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Carbon pricing, compensation, and competitiveness: Lessons from UK manufacturing☆

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ABSTRACT

Carbon pricing is often paired with compensation to carbon-intensive firms to mitigate the risk of carbon leakage. This paper empirically examines the effects of indirect carbon cost compensation on UK manufacturing firms. Using administrative microdata, we combine difference-in-differences and fuzzy regression discontinuity designs to exploit firm-level eligibility criteria and identify the causal impact of compensation. We find that compensation reduces output contraction but also increases electricity consumption and emissions. These findings highlight a key policy trade-off – while compensation can help protect firms' competitiveness and reduce leakage risks, it may also delay industrial decarbonization and increase the overall cost of achieving national emission targets.

1. Introduction

Policies to establish a carbon price have proliferated in recent years, with 75 such initiatives now collectively covering 24 % of global emissions (The World Bank, 2024). While carbon pricing is widely recognized as an essential component of cost-effective decarbonization, there is long-standing concern that its effectiveness is being undermined by concessions granted to industry

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(Fischer and Fox, 2007; Sterner and Muller, 2008; Rosendahl, 2008). To address competitiveness concerns and reduce the risk of carbon leakage, many jurisdictions provide cost containment measures for energy-intensive and trade-exposed industries. Examples include energy tax exemptions (Gerster and Lamp, 2023), free allocation of allowances under the EU Emissions Trading System (EU ETS), and compensation for indirect carbon costs embodied in electricity prices. These measures increase the political acceptability of carbon pricing and are often justified as necessary to alleviate the risk of carbon leakage (Sato et al., 2022).[1] However, they also impact carbon price incentives, raising questions about their overall impact on decarbonization.

By shielding firms from the full carbon cost, these cost containment measures can weaken incentives for firms to reduce energy use and emissions. Prior research has shown that linking free allocation to current production volumes can induce firms to increase emissions in the present to secure future allocations (Rosendahl, 2008),[2] challenging earlier claims that market outcomes and efficiency are independent of how allowances are allocated (Montgomery, 1972).[3] More broadly, when compensation is tied to current production volumes, it functions as an implicit production subsidy. This weakens the carbon price signal, limits carbon cost pass-through to consumers, and reduces demand-side substitution – ultimately increasing the overall cost of achieving emissions reduction targets (Fischer and Fox, 2007; Fowlie et al., 2016; Meng, 2017). Although these perverse production incentives have been widely discussed in the literature (Fischer, 2001; Demailly and Quirion, 2008; Böhringer et al., 2012; Fischer and Fox, 2011), they have received relatively little attention in policy debates - in part due to a lack of rigorous empirical evidence on their effects.

This paper addresses the empirical gap by providing causal evidence on the impact of compensation schemes. We examine the effects of the UK's indirect carbon cost compensation program – introduced in 2013 – on manufacturing firms' energy consumption, emissions, and production outcomes. Since 2013, European countries have been permitted to compensate electro-intensive firms for higher electricity prices caused by carbon pricing in the power sector, and several countries have adopted compensation schemes (European Commission, 2020b). Compensation for indirect carbon costs is expected to continue across Europe, with Germany, France, and Poland, committing to total payments of €27.5 billion, €13.5 billion and €10 billion, respectively, between 2021 and 2030 (European Commission DG Competition, 2022). Yet despite the scale of these subsidies and their potential distortionary effects, there is surprisingly little empirical evidence on their impacts.[4]

In the UK, the combination of the EU ETS and the unilateral Carbon Price Floor, introduced in 2013, more than tripled the carbon cost of power sector emissions, making the compensation for indirect carbon costs relatively generous.[5] The magnitude of the UK compensation payments is economically important, accounting for 6–20 % of electricity prices between 2013 and 2019 (see Fig. 2). This makes the UK a valuable case study for understanding the trade-offs between shielding firms from carbon costs and maintaining strong decarbonization incentives during a period of low EU ETS prices.

We employ two quasi-experimental research designs: a difference-in-difference (DiD) design with inverse propensity score weighting and a "fuzzy" regression discontinuity (RD) design with three-way fixed effects. These methods complement each other by addressing different types of selection biases and yielding different types of treatment estimates. Both approaches exploit variation generated by the UK's eligibility rules for compensation to identify causal effects. To qualify for the program, a firm must meet three criteria. First, it must operate in a 4-digit NACE industry designated as eligible for compensation. Second, the *firm's* electricity costs must account for at least 5 % of gross value added (GVA), based on historical values. Third, the firm must formally apply for compensation and provide documentation confirming that it satisfies the first two criteria. These latter two requirements imply that both compensated and uncompensated firms may operate plants within the same narrowly defined industries. This within-industry variation allows us to identify how plants respond to higher indirect carbon costs – with and without compensation in place.

To examine how plants respond to indirect carbon cost compensation, we combine confidential microdata from the UK Secure Data Lab on economic variables and energy use at the plant level with a publicly available list of firms that received compensation. While eligibility for compensation is determined at the firm level, the amount of compensation paid is calculated at the plant level and is linked to the plant's output. Compared to firm-level analysis, more disaggregated plant-level data are advantageous because firms may operate multiple plants across different sectors. By comparing similar plants belonging to compensated and uncompensated firms, we isolate the effects of compensation for indirect carbon costs. This allows for a more precise identification of causal effects than previous studies relying on cross-sectoral or cross-country variation (Ferrara and Giua, 2022).

As a first step, we develop a static conceptual framework to examine how compensation payments influence firms' responses to indirect carbon costs. Under the implemented policy design, compensation is based on historical output and an electricity intensity benchmark. However, if a plant significantly extends (reduces) its production, the baseline output can be increased (reduced) to reflect the change in capacity or production levels. Our framework demonstrates that, analogous to output-based allocation with

[1] Carbon leakage refers to the policy-induced relocation of production (and associated emissions) to countries with less stringent carbon pricing.

[2] This approach, known as "output-based" allocation, contrasts with historical allocation (also referred to as "grandfathering" or "ex-ante" allocation), where allocation is based on past output or emissions. While output-based allocation in theory involves continuously updating allocation, in practice, compensation is typically based on historical data and adjusted only for significant changes in production levels, as is the case for the UK compensation studied here (cf. Section 3.1).

[3] Free allocation does not alter the emissions cap and therefore does not affect the aggregate effectiveness of a carbon market. However, it is associated with efficiency losses.

[4] In 2020, at least 14 EU countries – including the UK, Germany, Belgium, the Netherlands, Greece, Lithuania, Slovakia, France, Finland, Luxembourg, Poland, Romania, Spain, and Norway – provided monetary compensation for indirect carbon costs to electro-intensive firms. Total payments amounted to €694 million in 2017 alone (European Commission, 2018).

[5] The Carbon Price Floor was introduced to accelerate power sector decarbonization by ensuring a minimum carbon price. When the EU ETS allowance price fell below the target, the UK government imposed a tax to bridge the gap. From 2016, the tax was frozen at £18/tCO_2, effectively functioning as a carbon tax. See Section 3.1 for details.

benchmarking in emissions trading systems, compensation for indirect carbon costs embodied in electricity prices weakens incentives to reduce output, while leaving incentives to improve electricity intensity unchanged. As a result, overall electricity use is expected to increase among compensated firms relative to their uncompensated counterparts.

Our empirical analysis delivers three key results. First, consistent with our theoretical predictions, we find that compensated plants experienced smaller output contractions compared to uncompensated plants. The DiD estimates indicate that compensation increased output – proxied by sales of own goods – by around 16 % during the post-treatment period (2013–2015). This result is corroborated by the fuzzy RD design, which estimates a 30 % increase in output for compensated plants, with a lower bound estimate of 26 % based on reduced form results. Second, we find that compensation led to an increase in electricity consumption, measured in physical units, by around 22 %. Indirect carbon emissions increased by a similar magnitude – around 22 % – among compensated plants vis-á-vis their uncompensated counterparts. Third, we find no statistically significant effect on energy intensity in either the DiD or RD models, suggesting that incentives to improve energy intensity were not materially altered. However, due to limited statistical power, we cannot rule out the possibility of small effects on energy intensity. Taken together, our findings provide robust evidence that carbon cost compensation - by shielding firms from the full carbon price internalization - dampens incentives to reduce output and, consequently, energy use and emissions. This underscores a fundamental trade-off in carbon pricing design: while compensation may alleviate competitiveness concerns, it comes at the cost of weakening incentives for industrial decarbonization.

Our findings have several important policy implications for carbon pricing in the UK and other jurisdictions where concerns about carbon leakage remain central to the policy debate. Free allocation, compensation, and tax exemptions remain commonplace across carbon pricing schemes (European Commission, 2020a; The World Bank, 2023), despite limited empirical evidence supporting significant carbon leakage – particularly in settings with relatively low carbon prices and generous free allocation (e.g. Naegele and Zaklan, 2019; Verde, 2020; Colmer et al., 2024). Drastically cutting back these concessions may prove difficult in an increasingly fragmented political landscape where global convergence in carbon pricing is unlikely (Neuhoff et al., 2025). Addressing the downsides of compensation is therefore critical to enabling a successful industrial transition to net zero. Our analysis provides robust evidence that compensation discourages firms from reducing output and, consequently, emissions. This underscores the need for stronger complementary incentives and support to enable rapid industrial decarbonization. Moreover, compensation schemes are fiscally costly, often absorbing a significant share of ETS auction revenues – funds that could otherwise be used to finance clean industrial upgrades. This suggests the need for alternative financing mechanisms to deliver rapid industrial transitions to carbon neutrality.

Our paper contributes to the broader literature on the incentive effects of industry compensation in climate policy. Free allocation under emissions trading systems is a widely used mechanism to compensate industry, and the distortions that can arise from specific allocation designs have been extensively studied (e.g. Martin et al., 2014; Rosendahl and Storrøsten, 2015; Fowlie and Reguant, 2022).[6] Some studies have explored other cost containment measures including refunding of emission payments (Martin et al., 2014; Hagem et al., 2020), as well as exemptions and rebates for energy taxes (Ito, 2015; Gerster and Lamp, 2023).

Particularly relevant to our study is Gerster and Lamp (2023), who examine the effects of renewable energy levy exemptions in Germany by exploiting changes in eligibility thresholds. They find that exempted plants increase electricity consumption, with no significant effects on production, export shares, or employment. Compared to their findings, our results suggest that the UK's indirect carbon cost compensation scheme may have avoided some of the unintended consequences observed under tax exemption schemes, such as weakened incentives to improve energy efficiency.[7] Finally, Ferrara and Giua (2022) also examine compensation for indirect carbon costs, similar to our study. However, their empirical strategy uses firms in other countries or sectors without compensation as controls, which may introduce bias since countries and sectors offering compensation are likely to differ systematically.[8] In contrast, our identification strategy mitigates these concerns by comparing compensated and uncompensated firms within the same country and sector, thus better accounting for common trends in technology, trade exposure, and policy context.

Our study also complements and extends the literature on the effects of carbon pricing on carbon and energy-intensive firms (Martin et al., 2014; Petrick and Wagner, 2014; Aldy and Pizer, 2015; Klemetsen et al., 2020; Marin and Vona, 2021; Dechezleprêtre et al., 2023; Colmer et al., 2024),[9] including research on the UK Carbon Price Floor (Abrell et al., 2022; Leroutier, 2022). These latter two studies examine the direct impact of the UK carbon tax on power sector emissions and decarbonization. In contrast, our research focuses on the indirect effects of carbon pricing on manufacturing firms through higher electricity prices. By analyzing how these indirect carbon costs are mediated via a compensation scheme, we provide new insights into the broader implications of such policies on firm behavior.

The remainder of the paper is structured as follows. We first lay out a simple conceptual framework to characterize the compensation scheme's impacts in Section 2. We then provide some essential policy background on the UK carbon pricing and compensation scheme, introduce the data, and present descriptive statistics in Section 3. Section 4 details our two empirical strategies. Section 5 presents our main results and compares the estimates from both strategies. Section 6 presents some back-of-the-envelope calculations to provide perspective on the trade-offs between preventing leakage and fostering abatement, before we conclude in Section 7.

[6] Sato et al. (2022) provides a summary of the recent literature.

[7] The UK scheme may also have reduced bunching behavior around eligibility thresholds by setting the baseline period sufficiently far in the past. In contrast, Germany's scheme, which used a two-year gap between the baseline year and the exemption award, exhibited clear evidence of bunching.

[8] For example, countries that provide compensation are typically wealthier, and eligible sectors are often more energy-intensive and more exposed to relocation risk, making them inherently different from non-eligible sectors.

[9] See Laing et al. (2014), Martin et al. (2016) and Dechezleprêtre et al. (2023) for EU ETS reviews and Green (2021) for a review of the empirical carbon pricing literature.

2. Conceptual framework

Here, we develop a simple framework to characterize the theoretical predictions of manufacturing plants' behavior in response to indirect carbon costs, both with and without compensation, drawing on Hagem et al. (2020) and Fowlie et al. (2016). Suppose that production causes direct carbon emissions from fossil fuel combustion, where e_i is the emission intensity (emissions per unit of output q_i) for firm i, as well as indirect carbon emissions through electricity use, where el_i denotes firm-specific electricity intensity. Each firm can reduce its overall emissions ($e_i \cdot q_i$) and electricity use ($el_i \cdot q_i$) by either reducing production (q_i) or lowering the respective intensities – such as by installing abatement equipment to reduce e_i or adopting electricity saving technology to decrease el_i.

Firms face two types of carbon costs. First, they incur a ***direct carbon cost***, which depends on output, q_i, emission intensity, e_i, and the equilibrium emission permit price, τ, representing, more generally, the monetized damages of an additional tonne of carbon emissions. Second, firms face an *indirect carbon cost* due to carbon embodied in electricity prices, which is a function of output, q_i, electricity intensity, el_i, and the electricity price, $p_{el}(\tau_{el})$. Note that the electricity price depends on the carbon tax imposed on the power sector: $p_{el}(\tau_{el})$. We assume full (100 %) pass-through of carbon taxes in the power sector, meaning that the tax is fully reflected in electricity prices.

We consider a sector composed of firms indexed by $i = 1, ..., n$, each producing quantity q_i of a homogeneous good. Firms operate in perfectly competitive global markets, where all firms are price takers.[10] We assume that marginal costs of production are positive and increasing, such that $c_i' > 0$, and $c_i'' > 0$, and we abstract from entry and exit decisions. The profit of a single plant is given by:

$$\pi_i = pq_i - c_i(q_i) - my_i - nz_i - \underbrace{\varphi_i(q_i, e_i(y_i), \tau)}_{\text{Direct carbon costs}} - \underbrace{\psi_i(q_i, el_i(z_i), p_{el}(\tau_{el}))}_{\text{Electricity costs}} \tag{1}$$

where p is the product price, $c_i(q_i)$ represents the cost of output q_i (excluding electricity use), m is the annuity price per unit of abatement equipment y_i, and n is the annuity price per unit of electricity saving equipment z_i. The parameter $\varphi_i(q_i, e_i(y_i), \tau)$ represents the direct carbon costs, while the parameter $\psi_i(q_i, el_i(z_i), p_{el}(\tau_{el}))$ represents the electricity costs. The electricity costs include an *indirect* carbon cost component, represented by τ_{el}, which is the carbon price in the electricity sector.

For direct carbon costs, we assume that permits are allocated based on an output-based allocation approach, multiplying output levels and an industry-specific emission intensity benchmark, $\bar{e_j}$. Thus, direct carbon costs (φ) incurred by the firm are given by:

$$\varphi(q_i, e_i(y_i), \tau) = q_i \cdot \tau(e_i(y_i) - \bar{e_j}). \tag{2}$$

(i) No compensation for indirect carbon costs

Under no compensation for indirect carbon costs, the cost of electricity consumption (ψ) to the firm is given by:

$$\psi(q_i, el_i(z_i), p_{el}) = q_i \cdot el_i(z_i) \cdot p_{el}(\tau_{el}), \tag{3}$$

where τ_{el} is the carbon price faced by electricity generators. Intuitively, any increase (decrease) in τ_{el} or electricity intensity el_i would translate into higher (lower) ψ.

Maximizing the profit function with respect to output q_i and electricity saving investments z_i yields the following first-order conditions:

$$\frac{p - c_i'(q_i) - \overbrace{\tau \cdot [e_i(y_i) - \bar{e_j}]}^{\Delta\text{Direct carbon costs}}}{el_i(z_i)} = p_{el}(\tau_{el}) \tag{4}$$

$$-\frac{n}{q_i \cdot el_i'(z_i)} = p_{el}(\tau_{el}) \tag{5}$$

The left-hand side of Eq. (4) expresses the marginal cost of reducing electricity use through output reductions, and the left-hand side of Eq. (5) expresses the marginal cost of reducing electricity use through technology investments.

(ii) Compensation for indirect carbon costs

Suppose that compensation is introduced to offset the indirect carbon cost component embedded in electricity prices. If compensation is based purely on historical output or emissions (known as grandfathering, ex-ante allocation, or lump-sum transfer), then compensation would not affect current production decisions, and incentives to reduce electricity use would remain the same for both compensated and uncompensated firms. Here instead, compensation is based on baseline output and industry-specific electricity intensity benchmarks (denoted by $\bar{el_j}$), subject to dynamic updating.[11] Then it follows that the cost of electricity consumption (ψ) to the firm will be:

[10] This assumption aligns with the UK Government's underlying assumption that UK firms cannot pass domestic carbon taxes onto product prices.

[11] Here, we simplify the compensation scheme by assuming that payments depend on current production. In the UK compensation scheme, a hybrid approach is used whereby compensation payments are initially based on historical (baseline) production levels, but with the possibility of dynamic updating. See Section 3.1 for further details.

$$\psi(q_i, el_i(z_i), p_{el}) = q_i \cdot \Big[\underbrace{el_i(z_i) \cdot p_{el}(\tau_{el})}_{\text{Electricity cost per tonne}} - \underbrace{\bar{el}_j \cdot \tau_{el} \cdot A_i}_{\text{Compensation per tonne}} \Big] \tag{6}$$

where $\bar{el}_j$ is the electricity intensity benchmark for industry j (tCO_2/tonne) and A is the aid intensity, which is the proportion of eligible indirect carbon costs that may be compensated by government support (state aid). With full pass-through of the power sector carbon price τ_{el} to electricity prices, $A_i = 1$, and $el_i = \bar{el}_j$, the firm's compensation per tonne equals the increased electricity cost per tonne from τ_{el}. If $el_i < \bar{el}_j$, compensation per unit output exceeds the carbon price-induced electricity cost increase.

Conditional on being compensated, maximizing the profit function with respect to output q_i and electricity saving investments z_i yields the following first-order conditions:

$$\frac{p - c_i'(q_i) - \overbrace{\tau \cdot [e_i(y_i) - \bar{e}_j]}^{\Delta\text{Direct carbon costs}}}{el_i(z_i)} = p_{el}(\tau_{el}) - \overbrace{\bar{el}_j \cdot \tau_{el} \cdot A_i}^{\text{Compensation}} \tag{7}$$

$$-\frac{n}{q_i \cdot el_i'(z_i)} = p_{el}(\tau_{el}) \tag{8}$$

As (5) = (8), the first-order condition with respect to the electricity-saving investment z_i is the same regardless of the compensation payments. Put differently, the social marginal cost of electricity reduction through technology investments is equal to the level of the electricity price for all firms. However, we see that the first-order condition with respect to output, q_i, has changed relative to no compensation: (4) ≠ (7). From (7) we see that the marginal cost of lowering electricity use through output reductions is no longer equal to the electricity price $p_{el}(\tau_{el})$, but equal to $p_{el}(\tau_{el})$ minus the compensation payments per unit of output ($\bar{el}_j \cdot \tau_{el} \cdot A_i$).

Introducing compensation payments for indirect carbon costs hence increases the cost of reducing electricity use through output reductions, as lower production results in reduced compensation payments. This marginal loss of compensation due to reduced output is given by $\bar{el}_j \cdot \tau_{el} \cdot A_i$. As a result, the firm's marginal cost of reducing output exceeds the social cost of doing so. While higher electricity prices induced by carbon pricing in the power sector increase production costs, compensation payments counteract this effect by making it less costly for firms to maintain output levels.

Since carbon leakage arises from the relocation of domestic production, compensation that mitigates domestic output decline could be interpreted as reducing carbon leakage. However, we cannot ascertain whether the compensation scheme actually prevents the reallocation of production to regions with more lenient carbon policies, as this would require direct evidence of firms' trade flows and investment location decisions.

Testable predictions of firms' production behavior

By comparing models (i) and (ii), we formalize the following hypothesis of how plants respond to an increase in the indirect carbon cost τ_{el}:

- **Prediction 1** *Compensated plants' production will decrease less vis-á-vis uncompensated plants.*
- **Prediction 2** *Compensated and uncompensated plants have the same incentives to invest in electricity-saving technology. Therefore, a similar effect of an increase in τ_{el} on the electricity intensity is expected for compensated and uncompensated plants.*
- **Prediction 3** *Based on predictions 1 and 2, we expect that compensated plants' overall electricity use will decrease less vis-á-vis uncompensated plants.*

These predictions compare the effects of indirect carbon costs on compensated versus uncompensated firms. By contrast, previous studies usually compare the effects of output-based allocation vis-á-vis other allocation methods such as auctioning or grandfathering (e.g. Rosendahl, 2008; Fowlie et al., 2016; Hagem et al., 2020).

In the following sections, we empirically test our three predictions using a difference-in-differences and regression discontinuity design applied to the UK indirect carbon cost compensation scheme. The next section describes our research designs and data used in our empirical analysis.

3. Research design and data

3.1. Policy background

The design of the carbon pricing and compensation schemes plays a central role in our empirical strategy. In this section, we outline how the relevant policies were implemented.

In 2005, an EU-wide carbon price was introduced for the manufacturing and power sectors with the launch of the EU ETS. The carbon price affects manufacturing firms through two main channels. First, regulated firms must purchase and surrender EU Allowances (EUAs) for each tonne of CO_2 emitted in the previous year (*direct* ETS costs). Second, firms also pay for the carbon price reflected in higher electricity prices (*indirect* ETS costs) due to electricity producers passing forward the carbon price to consumers (Sijm et al., 2006; Fabra and Reguant, 2014; Hintermann, 2016). To mitigate the risk of carbon leakage, the ETS Directive provides free allowance allocations to certain installations to limit their exposure to direct carbon costs. Beginning in 2013, the "2012 Guidelines" also permitted EU ETS countries to grant state aid to selected electro-intensive industries to compensate for indirect carbon costs embedded in electricity prices (European Commission, 2020b).

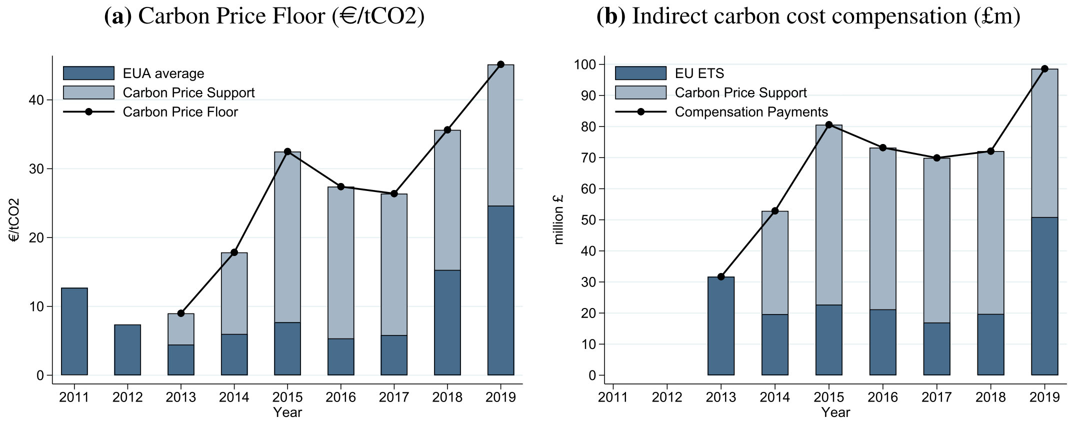

Fig. 1. Carbon prices and compensation payments in the UK. *Notes:* Panel (a) illustrates the two elements of the carbon price faced by UK power plants. For the period 2013 to 2015, the Carbon Price Support, i.e., the tax, was set to 4.94, 9.55, and 18.08 £/tCO_2. From 2016, the Carbon Price Support (tax) was frozen at £18/tCO_2. Approximate calculations using the yearly average of EUA prices in €/tCO_2 from sandbag.org.uk, the Carbon Price Support rates in £/tCO_2 from Hirst (2018), and GBP/EUR exchange rates. Panel (b) summarizes the annual compensation payments made by the UK government for EU ETS and CPS indirect carbon costs communicated directly by the Department for Business and Trade through a freedom of information request.

In the UK, alongside the EU ETS, a unilateral Carbon Price Floor was introduced on April 1, 2013, applying only to electricity generation. This policy immediately increased the effective carbon price faced by UK power producers. The original design entailed setting a desired carbon price path (floor) and specifying the Carbon Price Support (CPS) rate needed to top up the EUA price to reach that floor. From 2016 onward, however, the UK Government froze the CPS at £18/tCO_2, effectively transforming the policy into a fixed surcharge, or tax, on top of the EUA price. As shown in Panel (a) of Fig. 1, the total CO_2 price faced by UK power plants was between two and five times higher than the prevailing EUA price.

The UK's CPS was introduced with the aim of accelerating decarbonization in the power sector. It emerged in response to the general concern in the years leading up to phase III of the EU ETS that the EUA price was too low (UK BEIS, 2019); in 2012, the average allowance price was around €7/tCO_2. At the same time, however, the CPS sparked significant concerns about carbon leakage and the potential loss of competitiveness for UK electro-intensive manufacturing firms relative to competitors abroad.[12] To mitigate the potential adverse effects on domestic firms and secure political support, the CPS was accompanied by a compensation scheme designed to offset the additional electricity costs it imposed on electro-intensive firms. Although the scheme was intended to begin in 2013, it was only approved by the European Commission in March 2014, when it came into effect. The compensation scheme for costs induced by the CPS was combined with a separate scheme introduced on January 1, 2013, which aimed to partially offset the indirect carbon costs passed through electricity prices under the EU ETS.

Since 2013, carbon prices have been higher in the UK due to the CPS (see Figure A.1 in Appendix A), but so have the compensation payments. Panel (b) in Fig. 1 summarizes the annual compensation provided by the UK government for indirect carbon costs associated with both the EU ETS and CPS. Payments linked to the EU ETS have increased in recent years, reflecting the rise in EUA prices. Fig. 2 illustrates the magnitude of the compensation payments by comparing the carbon cost compensation per MWh to the average electricity price faced by manufacturing firms. Between 2013 and 2019, compensation payments accounted for between 6 % and 20 % of average electricity prices.

Two eligibility thresholds: (i) industry and (ii) indirect carbon cost share

We exploit a discontinuity in the eligibility rules for indirect carbon cost compensation to assess the impact of the compensation on firms' economic and environmental outcomes. Eligibility for compensation under both the EU ETS and the Carbon Price Support was determined by two criteria. First, firms had to manufacture a product in the UK within an eligible sector, defined by the 4-digit NACE code. The European Commission designated eligible sectors as those facing a high risk of carbon leakage.

Second, to create a more targeted compensation scheme, the UK Government introduced an additional eligibility criterion: firms had to demonstrate that their indirect carbon costs – defined as the combined costs of the EU ETS and the Carbon Price Support – amounted to at least 5 % of their gross value added (GVA). Specifically, this so-called 5 % filter test was calculated as follows:

$$\frac{\text{Electricity consumption (MWh) x Price impact (£/MWh)}}{\text{Gross Value Added (£)}} \geq 5\ \%, \tag{9}$$

[12] Even prior to the Carbon Price Floor, UK industrial sectors voiced strong concerns about electricity prices for several reasons. Over the past decade, UK manufacturing companies have paid relatively high electricity prices compared to their counterparts in neighboring countries such as France, Germany, and Italy, but the differences are mitigated by compensation for policy costs; see Figure A.1 in Appendix A. Electricity has been the main source of energy in the UK manufacturing sector as a whole since 2006 (UK BEIS, 2018).

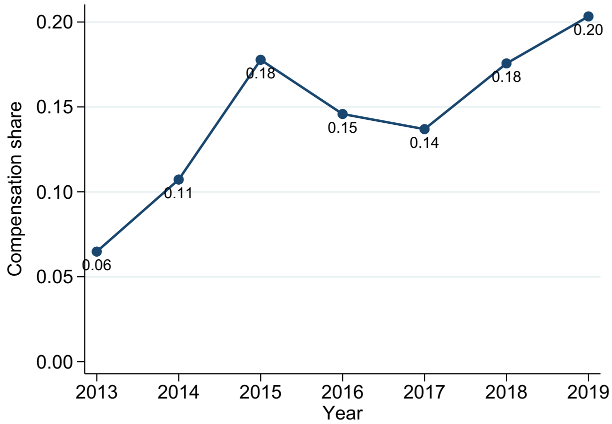

Fig. 2. Compensation payments as a share of electricity prices (2013–2019). *Notes:* Figure shows the compensation payments per MWh as a share of average electricity prices faced by UK manufacturing firms. Calculations are based on the compensation formula (Eq. 10) and average electricity prices from the Department for Energy Security and Net Zero (UK). See Appendix A for details.

where electricity consumption and GVA were averaged over the period 2005–2011 (i.e., before the 2012 announcement of the compensation scheme), and the price impact was set at £19/MWh in real 2007 prices. Because the calculation was based on historical values, firms were unable to adjust their electricity consumption or production to influence their eligibility.

Both electricity costs and GVA had to be calculated at the aggregate legal entity (firm) level. For multi-plant firms, this meant that parts of the electricity use and GVA could stem from activity unrelated to the manufacture of the eligible product(s). If these other activities were less energy-intensive, they would reduce the firm's average electricity intensity, making it more difficult to meet the second eligibility criterion.

Even if a firm met both eligibility thresholds, it was still required to submit an application in order to receive compensation. Crucially for identification, the multiple eligibility criteria imply that we might have three types of firms within a narrowly defined eligible industry: firms that passed the 5 % filter test and successfully applied for compensation; firms that *would* have passed the 5 % filter test but did not apply; and firms that did not pass the 5 % filter test. The existence of the second group may be attributed to behavioral factors, transaction costs, informational barriers, or low salience - monetary compensations may have been insufficient to incentivize firms to apply, particularly in the earlier years when compensation shares were lower, as shown in Fig. 2. The presence of these three groups makes it possible to exploit within-industry variation to estimate impacts of the compensation scheme.[13]

Compensation calculation

While eligibility is determined at the industry and *firm* level, the amount of compensation is calculated based on *plant*-level data.[14] This implies that two plants with identical electricity intensity (electricity use per unit of GVA) may differ in compensation eligibility if only one of the plants belongs to a firm that meets the eligibility criteria. Eq. (10) outlines how the compensation payments are calculated.

$$\begin{aligned}&\text{Baseline output of product X (tonne)} \times\\&\text{Electricity consumption efficiency benchmark (MWh/tonne)} \times\\&\text{Emission factor (tCO2/MWh)} \times\\&[\text{Carbon Price Support} \,(\pounds/\text{tCO2}) + \text{EUA forward price at year t} - 1\,(\pounds/\text{tCO2})] \times\\&\text{Aid intensity}\,(\text{e.g. } 80\ \%).\end{aligned} \tag{10}$$

With the exception of baseline output, all terms are exogenously set by the regulator and are fixed either across plants or within industries. The electricity consumption efficiency benchmarks are defined as the product-specific electricity consumption per tonne of output achieved using the most electricity-efficient methods of production for the product considered. The aid intensity was set by the UK Government at 85 % for 2013–2015, 80 % for 2016–2018, and 75 % for 2019 – 2020.[15]

The only term that is plant-specific is the baseline output, defined as the average annual production of the eligible product (in tonnes) at the plant during the reference period 2005–2011. However, some dynamic updating of the baseline is needed to prevent the policy from functioning as a lump-sum transfer, which could encourage industry lobbying and lead to substantial windfall profits

[13] In addition to the listed criteria, a firm was also eligible for compensation if it could document that a close competitor received compensation. A close competitor is defined as a firm producing the same product, based on the 8-digit Prodcom classification. A firm could also qualify for compensation if it demonstrated that it failed the 5 % test due to the inclusion of business activities unrelated to the manufacture of the eligible product(s).

[14] In this paper, the terms plant and installation are used interchangeably. Under the compensation scheme, an installation is defined as a stationary technical unit where one or more activities associated with the manufacture of the eligible product are carried out.

[15] EU Commission recommendations state that aid intensity should not exceed 85 % of eligible costs incurred in 2013, 2014 and 2015, and 80 % of eligible costs incurred from 2016 onwards (EU 2012/C 158/04).

for polluters (Sato et al., 2022). In practice, if a plant significantly expands its capacity for producing eligible products, its baseline output can be increased in proportion to the production extension. This ensures that the compensation scheme is neither strictly *ex-ante* nor fully output-based.[16] For a production increase to qualify as "significant", two conditions must be met. First, there needs to be a physical change related to the installation's technical configuration and functioning. Second, the installation must be capable of operating at a capacity that is at least 10 % higher compared to the initial installed capacity, resulting from a physical capital investment.

Additionally, if a plant significantly reduced its production, the compensation amount would be reduced according to a stepwise function.[17] Payments were made to firms on a quarterly basis, and firms were required to report any significant increases or reductions in production to the UK Government each quarter. While the compensation calculation is not fully output-based in the traditional sense, the presence of dynamic updating of the baseline introduces an output-sensitive component. As a result, compensation payments may influence a firm's production decisions.

3.2. Data sources

To examine the indirect effect of carbon pricing on manufacturing, we combine multiple data sources at both the firm and plant levels, drawing primarily on confidential microdata accessed through the UK Secure Data Lab. While this disaggregated data offers rich detail, it also presents analytical challenges due to the relatively small sample size, as some sources are survey-based.[18]

Compensation schemes: Annual lists of firms that received compensation for indirect carbon costs in 2016, 2017, 2018, and 2019 are publicly available on the website of the Department for Energy Security and Net Zero (DESNZ). We assume that the firms that received compensation in 2016 also received compensation in 2015, 2014, and 2013.[19] In total, 59 firms received compensation in 2016 for indirect costs induced by the EU ETS and the Carbon Price Support.

Economic data: We use plant-level data[20] on employment and economic outcomes from restricted ONS microdata. Our core dataset is the Annual Business Survey (ABS) (Office of National Statistics, 2021), covering production, construction, distribution, and service industries. The ABS, the largest ONS business survey, covers around 62,000 plants and uses a stratified random sample based on employment, geography, and 4-digit Standard Industrial Classification (SIC) codes. From the ABS, we extract data on SIC codes, employment, sales of own goods, production value, turnover, GVA, and energy expenditures for the years 2005 – 2019.[21] All monetary values are inflation-adjusted to constant 2010 prices using the UK GDP deflator.

Energy and electricity use: To assess the impacts on electricity use, we rely on the Quarterly Fuels Inquiry (QFI) (Department of Energy and Climate Change, 2016), which provides quarterly data on fuel quantities and expenditures for a sub-sample of UK manufacturing plants. Before 2008, the QFI covered around 1,200 plants, and around 600 plants thereafter. Maintained by the ONS on behalf of the DESNZ, the dataset is not available beyond 2015. We aggregate the data to the annual level and link it to the ABS.

Given the smaller sample size and temporal limitations of the QFI, we supplement our analysis with reported energy costs from the ABS as a proxy to ensure sufficient statistical power for testing our hypotheses. For evaluating the second eligibility criterion, we predict electricity use for a larger sample (Section 3.4).

Electricity-related indirect emissions: We calculate indirect emissions by combining QFI data on electricity use in physical units with emission factors from Department for Energy Security and Net Zero (UK).

3.3. Descriptive statistics

Table 1 presents summary statistics for selected variables by compensation status, based on plant-level microdata from the ABS and QFI. The sample is restricted to manufacturing industries, defined by SIC codes 7–33.

To test our first hypothesis on the effect of compensation on production, we use sales of own goods as our main outcome variable (Panel A). As robustness checks, we also consider alternative proxies for production volumes, including total output, turnover, and gross value added (GVA). Given that protecting jobs is a frequently used argument to justify compensation, we also examine the

[16] On average, this adjustment is made for one or two plants per year in the UK, according to direct communication with representatives from the former Department for Business, Energy and Industrial Strategy (BEIS).

[17] If production was reduced by less than 50 %, there would be no reduction in the compensation amount. For reductions between 50 % and 75 %, a plant would receive 50 % of the compensation amount. For reductions between 75 % and 90 %, a plant would receive 25 % of the compensation amount. If production fell by 90 % or more, a plant would not receive any compensation.

[18] The microdata used in this study are subject to strict disclosure restrictions, which prevent public release of detailed descriptive statistics and disaggregated results. These data are available through the UK Data Service under controlled access.

[19] Although information on compensation recipients before 2016 is not publicly available, conversations with representatives from the former Department for Business, Energy & Industrial Strategy (BEIS) confirmed that the 2016 list is representative of 2013–2015.

[20] A "plant" corresponds to a "reporting unit", which is the entity holding the mailing address for the business and serves as the unit of observation for surveys conducted by the UK Office for National Statistics (ONS). It represents the lowest aggregation level for which most business data are available. In our sample, approximately 16 % of compensated enterprise units are multi-plant firms. For further details see Criscuolo et al. (2003).

[21] This period includes the post-2016 EU Referendum years. Robustness tests confirm consistent results when including 2016–2019.

Table 1
Summary statistics for the period 2005–2011, by compensation status.

	Compensated	N	Other	N	Difference
Panel A: Variables from the Annual Business Survey (ABS)					
Sales of own goods	10.35	111	7.086	14,770	3.264***
	(1.506)		(2.223)		(0.211)
Total output	10.36	112	7.208	15,503	3.149***
	(1.457)		(2.182)		(0.207)
Total turnover	10.38	112	7.303	15,713	3.073***
	(1.436)		(2.167)		(0.205)
Production value	10.98	70	7.157	8,976	3.819***
	(1.219)		(2.483)		(0.297)
GVA	16.04	118	13.37	15,463	2.662***
	(1.447)		(2.074)		(0.191)
Employment	5.157	119	2.925	16,180	2.233***
	(1.161)		(1.694)		(0.156)
Productivity	5.432	118	4.345	15,708	1.087***
	(0.806)		(0.900)		(0.0831)
Energy purchases (£)	6.583	99	3.275	15,272	3.308***
	(1.791)		(2.232)		(0.225)
Energy purchases / sales	−3.124	118	−3.898	14,284	0.773***
	(0.879)		(0.893)		(0.0825)
Panel B: Variables from the Quarterly Fuels Inquiry (QFI)					
Electricity use (kWh)	17.44	33	14.99	729	2.451***
	(1.796)		(1.768)		(0.315)
Electricity use / sales	6.148	32	4.974	706	1.174***
	(1.044)		(1.213)		(0.218)
	(1.321)		(1.353)		(0.248)
Electricity emissions	24.31	24	22.30	287	2.019***
	(1.288)		(1.356)		(0.287)
Panel C: Variables that are calculated based on the ABS and QFI					
Predicted electricity use (kWh)*	15.05	93	12.01	15,248	3.037***
	(1.563)		(2.141)		(0.222)
Electricity intensity based on Eq. (9)	−3.889	116	−5.110	7130	1.220***
	(0.942)		(0.980)		(0.0917)

Notes: The table reports plant-level summary statistics for the period 2005–2011, which is the baseline period for determining eligibility under the compensation scheme. All variables are expressed in logs. The sample is restricted to manufacturing industries (SIC codes 7–33). N refers to the number of plants. Productivity is measured as turnover per employee. GVA is measured at market prices. Source: ABS and QFI. *See Section 3.4 for details. See Table B.1 for summary statistics on alternative energy intensity measures.

effects of the compensation scheme on employment, although we consider this outcome to be less tightly linked to production volumes. Comparing compensated and uncompensated plants, we see from Panel A that compensated plants are larger in terms of production, employment, and GVA. Depending on the specific variable, the sample includes between 70 and 119 compensated plants and between 8,976 and 16,180 uncompensated manufacturing plants.

To test our second hypothesis on electricity intensity impacts, we focus on electricity use in kWh (from the QFI) as a share of sales of own goods (Panel B). In the absence of electricity use data for all plants, we also use energy purchases as a share of sales (Panel A) as an alternative proxy. Additional energy intensity measures are considered in robustness checks.

To test our third hypothesis on the effects of electricity consumption, we focus on electricity use in kWh as the primary variable (Panel B), as it most directly captures the concept we aim to measure. However, due to the limited sample size in the QFI (33 compensated plants and 729 uncompensated plants), we also examine effects on energy purchases from the ABS (Panel A), which serves as a proxy for electricity use.

Because compensated plants are systematically larger, consume more energy, and are more energy-intensive, it is essential to account for this selection bias in our estimation strategy to recover causal estimates of the compensation scheme's effects. Table 1 also highlights the challenge posed by sample size, particularly the limited number of compensated plants relative to non-compensated ones – especially within the QFI sample. Additional descriptive evidence on our key outcome variables, including figures showing their development over time, is provided in Appendix B.

3.4. Using predicted electricity use to calculate the eligibility criterion

A key data challenge we face is the limited availability of plant-level electricity consumption data. The QFI dataset covers a relatively small sample, with electricity use in kWh available for only a small subset of plants (Table 1, Panel B), and only up to 2015. To circumvent this problem, we use the relationship between energy purchases (in £) from the ABS and electricity use (in kWh) from the QFI sample to predict electricity consumption for the larger ABS sample. This predicted electricity consumption is used solely to evaluate the eligibility criterion described in Eq. (9). Importantly, we do not use predicted electricity use, or any variable derived from it, as an outcome in the main analysis presented in Section 5.

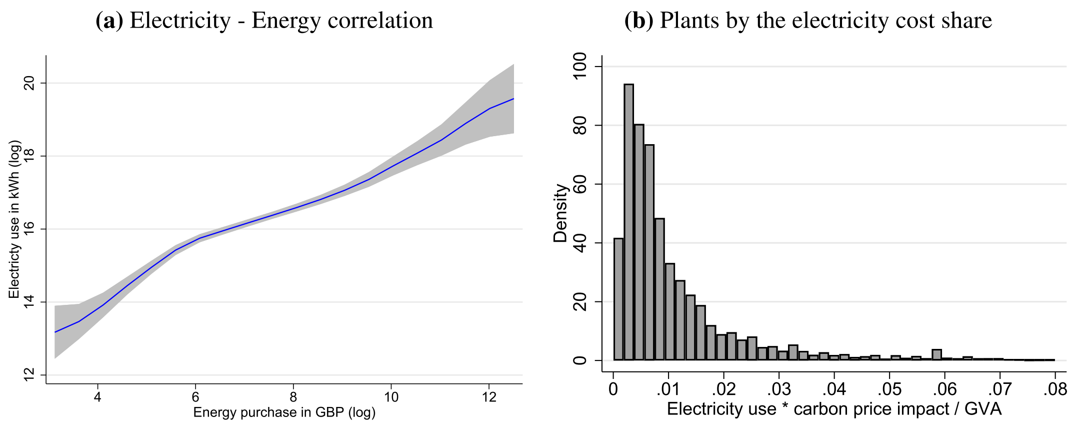

Fig. 3. Predicting electricity consumption from energy purchases. *Notes:* Panel (a) displays the correlation between the log of electricity use and the log of energy purchases in 2011, including a 95 % confidence interval and a locally smoothed polynomial fit. Smoothing is performed using an Epanechnikov kernel with 20 points. Panel (b) shows the distribution of plants by electricity cost share, calculated using the formula in Eq. (9) and predicted electricity use (see Appendix C for details). Data source: the Annual Business Survey (ABS) and the Quarterly Fuels Inquiry (QFI). The sample is restricted to plants in manufacturing industries (SIC codes 7–33).

Fig. 3, panel (a) illustrates the near-linear relationship between the log of electricity use and the log of total energy purchases in the raw data. The raw correlation between energy purchases and electricity use ranges from 0.91 to 0.93, depending on sample restrictions (Appendix Table C.1). Appendix C provides further details on the procedure used to generate predicted values of electricity consumption. In short, we regress the log of electricity use (in kWh) on the log of total energy purchases (in £) for the sub-sample of plants with non-missing values for both variables. The regression includes industry fixed effects and controls for employment and turnover. We then apply the estimated equation to predict plant-level electricity use for the broader ABS sample. Appendix Figure C.1 shows that the distributions of predicted and reported electricity use closely overlap for the sub-sample of plants for which reported electricity use is available, supporting the validity of our prediction approach.[22]

As we will see in Section 4, having a measure of predicted electricity use is important for our empirical strategies. First, we define the electricity intensity of plants as the use of electricity relative to GVA, and use this variable as a matching variable in the DiD estimation (Section 5.1). Second, we calculate the electricity cost intensity for all plants in the sample to assess the eligibility criterion defined in Eq. (9), which serves as one of the two assignment variables in the fuzzy RD design with three-way fixed effects (Section 4.2).

Panel (b) in Fig. 3 shows the distribution of the calculated electricity intensity criteria, based on Eq. (9). The majority of firms have an electricity intensity well below the 5 % threshold required for compensation eligibility. There is no evidence of bunching or strategic behavior around the threshold, which is consistent with expectations: while plants can, in theory, influence the magnitude of compensation payments, they cannot affect their eligibility by adjusting production levels. A McCrary test also finds no indication of bunching at the 5 % eligibility cut-off; see Appendix F for details.

4. Empirical strategy

Given the challenges posed by selection bias and limited sample size, our analysis of the indirect impacts of carbon pricing – via electricity prices – on manufacturing plants, both with and without compensation, follows a dual empirical strategy. Recognizing that no single approach can adequately overcome all threats to identification, we employ two empirical strategies: i) a difference-in-differences (DiD) design with inverse propensity score weighting and industry-specific time trends; and ii) a "fuzzy" RD design with three-way fixed effects, leveraging the discontinuous jump in the probability of receiving compensation at the eligibility thresholds. We compare results across both strategies to assess the robustness of our findings.

4.1. Difference-in-differences (DiD)

Our first empirical strategy exploits variation within narrowly defined industries in a difference-in-differences (DiD) framework. When $Comp_{ijt}$ is a dummy that indicates if plant i belonging to firm f in industry j receives compensation payments at time t, the DiD estimator is specified as:

$$y_{ijt} = \beta_1 Comp_{ijt} + \gamma_i + \delta_{jt} + \epsilon_{ijt}, \tag{11}$$

where y_{ijt} is a placeholder for a relevant plant-level outcome (e.g., production, electricity use, or electricity intensity). The term γ_i captures plant fixed effects, while ϵ_{ijt} is the idiosyncratic error term. A plant is defined as compensated ($Comp_{ijt} = 1$) if it belongs

[22] Note that the predictions are made within the estimation sample, not out of sample. Producing true out-of-sample predictions would require setting aside part of the already limited sub-sample of plants with electricity information for model estimation.

to a firm receiving compensation for indirect carbon costs induced by the EU ETS and the UK Carbon Price Support. Accordingly, β_1 can be interpreted as the average effect of firm-level treatment at the plant level. We cluster standard errors at the firm level in all regressions, which corresponds to the level at which treatment is assigned. The key identifying assumption is that, in the absence of compensation payments, compensated and uncompensated plants would have followed parallel trends in the outcome variable. A potential threat to identification is the presence of industry-specific shocks. To mitigate this concern, we include industry-by-year fixed effects, δ_{jt}, that reflect the industry a plant operates in, which absorb time-varying shocks at the 3-digit industry level. This ensures that identification is based on variation within narrowly defined industries.[23]

However, selection bias may still persist within industries between treated and non-treated plants – for example, due to systematic differences in electricity intensity. To address these within-industry differences in observables, we combine the DiD design with inverse propensity score weighting (IPW). Specifically, we use a propensity score estimator to reweight plants in Eq. (11), adjusting for pre-treatment differences between compensated and uncompensated plants. We estimate the propensity score for each plant by pooling treated and control observations across all years and modeling the probability of receiving compensation as a function of observable pre-treatment characteristics. The estimated propensity score ($\hat{p}$) is based on: (i) the plant's average electricity intensity, $el_{i,j}$, and (ii) average values of the plant-level outcome variable, y_{ijt}, both measured over the baseline period (2005–2011).[24] The propensity scores are estimated using a probit model:

$$\hat{p}_{ij} = P(\text{Comp}_{ij} = 1) = \Phi\left[\beta_{0,j} + \beta_{1,j}\ln(\text{el}_{ij,2005\text{-}2011}) + \beta_{2,j}\ln(y_{ij,2005\text{-}2011}) + \epsilon_{ij}\right]. \tag{12}$$

For our baseline specification, we use a measure of electricity intensity that draws on the eligibility criteria outlined in Eq. (9), where electricity intensity is defined as electricity use relative to GVA. Due to the limited sample size in the QFI dataset, which contains direct observations of electricity use, we instead rely on predicted electricity use to calculate the eligibility criteria (see Section 3.4). We estimate the propensity score separately for each 3-digit SIC industry (j) using data from the 2005–2011 period. This time frame corresponds to the reference period used by the UK Government to calculate the electricity cost share, which determines whether a firm (and plants belonging to it) meets the 5 % filter test. The estimated propensity scores are then transformed into weights and used in the panel regressions.

Following Guadalupe et al. (2012), we restrict the analysis to plants that lie within the region of common support and winsorize the weights at one percent to mitigate the influence of outliers. Specifically, we weight each compensated plant by $1/\hat{p}$, and weight each uncompensated plant by $1/(1-\hat{p})$. This approach enables us to recover an estimate of the average treatment effect (ATE) of compensation on the outcome of interest (Imbens, 2004).[25]

Identifying assumptions

To assess the comparability of *compensated* and *uncompensated* plants after applying IPW, we conduct a mean comparison test on electricity intensity and the relevant outcome variable during the pre-treatment period, categorized by treatment status. The associated p-values are reported alongside the DiD estimates in Section 5.1. Finding no statistically significant difference between groups prior to treatment increases confidence in the plausibility of the parallel trends assumption. Additionally, we estimate a dynamic DiD model with leads and lags to examine how the treatment effect evolves over time. Specifically, we interact the treatment variable, $Comp_{ijt}$, with time dummies, using the year prior to the first treatment year as the reference category. If we denote M as the number of leads and K as the number of lags, we can estimate the unfolding of the treatment effect with the following regression:

$$y_{ijt} = \sum_{m=0}^{M} \beta_{-m} Comp_{ijt-m} + \sum_{k=1}^{K} \beta_{+k} Comp_{ijt+k} + \gamma_i + \delta_{jt} + \epsilon_{ijt}. \tag{13}$$

Here, lead m captures potential deviations occurring m years before the treatment, while lag k represents the effect of the policy k years after the treatment begins.

Even if pre-treatment trends are parallel, and observable differences in initial electricity cost intensities are accounted for, there may still be a component of non-random self-selection into the compensation scheme that affects post-intervention outcomes such as production, energy use, and financial performance. For instance, firms applying for compensation are likely to incur fixed costs related to the preparation of necessary accounting and administrative documentation. As a result, firms with relatively low electricity use – though still above the eligibility threshold – may find the application process too costly, choosing not to apply.

While, in principle, such selection effects can be mitigated by controlling for additional time-varying covariates and matching on pre-treatment observables, some degree of selection may be driven by unobserved factors – such as expectations about future investments and production growth. If these unobserved factors systematically influence both the decision to apply for compensation and post-treatment outcomes, this could bias our estimates upward, attributing to the compensation scheme effects that are partly due to selection. In practice, we observe that the number of compensated firms in our sample would be approximately 10 % higher if all eligible firms had applied.

[23] In robustness checks, we also estimate models using year fixed effects specific to 2-digit industries; see Section 5.1.2. Due to the limited sample size, there is a trade-off between capturing more granular industry-specific trends and maintaining sufficient statistical power for precise estimation.

[24] We also estimate the propensity score using firm-level electricity intensity and obtain comparable results; see Table D.11 in the Appendix.

[25] This weighting approach preserves a larger estimation sample and improves statistical power by avoiding the need to discard unmatched observations. See, for example, Guadalupe et al. (2012) for a similar implementation.

4.2. Fuzzy RD design with three-way fixed effects

In our DiD framework, we aim to make the treated and control plants more comparable by reweighting plants based on their estimated treatment probability. This implies that we aim to compare plants that have a similar electricity intensity prior to the intervention.

An alternative approach is to take advantage of the thresholds that influence compensation eligibility, by comparing plants that belong to firms that are right above and right below the 5 % electricity intensity cut-off. As there are two eligibility criteria in our case, we can compare firms below and above the 5 % cut-off in both eligible and non-eligible industries. As being above the 5 % cut-off for firms operating in non-eligible industries would not normally lead to compensation, this additional dimension could be interpreted as a placebo group, or as a way of absorbing potentially systematic differences between firms above and below the cut-off.

The two eligibility criteria based on industry code and the firm's past electricity intensity do not perfectly determine whether a plant gets compensation but instead create a discontinuity in the *probability* of treatment. This setting resembles a fuzzy regression discontinuity (RD) design, in which there is a jump in the probability of assignment to treatment at a given threshold (Imbens and Lemieux, 2008).[26] While standard fuzzy RD designs are typically applied in cross-sectional settings with a single assignment (or running) variable, our context differs in two key respects: (i) treatment assignment is based on two criteria – industry code and electricity intensity – and (ii) we exploit variation over time by comparing pre- and post-intervention periods. As such, our empirical setting resembles that of Ito (2015), who estimates an RD design with three-way fixed effects.[27] Our design can also be interpreted as a triple-differences (DiDiD) instrumental variable (IV) design, leveraging variation along three dimensions: i) above versus below the electricity intensity cut-off, ii) eligible versus non-eligible industries, and iii) pre- versus post-treatment periods.

Irrespective of whether we refer to our design as a fuzzy RD with three-way fixed effects or as a DiDiD-IV, the treatment effect can be estimated using two-stage least squares. Let $Comp_{ijt}$ be an indicator variable equal to 1 if plant i, belonging to firm f in industry j, receives compensation payments at time t. Let $\mathbb{1}\{c_f \geq c_0\}$ denote whether a firm's historical electricity intensity, c_f, exceeds the eligibility cut-off, c_0, where electricity intensity is defined as in Eq. (9). Further, let $\mathbb{1}\{elig_j = 1\}$ indicate whether the firm operates in a 4-digit industry eligible for compensation. The first stage, reduced form, and second stage can then be expressed as:

First stage:

$$Comp_{ijt} = \pi_1 \underbrace{post_t \times \mathbb{1}\{c_f \geq c_0\} \times \mathbb{1}\{elig_j = 1\}}_{Instrument} + \gamma_f + \theta_{jt} + \lambda_{bt} + \mu_{ijt} \tag{14}$$

Reduced form:

$$y_{ijt} = \pi_2 \underbrace{post_t \times \mathbb{1}\{c_f \geq c_0\} \times \mathbb{1}\{elig_j = 1\}}_{Instrument} + \delta_f + \rho_{jt} + \phi_{bt} + e_{ijt} \tag{15}$$

Second stage:

$$y_{ijt} = \beta_1 \widehat{Comp}_{ijt} + \eta_f + v_{jt} + \psi_{bt} + \epsilon_{ijt}, \tag{16}$$

where $post_t$ is equal to 1 for the year 2013 and onward, and 0 otherwise. Our instrumental variable is the interaction between the two eligibility criteria and the post dummy: $post_t \times \mathbb{1}\{c_f \geq c_0\} \times \mathbb{1}\{elig_j = 1\}$. The first stage (Eq. 14) estimates the probability of a plant receiving compensation, where the instrumental variable estimate is given by π_1. γ_f indicates firm fixed effects and absorbs time-invariant differences across firms. θ_{jt} indicates industry times post fixed effects and allows (eligible) industries to develop differently over time. Due to the small sample size, we measure industry at the 2-digit level. Including post fixed effects that are specific to industries at the 4 (or 3) digit level would leave us with very little identifying variation. λ_{bt} are post fixed effects that are specific to bin b of the assignment variable c_i, where bins are constructed as equal-sized bins of 0.01 on each side of the 0.05 cut-off. This specification is inspired by Ito (2015) and allows for plants belonging to firms with different initial electricity intensities to develop differently over time.[28] If we compare our research design to a standard DiDiD-IV, our specification allows for controlling for $post_t \times \mathbb{1}\{c_f \geq c_0\}$ in a more refined way by including post effects that are specific to each electricity intensity bin (λ_{bt}).[29]

Eq. (15) represents the reduced form effect, i.e., the direct effect of the instrumental variable on the outcome of interest, π_2. To recover a causal effect of the compensation scheme, we need to rescale the reduced form effect by the probability of treatment, i.e.,: $\beta_1 = \frac{\pi_2}{\pi_1}$. This estimate corresponds to β_1 in the second stage estimation (Eq. 16). Our estimated treatment effect captures the effect of

[26] In general, regression discontinuity designs (RD) can be either sharp or fuzzy. A sharp RD exploits the fact that passing a specific cut-off value deterministically leads to treatment.

[27] Ito (2015) estimates the effect of being assigned to an energy rebate program on households' energy use by applying an RD design with three-way fixed effects: household-level fixed effects, time fixed effects that are specific to the main group and the "placebo" group, and time fixed effects that are specific to each bin of the running variable. In our setting, the placebo group would correspond to the non-eligible industries, and the running variable would correspond to the historical electricity intensity.

[28] In robustness checks, we allow for other functional forms of λ_{bt}, and show that our main results are robust to interacting a post dummy with a linear or second-degree polynomial distance from the cut-off (Appendix F).

[29] A DiDiD-IV design requires controlling for each of the three groups ($c_f \geq c_0$, $elig_j$, $post$), as well as their (simple) interactions. In Eq. (14) we control for all these terms: the firm fixed effects (γ_f) absorb $c_f \geq c_0$, $elig_j$, and their interaction. The industry times post fixed effects (θ_{jt}) largely absorb $elig_j \times post$, and the electricity intensity bin times post (λ_{bt}) absorb $c_f \geq c_0 \times post$.

compensation status on a plant-level outcome, y_{ijt}, conditional on controls. As we regard all plants belonging to a compensated firm as treated, β_1 can also be interpreted as an average effect of firm treatment at the plant level.

To ensure compensated and uncompensated plants are as similar as possible, we restrict the estimation sample to a subset of plants located close to the 5 % eligibility cut-off, c_f. Calonico et al. (2020) provide a data-driven procedure to identify the optimal estimation windows, or bandwidth, in an RD setting. Their procedure is developed for a conventional RD setting where identification relies on local randomization around the cut-off. Our empirical strategy, however, does not require local randomization as we absorb time-invariant differences above and below the cut-off. Ensuring local randomization, however, strengthens the likelihood that our identifying assumptions hold. We therefore apply their procedure to select the estimation window, but also test the sensitivity of our results to alternative sample selections in robustness checks (see Appendix F). In our main analysis, we use a $+/$-0.007 bandwidth around the cut-off, which corresponds to firms whose electricity cost share is between 4.3 % and 5.7 %.

Our estimated treatment effect, β_1, should be interpreted as a local average treatment effect (LATE), rather than an average treatment effect (ATE), for two reasons. First, β_1 captures the causal effect of compensation for the subpopulation that is induced to take up the treatment by the instrumental variable, i.e., the compliers. Second, β_1 captures the causal effect for a small subgroup of the sample: plants belonging to electricity-intensive firms near the 5 % cut-off. As shown in Fig. 3b, the 5 % threshold is in the right tail of the electricity intensity distribution, and the LATE therefore reflects treatment effects for highly electricity-intensive firms. By contrast, the estimated ATE in our DiD design is based on a larger set of plants with a lower average electricity intensity.

Identifying assumptions

A causal interpretation of β_1 relies on several identifying assumptions. First, the instrumental variable needs to predict compensation status. This implies that the probability of treatment has to jump at the cut-off, c_0, for eligible plants in the post period, conditional on controls. This assumption is usually evaluated by looking at the first stage (see Section 5.2.1).

The second identification assumption is that plants cannot manipulate the assignment variables, which in our case are the industry code and the firm's historical electricity cost share. The latter variable is calculated based on electricity consumption and gross value added between 2005 and 2011 – a period that pre-dates the first announcement of the compensation policy in 2012. The scope for strategically manipulating this assignment variable therefore appears very limited and would require firms to have knowledge of the policy and its detailed design before the announcement. A McCrary test also shows no sign of bunching around the threshold value (see Section 3.4 and Appendix F). As for the former assignment variable, a firm usually decides on the most appropriate industry code when officially registering their business. The industry code is meant to reflect the main activity of the plant or firm and usually cannot be changed unless the underlying activity changes. There is hence very limited scope to strategically manipulate this variable.

Third, the exclusion restriction requires that the instrumental variable, conditional on controls, only affects the outcome variable of interest via the compensation status. This assumption cannot be tested explicitly. However, by including three-way fixed effects and a narrow bandwidth, we make it less likely that our instrument is working through other mechanisms than the compensation scheme. If we are unsure whether the exclusion restriction holds, it is also possible to focus on the reduced form effect, which does not rely on this assumption.

Fourth, we must assume monotonicity, i.e., that crossing the thresholds cannot simultaneously cause some units to get compensation and others to exit the scheme. In our setting, it seems unlikely that the two assignment variables, i.e., being above the electricity intensity cut-off and operating in an eligible industry, make it $less$ likely to receive compensation.

Beyond these identifying assumptions, a key analytical challenge is the small sample size, particularly the limited number of compensated plants in the QFI dataset. This constraint may affect the precision of our coefficient estimates, making it more difficult to detect statistically significant effects.

5. Treatment effects of the indirect carbon cost compensation

5.1. DiD estimates of the average treatment effects

Tables 2 and 3 present the main results from the DiD estimation (Eq. 11) using data from the ABS and QFI, respectively. As a reminder, the ABS sample is larger but relies on energy purchases as a proxy for electricity consumption, whereas the QFI provides direct measures of electricity use for a smaller subset of plants. The tables also report p-values from mean comparison tests of average electricity intensity and lagged outcomes over the baseline period (2005-2011), categorized by treatment status. These tests provide additional evidence on the credibility of the parallel trend assumption after IPW.

First, in terms of production, and in line with Prediction 1, our results indicate that compensation increased output. Using our primary proxy indicator, "sales of own goods", we estimate a 16 % increase in the post-treatment period. This effect is derived from a comparison of compensated and uncompensated plants with similar pre-treatment electricity intensity and sales figures (see column (1) in Table 2). The estimated treatment effect is robust across a number of tests, detailed in Section 5.1.2.

Our findings show that compensation mitigates the decline in output volumes, suggesting it plays a role in limiting carbon leakage. Notably, an alternative policy design in the form of a carbon cost exemption may not have delivered the same effect on output and, consequently, on leakage protection (Gerster and Lamp, 2023). However, the exact magnitude of the output effect remains imprecise. While we can confidently rule out no effect or a negative effect within the 95 % confidence intervals, the limited sample size reduces statistical power, making it difficult to precisely estimate the true effect size, which could range from a modest (3 %) to a sizable (28 %) impact. Interestingly, we do not detect any significant effects on employment (cf. Table D.1), productivity, or GVA (cf. Figure E.1). In other words, our results fail to support claims that higher carbon prices or energy costs lead to job losses.

Table 2
Average treatment effects of compensation on sales, energy purchases and energy intensity (2010–2015).

	Source: ABS		
	(1) Sales of own goods	(2) Energy purchases	(3) Energy intensity
Compensation	0.156**	0.300	−0.123
	(0.0638)	(0.182)	(0.102)
Observations	532	303	688
N Compensated	27	14	27
N Other	97	65	157
Plant FE	✓	✓	✓
Year×Industry FE (3-digit SIC code level)	✓	✓	✓
Mean electricity intensity 05–11: compensated	0.035	0.036	0.031
Mean electricity intensity 05–11: other	0.035	0.033	0.036
P-value: mean-comparison test	0.725	0.524	0.107
Mean outcome 05–11: compensated	11.135	7.346	−3.147
Mean outcome 05–11: other	11.147	7.327	−2.980
P-value: mean-comparison test	0.944	0.961	0.248

Notes: Table shows the coefficient β_1 estimated from Eq. (11). Dependent variables are in logs. The treatment group is defined as plants belonging to a firm that received compensation for the indirect carbon costs induced by the EU ETS and the UK Carbon Price Support. Standard errors are clustered at the firm level. All regressions include year × industry fixed effects at the 3-digit SIC code level, and are weighted by the inverse propensity score. The sample is restricted to manufacturing industries (SIC 7–33) and plants with at least one observation in the post-treatment period. We also drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS) and Quarterly Fuels Inquiry (QFI). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3
Average treatment effects of compensation on electricity use, electricity intensity and indirect CO_2 emissions (2010–2015).

	Source: QFI		
	(1) Electricity use	(2) Electricity intensity	(3) Indirect CO_2 emissions
Compensation	0.220**	0.140	0.225**
	(0.0900)	(0.189)	(0.0884)
Observations	413	598	426
N Compensated	15	16	14
N Other	65	106	68
Plant FE	✓	✓	✓
Year×Industry FE (1-digit SIC code level)	✓	✓	✓
Mean electricity intensity 05–11: compensated	0.036	0.034	0.037
Mean electricity intensity 05–11: other	0.037	0.034	0.037
P-value: mean-comparison test	0.795	0.958	0.583
Mean outcome 05–11: compensated	17.305	5.936	23.491
Mean outcome 05–11: other	17.392	5.995	23.541
P-value: mean-comparison test	0.607	0.706	0.795

Notes: Table shows the coefficient β_1 estimated from Eq. (11). Dependent variables are in logs. The treatment group is defined as plants belonging to a firm that received compensation for the indirect carbon costs induced by the EU ETS and the UK Carbon Price Support. Standard errors are clustered at the firm level. All regressions include year × industry fixed effects at the 1-digit SIC code level, and are weighted by the inverse propensity score. The sample is restricted to manufacturing industries (SIC 7–33) and plants with at least one observation in the post-treatment period. We also drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS) and Quarterly Fuels Inquiry (QFI). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In terms of electricity intensity, both our results using the QFI measure (electricity use/sales, Table 3, column 2) and the ABS measure (energy purchase/sales, Table 2, column 3) indicate no statistically significant difference between compensated and uncompensated plants. This aligns with Prediction 2, suggesting that incentives to reduce energy intensity remain unchanged. However, given the limited statistical power, these estimates should be interpreted with caution.[30] Nevertheless, our findings suggest

[30] These variables are more noisy than log sales. Outcome variables with high standard deviations relative to their means require greater statistical power to detect significant effects; see, for example, Egerod and Hollenbach (2024). In our case, energy intensity exhibits a standard deviation-to-mean ratio twice that of log sales, contributing to this limitation.

that compensation schemes may have an advantage over tax exemptions, as they do not appear to discourage energy intensity improvements, unlike tax exemption policies (Gerster and Lamp, 2023).

Finally, we find that electricity consumption increases for compensated plants relative to uncompensated plants, consistent with Prediction 3. This suggests that compensation dampens the carbon price signal, thereby reducing its intended effect of discouraging energy use and, consequently, emissions. Estimates based on physical electricity use data from the QFI (Table 3 columns 1 and 3) indicate that compensation increased electricity use by an average of 22 % and electricity-related carbon emissions by 23 %. Using energy purchases from the ABS as a proxy, we also find a positive effect, although it is not statistically significant (Table 2, column 2). It is important to note that the ABS records only the monetary value of purchased energy and excludes self-generated electricity. As a result, ABS-based estimates may understate actual energy consumption for plants that produce some of their electricity on-site.[31]

5.1.1. *Average treatment effects over time*

Fig. 4 shows how treatment effects unfold over time by plotting the annual DiD coefficients estimated from Eq. (13). The figure also helps evaluate the validity of the parallel pre-treatment trends leading up to 2013, when compensation was first paid out (for indirect costs incurred in 2012). Fig. 4, Panel (a) shows that the difference in production levels between compensated and uncompensated plants emerged already in 2013, but increased in 2014. Fig. 4, Panel (b) instead shows that for electricity intensity (proxied by energy purchases over sales), the gap widened in 2013 but closed in subsequent years.

Our main estimates are based on a post-treatment period that ranges from 2013 to 2015, as this is the only estimation window where both ABS and QFI data are available. However, ensuring comparability across different outcome variables comes at the cost of reducing the estimation sample size. To further validate our findings, Table 4 presents additional results for outcome variables available beyond this period, complementing the findings in Table 2. Each column reports the average treatment effect estimated for the period 2010 to the year indicated by the column heading. Extending the sample to 2019 results in an estimated average treatment effect on sales of own goods of 14 % (Panel A), closely aligning with our main estimate of 16 %. For energy purchases (Panel B), estimates decline over time and remain statistically insignificant. Lastly, estimated effects on energy intensity remain small and statistically insignificant when extending the sample to 2019 (Panel C). The corresponding results for employment are provided in Table D.1 in the Appendix.

5.1.2. *Robustness checks for DiD estimation*

Our DiD results are robust to a series of sensitivity tests. We begin by examining the robustness of our estimates to the computation of p-scores through three exercises. First, we re-estimate p-scores at the firm level, rather than the plant level, to match the aggregation level used in the 5 % eligibility criterion (see Appendix Table D.11). Second, to address concerns that the global financial crisis might have affected the computation of p-scores and influenced our estimates, we verify that our results remain robust when using an alternative time window (2010-2012) to compute our p-scores (see Appendix Table D.12). Third, we also show that our main results are robust to trimming the sample, by dropping plants with electricity intensity (as per Eq. 9) below different thresholds (see Appendix D). Additionally, Appendix D presents results using industry-specific effects at a broader 2-digit sectoral level, thereby trading off some

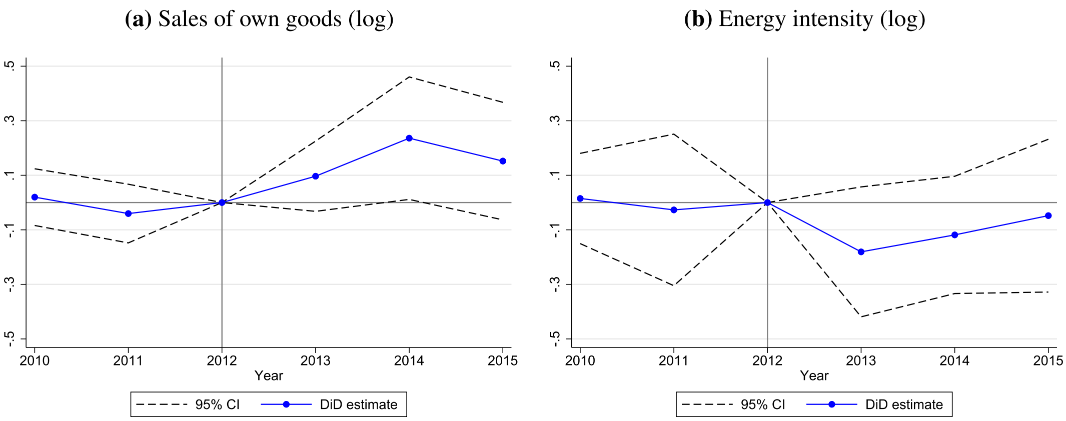

Fig. 4. Treatment effects of compensation, by year (2010–2015). *Notes:* Figures plot the coefficients $\sum_{m=0}^{M} \beta_{-m}$ and $\sum_{k=1}^{K} \beta_{+k}$ estimated from Eq. (13). The dependent variable is given by the subfigure headings. All dependent variables are in logs. The connected lines depict the estimated yearly treatment effect, while the dashed lines indicate 95 % confidence intervals. We drop plants with an electricity intensity based on Eq. (9) below 0.01. All regressions include plant fixed effects and industry specific year dummies at the 3-digit level. Standard errors are clustered at the firm level. Data sources: Annual Business Survey (ABS) and Quarterly Fuels Inquiry (QFI).

[31] By 2015, approximately 10 % of industrial electricity demand in the UK was met through autogeneration (Department for Business, Energy & Industrial Strategy).

Table 4
DiD estimates of compensation on ABS variables (2010–2019).

	2015	2016	2017	2018	2019
Panel A: Sales of own goods					
Compensation	0.156**	0.164**	0.126*	0.147**	0.144**
	(0.0638)	(0.0763)	(0.0705)	(0.0701)	(0.0693)
Obs	532	717	851	1069	1186
N compensated	27	36	39	40	40
N other	97	127	132	156	158
Energy intensity 05–11 (Treat)	0.035	0.035	0.035	0.034	0.034
Energy intensity 05–11 (Control)	0.036	0.037	0.036	0.036	0.036
P-value (mean-comparison test)	0.725	0.424	0.628	0.596	0.597
Outcome 05–11 (Treat)	11.135	10.867	10.831	10.744	10.743
Outcome 05–11 (Control)	11.147	10.905	10.906	10.844	10.844
P-value (mean-comparison test)	0.944	0.822	0.664	0.558	0.553
Panel B: Energy purchases					
Compensation	0.300	0.259	0.145	0.0674	0.0862
	(0.182)	(0.176)	(0.192)	(0.176)	(0.172)
Obs	303	453	548	730	822
N compensated	14	24	26	28	28
N other	65	92	95	115	119
Energy intensity 05–11 (Treat)	0.036	0.035	0.035	0.034	0.034
Energy intensity 05–11 (Control)	0.033	0.036	0.036	0.035	0.035
P-value (mean-comparison test)	0.524	0.773	0.809	0.688	0.720
Outcome 05–11 (Treat)	7.346	7.225	7.174	7.224	7.221
Outcome 05–11 (Control)	7.327	7.052	7.034	7.136	7.115
P-value (mean-comparison test)	0.961	0.532	0.619	0.745	0.694
Panel C: Energy intensity					
Compensation	−0.123	−0.0421	0.0519	0.0158	0.0177
	(0.102)	(0.112)	(0.163)	(0.111)	(0.108)
Obs	688	989	1222	1445	1611
N compensated	27	42	45	45	45
N other	157	202	218	233	239
Energy intensity 05–11 (Treat)	0.031	0.030	0.037	0.031	0.031
Energy intensity 05–11 (Control)	0.036	0.034	0.037	0.036	0.035
P-value (mean-comparison test)	0.107	0.056	0.907	0.059	0.107
Outcome 05–11 (Treat)	−3.147	−3.087	−2.750	−3.089	−3.090
Outcome 05–11 (Control)	−2.980	−2.999	−2.990	−3.005	−3.013
P-value (mean-comparison test)	0.248	0.416	0.029	0.415	0.456

Notes: Table shows the coefficient β_1 estimated from Eq. (11). Each panel shows results from 5 separate regressions using different time periods, where the column headings indicate the end year. Panel A shows estimated effects on sales of own goods. Panel B shows the estimated effects on energy purchases. Panel C shows estimated effects on energy intensity, where energy intensity is defined as energy purchased divided by sales. Dependent variables are in logs. The treatment group is defined as plants belonging to a firm that received compensation for the indirect carbon costs induced by the EU ETS and the UK Carbon Price Support. Standard errors are clustered at the firm level. All regressions include year × industry fixed effects at the 3-digit SIC code level and are weighted by the inverse propensity score. The sample is restricted to manufacturing industries (SIC 7–33) and plants with at least one observation in the post-treatment period. We also drop plants with an electricity intensity based on Eq. (9) below 0.01. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

precision in the identification with an expanded estimation sample. In this case, we compute p-scores at the 2-digit level, allowing for more flexible matching within broader industry classifications.

We also examine how our results are affected by excluding multi-plant firms. Since compensation is calculated at the plant level, multi-plant firms could in theory benefit more from reallocating production between their plants, given their greater operational flexibility. While our data do not provide sufficient variation to separately analyze the responses of different types of firms, we find that excluding multi-plant firms does not materially alter our results, yielding comparable findings (see Appendix Table D.13). This suggests that the behavior of multi-plant firms does not significantly drive our overall conclusions. Finally, Tables D.7 - D.9 in the Appendix present alternative estimations using different proxies for production and energy intensity from the ABS sample. These findings are visually summarized in Figures E.1 - E.2 in the Appendix, which provide a graphical comparison of estimated effects across the array of robustness tests and outcome variables.

5.2. *Fuzzy RD estimates of the local average treatment effects*

Turning to the fuzzy RD estimation, we begin by presenting graphical evidence and estimated coefficients for the first stage and reduced form. We then proceed to the instrumental variable (IV) estimates in the second stage.

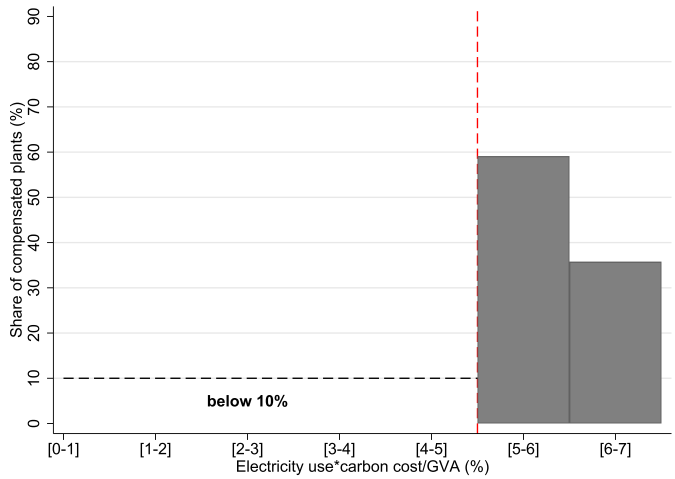

Fig. 5. Share of compensated plants by electricity cost share. *Notes:* The figure shows the share of compensated plants by the average electricity cost share (electricity use*carbon price impact/GVA) in 2005–2011, using predicted electricity use. The height of the bars reflect mean values for plants located within the indicated electricity cost share bins. The precise height of the bars located to the left of the indicated threshold is censored due to disclosure concerns. The sample is restricted to eligible 4-digit industries and considers all plants that are included in the ABS database from 2010 to 2019 to facilitate meeting the disclosure concerns. Additional graphical evidence of a discontinuity at the cut-off, based on narrower sample definition corresponding to our estimation sample, is available in Appendix F. This evidence demonstrates a more pronounced jump (to around 80 %) at the 5 % cut-off.
Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).

5.2.1. *First stage*

In our first stage, we predict a plant's compensation status based on three dimensions: i) whether the plant passes the 5 % cost intensity threshold ($\mathbb{1}\{c_i \geq c_0\}$), ii) whether the plant operates in an eligible industry ($\mathbb{1}\{elig_j = 1\}$), and iii) whether the year is post intervention ($post_t$). Our instrumental variable is constructed by taking the interaction of these three variables.

Fig. 5 illustrates the variation in the first stage from the 5 % eligibility threshold. The figure shows the share of compensated plants across intervals of electricity cost intensity, where the sample is restricted to all plants in eligible industries. Averages within each bin are based on data from 2005–2011, with predicted electricity consumption used to calculate electricity cost intensity. As expected, there is a sharp discontinuous jump in the share of compensated plants at the eligibility cut-off. Nearly 60 % of plants with a 5–6 % electricity cost intensity receive compensation, while below the cut-off, less than 10 % do. Bar heights to the left of the threshold are suppressed for confidentiality reasons. Additional graphical evidence, based on a narrower sample that includes only plants in our estimation sample, demonstrates a more pronounced jump (to around 80 %) at the 5 % cut-off (Figure F.2). While this sharp jump is reassuring, it is worth remembering that we measure eligibility with some error, and we do not perfectly observe whether a firm meets the 5 % electricity cost intensity.

Our instrument is based on variation not only in initial energy intensity, but also across eligible and non-eligible industries and over time. Table 5, Panel A, reports the estimated first stage based on Eq. (14), where we include both eligible and non-eligible industries as well as firm- and industry×year fixed effects. The sample is restricted to plants with a calculated electricity cost intensity between 4.3 % and 5.7 %. The estimated coefficients reflect the change in the probability of receiving compensation payments if a plant is above the 5 % eligibility cut-off *and* operates in an eligible industry. The estimated probability of receiving compensation is 0.88 and the F-statistic of the excluded instrument is around 75. Thus, our first-stage results show that our instrument is a strong predictor of receiving compensation for the sub-sample of plants close to the 5 % eligibility cut-off.

5.2.2. *Reduced form*

Fig. 6 illustrates the discontinuous jump in our main outcome variables at the 5 % eligibility cut-off when the sample is restricted to eligible industries in the post period. Each dot in the panels represents the local mean of the outcome variable, as indicated by the sub-headings, for quantile-spaced bins. The lines are polynomial regressions over these bins.[32] The figure shows a jump in sales of own goods (Panel (a)) and electricity consumption (proxied by energy purchases, (Panel (c)) at the 5 % threshold value, while there is no detectable jump in the energy intensity (Panel (b)). While these plots suggest that the compensation had an effect on sales and energy use, the discontinuous jumps are based on only one dimension of our instrumental variable (i.e., $c_i \geq c_0$). In our main estimation of the reduced form effect, we take advantage of two more dimensions: eligible vs. non-eligible, and pre vs. post.

Table 5, Panel B, reports the reduced form estimates based on Eq. (15), where we also exploit variation across eligible and non-eligible industries, and over time. We find a statistically significant effect on sales of own goods (0.26), a similar-sized but noisy effect on energy purchases (0.21), and a small and insignificant effect on energy intensity (-0.056). These coefficients represent a lower bound of the effect of the compensation scheme in the RD sample, as not all plants meeting the eligibility criteria receive

[32] We follow Calonico et al. (2015) to determine the optimal number of bins in this cross-sectional setting.

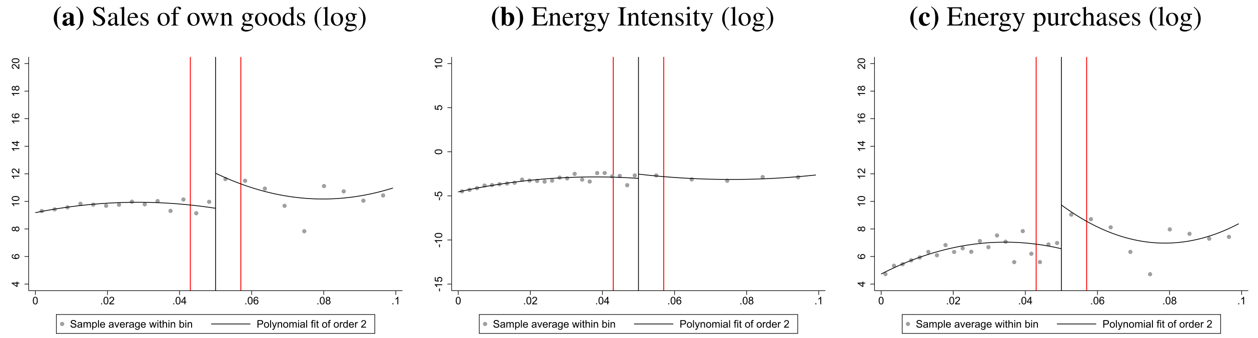

Fig. 6. RD Plot based on quantile spaced number of bins (2013–2015). *Notes:* The figure shows data-driven regression discontinuity plots using polynomial regression based on quantile-spaced numbers of bins. The optimal number of bins has been selected following Calonico et al. (2015). Cut-off: 0.05. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). The sample is restricted to 2-digit industries with at least one eligible firm, and to the post period (2013–2015). To comply with the disclosure requirements of the UK Secure Lab, bins with insufficient observations have been suppressed. Additionally, the sample was restricted to 2-digit industries—rather than the more granular 4-digit level—to ensure a larger number of observations that align with these disclosure requirements.

compensation. The reduced form estimates could be interpreted as “intention to treat” (ITT) estimates, which have the advantage of not relying on the exclusion restriction for unbiasedness.

5.2.3. Main RD estimates

To recover a local average treatment effect (LATE) of compensation, we rescale the jump in outcomes (reduced form) by the jump in the treatment probability (first stage). A causal interpretation of the estimated treatment effect relies on the assumption that crossing the 5 % eligibility threshold only impacts plants via the probability of receiving compensation. Due to the smaller sample size around the threshold, we only present treatment effects for outcomes from the ABS sample.[33] For our main results, we report RD estimates with a $+/-0.007$ bandwidth (which restricts the sample to companies whose electricity cost share amounts to an interval between 4.3 % and 5.7 %), following the data-driven procedure to identify optimal estimation windows in RD settings by Calonico et al. (2020). More details on this procedure can be found in Appendix F.

Table 5, Panel C, reports the second-stage RD estimates. We find evidence of a causal effect of compensation on production, proxied by sales, which increased by 30 % for compensated plants relative to similar uncompensated plants. The effect on electricity consumption, proxied by energy purchases, is positive and large (24 %), though not statistically significant. Meanwhile, we find a negative but non-significant effect on energy intensity. Overall, these findings are in line with our three theoretical predictions and DiD results, and provide additional evidence that compensation for higher electricity prices particularly encourages maintaining output levels for compensated plants. While our limited sample size may reduce statistical power and prevent small p-values, the robustness of results across different outcomes, specifications, and estimation strategies strengthens our confidence in the overall pattern of effects. Finally, while both approaches lead to similar conclusions, the RD estimates are larger in magnitude compared to our ATEs, suggesting – as expected – that the effects of compensation tend to be stronger for more electricity-intensive plants.

5.2.4. Robustness checks

We conduct a series of robustness tests to further assess the validity of our baseline RD findings. Specifically, we estimate RD models with different functional form assumptions by modifying Eqs. (14–16) to account for the linear (see Appendix Table F.1) and quadratic (see Appendix Table F.2) distances of each observation from the threshold (cf., Section 4.2) (Lee and Lemieux, 2010). Additionally, we test the robustness of our main estimates using different bandwidth choices and generate a distribution of estimated effects across various estimation window sizes (see Appendix F).

5.3. Comparing the DiD and RD estimates

The balance of evidence from the DiD and RD approaches is summarized in Fig. 7. Both methods indicate that the compensation scheme increased sales and energy consumption relative to uncompensated plants. We find no detectable significant effects on energy intensity, but these estimates are also very noisy. The RD approach yields larger local average treatment effects (LATEs) than the average treatment effects (ATEs) from the DiD approach. However, given the large standard errors, these differences should be interpreted with caution.

Still, one possible reason for the differing magnitudes, although noisy, is that the two strategies focus on different populations. The RD approach focuses on a subset of plants near the discontinuity threshold in the electricity intensity distribution (see Panel (b) in Fig. 3). This means that the RD approach may be interpreted as the treatment effect of the compensation scheme for plants

[33] The RD estimates for QFI variables yield statistically inconclusive results due to the very limited sample size within the bandwidth considered for the estimation and cannot be reported due to disclosure concerns.

Table 5
Local average treatment effects of compensation (2010–2015). Fuzzy RD.

	(1) Compensation	(2) Compensation	(3) Compensation
Panel A: First stage			
Post × Above cut-off × Eligible industry	0.879***	0.879***	0.879***
	(0.101)	(0.101)	(0.101)
	(1) Sales of own goods	(2) Energy purchases	(3) Energy intensity
Panel B: Reduced form			
Post × Above cut-off × Eligible industry	0.264**	0.209	−0.0562
	(0.125)	(0.187)	(0.134)
Panel C: Second stage			
Compensation	0.301**	0.238	−0.0639
	(0.131)	(0.199)	(0.156)
Panel D: OLS			
Compensation	0.164	0.263**	0.103
	(0.102)	(0.130)	(0.105)
Observations	253	249	335
N Compensated	20	20	20
N Other	49	48	27
F statistics	75.47	75.44	75.39
Functional form	Bins	Bins	Bins

Notes: Tables show the coefficients estimated from the first stage, reduced form, and second stage of the fuzzy RD design with three-way fixed effects. Dependent variables are given by the column headings. In panels B–D, outcome variables are measured in logs. The treatment group is defined as plants belonging to a firm that received compensation for the indirect carbon costs induced by the EU ETS and the UK Carbon Price Support. Standard errors are clustered at the firm level. The sample is restricted to manufacturing industries (SIC 7–33) and plants with at least one observation in the pre and post-treatment period. The sample estimation period is 2010–2015. Cut-off value: 0.05. Bandwidth: cut-off value $+/-0.007$. Bandwidth refers to the range of electricity intensity values (electricity use * carbon cost / GVA) used to restrict the sample. Each stage of the estimation includes firm-level and 2-digit sector × year fixed effects. Data source: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

most likely to be affected by the policy. In contrast, the DiD approach estimates the average effect across a broader population of manufacturing plants, regardless of their relative position in the electricity intensity distribution.

Another reason why a LATE estimate may be larger than an ATE estimate is linked to the identification strategy. Our DiD approach combined with IPSW assumes that the weighted treatment and control groups are comparable in all respects other than the treatment. However, this assumption may not hold if there are unobservable differences between the treatment and control groups that affect the outcomes of interest. The RD approach, on the other hand, relies on a discontinuity in the policy rule to identify the treatment effect. Thus, the RD approach may better account for unobservable factors that could influence the outcomes of interest.

6. Policy implications

Assessing the effectiveness of anti-leakage policies typically involves looking for evidence of leakage occurring, without explicitly considering the costs of measures. This section aims to shed light on the trade-offs between preventing leakage and forgoing abatement.

Table 6 reports back-of-the-envelope calculations on the costs and benefits of the compensation scheme based on our DiD estimates. The estimated value associated with maintaining output levels, calculated based on our average treatment effect estimates in Table 2, is in the ballpark of £2 billion per year. Lower output reduction also led to less contraction in electricity use: we estimate an additional 2.35 TWh of electricity consumption annually, equivalent to 2.5 % of total industrial electricity consumption. This, in turn, resulted in an estimated annual increase of approximately 1.5 million tonnes of indirect CO_2 emissions Given that total emissions are capped under the ETS, any increase in emissions from electro-intensive sectors must be offset by reductions elsewhere in the system. This implies that the compensation scheme may impose additional abatement costs on uncompensated plants and sectors, as they must adjust more to meet the overall emissions constraint.

The foregone reductions in indirect carbon emissions are valued at approximately £36 to 377 million per year, depending on the CO_2 price assumption used. The upper-bound estimate is based on the UK government's current official recommendation for the social cost of carbon (SCC)[34] while the lower-bound estimate uses the average EU Allowance clearing prices as an alternative market-based proxy for the cost of carbon. Given the cap on total emissions under the EU ETS, these values reflect the cost of abatement that must

[34] The UK government recommends a social cost of carbon (SCC) of £241 per tonne of carbon dioxide emitted (in 2020 £) for use in policy appraisal and evaluations. See Department for Energy Security and Net Zero (UK) for further details.

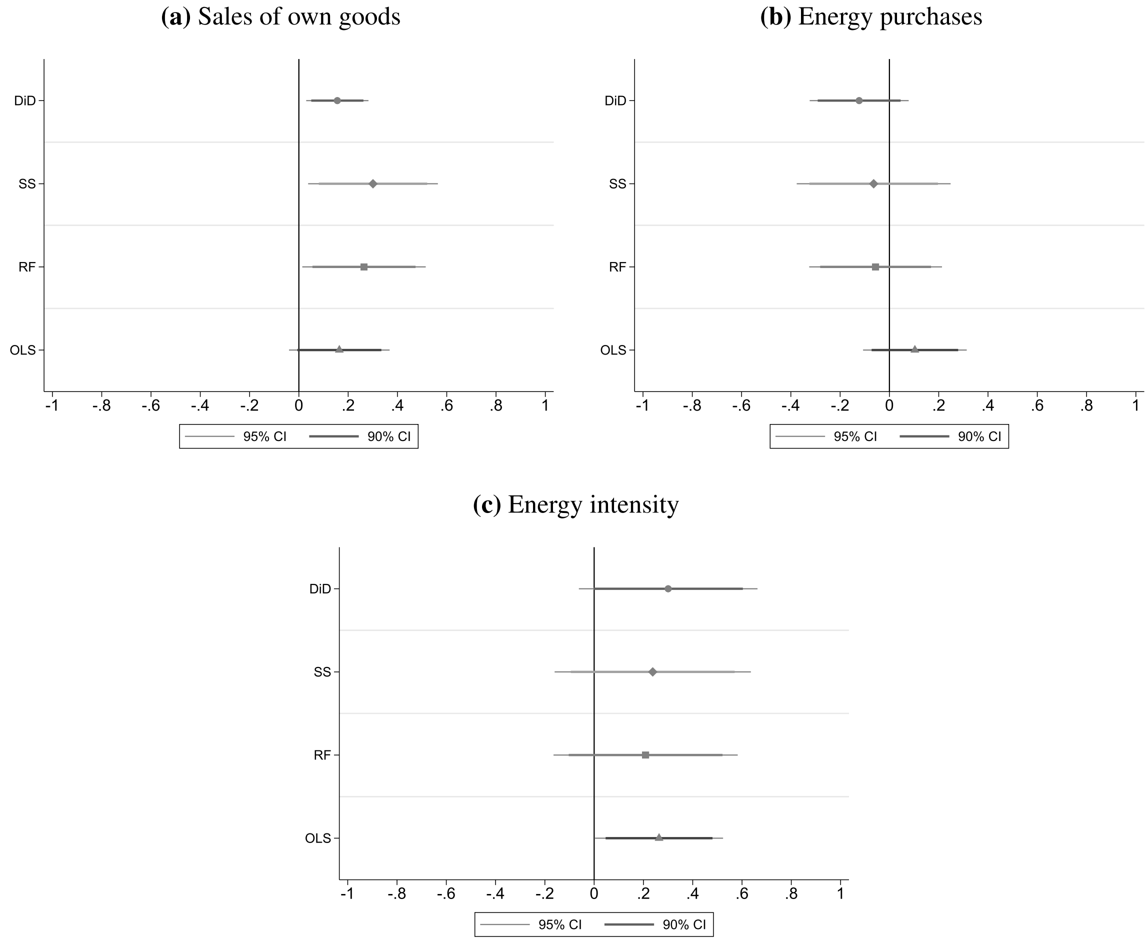

Fig. 7. Comparing ATEs and LATEs across ABS outcome variables. *Notes:* Figure compares estimated coefficients across different empirical strategies and estimation samples. DD refers to the Difference-in-difference (DiD) estimates presented in Section 5.1. SS, RF, and OLS refer to the second stage, reduced form, and OLS estimates, respectively, presented in Section 5.2. All outcome variables are in log terms. Data sources: Annual Business Survey (ABS), and Quarterly Fuels Inquiry (QFI).

occur elsewhere in the system to offset increased emissions from compensated firms. As such, they offer insights into the potential trade-offs in societal benefits stemming from the implementation of compensation schemes alongside carbon pricing.

Output levels are estimated to be substantially higher under the compensation scheme, indicating that it contributed to shielding energy-intensive firms from the expected output-reducing effects of higher electricity costs. This pattern is consistent with the scheme functioning as an implicit production subsidy. When comparing the estimated benefits to the direct fiscal cost - approximately £72 million annually (Section 3.1) – the return appears substantial: each pound of compensation yielded, on average, more than one pound in increased production value (proxied by sales) and GVA. However, these economic gains come with a notable environmental cost. The increase in electricity-related emissions among compensated energy-intensive plants is sizable, amounting to around 4.3 % of total annual industrial emissions from electricity use – or 1.3 % of nationwide emissions from electricity.

Therefore, consistent with empirical studies showing limited evidence of carbon leakage from the EU ETS – largely attributed to generous free allocation (e.g. Naegele and Zaklan, 2019) – our findings suggest that indirect carbon cost compensation schemes work, insofar as production displacement and carbon leakage are being discouraged. We also find no evidence of adverse impacts on employment, GVA, productivity, or energy intensity improvements (Section 5.1), with the caveat of large standard errors. This is in contrast to the documented disincentive effects of tax exemption policies, which have been shown to impede electricity intensity improvements (Gerster and Lamp, 2023). However, as expected, the preservation of output under compensation also leads to higher emissions relative to a counterfactual without compensation.

While the ETS cap guarantees the overall environmental effectiveness of the system, our findings show that indirect CO_2 cost compensation has important implications for both economic efficiency and distributional outcomes. By targeting compensation to energy-intensive sectors, the policy shifts abatement responsibilities toward less energy-intensive sectors, which may face higher marginal abatement costs – thereby raising the overall compliance costs of the ETS (Böhringer et al., 2022). In addition, output-linked compensation reduces the pass-through of CO_2 costs to product prices, weakening price signals and dampening incentives for consumers to shift toward low-carbon alternatives (Quirion, 2009). This suggests the need to put in place additional incentives and

Table 6
Costs and benefits.

	Total
Number of compensated firms	59
Estimated value of maintaining production	£2,000 million / year
Estimated value of higher GVA	£232 million / year
Estimated forgone reduction in electricity use	2.35 TWh / year
Increased indirect emissions	1.56 million tonnes / year
Value of increased indirect emissions - lower bound	£36 million / year
Value of increased indirect emissions - upper bound	£377 million / year
Compensation for CO_2 costs	£72.4 million / year
Increase in production per £ of compensation	£27.6
Increase in GVA per £ of compensation	£3.2
Value of increased indirect emissions per £ compensation - lower bound	£0.5
Value of increased indirect emissions per £ compensation - upper bound	£5.2

Note: £ are reported in 2020-values. Compensation payments are computed by averaging the values reported between 2013 and 2019 (Section 3.1). We calculate increases in production and indirect emissions for the average compensated firm in our sample by leveraging our DiD estimates of the average treatment effect presented in Table 2 and 3. Specifically, we calculate plant-specific mean increases in sales (as a proxy for production) and indirect emissions by multiplying the corresponding estimated ATE from Eq. (11) with mean pre-treatment outcome levels of sales (with a mean value of 173,749 thousand £) and indirect emissions (with a mean value of 117,904 tonnes) in each compensated firm. We additionally compute the implied increase in GVA leveraging our additional estimates summarized in Figure E.1. We obtain cumulative values by multiplying the estimated mean plant-level increases by the total number of compensated plants. *Lower bound* increased indirect emissions (£) are calculated based on the average EUA price in 2020 (which amounted to 22.83 £). Upper bound increased indirect emissions (£) are estimated using UK official guidelines on the social costs of carbon (SCC) of £241 / tonne of carbon dioxide emitted.

support to enable rapid industrial decarbonization, such as supplementary consumption-based measures (Grubb et al., 2022). Finally, using ETS auction revenues to fund compensation diverts resources away from climate-related investments or public redistribution, potentially undermining the public acceptability of carbon pricing (Baranzini et al., 2017; Douenne and Fabre, 2022). Continuing compensation would therefore warrant discussions around alternative financing mechanisms to cover the incremental costs of industrial clean technology investments, as well as broader public debate about the fairness and long-term role of industry compensation in climate policy.

7. Conclusion

Governments pursuing ambitious climate policies face a complex challenge: balancing the need to incentivize emission reductions while mitigating the risk of carbon leakage and competitive disadvantage for domestic industries. Addressing this tension often requires comprehensive strategies. One common approach is to pair carbon pricing with compensation schemes for energy-intensive firms to offset higher carbon costs or electricity prices. These measures help reduce leakage risk and secure political buy-in from industry. However, substantial transfers that disproportionately benefit a small number of energy-intensive firms and their capital owners raise equity concerns. Moreover, such carbon cost containment measures may delay industrial decarbonization, by weakening price signals, thereby shifting the mitigation burden to other sectors or consumers. While the theoretical downsides of output-based free allocation and compensation have long been recognized, they may have been downplayed due to the lack of empirical evidence. Our study helps close this evidence gap by exploiting detailed UK plant-level microdata and quasi-experimental variation in eligibility criteria to quantify the causal effects of compensation for indirect carbon costs.

We find robust evidence that compensation payments, as intended, reduce output contraction among energy-intensive plants. Our back-of-the-envelope calculations suggest that each pound of compensation yields more than a pound in production value and gross value added, indicating that compensation may help contain carbon leakage. We also find no evidence that compensation weakens incentives to improve electricity intensity – unlike tax exemptions, which have been shown to do so (Gerster and Lamp, 2023). However, by shielding firms from the full carbon price, compensation attenuates the incentive to reduce output, leading to increased electricity consumption and emissions. This comes at an environmental cost and has implications for the distribution of compliance burdens. Specifically, it shifts mitigation efforts to other sectors with potentially higher marginal abatement costs, thereby raising allowance prices and the overall cost of achieving emissions reductions under a cap.

Despite these drawbacks, compensation is likely to persist. The UK government continues to provide compensation in 2025, and several European countries plan to continue payments until 2030. Free allocation under the EU ETS is also set to continue until 2034 (European Parliament, 2021) even with the introduction of the Carbon Border Adjustment Mechanism (CBAM). More broadly, free allocation and compensation remain the dominant anti-leakage instruments across emissions trading schemes worldwide. Their persistence reflects both the political necessity of securing industry support for carbon pricing (Sato et al., 2022) and the continued risk of carbon leakage in an increasingly fragmented global political landscape (Neuhoff et al., 2025). Our findings make the costs and trade-offs of these policies more explicit. While shielding energy-intensive firms from carbon costs may help preserve competitiveness, it comes at the expense of higher electricity use and emissions – ultimately limiting the effectiveness of carbon pricing as a tool for industrial decarbonization.

CRediT authorship contribution statement

Piero Basaglia: Writing – review & editing, Writing – original draft, Validation, Software, Methodology, Investigation, Formal analysis, Data curation. **Elisabeth T. Isaksen:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Misato Sato:** Writing – review & editing, Writing – original draft, Software, Project administration, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Appendix A. Supplementary data

Supplementary data to this article can be found online at doi:10.1016/j.jeem.2025.103208.

Data availability

The data that has been used is confidential.

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