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The joint impact of the European Union emissions trading system on carbon emissions and economic performance☆

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ABSTRACT

This paper investigates the impact of the European Union Emissions Trading System (EU ETS) on carbon emissions and economic performance based on a matching methodology exploiting installation-level inclusion criteria combined with difference-in-differences. Installation-level data from national Polluting Emissions Registries in France, Netherlands, Norway and the United Kingdom point to a reduction in carbon emissions in the order of −10% between 2005 and 2012, in line with existing micro and macro evidence. Meanwhile, firm-level data on the 31 ETS-regulated countries shows that the EU ETS had no significant impact on profits and employment, and led to an increase in regulated firms' revenues and fixed assets. We explore various explanations for these findings.

1. Introduction

Emissions trading programs have assumed an ever more prominent role in environmental policy over the last few decades. In 2021, the European Union Emissions Trading System (EU ETS) was still the largest cap-and-trade program in the world by traded value.[1] The EU ETS was launched in 2005, allocating tradable emissions permits to over 12,000 power stations and industrial plants in 31 countries, accounting for over 40% of the EU's total greenhouse gas emissions.

Between 2005 and 2012, verified emissions by regulated installations were reduced by 17% at constant scope,[2] but this reduction might not have been caused by the EU ETS, because many other factors affected emission trends, such as the economic crisis of 2008, fuel prices and business-as-usual industrial trends. These factors, combined with relatively low carbon prices on the EU ETS market, have led many to wonder whether emissions would have been higher without the policy (Martin et al., 2016).

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[1] China now hosts the world's largest carbon market by emissions.
[2] This reduction accounts for the fact that countries and sectors were added over time (European Environmental Agency, https://www.eea.europa.eu/data-and-maps/dashboards/emissions-trading-viewer-1).

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Like all of the new emissions trading initiatives around the globe, the EU ETS was expected to reduce carbon emissions in a cost-effective manner, and to spur the development of new low-carbon technologies. However, right from its introduction, there have been concerns about the potential impacts of the EU ETS on the competitiveness of regulated businesses. Indeed, environmental regulations may add costs to companies and divert resources away from productive activities, thereby slowing down productivity growth. Some, therefore, expected the EU ETS to affect the competitiveness of the European industry, in particular since the stringency of climate change policies is lower outside Europe, putting companies regulated under the EU ETS at a disadvantage with respect to their foreign competitors. As a consequence, European businesses may move manufacturing capacity to countries with relatively laxer policies, causing policy-induced pollution leakage, as predicted by the pollution haven hypothesis (Levinson and Taylor, 2008).

An alternative view, articulated by Michael Porter (Porter, 1991), is that environmental regulations such as the EU ETS might lead regulated firms and the economy as a whole to become more competitive internationally by providing incentives for environment-friendly innovation that would not have happened in the absence of policy. Both of these views have received much attention by policy makers, particularly in the context of the 2008 financial crisis. Indeed, EU policy makers have often articulated their vision that the EU ETS would be a driving force of low-carbon innovation and economic growth (see, for instance: European Commission, 2005 and European Commission, 2012). Empirical evidence has shown that the EU ETS has increased innovation activity in low-carbon technologies among regulated entities by as much as 30% compared to a counterfactual scenario (Calel and Dechezleprêtre, 2016), but this does not imply that the competitiveness of regulated companies has consequently improved.

In this paper, we conduct the first comprehensive, European-wide investigation of the impact of the EU ETS on carbon emissions and economic performance of regulated companies from 2005 to 2014. The EU ETS offers a unique opportunity to investigate the causal impact of an environmental policy on firms' environmental performance and economic outcomes, because the policy was designed to cover only installations above a certain production capacity threshold. Installations falling below this threshold are not covered by EU ETS regulations, even though they can be very similar to regulated entities. We can thus exploit these installation-level inclusion criteria to compare installations or firms operating in the same country and sector and of similar characteristics, but which have fallen under different regulatory regimes in 2005. This provides an opportunity to apply the sort of quasi-experimental techniques most suited to assessing the causal impacts of environmental policies (Greenstone and Gayer, 2009; List, Millimet, Fredriksson and McHone, 2003). We compare ETS and non-ETS entities both before and after the EU ETS started, applying a matched difference-in-differences study design that enables us to control for confounding factors which affect both regulated and unregulated entities (demand conditions, input prices, sector- and country-specific policies, etc.), as well as installation- and firm-level heterogeneity (Abadie and Imbens, 2008; Abadie, 2005; Heckman, Ichimura, Smith, and Todd, 1998; Heckman, Ichimura and Todd,1998; Smith and Todd, 2005).

A casual look at sector-level data suggests that CO2 emissions have been declining more rapidly after the implementation of the EU ETS in each sector covered by the policy. To evaluate the causal impact of the EU ETS on carbon emissions, we use emissions data at the installation level from the national Pollution Release and Transfer Registers (PRTRs) of France, the Netherlands, Norway, and the United Kingdom. In contrast to most other European countries, the PRTRs of these four countries are characterized by a low reporting threshold for carbon emissions (below 10 kt per year) and therefore include data on many installations that are not covered by the EU ETS, which may offer a suitable control group against which to compare the emissions performance of regulated installations. Our emissions results are based on 240 pairs of EU ETS and similar non-EU ETS installations across the four countries. This sample size is arguably small, but this is a usual feature of matching studies: by restricting the sample to installations that are closely comparable, one necessarily reduces the sample size (excluding the largest firms), but to the benefit of accurately determining the policy impact around the inclusion threshold.

To evaluate the impact of the EU ETS on firm performance, we use a newly constructed data set combining financial data from Orbis – which records key firm characteristics, including sector of activity, revenue, assets, profit and number of employees – with the European Union Transaction Log (EUTL), which records regulatory status with respect to the EU ETS. Our data set includes information on over 1 million firms across 31 countries, of which we identify over 8200 firms regulated under the EU ETS. Using this data set, we are able to compare regulated and unregulated firms both before and after the EU ETS was launched. Our matching procedure allows us to construct a group of 1787 EU ETS firms matched with the closest non-EU ETS firms based on country, sector and pre-ETS firm-level characteristics.

Our results suggest that the introduction of the EU ETS was associated with a statistically significant reduction of carbon emissions in the order of −10% in the first two trading phases between 2005 and 2012, with sensitivity tests pointing to effects between −7% and −16%. While these findings are limited by the small sample size and might not be externally valid beyond this set of installations in the four countries covered, they add to a growing body of evidence which points to significant impacts of the EU ETS based on country-specific micro-data studies (Colmer et al., 2022; Klemetsen et al., 2020; Petrick and Wagner, 2014) and sector-level data (Bayer and Aklin, 2020). Most of the emission reduction is observed in the second trading phase of the EU ETS: the impact is −6% for the first phase and −15% in the second phase. The effect is strongest for larger installations, in line with the idea that pollution control is capital intensive and involves high fixed costs. Finally, our results suggest that a less generous allocation of free allowances creates a stronger incentive to reduce emissions, as under-allocated installations seem to have reduced emissions to a larger extent.

Regarding firm performance, we find that – contrary to what could have been expected – the EU ETS led to a statistically significant increase in revenue and in fixed assets of regulated firms, and did not have a statistically significant impact on regulated firms' number of employees and profit. These findings suggest that the EU ETS induced regulated companies to increase investment which, in turn, may have increased productivity (output per worker). We conclude from our analysis that the EU ETS, in its first and second phases, led to carbon emissions reductions without negatively affecting the economic performance of regulated firms and thus the competitiveness of the European industry.

The paper contributes to the growing empirical literature on the impacts of environmental policies, and of climate change policies in particular, on environmental and economic performance. Overall, this nascent literature shows that environmental regulations tend to improve environmental performance while not weakening economic performance (Dechezleprêtre and Kruse, 2018; Dechezleprêtre et al., 2019). The literature focusing on the competitiveness effects of environmental regulations has found that environmental policies can lead to statistically significant adverse effects on trade, employment, plant location and productivity in the short run, in particular in a well-identified subset of pollution- and energy-intensive sectors, but that these impacts are temporary and small relative to general trends in production (Dechezleprêtre and Sato, 2017).

The paper proceeds as follows. Section 2 presents some background information on the EU ETS and provides some description of emissions trends. Section 3 surveys the evidence on the impact of the EU ETS on carbon emissions and economic performance. In section 4 we present a causal analysis regarding the impact of the EU ETS on carbon emissions using installation level data. In section 5 we estimate the impact of the EU ETS on the economic performance of regulated firms based on financial data. Section 6 concludes by considering some of the potential policy implications of our findings, and directions for future research.

2. Background on the EU ETS and trends of GHG emissions

The EU ETS was launched in 2005. It currently covers 30 countries across Europe (all 27 European Union Member States plus Iceland, Liechtenstein and Norway).[3] Currently, the EU ETS covers around 10,000 power stations and industrial facilities, representing roughly 40% of the EU's total greenhouse gas emissions. As in any cap-and-trade system, at the end of each year EU ETS installations are required to surrender as many permits as they emit GHG-emissions. Prior to the compliance date, installation operators can freely trade permits with each other (as well as with financial intermediaries and private citizens).

The EU ETS has been divided into a number of trading phases, with successively more stringent emissions caps for each phase. For the first phase, the emissions cap was fixed at 2298 Mt CO2e per year. Fig. 1 plots the emission caps along with the verified emissions over time of all regulated installations based on the emissions data from the European Union Transaction Log (EUTL), the European Commission's centralized carbon emissions inventory which records emissions of all regulated installations. As can be seen from Fig. 1, the verified emissions of installations covered by the EU ETS have been declining over time. One of the objectives of this paper is to understand whether this decrease can be attributed to the EU ETS or whether it is the result of a longer lasting trend or of other factors.

Phase 1, running from 2005 to 2007, was insulated from later phases by prohibiting banking of permits across the phase boundary. Fig. 1 indicates an oversupply of permits in the first phase (with total allocation greater than verified emissions), which is why the permit price approached zero at the end of the first trading period (see Fig. 2). Phase 2 (2008–2012) and Phase 3 (2013–2020) allow firms to bank unused permits for later use, as well as a limited form of borrowing[4] against future emissions reductions. This explains why the price has remained above zero despite over-allocation, and also why verified emissions exceeded the cap in 2008. With Phase 3, the coverage of the EU ETS also became broader and previously unregulated sectors such as aviation and the production of aluminium became regulated.[5]

Fig. 2 presents the price of EU ETS allowances between 2005 and 2015. The average spot price in this period was around €10–15, but has varied between €0 and €30. In the beginning of the third phase, the spot price has ranged between €5–7.[6] Hintermann et al. (2016) discuss the main price-drivers in the EU ETS, such as fossil fuel prices, economic growth, weather, marginal abatement costs and uncertainty. If companies see a (real) option value in holding allowances for future compliance, they may abate emissions at a marginal cost exceeding the carbon price. Or inversely, barriers to investment may lead to marginal abatement costs below the carbon price. So, although the carbon price is observed, the induced abatement remains an empirical question.

To understand if the declining trend in emissions observed in Fig. 1 is a consequence of the EU ETS, we first take a casual look at aggregate trends. Since the EUTL does not include emissions data before the introduction of the EU ETS, we use data from the national GHG inventory of the United Nations Framework Convention on Climate Change (UNFCCC), which provides carbon emissions data at the sector level for all Annex I countries from 1990 to 2014. Under the UNFCCC, each country is required to report its national greenhouse gas inventory on an annual basis according to a standardized methodology developed by the Conference of the Parties (COP)[7] which makes inter-country comparisons possible. The inventory report covers all emissions and removals of direct GHGs at a disaggregated sectorial approach. We retrieve emissions from the six sectors with the highest emissions: electricity and heat; petroleum refining and coke production; metals including iron and steel; chemicals; pulp and paper; and non-metallic minerals.[8]

Merging the UNFCCC emissions with the data from the EUTL allows us to calculate sector-specific EU ETS coverage rates. These are displayed in Fig. 3. The electricity and heat sector has the highest coverage rate by the EU ETS with 82% of emissions regulated. This is followed by the pulp and paper sector (78%) and the mineral sector (75%) while the chemical sector displays the lowest coverage rate with 42%. As is clear from Fig. 3, the EU ETS only covers a part of each sector's emissions, implying that there exist many installations

[3] A UK Emissions Trading Scheme (UK ETS) replaced the UK's participation in the EU ETS on 1 January 2021.

[4] For emissions in year X, firms receive free allocation on the 28th of February in year X and have to surrender allowances on the 30th of April in year X+1.

[5] Ellerman et al. (2010) give a more comprehensive review of the design and implementation of the EU ETS.

[6] The price increased above €20 from 2018 onwards, after the introduction of a mechanism that eliminates allowances in case of excessive banking.

[7] See http://www.ipcc-nggip.iges.or.jp/public/2006gl/.

[8] Table A1 in the Appendix shows the sector name, the UNFCCC code and the corresponding NACE Rev. 2 code.

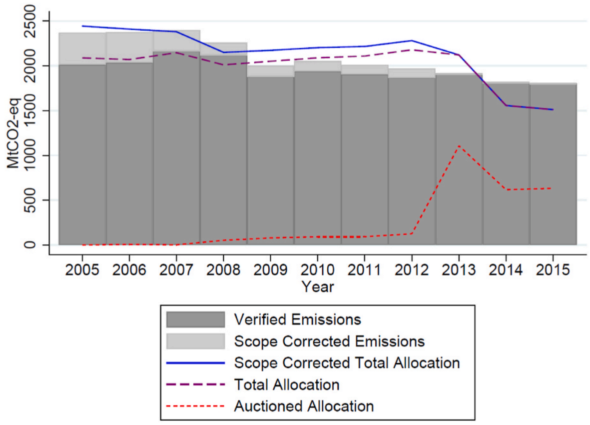

Fig. 1. Overall cap and verified emissions from EU ETS stationary installations 2005–2015.
Source: EEA data viewer. The scope correction adjusts for the inclusion of new sectors and countries over time. Total allocation includes free allocation and auctioned allocation (EUA's) and excludes the use of Kyoto credits (CER and ERU) in the period 2005–2012.

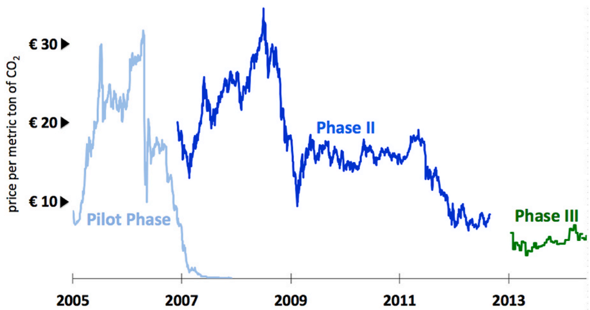

Fig. 2. Price of EU ETS allowances 2005–2015.
Source: European Environment Agency and Intercontinental Exchange.

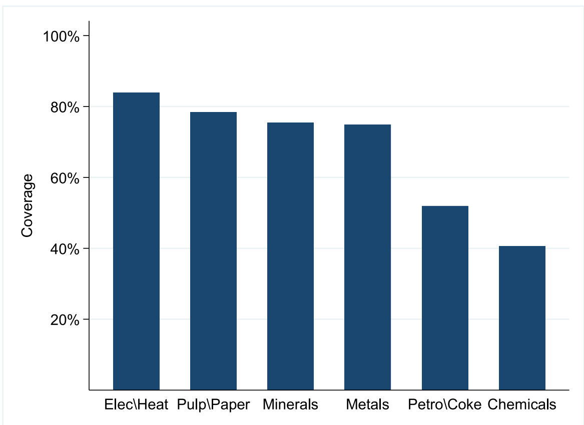

Fig. 3. Coverage rate of EU ETS emissions for specific sectors.
Source: EUTL and UNFCCC, own calculations. Sectors are defined by the ETS regulation and by UNFCCC guidelines

which are not covered by the EU ETS and, thus, can serve as a potential control group in the causal analyses presented in Sections 4 and 5.

Fig. 4 presents the trend in emissions in the six main sectors covered by the EU ETS. To compare the sectors with each other, we normalize emissions in the year 2005 to 100. Fig. 4 suggests that emissions have been declining more rapidly after the implementation of the EU ETS in each and every sector. However, we cannot claim that the EU ETS has been causing this decline in emissions because it

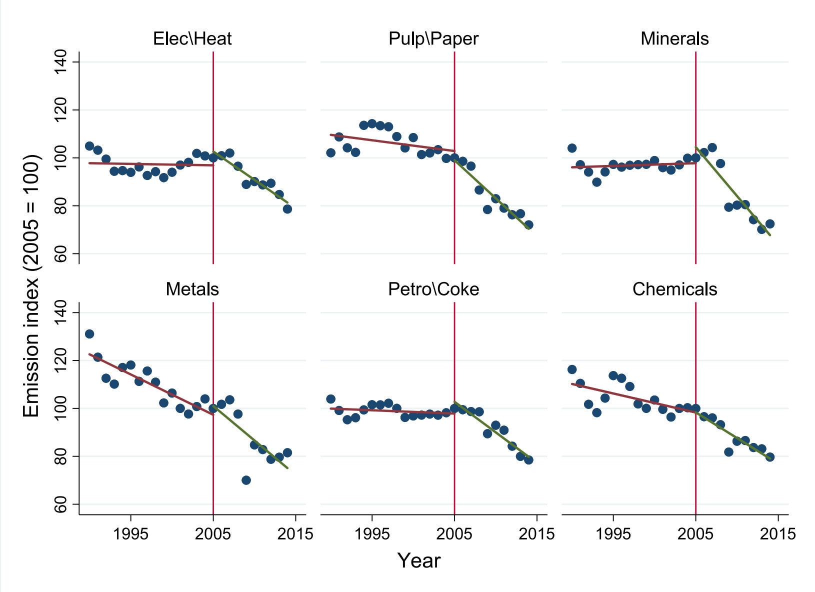

Fig. 4. Emission trends by sectors
Note: Emissions for all EU countries per sector reported to the UNFCCC. Sectors are ordered according to coverage rate by the EU ETS (see Fig. 3). Lines are OLS regressions before and after the introduction of the EU ETS in 2005.

could have been driven by other factors, particularly by the global financial crisis in 2009, which was associated with a massive drop in emissions across all sectors. The installation-level analysis presented in section 5 sheds light on this issue.

3. Previous literature on the impacts of the EU ETS

3.1. Impact on carbon emissions

Martin et al. (2016) review the literature on ex-post evaluation of the EU ETS on emission reductions and distinguish between two different approaches to estimate business-as-usual (BAU) emissions: estimates based on aggregate emissions and estimates based on emission data at the firm or plant level.

Aggregate emissions at the sector level have been used to estimate the counterfactual BAU emissions for the first trading period (2005–2007). Based on different data sources, these studies find EU-wide emission reductions between 2.5% and 5% (Ellerman and Buchner, 2008; Anderson and Di Maria, 2011; Ellerman et al., 2010). For Germany, Ellerman and Feilhauer (2008) estimate emission reductions of 5% and, in the electricity sector in the UK, McGuinness and Ellerman (2008) find emission reductions of 17%. While taking into account observed factors such as GDP, these studies estimate a post-ETS counterfactual by extrapolating pre-ETS emission intensity trends, which makes it difficult to claim causality. More recently, Bayer and Aklin (2020) use a generalized synthetic control approach which allows them to construct a counterfactual emissions path for sectors covered under the EU ETS. They find that EU ETS sectors emitted 11.5% less than they would have in a world without the EU ETS (thus, on top of any emission reductions from reduced economic output induced by the 2007/2008 financial crisis). In addition, their results suggest that the effect of the EU ETS was greater in the second phase (starting in 2008) than in the first phase (2005–2007).

Five studies to date have included observed emissions by both regulated and non-regulated plants, before and after the start of the ETS, to provide causal estimates of the effect of the EU ETS on regulated installations' carbon emissions. The five studies are based on data from a single country, France, Germany, Norway, Lithuania and the UK respectively. Using plant-level data for around 9500 French manufacturing firms, Colmer et al. (2022) show that ETS-regulated manufacturing firms in France reduced emissions by an average of 8–12% compared to a control group of similar but unregulated firms. All of the impact occurs during Phase II, a period during which allowance prices fluctuated between €15 and €30. Petrick and Wagner (2014) use comprehensive panel data from the German production census. They find evidence that phase II of the EU ETS caused regulated plants to reduce their emissions by around 25% compared to non-regulated plants. Klemetsen et al. (2020) exploit plant-level data from the Norwegian Environment Agency for the period 2001 to 2013. They find weak evidence that regulated plants may have reduced emissions by a large amount (−30%) in the EU ETS′ second phase, but no evidence that emission intensity decreased in any of the phases. Next, Jaraite and Di Maria (2016) analyse the impact of the EU ETS on CO2 emissions and economic performance in Lithuania for the period 2005–2010 using plant-level data. They find no reductions in emissions and a slight improvement in emissions intensity in 2006–2007. Finally, Calel (2020) exploits a specific policy feature in the UK, where some firms entered the EU ETS a few years later because they were regulated by pre-existing climate policy. He finds that the EU ETS did not lead to an improvement in firms' carbon intensity between 2005 and 2012. However, the study finds a positive effect on low carbon patenting and R&D.

Compared with the existing literature, this paper is the first multi-country study based on installation-level emission data. It combines emission data from four countries, starting with a larger database than existing single-country studies, spanning the different national contexts. This allows us to provide the first cross-country micro-level evidence and to provide a robust estimate based on a carefully selected set of installation pairs that are most comparable. We also report results using our entire (non-matched) database of 2683 installations.

3.2. *Impact on economic activity*

The introduction of carbon pricing in Europe generated wide concerns about the potential cost burden on industry. Model-based studies predicted that with carbon prices around €20-€30/tCO2, the marginal cost impacts would be small for the large majority of industrial activities, but large impacts could occur in upstream segments within several energy-intensive sectors, including fertilizers, iron and steel, aluminum, paper, basic organic chemicals or coke oven production (Sato et al., 2014).

To provide causal estimates of the effect of the EU ETS on regulated firms' economic performance, the literature has – similarly to analyses of the impact on emissions – relied mostly on methods comparing firms operating installations regulated under the EU ETS with similar firms owning unregulated installations. The main difference is that emissions are typically observed at the installation level while economic performance indicators are observed at the firm level, which makes finding suitable comparators easier.

Early micro-level studies either determined treatment status at the sector level (Commins et al., 2011), incorrectly labelling firms operating unregulated installations as treated, or matched EU ETS firms with firms in non-EU ETS sectors (Abrell et al., 2011), thus failing to control for sector-specific time-varying factors.

In contrast to these earlier studies, Chan et al. (2013) estimate the impact of the EU ETS on economic outcomes by comparing firms regulated under the EU ETS with unregulated firms in the same sector. However, they cover only three sectors (cement, steel and power production) over a relatively short period (2001–2009). They find no impact on economic variables in cement and iron and steel industries, but a significant positive effect on revenue in the electricity sector, which could be explained by cost pass-through.

The four studies mentioned above based on data from France, Germany, Norway or Lithuania also analysed the causal effect of the EU ETS on various economic performance outcomes. Venmans, Ellis and Nachtigall (2020) and Martin et al. (2016) give a detailed overview of this literature. The general picture is as follows. More often than not, empirical studies conclude that the ETS increased investment (Marin et al., 2018; Colmer et al., 2022) and productivity (Marin et al., 2018; D'Arcangelo et al., 2022;; Klemetsen et al., 2020; Lundgren et al., 2015).[9] Regarding turnover and value added, the picture is more mixed. Large increases are found by Klemetsen et al. (2020) in Norway and by Petrick and Wagner (2014) in Germany, while other studies find insignificant results.[10] When considering employment, most studies find an insignificant effect.

Overall, therefore, only a handful of studies have used micro-data and quasi-experimental techniques to analyse the causal effect of the EU ETS on regulated firms' economic (and environmental) performance. However, these studies have mostly looked at different countries individually, two of them with a very small number of covered installations. They have also looked at different outcome variables based on data availability.

The only exception is Marin et al. (2018), who use a similar identification strategy as Chan et al. (2013), but add matching. This means that only the most similar non-ETS firms serve as a comparison to the ETS-firms and that ETS firms without a reasonable comparator are excluded. They investigate the manufacturing sector (representing 32.8% of all ETS emissions) in 19 countries. However, whereas they identify 3445 manufacturing EU ETS firms in ORBIS, accounting for 88% of the EU ETS manufacturing emissions, we were able to identify 8200 EU ETS firms, representing 99% of all emissions. This significantly reduces the risk of using an ETS firm as a control firm. We also include all sectors in all countries participating in the ETS. This results in 1787 ETS firms in our matched sample, which is much larger than Marin et al. (2018)'s matched sample of 759 ETS firms. Our larger sample allows us to test the heterogeneity of results according to company size, region and sector, and to add extra consistency checks such as controlling for country- and sector-specific trends, different treatments of missing variables and different definitions of sectors. Thus, compared with the available literature, our paper offers a more systematic analysis of the impact of the EU ETS on firms' economic and environmental performance on the largest possible sample.

4. Methods

4.1. *Matching*

To establish a causal relationship between the implementation of the EU ETS and the environmental and economic performance of regulated entities, we exploit a special design feature of the EU ETS, namely the sector-specific inclusion thresholds based on production capacity. In order to minimize administrative costs, sector-specific capacity criteria determine which installations are included in the EU ETS and which installations are exempt from the regulation. For instance, only combustion installations with a rated thermal capacity exceeding 20 MW are covered; steel plants are included if their production capacity exceeds 2.5 tons per hour; glass and glass

[9] Commins et al., 2011 find a negative effect on investment and total factor productivity in the EU, while Löschel et al., 2019, find an insignificant result on total factor productivity in Germany.

[10] Colmer et al. (2022) in France, Abrell et al. (2011) for the EU and Anger and Oberndorfer (2008) in Germany.

fiber plants are included only if their melting capacity exceeds 20 tonnes per day, etc. Since inclusion criteria are based on the capacity of the plant and not on the company, multi-plant companies may have a total capacity above these thresholds, while each installation individually stays below the threshold. This design feature makes it possible to (1) compare regulated and unregulated installations and single-plant firms around the inclusion threshold, and (2) compare firms whose individual installations' capacity is below the inclusion threshold (and therefore are unregulated) with firms operating at least one installation above the threshold (therefore regulated), but otherwise similar.[11] With this, we can construct a quasi-experimental design setting which allows for assessing the causal impact of the EU ETS.

Ideally, we would like to compare installations that are similar in all dimensions prior to the implementation of the EU ETS so that it becomes difficult to explain away any difference in the outcome by other factors than the EU ETS. We try to obtain this by matching each ETS entity to similar non-ETS entity. First, we apply exact matching on country and sector at the 3-digit level of the NACE Rev. 2 industry classification.[12] This ensures that matched entities are subject to very similar regulatory environments (other than the EU ETS) and face similar economic environments (demand conditions, input prices, etc.). Next, we also match on continuous variables. Since we have two different datasets, one to assess the effect on emissions and another one to assess economic impacts, we use two different matching strategies.

Our emissions dataset at installation level does not include data on capacity or on actual output; therefore, we use pre-ETS emissions as a proxy for both. More precisely, we match on the mean of pre-ETS emissions as well as on the pre-ETS emission growth rate, in order to account for emissions trends that were already present before the ETS was launched. If a similar installation cannot be found, the observation is excluded (we use a caliper of 0.3 on log-transformed variables, imposing a maximum difference of 35%). We apply full matching, meaning that one treated installation can be matched to many control installations and vice versa.[13] Thus, each matched pair consists either of one ETS installation matched to one or several control installations or of one non-ETS installation matched to one or several treated ones.[14] Using the same installation for more than one matched pair allows for a larger number of matches which, in turn, translates into a larger sample size, thereby increasing efficiency. This is particularly important when the sample size is rather small. The drawback of this procedure is that the average distance between an installation and its matches is larger.

For the economic performance dataset at the firm level, we use data from the pre-regulation period 2002–2004 to assign each EU ETS firm to the closest possible firms in terms of revenue, fixed assets, number of employees and EBIT (earning before interests and taxes, i.e. operating profit or loss). The similitude on these four criteria is based on the Mahalanobis distance, which takes into account the correlation between the criteria. We impose a minimum similitude using a caliper of 0.85. Here also, we restrict ourselves to more closely resembling firms, excluding a number of EU ETS companies for which no good match can be found (for example, we cannot find an electricity production company in France which is of a similar size as EDF, so EDF is left out of the matched sample).

Here, we apply one-to-one matching with replacement, selecting only the single closest non-ETS firm for each ETS firm and allowing that a non-ETS firm can serve more than once. One-to-one matching leads to a higher similitude between treated and control firms (small bias) at the expense of a smaller dataset of unique observations (lower efficiency). This focus on small bias is justified by our large firm-level dataset compared to our installation-level emission dataset.

It is important to note that, as part of the matching procedure across both datasets, we give up a potentially much larger sample in favour of a more focused comparison from which we are able to draw causal estimates of the impact of the EU ETS. What is lost in sample size, however, is regained in terms of accuracy and robustness (Dehejia and Wahba, 1999). This is the main advantage of matching over standard regression. In doing so, our estimates pertain by construction to smaller firms and plants in the EU ETS (which have unregulated comparators) rather than to regulated plants generally. In statistical terms, given that matching with calipers only uses entities in (or close to) the area of common support, our baseline results estimate a local average treatment effect (LATE) with a higher weight placed on smaller companies, rather than the average treatment effect on the treated (ATT). For this reason, we test for heterogenous treatment effects across plant and firm size. We find that larger installations experience relatively larger emission reductions. This means that the Average Treatment Effect on the Treated is likely larger than the Local Average Treatment Effect and that our baseline estimator probably underestimates the true ATT. For our analyses on economic performance, we find relatively small differences between larger and smaller companies.

4.2. Difference-in-differences

After applying matching, we compare the outcomes of ETS and non-ETS entities before and after the implementation of the EU ETS using a difference-in-differences approach. We estimate the causal effect of the EU ETS using the following regression:

$$Y_{it} = \gamma\ ETSpost_{it} + \delta_i + \theta_t \qquad \text{(equation 1)}$$

[11] For emissions, we only have installation-level data, not the firm-level data. Hence, we only use the first identification strategy.

[12] For example, within the sector 'manufacture of fabricated metal products', the three-digit nace classification distinguishes between 'Manufacture of structural metal products' (nace 251), 'Manufacture of tanks, reservoirs and containers of metal' (nace 252), 'Manufacture of steam generators, except central heating hot water boilers' (nace 253), 'Manufacture of weapons and ammunition' (nace 254). This list illustrates how narrowly defined these sectors are.

[13] We apply the command fullmatch from the optmatch package in R provided by Hansen and Fredrickson (2016).

[14] Note that the second case is equivalent to matching with replacement.

where Y_{it} is the outcome variable (installation-level emissions or firm-level turnover, assets, number of employees, EBIT or return on assets); $ETSpost_{it}$ is a dummy variable equal to 1 whenever a company is regulated in a given year. To account for potential pre-treatment differences between ETS and non-ETS entities before the implementation of the EU ETS, the regressions include individual (firm or installation) fixed effects denoted by δ_i. Year dummies denoted by θ_t capture any shock that is common to all installations, such as macroeconomic fluctuations or changes in international energy prices. In the analysis of economic performance, these year effects are both country-specific and sector-specific.[15] Our main results estimate equation (1) using Poisson Pseudo-Maximum Likelihood. In the appendices, we also use Ordinary Least Squares (OLS), in which case we use the log of the dependent variable.

We also compare closures between ETS and non-ETS firms in 2014 (the final year in our sample) using a probit model. We do not use a panel structure in this case, because both ETS firms and control firms are operating in 2005 by design and closure is irreversible in our data.

4.3. Adjusted standard errors

Abadie and Spiess (2022) demonstrate that the standard errors of regression coefficients in matched samples without replacement are consistent provided that they are clustered at the level of the matched sets. The intuition behind this result is that matching on covariates makes regression errors statistically dependent among units in the same matched set. Clustering at the matched set level accounts for this dependency. In our panel setting, this also corrects for serial correlation. However, the result of Abadie and Spiess (2022) only holds for matching without replacement. There is to the best of our knowledge no consistent estimator for standard errors for matching with replacement.[16] However, following Colmer et al. (2022), we reduce the effect of the dependence of errors of repeated observations by using two-way clustering, both on matched sets and on company-years.

As explained above, matching with replacement leads to lower bias in the estimates, because matches will be more alike. By using matching with replacement, we minimize bias of the coefficients, but at the price of higher standard errors.

4.4. 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 entities would have followed parallel trends with matched non-ETS entities.

This assumption could be violated if governments were to develop specific policies targeting non-ETS firms, to compensate for the absence of the ETS regulation. In that case, we would underestimate the effect of the ETS on abatement. Though we cannot exclude this potential bias, we are not aware of large governmental programs specifically targeting non-ETS firms, with the exception of the French carbon tax which was introduced long after the end of our sample period.

It could also be that other policies such as energy efficiency covenants, renewable energy subsidies or command and control regulation, though applicable to all firms, would affect ETS firms in a different way. For example, in our database on economic performance, emissions are unobserved. If ETS firms would have larger emissions, these other policies may have a different effect on ETS firms and our estimated ETS effect would partly be attributable to these other climate policies. However, substantial differences in emissions are unlikely because our matched companies are chosen from the same country, the same sector and have a similar size and investment capacity. To make the case that our matched sample is also similar in unobserved dimensions, we show that ETS and non-ETS firms in our matched sample have a statistically indistinguishable distribution of age (years since incorporation), initial capital (stocks), working capital, cash flows, taxes paid and credit period days. None of these variables was used to construct the matches, suggesting that matching has also achieved balance on unobserved variables, thereby recovering the central identifying condition of a randomized experiment. We also show that trends of our economic variables of interest are statistically indistinguishable before the start of the ETS.[17]

Our analysis on economic performance uses firm level data rather than installation level data. Therefore, a non-ETS firm running two smaller gas power installations may be matched to an ETS firm running a single large combined-cycle gas generator. Both firms would have similar total emissions, revenue, assets, etc., but, since the smaller plants use another technology, they may react differently to a change in the gas/coal price ratio, for example, violating the parallel trend assumption.[18] Since this problem is more likely in the electricity sector, we provide estimates by sector in the appendix. Note that this is not a problem for differences which follow parallel trends, because they are filtered out by our difference-in-differences approach. For example, although carbon efficiency tends to be better in larger plants, this difference will often follow a parallel trend.

[15] Identical results are obtained using equation $Y_{it} = \alpha ETS_i + \beta post_t + \gamma ETS_i{*}post_t + \delta_i + \theta_t$ where ETS_i is a time-invariant dummy variable equal to one for installations or firms that become regulated by the EU ETS in 2005 or later; $post_t$ is a dummy variable equal to one for the post-treatment period (2005 for most firms) and $ETS_i{*}post_t$ is the interaction between these two variables. This specification requires two reference categories for both company and time fixed effects, to avoid perfect collinearity with the fixed effects.

[16] For example, bootstrapping allows for consistent standard errors for matching without replacement (Abadie and Spiess, 2022), but not for matching with replacement (Abadie and Imbens 2008).

[17] Pre-ETS trends in emissions are also statistically indistinguishable, but that is not a placebo test because trends were used in the matching algorithm.

[18] We thank a referee for this meaningful example.

The parellel trends assumption implies absence of anticipation effects. For emissions, anticipation is unlikely given that most Member States allocated free emissions certificates based on historic emissions using some average over a specific period between 1990 and 2002 (Neuhoff et al., 2006), well in advance of the start of the EU ETS.

Though the parallel trends assumption cannot be tested empirically, the fact that our economic outcomes have parallel trends before the start of the ETS is reassuring. In the case of emissions, parallel trends before 2005 is imposed by the matching procedure. This increases the likelihood of parallel trends after 2005.

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.

A potential violation of SUTVA are general equilibrium effects. ETS firms and non-ETS firms operating in the same country and in the same sector may be competitors. A standard example would be that ETS firms pass-through carbon costs in their sales prices, which improves the profitability or market share of non-ETS competitors. Therefore, if SUTVA is violated, our estimates correspond to a relative competitiveness effect, adding up the effects of carbon pricing on ETS firms net of the spill-over effects on non-ETS firms. As a consequence, the absolute competitiveness gains would be even larger than our estimate. General equilibrium effects can push non-ETS emissions in both ways. Higher sales would increase their emissions, whereas green technological spill-overs would decrease their emissions. If the former dominates, the true emission reductions would be more modest than our estimate, whereas if the latter dominates, the true emission reductions would be larger.

Another violation of SUTVA would occur if a firm would have both non-ETS and ETS installations and shift production to its nonregulated entity. To limit this problem, we define an ETS firm as a firm that owns at least one ETS installation and exclude from our non-ETS sample any company that belongs to a group which includes at least one ETS-regulated company (using the Domestic Ultimate Owner in Orbis). In the installation-level analysis, we show as a robustness check that removing ETS or non-ETS installations that belong to the same owner actually *increases* our estimated effect on emissions (Annex C). This indicates that within firm leakage is unlikely.

It remains that many European installations are not directly regulated by the EU ETS, but indirectly through higher electricity prices (as regulated electricity producers pass-through costs onto their customers). Therefore, the results of the analyses have to be interpreted as the effect of being directly regulated by the EU ETS compared to being unregulated or indirectly regulated through carbon price-inclusive electricity prices.

5. Impact on carbon emissions

5.1. Data and descriptive statistics

In our analysis, we make use of data from several national 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 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).[19] 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.

To identify the causal impact of the EU ETS on CO_2 emissions, we need data on emissions for the period before and after the introduction of the EU ETS, and for both regulated installations and unregulated installations which can serve as a control group. The European Union Transaction Log (which reports verified emissions from installations regulated under the EU ETS) does not report information on unregulated installations. Similarly, 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 collected 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 (see Table 1). National PRTRs from these four countries include data for both ETS and non-ETS installations, and all start before the implementation of the EU ETS.

How representative are these four countries of the EU ETS as a whole? Table 2 indicates that installations in the four countries of our analysis only show slightly different patterns relative to the whole population of EUTL installations. Both country groups have similar average and median emissions. The distribution of emissions, number of installations, and average emissions per installation across sectors shows a broadly similar pattern. This suggests that the sample of countries that we focus on might provide a reasonable indication of what the broader impact of the EU ETS across Europe may have been, even if we stress that the validity of our findings cannot strictly speaking be extended beyond the four countries of focus.

Except for France, neither the national PRTRs nor the E-PRTR provide information on whether or not an installation is covered by the EU ETS. Hence, we link national PRTRs with the EUTL to identify which installations in the national PRTRs are ETS-regulated. To do so, we use string matching algorithms based on installations' name, zip code, and address, complemented with extensive manual verification.[20] With this, we are able to identify virtually all ETS-regulated installations in the national PRTRs, with a 99% coverage in

[19] In 2007, the European Pollution Transfer and Release Register (E-PRTR) replaced the European Pollutant Emission Register (EPER) that was enacted in 2001.

[20] We made use of the STATA package 'reclink' for string matching.

Table 1
Characteristics and coverage of national PRTR datasets.

Country	Coverage since	Reporting threshold	# Installations with reported CO_2 emissions	# Installations with reported CO_2 emissions pre and post ETS	# Out of which covered by ETS
France	2003	10 kt	1694	1181	793
Netherlands	1990	<1 kt	1601	775	172
Norway	1997	<1 kt	499	257	111
United Kingdom	1998	10 kt	3295	470	279
Total			7089	2683	1355

Source: National PRTRs from France, Netherlands, Norway and the United Kingdom.

Table 2
Comparison of the four analysed countries with all countries in the EUTL.

Variable	4 countries of analysis	All countries
Verified emissions		
Mean per installation	192,952	179,423
Median installation	16,946	13,369
Share of verified emissions by sector		
Chemicals	4.0%	2.9%
Non-metallic minerals	6.2%	9.0%
Basic metals	11.0%	7.9%
Electricity, gas and steam	52.4%	54.1%
Other	26.4%	26.1%
Share of installations by sector		
Chemicals	5.0%	4.1%
Non-metallic minerals	8.9%	14.5%
Basic metals	1.8%	3.0%
Electricity, gas and steam	14.2%	26.8%
Other	70.2%	51.6%
Mean of verified emissions per installation by sector		
Chemicals	101,419	92,132
Non-metallic minerals	88,333	80,988
Basic metals	789,151	341,051
Electricity, gas and steam	471,820	263,686
Other	47,927	65,633
Median of verified emissions per installation by sector		
Chemicals	30,507	32,441
Non-metallic minerals	24,036	15,911
Basic metals	59,397	47,306
Electricity, gas and steam	23,544	10,797
Other	13,699	11,937

Data source: EUTL. Sectors as defined by the EU EUTL for the period 2005–2012.

terms of emissions.

The EUTL data also allows us to validate the accuracy of self-reported PRTR emissions, at least for regulated installations. We expect the emissions in the EUTL to be accurate because emissions are carefully monitored and verified by third-party auditors and by the regulating authorities. The coefficient of correlation between self-reported emissions in the PRTR and verified emissions in the EUTL is 0.989 based on 8944 installation-year observations, indicating a very good quality of the PRTR data. It is worth noting that installations not regulated by the EU ETS are not subject to the independent verification of emissions as is the case with the EU ETS. Therefore, some measurement error might be present for non-ETS installations, because they should not be expected to devote the same resources as ETS installations to accurately calculate their emissions. However, this would only be a problem for the analysis if non-ETS installations would have an incentive to systematically under-report their emissions after the introduction of the EU ETS, which is unlikely since they do not face any carbon regulation. The presence of measurement error, therefore, could just make our estimates less precise, if anything. To improve the data quality, we however remove installation-year observations that we considered as unrealistic. This includes in particular 'spikes', i.e. emissions of an installation that fall by more than a certain factor from one year to the other, but increase by more than the same factor in the subsequent year or vice versa.[21]

While the Dutch, Norwegian, and UK PRTRs directly report the CO_2 emissions from the combustion of fossil fuels, the French PRTR distinguishes between total CO_2 emissions and CO_2 emissions from biofuels. We take the difference between total and biofuel emissions

[21] We somewhat arbitrarily chose the factor to be the 95% percentile of the distribution of year-on-year changes, equivalent to a value of 3.74, but check robustness of the results to alternative factors. Note that this step does not remove any installation, but only the respective installation-year observations to not further reduce the sample size. Results are not sensitive to removing all installations with unrealistic data patterns.

as the relevant emission value for French installations because the regulation of the EU ETS sets the emission factor of biomass to zero, meaning that installations do not need to surrender any allowances for emissions originating from the combustion of biofuels.[22] Cross-checking with the EUTL data reveals that this difference coincides in over 99% of the cases with the verified emissions in the EUTL data.

We limit the analysis to the first and second EU ETS trading periods because the coverage of the EU ETS broadened substantially in 2013 with the beginning of the third trading phase, leaving only few control observations in some sectors. Our final dataset comprises 2683 installations (1355 regulated installations and 1328 unregulated ones). Simply estimating different specifications of equation (1) using a basic difference-in-differences approach with plant-level fixed effects on the whole sample without matching yields non-significant point estimates between −4% and −7%, with p-values close to conventional significance levels (0.102–0.176, depending on the specification).[23] However, this approach does not take into account that regulated and non-regulated installations are very different from each other due to the design of the EU ETS. Since regulated installations are relatively larger by construction of the ETS, this also translates into higher emission values. Fig. 5 shows the distribution of CO_2 emissions for unregulated and regulated installations before the implementation of the EU ETS. As expected, the average emissions of ETS installations are more than ten times higher than average emissions of non-EU ETS installations. In the following, we explain how we restrict our sample to comparable ETS and non-ETS installations.

5.2. Matching

Our matched sample consists of 240 EU ETS-installations (out of 1355 in the full sample) and 280 non-ETS installations (see Table 3).[24] Table B.2. in the appendix provides more information on the distribution of installations by country and sector. Note that although our sample covers 18% of the ETS installations in France, the Netherlands, Norway and the UK, it covers 2.5% of total regulated emissions, because the matching procedure excludes very large installations without suitable comparators.

Table 4 reports the matching quality in terms of the differences between ETS and non-ETS installations pre-ETS for the matching variables for the matched sample as a whole and for various economic sectors. While ETS installations have slightly higher pre-regulation emissions than non-ETS installations, the difference between treatment and control emissions is not statistically significantly different. This also holds true for single sectors within the matched sample. Comparing the empirical distribution of ETS with non-ETS installations, Fig. 6 indicates that there is no fundamental difference in terms of pre-ETS emissions between both sets of installations.

5.3. Main results

Probably the most intuitive way to assess the impact of the EU ETS is by looking at the trend of emissions for matched ETS and non-ETS installations both before and after the implementation of the EU ETS, as done in Fig. 7. Prior to 2005, ETS and non-ETS installations exhibit similar trends, and over this period, emissions of ETS installations are higher on average than those of non-ETS installations. However, this picture reverses after the implementation of the EU ETS in 2005, when emissions of non-ETS installations quickly start exceeding those of ETS installations.[25]

To estimate the effect of the EU ETS, we estimate Equation (1) above on the matched sample. We use Poisson regressions instead of a log-linear regression, following the approach of Silva and Tenreyro (2006).

Regression results are reported in Table 5. Column (1) includes installation fixed effects, which control for all time-invariant unobserved differences between treated and control installations. Column (2) adds year fixed effects, avoiding omitted variable bias from common shocks to all installations (such as the economic crisis in 2008/2009). Column (3) adds linear country- and sector-specific time trends to control for macroeconomic trends at the national and sectoral levels, such as the impact of increased globalisation or changes in demand for products of specific sectors.

All specifications yield a statistically significant treatment effect of −0.10 to −0.11, meaning that the EU ETS is associated with a reduction of emissions of roughly 10%. This corresponds to roughly 5 kt for the average installation. Reassuringly, estimating a standard linear regression model with installation and year fixed effects also yields a point estimate of around -5kt, though the coefficient is not significant at conventional levels (see Annex B, Table B.1).

Estimating the treatment effect for each year, Fig. 8 suggests that most of the emissions reduction was realised in the second trading phase of the EU ETS. While the treatment effect is systematically negative from 2006, it is highest and statistically significant in the last two years of the second trading phase. Breaking down the estimate by phase, we observe a statistically insignificant emissions reduction of 6% for the first phase and a statistically significant reduction of 15% in the second phase. This is in line with the findings of

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[22] See Annex IV of Directive (2003)/87/EC.

[23] We tried different specifications of equation (1), including equation (1) as such, as well as adding sector-year or country-year trends and sector-year or country-year fixed effects with different sectoral granularities (e.g. two digit or three digit NACE codes).

[24] In the matched sample of the emissions dataset, there are 17 installations that are used more than 3 times as a control (non-ETS) or a treated (ETS) installation. In the 3 most extreme cases, one non-ETS installation was matched to 21, 12, and 9 ETS installations respectively. Restricting the number of replacements to 10, 5 or 3 does not alter the results qualitatively (see Annex C).

[25] Note that Norwegian installations became regulated by the EU ETS from 2008 onwards. However, these installations only constitute a very small part of our matched sample.

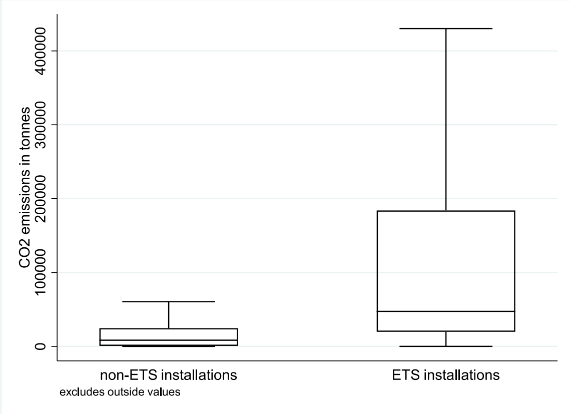

Fig. 5. Distribution of emissions for ETS and non-ETS installations
Note: CO2 emissions data from the four national PRTRs. Boxplots show the 25th, 50th and 75th percentiles, and the whiskers indicate the adjacent value of 1.5 times the interquartile range.

Table 3
Number of installations and observations in the matched sample by country.

Country	# Installations		# Observations	
	ETS	non-ETS	ETS	non-ETS
France	169	185	1353	1457
Netherlands	38	57	190	244
Norway	7	7	84	75
United Kingdom	26	31	305	319
Total	240	280	1932	2097

Note: Full matching with replacement. Exact matching on country and NACE3 sector. Mahalanobis distance matching using the mean of pre-ETS emissions and the pre-ETS emissions growth rate with a maximum distance (caliper) of 0.3. Non-ETS installations that are matched to multiple ETS installations are duplicated (there are 168 unique non-ETS installations). The number of observations refers to the period 1990–2012.

Table 4
Paired t-tests for ETS and non-ETS installations in matched sample.

Sector	Variable	ETS	non-ETS	Difference	p-value
All sectors	pre-ETS emissions	46,966	42,086	4880	0.53
	growth-rate	−0.012	0.003	−0.015	0.91
Food	pre-ETS emissions	16,324	17,139	−815	0.76
	growth-rate	−0.016	−0.001	−0.015	0.53
Chemicals	pre-ETS emissions	85,480	67,194	18,285	0.40
	growth-rate	0.091	0.017	0.074	0.64
Minerals	pre-ETS emissions	35,682	34,585	1097	0.95
	growth-rate	0.032	0.024	0.008	0.98
Electricity, gas and steam	pre-ETS emissions	26,379	26,626	−247	0.97
	growth-rate	−0.090	−0.106	0.016	0.97
Other manufacturing	pre-ETS emissions	45,055	44,140	916	0.92
	growth-rate	0.007	0.006	0.001	0.99

Colmer et al. (2022) and Petrick and Wagner (2014), who also report emissions reductions to be highest for the second phase, in which the permit price almost never fell below 10 euros per ton of CO_2.

Since pollution control is capital-intensive and typically involves high fixed costs, one might expect large installations to be more responsive to carbon pricing. For example, larger installations may be able to spread fixed costs induced by the new environmental regulation over a larger output, thus lowering the cost per unit of production. Thus, we would expect larger installations to have higher treatment effects.

Our dataset allows testing this hypothesis. Since we do not have data on installation size, we use installations' pre-ETS emissions as proxy for size. The larger these emissions, the higher should be the installation size. We split our matched sample into four quartiles and estimate the treatment effect for each quartile. Fig. 9 shows the treatment effect along with the 95% confidence interval of the estimates.

The results show a clear relationship between installation size and the treatment effect. While the largest installations (4th quartile) significantly reduced their emissions compared to their control group, we do not find a statistically significant effect for the medium

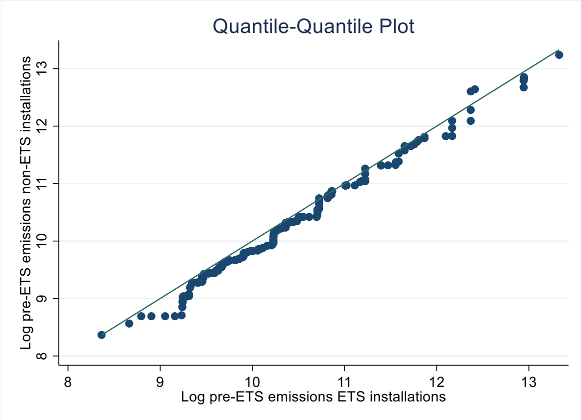

Fig. 6. Comparison of ETS and non-ETS installations in matched sample
Note: Empirical quantile-quantile (e-QQ) plot for pre-ETS emissions on a logarithmic scale. Each dot gives the value for the nth largest emissions of ETS installations and the nth largest emissions of non-EU ETS installations. If both sets of installations have the same probability distribution, the dots are close to the 45° line.

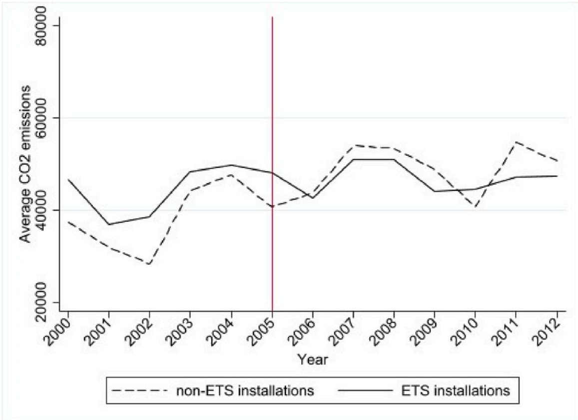

Fig. 7. Emission trend before and after the EU ETS after matching (matched sample)
Note: Average CO_2 emissions reported in the PRTR for ETS and matched non-ETS installations over the period 2000–2012 after implementation of full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Data for 2000–2002 covers only Norway and the UK. The sample includes 240 ETS installations and 280 non-ETS installations in France, the Netherlands, UK and Norway.

Table 5
Effect of the EU ETS on emissions (baseline regressions).

	(1)	(2)	(3)
Dependent Var.	CO2 emissions		
Estimation Method	Poisson		
ETS*Post	−0.10*	−0.10*	−0.11*
	(0.06)	(0.06)	(0.06)
Installation FE	Yes	Yes	Yes
Year FE	No	Yes	Yes
Country and sector trends	No	No	Yes
# Observations	4027	4027	4027
# Installations	520	520	520

Note: Poisson regressions based on equation (1) on the matched sample. The dependent variable is log emissions. Standard errors are shown in parentheses are clustered at both the match and the installation-year level (for repeated control installations). Significance levels are given by: *$p < 0.1$, **$p < 0.05$ and ***$p < 0.01$.

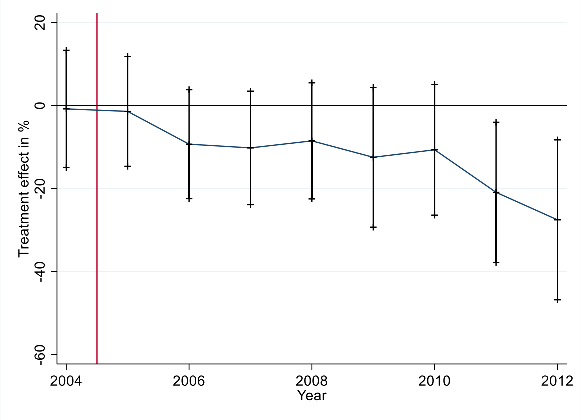

Fig. 8. Treatment effect by year (matched sample)
Note: Poisson regression using equation (1) on the matched sample with installation and year fixed effects and country and sector trends (specification (3) in Table 5). The variable of interest is interacted with each year. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Standard errors are clustered at both the match and the installation-year level (for repeated control installations). Upper and lower bar indicate 95% confidence interval of point estimates.

sized installations (2nd and 3rd quartile) and even find that the 25% smallest installations show a significantly positive treatment effect, meaning that the emissions of ETS-installations have not declined as much as those of the control group after the implementation of the EU ETS. Hence, the overall effect of the EU ETS on emission reduction seems to be predominantly driven by large installations in our matched sample.[26] Given that large installations are underrepresented in our sample, the average treatment effect on the treated is likely to be larger than our estimates in Table 5.

5.4. *Allocation of free emission allowances*

There is an ongoing debate around the neutrality of the allocation mechanism of emission trading schemes in general (Hahn and Stavins, 2011) and the EU ETS in particular (De Vivo and Marin, 2018; Zaklan, 2016). If a company is a permit buyer, any abatement measure will allow the company to buy less permits. If a company is a permit seller, the same abatement measure allows the company to sell more of them. In the former case, the ETS imposes a cash cost, whereas in the latter it is an opportunity cost of the same magnitude. Therefore, we expect the same distribution of abatement choices independently from the allocation mechanism applied. However, this ‘independence property’ might not hold in a real-world trading scheme, because of transaction costs (Stavins, 1995), imperfect competition (Hahn, 1984), and behavioural anomalies such as an endowment effect (Kahneman et al., 1990; Venmans, 2016).

Our dataset allows us to examine whether the magnitude of the treatment effect differs according to the generosity of the allocation of free allowances. As a proxy for generosity, we use the ratio between the average annual amount of freely allocated certificates in the first two phases and the average of pre-ETS emissions.[27] Using pre-ETS emissions instead of actual emissions for the construction of this measure guarantees that the ratio is not confounded by emissions abatement due to the EU ETS. A ratio larger than one indicates that, on average across the two phases, free certificates exceed pre-ETS emissions. We then interact the ratio with the treatment effect variable and estimate our baseline model, including installation and year fixed effects. The coefficient of the interaction term is positive and statistically significant at the 1% level, indicating that the reduction of emissions induced by the EU ETS differs according to the allocation of free allowances. Fig. 10 illustrates this finding by plotting the size of the treatment effect as a function of the ratio of free allowances over pre-ETS emissions. At a ratio of 1.5, the expected effect of the ETS on emissions is zero.

5.5. *Heterogeneity and robustness*

Annex B reports effects by sector and country (Table B.3 and B.4). Most point estimates at the sector level have a negative sign, ranging between −1% and −18%, though most effects are not significant at the sectoral level due to the large within-sector and country

[26] The baseline Poisson regression for all companies in Table 5 gives loosely speaking the % increase of the aggregate emissions of all ETS firms compared to the aggregate emissions of all non-ETS firms. By construction, larger emitters’ reductions have more weight in aggregate emissions (see Annex F.1.).

[27] We also use the last observed pre-ETS emission value instead of the average pre-ETS emissions. While this yields similar results, this metric is potentially subject to a mean-reversion effect to the extent that if emissions in the last pre-ETS year are above average, then reversion to the mean post-treatment could drive the results.

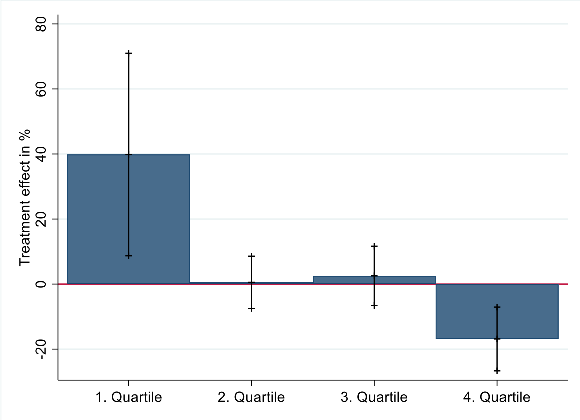

Fig. 9. Treatment effect by installation size (matched sample)
Note: Poisson regression using equation (1) on the matched sample with installation and year fixed effects and country and sector trends (specification (3) in Table 5). The variable of interest is interacted with each quartile based on the mean of pre-ETS emissions. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Standard errors are clustered at both the match and the installation-year level (for repeated control installations). Upper and lower bar indicate 95% confidence interval of point estimates.

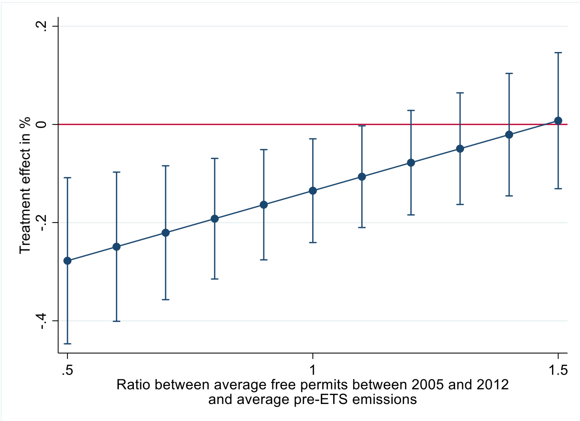

Fig. 10. Relationship between allocation of free allowances and treatment effect (matched sample)
Note: Poisson regression using equation (1) on the matched sample with country, sector and year fixed effects as well as pre-ETS emissions. The variable of interest is interacted with the ratio of average free permits between 2005 and 2012 and average pre-ETS emissions. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Standard errors are clustered at both the match and the installation-year level (for repeated control installations). Upper and lower bars indicate 95% confidence interval of point estimates.

heterogeneity combined with our limited sample size. Reductions in the chemical sector are larger and statistically significant in all specifications.

We ran various robustness checks and the main ones are reported in Annex C. We exclude the 1% largest installations based on their pre-ETS emissions, or exclude the 1% observations with the largest impact (DFBETA) to test whether our results are driven by outliers. We also remove ETS or non-ETS installations that belong to the same mother company to control for potential intra-firm leakage. We include biofuel emissions of biomass, treat missing values in three different ways, use less stringent matching criteria in terms of sector and pre-ETS emissions, use different matching procedures (nearest neighbour) or limit the number of replacements in matching. Reassuringly, nearest neighbour matching, using a larger sample (n=855), although violating the common trend assumption, leads to a negative and significant point estimate of −0.08***.[28] We also estimate the effect of leaving out one country or sector at a time to test

[28] Nearest neighbour matching is based on matching with replacement using exact matching on country and NACE 2 sector, matching on average pre-ETS emissions and emission growth rates and a caliper of 1. We choose the largest caliper for which the difference of the matching variables between the ETS and non-ETS is not statistically significant. A larger caliper (1 instead of 0.3 as in our main specification) results in a larger sample size of nearest neighbour matching (n = 855) which explains the highly significant result.

that results are not driven by a particular country or sector. Overall, our results appear stable and the estimated treatment effects are mostly significant, ranging between 6% and 13% in all but three specifications.[29]

Despite the robustness of our results, our findings should be interpreted carefully. First, we match on pre-ETS emissions levels and emissions growth rate, which produces a parallel trend by construction, but might be vulnerable to bias from regression to the mean.[30] Second, because of the design features of the EU ETS, our results are based on a rather small matched sample of regulated and unregulated installations, which raises questions about external validity. In particular, installations in our matched sample may not be representative of the population of ETS installations at large.[31] Third, the results in our preferred specification, though robust across a number of alternative specifications, are only significant at the 10% level because of the small sample size.

6. Results on economic performance

Having established that the EU ETS has reduced carbon emissions by around 10%, we now ask whether the economic performance of regulated companies was also affected. The analysis is based on the same matching methodology but the main difference is that the economic performance data is observed at the level of companies and not installations. The working dataset is also much larger, as we are not constrained by the reporting thresholds of national pollution registries.

6.1. Data and descriptive statistics

The first step of the data construction consists in identifying companies in Europe that are regulated by the EU ETS, i.e. firms that operate at least one EU ETS-regulated installation. For eight of the countries in our sample, the company registration numbers of the installation operators were obtained directly, either from national emissions trading registries or from the European Union Transactions Log (EUTL), the EU body to which national registries report. For the remaining 23 countries in our data set that participated in the 2005 launch of the EU ETS, a combination of exact and approximate text matching methods were used to establish a link between firm data and regulatory data. This was complemented by further manual searches, and extensive manual double-checking. We also cross-check the quality of our matching with the European University Institute's Ownership Links and Enhanced EUTL Dataset Project, which provides a publicly available link between EU ETS accounts and parent companies.[32]

Our linking between the EUTL and company registration numbers leaves us with over 8200 firms operating more than 12,000 installations regulated under the EU ETS, together accounting for over 99% of EU ETS-wide emissions. We then merge EU ETS firms with Bureau Van Dijk's Orbis database using the company registration numbers. From Orbis, we extract financial information on both EU ETS and non-EU ETS firms on turnover, fixed assets, profits and employment, as well as companies' core activity code at the NACE 4-digit level. Our data covers the period 2002–2014.

We apply a cleaning procedure to the dataset before moving on to the analysis. We exclude firms with missing data before 2005 or missing NACE activity code, and firms operating in sector-country combinations with only EU ETS firms or only non-EU ETS firms. We also carefully check for outlying observations that obviously correspond to errors in the data collection (for example, a company that has 300 employees in one year, 30 the next year and again 300 the year after) and replace those values with missing values. Finally, the financial data set allows us to identify majority ownership. Using this information, we exclude non-EU ETS firms that were owner, sister company, or subsidiary to an EU ETS firm. This reduces the chance of matching two potentially dependent observations.

After implementing this cleaning procedure, we are left with 448,489 firms, of which 4285 are EU ETS firms. Annex D shows the distribution of EU ETS and non-EU ETS firms across countries and sectors.

6.2. Matching

Even though we have a very large pool of firms to start with, sometimes there are no comparators of similar size available within the same country and sector. Therefore, the final matched sample contains 1787 ETS firms and 1280 non-ETS firms.

Table 6 shows that, after matching, the economic outcome variables are not statistically different between the two groups.

The primary challenge for any matching study is to justify the assumption that firms that appear similar are similar in unmeasured dimensions as well—called selection on observables. A simple test of whether matching has achieved balance on unobserved variables

[29] The three specifications include: First, when missing emissions data is replaced by EUTL data, the ETS effect is −16%, but since there are no EUTL data for non-ETS firms, this estimate might be biased. Second, removing the UK which leads to an effect of −17% (and significant at the 1% level). Third, removing the chemicals sector reduces the point estimate to 1% (not significant).

[30] While matching on time-dependent variables is common practice in applied research (Ryan et al., 2015), it is vulnerable to bias from regression to the mean (Daw and Hatfield, 2018). This occurs when matching variables are mean-reverting towards a different mean for ETS and non-ETS firms. This would most likely underestimate the ETS emission reductions. Matching on trends may moderate this bias but can also create overconfidence because the parallel trend assumption is no longer testable (Daw and Hatfield, 2018). By taking several years as baseline, we limit concerns associated with regression to the mean (Barnett et al., 2005).

[31] For example, installations in the electricity, gas and steam sector in our matched sample are mostly district or industrial heating plants. These plants are very different from and certainly not representative for large coal or gas power plants. We thank an anonymous reviewer for pointing this out.

[32] http://fsr.eui.eu/climate/ownership-links-enhanced-eutl-dataset-project/.

is to look at variables that were not used to construct the matches. Table 6 shows that the companies in our matched ETS and non-ETS sample are not statistically distinguishable across six variables not used in the matching procedure: stocks (capital raised by issuing common and preferred stock), cash & equivalents (most liquid assets), working capital (current assets-accounts payable within one year), credit period days (mean duration of accounts payable), taxation and firm age (years since incorporation). This suggests that both sets of firms are similar in other dimensions than the variables used in the matching procedure, as would be the case in a randomized experiment.

In addition, Table 7 formally shows that trends in our variables of interest before the start of the ETS are statistically indistinguishable between ETS and non-ETS control firms (recall that only pre-ETS means were used for matching, not pre-trends). This confirms the validity of the parallel trends assumption before the start of the policy.

Fig. 11 shows QQ-plots, comparing the empirical probability distributions of EU ETS and non-EU ETS firms in our matched sample on the key variables used to construct the match. The empirical distributions of EU ETS and non-EU ETS firms are extremely close to the 45° line, suggesting that the matching has done a good job at creating similar groups of firms.

6.3. Baseline results

The most transparent and intuitive way to view the results is with the aid of a simple graph plotting the revenue, assets, employment and profit of matched EU ETS and non-EU ETS firms both before and after the EU ETS came into effect (see Fig. 12). In all four cases, the EU ETS and non-ETS firms follow parallel trends before the introduction of the EU ETS (up to 2004), and statistical tests confirm this visual observation (Table 7).

We observe that the trends remain roughly parallel for ETS firms and non-ETS firms for all outcome variables. Comparing the two groups of firms, however, the gap between EU ETS and non-EU ETS firms seems to widen for revenue, fixed assets and the number of employees. There is almost no difference in terms of profits in the period following the EU ETS introduction. We now assess whether this apparent widening gap is significant from a statistical point of view.

In order to determine the causal impact of the EU ETS on regulated companies, we run regressions based on equation (1) above. In order to avoid any potential bias created by any remaining pre-treatment differences, our matching procedure is combined with a difference-in-differences approach. This means that we estimate the change in outcome variable between 2003 and 2004 (before the introduction of the EU ETS) and the 2005–2014 period.

Our main results can be found in Table 8 (and in Annex G for a graphical representation). The causal effect of the ETS is captured by the interaction dummy ETS_i*post*. We find that ETS-firm's revenues were 15% higher on average during the ETS period compared to what they would have been had they not been regulated. There are several ways in which the ETS can increase revenues.

First, revenues can increase if companies pass through the opportunity costs of free allowances. At a price of 20€/tCO_2 and 100% cost pass through, the potential carbon opportunity costs as a percentage of gross value added can be substantial for many core sectors in the EU ETS: lime (57%), cement (40%), fertilizers and nitrogen compounds (63%), aluminium (10%), other basic inorganic chemicals (10%), refined and petroleum products (10%), basic iron and steel (8%), paper and board (7%), flat glass (7%) (ETS directive 2009/29/EC).[33] However, given that these are percentages of gross value added and that companies do not pass through 100% of the costs, our estimated increase in revenue exceeds the cost pass-through of free allowances. Note that our revenue variable is defined as operational revenue, which does not include the revenue from selling allowances, because they are considered as financial revenues.

Second, the ETS may have increased product quality and sales prices. Some investments in energy efficiency, which become only profitable when carbon is priced, require new kilns, heat recovery systems and production lines in general. More modern installations may increase product quality.

Third, the EU ETS may increase quantities sold. This could come from the introduction of new products and diversification, or from ETS-induced cost reductions (especially energy costs) which would more than compensate the carbon price and enable firms to lower sales prices and expand market share. Such arguments would be in line with the Porter hypothesis, with the ETS spurring innovation and leading to positive competitiveness effects.

Increases in revenue were much larger in phase 2 and 3 compared to the first phase (Table E.1.), in line with the hypothesis of a gradual innovation and investment process. Our results are similar to Marin et al. (2018) who find that the EU ETS has increased turnover by 15%. In their study, value added increased less than turnover (+6%), suggesting that the EU ETS, while driving up sales, also increased material and other variable costs.

ETS firms also increased their fixed assets by 8% compared to the counterfactual scenario of no EU ETS. The effect is significant at the 10% level only, but is slightly higher and significant at the 1% for the OLS specification. This difference is due to the fact that the larger increases occurred in smaller companies, which are given a lower weight in Poisson regressions compared to OLS (see Annexes E2 and F1). A natural explanation for this impact on fixed assets is that regulated companies reacted to the introduction of the EU ETS by adopting costly emissions-reduction technologies. As we have seen in Section 2, the price of carbon allowances on the European market has been unexpectedly low since 2005. While this could have reduced incentives to invest in emissions-reduction technologies, it is fundamentally *expectations* over future carbon prices which drive technology adoption. Since the overall cap of emissions under the EU ETS is known until 2030, and is planned to decrease every year, companies subject to the regulation should anticipate the future

[33] Note that in the electricity sector, the opportunity cost of allowances in the marginal technology (often gas or coal) will increase the sales price for all producers, including producers with lower carbon costs, such as hydro and nuclear.

Table 6
Paired t-tests for matched EU ETS and non-EU ETS firms.

Matching variable	Mean of EU ETS firms	Mean of non-EU ETS firms	Difference in means	p-value for H0: Difference in means = 0
Revenue (th. EUR)	78,786	76,813	1973	0.21
Fixed assets (th. EUR)	50,851	49,737	1113	0.37
Number of employees	286.6	285.0	1.6	0.72
EBIT (th. EUR)	4571	4311	260	0.32
Placebo tests: variables not used in matching				
Stocks (th. EUR)	7204	7421	−217	0.63
Cash & equivalents (th. EUR)	3947	4515	−569	0.14
Working capital (th. EUR)	11,300	11,390	−89	0.89
Credit Period Days	41.8	41.1	0.7	0.40
Taxation (th. EUR)	1047	1222	−175	0.11
Firm age (years)	37.8	36.9	0.8	0.25

Table 7
Tests on parallel trends before 2005.

Matching variable	Trend of EU ETS firms ($\beta_2 + \gamma$)	Trend of non-EU ETS firms (β_2)	Difference in trends (γ)	p-value for H0: Difference in trends = 0
Revenue	18.3%	18.5%	−0.18%	0.96
Fixed assets	16.3%	15.8%	0.53%	0.88
Number of employees	0.025%	0.047%	0.0022%	0.99
EBIT (th. EUR/year)	1032	937	95	0.79

Note: We run the regression $Y_{it} = \beta_0 + \beta_1 ETS_i + \beta_2 year + \gamma ETS_i{*}year + e_{it}$ for data until 2004 and report the p-value of γ.

tightening of the cap and the implied increase in future carbon prices by investing today in carbon-saving technologies. In fact, it has been demonstrated elsewhere that EU ETS-regulated firms reacted to the introduction of the policy by filing 30% more patents in low-carbon technologies compared to a counterfactual scenario (Calel and Dechezleprêtre, 2016).

For the effect of the ETS on employment, our results are positive but non-significant. Note however that the OLS regressions in the appendix indicate a significant increase in employment by 8%. Again, this difference between the Poisson and OLS regression occurs because increases in employment were larger in smaller companies (Annex E2 and F1).

Finally, we find a non-significant (but positive) effect of the EU ETS on profits (EBIT and return on assets) and a non-significant likelihood of closure.

6.4. *Heterogeneity and robustness*

Table 9 shows results per sector. The first notable result is that no sector has experienced a significant negative impact from the EU ETS. The Chemicals and Refinery sector shows negative competitiveness effects, but these are not significant. The largest positive effect of the EU ETS on revenue is driven by four sectors: minerals, metals, electricity and steam&gas, with a particularly high impact in Metals. Overall, the electricity production seems to have most benefited from the EU ETS, with statistically significant increases in revenue, employment and profit.[34] Although not significant, the 2% increase in return on assets is larger than for the other sectors. The increase in profitability is in line with windfall profits stemming from the combination of cost pass-through and free allocations (see Annex E).

Annex E also shows separate results for phases 1, 2 and 3 and Annex G shows the treatment effect by year. Both the increase in revenues and in fixed assets for ETS firms is more pronounced in the 2nd and 3rd phases. Assets may take time to build up. Our interpretations for the positive effect on revenue are compatible with larger effects in phases 2 and 3: innovation takes time to bring new products to the market and increase market share; the cost pass-through of carbon opportunity costs may be a gradual process in oligopolistic markets; and finally, allocating free allowances in the third phase based on past production volumes creates an incentive to increase production (Böhringer and Lange, 2005).

Annex F further shows results by region and country, indicating that although there are differences, no region was particularly hit by the EU ETS. Finally, we also show results per sector. Sectoral differences are not statistically significant, due to the lack of power of our tests in smaller samples. However, the positive effects for the ETS are larger for sectors deemed at risk of carbon leakage compared to the other sectors.

Annex E shows robustness checks using OLS specifications, changing the set of included fixed effects, treating missing variables differently and defining sectors at NACE4 or NACE2 level instead of NACE3. None of these sensitivity checks finds a negative effect on the economic performance of regulated firms. The most robust and stable impact is on revenues, which are estimated to increase by 13%–19% across specifications. The positive impact on assets is estimated between 3% and 10%. It is always significant in the OLS

[34] Total assets have not increased (or much less) possible because abatement is obtained by a switch in merit order rather than new technology.

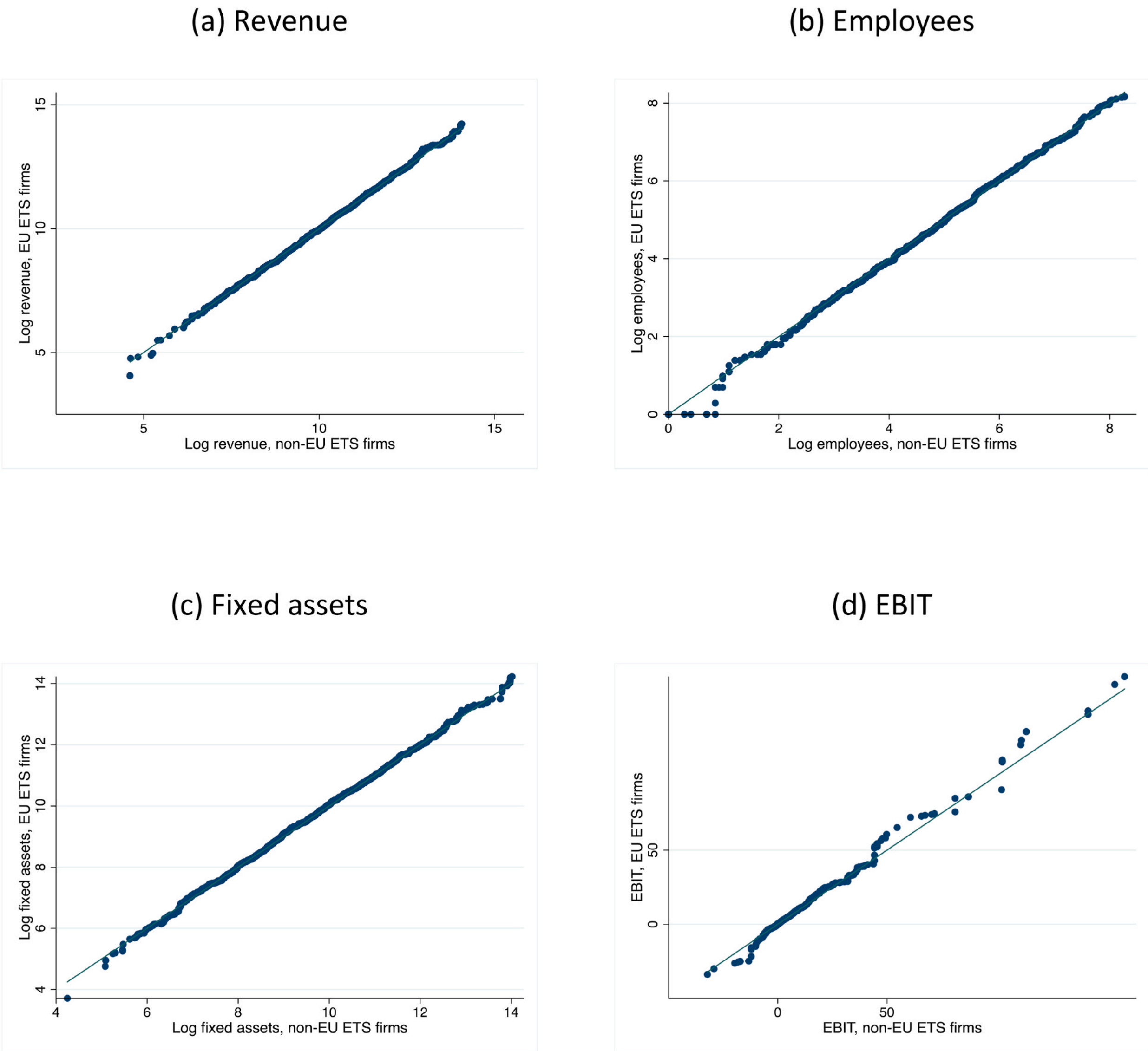

Fig. 11. Comparison of matched ETS and non-ETS firms.

Note: Panel (a) displays the empirical quantile-quantile (e-QQ) plot for revenue on a logarithmic scale in 2002–2004, the 3 years before the EU ETS. Each dot gives the value for nth largest revenue of ETS firms and the nth largest revenue of non-EU ETS firms, shown on logarithmic scales. If both sets of firms have the same probability distribution, the dots are close to the 45° line. Panels (b), (c) and (d) show the e-QQ plots for the number of employees, fixed assets and profit (EBIT). Panels (b) and (c) are shown on logarithmic scales, but not panel (d), as profits can be negative.

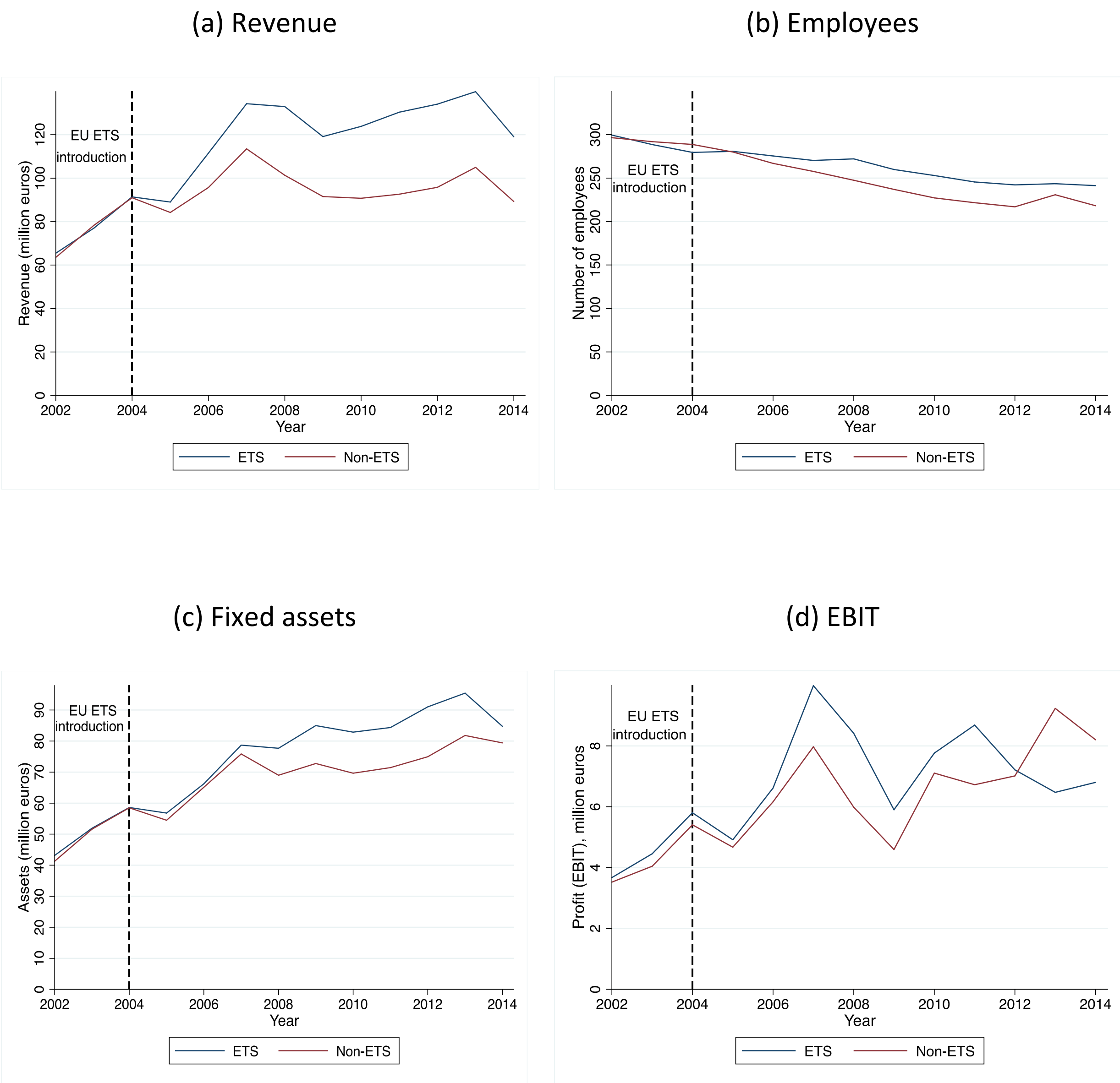

Fig. 12. Revenue, employment, fixed assets and profits for matched ETS firms and non-ETS firms, 2002–2014.
Note: Graphical representation of the difference-in-difference approach. Average outcome variables for the ETS and matched non-ETS firms over the period 2002–2014. One-to-one matching with replacement. Exact matching on country and NACE3 sector, Mahalanobis distance matching using 2002–2004 mean of log revenue, log employment, log fixed assets and EBIT, with caliper 0.85. Sample of 1787 ETS firms and 1280 non-ETS firms in all EU ETS countries.

Table 8
The effect of the EU ETS on revenue, employment, assets and profits.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	revenue	assets	employees	ebit	ROA	closure
Estimator	Poisson	Poisson	Poisson	OLS	OLS	Probit
ETSpost	0.150***	0.082*	0.041	370.428	0.000	−0.108
	(0.034)	(0.038)	(0.023)	(243.068)	(0.005)	(0.085)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	No
Sector-Year F.E.	Yes	Yes	Yes	Yes	Yes	No
Country-Year F.E.	Yes	Yes	Yes	Yes	Yes	No
Sector F.E.	No	No	No	No	No	Yes
Country F.E.	No	No	No	No	No	Yes
# Observations	43,626	43,663	39,993	42,732	41,540	3028
# Pairs	2314	2313	2301	2303	2306	

Note: Regressions using equation (1) on our matched sample. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. The causal effect of the EU ETS corresponds to the coefficient of ETS*Post. Profit (EBIT) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). The probit regression for closure uses 2014 data only and reports the parameter estimate, the marginal effect is a 1.2% decreased likelihood of closure (non-significant, s.e. 1.0%). Sectoral fixed effects are based on 2 digit NACE codes for closures and 3 digit NACE codes for other regressions. Standard errors are clustered at both the match and the company-year level (for repeated control firms). Significance levels are given by: $^{*}p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

Table 9
Effects by sector.

	1	2	3	4	5	6	7	8	9	
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit	
	revenue	log (rev.)	assets	log (assets)	employees	log (empl)	ebit	ROA	closure	#obs
Food & Beverage	0.072	0.084	0.044	0.084	0.015	0.041	−151	−0.015	−0.039	5266
	(0.047)	(0.054)	(0.143)	(0.055)	(0.038)	(0.039)	(593)	(0.014)	(0.215)	
Paper	0.104	−0.044	0.329***	0.065	0.112	−0.047	359	−0.008	−0.029	4211
	(0.114)	(0.107)	(0.097)	(0.099)	(0.093)	(0.066)	(577)	(0.019)	(0.233)	
Chemicals & Refinery	0.078	−0.108	−0.040	−0.015	−0.077	−0.079	−1673	−0.021	0.090	2876
	(0.130)	(0.108)	(0.138)	(0.106)	(0.070)	(0.067)	(1467)	(0.027)	(0.361)	
Non-Metallic Minerals	0.126*	0.213**	0.008	0.158**	0.055	0.069	210	0.006	−0.166	8772
	(0.054)	(0.070)	(0.090)	(0.057)	(0.051)	(0.046)	(358)	(0.014)	(0.153)	
Basic Metals	0.262**	0.575**	0.134	0.081	0.134	0.118	3576	0.015	0.058	1231
	(0.088)	(0.209)	(0.155)	(0.136)	(0.073)	(0.097)	(2218)	(0.042)	(0.595)	
Electricity Production	0.298**	0.277***	0.114	−0.027	0.098	0.183**	2412*	0.022	0.106	2711
	(0.095)	(0.062)	(0.103)	(0.067)	(0.054)	(0.059)	(1037)	(0.012)	(0.337)	
Steam, Gas, Electr services	0.203**	0.309***	0.125	0.181*	0.125*	0.215*	−81	0.012	0.000	9367
	(0.071)	(0.070)	(0.065)	(0.072)	(0.054)	(0.097)	(341)	(0.009)	(.)	
Other Sectors	0.158*	0.234***	0.071	0.072	0.014	0.092	735	−0.005	−0.071	9839
	(0.073)	(0.057)	(0.064)	(0.052)	(0.046)	(0.051)	(596)	(0.011)	(0.162)	
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	
Sector F.E.	No	No	No	No	No	No	No	No	Yes	
Country F.E.	No	No	No	No	No	No	No	No	Yes	
N	43,626	42,656	43,663	42,553	39,993	38,738	42,732	41,540	2741	
#Pairs	2314	2308	2313	2308	2301	2281	2303	2306		

Note: Regressions using equation (1) on our matched sample. *,**,*** = significant at 10%, 5%, 1%. Sector definitions are based on 2 digit NACE codes, except for sector 35 which is split between "Electricity production" (NACE 3510 and 3511) and "Steam, gas and other electricity services". The category "Other sectors" is composed of manufacturing firms that are regulated by the EU ETS because they burn fuel on site. Standard errors are clustered at both the match and the company-year level.

regressions, but only significant in the Poisson regressions in our baseline model and model with country-sector-year effects. The results on employment are smaller and least robust, with an increase between 0% and 8%. The increase in employment in the OLS specifications is larger, between 5 and 8%, and always statistically significant. The EU ETS has had no statistically significant impact on operating profit and closure.

7. Conclusion

This paper explores the joint impact of the European Union Emissions Trading System, Europe's flagship climate change policy, on both carbon emissions and economic performance, based on a combination of macro and micro data collected from various sources.

We investigate the causal impact of the EU ETS on carbon emissions using installation-level data from the national PRTRs of France, the Netherlands, Norway, and the United Kingdom. Applying a matched difference-in-differences study design, we find that the EU ETS is associated with a statistically significant reduction of carbon emissions of around 10%. In line with findings of previous studies, most of the reduction is found in the second trading phase from 2008 to 2012, and is primarily driven by large installations. Among the economic sectors analysed, the chemical sector shows the largest reductions. Our results also suggest that larger amounts of free allowances are associated with a lower treatment effect. Although our results are robust to various sensitivity checks, it should be noted that measuring the causal effects on emissions is much harder than measuring effects on economic outcomes. The starting sample is limited as very few countries report emission data for non-ETS installations. Moreover, it is difficult to find good comparators in several sectors and for large emitters. As a result, our results are based on a relatively small sample of 240 ETS installations and 280 non ETS installations.

We then turn to investigating the impact of the EU ETS on the economic performance of regulated businesses. To do so, we rely on company data and a matched difference-in-differences study design. We compare close to 2000 ETS firms over time with unregulated firms within the same country and sector and with comparable turnover, fixed assets, employment and profit in the 3-year period preceding the introduction of the EU ETS. The EU ETS and non-EU ETS groups of firms experience parallel trends in all of these outcome variables prior to the introduction of the EU ETS, suggesting that the control group offers a valuable counterfactual against which to analyse the causal effect of the EU ETS. We find that the EU ETS led to a statistically significant increase in revenues of regulated firms, as well as to an increase in fixed assets. These results are robust to various sensitivity tests.

Further research is needed to explore the drivers of these findings. At present, we can only observe that they are in line with widely available evidence of cost pass-through of carbon prices in various EU ETS sectors despite generous free allocations, and with previous evidence that the EU ETS induced regulated companies to increase R&D activity in carbon-saving technologies. They are also compatible with the Porter hypothesis, but further analyses will shed light on these issues. We can, however, conclude with a high degree of confidence that the EU ETS seems to have led to some carbon emissions reductions with no negative impacts on the economic performance of regulated firms and thus on the competitiveness of the European industry.

Declaration of competing interest

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

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Annex A. Definition of sectors

Table A.1
Sectors, UNFCCC categories and NACE codes

Sector name	UNFCCC category	NACE code
Electricity and Heat	1.A.1.A	35
Petroleum and Coke	1.A.1.B/1.A.1.C	19
Basic Metals	1.A.2.a/2.C	24
Chemicals	1.A.2.c/2.B	20
Pulp and Paper	1.A.2.d	17
Non-metallic Minerals	1.A.2.f/2.A	23

Annex B. Emissions effects on levels and by sector and by country

Table B.1 reports the effects of the EU ETS on the average installation using a level model.

Table B.1
Effect of the EU ETS using CO2 levels as dependent variable

	(1)	(2)	(3)
Dependent Var.	CO2 emissions	CO2 emissions	CO2 emissions
Estimation Method	OLS	OLS	OLS
ETS*Post	−5571	−5056	−5043
	(3668)	(3507)	(3421)
Installation FE	Yes	Yes	Yes
Year FE	No	Yes	Yes
Country and sector trends	No	No	Yes
# Observations	4027	4027	4027
# Installations	520	520	520

Note: OLS regressions using equation (1) on the matched sample. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Dependent variable is emissions (unlike log emissions in main analysis). Standard errors are shown in parentheses and are clustered at both the match and the installation-year level (for repeated control installations). Significance levels are given by: $^*p < 0.1$, $^{**}p < 0.05$ and $^{***}p < 0.01$.

Table B.2 reports the number of installations per sector and per country. Most installations are found in the chemicals sector, followed by 'Other manufacturing' and 'Electricity, gas and steam'.

Table B 2
Number of installations by country and sector

	Sector				
Country	Manufacturing of food, beverages and tobacco	Manuf. of chemicals, pharma., rubber & plastic	Manufacturing of non-metallic mineral products	Electricity, gas and steam	Other manufacturing
FR	50	76	56	88	84
GB		28	2	12	15
NL	27	38	18		12
NO	4	8			2

Note: Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Non-ETS installations that are matched to multiple ETS installations are duplicated. 168 unique non-ETS installations.

Table B.3. reports the EU ETS impact on emissions by sector. All sectors except manufacturing of food and beverages display a negative treatment effect between −1% and −18%. Because of the small sample size for individual sectors, the coefficient is not statistically significant except for the chemicals sector. Our preferred specification (shown in column 2) suggests that the effect of the EU ETS may have been stronger in the chemicals, non-metallic mineral products and other manufacturing sectors, but the estimated coefficients lack the required precision to state that these effects are statistically greater than in other sectors. To conclude, there may have been heterogeneity of the impact across sectors, but all sectors, with maybe the exception of manufacturing of food, seem to have experienced a decline in their carbon emissions.

Table B.3
Effect of the EU ETS by sector

Dep. Var.: emissions	(1)	(2)	(3)	(4)	(5)
Sector	Treatment effect			# Installations	# Observations
Manufacturing of food, beverages and tobacco	0.03	0.09	0.05	81	515
	(0.09)	(0.09)	(0.07)		
Manuf. of chemicals, pharma., rubber & plastic	−0.16*	−0.17*	−0.18**	150	1239
	(0.08)	(0.09)	(0.09)		
Manufacturing of non-metallic mineral products	−0.04	−0.02	−0.02	76	548
	(0.07)	(0.07)	(0.07)		
Other manufacturing	−0.03	−0.02	−0.02	113	944
	(0.07)	(0.07)	(0.07)		
Electricity, gas and steam	−0.01	0.01	0.01	100	781
	(0.11)	(0.16)	(0.15)		
Estimation Method	Poisson				
Installation FE	Yes	Yes	Yes		
Year FE	No	Yes	Yes		
Country and sector trends	No	No	Yes		

Note: Poisson regressions using equation (1) on the matched sample. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Dependent variable is log emissions. Standard errors are shown in parentheses are clustered at both the match and the installation-year level (for repeated control installations). Significance levels are given by: *p < 0.1, **p < 0.05 and ***p < 0.01.

Next, we investigate the heterogeneity of the impact across the four countries. Table B.4. reports the treatment effect by country. Not surprisingly, the impact in France is very close to our baseline impact reported above. This was expected since France constitutes the majority of our dataset. The effect in the Netherlands is even higher in our preferred specification (column 2). The effect in Norway is even higher, but given the small number of observations in this country we caution against interpreting too much of these results. For the UK, we estimate a positive but non-significant effect for all specifications.

Table B.4
Effect of the EU ETS by country

Dep. Var.: emissions	(1)	(2)	(3)	(4)	(5)
Country	Treatment effect			# Installations	# Observations
France	−0.13*	−0.12**	−0.10**	354	2810
	(0.07)	(0.05)	(0.05)		
Netherlands	−0.08	−0.16*	−0.23**	95	434
	(0.08)	(0.08)	(0.09)		
Norway	−0.51***	−0.44***	−0.40**	14	159
	(0.12)	(0.13)	(0.12)		
United Kingdom	0.02	0.09	0.12	57	624
	(0.08)	(0.06)	(0.08)		
Estimation Method	Poisson				
Installation FE	Yes	Yes	Yes		
Year FE	No	Yes	Yes		
Country and sector trends	No	No	No		

Note: Poisson regressions using equation (1) on the matched sample. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Dependent variable is log emissions. Standard errors are shown in parentheses are clustered at both the match and the installation-year level (for repeated control installations). Significance levels are given by: *p < 0.1, **p < 0.05 and ***p < 0.01.

Annex C. Robustness of emissions effects

We ran various robustness checks regarding the effect of the EU ETS on emissions. The main results are reported in Table C.1. Overall, our results are fairly stable and the estimated treatment effects range between 6% and 13%, but in three specifications.

A first concern is that the results might be driven by a small number of installations which have a large impact on the size of the point estimates. This is particularly relevant in our case because we are matching with replacement, meaning that the weight of an unregulated installation may be high when it serves as match for several regulated installations. We address this concern by removing installations with the 1% largest impact on the point estimate in each direction.[35] The treatment effect increases to −0.13**. A second concern is that our estimates are influenced by installations that have switched from combusting fossil fuels to using biofuels. To address this, we take the reported total CO_2 emissions without subtracting the emissions stemming from biofuels, and find the treatment effect to slightly increase to −0.12*. This shows that the results are not driven purely by a switch toward biomass. We also restrict the estimation to a balanced panel. The point estimate barely changes, although it is not statistically significant anymore because the number of installations is almost halved. Finally, we remove ETS or non-ETS installations that belong to the same mother company. This reduces the sample size to 502 installations. The estimated effect slightly increases to −0.12*.

In some cases regulated installations do not report emissions to the PRTR but do report emissions to the EUTL. In the baseline results, we conservatively only use PRTR emissions, but as a robustness check we exploit this additional information by substituting the emission value from the EUTL whenever it is missing in the PRTR. The estimated emissions reduction increases from 11% to 16%. The reason for the increase is that most of the newly introduced emission values are below the reporting threshold and come from installations that managed to reduce their emissions post-ETS to levels below the PRTR reporting threshold. Substituting these values from the EUTL adds many small values for the treatment group and strengthens the treatment effect. In another robustness check, we substitute the values from the EUTL if and only if the matched control installation has a non-missing emission value in that year.[36] The point estimate increases to 12% for the same reason as above. The PRTR data also enables us to infer some information regarding non-reported emissions value. CO_2 emissions reports might be missing because the installation might have forgotten to submit the CO_2 emissions, because the emissions value is below the reporting threshold or because the installation does not operate anymore. We can exploit PRTR information to detect the last case by looking at all other reported substances of each installation. If an installation stopped to report any substance to the PRTR, then it is likely to have exited the market, in which case we assign an emissions value of zero. Doing this increases the emissions reduction slightly to −0.13*.

Even if our estimate of the impact of the EU ETS on carbon emissions reductions appears fairly robust within our matched sample, a more serious challenge to our conclusion is the small number of observations which might prevent us from extrapolating from this set of installations to all EU ETS installations in the four countries (and, beyond, to all EU ETS installations across Europe). In order to address this issue, we start by relaxing our matching procedure and match exactly on the 2-digit instead of on the 3-digit NACE code. Although one would naturally want to match firms using the finest possible sector definition, we face a difficult trade-off. First, some sectors were almost fully regulated by the EU ETS, as explained in Section 2. Therefore, matching at the NACE 3-digit sector, although appealing because we can in theory better control for sector-specific trends, leaves us with fewer matches than matching at a higher level. Matching at NACE 2-digit level therefore allows to substantially increase our sample size, but at a potential cost in terms of accuracy. Secondly, matching firms using the finest possible sector definition makes it theoretically possible that we match close competitors with each other. Imagine for example that there are two plants in a country that both produce the exact same output (e.g., the same type of cement). Once the EU ETS regulation is passed, cement produced by the EU ETS plant should become more expensive, inducing customers to favour cement from the non-ETS plant. This implies that the policy has also indirectly affected the non-ETS plant. Matching at a higher level reduces the likelihood that two close competitors are matched with each other, but at the cost that the two installations might not face the same demand conditions and thus follow parallel trends.

Matching at NACE 2-digit level increases the matched sample by more than 150 installations while potentially reducing the matching quality (because installations can now be matched to installations operating in different sub-sectors) but reducing the probability of matching competitors together. For example, within the non-metallic minerals sector an installation manufacturing glass can now be matched to an installation that produces cement. Applying this matching procedure reduces the point estimate to −0.07*.

We also check for a less robust matching procedure by allowing matches between installations that have a higher distance in terms of emissions and emissions growth rate, but which are matched exactly on NACE 3-digit and country. Doing this increases the sample size to 855 installations, but comes at the cost that regulated and unregulated installations are not so similar anymore. The treatment effect of this DiD estimator remains at −0.07, but fails to be statistically significant due to the larger heterogeneity of the matched sample. In addition, this procedure fails to produce parallel trends pre-ETS.

We also use nearest neighbour and a simple DiD (without matching). Nearest neighbour matching violates the common trend assumption, which is why the result should be treated with caution. Nearest neighbour matching and DiD without matching produce negative point estimates of −0.08*** and −0.06 respectively while also resulting in a larger sample than our baseline regression.

We check whether our results are driven by a few installations that are matched to many control or treated installations by limiting the number of replacements to 10, 5 and 3. This does hardly affect the point estimates, but further reduces the sample size. Yet, all coefficients remain significant.

Finally, we also check the sensitivity of the results to including particular countries or sectors by excluding one country/sector at a time. The results are fairly stable with the exception of the chemicals sector and removing the UK, both of which is in line with the results reported in Annex B.

[35] Technically, we estimate our specification several times, each time excluding one installation from the matched sample. The impact of each installation on the treatment effect is then the difference between the point estimate of the full sample and the point estimate of the restricted sample that excludes this installation.

[36] If a treated installation is matched to many controls, then we substitute the value from the EUTL if and only if at least half of the matched control installations report a non-missing emissions value.

In sum, our estimate of the impact of the EU ETS on carbon emissions reductions appears fairly robust and is with few exceptions in the range of −6% to −13%. However, it remains that our results cannot easily be generalized to the whole EU ETS beyond the four countries for which data could be collected.

Table C.1
Robustness checks

Robustness check	EU ETS effect	Standard error	# Installations	# Observations
Remove most influential installations	−0.13**	0.05	512	3951
Not subtract emissions from biofuels	−0.12*	0.06	520	4027
Keep only balanced installations	−0.11	0.07	324	2870
Remove installations belonging to the same mother company	−0.12*	0.06	502	3894
Add verified emissions from EUTL	−0.16***	0.06	520	4341
only if matched control is non-missing	−0.12**	0.06	520	4136
Add zero emissions for exiting installations	−0.13*	0.06	520	4206
Matching				
Match on NACE 2-digit code	−0.07*	0.04	673	5393
Less exact matching	−0.07	0.05	855	8778
No Matching	−0.06	0.04	2646	21,064
Nearest Neighbour Matching	−0.08***	0.03	1611	12,439
Limit replacement to 10	−0.11*	0.06	494	3851
Limit replacement to 5	−0.12*	0.06	453	3572
Limit replacement to 3	−0.12*	0.07	410	3242
Removing countries or sectors				
France	−0.07	0.13	166	1217
Netherlands	−0.07	0.05	425	3593
Norway	−0.11*	0.06	506	3868
United Kingdom	−0.17***	0.05	463	3403
Food	−0.12*	0.06	439	3512
Chemicals	−0.01	0.04	368	2788
Minerals	−0.12*	0.07	444	3477
Electricity, gas and steam	−0.12*	0.06	420	3246
Other manufacturing	−0.13*	0.07	409	3085

Note: Poisson regressions using equation (1) on the matched sample with installation and year fixed effects and sector and country trends (specification (3) in Table 5). Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using pre-ETS emissions and pre-ETS emissions growth rate, caliper of 0.3). Dependent variable is log emissions. Standard errors are clustered at both the match and the installation-year level (for repeated control installations). Significance levels are given by: *$p < 0.1$, **$p < 0.05$ and ***$p < 0.01$.

Annex D. Distribution of ETS and non-ETS firms by country and sector

Table D.1
Number of EU ETS and non-EU ETS companies by country before and after matching

Country	Number of EU ETS firms	Number of non-EU ETS firms	Matched EU ETS firms	Matched non-ETS firms
Austria	40	555	1	1
Belgium	182	2842	69	59
Bulgaria	79	2389	10	8
Czech Republic	187	6605	75	58
Denmark	59	3485	23	21
Estonia	22	230	8	7
Finland	114	2270	64	40
France	348	64,808	145	101
Germany	437	12,029	139	103
Greece	77	1990	32	21
Hungary	31	357	10	7
Iceland	1	2	0	0
Ireland	2	2	0	0
Italy	510	98,825	284	246
Latvia	38	735	12	10
Lithuania	40	260	12	11
Luxembourg	2	2	0	0
Netherlands	62	4432	16	15
Norway	58	8629	17	15
Poland	453	6500	265	139
Portugal	19	36	1	1
Romania	168	34,335	54	41
Slovakia	66	675	16	15
Slovenia	59	783	10	10
Spain	689	157,101	410	274
Sweden	205	10,470	93	44
United Kingdom	337	23,857	117	99
Total	4285	444,204	1883	1346

Note: the pre-matched sample is the cleaned sample where we exclude companies without data before 2005, sectors without ETS firms, suspicious outliers and groups with both ETS and non-ETS firms (details in section 6).

Table D.2
Number of EU ETS and non-EU ETS companies by sector before and after matching

Sector	Pre-matching EU ETS firms	Pre-matching non-EU ETS firms	Matched EU ETS firms	Matched non-EU ETS firms
Electricity, gas and steam	410	2446	215	140
Basic Metals	211	3125	56	38
Cement & Lime	123	350	39	27
Ceramics	460	1445	221	113
Chemicals	288	6190	112	90
Glass	156	1596	60	41
Paper	413	3881	182	122
Other Sectors	2224	425,171	998	775
Total	4285	444,204	1883	1346

Note: the pre-matched sample is the cleaned sample where we exclude companies without data before 2005, sectors without ETS firms, suspicious outliers and groups with both ETS and non-ETS firms (details in section 6).

Annex E. The heterogeneity of competitiveness effects

In heterogeneity analysis, we systematically report results for OLS and Poisson regressions. Loosely speaking, the Poisson regression coefficients can be interpreted as the aggregate, overall % increase for the ETS firms in our sample. Larger companies will have a larger effect on the overall increase. By contrast, the OLS results report the mean % increase, i.e. a 1% increase of a small firm has a similar contribution to our estimate as a 1% increase of a large firm[37] (see annex F1).

Effect of the EU ETS by phase

We start by exploring the impact of the EU ETS across its three phases (2005–2007; 2008–2012; 2013–2014). Results are shown in Table E.1. We find that, the impact of the EU ETS on revenue and assets is much larger in Phases 2 and 3 than in Phase 1. This might be surprising, given that the allocation of free allowances was more generous in Phase 1 than in Phases 2 and 3. However, it is important to keep in mind that the "operational revenues" variable we use does not include potential financial revenues from selling allowances on the market. It could, however, be at least partly influenced by cost pass-through of carbon prices on product prices. Effects on employees depend on the type of regression, as in the main specification, but again they are larger during phase 2 and 3. In phase 2, where our test has the largest power, both specification indicate an increase in employment. We do not find significant results on profits, returns nor on assets.

Table E.1
Effects by EU ETS phase

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS
ETS*phase1	0.070*	0.107***	−0.020	0.051*	0.005	0.045*	261.400	0.005
	(0.027)	(0.020)	(0.030)	(0.022)	(0.018)	(0.021)	(242.713)	(0.006)
ETS*phase2	0.189***	0.222***	0.117**	0.112***	0.062*	0.102***	542.199	0.002
	(0.040)	(0.037)	(0.044)	(0.032)	(0.030)	(0.031)	(298.706)	(0.007)
ETS*phase3	0.201***	0.225***	0.158*	0.124**	0.067	0.104*	89.168	−0.014
	(0.051)	(0.055)	(0.064)	(0.045)	(0.045)	(0.047)	(424.569)	(0.009)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	43,626	42,656	43,663	42,553	39,993	38,738	42,732	41,540
# Pairs	2314	2308	2313	2308	2301	2281	2303	2306

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Sectoral fixed effects are based on 3 digit NACE codes.

[37] In fact, to avoid excessive influence of very small companies, we weight the OLS regressions by the $\log\left(\sum_{preETS} Y\right)$.

Effect of the EU ETS by company size

We also explore the heterogeneity of the effect by firm size, using the definition of what constitutes a Small, Medium-sized and Large company according to the European Commission. To avoid reverse causality, we define size before the start of the ETS. Results are reported in Table E.2. We find that the impact on revenue in statistically significant and of similar magnitude for the three types of firms. The impact of the ETS on assets are large for small and medium enterprises, and small (5%–8%) and non-significant for large companies. Regarding employment, the Poisson regressions show that the aggregate effect on employment is larger in small companies. Yet the OLS regressions show that when firms are more equally weighted within size bins, large companies saw the largest increase in employment. Effects on profits, return on assets and closure are non-significant.

Table E.2
Effects by company size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETSpost_Small	0.308**	0.220**	0.303*	0.116*	0.155	0.043	112.069	−0.013	−0.048
	(0.118)	(0.082)	(0.127)	(0.058)	(0.086)	(0.047)	(127.836)	(0.013)	(0.130)
ETSpost_Medium	0.247*	0.228***	0.250**	0.133**	0.084	0.065	358.899	0.015	−0.160
	(0.099)	(0.049)	(0.080)	(0.047)	(0.045)	(0.048)	(193.339)	(0.009)	(0.105)
ETSpost_Large	0.183***	0.221***	0.049	0.076	0.046	0.125**	1068.771	0.006	−0.196
	(0.038)	(0.046)	(0.047)	(0.044)	(0.026)	(0.038)	(650.145)	(0.009)	(.)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector F.E.	No	No	No	No	No	No	No	No	Yes
Country F.E.	No	No	No	No	No	No	No	No	Yes
N	43,626	42,656	43,663	42,553	39,993	38,738	42,732	41,540	3028
# Pairs	2314	2308	2313	2308	2301	2281	2303	2306	

Note: Small, Medium-sized and Large company according to the European Commission's definition. Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

Effect of the EU ETS by geographic region

We classify countries in our dataset into four different regions: West (Austria, Belgium, France, Germany, Netherlands, UK), East (Bulgaria, Czech Republic, Hungary, Poland, Romania, Slovakia), North (Denmark, Estonia, Finland, Latvia, Lithuania, Sweden, Norway) and South (Greece, Italy, Portugal, Slovenia, Spain). We then explore the heterogeneity of the impact of the EU ETS by region. Results are reported in Table E.3.

We find some differences in the impact of the EU ETS across European regions. The general picture is the following: the increase in competitiveness effects from the EU ETS is larger in Eastern Europe. This is in line with larger free allocation in this region. But even in Southern, Northern and Western Europe, most ETS effects are positive. The effect on revenue appears similar in the different regions. The increase in investment in fixed assets is particularly pronounced in Eastern Europe and in the North, but much smaller and not statistically significant in the West and the South. The EU ETS may have led firms in Southern and Eastern regions to increase employment (depending on Poisson or OLS). Finally, the ETS decreased the likelihood of firm closure in the North by 10%.[38] The overall pattern suggests that no region was particularly hit by the regulation.

Table E.3
Effects by region

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETSpost_West	0.158**	0.151*	0.027	0.058	−0.005	0.058	1085.858	0.007	−0.261
	(0.055)	(0.059)	(0.055)	(0.052)	(0.034)	(0.043)	(716.556)	(0.012)	(0.183)
ETSpost_North	0.151*	0.159*	0.297**	0.142*	0.082	0.045	−283.777	−0.023	−0.984**
	(0.073)	(0.069)	(0.114)	(0.062)	(0.077)	(0.063)	(463.364)	(0.012)	(0.408)
ETSpost_South	0.151***	0.173***	0.060	0.030	0.095*	0.040	351.284	0.002	0.080
	(0.043)	(0.045)	(0.068)	(0.036)	(0.047)	(0.027)	(316.693)	(0.009)	(0.111)
ETSpost_East	0.102	0.265***	0.199***	0.230**	0.047	0.187*	−27.781	0.003	−0.257
	(0.073)	(0.062)	(0.055)	(0.074)	(0.044)	(0.078)	(365.761)	(0.011)	(0.201)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector F.E.	No	No	No	No	No	No	No	No	Yes
Region F.E.	No	No	No	No	No	No	No	No	Yes
N	43,626	42,656	43,663	42,553	39,993	38,738	42,732	41,540	3544
# Pairs	2314	2308	2313	2308	2301	2281	2303	2306	

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

Effect of the EU ETS by country

In order to dig deeper into the geographical heterogeneity of the impact of the EU ETS, we also report results by country. The small number of observations in some small countries prevents us from accurately measuring the impact in all countries of the sample. Therefore, we limit the results to those countries where the total number of firms (EU ETS and control firms) is greater than 50. These countries include Belgium, Czech Republic, Finland, France, Germany, Italy, Poland, Romania, Spain, Sweden and the UK. Results are reported in Table E.4. We caution against overinterpreting these results because of the risks associated with multiple testing (the more statistical tests are made, the more likely is it that some erroneous inferences occur, i.e. false positives).

[38] The regression table gives the effect on the z score while the text gives the marginal effect on the likelihood of closure (at the mean).

Table E.4
Effects by country

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
BE	0.149	−0.003	−0.200	−0.040	0.012	0.013	−228.857	0.001	0.503
	(0.152)	(0.144)	(0.164)	(0.160)	(0.071)	(0.096)	(1672.958)	(0.034)	(0.370)
CZ	0.095	0.247	0.068	0.205	0.062	0.062	485.525	0.006	0.287
	(0.218)	(0.135)	(0.136)	(0.166)	(0.089)	(0.115)	(652.748)	(0.019)	(0.437)
DE	0.232***	0.201**	0.041	0.042	0.045	0.122	2570.491	0.025*	0.000
	(0.060)	(0.071)	(0.069)	(0.063)	(0.057)	(0.096)	(1495.211)	(0.012)	(.)
ES	0.099	0.121*	0.144	0.011	0.044	0.035	111.299	−0.002	0.127
	(0.059)	(0.058)	(0.111)	(0.043)	(0.041)	(0.032)	(389.516)	(0.012)	(0.150)
FI	0.177*	0.151	0.302	0.133	0.111	0.026	−92.486	−0.005	0.000
	(0.078)	(0.082)	(0.239)	(0.133)	(0.159)	(0.135)	(663.895)	(0.012)	(.)
FR	0.151	0.152	0.278	0.236*	−0.001	0.046	−607.059	−0.032	−0.246
	(0.140)	(0.152)	(0.174)	(0.111)	(0.067)	(0.070)	(1134.568)	(0.030)	(0.363)
GB	0.185	0.211	0.029	0.033	−0.015	0.021	3067.066*	0.041	−1.150***
	(0.100)	(0.114)	(0.105)	(0.108)	(0.062)	(0.083)	(1513.128)	(0.024)	(0.309)
IT	0.198**	0.219**	−0.045	0.027	0.154	0.066	642.611	0.010	−0.060
	(0.061)	(0.075)	(0.064)	(0.063)	(0.085)	(0.049)	(564.669)	(0.013)	(0.177)
PL	0.107	0.231**	0.245***	0.199*	0.059	0.163	328.751	0.020	−0.726*
	(0.085)	(0.072)	(0.071)	(0.099)	(0.053)	(0.106)	(452.566)	(0.015)	(0.315)
RO	0.124	0.438	0.289	0.549**	0.068	0.454	−700.526	−0.038	0.000
	(0.252)	(0.252)	(0.152)	(0.207)	(0.129)	(0.242)	(1215.941)	(0.030)	(.)
SE	0.265	0.238**	0.388**	0.221**	0.107	0.102	437.027	−0.034*	0.000
	(0.139)	(0.090)	(0.132)	(0.084)	(0.094)	(0.079)	(885.789)	(0.017)	(.)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector FE	No	No	No	No	No	No	No	No	Yes
Country FE	No	No	No	No	No	No	No	No	Yes
N	43,626	42,656	43,663	42,553	39,993	38,738	42,732	41,540	2852
# Pairs	2314	2308	2313	2308	2301	2281	2303	2306	

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

The main result that comes out of this analysis is that no country experiences a statistically significant negative impact on revenue, fixed assets, employment or profits. However, we observe the presence of heterogeneity in the impact of the EU ETS across countries, but this heterogeneity differs across outcomes. The revenue effect appears quite stable across countries, while the reaction in terms of investment differs markedly, from a non-significant negative effect in Belgium to over 50% increase in Romania (although the latter coefficient is not very precisely estimated). Poland, Romania and Sweden stand out, with positive and statistically significant impacts on both revenue and fixed assets. In Poland and Great-Britain, the ETS decreases the likelihood of firm closure.

Effect of the EU ETS by sector

Sectoral results are discussed in the main text (Table 8). Here we give some more background information about the observed increase in profits in the electricity sector, while in other sectors, increases in profit are non-significant (although they are high in the metals sector).

Over 99,8% of emission allowances were allocated for free during the first phase and 97% during the second phase (Schleich et al., 2009). In the meantime the carbon cost is passed through to output prices in particular in the electricity sector which faces little international competition (Fabra and Reguant, 2014; Sijm et al., 2008; Fell et al., 2015; Hintermann, 2016). Since electricity prices are determined by marginal costs of the marginal production technology, often coal or gas, the opportunity costs created by free allowances were also included in electricity prices and led to an increase in firms' profits, a phenomenon that has been labeled "windfall profits" (Wietze et al., 2010; Sijm et al., 2006). Windfall profits are likely to be smaller in other sectors where international competition leads to lower cost pass through.

Effect of the EU ETS on sectors deemed at risk of relocation

An important concern of policy makers is the impact that the EU ETS could have on competitiveness, resulting in job losses. In order to protect industry from potential relocation risks, a list of sectors deemed at risk of carbon leakage was designed by the European Commission, and companies operating in these sectors qualify for free allowances while others progressively have to resort to auctioning. Here we explore both the effects of the ETS and being part of the sectors “at risk”. To do so we create interaction variables between the EU ETS dummy variable, the post-treatment dummy variable and an additional dummy variable equal to one if the company belongs to one of the sectors considered “at risk”. Results are shown in Table E.5.

Table E.5
Effects on sectors deemed at risk of relocation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETS*post*AtRisk	0.269**	0.196**	0.089	0.165*	0.126	0.136	948.079*	0.020	−0.099
	(0.084)	(0.071)	(0.100)	(0.068)	(0.066)	(0.075)	(398.268)	(0.010)	(0.182)
Post*AtRisk	−0.156*	−0.163*	−0.095	−0.233***	−0.118*	−0.115	−946.432*	0.007	0.215
	(0.065)	(0.067)	(0.075)	(0.060)	(0.048)	(0.068)	(317.602)	(0.010)	(0.156)
ETS*post	0.104**	0.134***	0.054	0.051	0.013	0.048*	69.338	−0.006	−0.061
	(0.039)	(0.030)	(0.042)	(0.026)	(0.029)	(0.024)	(298.525)	(0.006)	(0.091)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country FE	No	No	No	No	No	No	No	No	Yes
N	43,697	42,740	43,735	42,639	40,102	38,856	42,824	41,658	3198
# Pairs	2314	2308	2313	2308	2302	2282	2304	2306	

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

The reference category is composed of non-ETS firms, in sectors not deemed at risk.[39] The variable post*AtRisk correspond to the difference between sectors at risk and not at risk for non-ETS firms after 2005. Results show that non-ETS companies operating in sectors at risk of carbon leakage indeed experienced difficulties compared to firms not judged at risk: after 2005, their revenue, assets and employment decreased by 10%–20% and the risk of closure increased. This suggests that sectors considered at risk by the European Commission indeed operate in fiercely competitive markets and faced more difficult economic times after 2005. The coefficients on the third line, relating to the variable ETS*post show the effect of the ETS within the sectors not deemed at risk. It shows that the ETS had a smaller effect in the sectors not deemed at risk (revenues +10%, assets +5% but not significant). The first line shows the interaction effect of both being regulated by the ETS and being in a sector deemed at risk.[40] Interestingly, within sectors at risk, companies regulated under the EU ETS performed relatively better than their non-ETS counterparts, as shown by the positive coefficients on the first results line of Table E.5. This indicates that the distribution of free allowances may have more than compensated EU ETS firms at risk for the induced carbon abatement costs of the regulation.

Annex F. Robustness of competitiveness effects

We explored the robustness of our findings in a number of ways and report the main sensitivity checks we conducted in this appendix.

OLS versus Poisson regression

Table F.1. compares Poisson regressions with OLS regressions where the dependent variable is log-transformed. Loosely speaking, the Poisson regression coefficients can be interpreted as the aggregate, overall % increase for the ETS firms after the introduction of the ETS, compared to what would have happened if they had the same aggregate increase as the non-ETS firms. Since we consider aggregates, larger companies will have a larger effect on the overall increase.[41] By contrast, the OLS results report the mean % increase of

[39] Firm fixed effects capture the difference between ETS and non-ETS and time fixed effects capture the common differences before and after 2005.

[40] The effect of ETS in sectors at risk, compared to non-ETS firms not at risk is the sum of the coefficients on the 3 variables post*ETS*AtRisk, post*AtRisk and post*ETS.

[41] The first order conditions for the maximization of the loglikelihood of the Poisson estimator are $\sum_i (y_i - e^{x_i'\beta})x_i = 0$. Consider a simple regression Y $= \beta_1 + \beta_2 X$, where X is a dummy variable. As a result, $\beta_1 = \ln(\overline{Y}|X = 0); \beta_1 + \beta_2 = \ln(\overline{Y_i}|X = 1)$.

By contrast the first order conditions for the OLS estimator are $\sum_i (lny_i - x_i'\beta)x_i = 0$

For our simple regression lnY $= \beta_1 + \beta_2 X$, where X is a dummy variable, we have $\beta_1 = \overline{ln\, y_i}|X = 0; \beta_1 + \beta_2 = \overline{ln\, y_i}|X = 1$.

the ETS firms, in which small and large companies have the same weight.[42] The Poisson regression has several advantages over OLS. Firstly, it is less sensitive to bias in the presence of heteroscedasticity (Silva and Tenreyro, 2006). Next, it allows us to include values of zero and is less sensitive to small observations (in OLS, a company evolving from 1 to 10 employees has an increase of 900% and is an outlier). Third, from an economic perspective, the aggregate % increase is more important than the average % increase of each separate company. For example, if a large company increases employment by 1% and a small company decreases employment by 1%, the aggregate employment (as reported by Poisson) would increase, while the mean relative change (as reported by OLS) would be zero.

Poisson regressions have also minor disadvantages. Firstly, our data shows overdispersion, meaning that the variance of our outcome is larger than the mean, violating the assumption of a Poisson distribution. This reduces the efficiency of the Poisson estimator, although the estimator remains consistent. Second, the Poisson regression is more sensitive to large outliers, compared to OLS.

Table F.1
Poisson versus OLS regression

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS
ETSpost	0.150***	0.184***	0.082*	0.093***	0.041	0.082**
	(0.034)	(0.029)	(0.038)	(0.027)	(0.023)	(0.026)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Sector-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Sector F.E.	No	No	No	No	No	No
Country F.E.	No	No	No	No	No	No
Year F.E.	No	No	No	No	No	No
N	43,626	42,656	43,663	42,553	39,993	38,738
# Pairs	2314	2308	2313	2308	2301	2281

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. OLS regressions are weighted by $\log\left(\sum_{preETS} Y\right)$. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Sectoral fixed effects are based on 3 digit NACE codes.

Limiting the number of fixed effects

In the baseline results, we control for country- and sector-specific year-fixed-effects. That means that for each sector and each country, we have 14 year-fixed-effects. For example, revenue, assets and profits are expressed in nominal values. These effects can capture country- and sector-specific inflation which could partly affect the magnitude of our point estimates. The matching procedure is designed to control for country- and sector-specific trends, since each control firm is selected from the pool of firms operating in the exact same country and sector as the treated firm. However, the final distribution of matched firms across countries and sectors could affect our point estimates if macroeconomic trends differ across countries and sectors.

The disadvantage of the fixed effects is that they also filter out most of the variation. Therefore we show two approaches where we limit the number of fixed effect. In Table F.2, we replace the 14 year-specific effects for each sector and each country by a single linear trend. In Table F.3., we use only 14 year-fixed effects, which are common to all countries and sectors. Note that our baseline results on closure have already a limited number of fixed effects. This is because whenever a dummy variable designates a group of observations without closure, this group of observations is omitted because the variable would predict non-closure perfectly (the effect on the z-score would be minus infinity).

[42] In fact, in order to avoid excessive influence of very small companies, we weight the OLS regressions by the $\log\left(\sum_{preETS} Y\right)$.

Table F.2
Controlling for country- and sector-specific trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS
ETSpost	0.130***	0.174***	0.067	0.079**	0.035	0.076**	310.081	0.001
	(0.033)	(0.028)	(0.038)	(0.025)	(0.021)	(0.025)	(238.190)	(0.005)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	43,699	42,742	43,735	42,639	40,117	38,868	42,830	41,664
# Pairs	2314	2308	2313	2308	2302	2282	2304	2306

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Sectoral trends are based on 3 digit NACE codes.

Table F.3
Only Firm and year fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETSpost	0.135***	0.163***	0.058	0.071**	0.023	0.064*	283.648	0.000	−0.061
	(0.036)	(0.028)	(0.042)	(0.025)	(0.022)	(0.025)	(238.755)	(0.005)	(0.078)
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
N	43,699	42,742	43,735	42,639	40,117	38,868	42,830	41,664	3766
# Pairs	2314	2308	2313	2308	2302	2282	2304	2306	

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate.

As shown in Table F.2., omitting these additional control variables actually leads to slightly lower point estimates. We find that the EU ETS led firms to increase revenues by 13%–17%, assets by 7%–8% and employment by 3–8%. Table F.3. shows that leaving out sector and country trends altogether, gives again similar results, sometimes 1% lower. By showing similar results with fewer fixed effects, we rule out that our results are only valid after filtering out a large proportion of the variability in the data.

*Controlling for Country*Sector*year fixed effects*

Imagine that in a given country there would be a sector-specific policy, input price shock or demand shock, affecting the outcomes of both ETS and non-ETS firms equally. In such a case we would need a country-sector-specific time-fixed-effect to avoid bias (especially if this country's sector was overrepresented in our sample, for example, there would be many good non-ETS matches by chance). Table F.4. reports results for country-sector specific time fixed effects. Results are very similar, approximately 1 percentage point higher than in the baseline on revenue, assets and employees. The results on the three main variables of interest are now all statistically significant, including the effect on employees for the Poisson regression.

Table F.4
Controlling for country-sector-specific trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS
ETSpost	0.163***	0.192***	0.099*	0.098***	0.066**	0.082**	389.793	0.001
	(0.034)	(0.030)	(0.042)	(0.028)	(0.024)	(0.027)	(242.008)	(0.006)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector*Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	No	No	No	No	No	No
Country FE	No	No	No	No	No	No	No	No
Year F.E.	No	No	No	No	No	No	No	No
N	43,140	42,089	43,197	41,992	39,312	38,011	42,198	40,854
# Pairs	2312	2306	2310	2305	2297	2276	2299	2302

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Sector*Country*Year fixed effects are based on 3 digit NACE codes.

Keeping only firms observed throughout the whole sample period

The baseline results have been obtained by using all possible observations, irrespective of whether some of the data might be missing for some years (for example, a firm might have missing data for the number of employees in a specific year). However, our data suggests that the number of missing values is on average twice as high for non-EU ETS than for EU ETS firms. This could signal that EU ETS firms have better institutional capacity or that they differ from non-EU ETS firms in some unobserved but systematic way. To deal with this issue, we run two tests: first, we apply a procedure to replace any observed value with a missing if that value is missing for the matched firm within each pair. For example, if an EU ETS firm has non-missing employee data in 2010 but its control has missing employee data in that year, we replace the 2010 value for EU ETS firms with a missing. Secondly, we restrict the set of firms to those that are observed throughout the whole sample period for all our variables of interest. This should remove any remaining systematic difference between the treated and the control group.

Results are presented in Table F.5. and Table F.6. Implementing the "pairwise missing replacement" procedure reduces the number of observations by about 10% but leaves the results more or less unchanged. Poisson effects on assets and employees become smaller and non-significant, whereas the OLS estimates remain significant. Restricting the set of firms to those that are observed throughout the whole sample period for all our variables of interest further reduces the sample to around 65% of firms in the baseline sample. Again, the results are very similar in magnitude. The Poisson estimates on assets and employees are larger than in the "pairwise missing replacement" approach (the effect on assets is even larger than in the baseline), but non-significant.

Table F.5
Pairwise missing replacement procedure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS
ETSpost	0.134***	0.164***	0.043	0.080**	0.001	0.060*	325.643	0.003
	(0.037)	(0.030)	(0.043)	(0.028)	(0.025)	(0.028)	(263.126)	(0.006)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	No	No	No	No	No	No
Country FE	No	No	No	No	No	No	No	No
Year F.E.	No	No	No	No	No	No	No	No
N	37,245	36,205	37,418	36,252	33,020	31,593	36,539	35,325
# Pairs	2156	2146	2149	2138	2127	2088	2140	2138

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Sectoral fixed effects are based on 3 digit NACE codes.

Table F.6
Keeping only firms observed throughout the sample period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETSpost	0.181***	0.145***	0.094	0.088**	0.037	0.056	531.222	−0.000	−0.306**
	(0.042)	(0.031)	(0.050)	(0.033)	(0.031)	(0.031)	(310.847)	(0.007)	(0.110)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector FE	No	No	No	No	No	No	No	No	Yes
Country FE	No	No	No	No	No	No	No	No	Yes
N	29,095	28,312	29,181	28,329	26,661	25,740	28,535	27,772	1866
# Pairs	1702	1694	1701	1694	1697	1685	1694	1694	

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

Matching at NACE 2-digit or 4-digit level

The baseline results have been obtained by matching exactly on the EU ETS firms' core sector of activity as reported in Orbis at the NACE 3-digit level. Although one would ideally match firms using the finest possible sector definition, we face a trade-off here as some sectors were almost fully regulated by the EU ETS, as explained in Section 2. Therefore, matching at the NACE 4-digit sector, although appealing because we can then better control for sector-specific trends, leaves us with fewer matches than matching at a higher level. Symmetrically, matching at NACE 2-digit level allows to substantially increase our sample size, but at a potential cost in terms of accuracy.

Therefore, we ran the same analysis as above but matching companies at the NACE 4-digit level or the NACE 2-digit level. Matching at NACE 2-digit leaves us with 4448 unique firms (up from 3766) while matching at NACE 4-digit reduces this to 2984 unique firms. Matching at NACE 4-digit leaves the results virtually unchanged. However, matching at NACE 2-digit reduces the impact on revenue at +10% and the OLS estimate on employment is lower at 3%, and statistically non-significant. At any rate, these robustness checks provide reasonable bounds for the treatment effect.

Table F.7
Matching at NACE 4-digit level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS
ETSpost	0.149***	0.198***	0.028	0.084**	0.036	0.062*	382.854	−0.000
	(0.038)	(0.035)	(0.051)	(0.031)	(0.029)	(0.031)	(230.832)	(0.006)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE	No	No	No	No	No	No	No	No
Country FE	No	No	No	No	No	No	No	No
Year F.E.	No	No	No	No	No	No	No	No
N	34,390	33,514	34,474	33,469	31,550	30,429	33,757	32,693
# Pairs	2154	2148	2152	2147	2143	2122	2140	2143

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE4 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Sectoral fixed effects are based on 3 digit NACE codes.

Table F.8
Matching at NACE 2-digit level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETSpost	0.111***	0.096***	0.023	0.071**	0.024	0.034	441.428	0.001	−0.039
	(0.030)	(0.025)	(0.038)	(0.027)	(0.022)	(0.024)	(252.114)	(0.005)	(0.072)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country*Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector FE	No	No	No	No	No	No	No	No	Yes
Country FE	No	No	No	No	No	No	No	No	Yes
N	51,404	50,308	51,430	50,246	47,349	45,987	50,368	49,094	3589
# Pairs	3417	3412	3416	3412	3405	3380	3405	3410	

Note: Regressions using equation (1) on our matched sample. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE2 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

Limit the number of replacements

Since we apply matching with replacement, in some non-ETS firms that are very similar to ETS firms can serve as a match many time. This reduces bias, because firms are similar, but the repeated non-ETS firms may have a large effect on the results. 78% of non-ETS firms serve only once as a match, but 5% (155 firms) serve more than 3 times. Therefore, below we show results where we omit pairs for which the control firm serves more than 3 times (we randomly keep the first 3 occurrences in the database). Results are shown in Table F.9. and are very similar to our baseline results.

Table F.9
Non-ETS match can serve maximum 3 times

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA	closure
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS	Probit
ETSpost	0.122***	0.163***	0.062	0.078**	0.024	0.072**	310.722	0.003	−0.143
	(0.034)	(0.028)	(0.038)	(0.025)	(0.026)	(0.023)	(239.152)	(0.006)	(0.083)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Country-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Sector F.E.	No	No	No	No	No	No	No	No	Yes
Country F.E.	No	No	No	No	No	No	No	No	Yes
N	39,876	38,988	39,924	38,932	36,446	35,735	39,021	37,979	2688
# Pairs	2248	2242	2248	2243	2217	2226	2237	2241	

Note: Regressions using equation (1) on our matched sample, excluding pairs for which non-ETS firm has been used more than 3 times. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at both the match and the company-year level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Probit regression for closure reports the parameter estimate. Sectoral fixed effects are based on 2 digit NACE code for closure and 3 digit NACE code for other regressions.

Diff-in-diff without matching

In Table F.10., we show results without matching. The results are quite different from the baseline, since we compare companies that are very different from each other to begin with. This is the reason why we apply matching in the main analysis.

For example, consider the results on profits (EBIT). Profits (EBIT) are expressed in thousands of euros and not in %, because they can be both negative and positive. On average, both ETS firms and non-ETS firms have increased profits after the start of the ETS, but the same relative increase in profits, will lead to a larger absolute increase in the larger firms, which are more often in the EU ETS. The positive effect in our regression is therefore likely to be driven by mere size.

Regarding employment, the results show a decreasing effect on employment in the Poisson regression (no effect in the OLS regression). Although employment is considered in relative terms, employment trends can still be different for small and large companies. Both ETS firms and non-ETS firms have reduced employment over the period. But since employment trends can be affected by both size and the ETS, it is unclear if this larger reduction in ETS firms is due to their size or to the ETS. Therefore, we do not include the results without matching when we summarize the robustness in the main text.

Table F.10
No matching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable	revenue	log (rev.)	assets	log (assets)	employees	log (empl.)	ebit	ROA
Estimator	Poisson	OLS	Poisson	OLS	Poisson	OLS	OLS	OLS
ETSpost	0.021*	0.084***	−0.009	0.030***	−0.031***	−0.006	228.695***	0.016*
	(0.009)	(0.007)	(0.012)	(0.007)	(0.007)	(0.005)	(28.880)	(0.007)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,326,241	1,318,642	1,353,577	1,331,082	1,142,916	1,123,082	1,282,699	1,254,835
# Pairs	148,526	148,444	149,031	148,597	145,548	145,133	145,403	144,676

Note: Regressions using equation (1) and all firms in the sample, without matching. *,**,** = significant at 10%, 5%, 1%. Standard errors are clustered at the firm level. Nearest neighbour matching with replacement. Exact matching on country and NACE3 sector and Mahalanobis distance matching on the 2002–2004 mean of log revenue, log assets, log employees and EBIT with caliper 0.85. Profit (ebit) is expressed in thousands of euros. ROA is Return on Assets (EBIT/Total Assets). Sectoral fixed effects are based on 3 digit NACE codes.

Annex G. Treatment effect by year for competitiveness effects

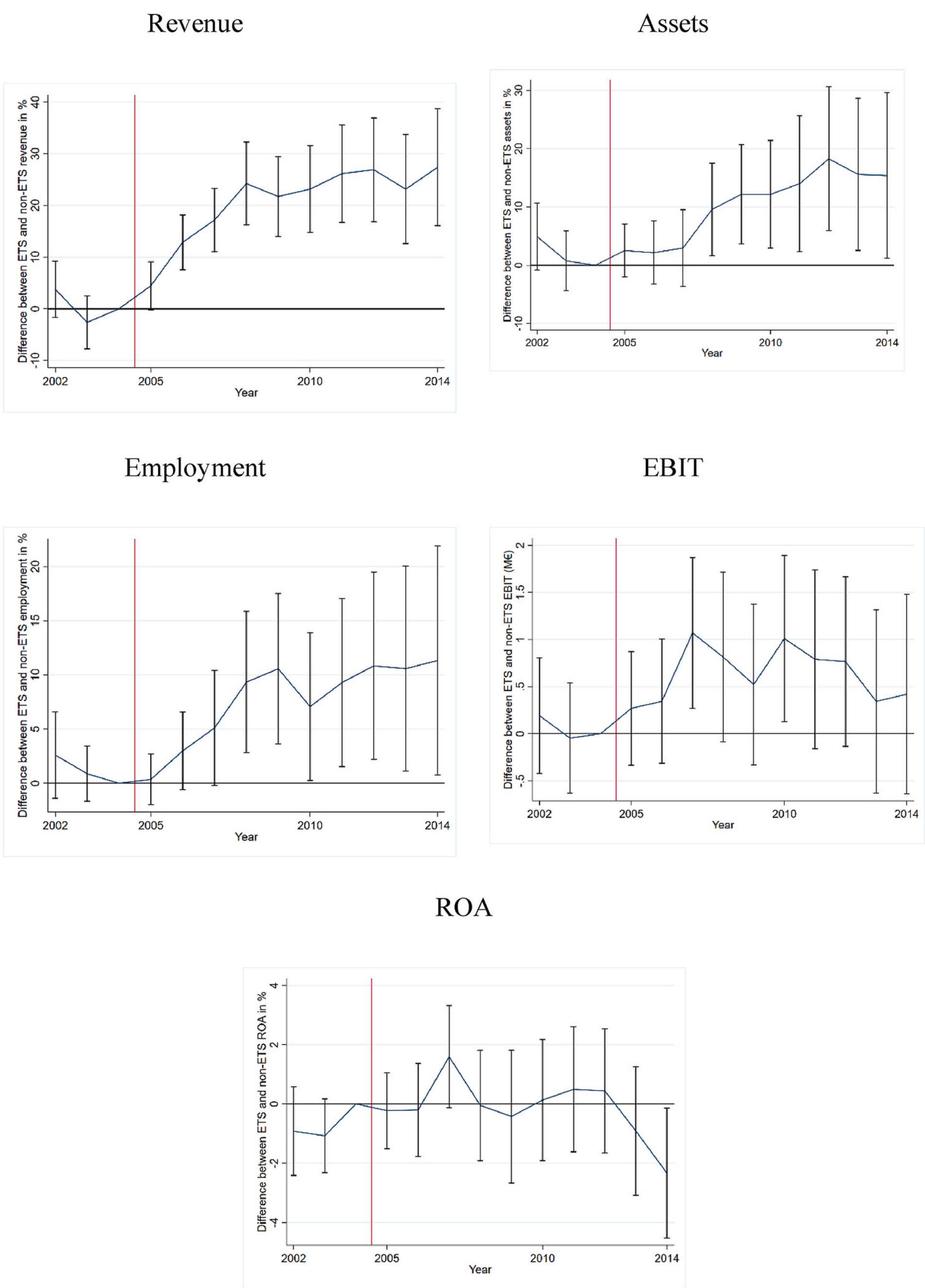

Fig. 13. Difference between the ETS and non-ETS firms by year for revenue, employment, fixed assets, profits and return on assets in matched sample.

Note: Regression using equation (1) on the matched sample with firm, installation-year and sector-year fixed effects (as in Table 7). The ETS variable is interacted with each year (except for 2004). Poisson regression for revenue, assets and employment, weighted OLS for EBIT and ROA. Full matching with replacement (exact matching on country and NACE3 sector, Mahalanobis distance matching using 2002–2004 mean of log revenue, log employment, log fixed assets and EBIT, with caliper 0.85. Sample of 1787 ETS firms and 1280 non-ETS firms in all EU ETS countries. Standard errors are clustered at both the match and the installation-year level (for repeated control installations). Upper and lower bar indicate 95% confidence interval of point estimates.

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