Dinda (2004) for a review of the early literature.

¹⁸See, for example, (Davis and Kilian, 2011).

Table 1: Effect of carbon pricing on per capita CO₂ emissions

	(1)	(2)	(3)	(4)	(5)
Estimators	TWFE	Event Study	S&A	dC&D	C&S
Carbon pricing	-0.138 (0.044)	-0.126 (0.030)	-0.114 (0.030)	-0.081 (0.035)	-0.118 (0.026)
Placebo test (p-val)	–	0.629	0.864	0.654	0.552
Num. of obs.	4592	4592	4592	4592	4592
Control set	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>

Notes: This table presents the estimated effects of carbon pricing on emissions per capita based on standard TWFE and event-study estimations, and estimators introduced by Sun and Abraham (2021) (S&A), de Chaisemartin and D’Haultfoeulle (2024) (dC&D), and Callaway and Sant’Anna (2021) (C&S). The dependent variable is per capita CO₂ emissions from fossil fuel combustion in logarithms. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The control set *Core* includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors clustered at the country level are in parentheses. dC&D and C&S standard errors are bootstrapped with 800 replications. Placebo tests are on the joint significance of estimated placebo treatment effects for the 10 years preceding the treatment.

likely to qualify as the so-called “bad controls” (such as energy intensity), which refers to variables that are themselves likely to be affected by the treatment. The elements of our core control set are not likely to be bad controls. First, we do not see a significant reason why carbon pricing can affect demographic variables. Second, a common finding in the literature is that carbon pricing does not affect the level of economic activity, measured by GDP per capita in our application.¹⁹

5 Baseline results

In this section, we present our baseline results. We start by presenting the estimated average effects of carbon pricing. Next, we present the results of dynamic treatment effect estimations. Finally, we outline the extensive robustness checks on these baseline results presented in the appendix. In the next section, we provide further analyses on potential mechanisms underlying our baseline estimations.

Average effect of carbon pricing. Table 1 shows the estimated average effect of carbon pricing on per capita CO₂ emissions from fossil fuel combustion using the various estimators described earlier, together with the results of corresponding pre-trend tests. The first two columns present the results from the standard TWFE and event-study estimations, serving as benchmarks for our preferred estimation strategies. The following columns present our main results from using the estimators that are robust to treatment effect heterogeneity, introduced by Sun and Abraham (2021) (S&A), de Chaisemartin and

¹⁹See, for example, Colmer et al. (2024).

D’Haultfoeulle (2024) (dC&D), and Callaway and Sant’Anna (2021) (C&A), respectively.

The estimated ATETs, presented in Table 1, suggest that implementing a carbon pricing policy reduces emissions per capita by 8 to 14 percent on average in the posttreatment period compared to a counterfactual in which these policies had not been implemented.²⁰ Standard errors are small; therefore, the coefficients are all statistically significant with respect to conventional benchmarks, such as the 1 percent significance level. The estimated average treatment effect from TWFE is 14 percent. The following columns present the results from allowing for dynamic treatment effects. The standard event-study regression yields results similar to those of the TWFE estimation. The estimated average effect is 13 percent. The last three columns include the results from the estimators specifically designed for the subtleties in the presence of staggered treatment. Using the S&A and the C&S estimators does not change the conclusion from the TWFE and the standard event-study results. The estimated effects are 11 and 12 percent, respectively. Compared to these results, the estimated effect from using the dC&D estimator is lower, 8 percent.

To clarify the differences in these estimation results in Table 1, Figure 5 illustrates the estimated coefficients in absolute terms together with the 90 and 95 percent confidence intervals. The figure shows that the differences between the estimation results are not large given their confidence intervals. The dC&D estimate is noticeably conservative compared to the others, which is the general pattern in our robustness checks. However, in Appendix D, we show that the difference between the dC&D and C&S estimates largely disappears when we repeat our estimations with a balanced sub-sample from our dataset, which is due to the way the two estimators deal with missing covariate values.²¹ When we use a balanced subsample, the estimated effects from the dC&D and C&S estimators are both 14 percent and 3 or 4 percentage points smaller than those from other estimators. In the main text, we present the results from using our full panel, which is unbalanced. These results are more conservative than those from using a balanced subsample. In Appendix D, we show that all our conclusions are robust to using a balanced sub-sample.

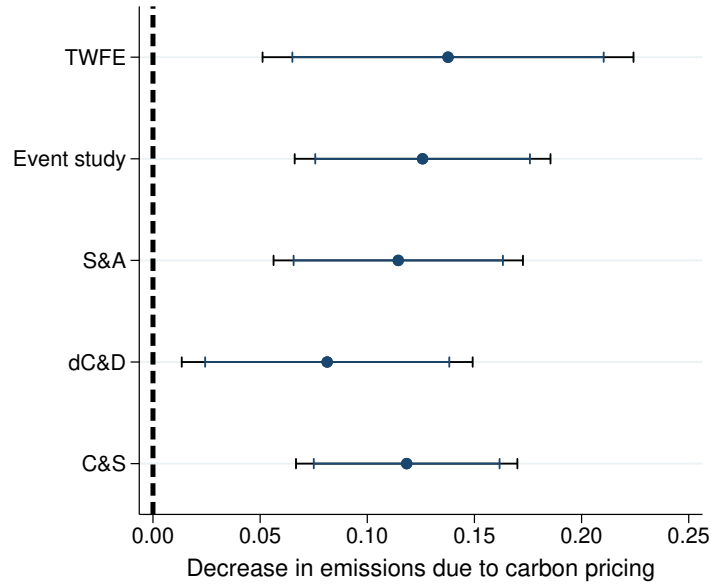
Dynamic effects. Figure 6 shows the dynamic treatment effect estimates underlying the estimated average effects and the corresponding pretrend tests presented in Table 1. The figure includes the results from the S&A, dC&D, and C&S estimators.²² The estimated placebo treatment effects in the pretreatment period are insignificant in all

²⁰Throughout the paper, we interpret the estimated coefficients for the treatment effect as percentage changes in the outcome variable, which is an approximation. The exact percentage change in the outcome variable is given by $100 \times (\exp(\hat{\beta}) - 1)$. The difference is small as long as the coefficient is not too large. For guidance, the exact percentage changes for $\hat{\beta} = \{-0.05, -0.10, -0.15, -0.20, -0.25\}$ are $\{-4.88\%, -9.52\%, -13.9\%, -18.1\%, -22.1\%\}$. Given our point estimates and the associated confidence intervals, we use the approximate interpretation throughout the paper.

²¹The dC&D estimations drop observations with missing covariate values in the estimation of each cohort-time specific treatment effect, while the C&S estimations use pair-balanced data.

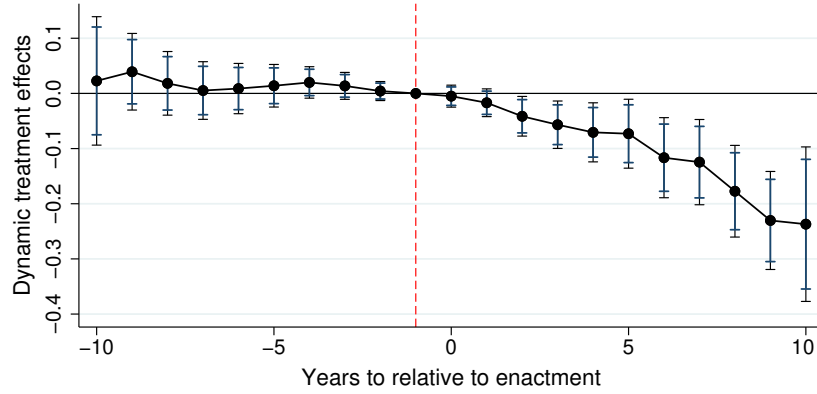
²²The results of the standard event study estimation are similar and are provided in Appendix C.

Figure 5: Decrease in per capita CO₂ emissions due to carbon pricing

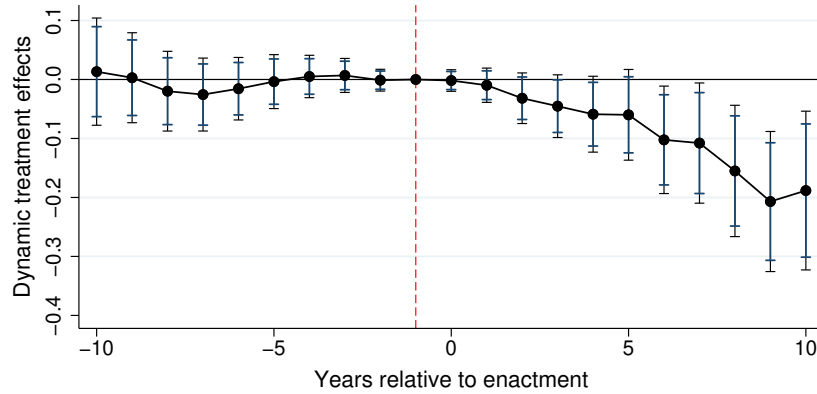


Notes: This figure presents the estimated effects of carbon pricing on per capita CO₂ emissions from fossil fuel combustion based on standard TWFE and event-study estimations, and estimators introduced by Sun and Abraham (2021) (S&A), de Chaisemartin and D'Haultfœuille (2024) (dC&D), and Callaway and Sant'Anna (2021) (C&S). Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors are clustered at the country level. dC&D and C&S standard errors are bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

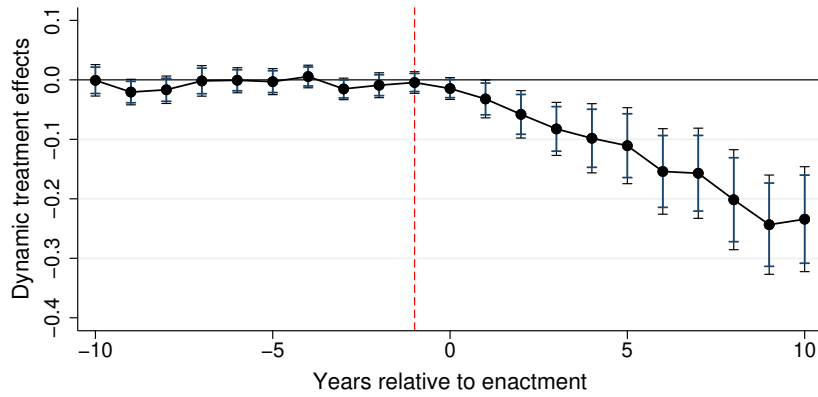
Figure 6: Dynamic treatment effects



(a) Sun and Abraham (2021)



(b) de Chaisemartin and D'Haultfœuille (2024)



(c) Callaway and Sant'Anna (2021)

Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the estimators introduced by Sun and Abraham (2021), de Chaisemartin and D'Haultfœuille (2024), and Callaway and Sant'Anna (2021). Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population and the logarithm of urban population share. Standard errors are clustered at the country level. dC&D and C&S standard errors are bootstrapped with 800 replications. The capped bars show the 90 and 95 percent confidence intervals.

estimations.²³ The pretrend tests presented in Table 1 are about the joint significance of these dynamic placebo treatment effects. More specifically, we test the null of all placebo effects to be zero, which is rejected if at least one of them is significant. We cannot reject this null in all tests. These results indicate that our control set is able to explain the pretreatment differences in the evolution of emissions per capita between the treatment and control groups, which supports the imposed conditional parallel trends assumptions.

In Figure 6, the parameter estimates represent the percentage reduction in emissions relative to the year before the implementation of a carbon pricing policy, represented by the relative year -1 . Accordingly, the estimated ATETs presented in Table 1 are the averages of these estimated dynamic treatment effects depicted in Figure 6. The results suggest that the estimated effect of carbon pricing has been realized gradually and steadily over the ten years following their enactment. Overall, 10 years after enacting a carbon pricing policy, the estimated reduction in emissions per capita relative to the year before implementation ranges between 19 and 23 percent.

Robustness checks. In the appendix, we present an extensive set of sensitivity checks on our baseline results, some of which we have already mentioned. For convenience, we summarize all these checks here. In these appendices and also in the rest of the paper, we mainly use the dC&D estimator, as it yields the most conservative point estimates and confidence intervals, as shown in the preceding analysis. This estimator fits our purposes as it allows for time-varying covariates and the use of later-treated units in the control group.

In Appendices B and C, we provide further details and complementary results for our baseline analysis. Appendix B provides details of the TWFE estimation results, where we also present the coefficient estimates for our core control set. We present the estimated dynamic treatment effects obtained from the standard event-study method in Appendix C.

In Appendix D, we show that our results are robust to using a balanced sub-sample of our baseline sample, and using an unbalanced sample tends to yield slightly conservative results with respect to the efficacy of carbon pricing. These results confirm that our conclusions are not driven by the differences between estimation algorithms in their way of handling missing values.

In Appendix E, we show that our results are robust to having only the later-treated countries in our control group. This strategy may improve the comparability of the treatment levels by restricting the control group to those countries that eventually implemented a carbon pricing policy in our sample period.

²³We follow the common approach for each estimator in estimating the placebo effects. The estimated placebo effects with the C&S estimator are based on the so-called short comparisons between consecutive relative years, and the others are based on long comparisons with respect to the relative year -1 . This choice does not affect any of our conclusions.

In this section and also in the remainder of the paper, we use the core control set described in Section 4. In Appendix F, we use additional control variables for robustness checks. These variables represent factors often hypothesized to be the determinants of CO₂ emissions in the literature. They include characteristics related to the energy sector, further policy measures targeting GHG emissions, and some institutional aspects of the implementing jurisdictions. Some of these variables are likely to be bad controls and some restrict our sample size considerably and do not have much explanatory power. For completeness, we show that our results are robust to controlling for such factors.

We retrieve energy price information from the IEA and perform robustness checks for our baseline results. The enactment of carbon pricing policies can be considered as an exogenous variation in energy prices. Hence, including energy prices in our specification could potentially improve the precision in our analysis. However, using individual resource and energy prices reduces our sample size by more than 75 percent. That is why they are not part of our preferred specification. Nevertheless, in Appendix G, we show that our results are robust to including them as additional covariates in our specification.

6 Further analyses and robustness checks

In this section, we focus on a set of supplementary analyses and further robustness checks to assist in interpreting our baseline results and shed light on the potential mechanisms through which carbon pricing could have affected CO₂ emissions. As explained earlier, we mainly use the dC&D estimator. We start by estimating the elasticity of per capita carbon emissions to effective carbon prices. Next, we proceed by presenting an analysis on the long-term effects of carbon pricing instruments beyond the 10-year window that we used in our baseline estimations. Third, we present a decomposition analysis. We show that the estimated effects are mainly driven by resource substitution rather than energy efficiency improvements. Fourth, we provide compelling evidence that our results are not likely to be driven by renewable energy policies that may have been implemented simultaneously with carbon pricing policies. Finally, we analyze the role of potential spillover effects of carbon pricing policies across countries.

6.1 Responsiveness to carbon prices

Our baseline findings imply a substantial decrease in emissions in countries that had implemented a carbon pricing policy compared to those where a carbon pricing policy had not been implemented. On the other hand, in Section 2, we show that the carbon prices and/or their coverage observed in our sample had been much lower than the levels required to achieve climate goals. These findings imply that our results are not likely to be driven by contemporaneous responses to changes in carbon prices. In this section,

we address this issue formally by estimating the elasticity of demand for emissions with respect to effective carbon prices.

The effective carbon prices presented in Section 2 are based on global coverage ratios provided by the World Bank (World Bank, 2023a). To calculate country-level coverage ratios and implied effective prices, we incorporate additional information from the EDGAR Database on GHG Emissions (Crippa et al., 2024; EDGAR, 2024) and from the European Environment Agency on verified emissions in the EU ETS (EEA, 2024). We describe the data construction and the resulting effective carbon prices in Appendix H in detail.

Table 2: Responsiveness to carbon prices

	(1)	(2)	(3)	(4)
	Sample restrictions on carbon prices			
	>0	> 25 th percentile	> 50 th percentile	> 75 th percentile
Effective carbon prices (in log.)	0.006 (0.009)	-0.044 (0.015)	-0.053 (0.022)	-0.064 (0.048)
Adjusted R^2	0.66	0.72	0.69	0.74
Observations	497	372	248	124

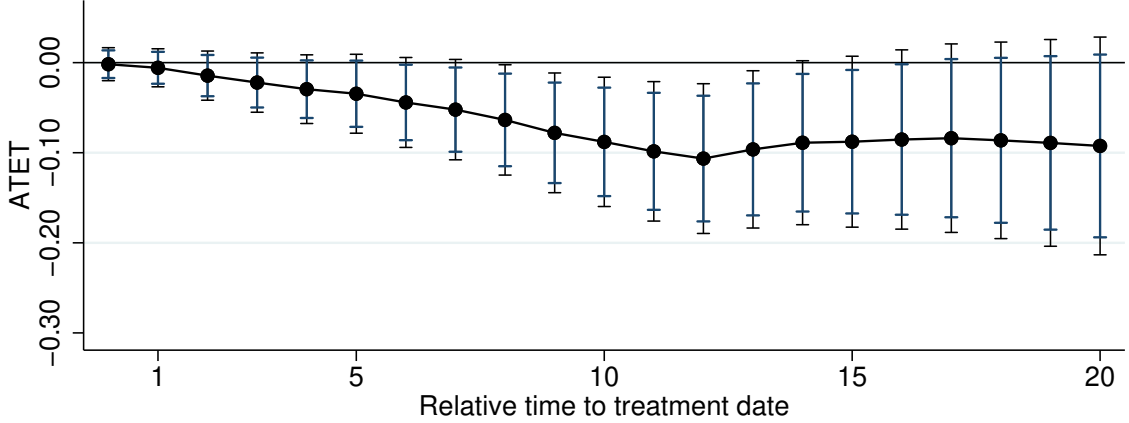
Notes: This table presents the estimated elasticities of emissions per-capita with respect to effective carbon prices. The dependent variable is the logarithm of CO₂ emissions per capita. Effective carbon prices are in logarithms. All regressions control for country and year fixed effects, and the core control set. Standard errors clustered at the country level are in parentheses.

To estimate elasticities, we regress the logarithm of emissions per capita on the logarithm of effective carbon prices by controlling for country and year fixed effects together with our core control set.²⁴ The results are presented in Table 2. In column (1), the estimated elasticity is positive but close to zero. In the following columns, we gradually restrict the estimation sample to higher carbon prices, by trimming the sample at the 25th, 50th and 75th percentile values of carbon prices. We observe several important patterns in these results. First, the estimated effects are negative and significant in columns (3) and (4). Second, the estimated effects are stronger at higher effective prices. Third, the estimated elasticities are still small, around 0.05.

These results confirm that the substantial emission savings implied by our baseline results are not likely to be driven by short-run responses to the year-to-year variations in carbon prices. We observe statistically significant responses only at higher effective prices, which suggests that future increases in effective carbon prices have the potential to be effective in inducing further emission reductions.

²⁴In Appendix H, we repeat this analysis by using a log-linear specification, where we also provide additional results by keeping zero-carbon prices in the estimation sample.

Figure 7: Average treatment effects over relative years



Notes: This figure presents the average effect of carbon pricing on emissions per capita based on the de Chaisemartin and D’Haultfœuille (2024) estimator. Each estimate is the average of the estimated dynamic treatment effects up to a relative year. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population and the logarithm of the share of urban population. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

6.2 Beyond 10 years

In our baseline analysis, we chose a window width of 10 years before and after the treatment. Estimation of dynamic treatment effects beyond 12 years relies on a smaller number of treated units, which leads to imprecision. More specifically, the 2005 cohort, consisting of the EU member states that participated in the first phase of the EU ETS, is not included in the estimation of dynamic treatment effects after the relative year 12, as our sample ends in 2017. Still, given that carbon pricing instruments are intended for long-term impacts, the effects beyond 10 years are of interest. Therefore, we follow several strategies to overcome these difficulties in estimating long-term impacts in our setup.

In Figure 7, we depict the results from one of these analyses, namely the estimated average effects corresponding to the dynamic treatment effect estimations for a window width of 20 years. In this figure, each estimated parameter corresponds to the average treatment effect estimated from the dynamic treatment effects up to a relative year. Hence, for example, the estimated parameter for relative year 10 corresponds to the result presented in Table 1. Inspecting these averages in this extended estimation window can be more informative than the relatively unstable and imprecise individual dynamic treatment effects after the relative year 12, as the former strategy exploits more information.

Figure 7 shows that the ATETs stabilize after about 10 years. Hence, there is no evidence for further emission reductions.²⁵ It would be difficult to explain this pattern

²⁵Suppose that the dynamic effects stabilize after relative year l . Then the average effects would increase and converge to the level of the l^{th} -year dynamic effect as $l \rightarrow \infty$. That is, a stabilization of dynamic treatment effects implies a decelerating increase in the average effects and a gradual stabilization.

with increasing carbon prices. As detailed in Section 2, in our sample period, the global coverage of carbon pricing instruments and their average price level were low. In the previous subsection, we show that effective carbon prices do not explain the variation in per capita CO₂ emissions. Given these findings, the estimated effect of carbon pricing, which is considerable, is likely to be driven by the formation of expectations after the enactment of carbon pricing policies. In fact, the beliefs of the fossil fuel-consuming sectors after the commitment of governments to climate targets may play a crucial role in inducing emission-saving investments. Anticipating future stringency of climate policies, firms and households take gradual steps toward less emission intensive technologies, processes, and/or investments. It is reasonable that such adjustments take place gradually over some years and ultimately their effects saturate in the absence of a strong increase in carbon prices.

In Appendix I, we present the underlying estimated dynamic treatment effects and gauge the long-term impacts of carbon pricing following several more strategies, including extending our sample period up to 2021. As explained earlier, we capped our sample period at 2017, mainly due to the unprecedented surge in the prices of emission allowances in the EU ETS after 2018 and the global COVID outbreak in 2020 which constitute large shocks to energy markets. All our results are robust to extending our sample period up to 2021 which is the latest period with available data. In Appendix I, we also perform a formal test on the stabilization of the dynamic treatment effects within rolling windows over the relative years. The results from these tests support our conclusion in this section that the effects stabilized after about 10 years from the enactments.

These results suggest that the emission savings due to the carbon pricing policies enacted in our sample period will eventually saturate in the absence of new carbon pricing initiatives or significant increases in the level and coverage of existing carbon prices.

6.3 Composition of the total effect

In this section, we present a decomposition analysis which we motivate with the IPAT and Kaya identities (Commoner, 1972; Ehrlich and Holdren, 1971; Yamaji et al., 1991). Scenario analyses and future projections for energy use and emissions by the IEA and IPCC are largely based on these accounting identities. In our context, the IPAT identity can be expressed as follows:

$$\underbrace{CO_2}_{\text{Impact}} \equiv \text{Population} \times \underbrace{\frac{\text{Income}}{\text{Population}}}_{\text{Affluence}} \times \underbrace{\frac{CO_2}{\text{Income}}}_{\text{Technology}}.$$

Here, the environmental impact (I), measured by CO₂ emissions in a given period, is decomposed into three multiplicative components: Population (P), GDP per capita termed

Table 3: Decomposing the total effect of carbon pricing

	(1)	(2)	(3)	(4)
Outcome variable (in log.)	CO ₂ pc.	CO ₂ to GDP	Energy to GDP	CO ₂ to Energy
Carbon pricing	-0.081 (0.035)	-0.065 (0.042)	-0.001 (0.044)	-0.066 (0.020)
Placebo test (p-val)	0.654	0.373	0.397	0.699
Num. of obs.	4592	4592	4592	4592
Control set	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>

This table presents the estimated effects of carbon pricing on the individual components of the IPAT and Kaya identities, namely the CO₂ intensity of GDP, the energy intensity of GDP and the CO₂ intensity of energy use, based on the de Chaisemartin and D’Haultfœuille (2024) estimator. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The control set *Core* includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors clustered at the country level and bootstrapped with 800 replications are in parentheses. Placebo tests are on the joint significance of estimated placebo treatment effects for the 10 years prior to treatment.

affluence (A), and CO₂ intensity of GDP termed technology (T). The Kaya identity can be seen as an extension of the IPAT identity, decomposing the technology term T into two further elements as follows:

$$CO_2 \equiv Population \times \frac{Income}{Population} \times \frac{Energy}{Income} \times \frac{CO_2}{Energy}.$$

The first of these additional terms, *energy intensity* of GDP, reflects energy efficiency. The second one, *CO₂ intensity* of energy use, reflects resource mix. As a result, the Kaya identity decomposes our outcome variable, per capita emissions, into three multiplicative elements: GDP per capita, energy intensity, and emission intensity. We proceed by analyzing the effect of carbon pricing on the latter two components. Note that we have already conditioned our estimations on the first element, GDP per capita, which is in our core control set. We use the total final energy consumption (TFC), retrieved from the IEA database (IEA, 2024b), to calculate the intensity variables. Our results are not sensitive to using total energy supply (TES) instead of TFC.

The results of our decomposition analysis are presented in Table 3. We present the underlying estimated dynamic treatment effects in Appendix J. In the first column, we present our baseline result to ease comparison, namely the effect on per capita CO₂ emissions, which represents the total impact of carbon pricing. For completeness, we start our decomposition analysis with the technology term in the IPAT identity measured by the CO₂ intensity of GDP. In column (2), the estimated effect on this technology term is close to the total effect. This is not a surprising outcome. In the IPAT identity, affluence times technology ($A \times T$), or their sum in logarithms, represents the total per capita impact measured by our outcome variable per capita CO₂ emissions. In all of our regressions, we control for the affluence term on the right-hand side. Hence, moving the

affluence term to the right-hand side of our empirical specification simply recalibrates the coefficient of GDP per capita. Note that we have already imposed the assumption that carbon pricing does not have an effect on income (see the discussion in Section 4). Therefore, the result that the total effect and the effect on the technology term are similar is in line with this design.

Having the IPAT identity confirmed, we now turn to the two crucial terms in the Kaya identity: the energy intensity of GDP and the emission intensity of energy use. Together, these two terms make up the technology term in the IPAT identity. The estimated effects on energy and emission intensities are presented in the third and fourth columns, respectively. The estimated effect on the energy intensity of income is close to zero, while the estimated effect on the emission intensity of energy use is very close to the effect on the technology term. These results suggest that the estimated effects are mainly driven by a reduction in the emission intensity of energy use. A decrease in the emissions-to-energy ratio indicates a change in the resource mix. In fact, the starting point for the emission calculations by the IEA is to multiply the amount of a specific resource by its emission coefficient. This coefficient is a constant indicating the emission per unit of resource use, available at a very disaggregated level for different resource types. Therefore, country-level emissions per energy use can be interpreted as an average emission factor weighted by the amount of resources used to produce that amount of energy.

These results suggest that carbon pricing instruments have reduced emissions by causing a substantial change in the resource mix, rather than reducing the efficiency of productive resources in energy use. By changing the relative prices of energy resources, carbon pricing can create incentives to substitute emission-intensive primary energy resources, such as coal and oil, with cleaner alternatives.

6.4 The role of renewable energy policies

One major concern in identifying causal effects of policies by using aggregate information is the presence of other accompanying policies targeting the same outcomes. While carbon pricing is considered an important instrument in the fight against climate change, it is not the only one in the toolkit of governments. Most prominently, throughout our sample period, many countries have applied both supply- and demand-side policies to achieve electric vehicle (EV) transition, implemented energy efficiency measures, and promoted renewable energy technologies. In this section, we focus on renewable energy policies, as it is less likely that our results are driven by EV policies and/or energy efficiency measures. EV shares in the overall vehicle fleet are still very low. For example, although the Netherlands is one of the most ambitious countries in promoting EVs and consequently has achieved one of the highest EV shares in total vehicle sales worldwide, the market shares for battery electric vehicles (BEVs) and plug-in hybrid electric vehicles (PHEVs)

Table 4: Controlling for renewable energy policies

	(1)	(2)	(3)	(4)
Controlling for	Wind and solar share in		Renewable policies	
	TES	Electricity	TFC	
Carbon pricing	-0.077 (0.034)	-0.057 (0.034)	-0.081 (0.035)	-0.082 (0.034)
Placebo test (p-val)	0.68	0.656	0.614	0.656
Num. of obs.	4583	4586	4592	4583
Control set	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>

Notes: This table presents the estimated effects of carbon pricing on emissions per capita based on the de Chaisemartin and D’Haultfoeuille (2024) estimator by controlling for the potential effects of renewable energy policies. The dependent variable is CO₂ emissions per capita in logarithms. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The control set *Core* includes logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of the share of urban population. Standard errors clustered at the country level and bootstrapped with 800 replications are in parentheses. Placebo tests are on the joint significance of estimated placebo treatment effects for the 10 years preceding the treatment.

were only at 3.1 and 1.7 percent in 2022, respectively.²⁶ Energy efficiency policies aim at reducing energy consumption per unit of output rather than changing the carbon intensity of resource mix. Our results in Table 3 suggest that emission reductions have been largely driven by changes in the resource mix rather than improvements in energy efficiency.

In order to control for the effect of renewable energy policies, we follow two strategies. Our first strategy is an indirect one where we partial out the variation in per capita CO₂ emissions that might have been caused by the surge in renewable energy production. More specifically, we repeat our estimations by controlling for the shares of solar and wind energy in total primary energy supply (TES), electricity generation, and total final energy consumption (TFC).²⁷ For brevity, we will refer to wind and solar as renewable energy sources. The increase in the share of renewables in the last two decades is, of course, not only due to renewable energy policies and may partially have been driven by carbon pricing policies. Hence, this approach is conservative by blocking this potential effect of carbon pricing, and the estimated treatment effects should be interpreted as lower bounds in absolute terms for the effect of carbon pricing.

The difference between TES and TFC is important for the interpretation of these results. TFC includes electricity without distinguishing the various energy sources in its generation, including renewables. Hence, the renewable share reflects nonelectricity use by final energy consumers. However, TES includes all primary energy resources, whether they are used in electricity generation or for some other purposes. The results from controlling for the share of renewable in TES, electricity, and TFC are presented in the

²⁶See the figures provided by the European Commission’s Alternative Fuels Observatory at <https://alternative-fuels-observatory.ec.europa.eu/transport-mode/road/netherlands>.

²⁷We retrieved all data from the World Energy Balances database of the IEA (IEA, 2024b).

first three columns of Table 4. The estimated effects are close to our baseline estimate, especially when we control for the renewable shares in TES and TFC, suggesting that the effect of carbon pricing on CO₂ emissions was largely independent of developments in the renewable energy sector. It is not surprising that the only notable departure from our baseline result seems to occur when we control the share of renewable energy in electricity generation, which is the main outcome targeted by renewable energy policies. In this case, the estimated effect of carbon pricing is a 6 percent reduction in per capita CO₂ emissions, which falls 2 percentage points short of our baseline estimate. This estimate should be considered a lower bound for the effect of carbon pricing, as the remaining reductions following the enactment of carbon pricing policies could also be partially due to carbon pricing.

Our second approach is to construct measures reflecting the most prominent renewable energy policies, namely feed-in tariffs and renewable portfolio standards. Each variable is an indicator variable describing whether a country has such a policy in place in a certain year based on the information we retrieved from the IEA Policies and Measures Database (IEA, 2021). We repeat our estimations by controlling for these policy indicators. The results presented in the last column of Table 4, are virtually the same as our baseline results.

We provide further details of the analysis in this section, including the dynamic effects, in Appendix K.

6.5 Spillover effects

In our context, there are potential mechanisms that may have caused positive and/or negative spillover effects. Positive spillovers can be, for example, due to technology spillovers following green innovations spurred by implemented policies. The presence of such spillover effects would attenuate the estimated effect of carbon pricing; therefore, our estimates could be seen as a lower bound in such a scenario. There are also potential mechanisms that can cause spillover effects in the opposite direction. That is, never-treated and not-yet-treated countries may increase their emissions in response to carbon pricing policies in treated countries, which is called carbon leakage. Ex post assessments of GHG mitigation policies in the literature suggest that carbon leakage has been a minor issue in our sample period.²⁸

In this section, we analyze whether the observed carbon pricing initiatives have caused

²⁸Dechezleprêtre et al. (2022) use data from multinational companies to assess the effect of the EU ETS on the geographic distribution of carbon emissions by multinational companies. They do not find any evidence of carbon leakage. Using firm-level data, Dechezleprêtre et al. (2023) find no effect of the EU ETS on the number of employees and profits in the period 2005-2012. Verde (2020) reaches the same conclusion by reviewing the literature. Martin et al. (2014) use firm-level data and find no effect of the UK Climate Change Levy on employment, revenue, and plant exit for 2001-2004. Naegelé and Zaklan (2019) analyze trade flow data and do not find evidence for carbon leakage due to the EU ETS.

Table 5: Testing for spillover effects

	(1)	(2)	(3)	(4)
Proximity group	Above median based on			Contiguity
	FDI	Trade	1 / Distance	
Spillover effect	-0.025 (0.053)	-0.036 (0.056)	-0.007 (0.049)	-0.007 (0.058)
Num. of obs.	1945	1923	1923	1923
Control set	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>

This table presents the estimated spillover effects of the EU ETS based on standard TWFE estimations. The dependent variable is CO₂ emissions per capita in logarithms. The estimation sample is the never-treated group. The spillover effects are represented by a dummy variable that indicates whether a country is in the high-proximity group where potential spillover effects are more likely to be observed based on various measures. The control set *Core* includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of the share of urban population. Standard errors clustered at the country level are in parentheses.

spillover effects on the emissions of countries that have not implemented such policies. Such potential spillover effects have important implications for the interpretation of our results. First, an interesting question that follows from our analysis is whether future carbon pricing initiatives can yield similar results to those we observe in our sample. At least, the presence of spillover effects would not allow a straightforward extrapolation of our results to future initiatives, as spillover effects are destined to vanish gradually as more countries apply carbon pricing. Second, the implications of our results for global emissions and therefore for the efficacy of climate action depend crucially on the potential spillover effects of implemented carbon pricing policies in unregulated jurisdictions. In the extreme scenario in which the estimated reductions are completely driven by spillovers in the form of carbon leakages, the reduction in global emissions due to implemented carbon pricing policies so far would be zero.

To investigate whether there have been spillover effects of carbon pricing policies, we perform a simple analysis, but conservative with respect to the presence of carbon leakage. Specifically, we examine whether the EU ETS had heterogeneous effects in the never-treated group with respect to several dimensions that can potentially capture the intensity of possible spillover effects. This focus avoids the complications arising from the staggered design. We measure the potential to observe spillover effects with several proximity measures, namely the stock of foreign direct investment (FDI), trade volume, inverse distance, and contiguity between the never-treated countries and the EU.²⁹ For each measure, except for contiguity, we divide the never-treated countries into above and below median groups and investigate whether there are differential patterns in per capita emissions between these groups following the implementation of the EU ETS. In constructing the FDI stock and trade volume indicators, we restrict our sample to flows

²⁹The data on the stock of foreign direct investment is from OECD Database (OECD, 2024). All other data are from the CEPII database (Conte et al., 2022).

from the EU to the countries in the never-treated group. The distance measure is simply the average of the distances to the EU countries of a never-treated country. We use a simple geometric distance measure; however, our results are robust to using other available more sophisticated distance measures in the CEPII dataset, such as population-weighted distance measures. The contiguity variable simply indicates whether a country has a shared border with an EU country. We restrict our sample to the period 1996-2017 and use the presample observations (1995) to obtain an exogenous measure of proximity. Our choice of estimation sample is based on a trade-off between the number of observations to construct the proximity measures and having sufficient pretreatment periods. Our results are not sensitive to reasonable alterations in these choices.

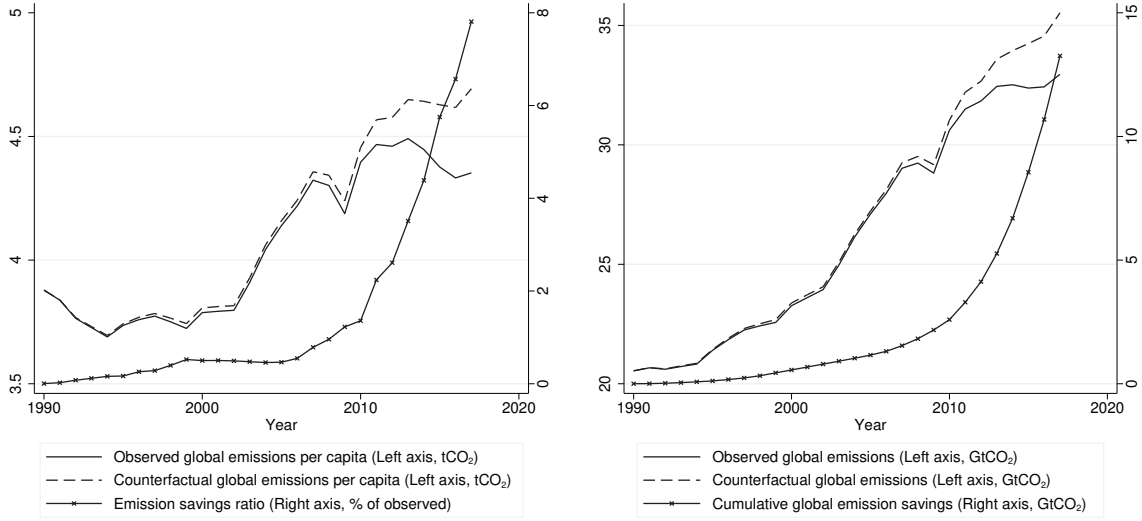
The results are presented in Table 5, where each column uses a different proximity measure. In all regressions, the base category is the group with the lowest proximity. The estimated effects of EU ETS on the proximity groups are small and statistically insignificant at conventional levels of significance. If anything, the estimates are consistently negative across the proximity measures, meaning that emissions per capita of the high-proximity group tend to decrease slightly relative to those of the low-proximity group following the implementation of the EU ETS. This result suggests that, if there were spillovers, their direction was driven by positive spillovers. In this case, our baseline estimate of the effectiveness of carbon pricing should be considered conservative.

7 Implications for global emissions

In this section, we illustrate the implications of our results for global CO₂ emissions. We perform this analysis based on our dynamic treatment effect estimates from the dC&D estimator, which are more conservative compared to those from other estimators. Given our results in the previous section, we ignore potential spillover effects. Our results are presented in Figure 8. The figure shows the global per capita CO₂ emissions on the left panel and the level of global CO₂ emissions on the right panel, both from fossil-fuel combustion. We retrieved these series directly from the IEA database (IEA, 2024a). Each panel also depicts the counterfactual series representing a scenario in which the observed carbon pricing initiatives were not implemented. We obtain these counterfactuals by calculating the emission savings for each treated country based on the estimated dynamic average treatment effects. In line with our results in the previous section, we assume that the dynamic effects stabilize after 10 years from enactment. Next, we aggregate these savings to obtain global savings, both in levels and in per capita terms. The counterfactual series are the sum of these estimated savings and observed emissions.

In the left panel, we observe that the difference between the counterfactual and observed per capita emissions, which we refer to as emission savings, rapidly increases after 2010. This pattern can also be discerned from the depicted emission savings ratio, which

Figure 8: Implications for global emissions



Notes: This figure presents the implications of our baseline dynamic treatment effect estimations for global CO₂ emissions from fossil fuel combustion. The counterfactuals are calculated based on the estimator introduced by de Chaisemartin and D’Haultfœuille (2024). The emissions savings ratio is the difference between the observed and counterfactual emissions expressed as a percent share in observed emissions.

we define as the ratio of emission savings to observed emissions. Note that this ratio is the same in per capita terms or in levels. The emission savings ratio is 8 percent at the end of our sample period 2017. This result suggests that global CO₂ emissions in 2017 would have been 8 percent higher in the absence of implemented carbon pricing policies around the world.

The right panel shows the levels of global observed and counterfactual emissions and cumulative emission savings due to carbon pricing policies since 1990, the year in which the first carbon pricing initiatives were enacted. The counterfactual global emissions are steadily increasing following the historical trend, while the observed emissions increase at a considerably lower rate. In absolute terms, the savings from implemented carbon pricing policies in 2017 are 2.5 gigatons of CO₂. The estimated cumulative emissions savings by 2017 are 13 gigatons of CO₂, most of which are realized after 2010.

In the Sixth Assessment Report of IPCC, the average emission trajectories under the policies that were implemented by the end of 2020 are characterized by a stabilization of annual global emissions and a warming of 2.2°C to 3.5°C.³⁰ Our results show that carbon pricing policies have made a significant contribution to this predicted trajectory.

In the previous section, we have also shown that the estimated effects of carbon pricing policies are likely to have been realized within the first ten years after their enactments, after which these effects level off. Therefore, an out-of-sample prediction would be that,

³⁰See, for example, Figure SPM.5 in the Synthesis Report (IPCC, 2023).

in the absence of new carbon pricing initiatives or significant increases in the existing effective carbon prices, the steep increase in the emission savings ratio after 2010 shown in Figure 8 will eventually decelerate and stabilize. Therefore, existing policies are unlikely to be sufficient to achieve a net-zero emission pathway.

8 Conclusion

The world has over three decades of experience with carbon pricing instruments. In this paper, we assess the extent to which these instruments induced reductions in emissions from fossil fuel combustion, using a country-level panel dataset for the period 1981-2017 covering 138 countries. In the 1990s, a first set of countries implemented carbon pricing policies. In 2017, more than 40 countries had a carbon pricing policy in place. We estimate the effect of carbon pricing on emissions using robust dynamic treatment effect estimators that explicitly address subtleties in this staggered adoption of carbon pricing instruments.

Our results suggest that carbon pricing can be an effective policy instrument in addressing the threat of climate change. We show that the carbon pricing policies, enacted between 1990 and 2017, reduced per capita CO₂ emissions from fossil fuel combustion by 8 to 12 percent on average. By applying dynamic estimation methods, we find gradual adjustments after implementation, resulting in a 19 to 23 percent decrease in per capita emissions after 10 years. These estimates imply that, in 2017, global CO₂ emissions from fossil fuel combustion would have been at least 8 percent higher in the absence of these policies, which corresponds to 2.5 gigatons of CO₂ savings. The implied cumulative CO₂ savings between 1990 and 2017 are 13 gigatons. However, we also show that the estimated effects of carbon pricing policies stabilize after a decade following their enactment. This result suggests that, in the absence of new carbon pricing initiatives or significant increases in existing effective carbon prices, the estimated global emission savings will eventually stabilize.

We also provide evidence that these effects are unlikely to be driven by the variation in carbon prices. Our results are more in line with a mechanism of shifting expectations: as a country commits to a carbon price to reduce its future emissions, firms and households adjust accordingly by anticipating the future costs of inaction. This result challenges the idea that carbon pricing may not be necessary in low-emitting countries, such as those in Africa. According to our results, carbon pricing has the potential to set expectations in line with climate targets. Therefore, a timely implementation can prevent the accumulation of an emission-intensive capital stock and the building up of a dirty infrastructure.

We also find that carbon pricing policies achieved this reduction by triggering a substitution of CO₂ intensive fuels with cleaner alternatives, rather than increasing the energy

efficiency of economic activity. Furthermore, we show that the effects of carbon pricing policies do not overlap with the potential effects of renewable energy policies to a large extent. These results highlight the importance of complementarities between renewable energy policies, energy efficiency measures, and carbon pricing policies.

References

- Adler, M., Anthoff, D., Bosetti, V., Garner, G., Keller, K., & Treich, N. (2017). Priority for the worse-off and the social cost of carbon. *Nature Climate Change*, 7, 443–449. <https://doi.org/10.1038/nclimate3298>
- Aghion, P., Dechezleprêtre, A., Hémous, D., Martin, R., & Van Reenen, J. (2016). Carbon taxes, path dependency, and directed technical change: Evidence from the auto industry. *Journal of Political Economy*, 124(1), 1–51. <https://doi.org/10.1086/684581>
- Andersson, J. J. (2019). Carbon taxes and CO₂ emissions: Sweden as a case study. *American Economic Journal: Economic Policy*, 11(4), 1–30. <https://doi.org/10.1257/pol.20170144>
- Baker, A. C., Larcker, D. F., & Wang, C. C. (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics*, 144. <https://doi.org/10.1016/j.jfineco.2022.01.004>
- Baranzini, A., & Carattini, S. (2017). Effectiveness, earmarking and labeling: Testing the acceptability of carbon taxes with survey data. *Environmental Economics and Policy Studies*, 19, 197–227. <https://doi.org/10.1007/s10018-016-0144-7>
- Bayer, P., & Aklin, M. (2020). The European Union Emissions Trading System reduced CO₂ emissions despite low prices. *Proceedings of the National Academy of Sciences of the United States of America*, 117, 8804–8812. https://doi.org/10.1073/PNAS.1918128117/SUPPL_FILE/PNAS.1918128117.SAPP.PDF
- Best, R., Burke, P. J., & Jotzo, F. (2020). Carbon pricing efficacy: Cross-country evidence. *Environmental and Resource Economics*, 77, 69–94. <https://doi.org/10.1007/s10640-020-00436-x>
- Borusyak, K., Jaravel, X., & Spiess, J. (2022). Revisiting Event Study Designs: Robust and Efficient Estimation. *SSRN Electronic Journal*. <https://doi.org/10.2139/SSRN.2826228>
- Calel, R. (2020). Adopt or innovate: Understanding technological responses to cap-and-trade. *American Economic Journal: Economic Policy*, 12(3), 170–201. <https://doi.org/10.1257/pol.20180135>
- Callaway, B., & Sant’Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225. <https://doi.org/10.1016/j.jeconom.2020.12.001>
- Carattini, S., Baranzini, A., Thalmann, P., Varone, F., & Vöhringer, F. (2017). Green taxes in a post-Paris world: Are millions of nays inevitable? *Environmental and Resource Economics*, 68, 97–128. <https://doi.org/10.1007/s10640-017-0133-8>

- Coglianesi, J., Davis, L. W., Kilian, L., & Stock, J. H. (2017). Anticipation, tax avoidance, and the price elasticity of gasoline demand. *Journal of Applied Econometrics*, 32, 1–15. <https://doi.org/10.1002/jae>
- Colmer, J., Martin, R., Muûls, M., & Wagner, U. J. (2024). Does pricing carbon mitigate climate change? Firm-level evidence from the European Union Emissions Trading System. *The Review of Economic Studies*. <https://doi.org/doi.org/10.1093/restud/rdae055>
- Commoner, B. (1972). The environmental cost of economic growth. *Chemistry in Britain*, 8 2, 52–6 passim.
- Conte, M., Cotterlaz, P., & Mayer, T. (2022). *The CEPII Gravity Database* (Working Paper No. 2022-05). CEPII Research Center.
- Crippa, M., Guizzardi, D., Pagani, F., Banja, M., Muntean, M., Schaaf, E., Monforti-Ferrario, F., Becker, W., Quadrelli, R., Risquez Martin, A., Taghavi-Moharamli, P., Köykkä, J., Grassi, G., Rossi, S., Melo, J., Oom, D., Branco, A., San-Miguel, J., Manca, G., ... Pekar, F. (2024). *GHG emissions of all world countries*. Publications Office of the European Union. <https://doi.org/10.2760/4002897>
- Cuddington, J. T., & Dagher, L. (2011). A primer on estimating short and long-run elasticities : Energy sector applications. *Quarterly Journal of the IAEE's Energy Economics Education Foundation*, 36, 1–35.
- Cui, J., Wang, C., Zhang, J., & Zheng, Y. (2021). The effectiveness of China's regional carbon market pilots in reducing firm emissions. *Proceedings of the National Academy of Sciences*, 118, e2109912118. <https://doi.org/10.1073/pnas.2109912118>
- Dahl, C. A. (1993). *A survey of energy demand elasticities in support of the development of the NEMS* (tech. rep.).
- Davis, L. W., & Kilian, L. (2011). Estimating the effect of a gasoline tax on carbon emissions. *Journal of Applied Econometrics*, 26, 1187–1214. <https://doi.org/10.1002/jae.1156>
- de Chaisemartin, C., & D'Haultfœuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9), 2964–96. <https://doi.org/10.1257/AER.20181169>
- de Chaisemartin, C., & D'Haultfœuille, X. (2023). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *Econometrics Journal*, 26(3), C1–C30. <https://doi.org/10.1093/ectj/utac017>
- de Chaisemartin, C., & D'Haultfœuille, X. (2024). Difference-in-differences estimators of intertemporal treatment effects. *The Review of Economics and Statistics*, 1–45. https://doi.org/10.1162/rest_a_01414
- Dechezleprêtre, A., Gennaioli, C., Martin, R., Muûls, M., & Stoerk, T. (2022). Searching for carbon leaks in multinational companies. *Journal of Environmental Economics and Management*, 112, 102601. <https://doi.org/10.1016/j.jeem.2021.102601>

- Dechezleprêtre, A., Nachtigall, D., & Venmans, F. (2023). The joint impact of the European Union emissions trading system on carbon emissions and economic performance. *Journal of Environmental Economics and Management*, 118, 102758. <https://doi.org/10.1016/j.jeem.2022.102758>
- Dinda, S. (2004). Environmental kuznets curve hypothesis: A survey. *Ecological Economics*, 49(4), 431–455. <https://doi.org/10.1016/j.ecolecon.2004.02.011>
- Douenne, T., & Fabre, A. (2022). Yellow vests, pessimistic beliefs, and carbon tax aversion. *American Economic Journal: Economic Policy*, 14, 81–110. <https://doi.org/10.1257/pol.20200092>
- EC. (2015). European Commission, memo, 14/39.
- EDGAR. (2024). Emissions database for global atmospheric research - report 2024.
- EEA. (2024). European Environment Agency, Emissions Trading Viewer [Accessed: 2024-09-24].
- Ehrlich, P. R., & Holdren, J. P. (1971). Impact of population growth. *Science*, 171(3977), 1212–1217. <https://doi.org/10.1126/science.171.3977.1212>
- Feenstra, R. C., Inklaar, R., & Timmer, M. P. (2015). The next generation of the Penn World Table. *American Economic Review*, 105(10), 3150–82. <https://doi.org/10.1257/aer.20130954>
- Felder, S., & Rutherford, T. S. (1993). Unilateral CO₂ reductions and carbon leakage: The consequences of international trade in oil and basic materials. *Journal of Environmental Economics and Management*, 25, 162–176. [https://doi.org/10.1016/0095-0696\(91\)90023-C](https://doi.org/10.1016/0095-0696(91)90023-C)
- Fowlie, M., Holland, S. P., & Mansur, E. T. (2012). What do emissions markets deliver and to whom? Evidence from Southern California’s NOx trading program. *American Economic Review*, 102(2), 965–93. <https://doi.org/10.1257/aer.102.2.965>
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225. <https://doi.org/10.1016/j.jeconom.2021.03.014>
- He, G., Wang, S., & Zhang, B. (2020). Watering down environmental regulation in China. *The Quarterly Journal of Economics*, 135, 2135–2185. <https://doi.org/10.1093/qje/qjaa024>
- Hepburn, C. (2017). Make carbon pricing a priority. *Nature Climate Change*, 7, 389–390. <https://doi.org/10.1038/nclimate3302>
- Hoel, M. (1991). Global environmental problems: The effects of unilateral actions taken by one country. *Journal of Environmental Economics and Management*, 20, 55–70. [https://doi.org/10.1016/0095-0696\(91\)90023-C](https://doi.org/10.1016/0095-0696(91)90023-C)
- IEA. (2021). International energy agency, policies and measures database [Last accessed on 2021-10-23].

- IEA. (2022). International Energy Agency, Energy Prices [Licence: Terms of Use for Non-CC Material].
- IEA. (2024a). International Energy Agency, Greenhouse Gas Emissions from Energy [Licence: Terms of Use for Non-CC Material].
- IEA. (2024b). International Energy Agency, World Energy Balances [Licence: Terms of Use for Non-CC Material].
- IPCC. (2014). *Mitigation of Climate Change. Contribution of Working Group III to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change* (tech. rep.). Cambridge, United Kingdom, New York, NY, USA, Cambridge University Press.
- IPCC. (2023). *Climate change 2023: Synthesis report. contribution of working groups i, ii and iii to the sixth assessment report of the intergovernmental panel on climate change* (tech. rep.). Core Writing Team, H. Lee, and J. Romero (eds.), Intergovernmental Panel on Climate Change. Geneva, Switzerland. <https://doi.org/10.59327/IPCC/AR6-9789291691647>
- Jaraite, J., & Di Maria, C. (2016). Did the EU ETS make a difference? An empirical assessment using Lithuanian firm-level data. *The Energy Journal*, 37, 68–92. <https://doi.org/10.5547/01956574.37.2.jjar>
- Kallbekken, S., & Sælen, H. (2011). Public acceptance for environmental taxes: Self-interest, environmental and distributional concerns. *Energy Policy*, 39(5), 2966–2973. <https://doi.org/https://doi.org/10.1016/j.enpol.2011.03.006>
- Klemetsen, M., Rosendahl, K. E., & Jakobsen, A. L. (2020). The impacts of the eu-ets on norwegian plants’ environmental and economic performance. *Climate Change Economics*, 11, 2050006. <https://doi.org/10.1142/S2010007820500062>
- Klenert, D., Mattauch, L., Combet, E., Edenhofer, O., Hepburn, C., Rafaty, R., & Stern, N. (2018). Making carbon pricing work for citizens. *Nature Climate Change*, 669–677. <https://doi.org/10.1038/s41558-018-0201-2>
- Klier, T., & Linn, J. (2015). Using vehicle taxes to reduce carbon dioxide emissions rates of new passenger vehicles: Evidence from France, Germany, and Sweden. *American Economic Journal: Economic Policy*, 7, 212–242. <https://doi.org/10.1257/pol.20120256>
- Li, S., Linn, J., & Muehlegger, E. (2014). Gasoline taxes and consumer behavior. *American Economic Journal: Economic Policy*, 6, 302–342. <https://doi.org/10.1257/pol.6.4.302>
- Lin, B., & Li, X. (2011). The effect of carbon tax on per capita CO₂ emissions. *Energy Policy*, 39(9), 5137–5146. <https://doi.org/https://doi.org/10.1016/j.enpol.2011.05.050>

- Liu, Z., Deng, Z., Davis, S., & Ciais, P. (2023). Monitoring global carbon emissions in 2022. *Nature Reviews Earth & Environment*, 205–206. <https://doi.org/10.1038/s43017-023-00406-z>
- Löschel, A., Lutz, B. J., & Managi, S. (2019). The impacts of the EU ETS on efficiency and economic performance – an empirical analyses for German manufacturing firms. *Resource and Energy Economics*, 56, 71–95. <https://doi.org/https://doi.org/10.1016/j.reseneeco.2018.03.001>
- Maddala, G. S., Trost, R. P., Li, H., & Joutz, F. (1997). Estimation of short-run and long-run elasticities of energy demand from panel data using shrinkage estimators. *Journal of Business and Economic Statistics*, 15.
- Marcus, M., & Sant’Anna, P. H. (2021). The Role of Parallel Trends in Event Study Settings: An Application to Environmental Economics. *Journal of the Association of Environmental and Resource Economists*, 8(2), 235–275. <https://doi.org/10.1086/711509/ASSET/IMAGES/LARGE/FG5.JPEG>
- Marin, G., Marino, M., & Pellegrin, C. (2018). The impact of the European Emission Trading Scheme on multiple measures of economic performance. *Environmental and Resource Economics*, 71, 551–582. <https://doi.org/10.1007/s10640-017-0173-0>
- Marshall, M. G., Jagers, K., & Gurr, T. R. (2020). *Manual polity IV project: Political regime characteristics and transitions, 1800-2018*. Center for Systemic Peace.
- Martin, P., & Ottaviano, G. I. P. (2001). Growth and agglomeration. *International Economic Review*, 42(4), 947–968. <https://doi.org/https://doi.org/10.1111/1468-2354.00141>
- Martin, R., de Preux, L. B., & Wagner, U. J. (2014). The impact of a carbon tax on manufacturing: Evidence from microdata. *Journal of Public Economics*, 117, 1–14. <https://doi.org/10.1016/j.jpubeco.2014.04.016>
- Meng, K. C., & Rode, A. (2019). The social cost of lobbying over climate policy. *Nature Climate Change*, 9, 472–476. <https://doi.org/10.1038/s41558-019-0489-6>
- Monasterolo, I. (2020). Climate change and the financial system. *Annual Review of Resource Economics*, 12(1), 299–320.
- Naegele, H., & Zaklan, A. (2019). Does the EU ETS cause carbon leakage in European manufacturing? *Journal of Environmental Economics and Management*, 93, 125–147. <https://doi.org/10.1016/j.jeem.2018.11.004>
- Nordhaus, W. D. (1991). A sketch of the economics of the greenhouse effect. *The American Economic Review*, 81(2), 146–150.
- OECD. (2024). Organisation for Economic Co-operation and Development, FDI Stocks (Indicator) [Last accessed on 01 April 2024].
- Pearce, D. (1991). The Role of Carbon Taxes in Adjusting to Global Warming. *The Economic Journal*, 101(407), 938–948. <https://doi.org/10.2307/2233865>

- Perino, G. (2018). New EU ETS Phase 4 rules temporarily puncture waterbed. *Nature Climate Change*, 8(4), 262–264. <https://doi.org/10.1038/s41558-018-0120-2>
- Perino, G., Willner, M., Quemin, S., & Pahle, M. (2022). The European Union Emissions Trading System Market Stability Reserve: Does It Stabilize or Destabilize the Market? *Review of Environmental Economics and Policy*, 16(2), 338–345. <https://doi.org/10.1086/721015>
- Pretis, F. (2022). Does a carbon tax reduce CO₂ emissions? evidence from British Columbia. *Environmental and Resource Economics*, 83, 115–144. <https://doi.org/10.1007/s10640-022-00679-w>
- Rafaty, R., Dolphin, G., & Pretis, F. (2021). *Carbon pricing and the elasticity of CO₂ emissions* (RFF Working Paper Series No. 21-33). Resources for the Future.
- Reiss, P. C., & White, M. W. (2005). Household electricity demand, revisited. *Review of Economic Studies*, 72, 853–883. <https://doi.org/10.1111/0034-6527.00354>
- Rennert, K., Errickson, F., Prest, B. C., Rennels, L., Newell, R. G., Pizer, W., Kingdon, C., Wingenroth, J., Cooke, R., Parthum, B., Smith, D., Cromar, K., Diaz, D., Moore, F. C., Müller, U. K., Plevin, R. J., Raftery, A. E., Ševčíková, H., Sheets, H., ... Anthoff, D. (2022). Comprehensive evidence implies a higher social cost of CO₂. *Nature*, 610(7933), 687–692. <https://doi.org/10.1038/s41586-022-05224-9>
- Rivers, N., & Schaufele, B. (2015). Salience of carbon taxes in the gasoline market. *Journal of Environmental Economics and Management*, 74, 23–36. <https://doi.org/10.1016/j.jeem.2015.07.002>
- Rosenbloom, D., Markard, J., Geels, F., & Fuenfschilling, L. (2020). Why carbon pricing is not sufficient to mitigate climate change—and how “sustainability transition policy” can help. *Proceedings of the National Academy of Sciences of the United States of America*, 117, 8664–8668. <https://doi.org/10.1073/pnas.2004093117>
- Ryan, S. P. (2012). The costs of environmental regulation in a concentrated industry. *Econometrica*, 80, 1019–1061. <https://doi.org/10.3982/ECTA6750>
- Sato, M., Rafaty, R., Calel, R., & Grubb, M. (2022). Allocation, allocation, allocation! The political economy of the development of the European Union Emissions Trading System. *WIREs Climate Change*, 13(5), e796. <https://doi.org/10.1002/wcc.796>
- Semieniuk, G., Campiglio, E., Mercure, J.-F., Volz, U., & Edwards, N. R. (2021). Low-carbon transition risks for finance. *WIREs Climate Change*, 12(1), e678.
- Sen, S. (2015). Corporate governance, environmental regulations, and technological change. *European Economic Review*, 80, 36–61. <https://doi.org/10.1016/J.EUROECOREV.2015.08.004>

- Sen, S., & Vollebergh, H. (2018). The effectiveness of taxing the carbon content of energy consumption. *Journal of Environmental Economics and Management*, 92, 74–99. <https://doi.org/10.1016/J.JEEM.2018.08.017>
- Sen, S., & von Schickfus, M.-T. (2020). Climate policy, stranded assets, and investors' expectations. *Journal of Environmental Economics and Management*, 100, 102277. <https://doi.org/https://doi.org/10.1016/j.jeem.2019.102277>
- Standaert, S. (2015). Divining the level of corruption: A Bayesian state-space approach. *Journal of Comparative Economics*, 43. <https://doi.org/10.1016/j.jce.2014.05.007>
- Stern, N. H. (2007). *The economics of climate change: The Stern review*. Cambridge University press.
- Sterner, T., Bateman, I., van den Bijgaart, I., Crépin, A.-S., Edenhofer, O., Fischer, C., Habla, W., Hassler, J., Johansson-Stenman, O., Lange, A., Polasky, S., Rockström, J., Smith, H. G., Steffen, W., Wagner, G., Wilen, J. E., Alpízar, F., Azar, C., Carless, D., ... Robinson, A. (2019). Policy design for the anthropocene. *Nature Sustainability*, 2, 14–21. <https://doi.org/10.1038/s41893-018-0194-x>
- Stiglitz, J. E., Stern, N., Duan, M., Edenhofer, O., Giraud, G., Heal, G., la Rovere, E. L., Morris, A., Moyer, E., Pangestu, M., Sokona, Y., Shukla, P. R., & Winkler. (2017). *Report of the High-Level Commission on Carbon Prices* (tech. rep.). Carbon Pricing Leadership Coalition.
- Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225, 254–277. <https://doi.org/10.1016/j.jeconom.2020.09.006>
- UNFCCC. (2024). *A synthesis report: Carbon pricing approaches in West Africa* (Reports). UNFCCC, Regional Collaboration Centre Lomé.
- van den Bergh, J., & Botzen, W. (2020). Low-carbon transition is improbable without carbon pricing. *Proceedings of the National Academy of Sciences of the United States of America*, 117, 232019–23220. <https://doi.org/10.1073/pnas.2010380117>
- van den Bergh, J. C. J. M., Angelsen, A., Baranzini, A., Botzen, W. J. W., Carattini, S., Drews, S., Dunlop, T., Galbraith, E., Gsottbauer, E., Howarth, R. B., Padilla, E., Roca, J., & Schmidt, R. C. (2020). A dual-track transition to global carbon pricing. *Climate Policy*, 20(9), 1057–1069. <https://doi.org/10.1080/14693062.2020.1797618>
- van der Ploeg, F. (2020). *Macro-financial implications of climate change and the carbon transition* (tech. rep.). European Central Bank.
- Verde, S. F. (2020). The impact of the EU Emissions Trading System on competitiveness and carbon leakage: The econometric evidence. *Journal of Economic Surveys*, 34(2), 320–343. <https://doi.org/10.1111/joes.12356>

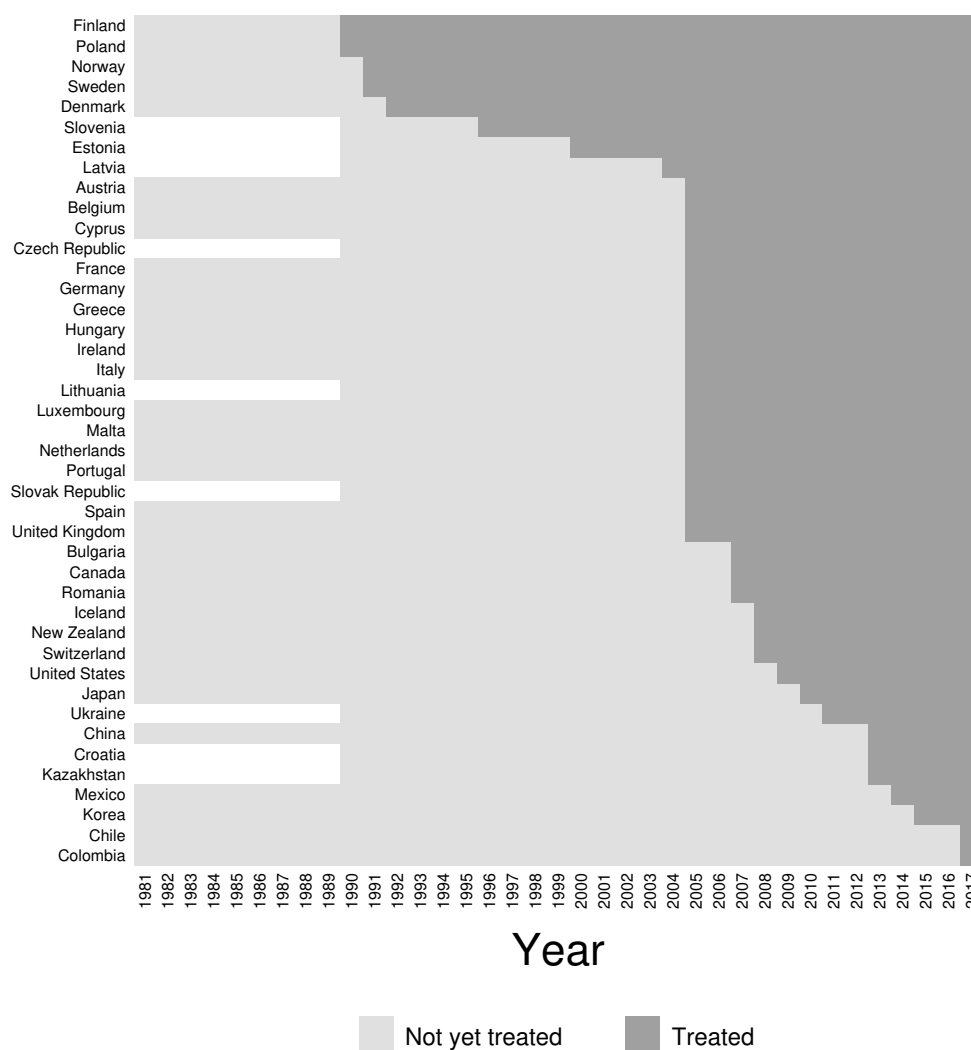
- von Dulong, A., Gard-Murray, A., Hagen, A., Jaakkola, N., & Sen, S. (2023). Stranded assets: Research gaps and implications for climate policy. *Review of Environmental Economics and Policy*, 17, 161–169. <https://doi.org/10.1086/723768>
- Walker, W. R. (2013). The transitional costs of sectoral reallocation: Evidence from the clean air act and the workforce. *The Quarterly Journal of Economics*, 128, 1787–1835. <https://doi.org/10.1093/qje/qjt022>
- World Bank. (2023a). *Carbon Pricing Dashboard*.
- World Bank. (2023b). World development indicators — data [<https://databank.worldbank.org/>, Last accessed on 2023-10-20].
- Yamaji, K., Matsushashi, R., Nagata, Y., & Kaya, Y. (1991). An integrated system for CO₂/energy/GNP analysis: Case studies on economic measures for CO₂ reduction in Japan. *Workshop on CO₂ reduction and removal: measures for the next century*, 19.

Appendix

A More on the dataset

In this section, we provide further details on our dataset and some descriptive statistics. Figure A.1 lists the countries in our treatment group and shows the historical development of the adoption of carbon pricing schemes in our dataset. These countries are the later-treated units before receiving the treatment, or part of the not-yet-treated group together with the never-treated units. In our dataset, we have 95 never-treated countries that are not listed in the figure. In this figure, the white spaces indicate missing data in at least one of the variables in our baseline regressions.

Figure A.1: Countries with a carbon pricing policy



Notes: This figure presents the staggered adoption of carbon pricing policies in our baseline sample period. White spaces indicate missing data in at least one of the variables in our baseline regressions.

Table A.1: Descriptive statistics

	Units	Observations	Mean	Median	St. Dev.	Min.	Max.
Emission pc (log)	kg CO ₂ pc	4826	7.70	8.05	1.55	2.66	10.63
GDP-PPP pc (log)	2017 U.S. dollar pc	4629	9.12	9.19	1.19	5.94	12.32
Population (log)	Millions	4789	16.23	16.14	1.59	11.77	21.06
Urban pop. sh. (log)	Percent	4789	4.00	4.11	0.46	1.85	4.61

Note: This table presents descriptive statistics for the outcome and core control variables, and their units before any functional transformation. All variables are in logarithm. Here, pc stands for per capita.

In Figure A.1, a country switches into the treatment group after adopting a carbon pricing policy at the regional, national, or subnational levels. EU ETS is a regional policy that covers EU member states. The first phase of the EU ETS started in 2005. Its regional coverage changed over time with the enlargement of the EU. For example, Bulgaria and Romania joined the EU ETS in 2007. Also note that some of the EU ETS participants had already implemented a carbon pricing policy in the 1990s, such as Denmark.

Our treated group includes a few countries where initial carbon pricing policies were implemented at a subnational level. For example, China launched the pilot phase of their national emission trading system in 2013 in seven pilot regions, namely Beijing, Chongqing, Guangdong, Hubei, Shanghai, Shenzhen and Tianjin. Together, they account for about a quarter of China’s total GDP. In the United States, the Regional Greenhouse Gas Initiative (RGGI) is an emissions trading scheme covering power plants. It became operational in 2009 in 11 states, namely Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Rhode Island, Vermont, and Virginia. Subsequently, the California Cap-and-Trade Program came into force in 2012 aiming to reduce GHG emissions by 80 percent by 2050 compared to 1990 levels.

In Table A.1, we present some descriptive statistics for our outcome and core control variables in logarithms, together with their original units of measurement. Our dataset is unbalanced with a small number of missing values, which can also be inspected in Figure A.1 where the missing values are indicated with white regions. It can be seen that balancing our dataset by dropping countries with missing observations retains most of our observations. We provide robustness checks with this subset.

B TWFE estimation results in detail

In this section, we present the detailed results of the TWFE estimation, including the coefficient estimates for our core control set. Table B.1 presents the results. In Panel A, we use all the available data per variable. We introduce our control variables gradually column-by-column. In the first column, where we do not use any control variables, the estimated parameter indicates that implementing a carbon pricing policy reduces

emissions per capita by 35 percent on average. However, as soon as we introduce GDP per capita and its square as controls in the second column, the estimated effect reduces drastically to 27 percent. Introducing further covariate sets in the following columns further reduces the estimated effect. The last column presents the results from using our full control set, which suggest a 14 percent reduction in per capita emissions from fossil fuel combustion due to carbon pricing policies.

Table B.1: TWFE Estimations

	(1)	(2)	(3)	(4)
Panel A: Full sample				
Carbon pricing	-0.348 (0.044)	-0.266 (0.041)	-0.179 (0.047)	-0.138 (0.044)
GDP pc (log)		1.560 (0.432)	1.591 (0.469)	1.067 (0.453)
GDP pc (log) squared		-0.066 (0.023)	-0.067 (0.025)	-0.038 (0.024)
Population (log)			0.325 (0.160)	0.223 (0.136)
Urban pop. sh. (log)				0.988 (0.233)
Fixed effects		Country and year fixed effects.		
Adjusted R^2	0.19	0.38	0.39	0.44
Observations	4826	4629	4592	4592
Panel B: Restricted sample				
Carbon pricing	-0.356 (0.043)	-0.259 (0.041)	-0.179 (0.047)	-0.138 (0.044)
Adjusted R^2	0.21	0.37	0.39	0.44
Observations	4592	4592	4592	4592

Notes: This table presents the estimated effects of carbon pricing on emission per-capita based on TWFE regressions. Carbon pricing is a dummy variable indicating whether a country has implemented a carbon pricing policy or not. In the first panel, the regressions use all available data points. Panel B uses the restricted sample which refers to the sample when we use the full control set in the last column in Panel A. Standard errors clustered at the country level are in parentheses.

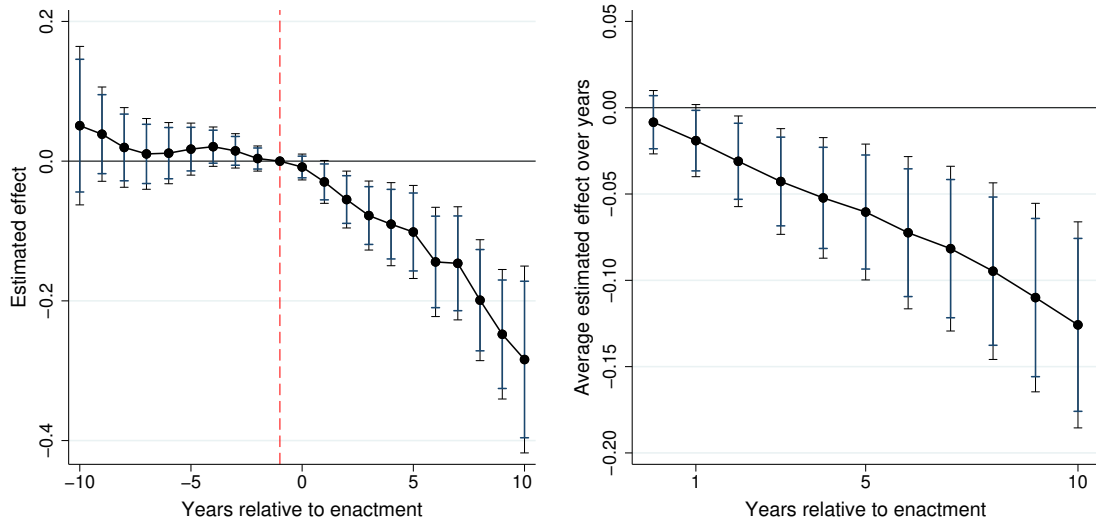
In Table B.1, the column-by-column changes in the estimated effects as we introduce additional covariates are considerable, which highlights the role of our covariate set in explaining the variation in per capita emissions. The number of observations across these regressions changes slightly. In panel B, we verify that this pattern is not driven by sample restrictions due to using further covariates. Here, we repeat our estimations by using the sample of Column (4) of Panel A. The estimated effects per column do not change much with this sample restriction, which verifies the importance of our control variables.

C Standard event-study estimations

Figure C.1 presents the results based on the standard event-study specification (see equation (2)). The placebo treatment effects in the pretreatment period are insignificant. Hence, our model appears to be able to capture the pretreatment differences between the treatment and control groups. However, the estimated placebo treatment effects have a pattern of being positive throughout the pretreatment period, along with a downward trend. The estimated reduction in per capita emissions after 10 years from the enactment is almost 30 percent, which is above the range implied by our baseline estimations.

The right panel presents the average effects up to a relative year after implementation. The estimated parameter for relative year 10 in the right panel corresponds to the estimated parameter based on the standard event-study specification presented in Table 1 in the main body of the text.

Figure C.1: Standard event-study estimations



Notes: This figure presents the results from using the event-study specification in Equation (2). The regressions are based on the core control set. The left panel depicts the dynamic effects on emissions relative to the year before implementation. The right panel presents the average effects up to a year after implementation. The capped vertical bars indicate the 90 and 95 percent confidence intervals based on standard errors clustered at the country level.

D Results from balanced panel

In this section, we show that the dC&D and C&S estimators yield similar results with a balanced subsample. We construct a balanced panel from countries for which we observe all our variables from 1981 to 2017. The results of using this balanced panel are presented in Table D.1. We observe two important patterns. First, our general conclusions from our baseline results do not change. However, the estimated average effects are generally

larger. Second, the dC&D and C&S estimators yield similar results. These results, when interpreted together with our baseline results, imply that the S&A estimations are generally more aligned with the TWFE and standard event-study estimations, and dC&D tend to estimate relatively smaller effects. For visual inspection, we also illustrate the 90 and 95 percent confidence intervals for each estimate in Figure D.1.

Table D.1: Effect of carbon pricing on per capita CO₂ emissions with a balanced panel

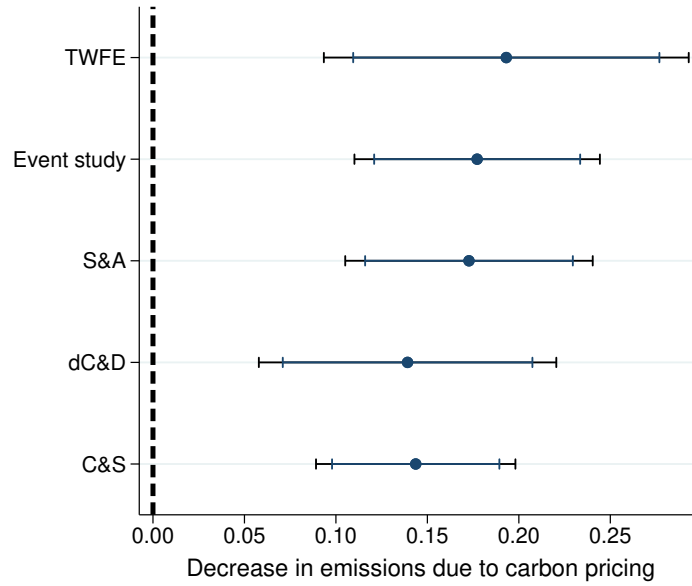
	(1)	(2)	(3)	(4)	(5)
Estimators	TWFE	Event Study	S&A	dC&D	C&S
Carbon pricing	-0.193 (0.051)	-0.177 (0.034)	-0.173 (0.035)	-0.139 (0.041)	-0.144 (0.028)
Placebo test (p-val)		0.167	0.589	0.239	0.387
Num. of obs.	3848	3848	3848	3848	3848
Control set	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>	<i>Core</i>

Notes: This table presents the estimated effects of carbon pricing on emissions per capita based on standard TWFE and event-study estimations, and estimators introduced by Sun and Abraham (2021) (S&A), de Chaisemartin and D’Haultfoeuille (2024) (dC&D), and Callaway and Sant’Anna (2021) (C&S), using a balanced panel. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The control set *Core* includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of the share of urban population. Standard errors clustered at the country level are in parentheses. dC&D and C&S standard errors are bootstrapped with 800 replications. Placebo tests are on the joint significance of estimated placebo treatment effects for the 10 years preceding the treatment.

The results of the pretrend tests in Table D.1 are in line with our baseline results. In Figure D.2, we present detailed results on the dynamic treatment effect estimations using the dC&D estimator. The results are similar, except that the estimated reduction in emissions per capita after 10 years from the enactment is 30 percent. The dynamic estimates from the other estimators, which we ignore for brevity, reflect the patterns presented in Table D.1. C&S yields very similar results to those from dC&D. The estimated dynamic treatment effects by S&A are slightly higher, implying almost a 40 percent reduction after 10 years. In all estimations, the placebo treatment effects are insignificant.

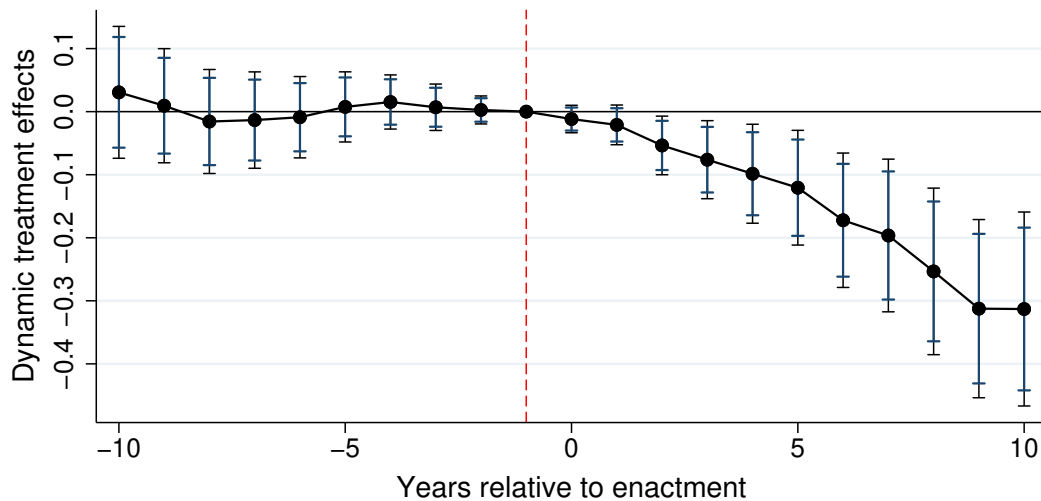
In the preceding analysis, we balanced our dataset at 1981. That is, we drop all countries with at least one missing value from 1981 onward for any variables in our baseline specification. In general, we have more country observations when we restrict the time span to a more recent period. We also performed robustness checks by balancing our sample at 1990 and obtained similar results.

Figure D.1: Decrease in per capita CO₂ emissions due to carbon pricing with a balanced sample



Notes: This figure presents the estimated effects of carbon pricing on emissions per capita based on standard TWFE and event-study estimations, and estimators introduced by Sun and Abraham (2021) (S&A), de Chaisemartin and D'Haultfœuille (2024) (dC&D), and Callaway and Sant'Anna (2021) (C&S), using a balanced sample. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors are clustered at the country level. dC&D and C&S standard errors are bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

Figure D.2: Dynamic treatment effects with a balanced sample

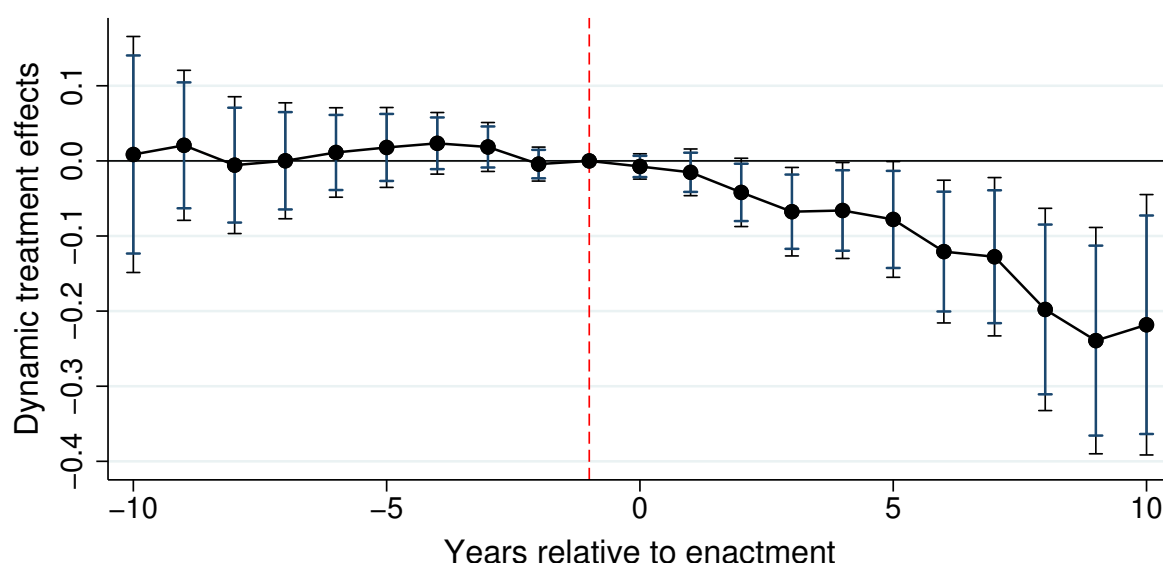


Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the de Chaisemartin and D'Haultfœuille (2024) estimator by using a balanced sample. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population and the logarithm of the share of urban population. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

E Only later-treated units in the control group

In this section, we provide a robustness check by restricting our control group to the later-treated units. This control group includes countries that have eventually implemented a carbon pricing policy at a point in time in our sample period. Therefore, the treated cohorts and the associated control group, which receives the treatment at a later date, are arguably more comparable in terms of the probability of being treated. Figure E.1 shows the results based on the dC&D estimator. The estimated dynamic treatment effects and the placebo treatment effects are close to those in our baseline estimations.³¹

Figure E.1: Dynamic treatment effects with only later-treated units in the control group



Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the estimator introduced by de Chaisemartin and D'Haultfoeuille (2024) by using only the later-treated units as the control group. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population and the logarithm of the share of urban population. Standard errors are clustered at the country level and bootstrapped with 800 replications. The capped bars show the 90 and 95 percent confidence intervals.

³¹The S&A estimator only allows for never-treated units in the control group. The C&S estimator yields similar results, except that the estimated effects are larger, reflecting the pattern in our baseline estimations.

F Extended control set

We gather data on factors commonly utilized in the literature on the determinants of CO₂ emissions and perform robustness checks. This expanded control set includes country characteristics in various dimensions: (i) characteristics related to the energy sector, (ii) relevant policy measures, other than carbon pricing, that can reduce emissions, and (iii) institutional factors.

The first subset of additional control variables includes the share of energy in GDP and the shares of coal, oil, and gas in total energy consumption. These are common control variables in the literature. Carbon pricing can also affect emissions through its effect on resource shares and energy intensity. Therefore, these variables are likely to be bad controls. However, the resulting bias in the estimated treatment effects due to their inclusion is likely to be towards zero, yielding a conservative estimate for the effect of carbon pricing. Following the common practice to alleviate this problem, we lag these variables by ten years. This strategy may alleviate potential bias.

One major concern in identifying causal effects of policies by using aggregate information is the presence of other accompanying policies targeting the same outcomes. As the most prominent other policy tools, we also include renewable energy policy variables for the electricity sector, such as feed-in-tariffs and renewable portfolio standards. Each variable is an indicator variable that describes whether a country has such a policy in place in a certain year.

We also control for political factors such as the level of democracy (Marshall et al., 2020) and corruption (Standaert, 2015). Democratic countries tend to have more stringent climate regulations, and corruption can lead to weaker climate regulations. Hence, these variables could have the power to predict the treatment probability.

Table F.1 presents the TWFE estimation results from using our extended control set. For convenience, the first column includes our baseline results based on our core control set. The results show that our baseline results are robust to using these additional covariates that are frequently employed in the relevant literature.

These additional variables are likely to qualify as bad controls, or they do not improve the explanatory power of our model while restricting the sample size considerably. We use other strategies to address the related concerns that these variables may alleviate by using our preferred estimation strategies. The results of these analyses are presented in Section 6 in the main body of the text.

Table F.1: TWFE estimations with the extended control set

	(1)	(2)	(3)	(4)	(5)
Carbon pricing	-0.138 (0.044)	-0.189 (0.045)	-0.151 (0.041)	-0.149 (0.039)	-0.170 (0.037)
GDP pc (log)	1.067 (0.453)	1.163 (0.457)	1.015 (0.434)	1.014 (0.434)	0.814 (0.416)
GDP pc (log) squared	-0.038 (0.024)	-0.046 (0.024)	-0.038 (0.023)	-0.038 (0.023)	-0.029 (0.022)
Population (log)	0.223 (0.136)	0.049 (0.133)	0.062 (0.126)	0.061 (0.127)	-0.006 (0.119)
Urban pop. sh. (log)	0.988 (0.233)	0.876 (0.231)	0.665 (0.230)	0.661 (0.232)	0.847 (0.217)
Energy intensity (lagged 10 y.)		0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Coal share (lagged 10 y.)			1.175 (0.408)	1.175 (0.405)	1.097 (0.381)
Oil share (lagged 10 y.)			0.752 (0.237)	0.756 (0.237)	0.715 (0.216)
Gas share (lagged 10 y.)			0.907 (0.273)	0.909 (0.275)	0.951 (0.269)
Feed-in-tariffs				0.008 (0.034)	0.005 (0.031)
Renewable portfolio st.				-0.017 (0.043)	-0.029 (0.043)
Democracy					-0.006 (0.007)
Corruption					-0.008 (0.007)
Adjusted R^2	0.44	0.46	0.49	0.48	0.49
Observations	4592	4350	4346	4346	3820

Notes: This table presents the estimated effects of carbon pricing on emissions per-capita based on the standard TWFE regressions. Carbon pricing is a dummy variable indicating whether a country has implemented a carbon pricing policy or not. In the first panel, the regressions use all available data points. Panel 2 uses the restricted sample which refers to the sample when we use the full control set in the last column in Panel 1. Standard errors clustered at the country level are in parentheses.

G Controlling for energy prices

In our baseline analysis, we do not control for the potential effects of general energy prices and relative prices of different energy resources on CO₂ emissions, as including energy prices substantially reduces our sample size. Hence, we rely on the assumption that the variation in energy prices induced by carbon pricing policies is an exogenous source of variation, which is a plausible assumption often used in the literature (see, for example, Davis and Kilian, 2011).

Table G.1: Controlling for energy prices

	(1)	(2)	(3)	(4)	(5)
	Controlling for the price(s) of				
	Energy	Major Resources	Major Resources + Coal	All res.	All. res. + Coal
Carbon pricing	-0.079 (0.032)	-0.123 (0.037)	-0.107 (0.062)	-0.091 (0.046)	-0.092 (0.061)
Placebo test (p-val)	0.041	0.136	0.011	0.211	0.434
Num. of obs.	1147	956	548	923	546

This table presents the estimated effects of carbon pricing on emissions per-capita based on the de Chaisemartin and D’Haultfœuille (2024) estimator by controlling for a general price index and price indices for various energy resources. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors clustered at the country level and bootstrapped with 800 replications are in parenthesis. Placebo tests are on the joint significance of estimated placebo treatment effects for the 10 years preceding the treatment.

In this section, we present a robustness analysis in which we augment our baseline specification by controlling for a general energy price index and price indices for different energy resources. All additional data on energy prices come from the IEA database (IEA, 2022). The results are presented in Table G.1. In the first column, we control for the energy consumer price index normalized by the general consumer price index. In the second column, we additionally control for the prices of major energy resources. These resources are oil products, natural gas, and electricity. We introduce coal prices in the next column, as it reduces our sample size even further. In Columns (4) and (5), we augment the specifications underlying the results in Columns (2) and (3) with the price indices of diesel, gasoline (excluding premium grade) and high-sulfur fuel oil for industry. All individual price indices for these energy resources are normalized with the general energy price index.

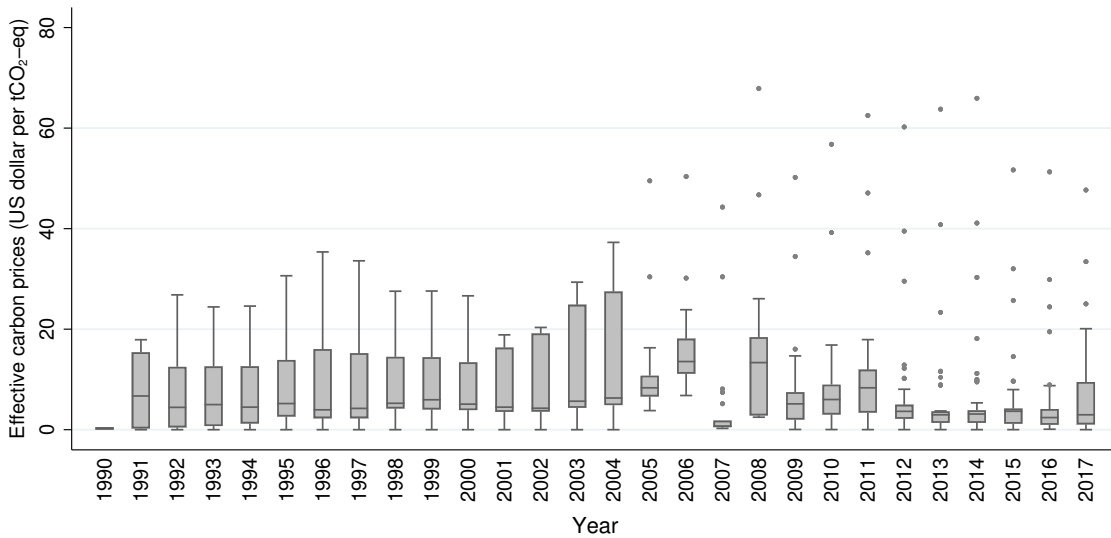
The results show that, despite the reduction in our sample size by more than 75 percent, our baseline results are robust to including the energy and resource prices as additional covariates in our specification.

H More on the effective carbon prices

In this section, we start by describing the construction of our data on effective carbon prices and provide some descriptive statistics. Next, we present the results from a robustness check on the estimation of short-run responsiveness to carbon prices.

Effective carbon prices. Effective carbon price is the weighted average of carbon prices across all initiatives in a country. The weights are the coverage ratios which are the share of emissions covered by an initiative in total emissions of a country. To calculate effective prices, we compile a dataset from three sources: Carbon Pricing Dashboard (CPD) by the World Bank (World Bank, 2023a), the EDGAR Database on GHG emissions (Crippa et al., 2024; EDGAR, 2024), and emission trading data on EU ETS from the European Environment Agency (EEA, 2024). CPD provides data on the share of emissions covered by each initiative in global GHG emissions (see Section 2). The CPD data are based on the EDGAR database on GHG emissions, which we use to convert the global coverage ratios to country-level coverage ratios. As the CPD dataset is at the initiative level, it does not provide a detailed account of the coverage of EU ETS across the participating countries. Therefore, we use verified emissions of each member state provided by the EEA to calculate the country-level coverage ratios. The EEA offers a dataset compiled from emissions trading data from the European Union Transaction Log (EUTL) managed by the European Commission. EUTL monitors and records all transactions within the trading scheme. Verified emissions are based on the number of emission allowances surrendered by all participating firms.

Figure H.1: Effective carbon prices



Notes: This figure illustrates the distribution of effective carbon prices over years excluding zero values. The boxes indicate the median, and upper and lower 25th percentiles. The capped bars show the adjacent values detecting outliers depicted with dots.

Figure H.1 shows the distribution of nominal effective carbon prices over the years. The median values are rarely above 10 US dollars per ton of CO₂-equivalent and observed prices are rarely above 20 dollars.

Robustness checks on the responsiveness to carbon pricing In Section 6.1, we show that the estimated elasticities of emissions per capita with respect to effective carbon prices are close to zero. We proceed by presenting a robustness check on this result. In all these estimations, we deflate the nominal prices depicted in Figure H.1 with a consumer price index retrieved from the PENN World Table (Feenstra et al., 2015).

Table H.1: Responsiveness to carbon prices: semi-elasticities

	(1)	(2)	(3)	(4)	(5)
	Sample restrictions on carbon prices				
	Full sample	Positives	> 25 th percentile	> 50 th percentile	> 75 th percentile
Effective carbon prices	-0.004 (0.002)	-0.001 (0.001)	-0.002 (0.001)	-0.002 (0.001)	-0.003 (0.002)
Implied elasticity	-0.050 (0.019)	-0.016 (0.013)	-0.024 (0.011)	-0.029 (0.013)	-0.036 (0.019)
Adjusted R^2	0.44	0.66	0.72	0.66	0.61
Observations	4592	497	372	248	125

This table presents the estimated semi-elasticities of emissions per-capita with respect to carbon prices. All regressions control for country and year fixed effects, and the core control set. Standard errors clustered at the country level are in parentheses. The implied elasticity is calculated at the average of all positive effective carbon prices.

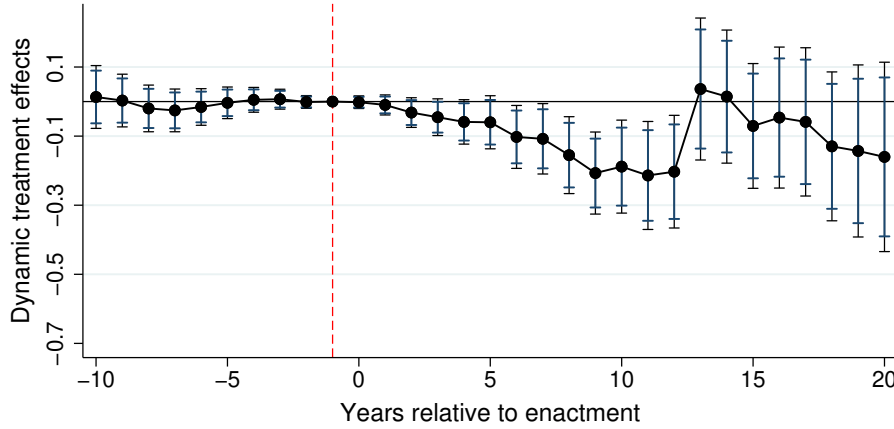
In our sample period, carbon prices are effectively zero in the pretreatment periods. Therefore, we start by estimating semi-elasticities by using a log-linear specification which retains the zeros. Then, we recover elasticities at the mean of effective prices excluding zeros. The results are presented in Table H.1. In the first column, we use our full sample. The coefficient estimate is significant at 5 percent significance level and suggests that a one Euro increase in effective carbon prices is associated with a 0.4 percent decrease in per capita emissions. In line with our results presented in the main text, the implied elasticity, calculated at the mean value of all positive effective carbon prices, is small. Given that the average effective carbon price in the treatment period is 10 US dollars per tonne of CO₂, the estimated coefficient imply that a 4 percent decrease in emissions would require doubling the effective prices. In this estimation, we exploit annual changes in carbon prices, including jumps from zero to a positive value. Therefore, the results might also be partially driven by the expectations mechanism. As a result, we consider this estimate as an upper bound for the carbon price elasticity. In Column (2), we exclude the zeros and exploit only the changes in positive carbon price levels. The estimated association is smaller and insignificant. In columns (3) to (5), we drop the observations with a carbon price lower than the 25th, 50th and 75th percentile values of carbon prices, respectively.

The estimated effects are stronger at higher effective prices, and they are statistically significant at conventional levels. However, the implied elasticities are even smaller than our baseline estimates.

I Beyond 10 years: details and further analyses

Figure I.1 shows the estimated dynamic treatment effects that lead to the average treatment effects in the main body of the text. The results reveal two patterns that support our conclusion that emission reductions were realized in the first 10 years after the enactment of carbon pricing policies. First, the estimated effects between the relative years 9 and 12 are very stable. Second, on average, the estimation results after the relative year 12 do not show any indication of further emission reductions. In the following, we discuss these two patterns in more detail. Next, we will provide further evidence to support our conclusions through some diagnostic tests and by extending our dataset up to 2021.

Figure I.1: Dynamic treatment effects beyond 10 years

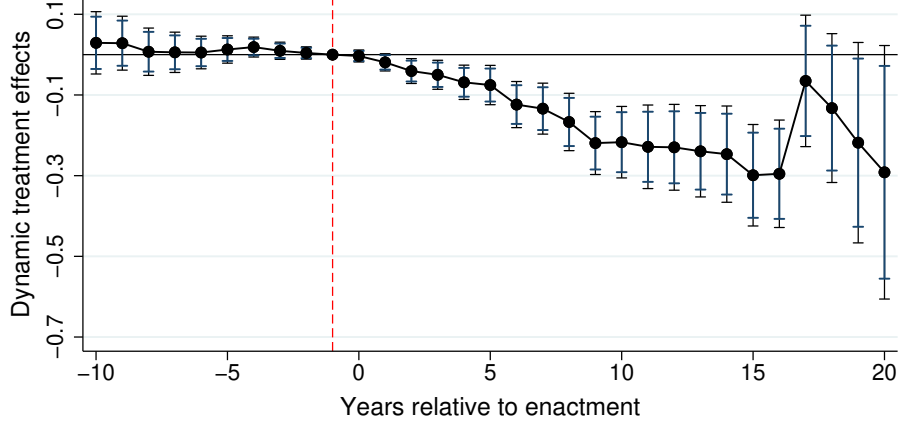


Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the estimator introduced by de Chaisemartin and D’Haultfœuille (2024), by extending the event window to 20. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population and the logarithm of the share of urban population. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

We start by discussing the second pattern described above. The stabilization in the average treatment effects, presented in Figure 7 in the main text, is mainly driven by the estimated dynamic treatment effects after the relative year 12. Hence, we put some caution in interpreting these results. Figure I.1 clarifies the reason for this caution. In all estimations, there is a sharp break in the pattern of estimated effects after the relative year 12. These estimates lack robustness and exhibit a higher degree of imprecision compared to those for earlier relative years. This pattern is not a coincidence. As depicted in Figure 3, until the introduction of the EU ETS in 2005, we have eight treated units. As our baseline sample ends at 2017, the estimation of dynamic treatment effects after the relative year 12 are based on these eight countries, which introduced a carbon pricing scheme before 2005. Therefore, rather than inspecting the estimated dynamic treatment effects, inspecting their averages can be more informative, which is our strategy in the

main text. As a result, on average, we did not find any evidence for further emission reduction after the relative year 12.

Figure I.2: Dynamic treatment effects beyond 10 years with extended sample (1981-2021)



Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the estimator introduced by de Chaisemartin and D’Haultfoeuille (2024), by extending the event window to 20 and using the extended sample (1981-2021). Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population and the logarithm of the share of urban population. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

The first pattern observed in Figure I.1 is a more convincing piece of evidence towards our conclusion. The estimated treatment effects between relative years 9 and 12 are very stable compared to the preceding periods. Note that the estimation of these parameters does not suffer from the subtleties in the following periods. In order to verify this interpretation, we repeat our analysis by extending our sample period up to 2021. In this analysis, we use the GDP series from the World Development Indicators (World Bank, 2023b), as our preferred GDP series from the Penn World Table is only available up to 2019. As illustrated in Section 2, there was an unprecedented surge in the global effective carbon prices starting in 2018. Therefore, one may expect a momentum in emission reductions in this period, which would work against our conclusion that the effect of policies stabilized after 10 years.

The results from our extended sample are presented in Figure I.2, which verifies our previous conclusions. First, the break in the estimated dynamic treatment effects is now at relative year 17 instead of 13 as a result of using four more years of observations. This finding clarifies the factor driving the break. Second, the estimated effects are still stable for eight years starting from period 9 up to the period where the break takes place.

To formally confirm this observation, we test the equality of the dynamic treatment effects shown in Figure I.2 over a rolling period of ten years starting from period zero. Table I.1 shows the test results. The first row tests whether all the dynamic treatment

Table I.1: Testing the equality of dynamic treatment effects

Starting period	End period	Chi-square	p-values
0	9	95.113	0.000
1	10	35.581	0.000
2	11	10.499	0.001
3	12	5.341	0.021
4	13	4.221	0.040
5	14	3.624	0.057
6	15	1.752	0.186
7	16	1.632	0.201
8	17	0.625	0.429
9	18	0.021	0.885
10	19	0.932	0.334
11	20	0.692	0.406

This table presents the results of the tests of the equality of the dynamic treatment effects over a rolling period of ten years. The Chi-square test statistics and the corresponding p -values are reported.

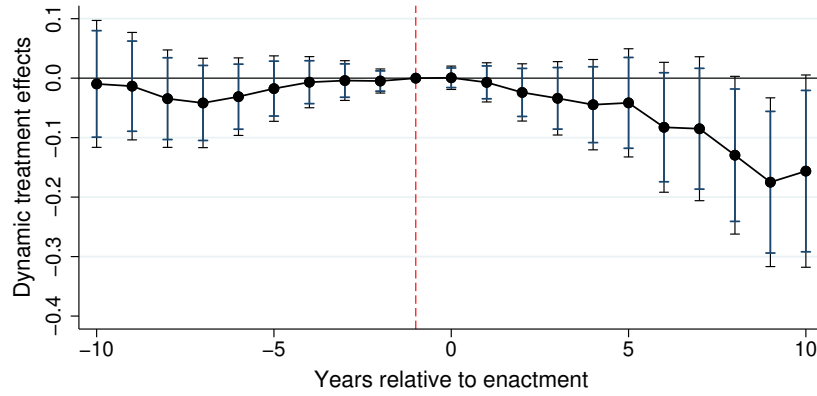
effects between relative years 0 and 9 are equal, which is rejected with a high Chi-square statistic. The test statistic gradually becomes smaller in the subsequent rows as the tests cover more periods after relative year 9. We cannot reject the null of equality at conventional significance levels starting from the seventh row. These results support our earlier conclusions based on visual inspection of the estimated dynamic treatment effects. Despite the sharp increase in carbon prices after 2018, we do not find any evidence for further emission reductions after 10 years of the enactment of carbon pricing policies.

All our results presented in this appendix and in Section 6.2 are robust to using the S&A and C&S estimators which we do not present for brevity.

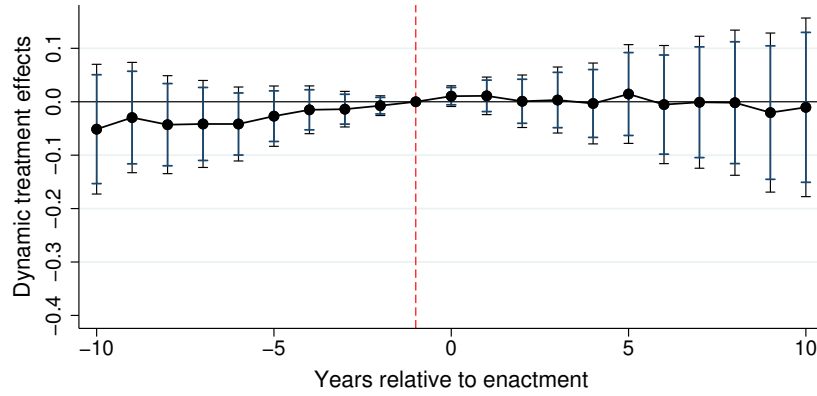
J More on the decomposition analysis

We present the results of our dynamic treatment effect estimations for the components of IPAT and KAYA identities in Figure J.1. In all estimations, the pre-event placebo treatment effects are insignificant. In Panels (a) and (c), the estimated dynamic treatment effects have similar patterns to our baseline results. The estimated reductions in the emission intensity of GDP and energy use are around 15 percent. The dynamic treatment effects for the energy intensity of GDP are all very close to zero.

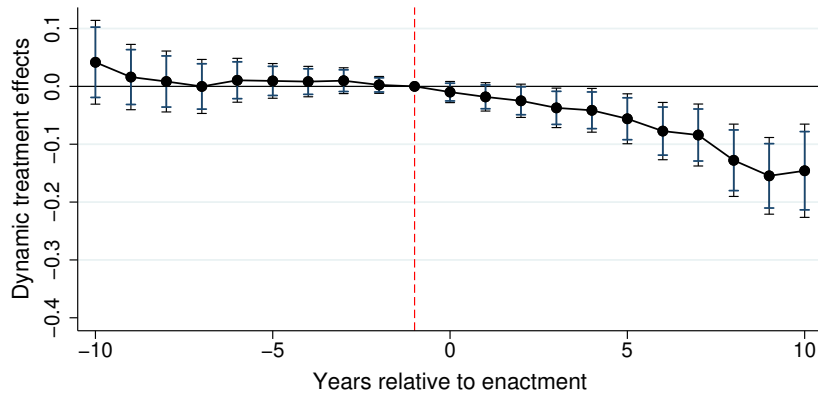
Figure J.1: Composition of treatment effects: dynamic estimations



(a) CO₂ intensity of GDP



(b) Energy intensity of GDP



(c) CO₂ intensity of energy use

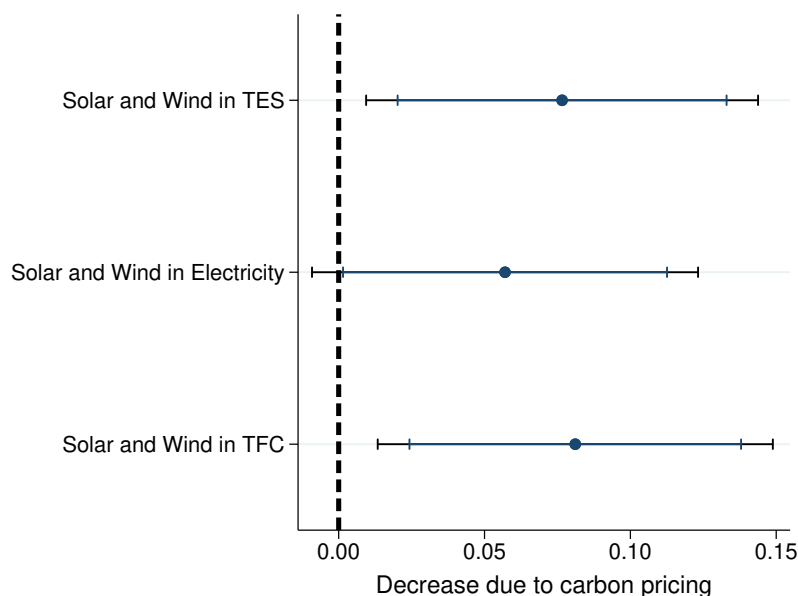
Notes: This figure presents the estimated dynamic effects of carbon pricing on the components of the IPAT and KAYA identities based on the de Chaisemartin and D'Haultfœuille (2024) estimator. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, the logarithm of the share of urban population, and the other components of the IPAT or KAYA identities other than the dependent variable. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

K More on the role of renewable energy policies

In this section, we present some further details on the estimation results presented in Section 6.4, where we presented the results of following several strategies to account for the potential role of renewable energy policies.

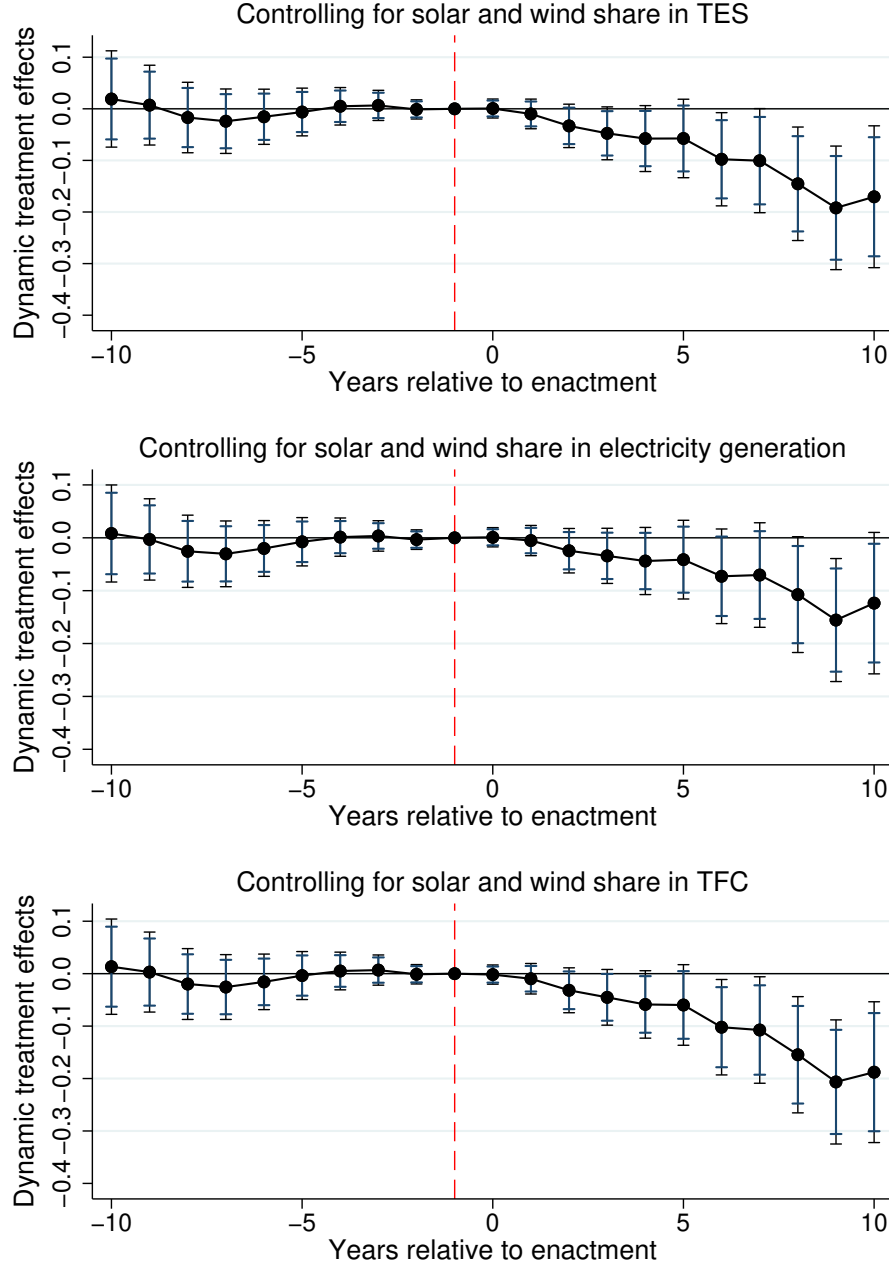
Figure K.1 visualizes the results presented in the first three columns of Table 4, where we repeat our estimations by additionally including the solar and wind energy shares in TES, electricity generation, and TFC, respectively. It is clear that, although the estimated effect is slightly smaller when we control for the share in electricity generation, all the estimated effects in these three regressions are not statistically different. Figure K.2 shows the estimated dynamic treatment effects. The estimated patterns are similar and close to our baseline regressions. However, the estimated effect after 10 years when we control for the share in electricity generation is considerably smaller compared to our baseline estimate. Here, the estimated effect implies an 11 percent reduction after 10 years, while our baseline results suggest a 19 percent reduction. Finally, the estimated dynamic treatment effects by controlling for renewable energy policies are depicted in K.3. The estimated patterns and the size of the dynamic treatment effects are again very close to our baseline estimates.

Figure K.1: Average treatment effects by controlling for renewable energy shares



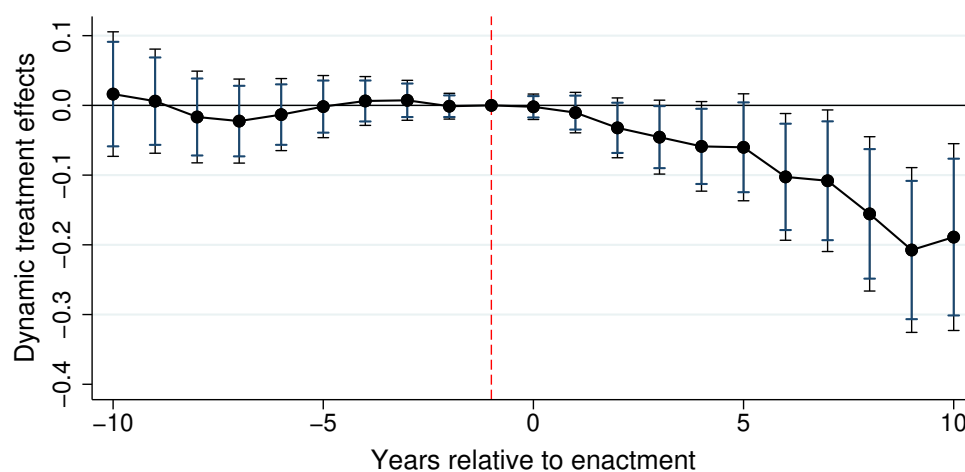
Notes: This figure presents the estimated effects of carbon pricing on emissions per capita based on the de Chaisemartin and D'Haultfœuille (2024) estimator by controlling for the share of renewable energy in TES, electricity generation, and TFC. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

Figure K.2: Dynamic treatment effects by controlling for renewable energy shares



Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the de Chaisemartin and D'Haultfœuille (2024) estimator by controlling for the share of renewable energy in TES, electricity generation, and TFC. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.

Figure K.3: Dynamic treatment effects by controlling for renewable energy policies



Notes: This figure presents the estimated dynamic effects of carbon pricing on emissions per capita based on the de Chaisemartin and D'Haultfœuille (2024) estimator by controlling for the presence of renewable energy policies, namely feed-in-tariffs and renewable portfolio standards. Carbon pricing is a dummy variable that indicates whether a country has implemented a carbon pricing policy or not. The estimations use the *Core* control set which includes the logarithm of GDP per capita and its square, the logarithm of population, and the logarithm of urban population share. Standard errors are clustered at the country level and bootstrapped with 800 replications. Capped bars show the 90 and 95 percent confidence intervals.