# Multinational corporations and the EU Emissions Trading System: Asset erosion and creeping deindustrialization?*

Nils aus dem Moore[†], Philipp Großkurth[‡], Michael Themann[§]

December 2016

**Abstract**: The EU Emissions Trading System (EU ETS) is the EU's flagship tool to combat climate change and the world's leading carbon market. However, it may threaten the international competitiveness of the firms subject to it, which could lead to a relocation of economic activity. This study explores industrial relocation in the context of multinational enterprises (MNEs) by exploiting the installation-level inclusion criteria of the EU ETS. After identifying ownership structures for our full sample of firms, we employ a matched difference-in-differences approach to account for confounding factors. Our results indicate that overall the EU ETS had a positive impact on the regulated firms' tangible fixed asset bases. We also find a negative treatment interaction effect for a subgroup of multinational firms with special characteristics. However, the latter effect is not robust across all considered ownership specifications.

**Keywords**: MNE; EU ETS; asset erosion; creeping deindustrialisation
**JEL Classification**: F23, F64,Q01, Q48, Q54, Q58

*Preliminary version - please do not cite or circulate*

*The authors wish to thank Luise Voget, Julian Roeckert, Maximilian Zettler and Marc Teipel for excellent research assistance and data preparation. We are grateful to Ralf Martin, Ulrich Wagner and the organizers and participants of the "Research Seminar in Environment, Resource and Climate Economics" (RSERC) for comments and helpful suggestions. Bureau van Dijk provided feedback regarding the ownership data and other ORBIS-specific issues. Supportive information regarding the EU ETS was provided by DEHSt. Additional feedback was provided by participants at AERE 2016, ECOCEP 2016, Barcelona GSE Summer School on Microeconometrics and Geneva Summer School on Evaluating Policy Evalutions. We thank our colleagues Annekathrin Schoofs, Thorben Korfhage and Katja Fels for feedback and support.

[†]RWI - Leibniz Institute for Economic Research, Berlin Office, nils.ausdemmoore@rwi-essen.de
[‡]RWI - Leibniz Institute for Economic Research, Berlin Office, philipp.grosskurth@rwi-essen.de
[§]RWI - Leibniz Institute for Economic Research, Berlin Office, michael.themann@rwi-essen.de

# 1. Introduction

*To be completed*

# 2. Background

## 2.1. The EU Emissions Trading System

Launched in 2005, the EU ETS is today the European Union's flagship policy to comply with European and international commitments that seek to mitigate climate change.[1] It is the largest emissions trading system worldwide and imposes a cap on the total amount of greenhouse gas (GHG) emissions in 31 European countries from approximately 12.000 heavy energy-using sources, mostly industrial and power plants. As of 2016, this system covers around 45% of all GHG emissions of the 28 EU member states, plus Iceland, Lichtenstein and Norway. The main organizing principle of the EU ETS is "cap-and-trade": At the start of a trading period a cap is set on the total amount of emissions. Emissions allowances ("EU Allowances" - EUAs) are then allocated - either for free (so called free allocation) or via auctioning - to regulated entities. Each allowance corresponds to one ton of CO2-equivalent. At the end of each year, firms have to report their emissions and surrender allowances equal to the number of verified emissions. Non-compliance with this results in substantive penalties.[2] Within a given period, market participants can trade their allowances freely. This, combined with the induced scarcity, establishes a price for the ton of emissions. The total amount of allowances per period, i.e. the cap, is gradually reduced from period to period, thus causing total emissions to decrease over time.

The first trading period of the EU ETS (2005-2007), known as the pilot phase, was characterized by almost entirely free allocation of emission allowances and a cap that was highly decentralized and set on the member state level.[3] Banking of allowances was not permitted, thus making the cap detached from future periods.[4] Phase II (2008-2012) represented the first commitment period under the Kyoto protocol and established an EU-wide cap with a single Union Registry covering all regulated installations. While free allocation was still the default mode of allowance allocation (around 90%), banking allowances for future periods was now possible. The scope of the EU ETS expanded in terms of countries, sectors and regulated emissions.[5] Phase II also saw a decrease in the overall cap (6.5% lower than in 2005). However, phase II coincided with the 2008 financial

[1] The European Commission provides extensive information on the EU ETS online: http://ec.europa.eu/clima/policies/ets_en.

[2] In the first trading period (2005-2007), the fine was 40€ per ton CO2-equivalent. In the second period (2008-2012), the fine was 100€.

[3] For a more comprehensive review of the EU ETS design features, see Martin et al. (2016).

[4] Source: https://ec.europa.eu/clima/policies/ets/pre2013_en.

[5] Three countries (Iceland, Lichtenstein and Norway) joined the EU ETS in phase II. In terms of regulated firms, the aviation sector was brought into the scheme. Since its regulatory conditions are very different from other sectors, we do not cover it in our analysis. In 2013, (phase III) Croatia joined the scheme.

crisis that led to a global economic depression. As a consequence, economic activity and associated emissions were substantially lower during phase II than originally expected. At the end of phase II, the market had accumulated a large surplus of allowances and credits from international abatement projects. The average allowance price during phases I and II was slightly above 14 Euros. However, at the end of each period, the price per permit dropped considerably below 10 Euros.[6] For the third phase of the EU ETS (2013-2020), auctioning is targeted to becoming the default mode of allowance allocation.[7]

Two primary benefits are expected from the EU ETS: The first is achieving substantial emissions reductions in a cost-effective manner. In theory, the trading of emissions allowances between market actors, should, in equilibrium, lead to the price of CO2 per ton being equal to the marginal abatement costs, i.e. the cost of the last ton emitted. The second benefit that the system seeks to provide are incentives for firms to innovate, e.g. by developing and employing new low carbon technologies and processes. Preliminary results by Petrick and Wagner (2014) for Germany and unpublished work by Wagner et al. (2014) for France suggest that the EU ETS did incentivize regulated firms to reduce emissions, although this effect seems to have been largely driven by the second phase. The first study indicates that this could have been driven by fuel switching and gains in energy efficiency. In terms of innovation, Calel and Dechezleprêtre (2016) find that, on average, the EU ETS led to a 10% increase in low-carbon patents among regulated firms.

Besides assured emission reductions and expected innovation effects, possibly adverse impacts of the EU ETS on the economic performance of regulated firms are the third outcome dimension that has been intensely discussed since the early inceptions of the EU ETS. Imposing a carbon price can increase the production costs of regulated firms through two different channels Ellerman et al. (2016). First, firms either have to implement costly abatement measures or purchase permits on the market. Costs for firms increase further if EUAs are allocated via auctioning, although this has not been the default allocation mode in phases I and II. Even if the initial permit endowment for each phase is mostly based on free allocation, the obligation to hold permits per se creates an opportunity cost.[8] Second, if the power sector passes down such cost increases to consumers, this leads to further indirect costs of the EU ETS for manufacturing companies regardless of whether or not they are part of the scheme.[9] However, costs that are imposed on regulated companies through an environmental policy must not per se undermine their competitiveness. While the effect may be negative in the short run as firms incur costs to comply (e.g. invest in

[6] Calculations based on ICE Futures Europe EU Allowance data. In December 2012, the average EUA future price was 7.2 Euros. In neither of the two phases did the price drop to zero.

[7] According to the EU Commission, around 50% of total allowances are set to be auctioned (2013: around 40%).

[8] A firm can obtain a benefit from abating an additional ton of CO2-equivalent if the marginal benefit gained from selling the permit is bigger than the marginal abatement costs.

[9] In the subsequent analysis, we focus on the first, average effect of the EU ETS on regulated firms rather than any possible indirect effects the system may induce.

new technology) (Greenstone et al., 2012), in the long run stringent environmental policies may for instance increase productivity levels as firms reap the benefits of e.g. more economically efficient production processes (Albrizio et al., 2014).

However, the market conditions under which a firm operates may crucially determine its capability to adapt to an environmental regulation such as the EU ETS. For companies pertaining to the power sector, effects undermining their competitiveness may well be limited. Limitations in grid transmission capacities make it less probable that these companies compete with rivals that do not face the additional cost of a carbon price. In contrast, for manufacturing companies competing in international markets it may not be possible to pass down regulatory costs to their consumers without losing market shares. This may not only lead to a short term decrease in production and employment levels, but could also engender a relocation of economic activity of firms towards areas with less regulation.

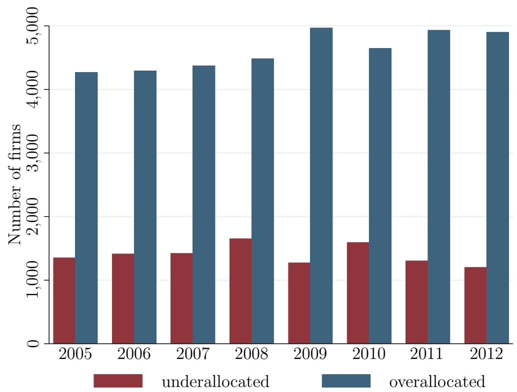



**Figure 1:** Firms by certificate allocation status

On the other hand, it seems to be highly quesionable whether the EU ETS should be expected to have an effect on firm behaviour at all besides the assured reductions of emissions. For in the two first phases, the regulation simply "did not bite": Firms were allocated more certificates than they actually needed, which turned the regulation into a de-facto subsidy for a large number of firms. Figure 1 shows this graphically for all firms.[10] Allocated emissions can surpass verified emissions substantially and consistently. Hence, a large share of regulated firms experienced windfall profits. The economic crisis after 2008 increased the share of overallocated firms further, leading to a stable surplus of certificates in the system. Consequently, certificate prices plummeted in phase I and stabilized on low levels in phase II.

## 2.2. Industrial relocation and asset erosion

Over time, impacts of the regulation might also become apparent in structural changes that take longer to manifest. The EU ETS is in principle a unilateral carbon pricing policy and the first of its kind to introduce very stringent regulation on polluting activities

[10] The situation looks qualitatively very similar if the sample is restricted to include manufacturing sectors only. We calculate the ratio of allocated to verified emissions per year based on EUTL data. Appendix A.1.1 provides detailed information about the data and their preparation. All figures are based on our own calculations.

within the EU. This setup may induce a pollution-haven effect, in which companies move their carbon-intensive production assets to lesser regulated countries and regions. Such a process may not only cost jobs and economic activity in Europe, but undermine the effectiveness of the EU ETS as a tool to combat climate change since emissions would relocate along with production capacities. In the worst case, this phenomenon, often referred to as "carbon leakage", could mean that the effectiveness of the EU ETS as a tool to achieve relevant emission reductions on a global scale would be severely undermined (Reinaud, 2008).

The threat of industrial relocation has been a major concern for EU and national policy makers and is frequently used by industry groups to obtain concessions.[11] In response, free allocation rather than auctioning became an important design feature for the first two regulatory phases.

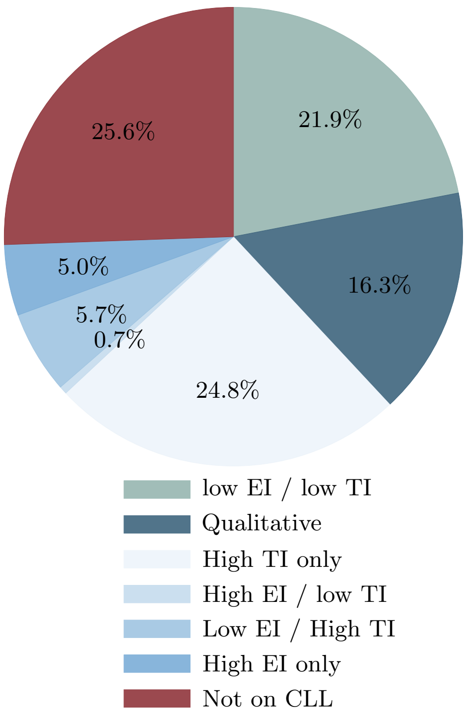



**Figure 2:** Firms in manufacturing by CLL status, 2012

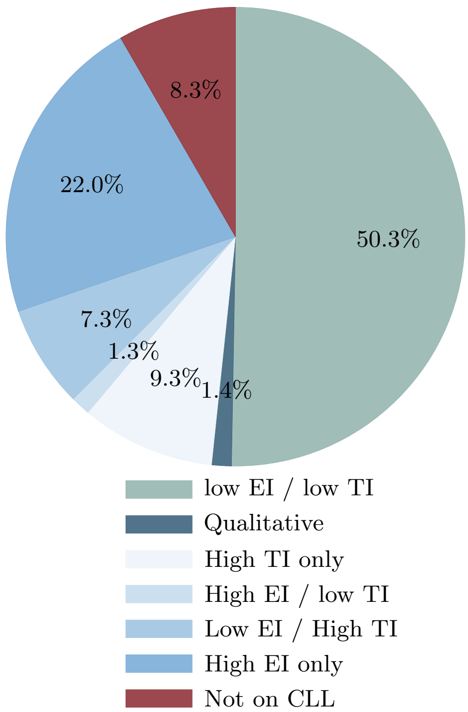



**Figure 3:** Verified emissions in manufacturing by CLL status, 2012

In addition, in 2010 the EU commission introduced a "carbon leakage list" that comprises all manufacturing sectors and subsectors deemed to be at a very high risk of shifting activities in response to the EU ETS.[12] In essence, this is done by calculating energy and

[11]For Germany, various newspaper articles provide anecdotal evidence for this. For example, Vorholz (21.06.2007) (in German) highlights the heated discussions around the issue of climate change that took place in Germany during the first phase of the EU ETS. Essentially, industry representatives at the time accused chancellor Merkel of "deindustrializing" the country. Nicola and Andresen (21.01.2014) provide a more recent quote by Ulrich Grillo, president of the Federation of German Industries (BDI): "Already today one can prove that there's a creeping emigration among the energy-intensive industries".

[12]According to Directive 2003/87/EC, referenced in Commission Decision 2014/746/EU, there are several

trade intensities for each sector. If certain thresholds are surpassed, firms enter the list either because of their high exposure to international competition and/or the energy intensity of their production. For regulatory phase III, companies on this list receive a higher share of allowances for free. However, the optimality of the inclusion criteria has been disputed (Martin et al., 2015). Figures 2 and 3 show how many firms are on the list and which criteria are applied.[13] Three quarters of firms in manufacturing are considered to be at risk of carbon leakage, a sample that emits over 90% of all verified emissions of that sector. Around 50% of these emissions correspond to firms with either low emissions or trade intensities, which stresses the point made by Martin et al. (2014).

If a firm faces increased production costs due to carbon regulation, it must not only balance abatement with permit related costs but is also likely to consider the possibility of evading the regulation by (partial) relocation. The multistep decision whether to relocate in the first place and the scale of any potential reorganization of business structures on a global level depend largely on the expected benefits and costs associated with this process. Hence, scale and type of relocation will vary considerably depending on the firm type and the conditions under which it is operating.

On the other hand, remaining under the regulatory umbrella of the EU ETS can also, as already described above, create a number of short and long term benefits that a firms needs to weigh against the potential costs. Aside from the unintended windfall profits of the first phases, the EU ETS provides a certain degree of regulatory stability. Areas without any form of carbon pricing might introduce their own systems in the future, which constitutes a risk any investor would have to take into account. Furthermore, firms may obtain an advantage over international competitors in the long run if cleaner technologies turn out to be more competitive. If the EU ETS would induce the development of such technologies, learning how to adapt to the regulation could create first-mover advantages. [14]

criteria according to which a sector can be deemed as exposed to a significant risk of carbon leakage. Here we label firms as having a low(high) emission intensity when the sum of direct and indirect additional costs induced would lead to a substantial increase of production costs, calculated as a proportion of the gross value added, of at least 5% (30%) and as having a low(high) trade intensity when the intensity of trade with third countries, defined as the ratio between the total value of exports to third countries plus the value of imports from third countries and the total market size for the Community (annual turnover plus total imports from third countries), is above 10% (30%). Furthermore, some sectors meet qualitative requirements. As outlined in Commission Decision 2014/746/EU, several combinations of the criteria are applied.

[13] We created a sectoral database of the criteria of the carbon leakage list before merging it to the EUTL data. Appendix A.1 provides further details.

[14] A long-lasting discussion of the potential positive impacts of environmental regulation on affected firms' competitiveness was started by Porter and van der Linde, Claas (1995), the results of which have been summarized in Ambec et al. (2013). They conclude that "the evidence for the "weak" version of the Porter Hypothesis (that stricter environmental regulation leads to more innovation) is fairly clear and well established. However, the empirical evidence on the strong version of the Porter Hypothesis (that stricter regulation enhances business performance) is mixed, but with more recent studies providing clearer support."

Firms subject to the EU ETS will thus optimize their decisions on (short and long term) investments and divestments based on their expectations of future benefits and costs. These expectations depend crucially on whether companies believe in the long-term persistence of the regulation. Since the EU ETS is based on a multinational agreement with strong repercussions upon unilateral exit, its credibility is substantially higher compared to national regulation.[15] Hence, firms may consider the EU ETS likely to be the permanent key instrument to achieve the EUs goals to achieve significant reductions in GHG emissions in the long run. Regardless of short-term market signals, it could then safely be assumed that the tightening of the cap over time would induce higher certificate prices in the market.

Empirical evidence on the direct impact of the EU ETS in terms of competitiveness has so far provided mixed results. Some studies using comprehensive firm micro data find small to sizeable positive impacts on the turnover and sales of regulated firms that hint at a possible pass through of regulatory costs to product prices ( Chan et al. (2013); Petrick and Wagner (2014); Koch and Basse Mama (2016)). In contrast, a study by Abrell et al. (2011) that compares EU ETS firms with firms from non-regulated sectors as well as research by Wagner et al. (2014) on regulated French companies do find a statistically significant decrease of employment. However, this effect is not corroborated in the other studies. Empirical studies investigating possible causal effects of environmental policies on relocation have mainly looked at two potential channels, namely a change in trade flows Aichele and Felbermayr (2015), with respect to signatories of the Kyoto protocol) and a direct relocation of production through foreign direct investment (Hanna (2010), with respect to the response of US-based multinationals to the Clean Air Act Amendments).

In the context of the EU ETS, this strand of the literature is still in its infancy. Naegele and Zaklan (2016) analyze trade flows at the sectoral level. While Borghesi et al. (2015) investigate whether Italian firms regulated by the EU ETS increased their propensity to open up new plants in non-EU ETS regions, Koch and Basse Mama (2016) analyze if German multinational firms increased their outward FDI. Dechezleprêtre et al. (2015) assess emission leakage within a small sample of multinational companies. All of these studies reject the presence of strong relocation effects caused by the EU ETS. However, Koch and Basse Mama (2016) do find that a small group of regulated firms significantly increased its outward FDI by a sizeable amount. Interestingly, these firms are not operating in emission-intensive sectors but rather pertain to sectors with low capital intensities. This supports the hypothesis that relocation costs play a major role when assessing industrial relocation.

[15] The Directive 2003/87/EC of the European Parliament and of the Council of 14 October 2003 as amended by Directive 2009/29/EC of the European Parliament and of the Council of 23 April 2009 constitutes the foundation of the EU ETS. The Directive is one result of a long process on how to meet the requirements of the Kyoto protocol that began as early as the 1990s (Convery, 2009).

In the medium term, a total relocation of firms subject to the EU ETS, even for those at high risk of carbon leakage, seems unlikely. After all, relocation is associated with a number of costs such as administrative costs or costs related to the sale or deconstruction of assets in the home and the acquisition or construction of new facilities in the host country. It is important to note that many sectors covered by the EU ETS are capital-intensive. In these sectors, fixed assets such as production facilities or power plants can operate for very long time horizons of several decades, meaning that these sunk costs can make firms less geographically mobile. Mobility can also be restrained by other factors, such as high transportation costs that require a close proximity to product markets (e.g. in the electricity or cement sector). However, if the assumption seems justified that the EU will enforce a continued reduction of regulated emissions that will exceed comparable measures in other countries and regions of the world for many years to come, the possibility of long-term structural adjustments of regulated firms has to be taken into account.

Since our prime objective is to study the effect of the EU ETS on industrial structures and to provide insights on their possible relocation, we chose the level of a company's tangible fixed assets (TFAS) at a given location as our main variable of interest. The variable captures the financial value of a firm's physical (tangible) assets as recorded in its yearly balance of accounts and closely resembles production capacities such as machinery, plants and equipment. It is important to note that tangible fixed assets are subject to depreciation, i.e. the book value of a given base of tangible fixed assets declines over time if no investment occurs. Older assets have to be continuously replaced by new assets with frequencies depending on the asset type. Thus, the recorded value of a firm's tangible fixed assets rises if investment exceeds the depreciation of the current asset stock. Such a rise could signal both the growth of a company (quantity effect) or an improvement of its asset base (quality effect), e.g. through investment in new abatement technologies.

Focusing our analysis on tangible fixed assets also follows the rationale that even if the short term costs imposed by the EU ETS are dampened by free allocation, investment decisions into production capacities have to consider long time horizons and, as already mentioned above, take the regulatory pressure induced by the EU ETS in the long run into account. Since the variable reflects production capacity it is very likely to be affected by these considerations, while at the same time being less sensitive to short term dynamics.

Note that the ORBIS data base does not contain information on emissions or energy intensities. The lack of reliable GHG emissions data at the firm level has been a challenge for empirical research, especially for studies that focus on carbon leakage rather than industrial relocation (Dechezleprêtre et al., 2015). Notable exceptions with regards to emissions are some country-specific studies with administrative data (Petrick and Wagner (2014); Wagner et al. (2014)). To the best of our knowledge, there exists no firm-level database for emissions on a global scale that takes corporate structures into account.

Consequently, in the absence of such data, we cannot verify within a causal analysis framework whether or not the EU ETS lead to the actual implementation of abatement measures in MNEs. However, we do observe emissions for regulated EU ETS companies and use this data to investigate the functional relationship between a regulated company's tangible fixed assets and its emissions.

While tangible fixed assets and verified emissions exhibit a positive correlation (Appendix A.2, Figure 15), significant movement of firms between phase I and phase II suggests that abatement efforts might have taken place. Furthermore, emissions are strongly correlated with economic activity (Appendix A.2, Figure 16) and thus subject to external shocks (e.g. the financial crisis of 2008).

Table 1 shows the results of a two-way fixed effects regression of tangible fixed assets in logs on verified emissions in logs for the phases I and II of the EU ETS (2005-2012). Once operating revenue is taken into account, the coefficient for tangible fixed assets remains negative and highly significant.[16].

**Table 1:** Explaining verified emissions in the EU ETS

| | (1) | (2) |
|---|---|---|
| Tangible fixed assets in logs | 0.083*** | −0.050*** |
| Operating revenue in logs | | 0.529*** |
| Number of observations | 32878 | 31681 |
| Number of firms | 5219 | 5074 |
| R2 adjusted | 0.07 | 0.18 |

Significance levels: * p <0.10, ** p <0.05, * * * p <0.01

Standard errors are clustered on the firm level.

Year fixed effects and firm-level fixed effects included.

While the coefficient of tangible fixed assets is small compared to operating revenue, it indicates that for regulated firms, higher levels of tangible fixed assets correspond ceteris paribus to lower levels of emissions. Since firm size effects and yearly shocks are explicitly controlled for, this suggests that the asset value of low carbon production technology has been higher than the value of emissions-intensive technology. Thus, an increase in fixed assets by regulated companies from 2005-2012 may reasonably well capture abatement investment.

[16]This finding is robust to the inclusion of outliers, using sector-year fixed effects, using first differences and including total assets to control for firm size effects

## 2.3. Business groups within the EU ETS

For the purpose of our analysis, we distinguish between two types of Global Multinational Enterprises (MNEs): *Global MNEs with a functional link* possess at least one subsidiary outside of the EU ETS area that is active in the same sub-industry as a regulated subsidiary of the same global MNE inside the EU ETS. *Global MNEs without a functional link* possess at least one subsidiary outside of the EU ETS but not in the same sub-industry as a regulated subsidiary.[17]

If a firm that is subject to the EU ETS belongs to such a network, it may react very differently to the policy than other firms. One reason is that administrative costs to transfer business operations can be lower since the network is already operating in the given non-ETS location. Even more important may be the fact, that in the case of Global MNEs with a functional link, the foreign subsidiary is already part of the same sector as its EU ETS counterpart. For instance, consider a firm network that owns two steel plants, one operating in Europe and the other one in the United States. If the network expects the relative costs of production in Europe to increase substantially over a given time horizon due to a carbon price it may decide to shift its investment priorities with regards to its fixed asset bases in the two locations.

In our given context, this could mean that, everything else being equal, the relative share of future investments that the network dedicates to its steel operations at the European subsidiary, either for the replacement of old assets or for capacity expansions, may decrease as a direct result of carbon pricing in the EU. Instead, relatively more investments would be dedicated to the already existing plant in the United States. A similar argument can be made for Global MNEs that do not (yet) include a functional link.

Note that these shifts in investment patterns into (already existing) asset bases can take place gradually, allowing the multinational network to adapt over time to changes in expectations of costs related to carbon policy. This is why this potential channel of industrial relocation may be very important in the policy context at hand. In contrast to a rapid divestment, this process has also been described as "creeping deindustrialization" and would not manifest itself in large and sudden shifts, but rather take place through a slow restructuring of assets over time.[18]

Given the high levels of free allocation in regulatory phases I and II and the mixed findings by empirical studies, it is not very plausible to expect that regulated companies

[17] We are also able to identify national business groups, multinational enterprises that are located fully within the EU ETS and global MNEs that operate entirely outside of the EU ETS. However, we assume that the EU ETS does not induce relocation activities within these groups. Appendix A.1 provides further detail on the applied MNE criteria.

[18] Cowie and Heathcott (2003) illustrates the changes in the US manufacturing sector in the 1980s.

already divested strongly and moved their assets to an entirely new location, which would result in substantial amounts of foreign direct investment. In contrast, it is more likely that a shift in priorities of investments into asset bases might have taken place at those regulated companies that face lower relocation costs. The commitment to such a shift is also easier to reverse for these companies.

In our subsequent analysis of the EU ETS, we first seek to investigate its impact on the tangible fixed assets of all regulated firms. Due to data limitations, we refrain from assessing shifts in investment priorities on the global level and instead focus on the European firm level.[19] In general, the EU ETS could have either a positive or a negative effect on companies' assets, or none at all. Whereas a positive effect could point towards an increased abatement by regulated firms, a negative effect may indicate a downsizing of operations.

In a second step, we then analyze if regulated firms that are part of a global network react differently. Due to potentially sizable differences in relocation costs as well as a fundamentally different corporate structure, we would expect regulated firms that are part of a global multinational network to either commit to a lesser degree to abatement if the asset effect is positive, or, in case of a downsizing of assets, would expect them to divest their assets more strongly. In both cases, this would indicate that indeed, in a direct response to the EU ETS, these companies are obviously less committed to maintaining their asset bases and hence their business operations in Europe.

If, on the other hand, the effect is not negative and multinational firms do not behave systematically different, this would provide evidence that the EU ETS did not lead to an erosion of European asset bases during the first two trading phases and therefore dispute the idea of an already ongoing process of creeping deindustrialization through global firm networks.

The relevance of our research question depends on the relevance of business groups within the EU ETS. We find that business groups with the potential means to shift emissions abroad are not only responsible for large shares of emissions, they also connect a large number of regulated firms. To the best of our knowledge, we provide the first detailed description of the population of firms regulated by the EU ETS with respect to these structural characteristics.

[19] First exploratory analysis revealed substantial data quality heterogeneity between countries outside of Europe. Consequently, we can only provide evidence for effects on individual firms with special characteristics (specific MNE status). Assessing comprehensive network effects in a causal analysis framework is not possible due to a lack of data in earlier periods.

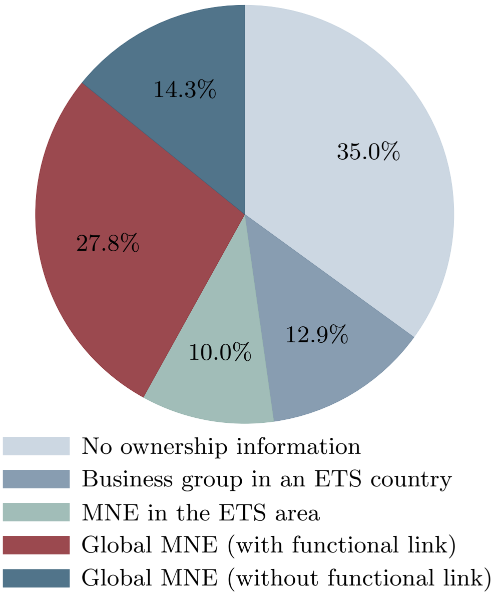



**Figure 4:** Firms by firm type in manufacturing, 2012

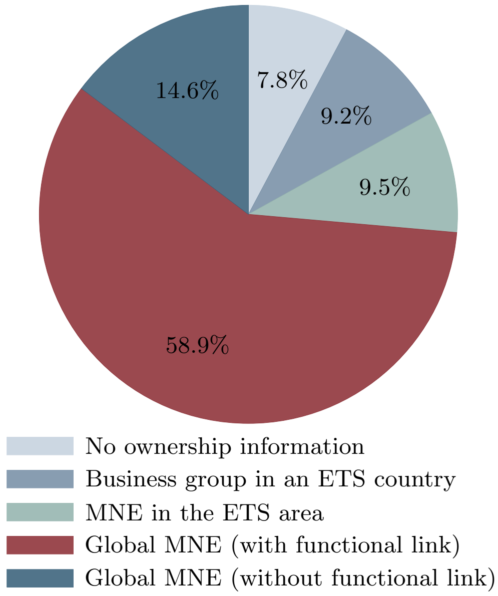



**Figure 5:** Verified emissions by firm type in manufacturing, 2012

Figure 4 splits the sum of firms by firm type while Figure 5 does the same for the verified emissions. To our surprise, only around a third of all firms within the EU ETS are actually independent and largely irrelevant in terms of their emissions.[20] Firms that are active in several countries account for the vast majority of all verified emissions.

Since the majority of firms are connected in some way, we can aggregate emissions connected to firms at the top of their respective corporate hierarchies. Figure 6 lists the shares of verified emissions that correspond to the groups controlling the largest amounts of emissions within the EU ETS. In total, the 10 largest emitters accounted for over 30% of all verified emissions from 2005-2012. More than half of all emissions can be attributed to the top 50 business groups, connecting a total of 869 regulated subsidiaries.[21] Although our figures are certainly dependent on matching and data accuracy, the notion that business groups play a major role within the EU ETS cannot be disputed.

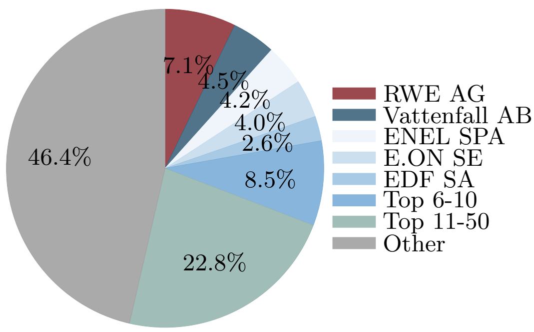



**Figure 6:** Largest emitters within the EU ETS, 2005-2012

[20] To calculate these shares, we attach ORBIS ownership information to the EUTL data. A detailed description of the extensive data preparation is provided in Appendix A.1. We focus on the manufacturing sector, but the impression is qualitatively similar for the full sample. We identify firms within the ORBIS database as independent from other firms if there is no ownership data available. However, this group could still include connected companies with missing data.

[21] The groups are of course much larger than the regulated firms we observe in the ETS data. We explore these structural details in depth in an accompanying paper.

Having underlined the relevance of large business groups in terms of their emissions, we next elucidate their respective spatial structures. The EUTL data includes information on the location of each regulated plant, which can be traced and geocoded. Furthermore, ORBIS includes information on the location of the firms as well (this information can overlap, but does not necessarily have to). We first visualize the regulated plants by geocoding their location and plotting their verified emissions on a map of Europe. Bubble sizes in figure 7 correspond to verified emissions in 2012. While we do not conduct a more detailed geospatial analysis, emission clusters can be identified visually with ease.

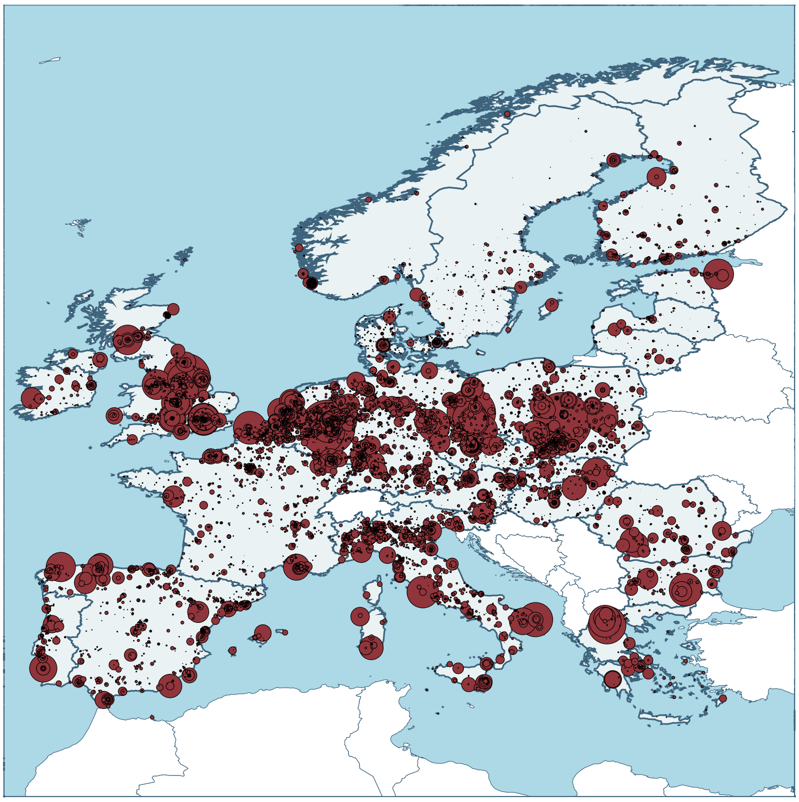


**Figure 7:** Verified emissions in 2012

Furthermore, assessing the network information of regulated business groups highlights their structural complexity. Figure 8 illustrates the structures of a selection of business groups in 2012 that control the largest amounts of emissions.[22] This visualization highlights that we have to continue cautiously when it comes to the fundamental assumptions of a causal analysis.

[22]Note, however, that several compromises had to be made in the process of constructing a simplified visualization. First, the structures connect each identified subsidiary directly to the corporate headquarter of the respective corporation. In reality, firms are connected through chains of control that manifest in intricate hierarchies with many levels. While we do have this information (and indeed use it to identify the structures), it is not considered here. Second, ownership structures are never static. The map is a snapshot of the data in 2012 and would look different for any other year. Third, we only map out regulated subsidies here. The full business groups are much larger.

In particular, Figure 8 casts doubt on the assumptions that (i) all treated individuals are independent and that (ii) the treatment intensity of the EU ETS is constant, because it is plausible that business groups shift resources within their networks. In our subsequent causal analysis, we try our best to resolve these issues and highlight the limitations of our results. What matters for *any* analysis that uses firm-level data is that these connections have to be taken into account. For example, if spillover effects within groups are plausible, all subsidiaries that are connected in some way to a treated firm can not be part of a valid control group. Furthermore, if spillover effects within business groups exist and matter, evaluating a regionally confined treatment in a regionally confined setup might not reveal an unbiased and comprehensive representation of its consequences. Recognizing these challenges, we try to shed some light on whether or not firms in business groups with certain relevant distinctions react to the EU ETS in different ways.

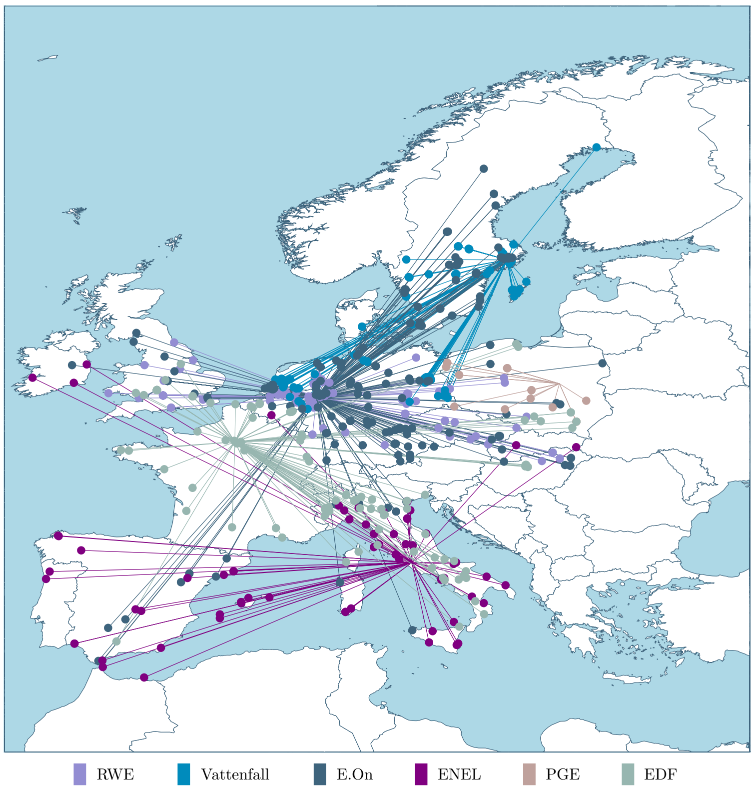



**Figure 8:** Selected business groups in 2012

## 3. Research Design

Our study attempts to identify the causal impact of the EU Emissions Trading System on the asset bases of treated firms between 2005 and 2012. Furthermore, we analyze if companies that are part of a global multinational network react differently to the regulation.

In a classic randomized controlled trial (RCT), random assignment of treatment status balances observed and unobserved firm characteristics across the treatment and control group. However, since we are working with observational data, treatment assignment is not random. A simple comparison of means between participating and non-participating firms will thus not yield a reliable estimate of the causal effect of the EU ETS if the distributions of observed and unobserved confounders are not balanced between the two groups.

We thus follow a two stage approach proposed by Heckman et al. (1998) that was applied in the context of the evaluation of the EU ETS in similar ways by Koch and Basse Mama (2016) and Zaklan (2016). In the first stage, our goal is to find a subgroup of non EU ETS firms that is very similar to our treated group of EU ETS firms in pre-2005 characteristics. In the second stage, we account for any time-invariant confounders that may remain after the design stage via a Difference-in-Differences (DiD) estimation. Combining the strengths of both strategies enables us to obtain credible estimates of the causal effects of the EU ETS (Blundell and Dias, 2009).

### 3.1. Stage I: Design stage

Our main goal for the design stage is to substantially improve the overlap in covariate distributions between treated firms of the EU ETS sample and untreated firms in the control group (Rosenbaum and Rubin, 1983). Balancing on observed covariates will also allow us to balance other firm characteristics that we do not observe, if these are related to our observed covariates. Intuitively, we want to make the two groups as similar as possible in terms of all pre-treatment characteristics that may confound our estimates of the causal impact of the EU ETS on asset bases. For such a sample, it is far less likely that a post-2005 shock will have a systematically different impact on these two groups and thus obscure the estimation of the causal effect.

In order to address this challenge, we exploit the unique design features of the EU ETS to obtain a sample of treatment and control firms that are equivalent in a whole set of potential confounders. In particular, whether a firm is subject to the system is not decided at the firm level, but at the installation level. Also, due to implementation costs, the EU ETS does not comprise all European installations in carbon-intensive industries. Instead, regulatory status of an installation is set via industry specific criteria such as capacity thresholds. For instance, a steel plant will be covered by the EU ETS if its production capacity is above 2.5 tons per hour, whereas for a plant producing ceramic products this threshold will be at 75 tons per day. In terms of combustion processes for power or heat generation, plants only enter the system if their annual thermal input exceeds 20 megawatt.[23]

The exploitation of the EU ETS inclusion criteria along the lines established by Calel and Dechezleprêtre (2016) should allow us, at least in principle, to find a suitable sample of EU ETS and control firms that are very similar in all aspects that matter for investment decisions into their asset bases except for the size of their installations. The key idea here is that our analysis is conducted at the firm level rather than at the plant level. Firstly, investment decisions are taken by the firm that owns the plant, not by the plant itself. Secondly, we can expect asset bases to be determined by a whole range of firm level characteristics (such as asset structure, overall size or the sector and country a firm operates in) and not exclusively by the size of a single installation.

We employ a propensity score approach (Rosenbaum and Rubin, 1985) to construct a sample that balances out our covariates. In our policy context, the propensity score stands for the probability of being subject to the EU ETS conditional on a set of observed characteristics. With a large set of potential confounders, finding an exact match for each ETS firm based on pre-treatment characteristics becomes a difficult task. Propensity scores solve this problem of dimensionality by compressing the information of the continuous variables used in the matching process into a single score. ETS firms are then matched to their closest neighbors from the reservoir of potential control firms based on the score. In order to determine for how many ETS firms we find a suitable neighbor, we assess the overlap in propensity score distributions (also called "common support") between our ETS and non ETS groups.

Restricting the sample to those firms with a sufficiently close neighbor based on the propensity score will thus improve balance in covariate distribution. Treated firms from our sample for which we do not find a sufficiently similar counterpart among non EU ETS firms are discarded. Thus, the main challenge is to develop a propensity score specification that balances out the main confounders without sacrificing too much sample size.

However, there are several steps of data preparation and processing that need to be applied before approaching the balancing procedure. First of all, we exclude obvious errors and data anomalies (e.g. negative total assets or observations with extreme jumps in consecutive years). Secondly, using the data on verified emissions, we identify all firms that have been active in both phases of the EU ETS. Since the data on stationary installations

[23] Directive 2003/87/EC of the European Parliament and of the Council as of 13 October 2003 amended by Directive 2009/29/EC of the European Parliament and of the Council as of 23 April 2009 provides detailed information on the capacity thresholds.

alone does not indicate when a firm entered the regulation, this step is crucial to ensure correct assignment of treatment status. Thirdly, we use our ownership data to identify all firms that are connected to our treatment group and exclude them from entering the control group. This step is required to ensure that we do not overestimate a potential treatment effect by sampling (potentially) affected firms into a control group. Not excluding these firms would lead to the construction of an invalid control group because the firms would, if spillovers within networks exist, be treated by the EU ETS as well. Thirdly, we reduce our data to a balanced panel. Since the EUTL data we use is reported in 2014, we can not exclude the possibility that firms that no longer exist in 2014 are also no longer included in this data. Assessing the attrition of firms between our treatment group and a control group based on unbalanced data indicated that firms in the control group disappeared at a faster rate than firms in the treatment group. Without yearly registry data from the EUTL we can not we can not distinguish whether this difference is related to the EU ETS or due to the reporting structure of the data. Our solution is to reduce the dataset to firms with data on tangible fixed assets and operating revenue in all periods, thus eliminating any potential attrition bias entirely. Finally, we also exclude the very largest of firms (outliers).[24] For those firms any matched control firm would likely differ substantially in treatment-relevant unobserved characteristics (e.g. emitting infrastructure), otherwise it would have been treated as well.[25] We discuss possible impacts of all of these choices with respect to data processing in the presentation of our main results in section 4.1.

For our processed sample of 325,445 companies, we estimate the propensity score using a probit model. We specify a function of the propensity score that allows us to take into account an extensive selection of firm level characteristics that can be important determinants of treatment status and our outcome variable, tangible fixed assets. These include relevant potential confounders $X$ such as information on tangible fixed assets, total assets, operating revenue, company age as well as asset, investment and profit ratios for each year of the entire pre-treatment period of 2002-2004.

Note that $X$ can only consist of variables that were not affected by the EU ETS. Otherwise, $X$ will be endogenous and will introduce a bias to our subsequent estimates. We account for this by balancing out the covariates only for the pre-treatment period of 2002-2004.

We then enforce an exact match on the sector-country level (NACE Rev. 2 two-digit

[24] A firm is considered to be an outlier under this category if it is among the 0.1% firms with the highest or lowest values of either total assets, tangible fixed assets, operating revenue, asset ratio, profit ratio or normalized growth rate in tangible fixed assets in any given year.

[25] This assumption is plausible not only in the energy sector, but also for very large firms in manufacturing. Assessing the impacts on these very large players could be done more appropriately in other empirical research designs.

level) between a given ETS firm and its nearest neighbor based on the propensity score.[26] Utilizing only the closest match for a given treated firm increases the chance of unbiased estimates of the treatment effect, while sacrificing precision. Not allowing for any of the one-to-one pairings to be operating in different sectors or countries is important, as imbalance between treatment and control group in these aspects can be problematic. For instance, a steel company may have very different investment patterns than a company operating in the chemical industry. We further explore this issue in section 4.3 by enforcing exact matching on NACE Rev. 2 three-digit level for different sub-samples.

Next, we trim the sample by restricting it to those EU ETS firms with common support, i.e. to those EU ETS firms for which we do have at least one nearest neighbor from the reservoir of possible control firms that exhibits a sufficiently similar propensity score. Trimming the sample to those companies on support comes at a certain price, i.e. we lose some degree of external validity. Hence, extending our findings to the whole population of regulated EU ETS firms will be somewhat less attainable. The clear benefit of a more consistent subsample is that this loss in sample size and external validity is more than compensated for by the resulting gain in internal validity. This means that our estimates, albeit reflecting the average treatment effect on the treated (ATET) only for a certain subpopulation of the EU ETS, will be more accurate and less prone to potential bias (Dehejia and Wahba (1999, 2002)).

To assess the covariate balance, we employ a set of different balance diagnostics. Standardized differences or standardized bias is considered a reliable measure for assessing balance that is robust to changes in sample size and comparable across covariates independent of scale. It is defined as

$$d = 100(\overline{x}_1 - \overline{x}_{0M}) / \sqrt{\frac{s_1^2 + s_{0R}^2}{2}}. \tag{1}$$

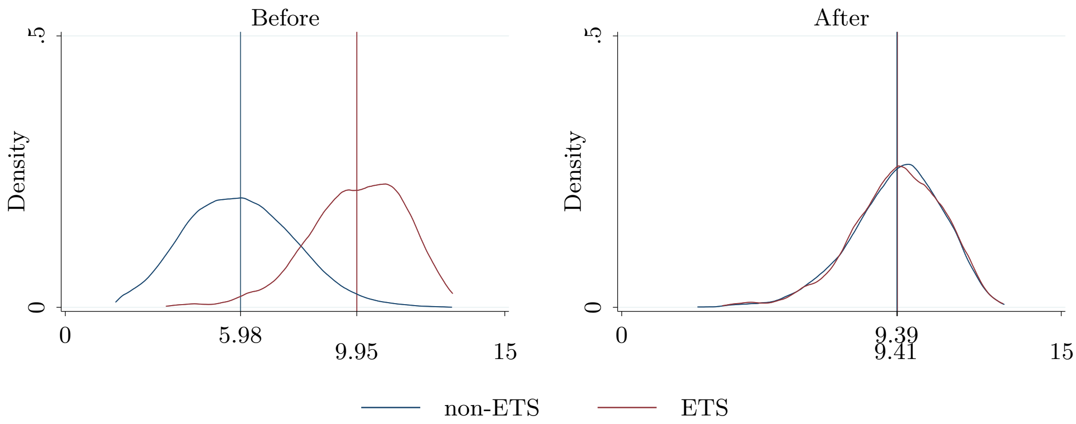
where for each covariate, $\overline{x}_1$ and $\overline{x}_{0M}$ are the sample means in the treated group and matched control group and $s_1^2$ and $s_{0R}^2$ are the sample variances in the treated group and control reservoir (Rosenbaum and Rubin, 1985). The results of the balancing process are reported in Appendix A.3, Table 12 (Sample 3, "Baseline"). For the full range of our covariates in all pre-treatment years, standardized differences are well below 10, indicating a very good balance.[27]

[26] Technically, this is done by constructing a second pseudo-propensity score. We spread the propensity score distribution by multiplying it with a scaled country-sector group variable. When balancing on this second distribution, applying a standard caliper ensures that firms can only be matched to their nearest neighbor within a given country-sector stratum. However, the absolute distances between nearest neighbors within a stratum remain the same.

[27] Suggested maximum values of standardized differences range from 10 to 25 percent. Thus, taking into consideration additional measures of balance is especially important in case these limits are surpassed (Garrido et al., 2014).

Since standardized differences focus on the comparison of means and are alone not sufficient to judge balancing quality, we proceed with a graphical analysis of the covariate distributions before and after balancing. Figure 9 illustrates the overlap in distributions for our variable of interest, tangible fixed assets (in logs) in 2004.[28] Before applying the steps outlined above, distributions between the two groups, EU ETS and non EU ETS firms, are significantly different. After balancing, however, they are very similar in terms of their mean, variance and skewness.

**Figure 9:** Tangible fixed assets (in logs) in 2004 before and after balancing



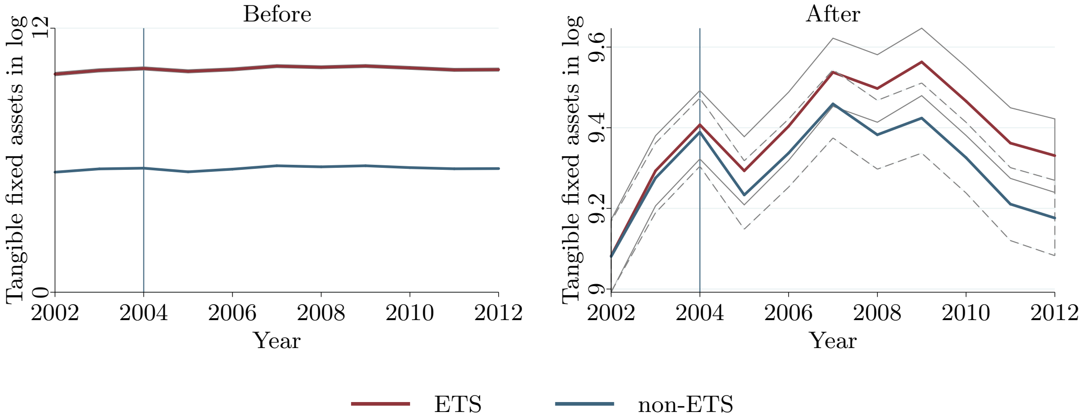

Matching a suitable neighbor to a regulated firm was not possible in all cases: Firstly, data on key financial variables such as tangible fixed assets was not available for all companies in pre-treatment years, leaving us with 325514 companies for matching. Secondly, as expected, some dissimilarities remain. Due to a lack of comparable control firms, we omit 594 treated firms from our sample. For our total sample of 7279 EU ETS firms, we establish a sample that consists of 1321 EU ETS firms and 1321 non EU ETS firms that is balanced in all potential key confounders for the entire pre-2005 period.

As a first intuitive step to look into the effects of the EU ETS on regulated firms' asset bases, we plot the mean of tangible fixed assets (in logs) over time for our groups of EU ETS and non EU ETS firms. Figure 10 (Before) shows that, before matching, both groups differ substantially in the size of their respective asset bases. Next, we assess our sample of matched EU ETS and non EU ETS companies. Figure 10 (After) shows that the design stage has provided us with two groups that are very similar in terms of their pre-treatment asset bases. Also, both groups do not seem to exhibit any different trend behavior previous to 2005. This strongly supports our assumption of a common trend. Most notable though is that after the introduction of the EU ETS in 2005, the levels of tangible fixed

[28] While we only report tangible fixed assets in logs here, the visual impression is essentially similar for all covariates. Additional visualizations are provided in Appendix A.4.

assets evolve differently in treatment and control group. The divergence becomes more apparent from 2008 onwards, which marks the beginning of the second phase of the system.

**Figure 10:** Means over time: Tangible fixed assets in log




## 3.2. Stage II: Causal analysis

Our objective in the second stage is to obtain the average effect of the EU ETS on regulated firms' asset bases (average treatment effect on the treated, ATET). While our general approach of combining matching with DiD follows Heckman et al. (1997) as summarized in Blundell and Dias (2009), our methodology and notation for DiD follows Lechner (2011). Capital letters denote random variables and small letters denote specific values or realizations.

$$\begin{aligned} ATET_t &= E\left[Y_t^1 - Y_t^0 | D = 1\right] \\ &= E\left[E(Y_t^1 - Y_t^0 | X = x, D = 1)| D = 1\right] \\ &= E_{X|D=1}\theta_t(x). \end{aligned} \tag{2}$$

where $D$ is the binary treatment variable, i.e. $d \in 0, 1$. $Y_t^d$ denotes the outcome that would be realized for a specific value of $d$ in period $t$, thus $Y_t^1$ corresponds to the outcome of a firm in the post-treatment period $t$ if it were regulated by the EU ETS. $x$ refers to particular values of random variables $X$.

Since we do not observe post-treatment outcomes for the treated firms if they had not been treated, i.e. the counterfactual $Y_t^1|D = 0$, a set of identifying assumptions must be fulfilled to allow for a causal interpretation of the obtained ATET.

### 3.2.1. Stable Unit Treatment Value Assumption

$$Y_t = dY_t^1 + (1-d)Y_t^0, \ \forall t \in \{0,1\}\,. \tag{3}$$

The Stable Unit Treatment Value Assumption (SUTVA) implies that there are no relevant interactions between members of the population. This assumption requires the absence of spill-over effects or treatment externalities and is notoriously difficult to justify in the context of firm-level data. For one, as matching of treatment and control group becomes more accurate, the more likely it becomes that firms are competitors. Furthermore, if firms are connected in business groups, direct spill-over effects could occur that would render all connected firms unusable as controls. As we discussed in the previous section, we prevent all connected firms from entering the control group.[29]

### 3.2.2. Exogeneity Assumption

$$X^1 = X^0 = X, \ \forall x \in \chi. \tag{4}$$

Assuming that the covariates $X$ are not influenced by the treatment is another standard assumption. However, we refrain from including additional covariates in the DiD estimation after the extensive balancing process outlined in the previous section.

### 3.2.3. Absence of anticipation effects

$$\theta_0(x) = 0; \ \forall x \in \chi. \tag{5}$$

In the context of the EU ETS, this assumption is rather difficult to justify. After a lengthy negotiation process, firms were well aware of the scheduled introduction of the regulation. What remains unclear is whether or not there was an incentive for firms to prepare in advance. The public discussion process had made it obvious that free allocation would induce windfall profits for many firms, so self-selection could have happened in both directions. Either way, in our setup it would lead us to underestimate the true effect of the regulation.

### 3.2.4. Common Trend Assumption

$$\begin{aligned} &E(Y_1^0|X=x, D=1) - E(Y_0^0|X=x, D=1) \\ &= E(Y_1^0|X=x, D=0) - E(Y_0^0|X=x, D=0) \\ &= E(Y_1^0|X=x) - E(Y_0^0|X=x); \ \forall x \in \chi. \end{aligned} \tag{6}$$

[29] This restriction is rather strong because connected companies are generally better candidates for matching if spill-overs are assumed to be nonexistent.

The key assumption of the DiD-approach, as outlined by Lechner (2011), is the common trend assumption. Figure 10 (After) illustrates the common trend of tangible fixed assets in treatment and control group in the pre-treatment period (2002-2004). Appendix A.5 does the same for other variables that were included in the balancing process. The assumption implies that, had the treated not been subject to the treatment, both subpopulations defined by $D = 1$ and $D = 0$ would have experienced the same time trends conditional on $X$. Since our balancing process ensures a common trend in all included variables and the selection of variables that enter the process is extensive, unobserved time-variant covariates are likely to be balanced as well.

#### 3.2.5. Common Support Assumption

$$\begin{aligned} &P\left[TD = 1 | X = x, (T, D) \in \{(t, d), (1, 1)\}\right] < 1; \\ &\qquad \forall (t, d) \in \{(0, 1), (0, 0), (1, 0)\}\,;\ \forall x \in \chi. \end{aligned} \tag{7}$$

The last of the key assumptions, common support is required so that the ATET is identified. While we lose some of the treated firms in our balancing process, the remaining sample satisfies this assumption. One issue that has been discussed by Lechner (2011) and Meyer (1995) is the scale dependence of the identifying assumptions. While one could argue that tangible fixed assets in logs in our unmatched data set (Figure 10 (Before)) follow a common trend, the difference in levels highlights that this would no longer be the case if the variable was not transformed in logs. Our balancing process, however, enforces not only common trends but common levels of the variables as well. Consequently, our matched baseline sample does no longer suffer from this shortcoming. To implement the DiD identification strategy we apply the common linear regression model to our balanced sample and since we used one-to-one nearest neighbor matching without replacement, there are no weights used in the final estimation.[30]

$$\begin{aligned} y_{it} = \alpha + \alpha_i + \beta ets_i + \gamma period_t + \delta treat_{it} \\ + \phi_1 mne\_link_{it} + \phi_2 \left[mne\_link_{it} * treat_{it}\right] \\ + \varphi_1 mne\_nolink_{it} + \varphi_2 \left[mne\_nolink_{it} * treat_{it}\right] \\ + \eta[industry_i * year_t] + \epsilon_{it} \end{aligned} \tag{8}$$

Here $y_{it}$ denotes the tangible fixed assets of a given company $_i$ at time $_t$, $\alpha$ is a constant, $\alpha_i$ is the firm-level fixed effect, $treat_{it}$ is the interaction of $ets_i$ and $period_t$, $year_t$ are yearly effects, $industry_i$ are sector-specific effects and $\epsilon_{it}$ stands for the error term. After demeaning the variables using the *within* transformation, the resulting fixed effects model only consists of the yearly effects (unreported) and the interaction terms. Standard errors are clustered at the firm level.

[30] For the future, constructing additional samples with different matching procedures could enhance the robustness of our findings.

# 4. Results

## 4.1. Main results

For our baseline sample we estimate three models: Model one contains the treatment dummy for measuring the ATET. Model two contains both the treatment dummy as well as an interaction term of the treatment effect with global MNE status. Model three employs two interaction terms instead of one, thus allowing us to differentiate between the two types of multinational company structures, i.e. firms that are part of a global MNE with a functional link, and firms that are part of a global MNE without such a link.[31] Since we are using tangible fixed assets in logs, our DiD estimator can be interpreted as the ATET given in percentage terms.

**Table 2:** Baseline effects

| | (1) | (2) | (3) |
|---|---|---|---|
| ETS treatment effect | 0.101*** | 0.112*** | 0.111*** |
| Global MNE and treated | | −0.042 | |
| Global MNE without functional link and treated | | | −0.098** |
| Global MNE with functional link and treated | | | 0.004 |
| Firms (T+C) | 2642 | 2642 | 2642 |

Significance levels: ∗ p <0.10, ∗∗ p <0.05, ∗ ∗ ∗ p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

The empirical results reported in Table 2 indicate that the EU ETS had a strongly significant positive effect on the treated firms' tangible fixed assets in the post-treatment period from 2005 to 2012. For each model, the DiD estimator yields a treatment effect that corresponds to an increase of treated firms' asset bases in the range of 10.1% to 11.2%. Note that the estimates are consistently significant at the 1% level. Interestingly, model 2 results do not indicate that MNE status per se explains a different treatment effect for this subpopulation of our sample. Although at a sizeable negative magnitude of minus 4.2%, the interaction term is insignificant.

The picture becomes clearer by looking at the results obtained from model 3. For MNEs with a functional link, the interaction term effect is not only insignificant but also of very low magnitude (0.4%), indicating that these firms do not behave differently than the remainder of treated firms. However, multinationals without a functional link do exercise a behavior that is significantly different from the rest of the sample. The interaction effect is highly significant at the 1% level and corresponds to a 9.8% decrease in tangible fixed

[31] Appendix A.1 explains our approach to identify a firm's MNE status in detail. Due to the frequency of ownership changes, we identify the firm status in each year. In Appendix A.6 we provide results for constant ownership categories.

assets relative to the remainder of the sample. For model 3, we can thus cautiously interpret these results in the following way: whereas most treated firms increased their tangible fixed asset bases, firms that are part of a multinational network without a functional link did not.

**Table 3:** ETS impact by phase

| | (1) | (2) | (3) |
|---|---|---|---|
| ETS Phase I treatment | 0.056*** | 0.062*** | 0.060*** |
| Global MNE and treated (I) | | −0.027 | |
| Global MNE without functional link and treated (I) | | | −0.065 |
| Global MNE with functional link and treated (I) | | | 0.008 |
| ETS Phase II treatment | 0.128*** | 0.146*** | 0.145*** |
| Global MNE and treated (II) | | −0.061 | |
| Global MNE without functional link and treated (II) | | | −0.124** |
| Global MNE with functional link and treated (II) | | | −0.009 |
| Firms (T+C) | 2642 | 2642 | 2642 |

Significance levels: ∗ p <0.10, ∗∗ p <0.05, ∗ ∗ ∗ p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

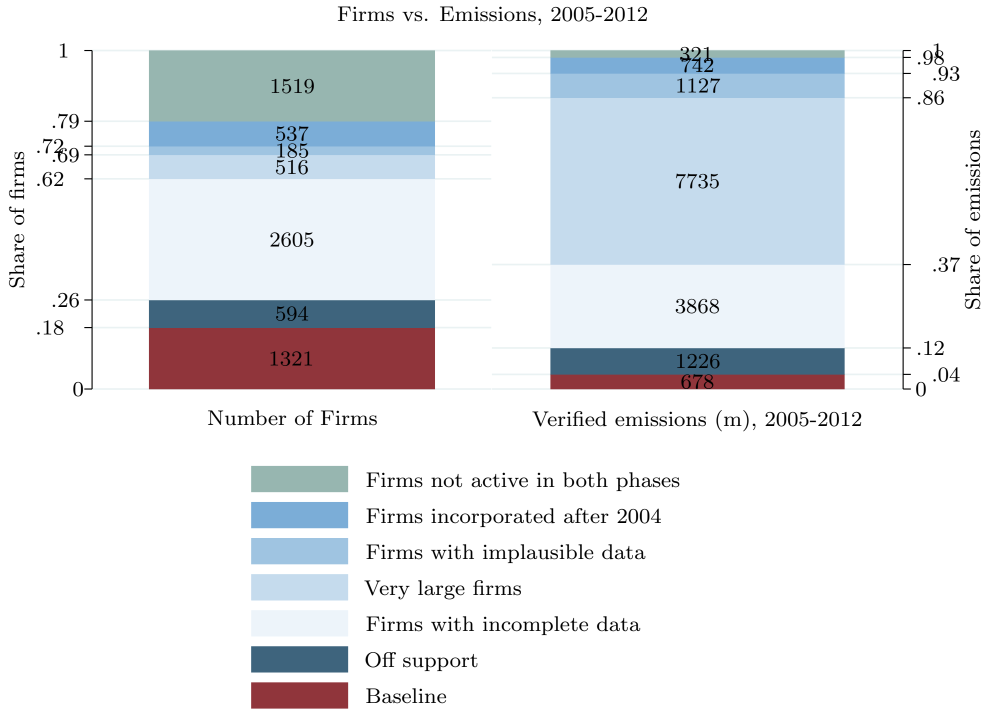
Table 3 shows these effects separately for the two regulatory phases of the EU ETS. The intuition derived from Figure 10 is confirmed in the sense that the ETS treatment effect in phase II is substantially more pronounced. In phase I, for each of the three models, the magnitude of the ATET ranges from 5.6 to 6.2%. In phase II, the treatment effect is considerably higher, ranging from 12.8 to 14.5%. Estimates for both phases are significant at the 1% level.

Again, model 3 results indicate that firms that are part of a global network without a functional link seem to react significantly different to the EU ETS than other firms. In phase II, the interaction term is highly significant at the 1% level and corresponds to a 12.4% decrease in tangible fixed assets relative to the rest of the sample. Hence, the phase II treatment effect for these firms corresponds to an increase in asset bases by only 2.1% (14.5% for other treated firms). For phase I, the magnitude of the effect even points to a slight decrease in assets by -0.5% (6%). However, the estimate for the interaction term is statistically insignificant, which may suggest that multinational companies with global networks still behaved similar to other companies in phase I.

## 4.2. External validity

In section 3.1, we outlined that obtaining a smaller sample that is more suitable for causal analysis comes at the cost of sacrificing a certain degree of external validity. Figure 11 demonstrates this tradeoff. Our baseline sample contains 1.321 (18%) of the 7279 EU ETS firms that we matched to ORBIS. This corresponds to 4% of the total greenhouse gas emissions covered by the system between 2005 and 2012.[32]

**Figure 11:** Baseline - External validity II - Attrition analysis




Furthermore, two groups of EU ETS firms that did not enter our balancing procedure account for an important amount of both firms and emissions. 516 very large firms compose only 7% of our original population, but represent 49% of total emissions. Also, 2605 firms that either did not pass our panel attrition test or did not have any pre-treatment data produce 25% of emissions. The remainder of 594 firms did pass both tests, but we could not find a decent matching partner for them (off support).[33]

We now, in a stepwise manner, relax our assumptions on the importance of accounting

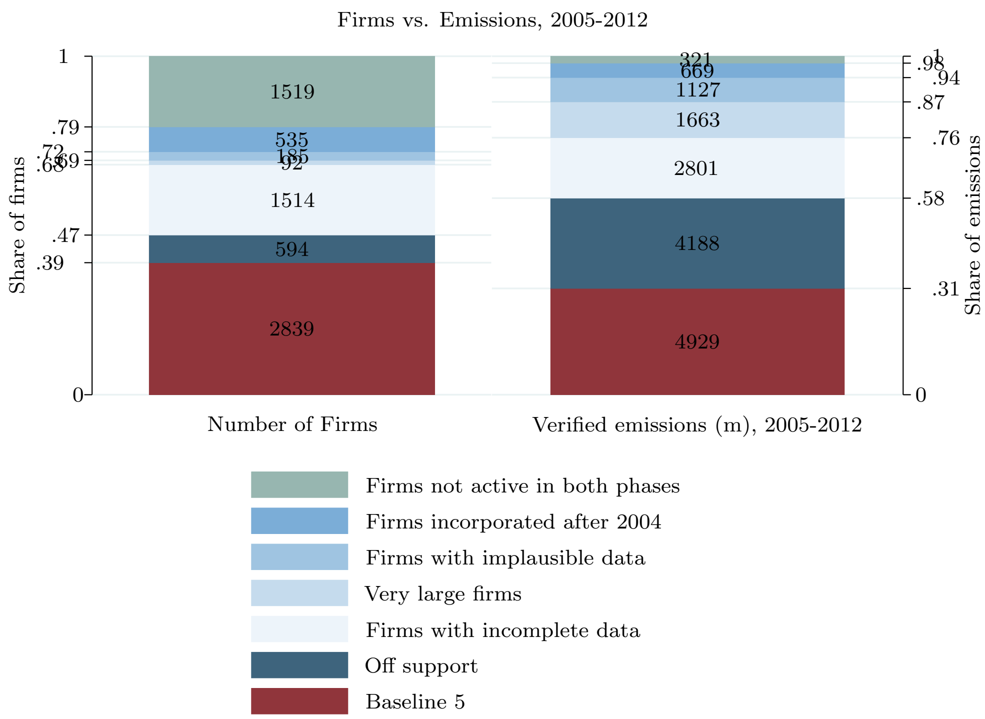
[32] Note that we originally identified 7.279 firms as subject to the EU ETS according to EUTL and ORBIS based information, but fewer are actually relevant for our analysis. 1.519 firms did not report verified emissions in either of the two treatment periods. 537 were incorporated into ORBIS only in the post-treatment period, meaning after the end of 2004. 185 of them did not report plausible financial data (e.g. negative assets or unplausibly large jumps in variable values of consecutive years).

[33] The figures reported here are subject to the order in which we apply the steps of the procedure. Also consider that allowing these firms to enter our design stage would not necessarily mean that we could find a suitable match for them.

for outliers, non-surviving firms, and connected firms, as well balancing on potential confounders in all pre-2005 periods. This allows us to increase our sample size substantially and to investigate if our findings hold in less restrictive samples, but makes the results also more difficult to interpret. In addition, we can assess how excluding certain types of firms affects our results.

In Figure 12, we display the gains in coverage of firms and emissions for our least demanding sample, which requires matching only on covariates in 2004 during the design stage. This sample contains 2839 ETS firms which are responsible for 31% of the relevant emissions. Again, for 594 firms which account for 27% of relevant emissions we do not find a suitable match. Another 1514 firms corresponding to 18% of emissions do not have pre-treatment records for our outcome variable.

**Figure 12:** Baseline 5 - External validity II - Attrition analysis




These findings underline the argument presented in section 3.2: Given the fact that treatment assignment is clearly not random but based on capacity thresholds, it is plausible to find a suitable control group for small and medium sized emitters. In contrast, it can be considered highly unlikely that we would find a sibling for a very big polluter with accordingly high levels of e.g. assets, profits or revenues outside of the EU ETS. These large EU ETS regulated companies are simply too different from untreated firms outside of the EU ETS. Therefore, we focus on a sample of firms where potential confounders are balanced after applying our design stage.

However, we also assess both of these sample in terms of their sectoral and structural composition. Appendix A.8 highlights that both samples are have nearly identical compositions of firm types compared to the raw data. In terms of the sectoral composition, manufacturing firms are slightly overrepresented in our baseline sample while in the second extended sample they are slightly underrepresented. Consequently, the share of emissions that we evaluate with our samples remains representative for the EU ETS with respect to these characteristics.

**Table 4:** Relaxing the sample restrictions

| | (1) Baseline | (2) With large firms | (3) Without restrictions | (4) Simple balancing |
|---|---|---|---|---|
| ETS treatment effect | $0.111^{***}$ | $0.157^{***}$ | $0.133^{***}$ | $0.115^{***}$ |
| Global MNE without functional link and treated | $-0.098^{**}$ | $-0.056$ | $-0.028$ | $-0.023$ |
| Global MNE with functional link and treated | 0.004 | 0.022 | 0.012 | 0.006 |
| Firms (T+C) | 2642 | 2834 | 4720 | 5675 |

Significance levels: $*$ p <0.10, $**$ p <0.05, $***$ p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

Table 4 displays the results for our baseline sample and three samples with fewer restrictions imposed. Going through the columns from left to right, we relax the restrictions stepwise. Sample 2 allows for very large firms, sample 3 for very large firms, non-survivors and connected companies, and sample 4 allows for all these firms to enter the design stage and only requires a balancing on 2004 firm characteristics.

Results on covariate balance obtained after applying the design stage are reported in Appendix A.3, Table 12. Pre-treatment covariates are very well balanced for all four samples with standardized differences well below 10.[34] In the case of the samples with large firms and without restrictions, balance even improves for some covariates to some degree.[35] This can be attributed to the fact that we now are less restrictive about the choice of the matching partner for each treated firm.[36]

In Table 4, for all four samples the DiD estimator yields a positive EU ETS treatment effect corresponding to an increase of asset bases in the range of 11.1 to 15.7%. Also, the estimates given for the EU ETS treatment effect remain highly significant at the 1% level. Interesting to note is that including size outliers into the sample drives the treatment term up from a magnitude of 11.1 to 15.7%. This suggests that a small number of large firms indeed influences the means in both groups post-2005 and may thus lead us to exaggerate the effect for the remainder of the sample. However, this process wears off as we allow

[34] The only exception is the investment ratio in 2003 for the simple balancing sample (-11.5).
[35] This is the case for total assets, tangible fixed asset ratio and operating revenue.
[36] In unreported graphical analysis we find evidence that supports our common trend assumption in these samples; i.e. parallel trends between ETS and non ETS groups in pre-treatment outcomes.

more firms to enter the sample. In sample 4 the magnitude of the effect corresponds to an 11.5% increase in assets.

Noteworthy are the apparent changes for the two interaction terms that are based on the global MNE status. Going in Table 4 from left to right shows that the differential effect found in the baseline, sample 1, for multinational firms without a functional link does not seem to hold for the other samples. The direction of the term is still negative but it becomes smaller in magnitude. For our largest sample, sample 4, the estimate corresponds to a 2.3% decrease in tangible fixed assets relative to the remainder of the sample (sample 1: 9.8%). However, the term is insignificant in samples 2-4. Thus, we cannot conclude that this group of MNEs behaves differently compared to other treated firms for these more sizeable samples.

### 4.3. Robustness

To assess the robustness of the results we need to verify if the main identifying assumptions, unconfoundedness and the stable unit treatment value assumption, are plausible in our specific policy context. One challenge to the unconfoundedness assumption is the potential presence of unobserved covariates that directly affect treatment status and our outcome variable, tangible fixed assets, thus confounding our post-2005 estimates (omitted variable bias). Hence, the question arises if regulated and non-regulated firms that are observationally equivalent are also similar in terms of unobserved characteristics. This can be tested to a certain degree by assessing the results of our design stage.

Table 12 in Appendix A.3 reports balancing results both for financial covariates that entered the design stage and for those additional variables that were not part of the process. As depicted in section 4.2, the simple balancing sample requires balance only on 2004 firm characteristics. We not only achieve a very good balance for each covariate in 2004, but firms in both groups are also very similar for their respective covariates in 2003 and 2002. For instance, we achieve an excellent balance for tangible fixed assets in 2003 and 2002 (standardized differences -0.4 and 0.5 respectively), although our pre-treatment outcome only entered the design stage for the year 2004. This finding highlights that achieving covariate balance for a given year is conceivable to produce balance in these covariates for other pre-treatment years that we do not observe, i.e. years previous to 2002.

However, unobserved covariates that are not part of the design stage at any point in time might still exhibit meaningful imbalances. For the purpose of looking into this aspect, we assess the balance for a covariate that did not enter the design stage at all: the number of employees in logs. Distributions for this covariate are actually very similar between the two matched groups.[37] This gives us some indication that balancing on observables likely

[37] Results are available from the authors upon request.

produced a sample that is actually balanced in at least some unobserved covariates as well.

Another type of omitted variable bias may arise if the level of information we capture through our covariates is not precise enough. Again, we may compare two types of firms that are observationally almost identical, but here, an important layer of information is missing which then may explain the treatment effect we attribute to the EU ETS. This could be the case particularly for non-financial variables, i.e. the definition of sector affiliation and firm structure.

In terms of sector affiliation, we enforced exact matches on the country-sector level, employing the 2-digit NACE Rev. 2 codes. However, although this gives us a perfect balance in key covariates, the information may not be refined enough. For instance, the category "20 - Manufacture of chemicals and chemical products", contains both firms that mainly produce synthetic rubber but also firms that manufacture paints and coatings. What if rubber producers were systematically more likely to be subject to the EU ETS and at the same time possess different patterns with regards to their investment in asset bases than paint manufactures? This could create an imbalance between treated and control firms with respect to a key confounder and thus potentially explain the treatment effect.

In terms of firm structure, our research question rests on the very idea that being part of a network enables firms to adapt their asset bases more flexibly, e.g. by shifting resources more easily within these networks. If such firms were more likely to be regulated by the EU ETS, this again could obscure our estimate of the treatment effect. For our baseline sample, the design stage does achieve a certain degree of balance for these firm characteristics, i.e. if a firm is part of a global network. However, one could argue that these covariates are very relevant and that imbalances might obscure our estimates. For instance, firm structure may not only be a confounder by itself, but instead also be associated with other potential unobserved confounders that we may not balance out yet entirely, such as access to capital markets, or management quality and dimensions like overall performance and growth prospects of a company.

In order to address these potential sources of bias, we further refine the exact matching approach that we employed for the design stage as outlined in section 3.1 and are more restrictive about which EU ETS and non EU ETS firms are allowed to be matched. Table 5 compares our baseline results with the results obtained for the two samples with more demanding design stage constraints. Sample 2 again requires exact matching on the sector-country level. However, firms can now only be matched within smaller, 3 digit NACE subsectors. Sample 3 requires exact matching on the sector-country-firm type level. Hence, ETS and non EU ETS firms can only be matched if they not only operate in the same country and NACE 2 digit sector, but also had the same firm structure in 2004, i.e. either had no ownership links at all (independent companies) or were part of the same kind of firm network (national, EU, global with or without a functional link).

We report results on covariate balance in Table 12 in Appendix A.3. Compared to our baseline sample we sacrifice some degree of covariate balance and sample size for attaining samples that are more stringent with regards to sector affiliation or firm structure. For sample 2 (NACE 3 digit), standardized differences are above 10 for two covariates in 2002-2004 (tangible fixed asset ratio, operating revenue in logs) and the sample merely comprises 1902 firms (951 treated). For sample 3 (balanced MNE status), the standardized difference is above 10 for one covariate in 2002-2004 (total assets in logs) while the sample still contains 2504 firms (1252 treated). This attrition in both samples can be attributed to the fact that it is less probable to find a suitable matching partner within smaller strata. Despite this, unreported graphical analysis still finds evidence supporting the common trend assumption; i.e. parallel trends in pre-treatment outcomes between treated and control groups. If this assumption holds, results may still be considered credible.

**Table 5:** Tightening the sample restrictions

| | (1)<br>Baseline | (2)<br>NACE 3-digit | (3)<br>Balanced MNE status |
|---|---|---|---|
| ETS treatment effect | $0.111^{***}$ | $0.145^{***}$ | $0.135^{***}$ |
| Global MNE without functional link and treated | $-0.098^{**}$ | $-0.097^{*}$ | $-0.095^{**}$ |
| Global MNE with functional link and treated | 0.004 | 0.018 | 0.026 |
| Firms (T+C) | 2642 | 1902 | 2504 |

Significance levels: $*$ p <0.10, $**$ p <0.05, $***$ p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

In Table 5, the DiD estimator yields a positive EU ETS treatment effect corresponding to an increase of asset bases in the range of 11.1% to 14.5%. Note that these estimates are all highly significant at the 1% level. For samples 2 and 3 the magnitude of the effect is 2.4-3.4 percentage points higher than in the baseline. The interaction term for multinational firms without a functional link remains very similar in magnitude across all three samples. Note however that for Sample 2 (NACE 3 digit) the term is only significant at the 10% level. These results generally support the evidence provided by our baseline estimations, albeit we do observe some modest differences in terms of the magnitude of effects and their respective significance levels.[38] We can therefore have some confidence that our initial results are not the product of a bias arising from insufficient balance in terms of observable sector affiliation or firm structure.

As a final robustness check, we investigate whether our findings for the level of tangible

[38]Consider that we cannot determine whether these differences stem from a better balance in sector or firm structure related covariates, or other differences in sample composition.

fixed assets are also reflected in estimations with alternative outcome variables that capture asset growth and are therefore closely linked to the rate of firm-level investment. First, we construct the normalized growth rate in tangible fixed assets, similar to the approach in Zaklan (2016) and Greenstone (2002). It is defined as

$$\theta_1(TFAS)_{it} = \frac{TFAS_{it} - TFAS_{i(t-1)}}{(TFAS_{it} + TFAS_{i(t-1)})/2}. \tag{9}$$

Second, we utilize the growth in tangible fixed assets normalized to the total asset base of a given company (investment ratio). It is defined as

$$\theta_2(TFAS)_{it} = \frac{TFAS_{it} - TFAS_{i(t-1)}}{(TOAS_{it} + TOAS_{i(t-1)})/2}. \tag{10}$$

Both ratios are always within the [-2, 2] interval, where values approaching 2 indicate a firm entry (initial investment into tangible fixed assets) and values approaching -2 a firm exit (decrease of tangible fixed assets to zero). Similar to the log transformation we use in our original model, these ratios have the virtue that they reduce the influence of outliers with very large assets, thus ensuring that the magnitude of the treatment effect is not driven by these firms. Third, in unreported analyses, we also utilize first differences of tangible fixed assets in logs as an outcome variable that measures absolute changes as well as TFAS growth rates to measure relative changes.

**Table 6:** Baseline effects for $\theta_2(TFAS)_{it}$

| | (1) | (2) | (3) |
|---|---|---|---|
| ETS treatment effect | 0.004 | 0.004 | 0.004 |
| Global MNE and treated | | 0.002 | |
| Global MNE without functional link and treated | | | −0.000 |
| Global MNE with functional link and treated | | | 0.004 |
| Firms (T+C) | 2642 | 2642 | 2642 |

Significance levels: ∗ p <0.10, ∗∗ p <0.05, ∗ ∗ ∗ p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

Table 6 shows the results we obtain for our baseline sample with $\theta_2(TFAS)_{it}$ as the dependent variable. Judging by magnitude and significance levels, the EU ETS does not seem to have caused a relevant change for this outcome variable. Results for $\theta_1(TFAS)_{it}$ and First differences of TFAS are similar. Investigating the effects per phase does point to a deceleration of investment for multinational firms without a functional link in phase one, but the results remain inconclusive overall.

These insignificant results could be attributed to the fact that the ratio measures the absolute change in tangible fixed assets relative to the average total size of the company (average total assets). Apparently, the relative change in tangible fixed assets between our treatment and control group we observed in our original model is not enough to shift this relation, i.e. the EU ETS did not shift it decisively. Note that unreported graphical analysis indicates that the EU ETS does not seem to have had an impact on total assets.[39]

### 4.4. Heterogeneity of treatment effects

Until now we have looked into the causal impacts of the EU ETS on regulated firms' asset bases with a particular focus on the question whether companies that are part of global firm networks adapt differently to the regulation. However, it is plausible to assume that these reactions might also differ systematically across different sectors. An obviously important differentiation between sectors is their respective exposure to international competition: Whereas energy companies compete mostly nationally and within common electricity grid structures in Europe, manufacturing firms that produce tradable goods are often subject to intense competition on international markets.

To assess if the treatment effects we analyzed in the previous subsections indeed manifest themselves heterogeneously across sectors and firm types, we apply both stages, design stage and causal analysis, separately to three subgroups of firms: firms pertaining to the manufacturing sector, manufacturing firms considered to be at high risk of carbon leakage according to the carbon leakage list (CLL) of the EU, and energy companies. Balancing results for these subsamples are displayed in Appendix A.3, Table 11. For both the manufacturing and the energy sample most covariates are very well balanced with standardized differences well below 10, although covariate balance inevitably suffers in the smaller samples.[40]

**Table 7:** Effect heterogeneity

| | (1) Baseline | (2) Manufacturing | (3) CLL only | (4) Energy |
|---|---|---|---|---|
| ETS treatment effect | 0.111*** | 0.121*** | 0.223*** | 0.130** |
| Global MNE without functional link and treated | −0.098** | −0.108*** | −0.076* | −0.153 |
| Global MNE with functional link and treated | 0.004 | 0.022 | 0.069 | −0.080 |
| Firms (T+C) | 2642 | 1670 | 1184 | 596 |

Significance levels: * p <0.10, ** p <0.05, * * * p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

[39] We observe that parallel trends in means of log total assets between the treated and control groups observed pre-treatment also persist in the post-2005 period.

[40] We also find graphical evidence which lends support to the assumption of a common trend in average outcomes for all three subsector samples.

Secondly, we compare the estimation results for these subsamples with our baseline sample. This is reported in Table 7. For all of the three subsamples, the ETS treatment effect remains highly significant and at a sizeable magnitude.[41] The DiD estimates point towards a positive effect, i.e. an increase of asset bases ranging from 12.1 to 22.3%.
The interaction term for multinational firms without a functional link remains negative for all subsamples with magnitudes in the range of -10.8 to -15.3%. Important to consider here are the substantial differences in significance levels. Whereas the effect is highly statistically significant for manufacturing firms at the 1% level (baseline: significant at the 5% level), for the CLL-only sample the significance is at the 10% level. For energy firms the interaction term is insignificant.

Overall, the results for the baseline sample seem to be driven by the behaviour of manufacturing firms in general and firms at risk of carbon leakage in particular. The results indicate that, whereas most treated manufacturing firms increased their tangible fixed asset bases by 12.1% compared to the control group, firms that are part of a multinational network without a functional link did not. For energy and/or trade intensive firms (CLL-only sample) the effect of the EU ETS seems to be substantially larger. Note that sectors on the EU's "Carbon Leakage List" tend to receive far more emissions allowances for free than other sectors. This could indicate that, although in theory these firms may be at a higher risk of relocating resources internationally, overallocation has indeed worked in terms of preventing this from happening to a certain degree. However, as already described, these effects could be the result of imbalances in important confounders. In this particular sample, EU ETS firms seem to be systemically bigger than their non EU ETS counterparts in a number of pre-treatment characteristics, which could have led to an overestimation.

With regards to energy companies, we cannot attest that multinational firms without a functional link reacted differently than other treated firms. For this sample, results point towards a 13% increase in asset bases for all firms. However, the small sample size suggests caution when interpreting the results.

Thirdly, due to these challenges for the two smaller subsamples (energy companies and firms with a supposedly high relocation risk), we now focus on the more reliable manufacturing sample and test if the results hold under more relaxed and more restrictive conditions. Table 8 reports estimation results for the manufacturing (here: baseline) sample and three manufacturing samples with fewer restrictions. This means that analogous to section 4.2 (external validity), we we contrast the original effects with estimates from larger, more representative, albeit potentially less consistent samples. Balancing results for these samples are presented in Appendix A.3, Table 13 (columns 4 to 6). Despite some differences compared to the manufacturing baseline (column 3), most of the covariates are

[41] Manufacturing and CLL-only samples: Significant at the 1% level. Energy: Significant at the 5% level.

well balanced.

**Table 8:** Relaxing the sample restrictions - manufacturing

| | (1) Baseline | (2) With large firms | (3) Without restrictions | (4) Simple balancing |
|---|---|---|---|---|
| ETS treatment effect | 0.121*** | 0.147*** | 0.134*** | 0.133*** |
| Global MNE without functional link and treated | −0.108*** | −0.096** | −0.046 | −0.051 |
| Global MNE with functional link and treated | 0.022 | 0.040 | 0.050 | 0.008 |
| Firms (T+C) | 1670 | 1746 | 2860 | 3352 |

Significance levels: * p <0.10, ** p <0.05, * * * p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

Going from left to right in Table 8, we relax the restrictions stepwise. Overall, results are very similar to those obtained previously for the baseline sample (Section 4.2, Table 4. The main distinction is that in the manufacturing sample the interaction effect for multinational firms without a functional link does hold for Sample 2, which contains very large firms. In this particular sample, we can cautiously interpret the estimates in a sense that while the remainder of the treated firms did increase their tangible fixed asset bases by 14.7%, treated MNEs without a functional link did not.

**Table 9:** Tightening the sample restrictions - manufacturing

| | (1) Baseline | (2) NACE 3-digit | (3) Balanced MNE status |
|---|---|---|---|
| ETS treatment effect | 0.121*** | 0.157*** | 0.106*** |
| Global MNE without functional link and treated | −0.108*** | −0.145*** | −0.131*** |
| Global MNE with functional link and treated | 0.022 | 0.016 | 0.043 |
| Firms (T+C) | 1670 | 1050 | 1574 |

Significance levels: * p <0.10, ** p <0.05, * * * p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

Next, we test the results we find for the manufacturing baseline sample against more restrictive conditions. For this purpose, Table 9 displays estimation results for the manufacturing sample (here: baseline), a sample that enforces exact matches on the country-sector level employing NACE 3-digit codes, and a sample that requires identical country-sector-firm type combinations. Similar to the results for the sector-wide sample (section 4.3 Table 5), both samples seem to confirm the baseline findings. Noteworthy are the different magnitudes of the terms. For these two samples, it seems that treated multinational companies without a functional link react very differently compared to other treated firms. However, these results should be interpreted with caution. In both samples, balance in covariate distributions between treated and not treated firms had to be sacrificed for several covariates (see Appendix A.3, Table 13). For the NACE 3-digit sample, only pre-2005 tangible fixed assets are balanced.[42]

Table 10: Tightening the sample restrictions - manufacturing

| | (1) Baseline | (2) NACE 3-digit | (3) Balanced MNE status |
|---|---|---|---|
| ETS treatment effect | 0.018*** | 0.018*** | 0.014*** |
| Global MNE without functional link and treated | 0.006 | 0.007 | 0.005 |
| Global MNE with functional link and treated | 0.007 | 0.009 | 0.010 |
| Firms (T+C) | 1670 | 1050 | 1574 |

Significance levels: * p <0.10, ** p <0.05, * * * p <0.01

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

While these findings for the manufacturing sector are rather robust for tangible fixed assets in logs as the outcome of interest, our previous findings for the baseline sample suggest that they might not be for transformed outcome variables. Results for the manufacturing baseline sample and the two more restrictive samples with tangible fixed growth normalized to the total asset base as the dependent variable are presented in Table 10. Unlike for our initial baseline sample, estimates for the manufacturing sector suggest that here the EU ETS did contribute to an acceleration of normalized asset growth.[43] For all three samples, estimates indicate an acceleration of growth for all treated firms compared to the control group. Moreover, these results can be extended to all larger, less restrictive but more representative manufacturing samples, albeit there are some differences in magnitude.

Noteworthy is that we do not find any evidence suggesting a different behavior of treated multinational firms in this setup. This should be taken into consideration when interpreting our finding for our original outcome variable, tangible fixed assets (in logs).

## 5. Conclusion

Results for the log of tangible fixed assets suggest that the EU ETS had a positive impact on the regulated firms' asset bases across sectors and firm types. This effect is very robust in the manufacturing sector. For a characteristic subgroup of multinational firms, the impact is substantially smaller. This may hint at a shift of investment priorities within some MNEs. This effect is robust with respect to different matching procedures but not corroborated in additional estimations with an alternative outcome variable.

[42] Again, we do find graphical evidence supporting the common trend assumption.

[43] This is also the case for the CLL-only sample. We do not find any evidence for an acceleration in asset growth for the energy sample.

## References


Abrell, J., Ndoye, A., and Zachmann, G., 2011. Assessing the impact of the eu ets using firm level data.

Aichele, R. and Felbermayr, G., 2015. Kyoto and carbon leakage: An empirical analysis of the carbon content of bilateral trade. *Review of Economics and Statistics*, 97(1):104–115.

Albrizio, S., Zipperer, V., Botta, E., and Koźluk, T., 2014. *OECD Economics Department Working Papers*, volume 1176.

Ambec, S., Cohen, M. A., Elgie, S., and Lanoie, P., 2013. The porter hypothesis at 20: can environmental regulation enhance innovation and competitiveness? *Review of Environmental Economics and Policy*, 7(1):2–22.

Blundell, R. and Dias, M. C., 2009. Alternative approaches to evaluation in empirical microeconomics. *Journal of Human Resources*, 44(3):565–640.

Borghesi, S., Cainelli, G., and Mazzanti, M., 2015. Linking emission trading to environmental innovation: Evidence from the italian manufacturing industry. *Research Policy*, 44(3):669–683.

Calel, R. and Dechezleprêtre, A., 2016. Environmental policy and directed technological change: Evidence from the european carbon market. *Review of Economics and Statistics*, 98(1):173–191.

Chan, H. S., Li, S., and Zhang, F., 2013. Firm competitiveness and the european union emissions trading scheme. *Energy Policy*, 63(C):1056–1064.

Convery, F. J., 2009. Origins and development of the eu ets. *Environmental and Resource Economics*, 43(3):391–412.

Cowie, J. R. and Heathcott, J., 2003. *Beyond the ruins: The meanings of deindustrialization*. Cornell University Press.

Dechezleprêtre, A., Gennaioli, C., Martin, R., and Muûls, M., 2015. Searching for carbon leaks in multinational companies.

Dehejia, R. H. and Wahba, S., 1999. Causal effects in nonexperimental studies: Reevaluating the evaluation of training programs. *Journal of the American Statistical Association*, 94(448):1053–1062.

———, 2002. Propensity score-matching methods for nonexperimental causal studies. *Review of Economics and Statistics*, 84(1):151–161.

Ellerman, A. D., Marcantonini, C., and Zaklan, A., 2016. The european union emissions trading system: Ten years and counting. *Review of Environmental Economics and Policy*, 10(1):89–107.

Garrido, M. M., Kelley, A. S., Paris, J., Roza, K., Meier, D. E., Morrison, R. S., and Aldridge, M. D., 2014. Methods for constructing and assessing propensity scores. *Health Services Research*, 49(5):1701–1720.

Greenstone, M., 2002. The impacts of environmental regulations on industrial activity: Evidence from the 1970 and 1977 clean air act amendments and the census of manufactures. *Journal of Political Economy*, 110(6):1175–1219.

Greenstone, M., List, J., and Syverson, C., 2012. *The Effects of Environmental Regulation on the Competitiveness of U.S. Manufacturing*. Cambridge and MA: National Bureau of Economic Research.

Hanna, R., 2010. Us environmental regulation and fdi: Evidence from a panel of us-based multinational firms. *American Economic Journal: Applied Economics*, 2(3):p 158–189.

Heckman, J. J., Ichimura, H., and Todd, P., 1998. Matching as an econometric evaluation estimator. *Review of Economic Studies*, 65(2):261–294.

Heckman, J. J., Ichimura, H., and Todd, P. E., 1997. Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The Review of Economic Studies*, 64(4):605–654.

Jaraite, J., Jong, T., Kažukauskas, A., Zaklan, A., and Zeitlberger, A. C., 2013. Matching eu ets accounts to historical parent companies. a technical note.

Koch, N. and Basse Mama, H., 2016. European climate policy and industrial relocation: Evidence from german multinational firms. *SSRN Electronic Journal*.

Lechner, M., 2011. The estimation of causal effects by difference-in-difference methods. *University of St. Gallen, Discussion Paper*, 2010-28.

Martin, R., Muûls, M., de Preux, Laure B., and Wagner, U. J., 2015. On the empirical content of carbon leakage criteria in the eu emissions trading scheme. *Ecological Economics*, 105:78–88.

Martin, R., Muûls, M., and Wagner, U. J., 2016. The impact of the european union emissions trading scheme on regulated firms: What is the evidence after ten years? *Review of Environmental Economics and Policy*, 10(1):129–148.

Meyer, B. D., 1995. Natural and quasi-experiments in economics. *Journal of Business & Economic Statistics*, 13(2):151–161.

Naegele, H. and Zaklan, A., 2016. Europäischer emissionshandel: Besonderheiten im verhalten kleiner unternehmen. *DIW Wochenbericht*, 83(9):171–179.

Nicola, S. and Andresen, T., 21.01.2014. Eu must contain energy costs or risk 'deindustrialization'. *Bloomberg*, 2014.

Petrick, S. and Wagner, U. J., 2014. The impact of carbon trading on industry: Evidence from german manufacturing firms.

Porter, M. E. and van der Linde, Claas, 1995. Toward a new conception of the environment-competitiveness relationship. *The Journal of Economic Perspectives*, 9(4):97–118.

Reinaud, J., 2008. Issues behind competitiveness and carbon leakage. focus on heavy industrys. In *IEA information paper, International Energy Agency, OECD/IEA*.

Rosenbaum, P. R. and Rubin, D. B., 1983. The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1):41–55.

———, 1985. Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *The American Statistician*, 39(1):33.

Vorholz, F., 21.06.2007. Willkommen daheim. *DIE ZEIT*, 2007(26):27.

Wagner, U. J., Muûls, M., Martin, R., and Colmer, J., 2014. The causal effects of the european union emissions trading scheme - evidence from french manufacturing plants.

Zaklan, A., 2016. Free allocation and the endowment effect in cap-and-trade systems: Evidence from the european electricity sector. Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel - Session: Energy Markets. Kiel und Hamburg: ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft.

# A. Appendix

## A.1. Data

### A.1.1. EU ETS and other data

To construct our data set of firms subject to the EU ETS, we combine data from several sources. Data on the EU ETS is provided by the European Union Transaction Log (EUTL). This registry contains plant-level information on emissions and location.[44] Each plant is linked to an identification number that is unique for each country as well as the national identification number of the firm that owns it. In order to assess the quality of our approach of identifying global ultimate owners (GUOs) that are linked to the EU ETS, we also use the dataset provided by Jaraite et al. (2013). This file links EU ETS plants to their respective GUOs for the period 2005-2007.[45] We merge plant-level data on emissions, location and reference GUOs via the combination of installation identifier and country ISO code. In order to match this information on a company level with ORBIS, national identification numbers contained in both the EUTL and ORBIS are employed. In ORBIS, up to 12 kinds of different identifier types exist for each country. Hence, systematic errors in the EUTL are identified and corrected to make national identifiers compatible with the country specific formats in ORBIS. In some cases, companies could not be tracked via their national identifier and were matched via their name. In a few cases, identifiers and/or names were corrected manually based on the information found in ORBIS.

We successfully match 8.218 out of all 8.578 companies (96%) that hold installations regulated by the EU ETS as of March 2014. This corresponds to 14.507 out of a total of 15.043 plants (96%). We then retrieve the BvD identification number (bvdid), which is the unique identifier in ORBIS. The remainder of 360 companies (536 plants) could not be matched: In some cases, companies can simply not be found in ORBIS or their bvdids are not available. In others, the exact firm cannot be identified due to incomplete or inconclusive information in ORBIS. Many of the not-matched entries are hospitals, governmental agencies or universities.

In order to ensure correct matches, we run several consistency tests by comparing the

[44]The document "List of Stationary Installations in the Union Registry" contains all plants under the EU ETS as of February 27, 2014, national identifiers of their direct owners as well as contact data on their geographic location. It can be retrieved from http://ec.europa.eu/clima/policies/ets/registry/documentation_en.htm. The document "Classification of installations in the EUTL Registry based on the NACE 4 statistical classification" contains plant-level information on allocated, surrendered and verified emissions per year as well as contact data. It was constructed by the European Commission in preparation for the carbon leakage list for 2015-2019, released on March 11, 2014 and is available at http://ec.europa.eu/clima/policies/ets/cap/leakage/studies_en.htm.

[45]The "Ownership Links and Enhanced EUTL Dataset" covers phase I and II of the EU ETS and can be downloaded at http://fsr.eui.eu/EnergyandClimate/Climate/EUTLTransactionData.aspx.

companies' contact information between EUTL and ORBIS. For 98.2 % of the matched sample, the information between both sources is consistent. For 1.8% of the sample, part of the information differs. This is mostly related to changes in company names or mergers and acquisitions. In very few cases (57), a company could only be uniquely identified by the combination of its national identifier and further contact information.[46] Overall, results indicate a very high matching quality.

The matched firms are then reduced to those active in phase I or II of the EU ETS, using the emission data from the EUTL as an indicator of activity. Based on the matched bvdid we then add the financial information from ORBIS and correct for some obvious data errors, which results in a final sample of 7279 firms.[47]

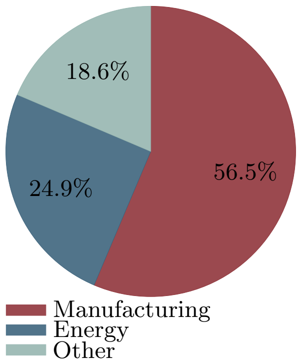



**Figure 13:** Firms by sector

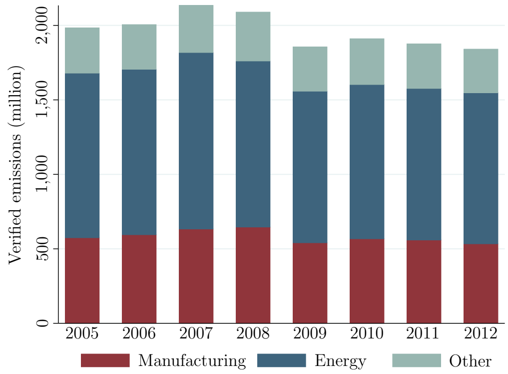



**Figure 14:** Verified emissions by sector

While firms in the manufacturing sector account for 56.5% of affected entities, firms in the energy sector only represent 24.9% of all regulated firms (Figure 13). However, in terms of verified emissions the picture looks different. Figure 14 splits the sum of verified emissions per year by the same sectoral criteria, revealing that the quarter of firms in the energy sector emit more than half of all verified emissions.

[46]This applies when companies are affiliates or branches and are thus registered under the same national identifier in ORBIS. They can be uniquely identified if they differ in terms of their contact information. However, we cannot account for affiliates that are not contained in ORBIS. In these rare cases it might happen that we wrongly match an affiliate in ORBIS that has the exact same contact and identification data as the correct affiliate outside ORBIS.

[47]Note that we rely on information from 2014 to identify the firms regulated by the EU ETS in phase I and II. It does include firms which were only active in phase I, but could exclude firms that ceased operations. We have no yearly data for national identification numbers or firm names.

### A.1.2. ORBIS financial data

We use the ORBIS database maintained by the commercial data provider Bureau van Dijk (BvD) to obtain financial information. ORBIS is a global firm-level database that harmonizes financial data into a global standard format, allowing for cross-country comparisons. To that extent, we greatly rely on the mechanics of the database. Given the scope of the data, we can only rely on algorithmic cleaning processes. Certain unique features of ORBIS have to be taken into account when preparing the data for analysis. For one, the data is updated weekly. This means that no two ORBIS bulk downloads are identical unless they were extracted in the same week. The financial data we use was extracted by Bureau van Dijk in the last week of November 2015 according to our own specifications. Included are all firms above a turnover of one million Euro, total assets of 2 million Euro or a total number of 15 employees in any period between 2005 and 2015, which amounts to a sample of around 12.5 million firms.

The original raw data did not have a yearly encoding, so we first construct one using the 31st of March of the following year as deadline. Next, we split the data by filing type. Data originates either from annual reports or local registry filings and we chose to only use local registry filings to ensure a high data quality. ORBIS also provides financial data in consolidated, unconsolidated or limited form. We only use unconsolidated financial information because consolidated information can comprise data from several firms.[48] Data classified as "limited" is generally of low quality and therefore discarded. We then reduce the data to the period from 2002-2012, because we have very little data before 2002 and we limit the scope of our analysis to the first two phases of the EU ETS. After this selection process we end up with unbalanced panel data for over 5.76 million firms from 1999-2015. Next, we drop all firms that were incorporated after 2004 because we can not use them for our analysis. Since ORBIS has grown quite substantially over the last decade, this deletes 23.5% of all observations and further reduces our dataset to 3.66 million firms. Finally, we limit the data to countries and sectors that are theoretically regulated because only firms that fulfill this requirement can be considered appropriate matches for a control group. Our final dataset consists of an unbalanced panel of 1.7 million firms and 14.5 million observations.

As mentioned before, ORBIS is updated frequently and strives to provide accurate information at the time of extraction. This is a challenge for the empirical researcher, because historical information in ORBIS *also* depends on the vintage of data extraction. This has positive and negative side effects. On the positive side, with time historical information in ORBIS can become more accurate because updates can change information in previous years and add information about the past that was not in the database before. However,

[48] The distinction between consolidated and unconsolidated data is rather important when dealing with business groups, because one firm can have several sets of data.

historical information can also be overwritten and restructured, as becomes apparent in the frequent changes of firms' Bureau van Dijk identification number (bvdid) that have to be corrected for.[49]

A major issue that needs to be corrected for relates to mergers and acquisitions of firms, because it is hard to determine what happened to a firm in cases where identifiers change. Moreover, the financial data does not indicate separately whether any special events happened to a firm in any given year. We try to limit the impacts of these events with an algorithm that identifies anomalies in the data. Essentially we transform all variables of interest into growth rates, then calculate the yearly distribution per variable and flag the 0.1% largest jumps per year per variable. A firm with one such event is then discarded completely. This method eliminates a wide range of data errors as well as firms that were subject to very special transformations (which we assume not to have anything to do with the EU ETS), but does not eliminate firms that are merely very large or growing very fast.[50] Firms with values that are implausible by definition (such as negative values for total assets) are included in the jump outlier category. We encode size outliers as being part of the 1% largest firms and deal with them separately. Applying the jump outlier correction affects 5.35% of observations and 5.25% of firms. Applying the size outlier category afterwards affects 2.21% of observations and 2.24% of firms. Outliers are calculated yearly, but firms are excluded entirely if they reported at least one anomaly.

### A.1.3. ORBIS ownership data

Aside from the financial data, we also use ORBIS ownership information to identify business groups. ORBIS remains the only available data source that allows for the identification and tracking of ownership relationships across time and space with, in principle, global reach. Similar to the financial information, ownership data is also provided on a most-recent basis and considerable efforts are required to make the data usable for empirical research.

To construct the ownership structures, we first extract data for all firms in ORBIS above a chosen threshold for small companies. Our sample of firms drawn from the database are all firms above a turnover of one million Euro, total assets of 2 million Euro or more or a total number of at least 15 employees in any period between 2005 and 2015. We enhance this sample of 12.5 million firms with the first-level top shareholders of these firms in any period as well as their current subsidiaries, regardless of any other criteria.[51] Our selection

[49] We were able to download data for all changes made until August 2015. A total of 45.22 million unique changes to bvdids occurred, many identification numbers were also changed repeatedly. We use this data to update all bvdids. Changes can occur in cases of mergers and acquisitions, but also when data providers change their reporting structure.

[50] Yearly threshold values are driven entirely by data outliers.

[51] Subsidiaries are only available in ORBIS on a most-recent basis. We add all current subsidiaries as of August 2015 to this sample regardless of their size. Note that these firms, to be an addition to our

of firms for which we extract ownership information then adds up to a total number of 14.4 million firms. For these firms we then manually downloaded the available ownership data in batches of 25000 firms for each year from 2002-2014. All of our exports took place between January and August 2015 and were verified against a unified backbone identification dataset. To minimize human error we relied heavily on a feature of the database which allows the storage of export profiles, thus making sure that the settings did not change between exports. Using several specialized algorithms, we also corrected for remaining human errors in this process, re-appended the data and verified its integrity to the best of our ability. Changes in bvdids were corrected at each step and information was updated.[52]

Only 3.2 million firms have a top shareholder in at least one period between 2002 and 2012 and only a tiny share of firms has constant data. Furthermore, ownership data has been added to ORBIS over time and is growing continuously. Only a fraction of information is available for the earlier periods.

We then use this information to construct chains of ownership. Following a similar methodology as Jaraite et al. (2013), we link firms with more than 50.01% ownership shares until we reach the highest level. We are thus able to identify global ultimate owners, but can also fully map business group structures. Since we repeat this process yearly, we can also identify structural changes over time. The resulting structures remain highly complex, and for the purpose of our analysis we chose to categorize business groups in several different types:

- *Independent firms*
  All firms without ownership data are considered to be independent firms. Consequently, even firms placed on top of corporate hierarchies are not considered to be independent, but firms which are part of a corporate network but do not report ownership information are.

- *National business groups*
  The smallest corporate structures typically consist of one firm controlling another firm via a majority share with both firms being located in the same country. Fully national business groups can be large as well, but typically internationalization occurs relatively early. Any firm connected to such a business group is referred to as national business group (firm).

original raw sample, have to be either smaller than our initial selection or empty in terms of their financial information and are thus only considered to close gaps in our chains of ownership.

[52] As outlined above, changes of the bvdid happen quite frequently and for a variety of reasons. Aside from updates of the identification number, there are also cases in which two or more numbers merge into one number or one number changes into an already existing one. We discard the former cases because they represent a negligible share of our sample. We correct the latter cases by keeping the already existing identification numbers.

- *MNEs operating within the EU ETS area*
  These groups are a special case of a multinational business group, but the distinction is relevant in our context. While these groups already ventured into another country (typically operating in adjacent countries) and are thus already operating internationally, they are fully affected by the regulation we set out to examine. Any firm connected to such a business group is referred to as ETS MNE.

- *Global MNEs without functional link*
  Throughout this paper we consider business groups as global multinational enterprises if they include at least one firm based in a country that is not regulated by the EU ETS. Any firm that is connected to such a business group is referred to as global MNE.

- *Global MNEs with functional link*
  If a firm is part of a global MNE and there exists another firm within this global MNE that is outside of the EU ETS are and operates in the same NACE 2-digit sector as our firm, we consider our firm a global MNE with a functional link.

These categories refer to a firm's MNE status and it is important to note that all of our analyses are conducted on the firm level regardless of a firm's positioning withing a network. Assessing GUO-level effects, shifts within business groups or potential structural adjustments are beyond the scope of this paper.

## A.2. Tangible fixed assets, operating revenue, and emissions

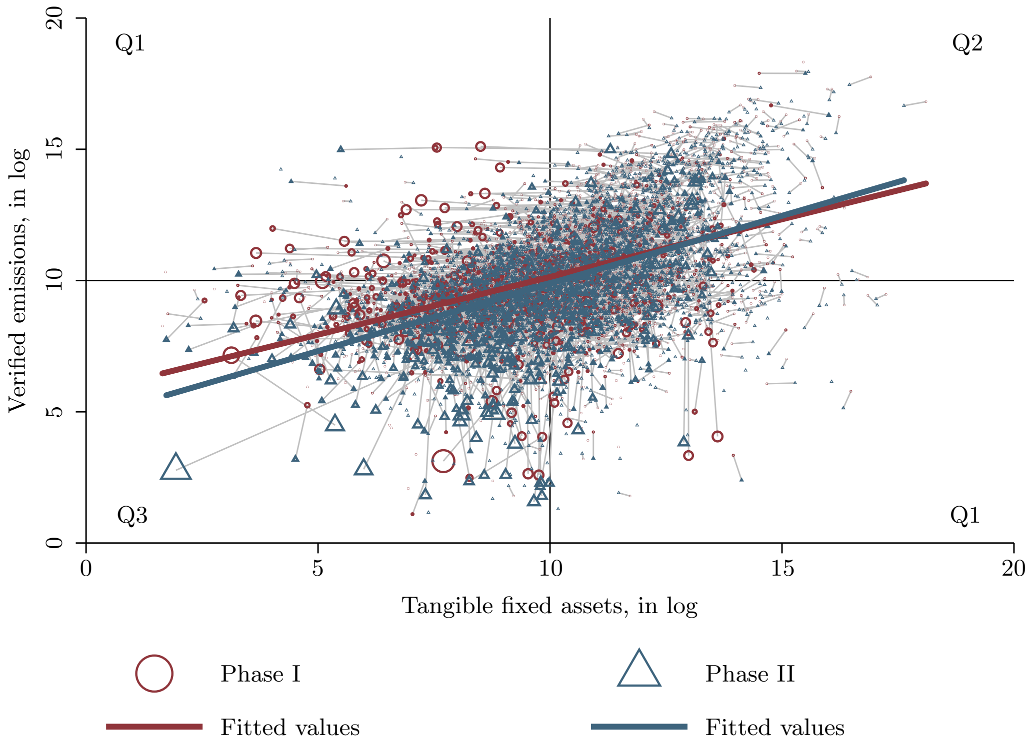



**Figure 15:** Adjustment effects between phase I and phase II, TFAS

Figure 15 displays the correlation between the logs of verified emissions and tangible fixed assets for the years 2005-2012.[53] A very intuitive result is that size in terms of this asset type per se is associated with higher emission levels. Also, we may gain some initial insights into potential adjustment effects between phases I and II.

While the visual impression is not entirely clear, there seems to be an increase in tangible fixed assets by emission intensive firms (Q1). At the same time, some firms seem to drop in emissions while keeping their level of TFAS (Q3), which could indicate the establishment of less emission-intensive production. However, since this is not a causal framework, adjustment processes might reflect changes in economic activity rather than changes in production structure due to the EU ETS. Also, this type of visualization is susceptible to outliers as the relationship cannot be clearly displayed for the bulk of the firms under the EU ETS. Hence, to identify the actual connection between TFAS and emissions, visual analysis alone is not sufficient. Figure 16 conducts the same graphical

[53]Both variables have been averaged per phase and then transformed in logs. Lines connect pairs of values per firm, outliers have been excluded. Points have been rescaled to highlight firms with the highest "adjustment effort". The following formula was used to calculate weights on the firm level before transforming the period averages in log: $W = \left[ (\frac{|\Delta emissions|}{\varnothing emissions})^2 + (\frac{|\Delta TFAS|}{\varnothing TFAS})^2 \right]^2$. Weights are used only for visualization.

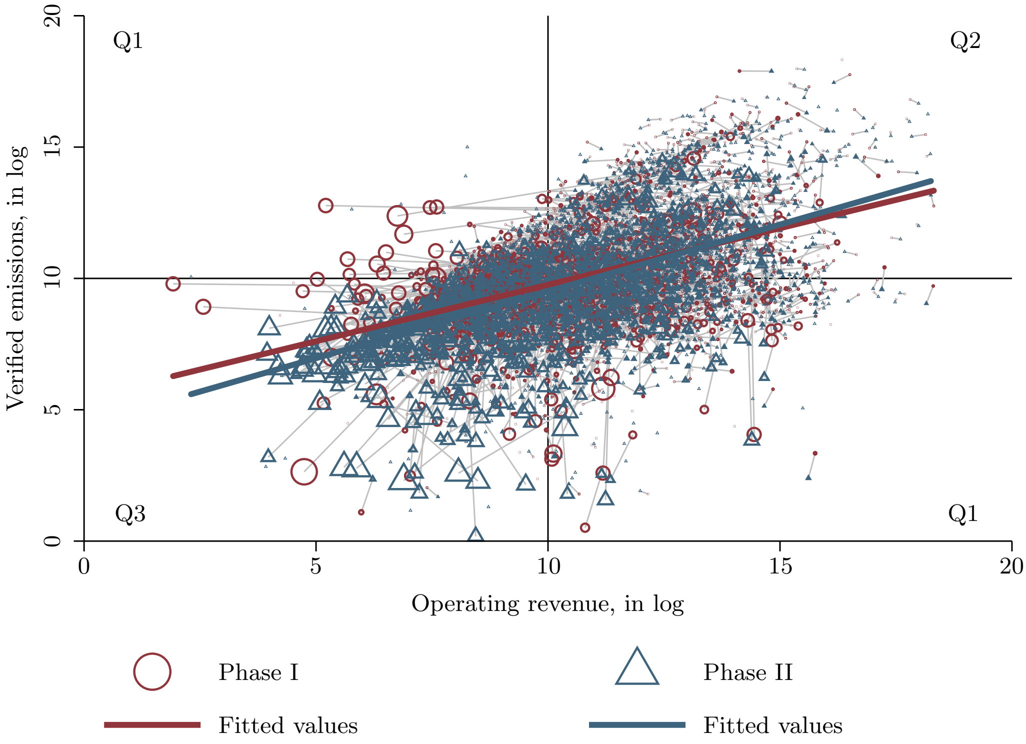



**Figure 16:** Adjustment effects between phase I and phase II, Operating Revenue

analysis for operating revenue.

## A.3. Covariate balancing - Standardized differences

**Table 11:** Subsamples to assess effect heterogeneity

| | (1) (Baseline) | (2) (Manufacturing) | (3) (CLL only) | (4) (Energy) |
|---|---|---|---|---|
| Very large firms excluded | ✓ | ✓ | ✓ | ✓ |
| Firms connected to the treatment group excluded | ✓ | ✓ | ✓ | ✓ |
| Firms with missing asset data excluded | ✓ | ✓ | ✓ | ✓ |
| Balancing on 2004 MNE status | | | | |
| Sector | All | Manuf. | CLL only | Energy |
| NACE code | 2-digit | 2-digit | 2-digit | 2-digit |
| Caliper | 0.2 | 0.2 | 0.2 | 0.2 |
| On support | 325514 | 90289 | 46659 | 1580 |
| Treatment group | 1321 | 835 | 592 | 298 |
| Control group | 1321 | 835 | 592 | 298 |
| Off support | 594 | 377 | 399 | 222 |
| Tangible fixed assets (in logs), 2004 | 1.0 \| *in* | −0.1 \| *in* | −0.6 \| *in* | 4.6 \| *in* |
| Tangible fixed assets (in logs), 2003 | 1.0 \| *in* | 1.8 \| *in* | −0.9 \| *in* | 4.8 \| *out* |
| Tangible fixed assets (in logs), 2002 | 0.1 \| *in* | 2.2 \| *in* | −1.6 \| *in* | 3.3 \| *out* |
| Total assets (in logs), 2004 | 7.2 \| *in* | 4.5 \| *in* | 9.4 \| *in* | 8.5 \| *in* |
| Total assets (in logs), 2003 | 7.3 \| *in* | 5.4 \| *in* | 9.2 \| *in* | 7.5 \| *out* |
| Total assets (in logs), 2002 | 7.1 \| *in* | 6.6 \| *in* | 10.1 \| *in* | 9.0 \| *out* |
| Tangible fixed asset ratio, 2004 | −5.7 \| *out* | −10.1 \| *out* | −24.4 \| *out* | 1.4 \| *out* |
| Tangible fixed asset ratio, 2003 | −5.1 \| *out* | −5.5 \| *out* | −22.0 \| *out* | 2.5 \| *out* |
| Tangible fixed asset ratio, 2002 | −6.1 \| *out* | −6.2 \| *out* | −25.8 \| *out* | 0.8 \| *out* |
| Operating revenue (in logs), 2004 | 4.0 \| *in* | 6.6 \| *in* | 12.5 \| *in* | 5.0 \| *in* |
| Operating revenue (in logs), 2003 | 4.1 \| *in* | 6.7 \| *in* | 11.2 \| *in* | 5.2 \| *out* |
| Operating revenue (in logs), 2002 | 3.8 \| *in* | 6.6 \| *in* | 11.9 \| *in* | 3.8 \| *out* |
| Profit ratio, 2004, winsorized | 5.4 \| *in* | −11.7 \| *in* | −4.6 \| *in* | 28.1 \| *in* |
| Profit ratio, 2003, winsorized | −1.3 \| *in* | −15.2 \| *in* | −14.8 \| *in* | 17.1 \| *out* |
| Profit ratio, 2002, winsorized | −2.1 \| *in* | −14.1 \| *in* | −15.1 \| *in* | 16.1 \| *out* |
| Investment ratio, 2004 | −2.9 \| *in* | −12.7 \| *in* | −6.8 \| *in* | 4.1 \| *out* |
| Investment ratio, 2003 | 3.0 \| *in* | −7.7 \| *in* | −8.4 \| *in* | 3.9 \| *out* |
| Date of incorporation | −2.1 \| *in* | 0.9 \| *in* | −3.4 \| *in* | −5.6 \| *in* |

Treatment groups include only firms known to have been active in both phases of the EU-ETS.

(out) indicates variables that were not part of the balancing process.

Corrections done in all samples: pre-balancing max value threshold, exclusion of firms with implausible data.

Significance levels: $*$ p <0.10, $**$ p <0.05, $***$ p <0.01

All samples based on 1-1 nearest neighbour matching, exact matching on country and sector.

**Table 12:** Baseline samples

| | (1) (Balanced MNE status) | (2) (NACE 3-digit) | (3) (Baseline) | (4) (With large firms) | (5) (Without restrictions) | (6) (Simple balancing) |
|---|---|---|---|---|---|---|
| Very large firms excluded | ✓ | ✓ | ✓ | | | |
| Firms connected to the treatment group excluded | ✓ | ✓ | ✓ | ✓ | | |
| Firms with missing asset data excluded | ✓ | ✓ | ✓ | ✓ | | |
| Balancing on 2004 MNE status | ✓ | | | | | |
| Sector | All | All | All | All | All | All |
| NACE code | 2-digit | 3-digit | 2-digit | 2-digit | 2-digit | 2-digit |
| Caliper | 0.2 | 0.05 | 0.2 | 0.2 | 0.2 | 0.2 |
| On support | 325445 | 325144 | 325514 | 327921 | 549919 | 806468 |
| Treatment group | 1252 | 951 | 1321 | 1417 | 2360 | 2839 |
| Control group | 1252 | 951 | 1321 | 1417 | 2360 | 2839 |
| Off support | 663 | 964 | 594 | 803 | 533 | 594 |
| Tangible fixed assets (in logs), 2004 | 7.8 \| *in* | −1.2 \| *in* | 1.0 \| *in* | 1.9 \| *in* | −1.4 \| *in* | −2.7 \| *in* |
| Tangible fixed assets (in logs), 2003 | 7.7 \| *in* | −2.5 \| *in* | 1.0 \| *in* | 1.6 \| *in* | −1.8 \| *in* | −0.4 \| *out* |
| Tangible fixed assets (in logs), 2002 | 6.6 \| *in* | −3.4 \| *in* | 0.1 \| *in* | 0.6 \| *in* | −2.5 \| *in* | 0.5 \| *out* |
| Total assets (in logs), 2004 | 11.5 \| *in* | 8.7 \| *in* | 7.2 \| *in* | 7.7 \| *in* | 1.5 \| *in* | 0.2 \| *in* |
| Total assets (in logs), 2003 | 11.6 \| *in* | 8.7 \| *in* | 7.3 \| *in* | 7.6 \| *in* | 1.7 \| *in* | 0.3 \| *out* |
| Total assets (in logs), 2002 | 11.6 \| *in* | 8.2 \| *in* | 7.1 \| *in* | 7.0 \| *in* | 1.5 \| *in* | 2.0 \| *out* |
| Tangible fixed asset ratio, 2004 | −0.6 \| *out* | −14.5 \| *out* | −5.7 \| *out* | −6.0 \| *out* | −3.4 \| *out* | −2.8 \| *out* |
| Tangible fixed asset ratio, 2003 | 0.3 \| *out* | −15.6 \| *out* | −5.1 \| *out* | −5.6 \| *out* | −3.4 \| *out* | 1.5 \| *out* |
| Tangible fixed asset ratio, 2002 | −1.6 \| *out* | −15.9 \| *out* | −6.1 \| *out* | −6.5 \| *out* | −4.7 \| *out* | 3.3 \| *out* |
| Operating revenue (in logs), 2004 | 8.8 \| *in* | 10.2 \| *in* | 4.0 \| *in* | 2.6 \| *in* | −0.8 \| *in* | −2.5 \| *in* |
| Operating revenue (in logs), 2003 | 8.6 \| *in* | 10.4 \| *in* | 4.1 \| *in* | 2.4 \| *in* | −0.5 \| *in* | −1.2 \| *out* |
| Operating revenue (in logs), 2002 | 8.5 \| *in* | 11.4 \| *in* | 3.8 \| *in* | 2.8 \| *in* | −0.1 \| *in* | −3.6 \| *out* |
| Profit ratio, 2004, winsorized | 4.1 \| *in* | −0.7 \| *in* | 5.4 \| *in* | 4.1 \| *in* | 4.6 \| *in* | 1.0 \| *in* |
| Profit ratio, 2003, winsorized | −0.5 \| *in* | −4.0 \| *in* | −1.3 \| *in* | −0.4 \| *in* | 3.7 \| *in* | −2.0 \| *out* |
| Profit ratio, 2002, winsorized | −0.2 \| *in* | 0.4 \| *in* | −2.1 \| *in* | 0.2 \| *in* | 3.2 \| *in* | 0.3 \| *out* |
| Investment ratio, 2004 | −3.2 \| *in* | −1.6 \| *in* | −2.9 \| *in* | −1.4 \| *in* | −1.9 \| *in* | −5.6 \| *out* |
| Investment ratio, 2003 | 5.1 \| *in* | −2.3 \| *in* | 3.0 \| *in* | 4.9 \| *in* | 2.3 \| *in* | −11.5 \| *out* |
| Date of incorporation | −6.6 \| *in* | −6.3 \| *in* | −2.1 \| *in* | −0.6 \| *in* | −4.1 \| *in* | −2.7 \| *in* |

Treatment groups include only firms known to have been active in both phases of the EU-ETS.

(out) indicates variables that were not part of the balancing process.

Corrections done in all samples: pre-balancing max value threshold, exclusion of firms with implausible data.

Significance levels: $*$ p <0.10, $**$ p <0.05, $* * *$ p <0.01

All samples based on 1-1 nearest neighbour matching, exact matching on country and sector.

**Table 13:** Manufacturing samples

| | (1)<br>(Balanced MNE status) | (2)<br>(NACE 3-digit) | (3)<br>(Manufacturing) | (4)<br>(With large firms) | (5)<br>(Without restrictions) | (6)<br>(Simple balancing) |
|---|---|---|---|---|---|---|
| Very large firms excluded | ✓ | ✓ | ✓ | | | |
| Firms connected to the treatment group excluded | ✓ | ✓ | ✓ | ✓ | | |
| Firms with missing asset data excluded | ✓ | ✓ | ✓ | ✓ | | |
| Balancing on 2004 MNE status | ✓ | | | | | |
| Sector | Manuf. | Manuf. | Manuf. | Manuf. | Manuf. | Manuf. |
| NACE code | 2-digit | 3-digit | 2-digit | 2-digit | 2-digit | 2-digit |
| Caliper | 0.2 | 0.05 | 0.2 | 0.2 | 0.2 | 0.2 |
| On support | 90241 | 89979 | 90289 | 90557 | 142963 | 190251 |
| Treatment group | 787 | 525 | 835 | 873 | 1430 | 1677 |
| Control group | 787 | 525 | 835 | 873 | 1430 | 1677 |
| Off support | 425 | 687 | 377 | 523 | 390 | 453 |
| Tangible fixed assets (in logs), 2004 | 6.4 \| *in* | 0.6 \| *in* | −0.1 \| *in* | −0.0 \| *in* | 0.3 \| *in* | −0.4 \| *in* |
| Tangible fixed assets (in logs), 2003 | 7.7 \| *in* | 1.4 \| *in* | 1.8 \| *in* | 0.3 \| *in* | 0.3 \| *in* | 2.5 \| *out* |
| Tangible fixed assets (in logs), 2002 | 7.9 \| *in* | 1.2 \| *in* | 2.2 \| *in* | 1.2 \| *in* | 1.0 \| *in* | 2.8 \| *out* |
| Total assets (in logs), 2004 | 11.0 \| *in* | 11.8 \| *in* | 4.5 \| *in* | 5.8 \| *in* | 4.5 \| *in* | 3.8 \| *in* |
| Total assets (in logs), 2003 | 11.9 \| *in* | 12.8 \| *in* | 5.4 \| *in* | 6.3 \| *in* | 4.7 \| *in* | 4.3 \| *out* |
| Total assets (in logs), 2002 | 13.3 \| *in* | 13.3 \| *in* | 6.6 \| *in* | 7.1 \| *in* | 5.4 \| *in* | 4.4 \| *out* |
| Tangible fixed asset ratio, 2004 | −7.6 \| *out* | −26.3 \| *out* | −10.1 \| *out* | −13.4 \| *out* | −7.9 \| *out* | −10.4 \| *out* |
| Tangible fixed asset ratio, 2003 | −4.1 \| *out* | −24.7 \| *out* | −5.5 \| *out* | −11.1 \| *out* | −7.1 \| *out* | −6.4 \| *out* |
| Tangible fixed asset ratio, 2002 | −5.5 \| *out* | −25.5 \| *out* | −6.2 \| *out* | −10.5 \| *out* | −7.8 \| *out* | −6.1 \| *out* |
| Operating revenue (in logs), 2004 | 13.1 \| *in* | 19.1 \| *in* | 6.6 \| *in* | 8.6 \| *in* | 6.4 \| *in* | 6.2 \| *in* |
| Operating revenue (in logs), 2003 | 12.6 \| *in* | 19.6 \| *in* | 6.7 \| *in* | 8.2 \| *in* | 5.8 \| *in* | 6.5 \| *out* |
| Operating revenue (in logs), 2002 | 13.3 \| *in* | 19.9 \| *in* | 6.6 \| *in* | 8.8 \| *in* | 6.2 \| *in* | 5.3 \| *out* |
| Profit ratio, 2004, winsorized | −7.5 \| *in* | −16.1 \| *in* | −11.7 \| *in* | −14.2 \| *in* | −7.3 \| *in* | 1.3 \| *in* |
| Profit ratio, 2003, winsorized | −12.5 \| *in* | −15.2 \| *in* | −15.2 \| *in* | −17.6 \| *in* | −7.1 \| *in* | −9.0 \| *out* |
| Profit ratio, 2002, winsorized | −7.6 \| *in* | −15.6 \| *in* | −14.1 \| *in* | −15.7 \| *in* | −6.7 \| *in* | −1.0 \| *out* |
| Investment ratio, 2004 | −9.7 \| *in* | −10.8 \| *in* | −12.7 \| *in* | −7.1 \| *in* | −2.1 \| *in* | −13.8 \| *out* |
| Investment ratio, 2003 | −6.0 \| *in* | −12.0 \| *in* | −7.7 \| *in* | −8.5 \| *in* | −3.3 \| *in* | −12.6 \| *out* |
| Date of incorporation | −6.8 \| *in* | −14.1 \| *in* | 0.9 \| *in* | −4.5 \| *in* | −5.1 \| *in* | −2.2 \| *in* |

Treatment groups include only firms known to have been active in both phases of the EU-ETS.

(out) indicates variables that were not part of the balancing process.

Corrections done in all samples: pre-balancing max value threshold, exclusion of firms with implausible data.

Significance levels: $*$ p <0.10, $**$ p <0.05, $***$ p <0.01

All samples based on 1-1 nearest neighbour matching, exact matching on country and sector.

## A.4. Covariates before and after balancing (Baseline)

**Figure 17:** Total assets (in logs) in 2004 before and after balancing

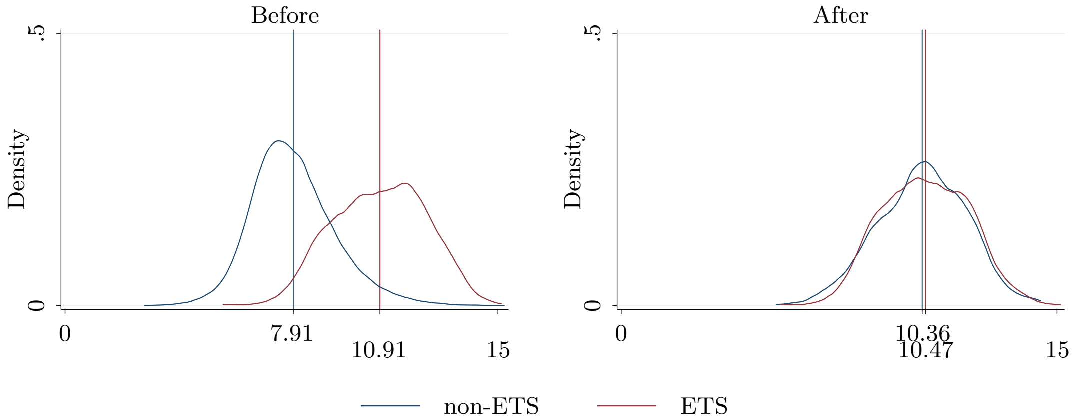



**Figure 18:** Tangible fixed asset ratio in 2004 before and after balancing

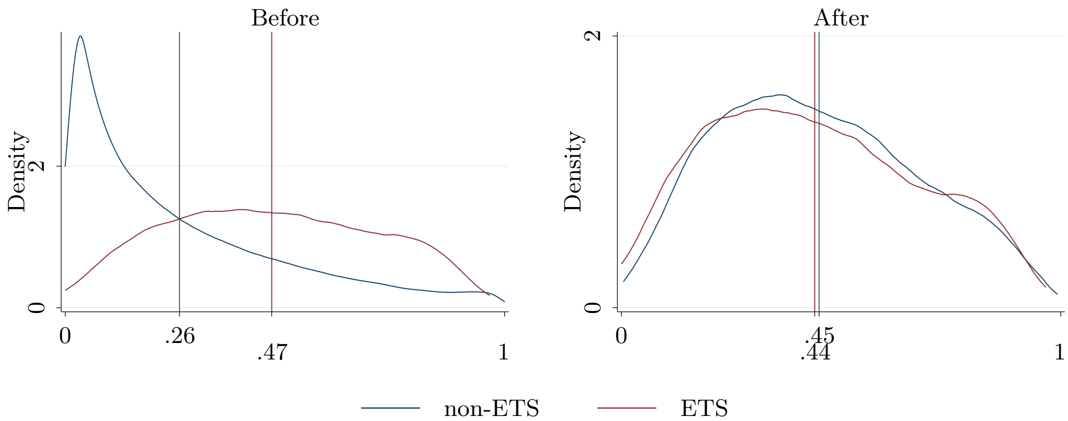


**Figure 19:** Investment ratio in 2004 before and after balancing

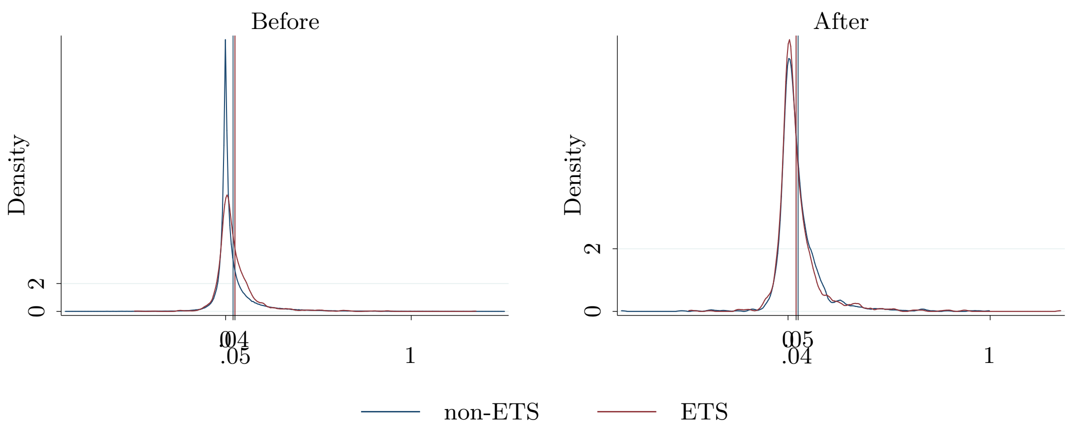



**Figure 20:** Operating revenue (in logs) in 2004 before and after balancing

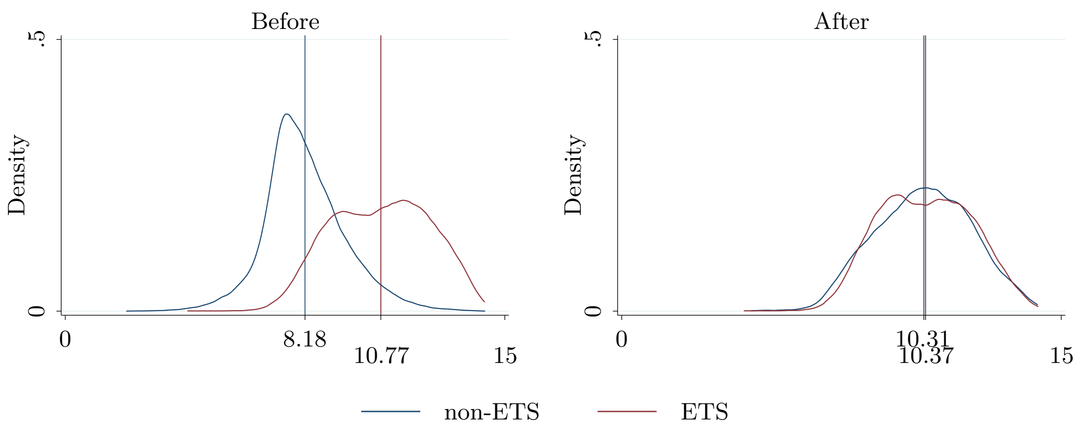
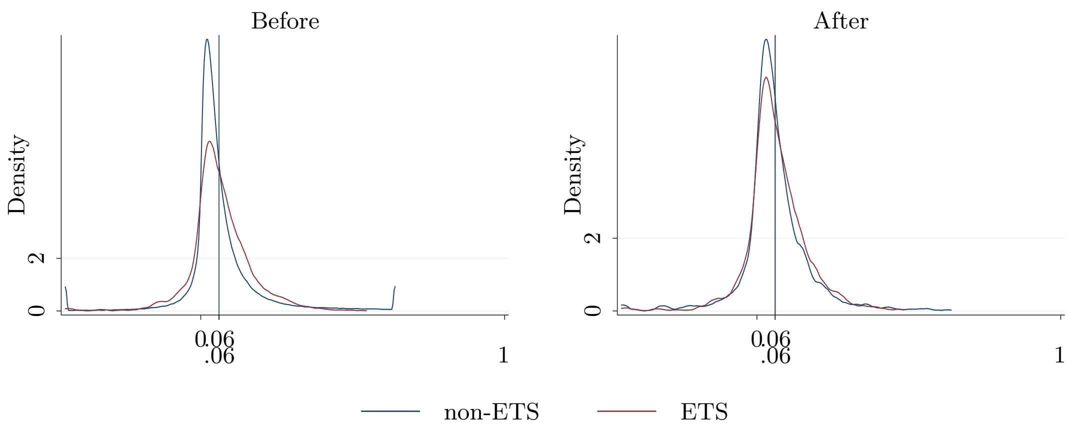



**Figure 21:** Profit ratio in 2004 before after balancing



## A.5. Means over time

**Figure 22:** Means over time: Total assets in log

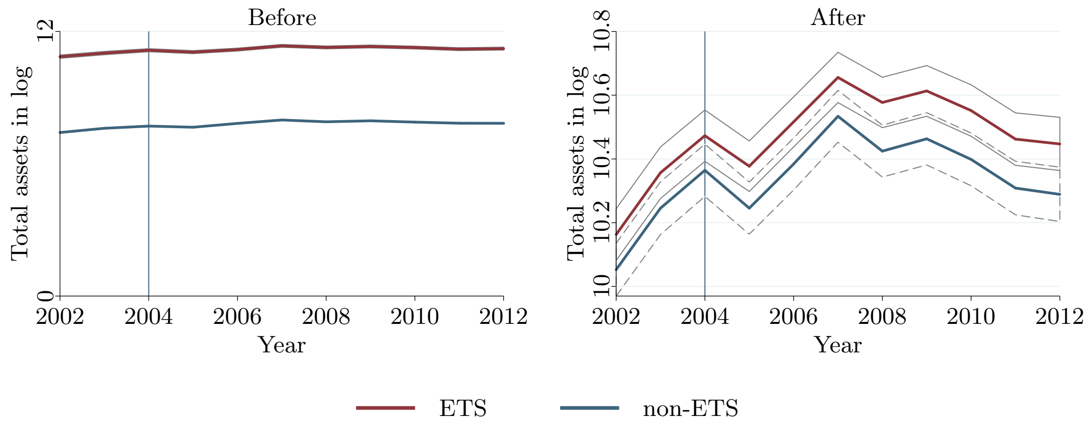



**Figure 23:** Means over time: Tangible fixed asset ratio

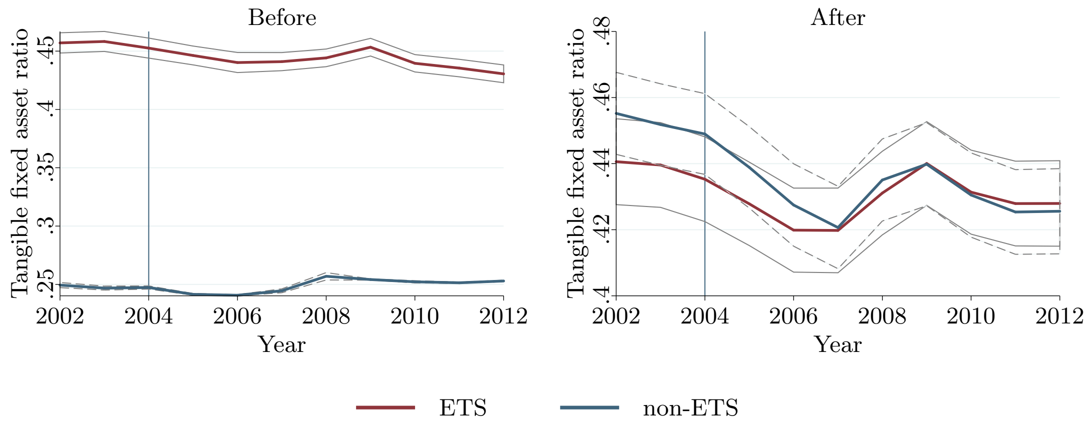


**Figure 24:** Means over time: Investment ratio

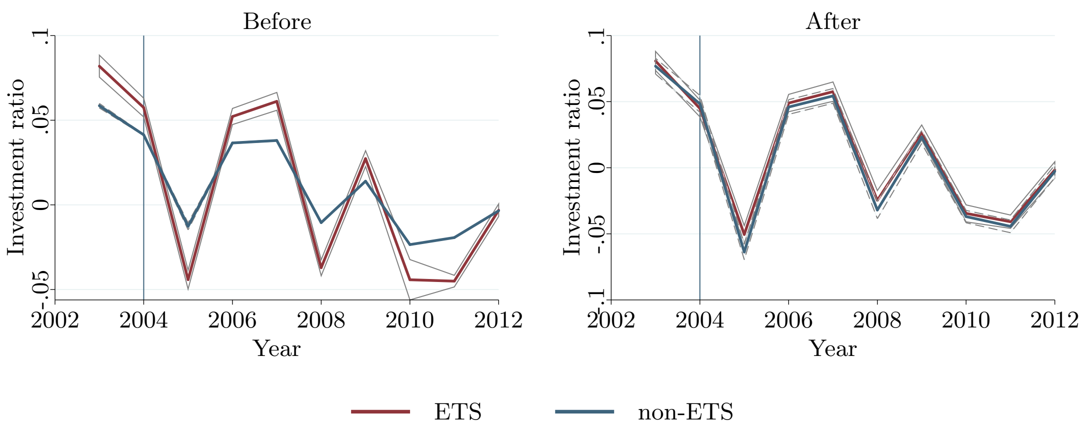



**Figure 25:** Means over time: Operating revenue in log

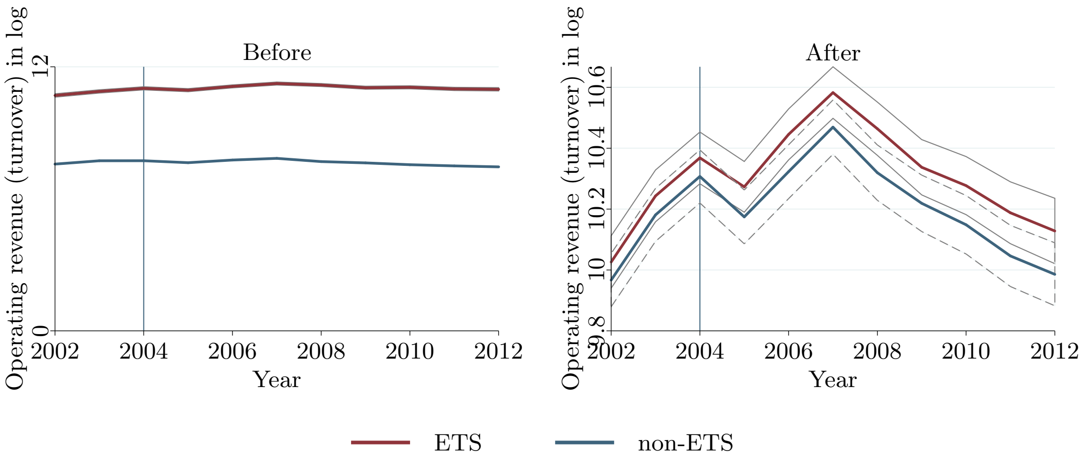
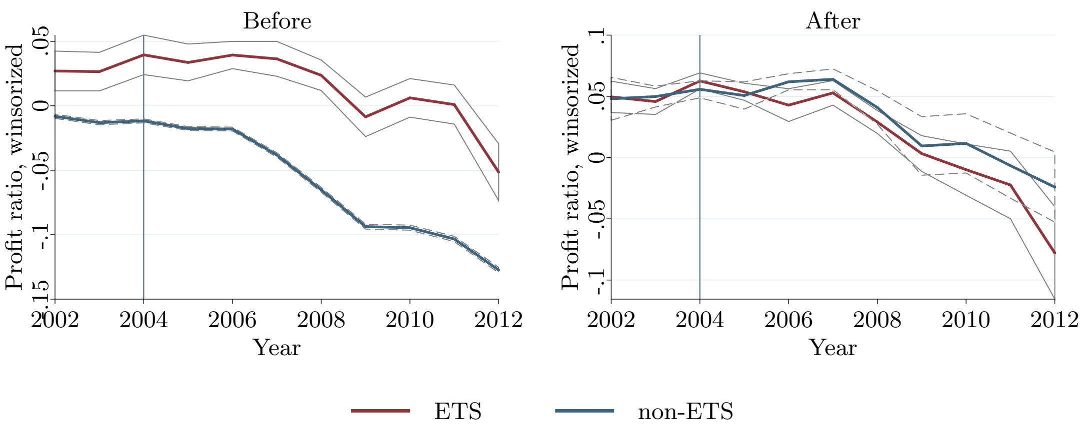



**Figure 26:** Means over time: Profit ratio, winsorized



## A.6. Tangible fixed assets - MNE dummy robustness

**Table 14:** 2004 constant ownership

| | (1) Baseline MNE | (2) Manufacturing MNE | (3) CLL only MNE | (4) Energy MNE |
|---|---|---|---|---|
| ETS treatment effect | $0.103^{***}$ | $0.102^{***}$ | $0.211^{***}$ | $0.145^{**}$ |
| Global MNE without functional link and treated | $-0.099$ | $-0.111$ | $-0.135$ | $-0.224$ |
| Global MNE with functional link and treated | 0.054 | 0.190 | 0.246 | $-0.534^{**}$ |
| Firms (T+C) | 2642 | 1670 | 1184 | 596 |

Significance levels: $*$ p $<0.10$, $**$ p $<0.05$, $***$ p $<0.01$

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

**Table 15:** 2012 constant ownership

| | (1) Baseline MNE | (2) Manufacturing MNE | (3) CLL only MNE | (4) Energy MNE |
|---|---|---|---|---|
| ETS treatment effect | $0.110^{***}$ | $0.130^{***}$ | $0.233^{***}$ | $0.132^{**}$ |
| Global MNE without functional link and treated | $-0.076$ | $-0.149^{**}$ | $-0.131^{*}$ | $-0.034$ |
| Global MNE with functional link and treated | 0.003 | 0.008 | 0.049 | $-0.144$ |
| Firms (T+C) | 2642 | 1670 | 1184 | 596 |

Significance levels: $*$ p $<0.10$, $**$ p $<0.05$, $***$ p $<0.01$

Standard errors are clustered on the firm level.

Sector-Year fixed effects and firm-level fixed effects included.

## A.7. Selected Summary Statistics

**Table 16:** Summary Statistics (2004) - Baseline

**Before Balancing**

| | Treated | | | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.954 | 1.659 | 10.071 | 3.445 | 13.219 | 1915 | 5.980 | 1.873 | 5.897 | 1.722 | 13.187 | 324193 |
| Total assets (in logs) | 10.914 | 1.586 | 10.981 | 5.476 | 15.121 | 1915 | 7.908 | 1.440 | 7.771 | 2.746 | 15.224 | 324193 |
| Tangible fixed asset ratio | 0.469 | 0.238 | 0.462 | 0.000 | 0.966 | 1915 | 0.260 | 0.241 | 0.181 | 0.000 | 1.000 | 324193 |
| Operating revenue (in logs) | 10.766 | 1.658 | 10.845 | 4.179 | 14.309 | 1915 | 8.182 | 1.375 | 8.033 | 2.092 | 14.319 | 324193 |
| Profit ratio | 0.064 | 0.117 | 0.054 | −2.475 | 0.546 | 1915 | 0.042 | 1.153 | 0.040 | −328.916 | 3.043 | 324193 |
| Investment ratio | 0.049 | 0.117 | 0.023 | −0.491 | 1.349 | 1915 | 0.037 | 0.121 | 0.006 | −0.864 | 1.503 | 324193 |
| Date of incorporation | 1976.468 | 26.897 | 1987.000 | 1748.000 | 2004.000 | 1915 | 1986.957 | 13.890 | 1991.000 | 1383.000 | 2004.000 | 324193 |

**After Balancing**

| | Treated | | | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.407 | 1.574 | 9.502 | 3.445 | 13.011 | 1321 | 9.389 | 1.571 | 9.531 | 2.595 | 13.052 | 1321 |
| Total assets (in logs) | 10.473 | 1.492 | 10.470 | 5.476 | 15.121 | 1321 | 10.364 | 1.520 | 10.400 | 5.333 | 14.432 | 1321 |
| Tangible fixed asset ratio | 0.435 | 0.238 | 0.413 | 0.000 | 0.966 | 1321 | 0.449 | 0.227 | 0.429 | 0.005 | 0.992 | 1321 |
| Operating revenue (in logs) | 10.368 | 1.574 | 10.360 | 4.179 | 14.209 | 1321 | 10.307 | 1.616 | 10.331 | 4.421 | 14.189 | 1321 |
| Profit ratio | 0.063 | 0.122 | 0.053 | −2.475 | 0.486 | 1321 | 0.056 | 0.129 | 0.047 | −1.053 | 0.730 | 1321 |
| Investment ratio | 0.045 | 0.117 | 0.019 | −0.491 | 1.349 | 1321 | 0.048 | 0.118 | 0.023 | −0.824 | 0.997 | 1321 |
| Date of incorporation | 1976.980 | 25.531 | 1987.000 | 1753.000 | 2004.000 | 1321 | 1977.436 | 27.832 | 1988.000 | 1710.000 | 2003.000 | 1321 |

**Table 17:** Summary Statistics (2004) - Manufacturing

**Before Balancing**

| | Treated | | | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 10.027 | 1.608 | 10.215 | 4.264 | 13.177 | 1212 | 6.440 | 1.718 | 6.418 | 2.133 | 13.187 | 89454 |
| Total assets (in logs) | 11.112 | 1.582 | 11.265 | 5.476 | 15.121 | 1212 | 8.124 | 1.430 | 8.000 | 2.774 | 14.882 | 89454 |
| Tangible fixed asset ratio | 0.394 | 0.190 | 0.380 | 0.003 | 0.929 | 1212 | 0.271 | 0.202 | 0.226 | 0.000 | 0.999 | 89454 |
| Operating revenue (in logs) | 11.110 | 1.614 | 11.271 | 5.218 | 14.309 | 1212 | 8.439 | 1.335 | 8.274 | 2.667 | 14.235 | 89454 |
| Profit ratio | 0.064 | 0.100 | 0.053 | −0.513 | 0.546 | 1212 | 0.046 | 0.264 | 0.042 | −47.262 | 1.157 | 89454 |
| Investment ratio | 0.038 | 0.107 | 0.016 | −0.388 | 1.349 | 1212 | 0.035 | 0.114 | 0.006 | −0.765 | 1.462 | 89454 |
| Date of incorporation | 1973.147 | 27.814 | 1982.000 | 1748.000 | 2004.000 | 1212 | 1984.969 | 15.433 | 1989.000 | 1697.000 | 2004.000 | 89454 |

**After Balancing**

| | Treated | | | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.389 | 1.447 | 9.498 | 4.264 | 12.618 | 835 | 9.391 | 1.435 | 9.501 | 4.077 | 12.671 | 835 |
| Total assets (in logs) | 10.538 | 1.455 | 10.546 | 5.476 | 15.121 | 835 | 10.470 | 1.506 | 10.498 | 5.141 | 14.796 | 835 |
| Tangible fixed asset ratio | 0.372 | 0.184 | 0.355 | 0.003 | 0.929 | 835 | 0.392 | 0.185 | 0.371 | 0.016 | 0.929 | 835 |
| Operating revenue (in logs) | 10.588 | 1.545 | 10.594 | 5.218 | 14.209 | 835 | 10.490 | 1.555 | 10.464 | 5.114 | 14.192 | 835 |
| Profit ratio | 0.055 | 0.096 | 0.046 | −0.467 | 0.486 | 835 | 0.049 | 0.481 | 0.052 | −13.454 | 0.972 | 835 |
| Investment ratio | 0.036 | 0.109 | 0.015 | −0.388 | 1.349 | 835 | 0.050 | 0.108 | 0.022 | −0.412 | 0.849 | 835 |
| Date of incorporation | 1975.013 | 24.526 | 1983.000 | 1822.000 | 2004.000 | 835 | 1974.818 | 29.281 | 1985.000 | 1710.000 | 2002.000 | 835 |

**Table 18:** Summary Statistics (2004) - Carbon leakage list only

| | **Before Balancing** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treated | | | | | | Control | | | | | |
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.953 | 1.625 | 10.080 | 4.328 | 13.177 | 991 | 6.503 | 1.760 | 6.504 | 2.166 | 13.187 | 46067 |
| Total assets (in logs) | 11.034 | 1.602 | 11.159 | 5.476 | 15.121 | 991 | 8.298 | 1.450 | 8.169 | 3.060 | 14.796 | 46067 |
| Tangible fixed asset ratio | 0.397 | 0.192 | 0.382 | 0.003 | 0.929 | 991 | 0.252 | 0.197 | 0.203 | 0.000 | 0.999 | 46067 |
| Operating revenue (in logs) | 10.998 | 1.630 | 11.153 | 5.218 | 14.309 | 991 | 8.557 | 1.370 | 8.396 | 2.667 | 14.235 | 46067 |
| Profit ratio | 0.069 | 0.102 | 0.057 | −0.513 | 0.546 | 991 | 0.041 | 0.444 | 0.043 | −60.192 | 1.157 | 46067 |
| Investment ratio | 0.037 | 0.101 | 0.015 | −0.256 | 0.802 | 991 | 0.030 | 0.102 | 0.005 | −0.503 | 1.168 | 46067 |
| Date of incorporation | 1973.183 | 28.244 | 1982.000 | 1748.000 | 2004.000 | 991 | 1984.022 | 16.513 | 1989.000 | 1700.000 | 2004.000 | 46067 |

| | **After Balancing** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treated | | | | | | Control | | | | | |
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.207 | 1.495 | 9.270 | 4.328 | 12.266 | 592 | 9.218 | 1.523 | 9.237 | 4.094 | 12.671 | 592 |
| Total assets (in logs) | 10.399 | 1.527 | 10.390 | 5.476 | 15.121 | 592 | 10.256 | 1.598 | 10.286 | 5.077 | 14.671 | 592 |
| Tangible fixed asset ratio | 0.359 | 0.183 | 0.339 | 0.003 | 0.929 | 592 | 0.407 | 0.186 | 0.390 | 0.006 | 0.944 | 592 |
| Operating revenue (in logs) | 10.427 | 1.604 | 10.329 | 5.218 | 14.209 | 592 | 10.239 | 1.660 | 10.236 | 5.326 | 14.192 | 592 |
| Profit ratio | 0.058 | 0.097 | 0.046 | −0.419 | 0.486 | 592 | 0.059 | 0.147 | 0.052 | −1.771 | 0.972 | 592 |
| Investment ratio | 0.032 | 0.094 | 0.012 | −0.245 | 0.802 | 592 | 0.039 | 0.097 | 0.018 | −0.468 | 0.575 | 592 |
| Date of incorporation | 1974.775 | 25.610 | 1983.000 | 1822.000 | 2004.000 | 592 | 1975.563 | 30.002 | 1987.000 | 1710.000 | 2002.000 | 592 |

**Table 19:** Summary Statistics (2004) - Energy

**Before Balancing**

| | Treated | | | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.852 | 1.674 | 9.879 | 3.856 | 13.219 | 520 | 8.090 | 2.200 | 8.235 | 2.060 | 13.052 | 1282 |
| Total assets (in logs) | 10.391 | 1.492 | 10.323 | 6.700 | 14.096 | 520 | 9.130 | 1.738 | 9.000 | 3.865 | 14.352 | 1282 |
| Tangible fixed asset ratio | 0.658 | 0.215 | 0.704 | 0.005 | 0.966 | 520 | 0.519 | 0.279 | 0.571 | 0.000 | 0.999 | 1282 |
| Operating revenue (in logs) | 9.948 | 1.485 | 9.749 | 4.179 | 13.818 | 520 | 8.547 | 1.793 | 8.276 | 3.037 | 13.725 | 1282 |
| Profit ratio | 0.064 | 0.151 | 0.060 | −2.475 | 0.405 | 520 | 0.052 | 1.578 | 0.094 | −54.980 | 0.607 | 1282 |
| Investment ratio | 0.072 | 0.133 | 0.054 | −0.491 | 0.882 | 520 | 0.052 | 0.148 | 0.020 | −0.824 | 1.176 | 1282 |
| Date of incorporation | 1984.440 | 21.509 | 1992.000 | 1856.000 | 2002.000 | 520 | 1983.563 | 23.642 | 1992.000 | 1842.000 | 2003.000 | 1282 |

**After Balancing**

| | Treated | | | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Median | Min. | Max. | N | Mean | Std. Dev. | Median | Min. | Max. | N |
| Tangible fixed assets (in logs) | 9.410 | 1.788 | 9.290 | 3.856 | 13.014 | 298 | 9.321 | 1.704 | 9.438 | 4.800 | 13.052 | 298 |
| Total assets (in logs) | 10.064 | 1.535 | 9.857 | 6.700 | 13.879 | 298 | 9.926 | 1.629 | 9.985 | 5.582 | 14.352 | 298 |
| Tangible fixed asset ratio | 0.621 | 0.245 | 0.687 | 0.005 | 0.966 | 298 | 0.618 | 0.219 | 0.651 | 0.009 | 0.978 | 298 |
| Operating revenue (in logs) | 9.642 | 1.544 | 9.349 | 4.179 | 13.642 | 298 | 9.560 | 1.733 | 9.427 | 3.515 | 13.725 | 298 |
| Profit ratio | 0.080 | 0.177 | 0.080 | −2.475 | 0.383 | 298 | 0.027 | 0.287 | 0.058 | −3.155 | 0.452 | 298 |
| Investment ratio | 0.065 | 0.146 | 0.035 | −0.491 | 0.882 | 298 | 0.059 | 0.153 | 0.038 | −0.824 | 1.119 | 298 |
| Date of incorporation | 1982.822 | 24.474 | 1992.000 | 1856.000 | 2002.000 | 298 | 1984.087 | 23.526 | 1993.000 | 1842.000 | 2003.000 | 298 |

## A.8. External Validity

**Figure 27:** Baseline - External validity - MNE status

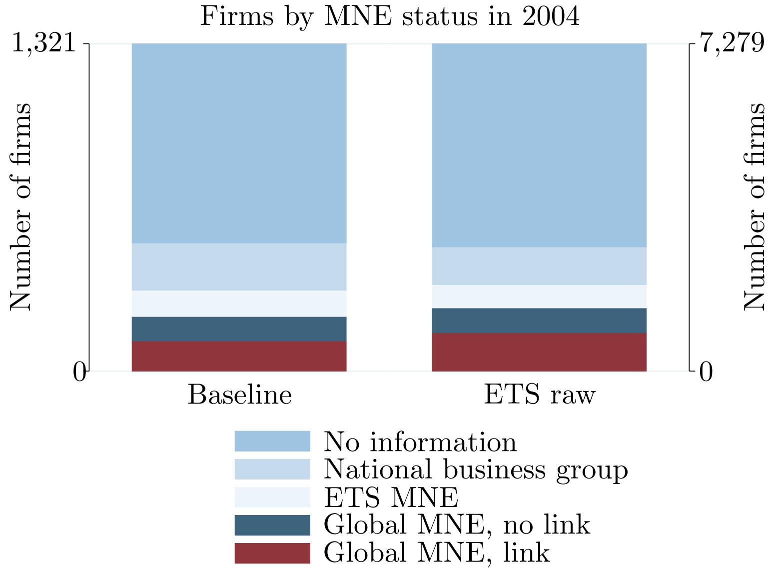



**Figure 28:** Baseline - External validity - Emissions by sector

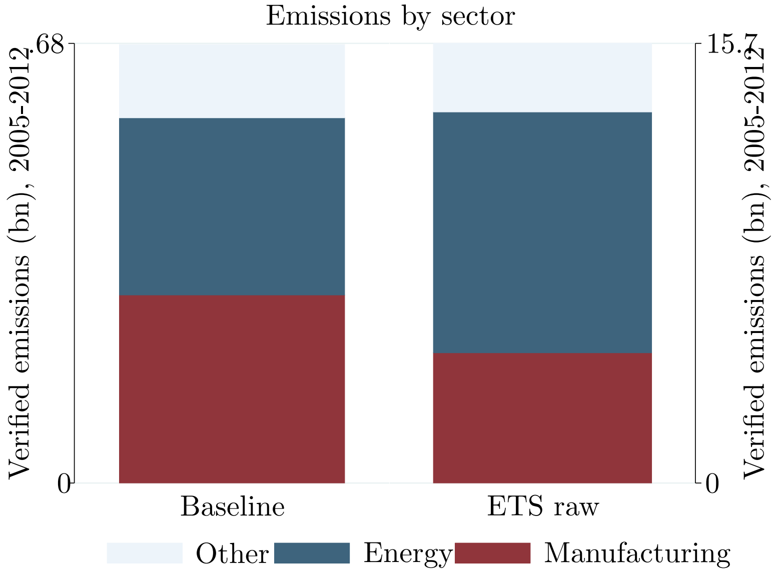


**Figure 29:** Baseline 5 - External validity - MNE status

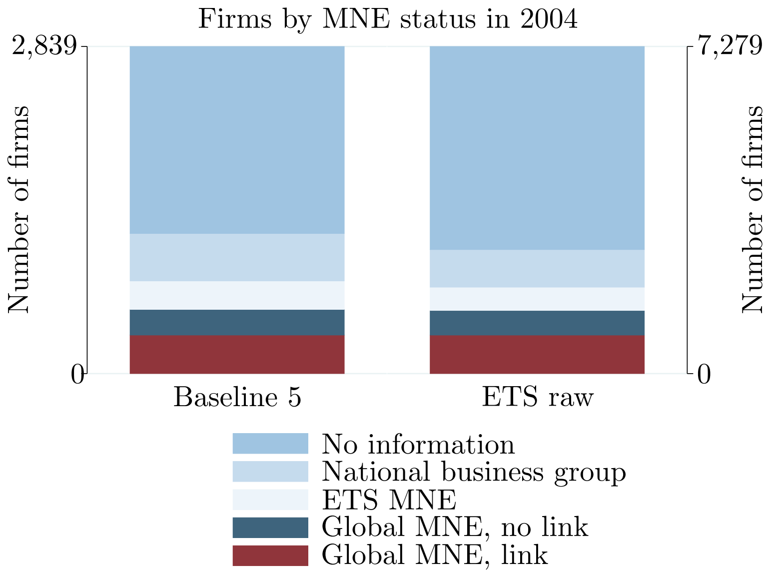



**Figure 30:** Baseline 5 - External validity - Emissions by sector