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Mitigation versus Competitiveness? Industry Compensation in the European Union Emissions Trading System

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Abstract

Carbon pricing policies are usually combined with compensation for exposed firms to prevent adverse competitiveness effects. In cap-and-trade systems, this carbon cost compensation mostly occurs through free allocation of emission permits. Using an administrative panel of German manufacturing firms, this paper investigates how free allocation in the European Union Emissions Trading System affects firms' emission reductions and competitiveness. Leveraging a reform of free allocation rules in a continuous difference-in-differences design, we find that a reduction of freely allocated emission permits decreased firms' emissions and emission intensity. For firms deemed to be at risk of carbon leakage, our results suggest that this decrease is driven by energy efficiency improvements instead of outsourcing of emission-intensive production. On the other hand, we do not find statistically significant effects on firms' employment, sales, value added, capital and exports – indicating that the reduction in free permits did not negatively affect firms' competitiveness.

JEL-Classification: Q54, Q58, H23, D22, F18

Keywords: Cap and Trade; Permit Allocation; Industry Compensation; Greenhouse Gas Emissions; Competitiveness; Manufacturing Firms

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1 Introduction

Carbon pricing is one of the most widespread policies to incentivize climate change mitigation. The majority of carbon pricing initiatives covering industry include some form of compensation or exemptions to protect the competitiveness of the regulated industries and to prevent carbon leakage (Venmans et al. 2020; Sato et al. 2022). With carbon taxes, such protection typically involves exempting some firms or sectors from paying the carbon tax whose international competitiveness is deemed to be at risk. In cap-and-trade systems, protection usually takes the form of allocating emission permits for free. These free allowances provide large implicit transfers to industry. For example, in the European Emissions Trading System (EU ETS), they amounted to more than 400 billion EUR[1] between 2005 and 2025. Given the large fiscal implications of free allowances, rigorous causal evaluation of their effects on emissions and competitiveness of industrial firms in the EU ETS is required.[2]

The Coase theorem (Coase 1960) famously states that, under certain conditions,[3] incentives to reduce emissions are not affected by the initial allocation of pollution rights. However, in practice, free allocation rules often have specific features which may distort abatement incentives in multiple ways. For example, firms may strategically alter their current level of emissions and production anticipating that this could increase their free allowances in the future (Neuhoff et al. 2006; Rosendahl 2008; Harstad and Eskeland 2010; Branger et al. 2015). Such intertemporal or dynamic elements of free allowance allocation also affect firms' marginal costs (Basaglia et al. 2025; Fowlie et al. 2016; Fischer and Fox 2011), and firms may potentially adjust their production at the intensive margin. Free allowances can also affect production decisions at the extensive margin, as firms lose their free allowances if they shut down or relocate plants. These channels suggest that free allocation may affect industrial firms' emissions and competitiveness in spite of the Coase theorem's theoretical prediction that allowance allocation and production decisions should be independent.

In this paper, we propose a new identification strategy to identify the effects of free allowances on firms' emissions and competitiveness-related variables. Our identifying variation exploits within-sector firm-level changes in free allowances after a reform of free allocation rules in 2013. Compared to previous studies, which use across sector differences in free allowances (Vivo and Marin 2018; Locatelli et al. 2022; Guerriero and Pacelli 2023; Alder et al. 2026), our empirical strategy does not rely on challenging comparisons of treated and control firms from different sectors. Using within-sector variation also permits estimation of heterogeneous effects across sectors with different exposure to carbon leakage. We apply this novel identification strategy to estimate effects on a broad set of firm-level outcomes related to emissions, carbon leakage and competitiveness.

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[1] Own calculation.

[2] Ellerman and Reguant (2008) and Zaklan (2023) provide causal evidence for electricity generators in the EU ETS. Some papers (Vivo and Marin 2018; Locatelli et al. 2022; Guerriero and Pacelli 2023; Alder et al. 2026) have evaluated the effects of free allowances using binary treatment indicators reflecting differences in free allocation across sectors. As we discuss below, this identification strategy relying on across sector differences faces several problems.

[3] The most important ones are that pollution rights are clearly defined and that transaction costs are negligible.

Following a reform of EU ETS free allocation rules in 2013, the number of free allowances were reduced for the vast majority of industrial firms. These free allocation changes varied across firms depending on the distance of their installations' emission intensities from sector-level benchmarks.

We use these heterogeneous changes in free allocation to apply a continuous difference-in-differences design, where the treatment dose is a firm's reduction in free allowances. The main identification challenge is that firms with a higher treatment dose tend to have installations with a higher emission-intensity than low-dose firms, and, thus, may also differ in other characteristics. To improve the comparability of high-dose and low-dose firms we apply entropy balancing. This balancing re-weights the sample in order to remove the correlation of the treatment dose with the observable characteristics. Intuitively, it gives more weight to low-dose firms that resemble high-dose firms and vice versa.

Our results indicate that free allocation reductions led to declines in firms' emissions and emission intensity. On average, a 1% decrease in free allowances induced emission intensity and emission reductions of 0.1%-0.5%. On the other hand, we do not find any evidence that a reduction in free allowances negatively affected outcomes related to firm's competitiveness, as measured by value added, sales, employment, exports and capital.

As climate policy aims to reduce global emissions, a concern is that the observed emission reductions may be offset by increased emissions abroad. This may occur if free allocation removal leads to a shift of production and emissions of affected firms to countries abroad. However, we do not find evidence for this channel as treatment effects on economic outcomes are insignificant. The emission reductions also do not seem to be driven by outsourcing of emission-intensive production steps. First, a larger share of outsourced materials should be a reflected in a lower value added, which we do not find throughout all specifications. Second, we do not find evidence that firms with a larger decline in free allowances increased their imports and embodied emissions in imported goods from outside the EU.

Our heterogeneity analysis reveals that the negative effects on emissions and emission intensity are present across most sectors,[4] indicating that the results are not driven by a specific subset of sectors. We furthermore find that the mechanisms behind the observed emission reduction differ between firms deemed to be at risk of carbon leakage and firms that are not on the EU's carbon leakage risk list. Firms on the leakage risk list reduced their emissions by improving their energy efficiency while firms not deemed to be at risk of carbon leakage substituted away from fossil fuels.

It is important to mention that we evaluate the effects of free allowances on emissions, competitiveness and leakage-related outcomes along the intensive margin. Free allocation may also impact firm closures and the setup of new plants in the EU or third countries. Our dataset does not allow us to reliably study these extensive margin effects, which remain an interesting avenue for future research. Moreover, our analysis covers the years 2008-2019 with low to

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4. Most sector-specific effects are, however, insignificant, due to lower statistical power when estimating sector-specific coefficients.

moderate carbon prices in the EU ETS.[5] While we do not detect negative competitiveness effects, results may differ under higher carbon prices.

We primarily contribute to the empirical literature on the effects of permit allocation in cap-and-trade systems on emissions, carbon leakage, and competitiveness. Several studies have analyzed whether permit allocation rules affect emission outcomes, finding no evidence for a violation of the independence property for electricity producers in the EU ETS (Ellerman and Reguant 2008; Zaklan 2023) and for industrial emitters in California's RECLAIM cap-and-trade program (Fowlie and Perloff 2013). In contrast, non-causal evidence based on interviews with managers (Venmans 2016) and evidence on heterogeneous emission reductions under the EU ETS (Dechezleprêtre et al. 2023) points to a violation of the independence property for energy-intensive manufacturing firms. More closely related to our setting, several studies exploit changes of free allocation rules in difference-in-differences designs (Vivo and Marin 2018; Locatelli et al. 2022; Guerriero and Pacelli 2023; Alder et al. 2026), finding that free allocation affects emissions, while evidence on competitiveness is mixed.

The latter studies use binary treatment indicators comparing firms in carbon leakage risk sectors with those in non-risk sectors. As detailed in Section 2, the firms in non-risk sectors experienced a much stronger relative reduction in free allowances after the introduction of benchmarking, and therefore typically serve as the treatment group. However, this identification strategy faces substantial challenges. Carbon leakage risk sectors are more energy-intensive, more upstream, and more exposed to international competition than non-risk sectors. Treated and control groups thus differ in production technologies and abatement potential — raising doubts about the validity of the control group. Furthermore, the estimated treatment effects apply to sectors with low carbon and trade intensity, making it unlikely that they generalize to carbon leakage risk sectors, which account for more than 95% of regulated emissions (see Figure A.1).

We contribute to this literature by applying an alternative identification strategy exploiting within-sector variation across firms rather than relying on across-sector comparisons. This approach not only overcomes the identification challenges outlined above, but also allows us to estimate sector-specific treatment effects — including separate effects for carbon leakage risk and non-risk sectors. Moreover, we examine a broad set of outcomes, enabling a detailed analysis of the mechanisms underlying the emission effects. In particular, we are able to rule out carbon leakage through the outsourcing of emission-intensive production steps.

By estimating sector-specific treatment effects we further contribute to the literature on the design and targeting of carbon cost compensation (Martin et al. 2014a, 2014b; Sato et al. 2015), and help inform which sectors should be compensated.

We also relate to the literature studying other types of compensation or exemptions from environmental regulations such as indirect carbon cost compensation for electricity prices in the EU ETS (Ferrara and Giua 2022; Basaglia et al. 2025) and exemptions from electricity levies (Gerster and Lamp 2024). We complement this literature by studying the environmental and

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[5] Figure A.3 in Appendix A.1 displays the evolution of the price of EU ETS allowances during our sample period.

competitiveness effects of direct carbon cost compensations through free allocation of emission permits.

More broadly, we add to empirical evidence on the effects of environmental regulation on competitiveness and pollution offshoring (Jaffe et al. 1995; Keller and Levinson 2002; Greenstone 2002; Levinson and Taylor 2008; Hanna 2010; Aldy and Pizer 2015; Dechezlepr and Sato 2017; Cohen and Tubb 2018). For the EU ETS, most studies find that exposed firms reduced their emissions (Dechezleprêtre et al. 2023; Colmer et al. 2024) and small or insignificant effects on firms' competitiveness or carbon leakage (Jaraitė and Di Maria 2016; Marin et al. 2018; Naegele and Zaklan 2019; Löschel et al. 2019; Koch and Basse Mama 2019; Dechezleprêtre et al. 2022; Dechezleprêtre et al. 2023; Colmer et al. 2024; Bremer and Sommer 2025). Several scholars have attributed the absence of competitiveness effects in the first two phases of the EU ETS to the high share of free allocation (Joltreau and Sommerfeld 2019; Verde 2020; Venmans et al. 2020). This paper tests this hypothesis by applying a rigorous identification strategy to estimate the effect of free allocation on outcomes related to firm's competitiveness, carbon leakage and emissions. We thereby contribute to the understanding of what extent free allowances moderated potential anti-competitive effects of the EU ETS and if they impeded incentives to reduce emissions.

The rest of the paper is organized as follows: In Section 2, we describe the 2013 reform of EU ETS free allocation rules. The data are presented in Section 3. In Section 4, we explain our empirical strategy and discuss identification. The empirical results of the analysis are presented and discussed in Section 5. Section 6 concludes.

2 Reform of free allocation in the EU ETS

From 2005-2012, free allowances for industrial emitters were allocated based on historical emissions in the EU ETS. This *grandparenting* of allowances resulted in over-allocation such that firms' total free allocation exceeded their actual emissions (Sato et al. 2022; Quirion 2021). This changed in 2013, when free allocation rules for industrial installations were changed from grandparenting to *benchmarking*. With benchmarking, an installation's free allowances depend on its historic activity level and product specific emission intensity benchmarks (2011/278/EU 2011).[6] Benchmarks are calculated as the average emission intensity of a sector's best-performing installations. Consequently, the reform resulted in a substantial reduction of free allocation on average (see Figure 1), and for the vast majority of industrial installations and firms (see Figure 2).

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[6] In some cases, where product specific benchmark emission intensities could not be determined, the number of free allowances is calculated applying "fallback" benchmarks, i.e. more general heat- or fuel-benchmarks. If neither of these fallback benchmarks can be applied, then the number of free allowances are determined using a "process emissions approach", which is based on historical emissions (European Commission 2015).

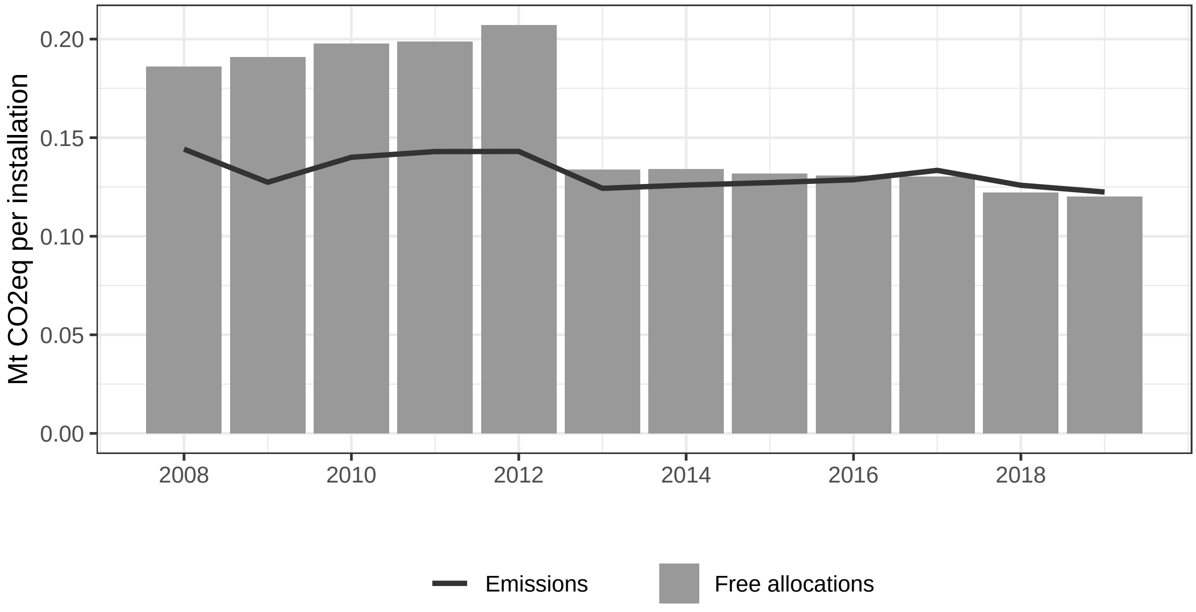

Figure 1: Free allocations and emissions per installation

Notes: The figure plots the average number of free allocations and emissions for ETS installations in the German manufacturing sector. The data source is the prepared data of the EU Transaction Log provided by Abrell (2024). This dataset indicates the NACE sector for most installations, which serves to identify manufacturing installations.

After the introduction of benchmarking in phase 3 of the EU ETS (2013–2020), the number of free permits allocated to installation i producing product s in year t, FA_{ist}, were calculated according to the following formula (European Commission 2015):

$$FA_{ist} = BM_s \cdot HAL_i \cdot CSCF_t \cdot CLEF_{st},$$

where BM_s is the product benchmark corresponding to the average emissions of the 10% least CO_2-intensive installations producing product s.[7] HAL_i is the historical activity level of installation i indicating its median historical yearly production between 2005–2008 or 2009–2010. $CSCF_t$ is the cross-sectoral correction factor, which is uniform across installations and ensures that the total amount of freely allocated emission permits does not exceed the aggregate cap multiplied with the share of baseline emissions in the manufacturing sector. $CLEF_{st}$ is the carbon leakage exposure factor. This factor equals 100% for installations in sectors on the carbon leakage list and declines from 80% in 2013 to 30% in 2020 for all other installations. Installations in carbon leakage risk sectors therefore receive the full benchmark allocation (i.e., $BM_s \cdot HAL_i \cdot CSCF_t$), whereas other installations receive only a share of this benchmark allocation for free.

Whether a sector is deemed to be at risk of carbon leakage is generally determined via

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[7] If an installation produces more than one product, it is divided into several "sub-installations". The boundaries of each sub-installation are set such that only one benchmark applies (European Commission 2015).

two quantitative criteria: the carbon intensity and the trade intensity. The carbon intensity is defined as the share of EU ETS induced carbon costs over gross value added (GVA). The trade intensity is defined as the value of non-EU exports + non-EU imports over the total EU-market size of the sector (EU production + extra-EU imports). A sector is deemed to be at risk of carbon leakage if (1) the carbon intensity is above 30%; or if (2) the trade intensity is above 30%; or if (3) carbon intensity is higher than 5% and trade intensity is higher than 10%.[8]

Installations in sectors on the carbon leakage list account for more than 85% of German industrial installations and over 95% of industrial emissions regulated by the EU ETS (see Figure A.1). As mentioned in the introduction, an identification strategy exploiting the differential reduction of free allowances between sectors with and without carbon leakage risk status, necessarily estimates a narrowly defined local treatment effect for the non-carbon leakage group. This group covers less than 15% of installations and less than 5% of emissions. To also estimate treatment effects for firms in carbon leakage risk sectors, we employ an alternative identification strategy, which we present in Section 4.

3 Data

The main data source for this project is an administrative panel of German manufacturing firms ("Amtliche Firmendaten für Deutschland" (AFiD)).[9] This dataset consists of different modules.[10] We combine three modules of this dataset. First, the *AFiD-Panel Manufacturing Firms* provides information on sales and employment for all German manufacturing firms with at least 20 employees. Moreover, a representative subsample reports its detailed cost measures including material spending and investments. Based on these data we calculate value added and capital stocks.

Second, the *AFiD-Module Energy Usage*, collects data on the physical quantity used, sourced and sold of 14 different energy carriers for all German manufacturing plants with more than 20 employees, which we aggregate at the firm-level. These two datasets have been previously used to analyze the impact of the EU ETS on German manufacturing firms (see e.g. Wagner and Petrick (2014) and Löschel et al. (2019)) and we refer the interested reader to these papers for a detailed description of the data.

Third, we obtain data on imports and exports of firms from the module *AFiD-Panel Foreign Trade Statistics*, which covers firm-level trade flows at the 8-digit product level differentiated by partner countries for the period 2011 to 2019.[11]

We augment these data with information on the firms and installations regulated by the EU ETS, the number of freely allocated allowances and emissions per installation and year from Abrell (2024), which is based on the European Union Transaction Log (EUTL) and covers all installations that are regulated under the EU ETS. We merge this EU ETS data on free

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[8] Figure A.2 in the appendix provides a graphical illustration of the EU's carbon leakage risk criteria.

[9] The data are not publicly available, but can be analyzed in regional research data centers of the German Statistical System.

[10] For detailed metadata of the data modules used in this paper see RDC (2022, 2023b, 2023a).

[11] See Fauth et al. (2023) for a detailed description of the trade dataset.

allowances and emissions to the AFiD-panel at the firm level via the company registration number, location of the registry and firm address.

The beginning of the third trading phase of the EU ETS involved not only a change in free allocation rules from grandparenting to benchmarking, but also an extension of the EU ETS to additional industrial activities. Thus, large parts of the chemical industry, the production of gypsum and the processing of ferrous metals have been newly regulated by the EU ETS since 2013. Moreover, the definition of combustion activities was broadened beyond energy-related activities and the third phase introduced changes in the administrative classification and definition of installations. Some installations operated by the same operator at the same location that had previously been monitored as separate installations have been grouped together and recorded as a single installation since the start of the third phase (Deutsche Emissionshandelsstelle (DEHST) 2014).

The extension of the sectoral scope of the EU ETS is likely correlated with firm-level changes of free allowances in 2013 - the first year of benchmarking. If an additional installation of a firm is regulated in 2013, this installation will receive free allowances. Thus, such firms might appear to have received a higher number of free allowances in 2013 compared to 2012 even if all of its installations faced a reduction due to the introduction of benchmarking. To separate the benchmarking-induced change in free allowances in 2013, from changes in firm-level free allowances due to newly regulated installations, we only include installations in our sample that were already active in the second phase of the EU ETS. Due to analogous considerations, we drop installations that closed in 2012 if they belong to firms with no other installation.[12] For firms with several installations out of which at least one installations closed at the end of 2012, we need to understand if this installation actually closed or only appears to have closed because it was merged with another installation. In all these cases we manually check whether this installation is merged with another continuing installation and exclude installations that actually closed at the end of 2012, whereas we keep those that have been merged to other installations in 2013.[13]

Moreover, we use the environmentally-extended input-output database Exiobase to calculate emissions embodied in imported products at the firm level.[14] Finally, we create an indicator whether a firm is in a carbon leakage risk sector based on the published carbon leakage risk sectors at a four digit sector level.

Our sample covers the years 2008 to 2019 and hence the entire second and third trading period except for the year 2020, which we omit due to the COVID-19 pandemic. We clean the dataset by excluding observations with evident data entry errors[15] and delete outliers.[16]

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[12] This firm would appear to have a drop of free allocation to zero although it stopped being regulated by the EU ETS.

[13] We need to keep installations that were merged to other installations at the beginning of phase 3 as excluding them (in phase 2) may lead to spurious correlation between 2013-changes in free allowances and changes in emissions at the firm-level.

[14] See Stadler et al. (2018) for a description of Exiobase and Köveker et al. (2025) for details on the calculation of emissions embodied in firm-level imports for German manufacturing firms.

[15] We exclude observations with negative free allowances, negative emissions, no more than one Euro of sales or no more than one worker. We also exclude firms that ever reported negative value added.

[16] We drop firms whose percentage changes in free allowances were below the 1^{st} or above the 99^{th} percentile

Moreover, we exclude firms that switch their main operating sector at a 2 digit level to ensure that our sector level results are not driven by changes in the composition of firms. Table 1 reports summary statistics of our final sample.

		Median	Mean	Std. dev.	N
1	Treatment (free allocation reduction in %)	30.15	31.31	24.73	3084
2	Value added (1,000,000 EUR)	50.50	543.80	2234.72	3084
3	Sales (1,000,000 EUR)	207.17	1889.72	7451.66	3084
4	Employees (number)	557.00	2979.84	10475.17	3084
5	Investments (1,000,000 EUR)	7.32	60.51	246.96	3084
6	Capital stock (1,000,000 EUR)	160.40	850.99	2734.76	3084
7	Labor productivity (1000 EUR/employee)	96.70	214.34	897.10	3084
8	Imports (1,000,000 EUR)	21.93	482.84	2030.01	2372
9	Exports (1,000,000 EUR)	41.49	815.43	3856.88	2372
10	Emissions (1,000,000 tCO2eq)	0.05	0.40	1.45	3084
11	Emission intensity (tCO2eq/1000 EUR of value added)	1.23	4.29	11.01	3084
12	Emission intensity (tCO2eq/1000 EUR of sales)	0.32	1.15	2.32	3084
13	Emissions embodied in non-EU imports (1,000,000 tCO2eq)	0.00	0.13	0.48	2372
14	Energy intensity (kWh/EUR of value added)	8.82	15.83	26.30	3084
15	Energy intensity (kWh/EUR of sales)	2.22	3.84	5.72	3084
16	Share fossil fuels (%)	83.29	75.51	24.02	3084
17	Share electricity & district heat	14.19	22.04	22.86	3084

Table 1: Descriptive statistics of key variables

4 Empirical strategy

The introduction of benchmarking in 2013 led to a stark overall reduction in the number of free allowances. We are interested in estimating the effect of this change in free allocation on various firm-level outcomes. Our empirical strategy relies on a continuous (or dosing) difference-in-differences design, in which we exploit the heterogeneous change in free allocation of emission permits that firms experienced following the switch from grandparenting to benchmarking. We define a continuous treatment variable, where the treatment dose corresponds to a firm's percentage reduction in freely allocated emission permits between 2012 and 2013:

$$d_i = \frac{FA_{i,2012} - FA_{i,2013}}{FA_{i,2012}}. \tag{1}$$

The treatment dose d_i is high for firms that experienced a large reduction in free allowances in 2013 and low for firms that experienced little change in their free allocation endowment after the introduction of benchmarking. The post-benchmarking change of free allocation varies across firms depending on the distance of their ETS installations to their product-specific benchmark value. As the benchmark value corresponds to the average of the first decile of the emission intensity distribution, the vast majority of firms has a positive treatment dose as most installations experienced a reduction in freely allocated emission permits. However, this reduction was heterogeneous across firms (see Figure 2). Firms with more emission-intensive installations (compared

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in 2013 - the year of our treatment. Similarly, we exclude firms whose percentage changes in emissions were below the 1^{st} or above the 99^{th} percentile in 2013 or if emissions increased by more than 100% in that year.

to their respective sectoral benchmark) experienced a larger reduction of free allowances, and, thus, have a higher treatment dose. Firms with installations whose emission intensity is close to their sector-specific benchmark experienced little change in their freely allocated permits and accordingly have a low treatment dose.[17]

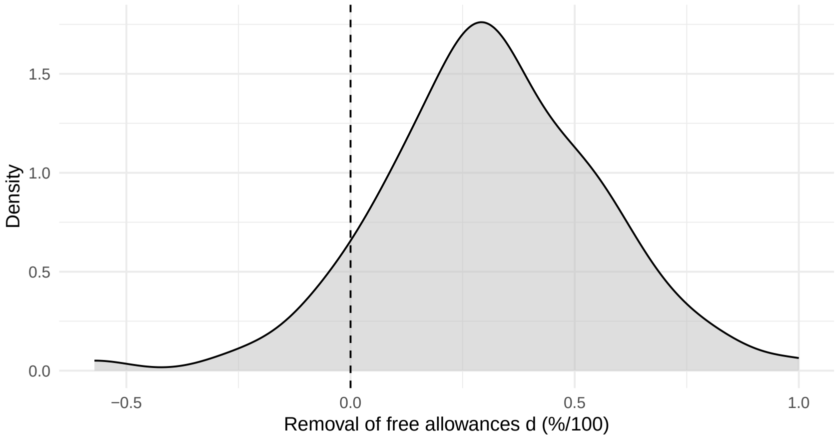

Figure 2: Density of the treatment

Notes: The figure shows the kernel density of $d_i = \frac{FA_{i,2012} - FA_{i,2013}}{FA_{i,2012}}$. Due to data confidentiality, the density estimation is based on a grouped dataset of at least three firms.

We use the following main specification to estimate the effect of changes in free allowances following the transition to benchmarking-based allocation:

$$\ln(y_{it}) = \alpha_i + \eta_t + \beta\, d_i\, \mathbb{1}_{t \geq 2013} + \Psi_{it} + \varepsilon_{it}, \tag{2}$$

where y_{it} is the outcome of interest of firm i at time t, α_i and η_t are firm and time-fixed effects and Ψ_{it} includes baseline controls and sector dummies, both interacted with time fixed effects. The coefficient of interest is β, which indicates the percentage increase in the outcome y_{it} attributable to a one-percent post-benchmarking reduction in free allowances. To account for the presence of zeros, we estimate (2) with the Pseudo-Poisson-Maximum-Likelihood estimator (PPML) proposed by Silva and Tenreyro (2006) for investments, exports, imports and emissions embodied in imports as dependent variables.

To identify the causal effect of d_i on the firm-level outcomes y_{it}, we rely on a set of well-known difference-in-differences assumptions.

The key identification assumption of this continuous difference-in-differences design is that of parallel trends of untreated potential outcomes between firms experiencing different doses d_i. The parallel trends assumption, thus, implies that firms' outcomes would on average have followed parallel paths, had their free allowances remained at the 2012 level, i.e. $d_i = 0$, regardless

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[17] Some firms have installations with a lower emission intensity than the sectoral benchmark. These firms even experienced an increase in their free allocation, and will have a negative treatment dose. However, this is only the case for a very low share of firms (8% of observations) as the benchmark is the average of the first decile of a sector's emission intensity distribution. Moreover, the median dose of firms that experienced an increase in their free allowances, is in absolute value much smaller than the median dose of firms that experienced a reduction (10% vs. 32%).

of their actual change in free allocations between 2012 and 2013. Put differently, the assumption requires that if the benchmarking reform would not have been introduced, outcomes would have evolved the same for firms that experienced small and large post-benchmarking changes in free allocation.

Testing parallel trends is infeasible given its counterfactual nature. We gauge the plausibility of parallel trends using an event study design, in which we replace the interaction of the treatment variable and the post dummy with interactions of the treatment variable and year dummies:

$$\ln(y_{it}) = \alpha_i + \eta_t + \sum_{\tau=2008,\neq 2012}^{2019} \beta_\tau \, d_i \, \mathbb{1}_{\tau=t} + \Psi_{it} + \varepsilon_{it}. \tag{3}$$

We omit the interaction of d_i with 2012 so that all event-study coefficients are measured relative to the last year before the introduction of benchmarking. If there were no differential outcome trends in the pre-period between firms with higher and lower doses d_i, then the event-study coefficients of the pre-period should not be significantly different from zero. Figure 3 and Figure 4 support the assumption of parallel trends for all of our estimations.[18] However, parallel trends in the pre-period are only indicative of the required, but untestable, parallel trends in the post-period.

To improve the plausibility of the parallel trends assumption, we therefore additionally perform entropy balancing along a set of baseline covariates following Tübbicke (2022). This approach assigns weights to each observation such that the correlation between the treatment and the specified balancing covariates is minimized. Thus, it improves the comparability of high and low-dose firms. We use the logarithmic value of value added, labor, emissions and of the capital to labor and energy to labor ratio, as well as the share of electricity from the grid in total energy consumption as balancing variables. To gauge whether the entropy balancing is also successful in removing differences in unobserved confounders, we perform placebo tests by checking the correlation of the treatment-dose with covariates excluded from the balancing algorithm in the reweighted sample. Figure A.4 in the appendix shows that already before balancing, most balancing variables as well as placebo variables are uncorrelated with the treatment. The entropy balancing removes significant correlations of covariates with the treatment except for the capital labor ratio.[19]

Second, to interpret β in equation (2) as the causal effect of d_i on y_{it}, we additionally need to invoke the no treatment anticipation assumption. The German National Implementation Measures (NIM), which determined the number of free allowances each installation would receive after the introduction of benchmarking, were published in May 2012 (Umweltbundesamt 2012).

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[18] Only for investments, two of the pre-treatment event study coefficients is significantly different from zero at the 5%-level in 2008 and 2010– however, this may be related to the turmoil during financial crisis and is unlikely to indicate fundamental differences in pre-trends.

[19] The entropy balancing algorithm sets the correlation of the balancing variables and the treatment variable exactly to zero (Hainmueller 2012; Tübbicke 2022). We perform the entropy balancing on the cross-section of firms in our baseline year 2012. In Figure A.4 the correlation of balancing variables and the treatment variable is however not exactly zero for two reasons: First, we truncate large weights to a maximum value of 2% – as suggested by Tübbicke (2022) – to limit on overly large influence by individual firms. Second, we apply the balancing weights from the baseline year to observations from all years of our unbalanced panel.

The information about firms' individual treatment doses was, thus, published just over half a year before the onset of treatment leaving firms little time to adjust their operations in anticipation of the treatment. As a precaution, we nevertheless perform a robustness check, in which we exclude the year 2012 from the estimation to control for any potential anticipatory behavior (see Section 5.2.1 and 5.2.2).

Third, the treatment needs to be independent of any other changes that occurred in 2013, and which were both correlated with treatment assignment and the outcome. In 2013, there was a change in the eligibility criteria of the national Special Equalization Scheme (Besondere Ausgleichsregelung), which allows electricity-intensive firms to apply for a reduction of the Renewable Energies Act levy (EEG-Umlage). To control for this regulatory change, we include a dummy indicating if a firm was affected by the change in eligibility criteria and interact it with year fixed effects. Moreover, with the beginning of the third trading period, EU ETS regulations on monitoring and free allocation of industrial waste gases were changed for some sectors (Ecofys 2009). To control for such changes, we include a firm's baseline waste gas production in 2012 interacted with year fixed effects. Finally, starting in 2013, the EU allowed countries to provide electricity price compensation to their national industries deemed to be at risk of carbon leakage, to compensate them for carbon costs in electricity prices. We control for the introduction of the electricity price compensation in Germany by including a firm's share of electricity that is eligible for compensation in the entropy balancing. We thereby ensure that the treatment dose d_i is not correlated to the amount of electricity price compensation that a firm receives.

Finally, we need to make one additional assumption as our setup is a continuous (or dosing) difference-in-differences design. For identification, we exploit the difference in outcome changes between high and low-dose firms. In this setup, we need to invoke the assumption that the average dose-response function is uncorrelated to treatment assignment. In other words, firms with a high dose would need to have had similar treatment effects, had they received a low dose, as low-dose firms that actually received a low dose (Callaway et al. 2024). The assumption that there is no correlation of the dose-response function with the actual treatment dose is more plausible if there is no correlation between the dose and observable covariates. Following Cook et al. (2023), we perform a balancing test, where we regress the treatment dose on various covariates. The results are summarized in Figure A.4 showing that there is no significant correlation between the treatment dose and observable covariates in the weighted sample that we use in our main estimation. As the entropy balancing also removed correlations of the treatment dose with covariates not included in the balancing algorithm, we can be confident that it was also successful in removing any potential correlation of the dose with the unobservable dose-response function.

5 Results

5.1 Effects of free allocation changes

5.1.1 Effects on emissions

Table 2 shows the results of estimating equation (2) with outcomes related to firm's emission intensity. The first two columns indicate that the removal of free allocation due to the switch to benchmarking, led to a significant decline in firms' emissions and emission intensity. A 1% reduction in free allocation on average induced firms to reduce their emissions and their emission intensity per EUR of sales by 0.1%–0.5% (95% confidence intervals).[20] The switch to benchmarking implied an average loss of free allowances of about 31% (Table 1). Our estimates imply that, at the firm-level, a corresponding reduction of free allowances lowered the emission intensity by 2.7%–15.9% and emissions by 2.5%–14.2% on average. Thus, our results suggest that the transition to benchmarking and the associated free allocation removal have led to modest emission intensity reductions, but did not induce deep decarbonization.

	Log Emission intensity	Log emissions	Log Energy intensity	Fossil share	Elec. + D.H. share	Emb. emissions in imports
Treatment	-0.297***	-0.267***	-0.0985	-0.0205	-0.00423	-0.0888
	(0.107)	(0.0951)	(0.113)	(0.0345)	(0.0310)	(0.409)
Observations	3,084	3,084	3,084	3,084	3,084	2,372

Table 2: Emission effects

Notes: Regressions of the dependent variables in columns on the change in free allowances between 2013 and 2012 (Treatment) according to specification (2). Standard errors (in parentheses) are clustered at the firm-level. Emission intensity and energy intensity are measured as emissions and energetic use of energy carriers relative to sales. Fossil fuel share is the share of oil, natural and industrial gases, coal and waste in the total energetic use of energy carriers. The fifth column shows the results for the share of electricity and district heating consumption in the total energetic use of energy carriers and the last column those for the emission embodied in firm-level imports from outside the EU. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk list, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and firms' baseline production of industrial gases. The first five columns are estimated using linear OLS and the last one with the Pseudo-Poisson-Maximum-Likelihood estimator.
Significance values: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1

What mechanisms drive these observed effects on emissions and emission intensity? Did firms reduce their emission intensity in production or did they outsource the most emission-intensive production steps? The latter would signal carbon leakage as the emission intensity

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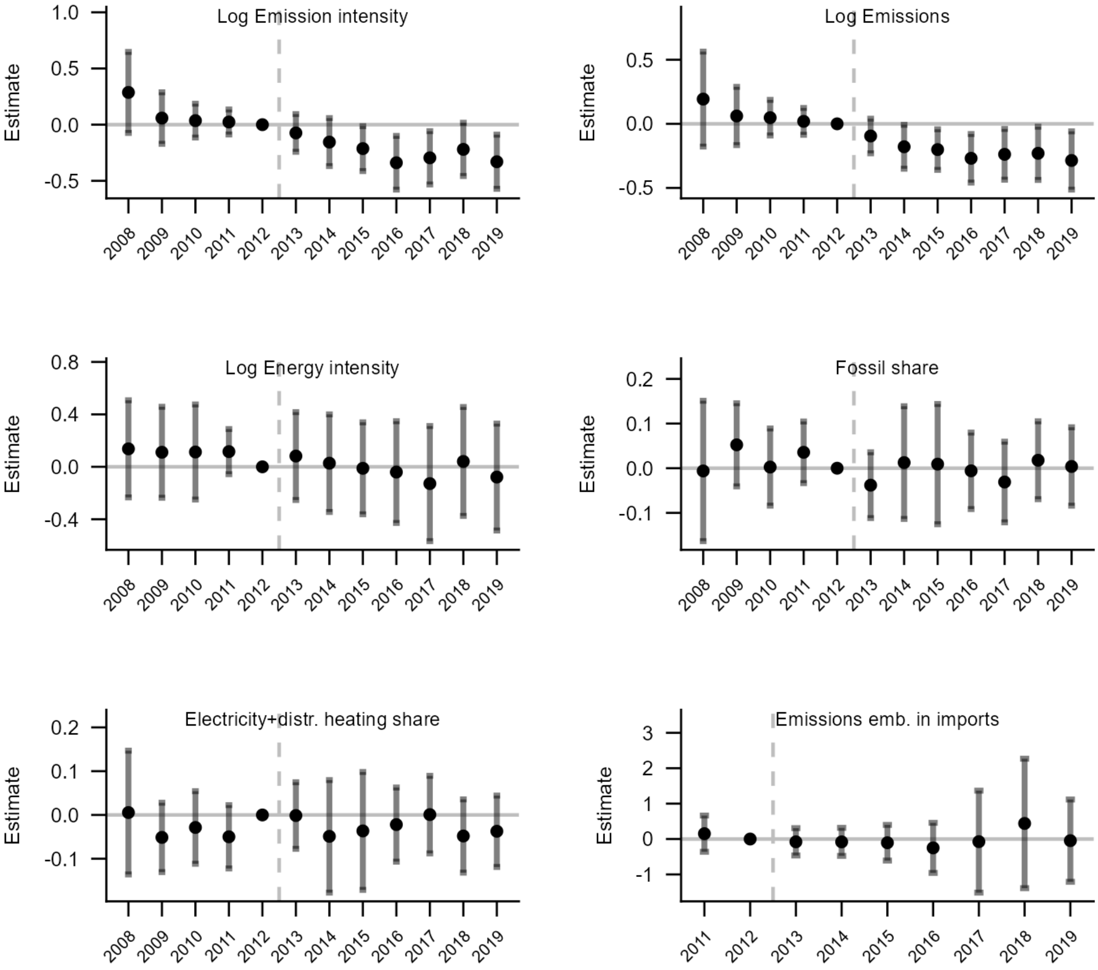
[20] An effect of similar magnitude is found on emissions per EUR of value added (see column 1 of Table A.1 in appendix A.3).

improvements are offset by an increase in emission-intensive production elsewhere.

Columns 3 - 5 show the estimated treatment effects on energy intensity and fuel switching. We find no statistically significant effects on either outcome, suggesting that the removal of free allowances did not lead firms to improve energy efficiency or substitute from fossil fuels toward electricity on average. The estimated effect on emissions embodied in non-EU imports in column 6 is likewise statistically insignificant and imprecisely estimated. Consistent with this, treatment effects on imports and import intensity (reported in columns 4 and 5 of Table A.1 in Appendix A.3) are also not statistically significant at conventional levels. We thus do not find evidence that the removal of free allocation has induced firms to import emission-intensive products.

While we cannot conclusively identify the mechanisms underlying the aggregate effects – potentially due to limited statistical power to detect small effect sizes – we explore these channels further in Section 5.3.1 by analyzing heterogeneous effects.

Interpreting these results as causal treatment effects relies on the parallel trends assumption. Figure 3 shows the event-study estimates according to specification (3) for the dependent variables in Table 2. The 95% confidence intervals of all pre-treatment coefficients include the zero, which indicates that firms with high and low treatment doses did not follow differential outcome trends prior to the reform. This supports the assumption that trends in untreated potential outcomes are also parallel after the reform.


Log Emission intensity

Log Emissions

Log Energy intensity

Fossil share

Electricity+distr. heating share

Emissions emb. in imports

Figure 3: Event study estimates on outcomes related to firms' emission decisions

Notes: The point indicates the event study estimate according to specification (3) and the whiskers the corresponding 95% confidence interval based on standard errors clustered at the firm level. Emission intensity and energy intensity are measured as emissions and energetic use of energy carriers relative to sales. Fossil fuel share is the share of oil, natural and industrial gases, coal and waste in the total energetic use of energy carriers. Electricity and district heating share is the share of electricity and district heating consumption in the total energetic use of energy carriers. The last panel shows the event study for the emission embodied in firm-level imports from outside the EU. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk list, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and firms' baseline production of industrial gases. The first five panels are estimated using linear OLS and the last one with the Pseudo-Poisson-Maximum-Likelihood estimator.

5.1.2 Effects on competitiveness

This section examines whether the removal of free allocation affected firms' competitiveness. Table 3 shows that the treatment did not have significant impacts on value added, employment, sales, capital, investments or exports.[21]

	Log Value Added	Log Employment	Log Sales	Log Capital	Investment	Exports
Treatment	0.0761 (0.129)	0.0667 (0.0461)	0.0303 (0.0794)	-0.00211 (0.0362)	0.0377 (0.152)	0.388 (0.302)
Observations	3,084	3,084	3,084	3,084	3,084	2,372

Table 3: Competitiveness effects

Notes: Regressions of the dependent variables in columns on the change in free allowances between 2013 and 2012 (Treatment) according to specification (2). Standard errors (in parentheses) are clustered at the firm-level. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk list, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and firms' baseline production of industrial gases. The first four columns are estimated using linear OLS and the last two with the Pseudo-Poisson-Maximum-Likelihood estimator.
Significance values: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1

As shown in Figure 4, parallel trends hold prior to the introduction of benchmarking for almost all dependent variables – only for investments we observe significant pre-period effects in 2008 and 2010. The removal of free allocation did not significantly impact firms' value added, employment, sales, capital and exports. Due to the parallel trends violation in the investment event study, we can however not rule out that the treatment had a significant negative effect on firms' investment, in spite of the insignificant coefficient in column 5 of Table 3.

Overall, our results indicate that the reduction of free allocation after the introduction of benchmarking in phase 3 of the EU ETS did not significantly impact firms' competitiveness. This absence of competitiveness effects on average is largely in line with the previous literature on carbon pricing and free allocation as discussed in the introduction.

We also do not find evidence for carbon leakage. If production and emissions of firms with high treatment doses shifted abroad, then this should be visible in negative effects on economic outcomes – such as sales – compared to firms with lower treatment doses. However, in Table 3 we do not find significant effects on any economic outcomes, suggesting that the removal of free allocation did not lead to a shift of market shares to regions with less stringent climate regulations. The observed effects on emission-related variables also do not seem to be driven by outsourcing of emission-intensive production steps. Outsourcing should be reflected in lower value added, however, the corresponding treatment coefficient is insignificant. Moreover, the treatment effects on imports and on emissions embodied in imported goods from non-EU countries are both insignificant.

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[21] In Appendix A.3 Table A.1, we also show the effect of the treatment on export intensity, which is also insignificant.

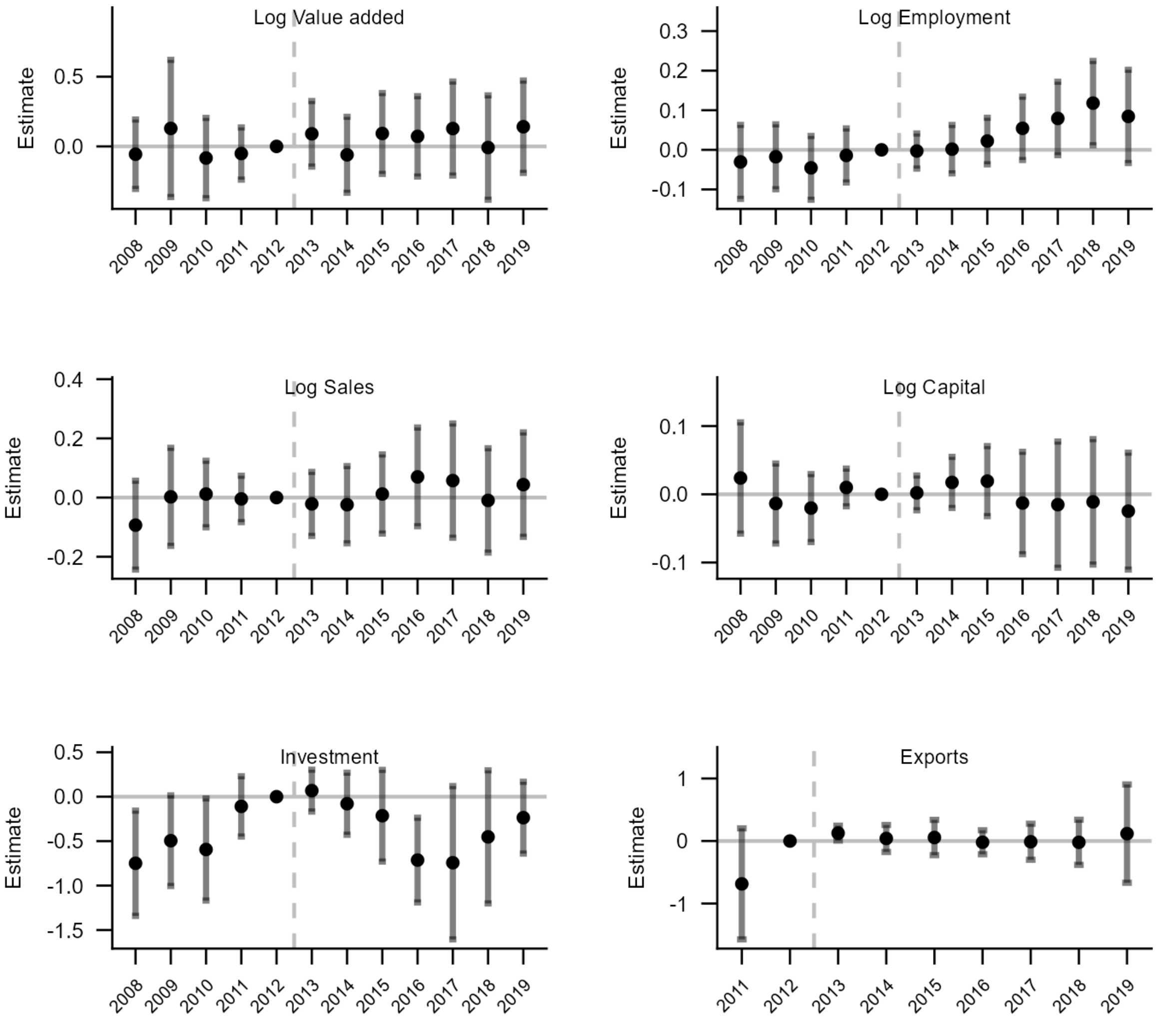

Figure 4: Event study estimates on outcomes related to firms' competitiveness

Notes: The point indicates the event study estimate according to specification (3) and the whiskers the corresponding 95% confidence interval based on standard errors clustered at the firm level. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: 2-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and the baseline production of industrial gases. The first four panels are estimated using a log-linear specification and the last two using the Pseudo-Poisson-Maximum-Likelihood estimator.

5.2 Robustness

5.2.1 Robustness of emissions effects

The validity of our estimates rests – among others – on the no anticipation assumption. As discussed in Section 4, the National Implementation Measures (NIM), which specified the provisional number of free allowances allocated to each installation following the introduction of benchmarking, were published by the German government in May 2012. This left firms with little time to adjust in anticipation of the treatment, which took effect in 2013. We nevertheless run a robustness check in which we exclude the year 2012 from our estimation to control for potential treatment anticipation. Panel A of Table A.2 in Appendix A.4 displays the results of estimating our main specification excluding the last pre-treatment year 2012. All coefficients have the same sign and significance levels as well as similar effect sizes as the ones from the main specification in Table 2, which supports the validity of the no-anticipation assumption.

Another concern may be that the estimation of our main specification is using an unbalanced panel. The panel is unbalanced for a number of reasons, including random sampling for small firms in the German manufacturing census, firm closures and the emergence of new firms. To address concerns that the unbalanced nature of our panel may affect our results, we perform a robustness check using only those firms that are observed throughout all years of our sample period. The results from this balanced panel specification are fully consistent with the baseline estimates in terms of coefficient magnitudes, signs, and statistical significance (see panel B of Table A.2).

Finally, we perform a robustness check in which we enforce entropy balancing by sector. In our main specification, we do entropy balancing across the whole sample. This ensures covariate balance between firms with lower and higher treatment doses in the entire sample. However, one may be concerned that imbalances remain within sectors. Panel C of Table A.2 shows that also this last robustness check yields very similar results to our main specification.[22]

5.2.2 Robustness of competitiveness effects

We run the same three robustness checks for our estimations with competitiveness-related dependent variables. The results are displayed in Table A.3 in Appendix A.4.

The results from the robustness check excluding the year 2012 to control for potential treatment anticipation are shown in Panel A. All coefficients are insignificant, mirroring the results of the main specification in Table 3.

The second robustness check, reported in Panel B using a balanced panel, yields results that are largely consistent with the baseline specification. The only notable difference is that the coefficient on log employment becomes statistically significant. Similarly, the final robustness check, reported in Panel C and based on entropy balancing by sector, produces estimates that closely mirror the baseline results. The only exception is the statistically significant coefficient on exports. In both cases, the estimated effects are positive and therefore do not alter our main result that reductions in free allocation did not adversely affect firms' competitiveness-related outcomes.

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[22] Entropy balancing by sector does not converge for refineries, which are thus excluded in this estimation.

5.3 Effect heterogeneity

5.3.1 Heterogeneous effects on emissions

Our analysis has so far focused on estimating average effects for the entire sample. We now perform several heterogeneity analyses to investigate how the treatment effects vary along several characteristics across firms.

First, we are interested in understanding whether the treatment effects vary with firm size. Zaklan (2023) finds that the independence property holds for large firms but not for small firms in the electricity sector. If this pattern also applies to manufacturing firms, we should find significant effects of the treatment on emission-related variables for small but not for large firms. We interact the treatment with an indicator variable that takes the value of one if a firm's baseline sales are above median sales and zero otherwise. From this interaction we can compute group-specific treatment effects for large firms (with sales above the median) and small firms (with below median sales). Panel A of Table 4 shows that there are no significant differences between the coefficients for emissions-related outcomes of large and small firms. In particular, the significant negative effect of the treatment on emission intensity and emissions can be found in both subgroups.

Next, we investigate effect heterogeneity with respect to baseline emission intensity. The idea is to analyze whether firms with a larger emission intensity in the pre-treatment period had more emission reduction potential and did thus react more strongly to the treatment. We interact the treatment with a binary variable that indicates if a firm's baseline emissions are above or below the median emission intensity. The resulting group-specific treatment effects for firms with above and below median baseline emission intensity are reported in Panel B of Table 4. We again do not find any significant differences between the coefficients of the two groups.

In Panel C of Table 4, we investigate whether firms operating in sectors on the EU's carbon leakage risk list reacted differently to a removal of free allowances than firms that are not deemed to be at risk of carbon leakage. The treatment effects on emissions and emission intensity are now only significant for the firms on the carbon leakage list, but not anymore for firms that are not on the list. However, both coefficients are negative and do not differ from each other at conventional significance levels. The loss of significance of the emission-related coefficients for non-carbon leakage firms may thus be due to the smaller sample size and lower statistical power in that group. The abatement mechanism varies between firms in sectors on the carbon leakage risk and those that are not. The former seem to have reduced their emissions by lowering their energy intensity, while non-carbon leakage risk firms appear to have substituted away from fossil fuels. In particular, energy intensity improvements are abatement options situated at the lower end of the Marginal Abatement Cost Curve (Naucler and Enkvist 2009). Firms' reliance on these relatively inexpensive abatement options is consistent with the low carbon prices during our sample period.

	Log Emission intensity	Log emissions	Log Energy intensity	Fossil share	Elec. + D.H. share	Emb. emissions in imports
	Panel A: median split by sales					
Below med. sales	-0.310***	-0.202**	-0.140	-0.0338	0.00260	-0.200
	(0.105)	(0.0988)	(0.156)	(0.0338)	(0.0291)	(0.765)
Above med. sales	-0.285**	-0.332***	-0.0573	-0.00723	-0.0110	-0.0872
	(0.132)	(0.110)	(0.118)	(0.0457)	(0.0432)	(0.413)
Observations	3,084	3,084	3,084	3,084	3,084	2,372
	Panel B: median split by emission intensity					
Below med. emission intensity	-0.390***	-0.365***	0.00682	-0.0324	-0.00225	-0.256
	(0.135)	(0.129)	(0.203)	(0.0453)	(0.0418)	(0.432)
Above med. emission intensity	-0.234**	-0.200**	-0.170	-0.0125	-0.00557	0.465
	(0.107)	(0.0889)	(0.104)	(0.0401)	(0.0371)	(0.511)
Observations	3,084	3,084	3,084	3,084	3,084	2,372
	Panel C: carbon leakage risk					
Not on CL risk list	-0.613	-0.406	0.673	-0.239**	0.0668	0.142
	(0.380)	(0.376)	(0.602)	(0.114)	(0.101)	(0.621)
On CL risk list	-0.255**	-0.248***	-0.201**	0.00839	-0.0136	-0.0932
	(0.107)	(0.0920)	(0.0908)	(0.0326)	(0.0311)	(0.415)
Observations	3,084	3,084	3,084	3,084	3,084	2,372

Table 4: Heterogeneous emission effects

Notes: Regressions of the dependent variables in columns on the change in free allowances between 2013 and 2012 (Treatment) interacted with indicators for different subgroup. The rows indicate the subgroup-specific treatment effects. Panel A shows estimates separately for firms with sales below and above the median in our sample in 2012. Panel B shows estimates separately for firms with emission intensity in terms of sales below and above the median in our sample in 2012 and Panel C separately for firms in sectors, which fall under the carbon leakage risk list of 2014 (EC Decision C(2014) 7809)). Standard errors (in parentheses) are clustered at the firm-level. Emission intensity and energy intensity are measured as emissions and energetic use of energy carriers relative to sales. Fossil fuel share is the share of oil, natural and industrial gases, coal and waste in the total energetic use of energy carriers. The third column shows the results for the share of electricity and district heating consumption in the total energetic use of energy carriers. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk list, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for industrial electricity price compensation, and firms' baseline production of industrial gases. The first four columns are estimated using linear OLS and the last one with the Pseudo-Poisson-Maximum-Likelihood estimator.

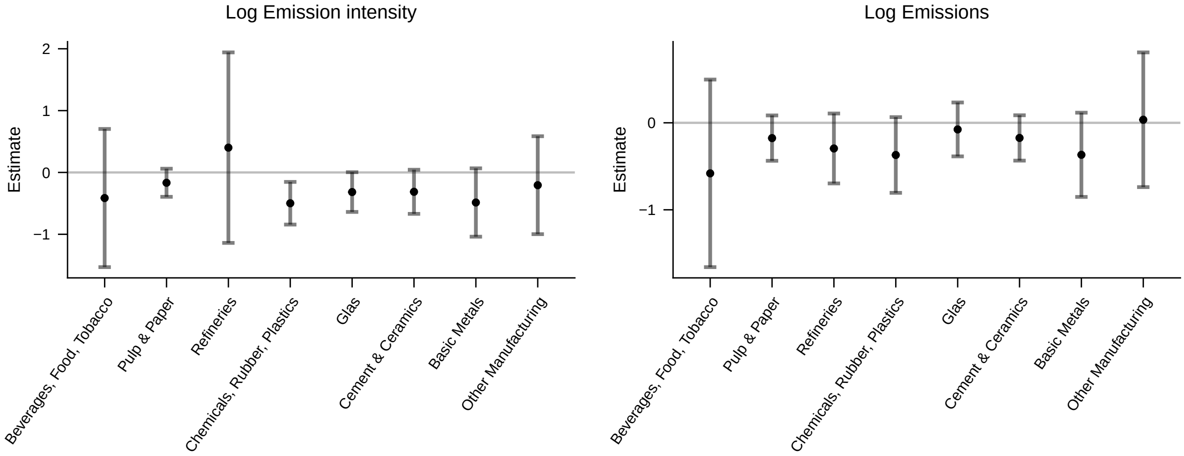

Figure 5: Effects on the emissions and emission intensity by sector

Notes: Point estimates indicate estimates for the corresponding sector (2) and the whiskers the corresponding 95% confidence intervals based on standard errors clustered at the firm level. Estimates based are on a regression with an interaction of sector dummies and the treatment. Emission intensity is measured in terms of sales. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and the baseline production of industrial gases.

Finally, we investigate how the effects of free allowance removal differ by sector.[23] Figure 5 displays the sector-specific treatment effects for the two variables for which we found significant effects on average (log emission intensity and log emissions). The left panel shows that the removal of free allowances led to significant emission intensity reductions in the chemicals, rubber and plastics sector and to marginally significant emission reductions (at the 10% significance level) in the glass, cement & ceramics and basic metals sectors. The (marginally) significant treatment effects are concentrated in the energy-intensive basic material sectors. However, all other sectors display negative coefficients as well, albeit not statistically significant (with the exception of refineries, where the effect is insignificantly positive and very imprecisely estimated). The average negative treatment effect on emission intensity is thus not strongly driven by a particular subset of sectors, but seems to be present across most sectors.

The right panel of Figure 5 displays a similar picture for the sector-specific treatment effects on emissions. Almost all sectors have negative coefficients with the exception of other manufacturing, where the coefficient is positive but very close to zero. Due to lower statistical power in

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[23] The sectors are based on NACE 2 and NACE 3 codes, and are more granular than the NACE 2 classification for energy-intensive and trade-exposed basic material sectors, e.g. in the case of glass, ceramics and cement. For other less energy-intensive manufacturing sectors, our sector definition is less granular than the NACE 2 classification. This sector definition reflects the fact that most regulated EU ETS installations are in energy-intensive sectors, and only few regulated installations are in less energy-intensive manufacturing sectors allowing us to have a higher granularity in energy-intensive sectors than in less energy-intensive sectors, where we need to combine several NACE 2 codes to have a sufficient sample size for our estimation.

the sector-specific estimation compared to the estimation of the average treatment effect, most of these coefficients are insignificant. Only the negative treatment effect of the chemicals, rubber and plastics sector is marginally significant at the 10% level.

The sector-specific treatment effect estimates for the other emission related variables are displayed in Figure A.5 in Appendix A.5. The only sector with a significantly negative treatment effect on energy intensity is basic metals. However, all other sectors with (marginally) significant negative treatment effects on emission intensity have negative (although insignificant) energy intensity coefficients as well, which may suggest that the emission intensity reductions could be driven by improvements in firms' energy intensity. Treatment effects for the share of fossil fuels in total energy consumption are mostly insignificant and close to zero. The only exception is the beverages, food and tobacco sector where the treatment effect is negative and marginally significant at the 10% level, possibly reflecting the sector's good ability to substitute fossil fuels with biomass and biogas. The treatment also did not lead to electrification as effects on the share of electricity and district heating are insignificant and close to zero across all sectors.
Moreover, we do not find evidence for any outsourcing of emission-intensive production as we do not observe any increase in firm-level imports and in emissions embodied in those imports in response to the treatment for most sectors. The only exception is the refinery sector where the treatment is associated with an increase in imports and embodied emissions. However, this rather seems to reflect an increase of intermediary trading activity of refinery firms with high treatment dosages as the sector's coefficient for exports is also significantly positive (see Figure A.5). Moreover, the sector is peculiar because many refinery firms only have their headquarters in Germany, but production is mostly outsourced to third countries (Richter and Schiersch 2017). We therefore refrain from further interpreting significant effects on trade related variables for the refinery sector.

5.3.2 Heterogeneous effects on competitiveness

Even though we did not find significant effects on any competitiveness related variable on average, there may nevertheless exist significant effects for some subgroups of firms.

As for the heterogeneity analysis of emission related variables, we first investigate whether effects on competitiveness related variables differ by firm size. Panel A in Table 5 shows that treatment effects remain insignificant for all variables for larger firms (with above median sales) as well as for smaller firms (with below median sales).

Similarly, Panel B shows that treatment effects remain insignificant across all competitiveness related variables for the subgroups of firms above and below the median emission intensity.

In Panel C, where we estimate treatment effects for the firms operating in sectors on the carbon leakage list and the group of firms not deemed to be at risk of carbon leakage, we again do not find any significant competitiveness effects of a reduction in free allocation. The previously insignificant positive coefficients on value added and employment become significant for firms not at risk of carbon leakage. We nonetheless refrain from placing too much emphasis on these effects: carbon costs represent only a small share of value added for firms outside the carbon leakage list, and should therefore not be a strong determinant of their operations.

Figure A.6 in Appendix A.5 shows sector-specific treatment effects for competitiveness related variables. In line with the absence of negative competitiveness effects in the previous estimations, we also do not find any significant negative sector-specific treatment effects.[24]

	Log Value Added	Log Employment	Log Sales	Log Capital	Investment	Exports
	Panel A: median split by sales					
Below med. sales	0.0410	0.0804	0.108	-0.0483	0.178	0.416
	(0.141)	(0.0495)	(0.0793)	(0.0364)	(0.338)	(0.296)
Above med. sales	0.111	0.0531	-0.0470	0.0440	0.0304	0.387
	(0.146)	(0.0647)	(0.104)	(0.0477)	(0.151)	(0.303)
Observations	3,084	3,084	3,084	3,084	3,084	2,372
	Panel B: median split by emission intensity					
Below med. emission intensity	0.158	0.0661	0.0243	-0.00343	0.0478	0.397
	(0.146)	(0.0583)	(0.0884)	(0.0462)	(0.158)	(0.310)
Above med. emission intensity	0.0206	0.0672	0.0344	-0.00121	-0.0278	0.251
	(0.143)	(0.0572)	(0.0941)	(0.0363)	(0.328)	(0.309)
Observations	3,084	3,084	3,084	3,084	3,084	2,372
	Panel C: carbon leakage risk					
Not on CL risk list	0.661***	0.236*	0.207	0.0564	0.466	-0.596
	(0.233)	(0.140)	(0.188)	(0.0927)	(0.318)	(0.583)
On CL risk list	-0.00126	0.0444	0.00693	-0.00985	0.0125	0.420
	(0.139)	(0.0477)	(0.0849)	(0.0388)	(0.157)	(0.314)
Observations	3,084	3,084	3,084	3,084	3,084	2,372

Table 5: Heterogeneous competitiveness effects

Notes: Regressions of the dependent variables in columns on the change in free allowances between 2013 and 2012 (Treatment) interacted with indicators for different subgroup. The row indicate the subgroup-specific treatment effects. Panel A shows estimates separately for firms with sales below and above the median in our sample in 2012. Panel B shows estimates separately for firms with emission intensity in terms of sales below and above the median in our sample in 2012 and Panel C separately for firms in sectors, which fall under the carbon leakage risk list of 2014 (EC Decision C(2014) 7809)). Standard errors (in parentheses) are clustered at the firm-level. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk list, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and firms' baseline production of industrial gases. The first four columns are estimated using linear OLS and the last two with the Pseudo-Poisson-Maximum-Likelihood estimator.
Significance values: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1

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[24] The only exception is the negative treatment effect on investment in the beverages, food and tobacco sector. However, we refrain from interpreting this as evidence for a negative effect of free allocation removal on investments due to the parallel trends violation for investments.

6 Conclusion

Most cap-and-trade systems use free allowances for competitiveness protection – including the EU ETS, which is the world's largest emission trading system in terms of value (ICAP 2024). Since the start of the EU ETS in 2005, emission permits worth several hundred billion EUR have been allocated free of charge to polluters. Given the economic magnitude of carbon cost compensation in the form of free allowances, there is surprisingly little empirical evidence on its effects.

In this paper, we exploit a change in free allocation rules of the EU ETS to empirically evaluate the effects of carbon cost compensation in the form of free allowances on German manufacturing firms. We find robust evidence that a reform-induced reduction in free allowances incentivized firms to reduce their emissions and emission intensity. We do not find any evidence that this reduction is driven by outsourcing of emission-intensive production steps. Instead our results point to improvements in energy efficiency – at least for firms operating in sectors on the carbon leakage risk list. This result casts doubts on whether the independence property of permit allocation holds in the manufacturing sector. Moreover, our estimates imply that the reduction in free allowances did not harm firms' competitiveness, as we do not find significant effects on sales, value added, exports, capital and employment on average.

Some caveats apply. First, our analysis covers the years 2008-2019 with low to moderate carbon prices in the EU ETS. It is possible that higher carbon prices would lead to different outcomes. Second, we only evaluate competitiveness effects along the intensive margin. Whether free allocation impacts firm closures and the set up of new plants in the EU or third countries is an interesting question for further research. Finally, our results are likely context-dependent with Germany being a developed economy embedded into the larger EU common market, such that many of its important trade partners are exposed to the same carbon price. Results may look different for other countries with different industry characteristics and a different trade structure.

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A Appendix

A.1 Additional Information on EU ETS and Free Allocation

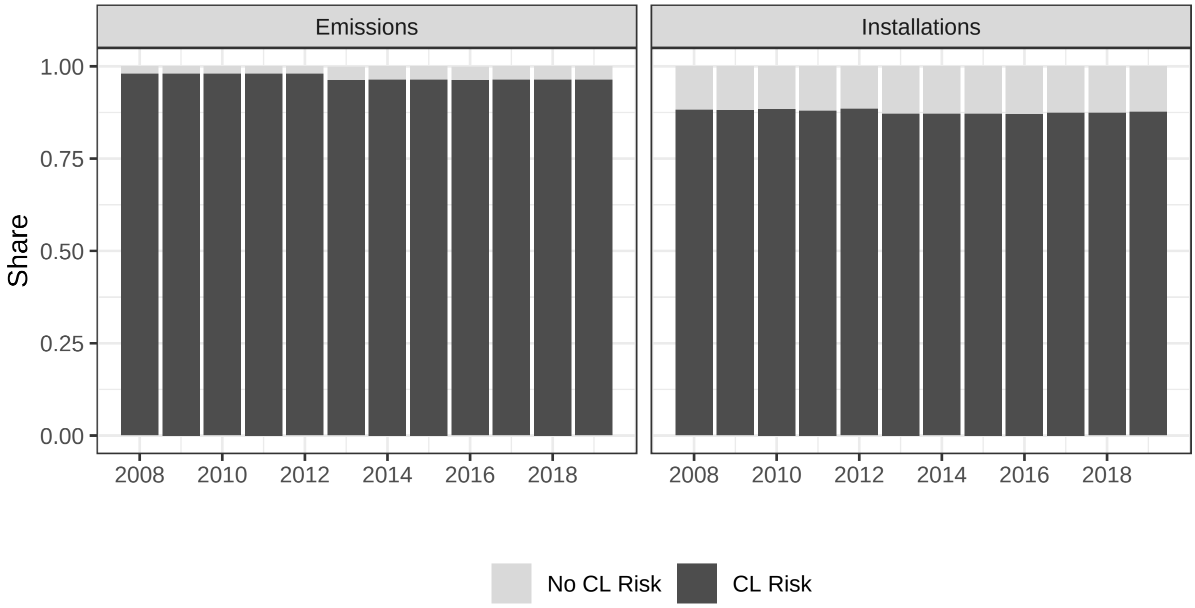

Figure A.1: Share of emissions and installations on carbon leakage risk list

Notes: The figure plots the share of emissions, and installations in German manufacturing separated by sectors, which fall under the carbon leakage risk list of 2014 (EC Decision C(2014) 7809)). The data source is the prepared data of the EU Transaction Log provided by Abrell (2024). This dataset indicates the NACE sector for most installations, which serves to identify manufacturing installations.

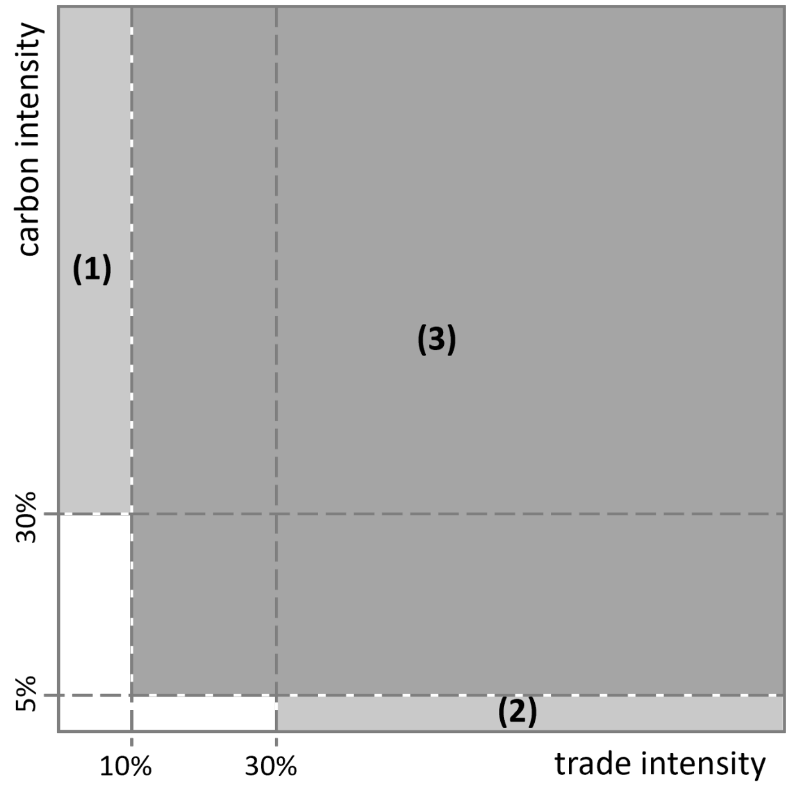

Figure A.2: Carbon leakage risk criteria

Notes: If a sector falls into one of the shaded areas, then one of the three EU ETS carbon leakage risk criteria applies and the sector is included in the carbon leakage list and receives 100% of the benchmark free allocation. If the sector is in the non-shaded area, then it is not considered to be at a significant risk of carbon leakage and it only receives a declining fraction of the benchmark free allocation (80% in 2013 and 30% in 2020).

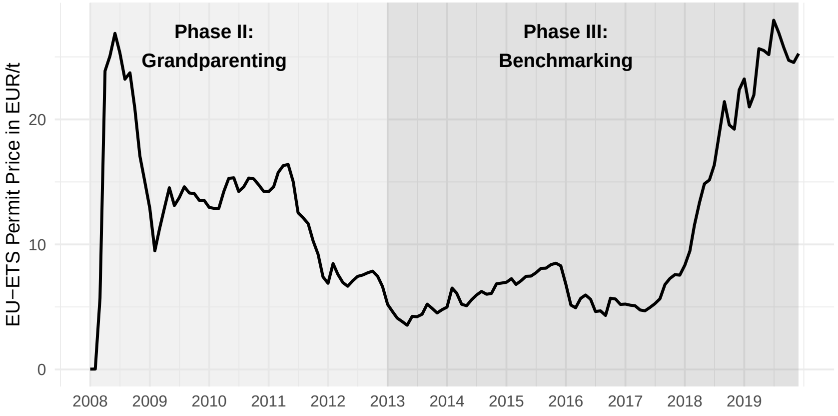

Figure A.3: Allowance price in the EU ETS

A.2 Covariate balance

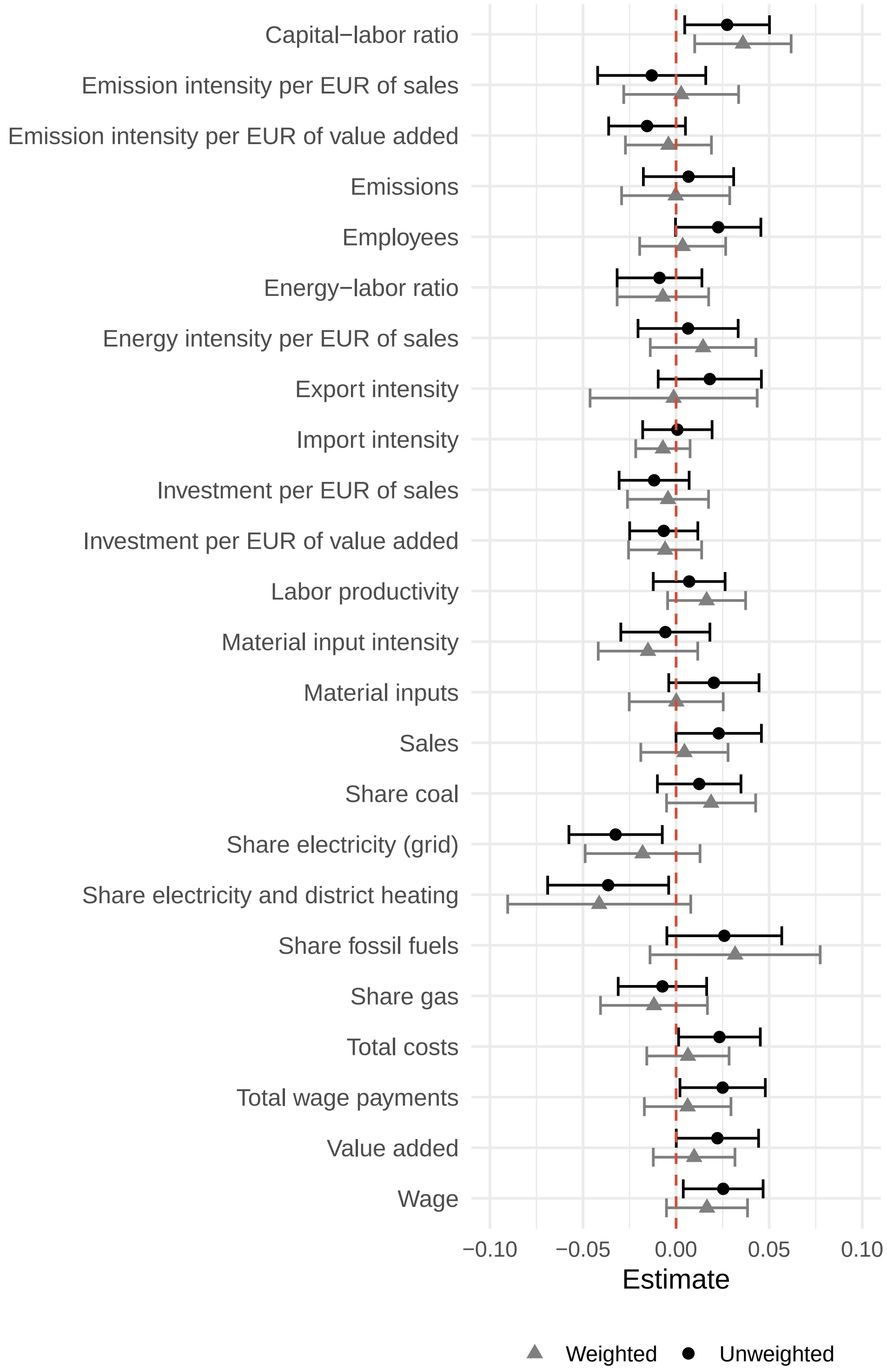

Figure A.4: Balance plot: Free allocation removal and covariates

Notes: The plot shows coefficients from regressing the treatment variable (benchmarking induced free allocation removal in percent) on various covariates. Each coefficient comes from a separate regression of the treatment on the respective covariate. All regressions include year fixed effects. To make coefficients comparable, the covariates have been standardized to have mean 0 and a standard deviation of 1. The horizontal bars represent 95% confidence interval and the black dots represent coefficient estimates from unweighted regressions, whereas the grey triangles represent coefficients from weighted regressions with entropy balancing weights.

A.3 Further outcomes (main specification)

	Log Emission intensity (VA)	Log Energy intensity (VA)	Export intensity	Import intensity	Imports
Treatment	-0.343**	-0.144	0.0295	0.143	-0.187
	(0.137)	(0.147)	(0.0562)	(0.132)	(0.218)
Observations	3,084	3,084	2,372	2,372	2,372

Table A.1: Further outcomes

Notes: Same specification as in Table 2, but with different dependent variables. Log emission intensity and energy intensity in terms of value-added. Export and import intensity measured as exports/imports per sales.
Significance values: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1

A.4 Robustness

A.4.1 Effects on emission related variables

	Log Emission intensity	Log emissions	Log Energy intensity	Fossil share	Elec. + D.H. share	Emb. emissions in imports
	Panel A: excluding anticipation effects					
Treatment	-0.336***	-0.287***	-0.134	0.0129	-0.0194	-0.311
	(0.114)	(0.110)	(0.105)	(0.0494)	(0.0380)	(0.365)
Observations	2,762	2,762	2,762	2,762	2,762	1,961
	Panel B: balanced panel					
Treatment	-0.289**	-0.234**	-0.0655	-0.0362	0.0125	-0.145
	(0.143)	(0.111)	(0.184)	(0.0464)	(0.0412)	(0.549)
Observations	2,352	2,352	2,352	2,352	2,352	1,764
	Panel C: entropy balancing by sector					
Treatment	-0.474***	-0.400**	-0.236	-0.0731	0.0186	-0.451
	(0.176)	(0.160)	(0.181)	(0.0606)	(0.0511)	(0.492)
Observations	2,915	2,915	2,915	2,915	2,915	2,242

Table A.2: Robustness checks: emission related variables

Notes: Specification and dependent variables as in Table 2. Standard errors (in parentheses) are clustered at the firm-level. Panel A excludes the year 2012 and calculates all baseline controls based on year 2011. Panel B shows estimates for the subsample of firms that are observed through the entire period, i.e. on a balanced sample. Panel C does the entropy balancing separately for each sector. This balancing does not converge for refineries, which are thus excluded in this estimation.
Significance values: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1

A.4.2 Effects on competitiveness related variables

	Log Value Added	Log Employment	Log Sales	Log Capital	Investment	Exports
	Panel A: excluding anticipation effects					
Treatment	0.131	0.0687	0.0488	0.00685	0.170	0.521
	(0.116)	(0.0475)	(0.0711)	(0.0394)	(0.162)	(0.437)
Observations	2,762	2,762	2,762	2,762	2762	1961
	Panel B: balanced panel					
Treatment	0.175	0.126**	0.0550	0.00301	-0.0165	0.573
	(0.164)	(0.0575)	(0.120)	(0.0546)	(0.180)	(0.371)
Observations	2,352	2,352	2,352	2,352	2352	1764
	Panel C: entropy balancing by sector					
Treatment	0.205	0.0291	0.0737	0.00599	0.195	0.625**
	(0.166)	(0.0652)	(0.104)	(0.0359)	(0.231)	(0.289)
Observations	2,915	2,915	2,915	2,915	2,915	2,242

Table A.3: Competitiveness effects

Notes: Specification and dependent variables as in Table 3. Standard errors (in parentheses) are clustered at the firm-level. Panel A excludes the year 2012 and calculates all baseline controls based on year 2011. Panel B shows estimates for the subsample of firms that are observed through the entire period, i.e. on a balanced sample. Panel C does the entropy balancing separately for each sector. This balancing does not converge for refineries, which are thus excluded in this estimation.

Significance values: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1

A.5 Effect Heterogeneity

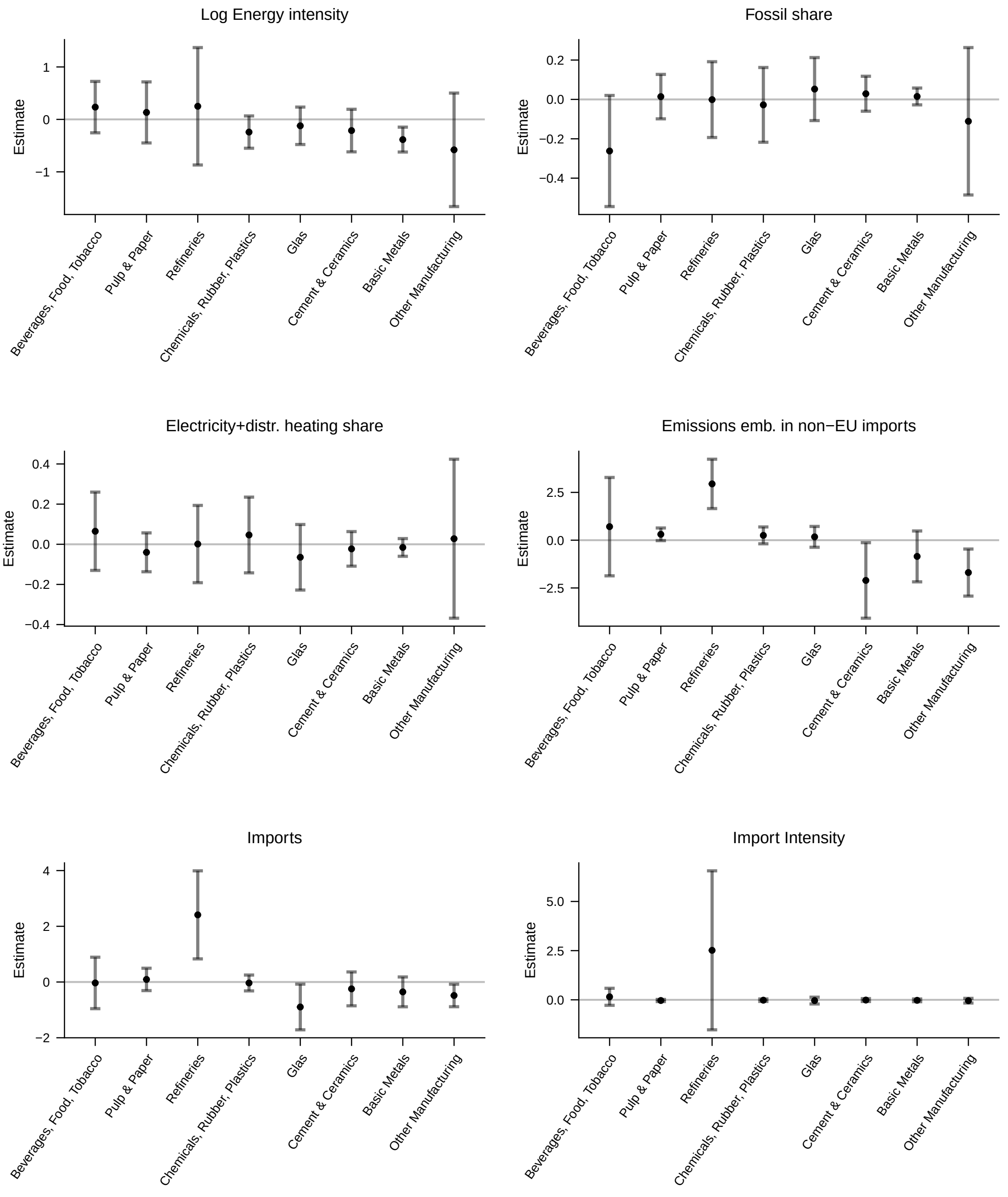

Figure A.5: Effects on further emission related variables

Notes: The point indicates the estimate for the corresponding sector and the whiskers the corresponding 95% confidence interval based on standard errors clustered at the firm level. Estimates based on a regression with an interaction of sector dummies and the treatment. Emission and energy intensity measured in terms of value added. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and the baseline production of industrial gases.

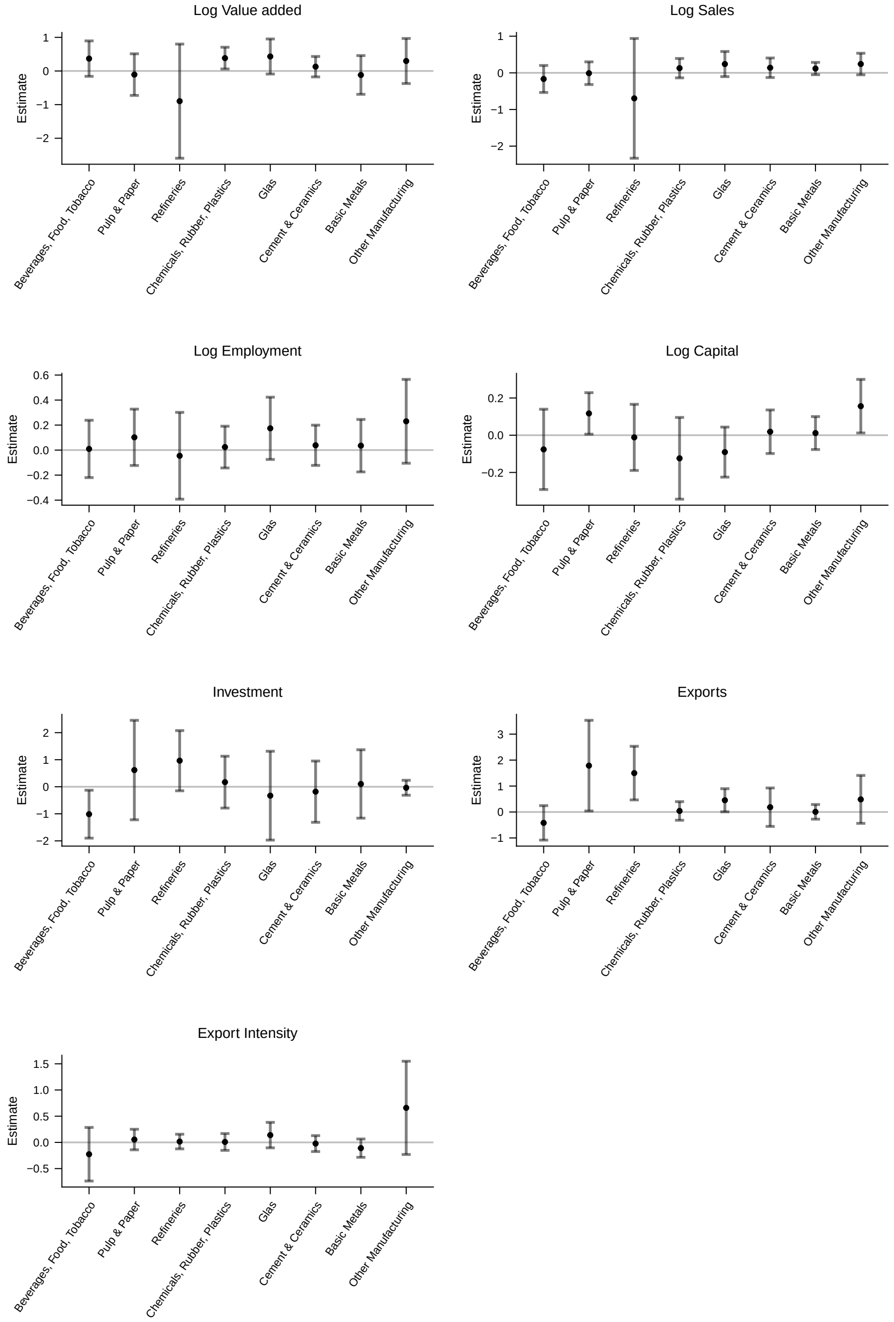

Figure A.6: Competitiveness effects by sector

Notes: The point indicates the estimate for the corresponding sector and the whiskers the corresponding 95% confidence interval based on standard errors clustered at the firm level. Estimates based on a regression with an interaction of sector dummies and the treatment. All regressions include firm-fixed effects and the interaction of the following variables with year-fixed effects: two-digit sector fixed effects, an indicator, whether a firm operates in a sector on the carbon leakage risk, an indicator, whether a firm was affected by the change in the eligibility criteria for the national Special Equalization Scheme, the share of baseline electricity that is eligible for electricity price compensation, and the baseline production of industrial gases. Outcomes in logs estimated with linear OLS and investments and exports with the Pseudo-Poisson-Maximum-Likelihood estimator.