The competitiveness effects of the European Union Emissions Trading System*

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Abstract

This paper investigates the impact of the European Union Emissions Trading System (EU ETS) on the competitiveness of regulated companies, exploiting installations-level inclusion criteria to estimate the System's causal impact on firms' revenue, assets and employment. Comparing 2381 ETS firms with their closest match within the same sector and country, we find that the EU ETS has had no impact on the competitiveness of firms. This result also holds when every year is considered seperately, including those years with a carbon price above 25 euros and for companies in leakage-exposed sectors.

1 Introduction

Emissions trading programs have assumed an ever more prominent role in environmental policy over the last few decades. In the US, the Acid Rain Program, the Regional Greenhouse Gas Initiative (RGGI), and California's cap-and-trade program are all examples of this trend. New Zealand and Quebec have all recently created their own cap-and-trade programs to regulate greenhouse gas emissions. China has initiated several pilot programs in anticipation of a national market that will be launched in 2016. Japan, South Korea, Brazil, Mexico, and Chile are individually making moves toward launching their own. Global carbon markets are worth over $175 billion a year according to recent figures (Kossoy and Guigon, 2012), and cover over 20% of global greenhouse gas emissions (Kossoy et al., 2013). With so many new initiatives in the works, these numbers will grow much larger in years to come. China's national carbon market, for example, is expected to cover between 3 and 4 billion tons of CO2 annually - around 10% of the world's emissions in 2014.

At present, the European Union Emissions Trading Scheme (EU ETS) is the largest cap-and-trade program in the world. The EU ETS was launched in 2005, allocating tradable emissions permits to over 12'000 power stations and industrial plants in 24 countries, accounting for over 40% of the EU's total greenhouse gas emissions. 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 the introduction of the Scheme, there have been concerns about its potential competitiveness impacts on regulated businesses. Indeed, economists traditionally think environmental regulations add costs to companies and divert resources

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away from productive activities, thereby slowing down productivity. 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 to their foreign competitors. As a consequence, European businesses may move manufacturing capacity to countries with relatively laxer policies, as predicted by the pollution haven hypothesis. An alternative view, articulated by Michael 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 environmentally-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 recent context of economic downturn. 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). Recent empirical evidence shows that the EU ETS has increased innovation activity in low-carbon technologies among regulated entities by as much as 30% compared to a conterfactual scenario, but this does not imply that the competitiveness of regulated companies has consequently improved.

In this paper we conduct the first comprehensive investigation of the impact of the EU ETS on competitiveness in the first two phases of the System's existence, from 2005 to 2012. The EU ETS offers a unique opportunity to investigate the impact of environmental policy on competitiveness. It is the first and largest environmental policy initiative of its kind anywhere in the world, which by itself would make it an interesting case to study. But more important is the fact that, in order to control administrative costs, the EU ETS was designed to cover only large installations. Firms operating smaller installations are not covered by EU ETS regulations, although the firms themselves might be just as large as those affected by the regulations[1]. We can thus exploit these installation-level inclusion criteria to compare firms operating in the same country and sector, of similar age and similar size (in terms of annual revenue and employment), but which have fallen under different regulatory regimes since 2005. This provides an opportunity to apply the sort of quasi-experimental techniques most suited to assessing the causal impacts of environmental policies (List et al., 2003; Greenstone and Gayer, 2009). Studies employing these methods have found that environmental regulations inhibit new-plant formation (List et al., 2003), but stimulate capital investment in existing plants (Fowlie, 2010).

We use a newly constructed data set that records key firm characteristics, including sector of activity, revenue, assets, number of employees and year of incorporation, and regulatory status with respect to the EU ETS. Our data set includes information on over 30 million firms across 30 countries. We identify over 8,200 firms operating more than 14,000 installations regulated under the EU ETS, accounting for over 99% of EU ETS-wide emissions. Using this data set, we are able to compare unregulated and would-be regulated firms both before and after the EU ETS launched. A matched difference-in-differences study design enables us to control for confounding factors that affect both regulated and unregulated firms (input prices, sector- and country-specific policies, etc.), as well as firm-level heterogeneity (Heckman et al., 1998b,a; Smith and Todd, 2005; Abadie, 2005). Our estimates provide the first comprehensive empirical assessment of the impact of the EU ETS on competitiveness.

We find that the EU ETS did not have any statistically significant effect on firms' revenue, employment or assets. This result holds in every single year of the policy, including those years with a carbon price above 25 euros per tonne of CO2 and for companies in leakage-exposed sectors. This shows that he EU ETS has had no impact on the competitiveness of regulated firms so far. While our results do not rule out the possibility that the EU ETS had some negative effects on some individual companies, they suggest that concerns over the competitiveness impacts of the EU ETS before its introduction might have been overplayed.

[1] Although the EU ETS regulations are applied at the level of the installation, we will often use 'EU ETS firms' or 'regulated firms' as shorthand for firms operating at least one EU ETS regulated installation.

The paper contributes to the growing empirical literature on the impacts of environmental policies, and of climate change policies in particular, on the competitiveness of affected firms. Overall, this literature shows that environmental regulations 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 small and temporary relative to general trends in production (for a review of this literature see Dechezleprêtre and Sato, forthcoming). Four studies to date have used firm-level data to analyse the joint effect of the EU ETS on regulated firms' and installations' environmental and economic performance, respectively in France, Germany, Norway and Lithuania. Using plant-level data for around 9,500 French manufacturing firms, Wagner et al. (2012) show that ETS-regulated manufacturing plants in France reduced emissions by an average of 15% and do not find any statistically significant impact on employment, suggesting that the EU ETS was effective at reducing carbon emissions of regulated plants with no statistically significant effect on domestic jobs. Petrick and Wagner (2014) analyse the causal impact of the EU ETS on German manufacturing firms using comprehensive panel data from the German production census. They find robust evidence that phase II of the EU ETS caused treated firms to reduce their emissions by around 25% compared to untreated firms, and find no statistically significant effects of the EU ETS on employment. Klemetsen et al (2016) examine the impacts of the EU ETS on the environmental and economic performance of Norwegian plants using plant-level data from the Norwegian Environment Agency for the period 2001 to 2013. They find weak evidence that regulated plants 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. They consider two measures of economic performance: value added at factor prices, which is the plant's annual gross production value minus the cost of intermediates plus subsidies and minus taxes (except VAT), and labor productivity, defined as value added at factor prices per man hour. For phase II, the estimated effects on both value added and productivity are positive and significant, and suggest increases of around 25%. These surprising effects could come from the impact that free allowances or cost pass-through may have had on value added. Finally, 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. They find no significant impacts of the EU ETS on Lithuanian firms' profitability.

The paper proceeds as follows. Section 2 surveys the evidence on the impact of environmental policy on competitiveness, especially in the context of emissions trading. Evidence from the US Acid Rain Program and early studies of the EU ETS inform us about how the EU ETS is likely to have impacted competitiveness. In section 3 we present some background information on the EU ETS and start exploring the data set. In section 4 we estimate the impact of the EU ETS on regulated firms. Section 5 summarizes and discusses the evidence in light of the broader empirical literature. We conclude by considering some of the potential policy implications of our findings, and directions for future research.

2 Previous literature

2.1 Environmental regulation and competitiveness

Ever since the first major environmental regulations were enacted in the 1970s, there have been concerns about their potential impacts on businesses. Managing the balance between environmental constraints and economic impacts has been an ongoing dilemma. The recent economic downturn, combined with increased competition from emerging economies, has made the debate even more acute, particularly in relation to climate change policies. The growing importance of this debate in policy circles has led to a large number of studies that attempt to quantify the impact of environmental regulations on businesses. These studies have analysed many aspects of the economic performance of regulated businesses, including productivity, innovation, employment, profitability, output and trade. Many studies focus on the impact of the US's Clean Air Act Amendments. Overall, the literature is not very conclusive. There seem to exist evidence for short-run negative impacts of environmental regulation on productivity. For example, Greenstone et al. (2012) use detailed production data from nearly 1.2 million plant observations from the 1972-1993 Annual Survey of Manufactures they investigate the economic costs of federal air quality regulations (the 1970 Clean Air Act). They use nonattainment designation, which affect counties whose concentrations of pollutants exceed the federal standard, as a measure of regulation. The number of observations allows controlling for many confounding factors that may affect both productivity and regulation. They find that total factor productivity declines by 4.8 percent for polluting plants in strictly regulated (nonattainment) counties compared to weakly or unregulated (attainment) counties in the year in which firms are required to take costly abatement actions, and the effect seems to persist through time. However, this result is not consistent across pollutants and industries. Albrizio et al 2014 conduct a multi-level analysis based on a dataset covering up to 23 OECD countries, 22 manufacturing sectors and 60,000 companies over 21 years. They find no evidence of permanent effects of environmental policy tightening on multifactor productivity growth (MFP). An increase in stringency of environmental policies does not harm productivity growth or productivity levels – neither from the perspective of the entire economy nor from that of manufacturing industries. In fact, a tightening in environmental policy stringency is associated with a subsequent short-run increase in productivity growth, which translates into permanently higher MFP levels. All effects tend to fade away within less than five years. Albrizio et al (2014) also find that the most productive industries and firms see the largest gains in productivity levels, while less productive firms see negative effects, possibly because highly productive firms, often the largest firms in the industry, may be best suited to profit rapidly from changing conditions – seizing new market opportunities, rapidly deploying new technologies or reaping previously overseen efficiency gains.

Studies looking at employment are not more conclusive than the ones on productivity. Kahn (1997) finds 10% lower growth rates in manufacturing employment in counties with stringent air pollution regulations compared to less regulated counties. Using the same approach and a long panel of United States plant level data (1972-1987), Greenstone (2002) finds that the Clean Air Act Amendments of the 1970s led to a loss of around 590,000 jobs in (strictly regulated) nonattainment counties relative to attainment ones (subject to more lenient regulation). This represents 3.4 percent of manufacturing employment in the United States and less than 0.5 percent of total employment. Part of this lost activity in nonattainment counties may have moved to attainment counties, so that the net national effect on employment is likely to be smaller. Moreover, a range of studies, including some that also focus on the US Clean Air Act, do not find evidence for such negative impacts of environmental regulation on employment. Morgenstern et al. (2002) use pollution abatement operating costs as a proxy for the stringency of environmental regulation and find that higher environmental spending generally does not cause a statistically significant change in employment. There are even statistically significant and positive effects in two industries, but total number of affected jobs remains quite small. These estimates suggest that, at most, environmental regulation accounted for 2 percent of the observed decline in employment from 1984 to 1994. Belova et al. (2013) also use pollution abatement operating costs as a measure of environmental regulatory stringency and find no evidence of negative employment effects from environmental regulations. Berman and Bui (2001b) compare petroleum refineries in the Los Angeles area, subject to some of the strictest air pollution regulations in the United States, to all other refineries in the country. They find no evidence that environmental regulation decreased labour demand, even allowing for induced plant exit and dissuaded plant entry. They actually find weak evidence that regulations may have resulted in a small net increase in employment, possibly because more labour is required for pollution control activities. The lower bound of their estimates implies fewer than 3,500 jobs lost due to regulation over 12 years, a number equivalent to the estimated deaths every year from pollution in counties not complying with national standards in the mid-1980s. Cole and Elliott (2007) estimate a similar model to Berman and Bui (2001) but use data for 1999-2003 on 27 industries in the United Kingdom. and found no evidence that environmental regulations reduce employment. Ferris et al. (2014) examine the employment effects of Phase I of the Title IV cap-and-trade program for SO2 emissions implemented under the 1990 Clean Air Act Amendments (CAAAs). Using a panel data set that includes 61 regulated and 109 unregulated plants, they examine the impact of environmental regulation on employment using propensity score matching followed by difference-in-differences estimation. They find little evidence that power plants subject to Phase I of the SO2 trading program experienced significant decreases in employment relative to non–Phase I power plants. employment is significantly lower in Phase I plants relative to non–Phase I plants in the first year of compliance but not in subsequent years. However, this results is not robust to aggregating the data at the utility level, suggesting firms might be simply relocating employees between plants. A few recent studies have looked at the impact of energy prices on employment, making it possible to examine the impact that a hypothetical carbon tax would have. Deschênes (2010) finds that employment rates are weakly related to electricity prices, a 1 percent increase in electricity prices leading to a change in full-time equivalent employment ranging from -0.16 percent to -0.10 percent. Aldy and Pizer (2011) also exploit the United States state-level variation in industry energy prices between 1990 and 2009 to estimate the price-employment relationship. They simulate the impact of a $15 per ton carbon tax corresponding to an 8 percent increase in electricity prices in the United States relative to the rest of the world and find that this would cut employment by 0.2 percent. Kahn and Mansur (2013) exploit variation in energy prices and in environmental regulation among adjacent counties and use a relatively long panel (1998-2009). They find evidence that energy intensive sectors locate in low electricity-price areas and that polluting sectors seek out low regulation areas, reducing employment in high regulation areas. The effects are modest for the typical manufacturing industry, but the most electricity-intensive industry, primary metals, has an implied price elasticity of employment of -1.65. The effect of a $15 per ton carbon tax would vary according to the carbon intensity of electricity production and to the energy intensity of the industry across states. For example employment would fall by 3.8 percent in Ohio compared to a mere 0.3 percent in California.

2.2 Studies on the EU ETS

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, aluminium, paper, basic organic chemicals or coke oven production (Sato et al., 2014). However, evidence suggests that most sectors did not see high cost increases due to a combination of generous free allocation and low carbon prices. In the electricity sector, where marginal costs were affected, high levels of carbon cost pass-through were observed, as theory would predict. Chan et al. (2013) compare 5,873 regulated and non-regulated firms between 2001 and 2009 across 10 European Union countries in the power, cement and iron and steel sectors. In the power sector, regulated firms on average experienced an increase in 'material costs' (including fuel) by 5 percent and 8 percent during Phase I and II of the European Union Emissions Trading System (EU ETS). This may be due also to the European Union renewable energy target. However, no such effects are found for the cement and steel sectors, because emissions trading permits were largely allocated to these sectors for free during this period.

A number of recent studies have examined the impact of the EU ETS on employment, and there is no evidence that the EU ETS might have negatively affected the economic performance of regulated firms (Martin et al., 2012). Anger and Oberndorfer (2008) compare EU ETS firms with each other, using the allocation factor (the ratio between allowances allocated for free and verified emissions) as an indicator of the stringency of the regulation at the firm level. They find no evidence of an impact of the allocation of EU emissions allowances on firm employment. Simiarly, Commins et al. (2011) do not find a statistically significant effect of the EU ETS on employment, however as mentioned above their definition of EU ETS regulation is at the sector level, and hence all small unregulated installations are wrongly considered as treated. Abrell et al (2011) use a better methodology. They estimate the impact of the EU ETS on regulated firms by matching each EU ETS firm with a similar firm - based on observable firm characteristics - in a non-EU ETS sector. In the period between 2004 and 2008, they find a statistically significant, slight decrease in employment at EU ETS firms of 0.9%. This result is driven by the non-metallic minerals sector. However, as the authors acknowledge, taking control firms only from non-regulated sectors is problematic because of the possible non-random selection of which sectors were regulated under the EU ETS. For this reason, the study is likely to suffer from selection bias at the sector level. 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 three sectors: cement, steel and power production. They cannot determine the sign of the effect with confidence for either of the three sectors analysed.

Aichele and Felbermayr (2012) investigate the effects of legally binding mitigation targets under the Kyoto Protocol on bilateral exports using a matching technique and find that Kyoto countries' exports are reduced by 13 percent to 14 percent. However, the validity of these results has been questioned. In particular, the signing of the Kyoto Protocol is a crude measure of environmental policy stringency and the results might be driven by factors other than the Kyoto Protocol commitments, most likely the effect of China joining the World Trade Organisation in 2002, which coincided with most Annex I countries' ratification of the Kyoto Protocol. The recent papers on the effects of the EU ETS on trade find limited empirical support (for example, Branger and Quirion, 2013). The combination of limited import penetration and the evidence of carbon cost pass-through suggests that measures designed to address competitiveness concerns (free allocation) may have prevented trade losses, but at the same time, European Union industries are protected by trade barriers to some degree, so that companies may have been over-compensated.

Commins et al. (2011) also use a large sample (200,000 firms) to study the impact of the EU ETS on productivity between 1996 and 2007 and find a negative effect of the EU ETS of around 6%. The main caveat of this study is that the treatment status of a firm was determined at the sector level, i.e. firms with small installations were incorrectly labelled as treated although only large installations in the sector were subject to regulation. Moreover, the estimated EU ETS effects therefore included the impact of sector level shocks to the outcome variables which were unrelated to the EU ETS.

3 Data and descriptive statistics

3.1 Background on the EU ETS

In 2005, the EU ETS launched in 24 countries across Europe, covering roughly 40% of the EU's total greenhouse gas emissions. Power stations and industrial plants across Europe were classified according to their main activity: "combustion", "cement", "paper and pulp", etc. Activity-specific size criteria then determine which installations would be included in the EU ETS. For instance, only combustion installations with a yearly thermal input exceeding 20 MWh were covered. Each year a smaller and smaller number of tradable emissions permits are then allocated to the more than 12'000 qualifying installations, which are each legally required to surrender enough permits every year to cover their emissions. Prior to the compliance date, however, installation operators can freely trade permits with each other (as well as with financial intermediaries and private citizens). The System has been implemented in 3 trading phases, with successively more stringent emissions caps for each phase. Phase 1, which ran from 2005–2007, was insulated from later phases by prohibiting banking and borrowing of permits across the phase boundary.

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 against future emissions reductions. See Ellerman et al. (2010) for a more comprehensive review of the design and implementation of the EU ETS. Since 2005, the spot price has varied between €0 and €30. The average price between 2005–2009 was around €15, although the actual price spent was much lower, since periods with low prices had higher trading volumes. The price of forward contracts has remained steadily above the spot price, though, suggesting firms are taking the progressive stringency of the cap into account. Installations, or rather the firms that operate them, can then make abatement and investment decisions according to the carbon price revealed in the market. In the European Union Emissions Trading System (EU ETS), free allowances were generously allocated in Phases I and II to compensate firms for the potential competitiveness impacts.

3.2 The database

For 8 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 Community Independent Transactions Log (CITL) (the EU body to which national registries report). For the remaining 13 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. The firm data set allows us to identify majority ownership. Using this information, we excluded 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.

4 Matching

Comparing two groups of firms that are more similar prior to 2005 makes it more difficult to explain away any difference in outcomes by factors other than the EU ETS. Ideally one would like to match each EU ETS firm with one or more non-EU ETS firms with similar resources available and facing similar demand conditions, regulations (other than the EU ETS), input prices, etc. Because of how the EU ETS was designed and implemented, this is at least theoretically possible. Regulatory status is determined by applying inclusion criteria to installations, not firms. For instance, installations for which the main activity is "combustion of fuels" are included only if their annual thermal input exceeds a threshold of 20 MWh. For steel plants, the relevant inclusion criterion is instead that installations have a production capacity exceeding 2.5 tonnes per hour. Installations manufacturing glass and glass fibre are included only if their melting capacity exceeds 20 tonnes per day. These three examples, taken from a longer list, make clear that regulated installations are bound to systematically differ from unregulated installations. Meanwhile, however, this configuration also means that what we refer to as EU ETS and non-EU ETS firms can in principle be identical in all respects relevant to their competitiveness, except for the size of a single installation. This allows us, in theory at least, to form groups of similar EU ETS and non-EU ETS firms. In practice, we restrict ourselves to more closely matched firms, excluding a number of EU ETS companies for which no good match can be found. What is lost in sample size, however, is regained in terms of accuracy and robustness (see, for instance, Dehejia and Wahba, 1999).

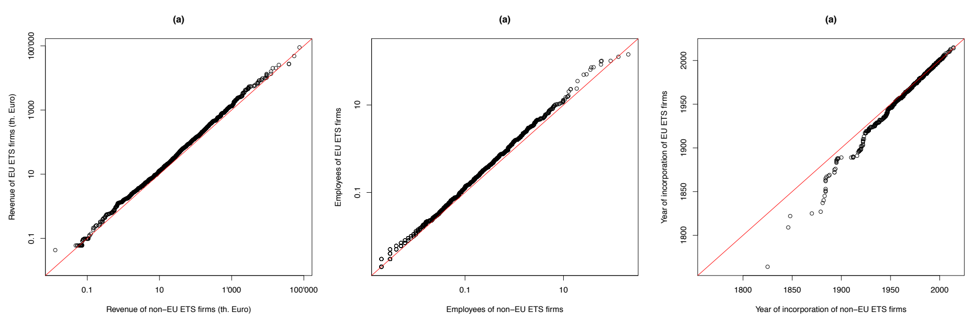
Our data set contains information on the country and economic sector in which firms operate as well as other firm-level information such as revenue, employment and age. Using this data, we have tried to assign to each of the 4,370 EU ETS firms a group of similar but unregulated firms. Though, this has not always been possible, for two main reasons. Firstly, the records for revenue and employees become incomplete the further back in time. In fact, we only have records on the turnover and employees in 2004 for 2,980 of the EU ETS firms. Secondly, though EU ETS regulations were applied at the installation level rather than directly to the firm, one might expect two very similar firms

Table 1: Comparison tests for ETS firms and matched non-ETS firms

t-test (H0: mean treated - mean matched = 0)		
	Mean difference between EU ETS and matched firm	p-value
Revenue in 2004	84541.55	0.233
Employees in 2004	284.13	0.111
Year of incorporation	1.54	0.018
Country	Exactly matched	
Economic sector	Exactly matched	

Figure 1: Comparison of matched ETS and non-ETS firms.

(a) revenue (b) employees (c) Year of incorporation



Notes: Panel (a) displays the empirical quantile-quantile (e-QQ) plot for revenue on a logarithmic scale in 2004, the year 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) and (c) show the e-QQ plots for the number of employees and the year of incorporation. Revenue, once again shown on logarithmic scales.

to receive the same regulatory treatment more than occasionally. Different regulatory fates are possible if, say, an EU ETS firm operates an installation just large enough to be covered by EU ETS regulations, while the matched control operates one or more installations just below the threshold. But even though we have a very large pool of firms to start with, sometimes there will be no such comparators available within the same country and sector. Therefore, the final matched sample contains 2381 ETS firms and 1803 non-ETS firms. There is one matched control firm for every ETS firm. Matching is applied with replacement which implies that sometimes one non-ETS firm serves as a match for several ETS firms. This explains why the control sample is smaller than the ETS sample. Both one-to-one matching and matching with replacement minimize bias at the expense of the efficiency of the estimator. The efficiency of the estimator is of lesser concern in this study, since we have a very large sample.

We start the matching procedure by restricting the pool of potential matches to those which operate in the same countries and economic sectors (NACE3 digits) as the EU ETS firms. This means that they are likely exposed to much the same business and regulatory environment, input prices, country and sector specific shocks and trends. The firms are also matched to have similar 2004 turnover, number of employees and age, since their available resources and capacity for R&D are likely important determinants of a firm's response to the EU ETS.

Figure 1 compares the empirical distributions of EU ETS and non-EU ETS firms in our matched sample on a few key variables used to construct the match. EU ETS regulated firms have slightly greater pre-EU ETS turnover on average. As can be seen in table 1, the difference in turnover and number of employees between ETS and non-ETS firms is insignificant. ETS firms are on average 1.54 years older than their matched non-

ETS firms. This difference is significant, but the qqplot shows that the result is mainly driven by firms that were founded before the First World War. We assume that this has a negligible effect on their comptetitiveness and that a firm founded in 1900 is comparable with a firm founded in 1910.

Because firms look similar within each match, the firms' 2004 observable characteristics do not help us predict (better than chance) which firm in each matched group would become regulated after 2005. Conditional on pre-EU ETS observable characteristics, the assignment of firms to the EU ETS appears random. In a naive sense, we have recovered the identifying conditions present in a randomized experiment.

In a further attempt to avoid potential bias, our matching procedure is combined with a difference-in-difference approach. This means that we do not compare revenue between ETS and matched non-ETS companies, but the increase of the revenue between 2004 (before the introduction of the EU ETS) and the 2005-2012 period. Therefore, the identification strategy is based on the assumption that conditional on the matching variables (sector, country, size, age) the outcome variables of ETS-frms and non-ETS firms would have followed a parrallel trend in the absence of the EU ETS.

5 Results

Perhaps the most transparent and intuitive way to view the results is with the aid of a simple graph plotting the revenue, employement and assets of matched EU ETS and non-EU ETS firms both before and after the EU ETS came into effect (see figure 2). In all three cases, the EU ETS and non-ETS firms follow parallel trends although the ETS firms have on average larger revenues, greater number of employes and assets. For both ETS firms and non-ETS firms revenues, employment and total assets are higher after the introduction of the ETS, but since this trend is common to ETS and non-ETS, it can not be attributed to the ETS policy.

Our main results can be found in the next three tables 2a to 2c. The causal effect of the ETS is captured by the interaction dummy ETS*Post2005. [2]ETS firms have a higher increase in revenue and assets and a lower increase in employment compared to non-ETS control firms, but the difference is allways smaller than 1 standard error. Therefore, we find no significant effect of the EU ETS on firms' revenue, employment and assets.

ETS-firm's revenues were €119M higher (+29%) on average during the ETS period compared to 2004. However non-ETS control firms too increased their revenues by €102M (+30%). So ETS firms increased their revenue by 17M more than non-ETS control firms, but this postive effect is non-significant.

The same trend is observed for total assets. Both the ETS and their non-ETS control firms have increased their assets during the ETS period compared to 2004. The former increased their assets by €710M , while the latter only by €468M. So ETS firms increased assets by €242M more, but this difference is non-significant.

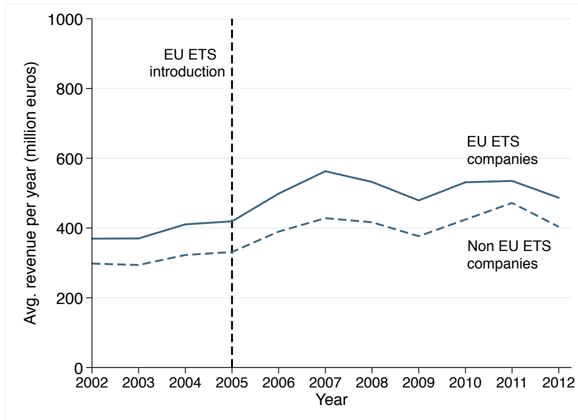
Concerning employment, the ETS firms have increase employment by 122, while their non-ETS control firms have increased employment by 170. Therefore the observed effect of the ETS is a reduction of 48 employees which again is non-significant.

Sectors at risk

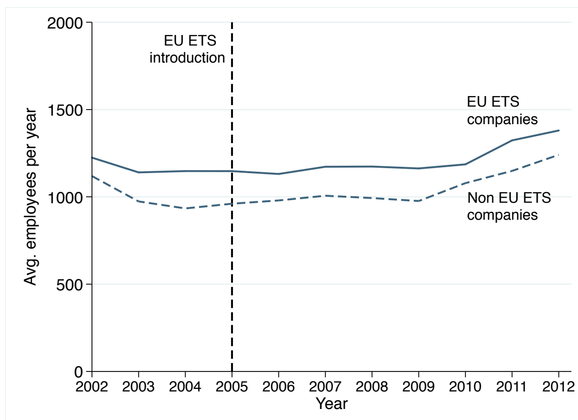
During the 3rd period, from 2013, sectors deemed at risk of carbon leakage will be allocated 100% of benchmarked emissions. Sectors that are not at risk of carbon leakage will be allocated only 80% of benchmarked emissions (and this proportion will decline to 30% over time), while the electricity sector will have no free allocation at all. Benchmarked emissions are defined as emissions per tonne of product of the 10% most carbon-efficient plants in the EU. Sectors at risk are defined as sectors at the NACE 4 code level for which at least one of the following criteria is met: 1) the sum of imports and exports exceeds

[2]The difference before and after 2005 for non-ETS firms is expressed by the coefficient on the Post-2005 dummy and the difference between the ETS and non-ETS in 2004 is expressed by the coefficient on the ETS dummy.

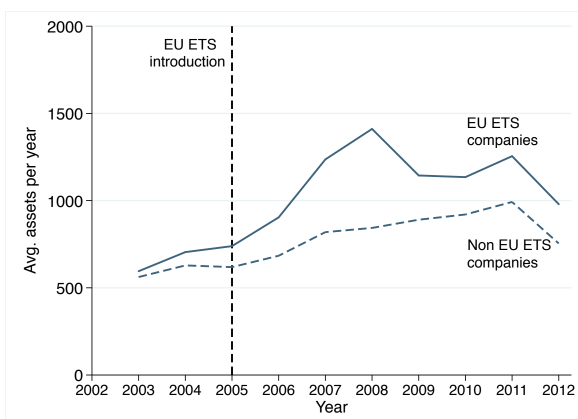
Figure 2: Revenue, employement and assets for matched ETS firms and non-ETS firms



(a) Revenue



(b) Employment



(c) Assets

Notes: Graphical representation of the difference-in-difference approach. The effect of the EU ETS is measured by comparing the different trends between matched ETS firms and non-ETS firms. Under the null-hypotesis of no impact of the ETS, the difference between both categories is constant over time.

Table 2: The effect of the EU ETS on revenue, employment and assets

(a) Revenue

	(1)	(2)	(3)	(4)	(5)
Dep. var.	Revenue (in th. EU)				
ETS * Post2005	16732.3	18094.7	2770.7	7251.9	31327.1
	(37963.36)	(37042.02)	(37205.35)	(36025.83)	(27146.93)
ETS	80335.7	113428.2	77302.0	82025.1	
	(78930.63)	(78383.72)	(73762.15)	(73612.92)	
Post-2005	102181.5***	156374.5***	122208.2***	156305.4***	
	(30187.99)	(33126.96)	(32725.53)	(32671.85)	
Year dummies	Yes	Yes	Yes	Yes	Yes
Country dummies	No	Yes	No	Yes	No
Sector dummies	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	No	Yes
Observations	38399	38399	38399	38399	38399

(b) Employment

	(1)	(2)	(3)	(4)	(5)
Dep. var.	Number of employees				
ETS * Post2005	-48.35	-62.62	-62.62	-106.2	-103.9
	(53.86)	(58.10)	(62.85)	(63.38)	(79.57)
ETS	171.3	252.3	201.3	210.2	
	(230.77)	(229.67)	(230.72)	(230.43)	
Post-2005	170.3***	277.2***	175.0***	258.0***	
	(70.01)	(89.60)	(84.11)	(92.62)	
Year dummies	Yes	Yes	Yes	Yes	Yes
Country dummies	No	Yes	No	Yes	No
Sector dummies	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	No	Yes
Observations	36168	36168	36168	36168	36168

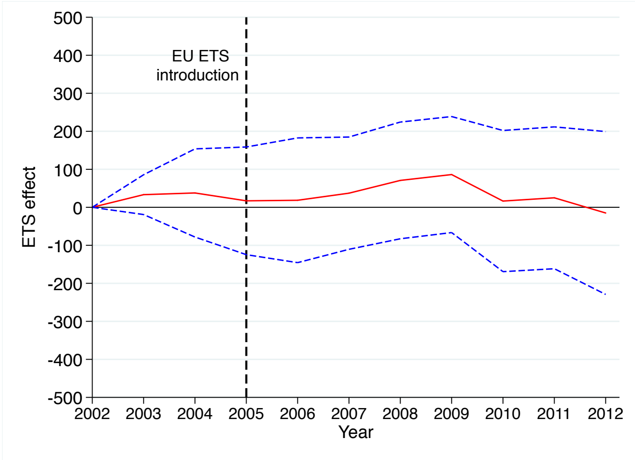
(c) Assets

	(1)	(2)	(3)	(4)	(5)
Dep. var.	Assets (in th. EU)				
ETS * Post2005	241952.3	317268.3	203903.3	237523.7	290351.2
	(270000)	(300000)	(250000)	(260000)	(260000)
ETS	76674.8	137655.3	164221.8	162007.6	
	(250000)	(260000)	(270000)	(270000)	
Post-2005	468536.2***	339934.5***	441841.5***	399367.6***	
	(130000)	(110000)	(160000)	(160000)	
Year dummies	Yes	Yes	Yes	Yes	Yes
Country dummies	No	Yes	No	Yes	No
Sector dummies	No	No	Yes	Yes	No
Firm fixed effects	No	No	No	No	Yes
Observations	37467	37467	37467	37467	37467

Notes: OLS regressions on matched sample. The effect of the EU ETS corresponds to the coefficient of ETS*Post2005. The coefficient of ETS indicates the difference between ETS and non-ETS firms. The coefficient on the dummy Post-2005 reports the mean increase between 2004 and the 2005-2012 period, it is calculated at the mean coefficient on the year dummies since 2005. Country dummies, sector dummies or firm fixed effects are different ways to control for country- sector- or firm-related effects that are constant over time. Clustered standard errors in parentheses (*$p<0.05$, ** $p<0.01$, ***$p<0.001$). Sectors are defined at the NACE3 level.

Figure 3: Effect over time of the difference between ETS firms and non-ETS firms.

(a) Revenue



(b) Employment

30% of turnover; 2) at a carbon price of e30, the carbon cost would exceed 30% of the value added 3) the sum of import and export exceeds 10% of turnover **and** the carbon cost would exceed 5% of the value added.

There has been some discussion about the relevance of the above criteria. Clo (2010) points out that only when a high trade exposure is combined with at high carbon intensity (the third criterion) there is a risk of leakage. According to his calculations, the third criterion only applies to 6 out of 257 sectors. The vast majority of sectors that are defined at risk are only trade exposed but not carbon intensive (131 sectors out of 257 sectors). Martin et al. (2014) based on more than 400 interviews with plant managers also conclude that the definition of leakage exposer was largely misspecified. For the same amount of free allocation, the aggregate risk of job losses could have been reduced by more than half.

In line with these results, Table 3 shows that if we restrict our sample to the sectors at risk, there is still no significant effect from the ETS on revenue, nor employment, nor assets. Figure 3 shows that the difference between ETS firms and non-ETS firms did not exceed the 95% uncertainty interval at any year.

Table 3: Results - Firms "at risk"

	(1)	(2)	(3)
Dep. var.	Revenue	Employment	Assets
ETS * Post2005 * At risk	-267690.3	585.1	-375256.7
	(180000)	(789.14)	(480000)
Year dummies	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes
Sector dummies	Yes	Yes	Yes
Firm fixed effect	Yes	Yes	Yes
Observations	11669	10771	11317

Notes: Clustered standard errors in parentheses (* p<0.05, ** p<0.01, *** p<0.001). All columns are estimated by OLS. Sectors are defined at the NACE 3 level.

6 Conclusion

Investigating 2381 ETS-firms over time and comparing each firm with an unregulated firm of comparable size and age within de same country and NACE3 sector, we show that the EU ETS has had no significant impact on revenue, employment and assets during the first and second phase of the EU ETS. The effect is insignificant despite the fact that the sample is very large. Moreover, the effect is also insignificant at any year taken separately, including 2005 and 2008 when carbon prices reached 25€ per tonne. Finally, when measuring the effect seperately for the sectors that were considered to be leakage exposed sectors, the effect of the EU ETS was insignificant.

The results can be understood by the fact that over 99,8% over emissions were allocated for free during the first phase and 97% in the second phase (Schleich, Rogge, Betz 2009). Moreover, free allocation covered more than verified emissions, leading to an unused exedent of allowances at the end of the first period when banking was not allowed. At the end of the second period in 2012, allocation had exceeded emissions by 1.8 billion allowances, which were banked into the 3rd period. Therefore, the only sector that was short in allowances on aggregate was the electricity sector. Since the electricity sector has little international competition, the cost pass-through of carbon costs in sales prices was very high. Moreover, since electricity prices are determined by marginal costs of the marginal production technology, the opportunity costs created by free allowances were also included in electricity prices and led to windfall profits (Sijm et al. 2008, Sijm, Hobbs 2010). Free allocation however comes at a cost. It creates perverse incentives when allocation is updated (Böhringer & Lange 2005, Rosendahl 2008), it distorts decisions for newcomers and plant closures (Ellerman 2008) and it refrains the externalities of pollution from being integrated in sales prices. Our findings therefore give support to the switch from free allocation towards auctioning of permits that was introduced after 2012.