Carbon Permits, Plant Emissions and Industry Dynamics: To Cut or To Quit?

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Abstract

Market-based climate policies aim to reduce greenhouse gas emissions while minimizing economic distortions, yet their full impact on firm behavior and survival remains debated. This paper examines how differences in free carbon permit allocations under the EU Emissions Trading System affected the emissions and exit decisions of French industrial plants. Using a difference-in-differences approach, I classify plants by allocation stringency, ownership structure, and pre-existing permit holdings. I find that plants facing stricter allocation cuts reduced emissions more than their peers, but that part of this decline reflects compositional effects from plant closures and within-firm reallocation of production. A complementary survival analysis shows that plants with higher compliance costs—stemming from inefficient emission intensity—were significantly more likely to exit and cease operations. These results suggest that tightening free permit allocations can lower emissions both through efficiency improvements among surviving plants and through market selection, thereby reshaping industry structure and the aggregate emission profile of remaining producers.

Keywords: Carbon emissions, Tradable permits, Allocation rule, Plant exit, Manufacturing
JEL: L11, L60, Q54, Q58

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1. Introduction

The European Union Emissions Trading System (EU ETS) is the world's largest carbon market. While extensive research has assessed its impact on emissions and firm outcomes, existing studies have mostly relied on comparisons between regulated and non regulated plants, potentially overlooking important within-policy variation in regulatory stringency. In this sense, evidence on exclusively ETS-covered plants that accounts for policy heterogeneity is scarce. On top of that, while it is generally confirmed that since its onset the EU ETS led to a modest overall decline in emissions (see Green (2021) for a review of studies), the question on whether this reduction stems from firm-level abatement or from compositional effects due to plant exits remains open. This distinction is crucial for evaluating the effectiveness of carbon pricing policies. Indeed, if emissions reductions primarily result from plant closures rather than technological improvements, this raises concerns with respect to industry competitiveness and possible economic distortions.

Addressing these gaps, this paper explores a research question that remains unresolved: Is the reduction in emissions, observed in much of the current literature, due to a general emission decline across all plants, or is it primarily driven by a compositional effect of the surviving sample?

By focusing on the sample of incumbent French ETS-covered manufacturing plants and developing three measures of policy stringency and policy exposure at the plant level, I analyze the impact that a 2013 change in ETS carbon permit allocation policy had on both plant-level emissions and plant exit rates of ETS-covered plants. I distinguish between two policy dimensions: (1) policy stringency, which is higher for plants with greater free permit reductions since 2013, relative to their activity peers; and (2) policy exposure, which is higher for plants with fewer pre-existing banked permits — that grants them limited ability to compensate for free allocated permits using past banked permits — and plants owned by single-plant firms — that have limited ability of relocating emissions to more efficient plants owned by the same parent firm.

I rely on a difference-in-differences (DiD) strategy to examine emission outcomes in industrial plants, defining treatment and control groups based on the relative policy stringency dimension. I find that industrial plants facing a relatively stronger permit policy stringency exhibited greater emission reductions than peers within the same activity group, and that plants began adjusting their emissions levels even before the policy change was implemented. However, at least one-third of these overall emission reductions are attributed to plant exits or within-firm emission reallocation, implying that selection and within-firm adaptation effects played some role in shaping aggregate outcomes[1]. I then develop a survival analysis model for plants subject to different levels of ex-ante policy exposure and policy stringency, and explicitly differentiate mere sample exits from full operational shutdowns. Results show that sample exit risk was at least 50% higher among plants that faced stricter permit constraints and up to 120% higher among plants owned by single-plant firms. Additionally, I decompose the free permit allocation changes into a level component, capturing initial permit overallocation, and a relative efficiency component, capturing the plant's distance from the newly-introduced EU product benchmarks. I show that the latter is what explains both sample exits and full shutdowns. In line with the initial goal of the new allocation policy, aiming to target dirtier producers, plants that exited the ETS sample and/or stopped production entirely were those whose emission intensity was further away from that of the EU's cleanest emitters within the same product group. Finally, I provide descriptive evidence that sectors on average more exposed to activity-level permit cuts are later associated with higher emission market concentration in 2020. Overall, these findings have important policy implications, since they suggest that the EU ETS not only reduced emissions through plant-level operational adjustments from dirtiest emitters, but also reshaped emission and industry composition of market incumbents.

This paper contributes to three current policy analysis challenges. First, it introduces a novel approach to assessing within-policy variation in the EU ETS by developing plant-level measures of policy stringency and exposure, relative to other plants within the same activity group. Second, it provides empirical evidence that a significant share of emission reductions stems from plant exits rather than uniform abatement across firms[2]. To my knowledge, it is the first to empirically model plant exits from the ETS scheme altogether, and most specifically distinguish among different types of exits. Third, it applies a survival analysis framework to examine how regulatory stringency influences exit probabilities, highlighting plant-ownership heterogeneity and the role of carbon pricing in possibly reshaping industry composition through endogenous plant exit of the dirtiest and smallest emission producers.

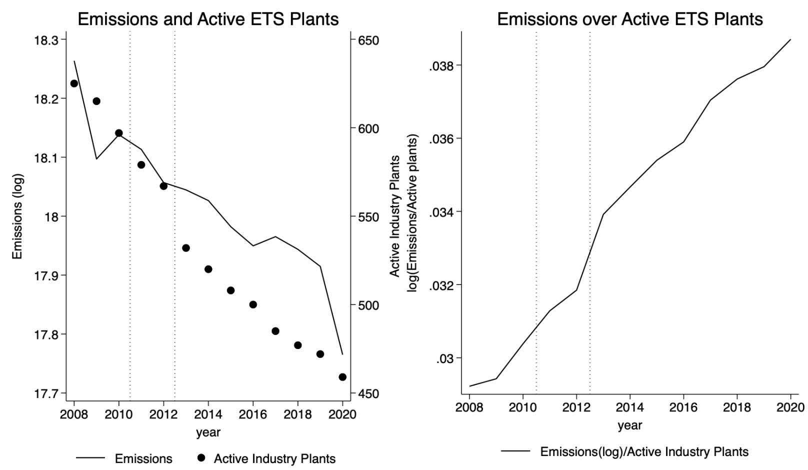
To illustrate the presence of these compositional effects, Figure 1 provides preliminary evidence on the relationship between emissions trends and number of active plants under the EU ETS. The figure shows the evolution of ETS-covered plant-level emissions and the number of active French industrial plants from 2008 to 2020, based on the sample described in Section 3. The number of active plants and their total verified emissions both decline,

[1]These results are the first to empirically confirm the novel theoretical model of Alder et al. (2025), connecting free carbon permit allocations to within-firm adaptation.

[2]The wide use of balanced firm-level samples in most current literature does not allow to analyse the impact of the ETS policy in this regard.

with a notable drop around 2013. Although both values decline over time, when computing the share of emissions over number of active plants, the ratio between the two appear to consistently increase, suggesting that surviving plants might on average emit more. This trend, later confirmed in the analysis, may indicate that smaller or less efficient plants exited the market, while larger or more efficient plants remained active[3].

Figure 1: Emissions and active French plants covered by the EU ETS



Notes: Active plants include all French manufacturing plants with positive values of verified emissions in 2008 subject to the EU ETS, i.e. incumbents (i.e. new entrants since 2009 are excluded). The two vertical lines correspond to the years of new allocation rule announcement for EU ETS Phase III (i.e. April 2011) and to its introduction (i.e April 2013). Data is taken from the European Union Transaction Log (EUTL).

A large body of literature provides evidence that carbon cap-and-trade mechanisms, and specifically the European Union Emissions Trading System (EU ETS), have successfully reduced overall emissions (e.g. Martin et al. (2014), Martin et al. (2016), Marin et al. (2018), Colmer et al. (2023), Dechezleprêtre et al. (2023)). However, much of this evidence is concentrated on the early phases of the policy (2005–2012), when the system was still in its infancy[4]. This temporal limitation is particularly important because, in these early years, the EU ETS was characterized by generous overallocation of free carbon permits to plants, which possibly diluted the effectiveness of the cap-and-trade mechanism. In this respect, my analysis challenges this perspective by arguing that the EU ETS began to have a more substantial impact in later phases only, particularly after 2013, when free allocation rules became progressively stricter, overall cap was reduced, and plants were forced to adjust more significantly.

In addition to this temporal limitation, the existing literature has largely relied on a binary comparison of ETS-covered and non-ETS plants, which introduces significant method-

[3]A similar finding is drawn under the entire EU-sample of plants ***. FIX THEM ADDING 2005-2007!

[4]See the literature review provided by Joltreau and Sommerfeld (2016)

ological challenges. Most studies match regulated firms with unregulated ones, to estimate treatment effects of being subject to ETS policy (see the literature reviews by Green (2021) and Joltreau and Sommerfeld (2016)), but this approach may understate the true impact of the policy according to the recent analysis by Barrows et al. (2023). Since ETS-covered plants face higher compliance costs, they likely increase prices to compensate for the additional environmental burden[5]. Crucially, non-ETS plants operating in the same output markets also have an interest in raising their prices in response to sector-wide cost pressures. In the context of existing DiD analyses on the EU ETS, this implies that the policy may have been even more impactful than initially thought, potentially driving stronger emission or selection effects (including plant exit) than previously captured[6]. To partly address this limitation, my sample includes only ETS-regulated plants. Rather than comparing them to non-ETS plants, I exploit variation in policy stringency and policy exposure among regulated plants only. This approach partly mitigates concerns raised by Barrows et al. (2023), since all plants in the sample are subject to the same climate regulation, but differ in terms of relative policy compliance costs due to different intensities of the permit allocation shock.

Finally, by focusing on the intensive margin of regulated and unregulated plants, much of the empirical literature mostly studies firm operational adjustments, while underexploring plant-level or market-level compositional effects[7]. If non-ETS plants are indirectly affected and ETS plants pass on costs through pricing, then market structure in certain sectors might itself change, potentially leading to increased plant exits and stronger composition effects than previously estimated. This methodological gap means that previous work may underestimate the EU ETS's role in shaping market dynamics, firm behavior, and composition of aggregate emissions reductions. By including an analysis on plant survival that relies on policy stringency, I provide a more accurate assessment of how carbon pricing policy affects overall emission reductions also through emission reallocation within survivors.

Related Literature. On top of the above-outlined literature gaps, the present paper relates to three main strands of literature. First, it connects to the literature on the Coase theorem in cap-and-trade systems (Coase (1960)) and its independence property, which broadly states

[5]Empirical evidence for this is, among others, provided by Fabra and Reguant (2014) for the power sector.

[6]In this regard, and consistent with the first concern, empirical studies that combine pre-post analysis with plant-matching are likely to produce lower-bound estimates. This is because not only they are subject to the critique outlined above, but they also analyse the effects of the EU ETS during its least stringent phase (Phase I).

[7]Many studies have focused on firm operational adjustments such as changes in output, investment, R&D, or carbon leakage to non-regulated plants (Martin et al. (2014), Calel (2020), Hintermann et al. (2020), De Jonghe et al. (2020), Dechezleprêtre et al. (2023)). Verde et al. (2019) and Guerriero and Pacelli (2023) are the only analyses that explicitly study plant entry and exit incentives under the ETS.

that if carbon markets are perfectly competitive and transaction costs are negligible, the initial allocation of carbon permits should not affect the *overall* level of emissions[8]. Several studies support this hypothesis, demonstrating that carbon emissions and permit allocations are largely independent due to the flexibility of trading mechanisms (Reguant and Ellerman (2008), Fowlie and Perloff (2013), Colmer et al. (2023))[9]. Although unable to test the hypothesis[10], this study questions the application of the Coase theorem at a more micro level, by examining how permit allocation interacts with plant-level constraints and policy exposure. In this regard, the present study closely relates to the recent work by Alder et al. (2025), which shows how the withdrawal of free allowances in the EU ETS reduced firms' emissions. They rationalize these effects through a multi-product model in which fewer free allowances raise fixed costs and force the least productive product lines within each firm to exit, implying that firms adjust mainly along the extensive margin. However, unlike the present paper, they are unable to directly test plant-level exit probabilities, since their empirical design relies on a balanced panel of firms that captures only the intensive margin of surviving firms.

Second, this paper builds on the literature on climate regulation and industry dynamics, which explores how environmental policies influence market composition, exits and acquisitions. Previous research (e.g. Fowlie et al. (2016), Barrows and Ollivier (2018), Verde et al. (2019), De Jonghe et al. (2020), Jo and Karydas (2023)) has established that stringent climate policies can accelerate firm restructuring and output, particularly affecting less competitive or more emissions-intensive firms. The present study extends this literature by focusing on the compositional effects of the EU ETS, examining not only whether and how plants adapt but also how the regulatory environment influences exit probabilities.

Finally, the paper connects to the literature on carbon trading and corporate finance, which examines plants' responses to carbon permit allocation and trading incentives (Martin et al. (2011), Venmans (2016), Bustamante and Zucchi (2022)). In this regard, my study provides evidence that policy announcement is enough to trigger within-plant adaptation to environmental policy changes, and that plants are differently exposed to permit allocation policy (and, possibly, differently financially-constrained) based on their previous positioning

[8]In this regard, what matters is the total cap, not how permits are distributed, since firms can trade freely to achieve the most cost-effective allocation.

[9]Studies challenging the Coase hypothesis are mostly focused on evidence in behavioral and managerial economics (Martin et al. (2011), Venmans (2016)) or on corporate finance models (e.g. Bustamante and Zucchi (2022)).

[10]A formal test would require allocation to be completely random, while I show that in the case of the 2013 permit allocation change this allocation was predetermined on past plant-level emission and output patterns.

in terms of banking of carbon permits. Additionally, plant ownership type is a key explanatory variable in climate policy adaptation, with multi-plant firms able to reallocate emissions between sister plants covered by the ETS but producing under a cleaner emission intensity. My results are in line with the recent work from Stillger (2025) which provides evidence that, under a theoretical model allowing for firm heterogeneity, higher carbon pricing regulation leads to a reallocation of production towards firms with a lower emission intensity.

The paper is structured as follows. Section 2 provides a context on the EU ETS and on its permit allocation policies across phases II and III. Section 3 outlines an overview of the main data sources and of sample construction. Section 4 presents treatment assignment, as well as the main methodologies used to analyze plant emissions and plant exit. Section 5 provides results on plant emissions and plant exits and comments on them. Section 6 outlines an analysis of how the policy mechanism, underlying the newly introduced allocation rule, influenced emission and sector compositions. Finally, Section 7 concludes the analysis.

2. The EU ETS and its Permit Allocation Policy

2.1. The EU Emissions Trading System (EU ETS)

The EU Emissions Trading System (EU ETS) is the largest and most established cap-and-trade program in the world, designed to regulate greenhouse gas emissions (GHGs) from high-emitting sectors such as power generation sector, industrial sector and other highly carbon intensive sectors, e.g. waste management[11]. Participation in the EU ETS is mandatory for combustion installations with a rated thermal input of 20 megawatts (MW) or more and that generate heat, steam or power on site. It is characterized by free allocation of non-expiring tradable carbon permits given to polluting plants, and by a secondary market for transactions of carbon permits (or EU Allowances, EUAs), where polluting plants can buy and sell unused permits. Established in 2005, the EU ETS covers approximately 12,000 plants across the EU-27, Iceland, Liechtenstein, and Norway.

The EU ETS functions by setting an overall emission cap that is gradually reduced over time. Plants are required to surrender one EUA for each ton of CO_2 they emit. Those that reduce emissions below their allocation can sell excess permits, while plants exceeding their

[11] The aviation sector was integrated into the EU ETS in 2012, though it is excluded from the scope of this study.

cap must purchase additional permits or invest in carbon abatement technologies[12]. This flexibility ensures that emissions reductions occur where it is more cost-effective to do so, aligning with economic principles of market-based regulation (e.g. Coase (1960), Baumol and Oates (1971)). Plants can obtain permits through three primary channels: (i) free allocation from the regulator (i.e. FA); (ii) auctioning of permits in the primary auction market, where permits are sold by regulatory authorities; and (iii) trading in the secondary market[13]. Once acquired, plants can use permits in three ways: (i) surrendering them at the end of the compliance year to match their verified emissions; (ii) selling excess permits in the secondary market; and (iii) banking the excess permits for future use in subsequent years or phases. Banking is permitted across compliance years and trading phases[14], though borrowing from future periods is prohibited. Thus within my timespan of analysis (2005-2020), permits issued in Phase II and III were non-expiring and could be banked across years only after 2007.

The EU ETS has evolved through distinct trading phases, each introducing refinements to the allocation mechanism and the scope of regulation. Phase I (2005-2007) served as a pilot phase with generous permit allocation and volatile carbon prices. Phase II (2008-2012), instead, introduced stricter caps and limited auctioning but retained the reliance on National Allocation Plans (NAPs) for free permit allocation within Member States. Phase III (2013-2020) marked a fundamental shift, eliminating NAPs, introducing benchmark-based allocation for non-power generating plants, and implementing full auctioning for power plants.

2.2. Permit Allocation Policies

2.2.1. Phase II (2008-2012)

During Phase II of the EU ETS (2008-2012), permit allocation was governed by National Allocation Plans (NAPs). Under NAPs, each Member State was requested to set its own

[12]All plants within established sectors, within established thresholds and within EU Members States are obliged to comply to it, unless they are willing to pay heavy fines on each additional ton of carbon emitted. The annual compliance period in the EU ETS follows a structured cycle to ensure regulated plants surrender enough EUAs to cover their verified emissions. Around February each year, plants receive free permits and must track their emissions throughout the year. By March 31 of the following year, plants are required to report their verified emissions from the previous year. By April 30, they must surrender a number of EUAs equal to their total emissions. Monitoring is conducted to ensure that surrendered permits (i.e., the emissions a plant reports having produced) match verified emissions (i.e., the emissions the regulator confirms as belonging to the plant). Recall that one EUA corresponds to one ton of CO_2.

[13]The secondary market operates through regulated exchanges, such as the European Energy Exchange (EEX), and over-the-counter (OTC) transactions, where buyers and sellers negotiate directly.

[14]The only exception on banking across trading phases was between the Phase I and Phase II, since permits emitted during Phase I were withdrawn by the market in 2007.

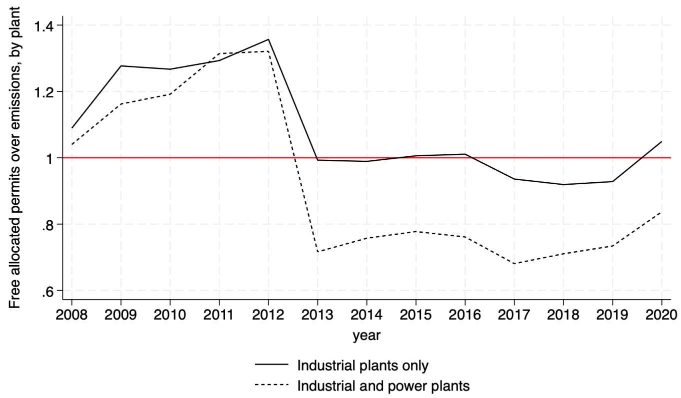
plant-level allocation rules. In the case of France, allocations were based on a combination of 2005 plant-level emissions data and projected sector growth for the 2008-2012 period[15]. The allocation policy during this phase raised several concerns. First, as documented in Rogge et al. (2006), allowing each EU Member State to determine carbon allocation rules for its own plants led to excessive permit distributions and raised concerns about potential political influence from industrial groups. Second, evidence of windfall profits in highly concentrated sectors emerged, with plants able to more than pass through the implicit cost of permits to consumers while continuing to receive free allocations (as later documented by Fabra and Reguant (2014) for the power sector and by Marin et al. (2018) for the manufacturing sector). Third, the 2008 financial crisis exacerbated permit overallocation. As industrial production declined, emissions in some sectors fell below sector-level projected growth rates included in NAPs, and thus resulted in an increased surplus of unused permits. Evidence of overallocation of permits to polluting plants is presented in Figure 2, based on the sample of French industrial plants described in Section3. Permit overallocation is measured at the plant level as the ratio of yearly free allocated permits over yearly verified emissions for each plant in the sample, and then collapsed into yearly averages. A value above 1 means that plants on average received more free permits than needed to cover their verified emissions, while a value below 1 means plants received fewer permits than their produced emissions, requiring them to purchase additional permits, use their banked permits, or reduce emissions to comply. The figure illustrates that industrial and power plants in Phase II (2008-2012) received more free permits than they on average required, with the average ratio of allocated permits to verified emissions well above 1 and possibly leading to a surplus of banked permits. As of 2013, the ratio broadly sets around 1, before dropping again in 2017, likely due to the announcement of the Market Stability Reserve (MSR) mechanism not analyzed in this paper. The reason for the steeper decline in the industrial and power plant sample after 2013, compared to industrial plant sample only, is linked to different allocation policy rules introduced as of 2013 for the two sectors, which are presented in the following section.

2.2.2. Phase III (2013-2020)

Partly due to concerns over market distortions under excessive overallocation, windfall profits, and pressure from domestic industrial groups on national regulators, the European Commission amended its permit allocation rules for Phase III (2013-2020). Permit allocation rules for Phase III were broadly based on two main European Commission Directives: Directive EC (2009) of 23 April 2009, and DirectiveEC (2011) of 27 April 2011.

[15] Allocation rules based on historical emission levels are referred to by Fowlie et al. (2016) as "pure" grandfathering rules.

Figure 2: Overallocation, by plant



Notes: Overallocation is computed at the yearly plant level as plant free allocated permits over yearly plant emissions. Calculation is based on the sample described in Section 3 and does not include permits auctioned from the primary market, nor carbon offsets. The decrease starting in 2017 is likely due to the announcement of the Market Stability Reserve (MSR) mechanism. For reasons explained in the following section, when including power generators the ratio of overallocation as of 2013 decreases below 1.

The 2009 Directive introduced distinct allocation rules for the power and non-power plants, with significantly different levels of clarity regarding their future obligations[16]. Article 10 of the directive explicitly mandates that from 2013 onwards, power generators must participate in full auctioning of emissions permits, thus explicitly eliminating free allocation for these plants[17]. In contrast, the situation for industrial plants (i.e., all other non-power sector installations) was much less clear. While Article 10a mentions that permit allocation will be set at the EU level, it did not define specific allocation rules. Instead, the directive set broad objectives, such as the intention to use EU-wide, ex-ante benchmarks based on the average performance of the 10% most efficient installations in each sector during 2007-2008 (so-called, product benchmarking), but did not specify how these benchmarks would be applied across different industries, nor did it present any rule to build these benchmarks[18].

Such clarification was only provided with the 2011 Directive EC (2011). Under the Phase

[16]The Directive 2003/87/EC (EP (2003)) defines an electricity or power generator as "*an installation that, on or after 1 January 2005, has engaged in an activity of fuel combustion for sale to third parties*". Therefore, in my analysis I broadly define as power generating plants those plants whose parent company is operating in NACE Rev.2 2-digits sector equal to "D35 - Electricity, gas, steam and air conditioning supply" and whose plant activity class equals "1 - Combustion installations with a rated thermal input exceeding 20 MW" or "20 - Combustion of fuels" (see Abrell (2021) Table C.1)

[17]The rationale behind this decision is at least partly due to the understanding that power companies can pass on the cost of emissions permits to consumers through electricity prices Fabra and Reguant (2014). Article 10a further clarifies that electricity generators would not receive any free allocation except in specific cases, such as district heating or high-efficiency cogeneration. This unambiguous policy direction meant that power plants had somehow full certainty regarding their future compliance obligations, allowing them to start adapting as early as 2009.

[18]Additionally, Article 10a(5) introduces a cross-sectoral correction factor (CF), suggesting potential adjustments to free allocation, but without clear details on how it would impact individual sectors.

III EU ETS reform, free permit allocation for industrial plants was expected to be based on a formula that tied allocation to both product best practices at the EU level, and plant-level historical output. Each plant's allocation was computed as the product of: (i) a product-specific benchmark based on the average emissions of the 10% most efficient EU producers in 2007–2008; (ii) pre-2010 plant's highest median product output; (iii) two reduction factors applied at the sectoral level and on the overall cap. For multi-product plants, the free allocations with respect to each product were then summed to build the overall plant-level allocation[19].

The structure of the policy implies two considerations. First, the construction of the allocation rule impedes a straightforward isolation of its product, plant, and benchmark components[20]. Indeed, the true exogenous treatment variability is given by the introduction of EU-wide best practice benchmarks, which are however defined only at a product level. Hence, even when isolating this exogenous element for the observed free allocations as of 2013, the remaining analysis would loose its plant-level granularity. On the other hand, when isolating its plant-level component of treatment variability, i.e. production levels, one would be focusing only on a mere policy predetermined variability. For this reason, in sections 4 and 6 I develop more sophisticated treatment assignments, which use plant activity information to proxy for unobservable product-benchmarks each plant was subject to. Second, given the complexity of the rule and the postponement of its finalization up until 2011 only, industrial plants, unlike power plants, remained uncertain until 2011 about the final allocation rules. Since power plants received a much stronger shock in free permit allocations, but still not attributable to product benchmarks, my study focuses only on industrial plants[21].

3. Data Sources and Data Overview

3.1. Data Sources

The present study combines two main sources of compiled data. First, plant-level emission data from the EUTL (or *European Union Transaction Log*). The EUTL is the official registry of the EU ETS and it provides a list of all regulated installations. This database serves as the core source for emissions, permit allocation, and compliance behavior. In addition, supplementary firm-level and plant-level data, used primarily to access control vari-

[19]More information on benchmarking as outlined in the 2011 Directive can be found in Appendix A.

[20]A full list of the 54 product benchmarks is presented in Annex I of EC (2011).

[21]As outlined in the next section, since non-power, non-industrial plants are a relatively small percentage of my sample (7%) and are subject to the same treatment as the industrial sector, I include it as part of this latter sector.

ables, are gathered by the French statistical institute INSEE[22]. Specifically, INSEE offers two main databases used in this analysis: FICUS-FARE[23] and EACEI[24]. The fiscal census FARE-FICUS offers annual income statements and balance sheets of the universe of French firms in manufacturing, mining, utilities and service sector. EACEI is a plant-level survey on energy intensity in the manufacturing sector[25].

Data on ETS covered plants are obtained from the EUTL, as processed by Abrell (2021). Plants in this database are recorded in terms of compliance information (i.e. number of yearly free allocated permits, surrendered permits, verified emissions, and daily transactions of permits in the primary and secondary market). Plants are registered in terms of city, postal code, geographical coordinates and activity identifier. An activity is the specific industrial production process carried out at a plant that makes it fall under the ETS coverage thresholds (e.g. producing cement clinker, refining oil, manufacturing glass), and it represents the legal basis for ETS coverage of plants (see Annex I of EC (2011)). Plants are also are mapped into broader sectoral categories with respect to the final product they produce [26]. Additionally, each ETS installation is linked to an account holder, typically a legal entity, whose company registration number corresponds to the French firm-level identifier (i.e. SIREN[27]). This allows for a link between ETS plants and their owning firms, enabling integration of EUTL with INSEE data.

Accordingly, plants can be mapped with their respective firm owners registered in the FARE-FICUS (and EACEI) database. FARE-FICUS provides general information about the firm (SIREN identifier, industry classification, head office address, total number of workers employed, age, etc.), the income statement (containing variables such as total turnover, total labour costs, and value added) as well as balance sheet information (e.g. various measures of capital, debt, and assets). While starting from a list of plant-level identifiers, i.e. SIRET codes (or *Système d'Identification du Répertoire des Etablissements*), one could trace back

[22]Or *Institut National de la Statistique et des Études Économiques.*

[23]Or the unification of *Fichier Complet Unifié de SUSE* until 2007 and *Fichier Approché des Résultats d'ESANE* since 2008.

[24]Or *Enquête annuelle sur les consommations d'énergie dans l'industrie.*

[25]EACEI survey is directed to all French manufacturing establishments, and the response rate is close to 90 percent. As presented in Colmer et al. (2023), it is important to note that not all establishments are covered, and that sampling rules have changed over time. For instance, as of 2013 only plants with employment above 20 employees have been surveyed. In my sample, however, just a handful of plant has recorded less than 20 employees in years before 2013, so this survey change should not drastically affect the representativeness of energy data in my sample.

[26]NACE-4 digits are imputed by Abrell (2024) based on the leakage assessment of the European Commission.

[27]Or *Système d'Identification du Répertoire des Entreprises.*

the firm level databases[28], the opposite is not true if one possesses the SIREN code only (as in my case). Hence, to identify the number of plants owned by a firm in a specific year, I then briefly take advantage of the annual employment database at the plant level (or DADS, *Déclarations Annuelles des Données Sociales).* From this database, I identify how many SIRET plant codes are connected to the same SIREN firm codes each year, and I consider this as the number of plants owned by a single firm in a specific year. From this same database, I also keep the geographical location of plants owned by a firm. I am then able to match the ETS plants with the EACEI survey (covering industrial plants only), based on the SIREN code and geographical location of plants in the survey[29]. From the EACEI survey, I observe employment per plant, as well as quantities and values of energy consumed by fuel type (i.e. electricity, steam, fossil fuels, and biofuels). As in Jo and Karydas (2023), I aggregate the consumption of different sources of energy to a clean and a dirty bundle for each plant, with the clean bundle including electricity, steam and renewables and the dirty bundle consisting of all other fuels (natural gas, petroleum products, etc.)[30]. I then compute annual plant-level (dirty) energy intensity variables and emission intensity variables as the amount of (dirty) energy consumption or emissions over annual employment in the establishment.

Finally, a plant exits the EUTL sample if it records a year of zero verified emissions in the ETS registry, and in the subsequent year emissions do not ramp up again. Although Verde et al. (2019) is the only study that analyses plant exits and entry dynamics in the EU ETS, it reports that exit rates are higher at the beginning of each regulatory phase. In this sense, exits in 2013 are particularly tricky, since they either might absorb most treatment effect (i.e. a plant that anticipates it will receive a huge negative shock in free allocated permits as of 2013 might decide to exit as soon as possible) or might mask exit reasons exogenous to the policy assignment. For this reason, I perform two manual checks. First, I reconcile EUTL data on free permits allocated in 2013 with a public French-level allocation list for 2013-2020 (Journal Officiel de la République Française (2014)), released to the public a few months after the beginning of Phase III. I find marginal differences in some plants that

[28]Indeed, the plant SIRET identifier is a 14 digit number whose first 9 digits correspond to the SIREN firm identifier.

[29]To avoid mismatching of firms owning more than one plant within the same postal code, I match ETS plants to energy surveyed plants only when the SIREN-postal code pair in the DADS database is one only. In other words, I avoid matching through SIREN firms that own multiple SIRET plants within the same postal code. I was still able to manually match most of the missing SIRET codes by manually compiling them based on the public data in the *Annuaire des Enterprises* République Française (2025).

[30]In this respect, France represents a relatively straightforward case study when it comes to classifying electricity production. Indeed, as of 2023 at least 65% of French electricity was produced by nuclear power Eurostat (2025).

had been assigned non-zero free allocation in 2013, but are recorded as receiving zero free permits in the EUTL registry. I therefore classify treatment using expected free allocation in 2013, to avoid post-treatment bias and better capture intention-to-treat. Second, I manually check press articles, company websites and inspection data present in the *Géorisques* portal (République Française (2025))[31] to confirm plant exits. Based on these data, I am able to classify the 77 sample of exits, corresponding to 15% of Phase II active plants, according to exit reason. Specifically, in the survival analysis I distinguish between all sample exits and operational exits, i.e. full production shutdown at the plant level, as outlined in Appendix B[32].

3.2. Main Sample

The main sample is constructed as follows. The EUTL sample of French plants active in 2005 outside of the aviation sector is composed of 1,542 plants. Treatment assignment is not defined for plants exiting before 2013, and the event study analysis requires pre-policy data from years before 2011. Hence, I exclude from the sample plants entering after 2005, and exiting before 2013. Additionally, as explained in the previous section, plants in the electricity generation sector are excluded[33]. In light of this sample, the current setting is agnostic to the presence of entrants and their possible composition effect in greening the overall industry, but instead focuses on the behavioral dynamics of incumbent plants only[34]. The final EUTL sample is then composed of 517 non-power generating plants, i.e. industrial plants owned by 373 unique firms, and followed yearly from 2005 to 2020. Out of the EUTL sample, using the SIREN code of the firm I match 506 plants (i.e. 358 firms) to FARE-FICUS data. I further match 428 plants (i.e. 398 firms) with the EACEI plant-level energy survey data, although for not all of those plants energy data is available on all years.

A summary statistics of the available variables for the full sample is presented in Table 1[35]. Emission levels verified by the regulator are overall lower than the value of allocated

[31] *Géorisques* is French official government online database that centralizes environmental-risk and industrial-site information of any industrial or agricultural operation that is likely to create risks or pollution for local residents.

[32] A table of confirmation of exits based on recorded plant-level variables is presented in Table C.5. A table presenting exit rates and emissions by 2005 active plants is presented in Table C.1

[33] Specifically, out of 1,542 plants, 560 are in the power generating sector and are thus excluded from this analysis. Additionally, 65 plants are registered in the EUTL but have never registered positive values of carbon emissions within the analysis time span. Of the remaining 925 plants, 355 entered the scheme after 2006 and 45 exited the ETS between 2005 and 2012.

[34] Descriptively, entrants have lower average emissions than incumbents, consistent with the idea that new plants adopt more efficient technologies and face stricter standards at entry, while incumbents operate with older, more emission-intensive capital. For an analysis of entrants in Phase II and Phase III, see Verde et al. (2019) and Guerriero and Pacelli (2023), respectively.

[35] A map of plant distributions over French districts is presented in Figure Appendix C.1

free permits, confirming that in the overall sample the free allocation policy was not on average strictly binding for most plants. As expected, permit banking is on average higher than verified emissions, supporting the idea that overallocation of permits is a concern in the timespan of analysis[36]. Turning to firm-level data, ETS plants are connected to firms that own on average more than 10 other plants, and that are considered quite sizeable in terms of employment and fixed assets.

Table 1: Summary of data sources and variables

	Main Sample			
	Mean	SD	Min	Max
EUTL: Plant-level data				
Verified emissions	132.84	583.83	0.00	12059.46
Free permits	137.76	594.18	0.00	11863.92
Banked permits	146.90	530.33	0.00	16095.53
Nr. plants	517			
Observations	8272			
FARE-FICUS: Firm-level data				
Output sold	768.16	2512.08	0.00	56290.26
Fixed assets	884.78	1867.15	0.00	18609.54
Nr. plants	10.60	19.34	1.00	225.00
Employment	1577.88	3379.09	0.00	44431.00
Nr. plants	506			
Observations	7739			
EACEI: Plant-level energy survey				
Employment	460.41	1181.97	2.00	15065.00
Clean energy consumption	117.41	243.48	0.00	2636.13
Dirty energy consumption	177.52	428.42	0.00	5902.00
Total energy consumption	294.93	546.02	0.00	6464.67
Dirty energy intensity	855.35	2296.32	0.00	47730.89
Energy intensity	1277.31	2441.57	0.00	56422.87
Emission intensity	875.52	3289.05	0.00	126850.50
Nr. plants	428			
Observations	6871			

Notes: The sample used here includes all plants and all years (pre and post treatment). Allocated permits, verified emissions, and banking of permits are expressed in thousands of EUAs. Fixed assets and output sold are expressed in thousands of Euros. Energy consumption variables are expressed in thousands of units, where "clean" is composed of the sum of electricity and steam, while "dirty" is composed of coal, oil and natural gas. Energy intensity is measured as energy consumption over plant employment. Dirty energy intensity and emission intensity are built similarly.

[36] Recall that permits across Phases II and III of the EU ETS are non-expiring and can be banked both across compliance years and across compliance phases. However, borrowing from subsequent periods is not allowed. The $bank_{i,t}$ variable, reported in the table as banked permits, is then a cumulative flow variable defined at plant i and year t which takes into account: (1) the non-negative amount of $FA_{i,t}$ to a plant; (2) the non-negative amount of banked permits at $t-1$; (3) the positive or negative net trading of permits performed by the plant at year t (i.e. permit auctioning from the primary market, and permit purchases or sales in the secondary market) (4) the non-negative amount of permits the plant has to surrender at the end of the compliance year in line with how much carbon it emitted.

4. Methodology and Treatment Assignment

4.1. Treatment Assignment

The main treatment variable, $highdrop_i$, identifies plants that experienced a greater-than-median reduction in freely allocated permits within their activity. Indeed, in absence of information on the actual product benchmarks a plant was subject to as of 2013, the closest proxy for it is the activity level of the plant[37]. The variable captures differences in policy stringency across plants and is defined as follows [38]:

$$
\begin{aligned}
highdrop_i &= \begin{cases} 1 & \text{if } drop_{i,2013} \geq drop_{a,median,2013} \\ 0 & \text{otherwise} \end{cases} \\
\text{where} \quad drop_{i,2013} &= -\frac{FA_{i,2013} - FA_{i,2012}}{FA_{i,2012}}
\end{aligned}
\tag{1}
$$

Here, $FA_{i,t}$ represents the quantity of free allocated permits to plant i in year t[39]. A plant is classified as $highdrop_i$ if its reduction in free permits between 2012 and 2013 was at least as severe as the median reduction observed within its activity peers. This variable thus reflects the relative policy stringency imposed on a plant, compared to other plants in the same activity. Since plant-level free allocated permits are still compared to their previous level when building the $drop_{i,2013}$ variable, and plant-level drop is later compared to other drops within the activity, this approach effectively takes into account both the plant-level component and the activity-level benchmarks outlined in Section 2. Plants that experience a high $drop_{i,2013}$ relative to their $FA_{i,2012}$ (or equivalently, to any year between 2008 and 2012, see Appendix A) can be interpreted as being more adversely affected by the shift to benchmarking in Phase III. Accordingly, plants assigned to the $highdrop_i$ treatment group experienced sharper reductions in free permit allocations relative to their activity peers[40]. While the drop in allocation may reflect pre-existing emissions intensity and output levels, the magnitude and timing of the change were driven by a policy reform that was exogenously

[37] An alternative but looser proxy for product benchmarks would be the plant NACE sector of the plant. An alternative treatment definition using a sector-based $highdrop_i$ variable is run as a robustness in Figure Appendix C.10. As a matter of fact, Figure Appendix C.2 reports the relationship between activities and sectors in the main sample: besides fuel generation (i.e. activity 20), all other activities map almost 1-to-1 to aggregated NACE sectoral codes.

[38] This treatment assignment resembles the one in De Jonghe et al. (2020).

[39] As explained in Section ??, the value of $FA_{i,t}$ are cross-checked with actual allocation recorded in Journal Officiel de la République Française (2014).

[40] A cumulative distribution of the $drop_{i,2013}$ variable and its distribution by activity are reported in Figure Appendix C.3 and Figure Appendix C.4. As visible from the distributions, an amount of 90 plants over 517 received an increase, and not a *drop* in permits as of 2013. The next section carefully presents results including them as well as excluding these plants.

imposed by the European Commission. Benchmark values and allocation rules were set at the EU level and applied retroactively, meaning plants could not influence their assignment post-announcement. Thus, while the allocation drop itself is mechanically related to past emissions and production, the variation in whether a plant would eventually fall above or below the activity median (i.e., treatment status) was not a choice by the firm, but the outcome of an externally determined policy formula and predetermined differences across plants in the same activity group. I therefore interpret $highdrop_i$ as a plausibly exogenous treatment indicator, conditional on plant-level controls and pre-policy parallel trends in the outcome variable. Overall, the treated group is composed of 262 industrial plants, while the control group is composed of 255 plants.

Two considerations help situate this treatment assignment within the existing literature. First, it is robust to the findings of Branger et al. (2015), who show that some firms, particularly in low-demand countries and sectors like cement, strategically increased output in 2012 to secure higher free allocations in 2013 through a specific clause in the EC 2011 Directive. Since my treatment variable is based on the relative drop in allocation between 2012 and 2013, such strategic behavior mechanically reduces observed permit cuts, making these "cheating" plants more likely to fall into the control group. As a result, some control plants may have actively avoided treatment status, biasing treatment effects downward. Second, Barrows et al. (2023) show that when firms compete in the same output market but face different regulatory intensity SUTVA is violated, as control firms are indirectly affected through output prices. While my analysis tries to limit these concerns by including only ETS-covered plants, i.e. plants that are still subject to the same regulatory framework, variation in allocation cuts within activity group may similarly induce spillovers. However, this again suggests that the estimated treatment effects likely understate the full impact of allocation stringency, and that treatment effects, as defined here, are lower bound estimates.

To explore heterogeneity in plants' ability to respond to the reform, plants are further categorized into two main heterogeneity variables. First, based on their banking behavior in the years prior to the policy shift, $highbank_i$, is used to analyze heterogeneous effects by distinguishing plants based on their pre-existing stock of banked permits prior policy announcement:

$$\begin{aligned} highbank_i &= \begin{cases} 1 & \text{if } bank_{i,08\text{–}10} \geq bank_{a,median,08\text{–}10} \\ 0 & \text{otherwise} \end{cases} \\ \text{where} \quad bank_{i,08\text{–}10} &= \frac{1}{3}\sum_{t=2008}^{2010} \frac{bank_{i,t}}{FA_{i,t}} \end{aligned} \tag{2}$$

The variable $bank_{i,08\text{–}10}$ represents the average stock of permits banked by plant i between 2008 and 2010. A plant is classified as a high-banking plant if its 2008–2010 average stock of banked permits is at least as large as the median within its activity, $bank_{a,08\text{–}10,\,median}$.[41] Compared to their activity competitors, these plants are considered better positioned to mitigate the compliance costs associated with the reduction in freely allocated permits in 2013, as they could draw on previously accumulated stocks. This variable thus captures a measure of relative policy exposure of a plant compared to other plants in the same activity.

Second, an additional channel through which plants may buffer the reduction in free allocation is ownership structure. A plant that belongs to a single-plant firm can only rely on its own allocation and banking decisions. By contrast, a plant that is part of a multi-plant firm may benefit from internal reallocation of permits across sister plants. In such firms, management can smooth compliance costs by transferring allowances from installations with a relative surplus to those facing a shortfall, effectively cross-subsidising compliance within the group. This provides multi-plant firms with an organizational buffer that is unavailable to stand-alone plants, even if both face the same decline in free allocation. I thus create a $multiplant_i$ dummy variable, coded using ownership identifiers (SIREN codes) across 2005-2020. Each plant is matched to its firm identifier prior to 2013, and coded with a $multiplant_i$ of 1 if its parent company owns multiple plants. To avoid misclassification due to changes in plant ownership over time, plants that are connected to different firm codes across years are excluded from this coding. This ensures that the measure solely captures the scope for internal reallocation of allowances at the time of the policy change, rather than reflecting later ownership restructuring[42].

Before turning to the main analysis, Figure 3 presents the mean emissions and total exits by treatment assignment $highdrop_i$, as of policy announcement and policy introduction. The above graph plots the mean of log emissions by year for treated plants (solid line) and control plants (dashed line). The trajectories show that before 2011 both groups followed broadly similar trends, while after 2011 already the treated group's emissions decline more steeply,

[41] Recall that permits issued in Phase II were withdrawn from the market at the beginning of 2008. Additionally, in the absence of accounting for free allocated permits in the construction of $highbank_i$ (an adjustment also used for the construction of the variable $highdrop_i$), the correlation between banked permits and verified emissions before 2010 is very high (0.93). This indicates that banking, without further weighting, would largely reflect installation size rather than a genuine buffer against policy changes.

[42] This results into the exclusion of 74 plants which change ownership between 2005 and 2020 when this heterogeneity analysis is performed. 210 plants coded as owned by single-plant firms, and 233 plants owned by multi-plant firms.

consistent with stronger exposure to the reduction in free allocation[43]. Most importantly, emission patterns start to differ already as of 2011, i.e. the year of announcement of the introduction of product benchmark as a new permit allocation rule. For this reason, the identification strategy below will mainly use 2010 as pre-policy baseline year.

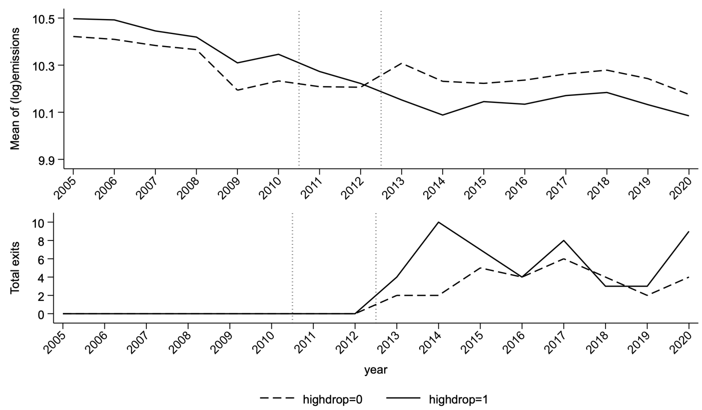
The bottom graph in 3 plots all sample exits as of policy announcement and policy introduction. In most years, the solid line lies above the dashed one, hence exits as of 2013 appear to mostly involve plants experiencing a high drop in allocated free permits compared to their activity median. Most importantly, the sample is restricted to plants that remain active until 2013, since only these plants receive free allocations that allow construction of the $highdrop_i$ treatment variable. Plants that exited before 2013 therefore have no treatment status assigned and cannot be included in the analysis. As a result, there are no pre-period exits observed across groups, which makes a standard difference-in-differences approach infeasible for the outcome of plant exit. Instead, as outlined in the next section, I rely on survival analysis, using $highdrop_i$, $highbank_i$, $multiplant$, and their interactions as treatment variables. Similar emission and exit figures by $highbank_i$ and $multiplant_i$ heterogeneity variables are presented in Figure Appendix C.5a and Appendix C.5b. Plants coded according to their $highbank_i$ and $multiplant_i$ classification display no strong changes in emission and exit pattern after policy announcement or introduction. A pre-treatment balance test for my main sample on pre-2011 observed variables is presented in Appendix Table C.3.

4.2. Identification Strategies

The present study makes use of two complementary identification strategies. First, I use a a difference-in-differences (DiD) model to study the causal impact that policy stringency and policy exposure had on plant-level emissions. Second, I use a survival analysis model to study the suggestive impact that policy stringency had on plant exit.

[43]Using emission levels instead of logs yields a different ranking of groups, with the control group exhibiting higher mean emissions before 2011. This discrepancy is explained by the presence of a small number of very large emitters in the control group, which drive up the arithmetic mean in levels but are down-weighted in the log specification. Logs therefore provide a more representative picture of the typical plant, and align with the specification used in the main analysis.

Figure 3: Emissions and exits, by highdrop



Notes: The upper panel plots mean log-emissions by treatment status, distinguishing plants with above-median allocation drops ($highdrop = 1$, solid line) from those with below-median drops ($highdrop = 0$, dashed line). The lower panel shows the total number of plant exits by year for each group. The two vertical dashed lines mark the year before the policy announcement (2011) and the year before implementation (2013).

4.2.1. Plant Emissions

To estimate the effects of carbon policy stringency on plant-level outcomes, a difference-in-differences (DiD) model is implemented using the following specification:

$$\log Y_{i,t} = \sum_{k=2005, k\neq 2010}^{2020} \beta_k highdrop_i \times \mathbb{1}[t = k] + \alpha_i + \lambda_t + \theta X_{i,t} + \epsilon_{i,t} \tag{3}$$

where $Y_{i,t}$ represents the main outcome of interest for plant i at time t (i.e. plant log-emissions). The term α_i captures plant fixed effects, absorbing time-invariant plant-specific characteristics. The coefficients λ_t include year fixed effects to account for common time trends. The vector $X_{i,t}$ represents additional plant-level control variables at baseline values (i.e. firm-level output sold and fixed assets, and plant-level employment, total energy consumption and dirty energy share) added as further robustness. Standard errors $\epsilon_{i,t}$ are clustered at the plant level to account for potential autocorrelation over time within plants. The coefficients β_k measure the estimated effect of being in the $highdrop_i$ treatment group relative to the baseline year of 2010 to account for possible policy anticipation. The interaction terms capture the relative effect of policy stringency on treated plants (i.e. experiencing an above-median drop in free allocated permits) versus untreated plants (i.e. experiencing a below-median drop in free allocated permits), with 2010 as the baseline reference year before policy announcement. Indeed, the validity of the difference-in-differences approach generally relies on two core assumptions of parallel trends and no anticipation effect (e.g. Angrist and Krueger (1991)). In the context of this study, these assumptions imply that, in the absence of the changes in the free permit allocation rule, the treated and control units would have followed similar trajectories, and that the exact new allocation for each plant, or more specifically their ranking with respect to other plants within a plant's activity, was unforeseen by producers. On one hand, although it is challenging to fully validate the parallel trends assumption, I employ a dynamic DiD approach to provide statistical evidence supporting its validity. On the other hand, the use of 2010 as baseline year, in light of the policy timeline explained in 2 and shown in Figure Appendix A.9, should suffice to account for anticipatory effects.

4.2.2. Plant Exit

To analyze the probability of plant exit over time, I estimate Cox proportional hazards models of the form:

$$h_i(t \mid a) = h_{0s}(t) \times \exp\Big(\beta_1 highdrop_i + \beta_2 multiplant_i + \beta_3(highdrop_i \times multiplant_i)\Big) \quad (4)$$

$$h_i(t \mid a) = h_{0s}(t) \times \exp\Big(\beta_1 highdrop_i + \beta_2 highbank_i + \beta_3(highdrop_i \times highbank_i)\Big) \quad (5)$$

where $h_i(t \mid a)$ denotes the hazard function for plant i at time t in activity a, conditional on survival up to t. The baseline hazard $h_{0a}(t)$ is left unspecified and varies freely across activities, thereby absorbing all time-invariant differences in exit risk at the activity level. The coefficients β capture relative risks associated with the treatment and heterogeneity variables, while the interaction terms allow for differential effects when both conditions are present. I estimate this model using a likelihood-based approach, specifically the Cox partial likelihood method,[44] and interpret the coefficients β_k in terms of hazard ratios.[45]

In order for the Cox proportional hazards model to provide valid inference, several assumptions must hold. First, the proportional hazards assumption requires that the hazard ratio between treated and control plants is constant over time. I assess the proportional hazards assumption by testing Schoenfeld residuals[46]. Second, the model assumes non-informative right-censoring, i.e. that plants still active at the end of the sample are not

[44] In applied work, Cox estimation is often described as maximum likelihood, but strictly speaking the coefficients are obtained via partial likelihood since the baseline hazard $h_0(t)$ is left unspecified.

[45] Hazard ratios are given by $\exp(\beta_k)$. A hazard ratio greater than 1 indicates an increased likelihood of plant exit compared to the baseline, while a hazard ratio less than 1 suggests a lower likelihood of plant exit.

[46] A global test for the main regressions used in 5.2 yields a p-value of 0.46, with no variable-specific violations, suggesting that the assumption is not rejected and hazard ratios can be interpreted as time-constant relative risks.

systematically different in unobserved exit risk, conditional on covariates[47]. Third, survival times are assumed independent across plants, such that one plant's exit does not directly alter another plant's hazard; clustering standard errors at the plant level relaxes this assumption by accounting for within-plant correlation. Finally, stratification by activity allows the baseline hazard $h_{0a}(t)$ to vary flexibly across activities, ensuring that unobserved time-invariant differences in exit risk at the activity level do not bias the estimated hazard ratios. Kaplan–Meier survival estimates for plants by treatment status are presented in Appendix C.11.

5. Main Results

5.1. Results on Plant Emissions

Table 2 reports estimates from static difference-in-differences (DiD) regressions, pooling the pre- and post-treatment periods into a single post-2011 indicator. Panel (a) presents results for the full sample of plants, while Panel (b) restricts the estimation to the balanced subsample of plants that remain active throughout the period of analysis. Columns (1) and (2) estimate the baseline model with only plant and year fixed effects, using either the full set of plants or the subsample for which baseline firm controls are available. Column (3) augments the specification by including firm-level baseline controls (production sold and fixed assets at 2010 levels). Column (4) switches to the restricted sample of plants with detailed plant-level data on energy use and employment, while column (5) further adds plant-level baseline controls (employment, total energy consumption and dirty energy share). Across all specifications and in both panels, the coefficient on the interaction term is negative and statistically significant at conventional levels. The magnitudes imply that plants subject to above-median drops in free allocation reduced emissions by roughly 10–17% more than plants in the control group after 2011. The inclusion of firm- and plant-level controls has little effect on the size or significance of the estimates, which remain robust across alternative samples and specifications.

To quantify dynamic effects of the policy and confirm the parallel trends assumption, Figure 4 presents event–study estimates corresponding to the baseline specification in column (1) of Table 2. Panel (a) uses log-emissions as the outcome and shows no significant pre-trends relative to the control group, supporting parallel trends. After 2011, and especially from 2013 onwards, the log-linear estimates indicate persistent reductions of at least 10%

[47]In this setting, censoring arises solely because the observation window for Phase III ends in 2020. Hence, plants that remain active at the end of the sample reflect the administrative end of the dataset rather than any systematic behavioral selection. I therefore treat censoring as non-informative and interpret the Cox estimates as valid within the 2005–2020 period, without making claims on post-2020 exit risks.

Table 2: DiD of highdrop on plant emissions, Full sample vs Active Plants

Panel (a): Full sample

	(1) log(emissions)	(2) log(emissions)	(3) log(emissions)	(4) log(emissions)	(5) log(emissions)
High drop × Post2011	-0.174***	-0.147***	-0.131**	-0.173***	-0.157**
	(0.050)	(0.051)	(0.051)	(0.066)	(0.063)
Plant FE	X	X	X	X	X
Year FE	X	X	X	X	X
Firm controls			X		X
Plant controls					X
Observations	7,923	7,202	7,202	4,315	4,323
R-sq	0.917	0.917	0.921	0.924	0.924

Panel (b): Active sample

	(1) log(emissions)	(2) log(emissions)	(3) log(emissions)	(4) log(emissions)	(5) log(emissions)
High drop × Post2011	-0.137***	-0.123**	-0.103**	-0.173***	-0.153**
	(0.049)	(0.051)	(0.050)	(0.065)	(0.061)
Plant FE	X	X	X	X	X
Year FE	X	X	X	X	X
Firm controls			X		
Plant controls					X
Observations	7,039	6,463	6,463	3,903	3,903
R-sq	0.924	0.923	0.926	0.926	0.930

Notes: Standard errors in parentheses, clustered at the plant level. All regressions include plant and year fixed effects. Column (1) uses the full sample of plants (Panel a) or the sample of plants that remain active after 2013 (Panel b). Column (2) restricts the sample to plants for which firm-level control variables are available. Column (3) adds firm-level controls at 2010 baseline levels(production sold and fixed assets). Column (4) switches to the plant-level sample for which detailed energy and employment data are available. Column (5) adds plant-level controls at 2010 baseline values: employment, total energy consumption, and dirty energy share.

compared to the baseline year. Restricting the sample to surviving plants only, the effect remains negative but slightly attenuated, suggesting that part of the overall reduction reflects composition effects from plant exits. Because the log specification drops zero outcomes, these estimates should be interpreted as lower-bound effects of allocation cuts: they mostly capture plants that remain active. Panel (b) instead uses a Poisson model that accommodates zeros and treats plant closures as genuine zero outcomes, as suggested in Chen and Roth (2023)[48]. This yields larger post-treatment effects (around $e^{-0.335} - 1 \approx -28.5\%$ for the full sample and $e^{-0.216} - 1 \approx -19.4\%$ for the active sample), reflecting both the intensive margin of surviving plants and the extensive margin of exits. The difference between the two (about –12 percentage points, or 36% of the total Poisson effect) can be attributed to plant closures[49].

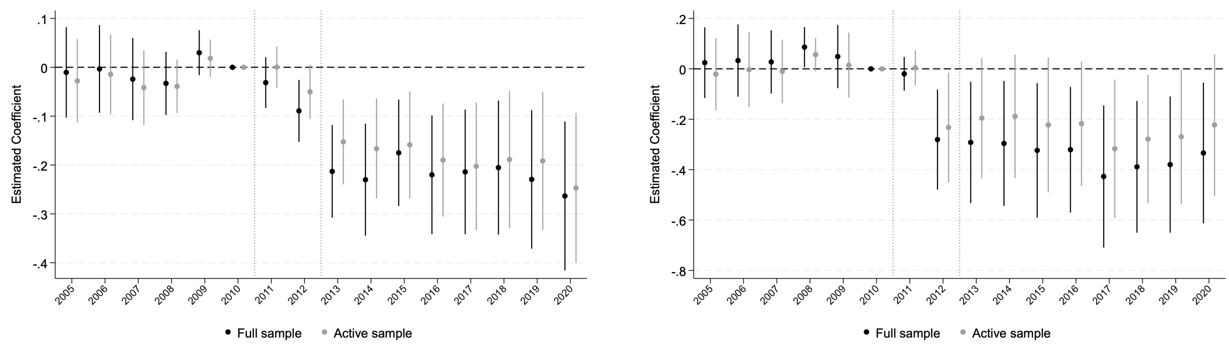
Overall, the two approaches provide a consistent picture: allocation cuts induced substantial reductions in plant-level emissions, with around one-third of the emission adjustment

[48]The Poisson PPML model accommodates zeros and models the conditional mean multiplicatively, so $e^{\beta} - 1$ can be read as the percent change in expected emissions, even when emissions reach zero.

[49]As discussed later, a robustness check is performed by excluding plants which received an *increase* in free permits as of 2013 to account for possible inflation of estimates under the full sample. Under this restricted sample and with treatment re-assigned accordingly, the Poisson full-sample effect suggests an around -27.2% reduction in emissions overall, and an around -17.2% reduction in the active sample (although not statistically significant). Even in this case, about –10 percentage points, or roughly 37% of the total Poisson effect, is attributable to plant exits. The estimates relative to this sample are presented in Figure Appendix C.9.

driven by exits. Since log-linear results are conservative and more easily interpretable, I rely on them for the main specification. At the same time, the Poisson estimates capture the total adjustment across both margins and highlight the importance of the extensive margin, a channel further examined in Section 4.2.2.

Figure 4: Dynamic DiD of highdrop on plant emissions, Full sample vs Active plants



(a) Log emissions (OLS)

(b) Level emissions (Poisson)

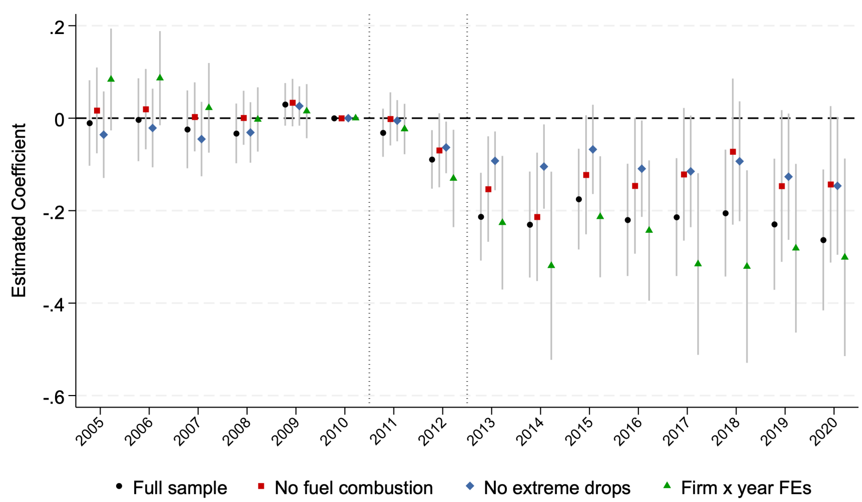
Notes: The two vertical dashed lines mark the year before the policy announcement (2011) and the year before implementation (2013). Panel (a) reports log-emission regressions (OLS with plant and year fixed effects), which imply an average post-2011 treatment effect of –0.176 (17.6%) for the full sample and –0.137 (13.7%) when restricting to surviving plants. Panel (b) reports Poisson regressions in levels, which yield stronger effects: –0.335 (28.5%) for the full sample and –0.216 (19.4%) for the active sample.

Robustness. I perform a series of robustness checks to assess whether the estimated effect of allocation cuts on plant emissions is sensitive to alternative sample restrictions or model specifications. Figure 5 reports the full sample estimates of treatment assignment on (i) a sample excluding plants in the combustion of fuels activity (ii) a sample excluding extreme values of the continuous treatment assignment variable $drop_{i,2013}$ and (iii) the main sample where firm-year fixed effects are added[50]. First, the exclusion of fuel-combustion plants is motivated by two reasons. On the one hand, it accounts for a very large share of the installations in the EU ETS; on the other hand, in Section 6 I explicitly set aside this activity to focus on activities where the permit allocation change more directly maps into abatement incentives. Excluding this activity therefore serves two purposes: it avoids the concern that one sector with many plants dominates the aggregate results, and it aligns the empirical analysis with the narrower set of activities for which a policy mechanism decomposition will be performed. Second, I remove extreme values of the treatment assignment (i.e. top and bottom 5% of the continuous allocation drop distribution) to verify whether outliers in treatment intensity drive the results. Because the $highdrop_i$ indicator is defined relative to the activity median, trimming the extremes tests whether plants at the tails of the alloca-

[50] Estimates for the sample of active plants only are presented in Appendix Appendix C.7.

tion distribution disproportionately shape the estimated treatment effect. Finally, while the baseline regressions already absorb plant fixed effects and year fixed effects, which control for time-invariant plant heterogeneity and aggregate shocks, I add firm-year fixed effects to absorbs all time-varying shocks common to plants belonging to the same parent firm in a given year (e.g. changes in firm-wide output demand, corporate investment strategies, financing constraints, or group-level permit management). This specification therefore isolates identification from within-firm, across-plant variation in allocation cuts. The estimates in Figure 5 remain essentially robust to all specifications for the full sample of firms, although not always significant for certain years under the specification excluding extreme values of $drop_{i,2013}$.

Figure 5: Event study DiD of $highdrop_i$ on plant-level emissions, robustness on the full sample

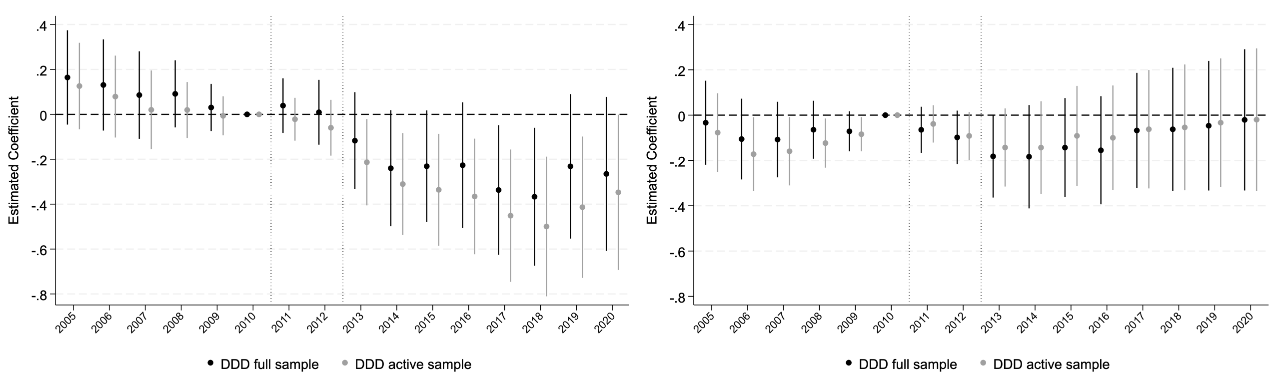


This latter finding further motivates additional robustness using the continuous $drop_{i,2013}$ directly as main treatment assignment. This exercise serves two purposes: (i) it tests whether the binary $highdrop_i$ indicator masks a more graded, dose–response effect of allocation tightening, and (ii) it assesses whether the main results are sensitive to the functional form of treatment definition. Appendix Table C.6 reveals a strong asymmetry: emission changes are essentially concentrated among plants facing cuts, while increases in allocation have a more modest effect[51]. The decile analysis in Figure Appendix C.6 further documents a clear dose–response pattern, with the largest reductions concentrated in the upper deciles of the distribution. These findings confirm that the binary treatment is a valid simplification, while the main results remain robust when using a continuous measure of treatment intensity.

[51] As anticipated before, I still perform a robustness analysis of treatment assignment excluding plants experiencing an *increase* in permit allocations, which is presented in Figure Appendix C.9

Heterogeneity. To further investigate the mechanisms underlying the main results, I implement a difference-in-difference-in-differences (DDD) strategy interacting the treatment with indicators of policy exposure, i.e. whether a plant belongs to a multi-plant firm ($multiplant_i$) or whether a plant possesses high levels of banked allowances at the start of Phase III ($highbank_i$). The results for the variable interactions are reported in Figure 6. Panel (a) focuses on the $multiplant_i$ dimension of heterogeneity. Treated plants that belong to multi-plant firms show significantly stronger post-2013 reductions in emissions compared to treated plants in single-plant firms. This difference is particularly visible in the active sample and becomes more pronounced in the years after the reform. The interpretation is consistent with a within-firm reallocation mechanism: when one plant in a multi-plant firm faces tighter allocation cuts, it reduces emissions more strongly, while the firm may partly offset this by shifting production or emissions to its other, unconstrained sister plants. By contrast, single-plant firms cannot reallocate across units, so the scope for such asymmetric adjustment is absent. In other words, multi-plant firms appear able to buffer aggregate shocks through internal reallocation, concentrating abatement at the most constrained installations. This finding complements the robustness analysis with firm-by-year fixed effects presented in 5, which also pointed to reallocation within firms as an important adjustment channel. Panel (b) instead examines heterogeneity by banking status. If large initial surpluses of allowances had provided flexibility, one would expect $highbank_i$ plants to reduce emissions less when facing cuts. However, the estimates provide little evidence of systematic differences between high-bank and low-bank plants.

Figure 6: Event study of $highdrop_i$ on plant-level emissions, $multiplant_i$ and $highbank_i$ heterogeneity (DDD)



(a) Multiplant

(b) Highbank

Taken together, the heterogeneity analysis shows that firm structure matters for the distribution of abatement across plants. While banking status had limited influence, multi-plant firms show clear evidence of within-firm reallocation, with constrained plants bearing a disproportionate share of emission reductions. This highlights an important implication of the baseline estimates: at the firm level, total reductions may be smaller than those observed at the most constrained plants, because some emissions are reallocated within firms. To complement these findings, I aggregate emissions at the firm level and recompute the treatment assignment for firms active whose plants are all active within the same activity group. Results presented in Figure Appendix C.8 show that, when moving to firm-level aggregates, the estimated treatment effect of high allocation cuts becomes weaker and less precisely estimated. While firm-wide emissions still decline after 2013, the magnitude is attenuated relative to the plant-level estimates, and confidence intervals widen considerably. The difference in point estimates between the plant-level and the firm-level Poisson estimate suggests that roughly 30–40% of the reductions observed at the plant level are offset by within-firm reallocation to unconstrained plants[52]. This pattern is consistent with partial offsetting of reductions at constrained plants through shifting of activity to less constrained plants within the same firm. Most importantly, it aligns with other findings in the literature (e.g. Fowlie and Perloff (2013)) which show that at the firm-level permit allocation should not impact emission production.

5.2. Results on Plant Exit

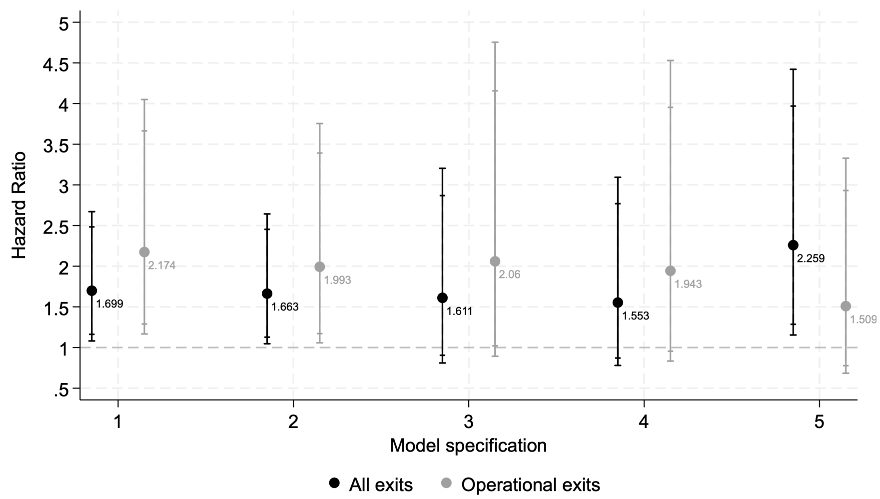
The present section further develops the overall impact the policy had on plant exits from the sample and on full closures[53]. The survival analysis provides additional insights into the extensive-margin response of plants facing strong allocation cuts. Figure 7 summarizes the hazard ratios for both all exits (black markers) and operational exits, i.e. full production shutdowns (gray markers), across alternative model specifications. Full estimates by $highbank_i$ and $multiplant_i$ assignment are presented in Appendix Tables C.7-C.10. Baseline model (1) is equal for the two figures, and reports the baseline hazard ratio for treated plants compared to control ones: estimates are always compared to baseline hazard levels equal to 1 (i.e. $highdrop_i = 0$). Models (2) add an indicator for multi-plant ownership or high-bank classification, to control for both types of policy exposure. Models (3) report the $highdrop_i$ hazard ratio for models (2), but restricted to the sample for which plant-level covariates from the energy survey are available (number of observations drops from 8,005 to

[52]This figure is obtained by comparing the estimated semi-elasticity of emissions at the plant level (Poisson estimate of about –0.33) with the corresponding firm-level estimate (about –0.19). The attenuation of the effect when moving from plants to firms implies that approximately one-third of the abatement observed at treated plants does not translate into net firm-level reductions, but is instead absorbed through within-firm reallocation of activity.

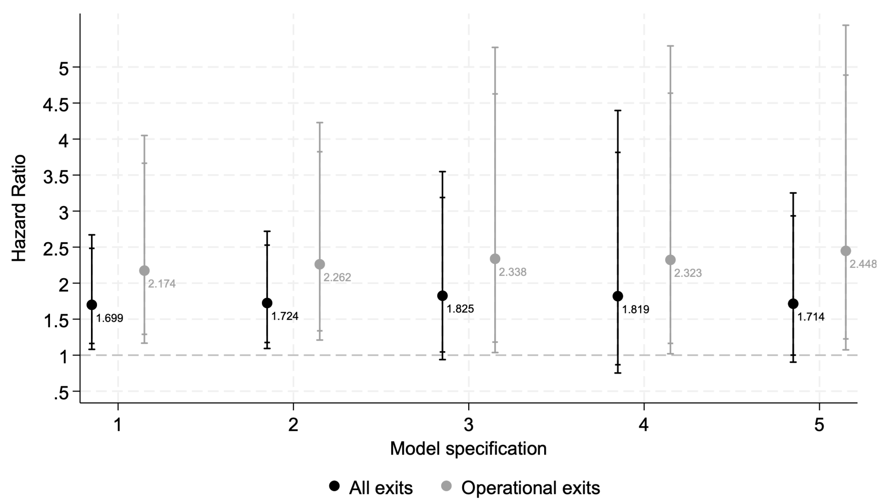
[53]See Appendix B for their classification. As a matter of fact, in this analysis I reclassify as operational exits all sample exits for which I find information of full shutdown or no information on emission or accounting reasons for exits.

4,361). Models (4) reports the $highdrop_i$ hazard ratio for model (2), but adds available-level controls at 2010 values (i.e. firm output sold, plant employment, plant energy consumption, plant dirty energy share). Finally, models (5) report the estimates for $highdrop_i$ adding interactions with $highbank_i$ and $multiplant_i$, and are then to be interpreted as the hazard ratio estimates of $highdrop_i = 1$ when $highbank_i = 0$ and $multiplant_i = 0$.

Figure 7: Hazard ratios of all exits and operational exits on $highdrop_i$



(a) Multiplant



(b) Highbank

Notes: The figure reports Cox proportional hazard estimates of plant exits (black markers) and operational exits, i.e. full shutdowns (gray markers) on the treatment indicator for strong allocation cuts ($highdrop_i$), controlling for $higbank_i$ (panel a) or $multiplant_i$ (panel b). Hazard ratios (exponentiated coefficients) are displayed, with robust standard errors and stratification at the activity level. Columns (1)–(2) and (5) use the full sample of plants: column (1) estimates the baseline effect of strong allocation cuts; columns (2) add an indicator for multi-plant or high-bank; column (5) restricts to plants belonging to single-plant firms. Columns (3)–(4) restrict the analysis to the EACEI energy survey subsample: column (3) estimates the baseline specification; column (4) adds controls for 2010 firm output sold, plant employment, plant energy consumption, and plant dirty energy share.

Looking at black estimates on all exits, the black hazard ratios consistently point to a higher likelihood of plant exit for those exposed to strong allocation cuts. The baseline estimate in specification (1) is around 1.7, implying that treated plants face roughly a 70% higher hazard of exiting relative to controls. Both under specifications (2) and models (3) and (4), hazard ratios do not strongly change, although for the latter two models confidence intervals widen, possibly due to the limited number of observations available. Most interestingly, specification (5) reports hazard ratios of 2.6 and 1.7, respectively, for treated plants owned by single-plant firms or with low levels of banking. Interestingly, the hazard ratio for plants owned by single-plant firms strongly increases in magnitude while remaining significant. This suggests that plants facing strong allocation cuts without internal firm reallocation margins are more than twice as likely to exit compared to untreated counterparts. Taken together, black estimates reveal persistent and robust hazard rates of exit between 1.7 and 1.8 across specifications, confirming that the association between strong permit cuts and plant exit is both stable and meaningful, even under more restrictive samples and controls.

When event failure is reclassified on the subsample of operational exits only, the gray estimated hazard ratios are on average even larger between 1.9 and 2.5, implying that severely constrained plants were more than twice as likely to fully shut down relative to their baseline counterparts. These findings suggest that allocation cuts not only reduced emissions at surviving plants, but also increased the likelihood of exit from the scheme and fully shutdown, reinforcing the importance of the extensive margin documented earlier in the Poisson results of Section 5.1.

6. Policy Mechanism: Emission Composition and Market Considerations

The reduced-form evidence in the past section has shown that plants receiving above-median cuts in free allocation within their activity reduced emissions more and were more likely to exit the ETS sample, and that, among exiters, a non-trivial share is represented by full operational shutdowns. While this establishes that the 2013 reform induced meaningful reallocation on both the intensive and the extensive margins, it does not by itself reveal which plants were selected out. Intuitively, two non-exclusive mechanisms can underlie a large cut: (i) a level channel, under which plants that had been relatively overallocated in Phase II faced a larger downward revision when benchmarking was introduced (i.e. *intial overallocation*); and (ii) a relative efficiency channel, under which plants whose pre-period emission intensity was far above the new EU product benchmarks suffered larger cuts (i.e. *distance-to-benchmark*). Distinguishing these channels matters both for interpretation of results and for policy implications: under the first hypothesis the reform mainly removes windfalls, whereas under the second one it primarily disciplines relatively dirty producers within the same product line.

To separate these mechanisms, I exploit two simple identities that map the legal allocation formulae into plant-level permit cuts (details on the decomposition is outlined in Appendix A). First, I define a relative-efficiency index (g_i) as the ratio of a plant's pre-period emission intensity to its historical-activity-weighted product benchmark, where available[54]. Second, I define a permit level index (θ_i) as the ratio of Phase-II free allocation to pre-period emissions, which captures how generous initial grandfathering was relative to the plant's pre-2013 emissions. Under the allocation rules for Phase II and Phase III, the 2013 change in allocation at the plant level can be written as inversely proportional to the product ($g_i \cdot \theta_i$). This accounting delivers a clean, two-factor decomposition of the reform's bite. A necessary check is whether this two-factor mapping actually reproduces the empirical treatment variable of $drop_{i,2013}$ that drives the reduced-form results above. As shown in Appendix A, the continuous drop variable is almost perfectly collinear with $(g_i \cdot \theta_i)^{-1}$ (correlation ~ 0.999). Moreover, the two-factor decomposition recovers the expected rankings and distributions of the groups used in the analysis[55]. With these components in hand, the question becomes whether exits are explained by level cuts *per se*, or by relative inefficiency.

Table 3 addresses this using Cox proportional-hazard models where the hazard of exit is related to $\log(g_i)$, $\log(\theta_i)$, and their interaction. Panel (a) considers all exits from the ETS sample. The coefficient on $\log(g_i)$ is large and precisely estimated: regardless of the plant's baseline level of g_i, a 10 percent increase in distance from the benchmark raises the exit risk by about 5 percent. In other words, if a plant's emission intensity is 10% further away from its product benchmark, its chance of exiting the sample goes up by about 5%. Most importantly, the effect is cumulative, meaning that, for instance, a plant at the 90th percentile of g (i.e. quite far from the benchmark) could face up to 40% higher exit risk than a plant closer to the benchmark. In contrast, $\log(\theta_i)$ is statistically indistinguishable from one once $\log(g_i)$ is included, and their interaction is small and imprecise. Panel (b) restricts attention to operational exits, i.e., full shutdowns. The basic pattern strengthens.

[54] Indeed, although neither plant emission intensity nor product benchmarks a plant was mostly subject are known, one can derive the ratio of the two and know whether this ratio is above 1 (i.e. dirtiest plants compared to the benchmark, since their emissions per unit of output were above best practice in 2007–2008), below 1 (i.e. cleaner plants that "beat" the EU benchmark) or equal to 1 (i.e. plants whose emission intensity was close to the EU benchmark). Most importantly, this product benchmark decomposition does not work for plants in Activity 20, i.e. combustion of fuels. Indeed as reported in EC (2011), "*Where deriving a product benchmark was not feasible, but greenhouse gases eligible for the free allocation of emission allowances occur, those allowances should be allocated on the basis of generic fallback approaches*" of heat and fuel benchmarks for installations classified under Activity 20.

[55] In other words, the benchmarking rule's plant-level heterogeneity can be summarized by two interpretable components, and that summary faithfully reproduces the continuous treatment dose.

The hazard associated with $\log(g_i)$ remains economically and statistically significant, while $\log(\theta_i)$ stays near one and insignificant. Notably, the interaction term becomes both sizable and significant: full shutdown risk is especially high for plants that are both far from the benchmark and had enjoyed more generous Phase-II permit allocations[56].

These mechanism results align closely with the reduced-form findings. First, they rationalize why high-drop plants reduce emissions and exit more: the high-drop label identifies plants with large $(g_i \cdot \theta_i)^{-1}$, and the hazard models show that it is the (g_i) component, i.e. distance from product-level best practices, doing the work. Second, they sharpen the interpretation of exit composition. Exiters are not simply plants that saw large level revisions in free allocation; rather, they are disproportionately the relatively dirtiest within their product categories, precisely the installations for which cleaner technologies are demonstrably feasible (i.e. production under a cleaner emission intensity *is* already available at the EU level) but were not adopted by the plant pre-reform.

Table 3: Survival analysis on $\log(g_i)$ and $\log(\theta_i)$, all exits vs operational exits

Panel (a): All exits

	(1) Exit hazard	(2) Exit hazard	(3) Exit hazard	(4) Exit hazard
$\log(g)$	1.51***		1.51***	1.56***
	(0.17)		(0.17)	(0.23)
$\log(\theta)$		0.92	0.99	0.99
		(0.14)	(0.16)	(0.16)
$\log(g) \times \log(\theta)$				1.07
				(0.12)
Strata	A	A	A	A
Observations	4021	4021	4021	4021
LR Chi-sq	14.16	0.35	14.25	13.44
Log-likelihood	-92.38	-94.66	-92.38	-92.29

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel (b): Operational exits

	(1) Exit hazard	(2) Exit hazard	(3) Exit hazard	(4) Exit Hazard
$\log(g)$	1.32*		1.34*	1.52***
	(0.20)		(0.21)	(0.24)
$\log(\theta)$		1.02	1.09	1.01
		(0.13)	(0.16)	(0.16)
$\log(g) \times \log(\theta)$				1.35**
				(0.17)
Strata	A	A	A	A
Observations	3912	3912	3912	3912
LR Chi-sq	3.40	0.03	3.43	8.53
Log-likelihood	-65.93	-66.79	-65.86	-64.87

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports Cox proportional hazard estimates of plant exit from the ETS sample and full operational shutdown, on indicators for distance from the benchmark ($log(g_i)$) andinitial permit allocation (([$log(\theta_i)$)). Hazard ratios (exponentiated coefficients) are estimated with robust standard errors and stratification at the activity level.

[56]This complementarity is intuitive. Benchmarking targets inefficiency, so high-g_i plants face structurally tighter constraints; if those same plants also lose a Phase-II windfall (higher θ_i), the cumulative cost pressure increases further, making exit more likely.

Additionally, to understand whether the sectors that experienced stronger cuts in free carbon permit allocations became more concentrated over time, I combined information from two different levels of the data: the activity level, where the EU ETS allocation rules most closely operate, and the sector level, where market concentration can be meaningfully measured. Starting from the median activity drop, I constructed an emissions-weighted exposure index at the sector level[57]. I did so by taking 2005 emissions share of each activity within that sector as weights, and combined them with the corresponding activity's median drop. This produces an indicator of how strongly, on average, a sector was affected by the activity-level reform, i.e. sector exposure to activity-level cuts. Sectors mostly composed of heavily targeted activities have more negative exposure values. On the outcome side, I measured how emission concentration evolved between 2005 and 2020 in a sample that includes all exiters and entrants. Within each sector, I calculated the share of total sectoral emissions accounted for by each installation and used these shares to compute a standard Herfindahl–Hirschman Index (HHI)[58].

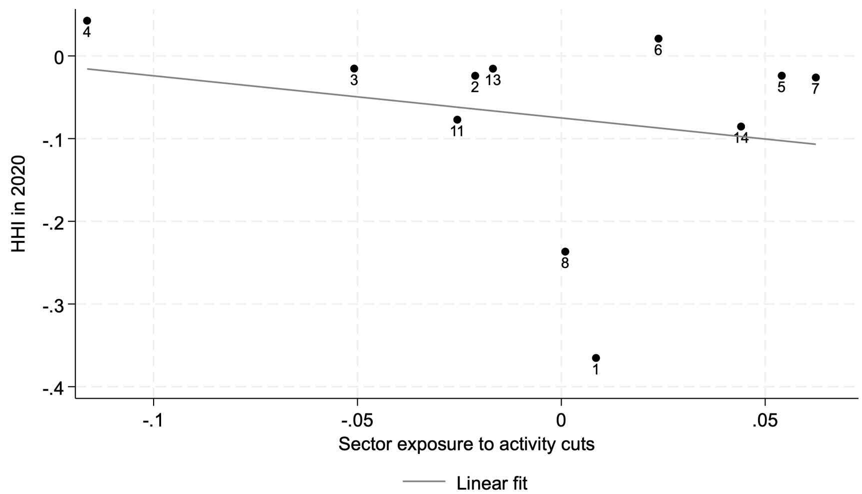
Finally, I brought the two pieces together. I matched each sector's exposure index with its emission concentration in 2005 and 2020, creating a small sample of eleven sectors. I first checked whether sectors that were more exposed to allocation cuts ended up with higher or lower concentration by 2020, by regressing 2020 HHI values on baseline 2005 values and the above-computed sector exposure to activity-level cuts. The regression results show a strong and significant negative association between sector exposure and HHI in 2020, even after controlling for each sector's initial concentration in 2005 (coefficient = –0.32, p-val = 0.002). Because exposure is negative for sectors facing stronger cuts, this negative coefficient means that the more exposed sectors became more concentrated by 2020. In other words, sectors where permit allocations were reduced more sharply saw emissions become more dominated by a few remaining installations. As shown in Figure 8, sectors whose activities were hit by stronger cuts in free permit allocations tended to show higher emission concentration by 2020, even after accounting for how concentrated they already were before the reform.

Overall, the evidence in this section indicates that the 2013 benchmarking reform altered market composition along a technologically meaningful margin. By tying free allocation to product-level best practice, it induced the largest contractions among plants that were far from the benchmark rather than merely those that had previously received generous grandfathering. The policy therefore appears to have operated not only through a uniform reduction in free allocation, but more importantly through targeted pressure on relatively

[57]Recall from Figure Appendix C.2 that each sector might include multiple activities.

[58]In this sense, this emission-based HHI measure increases when emissions become more dominated by a few large emitters, and decreases when they are spread across many production units.

Figure 8: Sector exposure to activity-level cuts and HHI emission concentration (controlling for baseline HHI)



Notes: Each point represents one sector. The horizontal axis plots the sector's exposure to activity-level permit cuts, constructed as a 2005 emissions-weighted average of the median allocation drop across its constituent activities. More negative values indicate sectors composed of more strongly targeted activities. The vertical axis plots 2020 emission concentration (in HHI), net of initial concentration. The fitted line shows the partial relationship between sector exposure and 2020 concentration from a regression controlling for baseline HHI and weighted by 2005 sectoral emissions. The negative slope (–0.32, p = 0.002) implies that sectors more exposed to permit allocation cuts became more concentrated by 2020, consistent with exit and reallocation dynamics among smaller emitters. Importantly, exposure is not correlated with initial concentration: testing the relationship between exposure and the 2005 HHI yields no significant link (p-val = 0.78).

inefficient producers. Additionally, while the sector analysis covers only eleven sectors and should therefore be seen as descriptive rather than causal, it also corroborates earlier findings in the analysis. As initially supposed in Figure 1, when free allowances are reduced more aggressively, smaller and/or less efficient plants became more likely to exit, leaving a smaller number of large emitters responsible for a greater share of sectoral emissions[59].

7. Conclusions

This paper examined the impact of a 2013 change in the free permit allocation rule for plants within the European Union Emissions Trading Scheme (EU ETS) on both plant-level carbon emissions and plant exit dynamics. While previous studies have documented emissions reductions under the EU ETS, much of the existing literature focuses on earlier phases of the EU cap-and-trade, when the policy lacked sufficient stringency and provided weaker incentives for emissions abatement. Additionally, no prior research has analyzed the impact of the ETS beyond the simple distinction between regulated and unregulated plants, nor specifically exploited variation in permit allocation within the sample of ETS-

[59] Again as descriptive evidence, Appendix Table C.4 use pre-policy baseline controls and show that plants later exiting had in 2010 significantly lower values of total energy consumption (i.e. were possibly smaller) and marginally higher dirty energy shares (i.e. were possibly dirtier).

regulated plants. Finally, evidence on potential compositional effects within this group remains extremely scarce, despite its relevance for understanding market restructuring and firm dynamics. This study contributes to filling these gaps by analyzing whether the observed emissions reductions since 2013 resulted from an overall decline in emissions across all plants, or were instead driven by a shift in emissions due to policy stringency and plant exits.

Using a difference-in-differences approach, I analyze how the 2013 reform in free carbon permit allocations affected plant-level emissions among French industrial installations. I distinguish industrial plants along two main policy dimensions: those facing stronger policy stringency relative to their activity peers, and those more exposed to the policy due to a limited stock of pre-existing banked permits or a lack of within-firm reallocation channels. The estimates show that plants experiencing above-median cuts in free allocations reduced emissions by at least 10% more relative to comparable plants with smaller cuts, with no significant pre-trends prior to the policy announcement. When accounting for plants that exited the sample, Poisson estimates indicate an overall reduction in verified emissions of around 25–30%, of which about one-third can be attributed to plant closures rather than operational abatement among survivors. Moreover, firm-level aggregation reveals that roughly 30–40% of the emission reductions observed at treated plants were offset by reallocation of activity to less constrained plants within the same firm, confirming the presence of a within-firm adjustment channel.

To examine the extensive margin, I estimate a survival model linking plant exits to policy stringency and exposure measures. The results confirm that tighter allocation constraints increased the likelihood of exit. Treated plants faced a 70–80% higher hazard of exiting than their counterparts, and this effect rose to over 120% among plants owned by single-plant firms with no scope for internal reallocation. By contrast, plants belonging to multi-plant firms were significantly more resilient, consistent with within-firm flexibility to redistribute production and compliance costs. Decomposing the magnitude of permit cuts reveals that exit probabilities are primarily explained by a plant's distance from the newly introduced EU product benchmarks rather than by previous overallocation, showing that relatively dirtier and less efficient plants were disproportionately selected out. At the sector level, industries composed of more strongly targeted activities became modestly more concentrated by 2020, consistent with selective exit of smaller and dirtier emitters.

These results show that the 2013 reform not only reduced emissions but also reshaped the industrial structure within regulated sectors. The policy induced a reallocation of emissions toward plants closer to the best-practice efficiency benchmarks introduced by the reform. While average emissions among surviving plants increased slightly, this pattern reflects a compositional shift: larger, more efficient plants expanded their output once smaller, less efficient ones exited. Overall, these findings provide empirical support for recent theoretical models of endogenous firm adaptation under tightening allocation constraints, highlighting that carbon pricing can achieve emission reductions both through efficiency improvements and market selection effects. Despite these insights, several limitations remain. The analysis focuses on plant-level behavior and should be complemented by evidence on firm-level exits to fully assess the broader industry composition effects. If plant closures also translate into firm exits in highly exposed sectors, the implications for competitiveness and market structure could be more substantial than captured here. Moreover, the study abstracts from detailed output and market share dynamics, which would be needed to quantify welfare and distributional consequences of the reform. A more structural modeling framework linking abatement, exit, and reallocation decisions could therefore provide a richer understanding of these adjustment margins. Finally, as the analysis covers incumbent producers only, it remains agnostic to the emission behavior of new entrants and to possible carbon leakage toward installations outside the EU ETS.

From a policy perspective, these findings underline the importance of allocation design and compliance flexibility in shaping both environmental and industrial outcomes. Tighter allocation rules can effectively reduce emissions, but their distributional and selection effects require careful consideration. This is particularly true in sectors characterized by high capital intensity, limited short-term abatement options, or pre-existing high concentration. By showing that part of the observed emission decline arises from compositional adjustments rather than uniform technological progress, this paper highlights that the EU ETS Phase III reform achieved its environmental objectives partly by accelerating structural change toward cleaner and more efficient producers.

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Appendix A. Decomposition of the Permit Allocation Change

Under assumptions *A1-A4*, the observed 2013 allocation drop for plants in activities explicitly subject to benchmarking (i.e. fuel combustion excluded) can be rewritten in terms of two interpretable plant-level components: (i) distance from product benchmark g_p and (ii) Phase II permit overallocation θ_p.

Assumptions.

1. ***A1*: Phase II allocation.** Under the French National Allocation Plan (NAP), free allocations during Phase II (2008–2012) were grandfathered based on 2005 emissions scaled by a sector-specific growth factor ϕ_s:

$$FA^{(II)}_{p,2012} = \phi_s \cdot E_{p,2005}.$$

This rule implied that Phase II allocations essentially reproduced past emissions, adjusted only for expected sectoral expansion, and were not benchmarked against relative efficiency.

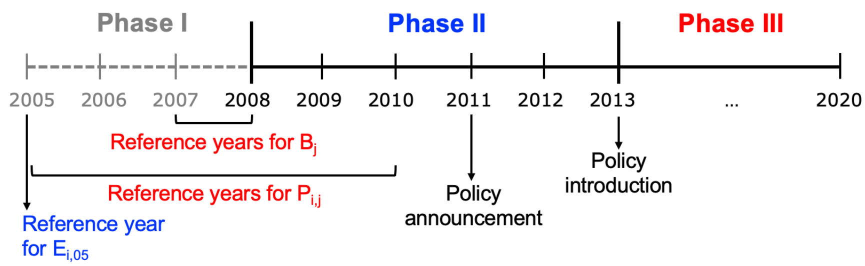
2. ***A2*: Phase III allocation.** According to the 2011 Directive EC (2011), and as modeled in Sartor et al. (2014), Phase III allocation for non-electricity generation plants was built on a benchmarking system:

$$FA^{(III)}_{p,j,2013} = B_j \cdot Y^{HAL}_{p,j} \cdot RF_{j,2013} \cdot CF_{2013}.$$

The term B_j (activity-level component of the policy) refers to 54 product benchmarks, calculated as the arithmetic average of the greenhouse gas performance of the 10% most emission-efficient installations in 2007–2008 across the EU, as detailed in Annex I of EC (2011). The parameter $Y^{HAL}_{p,j}$ (plant-level component of the policy) corresponds to the highest median historical production of product j by plant i in either 2005–2008 or 2009–2010, depending on which is greater, as specified in Annex III of EC (2011). The reduction factor $RF_{j,t}$ applies to products not deemed at risk of carbon leakage, and decreases linearly from 0.8 to 0.3 between 2013 and 2020 to progressively reduce overallocation. Annex VI of EC (2011) provides the list of sectors exempt from this factor because of carbon leakage risk. Finally, the correction factor CF_t is applied uniformly across all sectors, decreasing from 0.94 to 0.82, to ensure that the aggregate free allocation remains consistent with the EU-wide emissions cap, as required by Annex II of EC (2013). Accordingly, $FA^{(III)}_{p,j,t}$ represents the free carbon permits allocated to product j produced in plant i in year t. Summing across all products j gives the plant's total allocation $FA^{(III)}_{p,t}$ (as shown below).

This reform marked a sharp transition from the emission-based grandfathering of Phase II to a benchmarking rule harmonized at the EU level based on a combination of plant-level output and emission-intensity best practices at the EU level. Figure Appendix A.9 summarises a timeline of the different components of Phase III allocation rule.

Figure Appendix A.9: Timeline of ETS Phases and free allocation rules



As a clarifying example, Table A.4 illustrates two cement plants within the same activity. Under Phase II, both received similar free allocations tied to their 2005 emissions. In Phase III, however, the EU rule generated different allocation cuts (and different treatment assignments under this analysis) because of differences in their predetermined historical output levels and their relative distance from the benchmark.

Table A.4: Treatment Assignment Example

Year	FA for Plant A	FA for Plant B	$drop_{2013}$ Plant A	$drop_{2013}$ Plant B	Activity Median Drop	*highdrop* Plant A	*highdrop* Plant B
2008	323,219	323,989	-0.1515	-0.20197	-0.1791	0	1
2009	323,219	323,989	-0.1515	-0.20197	-0.1791	0	1
2010	323,219	323,989	-0.1515	-0.20197	-0.1791	0	1
2011	323,219	323,989	-0.1515	-0.20197	-0.1791	0	1
2012	323,219	323,989	-0.1515	-0.20197	-0.1791	0	1
2013	274,256	258,552	-0.1515	-0.20197	-0.1791	0	1
2014	269,492	254,061	-0.1515	-0.20197	-0.1791	0	1
2015	264,672	249,517	-0.1515	-0.20197	-0.1791	0	1
2016	259,803	244,926	-0.1515	-0.20197	-0.1791	0	1
2017	254,880	240,286	-0.1515	-0.20197	-0.1791	0	1
2018	249,910	235,600	-0.1515	-0.20197	-0.1791	0	1
2019	244,878	230,856	-0.1515	-0.20197	-0.1791	0	1
2020	239,828	226,096	-0.1515	-0.20197	-0.1791	0	1

Notes: Activity median drop is computed within activity 29 (cement clinker production). *highdrop* = 1 if the plant's drop in allocation in 2013 exceeds the median drop of the sector. Both plants are in the same activity but experience different treatment intensity due to different relative exposure to the 2013 benchmark rule.

3. ***A3*: Emission identity.** Plant-level emissions can be broadly decomposed as the product of plant-level output and plant-level emission intensity:

$$E_{p,t} = Y_{p,t} \cdot EI_{p,t}.$$

This simple identity allows me to express allocation rules in terms of relative efficiency (emissions per unit output), which is central for decomposing the 2013 drop.

4. ***A4*: Product mix stability.** I assume that plants do not radically change the composition of their product portfolio between the HAL period (2005–10, used for historical output) and the benchmark base period (2007–08, used to compute B_j). This ensures that comparisons of plant-level activity $Y_{p,j}^{HAL}$ and benchmarks B_j are consistent, and that the decomposition captures relative efficiency rather than shifts in product mix.

Definitions. Let E_p^{pre} be the historical emission baseline (higher of the 2005–08 or 2009–10 medians):

$$E_p^{pre} \equiv \max\{\text{median}_{2005\text{–}08} E_{p,t},\ \text{median}_{2009\text{–}10} E_{p,t}\}.$$

Under *(A3)-(A4)*, plant-level emission intensity can be written as:

$$E_p^{pre} \; = \; Y_p^{HAL} \cdot EI_p^{pre} \quad \Rightarrow \quad EI_p^{pre} \; = \; \frac{E_p^{pre}}{Y_p^{HAL}}$$

Under *(A2)*, each product j made by plant p receives the following allocation:

$$FA_{p,j,2013}^{(III)} = B_j \cdot Y_{p,j}^{HAL} \cdot RF_{j,2013} \cdot CF_{2013}$$

Summing over j gives plant-level allocation:

$$FA_{p,2013}^{(III)} = \sum_j B_j \, Y_{p,j}^{HAL} \, RF_{j,2013} \, CF_{2013}$$

Define the *HAL-weighted plant benchmark* and *HAL output*:

$$\overline{B}_p^{HAL} \equiv \frac{\sum_j B_j \, Y_{p,j}^{HAL}}{\sum_j Y_{p,j}^{HAL}}, \qquad Y_p^{HAL} \equiv \sum_j Y_{p,j}^{HAL},$$

and the *effective plant reduction factor* $RF_{p,2013}^{e} \equiv \frac{\sum_j B_j Y_{p,j}^{HAL} RF_{j,2013}}{\sum_j B_j Y_{p,j}^{HAL}}$ Then

$$FA_{p,2013}^{(III)} \; = \; \overline{B}_p^{HAL} \cdot Y_p^{HAL} \cdot RF_{p,2013}^{e} \cdot CF_{2013}$$

Define the plant's **distance from product benchmark** in terms of plant emission intensity:

$$g_p \; \equiv \; \frac{EI_p^{pre}}{\overline{B}_p^{HAL}} \; = \; \frac{E_p^{pre}}{\overline{B}_p^{HAL} \, Y_p^{HAL}} \; \approx \; (RF_{p,2013}^{e} \cdot CF_{2013}) \cdot \frac{E_p^{pre}}{FA_{p,2013}^{(III)}}$$

Define the plant's Phase II **initial overallocation** relative to its Phase II emissions[60]:

$$\theta_p \equiv \frac{FA^{(II)}_{p,2012}}{E^{pre}_p}$$

<u>*Decomposition.*</u> Starting from the ratio of post-reform to pre-reform free allocation:

$$\frac{FA^{(III)}_{p,2013}}{FA^{(II)}_{p,2012}} = \frac{\overline{B}^{HAL}_p \, Y^{HAL}_p \, RF^e_{p,2013} \, CF_{2013}}{FA^{(II)}_{p,2012}}$$

multiplying and dividing by E^{pre}_p and using the definitions above:

$$\frac{FA^{(III)}_{p,2013}}{FA^{(II)}_{p,2012}} = \underbrace{\frac{\overline{B}^{HAL}_p \, Y^{HAL}_p}{E^{pre}_p}}_{=1/g_p} \cdot \underbrace{\frac{RF^e_{p,2013} \, CF_{2013} \, E^{pre}_p}{FA^{(II)}_{p,2012}}}_{=\frac{RF^e_{p,2013} \, CF_{2013}}{\theta_p}} = \frac{RF^e_{p,2013} \, CF_{2013}}{g_p \, \theta_p}$$

the continuous allocation drop variable $drop_{p,2013}$ can be written as:

$$drop_{p,2013} \equiv \frac{FA^{(III)}_{p,2013} - FA^{(II)}_{p,2012}}{FA^{(II)}_{p,2012}} = \frac{FA^{(III)}_{p,2013}}{FA^{(II)}_{p,2012}} - 1 = \frac{RF^e_{p,2013} \, CF_{2013}}{g_p \, \theta_p} - 1.$$

where according to EC (2011) CF_{2013}=0.9427 and $RF^e_{j,2013}$=0.80 for activities mostly producing products not at risk of carbon leakage (e.g. 32). Hence, the heterogeneity in permit drops can be explained as driven entirely by the inverse product $(g_p\theta_p)^{-1}$.

<u>*Empirical validation.*</u> Table A.5 shows that the empirical correlation between $drop_{2013}$ and $1/(g_p \cdot \theta_p)$ is nearly one, confirming the decomposition. In contrast, correlations with g or θ alone are weaker.

Table A.5: Correlations between $drop_{2013}$, g_p, θ_p and $(g_p\theta_p)^{-1}$

	log_g	log_theta	inv_g_theta	drop_2013
log_g	1.000	-0.169	-0.419	-0.430
log_theta	-0.169	1.000	-0.180	-0.180
inv_g_theta	-0.419	-0.180	1.000	0.999
drop_2013	-0.430	-0.180	0.999	1.000

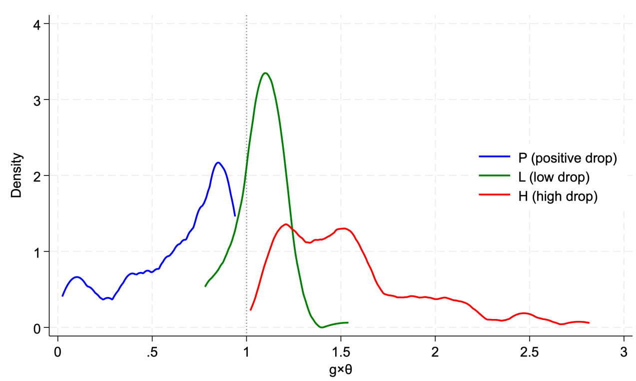
[60]Under (A1), $\theta_p = \phi_s \cdot \frac{E_{p,2005}}{E^{pre}_p}$. There is no need to additionally impose $E_{p,2005}$=E^{pre}_p, since for most plants highest emissions were produced in 2005-2008 compared to 2009-2010, E^{pre}_p is already well approximated by $E_{p,2005}$.

Starting from the treatment assignment in the main text, one can define three groups of plants as:

1. ***Group H***: plants for which $highdrop_i = 1$;
2. ***Group L***: plants for which $highdrop_i = 0$ and $drop_{i,2013} \leq 0$;
3. ***Group P***: plants for which $highdrop_i = 0$ and $drop_{i,2013} > 0$.

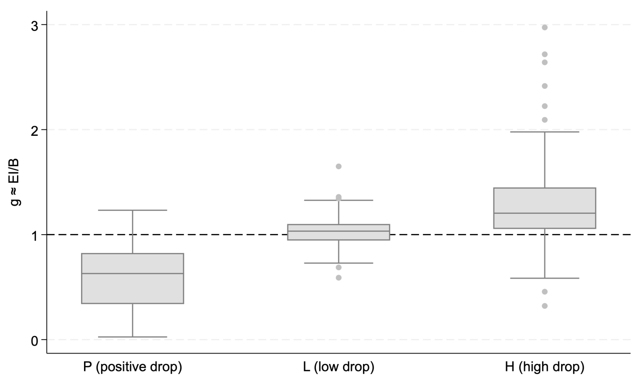
Based on the structure of the policy change, plants are likely to be assigned to group H if (1) either $g_p > 1$; or (2) $\theta_p > 1$; or (3) both. Specifically for g_p, a value above 1 implies $EI_p^{pre} > \overline{B}_p^{HAL}$, i.e. the plant average emission intensity in 2005-2010 was higher than the best practices in emission intensity under the newly introduced EU-benchmarks. A value below 1 implies $EI_p^{pre} < \overline{B}_p^{HAL}$, i.e. the plant emission intensity "beats" the EU benchmark. A value close to 1 implies that the plant emission intensity is very close to the EU benchmark. Accordingly, one would expect: (1) average g_p for group H (i.e. $\overline{g_H}$) to sit above 1; (2) average g_p for group L (i.e. $\overline{g_L}$) to sit close to 1; (3) average g_p for group P (i.e. $\overline{g_P}$) to sit below 1[61]. In line with this intuition, Figure Appendix A.10 and FigureAppendix A.11 confirm that the decomposition $g_p \times \theta_p$ closely replicates the distribution and rankings of the three groups.

Figure Appendix A.10: Density of $g_p \times \theta_p$, by groups

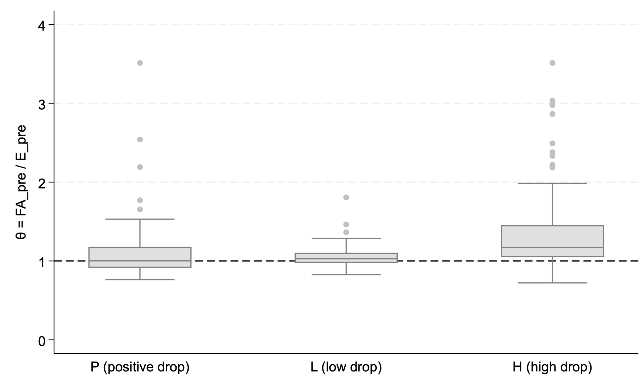


[61]A similar intuition holds for $\overline{\theta_p}$ rankings across the three groups.

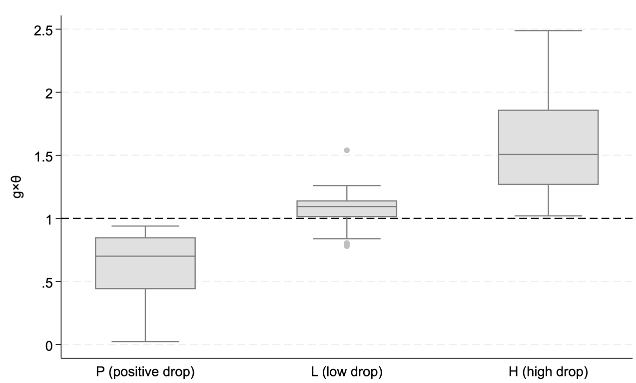
Figure Appendix A.11: Distribution of average g_p, θ_p and $g_p \times \theta_p$, by groups



(a) g_p



(b) θ_p



(c) $g_p \times \theta_p$

Appendix B. Exit Classification

As presented in Section 3, manual checks on exiting plants have been performed, crossing information between press articles, company websites and inspection data presented in the *Géorisques* portal. For many installations in *Géorisques*, identified based on the respective SIRET code, one can find information on location, activity and most recent expert inspections. Combustion activities are defined as under activity 2910, with code A.1 if the total nominal thermal power of the combustion installation is above 20MW, and with code A.2 if the value is below 20 MW.

I am therefore able to isolate out of the 77 exits initially found: 10 accounting exits (i.e. plants for which I find evidence of M&A with other plants); 30 operational exits (i.e. plants for which I find evidence of full plant shutdown); 23 emission exits (i.e. plants that are formally active but for which I find evidence in République Française (2025) that their latest recorded thermal combustion capacity is below the 20MW threshold); and 14 other exits, for which I do not find additional information neither on emission exit nor on accounting exit, and will therefore be kept together with operational exits in the operational exits analyses. Interestingly, the installed capacity of emission exits appear to bunch below 20MW, evidence of ETS policy-avoidant behaviour (see Figure Appendix B.12).

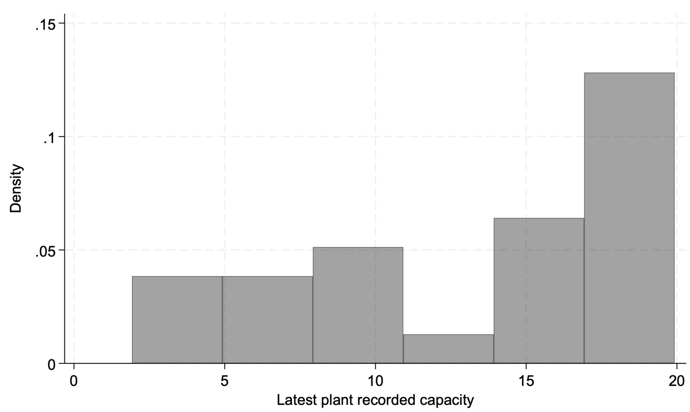
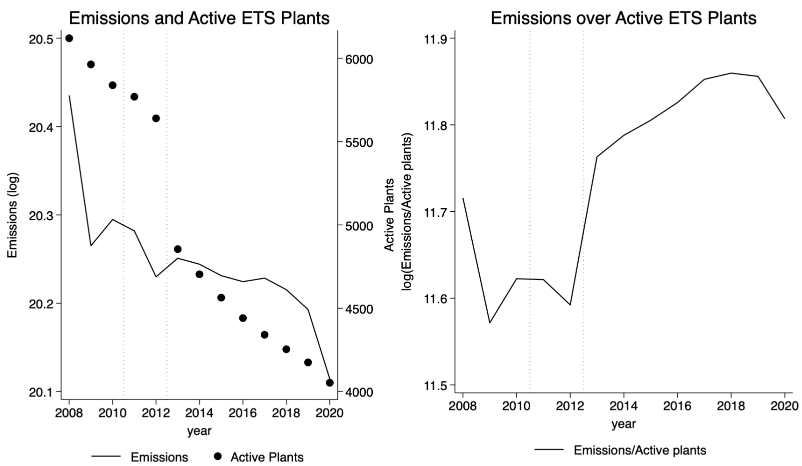


Figure Appendix B.12: Histogram of latest recorded capacity of plants classified as "emission exits"

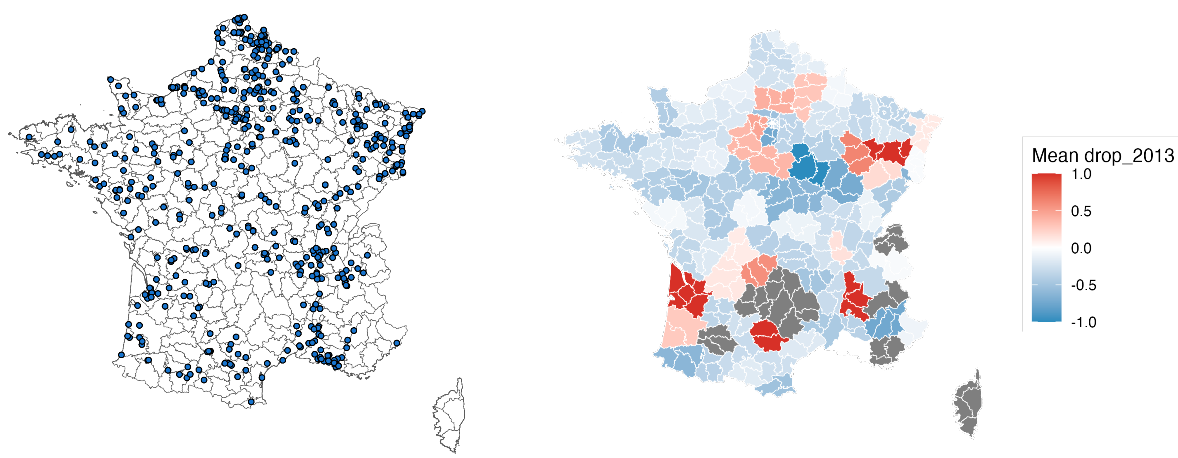
Appendix C. Additional Graphs and Tables

Appendix C.1. Descriptive graphs and tables



Notes: Emissions and active EU plants in the industry sector covered by the EU Emission Trading Scheme (EU ETS). "Active plants" include all plants with positive values of verified emissions in 2008 (i.e. new entrants since 2009 are excluded).

Figure Appendix C.1: Map of plants in the main sample, by districts



(a) Location of plants

(b) Mean $drop_{2013}$ of plants

Table C.1: Exit rates and emissions by activities and sectors

(a) By activity

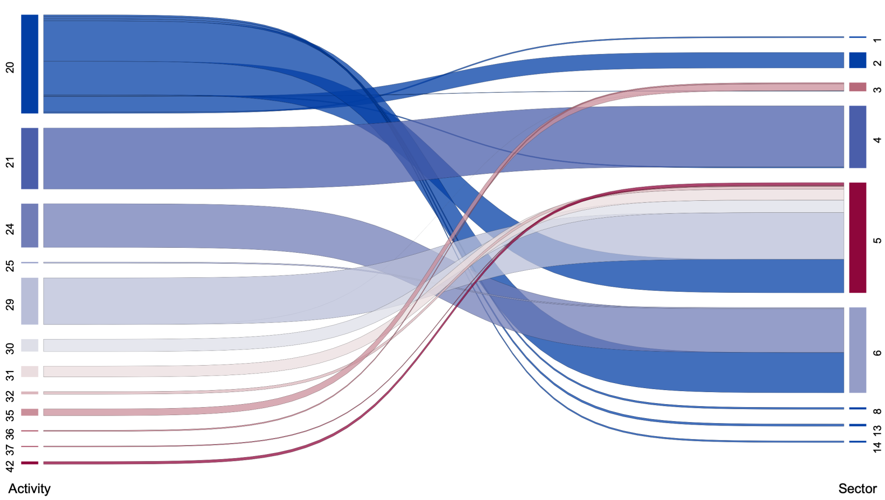
Activities	Description	E_{2005} (thous. CO_2)	Number of plants	Share of E_{2005}	Exiting plants	Exit rate	E_{2005} of exiters	Share of E_{2005} (exiters)
20	Combustion of fuels	27761	259	33.8%	51	19.7%	1022	3.7%
21	Refining of mineral oil	17381	13	21.2%	4	30.8%	3958	22.8%
24	Production of pig iron or steel	12452	17	15.2%	1	5.9%	33	0.3%
25	Production/processing of ferrous metals	362	4	0.4%	0	0.0%	0	0.0%
29	Production of cement clinker	13304	33	16.2%	3	9.1%	1536	11.5%
30	Production of lime/dolomite calcination	3547	16	4.3%	1	6.3%	329	9.3%
31	Manufacture of glass	3102	42	3.8%	1	2.4%	13	0.4%
32	Manufacture of ceramics	783	41	1.0%	5	12.2%	49	6.3%
35	Production of pulp	1957	73	2.4%	11	15.1%	278	14.2%
36	Production of paper/cardboard	366	9	0.4%	0	0.0%	0	0.0%
37	Production of carbon black	267	5	0.3%	0	0.0%	0	0.0%
42	Production of bulk chemicals	812	4	1.0%	0	0.0%	0	0.0%

(b) By sector

Sectors	Description	E_{2005} (thous. CO_2)	Number of plants	Share of E_{2005}	Exiting plants	Exit rate	E_{2005} of exiters	Share of E_{2005} (exiters)
1	Agriculture and Mining	385	3	0.5%	2	66.7%	380	98.7%
2	Food, Beverage and Tobacco	4407	99	5.4%	12	12.1%	154	3.5%
3	Textiles, Wood and Paper	2476	95	3.0%	16	16.8%	343	13.8%
4	Coke, Petrol and Plastics	17699	27	21.6%	8	29.6%	4024	22.7%
5	Chemicals and Pharma	31001	198	37.8%	19	9.6%	2162	7.0%
6	Metals	24282	27	29.6%	1	3.7%	33	0.1%
7	Electronics	22	3	0.0%	1	33.3%	5	23.3%
8	Machinery and Transport	596	23	0.7%	5	21.7%	34	5.8%
11	Water and Waste	35	3	0.0%	0	0.0%	0	0.0%
13	Trade, Transport and Storage	684	22	0.8%	5	22.7%	33	4.8%
14	Services	494	15	0.6%	8	53.3%	51	10.4%

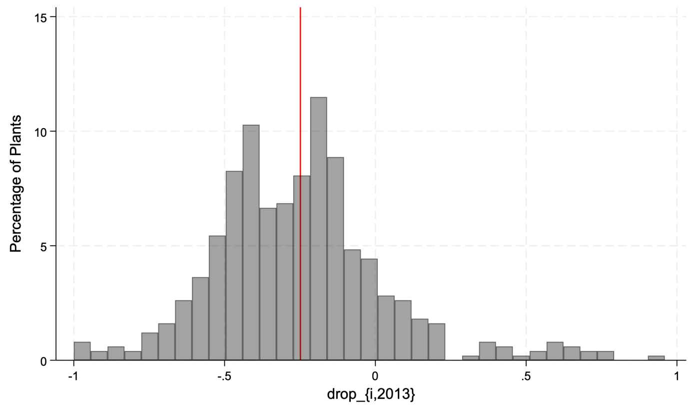
Notes: Exit rates and emissions in 2005 by activities and sectors. Exit rate is the ratio of number of 2005 active plants over exiters as of Phase III. Share of 2005 emissions (exiters) is the ratio of emissions produced in 2005 by exiters, over total emissions of plants in that activity/sector. Overall plant exit rate for the sample is 15%, corresponding to 9% of 2005 emissions. Rates are similar for the sample including plants with positive values of $drop_{i,2013}$. Panel (a) shows plant-level activities as defined in Table C.1 of Abrell (2021) Panel (b) shows aggregated sectors generated from NACE2 codes: 1=Agriculture and mining (1–6), 2=Food, beverages, and tobacco (10–12), 3=Textiles, wood, and paper (13–17), 4=Coke, petroleum, and plastic (19,22), 5=Chemicals, pharma, and non-metallic minerals (20,21,23), 6=Metals and metal products (24,25), 7=Electronics and electrical equipment (26,27), 8=Machinery and transport equipment (28–30), 9=Other manufacturing (31–33), 10=Electricity generation (35), 11=Water and waste (37–38), 12=Construction (41–43), 13=Trade, transportation, and storage (46–52), 14=Services (61+). Sectors 9, 10, and 12 are excluded due to insufficient observations or electricity generation.

Figure Appendix C.2: Overlap between plant activity and plant sector



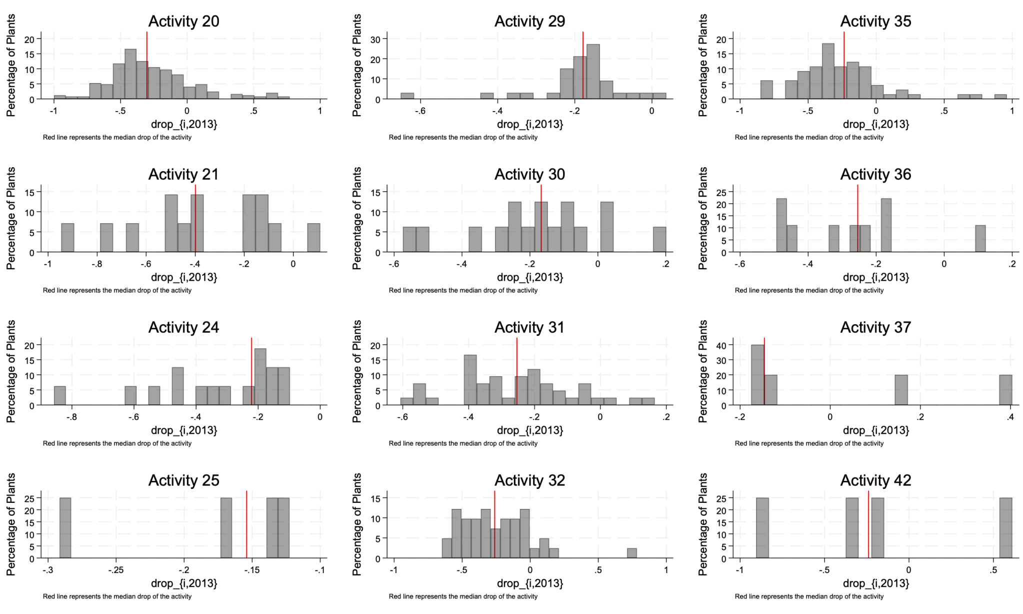
Notes: Plant-level activities are defined as defined in Table C.1 of Abrell (2021) Plant sectors are aggregated based on NACE2 codes: 1=Agriculture and mining (1–6), 2=Food, beverages, and tobacco (10–12), 3=Textiles, wood, and paper (13–17), 4=Coke, petroleum, and plastic (19,22), 5=Chemicals, pharma, and non-metallic minerals (20,21,23), 6=Metals and metal products (24,25), 7=Electronics and electrical equipment (26,27), 8=Machinery and transport equipment (28–30), 9=Other manufacturing (31–33), 10=Electricity generation (35), 11=Water and waste (37–38), 12=Construction (41–43), 13=Trade, transportation, and storage (46–52), 14=Services (61+). Sectors 9, 10, and 12 are excluded due to insufficient observations or electricity generation.

Figure Appendix C.3: Distribution of $drop_{i,2013}$ variable



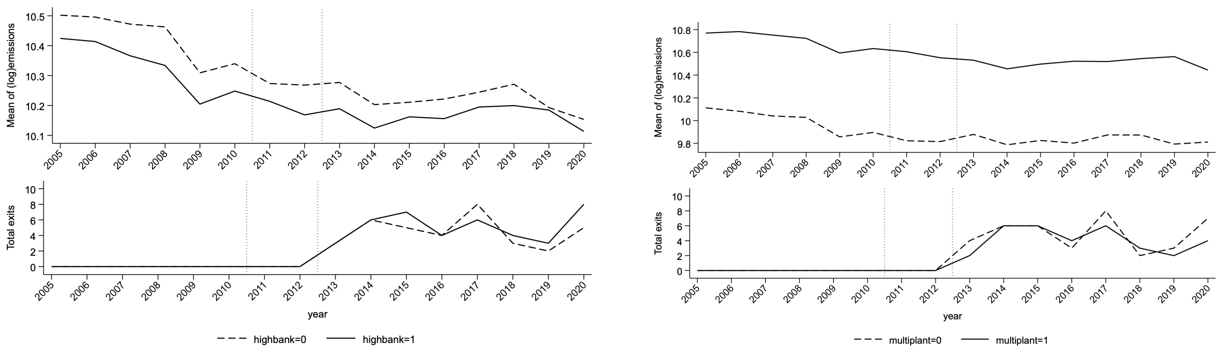
Notes: A value of $drop_{i,2013}$ close to 0 corresponds to plant i free permits in 2013 being close to what they were up until 2012; a value close to -1 signals that plant i experiences an almost full drop in free permits between the two years.

Figure Appendix C.4: Distribution of $drop_{i,2013}$ variable, by activity



Notes: A value of $drop_{i,2013}$ close to 0 corresponds to plant i free permits in 2013 being close to what they were up until 2012; a value close to -1 signals that plant i experiences an almost full drop in free permits between the two years. Plant activities are defined as in Table C.1 of Abrell (2021).

Figure Appendix C.5: Emissions and exits, by highbank and multiplant



(a) Highbank

(b) Multiplant

Table C.2: Composition of activity and sectors in Full sample vs. EACEI survey

(a) By activity

Activity	Description	Share in EUTL	Share in EACEI
20	Combustion of fuels	50.1%	43.6%
21	Refining of mineral oil	2.7%	1.4%
24	Production of pig iron or steel	3.3%	3.1%
25	Production/processing of ferrous metals	0.8%	1.0%
29	Production of cement clinker	6.4%	7.5%
30	Production of lime/dolomite calcination	3.1%	3.4%
31	Manufacture of glass	8.1%	10.6%
32	Manufacture of ceramics	7.9%	8.7%
35	Production of pulp	14.1%	16.5%
36	Production of paper/cardboard	1.7%	1.9%
37	Production of carbon black	1.0%	1.3%
42	Production of bulk chemicals	0.8%	1.0%

(b) By sector

Sector	Description	Share in EUTL	Share in EACEI
1	Agriculture and Mining	0.6%	0.3%
2	Food, Beverage and Tobacco	19.1%	15.6%
3	Textiles, Wood and Paper	18.4%	21.2%
4	Coke, Petrol and Plastics	5.2%	4.7%
5	Chemicals and Pharma	38.3%	44.9%
6	Metals	5.2%	5.0%
7	Electronics	0.6%	0.6%
8	Machinery and Transport	4.4%	3.8%
10	Construction	0.2%	0.1%
11	Water and Waste	0.6%	0.4%
12	Energy Supply	0.2%	0.1%
13	Trade, Transport and Storage	4.3%	1.9%
14	Services	2.9%	1.3%

Notes: Exit rates and emissions in 2008 by activities and sectors. Exit rate is the ratio of number of 2008 active plants over exiters as of Phase III. Share of 2008 emissions (exiters) is the ratio of emissions produced in 2008 by exiters, over total emissions of plants in that activity/sector. Overall plant exit rate for the sample is 15%, corresponding to 9% of 2008 emissions. Panel (a) shows plant-level activities as defined in Table C.1 of Abrell (2021) Panel (b) shows aggregated sectors generated from NACE2 codes: 1=Agriculture and mining (1–6), 2=Food, beverages, and tobacco (10–12), 3=Textiles, wood, and paper (13–17), 4=Coke, petroleum, and plastic (19,22), 5=Chemicals, pharma, and non-metallic minerals (20,21,23), 6=Metals and metal products (24,25), 7=Electronics and electrical equipment (26,27), 8=Machinery and transport equipment (28–30), 9=Other manufacturing (31–33), 10=Electricity generation (35), 11=Water and waste (37–38), 12=Construction (41–43), 13=Trade, transportation, and storage (46–52), 14=Services (61+). Sectors 9, 10, and 12 are excluded due to insufficient observations or electricity generation.

Table C.3: Balance test of control vs treated group, Pre-2011

	Mean		**Differences**	
	Control	Treated	Pre	Post
Free permits (p)	191.67	154.37	37.30	81.83***
Emissions (p)	194.58	143.39	51.19	75.43***
Banked permits (p)	214.55	165.34	49.21	11.36
Permit drop II-III	0.00	-0.45	0.45***	0.45***
Production sold (f)	500.74	600.30	-99.56**	125.91
Fixed assets (f)	507.60	809.19	-301.59***	-326.31***
Nr. plants (f)	7.60	11.16	-3.56***	-2.63***
Employment (f)	486.07	524.78	-38.71	12.20
Employment (p)	541.90	501.24	40.66	40.66
Clean energy C (p)	135.45	111.11	24.34*	43.13***
Dirty energy C (p)	163.84	148.93	14.91	30.60*
Total energy C (p)	299.29	260.04	39.25	73.73***
Energy intensity (p)	1215.19	1174.98	40.22	216.88**
Dirty intensity (p)	779.03	771.87	7.16	124.38
Emission intensity (p)	838.36	850.38	-12.03	255.83*
Observations	1530	1572	3102	5170

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: 2011 is kept as reference year due to possible policy anticipation at the 2011 announcement. Allocated permits, verified emissions, surrendered permits and actual net banking are expressed in thousands of EU carbon permits. Fixed assets and production sold are expressed in thousands of Euros. Energy consumption variables are expressed in thousands, where "clean" is composed of the sum of electricity and steam, while "dirty" is composed of coal, oil and natural gas. Energy intensity is measured as energy consumption over output sold. A t-test measuring the difference between the control and treated groups is presented in the last column.

Table C.4: Probit model on exiting plants, pre-2011 values

	(1)	(2)
	P[exit]	P[exit]
Employment (p)	0.039*	0.011
	(0.021)	(0.026)
Energy consumption (p)	-0.082***	-0.083***
	(0.023)	(0.021)
Dirty energy share (p)	0.010	0.026*
	(0.014)	(0.016)
Output (f)	0.011	0.002
	(0.030)	(0.032)
Fixed assets (f)	-0.005	0.015
	(0.025)	(0.029)
Activity FE		X
Observations	1,425	1,336
Clusters (plants)	272	257
Wald chi2(5)	13.77	25.61
Prob ¿ chi2	0.0172	0.0074
Pseudo R^2	0.0772	0.1440

Standard errors clustered at plant level in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Variables are expressed in logs based on their pre-2011 values. Coefficients are average marginal effects on the probability of exit. Standard errors clustered at plant level in parentheses. Conditional on activity, plants with larger energy consumption are less likely to exit, while a higher dirty energy share is positively (though less precisely) associated with exit.

Table C.5: Plant-level outcomes for exiters, post-2011

Panel (a): All exits

	(1) Firm output	(2) Plant employment	(3) Energy consumption	(4) Dirty energy	(5) Clean energy
post × exit	-0.493***	-0.453***	-0.290**	-0.211	-0.277**
	(0.152)	(0.092)	(0.132)	(0.177)	(0.117)
Observations	7,261	6,854	5,016	4,697	5,014
R-sq	0.849	0.922	0.891	0.792	0.936
Log-likelihood	-7,618.520	-3,102.769	-2,449.196	-5,026.837	-1,742.416

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel (b): Operational exits

	(1) Firm output	(2) Plant employment	(3) Energy consumption	(4) Dirty energy	(5) Clean energy
post × exit	-0.515**	-0.588***	-0.368**	-0.151	-0.428***
	(0.218)	(0.133)	(0.145)	(0.252)	(0.115)
Observations	7,261	6,854	5,016	4,697	5,014
R-sq	0.848	0.923	0.891	0.792	0.936
Log-likelihood	-7,634.697	-3,095.538	-2,446.383	-5,029.049	-1,722.932

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel (c): Operational exits, excluding activity 20

	(1) Firm output	(2) Plant employment	(3) Energy consumption	(4) Dirty energy	(5) Clean energy
post × exit	-1.049***	-0.985***	-0.452**	-0.254*	-0.489***
	(0.318)	(0.198)	(0.181)	(0.135)	(0.145)
Observations	3,766	3,595	3,098	2,886	3,096
R-sq	0.810	0.903	0.885	0.796	0.946
Log-likelihood	-4,203.662	-1,321.038	-1,434.402	-2,915.620	-834.956

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Coefficients report the post–2011 effect of exiting plants relative to survivors from a difference-in-differences specification of the form $y_{it} = \alpha_i + \gamma_t + \beta\,(Exit_i \times Post2011_t) + \varepsilon_{it}$, where outcomes y_{it} are in logs. Outcomes include firm output, plant employment, total energy consumption, dirty energy consumption, and clean energy consumption (electricity and steam). All regressions absorb plant and year fixed effects and cluster standard errors at the plant level. Panel (a) uses the full sample of exits, while Panel (b) excludes exits in Activity 20 (combustion of fuels). Reported estimates are the coefficients on $Exit \times Post2011$, interpreted as the average percentage change in outcomes for exiters after 2011 relative to survivors. Both panels confirm that exiters, as identified through zero emissions in the EUTL registry, do represent at least partially plant closures, especially when excluding combustion of fuels plants.

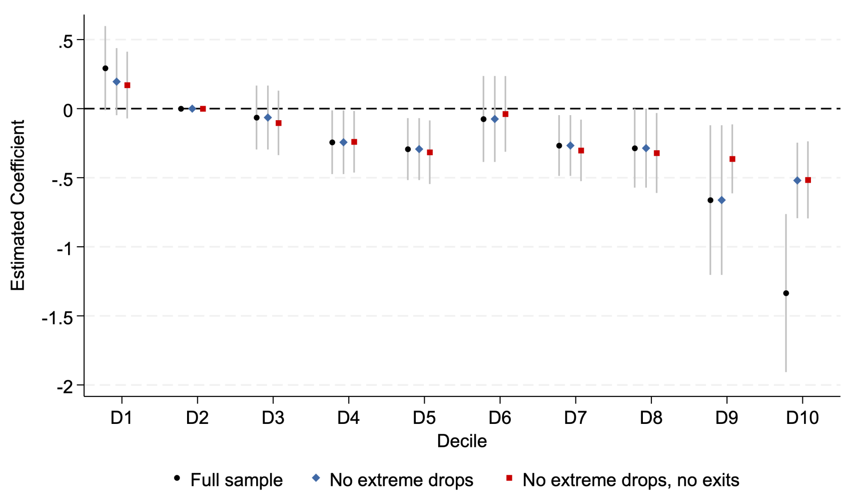
Appendix C.2. Plant emissions graphs and tables

Table C.6: Treatment effect of the continuous variable $drop_{i,2013}$ plant-level emissions

	(1) Log Emissions	(2) Log Emissions
post x drop10	0.009***	
	(0.001)	
post x drop10<0		-0.071***
		(0.016)
post x drop10>0		0.001***
		(0.000)
Plant FE	X	X
Activity x Year FE	X	X
Observations	7923	7923
R-squared	.9208634	.9229742

Notes: To simplify interpretation, variable $drop10 = -10 * drop_{i,2013}$ and is interpreted as a 10 p.p. tightening of the permit change. In column (1) I impose a single, symmetric slope using $drop10 \equiv -10 \times drop_{i,2013}$ (so higher values mean tighter cuts). In column (2) I allow different slopes for cuts vs. increases via $drop\_neg10 \equiv 10\max(-drop_{i,2013}, 0)$ and $drop\_pos10 \equiv 10\max(drop_{i,2013}, 0)$; each coefficient is the effect per 10 p.p. change on that side of zero. All OLS specifications include plant fixed effects and activity-by-year fixed effects; standard errors are clustered at the plant level. Column (2) documents strong asymmetry: a 10 p.p. cut in permits reduces emissions by about 0.071 log points ($\approx$ 6.9%), whereas a 10 p.p. increase raises emissions by only 0.0007 log points ($\approx$ 0.07%). An equality test of the two post-2011 slopes rejects symmetry ($F(1, 516) = 20.38$, $p < 0.001$). The single-slope estimate in column (1) (0.009 per 10 p.p.) masks this asymmetry and should be interpreted as a constrained average.

Figure Appendix C.6: Asymmetric effect of the continuous variable $drop_{i,2013}$ on plant-level emissions (levels), by decile



Notes: To simplify interpretation, variable $drop10 = -10 * drop_{i,2013}$ and is interpreted as a 10 p.p. tightening of the permit drop. I estimate post-2011 semi-elasticities with a PPML model in levels of emissions with plant and year fixed effects; the log link makes coefficients interpretable as log-point changes and lets me keep zeros while being robust to heteroskedasticity. Plants are binned into deciles of $drop10$ and I plot the post-2011 effect for each decile using D2 as the omitted (baseline) bin because it straddles "no change" in allocations (i.e., contains values closest to zero on both sides). Black markers use the full sample; blue trim the top/bottom 5% of $drop_{i,2013}$; red additionally exclude exiters. Effects are small around the baseline deciles and become increasingly negative in higher (tighter-cut) deciles, with the largest reductions concentrated in D9–D10; this pattern is robust to trimming and to excluding exiters, consistent with a dose–response relationship. A quadratic check of non-linearity using post–2011$\times drop_{i,2013}$ and post–2011$\times drop^2_{i,2013}$ finds the squared term economically tiny and statistically insignificant with rich fixed effects (-0.00016, s.e. 0.00018; $F(1, 516) = 0.78$, $p = 0.377$), so a linear specification in $drop_{i,2013}$ suffices for inference.

Figure Appendix C.7: Event study DiD of $highdrop_i$ on plant-level emissions, robustness on the active sample

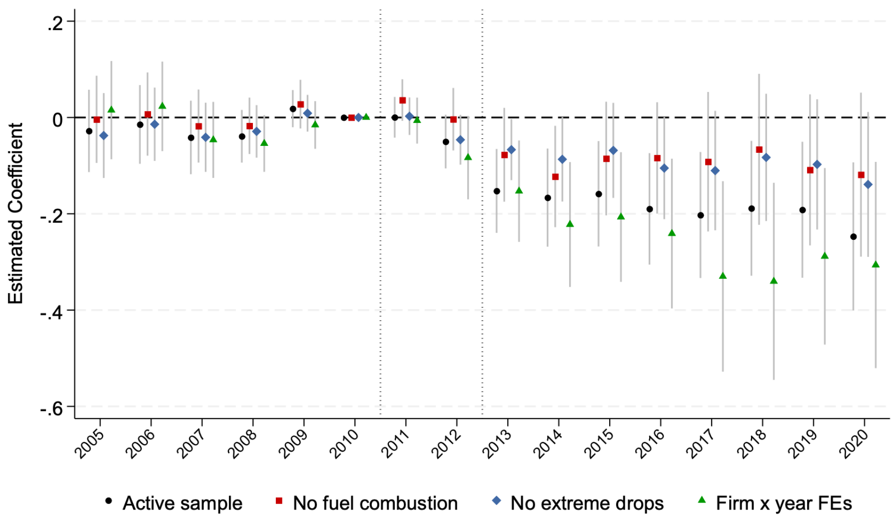
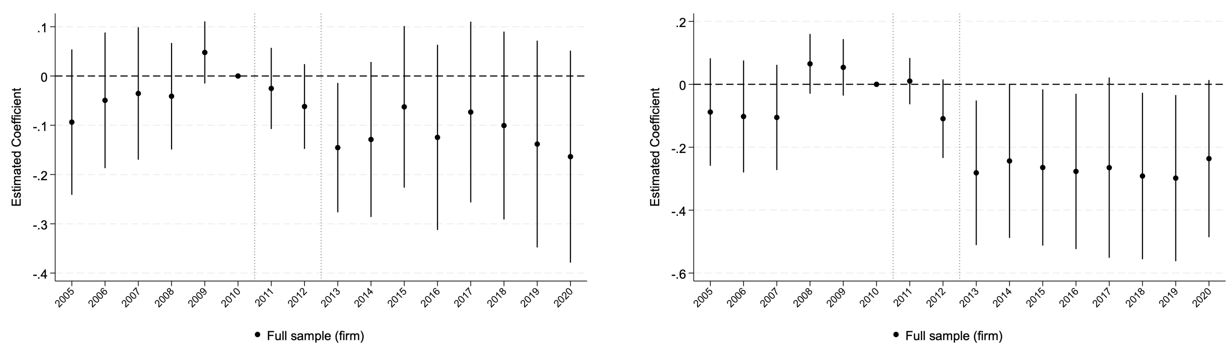


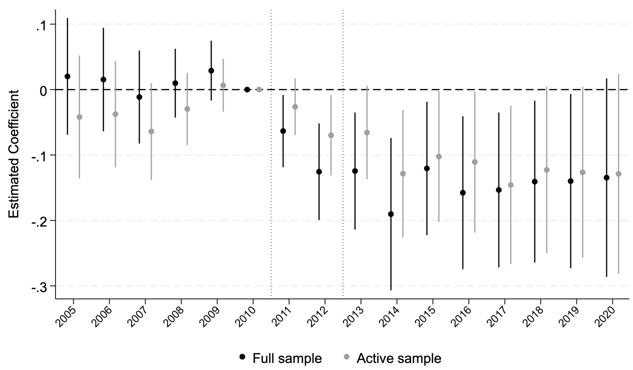
Figure Appendix C.8: Event study DiD of $highdrop_i$ on firm-level emissions, Full sample



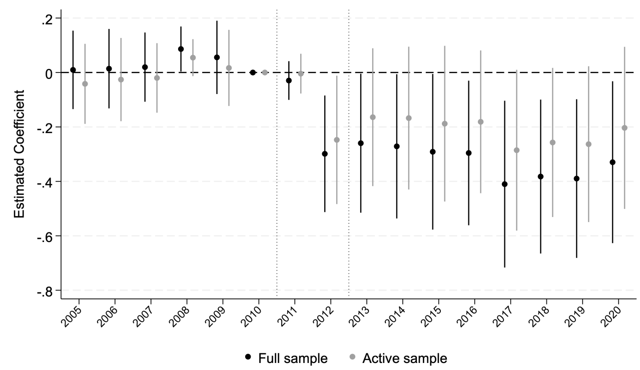
(a) Log emissions (OLS)

(b) Level emissions (Poisson)

Figure Appendix C.9: Event study of $highdrop_i$ on plant-level emissions ($drop_{i,2013} \leq 0$), Full vs Active sample



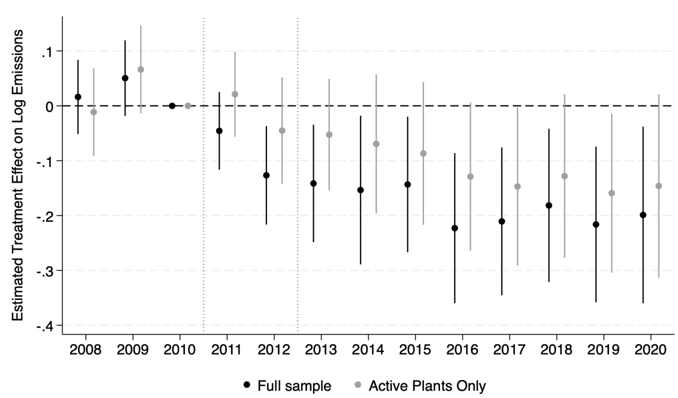
(a) Log emissions (OLS)



(b) Level emissions (Poisson)

Notes: The two vertical dashed lines mark the year before the policy announcement (2011) and the year before implementation (2013). Panel (a) reports log–emission regressions (OLS with plant and year fixed effects), implying an average post-2011 treatment effect of -0.144 ($e^{-0.144} - 1 \approx -13.4\%$) for the full sample and -0.074 ($\approx -7.1\%$; not significant) when restricting to surviving plants. Panel (b) reports Poisson regressions in levels, yielding -0.318 ($e^{-0.318} - 1 \approx -27.2\%$) for the full sample and -0.189 ($\approx -17.2\%$; not significant) for the active sample.

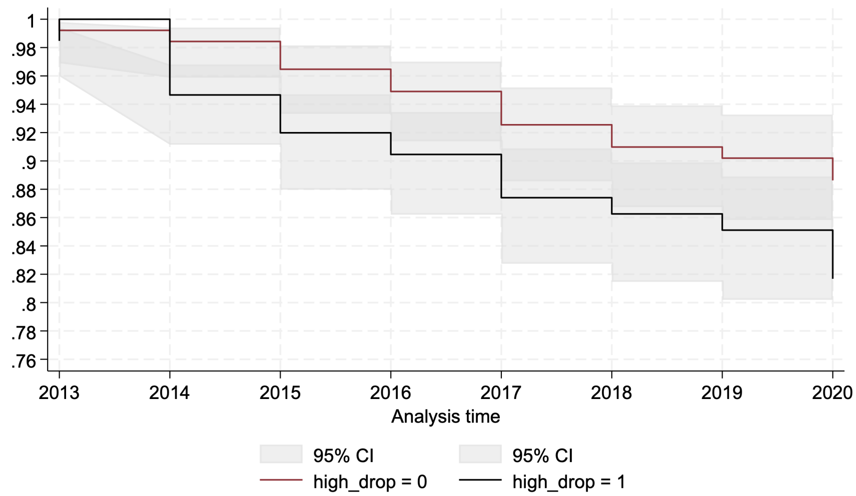
Figure Appendix C.10: Treatment effect of $highdrop_i$ on plant emissions, treatment defined at sector median drops



Notes: Standard errors clustered at the installation level; 95% CIs shown. Treatment assignment ($highdrop_i$) is here defined based on sector median drops and not on activity median drops. Event-study coefficients from plant-level DiD regressions are estimated separately for the full sample (black) and active plants only (gray) support the findings of the rest of the main analysis using activity median drops, i.e. permit drop affects plant emissions mainly through a composition channel.

Appendix C.3. Plant exit graphs and tables

Figure Appendix C.11: Survival Keplen-Merien estimates, by treatment group



Notes: The figure shows Kaplan–Meier survival curves by treatment status. Although the 95% confidence intervals overlap, the curves indicate a consistently higher exit risk for high-drop plants. Formal inference from the stratified Cox proportional hazards model confirms that this difference is statistically significant.

Table C.7: Survival analysis on $highdrop_i$ and $highbank_i$, all exits

	(1) Exit hazard Full	(2) Exit hazard Full	(3) Exit hazard Full	(4) Exit hazard Full	(5) Exit hazard Survey	(6) Exit hazard Survey
highdrop$_i$	1.699**		1.724**	1.714*	1.825*	1.819*
	(0.392)		(0.401)	(0.560)	(0.619)	(0.629)
highbank$_i$		0.963	0.896	0.890	0.794	0.724
		(0.216)	(0.202)	(0.329)	(0.260)	(0.261)
highdrop$_i$× highbank$_i$				1.012		
				(0.472)		
Strata	A	A	A	A	A	A
Plant controls						X
Observations	8005	8005	8005	8005	4361	4361
LR Chi-sq	5.281	0.028	5.519	5.508	3.907	9.702
Log-likelihood	-370.985	-373.589	-370.873	-370.872	-136.504	-127.980

Exponentiated coefficients; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports Cox proportional hazard estimates of plant exit from the ETS scheme on indicators for strong allocation cuts ($highdrop_i$) and high permit banking ($highbank_i$). Hazard ratios (exponentiated coefficients), with robust standard errors in parentheses and stratification at the activity level, are compared to baseline hazard levels equal to 1 (i.e. for $highdrop_i = 0$ and $highbank_i = 0$). Columns (1)–(4) use the full sample; Columns (5)–(6) restrict the analysis to the EACEI energy survey sample. Across specifications, $highdrop_i$ is associated with a higher hazard of exit of at least 60% compared to its baseline. The hazard ratio for $highbank_i$ alone is never significantly associated with a lower hazard of exit compared to its baseline of low banking plants within the same activity, implying no inherent difference in exit propensity absent treatment. The hazard rate in column (4) for $highdrop_i$ does not substantially change in magnitude, signaling that different banking levels do not change the impact of large allocation cuts. The interaction term is not statistically significant, confirming that high permit banking does not systematically buffer the impact of large allocation cuts. Results from the EACEI energy survey subsample in Columns (5)–(6) are qualitatively consistent with the main sample, though the estimates are less precise due to smaller sample size. Most importantly, the hazard rate estimate in column (5) is robust to the inclusion of additional baseline controls measured as of 2010 (i.e. firm output sold, plant employment, plant energy consumption, plant dirty energy share) in column (6).

Table C.8: Survival analysis on $highdrop_i$ and $multiplant_i$, all exits

	(1) Exit hazard Full	(2) Exit hazard Full	(3) Exit hazard Full	(4) Exit hazard Full	(5) Exit hazard Survey	(6) Exit hazard Survey
highdrop_i	1.699**		1.663**	2.259**	1.611	1.553
	(0.392)		(0.393)	(0.774)	(0.565)	(0.546)
multiplant_i		0.855	0.862	1.267	0.981	1.270
		(0.216)	(0.216)	(0.503)	(0.415)	(0.600)
$\text{highdrop}_i \times$				0.536		
multiplant_i				(0.260)		
Strata	A	A	A	A	A	A
Plant controls						X
Observations	8005	6829	6829	6829	3711	3711
LR Chi-sq	5.281	0.383	4.737	6.830	1.923	7.413
Log-likelihood	-370.985	-336.648	-334.380	-333.566	-119.566	-112.715

Exponentiated coefficients; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports Cox proportional hazard estimates of plant exit from the ETS scheme on indicators for strong allocation cuts ($highdrop_i$) and high permit banking ($multiplant_i$). Hazard ratios (exponentiated coefficients), with robust standard errors in parentheses and stratification at the activity level, are compared to baseline hazard levels equal to 1 (i.e. for $highdrop_i = 0$ and $multiplant_i = 0$). Columns (1)–(4) use the full sample; Columns (5)–(6) restrict the analysis to the EACEI energy survey sample. Across specifications, $highdrop_i$ is associated with a higher hazard of exit of at least 60% compared to its baseline. The hazard ratio for $multiplant_i$ alone is never significantly associated with a lower hazard of exit compared to its baseline of single-plant firms, implying no inherent difference in exit propensity absent treatment. The hazard ratio increases notably in Column (4), reaching over 2.2 for single-plant firms, suggesting that the effect of allocation cuts on exit is concentrated among firms with fewer margins for internal adjustment. The interaction term is not statistically significant, confirming that multi-plant firms are less responsive to allocation cuts, possibly due to reallocation of production across plants. Results from the EACEI energy survey subsample in Columns (5)–(6) are qualitatively consistent with the main sample, though the estimates are less precise due to smaller sample size. Most importantly, the hazard rate estimate in column (5) is robust to the inclusion of additional baseline controls measured as of 2010 (i.e. firm output sold, plant employment, plant energy consumption, plant dirty energy share) in column (6).

Table C.9: Survival analysis on $highdrop_i$ and $highbank_i$, operational exits

	(1) Exit hazard Full	(2) Exit hazard Full	(3) Exit hazard Full	(4) Exit hazard Full	(5) Exit hazard Survey	(6) Exit hazard Survey
highdrop_i	2.174**		2.262**	2.448**	2.338**	2.323**
	(0.690)		(0.722)	(1.029)	(0.970)	(0.976)
highbank_i		0.777	0.709	0.801	0.614	0.497
		(0.216)	(0.202)	(0.432)	(0.244)	(0.233)
$\text{highdrop}_i \times$				0.837		
highbank_i				(0.539)		
Strata	A	A	A	A	A	A
Plant controls						X
Observations	8114	8114	8114	8114	4394	4394
LR Chi-sq	5.975	0.710	7.170	7.772	6.344	9.938
Log-likelihood	-201.303	-204.046	-200.673	-200.637	-95.443	-87.950

Exponentiated coefficients; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports Cox proportional hazard estimates of plant operational exits on indicators for strong allocation cuts ($highdrop_i$) and high permit banking ($highbank_i$). Hazard ratios (exponentiated coefficients), with robust standard errors in parentheses and stratification at the activity level, are compared to baseline hazard levels equal to 1 (i.e. for $highdrop_i = 0$ and $highbank_i = 0$). Columns (1)–(4) use the full sample; Columns (5)–(6) restrict the analysis to the EACEI energy survey sample. Across specifications, $highdrop_i$ is associated with a higher hazard of operational exit of at least 100% compared to its baseline. The hazard ratio for $highbank_i$ alone is never significantly associated with a lower hazard of exit compared to its baseline of low banking plants within the same activity, implying no inherent difference in exit propensity absent treatment. The hazard rate in column (4) for $highdrop_i$ does not substantially change in magnitude, signaling that different banking levels do not change the impact of large allocation cuts. The interaction term is not statistically significant, confirming that high permit banking does not systematically buffer the impact of large allocation cuts. Results from the EACEI energy survey subsample in Columns (5)–(6) are qualitatively consistent with the main sample, though the estimates are less precise due to smaller sample size. Most importantly, the hazard rate estimate in column (5) is robust to the inclusion of additional baseline controls measured as of 2010 (i.e. firm output sold, plant employment, plant energy consumption, plant dirty energy share) in column (6).

Table C.10: Survival analysis on $highdrop_i$ and $multiplant_i$, operational exits

	(1) Exit hazard Full	(2) Exit hazard Full	(3) Exit hazard Full	(4) Exit hazard Full	(5) Exit hazard Survey	(6) Exit hazard Survey
highdrop$_i$	2.174**		1.993**	1.509	2.060*	1.943
	(0.690)		(0.644)	(0.609)	(0.879)	(0.839)
multiplant$_i$		0.588	0.602	0.358	0.984	1.421
		(0.204)	(0.205)	(0.225)	(0.462)	(0.754)
highdrop$_i$×				2.135		
multiplant$_i$				(1.516)		
Strata	A	A	A	A	A	A
Plant controls						X
Observations	8114	6936	6936	6936	3744	3744
LR Chi-sq	5.975	2.339	7.881	6.391	2.868	4.840
Log-likelihood	-201.303	-182.339	-130.033	-179.438	-84.641	-79.218

Exponentiated coefficients; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports Cox proportional hazard estimates of plant operational exits on indicators for strong allocation cuts ($highdrop_i$) and high permit banking ($multiplant_i$). Hazard ratios (exponentiated coefficients), with robust standard errors in parentheses and stratification at the activity level, are compared to baseline hazard levels equal to 1 (i.e. for $highdrop_i = 0$ and $multiplant_i = 0$). Columns (1)–(4) use the full sample; Columns (5)–(6) restrict the analysis to the EACEI energy survey sample. Across specifications, $highdrop_i$ is associated with a higher hazard of exit of at least 60% compared to its baseline. The hazard ratio for $multiplant_i$ alone is never significantly associated with a lower hazard of exit compared to its baseline of single-plant firms, implying no inherent difference in exit propensity absent treatment. The hazard ratio increases notably in Column (4), reaching over 2.2 for single-plant firms, suggesting that the effect of allocation cuts on exit is concentrated among firms with fewer margins for internal adjustment. The interaction term is not statistically significant, confirming that multi-plant firms are less responsive to allocation cuts, possibly due to reallocation of production across plants. Results from the EACEI energy survey subsample in Columns (5)–(6) are qualitatively consistent with the main sample, though the estimates are less precise due to smaller sample size. Most importantly, the hazard rate estimate in column (5) is robust to the inclusion of additional baseline controls measured as of 2010 (i.e. firm output sold, plant employment, plant energy consumption, plant dirty energy share) in column (6).