Grantham Research Institute on Climate Change and the Environment

The effect of the EU Emission Trading Scheme on emissions

Anton Bogachev and Frank Venmans

July 2026

Grantham Research Institute on
Climate Change and the Environment
Working Paper No. 455

ISSN 2515-5717 (Online)

Grantham Foundation
for the Protection of the Environment

LSE Global School of Sustainability

The Grantham Research Institute on Climate Change and the Environment was established in 2008 at the London School of Economics and Political Science. The Institute brings together international expertise on economics, as well as finance, geography, the environment, international development and political economy to establish a world-leading centre for policy-relevant research, teaching and training in climate change and the environment. It is hosted by the Global School of Sustainability at LSE and is funded by the Grantham Foundation for the Protection of the Environment.

www.lse.ac.uk/granthaminstitute

Authors' declaration of AI use: The authors used Claude Sonnet to write code to convert XML files to CSV files and to check the language of the final text. The authors revised the output and are fully responsible for the content of the publication.

This paper was first published in July 2026 by the Grantham Research Institute on Climate Change and the Environment at the London School of Economics and Political Science.

Suggested citation:

Bogachev A and Venmans F (2026) *The effect of the EU Emission Trading Scheme on emissions*. Grantham Research Institute on Climate Change and the Environment Working Paper 455. London: London School of Economics and Political Science

DOI: 10.21953/researchonline.lse.ac.uk.00138269

The Effect of the EU Emission Trading Scheme on Emissions *

Anton Bogachev, Frank Venmans

16 July 2026

Abstract

We assess the effect of the EU Emission Trading Scheme (ETS) on emission reductions between 2008 and 2023. To do so, we compare emissions of ETS installations with non-ETS installations of similar size in the same sector and country. We use data in the UK, the Netherlands, France and Norway, because these countries require companies to report CO_2 emissions at low levels, which are below the inclusion criteria of the ETS. We find that the ETS has reduced industrial emissions by approximately 12% in the second phase (2008-2012), by 18% in the third phase (2013-2020) and by 41% in the fourth phase (2021-2023). To obtain strong internal validity, our sample focusses on small companies. However, we find that the larger installations in our sample have made slightly larger emission reductions than the smaller companies.

JEL codes: Q58, Q54, H23

Keywords: EU Emissions Trading Scheme, carbon pricing, cap and trade, difference-in-differences, climate policy

1 Introduction

As the cornerstone of the European Union's climate policy, the EU Emission Trading System (EU ETS) plays a central role in the region's effort to achieve net-zero greenhouse gas emissions by 2050. Launched in 2005, the EU ETS is the world's first carbon market, covering over 10,000 installations across power generation, industry, and —since 2012— aviation within the European Economic Area. The system operates on a "cap-and-trade" principle: a cap is set on the total volume of emissions allowed from covered installations, and companies must surrender one allowance for each ton of CO_2 they emit. These allowances can be traded, enabling emissions reductions to occur where they are most cost-effective, while providing clear price signals to guide investment decisions. Participation in the scheme is mandatory. The ETS forms a key pillar of the EU's broader climate strategy, complementing regulatory and fiscal instruments such as renewable energy mandates and energy efficiency standards.

Figure 1 shows that the EU ETS emissions have declined by 50% between 2005 and 2024.[1] Yearly emissions are now one billion tonnes of CO_2 lower than at the start of the ETS. By contrast, the non-ETS sectors, which represented more than half of the EU emissions in 2005, have reduced their yearly emissions by merely 17%, that is, by only 0.3 billion tonnes. Before the start of the ETS in 2005, both ETS and

*Email for correspondence: F.Venmans1@lse.ac.uk. We would like to thank participants at the LSE Workshop for useful comments. We gratefully acknowledge funding from Carbon Cap Management.

[1]We have included Bulgaria and Romania and Croatia, who join in 2007, 2007 and 2013 respectively and the sectors who join the ETS in 2012.

non-ETS emissions were approximately stable. This suggests that the EU ETS not only has been successful at reducing emissions, but that it has been more successful than the other policies affecting residential, transport and agricultural emissions. Appendix E, shows that all ETS sectors have reduced their emissions substantially, with electricity the largest contributor while in the non-ETS sectors transport and agricultural emissions have merely declined. Yet, comparing ETS sectors with the non-ETS sectors does not give a causal estimate, because ETS sectors may have evolved differently, even without policy. For example, increased competition from China, plant closures during the global financial crisis, or technological shifts, may have reduced industrial ETS sectors' emissions regardless of policy and may explain why non-ETS emission reductions are lower.

Therefore it makes sense to compare emissions from industrial and power sectors with the same sectors in other developed countries, which have been subject to similar shocks. Figure 2 compares ETS-sector emissions in the EU with those of the same sectors in the US, Japan, New Zealand, and Australia. Emissions covered by the EU ETS have declined faster than in comparable sectors elsewhere, which is consistent with the view that the scheme was effective at reducing emissions. However, other factors also shaped emission trajectories in these countries — such as the shale gas revolution in the US, the nuclear phase-out in Japan, or carbon trading in New Zealand, California, and the northeastern US (RGGI) — making it difficult to isolate the effect of the EU ETS through cross-country comparisons.

To address this, the paper focuses on a more credibly causal estimate by comparing ETS installations with industrial non-ETS installations within the same country and sector. More specifically, we use a matched diff-in-diff methodology, comparing trends of ETS installations with trends of other installations in the same country (and sector) and with similar initial emissions.

2 The ETS policy design and literature

The evolution of the EU ETS can be broadly divided into four phases. Phase I (2005–2007) was a pilot phase intended to establish the basic infrastructure of the market. Emissions caps were set without verified emissions data, leading to substantial over-allocation and a collapse in allowance prices. Phase II (2008–2012) coincided with the first commitment period of the Kyoto Protocol and introduced tighter caps and partial harmonization, though free allocation remained the dominant method of allowance distribution. Despite improved data and governance, economic recession contributed to another surplus in allowances.

Phase III (2013–2020) marked a major overhaul: the cap declined linearly at an annual rate of 1.73% of 2010 emissions, and the allocation of allowances became more harmonized across Member States. Auctioning replaced free allocation as the primary method for sectors not deemed at risk of carbon leakage, while benchmarks and updated allocation rules applied to sectors still receiving free allowances. Furthermore, the Market Stability Reserve (MSR) was introduced to address the structural surplus of allowances.

Phase IV (2021–2030) builds on these reforms, with a steeper cap reduction rate (2.2% to 4.4% reduction per year), revised MSR rules, and a more targeted approach to free allocation. The share of auctioned allowances continues to grow, and there is a strengthened conditionality linking free allocation to decarbonization efforts. The Fit-for-55 package adopted in 2021 aims to align the EU ETS with the 2030 target of reducing net greenhouse gas emissions by at least 55% relative to 1990 levels, acknowledging that the ETS sector has abated much faster than the non-ETS sector. As a result, the ETS cap for 2030 is set at a 62% reduction below 2005 emissions, while the target for the non-ETS sector is a more modest 40% reduction over the same period.

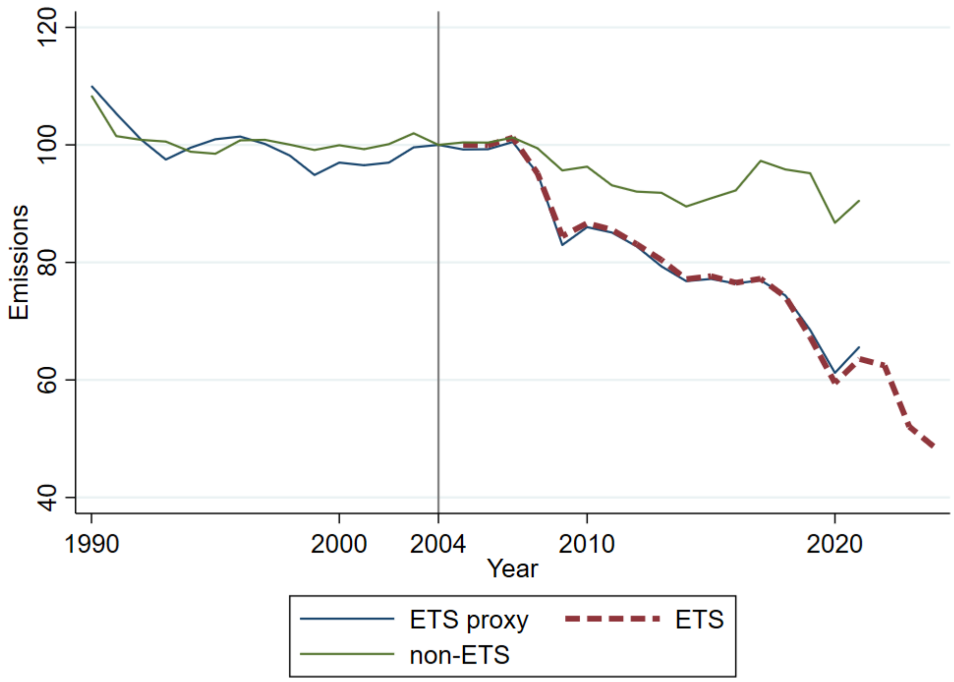


Figure 1: Evolution of emissions in 74 ETS and 266 non-ETS sectors, relative to 2004. Both sectors are proxied by a selection of UNFCCC sectors. The dotted line are the verified ETS emissions, showing that the approximation is very good. 43 UNFCCC sectors with both ETS and nonETS companies and less than 3% of aggregate emissions are excluded.

What would have happened to the ETS companies if they had not been regulated by the EU ETS in 2005? As the EU ETS has now been in place for 20 years, there is not an obvious answer to this question. The counterfactual emission scenario, which is at the heart of causal inference, becomes harder to estimate, the longer the policy is in place. Following the spirit of Figure 1, Bayer and Aklin [2020] build a counterfactual using a combination of non-industrial sectors which resembled the emission trajectory of ETS sectors before 2005. Their Generalized Synthetic Control method replicates each ETS sector with a linear combination of non-ETS sectors conditional on GDP growth. This replication is the 'synthetic ETS' and is designed to have the best fit before 2005. Next, the discrepancy between ETS and synthetic ETS after 2005 is the causal effect of the ETS. They find an average effect of 10% over the period 2005-2016, with a reduction around 20 to 25% by 2016.

The disadvantage of this approach is that it is sensitive to shocks that are specific to industrial sectors. As they use UNFCCC sectors, all industrial sectors are identified as ETS sectors so that the non-ETS sectors are quite different. For example, value added of the industrial sector as a proportion of GDP decreases over time even in the absence of EU ETS. Some shocks such as the Russian-Ukrainian war hit industrial sectors in a different way than other non-ETS sectors.

Alternatively, following the logic of Figure 2, one could argue that the European industry would have evolved similar to the industrial emissions in other industrial countries. Indeed, Biancalani et al. [2024] build a baseline using seven non-European countries (US, Canada, New Zealand, Israel, Japan, Australia, South Korea). The pre-2005 emissions of EU countries are replicated by a linear combination of the industrial emissions of other countries using a Matrix Completion Fixed Effect algorithm, which is a generalization

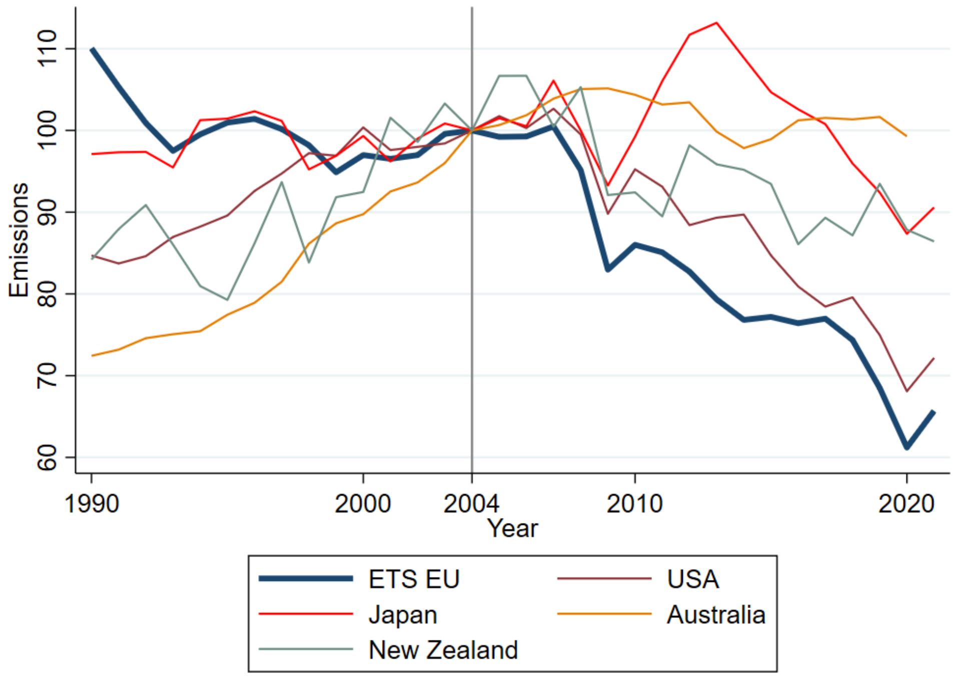


Figure 2: Evolution of emissions in 43 ETS sectors in different countries, using UNFCCC reported data.

of synthetic control. They find that the ETS reduced emissions by 15.4% on average, with roughly 35% reduction by 2020 (in other words, the median counterfactual has relatively constant emissions since 2005).

The disadvantage of this method is that these non-EU countries may have been differently exposed to shocks such as the 2010-2012 European sovereign debt crisis, international competition and the Russia-Ukraine war. Moreover, other countries also have climate policies in place, so we are measuring the difference in effectiveness between the EU ETS and other policies. The assumption is that in the absence of the EU ETS, the European industry would not have been in a vacuum of regulation, but would rather have had other policies in place.

Other studies show that the ETS matters for emissions, by comparing emission reductions in firms with little free allocation compared with firms with generous free allocation. Although the Coase theorem predicts that the incentive to abate is independent of free allocation, in practice it matters [Venmans, 2016]. For example, Jonghe et al. [2020] show that in the period 2017-2019, after a sudden price increase in the EU ETS, the firms which receive a low proportion of free allocations (less than the median) reduce their emissions by 7% more than the firms which have more generous free allocation. Bordignon and degl'Innocenti [2023] find similar results for emissions before and after 2012, when the electricity sector and a few other sectors stop receiving free allocation. Eslahi et al. [2024] build a counterfactual emission scenario using predictors of electricity emissions before 2005 (electricity demand, temperature, fuel prices, solar & wind generation, overlapping policy index...) and find a reduction of 19% in electricity emissions during the the third phase.

Yet the most common approach to estimate the emission reductions of the ETS is to build a counterfactual using installation level data of non-ETS plants. This is typically done with a difference-in-difference estimator, often combined with matching, so that the counterfactual is built using individual installation data in the same country and sectors, of similar size.

There are several studies investigating emission reductions in a single country. Colmer et al. [2025], find emissions reductions around 8-12% in France until 2012. Barrows et al. [2024] extend the French analysis up to 2015 and find emission reductions between 3 to 16% depending on the year. Klemetsen et al. [2020] find a reduction of 30% in the second phase in Norway on a relatively small sample. Next, Jaraite and Maria [2016] investigate the effects in Lithuania for the period 2005-2010 and find no reductions in emissions and a slight improvement in emissions intensity in 2006–2007. Calel [2020] finds no effect on emissions in the UK over 2005–2012, but documents a 20–30% increase in R&D spending and 100–150 additional low-carbon patents. Next, Petrick and Wagner [2014] find a reduction of 20% in the period 2007-2010 in Germany. Finally, Dechezleprêtre et al. [2023] investigates emission reductions until 2012 for France, the UK, the Netherlands and Norway and find emission reductions of roughly 10%.

In sum, the studies using firm-level data with matched difference-in-difference, typically find that the emission reductions were very modest in phase 1 and more substantial, in the neighbourhood of 10% on average in Phase 2. This paper uses the matched difference-in-difference method for France, the UK, the Netherlands and Norway, as in Dechezleprêtre et al. [2023], but updates it up to 2023.

3 Methods

Our identification strategy exploits the inclusion criterion as a quasi-random assignment to the EU ETS in 2005. Inclusion in the EU ETS is based on installation size. For example, all combustion installations with a rated thermal input exceeding 20 MW are covered, while smaller ones are not. Other inclusion criteria are 500 tonnes/day for cement rotary kilns, 20 tonnes per day for glass smelters and 2.5 tonnes per hour for steel plants. In 2005, Member states had the possibility to opt out (or opt in) certain installations to avoid double regulation, but these exceptions ended in 2007. [2] Inclusion criteria are based on production capacity and not on emissions, so that the inclusion criterion is orthogonal on emissions efficiency. As the size of an installation is hard to change, companies could not self-select into the ETS. Also, inclusion criteria are defined at the installation level. Therefore, a company with two small plants may not be included, while another company of similar size, but with a single installation may exceed the inclusion threshold. This is an attractive feature from an identification standpoint, because it provides data for installations managed by comparably-sized companies in the same sector.

3.1 Matching

We use the emissions data from the Pollutant Release and Transfer Register (PRTR) to have emissions from both ETS and non-ETS installations. We apply nearest neighbour propensity score matching on emissions, combined with exact matching on the country. In the sensitivity analysis, we also impose exact matching on sectors (NACE 2).

We apply a one-to-three matching procedure, selecting for each ETS company up to three similar non-ETS companies. Matching is done with replacement, meaning that a non-ETS company can be used as a match more than once. In our sensitivity analysis we also use 'full matching' meaning that the algorithm matches for each ETS company possibly several non-ETS companies, while also checking that for each non-ETS company there are potentially several matched ETS companies. (See R-package "matchit" Ho et al.

[2] In 2013, article 27 of the revised ETS directive allowed Member States to opt out small emitters to avoid large administrative costs. The UK opted out hospitals and some small emitters (below 35MW heating capacity or ¡25ktCO2/year), which we left out of our sample.

[2011]).

We exclude matches which have large differences in emissions. We apply a caliper of 0.2, excluding non-ETS firms with a propensity score that differs by more than 0.2 standard deviations from the ETS matched firm. This implies that we define a suitable match more strictly in countries with more observations because in these countries the propensity scores have lower standard deviations.

If there are no installations with sufficiently similar emissions, the company is excluded from the analysis. This means that our sample is tilted towards the smaller companies around the ETS inclusion criterion. As a result, we do not obtain the average treatment effect of the treated (ATT), but rather a size-weighted local treatment effect. We also report different treatment effects for smaller and larger companies to analyse the effect of size on emission reductions.

We use 2003-2007 as the reference period. This includes the first phase of the EU ETS, a.k.a. the trial phase. The effect of this trial period on emissions was modest, because the scheme was new, companies were over-allocated and allowances could not be banked into the next phase, leading to a collapse of the price after the data on 2005 emissions became available. This means that our results show emission reductions on top of the modest emission reductions of phase one. We show graphical results using pre-ETS years 2003 and 2004 as the reference period for a small subsample of installations with available data. Unfortunately, there are few observations in the PRTR before 2005 and even thereafter, data is often missing. To check for a systematic effect of missing data, we also apply a 'pairwise' missing test in the sensitivity analysis, where for each missing observation, the matched companies are also set to missing.

We exclude companies that enter the ETS in 2013, because there are only 46 companies with data in the PRTR (there are more companies in the EU ETS dataset, but with emissions below the PRTR inclusion criterion).

In all the above choices we face a trade-off between efficiency (larger sample size leads to lower standard errors) and avoiding bias (matched companies should be as similar as possible). A one-to-one matching on size, sector and country, without replacement, with reference period before 2005 would give us the lowest potential bias, but the sample size would be so small that we lack the statistical power to detect significant results. At the other extreme, the sensitivity analysis includes an unmatched regression on the full sample, which has low standard errors, but is likely to be confounded by fundamental differences between the two groups — a setting far removed from a like-for-like comparison.

3.2 Difference-in-difference

Once we have our matched sample, we construct a dummy variable $ETS_i \times Post_t$ which is one for ETS companies after 2008 and run the following Poisson regressions :

$$\text{Emissions}_{it} = \exp\Big(\underbrace{\alpha_i}_{\substack{\text{installation}\\ \text{fixed effect}}} + \underbrace{\tau_t}_{\substack{\text{year}\\ \text{fixed effect}}} + \beta \left(\text{ETS}_i \times \text{Post}_t\right)\Big) + \varepsilon_{it}, \tag{1}$$

$$\text{Emissions}_{it} = \exp\Big(\underbrace{\alpha_i}_{\substack{\text{installation}\\ \text{fixed effect}}} + \underbrace{\gamma_{s,t}}_{\substack{\text{sector–year}\\ \text{fixed effect}}} + \beta \left(\text{ETS}_i \times \text{Post}_t\right)\Big) + \varepsilon_{it}, \tag{2}$$

$$\text{Emissions}_{it} = \exp\Big(\underbrace{\alpha_i}_{\substack{\text{installation}\\ \text{fixed effect}}} + \underbrace{\delta_{c,t}}_{\substack{\text{country–year}\\ \text{fixed effect}}} + \beta \left(\text{ETS}_i \times \text{Post}_t\right)\Big) + \varepsilon_{it}. \tag{3}$$

In Equation (1), α_i absorbs all time-invariant heterogeneity across installations and τ_t captures common annual shocks — such as world market price shocks for fossil fuels. The coefficient β on the interaction $\text{ETS}_i \times \text{Post}_{it}$ is our difference-in-differences estimator, and ε_{it} is the error term. Equation 2 adds sector-year fixed effects $\gamma_{s,t}$, absorbing common sectoral shocks. For example, some sectors might be hit harder by the financial crisis than others. Equation (3) adds country–year fixed effects $\delta_{c,t}$, which flexibly absorb country-specific shocks to emissions that affect ETS and non-ETS firms equally. For example, we allow for gas price shocks to be different among countries. [3]

In our main analysis we apply a Poisson Pseudo Maximum Likelihood (PPML) regression using the emissions rather than log of emissions as dependent variable. The Poisson-regression gives lower weight to smaller companies such that β gives the aggregate emission reduction (in percent) of the sample (see Dechezleprêtre et al. [2023], Cohn et al. [2022]). In the sensitivity analysis, we also apply a standard fixed-effect panel estimator, which treats a given % reduction of a small and large company equally. We show robust standard errors, adjusted for heteroskedasticity.

3.3 Validity of the identification strategy and potential threats

The main hypothesis at the heart of our difference-in-difference approach is the Parallel Trend Assumption: in the absence of the ETS, conditional on our matching covariates, the potential outcomes of ETS installations would have followed parallel trends with matched non-ETS installations. For example, if there are shocks specific to small companies, we assume that by matching on size these shocks affect the ETS firm and matched non-ETS firm alike. As is common in causal inference, it is impossible to prove the Parallel Trend Assumption, but at least we report that emission trends on the matched sample are not different in the 2000-2005 period before the start of the ETS. Second, we show that our results are robust to matching on sectors. There are however two important caveats regarding the interpretation of our baseline.

First, Appendix B describes for each country how energy levies may affect ETS and non-ETS installations differently. The general picture is that before 2015, there are no or very few policies targeting the non-ETS installations in a different way than ETS installations. However, after 2015, all countries gradually introduce policies that affect non-ETS companies to a larger extent than ETS companies. So over the latest 8 years of our analysis, we effectively estimate the difference between ETS and non-ETS policies. This boils down to the assumption that the non-ETS policy is our baseline, that is, if there were no ETS, governments would

[3] To make our graphs, we add $\sum_{t\neq 2007} \beta_t (ETS_i \times year_i)$ as regressors. The coefficients on the year fixed effects are used to make the non-ETS lines, while β_t is added for the ETS lines.

have regulated the ETS firms in the same way as they regulated the non-ETS firms. This means that our estimate is lower than an estimate that considers a vacuum of regulation as the baseline.

Second, the Parallel Trend Assumption implies absence of anticipation effects. As said before, since we use the trial phase of the ETS as the the baseline period, we measure the effect of the ETS, on top of emission reductions that already may have happened during the trial phase. Figure 1 shows however that the large emission reductions in the EU ETS, and the difference with the non-ETS emissions start after 2007.

The second key assumption of our identification strategy is the Stable Unit Treatment Value Assumption (SUTVA). It requires that non-ETS firms are not affected by the ETS. For example, SUTVA is violated when companies with many plants transfer production from ETS installations to non-ETS installations. To avoid this, we exclude companies that have both ETS and non-ETS plants.

SUTVA may be a problem if ETS and non-ETS firms are competitors, selling in the same markets. The emission effect can go in both ways: if non-ETS companies gain market share, we overestimate the ETS effect, if there are green technology spillovers from ETS to non-ETS firms, we underestimate the effect. Most studies find that the ETS had a positive effect on turnover of ETS firms in the period 2005-2013 [Barrows et al., 2024, Dechezleprêtre et al., 2023, Marin et al., 2018]. This is in line with the Porter hypothesis [Porter and Linde, 1995]: policy can increase competitiveness, by decreasing market failures related to innovation, asymmetric information and split incentives. [Barrows et al., 2024] investigate the spillovers between ETS and non-ETS firms with a structural model using detailed French firm data until 2020. They find that non-ETS revenues are merely affected by the ETS (reduced by 0.2-0.7%). Similarly, emissions of non-ETS firms are merely affected (reduced by less than 1%), except for firms with both ETS and non-ETS plants, which we exclude. However, they find that the difference-in-difference estimator underestimates the effect of the ETS due to indirect price effects, and technology spillovers. So by excluding firms with both ETS and non-ETS plants, we will assume that spillovers are limited and that if they exist, they are more likely to lead to a modest underestimation of the effect.

Finally, SUTVA can be a problem when the ETS increases electricity prices, affecting the fossil fuel use in non-ETS firms.[4] More particularly higher electricity prices could affect non-ETS firms in the power & heat (combustion) sector in two opposite ways: a) non-ETS firms may face reduced demand for costlier electricity, b) but they may find it easier to increase their market share. Therefore, we show results for this sector separately.

To conclude, it seems prudent to consider our estimate as a lower bound of the true effect for four reasons. We compare ETS firms to non-ETS firms, which in later years have their own targetted policies. In other words, our baseline is not a policy vacuum, but the energy tax system that is applied to the non-ETS sector. Second, we report effects from 2008, ignoring abatement in the trial phase. Third, the ETS may have affected emissions in the non-ETS installations, yet this effect is most likely a modest reduction in emissions. Finally, we do not estimate the average treatment effect of the treated (ATT), but a local effect around the inclusion criterion, while showing that larger companies have larger emission reductions.

4 Data and descriptive statistics

We use emissions data from the Pollution Transfer and Release Registers (PRTR). Since the 1990s, PRTRs were established in most European countries to monitor the releases of specific pollutants to air, water

[4] Note that this does not matter for the indirect scope two emissions (via off-site electricity), because we only take into account scope one emissions, as the indirect emissions of electricity consumption are already in scope one of the electricity sector.

and soil at the installation level, covering a wide range of industrial activities such as power generation, manufacturing, and waste treatment. Beginning in 2001, large installations also had to report their pollutant releases to the Europe-wide register (EPER, later E-PRTR). The E-PRTR currently covers more than 30,000 installations that annually report their releases of 91 key pollutants including heavy metals, pesticides and greenhouse gases such as CO_2.

However, the E-PRTR reports CO_2 emissions only for installations emitting more than 100 kilo tonnes (kt) of CO_2 per year. This very high reporting threshold means that almost all installations which report CO_2 emissions to the E-PRTR are covered by the EU ETS, leaving us with very few unregulated installations to serve as a comparison group. Therefore, we collect data from the national PRTRs of France, the Netherlands, Norway, and the United Kingdom, which, unlike all other countries, have lower reporting thresholds than the European-wide registers (10 kt and lower). Although the PRTR data is self-reported, it is highly correlated with the EUTL emission data, which is verified by an external auditor (correlation coefficient of 0.989 reported by Dechezleprêtre et al. [2023]).

Except for France, the PRTR does not include information regarding regulation by the ETS. To identify ETS installations we merge the data with the EUTL compliance database, which is provided in a cleaned format by Abrell [2023]. We use the Levensthein Distance [Levenshtein, 1966] string matching on company names, combined with postcodes and other information. String matching is processed with the "stringdist" package in R [van der Loo, 2014], using a threshold of one. To ensure a high level of quality, the matches are checked manually for correctness and all non-matched ETS entities are manually matched to their respective PRTR installation. The EUTL database also allows us to observe which installations are regulated from 2012 onwards. The French PRTR data separates CO_2 emissions by total emissions and emissions from biomass [Dechezleprêtre et al., 2023]. As biomass emissions are not regulated in the EU-ETS, we subtract them from the total amount.

Table 1 shows the emissions and the number of ETS and non-ETS installations, both for the total sample and the matched sample. After matching, the emissions of both groups have a much more similar distribution, with a good overlap. Norway has the largest differences, because non-ETS installations are particularly small (80% are too small to be included) and because the algorithm accepts larger differences for small samples. Table 5, in the appendix C shows that the sectoral distribution of the matched sample is fairly balanced. The least balanced sectors are cement (1/4 non-ETS vs 3/4 ETS) and Paper (1/3 non-ETS and 2/3 ETS). To correct for sectoral imbalance we also report results for a sample which is matched on sectors. For data availability reasons, we do not include the aviation sector, which is also regulated by the ETS. We also exclude non-CO_2 greenhouse gases.

Table 1: Installation counts and emissions before 2008 by country and ETS status

Dataset	**Country**	**Non-ETS**				**ETS**				**Total**
		N	**Mean**	**p10**	**p90**	**N**	**Mean**	**p10**	**p90**	**N**
Total	France	710	44,884	50	69,170	907	137,575	11,528	291,966	1,617
	United Kingdom	1,797	12,160	79	22,709	771	81,265	725	170,015	2,568
	Netherlands	761	35,048	158	17,248	229	337,969	7,862	1,012,509	990
	Norway	321	5,632	69	12,848	63	166,229	5,024	380,579	384
	Subtotal	**3,589**	**22,903**	**59**	**32,203**	**1,970**	**139,748**	**1,413**	**306,133**	**5,559**
Matched	France	467	36,778	50	71,455	467	42,670	12,574	78,475	934
	United Kingdom	1,014	12,780	368	28,271	492	16,915	548	48,504	1,506
	Netherlands	262	35,433	1,412	43,171	113	88,370	13,734	220,539	375
	Norway	63	13,911	1,355	22,831	29	139,489	3,667	272,405	92
	Subtotal	**1,806**	**22,312**	**378**	**44,807**	**1,101**	**38,402**	**1,203**	**77,993**	**2,907**

5 Results

Our baseline results are reported in Table 2. We report results where time-specific shocks such as the financial crisis, Covid or the Russian-Ukrainian war are common for all companies (column 1), sector-specific (column 2) or country-specific (column 3). Across specifications, the estimated effect of the EU ETS on emissions ranges from -14% to -22%.[5] This is the average effect for the period 2008-2023, compared to emissions in 2003-2007.

Figures 3 and 4, and Table 3 report how the effect evolves over time, showing that in the second phase the effect is modest, around -12% as reported in Dechezleprêtre et al. [2023], whereas the effect becomes larger in the following phases, -18% in the third phase and -41% in the fourth phase. Adding up the yearly absolute emissions for each phase, we obtain a total emission reduction of 4039 $MtCO_2eq$ between 2008 and 2023.[6] A back-of-the envelope calculation using a modest social cost of carbon of €100/tCO2, implies a total of avoided climate damages of €400 Billion.

Table 2: Effect of the EU ETS on Emissions

	(1) Year FE	**(2) Sector×Year FE**	**(3) Country×Year FE**
ETS Effect (DiD)	-0.2506^{***}	-0.1484^{**}	-0.2244^{**}
	(0.0654)	(0.0492)	(0.0776)
Installation FE	Yes	Yes	Yes
Year FE	Yes	No	No
Sector×Year FE	No	Yes	No
Country×Year FE	No	No	Yes
Observations	36,533	36,533	36,533
Installations	1,973	1,973	1,973
Log-Likelihood	-165,755,036	-157,950,754	-157,091,058

Notes: Poisson pseudo-maximum likelihood (PPML) regressions, estimating equations 1- 3. Coefficients are exponentiated and correspond to a reduction of 22.1%, 13.7% and 20.0% respectively. Matching on country and on 2003-2007 emissions. ETS effect between 2008 and 2023. Heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$.

Table 3: EU ETS effects by phase

Phase	Estimate	Std. Error	p-value
Phase 2 (2008–2012)	-0.133^{*}	(0.0616)	0.030
Phase 3 (2013–2020)	-0.204^{***}	(0.0578)	0.0004
Phase 4 (2021–2023)	-0.538^{***}	(0.1520)	0.0004

Notes: Estimates are obtained from a Poisson pseudo-maximum likelihood (PPML) difference-in-differences specification including installation and country×year fixed effects. Heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

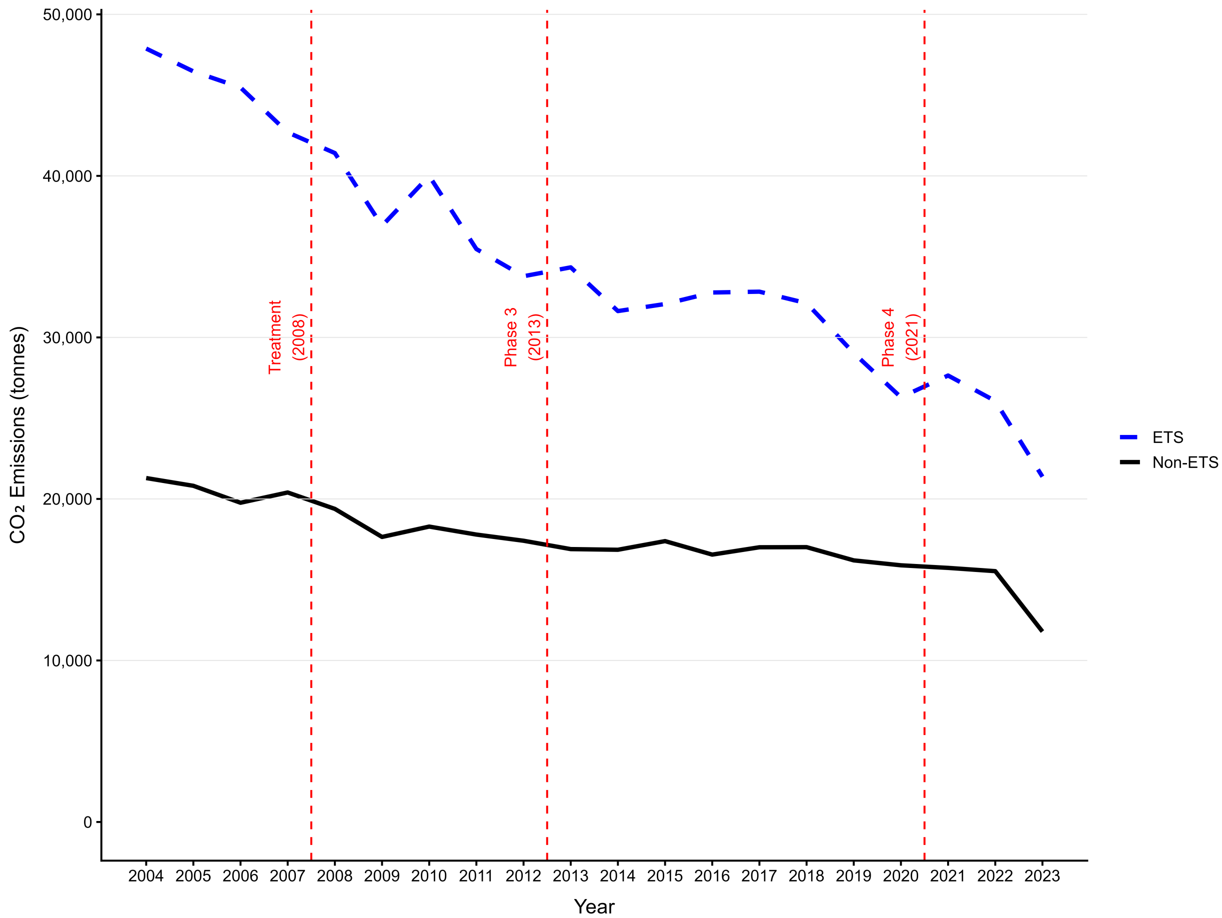
5.1 Heterogenous effects by size, sector and country

Appendix D shows that the effect of the ETS is larger in larger installations. For each increase in pre-ETS emissions by a factor of ten, emissions are decreased by an extra 13-25%. The quartile regressions show that

[5]The percentage reduction is defined as $Emissions_{ETS}/Emissions_{nonETS} - 1 = \exp(\beta) - 1$ (see equations 1-3). This equals β for modest reductions and is a bit smaller for for important reductions. We apply this conversion throughout the paper.

[6]For each year, the emission reduction is $E_t \frac{r}{1-r}$, with r the relative reduction rate of the phase $r = exp(\beta) - 1$.

Figure 3: Average emissions in the full matched panel



Notes: To avoid spurious effects from outliers, we regress $ln\left(Emissions_{it}\right) = \alpha_i + \sum_{t\neq 2007} \beta_t * \tau_t + \epsilon_{it}$ and report $Emissions_t = \overline{Emissions_{2007}} * exp(\beta_t)$ for both the ETS and non-ETS subsamples. The fixed effect avoids interpreting missings as zero and the log attenuates the difference between missings of large and small installations. Matching is based on country and emissions in years 2003-2007.

this result is mainly driven by the largest quartile. Reduction effects are similar in quartiles one to three (-14%), but much larger in quartile four (-34%).

Appendix E shows the results per sector. Effects are relatively homogeneous as all sectors reduce emissions by at least 15%. Appendix F reports results by country, both for our PPML and OLS specification. The effects are strongest for the Netherlands and France, ambiguous for the UK and positive but insignificant for Norway. As the Norwegian sample has only 89 firms, which moreover are quite different in size, we cannot draw a conclusion. For France and the Netherlands PPML estimates are larger than OLS estimates, indicating that the effect is mostly driven by larger installations. For the UK, we see the opposite.

5.2 Robustness

Finally, Appendix G shows robustness results. The OLS regression on log emissions shows a lower effect of -12% instead of -20% for PPML. This is in line with our heterogenous size effects. OLS on log emissions will treat each company with the same weight. By contrast, PPML gives more weight to larger emitters,

Figure 4: Treatment effect of the EU ETS by year.

Notes: Matching on emissions in years 2003-2007. Based on a variant of PPML regression 3, including installation and country×year fixed effects, $Emissions_{it} = \exp\left(\alpha_i + \delta_{c,t} + \sum_{t=2007}^{2023} \beta_t ETS_i * \tau_t\right) + \epsilon_{it}$. The graph plots β_t. Heteroskedasticity-robust standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

resulting in lower aggregate emissions.[7] As such, PPML is more economically meaningful [Cohn et al., 2022], but also more sensitive to large emitters. As a result, estimates for single countries are not significant, even when France, Norway and the UK are taken together. However, using an OLS specification, we show that the UK and France have significant (and large) emission reductions.

When we apply full matching, we obtain a 21% larger sample size, and find a larger ETS effect of -26%.

Our sample has many missing observations. As missing observations could be endogenous (badly managed plants are more likely to have missing data), we apply pair-wise non-missing procedure, that is, whenever an installation has missing data for a given year, the matched observation is set to missing too. This shows again a larger ETS effect of -28%.

Finally, we show that imposing a better match between ETS and non-ETS firms gives very similar results of -21% (caliper 0.1 instead of 0.2 standard deviations of the propensity score).

6 Discussion and conclusion

As a well-enforced cap-and-trade system [Calel et al., 2025], the EU ETS delivers precisely what it is designed to do: reduce emissions according to the regulator's cap. This certainty about the environmental outcome is an intrinsic advantage of cap-and-trade over a carbon tax, though it comes at the cost of less predictable economic effort (i.e., the uncertain carbon price). With a carbon price below €10/tCO_2 between Jan 2012 and Jan 2018, economic effort was considered below optimum by the European Commission which led to the creation of the Market Stability Reserve. Yet, we see that even in this period of a low carbon price, emission continued to decrease, because a cap-and-trade system creates a forward-looking incentive. Also, as an economic downturn leads to emission reductions in itself, the modest carbon price in these years avoided negative competitiveness effects in a period of increased industrial bankruptcies[Dechezleprêtre et al., 2023].

As the cap for 2030 is set at a reduction of 62% compared to 2005 emissions, we can safely assume that emissions will continue to decrease. If the current rules of the EU ETS remain unchanged, the annual reduction in the cap will lead industry (and the European air transport sector) to reduce net emissions to nearly zero by 2050 at the latest. After 2050, certificates for negative emitters (via BECCS and DACCS) will compensate residual emitters. As this outcome follows mechanically from the design of the policy, it is worth asking what are the major threats to the effectiveness of EU ETS in the future.

The greatest threat is political backlash leading to policy reversal, as witnessed in Australia under Tony Abbott and in the US under Donald Trump. Until now the ETS has been successful at avoiding widespread popular criticism. The ETS generates a substantial amount of government income, with a redistribution key which is advantageous for lower income countries. This makes the policy more likely to stick. Yet in 2027, a similar ETS 2 will start, covering all non-ETS fossil fuels, targeting activities that are closer to voters' preoccupations: home heating, transport and agriculture. Support measures for low income households will be key to sustain political buy-in.

The second threat to the environmental effectiveness are large competitiveness effects. As the cap is reduced, free allowances for remaining emissions do not help to compensate companies for increasing abatement costs. And in 2050, there are no more allowances to be distributed. Since 2026, the Carbon Border Adjustment Mechanism begins to phase in the same carbon price as in the EU ETS on trade-exposed carbon-intensive products such as steel, cement, aluminium, ammonia and hydrogen. This list might be extended in

[7] With OLS, a reduction of -2% of a small company and -4% of a three times larger company will be averaged as -3%. with PPML, the result will be -3.5%, which is the aggregate reduction.

the future, to avoid downstream leakage. Yet the CBAM does not avoid carbon leakage via export competitiveness effects. For example, EU steel exports represent a fifth of the EU production, almost as much as steel imports. So export compensation for abatement costs will be needed to avoid export emission leakage. Some politicians and NGO's are opposing these 'subsidies for polluting goods', yet they fail to see that unlike fossil fuel subsidies, these export rebates would reduce world emissions as they are designed to avoid the substitution of low-carbon European industrial products by more polluting imports.

For now, the EU ETS is a success story. The industrial and power installations covered by the ETS emitted 1,001 MtCO2eq in 2024. This is 50% less than in 2005. This absolute reduction in emissions is very straightforward to observe. Yet, to understand the causal effect of the ETS on emissions, we need to establish a baseline: what would have happened to the ETS emissions without regulation. Emissions of ETS companies would also have been reduced by other factors such as a shift to the service sector, a trend towards industrial products with high added value, energy efficiency driven by high fossil fuel prices etc. Using matched non-ETS firms emissions as a baseline, we find a causal effect of the EU ETS on emissions between -14 and -22% on average. Although effects are volatile from one year to the other, there is a very clear increasing trend in the effect with an average effect exceeding 41% in the period 2021-2023. This estimate of 41% reduction for the third phase represents roughly 800 $MtCO_2$ per year, making the EU ETS the most impactful climate policy in the world.

References

Jan Abrell. Database for the european union transaction log. Online, 2023. URL https://euets-info-public.s3.eu-central-1.amazonaws.com/Description_EUTL_database.pdf. Accessed: 2023-12-08.

Geoffrey Barrows, Raphael Calel, Martin Jégard, and Hélène Ollivier. Estimating the effects of regulation when treated and control firms compete: A new method with application to the eu ets. 2024. URL https://ssrn.com/abstract=4459815.

Patrick Bayer and Michäel Aklin. The european union emissions trading system reduced co2 emissions despite low prices. *PNAS*, 117, 2020. doi: 10.1073/pnas.1918128117/-/DCSupplemental.y. URL www.pnas.org/cgi/doi/10.1073/pnas.1918128117.

Francesco Biancalani, Giorgio Gnecco, Rodolfo Metulini, and Massimo Riccaboni. The impact of the european union emissions trading system on carbon dioxide emissions: a matrix completion analysis. *Nature Scientific Reports*, 14:19676, 12 2024. ISSN 20452322. doi: 10.1038/s41598-024-70260-6.

Massimo Bordignon and Duccio Gamannossi degl'Innocenti. Third time's a charm? assessing the impact of the third phase of the eu ets on co2 emissions and performance. *Sustainability*, 15, 4 2023. ISSN 20711050. doi: 10.3390/su15086394.

Raphael Calel. Adopt or innovate: Understanding technological responses to cap-and-trade. *American Economic Journal: Economic Policy*, 12:170–201, 8 2020. ISSN 1945-7731. doi: 10.1257/pol.20180135.

Raphael Calel, Antoine Dechezlepretre, and Frank Venmans. Policing carbon markets. *Climate Policy*, pages 1–19, 2025.

Jonathan B. Cohn, Zack Liu, and Malcolm I. Wardlaw. Count (and count-like) data in finance. *Journal of Financial Economics*, 146(2):529–551, 2022. ISSN 0304-405X. doi: https://doi.org/10.1016/j.jfineco.2022.08.004. URL https://www.sciencedirect.com/science/article/pii/S0304405X2200174X.

Jonathan Colmer, Ralf Martin, Mirabelle Muûls, and Ulrich J. Wagner. Does pricing carbon mitigate climate change? firm-level evidence from the european union emissions trading scheme. *Review of Economic Studies*, pages 1625–1660, 2025. doi: 10.2139/ssrn.3725482.

Antoine Dechezleprêtre, Daniel Nachtigall, and Frank Venmans. The joint impact of the european union emissions trading system on carbon emissions and economic performance. *Journal of Environmental Economics and Management*, 118, 3 2023. ISSN 10960449. doi: 10.1016/j.jeem.2022.102758.

Mohammadehsan Eslahi, Anna Creti, and Maria-Eugenia Sanin. Mission accomplished? a post-assessment of eu ets impact on power sector emissions. 12 2024. URL http://www.chair-energy-prosperity.org/category/publications/http://www.chair-energy-prosperity.org/en/category/publications-2/.

Daniel E. Ho, Kosuke Imai, Gary King, and Elizabeth A. Stuart. MatchIt: Nonparametric preprocessing for parametric causal inference. *Journal of Statistical Software*, 42(8):1–28, 2011. doi: 10.18637/jss.v042.i08.

Jurate Jaraite and Corrado Di Maria. Did the eu ets make a difference? an empirical assessment using lithuanian firm-level data. *Energy Journal*, 37:1–23, 2016. ISSN 01956574. doi: 10.5547/01956574.37.1.jjar. URL https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2382663.

Olivier De Jonghe, Klaas Mulier, and Glenn Schepens. Going green by putting a price on pollution : Firm-level evidence from the eu. 2020. URL www.nbb.be.

ME Klemetsen, KE Rosendahl, and AL Jakobsen. The impacts of the eu ets on norwegian plants' environmental and economic performance. *Climate Change Economics*, 1:2050006, 2020. doi: https://doi.org/10.1142/S2010007820500062. URL https://brage.bibsys.no/xmlui/handle/11250/2570307.

Vladimir Levenshtein. Binary codes capable of correcting deletions, insertions, and reversals. *Soviet physics doklady*, 10(8):707–710, 1966.

Giovanni Marin, Marianna Marino, and Claudia Pellegrin. The impact of the european emission trading scheme on multiple measures of economic performance. *Environmental and Resource Economics*, 71:551–582, 10 2018. ISSN 0924-6460. doi: 10.1007/s10640-017-0173-0. URL http://link.springer.com/10.1007/s10640-017-0173-0.

Sebastian Petrick and Ulrich J. Wagner. The impact of carbon trading on industry: Evidence from german manufacturing firms. *Kiel Working Paper, No. 1912*, 2014.

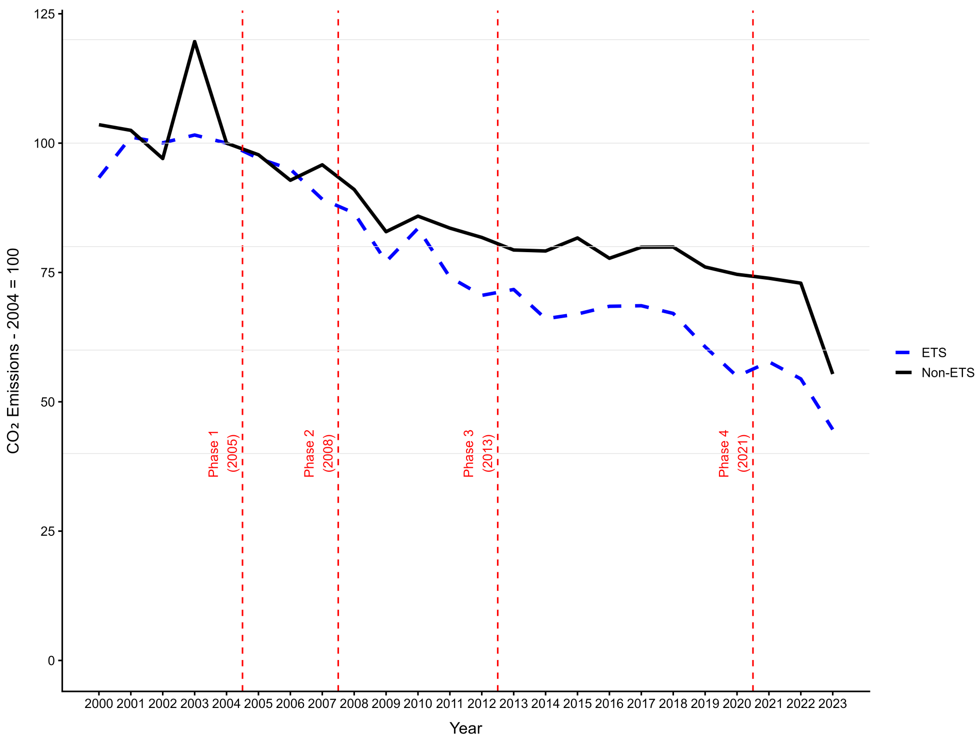
Michael E Porter and Claas Van Der Linde. Toward a new conception of the environment-competitiveness relationship the link from regulation to promoting innovation. *Journal of Economic Perspectives*, 9:97–118, 1995.

Mark P.J. van der Loo. The stringdist Package for Approximate String Matching. *The R Journal*, 6(1):111–122, 2014. doi: 10.32614/RJ-2014-011. URL https://doi.org/10.32614/RJ-2014-011.

Frank Maarten Jan Venmans. The effect of allocation above emissions and price uncertainty on abatement investments under the eu ets. *Journal of Cleaner Production*, 126:595–606, 2016. ISSN 09596526. doi: 10.1016/j.jclepro.2016.02.108. URL http://dx.doi.org/10.1016/j.jclepro.2016.02.108.

A Parallel trends

Figure 5: Pre-treatment Trend, Matched Sample



Notes: Same specification and construction as in Figure 3, indexed to 100 in 2007.

B Policies targeting non-ETS firms

In France, from 2014 onwards, non-ETS firms pay a €7/tCO_2 carbon tax (CCE Contribution Climat-Energie), which is linearly increasing to €44.6/tCO_2 in 2018 and remaining at that level thereafter.

In the UK, the climate change levy is a modest levy on energy use, with implicit carbon prices of €10 to €23/tCO_2 (since 2021 £8/MWh for electricity and £4/MWh for coal, oil and gas). ETS firms are more often exempt, than non-ETS firms.

In 1991, Norway introduced a sector-specific carbon tax at an average rate of roughly €50/tCO_2. When the ETS was introduced, companies continued to pay the carbon tax. However, by 2012-2015, ETS companies were increasingly exempt from the tax, with typical reductions from 50 to 80% (not for the petroleum sector). In 2023, most non-ETS companies faced a carbon tax around €75/tCO_2, though with several exemptions (greenhouses, waste incineration, process emissions...).

In 2019, the Netherlands have revised their energy tax system, increasing marginal tax rates for small consumers. The gas levy corresponds to an implicit €60/tCO_2 for the second energy tax bracket (below 1 million m^3 gas) and €20/tCO_2 for the next (up to 10 million m^3 gas). Similarly, the electricity levy is €50/MWh for the second tax bracket (10-50 MWh) and €13/MWh for the third bracket (50-10.000 MWh). To the extent that we match on pre-2005 emissions, the energy tax affects both groups equally, but as we tolerate differences, ETS firms emit on average more than the matched non-ETS firms.

To the extent that non-ETS firms are also treated by specific policies, our difference-in-difference method estimates the difference between the ETS and the policies in the non-ETS sectors.

C Sector definition and balance

Table 4: Mapping of NACE 2-digit codes to sector groups and UK ETS sector labels

Sector group	**NACE 2-digit**	**UK PRTR sector labels**
Power & heat (combustion)	35, 38	• Major power producers • Minor power producers • Other fuel production • Processing & distribution of natural gas
Refineries & chemicals	19, 20	• Oil & gas exploration and production • Chemical industry • Processing & distribution of petroleum products
Cement & minerals	23	• Cement • Lime • Other mineral industries
Metals	24	• Iron & steel industries • Non-ferrous metal industries
Pulp, paper & wood	16, 17	• Paper, printing & publishing industries
Food & beverages	10, 11	• Food, drink & tobacco industry
Other manufacturing	others	• Agriculture, forestry & fishing • Commercial • Electrical engineering • Mechanical engineering • Miscellaneous • Other industries • Public administration • Textiles, clothing, leather & footwear • Vehicles • Waste collection, treatment & disposal

Table 5: Sample distribution of observations by sector and ETS status

Sector group	**Control**	**Treated**	**Total**	**% Treated**
Other manufacturing	6 995	6 554	13 549	48.4
Power & heat (combustion)	3 189	2 941	6 130	48.0
Refineries & chemicals	3 646	2 748	6 394	43.0
Cement & minerals	2 159	1 913	4 072	47.0
Food & beverages	2 439	1 498	3 937	38.0
Pulp, paper & wood	596	1 005	1 601	62.8
Metals	1 209	333	1 542	21.6

D Size effects

Table 6: Heterogeneous ETS effects by pre-2008 installation size (continuous specification)

	Two-way FE	**Sector × Year FE**	**Country × Year FE**
ETS Effect	-0.1964^{***}	-0.0970^{*}	-0.1373^{*}
	(0.0670)	(0.0523)	(0.0798)
ETS × $\log_{10}$(Pre-treatment emissions)	-0.1416^{***}	-0.1450^{***}	-0.2821^{***}
	(0.0516)	(0.0516)	(0.0480)
Installation FE	Yes	Yes	Yes
Year FE	Yes	No	No
Sector × Year FE	No	Yes	No
Country × Year FE	No	No	Yes
Observations	36,369	36,369	36,369

Notes: Poisson pseudo-maximum likelihood (PPML) regressions. The dependent variable equals emissions in tCO_2e divided by 50,000. Pre-treatment installation size is measured as the base-10 logarithm of average emissions over 2003–2007 (normalized by 50,000). The coefficient on the ETS indicator therefore represents the treatment effect evaluated at a baseline installation size of 50,000 tonnes of emissions. The interaction term shows how the ETS effect changes when pre-treatment emissions increase by a factor of ten. Heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 7: Heterogeneous ETS effects by pre-2008 installation size (quartile specification)

	Country × Year FE
ETS Effect (Quartile 1 – smallest installations)	-0.1507^{**}
	(0.0735)
Additional effect: Quartile 2 vs Q1	0.0928
	(0.0988)
Additional effect: Quartile 3 vs Q1	-0.1956^{**}
	(0.0893)
Additional effect: Quartile 4 vs Q1	-0.2507^{***}
	(0.0778)
Installation FE	Yes
Country × Year FE	Yes
Observations	36,369

Notes: PPML regressions. Installations are grouped into quartiles based on average pre-treatment emissions over 2003–2007. The dependent variable equals emissions divided by 50,000. The coefficient on the ETS indicator represents the treatment effect for installations in the first quartile (smallest installations). Additional coefficients represent differential effects relative to Quartile 1. Installation fixed effects and country-year fixed effects are included. Heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

E Results per sector

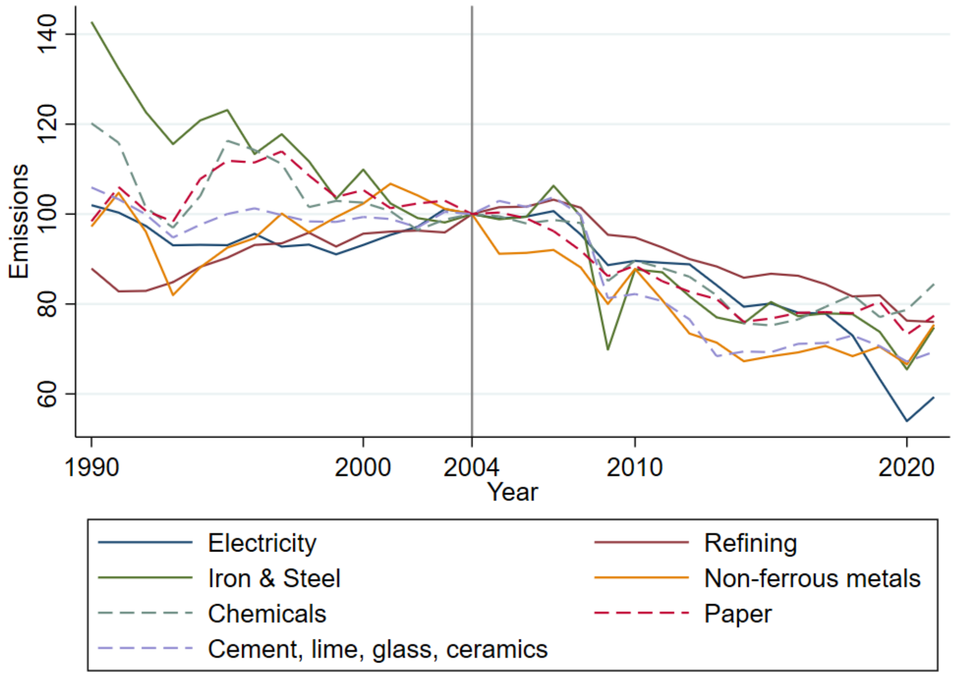


Figure 6: Evolution of non-process emissions in ETS sectors in the EU28, using UNFCCC reported data.

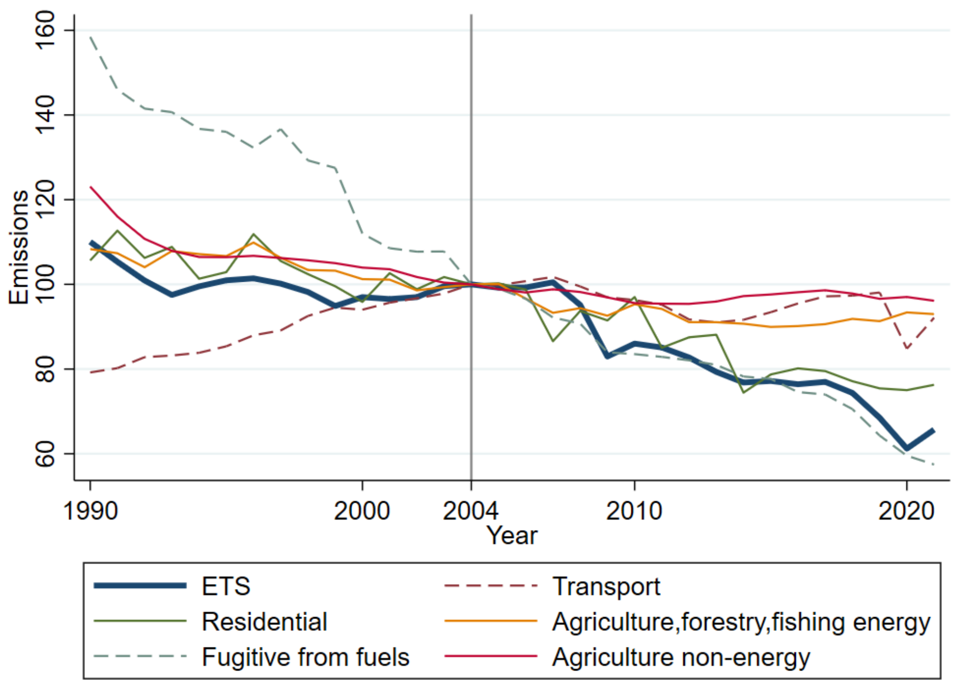


Figure 7: Evolution of emissions in non-ETS sectors in the EU28, using UNFCCC reported data.

Table 8: Heterogenous ETS effects by sector

Sector	Estimate	Std. Error	z	p
Cement & minerals	-0.217^{***}	0.061	-3.54	0.000
Food & beverages	-0.169^{**}	0.060	-2.83	0.005
Metals	-0.167^{*}	0.066	-2.54	0.011
Other manufacturing	-0.319^{***}	0.079	-4.02	0.000
Power & heat (combustion)	-0.167^{*}	0.082	-2.03	0.042
Pulp, paper & wood	-0.247^{***}	0.063	-3.95	0.000
Refineries & chemicals	-0.246^{***}	0.067	-3.68	0.000

Notes: Poisson pseudo-maximum likelihood (PPML) estimation with installation and year fixed effects (equation 1). N = 36,533. Matching on country and 2003-2007 emissions. Two-way heteroskedasticity-robust standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

F Results per country

This appendix shows emission results per country, using PPML regressions, OLS regressions and graphs. Figures are made with the same method as in Figure 3

Table 9: Country-specific ETS treatment effects (PPML estimates)

Country	Estimate	Std. Error	Installations	Observations
France	-0.2498	(0.1923)	792	11,052
Netherlands	-0.3842^{***}	(0.0774)	360	5,864
Norway	0.2309^{*}	(0.1292)	89	1,252
United Kingdom	0.0292	(0.0401)	1,417	18,365

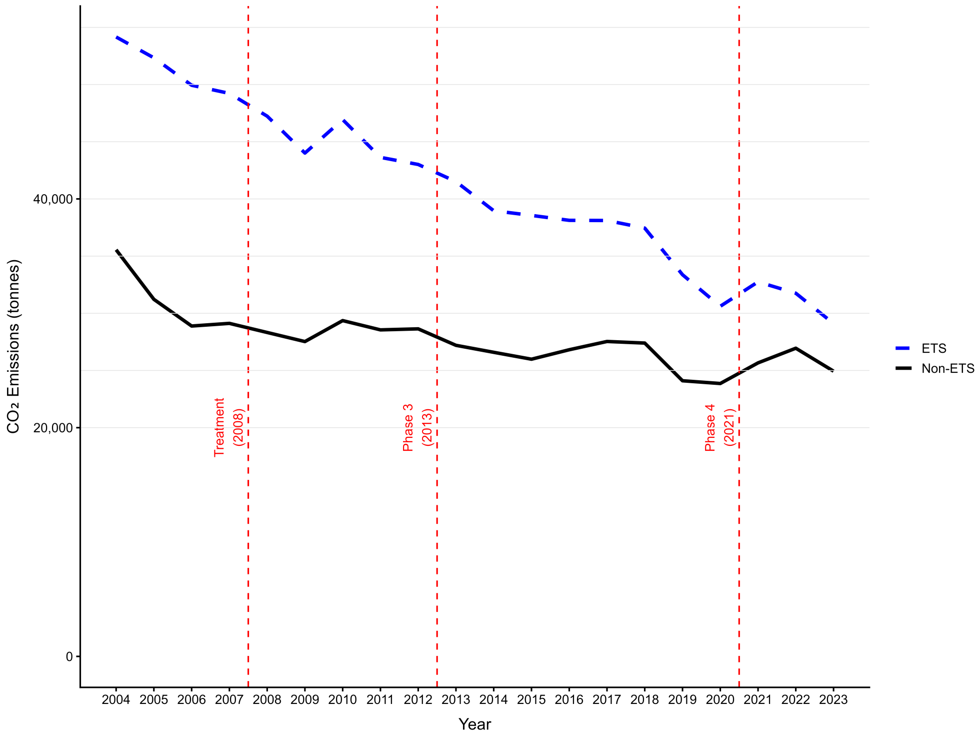
Notes: Each column reports Poisson pseudo-maximum likelihood (PPML) estimates of the ETS treatment effect estimated separately by country. All regressions include installation and year fixed effects. The dependent variable is emissions in levels. Two-way heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.10$.

Table 10: Country-specific ETS treatment effects (OLS estimates)

Country	Estimate	Std. Error	Installations	Observations
France	-0.1120^{***}	(0.0243)	772	10,329
Netherlands	-0.1260^{*}	(0.0603)	360	5,852
Norway	0.2432	(0.1694)	89	1,191
United Kingdom	-0.1744^{***}	(0.0331)	1,417	18,364

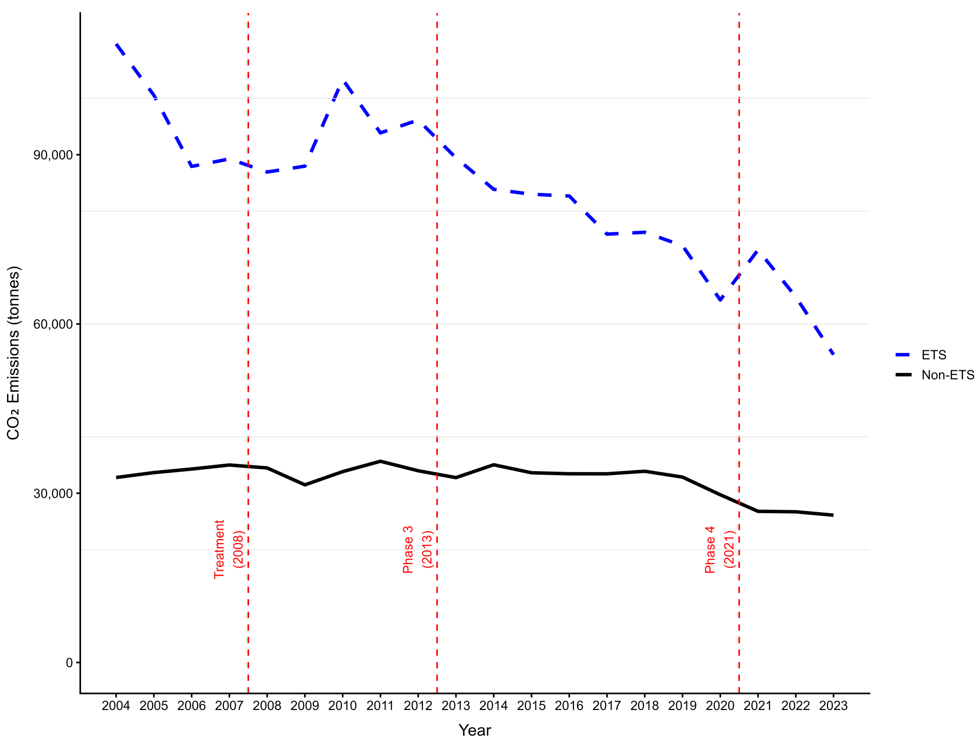
Notes: Each column reports log-linear OLS estimates of the ETS treatment effect estimated separately by country. All regressions include installation and year fixed effects. The dependent variable is log emissions. Two-way heteroskedasticity-robust standard errors in parentheses. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.10$.

Figure 8: Emissions of matched ETS and non-ETS installations in France.



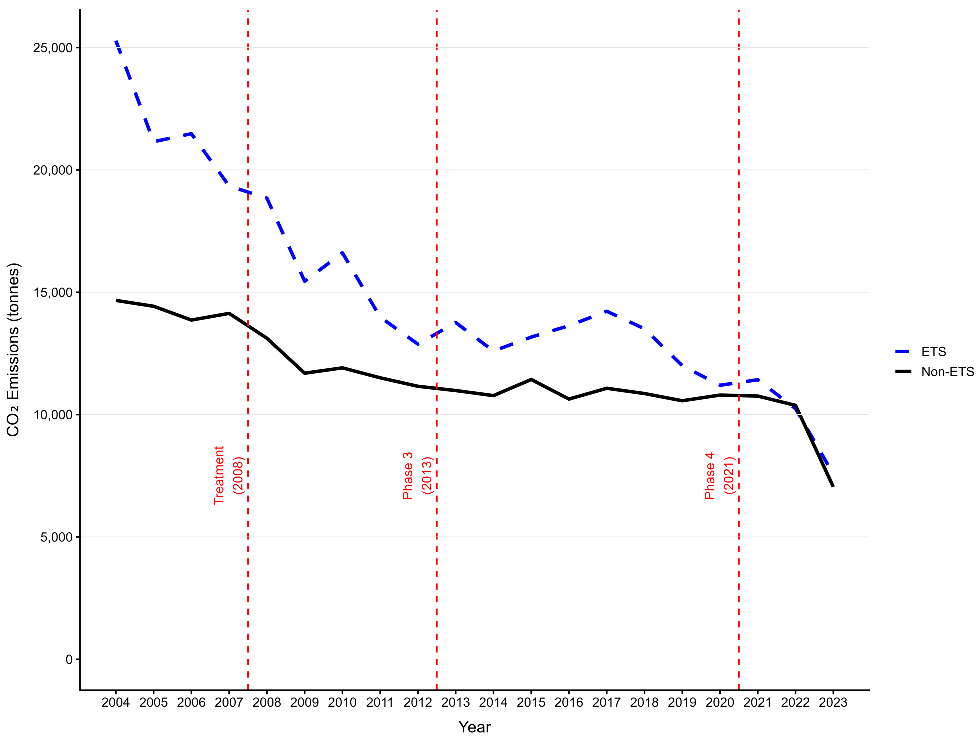
Notes: Same specification and construction as in Figure 3, applied to French sample.

Figure 9: Emissions of matched ETS and non-ETS installations in the Netherlands



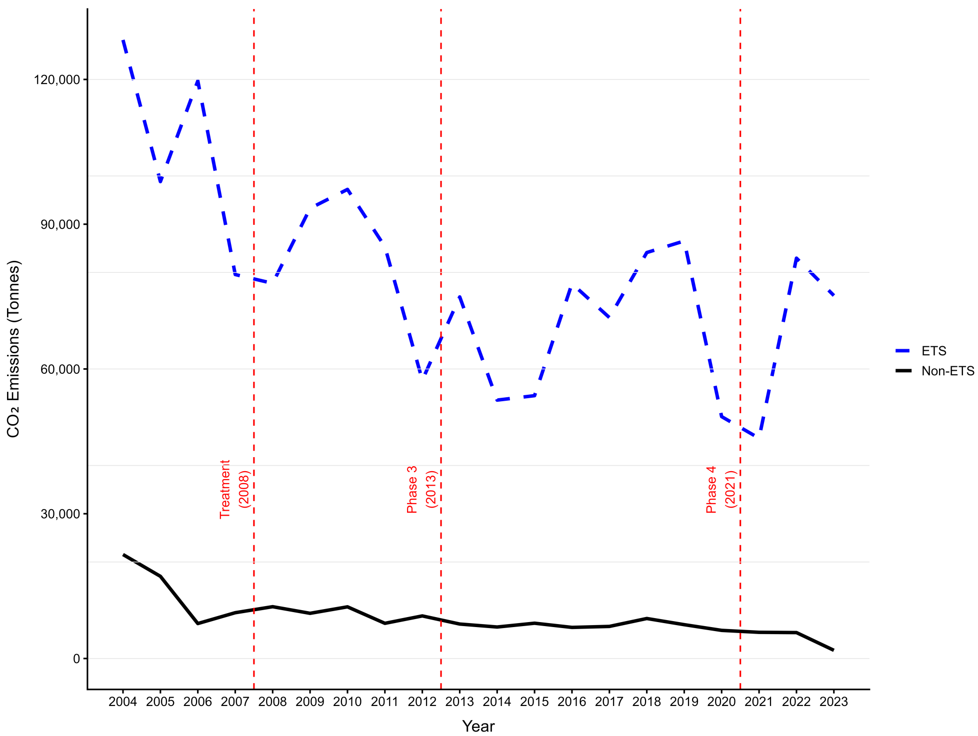
Notes: Same specification and construction as in Figure 3, applied to Dutch sample.

Figure 10: Emissions of matched ETS and non-ETS installations in the UK.



Notes: Same specification and construction as in Figure 3, applied to the UK sample.

Figure 11: Emissions of matched ETS and non-ETS installations in Norway.



Notes: Same specification and construction as in Figure 3, applied to Norwegian sample. Note that our caliper of 0.2 times the standard deviation of the propensity score accepts larger differences in emissions for countries with fewer observations. The sample includes 29 ETS companies and 63 non-ETS companies, which is much lower than in the other countries (Table 1). The sensitivity analysis with lower caliper 0.1 gives very similar results (last line table 11).

G Robustness checks

Table 11: Robustness checks

Specification	Estimate	Std. Error	Installations	Observations
Baseline (PPML)	-0.2244^{**}	(0.0776)	2,658	36,533
OLS (log, same matched sample)	-0.1322^{***}	(0.0217)	2,638	35,736
Omitting countries (PPML)				
Without United Kingdom	-0.3001^{**}	(0.0998)	1,241	18,168
Without France	-0.2095^{***}	(0.0501)	1,866	25,481
Without Netherlands	-0.1277	(0.1144)	2,298	30,669
Without Norway	-0.2365^{**}	(0.0794)	2,569	35,281
Alternative specifications				
Full matching (PPML)	-0.3010^{*}	(0.1708)	3,246	44,340
Pairwise non-missing (PPML)	-0.3276^{***}	(0.0559)	981	8,998
Naive estimator (OLS, full sample)	-0.1549^{***}	(0.0208)	4,910	58,694
Exact matching on sectors (PPML)	-0.3072^{***}	(0.0616)	1,922	26,133
Exact matching on sectors (OLS)	-0.1388^{***}	(0.0259)	1,915	25,791
Caliper 0.1 (PPML)	-0.2121^{*}	(0.0888)	2,462	33,800

Notes: Baseline and robustness specifications are estimated using Poisson pseudo-maximum likelihood (PPML) and include installation and country×year fixed effects, unless otherwise stated. The OLS specifications use log emissions as the dependent variable. The exact matching specifications additionally restrict the sample to installations matched exactly on sector classifications prior to estimation, precisely on NACE2 level for France, the Netherlands, and Norway, and the sector groups from Table 4 for the UK due to constraints in data availability. All models use heteroskedasticity-robust standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.